

Issue Activation and the Limits of Partisan Absorption: Immigration and Vote Choice in the 2012, 2016, and 2020 U.S. Presidential Elections

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Abstract

When do issues aligned with partisanship nonetheless exert independent effects on vote choice? Theories of partisan sorting predict that as issue positions converge with party identity, their independent electoral influence should diminish. We refine this expectation by introducing issue activation, a proposed process by which sustained presidential governance may shift a sorted issue from a background component of partisan identity toward an independent evaluative criterion. Using the 2012, 2016, and 2020 American National Election Studies, we show that immigration attitudes were not an independent predictor of presidential vote choice in either 2012 or 2016. By 2020, after four years of intensive immigration governance, immigration attitudes exerted a significant direct effect even in fully specified models controlling for partisanship and related predispositions. Mediation analysis provides suggestive evidence that immigration's influence shifted from operating primarily through partisan identity in 2016 to functioning as a more direct evaluative criterion in 2020. This pathway change occurred even as partisan sorting on immigration intensified. A parallel analysis of trade attitudes reveals no comparable activation pattern, suggesting that activation may depend on governance that reinforces rather than cross-pressures partisan alignments. These findings suggest that partisan sorting does not permanently determine how issues influence vote choice; under specific conditions, sorted issues can be reactivated as autonomous predictors.

Keywords: Issue activation, Immigration policy, Voting behavior, Valence politics, Partisan sorting, Presidential governance.

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Introduction

The relationship between issue attitudes and vote choice is among the most studied questions in political behavior. A large literature documents how salient issues become aligned with partisan identities through processes of elite cueing, issue evolution, and partisan sorting (Carmines and Stimson 1989; Levendusky 2009; Mason 2018). As issues align with partisanship, their independent effects on vote choice are expected to diminish. Once an issue is “sorted,” it becomes largely redundant with party identification as a predictor of electoral choice.

Yet the conditions under which sorted issues might regain independent electoral influence remain underspecified. Most scholars of sorting and polarization would accept that independent issue effects are possible in principle (even Lenz [2012], who provides the strongest case for partisan-led issue alignment, does not argue they are impossible). What is less clear is when and how such effects emerge. Immigration provides a useful case for investigating this question. Over the past two decades, immigration attitudes have become increasingly correlated with partisan identification (Abrajano and Hajnal 2015; Hajnal and Rivera 2014). By the logic of partisan sorting, immigration’s independent predictive power should have declined. Yet this expectation does not specify how the pathway through which an issue influences vote choice might change under different political conditions.

We seek to identify these conditions. Drawing on literatures on issue salience (Krosnick 1988; RePass 1971), agenda-setting (Iyengar and Kinder 1987), and campaign enlightenment (Gelman and King 1993), we propose that sustained presidential governance on a policy domain can shift the pathway through which a sorted issue influences vote choice – from indirect (mediated through partisan identity) to direct (operating as an autonomous evaluative criterion). We call this proposed process issue activation, and we specify it as conditional on governance that reinforces rather than disrupts the partisan structure of an issue.

The Trump presidency provides an opportunity to examine this possibility. Immigration

was central to Trump’s 2016 campaign but functioned primarily as a signal of partisan identity. Between 2017 and 2020, immigration became a governing reality through the travel ban, family separation policy, border wall construction, and sustained administrative enforcement. We investigate whether this sustained governance was associated with a change in how immigration attitudes related to vote choice.

Using fully specified logistic regression models estimated on the 2012, 2016, and 2020 ANES, we find evidence consistent with this account. Immigration attitudes did not independently predict vote choice in either 2012 or 2016. By 2020, immigration was a significant independent predictor ($p < .001$). The primary comparison, between the 2012 baseline and the 2020 post-governance election, is statistically robust (Wald $p < .001$). The 2016 evidence is supplementary: immigration was non-significant, consistent with the activation interpretation, but the 2016-2020 coefficient difference is not statistically significant, and we cannot rule out that differences in sample size, statistical power, or collinearity account for the different significance levels across those two elections. Causal mediation analysis (Imai, Keele, and Tingley 2010) provides suggestive evidence of a pathway shift: in 2016, immigration’s influence appears to have operated primarily through partisan identity; in 2020, it exerted a more direct, unmediated effect. These patterns emerged even as sorting intensified from $r = .057$ (2012) to $.335$ (2016) to $.412$ (2020). A parallel analysis of trade attitudes shows no comparable pattern, helping to specify a scope condition: governance that cross-pressures partisan alignments does not appear to produce activation.

Simultaneously, candidate honesty evaluations declined substantially from 2016 to 2020, consistent with a shift from character-based to policy-performance-based evaluation as voters accumulated governance information.

These findings contribute in three ways. First, we introduce issue activation as a plausible account rather than a confirmed causal mechanism, for when and how sorted issues might develop independent pathways to vote choice. Second, we document that sorting and significant independent issue effects can coexist in cross-sectional data, and we pro-

pose specific scope conditions under which this may occur. Third, we extend Gelman and King's (1993) enlightenment framework across election cycles, suggesting that governance and not just campaigning, may serve an informational function shaping how voters weight considerations.

Theory: From Partisan Sorting to Issue Activation

Issue Evolution, Sorting, and the Absorption Tendency

The issue evolution framework holds that new cleavages emerge when partisan elites take divergent positions on previously cross-cutting issues (Carmines and Stimson 1989). Over time, this produces partisan sorting: the alignment of issue positions with party identification (Levendusky 2009; Mason 2018). Adams (1997) documents this for abortion; immigration research suggests a parallel trajectory accelerated by Trump's 2016 candidacy (Abrajano and Hajnal 2015; Hajnal and Rivera 2014).

