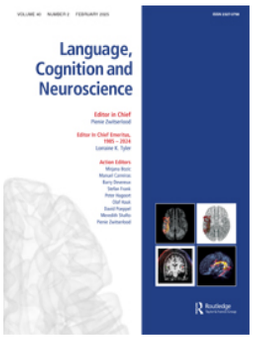

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Socially-mediated linguistic convergence and perceptions of social proximity

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ABSTRACT

Structural priming – the tendency to re-use syntactic forms after exposure to those forms – fits into a broader pattern of convergence between interlocutors at various linguistic levels. While sentence-level convergence is often explained in terms of cognitive mechanisms like implicit learning, recent work suggests that it can function to manage social distance with an interlocutor, as has been demonstrated for phonetic accommodation. Two experiments are presented that show that structural convergence is mediated by a speaker's perception of their social proximity to their interlocutor, and that these perceptions themselves can shift over the course of a conversation.

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Structural priming; dialogue; social distance; sentence production

1. Introduction

Evidence from dialogue studies show that interlocutors tend to converge with each other's language usage patterns at a number of linguistic levels. Phonetic imitation studies have shown that listeners adapt their speech to be more similar to that of speakers they have prior exposure to (Goldinger, 1998; Kim et al., 2011; Namy et al., 2002). Interlocutors also converge on a set of expressions to refer to items in the linguistic environment (lexical entrainment) – a phenomenon often explained in terms of referential pacts formed between interlocutors (Brennan, 1996; Brennan & Clark, 1996). At the sentential level, interlocutors tend to converge on the same syntactic forms (Bock, 1986; Bock & Griffin, 2000; Pickering & Branigan, 1998). While these varieties of convergence look alike superficially, they have received different kinds of explanations.

Phonetic adaptation has long been conceptualised as a tool for social distance management (Bourhis & Giles, 1977). For example, increased phonetic convergence is observed for socially desirable (or in-group) interlocutors (Babel, 2010, 2012; see also Abrego-Collier et al., 2011; Llamas et al., 2009). According to such a theory, where linguistic adaptation serves to manage social distance to a specific interlocutor, divergence is explained by speakers being socially motivated to distinguish their group status from their interlocutors'. Lexical entrainment has also often been characterised in interlocutor-related, functional terms – for example, the Minimize Collaborative Effort principle (Clark & Wilkes-Gibbs, 1986) works to make communication more efficient (see also the theory of conceptual pacts (Brennan & Clark, 1996)).

By contrast, most accounts of structural priming have invoked speaker-internal pressures. While the details of proposals differ, many attribute structural convergence to a syntactic form being more easily re-activated if it was activated previously, or the procedure of assembling that syntactic structure being facilitated by frequent re-use (Bock, 1986; Bock & Griffin, 2000; Branigan et al., 2000; Ferreira & Bock, 2006; Kaschak, 2007; Pickering & Branigan, 1998). In some such proposals – for example, Pickering and Garrod's (2004) Interactive Alignment Model – incremental, automatic priming at one linguistic level (e.g. syntax) increases alignment at all levels, resulting in shared situation models (as in Zwaan & Radvansky, 1998). This convergent mental representation could be viewed as a state in which communication is maximally efficient, in the "Minimize Collaborative Effort" sense (Clark & Wilkes-Gibbs, 1986); however, convergence in such a model is still fundamentally driven by an automatic, speaker-internal cognitive mechanism, rather than by socially-motivated intentional shifts in language use. Also supporting a speaker-internal view of convergence, individual cognitive capacity measures (e.g. working memory) have been invoked to explain the

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strength of convergence under different task conditions (Heyselaar & Segaert, 2019).

However, previous work has shown that structural convergence is sensitive to speakers' perceptions of interlocutor characteristics, including whether they are a native speaker (Kim & Chamorro, 2021), whether they speak the same or a familiar language variety (Chun et al., 2016; Chun & Kaan, 2022), or how similar or socially-desirable they are judged to be to the speaker on a number of socio-cultural dimensions (Balcetis & Dale, 2005; Hwang & Chun, 2018; Weatherholtz et al., 2014), even when interacting with computer avatars (Heyselaar et al., 2017). Linguistic alignment has even been proposed to enhance romantic attraction (Ireland et al., 2011).

Here, we use structural priming as a measure of speakers' convergence with their interlocutor to address two questions raised by prior research: whether native-speaker status and perception of social proximity have independent effects on convergence (Experiment 1), and whether any shifts in speakers' perceptions about their proximity to their interlocutors over the course of an experimental session are associated with convergence observed during the session (Experiment 2). Of multiple correlated measures of social proximity collected from participants, the one resulting in the best model fit was included in the analyses of Experiment 1 (likelihood of having similar backgrounds) and Experiment 2 (estimated geographical distance between home towns).

2. Experiment 1: nativeness, social proximity and ungrammaticality

Experiment 1 probes the relationship between perceptions of native speaker status and social proximity in predicting syntactic convergence in dialogue. Kim and Chamorro (2021) found that native English speakers converged to a greater extent with another native English speaker relative to a foreign-accented interlocutor. Notably, this effect extended to cases where the syntactic structure they converged on was dispreferred, or even ill-formed, in English. One way to conceptualise the native-speaker effect is as an indicator of (un)certainty: while a non-native speaker may produce atypical or ungrammatical sentences due to a lack of competence, a native speaker is likely to have high certainty about the well-formedness of sentences they produce. A native comprehender might therefore "trust" the well-formedness of another native speaker's utterances more than they do those of a non-native speaker, resulting in greater convergence with the native speaker.

