

County-Level Radiologist Workforce Density, Cancer Mortality, and the Modeled Impact of AI-Augmented Diagnostic Capacity: A U.S. Ecological Analysis

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Abstract

Timely cancer diagnosis depends on adequate diagnostic imaging capacity, yet radiologist workforce availability remains unevenly distributed across U.S. counties. This study examined the association between county-level diagnostic radiologist density and age-adjusted cancer mortality across 3,073 U.S. counties using cross-sectional ecological analysis integrating provider enrollment, cancer registry, socioeconomic, and population data. In the fully adjusted model, higher log-transformed radiologist density was significantly associated with lower cancer mortality ($\beta = -2.01$ per 100,000; 95% CI: -2.82 to -1.19 ; $p < 0.001$), with results robust to spatial autocorrelation (spatial error model $\beta = -1.96$) and twelve sensitivity analyses. Counties with zero radiologist presence ($n = 1,334$; population 22.6 million) were identified as having the greatest modeled mortality burden. Decision-analytic scenario modeling estimated that AI-equivalent augmentation matching the median nonzero-county density ($\Delta AI = 6.05$ per 100,000) would correspond to a modeled annual mortality difference equivalent to approximately 887 deaths in this subgroup. The modeled association showed diminishing marginal differences, with larger estimated differentials when capacity was introduced into zero-radiologist counties than in higher-density settings. These findings suggest radiologist workforce distribution may represent a policy-relevant marker of diagnostic capacity and highlight zero-radiologist counties as targets for further evaluation of capacity-expanding interventions, including AI-equivalent approaches.

Introduction

Cancer remains a leading cause of mortality worldwide, with an estimated 19.3 million new cases and nearly 10 million deaths annually, imposing a substantial and growing global health burden [1]. Despite advances in treatment, a critical determinant of cancer survival remains the timeliness of diagnosis, with delays frequently associated with advanced-stage disease and increased mortality [2–5]. However, timely diagnosis is not uniformly accessible. Persistent disparities in access to diagnostic services continue to undermine improvements in cancer outcomes, contributing to substantial inequities across populations and healthcare systems [6–8].

A central yet under-recognized constraint in cancer care delivery is the capacity of diagnostic systems. Radiology services are integral to cancer detection, staging, and treatment planning, but in the United States, the availability of radiologists is both limited and unevenly distributed [9–12]. At the same time, demand for diagnostic imaging has grown rapidly, outpacing workforce expansion and placing increasing strain on system capacity [13]. This growing imbalance between demand and supply creates bottlenecks in diagnostic pathways, leading to delays that may propagate through the care continuum and ultimately affect survival outcomes. More broadly, prior evidence demonstrates that geographic variation in physician availability is associated with differences in population health outcomes, highlighting the role of workforce distribution as a structural determinant of health [14, 15].

Recent advances in artificial intelligence (AI) have been proposed as a strategy to alleviate these system-level constraints. AI systems have demonstrated expert-level performance across a range of diagnostic

imaging tasks, including cancer detection and classification [16–19]. Importantly, the potential value of AI extends beyond improvements in diagnostic accuracy. By reducing time spent on specific tasks, improving workflow efficiency, and enabling redistribution of clinical workload, AI may effectively expand diagnostic capacity without proportional increases in workforce size [20–23]. Empirical studies suggest that AI-integrated workflows can shorten interpretation time, improve prioritization of high-risk findings, and accelerate communication of results, thereby enhancing the overall efficiency of radiology services [24, 25]. These capabilities position AI as a scalable intervention that may mitigate capacity constraints in resource-limited settings.

Despite this promise, a critical gap remains between demonstrated algorithmic performance and real-world clinical impact. Existing research has largely focused on model accuracy in controlled settings, with comparatively limited evaluation of how AI deployment influences population-level health outcomes [26, 27]. In particular, quantitative evidence linking diagnostic workforce capacity to cancer outcomes at scale remains limited, and even fewer studies have examined how technology-driven capacity expansion may alter these relationships under real-world constraints. As a result, current evidence provides limited guidance for policymakers seeking to allocate resources or evaluate the potential impact of AI as a system-level intervention.

To address these limitations, this study evaluates the association between radiologist workforce availability and cancer outcomes at the county level in the United States and examines the potential role of AI as a capacity-expanding intervention. Using an association-based modeling framework, workforce distribution, population health data, and scenario-based simulations were integrated to estimate how hypothetical AI-equivalent augmentation of diagnostic capacity would alter modeled mortality differentials across regions. By explicitly modeling both baseline conditions and counterfactual AI scenarios, this study aims to provide policy-relevant insight into the relationship among workforce constraints, modeled capacity augmentation, and population cancer outcomes.

Results

Descriptive Statistics

The analytic sample comprised 3,073 counties across 49 states and the District of Columbia. Mean age-adjusted cancer mortality rate was 163.7 per 100,000 population (SD = 28.3; range 63.7–412.2). Radiologist workforce density was highly right-skewed and zero-inflated: the median county density was 0.33 per 100,000 (mean 5.29), and 1,334 counties (43.4%) in the analytic sample had no registered diagnostic radiologist. Among the 1,739 counties with at least one provider, density ranged from near zero to 392.0 per 100,000, with a median of 6.05. The geographic distribution of shortage status is shown in Fig. 1. Across all 3,135 counties in the full dataset (including those with suppressed mortality data), 1,394 counties (44.5%) had zero radiologist density, and 1,829 (58.3%) fell below the severe shortage threshold (density < 2.42 per 100,000). The mean Rural-Urban Continuum Code (RUCC) score

was 5.2, reflecting a predominantly non-metropolitan sample. The mean county-level median household income was \$58,849, and the mean proportion of uninsured adults was 13.9%.

Counties (N = 3,135) are classified into three shortage categories based on radiologist density per 100,000 population: extreme shortage (density = 0; n = 1,394, 44.5%), severe shortage (density < 2.42 per 100,000, inclusive of extreme shortage counties; n = 1,829, 58.3%), and non-shortage (density $\geq$ 2.42 per 100,000; n = 1,306, 41.7%). Alaska and Hawaii are shown as insets. County boundaries derived from U.S. Census Bureau TIGER/Line 2021 shapefiles. Map projected to Albers Equal Area (EPSG:5070).

