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Rethinking inhibition theory: On the problematic status of the inhibition theory for forgetting ☆

Jeroen G.W. Raaijmakers *, Emőke Jakab

University of Amsterdam, Amsterdam, The Netherlands

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ABSTRACT

The standard textbook account of interference and forgetting is based on the assumption that retrieval of a memory trace is affected by competition by other memory traces. In recent years, a number of researchers have questioned this view and have proposed an alternative account of forgetting based on a mechanism of suppression. In this inhibition account, such forgetting is due to an inhibitory control process that operates whenever non-target information hinders the retrieval of a specific target item. It is assumed that the memory traces of these non-target items are suppressed or inhibited in order to overcome their interfering effects and it is claimed that this inhibition has a longer-lasting effect on the strength of the suppressed memory traces. In this paper we critically review the claim that the inhibition theory provides a better account of forgetting than more traditional competition-based theories. We discuss the explanations that have been proposed to account for retrieval induced forgetting, the think/no-think paradigm, directed forgetting, the part-list cuing effect, output interference and list-strength effects. We conclude that the theoretical status of inhibition as an explanation for interference and forgetting is problematic. We show that the claim that these findings cannot be explained by standard competition-based accounts is incorrect.

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Introduction

During the past 15 years there has been a revival of interest in the idea that forgetting in human memory might be due to repression, a process whereby inappropriate or unwanted memories are actively suppressed and thereby (at least temporarily) forgotten (Anderson, 2003; Anderson, Bjork, & Bjork, 1994). Although such Freudian ideas have been highly controversial in the past, current versions of the inhibition hypothesis claim to be able to explain a large variety of experimental phenomena, including retroactive interference, list strength effects, part-list cuing, and output interference. Inhibition theorists have also speculated that these results may provide an explanation for such clinical phenomena as repressed memories (see Anderson, 2001; Anderson & Huddleston, 2011; Brewin & Andrews, 2000).

According to this hypothesis, inhibition arises as an adaptive response of the memory system whenever a target memory trace *A* needs to be retrieved in the presence of a strong competitor *B*. In such a situation the system engages a control process that suppresses the competitor item *B*, thus facilitating the recall of the target item *A*. Such active suppression of *B* then leads to a longer-lasting decrease in the strength of its memory trace (especially when it is suppressed on multiple occasions).

Although the general concept of inhibition is of course not new in theories of forgetting (see Anderson & Neely, 1996, for a review), there are a number of features that distinguish this inhibition account from previous proposals

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* Corresponding author. Address: Department of Psychology, University of Amsterdam, Weesperplein 4, 1018 XA Amsterdam, The Netherlands.

E-mail address: J.G.W.Raaijmakers@uva.nl (J.G.W. Raaijmakers).

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that employed the term "inhibition" or a similar concept. For example, in the 1970s, researchers such as Roediger (1973, 1974, 1978) investigated what were called "inhibiting effects of recall" but these effects simply referred to the negative effect that recall of one member of a set of items has on the probability of recalling the remaining members (i.e., output interference) without any implication that such effects were due to these items being suppressed. Similarly, one of the factors in the classic Two-Factor Theory for forgetting (Melton & Irwin, 1940) was "unlearning", described as a weakening of the cue-target association. The crucial difference between the inhibition theory as proposed by Anderson and colleagues and the older "unlearning" concept is that it is now assumed that the suppression does not lead to a decrease in the strength of the association between the cue and the target item but to a decrease in the strength of the memory trace itself of the target item.

The inhibition theory represents a radical departure from more traditional theories of forgetting such as ACT (J.R. Anderson, 1983) or SAM (Mensink & Raaijmakers, 1988) that are based on the assumption that the reason that the competitor item *B* is recalled less well is due to the increased strength of the memory trace representing the *A* item. These theories have been highly successful in explaining a large number of classic results in the field of interference and forgetting. Nevertheless, inhibition theorists have argued that a number of experimental findings contradict the predictions of these competition-based accounts of forgetting while supporting the inhibition account.

In this article we will present a critical analysis of the evidence advanced by inhibition theorists against competition-based accounts. We will argue that there are serious problems and inconsistencies in the way the inhibition account has been used to explain these findings. It is not at all clear that the inhibition account provides a better explanation than the more traditional competition-based accounts. Thus, we do not believe that there is sufficient reason to reject the traditional competition-based accounts in favor of the inhibition hypothesis as a general theory of human forgetting.

In the next sections, we will first review the classical theories for interference and forgetting followed by a review of the main theoretical assumptions of the inhibition account within the context of the retrieval practice paradigm. After that, we will present a detailed discussion of the application of the inhibition theory to a number of other standard empirical phenomena, including output interference and list strength effects, part-list cuing and directed forgetting.

Traditional accounts of interference and forgetting

The classical theory for interference and forgetting

The interference theory for forgetting was developed to account for the phenomena of retroactive and proactive interference that were observed in experiments in which two lists of (usually) paired associates were learned in succession (denoted as A–B and A–C), followed, after a retention interval, by a final test for either the first or the second list. Retroactive interference refers to the finding that when the final test involves the first list (A–B), performance is lowered due to the interpolated learning of the second list (A–C). Similarly, proactive interference refers to the decrease in performance on the second list, A–C, due to the prior learning of the first list, A–B, in comparison to a control condition in which either no interfering list is learned or an unrelated list (A–B, C–D).

The initial version of the interference theory (McGeoch, 1932, 1942) relied exclusively on the notion of response competition. It was, however, soon replaced by a two-factor account in which the forgetting that was observed was explained by the two factors of response competition and unlearning. Unlearning was defined as a decrease in the strength of the first-list associations due to the learning of the second-list associations (Melton & Irwin, 1940). Although the initial evidence for the unlearning assumption was not particularly strong (see Thune & Underwood, 1943; Underwood, 1945, 1948), later experiments by Barnes and Underwood (1959) provided more definitive evidence. They demonstrated using a procedure that was thought to eliminate response competition (the so-called MMFR method, basically allowing the recall of both responses if possible) that there was still evidence for retroactive interference. By the 1960s, the so-called Two-Factor Theory for interference (response competition and unlearning) was the dominant explanation for interference and forgetting. The theory was highly successful in explaining the existing data on both retroactive and proactive interference and was considered by many to be one of the most significant contributions of the functionalist approach to experimental psychology. Postman (1961) was not exaggerating when he started a review of the status of interference theory with the sentence:

> "Interference theory occupies an unchallenged position as the major significant analysis of the process of forgetting" (Postman, 1961, p. 152).

However, 15 years later in an Annual Review chapter, the same author wrote:

> "Interference theory today is in a state of ferment if not disarray (...) There is no lack of new data (...) but so far they have failed to resolve the basic theoretical issues." (Postman, 1975, p. 327).

Clearly, something had happened. So what were these "basic theoretical issues" that had not been resolved?

The basic problem had to do with the unlearning assumption. This assumption led to a number of predictions that could not be verified. First, no interference was observed when a recognition test was used (using a four-choice recognition procedure as was common in those days). However, if the associations had been unlearned (i.e., were at least temporarily unavailable), an effect should also have been present with recognition testing. Second, the assumption that MMFR testing eliminates response competition, led to the prediction that proactive interference should not be found when a MMFR test is used. This prediction followed from the fact that the second list responses could not have been unlearned and if both response competition and unlearning were not present, no interference could be predicted by the two-factor account. The data, however, showed clear evidence for proactive interference (Koppenaal, 1963). Third, the unlearning assumption seemed to lead to the prediction that in an A–B, A–C paradigm, the probabilities of recalling B and C should be negatively correlated (Greeno, James, & DaPolito, 1971; Greeno, James, DaPolito, & Polson, 1978). Since unlearning of A–B results from the learning of A–C, the theory should predict that the better A–C is learned, the more A–B should be unlearned. Such a correlation was not observed and the data clearly indicated that the recall probabilities of B and C were uncorrelated (see Greeno et al., 1971).

Although in later years new assumptions were proposed to adapt the classical interference theory to the problematic data (e.g., the response-set suppression hypothesis proposed by Postman, Stark, and Fraser (1968)), these never gained much popularity, primarily because a number of issues such as the occurrence of proactive interference on MMFR tests remained problematic.

Modern competition-based theories

In the 1980s a number of mathematical models were developed that were able to resolve the inconsistencies of the classical interference accounts (ACT*, see J.R. Anderson, 1981, 1983; SAM, see Mensink & Raaijmakers, 1988). What was crucial in these models is that they dropped the unlearning assumption and reinstated competition as the basic causal factor in interference. As such, these theories are closer to McGeoch's original formulation than to the two-factor formulation (see Mensink & Raaijmakers, 1988, p. 452).

The reason that these theories could dispense with the unlearning assumption was that they gave a more structural interpretation to the concept of competition. While the classic interference theories had assumed that competition was between available responses (and hence might be eliminated if subjects were instructed to give both responses, as in MMFR testing), these newer theories assumed that the competition was more intrinsic to the retrieval process, making it impossible to eliminate the competition by a simple instruction. In ACT* it was assumed that activation spreads to associated traces in proportion to their strength. The SAM model assumed that retrieval consists of two processes, sampling and recovery, and the probability of sampling a trace was assumed to be a function of the relative strength of the target trace compared to all other traces associated to the cues used (see Raaijmakers & Shiffrin, 1981). Thus, in these models the locus of the competition is before the generation of the responses rather than after the responses have been generated, and, as a result, the use of a MMFR test will not eliminate all competition.

Mensink and Raaijmakers (1988) showed that many of the inconsistencies that had plagued the traditional interference account could be resolved by such a model. In particular, they showed that a multiple-choice recognition test was unlikely to show an interference effect except under special circumstances. Second, the model could handle phenomena that had been problematic for the two-factor account such as proactive interference with MMFR testing and the lack of a negative correlation between the recall probabilities of B and C as observed by Greeno et al. (1971). Mensink and Raaijmakers (1988) thus demonstrated that a coherent account of the older interference literature was possible using a model based on a competitive retrieval process and incorporating the notion of contextual cuing.

Perhaps even more importantly, these models were able to explain not "just" the data from the interference literature but also data from a variety of other paradigms such as single-list free recall and recognition. For example, the SAM model for free recall accounts for serial position effects, list-length effects, interresponse times, hypermnesia, and part-list cuing. More recently, this framework (now termed SAM–REM) has been generalized and applied to a number of other memory paradigms (e.g., recognition, implicit memory, lexical decision, and directed forgetting). For a brief review of the SAM–REM theory, see Raaijmakers (2008).

So by the end of the 1980s, there seemed to be a consensus among memory researchers about the idea that interference phenomena were due to retrieval being sensitive to the relative strength of associations. However, in the following years this state of affairs changed quite drastically, primarily due to a number of publications by Anderson et al. (1994), Anderson, Bjork, and Bjork (2000) and Anderson and Spellman (1995). These authors criticized the idea of competition as a cause for forgetting and reintroduced the unlearning assumption (albeit in a somewhat different form) as a major factor in interference and forgetting. In the remainder of this article we will evaluate this inhibition account of forgetting. Our objective is not to give an extensive review of all experiments done in this area. Rather, we will critically examine the major findings in order to determine whether they indeed provide conclusive evidence against the competition-based accounts of forgetting.

Inhibition theory: retrieval induced forgetting

The inhibition account of forgetting, initially proposed by Anderson et al. (1994),[1] represents a radical break with competition-based theories for interference. They assumed that the retroactive forgetting that is observed in A–B, A–C interference tasks is not due to the A–C association competing with retrieval of A–B at the time of testing but is a consequence of an adaptive control mechanism that operates during the learning of the A–C list. That is, while the subject is trying to retrieve the C response, the (stronger) B response is competing and in order to resolve the competition, the memory trace for B has to be suppressed. The crucial assumption of the inhibition theory is that such repeated suppression leads to a longer-lasting inhibition of the memory trace for B. Hence, the forgetting observed at the later test is due to the fact that the memory trace for B has become weaker and is now less likely to be retrieved.

[1] In this article we reserve the term "inhibition theory" for the account proposed by M.C. Anderson and his colleagues. We realize that there are other theoretical proposals that make use of the concept of inhibition but the Anderson account is the best developed one and is generally regarded as the most prominent representative of the general notion of inhibition.

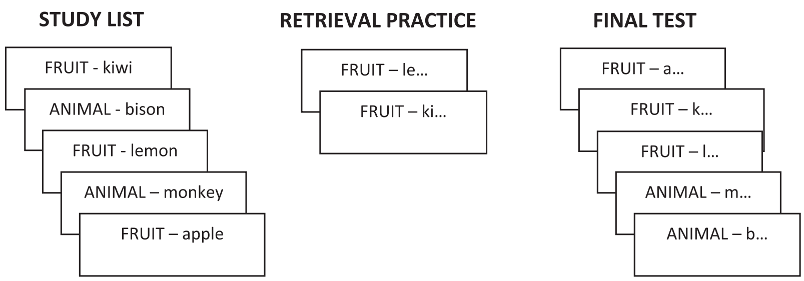

Fig. 1. Standard design of a retrieval induced forgetting experiment. In this example, *lemon* and *kiwi* are practiced or RP+ items, *apple* is a nonpracticed items from a practiced category (a RP− item), and *monkey* and *bison* are items from a nonpracticed category (NRP items).

