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Socially-mediated linguistic convergence and perceptions of social proximity

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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.

Keywords: 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; Namy, Nygaard & Sauerteig, 2002; Kim, Horton & Bradlow, 2011). 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; Pickering & Branigan, 1998; Bock & Griffin, 2000). 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 Llamas, Watt & Johnson, 2009; Abrego-Collier et al., 2011). 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; Pickering & Branigan, 1998; Branigan, Pickering & Cleland, 2000; Bock & Griffin, 2000; Ferreira & Bock, 2006; Kaschak, 2007). 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 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; Weatherholtz et al., 2014; Hwang & Chun, 2018), 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 inter-personal 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.

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	+	–

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.

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 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?

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.

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2.1.1. Participants

Seventy-two native Spanish speakers were recruited using Prolific and were paid £10 each to participate. 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, Blumenfeld & Kaushanskaya, 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).

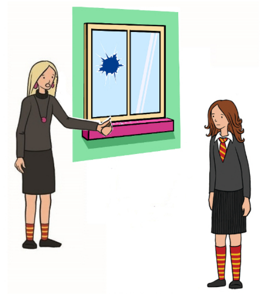
2.1.2. Materials

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.

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

SHOW



REPORT

Prepositional dative	*Ron is showing [! " the painting] [" " to Luna].*	*Luna is reporting [! " the broken window] [" " to Hermione].*
Double object	*Ron is showing [! " Luna] [! " the painting].*	**Luna is reporting [! " Hermione] [! " the broken window].*

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

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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 excused 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 order, Proficiency (from pre-test LEAP-Q), 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

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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[3], Trial type (Describe, Respond), Trial order, Similar backgrounds, English proficiency, 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. 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.

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The mean proportions of DO productions by Speaker type and PD-bias, aggregated by participant, are shown in Figure 1 (left panel). Data from Experiments 1 and 2 are available at https://doi.org/10.22024/UniKent/01.01.498.

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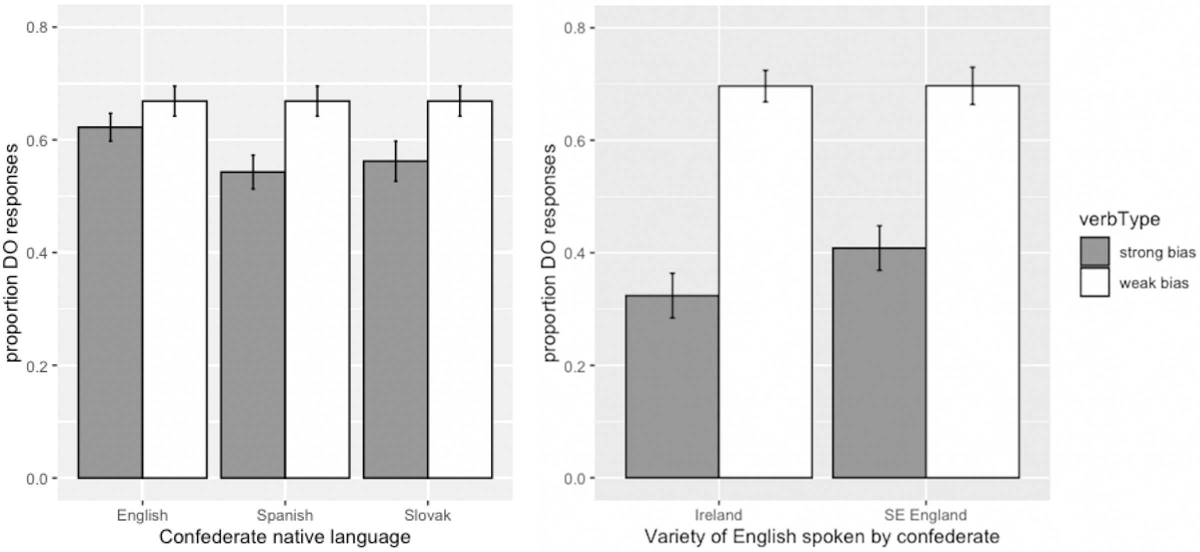

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Figure 1. Experiment 1 (left) and Experiment 2 (right): Mean proportions of DO responses by Speaker type and PD-bias. Strong/weak bias: greater/lower than median PD-bias. Error bars reflect standard error.

