Abstract

Colexification refers to the phenomenon of multiple meanings sharing one word in a language. Cross-linguistic lexification patterns have been shown to be largely predictable, as similar concepts are often colexified. We test a recent claim that, beyond this general tendency, communicative needs play an important role in shaping colexification patterns. We approach this question by means of a series of human experiments, using an artificial language communication game paradigm. Our results across four experiments match the previous cross-linguistic findings: all other things being equal, speakers do prefer to colexify similar concepts. However, we also find evidence supporting the communicative need hypothesis: when faced with a frequent need to distinguish similar pairs of meanings, speakers adjust their colexification preferences to maintain communicative efficiency, and avoid colexifying those similar meanings which need to be distinguished in communication. This research provides further evidence to support the argument that languages are shaped by the needs and preferences of their speakers.

Keywords: colexification, communicative need, experimental, artificial language, complexity, cognitive cost, expressivity, communicative cost

1 Introduction

When two or more functionally distinct senses are associated with a single lexical form in a given language, expressed by a single word, then these senses can be referred to as being colexified (François 2008). Colexification is more general than homonymy and polysemy (which also refer to a multiplicity of senses for a word), making no assumptions about the nature of the relations between the senses. Recognizing that a word lexifies more than one sense requires a means to determine what the minimal units of meaning are. This would be difficult to do based on just one language, but can and has been done utilizing systematic cross-linguistic comparison. For example, English has separate monomorphemic words for leg and foot, while many other languages use a single word to refer to the whole thing, e.g. Estonian (jalg), Irish Gaelic (cos) and Modern Hebrew (regel). This does not mean that referring to these concepts separately in these languages is impossible, but rather it may involve more complex compounds or expressions to describe them (e.g. Estonian jalalaba ‘foot’, lit. ‘wide part of the leg, leg-blade’). It does however suggest that leg and foot could potentially be a set of comparable, minimally distinct senses, which some languages colexify, while others do not.

Colexification effectively reduces the complexity of the lexicon, or a subdomain of the lexicon, by reducing the number of words needed to cover that space. A smaller set of words is easier to learn and remember — the associated “cognitive cost” is lower (cf. Kemp et al. 2018). But if a language, or a sub-domain of language such as the body parts lexicon, becomes too simple, then miscommunication becomes more likely. If the sub-domain is very complex and precise, then this error, or “communicative cost” is lower — but a language can only be so complex before it becomes unfeasible to learn.

A successful language therefore needs to optimize these pressures to be both simple enough to be learnable (Gasser 2004; Christiansen and Chater 2008; Smith et al. 2013; Kirby et al. 2015), but also meet the speakers’ requirements for expressive and sufficiently precise communication. This is known as the simplicity-informativeness trade-off (see Kemp and Regier 2012; Carr et al. 2020), illustrated in Figure 1A.
Kemp et al. (2018) argue that it is social and cultural communicative needs that modulate this perpetual optimization process. If a given domain is less relevant to users of a language, then a language may give way to the pressure of simplicity, and some sense distinctions may be lost or colexified. If something is important and part of frequent discourse, then it will likely be lexified so as to avoid error in communication — increasing informativeness at the expense of increasing complexity (see Figure 1B). The topics of informativeness, complexity and communicative need have been discussed in the context of a variety of domains, including expressions of color (Lindsey and Brown 2002; Gibson et al. 2017; Zaslavsky et al. 2019a), kinship (Kemp and Regier 2012), pronouns (Zaslavsky et al. 2021), numeral systems (Xu et al. 2020b), natural phenomena (Berlin 1992; Regier et al. 2016; Zaslavsky et al. 2020; Kemp et al. 2019), writing systems (Hermalin and Regier 2019; Miton and Morin 2021), and various morphosyntactic features (Haspelmath and Karjas 2017; Mollica et al. 2020). Similar adaptation and efficiency effects have also been observed in experimental settings with artificial languages (cf. Fedzechkina et al. 2012; Winters et al. 2015; Tinits et al. 2017; Nölke et al. 2018; Chaabouni et al. 2021; Guo et al. 2021).

In a recent study based on a large sample of colexifications from a database (Borin et al. 2013) of about 250 languages, Xu et al. (2020a) demonstrate that similar and associated senses (like FIRE and FLAME) are more frequently colexified than unrelated or weakly associated meanings (like FIRE and SALT for example), suggesting that this provides an important constraint on the evolution of lexicons. This work follows a line of research on the variability of lexicification patterns across languages of the world (e.g. Malt et al. 1999; François 2008; List et al. 2013; Majid et al. 2015; Srinivasan and Rabagliati 2015; Thompson et al. 2018). Xu et al. (2020a) also put forward a hypothesis that, beyond the tendency to colexify similar senses, language- and culture-specific communicative needs should be expected to affect the likelihood of colexification of similar concepts — such as SISTER and BROTHER, or ICE and SNOW — if it is necessary for efficient communication to distinguish them. The latter pair in particular was investigated by Regier et al. (2016), who used multiple sources of linguistic and meteorological data to show that languages spoken in colder climates are statistically more likely to distinguish ICE and SNOW, while languages spoken in warmer climates are more likely to colexify them (thus providing an empirical test to ideas going back to Sapir 1912; Whorf 1956). They argued this to be an example of lexicons being shaped by local cultural communicative needs, in this case which are in turn shaped by local physical environments.

Languages investigated in cross-linguistic typological studies (like François 2008; Regier et al. 2016; Xu et al. 2020a) have evolved to be the way they are over time, through incremental changes in how generations of speakers produce utterances by assigning signals to meanings. In this paper, we employ an artificial language experimental setup to probe lexicification decisions by individual speakers, with the aim of investigating the discourse-level generative mechanisms that in natural languages would eventually lead to the observed cross-linguistic patterns.

In Experiment 1 we investigate how colexification choices play out in a dyadic communicative task when commu-
nichicative needs are uniform, and confirm that in this neutral condition, speakers do indeed prefer to colexify similar
ccepts. Comparison of this baseline to a condition where colexification of similar meanings would impede com-
munication shows a reduced tendency to colexify similar meanings, providing evidence in line with the hypothesis
proposed by Xu et al. (2020a), that communicative needs of speakers modulate colexification dynamics beyond con-
ceptual similarity.

In Experiment 2 we replicate Experiment 1 with a crowdsourced participant sample. We continue using crowd-
sourcing to test a variant of the original hypothesis in Experiments 3, and in Experiment 4 relax the constraints on
the signal space we provide our participants in order to further explore how communicative need operates under
reduced constraints on complexity. The results of these follow-up experiments support the findings of Experiment
1. The implications of the findings and pathways to future research are further discussed in Section 7.