Primary Regression Analysis

Results from the primary OLS regression are presented in Table 1. In the unadjusted model (M1), higher log-transformed radiologist density was associated with lower cancer mortality rate ($\beta = -4.03$, 95% CI: -4.88 to -3.18, $p < 0.001$; $R^2 = 0.028$). After adjustment for RUCC, median household income, and proportion of uninsured adults, the association attenuated but remained statistically significant (M2: $\beta = -2.01$, 95% CI: -2.82 to -1.19, $p < 0.001$; $R^2 = 0.286$). The attenuation from M1 to M2 indicates partial confounding by rurality and socioeconomic factors, while the persistent association in M2 indicates that the association was maintained after adjustment for these measured county-level covariates. The exposure variable's independent contribution to model fit was modest (partial $R^2 = 0.006$, roughly 2% of M2's total R^2), indicating that most of the variance explained by M2 is attributable to the covariates rather than radiologist density itself; the exposure coefficient nonetheless remained precisely estimated and highly significant.

Table 1

Association between diagnostic radiologist density and cancer outcomes: primary regression, spatial robustness, and sensitivity analyses.

Model	β	95% CI	p	R ²	N
Primary Analysis					
M1: Unadjusted	-4.03	-4.88, - 3.18	< 0.001	0.028	3,073
M2: Fully adjusted†	-2.01	-2.82, - 1.19	< 0.001	0.286	3,073
Spatial Robustness					
Spatial error model	-1.96	—	< 0.001	0.282	3,073
SEM (KNN k = 5, robustness)	-1.95	—	< 0.001	—	3,073
Sensitivity Analyses (Pre-specified)					
S1: Primary taxonomy only	-2.10	-2.93, - 1.27	< 0.001	—	3,073
S2: Pathologist density	-3.36	-4.27, - 2.44	< 0.001	—	3,073
S3: Incidence outcome	-2.39	-4.22, - 0.56	0.011	—	3,018
S4: Exclude top 1% density	-2.04	-2.86, - 1.22	< 0.001	—	3,042
S5: Non-metropolitan only	0.66	-0.69, 2.01	0.335	—	1,897
S6: RUCC-only adjustment	-2.76	-3.71, - 1.81	< 0.001	—	3,073
Post-hoc Analyses (Exploratory)‡					
S7: 2026 NPPES snapshot	-2.23	-3.00, - 1.47	< 0.001	—	3,073
S8: Non-metro, RUCC 4–5	1.99	-0.59, 4.57	0.130	—	275
S8: Non-metro, RUCC 6–9	0.69	-0.85, 2.23	0.379	—	1,622
S9: Density × rurality interaction	4.61	3.08, 6.14	< 0.001	—	3,073
S10: Non-metro, exclude outliers	0.57	-0.84, 1.98	0.427	—	1,878
S11: Negative binomial (pop. offset)§	-0.031	-0.065, 0.002	0.068	—	3,073
S11b: Negative binomial (age-std. offset)§	-0.019	-0.053, 0.014	0.259	—	3,073

†M2 adjusted for Rural-Urban Continuum Code (RUCC), median household income, and proportion of uninsured adults.

All models use HC3 heteroskedasticity-consistent robust standard errors except the spatial error model (maximum likelihood) and S11/S11b (negative binomial regression).

β = change in age-adjusted cancer mortality rate per 100,000 population per one-unit increase in $\log(\text{radiologist density} + 1)$, except S9 (interaction coefficient) and S11/S11b§ (log incidence-rate ratio from negative binomial regression, not directly comparable in scale to the other β coefficients).

S1: primary taxonomy designation only; S2: pathologist density substituted as exposure; S3: cancer incidence rate as outcome;

S4: top 1% density counties excluded; S5: non-metropolitan counties only (RUCC ≥ 4); S6: RUCC as sole covariate. ‡S7–S11b were conducted post hoc following the unexpected null finding in S5 and are reported as exploratory: S7, independent 2026 NPPES workforce extract with 2021 population denominator; S8, non-metropolitan counties stratified by USDA urban-population tier; S9, density $\times$ rurality interaction, full sample; S10, non-metropolitan subsample with top 1% density excluded; S11/S11b§, negative binomial regression on raw mortality counts (total-population and age-standardized expected-deaths offsets, respectively).

Spatial error model SE = 0.36; KNN (k = 5) spatial error model λ = 0.47. CI = confidence interval; RUCC = Rural-Urban Continuum Code.

Sensitivity Analyses

Results across the six pre-specified sensitivity analyses are shown in Table 1. Restricting the exposure to primary taxonomy designation only (S1: $\beta = -2.10$, $p < 0.001$) and substituting pathologist density as a specificity check (S2: $\beta = -3.36$, $p < 0.001$) both yielded associations directionally consistent with the primary estimate. Replacing the primary outcome with the cancer incidence rate yielded a smaller but statistically significant association (S3: $\beta = -2.39$, $p = 0.011$), suggesting that the radiologist density–outcomes relationship extends beyond mortality to detection-stage indicators. Excluding the top 1% of counties by density did not meaningfully change the estimate (S4: $\beta = -2.04$, $p < 0.001$), indicating that high-density outlier counties were not driving the primary result. In contrast, restricting the sample to non-metropolitan counties only (S5: $\beta = 0.66$, $p = 0.335$) yielded a null result. Estimating the model with RUCC as the sole covariate (S6: $\beta = -2.76$, $p < 0.001$) produced an estimate intermediate between M1 and M2, consistent with additional confounding by socioeconomic factors not captured by rurality alone.

Following this unexpected null finding in S5, six further analyses were conducted post hoc and are reported as exploratory (Table 1). Substituting an independent, more recent NPPES workforce extract (2026) while holding the 2021 population denominator fixed yielded an association of similar magnitude and significance (S7: $\beta = -2.23$, $p < 0.001$), supporting robustness to the choice of workforce snapshot. Stratifying non-metropolitan counties by USDA urban-population tier did not clarify the S5 null finding: neither the larger-town tier (S8, RUCC 4–5: $\beta = 1.99$, $p = 0.130$) nor the more remote tier (S8, RUCC 6–9: $\beta = 0.69$, $p = 0.379$) reached significance, suggesting the attenuation is not concentrated specifically in the most remote counties. A density-by-rurality interaction term estimated on the full analytic sample, however, was strongly significant (S9: $\beta = 4.61$, 95% CI: 3.08 to 6.14, $p < 0.001$), indicating that the

density–mortality association differs by rurality and suggesting that the S5 null finding is unlikely to be explained by limited subgroup power alone. Excluding outliers within the non-metropolitan subsample did not alter the null finding (S10: $\beta = 0.57$, $p = 0.427$), indicating S5 was not driven by extreme values within that subgroup.

