

Pageants and the President: The Liability of Celebrity Politicians*

HENG CHEN

University of Hong Kong

YANG XU

University of Hong Kong

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Abstract. We study when celebrity status becomes a liability for politicians once a scandal breaks, using the leak of the Access Hollywood tape during the 2016 U.S. presidential campaign and Donald Trump's prior two-decade ownership of the Miss USA pageant. Exploiting cohort-by-county variation in voters' pre-scandal exposure to Trump-as-the-pageant-owner, we find that the scandal produced a sharper attitudinal and electoral shift among voters with greater prior exposure, with the more exposed counties experiencing an additional decline in Trump's 2016 vote share. Our findings suggest voters' prior exposure to a candidate shapes how they attend to and interpret later political information.

Keywords: celebrity politics; political scandal; voter behavior; gender norms; polarization.

JEL Classification. D72, D78, J16, L82, Z13.

*Heng CHEN: The Faculty of Business and Economics, The University of Hong Kong; Email: hengchen@hku.hk. Yang XU: The Faculty of Business and Economics, The University of Hong Kong; Email: xyethan@connect.hku.hk.

1. Introduction

Celebrity politicians have become a salient feature of modern democracy, where figures across countries and domains have repeatedly translated pre-political fame into electoral support (Wheeler, 2013). Arnold Schwarzenegger went from action-film stardom to the California governorship; Volodymyr Zelenskyy carried a comedy career into the Ukrainian presidency. Bill Bradley leveraged a Hall of Fame NBA career into three terms as a U.S. Senator; Imran Khan rose from cricket stardom to the prime ministership of Pakistan. The common feature across these trajectories is that each figure entered the political arena with substantial name recognition and a fully formed public persona built outside politics (West and Orman, 2003).

How pre-political fame translates into votes has been documented in a growing empirical literature. Xiong (2021) shows that voters with greater pre-political exposure to Ronald Reagan’s Hollywood career were more likely to support him at the polls; Durante et al. (2019) demonstrate that exposure to Silvio Berlusconi’s Mediaset entertainment programming shifted Italian viewers toward his party; Kim and Patterson Jr (2025) find that viewership of *The Apprentice* raised favorability for Donald Trump; and Garthwaite and Moore (2013) estimate that Oprah Winfrey’s endorsement of Barack Obama generated over one million additional primary votes.¹ Across this body of work, the finding is consistently positive: prior public exposure to a candidate’s persona tends to confer an electoral premium.

However, celebrity status is plausibly a double-edged sword: the same reach that builds voter support could amplify the cost of a scandal. Yet this downside has received little attention in the existing literature. One might in fact argue that celebrity status mostly works in the politician’s favor: audiences who come to know a politician through entertainment may form warm prior associations and be more willing to forgive personal missteps. The downside of celebrity politicians is therefore less obvious than it might seem, and calls for evidence. Our first contribution is to provide it.

Moreover, scandals vary in what they reveal about a politician. Most empirical work focuses on policy-relevant misconduct—corruption (Ferraz and Finan, 2008, 2011) and other forms of malfeasance—which voters can evaluate as a failure of office (Kendall et al., 2015; Arias et al., 2022). By contrast, scandals over personal character, particularly gender-norm violations, engage voters through judgments about who the

¹Xiong (2021) exploits geographic variation in pre-political exposure to Ronald Reagan’s Hollywood career and finds that voters in counties with greater exposure subsequently supported him at higher rates as a political candidate. Durante et al. (2019) use the staggered rollout of Berlusconi’s Mediaset entertainment network across Italian municipalities and find that exposure during formative years raised long-run vote shares for Berlusconi’s party. Kim and Patterson Jr (2025) draw on Nielsen viewership data and find that exposure to Donald Trump’s reality show *The Apprentice* raised favorable attitudes toward him.

politician is rather than how he governs. Whether voters care about these character-based scandals—and whether their response translates into electoral punishment—is far less well understood. Our second contribution is to address this question.

It is challenging to study when celebrity status becomes a liability under scandal: national scandals offer little cross-sectional variation across voters or locations. We overcome this challenge by exploiting an October surprise from the 2016 U.S. presidential campaign: the leak of the Access Hollywood tape on October 7, in which Donald Trump bragged about predatory behavior toward women. The leak triggered one of the largest media firestorms of the campaign. Within days, Trump released a rare public apology video, and his national polling fell sharply behind Clinton, with some surveys placing him as much as eight points behind by mid-October (NBC, 2016). Though Trump ultimately won the Electoral College, the tape was widely seen at the time as the moment most likely to end his candidacy, and the polling evidence confirms it was a substantial blow on the campaign trail.²

The content of the tape reached deep into Trump’s pageant past. The woman whose treatment Trump discussed, Nancy O’Dell, was herself a Miss USA pageant alumna who had worked directly with Trump during his ownership. Trump’s own words in the recording invoked his authority as pageant owner. The scandal thus tied his misconduct directly to the nationally televised Miss USA pageant, which Trump personally owned for nearly two decades (1996–2015). State titleholders, crowned in independently licensed state-level contests, all advance to Trump’s national final: when a woman from a given county appeared on that stage, residents of her home county were more likely to follow the pageant and encounter Trump-as-the-pageant-owner.

This institutional tie generates the variation we exploit. How *salient* the scandal is to a voter therefore varies, depending on her prior exposure to Trump-as-the-pageant-owner. In a news environment saturated with political stories, prior connection is what determines which ones cut through: voters with greater prior exposure carried a personally relevant connection to the institution at the center of the eventual scandal, attending more closely to its content than voters who could process the news at arm’s length. This variation in salience is consistent with recent evidence that the same news event captures more of one’s attention when she has prior personal exposure to its subject matter (Link et al., 2024).

To operationalize this idea, we restrict our sample throughout to counties that have ever produced a Miss USA state titleholder, mitigating the obvious selection concern that arises when comparing pageant-producing and non-pageant-producing

²Within days, Republican officeholders publicly condemned the tape in unusual numbers: by October 11, roughly a third of sitting Senate Republicans had said they would not vote for Trump, and House Speaker Paul Ryan announced he would no longer publicly defend Trump’s candidacy.

communities. Within this sample, whether a voter accumulated exposure depends on two factors: whether her county produced a Miss USA winner during Trump’s ownership, and whether she was old enough at the time to follow the pageant. We use the resulting cross-sectional variation across counties and birth cohorts to estimate how prior exposure to Trump-as-the-pageant-owner shaped voters’ response to the tape.

This paper proceeds in two steps. We first examine how the release of the Access Hollywood tape shifted political attitudes toward Donald Trump among voters with prior exposure to him through the Miss USA pageant, and we then investigate whether these attitudinal shifts translated into measurable electoral costs at the polls. Our empirical analysis combines novel data on beauty pageants, high-frequency survey responses, and electoral outcomes. We begin by assembling a comprehensive dataset of Miss USA state titleholders—the winners of each state’s preliminary contest who go on to compete in the nationally televised final round—from 1952 to 2015, when Trump sold the pageant in the early months of his presidential campaign. The data include detailed hometown information that allows us to map these winners to 708 unique U.S. counties of origin.

The first step of our investigation is to test whether the Access Hollywood tape shifted voters’ attitudes toward Trump, exploiting cohort-level differences in prior exposure to the Miss USA pageant. To carry out this test, we draw on the 2016 Cooperative Congressional Election Study (CCES), an individual-level survey whose rolling fielding design and rich content together make the test possible. The CCES was administered daily throughout the fall of 2016, so some respondents were interviewed before the Access Hollywood tape was released on October 7 and others afterward. The pre-election wave covers 64,600 respondents from 2,668 counties. The CCES records each respondent’s county of residence and birth year—allowing us to match respondents to the pageant data on location and birth cohort—along with vote intentions, realized vote choices, partisan affiliation, ideology, and demographics.

In our design, we restrict attention to counties that produced at least one state titleholder and, within this set, exploit two sources of variation. Across counties, the timing of when a winner emerges is plausibly quasi-random, driven by the idiosyncratic outcome of each year’s competition rather than by systematic county characteristics. Within a pageant-producing county, residents of different birth cohorts accumulate different amounts of exposure: an older cohort may have been old enough to follow several local winners crowned during Trump’s ownership of the pageant (1996–2015), while a younger cohort would have been too young when some of the same winners were crowned. The interaction of cross-county timing and within-county cohort produces variation in exposure intensity that is plausibly orthogonal to political preferences. Operationalizing this variation, we measure each respondent’s exposure as the

number of Miss USA winners from her county of residence crowned during years in which she was aged 10 or older—the World Health Organization’s threshold for the onset of adolescence and a plausible minimum age for following televised pageants.

This county-cohort-level exposure measure captures the salience of Trump-as-the-pageant-owner to voters in a given cell—that is, the likelihood that they encountered him in this role. Building on this measure, we implement a difference-in-differences (DID) design. Because the CCES was fielded daily throughout fall 2016, we can compare attitudinal shifts around the October 7 release of the tape across cohorts with differing prior exposure to Trump through the pageant. The DID coefficient identifies whether high-exposure cohorts moved against Trump more sharply than low-exposure cohorts after the leak.

Our DID estimates confirm this differential shift. Before October 7, exposure is positively associated with stated support for Trump—consistent with the electoral premium that pre-political exposure typically confers—but after October 7, this association is more than reversed: a one-unit increase in the exposure measure reduces the probability of voting for Trump by 2.9 percentage points. An event-study specification traces this response day by day around the tape’s release: there are no differential pre-trends in the days leading up to October 7, supporting the parallel-trends assumption, and the negative effect emerges immediately after the leak, peaks on the day following, and persists through Election Day, consistent with evidence that vivid narrative information lingers in memory longer than statistical information (Graeber et al., 2024).

A set of placebo tests confirms that the effect is specific to Trump and to the scandal rather than reflecting ideological reassessment, pre-existing cohort heterogeneity, or a general partisan shift. First, exposure has no effect on respondents’ assessment of Trump’s ideological position, indicating that the shift concerns how voters feel about Trump as a person rather than his policy stances. Second, exposure shows no relationship with respondents’ primary-election votes for Trump—cast before the tape’s release—mitigating concerns about pre-existing cohort differences. Third, exposure does not affect vote intentions for other Republican candidates running for Congress, the Senate, or governorships, confirming that the response is Trump-specific rather than a broader anti-Republican shift. Together, these placebos pin the channel on Trump’s character as revealed by the scandal rather than on ideology or partisan reassessment.

We address several potential concerns about our findings. First, a potential concern is that our cohort-based exposure measure may be correlated with cohort-specific dynamic effects that could confound the estimates. We address this by including interactions between birth-year fixed effects and survey-date fixed effects (or, alternatively, between a linear birth-year trend and survey-date fixed effects); our estimates remain

robust under both. Second, one might worry that some unobserved individual-level factor is correlated with both pageant exposure and political attitudes, generating a spurious effect. To address this, we conduct a permutation test that randomly reassigns the exposure measure across individuals and re-estimates the main specification 1,000 times, generating a null distribution of coefficients. The observed coefficient falls far in the tail of this null distribution, mitigating the concern that an unobserved individual-level factor drives the estimated effect. Third, the estimates are robust to the age threshold used to define exposure: cutoffs near 10 yield similar estimates in magnitude and significance. Finally, a leave-one-state-out exercise produces estimates that remain stable in magnitude and significance across all specifications, mitigating concerns that any single state drives the findings.

Our second step is to see whether the attitudinal shifts documented above translated into measurable electoral consequences for Trump. Paralleling the individual-level design, we move from individuals to counties and aggregate the individual exposure measure across each county's voting-eligible population, yielding a county-level exposure share—the fraction of voters in the county for whom the individual exposure measure was positive during Trump's ownership of the pageant. We continue to restrict the sample to counties that have ever produced a Miss USA winner, so that variation in the exposure share comes from the timing and intensity of Trump-era victories.

Using county-level election returns from 2012 to 2024, we find that counties with higher exposure shares experienced a significantly lower Republican vote share in the 2016 presidential election. The estimated magnitude implies that a county moving from zero to full exposure saw Trump's vote share decline by approximately 0.8 percentage points. This effect persists into the 2020 election at a smaller magnitude and fully dissipates by 2024.³ A placebo test using the 2012 Romney vote share—a pre-scandal outcome—shows no relationship with Miss USA exposure, mitigating the concern that pre-existing political trends across high- and low-exposure counties drive our results. A parallel test using county-level turnout as the outcome yields a null effect, indicating that the electoral cost we document reflects vote switching rather than disengagement, an intensive-margin response rather than an extensive-margin one.

Two further tests strengthen the causal interpretation. First, we use randomization inference to address the concern that some unobserved factor drives both the timing of winner production and political outcomes. We re-simulate the pageant history 1,000 times by randomly reassigning Miss USA victories within state-year cells, and re-estimate the main specification on each simulated history. The observed 2016 and

³This pattern—a persistent but attenuating electoral imprint—echoes findings in the collective memory literature (Rozenas and Zhukov, 2019; Belmonte and Rochlitz, 2019; Fouka and Voth, 2023; Ochsner and Roesel, 2024): political events leave latent associations whose electoral effects gradually fade as the originating shock recedes from recent memory.

2020 coefficients fall far in the tails of the resulting null distributions, while the 2012 and 2024 coefficients are not significant, indicating that the results are unlikely to be driven by unobserved determinants of timing.

Second, we introduce another pageant—Miss America, a similar national contest never owned by Trump—through two complementary tests. The first substitutes the comparison group: pooling Miss USA and Miss America counties (under both OLS and propensity-score-matching) yields no corresponding effect in Miss America counties. The second substitutes the treatment: replacing the Miss USA exposure share with an analogously constructed Miss America exposure share also yields no significant effect. Together, these tests confirm that the observed electoral consequences are specific to prior exposure to Trump’s persona rather than reflecting a more general feature of pageant-producing communities.

