Abstain and Validate: A Dual-LLM Policy for Reducing Noise in Agentic Program Repair

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Abstract

Agentic Automated Program Repair (APR) is increasingly tackling complex, repository-level bugs in industry, but ultimately agentgenerated patches still need to be reviewed by a human before committing them to ensure they address the bug. Showing unlikely patches to developers can lead to substantial noise, wasting valuable developer time and eroding trust in automated code changes. We introduce two complementary LLM-based policies to reduce such noise: bug abstention and patch validation policies. Bug abstention excludes bugs that the agentic APR system is unlikely to fix. Patch validation rejects patches that are unlikely to be a good fix for the given bug. We evaluate both policies on three sets of bugs from Google's codebase, and their candidate patches generated by an internal agentic APR system. On a set of 174 human-reported bugs, removing bugs and patch trajectories rejected by our policies can raise success rates by up to 13 percentage points and 15 percentage points, respectively, and by up to 39 percentage points in combination. On null pointer exceptions and sanitizer-reported bugs with machine-generated bug reports, patch validation also improves average single-sample success rates. This two-policy approach provides a practical path to the reliable, industrial-scale deployment of agentic APR systems.

1 Introduction

A substantial portion of software development involves diagnosing and repairing bugs. To mitigate this cost, the research community has long invested in Automated Program Repair (APR) [16–18, 23]. More recently, advanced language models have enabled more complex, autonomous "repository-level" program repair, where a system manages diagnosis, localization, and repair. Systems like SWE-Agent [35], AutoCodeRover [36], SpecRover [28], and RepairAgent [3] show that agent-based APR can tackle such challenge, achieving pass@1 bug resolution rates up to 75.20% on benchmarks like SWE-Bench-Verified (TRAE as of 09/02/2025 [11]).

In an industry setting, however, maximizing the rate of successful patches shown to a developer from an APR system is as important as maximizing the system's overall bug resolution rate. While pass@1 rates are improving, deploying these agents in production still requires a human developer to review and validate the generated patches. Because developer time is limited, reviewing patches for intractable bugs or unlikely fixes wastes valuable time. Reducing such noise from unproductive APR attempts is thus crucial, not only to save developer time but also to build developer trust in automated

code changes, which is essential for AI tool adoption [4, 24]. This work aims to tackle this challenge by proposing two policies.

First, inspired by abstention in classification [6, 12, 13], we introduce bug abstention (Section 3.1). We use an LLM to predict if our APR system is likely to produce an acceptable patch for a given bug, using only the bug report information. This check is performed before the agent attempts a repair. If the predicted probability of success is below a threshold, we abstain from running the repair agent. This avoids showing developers unlikely patches and saves compute costs from futile repair attempts.

Second, for any patches produced, we employ a multi-stage patch validation policy to predict if the patch is correct (Section 3.2). This policy composes deterministic checks (e.g., running tests) and an LLM-based filter. The LLM filter first generates a "fix specification" —a guideline for a correct fix—based on the bug description and original source files, without knowledge of any ground-truth solution. A second LLM call uses this specification and the agent's trajectory to produce a boolean (fixed or not) outcome, an explanation, and a confidence score derived from token-level log probabilities.

We evaluate the bug abstention and patch validation policies both independently and in combination (Section 4). Our combined, two-policy approach first employs bug abstention to screen out bugs unlikely to be fixed, then runs the APR system on the remaining bugs, and finally uses patch validation to filter the generated patches. Our evaluation shows these policies are complementary, improving success rates most effectively when combined.

We evaluate our policies on three sets of bugs from Google's codebase (Section 5). Similar to recent work [14, 20, 27], the first set has 174 human-reported bugs with known ground-truth fixes and fail-to-pass reproduction tests. We evaluate both policies on repair trajectories generated by our ReAct-style APR system for this set. We further study patch validation on 198 machine-reported Java null pointer exception (NPE) bugs from a live deployment and 50 historical, sanitizer-reported bugs with known reproduction tests.

Because our goal is to reduce developer noise, we introduce "filtered-success@k". This metric computes success (e.g., fail-to-pass or human acceptance) only on the subset of patches and bugs that are "allowed" by our policies, rather than over the entire population.

On our human-reported bug set, the baseline fail-to-pass@1 rate is 0.11. Applying bug abstention or patch validation alone raises this rate to 0.21 or 0.29, respectively. When combined, these policies form a configurable funnel that significantly improves the quality of patches shown to developers. A moderately permissive setting raises the filtered fail-to-pass@1 to 0.35. A stricter funnel configuration raises this rate to 0.53 - a = 1 - a - a = 1 - a - a = 1 - a

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Figure 1: Bug abstention rejects issues that are unlikely to be fixed by our APR system. After the agent generates patches, validation decides whether these are likely to have addressed the issue. Jointly they reduce developer noise from APR.

the cost of showing patches for fewer bugs. On machine-reported bug sets, patch validation alone also shows significant gains: for NPE bugs, it raises the filtered-accept@1 from 0.38 to 0.62, and for sanitizer bugs, it improves the rate by up to 15 percentage points over a baseline using known reproduction tests.

To summarize, our contributions in this work are:

- A bug abstention policy to select bugs suitable for agentic APR.
- A multi-stage patch validation policy to assess candidate patch correctness.
- An extensive evaluation on three industrial bug sets.

2 Background: ReAct-style Agentic APR

Our agentic APR system consists of an LLM-driven ReAct-style loop with coding tools designed to interact with an isolated Google environment [26]. At the start of the process, the agent is provided with a bug report, which may contain varying degrees of information about the underlying bug. The agent then proceeds for a sequence of up to N steps, where each step is defined as one LLM output and an associated tool execution. The tools are: code search, viewing a file, editing a file, running tests, and finishing the repair trajectory. The LLM output consists of two portions: a thought, describing the agent's intentions in natural language, and an action, a simple Unix-style function calling format which we parse and then use to execute the associated tool *. The LLM output and environment responses are added to an append-only history. For more details on the underlying agent, we refer the interested reader to [27].

3 Abstention and Validation Policies

We introduce two key components into an agentic APR system to reduce developer noise. In Figure 1, we augment the core APR Agent module (in blue) with a component before the repair loop (Bug Abstention) and a component after the repair loop (Patch Validation). The bug abstention policy filters out bugs that the APR agent is unlikely to fix, while the patch validation policy filters out patches that are unlikely to be a correct fix for a given bug. We now describe these policies in more detail.

3.1 Bug Abstention Policy

Abstention is a well-known technique in classification [8], and amounts to allowing a *K*-label classifier to produce an extra label

corresponding to "unknown" (i.e. abstaining from making a decision). Analogously, in the repair setting, we say a system abstains from a bug if it decides that it will not attempt repair.

Importantly, the abstention policy can only access the bug, which comprises textual bug title, description, and accompanying metadata, to make its decision; it does not receive any additional human feedback. We also choose not to provide codebase access to the abstention policy, which may otherwise require making the policy agentic and losing some efficiency gains.

We choose to model this problem in a probabilistic fashion to produce a score, rather than a boolean decision. Intuitively, we want this score to approximate the inherent probability of a single repair attempt succeeding. We then convert this into a decision by comparing it to a configurable threshold, τ .

3.1.1 Formalization. Let $b \in \mathcal{B}$ be a bug in a set of bugs that has been provided to our repair system as they require code changes for their resolution. We assume that knowing that these bugs require explicit code changes is already determined in a pre-filtering phase or because a user has informed the system. Let there be n independent Bernoulli trials reflecting n independent repair attempts by the repair agent on b. The outcome of the i-th trial for bug b, $X_{b,i}$, is 1 if the agent generates a successful patch in trial i and 0 otherwise.

Accordingly, we define the **empirical probability of the agent's success on bug** b as $\hat{\pi}_b = \frac{1}{n} \sum_{i=1}^n X_{b,i}$, or the expected value of a repair trial.

