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## **Specificity of Future Thinking in Depression: A Meta-Analysis**

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### Abstract

Reduced specificity of autobiographical memory has been well established in depression, but whether this ‘overgenerality’ extends to future thinking has not been the focus of a meta-analysis. Following a preregistered protocol, we searched six electronic databases, Google Scholar, personal libraries, and contacted authors in the field for studies matching search terms related to depression, future thinking, and specificity. We reduced an initial 7,332 results to 46 included studies, with 89 effect sizes and 4,813 total participants. Random effects meta-analytic modelling revealed a small but robust correlation between reduced future specificity and higher levels of depression ( $r = -.13, p < .001$ ). Of the 11 moderator variables examined, the most striking effects related to the emotional valence of future thinking ( $p < .001$ ) and the sex of participants ( $p = .025$ ). Namely, depression was linked to reduced specificity for positive (but not negative or neutral) future thinking, and the relationship was stronger in samples with a higher proportion of males. This meta-analysis contributes to our understanding of how prospection is altered in depression and dysphoria and, by revealing areas where current evidence is inconclusive, highlights key avenues for future research.

*Keywords:* overgenerality, episodic simulation, detail, MDD, dysphoria

## Introduction

Depression has long been associated with vagueness in recalling the past—often referred to as *overgeneral* autobiographical memory (Williams & Scott, 1988). When prompted to recall a personal event, people with depression tend to retrieve memories that are broad or categorical (e.g., “all the times I’ve lost at sports”) rather than specific to a place and time (e.g., “playing squash at the club last Thursday”). Overgeneral memory in depression is now well-established, with moderate-large effect sizes reported in two meta-analyses (van Vreeswijk & de Wilde, 2004; Williams et al., 2007). Growing research since the mid-1990s suggests this overgenerality may extend to various forms of autobiographical *future* thinking, in line with evidence that memory and future simulation rely on the same brain network (Benoit & Schacter, 2015). For instance, reduced specificity of future thought has been found in suicidally depressed (Williams et al., 1996), currently depressed or remitted (Addis, Hach, & Tippett, 2016) and dysphoric individuals (Dickson & Bates, 2006; MacLeod & Cropley, 1995). Impairments in depression are evident also on measures that are closely associated with specificity of future thinking, such as level of episodic detail in future simulations (King, MacDougall, Ferris, Herdman, & McKinnon, 2011), vividness of future imagery (Stöber, 2000), and concreteness of personal goals (Emmons, 1992).

Yet not all studies have found a negative link between depression and future specificity (e.g., Boelen, Huntjens, & van den Hout, 2014; Robinaugh, Lubin, Babic, & McNally, 2013; Sarkohi, 2011), which makes it difficult at this stage to evaluate the robustness of the effect. And although a recent meta-analysis on prospection in psychopathology found a large effect of reduced future specificity in depression, the analysis included only seven studies (Hallford, Austin, Takano, & Raes, 2018). We believe there are many additional studies relevant to this issue, and aim to synthesise these findings here. Given that imagining specific, detailed future scenarios is associated with a myriad of benefits, such as helping us to plan, pursue goals, and regulate emotions (Brown, MacLeod, Tata, & Goddard, 2002; Schacter, Addis, & Buckner, 2007; Taylor, Pham, Rivkin, & Armor, 1998), it is worthwhile to quantify precisely how this ability is affected in depression.

## Present Study

Here, we aimed to extend the meta-analysis by Hallford et al. (2018) in several important ways. First, we took a broader view of depression, acknowledging that depressive symptoms exist along a continuum of severity throughout the population (Ayuso-Mateos et al., 2010), and included studies of participants with sub-threshold depressive symptoms (often used interchangeably with the term “dysphoria”; e.g., Anderson, Boland, & Garner, 2015; Cropley & MacLeod, 2003; Holmes, Lang, Moulds, & Steele, 2008). Second, we took a wider view of future thinking, and included any of the four modes of future thinking described in Szpunar, Spreng and Schacter’s (2014) ‘taxonomy of prospection’: simulation, prediction, intention, and planning<sup>1</sup>. Essentially, we were interested in any kind of future thinking for which specificity can be (and has been) measured in depression.

And third, we employed what we see as a more complete approach to the concept of “specificity”. Stemming from Tulving’s (1972) seminal work on episodic memory, and building on earlier studies examining the specificity of autobiographical memory in suicide attempters (Williams & Broadbent, 1986), Williams et al. (1996) conceptualised a “specific” future thought as one that has a unique spatiotemporal context; i.e., occurring at a particular place and time. However, in addition to spatiotemporal specificity, Tulving (1972) also described the “perceptible properties” of events as being a key component of episodic specificity. Recent research has similarly treated the notion of episodic specificity more broadly than mere spatiotemporal specificity, by also considering the level of episodic details *within* future events, such as information about specific people, objects, actions, feelings, and perceptual details (Addis, Wong, & Schacter, 2008; Jing, Madore, & Schacter, 2016).

In their meta-analysis on episodic specificity, Hallford et al. (2018) included measures of episodic detail but excluded measures of the “vividness” of future thinking; nonetheless, vividness can be an important indicator of episodic detail (Nelson, Moskowitz, & Steiner, 2008; Martin et al. 2013). Moreover, vividness captures the clarity and subjective feeling<sup>2</sup> of “pre-experiencing” a future

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<sup>1</sup> While there is a vast literature on different types of future-oriented cognitions (e.g., Bandura, 1986; Locke & Latham, 1990; Mischel, 1973; Oettingen & Mayer, 2002; Taylor & Schneider, 1989), this taxonomy (Szpunar et al., 2014) is intended to encompass the majority of these cognitions, and indeed we found that all studies included in the meta-analysis could be categorised into one of the four modes of prospection.

<sup>2</sup> The Oxford English Dictionary (n.d.) defines “vivid” as “producing powerful feelings or strong, clear images in the mind.”

event, which are key features of projecting oneself into specific future episodes (Atance & O'Neill, 2001; Tulving, 1985). We therefore considered measures of vividness to capture an important aspect of episodic specificity, in line with Tulving's (1972) original definition. The inclusion of studies across a range of depressive symptoms, future thinking, and specificity should not imply that we viewed differences within each of these variables as trivial; on the contrary, we examined the effects of these differences via the moderator analyses described below.

Following a preregistered protocol for meta-analysis (available on the Open Science Framework at [osf.io/evdkf](https://osf.io/evdkf)), we investigated three research questions. First, what is the relationship between depression and specificity (as earlier defined) of future thinking? Second, to what extent can level of depression be explained by concurrent variation in specificity of future thinking? While being careful not to infer causality, investigating this question will give a sense of the degree to which depression can be predicted from specificity of future thinking. And third, what moderating variables can explain any heterogeneity in results across studies? We investigated the effects of two categories of moderator variables: those pertaining to participant characteristics, and to research methodologies. We had specific theoretical predictions about the effect of each moderator.

### **Moderators in Participant Characteristics**

Research on the specificity of future thinking in depression has spanned a range of demographics and clinical groups. Establishing whether the relationship differs across participant characteristics will help inform who may benefit most from interventions to enhance specificity. We tested the effects of three preregistered moderating variables relating to participants: *clinical status of depression*, *comorbid anxiety*, and *age*.

*Clinical status of depression* refers to the status of patients or participants under investigation. Some studies have examined specificity in individuals with Major Depressive Disorder (MDD; compared to healthy controls), while other studies have focused on individuals with dysphoria, remitted depression, or even levels of depressive symptoms in those scoring *below* cut-offs for mild depression. We predicted that reduced specificity of future thinking would be evident at higher levels of depression in the above statuses because (a) subthreshold depressive symptoms are thought not to differ qualitatively with full-blown clinical depression (Ayuso-Mateos et al., 2010) and (b) reduced

specificity of past and future thinking has been found to persist even in individuals with remitted depression (Addis et al., 2016; Brittlebank, Scott, Williams, & Ferrier, 1993; Mackinger, Pachinger, Leibetseder, & Fartacek, 2000).

*Comorbid anxiety* denotes whether participants with depression/dysphoria also had a diagnosis or high level of anxiety. Some studies have indicated *enhanced* prospective imagery for negative events in those with anxiety (Morina, Deepro, Pusowski, Schmid, & Holmes, 2011; Stöber, 2000) but the more common result is of reduced specificity (Brown et al., 2013; Kleim, Graham, Fihosy, Stott, & Ehlers, 2014; Mc Nally, Lasko, Macklin, Pitman, & McNally, 1995; McNally, Litz, Prassas, Shin, & Weathers, 1994; Wu, Szpunar, Godovich, Schacter, & Hofmann, 2015). A central feature of anxiety—worry—is thought to be mostly verbal, non-episodic, and non-concrete (Borkovec & Ray, 1998; Miloyan, Bulley, & Suddendorf, 2016). Additionally, boosting the specificity of future imagination through an ‘episodic specificity induction’ has been linked to *decreased* anxiety toward future events (Jing et al., 2016). Considering these findings, we predicted that future specificity would not be enhanced in those with comorbid anxiety compared to depression alone.

