

# Wind farms occupy fire-prone terrain but show no evidence of targeting burnt land: a multi-scale, time-ordered analysis for Greece

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## Abstract

A recurring claim in Greek public discourse holds that wildfires are set to clear land for wind farms, so that turbines end up on previously burnt ground. We test this claim with a design built to avoid the base-rate fallacy of simple overlap statistics, comparing the operational Greek wind fleet against wind-viable land with a spatial case-control model and a time-ordered survival model, on a scale ladder from the parcel to 5 km. At the parcel scale there is no association: no evidence of site-level targeting. At the landscape scale (2–5 km), when each wind farm is weighted equally, farms sit near burnt land with an odds ratio of about 1.6 compared with the wind-viable base rate; a fire-prone fuel covariate explains roughly half, leaving a residual near 1.4 that survives adjustment for terrain, fuel and protected status. This landscape signal disappears when individual turbines rather than farms are the unit, because larger developments are less frequently adjacent to recorded burnt areas and dominate the turbine count. The time-ordered test is not robust, and the median fire-to-installation gap is 6–7 years. The evidence does not support deliberate burning to build; it does show that wind farms occupy fire-prone landscapes to a degree that measured landscape covariates only partly explain. A secondary result is that the apparent association depends on the scale of development: larger farms are less often adjacent to burnt land than smaller ones, so counting projects and counting turbines give different answers.

**Keywords:** wind energy; wildfire; land-use change; use-availability; resource selection; survival analysis; Greece

## 1 Introduction

The rapid expansion of onshore wind energy in fire-prone Mediterranean landscapes has been accompanied, in Greece, by a persistent public accusation: that wildfires are deliberately set to clear forest that would otherwise block development, so that wind turbines are subsequently installed on the burnt land. If true, this would imply that turbine locations coincide with previously burnt ground at a rate above what siting on wind grounds alone would produce.

Testing this claim rigorously is harder than it appears, and naive tests mislead in two ways. First, turbines occupy very little ground, so any statistic of the form “turbine footprint as a fraction of burnt area” is tiny by construction and says nothing about preference. Second, the relevant comparison is not against all land or all burnt area but against comparable wind-viable land. Greece burns extensively every year, and wind development concentrates on exposed ridges that are themselves fire-prone; a raw overlap is therefore expected even under innocent siting. Establishing preference requires a base rate defined on the land where turbines could plausibly go, and establishing the causal direction of the accusation requires time-ordering fire against installation.

The problem is an instance of a general one, and we borrow accordingly. Asking whether a set of chosen locations is drawn preferentially from an available landscape is the use-availability problem of habitat and resource-selection analysis [2, 3, 4]; contrasting chosen sites against a random sample of eligible background, with covariate adjustment, is the case-control logic of spatial epidemiology [5]; and disentangling siting preference from landscape confounding is a recurring difficulty in the study of where infrastructure is built [6]. The methods used here are therefore broadly applicable to contested siting claims wherever a defensible “available” set and a time-ordering can be constructed.

The only prior public analysis of this specific claim we are aware of is a conference poster by the Hellenic Wind Energy Association [1]. It reports that wind farms occupy at most 0.07% of legally reforestable land and that turbines installed after a fire cover 0.03% of the area burnt over 2008–2024, and concludes there is no correlation. Those figures are footprint-over-area ratios, small by construction, taken over reforestable or burnt land rather than wind-viable land, and carry no inferential test. They can motivate the question but cannot resolve it.

Our central result is more nuanced than a clean null. We find no evidence of parcel-level targeting or of deliberate burning to build; but we do find a robust landscape-scale association between wind farms and burnt land that measured covariates only partly explain, and we are careful to distinguish that residual association from the causal claim the accusation makes.

## 2 Data

**Wind fleet.** The unit is the operational Greek Wind Power Station in the RAAEY production-licence registry (operating-licence status; 437 stations), augmented with turbine geometry from OpenStreetMap (2,995 turbines; 2,958 assigned to a station within 5 km, 37 orphans). The 437 operational stations are the subset, of a larger 508-station licensed fleet reported by industry, that carry an operating (rather than merely production or installation) licence; the remainder are excluded because they are not yet operational or lack the licence status we require for a commissioning date. The 37 orphan turbines, more than 5 km from any station, are excluded from all farm-level analyses and never enter Tests A–C. Commissioning dates were resolved to exact (Diávgeia act; 102 stations), bounded (113), ADMIE-bracketed (117), or unresolved (105; mostly pre-2008 or non-interconnected islands).

