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Visible Results in Completed HCC-Search Trials: A Registry Reporting Audit

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Abstract

An eligible trial may be registered yet have no results table visible in its registry record. We captured all 1,382 completed records returned by a hepatocellular-carcinoma condition search and retained 704 interventional studies completed in 2010-2023 with positive enrollment. Of these, 233 had a first-results-post date. Industry sponsorship accompanied a 16.82 percentage-point higher posting probability after enrollment, completion-year, and phase adjustment; its HC3 interval was 8.57 to 25.06 points. Enrollment size alone had an uncertain association. These are search-cohort reporting patterns, not treatment effects or legal-compliance findings. The cohort includes unadjudicated related-condition and basket studies and should not be equated with first-line advanced-HCC trials.

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1. Evidence availability precedes synthesis

A treatment comparison requires more than the existence of a registered trial. Researchers need accessible outcomes, clear study descriptions, and enough information to judge whether evidence fits the intended question. Registry result tables are one route to that information. Their absence, however, is not equivalent to absence of a journal publication or evidence that a treatment failed. We examine visible registry result posting as a distinct, measurable property of a public search cohort.

The network meta-analysis of first-line immunotherapy and targeted therapy in advanced hepatocellular carcinoma by Li et al., 2026 provides substantive motivation for attention to evidence availability and subgroup reporting. Our cohort is broader and does not reconstruct that review's included trials. It does not estimate relative treatment efficacy, safety, HBV-specific benefit, or HCV-specific benefit. The connection is methodological: incomplete visibility can complicate evidence identification before a treatment synthesis begins.

The ClinicalTrials.gov data structure defines the fields used in this audit (National Library of Medicine, 2026a), and the results-database account in Zarin et al., 2011 explains the importance of structured public results. Outcome-reporting concerns documented by Chan et al., 2004 further motivate distinguishing available evidence from registered intentions. The present analysis does not establish selective reporting of particular outcomes, because it uses a first-results-post date rather than comparing protocols with individual published outcome tables.

2. A reporting endpoint with strict boundaries

We define visible results as the presence of a resultsFirstPostDateStruct date in the captured status module. This is an ascertainment rule on the registry snapshot. It is not the number of outcomes reported, the completeness of adverse-event information, the timeliness of posting, or the existence of a peer-reviewed article. A record without that date may still be associated with public evidence elsewhere. A record with the date may still require substantial critical appraisal.

The review-reporting purpose of Page et al., 2021 differs from the reporting-compliance cohort studied by DeVito et al., 2020. Guidance in Guyatt et al., 2011 and Guyatt et al., 2008 concerns the interpretation and certainty of evidence. Our binary registry endpoint is narrower than all these purposes: it records whether

the captured study has a visible results date. It does not adjudicate legal compliance, reproduce the earlier cohort, assess completeness of a publication, or establish high-certainty treatment evidence.

Treatment-network reporting and consistency are discussed by Hutton et al., 2015, van Valkenhoef et al., 2012, and Higgins et al., 2012. Higgins and Thompson, 2002 addresses heterogeneity between studies. The separate registry-compliance assessment in DeVito and Goldacre, 2021 concerns reporting processes rather than treatment synthesis. Our registry regression estimates none of those network or compliance quantities. Its endpoint remains a visible results date in the captured search cohort, before adjudication of any treatment network or legal reporting obligation.

3. Complete pagination of a defined search

The source query uses the condition term Hepatocellular Carcinoma, overall status COMPLETED, a page size of 200, and selected study fields. We follow every returned pagination token and capture seven pages. The API reports 1,382 matching records; the final assembled file contains exactly 1,382 unique study identifiers and no remaining page token. The capture finished on 6 November 2026 UTC. This completeness claim applies to the specified search response, not to every HCC study conducted worldwide.