*Age* was also examined as a moderator, as future specificity is known to vary with age: Older adults tend to generate fewer episodic details and more semantic details than younger adults (Schacter, Gaesser, & Addis, 2013). It has been suggested that age may moderate (increase) the relationship between depression and overgenerality of memory, as age is associated with more previous depressive episodes and, relatedly, more damage to the hippocampus (King et al., 2010). Whether age influences the relationship between depression and future specificity has, to our knowledge, not been investigated. We predicted that age would increase the strength of the relationship, as future thinking in older depressed adults may suffer the compounding effects of both age and depression.

### **Moderators in Research Methodology.**

In one of the first studies in this area, MacLeod and Cropley (1995, p. 48) wrote: “What is clear is that there are important distinctions to be made in future-thinking and that any demonstrated relationship between mood disturbance and future-thinking will be affected by the particular measure of future-thinking used.” Considering the assortment of methods that have emerged—along with

mixed results—their statement is truer now than ever. We assessed four moderators relating to measures of future thinking: *emotional valence of simulations*, *macro- versus micro-level specificity*, *cue type*, and *specificity self- versus researcher-rated*. We also examined two moderators pertaining to measures of depression: *depression self- versus researcher-rated*, and *categorical versus dimensional depression*.

*Emotional valence of simulations* refers to the positivity or negativity of future simulations.<sup>3</sup> Overly negative future thinking has featured prominently in models of depression since Beck's (1976) cognitive triad and has even been described as “the primary cause” of depression (Roepke & Seligman, 2015, p. 8); it is thus worthwhile to clarify how emotional valence interacts with specificity in this disorder. Some measures such as the Future Event Task (based on the Autobiographical Memory Test; AMT; Williams et al., 1996) prompt participants with positive, negative, or neutral cue words to elicit future simulations of these valences, but measures of emotional valence may also refer to participant self-ratings following a simulation (e.g., as collected by Addis et al., 2016). A common finding in the memory literature is that specificity of memory is reduced broadly (across valences) in depression, but is particularly impacted for positive events (reviewed in van Vreeswijk & de Wilde, 2004). There is evidence for similar effects in future thinking (Dickson & Bates, 2006; Stöber, 2000), but not all findings are consistent. Dysphoric individuals have been found, for example, to generate specific negative events even faster (MacLeod & Cropley, 1995) and more vividly (Holmes et al., 2008) than healthy controls. As episodic memory and simulation rely on similar neurocognitive processes (reviewed in Schacter et al., 2012; Addis, 2018), we predicted future thinking in depression would reveal a similar pattern to that evident for memory; that is, it would be less specific for all emotional valences, but the deficit would be strongest for positive future thinking.

*Macro- versus micro-specificity* distinguishes the event level at which specificity is measured.<sup>4</sup> Following the approach of Hach, Tippett, and Addis (2016), macro refers to

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<sup>3</sup> Although this distinction is typically made for future *events*, we also coded the emotional valence of other types of future simulations, such as goals and plans—if the distinction was clear. For example, approach goals were classified as *positive*, as they reflect movement toward a desired positive state, and avoidance goals as *negative*, as they reflect movement away from an unwanted negative state (Elliot & Sheldon, 1997).

<sup>4</sup> Similarly, in addition to events, we applied the *macro versus micro* distinction to measures of specificity of other types of future simulations, such as goals and plans (Dickson & MacLeod, 2004; Emmons, 1992).



spatiotemporal specificity of the future event itself; for example, whether the event is localised to a time and place. Such measures are typically categorical; events are scored as specific, or not (e.g., Williams et al., 1996). Micro refers to measures of specificity *within* future simulations, such as the number of episodic details (King et al., 2011), self-reported level of detail (e.g., Addis et al., 2016), and vividness/clarity of future imagery (e.g., Stöber, 2000). Micro measures are often continuous (e.g., when the outcome variable is the number of details). We are not aware of any thorough attempt to examine how macro- and micro-specificity relate differentially to depression. We predicted that both levels of specificity would be impaired; additionally, considering that categorisation of continuous variables reduces statistical power to detect true effects (Hunter & Schmidt, 1990), we predicted macro measures would evidence smaller reductions than micro measures.

*Cue type* refers to the prompt used to elicit a future simulation or event. For instance, the AMT typically uses single words as cues (e.g., “laughing,” “friendly,” “bread”; e.g., Williams et al., 1996), whereas other studies have used event cues (e.g., “New Year’s Eve,” “an accident,” “an election”; e.g., Addis et al., 2016). Event cues are thought to be more supportive than single word cues for retrieving and generating specific events (Addis et al., 2016); hence we predicted that reduced specificity in depression would be less pronounced in studies using the more supportive event cues, compared to single word cues.<sup>5</sup>

*Specificity self- versus researcher-rated* describes whether measures of specificity were attained through participant self-report or researcher coding/count. We predicted the relationship between depression and specificity would be stronger (more negative) for self-reported than researcher-rated outcomes, given a tendency for depressed individuals to underestimate their performance on cognitive tasks (Farrin, Hull, Unwin, Wykes, & David, 2003).

*Depression self- versus researcher-rated* refers to whether depression/dysphoria was quantified via self-report questionnaires, such as the Beck Depression Inventory (BDI; Beck, Ward, Mendelson, & Mock, 1961) or Centre For Epidemiological Studies Depression Scale (CESD; Radloff,

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<sup>5</sup> We noted that differences in *cue type* may be confounded by other differences in scoring method (e.g., studies using single word cues typically employ *macro* measures of specificity)—which could make these effects difficult to tease apart.

1977), or was evaluated by a clinician or researcher using, for example, the Hamilton Rating Scale for Depression (HAM-D; Hamilton, 1960) or Structured Clinical Interview for DSM Disorders (SCID; First, Spitzer, Williams, & Gibbon, 1995). Reductions in specificity have been found in both self-rated and clinically diagnosed depressed samples, and we are not aware of any theoretical basis to suggest the effect is heightened for either type of measure; thus we predicted no effect of this moderator.

*Categorical versus dimensional depression* denotes whether researchers used a categorical (group) or dimensional (continuous) design to assess depression. A recent meta-analysis on interpretation biases in depression found significantly stronger effects in studies with dimensional than categorical designs (Everaert, Podina, & Koster, 2017). This finding makes sense statistically—dichotomisation of continuous variables can lead to underestimation of the strength of relationships (Hunter & Schmidt, 1990). We thus predicted the relation between depression and future specificity would be stronger for studies that were dimensional rather than categorical.

### Method

We designed, preregistered ([osf.io/evdkf](https://osf.io/evdkf)), and reported the results of the meta-analysis in accordance with the Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) statement (Moher et al., 2015). The flowchart in Figure 1 depicts the major steps of the meta-analysis; any deviations from the preregistration are noted in the text and footnotes below.

### Inclusion criteria

The criteria for including a study in the meta-analysis are listed in Figure 1 according to the PICOS categories<sup>6</sup> (Moher et al., 2015). One requirement was that a study measured some aspect of specificity in future thinking. As described *a priori*, we included measures that directly assessed one or both key components of episodic specificity (Tulving, 1972): (1) spatiotemporal specificity (usually reported as a proportion of all events that are specific in time and place); and (2) measures of perceptible properties of events (e.g., number of details, vividness). Moreover, measures of constructs

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<sup>6</sup> The PICOS categories Interventions and Comparisons were excluded as they cannot be applied to meta-analyses of observational studies.

that are close semantic associates of “specific” (i.e., lay within two degrees of separation from “specific” in the Oxford English Thesaurus) were also included.<sup>7</sup>

A common measure of future thinking in psychopathology has been the Future Thinking Task (FTT), which assesses verbal fluency for future events (MacLeod & Byrne, 1996).<sup>8</sup> As planned, strict measures of future event fluency were not included in the meta-analysis, for two reasons. First, even with instructions for participants to be as specific as possible, one of the only studies to code responses for specificity found that a substantial proportion of named events were in fact general (27.5% and 14% of events generated by dysphoric and control subjects, respectively; MacLeod & Cropley, 1995). This observation suggests that without coding of events for specificity, the FTT score does not necessarily capture specificity. The second reason was theoretical—future fluency tasks have been proposed to largely measure the ability to access *abstract* information about the future (D’Argembeau, Ortoleva, Jumentier, & Van der Linden, 2010). Thus FTT data on *number* of events generated (unless coded for specificity) were not included.