**Fire perimeters.** We use the Copernicus EFFIS MODIS burnt-area product (nominal 30 ha floor, one dated polygon per event). The public layer covers only ~2016 onward; 2010–2015 perimeters were obtained through the EFFIS data-request service in the same product family. The merged 2010–2024 dataset has 1,644 perimeters totalling 607,003 ha, consistent with the 615,363 ha reported for 2008–2024 [1] after allowing for the two excluded years.

**Covariates.** Wind speed at 100 m is from the Global Wind Atlas 3 [12]; slope from the Copernicus GLO-90 DEM [13]; land cover from CORINE 2018 [11]; protected status from Natura 2000 [14].

**Table 1:** Data sources.

Layer	Product	Native res.	Role
Turbines	RAAEY licences + OSM	point	cases / events
Fire	EFFIS MODIS burnt areas 2010–2024	~250 m	exposure
Wind	Global Wind Atlas 3, 100 m mean speed	~250 m	universe gate / covariate
Elevation	Copernicus GLO-90	~90 m	slope, aspect covariates
Land cover	CORINE 2018	100 m	exclusions, fuel covariate
Protected	Natura 2000 (2019)	vector	covariate

All raster analysis is on a common 100 m grid in EPSG:3035 (Table 1).

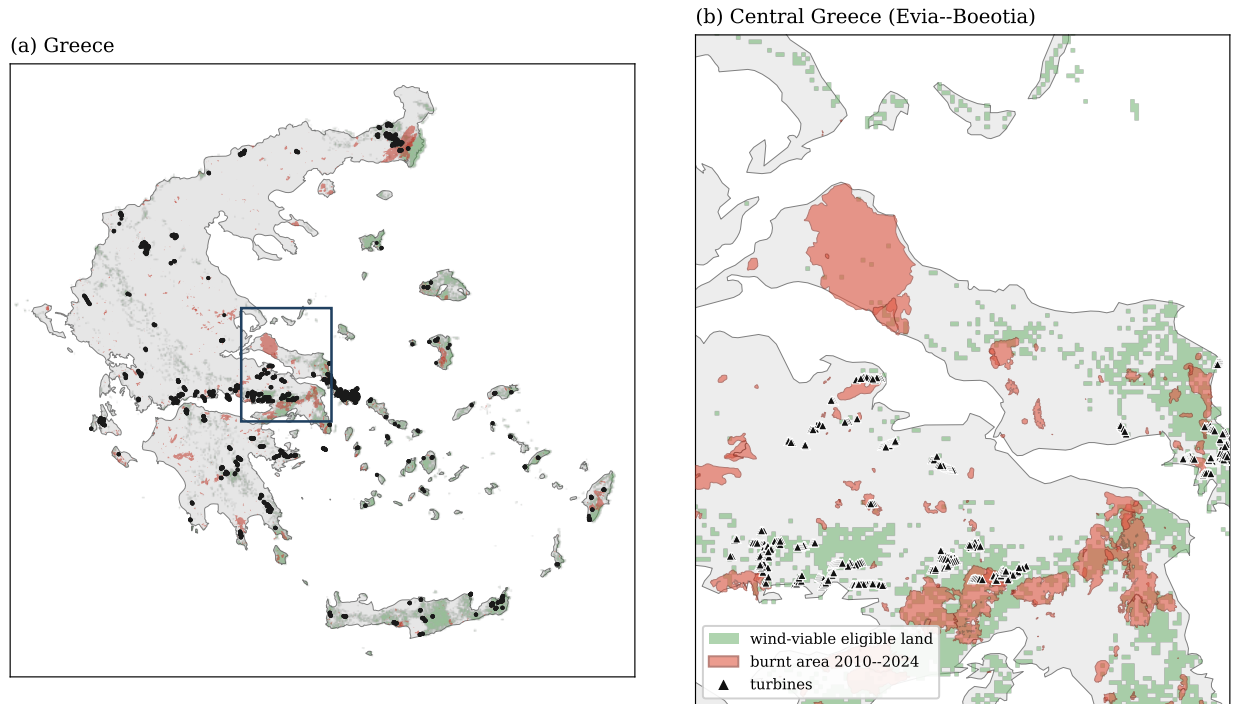
### 3 Methods

**Eligible universe.** The eligible universe is the set of wind-viable land cells, the denominator for base-rate comparisons. Both thresholds are fleet-calibrated: the wind floor is the 5th percentile of Global Wind Atlas speed at existing turbines ( $5.96 \text{ m s}^{-1}$ , close to the common  $6.0 \text{ m s}^{-1}$  value), and the slope ceiling is the 95th percentile of slope at existing turbines ( $16.07^\circ$ ). We exclude open water, glaciers and dense artificial surfaces (CORINE 511–523, 335, 111, 112, 123, 124, 141) and mask to Greek land. The primary universe is  $17,361 \text{ km}^2$ ; 89.5% of turbines fall inside it, as expected under fleet calibration. Fleet calibration uses cases to help define the universe, a mild circularity we probe with a  $\text{wind} \times \text{slope}$  threshold grid (below). Figure 1 shows the study area.

**Test A (temporal precedence).** For each station we form the convex hull of its turbines, buffer it (500 m–5 km), and compare intersecting fire dates to the commissioning window (predates / ambiguous / postdates / none). Test A is descriptive; it has no base rate.

**Test B (use-availability case-control).** Controls are 30,000 cells drawn uniformly at random without replacement from the eligible universe, one point per cell, unweighted and unmatched; the count was a practical choice and estimates are stable to it. The exposure is burnt-in-window: a case or control point lies within radius  $S$  of a 2010–2024 perimeter. We report two case definitions that answer different questions. The *turbine* unit takes the 2,680 turbines inside the universe as cases with standard errors clustered by station; because it weights each farm by its turbine count, it estimates the probability that an installed *turbine* lies near burnt