Condition search can return basket trials and related-condition records. We do not manually adjudicate whether each is a first-line systemic-treatment trial in advanced HCC. The label HCC-search cohort is therefore retained throughout. Using a familiar disease term without this qualification would incorrectly narrow the population and make the reporting rate appear specific to a treatment setting the eligibility process never verified.

We restrict the assembled response sequentially. First, we exclude 368 noninterventional records. Second, we exclude 309 records whose completion year is outside 2010 through 2023 or is unavailable for that restriction. Finally, one record has nonpositive or missing enrollment and is excluded. The resulting cohort has 704 studies. Sequential exclusion counts are preserved so that their denominators are clear and can be reconstructed from the source snapshot.

The completion-year restriction leaves time between completion and capture, but does not define a common legally required reporting deadline. No regulatory category, applicable jurisdiction, exemption, or delay determination is evaluated. We therefore make no compliance accusation about individual studies. This is a descriptive reporting-visibility sample with a declared completion window, rather than a legal audit or an estimate of misconduct.

4. Features available in the requested fields

The model uses log enrollment count, an industry lead-sponsor indicator, completion year centered at 2016, and a three-category phase variable. A study is late phase if its recorded phases include phase three or four; otherwise it is early if they include early phase one, phase one, or phase two. The remaining category covers absent or not-applicable phase information. Mixed early phase one/two records remain in the early group under this declared rule.

There are 401 early-phase studies, 105 late-phase studies, and 198 in the absent-or-not-applicable group. Median enrollment is fifty, while mean enrollment is 139.05 and the maximum is 12,386. That skew motivates a logarithmic enrollment covariate. It does not establish that the largest record is erroneous, and we retain it. The search-based cohort can contain heterogeneous interventions and designs, so descriptive distribution checks remain essential.

The industry indicator concerns the recorded lead sponsor only. It does not identify every funding source, commercial collaboration, or contributor. Completion year is a coarse calendar adjustment; it cannot represent all policy, database, or publication changes over time. Measurement-error considerations in Carroll et al., 2006 help frame these limitations. The variables are registry descriptors whose accuracy and meaning may vary, not externally adjudicated measures of sponsor behavior.

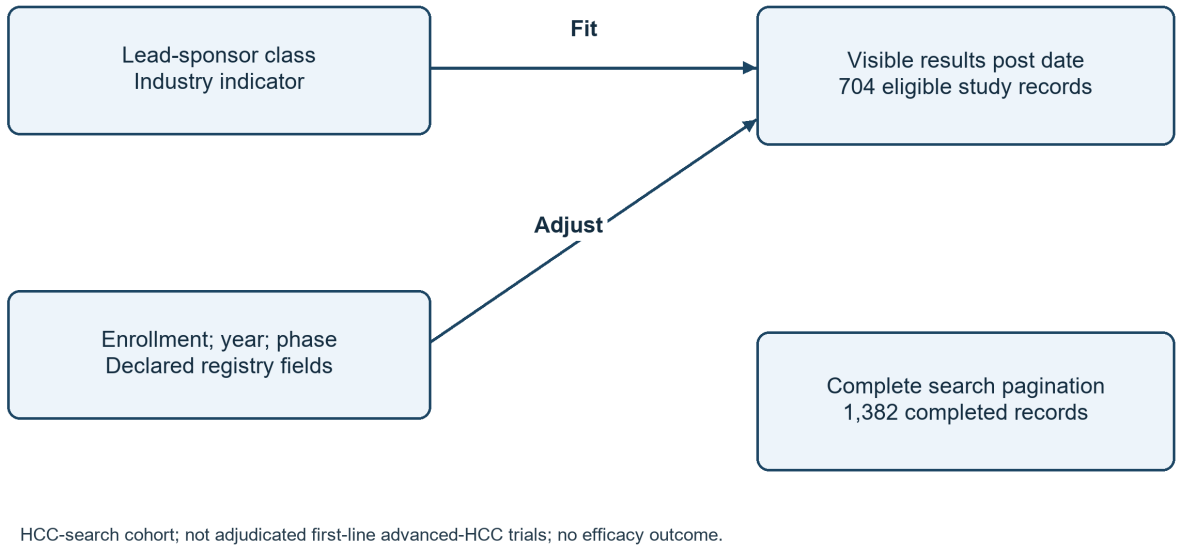


Figure 1. Sponsor class, enrollment, phase, and completion year describe the probability of a visible first-results-post date within the explicitly filtered registry search cohort.

