[1] What paradigms can webcam eye-tracking be used for? Attempted replications of 5

[2]   "classic" cognitive science experiments

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38   Abstract

39   ADD LATER

40   *Keywords:* eye-tracking, online, webcam, jsPsych, cognitive science

41   Word count: X

42   What paradigms can webcam eye-tracking be used for? Attempted replications of 5

43   "classic" cognitive science experiments

44   The use of eye-tracking to study cognition took off when Alfred Yarbus used suction

45 cups to affix a mirror system to the sclera of the eye in order to monitor eye position

46 during the perception of images (Yarbus, 1967). In one study, participants viewed a

47 painting depicting multiple people in a complex interaction inside of a 19th century

48 Russian home. Yarbus showed, among other things, that the scan paths and locations of

49 fixations were largely dependent on the instructions given to participants (e.g., View the

50 picture freely vs. Remember the position of the people and objects in the room). In other

51 words, the cognitive processing that the individual is engaged in drives the visuo-motor

52 system. Since these findings, eye-tracking has become a central method in cognitive science

53 research Rayner (1998). For example, gaze location during natural scene perception is used

54 to test theories of visual attention (e.g., Henderson & Hayes, 2017). And eye-movements

55 during auditory language comprehension, using the "visual world paradigm," demonstrated

56 the context-dependent and incremental nature of language processing (e.g., Tanenhaus,

57 Spivey-Knowlton, Eberhard, & Sedivy, 1995).

58   An important limitation of the eye-tracking methodology is that it has typically

59 required costly equipment (eye-trackers can range in price from a few thousand dollars to

60 tens of thousands of dollars), particular laboratory conditions (a quiet room with

61 consistent indoor lighting conditions), and a substantial time investment (e.g., bringing

62 participants into a laboratory one at a time). This limits who can conduct eye-tracking

63 research – not all researchers have the necessary resources – and who can participate in

64 eye-tracking research. Most eye-tracking study participants are from western, educated,

65 industrialized, rich, and democratic [WEIRD; Henrich, Heine, and Norenzayan (2010)]

66 convenience samples (but see Ryskin, Salinas, Piantadosi, & Gibson, 2023), which

67 diminishes the generalizability of the findings and the scope of conclusions that can be

68   drawn about human cognition.

69   Advances in software for online data collection (de Leeuw, 2015; Papoutsaki et al.,

70   2016) have the potential to address this shortcoming by expanding access to eye-tracking

71   technology for researchers and making it feasible to broaden and diversify the participant

72   samples. In particular, Webgazer.js (Papoutsaki et al., 2016) is a webcam-based Javascript

73   plug-in that works in the browser. It can be integrated with any Javascript web interface.

74   As a result, it can be used in conjunction with many existing platforms for online

75   behavioral data collection that are familiar to cognitive scientists, such as jsPsych (de

76   Leeuw, 2015), Gorilla (Anwyl-Irvine, Massonnié, Flitton, Kirkham, & Evershed, 2020), or

77   lab.js (Henninger, Shevchenko, Mertens, Kieslich, & Hilbig, 2021). However, the added

78   convenience comes at the cost of spatial and temporal resolution. The extent of this loss of

79   precision and its impact on the kinds of research questions that webcam eye-tracking is

80   appropriate for are not yet known.

81   A few previous studies have used webcam eye-tracking in the context of

82   behavioral/cognitive science experiments and reported on the quality of the data. In what

83   follows, we provide a brief review of the published work that we are aware of (note that we

84   focus on studies with adult participants as the considerations for eye-tracking of children

85   are substantially different, but see e.g., Bánki, Eccher, Falschlehner, Hoehl, & Markova,

86   2022).

87   Semmelmann and Weigelt (2018) compared eye-tracking results between in-lab

88   (n=29) and online (n=28) studies in three tasks: fixation, pursuit, and free viewing. They

89   used the Webgazer.js library (Papoutsaki et al., 2016) and programmed tasks in

90   HTML/Javascript directly. The first two tasks tested measurement of basic gaze

91   properties. In the fixation task, participants were asked to fixate a dot, and in the pursuit

92   task, they were asked to follow a dot with their eyes. In the free viewing task, the eye

93   movements may have been more semantically driven: participants were shown a picture of

a human face and asked to look wherever they wanted to on the image. Webgazer was successful in accurately detecting fixation locations and saccades in all three tasks, though online data (collected through a crowdsourcing platform) were slightly more variable and delayed. The free viewing task replicated previously observed statistical patterns (e.g., more fixations on eyes than mouths).

Similarly, Yang and Krajbich (2021) used the Webgazer.js library (Papoutsaki et al., 2016) combined with the jsPsych library for conducting behavioral experiments in a web browser (de Leeuw, 2015), to replicate a well-established link between value-based decision-making and eye gaze Krajbich, Armel, & Rangel (2010). Online participants (n=38) first saw a series of images of 70 snack foods and rated how much they liked each one. During the primary task, on each of 100 trials, two of the snack food images were displayed on the left and right sides of the display and participants chose the one that they preferred while their gaze was monitored. As in previous work, participants were biased to choose the option they had spent more time looking at. The authors also implemented a code modification to address temporal delays in WebGazer.

Finally, Slim and Hartsuiker (2022) used the Webgazer.js library via the PCIbex online experiment platform (Zehr & Schwarz, 2018) to replicate a visual world paradigm study in which participants (n=90) listened to sentences (e.g., Mary reads a letter) while viewing four pictures displayed across the four quadrants of the screen (e.g., a letter, a backpack, a car and a wheelchair) (Dijkgraaf, Hartsuiker, & Duyck, 2017). When the verb in the sentence was *constraining* (e.g., read) with respect to the display (only the letter would be an appropriate continuation), listeners made more fixations to the target image (letter) than when the verb was *neutral* (e.g., Mary steals a letter), as in previous work. However, they observed a substantial delay of ~200ms in the onset of the effect relative to the in-lab study with an infrared eyetracker and a reduction in effect size (despite a threefold increase in sample size relative to the original). The authors noted that delay was particularly problematic given that the purpose of the original study was to capture

aspects of predictive or anticipatory processing: in the original study, but not in the web
replication, the difference between *constraining* and *neutral* conditions emerged before the
onset of the final noun (e.g., letter).

Present work

In order to validate online eyetracking measures, we set out to reproduce five
previously published studies representing a variety of questions, topics, and paradigms.
The goal was to examine the strengths and weaknesses of webcam eye-tracking for common
paradigms in cognitive science. Ideally, we would only attempt to replicate studies where
the original measurements have small error bars and are known to replicate; otherwise, it
can be difficult to distinguish a failure of the method (online eyetracking does not work)
from a failure of the original study to replicate.

In practice, replications (successful or otherwise) have only been reported for a small
number of studies, so we ultimately included some studies with unknown replicability. We
addressed this in several ways. First, replicating five very different studies from different
research traditions decreases our reliance on any one study. Second, we include several
"sanity check'' analyses, such as the correlation between calibration accuracy and effect
size. (If the effect is real but there is noise from low-accuracy eyetracking, this correlation
should be substantial.) Third, for XXX of the studies, we had comparison data collected
in-lab either using jsPsych or a more traditional eyetracker technology, allowing us to
direct assess the impact of differences in subject population and equipment.

Selection of Studies

We chose five high-impact eyetracking studies involving adult subjects. (Given the
additional difficulties of recruiting and retaining child participants, we excluded
developmental studies.) Our goal was to include experiments from a range of topic areas

Table 1

Studies selected for replication attempts

Citation	Topic Area	Paradigm	Citations (June 2022, Google Scholar)
Altmann & Kamide, 1999	Psycholinguistics	Natural Scenes	1,840.00
Johansson & Johansson, 2013	Memory	Four Quadrants	190.00
Manns, Stark, & Squire, 2000	Memory	Two Halves	127.00
Snedeker & Trueswell, 2004	Psycholinguistics	Four Quadrants	448.00
Shimojo et al., 2003	Decision Making	Two Halves	964.00

145 (e.g., memory, decision making, psycholinguistics) and paradigms (two halves of the screen,

146 visual world paradigm with four quadrants, visual world paradigm with "natural" scenes).

