

Forward-testing mineral prospectivity models with discovery dates: Quebec graphite and nickel

Author

Maheswara Reddy Vallabha Reddy

Independent Researcher, Sheffield, United Kingdom

Email: mahireddy232001@gmail.com

ORCID: <https://orcid.org/0009-0009-8809-725X>

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Abstract

Mineral prospectivity models are usually evaluated with spatial cross-validation, but known occurrences cluster around past exploration, so this may measure performance within the explored footprint rather than the ability to rank later discoveries. We trained seven prospectivity models on graphite and nickel occurrences known in Quebec, Canada, before 2000 and tested how they ranked cells with occurrences first recorded from 2000 onward. Spatial block cross-validation was compared with the cluster-weighted share of these discoveries captured in the highest-ranked 5% of eligible cells, across eight related settings per commodity (cutoff year, grid resolution, geophysical coverage, predictor dates and label definition). For graphite, whose later discoveries lay a median 88 km from earlier ones, cross-validation overestimated forward capture for at least six of seven models in every setting and in ten repeated spatial partitions. The proximity-to-known-occurrences baseline lost the most: 0.68 in cross-validation against 0.26 in the forward test, with non-overlapping intervals. For nickel, whose discoveries stayed near established camps (median 14 km), cross-validation and forward rankings agreed more closely. Which model cross-validation selected depended on whether never-worked showings were counted as discoveries. Fixing exploration effort at a common value when scoring never clearly improved graphite capture and consistently reduced nickel capture. Across primary-label settings, no model captured more than 18% of discoveries in cells without recorded prior exploration, a rate not clearly different from random selection. Where discovery dates exist, forward tests should accompany spatial cross-validation.

Keywords: Mineral prospectivity mapping · Spatial cross-validation · Temporal validation · Exploration bias · Graphite · Quebec

1. Introduction

Mineral prospectivity models help exploration geologists prioritise large regions by ranking areas according to their similarity to known mineralised environments (Carranza 2008; Lawley et al. 2022). These models commonly combine regional geology, geophysical data, field observations and the locations of known mineral occurrences. Their practical value depends not

only on how well they fit historical data, but on whether the areas they rank highly contain discoveries made after the model was trained.

That question is hard to evaluate because mineral occurrences are spatially clustered. Random cross-validation can place training and evaluation occurrences from the same district in different folds, which inflates performance estimates (Roberts et al. 2017; Airola et al. 2019). Spatial block cross-validation reduces this problem by keeping nearby cells together, and is widely used to limit the influence of spatial autocorrelation (Valavi et al. 2019; Ploton et al. 2020).

Even spatial cross-validation, however, evaluates a model within the historical pattern of exploration and documentation. It asks whether a model can separate known occurrences from unrecorded cells elsewhere in the same explored landscape. It does not ask whether the model will rank discoveries made after the training data were collected. Validation after deployment is rarely possible in mineral exploration, because discoveries are few and slow to accumulate (Zhang et al. 2025a).

This distinction matters because known occurrences are not neutral observations of geology. They record places where exploration occurred, mineralisation was recognised, and results were documented. Exploration is spatially clustered: companies commonly work near existing discoveries, revisit established camps, or expand gradually from known ground. Predictors such as distance to known occurrences and prior exploration effort may therefore encode an exploration footprint as well as geological favourability. A model that performs well within that footprint may perform poorly when exploration moves into new regions.

Discovery databases add a further complication. Records range from early showings with little follow-up work to prospects with substantial exploration and to producing mines. The date attached to a record may reflect first documentation rather than first observation, and the point at which an occurrence becomes a deposit is often unclear. These choices affect both the training labels and the events used to test a model, yet their influence on model comparison is seldom examined, although the choice of negative labels has recently received attention (Zhang et al. 2025b).

To our knowledge, few studies evaluate mineral prospectivity models against discoveries made after the training cutoff, and fewer still compare this forward performance with spatial cross-validation across label definitions, cutoffs, grid resolutions and commodities. Existing validation approaches largely use spatial blocks, random splits of known deposits, success-rate curves or independent drilling sites, which assess spatial generalisation or agreement with known mineralisation rather than performance on later discoveries. A recent exception is a retrospective blind check of six Western Australian gold discoveries announced in 2023–2025, which its authors note is too small for statistical validation (Zhang Y. et al. 2026). Our design is a retrospective forward test: it uses discoveries that were made independently of the models, so it does not capture how a deployed map would itself change where exploration happens. Here we forward-test mineral prospectivity models using discovery dates from SIGEOM, the Quebec

geoscience database. We train models on graphite and nickel occurrences known before 2000 and evaluate how well they rank discovery cells first recorded from 2000 onward. Graphite and nickel were chosen because their post-2000 exploration histories differ: new graphite discoveries were made far from earlier ones, whereas nickel discoveries stayed near established camps.

We compare spatial block cross-validation with this forward test, examine whether fixing exploration effort at a common value when scoring improves performance, and assess whether models rank discoveries made in cells without recorded prior exploration. We repeat the analysis with:

- a 2010 cutoff;
- 1 km and 5 km grids;
- alternative definitions of electromagnetic survey coverage;
- predictor records restricted to before 1998;
- a restricted label set that excludes never-worked showings.

We address four questions:

1. Does spatial cross-validation predict how models rank post-cutoff discoveries?
2. Does effort-fixed prediction improve forward performance?
3. Can models rank discoveries in cells without recorded prior exploration?
4. How sensitive are these conclusions to the definition of a discovery?

The study makes three contributions:

- A pre-specified forward-testing design, frozen and fingerprinted before the test was run, that separates predictor information, training labels and test discoveries in time.
- A comparison of spatial cross-validation with forward performance across two commodities with contrasting exploration histories, two cutoffs, three spatial resolutions and two label definitions.
- Evidence that cross-validation optimism and the poor transfer of proximity-based information can be robust even when model-selection conclusions depend on how low-evidence occurrences are labelled.

2. Methods

2.1 Study area, data and temporal design

The study area is the province of Quebec, Canada, defined by the Natural Earth 1:10M boundary and projected to Quebec Lambert (EPSG:32198). All mineral, drilling, outcrop and geophysical records come from SIGEOM, the provincial geoscience database of the Ministère des Ressources

naturelles et des Forêts. The national geology layer is Geological Survey of Canada Map D1860A, *Geological Map of Canada*, published in 1996 and released digitally in 1997 (Wheeler et al. 1996).

We study two commodities chosen for their contrasting exploration histories. After 2000, graphite exploration expanded into new regions. The timing is consistent with growing interest in battery minerals, although SIGEOM does not record why exploration took place. Nickel exploration largely stayed in established camps.

Graphite labels are SIGEOM non-metallic occurrences with symbol code GNMGP, dated by discovery year (AN_DECV). Nickel labels are metallic occurrences whose principal substance is Ni, dated by the earliest discovery year recorded for each deposit (DECV_AN). Undated records (22 graphite, 1 nickel) are excluded from both training and testing.

The temporal design uses a single cutoff year $T = 2000$:

- **Training data.** Every input variable uses only information dated before T . Training labels are occurrences discovered before T .
- **Test data.** Test labels are cells containing an occurrence discovered in or after T , provided nothing of the same commodity was known in that cell before T .

Test cells are therefore cells with no previously recorded occurrence of the target commodity; a test cell may still lie within a previously explored property or district. A second cutoff ($T = 2010$) is used as a robustness setting (Section 2.6).

At $T = 2000$ the graphite data contain 95 training cells and 89 test cells, forming 50 discovery clusters. The nickel data contain 243 training cells and 144 test cells in 102 clusters.

2.2 Label definitions

What counts as a “discovery” is a modelling choice. Two label sets were analysed, and a third was considered and found infeasible.

Terms are used as follows throughout:

- **Occurrence:** any SIGEOM mineral occurrence record of the commodity.
- **Showing:** an occurrence with little or no recorded follow-up work.
- **Deposit or mine:** the strict SIGEOM classes. These are not used in the analyses below.
- **Discovery cell:** a grid cell containing a post-cutoff occurrence and no earlier occurrence of the same commodity.
- **Discovery cluster:** a group of discovery cells merged under the 5 km rule (Section 2.5).

Table 1. Label sets.

