Raising a Small Language Model: From Imitation to Curiosity-Driven Learning in Dialogue Games

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Abstract

Children do not learn language from text corpora, rather they learn it in interaction by imitating competent speakers, receiving feedback, practicing, being corrected, and playing. The LM Playschool Challenge asks whether language models can profit from analogous regimes. We take the smallest recommended base model, Qwen3.5-2B (clemscore 13.63 in our evaluation environment), and traverse a sequence of five interaction-derived learning regimes that mirror a developmental progression: imitation of competent others (supervised fine-tuning on success-filtered transcripts), learning from outcome contrasts (direct preference optimisation), self-imitation (fine-tuning on the model's own successes), corrective feedback (contrastive pairs from the model's own failures against its own and expert successes), and intrinsically motivated trial-and-error (online GRPO with a novelty bonus). The offline regimes carry the model to 67.6 clemscore on our validation evaluation ($+54.0$ over the base model). On the organisers' held-out evaluation, our two submitted checkpoints achieve the largest out-of-domain gains of any submission in the challenge ($+11.9$ and $+10.8$ clemscore, against a maximum of $+6.5$ for any other entry), with in-domain gains of $+33.2$ and $+37.3$ and static-benchmark performance unchanged. Online GRPO *regresses* from its initialisation (-5.0) unless augmented with a random-network-distillation novelty bonus, which fully recovers the loss ($+5.0$ over the plain-GRPO control), evidence that the bonus primarily counteracts the forgetting that narrow online training induces on untrained games.

1 Introduction

The majority of language models knowledge comes from learning by observation: next-token prediction over text produced by others, refined by single-turn instruction tuning. However, human language acquisition looks different. It is usage-based and interactive (Tomasello, 2003): competence grows through joint activity with more capable partners—the zone of proximal development (Vygotsky, 1978), through feedback on one's own productions, and through self-motivated play at the edge of one's ability, sought out by intrinsically motivated exploration (Berlyne, 1960; Oudeyer et al., 2007). The LM Playschool Challenge operationalises this contrast: models are placed in Game-Master-mediated dialogue games (Chalamalasetti et al., 2023) inside the playpen environment (Horst et al., 2025), and submissions are judged by how much interactive competence (*clemscore*) they gain over a base model, while a static-benchmark (*statscore*) checks that general abilities survive. Appendix A describes the games and five static benchmarks.

We treat the challenge as an opportunity to sweep the space of interaction-derived developmental learning regimes in a controlled way, on a deliberately small model where every effect is visible. Qwen3.5-2B (Qwen Team, 2026) starts at clemscore 13.63: it does not so much lose games as fail to play them, aborting episodes by violating the games' strict output formats. From this starting point we apply in sequence, five regimes chosen to mirror a developmental progression (Section 3): **(R1)** imitation of competent others; **(R2)** learning from success/failure contrasts; **(R3)** self-imitation of one's own successes; **(R4)** corrective feedback on one's own failures; and **(R5)** intrinsically motivated online play, with and without a curiosity signal.

Our contributions are: (i) a compute-light offline recipe (R1+R2, one GPU, under five training hours) that lifts a 2B model by $+37.3$ clemscore in-domain and $+10.8$ out-of-domain on the challenge's held-out evaluation—the largest out-of-domain improvement among all submissions, at every base-model scale—while leaving statscore unchanged; (ii) a controlled demonstration that on-

line GRPO from a strong initialisation *regresses* unless equipped with an intrinsic novelty bonus, which recovers the regression exactly (R5); (iii) a systematic comparison showing that regimes using the model's own behaviour (R3–R5) at best match the strongest offline regime and more often regress, at this scale and budget.

2 Background and Related Work

Dialogue games as curriculum. clembench (Chalamalasetti et al., 2023) investigates functional language competence through games with hard, parser-enforced move formats; playpen (Horst et al., 2025) turns these games into a learning environment and releases a corpus of gameplay transcripts. The playpen paper's reference experiments found SFT on successful transcripts helps in-domain but erodes other skills, while online GRPO (Shao et al., 2024) gives smaller, balanced gains—a trade-off our regime sweep revisits with explicit controls.

Learning regimes and their cognitive analogues. Imitation of filtered demonstrations underlies STaR (Zelikman et al., 2022), rejection-sampling fine-tuning (Yuan et al., 2023), ReST (Gulcehre et al., 2023) and SPIN (Chen et al., 2024); it is the machine analogue of observational learning from competent others (Tomasello, 2003). Preference learning (Christiano et al., 2017; Stiennon et al., 2020; Ouyang et al., 2022; Rafailov et al., 2023; Ethayarajh et al., 2024) corresponds to learning from evaluative feedback. Intrinsic motivation—curiosity as prediction error (Pathak et al., 2017; Burda et al., 2019) or as learning progress (Oudeyer et al., 2007)—formalises self-directed play; we use random network distillation (Burda et al., 2019), to our knowledge its first application to Game-Master-mediated dialogue games.

Forgetting. Narrow fine-tuning erodes prior abilities (McCloskey and Cohen, 1989; Kirkpatrick et al., 2017; Luo et al., 2023); replaying general data mitigates it (Scialom et al., 2022). The challenge's statscore (BBH, MMLU-Pro, IFEval, CLadder, EQ-Bench; Suzgun et al., 2023; Wang et al., 2024; Zhou et al., 2023; Jin et al., 2023; Paech, 2023) makes the erosion measurable; a 12% SmolTalk (Hugging Face TB Research, 2024) mix in R1 is our (successful) mitigation.