Re-estimating the association using negative binomial regression on raw mortality counts—a specification better suited to count outcomes than the linear model used for M2—yielded an association of similar direction but attenuated significance when total population served as the offset (S11: log-IRR = -0.031 , $p = 0.068$). A Poisson dispersion statistic of 7.77 confirmed substantial overdispersion, justifying the negative binomial specification over Poisson regression. Because S11's offset does not account for county age structure, whereas M2's outcome is itself age-adjusted, an indirect age-standardized specification was also estimated using expected deaths (based on national age-specific mortality rates for ages < 65 and ≥ 65) as the offset. This age-standardized specification did not increase the significance of the association relative to S11 (S11b: log-IRR = -0.019 , $p = 0.259$); if anything, the association was weaker. This indicates that unadjusted age structure does not fully explain the attenuated significance observed under the negative binomial specification, and the source of the discrepancy between the OLS and negative binomial results remains an open question.

Model diagnostics for M2 indicated no meaningful multicollinearity among covariates (variance inflation factors: 1.30 for RUCC, 1.46 for income, 1.16 for uninsured proportion). A Breusch-Pagan test indicated heteroscedasticity ($p < 0.001$), consistent with the use of HC3 robust standard errors throughout; residuals departed from normality (skew = 0.74, kurtosis = 9.16, Jarque-Bera $p < 0.001$), though the large sample size supports the asymptotic validity of the OLS estimates.

Spatial Regression Analysis

Moran's I test indicated significant positive spatial autocorrelation in OLS residuals ($I = 0.428$, $p = 0.001$), confirming that geographically adjacent counties share unmeasured characteristics that violate the OLS independence assumption. Lagrange multiplier diagnostics favored a spatial error specification over a spatial lag model (LM-error = 598.08, $p < 0.001$; LM-lag = 555.44, $p < 0.001$). The spatial error model (SEM) estimated a coefficient of $\beta = -1.96$ (SE = 0.36, $p < 0.001$), a 2.5% change in magnitude from the OLS M2 estimate of -2.01 , indicating that the estimated association was largely unchanged after accounting for residual spatial dependence. The spatial autoregressive parameter (λ) was 0.49 ($p < 0.001$), indicating substantial residual spatial autocorrelation accounted for by the model. Pseudo- R^2 for the SEM was 0.282. Four counties, all in Hawaii, had no neighbors under the Queen contiguity criterion used to construct the spatial weights matrix. As a robustness check, the SEM was re-estimated using a k-nearest-neighbors ($k = 5$) weights matrix, which by construction has no islands; the resulting coefficient ($\beta = -1.95$, $\lambda = 0.47$) differed from the Queen contiguity estimate by only 0.01 and from the OLS estimate by 2.9%, confirming that the primary association is not sensitive to the choice of spatial weighting scheme.

Decision-Analytic Scenario Modeling

The differential mortality burden associated with AI-equivalent capacity expansion varied substantially by baseline shortage status, as shown in Fig. 2. Under the moderate scenario ($\Delta\text{AI} = 6.05$ per 100,000, the median density among counties with at least one provider), counties with zero radiologist density (extreme shortage subgroup, $n = 1,334$; total population 22.6 million) showed a mean modeled mortality differential of 3.92 per 100,000, corresponding to a modeled annual mortality difference equivalent to approximately 887 deaths. By contrast, non-shortage counties ($n = 1,304$; total population 277.7 million) showed a mean differential of 1.05 per 100,000 under the same scenario, reflecting the diminishing marginal benefit of additional capacity at higher baseline density levels. Across the full continuous gradient ($\Delta\text{AI} = 0$ to 11.63), the mortality differential increased monotonically but with diminishing returns, as illustrated in Fig. 2.

Estimated mortality differential (deaths per 100,000 population) is plotted as a continuous function of a hypothetical AI-equivalent capacity increment (ΔAI , expressed in radiologist-equivalent units per 100,000 population) for three county subgroups within the analytic sample: extreme shortage (density = 0; $n = 1,334$), severe shortage (density < 2.42; $n = 1,769$), and non-shortage (density ≥ 2.42 ; $n = 1,304$). Vertical dashed lines indicate the three scenario anchors, anchored to the nonzero-county density distribution ($n = 1,739$): low ($\Delta\text{AI} = 2.42$, 25th percentile), moderate ($\Delta\text{AI} = 6.05$, median), and high ($\Delta\text{AI} = 11.63$, 75th percentile). Estimates derived from the primary OLS regression coefficient (M2: $\beta = -2.01$) using the formula $\Delta\text{mortality} = \beta \times [\log(\text{density} + \Delta\text{AI} + 1) - \log(\text{density} + 1)]$. Results reflect modeled associations and should not be interpreted as causal predictions.

For the severe shortage subgroup (density < 2.42 per 100,000; $n = 1,769$; total population 50.6 million), the moderate scenario was associated with a mean modeled mortality differential of 3.73 per 100,000, corresponding to a modeled annual mortality difference equivalent to approximately 1,885 deaths. Under the high scenario ($\Delta\text{AI} = 11.63$), estimated annual mortality differentials reached approximately 1,152 for the extreme shortage subgroup and 2,467 for the severe shortage subgroup. These estimates reflect modeled associations under stated assumptions. The geographic distribution of the modeled mortality differential under the moderate scenario across U.S. counties is shown in Fig. 3.

Discussion

This county-level ecological analysis identified a statistically significant inverse association between diagnostic radiologist workforce density and age-adjusted cancer mortality across 3,073 U.S. counties, with a fully adjusted estimate of -2.01 deaths per 100,000 per one-unit increase in log-transformed radiologist density—a finding with direct relevance given that five-year cancer survival rates remain strongly stage-dependent [28]. While prior work has characterized radiologist workforce maldistribution [9–12] and demonstrated AI diagnostic performance across imaging tasks [16–19], few studies have directly examined the county-level association between workforce availability and population cancer mortality at scale. The present findings address this gap and support consideration of radiologist

workforce distribution as a system-level indicator of diagnostic capacity associated with population cancer mortality.

