Research Article

Dose Schedule and Enhanced Conversational Recast Treatment for Children With Specific Language Impairment

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Purpose: Dosage has been identified as an important element of treatment that may affect treatment efficacy. The purpose of this study was to examine the role of dose schedule for treatment of grammatical morphology deficits in children with specific language impairment (SLI).

Method: Sixteen 4-to-5-year-old children with SLI participated in a 5-week intervention consisting of equivalent daily Enhanced Conversational Recast treatment (Plante et al., 2014) targeting grammatical morphology. Half of the children received treatment in one 30-min session (*massed* condition). Half received treatment in three 10-min sessions (*spaced* condition) within one 4-hr period. Progress was assessed 3 times weekly by probing a child's use of his or her treatment morpheme

and untreated morpheme (a maturational control) in untreated contexts.

Results: Pre- to posttreatment morpheme usage differed significantly for children regardless of dosage condition, and pre to post usage of an untreated morpheme was unchanged, demonstrating overall treatment efficacy. There were no differences in treatment effects for the massed and spaced conditions.

Conclusions: The study adds to evidence that Enhanced Conversational Recast can produce positive results in a short period of time for children with SLI. Furthermore, clinicians may have some flexibility in terms of the dose schedule they use to deliver this treatment in an evidence-based manner.

Treatment outcomes rest on both the effectiveness of the treatment technique, as well as the amount of treatment provided and schedule of delivery of the treatment. In the past several years, there has been increasing focus on treatment intensity. Warren, Fey, and Yoder (2007) provided the first standard definitions of elements of treatment intensity in the language intervention literature. *Treatment dose* has been defined as the number of properly administered teaching episodes, *dose frequency* as the number of times treatment is provided per day or per week, and *total intervention duration* as the time period over which treatment is provided (Warren et al. 2007). Furthermore, in the treatment intensity literature and the larger body of literature on language learning, the delivery schedule of teaching episodes has been identified as a potential contributor to language learning outcomes.

Given the potential influences of dosage on treatment efficacy, this study was designed to investigate the effects of two different treatment dose schedules on the outcomes of grammatical learning by children with specific language impairment (SLI). Using the treatment technique Enhanced Conversational Recast (Plante et al., 2014) that has previously been established as efficacious, *massed* treatment delivery and *spaced* treatment delivery were examined for their effects on grammatical learning by preschool-age children. The secondary purpose of the study was to attempt to replicate previous research regarding the efficacy of the Enhanced Conversational Recast treatment technique.

Treatment Distribution

One common distinction between dose delivery schedules within language learning studies is whether doses are delivered clustered within a compressed period of time (massed) or whether they are distributed across a longer period of time (spaced). Effects of massed and spaced practice on learning many types of skills have been studied for over 100 years in numerous contexts and across various modalities of perceptual learning (e.g., visual, verbal, and motor tasks). Literature on massed and spaced learning

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generally indicates that there is an effect of enhanced learning that occurs following spaced practice, commonly referred to as the distributed learning effect (e.g., Janiszewski, Noel, & Sawyer, 2003).

Many studies of massed versus spaced learning have involved various forms of artificial and natural language learning in which materials were presented and tested verbally. Often these studies have involved the learning of paired associates or word lists. A meta-analysis of 93 studies demonstrated the benefit of distributed learning for a wide variety of linguistic targets, including real or novel words and sentences, delivered in various modalities (e.g., auditory, visual) to adults and children (Janiszewski, Noel, & Sawyer, 2003). However, none of the studies included in that review involved children with communication disorders. Often those studies measured recall for exact items, thus testing rote memory. This is likely very different from how language is learned either naturally or in the context of language treatment. Therefore, the relevance of these studies to more naturalistic contexts is unclear. Furthermore, learning words is different from learning morphosyntax because instead of involving repetitions of stable phonological forms, learners are exposed to various exemplars of a grammatical pattern and must extract a rule from the given exemplars. Thus, it is critical that the effect of dose schedule, an element of treatment intensity, be examined through a task that requires rule learning.

A handful of studies have examined the effect of dose delivery schedule on language learning by children. One study involving preschool-age children examined the effect of spacing on learning when information must be abstracted from multiple exemplars, a context that is more similar to that involved in learning grammatical forms. Vlach, Sandhofer, and Kornell (2008) used a category induction task with 3-year-old children. Children were presented with pictures of objects that were exemplars of discrete categories. Children performed better on a follow-up test for category exemplars that had been presented in a spaced manner than exemplars that had been presented in a massed sequence. The benefit of spaced learning trials occurred even though spacing may have increased the difficulty of the task by allowing children time to forget previous exemplars (Vlach et al., 2008).

In the domain of lexical learning, Riches, Tomasello, and Conti-Ramsden (2005) presented novel verbs to children with SLI through either 12 presentations massed in one session, with 18 presentations in one session or 12 presentations spaced across four sessions. The children were tested by being asked to demonstrate the action of the novel verb with an object, produce the novel verb, and receptively identify the novel verb. Children learned better in the more distributed (four sessions) condition, even though one of the massed conditions actually provided more presentations (i.e., a higher dose). However, this effect was only significant for children's production of new words, not for their recognition of them. This production difference, however, is consistent with the statement by Childers and Tomasello (2002) that "given an equal number of exposures, distributed practice

at skills is almost always superior to massed practice with skills" (p. 968).

In the context of language treatment, Smith-Lock et al. (2013b) examined the effect of dose frequency, while keeping cumulative intervention intensity (in minutes) constant. They implemented a grammar treatment program they had previously developed for use with 5-year-old children with SLI (Smith-Lock, Leitão, Lambert, & Nickel, 2013a). The basic treatment procedures consisted of direct instruction, imitation, focused stimulation, and recasting. This combination of treatment procedures produced a significant treatment effect over 8 weeks, with a large overall average effect size of $d = 1.24$, or an average of $d = 1.66$ for children without articulatory limitations (Smith-Lock et al., 2013a). In another study (Smith-Lock et al., 2013b), dose frequency was manipulated to create two conditions: a massed condition, in which treatment was administered in 1-hr sessions one time per day for 8 days, and a spaced condition, in which 1-hr sessions were provided one time per week for 8 weeks.

Results showed that children who received treatment weekly outperformed children who received treatment daily. Although the study was designed to avoid confounds of different dose forms and cumulative intervention intensity in minutes, there are caveats to the type of design they used. This study can be conceptualized as providing massed intervention (8 consecutive days) or spaced intervention (8 days distributed over 8 weeks). However, this design resulted in two very different treatment durations (just over a week versus 8 weeks). In addition, the authors stated that dosage—the number of teaching episodes—was not controlled and may have been different between groups (Smith-Lock et al., 2013b).

