Is a Procedural Learning Deficit a Causal Risk Factor for Developmental Language Disorder or Dyslexia? A Meta-Analytic Review

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Impaired procedural learning has been suggested as a possible cause of developmental dyslexia (DD) and developmental language disorder (DLD). We evaluate this theory by performing a series of meta-analyses on evidence from the six procedural learning tasks that have most commonly been used to test this theory: the serial reaction time, Hebb learning, artificial grammar and statistical learning, weather prediction, and contextual cuing tasks. Studies using serial reaction time and Hebb learning tasks yielded small group deficits in comparisons between language impaired and typically developing controls ($g = -.30$ and $-.32$, respectively). However, a meta-analysis of correlational studies showed that the serial reaction time task was not a reliable correlate of language-related ability in unselected samples ($r = .03$). Larger group deficits were, however, found in studies using artificial grammar and statistical learning tasks ($g = -.48$) and the weather prediction task ($g = -.63$). Possible reasons for the discrepancy in results from different tasks that all purportedly measure procedural learning are highlighted. We conclude that current data do not provide an adequate test of the theory that a generalized procedural learning deficit is a causal risk factor for developmental dyslexia or developmental language disorder.

Keywords: developmental dyslexia, developmental language disorder, meta-analysis, procedural learning, specific language impairment

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Proficient reading and oral language skills are critically important for school performance, employment prospects and psychosocial well-being. Unfortunately, many children struggle to develop such skills. Understanding the cognitive causes of such difficulties is a crucial step toward developing the best assessments and treatments for those at risk. According to the procedural deficit hypothesis (Nicolson & Fawcett, 2007, 2011; Ullman, 2004; Ullman & Pierpont, 2005) a key risk factor for language learning disorders, such as developmental dyslexia (DD) and developmental language disorder (DLD), is impaired procedural learning (a deficit in an unconscious learning system that is critical for abstracting the rule-based structures of language). This theory has generated much scientific debate. A search in Google Scholar of the terms "dyslexia + procedural + memory + deficit" returns more than 9,000 results, while a similar search substituting "specific

language impairment" for "dyslexia" returns more than 6,000 results. However, results from studies examining the theory are highly inconsistent. Here we present a series of meta-analyses to evaluate the evidence relevant to this theory. We focus both on the nature of language learning impairments and on the mechanisms of language learning. Evaluating the evidence for a procedural memory impairment in language learning disorders is also relevant to a central theoretical debate within cognitive psychology: the putative distinction between procedural and declarative learning (or implicit (unconscious) versus explicit (conscious) learning).

Developmental disorders of language learning involve difficulty in processing linguistic information, which can affect production and understanding of both spoken and written language (Hulme & Snowling, 2009). Two of these disorders are the focus of much research: developmental language disorder and developmental dyslexia. Children with developmental language disorder (previously referred to as specific language impairment or SLI) have difficulty in learning language, and sometimes impaired processing and production of the sounds of speech, in spite of normal nonlinguistic cognitive development (Bishop, 2006; Bishop et al., 2016). The patterns of impairment seen in developmental language disorder are highly variable (Bishop et al., 2016; Hulme & Snowling, 2009), and these difficulties can persist into adulthood. Developmental dyslexia is characterized by problems in learning to decode printed words, which adversely affect reading fluency and spelling, as well as reading accuracy (Hulme & Snowling, 2016; Snowling, 2013; Vellutino et al., 2004).

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Developmental language disorder and developmental dyslexia are distinct but frequently comorbid disorders (Bishop & Snowling, 2004), not least because both reading and writing are scaffolded on oral language (Fletcher, 2009). They are also frequently comorbid with other developmental disorders, including attention deficit hyperactivity disorder (McGrath et al., 2011; Pennington, 2006) and developmental coordination disorder (Hill, 2001; Hulme & Snowling, 2009). Both disorders occur in around 3% to 7% of the population (Snowling, 1998; Tomblin et al., 1997), although prevalence estimates vary depending on the diagnostic criteria used (Snowling, 2013). The impairments seen in both disorders are dimensional in nature (Fletcher, 2009; Shaywitz et al., 1992). People with developmental language disorder or developmental dyslexia represent the lower end of a normal distribution in spoken language or reading ability, rather than a distinct category. As such, research into these disorders and into the normal development of language and reading can be seen as two sides of the same coin.

The Procedural Deficit Hypothesis

Much research into developmental language disorder and developmental dyslexia has focused on understanding their cognitive bases, not least because establishing the exact nature of the cognitive impairments that underpin these disorders is crucial for guiding effective interventions. A recent cognitive level explanation for developmental language disorder and developmental dyslexia claims that both disorders may arise from a deficit in a procedural memory system (Nicolson & Fawcett, 2007; Ullman & Pierpont, 2005).

The procedural deficit hypothesis claims that an implicit (procedural) memory system is involved in the learning, storage and retrieval of the statistically regular, rule-based, features of grammar and phonology (Ullman, 2004). It has been suggested that sequence-based implicit learning is particularly implicated in language disorders (Nicolson & Fawcett, 2010), whereas a declarative memory system functions normally and is responsible for the associative binding of phonological or orthographical representations and meanings (Ullman, 2004). The procedural deficit hypothesis proposes that lexical knowledge in developmental language disorder may often be less impaired than grammatical skills, because acquiring lexical knowledge depends on a relatively intact declarative memory system that may compensate for weaknesses in the procedural memory system (Ullman & Pierpont, 2005). We should note that this theory rests on making a distinction between stored and computed linguistic forms (lexicon vs. grammar) which depend for their acquisition on distinct memory systems (procedural vs. declarative). However, this claim is controversial and there is growing evidence against the existence of a clear distinction between words and larger sequences, and the mechanisms used to process them (e.g., Christiansen & Arnon, 2017; McClelland et al., 2010; Snider & Arnon, 2012).

It may be useful to clarify terminology. Procedural learning as it relates to the procedural deficit hypothesis of language learning disorders is also sometimes referred to as implicit or statistical learning. The terms procedural learning, implicit learning and statistical learning refer to the same incidental learning processes and are largely synonymous (Berry & Dienes, 1993; Perruchet & Pacton, 2006; Shanks, 2005) or at least overlapping (Seger, 1994). A task is allegedly learned implicitly if procedural knowledge develops without, or at least before, any declarative knowledge: that is to say, a person has developed the ability to perform a task without, or before, being able to give an explicit account of what they are doing. In this review the terms implicit learning and procedural learning will be used interchangeably, in line with the terminology used in different groups of studies, but this should not be taken to imply that different learning mechanisms are involved in the different tasks used.

Measuring Procedural Memory

There are three important issues that should be noted before we proceed to consider the empirical evidence that has been used to test the theory. First, the procedural deficit hypothesis rests on the assumption that there are separable, but interconnected, procedural and declarative memory systems in the brain (Ullman, 2004). Such a claim remains controversial and is far from universally accepted (e.g., Henke, 2010; Perruchet & Pacteau, 1990).

Second, from a methodological perspective, it is far from clear which tasks actually measure implicit learning (e.g., Arnon, 2020; Erickson et al., 2016; Krishnan & Watkins, 2019; Siegelman et al., 2017; West et al., 2018, 2019) and to what extent the different tasks used measure a common (procedural or implicit) learning mechanism. In this review we therefore take a broad perspective, including all tasks that have typically been used as measures of procedural learning in the literature (summarized in Table 1). In spite of the large variation in tasks, Ullman and Pierpont (2005) claim that one of the strengths of the procedural deficit hypothesis is that it makes "testable predictions" (p. 401), and proponents of this view assert that the tasks used are valid measures of procedural learning.

There are now many studies assessing procedural learning, language, and language disorders using several different tasks. However, the results across studies are highly inconsistent (e.g., Conti-Ramsden et al., 2015; Mayor-Dubois et al., 2014). These studies typically use extreme group designs (whereby a group of participants with severe reading or language difficulties is compared with a control group with normal reading or language skills). In addition, research using correlational designs that explore the relationship between implicit learning and language ability in samples unselected for ability is less plentiful, but findings are also mixed. A major aim of our review is to clarify what empirical claims are justified by results to date. Clarifying the pattern of empirical findings is critical for then moving on to reach conclusions about the theoretical status of the procedural deficit hypothesis.

Finally, a major logical problem for the procedural deficit hypothesis is that it claims to provide the same cognitive explanation for two disorders with markedly different cognitive profiles (developmental dyslexia and developmental language disorder). It is not clear how a common deficit in procedural learning can account for why some children develop a language disorder (a heterogeneous condition involving multiple problems in acquiring both receptive and expressive language skills that cut across the domains of syntax, semantics, and phonology), whereas other children develop dyslexia (a specific deficit in learning to decode printed words accurately and fluently).

Reviews in this area also present inconsistent findings (e.g., Lum et al., 2013; Lum et al., 2014; Schmalz et al., 2017; van Witteloostuijn et al., 2017). Notably, all previous meta-analyses limit their coverage to a single language disorder and exclude measures that are relevant to the debate. It should also be noted that some have reported effect sizes that are almost certainly inflated (Lum et al., 2013; Lum et al., 2014; Obeid et al., 2016). These previous meta-analyses calculated effect

Table 1
Implicit Learning Tasks

Task	Task description	Implicit learning measure
The serial reaction time task (SRT: Nissen & Bullemer, 1987)	Participants press buttons to indicate which of four possible locations a stimulus occupies on a screen. Successive stimuli may be random or conform to a covert sequence.	Shorter response times to trials conforming to a covert sequence, compared to random trials, provide evidence of implicit learning.
The Hebb serial order learning task (Hebb, 1961)	Participants recall sequences of items in order. A covert repeated sequence is included at regular intervals across trials.	Better recall for the repeated as opposed to nonrepeating sequences provides evidence of implicit learning.
Artificial grammar (AGL: Reber, 1967) and statistical learning tasks (SL: Arciuli & Simpson, 2011)	Participants are presented with strings of stimuli that conform to an undisclosed set of combinatory rules (grammar) and judge whether new strings conform to, or violate, these rules. AGL grammars are usually complex, while stimulus strings in SL tasks conform to a simpler (base triplet) structure.	Above chance performance is taken to reflect implicit learning. (NB: this measure reflects declarative knowledge for implicitly learned information).
The weather prediction task (Knowlton et al., 1994)	Participants classify combinations of four possible stimuli into one of two possible outcomes. The stimuli each have a fixed probability of a certain outcome.	A trial is scored correct if it accords with the conditional probabilities of the stimuli shown. Above chance performance is taken as evidence of implicit learning.
Contextual cuing task (Chun & Jiang, 1998)	Participants identify the location of a target stimulus within matrices of distractor stimuli as quickly as possible. The target position in some matrices is predictable.	Shorter response times to predictable compared to unpredictable matrices provides evidence of implicit learning.

sizes using a single standard deviation for each group for the difference between sequenced and random trials (see Siegert et al., 2006). This method, however, will result in an overestimated effect size, since the standard deviation in the raw scores for each condition will be larger than the standard deviation of the difference scores (Lund, 1988; Morris & DeShon, 2002; Ray & Shadish, 1996). See Supplemental Material 1 (Elaborated Methodological Information) for a more detailed discussion of this issue.

