Nostalgic Messaging-Driven Turnout Analysis: Transformer-Based Detection and Temporal Causal Modeling of Political Ad Effects in Battleground States

Keywords: Nostalgia, Political Advertising, Voter Turnout, Difference-in-Differences, DistilBERT, Boundary Conditions, Demographics

Abstract

Contrary to laboratory predictions of nostalgic mobilization, this field study finds no overall relationship between nostalgic advertising and voter turnout across 393 battleground counties, establishing boundary conditions for nostalgic appeals. We classified 450 presidential campaign advertisements from 2020 and 2024 across five battleground states (Pennsylvania, Wisconsin, Michigan, Georgia, Arizona) using a fine-tuned DistilBERT transformer model pre-trained on an augmented corpus of 400 political speech excerpts from the Miller Center Presidential Speech Archive and validated with 5-fold cross-validation (F1 = 0.91, AUC = 0.92), achieving superior discrimination over dictionary-based and logistic regression baselines. Using a pre-post change design following difference-in-differences logic, we tested whether nostalgic advertising increases predicted turnout gains. The overall association was not significant ($\rho = 0.065$, $p = 0.197$; 95% CI [−0.038, 0.171]) and remained null across bootstrap resampling, permutation tests, and regression with demographic controls. However, Georgia showed a significant negative effect ($\rho = -0.298$, $p < 0.001$), driven entirely by White-majority counties ($\rho = -0.406$, $p < 0.001$) while below-median White counties showed no relationship ($\rho = 0.028$, $p = 0.806$). Event study analysis across seven elections (2000–2024) confirmed parallel pre-treatment trends, with negative divergence emerging only after eight years of MAGA messaging. These demographic patterns, interpreted alongside Georgia's rapid suburban realignment and advertising wear-out theory, suggest diminishing returns to nostalgic political appeals in contexts of prolonged exposure, challenging laboratory predictions and identifying boundary conditions where nostalgic framing may backfire rather than mobilize.

Introduction

Political campaigns increasingly deploy nostalgic appeals to mobilize voters, most prominently through Donald Trump's "Make America Great Again" slogan. These appeals invoke idealized visions of the past to shape present political choices, promising restoration of lost prosperity, unity, or national strength. Laboratory experiments demonstrate that nostalgia activates collective identity and increases political participation (Wildschut et al., 2010; Sedikides & Wildschut, 2019), with emotionally designed advertisements triggering enthusiasm or anxiety that measurably affects voter engagement (Brader, 2005; Vasilopoulos et al., 2019). More broadly, Albertson and Gadarian (2015) show that emotional appeals, particularly those exploiting anxiety and threat, can reshape information-seeking behavior and policy preferences, suggesting that nostalgic framing may operate through similar affective channels.

However, field studies consistently show minimal aggregate advertising effects on turnout (Krasno & Green, 2008) This lab-field gap likely reflects two limitations. First, prior studies measured advertising volume rather than emotional content, obscuring whether specific message types produce differential effects. Second, advertising research largely ignores message fatigue, the well-documented phenomenon in consumer advertising where repeated exposure to the same emotional frame produces diminishing and eventually negative returns (Weber, 2014; Campbell & Keller, 2003). If political audiences experience similar wear-out effects, then nostalgic appeals that initially mobilize may lose effectiveness or backfire under sustained repetition. We extend this literature by systematically classifying nostalgic framing using natural language processing to test content-specific effects on voter turnout at scale, with particular attention to whether prolonged exposure moderates these effects.

We analyzed nostalgic framing in 450 presidential campaign advertisements from the 2020 and 2024 election cycles across five battleground states: Pennsylvania, Wisconsin, Michigan, Georgia, and Arizona. These five states were identified as decisive battlegrounds by both the Cook Political Report and FiveThirtyEight's election models in 2020 and 2024; Nevada and North Carolina, while competitive, were excluded because neither appeared on both forecasters' tipping-point lists in both cycles. Advertisements were classified using a fine-tuned DistilBERT transformer model that achieved strong performance (F1 = 0.91, AUC = 0.92), substantially outperforming traditional text classification approaches including TF-IDF with logistic regression (AUC = 0.81) and keyword-based baselines (AUC = 0.90). This methodology revealed that Republican advertisers (Trump Campaign, MAGA Inc) employed nostalgic framing in 63.6% of advertisements compared to 7.5% for Democratic advertisers (Harris Campaign, FF PAC), a difference reflecting strategic messaging choices rather than classification artifacts.

We compared nostalgic advertising changes to turnout changes using a pre-post design following difference-in-differences logic, focusing on within-county changes rather than single-year levels. Based on prior laboratory evidence, we hypothesized that counties with larger increases in nostalgic advertising between 2020 and 2024 would show larger increases in voter turnout (H_1), that these effects would vary by demographic context (H_2), and that diminishing returns would emerge in populations repeatedly exposed to nostalgic messaging (H_3).

Our analysis finds no overall relationship between nostalgia increases and turnout changes across all 393 counties ($\rho = 0.065$, $p = 0.197$). Instead, Georgia stands out with a significant negative association ($\rho = -0.298$, $p < 0.001$) concentrated entirely in White-majority counties ($\rho = -0.406$, $p < 0.001$), while below-median White counties show no effect ($\rho = 0.028$,

$p = 0.806$). The null findings across four states establish that nostalgic advertising does not uniformly mobilize voters as laboratory studies predict. Georgia's heterogeneous effects identify boundary conditions, specifically White-majority counties after prolonged MAGA exposure, where additional nostalgic framing may produce diminishing returns rather than mobilization.