In order to better understand the inhibition account, it is helpful to consider the paradigm that has been most frequently used to demonstrate inhibition, i.e., the *retrieval induced forgetting* (RIF) paradigm, introduced by Anderson et al. (1994). Fig. 1 describes the basic structure of such an experiment. First, a list of word pairs is presented for study, each word pair consisting of a category name and an exemplar from that category. All items are taken from a relatively small number of categories (usually about 8), each category represented by about 6–8 exemplars. Thus the list might consist of pairs such as *FRUIT-lemon*, *ANIMAL-bison*, *SPORT-sailing*, and *FRUIT-banana*. After this list has been presented once or twice, some of the items from some of the categories are given additional retrieval practice in which the category name and the first letter or letters of the target item are given as cues (e.g., *ANIMAL-bi* . . .). Not all categories are present in the retrieval practice task, hence there are practiced and nonpracticed categories (denoted as RP and NRP categories). In addition, not all items from a practiced category are given additional practice. The practiced items are denoted as RP+, the nonpracticed items from a practiced category are denoted as RP−. Thus, there are three types of items: RP+, RP− and NRP items. In a standard RIF experiment, each RP+ item is given three retrieval practice trials. A potentially important feature of these experiments is that no feedback is given during the retrieval practice phase of the experiment. If an item is not retrieved on the first retrieval practice trial, it is unlikely to be retrieved on later trials and hence such a RP+ item will in fact not be strengthened. Finally, after an unrelated intervening task (usually lasting about 20 min) a final test is given in which all items from all categories are tested, either using a category-cued free recall procedure or by giving the category name plus the first letter of the target item. The latter procedure is preferred since it eliminates contamination of the results by output interference effects.[2]

[2] Although the standard procedure involves presenting the category name plus the two initial letters during retrieval practice and presenting the category name plus the initial letter during the final test, a more optimal procedure would be to give only the first letter during the retrieval practice and the two initial letters during the final test since in that way one would increase the competition during the retrieval practice (hence maximizing the inhibition effect) and decrease the competition during the final test. The standard procedure on the other hand, decreases competition during retrieval practice and increases competition at the final test. The rationale for this choice is not clear to us.

According to the inhibition account, the probability of recall for the RP− items will be lower than that for the NRP items since the memory traces of the RP− items have been inhibited during the retrieval practice on the RP+ items (the difference between the recall of the NRP and RP− items is termed the *RIF effect*). That is, since the RP− items are activated during the attempt at retrieval of the RP+ items, there is a conflict that has to be resolved by an active control process that is aimed at reducing the activation of the incorrect responses (i.e., the RP− items). It is this cognitive control process that is responsible for the later forgetting of the RP− items, not the fact that the practiced items (RP+) have been strengthened. As stated by Anderson (2003, p. 416):

> "By this view, memory retrieval presents a special case of a broad class of situations that recruit executive control processes; it is the executive control mechanism that overcomes interference – inhibition – that causes us to forget, not the competition itself. This view departs from the common assumption that forgetting is a passive side effect of the ever-changing structure of memory. The mere storage of interfering traces is not what causes memories to grow less accessible with time. Rather, forgetting, whether incidental or intentional, is produced as a response to interference caused by activated competitors in memory."

Anderson (2003) lists a number of properties of this inhibition effect that supposedly uniquely support the inhibition hypothesis:

(a) *Interference dependence*: Whether or not an effect will be observed, depends on the extent to which the RP− items are competing during the practice of the RP+ items. Hence, strong competitors should be inhibited more than weak competitors.
(b) *Cue independence*: Since it is assumed that inhibition leads to a decrease in the trace strength of the RP− items, the effect should not only be seen in tests using the original category cue but also when a different cue (that normally would lead to the retrieval of the RP− item with some nonzero probability) is used.
(c) *Retrieval specificity*: The inhibition account predicts that a RIF effect should only occur if the retrieval practice trials are such that they involve the active retrieval of the RP+ items in a way that makes it

possible for the RP− items to hinder the retrieval of the target RP+ item. Only in that case will there be a need for the suppression of the memory traces of the RP− items. Any other type of practice may lead to strengthening of the memory traces of the practiced items but will not lead to inhibition of the memory traces of the RP− items.

(d) *Strength independence*: It is assumed that the size of the effect is determined only by the strength of the competing items, not by the extent to which the memory traces of the practiced items (RP+) have been strengthened.

Clearly, these properties are not independent (for example, both (a), (c), and (d) are based on the assumption that it is the competition during retrieval practice that determines the amount of inhibition observed). However, since these properties have been discussed separately in the literature, we will follow this convention and structure our discussion accordingly.

In many ways the inhibition hypothesis is quite similar to the older Two-Factor Theory. In both accounts it is assumed that study-test trials on new items lead to the weakening (unlearning) of previously stored memory traces and that competition effects at recall are basically blocking effects that can be eliminated by using an appropriate testing procedure. Thus, the Two-Factor Theory assumed that competition could be eliminated by the use of a MMFR test or by recognition testing. Similarly, proponents of the inhibition account have assumed that competition does not affect the probability of recall on item-specific tests (including recognition tests). There is a crucial difference though in that the inhibition hypothesis assumes that it is not the A–B association that is weakened but that some more generic representation of the B response is inhibited. Hence, this inhibition should be seen not just when A is used as a cue but also in other tasks where B has to be retrieved.

The assumption that competition does not affect the probability of recall is rather remarkable, especially since competition is said to be the reason why there is a need for inhibition in the first place. One could easily imagine a version of the theory that would assume that both competition and inhibition jointly determine the amount of forgetting observed, just as in the classical two-factor interference theory. However, such a model would not make the predictions described above. For example, such a theory would no longer predict that the amount of forgetting is unrelated to the extent to which the practiced items have been strengthened (i.e., the strength independence property). One interpretation of this somewhat paradoxical assumption might be that although competition does not affect the probability of recall, it does affect the latency of recall (Shivde & Anderson, 2001, p. 176), and that the inhibition serves to decrease the effect of competing items on the latency of generating the target item (i.e., speeding up its retrieval).

To avoid misunderstanding it should be mentioned that the inhibition theory does not assume that competition never affects recall. There are several situations in which competition may have an effect on recall. First, the inhibition account assumes that when the ability for inhibitory control is for some reason lacking or compromised, stronger responses may block weaker ones (Anderson, 2003, pp. 439–440). Second, when response time is limited or when subjects are instructed to make the first response that comes to mind, competition may also affect the probability of recall. Third, in a free recall like situation in which the stronger items are output first, the weaker items may suffer from output interference effects. However, when such factors are absent, the inhibition account assumes that the strength of competing items does not affect the probability of recall (see also Bäuml, 2008). Clearly, this is quite different from the way in which the concept of competition is used in modern interference accounts such as SAM and ACT. We will return to this issue in the general discussion section.

In the next sections we will first discuss the four abovementioned properties of the inhibition account, and evaluate whether they indeed provide unique evidence in favor of the inhibition proposal and against non-inhibitory accounts of forgetting based on strength-based competition such as SAM or ACT.

Interference dependence

According to this principle, the inhibition should be dependent on the extent to which the response B competes with the retrieval of C. That is, when the item A–C is given additional retrieval practice, there will be no need to suppress the B item if B does not intrude or compete for retrieval in response to the retrieval cue A. Hence, inhibition will only occur when B is sufficiently strong to interfere with the recall of C. If the association A–B is weak and does not compete for retrieval with A–C, no inhibition of the B trace will occur. Note that what is important here is that B competes for recall, not whether retrieval of C eventually succeeds (see Storm, Bjork, Bjork, & Nestojko, 2006, for a demonstration that retrieval success is not required for inhibition to occur).

Anderson et al. (1994) tested this assumption in an experiment in which they compared the amount of RIF obtained for categories of which all presented items were strong exemplars of the category versus categories of which the presented items were all weak exemplars. Consistent with the interference dependence assumption they observed a RIF effect only for the strong categories. Anderson et al. (1994) argued that competition-based accounts of forgetting made the opposite prediction (although in their Appendix they showed that such a prediction might not be a necessary property of more elaborate competition-based models, a conclusion we verified using simulation results, see Jakab & Raaijmakers, 2009, p. 608).

A problematic aspect of these experiments is that no feedback was given during the retrieval practice. Since items that are not retrieved on the first practice trial will most likely not be retrieved on subsequent trials, this implies that any increase in associative strength for the practiced items (RP+) will be limited to the items that are correctly retrieved. Since there will be more of these for the strong categories, this may also lead to a smaller RIF effect for the weak categories according to a competition-based account.

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Although earlier formulations of the inhibition account might lead to a different conclusion, more recent formulations (see Anderson & Levy, 2010) suggest that the inhibition account will not always predict a larger RIF effect for stronger competitors. According to Anderson and Levy (2010, pp. 120-122), inhibition may fail if the competitors are too strong. In such cases, the measurement of inhibition becomes difficult due to what they termed the *Demand/Success Tradeoff* problem. That is, as the demand for inhibition increases, the likelihood of its success decreases. Hence, the attempt at inhibition may not succeed and the RP– items may in fact become stronger than they would have been without the retrieval practice on the RP+ items (this is referred to as the *carryover assumption*). Thus, "retrieval practice may cause no RIF or even facilitation of items that one might expect to be inhibited" (Anderson and Levy, p. 121). According to these authors, inhibition will become visible if the number of retrieval practice trials is increased, hence they predict a non-monotonic relation between the number of retrieval practice trials and the amount of RIF observed. Thus, the critical evidence for inhibition no longer seems to be a decrease relative to the NRP condition but a decrease as the number of retrieval practice trials increases, even when after a large number of practice trials the performance on the RP– items is at the same level or even higher than that on the NRP items.

Although this is not emphasized in the discussion by Anderson and Levy (2010), it should be clear that the prediction of the non-monotonic relation cannot be deduced from the demand/success hypothesis itself but depends on yet another hypothesis, i.e., that the effectiveness of the cognitive control processes that lead to the inhibition increases with the number of retrieval practice trials. Otherwise, one would predict more and more facilitation since with each retrieval practice trial the RP– items become even stronger than they were at the outset. Anderson and Levy do mention this additional assumption, although only several pages after the demand/success hypothesis itself is introduced: "Interestingly, despite the carryover, which ought to make the competitor even more intrusive, people appear to be able to dynamically adjust cognitive control to better handle high levels of competition on subsequent trials – a finding reminiscent of evidence for conflict adaptation in studies of executive control (see Botvinick, Cohen, & Carter, 2004)." Anderson and Levy (2010, p. 125). However, although dynamic adjustment of cognitive control has been well established in other tasks, the exact nature of the dynamic adjustment in the present tasks remains to be specified (e.g., whether it operates on the list level or on the item level, see Blais, Robidoux, Risko, & Besner, 2007).

Obviously, with these assumptions, the interference dependence property becomes difficult to test empirically since the theory may predict both more inhibition for stronger competitors as well as less inhibition or even facilitation. This is most clearly illustrated by a consideration of the experiments of Shivde and Anderson (2001). In his review article Anderson (2003) claimed that these experiments provided strong evidence for the interference dependence assumption. We will argue that such a conclusion is not really justified.

In these experiments, the participants were given associated word pairs in which the cue item was a homograph that was either paired with an item associated with the dominant meaning or with the subordinate meaning (e.g. *Arm* was paired with either *Shoulder* or *Missile*). The number of retrieval practice trials was either 0 (the NRP condition), 1, 5 or 20. When the retrieval practice was on the dominant meaning, no RIF effect was observed, even after 20 retrieval practice trials. According to Anderson (2003, p. 421), "Practice on the subordinate sense, however, caused retrieval induced forgetting of the dominant sense." However, we do not believe that the evidence justifies such a conclusion. In Experiment 1 of Shivde and Anderson (2001), the RIF effect was about 5% when a standard retrieval practice procedure was used, compared to 14% when the practice procedure consisted of extra exposures of the subordinate meaning (which should not lead to inhibition since there is no need to retrieve the studied items when they are presented, according to the *retrieval specificity* assumption). In Experiment 2 the RIF effect was even smaller (about 1%) after 20 retrieval practice trials, while the conditions with 1 or 5 retrieval practice trials resulted in a reversed RIF effect or facilitation (i.e., an improvement in recall compared to the NRP condition).

According to Anderson and Levy (2010) such results are to be expected according to the inhibition account if one takes the Demand/Success Tradeoff problem into account. In this view, what is important is not so much the initial rise in recall but the fact that with an increasing number of retrieval practice trials the probability of recall of the RP– items declines. In their Experiment 2, the decline in RP– recall as a function of the number of practice trials was indeed larger in the retrieval practice condition (61%, 67%, 64%, and 60% recall after 0, 1, 5, and 20 trials) compared to the extra exposures condition (60%, 60%, 63%, and 62% recall after 0, 1, 5, and 20 trials). However, in Experiment 1, the decline was quite a bit larger in the extra exposures condition (83%, 77%, 74%, and 69% after 0, 1, 5, and 20 trials) compared to the retrieval practice condition (81%, 77%, 73%, and 76% after 0, 1, 5, and 20 trials). If the decline in the retrieval practice condition is due to the dynamic adjustment of cognitive control, one wonders what is causing the decline in the extra exposures condition.

Moreover, the Demand/Success Tradeoff hypothesis is based on the idea that the inhibitory control processes are unsuccessful due to the fact that the competing items are too strong. If this is correct, one would expect that in experiments that show such facilitation rather than inhibition after only a few retrieval practice trials, the competitor items should be quite a bit stronger than in experiments in which no such facilitation is observed. One measure of the strength of the competitor items may be obtained from the probability of recall for the corresponding NRP items (that were presented only on the original list). In Experiment 1 of Shivde and Anderson (2001), the probability of recall for the dominant meaning was 82%. However, in Experiment 2 of Anderson et al. (1994) in which the stem-completion procedure was also used, the probability of NRP recall for the strong items was 83%, hence nearly equivalent. Nevertheless, only the Shivde and Anderson experiment showed the reversed RIF effect. Similarly, in

Johnson and Anderson (2004) there was a nonmonotonic relation in Experiment 1 but not in Experiment 2 although performance on the NRP items was about 20% better in Experiment 2. Although such results might perhaps be accounted for by task differences, it does show that there is no simple rule that predicts when the competitors are too strong.