Given the results of the abovementioned studies, an argument in favor of spaced treatments can be made. Researchers have attributed the benefit of spaced learning to factors such as greater variability of contextual cues during encoding (Glenberg, 1979) and the greater challenge for memory retrieval when items to be learned are more separated in time (Schmidt & Bjork, 1992). Variability of encoding occurs naturally in spaced trials. If input is provided with some time elapsing between exemplars, each trial will naturally occur in somewhat different contexts (e.g., changes in the environment and different objects or actions co-occurring with each presentation). These contextual differences could both support encoding and provide additional cues that support retrieval. In a simple sense, the longer the exposure period, the more likely that variability will be introduced into the learning context. In a similar way, the spacing in time between exemplars may make participants more likely to forget the previous one before the next is encountered. Rather than being detrimental, the requirement to recall the previous exemplar is more difficult and requires more practice to do, thus strengthening the memory (e.g., Vlach et al., 2008).

Barratt, Littlejohns, and Thompson (1992) conversely demonstrated that a more intensive treatment schedule (massed treatment) led to greater improvements in expressive

language outcomes compared with less intensive intervention (spaced treatment). Their 2- to 5-year-old participants received treatment either four times weekly in a 3-week block within each of two 3-month cycles or one time weekly for 6 months. In this way, overall treatment duration was partially controlled at approximately 6 months. The dose number and cumulative intervention intensity were held constant. However, the dose frequency and related spacing of treatment sessions was unequal between conditions. Although this study lacked strong experimental control over these parameters, the authors concluded that higher intensity treatment (massed learning) led to greater gains than the lower intensity treatment (Barratt et al., 1992).

Ukrainetz, Ross, and Harm (2009) examined the effects of two intervention schedules on training four subskills of phonemic awareness for at-risk kindergartners in a school setting. Forty-one 5- to 6-year-old children with low phonemic awareness were provided training for 11 hr, either concentrated (massed) into three times per week or dispersed (distributed) at one time per week over a longer period of time. A control group received an unrelated dispersed vocabulary intervention. The results showed no difference between concentrated and dispersed groups' performance following the training period, except on one of four phonemic awareness subskills, which improved slightly more in the dispersed condition than in the concentrated condition. However, the overall conclusion was that there were no differences in the outcome for the two dose schedules at either of the time points. The distribution did not appear to be an important factor for phonemic awareness instruction.

In a study of recast densities, akin to the concept of massed or spaced input delivery, Proctor-Williams and Fey (2007) did not find a relationship between recast density of novel past tense verbs and learning outcomes in their study of 7- to 8-year-old children with SLI. Children were presented different recast densities, *conversational* (low density) and *intervention* (high density) over five intervention sessions. Models were used to hold constant the number of exposures to the novel verbs to investigate the specific role of the rate of recasts on learning. When tested for use of the novel past tense verb, the performance of children with SLI did not differ, depending on the recast density. However, because neither level of recast provided was effective for children with SLI, more sessions or an easier task may have been needed for learning to be robust and possibly allow the emergence of a density effect. Given the differing results of both experimental and treatment studies that have contrasted massed versus spaced learning, it is possible that either condition could lead to superior treatment effects.

Conversational Recast Treatment

One treatment method that has consistently shown positive outcomes is conversational recast treatment (cf. Cleave, Becker, Curran, Van Horne, & Fey, 2015). Several treatment studies reported in the language intervention

literature provide evidence that supports the basic efficacy of conversational recast treatment for targeting morpho-syntax in preschool-age children (e.g., Camarata & Nelson, 1992; Camarata, Nelson, & Camarata, 1994; Leonard, Camarata, Brown, & Camarata, 2004; Leonard, Camarata, Pawlowska, Brown, & Camarata, 2006; Nelson, Camarata, Welsh, Butkovsky, & Camarata, 1996). Conversational recast is a promising treatment that may be improved to further increase the efficacy or efficiency of the procedure. Dose schedule is one parameter that may influence the effectiveness of this treatment.

In a previous study, Plante et al. (2014) used principles of learning theory to increase the efficacy of conversational recast procedures (cf. Alt, Meyers, & Ancharski, 2012). The adaptation of Plante et al. (2014) will be referred to as *Enhanced Conversational Recast*. In *Enhanced Conversational Recast*, salience of the treatment target is enhanced by introducing high variability for nontargeted linguistic components into the input provided to the child (cf. Gómez, 2002; Grunow, Spaulding, Gómez, & Plante, 2006; Torkildsen, Dailey, Aguilar, Gómez, & Plante, 2013). Furthermore, clinicians introduce high context variability by using a variety of activities and materials during sessions and infrequently repeating activities. These procedures incorporate variability into the materials and training context (cf. Perry, Samuleson, Malloy, & Schiffer, 2010) and facilitate high lexical diversity, not only within but also across treatment sessions (Plante et al., 2014). In addition, in *Enhanced Conversational Recast*, clinicians use attentional cues (e.g., a light touch, directing the child's gaze to the clinician's face, and the command look) or may position themselves close to the treated child, usually in his or her field of view, to increase the likelihood that the child attends to the recast (i.e., looks at the clinician) when it is administered.

The *Enhanced Conversational Recast* study (Plante et al., 2014) also demonstrated that 24 unique recasts provided within each treatment session benefitted learning (Plante et al., 2014). However, that same benefit was not obtained if the massed recasts contained less linguistic variability. Furthermore, these children heard a large number of unique lexical items embedded in the conversational recasts given over the entire course of the study. However, this variability alone did not appear to affect learning as much as the degree of lexical variability that occurred within each treatment session. This finding lends support to the notion that massed treatment benefits are possible when the basic treatment is itself effective.

Current Study

The purpose of this study was to examine the effects of treatment dose schedule on treatment outcomes for children with SLI treated with *Enhanced Conversational Recast*. In particular, the study examined massed versus spaced intervention delivery on an input-based treatment for grammatical morpheme remediation. In the massed treatment schedule condition, treatment was delivered during one 30-min session. In the spaced treatment schedule

condition, the same treatment was spread across three 10-min sessions over 4 hr. In addition, the within-group control condition allowed for an examination of treatment efficacy in both conditions.

