

Not so opaque after all: Conventions formed in reference games are mostly understandable to outsiders

Anonymous CogSci submission

Abstract

In-groups can create conventionalized language, but this jargon may be understandable to those outside the group. The formation of temporary linguistic conventions between individuals is often studied in iterated reference games, where over repeated reference to the same targets, a describer-matcher pair establishes partner-specific shorthand names for targets. One open question is how understandable these referring expressions are to others who were not part of the convention formation process. We take an outside angle on understanding convention formation, using experiments with naive matchers and computational models to assess the opacity of descriptions from iterated reference games. Both human matchers and the computational model are well above chance accuracy, with variation in performance primarily driven by the target image rather than where or when the description came from. This additional perspective can inform work on how conventions are formed and how efficient the conventions actually are.

Keywords: Reference games; convention formation; computational model;

Introduction

Groups of people often have terms that are used within a group, such as regional dialects, field specific jargon, or terms of art related to a specific theoretic orientation. Those who are not part of group may use different words for the same targets, but may be able to understand or guess at the meanings, even as they do not produce them. While these terms can represent stable conventions shared by sizeable communities, temporary naming conventions can develop rapidly among small groups of people when there is a need to refer to something without a canonical name that distinguishes it in context.

This formation of temporary linguistic conventions between individuals is often studied in iterated reference games. In these games, a describer tells their partner how to sort or match a series of abstract images (Clark & Wilkes-Gibbs, 1986; Hawkins et al., 2020). Over repeated rounds of referring to the same targets, pairs usually develop conventionalized nicknames for the target images. These nicknames are often partner-specific, in that different pairs develop different nicknames for the same targets. When describing the shapes for people who were not part of the group, people go back to more elaborated descriptions, indicating an expectations that others may be unable to understand the convention, and need a longer, or different referential description (Hawkins et al., 2021; Wilkes-Gibbs & Clark, 1992; Yoon & Brown-Schmidt, 2018). Participants treat the later-stage conventions as more

opaque to outsiders than earlier-stage descriptions, and the differentiation of labels across groups over time is a signal of arbitrariness in the conventions (Boyce et al., 2024; Hawkins et al., 2020).

How arbitrary and opaque are the conventions, really? Another approach to studying convention formation is to look at how understandable the conventions are to others who were not part of the interaction that originated the convention. A more arbitrary convention should be harder to understand.

Asking naive matchers to interpret utterances produced during iterated reference games thus can provide an additional perspective by determining how the features of the utterances and the conditions they were produced under affect how opaque they are, and provide empirical tests of whether descriptions become more opaque over the course of the reference game.

Some prior work has investigated how well naive matchers can understand the descriptions produced in the course of an iterated reference game, with a focus on the role of conversational shared history.

Murfitt & McAllister (2001) recorded descriptions from 8 participants who described shapes either solo or in a matching game with a partner and played the descriptions to new matchers, either in order or in reverse order. Naive listeners were more accurate when they heard descriptions in order. In Schober & Clark (1989) matchers in the iterated reference game had achieved mean accuracies of 93% in the first round to 100% in the 6th round. Overhearers who listened to the entire game in order had accuracies that rose from 81% to 95%. Overhearers who listened to recordings starting in the 3rd round did worse, with accuracies rising from 55% to 73%. With 16 target options, even the late-start overhearers were far above chance.

In an iterated reference game using drawing (rather than text) as the communication modality, Hawkins et al. (2023) compared the accuracy of in-game matchers to naive matchers in yoked and shuffled conditions. The yoked matchers saw all the trials from one game in order, while the shuffled matchers saw trials sampled from 10 games but in trial order. In-game matchers were more accurate overall (88% on 4-way choice) than yoked matchers (75%) who were in turn more accurate than shuffled matchers (69%). Over the course of trials, both in-game and yoked matchers showed steeper improvement in accuracy than shuffled matchers.

Leung et al. (2024) did have adults and children serve as naive matchers, listening to descriptions from the the first and last rounds of a parent-child iterated reference game. They found no significant difference in how comprehensible early and late descriptions were to naive matchers, and while naive matchers had high accuracy (88% on 2-way choice), they were slightly less accurate than in-game matchers overall.