There were main effects of PD-bias (! = -1.68, *SE* = 0.62, *p* = .007), with stronger PD-biased verbs less likely to be produced in DO form, and Trial (! = 0.014, *SE* = 0.0020, *p* < .001), reflecting an overall increase in DO production over trials. A main effect of Trial type (! = 1.26, *SE* = 0.10, *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 the post-test Nativeness variable: as participants' perception of their interlocutor as a native English speaker increased, they produced more DO sentences (! = 0.76, *SE* = 0.26, *p* = .0034). This effect strengthened over Trials (! = 0.0040, *SE* = 0.0013, *p* = .0024; see Appendix B, Table B1 for full model, Table B2 for simple slopes analyses).

A PD-bias:Nativeness interaction (! = 0.39, *SE* = 0.20, *p* = .049) indicates that, as participants' certainty increased of their interlocutor being a native English speaker, their unwillingness to use the DO form with strongly PD-biased verbs decreased. Anomalous DO sentences using PD-biased verbs may therefore have been judged as less marked when produced by a confederate perceived as more native-like, corroborating prior findings that nativeness mediates convergence. This interaction was further mediated by Proficiency (! = 0.27, *SE* = 0.12, *p* = .030): the difference in PD-bias effect by perceived Nativeness was greater for higher Proficiency. The lowest proficiency participants produced fewer DO sentences for strongly PD-biased verbs and for an interlocutor they perceived as unlikely to be a native English speaker. However, for the highest proficiency participants, there was no reliable PD-bias penalty when participants had high certainty that their interlocutor was a native speaker.

Finally, there was a Similar backgrounds:Proficiency:Trial interaction (! = 0.0044, *SE* = 0.0020, *p* = .027): for higher but not lower proficiency participants, the rate of DO production increased more rapidly over Trials as Similar backgrounds increased. The interactions involving Proficiency have in common that some additional factor (perceived Similar backgrounds, perceived interlocutor Nativeness) boosts DO production only for participants who rated themselves as highly proficient.

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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[4]. 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, Baese-Berk & Bradlow, 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.

Two aspects of participants' assessment of their interlocutor affected DO production differently depending on their proficiency in English: higher proficiency participants showed the greatest convergence with their interlocutors as a function of perceived similar backgrounds or perceived native speaker status. The absence of a PD-bias effect for the highest proficiency participants when they perceived their interlocutor as a native English speaker is particularly striking, since higher proficiency should result in greater certainty about grammatical possibilities, including the unacceptability of the DO form for strongly PD-biased verbs. A possible explanation is that Proficiency in Experiment 1 tracks participants' perceptions of their own English ability more than it does their actual proficiency. The above interaction might therefore suggest that individuals who see themselves as highly proficient are more likely to want to "learn" from the usage patterns of someone they see as a native speaker.

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Shared 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 Shared backgrounds and Nativeness in Experiment 1 suggest these variables are not explaining the same pattern in the data. Unlike perceived native speaker status, perceived similar backgrounds did not reduce the PD-bias effect. Instead, it resulted in a faster overall increase in DO production for higher self-assessed proficiency, when participants thought they were speaking with a native speaker. This effect influences early trials, with the DO rate reaching ceiling earlier in the session – even in a situation like the one presented in this study, where participants have never interacted with their interlocutor before and are not even able to see them. The absence of a decline in DO sentences in late trials suggests that the DO boost in early trials was not merely to ensure successful communication at the beginning of a conversation with an unfamiliar interlocutor.

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.

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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).

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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 order, 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 order, 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.!