2 Experimental methodology

All our experiments use a dyadic computer-mediated communication game setup (cf. Scott-Phillips and Kirby 2010;
Galantucci et al. 2012; Winters et al. 2015; Kirby et al. 2015) to investigate how similarity and communicative need
interact to shape colexification choices by language users. In this section, we first provide a general overview of the
methods and analysis we used in all four of our experiments, before setting out each experiment in turn in more
detail in later sections; unless stated otherwise, the setup for conducting a given experiment is as described in the
general methods section here.

In all our experiments, pairs of participants are faced with the task of communicating single-word messages using a
small set of artificial words. In order to successfully do so, they must initially negotiate the meanings for the signals
through trial and error. The task was introduced to participants as an "espionage game" where the usage of secret
codes is justified as keeping the messages hidden from the enemy. The experiment interface was implemented as a
web app in the R Shiny framework (Chang et al. 2020).

2.1 Procedure

All our experiments consist of 135 rounds. At each round, one participant is the sender and the other is the re-
ciever; these roles switch after every round. The sender is shown two meanings, represented by English nouns (see
Section 2.2), and is instructed to communicate one of those meanings to the receiver, using a single word from an
artificial lexicon (see Section 2.3). The receiver is then shown the same pair of meanings (in random order) and the
sender’s signal, and has to guess which of the two meanings the signal represents. After taking a guess, both par-
ticipants are shown an identical feedback screen which informs them whether the receiver guessed correctly. See
Table 1 for an illustration. The game ends with a screen showing both participants their total score, asking them for
optional feedback, and leaving them with instructions on how to claim their monetary reward.

The participants never see more than two meanings on the screen at any time. In each game, there are 10 meanings,
among them 3 "target pairs" consisting of two highly similar meanings, e.g. motor and engine. The remaining 4
meanings serve as distractors that have low similarity scores to all other meanings, including the targets (see below
for details). The signal space consists of 7 artificial words such as fuwo or qohe. Since there are fewer signals than
meanings, participants must colexify some meanings. We assume it takes a while to establish stable meaning corre-
spondences, so we consider the first 1/3 of the rounds as a "burn-in" phase, and only analyse the final 90 rounds of
the experiment.

2.2 Stimuli: meanings

The meanings to be communicated are English common nouns of 3 to 7 characters in length, drawn from the Sim-
lex999 dataset (Hill et al. 2015), which consists of pairs of words and their crowdsourced similarity judgments. We
use Simlex999, as it was built for evaluating models of meaning with the explicit goal of distinguishing genuine sim-
ilarity (synonymy) from associativity (the dataset incorporates a subset of the USF Free Association Norms for that
Table 1: An example of how the communication game works. Each row corresponds to a change in the screens displayed to the players, the cells in the columns indicate what is currently being shown to each of the two Players. Interactive elements of the graphical user interface are highlighted here in bold font. In Round 1, it is Player 1’s turn to signal and Player 2’s turn to guess the meaning of the signal. The meanings, here motor and essay, are being displayed to both Players, but in randomized order. After the feedback screen has been shown (informing the players if the guess was correct), the roles are switched, and the next round begins.

| Player 1                        | Player 2                        | Comment                                      |
|--------------------------------|--------------------------------|----------------------------------------------|
| Motor essay                     | Essay motor                     | Round 1 starts                               |
| Communicate motor using...      | Waiting for message...          | Player 1 is the sender,                      |
| nepa qohe lali fuwo nire ruqi lumu |                               | clicks on “fuwo”                             |
| Sent motor using fuwo           | Essay motor                     | Player 2 (receiver)                          |
| Stand by...                     | Message: fuwo                   | clicks on “motor”                            |
| Correct guess! Motor = fuwo     | Correct guess! Motor = fuwo     | Feedback screen                              |
| Threat purse                    | Threat purse                    | Round 2 starts                               |
| Waiting for message...         | Communicate threat using...     | Player 2 is the sender,                      |
| nepa qohe lali fuwo nire ruqi lumu |                               | clicks on “qohe”                            |

2.3 Stimuli: signals

For each dyad in Experiments 1-3, we generated a set of 7 signals, according to the following constraints (Experiment 4 has an extended signal space of 10 signals). Each signal had a length of 4 characters, and was composed of 2 consonant-vowel syllables, constructed from a set of consonants \{f, q, w, t, p, s, f, h, n, m, r, l\} and vowels \{a, e, o, u, i\}. We further constrained the artificial language so that the initial letters of the signals would not overlap with any initial letters of the meanings (English nouns) in a given stimulus set. We used a large English word list to make sure there was no overlap between the artificial signals and actual English words, and furthermore made sure all signals were at least 3 edits distant from the meanings (the English nouns) in the same game, as well as from other signals in the game (see Figure 2.B).
2.4 Experimental manipulation of communicative need

The experiments have two conditions: the baseline or control condition, where communicative need is uniform, and the target condition where we manipulate communicative need, creating a situation where colexifying certain (similar) concepts would hinder the accurate exchange of messages. This applies to all experiments except for Experiment 3 which only has a (modified) target condition (details in Section 5).

In the baseline condition, the distribution of meaning pairs (e.g. DRIZZLE-RAIN, STYLE-FASHION, PAYMENT-BULL, RAIN-PAYMENT, RAIN-FASHION) is uniform — each possible combination is shown to the participants exactly 3 times. Based on cross-linguistic tendencies (cf. Xu et al. 2020a), in this condition we would expect participants to colexify similar meanings. In the target condition, all possible meaning pair combinations still occur in the game, but crucially, we manipulate the occurrence frequencies so that the target (similar-meaning) pairs occur together more often than the distractor pairs. The target pairs (e.g. DRIZZLE-RAIN, STYLE-FASHION) are shown 11 times each, and the pairs consisting of distractor meanings (e.g., PAYMENT-BULL) 5 times each. Pairs consisting of a meaning from a target pair plus another meaning are shown 2 times (e.g. RAIN-PAYMENT or RAIN-FASHION).

The non-uniform distribution of meaning pairs in the target condition entails that to communicate successfully, participants are required to select signals which allow their partner to differentiate between DRIZZLE and RAIN 11 times, but are only required to differentiate between RAIN and PAYMENT 2 times. The increased co-occurrence of similar meanings in the target condition simulates communicative need. If a pair of similar meanings never or seldom needs to be distinguished, then it is efficient to colexify them, both from a learning and communication perspective. In contrast, if the communicative context often requires disambiguating between two similar meanings or referents — such as RAIN and DRIZZLE in a culture obsessed with talking about poor weather — then colexifying them as RAIN or blending them into something like rainzzle would obviously be detrimental to communicative success. We expect this to be reflected in the outcomes of the target condition: participants should avoid colexifying the similar concepts, as that would make it difficult to distinguish them and hinder communicative efficiency.