The absorption tendency follows naturally: as an issue sorts into the partisan bundle, its independent predictive power should decline because its variance is increasingly captured by party identification. Lenz (2012) provides the strongest version, arguing that voters generally follow their party's lead on issues. Even so, Lenz and others allow that independent issue effects can emerge under certain conditions—the question is what those conditions are. Our contribution is not to argue that absorption is wrong, but to specify conditions under which the pathway through which a sorted issue influencing vote choice may shift, even as sorting itself continues.

Issue Salience, Agenda-Setting, and the Activation Mechanism

A parallel literature on issue salience suggests relevant conditions. Krosnick (1988) shows issues matter more for voters who consider them personally important. Iyengar and Kinder (1987) show that media and discourse prime particular considerations. RePass (1971) argues

which issues voters use to evaluate candidates varies by electoral context.

These insights suggest that sorting determines which direction voters lean on an issue, but salience determines whether that issue functions as an independent evaluative criterion or merely as a background component of partisan identity. However, we draw an important distinction between salience and activation. An issue can be highly salient; that is, prominent in voters' minds, dominating media coverage, and yet remain fully absorbed into partisan identity. Immigration was extremely salient in 2016, yet our models find it was not an independent predictor of vote choice that year. Salience appears to be a necessary but not sufficient condition for activation. What salience alone may not provide is the independent evaluative information that allows voters to assess an issue on its own terms. Campaign rhetoric can make an issue salient; we hypothesize that sustained governance, with concrete, observable policy outcomes, is what provides the experiential basis for independent evaluation.

Governance as Enlightenment: Extending Gelman and King

Gelman and King (1993) argue that campaigns serve an enlightenment function: they provide information that helps voters align choices with fundamental predispositions. We extend this logic across election cycles. If campaigns enlighten voters within an election, governance may enlighten voters across elections. When a president governs heavily on a particular issue (implementing concrete policies with observable consequences), voters accumulate experiential information that may allow them to develop evaluative criteria beyond their general partisan orientation.

This is the proposed activation mechanism. Sustained governance may shift an issue from a signal of partisan identity to a basis for independent performance evaluation. The issue does not become less sorted. Rather, it may remain highly correlated with partisanship, but it may gain an independent pathway to vote choice alongside the partisan channel. Importantly, we hypothesize that activation changes the pathway of influence, not necessarily the overall

magnitude of the effect.

We draw on the policy feedback literature to specify why governance that reinforces partisan alignment might nonetheless produce independent issue effects. Pierson (1993) argues that implemented policies reshape the political landscape by generating new information and new stakes. Mettler and Soss (2004) show that policy implementation gives citizens direct experiential knowledge that is qualitatively different from elite cues. Campbell (2012) demonstrates that policies create constituencies who evaluate politicians on policy-specific grounds. Applied to immigration, this suggests that while Trump’s campaign provided a uniform partisan signal (restrictionism as Republican identity), his governance produced heterogeneous experiential information. Concrete policies like family separation, the travel ban, and administrative enforcement affected voters differently depending on their geographic proximity, occupational exposure, community composition, and moral priors. This within-party variation in experiential exposure may be what generates independent evaluative capacity: voters develop immigration-specific assessments that do not map perfectly onto their general partisan orientation. The key distinction is between symbolic information (which reinforces partisan signals) and experiential information (which can create evaluative dimensions partly orthogonal to partisanship). Campaign rhetoric provides the former; governance provides both simultaneously, which is why activation may produce independent effects even as it deepens partisan alignment.

Valence in Context

A final strand concerns candidate valence (Stokes 1963; Stone and Simas 2010; Clarke et al. 2009). Rather than treating valence as fixed, we suggest its relative importance varies by informational environment. In elections with novel candidates (2016), character assessments may loom large. As governance accumulates, the environment may shift toward policy-performance information, reducing the relative weight of character heuristics.

The Case of Immigration, 2012–2020

Immigration provides a useful case because of dramatic contextual shifts. In 2012, immigration was present in political discourse but not a defining presidential issue. In 2016, Trump elevated immigration with unprecedented rhetorical intensity, although these were campaign promises, not governing realities. Between 2017 and 2020, immigration became governing policy: Executive Order 13769, “zero tolerance” family separation, border wall construction, refugee admission reductions, and multiple administrative actions (Sides, Tesler, and Vavreck 2018). This transition from rhetoric to governance provides variation in the key theorized mechanism.

Hypotheses

From this framework, we therefore derive the following expectations:

Hypothesis 1: Immigration attitudes will exert a significant direct effect on presidential vote choice in 2020, independent of partisan identification, whereas in 2012 and 2016 immigration’s influence will operate primarily through its association with partisanship. The expected difference across elections is in the pathway of influence, not necessarily the overall magnitude.

Hypothesis 2: Candidate valence attributes, particularly honesty evaluations, will have a weaker effect on vote choice in 2020 than in 2016, consistent with a shift from character-based to policy-performance-based evaluation.

Hypothesis 3: In mediation analysis, a larger proportion of immigration’s total effect on vote choice will be retained as a direct effect (not mediated through PID) in 2020 compared to 2016, consistent with immigration shifting from an absorbed component of partisanship toward an autonomous predictor.

Data and Methods

Our analysis relies on the American National Election Studies (ANES) 2012, 2016, and 2020 Time Series surveys. The three-election design allows us to establish a pre-Trump baseline (2012), evaluate the campaign-without-governance condition (2016), and test the post-governance hypothesis (2020).

After listwise deletion, our analytic samples are approximately 3,708 (2012), 1,904 (2016), and 4,746 (2020) respondents. Full variable names, ANES codes, and coding decisions are in Appendix Table A1.