Scope and Aims of the Current Review

The current review takes a far wider view of implicit learning and language learning than any previous meta-analyses and examines some moderating factors that may explain why studies have reached different conclusions. In particular, each meta-analysis reported here includes studies of participants with developmental language disorder, as well as those with dyslexia. While previous meta-analyses have confined themselves to a single disorder, this exclusivity is questionable, given the heterogeneity of symptoms in language disorders (Peterson & Pennington, 2015; Webster & Shevell, 2004) and their frequent comorbidity (Bishop & Snowling, 2004; Catts et al., 2005; Krishnan et al., 2016). For example, in one study (Hedenius et al., 2013), participants categorized as dyslexic, displayed scores on a test of receptive grammar (TROG: Bishop, 1982) that were on average 17 points lower than the typically developing group. The TROG test is frequently used diagnostically for developmental language disorder. Additionally, the diagnostic criteria involved in categorizing participants to groups can also vary greatly from study to study. Language-disordered groups from different studies may not, therefore, reflect the same behavioural symptoms or underlying cognitive impairments. Importantly, the procedural deficit hypothesis claims that deficits in the procedural memory system are the basis of the impairments seen in both disorders. Examining the moderating influence of disorder type may help to clarify the extent to which

the symptoms of dyslexia and developmental language disorder relate to a common procedural memory impairment.

We also examine whether the severity of language disorder in extreme groups explains variation between studies. Because there is no agreed cut-off for dyslexia or developmental language disorder, studies use samples with differing severity of symptoms. The procedural deficit hypothesis predicts that the more severe the language learning disorder, the larger the deficit in procedural learning compared with controls should be. We, therefore, examine the extent to which severity of disorder is able to explain variation in procedural learning between studies.

Another important factor to consider is the type of task used. Previous meta-analyses have typically examined only one or two different tasks. The series of meta-analyses reported here includes all of the implicit learning tasks most commonly used in this area: serial reaction time tasks, artificial grammar and statistical learning tasks, Hebb learning tasks, and probabilistic category learning tasks. Including a range of different tasks, each within its own separate meta-analysis, will allow us to examine differences between the tasks in one consistently conducted review and highlight whether some tasks demonstrate larger group differences than others. A meta-analysis of studies including contextual cuing tasks ($n = 4$) was not conducted, because there were insufficient data in the studies to calculate effect sizes.

We also examine possible moderators operating within each type of task (i.e., serial reaction time, Hebb, artificial grammar or statistical learning and weather prediction). Task modality, that is, whether task stimuli are verbal or nonverbal, may influence study results. A key claim of the procedural deficit hypothesis is that any procedural deficit in language disorders is domain general in nature (Ullman & Pierpont, 2005). Impaired learning on predominantly nonverbal serial reaction time tasks in language disordered groups has been interpreted as evidence of domain generality, but any group differences may be task specific in nature and therefore, misleading. We, therefore,

investigate the moderating effect of task modality (verbal vs nonverbal) within the same experimental tasks where possible.

Participant age is another potential moderator. Previous research points mainly to one of two possibilities: that procedural learning is age invariant (e.g., Meulemans et al., 1998; Reber, 1993) or declines in adulthood (Janacsek et al., 2012; Zwart et al., 2019). The meta-analyses here include studies in both children and adults, enabling us to investigate this question.

The majority of extreme group design studies, but not all, reported no significant differences between groups in nonverbal IQ. However, even where group differences are not significant, this may be due to limited sample sizes, and group differences in nonverbal IQ are still potentially important. For this reason, the extent to which variation in the difference in nonverbal IQ between groups reflects variation in the effect size for procedural learning is also investigated, as is the relationship between nonverbal IQ and implicit learning in correlational studies.

Research Questions in the Current Review

The research questions addressed in the current study are outlined below. Because the results from previous reviews are inconsistent, we choose not to provide hypotheses.

1. Is there evidence for a deficit on procedural learning tasks in groups with developmental language disorder and groups with developmental dyslexia? If so, are equivalent relationships found in correlational studies that examine the full variation of language/reading skills and procedural learning?

2. Are participants with developmental language disorder or dyslexia differentially impaired on measures of procedural learning?

3. Is there variability in the severity of the impairment in language disordered groups across studies and, if so, is severity a moderator of the size of procedural memory deficits across studies?

4. To what extent is any procedural learning deficit associated with these disorders domain general (i.e., is it present with both verbal and nonverbal tasks) and is the size of the deficit related to whether a task measures implicit sequence learning, rather than, for example, probabilistic categorization or visual search efficiency?

5. Can other moderating factors, such as participant age or nonverbal IQ, explain any of the inconsistency in results across studies?

Method

Inclusion Criteria

The review was preregistered on the PROSPERO database (registration number: CRD42016048759), and the meta-analyses were conducted according to the recommendations of the PRISMA statement (Moher et al., 2009). Also, additional material and the full data set is available in the project depository in the open science framework (www.osf.io).

To be included, a study had to report either (a) a group design that compared a measure of implicit learning in children or adults with developmental language disorder or dyslexia with performance of a control group(s) or (b) a correlational design that measured the relationship between performance on an implicit learning task and language measures in individual participants. In line with methodological recommendations for meta-analyses, group and correlational designs were entered into separate analyses (Borenstein et al., 2009).

Eligible studies needed to report data on at least one of the implicit learning tasks considered here (serial reaction time; Hebb learning; artificial grammar or statistical learning; probabilistic categorization; contextual cuing). See Table 1 for an explanation of these tasks. For the purposes of our meta-analyses we combined studies of artificial grammar learning (AGL: Reber, 1967) and statistical learning (Arciuli & Simpson, 2011); because these are highly similar tasks (although the extent to which they rely on the same mechanisms has been debated; see e.g., Misyak & Christiansen, 2012). The statistical learning task has been most frequently used with children, because it involves less complex rules than the artificial grammar learning task.

Group studies needed to include means and standard deviations for performance on the tasks to enable an effect size for the difference between groups to be calculated. Correlational studies had to include a measure of effect size (r) for the relationship between implicit learning and language skills. However, in practice many of the eligible studies did not include sufficient information. For serial reaction time tasks, for example, means and standard deviations for the task were usually reported only graphically. Whenever the relevant information was implied but not reported, the study was considered eligible in the first instance and the authors were contacted and additional data requested.

Search Strategy

Searches were conducted using the following electronic databases: Medline, PsycINFO, Web of Science, ERIC ProQuest, Google Scholar and ProQuest Theses and Dissertations. Figure 1 shows the search and flow of studies through the review. The search strategy combined terms relating to implicit learning with terms relating to language and language disorder and was developed in collaboration with subject specialist librarians at University College London (see Table S1 in Supplemental Material 2, for the search syntax).

Studies that shared authors, had equal number of participants, reported the same results, or used identical task measures were further investigated to limit the risk of coding the same data twice. Duplicate reports of the same study were treated as one collective report. Data reported in theses were excluded where they were possibly the same, or partially the same, as data reported in subsequent peer-reviewed papers included in the meta-analyses.

Figure 1

PRISMA Flow Chart for the Search and Inclusion Criteria for Studies in This Series of Meta-Analyses
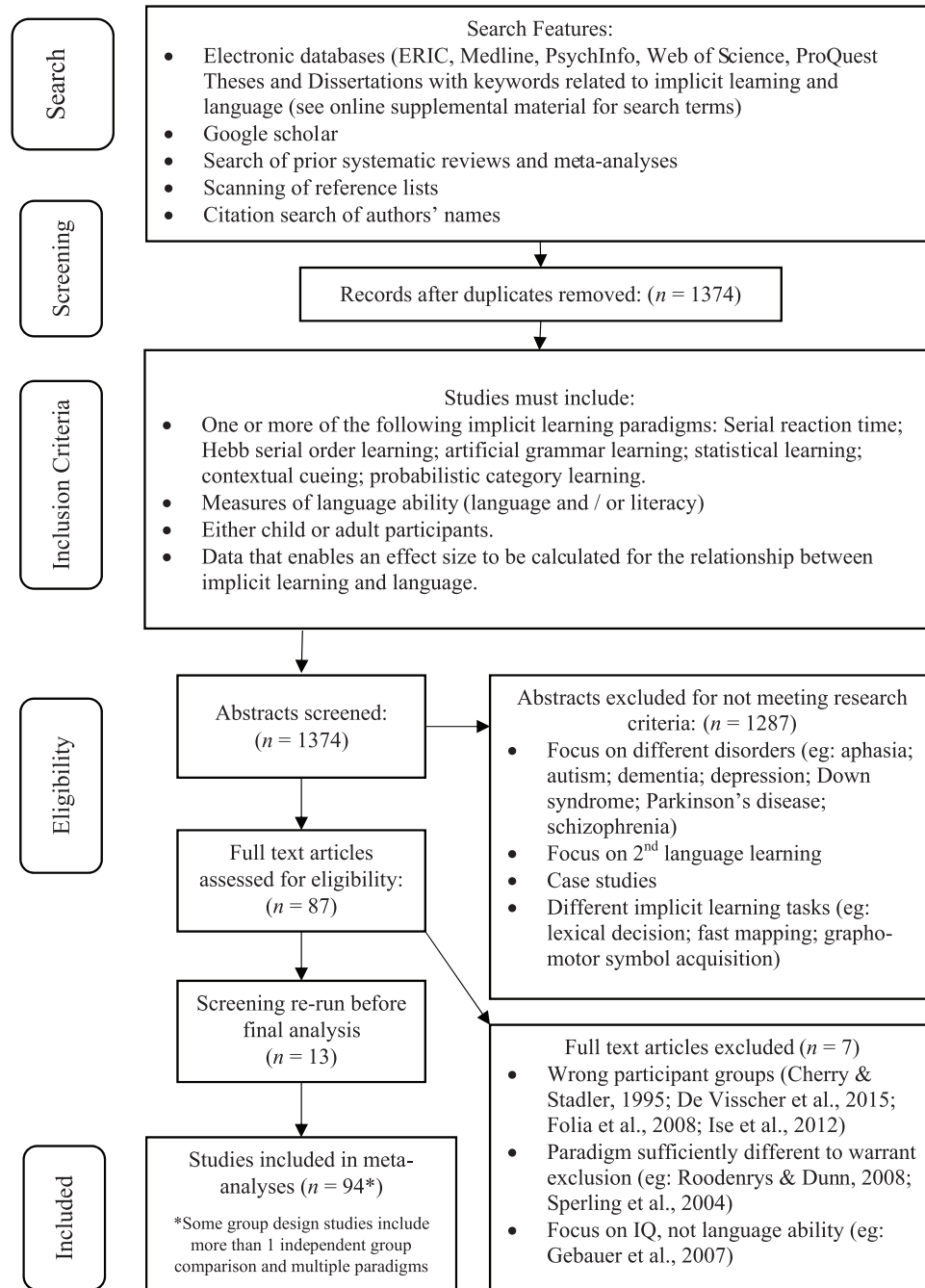
Coding and Meta-Analytic Procedure

