The Role of Executive Control in Collaborative Recall

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Declaration

I, Nikolas Williams, certify that the work in this thesis entitled "The Role of Executive Control in Collaborative Recall" has not been previously submitted for a degree, nor has it been submitted as part of the requirements for a degree to any university or institution other than Macquarie University.

I also certify that the thesis is an original piece of research and it has been written by me. Any help and assistance that I have received in my work, and the preparation of the thesis itself has been appropriately acknowledged.

In addition, I certify that all information sources and literature used are indicated in the thesis.

The research presented in this thesis was approved by the Macquarie University Human Ethics Review Committee, reference number: **5201400528**.

Signed:

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Thesis Summary

The purpose of this PhD program of research was to investigate the role of executive control in collaborative recall. While the costs and benefits of collaborative recall have been well-defined, little research has examined how the individuals within collaborative groups affect the outcomes. I hypothesized that executive control would influence collaborative recall because executive control is implicated in modulating individuals' susceptibility to disruption and their ability to inhibit—two processes which are posited to contribute to collaborative costs.

In a series of three experiments I investigated how the abilities that people brought into collaboration influenced collaborative recall as well as subsequent, post-collaborative recall. That is, how did individual differences in executive control influence: What people brought in to collaboration? How they performed in collaboration? and What they took away from collaboration? I examined executive control ability at the individual level as well as the group level. At the group level, I investigated both the average ability of group members as well as the difference between group members' abilities.

In Experiment 1 I found that neither individual ability nor group-level ability influenced collaborative recall. However, the difference in ability between group members predicted attenuated post-collaborative benefits. Lower ability individuals who had collaborated in groups with partners whose abilities were much higher than their own tended to forget previously remembered information.

In Experiment 2 I pre-screened individuals and composed groups based on differing ability compositions. While I did not replicate results from Experiment 1, I did observe the standard costs and benefits of collaboration where people who collaborated were disrupted during collaboration, but recalled more information and became more accurate on subsequent individual recall compared to people who did not collaborate.

In Experiment 3 I used a battery of cognitive tasks to derive an executive control component score intended to assess latent ability. I found a similar pattern of forgetting to Experiment 1, such that the difference between group members' abilities predicted forgetting for the lower ability partners. This was further qualified by lower recognition performance for the lower ability partners, which suggested an inhibitory process was responsible.

Overall, the findings from my PhD research suggest that collaborative recall does not influence all collaborators in the same way. In some groups, lower ability individuals do not benefit from collaboration to the same extent as their higher ability partners. This is due to losing originally remembered material, which questions the generally accepted wisdom that collaboration is beneficial for later individual memory. However, across all three studies I found evidence that the use of retrieval strategies might mitigate this effect. I discuss the theoretical implications, as well as practical implications for education and ageing.

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Chapter 1

General Introduction

General Introduction

When we think of memory, we often think of it as a process that happens within us; a phenomenon that is constrained by the boundaries of our individual mind. However, remembering is not always an individual activity. We constantly interact with our environment to scaffold our memory (Barnier, 2010; Barnier, Sutton, Harris, & Wilson, 2008; Sutton, 2006). Indeed, it is easy to think of myriad memory aids that people often use: post-it notes, journals, diaries, calendars, even strings around fingers. The smartphone revolution has created what is presently the zenith of memory aids in the form of a computer that the majority of people living in the developed world carry around in their pocket (Barnier, 2010; Miller, 2012). What is the capital of Brazil? A few thumb-clicks on our device reminds us of information that we once remembered but can no longer recollect. We even strategically offload memories in an effort to reduce the demands of storing them and to increase the probability that they will be later remembered (Barnier et al., 2008; Sutton, 2006; Sutton, Harris, Keil, & Barnier, 2010). That dentist appointment next week? Best to create a reminder in the calendar application on my phone. No college student (at least not a studious one) would dream of going to her bio-chemistry class without a notebook on which to transcribe the future memories that she needs to create in order to pass her exams. Even setting the empty milk carton out on the kitchen counter at night as a way to remind one's self the next morning that milk needs to be added to the day's shopping list constitutes a conjunction between internal remembering processes and our environment (Dixon, 2013).

Perhaps a less obvious example of the strategic distribution of our memory is the social environment in which we live out our lives. The people we interact with, especially those with whom we are close, can critically influence the quantity and quality of our memories (Barnier et al., 2008; Harris, Keil, Sutton, Barnier, & McIlwain, 2011; Johansson, Andersson, & Rönnberg, 2005). I may have completely forgotten that John spilled his wine at dinner last week until our mutual friend, Jane, brought it up in an anecdote during a conversation. Certainly, my failure to spontaneously recall John's clumsiness would be unlikely to have profound impact on my life, but the social nature of memory can have pronounced positive and negative effects that extend beyond colourful autobiographical enrichment. For example, if Jane and I were working on a team at a corporation and were tasked with presenting on a new business strategy, we might divide the presentation such that Jane was responsible for remembering our corporate partners' marketing strategies and I was responsible for remembering their financial holdings. In this case, we would need to work cooperatively during the presentation to convey knowledge that we hold as a unit that is greater than the knowledge either of us hold alone (see Uitdewilligen, Waller, & Zijlstra, 2010). In a more intimate scenario, imagine that Jane and I had been married many years and Jane was very good at remembering which medications I needed to take and at what times in which doses. I would likely rely on Jane's memory, and in practice I remember to take these medications because I have access to memories that reside outside my own mind (e.g., Dixon, 2013; Harris, Barnier, Sutton, Keil, & Dixon, 2017; Harris et al., 2011; Harris, Paterson, & Kemp, 2008). Examples of this happen quite frequently in real life, where although an individual may not remember information his or her self, because he or she knows where to find that information—in the memory of an intimate partner, for example—he or she in fact does in practice remember that information (Wegner, 1987). I will discuss this phenomenon, termed *transactive memory*, shortly.

Evidence continues to mount that when people come together to remember shared information cooperatively, they remember differently—in terms of quantity, quality, and processes—than they would if they were remembering alone (Basden, Basden, Bryner, & Thomas, 1997; Basden, Basden, & Henry, 2000; Harris et al., 2011; Harris et al., 2008; Rajaram & Pereira-Pasarin, 2010). Termed *collaborative recall,* remembering within a group can have both negative and positive effects on memory. These occur during the actual act of collaboration, and also have ongoing influence after collaboration when the individual later remembers alone (Basden et al., 1997; Basden et al., 2000; Rajaram & Pereira-Pasarin, 2010). Past research has shown aspects of collaborative recall that predict successful outcomes (i.e., better memory) and others that predict unsuccessful outcomes (i.e., worse memory). For example, the way in which collaborators communicate with one another can benefit memory (e.g., Harris et al., 2011; Meade, Nokes, & Morrow, 2009), while the items produced during collaboration can disrupt collaborators' memory (e.g., Basden et al., 1997; Rajaram, 2011). Most of this research, however, has been focused on parameters of the collaborative recall paradigm (e.g. the size of groups or the nature of the word lists; Marion & Thorley, 2016; Rajaram & Pereira-Pasarin, 2010; Thorley & Dewhurst, 2007; Weldon & Bellinger, 1997), the relationships between collaborators (e.g. strangers vs. intimate groups; Andersson & Rönnberg, 1995; Harris, Barnier, & Sutton, 2013; Harris et al., 2017; Harris et al., 2011; Hollingshead, 1998; Rajaram & Pereira-Pasarin, 2010), or the communication processes occurring during collaboration (Harris et al., 2011; Meade et al., 2009). However, very little research so far has tested the role of the individual characteristics of collaborators themselves. Most of the research to date carries an implicit assumption that collaborative groups are homogenous. That is, the individuals within collaborative groups are all influenced by collaboration in uniform ways, and that collaboration in general either has costs of benefits for memory. While this type of group analysis may be appropriate depending on the hypotheses being tested, it misses an important aspect of collaborative recall: How do the pre-existing characteristics that individuals bring to bear in collaboration influence what collaborative groups can achieve? And is the persistence of collaborative influences altered by the compositions of these characteristics within these groups? That is, do the costs and benefits of collaborative recall depend on *who* is collaborating? After all, research on memory at the individual level has long been linked with individual cognitive ability. Thus, ignoring individual differences within memory “units” may overlook important theoretical implications.

The purpose of the research described in this PhD thesis was to elucidate how the cognitive characteristics that individuals bring into collaborative recall influence the outcomes of collaboration. Further, I aimed to question the assumption that the outcomes of collaboration are similar for all individuals. My goal was to systematically examine how ability might interact within collaborative groups and how that might influence post-collaborative memory. The specific cognitive ability that I focused on was executive control. Executive control is a broad construct, but for the purposes of this thesis, can be thought of as the ability to engage in goal-directed behaviour while simultaneously ignoring distracting or irrelevant stimuli (Baddeley & Della Sala, 1996; Baddeley, Lewis, Eldridge, & Thomson, 1984; Baddeley & Hitch, 1974; Baddeley & Hitch, 1994; Kane & Engle, 2002). I focused on executive control for two reasons: 1) It is a cognitive construct that modulates individuals’ ability to effectively deal with distraction and interference (Baddeley & Hitch, 1994; Engle & Kane, 2004; Hasher et al., 2007; Kane & Engle, 2000); and 2) It is a cognitive construct that is intimately linked with episodic memory (Bugaiska et al., 2007; Ferrer-Caja, Crawford, & Bryan, 2002; McCabe, Roediger III, McDaniel, Balota, & Hambrick, 2010; Park et al., 1996). As collaborative recall involves disruptive processes (as I will discuss shortly), it has the potential to be influenced by individual differences in executive control. Additionally, the intimate relationship between episodic memory and executive control should exert demonstrable effects on collaborative recall.

Research into individual cognitive characteristics and collaborative recall is scant. My PhD research represents, to my knowledge, the first attempt to directly investigate whether individual differences in executive control modulate the outcomes of collaborative recall. In this thesis, I begin by presenting a theoretical framework in which I outline critical research on the costs and benefits associated with collaborative recall and the positive and negative mechanisms involved in bringing about these effects. I then review relevant research on the relationship between executive control and episodic memory. I describe why executive control may be expected to modulate the various cognitive mechanisms involved in collaborative recall. In three experimental chapters, I describe a program of empirical research, conducted with the objective of investigating how individual executive control ability influences collaborative recall and subsequent post-collaborative performance. In this empirical research, I addressed my objective from the perspectives of the overall ability of collaborating groups, i.e., *Group Ability*, as well as the difference between the ability of group members, i.e., *Group Discrepancy*. In my final chapter, I discuss my findings and how they relate to and inform current theories of collaborative recall.

Social Remembering

To understand the individual factors that might influence collaborative remembering, I will briefly discuss how some social factors can converge to influence peoples' memory, and thus how remembering in groups is different to remembering alone. This difference between group and individual memory can be in the quantity of information recalled (e.g., remembering more or less information, see Rajaram, 2011; Rajaram & Pereira-Pasarin, 2010), the quality of information recalled (e.g., accuracy, richness, and detail, see Harris, Barnier, & Sutton, 2012), or the processes involved in recall (e.g., communication strategies, see Harris et al., 2012; Harris et al., 2011; Meade et al., 2009). As I briefly addressed earlier, individuals within social groups have access to information that they may not be directly possess within their own mind, particularly in groups that are accustomed to remembering together (Barnier, Klein, & Harris, 2017; Wegner, 1987; Wegner, Erber, & Raymond, 1991). This theory of *transactive memory*, first proposed by Wegner (1987), posits that communication between individuals within a group contributes to an emergent memory that is greater than the sum of individual memories combined (for a recent review see Barnier et al., 2017). Much as I can check my smart phone for information that I cannot readily recall, I can check my close friend for similar information. However, such transactive recall is constrained by two factors. First, I must have a sense of the knowledge held by my friend, whether through explicit external information about their knowledge store (Moreland & Myaskovsky, 2000) or simply from the passage of time and my experience interacting with them (Wegner et al., 1991). Second, I must deem my friend a credible source (Lewis, 2003). If I do not have faith in the accuracy of my friend's knowledge, then I am unlikely to rely on them as an auxiliary memory source.

The factors described above are important as they imply the requirement of meta-cognitive knowledge about the group in which I am collaborating. In order to affect efficient transaction of memory, I need to know something about my friend's memory as well as my friend's ability. Further, my acquisition of this meta-cognitive knowledge may be contingent on my own ability (Winne, 1996). Additionally, to make use of memory transactions, my friend and I must engage in effective communication so that we can exchange memories between one another; a process that may also be influenced by our individual abilities (Barnier et al., 2017). Thus, coordinated retrieval strategies may be crucial for effective collaborative recall.

Inherent in transactive memory theory is some form of intimacy between group members. Whether this intimacy is in the form of platonic relationships (Andersson & Rönnberg, 1996; Harris et al., 2013), romantic relationships (Barnier et al., 2014; Harris, Barnier, Sutton, & Keil, 2014; Harris et al., 2017), or professional relationships (Smith-Jentsch, Kraiger, Cannon-Bowers, & Salas, 2009; Zhang, Hempel, Han, & Tjosvold, 2007), the connection between group members influences the memories that a group produces. Some research suggests that this is due to the processes taking place during remembering. For example, Harris et al. (2011) observed that when long-married couples engaged in specific remembering strategies, they exhibited better collaborative memory performance than their individual performance would predict. Likewise, a study by Meade et al. (2009) showed that the interactions between expert pilots produced collaborative recall output that was superior to their own individual recall and to collaborative recall of novice pilots. The authors suggested that this was because expert pilots engaged in conversations that were more complex and elaborative than their novice counterparts. Individual cognitive ability could certainly augment or even mediate such process-variables as research suggests that individuals with greater working memory capacity are more likely to engage in strategic remembering processes than are individuals with lower working memory capacity (Cokely, Kelley, & Gilchrist, 2006). Thus, while much of the research on social memory has examined the relationship and interactions between group members, there has been less focus on the group members themselves; that is, the characteristics of group members that may influence their collaborative recall.

While ability is theoretically important with respect to social remembering, there are also practical, applied reasons to consider its role in collaborative recall. For instance, there has been recent interest in whether collaborative recall – especially with an intimate partner – may help to scaffold memory with age (Blumen, Rajaram, & Henkel, 2013; Harris et al., 2011; Johansson et al., 2005; Johansson, Andersson, & Rönnberg, 2000). Johansson et al. (2005) found that older couples who reported using certain strategies when recalling together showed higher levels of recall compared to the pooled recall of couples who were recalling by themselves. Further, these couples recalled more than other collaborating couples who did not employ a strategy. Likewise, Harris et al. (2011) found that communicative processes predicted positive collaborative performance in older couples. However, little is known about how such scaffolding may interact with ability, particularly as one or both members of the couple begin to experience age-related memory declines (Balota, Dolan, & Duchek, 2000). The burden of such scaffolding could affect performance of the "scaffolder" as it may increase their overall cognitive load and thus diminish their own memory performance (Engle, 2002). This effect may be subject to the scaffolder's own individual ability and may be further exacerbated by increasingly disproportionate abilities between partners. The same could be said of educational settings which often demand cooperative learning (Johnson & Johnson, 2009). If groups are formed on the basis of individual ability, then are there cases in which benefits received by the scaffolded would come at the expense to the scaffolder?

The aim of my PhD research was to systematically investigate how differing abilities that people bring into collaboration might influence collaborative outcomes. I wanted to understand how peoples' abilities within groups influenced collaborative and post collaborative performance. To do this, I adopted the methodology of the collaborative recall paradigm, which I describe next.

Collaborative Recall

In the typical methodology of the collaborative recall paradigm, individuals study a word list. In the collaborative condition, groups of individuals in *collaborative groups* work together to recall shared information in an interactive fashion. In the control condition, individuals recall alone, and their individual recall output is pooled together to form *nominal groups* of the same size as the collaborative groups (e.g., dyads or triads). Redundant items (i.e., those items recalled by more than one individual in the nominal group) are only included once in the nominal group recall (see Figure 1). The standard finding is *collaborative inhibition* where collaborative groups recall fewer items than nominal groups (Basden et al., 1997; Basden et al., 2000; for reviews see Harris et al., 2008; Marion & Thorley, 2016; Rajaram & Pereira-Pasarin, 2010; Weldon & Bellinger, 1997). However, remembering together also has *post-collaborative benefits*, such that individuals who previously collaborated generally remember more information when recalling later than people who did not collaborate (Finlay, Hitch, & Meudell, 2000; Weldon & Bellinger, 1997). While socially motivated explanations of collaborative inhibition (e.g., social loafing) are intuitively appealing, research has discounted them as a factor (Weldon, Blair, & Huebsch, 2000) and generally cognitive explanations are the best supported mechanisms responsible for collaborative inhibition (Barber, Harris, & Rajaram, 2014; Blumen, Young, & Rajaram, 2014; Hyman, Cardwell, & Roy, 2013; Marion & Thorley, 2016; Rajaram & Pereira-Pasarin, 2010). Thus, collaborative recall is characterized by costs at the interactive stage and benefits at the subsequent individual stage and these are due to cognitive, rather than social, factors.

Of central interest to this thesis is the fact that the standard method of investigating joint remembering makes assumptions regarding the homogeneity of individuals in collaborative groups. Specifically, it assumes that people bring similar individual characteristics into collaborations. This has been an effective approach when considering that much of the collaborative recall research has been aimed at answering *1st generation questions* (Barnier, Harris, & Congleton, 2013), such as "Are there costs and/or benefits in collaborative recall?" However, given the robust extant literature on these costs and benefits, it may be time to address *2nd generation questions*; questions such as, "For whom are there costs and benefits?" It seems unlikely that the costs and benefits of collaboration are uniform and this represents an area of research ripe for exploration in order to better understand the outcomes of collaborative recall, and when (and for whom) collaboration has costs and benefits.

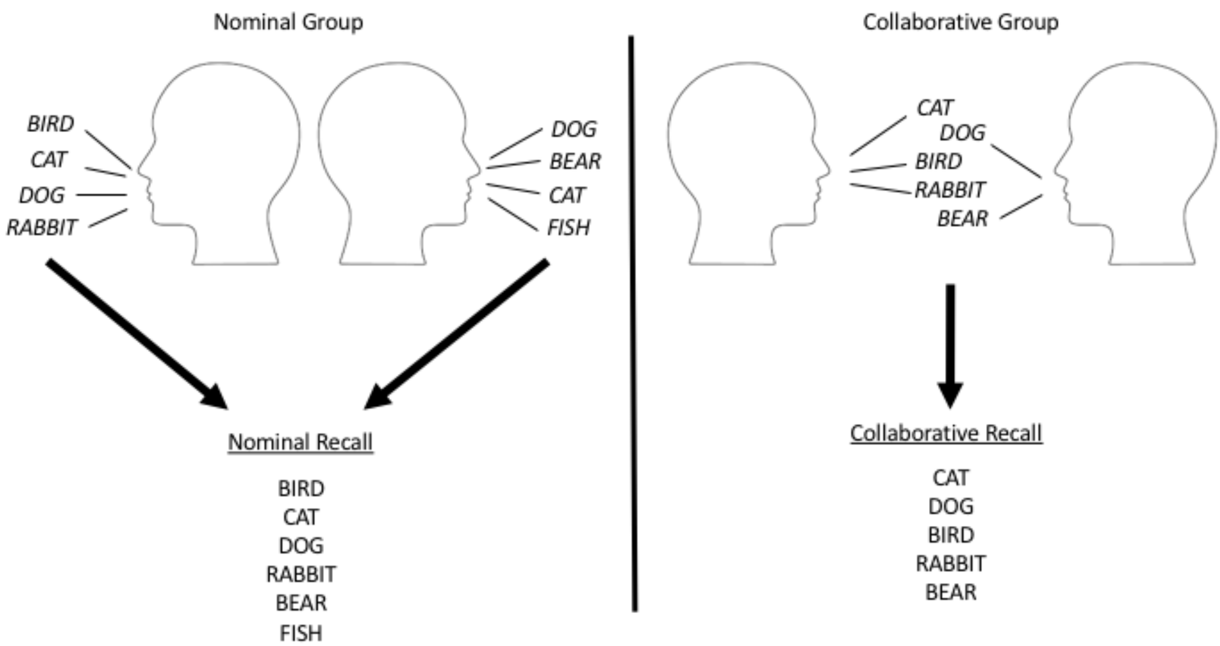

Figure 1. Recall output for Nominal and Collaborative groups in the collaborative recall paradigm.

The Collaborative Recall Model

In their theoretical model, Rajaram and Pereira-Pasarin (2010) outlined a variety of simultaneous cognitive mechanisms proposed to operate during collaborative recall (see Figure 2). Some of these mechanisms have costs for memory and some have benefits. During the actual act of collaboration, the costs generally outweigh the benefits, creating a net negative effect that results in collaborative inhibition (for reviews see Marion & Thorley, 2016; Rajaram & Pereira-Pasarin, 2010). At the post-collaborative stage, the balance of these costs and benefits generally shifts and causes a net positive effect, such that collaborators typically exhibit superior subsequent individual recall compared to non-collaborators (Finlay et al., 2000; Weldon & Bellinger, 1997). Notably, although the Rajaram and Pereira-Pasarin (2010) model explicitly acknowledges individual differences in the form of “Individual pre-existing cognitive structure” and “Idiosyncratic cognitive organisation”, typical collaborative recall experiments do not focus on individual outcomes.

Negative mechanisms in collaborative recall. The most widely observed and studied negative mechanism in collaborative recall is *retrieval disruption* (Basden et al., 1997). Retrieval disruption adversely affects group output due to the fact that individuals encode information idiosyncratically; that is, they encode information in an individually unique way and use this organization to direct later recall. Upon coming together to recall information, misalignment between individual retrieval strategies lowers each collaborator’s output thereby lowering the overall group output (Basden et al., 1997). Since the proposition of retrieval disruption, much research has tested its predictions with mixed results. Some studies have found evidence in line with the prediction that similar encoding strategies amongst collaborators would lead to similar retrieval strategies and thus reduced collaborative inhibition (e.g., Barber, Rajaram, & Fox, 2012; Finlay et al., 2000; Garcia-Marques, Garrido, Hamilton, & Ferreira, 2012; Harris et al., 2013) while others have not (Barber & Rajaram, 2011b; Dahlström, Danielsson, Emilsson, & Andersson, 2011). Likewise, some research has supported the prediction that experimental methods that impose retrieval strategies on participants (e.g., cued-recall) should abolish retrieval disruption and thus collaborative inhibition (because these formats should be equally disruptive to all participants; e.g., Barber, Rajaram, & Aron, 2010; Finlay et al., 2000; Thorley & Dewhurst, 2009), while others have not (e.g., Andersson & Rönnberg, 1996; Kelley, Reysen, Ahlstrand, & Pentz, 2012; Meade & Roediger, 2009). Although previous research has been mixed with regards to retrieval disruption, a recent meta-analysis of 64 published studies has provided compelling evidence that retrieval disruption is a main cause of collaborative inhibition (Marion & Thorley, 2016). *Retrieval blocking* is also proposed to contribute to collaborative inhibition. Here the activation of some information (e.g., partner-recalled items) impedes access to memory representations of other information (e.g., information an individual would have recalled). In the context of collaborative recall the probability of recalling blocked information is lower when recalling with others than it would be had an individual been recalling alone (Rundus, 1973). Some evidence supports retrieval blocking. Specifically, Hyman et al. (2013) found that collaborative dyads sampled from fewer categories during recall than did nominal dyads, which suggested that they explored their memory less effectively, presumably because access to some categories was blocked due to the activation of other categories. This is also supported by research suggesting that

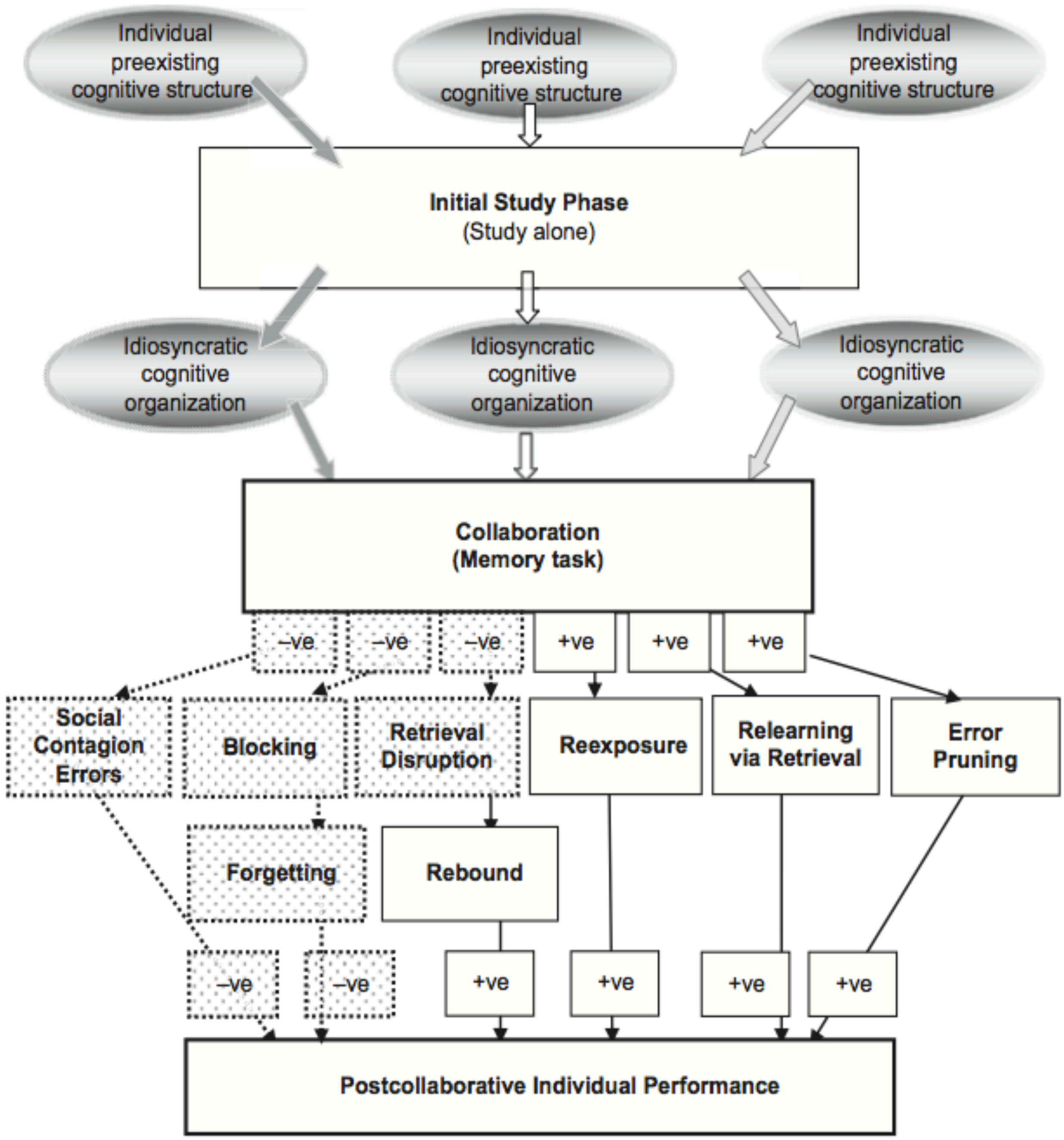

Fig. 1. A theoretical framework for the cognitive mechanisms underlying the effects of collaboration on memory. The top three ovals refer to the preexisting cognitive structures that three individuals (later to serve as a collaborating triad) might bring to the experimental situations. The bottom three ovals represent the idiosyncratic organization of the study materials each individual develops for the study materials. The symbol "–ve" refers to a negative influence of collaboration (a process that impairs accurate retrieval), and the symbol "+ve" refers to a positive influence.

Figure 2. Collaborative recall model. Reprinted from Rajaram, S., & Pereira-Pasarin, L.P. (2010.) Collaborative memory: Cognitive research and theory. Perspective on Psychological Science, 5, 649-663.

competition from semantically related information can make memories temporarily inaccessible (Roediger & Neely, 1982).

Although Rajaram and Pereira-Pasarin (2010) did not include *retrieval inhibition* in their original model, recent research has suggested that it might also play a role (Barber et al., 2014). Retrieval inhibition is similar to, though distinct from, blocking. Here activated representations suppress non-activated representations effectively making the non-activated memories unavailable for recall (Bäuml & Aslan, 2006). Both blocking and inhibition can lead to subsequent post-collaborative forgetting, though there is usually a rebound from blocking while inhibition causes more persistent, long-term forgetting. For example, Henkel and Rajaram (2011) found that after collaborating, individuals forgot significantly more items that they had initially remembered as compared to individuals who never collaborated. This was presumably due to blocking or inhibition. In other studies, collaborative inhibition has been observed even when groups study unshared information (for which retrieval disruption should be minimal or absent; Barber et al., 2014; Meade & Gigone, 2011). Barber et al. (2014) observed that memory impairment following collaboration persisted on an individual recognition test. As recognition memory should have been intact even if disrupted or blocked (Aslan, Bäuml, & Grundgeiger, 2007; Bäuml & Aslan, 2006), this suggested that the memory traces were unavailable for retrieval.

Finally, the addition of erroneous information into later recall can also occur as a result of collaboration. This occurs when group members offer false items that an individual subsequently recalls as true; a phenomenon termed *social contagion* (Rajaram & Pereira-Pasarin, 2010; Roediger, Meade, & Bergman, 2001). Thus, collaborative recall can have the deleterious effect of making individuals' memories less accurate as a result of the collaboration.

Positive mechanisms in collaborative recall. Though their effects are often not observed during collaboration when negative mechanisms outweigh them, positive mechanisms can manifest later. In concert with a rebound from retrieval disruption and blocking, positive mechanisms generally benefit subsequent individual recall in the form of increased quantity of recall and increased accuracy via fewer errors. During collaborative recall, participants are re-exposed to other group members' output, and thus essentially receive an additional opportunity to learn these items. This *reexposure*, results in post-collaborative benefits, as individuals subsequently remember items after collaboration that they would not have remembered had they worked alone (Blumen & Rajaram, 2008; Weldon & Bellinger, 1997).

Relearning through retrieval, on the other hand, is an intra-individual process whereby memory is strengthened due to repeated attempts at retrieval. Long-term individual retention is better enhanced through repeated retrieval as compared to repeated study (i.e., the testing effect; Karpicke & Roediger, 2007; Roediger & Karpicke, 2006). In the case of collaborative remembering, recalling items acts as another testing opportunity and increases the probability that retrieved items will be recalled later. Effectively, it is a collaborative retesting opportunity. However, relearning through retrieval should also benefit those in the nominal condition who similarly engage in repeated retrieval tests (see Blumen & Rajaram, 2008; Blumen et al., 2014; Congleton & Rajaram, 2011)

Finally, *error pruning* during collaboration can reduce individual errors on post-collaborative recall. This can occur when other group members disconfirm incorrect information that the individual offers during collaboration (Rajaram & Pereira-Pasarin, 2010). Thus, whereas collaboration can lead to less accurate memories when individuals transmit errors to one another, it can also do the opposite when individuals correct one another (Blumen & Rajaram, 2008; Congleton & Rajaram, 2011; Finlay et al., 2000; Pereira-Pasarin & Rajaram, 2011).

To summarize, multiple processes are theorized to interact during collaborative recall (Rajaram & Pereira-Pasarin, 2010). During collaboration, retrieval disruption, blocking, and inhibition generally lead to a net negative outcome such that groups do not perform to their full potential and experience collaborative inhibition (Barber et al., 2010; Blumen & Rajaram, 2008; Blumen et al., 2014; Marion & Thorley, 2016; Rajaram & Pereira-Pasarin, 2010). However, following collaboration, recovery from retrieval disruption and blocking along with boosts to memory in the form of reexposure and repeated retrieval, generally lead to superior memory performance for individuals who previously collaborated compared to individuals who previously recalled alone (Barber & Rajaram, 2011a, 2011b; Blumen & Rajaram, 2008; Blumen et al., 2014; Rajaram & Pereira-Pasarin, 2010; Weldon & Bellinger, 1997). Clearly collaboration has costs and benefits. However, what is not clear is for whom are there costs and for whom are there benefits?

What Influences Collaborative Recall?

Collaborative inhibition is remarkably robust and occurs for a variety of stimulus materials including categorized word lists (Basden et al., 1997; Basden et al., 2000), story narratives (Takahashi & Saito, 2004), film clips (Andersson & Rönnberg, 1995), and unrelated words (Andersson, Hitch, & Meudell, 2006; Blumen & Rajaram, 2008; Meudell, Hitch, & Kirby, 1992; Weldon & Bellinger, 1997) and even when partners study different information (Barber, Harris, & Rajaram, 2015; Meade & Gigone, 2011). While previous research has shown that collaborative outcomes are influenced by the ways in which people interact with each other (e.g., Harris et al., 2011; Meade et al., 2009) and by experimental design (e.g., Basden et al., 1997; Congleton & Rajaram, 2011; Pereira-Pasarin & Rajaram, 2011), a common theme that unites many of these findings is the ways in which individuals organise their memories. For example, repeated study opportunities lead to lower levels of collaborative inhibition (Pereira-Pasarin & Rajaram, 2011). Likewise, repeated retrieval of study lists can completely abolish collaborative inhibition (Congleton & Rajaram, 2011). In both of these cases, repeated study and/or retrieval sessions strengthened individuals' organization and attenuated retrieval disruption (Congleton & Rajaram, 2011). Strategic encoding also strengthens organisation and these strategies can help guide retrieval during subsequent recall. For example, stimulus materials with a high degree of inter-item association (e.g., categorised study lists) generally produce collaborative inhibition as they promote encoding organisation and thus increase the potential for disruption. However, smaller category size decreases the number of possible organisational schemes. This generally lessens collaborative inhibition as individuals will likely have more similar organisations to their partners (Basden et al., 1997; though see Marion & Thorley, 2016).

Paradoxically, while strengthening the organization of studied material can attenuate or abolish collaborative inhibition, weakening organization can also have a similar effect such that nominal groups and collaborative groups recall a similar amount (Pereira-Pasarin & Rajaram, 2011). This is because there is less to disrupt in weakly organized material. Divided attention paradigms provide evidence that disrupted encoding can lead to weakly organised material and thus can reduce or abolish collaborative inhibition. In a typical divided attention study, participants encode to-be-remembered information while simultaneously performing a secondary task. Recall is generally poorer for individuals in a divided attention condition as compared to a non-divided attention condition (Craik, Govoni, Naveh-Benjamin, & Anderson, 1996). In a study investigating divided attention and collaborative recall, researchers found that manipulating participants' attention weakened their memory organization and abolished collaborative inhibition (Pereira-Pasarin & Rajaram, 2011; Experiment 2). This suggests that the attention that individuals are able to allocate to encoding moderates susceptibility to retrieval disruption: those with less attention at encoding are less disrupted by collaborative recall. This idea is critical to my thesis as it exemplifies how individual ability might affect retrieval disruption and thus collaborative inhibition. If manipulating available attention resources influences collaborative inhibition, then a de facto manipulation of attentional resources—via intrinsic attentional ability—of collaborating individuals would likely do the same.

In sum, the literature suggests that multiple factors can influence collaborative inhibition. Experimental design and choice of materials are perhaps the most easily manipulable, though artificial, modulators of collaborative recall. The manner in which collaborators interact also appears to play a role in attenuating the detrimental effects of collaboration, as do the cognitive resources participants are able to deploy when collaborating. What is missing from the collaborative recall literature, however, is an assessment of how individual ability might influence collaboration.

Assessing Costs and Benefits in Collaborative Recall

Determining how individuals and groups are influenced by collaboration can be done from two perspectives: the group-level and the individual-level. First, how does collaboration affect group output during the actual act of collaboration itself? And second, how does collaboration affect individual output following collaboration, on post-collaborative individual recall? At the group level, collaborative group output is simply compared to nominal group output (as discussed previously). At the individual level, there are multiple ways of assessing costs and benefits and any/all may be appropriate, depending on the hypotheses being tested. The simplest method is to compare the subsequent post-collaborative individual recall between previous non-collaborators and previous collaborators (e.g., Barber & Rajaram, 2011a; Congleton & Rajaram, 2011). If non-collaborators recall less than collaborators, then collaborators benefited from remembering within a group. The converse is also true. This type of approach, however, does not take into account any differences in initial recall. In order to calculate this, an initial pre-collaborative individual recall test must be administered. This provides a baseline against which final, individual performance can be compared. While this approach is useful for examining the magnitude of post-collaborative benefits, it says nothing about the source of the cost and benefits. The source of benefits can come in two forms: previous collaborators can gain more items through collaboration compared to previous non-collaborators; or previous collaborators can lose fewer items through collaboration compared to previous non-collaborators. If either of these outcomes occur or co-occur, then previous collaborators will demonstrate post-collaborative benefits compared to previous non-collaborators. This distinction is generally calculated as individual items gained versus individual items lost (e.g., Blumen & Rajaram, 2008; Henkel & Rajaram, 2011).

Why Would Ability Matter?

Individual ability is likely to matter because sometimes groups outperform their potential (i.e., recall more than nominal groups) and sometimes they underperform their potential (i.e., recall less than nominal groups). That is, there are mixed findings regarding the outcomes of collaboration (see meta-analysis by Marion & Thorley, 2016, for an excellent examination of this issue). So far, we do not have a thorough understanding of why some groups are better collaborators than others. As the retrieval disruption account of collaborative inhibition is predicated on the assumption that individual differences in the organization of remembered material leads to incompatibilities in retrieval strategies, then it seems logical that other individual cognitive differences would also influence group retrieval. Of the potential higher-order cognitive characteristics, executive control represents the most likely modulator of collaborative recall. This is because executive control is often intimately linked with memory performance (see among many others Bugaiska et al., 2007; Ferrer-Caja et al., 2002; McCabe, Roediger, McDaniel, & Balota, 2009; Park et al., 2002; Park et al., 1996; Taconnat, Clarys, Vanneste, Bouazzaoui, & Isingrini, 2007; Troyer, Graves, & Cullum, 1994).

Executive control is a very broad concept, but is typically thought of as a collection of abilities or processes that allow for directing attention toward goals and task demands while simultaneously inhibiting irrelevant and extraneous stimuli (Baddeley & Della Sala, 1996; Baddeley et al., 1984; Baddeley & Hitch, 1974; Baddeley & Hitch, 1994; Kane & Engle, 2002). As executive control is traditionally conceptualised as a latent construct, it is difficult to operationalise it under one unitary definition. The construct is sometimes referred to as the ‘central executive’ (e.g. Baddeley & Hitch, 1974) and sometimes the ‘supervisory attention system’ (e.g. Norman & Shallice, 1986). Generally, it is thought of as a construct with attention-directing and inhibitory components that subsumes both working memory capacity and executive function. For example, using structural equation modelling McCabe et al. (2011) demonstrated evidence for a latent ability that predicted both executive functioning and working memory capacity. They termed this ability ‘executive attention’. Though the McCabe et al. (2011) term of executive attention is conceptually identical to the construct that I investigated in this thesis, I chose to use the term executive control in order to remain congruous with work by Barber & Rajaram (2011). This latter work represents virtually all of the previous research that has directly investigated collaborative recall and attention/working memory ability. Thus, for the purposes of this thesis executive control represents an underlying latent ability involving the control of attention (e.g. Blair, 2006; Duncan et al., 1996), and working memory (e.g. McCabe et al., 2011).

Measuring Executive Control

As previously noted, executive control is a latent construct and as such cannot be directly assessed. Instead it is typically measured via component predictors such as working memory capacity and attention. The most commonly used tasks that measure working memory capacity are the complex span tasks such as the Operation Span, Reading Span, and Symmetry Span. These tasks include both a storage and processing component such that an individual must hold information in mind while simultaneously performing a secondary task. This is in contrast to short term memory tasks which only require maintenance of information. Attention tasks typically require participants to direct their attention towards specific goals whilst ignoring or inhibiting distracting information. Examples of attention tasks are the Stroop task, the n-back task, and the antisaccade task. As both working memory capacity and attention tasks predict higher-order executive control construct, it is often sufficient to use only one or the other (Conway et. al., 2005; McCabe et al., 2011).

Another aspect worth considering with respect to individual executive control ability is whether executive control is an immutable ability or whether increases in ability are possible through training. The literature is unclear in this regard. For example, a meta-analysis by Karbach & Verhaeghen (2014) provided evidence to suggest that executive function and working memory training resulted in significant increases on the trained tasks as well as near-transfer tasks (i.e. tasks which were very similar to those used in training). However, a more recent meta-analysis by Melby-Lervag, Redick, & Hulme (2016) suggested that improvements were task-specific and not reflective of actual increases in executive ability. Although this debate is unsettled, within this thesis I will use the term *individual ability* to refer to the inherent executive control ability that individuals bring into the experiment at the time of testing. Further it must also be acknowledged that the debate regarding the exact nature and parameters of executive control is ongoing. This debate is outside the scope of this thesis, however (for more on this issue please see Salthouse, 2013; Salthouse, 2015; and Salthouse, 2017).

Executive Control and Episodic Memory

Executive control is often found to be highly correlated with episodic memory performance. For example, working memory capacity has been demonstrated to mediate age-related memory differences (Park et al., 2002; Park et al., 1996). This is also true for executive functioning (Bugaiska et al., 2007; Ferrer-Caja et al., 2002; McCabe et al., 2009; Taconnat et al., 2007; Troyer et al., 1994). McCabe et al. (2010) examined the relationship between working memory capacity and executive functioning and found that when controlling for age-related declines in working memory capacity and executive functioning, age-related memory differences were reduced or eliminated completely. This suggests that executive control may be a true mediator of age-related memory decline. Studies investigating pathological cognitive impairment have also observed a relationship between executive function and memory decline (e.g., Baudic et al., 2006; Buckner, 2004). More generally, in young adults working memory capacity is predictive of recall accuracy as well as general fluid intelligence (Unsworth, 2009).