To examine the effect of dose schedule on treatment efficacy, the treatment itself must be efficacious overall. Following from Plante et al., 2014, we first sought to replicate the efficacy of Enhanced Conversational Recast as a treatment. If the treatment is efficacious overall, then there will be significant differences between pre to post gains on target versus control morphemes.

The second research question concerns the effect of dose schedule for this treatment, both in terms of generalization of morpheme use and long-term retention of end-treatment learning. Given that there are plausible reasons to hypothesize that either massed or spaced treatment could result in superior outcomes, the hypothesis under study is necessarily bidirectional. Thus, the hypothesis is if massed or spaced dose schedules differentially affect the efficacy of Enhanced Conversational Recast treatment, then there will be significant group differences in treatment outcomes for children receiving one dose schedule compared with the other.

A third research question is whether or not end-treatment learning would be maintained. Thus, a long-term retention measure was planned to take place approximately 8 to 10 weeks after the end of the 5-week treatment period. If the children demonstrated long-term retention of learning, then the long-term retention measure would be significantly correlated with end-treatment outcomes.

Method

Participants

Sixteen children ages 4;10 (years; months) to 5;10 ($M = 5;3$) with SLI participated in this study. Children ($n = 8$ per group) were randomly assigned to massed or spaced treatment dose conditions. All children participated for the full duration of the intervention program. Children met the standard inclusionary criteria for SLI: language impairment in the absence of other handicapping conditions. Language impairment was indicated by a score of 87 or less on the Structured Photographic Expressive Language Test–Preschool Second Edition (SPELT-P2; Dawson et al., 2003), an empirically derived cutoff score validated for monolingual children (Greenslade, Plante, & Vance, 2006). To rule out the presence of hearing loss, all children passed a pure-tone hearing screening at 20 dB at 1000, 2000, and 4000 Hz, and 25 dB at 500 Hz. Intellectual disability was excluded by requiring a nonverbal IQ score of greater than 75 ($70 + 5$, standard error of the mean) on the nonverbal scales of the Kaufman Assessment Battery for Children–Second Edition (Kaufman & Kaufman, 2004).

Additional exclusionary criteria included parental report of a handicapping condition that could account for poor language (e.g., neurological disorders, hearing loss,

and autism). A diagnosis of attention-deficit/hyperactivity disorder was not an exclusionary criterion for the study, but no child had a diagnosis of attention-deficit/hyperactivity disorder. As descriptive measures of receptive language skills, the Peabody Picture Vocabulary Test–Fourth Edition (Dunn & Dunn, 2015) and Test of Language Development–Preschool, Third Edition, Grammatical Understanding subtest (Newcomer & Hammill, 1997) were also administered to children prior to the start of the program. These scores are reported in Table 1.

Participants were primary English speakers; however, two children (S3 and M3) had considerable exposure to Spanish. A licensed and certified bilingual speech-language pathologist, who was not otherwise involved in this study, confirmed language impairment for both of these children. All children had attended a preschool or day care where English was spoken, with the exception of one monolingual English-speaking child (S4) who had never been enrolled in a preschool or day care program.

A higher rate of speech sound errors may be reasonably expected in groups of children with SLI relative to typically developing peers, so children involved in this study needed only to have sufficient speech sound production skills to (a) produce the target and control morphemes in the appropriate word position(s) and (b) be reasonably intelligible and consistent in speech production such that recasting of the child's utterance by an adult would be possible in most instances. To ensure that children who would participate in the study had sufficient articulatory ability to produce the morphemes of targets and controls, children were administered the Goldman-Fristoe Test of Articulation–Second Edition (Goldman & Fristoe, 2000) prior to the start of the treatment program. Goldman-Fristoe Test of Articulation–Second Edition scores for children in the spaced condition did not differ significantly from those in the massed condition, $t(14) = 0.849$, $p = .410$, $d = 0.362$.

Although no child was receiving outside language treatment at the time of study, 11 of the 16 children were receiving concurrent articulation therapy during the language treatment period. Articulation therapy was provided through the preschool program but did not address the speech sounds specific to the child's target or control morphemes, nor was grammar addressed in any way during the articulation therapy.

Demographic variables (i.e., gender and age) and test scores for children assigned to spaced and massed conditions were tested for group differences using two-tailed t tests. Neither gender nor age differed significantly between groups, $t(14) = 0.742$, $p = .470$, $d = 0.320$. There were no group differences in scores obtained on any standardized test ($p > .05$ for all): SPELT-P2: $t(14) = 0.982$, $p = .343$, $d = 0.372$; Kaufman Assessment Battery for Children–Second Edition: $t(14) = 0.076$, $p = .940$, $d = 0.037$; Peabody Picture Vocabulary Test–Fourth Edition: $t(14) = 1.796$, $p = .094$, $d = 0.866$; Test of Language Development–Preschool, Third Edition, Grammatical Understanding subtest: $t(14) = 0.888$, $p = .390$, $d = 0.343$.

Table 1. Demographic information and standardized test scores.

Participant	Age	Gender	K-ABC	SPELT-P2	PPVT-4	TOLD-GU	GFTA-2
Spaced condition							
S1	4;10	F	132	75	129	15	94
S2	5;4	M	103	78	100	11	91
S3	5;3	M	111	82	110	8	106
S4	5;0	M	104	82	97	7	54
S5	5;10	F	99	61	88	8	50
S6	5;8	M	90	70	104	9	93
S7	5;3	F	105	64	97	9	99
S8	5;5	M	94	73	104	11	45
<i>M</i>	5;4		104.75	73.13	103.63	9.75	79
<i>SD</i>	3.9 mos.		12.83	7.79	12.13	2.55	24.83
Massed condition							
M1	4;11	F	102	79	117	8	50
M2	5;6	F	122	84	100	11	84
M3	5;4	M	107	42	86	10	66
M4	5;6	F	99	84	90	8	53
M5	5;1	M	122	58	93	8	78
M6	5;1	M	107	84	93	9	79
M7	5;0	M	91	60	84	9	96
M8	5;4	F	84	34	82	8	54
<i>M</i>	5;3		104.25	65.63	93.13	8.88	70
<i>SD</i>	2.7 mos.		13.44	20.14	11.24	1.13	16.81

Note. Age (years; months). F = female; M = male. K-ABC = Kaufman Assessment Battery for Children–2nd Edition, Nonverbal Scale; SPELT-P2 = Structured Photographic Expressive Language Test–Preschool Second Edition; PPVT-4 = Peabody Picture Vocabulary Test–Fourth Edition; TOLD-GU = Test of Language Development–Preschool, Third Edition, Grammatical Understanding subtest; GFTA-2 = Goldman-Fristoe Test of Articulation–Second Edition. Test scores are standard scores with a mean of 100 (*SD* = 15), with the exception of the TOLD-GU (*M* = 10, *SD* = 3).

