Look more carefully: Even your data show sleep makes memories more accessible.

A reply to Schreiner and Rasch (in press)

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1. Introduction

Experiments on declarative memory (i.e., about facts and episodes that we can consciously recollect) usually involve more than one item (e.g., pictures, words, locations, voices, etc. or associations between these elements). In other words, the net performance in these experiments *by construction* hides the presence of two opposing forces at work at the item-level: 'forgetting' (i.e., 'oblivescence'), which is the inability to express previous knowledge, and 'reminiscence', which refers to the gained, or regained, ability to express knowledge that was previously inaccessible (e.g., Erdelyi, 2010). Until recently, studies looking at how sleep influences this type of memory have never really tracked these undercurrents. As a result, because the benefit of sleep, over being awake, in most experiments comes about as a smaller decline in net performance between the immediate test and the retest, the default view has been that sleep stabilizes the learnt information, missing out on the possibility that it also promotes reminiscence.

In Dumay (2016), I examined this possibility by reanalysing two data sets (Dumay & Gaskell, 2007; Tamminen, Payne, Stickgold, Wamsley, & Gaskell, 2010) that showed a benefit of sleep on both free recall (i.e., Now, tell me what you know) and old/new recognition (i.e., Have you heard this before?) of made-up words, distinguishing between items remembered at the 0-hr test and not forgotten (i.e., 'maintained') by the 12-hr retest, and items not remembered at the test, but eventually recovered (i.e., 'gained') at the retest (i.e., the reminiscence cases). For free recall, this 'item-fate' analysis confirmed the anti-forgetting effect of sleep, with sleep increasing the likelihood of *still* remembering an item at retest. But it also showed a significant contribution of sleep to reminiscence: recovering an item that was inaccessible at the immediate test was more likely after sleep than after an equivalent period of wake. The recognition data confirmed the effect of sleep on reminiscence, but failed to reproduce the benefit for maintained items, yet revealed by free

recall. I concluded that sleep protects our memories against forgetting, but at the same time also makes them more accessible. Given the presence of significant interactions between group (sleep vs. wake) and item-fate in favour of gained items, I also claimed that the benefit of sleep to memory was primarily due to increased reminiscence.

The dissociation between the two effects across tasks (i.e., recognition shows no sleep effect on protection against loss, but only an effect on reminiscence) suggests that reminiscence may be driven partly by an overnight increase in stimulus familiarity, useful only for recognition, whereas protection against loss may not. This separation of the two effects was corroborated by the presence of correlations between the probabilities of reminiscence and forgetting only for the wake group. In view of these elements, I suggested that stronger reminiscence after sleep and protection against loss reflected only *partially* overlapping mechanisms (i.e., they should not be taken exactly as the two facets of the same coin).

Stronger reminiscence after sleep is hard to explain by reduced retroactive interference in the hippocampus, especially during slow-wave sleep. Less interference should not boost memory accessibility beyond the level achieved at exposure (Mednick, Cai, Shuman, Anagnostaras, & Wixted, 2011). This effect seems more readily explained by active consolidation mechanisms, such as neural replay (Feld & Born, 2017 for a review): if spontaneous reactivation of hippocampal memories strengthens copies in adjacent regions and/or associations in the neocortex, it would be surprising not to find increased accessibility the next day. But even at the cellular level, 'synapse homeostasis' (Tononi & Cirelli, 2014) may predict more reminiscence after sleep. After growing in number (or efficacy) throughout the day, synaptic spines are pruned (or weakened) overnight, most likely to keep only those showing strong and consistent reactivation. If this pruning retains only the fittest representations and hence reduces noise, the refinement that this implies would lead us to

expect more reminiscence *soon* after waking-up. In comparison, the mapping between protection against loss and underlying mechanisms is less logically constrained, and although passive consolidation may first come to mind, this effect could reflect a mixture of both active and passive influences. On this basis, and given the behavioural dissociation described above, I predicted that changes in active consolidation processes, as produced, for instance, by cued reactivation during sleep, may be more visible on, though not necessarily restricted to, gained items (i.e., reminiscence), as compared to maintained items (i.e., protection against forgetting).