One of the ways executive control influences episodic memory is by modulating the attentional resources available during encoding and retrieval processes. For example, when manipulated at encoding, divided attention adversely affects the subsequent recall of information (Anderson & Craik, 1974; Baddeley et al., 1984; Craik et al., 1996; Craik, Naveh-Benjamin, Ishaik, & Anderson, 2000; Fernandes & Moscovitch, 2000; Murdock, 1965; Naveh-Benjamin, Craik, Guez, & Dori, 1998). Dividing attention at retrieval typically does not produce this effect (Baddeley et al., 1984; Kellogg, Cocklin, & Bourne Jr, 1982; Naveh-Benjamin et al., 1998). However, executive control does play a role in retrieval under circumstances where interference or response competition is present. For example, working memory capacity facilitates retrieval by focusing strategic search of long-term memory (Unsworth, Spillers, & Brewer, 2012) while also inhibiting irrelevant information (Kane & Engle, 2000). Differences in memory ability due to individual differences in executive control generally emerge under recall conditions requiring controlled, effortful search but not those based on automatic activation (Conway & Engle, 1994).

As evidence from divided attention tasks suggests that executive control may modulate the attention an individual can bring to bear on a task (Kane & Engle, 2000), it may influence how disrupted they are during collaboration. One could argue that the act of collaborating with another individual is itself a secondary task that requires additional cognitive resources, effectively making collaborative recall a divided attention task. If this were the case, then individual differences in executive control would likely modulate each group members' level of disruption, such that individuals higher in executive control would be less disrupted by collaboration than individuals lower in executive control. This would thereby affect group performance. In the divided attention and collaborative recall task previously mentioned, Pereira-Pasarin and Rajaram (2011; Experiment 2) found that manipulating participants' attention at encoding weakened their memory organization and led to abolished collaborative inhibition.

Like many, if not all, collaborative recall studies, the focus of the Pereira-Pasarin and Rajaram (2011) study was on group performance. However, if divided attention—or collaboration, for that matter—differentially influenced people depending on their underlying abilities, then these effects may be masked by group-level analysis. For example, an individual with high executive control ability may be more resistant to the effects of divided attention (Kane & Engle, 2000), which make them relatively less impacted by the distraction provided by collaborative recall. On the other hand, an individual with high executive control ability is also likely to have a stronger individual organisation when they encode the information (Cokely et al., 2006), meaning they may have "more to disrupt" compared to lower ability individuals. These contradictory predictions regarding the role of executive control in collaborative recall have been noted in previous research (e.g., Barber & Rajaram, 2011a). However, no studies have examined these questions from an individual differences perspective. Would high ability individuals be more or less disrupted than low ability individuals? And what would this mean for the output of the group and what group members take away from collaboration? Approaches that exclusively examine the effects of collaboration on groups, rather than individuals with varying abilities, may mask potentially interesting effects.

Executive Control and Collaborative Inhibition

To my knowledge, only one published study exists that examines the role of executive control in collaborative inhibition (Barber & Rajaram, 2011a; Experiment 2). In this study, executive control was manipulated by using a depletion task (a difficult task designed to over-tax participants' cognitive resources). They found no significant recall differences between nominal and collaborative groups as a function of executive depletion. However, executive control was related to post-collaborative benefits whereby individual working memory capacity predicted post-collaborative individual recall: individuals with higher executive control ability had superior post-collaborative recall to individuals with lower executive control ability. Thus, cognitive ability appeared to play a role in the carry-over benefits of collaborative recall.

The Barber and Rajaram (2011a) study suggests that executive control at the group level does not influence collaborative recall. Depleted collaborators, and thus depleted groups, exhibited collaborative inhibition to the same extent as non-depleted collaborators. What it does not illustrate, nor was it their aim, is how cognitive differences *within* a group might influence individual disruption and thus collaborative recall. High ability individuals are more disrupted than low ability individuals (Barber & Rajaram, 2011a; Cokely et al., 2006), but random sampling would presumably cancel out any effect that the individual abilities of group members might have on group recall. In order to address this question, systematic analysis of group members' abilities needs to be conducted. In addition, there may be carry-over effects of these individual differences on subsequent individual memory. Indeed, the Barber and Rajaram (2011a) study supports the proposition that high and low ability individuals may experience different post-collaborative benefits. Thus, I hypothesize that executive control would not only influence collaborative recall, but also subsequent individual recall. Further, these influences may be contingent upon the executive control ability make-up of collaborating groups (i.e., the mix of characteristics of the individuals within collaborating groups). The influence of executive control ability on collaborative and post-collaborative recall would likely manifest through the mechanisms outlined in the Rajaram and Pereira-Pasarin (2010) model. I will next discuss how ability might influence each of these mechanisms.

Executive Control and Collaborative Costs

People experience a range of negative mechanisms when they recall in groups (Rajaram, 2011; Rajaram & Pereira-Pasarin, 2010). These costs can be present during collaboration and some can continue to have an influence following collaboration. How might an individual's ability impact how much they are affected by these collaborative costs?

How would ability influence retrieval disruption? As previewed above, how an individual's executive control ability might influence their experience of retrieval disruption is unclear as contradictory predictions are possible. Paradoxically, research suggests that individuals high in executive control are likely to experience more retrieval disruption than individuals low in executive control. For example, Cokely et al. (2006) observed a relationship between working memory capacity and retrieval disruption in the part-list cuing paradigm. Part-list cuing is similar to collaborative recall in that participants are provided with part of a studied list during recall (for a review of part-list cuing see Nickerson, 1984). Inferior recall under these conditions, as compared to conditions where no study cues are provided, is due to incompatibilities between the cues and the participant's idiosyncratic organization (Basden & Basden, 1995). In the Cokely et al. (2006) study, individuals with higher working memory capacity were more disrupted than individuals with lower working memory capacity. However, when participants were required to elaborately encode items by linking them together in a story, low working memory capacity participants were equally disrupted. These results suggested that working memory capacity modulated the probability of engaging in strategic processing at encoding. When lower ability participants were encouraged to also engage in strategic encoding they were equally disrupted by the presence of cues. Much like the Pereira-Pasarin and Rajaram (2011) study, these findings demonstrate that executive control can influence retrieval disruption by way of modulating initial encoding. Thus, according to these findings the organisation that individuals bring into collaboration is a factor of their individual ability. Ability should therefore indirectly influence levels of disruption during collaborative recall, such that higher ability individuals are most disrupted due to their stronger idiosyncratic organisation of the material.

Barber and Rajaram (2011a; Experiment 1) also found that working memory capacity was related to retrieval disruption in the part-list cuing paradigm. In that experiment, some participants performed a difficult task designed to deplete their executive control while other participants performed an easy task. Regardless of depletion condition, all participants were equally disrupted by the presence of cues. However, the authors observed relationship involving individual differences in ability: participants with higher working memory capacity were more disrupted than participants with lower working memory capacity. They concluded that this relationship was due to differences in encoding strategies rather than differences in retrieval strategies. Like the Cokely et al. (2006) study, this study implied that ability may indirectly influence collaborative recall by modulating initial encoding. Despite the fact that neither of these studies were focused on individual differences in ability, the findings nonetheless suggest that retrieval disruption may be influenced by individual ability and that this may influence collaborative recall and post-collaborative recall.

How would ability influence retrieval blocking and inhibition? According to the Rajaram and Pereira-Pasarin (2010) model, another negative mechanism that operates during collaborative recall is retrieval blocking. Retrieval inhibition is also posited to operate at the collaborative stage (Barber et al., 2014; Hyman et al., 2013) Executive control may modulate both blocking and inhibition during collaborative recall, as one of the hallmarks of executive control is its relationship with inhibitory ability whereby individuals high in executive control are better able to suppress extraneous information than individuals low in executive control (Cantor & Engle, 1993; Connelly, Hasher, & Zacks, 1991; Kane, Conway, Hambrick, & Engle, 2007; Kane & Engle, 2000). These findings could imply greater forgetting for higher ability individuals as they would successfully block or inhibit their own items in order to engage with the items recalled by their other group members. On the other hand, they may experience less blocking as they are better able to ignore items offered by their partners in order to focus on their own retrieval.

In sum, there is evidence that executive control ability may influence the various collaborative costs in collaborative recall. As previous research has not focused on the individual differences of group members, there is no direct evidence of how the group members may be affected. However, previous findings suggest that it is likely that higher ability individuals would experience more collaborative inhibition and more post-collaborative benefits than lower individuals, due to their stronger individual organisation of the learned material (Cokely et al., 2006).

Executive Control and Collaborative Benefits

In contrast to the costs of collaboration, there are also benefits. The effects of these benefits are typically not observed during collaboration when they are outweighed by the costs, but their influence is often observed during post-collaborative recall (Barber & Rajaram, 2011a, 2011b; Blumen & Rajaram, 2008; Choi, Blumen, Congleton, & Rajaram, 2014; Weldon & Bellinger, 1997). Also in certain groups that are able to minimise the costs, benefits may become evident during collaboration (e.g., Barnier et al., 2014; Harris et al., 2013). Further, it is possible that these benefits may affect people with varying abilities differently.

How would ability influence reexposure and relearning via retrieval? In collaborative recall, retrieving a memory during a recall test serves as another testing opportunity and research suggests that this may vary as a function of available attention resources (Pereira-Pasarin & Rajaram, 2011). Research directly looking at how ability influences reexposure and relearning via retrieval is virtually non-existent. However, in one study Pereira-Pasarin and Rajaram (2011) found that repeated study of items at encoding led to an increase in retrieval organisation and resistance to collaborative inhibition. That is, multiple study attempts before collaboration led to stronger individual retrieval strategies that were less susceptible to the detrimental effects of collaboration. If executive control ability relates to the probability of associative encoding (e.g., Cokely et al., 2006) then perhaps it would relate to retrieval organisation whereby higher ability individuals were more resistant to disruption during collaboration. This would lead to greater collaborative recall which would create more opportunity for reexposure. This would in turn influence post-collaborative recall. Further hints can be gleaned from other areas of literature. With respect to relearning via retrieval, it has been found that lower working memory capacity individuals demonstrated greater benefits of retrieval practice relative to higher ability individuals (Agarwal, Finley, Rose, & Roediger III, 2016). This implies that lower ability individuals would gain more items during collaboration through relearning via retrieval than high ability individuals.

Executive control also relates to susceptibility to proactive interference where individuals high in executive control are less susceptible than individuals low in executive control (Kane & Engle, 2000). Proactive interference is the finding that information currently in memory interferes with the integration of new information (Keppel & Underwood, 1962). Thus, high ability individuals may receive greater benefits of reexposure as they are better equipped to integrate information into their existing memory representations than are lower ability individuals. Likewise, lower ability individuals may receive fewer benefits as they are more susceptible to proactive interference and would thus be less able to integrate reexposed items with previously learned material (Kane & Engle, 2000). Taken together, these findings provide tentative evidence that individuals with different abilities might vary in benefits due to reexposure and relearning via retrieval. Further, dissociating the two types of benefits may be achieved by examining how items are generated during collaboration. For example, items that are produced by an individual's partners during collaboration would be a reexposure opportunity, whereas items produced by an individual themselves during collaboration would be an opportunity for relearning via retrieval. To my knowledge, no studies have examined the individual contributions of each partner during collaborative recall. This information, however, may be valuable for understanding how reexposure and relearning via retrieval influence individuals.

Summary

To summarize, executive control is likely to influence collaborative remembering, but the effects are unclear and contradictory predictions are possible with respect to the costs of collaborative recall. On one hand, research suggests higher ability individuals may be relatively more disrupted than lower ability individuals since they have stronger individual organisation of material to disrupt (e.g., Barber & Rajaram, 2011a; Cokely et al., 2006). On the other, higher ability individuals (e.g., Barber et al., 2014) may be more resistant to disruption and interference than lower ability individuals (e.g., Cantor & Engle, 1993; Connelly et al., 1991). Executive control may modulate benefits of collaborative recall as well, although the nature of this effect is not straightforward either. Higher ability individuals exhibit superior post-collaborative recall compared to lower ability individuals (Barber & Rajaram, 2011a), though whether this is due to superior gains or attenuated losses is uncertain. However, higher ability individuals, relative to lower ability individuals, may be better able to suppress distracting information (Cantor & Engle, 1993). Assuming partner output during collaboration is distracting information, then higher ability individuals may experience less benefit from reexposure because they pay less attention to the output of other group members. They would, however, be better able to retain their own initially remembered items. That is, they would show fewer gains through collaboration, but they would also show fewer losses. To disentangle differential post-collaborative benefits due to individual differences in cognitive abilities, it is necessary to examine both gained items and lost items, tracking the fate of individual items across recall tests. By doing this, one could analyse the source of collaborative costs and benefits, rather than simply determining their existence.

Considering the extant evidence, there is a gap in the literature with respect to individual differences in collaborative recall. Traditionally, collaborative recall research has assumed homogeneity in collaborating groups. This is sensible from a group studies perspective which are focused on answering 1^{st} generation questions. However, the applied value of collaborative recall research often centres around intervention therapies for age-related memory deficits (see Blumen et al., 2013, for more on this issue) as well as group pedagogies and learning practices (e.g., Blumen et al., 2014). Thus, it would be ideal to assemble the most comprehensive model of collaborative recall possible and to understand how mechanisms operating in collaborative recall influence people of varying abilities. The evidence offered by the

1st generation of collaborative recall research questions have defined a suitable general model of collaborative recall in which the general costs and benefits have been parameterised. However, this model may not generalise to people whose abilities are incompatible with the groups, either organic or institutional, in which they find themselves. In the three experiments which follow, I attempted to take the first steps toward answering 2nd generation questions by identifying how the cognitive abilities of collaborating individuals might influence the costs and benefits of collaborative recall. Is collaboration most successful when collaborators are higher or lower, similar or dissimilar in ability? And what effects do these different types of collaborations have on individual post-collaborative memory? Does it matter who the individual is, and does it matter who they collaborated with?

Thesis Overview

The overarching aim of my PhD research was to investigate how the individual abilities that people bring into collaboration influence what they remember. Conceptually, I consider individual abilities as what individuals “bring into the group.” Then I consider how they “perform in the group” by comparing collaborative group performance with nominal group performance in groups of different ability profiles. Finally, I consider what they “take away from the group”, by comparing post-collaborative individual recall of former collaborators and former non-collaborators, and testing the effect of both an individual’s ability and the ability of the partner with whom they were collaborating. See Table 1 for a conceptual overview of my thesis research.

In the following series of three experiments, I used the collaborative recall paradigm to examine how executive control ability influenced costs and benefits of remembering together. Across all experiments I employed a three-recall methodology

Table 1
Conceptual Overview of Research Thesis

Levels of Analysis	Abilities	Main Questions
What do they bring into the group? **Recall 1: Baseline Recall**	Individual ability	Does individual ability influence recall performance?
How do they perform in the group? **Recall 2: Collaborative Recall**	Individual ability	Does relative individual ability influence how the individual collaborates?
	Average group ability	Does the overall ability of the group influence collaboration?
	Differences in group member abilities	Do the differences between group members' abilities influence collaboration?
What do they take away from the group? **Recall 3: Post-collaborative Recall**	Individual ability	Does individual ability influence collaborative costs/benefits? Does relative individual ability influence how individuals collaborate?
	Ability of group in which individuals previously collaborated	Does the overall ability of the group influence collaborative costs/benefits?
	Discrepancy of group in which individuals previously collaborated	Do the differences between group members' abilities influence collaborative costs/benefits?
	What is the source of costs/benefits?	Do individuals gain items, lose items, or both? Does this depend on individual and/or group-level ability?

in which the first recall was always individual. This provided a baseline to assess the influence of individual ability on individual recall and against which I could measure changes in performance. The second recall was manipulated between-groups and was either collaborative or individual. That is, participants either worked with a partner to recall studied information or they recalled alone and were pooled into nominal groups of two. The third and final recall was always individual. By comparing the initial individual performance on Recall 1 with the final individual performance on Recall 3, I could determine whether changes in performance were related to whether or not a participant had collaborated. I further parsed changes in recall performance into change scores, which consisted of a *gained items* component and a *lost items* component. With these data, I could directly examine the source of any post-collaborative cost and benefits; i.e., whether benefits were due to gaining additional items through collaboration or whether costs were due to losing originally remembered items through collaboration.

My central questions of interest were based on individual and group executive control ability. My first measure of interest was *Individual Ability.* In the first two experiments, I assessed individual executive control ability via working memory capacity, which I measured using the automated version of the Operation Span (Unsworth, Heitz, Schrock, & Engle, 2005). This task is convenient and user-friendly and provides a valid and reliable measure of working memory (Conway et al., 2005; Unsworth et al., 2005). The OSPAN is correlated with measures of fluid intelligence (Unsworth, 2009; Unsworth et al., 2005; Unsworth, Redick, Heitz, Broadway, & Engle, 2009) and crystallised intelligence (Unsworth, 2010a, 2010b). The reason I used this task, rather than other tasks in in my first two experiments was because of its relatively short duration, validity and reliability (Conway et al., 2005; Unsworth et al., 2005), and correlation with the latent construct of executive control (McCabe et al, 2010). Further, because the OSPAN captures both processing and storage components (Conway et al., 2005), I theorised that it would be a suitable measure with which to relate individual differences in ability to the disruptive processes involved in collaborative remembering.

In my third experiment, I employed a battery of three tasks. One was a measure of working memory capacity, which was again the OSPAN, although in this case I used a shortened version (Foster et al., 2015). In addition, I administered two other measures: the antisaccade task and the Stroop task. Using data from these three tasks I performed a principal components analysis to derive an executive control component score. This score represented variance common to all three tasks and was more reflective of individual ability than any one task alone (for a discussion, see Conway, Kane, & Engle, 2003). As the OSPAN can only predict so much of the variance in the construct of executive control (McCabe et al., 2010), employing a component score approach would theoretically provide a more sensitive measure than simply using the OSPAN. Further, the addition of attention-specific tasks (i.e. the Stroop and antisaccade tasks) should capture information regarding latent ability that may be missed in the first two experiments in which I used only the OSPAN. Thus, any relationship between ability and collaborative remembering that went undetected in the first two experiments would be more likely to be detected in the third experiment.

I operationalised group executive control ability in two ways: (1) I derived a *Group Ability* measure, which was simply the average of the individual executive control abilities of group members; (2) I derived a *Group Discrepancy* measure, which was the difference between the individual executive control abilities of group members. Using these measures, I could compare how individual differences in executive control ability related to collaborative and post-collaborative performance. My key questions were: (1) Does Group Ability affect collaborative recall? (2) Does Group Discrepancy affect collaborative recall? (3) Does Individual Ability predict post-collaborative recall? (5) Does Group Ability predict post-collaborative recall? And (4) Does Group Discrepancy predict post-collaborative recall?

I also examined whether the self-reported use of retrieval strategies was related to recall success across both collaborative and individual recall tests, and whether this was contingent on the characteristics of the individual and/or the group. Research has shown that strategic encoding of material relates to the potential for disruption (e.g., Cokely et al., 2006), and influences post-collaborative benefits (Marion & Thorley, 2016). Additionally, other research has suggested that strategy use is related to positive collaborative outcomes such as collaborative facilitation (Harris et al., 2011; Meade et al., 2009).

Thus, my objective was to identify how the abilities a person brings into collaboration influences group performance and how the interaction of their ability and the experience of collaborating affects their memory subsequent to collaboration. Further, what role does the ability of their partner play in these dynamics? That is, do what you and your partner bring into collaboration matter for how well you do together and for what you take away?

Chapter 2

Experiment 1

A Continuous Measure Approach to Investigating the Role of Executive Control in Collaborative Recall

Experiment 1

Although we know a great deal about the general effects of collaboration on recall, there has been little research examining whether these effects depend on the cognitive characteristics of the individuals within the group. In this study, I examined how Individual Ability, Group Ability, and Group Discrepancy influenced both collaborative recall and carry-over effects in post-collaborative recall. I also examined the Relative Rank of partners within each dyad (i.e., higher ability partner vs lower ability partner) to investigate whether collaboration affected these individuals differently. I used a three-recall methodology in order to track items from baseline to post-collaborative recall, giving insight into how executive control characteristics influenced the costs and benefits of collaboration. Finally, I administered a post-experimental questionnaire to probe participants' use of individual and group retrieval strategies.

Method

Participants

Eighty-six undergraduate students from Macquarie University participated in the experiment in return for partial course credit. Of these participants, six were removed from analysis. Four participants solved fewer than the required 85% of math operations in the OSPAN (see Unsworth et al., 2005). Two participants did not attempt recall and were removed from the analyses. Thus, 80 participants (24 men, 56 women) were included in the final analyses with a mean age of 21.32 years (*SD* = 6.74). Twenty dyads (40 participants) were assigned to each of the two conditions (Nominal vs Collaborative). In similar previous collaborative recall experiments, the number of groups in each condition typically ranges from 12 (e.g. Congleton & Rajaram, 2014) to 16 (e.g. Pereira-Pasarin & Rajaram, 2011; Barber & Rajaram, 2011). Thus, in Experiment 1 I selected my sample size (20 dyads in each of the two conditions comprising 80 participants total) such that it would exceed that of other similar studies. I did this in order to increase my statistical power as it is possible that the effects of executive control ability on collaborative and post-collaborative recall are small.

Design

Experiment 1 was a between-subjects design with the Condition factor having two levels: Nominal group recall vs Collaborative group recall. In some analyses, an additional factor of Relative Rank (Higher vs Lower) was considered. In some analyses, Recall Session (Recall 1 vs Recall 3) was considered as a within-subjects factor. The individual and group-ability measures were continuous.

Materials

Word lists. Study stimuli consisted of six categorized words lists of 14 items each. These were constructed from the Van Overschelde, Rawson, and Dunlosky (2004) norms, which are an update of the Battig and Montague (1969) norms. Word lists used in the current experiment were identical to those used by Barber and Rajaram (2011a) except that two words were changed to culturally appropriate equivalents for the Australian sample (i.e., *beet* was changed to *beetroot* and *pepper* was changed to *capsicum*). I generated three pseudo-randomized lists with the constraint that no more than two words from the same category appeared consecutively. I counterbalanced the three lists across conditions. See Appendix A for word lists used in Experiment 1.

Automated Operation Span. To assess executive control ability, I used an automated version of the Operation Span task (OSPAN; Unsworth et al., 2005). The OSPAN is available to download by request at http://englelab.gatech.edu/tasks.html.

The computer program runs on the E-Prime 2.0 psychological testing application software. The OSPAN requires participants to mentally solve arithmetic expressions while simultaneously remembering presented letters of the alphabet. The OSPAN begins with a practice block that is broken into sections. In the first practice section, participants practice the letter recall portion. Letters appear one at a time on the screen for 800ms each. Following presentation of the letters, participants see a 4 x 3 grid of letters on which they use the mouse to click the letters in the order they remember them having been presented. The practice recall is untimed and participants receive feedback regarding their accuracy. In the second section of the practice phase, participants practice the math portion. An arithmetic expression is presented on the screen (e.g., 2 * 5 - 1 = ?) and participants are instructed to solve the operation as quickly as possible and then click the mouse to advance to the next screen where a digit (e.g., 9) is presented. Participants then click either “true” or “false”. After each operation, participants are given feedback on their accuracy. The purpose of the math practice section is to familiarise the participants with the operations portion of the task as well as establish an individual baseline measure of how long each participant took to solve the operations. This measure is used to account for individual differences in math processing speed by calculating an individual response deadline for use during the actual experimental session. The response deadline is set to each individuals’ mean time taken to solve practice operations plus 2.5 standard deviations. Participants complete 15 math operations in the practice sessions.

In the third and final practice block, participants perform both the letter recall and math portions. These trials are identical to the actual experimental trials. A math operation is presented. After solving it, the participants click the mouse and are presented with a possible answer. After clicking “true” or “false”, participants are presented with a letter that they aere required to remember. If the participants take longer than their individual response deadline, the program automatically moves on and scores that trial as an error. This is done to prevent rehearsal of letters and ensure that the OSPAN measures both processing and storage (i.e., working memory). The practice session finishes after participants have completed three practice blocks, each with two trials.

After the practice session, participants complete the experimental trials. There are three blocks of trials. Within each block, the trial sizes (number of operations and letters to be remembered) ranged from three to seven. Each block contains three sets of each of the trial sizes in a random order for a total of 75 letters and 75 math problems.

Post-experimental questionnaire. At the end of the session, participants completed post-experimental questionnaires. These questionnaires collected demographic information such as age and gender. In addition, they asked participants to indicate whether they had employed a specific recall strategy when recalling individually. For participants in the Collaborative condition, there was an additional question that asked participants to indicate whether the group in which they collaborated had employed a specific recall strategy when recalling Collaboratively.

Procedure

The experiment consisted of seven phases: (1) A study phase; (2) A distractor task; (3) Recall 1; (4) Recall 2; (5) Recall 3; (6) the OSPAN; and (7) Post-experimental questionnaire and debrief. Experimental sessions were always scheduled to include two participants, with dyads randomly assigned to either the Nominal or the Collaborative condition upon their arrival. If one of the participants failed to show up for their scheduled time, the remaining participant was assigned to the next open Nominal group by default.

Before the experiment began participants read and signed information and consent forms. Participants in the Nominal condition were told that they were completing a study that investigated how working memory influenced the way in which people remembered both alone and in groups.

In addition, participants collaboratively recalled in a ‘free-for-all’ manner rather than a ‘turn-taking’ manner. That is, participants were allowed to offer a word at their discretion rather than according to a predetermined order. While both methods are commonly used in collaborative recall, free-for-all recall would have more ecological validity as it more closely mimics extra-laboratory conditions than does a turn-taking procedure.

Study phase. Participants completed the study phase in the same room seated side by side at computers facing the same direction. They were separated by a distance of two meters with a screen between them. All participants saw one of the three counter-balanced lists displayed on a computer screen. The words were presented using E-prime 2.0 software and appeared in lowercase letters in black text on white background in the centre of the screen where they remained for three seconds each. A one second buffer separated the presentation of each word. Participants always saw the same list as the other member of the dyad with whom they were paired. The participants were not told that the words belonged to categories. The verbatim instructions in both conditions were:

> We’ll start today by studying a list of words presented on a computer screen. Please pay attention to each word as it is presented, as your

memory for these words will be tested later in the session. Are there any questions at this time? Okay, we'll now begin the study session.

Distractor task. Following the study phase, participants moved to a table behind them and were seated at opposite ends, facing each other. The table was two meters long and participants could not see what the other was writing. They were given a difficult Sudoku puzzle and spent 4 minutes attempting to solve it. If participants were unfamiliar with the task, brief verbal instructions were given. Written instructions also appeared on the puzzle. The distractor task was always performed individually.

Recall 1: Baseline individual recall. Participants were given a sheet of paper that contained three columns of 22 blank lines each (66 total lines) and were instructed to recall as many studied words as possible. They were advised that they could recall the words in any order they wished, but asked to produce only one word per line without skipping any lines. All participants completed Recall 1 individually and were allowed four minutes for this task. Verbatim instructions in both conditions were:

> Now I'm going to give each of you a piece of paper and I'd like you to recall as many words as you can from the list that you saw on the computer. You can recall the words in any order you wish, but only enter one word per line and do not skip any lines. This means no skipping around. Just enter each word one after the other. You have 4 minutes to try to remember as many words as possible. You may begin.

Recall 2: Group recall. Immediately following Recall 1, participants completed a second recall. Participants in the Nominal condition completed Recall 2 in an identical manner to Recall 1. Participants in the Collaborative condition moved to sit together at one side of the table and were given the same recall instruction as participants in the Nominal group, with the additional instructions that they both must agree that a word was present in order to record it. This *consensus* collaboration has been used in previous research (e.g., Harris et al., 2012). Collaborative sessions were audio-recorded and the participants were advised as such before the commencement of recall. Participants in both conditions were given four minutes to recall as many of the original study items as possible. They wrote their responses on lined paper that was identical to Recall 1. Verbatim instructions in the Nominal condition were:

> Now that you've had a chance to recall these words I'm going to ask you to try recalling them again. Remember, you are recalling the original words that you saw on the computer. You can recall the words in any order you wish, but again, no skipping around. You'll have another 4 minutes. You may begin now.

Verbatim instructions in the Collaborative condition were:

> Now that you've had a chance to recall the words by yourself, I'd like for you to try to recall as many words as you can again, but this time you can work together. Pick one person to write down the words on this sheet of paper. Remember, you are recalling words that you originally saw on the computer. You can recall the words in any order you wish but please do not skip around and enter only one word per line one word after another. Also, you both most agree that a word was present. Please settle any disagreements amongst yourselves. As we are interested in the things people say when they are remembering together, I'd like to record this session. Do I have your consent to begin audio recording? Okay. I'm going to turn on the recording device. Now

> please state your participant number found at the top of your recall sheet from the first recall. Are there any questions at this time? You now have 4 minutes to recall as many words as you can. You may begin now.

Recall 3: Final Individual Recall. Following group recall, all participants completed a final individual recall. Materials were identical to Recall 1. Verbatim instructions were:

> Now we only have one more task to do. You'll be recalling the words one last time. Again, you need to recall words that you originally saw on the computer and don't forget that you must write only one word per line without skipping any lines. You have 4 minutes. Begin now.

Assessment of executive control. Following the final recall, all participants returned to the computers on which they had completed the study phase and completed the OSPAN.

Debrief and post-experimental questionnaire. At the end of each session participants completed post-experimental questionnaires and were debriefed.

Measures and Scoring

Individual Ability. I operationalised Individual Ability in terms of executive control as working memory capacity, which I assessed using the OSPAN. When a participant completes the computerised OSPAN, a data file is generated which contains the OSPAN score. In my experiments, I used the partial scoring method, as opposed to the absolute scoring method. In the partial scoring method, participants receive credit for each correct letter recalled regardless of whether all letters in the sequence were recalled. The partial scoring method is generally deemed to possess superior psychometric properties to the absolute scoring method as it has higher internal consistencies (Conway et al., 2005; Friedman & Miyake, 2004) and exhibits stronger relationships with reading comprehension (Friedman & Miyake, 2004) and matrix reasoning (Unsworth & Engle, 2007). Partial scores in the OSPAN can range from 0 to 75, with higher scores indicating higher working memory capacity (i.e., better executive control).

Group Ability. I used individual OSPAN performance to calculate *Group Ability,* capturing the overall ability of each dyad. To calculate Group Ability, I simply averaged the OSPAN performance of both members of each dyad. For example, if one participant scored 40 and the other participant scored 50 on the OSPAN, the Group Ability of the dyad was 45.

Group Discrepancy. I also used individual OSPAN performance to calculate *Group Discrepancy,* capturing the degree to which individuals within each dyad were similar or different. To calculate Group Discrepancy, I calculated the absolute difference between the OSPAN scores of group members in each dyad. Using the previous example of scores of 40 and 50, Group Discrepancy would be equal to 10. Using individual OSPAN performance, I also distinguished participants according to their Relative Rank within the dyad. Within each dyad, individuals with the higher OSPAN score were classified as *Higher* and individuals with the lower OSPAN score were classified as *Lower*.

Individual recall performance. I calculated individual recall performance (Recall 1 and Recall 3) as the proportion of correctly recalled words out of the number of originally studied words. Thus, in Experiment 1 recall performance was equal to the number of correctly recalled words divided by 84. I considered misspellings and the plural forms of a singular words as correct.

Group recall performance. Participants completed group recall (Recall 2) as part of a Nominal or Collaborative dyad according to condition. To calculate Nominal group performance, I pooled the items correctly recalled by each participant in the dyad, only counting redundant items once. Nominal group performance was defined as the proportion of correctly recalled non-redundant pooled items out of the number of originally studied items. Collaborative group performance was the proportion of correct items each dyad recalled out of the number of originally studied items.

Individual intrusion rates. I calculated individual intrusion rates (Recall 1 and Recall 3) by first recording the number of intrusions each participant made. An intrusion was any single item that a participant generated that did not appear on the original study word lists. To derive intrusion rate, I divided the sum of intrusions by a participant's total output. That is, intrusion rate was equal to intrusions divided by the sum of correctly recalled items plus intrusions.

Group intrusion rates. Nominal group intrusion rates (Recall 2) were calculated by counting the pooled intrusions from each member of the dyad. If an intrusion happened to be recalled by both members of the Nominal dyad, it was only counted once. Similar to individual intrusion rates, Nominal intrusion rates were then calculated as a proportion of intrusions out of the total output of the Nominal dyad (i.e., the sum of the correctly recalled and incorrectly recalled items). Collaborative intrusion rates (Recall 2) were simply the number of intrusions each Collaborative dyad recalled divided by their total Collaborative output.

Change scores. I calculated change scores for both conditions as individuals' *Gained items* or *Lost items* from Recall 1 to Recall 3 as a proportion of items initially recalled on Recall 1. Gained items were items that a participant did not generate during Recall 1 but generated during Recall 3. In other words, items that were "picked up" during the intervening Collaborative or Non-collaborative Recall 2. To calculate the proportion of Gained items, I calculated the number of items that appeared in Recall 3 that did not appear in Recall 1. I then divided this number by the total number of items recalled at Recall 1. Thus, the Gained item measure was a proportion of the number of items gained out of initial, individual recall. Alternatively, Lost items were items that a participant *did* generate during Recall 1 but did *not* generate during Recall 3. These are items that were "dropped" or forgotten during the intervening collaboration. To calculate the proportion of Lost items, I calculated the number of items that did not appear in Recall 3 but that had appeared in Recall 1. I then divided this number by the total number of items recalled at Recall 1. Thus, the Lost item measure was a proportion of the number of items lost out of initial, individual recall.

Results

Statistics

For all inferential statistics using null-hypothesis testing, I used a two-tailed analysis and a traditional significance criterion ($\propto$ = .05). For all follow-up tests (i.e., t-tests), I used a Bonferroni-corrected significance criterion (i.e., $\propto$ = .05/the number of comparisons).

Baseline

Prior to collaboration, I obtained baseline data for all participants in the sample. This baseline data consisted of individual recall performance and intrusion rates. I also considered executive control as a baseline measure and analysed these data first even though the OSPAN was administered at the end of the session. This was because executive control represents an underlying individual ability and thus a characteristic that an individual brings into the study.

Individual Executive Control. Mean OSPAN performance for participants in the Nominal condition was 58.40 (*SD* = 11.6) with scores ranging from 23 to 75. Mean OSPAN performance for participants in the Collaborative condition was 56.85 (*SD* = 13.4) with scores ranging from 27 to 73. An independent samples t-test of individual OSPAN scores indicated no difference between conditions, $t(78) = -.55$, $p = .58$. suggesting that there were no pre-existing differences in executive control between participants in the Nominal condition and participants in the Collaborative conditions. Overall, my data were consistent with published norms (see Table 2).

Table 2
Comparison of OSPAN Scores by Percentile

	Percentile						
	5	25	33.3	50	66.6	75	95
Experiment 1 (N = 80)	32.15	51.25	54.97	62	65	67	72
Redick et al. norms (N = 6,236)	29	51	55	61	65	67	73

Note. OSPAN = Operation Span scores using partial scoring method. Redick et al. norms represent normative data from Redick et al. (2012).

Recall 1: Initial individual recall and intrusions. The mean proportion of correctly recalled items by individuals assigned to the Nominal condition was 0.24 (*SD* = 0.08), with performance ranging from 0.07 to 0.45. The mean proportion of correctly recalled items by individuals assigned to the Collaborative condition was 0.25 (*SD* = 0.08) with performance ranging from 0.10 to 0.39. An independent samples t-test indicated no differences between conditions, $t(78) = -0.76$, $p = .450$. That is, future non-collaborators and future collaborators did not differ in baseline recall with participants recalling around one-fourth of the list across both conditions.

The mean intrusion rate by individuals assigned to the Nominal condition was 0.05 (*SD* = 0.06) with scores ranging from 0.00 to 0.32. The mean intrusion rate by individuals assigned to the Collaborative condition was 0.04 (*SD* = 0.06) with scores ranging from 0.00 to 0.23. Overall, intrusions rates were very low, but there was large variability. An independent samples t-test indicated no difference between conditions, $t(38) = -0.13$, $p = .894$. That is, future Non-collaborators and future collaborators did not differ in baseline intrusion rates.

I also tested whether individuals who reported using specific retrieval strategies differed in their baseline recall performance. Of the 80 participants in my sample, 61 reported using an individual strategy to guide their recall on the post-experimental questionnaire and 19 reported using no strategy. Mean recall for those who reported using a strategy was 0.26 (*SD* = 0.08). Mean recall for those who did not report using a strategy was 0.20 (*SD* = 0.05). There was a significant difference between the two groups, $t(78) = 2.97$, $p = .005$, $d = .90$, which suggested that using a strategy helped participants to remember more words.

Recall 1: Relationship with executive control. To test whether individual performance was related to individual executive control ability, I obtained Pearson bivariate correlations between OSPAN scores and the Recall 1 performance of my entire sample. I found no significant relationship between individual OSPAN scores and individual initial recall, $r(78) = 0.14$, $p = .214$ (see Figure 3). I also examined whether there was a relationship between individual intrusion rates and individual executive control ability. I found no evidence of a relationship between individual

OSPAN scores and intrusion rates, *r*(78) = 0.03, *p* = .610 (see Figure 4). These findings suggested that participants' executive control ability was not related to their performance on the initial memory test with respect to either correctly or incorrectly recalled items.

Finally, I tested whether individuals who, on the post-experimental questionnaire, reported using specific strategies to guide their individual retrieval differed in their executive control ability. Mean OSPAN scores for those who used a strategy was 57.85 (*SD* = 11.94). Mean OSPAN scores for those who did not use a strategy was 56.89 (*SD* = 14.4). The difference between the two groups was not significant, *t*(78) = 0.29, *p* = .772, which suggested that people who employed specific retrieval strategies were not systematically different in executive control ability from individuals who did not employ a strategy.

Collaboration

During collaborative recall, I collected group-level data in order to assess dyads' recall performance and how this might be influenced by the composition of the groups in terms of executive control. These data included group recall performance and group intrusion rates (Nominal and Collaborative groups) as well as Group Ability and Group Discrepancy measures that I derived as previously described.

Recall 2: Group recall. To test for collaborative inhibition, I calculated Recall 2 proportions for each condition. The mean proportion of correctly recalled items in the Nominal condition was 0.44 (*SD* = 0.09), with scores ranging from 0.26 to 0.57. The mean proportion of correctly recalled items in the Collaborative condition was 0.32 (*SD* = 0.10), with scores ranging from 0.13 to 0.48. Consistent with previous research, Nominal groups outperformed Collaborative groups, *t*(38) = 4.05, *p* < .001, *d* = 1.26.

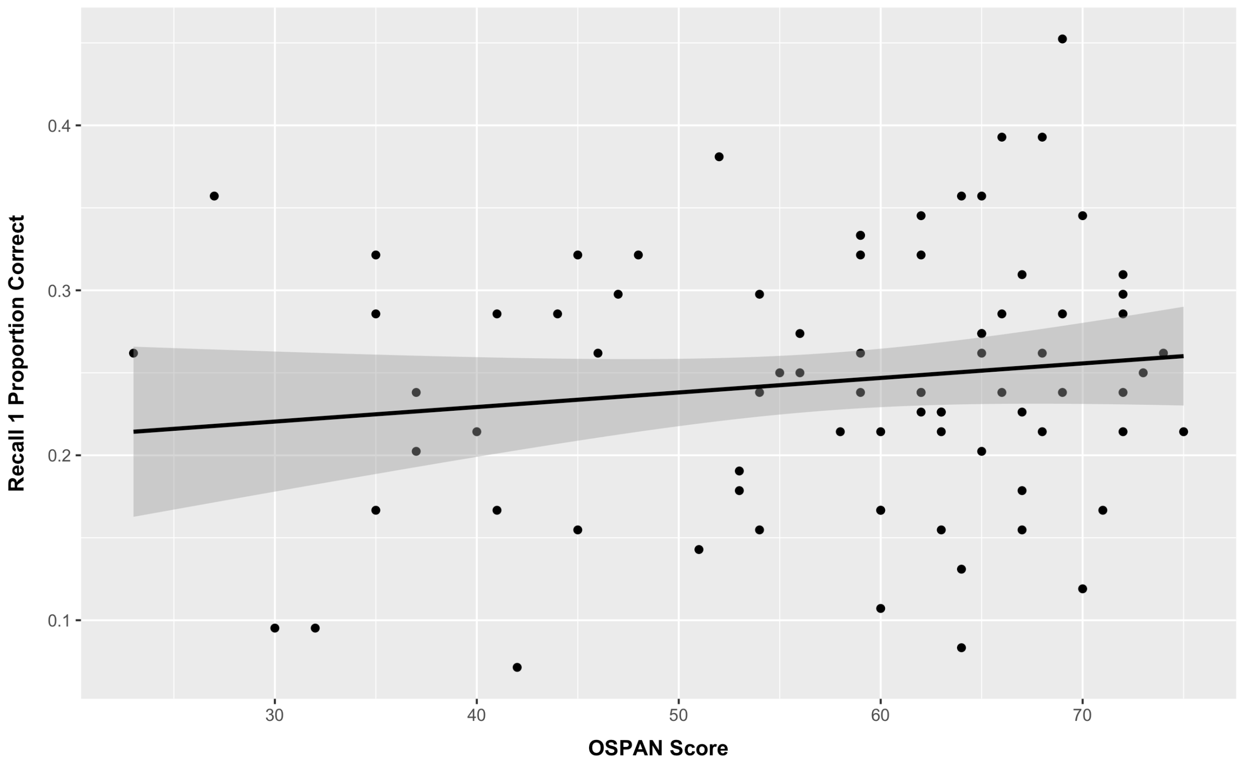

Figure 3. Recall 1 performance as a function of Individual Ability. Shaded area represents standard error.