This empirical value $\hat{\pi}_b$ is an observation-based estimate of the **unknown, true probability of success for bug** b, with respect to a particular repair system, which we denote as $\pi_b = P(X_{b,i} = 1)$. Estimating $\hat{\pi}_b$ requires a success criterion, such as a test oracle or human reviewer. During deployment, however, such criterion is either unavailable (no test), or renders the estimation unnecessary (manual review on all patches). Thus, for a given bug b, we leverage an abstention model P_a to produce $P_a(b)$, which then is used to estimate $\hat{\pi}_b$, a priori. We aim to turn $P_a(b)$ into a decision policy that allows us to filter down the original bug set \mathcal{B} to a new set \mathcal{B}' , such that $\mathcal{B}' \subseteq \mathcal{B}$ and $\mathbb{E}_{b' \in \mathcal{B}'}[\pi_{b'}] > \mathbb{E}_{b \in \mathcal{B}}[\pi_b]$, demonstrating a reduced probability of wasted repair attempts.

Explicitly, the abstention model's prediction can be turned into a decision policy through a threshold. Let this be expressed as a function, D_a , which maps the model's predicted success likelihood to an action from the set {AttemptRepair, Abstain}. Given a predefined threshold, $\tau \in [0, 1]$, the decision rule is formalized as:

 $[\]ensuremath{^*}\textsc{One}$ of our experiments relies on Gemini function calling instead of custom parsing.

$$D_a(P_a(b),\tau) = \begin{cases} \text{AttemptRepair} & \text{if } P_a(b) \geq \tau \\ \text{Abstain} & \text{if } P_a(b) < \tau \end{cases}$$

Our abstention policy queries an LLM to produce a prediction of the (unobserved) probability of an attempt successfully addressing a given bug. We use an instruction-style prompt and instruct the model to produce one of two possible outcomes, success (attempt repair) or failure (abstain), given a prompt with some representation r(b) of the given bug b. We define $P_a({\rm outcome})$ as the model predicted probability of that token. Since success and failure are the only two possible outputs based on the instructions, the LLM's decoding should place relatively little weight on other tokens and so $P_a({\rm success}|r(b)) + P_a({\rm failure}|r(b)) \approx 1$. We confirmed this property on a held-out set of bugs during our development.

- 3.1.2 Bug representations. As is well known, the prompt used to query an LLM can have a substantial impact on its performance [19, 31]. We describe two possible bug representations for our prompt.
 - r₁(b) Instruction Prompting with Title and Description. The LLM is provided with the title and description of from the bug report, along with the capabilities of the agent used in our experiments. Specifically, we provide information on the commands available to the agent, as well as the execution budget for the expected agent runs. The associated prompt (shown in the Appendix as Listing 1) instructs the model to analyze the bug's title and description and output only success or failure.
 - $r_2(b)$ Instruction Prompting Adding Guidelines. This representation builds on top of $r_1(b)$ by incorporating guidelines that can be used to determine the likelihood of successful bug resolution. We derived these guidelines —detailed in Listing 2 (Appendix)— through a manual process on a held-out set of 50 human-filed bugs. Four authors inspected the bug titles and descriptions, determined subjective difficulty levels, and provided short guideline that would help identifying bugs that could be resolved by reasonable attempts from an agent. One author collated the inspection outcomes into 5 basic guideline categories, such as "Clear Problem & Action" and "Precise Code Localization". These guidelines are added to the prompt.

3.2 Patch Validation Policy

Patch validation performs an analogous accept/reject decision at the patch-level. Specifically, it predicts whether a **particular** repair trajectory produced a successful patch for a specific bug.

3.2.1 Formalization. Let t be a particular trajectory from the repair agent when attempting to repair bug b. Given some representation of the trajectory, r(t), the policy predicts a triplet $P_v(b,r(t)) \in \{\text{false}, \text{true}\} \times \Sigma^* \times \mathbb{R}, \text{where the first element is a binary judgment for patch correctness, the second element is a natural language explanation of the judgment, and the third element is an optional confidence score for the patch's correctness. We then define a decision function over this triplet, which can use the binary judgment or the patch correctness confidence score to filter out patches and allow for the intelligent allocation of human effort, filtering out less likely patches as noise and reducing review time.$

In our implementation, we compute P_v using a multi-stage approach. We first perform build and test regression checks to remove patches that have known issues. Specifically, we use Google's unified build system to extract targets that have any modified files as a direct dependency. Alternatively, if the bug we are tackling has a known reproduction test, we replace this regression check with a filter that rejects any patches that fail to pass the reproduction test.

Next, our reviewer applies a simple heuristic which removes trajectories that have a final test command issued by the agent that results in a failure. Note that this may capture different test targets than the build/test check as the agent is not restricted to executing only direct dependencies. Finally, our validation employs a two-staged LLM prediction, which we now describe in detail.

3.2.2 Fix specification. Past literature in the area of LLMs-as-judges has explored the use of generated rubrics to perform more accurate scoring of an answer [32]. However, this rubric is often based on additional information such as the reference answer [37], which is naturally unavailable in our setting, where we are attempting to repair a program without knowing the fix ahead of time.

However, we do take inspiration from this line of research, and explore the LLM's ability to generate a set of criteria for determining if an agent attempt has successfully addressed a bug without access to any ground truth-we refer to this as a "fix specification." We simply provide this specification as part of the LLM judge prompt to produce a score for the patch. To generate the fix specification, we prompt an LLM with the bug information and the original source code contents of the files that were edited in the agent's candidate patch. Importantly, this connects the fix specification and the agent's attempt by re-using the localization information from the agent's trajectory to generate the fix specification. Note that this agent trajectory does not contain any information from the ground-truth fix-that is, the specification can only be derived from information known at the start of the repair attempt. We do not impose any format or structure on the generated specification and instead allow the LLM to freely generate this content. In practice, the model often generates clear Markdown with a diagnosis and list of expected properties of a correct change.

3.2.3 Reviewing with a specification. Once the LLM has generated a fix specification, a second LLM call judges the agent's attempt. This prompt includes the bug information, the unified-diff-format patch produced from the agent's trajectory, test result information (extracted from the trajectory) for any test targets run by the agent, and the generated fix specification.

A test result consists of a numbered test attempt for a particular test suite target, and the associated environment response (including logs for a failure). If no test result information is found, we simply state "No testing information is available".

- 3.2.4 Reviewing without a specification. Besides our specification-based validation, we experiment with a variant of validation that consumes the bug description and the concatenation of all prompt messages and responses that were produced during the agent's execution. Recent work [25] has used this representation to judge an agent's attempts to solve SWE-Bench tasks.
- 3.2.5 Patch validation outputs. As described previously, the output of the validation policy is a triplet consisting of a judgment, a

natural-language explanation, and a real-valued confidence score. To enforce this format, we employ constrained decoding [10, 29].

We define the patch correctness confidence score for the response heuristically. If validation returns a negative binary judgment, this score is set to zero. If the validation returns a positive binary judgment, we set this score to the exponential of the average token log probabilities. While not a well-defined probability, our evaluation shows this score can improve performance over binary labels.

3.2.6 Decisions over patch validation outputs. We can make decisions based on the binary judgment or the patch correctness confidence score extracted from the validation outputs. We consider two variants (1) **binary**, where we use the binary judgment and accept patches with a positive label; (2) **percentile of patch correctness confidence score**, where we estimate a particular percentile cutoff across all patches for all bugs in the given set and then accept patches with a score at or above that cutoff. While percentile thresholding needs a set of patches to perform a judgment, it mitigates the risks of score drift, e.g. due to population or model changes, and closely aligns with the idea of budget-based reviewing (i.e., a human reviews the top $\leq k$ patches).

4 Composing Abstention and Validation

While abstention and validation policies can be used independently, these two policies can also be combined. We present and evaluate a simple sequential combination of the two policies.

Let R represent our repair system, which, given a b bug, generates multiple trajectories independently. Each trajectory t has a patch property representing all changes made by the agent. Let $P_a(b)$ be the probability predicted by the abstention policy for sampling a successful fix for bug b. Let $P_v(b,t)$ be the patch validation policy output. Let τ_a be the abstention threshold and D_v be the decision function over the validation's output. Then the set of patches ultimately shown for human review is: $\{t.\text{patch}|P_a(b) \geq \tau_a, t \in R(b), D_v(P_v(b,t))\}$. We leave alternate compositions (e.g., probability-based) or further optimizations (e.g., accounting for bug/review cost and fix value) for future work.

5 Evaluation

We now describe our datasets, metrics, and research questions.