### **Literature search and coding**

To be indexed, studies had to mention at least one search term relating to each of three key variables: depression, future thinking, and specificity (see Table 1 for a list of all search terms). We searched the databases PsychINFO, Scopus, PubMed, ScienceDirect, Web of Science, and ProQuest Dissertations and Theses on 1<sup>st</sup> December 2017, and again on 14<sup>th</sup> January 2019 while this paper was under review. Google Scholar was searched on 13<sup>th</sup> December 2017 (and again on 14<sup>th</sup> January 2019), using modified search terms due to the 256 character search limit. No study design, date or language limits were imposed on the searches<sup>9</sup>. Following the original search, emails were sent to 40 authors of articles on future thinking in depression, requesting unpublished or in-press data, with a deadline for responses set to 26<sup>th</sup> January 2018. We also scanned reference lists of included studies and related reviews ( $n = 20$ ), and searched our personal files to capture as many relevant studies as possible.

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<sup>7</sup> For example, *specific* is synonymous with *definite*, which is synonymous with *concrete*; we hence took measures of the concreteness of future thinking as capturing some aspect of specificity.

<sup>8</sup> In the FTT participants are asked to orally name as many future events they are looking forward to, and not looking forward to, as possible, with the typical outcome measure being the total number of named events.

<sup>9</sup> The syntax used to search each database can be downloaded from [osf.io/dk6ws](https://osf.io/dk6ws).

A spreadsheet of all the search results ( $n = 7,332$ ) is available at [osf.io/x3jy6](https://osf.io/x3jy6). BG screened all titles and abstracts, then removed duplicates ( $n = 798$ ) and any studies clearly not meeting inclusion criteria ( $n = 6,344$ ). BG then reviewed full reports of titles appearing to meet inclusion criteria or where there was any uncertainty<sup>10</sup> ( $n = 188$ ). For any study excluded, the primary inclusion criteria not met was recorded. We sought additional information from study authors where necessary to resolve questions about eligibility or where data were insufficient to calculate an effect size. The full-text article for each study deemed eligible by BG ( $n = 46$ ) as well as any ambiguous cases ( $n = 1$ ) were further reviewed by DM and DRA for confirmation of inclusion. Agreement between all three authors was unanimous in all cases (i.e., 100%). We identified 46 articles meeting inclusion criteria, with 52 independent samples and 89 effect sizes ( $N = 4,813$ ). Next, BG extracted reference information, methodological characteristics and results from eligible studies. DM reviewed the data extracted by BG and compared it to the full text articles to confirm that the extracted data were accurate; no discrepancies were found. The final data file used for analysis, which contains the extracted data, describes all variables for which data were sought, and includes notes on any difficult decisions during coding, is openly available at [osf.io/a6q5y](https://osf.io/a6q5y).

### **Effect sizes and moderator variables**

The measure of effect size was the correlation between level of depressive symptoms and the specificity of future thinking. We took a correlational rather than categorical approach because, as mentioned, depressive symptoms are thought to occur along a continuum of severity throughout the population (Ayuso-Mateos et al., 2010). For studies in which only group-level comparisons were reported (e.g., MDD patients versus healthy controls), standardised mean differences (Cohen's  $d$ s) were converted to biserial-correlations (Becker, 1986; Hunter & Schmidt, 1990).<sup>11</sup>

The coding of moderator variables was mostly straightforward and followed the preregistration. For the moderator variable *cue type*, an additional (unplanned) subgroup was needed

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<sup>10</sup> Full reports of two potentially relevant articles could not be obtained and the authors did not respond to emails.

<sup>11</sup> For one effect size (the positive valence condition in Dickson & Bates, 2006) the conversion yielded a nonsensical value of  $r = -1.05$ . Rather than excluding this effect size we converted Cohen's  $d$  to a point-biserial-correlation, which assumes the groups were categorically different in some way, yielding a value of  $r = -.83$ .

to accommodate studies that did not fit into the categories of single word or event cues. The new subgroup *open* was created for cues that were more open-ended, such as those used in sentence completion tasks (e.g., “I can imagine that, shortly, I...”; Boelen et al., 2014). Thirty-eight effect sizes fell into the *open* category, so we considered it an important addition despite deviating from the preregistration. For the moderator variable *emotional valence*, a new subgroup *combined* was added to include studies for which only results of positive and negative conditions together were reported.

As a rule of thumb, a minimum of four independent samples are recommended per subgroup for meta-regression (Fu et al., 2011). For two moderator variables, some subgroups did not meet this threshold: Three of the six subgroups of the moderator *clinical status of depression* contained effect sizes from just two independent samples, and the two subgroups of the moderator *comorbid anxiety* contained effect sizes from only three independent samples.<sup>12</sup> Rather than exclude subgroups with a small number of cases, we ran the moderator analyses as planned but interpreted results with caution, considering possibly insufficient power to detect true effects in those subgroups.

### **Meta-analytic procedure**

We used random effects meta-analysis modelling<sup>13</sup> with restricted maximum likelihood<sup>14</sup> to estimate overall effects and the heterogeneity across included studies. Additionally, we used mixed-effects meta-analysis modelling to test if differences in the strength of effect sizes across studies could be explained by the moderator variables. Analyses were run in R using the ‘metafor’ package (Viechtbauer, 2010), and our R script is available online ([osf.io/35kzx](https://osf.io/35kzx)). Some studies included multiple measures of future thinking or depression within the same sample, so we accounted for dependency by adding random effects corresponding to each independent sample (Viechtbauer, 2010). We calculated 95% confidence intervals (CI) for the overall effect size and inferred confidence in the cumulative estimate from a combination of the magnitude and precision of the effect size, and risks of publication and reporting bias. Biases were assessed using a combination of trim-and-fill and

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<sup>12</sup> Most studies in the meta-analysis either did not measure anxiety or did not group participants according to anxiety levels.

<sup>13</sup> Random (unlike fixed) effects meta-analysis does not assume the true effect size is the same across included studies, so is the preferred option when heterogeneity is present (Riley, Higgins, & Deeks, 2011).

<sup>14</sup> Restricted maximum likelihood is the standard option for this analysis and thought to be relatively unbiased (Viechtbauer, 2010).

p-curve analysis, published versus unpublished study comparisons, and examination of study quality. BG and DM assessed study quality<sup>15</sup> using the 18-item Checklist for Measuring Quality (Everaert et al., 2017) adapted by Downs and Black (1998) to exclude items related to interventions, making it suitable for the current meta-analysis.

Outliers were pre-defined as correlations whose residuals had  $z$  scores  $> 3$ . No studies met this threshold (see Figure S1 in Supplemental Materials), and so none were excluded from primary analyses. Noting that some effect sizes deviated greatly from the mean (see Figure 2), we also explored an alternate measure of outliers, Cook's distance ( $D_i$ ), which indicates the relative influence of each effect size on the summary estimate. A standard rule of thumb is that  $D_i$  values greater than three times the mean  $D_i$  may be potential outliers. Seven effect sizes exceeded this threshold (see Figure S2), and so we ran exploratory analyses after their exclusion as a sensitivity check. No substantial changes occurred to the summary estimate or results of moderator analyses, except for the moderator *categorical versus dimensional depression*, which shifted from borderline statistically significant to non-significant after the removal of outliers; this point is addressed further in the Discussion. These secondary analyses are available online ([osf.io/cvqg4](https://osf.io/cvqg4)) but we focus here on the initial analyses, run with all effect sizes.

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<sup>15</sup> It was planned that BG and DM would assess quality ratings independently, but for practical purposes the authors assessed quality in consultation and the values reported are consensus scores.