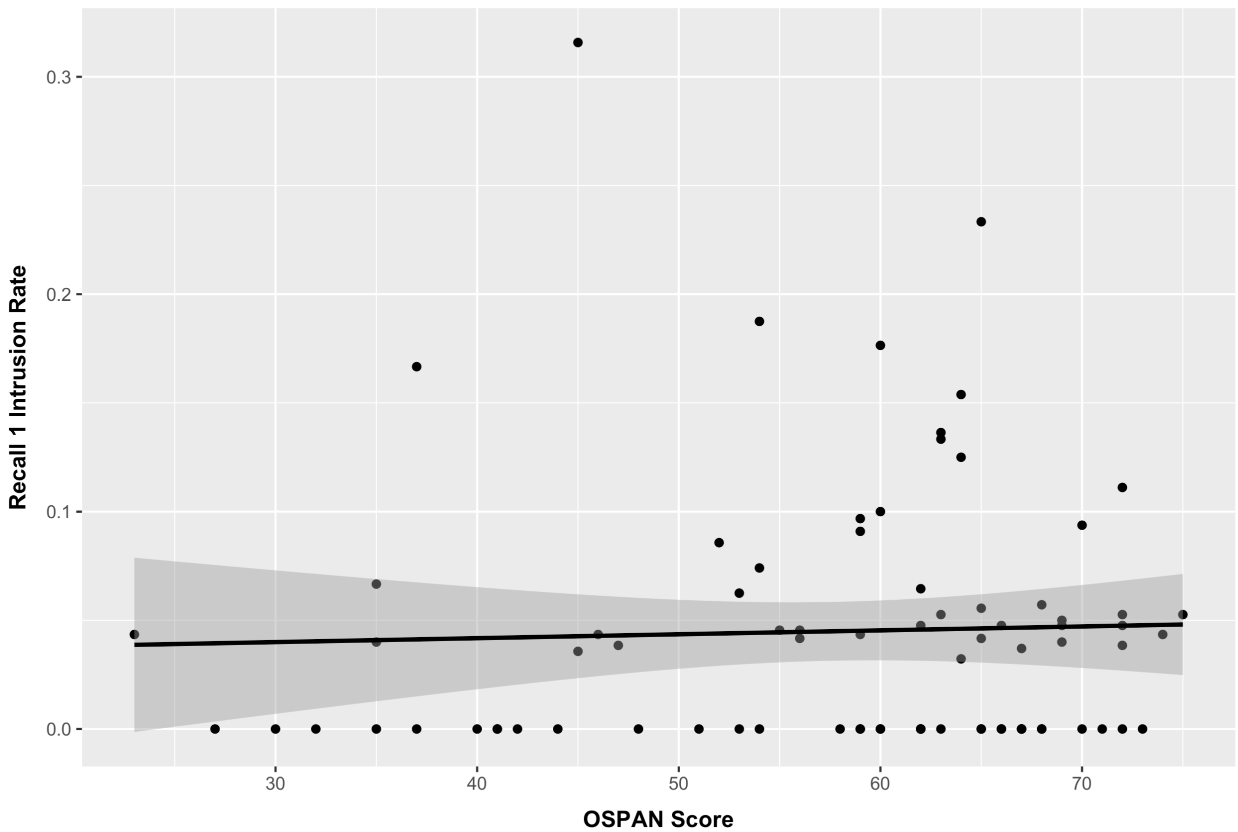

Figure 4. Recall 1 intrusion rates as a function of Individual Ability. Shaded area represents standard error.

That is, Collaborative groups demonstrated the standard collaborative inhibition effect.

I also tested whether group-level retrieval strategies affected Collaborative group performance. To do this, I analysed the post-experimental questionnaire data of participants in Collaborative groups and distinguished groups according to three categories: 1) No Strategy (i.e., those dyads in which neither member reported that the group used a strategy; n = 10); 2) Disagree Strategy (i.e., those dyads in which one member reported that the group used a strategy and one member reported that the group did not use a strategy; n = 2); and 3) Agree Strategy (i.e., those dyads in which both members reported that the group used a strategy; n = 8).

I then performed a one-way ANOVA on the Collaborative groups' Recall 2 data with Strategy Use (No Strategy vs Disagree Strategy vs Agree Strategy) as a between-subjects factor (see Figure 5). Results indicated a significant effect of Strategy Use, $F(2,17) = 3.72$, $p = .045$, $\eta^2_p = .30$. Follow-up pairwise t-tests revealed a significant difference in Recall 2 performance between the Disagree Strategy and the Agree Strategy dyads, $t(8) = 3.05$, $p = .016$, $d = 1.98$. The comparison between No Strategy and Agree Strategy failed to reach the corrected significance level (α = .017), $t(16) = 2.32$, $p = .034$. The comparison between No Strategy and Disagree Strategy was not significant, $t(10) = 0.62$, $p = .552$. This suggested that dyads in which both members used a retrieval strategy recalled more than dyads in which members disagreed on strategy use. Further, the numerical data was suggestive that dyads in which individuals disagreed on whether a strategy was used were the least successful.

Recall 2: Group intrusions. To determine whether collaboration influenced inaccurate recall, I calculated Recall 2 intrusions rates for each condition. The mean

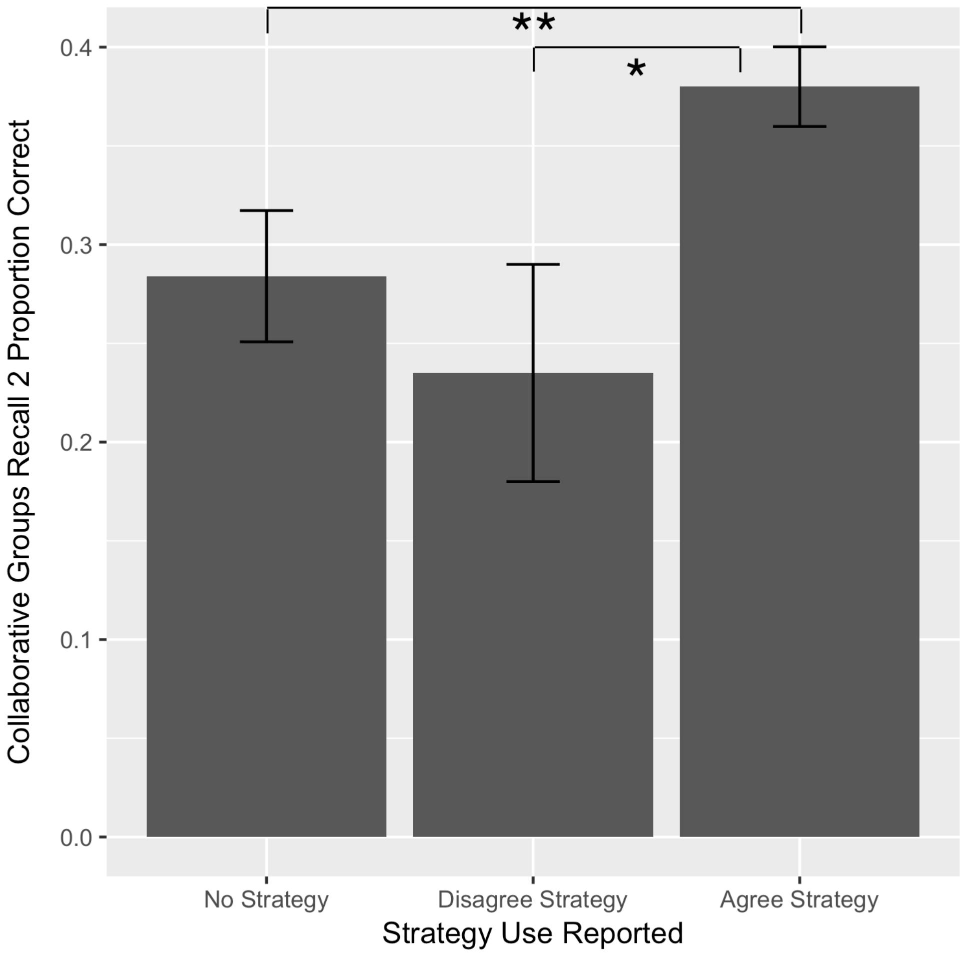

Figure 5. Collaborative group Recall 2 performance as a function of group strategy use. *p = .016 **p = .034

intrusion rate in the Nominal condition was 0.03 (*SD =* 0.04) with scores ranging from 0.00 to 0.15. The mean intrusion rate in the Collaborative condition was 0.03 (*SD =* 0.05) with scores ranging from 0.00 to 0.20. Overall, intrusion rates were very low across conditions and I found no evidence to suggest any difference between the intrusion rates of the Nominal and Collaborative conditions, $t(38) = -0.32$, $p = 751$.

I also tested whether group retrieval strategies influenced Collaborative groups' intrusion rates. I performed a one-way ANOVA on the intrusion rate data with Strategy Use as a between-subjects factor. Results indicated no significant effect of Strategy Use, $F(2,17) = 0.36$, $p = .704$. Thus, the use of group retrieval strategies did not influence the accuracy of Collaborative groups.

Group Ability and Group Discrepancy. A key research question was whether the executive control characteristics of groups influenced the outcomes of collaboration. To address this, I calculated both overall ability of each group (*Group Ability)* as well as a measure of difference in ability between members within each group (*Group Discrepancy).* The mean Group Ability score in the Nominal condition was 58.40 (*SD* = 8.64) and ranged from 41.50 to 73.00. The mean Group Ability score in the Collaborative condition was 56.85 (*SD* = 9.29) and ranged from 36.00 to 68.50. To test whether dyads in each condition differed with respect to Group Ability, I conducted an independent samples t-test of the Group Ability measure. I found no effect of condition, suggesting that Group Ability did not differ between the Nominal and Collaborative conditions, $t(38) = 0.54$, $p = .588$.

The mean Group Discrepancy score in the Nominal condition was 12.40 (*SD* = 9.46) and ranged from 2 to 37. The mean Group Discrepancy score in the Collaborative condition was 14.80 (*SD* = 12.79) and ranged from 1 to 45. To test whether dyads in each condition differed with respect to Group Discrepancy, I conducted an independent samples t-test of the Group Discrepancy measure. I found no effect of condition suggesting that Group Discrepancy did not differ between the two conditions, $t(38) = 0.67$, $p = .504$. Overall, this indicated that group characteristics in my sample were equivalent between the two conditions [1].

Recall 2: Relationship with executive control. To test my question of whether group-level executive control characteristics influenced collaboration, I conducted separate correlational analyses for each condition between each of the group-level measures and group performance. Group Ability was not related to Recall

[1] Visual examination of both the Group Ability and Group Discrepancy data suggested that these measures exhibited a non-normal distribution. Thus, for significant effects in the following presentation of results I subsequently converted these data to *z*-scores and reanalyzed. This reanalysis produced similar results.

2 performance in either the Nominal, *r*(18) = 0.24, *p* = .315, or the Collaborative groups, *r*(18) = 0.09, *p* = .720 (see Figure 6). Likewise, Group Discrepancy was not related to Recall 2 performance in either the Nominal, *r*(18) = -0.33; *p* = .156, or the Collaborative groups, *r*(18) = 0.07, *p* = .773 (see Figure 7). Thus, contrary to my expectations, group-level executive control characteristics were not related to collaborative recall.

To examine whether there was a relationship between group-level executive control measures and intrusions, I conducted separate correlational analyses for each condition between for each of the group-level measures and group intrusion rates. Group ability did not relate to Recall 2 intrusion rates in the Nominal, *r*(18) = 0.12, *p* = .609, or the Collaborative groups, *r*(18) = 0.03, *p* = .885 (see Figure 8). Likewise, Group Discrepancy did not relate to Recall 2 intrusion rates in the Nominal, *r*(18) = 0.04; *p* = .865, or the Collaborative groups, *r*(18) = 0.05, *p* = .825 (see Figure 9). Thus, group-level executive control measures did not relate to group recall performance or accuracy.

Strategy use and group-level executive control. I tested whether the executive control characteristics of Collaborative groups influenced whether groups engaged in specific retrieval strategies. To test whether strategy use was reported by groups with different abilities, I performed a one-way ANOVA on Collaborative groups' Group Ability measure between the No Strategy, Disagree Strategy, and Agree Strategy dyads. Results indicated that there were no differences in Group Ability between the different strategy groups, *F*(2,17) = 1.62, *p* = .308. This suggested that the groups who employed retrieval strategies did not differ in their overall ability compared with groups who did not employ retrieval strategies.

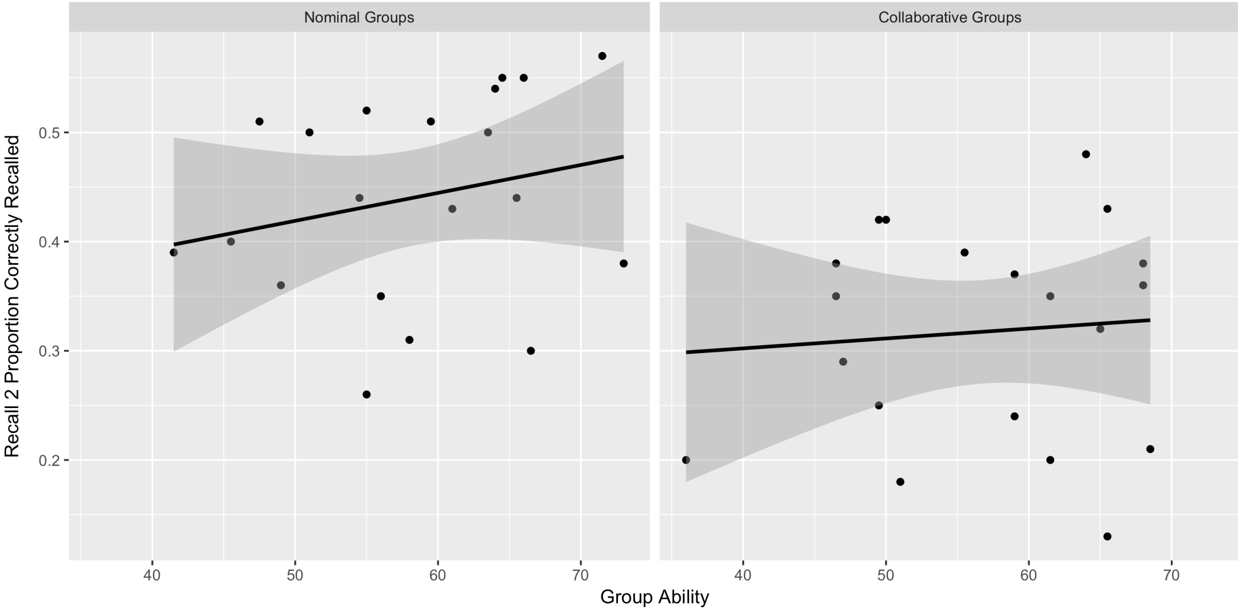

Figure 6.Group recall performance in the Nominal and Collaborative conditions as a function of Group Ability. The shaded area represents standard error.

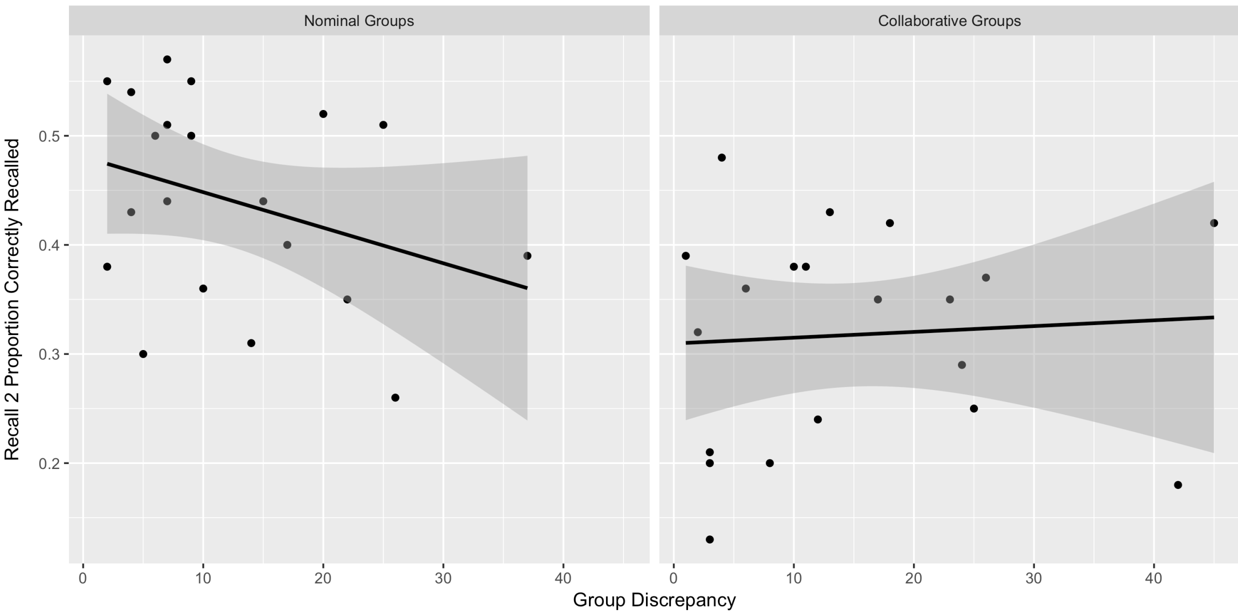

Figure 7. Group recall performance in the Nominal and Collaborative conditions as a function of Group Discrepancy. The shaded area represents the standard error.

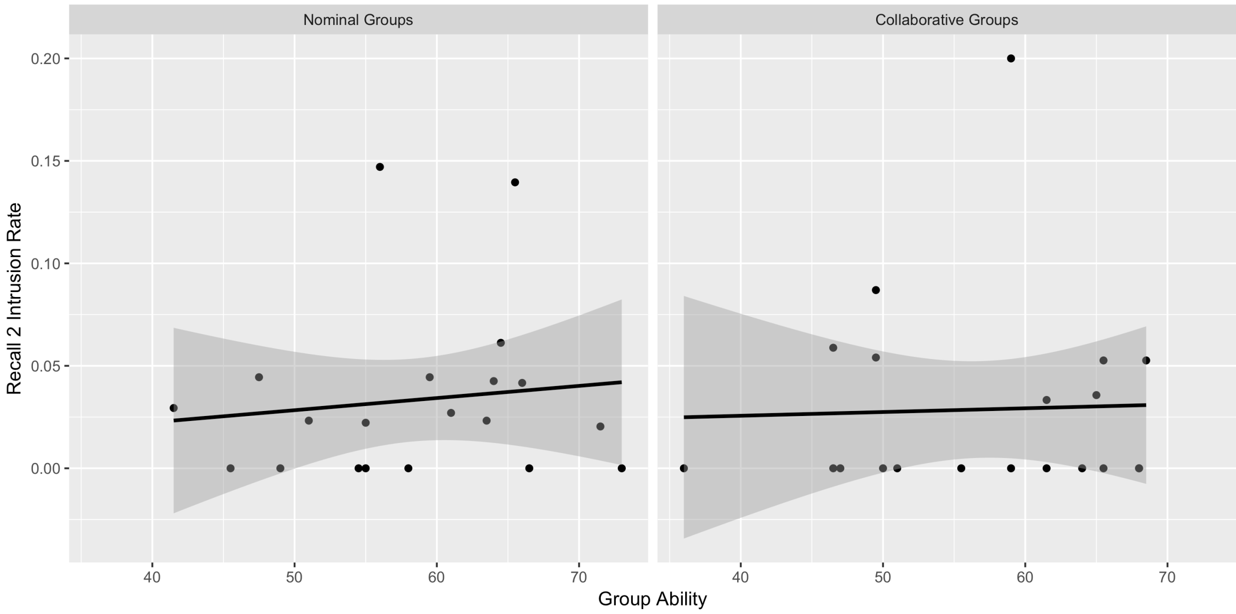

Figure 8. Group intrusion rates in the Nominal and Collaborative conditions as a function of Group Ability. The shaded area represents standard error.

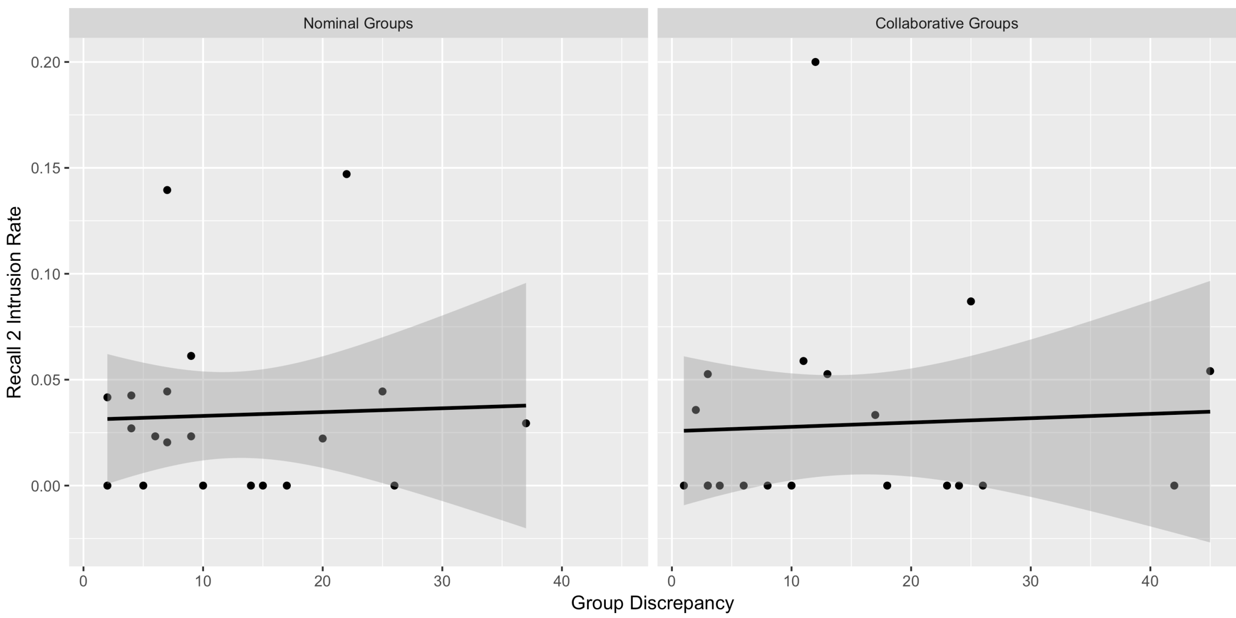

Figure 9. Group intrusion rates in the Nominal and Collaborative conditions as a function of Group Discrepancy. The shaded area represents standard error.

To test whether the strategy use was reported by groups with varying discrepancies in their abilities, I performed a one-way ANOVA on collaborative groups' Group Discrepancy score between the No Strategy, Disagree Strategy, and Agree Strategy dyads (see Figure 10). Results of the Group Discrepancy ANOVA suggested a difference between the types of strategy use, $F(2,17) = 5.55$, $p = .014$, $\eta^2_p = .40$. Follow-up t-tests indicated a significant difference in Group Discrepancy between Disagree Strategy and Agree Strategy dyads where dyads in which partners disagreed on whether they employed a strategy were more discrepant with respect to executive control ability than were dyads in which partners agreed that no strategy was used, $t(10) = 4.10$, $p = .002$, $d = 1.10$. The comparison between No Strategy and Agree Strategy failed to reach the corrected significance level ($\alpha = .017$), $t(16) = 2.14$, $p = .048$. The comparison between Disagree Strategy and Agree Strategy was not significant, $t(8) = 1.36$, $p = .211$. This suggested that groups in which partners disagreed on whether they had employed a specific retrieval strategy were more likely to be discrepant in ability than groups in which partners agreed that they had not used a strategy. Further, examining the numerical trend in the data suggests that groups in which individuals disagreed on strategy use were the most discrepant overall. In addition, subsequent examination of the data revealed that in dyads that disagreed on strategy use, all participants who reported that the group did not use a strategy were the relatively Lower ability partners while all the participants who reported that the group used a strategy were the relatively Higher ability partners.

Group recall: Relative ability and relative contribution. As I was interested in how the composition of groups with respect to executive control ability influenced collaborative recall, I also examined group recall data from the perspective of the relative ability of each dyad member. That is, how did the performance of the

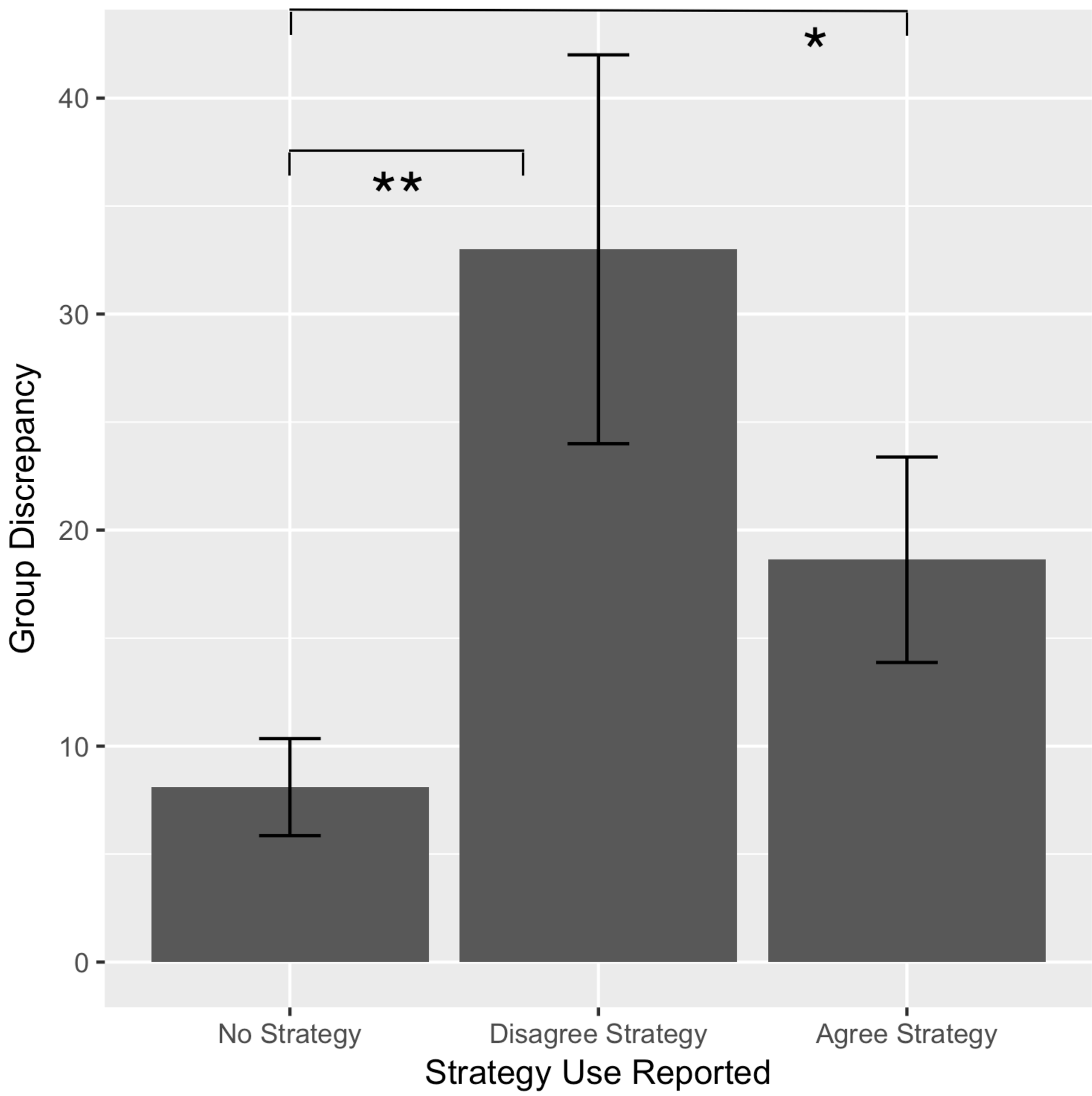

Figure 10. Group Discrepancy as a function of reported retrieval strategy use. Error bars represent SEM. *p = .048, **p = .002

relatively Lower ability member of a dyad relate to the performance of the relatively Higher ability member of a dyad? To investigate this, I distinguished each participant according to their Relative Rank within the dyad: *Lower* or *Higher*. I then listened to, and transcribed, the audio recordings of the Collaborative groups to score each individuals' unique recall output during the collaboration (i.e., Recall 2). Nominal individual Recall 2 output was simply the separate items recorded by each individual in the Nominal condition (see Figure 11).

To test whether the proportion of correct items recalled by each individual differed by their rank (i.e., Lower vs Higher) within the dyad, I conducted a 2 x 2

ANOVA with Condition (Nominal vs Collaborative) and Relative Rank (Lower vs Higher) as between-subjects factors. Results indicated a significant main effect of Condition, $F(1,76) = 30.56$, $p < .001$, $\eta^2_p = .29$, but not of Relative Rank, $F(1,76) = 0.03$, $p = .871$. The interaction was also non-significant, $F(1,76) = 0.06$, $p = .814$. This suggested that the contribution of correct items during the collaborative recall phase was equivalent for both the relatively Higher and Lower ability partners.

In summary, I found no evidence that group-level executive control measures influenced collaborative recall. In addition, there was no evidence to suggest that relative ability within groups related to the number of correct items individuals offered during collaborative recall. Although this was unexpected, effects of collaborative recall are not limited only to the collaboration, but collaboration has carry-over effects in the form of superior subsequent performance for former Collaborators versus former Non-collaborators (Barber & Rajaram, 2011b; Blumen & Rajaram, 2008; Congleton & Rajaram, 2011; Henkel & Rajaram, 2011). Next, I analysed post-collaborative performance and its relationship to individual and group-level measures of executive control.

Post-Collaboration

Another of my key research questions was whether the characteristics of the group in which an individual collaborated would influence carry-over effects of collaboration. To examine this question, I derived Recall 3 proportions in an identical manner to Recall 1. With this information, I compared subsequent, post-collaborative recall to the baseline initial individual recall data for both Non-collaborators and Collaborators. Non-collaborators were former members of Nominal groups; Collaborators were former members of Collaborative groups.

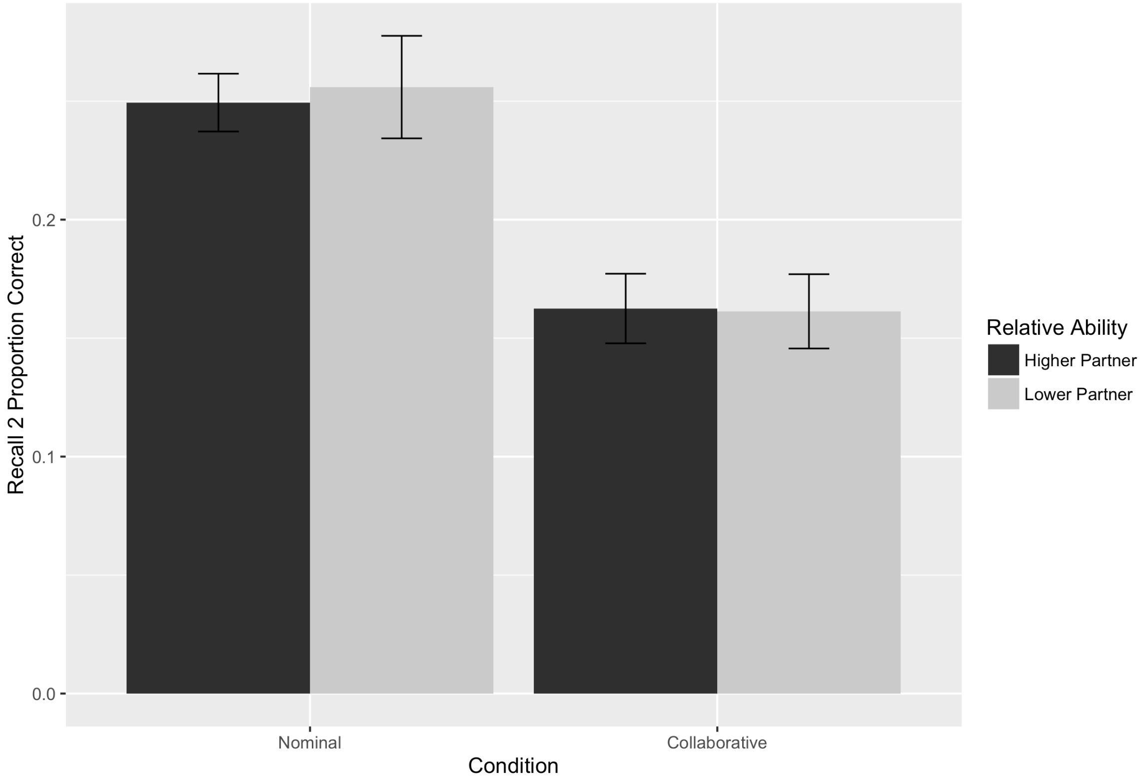

Figure 11. Proportion of correct items offered by each dyad member during group recall according to their Relative Rank within the dyad. Error bars represent SEM.

Recall 3: Final individual recall and intrusions. The mean proportion of correctly recalled items by former Non-collaborators was 0.26 (*SD* = 0.09) with scores ranging from 0.07 to 0.49. The mean proportion of correctly recalled items by former Collaborators was 0.29 (*SD* = 0.09) with scores ranging from 0.11 to 0.46. The mean intrusion rate by Non-collaborators was 0.06 (*SD* = 0.08) with scores ranging from 0.00 to 0.38. The mean intrusion rate by Collaborators was 0.04 (*SD* = 0.06) with scores ranging from 0.00 to 0.27.

To test whether Individual Ability was related to post-collaborative performance, I conducted correlational analyses between individual OSPAN scores and Recall 3 scores for both Non-collaborators and Collaborators. The relationship was not significant for either Non-collaborators, $r(38) = .09$, $p = .594$, or Collaborators, $r(38) = .24$, $p = .128$. This suggested that Individual Ability was not related to post-collaborative benefits.

To test whether Individual Ability was related to post-collaborative intrusions, I conducted correlational analyses between individual OSPAN scores and Recall 3 intrusion rates for both Non-collaborators and Collaborators. The relationship was not significant for either Non-collaborators, $r(38) = .09$, $p = .590$, or collaborators, $r(38) = -.89$, $p = .632$. This suggested that Individual Ability did not influence post-collaborative intrusion rates.

Recall 1 vs Recall 3. To assess whether collaboration benefited previous Collaborators relative to previous Non-collaborators, I performed a 2 x 2 mixed-model ANOVA with Condition (Nominal vs Collaborative) as the between-subjects factor and Recall Session (Recall 1 vs Recall 3) as the within-subjects factor. Results indicated no significant effect of Condition, $F(1,78) = 0.22$, $p = .638$, but a significant main effect of Recall Session, $F(1,78) = 53.62$, $p < .001$, $\eta^2_p = .41$, which was further qualified by a significant interaction between the two variables, $F(2,78) = 24.71$, $p < .001$, $\eta^2_p = .24$. Follow-up t-tests indicated that whereas Non-collaborators did not exhibit an increase in recall from Recall 1 to Recall 3, $t(39) = 0.56$, $p = .575$, Collaborators did, $t(39) = 2.86$, $p = .006$, $d = .92$ (see Figure 12). Thus, the act of collaboration enhanced later individual performance, a finding consistent with past post-collaborative benefits noted in previous literature.

I also performed a 2 x 2 mixed-model ANOVA on intrusion rates (see Figure 13). There were no significant effects, all Fs < 0.57, all ps $> .427$. Thus, there were no significant changes in intrusion rates from Recall 1 to Recall 3 for either Non-collaborators or Collaborators. In addition, these individuals showed equivalent intrusion rates relative to each other. That is, I found no evidence of the expected post-collaborative benefits of error-pruning and error rates were low across conditions.

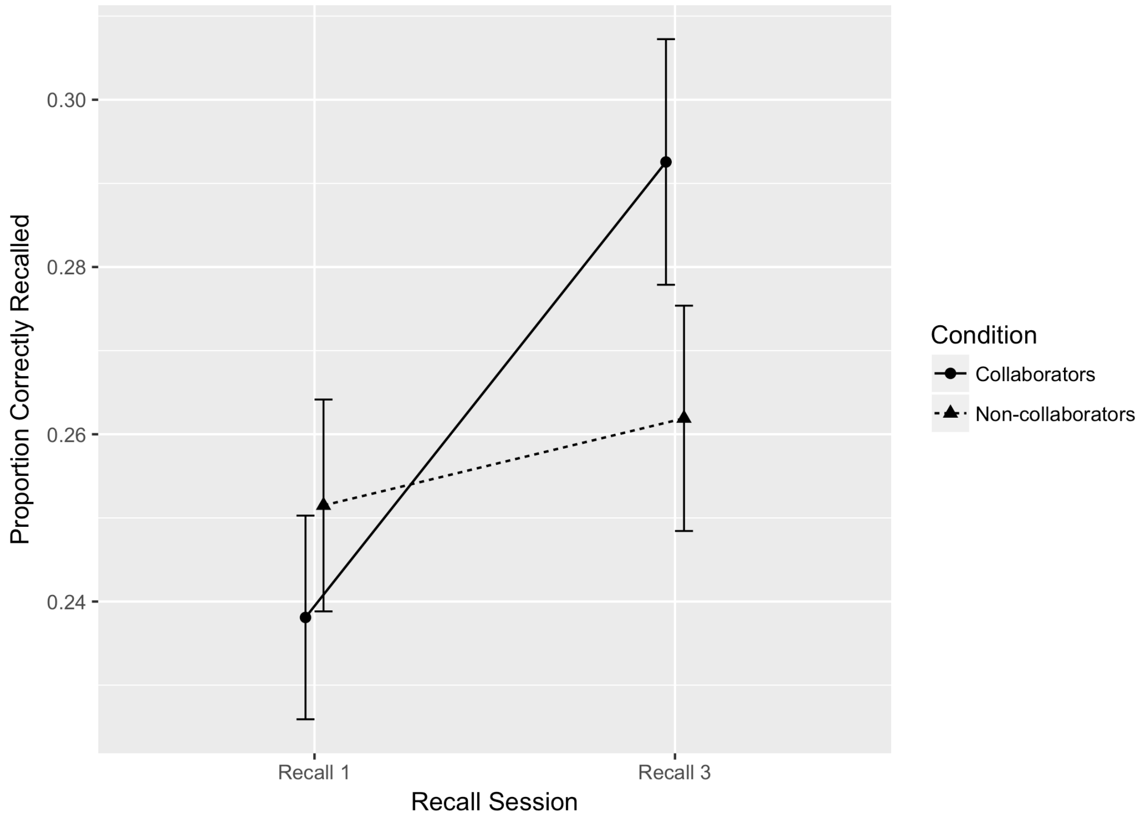

Figure 12. Individual recall performance from Recall 1 to Recall 3 by condition. Error bars represent SEM.

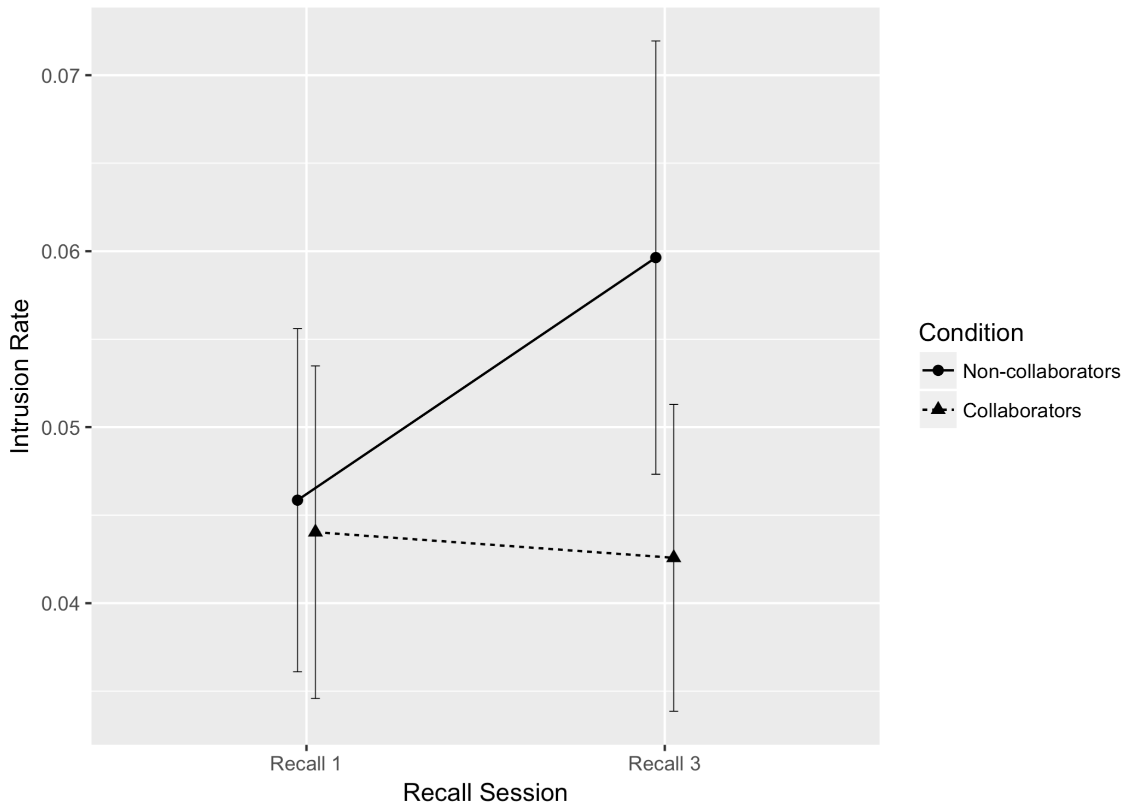

Figure 13. Individual intrusion rates from Recall 1 to Recall 3 by condition. Error bars represent SEM.