5.1 Datasets

We use three industrial bug datasets (Table 1) for evaluation. Bug abstention only applies to human-reported bugs. Machine-reported bugs (i.e., NPE and sanitizer bugs) are all good candidates for APR by construction, as they contain reproduction tests or stack traces.

5.1.1 Human-reported bugs: From Google's monorepo, we collected a set of 174 human-reported bugs filed within a year by following a curation process similar to that for popular benchmarks like SWE-Bench and recent agentic APR work in industry [20, 27]. Specifically, we adhere to the curation process detailed in [27] and additionally restrict examples to those with at most 31 test targets that depend on the associated ground truth files. Bug curation steps detailed in Appendix A.

We then generated 20 trajectories with a 25-step limit for each of the 174 bugs using our agentic APR system (Section 2) using Gemini 2.5 Pro [7] with temperature 0.2 and top_p 0.95.

We consider the success criterion to be whether the held-out fail-to-pass test succeeds. We have found this criterion closely matches correctness in human-reported bugs from our codebase [27].

5.1.2 Null pointer exception (NPE) bugs. We collected a set of 198 machine-reported NPE bugs that our agent attempted to repair during a live deployment. Every bug report includes a stack trace of the exception. The report may also include a section (message blob) that provides additional machine-generated debugging information.

During deployment, our system used patch validation with fix specification to filter patches, normalize patches, and choose a single patch per bug based on majority voting. The normalization step includes normalizing identifier strings (mapping each unique likely identifier string to a unique index) and post-patch least-line-count tie-breakers. A patch was then sent for human review, and submitted to the codebase if a code owner accepted it as fix.

During live deployment, our APR system setup differed in the following: function-calling is through Gemini instead of custom ReAct parsing, asynchronous step execution to allow long-running calls without blocking concurrent repairs nor overloading services, at most 5 trajectories were generated with 25-step limit per bug.

5.1.3 Sanitizer bugs. We collected a set of 50 sanitizer-reported bugs with a known ground-truth fix following the curation procedure described in our past work [27]. These bugs cover issues such as memory errors, data races, datatype overflows, typecasting errors, and fuzzer-detected errors. We use the same APR agent configuration as for our human-reported bug set.

5.2 Metrics

We report precision and recall for both policies. For repair performance, we focus on a standard pass@k metric, and a variant where bugs and patches are filtered by the corresponding policies. Let \mathcal{B}' be the set of bugs with at least one repair run/trajectory available after bug abstention has filtered bugs and patch validation has filtered patches, n' be the total number of runs remaining after applying patch validation, and c' be the successful count among those. Then the filtered-success@k metric is defined as:

$$\text{filtered-success@} k = \mathbb{E}_{\mathcal{B}'} \big[1 - \frac{\binom{n' - c'}{k}}{\binom{n'}{k}} \big]$$

The success criterion differs by bug types. For human-reported bugs, we use the held-out ground-truth test to compute fail-to-pass and report filtered-fail-to-pass@k. For NPE bugs, which were open at the time of collection and may not have any tests, we use human acceptance annotations and report filtered-accept@k. The annotations are from real code owners, or authors if the patch had not been reviewed by owners during live deployment, e.g., a patch not picked by majority voting (§5.1.2). For sanitizer bugs, which have known ground-truth fixes and a reproduction test at reporting time for filtering patches, we manually compare patches to ground-truth fixes for semantic equivalence, and report filtered-accept@k.

For both bug policies, we also include comparisons to a no-skill policy which always accepts bugs (or patches, respectively). Under

Set	Description	#	Policies	Success Cri- teria
Human	Human- reported bugs with known fix.	174	Abstention +Validation	Passes held- out reproduc- tion test.
NPE	Open bugs from a live deployment of a repair system.	198	Validation	Confirmed correct by code owner or manual inspection.
Sanitizer	Sanitizer- reported bugs with known fix.	50	Validation	Confirmed correct by manual inspection.

Table 1: Benchmarks, policies applied, and success criteria.

this no-skill policy, the precision of predictions corresponds to the prevalence successful cases in the population.

5.3 Research Questions

We aim to answer the following four research questions:

RQ1: Can bug abstention successfully abstain on our human-reported bugs? Does this improve filtered-fail-to-pass@k?

RQ2: Can patch validation successfully predict if a patch should be rejected on our human-reported bugs? Does this improve filtered-fail-to-pass@k?

RQ3: Does combining bug abstention and patch validation further improve filtered-fail-to-pass@k on our human-reported bugs?

RQ4: How does patch validation improve filtered-accept@k on bugs with systematically-generated reports (NPE and sanitizer bugs) and known reproduction tests (sanitizer bugs)?

6 Results

We now present our experimental results. At the time of these experiments, our abstention policy uses Gemini 2.0 Flash (newer models are now available) patch validation policy uses Gemini 2.5 Pro, and both perform greedy decoding (temperature=0). We expect to use a smaller model for abstention for efficiency as it runs on a larger number of inputs than patch validation.

6.1 RQ1: Bug Abstention Policy

We evaluate bug abstention on the two bug representation variants: with and without the use of general guidelines (Section 3.1.2).

Figure 2 shows a comparison of model-predicted probabilities and the empirical success rate (Section 3.1.1) of repair runs for the 174 human-reported bugs. The empirical success rate (green) is the fraction of trajectories that have a passing outcome on the held-out reproduction test, which indicates the inherent difficulty of APR. As shown, it is heavily skewed, with the vast majority of bugs having a very low true success rate, concentrated in the 0–15% range. The skewness confirms that an agentic APR system can benefit from an abstention mechanism to avoid wasting human review effort.

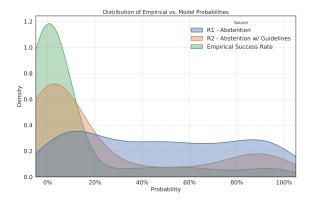


Figure 2: Using abstention with guidelines better aligns model probabilities with observed (left-skewed) rates.

Abstention with representation r_1 (i.e., without guidelines) produces a more uniform distribution of repair success probability. In contrast, abstention with r_2 (i.e., with guidelines) produces a distribution that much more aligns to that of the empirical success rate. We further measured the Wasserstein distance [1] between each predicted distribution and the empirical success rate distribution: the distance from r_2 (0.126) was nearly three times smaller than that from r_1 (0.363), indicating that r_2 is a much better overall fit for bug abstention. Like the empirical data, abstention predictions with r_2 are correctly concentrated at lower probability values, while still assigning a higher repair success probability to easier bugs.

Figure 3 presents precision-recall curves for both abstention policy variants, a horizontal line for the prevalence of a success, and a summary average precision (AP) score. For purposes of this classification, we define success as any bug that has at least one repair trajectory that passes the held-out reproduction test among the 20 trajectories of the bug. The prevalence of success corresponds to the precision that could be expected from a no-skill policy, which just accepts all bugs, and all patches for these bugs. For our human-reported bug set, this prevalence corresponds to 27% of all bugs having at least one successful repair trajectory without any abstention filtering. We observe that both abstention policy variants can improve over prevalence; adding guidelines further provides improvements across a range of recall values.

To turn the abstention predictions into a decision policy, we select $\tau=0.5$ as a sensible default for the abstention policy threshold based on a separate development bug set. Using this threshold produces a precision/recall of 0.44/0.41 in that set. This threshold is also intuitive, as it prioritizes bugs with a predicted success probability greater than failure. More importantly, it achieves a strong balance between precision and recall, effectively filtering out a large number of bugs unlikely to be fixed while retaining a significant portion of the fixable ones.