147 As noted above, we had a preference for well-established findings that are known to

148 replicate, though for sake of diversity this was not always possible.

149 Table **??**tab:studies-table) provides an overview of the five studies we selected.

General Methods

Participants

152 Participants completed the experiment remotely and were recruited through the

153 Prolific platform. In order to have access to the experiment, participants had to meet the

154 following criteria: 18 years of age or older, fluency in English, and access to a webcam. All

155 participants provided informed consent. The studies were approved by the Vassar College

156 Institutional Review Board.

157 In addition, in-person replications were conducted for Experiments 1 and 4.

158 Information about those samples is given in the corresponding Method sections.

In order to have adequate statistical power and precision, we aimed for 2.5x the sample size of the original experiment, following the heuristic of Simonsohn (Simonsohn, 2015). In Experiment 5, the original sample size was so small that we opted to collect 5x the number of participants to increase precision. Because of budget and time constraints we were unable to replace the data for subjects who were excluded or whose data was missing due to technical failures.

Equipment

We used a fork of the `webgazer.js` library for webcam eyetracking (Papoutsaki et al., 2016), implemented in `jsPsych`, a Javascript library for running behavioral experiments in a web browser (de Leeuw, 2015). Our fork included changes to webgazer.js in order to improve data quality for experiments in which the precise timing of stimulus onsets is relevant. Specifically, we implemented a polling mode so that gaze predictions could be requested at a regular interval, which improved the sampling rate considerably in informal testing. This modification is similar to what Yang and Krajbich (2021) reported improved the sampling rate in their study of webgazer. We also adjusted the mechanism for recording time stamps of each gaze prediction, so that the time stamp reported by webgazer is based on when the video frame is received and not when the computation of the gaze point is finished.

Eye-tracking Calibration and Validation

When participants began the experiment, they were notified the webcam would be used for eye tracking but no video would be saved. They were asked to remove glasses if possible, close any other tabs or apps, turn off notifications, and make sure their face was lit from the front. The webcam's view of the participant popped up on the screen, and participants were asked to center their face in the box and keep their head still. The

experiment window then expanded to full screen, and participants began the eye-tracking calibration.

During the calibration, dots appeared on the screen one at a time in different locations, and the participants had to fixate them and click on each one. Once they clicked on a dot, it would disappear and a new one would appear in a different location on the screen. The locations of calibration dots were specific to each experiment (details below) and appeared in the areas of the screen where the visual stimuli would appear during the main task in order to ensure that eye movements were accurately recorded in the relevant regions of interest. After the calibration was completed, the validation began. Participants were asked to go through the same steps as the calibration, except that they only fixated the dots as they appeared in different locations on the screen. If accuracy on the validation was too low (fewer than 50% of looks landed within a 200 px radius of the validation points), participants were given an opportunity to re-start the calibration and validation steps. If the second attempt also lead to low validation accuracy, participants were informed that they could not participate in the study.

Data pre-processing

We used R (Version 4.2.1; R Core Team, 2021) and the R-packages *afex* (Version 1.2.1; Singmann, Bolker, Westfall, Aust, & Ben-Shachar, 2021), *broom.mixed* (Version 0.2.9.4; Bolker & Robinson, 2020), *dplyr* (Version 1.0.10; Wickham, François, Henry, & Müller, 2021), *forcats* (Version 0.5.2; Wickham, 2021a), *ggplot2* (Version 3.4.0; Wickham, 2016), *jsonlite* (Version 1.8.4; Ooms, 2014), *lme4* (Version 1.1.31; Bates, Mächler, Bolker, & Walker, 2015), *lmerTest* (Version 3.1.3; Kuznetsova, Brockhoff, & Christensen, 2017), *Matrix* (Version 1.5.3; Bates & Maechler, 2021), *papaja* (Version 0.1.1; Aust & Barth, 2020), *readr* (Version 2.1.3; Wickham & Hester, 2020), *shiny* (Version 1.7.4; Chang et al., 2021), *stringr* (Version 1.5.0; Wickham, 2019), *tidyr* (Version 1.3.0; Wickham, 2021b), and *tinylabels* (Version 0.2.3; Barth, 2022) for all our analyses.

Experiment 1

The first study was a replication attempt of Altmann and Kamide (1999). Altmann and Kamide used the visual world eye-tracking paradigm (Tanenhaus et al., 1995) to show that meanings of verbs rapidly constrain the set of potential subsequent referents in sentence processing. For example, when looking at the display in Figure 2 and listening to a sentence like "The boy will eat the. . . ," participants are more likely to look at the cake than when they hear "The boy will move the. . . ," in which case they tend to look at the train, presumably because cakes are edible and trains are not. Semantic information available at the verb is used to anticipate upcoming linguistic input.

We first collected data from participants via Prolific, then conducted a follow-up in-lab replication; both the original and the replication used WebGazer and identical materials and procedures.

Methods

All stimuli, experiment scripts, data, analysis scripts, and a pre-registration are available on the Open Science Framework at https://osf.io/s82kz.

Participants.

Remote sample. Sixty participants were paid $2.60 for their participation. Our sample size of participants was determined by the total run time of our experiment, ~10 minutes, and the allotted funding from the Vassar College Cognitive Science Department. From this information, we calculated a reasonable number of participants we could afford to compensate on Prolific. Note that the sample size of the original study was 24. For unknown reasons, 2 of the subjects' results were not recorded, so in the analysis, we worked with data collected from 58 participants.

In-lab sample. Forty-nine participants were (AJ: help. Insert how they were compensated, relevant IRB/funding stuff, any other info).

234   **Procedure.**   The task began with a 9-point eye-tracker calibration and validation

235 (Figure **??**). During the experiment, the participants were simultaneously presented with a

236 visual image and a corresponding audio recording of a spoken sentence. Participants had to

237 input a keyboard response indicating "yes" or "no" as to whether the sentence they heard

238 was feasible given the visual image. There were two practice trials to ensure that

239 participants understood the instructions before they undertook the main portion of the

240 experiment. Participants' reaction times, keyboard responses, and looks to objects in the