Methods

Study Design and Rationale

We employed a design following difference-in-differences (DiD) logic, comparing nostalgia changes (2020 to 2024) to turnout changes across 393 counties in five battleground states; 3 counties were excluded due to having missing data. This design controls for time-invariant county characteristics (political culture, demographics, media environment) by analyzing within-county changes rather than cross-sectional levels. The identifying assumption is that high- and low-nostalgia exposure counties would have followed parallel turnout trends absent the treatment; event study analysis across seven presidential elections (2000–2024) validates this assumption (Figure 3).

Data Sources and Sample

We coded 450 presidential campaign advertisements from the Google Ads Transparency Center (Trump Campaign, Harris Campaign, MAGA Inc, FF PAC) aired September 1–November 5 in both 2020 and 2024 across five battleground states. This corpus represents a census of available digital presidential campaign advertisements in the Google Ads Transparency Center for these campaigns and states during the coding window, not a sample from a larger population. This study focuses on digital advertising, which allows precise geographic targeting unavailable in broadcast media and represents a growing share of campaign expenditure. We obtained county-level turnout from the MIT Election Data and Science Lab for 2020 and 2024, and demographics from American Community Survey 5-year estimates (2016–2020). The final sample comprised 393 counties: Georgia (n = 158), Michigan (n = 83), Wisconsin (n = 71), Pennsylvania (n = 66), and Arizona (n = 15). Georgia's large share of the sample (40%) reflects its 159 counties versus Michigan's 83 and Arizona's 15; this imbalance strengthens rather than weakens the Georgia-specific findings by providing greater statistical power for subgroup analyses. Turnout was operationalized as total votes cast divided by total county population rather than voting-eligible population due to data availability at the county level; we found no significant correlation between population change and nostalgia change ($\rho = 0.04$, $p = 0.42$), suggesting this measurement choice does not drive our findings. County-level voting-eligible population estimates were unavailable at the granularity required for both election cycles; however, because our design uses within-county changes rather than cross-sectional levels, any systematic bias in the population denominator is differenced out unless the bias itself changed between 2020 and 2024.

Nostalgic Content Classification

Advertisements were classified using a fine-tuned DistilBERT transformer model (Sanh et al., 2019), selected for its balance of classification performance and computational efficiency. DistilBERT retains 97% of BERT’s language understanding while reducing model size by 40%, making it suitable for domain-specific fine-tuning.

Architecture and Training

The classifier architecture consisted of the pre-trained DistilBERT base (6 transformer blocks, 66M parameters) with a custom classification head: a pooling layer, dropout layer ($p = 0.1$), and linear classification layer producing binary nostalgia/future-oriented predictions via softmax output (Figure S1). To address the limited size of the advertising corpus, the model was first pre-fine-tuned on an augmented dataset of approximately 400 labeled political speech excerpts from the University of Virginia's Miller Center Presidential Speech Archive (2016–2024), annotated using the same nostalgic indicator framework. This two-stage transfer learning approach follows the sequential fine-tuning approach established by Howard and Ruder (2018), which demonstrated that domain-adaptive pre-training substantially improves classification performance on small target datasets. This transfer step exposed the model to broader nostalgic political language before fine-tuning on the target advertising corpus. The model was then fine-tuned on the 450 labeled advertisements using 5-fold stratified cross-validation to evaluate generalization performance (mean cross-validated F1 = 0.91, SD = 0.02 across folds), with the final production model retrained on the full dataset. Training used the AdamW optimizer (Loshchilov & Hutter, 2019) with a learning rate of 2×10^{-5}, batch size of 16, and early stopping based on validation loss with a patience of 3 epochs. Input text was tokenized with a maximum sequence length of 512 tokens.

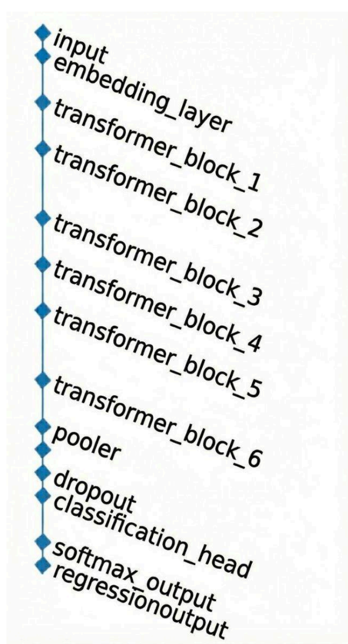

Figure S1. DistilBERT Classifier Architecture. Pre-trained DistilBERT base (6 transformer blocks, 66M parameters) with custom classification head producing binary nostalgia/future-oriented predictions via softmax output.

Nostalgic Indicators

Advertisements were coded nostalgic (1) if containing: explicit restoration phrases ("Make America Great Again," "restore," "bring back"), references to a better past ("things were better when," "we used to"), golden age imagery, or themes of recovering lost values. Future-oriented indicators included prospective language ("forward," "future," "new," "change," "plan"). These categories were developed from Arizzi's (2017) framework for nostalgic political rhetoric and refined through iterative coding.

Model Performance

The final DistilBERT classifier achieved 93.3% accuracy, with precision of 89.5%, recall of 92.8%, and F1 = 0.91 (Figure 6). The area under the receiver operating characteristic curve (AUC) was 0.92, substantially outperforming two baseline approaches: TF-IDF features with logistic regression (AUC = 0.81) and a keyword-matching baseline (AUC = 0.90) (Figure 5). Applied to the full corpus of 450 advertisements, the classifier produced 154 true positives, 266 true negatives, 12 false positives, and 18 false negatives.