One plausible explanation operates through delays in diagnosis. In counties with limited or absent radiologist capacity, access to diagnostic imaging is constrained, abnormalities remain uncharacterized, and cancers are more likely to be diagnosed at later stages, narrowing the window for curative treatment [2–5]. This interpretation situates radiologist supply within a broader diagnostic pathway in which capacity constraints may propagate through the care continuum to influence survival outcomes, although the present ecological design cannot directly test this pathway. Geographic disparities in cancer mortality across rural and socioeconomically disadvantaged counties are well-documented [7, 8, 29], and the concentration of zero-radiologist counties in non-metropolitan areas documented in this study suggests that diagnostic workforce maldistribution may be a contributing structural factor within that broader pattern.

The stability of the primary association across multiple specifications strengthens confidence in this interpretation. The spatial error model coefficient of -1.96 was nearly identical to the OLS estimate of -2.01 , a difference of only 2.5%, indicating that the primary association is robust to spatial dependence in residuals and was not materially sensitive to the modeled spatial dependence structure, despite statistically significant spatial autocorrelation (Moran's $I = 0.428$, $p = 0.001$); results were similarly consistent when spatial weights were constructed using a k -nearest-neighbors rather than a contiguity-based specification. The association also persisted when an independent, more recent NPPES workforce extract was substituted for the primary exposure, supporting robustness to the specific workforce snapshot used. Sensitivity analyses further support robustness across alternative exposure definitions and outcome measures; results were more heterogeneous across sample restrictions, and a negative binomial specification on raw mortality counts showed attenuated significance under an alternative offset structure, both discussed further below. Together, these results support interpreting radiologist availability as a system-level constraint on diagnostic capacity rather than a proxy for isolated local characteristics.

Importantly, the observed association does not operate uniformly across the distribution of counties. The null finding in the non-metropolitan subgroup (S5: $\beta = 0.66$, $p = 0.335$) initially raised the possibility that the primary association is concentrated in counties spanning the full urban-to-rural gradient and is not detectable within exclusively rural settings alone. Stratifying non-metropolitan counties further by USDA-defined urban-population tier did not clarify this pattern, as neither tier reached significance, and excluding outliers within the rural subsample likewise did not alter the null finding, indicating it was not driven by a small number of extreme observations. A density-by-rurality interaction term estimated on the full analytic sample, however, was strongly significant (S9: $\beta = 4.61$, 95% CI: 3.08 to 6.14, $p < 0.001$), providing evidence of effect heterogeneity across the rurality gradient and suggesting that the S5 null finding is unlikely to be explained by limited subgroup power alone. Structural barriers such as geographic access, transportation infrastructure, and insurance coverage may attenuate the marginal benefit of workforce density increases specifically within already-rural settings [30, 31]. Instead, the

primary association appears to be driven substantially by the contrast between zero-supply and any-supply settings; because the primary model specification (log-transformed density) itself encodes diminishing marginal effects, the modeled association was necessarily largest at the lower end of the workforce-density distribution, particularly among zero-supply counties, and the present analysis cannot formally distinguish a true capacity threshold from this functional-form property, a distinction that would require dedicated nonlinear or spline-based modeling in future work. Taken together, these findings suggest that diagnostic capacity in health systems may be associated with disproportionately large modeled outcome differentials from the transition out of zero-provider status relative to incremental expansion at higher levels of supply (mean modeled mortality differential: 3.92 per 100,000 in extreme shortage counties vs. 1.05 per 100,000 in non-shortage counties under the moderate scenario). Accordingly, zero-provider counties represent a policy-relevant subgroup for evaluating whether introduction of baseline diagnostic capacity could produce larger marginal gains than equivalent expansion in already-served settings.

The decision-analytic scenario modeling translates this capacity-gradient structure into a policy-relevant framework. Under the moderate scenario, the modeled mortality differential in extreme shortage counties corresponded to a modeled annual mortality difference equivalent to approximately 887 deaths across a population of 22.6 million. While not interpretable as a causal prediction, this estimate defines the scale of mortality burden associated with diagnostic capacity gaps at the observed association magnitude. The concentration of this burden in zero-supply counties suggests that resource allocation strategies focused on marginal gains in higher-density regions are likely to yield comparatively smaller population-level benefits. Prioritizing the transition from zero to baseline capacity, therefore, may represent a higher-yield intervention margin within the observed, associational system structure.

Within this framework, these findings suggest that AI may be more appropriately conceptualized not solely as an efficiency-enhancing technology but as a potential mechanism for expanding effective diagnostic capacity under workforce constraints [32]. This scenario framework defines AI-equivalent capacity as, by construction, an equal increment in radiologist density, a simplifying assumption that does not capture differences in diagnostic scope, accuracy profile, or degree of clinical integration between AI systems and human radiologists. In zero-provider settings, where conventional efficiency gains are inapplicable, AI-enabled workflows may function as an alternative means of introducing diagnostic capability [16, 20–23]. By contrast, in higher-density settings, the marginal impact of AI is attenuated, consistent with diminishing returns observed across the scenario gradient (mean modeled mortality differential: 3.92 per 100,000 in extreme shortage counties vs. 1.05 per 100,000 in non-shortage counties under the moderate scenario [$\Delta\text{AI} = 6.05$ per 100,000]). This distinction implies a deployment strategy in which AI serves different system roles depending on baseline capacity: an access-enabling mechanism in extreme shortage contexts and an augmentative efficiency tool in already-served environments. More broadly, these scenario results illustrate how technology-enabled interventions may interact with underlying capacity gradients to reshape system performance rather than uniformly shifting outcomes across all settings.

Interpreted cautiously given the associational, cross-sectional design of this study, these findings collectively suggest a policy framework for addressing diagnostic capacity constraints. First, resource allocation may yield the greatest population-level benefit when directed toward counties with zero radiologist presence, where the modeled marginal impact of capacity introduction is largest. Second, existing delivery mechanisms such as teleradiology provide a scalable pathway for extending coverage across geographic gaps and warrant systematic evaluation as a component of system-level capacity redistribution [33]. Third, AI deployment strategies can be aligned with baseline capacity conditions, with particular relevance in settings where AI functions as a means of introducing diagnostic capability rather than incrementally enhancing existing infrastructure. More broadly, these findings provide a generalizable framework for understanding how capacity constraints translate into population health outcomes, emphasizing that uniform expansion strategies may fail to capture the nonlinear capacity effects inherent in complex health systems. As health systems navigate persistent workforce constraints alongside the rapid proliferation of AI-enabled diagnostic tools, county-level capacity metrics of the kind examined here may provide actionable targets for identifying where technology deployment is likely to yield the greatest population-level benefit.