241 scene were recorded for each trial.
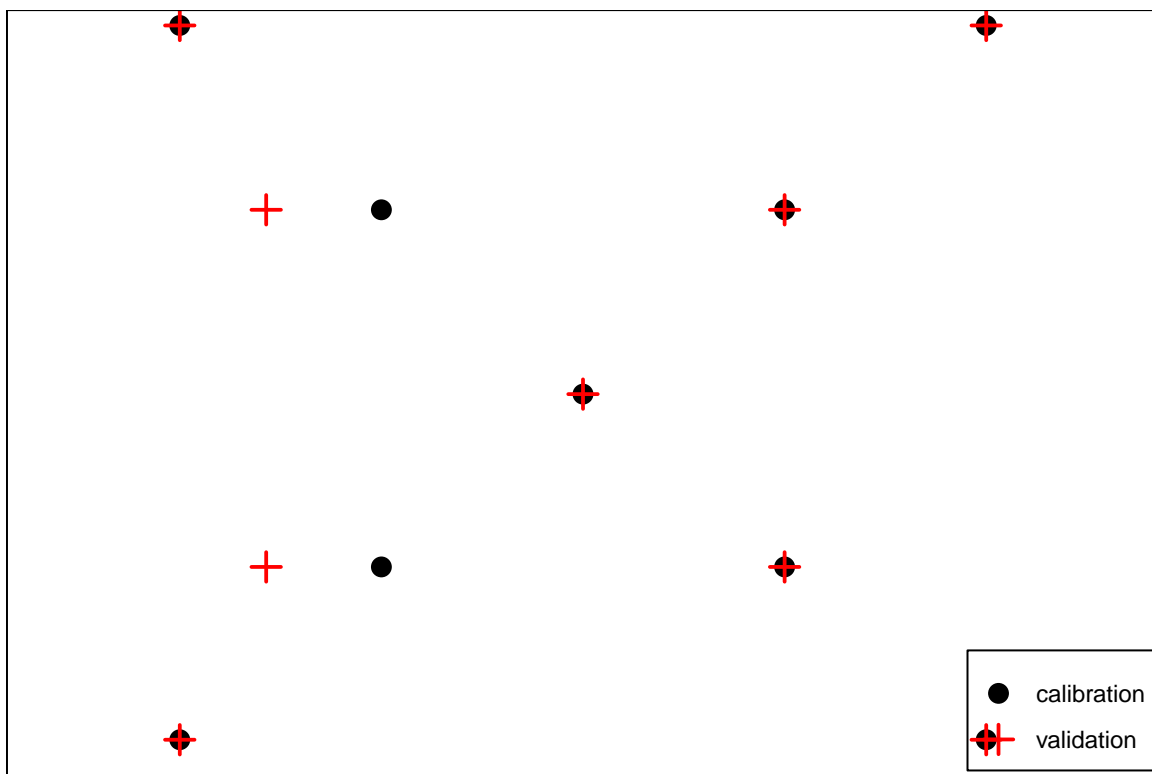
Figure 1. Calibration and validation point locations for Experiment 1. Black points were
used for calibration. Red crosses were used for checking the accuracy of the calibration.

242   **Materials & Design.**   The visual stimuli were created through Canva and depicted

243 an agent accompanied by four to five objects in the scene (see Figure 2). On critical trials,

244 participants heard one of two sentences associated with the scene. In the restrictive

245 condition, the sentence (e.g., "The boy will eat the cake") contained a verb (e.g., "eat")

which restricts the set of possible subsequent referents (e.g., to edible things). Only the target object (e.g., the cake) was semantically consistent with the verb's meaning. In the non-restrictive condition, the sentence (e.g., "The boy will move the cake") contained a verb (e.g., "move") which does not restrict the set of possible subsequent referents. The target object (e.g., the cake) as well as the distractor objects (e.g., the train, the ball, etc.) were semantically consistent with the verb's meaning. Both sentences were compatible with the scene, such that the correct keyboard response for the critical trials was "yes." Filler trials consisted of scenes that looked similar to critical scenes but were paired with inappropriate sentences. The correct keyboard response for the filler trials was "no."

Each participant was presented with sixteen critical trials (eight in the restrictive condition, eight in the non-restrictive condition) and sixteen fillers for a total of 32 trials. The order of trials and the assignment of critical scene to condition was random on a subject-by-subject basis.

Data pre-processing and analysis. Looks to the objects in the scene were time-locked to the onset of the verb, the offset of the verb, onset of the post-verbal determiner, and onset of the target noun. Regions of interest (ROIs) were defined by creating boxes around each object in the scene. The size of each box was determined by taking the height and width of the given object and adding 20 pixels of padding. Each scene contained an agent region, a target region, and three or four distractor regions.

Results

Remote sample.

Minimal Exclusion. The first set of analyses used minimal exclusion criteria. First, we eliminated participants with 0 percent of fixations in any ROIs. This resulted in the elimination of 1 participants. Second, we excluded participants with validation accuracy under 10 percent, resulting in an additional 5 excluded participants. The

Figure 2. Example trial from Experiment 1. Participants would hear a sentence (e.g., "The boy will eat the cake") and respond according to whether the sentence matched the picture.

following analyses included 52 participants.

*TO-DO: Confirm: what was done with fixations initiated prior to verb onset that persisted past verb onset (I think I should be able to tell from looking at the code) whether there's a 200-ms scootch.

Cumulative Fixation Probabilities. For each sentence, the target time window began at the onset of the verb and ended 2000 milliseconds later. This window was then divided into 50-ms bins; for each participant and each trial, we recorded whether each object was fixated during the 50-ms bin. Collapsing over trials and participants, and

279 averaging across distractors, we calculated the cumulative probability of fixation, shown in

280 Figure 3.
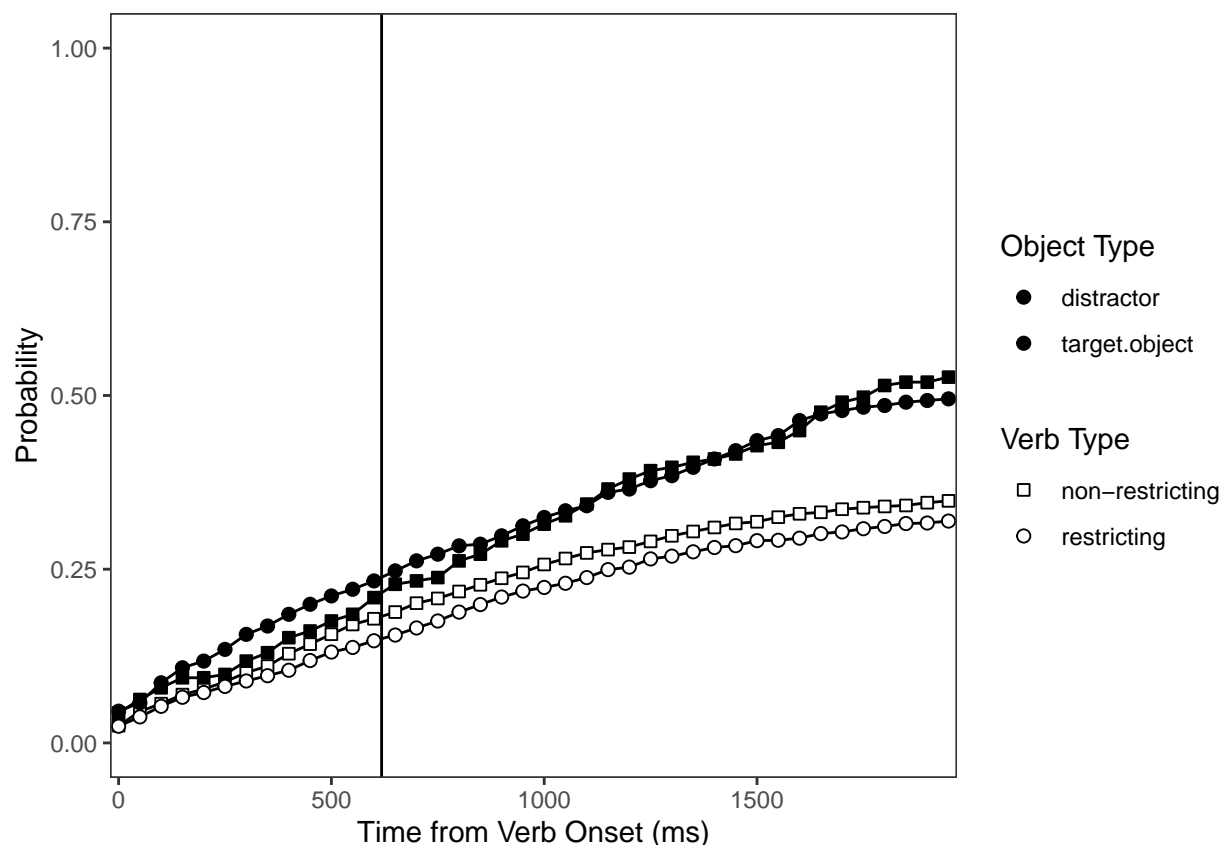
Figure 3. Cumulative probability of fixating distractor and target objects across conditions over time, with 0 ms aligned to the verb onset time. The vertical line marks the mean noun onset time across trials and conditions.