3 Learning Regimes

Throughout, π_θ denotes the policy, trained with LoRA adapters (Hu et al., 2022) that are merged after each regime; R3–R5 each initialise from the merged R2 model.

3.1 R1: Imitation of competent others

Rationale. The first thing a novice does in a new social practice is watch someone who can already do it. Observational learning (Bandura, 1977) and usage-based accounts of acquisition (Tomasello, 2003) agree that imitation of competent others is the entry point into a convention-governed activity, and dialogue games are nothing if not convention-governed as demonstrated here too: the base model's 13.63 is overwhelmingly a *protocol* failure (aborted episodes), not a strategic one. Our hypothesis for R1 was therefore narrow: imitating successful play should first of all teach the move grammar, with strategy learning transferred as a by-product.

Construction. From the public playpen-data `interactions` corpus (34,909 per-player trajectories annotated with game, experiment, task id, role, source model and outcome) we keep only *successful* episodes, map all Game-Master and partner turns to the user role (merging consecutive same-role turns), and drop trajectories without an assistant turn (≈20.2k trajectories) for SFT. Anticipating the forgetting cost that imitation of a narrow practice incurs (Luo et al., 2023), we mix in general instruction data (SmolTalk, smol-magpie-ultra) at ≈12%, for 22,626 conversations, and minimise cross-entropy on assistant tokens only—to ensure the model learns to *produce moves*, not to imitate the Game Master.

3.2 R2: Learning from outcome contrasts

Rationale. Imitation shows the learner only what success looks like and it carries no information about failure, and the R1 model's residue confirmed the gap: format compliance was nearly solved while move *quality* lagged. Learners, by contrast, exploit evaluative feedback—signals that grade their productions rather than merely exemplify good ones. Explicit correction is rare and noisy in child-directed speech (Marcus, 1993); children encounter the second signal largely through communicative outcome, when an utterance fails to achieve its interactive goal and the ensuing repair sequence turns the conversation itself into evidence

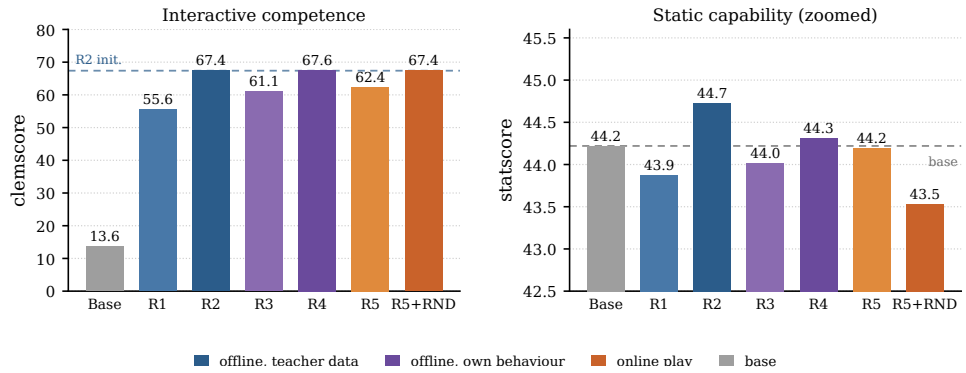

Figure 1: Learning regimes across both metrics, in a single frozen evaluation environment. Left: clemscore; the dashed line marks the R2 initialisation shared by R3–R5. Right: statscore on a zoomed axis, flat across all regimes. Each regime applies one training stage on top of the previous model: SFT (R1 - Imitation, R3 - Self-imitation), DPO (R2 - Contrast, R4 - Correction), and online GRPO without and with an intrinsic novelty bonus (R5, R5+RND). Colour encodes the source of the learning signal.

(Clark, 2020). The games make such feedback almost free: because episodes on the same instance share an identical initial state, a successful and a failed first move form a minimal pair whose only difference is quality. R2 tests whether this contrastive signal repairs exactly the residue imitation left.

Construction. For a fixed (game, experiment, task id, role) we pair the first move y^+ of a successful episode with the first move y^- of an aborted episode (falling back to lost ones), oversampling still-abort-prone games $4\times$ (3,418 pairs), and optimise the DPO objective (Rafailov et al., 2023)

$$\mathcal{L}_{\text{DPO}} = -\log \sigma\Big(\beta \log \frac{\pi_\theta(y^+|x)}{\pi_{\text{ref}}(y^+|x)} - \beta \log \frac{\pi_\theta(y^-|x)}{\pi_{\text{ref}}(y^-|x)}\Big),$$

with the frozen R1 model as π_{ref}: learning from contrast, anchored to what imitation established.

3.3 R3: Self-imitation

Rationale. R1 and R2 learn *from records of* interaction; the challenge's deeper question concerns learning *in* interaction. The intuitive step across that line is practice: let the model play, keep what worked, and rehearse it—the machine analogue of a child repeating a newly mastered routine, and the pattern behind ReST-style methods (Gulcehre et al., 2023). Because the R2 model now succeeds often, its own play is a plentiful source of positive examples; R3 asks whether rehearsing them consolidates competence.

Construction. We play the R2 model through the full public instance sets of eight games (490 episodes, 63% successful; validation-overlapping instances excluded), reconstruct per-player trajectories from the interaction logs (516 examples), and run one light SFT epoch. Unlike R2, this objective has *no anchor*: whatever gradient the data carries, the policy follows.