We also provide a set of robustness checks. First, the findings are robust to alternative constructions of the exposure measure, to excluding counties at the extremes of the population distribution, and to restricting the sample to counties with low population mobility. Second, individual-level voting data from the post-election CCES corroborate the county-level findings: individuals with greater Miss USA exposure were significantly less likely to have voted for Trump in 2016, with no corresponding relationship for the 2012 Romney vote.

These average effects, however, mask substantial heterogeneity across partisan attachment. At the individual level, the post-scandal shift concentrates among swing voters—a 3.9 percentage point decline per unit of exposure—with null effects among strong Democrats and strong Republicans. At the county level, a parallel pattern emerges: the cost concentrates in politically competitive (40–60 percent Republican) counties and is silent in red and blue strongholds.

Two takeaways follow. The first concerns the liability side of celebrity politics. Trump’s celebrity exposure through the Miss USA pageant translated into a real and measurable electoral liability once the Access Hollywood scandal broke and reframed his earlier public persona. Notably, the same exposure was positively associated with support for Trump before the scandal—the asset side of the same celebrity reach—but the tape more than reversed that pre-existing advantage. The same celebrity reach that builds a politician’s recognition is converted into a liability when scandal lands. More precisely, what we identify is a *scandal-amplification channel* of celebrity: conditional on a scandal that reactivates a specific component of a politician’s celebrity persona, prior exposure to that persona amplifies the electoral cost.⁴ Our findings speak to how pre-political celebrity shapes voters’ attention to and interpretation of later political

⁴We do not estimate the full net electoral return to celebrity status, nor directly compare celebrity politicians to non-celebrity counterparts.

information about a candidate, beyond the particular case of Trump and Miss USA.⁵

The second takeaway speaks to a question in the contemporary public discourse. A widely held pessimistic view—articulated in both popular and academic work—holds that in today’s polarized electorate, partisan identity has come to dominate voter evaluation so thoroughly that character-based scandals, including gender-norm violations, no longer move votes (Mason, 2018; Klein, 2020; Iyengar et al., 2019).

Our findings offer a more nuanced picture: scandal-driven accountability is real, but bounded along two dimensions. First, it is bounded by partisanship. Strong partisans on both sides remain insulated from character revelations, consistent with the broader literature on partisan moderation (Gentzkow and Shapiro, 2006; Druckman et al., 2013; Bonomi et al., 2021; Moreira, 2025); voters in the persuadable middle, in contrast, still punish powerful men for gender-norm violations even when those violations are entirely orthogonal to policy. Second, it is bounded by time. The electoral effect attenuates by 2020 and fully dissipates by 2024, suggesting that scandals can deliver short-term electoral consequences without producing lasting realignment.

2. Background

2.1. Miss USA

Miss USA is the institutional anchor of our identification strategy: it is the pageant Donald Trump owned for two decades, and the pipeline through which women from particular U.S. communities became nationally visible figures whose appearances on the televised final tied them to Trump as the pageant owner. This subsection describes the pageant’s structure, Trump’s ownership, and direct evidence that his ownership produced a localized, Trump-specific visibility shock in winners’ home counties.

The pageant was established in 1952, and its winner represents the United States at the Miss Universe pageant. It operates through a two-stage structure. First, each of the fifty states and the District of Columbia holds a preliminary competition, licensed to local directors, to crown a “Miss [State] USA”—the state titleholder. Each state-level contest evaluates candidates on interview performance, swimsuit presentation, and evening gown appearance, with judges scoring on poise, confidence, and personality. Each state’s titleholder then advances to the nationally televised final, where an independent celebrity judge panel selects semifinalists and, ultimately, the winner through additional rounds of swimsuit, evening gown, and an on-stage interview.

⁵While we estimate this effect for an entertainment celebrity, the underlying logic—pre-political fame as both an electoral asset and a scandal-amplification liability—should not be specific to entertainment. Any politician who enters office with name recognition built outside the political arena carries a stock of brand capital that magnifies both upside and downside.

Trump acquired the Miss Universe Organization—which includes the Miss USA and Miss Teen USA franchises—in 1996. He brokered a broadcasting deal with NBC in 2002, granting the network half-ownership and moving the televised finals to primetime. He maintained control of the organization until September 2015, when he divested his interest to WME/IMG after NBC and Univision severed ties with the pageant following his remarks about Mexican immigrants in the early months of his presidential campaign.

Across two decades of ownership, Trump used the nationally televised final as a recurring platform for personal-brand exposure: he appeared on stage at the final ceremony, and winners' acceptance remarks frequently credited him by name. Alyssa Campanella (Miss USA 2011), for example, stated in her acceptance remarks that the opportunity "could not have been possible if it weren't for Mr. Donald Trump"—the kind of public testimonial that, broadcast nationally and replayed locally in the winner's home community, anchored Trump's persona to the institution in a way that voters with a hometown connection were especially likely to encounter.

Trump's prolonged ownership and branding efforts forged *place-specific* associations between his public persona and the pageant, especially in the home counties of Miss USA winners crowned during his tenure. Each time a local woman won the Miss [State] USA title and advanced to the national stage, residents in her community were more likely to follow the pageant and, in doing so, encounter Trump's public persona—a localized celebrity exposure that accumulated over two decades.

We illustrate this localized visibility using Google Trends data. Because Google Trends data are available at the designated market area (DMA) level—where each DMA aggregates the counties sharing a common primary television market—we match each state titleholder's home county to its corresponding DMA and measure search volume in that area during her crowning year. Figure 1 reports the results for three search terms. A Miss USA victory is associated with elevated "Miss USA" search interest in the winner's DMA, confirming that the search data capture local attention to the pageant. The same victory is associated with a rise in "Trump"-related search activity, linking the pageant to Trump-specific local visibility. By contrast, we find no comparable pattern for "Clinton," ruling out a general rise in political attention. Together, these patterns support the premise that Trump's ownership of Miss USA created a unique, localized association between his persona and the pageant—an association that, as we show in subsequent sections, became politically consequential once the Access Hollywood tape was released.

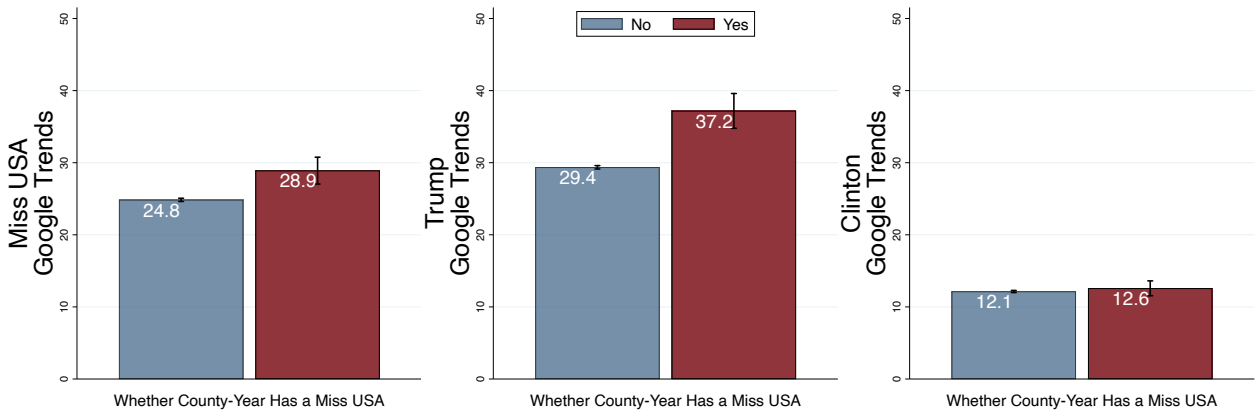


Figure 1. Google Trends for “Miss USA,” “Trump,” and “Clinton” in Miss USA State Titleholder’s Hometown

Notes: This figure plots the Google Trends search interest for the terms “Miss USA,” “Trump,” and “Clinton” in a panel dataset of all counties from 2004 to 2015. Whether County-Year Has a Miss USA indicates whether the county produced a Miss USA state titleholder in that year.

2.2. The Access Hollywood Tape Scandal

On October 7, 2016, approximately one month before the presidential election, The Washington Post published a leaked 2005 video recording (the “Access Hollywood” tape) in which Donald Trump made predatory and lewd remarks about women. The release triggered one of the most significant media firestorms of the campaign.

The discourse surrounding the tape directly linked Trump’s personal conduct to his professional history as the pageant’s proprietor. The woman discussed in the recording, Nancy O’Dell, was herself a Miss USA pageant alumna—Miss South Carolina USA 1987 and a co-host of both Miss USA and Miss Universe from 2004 to 2007 who had worked directly with Trump during his ownership. Crucially, Trump’s own words in the recording invoked his authority as pageant owner: in a separate 2005 interview with Howard Stern, he had explicitly tied his ownership to his access to contestants, stating “I’m allowed to go in [the dressing rooms] because I’m the owner of the pageant.” The tape thus reactivated a pre-existing association in voters’ minds between Trump and his pageant persona.

The tape redirected voter attention toward Trump’s historical role as the owner of Miss USA—a role he had frequently leveraged for public visibility. This is the *salience* channel: in a campaign-period news environment crowded with competing political stories, prior exposure to Trump-as-the-pageant-owner gave voters with a hometown connection a personally relevant entry point into the scandal, increasing the likelihood that they attended to its content rather than processing it at arm’s length. Consistent with this channel, public interest in the pageant spiked alongside interest in the scandal itself: Figure 2 shows that search interest for “Miss USA” surged in tandem with queries

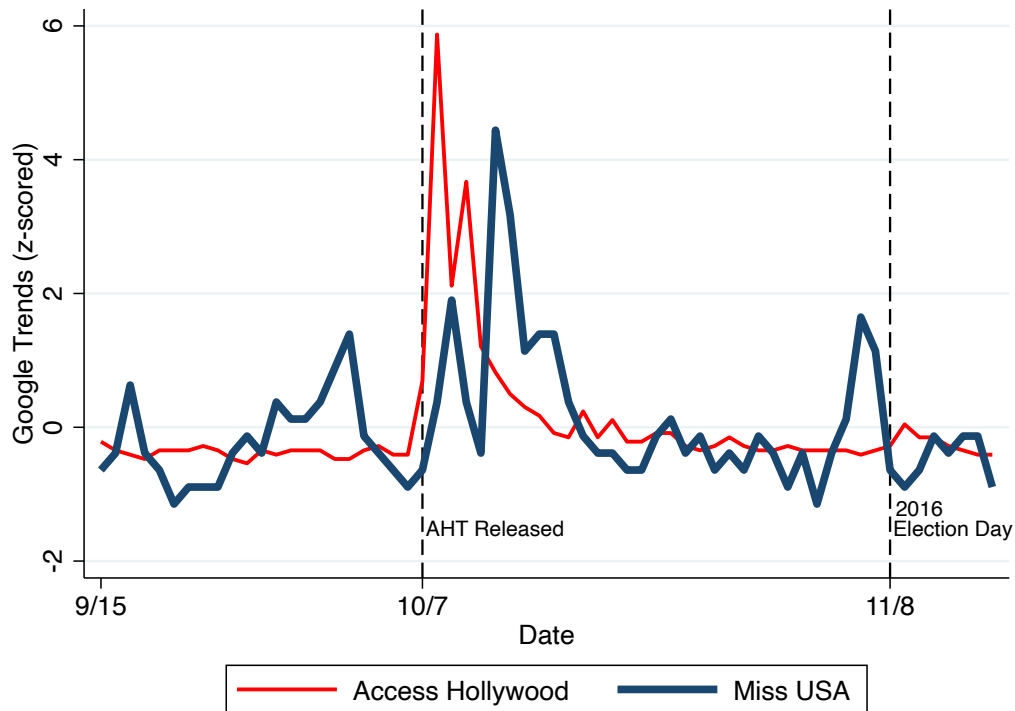


Figure 2. Google Trends for “Access Hollywood” and “Miss USA”

Notes: This figure plots the Google Trends search interest for the terms “Access Hollywood” and “Miss USA” from Sep 15 to Nov 15, 2016. The search interest is z-score normalized.

for “Access Hollywood” immediately following the leak, suggesting that the public connected the scandal to Trump’s pageant ownership.

2.3. The Tape as a Scandal

Did voters really treat this news as a scandal? Trump’s national polling fell behind Clinton in the days after the leak (Sides et al., 2018; Greenlee et al., 2020)—an unsurprising response for a story that dominated national news cycles. The more interesting question is whether Miss USA viewers themselves received the tape as a scandal at all. They may not have. Years of watching the pageant gave these viewers a certain *familiarity and fondness* for the institution and for Trump as its longtime owner—warm prior associations that could plausibly insulate them from a negative story involving the same figure.

We use YouTube comment data to provide some suggestive evidence. We collect user comments from the livestreams of the 2016 presidential debates on the NBC News YouTube channel using the YouTube API, and match these commenters to Miss USA viewers through user identifiers obtained from the official Miss USA YouTube channel. As a comparison group, we collect comments from the American Idol and The Voice

official YouTube channels and link debate commenters to these programs through the same user identifiers.

Before the tape’s release, Miss USA viewers’ debate comments exhibited no discernible difference in sentiment toward Trump relative to other commenters, and the same was true for the comparison programs. After the leak, the pattern diverged: Miss USA viewers expressed more negative sentiment toward Trump, while viewers of American Idol and The Voice did not show a comparable shift. Appendix Figure A1 reports the underlying coefficients. We do not interpret this exercise as a rigorous test; we read the patterns as consistent with the idea that the news was more likely treated as a scandal by Miss USA viewers.