281 **_Pre-noun fixations._**   In our first two analyses, we ask whether participants looked

282 more to the target than to the distractor during the predictive time window, given that the

283 verb is restricting. The first model tested whether there were more fixations to the target

284 object than to the distractor in the time window before the onset of the target noun. We

285 ran a regression model predicting the cumulative fixation probability in the last 50-ms bin

286 before noun onset from the verb condition (restricting = 1 vs. non-restricting = 0), object

287 type (target = 1 vs. distractor = 0), and their interaction, along with random effects for

participants and images (with no covariance between random effects because the model cannot converge with full covariance matrix). There were no significant effects, although the critical interaction was in the right direction [bar graph?] ($b = 0.05$, $SE = 0.03$, $p=0.15$).

Pre-verb-offset fixations.   Altmann & Kamide tested a second model, aligning the predictive time window with the offset of the verb rather than the onset of the noun as above. When we do the same, we again see that the critical interaction is not significant but numerically in the expected direction ($b = 0.05$, $SE = 0.03$, $p=0.20$).

First target fixations after verb.   Finally, we address whether participants look to the target faster in the restrictive vs. the non-restrictive condition, starting after the onset of the verb. [TO-DO: On average, participants looked to the target X ms after (AK's Table 1)..., I'll also want to say the lengths of the verbs. AK's Table 2] We ran a regression model predicting the timing of the first fixation to the target object, relative to the onset of the noun, with verb condition as a predictor, mean-centered verb duration as a covariate, and random intercepts and condition slopes for participants and scenes. There were no significant effects; participants looked sooner at the target in the restrictive condition, while accounting for verb duration and its interaction with condition, but this was not a statistically significant effect ($b = $ -121.91, $SE = 90.57$, $p=0.20$).

Aggressive Exclusion.   The second set of analyses used more aggressive exclusion criteria. First, we eliminated participants with 20 percent of fixations in any ROIs. This resulted in the elimination of 15 participants. Second, we excluded participants with validation accuracy under 50 percent, which eliminated an additional 35 participants. The following analyses included 22 participants.

We tested the same three models under these more aggressive exclusion criteria. The first two models, comparing target and distractor fixations in the predictive window, produced very similar results; the critical interaction was not statistically significant

314   (Pre-noun-onset window: $b = 0.07$, $SE = 0.06$, $p=0.23$; Pre-verb-offset window: $b = 0.05$,

315   $SE = 0.05$, $p=0.28$). However, the final model, which tested the effect of verb condition on

316   saccades to the target, yielded a statistically significant result, unlike in the previous set of

317   analyses ($b = $ -193.35, $SE = 96.33$, $p=0.05$).

318   ***Calibration.***   Participants' calibration quality was measured as the mean

319   percentage of fixations that landed within 200 pixels of the calibration point. Calibration

320   quality varied widely, ranging from 3.16% to 98.87%.

321   We tested whether a participant's calibration quality was correlated with their effect

322   size. There were three effects of interest: the verb-by-object interaction in predicting

323   fixation probabilities, both in the (1) pre-noun-onset and (2) pre-verb-offset windows

324   (calculated as the difference in target-over-distractor preference between verb conditions),

325   and (3) the effect of verb on the timing of the first target fixation (calculated as the

326   difference in target latency between verb conditions). Across the three effects of interest,

327   calibration quality was not significantly correlated (Effect 1: Pearson's $r = 0.03$, $p = 0.83$,

328   Effect 2: Pearson's $r = $ -0.05, $p = 0.73$, Effect 3: Pearson's $r = 0.04$, $p = 0.78$. However,

329   when the two interaction effects are calculated as the target advantage in the restricting

330   condition only (i.e. rather than a difference of differences), we see a significant correlation

331   between target advantage and calibration quality in the wider pre-noun window (Pearson's

332   $r = 0.21$, $p = 0.14$).

In-lab sample.

333

334   ***Minimal Exclusion.***   As in the remote sample, we checked whether there were

335   participants with 0 percent of fixations in any ROIs and there were none. We then

336   excluded participants with validation accuracy under 10 percent, resulting in 2 excluded

337   participants. The following analyses included 47 participants.

338   ***Cumulative Fixation Probabilities.***   For each sentence, the target time window

339   began at the onset of the verb and ended 2000 milliseconds later. This window was then

340 divided into 50-ms bins; for each participant and each trial, we recorded whether each

341 object was fixated during the 50-ms bin. Collapsing over trials and participants, and

342 averaging across distractors, we calculated the cumulative probability of fixation, shown in

343 Figure **??**.

344   ***Pre-noun fixations.***   In our first two analyses, we ask whether participants looked

345 more to the target than to the distractor during the predictive time window, given that the

346 verb is restricting. The first model tested whether there were more fixations to the target

347 object than to the distractor in the time window before the onset of the target noun. We

348 ran a regression model predicting the cumulative fixation probability in the last 50-ms bin

349 before noun onset from the verb condition (restricting = 1 vs. non-restricting = 0), object

350 type (target = 1 vs. distractor = 0), and their interaction, along with random effects for

351 participants and images (with no covariance between random effects because the model

352 cannot converge with full covariance matrix). There were no significant effects, although

353 the critical interaction was in the right direction [bar graph?] (b = -0.05, SE = 0.05,

354 p=0.25).

355   ***Pre-verb-offset fixations.***   Altmann & Kamide tested a second model, aligning

356 the predictive time window with the offset of the verb rather than the onset of the noun as

357 above. When we do the same, we again see that the critical interaction is not significant

358 but numerically in the expected direction (b = -0.06, SE = 0.04, p=0.17).

359   ***First target fixations after verb.***   Finally, we address whether participants look

360 to the target faster in the restrictive vs. the non-restrictive condition, starting after the

361 onset of the verb. [TO-DO: On average, participants looked to the target X ms after (AK's

362 Table 1)..., I'll also want to say the lengths of the verbs. AK's Table 2] We ran a

363 regression model predicting the timing of the first fixation to the target object, relative to

364 the onset of the noun, with verb condition as a predictor, mean-centered verb duration as a

365 covariate, and random intercepts and condition slopes for participants and scenes. There

366 were no significant effects; participants looked sooner at the target in the restrictive

367 condition, while accounting for verb duration and its interaction with condition, but this

368 was not a statistically significant effect ($b = 21.70$, $SE = 115.32$, p=0.85).

369 ***Calibration.*** As before, participants' calibration quality was measured as the mean

370 percentage of fixations that landed within 200 pixels of the calibration point. Calibration

371 quality ranged from 5.13% to 97.89%.

372 We tested whether a participant's calibration quality was correlated with their effect

373 size. Across the three condition effects of interest, calibration quality was not significantly

374 correlated (Effect 1 (pre-noun-onset): Pearson's r = -0.24, $p = 0.11$, Effect 2

375 (pre-verb-offset): Pearson's r = -0.19, $p = 0.20$, Effect 3 (first fixation): Pearson's r =

376 -0.12, $p = 0.41$. However, when the two interaction effects are calculated as the target

377 advantage in the restricting condition only (i.e. rather than a difference of differences), we

378 see a significant correlation between target advantage and calibration quality in the wider

379 pre-noun window (Pearson's r = -0.16, $p = 0.29$).

Discussion

380

Experiment 2

381

382 The second study was a replication attempt of Johansson and Johansson (2014),

383 which examined how visuospatial information is integrated into memory for objects. They

384 found that, during memory retrieval, learners spontaneously look to blank screen locations

385 where pictures were located during encoding (see Spivey & Geng, 2001) and that this

386 spatial reinstatement facilitates retrieval of the picture.

Methods

387

388 All stimuli, experiment scripts, data, analysis scripts, and a pre-registration are

389 available on the Open Science Framework at https://osf.io/xezfu/.