Across the prior literature, while naive matchers have worse accuracy than in-game matchers, their performance is still far above chance, suggesting that the convention-target relationship is not purely arbitrary. In fact, even when pairs of participants try to obfuscate their meaning to match images with each other but not an overhearer, an overhearing participant can still do quite well (Clark & Schaefer, 1987).

Across these studies, receiving more context from one interaction, and in particular, having that context be in order, is beneficial to matchers. Except for the shuffled condition of Hawkins et al. (2023), these studies do not address how opaque descriptions from different points in the game are without prior context.

One way to conceptualize the opacity of a referring expression is by considering the semantic distance between the signifier and the referent. Under the assumption of a modality-independent global semantics (i.e., not conditioned by partner-specific meaning), expressions that are transparent have signifiers and referents that are semantically close, such that any member of the sociolinguistic community sharing the global semantics should be able to identify the appropriate referent given the signifier. In contrast, expressions that are opaque have signifiers and referents that are semantically distant, such that the relations between them are more arbitrary and inaccessible to the general community without the formation of additional conventions (which may be partner- or group-specific).

Assessing opacity thus requires us to have a measure of modality-independent global semantics. Such semantics are difficult to directly obtain for humans, since we rarely have explicit semantic formulations of the stimuli we encounter, much less formulations that are unified across multiple modalities. However, the ability of a naïve comprehender to understand a referring expression that is presented without context provides a proxy measure, since the comprehender’s judgments are not conditioned on any context-specific conventions. Thus, prior studies on naïve matchers provide one approach to estimating the opacity of the utterances produced in reference games.

Another possible measure of modality-independent global semantics is computational in origin. Computational methods have enabled the projection of various stimuli (including image and text stimuli) into embeddings in high-dimensional spaces; these embeddings have demonstrated properties which suggest that they are reasonable approximations of humans’ semantic spaces, including similarity in representational geometries (e.g., Grand et al., 2022; Mutenhaler & Hebart, 2021). Indeed, embeddings from neural



Figure 1: Experimental Setup and Procedure. TODO

network models have been used as a form of semantics in a range of reference game scenarios (e.g., Gul & Artzi, 2024; Ji et al., 2022; Kang et al., 2020; Le et al., 2022; Ohmer et al., 2022). In particular, such embeddings can be treated as context-independent semantic representations, since they are not updated to account for convention formation within an iterated reference game; hence, they can serve as a computational comparison to human performance on a naïve matching task.

In the current work, we address how the process of convention formation shapes the levels of opacity of the referring expressions created at different time points in an iterated reference game. Using reference expressions created in different games from Boyce et al. (2024), we use both human experiments and models to assess when and why expressions are opaque or understandable to outside observers.

Task Setup

Materials

We draw on the corpus of reference game transcripts and results from Boyce et al. (2024). This corpus is made up of 6 round iterated reference games using the same 12 target images. Games were played in conditions varied in how large the describer-matcher groups were (2-6 participants) and how “thick” the communication channels were. For our naive matcher experiments, we sample different subsets of this corpus. In presenting the transcripts, we exclude utterances that were marked by Boyce et al. (2024) as not containing any referential content (i.e. purely greetings, meta-commentary, or off-topic chitchat). Within the samples, we also do not show transcripts that contained swear words or crude or sexual language. We use the entire corpus for our computational modelling component.