Recently, Schreiner and Rasch (in press) answered my call by reporting a fate analysis of paired-associate translation learning data collected in the cued reactivation paradigm. German speakers exposed to Dutch translations of German words during the day are re-exposed to some of these pairs, or at least their non-native Dutch words, either within the same unbroken wake interval or during the next episode of slow-wave sleep. In this paradigm, cuing by the Dutch words during slow-wave sleep (as opposed to wake) improves subsequent cued recall. Their first analysis looks at the influence of sleep on forgetting and reminiscence, independently of whether or not participants were later re-exposed to the information. The authors find that sleep reduces forgetting, which concurs with my own recall data, but has no reliable effect on the likelihood of recalling previously inaccessible memories (i.e., reminiscence). In addition, they point out that my comparing the magnitude of sleep's contribution to reminiscence vs. protection against loss is conceptually questionable. As forgetting can be quantified in terms of either the number of items maintained between test and retest, or the number of items lost, relative to the number of items recalled at the immediate test, two complementary probabilities are available for one and the same construct. Their second analysis moves away from directly comparing sleep to wake. It shows that cuing both alleviates forgetting and promotes reminiscence, but only

when the cue is experienced in slow-wave sleep. The authors conclude that sleep mainly benefits the maintenance of newly acquired memory and that active consolidation at the systems level is the driving force behind this effect.

In this reply, I take Schreiner and Rasch's (in press) point about the conceptual difficulty one runs into when comparing two effects that can each be quantified by two complementary probabilities. However, contrary to what the authors would like readers to think, the problem can easily be solved. Further, I show that their view of post-sleep reminiscence as being just an epiphenomenon of the learning conditions of the experiments that I reanalysed is unwarranted.

2. To keep or not to lose?

To avoid the pitfalls of raw item counts in assessing the presence of a sleep effect on forgetting and reminiscence, Dumay (2016) relied on proportions in order to take into account the total number of items available to protection against loss vs. reminiscence for each participant. Protection against forgetting was operationalized as the number of items maintained at the retest, relative to the number of items recalled at the test (i.e., the maximum number of items that the participant could have maintained). Reminiscence was the number of items gained at the retest, relative to the total number of trained items minus the number of items recalled at the test (i.e., the maximum number of items that could have been gained; Roediger and Challis, 1989). These proportions are fine for detecting a difference between sleep and wake in the likelihood of each phenomenon. However, as Schreiner and Rasch (in press) point out, using proportions to compare the magnitude of the sleep effect in each case is unsafe, as forgetting can be quantified either 'optimistically', as I did it, by focusing on maintained items, or 'pessimistically', by focusing on lost items. In other words, two probabilities are available to represent the same concept, with the result that the value of the

interaction between group (sleep vs. wake) and item trajectory (protection against loss vs. reminiscence) changes depending on which of the two probabilities is used, a fact that I had overlooked.

Schreiner and Rasch's (in press) solution to this conundrum, however, is not satisfying, as they go on to compare gained items simultaneously to maintained vs. lost items, and that, certainly, does not remove the ambiguity. Further, they miss out on the fact that their logic applies also to reminiscence, which, like forgetting, can be coded in one of two ways, namely, focusing on either gained items or ungained items. Thus, their multiple-comparison based approach is itself slippery, as the picture will change again from the moment reminiscence is operationalized in pessimistic terms, looking at ungained items instead. Besides, the authors provide no explanation as to how one should adjudicate between one and the other interaction terms obtained using this approach. For my free recall data, for instance, even though the wake-to-sleep ratio for lost items correctly estimated by the authors at 1.72 (wake: 54.4% vs. sleep: 31.7%) is still numerically smaller than the sleep-to-wake ratio for gained items, the interaction between group (i.e., sleep vs. wake) and item fate is not significant when, instead of maintained items as in Dumay (2016), lost items are used as comparison point (free recall: $F(1,68) = 1.67$, $p > .20$, $\eta^2_p = .024$). A similar ambiguity arises for old/new recognition, with a ratio for lost items of 1.27 (wake: 21.7% vs. sleep: 17.0%) and an interaction not significant anymore if maintained items are swapped for lost ones ($F(1,58) = 1.15$, $p > .28$, $\eta^2_p = .019$). Without a principled approach telling us how to deal with these conflicting outcomes, at this point I would not even risk comparing the two effects using such floating probabilities.