3.4 R4: Corrective feedback

Rationale. R3 regressed (Section 5), and its failure suggested a diagnosis with a developmental intuition: rehearsing one's own successes adds little information and when delivered without an anchor, it perturbs what already works. What children demonstrably profit from instead is correction of their *errors*—adult reformulations, or *recasts*, that follow a faulty production with a well-formed one in the same context (Chouinard and Clark, 2003). On the contrast theory of negative input, the corrective signal lies precisely in the juxtaposition of the learner's erroneous form with a correct alternative for the same intended meaning (Saxton, 1997), and such input improves subsequent grammaticality where positive models alone do not (Saxton et al., 2005). The games again supply this almost for free: wherever our model failed an instance, the transcript corpus usually contains a stronger model succeeding on the identical instance. R4 therefore reuses the model's own play but as *contrastive* evidence—pairing its failures against its own or a stronger model's successes, taught through the anchored objective that R2 validated, testing at once the recast hypothesis and the anchoring hypothesis raised by R3.

Construction. From two rollout passes of the R2 model (greedy and sampled, t=0.7) we build 201 first-move preference pairs from two sources: (i) *on-policy* pairs (107) from instances where the two passes disagreed in outcome—chosen is the model's own successful first move, rejected its own failed one on the identical instance; and (ii) *hybrid* pairs (94) from instances the model failed in both passes—chosen is a stronger model's success on that instance (a recast), rejected the model's own failure. We then run one DPO epoch from the merged R2 model. The on-policy pairs are drawn precisely from the model's competence frontier (instances of variable outcome), linking R4 to the learning-progress principle we exploit in R5.

3.5 R5: Intrinsically motivated play

Rationale. Our last regime is: genuine online play, where the learner's own exploration generates the curriculum. Developmental theory is specific about what makes play educational: children do not sample activities uniformly but gravitate toward tasks of intermediate difficulty—neither mastered nor impossible—driven by intrinsic signals such as novelty and learning progress (Berlyne, 1960; Kidd and Hayden, 2015; Oudeyer et al., 2007); machine counterparts formalise curiosity as prediction error (Pathak et al., 2017; Burda et al., 2019). This predicts a specific failure mode for *plain* online RL in our setting: since the R2 policy already succeeds on most training instances, many rollout groups are uniformly successful or uniformly failed, outcome-based advantages degenerate, and the surviving gradient is noise that can only damage a competent policy - in particular by drifting behaviour on games outside the online training subset. This is akin to a forgetting effect that in theory a novelty bonus should curb by preserving informative gradient. R5 tests the prediction with a controlled pair: GRPO with and without a novelty bonus, all else identical.

Construction. For each sampled instance x we roll out K episodes $\{\tau_1, \ldots, \tau_K\}$ through the Game Master and compute group-relative advantages over episode returns (Shao et al., 2024):

$$A_i = \frac{r_i - \mathrm{mean}(r_{1:K})}{\mathrm{std}(r_{1:K}) + \varepsilon}.$$

The policy is updated on the agent's own tokens only (environment tokens masked), with importance ratios $\rho_{i,t} = \pi_\theta(a_{i,t} \mid s_{i,t}) / \pi_{\theta_{\text{old}}}(a_{i,t} \mid s_{i,t})$ against the pre-update policy, and a per-token KL penalty to the frozen R2 reference via the low-variance k_3 estimator (Schulman, 2020), with $\Delta_{i,t} = \log \pi_{\text{ref}} - \log \pi_\theta$:

$$\mathcal{L} = -\tfrac{1}{|T|} \sum_{i,t} \Big[\min\big(\rho_{i,t} A_i,\ \mathrm{clip}(\rho_{i,t}, 1 \pm \epsilon) A_i\big) - \beta_{\mathrm{KL}} \big(e^{\Delta_{i,t}} - \Delta_{i,t} - 1\big) \Big].$$

Intrinsic novelty (RND). In the intrinsic arm, we add a bonus that is large when the agent produces behaviour unlike what it has produced before, and decays as that behaviour becomes familiar. Following random network distillation (Burda et al., 2019), familiarity is measured by how well a small trained network can predict the output of a fixed, randomly initialised one: both see the same input, the target f is frozen, and the predictor $\hat{f}$ is trained to match it on states actually visited. On behaviour seen many times the predictor has converged and the error is small; on novel behaviour it has not, and the error is large. The input is $\phi(\tau)$, the mean of a frozen random embedding of the agent's own action tokens in episode τ; the embedding is frozen so that the novelty measure does not shift as the policy itself is updated. With running normalisation σ_b over recent errors,

$$b(\tau) = \mathrm{clip}\left(\tfrac{\|\hat{f}(\phi(\tau)) - f(\phi(\tau))\|_2^2}{\sigma_b},\ 0,\ 5\right),$$
$$r_i^{\text{tot}} = r_i + \lambda\, b(\tau_i), \qquad \lambda = 0.1,$$

so each episode's return is augmented by its novelty before advantages are computed. Because advantages are group-normalised, any bonus shared by all K rollouts of an instance cancels: only *within-group* novelty differences carry gradient, so the signal rewards the more novel of two attempts at the same game state rather than novelty in general. Crucially, this keeps gradient flowing when outcomes alone provide none—when all K rollouts succeed, or all fail, the extrinsic advantages are uniformly zero but the novelty differences are not. The bonus exists only during training; evaluation uses game outcomes exclusively.

4 Experimental Setup

Base model and adaptation. We adapt Qwen3.5-2B, more details are provided in Appendix B.