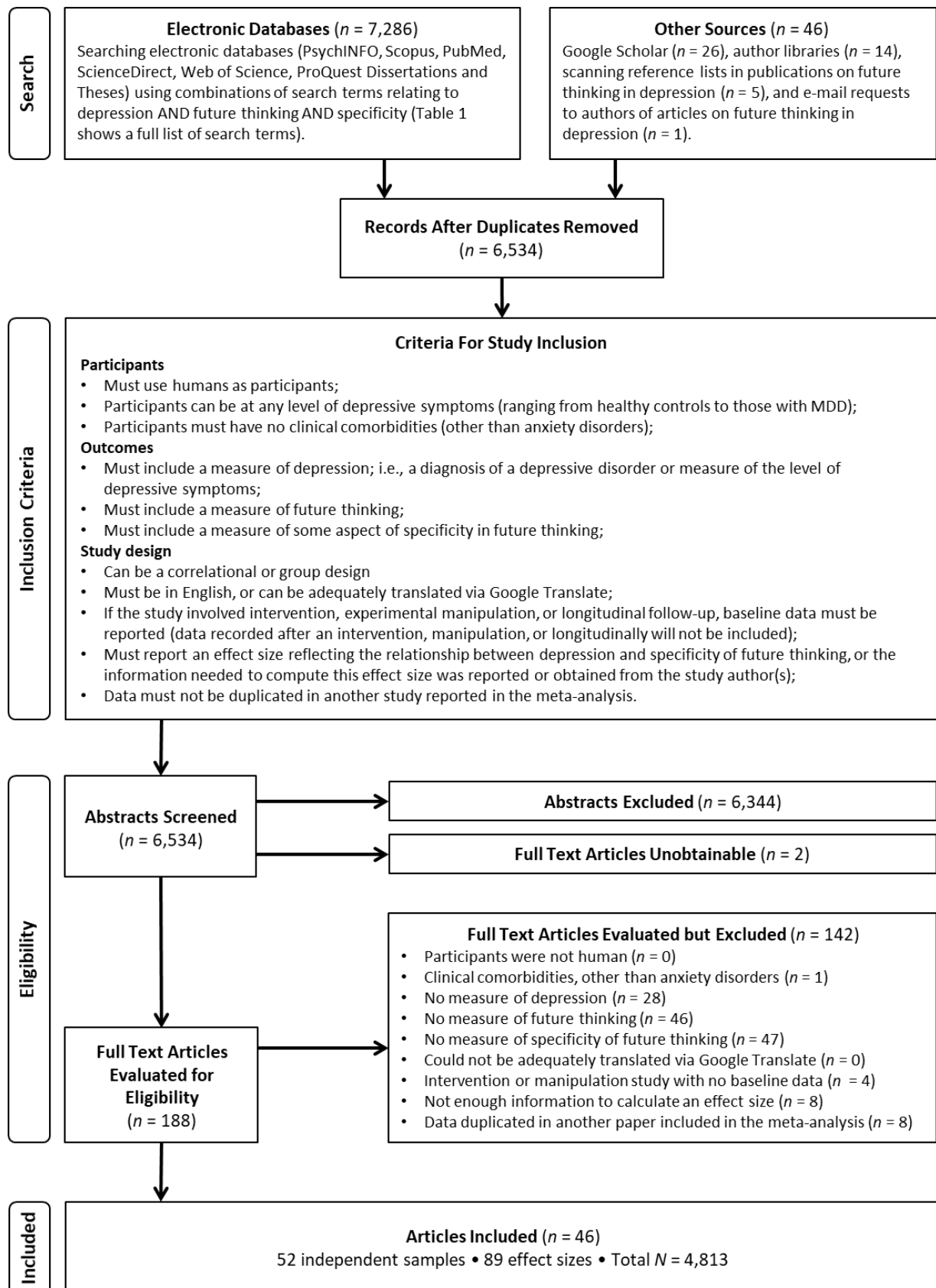
**Table 1:** Literature search terms.

<b>Depression</b>	<b>Future thinking</b>	<b>Specificity</b>
depress*	future thinking	more specific
dysphor*	future thought*	less specific
dysthymi*	future-directed thinking	number of specific
	future-directed thought*	greater detail*
	prospective	reduced detail*
	episodic simulation*	increased detail*
	future simulation*	number of detail*
	future event*	internal detail*
	future imag*	external detail*
	autobiographical plan*	episodic detail*
	personal project*	semantic detail*
	personal striving*	vivid*
	scene construction	enhanced imag*
	constructive daydream*	reduced imag*
		overgeneral*
		concrete*

<sup>1</sup> To be indexed, studies had to mention at least one search term from each column (i.e., depression AND future thinking AND specificity).

<sup>2</sup> The search terms deviate slightly from those listed in the preregistration, which initially yielded an impractically large number of results (e.g., the first search in Scopus returned 40,984 articles). To limit irrelevant results, broad terms such as *goal*, *specificity*, *more detail*, and *less detail* were removed, and *prospect* was changed to *prospective* (which rendered the terms *prospective imag\** and *prospective cognition\** redundant so these were also removed).

<sup>3</sup> The wide array of search terms reflects the plethora of constructs researchers have used to assess aspects of future thinking.



**Figure 1.** PRISMA flow diagram of the literature search and study coding. The last inclusion criterion (that data were not duplicated) was added subsequent to the preregistration as it was not anticipated during planning; the phrasing of some criteria has also been amended for clarity.  $n$  = number of studies;  $N$  = number of participants across all included studies.



## Results

The studies encompassed a range of participants, from symptom-free to clinically depressed, and a multiplicity of future thinking tasks—across the 46 included articles, researchers used 26 different measures to capture some aspect of specificity in future thinking (study characteristics for each effect size are described in Table S1). Figure 2 shows that almost two thirds (57 of 89) of correlations between depression and future specificity were negative, i.e., higher levels of depression were associated with reduced specificity of future thinking. The meta-analytic average correlation was  $r = -.13$ , 95% CI<sup>16</sup> [-.20, -.05],  $p < .001$ , indicating future specificity could explain 1.6% [0.3%, 4.0%] of the variance in depression, leaving 98.4% of the variance unexplained. There was a high degree of heterogeneity across effect sizes, as might be expected from the diversity of future thinking tasks. The  $I^2$  statistic, which indicates the percentage of between-studies variability in effect sizes due to heterogeneity rather than random error, was  $I^2 = 88.3$  for the overall model. Given the high level of heterogeneity, we characterised the underlying distribution of effect sizes via mixture modelling, following the procedure described in Moreau and Corballis (2019). We estimated that the distribution of effect sizes was well characterised by a single-component distribution, suggesting no manifest departure from normality (see Figure S8). In the analyses reported next, we investigated whether some of the heterogeneity across studies could be explained by the moderator variables.

### Moderator analyses of participant characteristics

The effect of *clinical status of depression* was not significant,  $Q(5) = 9.19$ ,  $p = .10$ . As predicted, the correlation between specificity and depression was negative and significant in samples spanning non-depressed to clinically depressed participants ( $r = -.23$  [-0.40, -0.05],  $p = .010$ ,  $k^{17} = 16$ ) and non-depressed to dysphoric participants ( $r = -.12$  [-.20, -.04],  $p = .005$ ,  $k = 61$ ). We predicted the effect would hold across all statuses of depression, but this was not the case: The effect was non-significant in samples spanning non-depressed to remitted participants ( $r = .10$  [-.17, .36],  $p = .48$ ,  $k = 2$ ), and samples including only (i.e., with no comparison group) participants who were non-depressed ( $r = -.13$  [-.33, .07],  $p = .20$ ,  $k = 5$ ), clinically depressed ( $r = .07$  [-.31, .44],  $p = .72$ ,  $k = 3$ ), or