Six separate meta-analyses were conducted, one for each of the main outcomes (see Table 2). Data were analyzed using the Comprehensive Meta-Analysis program (CMA: Borenstein et al., 2005).

Effect Size Calculation for Meta-Analyses of Group Design Studies

The standardized mean difference in procedural learning between groups was coded for group designs, using Hedges' *g* to correct for small sample sizes (Hedges & Olkin, 1985). A negative

Table 2
Series of Meta-Analyses With Numbers of Eligible Studies and Final Inclusion Numbers

Meta-analysis	Experimental design	Eligible studies[a]
Serial reaction time task	Group	52 (29)
Serial reaction time task	Correlational	6 (5)
Hebb serial order learning task	Group	9 (8)
Artificial grammar learning and statistical learning tasks	Group	31 (23)
Artificial grammar learning and statistical learning tasks	Correlational	5 (5)
Probabilistic category learning tasks	Group	6 (5)
Contextual cuing task	Group	4 (Insufficient[b])

[a] Number of studies considered eligible. The final number included in the meta-analysis is noted in brackets. [b] Four studies were eligible, but there were insufficient data for meta-analysis.

g indicates that the language disordered group is performing more poorly than the control group. Hedges' *g* is interpreted in terms of standard deviation units, i.e. $g = -1$ indicates that the group with a learning disorder are one standard deviation below the scores of the control group.

Results from group design studies for serial reaction time and Hebb learning tasks represent a group difference in the difference between conditions. For the meta-analyses of studies using these tasks, effect sizes for the group difference in procedural learning on the tasks were, therefore, calculated from the means and standard deviations of the component conditions on the tasks (serial reaction time task: means and standard deviations in milliseconds for sequenced trials and for random trials; Hebb task: means and standard deviations as a percentage score for repeated and for nonrepeated trials). This method of calculating an effect size is directly analogous to the way effect sizes are calculated for randomized control trials, otherwise known as pretest posttest control group designs (Lund, 1988; Morris, 2008; Ray & Shadish, 1996). This is the experimental design that these extreme groups studies most closely resemble structurally (see Figure 2).

Effect Size Calculation for Meta-Analyses of Correlational Studies

The correlations between procedural learning and language and/ or decoding measures (Pearson's *r*) were coded directly into CMA where the calculations are done with Fishers *z* and then transferred back to Pearson's *r* to ease interpretation.

Mean Effect Size and Heterogeneity

For all meta-analyses, random effects models in CMA were used to calculate weighted averages of individual effect sizes, in order to estimate an overall effect size for each meta-analysis; 95% confidence intervals (CIs) are given for each pooled effect size. In studies with more than one outcome of the same construct, the composite score of the mean of the measures was calculated. The impact of any potential outliers was examined using sensitivity analyses, which give an adjusted overall effect size after removing studies one at a time. The variation in effects sizes between studies was examined, using the *Q*-test of homogeneity (Hedges & Olkin, 1985) and I^2 was used to examine the degree of any true heterogeneity that was not attributable to sampling error (Borenstein et al., 2009).

Moderator Analyses

The potential role of categorical moderators was investigated using the grouping function in CMA, while metaregression in CMA was used to examine potential continuous moderators. The following moderators were coded:

**Age.** Studies were categorized as including adult or child participants.

Type of Learning Disorder. The language disorder (dyslexia or developmental language disorder) was coded for all group design studies.

Severity of the Disorder. Hedges' *g* was calculated for the differences in language or reading scores between the learning disorder group and controls. Thus, the larger the group difference, the more severe the disorder.

Nonverbal IQ. Measures of nonverbal IQ for experimental and control groups were coded. These were predominantly matrix reasoning tests, e. g. Raven's standard progressive matrices (Raven, 1986) or nonverbal IQ composites from standardized batteries (e.g., WISC–III: Wechsler, 1991).

Figure 2
Analogous Design Structure for Randomized, Controlled Trials and Implicit Learning Tasks Based on Difference Scores
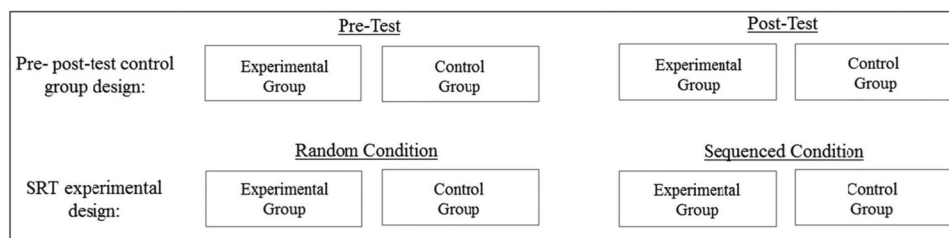
Note. SRT = serial reaction time.

Task Specific Moderators

For the serial reaction time task, the following moderators were coded: task type (deterministic, alternating or probabilistic), sequence type (first or second order conditional structure), sequence length, as well as the number of sequence repetitions prior to the introduction of the random sequence (deterministic tasks) or repetitions of the sequence across the task (alternating and probabilistic versions). For the Hebb learning tasks, the following moderators were coded: modality (verbal or nonverbal) and number of repetitions of the Hebb sequence. In addition, verbal tasks were further subdivided into auditory-verbal or visual-verbal tasks, although insufficient numbers of studies meant this could not be examined as a moderator. For artificial grammar and statistical learning tasks, modality (verbal or nonverbal, including a further distinction between visual-verbal and auditory-verbal tasks) and complexity (finite artificial grammar or simple triplet structure) were coded. For probabilistic classification tasks, modality (verbal or nonverbal), number of trials and variations in cue probabilities were coded.

Publication Bias

Publication bias was assessed using funnel plots in meta-analyses with significant overall effect sizes, provided they contained sufficient numbers of studies. Lau et al. (2006) do not recommend the use of funnel plots in meta-analyses with less than ten comparisons. However, funnel plots for random effects models can be difficult to interpret visually (Lau et al., 2006). For this reason, a trim-and-fill analysis (Duval & Tweedie, 2000) was used to impute the missing values needed to make the funnel plot symmetrical and to calculate an adjusted overall effect size based on inclusion of these imputed studies. The strength of funnel plots is that they use only the data in the meta-analysis to investigate bias and are, therefore, entirely representative of the result of the meta-analysis. However, we also estimated p curves for each meta-analysis, including all eligible studies to investigate the potential for bias in the published literature as a whole. These are included as Supplemental Material 3, accompanied by disclosure tables (Tables S2–S5 in the online supplemental materials) that detail the results for the principal measures of implicit learning in every eligible study.

Inclusion and Coding Reliability

To establish reliability of the inclusion of studies, 25% of the abstracts were coded by two independent raters. The results showed a high interrater reliability, and the agreement rate was 92%. For coding accuracy assurance, all the studies included in the meta-analyses were double coded by two independent raters. Overall, across all outcomes and moderators the agreement rate was 78% while the Pearson correlation was .86. For details about this for the different measures, see Supplemental Material 4. Any disagreements were resolved by reviewing the original article and discussion.

Results

Characteristics of the studies in each meta-analysis are in Supplemental Material 5 (Tables S6–S12). The tables include all eligible studies, indicating those for which sufficient data for an effect size was forthcoming.

Serial Reaction Time Task

Comparisons of Language-Disordered Groups and Age-Matched Controls

Fifty-two eligible studies were found for the meta-analysis of group design studies including deterministic, alternating, and probabilistic serial reaction time tasks (see Table S6 in the online supplemental materials), but only 20 of these reported or were able to supply data as means and standard deviations by group for each of the sequence types separately. Nine further studies contained fully labeled figures of sufficient quality to enable data extraction using WebPlotDgitizer (Rohatgi, 2017). This meant that the final meta-analysis contained 29 independent comparisons of procedural learning with language-disordered groups and age-matched controls on serial reaction time tasks (see Figure 3).

In total these studies included 610 participants with language disorder (mean sample size = 21.03, SD = 9.69, range 7 to 46) and 698 control participants (mean sample size = 24.07, SD = 15.11, range 10 to 87). The overall mean effect size was significant, g = −.30, 95% CI [−.43, −.16]. The variation in effect sizes was not significant, although approximately 39.93% of the variance was due to true variation between the studies rather than sampling error, $Q(28)$ = 39.93, p = .07, I^2 = 29.87, τ^2 = .04. One effect size in the analysis was much larger than the others, so a sensitivity analysis was conducted, which showed that the overall effect size was in the range of (g = −.26, 95% CI [−.38, −.14] to g = −.32, 95% CI [−.45, −.18]).