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3.2.1. Pre-test survey

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

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)

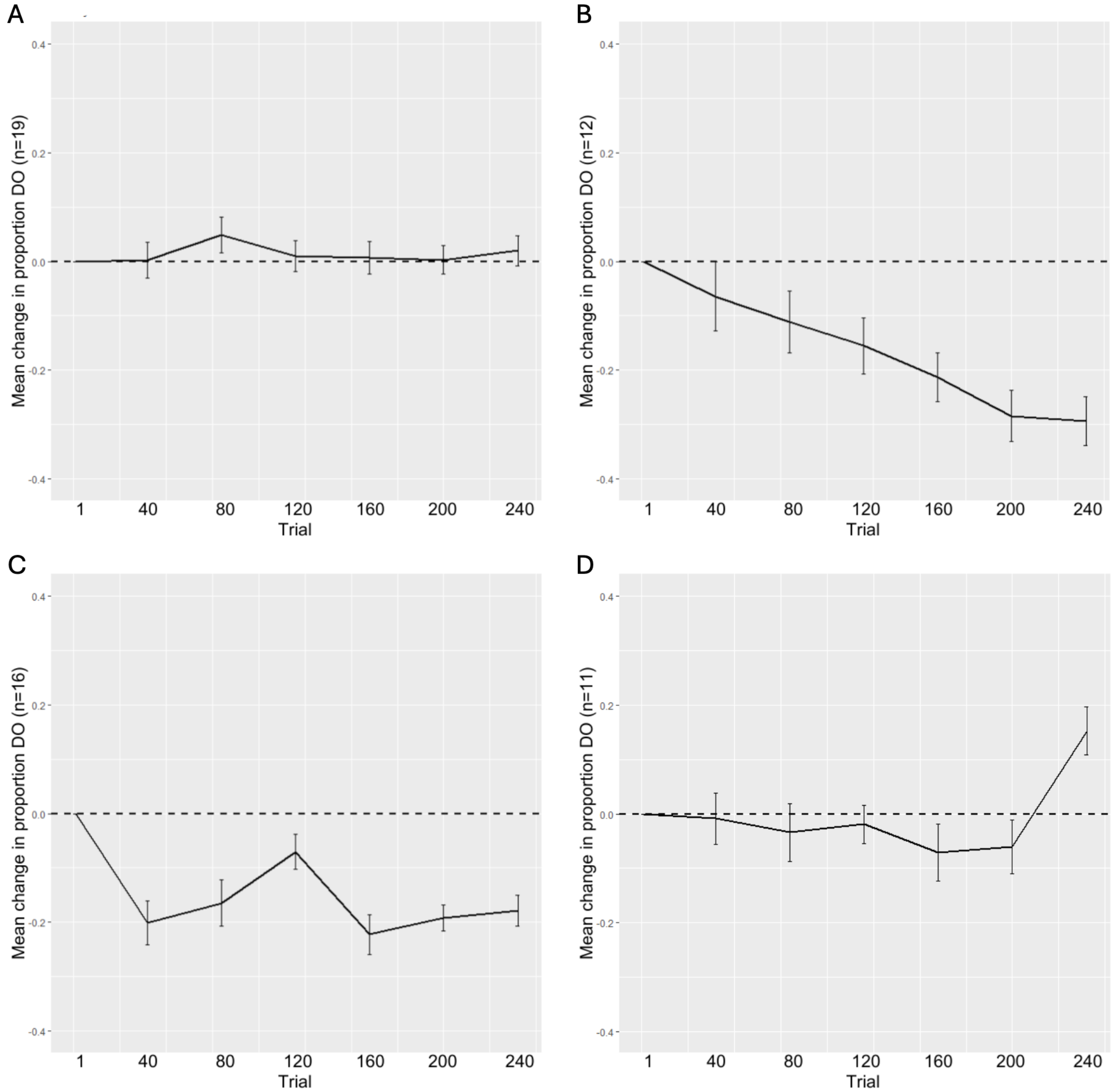
Standardised Map distance estimates were included in the analysis of convergence (section 3.2.2). We return to the difference in pre- to post-test responses in section 3.2.3.

3.2.2. Convergence

Mean proportions of DO responses by speaker type (variety of English spoken by the confederate) and PD-bias are shown in Figure 1 (right panel; note that this is for illustration only, and that speaker type was not used as a predictor in the analysis).