5. Descriptive probability regression

We fit a linear probability model of visible posting on the declared features, with early phase as the reference category. HC3 standard errors and t-based intervals address heteroskedasticity, following White, 1980. The model has 704 study rows. It is not a patient-level model and contains no treatment-response observations. The coefficients are adjusted study-level associations expressed in probability units, not risk differences for clinical outcomes.

Shared sponsors or research networks could produce dependence between studies. Cameron and Miller, 2015 motivates considering such clusters, but the requested reduced field set does not provide an adjudicated sponsor hierarchy. The binary industry field cannot serve as thousands of distinct sponsor identifiers, and clustering into only two sponsor classes would not be a credible repair. We therefore disclose the independent-study HC3 assumption rather than claiming to have controlled all institutional dependence.

Causal reasoning in Greenland et al., 1999, observational-design perspectives in ROSENBAUM and RUBIN, 1983, and matching discussion in Stuart, 2010 clarify why adjustment is not causal identification. Sponsor class bundles resources, trial types, geography, and reporting practices. Unmeasured differences may explain the association. We do not estimate what would happen if an otherwise identical trial were assigned an industry sponsor, and no recommendation about choosing a sponsor follows from the coefficient.

Prediction-model standards in Wolff et al., 2019 and modeling guidance in Steyerberg et al., 2010 also distinguish descriptive fitting from validated prediction. Our model is not evaluated on a future registry snapshot and is not intended to predict nonreporting for individual trials. It has no selected action

threshold, external calibration, or assessed decision benefit. Treating a fitted probability as an accusation against a record would be especially inappropriate under these design limitations.

Table 1. Adjusted associations with a visible first-results-post date. Phase baseline is early; all intervals are HC3. The outcome is reporting visibility at capture, not clinical efficacy or compliance.

Model	Term	Estimate (SE)	95% CI	N
M1	Late vs early phase	-0.0511 (0.0546)	[-0.1583, 0.0561]	704
M1	Other/absent vs early phase	-0.1065 (0.0439)	[-0.1928, -0.0203]	704
M1	Log enrollment	0.0177 (0.0158)	[-0.0133, 0.0487]	704
M1	Industry lead sponsor	0.1682 (0.0420)	[0.0857, 0.2506]	704
M1	Completion year minus 2016	0.0075 (0.0045)	[-0.0013, 0.0163]	704
M1: fit	R-squared = 0.0523	-	-	704

Model labels: M1 = Visible registry results associations.

6. What is visible in the captured cohort

Among the 704 eligible studies, 233 have a recorded first-results-post date, a fraction of 0.3310. This is an exact descriptive count under our eligibility and snapshot rules. It is not a publication rate, a rate of on-time reporting, or a rate for all advanced-HCC systemic-treatment trials. Those interpretations require additional sources and adjudication that were not performed in the current audit.

Industry lead sponsorship has an adjusted coefficient of 0.1682, with standard error 0.0420 and interval 0.0857 to 0.2506. The log enrollment coefficient is 0.0177, with interval -0.0133 to 0.0487. Completion year has coefficient 0.0075 per year, with interval -0.0013 to 0.0163. These latter associations remain uncertain after the stated adjustments; the source does not justify a simple claim that bigger or more recent studies always post more often.

Relative to early phase, the late-phase coefficient is -0.0511, with interval -0.1583 to 0.0561. The absent-or-not-applicable phase category has coefficient -0.1065, with interval -0.1928 to -0.0203. The full model R-squared is 0.0523. Category differences can reflect heterogeneous study purposes and registry conventions, so the negative coefficient should not be read as a deficiency attributable to not having a phase designation.