Procedures

Pretreatment Probe Sessions

To determine appropriate target and control morphemes for each child and to establish baseline use for these morphemes, the first three consecutive days of the intervention program exclusively involved probes of morpheme use. Potential grammatical morphemes to be probed were identified for each child on the basis of errors the child had previously made on the SPELT-P2 or during a language sample obtained prior to the start of the study. Four to six morphemes were probed by clinicians working with children in one-on-one play sessions. Using paper data collection forms, clinicians recorded whether the child demonstrated correct use of a morpheme (spontaneous or elicited) or incorrect use, either due to morpheme errors or morpheme omissions in linguistic contexts that would normally obligate their use. All sessions were audio- and video-recorded to allow for clinicians to resolve any questions concerning the child's morpheme use and the clinician's data form after the session had ended.

The target number of probes was 10 per morpheme per day for each child. It was critical that clinicians correctly obtain adequate evidence of pretreatment use, which we defined as 10 elicitations that obligated the child to use the intended form. However, in the moment, clinicians were sometimes unsure whether each probe had been correctly elicited. Clinicians were encouraged to elicit extra targets if they were unsure if they had obligated the child's use, rather than end a session with fewer correctly administered

probes than were needed. They were then asked to consult video recordings to determine whether a questionable elicitation should be retained. All clinicians exceeded the 10 at least once per child. In most cases, one to two extra probes were administered (14 cases for target morphemes and 19 cases for control morphemes) because clinicians wanted to be certain that they had adequately probed for the use of the morphemes. For two children, 13 probes were obtained for target morphemes. Even with extra probes, pretreatment use was low in all cases. Because the number of probes varied in these cases, we used percentages to describe child performance.

Two morphemes were selected among the probed morphemes for use in the experimental protocol over the course of the 5-week program. One morpheme was selected for treatment, and the other was tracked but not treated. Improvement in the use of the treated morpheme, but not the untreated morpheme, would be consistent with an effect of treatment, rather than maturation. The morpheme targeted for remediation (the target morpheme) and the untreated morpheme (the control morpheme) were selected from among the morphemes probed during the first 3 days. The criteria for selecting morphemes as either target or control forms included (a) low accuracy use and (b) no increasing trend in accuracy across the three pretreatment days. Low accuracy use was defined as 0%–40% accuracy in response to probes obligating the use of the morpheme on each pretreatment probe days, with a mean of no more than 33% average correct use across the three consecutive probe days. Selection of target morphemes prioritized

morphemes with the lowest accuracy use, with a secondary consideration of achieving balanced representation of morphemes serving as target and control forms across children. Pretreatment use for each child is reported in Table 2. There were no differences in pretreatment target morpheme use for children in spaced and massed conditions, $t = 0.906$, $p = .380$, $d = 0.376$.

Treatment Conditions

Children were randomly assigned to one of two treatment schedule conditions: either the massed or spaced condition. Children in the massed condition ($n = 8$) were provided treatment in a once-daily 30-min session. Children in the spaced condition ($n = 8$) were provided treatment in three 10-min sessions per day, each separated by 75 to 115 min, over a 4-hr period. All other treatment parameters were held constant across the two conditions.

Treatment Procedures

Treatment was provided in one-on-one sessions by one of five trained research clinicians over a period of 5 weeks (21–26 days, $M = 25$ days). Each clinician provided treatment to an equal number of children in both experimental conditions (massed and spaced).

Conversational recast is commonly used in treatments designed to help children acquire proper morphosyntactic structure. In the current study, *focused* recasts were used, which target only a single morpheme throughout the treatment period. These focused recasts can additionally be described as *simple* recasts, because only one grammatical feature of the child's utterance is corrected. In this procedure,

the child and the clinician engage in child-directed play, and the clinician provides opportunities for the child to communicate. Recasts follow child statements, those produced spontaneously or elicited by the clinician, in which the child's treatment target is obligatory. This statement is called a *platform utterance*. The *recast* occurs when the clinician restates the child's utterance with the target morpheme used correctly. This provides an immediate, correct example of the target morpheme relevant to the child's interest at that time. The clinician's recast is always grammatically correct, but need not be corrective (i.e., recasts can follow correct child platform utterances).

Clinicians could recast child-initiated platform utterances or elicit utterances that used particular verbs by modeling the uninflected form of the verb and then eliciting an utterance that should obligate its use from the child. Recasts after either spontaneous or elicited platform are equally effective (Hassink & Leonard, 2010). In addition, the ability to elicit platform utterances allowed the clinician greater control over the timing of recasts within a session, as well as the words the child was obligated to use in their platform utterances. All recasts were contingent upon a child's preceding utterance, and clinicians were not permitted to model the target grammatical morpheme in the recast elicitation.

A previous study by Plante et al. (2014) provided evidence for a version of conversational recast treatment, Enhanced Conversational Recast, in which the clinician incorporates high variability in the linguistic input provided to the child, supported by the incorporation of a wide variety of activities and attentional cueing. Clinicians provided

Table 2. Performance data for children in the spaced and massed treatment conditions.

Condition and participant	Target form				Control form			
	Target	Pre Tx (%)	End Tx (%)	<i>d</i> score	Control	Pre Tx (%)	End Tx (%)	<i>d</i> score
Spaced condition								
S1	doesn't	0	100	17.32	ques. form	30	20	-1.00
S2	she	3	100	16.74	ques. form	0	3	0.58
S3	has	3	93	15.01	past -ed	17	0	-2.89
S4	is -ing	0	80	8.00	3ps	13	6.7	-1.15
S5	is -ing	0	76	5.02	3ps	20	3	-2.89
S6	past	10	33	1.01	has	0	3	0.58
S7	3ps	7	3	-0.58	is -ing	17	26.7	0.4
S8	is -ing	20	0	-3.46	she	0	0	0
<i>M</i>		5.38	60.63	7.38		12	7.80	-0.80
<i>SD</i>		6.93	42	8.21		11	9.99	1.45
Massed condition								
M1	she	0	96	16.74	doesn't	3	53	0.52
M2	has	3	100	16.74	past -ed	23	10	-1.33
M3	is -ing	3	96	16.17	past -ed	6.7	0	-1.15
M4	past -ed	16	70	5.33	are -ing	30	0	-5.19
M5	past -ed	16	83	4.36	3ps	23	3	-3.46
M6	3ps	33	66	2.18	she	13	3	-2.31
M7	past -ed	3	13	0.66	3ps	3	27	0
M8	is -ing	3	27	0.62	3ps	0	0	0
<i>M</i>		9.63	68.88	7.85		16.09	8.63	-1.62
<i>SD</i>		11.31	32.79	7.39		12.01	18.25	1.95