Label set	Definition	Graphite (train / test cells, test clusters)	Nickel (train / test cells, test clusters)
Primary	All dated SIGEOM occurrences, including showings with no recorded follow-up work	95 / 89, 50	243 / 144, 102
Restricted (post hoc)	Occurrences with recorded exploration or assessment work; never-worked showings excluded	54 / 38, 20	135 / 74, 51
Deposits and mines only	Occurrences classed as deposits or mines	not analysed: only 7 post-2000 records	not analysed: only 10 post-2000 records

The restricted set does not identify deposits. It removes the most ambiguous records, the showings with no recorded exploration or assessment work, and so tests whether the conclusions depend on including low-evidence occurrences. These showings were removed from the training labels, the test labels, and the “other showings” effort variable. In addition, a cell with any occurrence of the commodity before the cutoff could not count as a discovery cell.

The strict deposits-and-mines definition gives too few post-2000 events for a stable comparison of seven models, so it was not run. The restricted set has only 20 graphite discovery clusters and its intervals are wide; it is therefore treated as a sensitivity analysis, not as the primary test.

2.3 Grid, predictors and leakage control

Quebec is divided into 2 km square cells, with the grid origin aligned to a 10 km lattice. This gives 372,900 cells inside the provincial boundary. Each cell receives the predictor groups below.

Table 2. Predictor groups (all dated before the cutoff T).

Group	Content	Source and dating
National map (GMC)	154 map units, granulite flag, distance to faults and contacts	Map D1860A (1996–97), so it predates both cutoffs
Local geology (GEO)	rock-type fractions and families, number of observations	SIGEOM field observations and dated compilation outcrops, before T
Observations (OBS)	graphite noted in drill logs or outcrop descriptions	Drilling year or observation date, before T
Geophysics (EM)	whether the cell was surveyed; electromagnetic conductor count	Report year, before T; the count is missing (not zero) where the cell was not surveyed
Effort (EFF)	drilling, government mapping, compilation outcrops and other-commodity showings, in the cell	Same dating rules, before T

Group	Content	Source and dating
	and within 10 km	
Missingness (MISS)	indicators for cells with no observations	Derived from the groups above

Two rules guard against leakage of future information:

- **Dating.** A record is used only if it carries a date before the cutoff. Records with unknown dates are excluded from the predictors (details in Supplementary Methods S1).
- **No target in the inputs.** The “other showings” effort variable originally included the target commodity itself, which is a direct label leak. That commodity was subtracted before modelling.

A per-variable audit is provided as supplementary material. It identified one main remaining risk: publication lag. Dates in SIGEOM record the fieldwork or report year; they are not public-release or database-entry dates, so filtering a modern database by activity date does not exactly reconstruct the information available at the cutoff. A drill hole or field observation dated before T may have been made public after T. Sensitivity to this is tested in Section 2.6.

Overlap between observations and labels. This is a different issue from leakage of future information. The OBS predictors record graphite noted in drill logs or outcrop descriptions before the cutoff, and they often coincide with known occurrences. For example, graphite was noted in drill logs in 28% of known graphite cells, against 0.9% of Quebec. In cross-validation, part of the score may therefore measure how well a model recognises previously recorded graphite evidence, not how well it predicts undiscovered occurrences. The forward test avoids this particular overlap, because every test cell has no earlier recorded occurrence of the commodity, although it remains subject to the provenance limitations above.

2.4 Models

Seven models were fixed in advance. Each is defined by the predictors it uses:

Table 3. The seven models.

Model	Predictors	Purpose
R0	Distance to the nearest known occurrence	Proximity baseline
R1	Effort only (9)	Does exploration effort alone predict discoveries?
M1-A	National map (42)	Regional geology only
M1-B	Map + local geology, observations, EM, missingness (78)	Adds local data
M1-C	M1-B + effort (87)	All information

Model	Predictors	Purpose
M4-A	Map + effort (51); effort fixed when scoring	Effort-fixed map model
M4-C	As M1-C (87); effort fixed when scoring	Effort-fixed full model

The counts in brackets are the numbers of inputs used for graphite, after exactly duplicated columns (for example, indicator columns that are zero in every cell) were removed; nickel models used 42, 76, 85, 51 and 85 inputs respectively.

The M4 models are trained with effort included. When they score cells, every effort column is set to a common reference value, its median in the training background (“effort-fixed prediction”). For an additive logistic model this is equivalent to ranking cells by the non-effort terms of a model fitted with effort as a covariate: the reference value adds the same constant to every cell, so its exact choice does not affect the ranking. The procedure removes the cell-to-cell variation in the effort terms; it does not by itself recover an unbiased geological signal.

M1-C and M4-C use the same inputs, training data, background sample and selected C (0.01), so they share identical fitted coefficients and differ only in how effort is handled at prediction time. M4-A and M1-A differ in their training inputs as well (map plus effort, against map only), and C was tuned separately (graphite M4-A: 0.1; all other logistic models: 0.01), so the M4-A – M1-A comparison does not isolate a prediction-time intervention. For gradient-boosted trees the model is not additive and fixing effort is not equivalent to dropping its terms.

The main learner is L2-regularised logistic regression on standardised inputs, implemented in scikit-learn (Pedregosa et al. 2011). The regularisation strength C was chosen from {0.01, 0.1, 1} by spatial cross-validation with 50 km blocks. Gradient-boosted trees (histogram gradient boosting; settings in Supplementary Methods S2) were fitted as a secondary learner for M1-A, M1-C and M4-C.

Positives are the training cells. Negatives are 20,000 background cells drawn at random, without replacement, from the cells with no known occurrence (a presence–background design; Phillips et al. 2009), with balanced class weights.

- **Shared background.** The draw uses a fixed seed (0), so every model trained on the same data uses the same background cells.
- **Where background is drawn.** In cross-validation it comes from the training folds of each split. In the forward test it comes from all cells with no pre-cutoff occurrence.
- **Future discoveries.** Background cells can include cells that later contain a post-cutoff discovery. This is inherent in presence–background designs, where true absences are unknown; Zhang et al. (2025b) call this one-directional error “label crossover”.

Models are compared only by how they rank cells, so absolute probabilities are not interpreted. Sensitivity to the size of the background sample was not tested.

2.5 Validation schemes and metrics

Each model was evaluated three ways.

- **V1, random cross-validation.** Five folds, with training cells assigned to folds at random.
- **V2, spatial block cross-validation.** Five folds made of square blocks of 25, 50 or 100 km; the 50 km blocks are the primary setting. Spatial block cross-validation is widely used to reduce the influence of spatial autocorrelation between training and evaluation data.
- **V3, forward test.** Each model is trained once on everything before T, then ranks all cells in Quebec. The ranking is scored against the post-cutoff discovery cells.

The primary metric is **capture at 5%**: the cluster-weighted share of post-cutoff discovery cells that fall among the 5% of cells the model ranks highest.

$Capture_5\% = (\sum w_i \text{ over discovery cells } i \text{ in the top 5\%}) / (\sum w_i \text{ over all discovery cells})$, with $w_i = 1 / m_i$,

where m_i is the size of the cluster containing cell i .

Cells are ranked among those with no pre-cutoff occurrence of the commodity: 372,805 cells for graphite and 372,657 for nickel at T = 2000. The top 5% is therefore about 18,640 cells. Tied scores share their average rank. Capture at 0.5%, 1% and 10%, and ROC AUC, are also reported.

The two scores are computed as comparably as the designs allow (Table 4). In each cross-validation split, every model, including the proximity baseline, is refitted on the training folds only: R0 distances are computed to training-fold occurrences, so held-out occurrences never contribute to their own predictor. No other predictor is derived from the target labels.

Table 4. How capture at 5% is computed in each validation scheme.

	V1 / V2 cross-validation	V3 forward test
Targets	pre-cutoff occurrence cells in the held-out fold	post-cutoff discovery cells
Ranking universe	all grid cells in the held-out fold (about one fifth of Quebec)	all cells with no pre-cutoff occurrence of the commodity
Top-5% threshold	5% of the cells in each held-out fold, by area	5% of all eligible cells, by area
Weights	1 / cluster size, 5 km clusters of pre-cutoff occurrences	1 / cluster size, 5 km clusters of discoveries
Aggregation	out-of-fold ranks pooled across the	one weighted capture

	V1 / V2 cross-validation	V3 forward test
	five folds, then one weighted capture	
Occurrence-derived predictor (R0)	distances to training-fold occurrences only	distances to all pre-cutoff occurrences

Thresholds are therefore area-based in both schemes, not based on the class-balanced training sample. The remaining difference is that cross-validation ranks within each held-out fold rather than across the whole province; because spatial blocks are assigned to folds at random, each fold spans most of Quebec.