However, native speaker status is likely to align with other social features that need not be linguistic: in assessing their social proximity to an interlocutor, a speaker may infer that they have more in common with another native speaker than with a foreign-accented speaker. While the observed effect on linguistic behaviour may look the same, convergence driven by social proximity inferences is a fundamentally different kind of explanation than native speaker status: the language usage of an interlocutor serves as a cue to their socio-cultural background, allowing a speaker to approximate how similar or different their interlocutor's background is to their own. To the extent that different language backgrounds (native vs. non-native) tend to correspond to different cultural backgrounds, the potential effects of social proximity inferences cannot be disentangled from native speaker effects if participants are native speakers, as illustrated in Table 1.

To address this issue, Experiment 1 uses participants who are proficient non-native English speakers (native language Spanish), with confederates from three different language backgrounds: native English speakers, native Spanish speakers, and native Slovak speakers (Table 2), all of whom were speaking in English.

In addition to making use of different confederate backgrounds, including indicators of both nativeness and social proximity allows us to assess whether convergence is driven primarily by just one of these factors, or whether they have independent effects on convergence. Using individual participants' assessments of their interlocutors' nativeness also allows us to test a prediction of the native speaker hypothesis: that the strength of convergence should depend on the participant's perception of their interlocutor's language competence. In particular, in speaker pairs where neither is a native speaker of English, does the extent of convergence depend on a participant's assessment of how native-like their interlocutor's English is compared to their own?

Table 1. Convergence predictions for native English speakers. "+"/"−" represent greater/less predicted convergence.

	Confederate language background	
	Native English	Native Spanish
Predictions from nativeness	+	−
Predictions from social proximity	+	−

Table 2. Convergence predictions for non-native English speakers/native Spanish speakers. "+"/"−" represent greater/less predicted convergence.

	Confederate language background		
	Native English	Native Spanish	Native Slovak
Predictions from nativeness	+	−	−
Predictions from social proximity	−	+	−

In both experiments presented here, participants played a dialogue picture-matching game with another "player" who was a confederate, who consistently used only Double Object (DO) forms. In English, whether a verb participates in the dative alternation – between the Prepositional Dative (PD) and DO forms – is largely an arbitrary lexical property, as illustrated by (1)-(2) (from Ferreira, 1996; "*" in (2b) indicates ungrammaticality).

(1) GIVE
  a. The widow gave the car to the church.
     [PD: *the car* = theme, *the church* = recipient]
  b. The widow gave the church the car.
     [DO: *the church* = recipient, *the car* = theme]
(2) DONATE
  a. The widow donated the car to the church.
     [PD: *the car* = theme, *the church* = recipient]
  b. *The widow donated the church the car.
     [DO: *the church* = recipient, *the car* = theme]

We refer to verbs like "give" as *alternating* verbs, because they can alternate between DO and PD forms, and verbs like "donate" as *non-alternating*, because they can only be used in PD form (see Section 2.1.2.2. for how verb alternation status was determined empirically as the strength of preference for one form over the other). Because confederates only produced DO sentences, they frequently produced sentences that were ill-formed in English, when a trial featured a non-alternating verb.

The outcome measure was the sentence structure produced by the participant: while structural priming predicts increased production of DO sentences, Experiment 1 asks whether the penalty for using a DO form with increasingly PD-biased verbs is mediated by a participant's perception of their interlocutor's linguistic and/or socially-defined attributes.

Table 3. Example experimental items. The descriptions represent the two sentence structures that participants could have produced using the specified verb (NP = noun phrase, PP = prepositional phrase).

	Alternating verb	Non-alternating verb
Prepositional dative	 SHOW Ron is showing [$_{NP}$ the painting] [$_{PP}$ to Luna].	 REPORT 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].

2.1. Method

2.1.1. Participants

Seventy-two native Spanish speakers were recruited using Prolific and were paid £10 each to participate. Each session took approximately 50 min. Participants were limited to those with IP addresses in Spain who considered themselves "fluent in English"; they were aware they would be participating in English. Based on their responses to the Language Experience and Proficiency Questionnaire (LEAP-Q) (Marian et al., 2007), 2.8% of participants rated themselves in English speaking proficiency as "slightly less than adequate (4/10)", 2.8% as "adequate (5/10)", 8.3% as "slightly more than adequate (6/10)", 29.2% as "good (7/10)", 37.5% as "very good (8/10)", 16.7% as "excellent (9/10)", and 2.8% as "perfect (10/10)". Participants ranged in age from 18 to 54 years (M = 26.4, SD = 7.9).

2.1.2. Materials

2.1.2.1. Dialogue game. A computer-based picture-matching game was used 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 3. 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. The two players were in separate locations, and communicated by voice only using web-based videoconferencing.

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 with "Yes/No" followed by a full sentence describing what they saw on their screen. On *Respond* trials, the roles were reversed: the other player produced a description of their image, and the participant replied indicating whether their image matched or did not match the description. 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) Respond trial:
a. Confederate: Ron is showing Luna the painting.

b. Participant: No, Hermione is showing Luna the painting.

Describe trial:

c. Participant: Luna is reporting the broken window to Hermione.
d. Confederate: Yes, Luna is reporting Hermione the broken window.