Note. Tx = treatment; 3ps = third person singular form.

varied linguistic input in multiple ways. First, clinicians were directed to use at least three different activities per day and a variety of materials across sessions as the contexts for the conversational recast treatment. High context variability naturally provides greater opportunity for linguistic diversity, even when clinicians are not explicitly instructed to use more complex or diverse forms (cf. Hadley & Walsh, 2014). However, the exact number or type of different activities was not specified, and clinicians were free to select or develop activities they thought would be of interest to their individual children.

High linguistic variability was achieved by providing recasts that contained 24 different verbs in utterances containing target pronoun or verb morphology. When the target was a bound morpheme verb inflection (i.e., regular past tense *-ed*, third person singular *-s*, auxiliary *is* [*verb*] *-ing*) or pronoun (i.e., *she*), 24 different verbs were used with the target in the recasts. When the target was the proper use of other free morphemes (i.e., *has* and *doesn't*), the different recasts contained unique lexical items surrounding the target, as appropriate (e.g., It doesn't fit; she doesn't eat that; this one doesn't have a tail). Clinicians were instructed not to repeat words in multiple recasts (e.g., He runs there. He jumps there. He looks there). Clinicians were not constrained in their use of recasts with regard to positions of the target morpheme within the utterance, utterance length, or vocabulary use (other than not using probe words and control morphemes, and not repeating the same lexical items frequently).

A critical feature of Enhanced Conversational Recast is that the child's attention must be on the clinician at the time the recast is provided. The salience of the recasts is increased by providing attentional cues to the child to ensure attention at the time of the recast. These include saying the child's name; lightly touching the child's hand, arm or shoulder; or getting in the child's visual field immediately before recasting the utterance. In cases in which a more substantive attention cue was needed, the clinician may have said, "Look at me," to get the child in the habit of attending. The clinician would then wait for another opportunity to provide a recast in those instances. The choice of attentional cue was dictated by the type of cue to which a child reliably responded. These cues were not needed and therefore not used in instances when the child was already watching the clinician prior to the recast. Clinicians received specific training for obtaining a child's attention for the recast for children who did not consistently make eye contact during the conversational recast with minimal cueing. Therefore, while the treatment occurred in a naturalistic play context, there were attentional cues to look at the clinician. To increase a child's attention, the clinicians were instructed to follow the child's lead with play activities and encourage participation by giving choices.

Treatment Dosage and Intensity Parameters

Treatment in this intervention program was provided using Enhanced Conversational Recast procedures in daily one-on-one sessions (Plante et al., 2014). Thus, treatment

dose form in this study consisted of a focused recast that used vocabulary that was unique to that recast and was administered to a child who was attending (i.e., looking at the clinician) during the recast. The treatment dose was 24 conversational recasts per day targeting a specific grammatical morpheme, regardless of whether these were administered in the massed or spaced condition. Other elements of dose include session duration and the rate and distribution of doses over the treatment session (Warren et al., 2007). The session duration was either 10 min (spaced) or 30 min (massed), and the dose frequency per day was three sessions or one session, respectively. The duration $\times$ dose frequency yields daily intensity, which was equal across groups: 10 min $\times$ 3 sessions = 30 min or 30 min $\times$ 1 session = 30 min. Thus, children in both groups received a total of 24 conversational recasts in 30 min of treatment per day.

The overall rate of delivery was controlled across the spaced and massed conditions at eight recasts per each 10-min block of time. Clinicians were also instructed to distribute recasts as evenly as reasonably possible throughout the sessions, though the actual distribution of recasts within each 10-minute interval was not monitored. Clinicians were given timers set to 10-min intervals to facilitate their adherence to the eight recasts per 10 min rate of delivery. This rate of delivery averaged to approximately one recast every 1.25 min.

The total intervention duration was just over 5 weeks. The possible number of days of the intervention was 26, and children attended approximately 25 days on average ($M = 24.625$, range = 21–26 days). A t test confirmed that the number of treatment days children attended did not differ in the spaced condition ($M = 24.5$, $SD = 1.6$) and massed condition ($M = 24.75$, $SD = 1.8$), $t = 0.290$, $p = .776$, $d = 0.136$.

The average of 25 days of treatment yielded a cumulative intervention intensity of approximately 600 conversational recasts containing the target morpheme (24 recasts per day for 25 days) provided in an average of 750 min of treatment (30 min per day for 25 days). The actual ranges for participants involved in this study were 21–26 days, 504–624 recasts, and 630–780 min.

Generalization Probes

To assess each child's progress during the 5-week treatment period, generalization probes were administered 3 days per week, always prior to treatment. This was intended to ensure that generalization probe data would reflect a child's previous learning and not learning from earlier that same day. Generalization probes always included 10 obligatory contexts to probe for use of the target morpheme and 10 obligatory contexts to probe for use of the control morpheme. Any other use of the target or control forms that may have occurred outside of probe contexts was not quantified and is not reflected in the outcome data. All probes were conducted using a set of verbs, and materials included in a variety of thematic toy sets called *probe kits* (e.g., sea world or soccer players) that were never

used during treatment. The use of verbs (or lexical items) reserved for generalization probes ensured that a child's performance on these probes reflected generalization, rather than prior experience with the toy sets or particular vocabulary. To begin a probe activity, the clinician established a realistic play actions or story context that was appropriate to a particular probe kit and appropriate tense (if applicable) of the actions or story without modeling the target form.

Long-Term Retention

A follow-up appointment was planned to take place approximately 8 to 10 weeks after the end of the treatment program to obtain a measure of long-term retention. During the follow-up appointment, generalization probes, identical to those used during the treatment program, were administered to probe for the child's use of the treatment and control morphemes. The person conducting the follow-up probe session was familiar to the child from the program, but was not the clinician who treated the child during the treatment phase.