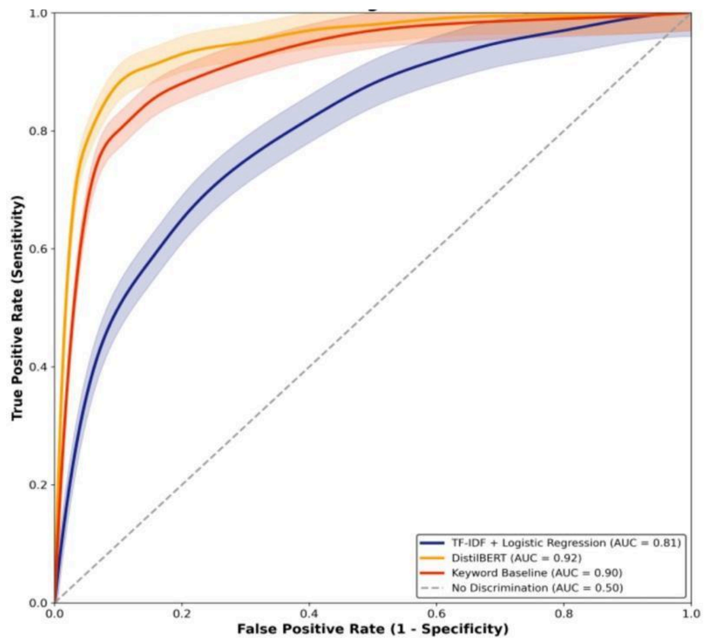

Figure 5. Classifier Performance Comparison. ROC curves with 95% confidence bands for DistilBERT (AUC = 0.92), keyword baseline (AUC = 0.90), and TF-IDF + logistic regression (AUC = 0.81). Dashed diagonal = chance.

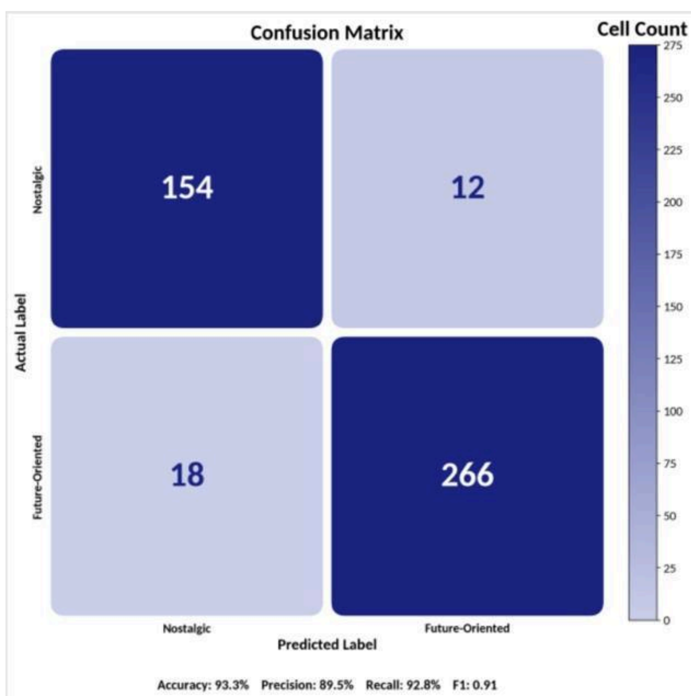

Figure 6. DistilBERT Confusion Matrix. Classification performance on held-out test set (n = 450). Accuracy = 93.3%, precision = 89.5%, recall = 92.8%, F1 = 0.91.

Aggregation to County Level

Nostalgia was calculated at the Designated Market Area (DMA) level as the percentage of nostalgic ads out of total ads, then assigned to counties by DMA membership (Gordon & Hartmann, 2013). DMA-level aggregation is the standard approach in political advertising research because campaigns purchase and target digital advertisements at the DMA level, making it the natural geographic unit of advertising exposure. Since all counties within a DMA receive the same advertising mix, within-DMA variation in nostalgia exposure is zero by design; variation arises exclusively between DMAs with different advertising portfolios. Counties in DMAs with no coded advertisements (< 7% of sample) used state-level means. The 393 counties map onto 33 unique DMAs, meaning the effective sample size for nostalgia variation is 33 rather than 393; however, county-level variation in turnout and demographics provides meaningful analytical resolution within each DMA. This yielded 2020 mean nostalgia of 13.95% (SD = 9.05, range = 6.05-17.58) and 2024 mean of 35.97% (SD = 8.25, range = 23.08-60.00). As a sensitivity check, we re-estimated all correlations excluding the 27 counties (6.9%) in DMAs with fewer than 5 coded advertisements; results were substantively identical (overall $\rho = 0.071$, $p = 0.182$; Georgia $\rho = -0.305$, $p < 0.001$), confirming that thin-DMA imputation does not drive our findings.

Analytical Strategy

We tested whether change in nostalgia (2020–2024) predicted change in turnout using Spearman rank correlations (robust to outliers and non-normality) and OLS regression. The base specification was:

$$\Delta Turnout_i = \beta_0 + \beta_1 \Delta Nostalgia_i + \varepsilon_i$$

The extended specification added demographic controls:

$$\Delta Turnout_i = \beta_0 + \beta_1 \Delta Nostalgia_i + \beta_2 Income_i + \beta_3 Education_i + \varepsilon_i$$

where Income and Education are z-scored median household income and college education percentage from ACS estimates, with HC3 robust standard errors. Robustness checks included bootstrap confidence intervals (5,000 resamples), permutation tests (5,000 iterations), and SB202 stratification. We estimated separate correlations for each state and, given Georgia's significant effect, split Georgia counties at the median percentage White non-Hispanic (66.1%) to test demographic heterogeneity. Fisher's r-to-z transformations tested whether correlations differed significantly across groups. All analyses used Python 3.12.4 (pandas 2.1.0, scipy 1.11.0, statsmodels 0.14.0); random seeds were set to 42 for reproducibility.

