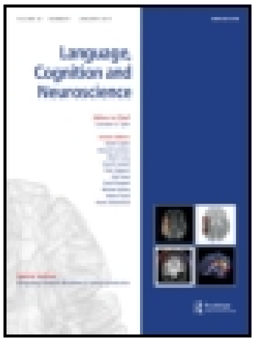

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Nativeness, social distance and structural convergence in dialogue

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ABSTRACT
This study extends the logic of prior studies showing phonetic convergence between interlocutors to the structural domain. We ask whether listeners' adaptation of the syntactic forms they produce depends on their perceptions about their interlocutor's social proximity and linguistic competence, using structural priming as a measure of convergence. Two experiments compared structural priming in dialogues between native British English speakers and (i) other native British English speakers, (ii) native speakers of North American English, and (iii) non-native speakers of English, to assess to what extent interlocutor characteristics influence structural convergence in dialogue. Our findings suggest that rates of structural convergence depend both on a speaker's pre-existing structural biases for particular verbs, and their perception of (linguistic or social) similarity to their interlocutor. This suggests that low-level mechanisms underlying structural convergence may be mediated by beliefs about how interlocutors are socially situated with respect to each other.

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KEYWORDS
Structural priming; dialogue; non-native speakers; sentence production

1. Introduction

Substantial work exists on how second language (L2) speakers adapt in a first language (L1) community (L2 acquisition: Abrahamsson & Hyltenstam, 2009; Hopp, 2010; White & Genesee, 1996) and how speakers' L1 can change after prolonged exposure to a different language community (L1 attrition: Bergmann et al., 2016; Chamorro et al., 2016; Tsimpli et al., 2004). In addition, evidence from phonetic imitation studies has shown that listeners adapt their speech to be more similar to that of a speaker they have prior exposure to: producing single words in a word-shadowing task (Goldinger, 1997, 1998; Namy et al., 2002), phonetic convergence in interactive conversation (Pardo, 2006), perceptual adaptation to speakers with different phonemic contrasts (Kraljic & Samuel, 2007). Furthermore, the extent of convergence appears to be modulated by listeners' perceptions of speaker characteristics such as the attractiveness of their voice, or the typicality of their accent (Babel & McGuire, 2015). Both social and cognitive explanations have been proposed: convergence could be motivated by the listener wanting to increase their similarity to an "in-group" or socially well-positioned individual (Babel, 2010, 2012), or by automatic processes that detect speech characteristics like typicality or distinctiveness (Kim et al., 2011). The current study extends the logic of this work to the structural domain, asking whether listeners' adaptation of syntactic forms they produce depends on their perceptions about their interlocutor's social proximity and linguistic competence.

We use structural priming (e.g. Bock, 1986; Bock & Loebell, 1990; Ferreira et al., 2008; Pickering & Branigan, 1998) as a measure of listeners' convergence with their interlocutor. Structural priming typically involves syntactic alternations, such as the dative alternation in (1) or the passive alternation in (2).

(1) a. *Double object* form: Hermione wrote Ron a letter.
b. *Prepositional dative* form: Hermione wrote a letter to Ron.
(2) a. *Active* form: Hermione baked the cake.
b. *Passive* form: The cake was baked by Hermione.

Purely descriptively, structural priming occurs when the use (production or comprehension) of one variant of an alternation increases the likelihood that the same variant will be used subsequently, in a situation that calls for that argument structure. For example, if a speaker describes a ditransitive event by producing the sentence in (1a), they are more likely to describe a subsequent ditransitive event by producing a sentence using the double object variant, like *Harry loaned Luna some money*, relative to describing the same event using a prepositional dative structure, as in *Harry loaned some money to Luna*.

Structural priming has been a useful tool for probing abstract syntactic representations in large part due to its

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implicit nature (Bock & Griffin, 2000), which has led some researchers to liken it to implicit procedural learning (Chang et al., 2000, 2006). We also know from priming studies with bilinguals that cross-linguistic priming occurs with structures that are similar across languages, which suggests that those constructions share a single integrated representation (Bernolet et al., 2007; Hartsuiker et al., 2004). The current study asks whether such implicit effects are modulated by a speaker's awareness about their interlocutor's linguistic competence. We focus on conversations between L1 and L2 speakers, where the latter has incomplete linguistic competence, and may be influenced by their L1.

1.1. Reasons for convergence or divergence in dialogue

Why might native speakers exhibit different linguistic behaviour – greater or reduced convergence – when speaking to a non-native speaker, compared to another native speaker? We consider three hypotheses that differ in the cognitive mechanisms that are assumed to underlie structural convergence.

First, listeners may adapt more to speakers that they perceived to have native competence in the language, which is indicative of their level of certainty with respect to the acceptability of syntactic forms (*Competence* hypothesis). This is supported by Brehm et al. (2018), who show that ungrammaticality is more likely to be interpreted as misperception for typical native speakers than for native speakers with atypical dialects or L2 speakers. Note that we only consider cases where participants have native competence; it is still possible, however, for a participant to perceive native competence in an interlocutor who speaks differently than them, provided they are perceived to be speakers of a different dialect. In such cases, listeners may treat native speakers of different dialects as linguistically competent, but with plausible syntactic differences from their native dialect. Kim et al. (2011) show that same-dialect dyads reveal greater phonetic convergence than both different-dialect and different-L1 dyads. The same pattern at the syntactic level would suggest that listeners assess not only nativeness, but also the plausibility of a speaker having a different grammar (as with a different dialect or L1). In terms of strength of convergence, the Competence hypothesis predicts greater convergence between pairs of native speakers (whether they speak the same or different dialects).

Second, listeners might tailor their utterances to the perceived needs of their interlocutors (*Communicative Design* hypothesis; from Branigan et al., 2011). If a speaker is perceived as lacking native competence, listeners might go out of their way to make themselves more easily understood by their interlocutor by using structural forms that the interlocutor has previously produced themselves, and therefore can be assumed to be able to interpret. In a study comparing human speakers producing utterances for either another human or a computer, Branigan et al. (2011) found that participants tailored their communications for the "audience" to a greater extent when that audience was a computer, and therefore known to have non-human-like linguistic capabilities. As the authors note, this kind of adaptation differs from typical explanations of structural priming, which invoke implicit cognitive mechanisms. Rather, participants' behaviour resembles lexical entrainment – the tendency to re-use the same referential forms across a dialogue with the same interlocutor. Entrainment is often couched in terms of Conceptual Pacts formed between interlocutors, which may involve explicit reasoning about the appropriateness of an expression for a particular interlocutor. We revisit questions related to underlying mechanisms in the General Discussion. In terms of structural convergence, the Communicative Design hypothesis predicts greater convergence of a native speaker participant with a non-native confederate, due to the perceived communicative needs of the non-native speaker.

Finally, listeners may adapt more to speakers that they perceived to be socially similar to themselves, as indicated by the accent associated with their dialect or non-native status (*Social proximity* hypothesis). Existing work suggests that at least phonetic alignment is sensitive to social signalling pressures (Babel, 2010, 2012). Familiar-sounding speakers have also been shown to be socially preferred (Babel & McGuire, 2015). In this case, greater convergence is predicted when listeners perceive their interlocutor to be more similar to themselves, where similarity is used broadly to mean social, cultural, or linguistic similarity, based on how the speaker sounds.

Note that, while these hypotheses make different predictions about patterns of structural convergence, they have in common that the mechanisms implicated are mediated by a judgment or belief about some aspect of a speaker (such processes need not involve higher-level reasoning, as discussed in Branigan et al., 2011).

1.2. Using structural priming as a measure of convergence

We use structural priming as our primary indicator of convergence because it is well-documented and robust as a behavioural phenomenon: it has been reported for spoken and written sentence production

(Bock, 1986; Bock & Loebell, 1990; Chang et al., 2006; Cleland & Pickering, 2003; Corley & Scheepers, 2002; Griffin & Bock, 2000; Hartsuiker et al., 2008; Pickering & Branigan, 1998) and comprehension (Arai et al., 2007; Bock et al., 2007; Kim et al., 2014; Ledoux et al., 2007; Thothathiri & Snedeker, 2008; Tooley et al., 2009), within the same language and across different languages spoken by bilinguals (Hartsuiker et al., 2004; Loebell & Bock, 2003; Meijer & Fox Tree, 2003; Shin & Christianson, 2009; Weber & Indefrey, 2009), and using different experimental paradigms (picture description, sentence completion, self-paced reading, corpus studies).