The effect size was slightly lower when excluding the seven studies for which data had been digitally extracted from figures (k = 20, g = −.26, 95% CI [−.39, −.13]).
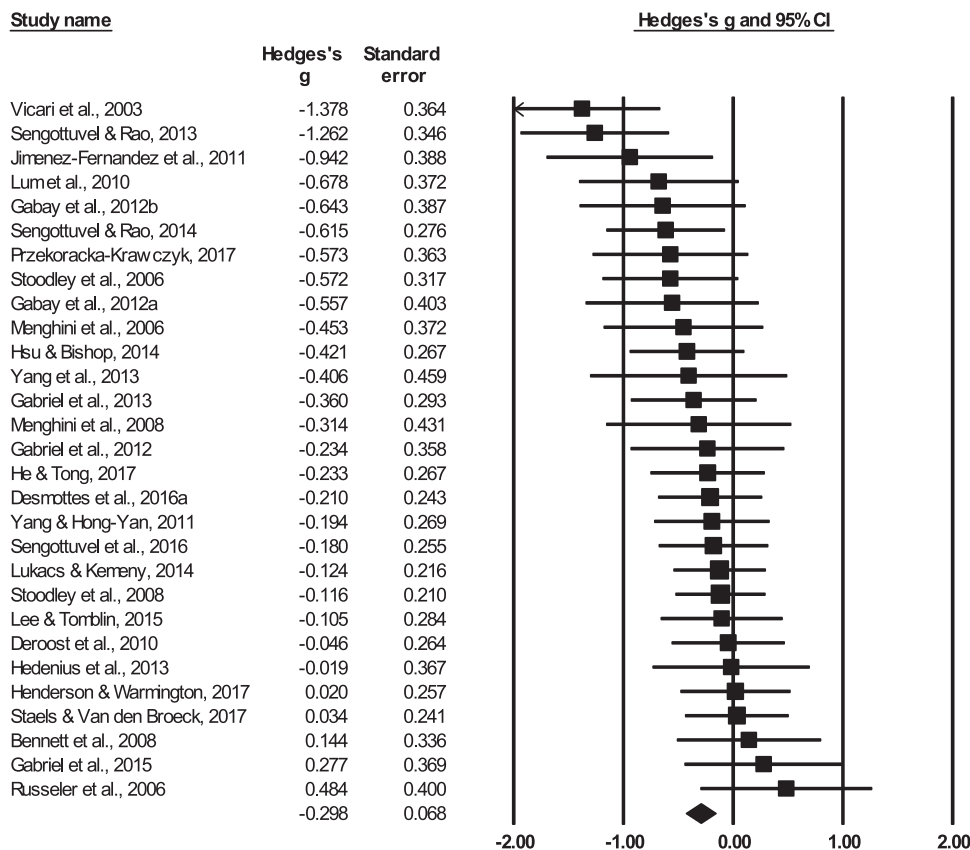
Moderator Analyses

Analysis of categorical moderator variables showed that the difference between samples with dyslexia and samples with developmental language disorder was not significant, $Q(1)$ = .17, p = .68, g (dyslexia) = −.28, k = 18, 95% CI [−.46, −.09], p < .01, g (language disorder) = −.33, k = 11, 95% CI [−.54, −.13], p < .01. Studies testing children showed a larger effect size than studies with adults, although this moderator was not significant either, $Q(1)$ = .48, p = .49, g (Adults) = −.23, k = 10, 95% CI [−.45, .01], p = .04, g (Children) = −.34, k = 19, 95% CI [−.50, −.16], p < .01.

We also considered whether the type of serial reaction time task might have a moderating effect on the overall effect size (deterministic, adapted deterministic or alternating). The difference in effect size between the studies using different types of task was significant, $Q(2)$ = 6.35, p = .04. The effect size for tasks with a standard deterministic structure was small to moderate and significant, g = −.28, 95% CI [−.42, −.14], k = 22, p < .01. Two studies used an adapted deterministic structure (Sengottuvel & Rao, 2013, 2014), taking the measure for random trials toward the beginning of the task (rather than at the end) when task acclimatization and effects of motor learning may have differentially contributed to speed of response across groups. These studies had by far the largest effect size, g = −.91, 95% CI [−1.54, −.28], p = .01. The four

Figure 3

Forest Plot Showing Effect Sizes for the Group Difference in Performance on the Serial Reaction Time Task

Study name	Hedges's g	Standard error	Hedges's g and 95% CI
Vicari et al., 2003	-1.378	0.364	
Sengottuvel & Rao, 2013	-1.262	0.346	
Jimenez-Fernandez et al., 2011	-0.942	0.388	
Lum et al., 2010	-0.678	0.372	
Gabay et al., 2012b	-0.643	0.387	
Sengottuvel & Rao, 2014	-0.615	0.276	
Przekoracka-Krawczyk, 2017	-0.573	0.363	
Stoodley et al., 2006	-0.572	0.317	
Gabay et al., 2012a	-0.557	0.403	
Menghini et al., 2006	-0.453	0.372	
Hsu & Bishop, 2014	-0.421	0.267	
Yang et al., 2013	-0.406	0.459	
Gabriel et al., 2013	-0.360	0.293	
Menghini et al., 2008	-0.314	0.431	
Gabriel et al., 2012	-0.234	0.358	
He & Tong, 2017	-0.233	0.267	
Desmottes et al., 2016a	-0.210	0.243	
Yang & Hong-Yan, 2011	-0.194	0.269	
Sengottuvel et al., 2016	-0.180	0.255	
Lukacs & Kemeny, 2014	-0.124	0.216	
Stoodley et al., 2008	-0.116	0.210	
Lee & Tomblin, 2015	-0.105	0.284	
Deroost et al., 2010	-0.046	0.264	
Hedenius et al., 2013	-0.019	0.367	
Henderson & Warmington, 2017	0.020	0.257	
Staels & Van den Broeck, 2017	0.034	0.241	
Bennett et al., 2008	0.144	0.336	
Gabriel et al., 2015	0.277	0.369	
Russeler et al., 2006	0.484	0.400	
	-0.298	0.068	

Note. Means and standard deviations are used for both sequenced and random trials per group in all 29 included studies. Although Lukács and Kemény (2012) reported *z*-transformed scores in their paper, means and *SD*s in milliseconds supplied by the authors were used in the meta-analysis.

comparisons using tasks with an alternating structure had a very small and nonsignificant mean effect size (but note that the power here is low), $g = -.04$, 95% CI [$-.33$, .24]. The potential moderating effect on the overall effect size of sequence length and number of repetitions of the sequence was considered for the deterministic tasks. Metaregressions showed that the effect of sequence length was not significant, $\beta = .00$, $p = .54$, $R^2 = .00$, $k = 22$, neither was the number of sequence repetitions $\beta = .00$, $p = .42$, $R^2 = .00$, $k = 22$.

We also examined whether the severity of language and decoding problems in the language-disordered samples related to group differences in procedural learning on the serial reaction time task. For language skills, there was a large amount of variation in the degree of difference between the language-disordered groups and the comparison groups. The mean difference was $g = -1.81$, 95% CI [-2.16, -1.45], ranging from $g = -.27$ to $g = -4.29$ and there was significant heterogeneity between the studies $Q(23) = 139.56$, $p < .001$, $I^2 = 83.52\%$, $k = 24$, $\tau^2 = .63$. However, a metaregression showed that the degree of severity of disorder in the disordered group did not explain any significant variation in the relationship between language ability and implicit learning on the task, $\beta = .43$, $p = .52$, $R^2 = .00$, $k = 12$.

For decoding, the variation between the degree of difference between the disordered and comparison group was even more

marked. The mean difference was $d = -2.20$ 95% CI [-2.59, -1.81], ranging from $d = -.63$ to $d = -7.48$ and there was significant true heterogeneity between the studies, $Q(20) = 122.91$, $p < .01$, $I^2 = 82.92\%$, $k = 22$, $\tau^2 = .67$. In this case, the metaregressions did show that the degree of severity of disorder in the disordered group did explain a very small amount of variation in the relationship between decoding ability and implicit learning on the task, $\beta = 1.45$, $p = .02$, $R^2 = .05$. However, one study (Jiménez-Fernández et al., 2011) contained an extremely large effect size for the difference between groups on measures of word and nonword reading accuracy ($g = -7.48$). Excluding this study changed the results of the metaregressions, $\beta = .41$, $p = .40$, $R^2 = .00$, such that severity of disorder, once again, no longer explained any of the variation in the relationship between decoding ability and serial reaction time performance.

Finally, there was variation between the language disordered and control groups on the measures of nonverbal IQ (NVIQ) used in the studies, mean difference was $g = -.35$, 95% CI [$-.49$, $-.20$], ranging from $g = .51$ to $g = -1.84$. There was significant true variation in effect sizes, $Q(31) = 61.78$, $p < .01$, $I^2 = 49.83\%$, $k = 32$, $\tau^2 = .09$. However, once again the metaregressions showed that the degree of disparity in NVIQ between groups did not

explain significant variation in the relationship between NVIQ and implicit learning on the task, $\beta = .01$, $p = .96$, $R^2 = .0$.

Publication Bias

A funnel plot with a trim and fill analysis (Duval & Tweedie, 2000) showed no evidence of publication bias across the 29 studies entered into the meta-analysis (see Figure 4).

Correlational Studies

Six studies examined the relationship between serial reaction time performance and language ability using correlational designs (see Table S7 in the online supplemental materials). Five of these studies, including 441 participants (mean sample size = 88.2, SD = 25.97, range = 58 to 120), included sufficient information to be entered into a meta-analysis that calculated the effect size (r) for the relationship between implicit learning on the serial reaction time task and measures of language and decoding. The pooled effect size was negligible and nonsignificant ($r = .03$, 95% CI [$-.06$, .13]), with nonsignificant variability between samples, $Q(4) = 3.12$, $p = .51$, $I^2 = .00\%$, $\tau^2 = .00$).

Three out of five of the studies also contained sufficient information to calculate an effect size for the relationship between NVIQ and serial reaction time task implicit learning performance. The overall effect size was not significant ($r = .10$, 95% CI [.01, .21], $p = .08$), although with only 3 studies included in this analysis, power was low.