3. Data

This section describes our three primary data sources—Miss USA state-titleholder data, the 2016 CCES, and county-level election returns—together with the sample restriction we maintain throughout the analysis.

3.1. Data and Sample Construction

Miss USA State-Titleholder Data. We assemble a comprehensive dataset of Miss USA state titleholders from 1952 to 2015, drawing primarily on publicly available records compiled in Wikipedia entries, which themselves draw on *Newspapers.com* and other archival sources. For each titleholder, we collect city-level hometown information and match her to a county of origin, yielding a dataset of 2,594 state titleholders from 708 unique hometown counties.⁶ Figure A2 displays the geographic distribution of Miss USA state titleholders across U.S. counties.

Cooperative Congressional Election Survey Data. To study changes in individual-level political attitudes and behavior, we use data from the 2016 Cooperative Congressional Election Study (CCES). The CCES follows a two-wave design: a pre-election wave fielded *daily* between September 28 and November 7, 2016, and a post-election wave completed by 52,899 of the 64,600 initial respondents after Election Day. The pre-election wave covers 64,600 respondents from 2,668 counties, of whom 16,990 were interviewed prior to the release of the Access Hollywood tape on October 7 and 47,610 thereafter. The CCES provides detailed information on voting behavior, political attitudes, and demographic characteristics, including respondents’ county of residence, birth year, interview date, gender, education, race, employment status, party registration, social media usage, and ideological self-placement.

⁶The shortfall relative to the upper bound ($51 \text{ jurisdictions} \times 64 \text{ years} \approx 3,260 \text{ entries}$) reflects incomplete early rosters: in the inaugural 1952 competition only 30 delegates competed, and the pageant grew organically through the 1950s and 1960s.

The pre-election wave elicits respondents' immediate presidential vote preferences. Among those who report an intention to vote and choose between Donald Trump and Hillary Clinton, 16,741 out of 40,573 express support for Trump. The survey also asks whether respondents voted for Trump in the primary elections, which we use as a placebo test.

The post-election wave, completed after Election Day, records respondents' actual general election votes. Because actual vote choices reflect the cumulative effect of the campaign—including the Access Hollywood tape—this wave allows us to translate the attitude shifts documented in the pre-election wave into estimates of the electoral cost of Trump's pageant exposure. In this post-election sample, 18,755 out of 52,899 respondents reported voting for Trump.

Appendix Table A1 reports summary statistics for the CCES individual-level sample. The sample includes 40,573 respondents, of whom 41.3% report a preference for voting for Donald Trump. The average birth year is 1966, and 48% of respondents are male. Educational attainment spans a wide range, with roughly half of the sample holding at least a four-year college degree or higher. The sample is predominantly White (72.9%), with Black and Hispanic respondents accounting for 13.1% and 7.4%, respectively. In terms of employment status, 45% of respondents are employed full-time, while 23.6% are retired. Politically, 47% of respondents are registered Democrats, 29.3% Republicans, and 21.8% report no party affiliation. On average, 23.4% report forwarding political content on social media, and the mean ideology score is 3.08 on a five-point scale.

County-Level Data. County-level presidential election returns for 2012–2024 are obtained from the MIT Election Data and Science Lab's *County Presidential Election Returns*. We augment these data with a rich set of socioeconomic and demographic controls, including population size, racial composition, gender and age distributions from the 2016 American Community Survey (ACS); payroll and employment statistics from the County Business Patterns; and income and poverty measures from the Small Area Income and Poverty Estimates (SAIPE).

Appendix Table A2 reports county-level summary statistics for the main analysis sample of 708 counties. The average Republican vote share is 55.5% in 2016, with comparable levels in prior presidential elections. Counties in the sample have an average poverty rate of 14.4% and a median household income of approximately \$55,000. On average, counties have a population of 0.30 million, with Black, Hispanic, and Asian population shares of 10.3%, 9.3%, and 2.7%, respectively.

3.2. Counties with Miss USA Winners

A natural question is whether counties that produced Miss USA state titleholders differ systematically from the rest of the country. Appendix Table A3 reports linear probability regressions in which the dependent variable is an indicator for whether a county produced a Miss USA state titleholder. Column (1) compares Miss USA counties to all other U.S. counties, including state fixed effects. Several county characteristics are significant predictors: Miss USA counties tend to have higher median household incomes, greater employment levels, a younger age profile, and larger Asian, Black, and Hispanic population shares. These patterns indicate that Miss USA counties are not a random draw from the national distribution; they differ from the broader population of U.S. counties in ways that could correlate with political outcomes.

This selection concern motivates our sample restriction. Comparing Miss USA counties to the full national sample would conflate the effect of pageant exposure with pre-existing differences between pageant and non-pageant counties. We therefore restrict the baseline sample throughout to the 708 counties that have ever produced a Miss USA state titleholder. Within this restricted set, all counties share the characteristic of pageant participation; identification comes from cross-cohort, within-county variation in prior exposure to Trump-as-the-pageant-owner—whether individuals in a given birth cohort were old enough to encounter Trump in this role when their county’s titleholder was crowned during 1996–2015. We report results using the full national sample only as a robustness check.

4. The Shift of Attitudes

This section examines how the Access Hollywood tape shifted political attitudes toward Trump among voters with prior exposure to him through Miss USA. We first describe the identification strategy, then report the main difference-in-differences estimates and a set of placebos, document heterogeneity by party identification, and present a battery of robustness checks.

4.1. Identification Strategy

Our identification strategy exploits variation in the timing and geography of Miss USA state titleholders during Trump’s ownership (1996–2015). We restrict attention to counties that have ever produced a Miss USA state titleholder, so that all counties in our sample share the common feature of being “pageant-producing” communities. Within this set of counties, state titleholders were crowned in different years, and residents of different birth cohorts were at different stages of life when a local titleholder appeared. We leverage this cross-cohort variation in prior exposure to Trump-as-the-pageant-

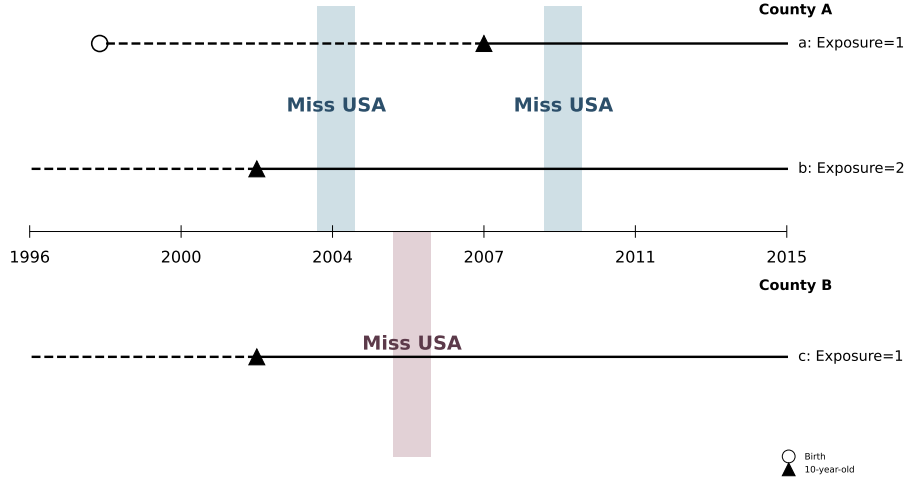


Figure 3. Research Design

Notes: Example of Individual-Level Exposure Design: A respondent born in 1990 in county A that produced Miss USA state titleholders in 2004 and 2009 would have an exposure measure of 2, as they were aged 14 and 19 during those years. In the same county, a respondent born in 1997 would have an exposure measure of 1, as they were aged 7 in 2004 (below the adolescence threshold) and 12 in 2009 (above the adolescence threshold). A respondent born in 1990 in county B that produced a Miss USA state titleholder in 2005 would have an exposure measure of 1.

owner to identify the causal effect of the Access Hollywood tape on political attitudes.

A central feature of our design is the definition of who counts as “exposed” to a local Miss USA state titleholder. Young children are unlikely to follow or be aware of beauty pageants; exposure to Trump-as-the-pageant-owner plausibly requires a minimum level of media awareness and cultural engagement that develops during adolescence. We therefore define an individual as exposed to a local state titleholder only if the individual was aged 10 or older at the time of the victory, following the World Health Organization’s definition of the onset of adolescence. We vary this age threshold in Section 4.5 to test robustness.

Using the CCES data, we construct an individual-level exposure measure by aggregating the total number of Miss USA state titleholders originating from a respondent’s hometown county during years in which the respondent was aged 10 or older. The key idea is illustrated in Figure 3. Specifically, the measure is defined as:

$$Exposure_{icb} = \sum_{t=1996}^{2015} \mathbb{1}(t - b \geq 10)_{ibt} \times Miss\ USA_{ct}, \quad (1)$$

where $Exposure_{icb}$ is the exposure to Trump-as-the-pageant-owner of individual i born in year b from county c , and $Miss\ USA_{ct}$ is an indicator for whether a Miss USA state titleholder originated from county c in year t . This measure captures cumulative

exposure over Trump’s entire ownership period, with variation driven by the interaction of a respondent’s county of residence and birth cohort.

Is this exposure measure balanced with respect to pre-existing political characteristics? If individuals with different levels of exposure within the same county already hold systematically different political preferences, any differential change in attitudes we later observe could reflect those pre-existing differences rather than a response to the scandal. We therefore conduct a balance test, regressing a set of pre-determined policy preferences—including approval of the Garland Supreme Court nomination, support for repealing the Affordable Care Act, and attitudes toward Mexican border control—on our exposure measure, conditioning on the full set of individual-level controls and fixed effects. Appendix Figure A3 plots the results: none of the coefficients are statistically significant, indicating that individuals with varying levels of exposure are similar along these politically relevant dimensions.

Because the CCES was fielded daily throughout the fall of 2016, a subset of respondents were interviewed before the release of the Access Hollywood tape on October 7 and the remainder after. This natural pre–post structure, combined with the cross-cohort variation in exposure intensity described above, motivates a difference-in-differences (DID) design. We compare changes in voting preferences before and after the scandal across individuals with varying levels of prior exposure to Trump-as-the-pageant-owner:

$$Y_{icb\tau} = \alpha + \beta \text{Exposure}_{icb} \times \text{Post}_{\tau} + \rho \text{Exposure}_{icb} + \phi_b + \theta_{c\tau} + \Gamma' X_i + \epsilon_{icb\tau}, \quad (2)$$

where $Y_{icb\tau}$ is the voting preference of individual i in county c and birth cohort b , surveyed at date τ . Exposure_{icb} is the individual-level exposure measure defined above, and Post_{τ} is an indicator equal to one if the respondent was surveyed after October 7, 2016. The specification includes birth cohort fixed effects ϕ_b , which absorb any level differences in political attitudes across generations, and county-by-survey-date fixed effects $\theta_{c\tau}$, which absorb all time-varying county-level shocks—including local economic conditions, campaign activity, and media markets—that might confound the relationship between exposure and attitudes. X_i is a vector of individual-level controls, including gender, education, race, employment status, party registration, social media usage, and ideological self-placement. Standard errors are clustered at the county level.

The coefficient of interest is β , which captures the differential change in attitudes toward Trump after the scandal for individuals with greater prior exposure to Trump-as-the-pageant-owner. Identification of β comes from within-county, cross-cohort variation in exposure interacted with the timing of the tape’s release. That is, we compare individuals living in the same county—and therefore subject to the same

local shocks—but who differ in how many Miss USA state titleholders they were old enough to have been exposed to during Trump’s ownership. A negative β would indicate that individuals with greater exposure shifted more strongly against Trump after the scandal, consistent with the hypothesis that the tape reactivated place-specific associations formed through the pageant.

Finally, we estimate an event-study specification to trace the dynamic evolution of the scandal’s effect and to test for pre-trends:

$$Y_{icb\tau} = \alpha + \sum_{k=-3}^{5+} \beta_k \text{Exposure}_{icb} \times D_{k\tau} + \rho \text{Exposure}_{icb} + \phi_b + \theta_{c\tau} + \Gamma' X_i + \epsilon_{icb\tau}, \quad (3)$$

where $D_{k\tau}$ is an indicator for being surveyed k days relative to the release of the Access Hollywood tape. The coefficients $\{\beta_k\}$ trace how the relationship between exposure and voting preferences evolves day by day around the scandal, providing both a test for parallel pre-trends and a characterization of the effect’s persistence.

4.2. Effects on Vote Intentions

Main estimates. Table 1 presents our estimates. Panel A restricts the sample to counties that have ever produced a Miss USA state titleholder—our baseline—and Panel B includes the full sample. Column (1) shows that a one-unit increase in the exposure measure leads to a 2.9 percentage point decline in the probability of voting for Trump after the scandal, significant at the 1% level, with a nearly identical estimate of 3.1 percentage points in Panel B, indicating the baseline finding is not an artifact of the county restriction.

Double-edged sword. The positive and statistically significant coefficient on Exposure in Column (1) of Panel A (0.019, $p < 0.05$) indicates that, prior to the scandal, individuals with greater prior exposure to Trump through the Miss USA pageant were more likely to report an intention to vote for him—consistent with a double-edged-sword reading in which exposure to Trump-as-the-pageant-owner cultivated favorable prior associations before the tape lifted his pageant persona into the news. This positive pre-scandal association mirrors a well-established finding in the literature: prior exposure to a candidate’s pre-political persona, whether built in entertainment (Xiong, 2021; Durante et al., 2019; Kim and Patterson Jr, 2025) or through celebrity endorsement (Garthwaite and Moore, 2013), tends to confer an electoral premium.