Data. All game data derives from the public playpen-data release (interactions and instances configurations); general data from SmolTalk. Sizes

Model	clem	Δ	stat
Qwen3.5-2B (base)	13.63	–	44.22
R1 imitation (SFT)	55.61	+41.98	43.87
R2 outcome contrast (DPO)†	67.39	+53.76	**44.72**
R3 self-imitation	61.06	+47.43	44.01
R4 corrective feedback	**67.64**	**+54.01**	44.31
R5 GRPO (no intrinsic)	62.43	+48.80	44.19
R5 GRPO + RND	67.44	+53.81	43.53

Table 1: Frozen-environment results (validation split). Δ = clemscore gain over the base model measured in the same environment.† = the model submitted to the challenge (submission predates R4/R5). For cross-environment context, the organisers' held-out evaluation scores the base model at 13.41 in-domain and the untuned Qwen3.5-27B at 64.34 (Section 5.1)

per regime as in Section 3. No validation-split or test data is used for training; self-play instances overlapping the validation split are excluded (21/46 of 490 episodes in R3/R4 self-play data collection).

Compute. R1 ≈3.3 h and R2 ≈0.6 h on one A100 40GB; R3–R5 (generation + training) and all evaluations on one 96GB workstation GPU; total budget ≈25 GPU-hours.

Evaluation. Official pipeline (`playpen eval`) on the validation split, both suites, greedy decoding (pipeline default). All reported numbers come from a single frozen environment: Python 3.11 virtual environment, clemcore at the version pinned by the playpen.

5 Results

Table 1 and Figure 1 summarise all regimes.

Imitation buys protocol; contrast buys policy. R1 lifts the model $+41.98$, almost entirely by eliminating aborted episodes (most games reach 100% played). R2 adds another $+11.78$ with %-played essentially flat and quality rising—the two factors of the clemscore product respond to two different signals, cleanly separated by the two stages. Statscore never moves more than ± 0.7 from base across all seven rows: the R1 mix-in suffices, and no later regime reintroduces forgetting on the static side.

Self-imitation regresses; correction does not. R3 costs 6.3 points relative to its R2 initialisation. Practising one's own successes, delivered as unanchored SFT, perturbs behaviour both on games absent from the self-play data and on games present in it; we attribute latter to using no general-data replay (unlike R1) and trained on positives the policy already produced (since regression isn't reversion toward the pre-DPO model: although R3's score falls between R1 and R2, only 21% of its per-game deviation from R2 lies along the $R1 \rightarrow R2$ direction, remainder being orthogonal). R4, which delivers the model's own play as contrastive pairs through the KL-anchored preference objective, does not regress and posts the best score (67.64)—though its margin over R2 (+0.25) is within the run-to-run variability, so we describe R2, R4 and R5+RND as statistically indistinguishable at the top.

Online play needs curiosity. The controlled R5 comparison is our central finding, and the per-game breakdown (Appendix: Figure 4) explains it. On the four games R5 trained on, *both* GRPO arms improved or held relative to R2 (e.g., `adventuregame`: $26.7 \rightarrow 33.3 \rightarrow 40.0$; `textmapworld`: $65.0 \rightarrow 67.0 \rightarrow 68.2$). Plain GRPO's overall regression of 4.96 points came almost entirely from *untrained* two-player games (`codenames`: $30.8 \rightarrow 15.4 \rightarrow 23.1$; `guesswhat`: $33.3 \rightarrow 22.2 \rightarrow 27.8$; `taboo`: $66.7 \rightarrow 58.3 \rightarrow 66.7$). That is, online updates on a narrow subset of games induced forgetting on the rest—the same failure mode we saw in R3. The RND bonus, identical in seed, steps, data, and hyperparameters, and differing only in λ, recovers this loss almost entirely overall (67.44, $+5.01$ over the control), and the recovery is concentrated in exactly those untrained games. The intrinsic signal thus acts less as an explorer than as a regulariser against distributional drift; by keeping within-group gradients informative even where extrinsic outcomes are uniform, it limits how far the policy wanders on games it is not currently practising.

5.1 Held-out evaluation

The organisers evaluated all submissions on a closed set of games not released during the challenge: 16 in-domain games (the clembench suite, with adventuregame and the static benchmarks on reduced instance sets) and 13 newly written out-of-domain games. We submitted R1 and R2; R3–R5 postdate the submission deadline and were not evaluated.

Two results stand out. First, both checkpoints improve out-of-domain, and by a wider margin than any other submission: the next-best entry gains $+6.53$, and *every* submission built on a larger base (4B, 9B, 27B) *declines* out-of-domain, by up to

	ID Δ	OOD Δ	stat Δ
R1 imitation	+33.16	**+11.90**	+0.11
R2 contrast	**+37.34**	+10.84	−0.71
best other entry	+32.85	+6.53	—

Table 2: Held-out evaluation (organisers' run; deltas over the Qwen3.5-2B base, which scores 13.41 in-domain, 3.72 out-of-domain and 44.24 on statscore). "Best other entry" is the strongest competing submission on each metric across all base-model scales.

−13.49. What the offline recipe teaches transfers to games it has never seen, which is not true of the field's other approaches.

Second, the two regimes trade off in the direction our validation analysis predicts. R2 is clearly stronger in-domain (+37.34 vs. +33.16) but slightly weaker out-of-domain (+10.84 vs. +11.90), with a wider generalisation gap (−36.19 vs. −30.95). Imitation of expert transcripts installs competence that transfers; contrastive refinement against outcomes buys additional in-domain quality that is more specific to the games it was derived from. In absolute terms our best submission reaches 50.75 in-domain, short of the untuned 27B baseline (64.34): the advantage over that baseline that we observed on the validation split does not survive the held-out evaluation.