Summary

For the serial reaction time task we found evidence of a moderate group deficit in participants with language disorders ($g = -.30$). However, concurrent correlational studies showed essentially no relationship between language abilities and performance on this task ($r = .03$). The discrepancy between these different forms of evidence is considered in the discussion.

Hebb Serial Order Learning

Comparisons of Language-Disordered Groups and Age-Matched Controls

The meta-analysis included eight studies of Hebb learning tasks with language-disordered groups and age-matched controls, which contained 10 independent comparisons in total (see Table S8 in the online supplemental materials). All 10 comparisons calculated effect sizes using the standard deviations of the nonrepeating filler and Hebb sequences themselves, not the standard deviation for the difference between them. The studies included 200 participants with a diagnosis of language disorder (mean sample size = 22.22, SD = 5.74, range = 12 to 29) and 201 control participants (mean sample size = 22.33, SD = 6.58, range = 12 to 32).

Effect sizes with confidence intervals for the different studies are shown in Figure 5. The pooled effect size was significant, $g = -.32$, 95% CI [$-.52$, $-.12$], with language disordered groups in these studies showing less facilitation on repeated lists compared with age-matched controls without language difficulties. The variation in effect sizes between studies was not significant, $Q(9) = 10.36$, $p = .32$, $I^2 = 13.10\%$, $\tau^2 = .01$. Once again, we also calculated the effect size for these studies using the standard deviations

for the nonrepeated condition for both groups, which increased the effect size slightly, $g = -.35$, 95% CI [$-.56$, $-.13$], $p < .01$.

Overall, for the moderator analyses the true heterogeneity was 13% and not significant, indicating that there is little true variation between studies that might be explained by moderators. The analyses showed that the difference between samples with dyslexia and samples with developmental language disorder was not significant, $Q(1) = .03$, $p = .86$, g (dyslexia) = $-.33$, $k = 7$, 95% CI [$-.58$, $-.09$], g (language disorder) = $-.29$, $k = 3$, 95% CI [$-.69$, .11], however power was extremely low in this analysis, as there were so few studies of samples with developmental language disorder. There was also no significant difference between samples in children or adults $Q(1) = .15$, $p = .70$, although the effect size in samples in adults was higher than in children g (Adults) = $-.36$, $k = 5$, 95% CI [$-.65$, $-.07$], g (Children) = $-.28$, $k = 5$, 95% CI [$-.58$, .02].

The majority of Hebb tasks were verbal tasks (either visual or auditory). Because several comparisons included both verbal and nonverbal tasks, the question of whether modality influenced effect size was examined by calculating the effect size for verbal and nonverbal tasks separately. Given the language-related difficulties of the experimental groups, it was expected that the difference between groups for verbal tasks would be greater than for the overall analysis, but it was actually smaller $g = -.23$, $k = 9$, 95% CI [$-.43$, $-.04$]. There were only four comparisons for the nonverbal tasks, so power in this analysis was low, but the effect size was moderate, $g = -.66$, $k = 4$, 95% CI [-1.09, $-.22$].

The extent to which group differences in language skills and decoding respectively, related to the group difference in Hebb serial order learning was also analyzed. The mean difference in language skills between the groups was $g = -1.57$, 95% CI [-2.55, $-.60$], ranging from $g = -.53$ to $g = -2.99$. There was significant heterogeneity between the studies, $Q(3) = 25.94$, $p < .01$, $I^2 = 88.44\%$, $k = 4$, $\tau^2 = .87$; however, a metaregression showed that the severity of language disorder in the disordered group did not explain any significant variation in the relationship between language ability and implicit serial learning, $\beta = -.24$, $p = .88$, $R^2 = .00$.

For decoding the mean difference was $g = -1.90$, 95% CI [-2.21, -1.59], ranging from $g = -1.39$ to $g = -2.53$, but the variation between the degree of difference between the disordered and comparison group in the studies was nonsignificant in spite of a high value for I^2 ($Q(6) = 8.58$, $p = .20$, $I^2 = 30.04\%$, $k = 14$, $\tau^2 = .05$). A metaregression showed that the degree of severity of disorder in the disordered group did not explain any significant variation in the relationship between decoding ability and implicit learning on the task, $\beta = .70$, $p = .37$, $R^2 = .00$.

Finally, there was variation between the disordered and comparison groups for the measures of NVIQ used in the studies, mean difference was $g = -.29$, 95% CI [$-.57$, $-.01$], ranging from $g = .21$ to $g = -1.39$. Even with so little overall true variance to explain, this variation in effect sizes between studies was significant, $Q(9) = 20.74$, $p = .01$, $I^2 = 56.59\%$, $k = 10$, $\tau^2 = .11$. However, a metaregression showed that the degree of disparity in NVIQ between groups did not explain any of the variation in the relationship between language-related ability and implicit learning on the task, $\beta = -.26$, $p = .59$, $R^2 = .00$.

Figure 4

Funnel Plots Examining Evidence of Publishing Bias for the Meta-Analyses of Group Design Studies (Top to Bottom: SRT; Hebb Learning and AGL and Statistical Learning Tasks)
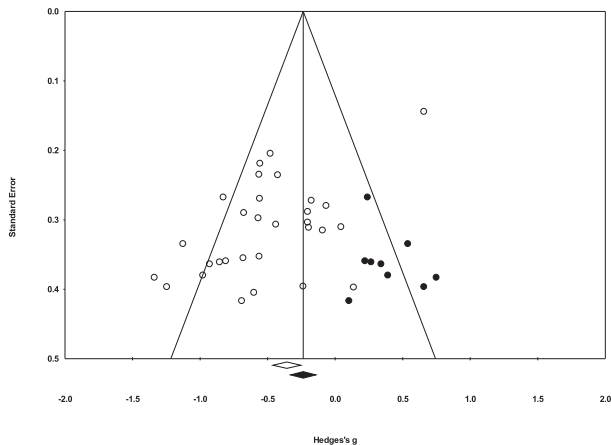
Funnel plot for group design studies using SRT tasks
(point estimate of d = -0.30, 95% CI [-0.43, -0.16]):
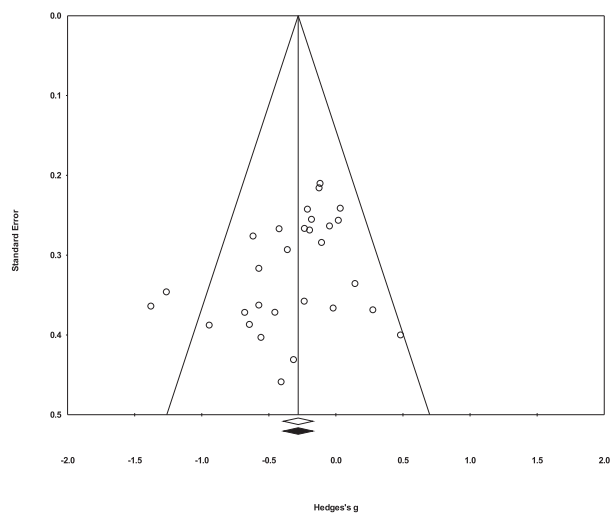
Funnel plot for group design studies using Hebb learning tasks
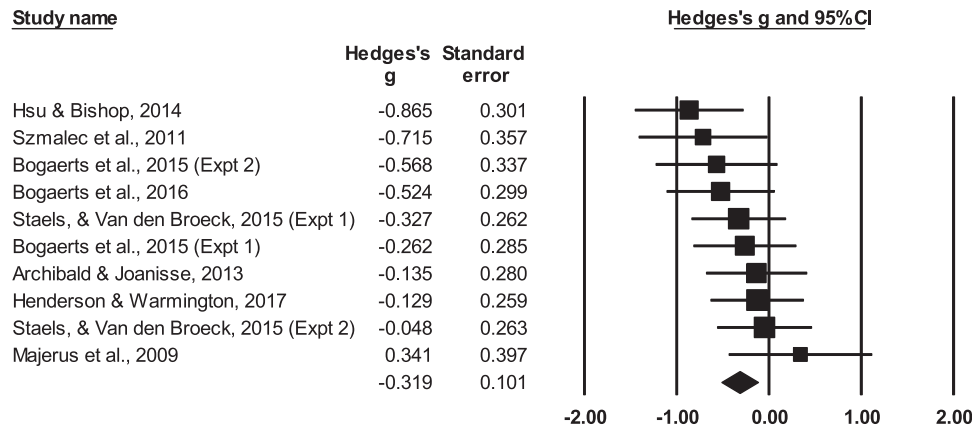(point estimate of d = -0.32, 95% CI [-0.52, -0.12]):
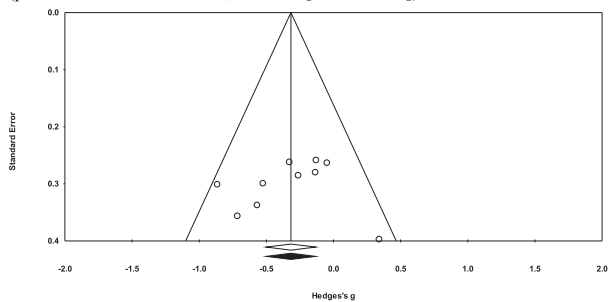
Funnel plot for group design studies using artificial grammar or statistical learning tasks
(adjusted point estimate of d = -0.30, 95% CI [-0.47, 0.12]):

Note. Open circles and diamond correspond to observed studies and point estimate. Filled circles and diamond correspond to imputed missing studies and adjusted point estimate, following Duval and Tweedie's Trim and Fill procedure. SRT = serial reaction time task; AGL = artificial grammar learning.

Figure 5
Forest Plot Showing Effect Sizes for the Group Difference in Performance on Hebb Tasks

Study name	Hedges's g	Standard error
Hsu & Bishop, 2014	-0.865	0.301
Szmalec et al., 2011	-0.715	0.357
Bogaerts et al., 2015 (Expt 2)	-0.568	0.337
Bogaerts et al., 2016	-0.524	0.299
Staels, & Van den Broeck, 2015 (Expt 1)	-0.327	0.262
Bogaerts et al., 2015 (Expt 1)	-0.262	0.285
Archibald & Joanisse, 2013	-0.135	0.280
Henderson & Warmington, 2017	-0.129	0.259
Staels, & Van den Broeck, 2015 (Expt 2)	-0.048	0.263
Majerus et al., 2009	0.341	0.397
	-0.319	0.101

Note. 95% confidence interval for each study; pooled effect size displayed by ◆.

