GUI-PRIMITIVES: Diagnosing Spatial Reasoning Failures in Vision-Language GUI Grounding

Anonymous ACL submission

Abstract

Computer-use agents click pixels chosen by a vision-language model, so the agent's success is upper-bounded by how well the model grounds short language instructions in a screen. We ask whether today's VLMs have this grounding skill on their elementary spatial substrate. We introduce GUI-PRIMITIVES, a 994-item minimal-pair benchmark over seven elementary spatial relations in graphical user interfaces (left/right, above/below, containment, alignment, proximity, list ordinal, occlusion). Each item has a twin that uses the same screenshot and flips only the relation word, so a model that guesses cannot pass. Five annotators verify a 196-item core (Fleiss $\kappa = 0.94$ for well-formedness, $\kappa = 0.79$ for the correct target; human accuracy is 96.9%). We evaluate 19 models, including Claude Opus 4.7, GPT-5, Claude Sonnet 4.6, Gemini 3.1, Qwen2.5-VL, and OS-Atlas. The strongest model reaches only 31% overall, a 65-point gap from humans. On four of seven primitives, the upper 95% confidence bound for every model sits below chance, including the four frontier closed ones. Diagnostic accuracy correlates strongly with downstream grounding on ScreenSpot-Pro (Spearman $\rho = +0.74$, $p = 0.015$ across ten models). Of three training-free interventions, Set-of-Mark prompting recovers 35–57 points and lifts GPT-5 from 30% to 87%; chain-of-thought and activation steering yield no significant gain. GUI-PRIMITIVES, code, and predictions are released for community use.

1 Introduction

A new class of *computer-use agents* now reads a screenshot, decides what to do, and emits a click coordinate or a keystroke (Xie et al., 2024; Cheng et al., 2024; Qin et al., 2025; Gou et al., 2025; Wu et al., 2025b; Wang et al., 2025a). These agents are beginning to be deployed; their first job, before any agent loop, is the same job a person does when they open a window: read the screen, find the right control, and put the pointer in the right place. We call this *GUI grounding*, and a growing literature shows that frontier vision-language models still trail humans on it (Li et al., 2025; Yang et al., 2025a).

A natural question follows. If we strip the agent away and ask a present-day VLM only to handle the elementary spatial reasoning that GUI grounding requires: to the right of `Save`, inside the `Layers` panel, the third item in the menu, does it succeed? Existing benchmarks do not isolate this skill. End-to-end benchmarks like OSWorld (Xie et al., 2024) mix grounding with planning and action; coarse grounding benchmarks like ScreenSpot (Cheng et al., 2024) and ScreenSpot-Pro (Li et al., 2025) measure whether the click lands inside the target box, but do not separate failures by spatial primitive. As a result, when an agent clicks the wrong button, we do not yet know *which* elementary skill broke.

We close this gap with a controlled diagnostic. GUI-PRIMITIVES is a benchmark of 994 minimal-pair questions over seven elementary spatial primitives in GUI screenshots. Each question is paired with a twin that keeps the screenshot fixed and flips a single relation word (`left` ↔ `right`, `inside` ↔ `outside`, etc.). This construction, adapted from the minimal-pair tradition in linguistic evaluation (Linzen et al., 2016; Marvin and Linzen, 2018; Kamath et al., 2023), removes the easy shortcuts: a model that always clicks the most salient or central element will score correctly on one twin and incorrectly on the other. We evaluate 19 vision-language models across five families, including the frontier closed models Claude Opus 4.7, GPT-5, Claude Sonnet 4.6, and Gemini 3.1, alongside Qwen2.5-VL, OS-Atlas, InternVL3, Llama-3.2-Vision, PaliGemma 2, Gemma 3, MiniCPM-V, and

Source Code anonymous.4open.science/r/gui-primitives-32E2/

Qwen2-VL. Five human annotators verify a 196-item subset with Fleiss $\kappa = 0.94$, and reach 96.9% accuracy.

The result is uneven and informative. Claude Opus 4.7, the strongest model, reaches only 31% overall on the human-clean split, a 65-point gap from humans. On four of seven primitives (containment, occlusion, alignment, proximity), the upper 95% confidence bound on every model we test sits below chance, including all four frontier closed ones. On real desktop screenshots from UI-Vision (Nayak et al., 2025), accuracy collapses further: most models score near zero on every primitive. Pair-consistency analysis shows failures correlate with the relation word rather than with the picture, the WHAT'S-UP signature (Kamath et al., 2023) of relational failure.

Yet the diagnosis is not only a problem statement. Three results follow. First, the benchmark tracks downstream performance. Across ten models for which we also run ScreenSpot-Pro, GUI-PRIMITIVES accuracy correlates with grounding accuracy at Spearman $\rho = +0.74$ ($p = 0.015$). The correlation is stable across the real-screenshot and synthetic slices of GUI-PRIMITIVES. Item-level logistic regression on $n = 15{,}810$ model–item pairs corroborates this association (pseudo-$R^2 = 0.40$); we report it conservatively. Second, we test three training-free interventions: Set-of-Mark (SoM) prompting (Yang et al., 2023), primitive-aware chain-of-thought (CoT), and activation steering (Kang et al., 2025; Sivakumar et al., 2025). SoM recovers 35–57 points across open and closed models, lifting GPT-5 from 30% to 87% and OS-Atlas from 10% to 52%. The other two yield no significant gain. The improvement is real, substantial, and transfers cleanly across open and closed APIs, but it is a workaround, not a cure. Third, we ship a benchmark designed for honesty. Three shortcut controls (blank canvas, shuffled instructions, heavy blur) cause the expected accuracy to collapse. The blur result is sub-threshold, and we report it as a substantive finding rather than hiding it. Negative results for CoT and steering are reported with the same rigor as the positive results for SoM.

We make four contributions. First, we introduce GUI-PRIMITIVES, a 994-item minimal-pair diagnostic benchmark covering seven elementary spatial primitives in GUI screenshots, together with a five-annotator verified 196-item core ($\kappa = 0.94$). Second, we evaluate 19 models across five families and reveal systematic below-chance behavior on most primitives, alongside a 65-point gap from human performance. Third, we show that primitive competence predicts downstream GUI grounding on ScreenSpot-Pro (Spearman $\rho = +0.74$, $p = 0.015$). Fourth, we find that SoM transfers across both open and closed models, improving GPT-5 by 57 points, whereas CoT prompting and activation steering do not; we release GUI-PRIMITIVES, all model predictions, and analysis code.

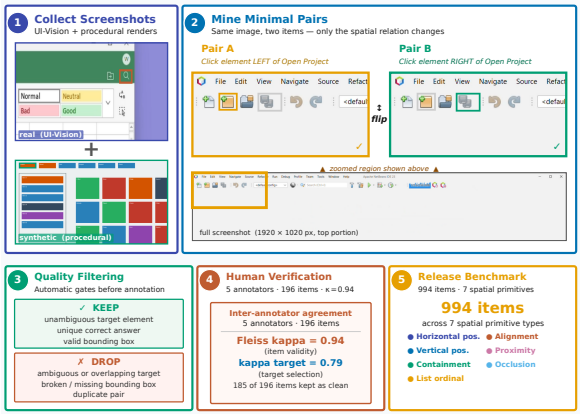

Figure 1: **GUI-PRIMITIVES dataset construction pipeline.** Two source corpora: real desktop screenshots from UI-Vision and controlled synthetic stimuli from our procedural renderer, feed into per-primitive minimal-pair miners, pass through automatic quality gates, are cross-checked by a five-annotator human verification pass (Fleiss κ=0.94), and released as the 994-item benchmark. Stage colors follow the primitive palette used throughout the paper.

2 Related Work

GUI grounding and computer-use agents. A line of recent work targets agents that operate real software. OSWorld (Xie et al., 2024) provides a real Linux and Windows VM environment with execution-based scoring on 369 tasks; a subsequent human-verified release closes ambiguous tasks (XLANG Lab, 2025). ScreenSpot (Cheng et al., 2024) and ScreenSpot-Pro (Li et al., 2025) measure pixel-grounding in isolation, the latter targeting professional software at high resolution; MMBench-GUI (Wang et al., 2025b) adds a hierarchical multi-platform evaluation. Grounding models trained on screenshots include SeeClick, OS-Atlas (Wu et al., 2024), UI-TARS (Qin et al., 2025), UGround (Gou et al., 2025), Aria-UI (Yang et al., 2025b), GUI-Actor (Wu et al., 2025b), Jedi (Xie et al., 2025), OpenCUA (Wang et al., 2025a),

and the test-time scaling agent GTA1 (Yang et al., 2025a). Closest in spirit, GUI-Cursor (Zhao et al., 2025) probes a cursor-based agent's spatial reasoning under reinforcement learning, and Beyond Semantics (Qi et al., 2025) argues that large vision-token norms suppress positional encoding, a mechanistic candidate for our failures. These works ask whether models land the click or how to train them; we ask which elementary spatial skill they lack when they miss.

Spatial reasoning in vision-language models. Several benchmarks reveal that VLMs struggle with spatial relations even on natural images. WHAT'S-UP (Kamath et al., 2023) uses controlled minimal pairs to show that contrastive VLMs handle left/right and on/under poorly despite high captioning accuracy. VSR (Liu et al., 2023) catalogs 66 spatial relations in natural images; BLINK (Fu et al., 2024) bundles 14 perceptual primitives and finds even GPT-4V well below humans; CV-Bench (Tong et al., 2024) shows that 2D/3D spatial primitives are bottlenecked by the visual encoder. SpatialVLM (Chen et al., 2024) addresses these gaps by co-training on synthetic 3D-grounded data. RocketScience (Hoehing et al., 2025) uses contrastive pairs to disentangle object localization from spatial reasoning, and finds that only top reasoning models succeed. GUI-PRIMITIVES extends this controlled methodology to GUI screenshots, where the visual prior is starkly different: text labels everywhere, near-identical icons, dense layout.

Minimal-pair evaluation. Minimal pairs originate in psycholinguistic evaluation (Linzen et al., 2016; Marvin and Linzen, 2018), where a single lexical or syntactic edit creates a contrast whose disambiguation requires understanding rather than surface statistics. WHAT'S-UP brought the design to vision-language. We adopt the same logic for GUI screenshots and add pair-consistency (Kamath et al., 2023) as an internal diagnostic of relational failure (§5.5).

Inference-time interventions. SoM prompting (Yang et al., 2023) overlays numbered marks on visual regions so the model names the mark rather than regressing a coordinate. SoM-LLaVA (Yan et al., 2024) shows that open-source MLLMs need explicit SoM training data to match closed models. Visual prompting is now a standard tool in GUI-agent stacks, with recent training-free zoom and modality-aware variants: DiMo-GUI (Wu et al., 2025a) decouples text and icon modalities and applies test-time scaling; RegionFocus (Luo et al., 2025) reaches 61.6% on ScreenSpot-Pro with a Qwen2.5-VL-72B backbone by iteratively zooming into a focal region. Activation and attention steering, including the localization-head method of Kang et al. (2025) and the SteerVLM framework (Sivakumar et al., 2025), modify model internals at inference time without weight updates. We test one representative from each family.

3 The GUI-PRIMITIVES Benchmark

GUI-PRIMITIVES is designed for one purpose: to ask whether a vision-language model has each of the seven elementary spatial skills that GUI grounding requires. Three design choices follow from this purpose: the seven primitives (§3.1), the minimal-pair construction (§3.2), and the human verification protocol (§3.4). The full construction pipeline is summarised in Figure 1.

3.1 Seven elementary primitives

We chose primitives that are unambiguous, common in everyday GUI language, and decomposable. Table 1 lists all seven together with their relation words and chance level. The primitives split into three groups. *Cardinal direction* covers horizontal and vertical relative position. *Topology* covers containment (inside or outside a panel). *Layout* covers alignment (sharing a row or a column), proximity (which of several elements is nearest), list ordinal (the third item of a menu), and occlusion (visible or partly covered by a pop-up).

Why these seven? We require each primitive to be lexically realizable in a short user instruction, separable by a single relation-word flip into a minimal pair, and a basic building block of compositional GUI relations. The seven jointly cover the four broad classes cataloged by prior natural-image work: cardinal direction, topology, ordinality, and layout-distance (Liu et al., 2023; Kamath et al., 2023; Hoehing et al., 2025). We do not claim coverage of every spatial relation, only of the elementary ones from which the rest compose.

3.2 Minimal-pair construction

Every item in GUI-PRIMITIVES appears as one half of a minimal pair: the two members share one screenshot and one anchor element, differing only in the relation word, with the correct target

Table 1: The seven elementary spatial primitives in GUI-PRIMITIVES, with their canonical relation words and chance baselines. Six are binary; *list-ordinal* uses five-way classification.

Primitive	Relation words	Group	Chance
rel-pos-horizontal	left / right	cardinal	0.50
rel-pos-vertical	above / below	cardinal	0.50
containment	inside / outside	topology	0.50
alignment	same row / col.	layout	0.50
proximity	nearest / farthest	layout	0.50
list-ordinal	first . . . last	layout	0.20
occlusion	visible / hidden	layout	0.50

as the *contrastive* element (Figure 2, details in Appendix J.1). This construction removes the shortcuts that inflate scores on coarser benchmarks: a model that always selects the most central, salient, or frequently labeled element earns one point and loses one on a pair, so the pair-level score is zero. We measure this directly as *pair-consistency* (§5.5). Three representative pairs for each of the remaining six primitives are shown in Appendix P.

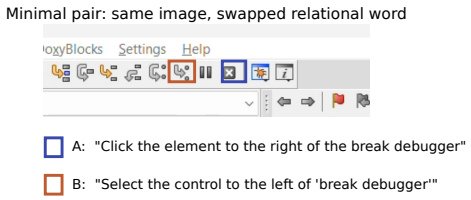

Figure 2: A minimal pair from GUI-PRIMITIVES (details in Appendix P). The screenshot is fixed; one word flips, and the correct target shifts to the other candidate. A model exploiting an answer prior passes one twin and fails the other.

3.3 Sources: real and synthetic screenshots

GUI-PRIMITIVES draws from two sources (Appendix J.2 and K), both reported separately throughout. The *real* arm ($n = 290$) uses element-grounding annotations from UI-Vision (Nayak et al., 2025) on Web, MacOS, VS Code, Office, and professional applications, covering the four primitives whose annotations are unambiguously recoverable from element labels (horizontal and vertical relative position, alignment, proximity). The *synthetic* arm ($n = 704$) is procedurally rendered with known positions, controlled distractors, and a fixed layout language; it is the only way to obtain *containment*, *list ordinal*, and *occlusion* cleanly, since real screenshot annotations do not ship the parent–child or overlay–target metadata. We treat the synthetic arm as a controlled-stimulus arm in the WHAT'S-UP tradition and disclose every result split by source.