Figure 4 shows the pass@k (when abstension is not applied) and the corresponding filtered-fail-to-pass@k (when abstention is applied) of our agentic APR system on the set of 174 human-reported bugs. We use $\tau=0.5$ when applying abstention. Applying abstention improves the system's efficacy, and the r_2 abstention variant demonstrates the most significant gain. The baseline (no

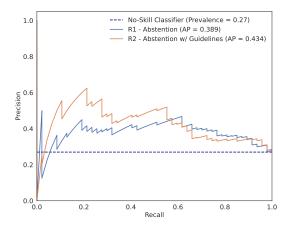


Figure 3: Both abstention variants outperform randomly sampling to identify bugs with at least one successful repair. Adding guidelines helps. (AP=average precision)

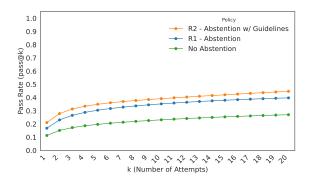


Figure 4: Abstention can raise fail-to-pass@k by removing bugs that are less likely to succeed with our APR system.

abstention) achieves pass@1 of 11.29%. By filtering bugs with the r_1 abstention policy, the filtered-fail-to-pass@1 rate increases to 16.81%. The r_2 abstention achieves an even higher filtered-fail-to-pass@1 of 21.05%. This trend of improvement continues as more repair attempts are made: when k=20, (filtered-fail-to-)pass@k climbs from 27.01% (no abstention) to 44.74% (r_2 abstention). If we consider having at least one passing trajectory in 20 as a ground-truth positive label for a bug and an abstention probability above our threshold as a predicted positive label, our abstention policy results in a 16.5% false positive rate, defined as a ratio of false positives to ground-truth negatives. These results strongly suggest that abstention effectively discards bugs with a low fix probability, allowing the APR system to concentrate on more-promising bugs.

6.2 RQ2: Patch Validation Policy

We compare three patch validation policy variants: (1) *no-skill policy*, i.e., prevalence, which reports the fraction of successful trajectories after filtering patches that fail build and test regression checks, (2)

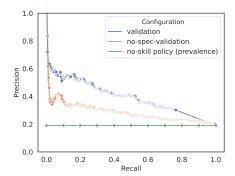


Figure 5: Patch correctness score allows more aggressive validation, with precision up to $3\times$ the success prevalence.

validation that uses fix specification (Section 3.2.3), and (3) no-specvalidation that does not use fix specification and reviews the agent's whole trajectory (Section 3.2.4).

To determine the build and test regression checks, we extract from Google's unified build system any targets that have a modified file as a direct dependency. We retrieve this direct dependency by identifying the build system rules that include the modified file in their definitions. Given Google's monorepo, transitive dependencies can quickly grow, and so we use this restricted set of rules as a practical approximation to identifying and removing regressions.

Our experiments show that while using fix specification improves both precision and recall over the ablation, the absolute precision remains modest at 0.3. The no-spec validation's binary judgment is heavily biased towards positive judgment, providing low recall of failing patches and prevalence-level precision for successful patches. The no-spec validation's bias towards positive judgment results in a high false positive rate (ratio of false positives to ground-truth negatives) of 0.64 compared to 0.28 for the variant that uses the fix specification.

We show that despite the modest predictive power of binary judgment, the validation's patch correctness score can be used to distinguish between predictions. As a reminder, we define this score as zero for negative binary judgments and as the exponential of the average log probabilities of the response tokens for positive binary judgments. Figure 5 shows precision-recall curves for both the validation and the no-spec validation, as well as a horizontal line for the no-skill policy after regression checks. At stricter thresholds, our spec-based validation (in blue) can yield a precision up to three times higher (at the cost of recall) than that of the prevalence rate, offering a substantial noise reduction for a subset of bugs. we find that the no-spec validation can improve over the no-skill policy but achieves lower precision at the same recall than the spec-based validation. With the 75th and 90th percentile cutoffs, the specbased validation achieves a false positive rate (FPR) of 0.13 and 0.04, respectively, while the no-spec validation achieves an FPR of 0.15 and 0.06, respectively.

We now study the impact of different decision procedures for patch validation with filtered-fail-to-pass@k. We evaluate two decision procedures described in Section 3.2.6 for validation. Specifically, one procedure uses binary judgment: validation-binary and no-spec-validation-binary. The other uses the 75th and 90th percentile thresholds of all patches' correctness scores: validation -p75 and validation-p90 (and similarly for no-spec-validation). Figure 6 compares the filtered fail-to-pass@k of these patch validation variants against build-and-test, which is a baseline that samples patches that pass build and test regression checks. The filtered fail-to-pass@k of build-and-test are 0.14 and 0.19 when k equals to 1 and 3, respectively. On no-spec validation, using binary judgment provides no improvement. However, using the 75th and 90th percentile thresholds of patch correctness scores, no-spec validation raises filtered fail-to-pass@1 to 0.16 and 0.22, respectively. Similarly, on spec-based validation, using 75th and 90th percentile thresholds of patch correctness scores better improve filtered fail-to-pass@1 than using binary judgment. Binary judgment only modestly improves filtered fail-to-pass@1 (0.16), while the 75th and 90th percentile thresholds improve filtered fail-to-pass@1 more substantially to 0.21 (+7 percentage points, +50% relative improvement) and 0.29 (+15 percentage points, +100% relative improvement), respectively. Overall, using an aggressive validation policy results in fewer bugs that have at least one patch that can be surfaced. However, given that our goal is to reduce developer noise, we prioritize increasing the likelihood of success for any patches that remain.

In Figure 6, filtered-fail-to-pass@k improves further as a validation variant with more aggressive filtering is applied. While this improvement trend matches our goal on reducing developer noise, it also presents a tradeoff occurring in patch validation between reducing developer noise (by discarding more agent trajectories) and increasing bug resolution rate.

To better understand this tradeoff, we compute "(pass & validation)@k", where at least one patch must both pass the reproduction test and satisfy the validation, and present the results in Figure 7. Figure 7 shows that: more aggressive filtering in validation reduces overall bug resolution rate. For example, (pass & validation)@1 decreases by 2, 4, and 7 percentage points for binary judgment, 75th percentile threshold judgment, and 90th percentile threshold judgment, respectively. In summary, when employing a validation policy, there is this tradeoff between wasted compute and developer attention to consider. The validation policy ultimately rejects many generated patches, which lowers overall bug resolution rates and leads to early flattening in Figure 6, but ensures the patches shown to developers are more likely to be plausible.

6.3 RQ3: Combined Abstention-and-Validation

We now evaluate a dual-policy framework that combines abstention and validation: it filters bugs via the abstention policy, then filters patches via the validation policy (Section 4). For evaluation, we set the abstention threshold to 0.5, rejecting any bugs with a predicted value below that. We use spec-based validation, and consider both binary and percentile-threshold judgments. Figure 8 compares (filtered-)fail-to-pass@k between the combined variants (combined-*), a policy that only applies abstention and excludes trajectories that produce no patches (abstention), and a baseline that applies neither abstention and validation (no-filter).

Combined abstention and binary validation can raise filtered failto-pass@1 to 0.29 over abstention alone (+5 percentage points) or

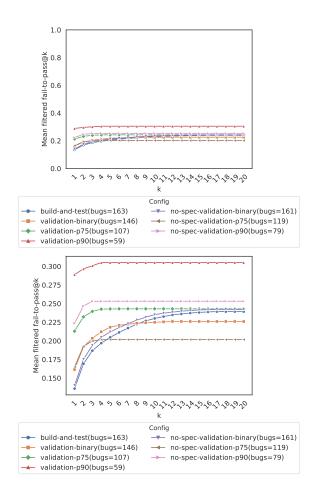


Figure 6: Validation can raise filtered-fail-to-pass@1 up to 15 percentage points compared to using only build and test regression checks. We show the same plot zoomed in for easy comparison. Parentheses show number of bugs with at least one patch after validation.

validation alone (+13 percentage points). More aggressive filtering, such as 75th percentile can increase filtered-fail-to-pass@1 to 0.35 (+11 percentage points relative to abstention alone and +14 to the same validation alone). These percentile thresholds create a configurable funnel for developer review. A strict policy (combined-p90) narrows the intake to just 12 bugs, but its output is high-confidence, with a filtered fail-to-pass@1 of 0.53 (a 1-in-2 success rate). A more permissive policy (combined-p75) widens the intake, doubling the number of bugs for inspection while still yielding a high success rate of roughly 1-in-3.