Experimental procedure

We recruited English-speaking participants from Prolific. Participants were directed to the experiment, where the task was explained to them. On each trial participants saw the full transcript from that trial, containing all the chat messages marked by whether they were from the speaker or a listener. Participants selected the image they thought was the target from the tableau of 12 (Figure 1). Participants received feedback on whether they were right or wrong on each trial. Ex-

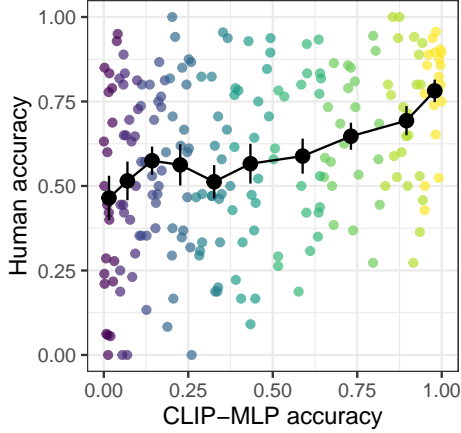


Figure 2: Correlation between human and CLIP-MLP accuracy across deciles of CLIP-MLP accuracy. Colored points are individual descriptions, black line is the bootstrapped mean and 95% CI across descriptions for each decile.

cept when the specific viewing order was part of the experimental manipulation, we randomized the order of trials, subject to the constraint that the same target could not repeat on adjacent trials. The task was implemented in jsPsych (Leeuw et al., 2023). We paid participants \$10 an hour plus a bonus of 5 cents per correct response. All our experimental code is at [TODO LINK](#).

Computational models that Ben gets to write

This is where Ben gets to write!

Experiment 1

Our CLIP-MLP computational model was optimized for task accuracy. To validate whether this objective also results in human-like response patterns, we conducted a calibration experiment to determine if, for any given utterance, the probability that the model assigns to the target image was aligned with the probability that a naïve human matcher would choose the target image.

Methods

We first obtained target probabilities from our CLIP-MLP model for all utterances from Boyce et al. (2024). We then used stratified sampling to select 217 trials by dividing model-predicted probabilities into deciles and choosing approximately 22 utterances per decile, spanning the 12 different possible target images.

We recruited 61 participants who each saw 64 trials randomly sampled from the 217 tested trials. On average, each trial was seen by 18 participants. This experiment was pre-registered at <https://osf.io/6pv5e>.

Results

We obtained human accuracies on each trial by dividing the number of participants who correctly selected the target image by the total number of participants who saw the trial, as shown

in Figure 2. There was a small but significant positive correlation between model-predicted probabilities and human accuracies ($r = 0.33$ [0.21, 0.45]). This result suggests that model predictions were calibrated to human response patterns, albeit not perfectly. It is theoretically possible to use these calibration results to project model predictions in order to better approximate human responses; we leave this approach for future work. Nonetheless, the observed positive correlation suggests that our computational model is a reasonable approximation of human accuracies, validating its use in subsequent experiments as a computational comparison.

Experiment 2

As a starting point for examining what makes referential expressions more or less opaque, we focused on referring expressions from the first and last rounds of games. Principles of convention formation and people’s behavior when they switch to a new partner suggest that later round utterances are more opaque and would be more difficult to understand. One argument in the opposite direction is that later rounds are the result of describers accumulated practice refining descriptions to be maximally communicative and pick out the most visually salient features. We included descriptions from games of different sizes and communication thicknesses. Based on the patterns of cross-game similarity in Boyce et al. (2024), we expected smaller and thicker games, whose descriptions diverged fastest, would have more idiosyncratic and opaque conventions than larger groups with thinner communication channels.

Methods

Experiment 2a To establish a baseline of how well naive matchers could understand descriptions without context, we ran a 2x2 within subjects experiment. We drew the target transcripts from 2- and 6-player games from Experiment 1 of Boyce et al. (2024) and from the first and last blocks of these games. These games had medium thick communication channels. We recruited 60 participants who each saw 60 trials (15 in each of the 4 conditions). Overall, participants saw 774 transcripts from 40 games. This experiment was pre-registered at <https://osf.io/k45dr>.

Experiment 2b After observing limited condition differences in experiment 2a, we ran a follow-up experiment on descriptions from Experiment 3 of Boyce et al. (2024), where the communication channel thicknesses were more extreme. Here, we used a 2x2x2 within subjects design, drawing our transcripts from the first and last rounds of thick and thin, 2- and 6- person games. In the thin condition, original matchers could only contribute to the chat by sending one of 4 emojis; as the emojis did not have referential content, we did not include them in the transcripts shown to naive matchers. For experiment 2b, we recruited 60 participants who each saw 64 trials (8 in each of the 8 conditions). Overall, participants saw 2392 transcripts from 163 games. This experiment was pre-registered at <https://osf.io/rdp5k>.