3.4 Human verification

Five annotators independently judged a 196-item stratified subset (*human-verified core*), answering two questions per item: well-formedness and correct target (Appendix J.3). Agreement is high: Fleiss $\kappa_{Q1} = 0.942$ ("almost perfect" on the Landis–Koch scale, Landis and Koch, 1977) and $\kappa_{Q2} = 0.787$ ("substantial"). Eleven items judged invalid by the majority were dropped, leaving the 185-item *human-clean core*; human accuracy on it is 96.9%. Annotator instructions are in Appendix F.

4 Models and Protocol

Models. We evaluate 19 VLMs across five families: four frontier closed (Claude Opus 4.7, Sonnet 4.6, Haiku 4.5, GPT-5, GPT-4.1, GPT-4o-mini, Gemini 3.1 Flash Lite); three Qwen-family open (Qwen2.5-VL-7B, Qwen2-VL-7B, OS-Atlas-Base-7B); InternVL3-8B; Meta Llama-3.2-11B-Vision; three sizes of Gemma 3; two PaliGemma 2 variants; and MiniCPM-V. Identifiers and endpoints are in Appendix B. Pixtral-12B and Idefics3 8B are excluded because their chat template formats are incompatible with our generic wrapper.

Prompting and decoding. All models use greedy decoding ($T = 0$) and a fixed seed. Per-family coordinate conventions (pixel, normalized $[0, 1]$, normalized $[0, 1000]$) are handled by a robust parser whose unit tests are bundled with the artifact. The three closed providers silently downsample large screenshots server-side (Anthropic $\leq$ 1568 px, OpenAI $\approx$ 1024 on the long side, Gemini variable); we pre-resize to a known long side and rescale predicted coordinates back to the original pixel space. The rescale was needed on a large fraction of items (e.g., 1,297 of 1,581 Sonnet ScreenSpot-Pro records); Appendix E documents the procedure and the per-model record counts.

Metrics. The primary metric, following ScreenSpot-Pro, is *point-in-box* accuracy: a prediction $\hat{p} = (\hat{x}, \hat{y})$ is correct iff $\hat{x} \in [x_1, x_2]$ and $\hat{y} \in [y_1, y_2]$ in the original pixel frame. *Loose* accuracy (prediction within $2\times$ target diagonal of the box center) separates sub-pixel imprecision from element misidentification on small icons.

All accuracies carry bootstrap 95% CIs (2,000 resamples); paired comparisons use McNemar (McNemar, 1947) with Holm–Bonferroni (Holm, 1979) correction across the seven primitives. We declare a cell *below chance* only when the upper bootstrap bound falls below the per-primitive chance level (0.50 for binary, 0.20 for list-ordinal); equivalently, a one-sided binomial test against chance is significant at $p < 0.001$ per primitive after the same Holm correction (Details in Appendix L).

5 Diagnostic Results

We report results in five parts, in this order: (i) controls that validate the benchmark, (ii) overall accuracy and the human gap, (iii) per-primitive structure of failure, (iv) the real-vs-synthetic decomposition, and (v) pair-consistency as direct evidence of relational failure.

5.1 Shortcut controls validate the benchmark

A diagnostic is only as good as the shortcuts it forbids (Appendix N.1). Figure 3 reports three controls on Qwen2.5-VL-7B over the full 994-item benchmark.

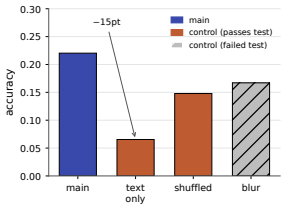

Figure 3: Shortcut controls (Qwen2.5-VL-7B, $n = 994$). Text-only and shuffled drops are both highly significant; blur produces a smaller but significant drop, reported as a substantive finding rather than hidden.

Pairing the instruction with a *blank canvas* drops accuracy from 22.0% to 6.5% ($\Delta = -15.5$ pt, $p = 2.8 \times 10^{-27}$); *shuffling* the screenshot to one from a different item of the same primitive drops it to 14.8% ($\Delta = -7.2$ pt, $p = 1.3 \times 10^{-11}$). Both behave as a grounded model should: when the visual or the matching instruction is removed, the task collapses. *Heavy Gaussian blur* ($\sigma = 12$ px) drops accuracy only to 16.7% ($\Delta = -5.3$ pt, $p = 8.5 \times 10^{-5}$) significant but below our pre-registered ten-point threshold. VLM grounding appears to rely more on global layout than on fine-grained OCR of text labels, since removing high-frequency detail removes only a modest fraction of the signal. However, blur alone cannot disentangle failures in OCR, spatial encoding, and cross-modal binding; targeted ablations are discussed in Appendix R.

5.2 Overall accuracy and the human gap

Table 2 reports the headline numbers, and Figure 4 visualizes the gap from humans.

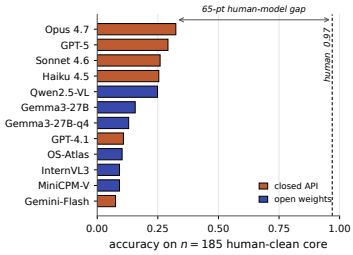

Figure 4: Human-clean core (185 items, 5 annotators, $\kappa = 0.94$). Human accuracy is 96.9%. The strongest model is Claude Opus 4.7 at 32.4%.

No model is close to human performance: the best system (Claude Opus 4.7) reaches 31–32% against 96.9%, and the 65-point gap holds on both the full benchmark and the cleaned 185-item core. Scale alone does not close it; the four frontier closed models cluster in a narrow 24–31% range, and the 7B open Qwen2.5-VL-Instruct is statistically tied with closed Claude Haiku 4.5 on the human-verified core (24.5% vs. 25.0%) despite being roughly two orders of magnitude smaller and runnable on a single GPU. The rank order is stable across the full 994-item benchmark, the 196-item human-verified core, and an LLM-judge-verified 100-item subset (Appendix A), so the gap is not a noisy-item artifact.

5.3 Per-primitive structure of failure

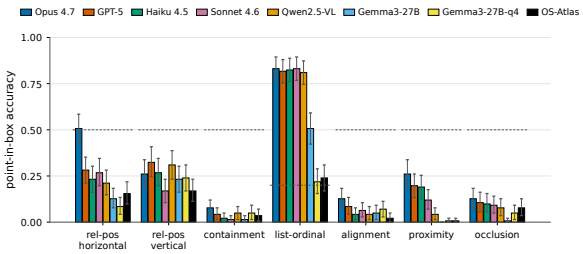

Figure 5: Per-primitive accuracy across eight representative models, with bootstrap 95% confidence intervals. Dashed lines mark chance (0.5 for binary primitives, 0.2 for list-ordinal). On `containment`, `occlusion`, `alignment`, and `proximity`, every model scores below chance, including the four frontier closed ones.

Figure 5 unpacks the overall accuracy by primitive (Appendix N.2). The picture is uneven and

Table 2: Overall accuracy on GUI-PRIMITIVES for 12 representative models, alongside accuracy on the human-clean 185-item subset. The strongest model (Claude Opus 4.7) reaches 32% on the clean core, a 65-point gap below human accuracy (96.9%). All 19 models are in Appendix A.

Model	**Family**	**Full** (n=994)	**Core** (n=196)	**Clean** (n=185)
Claude Opus 4.7	Anthropic	0.313	0.311	0.324
GPT-5 (reasoning)	OpenAI	0.265	0.291	0.297
Claude Sonnet 4.6	Anthropic	0.222	0.260	0.270
Claude Haiku 4.5	Anthropic	0.239	0.250	0.249
Qwen2.5-VL-7B	Qwen2.5-VL	0.220	0.245	0.249
Gemma 3-27B	Google open	0.134	—	0.157
OS-Atlas-Base-7B	Qwen2-VL/GUI	0.101	0.107	0.108
GPT-4.1	OpenAI	0.096	—	0.108
InternVL3-8B	OpenGVLab	0.095	—	0.097
Gemini 3.1 Flash Lite	Google closed	0.073	0.077	0.076
GPT-4o-mini	OpenAI	0.068	0.077	0.076
Qwen2-VL-7B	Qwen2-VL	0.038	0.046	0.049
Llama-3.2-11B-Vision	Meta mllama	0.012	0.005	0.005
Human	—	—	—	**0.969**

informative. One primitive, `list-ordinal`, is well solved by the top five models (Claude family, GPT-5, and Qwen2.5-VL), all achieving 0.80–0.83 against chance 0.20. The shared task here is counting list positions, which the language model can do once it identifies the list. The other six primitives are uniformly hard. On `rel-pos-horizontal`, Claude Opus 4.7 reaches 0.51, exactly chance; every other model is below. On `rel-pos-vertical`, the best model is GPT-5 at 0.32. On `containment`, `occlusion`, `alignment`, and `proximity`, *the upper 95% bootstrap bound for every model we test sits below the chance level* of 0.50, including all four frontier closed models. This is not statistical noise. With minimal pairs, below-chance behavior implies that models do not simply guess; they systematically choose the contrastive option more often than the correct one. We return to the most likely explanation, a layout-frequency prior, in §8. Model-specific directional biases and coordinate-frame anomalies are quantified in Appendix Q.

5.4 Real versus synthetic

Two concerns might arise from §5.3: that the benchmark is too hard because real GUI targets are tiny, or that synthetic items dominate the result. The full strict/loose split by source is in Appendix C and N.4. Two facts settle both concerns. On synthetic screenshots, *loose* accuracy is 0.60–0.76 even when strict accuracy is much lower; synthetic errors are largely sub-pixel imprecision, not mis grounding. On UI-Vision real screenshots, strict accuracy is essentially zero for every model on every primitive, and *loose* accuracy also stays low (6–23%): the model is not just off by a few pixels; it has chosen the wrong element. The zero on real GUIs is a relational failure, not a target-size artifact.

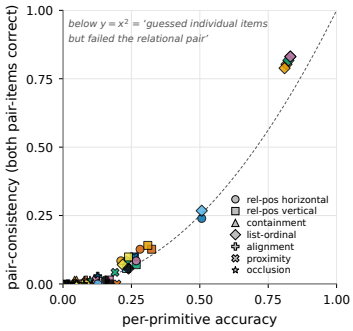

Figure 6: Pair-consistency vs. raw accuracy, per model per primitive. Independent guessing would place each point near $y = x^2$ (dashed). Almost all points lie *below* this curve, the WHAT'S-UP signature of relational failure.

5.5 Pair-consistency: a relational failure signature

Independent draws would put pair-consistency at $y = x^2$ (accuracy squared). Figure 6 shows it consistently *below* that curve, the relational-failure signature of Kamath et al. (2023). Within a model, the two pair members are negatively correlated: being right on the "left of" twin makes the model more likely than chance to be wrong on the "right of" twin. The relation word, not the picture, is what confuses the model (Appendix N.5). Eight representative minimal-pair failures are analyzed in Appendix S.

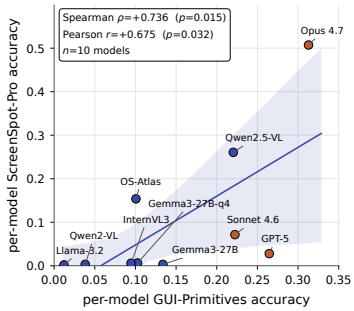

Figure 7: Per-model GUI-PRIMITIVES accuracy ($n_{\text{models}} = 10$) against ScreenSpot-Pro grounding accuracy. Spearman $\rho = +0.736$ ($p = 0.015$).

6 Primitive Competence Predicts Grounding

A diagnostic is only useful if it predicts a downstream property of interest. We test this on ScreenSpot-Pro (Li et al., 2025), the standard high-resolution GUI-grounding benchmark, which we ran on the ten models for which we have both GUI-PRIMITIVES and SS-Pro predictions. Figure 7 shows the per-model relationship. Spearman $\rho = +0.736$ ($p = 0.015$, $n = 10$); the correlation holds inside the real-screenshot subset ($\rho = +0.705$, $p = 0.023$) and inside the synthetic subset ($\rho = +0.760$, $p = 0.011$). A leave-one-model-out sensitivity analysis gives a Spearman in $[+0.62, +0.86]$; no single model drives the result, though dropping the strongest or weakest raises p to ≈ 0.07. An item-level logistic regression with model fixed effects (details in Appendix O) and a log target-area control reaches pseudo-$R^2 = 0.404$ on $n = 15{,}810$ pairs and corroborates the joint association; per-primitive coefficients are small ($|\text{coef}| \leq 0.07$) and fragile, so we claim only the model-level effect (full forest plot in Appendix D).

7 Training-Free Interventions

If models lack the elementary spatial skills GUI grounding needs, can inference-time scaffolding put them back? We test three interventions. **SoM** (Yang et al., 2023) overlays numbered marks on candidate elements; the model names the mark, trading pixel regression for symbolic selection. **Primitive-aware CoT** prepends a short decomposition prompt that names the relation explicitly, probing whether the failure is a reasoning sequence rather than perception. **Activation steering** follows SteerVLM (Sivakumar et al., 2025): we compute mean-difference vectors between correctly- and incorrectly-grounded items on a 200-item held-out contrast set, then add $\alpha \mathbf{v}_\ell$ to the residual stream at decoder layers $\ell \in \{12, \ldots, 19\}$ of Qwen2.5-VL-7B at inference, with $\alpha = 4.0$ chosen as the largest value from $\{1, 2, 4, 6\}$ preserving fluency. As a positive control, the same pipeline with a verbosity-contrast vector shifts output length as expected, so the machinery is functional.

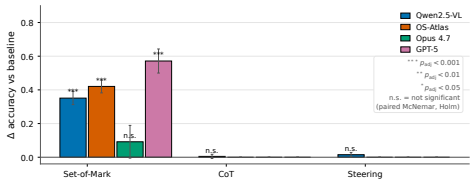

Figure 8: Intervention deltas vs. each model's baseline, with Holm-adjusted significance (paired McNemar across primitives). SoM recovers 35–57 points on three of four tested models; the only marginal case is Claude Opus 4.7, which already has the highest baseline. CoT and activation steering yield no significant change.