Publication Bias

A funnel plot with a trim and fill analysis (Duval & Tweedie, 2000) showed no evidence of publication bias across the 8 studies entered into the meta-analysis (see Figure 4).

Summary

For the Hebb serial learning task we found evidence of a moderate deficit in groups with language disorders ($g = -.32$).

Artificial Grammar and Statistical Learning Tasks

Comparisons of Language-Disordered Groups and Age-Matched Controls

Twenty-three of 31 eligible studies were entered into the meta-analysis (see Table S9 in the online supplemental materials), which included 30 independent comparisons of artificial grammar learning and statistical learning tasks with language-disordered groups and age-matched controls. The studies included 660 participants with language disorder (mean sample size 22.00, $SD = 12.40$, range = 12 to 77) and 907 control participants (mean sample size 30.23, $SD = 28.71$ range = 12 to 146).

Effect sizes with confidence intervals for the different studies are shown in Figure 6. The pooled effect size was moderate and significant, $g = -.48$, 95% CI [−.67, −.29], confirming that overall language disordered groups performed more poorly on artificial grammar learning and statistical learning tasks than age-matched controls without difficulties. The variation in effect sizes between studies was large and significant, $Q(29) = 91.10$, $p < .001$, $I^2 = 68.17\%$, $k = 30$, $\tau^2 = .18$. A sensitivity analysis showed that the overall effect size was in the range of $g = -.45$, 95% CI [−.64, −.27] to $g = -.52$, 95% CI [−.64, −.40].

Moderator Analyses

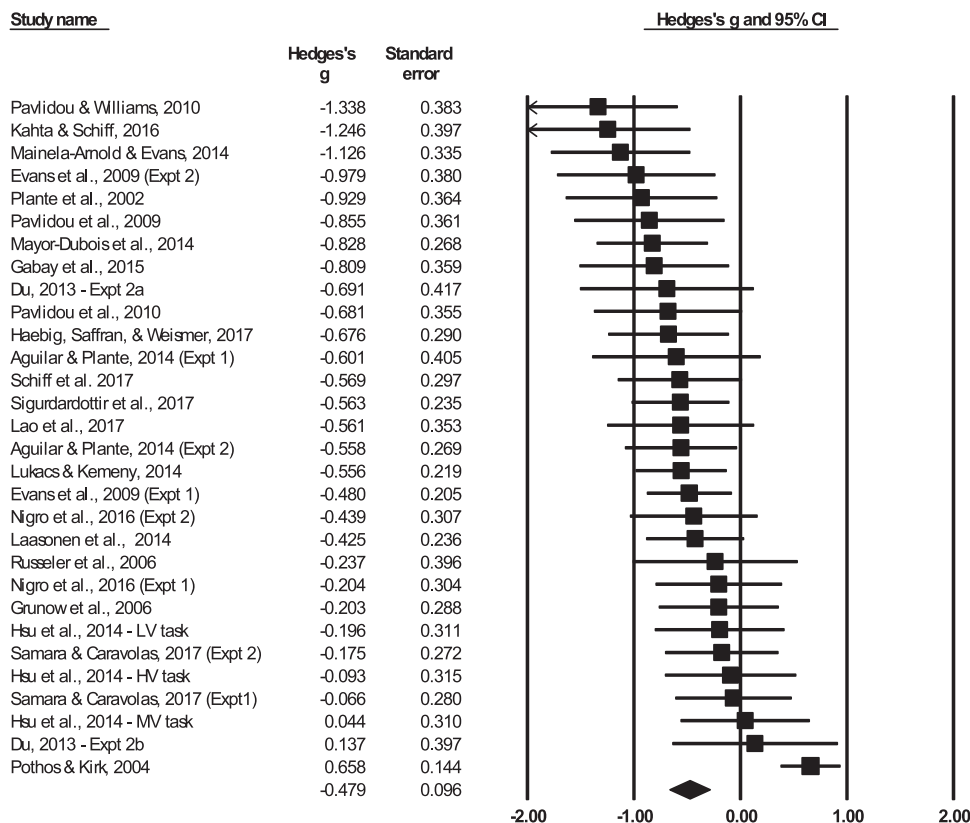The analysis of categorical moderator variables showed that the difference between samples with dyslexia and samples with developmental language disorder was not significant, $Q(1) = .29$, $p = .59$, g (dyslexia) = −.44, $k = 16$, 95% CI [−.75, −.13], g (language disorder) = −.53, $k = 14$, 95% CI [−.70, −.37]. Although studies with adults showed a smaller effect size than studies with children, this moderator was not significant either, $Q(1) = .99$, $p = .32$, g (Adults) = −.39, $k = 15$, 95% CI [−.69, −.08], g (Children) = −.57, $k = 15$, 95% CI [−.75, −.39]. The difference between studies using artificial grammar learning or statistical learning tasks was also not significant, $Q(1) = .01$, $p = .93$, g (AGL tasks) = −.48, $k = 16$, 95% CI [−.79, −.16], g (SL tasks) = −.49, $k = 14$, 95% CI [−.66, −.32]. Finally, the difference between studies using verbal or nonverbal stimuli was examined. This required the exclusion of two comparisons that had administered tasks of more than one modality to the same participants (Evans et al., 2009: Experiment 2; Gabay et al., 2015). Once again, the difference was not significant $Q(1) = .24$, $p = .62$, although the mean effect size for verbal tasks was significant and much larger than for nonverbal tasks, g (verbal) = −.51, $k = 18$, 95% CI [−.66, −.35], $p < .001$, g (nonverbal) = −.39, $k = 10$, 95% CI [−.83, .04], $p = .08$.

The extent to which severity of difficulties in language and decoding respectively, in language disordered groups, related to the group difference in artificial grammar or statistical learning task performance was also analyzed. For language skills, there was variation between the degree of difference between the language-disordered groups and the comparison group. The mean difference was $g = -1.70$, ranging from $g = -2.12$ to $g = -1.27$ and there was significant heterogeneity between the studies $Q(15) = 76.16$, $p < .001$, $I^2 = 80.31\%$, $k = 16$, $\tau^2 = .59$. However, a metaregressions showed that the degree of severity of disorder in the disordered group did not explain any significant variation in the relationship between language ability and implicit learning on the task, $\beta = -.20$, $p = .69$, $R^2 = .00$.

For decoding there was a large variation between the degree of difference between the disordered and comparison group (these were predominantly studies investigating dyslexia), mean difference was $g = -1.94$, ranging from $g = -2.37$ to $g = -1.51$. For decoding ability there was significant heterogeneity between the

Figure 6
Forest Plot Showing Effect Sizes for Group Difference in Performance on Artificial Grammar Learning and Statistical Learning Tasks

Study name	Hedges's g	Standard error	Hedges's g and 95% CI
Pavlidou & Williams, 2010	-1.338	0.383	
Kahta & Schiff, 2016	-1.246	0.397	
Mainela-Arnold & Evans, 2014	-1.126	0.335	
Evans et al., 2009 (Expt 2)	-0.979	0.380	
Plante et al., 2002	-0.929	0.364	
Pavlidou et al., 2009	-0.855	0.361	
Mayor-Dubois et al., 2014	-0.828	0.268	
Gabay et al., 2015	-0.809	0.359	
Du, 2013 - Expt 2a	-0.691	0.417	
Pavlidou et al., 2010	-0.681	0.355	
Haebig, Saffran, & Weismer, 2017	-0.676	0.290	
Aguilar & Plante, 2014 (Expt 1)	-0.601	0.405	
Schiff et al. 2017	-0.569	0.297	
Sigurdardottir et al., 2017	-0.563	0.235	
Lao et al., 2017	-0.561	0.353	
Aguilar & Plante, 2014 (Expt 2)	-0.558	0.269	
Lukacs & Kemeny, 2014	-0.556	0.219	
Evans et al., 2009 (Expt 1)	-0.480	0.205	
Nigro et al., 2016 (Expt 2)	-0.439	0.307	
Laasonen et al., 2014	-0.425	0.236	
Russeler et al., 2006	-0.237	0.396	
Nigro et al., 2016 (Expt 1)	-0.204	0.304	
Grunow et al., 2006	-0.203	0.288	
Hsu et al., 2014 - LV task	-0.196	0.311	
Samara & Caravolas, 2017 (Expt 2)	-0.175	0.272	
Hsu et al., 2014 - HV task	-0.093	0.315	
Samara & Caravolas, 2017 (Expt1)	-0.066	0.280	
Hsu et al., 2014 - MV task	0.044	0.310	
Du, 2013 - Expt 2b	0.137	0.397	
Pothos & Kirk, 2004	0.658	0.144	
	-0.479	0.096	

Note. 95% confidence interval for each study; pooled effect size displayed by ◆.

studies, $Q(19) = 118.01$, $p < .01$, $I^2 = 83.90\%$, $k = 20$, $\tau^2 = .76$. Once again, a metaregression showed that the degree of severity of disorder in the disordered group did not explain significant variation in the relationship between decoding ability and implicit learning on the task, $\beta = -.48$, $p = .45$, $R^2 = .02$. One comparison (Nigro et al., 2016: Experiment 2) contained extremely large effect sizes for the difference between groups on measures of word and nonword reading accuracy ($g = -7.9$). However, excluding this study did not significantly change the results of the metaregressions., $\beta = -.61$, $p = .24$, $R^2 = .08$.

Finally, there was variation between the disordered and control groups for the measures of NVIQ used in the studies, mean difference was $g = -.31$, ranging from $g = -.48$ to $g = -.13$. This variation in effect sizes between studies was significant, $Q(25) = 51.36$, $p < .01$, $I^2 = 51.32\%$, $k = 26$, $\tau^2 = .10$. On this occasion the metaregression showed that the degree of disparity in NVIQ between groups explained 41% of the variation in the relationship between language-related ability and implicit learning on the task, $\beta = .61$, $p = .01$, $R^2 = .41$.