Post-cutoff discoveries cluster in space. One new property can add a dozen occurrences within a few kilometres. To stop a single cluster from dominating the score:

- Test cells within 5 km of each other are merged into one discovery cluster. The rule is transitive (single linkage): chains of cells each within 5 km of the next form one cluster.
- Each test cell is weighted by $1 / (\text{size of its cluster})$, so every discovery counts once.

Confidence intervals (95%) come from a cluster bootstrap: 1,000 resamples of discovery clusters, seed 0. The bootstrap holds fixed the fitted models, the selected hyperparameters, the background sample, the spatial folds and the top-5% threshold; it describes uncertainty from the finite set of discoveries only. Differences between two models are computed within the same bootstrap resamples (paired). These paired comparisons were added after the forward test (**post hoc**); they are exploratory, and no correction for multiple comparisons was applied.

The regularisation strength C was tuned on the same 50 km folds used to report V2 capture, which can make the reported V2 values slightly optimistic for the logistic models (the three C values differed by at most 0.07 in capture). The proximity baseline has no tuned parameter. To check sensitivity to tuning and to the particular partition, V2 was recomputed (**post hoc**) with the frozen C values on ten further random assignments of 50 km blocks to folds, none of which was used for tuning, and with cluster-bootstrap intervals for the cross-validation estimate.

Agreement between cross-validation and the forward test is measured as the Spearman rank correlation between the seven models' V2 (50 km) and V3 capture at 5%. With only seven models, this correlation is descriptive, not a formal test. We therefore also report where CV's top model ranked in the forward test. Most single-model forward intervals overlap, so differences between models are called clear only when a paired interval excludes zero.

Pre-specification. The design was written down and frozen on 12 October 2026, before the forward test was run, and the frozen files were fingerprinted with SHA-256 hashes. The design was not lodged with an external registry. It fixed the models, predictors, metric, cutoff and four hypotheses:

- **H1 (optimism):** cross-validation overestimates forward performance, more for graphite than for nickel.
- **H2 (bias correction):** fixing effort when scoring improves forward performance for graphite more than for nickel.
- **H3 (input bias):** models using exploration-dependent inputs (outcrops, EM, graphite observations) lose more in the forward test than models using the national map only.
- **H4 (model selection):** cross-validation ranks the models differently from the forward test.

Safeguards against re-running the forward test are described in Supplementary Methods S3.

Software and AI assistance. Analyses used Python 3.13 with scikit-learn 1.9, pandas 3.0, NumPy 2.5, SciPy 1.18, pyproj 3.8, shapely 2.2 and matplotlib 3.11. AI tools were used substantially for writing analysis code and drafting text (see Statements and Declarations); all code outputs were checked against the frozen result files.

2.6 Robustness settings

The whole pipeline was rebuilt from raw data under seven other settings, changing one setting at a time. Gridding, labelling, cross-validation and the forward test were all rerun each time.

Table 5. Analysis settings.

Setting	Change from main analysis	Type
Main	2 km cells, T = 2000, EM coverage radius 4 km	Main analysis
Cutoff 2010	T = 2010	Pre-specified robustness
1 km grid / 5 km grid	cell size	Pre-specified robustness
EM 2 km / EM 8 km	radius used to infer EM survey coverage	Pre-specified robustness
Inputs < 1998	predictors limited to records dated before 1998; labels still split at 2000	Post hoc sensitivity, from the leakage audit
Worked showings only	restricted label set (Section 2.2)	Post hoc sensitivity, from expert feedback

Inputs < 1998 (publication lag). Restricting predictors to records dated before 1998 tests sensitivity to a two-year temporal buffer. It reduces exposure to short publication delays but does not exclude leakage from records released more than two years after the underlying work.

Worked showings only is the restricted label set defined in Section 2.2, run with every other setting as in the main analysis.

3. Results

Across the eight analysis settings for each commodity, spatial cross-validation overestimated the forward capture of graphite prospectivity models. Where graphite discoveries moved far from earlier exploration (the 2000 cutoff), the overestimate was largest for the proximity-to-known-occurrences baseline. For nickel, where post-cutoff discoveries stayed near established camps, cross-validation and forward performance agreed more closely. CV optimism and the poor transfer of proximity were robust to the settings tested. Which model CV would select was not: it changed when never-worked showings were excluded.

3.1 Where new discoveries were made

New graphite discoveries after 2000 were made a median of 88 km from the nearest pre-2000 graphite occurrence, and 82% were more than 20 km away. New nickel discoveries were a median of 14 km from known nickel, and 42% were more than 20 km away.

Table 6. Distance from post-cutoff discovery cells to the nearest earlier occurrence.

Commodity, cutoff	Test cells	Median distance to nearest earlier occurrence	Share more than 20 km away
Graphite, 2000	89	88 km	82%
Nickel, 2000	144	14 km	42%
Graphite, 2010	72	18 km	49%
Nickel, 2010	67	15 km	42%

At the 2000 cutoff, half of the new graphite discovery cells (44 of 89) were “frontier” discoveries, meaning there had been no exploration within 10 km before 2000 (defined in Section 3.5). For nickel the figure was one in five (29 of 144).

Graphite exploration therefore moved into new ground after 2000, while nickel exploration stayed close to known camps. After 2010 the graphite picture changes: new discoveries lay a median of only 18 km from earlier ones. We did not examine individually whether these were follow-up finds around districts opened in the 2000s.

Geologically, the move was mainly geographic rather than into new rock types. Of the graphite training cells, 94% lie in the Grenville Province (GSC Map D1860A); of the post-2000 test cells, 88% do, with most of the remainder (10%) in the Churchill Province. Paragneiss hosts 49% of training cells and 34% of test cells, and undivided gneiss 15% and 39%; only 8% of test cells fall in a map rock type with no training cell. Nickel training and test cells are spread over the Grenville, Superior and Churchill provinces in broadly similar proportions, and every nickel test cell lies in a rock type represented in training. At the national map’s 1:5,000,000 scale, therefore, later graphite discoveries mostly occupied the same geological settings as earlier ones, but in different places.

This gives two contrasts for the tests that follow. One is between minerals at the same cutoff; the other is within graphite, between the two cutoffs.

3.2 Cross-validation versus the forward test

Spatial cross-validation overstated graphite forward capture for 6 of 7 models with the primary labels and for all 7 with the restricted labels (Table 7; Fig. 1). The proximity baseline R0 had the largest overestimate under both label sets: 0.68 in CV against 0.26 forward with the primary labels, and 0.82 against 0.35 with the restricted labels.

R0 ranked 6th of 7 in the forward test both times. However, its margin over the last-placed model (R1 at 0.25 with the primary labels; M4-A at 0.31 with the restricted labels) was well within the uncertainty.

The proximity result does not depend on the particular partition or on tuning (**post hoc**). With cluster-bootstrap intervals, R0's cross-validated capture was 0.68 (0.56–0.81) against 0.26 (0.16–0.38) forward: the intervals do not overlap. Across ten further random partitions, R0 scored 0.63–0.79 in cross-validation and was the top cross-validation model in nine of the ten; in every partition, cross-validation exceeded forward capture for at least six of the seven graphite models (Supplementary Table S2). For the other graphite models the cross-validation and forward intervals overlap, so their individual overestimates are not statistically clear. The cross-validated values of the two effort-fixed models were also sensitive to the partition: M4-A scored 0.28 in the frozen partition but 0.33–0.58 in the others.

Which model CV would have *chosen* depends on the labels:

- **Primary labels.** CV selected R0, but the CV and forward rankings were weakly and negatively associated (Spearman -0.18).
- **Restricted labels.** CV selected M1-C, which came 2nd forward, behind M1-B (Spearman $+0.43$).