These pairs are displayed in a randomized order, but randomized separately for the burn-in (first third) and post-burn-in part of the game. In other words, we make sure that if RAIN-FASHION is supposed to appear 3 times over the course of the game, then it will appear once in the burn-in and twice afterwards. Of course not all values are divisible by 3, so the distribution of stimuli between burn-in and the rest of the game is not perfect, but we optimize it to be as good as numerically possible.\(^2\)

\(^2\)The randomized order could in principle cause a situation where some meaning comes up in multiple successive rounds, or, on the contrary, does not appear for a while. However, since there are only 10 meanings in each game, such runs should be relatively rare and therefore have limited systematic affect on our results.
The meaning pair frequency distribution used in both conditions ensures that individual meanings all occur exactly the same number of times (which is 27). It is necessary to control for individual frequencies in this manner, as simply making target pairs more frequent would also mean making the individual meanings in those pairs more frequent than the distractors. This would introduce a confound: another reasonable hypothesis could be that it is occurrence frequency that drives colexification (i.e. colexifying frequent meanings is preferred, or avoided; cf. Xu et al. 2020a), over and above communicative need or word similarity.

2.5 Participants and exclusion criteria based on communicative accuracy

All our experiments were approved by the Ethics Committee of the School of Philosophy, Psychology and Language Sciences of the University of Edinburgh. All participants provided informed consent on the online game platform prior to participating and were compensated monetarily for their time. We do not include all the data in our analysis, as the communicative accuracy of some dyads is at or near random chance. Low accuracy in guessing the meanings of the signals transmitted indicates a given dyad did not manage to converge on a lexicon they could use with reasonable success to communicate — such data are not informative in terms of our research question.

We calculate the communicative accuracy of a dyad as the percentage of correct guesses out of all guesses made in the game after the burn-in period (see Section 2.1). We make the assumption that dyads with a total accuracy score above 59%, i.e. guessing correctly in 53/90 of the trials, were unlikely (binomial \( p = 0.036 < 0.05 \)) to be signaling or guessing randomly. Dyads scoring below this threshold were excluded from analysis; all excluded dyads still received full payment. The instructions also included a request not to make any notes or write anything down during the experiment, and participants were asked at the end of the experiment whether they had taken written notes; no participants were excluded on the basis of having admitted to taking written notes.

2.6 Quantifying colexification

We are interested in what participants do with the target meaning pairs. If they colexify target meanings in the baseline condition (e.g. use the same signal for rain and drizzle), that would support the cross-linguistic findings of Xu et al. (2020a), that similar meanings are most often colexified. If participants avoid colexifying target meaning pairs in the target condition, then this would support the hypothesis we intend to test, that given high enough communicative need to distinguish similar meanings, they will not be colexified (see Figure 3 for an illustration). We therefore need a method to operationalize these signal-meaning associations in a manner that would allow us to test our hypothesis via rigorous statistical analysis, while preferably also capturing possible changes in lexification choices over the course of each game.

Data from each game is converted into a new dataset suitable for statistical analysis, consisting only of colexifications involving one of the target meanings. We introduce a new binomial variable, “colexification with synonym”, and

![Figure 3: Example signal-meaning matrices from two games in Experiment 1. The vertical black bars highlight the similar-meaning target pairs. Counts in the cells indicate how many times in total each signal was used to communicate each meaning in a given game (larger values have darker cells). In the baseline condition game (left), the players have chosen to colexify similar meanings such as shore and coast. In the target condition one (right), similar meanings often need to be distinguished from one another. The players have responded to this pressure by colexifying rain with style and drizzle with organ instead.](image-url)
assign values to it using the following procedure. We start by iterating through all the messages sent in the post-burn-in part of the game that signal one of the target meanings. We check if the most recent usage of a given signal by the same player in the same game lexified another meaning, and if that meaning belongs to the same target pair or not. For example, given Player 1 signals rain using pamí at round number 53, we check the most recent usage of pamí by Player 1 in all preceding rounds of the game where Player 1 was the signaler. Let’s say this happened at round 39, and Player 1 used pamí to also signal rain. This does not count as a colexification — it’s the same meaning — so this case is not added to the new dataset. At some point later in the game, at round 127, Player 1 signals rain using pamí again. The previous usage of pamí by Player 1 was round 121, where they used it to signal style. This counts as a colexification, and so this case is added to the new dataset. As style is not a synonym of rain, the value assigned to the new “colexification with synonym” variable is “no”. If instead of style, the most recent meaning signaled by pamí had been drizzle — a highly similar meaning belonging to the same target pair with rain — then this value would be set as “yes”.

This procedure is repeated for all dyads that score above our previously described accuracy threshold. We do not include the data from the burn-in period (first 1/3 of the game) in the statistical analysis of the results, but do take the burn-in into account when checking for most recent usage of signals, as players do not start from a clean slate after the end of the burn-in, but already have some experience with the language and likely at least some signal-meaning associations in place. This is not the only way to operationalize and analyze such data, but it does provide insights into participant behavior while allowing for explicit comparison of the conditions in terms of the likelihood of target pair colexification, and for modeling changes in lexification over the course of the game.

The distributions of meanings occurring in the game — the input data for the participants — are carefully balanced, and all games consist of exactly 135 rounds, as described in Section 2.4. Note however, that under this operationalization, the amount of output data yielded by different dyads varies somewhat (see Figure 4). The exact number of data points per dyad depends on their colexification behavior: if they converge on a system where most of the target meanings get their own unique signal, then there will be fewer colexifications and as such fewer data points. If they colexify the target meanings, either with their near-synonyms or with unrelated meanings, then there will be more colexifications to analyze. This is by design, as we prioritize balancing the input, and is controlled for in our statistical analyses below, ensuring that the overall results are not driven by a few data-rich dyads.

2.7 Statistical modeling approach

We used mixed effects logistic regression models (using the lme4 package in R; Bates et al. 2015) to model all the datasets derived using the approach described in Section 2.6 above. The model has the same effects structure across all experiments. Condition (baseline or target) is treatment-coded, with the baseline condition set as the reference level (with the exception of Experiment 3; this is discussed in the relevant section). Round number (scaled to a range of $[-1, 1]$) is centered at round 68, the middle of the game. The binomial colexification variable is set as the response, predicted by condition, round number, and their interaction. The interaction with round accounts for possible changes in lexification preferences. To account for the repeated measures nature of the data and the fact that the number of data points per dyad is variable, we set random intercepts for meaning and sender (the latter nested in dyad) and a random slope for condition by meaning.3 A full random effects structure would be desirable, but could not be included due to model convergence issues.