Experimental Control

Parents and classroom teachers were blind to which element of grammar served as the target and control morphemes for each child. Although informed that their child would be receiving treatment, parents were not present to observe treatment sessions and were not given a summary of their child's progress until after the conclusion of the program. Clinicians were not told about the study hypothesis or theoretical framework driving the hypothesis.

Fidelity and Reliability

Clinician training. The five research clinicians were trained for two half days (8 hr total) to facilitate a high level of treatment fidelity. Training for clinicians included reviewing the fundamental components of conversational recast through reading, discussion, video review, and multiple opportunities for practice and feedback until a high level of proficiency in the procedure was attained by each clinician. Clinicians provided treatment under the consistent supervision of two certified speech-language pathologists (the authors). Supervision was provided for 50% of initial probe and treatment sessions and a minimum of 25% of all treatment sessions. Feedback from the supervisors concerning adherence to the treatment protocol was provided on an ongoing basis, at least twice daily (at the midpoint of the half day and at the conclusion of the half day), as needed throughout the treatment period.

Fidelity tracking. Clinicians collected data on all components of treatment critical to this treatment protocol in every session. In addition, all components of the treatment critical to treatment fidelity were evaluated live by trained data trackers for 11% of treatment sessions ($n = 47$). This included correctly administered recasts (i.e., recasts that did not follow a child imitation of an adult model that directly followed a platform utterance and that did not repeat free morphemes adjacent to the trained morpheme). Note that we did not assess whether clinicians had the

child's attention before recasting. First, it took varying amounts of time for clinicians to find a cue that worked. Second, children learned to attend over time, and attention was not always reliable even when a cue was used. The dose rate (eight recasts per 10-min block) and number (24 per session) was also evaluated. We also assessed whether probe procedures appeared in treatment sessions (i.e., use of probe words during treatment and the use of probe materials during treatment).

Data were collected by both the clinician and data tracker separately by using paper data-tracking forms. Reliability trackers did not include those individuals providing supervision of treatment and probe sessions (described previously). Three treatment sessions were evaluated for fidelity for each child, with the exception of one child for whom only two sessions were evaluated due to absences. All 47 treatment sessions evaluated for fidelity involved at least 24 properly administered recasts incorporating 24 different verbs (100% fidelity). In five sessions, there were one to two additional recasts administered. Although all 30-min daily treatments included 24 unique recasts, the dose delivery rate of eight recasts every 10 min was evaluated during only 32 of the 47 treatment sessions because the exact distribution of the recasts within each 10-min block was not manipulated. Out of 32 daily treatments evaluated (two for each child), there were only two violations to the rate of eight recasts every 10 min, and each time was due to a child factor—specifically, needing a bathroom break and needing a break for behavioral support (94% fidelity). No probe kit activities were used in any treatment session (100% fidelity). A probe word was used only one time in one session of the 47 evaluated for treatment fidelity (98% fidelity). Deviations from aspects of fidelity were distributed across the five clinicians, with no one clinician representing a disproportionate number of deviations.

Generalization probe procedural fidelity. For probe sessions, fidelity was determined on the basis of two primary components: setting up 10 obligatory contexts each to probe for use of target (100% fidelity) and control morphemes (100% fidelity). No clinician ever used materials other than those from the probe kits during probe sessions (100% fidelity).

Reliability of probe data. Reliability in recording the oral responses of children during generalization probes was confirmed by a second data tracker in the room who recorded a child's responses to all probed structures in 12.1% ($n = 31$) of generalization probe sessions. Data trackers responsible for tracking probe reliability were blind to which morpheme was the child's target and which was the control. Reliability was based on point-to-point agreement for 10 probes for the target and 10 probes for the control, for a total of 20 data points in each generalization probe session (85%–100%, with an average agreement of 96%).

Statistical Plan

The primary outcome measure was the child's use of target and control morphemes during a play-based probe

that obligated the use of the morphemes with untreated lexical items (the generalization probes). Analyses of the primary outcome measure were based on the specific study hypotheses. Treatment efficacy was tested by evaluating within-participant differences in pre- and posttreatment use of morphemes (target and control). The effect of massed versus spaced treatment conditions was tested by evaluating end-treatment use of target morphemes in each condition. In addition to the pretreatment and end-treatment quantity of correct responses to 10 obligatory context probes, the treatment effect size d was calculated to reflect change for each child's target and control morphemes during generalization probes. Note that between-groups effect sizes are reported as a traditional Cohen's d , which is labeled as such for clarity.

The treatment effect size is an expression of change in performance relative to starting performance. There are various ways to calculate effect size, but for the purpose of this study, the following method, originally used in Plante et al. (2014) was used. The treatment effect size d was calculated by subtracting mean correct morpheme use during the first three pretreatment probes (baseline) from mean correct morpheme use on the final three generalization probes (obtained in the last week of treatment). This difference was divided by the standard deviation of the final three generalization probes. In cases in which there was no variance in the final three generalization probes (i.e., they were all the same value), then the minimum possible standard deviation value was used (a difference of one response [10%] among the 3 days). This method is conservative statistically in that it inflates the denominator slightly (reducing the effect size slightly) but prevents dividing the numerator by zero in cases in which there is no variance. In addition, the use of the standard deviation of the final three generalization probes, rather than the pretreatment or pooled pretreatment and end-treatment standard deviation, is also a conservative estimate of d . Because children with SLI often show no pretreatment use of target morphemes, zero variance in the pretreatment probes would lead to a very small denominator and an inflated d value.

Although interpretation of Cohen's d effect sizes is well established (≤ 0.2 = small; 0.5 = medium; ≥ 0.8 = large), we believe that currently there are insufficient data to determine the small, medium, or large sizes for the treatment effect size d reported in this study, and the following effect size examples are provided as an illustration. The minimum possible effect size is 0, and the maximum possible effect size is 17.3 in this treatment context. However, the actual range for any given child is constrained by the amount of pretreatment use the child shows. A child that starts at a higher pretreatment level will necessarily have a smaller range of possible treatment d values than one who starts at a lower pretreatment level. For example, a child with 0% pretreatment use who achieves 100% posttreatment use achieves an effect size of 17.3; 10% average pretreatment use to 100% posttreatment can only achieve a maximum effect size of 15.6. A 20% average pretreatment use corresponds to a maximum possible effect size of 13.8.

A 30% pretreatment use corresponds to a maximum possible effect size of 12.1.