Table 7. Graphite: cluster-weighted capture of post-2000 discovery cells in the top 5% of Quebec (logistic regression, 2 km grid). Spatial CV uses 50 km blocks. Forward values are shown with 95% cluster-bootstrap intervals. Bold marks the highest point estimate in each column. Most intervals overlap, so the forward ranking is uncertain.

Model	Primary labels: CV	Primary labels: forward	Restricted labels: CV	Restricted labels: forward
R0 proximity	0.68	0.26 (0.16–0.38)	0.82	0.35 (0.15–0.60)
R1 effort only	0.47	0.25 (0.15–0.37)	0.60	0.40 (0.20–0.60)
M1-A map	0.56	0.36 (0.24–0.50)	0.70	0.47 (0.29–0.67)
M1-B + local	0.55	0.33 (0.22–0.47)	0.73	0.60 (0.40–0.80)
M1-C + effort	0.60	0.37 (0.25–0.51)	0.89	0.56 (0.36–0.77)

Model	Primary labels: CV	Primary labels: forward	Restricted labels: CV	Restricted labels: forward
M4-A map, effort fixed	0.28	0.40 (0.27–0.53)	0.55	0.31 (0.13–0.50)
M4-C all, effort fixed	0.45	0.32 (0.21–0.46)	0.84	0.40 (0.20–0.60)

The forward ranking of the effort-fixed map model M4-A illustrates how much the labels matter: it had the highest forward capture with the primary labels (0.40) and the lowest with the restricted labels (0.31).

With the primary labels, a paired comparison (**post hoc**) shows M4-A beating R0 by +0.14 (95% CI 0.04 to 0.25). This comparison does not carry over to the restricted labels, where the difference was –0.05 (–0.27 to +0.16).

The forward intervals of most graphite models overlap. The forward ranking is therefore uncertain apart from the paired comparisons reported here and in Section 3.4.

For nickel (Table 8; Fig. 1), spatial CV exceeded forward capture for 4 of 7 models in the main analysis, and its ranking broadly agreed with the forward test (Spearman 0.64). CV’s top model, M1-C, came 2nd forward, 0.01 behind R0. Across the ten further partitions, nickel Spearman values were +0.54 to +0.71, compared with –0.32 to +0.16 for graphite, and M1-C was always the top cross-validation model (2nd forward). Among nickel models, the cross-validation estimate was clearly above the forward interval only for R1 and M1-C. The nickel results for the restricted labels are summarised in Section 3.3.

Table 8. Nickel, primary labels: capture at 5%, as in Table 7.

Model	Spatial CV	Forward
R0 proximity	0.39	0.43 (0.34–0.52)
R1 effort only	0.57	0.30 (0.22–0.39)
M1-A map	0.34	0.36 (0.27–0.45)
M1-B + local	0.40	0.41 (0.32–0.50)
M1-C + effort	0.66	0.42 (0.32–0.51)
M4-A map, effort fixed	0.28	0.27 (0.18–0.35)
M4-C all, effort fixed	0.31	0.30 (0.21–0.39)

Gradient boosting did not change the picture. For graphite, boosted M1-C scored 0.70 in cross-validation and 0.32 forward, the second-largest gap of all model–learner pairs.

3.3 Robustness across eight settings

The result most robust to the analysis settings is that cross-validation is optimistic. Graphite spatial CV exceeded forward capture for at least 6 of 7 models in every setting, and nickel CV for 2–5 of 7. Whether CV also *chose the wrong model* depended on how far exploration had moved, and on the label definition.

Table 9. Agreement between spatial CV (50 km) and the forward test, capture at 5% (Fig. 3A). The settings share most of their data and are related sensitivity analyses, not independent replications.

Setting	Mineral	Median km to earlier finds	Spearman (CV vs forward)	Forward rank of CV's top model (of 7)	Models where CV > forward
Main	Graphite	88	−0.18	6 (R0)	6
1 km grid	Graphite	85	−0.58	6 (R0)	7
5 km grid	Graphite	93	+0.07	4 (R0)	6
EM 2 km	Graphite	88	−0.18	6 (R0)	6
EM 8 km	Graphite	88	−0.04	6 (R0)	6
Inputs < 1998	Graphite	88	−0.09	6 (R0)	6
Cutoff 2010	Graphite	18	+0.79	1 (R0)	7
Worked showings	Graphite	209	+0.43	2 (M1-C)	7
Main	Nickel	14	+0.64	2 (M1-C)	4
1 km grid	Nickel	9	+0.54	2 (M1-C)	3
5 km grid	Nickel	19	+0.43	3 (M1-C)	4
EM 2 km	Nickel	14	+0.71	2 (M1-C)	4
EM 8 km	Nickel	14	+0.64	2 (M1-C)	4
Inputs < 1998	Nickel	14	+0.46	3 (M1-C)	3
Cutoff 2010	Nickel	15	+0.77	1 (M1-C)	5
Worked showings	Nickel	14	+0.57	3 (M1-C)	2

Three patterns stand out.

1. **Graphite at the 2000 cutoff, primary labels (six settings).** In all six, CV ranked R0 first, and R0 came 4th–6th in the forward test. Spearman ranged from −0.58 to +0.07. Changing the grid size, the EM radius or the input dates (the publication-lag check) did not change this.

2. **When discoveries stayed near earlier ones, CV ranked the models well.** This holds for graphite after 2010 (median 18 km; Spearman +0.79) and for nickel in every setting (+0.43 to +0.77). In these cases CV's top model came 1st–3rd in the forward test. The contrast within graphite between the two cutoffs is consistent with exploration shift as the mechanism. However, it rests on two cutoffs and two minerals, and it was identified after the results were seen.
3. **Worked showings only (post hoc).** CV's top graphite model was M1-C (0.89), not R0, and M1-C came 2nd forward (0.56; M1-B was best at 0.60). R0 still had the largest overestimate, from 0.82 in CV to 0.35 forward (6th of 7). The median distance from discovery cells to earlier graphite rose from 88 km to 209 km, but this reflects the smaller reference set rather than a move of the test set: the restricted test cells lie a median 90 km from all pre-2000 graphite occurrences, against 88 km for the primary test cells, whereas the primary test cells lie 198 km from the restricted training occurrences. With only 20 graphite discovery clusters, the intervals are wide (R0 forward 0.15–0.60).

The claim that CV picks the wrong graphite model therefore depends on including occurrences with no recorded work. Two narrower findings held more widely:

- **CV was optimistic for graphite in every setting**, overstating 6–7 of 7 models. For nickel it overstated only 2–5 of 7, so optimism was not universal there.
- **For graphite at the 2000 cutoff, the proximity baseline had the largest CV–forward gap.** This held in all seven graphite settings at the 2000 cutoff, including worked showings only, with gaps of 0.37–0.47. It did not hold for graphite after 2010 or for nickel. There the largest gaps belonged to M1-B, M1-C or R1 (0.21–0.40), and R0 sometimes did better forward than in CV.

3.4 Effect of effort-fixed prediction

Effort-fixed prediction never clearly improved forward capture for either mineral (Fig. 3B). For nickel it lowered capture in all 16 comparisons (8 settings × 2 models), and the 95% interval excluded zero in 14; the closest case, inputs before 1998 (map model), had an upper bound of −0.002. The 16 comparisons share data and are not independent experiments. For graphite the effect was neutral to negative.

Table 10. Paired difference in forward capture at 5% when effort is fixed at its median (M4 minus M1), with 95% intervals (**post hoc**). Unrounded endpoints are given in Supplementary Table S4.