Several limitations constrain interpretation. The cross-sectional design precludes causal inference and cannot establish temporal ordering between radiologist supply and cancer mortality; reverse causation is also plausible—for example, if counties with greater existing cancer burden subsequently attract greater investment in diagnostic infrastructure—which would be expected to bias the observed association toward the null rather than away from it. As with all ecological analyses, county-level associations cannot be extrapolated to individual-level relationships (the ecological fallacy), and residual confounding by unmeasured health system characteristics—including hospital density, primary care supply, and cancer screening utilization—cannot be excluded [34]. Re-estimating the association using negative binomial regression on raw mortality counts, a specification arguably better suited to count outcomes than the linear model used for the primary analysis, showed attenuated statistical significance; an indirect age-standardized specification did not resolve this attenuation and, if anything, showed a weaker association, indicating that unadjusted age structure does not fully explain the discrepancy between the OLS and negative binomial results. The source of this discrepancy remains unresolved and warrants examination using alternative count-based specifications in future work. Incidence-based sensitivity analyses additionally excluded all Kansas counties, which were absent from the incidence file for this period due to apparent statewide non-reporting; because Kansas mortality data were unaffected, this gap is unlikely to bias the primary mortality-based findings but may limit the generalizability of the incidence-based sensitivity analysis. Connecticut was excluded entirely from the primary analysis because of a geographic unit mismatch between legacy county FIPS codes used by the cancer registry and post-2022 Census planning region boundaries; findings from this study should therefore not be assumed to generalize to that state. NPPES data capture provider enrollment rather than active clinical practice, and multi-site practitioners are recorded under a single primary address, likely overestimating density in counties serving as practitioners' primary address and underestimating it in counties where secondary practice activity occurs. The proportional allocation of providers across ZIP codes spanning

multiple counties, based on business- and residential-address ratios, is an approximation of true practice location and may introduce additional measurement error into the density estimate. The exposure variable's independent contribution to overall model fit was also modest relative to the included covariates (partial $R^2 = 0.006$), indicating that most of the variance explained by the fully adjusted model is attributable to rurality and socioeconomic factors rather than radiologist density itself, although the exposure coefficient remained precisely estimated and statistically significant. Finally, AI scenario parameters represent hypothetical policy targets under an assumption of full equivalence between AI-enabled and human-provided capacity; realized population-level impact will depend on diagnostic performance across cancer types, integration into clinical workflows, and access to imaging infrastructure that remains constrained in many shortage counties [35, 36]. These limitations notwithstanding, the consistency of the association across the primary and most sensitivity specifications, together with the scale of the modeled mortality differentials, support treating radiologist workforce distribution as a policy-relevant system constraint in cancer care delivery.

Methods

Study Design and Analytic Framework

This study employs a system-level ecological framework to characterize county-level associations between diagnostic radiologist workforce density and cancer mortality across the United States. Data were integrated from multiple independent national sources—provider enrollment records, census population estimates, cancer registry statistics, and county health rankings—into a structured analytical dataset harmonized at the county level using FIPS codes as the common geographic identifier. All associations should be interpreted as ecological correlations; this design does not establish temporal ordering between exposure and outcome. The primary regression model, six sensitivity analyses, and spatial robustness check were determined in advance of outcome analysis; six additional analyses (S7–S11b, described below) were conducted post hoc to further probe an unexpected result and are reported as exploratory. This study used only publicly available, de-identified aggregate data; formal ethics review was not required under applicable federal regulations (45 CFR 46.101(b)).

Radiologist Workforce Data

Physician-level workforce data were obtained from the National Plan and Provider Enumeration System (NPPES) full replacement file [37], extracted in July 2021. Individual providers (Entity Type Code = 1) holding a diagnostic radiology taxonomy code (2085R0202X) or a pathology taxonomy code (207ZP0102X, 207ZP0101X, 207ZC0500X) in any of 15 available taxonomy fields were included; only active providers with a primary practice location within the 50 United States and the District of Columbia were retained. Providers holding both radiologist and pathologist codes were assigned to a single specialty group by primary taxonomy designation where resolvable; three providers for whom no primary designation could be resolved were excluded. Provider addresses were geocoded to the county level using the HUD ZIP-to-County Crosswalk (2021 Q2 release) [38]. For ZIP codes spanning multiple

counties, provider counts were allocated proportionally using business-address ratios, with residential-address and other-address ratios applied as sequential fallbacks where business-address ratios were unavailable for a given ZIP code. Provider counts were aggregated to the county (FIPS) level; because a ZIP code's postal-preferred state can differ from its county's actual state for boundary ZIP codes, county identity was determined by FIPS alone, with state subsequently assigned via the Census county reference table rather than via any single ZIP's postal designation. Radiologist density was a constructed exposure variable derived through provider identification, geographic allocation, and population normalization.

Density was calculated as providers per 100,000 residents using 2021 county-level population estimates (U.S. Census Bureau CO-EST2024) [39]. The primary exposure was log-transformed as $\log(\text{density} + 1)$ to address right skew (median 0.18, mean 5.19 per 100,000 across all 3,135 counties prior to outcome-based exclusions) and zero-inflation (44.5% of counties had no registered diagnostic radiologist); β represents the change in mortality rate per 100,000 per one-unit increase in $\log(\text{density} + 1)$ in the primary OLS model (M2).

Cancer Outcome Data

County-level cancer mortality and incidence data were obtained from the United States Cancer Statistics (USCS) via NCI State Cancer Profiles [40], combining SEER and NPCR data covering approximately 99% of the U.S. population. The primary outcome was the age-adjusted all-cancer mortality rate per 100,000 (2019–2023, five-year average); the secondary outcome was age-adjusted incidence (2018–2022), used in sensitivity analyses. Both were age-adjusted to the 2000 U.S. standard population. Connecticut was excluded upstream of all outcome-based exclusions described below, because of a geographic unit mismatch between NCI legacy county FIPS codes and post-2022 Census planning region boundaries; Connecticut counties are therefore absent from the full county dataset ($N = 3,135$) and from the exclusion counts that follow. Counties with suppressed or unavailable outcome values were excluded ($n = 62$ for mortality, comprising 59 suppressed counties and 3 counties excluded for administrative boundary changes, described below; $n = 117$ for incidence, comprising 105 Kansas counties absent from the USCS incidence file for this period due to statewide non-reporting, 9 suppressed counties, and the same 3 boundary-change counties). Three additional counties were excluded for administrative boundary changes (Chugach and Copper River Census Areas, Alaska; Bedford County, Virginia). For the age-standardized sensitivity analysis (S11b), county population by age group was obtained from the U.S. Census Bureau's county population estimates by age and sex (Vintage 2024 release) [41], and Ages-65+ mortality rates were obtained from the same USCS source and 2019–2023 period as the primary outcome [40].