As in Experiment 1, there were main effects of PD-bias (! = -6.85, SE = 1.20, p < .001) and Trial type (! = 1.42, SE = 0.15, 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 (! = -0.0022, SE = 4.93e-04, p < .001; see Appendix B, Table B3 for the full regression model, Table B4 for simple slopes analyses). Unlike in Experiment 1, there was a marginal negative main effect of Trial (! = -0.0026, SE = 0.0013, p = .056), with DO sentences decreasing as trials progressed. In other words, over the course of the session, participants tended to diverge from their interlocutor.

On closer inspection of DO production over trials for individual participants, however, there appear to be subgroups of participants that show distinct patterns of change in DO rate over trials. We examined each participant's DO production over trials, with proportion of DO utterances calculated by 40-trial bins to smooth local fluctuations, and grouped participants into the four subgroups shown in Figure 2 (2 of 60 participants were excluded due to missing trials). Participants' DO rates were set to zero in the first bin in order to compare changes in DO production across participants with differing baseline DO rates.



!

Figure 2. Mean proportions of DO utterances for four subgroups of participants: (A) stayed at a constant DO rate, (B) showed a steady decline, (C) showed durations of both increase and decrease with a net decrease over trials, (D) showed both increase and decrease with a net increase over trials. Proportions were set to 0 (dashed line) in the first bin; deviations from 0 in subsequent bins reflect deviations from participants' individual baselines. Error bars represent standard error.

!

We return to these distinct patterns of change in DO rate in section 3.2.3.

Finally, there was an interaction between Map distance and PD-bias (! = -1.17, *SE* = 0.46, *p* = .010): as a participant's perception of their geographical proximity with their interlocutor decreased, their unwillingness to also use DO sentences with strongly PD-biased verbs weakened.

3.2.3. Post-test survey

In order to assess whether the magnitude of change by participant influenced DO production, we fitted responses with the same regression model described in section 3.2.2, 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 model included an interaction between PD-bias and pre- to post-test difference scores (! = 0.80, *SE* = 0.37, *p* = .033; see Appendix B, Tables B5-B6), mirroring the interaction with Map distance in the model described in section 3.2.2.

Figure 3 shows mean pre- to post-test difference scores broken down by the four participant subgroups identified in Figure 2 (note that these means are calculated using one difference score per participant, and therefore represent a much smaller amount of data than the by-trial data in the regression models). While there are too few data points for statistically reliable differences to emerge, there is a marked difference in the overall distribution of mean difference scores across the three pre- and post-test questions. Same friends and Similar interests show similar patterns to each other, with the largest increase in ratings for the participant subgroup that showed net increases in DO production, a decrease for the subgroup that showed net decreases, and the smallest pre- to post-test change for participants whose DO rates remained relatively constant over trials.

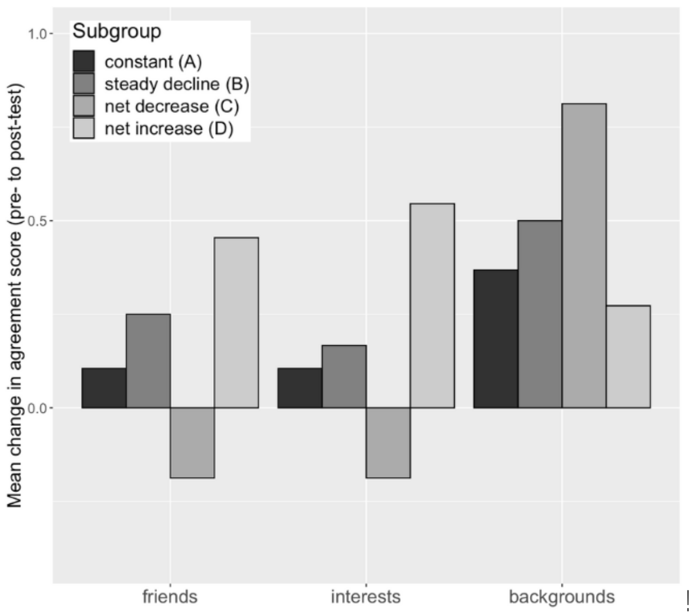


Figure 3. Mean difference scores (post-test - pre-test). Subgroups A-D correspond to the panels in Figure 2.

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Similar backgrounds shows a different pattern, with the smallest increase in the net increase in DO subgroups. While merely suggestive, it appears that different ways of asking about social proximity can have varied effects on convergence/divergence.