Setting	Graphite, map model (M4-A – M1-A)	Graphite, full model (M4-C – M1-C)	Nickel, map model	Nickel, full model
Main	+0.04 (0.00, 0.09)	−0.05 (−0.11, 0.00)	−0.10 (−0.18, −0.01)	−0.12 (−0.20, −0.04)

Setting	Graphite, map model (M4-A – M1-A)	Graphite, full model (M4-C – M1-C)	Nickel, map model	Nickel, full model
Cutoff 2010	0.00 (0.00, 0.00)	-0.08 (-0.15, -0.01)	-0.20 (-0.33, -0.07)	-0.24 (-0.37, -0.12)
1 km grid	0.00 (0.00, 0.00)	-0.08 (-0.16, -0.02)	-0.06 (-0.15, +0.03)	-0.11 (-0.20, -0.02)
5 km grid	+0.07 (-0.03, +0.17)	-0.11 (-0.23, 0.00)	-0.15 (-0.26, -0.06)	-0.04 (-0.14, +0.05)
EM 2 km	+0.04 (0.00, 0.09)	-0.05 (-0.11, 0.00)	-0.10 (-0.18, -0.01)	-0.13 (-0.21, -0.06)
EM 8 km	+0.02 (0.00, 0.06)	-0.06 (-0.16, +0.02)	-0.10 (-0.18, -0.01)	-0.12 (-0.20, -0.04)
Inputs < 1998	+0.05 (0.00, 0.11)	-0.09 (-0.17, -0.02)	-0.09 (-0.17, 0.00)	-0.09 (-0.17, -0.02)
Worked showings	-0.17 (-0.36, 0.00)	-0.17 (-0.38, +0.04)	-0.13 (-0.25, -0.03)	-0.11 (-0.22, -0.01)

For the graphite map model, fixing effort gave small gains of 0.00 to +0.07 under the original labels. No interval lay clearly above zero; several have a lower bound that rounds to 0.00. Under worked-showings labels the effect turned negative (-0.17). For the graphite full model, fixing effort lowered capture in every setting, and the interval excluded zero in 3 of 8.

H2 was not supported as originally formulated. Effort-fixed prediction did not clearly improve graphite forward capture in any setting, and it consistently reduced nickel forward capture. The contrast between the minerals therefore reflects a strong negative effect of effort-fixed prediction for nickel, not a robust benefit for graphite.

In the main analysis, M4-A beat the proximity baseline by +0.14 (0.04–0.25). This advantage was clear in 4 of the 6 graphite settings at the 2000 cutoff. It was not clear at 1 km (+0.04, -0.07 to +0.15) or 5 km (+0.11, -0.02 to +0.25).

3.5 Frontier discoveries and case studies

Discoveries in cells without recorded prior exploration were rarely ranked highly by any model. Absence of a dated record does not prove that nobody explored a cell, so “frontier” is used here as shorthand for an operational definition: a post-cutoff discovery cell counts as a frontier discovery if, before the cutoff, there was no exploration within 10 km and the cell lay outside the inferred EM survey coverage. Exploration here means drilling, government mapping, compilation outcrops or showings of other commodities. In the main analysis the EM condition made no difference: every discovery cell with no exploration within 10 km was also outside EM coverage (44 graphite and 29 nickel cells either way).

In the main analysis the 44 frontier graphite cells form 22 discovery clusters, and the 29 frontier nickel cells 23 clusters. Frontier cells keep the weights of their full discovery clusters (two graphite clusters contain both frontier and non-frontier cells). The model with the highest frontier capture was M4-A for both commodities: 0.14 (95% interval 0.03–0.28) for graphite and 0.13 (0.00–0.30) for nickel. Random selection of 5% of cells would capture 0.05 in expectation, which lies inside both intervals. R0 and R1 captured none, as expected, since frontier cells lie away from known occurrences and recorded effort by definition (Supplementary Table S3). Across the primary-label settings, the highest frontier capture by any model was 0–18% for graphite and 0–13% for nickel. The one exception was worked showings only, at 41% for graphite, but that figure rests on 17 cells. Both cutoff-2010 runs captured none of their frontier discoveries (15 graphite and 6 nickel cells).

No model, with or without effort-fixed prediction, ranked frontier discoveries clearly better than random selection of the same area.

Case studies (post hoc). Three major projects discovered after 2000 lie in cells the models could score: Uatnan and Matawinie for graphite, and Nunavik Nickel. The table gives the percentile rank of each project’s cell in the main analysis, where 100 is the highest-ranked cell in Quebec.

Table 11. Percentile rank of each project’s cell in the main analysis (2 km, T = 2000). Percentile ranks are oriented so that 100 is the highest-ranked cell in Quebec.

Project	R0	R1	M1-A	M1-C	M4-A
Uatnan (graphite)	83.6	90.1	97.6	98.3	97.6
Matawinie (graphite)	93.5	30.9	94.7	90.0	95.5
Nunavik Nickel	99.9	99.5	99.7	100.0	98.9

The geology-based models placed both graphite projects in roughly the top 2–10% of Quebec. For Uatnan they ranked the project above the proximity baseline (83.6); for Matawinie the proximity baseline also ranked it highly (93.5), above M1-C. But the top 5% of Quebec is still about 75,000 km² (5% of 372,900 cells of 4 km²). These cases show that the models ranked the projects highly; they do not show that the models would have guided exploration to them.

Several other well-known projects (Lac Knife, La Loutre, Lac-des-Îles, Raglan, Dumont) were already known before 2000. They are therefore training cells, not tests.

4. Discussion

4.1 Principal findings

Forward testing against dated discoveries gave a less favourable picture of prospectivity models than spatial cross-validation did. Three results held across the settings tested:

- **CV optimism.** Spatial CV overestimated how well graphite models ranked post-cutoff discovery cells.
- **Proximity transferred poorly.** Where graphite exploration moved into new ground, the proximity-to-known-occurrences baseline lost more than any other model between CV and the forward test.
- **Weak frontier performance.** No model ranked discoveries in cells without recorded prior exploration clearly better than random selection.

Which model CV would *select* was less stable, and depended on how low-evidence showings were labelled.

Against the pre-specified hypotheses: **H1** was supported (CV was optimistic for 6–7 of 7 graphite models in every setting, but for only 2–5 of 7 nickel models). **H2** was not supported (Section 3.4). **H3** was not supported: the CV–forward gap of the map-plus-local-data model (M1-B) was similar to that of the map-only model (M1-A) in most settings (for graphite at the main setting, 0.22 against 0.20), and the largest gap belonged to proximity, which uses no exploration-dependent inputs. **H4** was supported with the primary labels at the 2000 cutoff (Spearman -0.58 to $+0.07$) but not with the restricted labels or the 2010 cutoff.

4.2 Why cross-validation is optimistic

Two mechanisms plausibly contribute.

1. **Spatial autocorrelation.** Spatial blocks reduce, but do not remove, the similarity between training and evaluation cells. Known occurrences cluster, so a held-out block often still lies near training occurrences in the same district.
2. **Overlap between predictors and labels.** Graphite is recorded in drill logs or outcrop descriptions in 28% of known graphite cells, against 0.9% of Quebec. For models that use these observation predictors (M1-B, M1-C, M4-C), CV can partly reward recognising graphite evidence that was already documented. The forward test is not affected, because predictors and labels are separated in time.

The overlap cannot be the whole explanation. R0, R1 and M1-A use no observation predictors, yet they were also overestimated, and R0 most of all. Clustering of known occurrences around past exploration is a plausible, more general contributor. Together, these mechanisms mean that

spatial CV can estimate performance within the historical exploration footprint while giving little guidance about performance beyond it.

4.3 Why proximity transfers poorly

A known occurrence marks a place where someone explored and documented mineralisation, as well as a place with favourable geology. Proximity to known occurrences therefore encodes the exploration footprint as much as discovery potential. CV rewards this because held-out occurrences sit inside the same footprint.

In the forward test, proximity was competitive only where discoveries stayed near earlier ones. Where they did (nickel at both cutoffs, graphite after 2010), R0 was competitive: it gave the best nickel forward capture at the 2000 cutoff and was the forward winner for graphite after 2010. When graphite discoveries moved a median of 88 km from earlier ones, R0 fell to 6th of 7.

4.4 Why effort-fixed prediction did not help

Fixing exploration effort at a common value when scoring removes the cell-to-cell contribution of the effort terms; it was intended to reduce the advantage a cell gains simply from having been explored. For nickel, this consistently reduced forward capture. Where future exploration stays near past exploration, effort can carry information in at least three ways. It can act as a proxy for favourable geology the other inputs miss, since drilling and mapping go where geology looks promising. More work raises the chance that mineralisation is found and documented. And exploration clusters in space and time, so past effort predicts future effort. The forward test cannot distinguish these roles; it shows only that fixing effort reduced ranking performance for nickel. For graphite, it gave no clear gain in any setting. This specific procedure is therefore not a general correction for exploration bias. Its value depends on whether future exploration repeats the past.