Participants.   60 participants were paid for their participation. The sample size was motivated in part by budget constraints, but was nonetheless 2.5x larger than the original sample size of 24). Data from 1 participant were not properly recorded due to unknown technical issues, so data from 59 participants were included in all analyses to follow.

Procedure.   The task began with a 9-point eye-tracker calibration and validation (Figure **??**).

The experiment consisted of two blocks each composed of an encoding phase and a recall phase. During the encoding phase, participants saw a grid indicating the four quadrants of the screen. Each quadrant contained six images of items belonging to the same category (see Figure **??**). The four categories were humanoids, household objects, animals, and methods of transportation. Each of the four quadrants was presented one at a time. First, a list of the items in the quadrant was shown, then the pictures of items were displayed in the quadrant. For each item, participants used their arrow keys to indicate whether the object was facing left or right. After the participant identified the direction of each item, they would have an additional 30 seconds to encode the name and orientation of each item in the quadrant. Finally, after all four quadrants were presented, participants were shown the full grid of 24 items and had 60 seconds to further encode the name and orientation of each item.

During the recall phase, participants listened to statements and responded by pressing the 'F' key for false statements and 'T' for true ones. Each statement fell into either an interobject or intraobject condition. Interobject statements were those that compared two different items in the grid (e.g. "The skeleton is to the left of the robot"), while intraobject statements were those that asked about the orientation of a single item (e.g. "The bus is facing right"). There were 48 total statements, with 24 interobject and 24 intraobject statements split evenly among the four quadrants. While listening to these statements, in the free-viewing block, participants saw a blank screen and were allowed to

417 freely gaze around the screen. During the fixed-viewing block, participants were asked to

418 fixate a small cross in the center of the screen throughout the recall phase. In both cases,

419 the mouse was obscured from the screen. Participants were randomly assigned to see the

420 fixed-viewing or free-viewing block first. Different images were used in each block.

421   After completing both encoding-recall blocks, participants were asked to answer a few

422 survey questions (such as whether they wore glasses or encountered any distractions).

423   The primary methodological difference between this replication and Johansson and

424 Johansson's study was that the original study included two additional viewing conditions

425 that were omitted from this replication due to time constraints. In those two conditions,

426 participant were prompted to look to a specific quadrant (rather than free viewing or

427 central fixation) which either matched or mismatched the original location of the

428 to-be-remembered item.

Results

430   **Replication.   Eye-gaze**. Looks during the retrieval period were categorized as

431 belonging to one of four quadrants based on the x,y coordinates. The critical quadrant was

432 the one in which the to-be-retrieved object had been previously located during encoding.

433 The other three quadrants were semi-randomly labeled "first", "second," third" (e.g., when

434 the critical quadrant was in the top left, the "first" quadrant was the top right quadrant,

435 but when the critical quadrant was in the top right, "first" corresponded to bottom right,

436 etc.). In both the fixed- and free-viewing condition, participants directed a larger

437 proportion of looks to the critical quadrant (see Figure **??**). This bias appeared larger in

438 the free-viewing condition, suggesting that the manipulation was (somewhat) effective.

439   The proportions of looks across quadrants in the free-viewing condition were analyzed

440 using a linear mixed-effects model with quadrant as the predictor (critical as the reference

level). The model included random intercepts and slopes for participants[1]. Proportions of looks were significantly higher for the critical quadrant compared to the other three (first: $b = -0.06$, $SE = 0.01$, $p<0.001$, second: $b = -0.08$, $SE = 0.01$, $p<0.001$, third: $b = -0.05$, $SE = 0.01$, $p<0.001$)

Response Time and Accuracy. Participants' response times and accuracies on memory questions are summarized in Figure **??**. Both dependent variables were analyzed with linear mixed-effects model with relation type (interobject = -0.5, intraobject=0.5) and viewing_condition (fixed = -0.5, free=0.5) and their interaction as the predictors. The model included random intercepts for participants[2]. Accuracy did not differ significantly between interobject and intraobject questions ($b = -0.05$, $SE = 0.03$, $p=0.05$). Participants were less accurate in the free viewing condition than the fixed condition ($b = -0.06$, $SE = 0.03$, $p=0.03$). Response times were slower for interobject (e.g., "The train is to the right of the taxi.") than intraobject (e.g., "The train is facing right.") questions ($b = -555.60$, $SE = 105.24$, $p<0.001$). Response times were slower in the free viewing condition than the fixed condition ($b = 260.98$, $SE = 105.24$, $p<0.001$). The interaction was not a significant predictor for response times or accuracy. These behavioral results are inconsistent with the original findings.

One possibility is that in-lab participants were much more compliant with the instruction to keep their gaze on central fixation (though these data are not reported in the original paper). When analyzing results from the subset of participants (N = 25) who were most compliant during the fixed-viewing block (at least 25% of their looks fell within 20% of the center of the display), the viewing condition effects and the interactions were not

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[1] `lme4` syntax: `lmer(proportion ~ quadrant + (1+quadrant|subject_id))`. Among other limitations, this approach violates the independence assumptions of the linear model because looks to the four locations are not independent. This analysis was chosen because it is analogous to the ANOVA analysis conducted in the original paper.

[2] `lme4` syntax: `lmer(DV ~ relation_type*viewing_condition + (1|subject_id))`

signficant. Given the smaller sample size we do not interpret these results further.

Calibration. Participants' calibration quality, measured as the mean percentage of fixations that landed within 200 pixels of the calibration point, varied substantially (between 17.78 and 100 %). The quality of a participant's calibration was not significantly correlated with the participant's effect size (*Pearson's r*= 0.20, $p = 0.14$) as measured by the difference between the proportion of looks to the critical quadrant minues the average proportion of looks to the average of the other three quadrants.

Discussion

As in Johansson and Johansson (2014) and Spivey and Geng (2001), during memory retrieval, learners spontaneously look to blank screen locations where pictures were located during encoding, suggesting that visuospatial information is integrated into the memory for objects. However, we did not observe a memory benefit, in terms of speed or accuracy, of spatial reinstatement via gaze position during retrieval of the picture. We can speculate that this may be due to the fact that participants struggled to maintain their gaze fixed in the center in the fixed-viewing condition, such that the difference between the fixed- and free-viewing conditions was minimal. Crucially for the current purposes, the webcam-based eye-tracking measurements were successful in replicating the key eye-tracking results.

Experiment 3

The third study was a partial replication attempt of Manns, Stark, and Squire (2000). This experiment used the visual paired-comparison, which involves presenting a previously-viewed image and novel image together and measuring the proportion of time spent looking at each image. The expected pattern of results is that participants will look more at novel objects. They Manns et al. (2000) hypothesized that this pattern of behavior could be used to measure the strength of memories. If a viewer has a weak

memory of the old image, then they may look at the old and new images roughly the same amount of time. They tested this in two ways. First, they showed participants a set of images, waited five minutes, and then paired those images with novel images. They found that participants spent more time (58.8% of total time) looking at the novel images. They then measured memory performance one day later and found that participants were more likely to recall images that they had spent less time looking at during the visual paired-comparison task the previous day.

Methods

The stimuli, experimental code, and data and analysis scripts can be found on the Open Science Framework at https://osf.io/k63b9/. The pre-registration for the study can be found at https://osf.io/48jsv . We inadvertently did not create a formal pre-registration using the OSF registries tool, but this document contains the same information and is time stamped prior to the start of data collection.

Participants.   Our pre-registered target was 50 participants. 51 participants completed the first day of the experiment and 48 completed the second day. Following Manns et al., we excluded 3 participants due to perfect performance on the recognition memory test because this prevents comparison of gaze data for recalled vs. non-recalled images. Our final sample size was 45 participants.

Procedure.   The task began with a 7-point eye-tracker calibration (each point was presented 3 times in a random order) and validation with 3 points (each presented once). The point locations were designed to focus calibration on the center of the screen and the middle of the left and right halves of the screen (Figure **??**).