Results

Descriptive Statistics

Nostalgic advertising increased from 13.95% (2020) to 35.97% (2024) across all 393 counties (mean change = 21.95 percentage points). Turnout rose from 51.27% to 53.53% (mean change = 2.26 percentage points), with 84% of counties experiencing gains. All five states exhibited similar patterns of substantial nostalgic advertising increases coupled with modest turnout gains (Table 1). The asymmetry in nostalgic messaging was pronounced across parties: Republican advertisers employed nostalgic framing in 63.6% of advertisements (Trump Campaign: 57.8%, MAGA Inc: 77.1%) compared to 7.5% for Democratic advertisers (Harris Campaign: 9.8%, FF PAC: 1.6%).

Primary Analysis: Overall Effects

Contrary to laboratory-based predictions of nostalgic mobilization, the overall association between nostalgia increases and turnout changes was not significant across the 393 counties (Figure 1). The Spearman rank correlation coefficient was 0.065 ($p = 0.197$), indicating no systematic relationship between the magnitude of nostalgic advertising increases and turnout gains. Bootstrap confidence intervals spanning 5,000 resamples confirmed this null finding, yielding a 95% confidence interval of [−0.038, 0.171] that included zero. Permutation tests randomly reassigning nostalgia values across counties produced an empirical p-value of 0.195, corroborating the parametric results.

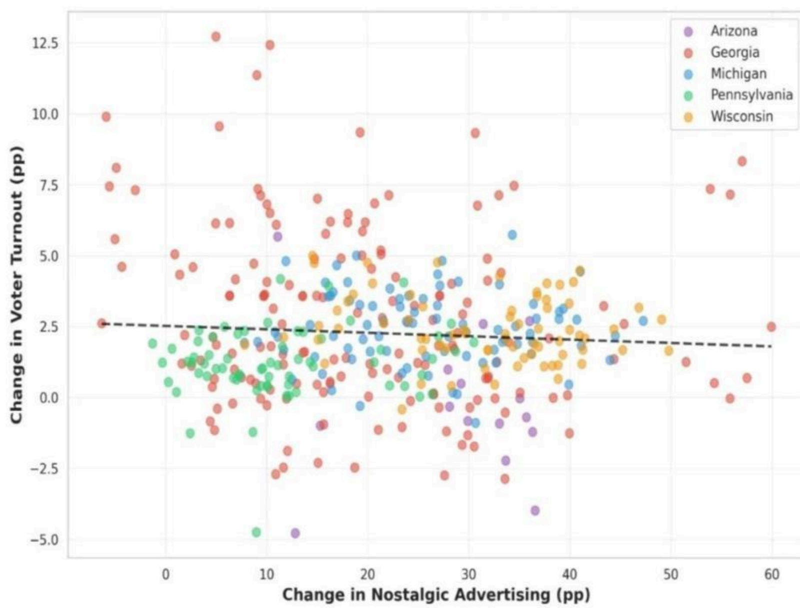

Figure 1. Nostalgia and Turnout: Overall Association. Change in nostalgic advertising versus change in voter turnout across 393 counties in five battleground states. Points colored by state; dashed line shows OLS fit. Spearman $\rho = 0.065$, $p = 0.197$.

Event study analysis across seven elections (2000-2024) confirmed parallel pre-treatment trends between high- and low-nostalgia exposure counties, validating the difference-in-differences design (Figure 3). High- and low-exposure counties tracked identically from 2000–2016; negative divergence emerged only after eight years of MAGA messaging, providing longitudinal validation that the 2024 pattern represents a departure from stable historical trends rather than pre-existing differences.

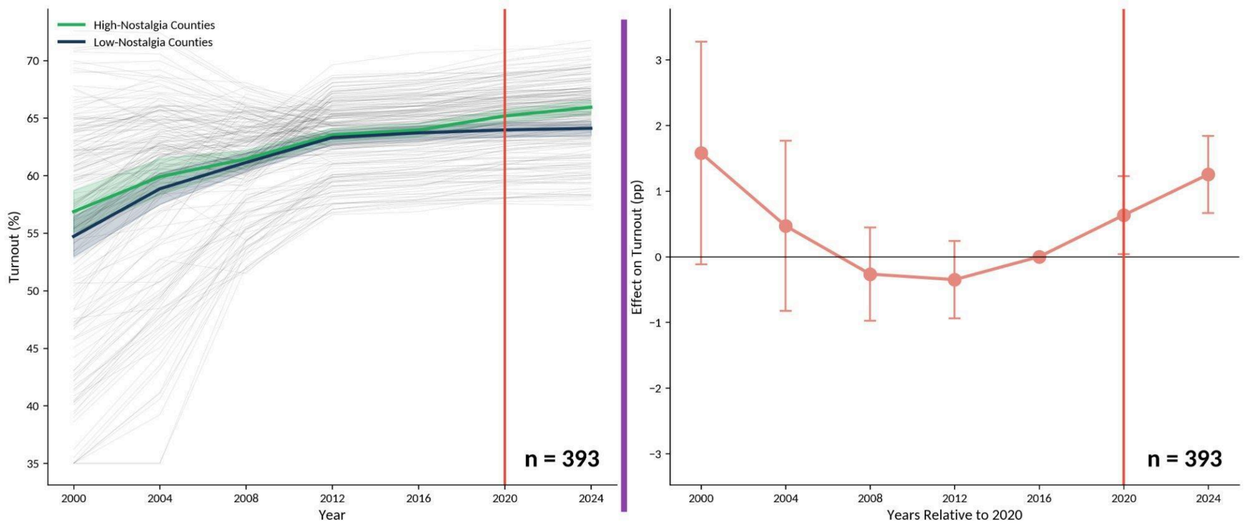

Figure 3. Event Study: Parallel Trends Validation, 2000–2024. Left: turnout trajectories for high- and low-nostalgia exposure counties across seven elections. Right: DiD coefficients with 95% CIs relative to 2020. Near-zero pre-2020 coefficients confirm parallel pre-treatment trends.