Change scores: Source of post-collaborative benefits. While the raw recall data can determine the presence or absence of post-collaborative benefits, it does not indicate the source of these benefits. To determine the source of post-collaborative benefits (or costs), the data can also be expressed as the proportion of items Gained or Lost between successive recall sessions. For example, if an individual recalled 10 items at Recall 1 and 13 items at Recall 3, they would show a net improvement of three items, or 0.3 change. However, a 0.3 improvement could be due to simply gaining three new items that they were re-exposed to during collaboration; or, it could be due to gaining eight new items while also losing five items that they had initially remembered. These two patterns are quite different and one could draw very different conclusions from them regarding the processes taking place during collaboration.

Did Individual Ability relate to items Gained and Lost? To test whether Individual Ability related to gains or losses that occurred as a result of collaboration, I obtained Pearson bivariate correlations between individual OSPAN scores and Gained and Lost scores for both Non-collaborators and Collaborators (see Table 3). None of the relationships were significant, all ps > .098. This suggested that, like overall post-collaborative benefits, the gains and losses from Recall 1 to Recall 3 were not due to any relationship with Individual Ability.

Table 3

Correlations Between Individual Ability and Change Scores

Condition	Gained	Lost
Non-collaborators	.11	.27
Collaborators	.02	-.20

Note. Values represent Pearson coefficients.

Did relative ability relate to items Gained and Lost? To test if the composition of groups with respect to executive control influenced post-collaborative gains, I performed a 2 x 2 ANOVA on the Gained item proportions with Condition (Nominal vs Collaborative) and Relative Rank (Lower vs Higher Partner) as between-subjects factors (see Table 4). Results indicated a significant main effect of Condition, $F(1,76) = 33.56$, $p < .001$, $\eta^2_p = .31$, but not of Relative Rank, $F(1,76) = 0.00$, $p = .980$. The interaction between the two factors was not significant, $F(1,76) = 0.12$, $p = .731$. These results suggested that individuals who had previously collaborated, relative to those who had not collaborated, gained a greater proportion of items from Recall 1 to Recall 3 (See Tables 4 and 5).

Table 4

Mean Proportion of Items Gained and Lost from Recall 1 to Recall

Condition & EC Rank		Gained	Lost	Net
Non-collaborators				
	Lower Partner	.16 (.21)	.10 (.10)	.07 (.15)
	Higher Partner	.18 (.11)	.15 (.06)	.03 (.11)
Collaborators				
	Lower Partner	.40 (.17)	.16 (.11)	.25 (.25)
	Higher Partner	.39 (.18)	.14 (.08)	.25 (.22)

Note. Net = Gained – Lost. Each item type is expressed as a proportion of initial recall. SD in parentheses.

To test if the composition of groups, with respect to executive control, influenced post-collaborative losses, I performed similar 2 x 2 ANOVA on Lost item proportions. Results indicated no significant main effect of Condition, $F(1,76) = 1.13$ $p = .291$, or Relative Rank, $F(1,76) = 0.83$, $p = .364$. The interaction between the two factors was marginally significant, $F(1,76) = 3.76$, $p = .056$, $\eta^2_p = .05$. Because the interaction between Condition and Relative Rank approached significance and was central to one of my main research questions, I performed follow-up t-tests on the

Lost item data. Although the difference between Non-collaborating Lower partners' and Collaborating Lower partners' Lost items was not significant, $t(38) = 1.78$, $p = .082$, the numerical pattern suggested that the Lower ability partners who previously collaborated may have lost a higher proportion of items than Lower ability partners who had not previously collaborated (0.16 versus 0.10, respectively; see Table 4).

Overall, these results suggested that collaboration benefited subsequent post-collaborative performance. Individuals in Collaborative groups, relative to Nominal groups, experienced benefits in the form of more items gained rather than fewer items lost. This pattern, however, was not contingent on an individuals' ability relative to their partners' (see Table 5).

Table 5

Mean Proportion of Items Gained and Lost from Recall 1 to Recall 3 by Condition (SD in parentheses)

	Gained	Lost	Net
Nominal	.17 (.17)	.13 (.09)	.05 (.13)
Collaborative	.40 (.18)	.15 (.10)	.25 (.23)

Note. Net = Gained - Lost. Each item type is expressed as a proportion of initial recall.

Did group-level characteristics relate to items Gained and Lost? To investigate the role group-level executive control characteristics of Collaborative groups played in modulating post-collaborative benefits, I conducted separate correlational analyses between each of the group-level executive control measures (Group Ability and Group Discrepancy) and the change scores for both Lower and the Higher partners (see Table 6). In these analyses, I included the Net change score data in order to determine whether group-level executive control characteristics related to post-collaborative benefits overall, rather than simply to losses or gains.

Table 6
Correlations between Collaborators' Group EC Measures and Change Scores

		Change Score		
Relative Rank by Group EC		Net	Gained	Lost
Lower Partner				
	Group Ability	-.02	-.03	-.02
	Group Discrepancy	-.44*	-.29	.53*
Higher Partner				
	Group Ability	.19	.14	-.23
	Group Discrepancy	-.24	-.19	.27

Note. Values represent Pearson coefficients. EC = executive control.
*$p < .05$

Only the relatively Lower ability partners demonstrated a significant relationship between post-collaborative effects and Group Composition. This came in the form of a significant negative relationship between Group Discrepancy and Net change scores. That is, the Lower ability dyad member experienced significantly fewer post-collaborative benefits as the discrepancy between their ability and their partner's ability increased ($p = .015$). Further, this relationship was driven by the proportion of Lost items as opposed to the proportion of Gained items ($p = .044$). This is interesting as it supports the marginal interaction and trend I observed previously where Lower ability individuals who had previously collaborated had numerically higher proportions of Lost items than did Lower ability individuals who had not previously collaborated. Taken together, a pattern began to emerge: Although there was no evidence that the overall ability of the group in which individuals collaborated related to post-collaborative benefits, it appeared that the difference in ability between the two collaborators within the group did. The relatively Lower ability partners who collaborated with partners whose abilities were more dissimilar (and higher) from themselves were more likely to experience diminished collaborative benefits as a result of losing items that they had initially remembered.

Which items were lost? Considering the evidence that Lower ability partners in collaborating groups were not getting the post-collaborative benefits that would be expected, I explored whether there was anything unique about the items that they were losing. Was it something about the way the items were generated during collaboration that related to Lower ability members forgetting them and would this imply something about reexposure and/or relearning via retrieval? For example, would the Lower ability partner be less likely to forget an item if she had generated it during collaboration than if her partner had generated the item? By examining how lost items emerged (or failed to emerge) during collaboration I could distinguish between the two processes that presumably influence post-collaborative recall. If the participant herself generated an item during collaboration, then this is an instance of relearning via retrieval. Likewise, if the participant's partner generated the item during collaboration, then this is an instance of reexposure.

To address this question, I used the transcript data of the collaborative sessions and classified all Lower partners' Lost items according to how they were generated during the collaborative session: N*on-recalled, Partner-recalled, or Self-recalled.* Non-recalled items were those items that were not mentioned during collaboration. Partner-recalled items were those items that were offered by the other member of the dyad during collaboration. Self-recalled items were those items that the participant offered his or her self during the collaboration.

I then calculated each item type as a proportion of initially recalled (Recall 1) items. Overall the proportion of Lost items for these Lower partners who collaborated were 0.15 (SD = 0.11). Breaking these items down into how they appeared in the collaboration: (1) The mean proportion of Lost items that were Non-recalled during collaboration was 0.08 (SD = 0.08); (2) The mean proportion of Lost items that were

Partner-recalled during collaboration was 0.01 (*SD* = 0.03); (3) The mean proportion of Lost items that were Self-recalled during collaboration was 0.07 (*SD* = 0.08). A one-way ANOVA indicated a significant difference between item types, $F(2,57) = 5.28$, $p = .008$, $\eta^2_p = .22$. Follow-up t-tests suggested that Partner-recalled items were less likely to be lost than both Self-recalled items, $t(19) = 2.77$, $p = .012$, $\eta^2_p = 1.27$, and Non-recalled items, $t(19) = 3.66$, $p = .002$, $\eta^2_p = 1.67$ (see Figure 14). This suggested that for Lower partners, being reexposed to their Higher partners' items led to more stable memory for these items than items they retrieved themselves.

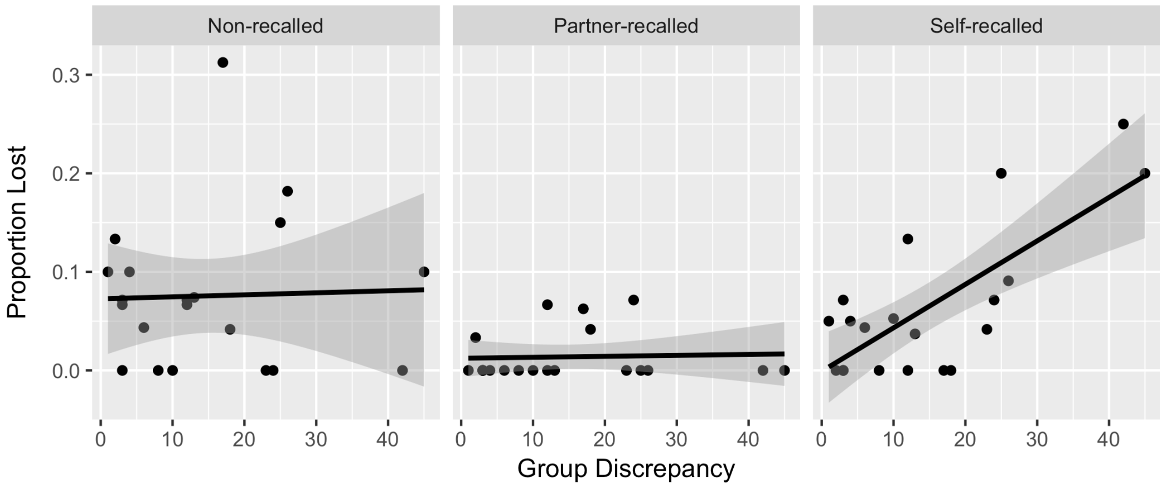

Figure 14. Lower partners' relationships between Group Discrepancy and Lost Items by how it was generated at collaboration.

To test if the relationship between Group Discrepancy and Lost items was contingent on how the items appeared during collaboration, I performed correlational analyses on the discrepancy measure and each of the three types of the Lower partners' Lost item (see Figure 15). Results indicated no significant relationship between Group Discrepancy and Lost items for Non-recalled Lost items, $r(18) = 0.03$, $p = .888$, and Partner-recalled Lost items, $r(18) = 0.05$, $p = .840$. I did, however, find a very large significant relationship between Group Discrepancy and Lower partners' Lost item proportions for Self-recalled Lost items, $r(18) = 0.75$; $p < .001$. This suggested that the items that the Lower partners Lost after working in discrepant groups were items that they themselves had produced during collaboration. In other words, these individuals were recalling items during group recall but forgetting them later.

Taken together, these results suggested that participants who collaborated with partners whose abilities were much higher than their own tended to benefit more from reexposure than from relearning via retrieval. That is, they exhibited a tendency to forget their own items but not their partners'.

Did retrieval strategies relate to post-collaborative benefits? To test whether individuals who reported using individual strategies experienced post-collaborative benefits that were different to those who reported using no strategies, I performed a 2 x 2 ANOVA on the Net change score data with Condition (Nominal vs Collaborative) and Strategy Use (No Strategy vs Strategy) as between-subjects factors (see Table 7). Results indicated a significant effect of Condition, $F(1,76) = 22.86$, $p < .001$, $\eta^2_p = .21$. The main effect of Strategy Use was marginally significant, $F(1,76) = 3.09$, $p = .083$, $\eta^2_p = .04$. The interaction between the two factors was not significant, $F(1,76) = 0.58$, $p = .447$. Although the Strategy Use factor was only marginally significant, examination of the numerical trend with respect to Collaborators' data was enlightening as it suggested that perhaps individuals who reported using no retrieval strategies demonstrated the largest gains and thus benefited the most from collaboration (see Figure 16).

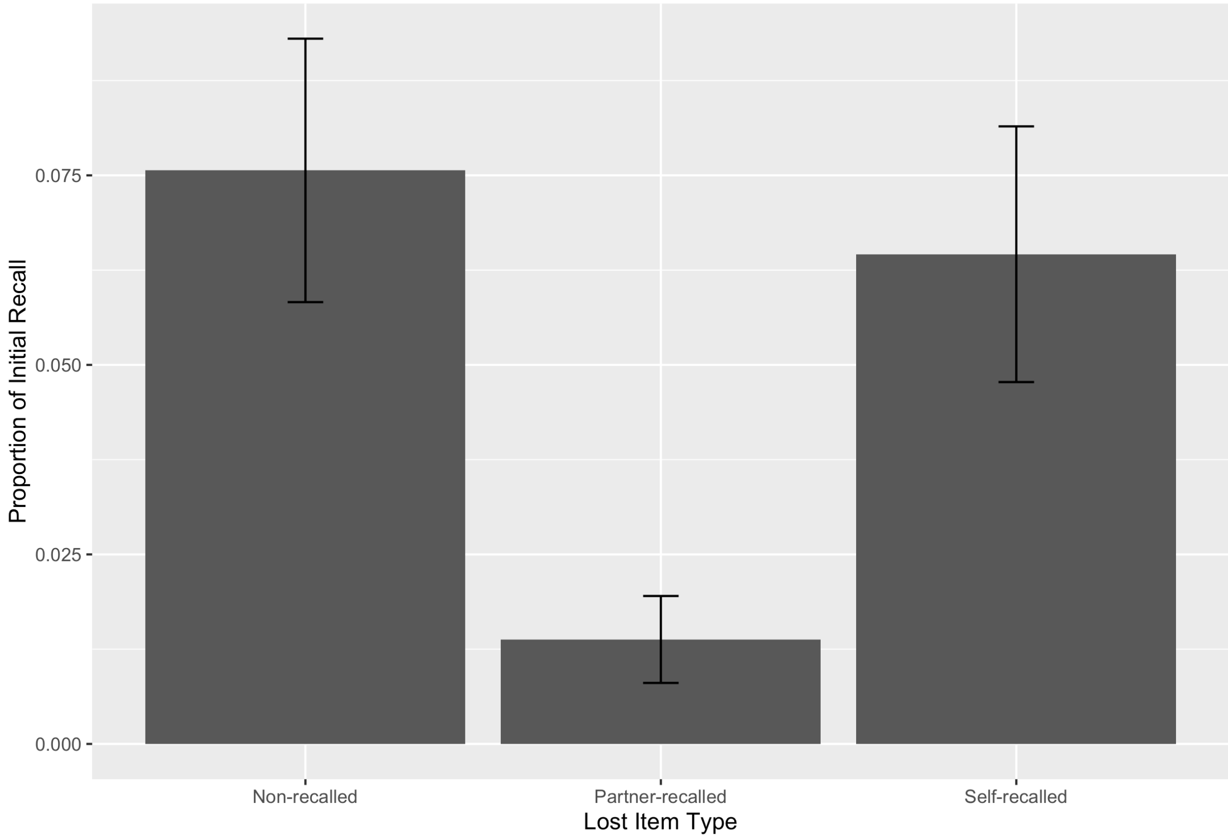

Figure 15. Lower Collaborative partners' Lost item proportions as a function of collaborative generation. Bars represent SEM.

Table 7
Net Change Scores by Condition and Reported Retrieval Strategy Use (SD in parentheses)

	No Strategy	Strategy
Non-collaborators	0.09 (0.19)[a]	0.04 (0.02)[b]
Collaborators	0.35 (0.27)[c]	0.22 (0.22)[d]

Note. No Strategy = participants who reported using no individual retrieval strategy. Strategy = participants who reported using an individual retrieval strategy.
[a]*n = 10*, [b]*n = 30*, [c]*n = 9*, [d]*n = 31*

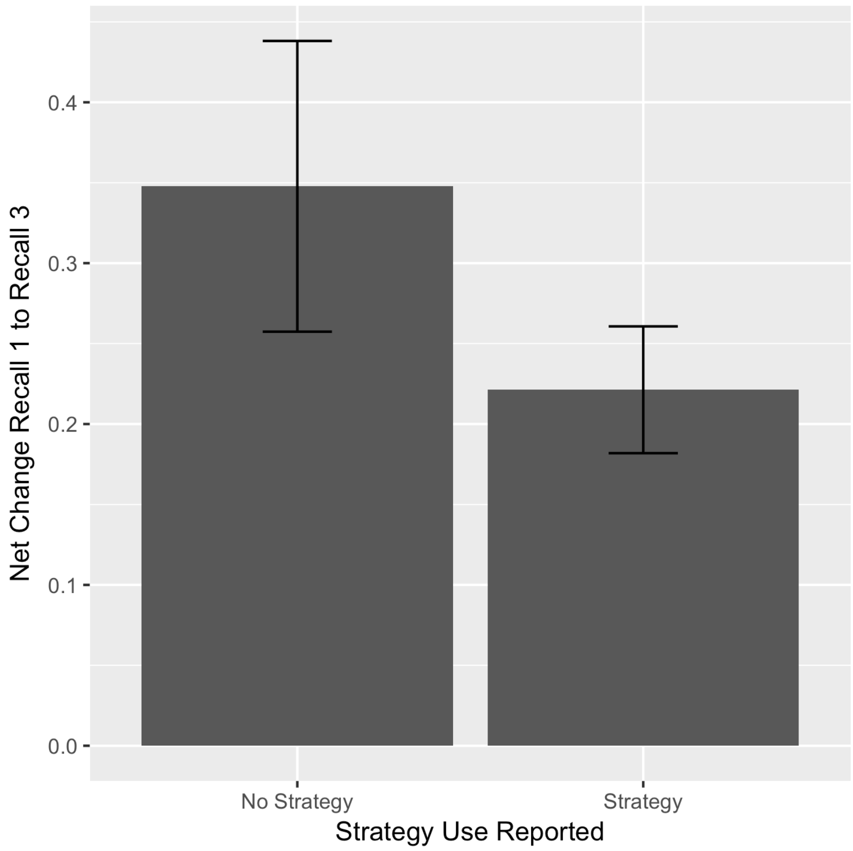

Figure 16. Collaborators' Net change scores as a function of individual retrieval strategy use.

Discussion

My major finding in Experiment 1 was that, although there were overall post-collaborative benefits, the lower ability partners in more discrepant groups did not benefit as much as the higher ability partners. This was because the lower ability partners lost items that they had initially remembered. This pattern was driven by the items that the lower partner themselves had recalled during collaboration, as the relationship did not exist for other item types. As I found no significant difference between either gains or individual contributions to Recall 2 for relatively higher versus lower ability partners, I assumed that the positive effects of collaboration (i.e., reexposure and relearning via retrieval) were similar for both low and high WMC individuals. That is, lower ability partners had just as much opportunity for both reexposure and relearning via retrieval as higher ability partners. However, the finding that lower ability partners lost their own items, but not the items offered by the higher ability partner during collaboration, suggests that relearning via retrieval had less influence on post-collaborative benefits than re-exposure. That is, re-learning via retrieval should enhance recall for one's own items – it is an intra-individual process. Conversely re-exposure should enhance recall for a partner's items – it is an inter-individual process. Overall, the finding that losses correlated with the discrepancy measure suggests that the costs of collaboration were more pronounced for lower ability partners than higher ability partners.

Which negative mechanism was responsible for my findings? Retrieval disruption, blocking, or inhibition were possible. As previous research would suggest a rebound from retrieval disruption on subsequent post-collaborative recalls (Finlay et al., 2000; Weldon & Bellinger, 1997), it is likely that either blocking or inhibition drove the effect that I observed. Because individuals lower in executive control are generally poorer inhibitors (Cantor & Engle, 1993), I had predicted that inhibition would have a greater impact on the higher ability individuals. Thus, blocking could be seen as the more likely candidate in the current study. However, previous research has demonstrated a role of inhibition in collaborative recall where collaboratively-induced memory impairments persisted on a subsequent post-collaborative recognition test (Barber et al., 2014). If this were the case in the current study, then it would imply that the lower ability individuals were inadvertently inhibiting their own items in order to attend to the items recalled by their higher ability partner. This would make sense, as one of the hallmarks of executive control is the ability to maintain information while attending to other tasks (Baddeley & Della Sala, 1996; Baddeley et al., 1984; Baddeley & Hitch, 1974; Kane & Engle, 2002). Thus, lower ability participants were unable to retain their own information while attending to the information provided by a partner. In this case, competition from items generated by the higher partner caused lower partners to suppress items that they had recalled and were currently holding in their working memory (Rundus, 1973). Activation of competing items may have then interfered with participants' own items, thereby degrading memory for items that they had recently generated (Barber et al., 2014; Bäuml & Aslan, 2004).

There are several reasons that the loss of previously generated items would occur for lower ability partners collaborating in discrepant groups. One possibility is that lower partners recognized the fact that their partner had superior ability and therefore placed higher priority on partner-generated items than self-generated items. This would suggest a meta-memory phenomenon, which would make for an interesting future study wherein participants were asked to rate their perception of their partner's memory ability relative to their own. In this manner, I could determine whether individuals were sensitive to their own abilities as well as the abilities of their partners.

My repeated measures analysis of Recall 1 to Recall 3 could be interpreted as inconsistent with the results of the correlational analysis. There was no overall effect of Relative Rank when rank was treated as a dichotomous variable, although there was a significant correlational relationship between discrepancy and Net Change. However, this makes sense as the distinction between lower and higher becomes essentially arbitrary as discrepancy decreases. For example, in a dyad where Participant A scored 65 on the OSPAN and Participant B scored 35 the distinction between lower and higher is informative and is commensurate with a discrepancy score of 30. However, in a dyad in which Participant A scored 45 and Participant B scored 44, the distinction between lower and higher is meaningless. The discrepancy measure of 1, however, is still informative as it is indicative of a dyad in which members' OSPAN scores are similar. The correlational analysis captures this range of discrepancies within the various groups. Another approach to investigating how group-level executive control influences collaborative recall might be to compose extreme groups based on individual executive control ability. For example, a dyad composed of high ability individuals would be a high ability, yet low discrepancy group. Likewise, a dyad composed of low ability individuals would be a low ability and low discrepancy group. I could then compare these groups to each other as well as to dyads composed of a high ability individual and a low ability individual or a high discrepancy group. I take this approach in my second experiment, presented in Chapter 3.

Finally, the use of both individual and group retrieval strategies appeared to impact recall at the individual and group level. Individuals who employed retrieval strategies recalled more on their baseline recall than individuals who did not. Further, collaborative groups in which both members agreed that they had employed a strategy were more successful than groups who disagreed. Interestingly, the data was suggestive of the fact that perhaps it was more discrepant groups that were likely to disagree on whether strategy was used. In these instances, it was always the lower ability partner who reported that no strategy was used whereas it was always the higher ability partner who reported that the dyad had used a strategy. This also appeared to carry-over into post-collaborative recall. The data suggested that perhaps participants who did not initially employ individual retrieval strategies received the most post-collaborative benefits. While these particular findings must be interpreted with caution, they nonetheless hint at an interesting possibility: that collaboration could help people who are not predisposed to use adaptive memory strategies.

To my knowledge, this experiment is the first to demonstrate a relationship between post-collaborative recall and the individual executive control differences of collaborative group constituents. Previous research has focused almost exclusively on the positive benefits of reexposure. However, my findings suggest that there are situations in which collaboration may not be as beneficial for long-term retention as generally assumed. Real-word situations abound in which group memory is emphasized and Experiment 1 suggests that Group Composition (i.e., who works with whom) should be considered. If diminishing collaborative returns come at the expense of the loss of previously acquired material, then perhaps the costs outweigh the benefits.

In sum, the results from this experiment represent a starting point for questioning the assumption that collaborative recall influences all individuals in the same way and to the same extent. It suggests that not only does collaboration affect people differently, but that also this effect is contingent upon the characteristics of other people in the group.

Chapter 3

Experiment 2

An Extreme Groups Approach to Investigating the Role of Executive Control in Collaborative Recall

Experiment 2

In Experiment 1, I found evidence to suggest that the composition of collaborative groups with respective to executive control ability modulated collaborative effects. While these effects were not observed during group recall, they were observed in post-collaborative individual recall such that collaboration between individuals with discrepant abilities led to a decrease in the expected post-collaborative benefits for the relatively lower ability partner. This was because, as groups became more discrepant, the lower ability partners lost more items that they had initially remembered. Further, this pattern was driven by items that the lower partner had contributed themselves during the collaboration. Lost items that were generated by the higher ability member of the group were significantly less likely to be forgotten by the lower ability member. I conducted Experiment 2 with the aim of replicating my findings from Experiment 1 using a potentially more powerful design; a design that would be more sensitive to the effects of group-level executive control characteristics.

As in Experiment 1, in Experiment 2 I examined how individual differences in group members' executive control abilities influenced collaborative recall. I also examined how these abilities influenced subsequent, individual memory on post-collaborative recall tests, from both the individual and group perspective. That is, I examined how the composition of groups influenced collaborative and post-collaborative recall. Whereas in Experiment 1 I randomly composed nominal and collaborative groups and measured individual and group-level executive control variables on a continuous scale, in the current experiment I pre-screened individuals before the experimental session and used this data to form stratified dyads. Using this *extreme groups* approach, I aimed to construct a design that would be better able to detect differences related to group-level executive control characteristics. Thus, I constructed three types of dyads, composed of either: 1) Two high ability individuals (*High-High*); 2) Two low ability individuals (*Low-Low*); or 3) One high ability and one low ability individual (*High-Low*). This allowed me to compare the effects of collaborative recall on groups that varied on two factors: Group Ability and Group Discrepancy.

Method

Participants

Two hundred and thirty-six undergraduate students from Macquarie University participated in this study. They were compensated with either course credit or $15. Of these participants, five solved fewer than 85% of the math problems correctly in the OSPAN (see Unsworth et al., 2005), two did not attempt recall, and 85 did not meet the criteria for high or low executive control participants (i.e., scored in the middle tertile) and were dismissed prior to recall. All of these participants were excluded from further analysis resulting in a sample of 144 individuals (116 women, 28 men) with a mean age of 20.59 years (*SD* = 5.81).

Three participants were scheduled for each session. All participants were initially pre-screened for executive control ability (discussed below) and those that fit my criteria (i.e., high or low ability) continued to the next phase of the study. If all three participants were high or low ability, then two participants were randomly assigned to the Collaborative condition and one participant was assigned to the Nominal condition. If only two participants met my criteria, then they were randomly assigned to either the Nominal or the Collaborative condition. If only one participant met my criteria, he or she was defaulted to the Nominal condition.

Similar previous collaborative recall experiments typically contain around 12 (e.g. Congleton & Rajaram, 2014) to 16 (e.g. Pereira-Pasarin & Rajaram, 2011; Barber & Rajaram, 2011) groups in each condition. As pre-selection of suitable participants according to their ability is both time and resource intensive, I selected my sample size (12 dyads in each of the six conditions resulting in 144 participants total) such that it would at least meet the standards of other similar studies, but still be manageable from a practical standpoint.

Design

Experiment 2 was a two-factor design with both Condition (Nominal vs Collaborative) and Group Composition (High-High vs Low-Low vs High-Low) as between-subjects factors. In some analyses Recall Session (Recall 1 vs Recall 3) was considered as a within-subjects factor. There were 12 dyads (24 participants) assigned to each of the six cells.

Materials

Word lists. Study stimuli were identical to Experiment 1 and consisted of 84 words comprised of six categories containing 14 items each. The list were constructed from the Van Overschelde et al. (2004) norms which are an update of the Battig and Montague (1969) norms and were the same words used in Experiment 1 (see Appendix A).

Automated Operation Span. To assess executive control ability, I used an automated version of the Operation Span task. This task was identical to that used in Experiment 1.

Post-experimental questionnaire. The post-experimental questionnaire used in Experiment 2 was identical to that used in Experiment 1.

Procedure

The experiment consisted of seven phases: (1) Pre-screen; (2) Study phase; (3) Distractor task; (4) Recall 1; (5) Recall 2; (6) Recall 3; and (7) Post-experimental questionnaire and debrief. Experimental sessions were always scheduled to include three participants. I did this because I used a tertile split according to normative data (discussed below) and since, theoretically, one-third of my overall sample would be unsuitable, I wished to maximize the probability that I would have at least two participants that met my executive control criteria.

Before the experiment began participants read and signed information and consent forms. Participants in the Nominal condition were told that they were completing a study that investigated how working memory influenced the way in which people remembered both alone and in groups.

Pre-screen of executive control. Upon arrival, participants signed information and consent forms and then were directed to individual testing booths where they completed an automated version of the operation span task (OSPAN; Unsworth et al., 2005). In the booths, participants were isolated such that they could not see the other participants that were also completing the OSPAN. Upon completion, I compared participants' OSPAN scores to established norms (Redick et al., 2012) in order to distinguish them as top 33.3 percentile (Highs), middle 33.3 percentile, or bottom 33.3 percentile (Lows). The specific cut-off scores were 65 and above for Highs and 55 and below for Lows. Only Highs and Lows continued to the next phase of the experiment

Study phase. Upon completion of the OSPAN, participants that met the executive control criteria were directed to a different room and were seated at computers lateral to one another and facing in the same directions. The study phase procedure in Experiment 2 was identical to Experiment 1.

Distractor task. Following presentation of the word list, participants spent 4 minutes attempting to solve a difficult Sudoku puzzle. The distractor task set-up and materials in Experiment 2 were identical to Experiment 1.

Recall 1: Baseline individual recall. Experiment 2 Recall 1 set-up and materials were identical to Experiment 1.

Recall 2: Group recall. Experiment 2 Recall 2 set-up and materials were identical to Experiment 1.

Recall 3: Final individual recall. Experiment 2 Recall 3 set-up and materials were identical to Experiment 1.

Measures and Scoring

Executive Control: Individual Ability. I operationalised executive control as working memory capacity, which I assessed using the OSPAN. Administration of the OSPAN was identical to Experiment 1. Like Experiment 1, I also used the partial scoring method. Higher OSPAN scores indicated higher working memory capacity (i.e., better executive control).

Recall performance and intrusion rates. All recalls were calculated as a proportion of correctly recalled out of total studied words. All intrusion rates were calculated as a proportion of intrusions out of the sum of correctly recalled items and intrusions. These calculations were identical to Experiment 1.

Change scores. Net change scores and Gained and Lost item proportions in Experiment 2 were calculated in an identical manner to Experiment 1.

Results

Statistics

As in Experiment 1, for all inferential statistics using null-hypothesis testing, I used a two-tailed analysis and a traditional significance criterion ($\propto$ = .05). For all follow-up tests (i.e., t-tests), I used a Bonferroni-corrected significance criterion (i.e., $\propto$ = .05/the number of comparisons).

Baseline

Individual executive control. Using the Redick et al. (2012) norms I distinguished individuals as either *Highs* (i.e., high ability; top 33.3 percentile) or *Lows* (i.e., low ability; bottom 33.3 percentile). Mean OSPAN performance for participants in the Nominal condition was 57.09 (*SD* = 14.45) with scores ranging from 15 to 75. Mean OSPAN performance for participants in the Collaborative condition was 57.97 (*SD* = 13.56) with scores ranging from 20 to 77. An independent samples t-test of individual OSPAN scores indicated no difference between conditions, $t(142) = 0.37$, $p = .709$, suggesting that there were no pre-existing differences in executive control between future Non-collaborators and future Collaborators. Overall, my data were consistent with published norms (Redick et al., 2012) and my previous experiment (see Table 8).

Table 8
Comparison of OSPAN scores by percentile

	Percentile						
	5	25	33.3	50	66.6	75	95
Experiment 2 (N = 144)	33	49	51	60	67	69	75
Experiment 1 (N = 80)	32	51	55	62	65	67	72
Redick et al. norms (N = 6,236)	29	51	55	61	65	67	73

Note. OSPAN = Operation Span scores using partial scoring method. Redick et al. norms represent normative data from Redick et al. (2012).

Whereas in Experiment 1 participants completed the OSPAN at the beginning of the experimental session, in Experiment 2 participants completed it at the end. To determine whether there were any potential order effects due to cognitive depletion, I conducted an independent samples t-test between Experiment 1 participants' OSPAN scores and Experiment 2 participants' OSPAN scores. Results indicated no significant difference between the two groups, $t(222) = 0.05$, $p = .962$, which suggested that administering the OSPAN at the beginning of the experimental session did not result in performance different from administering the OSPAN at the end of the session.

Recall 1: Initial individual recall and intrusions. I used initial individual recall performance as a baseline against which I compared subsequent group and individual recall performance. See Table 9 for Recall 1 descriptive statistics for each cell of the experimental design.

To test whether there were pre-existing differences in Recall 1 performance between participants of different ability constellations assigned to the two conditions, I conducted a 2 x 3 ANOVA with Condition (Nominal vs Collaborative) and Composition (High-High vs Low-Low vs High-Low) as between-subjects factors. Results indicated no significant main effects of either the Condition factor, $F(1, 138) = 0.27$, $p = .602$, or the Composition factor, $F(2, 138) = 0.78$, $p = .462$. The interaction between the two factors was also non-significant, $F(2,138) = 0.98$, $p = .378$. Thus, there were no pre-existing Recall 1 differences which suggested that initial individual recall was statistically equivalent for all groups.

I also calculated Recall 1 intrusion rates for each level of my design (See Table 10). A 2 x 3 ANOVA revealed a significant main effect of Condition, $F(1, 138) = 6.78$, $p = .010$, $\eta^2_p = .05$. The main effect of Composition was not significant, $F(2, 138) = 2.65$, $p = .075$, nor was the interaction of the factors, $F(2, 138) = 0.30$, $p = .742$.

Follow-up t-tests indicated that the Collaborative condition had higher intrusion rates (*M* = 0.08, *SD* = 0.10) compared to the Nominal condition (*M* = 0.05, *SD* = 0.07), *t*(142) = 2.59, *p* = .011, *d* = .43. Thus, prior to collaboration the participants assigned to the two conditions exhibited differential error rates. This finding was not expected since participants were randomly assigned to conditions, and likely represented random error. Nonetheless, I had to account for it and I address this issue later.

I also tested whether participants who reported using specific individual retrieval strategies on their post-experimental questionnaire differed in their baseline recall performance. Of the 144 participants in my sample, 82 reported using an individual strategy to guide their recall and 62 reported using no strategy. Mean recall for those who used a strategy was 0.25 (*SD* = 0.08). Mean recall for those who did not use a strategy was 0.20 (*SD* = 0.08). There was a significant difference between the two groups, *t*(142) = 3.32, *p* = .001, η^2_p = .62, which suggested that using a strategy helped participants to remember more words on baseline individual recall.

Recall 1 relationship with executive control. To test whether Individual Ability was related to Recall 1 performance I conducted an independent sample t-test comparing Highs and Lows on their Recall 1 performance. Mean proportion of correctly recalled items for High ability individuals was 0.23 (*SD* = 0.08) and ranged from 0.10 to 0.43. Mean proportion of correctly recalled items for Low ability individuals was 0.23 (*SD* = 0.08) and ranged from 0.06 to 0.39. An independent samples t-test indicated no difference between the Recall 1 performance of High versus Low ability individuals, *t*(142) = 0.16, *p* = .874. Thus, executive control ability was not related to initial individual recall.

Table 9

Recall 1 Descriptive Statistics for each Condition and Composition Type

		Mean Recall	Range	SD
Nominal				
	High-High	0.24	0.11 - 0.43	0.09
	Low-Low	0.23	0.08 - 0.37	0.09
	High-Low	0.23	0.07- 0.36	0.08
Collaborative				
	High-High	0.21	0.10 - 0.38	0.07
	Low-Low	0.21	0.06 - 0.39	0.09
	High-Low	0.25	0.10 - 0.38	0.08

Note. Performance was calculated as a proportion of correctly recalled items out of total items studied. Data represent individual performance.

Table 10

Recall 1 Intrusion Rate Descriptive Statistics for Each Condition and Composition Type

		Mean Intrusions	Range	SD
Nominal				
	High-High	0.04	0.00 - 0.23	0.06
	Low-Low	0.06	0.00 - 0.36	0.10
	High-Low	0.04	0.00 - 0.19	0.05
Collaborative				
	High-High	0.08	0.00 - 0.23	0.08
	Low-Low	0.11	0.00 - 0.44	0.14
	High-Low	0.06	0.00 - 0.22	0.07

Note. Intrusion rates were calculated as a proportion of the number of intrusions out of the total number of recalled items.

I also tested whether Individual Ability influenced intrusion rates in Recall 1. The mean intrusion rate for High ability individuals was 0.05 (*SD* = 0.07) with scores ranging from 0.00 to 0.23. Mean intrusion rate for Low ability individuals was 0.08 (*SD* = 0.11) with scores ranging from 0.00 to 0.44. There was no significant difference between the two groups, $t(142) = 1.60$, $p = .111$. Thus, Individual Ability was not related to baseline intrusion rates.

Finally, I tested whether individuals who, on the post-experimental questionnaire, reported using specific strategies to guide their individual retrieval differed in their executive control ability. Mean OSPAN scores for those who reported using a strategy was 59.13 (*SD* = 12.80). Mean OSPAN scores for those who reported they did not use a strategy was 55.42 (*SD* = 15.2). The difference between the two groups was not significant, $t(142) = 1.58$, $p = .115$, which suggested that people who employed specific retrieval strategies did not differ in executive control ability from individuals who did not employ a strategy.

Collaboration

One of my research questions was whether the executive control characteristics of groups in terms of ability and discrepancy would influence the outcome of collaboration. To investigate this question, I analysed Recall 2 data according to Group Composition (i.e., High-High versus Low-Low versus High-Low). See Table 11 for descriptive statistics of group-level executive control characteristics of each composition.

To test whether there were differences in Group Ability between dyads assigned to each of the conditions, I performed a 2 x 3 ANOVA on the Group Ability measures with Condition (Nominal vs Collaborative) and Composition (High-High vs Low-Low vs

Table 11

Group-level Executive Control Measures Descriptive Statistics

		Group Ability	Group Discrepancy
Nominal			
	High-High	69.54(2.25)	3.58(1.78)
	Low-Low	45.58(8.16)	7.33(6.36)
	High-Low	56.17(6.08)	26.33(11.44)
Collaborative			
	High-High	70.25(2.73)	3.00(1.71)
	Low-Low	44.33(7.45)	8.67(9.15)
	High-Low	59.33(2.96)	19.33(4.94)

Note. Group Ability was represented by calculating the average OSPAN score of each dyad. Group Discrepancy was represented by calculating the difference between the dyad members.

High-Low) as between-subjects factors. Results indicated a main effect of Composition, $F(2,66) = 124.13$, $p < .001$, $\eta^2_p = .61$. The main effect of Condition and the interaction between the two factors were not significant, both *F*s < .98, both *p*s > .381. Follow-up pairwise comparisons indicated that all three Compositions were significantly different from one another, all *t*s > 6.86, all *p*s < .001. Thus, as expected, the High-High groups had higher average ability than the High-Low groups, who had higher average ability than the Low-Low groups.

To test whether there were differences in Group Discrepancy between dyads assigned to each of the conditions, I performed a similar 2 x 3 ANOVA on the Group Discrepancy measures. Results indicated a main effect of Composition, $F(2,66) = 52.48$, $p < .001$, $\eta^2_p = .79$. The main effect of Condition and the interaction between the two factors were not significant, both *F*s < 2.41, both *p*s > .099. Follow-up pairwise comparisons indicated that all three Compositions were significantly different from one another, all *t*s > 2.91, all *p*s < .006. High-Low groups had the highest discrepancy as expected, but Low-Low groups also had significantly higher discrepancy than High-High groups.

Overall, these results suggested that preselecting participants based on executive control ability resulted in dyad compositions that were significantly different from each other, both in terms of overall ability and the differences in abilities between dyad members. These differences, however, did not vary between group recall condition.

Recall 2: Group recall. To test whether the executive control characteristics of groups influenced group recall, I performed a 2 x 3 ANOVA (Condition x Composition) on Recall 2 data (see Figure 17). Results indicated a significant main effect of the Condition factor, $F(1,66) = 24.34$, $p < .001$, $\eta^2_p = .27$, but not of the Composition factor, $F(2,66) = 0.28$, $p = .758$. The interaction between Condition and Composition was not significant, $F(2,66) = 0.27$, $p = .270$. This suggested that only Condition influenced group recall performance. Mean Recall 2 performance in the Nominal condition was 0.39 (*SD* = 0.09) with scores ranging from 0.23 to 0.63. Mean Recall 2 performance in the Collaborative condition was 0.29 (*SD* = 0.08) with scores ranging from 0.15 to 0.49. Thus, as expected I observed an overall collaborative inhibition effect where Collaborative groups recalled less than Nominal groups. However, this occurred regardless of the executive control composition of the groups.