From this analysis we may conclude that the recently proposed Demand/Success Tradeoff hypothesis does not provide a satisfactory account of these results. Moreover, the claim of Anderson (2003) that the Shivde and Anderson (2001) results provide strong evidence for the interference dependence assumption is clearly not justified.

Other research aimed at testing the interference dependence assumption also presents a mixed picture. Although the Anderson et al. (1994) results supported the assumption, other researchers have not been able to replicate these results (Williams & Zacks, 2001), showing instead equal amounts of RIF for weak and strong items. Jakab and Raaijmakers (2009) varied strength by manipulating the serial position of an item within the category (early positions showing higher recall) and by presenting some items twice during the initial study phase. In all three experiments, the RIF effect was equally large for the stronger and the weaker items. Moreover, this effect could not be attributed to the effects of integration (strong associations between the RP+ and RP− items, see Anderson & McCulloch, 1999).

Although some proponents of the inhibition account have tried to account for these results by attributing the results to the lack of control for output interference, such an argument is not really convincing. For example, Storm (2010, p. 96) argued that the Williams and Zacks' results could be explained by assuming that the final recall for the strong items was impaired as a consequence of inhibition during retrieval practice, whereas the final recall of the weak items was impaired as a consequence of output interference on the final test. A similar critique was raised by Storm and Levy (2012) against the Jakab and Raaijmakers (2009) results. However, output interference should affect the strong items as well, and in fact will have a larger effect on the strong items compared to the weak items (see Bäuml, 1998). Anderson et al. (1994, Exp. 2) did indeed find a reliable effect of output position for the strong items but no such effect for the weak items. However, even disregarding the fact that it is unclear why the strong items would not be affected by output interference, such arguments also ignore the fact that the weak NRP items should also suffer from output interference.[3] Hence, it is not likely that the discrepant results in these experiments can be explained by the lack of control for output order.

In addition, Perfect et al. (2004) pointed out that in later experiments (Anderson & Spellman, 1995) a sizable RIF effect was obtained for category exemplars that would seem to be equally weak as the weak exemplars from the Anderson et al. (1994) experiments. For example, in Anderson and Spellman (1995, Exp. 1) final recall for the control items (the NRP-S items in the unrelated condition) was 38% while the recall for the weak NRP items in Anderson et al. (1994) was 41%. It is not clear why a RIF effect should be obtained in the former case but not in the latter. Finally, Verde (2012) lists a large number of experiments that did obtain a RIF effect but for items that should have been considered weak (although there is obviously room for debate here given that there is no generally accepted measure for what should be considered a weak competitor).

Other experiments, on the other hand have provided evidence that can be seen as supporting the interference dependence assumption. Storm, Bjork, and Bjork (2007) tested the interference dependence assumption by manipulating the strength of the competitor items through a directed forgetting instruction. Their results showed that to-be-forgotten items did not show a RIF effect but to-be-remembered items did. Hence, the directed forgetting instruction appeared to have transformed the items to "weak" items that were no longer competing during the retrieval practice phase. However, a closer look at their procedure reveals problems. First, in this experiment the retrieval practice was given not on list items (the RP+ items) but on extralist items from the same category. On the final test, however, subjects were instructed to only recall the items presented on the original list. Such a procedure creates problems of list discrimination, especially for list items that were (perhaps inadvertently) activated during the practice on the extralist items. This will be more likely for to-be-remembered than for to-be-forgotten items, e.g., because these items may have been rehearsed after the presentation of the original list. Second, the probability of recall on the final test was about 61% for the to-be-remembered NRP items and about 50% for the to-be-forgotten NRP items. It seems unlikely that such a small difference would be sufficient to completely eliminate the RIF effect for the "weak" items. Moreover, as we will discuss later, the available evidence (Gelfand & Bjork, 1985) shows that a forgetting instruction by itself (without List-2 learning) does not lead to a directed forgetting effect, hence it is not clear why a directed forgetting effect should be observed for the NRP items as there was no second list for these items.

A novel approach to testing the interference dependence assumption was presented by Healey, Campbell, Hasher, and Ossher (2010). They presented their subjects lists that contained orthographically similar word pairs (such as ALLERGY and ANALOGY). In the second phase of the experiment, subjects were given a word fragment completion task (e.g., A_L__GY). It was assumed that the orthographically similar word (in this case ANALOGY) would interfere during the word fragment completion task. In order to resolve this competition, the memory representation of ANALOGY would have to be suppressed. The suppression was tested in the third phase of the experiment in which a word naming task was given. It was shown that the naming times for the supposedly suppressed words (ANALOGY) were slower when the orthographically similar

[3] We verified this conclusion using a version of the SAM model in which the strong items were assumed to be inhibited (by decreasing their strength prior to the start of the retrieval process) and in which a strong output interference effect was assumed. In all of the simulations the RIF effect was small for the weak items and not affected by whether there was output interference or not. If anything, the output interference slightly increased the difference in the size of the RIF effect for strong and weak items.

item had been tested in the fragment completion task (the interference condition) than when the fragment completion involved a different word (the no-resolution condition) or when there was no orthographically similar competitor presented on the initial list (the no-conflict condition). In both of these latter conditions there would have been no need for suppression.

Although Healey et al. (2010) interpreted these findings as "direct evidence for inhibition", it is not too difficult to see that these results are in fact perfectly compatible with a non-inhibitory, competition-based account if one assumes that the final naming task is itself sensitive to competition or interference (a not unreasonable assumption given that naming is affected by the activation of orthographic and phonological competitors, see Bowers, Davis, & Hanley, 2005; Burt, 2009; Grainger, 1990). This will affect the interference condition more than the no-resolution condition due to the fact that the competitor item has been strengthened in the fragment completion task.[4] In addition, Cho and Neely (2010) pointed out that the analysis carried out by Healey et al. (2010) in which they eliminated all trials in which the word fragment was not correctly solved, leads to an artificial decrease in performance in the interference condition. That is, such a procedure will bias the results because it will selectively eliminate the stronger competing items and since this only occurs in the interference condition, there will be a tendency for the items in that condition to be weaker than the items in the other conditions, quite independently of any inhibition that may have occurred. Hence, performance in the interference condition may be reduced, even when none of the items have been inhibited.

A similar experiment was reported by Cho and Neely (2010). After the presentation of a study list, they gave their subjects a number of retrieval practice trials using a word fragment completion task. On some of these trials the word fragment cued a nonstudied word (e.g., ELE_A___ for ELEVATOR) that was orthographically similar to a studied word (ELEPHANT). As expected, performance was indeed worse on these word fragments (by 11.5%) compared to nonstudied words that were not related to any of the list items. Thus, the orthographically similar list items were indeed interfering and hence should have been suppressed according to the inhibition account. However, a final cued test involving either semantically related cues (*giraffe*) or an orthographic cue (E__PH_NT) showed no evidence of inhibition. Instead, a facilitation effect was observed that increased with the number of retrieval practice trials (ranging from 1 to 10).

In sum then, the evidence for the interference dependence assumption is weak (see Verde, 2012, for a similar conclusion). Moreover, since the inhibition account does not consistently predict larger RIF effects for strong items compared to weak items due to the *Demand/Success Tradeoff* assumption, such experiments will not provide conclusive evidence one way or the other.

[4] This conclusion was reached independently by James Neely (personal communication, March 3, 2011).

Cue independence

The second fundamental prediction of the inhibition theory is that the inhibition should be cue-independent. That is, this inhibition of B should be seen not just when the original A cue is used but also when a different cue (that normally would lead to the response B with some nonzero probability) is used. The idea behind this prediction is that it is the trace itself that is inhibited, not the associative link between the cue and the target item (the trace of B itself is inhibited rather than the association A–B). This assumption clearly distinguishes the inhibition theory from competition-based accounts of interference that have assumed that learning a second association A–C does not change the strength of the A–B association.

Anderson and Spellman (1995) devised a procedure to test this prediction. They presented subjects lists that were composed in such a way that in one condition (the "related" condition) some items were presented as a member of one category but also belonged to another category on the list. For example, the item *tomato* was presented as a member of the category *RED* but also belonged to the category *FOOD*. Other items on the list were presented as a member of the *FOOD* category but also belonged to the *RED* category (e.g., the item *strawberry*, see Fig. 2). In the other condition (the "unrelated" condition) no such shared categories were present. One of these shared categories was given retrieval practice, the other category was not given retrieval practice (e.g. *RED* was the RP category and *FOOD* was the NRP category). What is of interest here is the comparison of the performance on the items from the NRP category between the related and the unrelated conditions, and in particular the performance on the similar item from the NRP category (*strawberry*, called the NRP-S item) after retrieval practice of the *RED* category. In the related condition, Anderson and Spellman observed a standard RIF effect for the nonpracticed members of *RED* category (the RP− items) but also for items in the nonpracticed category *FOOD* that also belonged to the *RED* category, the NRP-S items (i.e., the item *strawberry*). In the unrelated condition, no such effect was present for the corresponding items from the NRP category.

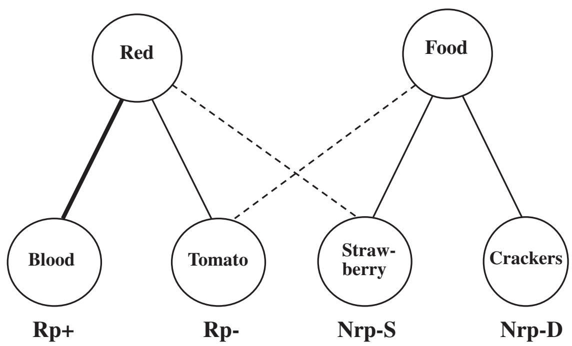

Fig. 2. Design used by Anderson and Spellman (1995, Exp. 1) to demonstrate crosscategory inhibition. Solid lines indicate studied category–exemplar pairs, dashed lines indicate preexisting relations not studied in the experiment. The heavy line indicates the category–exemplar pair that received additional retrieval practice.

Such a result seems to pose a problem for competition-based explanations of RIF since the other members of the NRP category have not been practiced, hence why should there be a decrease in performance on the NRP-S items when they are tested with the NRP category cue? These theories assume that the strength of the trace of the NRP-S item has not changed, hence any difference between the NRP-S and the unrelated NRP items (denoted as NRP-D for dissimilar) would have to be due to an increased strength of other traces associated to the *FOOD* retrieval cue. Since none of these are changed due to the retrieval practice of items from the *RED* category (none of which are *FOOD* items), the competition view would not predict a RIF effect for the NRP-S items. The inhibition account on the other hand provides a simple explanation for such a result: during retrieval practice of the *RED* category, the NRP-S items are also activated and hence these items will also have to be suppressed.

A closer look at the paradigm used by Anderson and Spellman (1995) shows that the evidence against non-inhibitory accounts may be less strong than originally assumed. First, it might be assumed that the NRP-S item *strawberry* is indeed (as assumed by the inhibition account) activated during the retrieval practice of the *RED* category. This might lead to the storage of additional features, in particular features corresponding to the RED category (since the item is activated in response to that cue). If this is the case, then the item might be less likely to be activated on a later test with the *FOOD* category cue. That is, contemporary non-inhibitory accounts (e.g., the SAM–REM model, see Raaijmakers, 2008) assume that the likelihood of activating an item is based both on the number of overlapping and the number of non-overlapping features and hence an increase in the number of *RED* features will decrease the likelihood of activating the item using the *FOOD* cue. Note that this explanation would predict a reversed inhibition effect if the NRP-S was tested using the *RED* cue rather than the *FOOD* cue.[5] Interestingly, this explanation attributes the decrease in recall for the NRP-S items to an increase in the number of stored *RED* features rather than a decrease in those features as proposed by the inhibition account.

Second, as already noted by Anderson and Spellman (1995, p. 78), there may be a general tendency for a decrease in recall for items that belong to several categories presented on the list, for example because subjects are confused as to which category was the "correct" one. In the Anderson and Spellman design, such a tendency would decrease performance for the RP− and NRP-S items, quite apart from any inhibition or competition.

Third, the cue independence finding has been criticized by a number of authors (Camp, Pecher, & Schmidt, 2005, 2007; Perfect et al., 2004). The basic critique is that the procedure used by Anderson and Spellman (1995) does not eliminate the possibility of covert cuing. That is, when subjects are given the independent cue (*FOOD*), they might be tempted to use the original category cue (*RED*), especially since half of the *RED* items were also *FOOD* items. Such covert cuing in effect makes the independent cue dependent.

In a recent paper, Hulbert, Shivde, and Anderson (2012) claim to provide crucial evidence against this covert cuing hypothesis. They showed in a replication of Anderson and Spellman (1995, Exp. 1) that a cross-category inhibition effect (the decrease in recall in the NRP-S condition for the related condition versus the unrelated condition) was only obtained when the RP+ items were given retrieval practice, not when these items were given additional study by extra presentations. In the latter condition there is no need for inhibitory control (i.e., the retrieval specificity assumption). According to a covert cuing explanation, such covert cuing should be present in both conditions, irrespective of whether the strengthening of the RP+ items was by retrieval practice or by extra study presentations. However, here again the result seems to be due mainly to differential performance in the unrelated conditions since the results for the related conditions are highly similar in the retrieval practice and the extra presentations conditions (see Table 1). Although this was not tested, it does not appear likely that there was a significant difference between these two conditions in the performance on the related categories. From an inhibition perspective such an equality is quite surprising since only the retrieval practice condition is assumed to be affected by retrieval inhibition.