Dependent Variable. Presidential vote choice from the post-election wave in all three years, coded 1 = Democratic candidate, 0 = Republican candidate. Post-election reported vote choice ensures temporal consistency and is standard practice (Ansolabehere and Hersh 2012).

Independent Variables. For 2016 and 2020, we build models incrementally across four specifications (M1-M4). For 2012, we present the fully specified model to establish the baseline.

Model M1 (Deep Predispositions): Gender, income, race, education, racial attitudes, PID (7-point), ideology.

Model M2 (Performance Evaluations): Adds economic evaluations and incumbent job approval.

Model M3 (Candidate Valence): Adds relative competence and relative honesty.

Model M4 (Issue Activation): Adds immigration attitudes (five-point scale) and authoritarianism (child-rearing values).

Estimation Strategy. We estimate year-specific logistic regression models and a three-year pooled model with year $\times$ immigration interactions to formally test cross-election differences. For mediation, we employ Imai, Keele, and Tingley (2010), decomposing immigration's total effect into direct and indirect paths through PID. We compute PID-immigration correlations by year with Fisher z -tests.

Post-election measurement and post-treatment bias. Immigration attitudes and authoritarianism are measured in the post-election wave, raising concerns about post-treatment bias: respondents may rationalize issue positions after voting. Several considerations mitigate this concern, though they cannot eliminate it. First, both measures are widely conceptualized as stable predispositions rather than campaign-induced attitudes (Feldman and Stenner 1997; Hainmueller and Hopkins 2014). Second, the 2020 model includes a pre-election incumbent immigration performance evaluation as a robustness check; it is independently significant ($p = .002$), suggesting the immigration-vote link is not purely a post-hoc rationalization artifact. Third, authoritarianism which is also post-election and conceptualized as stable, behaves differently across years (significant in 2016, null in 2020), a pattern difficult to reconcile with uniform rationalization since Trump is the Republican candidate in both years. Nonetheless, we interpret our results as descriptive evidence about changing associations rather than definitive causal claims. The ideal test would use panel data measuring immigration attitudes before and after the election (Lenz 2012).

Mediation assumptions and limitations. The causal mediation framework requires sequential ignorability; that is there are no unobserved confounders of the mediator-outcome relationship after conditioning on treatment and covariates (Imai, Keele, and Tingley 2010). This assumption is unlikely to be fully satisfied with observational data. Following Bullock, Green, and Ha (2010), we present mediation results as one component of a triangulated argument, alongside year-specific regressions, sorting analysis, and Wald tests, rather than as a standalone causal test. The mediation findings should be interpreted as suggestive decompositions that illuminate the structure of the immigration-vote relationship, not as definitive causal estimates.

**Table I: Logistic Regression Models of Vote Choice, 2016 and 2020
(Unweighted Estimates)**

Predictor	M1		M2		M3		M4	
	2016	2020	2016	2020	2016	2020	2016	2020
<i>Deep Predispositions:</i>								
Gender	0.003 (0.086)	−0.098 (0.067)	−0.017 (0.114)	−0.005 (0.092)	−0.019 (0.125)	0.010 (0.115)	−0.051 (0.133)	−0.016 (0.120)
Income	0.032** (0.010)	0.004 (0.009)	0.028* (0.013)	−0.009 (0.092)	0.021 (0.015)	−0.014 (0.013)	0.020 (0.015)	−0.021 (0.014)
Race	0.232*** (0.063)	−0.030 (0.040)	0.160* (0.078)	0.014 (0.057)	0.106 (0.099)	−0.011 (0.063)	0.121 (0.099)	0.018 (0.065)
Education	0.003 (0.012)	0.185*** (0.046)	0.013 (0.015)	0.102 (0.066)	0.020 (0.021)	0.060 (0.071)	0.015 (0.025)	0.043 (0.075)
Racial attitudes	−2.337*** (0.346)	−3.023*** (0.225)	−1.082** (0.418)	−1.883*** (0.325)	−0.897† (0.522)	−1.617*** (0.338)	−0.938† (0.533)	−1.121** (0.357)
PID	−0.913*** (0.059)	−0.909*** (0.041)	−0.656*** (0.070)	−0.387*** (0.056)	−0.545*** (0.088)	−0.295*** (0.062)	−0.564*** (0.089)	−0.272*** (0.065)
Ideology	−4.159*** (0.527)	−4.266*** (0.352)	−2.749*** (0.650)	−2.772*** (0.536)	−2.490** (0.849)	−2.634*** (0.571)	−2.122* (0.867)	−2.558*** (0.593)
<i>Performance:</i>								
Economic eval.			1.599*** (0.391)	−2.074*** (0.295)	0.600 (0.520)	−1.409*** (0.314)	0.635 (0.530)	−1.281*** (0.326)
Incumb. approval			2.816*** (0.409)	−4.454*** (0.289)	1.349** (0.518)	−2.775*** (0.330)	1.233* (0.531)	−2.444*** (0.353)
<i>Valence:</i>								
Rel. Competence					−0.432*** (0.074)	−0.449*** (0.072)	−0.423*** (0.077)	−0.411*** (0.073)
Rel. Honesty					−0.971*** (0.106)	−0.336*** (0.055)	−0.971*** (0.109)	−0.325*** (0.058)
<i>Issue Position:</i>								
Immigration							−0.056 (0.093)	−0.267*** (0.074)
<i>Post-wave:</i>								
Authoritarianism							−0.399* (0.159)	−0.004 (0.104)
N	1,932	4,792	1,905	4,771	1,905	4,771	1,904	4,746
AIC	878	1928.5	640.8	1019.7	441.8	897.9	438.1	876.2

Note: The dependent variable is presidential vote choice in the 2016 and 2020 elections. Age is not an included demographic because of restricted data limitations. Logit coefficients; standard errors in parentheses. * $p < .05$, ** $p < .01$, *** $p < .001$, † $p < .10$.