While it is not surprising that using validation can improve on bug-abstention-only success rates, as validation has access to more information (e.g., the agent-generated patch) and is judging a single instance, we also observe improvements over the validation-only version. This is interesting because patch validation—just like the bug abstention policy—has access to the bug report. We hypothesize, that given a patch, validation is more likely to be persuaded of the

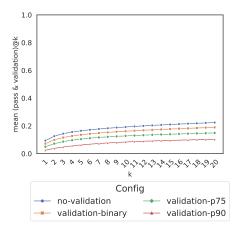


Figure 7: More aggressive filtering tradeoffs between reducing developer noise and increasing bug resolution rate.

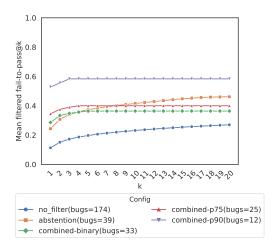


Figure 8: Abstention and validation are complementary, jointly they can raise filtered fail-to-pass@k.

success of the attempt, while the abstention policy is more likely to reflect overall quality of bug report.

Figure 9 visualizes the process of our dual-policy framework as a configurable funnel. The funnel presents the filtered fail-to-pass@1 rate on human-reported bugs at each stage. Starting with the full set of 174 bugs, where our APR system has a baseline pass@1 rate of 11.29%, the abstention policy first filters this down to 39 more promising bugs, increasing the filtered fail-to-pass@1 rate to 21.5%. The combined validation policies (combined-*) further refine this set. The combined-binary policy leaves 33 bugs with a rate of 29%. More aggressive filtering with combined-p75 and the strictest combined-p90 variant narrow the funnel to 25 and 12 bugs, respectively, boosting the quality of patches to achieve high filtered fail-to-pass@1 rates of 35% and 53%. Overall, Figure 9 demonstrates the trade-off between the volume of bugs attempted and the precision of the patches ultimately shown to developers.

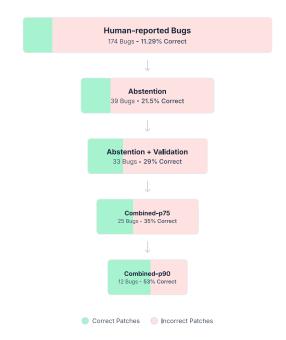


Figure 9: Abstention and Validation Funnel. Our combined policies filter an initial set of 174 bugs, progressively narrowing the set to increase the quality of patches shown to a developer. Each stage reduces the number of bugs but increases the filtered fail-to-pass@1 rate, from a baseline of 11.29% to 53% for the most selective configuration.

6.4 RQ4: Validation on NPE and sanitizer bugs

We further evaluate patch validation in bug categories that have more systematic information in their bug reports, and so are assumed good candidates for APR by construction (i.e., these bugs pass abstention by construction). We consider the machine-reported NPE and sanitizer bugs, whose bug reports always have a stack trace and reproduction test, respectively.

Figure 10 shows filtered-accept@k by manually inspecting the correctness of patches for NPE bugs, whose bug reports have stack traces. The baseline (build-and-test) filters patches that did not pass build and test regression checks. We observe that validation using binary judgment provide little improvement over these regression checks. However, validation using the more-aggressive percentile-threshold judgment can result in improvements to filtered-accept@1 of +8 percentage points and +24 percentage points, with 75th and 90th percentile thresholds, respectively. No-spec validation at 90th percentile produces a bigger filtered-accept@1 improvement than that at 75th percentile (+20 percentage points vs +24 percentage points relative to regression checks), but both of them perform worse than the spec-based variant at 90th percentile (i.e., validation-p90).

Figure 11 shows mean filtered-accept@k (based on manual inspection of patch correctness) for sanitizer bugs, whose bug reports have reproduction tests. As a result, we filter any patches first by the reproduction test and then apply the validation policy. The baseline (repro-test) filters patches that did not pass reproduction

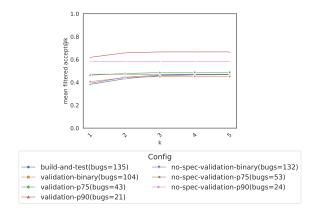


Figure 10: More aggressive validation can increase filtered accept@k for NPE bugs.

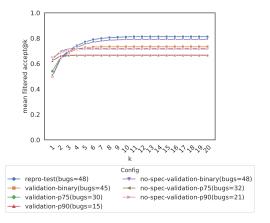


Figure 11: Aggressive validation increases filtered accept@1 for sanitizer bugs. But just reviewing more patches can lead to higher filtered-accept@k, with 4 patches sufficing.

tests. For sanitizer bugs, validation can lift filtered-accept@1 but increasing k quickly outstrips the validation policy. At k=4, sampling patches from the set that passes the reproduction test provides better results (0.74) than using the validation's filtering. When we consider k=1 and using 90th percentile threshold, no-spec variant no-spec-validation-p90 (0.65) outperforms the spec-based variant validation-p90 (0.62) and baseline repro-test (0.5).

We inspected whether patch validation performs differently based on the sanitizer bug type. Table 2 shows the lift for each sanitizer bug category, defined as the ratio of filtered-accept@1 to the raw accept@1, where per-bug accept@1 is averaged by sanitizer type. If a policy removes all patches associated with a sanitizer bug type or there are no patches (in the unfiltered set) that are correct, we enter NA in the table. We find that patch validation performance indeed varies by bug type. For the two main categories of bugs in our evaluation set—data races and use of uninitialized values—validation does not provide a meaningful improvement over just filtering with reproduction tests. For use of uninitialized value errors, adding the binary validation and 90th percentile validation

Sanitizer	# Bugs	repro-test	validation-binary	validation-p75	validation-p90
data_race	17	3.11	2.93	2.43	3.04
Fuzz+property testing	3	NA	NA	NA	NA
heap_buffer_overflow	1	1.43	1.43	1.54	1.54
invalid_bool_load	2	1.49	1.65	1.25	NA
invalid_enum_load	1	NA	NA	NA	NA
leak_detected	2	1.52	1.72	4.29	3.81
misaligned_pointer_use	6	1.34	1.33	2.14	1.43
null_pointer_use	1	2.86	3.33	NA	NA
signed_integer_overflow	2	1.2	1.27	1.56	2.5
stack_use_after_scope	2	1.85	1.85	1.67	3.33
use_of_uninitialized_value	13	2.05	1.93	2.05	1.95

Table 2: Lift (filtered-accept@1 / original accept@1) varies for different sanitizer bug types. The higher the better.

Metric	Abstain	AttemptRepair
Average # code-like identifiers	3.03	5.28
Average character length	771.67	1239.26
Fraction with repository root	0.12	0.31
Fraction possible build rule	0.01	0.03
Fraction Markdown code ticks	0.17	0.20

Table 3: Abstention-accepted bugs more frequently have the presence of the monorepo root directory.

to reproduction test filtering slightly reduces lift, while adding the 70th percentile validation keeps the same lift. For data races, adding validation actually reduces lift compared to just using reproduction tests. We hypothesize that: for these bugs, a patch that adds locking mechanism indeed resolves the data race and can pass patch validation, but still may not be an acceptable solution for a Google developer if it introduces downsides such as larger critical sections. For some categories, e.g. leaks and misaligned pointer usage, a less aggressive validation (75th percentile) achieves better lift than a more aggressive one (90th percentile).

We hypothesize that a true bug reproduction test, such as that included in the sanitizer's bug report, results in a set of plausible patches that are increasingly difficult for validation to assess, compared to the patches produced for the less-constrained bugs such as the human-reported ones. This points to a few future research directions. First, generating additional executable tests to help assess patch correctness is a particularly promising direction. For example, AutoCodeSherpa [15] generates property-based tests to assist reviewing and filtering agent-generated patches. As pointed out, after filtering with the reproduction test, sampling k=4 produces a mean filtered accept rate of 0.74. Supporting developers when inspecting more patches may help maintain a high success rate.

7 Discussion

The abstention policy has access to the bug report when making a judgment. We compute basic properties on our human-reported bug set to characterize their influence on policy outcomes. As shown in Figure 12 and Table 3, bugs accepted by abstention tend to have longer titles and descriptions (combined), and higher count of words that are possible code identifiers (based on underscore or capitalization changes). Similarly, accepted bugs tend to have a slightly-higher presence of Google's source repository root directory and possible code Markdown, indicated by triple backticks.