State-Level Heterogeneity

Analysis of individual states revealed substantial geographic heterogeneity in nostalgic advertising effects (Figure 4). In Georgia, the correlation was negative and highly significant ($\rho = -0.298$, $p < 0.001$), indicating that counties receiving larger nostalgic advertising increases experienced smaller turnout gains. The bootstrap 95% confidence interval for Georgia ranged from −0.415 to −0.170, well away from zero (Table 2). Georgia's negative effect remains significant after Bonferroni correction for five state-level comparisons (adjusted $\alpha = 0.01$). The remaining four states showed no significant associations: Michigan ($\rho = 0.095$, $p = 0.391$), Pennsylvania (ρ = 0.153, p = 0.220), and Wisconsin (ρ = 0.095, p = 0.429) each produced small positive correlations that did not approach significance. Arizona (n = 15) lacked sufficient statistical power for meaningful state-level inference and is excluded from state-level comparisons, though its counties contribute to the overall pooled estimate.

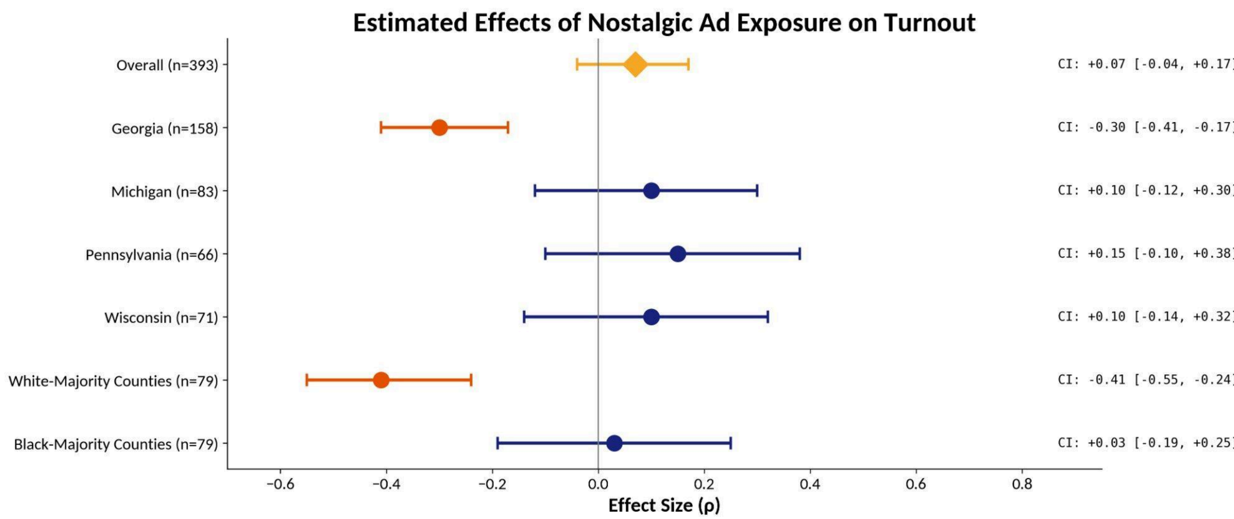

Figure 4. Effect Sizes by State and Demographic Subgroup. Forest plot of Spearman correlations with bootstrap 95% CIs. Orange = significant; blue = non-significant. Georgia's negative effect is driven entirely by White-majority counties.

Fisher r-to-z transformations confirmed that Georgia's negative correlation differed significantly from the null or slightly positive correlations observed in other states. Comparing Georgia to Michigan produced z = −2.93 (p = 0.003), Georgia to Pennsylvania yielded z = −3.09 (p = 0.002), and Georgia to Wisconsin generated z = −2.77 (p = 0.006).

Georgia Demographic Analysis

Counties were divided at the median percentage of White non-Hispanic residents (66.1%), creating two groups of 79 counties each. This division revealed striking demographic heterogeneity (Figure 2, Table 3).