The experiment was administered over the course of two consecutive days. It consisted of three sections: a presentation phase, a test phase, and a recognition test. The first two phases occurred on the first day, while the recognition test occurred on the second

day.

During the presentation phase, participants viewed 24 pairs of identical color photographs depicting common objects. Each pair was presented for 5 seconds and an interval of 5 seconds elapsed before the next pair was shown. The order of the photographs was randomized and different for each participant. After completion of the presentation phase, participants were given a 5-minute break during which they could look away from the screen.

After the break, they were prompted to complete the eye-tracking calibration again before beginning the test phase. During this phase, participants again viewed 24 pairs of photographs with an interstimulus duration of 5 seconds. In each pair, one photograph was previously seen during the presentation phase, while the other was new. Which pictures were old or new was counterbalanced across participants. For half of the participants in each counterbalancing group, the new and old photographs were reversed.

Approximately 24 hours after completing the first session, with a leeway interval of 12 hours to accommodate busy schedules, participants were given the recognition test. It consisted of 48 photographs, presented one at a time. Each was shown on the screen for 1 second, followed by a 1 second interstimulus interval. Half of the photographs had been viewed twice on the previous day and were deemed the "targets." The other half depicted an object with the same name as an object in one of the old photographs, but had not been viewed before, deemed "foils." Each photograph remained on the screen until the participants indicated whether or not they had seen it before by pressing 'y' for yes and 'n' for no. After they pressed one of the two keys, a prompt on the screen asked them to rate their confidence in their answer from 1 as a "pure guess" to 5 as "very sure." by clicking on the corresponding number on the screen. No feedback on their responses was given during the test.

The experimental design is visually depicted in Figure **??**

538   **Materials.**   Images were selected XXX. . .

539   There were two modifications we made to the methods of the original experiment. As

540   we are only replicating the declarative memory component of the original experiment, we

541   did not have a "priming group." Therefore, we followed only the procedure for the "looking

542   group." Additionally, for each section of the study, the stimuli was presented on a single

543   screen instead of two screens due to the constraints of the online experiment format.

Results

545   ***Day 1.***   During day 1 of the experiment, participants viewed pairs of images, one of

546   which was always familiar and the other unfamiliar. We calculated a looking score for each

547   participant, defined as the proportion of gaze samples in the ROI of the unfamiliar image

548   out of all the gaze samples that were in either ROI. Gaze samples that were not in either

549   ROI were not included in this analysis. A looking score of 0.5 indicates that participants

550   looked equally often at the familiar and unfamiliar images, while a looking score above 0.5

551   indicates a preference for the unfamiliar object and a looking score below 0.5 indicate a

552   preference for the familiar object.

553   Of the 1248 trials in the experiment, 78 had no fixations in either ROI, and so the

554   looking score was unknown. We removed these trials from this analysis.

555   The mean looking score was 0.55 ($SD = 0.10$). This significantly greater than 0.5,

556    $t(49) = 3.29$, $p = 0.00$, indicating that participants did show a preference for looking at the

557   novel objects.

558   ***Day 2.***   In all of these analyses, we excluded the 16 (out of 2304) trials where the

559   response time for the recognition judgment was greater than 10 seconds.

560   Participants correctly identified whether the image was familiar or unfamiliar 87.09%

561   ($SD = 10.49$) of the time. After excluding the 3 participants who responded correctly to all

562   images, the average confidence rating for correct responses (M = 3.51; SD = 0.41) was

563 significantly higher than their average confidence ratings for incorrect responses (M = 2.55;

564 SD = 0.75), t(44) = -9.36, p = 0.00 . Among the same subset of participants, response

565 times for correct responses (M = 1,443.49, SD = 413.94) were also significantly faster than

566 for incorrect responses (M = 2,212.65, SD = 1,733.76), t(44) = 3.43 , p = 0.00.

567   To see whether preferentially looking an the unfamiliar object on day 1 was

568 correlated with confidence and response time for correct responses on day 2, we computed

569 the correlation coefficient between day 1 looking scores and day 2 confidence/RT for each

570 participant. Following the original analysis, we transformed these values using the Fisher

571 p-to-z transformation. Using one-sample t-tests, we found no significant different from 0 for

572 the correlation between looking score and confidence ratings, t(38) = 0.46, p = 0.65

573 (excluding the subjects who gave the same confidence judgment for all images), nor the the

574 correlation between looking score and RT, t(46) = 0.49, p = 0.63.

575   **Effects of ROIs.**   In the original experiment, the two objects on day 1 were

576 presented on two separate monitors and gaze was coded by manually coding video

577 recordings. In our replication analysis, we analyzed eye movement data using ROIs defined

578 around the two images. In this section we explore an alternative coding of the eye

579 movement data by coding simply left half vs. right half of the screen. The coarser coding

580 may be more appropriate for webcam-based eyetracking.

581   The correlation between looking scores using the ROI method and the halves method

582 is 0.76.

583   ***Looking Scores.***   When looking scores are coded as left vs. right half of the screen,

584 we find that participants looked more at the novel object. The mean looking score was 0.54

585 ($SD = 0.08$). This was significantly greater than 0.5, $t(50) = 3.51$, $p = 0.00$.

586   ***Correlations with Day 2 Performance.***   Performance on day 2 remained

587 uncorrelated with day 1 looking scores after switching the coding of gaze. We found no

588 significant different from 0 for the correlation between looking score and confidence ratings,

t(39) = 0.74, p = 0.47 (excluding the subjects who gave the same confidence judgment for all images), nor the the correlation between looking score and RT, t(47) = 0.28, p = 0.78.

Calibration.

Calibration Accuracy.

Correlation with Effects.   To see if calibration success is correlated with the eye tracking effects, we calculated a calibration score for each participant. The calibration score was the average proportion of samples within XXX pixels of the validation points during the final validation phase before the eye tracking is performed.

Calibration scores were not correlated with looking scores, regardless of which method was used to calculate looking scores.

We then looked at the correlation of calibration scores with the correlation between day 2 memory performance and day 1 looking scores for both kinds of behavioral and looking measures. None of the four relationships showed a significant correlation.

Discussion

<div align="center">

Experiment 4

</div>

The fourth study was a replication attempt of Experiment 1 in Ryskin, Qi, Duff, and Brown-Schmidt (2017), which was closely modeled on Snedeker and Trueswell (2004). These studies used the visual world paradigm to show that listeners use knowledge of the co-occurrence statistics of verbs and syntactic structures to resolve ambiguity. For example, in a sentence like "Feel the frog with the feather," the phrase "with the feather" could be describing the frog, or it could be describing the instrument that should be used to do the "feeling." When both options (a frog holding a feather and a feather by itself) are available in the visual display, listeners rely on the verb's "bias" (statistical co-occurrence either in norming or corpora) to rapidly choose an action while the sentence is unfolding. .

Methods

The stimuli, experimental code, and data and analysis scripts can be found on the Open Science Framework at the following link, https://osf.io/x3c49/. The pre-registration for the study can be found at https://osf.io/3v4pg.

Participants.   57 participants were paid $2.50 for their participation. A sample size of 60 was initially chosen (but not reached in time) because we wanted to replicate the experiment with greater statistical power. Note that the original study had a sample size of 24.

Procedure.   After the eye-tracking calibration and validation (Figure **??**), participants went through an audio test so they could adjust the audio on their computer to a comfortable level. Before beginning the experiment, they were given instructions that four objects would appear, an audio prompt would play, and they should do their best to use their mouse to act out the instructions. They then went through three practice trials which were followed by 54 critical trials and 24 filler trials presented in a random order.