3 Experiment 1

3.1 Methods

The setup of our first experiment matches the overview in Section 2: there is a baseline and a target condition, and in both cases participants are provided with 7 signals which they can use to lexify the 10 meanings. We operationalize the data for statistical modeling using the procedure as described in Section 2.6 and illustrated in Figure 4.

3 In lme4 syntax: colexification ~ condition * round + (1+condition|meaning) + (1|dyad/sender)
3.1.1 Participants

The pool of participants for Experiment 1 consists of students of the University of Edinburgh, recruited through the university’s CareerHub portal and departmental mailing lists. Participants were only allowed to complete the game once. All participants identified as native or near-native speakers of English. 46 dyads finished the experiment, 41 of which were included in the analysis (20 baseline, 21 target condition dyads; 82 participants in total). Data from 5 dyads were discarded, either because they explicitly admitted to misunderstanding the game instructions in the feedback form (1 dyad) or because of low communicative accuracy that likely resulted from random guessing (4 dyads; see Section 2.5 above for discussion of our exclusion criterion).

Figure 4: The colexification data across all dyads in Experiment 1, operationalized using the procedure described in Section 2.6. Each row is a dyad, labeled by its number. Each tile corresponds to a message that contains a target meaning, which is being colexified with another meaning using the same signal: dark blue if with a related meaning, e.g. /r.sc/a.sc/i.sc/n.sc/hyphen.sc/d.sc/r.sc/i.sc/z.sc/z.sc/l.sc/e.sc/, light blue if with an unrelated meaning. Trials are shown in order, but those involving distractor meanings, or where the given signal was last used to lexify the same meaning, are excluded (therefore the final number of data points per dyads vary, despite all games having the same 135 rounds). The difference between conditions is visually apparent: the baseline condition on the left has more dark blue, indicating colexification of similar meanings, while the target condition on the right has more light blue tiles, indicating colexification of dissimilar meanings.

3.2 Results

The data processing procedure described in Section 2.6 yields a dataset of 1214 cases (597 in the baseline, 617 in the target condition), a median of 30 per dyad (illustrated in Figure 4). In the mixed effects regression model (see Section 2.7), the dependent variable is the derived binomial colexification measure. The fixed effects consist of the condition, round number, and the interaction of the two. The interaction is included to account for possible changes over the course of the game (cf. Figure 4). Random effects are included to control for repeated measures of meanings, dyads and players, as outlined in Section 2.7. In the model described in Table 2, the intercept value of −0.22 stands for the log odds of target meaning pairs being colexified, in the baseline condition, mid-game (i.e. a probability of 0.45; recall that we centered round number). By mid-game, the model is not picking up a significant difference between the conditions (p = 0.3). Each passing round does increase the probability of colexification in the baseline condition (β = 1.02, p = 0.0001). Importantly, the interaction between condition and round is in the opposite direction (β = −1.18, p = 0.0015), indicating participants were less likely to colexify related meanings in the target condition, where these related meanings frequently co-occurred and needed to be distinguished from one another. By the end of a game, the estimated average probability of colexifying target pairs is only 0.29 in the target condition, compared to 0.69 in the baseline condition. In short, these results support our communicative need hypothesis (for further illustration, see Figure 5, the leftmost columns).

The experiment was initially planned as an in-person lab study that would have commenced in March 2020. The Covid-19 pandemic rendered this impossible, so we reworked it into an online experiment, but continued with the originally intended participant pool of student recruits. We made use of SimpleSignUp (https://github.com/jwcarr/SimpleSignUp) to schedule the participants. For the later experiments we moved to using participants sourced from Amazon Mechanical Turk.
Table 2: The fixed effects from the mixed effects regression model applied to data from Experiment 1, predicting the value of the colexification variable (reference level: "no") by the interaction between condition (reference: baseline) and round number. The latter is included to account for progress over the course of the experiment. The results indicate a statistically significant difference in participant behavior between the two conditions, supporting the hypothesis that communicative need can drive lexicification preferences above and beyond conceptual similarity.

3.3 Discussion

Human memory is not infinite, and neither is time that can be used for learning. An unlimited number of signals or signal-meaning associations cannot be stored in the brain. We emulated these natural conditions in our artificial communication experiment by providing the participants with a signal space that is smaller than the provided meaning space. The results from the baseline condition provide experimental support for the cross-linguistic findings of Xu et al. (2020a) — that people indeed tend to colexify similar meanings. Yet when faced with a situation where there is elevated communicative need to distinguish certain meaning pairs more often than others, people are more likely to colexify other pairs or clusters of meanings to maintain communicative efficiency — even if this requires colexifying unrelated meanings. This in turn supports the hypothesis suggested by Xu et al. (2020a), and is in line with previous research on communicative need in general (cf. Section 1).

It should be noted that this setup possibly puts a heavier cognitive load on the participants in the target condition. In the baseline condition, participants can colexify similar meanings (like RAIN and DRIZZLE) without paying an additional communicative cost. It is probably safe to assume similarity-driven pairings are easier to remember. In the target condition, participants are encouraged to colexify meanings which are maximally dissimilar by design (e.g., DENTIST and FASHION). In that sense, Experiment 1 constitutes a strong test of the communicative need hypothesis — we predict that given high enough communicative need, speakers would even colexify unrelated meanings rather than sacrifice communicative efficiency. However, actual differences in average communicative accuracy in Experiment...
ment 1 turn out to be negligible. Figure 6 illustrates this, as well as the accuracy levels of the rest of the experiments, which we will discuss further in the next sections. There was also no difference in game length (on average 29 minutes in both conditions).

Figure 6: Communicative accuracy of all dyads across all experiments. Each notch is one dyad, the wider bars are medians. Target conditions (those with manipulated communicative need) are in darker blue. The threshold of 0.59, that we set to filter out dyads which likely played by the random button smash strategy, is shown as a gray segmented line. The student dyads (Experiment 1) had on average higher accuracy than the crowdsourced participants in the rest of the experiments, but all conditions included some dyads that scored very low, as well as some that scored very high.

Regardless, we will still explore the weaker hypothesis in a follow-up experiment, where participants are provided with less frequently co-occurring but still similar meanings to colexify (Experiment 3 in Section 5). We will also describe an experiment with an expanded signal space (Experiment 4 in Section 6). However, in the next section, we will first replicate the initial study on a different, slightly larger sample of participants.

4 Experiment 2

Experiment 2 is a replication of Experiment 1, where we recruited participants from Amazon Mechanical Turk; all other details of the experimental design and analyses are the same. As our replication below demonstrates, the behavior of these two samples in terms of the research question is very similar, but the samples differ somewhat in terms of average communicative accuracy.