I also tested whether retrieval strategies affected Collaborative group performance. To do this, I analysed the post-experimental questionnaire data of participants in Collaborative groups and distinguished groups according to three categories: 1) No Strategy (i.e., those dyads in which neither member reported that the group used a strategy; 2) Disagree Strategy (i.e., those dyads in which one member reported that the group used a strategy and one member reported that the

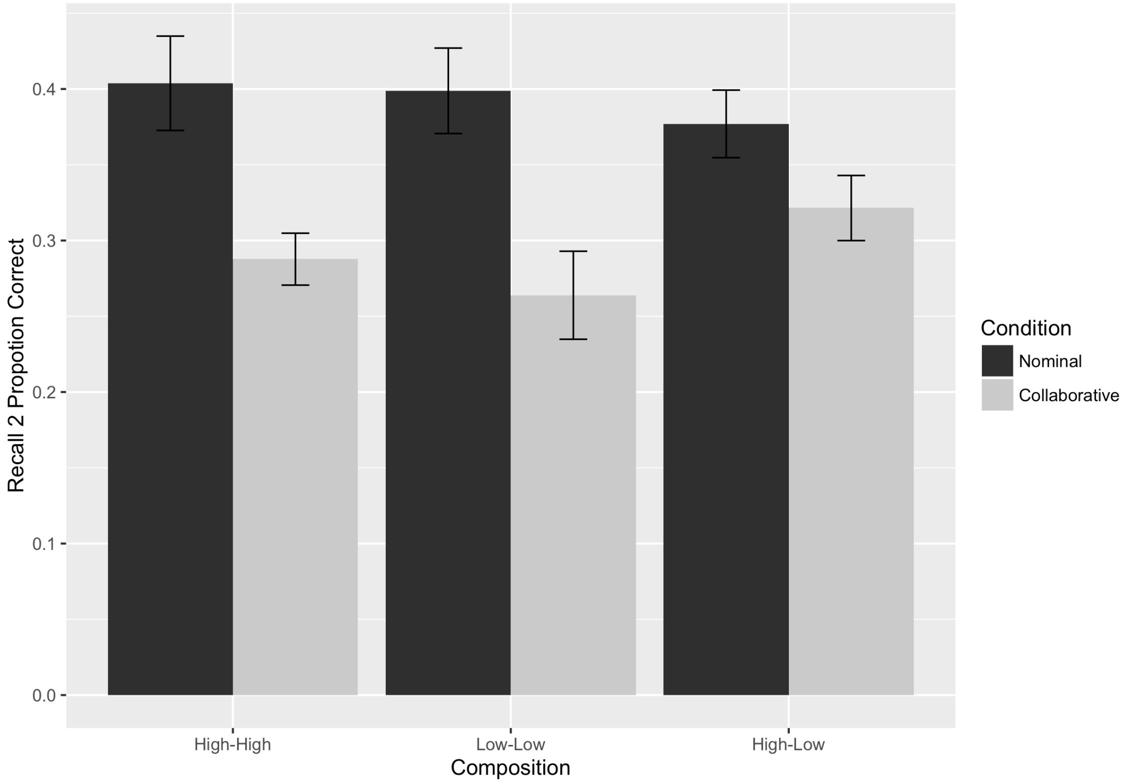

Figure 17. Recall 2 performance by condition and Group Composition. Errors bars represent SEM.

group did not use a strategy); and 3) Agree Strategy (i.e., those dyads in which both members reported that the group used a strategy). I then performed a one-way ANOVA with Strategy Use (No Strategy vs Disagree Strategy vs Agree Strategy) on Collaborative groups' Recall 2 data. Results indicated no significant effect of Strategy Use, $F(2,33) = 1.36$, $p = .271$. Thus, unlike Experiment 1 the use of strategies by Collaborative groups did not appear to influence their recall success.

Although strategy use did not influence collaborative recall, I also tested whether the different Group Compositions reported differing levels of strategy use. To do this, I calculated frequencies of each of the three types of group strategy use as described above (see Table 12). I then performed a chi-square test of independence to examine the relationship between Group Composition and strategy use. The relationship was not significant, $X^2 (4, N = 72) = 1.65$, $p = .799$. Thus, reports of strategy use did not differ between the types of collaborative dyads.

Table 12

Frequencies of Reported Group Strategy Use

Composition	No Strategy	Disagree Strategy	Agree Strategy
High-High	5	3	4
Low-Low	8	2	2
High-Low	6	3	3

Note. No Strategy = No dyad member reported strategy use. Disagree Strategy = Dyad members disagreed on strategy use. Agree Strategy = Both dyad members agreed a strategy was used.

Recall 2: Group intrusions. To test whether intrusion rates were influenced by group-level executive control characteristics I conducted a 2 x 3 ANOVA on Recall 2 intrusion rates. Results indicated a significant main effect of Condition, $F(1,66) = 12.34$, $p < .001$, $\eta^2_p = .16$, but not of Composition, $F(2,66) = 1.70$, $p = .191$. The interaction of Condition and Composition was also not significant, $F(1,66) = 0.59$, p .558 (See Figure 18). This suggested that only Condition influenced intrusion rates. Mean intrusion rate for the Nominal condition was 0.06 ($SD = 0.04$) with scores ranging from 0.00 to 0.17. Mean intrusion rate for the Collaborative condition was 0.03 ($SD = 0.03$) with scores ranging from 0.00 to 0.13. Thus, although intrusion rates were low, collaboration boosted accuracy by lowering rates of unstudied item intrusion.

Post-Collaboration

As in Experiment 1, I expected that the Group Composition in terms of ability and discrepancy would influence collaborative recall. Although I found no evidence that this was the case, I examined whether Individual Ability and Group Composition influenced post-collaborative recall.

Recall 3: Final individual recall and intrusions. I collected recall and intrusion data for Non-collaborators and Collaborators at Recall 3. Non-collaborators

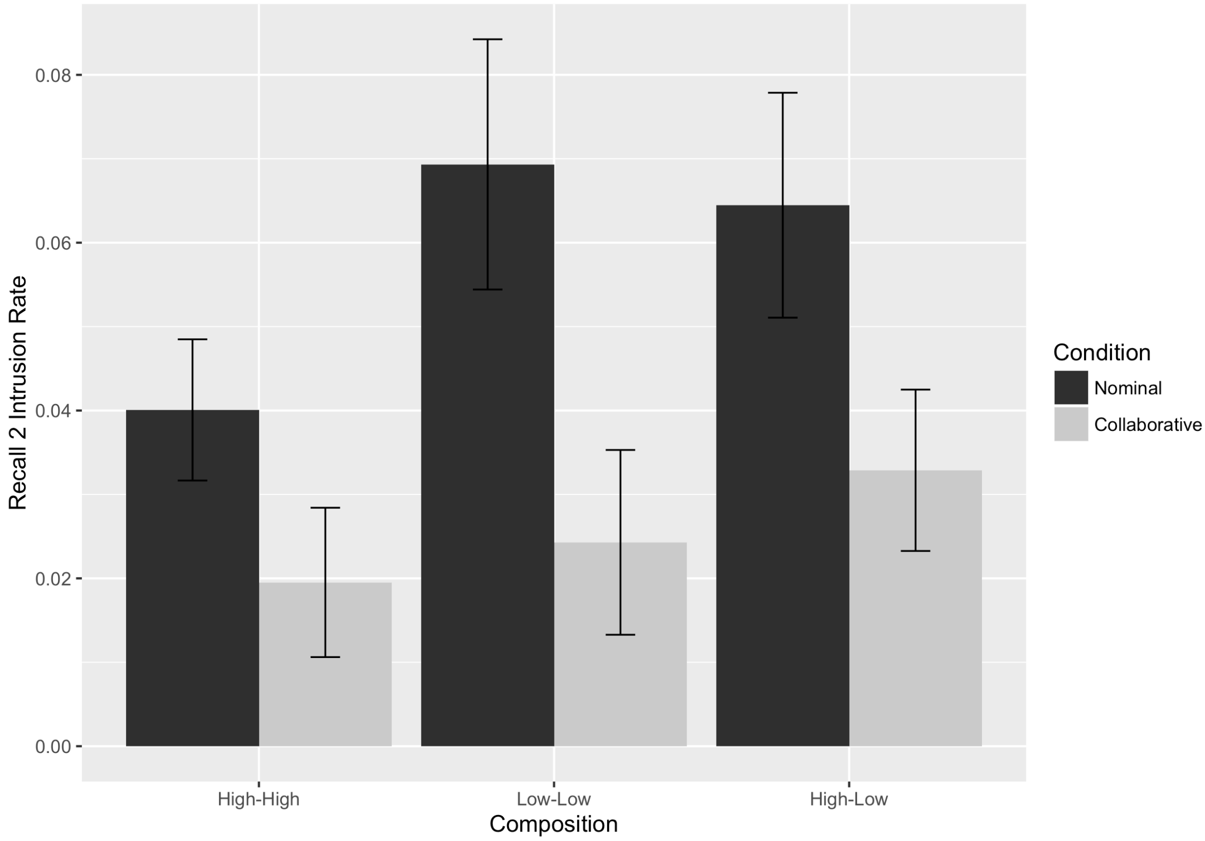

Figure 18. Recall 2 intrusion rate by condition and Group Composition. Errors bars represent SEM.

were former members of Nominal groups; Collaborators were former members of Collaborative groups. See Table 13 for Recall 3 descriptive statistics. See Table 14 for Recall 3 intrusion rate descriptive statistics.

To test whether Individual Ability was related to post-collaborative performance, I conducted correlational analyses between individual OSPAN scores and Recall 3 scores for both Non-collaborators and Collaborators. The relationship was not significant for either Non-collaborators, $r(70) = .16$, $p = .168$, or Collaborators, $r(70) = .17$, $p = .149$. This suggested that Individual Ability did not relate to post-collaborative benefits.

To test whether Individual Ability was related to post-collaborative intrusions, I conducted correlational analyses between individual OSPAN scores and Recall 3 intrusion rates. The relationship was not significant for either Non-collaborators, $r(70)$

= -.14, p = .254, or Collaborators, $r(70)$ = .18, p = .125. This suggested that Individual Ability did not influence post-collaborative intrusion rates.

Table 13

Recall 3 Descriptive Statistics for each Condition and Composition Type

		Mean Recall	Range	SD
Non-collaborators				
	High-High	0.25	0.11 - 0.49	0.10
	Low-Low	0.24	0.06 - 0.40	0.10
	High-Low	0.23	0.07 - 0.40	0.08
Collaborators				
	High-High	0.27	0.14 - 0.40	0.07
	Low-Low	0.26	0.11 - 0.43	0.08
	High-Low	0.31	0.19 - 0.40	0.06

Note. Performance was calculated as a proportion of correctly recalled items out of total items studied. Data was calculated at the individual level. Recall 2 was calculated at the group level.

Table 14

Recall 3 Intrusion Rate Descriptive Statistics for Each Condition and Composition Type

		Mean Intrusions	Range	SD
Non-collaborators				
	High-High	0.06	0.00 - 0.50	0.10
	Low-Low	0.10	0.00 - 0.55	0.15
	High-Low	0.05	0.00 - 0.23	0.06
Collaborators				
	High-High	0.04	0.00 - 0.30	0.07
	Low-Low	0.07	0.00 - 0.25	0.08
	High-Low	0.05	0.00 - 0.15	0.05

Note. Intrusion rates were calculated as a proportion of the number of intrusions out of the total number of recalled items.

Recall 1 vs Recall 3. To test whether the Group Composition in terms of ability and discrepancy influenced post-collaborative benefits, I performed a 2 x 3 x 2 mixed model ANOVA with Condition (Nominal vs Collaborative) and Composition

(High-High vs Low-Low vs High-Low) as between-subjects factors and Recall Session (Recall 1 vs Recall 3) as a within-subjects factor. I observed a main effect of Recall Session, $F(1,138) = 109.68$, $p < .001$, $\eta^2_p = .60$, such that, overall, participants' recall improved from Recall 1 ($M = 0.23$, $SD = 0.09$) to Recall 3 ($M = 0.26$, $SD = 0.09$), $t(143) = 8.85$, $p < .001$, $d = .33$. The interaction between Recall Session and Condition was also significant, $F(1,138) = 55.03$, $p < .001$, $\eta^2_p = .43$ (see Figure 19). Follow-up paired-samples t-tests indicated that both Non-collaborators, $t(71) = 2.53$, $p = .014$, $d = .60$, and Collaborators, $t(71) = 11.03$, $p < .001$, $d = 2.62$, improved from Recall 1 to Recall 3. To test whether Non-collaborators and Collaborators differed in Recall 3 performance, I performed another follow-up t-test which suggested that Collaborators performed better on Recall 3 ($M = 0.28$, $SD = 0.08$) than Non-collaborators ($M = 0.24$, $SD = 0.09$), $t(142) = 2.67$, $p = .008$, $d = .47$. The interaction between Recall Session and Composition was not significant, $F(2,138) = 1.68$, $p = .146$, and neither was the interaction between all three factors, $F(2,138) = 1.68$, $p = .190$. Overall, these results suggested that all participants benefited from repeated recall attempts, but, considering that there were no baseline recall differences between Non-collaborators and Collaborators, participants who collaborated experienced more benefits than participants who did not collaborate. That is, I found post-collaborative benefits. These benefits, however, were not contingent upon the Group Ability or Group Discrepancy.

I also tested whether Group Composition influenced post-collaborative intrusion rates. I performed a 2 x 3 x 2 mixed model ANOVA on the intrusion rate data with Condition (Nominal vs Collaborative) and Composition (High-High vs Low-Low vs High-Low) as between-subjects factors and Recall Session (Recall 1 vs Recall 3) as a within-subjects factor. Neither the main effects of Condition, $F(1,138) = 0.46$, $p = .500$, nor Recall Session, $F(1,138) = 0.26$, $p = .610$, were significant. The main effect of Composition was marginally significant, $F(2,138) = 2.74$, $p = .068$, $\eta^2_p = .04$. The interaction of Condition and Recall Session was significant, $F(1,138) = 27.04$, $p < .001$, $\eta^2_p = .16$, but the interaction of Composition and Recall Session was not, $F(2,138) = 0.37$, $p = .693$. The interaction between the three factors was also non-significant, $F(2,138) = 1.91$, $p = .152$. Follow-up paired t-tests revealed that whereas the former Collaborators' intrusions significantly decreased from Recall 1 ($M = 0.08$) to Recall 3 ($M = 0.05$), $t(71) = 3.92$, $p < .001$, $d = .93$, the former Non-collaborators' intrusions significantly increased from Recall 1 ($M = 0.05$) to Recall 3 ($M = 0.07$), $t(71) = 3.40$, $p = .001$, $d = .81$. These results indicated a benefit of collaboration: whereas Non-collaborators became more inaccurate over recall sessions, Collaborators became more accurate. However, this benefit did not depend on Group Composition.

Change scores: Source of post-collaborative benefits. To determine the source of post-collaborative benefits, I calculated Gained and Lost scores for each cell in my design. These included Gained item proportions, Lost item proportions, and Net change (see Table 15).

Table 15
Mean Gained and Lost Items from Recall 1 to Recall 3 (SD in Parentheses)

		Gained	Lost	Net
Non-collaborators				
	High-High	0.19 (0.18)	0.11 (0.08)	0.08 (0.20)
	Low-Low	0.14 (0.11)	0.12 (0.10)	0.02 (0.15)
	High-Low	0.13 (0.10)	0.12 (0.10)	0.01 (0.10)
Collaborators				
	High-High	0.53 (0.36)	0.15 (0.10)	0.37 (0.38)
	Low-Low	0.46 (0.36)	0.19 (0.13)	0.27 (0.35)
	High-Low	0.46 (0.23)	0.13 (0.08)	0.33 (0.24)

Note. Net = Gained – Lost.

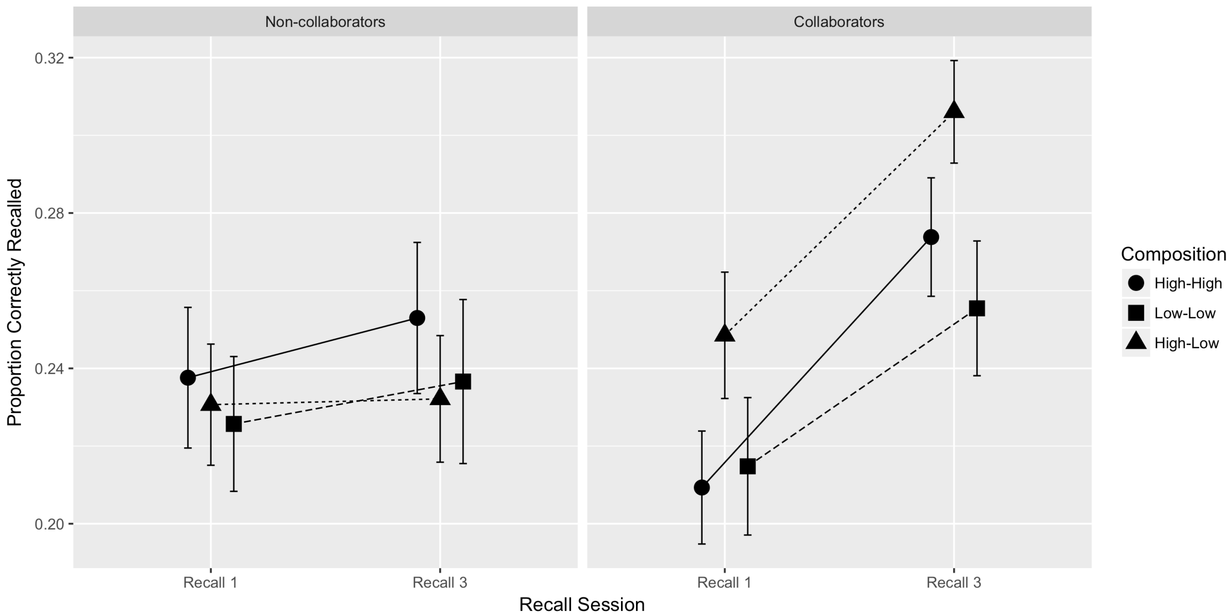

Figure 19. Individual mean recall performance from Recall 1 to Recall 3 by Condition and Composition. Error bars represent SEM.

Did Group Composition relate to items Gained and lost? To test whether the Group Composition in terms of ability and discrepancy influenced post-collaborative benefits, I performed separate 2 x 3 ANOVAs on Gained and Lost items with Condition (Non-collaborators vs Collaborators) and Composition (High-High vs Low-Low vs High-Low) as between-subjects factors. Results for Gained items indicated a significant main effect of Condition, $F(1,138) = 60.67$, $p < .001$, $\eta^2_p = .31$, but not of Composition, $F(2,138) = 1.24$, $p = .292$. The interaction between the two factors was not significant, $F(2,138) = 0.06$, $p = .947$. This suggested that, overall, Collaborators gained more items ($M = 0.47$, $SD = 0.32$) than Non-collaborators ($M = 0.15$, $SD = 0.14$), suggesting a benefit of collaboration.

Analysis of Lost items revealed the same pattern, with a significant main effect of Condition, $F(1,138) = 7.44$, $p = .007$, $\eta^2_p = .05$, but not Composition, $F(2,138) = 1.05$ $p = .354$. The interaction also was non-significant, $F(2,138) = 1.04$, $p = .355$. Overall, Collaborators lost more items ($M = 0.16$, $SD = 0.11$) than did Non-collaborators ($M = 0.12$, $SD = 0.09$), $t(142) = 2.73$, $p = .007$.

Given the net increase in recall for Collaborators, these results suggested that collaboration benefited individual recall by producing levels of re-exposure and relearning via retrieval that were sufficient for participants to gain enough new items to offset the items they forgot. Thus, these results support the findings in the collaborative recall literature where individuals benefit individuals once the detrimental effects of retrieval disruption and/or retrieval blocking have been removed. However, the ongoing loss of items suggests that the costs of collaboration continued on to post-collaborative recall, consistent with inhibition rather than retrieval disruption or blocking. These results illustrate how collaboration can result in retrieval that is more variable compared to individual remembering, and that analysing only Net change can mask substantial gains and losses in items. Importantly, these post-collaborative effects were not due to any detectable influences of Group Composition in terms of ability and discrepancy.

Did Individual Ability relate to items Gained and Lost? To test whether Individual Ability related to collaborative gains or losses, I obtained Pearson bivariate correlations between individual OSPAN scores and Gained and Lost scores for both Non-collaborators and Collaborators (see Table 16). None of the relationships were significant, all *p*s > .161. This suggested that, like overall post-collaborative benefits, the gains and losses from Recall 1 to Recall 3 were not due to any relationship with Individual Ability.

Table 16
Correlations Between Individual Ability and Change Scores

	Change Score	
Condition	Gained	Lost
Non-collaborators	.12	.04
Collaborators	-.10	-.17

Note. Values represent Pearson coefficients.
All *p*s > .05

Lower ability partners and Lost items. In the previous analysis, I found no evidence that Group Composition influenced Gained or Lost items. However, the fact that I found a relationship between Discrepancy and Lost items in Experiment 1 provided motivation to test whether a similar pattern was present in Experiment 2. Since in Experiment 1 this was true only for the relatively Lower ability partners, I performed an independent samples t-test on these individuals' Lost item proportions. Specifically, I tested whether there was a difference in Lost items between Lower ability partners who had collaborated in Discrepant groups (i.e., in the High-Low groups) versus Lower ability partners who had collaborated in Non-discrepant groups (i.e., in the Low-Low groups). Results indicated no difference between the two types of Collaborators, $t(34) = 0.72$, $p = .477$, with Discrepant Lower ability Collaborators losing a similar proportion of item ($M = 0.15$, $SD = 0.09$) as Non-discrepant Lower ability Collaborators ($M = 0.18$, $SD = 0.13$). This suggested that, unlike Experiment 1, Group Discrepancy did not play a role in the rate in which Lower ability Collaborators lost items from Recall 1 to Recall 3.

Overall, the results of the change score analyses provided no evidence that Group Composition in terms of ability and discrepancy influenced post-collaborative recall. This was unexpected, as I had observed a relationship between Group Discrepancy and Lost items in Experiment 1. However, this relationship was not replicated in Experiment 2 using an extreme groups approach. In sum, individuals who collaborated experienced overall post-collaborative benefits, and they both gained more items and lost more items compared to individuals who did not collaborate (See Figure 20). However, the benefits of reexposure outweighed the persistent costs of collaboration and provided Collaborators with superior individual recall compared to their non-collaborating counterparts. Further, these results demonstrated that collaboration can influence the content of memory as Collaborators showed less stable memory than Non-collaborators. Their memory was malleable and, due to the concurrent loss of originally remembered information and acquisition of previously unremembered information, changed over the course of the three recall sessions.

Did retrieval strategies relate to post-collaborative benefits? To test whether individuals who reported using individual strategies experienced differential post-collaborative benefits compared to those who did not report using strategies, I performed a 2 x 2 ANOVA on the Net change score data with Condition (Nominal vs Collaborative) and Strategy Use (No Strategy vs Strategy) as between-subjects factors. Results indicated a significant effect of Condition, $F(1,140) = 41.48$, $p < .001$, $\eta^2_p = .24$. The main effect of Strategy Use was not significant, $F(1,140) = 0.26$, $p = .614$, nor was the interaction between the two factors, $F(1,140) = 2.46$, $p = .119$. Thus, the use of individual retrieval strategies did not appear to provide any extra benefit to Collaborators' final, individual recall.

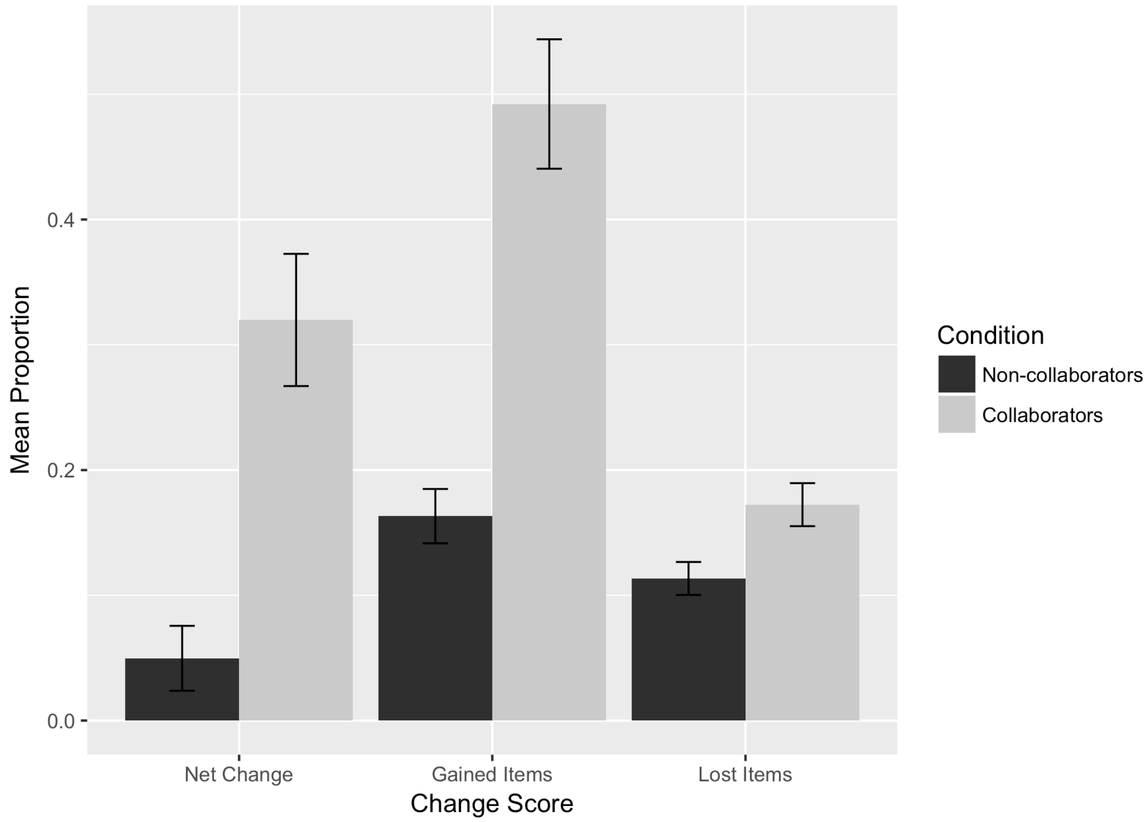

Figure 20. Mean Net Change, Gained Item, and Lost Item proportions for Non-collaborators and Collaborators. Error bars represent SEM.

Discussion

My major findings in Experiment 2 were that collaborative dyads recalled less than nominal dyads during collaborative recall. This was expected, as this is the standard collaborative inhibition effect widely observed throughout the collaborative recall literature (Barber et al., 2010; Basden et al., 1997; Basden et al., 2000; Blumen & Rajaram, 2008; Marion & Thorley, 2016; Rajaram, 2011; Rajaram & Pereira-Pasarin, 2010). I also observed another common effect in which individuals who recalled in collaborative dyads, relative to individuals who had recalled in nominal dyads, exhibited superior post-collaborative individual recall (Basden et al., 2000; Blumen & Rajaram, 2008, 2009; Marion & Thorley, 2016; Rajaram, 2011; Rajaram & Pereira-Pasarin, 2010). Collaboration reduced inaccurate individual recall as evidenced by a decrease in individuals' intrusion rates from Recall 1 to Recall 3

following collaboration. This is even more powerful when considering that future collaborators started with higher levels of baseline intrusions compared to future non-collaborators. Further, this decrease improved
accurate recall over levels that would have been expected had the collaborators been working independently. This was demonstrated in the cross-over interaction in which intrusion rates actually increased across the individual recall sessions for those who had not collaborated. This suggested that without collaborative error-pruning*,* individuals' intrusion rates would tend to worsen rather than improve.

With regards to my key research questions, I found no evidence that the composition of a group, with respect to executive control ability or discrepancy, influenced group recall. Nor did I find any evidence that Group Composition influenced the carry-over effect of collaboration on subsequent, individual recall. This was surprising as prior research suggested that executive control was likely to influence remembering (Barber & Rajaram, 2011a; Cokely et al., 2006; McCabe et al., 2010; Park et al., 2002; Park et al., 1996) and this should be borne out in both group recall and later individual recall. However, Experiment 1 and Experiment 2 are the first studies to directly examine the role of group-level executive control characteristics in collaborative recall and the evidence thus far is certainly not definitive. Although in Experiment 1 I found no support for a relationship between group-level executive control measures (i.e., Group Ability and Group Discrepancy) and group recall, I did find evidence that one of these measures influenced post-collaborative recall, whereby individuals who collaborated in discrepant groups, relative to those who collaborated in non-discrepant groups, experienced fewer post-collaborative benefits. This was because these individuals lost more items than their non-discrepant collaborating counterparts. This finding was not observed in Experiment 2.

In Experiment 2 I employed an extreme groups approach with the aim of increasing power to detect difference due to group-level executive control characteristics. While an extreme groups approach can increase power to detect effects (Preacher, Rucker, MacCallum, & Nicewander, 2005), the use of such techniques does not come without pitfalls. For example, by using categorical grouping (i.e., *low* vs *high*), I dichotomised data and lost potentially critical information (Cohen, Cohen, West, & Aiken, 2013) regarding individual differences with respect to executive control ability. This approach can mask individual differences as it essentially reduces them to binary indicators (i.e., high versus low, rather than a continuous measure). Thus, effects that I observed in Experiment 1 may have been masked in Experiment 2 by only sampling individuals from extreme ends of the ability spectrum. This could have significantly reduced my power to detect true relationships between variables (Preacher et al., 2005). For this reason, I returned to using a continuous executive control measure in Experiment 3.

One potential limitation in Experiment 1 is that I found no relationship between OSPAN and Recall 1 performance. This is problematic as it could have made this study insensitive to differential effects on collaborative recall as a function of individual differences in executive control. Previous research suggests that individuals with lower executive control ability generally recall fewer correct items than individuals with higher executive control ability (Unsworth, 2007; Unsworth & Engle, 2007). However, I used categorized word lists in Experiment 1 and found no difference between high and low ability recall. My choice of materials, while appropriate for examining a theoretically important mechanism (i.e., retrieval disruption), could have artificially boosted lower executive control individuals' recall. Indeed, research suggests that categorical recall can attenuate executive control-related recall differences as it helps low ability individuals constrain their search through long-term memory (Unsworth et al., 2012). Essentially, my choice of experimental materials may have differentially augmented lower executive control individuals' recall ability relative to higher executive control individuals' and eliminated executive control-related variations in initial individual recall.

Indeed, I found evidence to suggest that participants who used specific retrieval strategies at Recall 1 recalled more than participants who did not use strategies. Since categorised words lend themselves readily to strategy use, then it is possible that strategy use confounded individual ability-related recall differences. To examine this, I tested whether people who used a strategy had higher levels of executive control than people who did not. Previous research has found that individuals higher in executive control are more likely to engage in strategic encoding via inter-item associations (e.g., Cokely et al., 2006). Since I found no difference between strategy users' and non-strategy users' executive control levels, this suggests that participants of varying abilities used the easily identifiable retrieval strategy (i.e., categories) which possibly masked ability-related recall performance.

In Experiment 3, I addressed this issue by using unrelated word lists as study stimuli. Thus, I aimed to better distinguish performance that was contingent on individual differences in ability. I also employed several tests of executive control with which I aimed to derive a metric that was more sensitive to individual ability and could better detect executive control-modulated collaborative effects.

Chapter 4

A Component Score Approach to Investigating the Role of Executive Control in Collaborative Recall

Experiment 3

In Experiment 2, I found the standard collaborative recall effects: Collaborative dyads recalled both fewer correct items and fewer incorrect items than nominal dyads. These individuals also benefited from collaboration by way of superior post-collaborative recall relative to Non-collaborators. Further, they showed a decrease in post-collaborative intrusions relative to Non-collaborators. I did not find any evidence that executive control modulated either group recall or individual recall following collaboration. This was true at both the individual level and the group level. I had decided to use an extreme groups approach in Experiment 2 as a way of increasing power by composing groups with the characteristics in which I was specifically interested. However, as I addressed in the discussion of Experiment 2, this approach may have masked individual-difference-related effects. Considering this fact, combined with the simple logistical challenge of pre-screening individuals and only including two-thirds of those I screened, I decided to return to a continuous variable method of measuring ability.

Another potentially problematic aspect of Experiment 1 and Experiment 2 was that I failed to detect any individual-ability-related differences with respect to baseline recall. Though this fact did not necessarily negate my findings, it did potentially diminish my power to detect differences in individual and group recall. Thus, in Experiment 3 I used a battery of tasks designed to measure executive control via working memory and attention. This battery was originally employed by Hutchison (2007) and aims to measure an underlying latent variable common to performance in all three tasks. In the Hutchison (2007) study, the latent construct was termed *attentional control* and stems from the Kane and Engle (2002) construct, *executive* attention. For the purposes of this study, attentional control can be thought of as conceptually identical to executive control in that it involves orienting attention toward goals while simultaneously inhibiting extraneous stimuli (Baddeley & Della Sala, 1996; Baddeley et al., 1984; Baddeley & Hitch, 1974; Baddeley & Hitch, 1994; Kane & Engle, 2002). To maintain consistency, I continue to use the term executive control.

I also changed stimuli in Experiment 3. In my previous experiments, one possible reason why I did not find a relationship between ability and baseline recall performance was my use of categorized study lists. Because categorized lists are easier to organize, they could have provided low ability individuals with an implicit organizational scheme that boosted their performance and eliminated any effects of ability (Unsworth, Brewer, & Spillers, 2013). To address this, I used uncategorized lists in Experiment 3 with the aim of increasing the variance in baseline recall as a function of individual ability. As the culmination of my PhD experiments, I intended Experiment 3 to be as sensitive and conclusive as possible.

I also included a recognition test, administered after final individual recall. I included this in order to determine whether lost items were lost due to retrieval disruption/blocking or lost due to retrieval inhibition (as used by Barber et al., 2014). If a lost item was subsequently recognized, then this would suggest that the item was temporarily disrupted or blocked and may still be accessible at a later time (Hyman et al., 2013; Rundus, 1973). If a lost item was not subsequently recognized, then this would suggest that the item was actually inhibited and forgetting would be more persistent (Aslan et al., 2007; Bäuml & Aslan, 2006). Using this additional recognition test, I aimed to understand the mechanisms underlying the forgetting I observed in Experiment 1.

Finally, I added to the post-experimental questionnaire. In addition to asking participants to report on strategy use, I asked them to rate their own memory ability.

For participants assigned to the collaborative condition, I also asked them to rate their partner's memory ability, so that I could examine how collaborative participants viewed their own memory ability relative to their partners'. Using this relative rating, I aimed to investigate whether a meta-memory phenomenon was causing lower ability participants to lose items, such that they perceived their partner's ability to be higher than their own.

Method

Participants

Eighty-seven undergraduate students from Macquarie University participated in Experiment 3 in return for course credit. Of these participants, seven were removed from analysis. Two participants solved fewer than 85% of the operations in the math portion of the OSPAN correctly (see Unsworth et al., 2005) and five participants were removed due to technical issues during data collection (e.g., corrupted data files or computer crashes). Thus, 80 participants (59 women, 21 men) were included in the final analyses, with a mean age of 21.14 (*SD* = 7.02). Twenty dyads (40 participants) were assigned to each of the two conditions (Nominal vs Collaborative). In similar previous collaborative recall experiments, the number of groups in each condition typically ranges from 12 (e.g. Congleton & Rajaram, 2014) to 16 (e.g. Pereira-Pasarin & Rajaram, 2011; Barber & Rajaram, 2011). Thus, in Experiment 3 I selected my sample size (20 dyads in each of the two conditions comprising 80 participants total) to be in line with previous collaborative recall research.

Design

Experiment 3 was a between-subjects design with the Condition factor having two levels: Nominal group recall vs Collaborative group recall. In some analyses, an additional factor of Relative Rank (Higher vs Lower) was considered. In some analyses, Recall Session (Recall 1 vs Recall 3) was considered as a within-subjects factor. The ability-related measures were continuous.

Materials

Word lists. I changed the stimulus lists in Experiment 3 to increase variance in recall due to individual differences in ability. I created two 48 items word lists, list A and list B. This was accomplished by generating 96 random, uncategorised nouns using the MRC Psycholinguistic Database (Coltheart, 1981) and dividing them in two. Half the participants received list A and half received list B. See Appendix B for word lists used in Experiment 3.

Executive control battery. I administered three tasks to assess executive control. These were administered at the beginning of the experimental session.

OSPAN. The version of the OSPAN used in Experiment 3 was identical to that used in Experiments 1 and 2 with the exception that only one block was administered. This shortened version of the OSPAN has been shown to retain psychometric properties similar to the long version (Foster et al., 2015). Thus, the single OSPAN block contained three sets of each of the trial sizes (three to seven) for a total of 25 letters and 25 math problems (25 total trials).

Antisaccade task. The antisaccade task was based on versions employed by Kane, Bleckley, Conway, and Engle (2001) and Payne (2005). In the antisaccade task, participants are required to look toward the opposite side of the computer screen when a star (*) flashes in order to identify a target stimulus (either an *O* or a *Q*). Participants must then use the keyboard to indicate which target stimulus they see. The star appears 3^{o} to the left or right of a central fixation cross. After 100 ms, the target appears 3^{o} to the side of the fixation cross opposite of the star. The target remains for 100 ms before being immediately replaced with a backward pattern mask

(*###*). Participants then have 5 seconds to input their response. The timing of trials is such that if participants look toward the star (rather than away, as instructed) they will miss the target and be unable to identify it. Typically, individuals with better executive control are less likely to look toward the star and thus more likely to be able to identify the target than are individuals with poorer executive control. Performance on each trial of the antisaccade is measured dichotomously (correct or incorrect target identification).

Stroop task. The Stroop task requires participants to name the colour in which a word is presented. The version I used in Experiment 3 was based on the procedure used by Spieler, Balota, and Faust (1996). The stimuli consisted of either colour words (*red, green, blue,* and *yellow*) or neutral words (*bad, deep, poor,* and *legal*). In neutral trials, the colour and word are unrelated (e.g., *legal* in red font). In congruent trials, the colour matches the word (e.g., *red* in red font). In incongruent trials, the colour does not match the word (e.g., *red* in blue font). In these cases, participants must override their automatic response, which is to simply read the word that is presented. Stroop interference effects are calculated by subtracting the reaction times of congruent trials from the reaction times of incongruent trials. Thus, a higher score on the Stroop task indicates lower performance. Typically, individuals with better executive control exhibit less interference than individuals with poorer executive control.

Post-experimental questionnaire. At the end of the session, participants completed a post-experimental questionnaire. The questionnaire was completed on the computer using Qualtrics software. The questionnaire collected demographic information such as age and gender. In addition, it asked participants to indicate whether they had employed a specific recall strategy when recalling individually. The questionnaire also asked participants to give memory ratings. Participants in the Nominal condition were presented with an onscreen slider bar and asked to rate their own memory ability on a scale from 0 – 100 (see Figure 21). Participants in the Collaborative condition were presented with two onscreen slider bars and asked to rate both their own memory ability and their partner's memory ability on scales from 0 – 100 (see Figure 22). In this manner, I could examine Collaborative participants' relative ratings of their own and their partner's memory ability.

Procedure

The experiment consisted of eight phases: (1) Executive control battery; (2) Study phase; (3) Distractor task; (4) Recall 1; (5) Recall 2; (6) Recall 3; (7) Recognition test; and (8) Post-experimental questionnaire. Experimental sessions were always scheduled to include two participants, with dyads randomly assigned to either the Nominal or the Collaborative condition upon their arrival. If one of the participants failed to show up for their scheduled time, the remaining participant was assigned to the next open Nominal group by default. I formed 20 Nominal and 20 Collaborative dyads in this manner.

Before the experiment began participants read and signed information and consent forms. Participants in the Nominal condition were told that they were completing a study that investigated how working memory and attention influenced the way in which people remembered both alone and in groups.

Executive control battery. The three tasks in the executive control battery were always administered on a computer in the same order: OSPAN, antisaccade, and Stroop. The participants were given a brief overview of the tasks and advised that the instructions would be repeated on the computer screen before the beginning of each task. Participants completed the executive control battery in individual isolation

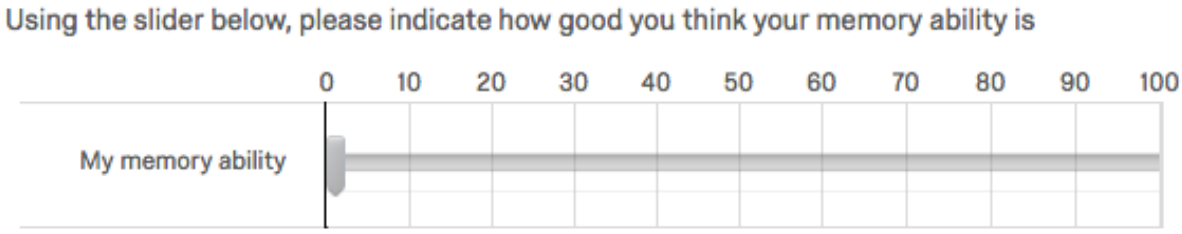

Figure 21. Example of memory rating item for participants in the Nominal condition.

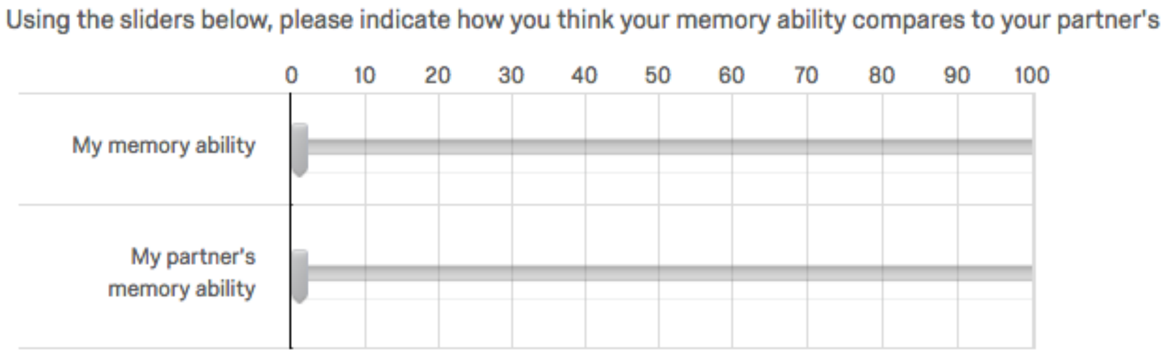

Figure 22. Example of memory rating item for participants in the Collaborative condition.

booths and could not see one another. Phase 1 of the experiment lasted approximately 15-20 minutes.

OSPAN. Participants completed the shortened version of the OSPAN which consisted of a total of 25 trials.

Antisaccade. Participants completed a total of 56 trials. Of these, 8 were practice trials and 48 were experimental trials. Participants were allowed to take a break halfway through the task.