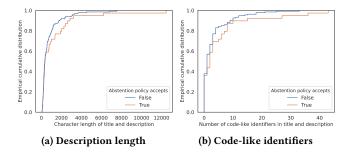


Figure 12: Bugs accepted by the abstention policy tend to have slightly longer title and descriptions and more words that could be possible code identifiers.

Similarly, we considered the size of the patches accepted by the 75th percentile-based patch validation for our human-reported bug set. We only consider patches that underwent LLM policy judgment. We count the number of line-level operations (addition/deletions) per patch. We find that validation-accepted patches had on average 18.38 operations, compared to 22.69 for rejected patches.

We now look at an example of a patch that validation (at 90th percentile) incorrectly accepted. Figure 13 shows the associated report indicating cases for a change in number formatting (when "points are chosen to be burnt"). The report leaves unspecified what should happen in other cases, but a human would likely take this to mean that other cases should still render the decimal point.

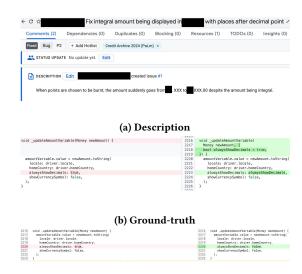
The ground-truth patch makes decimal rendering a function argument and modifies necessary call sites (not shown for brevity). In contrast, the agent patch modifies the constant true to false, which will impact all call sites. The generated specification identifies the correct method to change, but does not scope to particular call sites. The validation policy then incorrectly accepts.

8 Threats to Validity

Human and sanitizer-reported bugs were drawn from a set that has a known ground-truth patch, and some of our filtering relies on ground-truth patch properties (e.g., existence of a test). These constraints allow us to focus on patches within scope for an agent and employ ground-truth tests as an evaluation oracle. For sanitizer-reported bugs, where reproduction tests are already available at the time of reporting, having the ground-truth patch allows us to perform a manual comparison to determine correctness. This curation approach closely mirrors that used to create popular benchmarks like SWE-Bench, as well as industry practices [20, 27].

Manual correctness assessment may diverge from the decisions that would have been made by real code owners for patches that they did not inspect. To mitigate this risk, we employed multiple annotators, consulting others in cases of ambiguity.

Like other large-scale industrial software engineering studies [2, 12, 21], the bugs and code in this work are drawn from a single company (Google) and reflect internal practices. Projects or companies that write substantially different bug reports, or adhere to different development practices, may observe different policy impacts.



(c) Agent-generated candidate patch

Figure 13: A bug report indicates a case-dependent change. The validation's generated specification does not make this distinction so the policy incorrectly accepts the candidate.

We carried out experiments using Gemini 2.5 Pro and 2.0 Flash, and produced repair candidates using our previously published agent [27]. As new LLMs become available, we expect policy performance to vary (and improve), compared to what we report here.

9 Related Work

We used Passerine, our existing agentic APR system [27], to evaluate both bug abstention and patch validation. SpecRover [28] produces function-level specifications to guide the repair agent. Similarly, our validation policy employs a specification generation step to guide the patch judgment but this specification is at the file level, can be derived from multiple files, and is used only after the repair agent has concluded. Pan et al [25] showed that a fine-tuned LLM can be used to pick the best patch per bug based on the agent's trajectory. Our no-spec validation employs a similar concatenation of trajectory prompt messages. However, our LLM patch validation relies on prompting only and is used to filter out patches rather than pick a patch per bug. Our patch validation filters on build/test regression checks, or a reproduction test if available. Similarly, Agentless [34] uses (generated) tests to filter out unlikely candidates.

Abstention [5] has long been employed in classification to reduce mispredictions resulting from ambiguity or lack of information. Past work has explored this topic extensively, learning abstention policies in boosting algorithms [8], fairness guarantees [30], and, more recently, in the context of generative models [33].

Concurrent to our work, BouncerBench [22] introduces the concept of "bouncers" to reduce the overconfidence of an agentic software engineer. Bouncers reject underspecified tasks and unlikely patches, closely matching our abstention and validation policies. The benchmark BouncerBench opens up this challenge to the community and represents a complementary analysis to our own, which focuses on evaluating these policies in an industrial context.

Maddila et al. [20] concurrently introduced a patch critic in an industrial agentic APR system. Our work shows that abstention can reduce developer noise further and can be complementary to a validation policy. Our evaluation also covers additional bug categories (with and without known reproduction tests).

Works in LLM-as-judge for code, such as CodeJudge [32] and Crupi et al. [9], are closely related to our patch validation policy. In particular, reference-free judging is a close analogue to patch validation, but we focus exclusively on program repair and use judgments to filter patches to reduce noise (at the expense of recall).

10 Conclusion

We present a dual-policy framework to reduce developer noise from an agentic APR system. A bug abstention policy rejects bugs unlikely to be solved by the APR system, while a patch validation policy rejects unlikely candidate fixes. We evaluate these policies on three sets of bugs drawn from Google's codebase and on patches produced by our agentic APR system. On a set of human-reported bugs with known fixes, abstention and validation can raise success rates up to 13 percentage points and 15 percentage points, respectively, and up to 39 percentage points in combination.

In practical terms, this improvement transforms the developer experience: instead of expecting a successful patch roughly 1 in 10 times, developers can now expect a correct patch in 1 in 2 cases presented for their review (combined-p90). A more relaxed policy configuration (combined-p75) doubles the number of patches for inspection while still yielding a high success rate of roughly 1 in 3.

The patch validation policy can also be used to improve success rates for null pointer exceptions and sanitizer-reported bugs.

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A Human-Reported Bug Curation

In this section, we describe the criteria for curating human-reported bugs that have a known ground truth patch for completeness. The criteria closely follows [27], with the addition of the test target criteria. Namely, each bug must satisfy the following:

- (1) Was reported during a period of a year and closed with code changes after a model-based cutoff date in the recent past we use the cutoff to prevent old code changes that are more likely to be stale.
- (2) Has a known ground-truth fix and an associated fail-topass test that could be used as a held-out test oracle. This fail-to-pass test must execute without any build errors and fail in the pre-patch code and execute and succeed in the post-patch code.
- (3) The bug and submitted code changes have a 1:1 relationship, mitigating risk of tangled commits
- (4) Did not have screenshots in their description (multimodal repairs are out of scope for this work)
- (5) Affected files for any of the following languages: C++, C, Dart, Java, Kotlin, Typescript, Javascript, Go, and Python and conversely excluded changes that affected SQL, HTML, CSS, configuration languages, binary data files, or multimedia data files
- (6) Had a unidiff ground-truth commit of less than 150 lines
- (7) Had <= 31 test targets that depended on the files changed in the ground-truth patch.

B Bug Abstention Prompts

The section contains the full prompts for the bug abstention policies described in Section 3.1.2.

Listing 1: Prompt for bug representation $r_1(b)$.

```
You are tasked to predict whether an LLM-based agentic
    approach will successfully fix a bug.
The agent can use a suite of tools by issuing commands,
    including:
    - `code_search` to find relevant code.
    - `cat` to read file contents.
   - `ls` to list directory contents.
    - `find_and_replace_text` to modify files.
    - `bazel` to run tests.
The agent is presented with a bug report and can perform
    a maximum of 25 steps.
Analyze the following bug and predict if the agent will
    succeed. Your output must be a single word: either "
    success" or "failure". Do not provide any other text
    , explanation, or punctuation.
# Bug Title
{bug.title}
# Bug Description
{bug.description}
```

Listing 2: Prompt for bug representation $r_2(b)$.

```
# GUIDELINES

These guidelines provide criteria for determining if a bug is likely to be solved by the agent.

- Clear Problem & Action: The bug report explicitly and clearly describes the incorrect behavior, and the required change is well-understood.
```

Precise Code Localization: The report points to the exact file, code line, function, or configuration that needs attention, or it's easily identifiable from provided information (e.g., stack traces).
 Simple, Localized Fix Anticipated: The likely solution involves a small, straightforward, and localized change.
 Actionable Reproduction & Sufficient Context: The issue can be easily and reliably reproduced with the information provided, and sufficient context is available within the report for the agent to proceed effectively.
 Self-Contained and Complete: All necessary information for addressing the bug is provided and self-contained within the report, requiring minimal

C Patch Validation Prompts

external look-up or inference.