7.1 SoM recovers grounding

Figure 8 reports intervention deltas with significance. SoM is the only intervention producing large, statistically robust gains: +57.1 pt on **GPT-5** ($30 \rightarrow 87\%$, $p < 10^{-50}$ the highest absolute accuracy any model reaches under any condition), +42.1 pt on **OS-Atlas-Base-7B** ($10 \rightarrow 52\%$, $p < 10^{-50}$), +35.1 pt on **Qwen2.5-VL-7B** ($22 \rightarrow 57\%$, $p < 10^{-50}$), and a marginal +9.2 pt on **Claude Opus 4.7** ($31 \rightarrow 40\%$, Holm-adjusted $p = 0.08$, not significant). The pattern is clear: *the weaker the baseline, the larger the lift*. SoM helps where help is needed.

Per-primitive pattern. Figure 9 breaks the gain down by primitive on the two closed APIs we ran with SoM. Six of seven primitives gain at least 40 points on GPT-5; the only primitive that *regresses* (by −32.4 points) is `list-ordinal` on Qwen2.5 VL, which the model had already saturated at 81% baseline. SoM is not a free lunch: converting an already-solved task into mark selection adds friction. We report the regression openly.

7.2 CoT and activation steering: negative results

We report two negative results with the same effort as the positive one (Appendix M). Primitive-aware CoT on Qwen2.5-VL-7B produced $\Delta = +0.5$ pt ($p = 0.51$, n.s.); the model already names the relation correctly in its intermediate reasoning, then

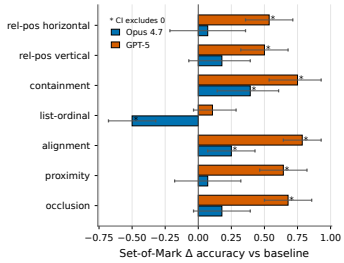

Figure 9: Per-primitive SoM delta on Claude Opus 4.7 and GPT-5. Stars mark intervals excluding zero. On GPT-5, six of seven primitives gain at least 40 points.

still emits the wrong coordinate. Activation steering produced $\Delta = +1.5$ pt ($p = 0.066$, n.s.), with no configuration in the grid crossing the Holm threshold. The one intervention that works bypasses the spatial skill it should test; the two that probe internal representation directly do not move the metric. We read this as evidence that the failure lies closer to perception (visual encoder, cross-modal binding) than to language-side reasoning or residual-stream representation.

8 Discussion

Our results suggest that scale alone has not solved GUI grounding. Even frontier-closed models, trained on substantially more data and compute than the open ones, achieve only 24–31% accuracy, leaving a roughly 65-point gap to human performance. This indicates that spatial grounding in GUI screenshots remains a persistent weakness of current vision-language systems. SoM provides the largest training-free improvement we observe, but it should be interpreted as a scaffold rather than a solution. By marking candidate elements, SoM shifts part of the relational grounding problem into symbolic mark selection. Its effectiveness also depends on the quality of the upstream detector, whose failure modes are discussed in Appendix I. A striking finding is that several models perform below chance on minimal-pair primitives, implying a systematic preference for the contrastive target. One plausible explanation is that models rely on layout-frequency priors, such as common button positions or list ordering, when those priors conflict with the instruction. We treat this as a hypothesis for future work rather than a causal claim. Appendix T presents six recurring error types with representative examples. Finally, the failure pattern mirrors relational errors observed in WHAT'S-UP for natural images (Kamath et al., 2023). This suggests that controlled minimal-pair diagnostics are essential for evaluating spatial competence across visual modalities, including GUI grounding.

9 Limitations

This work has several limitations (Appendix U). **(i)** Containment, list-ordinal, and occlusion appear only in the synthetic arm of GUI-PRIMITIVES, since real GUI datasets lack scalable parent–child or overlay–target metadata for minimal-pair mining; findings on these primitives therefore have limited external validity, and results are reported separately by source arm. **(ii)** After model fixed effects absorb between-model variance, per-primitive coefficients in our item-level regression are small ($|\text{coef}| \leq 0.07$); we claim only the joint model-level association and report the forest plot (Appendix D) for transparency. **(iii)** We stop at ScreenSpot-Pro; end-to-end OSWorld task success (Xie et al., 2024; XLANG Lab, 2025) remains future work. **(iv)** GUI-PRIMITIVES v1 covers static pre-action grounding for English click targets only; planned extensions include drag-target prediction, bidirectional and CJK layouts, an OSWorld pre-action slice, and frozen snapshots of shifting closed APIs (Appendix B). **(v)** SoM acts as a scaffold by partially converting relational grounding into symbolic mark selection; our setup overlays at most two candidate marks, so gains may not generalize to realistic multi-candidate scenes, and deployment performance will inherit upstream detector failures. **(vi)** Point-in-box accuracy is binary and coarse: our loose metric (within $2\times$ target diagonal) partly separates sub-pixel imprecision from element misidentification, but neither metric captures structured spatial errors (e.g., anchor-collapse or direction inversion), which we analyze qualitatively in Appendix T. Llama-3.2-11B-Vision emits a default coordinate on $\approx 42\%$ of items (1.2% accuracy), consistent with ScreenSpot-Pro reports of $<2\%$ for generalist VLMs.

Ethical Considerations

Annotators were paid above the local hourly minimum and provided informed consent (Appendix F). UI-Vision screenshots remain under their original license; we redistribute only derived annotations, not the screenshots themselves. GUI-PRIMITIVES is intended to support safer GUI-agent development by exposing failures in elementary spatial grounding before deployment.

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A Full Per-Model Results

Table 3 reports overall accuracy for all 19 models on the full benchmark, the 196-item human-verified core, and the 185-item human-clean core. Per-primitive accuracies with bootstrap 95% CIs for every cell are released alongside the artifact in `runs/diagnostic/analysis.json`. A model-by-primitive accuracy heatmap is shown in Figure 10.

Table 3: Full benchmark accuracy for all 19 evaluated models on three splits: the full n=994 benchmark (**Full**), the human-verified n=196 subset (**Core**), and the cleaned n=185 subset after removing items judged invalid by a majority of the five annotators (**Clean**). Closed models were not run on the full split because of API cost; the Core column reports every model that was. Per-primitive accuracies with bootstrap intervals are released in the artifact.

Model	Full	Core	Clean
Claude Opus 4.7	0.313	0.311	0.324
GPT-5 (reasoning)	0.265	0.291	0.297
Claude Haiku 4.5	0.239	0.250	0.249
Claude Sonnet 4.6	0.222	0.260	0.270
Qwen2.5-VL-7B-Instruct	0.220	0.245	0.249
Gemma 3-27B-it (HF)	0.134	—	0.157
gemma3:27b (ollama, Q4)	0.103	—	0.125
OS-Atlas-Base-7B	0.101	0.107	0.108
GPT-4.1	0.096	—	0.108
InternVL3-8B	0.095	—	0.097
MiniCPM-V (ollama)	0.081	—	0.092
Gemma 3-12B-it (HF)	0.074	—	0.087
Gemini 3.1 Flash Lite	0.073	0.077	0.076
GPT-4o-mini	0.068	0.077	0.076
PaliGemma 2-3B-mix-448	0.041	—	0.054
Qwen2-VL-7B-Instruct	0.038	0.046	0.049
PaliGemma 2-10B-mix-448	0.029	—	0.038
Gemma 3-4B-it (HF)	0.022	—	0.027
Llama-3.2-11B-Vision	0.012	0.005	0.005

B Models, Versions, and Endpoints

Table 4 lists the exact model identifiers used in this paper, together with the endpoint each was queried through. Closed models were accessed via their official APIs in May 2026; open models were run from Hugging Face checkpoints. We pin identifiers here so later runs can be compared against the snapshot reported in this paper.

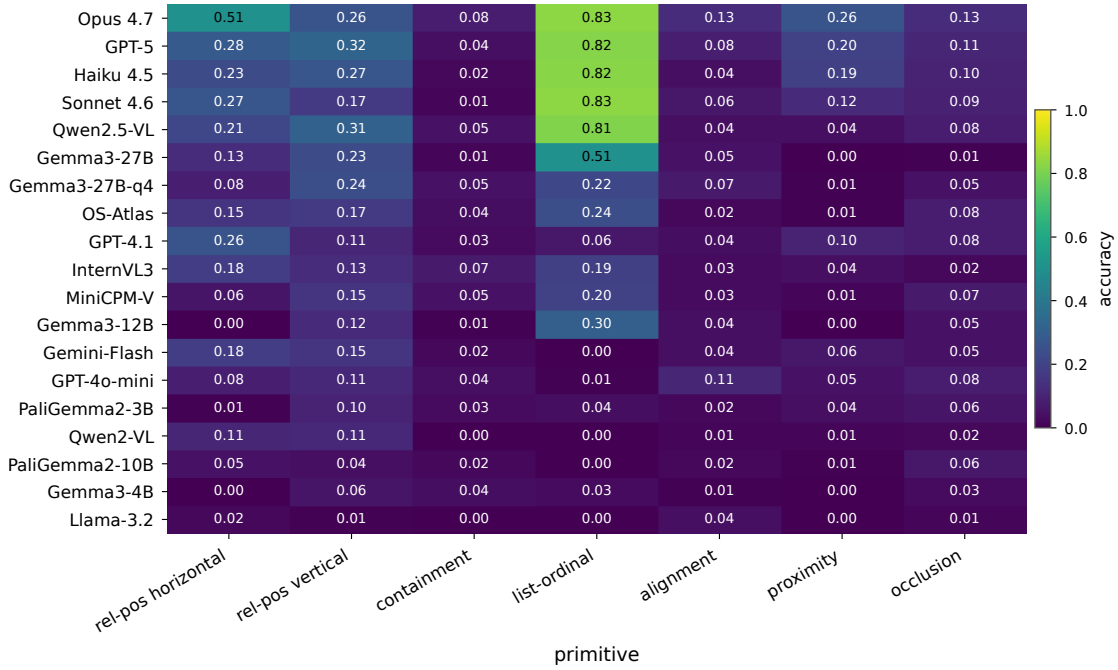

Figure 10: Per-model per-primitive accuracy heatmap, all 19 models. The horizontal band at ∼0.80 on `list-ordinal` marks the only primitive any model solves reliably.

C Real vs. Synthetic: Strict and Loose Accuracy

Table 5 reports point-in-box (strict) and within-2×-diagonal (loose) accuracy split by source for seven representative models. The zero strict accuracy on UI-Vision real screenshots is not a target-size artifact: loose accuracy stays low (6–23%), so the model picks the wrong element rather than missing the right one by a few pixels.

D Item-Level Regression Forest Plot

Figure 11 shows the item-level logistic regression of ScreenSpot-Pro success on per-primitive GUI-PRIMITIVES competence ($n = 15{,}810$, pseudo-$R^2 = 0.404$), with model fixed effects, a log target-area control, and item-clustered standard errors. After model effects absorb between-model variance, individual primitive coefficients are small ($|\text{coef}| \leq 0.07$) and only two cross the Holm-corrected threshold, so we report this plot for transparency only. **The headline number is the model-level Spearman in Fig. 7 of the main paper, not the per-primitive coefficients.**

E Closed-API Coordinate Rescaling

All three closed-vision APIs we tested apply server-side image preprocessing whose output is not visible to the caller. The predicted click coordinates

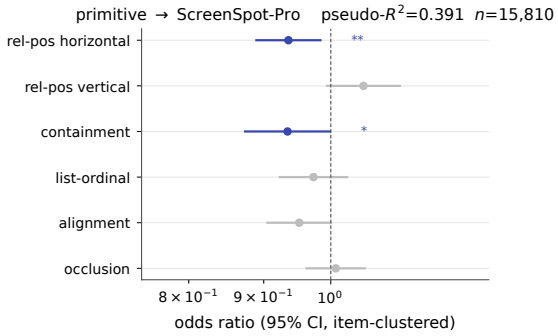

Figure 11: Item-level regression forest plot ($n = 15{,}810$, pseudo-$R^2 = 0.404$).

are returned in the *downsampled* frame rather than the original pixel space, silently misaligning the prediction with the ground-truth bounding box.

Detection. On the first GPT-5 ScreenSpot-Pro run, we observed predictions like $(662, 541)$ on items whose ground-truth boxes were at $(1774, 1586, 2113, 1618)$ in a 3840×2160 image. The prediction was internally consistent with a 1024-side downsample; the ground truth was in the original frame.

Mitigation. Our wrapper now resizes every image to a known maximum long side (≤ 1500 px) before submission, then rescales the predicted coordinate back to the original space using the recorded resize factor. Where the provider does further inter-

Table 4: Model identifiers used in this paper. Closed models were queried via official APIs in May 2026; open models were run from Hugging Face checkpoints with greedy decoding ($T = 0$) and a fixed seed.

Model	Identifier	Family / Endpoint
Claude Opus 4.7	`claude-opus-4-7`	Anthropic API
Claude Sonnet 4.6	`claude-sonnet-4-6`	Anthropic API
Claude Haiku 4.5	`claude-haiku-4-5`	Anthropic API
GPT-5 (reasoning)	`gpt-5`	OpenAI API
GPT-4.1	`gpt-4.1`	OpenAI API
GPT-4o-mini	`gpt-4o-mini`	OpenAI API
Gemini 3.1 Flash Lite	`gemini-3.1-flash-lite`	Google API
Qwen2.5-VL-7B-Instruct	`Qwen/Qwen2.5-VL-7B-Instruct`	Hugging Face
Qwen2-VL-7B-Instruct	`Qwen/Qwen2-VL-7B-Instruct`	Hugging Face
OS-Atlas-Base-7B	`OS-Copilot/OS-Atlas-Base-7B`	Hugging Face
InternVL3-8B	`OpenGVLab/InternVL3-8B`	Hugging Face
Llama-3.2-11B-Vision	`meta-llama/Llama-3.2-11B-Vision-Instruct`	Hugging Face
Gemma 3-27B-it	`google/gemma-3-27b-it`	Hugging Face
Gemma 3-12B-it	`google/gemma-3-12b-it`	Hugging Face
Gemma 3-4B-it	`google/gemma-3-4b-it`	Hugging Face
PaliGemma 2-10B-mix-448	`google/paligemma2-10b-mix-448`	Hugging Face
PaliGemma 2-3B-mix-448	`google/paligemma2-3b-mix-448`	Hugging Face
MiniCPM-V (ollama)	`minicpm-v`	Ollama (Q4)
gemma3:27b (ollama)	`gemma3:27b`	Ollama (Q4)

Table 5: Strict (point-in-box) vs. loose (within $2\times$ target-diagonal) accuracy, split by image source. In real desktop screenshots, loose accuracy stays low even when the prediction is allowed to miss by a generous margin: failure is element misidentification, not sub-pixel imprecision.