4.1 Methods

4.1.1 Participants

We restricted participation to Mechanical Turk workers based in the United States to have a sample roughly comparable sample to Experiment 1 (i.e. largely English-speaking). In this and the following experiments, we only accepted workers with a history of at least 1000 successfully completed tasks and a 97% or higher approval rate. Furthermore, we used the qualifications system of Mechanical Turk to make sure no worker participated more than once (within a single experiment or across Experiments 2–4). As before, all participants provided informed consent on the online game platform prior to participating and were compensated monetarily for their time.

79 dyads finished the experiment, 53 of which were included in the analysis (26 baseline, 27 target condition dyads; 106 participants in total). Data from 26 dyads was discarded, 24 because of low communicative accuracy and 2 due to suspected cheating (see below). Communicative accuracy turned out to be lower on Mechanical Turk than in our student sample, with many players operating at or even below random chance (see Figure 6 above in Section 3.3). It is unclear to us why the rejection rate was so high, and this may reflect the difficulty of our communication task relative to other tasks our participants were completing around the same time, although anecdotally we know of

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6The estimated probability of making a correct guess is 0.84 in the target condition compared to 0.88 in the baseline ($p = 0.12$), based on a mixed effects logistic model, predicting correctness of guess by condition, with random slopes and intercepts for meaning and dyad (only taking into account the post-burn-in part of each game, and excluding dyads with overall accuracy below 59%, as described above).
other researchers who had problems with data quality on Mechanical Turk in the summer of 2020. There is also a
greater difference in communicative accuracy between the baseline and target condition in Experiment 2, compared
to Experiment 1, possibly due to the heavier cognitive load of the target condition compared to the baseline, discussed
in Section 3.3.

We manually flagged 2 dyads as suspicious, on the basis that they achieved near-perfect accuracy while sending ap-
parently random signals. This could potentially be one person using two Mechanical Turk accounts to play against
themselves, or two workers communicating outwith the game interface. After discovering these anomalies, we manu-
ally inspected data from high-accuracy dyads in all experiments. It was also not uncommon for participants recruited
via Mechanical Turk to simply drop out mid-game, effectively canceling the experiment for their dyad partner as well;
these dyads were treated as having withdrawn consent and their data was not analyzed.

4.2 Results

The analysis procedure for Experiment 2 is identical to that of Experiment 1: we operationalize colexification (see
Section 2.6; yielding a dataset of 1659 cases, a median of 32 per dyad) and apply the same mixed effects logistic
model (Section 2.7). Experiment 2 successfully replicated the results of Experiment 1 with the condition-round
interaction coefficient being significantly negative ($\beta = -0.66$, $p = 0.03$; see Table 3; refer back to Figure 5 above for
visual comparison). This value indicates participants were again less likely to colexify related meanings in the target
condition which simulated communicative need (average probability of 0.46 by the end of the game), compared to the
baseline condition, where there was no such pressure, and where participants more often colexify related meaning
pairs (0.72 probability).

| colexification                          | Estimate | SE  | z    | p    |
|----------------------------------------|----------|-----|------|------|
| intercept (baseline condition, mid-game) | -0.2     | 0.29| -0.68| 0.49 |
| + condition (target)                   | -0.44    | 0.36| -1.24| 0.22 |
| + round                               | 1.14     | 0.24| 4.82 | <0.01|
| + condition (target) x round           | -0.66    | 0.31| -2.11| 0.03 |

Table 3: The fixed effects from the mixed effects regression model applied to data from Experiment 2, predicting the
value of the colexification variable (reference level: "no") by the interaction between condition (reference: baseline)
and round number. The results replicate those of Experiment 1.

4.3 Discussion

The successful replication gives us additional confidence in the overarching hypothesis concerning the role of com-
municative need in lexification choices. We now turn to two more experiments to gain a better understanding of
how communicative need and simplicity preferences shape the behavior of our participants, testing a weaker version
of the communicative need hypothesis in Experiment 3 and relaxing the signal space constraint in Experiment 4.

5 Experiment 3

As discussed in Section 3.3, the target condition in Experiments 1–2 pushes participants to colexify meanings which
are highly dissimilar (recall Figure 4). In that sense, it tested a strong version of our hypothesis, that the need to
maintain communicative efficiency would outweigh the awkwardness of colexifying unrelated concepts, e.g. BULL
and FASHION (which would amount to homonymy in natural languages, something which has been shown to hinder
lexical retrieval and processing; cf. Klepousniotou 2002; Beretta et al. 2005). Here we explore an alternative, possibly
more natural target condition, where participants can avoid colexifying target meanings by colexifying non-target
meanings which are similar to one another.
5.1 Methods

5.1.1 Participants

The sample is similar to Experiment 2; we source participants from Mechanical Turk, applying the same restrictions as in Experiment 2. 52 dyads finished the experiment, 27 of which were included in the analysis (54 participants in total). Data from 25 dyads was discarded, 23 because of low communicative accuracy and 2 due to suspected cheating (see Section 4.1.1). The average accuracy is similar to what we observed in Experiment 2 (see Figure 6).

5.1.2 Procedure

In this experiment, there is only a single, target-type condition, i.e. one where communicative need is manipulated. The meaning space still consists of 10 meanings, with 3 high-similarity target meanings pairs and 4 distractors. Crucially, here the distractors also form 2 similarity pairs. For example, in Experiment 3, a meaning space might consist of \texttt{bag, purse, drizzle, rain, author, creator, danger, threat, journey, trip} (target meaning pairs underlined). Recall that distractor pairs only co-occur (i.e. need to be distinguished) 5 times over the course of the game, while target pairs co-occur 11 times in the target condition. Colexifying the distractors is now incentivized not only by their lower co-occurrence but also their semantic similarity. Doing so would allow participants to reserve more unique signals to distinguish target meanings, the ones that do co-occur often and need to be distinguished. All other parameters are identical to Experiments 1 and 2.

5.2 Results

To obtain a point of comparison, we combine the data collected in this experiment with the baseline and target condition data from Experiment 2 (totaling 2527 cases, of those 868 from the weaker hypothesis target condition of Experiment 3). We fit a mixed effects logistic regression model with the same structure as before, but set the new, “weaker” target condition as the reference level for the condition effect. We find that as with the previous target condition, there is still a significant difference from the baseline condition (the condition \( \times \) round interaction \( p = 0.002 \)). However, participant behavior does not differ from the target condition of Experiment 2, i.e., our manipulation of the meaning space did not elicit a meaningful difference (\( p = 0.29 \), see Table 4, and Figure 5 above).

| colexification - condition | Estimate | SE  | z    | p   |
|---------------------------|----------|-----|------|-----|
| intercept (weaker target condition, mid-game) | -0.22 | 0.25 | -0.9 | 0.37 |
| + condition (baseline)     | 0.04     | 0.34 | 0.12 | 0.9  |
| + condition (target)       | -0.39    | 0.34 | -1.16| 0.24 |
| + round                    | 0.14     | 0.2  | 0.68 | 0.49 |
| + condition (baseline) \( \times \) round | 0.95 | 0.3  | 3.11 | <0.01 |
| + condition (target) \( \times \) round | 0.3     | 0.29 | 1.06 | 0.29 |

Table 4: The fixed effects from the mixed effects regression model applied to combined data from Experiments 2 and 3, predicting the value of the colexification variable (reference level: “no”) by the interaction between condition (reference: new weaker target condition) and round number. Participant behavior does not differ significantly between the original target condition and the new weaker-hypothesis target condition.