land, and is dominated by large developments. The *farm* unit takes the 292 station centroids inside the universe as cases, weighting each project equally; it estimates the probability that a wind-farm *project* is sited near burnt land. The farm unit most directly reflects project-level siting decisions, which concern projects rather than individual turbines. Both are logistic regressions of case status on burnt plus wind, slope, elevation, aspect (northness, eastness) and Natura status, with continuous covariates per standard deviation. For multi-turbine farms the catchment is taken from the representative farm point. Test B counts any overlap within the window, including fires that post-date installation; the temporal direction is left to Test C.

**Test C (time-varying survival).** We follow eligible locations from 1 January 2010; the event is installation (farm commissioning, 2010–2024, resolvable date) and the risk set is a random sample of the eligible universe, administratively censored at 31 December 2024. All units enter at the time



**Figure 1:** Study area. (a) Greece: national territory (gray), wind-viable eligible land (green), burnt-area perimeters 2010–2024 (red), and operational turbines (dark points); the box marks the inset. (b) Central Greece (Evia–Boeotia), showing turbines, burns, and eligible land and their co-occurrence within fire-prone terrain.

origin; there is no delayed entry or left-truncation. Stations installed before 2010, and unresolved stations, are prevalent rather than incident and are excluded from the event set; the number of incident events is 165 when events are restricted to the eligible universe (our primary specification); when all resolvable farms are admitted (a sensitivity), the number is 229. Fire enters as a time-dependent covariate that switches from 0 to 1 at the fire date, so only fires preceding a given time contribute and post-installation fires never count. Events and controls are treated symmetrically as points with the same catchment. We test the proportional-hazards assumption for the burnt effect with a fire-by-time interaction.

**Scale ladder, fuel covariate, robustness.** We evaluate Tests B and C from the point to 5 km, applying the same radius to cases and controls so the base rate moves with scale. To separate landscape fire-proneness from targeting we add a scale-matched fuel-fraction covariate: the fraction of fire-prone natural fuel (CORINE forest, shrub, sclerophyll, grassland, transitional woodland, sparse vegetation) within the catchment, computed independently of where fire actually occurred. For the headline farm-level residual we replace seed-to-seed anecdote with a permutation test (1,000 label permutations) and a wind $\times$ slope threshold grid. In the gap analysis a *short gap* means installation within three years of the fire. Analyses use Python with `geopandas`, `rasterio`, `statsmodels` [8], `scikit-learn` and `lifelines` [9].