Publication Bias

A funnel plot with a trim and fill analysis (Duval & Tweedie, 2000) indicated the presence of publication bias in the effect size
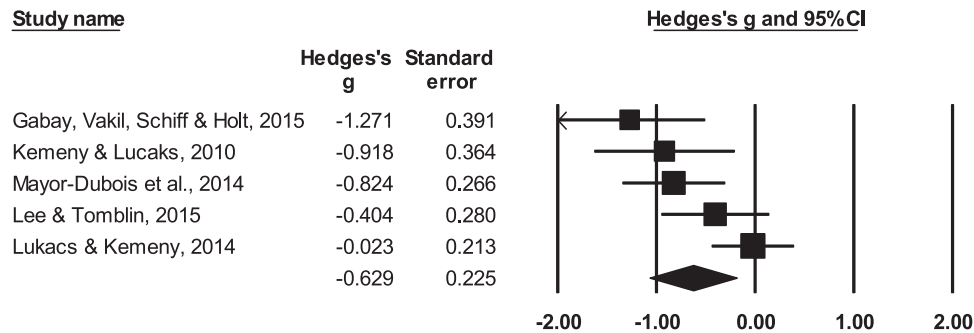
for artificial grammar and statistical learning group design studies (see Figure 4), suggesting that the true effect size in the meta-analysis should be much lower, adjusted point estimate $g = -.30$, 95% CI [$-.47$, $-.12$].

Correlational Studies

Five studies examined the relationship between performance on artificial grammar ($k = 1$) and statistical learning ($k = 4$) tasks and language ability using correlational designs, with a total of six independent samples (see Table S10 in the online supplemental materials).

The five studies, including 289 participants (mean sample size = 48.17, $SD = 17.44$, range = 30 to 72), were entered into a meta-analysis that calculated the effect size (r) for the relationship between implicit learning on the statistical learning task and measures of language and decoding. The pooled effect size in this meta-analysis was moderate and significant ($r = .30$, 95% CI [.19, .41]). The variability across samples was not significant ($Q(5) = 1.71$, $p = .88$, $I^2 = .00\%$, $\tau^2 = .00$). However, one of the studies (Qi et al., 2019), with a sample of 72 adults and children, included more in-depth reading measures for children only. When adding these word and nonword reading measures and, therefore, necessarily only including the subset of children (n = 36), the pooled

Figure 7

Forest Plot Showing Effect Sizes for Group Differences in Performance on Weather Prediction Tasks

Study name	Hedges's g	Standard error	Hedges's g and 95%CI
Gabay, Vakil, Schiff & Holt, 2015	-1.271	0.391	
Kemeny & Lucaks, 2010	-0.918	0.364	
Mayor-Dubois et al., 2014	-0.824	0.266	
Lee & Tomblin, 2015	-0.404	0.280	
Lukacs & Kemeny, 2014	-0.023	0.213	
	-0.629	0.225	

Note. 95% confidence interval for each study; pooled effect size displayed by ♦.

effect size was reduced ($r = .25$, 95% CI [.12, .36]), with nonsignificant variability across samples, $Q(5) = 2.82$, $p = .73$, $I^2 = .00\%$, $\tau^2 = .00$.

Summary

For the artificial grammar and statistical learning tasks we found evidence of a group deficit ($g = -.48$) that was also supported by a modest correlation between language abilities and these tasks in samples unselected for ability ($r = .30$).

Weather Prediction Task

Comparisons of Language-Disordered Groups and Age-Matched Controls

Six studies were entered into the meta-analysis (see Table S11 in the online supplemental materials), which included 101 participants with language disorder (mean sample size = 20.02, $SD = 5.81$, range = 15 to 29) and 208 control participants (mean sample size = 41.60, $SD = 32.58$, range = 15 to 87). Effect sizes with confidence intervals for the different studies are shown in Figure 7. The pooled effect size was significant, $g = -.63$, 95% CI [-1.07, -.19], indicating that overall language disordered groups perform poorly on weather prediction tasks compared with age-matched controls without language difficulties. The variation in effect sizes between studies was also significant, $Q(4) = 11.79$, $p < .02$, $I^2 = 66.09\%$, $k = 5$, $\tau^2 = 016$.

Moderator Analyses

Only one study (Gabay et al., 2015) tested dyslexic participants, so participant diagnosis was not examined as a moderator. The moderating effect of participant age was examined, even though power in this analysis was low (Adults $k = 2$, Children $k = 3$). The difference between studies with adults and with children was not significant, $Q(1) = .22$, $p = .64$, g (Adults) $= -.79$, 95% CI [-1.59, .01], g (Children) $= -.55$ 95% CI [-1.17, .07].

Only two studies reported data for language tests to accompany measures of effect size for the weather prediction task (with only one of these including decoding measures), so it was not possible to examine whether the severity of language disorder was related to performance on the weather prediction task. Three studies reported data for NVIQ measures, which showed that there was a

large variation between the disordered and control groups for the measures of NVIQ used in the studies, the mean difference was $g = -1.16$, 95% CI [-1.99, -.33], ranging from $g = -.36$, 95% CI [-1.06, .35] to $g = -1.27$, CI [-1.93, -.61]. This variation in effect sizes between studies was significant, $Q(2) = 9.07$, $p = .01$, $I^2 = 77.94\%$, $k = 3$, $\tau^2 = .42$. However, there were not enough studies in the analysis to be able to examine the effect of this difference in NVIQ group disparity on weather prediction task performance in a metaregression.

Publication Bias

There were too few studies to investigate the moderating influence of any task-related variables or to examine publication bias using a funnel plot.[1]

Summary

On the weather prediction task we found evidence for a large and significant ($g = -.63$) group deficit in participants with developmental language disorder and with dyslexia. However, it should be stressed that there is considerable doubt about the extent to which performance on this task depends on procedural rather than declarative memory processes (Fotiadis & Protopapas, 2014; Knowlton et al., 1994).

Contextual Cueing Task

Comparisons of Language-Disordered Groups and Age-Matched Controls

Four studies have investigated whether there is a difference in implicit learning performance on the contextual cuing task between groups with dyslexia and controls (see Table S12 in the online supplemental materials for study characteristics), two with children (Jiménez-Fernández et al., 2011; Staels & Van Den Broeck, 2017) and two with adults (Bennett et al., 2008; Howard et al., 2006). The studies reported insufficient data for the contextual cuing tasks to perform a meta-analysis, but all reported comparable levels of implicit learning for both groups on the task.

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[1] Funnel plots containing fewer than 10 studies are considered unreliable indicators of publication bias (Lau et al., 2006).

Indeed, Staels and Van den Broeck (2017) reported greater levels of facilitation for their group with dyslexia compared with typically developing children, but this was not significant. However, this result was found alongside better than chance performance on a posttest generation task for both groups, suggesting the facilitation effect over the course of the task was at least partially due to explicit learning.

Summary

There are only four studies that have examined differences between participants with language learning disorders and controls on the contextual cuing task. These studies did not provide sufficient information to allow a meta-analysis to be performed. However, there is no evidence for a deficit on the contextual cuing task in participants with language learning disorders.

Procedural Learning Deficits Across Multiple Tasks

Although impaired procedural learning in language disorders is sometimes found on both verbal and nonverbal tasks, a question still remains over the domain-generality of any implicit learning impairment. The procedural deficit hypothesis is embedded in the classic multiple systems model of memory which postulates the existence of two separable memory subsystems (procedural vs. declarative memory). This theory suggests that any procedural memory deficit should be domain-general, reflecting the role of a common procedural memory system that could be tapped by diverse memory tasks, involving both verbal and nonverbal stimuli. Impairments in procedural learning in language disorders would, therefore, be expected on both verbal and nonverbal procedural memory tasks.

Most studies examining the procedural deficit hypothesis have used a single implicit learning task and do not address the extent to which impairments in a language-disordered group generalize to

other, similar tasks. However, 14 group design studies (see Table 3) have used multiple tasks. Unfortunately, domain generality cannot be examined with meta-analytic techniques because most studies do not report correlations between tasks. Nevertheless, it is useful to examine the extent to which any implicit learning impairment extends across tasks. Table 3 suggests considerable task specificity, with impaired learning at a group level almost always being confined to a single task.

The idea that implicit learning is task specific is also supported by two correlational studies (Schmalz et al., 2019; West et al., 2018). Both studies found no correlation between implicit learning measures from a serial reaction time and an artificial grammar learning task and from the serial reaction time, Hebb learning, and contextual cuing tasks, respectively.

Discussion

The procedural deficit hypothesis of language learning disorders embodies two interrelated claims. First, that the human memory system is organized into separable procedural and declarative memory systems (Squire, 2004; Squire & Dede, 2015). Second, that a deficit in the procedural system is causally related to the language learning difficulties seen in children with developmental language disorder (Ullman, 2004; Ullman & Pierpont, 2005) and the difficulties in learning to decode print seen in dyslexia (Nicolson & Fawcett, 2007, 2011). Our review provides little support for this theory.

Major Findings

We reported separate meta-analyses of extreme group studies of four different tasks used to assess group deficits in procedural learning (the serial reaction time task, the Hebb task, artificial grammar or statistical learning tasks, and the weather prediction

Table 3

Extreme Groups Studies Testing Participants on More Than One Procedural Task, Highlighting the Tasks That Report Significant Implicit Learning Differences Between Groups

Study	Age	DLD/DD	Correlation between implicit tasks	SRT	AGL/SL	Hebb	WPT	CC	Other
Bennett et al., 2008	Adult	DD	Not reported	✓				x	
Du, 2013[a]		DD	Not reported	x	✓				
Henderson & Warmington, 2017	Adult	DD	Nonsignificant	x		✓			
Howard et al., 2006	Adult	DD	Not reported	✓				x	
Hsu & Bishop, 2014	Child	DLD	SRT and Hebb ($r = .23$, $p = .09$)	✓		✓			x[b]
Jiménez-Fernández et al., 2011	Child	DD	Different children	✓				x	
Laasonen et al., 2014	Adult	DD	Not reported	x	x				
Lee, 2012[a]	Adult	DLD	Not reported	x			✓		
Lee & Tomblin, 2015	Adult	DLD	Nonsignificant, except WPT and repetition priming ($r = .35$, $p = .01$)	x			✓		✓[b, c]
Lukács & Kemény, 2014	Child	DLD	Nonsignificant	✓	✓		x		
Mayor-Dubois et al., 2014	Child	DLD	Not reported	x			x		
Rüsseler et al., 2006	Adult	DD	Not reported	x	x				
Vakil et al., 2015	Child	DD	Not reported	x					x[d]
Vicari et al., 2005	Child	DD	Not reported	✓					✓[e]

Note. DLD = developmental language disorder; DD = developmental dyslexia; SRT = serial reaction time; AGL = artificial grammar learning; SL = statistical learning; WPT = weather prediction task; CC = contextual cueing; ✓ = significant implicit learning-related differences between groups; x = no significant difference between groups.
[a] unpublished thesis.   [b] pursuit rotor.   [c] repetition priming.   [d] Tower of Hanoi.   [e] mirror drawing.