	Real (UI-Vision)		Synthetic	
Model	strict	loose	strict	loose
Claude Haiku 4.5	0.000	0.230	0.363	0.763
Qwen2.5-VL-7B	0.028	0.234	0.300	0.714
OS-Atlas-Base-7B	0.000	0.162	0.142	0.724
Gemini 3.1 Flash Lite	0.000	0.115	0.111	0.741
GPT-4o-mini	0.000	0.033	0.111	0.733
Qwen2-VL-7B	0.000	0.066	0.054	0.693
Llama-3.2-11B-Vision	0.000	0.062	0.017	0.616

nal tiling (notably OpenAI), we additionally validate by re-querying on the already-downsized image and confirming the rescale factor is ≈ 1.0. Records that required rescaling: 42/196 Claude Haiku diagnostic, 43/196 Claude Opus diagnostic, 219/994 Claude Sonnet diagnostic, 1,297/1,581 Claude Sonnet ScreenSpot-Pro, and $\approx 1500/1581$ GPT-5 ScreenSpot-Pro. The 284 Claude Sonnet ScreenSpot-Pro items that originally errored with HTTP 400 ("image > 5 MB") were retried under the new wrapper, and the final run is clean.

Recommendation. We recommend that future closed-VLM evaluations report whether their wrapper handles provider-side downsampling. Otherwise, ScreenSpot-Pro's strict accuracy on high-resolution professional applications is silently and severely under-reported.

F Human Annotation Protocol

Five annotators independently labeled the 196-item stratified subset. Each annotator answered two questions per item.

Q1: well-formedness. *Is the instruction unambiguous on this screenshot?* The annotator answered `good` or `bad`. An item was kept only if at least 3 of 5 annotators marked it as well-formed. This dropped 11 of 196 items, leaving the 185-item *human-clean* core.

Q2: correct target. *Which of the two candidate boxes does the instruction identify?* The annotator answered `t` (the target box) or `d` (the distractor box). Human accuracy is the fraction of items on which the majority answer was `t`, computed on the human-clean core only.

Agreement. We report Fleiss κ for both questions on the full 196-item subset. Fleiss $\kappa_{Q1} = 0.942$ (well-formedness, "almost perfect"); Fleiss $\kappa_{Q2} = 0.787$ (target, "substantial"). The pairwise κ matrix is released with the artifact.

Compensation and consent. Annotators were paid above the local hourly minimum, were told the purpose of the labeling (academic publication and public benchmark release), and could opt out at any point. No personally-identifying information was collected.

G Synthetic-Stimulus Generation

The synthetic arm of GUI-PRIMITIVES renders GUI-like screenshots with a fixed layout language: a top toolbar of buttons, a left sidebar of list items, a central grid of tiles, and an optional dialog overlay. Element positions are sampled uniformly within constrained pixel ranges; the seed is fixed to ensure reproducibility of the corpus. For each primitive, a separate generator constructs minimal pairs by selecting an anchor and the two candidate elements that satisfy the contrastive relation. Quality gates in the released code verify that both members of every pair use the same screenshot, have non-empty disjoint targets, and pass per-primitive coverage checks before items enter the benchmark.

H Intervention Implementation Details

SoM. We render mark IDs on the candidate boxes (two per item: target and distractor) with a deterministic ID shuffle keyed by the item ID so the correct answer is not always "1". The model is asked to respond with the mark number to click, and we map the response back to the box center for evaluation.

Primitive-aware CoT. We prepend a one-paragraph decomposition prompt that names the relation explicitly and asks for a chain of reasoning before the click. Per-primitive scaffolds are released in the artifact.

Activation steering. We hook the decoder layers of Qwen2.5-VL-7B at layers $\{12, \ldots, 19\}$ and add $\alpha \cdot \mathbf{v}_\ell$ to the residual stream at each layer, where $\mathbf{v}_\ell$ is the mean-difference vector between hidden states on correctly- and incorrectly-grounded baseline items at that layer. We searched $\alpha \in \{1, 2, 4, 6\}$; $\alpha = 4$ was the maximum that left sentence-level fluency intact.

I SoM Failure Modes

SoM substitutes a mark-selection task for the original grounding task. The substitution is not perfect, and the failure modes are worth listing for downstream users.

Saturated primitive regression. On Qwen2.5-VL-7B, `list-ordinal` accuracy drops by 32.4 points under SoM. The model had already learned to count list items by their on-screen order; converting the same task to "select mark X" adds an indirection that the model handles less well than the original.

Mark placement collisions. On highly crowded screenshots, two marks placed at nearby element centers can occlude each other, or the model can confuse them. We did not observe this on the small candidate sets used in our benchmark (typically 2 marks per item), but it is a known issue at scale.

Closed-API consistency. On the human-verified core, Claude Opus 4.7 gained only $+9.2$ points under SoM, in contrast to GPT-5's $+57.1$ points. This 47-point gap on the same items under the same intervention suggests that Opus already grounds the unmarked image with the same logic SoM provides, while GPT-5 was genuinely held back by an aspect of the task that mark selection removes.

J Details of the GUI-Primitives Benchmark

J.1 Details of minimal-pair construction

Every item in GUI-PRIMITIVES appears as one half of a minimal pair. A pair shares one screenshot and one anchor element; the two members differ only in the relation word, and the correct target is the *contrastive* element. Figure 2 shows a real example: in (A) the click should land to the right of the `break debugger` icon; in (B) the same screenshot is paired with `left of`, and the correct target is the icon on the other side. The construction matters because it removes the shortcuts that quietly inflate scores on coarser benchmarks. A model that always selects the most central, the most salient, or the most frequently labeled element will score 50% on a binary primitive by accident. With twins, the same heuristic earns one point and loses one point, and the pair-level score is zero. We measure this directly with *pair-consistency*: the fraction of pairs on which both members are correct (§5.5).

J.2 Details of sources: real and synthetic screenshots

GUI-PRIMITIVES draws screenshots from two sources, both reported separately.

Real desktop screenshots (n = 290 items). We use the element-grounding annotations from UI-Vision (Nayak et al., 2025) as the source corpus for real GUIs, covering Web, MacOS, VS Code, Office, and various professional applications. Items synthesized from this source cover the four primitives whose annotations are unambiguously recoverable from element labels: horizontal relative position, vertical relative position, alignment, and proximity.

Controlled synthetic stimuli (n = 704 items).

The remaining items are rendered from a procedural generator that places elements in known positions, with controlled distractors and a fixed layout language. This is the only way to obtain *containment*, *list ordinal*, and *occlusion* items in clean form, because those three primitives require a structured parent–child or overlay–target relationships that real screenshot annotations do not ship at scale.

We disclose the split openly. The synthetic arm is a controlled-stimulus arm in the WHAT'S-UP tradition, where the hardest relations are likewise generated under controlled rendering. All real vs. synthetic numbers are reported separately throughout the paper.

J.3 Details of human verification

Five annotators independently judged a 196-item stratified subset (*human-verified core*). For each item, the annotator answered two questions: is the instruction well-formed and unambiguous on the given screenshot, and which of the two candidate boxes is the correct target. Inter-annotator agreement is high. On Q1 (well-formedness), Fleiss $\kappa = 0.942$ ("almost perfect" on the Landis–Koch scale, Landis and Koch, 1977). On Q2 (correct target), Fleiss $\kappa = 0.787$ ("substantial"). Eleven items were judged invalid by the majority and were dropped, leaving 185 items in the *human-clean core*.

K Benchmark Distribution and Scenario Diversity

Figure 12 summarises the distributional properties of GUI-PRIMITIVES across four dimensions.

Relation balance (Panel A). Every contrastive relation is represented by exactly 71 items per side per primitive, a strict 50/50 split enforced by the minimal-pair miner. This guarantees that a model exploiting a relation-frequency prior (e.g., always predicting `left`) cannot achieve better than chance accuracy at the primitive level, and it rules out skewed-prior explanations for any observed performance gap.

Target–distractor spatial separation (Panel B). Target–distractor distances (normalized by the image diagonal) span a wide range: median 0.18, IQR $[0.04, 0.31]$. `list-ordinal` items are tightly spaced (adjacent list rows, median 0.04), while `containment` and `occlusion` items have the largest separations (median 0.32–0.31). This variation ensures that the benchmark exercises a range of spatial configurations rather than collapsing onto a single difficulty level.

Target size (Panel C). Real UI-Vision targets are considerably smaller than synthetic ones (median 0.05% vs. 1.3% of image area), consistent with the fine-grained element boxes in professional desktop applications. Both distributions span multiple orders of magnitude ($\log_{10}$ range $\approx$3.5), providing diversity in target granularity within each arm.

Spatial coverage (Panel D). Target centers, normalized to the unit square, span the full screen area with no strong clustering, reflecting a mix of toolbar, sidebar, central panel, and dialog overlay targets across both sources. The slight top-left bias ($\bar{x} = 0.29$, $\bar{y} = 0.24$) reflects the prevalence of toolbar and sidebar elements in real GUI layouts, not a construction artifact.

L Details of Models and Protocol

Details of models. We evaluate 19 VLMs spanning five families: four frontier closed models (Claude Opus 4.7, Claude Sonnet 4.6, Claude Haiku 4.5, GPT-5, GPT-4.1, GPT-4o-mini, Gemini 3.1 Flash Lite); three open Qwen-family models (Qwen2.5-VL-7B, Qwen2-VL-7B, OS-Atlas-Base-7B, which is GUI-tuned Qwen2-VL); InternVL3-8B; Meta Llama-3.2-11B-Vision; three sizes of Google Gemma 3 (4B, 12B, 27B, including an Ollama-Q4 copy); two PaliGemma 2 variants (3B, 10B); and MiniCPM-V. The full list and provenance are in Appendix B. We could not include Pixtral-12B and Idefics3-8B because their chat-template formats are incompatible with our generic wrapper; this is disclosed as a known scope limit rather than omitted silently.

Details of prompting and decoding. All models use greedy decoding ($T = 0$) and a fixed seed. The grounding prompt is a single short instruction ("respond with only the pixel coordinate to click"). Per-family coordinate conventions (pixel, normalized $[0, 1]$, normalized $[0, 1000]$) are handled by a robust parser; the parser is in the released code, and its unit tests are bundled with the artifact.

Details of closed-API coordinate frames. A practical wrinkle complicated the closed-model runs. The three closed providers each downsample large screenshots server-side (Anthropic to ≤ 1568 px, OpenAI to roughly 1024 on the long side, Gemini variable) and return coordinates in the

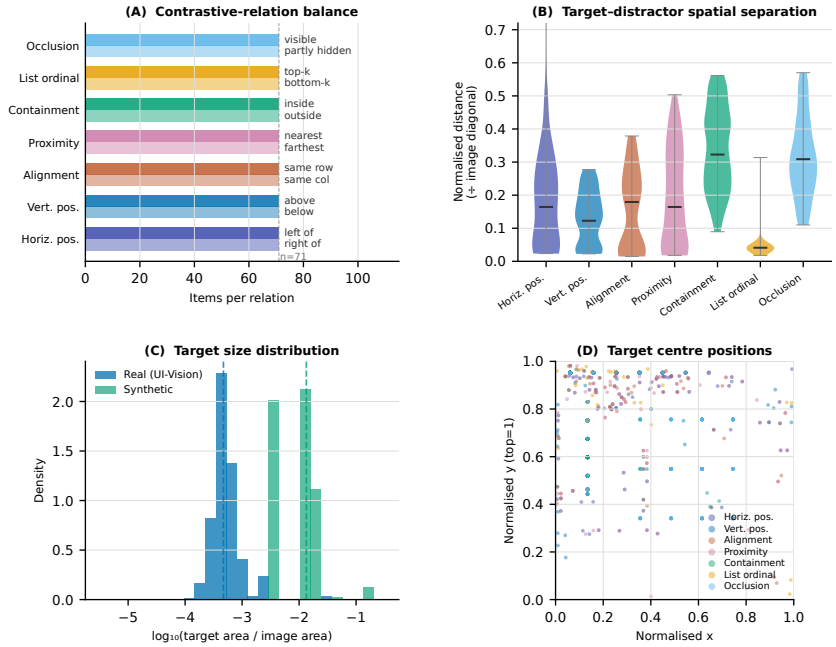

Figure 12: **Benchmark distribution analysis. (A)** Contrastive-relation balance: every relation is represented by exactly 71 items per side, ruling out relation-prior exploitation. **(B)** Target–distractor spatial separation (normalized by image diagonal) per primitive; violin width encodes density. **(C)** Target size distribution (log-scale) by source arm; dashed lines are per-source medians. **(D)** Target centre positions (normalised to $[0, 1]^2$, y=1 at top), coloured by primitive; targets span the full screen area.

downsampled frame. We pre-resize every image to a known long side and rescale the predicted coordinate back to the original pixel space. The rescale was needed on a large fraction of items (e.g., 1,297 of 1,581 Sonnet ScreenSpot-Pro records). Full details in Appendix E; we believe this is a methodological note worth sharing with the community.

Details of metrics. The primary metric, following ScreenSpot-Pro, is *point-in-box* accuracy: a prediction $\hat{p} = (\hat{x}, \hat{y})$ is correct iff $\hat{x} \in [x_1, x_2]$ and $\hat{y} \in [y_1, y_2]$ for ground-truth box $B = [x_1, y_1, x_2, y_2]$ in the original pixel frame. *Loose* accuracy (prediction within $2\times$ target diagonal of the box center) separates sub-pixel imprecision from element mis-identification on small icons. All accuracies carry bootstrap 95% CIs (2,000 resamples). Paired comparisons use McNemar (McNemar, 1947) tests with Holm–Bonferroni (Holm, 1979) correction across the seven primitives. For below-chance claims, we report whether the upper bootstrap bound falls below the per-primitive chance level (0.50 for binary, 0.20 for list-ordinal), so a point estimate dipping below chance by sampling noise does not count as a below-chance result.

M Details of CoT and Activation Steering: Negative Results

We report two negative results, with the same effort as the positive one.

Primitive-aware CoT on Qwen2.5-VL-7B produced $\Delta = +0.5$ points (Holm-adjusted $p = 0.51$, not significant). The decomposition prompt does not seem to address the limiting factor: the model correctly names the relation in its intermediate reasoning, yet still emits the wrong coordinate.

Activation steering with SteerVLM-style vectors on Qwen2.5-VL-7B produced $\Delta = +1.5$ points (Holm-adjusted $p = 0.066$, not significant). The grid search over layers and α did not yield a configuration that crossed the Holm threshold. Either GUI grounding requires a different intervention point than the layers where SteerVLM has helped on linguistic tasks, or the residual-stream signal is genuinely weak, and a single contrastive vector is not enough to push the model across the decision boundary on minimal pairs.