Respond trial:

e. Confederate: Harry is describing Hermione something.
f. Participant: Yes, Harry is describing Hermione something.

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.

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 convergence was contingent on the nativeness of the speaker, participants' perceptions of the speaker's socio-cultural attributes, or the alternation status of the verb.

2.1.2.2. 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 A for verbs as used in prime sentences). The verbs varied in terms of how unacceptable they were in the DO form, ranging from unbiased verbs permitting both DO and PD forms ("alternating" verbs), to strongly biased verbs which were only acceptable in the PD form ("non-alternating" verbs; see Table 3 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. The 30 images were shown two times each, once with a PD sentence using the relevant verb, and once with a DO sentence using the same verb, resulting in a total of 60 trials. These were shown in randomised order to 20 participants, who rated each sentence for how natural it sounded as a description of the picture. 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 mean ratings for DO forms from the mean ratings for PD forms (see Appendix A). Thus, a verb that was judged equally natural in the PD and DO forms would have received a score of 0, while increasing positive PD-bias scores reflect larger differences in how PD and DO forms were rated. In all the analyses presented, we use PD-bias as a measure of the pre-existing lexical bias associated with the verb. In our materials, PD-bias ranged from −0.096 to 0.60 ($M = 0.21$, $Mdn = 0.15$, $SD = 0.20$). In our unaggregated norming data, 14 out of 30 verbs showed significantly higher PD ratings than DO ratings; for the remaining 16 verbs, there was no reliable difference between PD and DO ratings. The verbs we initially selected as alternating and non-alternating based on Ferreira (1996) were separated into the same categories in the norming data.

2.1.3. Design and procedure

Participants were paired with one of three speaker types: (1) a native English speaker, (2) another L1-Spanish speaker, or (3) an L1-Slovak speaker. All confederates spoke exclusively in English.

All participants saw the same set of items, in one of three pseudorandom orders, distributed equally across speaker types. Fixed pseudorandomised lists were used to better control the distribution of verbs across trial. Each list contained 30 test items (ranging in PD-bias from unbiased to strongly PD-biased) and 20 fillers (sentences with intransitive or transitive verbs). To increase the number of test items per participant given the limited number of verbs for which we had norming data and images, test items were repeated 6 times each (3 times on Describe trials and 3 times on Respond trials), with an average minimum distance between repetitions of test items of 19.1 trials, resulting in 180 test trials.

Filler items were repeated between 3 and 6 times each, for a total of 78 filler trials. Filler trials were used to balance as evenly as possible the numbers of trial pairs (a sequence of one Respond trial and one Describe trial) where both the Respond and Describe trials featured matching pictures (eliciting "Yes" responses; 33 trial pairs across both test and filler trials), where both trials featured mismatched pictures (eliciting "No" responses; 30 trial pairs), and where Respond and Describe trials differed in (mis)match status (eliciting

either "Yes" followed by "No", or "No" followed by "Yes"; 30 and 36 trial pairs respectively). In total, there were 258 trials.

A brief break was included halfway through the session. The participant and the confederate conversed freely (in English) 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.

2.1.3.1. Pre-test survey. Participants completed the Peninsular Spanish version of the LEAP-Q, which included self-assessments of their English proficiency.

2.1.3.2. Post-test survey. After playing the game described above with the confederate, participants completed a survey where they were asked to indicate their agreement with statements about the other player, on a 5-point Likert scale (1 = *strongly disagree*, 5 = *strongly agree*), including "The other person was easy to understand" (Easy-to-understand), "The other person and I have similar backgrounds (education, socio-economic class, family)" (Similar backgrounds), "If the other person and I lived in the same place, we would be part of the same friend group" (Same friends), "The other person and I have similar interests" (Similar interests), and "The other person was a native speaker of English" (Nativeness).[2]

2.1.3.3. 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 excluded from analysis; they comprised 5.9% of the data. For the remaining data, a binary outcome variable was coded as 1 for DO and 0 for PD responses.

The Easy-to-understand post-test question was used to identify any cases where participants experienced comprehension difficulty. We excluded one participant who had responded "somewhat difficult to understand" (all other participants responded with "neither difficult nor easy to understand" or higher, with 97.2% responding "somewhat easy to understand" or "extremely easy to understand").

2.1.3.4. Model fitting. Because the pre-test variables coding social proximity (Similar backgrounds, Same friends, Similar interests) were all correlated with each other (all $r > .26$, all $p < .01$), as a first step, models were fitted predicting DO responses, with PD-bias, Trial type (Describe, Respond), Trial, Nativeness (participant's judgement about their interlocutor's Native speaker status), one of the aforementioned pre-test variables, and all interactions. The model using Similar backgrounds and its interactions as predictors was selected as having the best fit based on the Bayes Information Criterion (BIC).

Responses were fitted with mixed-effects logistic regression models predicting DO responses, with PD-bias, Trial type (Describe, Respond), Trial, Similar backgrounds, Nativeness, and up to four-way interactions included as predictors. 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. Terms were removed starting with highest order terms, and within same-order terms (e.g. four-way interactions), terms were removed in order of increasing z-value. 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, until the model converged (see Barr et al., 2013). Within same-order terms, random effects terms that accounted for the least variance were removed first.