6 Discussion

Two factors, two currencies. Imitation moved %-played and barely touched quality; contrastive feedback moved quality and barely touched %-played. Demonstrations specify the *support* of good behaviour (what a legal move looks like), while contrasts specify its *gradient* (which legal move is better)—and dialogue games, defined by conventions plus quality distinctions, reward exactly this two-course curriculum, in the order children meet it: immersion first, feedback on one's own attempts second.

Own behaviour is a treacherous teacher. Every self-generated-data regime underperformed the best offline one, but how they failed is the lesson. Unanchored self-imitation (R3) hurt; the same self-play episodes, used as contrastive evidence inside an anchored objective (R4) matched the best model; online updates from own play hurt without curiosity and were rescued by it (R5). Own-behaviour data is least informative exactly where the policy is already competent—successes teach little, outcome advantages vanish, so the residual gradient is dominated by noise, which unanchored objectives turn into drift. What worked either re-anchored the policy (KL to a trusted reference) or re-injected information where outcomes carried none (novelty at the competence boundary). Curiosity here acted as a stabiliser rather than the usual explorer-under-sparse-reward—consistent with learning-progress accounts (Oudeyer et al., 2007), and predicting that the RND advantage should shrink with larger groups and grow as the policy nears ceiling. Two-player games with a learning partner, longer horizons, and learning-progress-based instance selection are our natural next steps.

What transferred. The held-out evaluation supplies the test our validation split could not. Both offline regimes generalise to unseen games, and imitation generalises slightly better than contrast—consistent with the division of labour above, in which demonstrations specify what a legal, sensible move looks like while contrasts refine which of them is better in a particular game. That every larger-model submission lost ground out-of-domain suggests the effect turns on what the training signal encodes rather than on scale.

Limitations

All results are for one base model at one scale; our regime sweep is evaluated on a small validation split, and only R1 and R2 were run on the held-out set. R1, R2 and R4 depend on transcripts from stronger models, so the recipe does not demonstrate teacher-free learning; R5 covers only single-player games, as the training interface lacks a partner mechanism. The R5 comparison uses one seed per arm. The mechanism behind R3's regression remains unidentified; we rule out format mismatch and reversion toward the pre-DPO policy (Section 5). Belief-intensive games (privateshared, codenames clue-giving) remain the weakest and untargeted by our methods. Training curves (Appendix: Figure 3) are noisy over 60 steps and do not separate the arms; the RND effect appears in final evaluation.

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A The Playpen: Games and Benchmarks

Dialogue games (clemscore). The evaluation suite comprises fourteen games, each mediated by a programmatic Game Master that delivers instructions, validates every move against a strict format (violations abort the episode), and scores completed episodes for success and efficiency. The games cluster into four families that stress different competences. *Word games*: taboo (describe a word without using it or listed relatives, for a guessing partner), wordle and its variants wordle_withclue and wordle_withcritic (letter-feedback word guessing, optionally with a clue giver or a critic to negotiate with), and codenames (one-word clues that connect several target words while avoiding others). *Reference and drawing games*: referencegame (describe which of three grids is the target), imagegame (instruct a partner to reproduce an ASCII drawing), matchit_ascii (two players must decide whether they see the same image). *Information-tracking games*: guesswhat (twenty-questions-style search over a candidate list), privateshared (answer probes about which information has already been shared with the interlocutor—explicit bookkeeping of common ground). *Exploration games*: adventuregame (text-adventure task completion) and the textmapworld family (navigate a graph-structured world; variants add graph reasoning and goal rooms). Success requires simultaneously respecting the move grammar (*protocol*) and playing well (*policy*); clemscore is the product of macro-averaged %-played (non-aborted episodes) and macro-averaged quality on played episodes, so the two factors remain separately visible.

Static benchmarks (statscore). The second metric averages five standard single-turn benchmarks: BBH (Suzgun et al., 2023) (multi-step reasoning), MMLU-Pro (Wang et al., 2024) (broad knowledge, hard multiple choice), IFEval (Zhou et al., 2023) (verifiable instruction following), CLadder (Jin et al., 2023) (causal reasoning) and EQ-Bench (Paech, 2023) (emotional understanding). Its role is

	Objective	LoRA	Optimisation
R1	SFT	r=32, α=64	2 ep, lr 10^{-4}
R2	DPO	r=16, α=32	1 ep, lr 5×10^{-6}
R3	SFT	r=16, α=32	1 ep, lr 2×10^{-5}
R4	DPO	r=16, α=32	1 ep, lr 5×10^{-6}
R5	GRPO	r=16, α=32	60 steps, lr 5×10^{-6}

Table 3: Adaptation settings per regime. All regimes use LoRA on every attention and MLP projection with dropout 0.05, bf16, effective batch 16, and maximum length 4096; SFT computes loss on assistant tokens only. Cosine schedules throughout (warmup 0.03). DPO uses β=0.1; GRPO uses AdamW with gradient clipping 1.0, ϵ=0.2, β_{KL}=0.04, K=6 rollouts over 4 instances per step. Seeds: 42 (data, training), 0 (RND networks).

to detect the well-documented cost of narrow fine-tuning (McCloskey and Cohen, 1989; Kirkpatrick et al., 2017; Luo et al., 2023): a submission that turns the model into a pure game-player should pay here.