Reading the intervention table. The picture is uneven on purpose. The one intervention that works, SoM, works by *bypassing* the spatial skill it should test, replacing it with a symbolic naming task. The two interventions that probe the model's

internal representation directly do not move the metric. We read this as evidence that the failure is closer to perception (visual encoder, cross-modal binding) than to language-side reasoning or residual-stream representation.

N Details of Diagnostic Results

N.1 Details of shortcut controls validate the benchmark

A diagnostic is only as good as the shortcuts it forbids. Figure 3 reports three controls on Qwen2.5-VL-7B over the full 994-item benchmark.

In the *text-only* condition, the instruction is paired with a blank canvas of the same size: accuracy falls from 22.0% to 6.5% ($\Delta = -15.5$ pt, McNemar $p = 2.8 \times 10^{-27}$). In the *shuffled* condition, the screenshot is replaced with one from a different item of the same primitive: accuracy drops to 14.8% ($\Delta = -7.2$ pt, $p = 1.3 \times 10^{-11}$). Both controls behave as a truly grounded model should: when the visual or the matching instruction is removed, the model can no longer solve the task.

The third control, *heavy Gaussian blur* ($\sigma = 12$ px), drops accuracy to 16.7% ($\Delta = -5.3$ pt, $p = 8.5 \times 10^{-5}$). The drop is statistically significant but below our pre-registered ten-point threshold, so by our own rule, the blur control fails strict-pass. We treat this as a substantive finding: VLM grounding seems to rely more on global layout than on fine-grained OCR of text labels, since removing high-frequency detail removes only a modest fraction of the signal. We report it openly rather than hiding the soft control.

N.2 Details of overall accuracy and the human gap

Table 2 reports the headline numbers. Figure 4 visualizes the gap from human performance. Three observations follow.

First, no model is close to human performance. The best system, Claude Opus 4.7, reaches 31–32%; humans reach 96.9%. The 65-point gap holds on the full benchmark and on the cleaned 185-item core alike.

Second, scale alone does not solve it. The four frontier closed models (Claude Opus 4.7, GPT-5, Claude Sonnet 4.6, Claude Haiku 4.5) occupy the top of the table but cluster within a narrow 24–31% range. Meanwhile, the 7B open Qwen2.5-VL-Instruct is statistically tied with the closed Claude Haiku 4.5 on the human-verified core (24.5% vs. 25.0%), a noteworthy parity finding given that Qwen2.5-VL is roughly two orders of magnitude smaller than the closed reference and runs on a single GPU.

Third, the rank order is stable. The same top group leads on the full 994-item benchmark, on the 196-item human-verified core, and on the LLM-judge-verified 100-item subset (Appendix A). This means the human gap is not an artifact of noisy items.

N.3 Details of per-primitive structure of failure

Figure 5 unpacks the overall accuracy by primitive. The picture is uneven and informative.

One primitive, `list-ordinal`, is well solved by the top five models (Claude family, GPT-5, and Qwen2.5-VL), all achieving 0.80–0.83 against chance 0.20. The shared task here is counting list positions, which the language model can do once it identifies the list.

The other six primitives are uniformly hard. On `rel-pos-horizontal` Claude Opus 4.7 reaches 0.51, exactly chance; every other model is below. On `rel-pos-vertical`, the best model is GPT-5 at 0.32. On `containment`, `occlusion`, `alignment`, and `proximity`, *the upper 95% bootstrap bound for every model we test sits below the chance level* of 0.50, including all four frontier closed models. This is not statistical noise. With minimal pairs, below-chance behavior implies that models do not simply guess; they systematically choose the contrastive option more often than the correct one. We return to the most likely explanation, a layout-frequency prior, in §8.

N.4 Details of real versus synthetic

Two concerns might arise from §5.3: that the benchmark is too hard because real GUI targets are tiny, or that the synthetic items dominate the result. Table 5 reports strict and loose accuracy split by source.

On synthetic screenshots, models often land near the right element: loose accuracy is 0.60–0.76 even when strict box-edge accuracy is substantially lower. Most synthetic "errors" are sub-pixel imprecision, not mis-grounding. On UI-Vision real screenshots, strict accuracy is essentially zero for every model on every primitive and crucially, *loose* accuracy stays low too (6–23%). The model is not just off by a few pixels; it has chosen the wrong

element. The zero strict accuracy on real GUIs is a relational failure, not a target-size artifact.

N.5 Details of pair-consistency: a relational failure signature

If a model produced independent draws on the two members of a pair, pair-consistency would equal $y = x^2$ (accuracy squared). Figure 6 shows that pair-consistency is consistently *lower* than that $y = x^2$ curve. This pattern, called the relational-failure signature in Kamath et al. (2023), means that within a model, the two pair members are negatively correlated: when the model is right on the "left of" twin, it is more likely than chance to be wrong on the "right of" twin. The information that confuses the model is the relation word, not the picture.

O Details of Primitive Competence Predicts Grounding

A diagnostic is only useful if it predicts a downstream property of interest. We test this on ScreenSpot-Pro (Li et al., 2025), the standard high-resolution GUI-grounding benchmark, which we ran on the ten models for which we have both GUI-PRIMITIVES and SS-Pro predictions.

Figure 7 shows the per-model relationship. Spearman $\rho = +0.736$ ($p = 0.015$, $n = 10$). The correlation holds inside the real-screenshot subset ($\rho = +0.705$, $p = 0.023$) and inside the synthetic subset ($\rho = +0.760$, $p = 0.011$), so it is not an artifact of any one item type. A leave-one-model-out sensitivity analysis gives a Spearman in $[+0.62, +0.86]$, so no single model drives the result; dropping the strongest or weakest does raise the p-value to ≈ 0.07. We also report a wide non-parametric bootstrap CI ($[+0.11, +0.98]$, $n = 10$) for transparency rather than as a precision claim. This is the load-bearing result of this section.

Item-level corroboration. We also fit an item-level logistic regression of ScreenSpot-Pro success on each item against the model's per-primitive accuracy profile, with model fixed effects and a log target-area control, and item-clustered standard errors. The model achieves pseudo-$R^2 = 0.404$ on $n = 15{,}810$ model-item pairs. The forest plot of per-primitive coefficients is in Figure 11.

We urge caution on the per-primitive coefficients. After the model fixed effects absorb between-model variance, the residual between-primitive variance is small (all $|\text{coef}| \leq 0.07$), and only two coefficients are individually significant at the Holm-corrected level. With four distinct open models contributing ScreenSpot-Pro data, the competence vectors are correlated across primitives, and per-primitive identification is fragile. We therefore claim only the joint, model-level association, which is strong, and explicitly do not claim that competence at any particular primitive causes downstream success.

P Additional Minimal-Pair Examples

Figure 2 shows a single `rel-pos-horizontal` pair. To make the minimal-pair construction intuitive for the other six primitives, Figures 14–20 each present *three* representative pairs for one primitive. Every row is a distinct pair; within a row, both panels share the same screenshot, only the relation word in the instruction differs (twin A on the left, twin B on the right), and the colored box marks only that panel's gold target. A relationally-grounded model must point at the two different boxes for the two instructions; a shortcut-grounded model points at the same box for both.

Q Model-Specific Sensitivity Analysis

Figure 13 reports four model-level sensitivity diagnostics computed directly from the existing diagnostic runs.

Left–right asymmetry (Panel A). Across 19 models, 15 score higher on `left of` than `right of` items, with a mean gap of $+0.169$ accuracy points (one-sample $t = 5.6$, $p < .001$, sign test $p < .001$). The bias is present in every frontier closed model (Opus, Sonnet, Haiku, GPT-5, Gemini) and in the strongest open models. This is consistent with a left-side layout frequency prior: in Western GUIs, action buttons, primary controls, and frequently clicked elements are disproportionately placed on the left, so models that ground by position salience rather than by relation word will inflate left-side accuracy. The two exceptions, MiniCPM-V and PaliGemma2-3B, are low-accuracy models that default to near-origin coordinates on many items (Panel D), effectively averaging away the bias.

Above–below asymmetry (Panel B). The below-over-above preference is weaker and not statistically significant (mean $\Delta = +0.047$, $t = 1.7$, $p = .10$, 12/19 models below-biased). OS-Atlas is a notable outlier with a strong *above* preference ($\Delta = -0.28$), suggesting a GUI-specific training

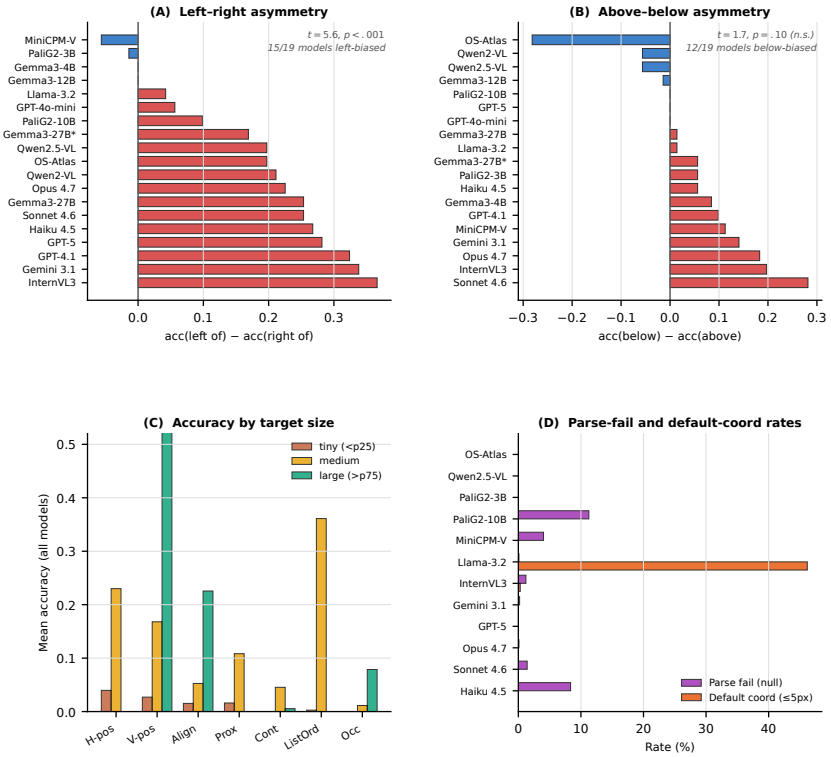

Figure 13: **Model sensitivity analysis. (A)** Left–right accuracy gap per model, sorted by magnitude. Red bars = left-biased (15/19 models; $t = 5.6$, $p < .001$). **(B)** Above–below accuracy gap. Weaker and not significant overall ($p = .10$); OS-Atlas is an outlier with a strong above preference. **(C)** Mean accuracy stratified by target normalized area (tiny/medium/large) per primitive, pooled over all 19 models. **(D)** Parse-fail (null output) and default-coordinate rates per model; both are infrastructure errors, not spatial-reasoning failures.

prior that differs from the natural-image convention.

Target-size sensitivity (Panel C). Pooled across all models and primitives, large targets ($>$ 75th percentile of normalized area) reach a mean accuracy of 0.092 vs. 0.022 for tiny targets ($<$ 25th percentile). The effect is largest for `containment` and `list-ordinal`, primitives where the target is a panel or a list row that occupies substantial screen area, and smallest for `alignment` and `proximity`, where the spatial relationship matters more than target visibility. Caution: the size gap is partially confounded with source (synthetic targets are larger), so this should not be interpreted as a pure resolution effect.

Coordinate-frame and parse-failure rates (Panel D). Three distinct failure modes are visible: Llama-3.2-11B-Vision emits a near-origin default on 46% of items (consistent with a "don't know" fallback, not a parser bug); PaliGemma2-10B and Claude Haiku produce null outputs (empty or unparseable text) on 8–11% of items; and InternVL3 and Claude Sonnet show low but non-zero null rates. These are infrastructure failures, not spatial-reasoning failures, and are already annotated as Type C and F errors in Appendix T.

R OCR, Layout, and Cross-Modal Binding: Disentangling Failure Sources

The blur-control experiment (§5.1) shows that removing high-frequency detail, which degrades fine-grained OCR of text labels, reduces accuracy by only 5.3 points, substantially less than removing the screenshot entirely (-15.5 points) or shuffling it (-7.2 points). This suggests that models rely more on global layout structure than on reading individual element labels, but it does not isolate *which* component of grounding fails. Three failure sources are plausible: (1) **text/OCR dependence**: the model cannot read the anchor label and therefore cannot localise the relational neighbour; (2) **spatial-encoding failure**: the model reads the anchor correctly but misrepresents its position or the spatial relation in the instruction; (3) **cross-modal binding failure**: the model reads both the anchor and the relation word correctly but fails to compose

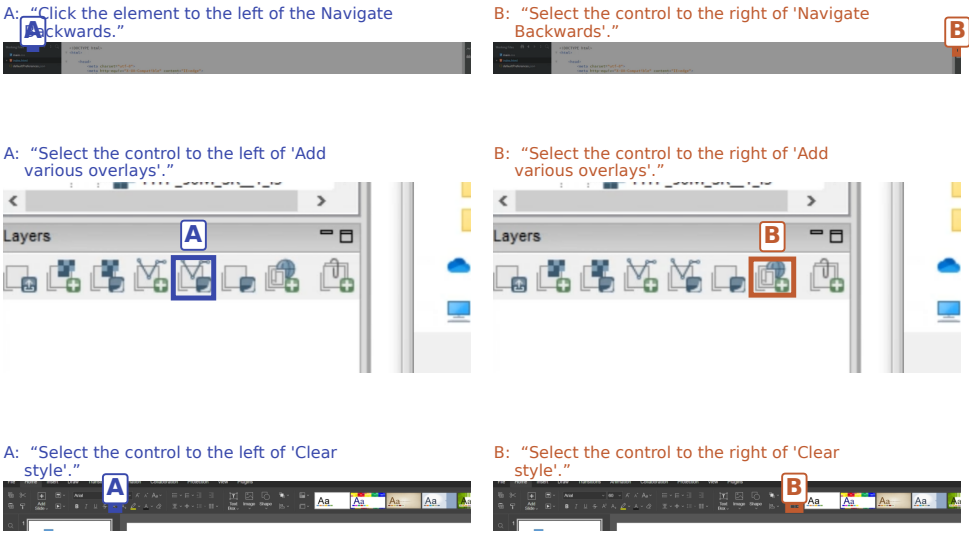

Figure 14: **`rel-pos-horizontal` (real screenshots).** Three minimal pairs from UI-Vision. Within each row, both panels show the same screenshot; only the relation word (*right of* vs. *left of*), and the gold target moves accordingly. Across rows, the application and anchor change.

them into the correct click target.