2.2. Results

Because each item (verb-image pair) appeared multiple times over trials in our materials, we present two sets of analyses. One uses the full dataset will all repetitions included, with the possibility that later occurrences of each verb were influenced by earlier occurrences via syntactic entrainment (Gruberg et al., 2019). To assess the extent to which our results rely on syntactic entrainment, we performed a second analysis using a subset of the data including only the first occurrence of each item. Data from Experiments 1 and 2 are available at https://doi.org/10.22024/UniKent/01.01.498.

2.2.1. Analysis including all verb repetitions

There were main effects of PD-bias ($\beta = -2.05$, $SE = 0.71$, $p = .0042$), with stronger PD-biased verbs less likely to be produced in DO form, and Trial ($\beta = 0.015$, $SE = 0.0018$, $p < .001$), reflecting an overall increase in DO production

over trials. A main effect of Trial type ($\beta = 1.32$, $SE = 0.12$, $p < .001$) indicates that DO sentences were more likely to be produced on Respond trials, where the confederate initiated the trial by producing a DO description first (following their script), than Describe trials, where the participant described their display first. There was also a main effect of Nativeness ($\beta = 0.47$, $SE = 0.19$, $p = .011$), with participants producing more DO sentences when they perceived their interlocutor as more likely to be a native English speaker. This may be because, regardless of whether confederates' use of DO sentences conformed to participants' own usage (as with low PD-bias verbs) or did not (as with high PD-bias verbs), sentences spoken by a native-sounding speaker were perceived as less ill-formed across the board. The effect of Nativeness strengthened over trials ($\beta = 0.0025$, $SE = 0.0011$, $p = .020$; see Appendix B, Table B1 for full model, Table B2 for simple slopes analyses).

The social proximity variable, Similar backgrounds, interacted with Nativeness ($\beta = 0.39$, $SE = 0.18$, $p = .032$): as participants perceived their interlocutor as likelier to have a similar background to themselves, their interlocutor's native speaker status had a greater impact on their own DO production. Specifically, when interlocutors were perceived to be native speakers, participants produced similarly high rates of DO sentences; however, for perceived non-native speakers, participants produced fewer DO sentences as their perceived shared background with their interlocutors increased. This effect became more pronounced over trials ($\beta = 0.0031$, $SE = 0.13$, $p = .015$). The mean proportions of DO productions over Trials, by Nativeness and Similar backgrounds are shown in Figure 1.

Finally, Similar backgrounds, Nativeness, and Trial interacted further with Trial type ($\beta = 0.0012$, $SE = 0.00039$, $p = .0016$): the three-way interaction was more pronounced for Respond than Describe trials.

2.2.2. Analysis including only first occurrence of each verb

It has been shown that speakers use the same syntactic structure for an event across repetitions of that event (syntactic entrainment – see Gruberg et al., 2019). Since our materials repeated each test item six times, it is possible that later occurrences of an item were influenced by the structure used for that item on earlier occurrences. To assess whether the central effects of Nativeness and Similar backgrounds emerge in the absence of any repetitions, we analysed the subset of the data including only the initial occurrence of each verb.

Responses were fitted with a mixed-effects logistic regression model predicting DO responses, as described above, except that, in order to allow the model to converge with the reduced dataset, only the following set of key predictors were included in the model: PD-bias, Trial, Similar backgrounds, Nativeness, and up to three-way interactions. Fixed effects and random effects structures were determined as described above.

The resulting model featured main effects of Trial ($\beta = 0.0073$, $SE = 0.0027$, $p = .0070$) and Nativeness ($\beta = 0.26$, $SE = 0.12$, $p = .024$), and the Nativeness:Trial interaction ($\beta = 0.0026$, $SE = 0.00088$, $p = .0032$), as in the full dataset model (see Appendix B, Tables B3–B4). In addition, there was a PD-bias:Nativeness:Trial interaction ($\beta = -0.0080$, $SE = 0.0029$, $p = .0067$), such that the Nativeness effect strengthened less over Trials with more highly PD-biased verbs. However, the Similar backgrounds:Nativeness interaction from the full dataset model is only marginally significant in the subset model ($\beta = 0.20$, $SE = 0.12$, $p = .089$), and no other terms involving Similar backgrounds emerged as significant predictors of DO responses.

2.3. Discussion

The results of Experiment 1 corroborate previous findings of native speaker effects: the overall increase in DO production over trials is more pronounced when participants judge their interlocutor to be a native English speaker. In addition, the more native-like a speaker perceives their interlocutor to be, the more they were willing to follow their lead in using ill-formed DO sentences with verbs strongly biased toward the PD form. This was reflected in more DO productions for verbs with stronger PD-bias relative to participants who rated their interlocutors as less native-like. Unlike previous studies (e.g. Kim & Chamorro, 2021), the native speaker category assigned to confederates was not based on whether they were actually native or non-native speakers of English – rather, each participant's individual assessment of how native-like their interlocutor/confederate seemed was used.[3] This distinction is relevant if the native speaker effect is driven by a speaker's reasoning about how confident they can be about their interlocutor's language competence. In fact, while a number of studies have demonstrated that non-native speech is measurably different from native speech on measures such as speech rhythm (Gut, 2003; Mok & Dellwo, 2008), speech rate variability (Morrill et al., 2016), and usage of discourse markers like "like" or "well" (Fuller, 2003), far fewer studies have asked to what extent these features are detected by non-native listeners (like the participants in Experiment 1), and how much they contribute to discriminating native from non-native speech.