Covariates

Rural-urban classification was obtained from the 2023 USDA Rural-Urban Continuum Codes (RUCC; 1 = most urban, 9 = most rural) [42]. Median household income and proportion of uninsured adults were obtained from the 2023 County Health Rankings dataset (University of Wisconsin Population Health

Institute) [43]; income was rescaled to units of \$10,000. County-level data availability precluded adjustment for hospital density, primary care supply, and cancer screening rates; residual confounding from these factors is addressed in the Limitations section.

Regression Analysis and Missing Data

The analytical strategy comprised a primary OLS regression, six pre-specified sensitivity analyses, a spatial error model, and a decision-analytic scenario simulation. The primary analysis estimated the association between log-transformed radiologist density and age-adjusted cancer mortality using OLS with HC3 robust standard errors; all hypothesis tests were two-tailed, with statistical significance set at $\alpha = 0.05$. The fully adjusted model (M2) included RUCC, median household income, and proportion uninsured; an unadjusted model (M1) was also estimated. Listwise deletion of missing values yielded a final analytic sample of 3,073 counties. Six sensitivity analyses were interpreted as consistency checks rather than independent hypothesis tests: S1 restricted to primary taxonomy designation; S2 substituted pathologist density (207ZP0102X, 207ZP0101X, 207ZC0500X); S3 replaced mortality with incidence; S4 excluded top 1% density counties; S5 restricted to non-metropolitan counties (RUCC ≥ 4) [44, 45]; S6 used RUCC as the sole covariate. No formal correction for multiple comparisons (e.g., Bonferroni) was applied to the sensitivity and exploratory analyses, because these analyses were designed to evaluate the robustness of a single pre-specified primary association (M2) under alternative specifications, rather than to test independent hypotheses; only the primary model (M2) constitutes a confirmatory hypothesis test. Following an unexpected null association within S5, six further analyses were conducted post hoc: S7 substituted an independent 2026 NPPES workforce extract [46], geocoded using the corresponding 2025 Q4 HUD crosswalk release [47], while holding the population denominator fixed at 2021; S8 stratified non-metropolitan counties by USDA-defined urban-population tiers (RUCC 4–5 versus 6–9) [45]; S9 tested a density-by-rurality interaction in the full sample; S10 excluded outliers within the non-metropolitan subsample; and S11 and S11b re-estimated the association using negative binomial regression on raw mortality counts, with a total-population offset (S11) and an indirect age-standardized expected-deaths offset (S11b), respectively; negative binomial regression was selected over Poisson regression based on a Poisson dispersion statistic indicating substantial overdispersion. Model diagnostics for M2 included variance inflation factors, Breusch-Pagan and Jarque-Bera tests, and the exposure variable's partial R^2 .

Spatial Regression Analysis

Spatial autocorrelation in OLS residuals was assessed using Moran's I with a row-standardized Queen contiguity weights matrix (2021 TIGER/Line shapefile) [48], with significance evaluated via permutation inference (999 permutations, $\alpha = 0.05$); four counties, all in Hawaii, had no Queen-contiguity neighbors under this specification. A spatial error model (SEM) was estimated using maximum likelihood and preferred over a spatial lag specification based on Lagrange multiplier diagnostics (LM-error = 598.08, $p < 0.001$; LM-lag = 555.44, $p < 0.001$). As a robustness check, the SEM was re-estimated using a k-nearest-neighbors ($k = 5$) weights matrix, which by construction has no islands; results were consistent with the Queen contiguity specification. Analyses used the libpysal and spreg libraries.

Decision-Analytic Scenario Modeling

AI deployment was modeled as an additive increment to county-level radiologist density. Three scenarios were anchored to the nonzero-county density distribution within the analytic sample ($n = 1,739$): low ($\Delta AI = 2.42$ per 100,000, 25th percentile), moderate ($\Delta AI = 6.05$, median), and high ($\Delta AI = 11.63$, 75th percentile). The severe shortage subgroup was defined as counties below the 25th percentile threshold (density < 2.42 ; $n = 1,769$). Mortality differentials were estimated as:

$$\Delta \text{mortality} = \beta \times [\log(\text{density} + \Delta AI + 1) - \log(\text{density} + 1)]$$

Analyses used β from the primary OLS model (M2) and were conducted for the extreme shortage subgroup (density = 0; $n = 1,334$) and severe shortage subgroup (density < 2.42 ; $n = 1,769$), both defined within the analytic sample. All scenario results reflect modeled associations under stated assumptions and should not be interpreted as causal predictions.

Figure Generation and Software

County-level maps were generated using geopandas and matplotlib, projected to Albers Equal Area (EPSG:5070), with Alaska and Hawaii repositioned as insets. Figure 1 displays the shortage classification across all 3,135 counties (extreme: density = 0; severe: density < 2.42 ; non-shortage: density ≥ 2.42). Figure 3 displays the projected mortality differential under the moderate scenario as a continuous choropleth. All analyses were conducted in Python 3.

Continuous choropleth map displaying the modeled mortality differential (deaths per 100,000 population) associated with a moderate AI-equivalent radiologist density increment ($\Delta AI = 6.04$ per 100,000) across all counties with available mortality data ($n = 3,073$), spanning extreme shortage (density = 0; $n = 1,334$), severe shortage (density < 2.42 ; $n = 1,767$), and non-shortage (density ≥ 2.42 ; $n = 1,306$) subgroups. Counties with suppressed mortality data (small population) are shown in light grey; counties without a shapefile or dataset match are shown in dark grey. Alaska and Hawaii are shown as insets. Map projected to Albers Equal Area (EPSG:5070).

Declarations

Author contributions

Y.Z. conceived and designed the study, acquired and processed the data, conducted the statistical analyses, generated the figures, and wrote the manuscript.

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Additional Information

Competing interests

The author declares no competing interests.