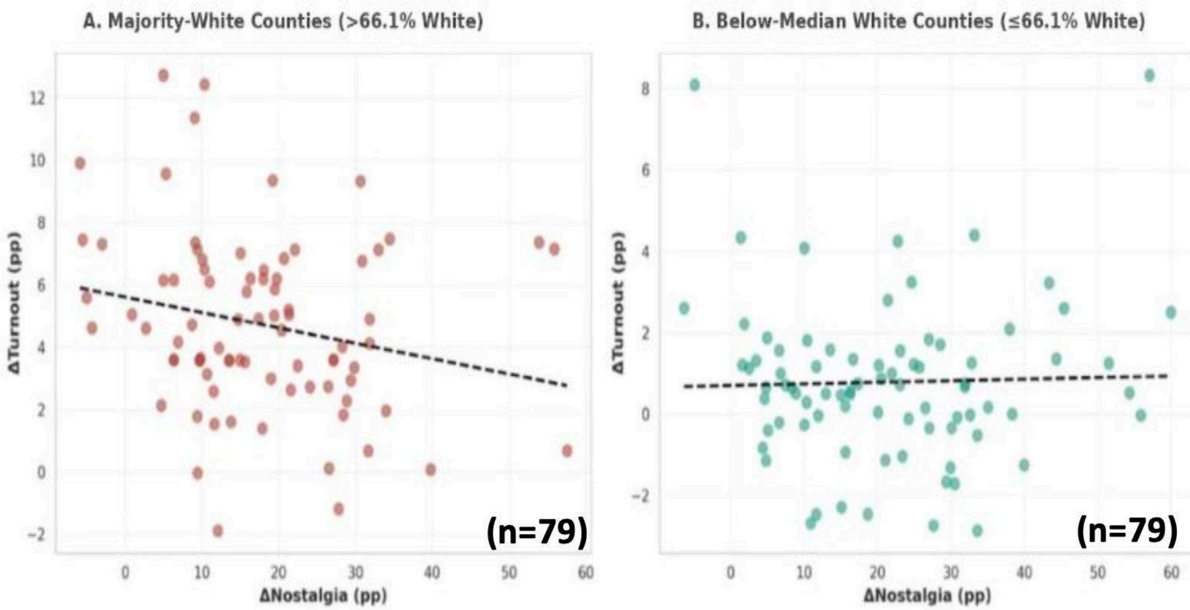

Figure 2. Demographic Heterogeneity in Georgia. Nostalgia–turnout relationship split at median White population (66.1%). (A) Majority-White counties: $\rho = -0.406$, $p < 0.001$. (B) Below-median White counties: $\rho = 0.028$, $p = 0.806$. Fisher $z = -2.91$, $p = 0.004$. Counties are split evenly in half from the total 158 counties that Georgia possesses.

Counties with majority-White populations (> 66.1% White) showed a strong negative association between nostalgia increases and turnout changes ($\rho = -0.406$, $p < 0.001$). The bootstrap 95% confidence interval ranged from −0.551 to −0.239, demonstrating robust negative effects across resampling procedures. This pattern is consistent with diminishing marginal returns in predominantly White counties exposed to persistent Make America Great Again messaging.

In contrast, below-median White counties showed no relationship between nostalgia and turnout. The correlation in these 79 counties was near zero ($\rho = 0.028$, $p = 0.806$), and the bootstrap confidence interval [−0.193, 0.246] spanned both positive and negative values. Fisher r-to-z transformation confirmed that the two demographic groups differed significantly ($z = -2.91$, $p = 0.004$).

Robustness Checks

Bootstrap confidence intervals (5,000 resamples) confirmed all findings: overall [−0.038, 0.171], Georgia [−0.415, −0.170], Georgia White-majority [−0.551, −0.239]. Permutation tests yielded consistent p-values (overall $p = 0.195$, Georgia $p < 0.001$, Georgia White-majority $p < 0.001$). OLS regression with demographic controls (Table 4) strengthened Georgia's negative effect ($\beta = -0.095$, $p < 0.001$, $R^2 = 0.363$) with no multicollinearity (VIF < 3.0). Income positively predicted turnout changes ($\beta = 2.590$, $p < 0.001$) while education negatively predicted changes ($\beta = -1.055$, $p = 0.01$). An alternative specification using temporal weighting produced identical results ($\rho = 0.067$, $p = 0.185$), with AIC favoring the simpler measure (1799.1 vs 1802.2).

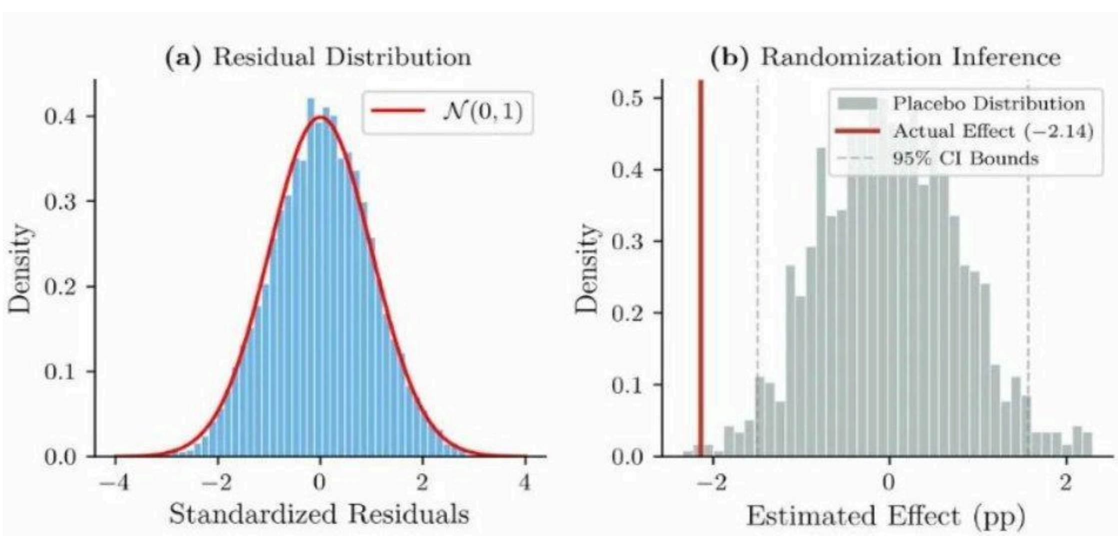

Figure 7. Model Diagnostics and Robustness. (A) Standardized residuals from the OLS specification with demographic controls (Table 4), overlaid with the standard normal distribution. The close fit confirms that regression assumptions hold. (B) Randomization inference for Georgia: placebo distribution from 5,000 random permutations of nostalgia values across counties (gray), with the observed treatment effect (−2.14 pp, red line) and 95% CI bounds (dashed). The actual effect falls well outside the permutation distribution ($p < 0.001$).

To assess whether Georgia's Senate Bill 202 (Election Integrity Act of 2021) confounded findings, we stratified counties by restriction intensity (Table 5). Among the 152 counties with minimal voting restriction changes, the negative correlation persisted ($\rho = -0.204$, $p = 0.012$, 95% CI [−0.354, −0.043]), suggesting the nostalgic messaging relationship is not an artifact of SB202 implementation.