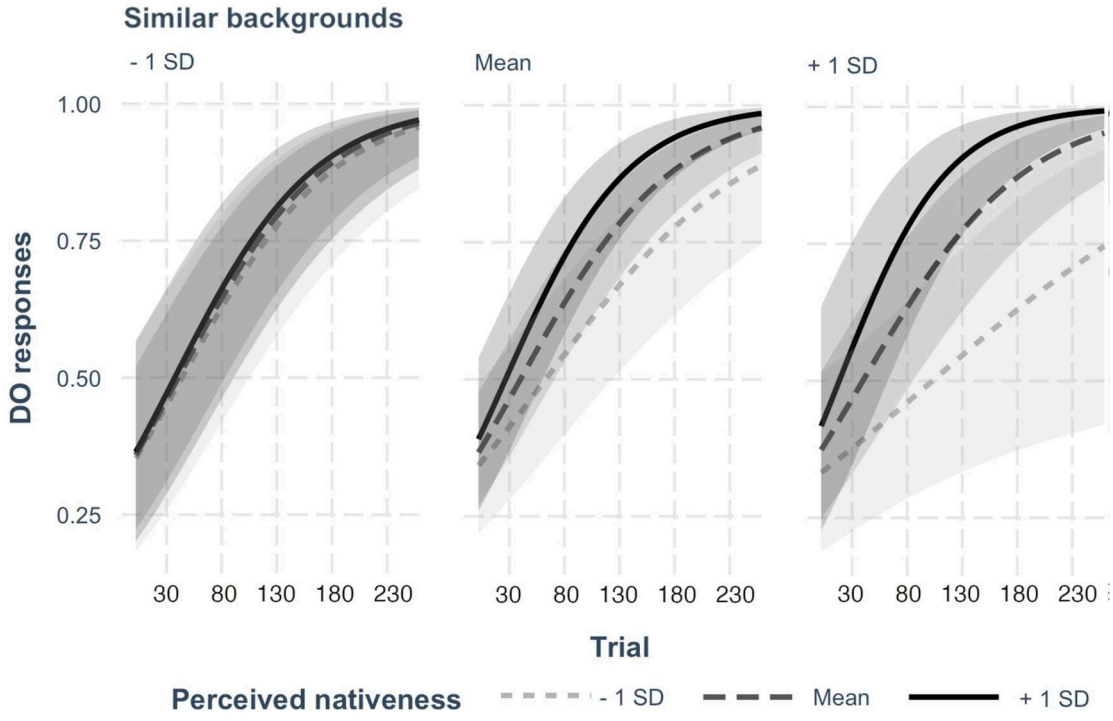

Figure 1. Experiment 1: Proportion of DO productions over trials, by perceived interlocutor Nativeness and social proximity (Similar backgrounds). Bands reflect 95% confidence intervals.

Similar backgrounds was one of a number of proxy variables we included as indicators of socio-cultural proximity, and we make no specific claim about how these measures might differ from each other. However, the different patterns of effects of Similar backgrounds and Nativeness in Experiment 1 suggest these variables are not explaining the same pattern in the data. For one thing, Similar backgrounds emerges only as moderators of the Nativeness effect in the full dataset model, and do not research significance at all in the subset model when verb repetitions are excluded. We therefore cannot disentangle any effect of this social proximity variable from the potential influence of syntactic entrainment (though it is also possible that the already weaker effect of social proximity did not survive due to the subset dataset including just a sixth of the trials in the full dataset).

To the extent that the Nativeness effect depends on perceived social proximity, it appears to affect how much participants converged with perceived non-native, rather than perceived native, speakers. For perceived native speakers, the increase in DO production over trials is comparable across levels of perceived social proximity (see Figure 1). This is consistent with participants having high confidence in the usage patterns of a native-sounding speaker, particularly because Experiment 1 participants were non-native speakers of English themselves.

For perceived non-native interlocutors, participants produced roughly the same proportion of DO sentences as for perceived native interlocutors in early trials. However, their DO production rate increased less over trials as they perceived their interlocutor as likelier to have similar backgrounds to them. Since participants had less of a reason to "trust" the usage patterns of another non-native speaker, their reasons for converging with such an interlocutor initially might have been more functional – to maximise comprehensibility, and reduce the risk of miscommunication. Indeed, many studies have shown that speakers compensate linguistically to accommodate listeners' perceived needs, e.g. by speaking more loudly over greater distances (Pelegrín-García et al., 2011) or simplifying sentence structure when communicating abstract concepts in an educational context (Medimorec et al., 2015). As trials progressed, this pressure may have weakened, but only when a perception of shared backgrounds gave participants the impression that communication would succeed – effectively compensating for any communicative risk associated with differences in usage patterns.

A difficulty in isolating social proximity effects is that shared native speaker status is likely to align with intuitions about social similarity. Experiment 2 therefore removes the native/non-native speaker element, and asks whether there is still evidence of social proximity effects. To do this, confederates

were used who were native speakers of different regional varieties of English. In addition, to assess whether experience with a speaker (here, during an experimental session) can influence perceptions of social proximity, participants were asked to provide judgements of social proximity to their interlocutor before and after the experimental session.