Maintaining the ambiguity, obviously, allows Schreiner and Rasch (in press) to motivate the use of the cuing paradigm as providing a better approach. I fail to see, however, how testing whether systems consolidation possibly underpins protection against forgetting

and/or reminiscence provides a means of gauging the spontaneous contribution of sleep to each phenomenon: the two approaches are complementary, not in competition.

To deal with the problem of shadow probabilities, what we need is a variable that expresses the two facets of each effect (i.e., protection against loss/forgetting; reminiscence/nonreminiscence) in strictly symmetrical, though opposite, terms. That way, whether one quantifies protection against loss 'optimistically', in terms of maintained items, or 'pessimistically', in terms of lost items, makes no difference; and likewise for reminiscence, quantified in terms of gained, or ungained, items. The *difference to the point of equilibrium* between these two opposite tendencies, relative to the maximum number of items available in the background gives us exactly that. Hence, I reanalysed my own datasets and Schreiner and Rasch's taking:

- *for protection against loss,* the number of maintained items minus half the number of items recalled at test (i.e., 50% of what could possibly be maintained), relative to the number of items that were recalled at test;

- *for reminiscence,* the number of gained items minus half the number of items that were *not* recalled at test (i.e., 50% of what could possibly be gained), relative to the number of items that were *not* recalled at test. This does not change the statistical reliability of the effect of group (sleep vs. wake) at each level of item fate, but it allows us to assess the interaction independently from the base likelihood of forgetting and reminiscence that polluted the normalized proportions used in Dumay (2016) and Schreiner and Rasch (in press).

The results presented in Fig. 1-a force me to revise my conclusions regarding the relative strength of protection against loss and reminiscence in how sleep benefits memory recall. Contrary to the picture produced by the sleep-to-wake ratios in Dumay (2016), the impact of sleep on free recall is primarily carried by protection against loss (.18 vs. -.04),

with a boost twice as large compared to reminiscence (-.33 vs. -43). This result was backed-up by a significant two-way interaction ($F(1,70) = 4.63$, $p < .04$, $\eta^2_p = .062$). Note, in passing, that this result is at odds also with the conclusions that Schreiner and Rasch would have reached by sticking to normalized proportions and coding forgetting in terms of lost items. As we saw above (second paragraph of Section 2), this approach, if anything, would still favour reminiscence over protection against loss, at least descriptively.

In comparison, the patterns observed for recognition and paired-translation learning are largely unchanged against what was conveyed in the individual papers. For old/new recognition (Fig. 1-b), the strong effect of sleep on reminiscence (.08 vs. -.14) was significantly larger than the nonsignificant tiny group difference in protection against loss (.33 vs. .28; $F(1,57) = 4.67$, $p < .04$, $\eta^2_p = .076$).[1] The dissociation between recognition and recall in terms of sleep effects on item fate was bolstered by a significant three-way interaction with task ($F(1,127) = 9.46$, $p < .004$, $\eta^2_p = .069$). [Again, Schreiner and Rasch's (in press) approach biased towards lost items would miss out on both of these interactions (Fs ≤ 1).] For paired-translation learning (Fig. 1-c), the results confirmed the stronger effect of sleep on protection against loss (.34 vs. .26) against the null effect that they reported for reminiscence (-.40 vs. -.41; $F(1,137) = 18.51$, $p < .0001$, $\eta^2_p = .119$). In sum, based on these nonfloating probabilities, the influence of sleep on the fate of recall is more similar across the two studies than it appeared initially. The question remains, however, as to why Schreiner and Rasch's (in press) paired translation learning data do not show any of the post-sleep reminiscence implicated in my recall and recognition data. I treat this question in the following section.

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[1] The *df*s are 57 (instead of an expected 58) due to one participant with a maximum number of items that could be gained of 0 (i.e., they had 100% correct at the 0-hr test).