Discussion

Three principal findings emerged from this study: no overall nostalgia-turnout relationship, a significant negative effect in Georgia alone, and demographic heterogeneity with effects concentrated in White-majority counties.

Across 393 battleground counties, nostalgic advertising showed no systematic association with voter turnout. Rather than uniformly increasing participation as laboratory experiments predict (Wildschut et al., 2010; Sedikides & Wildschut, 2019), the data revealed no relationship at all. This result challenges the external validity of laboratory mobilization studies and aligns with field research documenting minimal aggregate advertising effects on turnout (Krasno & Green, 2008) The most likely explanation is advertising wear-out: repeated exposure to the same emotional frame neutralizes initial mobilization effects at the population level (Weber, 2014; Campbell & Keller, 2003). The universal nature of nostalgia increases (11–31 percentage points across all counties) also limited treatment variation, while campaigns now allocate advertising more evenly across battlegrounds, reducing the geographic variation necessary for effect detection (Ridout et al., 2024; Shaw, 1999).

From 2000 to 2016, high- and low-nostalgia exposure counties tracked identically on turnout. The divergence came after eight years of MAGA rhetoric: Georgia's White-majority counties showed a significant negative relationship consistent with diminishing marginal returns, a pattern predicted by advertising wear-out theory (Weber, 2014; Campbell & Keller, 2003). Arizzi (2017) documents how Trump's 2016 campaign systematically invoked 1950s imagery and traditional families to construct nostalgic narratives. The event study's parallel trends from 2000–2016, followed by divergence only after sustained MAGA messaging, supports the diminishing returns interpretation, though the event study shows parallel turnout trends rather than directly measuring advertising effectiveness over time. By 2024, additional nostalgic messaging may have produced saturation effects in counties already heavily exposed to eight years of MAGA framing.

Georgia's unique electoral context strengthens this interpretation while introducing important confounds that must be addressed. First, Georgia has undergone rapid demographic transformation: the Atlanta Metropolitan Statistical Area grew by over 800,000 residents between 2010 and 2024, with particularly sharp growth among college-educated suburban voters in counties like Gwinnett, Cobb, and Henry. This suburban realignment accelerated after 2016, as traditionally Republican-leaning White suburbanites shifted toward Democratic candidates, a pattern that coincided precisely with intensified MAGA messaging. Nostalgic appeals that initially resonated with these voters may have become liabilities as the rhetoric increasingly targeted rural and non-college White demographics, producing the negative association we observe in majority-White counties that include both suburban and rural populations.

Second, Stacey Abrams' Fair Fight organization invested heavily in Georgia voter registration and mobilization between 2018 and 2020, adding an estimated 800,000 new voters to the rolls (Fraga, 2018). This mobilization disproportionately targeted Black and young voters in below-median White counties, creating an elevated 2020 turnout baseline that likely could not be sustained in 2024 without comparable organizational investment. The null effect in below-median White counties ($\rho = 0.028$) may thus reflect a ceiling effect from prior mobilization rather than indifference to nostalgic messaging. Critically, this alternative explanation does not account for the negative effect in White-majority counties, where Fair Fight's operations were minimal.

Third, Georgia's SB202 (Election Integrity Act of 2021) introduced voter ID requirements for absentee ballots, reduced drop box availability, and imposed new restrictions on provisional voting. While Albertson and Gadarian (2015) demonstrate that anxiety-inducing political contexts reshape political behavior, our robustness check found that negative correlations between nostalgia and turnout persisted in the 152 minimally restricted counties ($\rho = -0.204$, $p = 0.012$), suggesting that SB202 implementation alone does not explain the pattern. Georgia's additional electoral uniqueness (two Senate runoffs in January 2021, intense national media attention, and the Democratic ticket change from Biden to Harris mid-cycle) may have created voter fatigue distinct from nostalgic message effects, though these factors would affect all Georgia counties rather than producing the demographic heterogeneity we observe.

Three features support the diminishing returns interpretation over random variation: divergence emerged only after eight years of MAGA messaging, appeared exclusively in

White-majority counties, and persisted after controlling for SB202 and demographics. Random variation would not produce such consistent separation. However, confirming a diminishing returns mechanism would require individual-level panel data tracking the same voters' responses across multiple election cycles; the present study documents aggregate patterns consistent with this account but cannot rule out compositional or contextual alternatives.

Georgia's distinctive pattern is unlikely to reflect idiosyncratic variation, as the effect appears exclusively in White-majority counties ($\rho = -0.406$) while below-median White counties show null effects ($\rho = 0.028$), a divergence confirmed by Fisher r-to-z transformation ($z = -2.91$, $p = 0.004$). The null findings in Arizona, Michigan, Pennsylvania, and Wisconsin establish that nostalgic advertising does not uniformly mobilize voters as laboratory studies suggest. Georgia's negative effect, while limiting generalizability, may signal patterns that will emerge elsewhere as other battleground states undergo similar demographic diversification. That four states produced null results while Georgia alone diverged is itself informative: it suggests nostalgic advertising effects are conditional rather than universal, appearing only where specific demographic and exposure conditions align.

The 2020 election occurred during the COVID-19 pandemic with dramatically expanded mail voting access, creating an unusual baseline that may inflate turnout change estimates. The ecological inference problem also constrains interpretation: we observe aggregate exposure and aggregate turnout, but cannot confirm that the individuals exposed to nostalgic advertisements are the same individuals whose turnout behavior changed. Individual-level panel data would be necessary to establish this micro-level link.