3. Experiment 2: convergence and shifting judgments about social proximity

Experiment 2 asked whether social proximity effects on convergence emerge in the absence of differences in native speaker status, and whether convergence over the course of a conversation are associated with changes in speakers' judgements about their proximity with their interlocutor. Native British English speaker participants were paired with a confederate speaking one of two regional varieties of British English (confederates were from Cork, Ireland, and Southeast England). Before playing the game, participants heard a pre-recorded clip of the confederate they were paired with, and provided ratings about the speaker's attributes (same as in Experiment 1 post-test) based on the recorded speech. Participants' perceptions of their geographical proximity to the speaker were measured by asking them to indicate their own hometown and where they thought the speaker's hometown was on a map.

3.1. Method

3.1.1. Participants

Sixty native speakers of British English were recruited from the University of Kent community, and were paid £10 each to participate. They ranged in age from 18 to 39 years ($M = 22.2$, $SD = 4.6$).

Table 4. Map distance, pre- and post-test agreement scores (1 = "strongly disagree"; 5 = "strongly agree") for Experiment 2. Map distance was indicated by clicking locations on a map; the coordinates were converted to distances in km between the two locations.

	Pre-test *M* (*SD*)	Post-test *M* (*SD*)
Estimated distance (km) between interlocutor's and participant's home towns	316.3 (240.5)	–
If the other person and I lived in the same place, we would be part of the same friend group	2.1 (1.2)	2.7 (1.1)
The other person and I have similar interests	2.7 (0.8)	2.8 (0.9)
The other person has a similar background to me (education, socio-economic class, family)	2.8 (1.1)	3.3 (1.0)

3.1.2. Materials

The same dialogue game and stimulus materials were used as in Experiment 1.

3.1.3. Design and procedure

Participants were native British English speakers and were paired with one of the two confederates: either the one from Cork, Ireland, or the one from Southeast England. All participants saw the same set of items, in one of three pseudorandom orders, distributed equally across speaker types.

3.1.3.1. Pre-test survey. Before playing the game, participants heard a pre-recorded clip of the confederate they were paired with. Based on the recorded speech, participants provided ratings about the speaker's attributes – these were the same as the post-test ratings in Experiment 1 (Same friends, Similar backgrounds, Similar interests). In addition, participants' perceptions of their geographical proximity to the confederate were measured by asking them to indicate their own hometown and where they thought the speaker's hometown was on a map (Map distance).

3.1.3.2. Post-test survey. To assess whether convergence during the session shifted participants' judgements about their interlocutors, the pre-test questionnaire (excluding Map distance) was readministered to collect post-test judgements.

3.1.3.3. Data coding. Responses were coded as for Experiment 1. "Other" responses excluded from the analysis comprised 8.8% of the data.

3.1.3.4. Model fitting. As for Experiment 1, variables coding social proximity (Similar backgrounds, Same friends, Similar interests) were all correlated with each other (all $r > .31$, all $p < .05$). In addition, Map distance, which we also take to indicate perceived similarity, was correlated with Same friends ($r = -.25$, $df = 58$, $p = .05$) and Similar backgrounds ($r = -.30$, $df = 58$, $p = .02$). Because we had no *a priori* hypotheses about differences among these variables, we selected one for inclusion in the analysis based on the BIC. Among initial models predicting DO responses with PD-bias, Trial type, Trial, one of the above variables encoding similarity, and all interactions as predictors, the model including the Map distance variable was the most efficient.

Responses were fitted with mixed-effects logistic regression models predicting DO responses, with PD-bias, Trial type, Trial, Map distance, and up to four-way interactions included as predictors. Categorical

predictors were sum coded unless specified otherwise, and numerical predictors were centred. Map distance (km) was standardised.

3.2. Results

3.2.1. Pre-test survey

Pre- and post-test agreement scores and estimated map distance are in Table 4.

Standardised Map distance estimates were included in the analysis of convergence (Section 3.2.2). We return to the difference in pre- to post-test responses in Section 3.2.3.

3.2.2. Analysis including all verb repetitions

As in Experiment 1, there were main effects of PD-bias ($\beta = -6.63$, $SE = 0.98$, $p < .001$) and Trial type ($\beta = 1.41$, $SE = 0.14$, $p < .001$), with fewer DO productions for more strongly PD-biased verbs, and for Describe than Respond trials. The Trial type effect weakened over Trials ($\beta = -0.0020$, $SE = 0.00071$, $p < .001$; see Appendix B, Tables B5–B6). Unlike in Experiment 1, there was a negative main effect of Trial ($\beta = -0.0030$, $SE = 0.0013$, $p = .016$), with DO sentences decreasing as trials progressed. In other words, over the course of the session, participants tended to diverge from their interlocutor.

Finally, there were two interactions involving Map distance. The PD-bias effect was moderated by Map distance ($\beta = -1.08$, $SE = 0.37$, $p = .0032$): as a participant's perception of their geographical proximity with their interlocutor decreased, they became less unwilling to use DO sentences with strongly PD-biased verbs. Participants also produced fewer DO sentences on Respond trials as Map distance increased, while the lower DO production rate on Describe trials remained constant ($\beta = -0.24$, $SE = 0.11$, $p = .035$). Mean proportions of DO responses over trials by PD-bias and Map distance are shown in Figure 2.