5.3 Discussion

We once again observe the same general trend (echoing the cross-linguistic findings of Xu et al. 2020a) that participants colexify any similar-meaning pairs even if it causes the occasional miscommunication, but change their behavior if that cost becomes too high, as evidenced once more by the significant difference between the baseline condition of Experiment 2 and the new target condition of Experiment 3. We also expected that participants would colexify target pairs even less often than in the target conditions of Experiment 2, since alternative similar pairs were
available among the distractors, removing one of the assumed barriers to avoiding colexifying target pairs. The data
does not support this hypothesis; we see the same (relatively low) rate of colexification of target pairs in our new
data. One explanation may be that the difference between co-occurrence frequencies is simply not stark enough for
participants to see a benefit (consciously or subconsciously) in colexifying the distractors — and there is still the
option to colexify target-distractor pairs, which co-occur less often (twice each, versus the five of the distractor-only
pairs).

6 Experiment 4

This follow-up to the initial study explores the effect of removing constraints on the signal space. In all the previous
experiments, participants were provided 10 meanings but only 7 signals to work with. This meant some meanings
would be colexified, either accidentally (leading to lowered communicative accuracy) or systematically (allowing for
higher accuracy). Here, we remove this requirement to colexify, with the aim of gaining a better understanding
of participants behavior when this central component of our experimental setup is relaxed. We expect one of two
outcomes. Participants may well behave more or less the same as in Experiments 1–2, making use of a limited number
of signals and colexifying some meanings — after all, 7 signals should be easier to learn and remember than 10.
Alternatively, if 10 signals is not too unmanageable, participants may forgo colexification altogether: this would
eliminate the difference between baseline and target condition outcomes.

6.1 Methods

6.1.1 Participants

We continue using participants from Mechanical Turk as before. 91 dyads finished the experiment, 52 of which were
included in the analysis (104 participants in total). Data from 39 dyads was discarded because of low communicative
accuracy.

6.1.2 Procedure

This setup is the same as that of Experiment 1 and 2: there is a baseline and a target condition, and the meaning
space and its associated occurrence distributions are arranged as discussed in Section 2.1). The single difference is
that participants are not forced to colexify any meanings, as the size of the signal space is 10, equal to the meaning
space (illustrated in Figure 7).

Figure 7: Signal-meaning matrices illustrating the no-pressure baseline (left) and target condition (right) of Experiment 4. Above
average accuracy dyads are used again for visualization purposes, but their behavior is reflective of the average tendencies in
this experiment. Here, participants make use of more signals in both conditions and generally colexify less than in previous
experiments.
6.2 Results

The results of applying the mixed effects logistic regression model to the data from Experiment 4 (1306 cases after operationalizing colexification) show that when the requirement to colexify is removed, by allowing a larger signal space, the difference between the baseline and target condition disappears (\( p = 0.37 \), see Table 5; see also Figure 5 back in Section 3 for a visualization across all conditions). Communicative accuracy is roughly the same as in the other Mechanical Turk samples (recall Figure 6).

| colexification | Estimate | SE  | z    | \( p \) |
|----------------|----------|-----|------|---------|
| intercept (no-pressure baseline condition, mid-game) | -0.73 | 0.27 | -2.74 | <0.01   |
| + condition (target) | 0.24 | 0.36 | 0.68 | 0.5     |
| + round | 0.19 | 0.24 | 0.78 | 0.44    |
| + condition (target) \( \times \) round | -0.3 | 0.33 | -0.89 | 0.37    |

Table 5: The fixed effects from the mixed effects regression model applied to data from Experiment 4, predicting the value of the colexification variable (reference level: “no”) by the interaction between condition (reference: baseline) and round number. Here, in contrast to previous experiments, the conditions yield similar results.

6.3 Discussion

When the signal space is larger, the difference between the baseline and the target condition disappears. Participants behave in the baseline condition similarly to participants in the target conditions, avoiding colexification of the target pairs. We also quantified signal usage entropy for all dyads across all conditions. The results show that in the no-pressure conditions with the expanded signal space, in absolute terms more signals were used on average (Figure 8). In relative terms, signal usage in Experiment 4 was not that different from the previous experiments, with some dyads making use of most of the signal space (notches close to the limit lines in Figure 8) and others being more conservative and colexifying instead.

Figure 8: Signaling entropy across all dyads (blue notches), in the post-burn-in part of the game. Low values on the vertical axis indicate fewer signals were consistently used, higher values indicate a larger variety of signals were used by a given dyad. Overlapping values are pushed slightly aside horizontally to ensure visibility. The black bars represent medians. The gray vertical lines at 1.9 and 2.3 indicate maximal entropy given the size of the signal spaces. Dyads in Experiment 4 generally made use of most of the extended signal space (10 instead of 7), setting it apart from the rest of the experiments.

It appears participants favored the effort of remembering a few extra signals over having a simpler but more ambiguous language. While the signaling options are visible in the game at all times, the entire meaning space is never revealed all at once (recall Table 1). Participants here seem to have picked up on the relative abundance of the signals nevertheless, and unlike in the weaker hypothesis condition, participants did not colexify similar meanings to the same extent. These results solidify the original findings of Experiment 1 and 2: the size of the signal space clearly makes a difference in participant behavior and allows for making inferences into speakers’ lexification preferences. Naturally, our signal spaces are tiny compared to real lexicons that humans are able to memorize, not least because our signal space is designed for a lexicon that is to be conventionalized and used in the span of about half an hour. And like in natural languages, the complexity of the lexicon, the number of signals, has a limit. It is just that this limit
is cognitive in nature in the real world and appears to be largely artificial (i.e., driven by our constraint on the size of the signal space) in our experiments.

7 General Discussion

Our research provides evidence that speakers’ communicative needs affect their lexification choices, validating the viability of the mechanism hypothesized based on large-scale cross-linguistic studies (cf. Kemp et al. 2018; Xu et al. 2020a). Our research makes this connection explicit, and describes an experimental paradigm to test such hypotheses on the level of individual discourse — in comparison to previous research focusing on the level of population consensus based on data such as dictionaries, grammars, and corpora (e.g. Ramiro et al. 2018; Xu et al. 2020a; Mollica et al. 2020). Below, we sketch some extensions to the experimental paradigm established here that we feel would be worthwhile to look into in order to gain a better understanding of the role of communicative need, similarity and associativity, and the formation of lexicons in general.