The distinction between aggregate and individual inference emphasized by Robinson, 1950 is relevant even though these rows represent studies rather than geographic groups. A sponsor-class association does not describe every sponsor, trial team, or participant. Qualitative-evidence assessment in Lewin et al., 2015 also reminds us that understanding why results appear or remain absent can require contextual evidence beyond a short numerical field set. Our regression does not supply those explanations.

7. What a visibility audit leaves unresolved

The live registry can change after capture. A record without posted results may gain them later; a completion date or enrollment field can also be corrected. The source timestamp and original pages therefore matter as much as the final coefficient table. We do not merge later updates into the existing cohort while retaining the earlier capture date. A longitudinal follow-up should compare explicit snapshots and account for differing follow-up times.

Missing-data reasoning in Little and Rubin, 2002 applies to absent phase information, incomplete enrollment, and unavailable records outside the search system. We retain an explicit phase category rather than imputing a clinical phase from the title. The single enrollment exclusion is reported, but the more consequential uncertainty may concern disease and treatment eligibility. Manual review of basket studies would be required before claiming that the cohort represents the treatment question of a first-line HCC evidence synthesis.

Influence diagnostics following Cook, 1977 could examine the largest enrollment record, yet excluding it merely because it is large would alter the declared sample. The guidance of Wasserstein and Lazar, 2016 and concerns in Ioannidis, 2005 support focusing on the limited estimand and retaining uncertain covariates. Multiplicity frameworks in Benjamini and Hochberg, 1995 and Benjamini and Yekutieli, 2001 would be relevant to a larger discovery exercise; the present intervals remain exploratory and unadjusted, not a list of confirmed reporting determinants.

The audit should also remain separate from treatment guidance. No individual patient's care, prognosis, viral status, adverse events, or survival time enters the file. The target clinical review and the registry cohort are different evidence objects. Their relationship is the practical need to locate and assess available information, not a license to extrapolate registry posting patterns into claims about which therapy is best.

8. A reviewable reporting resource

The reporting fraction depends on the exact denominator. Including observational records, recent completions, or zero-enrollment records would change the cohort and potentially its posting pattern. We do not search across those alternatives for the largest sponsor association. The sequential restriction is part of the research definition, and any extension should retain its own counts. This is especially important when comparing with a review limited to advanced disease, first-line therapy, or specific viral subgroups, because those restrictions were not adjudicated in our source query.

Result submission and result posting are also different registry events. A submitted result can undergo quality-control processing before becoming public. Our outcome uses the public first-post date rather than a submission date, so it describes visible availability at capture. It cannot distinguish a study that never submitted from one whose submission was not publicly posted at that time. Without a separate processing-history analysis, attributing every absent post date to investigator behavior would exceed the field definition.

For evidence synthesis, the immediate use of this resource is to identify records requiring additional source checks. Reviewers could search publications, inspect registry history, and compare reported outcomes with protocols. Those actions would add evidence about availability and consistency that the current reduced field set lacks. The regression can prioritize a methodological question, but it should not replace record-level adjudication or be used to exclude a trial solely because its sponsor category has a lower average posting probability.

Source and processing documentation follows the reuse principles of Wilkinson et al., 2016 through exact query fields, pagination checks, capture time, sequential exclusions, and processed study rows. The package contains public registry metadata and no newly collected patient data. It preserves the distinction between the complete search response and the narrower analysis sample. Human review is still needed for clinical eligibility, source interpretation, and any intended publication claim.

The main empirical finding is that visible registry results are unevenly distributed in this completed HCC-search cohort, with a positive adjusted industry association and uncertain enrollment and completion-year associations. It provides a reproducible starting point for evidence-availability work. It

does not identify the cause of missing results, evaluate legal duties, or estimate the comparative effectiveness of the interventions named in those records.

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