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Figures

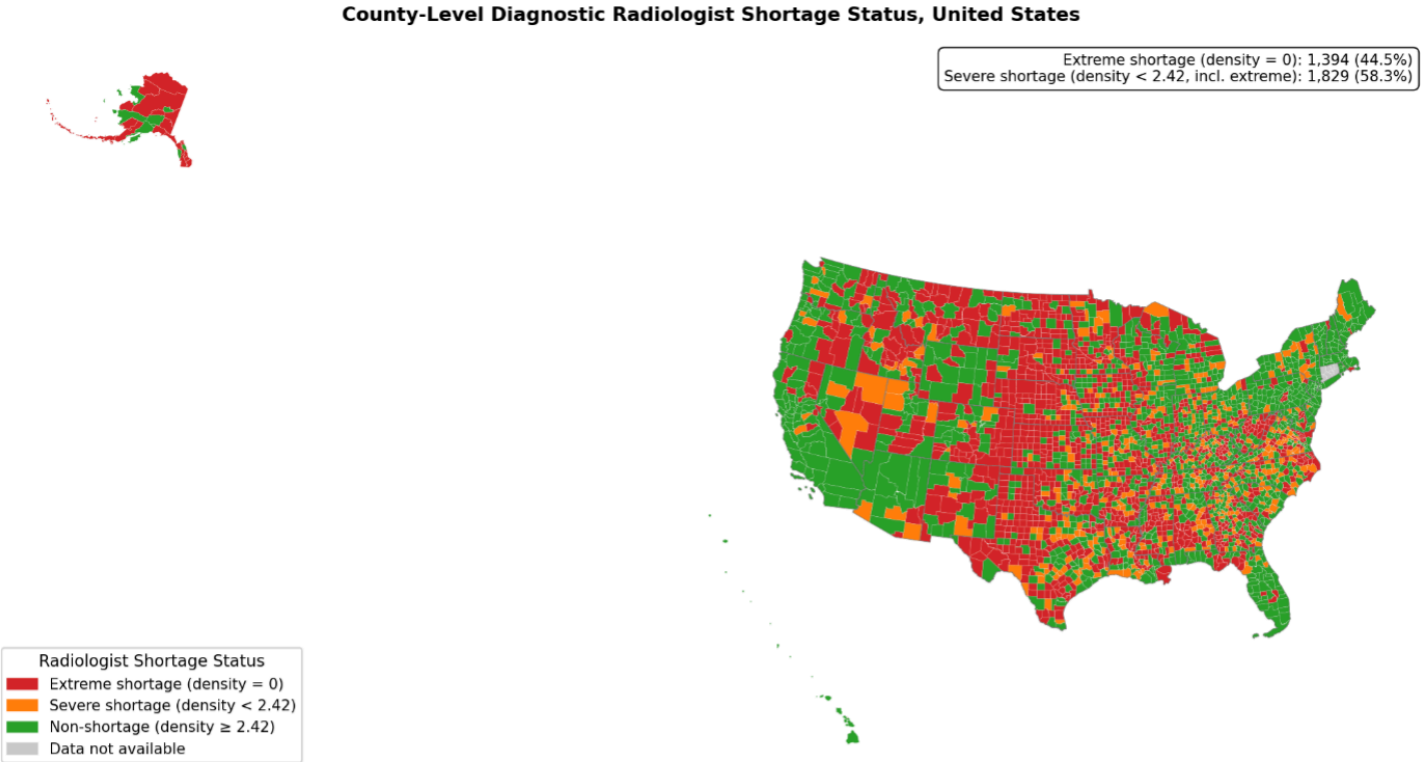


Figure 1

County-level diagnostic radiologist workforce density classification across the United States.

Counties (N = 3,135) are classified into three shortage categories based on radiologist density per 100,000 population: extreme shortage (density = 0; n = 1,394, 44.5%), severe shortage (density < 2.42 per 100,000, inclusive of extreme shortage counties; n = 1,829, 58.3%), and non-shortage (density ≥ 2.42 per 100,000; n = 1,306, 41.7%). Alaska and Hawaii are shown as insets. County boundaries derived from U.S. Census Bureau TIGER/Line 2021 shapefiles. Map projected to Albers Equal Area (EPSG:5070).