3. Post-sleep reminiscence: An epiphenomenon?

Because they fail to find a reliable effect of sleep on reminiscence, and presumably also because paired-associate learning task is the bread and butter of sleep and memory researchers, Schreiner and Rasch (in press) assume that the positive effects that I reported are specific to the lexical consolidation paradigm (Gaskell & Dumay, 2003), in particular its high number of exposures during training. Their dismissing the generality of the effect is obvious from statements like this one (on p. 2) in reference to their pattern of net performance showing the typical reduced memory loss after sleep and in which they imply that increased reminiscence is found only in cases of hypermnesia: "In contrast to the general results in Dumay (2016) where sleep led to improved performance, this pattern of result already suggests that the beneficial effects of sleep on memory in our paradigm expressed mainly in a relatively diminished forgetting in the sleep group (5.49 ± .86%) as compared to the wake group (15.5 ± 1.19%)". Evidently, if the pattern of averages already tells Schreiner and Rasch what is going on at the item-memory level, then why then carry out an item fate analysis? As my old/new recognition data illustrate, surprising differences can be found between what the net, average-based performance suggests (i.e., less forgetting after sleep than after wake) and what the items are actually doing in the background (i.e., showing only increased reminiscence after sleep; see Fig. 1-b below).

Their dismissing of sleep-induced reminiscence is also evident from their penultimate paragraph (on p. 5), where they go to great lengths to list the many alternative factors known to affect reminiscence, such as increased attentional resources, changes in memory search strategies, retrieval practice, and even chance, etc. These factors, of course, affect the likelihood of reminiscence (as they do also affect forgetting to some extent, for that matter). However, they are all largely orthogonal to the question, and cannot easily explain, without

the notion of memory consolidation, why reminiscence is more likely after sleep than after wake.

Actually, there is something puzzling about Schreiner and Rasch's results: they show that cuing during slow-wave sleep boosts gained responses the next morning, which suggests that spontaneous reactivation as part of systems consolidation is an open route to reminiscence. And yet, they show no reminiscence effect when simply comparing sleep to wake. This null effect is at odds with the free recall and recognition data reported in Dumay (2016), but also with the emerging literature on relational memory, which concerns the knowledge of the underlying structure of an episode and the associations between its various constituting elements encoded individually. Payne et al. (2009) showed that after overnight sleep, or even a post-lunch nap (as opposed to an equivalent amount of time awake), participants are more likely to recall erroneously an 'unseen' target word (e.g., 'window') when in fact they were presented only with a mini-list of semantic neighbours (e.g., 'door, glass, pane, curtain, ledge, etc.'; Deese, 1959; Roediger & McDermott, 1995). This suggests that, by virtue of their spontaneous reactivation during sleep, the mini-lists strengthen their internal structures to the point of filling the gaps by restoring missing semantic associates. In a similar vein, Dumay, Sharma, Kellen and Abdelrahim (in press) found response repetition priming (i.e., the fact that you save time when the current trial requires the same response as the previous trial; Bertelson, 1965) to be stronger when the stimuli (i.e., made-up words) presented on trials *n-1* and *n* for a classification of their printcolor had been both studied (uncolored) as part of the same item set seven days before the test, as opposed to just before it. As with semantic restorations in the Payne et al. (2009) study, this boost in response repetition priming as a result of consolidation most likely reflects implicit episodic associations developing after encoding between the items of the set (see Alger & Payne, 2016; Ellenbogen, Hu, Payne, Titone, & Walker, 2007; Lau, Tucker, & Fishbein, 2010, for

related work). Just on these grounds, as performance in a declarative task is inevitably determined, to some extent, by the ability of the items to evoke one another on the fly, the stronger bonds between the items after consolidation should result in less forgetting *and* more reminiscence.

To find what they find and reach the conclusion that sleep promotes reminiscence only in specific circumstances, Schreiner and Rasch (in press) take advantage of the 139 participants whom they tested in the various versions of their translation-associate cuing paradigm. One hundred and thirty-nine is an impressive number. The trouble is that the authors appear to have lost sight of the fact that for 43 participants (i.e., 60.6%) out of the 71 included in the sleep group, 36 (i.e., 30%) out of the 120 German-Dutch word pairs, which they were tested on, were not the same as for the rest of the sample, including the whole wake group.[2] In other words, the item fate analysis of the effect of sleep on memory that Schreiner and Rasch report is compromised by the phonetic, semantic and perhaps even emotional idiosyncrasies of these 36 pairs, as well as by resulting differences in the structural/relational properties of each item set. To illustrate with just two dimensions that are easily quantifiable (and I am not saying that these variables are implicated necessarily), whereas the Dutch words in these 36 pairs were on average 3.64 (sd: .83) phonemes long in the set given to these 43 participants, they were almost twice as long [6.11 (sd: 1.80)] in the corresponding pairs of the other set ($t(1,70) = 7.47$, $p < .0001$). Likewise, whereas 31 out of these 36 pairs were cognates (i.e., translations that are phonetically similar on all of their morphemes across the two languages; e.g., Flasche-fles [bottle/flask]) in the set given to these 43 participants, only 17 out of the 36 were cognates in the other set. Overall, these differences, at the level of the individual words, the pairs (such as their cognate status), and the whole set itself (see above), between the sleep and wake groups, are likely to have