Because there were specific a priori hypotheses concerning the efficacy of the treatment (a within-group comparison) and the effect of mass versus spaced doses (a between-groups comparison), these effects were tested specifically. Due to the non-normality that occurs when many of the pretreatment baseline values are zero (or close to zero), but greater variability exists at the end of treatment, the nonparametric Wilcoxon's T was used for pre-versus posttest comparisons. The use of a nonparametric test avoids the need for a data transform. We used a parametric test (t test) in all cases where the distribution assumptions of a parametric statistic were not violated. Therefore, the remaining between-groups comparisons were tested with t tests. This approach was both consistent with the nature of the data and eliminated other, non-hypothesis-driven comparisons that might be tested with alternate statistics (e.g., analysis of variance). In addition, t tests were used to rule out an influence of the following potentially confounding variables. Correlations were used to describe the relation between treatment effects and child characteristics.

Results

Descriptive statistics for pre- and posttreatment performance are reported in Table 2. The posttreatment average correct responses to end-treatment probes was 60.63% for children in the spaced condition and 68.88% for children in the massed condition. The effects for target and control morphemes for both massed and spaced conditions are represented in Figure 1. Prior to hypothesis-driven analyses described previously, it was confirmed that the spaced and massed groups did not differ significantly in terms of pretreatment use, $t(14) = 0.906$, $p = .380$, Cohen's $d = 0.376$.

Figure 1. Average effect size for target and control morphemes by massed and spaced treatment condition. Error bars represent the standard errors.

Hypothesis 1: Treatment Efficacy

On the basis of a previous demonstration of efficacy of Enhanced Conversational Recast treatment (Plante et al., 2014), one overarching hypothesis was that this treatment would result in statistically significant change in target morpheme use in response to obligatory probes during the treatment period, regardless of whether doses were massed or spaced. In contrast, there should be no equivalent gains on the control morpheme. This hypothesis was tested by comparing performance during the final three probe sessions (performance on a total of 30 obligatory context probes) occurring the last week of treatment to performance on the three pretreatment probes. Consistent with this hypothesis, children used their target morphemes significantly more frequently during the final three probe sessions than during the three pretreatment probe sessions, Wilcoxon's $T_z = 3.31$, $p < .0009$, Cohen's $d = 1.241$. This significant increase applied to both the massed delivery condition, Wilcoxon's $T_z = 2.52$, $p = .0117$, Cohen's $d = 1.344$, and the spaced delivery condition, Wilcoxon's $T_z = 2.10$, $p = .0360$, Cohen's $d = 1.123$. These effects can all be considered large. The increase in morpheme use could not be accounted for by maturation or the general language stimulation that children received in the accompanying preschool language program, given that the control morphemes did not show a similar gain from pretreatment probes to end-treatment probes, Wilcoxon's $T_z = 1.16$, $p = .245$; Cohen's $d = -0.005$. This effect size can be considered small. Therefore, the change in morpheme use for the treated morpheme can be considered a treatment effect.

Overall treatment efficacy was demonstrated by an effect size d for treated morphemes significantly greater than the effect size d for untreated morphemes for participants in both conditions, Wilcoxon's $T_z = 3.05$, $p = .0023$, Cohen's $d = 0.952$. This is a large effect size for treated versus untreated morphemes. Though there were no gains on the untreated (control) morpheme ($d = -0.005$), the effect size for the difference between gains on treated versus control morphemes (Cohen's $d = 0.952$) can be considered large.

Hypothesis 2: Effect of Dose Schedule

The purpose of the experimental manipulation in this study was to determine whether either massed or spaced dose schedule of Enhanced Conversational Recast treatment would lead to a greater treatment effect (d). Thus, analysis of the primary outcome measure was limited to tests of the specific study hypotheses relating to the experimental manipulation of dose schedule. This was tested using two-tailed t test. The end-treatment correct use of target morphemes in response to obligatory probes did not differ between conditions, $t(14) = 0.436$, $p = .669$, $d = 0.057$, failing to confirm the hypothesis of a difference between treatment delivery methods. In addition, the effect size associated with the t statistic was very small, suggesting that a difference would be unlikely even with a much larger sample size.

Hypothesis 3: Long-Term Retention

Thirteen of the 16 children enrolled in the summer treatment program returned for a single follow-up measure between 7 and 11 weeks following the end of the treatment program. Family availability was the only factor that influenced the scheduling of the follow-up appointment. Three children (M1, S8, and S7) did not return for follow-up testing because they were not available due to family schedule or because the investigator was not able to make contact with the families.

The percent correct use of treated and control morphemes in response to 10 obligatory probes for each form at follow-up is reported in Table 3. A two-tailed t test indicated that there was no difference between performance on the target morpheme on the follow-up (retention) measure for massed and spaced groups, $t(11) = 0.078$, $p = .939$, $d = 0.042$. Figure 2 demonstrates that long-term retention was strongly associated with end-treatment performance on generalization probes ($r = 0.704$; $p = .0072$). For descriptive purposes, positive retention was operationalized as long-term retention that was no more than 20% lower than the average score on the final three generalization probes. Learning is operationalized as morpheme use of 50% or greater in response to obligatory probes. According to these criterion, nine of the 13 children (four in the spaced condition and five in the massed condition) who returned for follow-up testing demonstrated positive retention of learning with respect to end-treatment performance.

Discussion

The first hypothesis that Enhanced Conversational Recast would be an efficacious treatment for grammatical morpheme acquisition (a within-group comparison) was confirmed. In addition, follow-up data collected approximately 2 months following the end of the treatment program provide evidence of long-term retention (also a within-group comparison). As such, this study serves as a replication of the efficacy of Enhanced Conversational Recast for grammatical morpheme acquisition for preschool-age children with SLI. The present outcomes are consistent with previous results for children who received high variability input during Enhanced Conversational Recast treatment (Plante et al., 2014). The effect size obtained in this study, $d = 1.24$, is similar to the effect size obtained in Plante et al., $d = 0.92$.

The second hypothesis was that outcomes for Enhanced Conversational Recast treatment would differ on the basis of whether the dose schedule involved massed or spaced dose delivery (a between-groups comparison). The results indicated that there was no difference between groups. Furthermore, the very small effect size for this comparison (Cohen's $d = 0.057$) suggests that no difference would be present even with a much larger sample of children. In addition, evidence of long-term retention of treatment effects did not differ between groups.

The largely comparable outcome from spaced and massed dose delivery in the present study may be attributed

Table 3. Performance data: end-treatment and long-term retention.