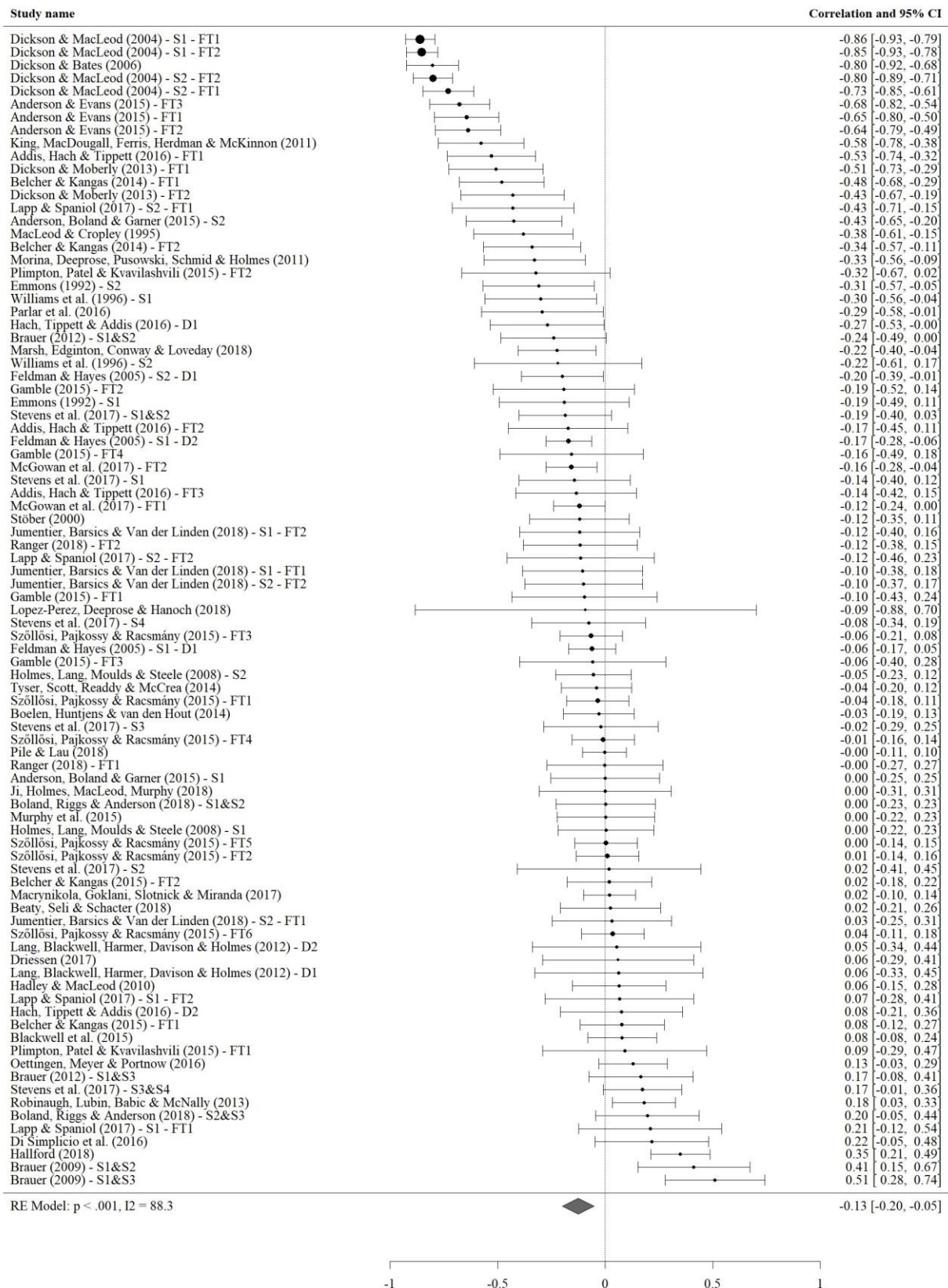
<sup>16</sup> Numbers in square brackets throughout Results represent 95% confidence intervals around the effect sizes.

<sup>17</sup>  $k$  denotes the number of effect sizes in each subgroup.

dysphoric ( $r = -.18$  [.45, .08],  $p = .17$ ,  $k = 2$ ). Given the paucity of effect sizes in these latter four subgroups, and hence the possibility of insufficient power, the null results should be interpreted with caution.

The effect of *comorbid anxiety* was not significant,  $Q(1) = 3.61$ ,  $p = .057$ , in line with our prediction. The relationship between specificity and depression was negative and significant in samples *without* comorbid anxiety ( $r = -.35$  [-.65, -.05],  $p = .023$ ,  $k = 6$ ), and negative but non-significant in samples *with* comorbid anxiety ( $r = -.26$  [-.57, .04],  $p = .087$ ,  $k = 6$ ). Again, the subgroups contained few effect sizes, possibly hampering our ability to detect a true difference.

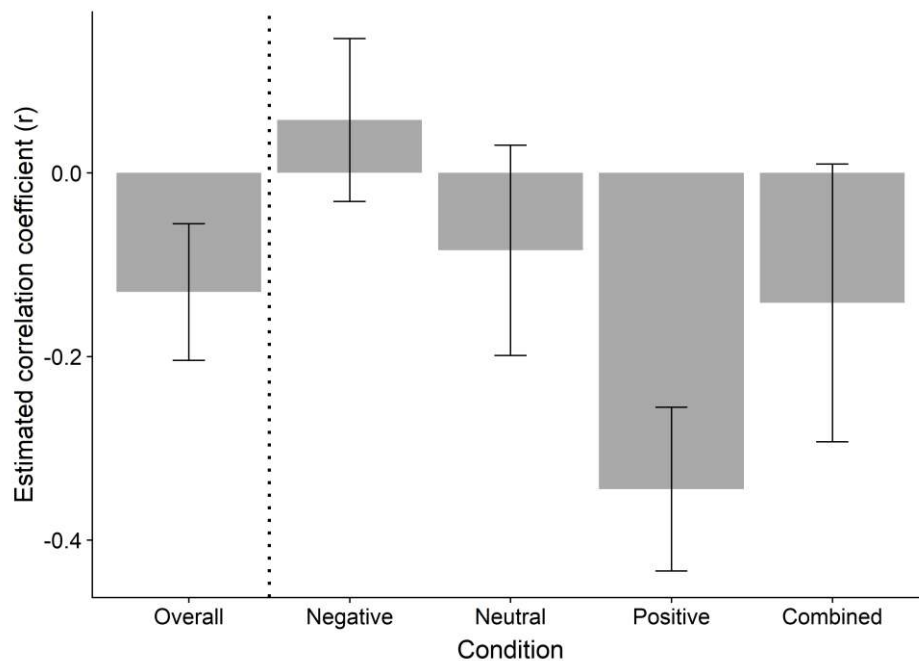
The effect of age was not significant,  $Q(1) = 0.07$ ,  $b = -.001$  [-.006, .005],  $p = .79$ , which diverged from our prediction of a stronger relationship between specificity and depression in older individuals. It is worth noting that samples were heavily skewed towards younger adults, with a mean age (weighted by  $n$ ) of 25.1 years, and that only five independent samples had a mean age greater than 50 years.



**Figure 2.** Correlations between levels of depression and specificity of future thinking. Correlations (dots) and 95% confidence intervals (CIs; lines) are shown for all effects in the meta-analysis. The size of each dot reflects the weight given to the observed effect during model fitting. The diamond at the bottom shows the meta-analytically weighted mean correlation (with 95% CIs). Multiple measures were adjusted for dependency (see ‘Meta-analytic procedure’ of Methods). Multiple independent samples within studies are reported separately (S1, S2, etc.), as are multiple measures of future thinking (FT1, FT2, etc.) and depression (D1, D2, etc.).

### Moderator analyses of research methodology

The effect of *emotional valence of simulations* was highly significant,  $Q(3) = 425.04$ ,  $p < .001$  (see Figure 3), and partly aligned with our hypotheses. As predicted, the relationship between depression and specificity was strongest (most negative) in conditions with positive cues or where future thinking was rated positively ( $r = -.34$  [-.43, -.25],  $p < .001$ ,  $k = 42$ ). We hypothesised the effect would be weaker but still significant across all emotional valences, but this was not the case: For future thinking that was neutral in valence, the effect was weaker, and did not reach significance ( $r = -.08$  [-.20, .03],  $p = 0.15$ ,  $k = 26$ ), and for negative future thinking, the effect was close to zero ( $r = .06$  [-.03, .15],  $p = .21$ ,  $k = 52$ ). We ran pairwise comparisons on these three subgroups of emotional valence, with Bonferroni-adjusted  $p$ -values to control the family-wise error rate. The difference between positive and neutral subgroups was significant,  $r_{diff} = .26$  [.13, .38],  $p < .001$ , as was the difference between positive and negative subgroups,  $r_{diff} = .40$  [.36, .43],  $p < .001$ . The difference between neutral and negative subgroups was not significant,  $r_{diff} = .14$  [.02, .26],  $p = .071$ . In cases where studies only reported values for positive and negative conditions combined, the correlation between depression and specificity was non-significant ( $r = -.14$  [.01, -.29],  $p = .067$ ,  $k = 13$ ). See



**Figure 3.** Strength of the relationship between depression and future specificity for the overall effect size (estimated from the main random effects model) and each subgroup of emotional valence. The ‘combined’ condition includes studies for which only results of positive and negative conditions together were reported. Error bars are 95% CIs.

Figure S3-S6 in Supplemental Materials for forest plots that visualise the effect sizes within each subgroup.

The effect of *macro- versus micro-level specificity* was not significant,  $Q(1) = 1.30, p = .25$ , diverging from our prediction of a stronger effect for micro measures. The relationship between depression and specificity was negative and significant for both macro ( $r = -.15 [-.23, -.06], p < .001, k = 38$ ) and micro measures of specificity ( $r = -.11 [-.19, -.03], p = .005, k = 51$ ).