This section contains the full prompts for the patch validation policy described in Section 3.2. Listing 3 shows the prompt for the specification generation step.

Listing 3: Using the original bug description and file contents to generate a specification.

```
SYSTEM:
You are an expert software engineer that reviews a bug
     and lists out a specification for the desired
    behavior.
You should cover properties desirable for a fix. Be
     specific, do not provide general properties that
    apply to all bugs. Do not provide properties that
    are about artifacts like documentation.
USER:
Here is a bug, please create a fix specification:
# Bug Title
{bug.title}
# Bug Description
{bug.description}
# Original source code
$ cat {file_path}
{file_content}
# Specification
```

Listing 4 shows the LLM prompt used for validating a patch, along with the use of the generated specification.

Listing 4: Using the original bug report, candidate patch, any test information, and the fix specification generated in the prior step, to decide if a patch is likely to fix a bug.

```
SYSTEM:
SETTING: You are a software engineer reviewing code changes to fix bugs. Your job is to review how a bug2code agent tried to fix a bug.
Return a ReviewResponse with whether the bug is fixed and explaining why.

USER:
```

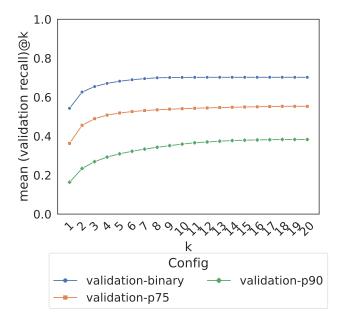


Figure 14: If we consider samples of size k of trajectories that pass the held-out reproduction test, more aggressive validation is more likely to incorrectly reject patches.

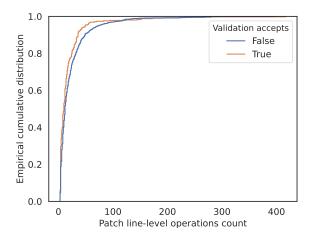


Figure 15: Patches accepted by the patch validation policy have slightly fewer line-level operations compared to those it rejected.

Approach	Binary Label	Precision	Recall
Prevalence (build/test)	TRUE	0.19	-
No-spec validation (binary)	FALSE	0.99	0.07
No-spec validation (binary)	TRUE	0.2	1
Validation (binary)	FALSE	0.91	0.58
Validation (binary)	TRUE	0.3	0.76

Table 4: Precision/Recall Table on the binary judgment: validation's binary judgment provides modest predictive performance. Removing the specification usage reduces performance substantially.

```
Here's a log of an attempt by a bug2code agent to fix a bug:

# Bug Title {bug.title}

# Bug Description {bug.description}

# Patch {patch}

# Test information {test_information}

# Specification:

Based on the bug report and source code, here is a specification for the desired behavior of the fix.

### Fix Specification {specification}
```

C.1 Additional Results

Table 4 presents the per-label precision/recall for binary validation policies.

To focus on the validation's ability to identify successful trajectories, we also consider a variant of recall. Specifically, we compute the average (over bugs) probability that at least one trajectory is accepted by the validation, given a sample of k trajectories that satisfy the held-out reproduction test. In effect, if our repair system produced only good fixes, how often would we show at least one repair to a user if we generated k trajectories. We denote this mean (validation recall)@k. To compute percentile-based thresholding we determine cutoffs over all trajectories that pass build and test regressions first, before filtering.

As shown in Figure 14, percentile-based thresholding results in more false negatives – rejecting patches that we should have shown to humans. When sampling a single successful patch, the binary validation will show a patch at a rate of 0.54, compared to 0.36 and 0.16 for 75th and 90th percentile thresholding, respectively.

Figure 15 shows that patches accepted by the validation policy have slightly more line-level operations compared to those rejected.

C.2 Patch Validation Decisions: False Positives

Listing C.2 shows the generated specification and validation policy output for the false positive example discussed in Section 7.

```
"Here is a fix specification for the bug.
### Desired Behavior Specification

1. **Conditional Decimal Formatting for Amounts**
    The amount displayed in the amount input field must
        be formatted based on whether it is an integral
        value.

* If the 'Money' object represents an integer (e.g., the fractional part is zero), it must be
    formatted as a string without decimal places (e.g., "500").
```



(a) Description

```
| 255 det two_destite_pref | 256 det two_destite_pref | 256 det two_destite_pref | 256 apraid detective_pref | 256 apraid detective_pref | 257 row; dict(str, Any), 258 amount | 257 row; dict(str, Any), 258 mode_pref | 258
```

(b) Ground-truth

```
| State | Stat
```

(c) Agent Patch

Figure 16: A false positive in patch validation.

- * If the `Money` object represents a non-integer value (e.g., the fractional part is non-zero), it must be formatted with the standard two decimal places (e.g., "500.50").
- 2. **Consistent Formatting on Reward Application**
 When a user applies <redacted> points (or any other
 deduction) that results in an integral final
 amount, the amount field must update to show the
 new integral amount without adding trailing
 decimals. For example, if the original amount is
 "500" and points worth "10" are applied, the
 field must display "490", not "490.00".
- 3. **Consistent Formatting on Reward Removal**
 When the user deselects the applied points and the
 amount reverts to its original integral value,
 the display must also revert to the format
 without decimals. For example, if the amount
 with points was "490" and the points are removed
 , the field must revert to displaying "500".
- 4. **Scope of Formatting Change**

The conditional decimal formatting logic should be applied in the `_updateAmountVariable` method.

This ensures that any programmatic update to the amount field, including the initial amount load, application of charges, and application/ removal of deductions, follows the same consistent formatting rule

is_bug_fixed: true

explanation: "The patch correctly fixes the bug. The bug description states that integral amounts are being displayed with unnecessary decimal points (e.g., \" XXX.00\") after rewards are applied. The fix specification requires conditional formatting: show decimals only for non-integer amounts. The patch modifies the `updateAmountVariable` method by changing the `alwaysShowDecimals` parameter from `true` to `false` in the `newAmount.toString()` call. This change ensures that the `Money` object\'s string representation will only include decimal places if the amount is not an integer, which directly addresses the root cause of the bug and fulfills all requirements of the specification." internal_score: 0.62615377

In the next case (Figure 16), the bug report provides relatively high-level discussion of the issue but does not specify what property should be used to check for links for a test sequencer module.

The ground-truth patch adds conditional handling based on the presence of a particular field (ts_version) in an input row. The agent's patch modifies this into an if-else statement. The else branch is equivalent to the fall-through case in the ground-truth patch. However, the if guard checks a different field (archive_file), which it inferred from the existing test files.

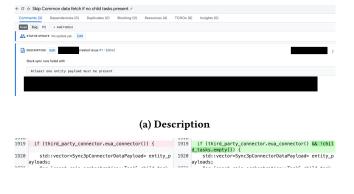
The generated specification correctly points out that links for a different module will need to be generated in a conditional fashion. It also elaborates on where these changes should be made and what field to check in the input. The validation's output correctly reflects the specification. However, the specification was wrong to begin with, as the field to be checked in the input is actually different: the field that the agent inferred was ultimately incorrect. In cases such as these, it is likely that a small piece of human feedback on the final patch would allow either the validation policy to make the right decision (reject) or the repair agent to update the patch.

```
Here is a specification for the fix.
### Fix Specification
The root cause of the bug is that the `test_details_href
     function unconditionally generates links to the
    test_details` page, even for test runs originating
    from the "Test Sequencer", which require a different
     details page ('/query/sequences').
The fix will involve modifying the `test_details_href`
    function to correctly identify Test Sequencer runs
    and generate the appropriate URL.

    **Conditional Link Generation in `test_details_href

   * The function `test details href` in `url builder.
        py` must be modified to conditionally generate
        one of two different URLs based on the
        properties of the test run.
2. **Identification of Test Sequencer Runs**
       A test run shall be identified as a "Test
        Sequencer" run if the `row` dictionary passed to
         `test_details_href` contains the key
        archive_file'`. The presence of this key is the
        designated indicator for a sequencer test result
```

3. **URL Generation for Test Sequencer Runs**



(b) Groundtruth



(c) Agent Patch

Figure 17: A true positive in patch validation.