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[2] These 43 participants are those of the Schreiner, Lehmann, and Rasch (2015) study.

affected not just learning and retrieval, and the base likelihood of forgetting and reminiscence, but also what the sleeping brain does with the learnt pairs during the interval. Spotting the error only requires comparing the stimuli listed in their Supplemental Materials. At a minimum, the authors should have counterbalanced the occurrence of the two item sets over a similar proportion of participants in the sleep and wake groups, as I did when I bundled the free recall data of Dumay and Gaskell (2007) and Tamminen et al. (2010). Without that, Schreiner and Rasch are comparing apples and oranges!

I reanalysed the Schreiner and Rasch (in press) data, excluding those 43 participants who had been given a different item set from the rest of the sample. This left me with 28 participants in the sleep group and a wake group of 68 untouched. Note that restricting the analyses to the items common to both sets, in order to keep all participants instead, is not a solution, as the context of learning, of consolidation, and of retrieval would still differ between the sleep and wake groups (see the second paragraph above).

The net performance on this restricted sample showed the typical protective effect of sleep as with the full sample, with less forgetting after sleep (-1.6%) than after wake (-7.6%), relative to the 0-hr test (52.3% for the sleep group vs. 50.8% for the wake group), and the interaction between group and session on arcsined proportions was significant ($F(1,94) = 37.04$, $p < .001$, $\eta^2_p = .283$). In contrast to Schreiner and Rasch's (in press) results, however, item fate showed a significant effect of sleep not just on protection against loss (.36 vs. .26; $t(89.14$, with unequal $\sigma^2) = -6.97$, $p < .0001$), but also on reminiscence (-.38 vs. -.41; $t(94) = -2.60$, $p < .02$; see Fig. 1-d). For comparison, these effects would correspond to a ratio of normalized proportions of 1.75 (instead of 1.47) for lost items and one of 1.31 (instead of 1.17) for gained items (cf. their Section 3.1). Still, the presence of a significant two-way interaction on these proportion differences confirmed the stronger impact of sleep on protection against loss ($F(1,94) = 14.40$, $p < .001$, $\eta^2_p = .133$). In short, when one looks more

carefully at Schreiner and Rasch's data, one does find reliable evidence that sleep promotes reminiscence.

Their cuing results, in contrast, should be relatively less affected by the presence of two item sets in the sleep group. Cuing was manipulated within participants. Further, the analysis reported by Schreiner and Rasch (in press) excludes all participants cued by full pairs, instead of just by the non-native Dutch words, and these participants are amongst the 43 trained on the alternate item set never shown to the wake group. In view of the dissociation of the two effects across tasks and the presence of covariation only during wake, I predicted that if the effect of sleep on reminiscence reflects the involvement of an active consolidation process (i.e., more than does the effect of sleep against forgetting), then the "effect [of cuing during sleep] should be most visible on gained (as opposed to maintained) items " (Dumay, 2016, p. 295). Schreiner and Rasch find that cuing during slow-wave sleep indeed boosts reminiscence more than it does protection against loss. Even though the authors insist that cuing affects lost and gained items equally (p. 2, paragraph 1), the presence of this interaction confirms my hunch.[3]

4. Conclusion

By pointing out the ambiguity linked to the presence of shadow probabilities in Dumay (2016), Schreiner and Rasch (in press) led me to reanalyse both data sets using a more appropriate measure of protection against loss and reminiscence. The outcome shows a pattern of item fate, for recall tasks at least, much more similar across the two studies, with sleep mostly preventing forgetting. I am therefore revising my earlier conclusions that sleep benefits memory *primarily* by increasing reminiscence. My story, however, does not change