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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 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.

While we had no specific hypotheses about what aspects of social proximity our pre- and post-test questions would tap into, we noted a marked difference in the distribution of pre- to post-test rating changes across the three questions. This post hoc examination also suggests that, in addition to convergence and divergence, another relevant difference across individuals may be how readily they adapt their linguistic behaviour in response to interaction with another speaker whose usage patterns differ from their own. We leave further investigation of these questions for future research.

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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 side by side (as in Figure 1), 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: while DO rates for weakly-biased verbs are roughly the same across experiments, native speakers are much less likely to use forms that sounded "bad" to them, even though their interlocutor was using those forms. Indeed, participants in Experiment 2 tended to diverge from their interlocutor, in contrast to the convergence observed in Experiment 1. This suggests a complex relationship between relative certainty about one's own versus an interlocutor's language competence, which could lead to divergence, convergence driven by functional pressures (e.g. to make communication more effective), and convergence or divergence driven by something approximating shared or dissimilar social contexts (here, coded by judgements about inter-personal similarity). Indeed, the contrasting patterns of effects for native speaker status and perceived social similarity in Experiment 1 highlight the different ways in which these factors influence sentence production.

A question we cannot directly address here is the direction of causality between convergence and perceptions of inter-personal similarity. From both experiments, we know that measures of inter-personal similarity predict level of convergence. 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. However, our findings are compatible with both perceived similarity leading to convergence, or convergence over the course of a conversation leading to perceived similarity – 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.

While we observe changes in judgments about inter-personal similarity from the beginning to the end of a session in Experiment 2, there are a number of plausible explanations for these shifts that are not distinguished by our data. Something as simple as becoming more accustomed to an interlocutor's voice as a conversation progresses could increase perceptions about similarity (see Chun et al., 2016; Chun & Kaan, 2022). From the perspective of pragmatics, participants and confederates were engaged in a cooperative task, with aligned goals (complete the dialogue game efficiently, with minimal miscommunications or corrections) – cooperating on a joint goal would also be a plausible explanation for the observed shifts in inter-personal similarity judgments. A question for future work is how social, linguistic, and goal-related factors collectively shape how interlocutors adapt to each other when they are all salient in a conversational context.

The divergence observed in Experiment 2 can be explained as the combined effect of participants who were native English speakers and therefore had high certainty about their own intuitions about sentence well-formedness, and interlocutors who were themselves native speakers and were using ill-formed sentences as though they were perfectly natural. This could encourage an understanding of the situation as two native speakers of different varieties of English who are "agreeing to disagree" about acceptable sentence structures. However, if perceived social proximity does under certain circumstances make convergence more likely, there should be circumstances under which interlocutors diverge from each other for social reasons (rather than linguistic ones). 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. Bourhis & Giles, 1977; Babel, 2010). 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.

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Declaration of Interest

The authors report no conflicts of interest.

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Appendix A: Experimental materials

Experiments 1-2 used the same set of materials. The DO forms produced by the confederates are given below, along with the PD-bias score, and *t*- and *p*-values for the comparison between DO and PD forms. PD-bias was calculated as the difference between the mean rating for PD descriptions and the mean rating for DO descriptions produced for a particular verb and ditransitive event picture in the norming study (0-100 ratings were divided by 100 and centred). A perfectly equibiased verb-picture pair would have a PD-bias of 0; higher positive PD-bias scores represent increasing bias toward the PD form relative to the DO form. The most PD-biased verb, *explain*, received a mean PD rating of 0.84 and a mean DO rating of 0.24. The most equibiased verb, *show*, received a mean PD rating of 0.77 and a mean DO rating of 0.78.