4.5 Why label construction matters

Excluding never-worked showings changed more than the sample size. It removed many of the earlier reference occurrences (raising the median distance from discoveries to earlier graphite from 88 to 209 km), changed which model CV selected, and moved M4-A from best to last in the forward test. National prospectivity datasets typically combine occurrence records of very uneven quality. These results suggest that the label definition should be reported explicitly and varied in sensitivity analyses, because it can change model-selection conclusions and not only performance estimates. Label construction thus joins the workflow choices already known to change prospectivity maps (Zhang et al. 2024).

4.6 What the models represent

Taken together, these results are consistent with the view that a model trained only on historical occurrence records is not a neutral geological map: it may partly learn where past exploration

and documentation took place, conditioned on geology. The analysis cannot fully separate exploration history from geological differences, data availability and changing targets, but the geological comparison in Section 3.1 suggests that later graphite discoveries mostly occupied the same map units as earlier ones, in different places. Discovery dates and exploration intent are therefore part of the data-generating process, not neutral metadata. The frontier result is consistent with this. The training data contain little information about ground no one had explored, and across the primary-label settings no model captured more than 18% of such graphite discoveries.

4.7 Practical implications

- Where discovery dates are available, evaluate prospectivity models with a forward (temporal) test as well as spatial CV, and report both.
- Report how far test discoveries lie from training discoveries. With the primary labels in this study, that distance separated the cases where CV ranked models well (median 9–19 km) from those where it did not (85–93 km). It is a diagnostic, not a rule: with the restricted labels, graphite discoveries lay a median of 209 km from the restricted training occurrences, yet CV’s top model came 2nd forward.
- Treat strong CV performance from proximity or effort-based predictors with caution when exploration is expanding into new regions.
- Do not assume that fixing effort variables at prediction time corrects for exploration bias. Test it forward.

4.8 Limitations

- **Exploration intent is not recorded.** The shift in graphite discoveries may reflect changing targets, such as demand for battery minerals, as much as model failure.
- **Discovery dates are imperfect.** The date an occurrence becomes significant is hard to define, and the discovery year may record first documentation rather than first observation. It was never later than the database entry year, and equal to it in 6% of records.
- **Samples are small.** There are 50 graphite and 102 nickel discovery clusters at the main cutoff, and 20 graphite clusters with the restricted labels. Most single-model intervals overlap.
- **The comparison is narrow.** Two commodities and two cutoffs were studied, in one jurisdiction. The exploration-shift explanation was identified after the results were seen and has not been tested on an independent commodity or region.
- **Publication lag** was tested with a single two-year buffer, and database dates are activity dates, not public-release dates.

- **Cross-validation tuning:** C was tuned on the reported 50 km folds; repeated partitions not used for tuning gave the same conclusions.
- **EM survey coverage** is inferred from buffers around reported anomalies, not from survey outlines, and surveys were not separated by the commodity they targeted. Coverage therefore reflects electromagnetic exploration in general, not graphite-specific effort; the source reports linked to each anomaly could be used to recover survey targets in future work.
- **The background sample** was a fixed random draw, shared by all models within an analysis. Sensitivity of forward capture to the size and spatial distribution of the background sample was not assessed. This may affect absolute capture values; whether it affects model order is untested.

5. Conclusions

Using discovery dates to run a retrospective forward test, we found that spatial cross-validation overestimated how well prospectivity models ranked later graphite discoveries in Quebec, and that the proximity-to-known-occurrences baseline transferred worst where discoveries moved far from earlier ones. Where nickel discoveries stayed near established camps, cross-validation was a reasonable guide. Which model cross-validation would select depended on how low-evidence showings were labelled, fixing exploration effort at prediction time did not help, and no model ranked discoveries in cells without recorded prior exploration clearly better than random. Forward tests on dated discoveries are a practical complement to spatial cross-validation and should be reported alongside it.

Figures

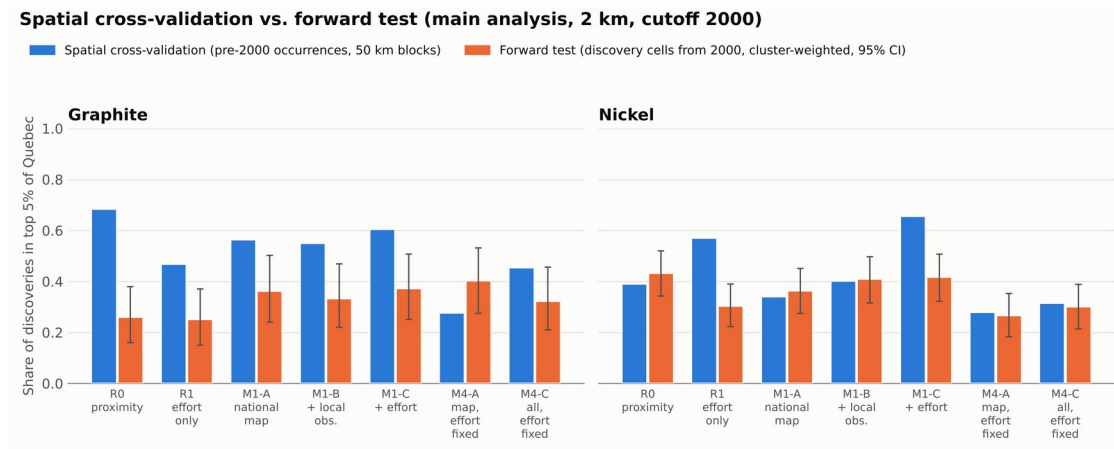


Figure 1. Spatial cross-validation (50 km blocks) versus forward-test capture at 5% for each model, for graphite (left) and nickel (right), main analysis (2 km grid, cutoff 2000). Error bars are 95% cluster-bootstrap intervals for the forward test.

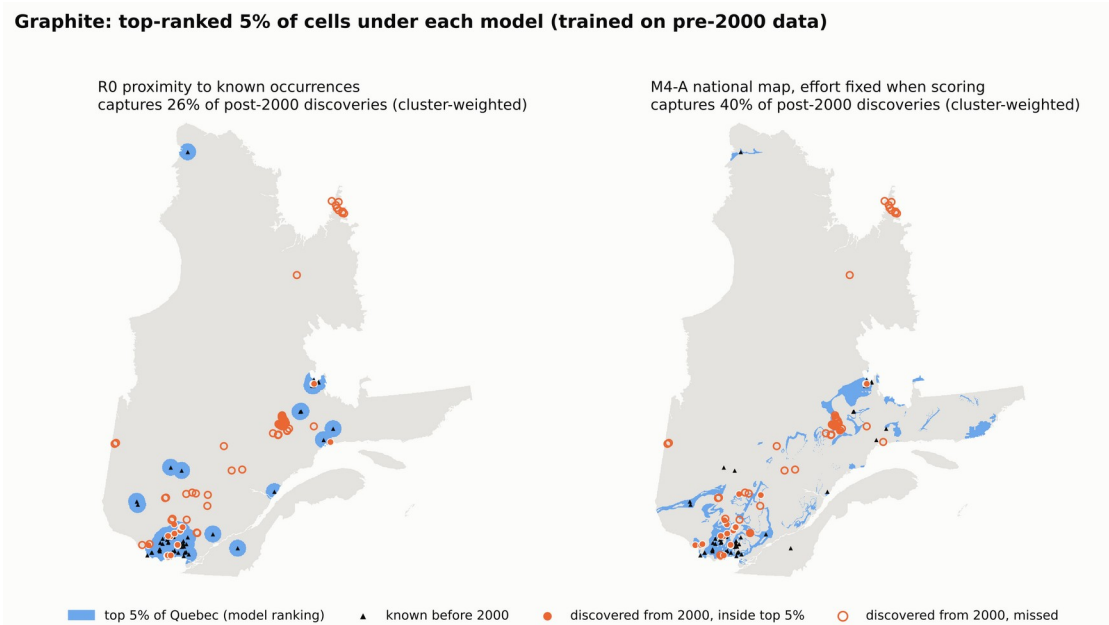


Figure 2. Graphite: cells in the top 5% of Quebec for the proximity baseline (R0, left) and the effort-fixed map model (M4-A, right) trained on pre-2000 data, with occurrences known before 2000 and discoveries made from 2000 onward (filled: inside the top 5%; open: missed).