During a trial, four pictures were displayed (target animal, target instrument, distractor animal, distractor instrument), one in each corner of the screen, and participants heard an audio prompt that contained instructions about the action they needed to act out (e.g., "Rub the butterfly with the crayon"; see Figure **??**)[3]. Using their cursor, participants could act out the instructions by clicking on objects and moving them or motioning over the objects[4]. After the action was completed, the participants were instructed to press the space bar which led to a screen that said "Click Here" in the middle in order to remove

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[3] In the original study, the pictures appeared one by one on the screen and their names were played as they appeared. We removed this introductory portion of the trial to save time

[4] As opposed to the original study we recorded mouse movement instead of clicking behavior since not all of the audio prompts required clicking. For example, the sentence "locate the camel with the straw" may not involve any clicking but rather only mousing over the camel.

bias in the eye and mouse movements from the previous trial. The experiment only allowed the participants to move on to the next trial once the audio was completely done playing and the mouse had been moved over at least one object.

Materials.   The images and audios presented to the participants were the same stimuli used in the original study (available here). The critical trials were divided into modifier-biased, instrument-biased, and equibiased conditions, and the filler trials did not contain ambiguous instructions. Two lists of critical trials were made with different verb and instrument combinations (e.g., "rub" could be paired with "panda" and "crayon" in one list and "panda" and "violin" in the second list). Within each list, the same verb was presented twice but each time with a different target instrument and animal. The lists were randomly assigned to the participants to make sure the effects were not caused by the properties of the animal or instrument images used. The list of verbs used can be found in Appendix A of the original study.

Results

Replication.   The location of initial mouse movements was used to assess whether the final interpretation of ambiguous sentences was biased by the verb. Figure **??** suggests that listeners were more likely to move their mouse first over the target instrument when the verb was equi-biased than when the verb was modifier-biased and even more so when the verb was instrument-biased. The opposite graded pattern can be observed for mouse movements over the target animal.

A mixed-effects logistic regression model was used to predict whether the first movement was on the target instrument with the verb bias condition as an orthogonally contrast-coded (instrument vs. equi & modifier: inst = -2/3, equi = 1/3, mod = 1/3; equi vs. modifier: inst = 0, equi = -1/2, mod = 1/2) fixed effect. Participants and items were

entered as varying intercepts with by-participant varying slopes for verb bias condition[5].

Participants were more likely to first move their mouse over target instruments in the

instrument-biased condition relative to the equi-biased and modifier-biased condition ($b =$

-1.50, $SE = 0.25$, $p < 0.01$). Further, participants were more likely to first move their

mouse over target instruments in the equi-biased condition relative to the modifier-biased

condition ($b =$ -1.10, $SE = 0.29$, $p < 0.01$)

Gaze fixations were time-locked to the auditory stimulus on a trial by trial basis and

categorized as being directed towards one of the four items in the display if the x, y

coordinates fell within a rectangle containing the image. Figure **??** suggests that the

participants made more fixations to the target animal when the verb was modifier-biased

compared to when the the verb was equi-biased and they looked at the target animal least

when the verb was instrument-biased. The pattern was reversed for looks to the target

instrument.

In order to assess how verb bias impacted sentence disambiguation as the sentence

unfolded, the proportion of fixations was computed in three time windows: the

verb-to-animal window (from verb onset + 200 ms to animal onset + 200 ms), the

animal-to-instrument window (from animal onset + 200 ms to instrument onset + 200 ms),

and the post-instrument window (from instrument onset + 200 ms to instrument onset +

1500ms + 200 ms). Mixed-effects linear regression models were used to predict the

proportions of fixations to the target animal within each time window with the verb bias

condition as an orthogonally contrast-coded (instrument vs. equi & modifier: inst = -2/3,

equi = 1/3, mod = 1/3; equi vs. modifier: inst = 0, equi = -1/2, mod = 1/2) fixed effect.

Participants and items were entered as varying intercepts[6]. In the *verb-to-noun* window,

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[5] lme4 syntax: glmer(is.mouse.over.instrument ~ verb_bias + (1 + verb_bias | participant) +
(1 | item), family="binomial", data=d)

[6] lme4 syntax: lmer(prop.fix.target.animal ~ verb_bias + (1 + verb_bias | participant) + (1
| item), data=d). A model with by-participant varying slopes for verb bias condition was first attempted

participants did not look more at the target animal in any of the verb bias conditions

(Instrument vs. Equi and Modifier: b = -0.01, SE = 0.02, p = 0.59; Equi vs. Modifier: b =

0, SE = 0.02, p = 1). In the *noun-to-instrument* window, participants looked more at the

target animal in the modifier-biased condition and equi-biased conditions relative to the

instrument-biased condition (b = 0.03, SE = 0.01, p < 0.01) and in the modifier biased

relative to the equi-biased condition (b = 0.02, SE = 0.01, p < 0.05). In the

post-instrument window, participants looked more at the target animal in the

modifier-biased condition and the equi-biased conditions relative to the instrument-biased

condition (b = 0.08, SE = 0.02, p < 0.01) but not significantly so in the modifier biased

condition relative to the equi-biased condition (b = 0.03, SE = 0.02, p = 0.15).

Comparison to in-lab data.   The web version of the study qualitatively replicates

the action and eye-tracking results of the original dataset (Ryskin et al., 2017). The mouse

click results from both studies are summarized in Figure **??**. The quantitative patterns of

clicks were similar to those observed in the original dataset, though for Instrument-biased

verbs, clicks were closer to evenly split between the animal and the instrument relative to

the in-lab study where they were very clearly biased toward the instrument.

The eye-tracking results from both studies are summarized in Figure **??**. For

simplicity, and to reflect the dependent variable used in analyses, we average the

proportion of fixations to the target animal within each time window. Though the

qualitative patterns are replicated, proportions of fixations to the target animal were much

lower in the web version of the study. This may reflect the fact that participants in the web

study are less attentive and/or the quality of the webgazer eye-tracking system is lower,

relative to the Eyelink 1000 which was used for the original study.

Calibration.   Participants' calibration quality, measured as the mean percentage of

fixations that landed within 200 pixels of the calibration point, varied substantially

———

but did not converge.

706 (between 2.22 and 97.36 %). The quality of a participant's calibration significantly

707 correlated with the participant's effect size (*Pearson's r*= 0.29, $p < 0.05$). The difference

708 in target animal fixation proportions between modifier and instrument conditions was

709 higher for participants with better calibration

710   Replicating the linear mixed-effects analysis (in the post-instrument onset time

711 window only) on a subset of 35 participants with calibration quality >50% suggests that

712 the effect of verb bias condition was larger in this subset than in the full dataset.

713 Participants looked more at the target animal in the modifier-biased condition and the

714 equi-biased conditions relative to the instrument-biased condition ($b = 0.10$, $SE = 0.02$, p

715 < 0.001) but not significantly so in the modifier biased condition relative to the equi-biased

716 condition ($b = 0.02$, $SE = 0.02$, $p = 0.29$).

717   Replicating the linear mixed-effects analysis (in the post-instrument onset time

718 window only) on a subset of 19 participants with calibration quality >75% suggests that

719 the effect of verb bias condition was larger in this subset than in the full dataset.

720 Participants looked more at the target animal in the modifier-biased condition and the

721 equi-biased conditions relative to the instrument-biased condition ($b = 0.11$, $SE = 0.03$, p

722 < 0.001) but not significantly so in the modifier biased condition relative to the equi-biased

723 condition ($b = 0.05$, $SE = 0.03$, $p = 0.13$).

724   **Effects of ROIs.**   Eye-tracking on the web differs critically from in-lab eye-tracking

725 in that the size of the display differs across participants. Thus the size of the ROIs differs

726 across participants. The current version of the web experiment used a bounding box around

727 each image to determine the ROI. This approach is flexible and accomodates variability in

728 image size, but may exclude looks that are directed at the image but fall outside of the

729 image (due to participant or eye-tracker noise) as show in Figure **??**a. Alternatively, The

730 display can be split into 4 quadrants which jointly cover the entire screen (see Figure **??**b).