Item	*Verb*	*DO sentence*	*PD-bias*	*t*	*p*
1	teach	*Hermione is teaching Harry a song.*	-0.096	0.90	.37
2	loan	*Hermione is loaning Ron some money.*	-0.019	0.19	.85
3	show	*Ron is showing Luna a painting.*	-0.010	0.13	.89
4	serve	*Harry is serving Ron and Luna ice cream.*	0.021	-0.19	.85
5	bake	*Ron is baking Hermione a cake.*	0.036	-0.40	.69
6	build	*Luna is building Harry a birdhouse.*	0.046	-0.47	.64
7	sell	*Luna is selling Ron some records.*	0.062	-0.68	.50
8	offer	*Harry is offering Ron some coffee.*	0.071	-0.59	.56
9	hand	*Luna is handing Ron some keys.*	0.074	-1.01	.32
10	read	*Ron is reading Hermione something.*	0.077	-0.85	.40
11	write	*Hermione is writing Harry a letter.*	0.086	-0.91	.37
12	give	*Luna is giving Harry presents.*	0.13	-1.24	.22
13	purchase	*Luna is purchasing Ron a laptop.*	0.13	-1.42	.16
14	get	*Harry got Ron a scarf.*	0.14	-1.54	.13
15	send	*Harry is sending Hermione a letter.*	0.15	-1.57	.13
16	heat	*Harry is heating Hermione some food.*	0.18	-1.86	.07
17	reserve	*Luna is reserving Ron a seat.*	0.20	-2.48	.02
18	display	*Harry is displaying Luna a trophy.*	0.22	-2.12	.04
19	acquire	*Hermione acquired Ron some medicine.*	0.25	-2.80	.008
20	present	*Luna is presenting Ron something.*	0.27	-2.82	.008

21	bequeath	*Harry is bequeathing the library money.*	0.35	-3.29	.002
22	apply	*Luna is applying Hermione makeup.*	0.35	-3.19	.003
23	deliver	*Harry is delivering Hermione the shopping.*	0.37	-3.83	<.001
24	announce	*Harry is announcing Hermione something.*	0.43	-4.57	<.001
25	whisper	*Luna is whispering Ron something.*	0.43	-4.24	<.001
26	describe	*Harry is describing Hermione a statue.*	0.47	-5.03	<.001
27	donate	*Harry is donating the school toys.*	0.48	-4.71	<.001
28	report	*Luna reported Ron the broken window.*	0.54	-6.15	<.001
29	warm	*Hermione warmed Ron some soup.*	0.56	-7.16	<.001
30	explain	*Hermione is explaining Harry something.*	0.60	-8.48	<.001

Appendix B: Regression models

Table B1. Experiment 1 model coefficient estimates.

DO ~ PD-bias + TrialType + Trial + Nativeness + SimilarBackgrounds + Proficiency
+ PD-bias:TrialType + PD-bias:Trial + TrialType:Trial + PD-bias:SimilarBackgrounds
+ TrialType:SimilarBackgrounds + Trial:SimilarBackgrounds + PD-bias:Nativeness
+ TrialType:Nativeness + Trial:Nativeness + SimilarBackgrounds:Nativeness
+ PD-bias:Proficiency + TrialType:Proficiency + Trial:Proficiency
+ SimilarBackgrounds:Proficiency + Nativeness:Proficiency + PD-bias:TrialType:Trial
+ PD-bias:Trial:Nativeness + TrialType:SimilarBackgrounds:Nativeness
+ Trial:SimilarBackgrounds:Nativeness + PD-bias:SimilarBackgrounds:Proficiency
+ TrialType:SimilarBackgrounds:Proficiency + Trial:SimilarBackgrounds:Proficiency
+ Trial:SimilarBackgrounds:Proficiency + PD-bias:Nativeness:Proficiency
+ TrialType:Nativeness:Proficiency + (1 + PD-bias + TrialType + Trial
+ SimilarBackgrounds + Nativeness + Proficiency | ItemID) + (1 + PD-bias + TrialType
+ Trial + SimilarBackgrounds + Nativeness + Proficiency | ParticipantID)