The larger-in-magnitude negative coefficient on Exposure $\times$ Post (-0.029 , $p < 0.01$) shows that this pre-existing advantage was more than reversed after the scandal, with the net post-scandal association turning negative overall. The same celebrity

Table 1. Access Hollywood Tape and the Shift of Attitudes

	Pref. Voting Trump	Asses. Trump Ideo.	Prim. Voted Trump
	(1)	(2)	(3)
Panel A: Counties with Miss USA			
<i>Exposure</i> × <i>Post</i>	-0.029*** (0.010)	-0.103 (0.066)	-0.004 (0.012)
<i>Exposure</i>	0.019** (0.009)	0.098 (0.061)	0.001 (0.011)
D.V. Mean	0.318	0.373	0.123
N	16613	16146	20606
R ²	0.709	0.279	0.394
Panel B: Full Sample			
<i>Exposure</i> × <i>Post</i>	-0.031*** (0.010)	-0.102 (0.066)	-0.005 (0.012)
<i>Exposure</i>	0.020** (0.009)	0.088 (0.059)	0.001 (0.011)
Birth Year FE	✓	✓	✓
County × Survey Date FE	✓	✓	✓
Controls	✓	✓	✓
D.V. Mean	0.339	0.396	0.130
N	20700	20206	26032
R ²	0.723	0.298	0.417

Notes: This table presents estimates of the effect of pageant exposure on voting preferences and attitudes. The dependent variable in column (1) is a dummy indicating the immediate presidential vote preference for Trump. The dependent variable in column (2) is a 7-point assessment of Trump's ideological position. The dependent variable in column (3) is a dummy indicating whether the respondent voted for Trump in the primary election, which serves as a placebo. Panel A restricts the sample to counties that have ever produced a Miss USA winner; Panel B includes the full sample. Controls include gender, education, race, employment status, party registration, social media usage, and ideological self-placement. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

exposure that benefited Trump became a source of electoral vulnerability once the scandal reframed how voters interpreted it.

Placebo tests. Column (2) examines whether the scandal shifted respondents' assessment of how conservative Trump is. If the attitudinal shift in column (1) were driven by a reassessment of Trump's ideological positions, we would expect higher-exposure individuals to also update their placement of Trump on the ideological spectrum. We find no significant effect in either panel. Taken together, the two results suggest that the scandal changed how people feel about Trump as a person rather than their views of his ideology or policy agenda.

Column (3) turns to the primary election outcome as a placebo. The CCES asks respondents whether they voted for Trump in the primary—a decision made before the Access Hollywood tape was released, regardless of when the respondent was surveyed. If the estimates in column (1) reflect a genuine post-scandal shift rather than pre-existing heterogeneity between high- and low-exposure individuals, exposure should have no differential effect on this pre-scandal outcome. Consistent with this prediction, neither panel yields a significant effect on primary voting.

Dynamic effects. To examine the dynamic effects of the scandal and the pre-trends, we reproduce the main specification using the event-study design. Figure 4 plots the estimates of β_k from Equation (3). There are no significant pre-trends before the scandal, supporting the parallel trends assumption. After the release of the Access Hollywood tape, individuals with higher exposure show a significant decline in their likelihood of voting for Trump, with the effect peaking on the first day after the scandal and remaining significantly negative through Election Day, suggesting a lasting impact.⁷

Down-ballot placebo. As a placebo, we examine whether the post-scandal shift is specific to Trump or extends to other Republican politicians on the ballot. If exposure to Trump-as-the-pageant-owner shapes attitudes toward Trump personally—through place-specific associations formed during his ownership—then the effect should not carry over to Republican congressional, gubernatorial, or senatorial candidates, who share no connection to the pageant. Appendix Table A4 shows that the estimated coefficient on Exposure $\times$ Post is statistically insignificant for all three Republican down-ballot races, confirming that the effect is confined to Trump and consistent with the place-specific association channel.

⁷One may conjecture that the timing of the leak was itself consequential for the 2016 outcome. Had the scandal broken substantially closer to Election Day, less of the post-peak attenuation visible in the event study would have transpired, and the documented effect at the ballot box would plausibly have been larger. Given Trump's narrow margins in pivotal swing states—Pennsylvania (0.7 percentage points), Michigan (0.2 percentage points), and Wisconsin (0.8 percentage points)—a fresher scandal could plausibly have flipped the Electoral College outcome.

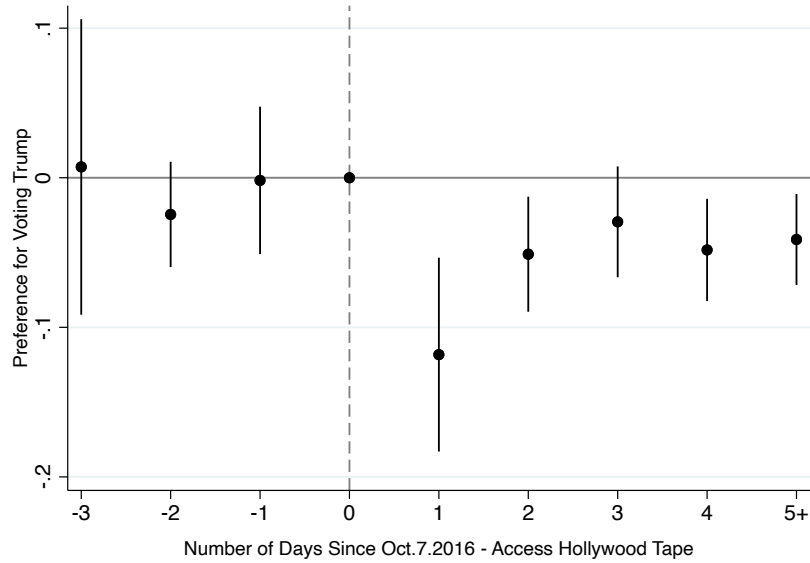


Figure 4. Event-Study Estimates of the Effect of the Scandal on Preference for Voting Trump

Notes: This figure plots the coefficients of regressing the preference for voting Trump on the interaction between the individual-level exposure measure and indicators for being surveyed at different time periods relative to the release of the Access Hollywood tape. The specification includes birth year fixed effects, county-by-survey-date fixed effects, and individual controls including gender, education, race, employment status, party registration, social media usage, and ideological self-placement. Robust standard errors are clustered at the county level.

4.3. Party Identification and Scandal

We explore whether the scandal's impact varies by party identification. If partisan identity shapes how voters process character information, the attitudinal response to the Access Hollywood tape should differ systematically across party groups. To investigate this, we partition the sample into three subgroups based on the distribution of the 7-point party identification scale. Specifically, we define Strong Democrats as those at the lowest end of the spectrum (point 1, representing the bottom 29% of the density), Strong Republicans as those at the upper tail (points 6–7, comprising the top 26%), and Swing Voters as the remaining mass in the middle of the distribution. This allows us to examine what role party identification plays in mediating how voters respond to character information about a candidate.

Table 2 reports the results. Column (1) shows that among strong Democrats the estimated coefficient is small and statistically insignificant. Column (2) shows that among swing voters, who comprise nearly half of the sample, a one-unit increase in exposure leads to a 3.9 percentage point decline in the probability of supporting Trump after the scandal, significant at the 5% level. Column (3) shows that among strong Republicans the coefficient is also statistically insignificant.

Table 2. Polarization and Scandal

	Preference for Voting Trump		
	Strong Democrats (1)	Swing Voters (2)	Strong Republicans (3)
<i>Exposure</i> × <i>Post</i>	-0.008 (0.006)	-0.039** (0.018)	-0.069 (0.061)
Birth Year FE	✓	✓	✓
County × Survey Date FE	✓	✓	✓
Controls	✓	✓	✓
D.V. Mean	0.012	0.289	0.906
N	5482	5496	2573
R ²	0.223	0.607	0.446

Notes: The table examines whether the effect of the Access Hollywood tape varies by party identification. Based on the distribution of the 7-point party identification scale, Strong Democrats are those at the lowest end of the spectrum (point 1, the bottom 29%), Strong Republicans are those at the upper tail (points 6–7, the top 26%), and Swing Voters are the remaining mass in the middle of the distribution. The estimation sample is smaller than the original due to the absorption of observations by county × time fixed effects. *Exposure* is the individual-level Miss USA pageant exposure measure defined in Equation (1), and *Post* is an indicator equal to one if the respondent was surveyed after October 7, 2016. Controls include gender, education, race, employment status, party registration, social media usage, and ideological self-placement. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The pattern suggests how partisan identity constrains information updating. Strong partisan commitments—whether Democratic or Republican—effectively foreclose meaningful response to character information, though through distinct channels. For strong Republicans, the mechanism is likely to be motivated reasoning: partisan identity acts as a cognitive prior that leads strong partisans to discount or reinterpret unfavorable information about their preferred candidate, rendering them largely unresponsive even to serious character shocks. For strong Democrats, the constraint may operate differently: having already formed negative evaluations of Trump prior to the scandal, they had effectively little room to revise their assessment further downward, and their voting intentions were firmly set regardless of the tape. In both cases, it is the strength of partisan identity—not the direction—that limits the scope for new information to alter behavior. Only swing voters, with weaker anchoring priors, update in response to the scandal.

This finding carries a mixed message for democratic accountability. On one hand, the significant response among swing voters suggests that character information does matter—citizens can and do hold politicians accountable for personal misconduct when they are open to updating their assessments. On the other hand, as political polarization intensifies and the share of swing voters shrinks, this accountability mechanism

weakens: in a highly polarized electorate, scandals may have diminishing aggregate electoral consequences precisely because the voters most likely to respond represent a shrinking fraction of the electorate.

4.4. Demographic Heterogeneity

Section 4.3 showed that the post-scandal shift concentrates among swing voters. A natural follow-up is to ask whether the response also varies along other dimensions of voter heterogeneity—demographic characteristics that could in principle shape how voters absorb and respond to scandal information.

To examine this, we re-estimate the main DID specification with triple interactions, augmenting Equation (2) with $Exposure \times Post \times Demographic$ terms for each characteristic in turn. We test five indicators: female, white, college-educated, employed full-time, and elderly (aged 60 or older). All specifications include the same birth-year fixed effects, county-by-survey-date fixed effects, and individual controls as the main specification, and standard errors are clustered at the county level.

Appendix Table A6 reports the results. Across all five columns, the estimated triple-interaction coefficient is small in magnitude and statistically insignificant. The main $Exposure \times Post$ coefficient remains negative and significant throughout. The post-scandal shift is therefore broadly distributed across demographic groups, not concentrated in any single subgroup.

This pattern is consistent with the salience-driven mechanism that operates uniformly through attention: voters with greater prior exposure attend more closely to the scandal regardless of who they are, in line with the broader logic that in a crowded news environment prior personal connection selects which stories receive notice. Notably, women do not respond more sharply than men despite the scandal involving misconduct against women, which weighs against a gender-identity reading in which female voters feel particularly targeted.

4.5. Robustness

Controlling for Cohort Dynamic Effects. A potential concern is that our exposure measure is correlated with cohort-specific dynamic effects that could confound the estimates. The baseline already includes birth-cohort fixed effects, which absorb level differences in political attitudes across generations, but does not rule out the possibility that older cohorts—who tend to have been exposed to more Miss USA titleholders—also shifted against Trump for reasons unrelated to the scandal. To address this, we re-estimate the main specification with two additional sets of controls: interactions between birth-year fixed effects and survey-date fixed effects, and, alternatively, interactions between a

linear birth-year trend and survey-date fixed effects. Appendix Table A5 reports the results: the estimated effect of exposure remains negative and significant under both specifications, confirming that the results are not driven by cohort-specific dynamics.

Permutation Test. One might worry that our results reflect unobserved location-specific factors correlated with the home counties of Miss USA state titleholders. We address this by randomly reassigning the exposure measure across individuals and re-estimating the main specification 1,000 times, generating a null distribution of coefficients. Appendix Figure A4 shows that the observed coefficient lies far outside this distribution, confirming that the effect is unlikely to arise by chance.

Age Threshold. We use age 10 as the threshold for adolescence, but the results may be sensitive to this choice. Appendix Figure A5 plots the estimated coefficient on Exposure $\times$ Post under alternative age cutoffs ranging from 6 to 12. The estimates are negative and statistically significant for cutoffs in the range of 7 to 11, and become insignificant at the extremes. This pattern has a natural interpretation. A cutoff below 7 misclassifies young children—who are too young to follow or pay attention to beauty pageants—as treated, moving genuine control individuals into the treatment group and attenuating the estimated effect. At a cutoff as high as age 12, the measure excludes individuals who were 10 or 11 at the time of a local titleholder—individuals who were genuinely old enough to have been exposed to Trump through the pageant—reducing variation in the exposure measure and lowering statistical power. That the effect is significant across the range of cutoffs centered around age 10 is consistent with our identifying assumption that meaningful exposure to Trump-as-the-pageant-owner requires having reached early adolescence.

Leave-One-State-Out Test. The results may be driven by a single influential state. We re-estimate the main specification excluding one state at a time. Appendix Figure A6 shows that the estimated effect remains significant and stable in magnitude across all specifications, ruling out that any single state drives the findings.

5. The Electoral Liability of Celebrity

The preceding section established that voters with greater prior exposure to Trump-as-the-pageant-owner shifted more sharply against him following the Access Hollywood tape. We now ask whether this attitudinal shift translated into a measurable electoral cost—whether the celebrity reach that built Trump’s local visibility also amplified the cost of the scandal at the polls.