One problem with the covert cuing hypothesis is that it has never been clearly spelled out how this is supposed to work. Is the covert cue used instead of the provided cue, and if so, why? According to Perfect et al. (2004), subjects notice that the independent cue FOOD is ineffective and therefore switch to the more effective cue RED. These authors loosely refer to the encoding specificity principle but the exact rationale for why the FOOD cue is ineffective and how subjects notice that is not specified. If on the other hand the covert cue is used after the independent cue or both cues are used jointly, it is not evident that a negative effect would be predicted. Huddleston and Anderson (2012) therefore argue that covert cuing might occur but that, if it occurs, it will mask the cue-independent forgetting rather than cause it. Camp et al. (2005), however, obtained an effect on an implicit memory test with independent cues only for those subjects that were aware of the relation of the cues to the previous phase of the experiment. On the assumption that the use of a covert cuing strategy is more likely for aware subjects, such a result seems to run counter to the idea that covert cuing masks the cue-independent forgetting.

Table 1
Percentage correct on the final recall test in Hulbert et al. (2012).

Practice condition	Category relatedness	RP+	RP−	NRP-S	NRP-D
Retrieval practice	Unrelated	65	22	35	42
	Related	65	25	24	37
Extra presentations	Unrelated	69	28	30	39
	Related	63	26	28	42

[5] We have tried to test this prediction but had to give up since we could not replicate the original finding. Initial results were presented in Raaijmakers and Jakab (2006).

In sum, the results of such experiments might be reconciled with a non-inhibitory account. However, the arguments that we mentioned will not provide an explanation for the results of another experiment of Anderson and Spellman (1995, Exp. 2). In this experiment they used categories that were unrelated (such as *SOUPS* and *GREEN*) but in such a way that some of the members of the *SOUPS* category and some of the members of the *GREEN* category could also be considered members of a shared implicit category (e.g., *VEGETABLE*). Thus the *SOUPS* category had members such as *onion* and the *GREEN* category had members such as *lettuce*, with *onion* and *lettuce* both members of the not-presented or implicit category *VEGETABLE* (these items that shared the category *VEGETABLE* were always either RP− or NRP-S, depending on whether their original category was practiced or not).

Anderson and Spellman (1995) observed not only a RIF effect for the RP− items but also for NRP-S items when the recall of these items was compared to the recall of the corresponding items when there was no relation between the members of the RP and NRP categories (the *unrelated* condition). This result should perhaps be qualified since, as Perfect et al. (2004) have argued, the RIF effect observed by Anderson and Spellman (1995) appears to be due more to a unexplained increase in performance in the control condition than to a real decrease in performance on the NRP-S items. Moreover, other researchers have not obtained the decrease in recall for the NRP-S items (Camp et al., 2005; Williams & Zacks, 2001).[6] Nevertheless, this is a surprising finding, not in the least since the original inhibition account as proposed by Anderson et al. (1994) would not have predicted it. That is, in this experiment forgetting was observed for items that would not have been activated during retrieval practice of the RP+ items (*onion* should not have been activated during practice of the *GREEN* category). Hence, there should have been no need to suppress these NRP-S items. It appears as though the inhibition that affects the RP− item spreads to items that are not related to the practiced category but are related to the suppressed RP− items.

In order to explain the RIF effect for NRP-S items, Anderson and Spellman (1995) introduced a new version of the inhibition theory, the *pattern-suppression* model. In this model it is assumed that the traces of the items may be represented as Venn diagrams with the elements of the sets consisting of the semantic features of the items (see Fig. 3). Similarity relations between two items (including categorical relations) are represented as overlap between the sets of features representing the two items. The pattern suppression model makes two basic assumptions about what happens when a RP− item is activated during the retrieval practice of the RP+ item: (1) features of an RP− item that it shares with the RP+ item are not suppressed as this would be counterproductive (it would decrease the effect of the retrieval practice), only features of the RP− item that are not shared with the RP+ item are suppressed and (2) if a given feature is suppressed, it will affect the traces of all items that share that feature. Thus, when the item *lawn* is practiced, the *GREEN* features of *lettuce* will not be inhibited (on the contrary, they will be strengthened due to the practice on the GREEN category, see Anderson & Spellman, 1995, p. 91) but the other features of *lettuce* will, including the features that represent the *VEGETABLE* category. The suppression of these latter features is responsible for the decrease in recall performance for the NRP-S item *onion*, leading to a RIF effect for *onion* despite the fact that the item *onion* itself was not suppressed.

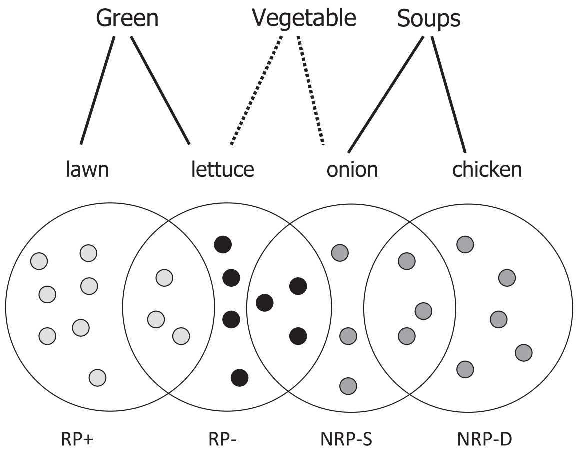

Fig. 3. Schematic illustration of the pattern suppression model as used by Anderson and Spellman (1995, Exp. 2) to account for inhibition for items that share an implicit categorical relation. Items are represented as sets of features (represented by the small circles). Light-gray circles indicate features of Rp+ that are strengthened, black circles indicate features that are suppressed, and dark-gray circles indicate features unaffected by the retrieval practice.

This pattern suppression model has been quite useful in explaining a number of findings. For example, it explains why there will be no or a reduced RIF effect when the RP− items share many features with the RP+ items. Conversely, when the RP− items share features with other RP− items (rather than with the RP+ items), the RIF effect will be larger. These predictions from the pattern suppression model were indeed verified by Anderson, Green, and McCulloch (2000) and Anderson and McCulloch (1999). However, despite these successes, there are a number of problems with the pattern suppression model.

First, the logic that led to the original prediction that strong category exemplars should be inhibited more than weak category exemplars (Anderson et al., 1994) becomes problematic in the pattern suppression model and will no longer necessarily hold. The problem is that the strong exemplars will obviously have more category features than the weak exemplars (after all, that's why they are generated more frequently). The pattern suppression model assumes that the category features of the RP− items will not be suppressed, only the other non-category features. But since the strong RP− items have fewer suppressable features than the weak RP− items, the magnitude of the RIF effect for strong items versus weak items will depend on the balance between the number of other features versus the likelihood that a weak item will be activated during

[6] During the preparation of this article we have heard about many other failures to replicate these results. However, most of these failures have not been published. We will restrict ourselves in this article to published experiments.

the retrieval practice of the RP+ items. That is, the weak RP− items will be less likely to be activated and suppressed but the number of features that might be suppressed is much larger than for the strong RP− items. In fact, under the pattern suppression model, it is not even guaranteed that the overall effect has to be negative for the strong RP− items. The reason for this is that the overall effect depends on (a) the effect of strengthening for the category features (due to the retrieval practice on the RP+ items) and (b) the effect of the suppression of the non-category features. There is nothing in the pattern suppression model that guarantees that the suppression effect has to outweigh the strengthening effect.

Second, the model explains the decrease for the NRP-S items in the shared implicit category experiment of Anderson and Spellman (1995, Exp. 2) through the assumption that the NRP-S item *onion* (see Fig. 2) shares features with the RP− item *lettuce*. However, this implies that the number of suppressed features of *onion* is a subset of the number of suppressed features of *lettuce* and hence the decrease for the NRP-S item *onion* has to be smaller than the decrease for the RP− item *lettuce*. In order to predict the observed equality in forgetting for *onion* and *lettuce* it would have to be the case that the number of suppressed features is equal in both cases. Although Anderson and Spellman (1995, p. 91) indeed make such an argument, this is unlikely to be the case since *lettuce* surely must have other (suppressed) features besides those related to the *VEGETABLE* and *GREEN* categories.

Finally, the assumption that features that an item shares with the practiced items (RP+) are strengthened rather than suppressed, leads to some unexpected consequences. First, as pointed out by Norman, Newman, and Detre (2007, p. 939), the model makes the prediction that extra study trials on the RP+ item (that do not involve competitive retrieval) should lead to facilitation of related RP− items (where generally no effect is observed). The predicted facilitation is due to the fact that extra study trials increase the strength of the features of the RP+ item (including shared features) and this increased strength then leads to a higher recall probability for the related RP− items (that share these features). Second, in the paradigm used by Anderson and Spellman (1995, Exp. 1), this assumption may lead to the curious prediction that the NRP-S condition might do worse than the RP− condition. In this experiment overlapping categories were used such that some of the items (e.g. *tomato* and *strawberry*) were members of two list categories (in this case *RED* and *FOOD*) although they were presented as a member of just one of the categories (see Fig. 2). If the item *blood* from the *RED* category is practiced, the pattern suppression model predicts that the NRP-S item *strawberry* will also be inhibited because it shares features with the RP− item *tomato* and because *strawberry* itself might also be activated during retrieval practice of the *RED* category and hence suppressed. However, according to the pattern suppression model, it is not the *RED* features of *strawberry* that are suppressed but the other features (i.e. the FOOD features and any features unique to this item). Since *tomato* was studied as a member of the RED category and *strawberry* as a member of the FOOD category, one would expect that *tomato* would have relatively more RED features (that are not suppressed) and that *strawberry* would have relatively more FOOD features (that are suppressed). Hence, performance on *strawberry* (NRP-S) might actually be worse than that on *tomato* (RP−).

In sum, it is not at all clear whether the pattern suppression model (or the inhibition account) does in fact provide a consistent explanation for these results. However, since these results are also inconsistent with non-inhibitory accounts, their relevance to the current debate is unclear. A more definitive conclusion should probably be suspended until it has been demonstrated that these results are in fact replicable. As mentioned before, a number of researchers (Camp et al., 2005; Williams & Zacks, 2001) have not been able to obtain the decrease for the NRP-S items that Anderson and Spellman (1995, Exp. 2) observed in their shared-implicit category experiment. The failure to replicate by Williams and Zacks (2001) is especially noteworthy since the design of their experiment contained many more subjects and more items than the original Anderson and Spellman (1995) experiment.

A number of studies have used a somewhat different way to examine cue independence. In these studies item-specific independent cues were used rather than category cues that were linked to several list items. That is, for each item on the original list (e.g., *guitar*) a novel cue item (*musical instrument*) was selected that had not been presented before and that was associated to the list item but not to the category with which that item was paired on the list nor to any of the other list items. Using such item-specific cues, Saunders and MacLeod (2006, Exp. 1) demonstrated a cue-independent RIF effect. In their experiment, the items were presented in the form of two brief stories about objects stolen from a house while the family was away. Although one might have expected such a procedure to lead to integration effects, this was apparently not a problem. Aslan, Bäuml, and Pastötter (2007, Exp. 2) also used item-specific cues on the final test but in a more standard RIF experiment. Again, a sizable cue-independent RIF effect was observed. On the other hand, Camp et al. (2007), also using a standard RIF design, did not obtain a RIF effect with item-specific (independent) cues in two experiments although they did find the usual RIF effect when the cue was the original category name (replicating the earlier results of Anderson et al., 1994).

Despite the fact that the evidence is mixed, one might argue that at least some experiments have provided evidence for a cue-independent effect and hence evidence against non-inhibitory accounts of RIF. However, such a conclusion may not be justified since there still are alternative explanations. First, if the RP− item is retrieved during the retrieval practice of the RP+ items (as assumed in the inhibition account), then it is still possible that additional features are stored in the RP− trace. If the independent cue is indeed independent, that is, its features have little or no overlap with the original cue, then the added features will mismatch the cue and this would decrease the effectiveness of the independent cue for the retrieval of the RP− item. Since the NRP items do not have these added features, the independent cue will be more effective for the NRP items than for the RP− items.

Second, although the likelihood of covert cuing should be reduced, recent evidence shows that it may not be completely eliminated. For example, in Camp, Pecher, Schmidt, and Zeelenberg (2009), subjects studied paired associate items that were weakly associated (e.g., *rope-sailing*, *sunflower-yellow*). Prior to the study of the list of paired associates some of the cue items (e.g. *rope*) had been presented in a pleasantness or frequency rating task. In the final phase of the experiment, new extralist cues were presented that were related to the target members of each pair (e.g. *sport* for *sailing* and *color* for *yellow*). The results showed that the targets for which the cue had received additional study were recalled better using the extralist associate, suggesting that it was easier to retrieve a target trace when the original cue was more available, even though the original cue was unrelated to the extralist cue given on the final test. Hence, the supposedly independent cue *sport* was not truly independent of the original cue *rope*.

In a recent paper, Huddleston and Anderson (2012) suggested, however, that a relatively large number of the independent probes used by Camp et al. (2009) were in fact related to the original cue items (although this may not be evident when using standard association frequencies) and hence cannot really be considered independent. Huddleston and Anderson give the example *zoo-tiger* with the independent cue *animal*. They showed that the effect observed by Camp et al. (2009) disappeared when using the materials from the Anderson and Green (2001) study. One problem here is that Huddleston and Anderson (2012) used translated materials so there may have been differences in relatedness for Dutch versus English subjects (e.g., Huddleston and Anderson used the triple Zoo-Tiger-Animal although the original stimuli were Zoo-Tiger-Four-footed animal where the latter in Dutch is not clearly related to Zoo).