	Estimate	*SE*	*z* value	*p*
(Intercept)	1.74	0.39	4.46	<.001
PD-bias	-1.68	0.62	-2.69	.0071
TrialType	1.26	0.10	12.18	<.001
Trial	0.014	0.0020	7.17	<.001
Nativeness	0.76	0.26	2.93	.0034
SimilarBackgrounds	-0.24	0.38	-0.64	.52
Proficiency	0.57	0.48	1.21	.23
PD-bias:TrialType	0.30	0.24	1.28	.20
PD-bias:Trial	0.0022	0.0028	0.76	.45
TrialType:Trial	0.00087	0.00066	1.31	.19
PD-bias:SimilarBackgrounds	-0.079	0.26	-0.30	.76
TrialType:SimilarBackgrounds	-0.045	0.86	-0.52	.60
Trial:SimilarBackgrounds	-0.00065	0.0019	-0.34	.73
PD-bias:Nativeness	0.39	0.20	1.97	.049
TrialType:Nativeness	0.098	0.055	1.76	0.078
Trial:Nativeness	0.0040	0.0013	3.04	0.0024

SimilarBackgrounds:Nativeness	0.33	0.34	0.95	0.34
PD-bias:Proficiency	0.0043	0.28	0.015	0.99
TrialType:Proficiency	-0.015	0.078	-0.19	0.85
Trial:Proficiency	0.00023	0.0017	0.14	0.89
SimilarBackgrounds:Proficiency	0.55	0.61	0.90	0.37
Nativeness:Proficiency	0.21	0.33	0.64	0.52
PD-bias:TrialType:Trial	-0.0016	0.0020	-0.78	0.44
PD-bias:Trial:Nativeness	-0.0011	0.0015	-0.75	0.46
TrialType:SimilarBackgrounds:Nativeness	-0.089	0.072	-1.23	0.22
Trial:SimilarBackgrounds:Nativeness	0.0020	0.0013	1.55	0.12
PD-bias:SimilarBackgrounds:Proficiency	0.49	0.27	1.83	0.067
TrialType:SimilarBackgrounds:Proficiency	-0.14	0.096	-1.45	0.15
Trial:SimilarBackgrounds:Proficiency	0.0044	0.0020	2.21	0.027
PD-bias:SimilarBackgrounds:Proficiency	0.27	0.12	2.17	0.030
TrialType:Nativeness:Proficiency	0.072	0.051	1.43	0.15

Table B2. Experiment 1: Simple slopes for interactions.

	Estimate	*SE*	*z* value	*p*
PD-bias:Nativeness – slope"%&'()* for:				
• Nativeness=*M*!)#(+,-,** - 1*SD*	-2.20	0.65	-3.39	<.001
• Nativeness=*M*!)#(+,-,**	-2.27	0.55	-4.13	<.001
• Nativeness=*M*!)#(+,-,** + 1*SD*	-1.84	0.58	-3.16	<.001
Nativeness:Trial – slope!)#(+,-,** for:				
• Trial=*M*./()0 - 1*SD*	0.22	0.15	1.50	.13
• Trial=*M*./()0	0.39	0.19	1.99	.05
• Trial=*M*./()0 + 1*SD*	0.69	0.25	2.73	.01
Trial:Proficiency:SimilarBackgrounds – slope./()0 for:				
• Proficiency=*M*"/12(3(,-34 - 1*SD*				
o SimBackgrounds=*M*5(67)38 - 1*SD*	0.02	<0.001	7.12	<.001
o SimBackgrounds=*M*5(67)38 + 1*SD*	0.01	<0.001	3.19	<.001
• Proficiency=*M*"/12(3(,-34 + 1*SD*				
o SimBackgrounds=*M*5(67)38 - 1*SD*	0.01	0.01	1.34	0.18
o SimBackgrounds=*M*5(67)38 + 1*SD*	0.02	<0.001	5.04	<.001
PD-bias:Proficiency:Nativeness – slope"%&'()* for:				
• Proficiency=*M*"/12(3(,-34 - 1*SD*				
o Nativeness=*M*!)#(+,-,** - 1*SD*	-1.71	0.69	-2.49	.01
o Nativeness=*M*!)#(+,-,** + 1*SD*	1.39	0.64	-3.15	<.001
• Proficiency=*M*"/12(3(,-34 + 1*SD*				
o Nativeness=*M*!)#(+,-,** - 1*SD*	-2.92	0.76	-3.83	<.001
o Nativeness=*M*!)#(+,-,** + 1*SD*	-1.14	0.65	-1.75	.08

Table B3. Experiment 2 model coefficient estimates (Map distance model).