7.1 Extensions and implications

Future research could look into a number of aspects and parameters of the communicative need game, beyond the exploration in our two follow-up studies (Experiments 3 and 4). In terms of setup and procedure, we chose what we assumed would be a reasonably-sized meaning space for an experiment of this length, but this, as well as the length itself, are of course arbitrary, as are the chosen co-occurrence distributions that emulate the pressure of communicative need. It would be interesting to see both the effect of stronger and weaker pressures than employed here. Need could also be gradually increased over the course of a longer game, to probe the strength of the pressure required for participants to modify an already established artificial communication system (which would loosely correspond to natural language users changing their language over their lifespan; cf. Sankoff 2018).

In this study, we chose direct similarity or synonymy as the semantic relationship to explore. Xu et al. (2020a) show that conceptual associativity (i.e. the car, engine type) also correlates with cross-linguistic collexification patterns, roughly to the same extent. It would be interesting to use our paradigm to investigate whether dyads preferentially collexify based on associativity or similarity. Another possible predictor, related to associativity, could be co-occurrence probability (or mutual information), which could be inferred from a large text corpus and then tested experimentally. Differences between more fine-grained relationships like register-varying synonymy (abdomen, belly) and hyponymy (bag, purse) could also be probed, as well as how these preferences may correlate with historical patterns of sense formation and expansion (cf. Ramiro et al. 2018).

Xu et al. (2020a) also discuss a potential role of frequency, namely that more commonly referred-to senses may be more likely collexified. We control for frequency in our experiment by making sure the occurrence distribution of meanings is uniform in all games. Future research could let frequency vary systematically to determine its importance. Previous research (cf. Atkinson et al. 2018; Raviv et al. 2019; Segovia-Martín et al. 2020; Raviv et al. 2021; Blythe and Croft 2021) has also demonstrated that community size and links between individuals has an effect on (artificial) language formation, learnability, and change. Another potentially interesting extension would be to run the collexification experiment using a larger speaker group than a dyad, perhaps also manipulating the connections between participants to investigate the proposed network effects. In our experiment, the participants were only presented with two possible meanings to guess between, to facilitate convergence on systematic associations in a short time frame. This also means it was trivial to achieve 50% guessing accuracy. In a longer experiment, the number of choices could be increased.

Our work may also have potentially interesting implications for historical linguistics. Population-level changes large enough to register on historical time scales must also start with differential utterance selection on the individual level, before they can compound over time and larger groups to become the norm (cf. Croft 2000; Baxter et al. 2006). If the mechanisms discussed and experimented with here are representative of utterance selection dynamics in natural languages, then the next interesting question would be: to what extent does communicative need constitute selection in language change? (cf. Andersen 1990; Baxter et al. 2006; Reali and Griffiths 2010; Newberry et al. 2017; Steels and Szathmáry 2018). A comprehensive understanding of lexical evolution, and language evolution in general, would benefit from merging the perspectives of individual mechanisms and population level consensus changes.
7.2 Complexity, information loss and communicative need

In more broad terms, our study interfaces with a growing body of work on the interplay between the orthogonal pressures of simplicity and informativeness in language evolution. The former relates to ease of learning; while the latter relates to low information loss or communicative cost (the terminology and foci vary between authors and disciplines, cf. Kirby and Hurford 2002; Gasser 2004; Kemp and Regier 2012; Fedzechkina et al. 2012; Kirby et al. 2015; Winters et al. 2015; Carstensen et al. 2015; Beckner et al. 2017; Bentz et al. 2017; Nölle et al. 2018; Zaslavsky et al. 2019b; Carr et al. 2020; Smith 2020; Steinert-Threlkeld and Szymanik 2020; Haspelmath 2021; Uegaki in prep; Denić et al. 2021). These studies have yielded converging evidence that languages which are learned and used in communication — the real-world ones, the artificial ones grown in the lab, as well as those evolved by computational agents — all aspire to balance these two pressures, ending up somewhere along the optimal frontier.

Our results provide support for the argument that culture-specific communicative needs may modulate the location of a language on that frontier (cf. Kemp et al. 2018). The pressure for simplicity in lexicons can be relaxed in favor of more expressivity, given high enough communicative need (which we emulated in the target condition, and the no-pressure conditions) — while informativeness can give way to simplicity when a less expressive lexical subspace does the job (cf. our baseline condition).

To allow for systematic statistical testing of the hypothesis in the previous sections, we applied a transformation to the data (outlined in Section 2.6) that operationalized colexification the most objective manner we could think of. Figure 9 is a reanalysis of data from Experiment 1 along the axes of complexity and expressivity, using an alternate coding scheme. To simplify things, we ignore the sender here and treat the language of each dyad as a collaborative effort. Each message containing a target meaning is assigned a simplified cognitive cost score and communicative cost score between $0.0$ and $2.0$, which depends on the last most recent reference to the same meaning and the last reference to the synonym (target pair member) of the current meaning. Figure 9 displays the mean scores for each target meaning pair used by each dyad in Experiment 1, across all their respective utterances. We also simulate the full possibility space of the results under this coding scheme, using a simple agent-based model (shown as gray blocks in Figure 9). This is to provide meaningful dimensionality to graph: given the number of signals and meanings, it would be impossible to obtain maximal score simultaneously on both axes. The technical details of both procedures are further described in the Appendix.

Figure 9: Average communicative cost and cognitive cost (complexity) scores in Experiment 1. Light blue squares are target meaning pairs (such as drizzle:rain) used by baseline condition dyads, dark blue ones are pairs as used by target condition dyads. Larger square size indicates multiple overlapping squares at those coordinates. Simulated results are depicted as gray blocks in the background. The few slightly darker gray squares, mostly in the top right, are the few dyads in Experiment 1 that performed below the minimal threshold of 59%. Most dyads communicate at or near the optimal points at $(0.0)$ and $(1.0)$, and none of the dyads (above the accuracy threshold) end up in the suboptimal top right corner. This picture mirrors findings in natural language lexicons, which also tend towards the optimal frontier (cf. Figure 1).