- * If a test run is identified as a Test Sequencer run (i.e., `'archive_file' in row` is true), the `test_details_href` function must delegate the URL creation to the existing `sequences_href` function.
- * The `parms` and `row` arguments received by `test_details_href` must be passed directly to `sequences_href` to ensure all necessary parameters (`archive_file`, `module`, `test_case`, `name`, `param`) are available for building the correct `/query/sequences` URL.
- 4. **Preservation of Existing Behavior for Non-Sequencer Runs**
 - * If a test run is not identified as a Test Sequencer run (i.e., `'archive_file'` is not in `row`), the function must retain its original behavior.
 - * It must proceed to build a URL pointing to `/ query/test_details` (or the page specified by the optional `page` parameter) using the existing `_build_url_from_parms_and_row` call and its original list of parameters.

is_bug_fixed: true

explanation: "The patch correctly addresses the bug by modifying the `test_details_href` function. It now checks if the `row` data contains an `archive_file` key, which is the designated indicator for a test sequencer run. If the key is present, it correctly calls the `sequences_href` function to generate the appropriate link. If not, it maintains the original behavior, generating a link to the older `test_details` page. This conditional logic perfectly matches the fix specification and resolves the issue of incorrect link generation."

internal_score: 0.59506536

C.3 Patch Validation Decisions: True Positives

For the two bugs below, the agent patch is able to pass the held-out ground-truth reproduction test. Additionally, manual inspection shows these are equivalent to the changes made in the ground-truth patch.

The first bug (Figure 17) requires that the agent add an emptiness check to avoid processing cases where there are no child tasks. The ground-truth patch tightens a guard to add this check.

The specification generation step clearly outlines what the changes necessary should be and the review explanation shows that the agent patch adheres to that specification and so is judged to be correct.

```
"Based on the bug report, here is a specification for the
     fix
### Desired Behavior Specification
1. **Conditional Common Data Fetch Execution:**
    * In the `<redacted>` function, the call to
        {\tt ExecuteCommonDataFetchUsingEua` must only be}
        performed if the list of generated `child_tasks`
         is not empty.
  **Graceful Handling of No-Op Syncs:**
* If `CreateChildTasks` returns an
                              returns an empty vector of
         `child_tasks` (for example, because all entities
         for the connector are already in a `RUNNING`
         state), the `HandleParent` function must skip
        the common data fetch logic.
       The function should then proceed to return a `<
         redacted>::Success` with an empty list of tasks.
         This indicates that the parent task has
         completed successfully without scheduling any
        new work.
3. **Preservation of Existing Validations:**
    * The argument validation inside
        ExecuteCommonDataFetchUsingEua` which checks if
        the `entity_payloads` vector is empty and
        returns an 'InvalidArgumentError' must not be
        removed. The fix must prevent this function from
         being called with an empty vector, rather than
         altering the function's internal guards.
is_bug_fixed: true
explanation: "The bug occurs when the common data fetch
    logic (`ExecuteCommonDataFetchUsingEua`) is called
    with an empty list of child tasks, leading to an \"
    Atleast one entity payload must be present\" error.
    The patch correctly fixes this by wrapping the
    entire common data fetch logic, including the
    unpacking of payloads and the call to
    ExecuteCommonDataFetchUsingEua`, inside a
    conditional block `if (!child_tasks.empty())`. This
    ensures that the data fetch is only attempted when
    there are actual child tasks to process, preventing
    the error and allowing the parent task to complete
    successfully with no new tasks scheduled, which is
    the correct behavior for a no-op sync."
internal_score: 0.65
```

In the next example (Figure 18), the bug description provides instructions on the expected edits to a calculation. The ground-truth patch updates the calculation and in addition removes a constant

that is no longer used. The agent patch *only* updates the calculation, but does so in the correct way.







(c) Agent Patch

Figure 18: A true positive in patch validation.

The generated specification closely reflects the bug description, given the level of detail in that report. However, notice it has additional requirements such as limiting the scope of the changes. The generated validation output judges the patch is correct based on the specification produced.

```
Here is a fix specification for the bug.
### **Specification for <redacted> Calculation Fix**
This specification outlines the required changes to
     correct the Total Cost of Ownership (TCO)
     calculation for the <redacted> in the pricing
     calculator.
#### **1. Functional Behavior**
The estimated backup storage calculation for <redacted>
     must be corrected to no longer multiply the
     incremental backup data size by a factor of 2.
    **Current Incorrect Calculation Logic:**
    `Estimated Storage = (Total Data Size + (Incremental
         Data Size * 2.0)) / 1.6`
    **Required Correct Calculation Logic:**
    `Estimated Storage = (Total Data Size + Incremental
         Data Size) / 1.6°
#### **2. Specific Requirements**
    **Removal of Redundant Multiplier:**
In the `calculateEstimatedBackupStorage` function
         within the `<redacted>` file, the multiplication
          by the `BACKUP_STORAGE_MULTIPLIER` constant
```

must be removed.

```
**Location:** `<redacted>`, line 111
        **Current Code:** `return (totalDataSize + (
        additionalDataSize * BACKUP_STORAGE_MULTIPLIER))
         / PD_COMPRESSION_RATIO
        **Required Code:** `return (totalDataSize +
        additionalDataSize) / PD_COMPRESSION_RATIO`
2. **Correct Aggregation of Storage Components:**
    The final estimated storage value must be the result
        of summing the initial full backup size (`
        totalDataSize`) with the aggregated incremental
        changes (`additionalDataSize`), and then
        dividing the sum by the `PD_COMPRESSION_RATIO`.
#### **3. Invariants (Behaviors that must not change) **
1. **Scope of Change:** The modification must be
    strictly confined to the `<redacted>`. Calculations
    for other Backup and DR services must not be
    affected.
   **Compression Ratio:** The `PD_COMPRESSION_RATIO` (
    value 1.6) must continue to be applied to the total
    sum of the base data and all incremental data.
   **Incremental Data Calculation:** The logic for
    calculating the 'additional Data Size' (as performed
    by the `<redacted>` function) must remain unchanged.
     The fix only addresses how this value is used in
    the final aggregation, not how it is derived.
4. **Base Data Inclusion:** The `totalDataSize` (
    representing the initial full backup) must continue
    to be included as the base amount of data stored
    before incremental changes are added.
is_bug_fixed: true
explanation: "The patch correctly addresses the bug by
    removing the multiplication by
    BACKUP_STORAGE_MULTIPLIER` in the `<redacted>` file.
     The bug report explicitly requested the removal of
    a hard-coded factor of 2 that was being applied to
    the incremental backup data size. The code change
    from `(totalDataSize + (additionalDataSize *
    BACKUP_STORAGE_MULTIPLIER)) / PD_COMPRESSION_RATIO`
    to `(totalDataSize + additionalDataSize) /
    PD_COMPRESSION_RATIO` directly implements this
    requirement, thus fixing the over-estimation in the
    <redacted> calculation.'
```

In general, more detailed bug reports are likely to produce better bug abstention and patch validation results. For the former, the validation policy can create a more precise set of guidelines, which can then be used to judge the agent's patches.

C.4 Optimizability of Decisions Based on Bug Abstention and Patch Validation policies

While in this work, we focus on reducing noise for human developers by avoiding showing them patches that are unlikely to be correct, the decision of what bugs to tackle and what patches to surface can be approached as a more complex optimization problem. For example, we might explicitly model the effort and cost of human review, and use this to guide our choices. Below we briefly introduce some simple metrics that could be used and leave this for future work.

Let V_b be the value of successfully closing a bug b with a fix. Let C_b be the cost on the code base of leaving the bug unrepaired. Let R_c be the cost to produce a repair trajectory. Let p^* be the true, unknown, probability of a trajectory solving bug b.

The expected net value of the abstention policy is then:

$$\begin{cases} llp^*(V_b) + (1-p^*)(-C_b) - R_c & \text{if } P_a(b) \ge \tau_a \\ -C_b & \text{otherwise} \end{cases}$$

Let H_c be the cost of having a human review a patch from trajectory t. The analogous expected net value for the patch validation policy is then:

$$\begin{cases} llp^*(V_b) + (1-p^*)(-C_b) - H_c & \text{if } D_v(P_v(b,t)) \\ -C_b & \text{otherwise} \end{cases}$$