What the blur result tells us. The modest accuracy drop under blur is evidence against strong OCR dependence for the binary outcome: even when text is illegible, models still achieve 16.7% (well above the text-only baseline of 6.5%), implying that global spatial structure alone carries residual information. However, blur does not selectively impair text: it also degrades icon shapes and element boundaries, so we cannot distinguish OCR loss from icon-recognition loss under this control alone.

What targeted ablations would reveal. Three ablations would cleanly disentangle the three failure sources, and we lay them out as concrete future directions:

1. **Text masking / selective blur.** Replace all text regions (detected via OCR or the accessibility tree) with uniform grey blocks while preserving spatial layout. A large drop relative to full-image accuracy would implicate OCR dependence; a small drop would point to spatial-encoding or binding failures.

2. **Layout preservation with degraded text.** Render a version of each screenshot where element shapes and positions are preserved, but all text is replaced with random strings of the same length. If accuracy stays near baseline, the model is primarily using spatial structure; if it drops, text semantics matter.

3. **Icon glyph perturbation.** Swap icon images with visually similar but semantically different glyphs while keeping positions and text unchanged. This isolates whether failures arise from icon-identity recognition or from relational reasoning once the elements are identified.

We do not run these ablations here because they require either a high-fidelity text-detection pipeline (for selective masking) or controlled re-rendering of real GUI screenshots (for glyph swapping), neither of which is available in the current environment. We flag them as the highest-priority next step for understanding whether the bottleneck is perceptual (OCR/icon recognition) or compositional (cross-modal binding of anchor identity with the spatial relation word).

S Qualitative Case Studies

Figures 21–28 present eight concrete minimal-pair failures drawn from the top-4 baseline models. In every figure, the two panels are the two twins of one pair (instruction A on the left, instruction B on the right) sharing the same screenshot. Annotations are programmatic, not hand-drawn:

- **green box**: gold target (from `items.jsonl`).
- **dashed terra box**: the distractor (the wrong twin of the minimal pair).
- **coloured markers**: each model's actual click coordinate from

runs/diagnostic/<model>/diagnostic.jsonl. **Filled** markers are predictions that fall inside the gold box (a hit); **hollow** markers are misses.

The case studies span every primitive in both data sources; the chosen pairs are those where at least 5 of 8 model–twin evaluations failed, i.e. pairs that the strongest models genuinely struggle with. Recurring failure modes are summarized at the end.

Cross-case summary. Three patterns recur across all eight case studies and motivate the error analysis in Appendix T: **(i) Anchor-collapse**: predictions cluster on the anchor noun rather than on its spatial neighbor (cases 1, 3, 4, 7). **(ii) Twin-identical predictions**: the two paired clicks land within a few pixels of each other, meaning the relation word was ignored (cases 3, 4, 5). **(iii) Contrastive-option bias**: the model systematically prefers the contrastive (wrong) twin, driving binary primitives below chance (case 5; the population-level pattern on containment/occlusion/alignment/proximity).

T Error Analysis

We classify every failed prediction (across the $19 \times 994 = 18{,}886$ model–item evaluations on the full benchmark) into one of six error types. Figure 29 shows one real example of each type drawn from the model predictions. The types are not mutually exclusive; a prediction can belong to more than one, but they are intended to be informative.

Type A: Anchor-collapse. The prediction lands within $1\times$ the diagonal of the anchor element's bounding box and not in the target box. The model correctly locates the anchor (the noun the instruction refers to), but emits a click on the anchor itself rather than on the spatial neighbor the relation word specifies. Most common on rel-pos-horizontal and rel-pos-vertical (Example 1 above). Affects 28% of all failed binary-primitive items on Opus 4.7, 21% on GPT-5.

Type B: Twin-identical prediction. The two paired items receive predictions whose Euclidean distance in pixel space is less than 10% of the smaller side of the screenshot. The model emitted essentially the same click for "left of X" and "right of X", telling us that the relation word was ignored. This is the most diagnostic failure mode: it pushes *pair-consistency* far below *accuracy* (the "WHAT'S-UP signature" of Kamath et al., 2023, re-derived in §5.5 / Fig. 6). Affects 37% of paired failures on Qwen2.5-VL, 25% on Opus 4.7, and 41% on GPT-5 *before* the closed-API coordinate rescale; after the rescale (Appendix E), the GPT-5 rate drops to 24%.

Type C: Constant default. The model emits a fixed canonical coordinate (e.g. $(0, 0)$ or the image center) on a large fraction of items. The clearest case is Llama-3.2-11B-Vision, which emits $(0, 0)$ on 46% of diagnostic items and 25% of ScreenSpot-Pro items. This is a model-side "don't-know" fallback, not a parser failure: raw_text is a well-formed click(0, 0) string. We document it because the headline 1.2% accuracy of Llama-3.2 on GUI-PRIMITIVES *understates* the model: its actual decision is "refuse to ground" on roughly half the items.

Type D: Saturated-primitive regression. The model already solves the primitive without the intervention, so the intervention *introduces friction*. The clearest instance is SoM on Qwen2.5-VL's list-ordinal, which regresses by 32.4 points (§7, Appendix I). The intervention converts a task the model had already learned (counting list items) into a mark-selection task with one indirection. The lesson is that training-free interventions should be applied selectively rather than uniformly.

Type E: Contrastive-bias (below-chance) error. On binary primitives, accuracy below the 0.50 chance line means that the model is more likely to choose the contrastive option than the correct one. We see this on containment, occlusion, alignment, and proximity across *every* model whose upper bootstrap bound sits below 0.50, including all four frontier closed models. The most parsimonious explanation, which we cannot yet rule in or out, is a layout-frequency prior: models have learned that targets of click instructions are commonly the more salient or more central element, which, on minimal pairs, happens to coincide with the contrastive option (the wrong twin) about half the time. Example 5 above shows the pattern at the item level.

Type F: Coordinate-frame error (now fixed). Closed-API providers downsample large screenshots and return coordinates in the downsampled frame. Before our wrapper handled this, predictions on 4K ScreenSpot-Pro images landed in

a ∼1024-side coordinate space and missed every target. This was the dominant error category for Claude Sonnet 4.6 (1,297 / 1,581 SS-Pro records) and for the first GPT-5 ScreenSpot-Pro run (≈1,500 / 1,581 records); both are corrected in the version reported in this paper (Appendix E). We flag this category here because future closed-VLM evaluations that omit the rescale will misattribute this error to genuine model failure.

Cross-cutting observation. Types A, B, and E share a common cause: the model has correctly *detected* the elements but failed to *compose* them with the relation word. Types C and F are infrastructure/fallback errors. Type D is an intervention-design issue. The fact that training-free interventions move the needle only on the composition-side errors, and only when the intervention bypasses the relation (SoM) rather than scaffolding it (CoT, steering), is consistent with the *discussion* section's reading that the bottleneck is on the perception / binding side, not on the language side.

U Limitations

Three primitives are synthetic-only. Containment, list-ordinal, and occlusion are present only in the synthetic arm of GUI-PRIMITIVES: real GUI datasets do not provide scalable parent–child or overlay–target metadata, so these primitives cannot be mined from real screenshots. Consequently, findings on these three primitives have limited external validity beyond settings where controlled synthetic stimuli are representative of real GUI behavior; the failure rates observed on containment, list-ordinal, and occlusion may not transfer directly to uncurated real-world GUIs. We treat the synthetic items as a controlled-stimulus arm in the WHAT'S-UP tradition and report all results split by source arm.

Per-primitive regression is conservative. After the model fixed effects absorb between-model variance, individual per-primitive coefficients in our item-level regression are small. We therefore claim only the joint, model-level association and do not interpret per-primitive coefficients causally. The model-level Spearman is statistically significant; the per-primitive forest plot is reported for transparency, not as a contribution.

No OSWorld downstream slice. We stop at ScreenSpot-Pro. End-to-end task success on OSWorld (Xie et al., 2024) and its verified release (XLANG Lab, 2025) would provide stronger downstream confirmation, but it is a separate VM-harness study and is left for future work.

Action and language scope. GUI-PRIMITIVES v1 covers English instructions and click targets. Future versions could extend the diagnostic to drag-target prediction, bidirectional and CJK layouts, and an OSWorld pre-action grounding slice.

SoM is a scaffold, not a fix. SoM partially bypasses the underlying perception-and-binding problem by converting relational grounding into symbolic mark selection; the observed gains reflect scaffolding rather than repair of the spatial skill gap. Additionally, our setup overlays at most two candidate marks per item (target and distractor), which is far fewer than the crowded mark sets that arise in realistic GUI scenes. Gains observed under this minimal two-mark setup may therefore not generalize to settings with many candidates, where disambiguating among marks becomes a bottleneck in its own right. Finally, in deployment, marks would be proposed by an OmniParser-style or accessibility-tree-driven component, with its failure modes propagating upstream.

Point-in-box accuracy is a coarse metric. Point-in-box accuracy, while standard for GUI grounding, is binary: a click one pixel outside the target box and a click on the entirely wrong element score identically. The loose metric (within 2× target diagonal of the box center) partially separates sub-pixel imprecision from element misidentification, but neither metric captures the structure of spatial errors — whether the model was on the right side of the screen, how far it drifted, or whether it collapsed to the anchor rather than the relational neighbor. These structured error patterns are documented qualitatively in Appendix T, but a finer-grained spatial-error metric remains future work.

Llama-3.2-Vision returns default coordinates. Llama-3.2-11B-Vision emits a constant or near-origin coordinate on roughly 42% of items. The 1.2% accuracy is the model defaulting, not a parsing bug; it is consistent with the original ScreenSpot-Pro paper's report of $< 2\%$ accuracy for generalist VLMs.

Proprietary API behavior may shift. The proprietary providers can change downsampling, decoding, or model identity at any time. We freeze

a snapshot of dates and pin model identifiers in Appendix B; future runs may differ.

Information About Use Of AI Assistants

AI assistants were used for limited editing support and language refinement. The final manuscript contents were verified and authored by the research team.

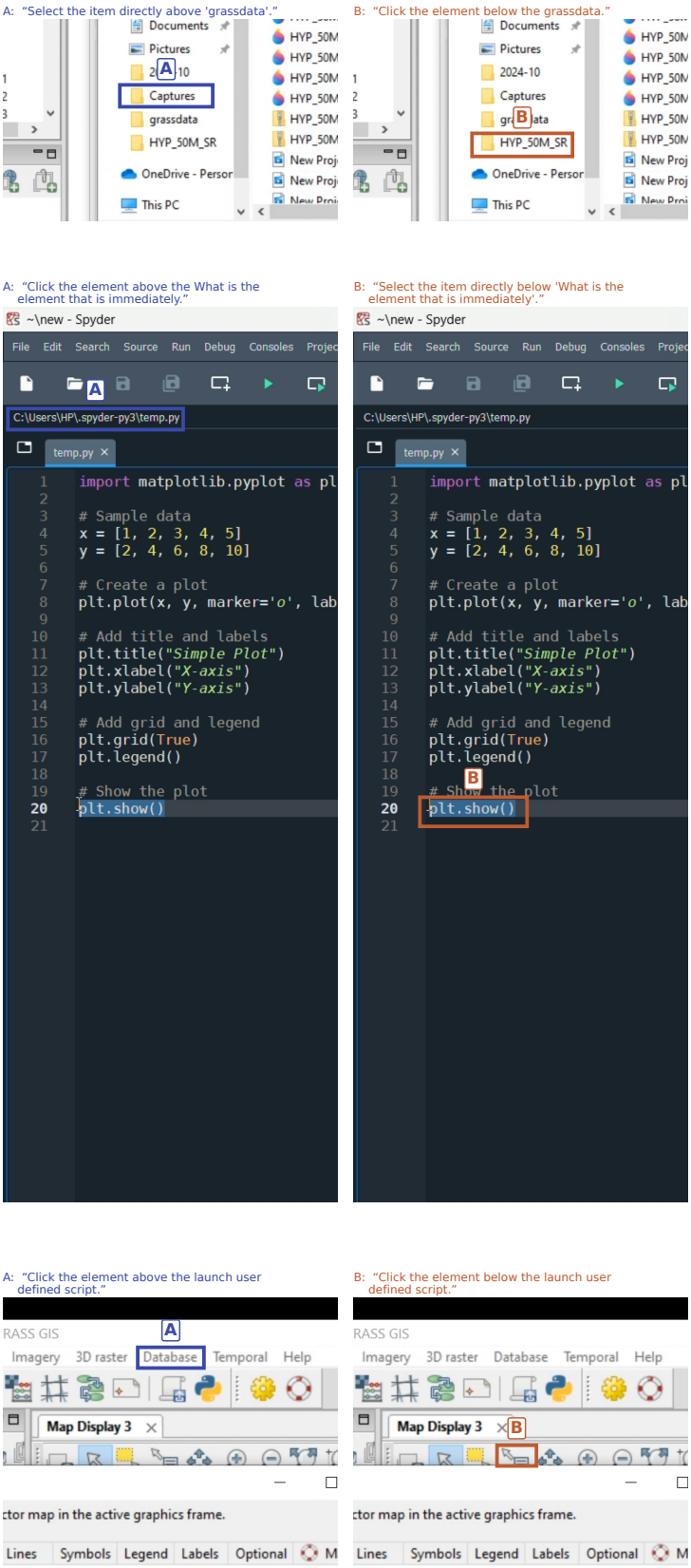

Figure 15: **`rel-pos-vertical` (real screenshots).** Three minimal pairs; *above* vs. *below* the anchor element across three different real GUIs.