3.2.3. Analysis including only first occurrence of each verb

As for Experiment 1, we analysed a subset of the data including only the first occurrence of each verb, to assess to what extent the effects from the full dataset model rely on syntactic entrainment resulting from multiple repetitions of each verb over trials. The subset model included fixed effects of PD-bias, Trial, Map distance, and their interactions; the random effects structure was determined as described for the full dataset model. The main effects of PD-bias ($\beta = -3.53$, $SE = 2.70$, $p = .19$) and Trial ($\beta = -0.0088$, $SE = 0.0080$, $p = .27$) observed in the full dataset model do not reach significance in the subset model (see Appendix B, Tables B7–B8). As in the full dataset model, there is a PD-bias:Map distance interaction, with participants becoming less unwilling to produce DO sentences as Map distance decreased ($\beta = -1.30$, $SE = 0.49$, $p = .0030$).

3.2.4. Pre- to post-test change in social proximity scores

In order to assess whether the magnitude of change in social proximity judgments by participant influenced DO production, we fitted responses with the same regression models described in Sections 3.2.2 and 3.2.3, except that the Map distance predictor was replaced with the pre- to post-test difference in Same friends agreement scores (selected from the pre- to post-test variables based on the BIC, as described in Section 3.1.3.4).

The resulting full dataset model included main effects of PD-bias ($\beta = -6.78$, $SE = 1.18$, $p < .001$) and Trial ($\beta = -0.0030$, $SE = 0.0015$, $p = .048$), and a three-way interaction between PD-bias, Trial, and pre- to post-test difference scores ($\beta = 0.0043$, $SE = 0.0020$, $p = .029$; see Appendix B, Tables B9–B10), mirroring the interaction with Map distance in the model described in Section 3.2.2. In the subset model, PD-bias ($\beta = -5.53$, $SE = 0.99$, $p < .001$), Trial ($\beta = -0.0020$, $SE = 0.00093$, $p = .032$), and pre- to post-test difference scores ($\beta = 0.41$, $SE = 0.12$, $p < .001$) appear as main effects (Appendix B, Table B11): while DO production decreased over trials, participants whose judgments increased the most about how likely they were to have the same friend group as their interlocutor produced more DO sentences overall.

3.3. Discussion

In line with Experiment 1, Experiment 2 revealed fewer DO productions for more strongly PD-biased verbs, as well as for Describe than Respond trials. However, even though participants were more likely to produce DO forms for Describe sentences as trials progressed, the overall production of DO sentences decreased as the session continued, unlike in Experiment 1. This divergence may be a reflection of participants' certainty about their own grammatical competence as, unlike in Experiment 1 where participants were non-native speakers of English, Experiment 2 tested only English native speakers.

In addition, participants were more likely to produce DO forms with strongly PD-biased verbs when they perceived their interlocutor to be closer to them geographically. Estimated geographical closeness is likely to reflect judgments about the similarity of the dialects spoken by the participant and their interlocutor: while our other

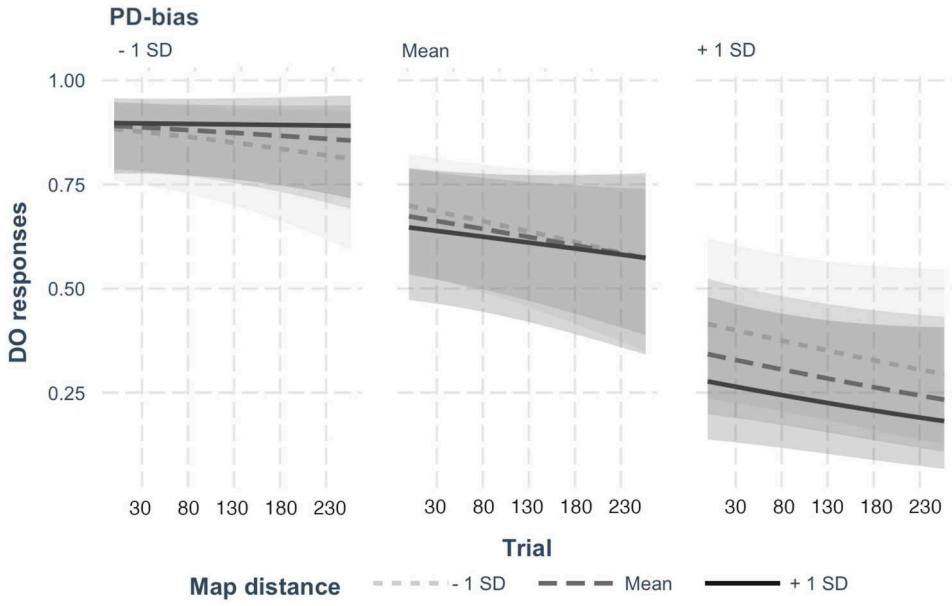

Figure 2. Experiment 2: Proportion of DO productions over trials, by PD-bias and Map distance. Bands reflect 95% confidence intervals.

similarity measures (Same friends, Similar interests, Similar backgrounds) can cross-cut region of origin (i.e. no matter one's home town, it is possible to judge other community members as more or less similar on these dimensions), being from the same region as another person is a more direct explanation for speaking similarly. This raises the question of whether the effect of geographical distance is a variety of the Nativeness effect observed in Experiment 1: native speaker participants might have had greater certainty about their interlocutor's native speaker status if they perceived them to be from somewhere near their own home town. However, the model including pre- to post-test difference scores instead of Map distance showed a similar pattern of effects for a social proximity variable (likelihood of having similar friend groups) that is less obviously explained by geographical proximity.