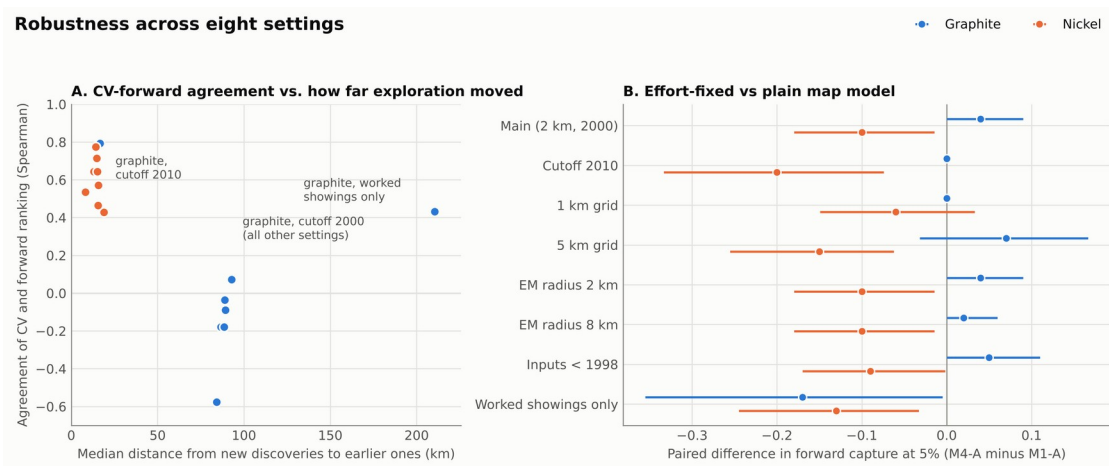


Figure 3. Robustness across eight related settings (sensitivity analyses sharing most of their data, not independent replications). (A) Agreement between cross-validation and forward rankings (Spearman) against the median distance from new discoveries to earlier ones. (B) Paired difference between the effort-fixed and plain map models (M4-A minus M1-A), which also differ in training inputs (Section 2.4), in each setting, with 95% intervals.

Data and code availability

All mineral, drilling, outcrop and geophysical records are publicly available from SIGEOM (Ministère des Ressources naturelles et des Forêts, Québec). The national geology map is GSC Map D1860A.

The analysis design, model specifications, predictor definitions and evaluation metrics were frozen before the initial forward test. The frozen design document, analysis code, model table and output files are archived with SHA-256 hashes at Zenodo (<https://doi.org/10.5281/zenodo.23684017>). Post-hoc analyses are identified as such in the text and were not used to change the pre-specified hypotheses.

Statements and Declarations

Funding. No funding was received for this work.

Competing interests. The author has no competing interests to declare.

Author contributions. M.R.V.R. conceived the study, acquired the data, ran the analyses, interpreted the results and wrote the manuscript.

Use of AI tools. AI tools (Claude, Anthropic) were used substantially for writing analysis code, running the analysis pipeline under the author's direction, and drafting and editing the manuscript. All analysis decisions, interpretations and final text were reviewed and approved by the author, who takes full responsibility for the content.

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Supplementary Material

Forward-testing mineral prospectivity models with discovery dates: Quebec graphite and nickel

Maheswara Reddy Vallabha Reddy, Independent Researcher, Sheffield, United Kingdom

Analyses in Sections S5–S7 were added after the forward test, in response to review comments (post hoc). They read the frozen outputs or refit models with the frozen settings; they did not replace any frozen result.

S1. Dating and exclusion rules

- **Drilling.** Dated by drilling year; holes with the unknown-year code 9999 are excluded.
- **Field observations.** Dated by observation date.
- **Compilation outcrops.** Dated by the year in the report number; assessment reports are dated by their drilling year (all at least two years before the cutoff). About 75% of compilation outcrops are undated and excluded, which under-counts pre-cutoff mapping (noise, not leakage).
- **EM anomalies.** Dated by report year; 22 undated old reports are excluded.
- **Labels.** Undated occurrences (22 graphite, 1 nickel) are excluded from training and testing.
- **Dates are activity or report dates.** SIGEOM does not record public-release dates; see main text Section 2.3.

20 A per-variable provenance and leakage audit (`predictor_audit.md`) is included in the code
21 archive.

22 **S2. Predictors, missing values and learner settings**

- 23 • **Predictor counts.** Before removing duplicated columns, the graphite input sets held 43
24 (map), 108 (map plus local data), 117 (all inputs) and 9 (effort) columns. Exactly
25 duplicated columns were removed once for the whole table (a check that uses no labels),
26 leaving 42, 78, 87 and 9 inputs for graphite and 42, 76, 85 and 9 for nickel. Nickel
27 models do not use the graphite-observation (OBS) inputs.
- 28 • **Missing values.** Where a cell has no local observation, rock-type fractions and EM
29 conductor counts are set to zero and a separate missing-indicator column (MISS) is
30 added; a few continuous inputs are filled with their median before log transformation.
31 EM conductor counts are therefore never treated as zero without a matching “not
32 surveyed” indicator.
- 33 • **Scaling.** Inputs are standardised with a scaler fitted on the training rows of each fit (each
34 CV split, and the forward training set).
- 35 • **Logistic regression:** L2 penalty, C chosen from {0.01, 0.1, 1} by 50 km spatial block
36 CV (ties broken towards stronger regularisation), balanced class weights. Selected C:
37 0.01 for all models except graphite M4-A (0.1).
- 38 • **Histogram gradient boosting:** maximum depth 3, learning rate 0.05, 200 iterations, at
39 least 20 samples per leaf, balanced class weights.
- 40 • **Software:** Python 3.13, scikit-learn 1.9, pandas 3.0, NumPy 2.5, SciPy 1.18, pyproj 3.8,
41 shapely 2.2, matplotlib 3.11.

42 S3. Reproducibility safeguards and implementation

- 43 • **Frozen design.** The design document (sha256 e22ceb54...) and frozen model list record
44 the sha256 of the model table used (3dfa0cd1...).
- 45 • **Forward test.** The forward-test script checks this hash and asks for explicit confirmation
46 before running again. Repeated runs are deterministic (fixed seeds); the safeguard is there
47 to keep the frozen outputs traceable, not because a re-run would change them.
- 48 • **Restored outputs.** After the forward test, a re-run of the cross-validation script
49 overwrote its output files. The original files were restored and kept as the reference
50 outputs; the re-run outputs were kept separately for comparison; and the script was
51 changed to write any re-run under a separate name. The post-hoc re-computation in S5
52 reproduced the frozen cross-validated capture of every main model to two decimals.
- 53 • **1 km runs.** Because of memory limits, these used compact data types and chunked
54 scoring. Their scores match the standard code to within 3×10^{-11} .

55 S4. Metrics

- 56 • **Capture at p%:** cluster-weighted share of target cells among the top p% of the ranking
57 universe (main text Table 4); $p = 0.5, 1, 5$ and 10 .
- 58 • **Clusters:** single-linkage (transitive) groups of target cells within 5 km; weight $1 / \text{cluster}$
59 size.
- 60 • **ROC AUC:** scikit-learn `roc_auc_score` over the ranking universe, with each target
61 cell weighted by its cluster weight and each other cell by 1.
- 62 • **Ties:** tied scores share their average rank.

63 S5. Cross-validation uncertainty and partition sensitivity (post hoc)

64 Table S1. Pre-2000 occurrence cells in each 50 km cross-validation fold (frozen partition).

Mineral	Fold 1	Fold 2	Fold 3	Fold 4	Fold 5
Graphite	13	10	31	31	10
Nickel	68	65	32	43	35

65 Table S2. Spatial CV capture at 5% (50 km blocks, frozen C) with cluster-bootstrap intervals, the
66 range across the five folds, and the mean and range over ten further random block-to-fold
67 assignments not used for tuning, compared with the forward test.