5.1. Constructing County-Level Exposure

We construct a county-level exposure share parallel to the individual-level exposure measure in Section 4.1, defined as the fraction of a county's voting-eligible population that was exposed to a Trump-era Miss USA state titleholder. Formally:

$$\text{Exposure Share}_c = \sum_b \mathbb{1}(\text{Exposed}_{cb}) \cdot \mathbb{1}(\text{Eligible}_b) \times \frac{\text{Pop}_{cb}}{\text{Pop}_c}, \quad (4)$$

where $\mathbb{1}(\text{Exposed}_{cb})$ equals 1 if cohort b in county c was aged 10 or older when a Trump-era Miss USA state titleholder was crowned—the same exposure definition used in Section 4.1—and $\mathbb{1}(\text{Eligible}_b)$ equals 1 if cohort b had reached voting age by November 2016. The eligibility condition is necessary here because, unlike the individual-level CCES sample which consists entirely of voting-age adults, the county-level measure is constructed from the full population across all birth cohorts, and only eligible voters can translate their attitudes into votes. Pop_{cb} and Pop_c are the cohort and total county voting populations, respectively, used to weight cohorts by their share of the electorate.

We next examine whether the exposure share is correlated with county-level characteristics. Since all counties in our sample have produced a Miss USA state titleholder, they do not differ in pageant participation—the variation in exposure share comes entirely from the timing of winning, specifically whether a county's titleholder was crowned early or late during Trump's ownership period (1996–2015). Such timing is plausibly unrelated to underlying county characteristics. We regress a rich set of county-level socioeconomic and demographic characteristics on the exposure share and find no systematic relationship, as shown in Appendix Figure A7.

We further validate the quasi-randomness of the timing directly. We define an indicator EarlyWinner_c equal to one if a county produced a Miss USA state titleholder in the early Trump era (1996–2005), and regress county-level characteristics on this indicator, conditioning on state fixed effects and the total number of Miss USA state titleholders. Appendix Figure A8 shows no systematic difference between early- and late-winning counties along any observable dimension, confirming that the timing of winning is plausibly random and unlikely to reflect pre-existing county-level differences that could confound the results.

5.2. Baseline Results

We start by estimating the following baseline county-level specification:

$$Y_{cs} = \alpha + \beta \times \text{Exposure Share}_c + \phi_s + \Gamma' X_c + \epsilon_{cs}, \quad (5)$$

where Y_{cs} is the Republican vote share in county c and state s , ϕ_s is the state fixed effects, and X_c is a vector of county-level controls including the previous election's Republican vote share, poverty rate, median household income, employment, annual payroll, number of firms, population, youth ratio, sex ratio, and Black, Hispanic, and Asian population shares. For each election year, we construct the exposure share according to the demographic structure of the eligible voting population in that year. Standard errors are clustered at the state level.

Table 3. County Baseline Results

	Vote Share for			
	Trump			Romney
	2016 (1)	2020 (2)	2024 (3)	2012 (4)
Exposure Share ²⁰¹⁶	-0.008*** (0.002)			
Exposure Share ²⁰²⁰		-0.005*** (0.002)		
Exposure Share ²⁰²⁴			-0.001 (0.001)	
Exposure Share ²⁰¹²				-0.001 (0.002)
State FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
D.V. Mean	0.556	0.556	0.557	0.556
N	707	707	697	707
R ²	0.969	0.986	0.997	0.986

Notes: The dependent variable is the Republican presidential vote share in the indicated election year. Exposure Share^{Year} is constructed according to the demographic structure of the eligible voting population in that year. Column (4) uses the 2012 Romney vote share as a pre-period placebo. Controls include the previous election's Republican vote share, poverty rate, median household income, employment, annual payroll, number of firms, population, youth ratio, sex ratio, and Black, Asian, and Hispanic population shares. Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Main estimate. Table 3 reports the results. Column (1) shows a significant negative effect in 2016: counties with a higher share of residents exposed to Trump-as-the-pageant-owner experienced a lower Trump vote share, with a coefficient of -0.008 significant at the 1% level, implying a 0.8 percentage point drop in vote share for a county moving

from zero to full exposure.⁸

Persistence and decay. Column (2) shows that the effect persists into 2020, though it decays to 0.5 percentage points, remaining significant at the 1% level. By 2024, the effect disappears entirely, with a small and statistically insignificant coefficient, as shown in column (3).

Pre-scandal placebo. Column (4) provides a placebo check using the 2012 Romney vote share, prior to the Access Hollywood tape. We find no significant effect, confirming that the negative association in 2016 is specific to the scandal rather than a pre-existing feature of high-exposure counties.

Individual-level corroboration. The county-level finding holds at the individual level. Using the post-election wave of the 2016 CCES, we link respondents' realized vote choices to their Miss USA exposure, identifying within-county across birth cohorts. Individuals with greater exposure to Trump-as-the-pageant-owner are approximately one percentage point less likely to have voted for Trump in 2016, with no corresponding effect on the 2012 Romney vote. Specification and results are reported in Appendix C.

Turnout. To examine whether the observed effect is driven by changes in voter turnout rather than shifts in candidate choice, we re-estimate Equation (5) using the county-level turnout rate as the dependent variable. Column (1) of Appendix Table A7 reports the result: no statistically significant relationship between Miss USA exposure and 2016 turnout rates. The observed electoral impact therefore operates on the intensive margin (vote choice) rather than the extensive margin (composition of the electorate).

5.3. Randomization Inference

The key source of variation in the county-level exposure share is the *timing* of Miss USA state titleholders: since all counties in our sample produced at least one titleholder during Trump's ownership period, what differs across counties is not whether they won but when. A potential concern is that this timing is not purely random—some unobserved county-level factor may have determined whether a county produced a titleholder early or late in the Trump era, and that same factor may independently drive political outcomes. Although we find no systematic relationship between the exposure share and observable county characteristics, we cannot rule out unobserved confounders driving the timing.

⁸A back-of-the-envelope translation illustrates the consistency: the individual-level post-scandal net effect—the sum of the Exposure coefficient (0.019) and the Exposure $\times$ Post coefficient (-0.029), or about -1 percentage point per unit of exposure—predicts a county-level decline of approximately one percentage point at full exposure, broadly consistent with the 0.8 percentage point estimate in Table 3. Both exercises deliver negative and statistically significant estimates of similar order of magnitude, and this consistency in sign and rough size across two designs strengthens our confidence in the findings.

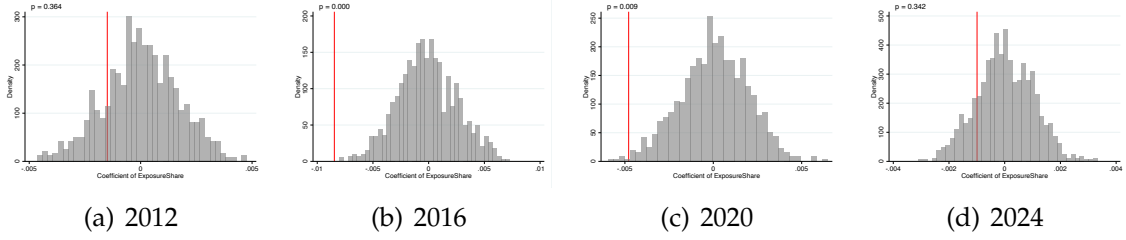


Figure 5. *Randomization Inference of the Effect of Miss USA Exposure*

Notes: This figure plots the distribution of the estimated coefficients from the randomization inference, where the county-level exposure share is re-simulated 1,000 times by randomly assigning the Miss USA state titleholders across counties within each state-year. The vertical line indicates the observed coefficient from the main specification.

We address this using randomization inference. We re-simulate the pageant history 1,000 times, each time randomly reassigning—within each state-year—which county receives the titleholder among all counties that ever produced a Miss USA state titleholder. As this procedure regenerates the timing of winning while preserving the structure of the pageant, we re-estimate Equation (5) for each simulated history. If an unobserved factor were driving both the timing and the results, we would expect it to generate similarly large coefficients in the simulated samples.

Figure 5 plots the resulting distributions across all four election years. The observed 2016 coefficient lies at the far left tail with a p-value below 0.001, and the 2020 coefficient is similarly in the left tail (p-value of 0.01). The observed effects are therefore unlikely to be driven by any systematic confounding factor of the timing. For 2012 and 2024, the actual estimates are not significant, consistent with the main findings in the previous table.

5.4. Trump vs. Pageant: Comparison with Miss America Counties

We now further strengthen the causal interpretation of the baseline result. The estimated β captures variation tied to Miss USA state-titleholder counties; it does not yet rule out the possibility that the effect reflects unmeasured features of pageant-producing communities rather than Trump-specific exposure. We address this directly using Miss America—a parallel national beauty pageant that has never been owned by Donald Trump—through two complementary tests that substitute different parts of the design. The first substitutes the comparison group; the second substitutes the treatment. Both should reject the pageant-community interpretation if our reading is correct.

We construct an analogous dataset of Miss America state titleholders using the identical procedure applied to Miss USA, yielding 2,771 titleholders from 880 distinct counties; in 230 cases the Miss America titleholder originates from the same county as a

Miss USA titleholder crowned in the same year. We first verify that Miss USA and Miss America counties are observationally similar. As reported in column (2) of Appendix Table A3—which regresses an indicator for being a Miss USA county on a rich set of county-level characteristics, restricting the sample to counties that produced either a Miss USA or a Miss America titleholder—none of Republican vote share, poverty rate, income, employment, population, or racial composition significantly discriminates between the two. Conditional on being a pageant-producing community, the two groups are observationally equivalent.

Test 1: Miss America counties as comparison group. We pool Miss USA and Miss America counties and re-estimate Equation (5). For each county we compute the Miss USA exposure share following the construction in Equation (4); counties with no Trump-era Miss USA state titleholder—including all Miss America-only counties—are therefore left with an exposure share of zero. The coefficient β captures the differential effect of Miss USA-specific exposure relative to otherwise similar Miss America counties. We further apply propensity score matching (PSM) to tighten the comparison, selecting the Miss America counties that most closely resemble Miss USA counties on observable characteristics.⁹

Appendix Table A8 reports the results across all four election years. Columns (1)–(4) present OLS estimates in the full pooled sample; columns (5)–(8) present PSM estimates. The 2016 coefficient is -0.008 in the OLS sample, significant at the 1% level, and -0.004 in the PSM sample, significant at the 5% level. The effect persists into 2020 at -0.004 , remaining significant at the 5% level in both samples. Coefficients for 2012 and 2024 are small and statistically insignificant in both approaches. The near-identical estimates across OLS and PSM confirm that Miss America counties are already a well-matched comparison group, and that the main result is not sensitive to further tightening the comparison.

Test 2: Miss America exposure as placebo treatment. The second test substitutes the *treatment* rather than the comparison group. We construct a Miss America exposure share using the identical recipe applied to Miss USA—the fraction of a county’s voting-eligible population exposed to a Miss America state titleholder—and use it in place of the Miss USA exposure share in Equation (5). Since Miss America has never been owned by Donald Trump, any negative effect of Miss America exposure on Trump’s vote share

⁹We employ 1:1 nearest-neighbor matching with a strict caliper of 0.01, matching on political preference (previous Republican vote share), socioeconomic status (poverty rate, median household income, and employment), economic activity (annual payroll and number of firms), and demographic composition (total population, youth ratio, sex ratio, and the proportions of Black, Asian, and Hispanic residents). The procedure yields a balanced sample of 887 counties (625 Miss USA and 262 Miss America). The mean standardized bias falls from 32.1% to 2.6%, the pseudo- R^2 from 0.099 to 0.002, and the joint significance test confirms balance ($p = 0.986$). Figure A9 plots the resulting propensity score distribution, showing substantial overlap between the two groups after matching.

would point to a pageant-community channel rather than Trump-specific exposure.

Appendix Table A9 reports the results across three samples: Miss USA counties only, Miss America counties only, and the combined sample. Miss USA exposure consistently predicts a lower Trump vote share, while Miss America exposure yields no significant effect in any sample.

Taking stock. The two tests substitute different parts of the design: Test 1 replaces the comparison group, using Miss America counties as the counterfactual for “pageant-producing community”; Test 2 replaces the treatment variable, using the Miss America exposure share as a placebo. Both reject the pageant-community interpretation, jointly supporting the reading that the effect is specific to exposure to Trump-as-the-pageant-owner rather than a general feature of pageant-producing communities.

5.5. Robustness Checks

Alternative Measures. To ensure that our results are not sensitive to the specific construction of the exposure measure, we construct three alternatives and re-estimate Equation (5) with each. The first is the log of an intensity-weighted exposure share, in which each cohort’s contribution is weighted by the number of Trump-era Miss USA state titleholders they were old enough to have been exposed to, rather than a simple binary indicator; this captures cumulative exposure intensity rather than mere presence. The second is a High Exposure County dummy equal to one if the annual average number of Miss USA state titleholders during Trump’s ownership period (1996–2015) exceeds the county’s annual average across all periods, indicating that the Trump era was disproportionately active in that county’s pageant history. The third jointly includes the raw count of Miss USA state titleholders during Trump’s era and the total count across all periods, separating Trump-era variation from the county’s general pageant presence. Appendix Table A10 reports the results. Across all three columns, the estimated effect on Trump’s 2016 vote share is negative and statistically significant, confirming that the baseline finding is not an artifact of how exposure is measured.

Sub-sample Results: Population. To examine whether the observed effect is driven by particularly large or small counties, we re-estimate our baseline specification on sub-samples that exclude counties at the extremes of the population size distribution, restricting to counties above the 5th and 10th population percentiles and below the 90th and 95th percentiles. Appendix Table A11 reports the results. We find consistent negative effects of Miss USA exposure on Trump’s vote share across all sub-samples, indicating the result is not driven by outliers in county size.

Sub-sample Results: Mobility. A potential concern is that population mobility introduces measurement error into the exposure measure: residents who moved into a county

after a local titleholder was crowned would not have been exposed to Trump-as-the-pageant-owner through that win, even though our measure assigns them the county's full exposure history. To address this, we re-estimate the baseline specification on the subsample of counties with low population mobility, where the mobility rate is the share of county residents who recently relocated, as reported in the American Community Survey. Appendix Table A12 reports the results across three alternative thresholds (under 3%, 5%, and 10%). The negative effect of Miss USA exposure on Trump's vote share remains significant and of similar magnitude across all three subsamples, confirming that the main finding is not driven by mobility-related measurement error.