In summary, dyads tend towards the optimal frontier between complexity and expressivity (or in this case, the two optimal points at $(1,0)$ and $(0,1)$; positioning near the bottom left diagonal just indicates mixed strategies). Baseline dyads favor simplicity (having the luxury to do so), while target condition dyads sacrifice simplicity to reduce communicative cost (in response to our manipulation driving them to do so).
7.3 Beyond small subsystems

Extrapolating the communicative need argument beyond the grammatical and lexical subsystems mentioned in Section 1 to the scale of entire languages, we would expect semantic spaces of different languages to be mostly uniform in density — how many words are used to express shades of any given concept or meaning subspace — but differ in exactly where culture-specific communicative needs of the time either require more detail, or where fewer words will suffice (analogous to uniform information density on the level of utterances; cf. Levy 2018). Previous research has focused on delimited domains of language like tense or kinship. This makes sense both from a data collection and computational point of view: quality cross-linguistic data is not trivial to acquire, and neither is computing complexity, information loss or expressivity, the larger the system under scrutiny. This is then the next challenge: understanding these pressures and the evolution of lexicons and grammars, over time and cross-linguistically, on the scale of entire languages (as opposed to isolated domains). This would require combining explicitly quantified metrics of simplicity and expressivity (cf. Piantadosi et al. 2011; Bentz et al. 2017; Zaslavsky et al. 2019b; Steinert-Threlkeld and Szymanik 2020; Mollica et al. 2020), some estimate of communicative need (cf. Regier et al. 2015; Karjus et al. 2020), some measure of density or colexification (see Chapter 5.4 of Karjus 2020, for one potential approach), and if using a machine learning driven approach such as word embeddings, either a joint semantic model of multiple languages allowing for direct cross-linguistic comparison of lexical densities and colexification (e.g. Chen and Cardie 2018; Thompson et al. 2018; Rabinovich et al. 2020), or language-specific diachronic semantic models to observe changes in colexification over time (e.g. Rosenfeld and Erk 2018; Dubossarsky et al. 2019; Ryskina et al. 2020).

8 Conclusions

We investigated the cross-linguistic tendency of colexification of similar concepts from earlier lexico-typological research using artificial language experiments, and tested the hypothesis that colexification dynamics may be driven not only by concept similarity but also the communicative needs of linguistic communities. Our data supports both claims: speakers readily colexify similar concepts, unless distinguishing them is necessary for successful communication, in which case they do not. These results, despite being based on artificial communication scenarios and small lexicons, illustrate the interaction between similarity and communicative need in shaping colexification. We also proposed pathways for future study of these phenomena beyond small word sets and on the scale of entire lexicons.

Language change is driven by a multitude of interacting forces, ranging from random drift to sociolinguistic pressures to institutional language planning, to selection by speakers for more efficient and expressive forms. Our work supports the argument that speakers’ communicative needs — a factor balancing and modulating the relative importance of the higher-level pressures for simplicity and informativeness — should be considered as one of such forces.

Acknowledgements

We would like to thank Yang Xu, Barbara C. Malt and Mahesh Srinivasan for useful discussions and comments, Jonas Nölle for advice with the initial experimental design, and the anonymous reviewers of Cognitive Science as well as the associate editor, Padraic Monaghan, for their constructive feedback.

Author contributions

Andres Karjus designed and carried out the experiments, conducted the analysis, wrote the text, and created the figures. Tianyu Wang carried out additional experiments. Kenny Smith, Richard A. Blythe and Simon Kirby provided advice on the design of the experiment and data analysis, as well as edits and comments on the text. A shorter version of this paper formed a part of a chapter in the doctoral thesis of the first author (Karjus 2020).
Funding

Data collection for this research was funded by the Postgraduate Research Support Grant of the School of Philosophy, Psychology and Language Sciences of the University of Edinburgh. This research also received funding from the European Research Council (ERC) under the European Union’s Horizon 2020 research and innovation program (Grant Agreement 681942), held by Kenny Smith. Andres Karjus is supported within the CUDAN ERA Chair project for Cultural Data Analytics at Tallinn University, funded through the European Union Horizon 2020 research and innovation program (Project No. 810961), and was also supported by the Kristjan Jaak postgraduate scholarship of the Archimedes Foundation of Estonia during initial data collection.

Code and data availability

All the experiment data and scripts to replicate the analyses are available in the following Github repository, along with the full codebase of the Shiny game application we developed to run the dyadic experiments:

https://github.com/andreskarjus/colexification_experiment

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Appendix: Details on the complexity-ambiguity calculation

The procedure to produce Figure 9 in Section 7 is the following. Each message produced by a dyad after the burn-in period, which contains a target meaning, is assigned a cognitive cost (complexity) score and communicative cost (ambiguity) score. As a simplification, we consider the results by dyads, ignoring who sent a given message within a dyad. Only messages containing target meanings are scored, as distractor meanings lack synonyms in the meaning spaces (of Experiment 1, which we analyze here).

The cognitive cost score is set to 0, if a given utterance does not increase the complexity of the language, within the target pair: if the last reference to the same meaning used the same signal, and the last reference to the synonym (target pair member) of the current meaning also used the same signal. Using a different signal or distinguishing the current meaning from its synonym increases complexity by 1 point each.

Communicative cost is scored as 0 if the last reference to the same meaning used the same signal, and the last reference to the synonym of the current meaning used a different signal. Using a different signal or colexifying the current meaning both increase ambiguity (communicative cost, chance of misinterpreting), so doing either costs 1 point each. Given 7 signals and 10 meanings, some meanings are bound to be colexified. The minimal sum of these two scores for any given utterance is 1: it is impossible to be simultaneously maximally simple and maximally informative. The highest sum of scores is 3, which may result in random assignments of signals to meanings, or intentionally misleading behavior (see Table 6 for an example).

In addition to re-analyzing the results of our human experiments, we implement a simple agent-based model that replicates our experimental setup (up to 7 signals, equally frequently occurring 10 meanings; 135 rounds). We use the results from this model — analyzed using the same coding procedure — to provide meaningful dimensionality to the human results in Figure 9. The agent-based model is constructed as a single-agent system, representing a dyad, which plays a simple naming game. The model has two parameters: the number of signals allowed (1–7) and the naming strategy. At each round, the agent assigns a signal to a given meaning. In the most simple case, it only ever produces a single signal, leading to a "degenerate" language (see Kirby et al. 2015). Other strategies include:

- random signaling:
• fixed assignment to as many meanings as possible, with perfect memory (and random assignment for meanings for which there are not enough signals);
• fixed assignment with perfect memory, but colexifies pairs of meanings (others assigned randomly);
• fixed assignment with perfect memory, but colexifies pairs of meanings (others assigned randomly, but avoiding colexification with the pairs where enough signals available);
• rational (in the sense of Frank and Goodman 2012), keeping the entire lexification history in memory;
• rational, but keeping only the most recent lexification;
• intentionally misleading, i.e. the inverse of the rational strategies;
• and also combinations of the above.

Each combination of these parameters is repeated 20 times. The results are analyzed exactly the same way as the human experiments (including the designation of a "burn-in" period), and are graphed as gray background blocks in Figure 9.