B Adaptation Settings

We adapt Qwen3.5-2B with LoRA; Table 3 lists the settings per regime. Three details are regime-specific. R2's preference training converged to 0.88 accuracy with a reward margin of 1.90, the movement lying almost entirely on the rejected side (chosen -0.18, rejected -2.07). R4 uses 201 preference pairs (107 on-policy, 94 hybrid) built from a greedy and a sampled (t=0.7) rollout pass. R5 samples at temperature 0.8 with top-p 0.95, 128 new tokens per turn and a 4096-token trajectory cap enforced during rollout, on the four single-player games (wordle, textmapworld, textmapworld_specificroom, adventuregame); the training interface provides no partner for two-player games. The RND networks are MLPs $256\to512\to128$ with predictor lr 10^{-4} and λ=0.1.

C Qualitative Examples

Qualitative example is shown in figures below, comparing the Base model (Figure 5) with the R2 model (Figure 6), on episode 2 of the game `wordle`.

D Reproducibility

1. Released checkpoints. All checkpoints are merged (no adapters) and public on the Hugging Face Hub under `varadsrivastava/`; each carries a model card describing its regime and results. Repository names retain the internal identifiers used during development.

Regime	Repository suffix
R1 imitation	`-sft`
R2 outcome contrast†	`-sft-dpo`
R3 self-imitation	`-iter3`
R4 corrective feedback	`-iter4`
R5 GRPO (control)	`-grpo-base-s42`
R5 GRPO + RND	`-grpo-rnd-s42`

Table 4: Checkpoint mapping. All repositories share the prefix `varadsrivastava/lm-playschool-qwen3.5-2b`. † = the model submitted to the challenge; the `s42` suffix records the training seed. A further repository, `-sft-dpo-vllm`, republishes the R2 weights with a composite (vision–language) `config.json` for vLLM compatibility and is not a separate model.

2. Evaluation environment: Python 3.11 venv; clemcore pinned via the playpen package; clembench pinned requirements.

3. Registry entry: huggingface_local backend, premade chat template, end-of-sequence culling on the `im_end` token, left padding.

4. Data: `colab-potsdam/playpen-data`, `HuggingFaceTB/smoltalk`.

5. Frameworks: Transformers, PEFT, TRL (R1–R4); R5 uses our from-scratch GRPO implementation.

6. Official results: our submissions appear as `DAIR__qwen3.5-2b__sft-v1` and `DAIR__qwen3.5-2b__sft-dpo-v2` in the challenge results release, together with full transcripts of the held-out runs: https://github.com/lm-playpen/lm-playschool-2026-final-results

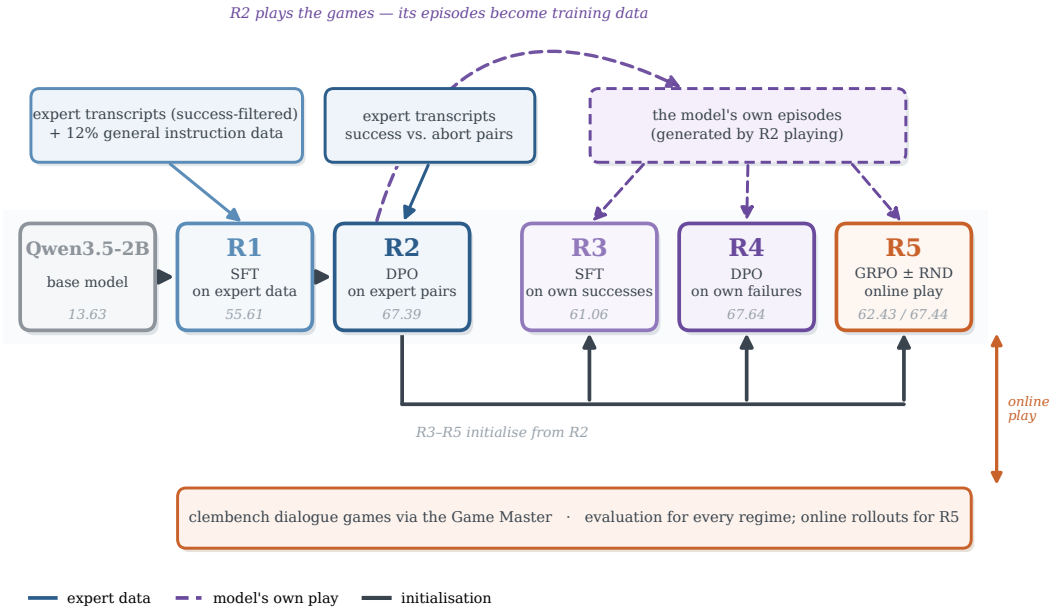

Figure 2: Data and initialisation flow across the five regimes. R1 and R2 learn from expert transcripts (solid blue); R3–R5 each initialise from R2 and learn from R2's own gameplay (dashed purple), generated by the loop at the top. R5 additionally interacts with the environment online. Validation clemscore is shown inside each box.