A quite different approach to the issue of cue independence was taken by Jonker, Seli, and MacLeod (2012). They made use of lists in which the items in a category belonged to one of two subcategories. The crucial element in their experiments was that these subcategories were chosen in such a way that subjects would not notice their presence during the original list presentation and no mention was made of these subcategories until the final test. During retrieval practice, subjects practiced either items from only one of the subcategories or from both subcategories (the mixed condition). When only the overall category name was given as cue on the final test, a standard RIF effect was observed. However, when all the RP+ items belonged to one of the subcategories (and hence all of the RP− items to a different subcategory) and the subcategory name was presented at test, no RIF effect was obtained. The result was not simply due to presenting the additional subcategory name as a cue since in the condition where items from both subcategories were practiced, a standard RIF effect was obtained. Hence, the RIF effect was cue-dependent in a way consistent with standard competition-based accounts.

Finally, it has been argued that the finding of a RIF effect in tests using item recognition also provides evidence for the cue independence assumption. We find this a rather peculiar way of defining cue independence since the test item (the cue) was part of the original study episode. In recognition, the cue that is presented is the category exemplar that has been studied previously as a member of a specific category (i.e. the original study episode was category name + item + context). Hence, it seems likely that the cue will activate the category name. However, if this is the case, then it becomes difficult to interpret the claim that such a test would involve independent cues. More importantly, there appears to be a misunderstanding among some inhibition proponents that non-inhibitory models would not predict interference and RIF effects in recognition.

It appears that proponents of the inhibition theory assume that on a recognition test in which the item is tested in isolation (i.e. without the category name) the only cue is the orthographic cue representing the tested item and since no other list item is associated to this orthographic cue, there should be no interference or competition by other list items. Such an assumption is however not correct, at least not according to most current models of recognition memory. Many current models of recognition memory (including the SAM–REM model) assume that recognition is based on global familiarity (the summed activation for all traces in memory). In any global familiarity model the test item will activate the memory traces of other items based on their similarity to the tested item (where similarity refers to more than just orthographic features). Hence, items from the same category will have much more of an effect than items from different categories (even if the subject does not implicitly retrieve the category name to be used as an additional cue). Presenting a category name might contribute to performance but it is not necessary for similarity effects. In the original SAM model and most other simple global familiarity models this immediately implies that there will be interference effects if some of the other category members have been strengthened (that is, the model will predict lower performance for the RP− items compared to the NRP items). In the REM model for recognition and other similar Bayesian recognition models, the same result is predicted if one makes the reasonable assumption that the final test context resembles the practice phase context. For an illustration of such interference effects (including simulations based on the SAM–REM model), see Criss, Malmberg, and Shiffrin (2011). Alternatively, if one adopts a dual-process model for recognition, a straightforward prediction would be that at least the recollection component should be sensitive to competition and interference effects (see Verde & Perfect, 2011).

What may have led inhibition theorists to assume that recognition tests would not be affected by competition is that many older experiments in the interference tradition failed to find such effects. However, as argued by Mensink and Raaijmakers (1988, p. 449), this was probably due to fact that these experiments used the four-choice matching test. They showed that strength-based competition models (such as SAM or ACT) will predict only small and often non-significant effects when such a four-choice testing method is used. Later experiments indeed showed quite clear retroactive interference in associative matching

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tests (see Chandler, 1989, Table 1, for a summary of results).

Hence, even if one considers such results as evidence for cue independence, it will not help deciding between the competing accounts since such effects are also predicted by non-inhibitory competition-based accounts.

In conclusion, although several experiments provide support for the cue independence assumption, they do not provide clear-cut evidence against the standard competition-based accounts of interference and forgetting.

Retrieval specificity

According to the inhibition theory, the decrease in performance on the RP− items is due to the inhibition that takes place during the retrieval practice of the RP+ items. Competition-based accounts on the other hand attribute the decrease to the fact that on the final test the competing RP+ items have become stronger due to the retrieval practice and therefore interfere more with the retrieval of the RP− items. Such a view would predict that it does not matter how the RP+ items have been strengthened, only that they have been strengthened. The inhibition account on the other hand would predict that a RIF effect should only occur if the retrieval practice trials are such that they do involve the active retrieval of the RP+ items in a way that makes it possible for the RP− items to hinder the retrieval of the target RP+ item. Only in that case will there be a need for the suppression of the RP− items.

Anderson, Bjork, and Bjork (2000) designed an experiment in which they tested the retrieval specificity of the RIF effect. They varied the nature of the retrieval practice task. In the competitive practice condition the standard retrieval practice task was used. In the noncompetitive practice condition, instead of giving the category name and the first letters of the target item, they gave the target item itself and the participants had to generate the category name (e.g., *FR – orange*). They observed about equal performance on the RP+ items (hence the two conditions should be equally interfering) but only observed a RIF effect for the competitive retrieval practice condition, just as predicted by the inhibition account.

Additional studies testing the retrieval-specificity assumption provided further support for the hypothesis (e.g. Anderson & Bell, 2001; Bäuml & Aslan, 2004; Ciranni & Shimamura, 1999; Hanslmayr, Staudigl, Aslan & Bäuml, 2010; Johansson, Aslan, Bäuml, Gäbel, & Mecklinger, 2007; Staudigl, Hanslmayr, & Bäuml, 2010; Wimber, Rutschmann, Greenlee, & Bäuml, 2009). In all of these studies, it was found that noncompetitive retrieval practice or restudy of the items leads to an increase in the later recall of the practiced items (RP+) but has no effect on the recall of the non-practiced items (RP−). Only under conditions that require competitive retrieval, a RIF effect is obtained. Note that the result that restudy and retrieval testing are supposed to be equally effective learning conditions, is remarkable in light of the recent findings by Roediger and his associates (see e.g. Karpicke & Roediger, 2008) concerning the large differences between these two conditions when tested after a delay. However, there is reason to doubt the (implicit) assumption that equal RP+ recall implies equal amounts of learning or equal strengths.

To see why this might be incorrect, one may note that in most of these experiments no feedback was given during retrieval practice. Hence, it is likely that after the retrieval practice in the competitive condition some items were learned very well and others not at all. That is, an item that is correctly retrieved on the first practice trial will be likely to be retrieved again at the second and third trials while an item that is not retrieved at the first trial, will most likely not be retrieved on the next trials.[7] Since additional retrievals will make the association to the category cue stronger, this procedure of no-feedback will tend to lead to a bimodal distribution of associative strength with most items at a very high level of strength and some items at a low level of strength. This additional strength will however have little effect on the recall probability (the item is already at ceiling) but will lead to an increase in the amount of interference of these RP+ items on the corresponding RP− items. It is also assumed that restudy or a very easy type of retrieval practice (as in the Anderson et al., 2000, experiment) will lead to some additional strength but not nearly as much as for the retrieval practice condition. For a very similar argument, although in a different context, see Kornell, Bjork, and Garcia (2011).

In order to check whether this hypothesis might indeed provide an explanation for the pattern of results observed by Anderson et al. (2000), Raaijmakers and Jakab (2012) ran a simple simulation in which on the initial (competitive) retrieval practice trial a proportion p of the items was successfully retrieved (i.e., for all types of items, a proportion p was initially strongly encoded and a proportion $1 - p$ was initially weakly encoded). Only the retrieved items received a (large) increase in strength. In the non-competitive (or extra exposures) condition, all practiced items were given a modest increase in strength (these items were all re-presented during the retrieval practice). For the final test, a simple sampling-plus-recovery model similar to SAM (Raaijmakers & Shiffrin, 1981) was used. For further details, we refer to Raaijmakers and Jakab (2012, Appendix).

Fig. 4 shows the results predicted by this simple competition-based model. As can be seen, such a model indeed predicts a larger RIF effect for the competitive condition, despite the fact that the probability of recall is identical in both conditions. With the parameter values used, the model also produces almost no RIF effect for the non-competitive condition. Of course, the model may predict a RIF effect for the non-competitive condition but only at the expense of a much higher recall performance for the RP+ items. For example, if the parameters are changed to create a modest RIF effect for the non-competitive condition of 5%, the recall probability for the RP+ items increases to 79% (compared to 58% for the competitive condition). Hence, if the RP+ recall is about equal in the two conditions, the RIF effect will always be much smaller in the

[7] This is a well-known result. See e.g. Otani and Whiteman (1994) who in a repeated recall study (without feedback) observed that P(correct on trial 2 given correct on trial 1) = 0.94 while P(correct on trial 2 given incorrect on trial 1) = 0.14.

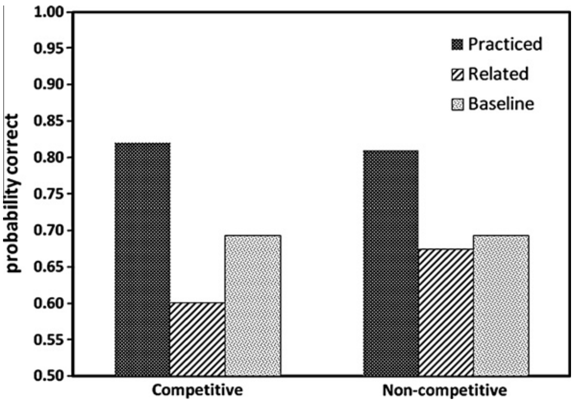

Fig. 4. Mean recall percentages following competitive and non-competitive retrieval practice for the different item types as predicted by a simple non-inhibitory retrieval model. Taken from Raaijmakers and Jakab (2012).

noncompetitive condition. These simulations demonstrate that such results are not incompatible with a competition-based account of retrieval induced forgetting and that the increase in recall performance for the RP+ items cannot be used as a measure for the amount of strengthening, contrary to what is frequently assumed.

According to this analysis, a RIF effect should be obtained in a non-competitive practice or an extra-study trials condition provided the practice or study is such that sufficient additional information is stored (especially with regard to the category-item associations). Raaijmakers and Jakab (2012) verified this prediction in two experiments in which the non-competitive retrieval practice was made more challenging and feedback was given after each practice trial. In both experiments a RIF effect was obtained, contrary to the expectations based on the retrieval specificity assumption. Support for this prediction also comes from a recent series of experiments reported by Jonker and MacLeod (2012). They showed that in a task that did not involve competitive retrieval practice, the occurrence of a RIF effect depended on whether or not a category retrieval task was included during the retrieval practice phase. This reinforces the idea that what is crucial is the extent to which category-item associations are strengthened rather than whether the task involves competitive retrieval as assumed by the inhibition account.

Other evidence claimed to support the retrieval specificity assumption (according to Anderson, 2003, p. 420) comes from an experiment by Bäuml (1996) using a retroactive interference paradigm. In this study, Bäuml presented an initial list of items followed by one to four interpolated lists. The amount of study time on the original and interpolated lists was varied (either 2 or 5 s). In the first experiment there was a free recall test following each of the lists followed by a final free recall test. According to the inhibition account the decrease in List 1 recall due to the interpolated lists is assumed to be due to the fact that List 1 items are activated during the recall of the interpolated lists and not to the competition of the interpolated lists on the final recall test. In this experiment, there was an effect of the strength (study time) of the interpolated lists. According to Bäuml (1996, p. 383) subjects recalled more items from the strong lists, hence they "may have had more occasions to suppress first-list items".[8] In addition, the subjects will probably have started their final recall with the items from the interpolated lists and will have recalled more items from the strong lists. Hence, the effect may have been due to extra output interference. In a second experiment, no immediate tests were given after the presentation of each interpolated list. In addition, at the final recall test, List 1 was always tested first, thus eliminating any differential output interference. Hence, in this second experiment there was no competitive retrieval during the learning of the interpolated lists. In contrast to the first experiment, no effect of the strength of the interpolated lists was observed.

Although these findings are considered by both Anderson (2003) and Bäuml (1996) as supporting the inhibition account, a closer look at the results shows problems for such a conclusion. First, even though retrieval practice (and thus inhibition) was eliminated in the second experiment of Bäuml (1996), there still was a large effect of the number of interpolated lists. It is not clear how the inhibition account would explain such an effect. Why would the number of interfering items have an effect but not the strength of those interfering items? Such an assumption bears similarity to the well-known list-strength effect in recognition where it has been observed that the strength of the other list items has no effect on recognition performance although the number of other list items does have an effect (i.e., there is a list-length effect). As shown by Shiffrin, Ratcliff, and Clark (1990), such a pattern of results is inconsistent with a large variety of models for memory retrieval. This problem seems to be even larger in recall than in recognition. Hence, if the inhibition account is intended to be a truly general theory of forgetting, it should provide some explanation for the differential effects of the number and the strength of the interpolated list items.

Second, when one compares the decrease in performance in List-1 recall due to the presentation of the interpolated lists between Experiments 1 and 2 for the high-low condition (high strength List-1, low strength for the interpolated lists), the size of the decrement is virtually identical, despite the fact that Experiment 1 includes an inhibition effect and Experiment 2 does not. Such a result implies that if inhibition factors contribute to the retroactive interference effect, their contribution must be quite modest (see Verde, 2012, for a similar conclusion). Finally, a more recent investigation by Delprato (2005) in which the strength of the interfering list was manipulated by varying the number of study cycles (without testing) obtained a clear effect of the strength of the interpolated list, despite the fact that the procedure minimized retrieval inhibition and output interference effects.

All in all then, we may conclude that the claim that the evidence from experiments testing the retrieval specificity

[8] This assumption that the inhibition depends on whether or not the item has been recalled, does not appear to be in line with the general inhibition approach in which inhibition arises when the subject tries to recall the target item. Whether or not this attempt is successful or not, should not matter (as demonstrated by Storm et al. (2006)).

assumption is inconsistent with non-inhibitory accounts of retrieval induced forgetting, is not correct. Moreover, recent evidence (Jonker & MacLeod, 2012; Raaijmakers & Jakab, 2012) shows that this assumption may in fact be incorrect.