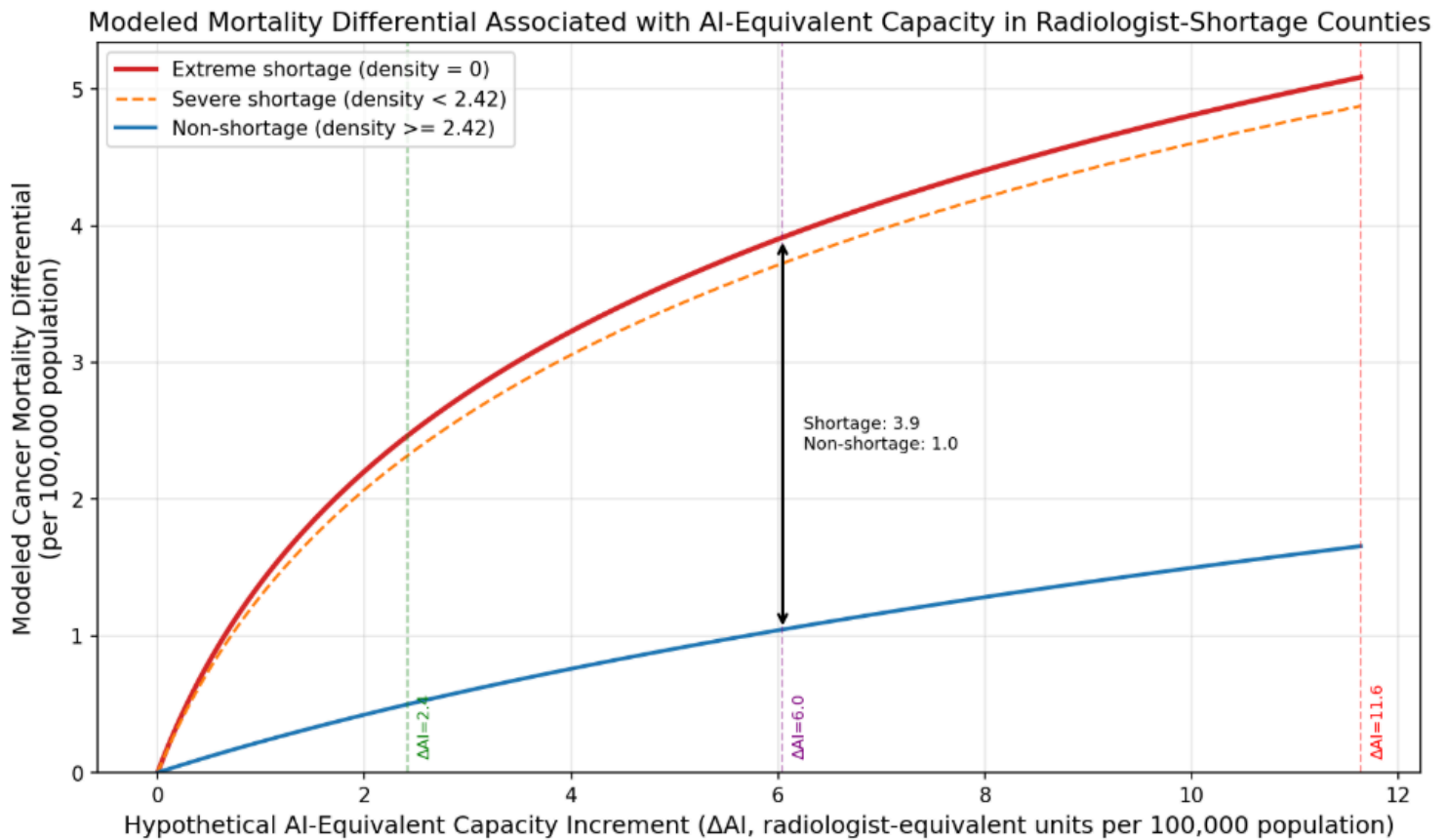


Figure 2

Modeled cancer mortality differential associated with AI-equivalent radiologist capacity augmentation across shortage subgroups.

Estimated mortality differential (deaths per 100,000 population) is plotted as a continuous function of a hypothetical AI-equivalent capacity increment (ΔAI , expressed in radiologist-equivalent units per 100,000 population) for three county subgroups within the analytic sample: extreme shortage (density = 0; $n = 1,334$), severe shortage (density < 2.42; $n = 1,769$), and non-shortage (density ≥ 2.42 ; $n = 1,304$). Vertical dashed lines indicate the three scenario anchors, anchored to the nonzero-county density distribution ($n = 1,739$): low ($\Delta AI = 2.42$, 25th percentile), moderate ($\Delta AI = 6.05$, median), and high ($\Delta AI = 11.63$, 75th percentile). Estimates derived from the primary OLS regression coefficient (M2: $\beta = -2.01$) using the formula $\Delta \text{mortality} = \beta \times [\log(\text{density} + \Delta AI + 1) - \log(\text{density} + 1)]$. Results reflect modeled associations and should not be interpreted as causal predictions.

**Modeled Cancer Mortality Differential Under AI-Equivalent Capacity Augmentation
Moderate Scenario ($\Delta AI = 6.04$ radiologist-equivalent units per 100,000)**

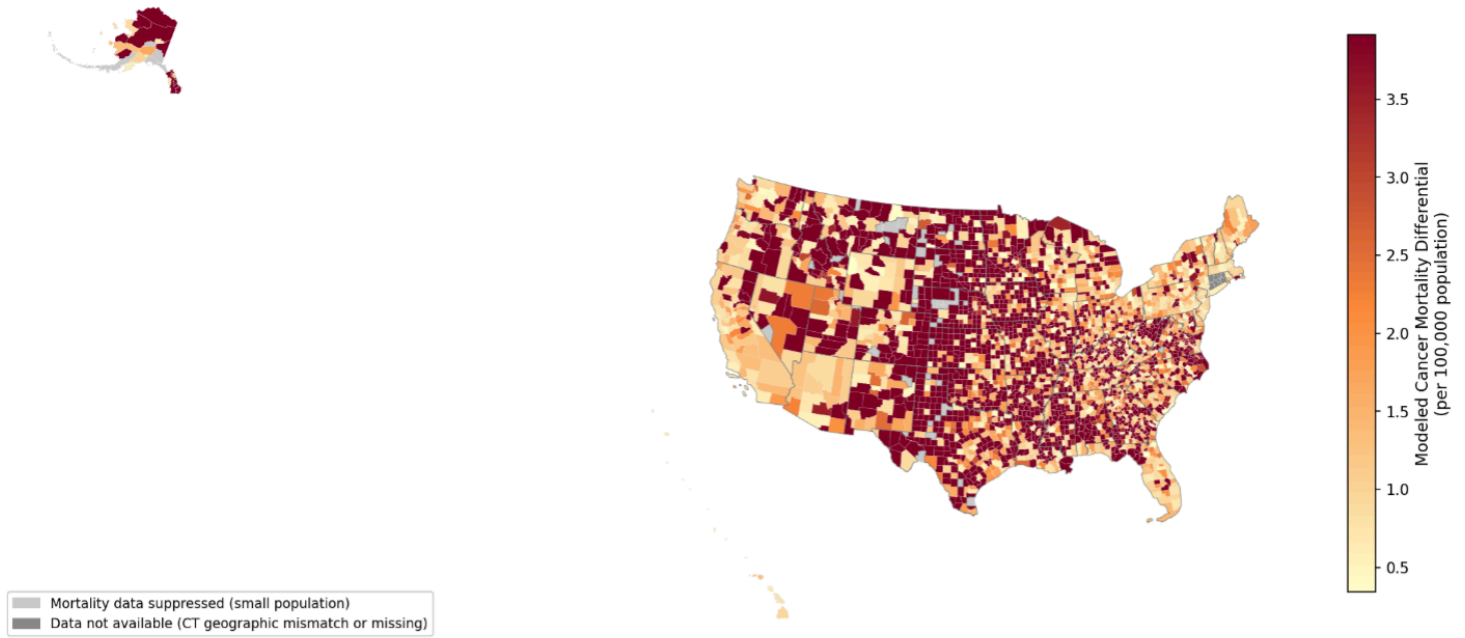


Figure 3

County-level projected cancer mortality differential under the moderate AI-equivalent capacity scenario.

Continuous choropleth map displaying the modeled mortality differential (deaths per 100,000 population) associated with a moderate AI-equivalent radiologist density increment ($\Delta AI = 6.04$ per 100,000) across all counties with available mortality data ($n = 3,073$), spanning extreme shortage (density = 0; $n = 1,334$), severe shortage (density < 2.42; $n = 1,767$), and non-shortage (density ≥ 2.42 ; $n = 1,306$) subgroups. Counties with suppressed mortality data (small population) are shown in light grey; counties without a shapefile or dataset match are shown in dark grey. Alaska and Hawaii are shown as insets. Map projected to Albers Equal Area (EPSG:5070).