There are long-standing debates about the mechanism underlying the observed patterns of language use, with much of the discussion centred around the *lexical boost* effect (stronger structural priming when the prime and target sentences use the same verb), which is well-documented in the structural priming literature (e.g. Cleland & Pickering, 2003; Pickering & Branigan, 1998; Pickering & Traxler, 2004). The lexical boost effect has been used to argue for a priming mechanism driven by residual activation of previously used verbs and subcategorisation frames. While it is not the purpose of this paper to argue for or against existing proposals about mechanism, we note that a number of studies have reported observing both lexical boost effects and long-term persistence of non-verb-specific structural forms, consistent with implicit learning accounts (e.g. Chang et al., 2006; Ferreira et al., 2008), just at different timescales: the verb-specific boost is short-lived, whereas the non-verb-specific priming has a longer duration (Bock & Griffin, 2000; Hartsuiker et al., 2008). Throughout, we measure structural priming in a way that assumes that the overall likelihood of a double object form increases after usage of double object forms *in general* – irrespective of whether the same verb is used, or whether the use of the form occurs by the participant producing or comprehending it. We compare priming in utterances produced in response to pre-recorded native or non-native speakers of British English (Experiment 1), and in live conversations with non-native or native speakers of British or North American English (Experiment 2), to assess to what extent interlocutor characteristics influence convergence or divergence of syntactic forms in dialogue.

Previous structural priming studies have rarely included ungrammatical sentences as primes (though see Ivanova, Pickering, McLean et al., 2012, where ungrammaticality is explicitly manipulated in structural priming involving speakers of the same language). The accounts of structural priming developed on the basis of these studies differ in terms of what they would predict for structurally ill-formed sentences. A residual activation account (Melinger & Dobel, 2005; Pickering & Branigan, 1998; Traxler & Pickering, 2005) links structural priming to specific lexical items; a verb that never appears in double object (DO) form would not be associated (or would only very weakly be associated) with the [NP][NP] configuration, and therefore would not be expected to prime subsequent DO structures – either with the same verb or a different one. Implicit learning accounts (Bernolet & Hartsuiker, 2010; Chang, 2002; Chang et al., 2000, 2006) predict that use of an [NP][NP] form with one verb will prime subsequent DO sentences regardless of lexical overlap. However, it is unclear how ungrammatical inputs would be treated. We return to this issue in the General Discussion.

Our experiments differ in another important respect from a number of other syntactic priming studies, in that we used by-item measure of a verb's likelihood of appearing in a PD or DO form (much like Bernolet & Hartsuiker, 2010). Using an item-specific measure of structural bias rather than a categorical one (e.g. alternating vs. non-alternating) means that we can expect more strongly biased verbs to have larger effects on the dependent measure than weakly biased ones, and we need not commit ourselves to the notion that whether a verb participates in the dative alternation is, in fact, categorical. We are therefore assuming that each verb "starts" from this baseline, and may or may not become more likely to appear in DO form as a function of the variables manipulated in our experiments. We are therefore asking not whether structural priming occurs, but rather whether it is contingent on prior lexical knowledge, the distribution of well-formedness in a conversational context, and various socio-linguistic properties of an interlocutor.

2. Experiment 1: nativeness and ungrammaticality

Building on the existing structural priming literature, Experiment 1 sought to establish a baseline pattern of structural priming in speakers responding to native or non-native speech. Would native British English speakers alter their linguistic behaviour at all when confronted with foreign-accented English? Experiment 1 used pre-recorded stimuli (see Methods for details), whereas Experiment 2 involved live confederates. As an initial step, we focused on two aspects of communication between native and non-native speakers.

First, non-native speakers are likelier than native speakers to produce ungrammatical or anomalous forms. For the dative alternation in English, for example, this may be due to incomplete learning of which lexical items participate in the alternation, or insufficient input to determine appropriate usage of structural variants in different linguistic contexts. Do native speakers show different degrees of convergence with syntactic forms produced by a non-native speaker relative to those produced by another native speaker? According to the *Competence* hypothesis, native speakers should show greater convergence with another native speaker than with a non-native speaker. By contrast, the *Communicative Design* hypothesis predicts native speakers should adapt more to non-native speakers, as a way to make their utterances more comprehensible to the non-native speaker. If native speakers do design their utterances with their interlocutor's comprehension in mind, the greatest convergence might be expected with non-alternating verbs, where a non-native speaker's usage of the DO form would highlight their incomplete knowledge of the language.

A second question we address has to do with generalisation. The classical versions of the residual activation and implicit learning accounts of structural priming do not build in speaker-specific representations; as such, priming from a conversation with one speaker would be expected to carry over to any subsequent language use. However, there is now a good deal of evidence that individuals track usage properties of specific speakers (Brennan & Hanna, 2009; Kleinschmidt & Jaeger, 2015; Pogue et al., 2016; Yildirim et al., 2016). This raises the question of whether speakers generalise the structural usage patterns of one interlocutor to a new interlocutor.

Recent work by Ostrand and Ferreira (2019) shows that, in situations where speaker-specific structural convergence does not facilitate communication beyond speaker-independent convergence, individuals align to aggregate partner-independent statistical distributions. Since speaker-specificity is observed when it does have communicative utility, the authors conclude that the mechanism underlying structural alignment is sensitive to communicative and social factors. In the context of our study, it might be communicatively efficient to "carry over" generalisations about one non-native speaker's usage of syntactic structures to a new speaker, if the new speaker is perceived to be from the same class of speakers (i.e. native speakers of Spanish), and crucially, if the atypical syntactic distributions of the first speaker are attributed to their being a member of this speaker class.

2.1. Method

2.1.1. Participants

Forty native British English speakers recruited from the student population at the University of Kent were paid £8 each to participate. They were assigned at random to one of the four groups (there were 10 participants in each group; see Section 2.1.3.).

2.1.2. Materials

2.1.2.1. Stimuli. The same test materials were used for both experiments. We chose 30 ditransitive verbs from the ones used in Ferreira (1996), and used them to create 30 images depicting a ditransitive event featuring characters from the Harry Potter series (see Appendix for verbs as used in prime sentences; images are available at https://osf.io/sm4ze/). Half of the verbs participated in the dative alternation in English, permitting both DO and prepositional dative (PD) forms, while the other half were only grammatical in the PD form (see Table 1 for example images and sentences).

To establish a baseline for how biased each verb was toward the PD or DO form, we conducted a norming study using the online crowd-sourcing platform Prolific Academic. The 30 images (with the intended verb printed below the image) were shown in randomised order to 20 participants, who typed how they would describe the scene using the verb into a text box. Participants were paid at an average rate of £6 per hour, were required to be native speakers of English, and were restricted to IP addresses in English-speaking countries.[1]

We calculated a *PD-bias* score for each verb by subtracting the proportion of DO forms produced from the proportion of PD forms (see Appendix). Thus, a verb that was equally likely to be produced in either form would have received a score of 0, while a verb that was never produced in DO form would receive a score of 1. In all the analyses presented, we use PD-bias as a measure of the pre-existing lexical bias associated with the verb. However, for ease of reference, we will occasionally refer to strongly PD-biased verbs as *non-alternating* verbs and weakly PD-biased or unbiased verbs as *alternating* verbs in the text.[2] The verbs we initially selected as alternating and non-alternating based on Ferreira (1996) were separated into the same categories based on the norming data.