Strength independence

A final assumption of the inhibition account is the assumption of strength independence (Anderson, 2003), the assumption that the amount of inhibition is independent of the strength that the RP+ items gain in the retrieval practice phase of the experiment. Most of the evidence for this assumption comes from the same experiments that support the retrieval specificity assumption. Many of these experiments show that not all types of strengthening of the RP+ items lead to inhibition for the RP− items. In particular, study procedures that do not involve competitive retrieval, increase the strength of RP+ but do not lead to inhibition. As we mentioned before (see the section on *retrieval specificity*), such results are in fact consistent with non-inhibitory accounts of RIF.

Although this is not often mentioned by inhibition proponents (see e.g. Hulbert et al., 2012), not all of the experiments that are claimed to support the strength independence assumption actually do provide unequivocal support. For example, as we mentioned previously, the experiments reported by Shivde and Anderson (2001) do show a RIF effect for the extra exposures condition that is actually larger than that in the retrieval practice condition. Similarly, in the experiment reported by Hulbert et al. (2012) there was no difference in the pattern of results for the related categories for the retrieval practice and the extra presentations conditions although only the first condition should suffer from inhibition.

A different approach that has been proposed to determine whether there is a relation between the strengthening of the RP+ items and the amount of RIF, is to look at the correlation (over subjects) between these two measures. Such analyses have been reported by Hanslmayr, Staudigl, et al. (2010), Staudigl et al. (2010), and Hulbert et al. (2012). The correlations reported were all close to zero, sometimes slightly negative, sometimes slightly positive. However, it has been known for a long time in the interference literature that such correlations are difficult to interpret (see the discussion on the independence findings of Greeno and his colleagues). The basic reason is that individual differences may mask the true relation (see Hintzman, 1972). Mensink and Raaijmakers (1988) demonstrated this using simulation results with the SAM model. This problem is also relevant here. In addition, in the present data, one of the scores used in this correlation (the RIF measure) is a difference score. A well-known statistical result is that such difference scores tend to have low reliability, necessarily leading to low correlation with any other score (Bereiter, 1963). Moreover, the higher the reliability of the separate scores, the more serious this problem will be (and recall scores usually have relatively high reliabilities). Hence, the null correlations reported by these authors are difficult to interpret and certainly cannot be taken as clear-cut evidence in favor of the strength independence assumption.

Probably the strongest evidence for the strength independence hypothesis comes from the experiments reported in Storm et al. (2006) and Storm and Nestojko (2010). In these experiments it was shown that a RIF effect could also be obtained when subjects were given a retrieval practice task using category-stem completion in which the stem could not be completed to a correct item (e.g., WEAPONS – wo – ; there is no weapon that begins with these two letters). Since in this case there is no RP+ item, it seems obvious that the RIF effect cannot be due to the strengthening of the RP+ item. However, the interpretation of these experiments is not as clear-cut as it might seem since it is quite possible that subjects did retrieve something from memory only to find out that it did not match the stem provided. These implicit retrievals may nevertheless provide a source for competition and interference on the later recall test.

In sum, we do not believe that there is conclusive evidence in favor of the strength independence assumption despite the claims of Hulbert et al. (2012) to the contrary.

Summary of the results so far

In this section, we have examined the experimental evidence that has been obtained within the retrieval induced forgetting paradigm, focusing on those results that have been claimed as strong evidence against non-inhibitory, competition-based accounts. According to the present analysis, none of the assumptions proposed by Anderson (2003) as crucial properties of the inhibition account, have received unequivocal support. Many studies have shown sizable RIF effects for items that have to be considered weak (violating the interference dependence assumption), the basic results advanced in support of the retrieval specificity and strength independence assumptions can be readily explained by non-inhibitory accounts, and the evidence obtained with regard to the cue-independence assumption appears to be mixed. In addition, the results of several recent experiments appear to reject one of more of these assumptions (Jonker & MacLeod, 2012; Jonker et al., 2012; Raaijmakers & Jakab, 2012). Clearly then, this does not provide a sound basis for dismissing competition-based, non-inhibitory accounts.

In the remainder of this article we will review the application of the inhibition theory to a number of other paradigms and phenomena, such as the think/no-think paradigm, output interference effects, part-list cuing, and directed forgetting. We will argue that there too the results do not clearly support the inhibition account and that non-inhibitory accounts in fact provide a better explanation for several standard findings obtained in these paradigms.

The think/no-think paradigm

Although most of the research on retrieval induced forgetting has used the retrieval practice paradigm, the evidence for the inhibitory effects of suppression obtained in such experiments is indirect at best. A more direct method to investigate the assumption that suppression leads to inhibition, was devised by Anderson and Green (2001). In this paradigm, the participants are instructed not to think about a specific learned target (to suppress the target). This suppression is then assumed to lead to inhibition of the trace of that target item. On first sight such a result seems unlikely, given the well-known finding that instructing a subject not to think about a specific item (e.g., a white bear) will make that item more available on a later test rather than less available (see Wegner, 1994; Wenzlaff & Wegner, 2000). Nevertheless, Anderson and Green (2001) did obtain an inhibition effect for the suppressed target item.

In the Anderson and Green think/no-think experiments, participants first studied a list of 40 unrelated word pairs such as *ordeal–roach* until a criterion of 50% correct recall. In the second stage of the experiment, participant were presented with the stimulus members of the pairs and either had to recall the response item (*think condition*) or had to suppress the response item (*no-think condition*). From the 40 word pairs, 15 were selected for the no-think condition. In order to be able to carry out these instructions, participants first familiarized themselves with the stimulus members of these pairs. Next, they were presented with one of the 40 stimulus members and either had to recall the associated response member or had to avoid thinking about the response member, depending on whether the stimulus member belonged to the no-think condition or not. The word pairs were presented 0, 1, 8 or 16 times (the word pairs with 0 trials in this phase of the experiment served as the baseline for further comparison). In the final phase of the experiment, subjects were tested on all word pairs. Two types of test were given, a *same probe* test in which the original stimulus member was given as a cue (*ordeal*), or an *independent probe* test in which the category name and the initial letter of the response member were given as cues (*insect-r*). The independent probe test was used in order to test whether the decrease in recall for the no-think condition was due to inhibition of the target response item or to other factors (e.g. thinking about another word when presented with the stimulus member).

The results of the experiment demonstrated a decrease in recall for the suppressed items, both in the same probe and the independent probe condition. No such decrease was observed in a further experiment in which the instruction for the think/no-think phase was modified so that participants were asked to recall the response item but not to say it aloud. These results were interpreted by Anderson and Green as clear evidence for the Freudian process of repression. In a later paper (Anderson et al., 2004) they claimed to have identified the neural systems underlying Freudian repression (although such a conclusion depends on acceptance of the repression interpretation, see Aron, 2007). However, a closer look at the details of the Anderson and Green (2001) experiment raises a number of questions.

First, the actual magnitude of the effect seems quite modest. Looking at the averaged data from Experiments 1–3, there is a decrease of just about 2% after 8 suppression trials (baseline 83%, no-think condition 81%). Only after 16 suppression trials does the effect become somewhat larger (a decrease to 73%). In view of the effect sizes often reported for the retrieval practice paradigm after just three retrieval practice trials (about 10%), the effect after 8 suppression trials seems rather small.

Second, the results from the experiment in which the participants were allowed to recall the item but not to say it aloud (the "withhold" condition) are a bit counterintuitive. In the "respond" condition there was a clear increase in the number of correct responses after 16 repetitions, but in the "withhold" condition performance did not increase at all as a function of the number of repetitions. It seems rather strange and counterintuitive to us that there would be such a large difference simply due to not making an overt response. After all, in both conditions the participants were asked to recall the item. We are not aware of any other results that demonstrate a similar effect.

Third, although Anderson and Green (2001) assume that the results from the independent probe condition demonstrate that participants did not form alternative associations as a means to avoid thinking about the to-be-suppressed item, the evidence for this assumption is far from conclusive. For example, one strategy that they might have followed is to think about a word from the same category as the target word, e.g. instead of thinking of *roach* they might think about *bug*. After all, if they formed an association between *ordeal* and *roach*, forming an association between *ordeal* and *bug* would seem to be a simple strategy. However, if they do follow such a strategy, the logic for the independent probe test no longer holds since *bug* is also associated to the category name *insect* (the supposedly independent cue) and will still interfere during the testing of *insect-r*.

This interpretation of the findings in think/no-think paradigm is supported by the results reported by Hertel and Calcaterra (2005). Hertel and Calcaterra used adjective-noun pairs (e.g. *racing – hound*) in the original list and provided participants in the suppression condition with aids, i.e., new nouns associated with the original adjectives (e.g. *racing – costume*) that they might use to substitute for the to-be-suppressed nouns. At the final test they were instructed to recall the original noun and that if they recalled two words (e.g. the aid and original noun) they might give both responses (i.e., an instruction similar to the MMFR tests in the older interference literature). The importance of recalling the original noun was however emphasized. The overall results showed no forgetting for the unaided condition, only for the aided condition. Furthermore, when the data from the unaided condition were split according to whether the participant reported the spontaneous use of a substitution strategy, an inhibition effect was only observed for the group that had used a substitution strategy. The group that had not used such a strategy showed facilitation rather than inhibition. Thus, these results support the idea that the inhibition effect in Anderson and Green (2001) study may be due to the use of such a strategy. In addition, Lemoult, Hertel, and Joormann (2010) showed that when aids were given, it did not make any difference in the amount of forgetting observed whether the aids were accompanied by the instruction to suppress or not. This reinforces the hypothesis that the use of substitute responses is responsible for the inhibition effect in the think/no-think task, not the suppression per se. Such a hypothesis is entirely consistent with non-inhibitory accounts based on competitive retrieval mechanisms.

Finally, there have been a number of failures to replicate the Anderson and Green (2001) results. For example, Hertel and Gerstle (2003) did not observe a decline as a function of the number of suppress trials in the same probe test. They did not include independent probe testing but given that the same probe test showed no forgetting, even the inhibition theory would not predict forgetting on an independent probe test. A more extensive attempt at replication of the Anderson and Green (2001) results was undertaken by Bulevich, Roediger, Balota, and Butler (2006). In three experiments they followed the procedure used by Anderson and Green (2001), with only minor changes in the initial two attempts at replication. In none of the experiments was there any sign of the type of forgetting observed by Anderson and Green (2001), neither in the same nor in the independent probe test. Although Bulevich et al. considered a variety of possible explanations (even exotic ones such as that the students that participated in the Anderson and Green (2001) study had suffered more traumatic events) they were unable to find a convincing explanation for the difference in results.

Additional failures to replicate have been reported by Bergström, Velmans, De Fockert, and Richardson-Klavehn (2007), Dieler, Plichta, Dresler, and Fallgatter (2010), Mecklinger, Parra, and Waldhauser (2009), Meier, König, Parak, and Henke (2011), and Waldhauser, Johansson, Bäckström, and Mecklinger (2011).

On the other hand, the suppression effect observed by Anderson and Green (2001) has been replicated in a number of other experiments (Anderson, Reinholz, Kuhl, & Mayr, 2011; Bergström, De Fockert, & Richardson-Klavehn, 2009; Depue et al., 2006; Hanslmayr, Leipold, & Bäuml, 2010; Hertel & McDaniel, 2010; Lee, Lee, & Tsai, 2007; Wessel, Wetzels, Jelicic, & Merckelbach, 2005). In some of these experiments, the effect was not present in the independent probe condition or such a condition was not included (Hanslmayr, Leipold, et al., 2010; Hertel & McDaniel, 2010; Lee et al., 2007; Wessel et al., 2005). In other experiments, the effect was only present in some of the conditions (e.g., Depue et al., 2006; Hanslmayr, Leipold, et al., 2010; Lee et al., 2007). Moreover, Meier et al. (2011) showed that the effect may reverse if the final test is given after a 1-week interval (a finding that is reminiscent of the rebound phenomenon observed in the "white bear" paradigm, see Wenzlaff & Wegner, 2000).

Clearly, the suppression effect in the think/no-think task is an elusive phenomenon at best, a conclusion shared by many researchers, including researchers that are more positive towards the inhibition hypothesis than we are (Bäuml, 2008; Lambert, Good, & Kirk, 2010; Mecklinger et al., 2009). At present, there seems to be no simple rule that explains why the effect is sometimes absent or very small. Anderson and Huddleston (2011) reported a meta-analysis of all published experiments showing clear overall inhibition effects. Of course, such an analysis is limited by the fact that null results usually do not get published. Anderson and Huddleston (2011) mention a number of reasons why such null results might be obtained. For example, in some experiments subjects might not have complied with the instructions to suppress (especially if they knew their memory was going to be tested). In other experiments, subjects may have become too fatigued leading to a decrease in the efficacy of cognitive control (although this would seem to be less likely in view of the nonmonotonic effect predicted according to the Demand/Success Tradeoff assumption).

We propose that the magnitude of the effect is affected by subjective strategies used by the participants in the no-think condition. Since in most experiments the instructions do not specify what the participants should do in the no-think condition (other than "do not think about the item"), it is likely that what they do will be highly variable. One strategy that is likely to be used, is the substitution strategy, i.e., thinking about something else, an alternative item or items. Hertel and Calcaterra (2005) and Lemoult et al. (2010) have shown that such strategies may be quite effective and will lead to an inhibition effect.