5.6. County Partisanship and Electoral Cost

In Section 4.3, we showed that in the DID design—which compares individuals with different levels of Miss USA exposure before and after the scandal—the response to the scandal is concentrated among swing voters: only among them does greater prior exposure translate into a larger post-scandal attitudinal shift. Among Democrats and Republicans, greater prior exposure to Trump-as-the-pageant-owner does not translate into a larger attitudinal shift, suggesting that partisan identity effectively shields these groups from the scandal's persuasive force. We now ask whether a parallel pattern holds for the actual electoral cost: does the negative effect of Miss USA exposure on Trump's vote share vary by the partisan character of the county? Examining this heterogeneity allows us to assess whether the individual-level mechanism—whereby only politically unanchored voters update in response to the scandal—aggregates into a corresponding pattern at the electoral level.

To investigate this, we classify counties into three groups based on their 2012 Republican vote share as a pre-scandal baseline: red counties (above 60%), swing counties (between 40% and 60%), and blue counties (below 40%). We then re-estimate Equation (5) separately within each subsample. This design maps naturally onto the individual-level analysis: red and blue counties are partisan strongholds likely dominated by strong partisans, while swing counties contain a higher share of persuadable voters—the county-level analogue of swing voters at the individual level.

Table 4 reports the results. Column (2) shows that among swing counties, those with a higher share of residents exposed to Trump-as-the-pageant-owner experienced a meaningfully lower Trump vote share, with a coefficient of -0.010 significant at the 1% level, implying a 1.0 percentage point drop for a county moving from zero to full exposure. Columns (1) and (3) show that the corresponding coefficients in red and blue counties are small and statistically insignificant.

Columns (2)–(4) of Appendix Table A7 report the corresponding turnout findings

Table 4. Polarization and County Results

	2016 Republican Vote Share		
	Red County (1)	Swing County (2)	Blue County (3)
Exposure Share ²⁰¹⁶	-0.003 (0.005)	-0.010*** (0.003)	-0.007 (0.007)
State FE	✓	✓	✓
Controls	✓	✓	✓
D.V. Mean	0.707	0.519	0.307
N	266	324	105
R ²	0.874	0.938	0.945

Notes: The dependent variable is the Republican presidential vote share in 2016. Exposure Share²⁰¹⁶ is constructed according to the demographic structure of the eligible voting population in 2016. Columns (1) - (3) categorize counties based on their 2012 Republican vote share: those with a share exceeding 60%, those between 40% and 60%, and those below 40%. Controls include the previous election's Republican vote share, poverty rate, median household income, employment, annual payroll, number of firms, population, youth ratio, sex ratio, and Black, Asian, and Hispanic population shares. Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

for the three subsamples. The null effect persists within swing counties and within partisan strongholds, consistent with the full-sample result.

Taken together, the county-level results are consistent with the individual-level heterogeneity in Section 4.3: the electoral cost of exposure to Trump-as-the-pageant-owner is confined to swing counties, where persuadable voters are more plentiful, just as the attitudinal response to the scandal was confined to swing voters at the individual level. Partisan strongholds on both ends bear no measurable electoral cost. The turnout analysis above reinforces this interpretation: the negative effect on Trump's vote share is not driven by shifts in the composition of the electorate, but by changes in vote choice among voters with weaker anchoring priors.

6. Conclusion

Our findings answer two questions. First, does celebrity reach become a liability when a scandal lands? Yes: voters with greater prior exposure to Trump-as-the-pageant-owner punished him more sharply after the tape. Second, do voters respond to scandals whose content is orthogonal to policy? Yes, but conditionally—along two dimensions. The response is bounded by partisanship and it is bounded by time. Taken together, these findings speak to a broader phenomenon: pre-political celebrity shapes the interpretation of later political information about a candidate—in our setting, by selecting which

voters attend to a scandal that reactivates the celebrity persona.

Our design leverages variations in how *salient* the scandal is across voters—an approach inspired by, and consistent with, the large literature on media persuasion (DellaVigna and Kaplan, 2007; Gerber et al., 2009; Chiang and Knight, 2011; Da Silveira and De Mello, 2011; Enikolopov et al., 2011; Durante and Knight, 2012; Adena et al., 2015; Barone et al., 2015; Martin and Yurukoglu, 2017; Spenkuch and Toniatti, 2018; Wang, 2021), which establishes that exposure to media content shifts political attitudes. In our case, prior connection or exposure may matter for what voters attend to in a media environment saturated with political news. And the connection was itself gendered, built through the Miss USA pageant—a feature that bears on how a scandal of this kind registers with the voters who carry it.

The double-edged-sword logic should extend to celebrity built in sports—Bill Bradley, Jack Kemp, Imran Khan, George Weah, Manny Pacquiao—whose pre-political brand is similarly non-political, and to the rapidly expanding class of business-celebrity politicians: Mike Bloomberg, Mitt Romney, Howard Schultz, Carly Fiorina, Tom Steyer, Andrew Yang, and Vivek Ramaswamy in the U.S.; Sebastián Piñera in Chile; Andrej Babiš in the Czech Republic.

More broadly, the asset side and the liability side are likely only two of the dimensions along which celebrity politics shapes electoral behavior; mapping the others—how celebrity reach affects voter attention, the issues voters prioritize, the qualities they reward, and the information they trust—is an equally important direction for future work.

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Online Appendix

(Not intended for publication)

A. Supplementary Figures

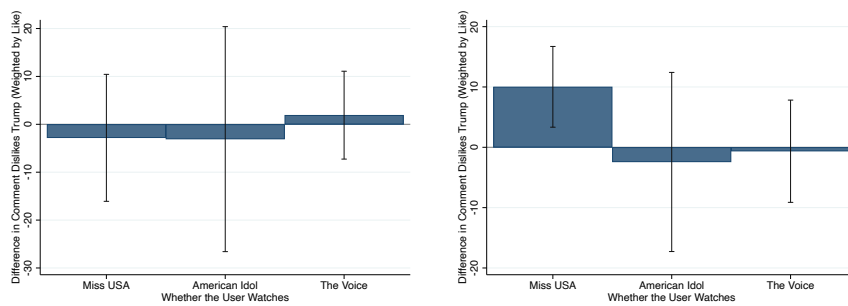


Figure A1. YouTube Comments on the Presidential Debate

Notes: This figure displays sentiment toward Trump in YouTube comments on the 2016 presidential debate livestreams before and after the Access Hollywood tape, separately by program affiliation (Miss USA, American Idol, The Voice). Negative-sentiment indices are weighted by comment likes.

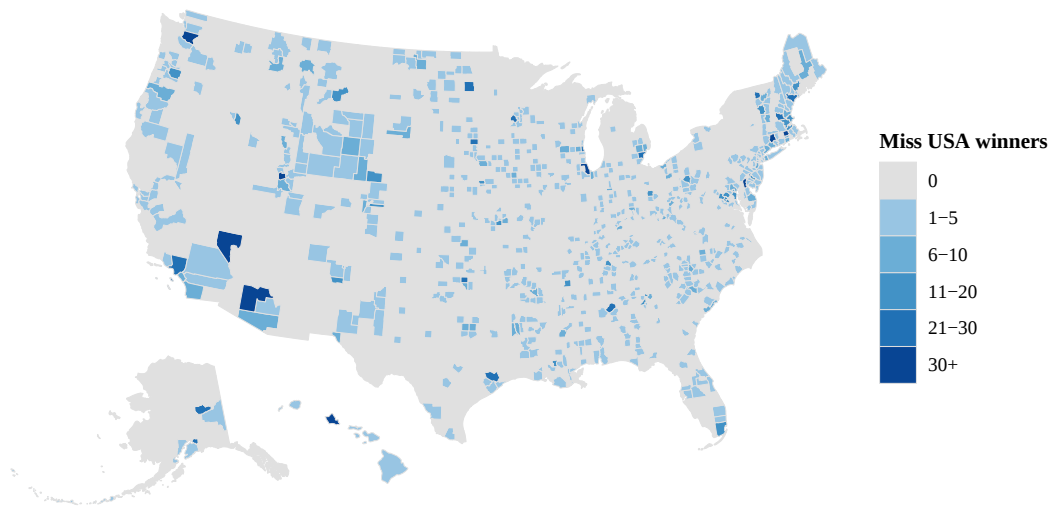


Figure A2. *Geographic Distribution of Miss USA State Titleholders*

Notes: This figure illustrates the geographic distribution of Miss USA state titleholders across U.S. counties from 1952 to 2015.

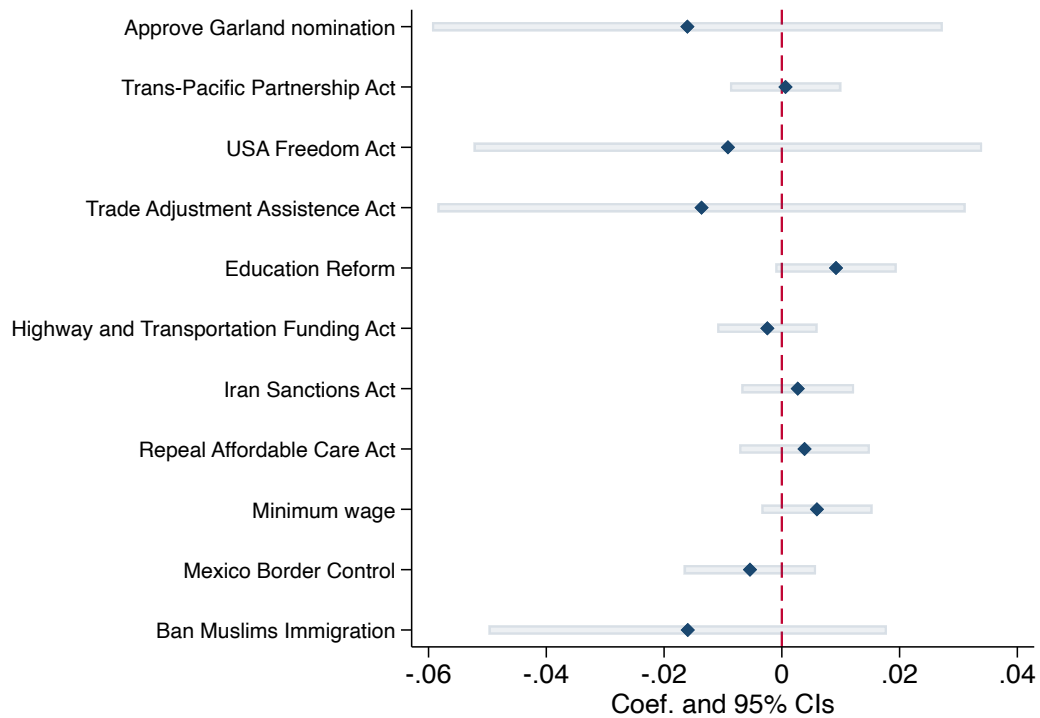


Figure A3. *Individual Exposure Balance Test*

Notes: This figure plots the coefficients of regressing a set of pre-determined policy preferences on the individual-level exposure measure, conditioning on individual characteristics, birth year fixed effects, and county-by-survey-date fixed effects. Robust standard errors are clustered at the county level.

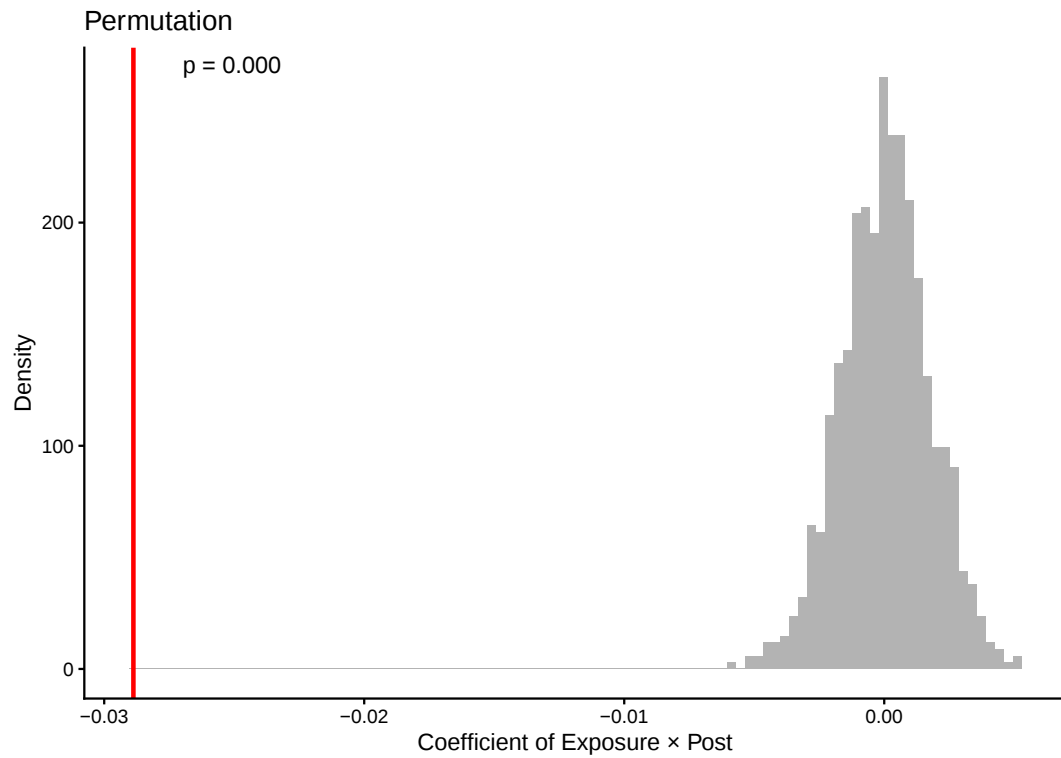


Figure A4. *Permutation Test*

Notes: This figure plots the distribution of the estimated coefficients from the permutation test, where the individual-level exposure measure is randomly reassigned across individuals. The vertical line indicates the observed coefficient from the main specification.