2.1.2.2. Task. We created a computer-based picture-matching game in order to elicit descriptions of ditransitive events from participants. The task was designed as a dialogue version of the picture-description task used in a number of classical structural priming studies (e.g. Bock, 1986). Participants sat in front of a computer screen and saw a series of pictures like those in Table 1. They were told that their objective was to determine whether each picture they saw matched the picture that the person they were playing the game with was seeing on their screen. In Experiment 1, participants played the game

Table 1. Example experimental items.

	Alternating verb	Non-alternating verb
		
Prepositional dative	*Ron is showing [$_{NP}$ the painting] [$_{PP}$ to Luna].*	*Luna is reporting [$_{NP}$ the broken window] [$_{PP}$ to Hermione].*
Double object	*Ron is showing [$_{NP}$ Luna] [$_{NP}$ the painting].*	**Luna is reporting [$_{NP}$ Hermione] [$_{NP}$ the broken window].*

listening to recordings of the other player's voice. Participants were told that the recordings came from a previous iteration of the game, where the speaker in the recording was responding to another recording as the participant was in the current session. They were instructed to make their descriptions maximally clear to the speaker in the recording, who would have to perform the same task using the participant's recorded descriptions.

On *Describe* trials (cued by a red box outlining the picture), participants had to describe their image using the verb printed at the bottom of the screen; the other player would then reply indicating whether their image matched or did not match the description. On *Respond* trials, the roles were reversed: the other player produced a description of their image, and the participant replied with "Yes/No" followed by a full sentence describing what they saw on their screen. Trials had no response time-limit; a new *Describe* or *Respond* trial started when the participant clicked either "Same picture" or "Different picture" on their screen. *Describe* trials alternated with *Response* trials, as shown in the sample trial sequence in (3):

(3) Describe trial:
- a. Participant: *Ron is showing Luna the painting.*
- b. Recording: *No, Hermione is showing Luna the painting.*

Respond trial:
- c. Recording: *Luna is reporting Hermione the broken window.*
- d. Participant: *Yes, Luna is reporting the broken window to Hermione.*

Describe trial:
- e. Participant: *Harry is describing something to Hermione.*
- f. Recording: *Yes, Harry is describing Hermione something.*

Respond trial:
- g. Recording: Harry is offering Ron coffee.
- h. Participant: *Yes, Harry is offering Ron coffee.*

Note that *Respond* trials were included to ensure participants attended to the descriptions they were hearing (without these trials, participants would not need to listen at all to successfully complete the experiment). In addition, the verification of matching/mismatching pictures provided a plausible collaborative goal for the dialogue.

In all experiments presented here, the other "player" was a confederate (either pre-recorded, as in Experiment 1, or live, as in Experiment 2), who consistently used only DO forms. This decision was made based on a characteristic of Spanish ditransitive sentences (while all the experimental materials were in English, our non-native confederates were native speakers of Spanish and spoke Spanish-accented English). In English, whether a verb participates in the dative alternation is largely an arbitrary lexical property, as illustrated by (4)–(5) (from Ferreira, 1996).

(4) GIVE
- a. The widow gave the car to the church. [PD/theme-recipient]
- b. The widow gave the church the car. [DO/recipient-theme]

(5) DONATE
- a. The widow donated the car to the church. [PD/theme-recipient]
- b. *The widow donated the church the car. [DO/recipient-theme]

In Spanish, however, the inclusion of a preposition before the goal/recipient argument permits virtually any verb to appear with either argument order, as in (6)–(7).

(6) DAR
- a. La viuda dio el coche a la iglesia. [theme-recipient]
- b. La viuda dio a la iglesia el coche. [recipient-theme]

(7) DONAR
- a. La viuda donó el coche a la iglesia. [theme-recipient]
- b. La viuda donó a la iglesia el coche. [recipient-theme]

Because our confederates (either pre-recorded or live) only produced DO sentences, they occasionally produced sentences that were ill-formed in English, when a trial featured a non-alternating verb.

Participants produced a complete description of the image on their screen on every trial (either describing their image initially or responding affirmatively or negatively to the other player), however they were never instructed to "repeat" what the other person had said – only to provide a complete description of their own picture using the relevant verb. The task therefore allowed us to ask whether participants became more likely to produce DO forms, and whether any such priming effect was contingent on the nativeness of the speaker, or on the alternation status of the verb.

2.1.3. Design and procedure

Speaker type (native British English speaker (BrE), non-native Spanish-accented speaker (NN)) and Change type (no speaker change from Block 1 to Block 2, new same-type speaker in Block 2, new different-type speaker in Block 2) were manipulated between subjects (see Table 2). For two of the four groups (C1, C2), the speaker in the recording remained the same across the two blocks of trials. A third group (C3) started in Block 1 with one non-native Spanish-accented speaker, then switched to a different non-native Spanish-accented speaker in Block 2. The fourth group (C4) started with a non-native speaker in Block 1, then switched to a BrE native speaker in Block 2. Both non-native Spanish-accented speakers were Erasmus students from Spain who had recently arrived at the University of Kent with a B2 level of English (i.e. upper-intermediate proficiency) at the time of testing. Their foreign accent was clearly perceivable, as confirmed by the post-test survey results from Experiment 2 (see Section 3.2.1).

Each group used the same list of items, each with a different pseudorandom order. Participants in the same group therefore saw the same item order. Fixed pseudorandomised lists were used to better control the distribution of verbs across trials (e.g. avoiding adjacent trials using the same verb). Each list contained two blocks: Block 1 had 64 trials, comprised of 22 alternating verbs, 22 non-alternating verbs, and 20 fillers; Block 2 had 80 trials, with 30 alternating verbs, 30 non-alternating verbs, and 20 fillers. Each alternating/non-alternating verb with its corresponding picture appeared twice in each block. In addition, 8 new verbs that had not appeared in Block 1 were included in Block 2, each one appearing twice. Filler trials featured intransitive or monotransitive verbs (e.g. "*Hermione fell asleep*", "*Harry is kicking a barrel*"), which are incompatible with the dative alternation. Half of the experimental trials were Describe trials, and half were Respond trials. Participants were instructed to provide full sentences as descriptions.

Table 2. Experiment 1 conditions.

	C1 (No change – BrE)	C2 (No change – NN)	C3 (Change – new NN)	C4 (Change – new native)
Block1 speaker type	BrE	NN	NN1	NN
Block2 speaker type	BrE	NN	NN2	BrE

Participants played the picture-matching game on a lab computer, with headphones to listen to the recordings. Sessions were recorded on a Zoom H4N handheld recorder. Each session took approximately 25 min.

2.1.3.1. *Data coding.* The recordings from the experimental sessions were transcribed, then coded for response type. Responses, which included both Describe and Respond trials, were coded as DO, PD (including sentences with "to", "for" and "from"), or other. "Other" responses included trials where participants failed to use the verb provided, failed to produce a full sentence, skipped a trial accidentally by pressing the spacebar twice, or produced sentences that used the verb provided, but not as the main verb (e.g. "*Luna is making a report about the broken window*"). "Other" responses were excused from analysis; they comprised 3.1% of the data. For the remaining data, we created a binary outcome variable coded as 1 for DO and 0 for PD responses.

2.1.3.2. *Model fitting.* Unaggregated responses from Block 1 and Block 2 were fitted with separate mixed-effects logistic regression models predicting DO responses, with Participant and Item included as random effects. The blocks were analysed separately because the predictors differed by block, as described below. Categorical predictors were sum coded unless specified otherwise, and numerical predictors were centred.

For all models presented, fixed effects were removed from the model using stepwise model comparison if they did not improve model fit or were collinear with other model terms. The random effects structure was determined by beginning with the maximal random effects model, which typically did not converge, then removing terms one by one (starting with higher order ones, and removing Item before Participant random effects) until the model converged (see Barr et al., 2013). Within same-order terms (e.g. two-way interactions), random effects terms that accounted for the least variance were removed first.

2.2. Results

2.2.1. Block 1 results

The Block 1 model included PD-bias (from the norming study), Trial number, Speaker type (BrE, NN), Trial type

(Respond, Describe), and two-way and three-way interactions as predictors. Trial was included to capture any overall changes to DO production rate as a function of how far into the experimental session participants were. If the strength of structural priming is correlated with the surprisal associated with the DO structure, along the lines argued by Jaeger and Snider (2013), we would expect priming to weaken over trials, because the repeated and invariant exposure to DO forms from the pre-recorded speaker would result in even ill-formed DO sentences decreasing in surprisal over the course of the experiment.