We conclude that the think/no-think paradigm does not provide conclusive evidence for the concept of repression. Although in several experiments a (modest) decline of performance has been observed for the to-be-suppressed items, such a decline may very well be the result of the use of a substitution strategy, consistent with non-inhibitory accounts. Moreover, if the substitute is semantically related to the original item, a non-inhibitory account would still predict a decline even when on the final test an independent cue is given. Thus, even though not all of the findings are easily accommodated by a non-inhibitory account, such a result is to be expected given the variable nature of the results obtained in this paradigm, making it hard for any theory (including theories based on inhibition) to provide a coherent account of all of the data.

Directed forgetting

Prior to the advent of the retrieval induced forgetting paradigm, the most popular experimental technique in the study of inhibitory processes in memory was the directed forgetting task. There are two variants of this task. In the *item method*, each item on the list is immediately followed by a cue indicating whether or not that item will be tested later on in the experiment. In the *list method*, a series of items is presented and about halfway through the series a cue is given to indicate that the previously studied items will not be tested and hence may just as well be forgotten. The results of many experiments over the past 40 years have shown that such an instruction to forget is usually effective: recall of the to-be-forgotten items (the F-items) at a surprise recall test in which the participants are requested to recall all previously presented items, is reduced, often accompanied by an increase (compared to a control condition) in the performance on the to-be-remembered items (the R-items). Although the item and list method designs are similar, there is a difference such that in the item method there is a decreased performance on the F-items with both recall and recognition testing, while in the list method no decrease is observed when a recognition test is given. For reviews, we refer the reader to Basden and Basden (1998), Bjork (1989) and MacLeod (1998).

In order to account for this difference, it was proposed that in the item method the decrease was due to differential rehearsal: upon the presentation of the forget cue, the F-item is no longer rehearsed and all attention is directed towards the R-items. Such an assumption implies that the stored strength for the F-items will be decreased relative to a control condition while the stored strength for the R-items is increased (this follows from the fact that the limited processing capacity can be directed to only these items). This difference will show up both with recall testing and in recognition testing (since it is based on "real" differences in memory strength). To explain forgetting in the list method, proponents of inhibitory explanations have usually assumed that in this case the F-items are inhibited and that when they are tested with recognition (i.e., a test where the F-item itself is presented) there is a release from inhibition (Bjork, 1989).

Although this inhibition account of directed forgetting has been (and still is) quite popular, there are a number of problems. First, the assumption that there is a release from inhibition in recognition testing seems a bit gratuitous since such an assumption is only made for this paradigm. As we discussed previously, for the standard retrieval induced forgetting paradigm inhibition is assumed to occur with both recall and recognition testing. Presumably for this reason, Bäuml (2008) proposed that in directed forgetting the forgetting is due to what he terms "route deactivation" (Bäuml, 2008, p. 216), a weakening of the association between the cue and the target item, rather than a weakening of the item representation itself (similar to the unlearning assumption in the traditional two-factor account of forgetting). However, this hypothesis appears to suffer from the same problem since it is unclear why the forgetting should be based on route deactivation in directed forgetting but not in retrieval induced forgetting tasks.

Second, Gelfand and Bjork (1985; cited in Bjork, 1989) showed that by itself the forget instruction does not lead to a decrease of the to-be-forgotten list, a directed forgetting effect does not occur when there is no second list to be learned. Clearly, such a result restricts the nature of the mechanism underlying the directed forgetting effect. An inhibitory account of the effect would have to assume that during the learning of the second list, the first list items intrude and have to be inhibited. However, it is not clear why this should be the case since it has been generally assumed by inhibition theorists that mere study of a list of items does not involve competitive retrieval and hence should not require inhibitory control processes. Moreover, since the List-1 items are not specifically related to the List-2 items, there is no reason why they should spontaneously be activated and then be inhibited (i.e., the interference dependence assumption). Hence, it remains unclear within the inhibition framework what is causing the effect, if it is not the instruction to forget itself.

Although inhibition once was the major explanation for directed forgetting, more recent theoretical accounts do not make any use of such a concept. Results reported by Sahakyan and Kelley (2002) were very influential in the move away from inhibitory accounts of directed forgetting. They showed that the "inhibition" observed in list method experiments could be well explained by assuming that the forget cue induces a change in the internal mental context. This contextual change is maintained during the final recall test leading to an advantage for the R-items, the items that were encoded in that context and a decrease in the recall of the F-items, the items that were encoded in the original context. Lehman and Malmberg (2009) recently implemented this explanation within the SAM–REM framework. Additional evidence for such an explanation of directed forgetting and against an inhibitory account was presented by Sahakyan and Goodmon (2010), Mulji and Bodner (2010), and Lehman and Malmberg (2011).

One important advantage of these contextual change accounts of directed forgetting is that they are firmly based on mechanisms that were not specifically proposed to account for directed forgetting and that are part of the core assumptions of several well-specified (quantitative) models of memory (e.g., the SAM–REM model, see Malmberg & Shiffrin, 2005; Mensink & Raaijmakers, 1988; Raaijmakers & Shiffrin, 1981; Shiffrin & Steyvers, 1997). These models have emphasized the importance of context matches in retrieval as well as the role of context differentiation to reduce interference from prior lists.

More importantly for our present purposes, these accounts show that a satisfactory explanation of directed forgetting effects is possible (if not likely) that does not rely in any way on inhibitory concepts. Of course, in defense of the inhibition account one might assume that both context change and inhibition affect performance in directed forgetting tasks or even that the context change itself entails a kind of inhibition or suppression (suppressing of the first list context). However, this would clearly be a less parsimonious explanation, especially since no one has ever proposed an inhibitory account of context dependent memory.

There are also a number of findings that do not seem to be compatible with an inhibition account. For example, both Sheard and MacLeod (2005) and Lehman and Malmberg (2009) showed that the effect of the forget instruction is mainly on the initial items from the list (the primacy part of the serial position curve). A similar result (although less clear) was observed by Sahakyan and Foster (2009, see e.g. their Fig. 9) and Pastötter and Bäuml (2010, see their Fig. 2). The inhibition account does not explain this result but it follows quite naturally from the context change account (due to the fact that the primacy effect is based on stronger contextual associations, see Lehman & Malmberg, 2011, for a quantitative account based on the SAM–REM theory). Thus, the Lehman–Malmberg model accounts for the full set of data without any appeal to inhibition. Based on these results, we conclude that non-inhibitory accounts provide a better explanation for directed forgetting effects than accounts based on the notion of inhibition.

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Part-list cuing

Another paradigm that has been used to support the notion of inhibition in memory retrieval is the part-list cuing paradigm (Slamecka, 1968, see Raaijmakers & Shiffrin, 1981 for a review). This paradigm was originally developed to test the notion that in studying a list of items for a later free recall test, participants build an associative network of interitem associations. To test this idea, Slamecka (1968) gave one group of participants a random half of the list items as cues to aid the retrieval of the remaining items. It was assumed that if they had indeed built an associative network, such cues would give them a number of entry points in the network for free and this should help them in finding the remaining noncued items. The results showed no such facilitation for the cued group, not even in lists that contained items that should have been easily associated. Later experiments even showed a small decrease in recall for the cued group. Thus, it seemed as though interitem associations did not play a role in free recall. This of course created a paradox since it was generally assumed that elaborative processing (i.e., connecting items from the list to one another) was a successful strategy to maximize recall.

Early accounts (Rundus, 1973) ascribed the negative effects of part-list cuing to an increase in strength of the cue items due to their presentation at the start of the recall process. A more sophisticated account was presented by Roediger (1973; see also Roediger, 1974, 1978). Roediger's account reconciled the positive effects often obtained with cuing in categorized lists with the negative effects obtained in part-list cuing. Roediger (1973) assumed that item cues will give access to the category or higher order unit but that once access to the category is achieved, presenting additional cues from that category will decrease the probability of recalling the remaining items from that category. Note that in these explanations it is assumed that direct interitem associations are either nonexistent or not involved in the retrieval process.

Over the years, several explanations for the part-list cuing effect have been proposed that do not assume the absence of interitem associations. For example, Basden, Basden, and Galloway (1977) proposed a strategy disruption account in which it was assumed that the part-list cues disrupt the preferred retrieval strategy. An explanation consistent with the inhibition theory for forgetting was proposed by Bäuml and Aslan (2004). They assumed that during recall there is a covert retrieval of the cue items and it is this covert retrieval of the cue items that leads to suppression of associated items, just as in the standard retrieval practice paradigm. Note that both of these accounts take the position that the cues are doing something "bad", either by disrupting the preferred retrieval strategy or by inhibiting the noncue items.

A quite different account was presented by Raaijmakers and Shiffrin (1981; see also Raaijmakers & Phaf, 1999). This account was formulated within the framework of the SAM theory and is probably the most detailed and sophisticated account of part-list cuing, although due to its complexity it also probably one of the least well understood explanations. Although the SAM explanation is sometimes depicted as similar to the strategy disruption account (see Bäuml & Aslan, 2004, 2006; Bäuml & Kuhbandner, 2003), it does not make any such assumption. On the contrary, one of the interesting aspects of the SAM explanation is that the cues are assumed to be equally effective in both conditions. The SAM account is in fact more similar to the explanation provided by Roediger (1973). The major difference is that it extends the analysis of Roediger (1973) to subjective, idiosyncratic categories or clusters based on the interitem associations that are formed during study of the list. Cues do lead to access to such subjective clusters but once access is achieved, additional cues belonging to that same subjective cluster will have a negative effect on the retrieval of the remaining items from that cluster.

An important aspect of the SAM analysis is that it takes into account that item cues will also be used in the noncued or control condition. The crucial difference is that the item cues that are used in the control condition are self-generated during the course of the retrieval process. In the cued condition subjects will start by using the experimenter-provided cues (following the instructions they were given). In a sampling model such as SAM there is a subtle factor that makes the experimenter-provided cues less effective in retrieving the noncued items (i.e., the items on which the cued and noncued conditions are compared). The experimenter-provided part-list cues will always lead to (subjective) clusters of items that contain at least one (and often more) cue items (namely the cue item itself). Hence, there is a sampling bias in favor of the cued items. In the noncued condition no such bias exists and this leads to the observed decrease in the probability of recalling noncued items for the part-list cuing condition. The most salient aspect of the SAM explanation is that the cues are assumed to be doing exactly the same as they always do, i.e., increasing the likelihood of activating items related to the cue items. Thus, the SAM account is able to account for the part-list cuing effect without making any special assumptions (i.e., the standard model was used and not adapted in any way to make it "fit" these results).

This explanation for the part-list cuing effect leads to a number of predictions that have been verified by the results of several experiments. First, the model predicts that the effect depends on whether or not the subjects in the noncued condition are able to generate a sufficient number of item cues. If the recall level is low, the effect will be reversed (see Raaijmakers & Phaf, 1999). Second, if the cues are not given immediately but only after an initial attempt at retrieving the list items, the effect will also be reversed (see Allen, 1969). Third, the effect will depend on the strengths of the contextual associations (see Basden, 1973; Blake & Okada, 1973). Fourth, when the cues are consistent with the stored associative structure (i.e., equally divided over the stored clusters of items) the effect will be positive rather than negative (see Raaijmakers & Phaf, 1999). Finally, when the list consists of a number of nonoverlapping and relatively small clusters of items, the effect will also be positive (see Raaijmakers & Phaf, 1999). All in all then, there is quite strong empirical evidence for the SAM account of the part-list cuing effect.

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As mentioned earlier, the inhibition explanation proposed by Bäuml and Aslan (2004) assumes that when the cues are processed there is an implicit retrieval attempt of these cue items and it is this covert retrieval of the cue items that leads to inhibition of associated items, just as in the standard retrieval practice paradigm. As formulated by Aslan, Bäuml and Grundgeiger (2007, p. 335): "At the heart of this explanation is the proposal that the presentation of part-list cues leads to early covert retrieval of the cue items and that this covert retrieval is similar in nature to overt retrieval of the same items, which, in studies on retrieval-induced forgetting, has been shown to inhibit nonretrieved items".

It should be evident that this inhibition account does not provide an explanation for many of the earlier findings in the part-list cuing literature. For example, it does not explain why the effect reverses when the recall level is low or when the cues are given after an initial stage of free (noncued) recall. It also fails to account for the finding that part-list cuing is positive rather than negative when the list consists of paired associates (see Raaijmakers & Phaf, 1999). Probably the most devastating evidence comes from experiments with categorized lists. Raaijmakers and Phaf (1999) reported an experiment in which the cues were either a random selection of the list items or consisted of one or two items from a selected number of categories. In all cases, the cues led to a substantial *increase* in the number of items recalled from the cued categories and a large *decrease* in the number of items recalled from the noncued categories. Cuing therefore does not lead to a decrease in the recall of items associated to the cues but rather to a decrease in the recall of the other items, the items not associated to the cues. This general finding has been known for a long time (see e.g., Raaijmakers & Shiffrin, 1981, p. 113; Roediger, 1978). Although such a pattern of results is understandable from a sampling point of view, it is exactly the opposite of what would be expected from the Bäuml and Aslan (2004) explanation of part-list cuing. The inhibition account predicts that there is an implicit retrieval of the cue items and that this implicit retrieval leads to suppression of items related to the cues. Hence, this account predicts a decrease in the recall of items related to the cues, not the unrelated items, contrary to what is observed.

The above discussion shows that a more convincing inhibition account of paradigms such as part-list cuing should take into account the fact that cues clearly do work (i.e., increase the probability of retrieving associated items). Although this might seem like a truism, it is completely ignored in current inhibition explanations for the part-list cuing effect. Until this problem is resolved, the inhibition account will not be able to provide a satisfactory explanation for the part-list cuing effect.