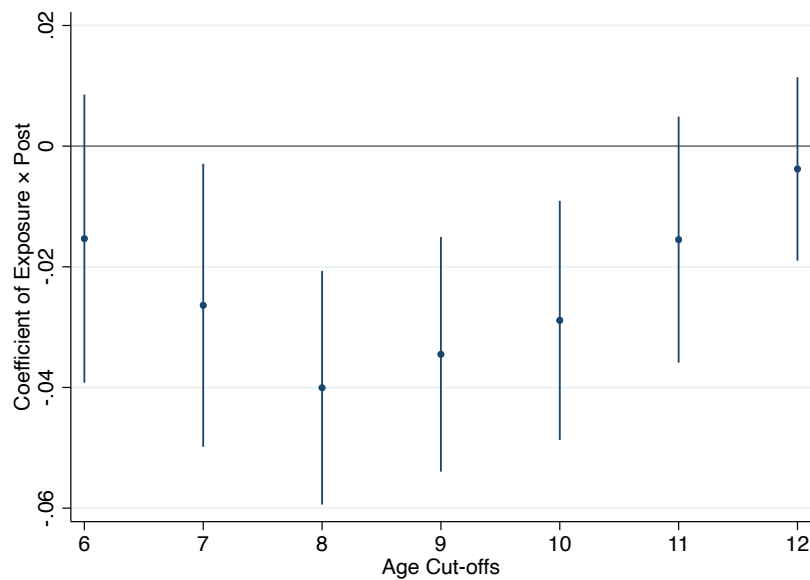


Figure A5. *Age Threshold Robustness Check*

Notes: This figure plots the coefficients from the age threshold robustness check, where the individual-level exposure measure is constructed using alternative age thresholds between 6 and 12.

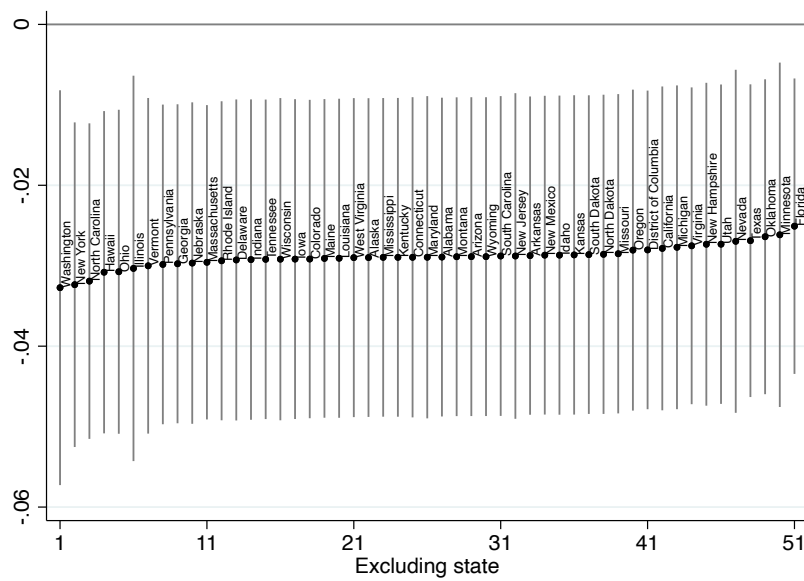


Figure A6. Leave-One-State-Out Test

Notes: This figure plots the coefficients from the leave-one-state-out test, where the main specification is re-estimated excluding each state one at a time.

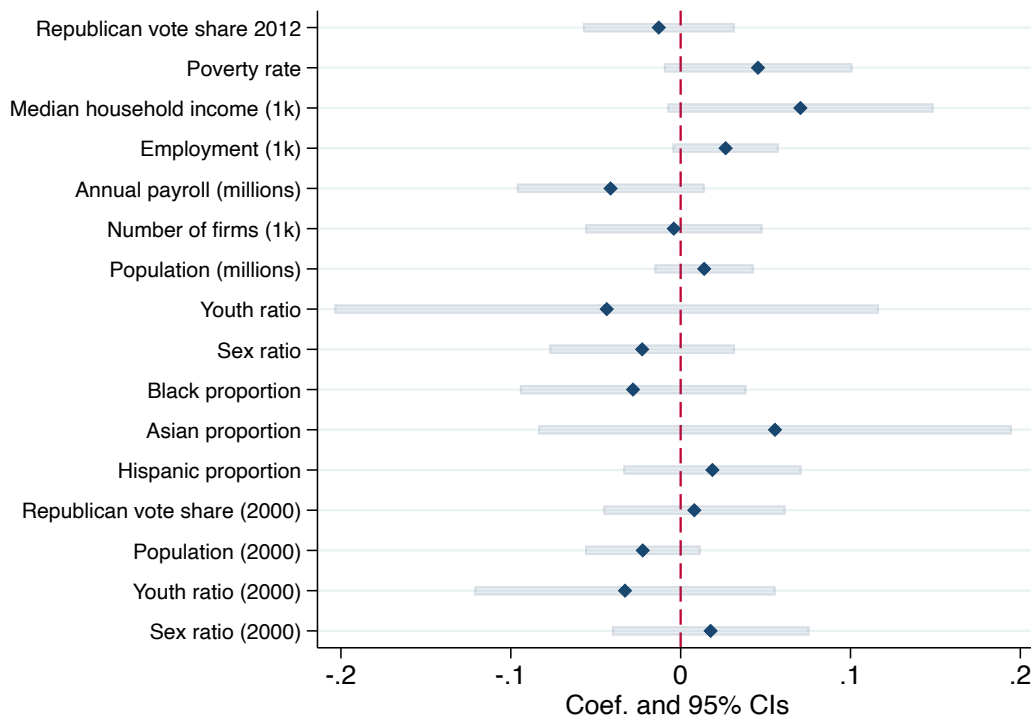


Figure A7. County Exposure Balance Test

Notes: This figure plots the coefficients of regressing a set of county-level characteristics on the county-level exposure share, conditioning on state fixed effects and additional covariates.

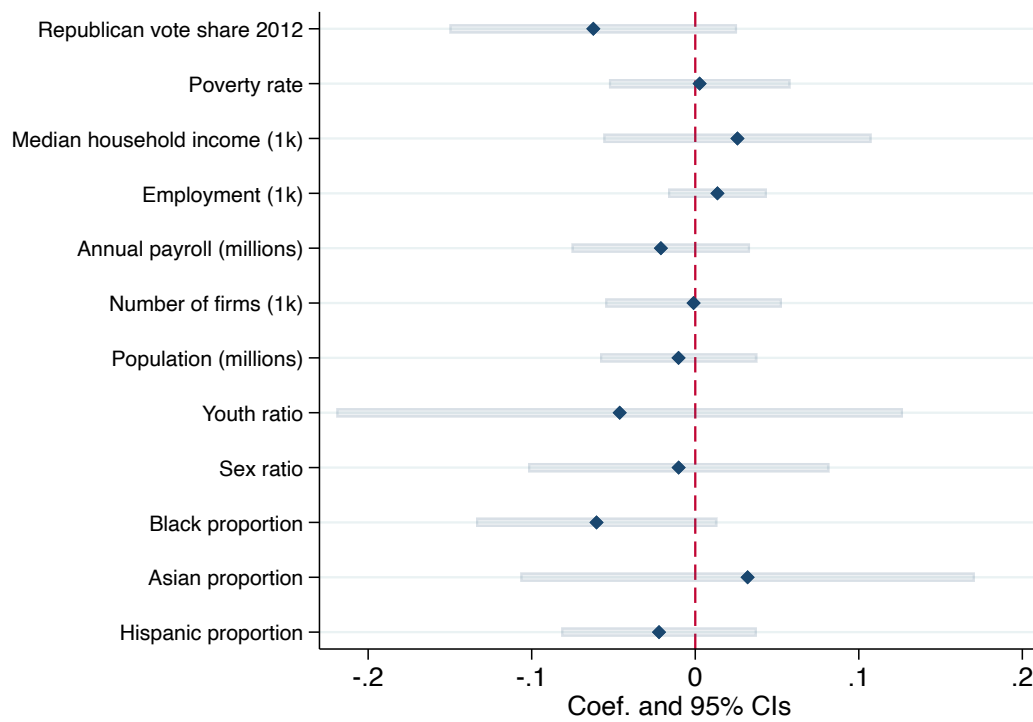


Figure A8. *Timing of the Winning Balance Test*

Notes: This figure plots the coefficients of regressing a set of county-level characteristics on the EarlyWinner, a dummy indicating the county has at least one Miss USA state titleholder in 1996–2005, conditioning on state fixed effects, the number of Miss USA state titleholders, and additional covariates.

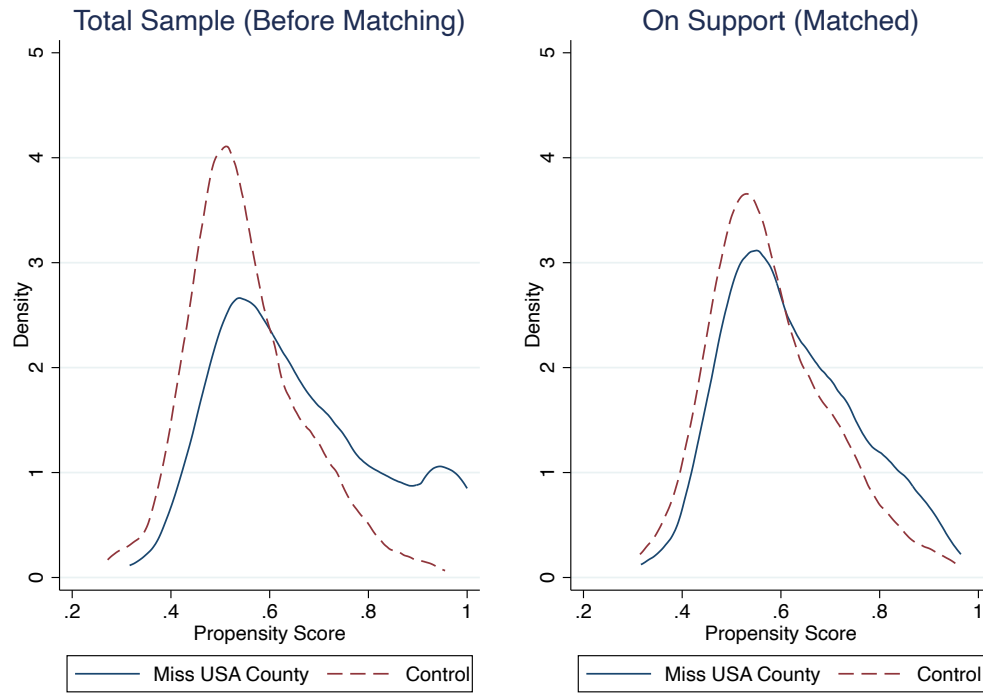


Figure A9. Propensity Score Distribution

Notes: This figure plots the propensity score distribution for Miss USA and Miss America counties.

B. Supplementary Tables

Table A1. Individual-Level Summary Statistics

	Obs.	Mean	S.D.
Preference for Voting Trump	40,573	0.413	0.492
Exposure	40,573	1.534	2.823
Birth year	40,573	1,965.637	16.567
Male	40,573	0.480	0.500
Education: No HS	40,573	0.019	0.136
Education: High school graduate	40,573	0.224	0.417
Education: Some college	40,573	0.243	0.429
Education: 2-year	40,573	0.114	0.317
Education: 4-year	40,573	0.253	0.435
Education: Post-grad	40,573	0.148	0.355
Race: White	40,573	0.729	0.444
Race: Black	40,573	0.131	0.337
Race: Hispanic	40,573	0.074	0.262
Race: Asian	40,573	0.026	0.161
Race: Native American	40,573	0.008	0.086
Race: Mixed	40,573	0.019	0.135
Race: Other	40,573	0.011	0.107
Race: Middle Eastern	40,573	0.002	0.040
Employment Status: Full-time	40,573	0.450	0.497
Employment Status: Part-time	40,573	0.102	0.302
Employment Status: Temporarily laid off	40,573	0.006	0.075
Employment Status: Unemployed	40,573	0.042	0.200
Employment Status: Retired	40,573	0.236	0.425
Employment Status: Permanently disabled	40,573	0.055	0.229
Employment Status: Homemaker	40,573	0.058	0.234
Employment Status: Student	40,573	0.034	0.181
Employment Status: Other	40,573	0.018	0.132
Party Registration: No Party, Independent, Decline to state	40,573	0.218	0.413
Party Registration: Democratic Party	40,573	0.470	0.499
Party Registration: Republican Party	40,573	0.293	0.455
Party Registration: Other	40,573	0.008	0.088
Social Media Forwarded Politics	40,573	0.234	0.423
Ideology Score(5)	40,556	3.075	1.216

Notes: This table reports summary statistics for variables used in the CCES analysis.