Table II: Cross-Election Comparison: Fully Specified Models (M4), 2012, 2016, and 2020

Predictor	2012	2016	2020
PID	−0.652 ^{***} (0.052)	−0.564 ^{***} (0.089)	−0.272 ^{***} (0.065)
Ideology ^a	−0.321 ^{***} (0.051)	−2.122 [*] (0.867)	−2.558 ^{***} (0.593)
Racial attitudes	−0.226 (0.227)	−0.938 [†] (0.533)	−1.121 ^{**} (0.357)
Econ. evaluation	1.381 ^{***} (0.231)	0.635 (0.530)	−1.281 ^{***} (0.326)
Incumb. approval	2.331 ^{***} (0.228)	1.233 [*] (0.531)	−2.444 ^{***} (0.353)
Rel. Competence	−0.176 ^{**} (0.055)	−0.423 ^{***} (0.077)	−0.411 ^{***} (0.073)
Rel. Honesty	−0.285 ^{***} (0.040)	−0.971 ^{***} (0.109)	−0.325 ^{***} (0.058)
Immigration	−0.074 (0.055)	−0.056 (0.093)	−0.267 ^{***} (0.074)
N	3,708	1,904	4,746
AIC	1010.5	438.1	876.2

Note: Select predictors shown; demographics and authoritarianism included in all models. The dependent variable is presidential vote choice in the 2012, 2016 and 2020 elections. Ideology is on the original scale in 2012 and rescaled 0–1 in 2016/2020; cross-year coefficient comparison for ideology is not appropriate. The immigration coefficient, which is the key variable of interest, is consistently coded across all three years. * $p < .05$, ** $p < .01$, *** $p < .001$, [†] $p < .10$.

Results

The Three-Election Pattern (H1)

Table II presents the central cross-election comparison. Immigration attitudes are non-significant in 2012 and 2016, but significant in 2020. The primary comparison, between the 2012 pre-Trump baseline and the 2020 post-governance election, shows a clear shift: immigration moved from a non-significant to a significant independent predictor in fully specified models. The intermediate result (2016) is consistent with the activation interpretation, as immigration was not independently significant despite being a dominant campaign issue, but we note below that the 2016-2020 contrast is not formally significant and is open to alternative explanations.

Candidate Valence (H2)

Consistent with Hypothesis 2, relative honesty was among the strongest predictors in 2016 (-0.971 , $p < .001$) but declined to -0.325 ($p < .001$) in 2020 – roughly one-third its 2016 magnitude. In 2012, honesty’s coefficient was -0.285 , suggesting the 2016 value was anomalously large rather than 2020 representing a decline from a stable baseline. This is consistent with the 2016 election’s unusual emphasis on candidate character. Relative competence was stable across 2016 and 2020 (-0.432 and -0.449), suggesting that competence evaluations may be less context-dependent than honesty judgments.

Collinearity and the 2016 Null

The non-significance of immigration in 2016 warrants careful interpretation because immigration attitudes are correlated with PID, ideology, and racial attitudes, and multicollinearity can suppress standard errors differently across samples. When immigration enters the 2016 model, ideology’s coefficient drops from -2.490 to -2.122 , suggesting shared variance. An alternative interpretation of the 2016 null is that collinearity prevented the model from

separating immigration's contribution from correlated predictors, rather than immigration having no independent influence.

Several considerations bear on this interpretation. First, sorting was stronger in 2020 ($r = .412$) than in 2016 ($r = .335$), meaning collinearity between immigration and PID was at least as severe in 2020, yet immigration was significant in 2020. If collinearity alone explained the 2016 null, we would expect greater suppression in 2020, not less. Second, the mediation analysis, which does not depend solely on the regression coefficient, provides convergent evidence of a pathway shift. However, we must also acknowledge that the 2020 sample is substantially larger ($N = 4,746$ vs. $1,904$), providing considerably more statistical power. We cannot rule out that the 2016-2020 pattern partly or wholly reflects this power differential rather than a genuine change in the structure of the immigration-vote relationship. If the true underlying effect were similar in both years, the larger 2020 sample would be more likely to detect it. The most cautious reading is that immigration's independent contribution to vote choice was either absent or undetectable in the 2016 specification, and clearly present in 2020, but that this contrast alone cannot establish that the underlying relationship changed.

Formal Cross-Year Tests

In a three-year pooled model with year $\times$ immigration interactions (2012 as baseline), the 2020 interaction is highly significant (Wald $\chi^2 = 27.47$, $p < .001$), confirming that immigration's conditional association with vote choice was significantly stronger in 2020 than in the pre-Trump baseline. This is the primary formal test in our analysis. The 2016 interaction is also significant relative to 2012 (Wald $\chi^2 = 6.75$, $p = .009$), indicating that immigration's raw association increased by 2016, though the year-specific H4 models show this elevated association was fully captured by partisanship, ideology, and racial attitudes.