731   Categorizing gaze location based on which of the four quadrants of the screen the

coordinates fell in, increases the overall proportions of fixations (see Figure **??**). In the

post-instrument window, participants looked more at the target animal in the

modifier-biased condition and the equi-biased conditions relative to the instrument-biased

condition ($b = 0.08$, $SE = 0.02$, $p < 0.01$) and marginally so in the modifier biased

condition relative to the equi-biased condition ($b = 0.04$, $SE = 0.02$, $p = 0.05$). Effect size

estimates appeared somewhat larger and noise was somewhat reduced when using the

quadrant categorization relative to the bounding box-based ROIs.

Discussion

Experiment 5

The fifth study was a replication attempt of Shimojo et al. (2003), which found that

human gaze is actively involved in preference formation. Separate sets of participants were

shown pairs of human faces and asked either to choose which one they found more

attractive or which they felt was rounder. Prior to making their explicit selection,

participants were increasingly likely to be fixating the face they ultimately chose, though

this effect was significantly weaker for roundness discrimination.

Note that Shimojo and colleagues compare five conditions, of which we replicate only

the two that figure most prominently in their conclusions: the "face-attractiveness-difficult

task" and the "face-roundness task".

Methods

All stimuli, experiment scripts, data, and analysis scripts are available on the Open

Science Framework at https://osf.io/eubsc/ (https://osf.io/eubsc/). The study

pre-registration is available at https://osf.io/tv57s (https://osf.io/tv57s).

Participants.   50 participants for the main task were recruited on Prolific and were

paid \$10/hour. 8 subjects, 4 from the attractiveness task group and 4 from the roundness

756 task group, were excluded for incorrect validations. After this data exclusion, we ended up

757 with 21 participants each for the attractiveness task and the roundness task. The original

758 sample size in Shimojo et al. (2003) was 10 participants total.

759   **Procedure and Design.**   At the beginning of the experimental task, participants

760 completed a 9-point eye-tracker calibration (each point appeared 3 times in random order)

761 and 3-point validation. The validation point appeared once at center, middle left, and

762 middle right locations in random order (see Figure **??**).

763   During each trial of the main task, two faces were displayed on the two halves of the

764 screen, one on the left and one on the right (as in Figure **??**). Participants were randomly

765 assigned to one of two tasks: attractiveness or shape judgment. In the attractiveness task,

766 participants were asked to chose the more attractice face in the pair and in the shape

767 judgment task participants were asked to pick the face that appeared rounder. They

768 pressed the "a" key on their keyboard to select the face on the left and the "d" key to select

769 the face on the right. A fixation cross appeared in the center of the screen between each set

770 of faces. Participants were asked to look at this fixation cross in order to reset their gaze in

771 between trials (???). The order of the 19 face pairs was random for each participant.

772   **Materials and Norming.**   The faces in our replication were selected from a set of

773 1,000 faces within the Flickr-Faces-HQ Dataset. (The face images used in Shimojo et

774 al. were from the Ekman face database and the AR face database.) These images were

775 chosen because the person in each image was looking at the camera with a fairly neutral

776 facial expression and appeared to be over the age of 18. 27 participants were recruited on

777 Prolific to participate in stimulus norming (for attractiveness). They were paid $XX for

778 completing the experiment. Data from 3 participants was excluded because their mode

779 response made up more than 50% of their total responses , for a total of 24 participants in

780 the norming. They each viewed all 172 faces and were asked to rate them on a scale from 1

781 (less attractive) to 7 (more attractive) using a slider. Faces were presented one at a time

782 and in a random order for each participant. Following Shimojo et al., 19 face pairs were

made by matching two faces that had a difference in mean attractiveness ratings that was 0.25 points or lower and that matched in gender, race, and age group (young adult, adult, or older adult).

Data analysis. In the original study, a video-based eye tracker was used. The eye movements of participants were recorded with a digital camera downsampled to 33.3 Hz, with eye position was then determined automatically with MediaAnalyzer software. In our study, subjects supplied their own cameras, so hardware sampling rate varied. However, data was collected at 20 Hz.[TODO - CONFIRM]

Results

Due to large variation in response time latency, Shimojo and colleagues analyzed eye gaze for the 1.67 seconds prior to the response. This duration was one standard deviation of the mean response time, ensuring that all timepoints analyzed have data from at least 67% of trials. In our dataset, one standard deviation amounts to 1.85 seconds. We then binned eyegaze data into 50 ms bins rather than the 30 ms bins used by Shimojo and colleagues, reflecting the different sampling rates.

Following Shimojo and colleagues, data for each condition were fit using a four-parameter sigmoid (Fig. **??**). These fit less well than in the original paper for both the attractiveness judgment ($R^2 = 0.84$ vs. 0.91) and the roundness judgment ($R^2 = 0.54$ vs. 0.91).

From these curves, Shimojo and colleagues focus on two qualitative findings. First, they note a higher asymptote for the attractiveness discrimination task relative to roundness discrimination. Qualitatively, this appears to replicate. However, their statistical analysis – a Kolmogorov-Smirnov test for distance between two distributions – is not significant (D = 0.19, p = 0.53), though it should be noted that this is a very indirect statistical test of the hypothesis and probably not very sensitive.

808   The second qualitative finding they note is that the curve for the roundness judgment

809   "saturates" (asymptotes) earlier than the curve for the attractiveness judgment. They do

810   not present any statistical analyses, but it is clear qualitatively that the result does not

811   replicate.

812   **Calibration.**   As in the previous experiments, calibration score was defined as the

813   average proportion of samples within 200 pixels of the validation point during the final

814   validation phase before the eye tracking is performed. The distribution across participants

815   is shown in Fig. **??**.

816   To determine whether calibration accuracy influenced our key effects, we calculated

817   the percentage of samples during the task in which the participant was fixating the face

818   they ultimately chose. There was a significant correlation for both the attractiveness

819   judgments (r = 0.47 [0.04, 0.75], p = 0.03) and the roundness judgments (r = 0.60 [0.23,

820   0.82], p = 0). Inspection of Fig. **??** reveals that this correlation is due to a handful of

821   particpiants with calibration values below 50%.

822   Thus, we re-analyzed the data, removing the participants whose calibration accuracy

823   was not greater than 50%. This slightly improved the fits of the sigmoids (Attractiveness:

824   $R^2 = 0.79$; Roundness: $R^2 = 0.60$). However, the difference between sigmoids remained

825   non-significant using the Kolmogorov-Smirnov test (D = 0.22, p = 0.36). Descriptively, the

826   results do not look substantially different (Fig. **??**).

827   **Effects of ROIs.**   In the original experiment, eye gazes that did not directly fixate

828   one or other of the faces were excluded. In this section we explore an alternative coding of

829   the eye movement data by coding simply left half vs. right half of the screen. The coarser

830   coding may be more appropriate for webcam-based eyetracking.

831   Only a small percentage of samples (7.00%) involved looks to anything other than

832   one of the two faces. Thus, not surprisingly, the correlation between percentage of time

833   spent fixating the to-be-chosen face using the ROI method and the halves method was near

834 ceiling (r = 0.97 [0.97, 0.98], p = 0). Since the choice of method had almost no effect on

835 whether participants were coded as fixating one face or the other, we did not further

836 investigate the effect of method choice on the analytic results.

837 **Discussion**

838 **Combined Analyses?**

839 • Pooling data from all experiments we can look at patterns in the calibration and
840 validation data

841 **General Discussion**

842 • E1:

843 • E2:

844 – replication of key result with 4 quadrants
845 – calib quality doesn't seem to matter
846 – if attempting to control where people are looking, think about modifying task. . .

847 • E3:

848 • E4:

849 • E5:

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