[probabilistic category learning] task). Participants with language learning disorders tend to perform poorly on all four tasks compared with age-matched controls. We should stress, however, that even if such group deficits could be shown to be genuine and specific they may just as easily be a consequence of a learning disorder (language learning disorder → procedural learning deficit) as a cause (procedural learning deficit → language learning disorder).

Power in a meta-analysis will increase as the number of studies included increases and if individual studies have larger sample sizes. The meta-analyses of extreme group studies reported here differ markedly in the number of studies included and the total sample sizes. The meta-analyses of the serial reaction time task (29 comparisons; 1,308 participants) and artificial grammar and statistical learning tasks (30 comparisons; 1,567 participants), therefore, most likely give a more accurate indication of true effect sizes than those from the much smaller samples available for the Hebb (10 comparisons; 410 participants) or the weather prediction tasks (5 comparisons; 309 participants).

We found evidence of a group deficit in children with language disorders on the serial reaction time ($g = -.30$) and Hebb ($g = -.32$) tasks. However, the size of these group deficits is small if they reflect an important causal risk factor. We might contrast these findings with the case of the phoneme awareness deficit seen in dyslexia (which is postulated to be a major causal risk factor). The group difference in phoneme awareness between children with dyslexia and controls was reported to be $d = -1.37$ (Melby-Lervåg et al., 2012). Thus, if poor procedural learning is an important causal risk factor for dyslexia and DLD, we would expect larger group deficits. Moreover, studies of samples unselected for language ability showed essentially no correlation between language abilities and performance on the serial reaction time task ($r = .03$). As we discuss below, this casts serious doubt on the reliability of the findings from extreme group designs using this task.

There was a moderate deficit in extreme group designs using artificial grammar or statistical learning tasks ($g = -.48$). However, the accompanying funnel plot supports the suggestion from a recent meta-analysis of artificial grammar learning in dyslexia (Schmalz et al., 2017), that the true effect size in these studies is likely to be small. Nevertheless, our meta-analysis of correlational studies yielded a moderate effect size ($r = .30$), which does suggest a genuine relationship between artificial grammar or statistical learning and language and reading problems. However, this may simply reflect a deficit in verbal learning among children with language learning disorders, since the effect size in studies using verbal stimuli is far larger than in studies using nonverbal stimuli.

Our final meta-analysis assessed evidence for impaired probabilistic category learning in the weather prediction task. The overall effect size was large and significant ($g = -.63$). However, there is considerable doubt about the extent to which performance on this task reflects implicit rather than declarative memory processes (Fotiadis & Protopapas, 2014; Knowlton et al., 1994).

Methodological Issues-Limitations of Extreme Group Designs

The majority of studies reviewed here involve extreme group designs in which groups with language learning disorders are compared with control groups. A second, less common, approach is to examine the correlation between measures of procedural learning and reading or language skills in samples unselected for ability. These two approaches should yield converging evidence. Reading and language skills are continuously distributed traits (like height and weight). Causal influences on reading or language development should therefore correlate with reading or language ability in representative (unselected) samples, as well as showing mean differences between groups at the extremes of the distribution.

The results for the serial reaction time task from extreme group designs and samples unselected for ability yield contradictory results: a group deficit in participants with language disorders, but no equivalent correlation in representative samples. In our view, this inconsistency likely reflects methodological problems inherent in extreme group studies. Extreme group designs suffer from regression to the mean and typically overestimate the true linear relationship between variables (Preacher et al., 2005) and are frequently underpowered, leading to publication bias because only positive (not null) results get published (Button et al., 2013). In addition, both the serial reaction time and Hebb learning tasks, have poor reliability (Bogaerts et al., 2018; West et al., 2018), which may reflect the fact that difference scores (Lord, 1958; Overall & Woodward, 1975) and scores derived from short tasks tend to be unreliable (Brysbaert & Stevens, 2018; Nunnally & Bernstein, 1994). Unfortunately, none of the studies reviewed here reported the reliability of their tasks.

We also need to consider comorbidities among participants in extreme group designs. Reading and language disorder are both highly comorbid with a range of other disorders, including problems of attention control (ADHD; Hulme & Snowling, 2009). Deficits on procedural learning tasks in children with language learning disorders may therefore reflect problems of attending during the task, rather than being a correlate of reading difficulties per se. These issues are probably better addressed in studies of unselected groups where reliable measures of reading, language and attention can be related to measures of procedural learning (see West et al., 2021).

One possible explanation for a discrepancy in results from studies using extreme groups versus samples unselected for ability might appeal to the existence of nonlinear effects. Children or adults at the bottom the distribution of reading (dyslexia) or language (DLD) might have deficits on the serial reaction time task, whereas in the rest of the population there is no linear relationship between the two measures. Only one study to date has investigated this possibility and found no support for it (West et al., 2021). Furthermore, even if such nonlinear effects existed (with associations in the bottom end of the distribution of reading/language skills only), we would nevertheless expect to find correlations (albeit perhaps weak ones) depending upon details of the distributions involved.

Theoretical Issues-Task "Purity" and Psychometric Issues in Identifying a Procedural Memory System

The procedural deficit hypothesis is founded on the claim that there are separable procedural and declarative memory systems (also known as implicit and explicit memory). It follows from this theory that diverse measures of procedural memory should tap into a common underlying procedural memory system (just as diverse measures on an IQ test should load onto a common psychometric "g" factor). Although there are only 14 relevant studies

(see Table 3), we found no evidence that any procedural learning deficit is domain-general; where more than one measure of procedural learning is used, there are typically inconsistent effects on the different tasks between groups with and without language learning difficulties.

It is important to emphasize that procedural (implicit) learning tasks are not process pure (e.g., Cleeremans et al., 1998; Reber, 1989; Shanks & St. John, 1994). For example, performance on artificial grammar and serial reaction time tasks may reflect conscious learning of fragmentary chunks of information (Buchner et al., 1997; Perruchet & Pacteau, 1990). Although few studies of procedural learning in dyslexia, or developmental language disorder, include separate measures of declarative learning (such as serial or free recall; see Table S6 in the online supplemental materials), those that do typically find poorer declarative learning in language disordered groups (Conti-Ramsden et al., 2015; Deroost et al., 2010; Henderson & Warmington, 2017; Lum & Bleses, 2012; Lum et al., 2012; Lum et al., 2010; Stoodley et al., 2006). Furthermore, a previous meta-analysis (Melby-Lervåg et al., 2012) demonstrated that children with dyslexia do poorly on one measure of verbal declarative memory (verbal memory span ($d = .71$), as do children with developmental language disorder (e.g., Gathercole & Baddeley, 1990).

Finally, many studies reviewed here simply assess a single measure of procedural learning (e.g., serial reaction time) in a group of children with developmental language disorder (e.g., Perlant & Largy, 2011). Such a design cannot establish a selective deficit in procedural learning. Establishing a differential deficit requires that groups are compared on at least two tasks that purportedly measure different abilities; though making direct inferences about differences in ability from differences in task performance is fraught with difficulties (Chapman & Chapman, 1973). If children with language learning disorders do poorly on a serial reaction time task this could have many explanations: They might perform more poorly than controls on any cognitive task. Furthermore, even if children with language learning disorders really do perform more poorly on the serial reaction time task than on a single measure of declarative memory, this might reflect specific features of the two tasks, rather than differences in the underlying constructs of procedural versus declarative memory. It will be essential for future studies in this area to use multiple measures of multiple constructs if they wish to make claims about specific deficits in a procedural memory system. The measures used must also have adequate reliability, since differences in reliability can lead to spurious conclusions about differences in performance between groups (more reliable tasks are more likely to detect group differences).

Our finding that there is considerable task specificity in implicit learning in language disorders (see Table 3) is consistent with evidence from several large-scale correlational studies. Gebauer and Mackintosh (2007) found no relationship at all between performance on a serial reaction time task, an artificial grammar learning task, and a complex systems process control task in teenagers. Similarly, Pretz et al. (2010) failed to find any significant relationship between serial reaction time and artificial grammar learning in adults. There is also mounting evidence that the same implicit learning tasks correlate poorly with one another across visual and auditory modalities (Erickson et al., 2016), as well as across verbal and nonverbal modalities (West et al., 2018). These findings all cast doubt on the psychometric validity of a procedural memory construct as assessed by current, widely used, measures.

Summary and Conclusions

The procedural deficit hypothesis of language learning disorders embodies two interrelated claims, both of which are controversial. First, that the human memory system is organized into separable procedural and declarative systems (Squire, 2004; Squire & Dede, 2015), and second, that a deficit in procedural memory is a causal risk factor for developmental language disorder (Ullman, 2004; Ullman & Pierpont, 2005) and dyslexia (Nicolson & Fawcett, 2007, 2011). In our view, it is impossible to reach clear conclusions about this hypothesis based on current evidence.

Theoretically, the claim that the human memory system is organized into separable procedural and declarative systems remains highly controversial (e.g., Henke, 2010; Perruchet & Pacteau, 1990). One major obstacle to progress is that existing measures of procedural learning are highly impure and, possibly as a consequence of this task impurity, tend to have very poor reliability. Many have noted that it is far from clear which tasks actually measure implicit learning (e.g., Arnon, 2020; Erickson et al., 2016; Krishnan & Watkins, 2019; Siegelman et al., 2017; West et al., 2018) and to what extent the different tasks used actually measure a common (procedural or implicit) learning mechanism. These problems of measurement need to be resolved before the procedural deficit hypothesis of language learning disorders can be adequately tested.

We have outlined at length a range of methodological limitations that plague this area. Future studies will need to use multiple measures of multiple constructs (procedural learning, declarative learning, and attention at a minimum) to demonstrate specific deficits. Our conclusion is that current evidence simply does not provide an adequate test of the procedural deficit hypothesis of language learning disorders.

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