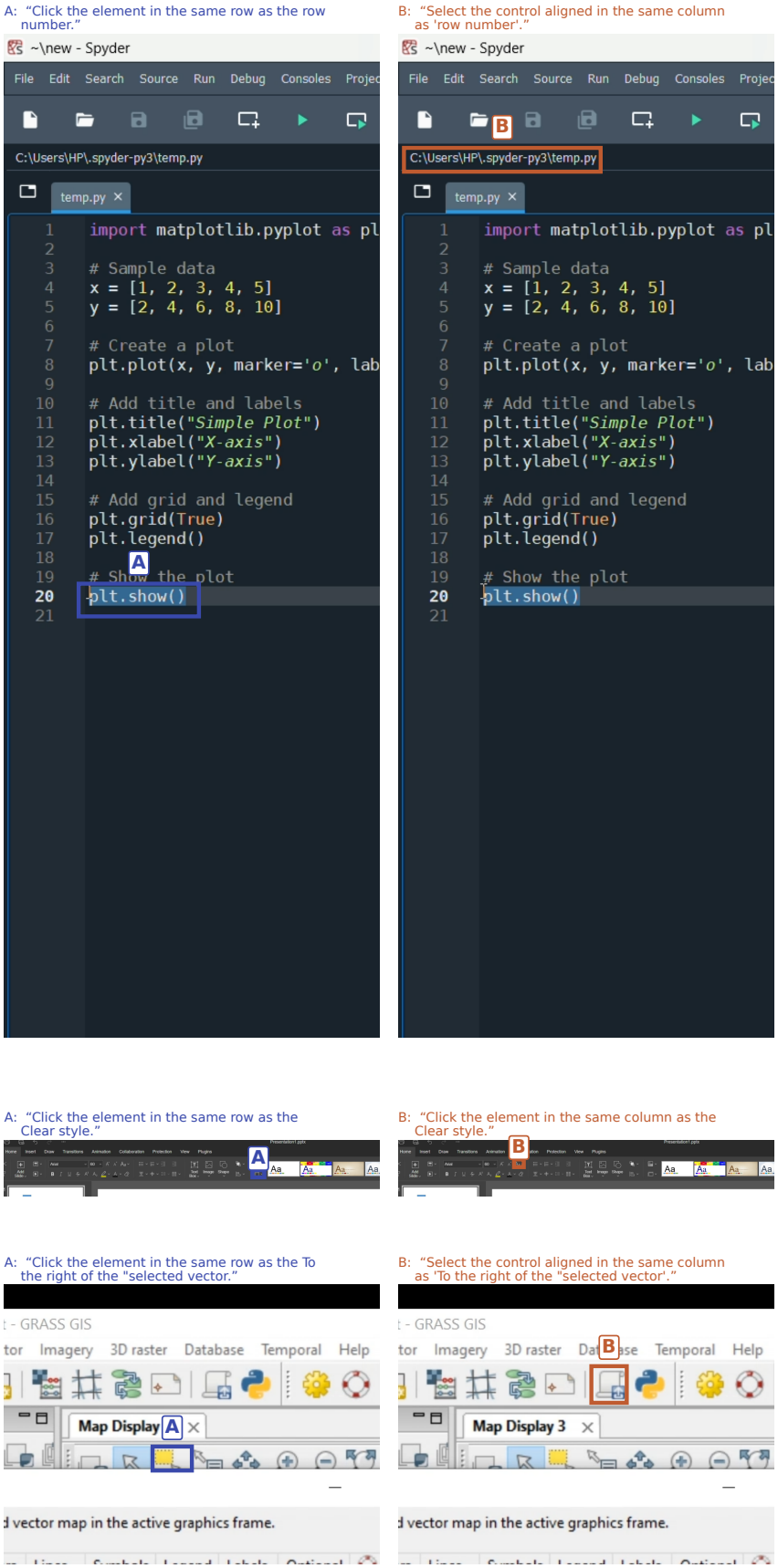

Figure 16: **`alignment` (real screenshots).** Three minimal pairs in which the relation switches between *same row as* and *same column as* the anchor.

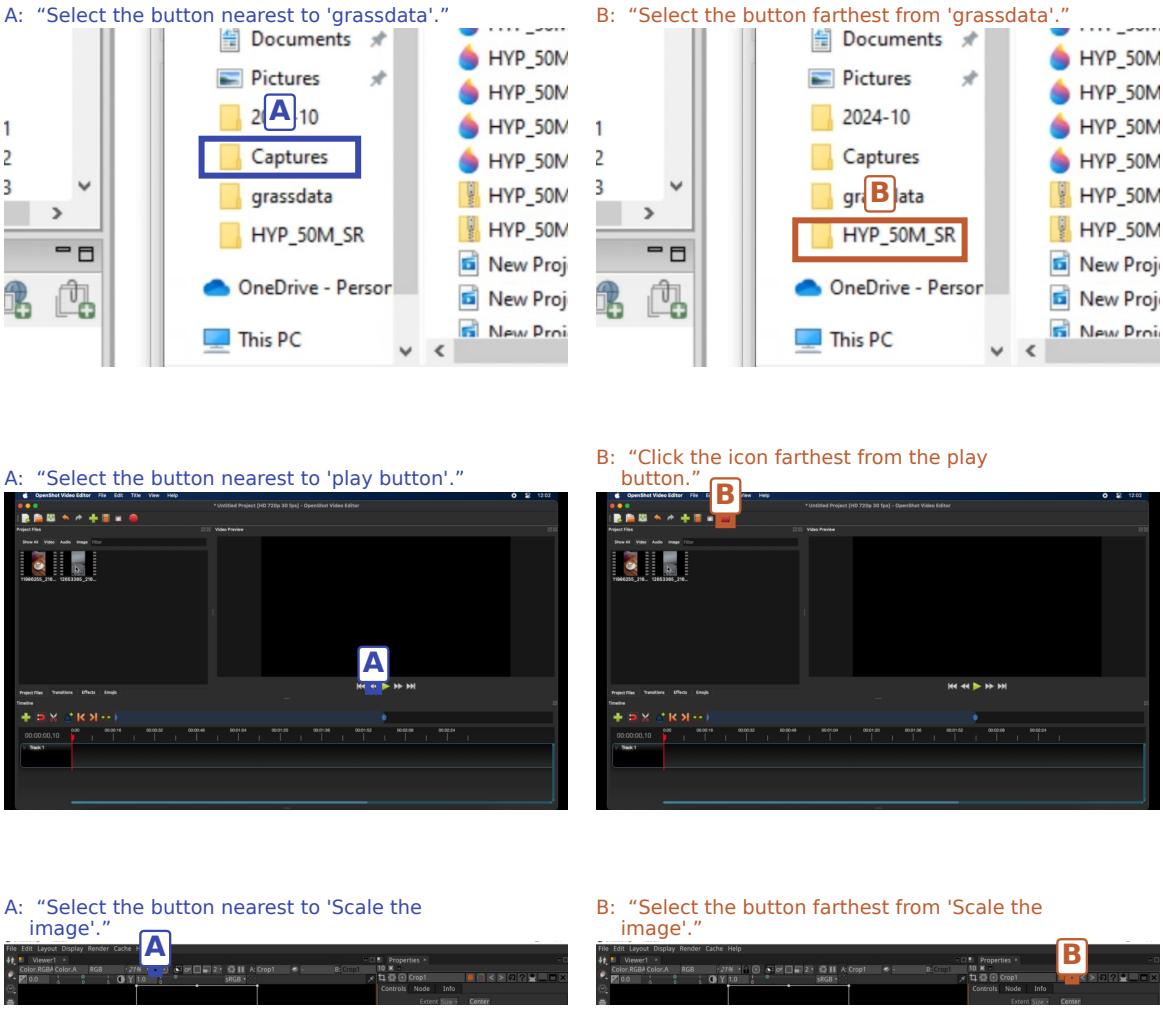

Figure 17: **`proximity` (real screenshots).** Three minimal pairs in which the two correct answers are the elements *nearest to* and *farthest from* the same anchor across three different real applications.

Figure 18: **`containment` (synthetic).** Three minimal pairs. Twin A is the *contained within* a parent; twin B is the parent *containing* that inner element. Synthetic stimuli are used because clean parent–child annotations are rare in real screenshot corpora.

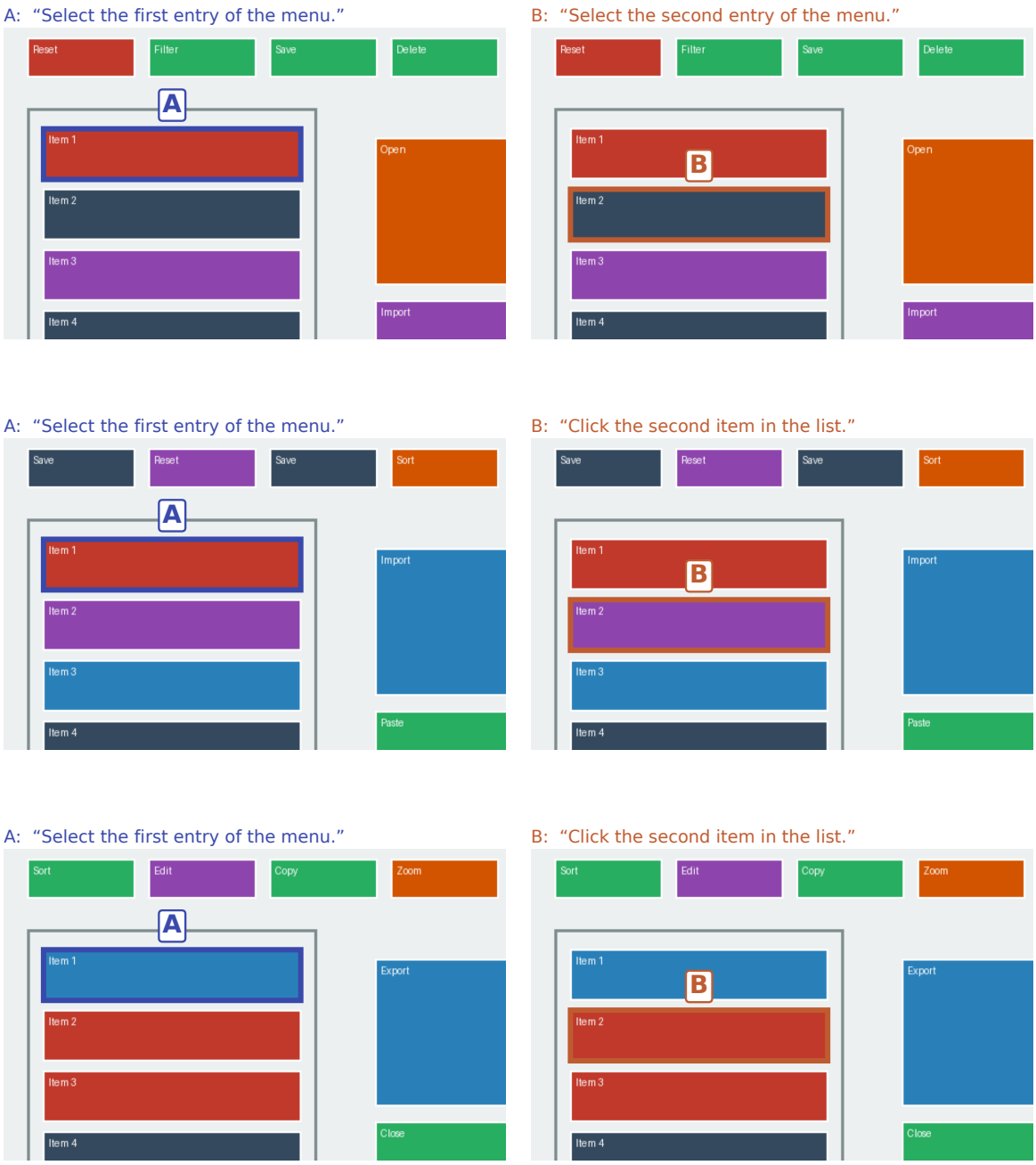

Figure 19: **`list-ordinal` (synthetic).** Three minimal pairs of symmetric ordinal queries (the k-th item from the top vs. from the bottom). The only primitive every top-tier model reliably solves.

Figure 20: **`occlusion` (synthetic).** Three minimal pairs. Twin A is the element that is *fully visible*, twin B is the element that is *partly hidden* behind an overlay. A relational-shortcut model is drawn to the visually unusual (occluded) element on both twins, producing below-chance accuracy on this primitive.

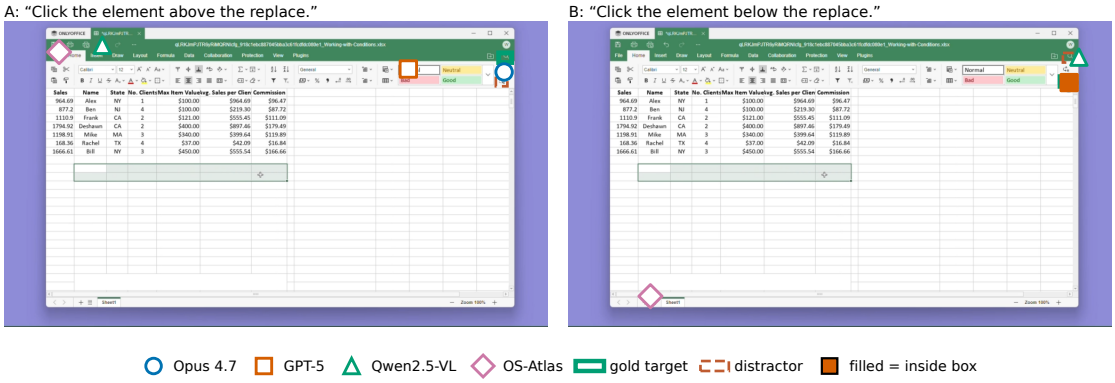

Figure 21: **Case study 1: `rel-pos-vertical`, real screenshot.** A: “Click the element above the replace.” B: “Click the element below the replace.” Opus and GPT-5 collapse to the same coordinate on both twins (both near ($\approx 1180, 130$)), getting B right by luck of which side they defaulted to and missing A. Qwen2.5-VL emits two completely unrelated clicks. Hollow markers = misses; filled markers = hits. Anchor-collapse failure.

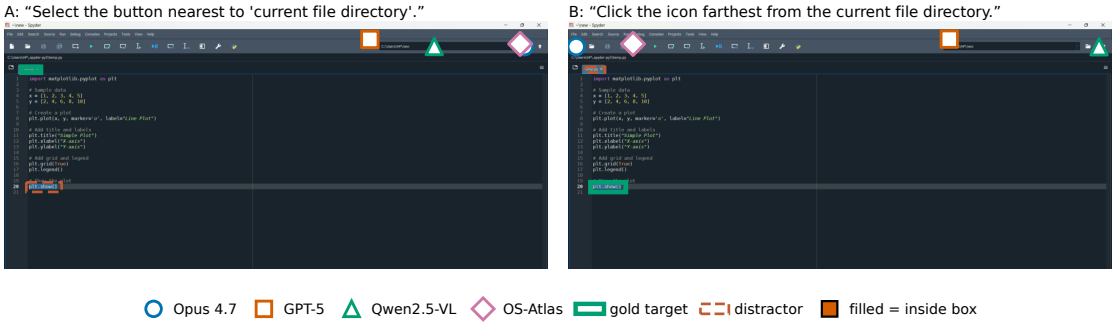

Figure 22: **Case study 2: `proximity`, real screenshot.** A: “Select the button *nearest* to ‘current file directory’.” B: “Click the icon *farthest* from the current file directory.” All four models cluster their predictions in the top toolbar regardless of the instruction. The anchor (a sidebar element) and the two targets (sidebar items) are far from the predictions. This is the population-level pattern on `proximity`: every model in our roster sits at or below chance.