Interactions with Trial also capture increases or decreases in the strength of other effects (e.g. lexical bias) as trials progressed. While there were no adjacent trials using the same verb, Respond trials did involve the participant producing a sentence using the same verb as the sentence just heard in the recording. Unlike Describe trials, Respond trials created sequences of sentences sharing a verb, where it would be plausible to see a lexical boost effect. We therefore included Trial type in our model, although we had no specific hypotheses about effects involving Trial type. The final model and coefficient estimates are given in Table 3. The mean proportions of DO productions, aggregated by participant, are shown in Figure 1 for Blocks 1 and 2.

There was a main effect of PD-bias: more strongly PD-biased (i.e. "non-alternating") verbs were less likely to be produced in DO form than weakly PD-biased (i.e. "alternating") verbs.[3] The PD-bias:Trial interaction indicates that the PD-bias effect increased over trials. This suggests that participants became more committed to their prior lexical biases over trials, as they encountered more instances of anomalous sentences (see Table 4 for simple slopes analyses for interactions). A main effect of Trial indicates that DO production rates decreased across trials, consistent with an explanation of structural priming that links strength of priming to surprisal, and ultimately to the minimisation of prediction error (Jaeger & Snider, 2013).

Table 3. Experiment 1, Block 1: Model coefficient estimates.

DO ~ SpeakerType + Trial + PD-bias + TrialType + SpeakerType:PD-bias + SpeakerType:TrialType + PDbias:Trial + (1 + PD-bias + SpeakerType + TrialType|ParticipantID) + (1 + PD-bias + SpeakerType + TrialType|ItemID)

	Estimate	Std. error	*z* value	*p*
(Intercept)	0.71	0.34	2.07	* [a]
SpeakerType	−0.12	0.24	−0.50	*n.s.*
Trial	−0.010	0.0032	−3.12	**
PD-bias	−6.16	1.06	−5.81	***
TrialType	1.98	0.17	11.58	***
SpeakerType:PD-bias	−1.59	0.61	−2.60	**
PD-bias:Trial	−0.047	0.017	−2.78	**
TrialType:Trial	−0.011	0.0033	−3.22	**

[a]Significance codes: '***' $p<0.001$; '**' $p<0.01$; '*' $p<0.05$; '.' $p<0.1$; '*n.s.*' $p>0.1$.

There was also a main effect of Trial type, with more DO sentences produced on Respond trials than on Describe trials. This effect weakened over trials. Recall that on Respond trials, participants first listened to a recorded description of a display, then used the same verb to describe the version of the display on their screen. These trials therefore bear some resemblance to prime-target sequences sharing a verb, where a lexical boost effect would be expected.

An alternative explanation for the Trial type effect is that it is driven by the subset of Respond trials where participants had to indicate that their display differed from the description in the recording. Though participants were never told to repeat the structure of the recorded sentence, it is possible they construed their response as a kind of correction (e.g. "*Luna described Harry the statue.*" … "*No, HARRY described LUNA the statue*" or "*No, RON described Harry the statue*"), where the purpose of the response was to highlight the contrast(s) between the recorded description and their own description of their display. In such a situation, the structural configuration of the verb would be backgrounded, and as a result, might not be as salient to participants, even when used anomalously. In order to rule out this possibility, we included ResponseType (Confirm, Correct) as a fixed effect in the Block 1 model, along with two- and three-way interactions with ResponseType (excluding three-way interactions involving Trial). None of the terms involving ResponseType survived model comparison, so we conclude that the difference between Confirm and Correct responses was not responsible for the Trial type effect.

Strikingly, there was a Speaker type:PD-bias interaction: the decrease in DO production for strongly PD-biased verbs was greater for non-native than for native speakers. In other words, participants produced more *anomalous* DO sentences using strongly PD-biased verbs when they were responding to a native British English speaker's recorded voice compared to when they responding to a non-native speaker's voice.

2.2.2. Block 2 results

Recall that, for two participant groups, the speaker in the Block 1 recordings remained the same in Block 2, while the speaker changed from Block 1 to Block 2 for the other two groups (either from one non-native speaker to another, or from a non-native to a native speaker). The Block 2 model included PD-bias, Trial number, Speaker type (BrE, NN), New speaker (whether the Block 2 speaker was new or the same speaker as in Block 1), Trial type (Respond, Describe), and two-way

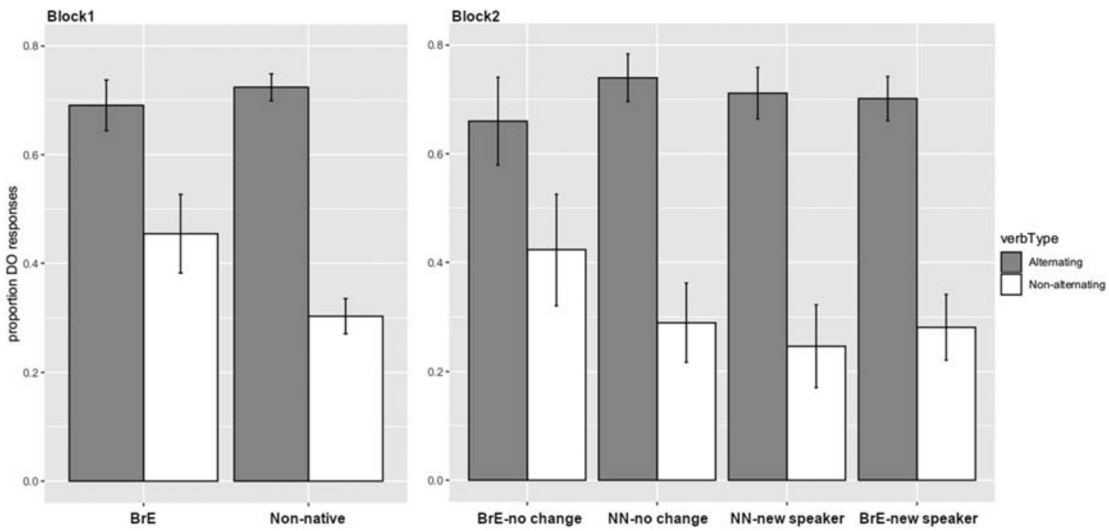

Figure 1. Mean proportions of DO productions by Speaker type (BrE = British English speaker, NN = Non-native speaker), Experiment 1 (left: Block1; right: Block2).

and three-way interactions as predictors. The final model and coefficient estimates are given in Table 5.

The strong negative effect of PD-bias from Block 1 persists in Block 2. As in Block 1, there was a Speaker type:PD-bias interaction: the difference in prime effectiveness by PD-bias was greater when the current (Block 2) recordings featured a non-native than a native speaker (see Table 6 for simple slopes analyses for interactions). In addition, there was an analogous interaction between PD-bias and whether the recordings featured a new speaker: the decreased priming from strongly PD-biased verbs was greater if the Block 2 speaker was a different speaker than in Block 1. In other words, with a new speaker, participants fell back on their prior lexical biases. If we construe participants' willingness to produce DO forms with particular verbs as a measure of their perceived acceptability, this interaction suggests that the exposure to a speaker producing anomalous forms does not influence the overall perceived acceptability of those forms. Rather, such exposure may result in speaker-specific learning.

Table 4. Experiment 1, Block 1: Simple slopes for interactions.