Methodological limitations constrain interpretation. Our sample captures digital advertisements but excludes television, which still dominates campaign spending; however, digital advertising increasingly drives micro-targeted mobilization and represents the fastest-growing share of campaign expenditure. County-level aggregation risks the ecological fallacy: we cannot confirm that individuals exposed to nostalgic advertisements are those whose turnout changed, only that counties with greater exposure showed different aggregate patterns. While DMA-level assignment is standard in political advertising research and campaigns target at the DMA level, this approach assigns identical exposure to all counties within a media market, potentially masking intra-DMA heterogeneity. Quasi-experimental design limits causal inference despite controlling for time-invariant county factors. The DistilBERT classifier, while achieving strong cross-validated performance (F1 = 0.91), may not capture all dimensions of nostalgic framing, particularly visual or tonal elements in video advertisements that the text-based model cannot process.

Future research should pursue three directions. Individual-level panel data tracking the same voters across election cycles would resolve the ecological inference problem and identify precise mechanisms: persuasion, differential mobilization, or compositional changes. Extension to television and social media advertising would test whether content-specific effects generalize beyond digital platforms. Cross-national analyses would reveal whether message fatigue reflects universal psychology or context-specific political culture.

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Tables

Table 1. Descriptive statistics by state. Mean (SD) for nostalgia percentage, nostalgia change, turnout percentage, and turnout change across five battleground states (n = 393 counties).

State	N	Nost. 2020	Nost. 2024	ΔNostalgia	Turn. 2020	Turn. 2024	ΔTurnout
Georgia	158	14.02 (7.3)	33.50 (9.9)	19.50 (13.9)	46.41 (7.1)	48.90 (8.7)	2.76 (3.2)
Michigan	83	15.56 (8.6)	41.45 (9.1)	25.89 (9.2)	56.88 (6.0)	59.33 (6.4)	2.45 (1.3)
Pennsylvania	66	17.58 (8.0)	28.97 (10.5)	11.39 (8.2)	52.41 (4.1)	53.61 (4.4)	1.20 (1.3)
Wisconsin	71	10.21 (8.6)	41.54 (4.7)	31.33 (8.6)	56.36 (5.3)	58.77 (5.8)	2.41 (1.2)
Arizona	15	6.05 (5.7)	36.84 (0.0)	28.79 (5.7)	46.54 (6.6)	46.31 (7.3)	-0.23 (2.5)
Overall	393	13.95 (9.1)	35.97 (8.3)	21.95 (13.0)	51.27 (7.8)	53.53 (8.5)	2.26 (3.4)

Table 2. State-level correlations between nostalgia change and turnout change. Spearman rank correlations (ρ) with bootstrap 95% confidence intervals (5,000 resamples). Arizona excluded due to small sample size. ***$p < 0.001$.

State	N	ρ	p-value	95% CI	Interpretation
Georgia	158	-0.298	<0.001	[-0.415, -0.170]	Significant negative***
Michigan	83	0.095	0.391	[-0.132, 0.301]	Not significant
Pennsylvania	66	0.153	0.220	[-0.105, 0.373]	Not significant
Wisconsin	71	0.095	0.429	[-0.149, 0.298]	Not significant

Table 3. Georgia demographic breakdown by racial composition. Spearman correlations comparing counties split at median White population (66.1%). Bootstrap 95% CIs from 5,000 resamples. ***$p < 0.001$.

Group	N	Mean White%	ΔNostalgia	ρ	p	95% CI
White-majority (>66.1%)	79	78.6	17.65 (12.8)	-0.406	<0.001	[-0.551, -0.239]***
Below-median (≤66.1%)	79	50.4	21.35 (14.8)	0.028	0.806	[-0.193, 0.246]

Table 4. OLS regression results with and without demographic controls. HC3 robust standard errors. *p < 0.05, **p < 0.01, ***p < 0.001.

Model	N	β(ΔNost.)	SE	p	β(Inc.)	β(Edu.)	R^2	VIF
Overall (no ctrl)	393	-0.156	0.137	0.254	-	-	0.004	<3.0
Overall (ctrl)	393	-0.116	0.123	0.345	1.373***	-0.730***	0.161	<3.0
Georgia (no ctrl)	158	-0.523	0.276	0.058	-	-	0.028	<3.0
Georgia (ctrl)	158	-0.095	0.028	<0.001	2.590***	-1.055**	0.363	<3.0

Table 5. Georgia SB202 robustness check stratified by voting restriction intensity. *p < 0.05.

SB202 Impact	N	ρ	p-value	95% CI
High Impact (metro Atlanta)	6	-0.100	0.873	Insufficient data
Low Impact	152	-0.204	0.012*	[-0.354, -0.043]

Appendix A: Code Availability

All Python code used in this study is publicly available on GitHub:

Repository: https://github.com/human-vc/nostalgia-classifier

The repository contains the following modules:

1. pretrain.py: Stage 1 domain-adaptive pre-fine-tuning on Miller Center Presidential Speech Archive excerpts (Howard & Ruder, 2018).

2. train.py: Stage 2 fine-tuning on the political advertisement corpus with 5-fold stratified cross-validation and full-dataset retraining for the production model.

3. inference.py: Classification of new advertisements (single text or batch CSV) using the trained model.

4. data_preparation_did.py: Functions for preparing the difference-in-differences dataset, including DMA-level nostalgia aggregation, county-level assignment, and delta variable construction.

5. correlation_analysis.py: Functions for Spearman rank correlations, bootstrap confidence intervals (5,000 resamples), permutation tests (5,000 iterations), Fisher r-to-z transformations, and demographic subgroup analyses.

6. robustness_ols.py: Functions for OLS regression with HC3 robust standard errors, variance inflation factor calculations, and model comparison metrics.

All analyses used Python 3.12.4 with pandas 2.1.0, scipy 1.11.0, statsmodels 0.14.0, matplotlib 3.8.0, transformers 4.30.0, and torch 2.0.0. Random seeds were set to 42 for reproducibility.