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[3] Contrary to what Schreiner and Rasch' (in press) state (on p. 2, paragraph 1), I never said that "cuing during sleep should primarily impact behavioural gains *without* significant effects on memory losses". This is misrepresenting my view.

for recognition memory, which dissociates by showing an effect of sleep only on reminiscence in a three-way interaction. Together with cuing affecting reminiscence more than forgetting, I still conclude that this dissociation tells us that different sets of processes are behind the effect of sleep in each case, with (by the Schreiner and Rasch probing-by-cuing logic) possibly a stronger causal relation existing between systems consolidation and reminiscence. In any event, post-sleep reminiscence, is not as specific as Schreiner and Rasch want it to be, and certainly it is not linked to hypermnestic cases: when analysed more carefully even their data show post-sleep reminiscence. Thus, from this angle, "to gain or not to gain [...]", as Schreiner and Rasch ask in their title is hardly the question at this point, and my original claim summarized in my title "Sleep not just prevents forgetting, but *also* makes memories more accessible" is supported, rather than challenged, by their data.

Acknowledgements

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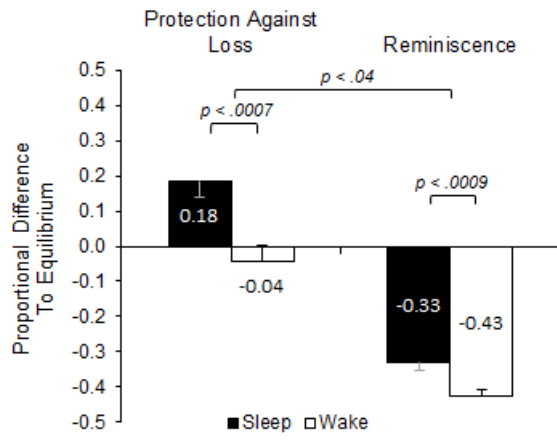
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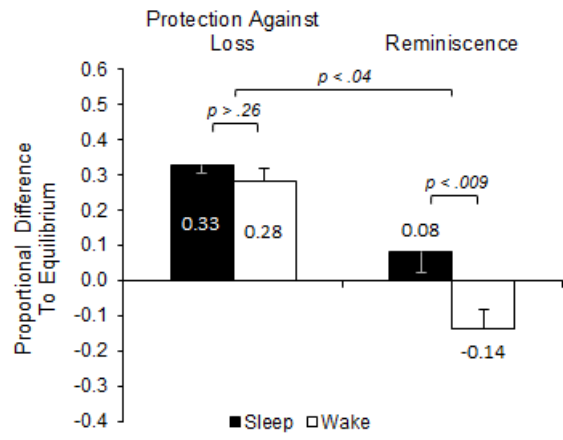
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Fig. 1. Mean difference to equilibrium by maximum number of items available for protection against loss (i.e., maintained items) and reminiscence (i.e., gained items), in the sleep and wake groups, for: (**a**) Dumay's (2016) free recall data; (**b**) Dumay's (2016) old/new recognition data; (**c**) Schreiner and Rasch's (in press) paired-associate learning (full sample); and (**d**) their sleep and wake groups trained on the same item set (restricted sample). Positive values indicate more protection against loss (or more reminiscence) than forgetting (or absence of reminiscence) on average. Error bars show standard errors. Also displayed are the *p*-values for simple effects and interactions of interest.
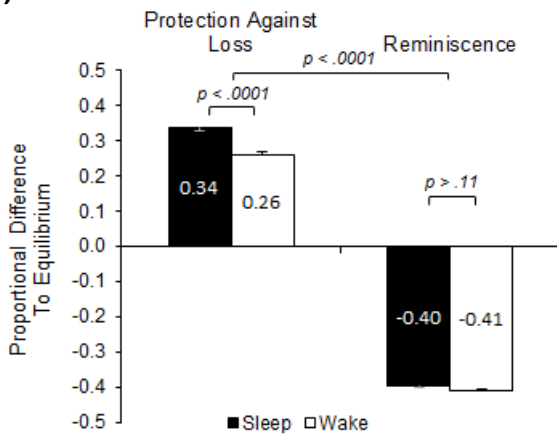
Fig. 1

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