**Table 2:** Test A, farm level (302 resolved stations; 96 unresolved reported separately).

catchment	predates	ambiguous	postdates	no fire
hull + 500 m	17	9	41	235
hull + 1 km	31	12	47	212
hull + 2 km	55	19	67	161
hull + 3 km	71	21	78	132
hull + 5 km	95	28	88	91

**Table 3:** Test B burnt odds ratio (95% CI), unadjusted and with the fuel covariate, by unit and scale.

scale	turbine unit (clustered)		farm unit	
	unadjusted	+ fuel	unadjusted	+ fuel
point	0.96 (0.62–1.47)	1.05 (0.69–1.61)	0.82 (0.50–1.33)	0.76 (0.47–1.24)
1 km	1.08 (0.82–1.41)	0.89 (0.67–1.18)	1.36 (1.03–1.79)	1.14 (0.86–1.50)
2 km	1.24 (0.97–1.58)	1.03 (0.80–1.33)	<b>1.64 (1.28–2.10)</b>	<b>1.40 (1.09–1.80)</b>
3 km	1.27 (0.99–1.64)	1.03 (0.79–1.35)	1.67 (1.30–2.14)	1.39 (1.08–1.78)
5 km	1.27 (0.95–1.70)	1.05 (0.76–1.44)	1.69 (1.29–2.21)	1.35 (1.02–1.78)

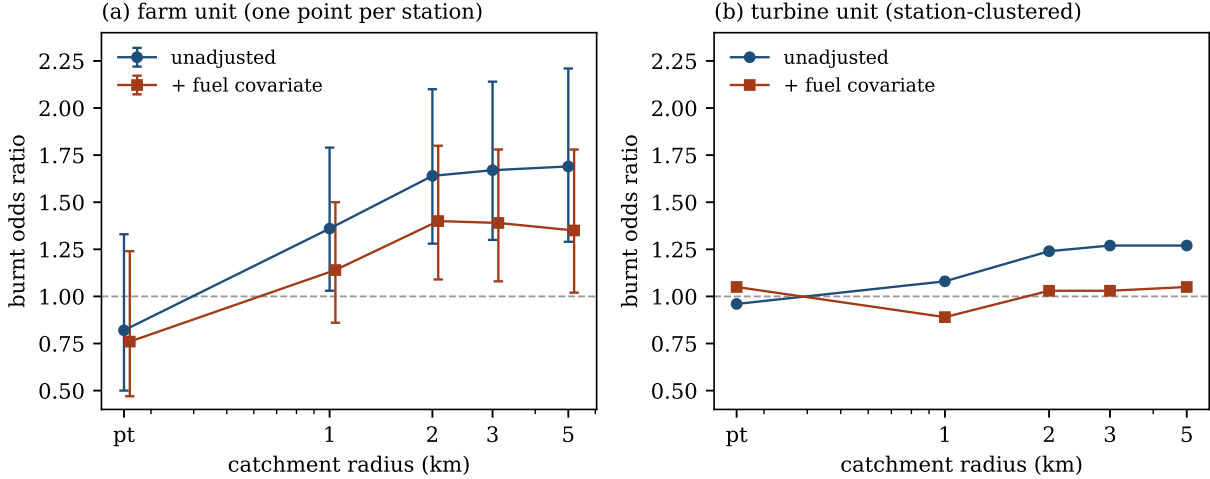
## 4 Results

**Temporal precedence (Test A).** Burnt-before farm counts grow with catchment, from 17 at 500 m to 95 at 5 km, but so do fires of every kind (Table 2); by 5 km almost every farm, and almost every random point, has a fire within range. A raw count cannot settle the claim.

**Spatial association depends on the unit (Test B).** The result hinges on whether farms are weighted equally or by turbine count (Table 3, Figure 2). Under the turbine unit, the burnt association is null at every scale and is fully removed by the fuel covariate. Under the farm unit, a strong and significant landscape-scale association appears: odds ratio 1.64 at 2 km ( $p < 10^{-4}$ ), rising to 1.69 at 5 km. Adding fuel explains about half of it but leaves a residual odds ratio of 1.40 at 2 km ( $p = 0.008$ ), 1.39 at 3 km and 1.35 at 5 km, all significant; at 1 km fuel removes it. The parcel scale is null in both units.

The farm-level residual is robust. A permutation test on the 2 km fuel-adjusted estimate (1,000 label permutations) gives  $p = 0.010$ : the observed standardized burnt coefficient (0.160) lies far outside the permutation null (mean  $\approx 0$ , SD 0.061), about 2.6 standard deviations above it. Across a wind $\times$ slope threshold grid (Table 4) the fuel-adjusted odds ratio ranges 1.26–1.47 and is significant in eight of nine universe definitions (the exception, the loosest wind and slope, at  $p = 0.052$ ).