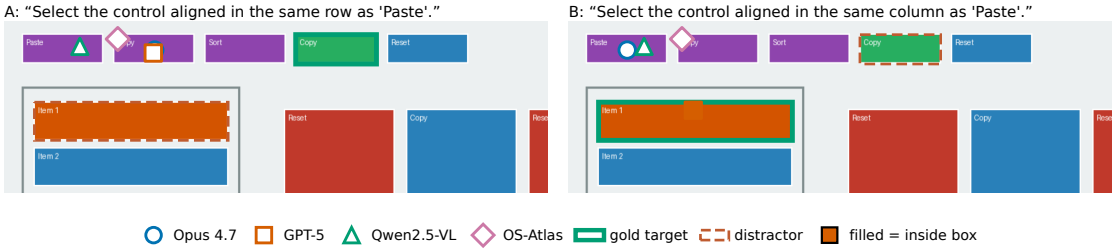

Figure 23: **Case study 3: `alignment`, synthetic.** A: “Select the control aligned in the same *row* as ‘Paste’.” B: “Select the control aligned in the same *column* as ‘Paste’.” Models click *the Paste anchor itself* or a visually similar icon, instead of the aligned neighbor. Qwen2.5-VL emits an *identical coordinate* for both twins, the canonical “relation-ignored” signature.

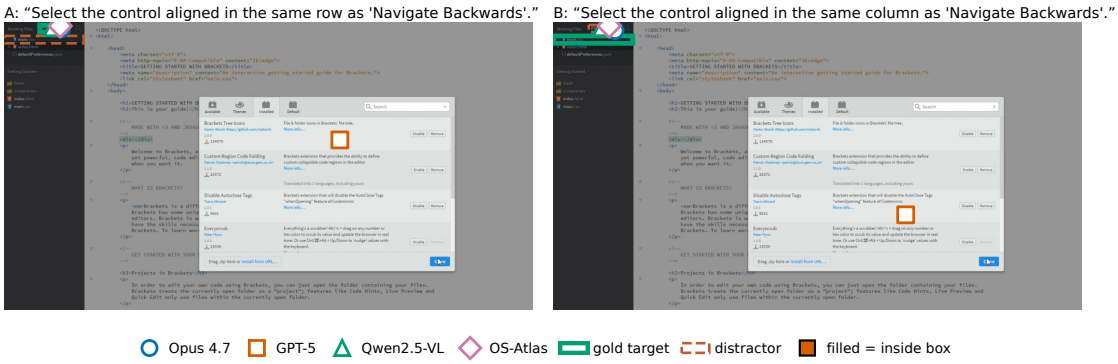

Figure 24: **Case study 4: `alignment`, real screenshot.** A/B: “aligned in the same row/column as ‘Navigate Backward’.” The real-screenshot version of case study 3. GPT-5 wanders far from the anchor on both twins. Qwen2.5-VL again emits the same coordinate for both. `alignment` on real GUIs is the hardest cell in the benchmark.

Figure 25: **Case study 5: `occlusion`, synthetic.** A: the ‘Reset’ button that is *fully visible*. B: the ‘Reset’ button that is *partly hidden* behind a dialog. All four models point to the partly-hidden button on *both* twins (paired predictions differ by <10 px). The models are drawn to the visually unusual element and never report the visible one, a below-chance bias toward the contrastive option.

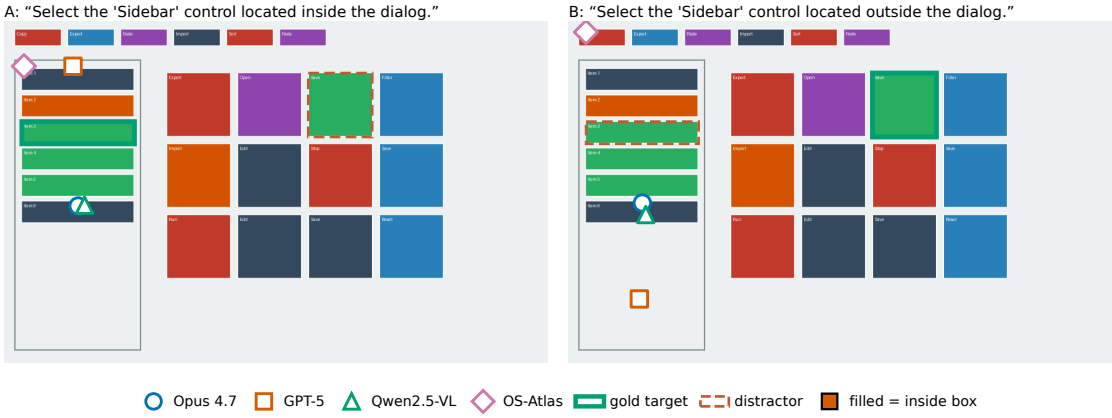

Figure 26: **Case study 6: `containment`.** A: target is the element *contained within* the parent. B: target is the parent *containing* the inner element. Models repeatedly click the inner element on both twins, missing the parent on twin B. `containment` is one of the four primitives whose upper bootstrap bound sits below 0.50 across the entire model roster.

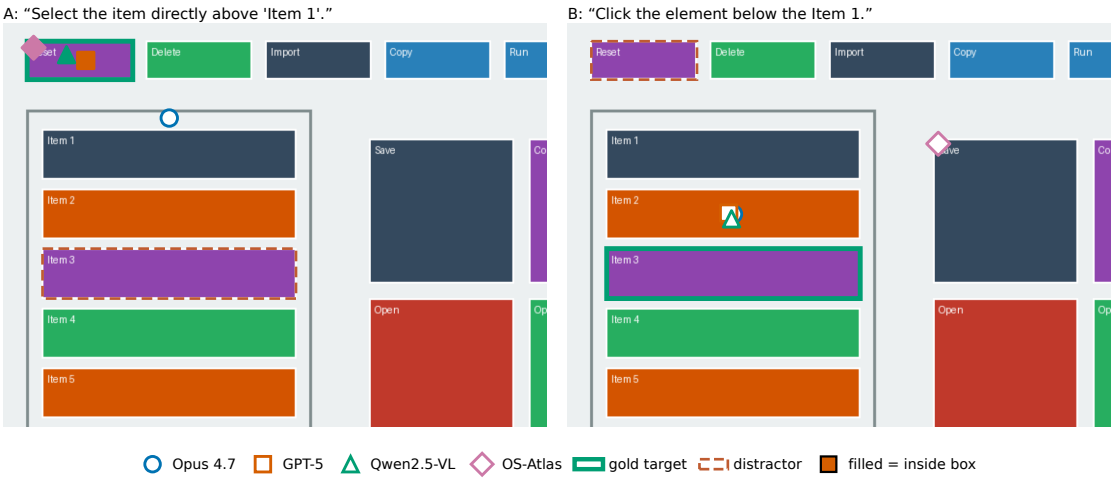

Figure 27: **Case study 7: `rel-pos-vertical`, synthetic.** The controlled-stimulus version of case study 1. Even on the clean, synthetic layout, the same anchor-collapse signature is visible: predictions cluster near the anchor instead of resolving above/below.

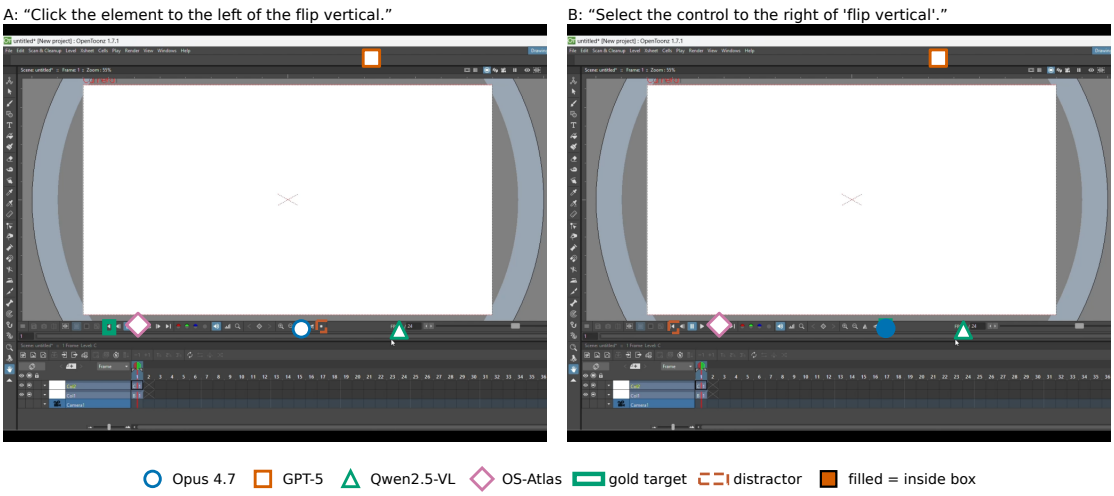

Figure 28: **Case study 8: `rel-pos-horizontal`, real screenshot.** A: “element to the right of *X*”. B: “element to the left of *X*”. The minimal-pair construction that opens the paper (Fig. 2) repeats here on a different application. Same shortcut, same failure mode.

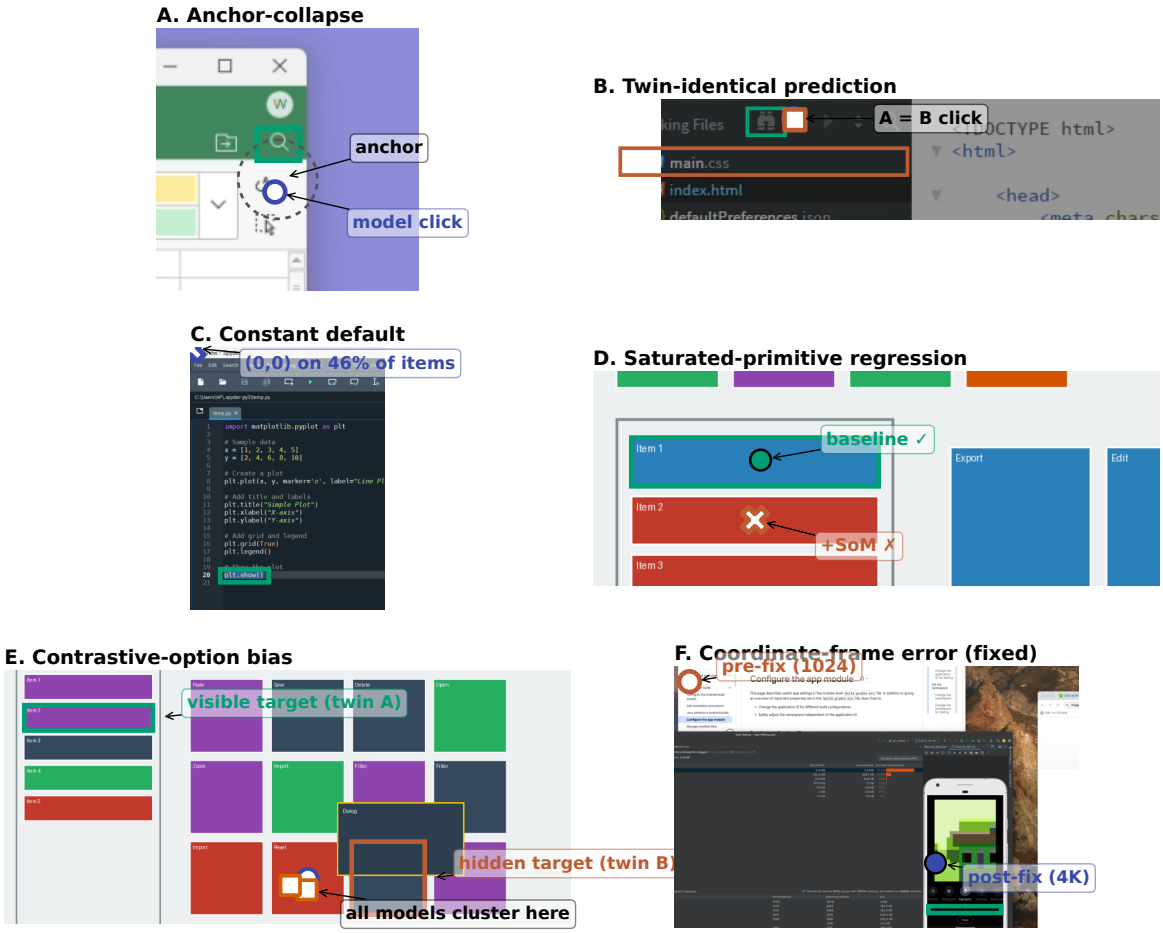

Figure 29: **Error-type cheatsheet: one real example per error type, drawn from the actual prediction JSONLs.** Right-of-marker labels sit in 50%-transparent boxes so the underlying screenshot remains visible. **(A) Anchor-collapse**: *click lands on the anchor noun, not the relational neighbor.* Opus 4.7's prediction on `ui_vision-199-0` ("click the element above the replace") falls inside the dashed "anchor" zone (the "replace" button), not the green target above it. **(B) Twin-identical prediction**: *same click for both twins → relation word ignored.* Qwen2.5-VL emits the same click on the two alignment twins of pair 81; both targets are shown for context. **(C) Constant default**: *model defaults to a canonical coordinate.* Llama-3.2-V emits literal $(0, 0)$ on $\sim$46% of diagnostic items; the prediction marker sits in the upper-left corner regardless of where the target is. **(D) Saturated-primitive regression**: *intervention hurts a primitive the model already solved.* On `list-ordinal` the green-filled marker shows that Qwen2.5-VL's baseline answer was correct, but the SoM adds an indirection and the same model misses (red $\times$). **(E) Contrastive-option bias**: *models pick the contrastive target on both twins, driving binary primitives below chance.* On the `occlusion` pair, both Opus (circles) and GPT-5 (squares) cluster on the partly-hidden target on *both* twins, ignoring the visible target. **(F) Coordinate-frame error (now fixed)**: *API returns coords in a downsampled frame.* GPT-5's pre-fix ScreenSpot-Pro prediction (terra hollow circle) lands in a 1024-side frame far from the 4K target; after our pre-resize + scale-back wrapper, the post-fix prediction (indigo filled circle) sits near the gold box.