Table A2. County-Level Summary Statistics

	Obs.	Mean	S.D.
Republican vote share 2016	708	0.555	0.165
ExposureShare2016	708	0.571	0.469
ExposureShare (Miss America)	448	0.596	0.468
Republican vote share 2012	708	0.542	0.150
Republican vote share 2008	708	0.520	0.137
Republican vote share 2004	708	0.565	0.127
Republican vote share 2000	708	0.535	0.121
Poverty rate	708	14.359	5.351
Median household income (1k)	708	55.296	15.614
Employment (1k)	708	123.826	277.156
Annual payroll (millions)	708	6.683	18.065
Number of firms (1k)	708	6.385	14.175
Population (millions)	708	0.297	0.630
Youth ratio	708	0.100	0.040
Sex ratio	708	0.983	0.071
Black proportion	708	0.103	0.136
Asian proportion	708	0.027	0.043
Hispanic proportion	708	0.093	0.118

Notes: This table reports summary statistics for variables used in the county-level analysis.

Table A3. Counties with Miss USA Characteristics

	Dep. Var.: 1(Counties with Miss USA)	
	Counties with Miss USA or Miss America (1)	All Counties (2)
Republican vote share 2012	-0.143 (0.165)	0.066 (0.080)
Poverty rate	-0.008 (0.006)	-0.006** (0.002)
Median household income (1k)	0.000 (0.002)	0.005*** (0.001)
Employment (1k)	0.000 (0.001)	0.003*** (0.000)
Annual payroll (millions)	-0.000 (0.005)	-0.024*** (0.003)
Number of firms (1k)	-0.003 (0.006)	-0.008* (0.004)
Population (millions)	0.085 (0.150)	-0.188* (0.108)
Youth ratio	0.158 (0.360)	1.744*** (0.195)
Sex ratio	0.170 (0.208)	-0.250*** (0.063)
Black proportion	0.198 (0.205)	0.399*** (0.092)
Asian proportion	0.027 (0.613)	3.151*** (0.402)
Hispanic proportion	0.113 (0.175)	0.323*** (0.079)
State FE	✓	✓
D.V. Mean	0.446	0.325
N	1588	3558
R ²	0.053	0.326

Notes: This tables compares the characteristics of counties with Miss USA winners to those with Miss America winners and all counties. The dependent variable is an indicator for whether a county has a Miss USA winner. The first column restricts the sample to counties with Miss USA winners and counties with Miss America winners, while the second column includes all counties. All regressions include state fixed effects. Standard errors are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A4. Down-ballot placebo tests

	Preference for Voting Republican		
	Congressman	Governor	Senator
	(1)	(2)	(3)
<i>Exposure</i> × <i>Post</i>	-0.005 (0.013)	0.010 (0.011)	-0.006 (0.015)
Birth Year FE	✓	✓	✓
County × Survey Date FE	✓	✓	✓
Controls	✓	✓	✓
D.V. Mean	0.254	0.041	0.179
N	20606	20606	20606
R ²	0.550	0.514	0.513

Notes: This table tests for effects of pageant exposure on preferences for other Republican candidates. The dependent variables are dummies indicating immediate vote preference for Republican Congressman, Governor, and Senator, respectively. *Exposure* is the individual-level Miss USA pageant exposure measure defined in Equation (1), and *Post* is an indicator equal to one if the respondent was surveyed after October 7, 2016. Controls include gender, education, race, employment status, party registration, social media usage, and ideological self-placement. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5. Main Results Controlling Cohort Dynamic Effects

	Preference for Voting Trump		
	(1)	(2)	(3)
<i>Exposure</i> × <i>Post</i>	-0.029*** (0.010)	-0.029*** (0.010)	-0.039*** (0.015)
Birth Year FE	✓	✓	
County × Survey Date FE	✓	✓	✓
Controls	✓	✓	✓
Survey Date × Birth Year Trend		✓	
Survey Date × Birth Year FE			✓
D.V. Mean	0.318	0.318	0.316
N	16613	16613	16285
R ²	0.709	0.710	0.730

Notes: *Exposure* is the individual-level Miss USA pageant exposure measure defined in Equation (1), and *Post* is an indicator equal to one if the respondent was surveyed after October 7, 2016. Controls include gender, education, race, employment status, party registration, social media usage, and ideological self-placement. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6. Heterogeneity by Demographic Characteristics

	Preference for Voting Trump				
	(1)	(2)	(3)	(4)	(5)
<i>Exposure</i> × <i>Post</i>	-0.029*** (0.010)	-0.033*** (0.010)	-0.034*** (0.011)	-0.029*** (0.011)	-0.032*** (0.010)
<i>Exposure</i> × <i>Post</i> × Female	0.001 (0.004)				
<i>Exposure</i> × <i>Post</i> × White		0.005 (0.004)			
<i>Exposure</i> × <i>Post</i> × College			0.006 (0.005)		
<i>Exposure</i> × <i>Post</i> × Employed				-0.000 (0.004)	
<i>Exposure</i> × <i>Post</i> × Elderly					0.004 (0.003)
Birth Year FE	✓	✓	✓	✓	✓
County × Survey Date FE	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓
D.V. Mean	0.318	0.318	0.318	0.318	0.318
N	16613	16613	16613	16613	16613
R ²	0.710	0.710	0.710	0.709	0.710

Notes: The table examines whether the effect of the Access Hollywood tape varies by demographic characteristics. College is a dummy indicating whether she completes college education or above. Employed is a dummy indicating whether she has a full-time job. Elderly is a dummy indicating whether she is over 60 years old. *Exposure* is the individual-level Miss USA pageant exposure measure defined in Equation (1), and *Post* is an indicator equal to one if the respondent was surveyed after October 7, 2016. Controls include all interactions, gender, education, race, employment status, party registration, social media usage, and ideological self-placement. Standard errors are clustered at the county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A7. Miss USA Exposure and County Turnout

	2016 Turnout Rate			
	Full Sample (1)	Red County (2)	Swing County (3)	Blue County (4)
Exposure Share ²⁰¹⁶	-0.000 (0.003)	0.003 (0.005)	-0.000 (0.005)	-0.007 (0.009)
State FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
D.V. Mean	0.444	0.427	0.459	0.443
N	707	266	324	105
R ²	0.740	0.799	0.788	0.822

Notes: The dependent variable is the presidential election turnout rate in 2016. Exposure Share²⁰¹⁶ is constructed according to the demographic structure of the eligible voting population in 2016. Columns (1) - (3) categorize counties based on their 2012 Republican vote share: those with a share below 40%, those between 40% and 60%, and those exceeding 60%. Controls include the previous election's Republican vote share, poverty rate, median household income, employment, annual payroll, number of firms, population, youth ratio, sex ratio, and Black, Asian, and Hispanic population shares. Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A8. PSM Results

	Republican Vote Share							
	OLS in Pooled Sample				PSM Sample			
	2012 (1)	2016 (2)	2020 (3)	2024 (4)	2012 (5)	2016 (6)	2020 (7)	2024 (8)
Exposure Share ²⁰¹²	-0.002 (0.002)				-0.003 (0.002)			
Exposure Share ²⁰¹⁶		-0.008*** (0.002)				-0.004** (0.002)		
Exposure Share ²⁰²⁰			-0.004** (0.002)				-0.004** (0.002)	
Exposure Share ²⁰²⁴				-0.000 (0.001)				-0.000 (0.001)
State FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓	✓	✓	✓
D.V. Mean	0.587	0.587	0.587	0.587	0.594	0.594	0.594	0.594
N	1139	1139	1139	1134	887	887	887	883
R ²	0.981	0.968	0.987	0.996	0.983	0.970	0.987	0.996

Notes: The Pooled sample includes all counties have produced Miss USA winners or Miss America winners. The PSM control group is selected in Miss America counties according to county-level controls. The dependent variable is Republican vote share from 2012 to 2024. Exposure Share^{Year} is constructed according to the demographic structure of the eligible voting population in that year. Controls include an indicator for having produced Miss USA winners, the previous election's Republican vote share, poverty rate, median household income, employment, annual payroll, number of firms, population, youth ratio, sex ratio, and Black, Asian, and Hispanic population shares. Standard errors are clustered at state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A9. County Level - Miss America Placebo Test

	2016 Republican Vote Share		
	(1)	(2)	(3)
Exposure Share	-0.008*** (0.002)		-0.012*** (0.003)
Exposure Share (Miss America)		-0.004 (0.003)	-0.006 (0.004)
State FE	✓	✓	✓
Controls	✓	✓	✓
D.V. Mean	0.556	0.582	0.527
N	707	879	447
R ²	0.969	0.968	0.972

Notes: The dependent variable is 2016 Republican vote share. Exposure Share is the fraction of voting-eligible residents who were exposed to a Trump-era Miss USA winner. Exposure Share (Miss America) is the fraction of voting-eligible residents who were exposed to a Trump-era Miss America winner. Controls include 2012 Republican vote share, poverty rate, median household income, employment, annual payroll, number of firms, population, youth ratio, sex ratio, Black, Asian and Hispanic proportions. Robust standard errors are clustered at state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A10. Alternative Measures

	2016 Republican Vote Share		
	(1)	(2)	(3)
Log(Exposure Intensity Share + 1)	-0.007*** (0.002)		
# Miss (Trump)		-0.003** (0.001)	
# Miss (AllPeriods)		0.001* (0.000)	
HighExposureCounty			-0.008*** (0.002)
State FE	✓	✓	✓
Controls	✓	✓	✓
D.V. Mean	0.556	0.633	0.556
N	707	3109	707
R ²	0.969	0.966	0.969

Notes: The dependent variable is 2016 Republican vote share. Exposure Intensity Share is the winners-weighted fraction of voting-eligible residents who were exposed to a Trump-era Miss USA winner. High Exposure County is a dummy indicating that the annual average number of Miss USA winners during Trump's ownership exceeds the all-period average. # Miss (Trump) is the number of Miss USA winners during Trump's era and # Miss (AllPeriods) is the number of Miss USA winners across all periods. Controls include 2012 Republican vote share, poverty rate, median household income, employment, annual payroll, number of firms, population, youth ratio, sex ratio, Black, Asian and Hispanic proportions. Robust standard errors are clustered at state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A11. Sub-sample Results

	2016 Republican Vote Share			
	Sample: Population			
	> 5% (1)	> 10% (2)	< 90% (3)	< 95% (4)
Exposure Share	-0.010*** (0.002)	-0.010*** (0.002)	-0.004** (0.002)	-0.005** (0.002)
State FE	✓	✓	✓	✓
Controls	✓	✓	✓	✓
D.V. Mean	0.547	0.538	0.580	0.568
N	673	637	634	672
R ²	0.968	0.969	0.969	0.970

Notes: The dependent variable is 2016 Republican vote share. Exposure Share is the fraction of voting-eligible residents who were exposed to a Trump-era Miss USA winner. Controls include 2012 Republican vote share, poverty rate, median household income, employment, annual payroll, number of firms, population, youth ratio, sex ratio, Black, Asian and Hispanic proportions. Robust standard errors are clustered at state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A12. Results in Low Mobility Counties

	2016 Republican Vote Share		
	Sample: Mobility Rate		
	< 3% (1)	< 5% (2)	< 10% (3)
Exposure Share	-0.010*** (0.004)	-0.011*** (0.003)	-0.008*** (0.002)
State FE	✓	✓	✓
Controls	✓	✓	✓
D.V. Mean	0.526	0.555	0.559
N	260	518	625
R ²	0.978	0.971	0.968

Notes: The dependent variable is 2016 Republican vote share. Exposure Share is the fraction of voting-eligible residents who were exposed to a Trump-era Miss USA winner. Mobility Rate is the share of county residents who recently relocated, as reported in the American Community Survey. Controls include 2012 Republican vote share, poverty rate, median household income, employment, annual payroll, number of firms, population, youth ratio, sex ratio, Black, Asian and Hispanic proportions. Robust standard errors are clustered at state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

C. Individual-Level Corroboration

The post-election wave of the 2016 CCES records each respondent’s actual vote choice, allowing us to link individual-level Miss USA exposure directly to individual voting behavior. We test whether the county-level finding—that greater exposure is associated with a lower Trump vote share—holds at the individual level within counties, after absorbing county fixed effects that remove any aggregate county-level confounders. Specifically, we estimate:

$$Y_{icb}^{POST} = \alpha + \rho Exposure_{icb} + \phi_b + \theta_c + \Gamma' X_i + \epsilon_{icb}, \quad (6)$$

where Y_{icb}^{POST} is a dummy for voting for Trump in the 2016 general election, $Exposure_{icb}$ is the individual-level Miss USA exposure measure defined in Section 4.1, ϕ_b and θ_c are birth cohort and county fixed effects, and X_i is a vector of individual controls. The county fixed effects ensure that identification comes from within-county, cross-cohort variation in exposure—the same source of variation as in the main individual-level DID design. Standard errors are clustered at the county level.

Table A13 reports the results. In both panels, column (1) shows that individuals with greater exposure to Trump-as-the-pageant-owner are approximately 1 percentage point less likely to have voted for Trump in 2016, significant at the 5% level, consistent with the county-level estimates. Column (2) provides a placebo check using the probability of voting for Romney in 2012. We find no significant association in either panel, suggesting the negative relationship in column (1) reflects the electoral consequence of the scandal rather than pre-existing differences in partisan voting behavior across cohorts.

Table A13. *Individual Miss USA Exposure and Voting Results*

	Vote Trump	Vote Romney
	(1)	(2)
Panel A: Full Sample		
<i>Exposure</i>	-0.010** (0.004)	-0.002 (0.005)
D.V. Mean	0.356	0.316
N	32240	32240
Panel B: Counties with Miss USA		
<i>Exposure</i>	-0.009** (0.004)	0.004 (0.006)
Birth Year FE	✓	✓
County FE	✓	✓
Controls	✓	✓
D.V. Mean	0.324	0.294
N	22554	22554

Notes: The dependent variables are dummies indicating voted for Trump in the 2016 election, and voted for Romney in the 2012 election. Controls include gender, education, race, employment status, party registration, social media usage, and ideological self-placement. Robust stand errors are clustered at county level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.