**The time-ordered test is not robust (Test C).** Treating events and controls symmetrically as points, the hazard ratio for prior burn is null when all 229 resolvable farms are admitted (2 km HR 1.12, 95% CI 0.93–1.35,  $p = 0.22$ ) but significant when events are restricted to the eligible universe (165 events; 2 km HR 1.32,  $p = 0.007$ ; 3 km HR 1.25,  $p = 0.017$ ). The fuel covariate does not change either version. In the significant specification the proportional-hazards assumption is violated: the fire-by-time interaction is strong and negative ( $p < 0.001$ ), so the effect is not constant over follow-up and the single hazard ratio is only a time-average. Because the estimate swings from null to significant across defensible design choices, and is non-proportional where positive, Test C



**Figure 2:** Test B burnt odds ratio with 95% confidence intervals, unadjusted and with the fuel covariate, across catchment radius. (a) Farm unit: a significant 2–5 km association with a residual near 1.4 after fuel. (b) Turbine unit: null and fully absorbed by fuel. The dashed line marks no effect.

**Table 4:** Farm-level Test B at 2 km, fuel-adjusted burnt odds ratio (95% CI), across eligible-universe thresholds.

	slope $\leq 15^\circ$	slope $\leq 18^\circ$	slope $\leq 20^\circ$
wind $\geq 5.5$	1.39 (1.08–1.79)	1.33 (1.05–1.68)	1.26 (1.00–1.58)
wind $\geq 6.0$	1.42 (1.10–1.83)	1.35 (1.06–1.72)	1.28 (1.01–1.62)
wind $\geq 6.5$	1.47 (1.14–1.91)	1.38 (1.07–1.77)	1.32 (1.04–1.69)

does not provide robust evidence on the temporal direction either way.

**Fire-to-installation gap.** For burnt-before farms the gap between fire and installation is long: median 6–7 years, dominated by the 5–10 year bin and running to 12 years (Table 6, Figure 3). Nearly every fire falls after the production licence (30 of 32 at 1 km), which superficially matches the accusation’s order but is mechanically forced: Greek licences were mostly issued before the 2010 start of the fire record, so any incidental fire in the long licence-to-installation window lands after the licence. The suspicious profile, a short gap (within three years) and a fire after the licence, appears in only 3–11 farms, consistent with chance given the roughly 10,000 vegetation fires of all sizes reported annually in Greece (the EFFIS perimeters used here, above  $\sim 30$  ha, number about 110 per year).

## 5 Discussion

The parcel scale is null in every test: there is no evidence that specific burnt parcels were selected for specific turbines. What survives is a landscape-scale spatial association, and its character is now clear.

**Why the unit matters.** Whether the association appears depends on the analysis unit, and the reason is concrete rather than merely technical. Larger farms are less fire-adjacent: the share of

**Table 5:** Eligible-universe area (km<sup>2</sup>) and number of farm cases for each threshold in Table 4.

	slope $\leq 15^\circ$	slope $\leq 18^\circ$	slope $\leq 20^\circ$
wind $\geq 5.5$	21,524 ( $n=295$ )	25,199 ( $n=326$ )	27,291 ( $n=345$ )
wind $\geq 6.0$	15,885 ( $n=281$ )	18,726 ( $n=311$ )	20,344 ( $n=329$ )
wind $\geq 6.5$	11,608 ( $n=265$ )	13,782 ( $n=291$ )	15,023 ( $n=307$ )

**Table 6:** Fire-to-installation gap and licence ordering for burnt-before farms (short gap: within three years).

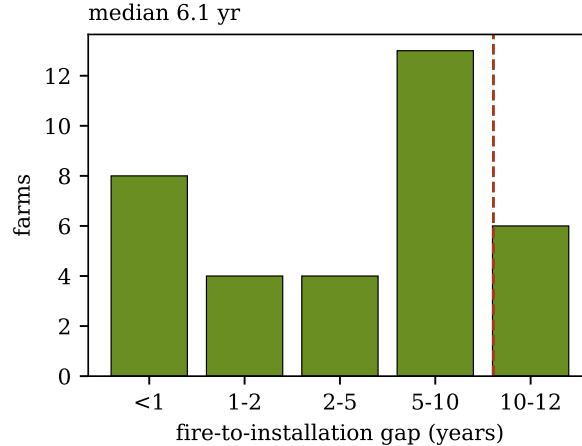
catchment	$n$	median gap (yr)	fire after / before licence	short-gap & post-licence
hull	12	6.8	11 / 0	3
500 m	20	6.8	19 / 0	4
1 km	35	6.1	30 / 2	11