Critically, the 2020-2016 contrast is not statistically significant (Wald $p = .361$). This means we cannot formally confirm that immigration's relationship with vote choice differed between the campaign election and the post-governance election. The non-significant

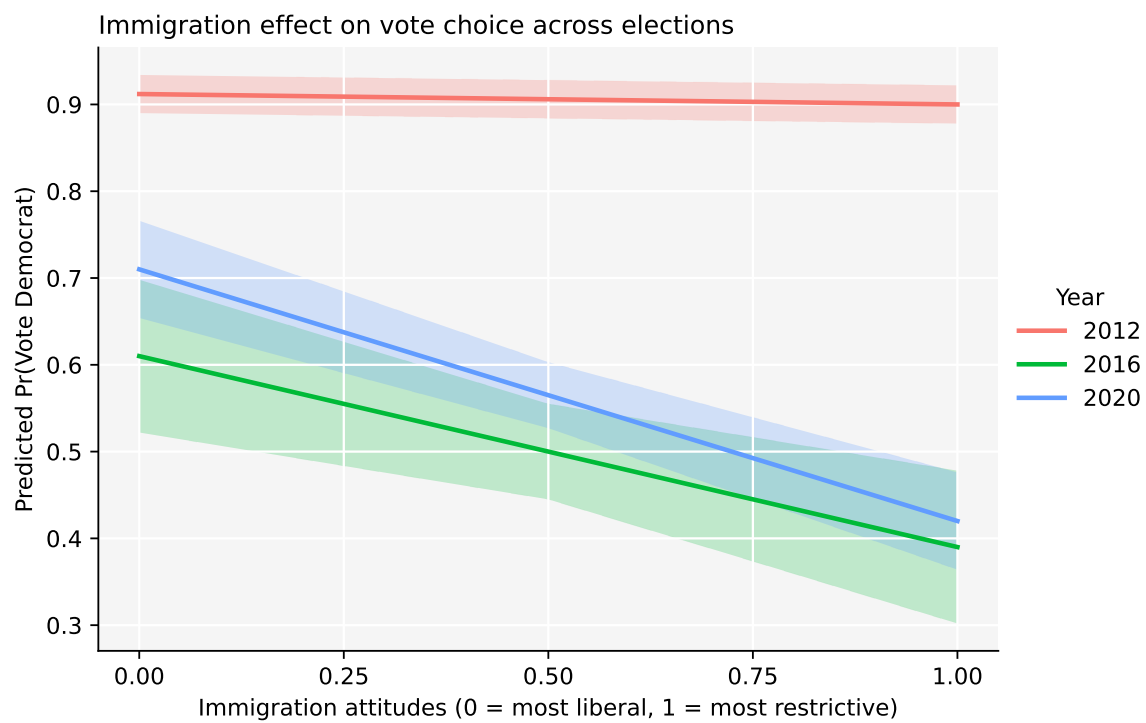
contrast is consistent with multiple interpretations: the underlying relationship may have genuinely changed but the test lacks power to detect it; the 2016 and 2020 effects may be more similar than our year-specific models suggest, with the different significance levels reflecting sample size differences; or the change may have occurred primarily between 2012 and 2016, with governance reinforcing rather than initiating it. We therefore frame the 2012-2020 comparison as our primary evidence and treat the 2016 results as supplementary, consistent with the activation account but not sufficient on their own to establish that governance specifically drove the change. As Gelman and Stern (2006) caution, the difference between “significant” and “not significant” is not itself statistically significant.

Figure I presents the predicted probabilities across all three elections. In 2012, immigration attitudes have virtually no relationship with vote choice: the line is nearly flat. By 2016, a moderate negative slope appeared, though with wide confidence intervals reflecting the collinearity noted above. In 2020, the slope is steepest and the confidence intervals tightest, consistent with immigration having developed a more distinct predictive profile.

Partisan Sorting

The PID-immigration correlation increased from $r = .057$ ($N = 5,914$) in 2012 to $r = .335$ ($N = 3,621$) in 2016 and $r = .412$ ($N = 7,350$) in 2020. The large jump occurred between 2012 and 2016, with continued increase through 2020. Sorting was already well underway by 2016, yet immigration was not an independent predictor until 2020 in our specifications. The coexistence of intensifying sorting and a newly significant independent effect is the empirical pattern most consistent with the activation account: an issue can become more correlated with partisanship at the bivariate level while simultaneously appearing to develop a more direct pathway to vote choice in multivariate analysis.

Figure I: Immigration Effect on Vote Choice Across Three Elections



Note: Predicted probabilities from year-specific M_4 models with covariates at medians. Immigration coded 0 (most liberal) to 1 (most restrictive). Shaded regions = 95% CIs.

Mediation Analysis (H3)

We employ the causal mediation framework of Imai, Keele, and Tingley (2010) to examine whether the structure of immigration’s relationship with vote choice differed across elections. Given the assumptions noted above, these results should be interpreted as suggestive decompositions, not definitive causal estimates.

Table III: Causal Mediation Analysis: Immigration $\rightarrow$ PID $\rightarrow$ Vote Choice (Full Specification)

	2016 (N = 1,884)			2020 (N = 4,752)		
	Estimate	[95% CI]	<i>p</i>	Estimate	[95% CI]	<i>p</i>
ACME (indirect)	0.001	[−0.000, 0.002]	.054 [†]	0.000	[−0.000, 0.000]	.998
ADE (direct)	−0.004	[−0.011, 0.003]	.244	−0.006	[−0.010, −0.003]	.002 ^{**}
Total Effect	−0.003	[−0.010, 0.003]	.366	−0.006	[−0.010, −0.003]	<.001 ^{***}
Prop. Mediated	~0.16	—		~0.00	—	

Note: 1,000 quasi-Bayesian simulations. Full controls included. **p* < .05, ***p* < .01, ****p* < .001, [†]*p* < .10.

In 2016, immigration’s total effect on vote choice was not statistically significant. The ACME (indirect path through PID) was marginally significant, while the ADE (direct path) was not. The direction of these estimates suggests that whatever influence immigration exerted in 2016 may have operated primarily through partisan identity. However, because the total effect is non-significant, the 2016 proportion-mediated estimate (~16%) is unstable and should be treated as directionally suggestive rather than a precise quantity. We draw no strong conclusions from the 2016 decomposition alone.