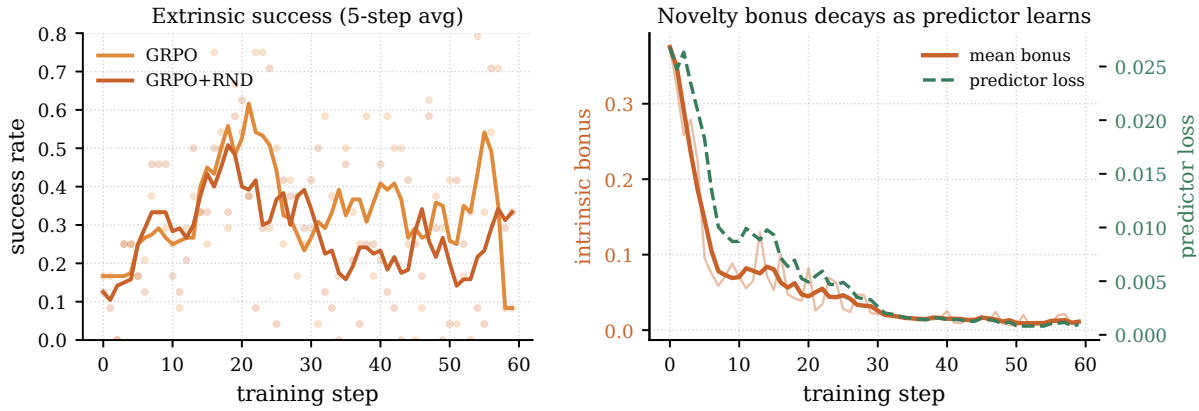

Figure 3: R5 training dynamics: extrinsic success rate and intrinsic bonus over training steps, GRPO vs GRPO+RND.

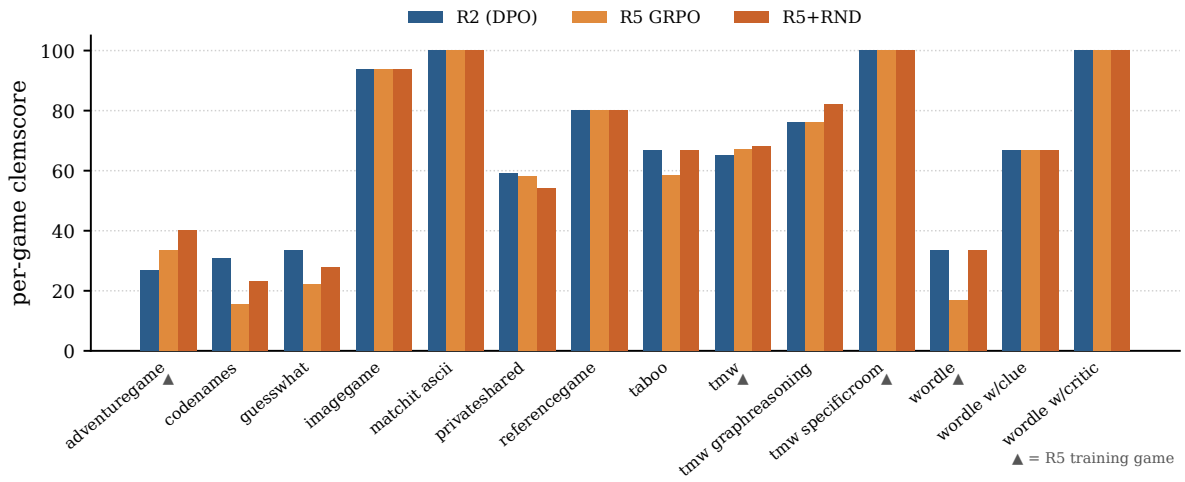

Figure 4: Per-game performance of R2, R5, and R5 + RND. ▲ are training-game markers referring to games R5 was trained on.