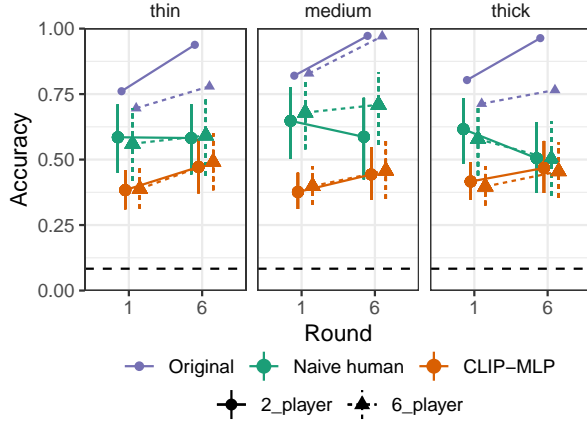


Figure 3: Accuracies for naive humans and the CLIP-MLP model for Experiment 2. Point estimates and 95% CrI are predictions from the fixed effects of logistic and beta regressions. Bootstrapped mean accuracy from the original matchers is included as a ceiling, and random chance as a baseline.

Results

Our primary outcome was naive matcher accuracy, i.e. how often they selected the correct target image.

Experiment 2a For Experiment 2a, we ran a mixed effects logistic model of naive matcher accuracy: $\text{correct} \sim \text{group_size} \times \text{round} + \text{trial_order} + (\text{group_size} \times \text{round} | \text{correct_tangram}) + (\text{group_size} \times \text{round} + \text{trial_order} | \text{workerid})$. Overall, naive matchers were right more often than not, which was far above the 1/12 expected by random chance (Odds Ratio: 1.93 [1.05, 3.62]). There were not large effects of condition (Figure 3 middle panel). Participants tended to be less accurate at descriptions from the last round (OR of last round: 0.77 [0.53, 1.1]). There was not a clear effect of original group size (OR of 6-player game: 1.15 [0.89, 1.47]), but there was an interaction between round and group size (OR: 1.49 [1.06, 2.1]). Later transcripts from larger games were easier to understand, but later transcripts from smaller games were easier to understand.

Much of the variation in accuracy was driven not by condition, but by the target image (OR of standard deviation of image distribution: 2.66 [1.88, 4.52]). Some images were much easier to identify as the target than others (Figure 4).

Experiment 2b For Experiment 2b we ran a similar mixed effects logistic model to consider the effects of group size, thickness, round, and their interactions. Naive matchers were above 50% accuracy (OR: 1.81 [1.06, 3.08], Figure 3). Similar to experiment 2a, there were not substantial effects of condition. Last round descriptions had slightly lower accuracy (OR of last round: 0.64 [0.47, 0.85]), but there was an interaction with thickness, where for thin games, last round descriptions were less opaque (OR: 1.55 [1.02, 2.33]). Again some of the uncertainty in estimating the fixed effects was driven by the strong variation based on target image (OR of SD of

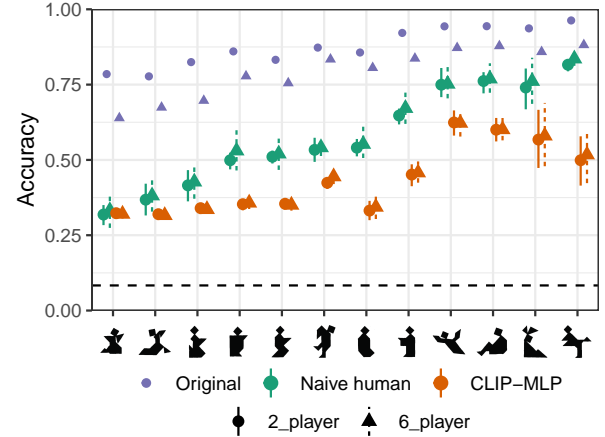


Figure 4: Accuracies for naive humans and the CLIP-MLP model for Experiment 2, split out by target image. Point estimates and 95% CI are predictions from the fixed effects and by-tangram random effects of logistic and beta regressions, bootstrapped across conditions. Bootstrapped mean accuracy from the original matchers is included as a ceiling, and random chance as a baseline.

images: 2.25 [1.67, 3.59], Figure 4).