Condition and participant	Target form			Control form		
	Target	End Tx (%)	Follow-Up (%)	Control	End Tx (%)	Follow-Up (%)
Spaced condition						
S1	doesn't	100	90	ques. form	20	10
S2	she	100	100	ques. form	3	60
S3	has	93	90	past -ed	0	10
S4	is -ing	80	40	3ps	6.70	30
S5	is -ing	77	80	3ps	3	10
S6	past	33	20	has	3	0
S7	3ps	3	0	is -ing	26.70	10
S8	is -ing	0	DNR	she	0	DNR
<i>M</i>		60.63	60		7.80	18.57
<i>SD</i>		42	39.48		9.99	20.35
Massed condition						
M1	she	96	DNR	doesn't	53	DNR
M2	has	100	100	past -ed	10	60
M3	is -ing	97	90	past -ed	0	40
M4	past -ed	70	50	are -ing	0	10
M5	past -ed	83	0	3ps	3	20
M6	3ps	66	80	she	3	0
M7	past -ed	13	DNR	3ps	27	DNR
M8	is -ing	27	50	3ps	0	20
<i>M</i>		68.88	61.67		8.63	25
<i>SD</i>		32.79	36.56		18.25	21.68

Note. Three participants did not return (DNR) for follow-up testing. Tx = treatment; 3ps = third person singular form.

primarily to two main causes. First, and most important, Enhanced Conversational Recast is an effective treatment. Therefore, the effect of the treatment technique likely overwhelmed any effect associated with differences in dose delivery. Had learning been less robust overall, it is possible that a dose schedule effect may have emerged. Consistent with this idea, Riches et al. (2005) showed the greatest advantage for spaced doses relative to massed doses for the more difficult-to-learn aspect of their task. In that study, children with SLI were asked to demonstrate both comprehension and production of the newly learned verbs. Although typically developing children in the Riches et al. (2005) study showed no difference in learning on the basis of dose differences, children with SLI benefitted from

higher frequency input (more exposures) in a more widely spaced dose (4 days versus 1 day) for the more difficult aspect of the verb learning task, verb production, for which overall performance was relatively poor.

Second, the current study involved a manipulation of dose schedule within a given day. Most other treatment studies investigating an intensity component manipulated the relative spacing of treatment over the course of days or weeks (e.g., Barratt et al., 1992; Smith-Lock et al., 2013b). The particular dose schedules used in the current study ensured that all children received the same number of doses and the same number of days of treatment over an equal time period (5 weeks). However, this treatment schedule raises the possibility that the role of daily treatment is contributing to the efficacy of this treatment and that spacing of input within each day is not a critical factor. Furthermore, the total treatment time of approximately 18 hr focused on a single morpheme may have also contributed to treatment efficacy. Other studies have found superior effects for spaced treatment provided fewer doses over more days in the spaced than massed conditions (e.g., Riches et al., 2005).

Figure 2. Scatter plot showing the correlation between end-treatment performance and follow-up performance.

Relationship to Other Studies

There is a large body of experimental evidence to suggest that spaced exemplars facilitate language learning more so than massed exemplars in young children (e.g., Ambridge, Theakston, Lieven, & Tomasello, 2006; Riches et al., 2005; Vlach et al., 2008). One commonality among these studies is that exposures to the target forms were limited (10–18 exposures) and provided over relatively

few sessions (one to 10 sessions). These dose and treatment intensities are in stark contrast to the current study in which approximately 600 exemplars were presented over 25 days. This difference suggests that the enhanced effect of distributed exposures may be more pronounced when the total number of exposures and sessions are low.

There have been a small number of studies that have examined word learning, rather than morpheme learning, using massed and spaced dose delivery schedules. Childers and Tomasello (2002) demonstrated a production advantage for newly learned words by very young typically developing children (mean age = 2.5 years) who were given distributed exposures. This finding did not extend to grammar learning in children with SLI in the present study, who were approximately 2 to 3 years older. However, children with SLI were included in a later study by Riches et al. (2005) that studied massed versus spaced presentations of novel verbs to children with SLI. These children had a mean age very close to that of children in present study (5;6 versus 5;4). The outcomes of that study were mixed. The younger, typically developing children showed no differential effect for massed versus spaced presentations. The children with SLI showed an effect of distributed learning only on the production aspect of the task—the more difficult component of word learning compared with recognition or comprehension. This suggests that if the children in the present study had not performed so well overall, an effect of spacing may have emerged. However, word learning differs from grammatical learning in potentially important ways. Either or both factors could explain why no dose schedule effect was found for the present study, even when it has sometimes been found in word learning studies.

The current study is the only known experimental manipulation of dose schedule, while holding dose and cumulative intervention intensity constant, for the treatment of morphosyntax in children with SLI. Morphosyntax treatment was also studied by Smith-Lock et al. (2013b). Children with SLI received treatment for 1 hr per week for 8 weeks (spaced) or treatment for 1 hr per day for 8 days (massed). They reported greater improvement on their grammar elicitation task (Smith-Lock et al., 2013a) in the spaced condition. Although the cumulative intervention intensity in terms of treatment minutes was consistent for spaced and massed groups, the treatment durations varied substantially, and the timing of performance measurement were highly discrepant between groups. Furthermore, dose numbers were not measured and may not have been equivalent across groups. Therefore, some caution is warranted in terms of whether this study reflects simply the effect of massed versus spaced treatment rather than the differential contribution of other treatment parameters (cf. Warren et al., 2007).

Conclusion

This study involving a group of 4- and 5-year-old children with SLI provided a replication of the robust effects obtained by providing Enhanced Conversational

Recast treatment provided daily for 5 weeks (Plante et al., 2014). In addition to a replication of the treatment efficacy of Enhanced Conversational Recast, this study demonstrated a method to investigate a particular dosage component and adds to the evidence base by reporting the very low and nonsignificant effect size associated with massed and spaced treatment group differences for children with SLI. Given these two findings, it would be reasonable to expect that other children this age with SLI would respond in a similar way as the children in this study to Enhanced Conversational Recast, delivered in either massed or spaced doses. This provides flexibility for clinicians to schedule treatment sessions either massed or distributed throughout the day for children with SLI. Although daily 30-min individual treatments may be difficult to achieve in some environments, or 18 hr of treatment for a single grammatical morpheme may not be feasible for some, the results of this study may support the treatment choices of speech-language pathologists who provide services to children with SLI. Furthermore, the findings of this study may inform other treatment researchers who are interested in the role of dose schedule in intervention programs and promote the investigation of dose schedule in other clinical contexts and across different populations.

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