	Estimate	Std. error	*z* value	*p*
PD-bias:SpeakerType − slope$_{PD\text{-}bias}$ for:				
SpeakerType = NN	−7.75	1.32	−5.86	***
SpeakerType = BrE	−4.57	1.11	−4.11	***
PD-bias:Trial − slope$_{PD\text{-}bias}$ for:				
Trial = mean$_{Trial}$ − 1SD	−6.09	1.17	−5.21	***
Trial = mean$_{Trial}$	−6.92	1.15	−6.00	***
Trial = mean$_{Trial}$ + 1SD	−7.80	1.19	−6.57	***
TrialType:Trial − slope$_{Trial}$ for:				
TrialType = Describe	0.00	0.00	0.81	*n.s.*
TrialType = Respond	−0.02	0.00	−30.94	***

As in Block 1, there was a Trial type effect, with more DO forms produced on Respond trials. This effect interacted with whether the recordings featured a new speaker. As with the PD-bias:NewSpeaker interaction described above, this suggests that interlocutors do not necessarily generalise their learning about the usage patterns of an atypical speaker, but instead "re-set" their expectations for a new speaker.

2.3. Discussion

The results of Experiment 1 (Block 1) revealed, unsurprisingly, that native English speaking participants were sensitive to the pre-existing lexical bias associated with particular verbs: the more strongly a verb was biased toward being produced in a PD structure in our norming study, the less likely participants were to produce it in a DO structure. Strikingly, this behaviour differed by the nativeness of the speaker: participants

Table 5. Experiment 1, Block 2: Model coefficient estimates.

DO ~ SpeakerType + PD-bias + Trial + TrialType + NewSpeaker + PD-bias: SpeakerType + PD-bias:NewSpeaker + PD-bias:TrialType + TrialType:Trial + (1|ParticipantID) + (1|ItemID)

	Estimate	Std. Error	*z* value	*p*
(Intercept)	0.37	0.34	1.09	*n.s.*
SpeakerType	−0.028	0.21	−0.13	*n.s.*
PD-bias	−9.08	1.09	−8.30	***
Trial	0.0034	0.0026	1.30	*n.s.*
TrialType	1.66	0.17	9.67	***
NewSpeaker	−0.19	0.30	−0.62	*n.s.*
PD-bias:SpeakerType	−1.69	0.62	−2.71	**
PD-bias:TrialType	0.55	0.53	1.03	*n.s.*
PD-bias:NewSpeaker	−1.46	0.62	−2.36	*
TrialType:Trial	−0.0030	0.0025	−1.17	*n.s.*
TrialType:NewSpeaker	0.31	0.15	2.03	*

Table 6. Experiment 1, Block 2: Simple slopes for interactions.

	Estimate	Std. error	z value	p
SpeakerType:PD-bias – slope$_{PD\text{-}bias}$ for:				
SpeakerType = NN	−10.80	1.36	−7.92	***
SpeakerType = BrE	−7.41	1.14	−6.49	***
PD-bias:NewSpeaker – slope$_{PD\text{-}bias}$ for:				
NewSpeaker = New	−10.54	1.27	−8.31	***
NewSpeaker = Old	−7.64	1.24	−6.14	***
TrialType:NewSpeaker – slope$_{TrialType}$ for:				
NewSpeaker = New	1.97	0.23	8.53	***
NewSpeaker = Old	1.35	0.23	5.89	***

who heard the recorded voice of a native speaker were likelier to produce anomalous DO forms with strongly PD-biased verbs, compared to participants who heard a non-native (Spanish-accented) speaker.

Returning to the hypotheses described in Section 1.1, the data from Experiment 1 appears to be inconsistent with the Communicative Design hypothesis, where speakers (either consciously or without awareness) match aspects of their utterances to more closely resemble the productions of interlocutors who might have specific communicative needs – in this case, incomplete competence in the language being used. Such behaviour would lead to stronger convergence (structural or otherwise) for non-native than for native speaker interlocutors, but we observe the opposite pattern in Experiment 1, with greater convergence for native than non-native speakers.

However, before ruling out a Communicative Design explanation, we want to consider a version of the Competence hypothesis that might explain why participants behave differently with native and non-native speaker recordings in Experiment 1. A simple competence-based strategy would be to assume that sentences produced by native speakers are always fully grammatical (the speaker is always "right"), whereas sentences produced by non-native speakers are only grammatical when they agree with the participant (the participant is always "right"). Such an explanation could have nothing at all to do with increasing the likelihood of communicative success with a specific interlocutor. To rule out this possibility, and give the Communicative Design hypothesis a truer-to-life test, we carried out Experiment 2, which involved live, interactive conversation instead of pre-recorded speech. Using live confederates created a situation where communicative success or failure had potential real world consequences (e.g. failure could lead to more clarifications and requests for repeated utterances, which would increase how long it took to complete the experimental session, and result in the participant making less money per unit time).

In addition to using live confederates, Experiment 2 tested a prediction of the Competence hypothesis: that participants should show comparable rates of convergence with native speakers of different varieties of English, as long as the speakers are perceived as having native competence.

3. Experiment 2: structural convergence in interactive dialogue

Experiment 2 used three types of English speakers: native speakers of British English (the same dialect spoken by participants), native speakers of North American English (a different recognisable dialect of English), and non-native Spanish-accented speakers. The North American speaker was included as an intermediate test case between the native and non-native speaker conditions in Experiment 1. If our participants in Experiment 1 were modulating how much they adapted to the speakers in the recordings based on their nativeness, the North American speaker in Experiment 2 should pattern like the British speaker in Experiment 1, since both speaker types are recognisable as having native competence in English (Competence hypothesis). The explanation for the SpeakerType:PD-bias interaction might then involve participants treating sentences produced by a native speaker as though they were well-formed even though they are syntactically anomalous, given the speaker's level of competence. By contrast, a non-native speaker producing anomalous sentences is easily explained by the speaker's incomplete competence.

Alternatively, if participants are sensitive to the native competence of their interlocutor but also take into account that different dialects might permit different syntactic forms, they might treat the North American speaker like the non-native speaker in Experiment 1 – someone whose grammar of English has a plausible reason to be different from their own. In broader terms, the *Social proximity* hypothesis predicts that native speakers' perception of similarity with their interlocutor will determine how strongly they adapt to them. This perception of (dis)similarity could be in direct linguistic terms, or socio-culturally, as indirectly signalled by an interlocutor's linguistic behaviour.

In addition to the Speaker type manipulation, Experiment 2 used live confederates instead of pre-recorded speech, as in Experiment 1. This change was motivated by participants' behaviour in Experiment 1, which did not reveal the standard lexical boost effect; using an interactive, live dialogue task would allow us to more appropriately compare our results to the many studies where such well-documented effects were observed. In fact, some dialogue studies have suggested that adaptation in dialogue may be dependent on interactive communication (Brown-Schmidt, 2009; Cane et al., 2017). However, Kuhlen and Brennan (2013) argue that the use of confederates in interactive experiments has the potential to yield misleading results. Our Experiment 2 falls into their category of using scripted confederates to produce stimuli that would not naturally occur in an unscripted dialogue.

To make the collaborative task as naturalistic as possible (i.e. conceal the fact of a confederate being used), we ensured that confederates had no knowledge of the hypotheses being tested, that they were instructed to respond to requests for repetition as they would in natural conversation (while still adhering to the wording in the script), and that they conversed freely with participants while coordinating non-experimental parts of the experiment (e.g. asking if the participant was prepared to start the practice trials together, discussing whether they had had a long enough break between experimental blocks). Because confederates were not visible to participants, their gestures or facial expressions were not potential sources of information that might have revealed the confederate's status to the participant. Using confederates in this way provided a naturalistic approximation to interactive dialogue, which we are ultimately trying to model. A methodological question, then, is whether we can replicate the main findings from Block 1 of Experiment 1 using live confederates instead of pre-recorded speech.

3.1. Method

3.1.1. Participants

Forty-eight native British English speakers recruited from the student population at the University of Kent were paid £10 each for participating. They were assigned at random to one of the three groups (there were 16 participants in each group; see Section 3.1.3).

3.1.2. Materials

The same materials were used as in Experiment 1. The picture-matching game was adapted to be played with a live confederate instead of a pre-recorded voice.