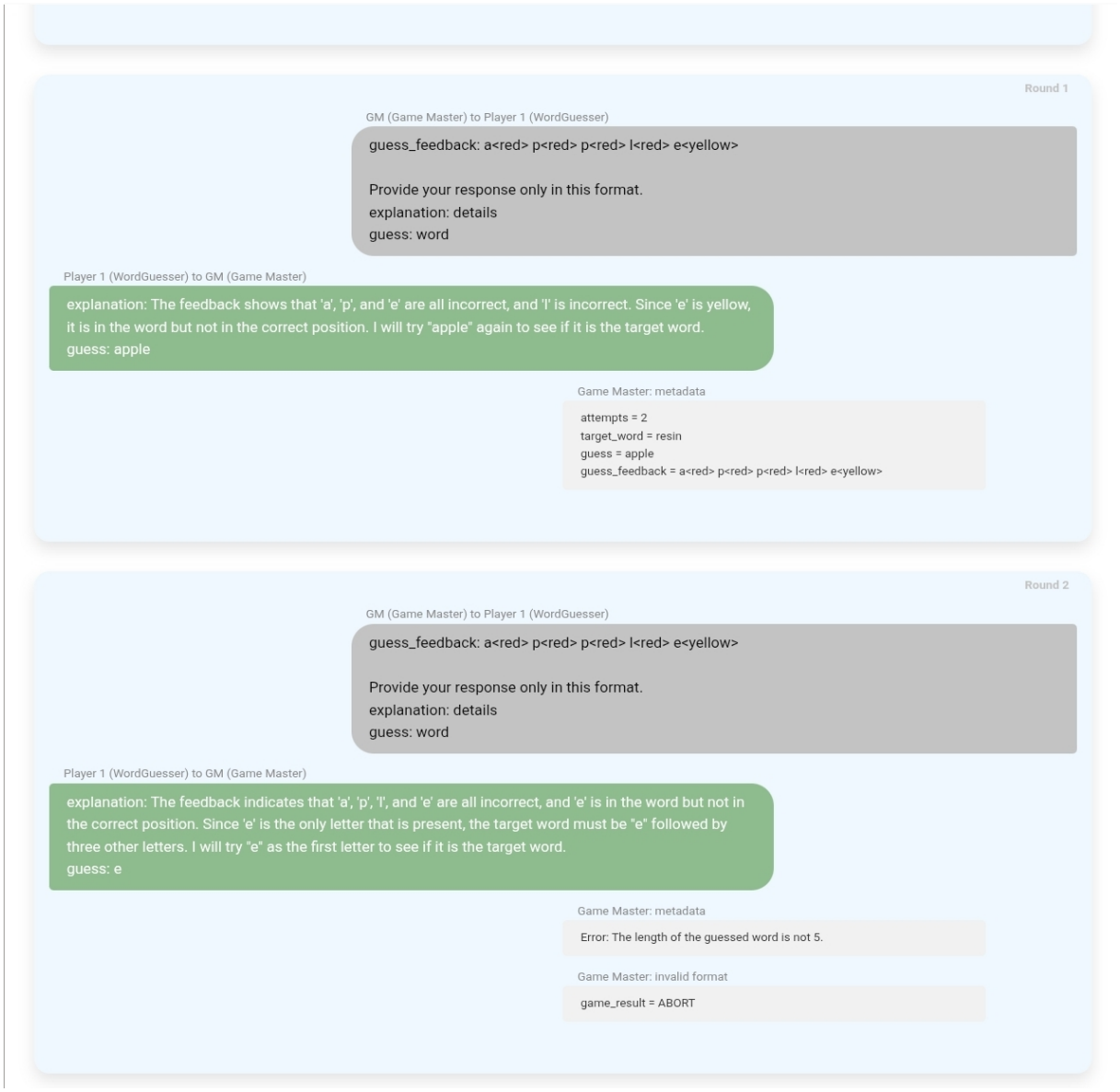

Figure 5: Example episode transcript before training.

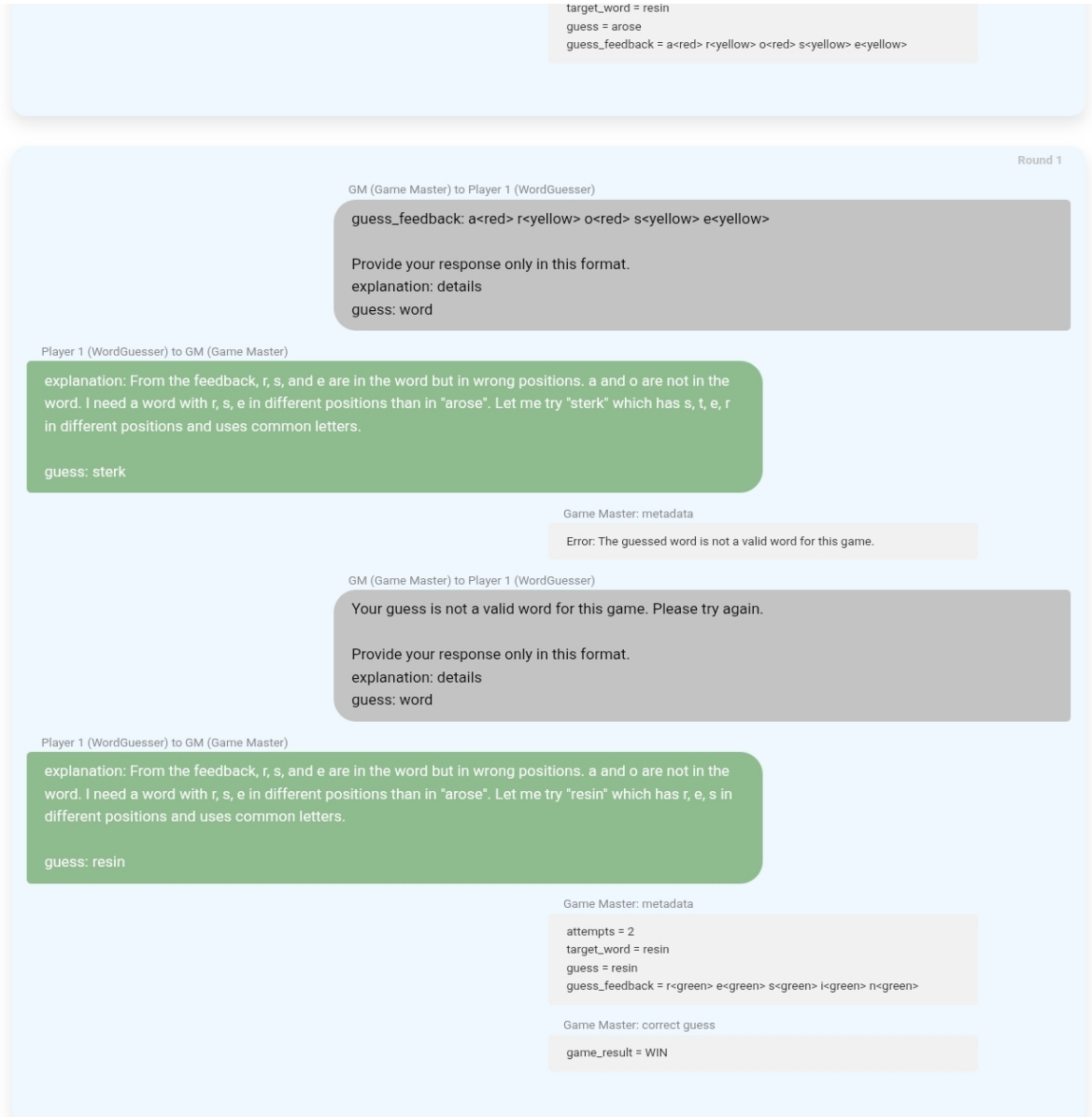

Figure 6: Example episode transcript after training (R2).