Additional Predictors As additional post-hoc predictors, we examined the accuracy of the in-game matchers from Boyce et al. (2024) and the length of the description. In both experiments, in-game accuracy was predictive of naive matcher accuracy (Expt 2a OR: 3.38 [2.46, 4.7], Expt 2b OR: 2.17 [1.7, 2.77]). The log number of words in the description was not predictive in Experiment 2a (OR: 1.05 [0.94, 1.17]), but longer descriptions were slightly beneficial in Experiment 2b (OR: 1.1 [1.01, 1.2]).

Model results

As a computational comparison, we looked at the CLIP-MLP model’s performance on the same descriptions. We used the probability the model assigned to the correct target as our dependent measure and fit a beta regression on the descriptions from Experiment 2: $\text{correct} \sim \text{group_size} \times \text{thickness} \times \text{round} + (\text{group_size} \times \text{thickness} \times \text{round} | \text{correct_tangram})$. The CLIP-MLP model was far above chance, but had lower accuracy than the human participants (OR: 0.6 [0.45, 0.82]).

None of the fixed effects in the model were significant, and there was wide uncertainty for all of them. There was substantial by-target image variation (1.58 [1.31, 2.15]) and substantial by-target variation in the effect of later round (1.56 [1.29, 2.09]).

As additional predictors, we checked the effect of in-game matcher accuracy and the length of the description. MLP-CLIP had higher accuracy when in-game matcher accuracy was higher (OR: 1.5 [1.33, 1.69]), and the model did better on shorter descriptions (OR for log words: 0.85 [0.82, 0.9]). Long descriptions may be more difficult because they are further further from the model’s training distribution of image

captions.

Interim Summary Overall, naive human matchers were fairly accurate overall, but less accurate than matchers in the original game. Perhaps surprisingly, this level of accuracy was fairly consistent across descriptions from different times in the game and different game conditions. The largest source of variability in accuracy was from the target images; while there was some variability in accuracy by images for the original matchers, there was substantially more variability for naive matchers. The computational model showed a similar pattern of large effects of image, but had overall lower accuracy than naive human matchers.

Experiment 3

The experiment of naive matchers in Experiment 2 differed from in-game matchers in several ways. In-game matchers received descriptions from a consistent group, received descriptions in the order they were created, and were present participants during the game. In Experiment 3, we focus on the role of context and group-specific interaction history to tease apart some of these differences.

Methods

We compared naive matchers in “yoked” and “shuffled” conditions. In the “yoked” condition, naive matchers saw all the descriptions from a single game in the order they originally occurred. In the “shuffled” condition, naive matchers saw all the descriptions from a single game in a randomized order. This is the same yoking but a different shuffling than that used in Hawkins et al. (2023).

Because some descriptions are already fairly comprehensible in isolation, we focused on the role of context for games that showed strong group-specificity. We hand-picked 10 games from Boyce et al. (2024) on the basis of high in-game matcher accuracy, strong patterns of descriptions shortening over repetition, and the use of idiosyncratic or non-modal referring expressions. Thus, these games showed the hallways of strong conventionalization to terms that were more likely to be opaque to outsiders.

We recruited 196 participants (99 in the yoked condition and 97 in shuffled) who each saw all 72 trials of 1 of the 10 games. This experiment was pre-registered at <https://osf.io/zqwp5>. Participants read the transcripts in a modified self-paced reading procedure where they uncovered the text word by word (revealed words stayed visible); only after uncovering the entire transcript could participants select an image. We do not analyse the reading time data here.

Results

Our primary question of interest was how much having the conversation history would help make later round descriptions more understandable to participants in the yoked condition.