Output interference and list strength effects

The final phenomenon that has been proposed as evidence for retrieval-induced inhibition is the finding of output interference effects in recall, the detrimental effects of prior recalls on the later retrieval of the remaining items. Such effects are assumed to affect performance in free recall but are also clearly present in cued recall paradigms (see Roediger & Schmidt, 1980). Traditionally (see e.g., Raaijmakers & Shiffrin, 1980) such effects have been explained as being due to the increase in association strength of the recalled items to the retrieval cues used. However, according to inhibition theorists (Anderson, 2003; Bäuml, 1998) output interference is not due to such an increase in competition by previously recalled items but rather to retrieval induced inhibition of the later items as a result of the retrieval of the initial items. Bäuml (1997) proposed that list strength effects (see Ratcliff, Clark, & Shiffrin, 1990) could also be explained as due to such output interference effects. We will therefore combine the discussion of these effects.

Bäuml (1998) presented evidence that strong items were much more vulnerable to output interference effects than weak items. Such a result is predicted from the inhibition account since weak items are less likely to interfere during the recall of the initial items and hence need not be suppressed. Bäuml (1998, p. 459) claimed that competition-based accounts of output interference would make exactly the opposite prediction. However, it is not difficult to see that such an assumption does not in fact follow from competition-based models that are based on a relative strength principle. Bäuml (1998) incorrectly generalized a similar assertion by Anderson et al. (1994) from designs in which the category exemplars were either all strong or all weak to designs in which mixed-strength categories were used. To illustrate, in the most simple ratio-model the probability of recalling an item from a mixed-strength list will be $P_1 = s/S$ when the item is recalled first (s equals the strength of the sampled item; S equals the combined strength of all items in the category or list). Note that in this case, the denominator of the sampling probability (S) is constant, i.e., does not depend on the strength of the target item (i.e., s). If the item is recalled second, the sampling probability will change to $P_2 = s/(S + Z)$ where Z equals the increment in strength due to the previous recall(s). Output interference is measured as the difference between P_1 and P_2 and will in this case be equal to: $P_1 - P_2 = sZ/[S(S + Z)]$, hence the output interference is predicted to increase linearly with the original strength of the item. Therefore, weak items are not predicted to suffer more from output interference according to such a simple ratio model.

A second issue that should be noted is that Bäuml (1997) appears to make the assumption that it is not the act of trying to retrieve the item that is causing the inhibition but rather its successful recall. This may be deduced from statements such as "the more items associated with a cue that are retrieved, the more impaired those related items will be" and "the impairment does not depend on the strength of the retrieved item but only on its successful recall" (Bäuml, 1997, p. 261). This assumption does not fit the general inhibition approach. Inhibition theory assumes that inhibition occurs because other items are activated while trying to recall the target item and these competitors then have to be suppressed to enable recall of the target item. As shown by Storm et al. (2006), it does not matter whether the retrieval attempt is successful or not. Assuming that the inhibition is dependent on successful recall, would suggest that the inhibition takes place after the target item has been recalled. It is not clear to us why there should be a need for suppression after the target item has been recalled. This assumption does however make the Bäuml account quite similar to non-inhibitory accounts such as the SAM model in which output interference is also due to an increase in associative strength after successful retrieval (i.e., what is termed "incrementing" in the SAM model, see Raaijmakers & Shiffrin, 1980).

Bäuml (1997) applied the same idea to the explanation of list strength effects. The list strength effect refers to the finding that recall of a target item is negatively affected by the strength of the other items associated to the same cue (Tulving & Hastie, 1972). It is most clearly demonstrated in a so-called mixed-pure design (see Ratcliff et al., 1990), in which the performance on the same strong items is compared in a pure list (in which all items are strong) versus a mixed list (in which half of the items are strong and half are weak). If there is a list-strength effect, the performance on the strong items will be better in the mixed list compared to the pure strong list. Similarly, the performance on weak items is predicted to be better in a pure weak list compared to a mixed list. List strength effects are largest in free recall, relatively weak in cued recall and absent in recognition. To account for such effects, Bäuml (1997) assumed that in free recall of a mixed list the strong items will be recalled before the weak items and hence strong items will suffer less output interference in mixed lists. That is, when the initial items are recalled, the other items are suppressed and hence later items suffer more from inhibition. A similar line of reasoning is assumed to hold for the weak items. According to Bäuml (1997, p. 261), "suppression predicts that recall performance of weak items should be lower from mixed lists than from pure weak lists, because, on average, there is less retrieval inhibition for the weak items in the pure weak lists than in the mixed lists."

The latter prediction is however less evident than it might seem. Even disregarding the fact that the need for inhibition in free recall is less evident than in cued recall (after all, in free recall there is not just one target item), the assumption that weak items suffer from retrieval inhibition, does not seem to be in accordance with the interference dependence assumption of inhibition theory, i.e., the assumption that weak items do not interfere and hence do not have to be suppressed. Second, if weak items are nevertheless activated during the recall of other items, one might argue that this should be more likely when the other items are weak rather than strong. Hence, this would lead to the prediction that performance on the weak items should be better in mixed lists than in pure weak lists (contrary to the data). However, the inhibition of weak items is consistent with the assumption that the inhibition is a function of the number of previously recalled items (there will be fewer items recalled in the initial stages in case of a pure weak list).

In an experiment in which output order was controlled, Bäuml (1997) observed no list strength effects when the data were grouped according to their test position (first three or last three positions), consistent with the output interference account. However, recent experimental data question the assumption that such effects are basically a form of output interference. Verde (2009, Exp. 2) presented his participants lists of paired associates in which each stimulus item was paired with six different response items (i.e., A–B, A–C, A–D, A–E, A–F, A–G), creating a kind of artificial category. In some of these categories three pairs were presented only once and the other three were presented three times. In the remaining categories all items were presented once. At test, a cued recall test was used in which the first word (A) and the first two letters of the target response item were given as cues. Hence, output order was controlled. The most important result involved the comparison of the effect of category type (mixed strength versus control) on the recall of the weak items (the items presented once) as a function of test position. The results showed that recall was lower for weak items from the mixed strength categories. This list strength effect was already present at the initial test position and of about the same size for the first and last test positions. Since the effect was already present at the first test position, it cannot be due to output interference or inhibition.

In conclusion then, it is doubtful whether list strength effects can be satisfactorily explained by inhibition. In order to explain the occurrence of list strength effects on the initial test positions, the inhibition account would have to come up with other additional factors besides output interference that would explain why weak items suffer from the presence of strong items on the list.

Concluding remarks

Anderson (2003) presented a radically new interpretation of forgetting due to interference. Instead of the traditional explanation in terms of associative competition, Anderson (2003) claimed that such interference phenomena are better explained using the notion of inhibition, the hypothesis that during the practice on the second list the first list responses are inhibited or suppressed and that it is this inhibition that is responsible for the lower recall at a later test of the first list. Evidence for this assumption was based on the results of the retrieval induced forgetting paradigm (Anderson et al., 1994) and the think/no-think paradigm (Anderson & Green, 2001).

In the previous sections we have critically examined the evidence for this inhibition theory for interference and forgetting. Our review makes it clear that the evidence is less convincing than is often claimed and that alternative interpretations certainly cannot be ruled out (a conclusion shared by Verde (2012)). More specifically, we have argued that many of the findings that have been claimed to be incompatible with competition-based accounts are in fact consistent with theories based on competitive retrieval and contextual cuing (e.g., SAM, see Mensink & Raaijmakers, 1988; Raaijmakers & Shiffrin, 1981).

As we have mentioned a number of times throughout this paper, one factor that may very easily lead to problems of interpretation is the fact that in the standard retrieval practice paradigm no feedback is given during the retrieval practice. We have argued that this will lead to differences in strength of the RP+ items that will not be reflected in the

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probability of recalling those items on the final test. We used this result to explain why there is a RIF effect for the standard competitive retrieval practice but usually not for non-competitive types of practice or when extra study trials are given, despite the fact that the observed recall for the RP+ items is equal in both cases. The same argument holds for a number of other results that have been claimed to uniquely support the inhibition hypothesis. For example, Koessler, Engler, Riether, and Kissler (2009) observed that the standard RIF effect disappears when the retrieval practice is given under stress. Similarly, Román, Soriano, Gómez-Ariza, and Bajo (2009) observed no RIF when a concurrent secondary task had to be carried out during retrieval practice. These results are often regarded as strong support for the inhibition hypothesis (inhibiting items involves a process of cognitive control and if such cognitive control becomes more difficult, the inhibition will decrease). However, it is not too difficult to see that there is a simple explanation within competition-based accounts. All one has to assume is that stress or a concurrent secondary task will decrease the amount of information stored for the retrieved RP+ items during retrieval practice. As explained above, this will have little effect on RP+ recall but it will have a clear effect on RP− recall (even to the extent that the RIF effect is eliminated). Hence, the idea that such results are uniquely supportive of the inhibition account is not justified. The explanation given here not only predicts the decrease in RIF that is observed in these experiments but also the result that little effect is observed for RP+ recall (which is surprising in view of the fact that e.g. concurrent tasks should affect memory storage and which is not explained by the inhibition account).

When one compares the inhibition theory for interference and forgetting to more traditional theories such as the Two-Factor Theory or the SAM based model proposed by Mensink and Raaijmakers (1988), the most striking difference appears to be the almost exclusive reliance on a variant of the unlearning assumption and the dismissal of competition as a factor that might account for at least part of the interference effects. This is surprising since the unlearning assumption was at the heart of the problems that plagued the Two-Factor Theory. One problem that the inhibition theory inherited from older theories based on unlearning is the difficulty accounting for proactive interference. The traditional Two-Factor Theory could not account for proactive interference on a so-called MMFR test since such a test was assumed to be free of response competition and unlearning did not apply to the items from the second list. Since competition and unlearning were the only factors within the Two-Factor Theory that could account for interference, proactive interference effects should not be present if both of these factors were eliminated. The inhibition theory cannot account for proactive interference for similar reasons to the extent that it denies any effect of competition on the probability of recall (since inhibition cannot lead to a decrease in the strength of the second-list items). Of course, one could claim that the inhibition account was not invented in order to explain proactive interference effects, but such a defense would seriously limit the adequacy of the inhibition theory as a general account of interference and forgetting.

More generally, we believe that the dismissal of competition as a factor in interference and forgetting is at the heart of the problems that the inhibition theory has in providing a consistent account of the phenomena from different experimental paradigms. Although there are special situations where the inhibition account does allow for an effect of competition (Anderson, 2003), the general assumption is that (with normal subjects) competition will have no effect on the probability of recall if the recall test uses item-specific cues (such as the initial letter or letters). The following quote from Bäuml (2007, p. 5) illustrates this assumption: "If retrieval-induced forgetting was caused by retrieval blocking, the forgetting should disappear once item-specific probes, i.e., items' unique initial letters or their unique word stems, were employed at test." It is not clear to us what might be the justification for making this assumption.

The problem with such an assumption is that it becomes unclear why in the standard retrieval practice paradigm inhibition would be needed. After all, the final test in such experiments usually involves a recall test in which, in addition to the category name, a single letter is provided as a retrieval cue, while the retrieval practice trials involve a similar test with the first two letters as cues. If the final test is free from strength-dependent competition, then the retrieval practice trials should certainly be free of competition. Why then would there be a need for inhibition? The assumption that item-specific cues can eliminate the competition from other traces is reminiscent of the assumption in the Two-Factor Theory that MMFR testing eliminates response competition. As argued by Mensink and Raaijmakers (1988), this assumption was responsible for the problems that the Two-Factor Theory had in giving a coherent account of interference and forgetting. In a similar vein, we believe that the assumption made by inhibition theorists that competition may be eliminated by giving item-specific cues is the main reason for the problems facing the inhibition account that we have reviewed in this article.

A second problematic aspect in the inhibition account is the assumption that additional study trials do not involve retrieval and hence do not require inhibitory control. The rationale for this assumption is not clear. For example, theories of spacing effects have often used a study-phase retrieval assumption, i.e., the second presentation of an item involves a recognition-like process in which the trace of the previous presentation is retrieved. Furthermore, the very substantial within-category serial position effects (see Jakab & Raaijmakers, 2009) are usually explained by assuming that later presentation of category exemplars leads to retrieval of the previous exemplars from the same category. The assumption that additional study trials do not involve a retrieval process also seems to be inconsistent with the way in which the inhibition account has been used to explain part-list cuing effects. According to Bäuml and Aslan (2004) presentation of the part-list cues leads to an implicit retrieval of those items that is similar to the retrieval that occurs on retrieval practice trials in the standard RIF paradigm. Given such an assumption it becomes unclear why a similar implicit retrieval should not occur on additional study trials.

We conclude that despite the large amount of research over the past 15 years, there has been no clear resolution regarding the theoretical status of inhibition as an explanation for interference and forgetting. Non-inhibitory explanations (such as the SAM/REM approach) cannot be ruled out as an explanation for the findings within the retrieval practice and think/no-think paradigms and provide a better explanation for the results obtained in directed forgetting and the part-list cuing paradigms. Moreover, a number of recently added assumptions such as the Demand/Success Tradeoff assumption make the inhibition account almost immune to empirical testing. What is needed is a much more clearly specified theoretical model that is used in a consistent way across experiments and allows actual predictions to be made. In addition, much work still needs to be done to resolve some nagging problems such as the nature and role of competition and the clarification of conflicting and difficult to replicate results.

All in all then, after having reviewed the current evidence for the inhibition hypothesis, we may end with a paraphrase of the conclusion from Postman (1975) that we quoted earlier:

> "Inhibition theory today is in a state of ferment if not disarray. There is no lack of new data but so far they have failed to resolve the basic theoretical issues."

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