Mineral	Model	CV, frozen partition (95% CI)	Per-fold range	CV, 10 further partitions: mean (range)	Forward (95% CI)
Graphite	R0	0.68 (0.56–0.81)	0.10–1.00	0.72 (0.63–0.79)	0.26 (0.16–0.38)
Graphite	R1	0.47 (0.33–0.60)	0.27–0.55	0.48 (0.39–0.53)	0.25 (0.15–0.37)
Graphite	M1-A	0.56 (0.44–0.69)	0.13–1.00	0.54 (0.51–0.56)	0.36 (0.24–0.50)
Graphite	M1-B	0.55 (0.42–0.69)	0.33–0.68	0.58 (0.45–0.64)	0.33 (0.22–0.47)
Graphite	M1-C	0.60 (0.48–0.73)	0.33–0.70	0.65 (0.51–0.71)	0.37 (0.25–0.51)
Graphite	M4-A	0.28 (0.17–0.40)	0.07–0.44	0.49 (0.33–0.58)	0.40 (0.27–0.53)
Graphite	M4-C	0.45 (0.34–0.59)	0.10–0.52	0.57 (0.45–0.64)	0.32 (0.21–0.46)
Nickel	R0	0.39 (0.32–0.47)	0.28–0.52	0.38 (0.35–0.41)	0.43 (0.34–0.52)
Nickel	R1	0.57 (0.49–0.65)	0.44–0.70	0.54 (0.53–0.56)	0.30 (0.22–0.39)
Nickel	M1-A	0.34 (0.27–0.42)	0.16–0.43	0.34 (0.30–0.37)	0.36 (0.27–0.45)
Nickel	M1-B	0.40 (0.32–0.48)	0.16–0.51	0.39 (0.36–0.41)	0.41 (0.32–0.50)
Nickel	M1-C	0.66 (0.58–0.72)	0.52–0.78	0.65 (0.62–0.68)	0.42 (0.32–0.51)
Nickel	M4-A	0.28 (0.21–0.35)	0.09–0.39	0.24 (0.18–0.28)	0.27 (0.18–0.35)
Nickel	M4-C	0.31 (0.24–0.39)	0.09–0.48	0.31 (0.27–0.35)	0.30 (0.21–0.39)

68 S6. Frontier discoveries (post hoc)

69 Table S3. Capture at 5% of frontier discoveries (no recorded exploration within 10 km before
70 2000 and outside inferred EM coverage), main analysis. Intervals: 1,000 bootstrap resamples of
71 frontier clusters. Random selection of 5% of cells captures 0.05 in expectation.

Mineral	Model	Frontier cells	Frontier clusters	Capture (95% CI)
Graphite	R0	44	22	0.00 (0.00–0.00)
Graphite	R1	44	22	0.00 (0.00–0.00)
Graphite	M1-A	44	22	0.07 (0.00–0.18)
Graphite	M1-B	44	22	0.02 (0.00–0.07)
Graphite	M1-C	44	22	0.02 (0.00–0.06)
Graphite	M4-A	44	22	0.14 (0.03–0.28)
Graphite	M4-C	44	22	0.02 (0.00–0.07)
Nickel	R0	29	23	0.00 (0.00–0.00)
Nickel	R1	29	23	0.00 (0.00–0.00)
Nickel	M1-A	29	23	0.09 (0.00–0.22)
Nickel	M1-B	29	23	0.09 (0.00–0.22)
Nickel	M1-C	29	23	0.04 (0.00–0.13)
Nickel	M4-A	29	23	0.13 (0.00–0.30)
Nickel	M4-C	29	23	0.09 (0.00–0.22)

72 S7. Paired differences, unrounded (post hoc)

73 Table S4. Paired differences in forward capture at 5% (1,000 cluster-bootstrap resamples). M4-A
74 – M1-A and M4-C – M1-C are the effort comparisons in main text Table 10; M4-A – R0 is the
75 comparison in Section 3.2.

Setting	Mineral	Comparison	Mean	2.5%	97.5%
Main	Graphite	M4-A-M1-A	+0.0396	+0.0000	+0.0900
Main	Graphite	M4-C-M1-C	-0.0512	-0.1100	+0.0000
Main	Graphite	M4-A-R0	+0.1394	+0.0400	+0.2461

Setting	Mineral	Comparison	Mean	2.5%	97.5%
Main	Nickel	M4-A-M1-A	-0.0966	-0.1798	-0.0146
Main	Nickel	M4-C-M1-C	-0.1168	-0.1953	-0.0416
Main	Nickel	M4-A-R0	-0.1664	-0.2794	-0.0539
Cutoff 2010	Graphite	M4-A-M1-A	+0.0000	+0.0000	+0.0000
Cutoff 2010	Graphite	M4-C-M1-C	-0.0751	-0.1489	-0.0106
Cutoff 2010	Graphite	M4-A-R0	-0.0930	-0.2300	+0.0426
Cutoff 2010	Nickel	M4-A-M1-A	-0.2006	-0.3333	-0.0741
Cutoff 2010	Nickel	M4-C-M1-C	-0.2392	-0.3706	-0.1204
Cutoff 2010	Nickel	M4-A-R0	-0.2375	-0.3889	-0.0926
1 km grid	Graphite	M4-A-M1-A	+0.0000	+0.0000	+0.0000
1 km grid	Graphite	M4-C-M1-C	-0.0839	-0.1635	-0.0189
1 km grid	Graphite	M4-A-R0	+0.0418	-0.0734	+0.1546
1 km grid	Nickel	M4-A-M1-A	-0.0590	-0.1491	+0.0330
1 km grid	Nickel	M4-C-M1-C	-0.1142	-0.1991	-0.0245
1 km grid	Nickel	M4-A-R0	-0.1924	-0.3010	-0.0849
5 km grid	Graphite	M4-A-M1-A	+0.0668	-0.0317	+0.1667
5 km grid	Graphite	M4-C-M1-C	-0.1110	-0.2331	+0.0050
5 km grid	Graphite	M4-A-R0	+0.1088	-0.0240	+0.2510
5 km grid	Nickel	M4-A-M1-A	-0.1532	-0.2552	-0.0625
5 km grid	Nickel	M4-C-M1-C	-0.0422	-0.1406	+0.0469
5 km grid	Nickel	M4-A-R0	-0.2664	-0.3751	-0.1509
EM 2 km	Graphite	M4-A-M1-A	+0.0396	+0.0000	+0.0900
EM 2 km	Graphite	M4-C-M1-C	-0.0512	-0.1100	+0.0000
EM 2 km	Graphite	M4-A-R0	+0.1394	+0.0400	+0.2461
EM 2 km	Nickel	M4-A-M1-A	-0.0966	-0.1798	-0.0146
EM 2 km	Nickel	M4-C-M1-C	-0.1296	-0.2075	-0.0605
EM 2 km	Nickel	M4-A-R0	-0.1664	-0.2794	-0.0539
EM 8 km	Graphite	M4-A-M1-A	+0.0211	+0.0000	+0.0600
EM 8 km	Graphite	M4-C-M1-C	-0.0623	-0.1550	+0.0203
EM 8 km	Graphite	M4-A-R0	+0.1209	+0.0199	+0.2370

Setting	Mineral	Comparison	Mean	2.5%	97.5%
EM 8 km	Nickel	M4-A-M1-A	-0.0966	-0.1798	-0.0146
EM 8 km	Nickel	M4-C-M1-C	-0.1198	-0.2035	-0.0392
EM 8 km	Nickel	M4-A-R0	-0.1664	-0.2794	-0.0539
Inputs < 1998	Graphite	M4-A-M1-A	+0.0495	+0.0000	+0.1100
Inputs < 1998	Graphite	M4-C-M1-C	-0.0918	-0.1703	-0.0200
Inputs < 1998	Graphite	M4-A-R0	+0.1493	+0.0450	+0.2620
Inputs < 1998	Nickel	M4-A-M1-A	-0.0861	-0.1699	-0.0016
Inputs < 1998	Nickel	M4-C-M1-C	-0.0921	-0.1667	-0.0172
Inputs < 1998	Nickel	M4-A-R0	-0.1560	-0.2680	-0.0457
Worked showings	Graphite	M4-A-M1-A	-0.1683	-0.3553	-0.0049
Worked showings	Graphite	M4-C-M1-C	-0.1711	-0.3801	+0.0400
Worked showings	Graphite	M4-A-R0	-0.0469	-0.2718	+0.1575
Worked showings	Nickel	M4-A-M1-A	-0.1324	-0.2451	-0.0327
Worked showings	Nickel	M4-C-M1-C	-0.1123	-0.2222	-0.0131
Worked showings	Nickel	M4-A-R0	-0.2314	-0.3725	-0.1046