The effect of *cue type* was not significant,  $Q(2) = 2.54, p = .28$ , diverging from our prediction of a stronger effect for single word than event cues. Although the difference between subgroups was not significant, the correlation between depression and specificity was largest for studies using single word cues ( $r = -.21 [-.35, -.07], p = .003, k = 27$ ); the effect was also significant for open-ended cues ( $r = -.11 [-.22, -.01], p = .037, k = 38$ ), but not significant for event cues ( $r = -.05 [-.20, .10], p = .53, k = 27$ ).

The effect of *specificity self- versus researcher-rated* was not significant,  $Q(1) = 3.22, p = .07$ , diverging from our prediction of a stronger effect for self-rated measures of specificity. The relationship between depression and specificity was negative and significant when researchers coded participants' responses ( $r = -.17 [-.26, -.08], p < .001, k = 47$ ). The effect was also negative, but non-significant, when participants rated their own thoughts ( $r = -.07 [-.17, .02], p = .14, k = 42$ ).

The effect of *depression self- versus- researcher-rated* was also not significant,  $Q(1) = 1.41, p = .23$ , in line with our prediction. The relationship between depression and specificity was negative and significant when depression was diagnosed/scored by a clinician or researcher ( $r = -.25 [-.46, -.03], p = .027, k = 7$ ) and when assessed via participant self-report ( $r = -.11 [-.18, -.03], p = .006, k = 79$ ).

The effect of *categorical versus dimensional measures of depression* was significant,  $Q(1) = 3.90, p = .048$ , but in the non-predicted direction. The relationship between depression and specificity was negative and significant for both subgroups, but stronger (more negative) for categorical ( $r = -.19 [-.29, -.09], p < .001, k = 35$ ) than dimensional measures ( $r = -.09 [-.17, -.01], p = .035, k = 54$ ). This was the only moderator for which the removal of outliers (described in Methods) altered the

interpretation of results; the effect shifted from significant to non-significant,  $Q(1) = 3.31, p = .07$ , after removing outliers.

### Exploratory moderator analyses

In addition to planned, preregistered analyses, we also explored the effects of two moderator variables not identified in the preregistration: *sex of participants* and *mode of future thinking*. We made no prediction about the moderating influence of *sex*, but were motivated to explore its effects given the previously reported sex differences in the detail and vividness of autobiographical memories (Grysmann & Hudson, 2013). The effect of sex was strong and statistically significant,  $Q(1) = 5.05, b = .39, [.05, .74], p = .025$ , indicating the correlation between depression and specificity was weaker (less negative) in samples with a higher proportion of females. In general, samples were skewed towards more females—across the 46 studies, 68% of all participants were female, and only three samples had more males than females.

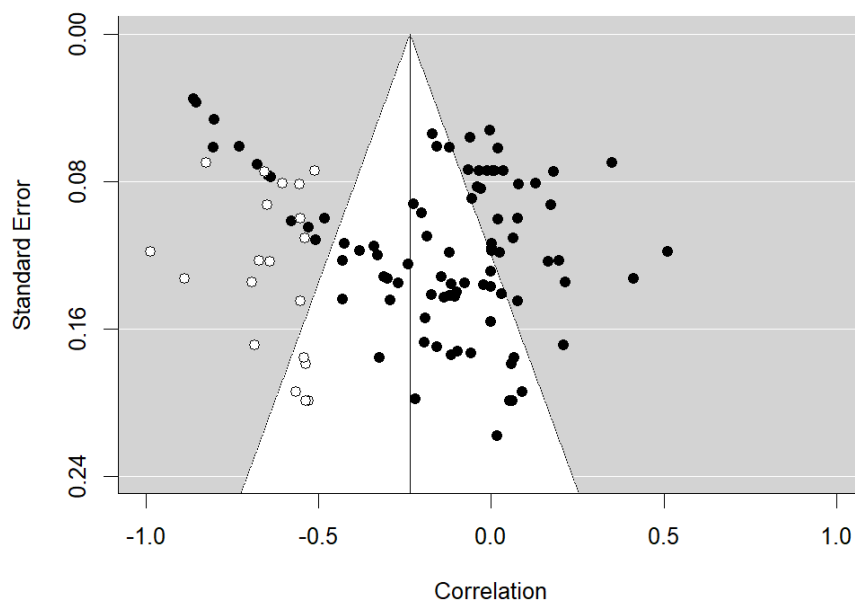
The moderator variable *mode of future thinking* was added to explore differences across simulation, intention, prediction, and planning modes of prospection (Szpunar et al., 2014). For example, some measures elicited future event simulations (e.g., the Adapted Autobiographical Interview in King et al., 2011), whereas others related explicitly to intentions (e.g., the Personal Strivings Listing in Emmons, 1992) and/or planning (e.g., the Measure for Eliciting Positive Future Goals and Plans used by Hadley & MacLeod, 2010). None of the included studies was categorised as being the prediction mode of future thinking<sup>18</sup>. We expected the relationship between specificity and depression to be stronger for intentions than simulations, given that goals are generally positive, and having noted the strong effect for positive future thinking described above. Depression has also been closely linked to goal-dysregulation in prior literature (Street, 2002). However, the effect of mode of future thinking was not significant,  $Q(1) = 0.46, p = .80$ , with the relationship between depression and specificity being of a similar magnitude for simulation ( $r = -.13 [-.21, -.05], p = .002, k = 69$ ), intention ( $r = -.10 [-.24, -.03], p = .134, k = 14$ ), and planning modes ( $r = -.13 [-.28, .02], p = .090, k = 6$ ).

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<sup>18</sup> We are unaware of any studies on the “specificity of prediction”; perhaps this would entail ratings not only on the probability of future events but on participant’s confidence around those predictions.

### Assessment of bias

We used several techniques to examine publication and reporting biases across the included studies. First, we examined if the meta-analysis showed evidence of “small-study effects”—wherein smaller studies often show different, stronger effects than larger studies, possibly reflecting publication bias (Schwarzer, Carpenter, & Rücker, 2015). To this end we inspected a funnel plot of the relationship between effect size and standard error. If a meta-analysis is free from small-study effects, effect sizes derived from larger samples (and thus with smaller standard errors) are expected to cluster around the mean, whereas effect sizes derived from smaller samples (and thus with larger standard errors) should be broadly dispersed and distributed symmetrically around the mean, forming a funnel-like shape. The funnel plot for this meta-analysis clearly deviated from an unbiased model (see Figure 4). Oddly, a number of effect sizes with small standard errors were dispersed far from the mean, particularly towards the left side of the plot; in other words, a few studies with apparent high precision showed very strong negative correlations between depression and future specificity. This suggested the meta-analysis may have missed other correlations that were also stronger (more negative) than the mean.



**Figure 4.** Funnel plot of observed effect sizes (black circles) and those estimated by trim-and-fill analysis to be missing from the meta-analysis (white circles).

A Duval and Tweedie (2000a, 2000b) trim-and-fill analysis supported this interpretation, estimating that 15 effect sizes were missing from the left side of the funnel plot (shown as white circles in Figure 4), and that inclusion of the missing effect sizes in the random-effects model would increase the magnitude of the summary estimate to  $r = -.24 [-0.30, -0.17]$ ,  $p < .001$ . Interestingly, this observation is the reverse of what would be expected from typical publication bias, where missing unpublished studies are those with weak or null effects.

Second, to test for inflation of the effect in published literature, relative to the true effect, we compared the magnitude of effect sizes in published versus unpublished studies. A moderator analysis showed that the effect of publication status was significant,  $Q(1) = 5.68$ ,  $p = .017$ , indicating stronger (more negative) correlations between depression and specificity in published ( $r = -.15 [-.22, -.07]$ ,  $p < .001$ ,  $k = 76$ ) versus unpublished studies ( $r = .06 [-.11, .23]$ ,  $p = .48$ ,  $k = 13$ ). In contrast to inspection of the funnel plot, this analysis indicated typical publication bias; that is, the true correlation between depression and specificity may be smaller than the summary estimate. It should be noted, though, that only six independent samples contributed to the unpublished subgroup of effect sizes.

Third, we assessed whether the quality of studies influenced the strength of the effect. The moderating effect of quality was not significant  $Q(1) = .31$ ,  $b = -0.01 [-.04, .03]$ ,  $p = .58$ , suggesting the included effect sizes were not biased by differences in methodological quality. Quality ratings for each study are presented in Table S1. Looking across included studies, marked strengths and weaknesses emerged—for example, nearly all studies clearly described hypotheses (98% of studies), tasks and measures (100%), and main findings (100%), but few reported power analyses (12%), withdrawals and dropouts (37%), or participant engagement with tasks (49%); see Table S2 for descriptive statistics for all 18 items.