The 2020 results are cleaner. Immigration’s total effect is significant, and the ADE is significant, while the ACME is essentially zero. The proportion mediated dropped to approximately 0.0%. In a supplementary analysis with a simpler specification (demographics and PID only; Appendix Table A2), both total effects are significant, and the proportion mediated declines from approximately 11.2% in 2016 to 5.5% in 2020. The simpler specification’s significant total effects make the proportion-mediated comparison more interpretable. Together, both specifications are consistent with the same pattern: immigration’s pathway

to vote choice appears to have become more direct between elections.

Taken together with the regression models and sorting evidence, the mediation results are consistent with a pathway shift. We interpret this pattern through the lens of issue activation: the possibility that sustained governance provided voters with independent evaluative information, while acknowledging that this is one plausible interpretation of a cross-sectional pattern, not a confirmed causal mechanism. The mediation rests on assumptions we cannot verify, the 2016–2020 coefficient difference is not significant, and increased statistical power in 2020 remains a viable alternative explanation for the different significance levels. The strongest evidence is the 2012–2020 contrast, where both the Wald test and the mediation point in the same direction.

Discussion and Conclusions

Summary of Findings

Our analysis finds evidence consistent with the proposition that partisan sorting does not permanently fix the pathway through which an issue influences vote choice. Immigration attitudes, which were not significant independent predictors in 2012 or 2016, became significant by 2020 in fully specified models. The primary comparison, which is the 2012 pre-Trump baseline versus the 2020 post-governance election, is statistically robust (Wald $p < .001$). The intermediate 2016 results are consistent with the activation interpretation but are not definitive: the 2016–2020 coefficient difference is not statistically significant, and we cannot rule out that differences in sample size, statistical power, or model-specific collinearity patterns partly account for the different significance levels across these two elections. Mediation analysis provides suggestive evidence that the nature of the relationship differed, from a predominantly indirect pathway through PID in 2016 toward a more direct pathway in 2020, though this decomposition rests on untestable assumptions. These patterns emerged even as partisan sorting intensified. Simultaneously, candidate honesty evaluations declined

substantially, consistent with a shift toward policy-performance evaluation.

Theoretical Implications

These findings speak to the literatures on issue evolution, sorting, and the relationship between issue attitudes and vote choice. Based on the sorting literature, the logic of Carmines and Stimson (1989), Levendusky (2009), and Mason (2018) is well supported, and Lenz's (2012) evidence that voters often follow party cues is substantial. We further identify a pattern that is consistent with a specific scope condition under which independent issue effects may emerge: sustained governance that reinforces partisan alignment. The 2012-2020 evidence supports this; the 2016-2020 evidence is suggestive but not conclusive. The contribution is the scope condition and the pathway distinction (direct vs. indirect influence), not a claim that independent issue effects are novel or that sorting's logic is fundamentally wrong.

The extension of Gelman and King's (1993) enlightenment framework across election cycles is necessarily speculative with the data available to us. Their argument that information helps voters align choices with fundamentals operates within a single election. We suggest that governance may perform an analogous function across elections, providing the experiential information that allows voters to evaluate an issue independently of partisan cues. This remains a hypothesis; testing it adequately would require panel data and additional issue domains.

The decline in honesty's predictive power refines our understanding of valence. Rather than valence being fixed (Stokes 1963; Clarke et al. 2009), its relative importance appears context-dependent – higher when candidates are novel, declining as governance information accumulates.

Scope Conditions: The Trade Counterfactual

To examine whether these patterns are specific or not to immigration, we conduct a parallel analysis of trade attitudes (Appendix B). The results reveal a contrasting pattern: PID-trade sorting decreased from $r = .198$ to $r = .157$, and trade's association with vote choice did not significantly change (Wald $p = .431$). This contrast helps specify a scope condition. Both immigration and trade received sustained governance attention, but immigration governance reinforced partisan alignments while Trump's heterodox trade positions cross-pressured them. The activation pattern appears conditional on governance that deepens rather than disrupts the partisan structure of an issue.

The Trump-Specific Question

An alternative interpretation is that these findings reflect the particular circumstances of the Trump presidency. We acknowledge this. Our contribution is to identify a potential process and document it in one case with a specific scope condition. Whether the same process operates for other issues—the ACA under Obama, economic bailout evaluations, trade policy—is an empirical question requiring further research, ideally with panel data (Carsey and Layman 2006; Highton and Kam 2011; Engelhardt 2021).

Limitations

Several limitations warrant acknowledgment. First, despite adding 2012, our analysis relies on cross-sectional data from three elections. Panel data tracking within-individual change would provide stronger evidence. Second, the 2016-2020 coefficient difference is not statistically significant ($p = .361$), meaning we cannot formally establish that governance specifically changed the immigration-vote relationship between those elections. The 2016 results are consistent with the activation interpretation but are also consistent with sampling variability, insufficient power in the smaller 2016 sample, or collinearity-driven model instability.

Our primary evidence is the 2012-2020 contrast, where the Wald test is robust. Third, the 2016 mediation results should be interpreted with particular caution: because immigration's total effect was not significant in the full specification, the proportion-mediated estimate is unstable. We rely on the clean 2020 results and the corroborating simpler specification for substantive conclusions. Fourth, sequential ignorability cannot be verified with observational data; our mediation is one component of a triangulated argument (Bullock, Green, and Ha 2010). Fifth, post-election measurement of immigration attitudes raises post-treatment bias concerns that we can mitigate but not eliminate. Sixth, collinearity between immigration, PID, and ideology complicates interpretation; increased statistical power in 2020 ($N = 4,746$ vs. 1,904 in 2016) remains a viable alternative explanation for the different significance levels. Seventh, ideology scaling differences in 2012 complicate cross-year comparisons for variables other than immigration. Finally, we examine only presidential elections; testing activation in congressional races would strengthen generalizability.