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

	Estimate	*SE*	*z* value	*p*
(Intercept)	1.76	0.37	4.71	<.001
PD-bias	-6.85	1.20	-5.69	<.001
TrialType	1.42	0.15	9.67	<.001
Trial	-0.0026	0.0013	-1.91	.056
MapDistance	0.012	0.24	0.50	.62
PD-bias:TrialType	0.68	0.39	1.73	.083
PD-bias:Trial	0.0019	0.0041	0.46	.65
TrialType:Trial	-0.0022	0.00049	-4.51	< .001
PD-bias:MapDistance	-1.17	0.46	-2.57	.010
TrialType:MapDistance	-0.13	0.11	-1.23	.22
Trial:MapDistance	-0.00043	0.00090	-0.48	.63
PD-bias:TrialType:MapDistance	0.24	0.22	1.12	.26

Table B4. Experiment 2: Simple slopes for interactions (Map distance model).

	Estimate	*SE*	*z* value	*p*
TrialType:Trial – slope$_{./()▯.49,}$ for:				
• Trial = $M_{./()▯}$ - 1*SD*	1.63	0.17	9.74	<.001
• Trial = $M_{./()▯}$	1.52	0.14	11.11	<.001
• Trial = $M_{./()▯}$ + 1*SD*	1.31	0.12	10.53	<.001
PD-bias:MapDistance – slope$_{"\%\&'()*}$ for:				
• MapDistance = $M_{:)9\%(*\#)-3,}$ - 1*SD*	-5.43	1.20	-4.52	<.001
• MapDistance = $M_{:)9\%(*\#)-3,}$	-6.01	1.10	-5.45	<.001
• MapDistance = $M_{:)9\%(*\#)-3,}$ + 1*SD*	-8.12	1.22	-6.67	<.001

Table B5. Experiment 2 model coefficient estimates (pre- to post-test difference score model).

DO ~ PD-bias + TrialType + Trial + Pre-Post_SameFriends + PD-bias:TrialType + PD-bias:Trial + PD-bias:Pre-Post_Friends + TrialType:Trial + TrialType:Pre-Post_Friends + Trial:Pre-Post_Friends + PD-bias:Trial:Pre-Post_Friends + (1 + PD-bias + TrialType + Trial + Pre-Post_Friends | ItemID) + (1 + PD-bias + TrialType + Trial + Pre-Post_Friends | ParticipantID)

	Estimate	*SE*	*z* value	*p*
(Intercept)	1.74	0.44	3.98	<.001
PD-bias	-7.54	1.63	-4.63	<.001
TrialType	1.53	0.16	9.30	<.001
Trial	-0.0029	0.0013	-2.31	.021
Pre-Post_SameFriends	-0.025	0.28	-0.089	.93
PD-bias:TrialType	0.00040	0.48	0.001	.99
PD-bias:Trial	0.0011	0.0037	0.31	.76
PD-bias:Pre-Post_Friends	0.80	0.37	2.14	.033
TrialType:Trial	-0.0018	0.00049	-3.63	.00029
TrialType:Pre-Post_Friends	0.092	0.089	1.03	.30
Trial:Pre-Post_Friends	0.00011	0.00084	0.14	.89
PD-bias:Trial:Pre-Post_Friends	0.0024	0.0019	1.27	.20

Table B6. Experiment 2: Simple slopes for interactions (pre- to post-test difference score model).

	Estimate	*SE*	*z* value	*p*
TrialType:Trial – slope ./()0.49, for:				
• Trial = *M* ./()0 - 1*SD*	1.51	0.17	9.05	<.001
• Trial = *M* ./()0	1.50	0.12	12.30	<.001
• Trial = *M* ./()0 + 1*SD*	1.38	0.13	11.01	<.001
PD-bias:MapDistance – slope "%&'()* for:				
• MapDistance = *M* :)9%(*#)-3, - 1*SD*	-6.74	1.46	-4.63	<.001
• MapDistance = *M* :)9%(*#)-3,	-6.83	0.95	-7.18	<.001
• MapDistance = *M* :)9%(*#)-3, + 1*SD*	-6.28	0.98	-6.39	<.001