Finally, we ran a  $p$ -curve analysis (Simonsohn, Nelson, & Simmons, 2014) to assess whether the  $p$ -value distribution for statistically significant ( $p$ -values  $< .05$ ) effect sizes in the meta-analysis aligned with the  $p$ -value distribution expected from a true effect. A  $p$ -curve for a true effect should be right-skewed; it should contain more low (.01s) than high (.04s) significant  $p$ -values (Simonsohn et al., 2014). The  $p$ -curve for this meta-analysis (generated via the app at [p-curve.com](http://p-curve.com)) was heavily right-skewed, indicating no evidence of publication bias or selective reporting of significant results in



included studies (see Figure S7 in Supplemental Materials). The *p*-curve analysis also provided an estimate of the statistical power of studies that yielded significant *p*-values; for this meta-analysis, power was estimated to be 99%, 90% CI [98, 99%], indicating these studies, on average, were well-powered to detect true effects.

### Discussion

Overgeneral memory has been well-established in depression (van Vreeswijk & De Wilde, 2004; Williams et al., 2007) but whether or not overgenerality extends from past to future thinking has not, until now, been the focus of a comprehensive meta-analysis. By examining the currently available evidence from a range of sources—including six electronic databases—this meta-analysis provides the most complete account to date of the links between depression and the specificity of future thinking. Importantly, we show how differences in methods and participant characteristics can have a profound impact on the strength of the effect.

We found that, on average, higher levels of depression were weakly correlated with reduced specificity of future thinking ( $r = -.13$ ). Variation in future specificity could explain only 1.6% of the variation in levels of depression; however, the relationship was highly significant, indicating a small but reliable effect. Results also revealed substantial heterogeneity across the true effect sizes of included studies. To examine the source of this heterogeneity, we ran moderator analyses on several variables related to differences in samples and study designs. Though most effects were non-significant, we identified three key variables that had a significant effect on the strength of the relationship between depression and specificity: the emotional valence of future thinking, whether depression was measured categorically or dimensionally, and the sex of participants. We discuss each significant moderator in turn, before addressing the other variables.

#### Significant moderators

We predicted depression would be linked to reduced future specificity of all emotional valences, but that the effect would be strongest for positive future thinking—a hypothesis based largely on prior empirical findings (e.g., Dickson & Bates, 2006; Stöber, 2000). Specificity was indeed most clearly reduced in depression for positive future thinking. The discrepancy in the effect

across emotional valences was even larger than expected—for neutral future thinking, the effect was small and non-significant, and for negative future thinking, the effect disappeared entirely.

These findings have important implications because they run counter to the most influential model of overgenerality in depression: the CaR-FA-X model (Williams et al., 2007). This model describes how rumination, functional avoidance, and executive dysfunction disrupt the search and retrieval of specific events, and although originally applied to overgeneral *memory*, the same factors should also hinder the generation of specific *future* events (Dickson & Bates, 2005; Williams et al., 2007), especially in light of emerging neuroscientific evidence that episodic memories and future simulations are instantiations of the same underlying process (Addis, 2018). The CaR-FA-X model sets up specific predictions regarding valence, namely that functional avoidance should result in the truncation of *negative* future thinking to avoid unpleasant or painful fragments coming to mind (Williams et al., 2007). In contrast, other components in the model such as poor executive function, should impact the specificity of future events irrespective of valence. Thus, by this account, future thinking of any valence is expected to be overgeneral in depression, but negative future thinking should be *especially* overgeneral.

Our finding that depressed and dysphoric people exhibited a significant reduction in specificity of positive but not negative events runs counter to the CaR-FA-X model. Rather, this pattern of results may be more parsimoniously explained—at least in part—by mood-congruence memory effects. Specifically, given that imagining specific future events relies on the retrieval of relevant details from memory (Schacter et al., 2012), it is plausible that the well-established reduction in the accessibility of positive memories with depressed/dysphoric mood (reviewed in Eich, 1995; Matt, Vázquez, & Campbell, 1992) could alter the ability to generate specific positive future events. One way to test this idea would be to examine overgenerality of future thinking in individuals with *remitted* depression; if mood-congruence largely accounts for overgenerality, and these individuals are no longer in a negative mood state, they should no longer exhibit the effect. Unfortunately, only two effect sizes in the meta-analysis were derived from samples exclusively of participants with remitted depression. Although these samples showed *no* overgenerality, we hesitate to draw inferences from so few cases. Future studies comparing the specificity of future thinking in those with

remitted versus current depression will help to resolve whether overgenerality can be better explained by trait or state (e.g., negative mood) factors.

Whatever the underlying mechanism, these findings have important clinical implications. Depressed and dysphoric individuals appear not to suffer a broad inability to imagine the future, but rather struggle to imagine specific *positive* futures. It follows that the relatively intact ability to imagine in these individuals might be harnessed or directed towards positive future thinking in a way that could be beneficial. For example, a recent study showed that the generation of vivid, positive mental imagery for planned rewarding activities increased motivation for (and actual completion of) those activities, relative to a non-imagery control condition (Renner, Murphy, Ji, Manly, & Holmes, 2019). Such effects might be particularly relevant for depressed and dysphoric individuals, who are less likely to plan and engage in rewarding activities, thus depriving themselves of potentially positive experiences, and perpetuating low mood (Holmes, Blackwell, Burnett Heyes, Renner, & Raes, 2016). The findings of this meta-analysis suggest that the underlying capacity to vividly imagine the future is still present in depression and dysphoria—and perhaps can be repurposed more positively.

Of the other preregistered moderators, only the effect of categorical versus dimensional measures of depression was statistically significant. Studies with categorical designs (e.g., healthy controls versus dysphoric subjects) showed a slightly stronger relationship between depression and future specificity than studies with dimensional designs (e.g., depression measured on a continuous scale such as the BDI-II). This is surprising statistically, as the dichotomisation of a continuous predictor variable—which is what occurs in categorically-designed studies—typically leads to reduced power and *underestimation* of the strength of relationships (Hunter & Schmidt, 1990; Maxwell & Delaney, 1993). A possible explanation is that this moderator was confounded by other variables. For instance, studies with categorical designs typically encompassed samples with a wider spectrum of depressive symptoms (e.g., spanning from healthy controls to MDD patients) than those with dimensional designs (e.g., spanning from healthy controls to dysphoric participants). Samples spanning a wider spectrum of depression showed a slightly stronger (though not significantly different) effect than those spanning a narrower spectrum of depression, which may have contributed to the slightly larger effect in categorical than dimensional designs. This moderator analysis was the

only one substantially impacted by outliers, as removal of the five most influential effect sizes (identified via Cook's  $D_i$ ) shifted the result to non-significant. As such, we hesitate to draw any strong conclusions about the moderating effect of categorical versus dimensional designs.

The third and final significant moderator variable was sex: The relationship between depression and specificity was weaker in samples with a higher proportion of females. Although the analysis was unplanned (exploratory), the effect was strong and significant and remained so after the removal of outliers. We are unaware of any previous suggestion that the effect of overgenerality may be stronger for depressed men than women. Some studies, though, have reported that women tend to generate more elaborate, detailed and vivid autobiographical memories than men (Grysmen & Hudson, 2013), and such a sex difference may extend to the imagination of future events (Wang, Hou, Tang, & Wiprovnick, 2011). If this is the case, perhaps women have more of a 'buffer' against the impacts of depressive symptomatology than men; that is, their ability to project richly and specifically into the future may remain intact for longer in depression. Interestingly, women are also more likely to engage in rumination than men (Nolen-Hoeksema & Jackson, 2001). If overgenerality in depression is driven partly by rumination, as the CaR-FA-X model suggests, specificity should be *particularly* reduced in women compared to men, and yet we found the opposite—possibly pointing to other causes of overgenerality as mentioned above. Our results also highlighted the relative lack of males in studies in this area (only 32% of the total 4,813 participants were men), suggesting future research should aim for more representative samples. We propose further examination of sex differences in overgenerality is warranted: If men are especially impacted in depression, men might also be especially amenable to future interventions that *improve* prospection.

### **Non-significant moderators**

We now turn to the moderator variables that yielded non-significant effects. As predicted, the clinical status of participants did not have a significant effect on the link between depression and specificity—specificity was impacted whether participants had subclinical depression (dysphoria) or a clinical diagnosis of MDD. As aforementioned, the effect was marginally stronger in samples that spanned healthy to MDD subjects, versus healthy to dysphoric subjects, but the difference was non-significant. These findings are generally consistent with a dimensional model of depression; i.e., that

depression varies along a continuum of severity throughout the general population, with no sharp distinction in the symptomology of those with sub-threshold and above-threshold depression (Ayuso-Mateos et al., 2010). If depression is truly continuous, one would also expect to see (small) correlations between specificity and depression *within groups* only with low-mild symptoms, or dysphoria, or MDD. Although we did not observe significant effects within these subgroups, correlations were similar in magnitude to the summary estimate. Considering this, and that these latter subgroups had only few cases, we cannot be sure whether the null findings reflect true null effects or simply insufficient power.