Stroop task. Participants completed a total of 140 trials. Of these, 20 were practice trials and 120 were experimental trials. Of the experimental trials, 40 were congruent trials, 40 were incongruent trials, and 40 were neutral trials. Participants were allowed to take breaks 1/3 and 2/3 of the way through the task.

Study phase. Following the executive control battery, participants moved to the same room used in Experiments 1 and 2. They were seated at computers side by side and facing in the same direction. They were separated by two meters with a screen between them. All participants saw one of the two lists displayed on a computer screen. The 48 words were presented using E-prime 2.0 software and appeared in lowercase letters in black text on white background in the centre of the screen where they remained for three seconds each. A one second buffer separated the presentation of each word. Participants always saw the same list as the other member of the dyad with whom they were paired.

Distractor task. Following presentation of the word list, participants spent 4 minutes attempting to solve a difficult Sudoku puzzle. The distractor task set-up and materials in Experiment 3 were identical to Experiments 1 and 2.

Recall 1: Baseline individual recall. Experiment 3 Recall 1 set-up and materials were identical to Experiments 1 and 2, with the exception that there were only two columns of 22 blank lines each (44 lines in total).

Recall 2: Group recall. Experiment 3 Recall 2 set-up and materials were identical to Experiments1 and 2, with the exception that there were only two columns of 22 blank lines each (44 lines in total).

Recall 3: Final individual recall. Experiment 3 Recall 3 set-up and materials were identical to Experiments 1 and 2, with the exception that there were only two columns of 22 blank lines each (44 lines in total).

Recognition test. Participants were given a sheet of paper that contained 96 words (three columns of 32 words each). Forty-eight of the words were words the participants had studied and 48 were new words from the other version of the word list that the participants had not studied. These words served as lures. Participants were instructed to read each word and circle it if they remembered having seen it during the study phase. The recognition test was not timed and participants were instructed to turn over their sheet of paper to indicate that they were finished.

Post-experimental questionnaire and debrief. At the end of each session participants completed the post-experimental questionnaire and were debriefed.

Measures and Scoring

Individual Ability. To derive a measure of individual executive control ability. I administered three tasks. These tasks were the OSPAN, antisaccade, and the Stroop task. I then performed a principal component analysis on these data to derive a single component score (EC Score) for each individual which represented their executive control ability.

OSPAN*.* I used the OSPAN to assess participants' working memory capacity. Upon completion of the task, a data file was generated which contained the OSPAN score. Scores could range from 0 to 25 with higher scores indicating greater working memory capacity. The potential range of OSPAN scores was different in Experiment 3 compared to Experiment 1 because I only administered one block, rather than three blocks, to each participant. Recent research suggests that shortened versions of the OSPAN are sufficiently valid for measuring working memory capacity (Foster et al., 2015). Like Experiments 1 and 2, in Experiment 3 I used the partial-scoring method.

Antisaccade*.* I used the antisaccade task to assess participants' executive control. Upon completion of the task, a data file was generated which contained each trial and a binary accuracy indicator (i.e., 0 = "incorrect" and 1 = "correct). The first 16 trials were discarded as practice trials. Antisaccade performance was the proportion of remaining trials in which the participant responded correctly.

Stroop. I used the Stroop task to assess participants' executive control. Upon completion of the task, a data file was generated that contained reaction times to each trial, trial condition (i.e., congruent, incongruent, or neutral), and what the correct response should have been. In addition, separate audio files were generated for each trial. These audio files contained the actual recording of the participants' responses. To score the Stroop task for accuracy, I listened to each audio recording and entered a binary indicator (i.e., 0 = "incorrect" and 1 = "correct") regarding accuracy. I only used correct responses in subsequent RT analyses. For each participant, I removed RT outliers similar to the procedure described by Selst and Jolicoeur (1994). Essentially, within each participant this method removes all RTs that fall above or below 2.5 standard deviations from the mean. This method removed 2.7% of the correct Stroop task RTs in my sample. I calculated Stroop RT interference effects by subtracting the mean RT for congruent trials from the mean RT for incongruent trials. Similarly, I calculated Stroop error interference effects by subtracting percent error for congruent trials from percent error for incongruent trials. With both Stroop interference RT and error, higher scores indicate higher levels of interference and thus lower executive control ability.

Group Ability. As in Experiment 1, I conceptualised Group Ability as the overall ability of each dyad. To calculate Group Ability, I simply averaged the EC scores of both members of each dyad. For example, if the EC score for one participant was -1.40 and 2.10 for the other, the Group Ability of the dyad was 0.30.

Group Discrepancy. As in Experiment 1, I conceptualised Group Discrepancy as the difference between abilities of group members within each dyad. To calculate

Group Discrepancy, I calculated the absolute difference between the EC scores of each member in each dyad. Using the previous example of scores of -1.40 and 2.10, Group Discrepancy would be equal to 3.50.

Recall performance and intrusion rates. All recall scores were calculated as a proportion of correctly recalled out of total studied words. All intrusion rates were calculated as a proportion of intrusions out of the sum of correctly recalled items and intrusions. These calculations were identical to Experiments 1 and 2 with the exception that recall performance was calculated as a proportion of 48 items, rather than 84.

Change scores. Net change scores and Gained and Lost items in Experiment 3 were calculated in an identical manner to Experiments 1 and 2.

Recognition performance. I calculated statistics for final recognition performance in order to determine whether the Individual Ability, Group Ability and Group Discrepancy related the probability of participants correctly recognizing previously studied items.

Hit rates. To calculate hit rates, I first calculated each participant's number of raw correct responses. That is, the number of items a participant correctly indicated that they had seen at study. I then divided this number by the number of originally studied items (48). Thus, participants' hit rates were a proportion of correctly recognised items out of the total number of studied items

***False alarms*.** To calculate false-alarm rate, I first calculated the number of lure items each participant incorrectly recognised as having been originally studied. I then divided this number by the number of lure items present on the recognition test (48). Thus, participants' false alarm rates were a proportion of incorrectly recognised items out of the total number of lures.

Corrected recognition. I also calculated a corrected recognition measure. To do this, I simply subtracted each participant's false-alarm rate from their hit rate.

Discriminability. I calculated a measure of *discriminability* for each participant. The is the ability to distinguish old from new items and is generally expressed as d′. However, given that I had some participants who made no false-alarms, I instead calculated a nonparametric discriminability measure, A′. To calculate A′, I used a formula outlined by Stanislaw and Todorov (1999) which was based on Snodgrass, Levy-Berger, and Haydon (1985). The formula was:

$$A' = .5 + \frac{(H - F)(1 + H - F)}{4H(1 - F)}$$

Where *H* was equal to the hit rate and *F* was equal to the false-alarm rate. A′ scores can range from 0 to 1, with 0.5 indicating chance performance.

Response bias. I also calculated a measure that indicated how likely an individual was to circle old words and not-circle new words, B″. To calculate B″ I used a formula outlined by Stanislaw and Todorov (1999). The formula was:

$$B'' = \frac{H(1 - H) - F(1 - F)}{H(1 - H) + F(1 - F)}$$

Where *H* was equal to the hit rate and *F* was equal to the false-alarm rate.

Results

Statistics

For all inferential statistics using null-hypothesis testing, I used a two-tailed analysis and a traditional significance criterion ($\propto = .05$). For all follow-up tests (i.e., t-tests), I used a Bonferroni-corrected significance criterion (i.e., $\propto = .05$/the number of comparisons).

Baseline

Prior to collaboration, I obtained baseline data for all the participants in my sample. These data consisted of an executive control score, individual recall performance, and intrusion rates.

Individual Ability. To derive an executive control component score (EC score) for each participant, I performed a principal components analysis (PCA) on the OSPAN, antisaccade, and Stroop data (see Table 17 for individual task descriptive statistics). Performing a PCA on the three tasks essentially reduces the data to variance that is common among all of the tasks. That is, it provides normalised scores that indicate performance measures on a latent component that is commonly measured by each of the three tasks. As such, the extraction of this component theoretically provides for a better measure of executive control ability than any one of the tasks alone. Prior to the PCA, Stroop RT and error effects were transformed to *z* scores and then averaged for each participant to derive an overall Stroop effect. I transformed the Stroop data in this manner as having two intercorrelated measures in the PCA might artificially inflate the loading of the Stroop data (see Hutchison, 2007). On the other hand, having tasks that do not exhibit any intercorrelations may be problematic for a PCA as there would be no common variance to extract. As can be seen in Table 18, this was not the case in my data.

Table 17
Executive Control Battery Descriptive Statistics

Task	*M*	*SD*	Range
OSPAN	19.10	4.55	7 - 25
Antisaccade	0.72	0.15	0.40 – 0.98
Stroop RT	123.34	61.92	0.95 - 306.23
Stroop error	3.92	5.88	-4.17 - 41.55

Note. OSPAN refers to raw Operation Span scores. Antisaccade refers to proportion of correct responses. Stroop RT and Stroop error refers to reaction time (ms) and percent error differences between incongruent and congruent conditions.

The EC component score derived from the PCA should, in theory, represent an underlying component common to all three of the tasks I employed in my study (see Hutchison, 2007, for use of this battery). Thus, variance that is common among the tasks represents a better indicator of ability than any one task alone. In this case, a component that would theoretically represent underlying executive control ability should receive positive loading for the OSPAN and anticsaccade measures and negative loadings for the Stroop task, which was indeed the pattern I observed (see Table 18).

The un-rotated PCA matrix indicated one significant component that explained 50.97% of the variance in performance across tasks. This is in line with previous studies using this battery (Hutchison, 2007; Hutchison, Heap, Neely, & Thomas, 2014). Individual EC component scores ranged from -2.24 to +4.86.

Table 18

Intercorrelations Among Executive Control Tasks and Principal Component Analysis

Task	OSPAN	Antisaccade	Stroop	PCA loading
OSPAN	----	.35**	-.21	+.61
Antisaccade		----	-.23*	+.62
Stroop			----	-.50

* $p < .05$ ** $p < .01$.

The mean EC score for individuals in the Nominal condition was -0.06 (SD = 1.16) with scores ranging from -2.66 to 2.24. The mean EC score for individuals in the Collaborative condition was 0.06 (SD = 1.32) with scores ranging from -4.86 to 1.76. An independent samples t-test of individual EC scores indicated no difference between the conditions, $t(78) = 0.46$, $p = .644$. Thus, there were no pre-existing executive control differences between participants in the two conditions.

Recall 1: Initial individual recall and intrusions. The mean proportion of correctly recalled items in the Nominal condition was 0.19 (*SD* = 0.07) with performance ranging from 0.06 to 0.40. The mean proportion of correctly recalled items in the Collaborative condition was 0.22 (*SD* = 0.12) with performance ranging from 0.04 to 0.56. An independent samples t-test of Recall 1 proportions indicated no significant effect of condition, $t(78) = 1.07$, $p = .289$. Thus, there were no pre-existing differences in initial recall between the two groups.

The mean intrusion rate in the Nominal condition was 0.18 (SD = 0.16) with scores ranging from 0.00 to 0.55. The mean intrusion rate in the Collaborative condition was 0.14 (SD = 0.14) with scores ranging from 0.00 to 0.50. An independent samples t-test of Recall 1 intrusions rates indicated no significant effect of condition, t(78) = -1.05, p = .297. Thus, there were no pre-existing differences in intrusion rates between the two groups, and about one sixth of items participants listed at Recall 1 were intrusions. These intrusion rates were substantially higher than in the previous two experiments, which was likely due to the change in stimuli.

I also tested whether individuals who reported using specific retrieval strategies differed in their baseline recall performance. Of the 80 participants in my sample, 53 reported using an individual strategy to guide their recall and 27 reported using no strategy. Mean recall for those who reported using a strategy was 0.24 (*SD* = 0.10). Mean recall for those who reported using no strategy was 0.14 (*SD* = 0.07). There was a significant difference between the two groups, $t(78) = 4.48$, $p < .001$, $d = 1.16$, which suggested that using a strategy helped participants to remember more words.

Recall 1 and Individual Ability. To test whether memory performance was related to executive control, I obtained Pearson bivariate correlations between EC scores and Recall 1 performance. Since there were no pre-existing differences between participants assigned to each condition, I collapsed across participants in this analysis. Across the entire sample, there was a marginally significant relationship between executive control ability and Recall 1 performance, $r(78) = .21$, $p = .060$ (see Figure 23). Thus, for the first time in my experiments there was evidence for an association – albeit a weak one – between individual executive control ability and baseline memory performance.

To further explore the relationship between executive control and individual baseline recall, I obtained Pearson bivariate correlations between each of the EC tasks (i.e. OSPAN, Antisaccade, and Stroop) and Recall 1 performance. I found a moderate correlation between Antisaccade performance and Recall 1 performance, $r(78) = .31$, $p = .005$. The other relationships were not significant. This suggested that performance on the Antisaccade task was the most accurate indicator of baseline recall.

To test whether baseline intrusion rates were related to executive control, I obtained Pearson bivariate correlations between EC scores and Recall 1 performance. Across the entire sample, there was a significant negative relationship between executive control ability and intrusion rates, $r(78) = -.23$, $p = .040$ (see Figure 24). This suggested that on initial recall, individuals with higher executive control were less likely to recall items that they had never actually studied than were individuals with lower executive control. As with baseline recall, I obtained correlations between each of the EC measures and baseline intrusion rates. The Antisaccade exhibited a weak, negative relationship with intrusions, $r(78) = -.22$, $p = .054$, which indicated that, like recall, performance on the Antisaccade task was the main driver of the relationship between EC and intrusions. Taken together, the results of my baseline

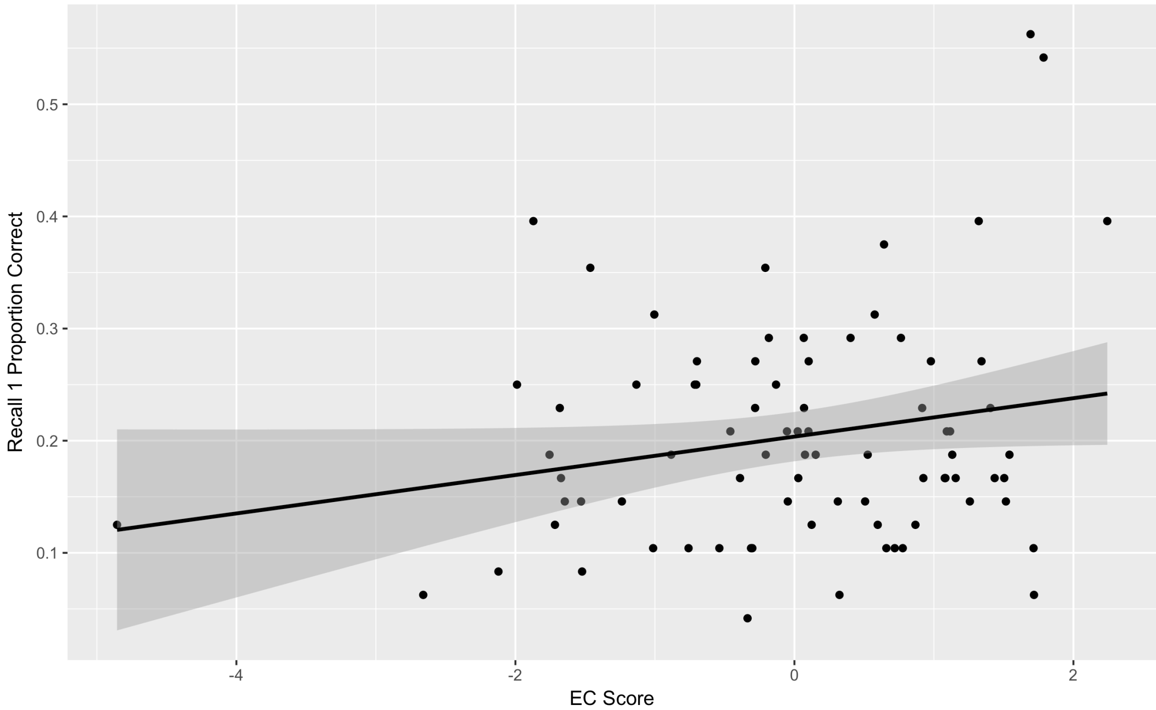

Figure 23. Recall 1 performance as a function of Individual Ability. Shaded area represents standard error.

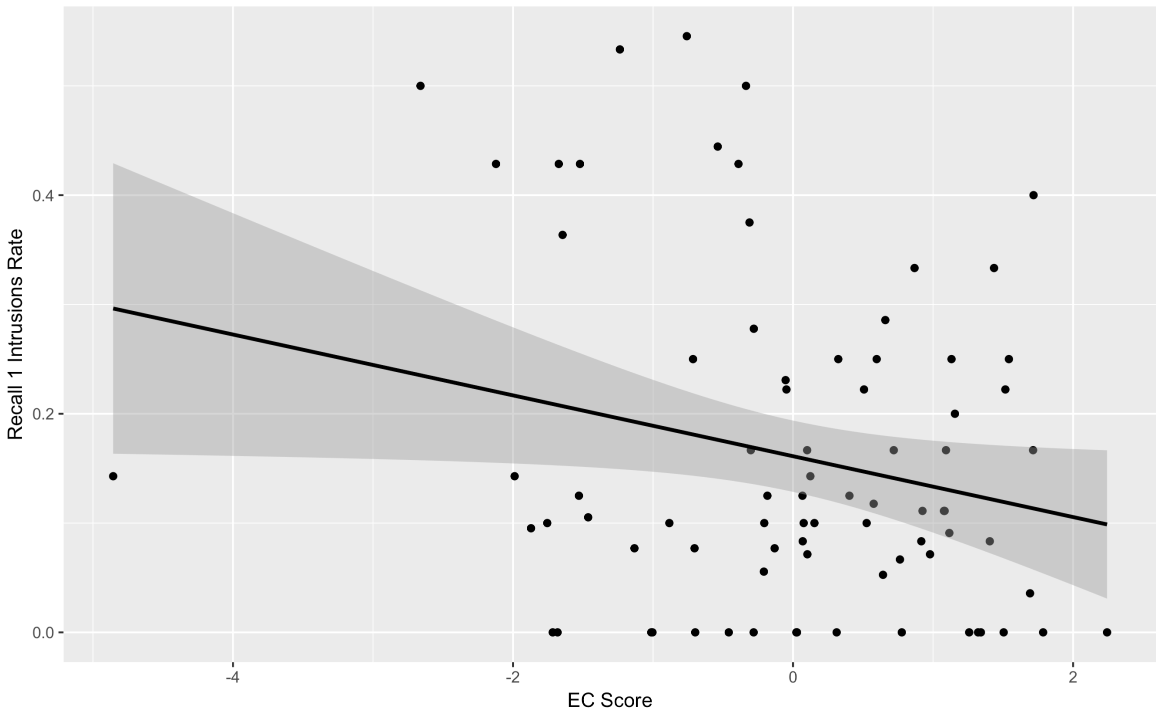

Figure 24. Recall 1 intrusion rates as a function of Individual Ability. Shaded area represents standard error.

measure analyses suggest that, as intended, the EC component score was a more sensitive indicator of Individual Ability than the OSPAN alone.

I also tested whether individuals who reported using specific retrieval strategies differed in their executive control ability. Mean EC scores for those who reported using a strategy was 0.09 (*SD* = 1.12). Mean recall for those who reported using no strategy was -0.17 (*SD* = 1.43). The difference between the two groups was not significant, $t(78) = 0.88$, $p = .381$, which suggested that people who employed specific retrieval strategies did not differ in ability from individuals who did not employ a strategy.

Collaboration

During the collaborative phase, I collected group-level data in order to assess dyads' recall performance and how this might be influenced by Group Ability and Group Discrepancy. These data included group recall performance and group intrusion rates (Nominal and Collaborative groups), as well as group-level executive control measures that I derived as previously described.

Recall 2: Group recall. I calculated Recall 2 proportions for both Nominal and Collaborative groups to determine whether group recall differed between the two conditions. The mean proportion of correctly recalled items in the Nominal condition was 0.31 (*SD* = 0.08) with performance ranging from 0.19 to 0.48. Mean proportion of correctly recalled items in the Collaborative condition was 0.30 (*SD* = 0.15) with scores ranging from 0.15 to 0.56. An independent samples t-test indicated no significant difference between the conditions, $t(38) = 0.21$, $p = .832$. Thus, I did not observe the standard collaborative inhibition effect in my sample.

Recall 2: Group intrusions. To determine whether collaboration influenced inaccurate recall, I calculated Recall 2 intrusion rates for each condition. The mean intrusion rate in the Nominal condition was 0.25 (*SD* = 0.15) with scores ranging from 0.05 to 0.57. The mean intrusion rate in the Collaborative condition was 0.09 (*SD* = 0.08) with scores ranging from 0.00 to 0.27. An independent samples t-test indicated a significant effect of Condition, whereby Collaborative groups demonstrated far fewer intrusions than did Nominal groups, $t(38) = 4.33$, $p < .001$, $d = 1.33$. As expected, collaboration benefited memory accuracy by lowering intrusion rates.

Group Ability and Group Discrepancy. One of my primary research questions was whether the executive control characteristics of groups would influence the outcomes of collaboration. To address this, I calculated both overall ability of each group (G*roup Ability)* as well as a measure of difference in ability between members within each group (G*roup Discrepancy)*[2]*.* See Table 19 for group-level EC measure descriptive statistics.

Table 19

Group-Level Executive Control Descriptive Statistics

	Group Ability			Group Discrepancy		
Condition	Mean	Range	SD	Mean	Range	SD
Nominal	-0.06	-1.56 - 0.79	0.72	1.57	0.24 - 3.28	0.90
Collaborative	0.06	-1.89 - 1.18	0.74	1.78	0.02 - 5.93	1.29

Note. Group Ability refers to the average of the executive control component scores of the dyad members. Group Discrepancy refers to the difference between the dyad members' executive control component scores.

To test whether the groups in my two conditions differed with respect to the group EC measures, I conducted independent samples t-tests of Group Ability and Group Discrepancy. Group Ability did not significantly differ between Nominal groups

[2] Visual examination of both the Group Ability and Group Discrepancy data suggested that these measures exhibited a non-normal distribution. Thus, for significant effects in the following presentation of results I subsequently converted these data to *z*-scores and reanalyzed. This reanalysis produced similar results.

and Collaborative groups, $t(78) = 0.80$, $p = .428$. Likewise, the Group Discrepancy did not significantly differ between Nominal groups and Collaborative groups, $t(78) = 0.86$, $p = .391$. This indicated that group characteristics were equivalent in my sample.

Recall 2: Relationship with executive control. To test whether group-level executive control characteristics influenced collaboration, I conducted separate correlational analyses for each condition between each of the group-level measures and group performance. Group Ability was not related to Recall 2 performance for either the Nominal, $r(18) = 0.07$, $p = .775$, or the Collaborative groups, $r(18) = 0.28$, $p = .224$ (see Figure 25). Likewise, Group Discrepancy was not related to Recall 2 performance for either the Nominal, $r(18) = 0.11$, $p = .645$, or the Collaborative groups, $r(18) = -0.18$, $p = .448$ (see Figure 26). Thus, as in Experiments 1 and 2, group-level executive control characteristics did not significantly influence collaborative recall.

To examine whether there was a relationship between group-level executive control measures and inaccurate recall, I conducted separate correlational analyses for each condition between the group measures Group and group intrusion rates. Group Ability did not relate to Recall 2 intrusion rates in either the Nominal, $r(18) = -0.06$, $p = .796$, or Collaborative groups, $r(18) = -0.11$, $p = .634$ (see Figure 27). Likewise, Group Discrepancy did not relate to Recall 2 intrusion rates in either the Nominal, $r(18) = 0.00$; $p = .998$, or the Collaborative groups, $r(18) = 0.00$, $p = .987$ (see Figure 28).

Group recall: Relative ability and relative contribution. As in Experiments 1 and 2, I examined individual contributions during group recall from the perspective of relative ability. To do this, I distinguished each dyad member according to their

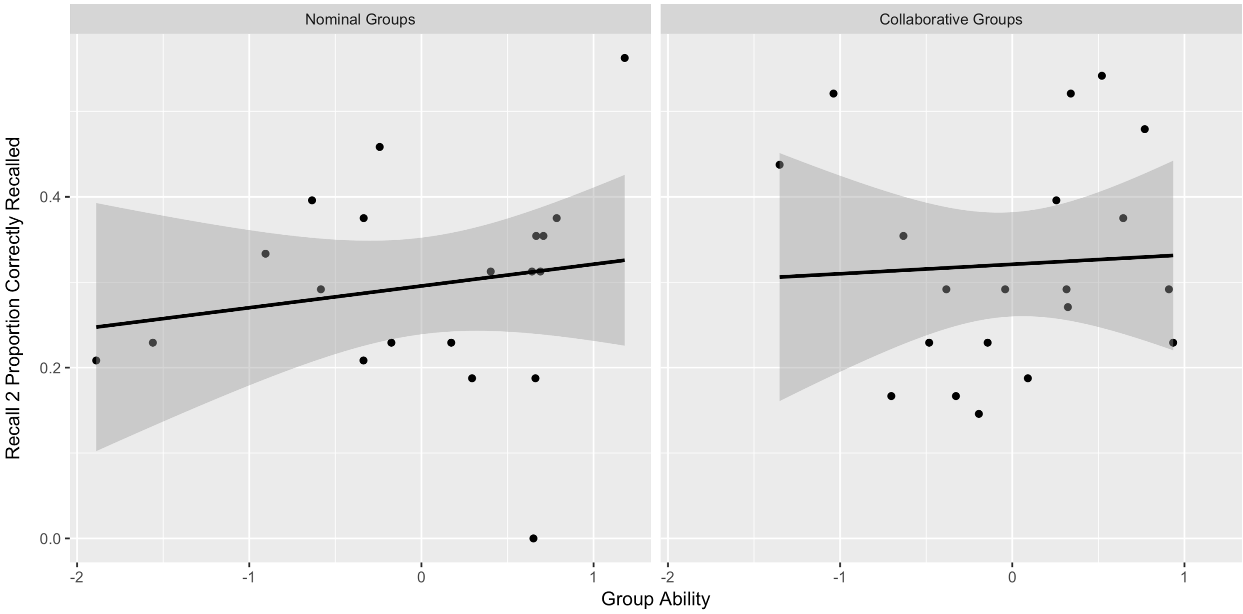

Figure 25. Group recall performance in the Nominal and Collaborative conditions as a function of Group Ability. The shaded area represents standard error.

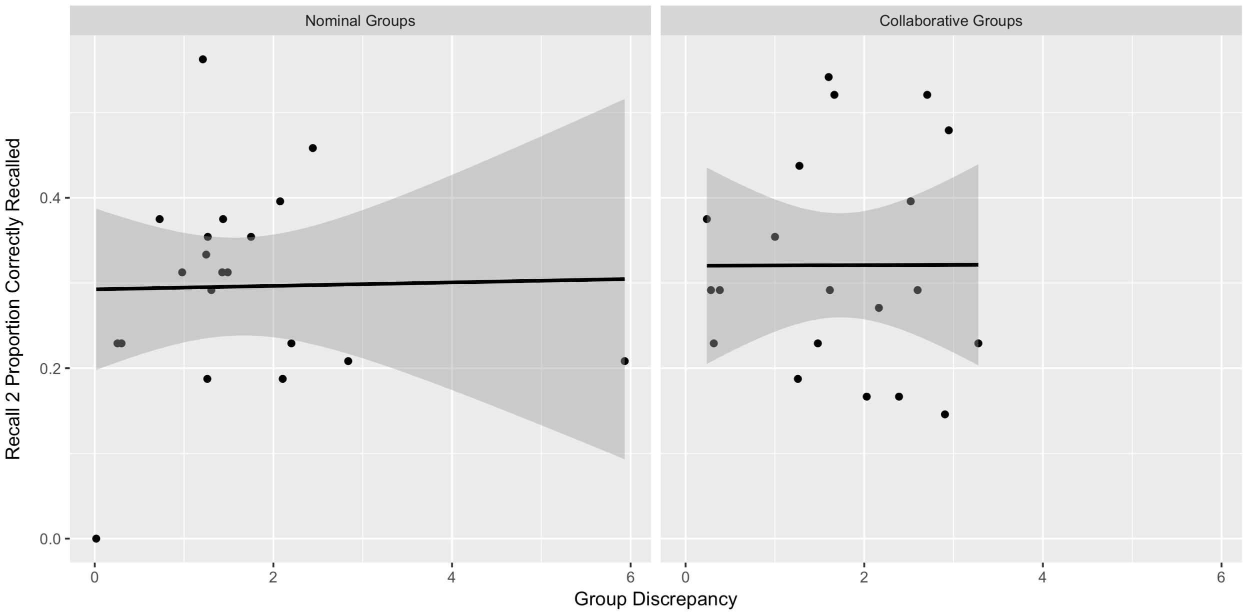

Figure 26. Group recall performance in the Nominal and Collaborative conditions as a function of Group Discrepancy. The shaded area represents standard error.

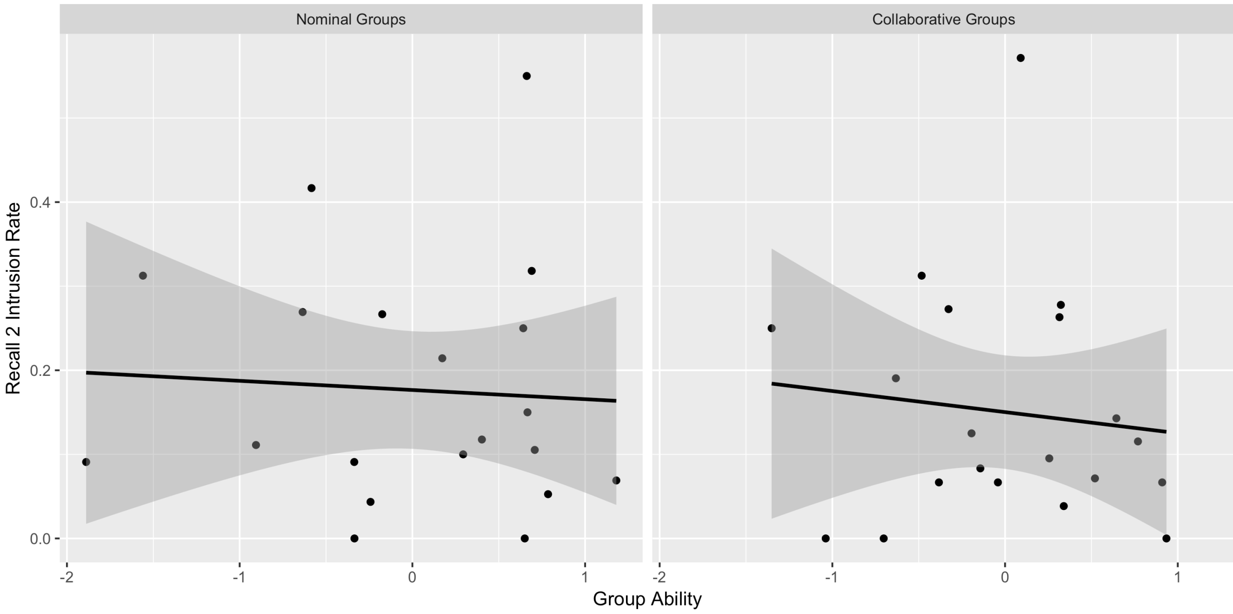

Figure 27. Group intrusion rates in the Nominal and Collaborative conditions as a function of Group Ability. The shaded area represents standard error.

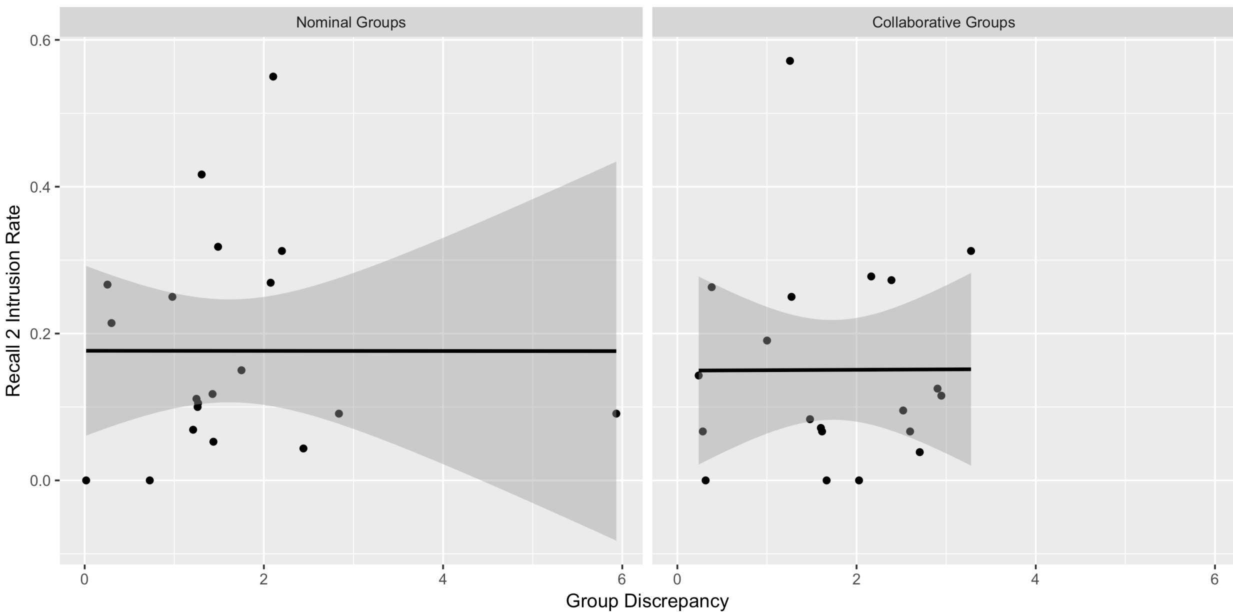

Figure 28. Group intrusion rates in the Nominal and Collaborative conditions as a function of Group Discrepancy. The shaded area represents standard error.

ordinal rank within the dyad. That is, I distinguished each member as either the *Higher* or *Lower* ability partner. I then tested whether Recall 2 output varied as a function of this distinction. In other words, did the Higher ability partners within a dyad recall more or less than the Lower ability partners? To test this question, I conducted a 2 x 2 ANOVA with Condition (Nominal vs Collaborative) and Relative Ability (Higher vs Lower) as between-subjects factors. Results indicated no significant effects of either Condition, $F(1,76) = 1.95$, $p = .167$, or Relative Ability, $F(1,76) = 0.49$, $p = .488$. The interaction of the two factors was also non-significant, $F(1, 76) = 0.20$, $p = .655$ (See Figure 29). This suggested that the contribution of items during collaboration was equivalent for both the relatively Higher and Lower ability partners.

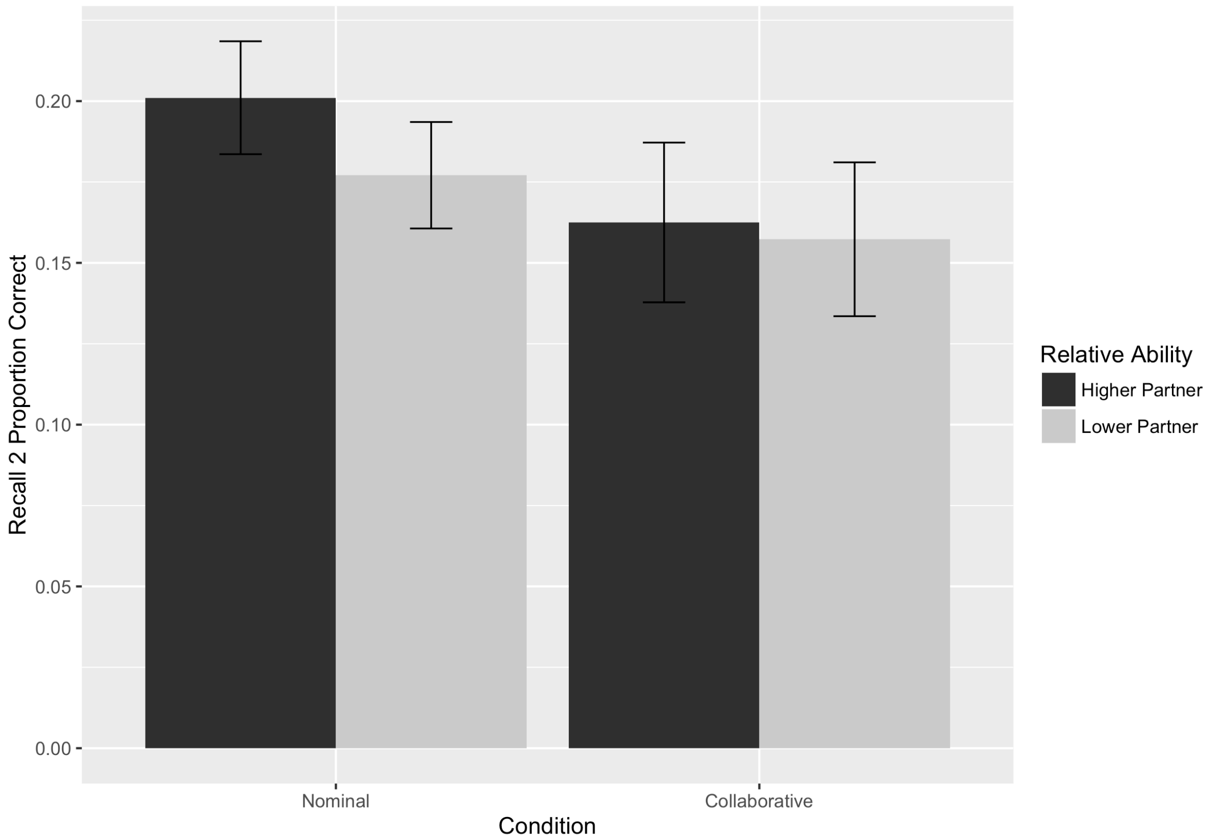

Figure 29. Proportion of correct items offered by each dyad member during group recall according to their Relative Rank within the dyad. Error bars represent SEM.

Post-Collaboration

Another of my research questions was whether group executive control characteristics influenced post-collaborative benefits. To examine this question, I derived Recall 3 proportions in an identical manner to Recall 1. Using these data, I could compare subsequent, post-collaborative recall to the baseline initial individual recall data for both Non-collaborators and Collaborators. Non-collaborators were former members of Nominal groups; Collaborators were former members of Collaborative groups.

Recall 3: Final individual recall and intrusions. The mean proportion of correctly recalled items by former Non-collaborators was 0.19 (*SD* = 0.08) with performance ranging from 0.06 to 0.42. The mean proportion of correctly recalled items by former Collaborators was 0.27 (*SD* = 0.11) with scores ranging from 0.10 to 0.56. The mean intrusion rate by Non-collaborators was 0.23 (*SD* = 0.20) with scores ranging from 0.00 to 0.67. The mean intrusion rate by Collaborators was 0.13 (*SD* = 0.12) with scores ranging from 0.00 to 0.43.

To test if Individual Ability was related to post-collaborative recall I performed correlational analyses between EC scores and Recall 3 scores for both Non-collaborators and Collaborators. The relationship was not significant for either Non-collaborators, $r(38) = .26$, $p = .102$, or Collaborators, $r(38) = .16$, $p = .337$. Thus, Individual Ability did not relate to final individual recall performance for either condition.

To test if Individual Ability related to intrusion rates I performed correlational analyses between EC scores and Recall 3 intrusion rates for both Non-collaborators and Collaborators. The relationship was not significant for either Non-collaborators, $r(38) = -.26$, $p = .109$, or Collaborators, $r(38) = .06$, $p = .700$. This suggested that even though collaboration yielded lower post-collaborative intrusion rates, this was not due to Collaborators' Individual Ability.

Recall 1 vs Recall 3. To assess whether collaboration benefited Collaborators relative to Non-collaborators, I performed a 2 x 2 mixed-model ANOVA with Condition (Nominal vs Collaborative) as the between-subjects factor and Recall Session (Recall 1 vs Recall 3) as the within-subjects factor. Results indicated a significant main effect of both Condition, $F(1,78) = 5.67$, $p = .020$, $\eta^2_p = .07$, and of Recall Session, $F(1,78) = 29.53$, $p < .001$, $\eta^2_p = .27$. The interaction between the factors was also significant, $F(2,78) = 31.89$, $p < .001$, $\eta^2_p = .29$. I then performed follow-up t-tests, which indicated that whereas Non-collaborators did not exhibit an increase in recall from Recall 1 to Recall 3, $t(39) = 0.30$, $p = .767$, Collaborators did, $t(39) = 5.93$, $p < .001$, $d = 1.90$ (See Figure 30). Thus, collaboration enhanced later individual performance.

I also performed a 2 x 2 mixed-model ANOVA on the intrusion rate data. Results indicated a significant effect of Condition, $F(1,78) = 3.99$, $p = .05$, $\eta^2_p = .05$, but not of Recall Session, $F(1,78) = 2.41$, $p = .120$. The interaction between the two factors was also non-significant, $F(1,78) = 1.59$, $p = .210$ (see Figure 31). This suggested that, although overall Collaborators had lower intrusion rates than Non-collaborators, this was not necessarily due to the effects of post-collaborative error-pruning.

Change scores: Source of post-collaborative benefits. To determine the source of post-collaborative benefits, I calculated Gained and Lost change scores. Using these, I investigated whether post-collaborative benefits were related to differences in executive control.

Did Individual Ability relate to items Gained and Lost? To test whether Individual Ability related to gains or losses that occurred as a result of collaboration, I obtained Pearson bivariate correlations between individual EC scores and Gained and Lost scores for both Non-collaborators and Collaborators (see Table 20). The relationship between Collaborators' EC scores and Lost items was marginally significant. None of the other relationships were significant, all *p*s > .233. To investigate which EC task(s) were responsible for this finding, I obtained correlations between each of the EC tasks and lost item proportions for Collaborators'. Both the OSPAN and Antisaccade tasks demonstrated a negative, weak correlation with lost items, though both relationships were marginally significant (OSPAN: $r(38) = -.28$, $p = .085$; Antisaccade: $r(38) = -.28$, $p = .078$. Overall, this suggested that as Collaborators' ability level decreased, their tendency to lose items through collaboration increased. Further, this effect was mainly driven by performance on the OSPAN and Antisaccade rather than performance on the Stroop task.