In addition, the native speaker effect in Experiment 1 and the social proximity effects in Experiment 2 have different profiles over the course of an experimental session. While the Nativeness effect increased markedly as trials progressed, both geographical distance and changes to Same friends judgments were less pronounced, and changed very little over trials, if at all. This suggests that the effects of Nativeness and social proximity have different underlying drivers.

4. General discussion

Together, these experiments demonstrate that structural convergence is mediated by a speaker's perception of their proximity to their interlocutor – independently of effects of the interlocutor's native speaker status, and that these perceptions can shift over the course of a conversation.

Looking at the two experiments together (Figures 1 and 2), there is a striking difference between Experiment 1 participants (non-native speakers of English) and Experiment 2 participants (native English speakers) in terms of their willingness to produce DO sentences with strongly PD-biased verbs, and their change in DO production over trials: the native speakers were much less likely to use forms that sounded "bad" to them, even though their interlocutor was consistently using those forms, and changed their rate of DO production markedly less than the non-native speakers. Indeed, participants in Experiment 2 tended to diverge from their interlocutor, in contrast to the convergence observed in Experiment 1. Thus, it appears that non-native speakers converge with their native speaker interlocutors due to low certainty about their own grammatical usage patterns relative to their interlocutors'. As confederates produced exclusively DO sentences in our experiments, repeated uses of the DO form with highly PD-biased verbs may have reinforced the perception that confederates were using such sentences with certainty about their well-formedness, resulting in increasing DO production by participants as conversations progressed.

The variables approximating social proximity in Experiment 2 are unlikely to be due to grammatical uncertainty, as all participants were native speakers of English, and indeed, they were generally unwilling to

produce sentences that sounded ill-formed to them. The slightly higher DO rate in early trials might be attributed to a pressure to minimise miscommunication risk with an unfamiliar interlocutor. From a pragmatic perspective, participants and confederates were engaged in a cooperative task, with aligned goals (complete the dialogue game efficiently, with minimal miscommunications or corrections) – any pressure to minimise miscommunication may have weakened over trials, as interlocutors became accustomed to each other's speech, and to the task. To the extent that perceived similarity affects this dynamic, it appears to loosen any pressure from miscommunication risk further, especially for highly PD-biased verbs for which DO sentences would have sounded the most ill-formed. This could be due to perceived similarity giving the impression that communication was unlikely to fail anyway, thereby "compensating" for differences in usage patterns between the interlocutors. If encoding syntactic structure uses domain general cognitive resources, as has been argued based on dual-task syntactic priming experiments (Heyselaar & Segaert, 2019), participants may have been motivated to allocate less resources to syntactic planning – and as a result showing less convergence with interlocutors and drifting back to their own usage patterns – as long as there was low perceived communicative risk.

Cooperating on a joint goal would also be a plausible explanation for the observed shifts in social proximity judgments, along with simply becoming more accustomed to an interlocutor's voice as a conversation progresses (see Chun et al., 2016; Chun & Kaan, 2022). In our experiments, these judgments were made on the basis of the confederate's speech alone, so we interpret these intuitions as socially-conditioned inferences about a speaker based on how they sound. While we cannot directly address the direction of causality between convergence and perceptions of social proximity, our findings are compatible with both perceived similarity leading to convergence, or convergence over the course of a conversation leading to perceived similarity – and indeed, these possibilities are not incompatible with each other.

In addition, as observed in Experiment 2, the different measures we used as proxies for similarity may affect or be affected differently by continued exposure to an interlocutor in dialogue. The differential sensitivity of speakers' adaptation to an interlocutor to different personal attributes is apparent in the literature: Weatherholtz et al. (2014), for example, found that alignment, and even alignment to different sentence structures, were affected differently by perceived agreement on political ideology with a speaker, the speaker's perceived smartness, and the perceived standardness of the speaker's accent. Similarly, Hwang and Chun (2018) found that students' alignment with their teacher was stronger when they had positive perceptions of the teacher, but weaker for students who scored high in social monitoring (tendency to adapt to social situations). Studies have also found contrasting effects for similar factors: while Weatherholtz et al. (2014) found greater alignment to more standard-sounding speakers, Chun et al. (2016) found weaker alignment to accents that participants were familiar with.

The miscommunication avoidance explanation suggested above reduces the social proximity effect to a functional one to ensure communicative success. However, such an explanation would predict different patterns of behaviour if successful communication was no longer a shared goal, or beneficial to one or both interlocutors. A way to disentangle that explanation from an alternative one – an independent inclination to converge with perceived in-group members tied not to goals but to a sense of affinity or shared identity – would be to separately vary communicative goals and social proximity. Indeed, divergence at the phonetic level has been observed in situations where there are social motivations to maintain, and even signal, inter-personal distance (e.g. Babel, 2010; Bourhis & Giles, 1977). By the same token, if having a joint goal is conducive to convergence, situations where cooperativity cannot be assumed – for example, in a competitive context, or one in which interlocutors have non-aligned goals – might be expected to reduce convergence, or result in divergence. We set these questions aside for further exploration in future research.

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. One post-test variable, "The other person and I talk similarly", was excluded because some participants were unsure about whether "similarly" referred to phonetic/phonological similarity or usage of similar sentence types.
3. We compared two versions of the model reported for Experiment 1: one including the individual rating of interlocutor nativeness (Nativeness) as a predictor, and another substituting this with a categorical Speaker type predictor (English, Spanish, Slovak, based on the actual native languages of the confederates). Based on

the BIC, the individual rating predictor model was better than the categorical predictor model.

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