The two other moderator variables relating to participants—comorbid anxiety and age—also had non-significant effects. Few studies grouped or excluded participants based on comorbid anxiety (each subgroup contained only six effect sizes), so little data was available for this analysis, and statistical power was low. It would be worthwhile to examine interactions between the moderators comorbid anxiety and emotional valence, as existing research suggests anxious individuals might show *enhanced* specificity for future negative events, but not positive events (MacLeod, Tata, Kentish, & Jacobsen, 1997; MacLeod et al., 2005; Morina et al., 2011). Again, it was not feasible to investigate this question in the current study; interaction analyses would require more data.

As for age, we expected the effect to be stronger in older samples, given future thinking in older depressed individuals may suffer compounding effects of normal ageing and repeated depressive episodes (King et al., 2010; Schacter et al., 2013). That age was non-significant ran contrary to our prediction, but the lack of older samples in the meta-analysis may have reduced the likelihood to detect an effect. Surprisingly few studies examined overgenerality and depression in older adults (only five independent samples had a mean age greater than 50 years), suggesting this question could be a key avenue for future research.

The type of cue used to prompt participants to think about the future was also a non-significant moderator. Nonetheless, this analysis did reveal that, when considering only effect sizes from studies that used more supportive ‘event’ cues, there was no evidence that specificity is reduced in depression. Event cues (e.g., “New Year’s Eve” or “Christmas dinner”) arguably provide a scaffold for accessing and generating more specific events than single word cues (e.g., “party” or “dinner”);

Addis et al., 2016). Whether the null result for event cues is due to insufficient data, or whether a more supportive scaffold does indeed mitigate overgenerality in depression, will need to be addressed in further studies.

That micro versus macro level measures of specificity also had no significant moderating effect suggests overgenerality in depression is robust regardless of the level at which specificity is measured. In other words, specificity seems to be similarly reduced in depression in terms of the spatiotemporal specificity of events themselves as well as the episodic detail or vividness within those events. Similar findings emerged for whether specificity and depression were measured by the researcher or participant self-report, and whether specificity pertained to future simulations, intentions, or plans. That is, reduced specificity was evident in depression regardless of differences in these methods or modes of future thinking—again suggesting this is a robust effect.

### **Are the results biased?**

Interestingly, the overall effect we observed here for future thinking and depression ( $r = -.13$ ) was far smaller than the moderate-large effect sizes reported in previous meta-analyses on overgeneral *memory* and depression. Williams et al. (2007) reported a mean Cohen's  $d$  of 1.12 across 12 studies, which translates to a biserial-correlation of .61, and van Vreeswijk and de Wilde (2004), across 14 studies, reported Spearman's rho correlations of -.66 and .59 between depression and specific positive memories, and depression and negative overgeneral memories, respectively. The latter meta-analysis did include samples with comorbid psychiatric diagnoses (in addition to anxiety), which may have inflated effect sizes compared to samples with depression alone. Similarly for the recent meta-analysis on depression and future specificity by Hallford et al. (2018), the reported effect size was large; across the seven included studies, Hedge's  $g$  was 0.79. It is difficult to explain the discrepancy in effect sizes, particularly as our meta-analysis included the seven studies examined by Hallford et al. (2018). Also, one might have expected specificity in depression to be even *more* impacted for future thinking than memory, as imagining novel scenes is more cognitively demanding than reconstructing the past (Addis et al., 2016). The discrepancies then beg the question: Are the results in the current meta-analysis affected by bias?

Across the four methods we used to assess publication and/or reporting bias, results were mixed. On one hand, the funnel plot (Figure 4) revealed a strange pattern of a few very large and precise effect sizes—precision that would not be expected from the small samples in those studies, and we ensured these effect sizes did not arise from obvious misreporting or mistakes in data extraction. Trim-and-fill analysis suggested the meta-analysis may have missed additional studies with large effect sizes, meaning the true effect size might be larger. On the other hand, a moderator analysis showed that unpublished studies had weaker correlations on average than published studies, indicating potential publication bias in the other direction; that is, weak or null effects might remain hidden in the “file drawer”. And yet, clouding the picture further, differences in study quality were not related to the size of effects, and the *p*-curve for significant results matched what would be expected from a true effect. So, overall, the mixed results make it difficult to draw conclusions about bias, although we find no particularly compelling evidence that the real effect size should be substantially different to the summary estimate. We have confidence in the current estimate as the search process was extensive, the number of included effect sizes and total *N* relatively large, and the analyses included multiple measures of specificity, corrected for dependency between outcomes. And, importantly, additional analyses were run without outliers to ensure conclusions were not impacted by a few extreme studies.

### **Future specificity in depression: What else matters?**

While some moderators had a notable effect on the relationship between depression and specificity, none came close to explaining all or even most of the heterogeneity across studies. What else might account for some of this variability? As mentioned, complex interactions between moderators (such as comorbid anxiety and emotional valence) may have affected the relationship between future specificity and depression, but the small samples in some subgroups precluded our ability to run interaction analyses. Other factors not included in the current meta-analysis may have also had important moderating effects. Antidepressant medication, for example, could mitigate some of the effects of overgenerality in depression. Chronic antidepressant treatment has been found to have a neuroprotective effect on the hippocampus (Huang et al., 2013), a brain region critical for episodic memory and future thinking (Addis & Schacter, 2012) that tends to be reduced in volume in

depression (Malykhin, Carter, Seres, & Coupland, 2010). Participants' history of depression may be another key moderator, as repeated depressive episodes over time could amplify the extent of overgenerality (King et al., 2010). It might also be valuable to examine the effect of participants' history of *therapy*, as common therapies such as CBT often involve shifting clients' perceptions of the future (Seligman, Railton, Baumeister, & Sripada, 2013). Finally, given the multitude of tasks tapping into aspects of future specificity (26 different tasks were used across included studies), there were likely subtle differences between the tasks not captured by the current coding of methodological moderator variables.

To meta-analyse the effect of antidepressant medication, history of depression, or history of therapy would require either access to participant-level data or the categorising of depressed participants into subgroups (such as medicated versus non-medicated), which was rarely done in the included studies. This situation is a clear example of the potential benefits of sharing (de-identified) participant-level data<sup>19</sup>; if this information had been collected and shared in some of the included studies, moderator analyses could be performed without the need to run additional studies. Moreover, running a 'mega-analysis' (i.e., on participant- rather than study-level data) would reduce the chance of both false positive and false negative effects (Costafreda, 2009). With online tools such as the Open Science Framework now making it easier to share data, we hope it will be possible to conduct more powerful (and thus more illuminating) analyses on specificity in depression in the coming years.

## **Conclusion**

Based on the currently available evidence derived from 46 studies, this meta-analysis has demonstrated a small but robust association between higher levels of depression and the reduced specificity of future thinking. While the magnitude of the effect was small, especially compared to previous meta-analyses on overgenerality in depression, we do not think it is necessarily trivial; even a small correlation may be relevant from a practical and clinical perspective when it relates to a variable as consequential as the severity of depression. Indeed, an intervention targeting future episodic specificity has already been shown to reduce negative affect, boost positive affect, and

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<sup>19</sup> Of course, such data sharing is only possible when participants have provided informed consent and when data sharing is permissible by a country's relevant privacy laws.



increase coping behaviours (Jing et al., 2016). This meta-analysis furthers our understanding of future thinking in depression and dysphoria by examining the people, and the conditions, for which overgenerality is most apparent; namely, specificity is most impacted when attempting to imagine positive future events (and not at all affected when imagining negative events) and seems to be more severe in depressed men than women. We hope future interventions, guidelines, and policies can take advantage of these findings to highlight aspects of future thinking that are most in need of improvement. Finally, with our data and analysis scripts publicly available, we aim to make this meta-analysis transparent—and easily updated as new research comes to light.

**Registration**

The meta-analysis protocol for this project was registered on the Open Science Framework (OSF) on 27 November 2017 (<https://osf.io/evdkf/>).

**Author contributions:**

BG drafted the manuscript. BG, DM and DRA contributed to the conception and design of the study. DRA and LJT provided expertise on future thinking in depression. DM provided statistical expertise. All authors read, provided feedback, and approved the final manuscript. BG is the guarantor.

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