Participants and confederates communicated over headsets from adjacent testing rooms, where they were not able to see each other. However, participants had evidence that the confederates were live as they could hear the experimenter "setting up" the confederate's experiment in the adjacent room. Participants would have had the impression that the experimenter was giving the same introductory instructions to the confederate as they had just heard themselves (informed consent, instructions to read on-screen instructions and do practice trials). In fact, the experimenter would have been confirming the condition/script for the current participant. The doors to the two testing rooms remained closed except to allow the experimenter to briefly enter or exit, so the experimenter's speech was not audible from the other room.

The participant and the confederate also conversed freely in order to coordinate doing the practice trials together after reading the instructions, verify that they were both ready to start the game, and determine when to re-start the game after the break between Blocks 1 and 2.

3.1.3. Design and procedure

There were three Speaker types, manipulated between subjects: native British English (BrE), native North American English (NorthAm), and non-native Spanish-accented (NN) speakers. The BrE and NN confederates were the same as in Experiment 1. Confederates used a script for their turns in the game. As for Experiment 1, all the sentences produced by the confederates had a DO structure, creating anomalous sentences for PD-biased (non-alternating) verbs.

To assess participants' perception of their interlocutor's language use, they were asked two post-test questions: (1) *Where do you think the speaker was from?* (response options: UK; North America; Europe; elsewhere), and (2) *How would you describe their usage of English?* (response options: Spoke pretty similarly to me; Knew English well but said some things differently than I would; Didn't know English well).

Experimental sessions took approximately 45 min each, and were recorded on the participant's computer.

3.1.3.1. Statistical power. The effects of greatest interest are those involving Speaker type. While we expect to find a contrast between BrE and NN speakers, as in Experiment 1, the contrast between BrE and NorthAm speakers is novel to Experiment 2, in addition to being a between-subjects manipulation. We therefore assessed statistical power for a main effect of Speaker type and interactions involving Speaker type, based on a number of assumptions about the final model. We were particularly interested in interactions involving the novel Speaker type contrast because the interaction between Speaker type and lexical bias was reliable in Experiment 1, even with a non-significant main effect of Speaker type.

We estimated effect sizes for the variables of interest, and simulated outcomes using the lme4 (Bates et al., 2015) and simr (Green & MacLeod, 2016) packages, based on a logistic regression model including the following fixed effects: PD-bias, Speaker type (Helmert-coded: contrast 1=BrE v. NorthAm, contrast 2={BrE, NorthAm} v. NN), Trial type (Respond, Describe), Trial, two-way interactions of Speaker type:PD-bias, PD-bias: Trial, and Trial type:Trial, and three-way interactions of Speaker type:PD-bias:Trial and Speaker type:Trial type: Trial. For predictors that appeared in the Experiment 1, Block 1 model (see Table 3), we assumed the same effect sizes. For the new predictors – Speaker type (contrast 1=BrE v. NorthAm) and its interactions – we assumed small effect sizes (Cohen's d = 0.2 is equivalent to log odds = 0.36; Borenstein et al., 2009).

To simulate the data, we assumed 16 participants per Speaker type group (BrE, NorthAm, NN), with 6 repetitions each of 30 items per participant. Because the same stimulus items were used in Experiment 2 as in Experiment 1, we used actual centred PD-bias values in the simulations. Of the 180 trials seen by each participant, half were Describe trials and half were Respond trials.

Response vectors were simulated based on the fixed effects structure specified above. We followed the procedure for simulating a random effects structure in Bates et al. (2015), including random intercepts and slopes for Speaker type, PD-bias, and Trial type, for both Participants and Items (models with more complex random effects structures did not converge for Experiment 1). We calculated the power to detect a main effect of Speaker type (contrast 1=BrE v. NorthAm) as 10%, a Speaker type (contrast 1):PD-bias interaction as 70%, and three-way interactions of Speaker type (contrast 1):PD-bias:Trial and Speaker type (contrast 1):Trial type:Trial as 99%. While Experiment 2 may therefore be underpowered for detecting a main effect of Speaker type (contrast 1), it should be possible to detect reliable interactions involving Speaker type, either as an interaction with lexical bias, as observed in Experiment 1, or in a higher order interaction reflecting change in behaviour across trials.

3.2. Results

3.2.1. Post-test survey

The responses to the post-test questions are shown in Figure 2. As their perception of the speaker's origin became more distant (post-test question 1), listeners became less likely to respond that the speaker spoke similarly to them (post-test question 2; UK: 51.7%; NorthAm: 16.0%; Europe: 3.0%). Listeners were also less likely to indicate that the speaker used language differently from themselves (post-test question 2) when the speaker was perceived to be from the UK (UK: 48.3%; NorthAm: 84.0%; Europe: 93.9%).

Because confederates used the same scripts in the experiment, the differences in perceived linguistic competence could not have been based on confederates' use of anomalous forms alone; rather, listeners must have based their judgments on the confederates' accent, perhaps together with their production of anomalous sentences.

3.2.2. Priming results

The recordings were transcribed and coded as described for Experiment 1. The data was fit with a mixed-effects logistic regression model predicting DO responses. The

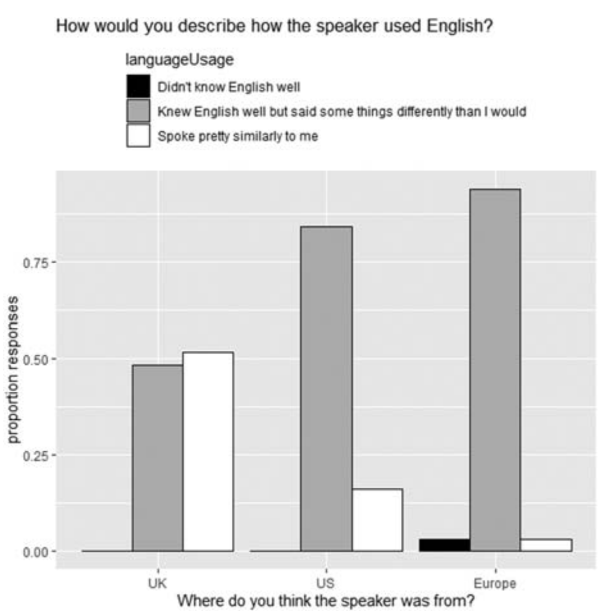

Figure 2. Listeners' perceptions of speaker competence by perceived region of origin.

Table 7. Experiment 2, Blocks 1–2: Model coefficient estimates.

DO ~ SpeakerType + PD-bias + TrialType + Trial + SpeakerType:PD-bias + SpeakerType:TrialType + SpeakerType:Trial + PD-bias:TrialType + PD-bias:Trial + TrialType:Trial + SpeakerType:TrialType:Trial + (1|ParticipantID) + (1||ItemID)

	Estimate	Std. Error	*z* value	*p*
(Intercept)	0.45	0.30	1.51	.
SpeakerType[1:NAm-BrE]	−0.26	0.21	−1.24	*n.s.*
SpeakerType[2:NN-Native]	−0.25	0.12	−2.08	*
PD-bias	−6.03	1.29	−4.67	***
TrialType	1.56	0.038	40.64	***
Trial	−2.85e-03	8.30e-04	−3.45	***
PD-bias:SpeakerType[1:NAm-BrE]	0.029	0.22	0.13	*n.s.*
PD-bias:SpeakerType[2:NN-Native]	−0.30	0.13	−2.20	*
TrialType:SpeakerType[1:NAm-BrE]	−0.032	0.044	−0.73	*n.s.*
TrialType:SpeakerType[2:NN-Native]	−0.064	0.025	−2.56	*
SpeakerType[1:NAm-BrE]:Trial	6.85e-04	9.26e-04	0.74	*n.s.*
SpeakerType[2:NN-Native]:Trial	−1.24e-03	5.30e-04	−2.34	*
PD-bias:TrialType	0.35	0.20	1.79	.
PD-bias:Trial	−2.12e-03	4.42e-03	−0.46	*n.s.*
TrialType:Trial	−3.68e-03	8.00e-04	−4.60	***
SpeakerType[1:NAm-BrE]:TrialType:Trial	−2.37e-03	9.27e-04	−2.56	*
SpeakerType[2:NN-BrE]:TrialType:Trial	−7.57e-05	5.28e-04	−0.14	*n.s.*

full model included the following fixed effects: Speaker type (treatment coded with BrE as the baseline: contrast 1=NorthAm vs. BrE; contrast 2=NN vs. BrE), PD-bias (from norming study), Trial type (Respond, Describe), Trial, and two-way and three-way interactions. Predictors were removed from the model if they did not improve model fit, beginning with higher order terms. The random effects structure was determined using the procedure described for Experiment 1. The final model and model coefficient estimates are given in Table 7. The mean proportions of DO productions, aggregated by participant, are shown in Figure 3 (Blocks 1 and 2 were pooled because there were no speaker changes between blocks, as in Experiment 1).