farms with a fire within 2 km falls from 0.50 in the smallest size tercile (median 2 turbines) to 0.34 in the largest (median 12), and burnt-proximity declines with farm size (odds ratio 0.57 per tenfold increase in turbine count,  $p = 0.056$ ). Because the turbine unit weights each farm by its turbine count, these large, less fire-adjacent developments dominate it, and reweighting the farm model by turbine count reproduces the turbine-level estimate (odds ratio 1.22). The pattern is consistent with large developments concentrating in established, less fire-prone wind corridors while smaller projects disperse into fire-prone mountain terrain. Weighting each project equally, the natural unit for a siting decision, the association is strong (odds ratio 1.6) and only half explained by fuel.

**What the residual means.** We are deliberately cautious here. The residual near 1.4 is robust to permutation and threshold choices, making it unlikely to be a sampling artifact. It shows that wind farms occupy fire-prone landscapes at the 2–5 km scale beyond what wind, slope, elevation, aspect, protected status and fuel composition explain. Fuel is only one landscape confounder; road and grid access, land ownership and availability, planning and military exclusions, terrain roughness and geology are all unmeasured and could co-locate wind development with fire-proneness. The analyses cannot distinguish development that preferentially selects land already cleared by *unrelated* fires from residual spatial confounding by unmeasured siting factors; and neither of those is the same as deliberately burning land in order to build on it. For the deliberate-burning claim specifically, the evidence is unsupportive: the time-ordered hazard is fragile, swinging from null to modest and violating proportional hazards where positive, and the fire-to-installation gaps are long (median 6–7 years). A mechanism in which fires were intentionally set to facilitate construction would more naturally produce shorter and more consistent fire-to-installation intervals together with a stable positive temporal association; we see neither.

This both sharpens and qualifies the industry poster’s flat “no correlation” [1]. A landscape-scale correlation does exist and is only partly explained by the covariates we measured; but it is not evidence for the accusation, which is specifically about burning land in order to build on it.

**Limitations.** CORINE is a single 2018 snapshot, and for fires before 2018 it already reflects post-fire cover, so the fuel covariate carries measurement error and probably under-adjusts; the true landscape-explained fraction may be larger than half, which would shrink the residual. The MODIS product maps fires above  $\sim 30$  ha only. Commissioning dates for bracketed stations use interval midpoints, and the incident-event set for Test C is small (165–229), so the time-ordered



**Figure 3:** Fire-to-installation gap for burnt-before farms (1 km catchment,  $n = 35$ ); median 6.1 years.

test has limited power and, as shown, limited robustness. Test B counts any within-window overlap including post-installation fires, appropriate for a spatial-association question but not a temporal one. The analysis speaks to the systematic pattern, not to any individual project, which it can neither implicate nor clear.

## 6 Conclusions

Across temporal-precedence, spatial case-control and time-ordered survival tests, evaluated from the parcel to 5 km and interrogated for landscape confounding and time gaps, we find no evidence that wind turbines in Greece are deliberately sited on burnt land: the parcel scale is null, the time ordering is fragile and non-proportional, and fire-to-installation gaps are long. We do find, robustly, that wind farms occupy fire-prone landscapes at the 2–5 km scale beyond what measured terrain, protection and fuel explain, a residual most plausibly attributable to unmeasured static co-location factors. We also find that this association is scale-of-development dependent: larger farms are less often adjacent to burnt land, so whether one counts projects or turbines changes the apparent result, a caution for future siting studies. The base-rate and time-ordered framework used here is portable to other contested siting claims, and its main lesson is methodological: the answer depends on the analysis unit, the catchment scale and the completeness of landscape controls, and honest treatment of all three is what separates a defensible finding from an artifact.

## Data and code availability

Derived datasets (merged fire perimeters, the eligible-universe raster, terrain and land-cover layers, and the case-control and per-farm tables) together with the full analysis code are archived on Zenodo (<https://doi.org/10.5281/zenodo.21261873>). Input products are third-party and used under their own licences: the RAAEY registry, OpenStreetMap, Copernicus EFFIS, the Global Wind Atlas, Copernicus GLO-90, CORINE, and Natura 2000.

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