Table 20

Correlations Between Individual Ability and Change Scores

Condition	Gained	Lost
Non-collaborators	.12	-.19
Collaborators	-.14	-.30*

Note. Value represent Pearson coefficients.
*$p = .064$

Did relative ability relate to items Gained and Lost? To test if relative ability influenced post-collaborative gains, I performed a 2 x 2 ANOVA on the Gained item proportions with Condition (Nominal vs Collaborative) and Relative Rank (Lower vs Higher Partner) as between-subjects factors (see Table 21). Results indicated a significant main effect of Condition, $F(1,76) = 27.19$, $p < .001$, $\eta^2_p = .26$, but not of Relative Rank, $F(1,76) = 0.61$, $p = .436$. The interaction between the two factors was not significant, $F(1,76) = 0.44$, $p = .508$. These results suggested that individuals who

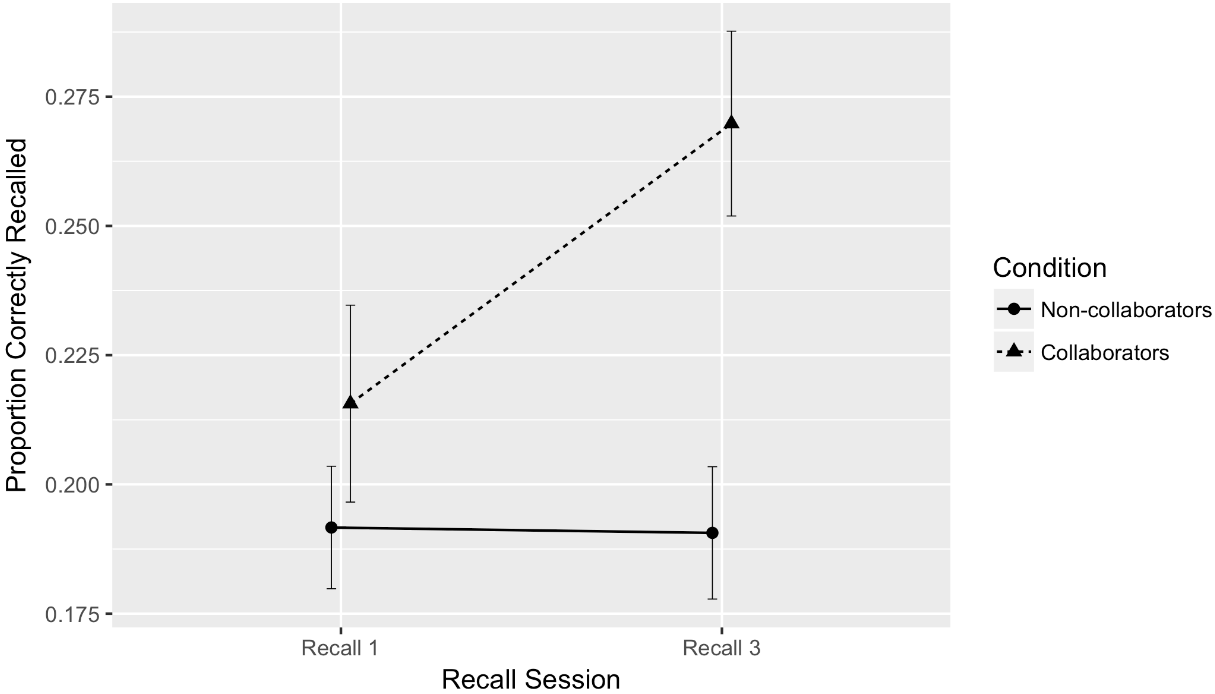

Figure 30. Individual recall performance from Recall 1 to Recall 3 by Condition. Error bars represent SEM.

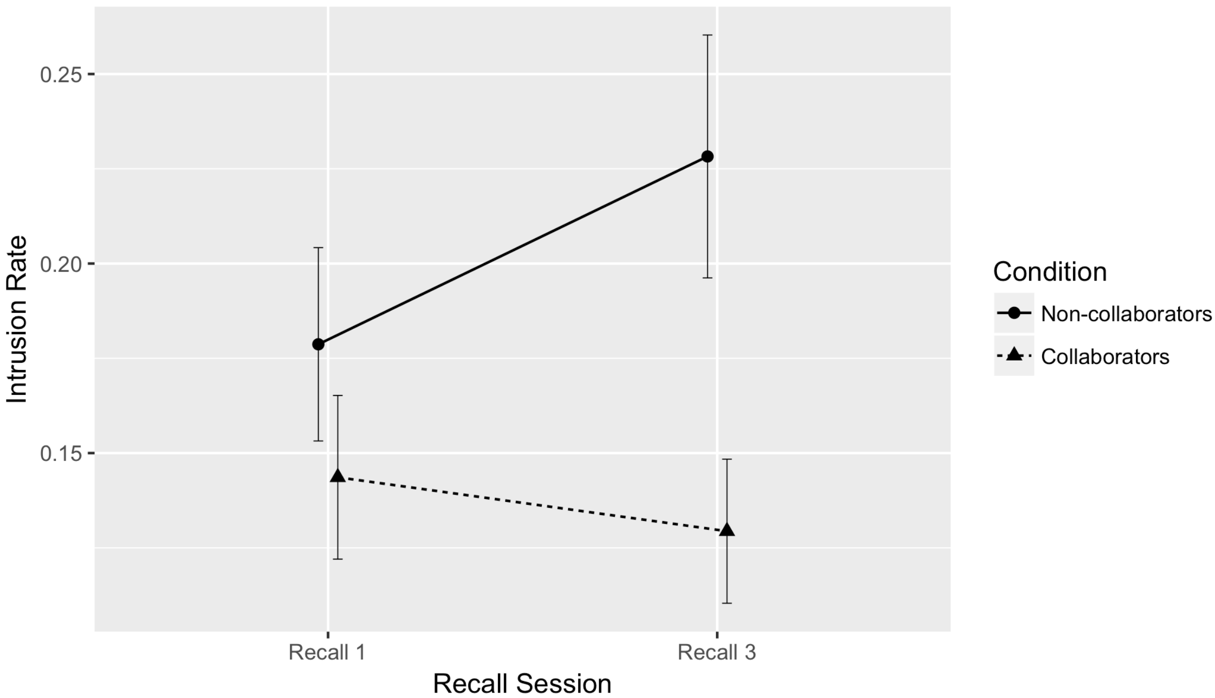

Figure 31. Individual intrusion rates from Recall 1 to Recall 3 by Condition. Error bars represent SEM.

had previously collaborated, relative to those who had not collaborated, gained a greater proportion of items from Recall 1 to Recall 3 (See Table 21).

Table 21

Mean Proportion of Items Gained and Lost from Recall 1 to Recall 3

Condition & EC Rank		Gained	Lost	Net
Non-collaborators				
	Lower Partner	0.08 (0.11)	0.12 (0.11)	-0.04 (0.14)
	Higher Partner	0.07 (0.10)	0.06 (0.09)	0.01 (0.09)
Collaborators				
	Lower Partner	0.61 (0.58)	0.15 (0.15)	0.47 (0.62)
	Higher Partner	0.48 (0.53)	0.10 (0.10)	0.38 (0.48)

Note. Net = Gained – Lost. Each item type is expressed as a proportion of initial recall. SD in parentheses.

To test if the composition of groups, with respect to executive control, influenced post-collaborative losses, I performed similar 2 x 2 ANOVA on Lost item proportions. Results indicated no significant main effect of Condition, $F(1,76) = 1.94$ $p = .168$. The main effect of Relative Rank was marginally significant, $F(1,76) = 1.93$, $p = .051$, $\eta^2_p = .05$. The interaction between the two factors was not significant, $F(1,76) = 0.05$, $p = .994$. The marginal main effect of Relative Rank suggested that perhaps the Lower ability partners tended to lose more items ($M = 0.13$, $SD = 0.13$) than the Higher ability partners ($M = 0.08$, $SD = 0.09$), just as in Experiment 1.

Overall these results suggested that collaboration benefited post-collaborative recall. This was achieved through superior gains by Collaborators relative to Non-collaborators, rather than fewer losses. However, this pattern of gains was not contingent on the participants' executive control ability relative to their partners' in the groups in which they had previously collaborated.

Did group-level characteristics relate to post-collaborative benefits? To test whether the executive control characteristics of the group in which an individual collaborated influenced the post-collaborative benefits, I conducted correlational analyses between the group-level executive control measures (Group Ability and Group Discrepancy) and Gained and Lost items (see Table 22).

The results of the correlational analyses indicated that the Lower partners' Discrepancy scores positively related to their Lost items. None of the other correlations exhibited a significant relationship, though the relationship between Higher partners' Discrepancy scores and Lost items was marginally significant (*p* = .076). These results replicated my Experiment 1 findings and suggested that the more dissimilar the group in which an individual collaborated, the more likely that individual

Table 22

Correlations between Collaborators' Group EC Measures and Change Scores

		Change Score		
Relative Rank by Group EC		Gained	Lost	Net
Lower Partner				
	Group Ability	.05	-.31	.12
	Group Discrepancy	.21	.45*	.08
Higher Partner				
	Group Ability	.07	-.03	.09
	Group Discrepancy	-.16	.41	-.25

Note. Values represent Pearson coefficients. EC = Executive Control.
$* p < .045$

was to later forget items that they had previously remembered. This was particularly true for the relatively Lower ability partners. Overall, these results suggest that although the overall ability of a group in which an individual collaborated did not relate to their post-collaborative benefit, the difference between collaborating partners related to post-collaborative losses.

Which items were lost? In both Experiment 1 and the current experiment, my findings suggested that there was a differential loss of previously remembered items for Lower versus Higher ability partners. In Experiment 1, there was evidence to suggest that these Lost items were items that the Lower ability partners had offered themselves during the collaborative session, which suggested these participants did not benefit from relearning via retrieval to the same extent as their partners. To test whether this was the case in the current study, I classified each item according to how it was generated during collaboration: N*on-recalled, Self-recalled, or Partner-recalled*. Using this taxonomy, I calculated the proportion of each Lost item type (out of the number of initially recalled items) for each of the Lower and Higher partners in collaborating dyads (see Figure 32). I then performed a 2 x 3 mixed-model ANOVA on Collaborators' Lost item proportions with Relative Rank (Lower vs Higher) as a between-subjects factor and Item Type (Non-recalled vs Partner-recalled vs Self-recalled) as a within-subjects factor. Results indicated no significant main effect of either Relative Rank, $F(1,111) = 1.49$, $p = .224$, or Item Type, $F(2,111) = 0.14$, $p = .866$. The interaction between the two factors was marginally significant, $F(2,111) = 2.43$, $p = .093$. Given the marginal significance of the interaction and the fact that I found similar results in Experiment 1, I conducted a follow-up t-test to test whether Lower and Higher partners exhibited differential rates of losing Non-recalled items. Results suggested that Lower partners lost a significantly higher proportion of Non-recalled items than did Higher partners, $t(38) = 2.03$, $p = .049$.

I then performed correlational analyses for each Item Type to determine whether there was a relationship between these items and the discrepancy measure (see Figure 32). The relationship between Lower partners' Non-recalled Lost items and Group Discrepancy was marginally significant, $r(18) = .40$, $p = .079$. There was a medium, though not significant, correlation between Self-recalled Lost items and Group Discrepancy, $r(18) = .37$, $p = .113$. The relationship between Partner-recalled

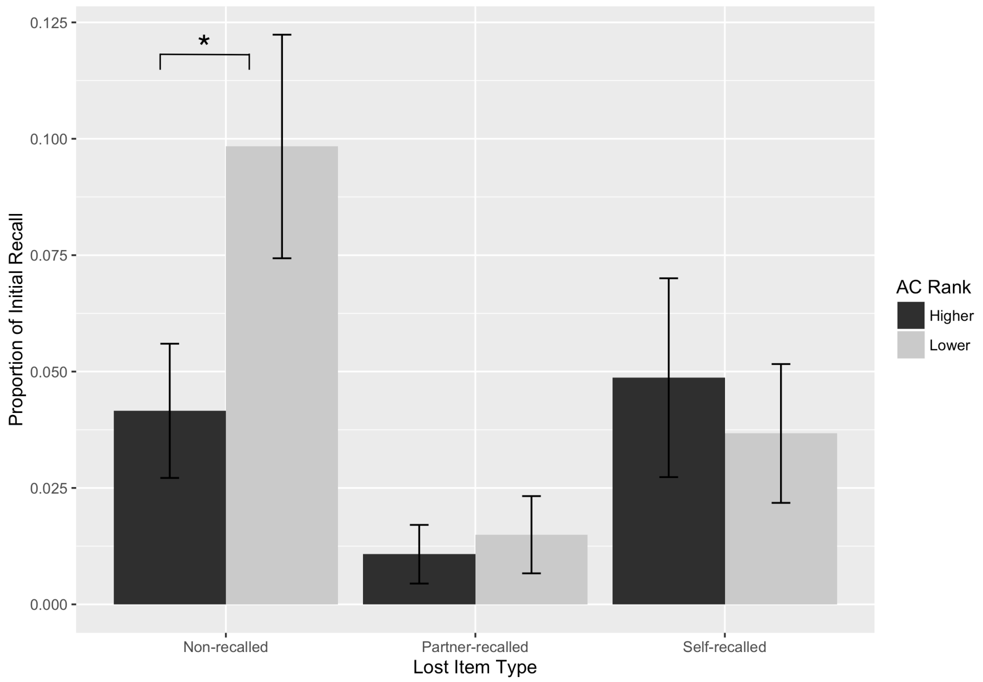

*Figure 32. Proportion of lost items as a function of collaborative generation. Bars represent SEM. *p = .049.*

Lost items and Group Discrepancy was also not significant, $r(18) = .02$, $p = .944$. Overall, this suggests that when working in dyads with relatively Higher ability partners, Lower ability partners were more likely than their Higher ability partners to lose items that they had initially remembered. Considering the trends in the data (see Figure 33), this may be partly related to how the items were generated at collaboration.

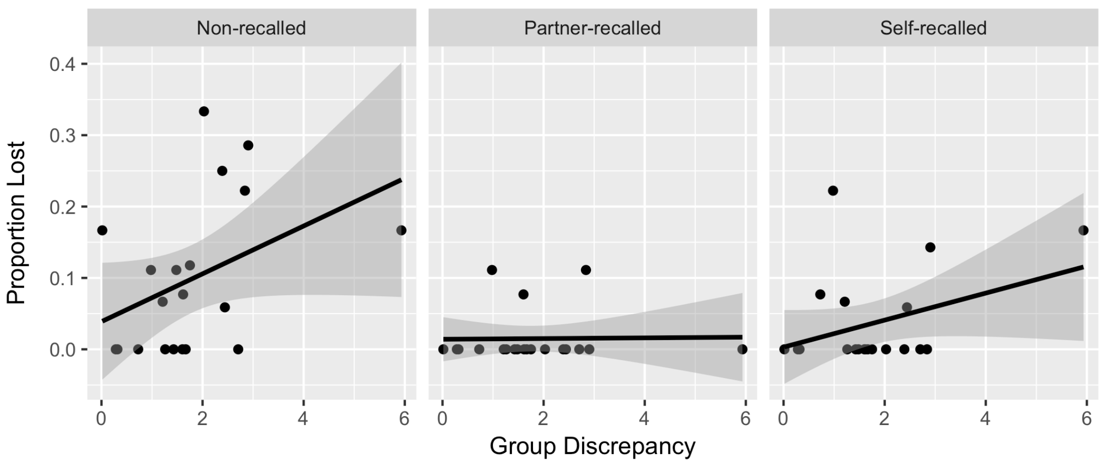

Figure 33. Lower partners' relationships between Group Discrepancy and Lost Items by how it was generated at collaboration.

Did retrieval strategies relate to post-collaborative benefits? To test whether individuals who reported using individual strategies experienced post-collaborative benefits that were different to those who reported not using strategies, I performed a 2 x 2 ANOVA on the Net change score data with Condition (Nominal vs Collaborative) and Strategy Use (No Strategy vs Strategy) as between-subjects factors (see Figure 34). Results indicated a significant main effect of both Condition, $F(1,76) = 29.41$, $p < .001$, $\eta^2_p = .33$, and Strategy Use, $F(1,76) = 11.68$, $p = .001$, $\eta^2_p = .13$. The interaction between the two factors was also significant, $F(1,76) = 11.62$, $p = .001$, $\eta^2_p = .13$. Follow-up t-tests indicated that Collaborators who did not use a strategy had significantly higher Net change scores compared to all other participants, all ts > 3.51, all ps < .002. These results mirrored my Experiment 1 findings and suggested that collaboration was most beneficial for individuals who did not employ individual retrieval strategies.

Individual and relative memory ratings. In the post-experimental questionnaire, I also collected data regarding how participants rated their own memory and also, in the case of Collaborators, how they rated their partner's memory. To test whether participants' ratings of their own memory ability were accurate, I performed a correlational analysis between Individual Memory Ratings and the EC scores for all participants. Results indicated a significant positive relationship where people with higher ability generally rated themselves as having higher memory ability, $r(78) = -.30$, $p = .007$ (see Figure 35). This suggested that, overall, individuals were accurate about how they perceived their own ability.

For Collaborators in my sample I also collected data regarding how the participants viewed their partners' memory ability. I then determined whether their relative ratings matched their actual Relative Rank. That is, did the Lower partners

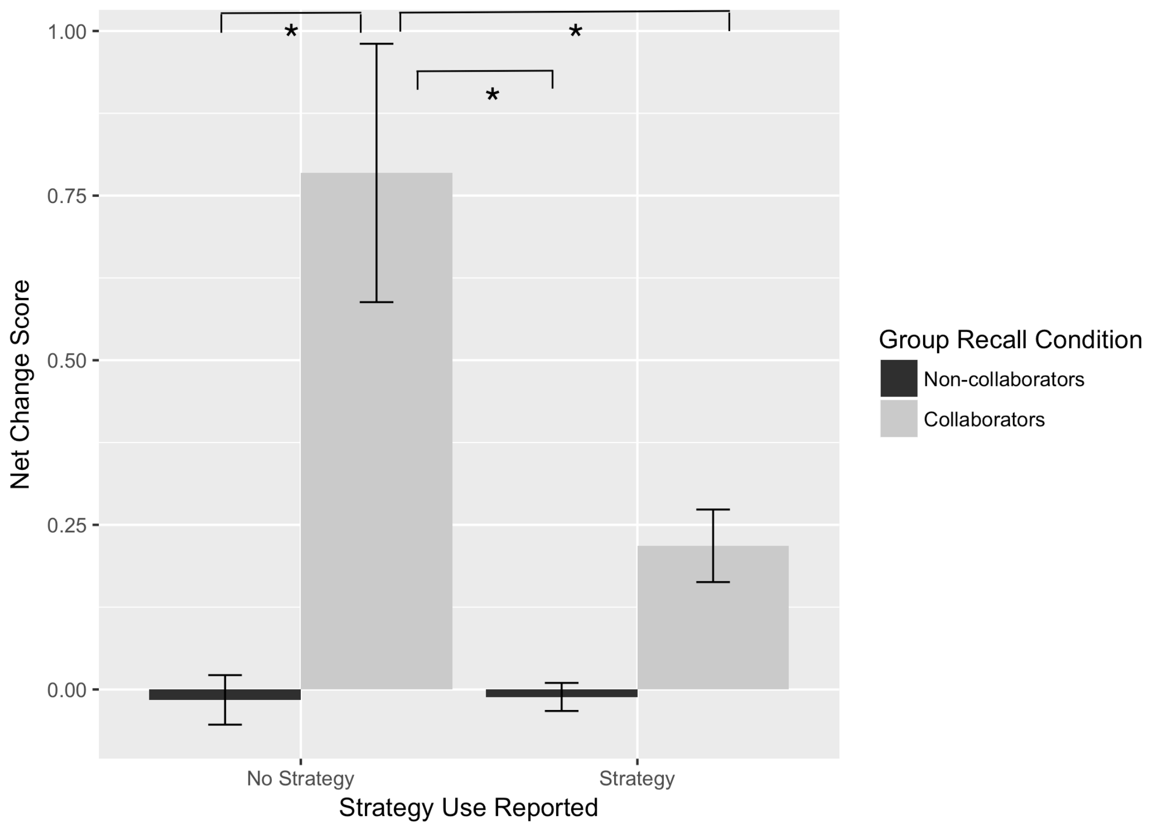

*Figure 34. Mean Net change scores as a function of strategy use and recall condition. Error bars represent SEM. *p< .05.*

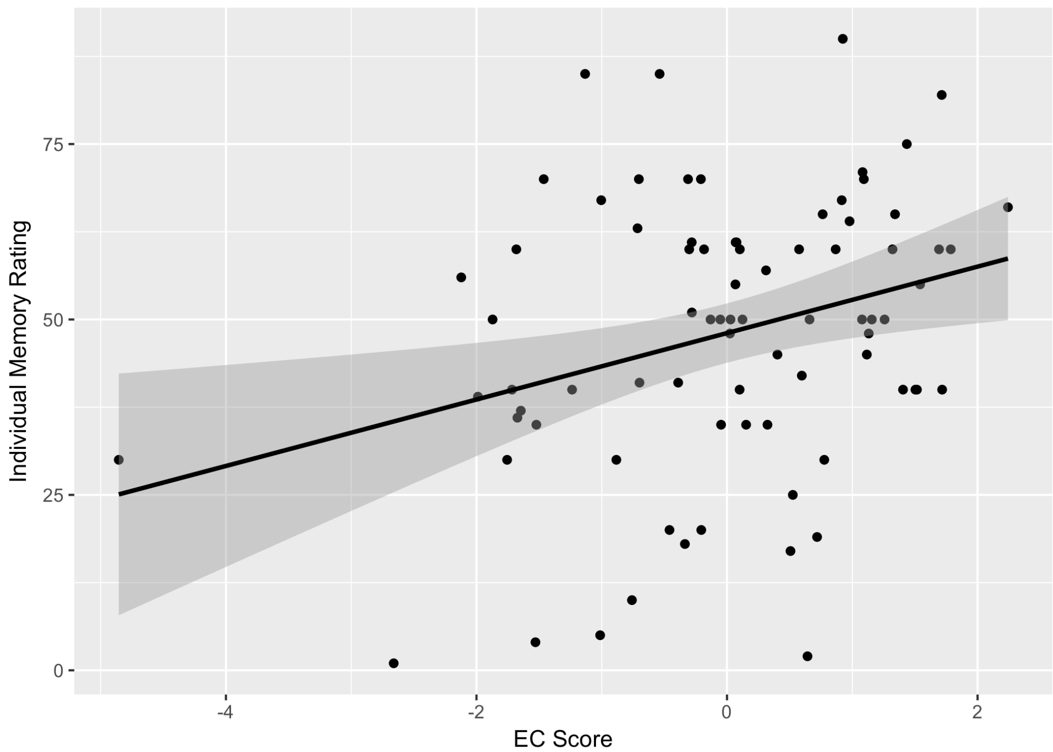

Figure 35. Individual self-memory ratings as a function of Individual Ability. Shaded area represents standard error.

rate themselves as lower and the Higher partners rate themselves as higher? I then calculated Match and No Match frequencies for each of the Lower and Higher ability partners (see Table 23). To test this frequency pattern, I conducted a Chi-square test of independence. The results indicated a significant effect such that the Lower ability partners were more likely to accurately judge memory ability, while the Higher ability partners were not.

Table 23
Participants' Memory Ratings Match Between Actual and Perceived Rank

Relative Rank	Match	No Match
Lower Partner	12 (75%)	8 (33%)
Higher Partner	4 (25%)	16 (66%)

Note. χ^2 = 5.10, df = 1, *p* = .024. Numbers in parentheses indicate column percentages.

Final individual recognition. I used a final recognition test to determine whether items had been lost due to retrieval blocking or to retrieval inhibition. If item loss was due to blocking, then there should be rebound and lost items would be as likely to be recognised as non-lost items. If item loss was due to inhibition, then there would not be rebound and lost items would be less likely to be recognised than non-lost items.

For each participant, I calculated measures of performance, which included Hit Rate, False-Alarm Rate, and Corrected Recognition. I also calculated measures of Discriminability and Response Bias. See Table 24 for recognition descriptive statistics.

To examine whether Non-collaborators and Collaborators differed in their ability to recognise previously studied items, and whether this was contingent on

Table 24
Recognition Performance Measures by Condition and Relative Rank (SD in Parentheses)

	Hit rate	False-alarm rate	Corrected recognition	A'	B''$_D$
Non-collaborators					
Lower Partner	0.53 (0.18)	0.13 (0.17)	0.40 (0.20)	0.80 (0.10)	0.51 (0.31)
Higher Partner	0.58 (0.17)	0.09 (0.08)	0.48 (0.15)	0.84 (0.06)	0.52 (0.34)
Collaborators					
Lower Partner	0.65 (0.14)	0.09 (0.08)	0.55 (0.17)	0.86 (0.07	0.51 (0.28)
Higher Partner	0.61 (0.16)	0.12 (0.11)	0.49 (0.17)	0.84 (0.07)	0.42 (0.32)

their Relative Rank within the collaborative group, I performed a 2 x 2 ANOVA on the Corrected Recognition data with Condition (Nominal vs Collaborative) and Relative Rank (Lower vs Higher) as between-subjects factors. Results indicated a significant main effect of Condition, $F(1,76) = 4.10$, $p = .046$, $\eta^2_p = .05$, but not of Relative Rank, $F(1,76) = 0.04$, $p = .851$. The interaction between the two factors was marginally significant, $F(1,76) = 3.57$, $p = .063$, $\eta^2_p = .04$. While the pattern of data was suggestive that Lower Collaborators had the highest Corrected Recognition scores, decomposition of the interaction indicated no significant differences, all *t*s < 1.43, all *p*s > .161. Thus, overall, Collaborators were better at subsequently recognising previously studied items than were Non-collaborators.

Recognition and Individual Ability. To investigate the relationship between Individual Ability and recognition performance, I performed correlational analyses between individual EC scores and the recognition measures. Overall, there was a weak relationship between EC and Corrected Recognition that was marginally significant, $r(78) = .20$, $p = .077$. Further, I observed a significant positive relationship between Antisaccade performance and Corrected Recognition, $r(78) = .28$, $p = .012$, whereas I found no relationship with the other EC measures. In addition, I also found a significant, weak relationship between EC and Discriminability (A′), $r(78) = .24$, $p = .034$, which, again, was driven by the Antisaccade component of the EC score, $r(78) = .32$, $p = .004$. The relationship between EC and Response Bias (B′′) was not significant, $r(78) = .11$, $p = .344$. Together, these results suggest that individuals with better executive control were somewhat better at recognising studied items and discriminating studied items from non-studied items than were individuals with poorer executive control ability. My findings also suggest that individuals' performance on the Antisaccade task was responsible for this relationship.

Recognition and group characteristics. To address a central aim of my study, I investigated whether the type of group in which an individual collaborated influenced their final recognition performance and whether this was influenced by their Relative Rank within the group. I performed correlational analyses between the Group Discrepancy and recognition measures (see Table 25). Only the relatively Lower Collaborating partners exhibited any notable relationship between Group Discrepancy and recognition performance. The relationships were all moderately negative, though marginally significant. However, this suggested that as the discrepancy within Collaborative groups increased, the Lower partners' ability to recognise studied item and discriminate between studied and non-studied items was diminished. That is, as discrepancy increased the Lower partners' recognition performance got worse. In addition, they also adopted less conservative strategies during recognition under these conditions. This makes sense as previous results suggested that these individuals also tended to lose more initially remembered items. This means that, not only did they lose more items following collaboration, but that they were less able to successfully recognise originally studied items. This was suggestive of an inhibitory phenomenon whereby these individuals' memory representations of initially remembered items was degraded following collaboration. I return to this point in the General Discussion.

Table 25
Correlations between Group Discrepancy and Recognition Measures for Collaborators

Relative Rank	Corrected recognition	A'	B"
Lower Partner	-.39[a]	-.41[b]	-.44[c]
Higher Partner	.11	.17	.11

Note. Values represent Pearson coefficients
[a]$p = .089$ [b]$p = .074$ [c]$p = .053$

Discussion

My major findings in Experiment 3 paralleled my findings in Experiment 1. Specifically, lost items were related to group discrepancy. That is, as the groups in which individuals collaborated became more dissimilar with respect to Individual Ability, the collaboration resulted in more lost items. Like Experiment 1, this relationship was significant only for the lower ability partners; although it must be noted that the correlation for higher ability partners was marginally significant ($p = .076$) with the magnitude of the relationship similar to lower ability partners ($r = .45$ for lower partners versus $r = .41$ for higher partners). Together, these results provided evidence of some relationship between the composition of groups in which individuals collaborated and how the individuals retained (or failed to retain) information following collaboration.

In Experiment 3 I also observed a significant, albeit weak, positive relationship between executive control ability and baseline recall for the first time in my program of research. Additionally, I detected a negative relationship between ability and baseline intrusions. Further, these relationships were driven by participants' performance on the Antisaccade task. Thus, the executive control battery appeared to be a more sensitive indicator of ability-related differences in individual recall. I also changed the stimuli in this experiment which may have contributed to my finding evidence for a relationship between individual ability and baseline recall. Uncategorised words are less amenable to explicit inter-item associative encoding than categorised words, and thus were likely better for dissociating individual performance according to ability. I also observed a relationship between baseline recall and reported individual retrieval strategy use, where individuals who reported using strategies recalled more than individuals who did not. However, the use of strategies was unrelated to individual executive control ability.

In Experiment 3, I did not observe the standard collaborative inhibition effect. As there were no differences between the Nominal and Collaborative conditions with respect to the group-level ability measures, I concluded that the composition of groups was not responsible for the absence of collaborative inhibition and that this finding was likely an artefact group size or stimuli.

At the post-collaborative stage, I observed the standard effects where collaborators exhibited superior performance to non-collaborators. Collaborators' superior performance, however, was not related to individual ability. These findings contradict earlier findings by Barber and Rajaram (2011a) where OSPAN scores were positively related to post-collaborative recall. However, that study utilised a two-recall methodology (i.e., no initial individual recall) and so direct comparison may be inappropriate as initial individual recalls can solidify participants individual retrieval strategies and make them less susceptible to the negative effects of collaboration (Blumen & Rajaram, 2009; Congleton & Rajaram, 2014). In the present study, collaborators recalled more than Non-collaborators on post-collaborative recall despite the fact they demonstrated greater losses. Their gains overshadowed losses to produce post-collaborative benefits. Moreover, these benefits were modulated by the characteristics of the collaborative groups. That is, group discrepancy was related to items lost. In addition, the relatively lower ability partners lost more Recall 2 Non-recalled items than higher ability partners. That is, lower ability partners were more susceptible to forgetting originally remembered items that did not appear at Recall 2 than were higher ability partners.

Another interesting pattern in my findings was that collaborators who reported using no individual retrieval strategies actually benefited the most from collaboration. In fact, these individuals exhibited almost a four-fold improvement over their strategy-using counterparts. This would indicate that, although they initially suffered by not employing a retrieval strategy, the benefits of collaboration are such that they experienced a substantial boost to post-collaborative recall. This is especially notable when compared to Non-collaborators, who demonstrated virtually no improvement over recall sessions. This makes sense in the context of a recent meta-analysis by Marion and Thorley (2016) who found that study materials that encouraged a high level of inter-item association generally yielded smaller post-collaborative benefits. This finding implies participants who encode and retrieve according to a specific strategy may experience fewer benefits (and vice-versa). This is exactly what I observed: strategy users experienced far fewer benefits than non-strategy users, which was consistent with the idea that there is “more to disrupt” when material is more highly organised.

I included a recognition test in this experiment with the goal of determining whether lost items where forgotten due to blocking or due to inhibition. Overall, collaborators were better at recognizing previously studied items than were Non-collaborators. Further, there was a weak relationship between individual ability and ability to discriminate between old and new items, where higher ability individuals had better discrimination ability. As with baseline recall, this relationship was primarily driven by performance on the Antisaccade task. I also examined the relationship between recognition and group-level executive control measures as a function of Relative Rank. It was only the relatively lower ability partners that demonstrated any notable relationships. As the groups in which they had previously collaborated became more discrepant, they exhibited poorer recognition, poorer discrimination, and more liberal response criterion. This indicated that when lower ability individuals collaborated with partners that had substantially better executive control ability, not only did they lose more initially remembered items, but they were worse at recognising studied items and discriminating between old and new items. This came despite being more liberal with their response criteria. Taken together, these results implied that losing items through collaboration may not have been a transient phenomenon, but persistent forgetting due to the inhibition of memories for the items.

I also included a relative memory rating in the post-experimental questionnaire in order to examine how participants perceived their partners. It appeared that the relatively lower ability individuals were more accurate in their perceptions relative to higher ability individuals. However, these results must be interpreted with caution as it could be the case that participants simply tended to be charitable when asked to make judgments about a partner and thus rated themselves lower relative to their partner.

Overall, the results of Experiment 3 suggested that individual ability did not exert influence during the collaboration itself. Neither did the interaction of abilities between collaborating group members. Further, individual ability alone did not appear to modulate post-collaborative performance. Individual ability only influenced post-collaborative benefits when considered in tandem with the ability of the partner with whom the individual collaborated. As such, Experiment 3 provides further evidence that researchers employing collaborative remembering paradigms should be wary of assuming that all groups are equal and that post-collaborative benefits are manifested only as a function of having collaborated or having not collaborated. Rather, the costs and benefits of collaboration are contingent not only on the individual, but also on the other person with whom that individual remembered.

Chapter 5

General Discussion

General Discussion

In this thesis, I investigated the role of executive control in collaborative recall. Most research has investigated the ways in which people interact (e.g., Barnier et al., 2017; Harris et al., 2014; Harris et al., 2011; Meade et al., 2009), or how parameters (for reviews see Marion & Thorley, 2016; Rajaram, 2011) influence collaborative recall. There are virtually no studies that have investigated how individual differences in group member abilities influence collaboration. Executive control ability is important as a potential influencer of collaborative recall. This is because it is closely linked with memory (Baudic et al., 2006; McCabe et al., 2010; Park et al., 2002; Park et al., 1996) and cognitive mechanisms related to interference and disruption (Cantor & Engle, 1993; Connelly et al., 1991; Kane & Engle, 2000). The scant research looking at individual executive control ability and collaborative recall suggests that it does not influence collaborative recall directly, but may indirectly influence individual post-collaborative recall by modulating pre-collaborative encoding (Barber & Rajaram, 2011a; Cokely et al., 2006). However, no research has directly examined how individual differences in ability, and how groups are composed in terms of ability, influence either group recall or subsequent individual recall.

In the program of work described in this thesis, I conducted a series of three experiments in which I sought to identify how the composition of groups influenced both collaborative recall and post-collaborative recall. I examined executive control ability in collaborative recall in three ways: 1) Individual Ability; 2) Group Ability; and 3) Group Discrepancy. In Experiments 1 and 2 I operationalised Individual Ability using the OSPAN. In Experiment 3, I operationalised Individual Ability using a component score that I derived from a battery of cognitive tasks. I operationalised Group Ability as the average ability of group members. I operationalised Group

Discrepancy as difference in ability between group members. Surprisingly, I found no evidence that group-level measures – either Group Ability or Group Discrepancy -- influenced collaborative recall. I did, however, find evidence that Group Discrepancy influenced what individuals took away from collaboration.

In Experiment 1, I found the typical costs and benefits of collaboration (Harris et al., 2008; Marion & Thorley, 2016; Rajaram & Pereira-Pasarin, 2010). There were costs during collaboration, such that collaborative groups recalled less than nominal groups. There were benefits after collaboration, such that collaborators recalled more than non-collaborators on post-collaborative individual recall. These post-collaborative benefits were not uniform, however, as the difference between an individual's ability and their partner's ability influenced the magnitude of post-collaborative benefits. Specifically, as the groups in which people collaborated became more discrepant, individuals lost more initially remembered items, diminishing the net post-collaborative benefits. Further, lower ability members drove this effect, and this effect was mostly due to the loss of items that the lower ability individuals had generated themselves during collaboration.

In Experiment 2 I aimed to build on my previous findings by using an extreme groups methodology. I selected participants based on their individual ability and assigned them to three kinds of dyads, that I established a priori with the aim of clearly distinguishing both Group Ability and Group Discrepancy. Using norms from Redick et al. (2012) I selected individuals in the top and bottom 33 percentiles and categorised them as *Highs* and *Lows*, respectively. I then assigned participants to dyads to create groups of three different compositions in terms of ability: *High-High, Low-Low,* and *High-Low.* Again, I found the typical costs and benefits of collaboration, such that collaborative groups recalled less than nominal groups, but collaborators recalled more than non-collaborators on a subsequent post-collaborative recall. However, the composition of the dyads was not related to any differences in collaborative recall. That is, while collaborative groups recalled fewer items overall relative to Nominal groups, there were no differences in recall between collaborative groups of different ability compositions. Thus, as in Experiment 1, the collaborative inhibition effect was present regardless of who was in the groups. There were post-collaborative benefits as collaborative groups improved over recall sessions whereas Nominal groups did not. Unlike Experiment 1, however, there was no relationship between Group Discrepancy and forgetting. Collaborators lost items at similar rates regardless of the composition of the groups in which they collaborated.

In Experiment 3 I aimed to address one potential shortcoming of Experiments 1 and 2. Namely, I was concerned that the OSPAN alone may not be a sufficiently sensitive indicator of individual executive control ability. To address this, I added two additional tasks to the executive control assessment, the Antisaccade and the Stroop task. By deriving composite EC scores through principal components analyses, I obtained individual measures that were more sensitive to ability than any one task alone, and for the first time in Experiment 3 I identified a relationship between baseline individual recall and executive control ability. While I did not observe the traditional collaborative inhibition effect in Experiment 3, I did observe the typical post-collaborative benefits where collaborators recalled more on post-collaborative recall than non-collaborators. Similar to Experiment 1, the more the abilities between collaborators diverged, the fewer benefits the lower partners subsequently realised. Again, this was due to losing a disproportionate amount of initially remembered items. In Experiment 3 I also administered a final recognition test to assess how persistent any forgetting may have been and to illuminate the cognitive mechanism involved. As for recall, the lower partners demonstrated a negative relationship between Group Discrepancy and recognition performance measures. This implied that, not only did lower ability partners lose items after having collaborated with substantially higher ability partners, but they were subsequently poorer at recognising them.

What Did Individuals Bring Into the Group?

Overall, the participants in my three experiments were similar in ability to what one would expect. That is, they were similar to available normative data (Redick et al., 2012). In Experiment 3 I derived a component executive control score intended to measure a latent underlying ability construct (see Hutchison, 2007; McCabe et al., 2010, for more on the use of this component score). In this experiment, I found that the component score related to baseline recall where higher ability individuals performed better than lower ability individuals. Additionally, the component score related to baseline intrusion rates where lower ability individuals demonstrated higher intrusion rates. This means that high ability individuals were initially better at both accurately recalling and avoiding inaccurate recall compared to low ability individuals. This is in line with previous research that suggests working memory capacity relates to levels of correct recall (e.g., Unsworth et al., 2013) and false recall (e.g., Unsworth & Brewer, 2010; Watson, Bunting, Poole, & Conway, 2005). Likewise, other research has suggested that inaccurate memory to be associated with lower frontal lobe functioning (i.e., lower executive control; Butler, Mcdaniel, Dornburg, Price, & Roediger, 2004).

I also asked participants to report on whether they used strategies to guide their individual retrieval. Participants who reported using strategies at baseline recall performed better than those who did not report using strategies. Strategy use is an effective meta-memory technique, and there are reports in the literature that it can vary according to individual differences in executive control (e.g., Cokely et al., 2006; Unsworth et al., 2013). There was no evidence in my studies, however, that participants who used strategies were different in their individual abilities than participants who did not. While strategy use helped people recall more items, it was not a technique employed by individuals with any particular characteristics. Thus, I concluded that individuals who used strategies in my sample were heterogeneous in their ability. This may be encouraging from a practical standpoint, as it implies that strategy is not necessarily a tool employed only by high ability individuals and strategy use has benefits for individuals across ability levels.

Overall, individuals came into these studies with an array of abilities. When assessed with a sensitive measure, these abilities predicted how well people performed, both in terms of overall recall and accuracy of recall. Though strategy-users generally performed better than non-strategy users, they were not distinguished by their executive control ability. From the standpoint of baseline assessment, it appeared that the characteristics of people did not strongly influence their performance when they were working alone. The central focus of my thesis, however, was how peoples' abilities would coalesce in groups to potentially influence group memory. Then when the groups split up, how would peoples' abilities and peoples' experience within the groups influence what they retained?

How Did Individuals Perform in the Group?