As in Experiment 1, there was a main effect of PD-bias, such that strongly PD-biased (non-alternating) verbs were less likely to be produced in DO form. It is possible that participants' behaviour was to some extent influenced by the confederate consistently using DO forms and, consequently, misaligning on a number of trials. This could conceivably lead to participants coming to associate strongly PD-biased verbs – which they would likely produce in PD form – with misalignment. However, note that the PD-bias effect also appeared in Experiment 1, where participants knew that the recorded speaker could not be responding (i.e. aligning or misaligning) to the sentences they were producing. In addition, participants often produced PD forms even for alternating verbs, where the DO form would have been acceptable.

Again as in Experiment 1, there was a main effect of Trial type, with more DO productions on Respond trials than on Describe trials. This effect weakens over trials, along with a general decrease in DO production over trials, as in Experiment 1. (Table 8).

Consistent with Experiment 1, more DO forms were produced with a native English speaker (BrE or NorthAm) than with a NN speaker (Speaker type contrast 2: NN v. BrE/NorthAm). This Speaker type effect interacted with PD-bias, such that the negative effect of strongly PD-biased verbs on DO production was stronger with the NN speaker than the BrE or NorthAm speakers. Speaker type also interacted with Trial type: the increase in DO production on Respond trials was greater with a native speaker than a NN speaker.

Finally, there was a reliable three-way interaction of Speaker type (contrast 1=BrE v. NorthAm), Trial type and Trial. While the effect of Trial type remained unchanged across trials with the BrE speaker, with the NorthAm speaker, the Trial type effect weakened: despite initially showing a similar DO boost on Respond trials as for the BrE speaker, NorthAm participants became less likely to produce DO sentences on Respond trials over trials.

3.3. Discussion

The live confederate paradigm we used in Experiment 2 reproduced the contrast between native British English speakers and non-native speakers observed in Experiment 1: participants were more willing to produce anomalous DO structures with a native interlocutor than with a non-native one. We can therefore rule out

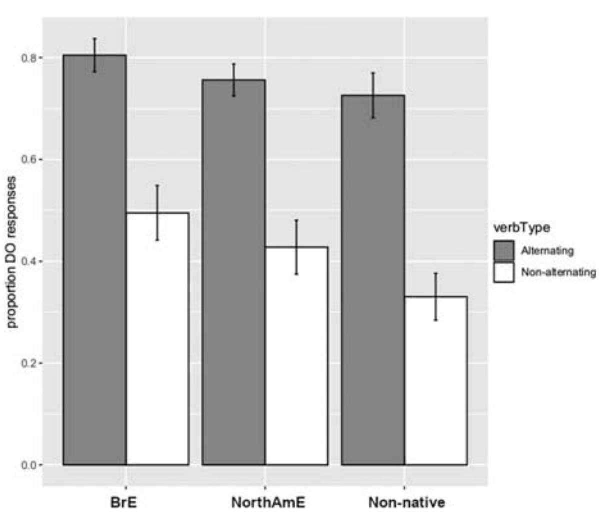

Figure 3. Mean proportions of DO productions by Speaker type (BrE = British English speaker, NorthAmE = North American English speaker, NN = Non-native speaker), Experiment 2 (pooled responses from both blocks).

Table 8. Experiment 2, Blocks 1–2: Simple slopes for interactions.

	Estimate	Std. error	*z* value	*p*
PD-bias:SpeakerType[2:NN-Native] – slope$_{PD\text{-}bias}$ for:				
SpeakerType = NN	−7.08	1.36	−5.22	***
SpeakerType = NorthAm	−5.89	1.34	−4.39	***
SpeakerType = BrE	−6.00	1.34	−4.46	***
TrialType:SpeakerType[2:NN-Native] – slope$_{TrialType}$ for:				
SpeakerType = NN	1.50	0.19	8.01	***
SpeakerType = NorthAm	1.95	0.20	9.92	***
SpeakerType = BrE	1.99	0.20	10.01	***
SpeakerType[2:NN-Native]:Trial – slope$_{Trial}$ for:				
SpeakerType = NN	−0.01	0.00	−4.79	***
SpeakerType = NorthAm	−0.00	0.00	−1.44	*n.s.*
SpeakerType = BrE	−5.79	1.32	−1.95	.
TrialType:Trial – slope$_{Trial}$ for:				
TrialType = Respond	−0.00	0.00	−0.68	*n.s.*
TrialType = Describe	−0.00	0.00	−1.92	.
TrialType:SpeakerType[1:NAm-BrE]:Trial:				
while SpeakerType = BrE, slope$_{TrialType}$ for:				
Trial = mean$_{Trial}$ − 1SD	2.06	0.21	9.84	***
Trial = mean$_{Trial}$	1.99	0.20	10.01	***
Trial = mean$_{Trial}$ + 1SD	1.91	0.21	9.30	***
while SpeakerType = NorthAm, slope$_{TrialType}$ for:				
Trial = mean$_{Trial}$ − 1SD	2.24	0.21	10.77	***
Trial = mean$_{Trial}$	1.95	0.20	9.92	***
Trial = mean$_{Trial}$ + 1SD	1.67	0.20	8.26	***

a version of the Communicative Design hypothesis where participants tailor their utterances to accommodate the perceived needs of their interlocutor (in this case, produce more anomalous sentences resembling those produced by the non-native speaker in order to facilitate communication). This speaker type effect (contrast 2: non-native vs. native) appears as both a main effect and an interaction with PD-bias in Experiment 2, in contrast to Experiment 1, where only the interaction with PD-bias was reliable (i.e. an increase in DO forms with native interlocutors only with non-alternating verbs). This difference may be related to the methodological difference between Experiments 1 and 2. We suspect there is an across-the-board relationship between perception of similarity and convergence, which is emphasised for more strongly PD-biased (non-alternating) verbs. The greater overall priming in the interactive dialogue paradigm results in this effect appearing as a main effect in Experiment 2, and emerging as an interaction (only for verbs with stronger PD-bias) in Experiment 1, where there were numerically more DO forms produced for native relative to non-native speakers overall.

There was a significant interaction of Speaker type (contrast 1: NorthAm vs. BrE), Trial type, and Trial: across the experimental session, participants were consistently more likely to produce sentences matching their BrE interlocutor. By contrast, this "boost" for Respond trials declined as trials progressed with a NorthAm interlocutor. This over-trials effect points to the possibility that a speaker's perception of degrees of similarity/difference with their interlocutor might influence how long adaptations like convergence last.

It remains an open question whether such Speaker type effects code a categorical difference based on native competence, or a graded, finer-grained measure of interpersonal similarity – whether that similarity is purely linguistic or also socio-cultural. Our current findings are compatible with either the Competence hypothesis or the Social proximity hypothesis. Note that neither alternative requires speaker awareness: while the basic idea is that some feature perceived by a speaker about their interlocutor mediates the strength of adaptation, this need not involve explicit reasoning (see Branigan et al., 2011, for an argument along these lines). While these two hypotheses differ in the kind of explanation they offer for convergence in dialogue, they predict very similar outcomes in the conditions examined in Experiment 2: both predict more convergence with native speakers (due to either perceived competence, or perceived social similarity) than with non-native speakers. In fact, nothing about either hypothesis precludes the other: both native-level competence and social distance could well be part of the information speakers track about their interlocutors.