We compared accuracy across the yoked and shuffled conditions with a logistic regression: $\text{correct} \sim \text{orig_repNum} \times \text{condition} + \text{matcher_trialNum} + (1|\text{gameId}) + (1|\text{cor-}$

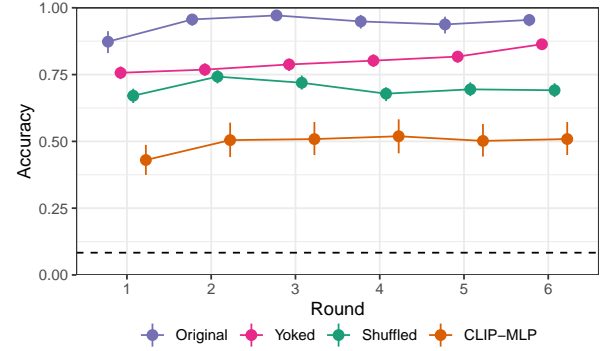


Figure 5: Accuracies for Experiment 3. Error bars are bootstrapped 95% CIs. TODO not using predictions because those fuzz out round to round differences.

rect_tangram) + (1|workerid). The descriptions were more transparent when they were presented in a yoked order (OR: 2.2 [1.63, 3], Figure 5). In the shuffled condition, there was no main effect of round number (OR for one round later: 0.99 [0.95, 1.02]), but there was a marginal interaction where the benefit of the yoked condition decreased for later rounds (OR for one round later: 0.94 [0.89, 1]). This was offset by matchers in both conditions improving at the task over time (OR for one trial later in matcher viewing order: 1.02 [1.02, 1.02]). In the yoked condition round and trial number were aligned, so an improvement over time could be either from matcher practice or from descriptions being easier to understand. In the shuffled condition, matcher practice effects did not correlate with position in the original game.

Comparing to the performance of in-game matchers, we can separate out the benefits of seeing the descriptions in order versus being a participant in the group: $\text{correct} \sim \text{orig_repNum} \times \text{order} + \text{orig_repNum} \times \text{setting} + \text{matcher_trialNum} + (1|\text{gameId}) + (1|\text{correct_tangram}) + (1|\text{workerid})$. There is a benefit to seeing the items in order (OR: 2.24 [1.63, 3.04]) and a larger benefit to being a participant during the game (OR: 4.35 [2.77, 6.89]). The benefit of seeing the items in order wanes in later blocks (OR: 0.94 [0.89, 1]), but the benefit of being in the game does not (OR: 1.06 [0.95, 1.18]). In all cases, there is a baseline improvement over trials (OR: 1.02 [1.02, 1.02]).

The accuracy of the CLIP-MLP model is worse than the shuffled human results, and does not show change across rounds (OR for one round later: 1.02 [0.97, 1.07]). The larger difference between naive human and CLIP-MLP accuracies in Experiment 3 than Experiment 2 could suggest that even the shuffled ordering still provides useful context that helps matchers understand the conventions. This history is not available to the CLIP-MLP model which sees every description as a one-shot task.

Discussion ?

potentially worth saying that the level of descriptiveness that is given to an intended target is meant to enable a higher

level of confidence and seem more supportive/cooperative than what a naive listener can understand Levels of understanding do cast doubt on whether the utterances produced are efficient

productions may be constrained by politeness/conversational norms and the describer's need to come up with a description more than what is minimally necessary to convey the information

questions about what is about some images that makes them much easier to identify – is it iconicity or a narrower prior about how to describe? distance to competitors?

sampling across more images (a la (ji?)) could address that. Also many sources of variation in how different groups described things so further analysis of differences within utterances at words and structure could help there, where models are useful b/c can run them in more conditions than people will put up with.

Part of discussion that Alvin gets to write??

Discussion

Understanding varies much more based on item than on anything else; potentially due to priors or iconicity of image (? that might be beyond scope – how well does this match up with say diversity of descriptions)

Models do pretty well? IDK what our model take away is

Especially when there is strong or idiosyncratic reduction, context helps

role of context

limitations, including out of distribution for models

might want to address language comprehension v inference

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