Conclusion

The sorting literature has documented how issues align with partisan identity over time. Our findings are consistent with the possibility that this alignment does not permanently fix the pathway through which issues connect to vote choice. Between the 2012 pre-Trump baseline and the 2020 post-governance election, immigration developed significant independent predictive power even as sorting intensified; a pattern we interpret through the lens of issue activation. Whether governance specifically drove this shift, as we hypothesize, cannot be conclusively established with our design: the 2016-2020 contrast is not statistically significant, and alternative explanations including power differences and collinearity remain viable. If the activation account is correct, it would suggest that partisan sorting constrains but does not permanently determine how issues shape electoral choice, and that sustained governance on an issue, when it reinforces partisan alignments, may provide the experiential information voters need to evaluate that issue independently. Testing this interpretation

with panel data, additional issue domains, and additional electoral contexts is an important direction for future work.

Appendix

Table A1: Variable Definitions and ANES Coding

Variable	2012	2016	2020	Coding	Wave
Vote choice	postvote_presvtwho	V162034a	V202073	1=Dem, 0=Rep	Post
Party ID	pid_x	V161158x	V201231x	1 (Str Dem)–7 (Str Rep)	Pre
Ideology	libcpo_self	V161126	V201200	See note ^b	Pre
Racial att.	aidblack_self	V161198	V201258	Aid to minorities; 0-1	Pre
Education	dem_edugroup_x	V161270	V201511x	Continuous	Pre
Income	inc_incgroup_pre	V161361x	V202468x	Income bracket	Pre/Post
Gender	gender_respondent_x	V161342	V201600	1=Female, 0=Male	Pre
Race	dem_racecps_1st	V161310x	V201549x	1=White, 0=Non-white	Pre
Econ. eval.	presapp_econ	V161235x	V201132x	Pres. handling; 4-pt	Pre
Job approval	paprofile_presperfimmig	V161082x	V201129x	Pres. approval; 4-pt	Pre
Rel. Comp.	ctrait_dpcknow/ctrait_rpcknow	V161161/66	V201373-74	Dem–Rep (–4 to +4)	Pre
Rel. Honesty	ctrait_dpchonst/ctrait_rpchonst	V161162/67	V201377-78	Dem–Rep (–4 to +4)	Pre
Immigration att.	immigpo_level	V162157	V202232	1 (lib.)–5 (restrict.)	Post
Authorit.	auth_ind	V162239-42	V202266-69	Child-rearing; std.	Post

Note: Ideology is on the original 1–7 scale in 2012 and rescaled 0–1 in 2016/2020. DK/RF = missing.

Table A2: Causal Mediation: Simple Specification (Demographics + PID Only)

	2016 (N = 2,468)		2020 (N = 5,729)	
	Estimate	<i>p</i>	Estimate	<i>p</i>
ACME (avg.)	–0.003	<.001 ^{***}	–0.001	<.001 ^{***}
ADE (avg.)	–0.023	<.001 ^{***}	–0.010	<.001 ^{***}
Total Effect	–0.026	<.001 ^{***}	–0.010	<.001 ^{***}
Prop. Mediated	~11.2%	<.001 ^{***}	~5.5%	<.001 ^{***}

Note: Includes only demographics (education, income, gender, race) and PID. Both total effects are significant, making proportion-mediated comparison more interpretable.

Table A3: Three-Year Pooled Logistic Regression with Year $\times$ Immigration Interactions (2012 Baseline)

Predictor	Coefficient	SE	<i>p</i>
Intercept	5.302	0.347	<.001 ^{***}
Immigration	−0.110	0.056	.049 [*]
Year: 2016	−1.847	0.374	<.001 ^{***}
Year: 2020	−1.391	0.308	<.001 ^{***}
PID	−0.693	0.031	<.001 ^{***}
Ideology (0–1)	−0.435	0.053	<.001 ^{***}
Racial attitudes	−1.358	0.157	<.001 ^{***}
Education	0.023	0.013	.083 [†]
Income	−0.014	0.006	.016 [*]
Gender	−0.048	0.066	.468
Race	−0.008	0.008	.297
Pres. job approval	0.596	0.165	<.001 ^{***}
Economic evaluation	0.271	0.165	.102
Rel. Competence	−0.665	0.036	<.001 ^{***}
Rel. Honesty	−0.520	0.030	<.001 ^{***}
<i>Immigration $\times$ Year 2016</i>	−0.790	0.304	.009 ^{**}
<i>Immigration $\times$ Year 2020</i>	−1.121	0.214	<.001 ^{***}
N	10,384		
AIC	2,944		
Null deviance	14,189		
Residual deviance	2,910		

Note: Logit coefficients. 2012 is the reference year. Interaction terms (italicized) test whether immigration’s conditional association with vote choice differs from the 2012 baseline. ^{*}*p* < .05, ^{**}*p* < .01, ^{***}*p* < .001, [†]*p* < .10.

Wald Tests from Three-Year Pooled Model

Hypothesis	Wald χ^2	<i>p</i>
Immigration effect in 2016 $\neq$ 2012	6.754	.009 ^{**}
Immigration effect in 2020 $\neq$ 2012	27.466	<.001 ^{***}
Immigration effect in 2020 $\neq$ 2016	0.835	.361

Note: Wald χ^2 tests from the pooled model above. The 2020 vs. 2012 comparison is the primary formal test. The 2020 vs. 2016 comparison is not statistically significant.