4. General Discussion

The motivation for this study was to better understand how native speakers of a language are influenced by dialogue with non-native speakers, compared to well-documented adaptation effects in dialogue between native speakers. Using structural priming as a measure of convergence, we observed across two experiments that native British English speakers showed greater convergence with interlocutors who were more similar to themselves. This speaker-dependent effect appeared as an interaction of speaker type with verb bias in Experiment 1, which used pre-recorded speech: there was greater convergence with native British English speakers than with non-native speakers for DO forms using verbs that do not participate in the dative alternation in English, with participants producing anomalous sentences like *Harry is donating the school some toys*. In Experiment 2, where participants interacted with live confederates in real time, we observed main effects of Speaker type, with greater convergence with interlocutors perceived by participants to be more similar to themselves. We suggest that the interactive dialogue paradigm yielded overall greater structural convergence than the pre-recorded stimuli, resulting in the same underlying speaker-dependent effects appearing as main effects driven by more strongly PD-biased verbs in Experiment 2 (but see Schoot et al., 2019; and Ivanova et al., 2020, who suggest that the advantages of interactive dialogue versus non-communicative paradigms may be smaller than previously thought).

While our findings are compatible with both competence and social proximity driving structural convergence, Experiment 2 showed that participants distinguish speakers of British English from speakers of North American English, suggesting that the effect of speaker type on convergence is based on something finer-grained than a binary distinction between native and non-native. This in turn suggests an explanation along the lines of participants becoming less certain about their own well-formedness judgments because they assume a native speaker will have high confidence about *their* judgments: in other words, "If this native English speaker is producing *Harry is donating the school some toys* as though it sounds perfectly normal, when they could have said *Harry donated some toys to the school*, perhaps I am wrong about how anomalous *Harry is donating the school some toys* sounds".

A possible alternative explanation is proposed by Heyselaar and Segaert (2019); we refer to this as the

Resource-sharing hypothesis. Based on a dual-task experiment, they argue that at least part of the language processing involved in structural priming is domain-general, and the strength of structural priming trades off with the cognitive demands associated with the secondary task (in that study, motion-object tracking). If domain-general attentional resources are needed for strong structural priming, and conversing with a non-native speaker also draws on the same resource pool, it would be plausible for the strength of priming to diminish in dialogues between native and non-native speakers. In other words, the less similar/more different the interlocutor's speech is, the more distracting it is for the participant, which leaves less attentional capacity to focus on their syntactic preferences, leading to less priming effects when interacting with a non-native speaker. Because the Resource-sharing hypothesis would link the (dis)similarity between interlocutors' speech directly to attentional resources available, and associates allocated attention with extent of convergence, it makes the same empirical predictions as our Social Proximity hypothesis. The results we present here do not directly rule out either explanation, however, it would in principle be possible to tease them apart. Even if we suppose that non-native speakers would generally incur more attentional cost than a native speaker of a different variety, it should be possible to compare this situation to one where the native speaker is more distant from a participant than the non-native speaker, on any number of non-linguitic socio-cultural dimensions (e.g. shared background, socio-economic context, interests). In such circumstances, the Resource-sharing hypothesis would predict greater convergence with the native speaker of a different variety, while the Social proximity hypothesis would predict greater convergence with the non-native speaker.

Our results superficially appear inconsistent with existing accounts of structural priming for ungrammatical – or merely highly infrequent – input. For example, Ivanova, Pickering, Branigan et al. (2012) demonstrated structural priming from morphologically ill-formed prime sentences, including a lexical boost effect of comparable magnitude to that observed with well-formed primes. However, the ways in which our studies evaluated the effect of lexical repetition differed in important ways. For instance, in one experiment, the authors showed that intransitive verbs used in ditransitive sentences (e.g. *The waitress exists the book to the monk/exists the monk the book*) are effective as structural primes. These are unlike our strong PD-bias items: while [*exists NP NP*] is ill-formed, it should have no preference for either ditransitive frame since it typically does not appear in ditransitive sentences at all. By contrast, [*donate NP NP*] is particularly disfavoured for production because it virtually always appears in the alternative [*donate NP PP*] form.

In this context, it seems striking that strongly PD-biased verbs would ever be produced in DO form, and the strong PD-bias effect seen in the two experiments presented here confirms that speakers do for the most part rely heavily on their own prior lexical knowledge about which syntactic frames are acceptable for which verbs. Looking across both experiments, we note that effects of Speaker type and Trial type, when they change over trial, appear to weaken as participants had more exposure to ill-formed sentences from either the recorded speaker or the live confederate. By contrast, the influence of pre-existing lexical bias (PD-bias) strengthens over trials in Experiment 1. This overall pattern suggests that, when faced with an interlocutor with atypical usage patterns, speakers readily fall back on their lexical knowledge, which is based on their cumulative experience of the usage patterns associated with specific verbs.[4] The interesting part is that this tendency varies by how the speaker perceives their interlocutor: when the interlocutor was either non-native or a speaker of a different dialect, speakers seemed more inclined not to adapt to their usage patterns, perhaps attributing those patterns to idiosyncrasies of a particular individual.

The fact that participants were reliably more likely to produce anomalous DO forms with fellow BrE speakers suggests that even strong lexical bias effects can be overcome in some circumstances. Indeed, Fraundorf and Jaeger (2016) showed that speakers generalise from a newly learned structure (i.e. the "needs" + past participle construction) from an unfamiliar dialect, to novel related forms. The authors suggest that adaptation to new constructions involves implicit learning about the distribution of syntactic structures. As Ivanova, Pickering, Branigan et al. (2012) conclude, a residual-activation-style model of structural priming could account for their findings, and ours, provided that syntactic form is represented in some abstract way – that is, not entirely contained within individual lexical entries.

Returning to the phonetic adaptation studies discussed in the Introduction, we might ask what the utility is of adaptation at the structural level. Adapting to a speaker's phonemic boundaries is likely to lead to better perceptual discrimination, and decrease the likelihood of miscommunication. Is there a comparable way that structural adaptation facilitates communication? One possibility is that there is a general preference for parallelism (see e.g. Chambers & Smyth, 1998).

Word order differences are often used to convey information about discourse/information status. It could be that communication is generally less effortful the more ways interlocutors' utterances are parallel to each other (in addition to structural parallelism: parallel mapping of thematic roles to surface positions, parallel information structure; but see Healey et al., 2014, who found structural divergence in ordinary conversation). That would seem like a bias built into discourse processing (i.e. automatic). What our findings suggest is that beliefs about group membership, linguistic competence, and perhaps utility moderate low-level priming effects. This converges with Weatherholtz et al. (2014)'s findings, where the degree of convergence between interlocutors was shown to be influenced by social factors (including perceived social similarity), and Ostrand and Ferreira (2019)'s demonstration that communicative utility mediates structural priming, which also suggests there must be space in the mechanism underlying structural convergence for social meaning.

Notes

1. Of the 20 participants, 17 had IP addresses in the UK, and 3 had IP addresses in the US. Because it is plausible that UK and US varieties of English differ in terms of structural preferences (see e.g. Gries, 2005), we inspected the responses for any marked differences by home country. The US-based participants' judgements fell within 1.5 SD from the mean for 26 of the 30 verbs, and within 2 SD from the mean for the remaining 4 verbs.
2. For reference, in our materials, PD-bias ranged from −0.096 to 0.60 (mean=0.21, median=0.15, SD=0.20).
3. The inverse preference effect (Jaeger & Snider, 2013; Bernolet & Hartsuiker, 2010) is observed when a less frequent structure in an alternation (e.g. passive voice in the passive alternation) has a larger impact as a prime than the more frequent alternant (i.e. active voice). Note that our main effect of PD-bias does not run contrary to this generalisation, as we measure the PD-bias of the target verb. The PD-bias effect in our models predicting DO responses simply reflects that the prior lexical bias of a verb is predictive of how that verb will be used.
4. Relatedly, Jaeger and Snider (2013) distinguish *prior surprisal*, based on an individual's cumulative linguistic experience, and *adapted surprisal*, based on the statistics of the immediate/recent communicative context.

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