At group recall, individuals demonstrated the robust pattern of costs and benefits of collaboration identified within the collaborative recall literature (see Marion & Thorley, 2016; Rajaram & Pereira-Pasarin, 2010). Though collaborative groups recalled less than Nominal groups, they were more accurate in the information that they did recall. My program of research was predicated on the idea that the individual abilities of people within collaborating groups may influence these collaborative costs and benefits. Although there was no research that directly addresses the role of abilities within collaborating groups, there were previous findings within the literatures of part-set cueing (e.g., Cokely et al., 2006), collaborative recall (e.g., Barber & Rajaram, 2011a) and individual differences related to retrieval and interference (e.g., Brewer & Unsworth, 2012; Kane & Engle, 2000) suggesting that such individual differences may be important. Thus, it was surprising that I did not observe any influence of executive control during collaboration. Across all three experiments I found no evidence that the composition of groups with respect to executive control ability was related to how much the groups recalled. Even when looking at individuals' relative ability within dyads (i.e., splitting them into higher and lower ability distinctions), there were no differences in recall, such that higher and lower ability individuals both contributed similar amounts to the collaborative recall and there was no evidence that one individual dominated. Executive control has been shown to be positively correlated with the part-set cuing deficit, where high ability individuals are more disrupted in the presence of cues than low ability individuals (Barber & Rajaram, 2011a; Cokely et al., 2006). The part-set cuing is often viewed as an analogue of collaborative recall in that the words offered by a partner during collaboration act as disruptive retrieval cues in the same manner that providing participants with a subset of previously studied words during recall also disrupts retrieval and lowers total amount recalled (Bäuml & Aslan, 2006; Nickerson, 1984; Roediger & Neely, 1982). Thus my findings are inconsistent with these previous literatures and provide a point of difference between collaborative recall and part-set cuing. My findings imply the possibility that collaboration can compensate for weaker individual performance. Even when lower ability individuals perform worse on individual recall (such as Recall 1, Experiment 3), their performance may be bolstered by collaboration such that during collaboration, the effects of ability on amount recalled disappear.

Like group recall levels, executive control did not appear to influence rates of group intrusions either. Even in Experiment 3 in which ability predicted baseline intrusion rates, there were no differences in intrusions that were attributable to group-level or individual-level executive control measures. Thus, although recalling with others generally hindered peoples' ability to recall, it had the positive effect of making them less prone to errors. Further, even when people demonstrated ability-modulated performance, their abilities did not appear to have the same influence when they collaborated. This is important because it means that group remembering scenarios in which production and accuracy of recall are critical are not subject to differences in the executive control abilities of the people in the group. That is, we should expect groups composed of all types of people (at least with regards to executive control ability) to demonstrate equivalent performance and accuracy.

Although the executive control abilities of collaborative group members did not influence collaborative recall, there were individual differences that influenced memory at this stage. I asked participants to report on their use of retrieval strategies during collaboration. In some groups, the members agreed on whether a group strategy was used while in others members disagreed. It would appear that when there was disagreement between dyad members, performance suffered. The groups in which one member reported that the group used a strategy and one member reported that the group did not use a strategy recalled fewer items collaboratively than groups in which both dyad members reported strategy use. The dyads in which members disagreed appeared to perform worst overall, as the numerical trend in the data further suggested that these groups had lower recall than even groups in which both members agreed that a strategy had not been used. Interestingly, these disagreeing groups were also the most discrepant with respect to dyad member executive control ability. Further, within the disagreeing groups, it was always the lower ability partner who said that a strategy had not been used while it was always the higher ability partner who said that a strategy had been used. This finding is intriguing considering that individual ability was not related to individual reported strategy use, or to contributions to the collaborative recall. This implies that perhaps there were ability-related differences in whether participants recognised that their partner was using a strategy and suggests that strategy use benefits collaborative recall, but only if both collaborating parties agree on a using one. Further, in order to maximise the benefit potential, the strategy use must be made explicit especially to relatively lower ability group members who may not adopt collaborative retrieval strategies organically. Indeed, findings in the literature would suggest that agreement on an explicit retrieval strategy benefits collaborative recall (e.g., Harris et al., 2011).

What Did Individuals Take Away From the Group?

Much of the previous research on collaborative recall agrees that people who collaborate show benefits because they remember more information following collaboration compared to people who do not collaborate (Barber & Rajaram, 2011a, 2011b; Blumen & Stern, 2011; Choi et al., 2014; Marion & Thorley, 2016; Weldon & Bellinger, 1997). My findings across three experiments support this: Collaborators' recall improved from initial individual recall to final individual recall whereas non-collaborators did not. While the executive control of groups did not relate to how well the groups performed, there was an indication that it influenced memory downstream following collaboration. Specifically, group discrepancy was related to the post-collaborative benefits typically observed in collaborative recall studies. In two of my three experiments, I found evidence that these post-collaborative benefits were contingent on the executive control composition of groups. People who collaborated with partners that were more discrepant to themselves tended to get fewer benefits than people who collaborated with partners that were more similar to themselves. This was because these individuals forgot more items that they initially remembered. While forgetting is not uncommon in collaborative recall (Basden et al., 2000; Finlay et al., 2000; Henkel & Rajaram, 2011), these results are interesting as they indicated that the loss of information was not uniform within the dyads. In both Experiment 1 and Experiment 3, it was the relatively lower ability partners who drove the relationship and, in Experiment 3, it was the items that they had generated themselves during the collaborative session that were being lost.

I speculated that perhaps lower ability partners were “dumping” their own items in favour of attending to their partners’ items. It was not apparent, however, whether this was a deliberate process or an automatic one. I attempted to adjudicate between the two possibilities by administering a recognition test and a post-experimental questionnaire in Experiment 3. Unfortunately, there were too few lost items in Experiment 3 to analyse them at recognition. Examining the recognition data overall, however, suggested that lower ability partners were less likely to recognise originally studied items after having collaborated in discrepant groups versus non-discrepant groups. This pointed to a persistent loss of items for lower ability partners which is consistent with an inhibitory process (see Barber et al., 2014). Further, these recognition failures came concurrent with diminishing response criterion, meaning that even when the lower ability partners were less disciplined in responding ‘old’ to originally studied items, they still performed at a lower level.

While collaboration generally led to increased post-collaborative recall, it also led to more accurate recall. In Experiment 2, this was even more striking considering that as collaborators become more accurate from Recall 1 to Recall 3, non-collaborators became more inaccurate. Reductions in intrusions are often a benefit of collaboration (Congleton & Rajaram, 2011; Harris et al., 2012, 2013; Henkel & Rajaram, 2011; Rajaram & Pereira-Pasarin, 2010; Ross, Spencer, Blatz, & Restorick, 2008), particularly for methodologies in which participants are free to provide error correction for each other during collaboration rather than when turn-taking is imposed (Harris et al., 2012). My findings in Experiment 2 indicated that not only did collaborators get accuracy benefits from collaborative error-pruning, they also avoided increases in intrusions that affected non-collaborators. Because people in nominal groups did not actually interact with each another, their increases in intrusions cannot be attributed to social contagion (Roediger et al., 2001). However, there is evidence that false memories can be exacerbated if not corrected by external influences (McDermott, 1996). This occurs when false recall increases over repeated testing, which is similar to participants in my nominal condition recalling three times alone.

Overall, collaborative error-pruning was not influenced by group-level executive control ability. This is a positive finding as it suggests that collaboration can result in error pruning, and that this is the case regardless of the individual executive control ability of group members. Further, the social contagion that can often affect collaborations (Rajaram & Pereira-Pasarin, 2010) may be offset and even outweighed by the effects of error-pruning (Rajaram & Pereira-Pasarin, 2010).

Similar to my finding that group strategy use influenced collaborative recall, individual strategy influenced post-collaborative benefits. In Experiment 1, collaborators who reported that they did not use individual retrieval strategies benefited more from collaboration than collaborators who reported that they used individual retrieval strategies. This finding was supported and expanded upon in Experiment 3 where I observed that people who initially did not use a strategy showed substantial gains over the course of recall sessions. Specifically, people who did not use a strategy when alone improved 78% on average. This is striking, particularly when compared to their non-strategy-using counterparts who demonstrated a 22% increase. It suggests that individuals not predisposed to use advantageous retrieval strategies may still overcome poorer initial recall through the experience of collaborating.

To summarise, the results from the three experiments of my program of research demonstrate the first steps toward a better understanding of how individual abilities within collaborating groups influences the outcomes of collaboration. Although the influence of collaborators' abilities did not necessarily manifest during the actual act of collaboration, the effects appeared downstream during individual post-collaborative recall. The fact that differences between group members' abilities influenced what they took away from collaboration suggests that not all collaborators are affected the same. Further, peoples' performance may not simply be at the mercy of pre-existing, and relatively unchangeable abilities, but may be modified and improved through the use of adaptive strategies. Now I will take a closer look at the individuals for whom benefits diminished and why they have diminished.

Attenuated Post-Collaborative Benefits

My findings suggest that collaboration does not have similar benefits for all kinds of individuals remembering in all kinds of groups. While collaborative recall may lead to an overall benefit where, on average, individuals remember more following collaboration, there may be individuals “slipping through the cracks” for whom such post-collaborative benefits do not occur. Within dyads, the lower ability partners showed diminished post-collaborative benefits relative to the higher ability partners with whom they collaborated. This pattern was exacerbated as the discrepancy between partners’ abilities increased. That is, when the lows got lower and the highs got higher, the lows suffered. Further, this pattern only existed for lost items, and was such that the increased loss of initially remembered items was great enough to partially overshadow the post-collaborative benefit of gaining new items through re-exposure.

Why were the lower ability partners losing items? I considered two possibilities: (1) that the loss was due to an automatic, non-deliberate process; or (2) that the loss was due to an deliberate, meta-memory process. Since the negative effects of retrieval disruption during collaboration are argued not to carry over to subsequent individual recall (Marion & Thorley, 2016; Rajaram, 2011; Rajaram & Pereira-Pasarin, 2010) the likely automatic candidates were retrieval blocking (e.g., Rundus, 1973) or retrieval inhibition (e.g., Bäuml & Aslan, 2004). If blocking was to blame, then compromised access to the lost items should be only transient and the items should be available to be recognised on a subsequent recognition test (Rundus, 1973). If inhibition was to blame, then there would be the items would be suppressed to the point where they would not be recognised and forgetting would be persistent (Barber et al., 2014; Bäuml & Aslan, 2006). I administered a final recognition test in Experiment 3 with the aim of delineating between these two possibilities. Though the low numbers of lost items precluded analysis of the lost items only, the recognition data did suggest that the relatively lower ability partners who collaborated in discrepant groups were less likely to recognise originally studied items in general.

These findings suggest the possibility of an inhibitory mechanism, but given that many of these unrecognised items were never initially recalled, future research with different stimuli is needed to fully answer this question.

The potential meta-memory explanation is that lower ability partners recognised that their higher ability partners had superior ability and so prioritised their attention toward their higher partners' output during collaboration. This might explain why it was the lower partners' self-generated items that drove the relationship between discrepancy and lost items. To examine this possibility, I asked collaborators to rate their memory ability relative to their partner's memory ability. These relative ratings provided a glimpse into how collaborators perceived themselves within the dyads. I found that lower partners were accurate regarding their perception whereas the higher partners were not. This supports the notion that there may have been a deliberate attempt by the lower partners to attend more to their partners items than their own, which could have led to persistent forgetting of their own, previously recalled items. However, higher partners also rated their partner's ability as higher than their own. Thus, I might have expected a similar loss of self-generated items for the higher partners, but this was not the case.

It is possible that, regardless of ability, participants were simply averse to rating themselves as superior to their partners; or it is possible that ability discrepancies were more salient to lower ability partners than to higher ability partners and thus made lower ability partners more accurate in their assessments. Ultimately, it was likely a combination of automatic and deliberate processes that were responsible for the lost item patterns.

Overall, these results could be concerning from the standpoint of conventional views of post-collaborative benefits. If post-collaborative benefits are experienced to a higher degree by individuals already functioning at a high level and these benefits come at the expense of individuals functioning at a low level, then perhaps the benefits are not worth the costs. There are certainly practical implications for this, and I will discuss those in the next section.

Implications

The findings from my three experiments suggest that post-collaborative benefits in the form of increased performance are not uniform and that they may vary depending on the abilities of people within the collaborating groups. That is, the composition of collaborative groups can influence the benefits collaborators experience following collaboration. This has important theoretical and practical implications and I will discuss these in turn.

Implications for the collaborative recall model. The findings from my PhD program of research inform specific aspects of the Rajaram and Pereira-Pasarin (2010) model of collaborative recall. My findings suggest that, though there was no demonstrable net effect of executive control ability during collaboration, there were processes occurring during collaboration that exerted subsequent influence on post-collaborative recall and these processes were differentially impacted by the composition of groups in terms of participant ability. I will discuss each of these processes in turn.

Did ability influence retrieval disruption? Retrieval disruption is the mechanism generally accepted as the cause of collaborative inhibition (Basden et al., 1997; Marion & Thorley, 2016; Rajaram, 2011). Retrieval disruption occurs due to a lack of alignment between collaborators' idiosyncratic organisation of to-be-remembered information. This negatively affects the recall of each person in the collaborating group which negatively affects overall group output (Basden et al., 1997). Thus, when retrieval disruption is present there is collaborative inhibition (i.e., lower recall for collaborative groups versus nominal groups). Unlike, other costs of collaboration (i.e., retrieval blocking and retrieval inhibition), the effects of retrieval disruption are limited to the collaborative session. Thus, there is typically a “rebound” at post-collaborative recall when individuals are free to use their own organisational retrieval strategies again (Rajaram & Pereira-Pasarin, 2010). In two of my three experiments, I observed collaborative inhibition, which indicates that retrieval disruption occurred during collaboration. However, I found no direct evidence to suggest that individual ability influenced levels of retrieval disruption.

Precisely examining the role of individual ability on retrieval disruption is problematic as collaborative recall is measured at the group level whereas individual ability is measured at the individual level. As such, the two measures cannot be directly related. To address this, I indirectly investigated this relationship via the group level ability measures. I found no direct evidence to indicate that the group-level measures influenced retrieval disruption as there were no relationships between the continuous group measures (Group Ability and Group Discrepancy, Experiments 1 and 3) and Recall 2. Likewise, in Experiment 2 there were no differences in Recall 2 performance between the pre-selected groups’.

In Experiment 1, I found that collaborations in which partners disagreed on strategy use were the most discrepant. Within these groups it was always the lower ability partner who reported that a strategy had not been used. As the lower ability partners were the individuals driving the relationship between group discrepancy and lost items, it was possible that there was some form of ability-related retrieval disruption occurring within these groups. However, if this was the case then then lower ability partners should have demonstrated lower collaborative contributions than higher ability partners. I found no pattern to support this and thus concluded that retrieval disruption was equally disruptive to people of all abilities.

Did ability influence retrieval blocking or inhibition? Collaborative costs due to retrieval blocking and inhibition persist into post-collaborative recall (Andersson et al., 2006; Barber et al., 2014; Hyman et al., 2013; Rajaram & Pereira-Pasarin, 2010). Generally, these costs are outweighed by the benefits of re-exposure and re-learning via retrieval such that there is a net positive effect and previous collaborators recall more than previous non-collaborators, demonstrating post-collaborative benefits (Marion & Thorley, 2016; Rajaram & Pereira-Pasarin, 2010). However, even if collaborative net effects are positive, loss of previously remembered information is still theoretically interesting as it is informative regarding specific collaborative mechanisms (Rajaram & Pereira-Pasarin, 2010). In Experiments 1 and 2, I found that participants who collaborated lost a greater proportion of initially remembered items than participants who did not collaborate. This loss of items following collaboration could have been due to retrieval blocking or retrieval inhibition (Barber et al., 2014; Hyman et al., 2013; Marion & Thorley, 2016; Rajaram & Pereira-Pasarin, 2010; Rundus, 1973). The retrieval blocking account suggests that words offered by an individual's partner during collaboration may block access to other words, rendering the other words inaccessible (e.g., Rundus, 1973). Thus, these words are less likely to be later recalled than other words which were accessible during collaboration. On the other hand, retrieval inhibition suggests that the words offered by an individual's partner during collaboration may actually suppress unrecalled words making them unavailable to be retrieved (e.g., Bäuml & Aslan, 2004). Because the memory representation is supressed and unavailable for retrieval, this mechanism leads to more persistent forgetting than retrieval blocking. For this reason retrieval inhibition should be evident as reduced performance on a recognition test, whereas retrieval blocking should not reduce recognition performance (e.g., Barber et al., 2014) .

I found evidence that ability influenced blocking and/or inhibition as collaborators repeatedly demonstrated higher levels of item loss (forgetting) than did non-collaborators. This has been shown in previous research (Basden et al., 2000; Finlay et al., 2000; Henkel & Rajaram, 2011), but in this thesis I extended this finding as I observed that forgetting was related to both individual ability (Experiment 3) and group-level ability measures (Experiments 1 and 3). As mentioned, these losses could be attributed to either blocking or inhibition as both would produce forgetting on post-collaborative individual recall (Bäuml & Aslan, 2006; Hyman et al., 2013; Rajaram & Pereira-Pasarin, 2010). However, I found that lower ability partners exhibited poorer recognition performance following collaboration in discrepant groups. This suggested that, for these individuals, persistent forgetting was a product of retrieval inhibition and would suggest that ability may play a role in modulating retrieval inhibition.

Did ability influence reexposure or relearning via retrieval? Collaboration can benefit subsequent individual recall as it serves as both another opportunity for study when individuals hear their partners output (i.e., reexposure) as well as another opportunity for testing (i.e., relearning via retrieval; see Rajaram & Pereira-Pasarin, 2010). Repeated retrieval attempts often benefit memory in general (i.e., the testing effect; see Karpicke & Roediger, 2007; Payne, 1987), and so relearning via retrieval would be expected to result in increased recall across tests for members of both nominal and collaborative groups. However, in the collaborative recall paradigm re-exposure benefits are unique to collaborative groups as nominal groups do not get the opportunity to hear output from other individuals. These re-exposure benefits can be observed during post-collaborative recall where former collaborators exhibit superior recall to former non-collaborators (e.g., Blumen & Rajaram, 2008; Blumen et al., 2014; Congleton & Rajaram, 2011). Thus, the benefits people get from collaboration become evident after they have collaborated.

In all of my experiments participants who collaborated, relative to those who did not, showed clear post-collaborative benefits due to reexposure, as evidenced by greater gains for collaborators compared to non-collaborators. Participants who had the opportunity to collaborate tended to “pick up” new items from their fellow collaborators which allowed them to increase their recall overall, despite the fact that they were concurrently forgetting more items than non-collaborators. These gains, however, were unrelated to executive control ability. This was true for both the group-level executive control measures as well as the individual ability measure. If ability had influenced either reexposure or relearning via retrieval, then I should have observed a relationship between participants’ gained items and the executive control measures. I found no such relationships across my three experiments, which was surprising as previous research has suggested a role for executive control in modulating post-collaborative benefits whereby participants with higher working memory showed greater post-collaborative performance (Barber & Rajaram, 2011a). However, since the focus in that study was not on the source of post-collaborative benefits and there was no breakdown of recall into items gained and lost, it is possible that higher ability participants simply lost fewer items rather than gained more items due to the influence of reexposure or relearning via retrieval. My results suggest that the benefits of re-exposure and relearning via retrieval are similar across individuals and collaborating groups of varying abilities.

The fact that the Antisaccade task was the main driver of the relationships between ability and performance was interesting. It suggests two things: 1) The disruption inherent to collaboration may not be due to individuals' inability to maintain their own organisation of material, but rather to an inability to inhibit the distracting information provided by a partner; and 2) The Antisaccade task may be a sufficient measure to use in research investigating EC and collaborative recall. The first point may be unsurprising, but still adds to the literature by further restricting the parameters and influencers of collaborative inhibition. The second point is useful from a methodological standpoint as it suggests that in future studies one could use only the Antisaccade task to measure executive control.

In sum, executive control did not appear to modulate the positive aspects of collaboration, but rather the negative aspects. Specifically, individual ability related to the degree to which items were lost following collaboration, such that lower ability individuals were more likely to forget initially remembered information. Further, this pattern of forgetting was increased as lower ability individuals collaborated with partners whose abilities increasingly exceeded their own, somewhat diminishing post-collaborative benefits for these individuals. Moreover, there was some evidence that this was brought about predominately by differences in the impact of retrieval inhibition rather than by blocking or retrieval disruption, as ability impacted post-collaborative recognition performance.

Implications for education. The implications from my findings may be applied to real-world scenarios in which group remembering is common. Academic coursework often demands collaborative learning and recall and students are often grouped according to ability (for a review of cooperative learning see Johnson & Johnson, 2009). Whether the groups are composed systematically or organically, if polarised group compositions were to cause detriments to students of lower abilities, then this could offset any gains rendered by cooperative learning. Indeed, my findings would suggest that when lower ability people remember with higher ability people, they do not benefit to the same extent as their higher ability partners. These attenuated benefits are further compounded by the fact that it might be the information that lower ability people had once possessed that is susceptible to loss. This means that not only are lower ability people subject to the disservice of losing out on potential gains, but they are losing information that they would have remembered had they not been put into these types of groups.

This pattern of information loss may not be inevitable, however. There is research that points methods that may benefit lower ability individuals. For example, Agarwal et al. (2016) observed a differential benefit of retrieval practice for students with lower working memory capacities. Here, students who were given the opportunity to repeatedly retrieve studied information showed greater levels of recall than those who only restudied items. On a delayed test two days later, students with lower working memory capacity showed greater benefits from the repeated retrieval sessions than did higher working memory capacity students. Applied to collaborative recall, this would imply that attenuated post-collaborative benefits for lower ability individuals might be ameliorated if these individuals engage in repeated collaborations in which they would have more opportunity for relearning through retrieval. Previous research supports this notion as well by showing that individuals with poorer memory and lower intelligence benefit more from retrieval attempts than individuals with better memory and higher intelligence (Unsworth et al., 2012). The collaborative recall literature also supports this idea as research indicates that repeated retrieval attempts benefit collaborative and post-collaborative recall (e.g., Blumen et al., 2014; Congleton & Rajaram, 2011). Of even greater benefit are those in which retrieval attempts occur collaboratively (e.g., Blumen & Rajaram, 2008). This is particularly encouraging if it means that not only would all parties benefit from multiple collaborations, but that lower ability individuals might benefit to the extent that they achieve recall performance equal to their higher ability counterparts. At the very least, their gains should offset their losses to a greater extent than evident in my research in which I used only a single collaborative recall.

Implications for memory and ageing. As I discussed in my introduction, there are significant implications for collaborative recall and age-related memory declines, especially with respect to aged couples and spouses (see Blumen et al., 2013). Research points to the fact that much of age-related memory declines can be attributed to declines in executive control (Bouazzaoui et al., 2014; McCabe et al., 2010). Other research also suggests that cognitively-compromised partners can benefit from the scaffolding provided by a healthier partner (Rauers, Riediger, Schmiedek, & Lindenberger, 2011). However, are they benefiting as much as they potentially could? My findings suggest that perhaps they are not. Extrapolating from my findings, it might appear that the partner experiencing declines is benefiting because they remember more on their own following collaboration with their spouse than they would if they had not collaborated. However, if these gains are small and come at the cost of the compromised individuals losing memories that they were unlikely to lose on their own, then these gains may not be worthwhile.

As in educational settings, there may be ways to mitigate attenuated benefits for lower ability individuals. Strategies such as division of responsibility (e.g., Johansson et al., 2005) and development of explicit retrieval strategies (e.g., Harris et al., 2011) can help to ameliorate some of the detrimental effects of collaboration. My findings also suggest that strategy use benefits individual recall and that collaboration can boost benefits for those not predisposed to using strategies. Thus, there may be a reciprocal relationship between strategy use and collaboration whereby strategy users are better collaborators and collaboration can help people who do not use strategies.

As previously noted, repeated collaborations also benefit individual memory (Blumen & Rajaram, 2008; Congleton & Rajaram, 2011) and these repeated retrieval events may benefit people suffering cognitive decline to a greater extent than healthy people (Agarwal et al., 2016). This is encouraging as it suggests that perhaps cognitively-compromised older adults specifically (and people with lower executive control generally) are better influenced by the positive effects of repeated collaboration. From the standpoint of my research, I would expect that perhaps repeated collaborations, compared to a single collaboration, would strengthen rather than weaken existing memories and decrease the probability that they would be forgotten by lower ability individuals working with higher ability individuals. Further, the strategies used by couples (e.g., Harris et al., 2011; Johansson et al., 2005) who are often experienced and skilled collaborators (Barnier et al., 2014; Harris et al., 2014) would likely further scaffold cognitive discrepancies, especially when considering that the goals of these individuals go beyond simple word list generation (Harris et al., 2014; Harris et al., 2011; Wegner et al., 1991).

Limitations

One of the main goals of my thesis was to investigate how peoples' inherent abilities influence memory performance when they come together to remember in groups. This individual differences approach can be problematic when studying collaborative recall as individual ability measures do not readily map on to group performance measures. That is, individual ability is defined at the level of the individual, whereas group performance is a measure of the pooled performance of two (or more) individuals. I addressed this issue by operationalising ability using two kinds of group-level ability indices that captured different information about the composition of groups in terms of the abilities of the individuals within them. By calculating a Group Ability measure, I aimed to quantify the overall ability of a group. By calculating a Group Discrepancy measure, I aimed to quantify the heterogeneity of abilities within a group. Both of these measures were influenced by the individual abilities that people bring into the groups, but they measure different things. For example, a low discrepancy group, in which members were similar in ability to one another, could have been composed of two high ability individuals or two low ability individuals. While these two groups are similar with respect to the disparity in group member ability, there are dissimilar with respect to the individuals' abilities within the groups. At times I analysed the data according to the relative ability of the individuals in groups. While likely a reliable distinction when the group members are sufficiently different, it becomes arbitrary as the group members become more similar. A dyad in which members scored 46 and 47 on the OSPAN would have a discrepancy score of one. This score is meaningful as it indicates that the members have similar ability. However, distinguishing them as low or high in this instance is arbitrary.

As I have noted before there is difficulty in integrating individual analyses and group analyses. This is a notable problem with, though not unique to, collaborative recall studies. In my first two experiments, I used the OSPAN as an indicator of executive control. The OSPAN is designed to be a measure a latent theoretical construct and, like any psychometric instrument, is imperfect and task impure (Conway et al., 2005; Luszcz, 2011). While an often-used and convenient metric, the

OSPAN is certainly not a definitive indicator of working memory. For example, Conway et al. (2005) found that 27% of participants in a sample were misclassified into the wrong quartile when only one complex span task was used versus when a composite of three tasks was used. While it is ideal to use multiple tasks to measure a latent construct, these tasks can be long in duration and this is not always feasible, which was the case in my first two studies. Ideally, if one were attempting to precisely (or precisely as possible) measure executive control, the OSPAN would be only one of several tasks employed. Taking an imperfect measure of individual ability and attempting to extrapolate it to a measure of the individual ability of dyad members certainly introduces a less-than-desirable amount of error. Indeed, this may have contributed to the fact that I did not observe a relationship between individual ability and individual baseline performance in Experiments 1 and 2. When I conceptualised and developed these two experiments, the only version of the OSPAN available was the long version that took approximately 20 to 25 minutes to administer. Logistical constraints of a one-hour time slot meant that employing additional measures of executive control, along with the collaborative recall sessions, was not feasible. As I was completing Experiment 2, researchers released a validated version of a shortened OSPAN (Foster et al., 2015) which required significantly less time to administer. This made it possible to administer additional measures of executive control while still satisfying the realities of a PhD candidate's resources. The addition of executive control tasks, the Stroop and Antisaccade, allowed me to derive a more precise metric of individual ability. Future research might use even more comprehensive cognitive profiles in order to examine how people of varying abilities are influenced by collaborations.

I suggested in this conclusion that individuals brought varying abilities into collaboration, but that these abilities did not distinguish them at baseline recall. I suggested that perhaps the influence of abilities might only be observed downstream into collaboration and beyond into post-collaboration. However, in addition to simple measure insensitivity to recall, there is another possible reason for not observing a relationship between ability and baseline recall. In Experiments 1 and 2 I used categorised study lists which could have confounded the relationship between ability and performance as it could have given lower ability individuals an implied encoding and/or retrieval strategy and boost their performance to equate with higher ability individuals' (Unsworth et al., 2012). Thus, uncategorised word lists appear to be a better option for investigating ability-related collaborative differences.

Though the failure to observe ability-related differences in baseline recall levels was unexpected, ultimately it did not detract from the central focus of my thesis. In my thesis, I was concerned with how individual-level and group-level ability influenced collaborative and post-collaborative remembering. Thus, even lacking discernible effects on initial individual performance, downstream effects would still be observable if they were to exist. This was the case in my thesis.

One of my main goals in Experiment 3 was to delineate between the potential cognitive mechanisms that may have been causing forgetting, namely retrieval blocking and retrieval inhibition. To do this I added a recognition test following free recall. However, in this experiment the low overall rates of forgetting produced a floor effect and precluded me from effectively tracking the ultimate fate of lost items and making solid conclusion either way. However, there was some indication that retrieval inhibition was responsible: the finding that group discrepancy was negatively related to recognition performance. This would suggest that, overall, participants who collaborated in discrepant groups, relative to those who collaborated in non-discrepant groups, were more likely to experience retrieval inhibition. This conclusion is tempered somewhat by the fact that is impossible to know whether items that were not recognised were in fact ever encoded. That is, because many of the unrecognised items were not recalled at baseline, I cannot be certain whether they were initially encoded. If they were not encoded, then there would be nothing to inhibit. In order to better examine this, one would need to increase overall rates of forgetting which would require higher baseline levels of recall. To achieve this, more easily remembered word lists would need to be used. Ultimately, disentangling blocking and/or interference from inhibition is difficult. This is because the net effect of either of these mechanisms is poorer subsequent recall. That is, both items that have been blocked and items that have been inhibited are more less likely to be later recalled than are items that were not subject to these processes. Further, though blocking and/or interference would produce transient effects, the duration of these effects is not easily quantified. This means that if the interval between test and retest is insufficiently long enough the effects of block and/or interference would be indiscernible from the effects of inhibition if one were simply using a net performance approach.

Finally, power limitations are often an issue in collaborative recall studies. This is because the number of observations is diminished when calculating group-wise measures. This effect becomes more pronounced as the number of individuals within a group increases. For example, since I used dyads in my studies the effective number of cases was halved when I analysed nominal and collaborative performance. In my studies, I used sample sizes that were consistent with previous collaborative recall research and post-hoc power analyses indicated that I had approximately 52%

power to detect a medium effect in my studies. Certainly, a higher level of power would have been more desirable. However, this must also be reconciled with the realities of laboratory testing. Resources are not unlimited and sample sizes must necessarily be constrained.

Future Research

My PhD program of research represents the first steps toward identifying the influence of individual differences in group members' executive control ability on the costs and benefits of collaborative recall. My findings suggest that the abilities of collaborators do not necessarily influence recall during collaboration, but these characteristics do influence what collaborators take away from the collaborative session. Specifically, the differences between collaborators' abilities appear to modulate how much they forget; and in some circumstances, it was items that the individual produced themselves during collaboration that was forgotten. Future research might focus on how other individual differences of characteristics might influence collaborative recall. Perhaps personality characteristics might play a role. For example, anxiety and neuroticism have been linked to lower levels of executive control (Eysenck, Derakshan, Santos, & Calvo, 2007). Likewise Duchek, Balota, Storandt, and Larsen (2007) found anxiety and neuroticism to be linked to pathological cognitive decline preceding dementia. Considering the intertwined relationship between executive control and memory (Bugaiska et al., 2007; Engle & Kane, 2004; Kane & Engle, 2000; McCabe et al., 2009; Park, 2004) it would be logical to think that trait anxiety would influence collaborative recall. Another study sought to identify a "collective intelligence" factor in group performance (Woolley, Chabris, Pentland, Hashmi, & Malone, 2010). The factor they identified that predicted how successful groups were was not related to any indices of intelligence, but in fact was the average social sensitivity of group members as well as their gender. This nicely illustrates that there may be many individual differences beyond cognitive characteristics that can influence collaborative success.

Additional research may also further explore the patterns of forgetting I found in discrepant groups. Henkel and Rajaram (2011) observed that although older adults gained fewer items following collaboration than did younger adults, older adults did not lose more items. In this study, however, collaborative groups were homogeneously composed age-wise so it is impossible to deduce how discrepancy might influence gains and losses. A complement to their design would be to add another type of collaborative group which would be composed of an older and a younger adult; that is, groups that are discrepant in terms of their age. In this manner, one could examine outcomes at collaboration as well as patterns of gains and losses in each of the two age-types of participants. Further, identifying and implementing retrieval strategies in order to determine how these influence gains and losses for both younger and older adults after they had collaborated with one another could implicate potential interventions and helpful strategies that could be used to prevent further losses.

Another theorised mechanism influencing collaborative recall is *cross-cuing.* Cross-cuing occurs when collaborating individuals hear words from their partners and are reminded of words they would have otherwise forgotten (Meudell, Hitch, & Boyle, 1995). I did not include cross-cuing in this thesis because it is notoriously difficult to detect (Blumen & Stern, 2011; Meudell et al., 1995; Meudell et al., 1992). This is because it occurs concomitantly with re-exposure. As the opportunity for reexposure increases (i.e., partners generate more words) the opportunity for cross-cuing increases (i.e., there are more words from which to be cued). Thus, any effects of cross-cuing are often difficult to disentangle from effects of reexposure. However, a delayed recall methodology (e.g., Blumen et al., 2014) might be useful for examining the influence of ability on cross-cuing. Delayed recall may dissociate cross-cuing and re-exposure because cross-cuing involves repeated retrieval attempts whereas re-exposure involves repeated study opportunities (Blumen et al., 2014). As repeated retrieval benefits retention more than repeated study (Karpicke & Roediger, 2007), the positive effects of cross-cuing may emerge following a delay when the effects of reexposure had somewhat decayed. Thus, isolating the influence of ability on cross-cuing would be more tenable when recall follows a delay rather than when it is immediate (Blumen et al., 2014; Congleton & Rajaram, 2011). When also considering other findings which suggest a differential retention benefit for lower versus higher ability individuals following a 2-day delay (Agarwal et al., 2016), it seems likely that cross-cuing may benefit some individuals more than others.

Concluding Remarks

The research contained in this thesis represents the first steps toward identifying how the cognitive characteristics of collaborators influence collaborative recall. Although I found no evidence that executive control influenced recall during collaboration, I found evidence to suggest that it influenced post-collaborative individual recall. The most interesting aspect of these findings was that the influences on post-collaborative recall were not contingent solely on individual ability, but also dependent on the ability of the individual with whom a person had previously collaborated. Intuitively, one would expect that if a lower ability individual collaborated with a higher ability individual, then this should benefit everybody involved. However, my research suggests that this may not be the case and that the lower ability individuals may lose out on some of the benefits enjoyed by the higher ability individuals. It would be troubling enough if the lack of benefits were due to missing out on gaining new information that an individual may not have otherwise. Even more troubling, is my finding that these lower ability individuals lost items that they had previously remembered. However, my findings suggest that there are ways losses can be mitigated. Collaborations between individuals of similar abilities are most effective. Strategy use helps, especially when the strategy is made explicit and is agreed upon by all collaborating parties. Finally, the literature would suggest repeated collaborations are key to long term retention (Blumen & Stern, 2011; Blumen et al., 2014; Congleton & Rajaram, 2011).

In conclusion, the findings of this thesis suggest that collaboration may not influence all individuals in a similar manner and that the influences may be dependent not only on the ability of the individual, but on the ability of the individual or individuals with whom a person collaborates. Individuals come in to collaboration with varying abilities and these abilities can be observed and measured prior to collaboration. The fact that the influence of these abilities goes undetected at collaboration does not necessarily mean that there is no influence at this stage. Rather, what is taking place may be below a detectable threshold and it is only later that their influences can be observed. People go into collaboration with certain abilities, these abilities influence how the individuals generate and process information within collaboration, and then it is when individuals leave collaboration that we finally begin to see how these influences play out.

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Appendices

Word Lists Experiments 1 & 2

List A		List B		List C	
flamingo	salmon	dolphin	penguin	badminton	cashmere
silk	crow	cheerleading	racquetball	velvet	lettuce
violin	cucumber	celery	tomato	duck	bass
piano	radish	cymbals	flamingo	trout	cardinal
banjo	turnip	hockey	keyboard	cymbals	harp
broccoli	cello	spandex	flounder	flannel	cello
guitar	cricket	cardinal	minnow	silk	volleyball
harmonica	bowling	capsicum	zucchini	clarinet	piranha
hockey	flannel	lacrosse	sparrow	beetroot	capsicum
badminton	duck	suede	bowling	flamingo	rayon
polyester	sparrow	hawk	lycra	sparrow	hockey
beetroot	lettuce	harmonica	pigeon	banjo	violin
chicken	catfish	wrestling	flute	salmon	dolphin
pigeon	racquetball	eagle	radish	keyboard	golf
trombone	nylon	turnip	cricket	celery	flounder
piranha	snapper	cashmere	trout	swimming	radish
velvet	tomato	crow	silk	lacrosse	satin
trout	satin	polyester	duck	ostrich	flute
asparagus	flute	raven	blowfish	turnip	onion
cardinal	cheerleading	piranha	violin	pigeon	blowfish
penguin	raven	clarinet	shark	baseball	bluejay
blowfish	tuna	bluejay	catfish	minnow	lycra
clarinet	capsicum	cucumber	badminton	cheerleading	polyester
vulture	lycra	snapper	trombone	horn	cricket
rayon	eagle	salmon	asparagus	shark	tennis
shark	tennis	ostrich	wool	zucchini	bowling
cabbage	suede	volleyball	onion	nylon	tomato
lace	wrestling	jean	harp	snapper	asparagus
bluejay	keyboard	nylon	flannel	cabbage	spandex
zucchini	celery	piano	rayon	tuna	corn
harp	swimming	angelfish	guitar	guitar	harmonica
spandex	horn	lettuce	polo	tambourine	piano
cymbals	wool	horn	tennis	hawk	wool
cashmere	lacrosse	satin	baseball	lace	polo
volleyball	bass	swimming	whale	raven	catfish
flounder	dolphin	vulture	tuna	wrestling	jean
golf	jean	broccoli	chicken	cucumber	broccoli
whale	corn	velvet	beetroot	vulture	trombone
angelfish	tambourine	tambourine	bass	racquetball	angelfish
polo	hawk	lace	cabbage	suede	crow
onion	ostrich	corn	golf	eagle	chicken
baseball	minnow	cello	banjo	penguin	whale

Word Lists Experiment 3

List A		List B	
KIT	KETTLE	WIRE	SILK
ESTATE	JACKHAMMER	CASHMERE	INK
BELLS	GARAGE	SKEWERS	ASPHALT
GUN	VINE	COIN	BRIGHT
CHAINSAW	PARK	COAL	TOWEL
SIREN	FIREWORKS	DOWN	KERNEL
THUNDER	TREE	RUBBISH	CLOUDS
LAWNMOWER	KNOW	SHADOW	ROBOT
LEAVES	GRASS	BAT	SHOE
MOSS	AVOCADO	ROOM	SPONGE
OPERA	BEVERAGE	TUXEDO	BOOTH
STARS	TRAIN	EASEL	HILL
MINT	EMERALD	WAVE	BLANKET
LILYPAD	TRAFFIC	COTTON	CATHEDRAL
CONTEST	FERRY	INSTRUCTION	PILLOW
HAT	CRAYON	SWITCH	NIGHT
TRUMPET	BROCCOLI	RAVEN	BALL
PLANE	TEACHER	WEIGHT	CHALKBOARD
DRUMS	WHEEL	LICHEN	MARSHMALLOW
CHAIN	MOON	TONGUE	GOLDFISH
COSTUME	DIGIT	ARROW	PICKLE
MONEY	LAND	WHISPER	RANK
FROG	PANCAKE	PUPIL	FLASK
NURSE	SHAMROCK	TYRES	FEATHERS

Ethics Approval Letter

From: **FHS Ethics** fhs.ethics@mq.edu.au
Subject: RE: HS Ethics Application - Approved (5201400528)
Date: 12 May 2014 2:03 pm
To: Dr Celia Harris celia.harris@mq.edu.au
Cc: Associate Professor Amanda Barnier amanda.barnier@mq.edu.au, Mr Nikolas Scott Williams nikolas.williams@students.mq.edu.au

Dear Dr Harris,

Re: "The Effects of Working Memory Capacity on Collaborative Memory"(5201400528)

The above application was reviewed by The Faculty of Human Sciences Human Research Ethics Sub-Committee. The Faculty Ethics Sub-Committee wishes to thank you for your well-written application. Approval of this application has been granted, effective 12th May 2014. This email constitutes ethical approval only.

This research meets the requirements of the National Statement on Ethical Conduct in Human Research (2007). The National Statement is available at the following web site:

http://www.nhmrc.gov.au/_files_nhmrc/publications/attachments/e72.pdf.

The following personnel are authorised to conduct this research:

Associate Professor Amanda Barnier
Dr Celia Harris
Mr Nikolas Scott Williams

NB. STUDENTS: IT IS YOUR RESPONSIBILITY TO KEEP A COPY OF THIS APPROVAL EMAIL TO SUBMIT WITH YOUR THESIS.

Please note the following standard requirements of approval:

1. The approval of this project is conditional upon your continuing compliance with the National Statement on Ethical Conduct in Human Research (2007).

2. Approval will be for a period of five (5) years subject to the provision of annual reports.

Progress Report 1 Due: 12th May 2015
Progress Report 2 Due: 12th May 2016
Progress Report 3 Due: 12th May 2017
Progress Report 4 Due: 12th May 2018
Final Report Due: 12th May 2019

NB. If you complete the work earlier than you had planned you must submit a Final Report as soon as the work is completed. If the project has been discontinued or not commenced for any reason, you are also required to submit a Final Report for the project.

Progress reports and Final Reports are available at the following website:

http://www.research.mq.edu.au/for/researchers/how_to_obtain_ethics_approval/human_research_ethics/forms

3. If the project has run for more than five (5) years you cannot renew approval for the project. You will need to complete and submit a Final Report and submit a new application for the project. (The five year limit on renewal of approvals allows the Committee to fully re-review research in an environment where legislation, guidelines and requirements are continually changing, for example, new child protection and privacy laws).

4. All amendments to the project must be reviewed and approved by the Committee before implementation. Please complete and submit a Request for Amendment Form available at the following website:

http://www.research.mq.edu.au/for/researchers/how_to_obtain_ethics_approval/human_research_ethics/forms

5. Please notify the Committee immediately in the event of any adverse effects on participants or of any unforeseen events that affect the continued ethical acceptability of the project.