Table A4: Partisan Sorting Indicators — PID–Immigration Correlation by Year

Year	N	r (PID, Immig.)	Change from prior	Fisher z (p)
2012	5,914	.057	—	—
2016	3,621	.335	+.278	—
2020	7,350	.412	+.077	—

Note: Pearson correlations between 7-point PID and immigration attitudes. Fisher z -test for the 2016 vs. 2020 difference: $z = 2.393$, $p = .017$. The largest increase occurred between 2012 and 2016 (Trump's campaign), with continued increase through 2020.

Appendix B: Trade Attitudes as a Counterfactual Test

To examine whether the patterns we observe are specific to immigration or reflect a general consequence of presidential governance attention, we replicate our analysis with trade attitudes. Trump’s trade governance was intensive: tariffs on Chinese goods, NAFTA renegotiation (USMCA), and trade conflicts with allies. If activation resulted from sustained governance on any issue, trade should show a similar pattern.

PID-trade sorting decreased from $r = .198$ (2016) to $r = .157$ (2020) – the opposite of immigration. In a pooled model with a year $\times$ trade interaction, the interaction is non-significant ($\beta = 0.438$, $p = .431$; Wald $\chi^2 = 0.619$, $p = .431$). Trade’s relationship to vote choice did not change.

This contrast is informative. Immigration governance reinforced existing partisan alignments; Trump’s trade positions cross-pressured them. The absence of activation for trade suggests that sustained governance attention alone is insufficient; the activation pattern appears conditional on governance that deepens rather than disrupts the partisan structure of an issue.

Figure A1: PID–Trade Sorting by Year

Sorting indicator: PID-Trade association by year

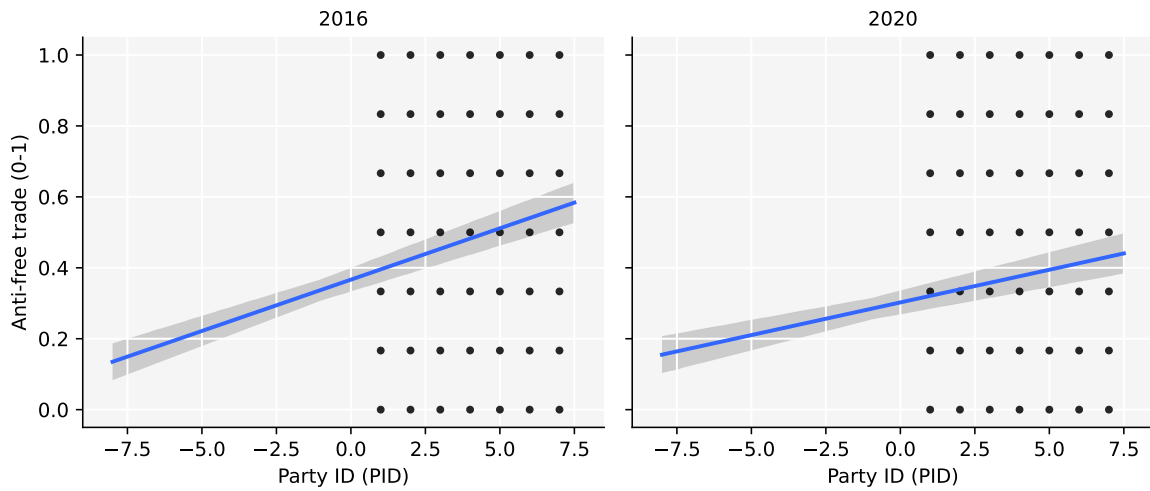
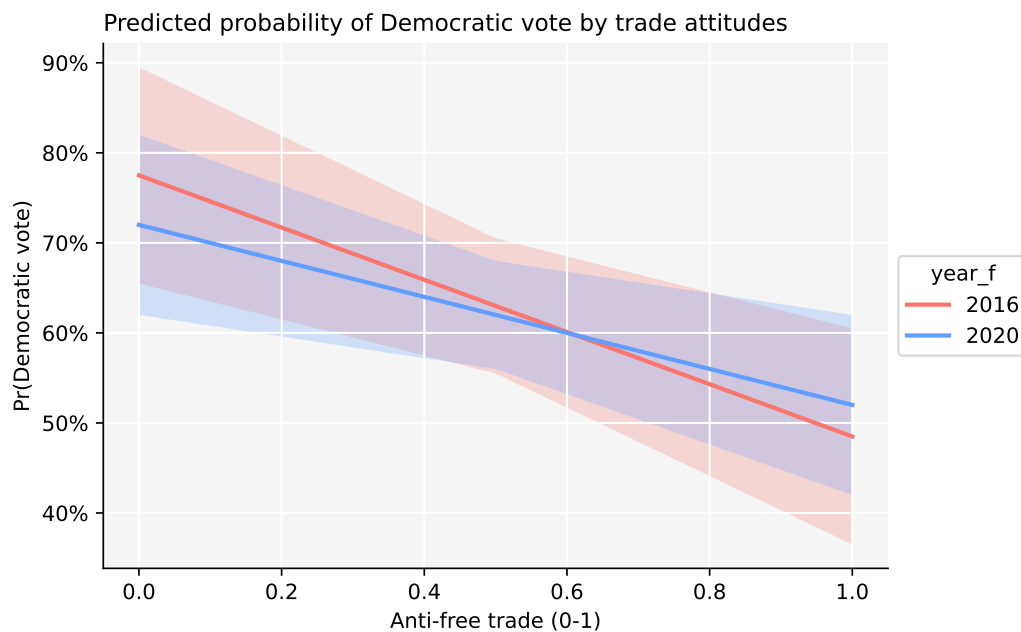


Figure A2: Predicted Probability of Democratic Vote by Trade Attitudes



Note: Trade model includes full controls. Wald test for year $\times$ trade interaction: $p = .431$.

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