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ORIGINAL ARTICLE

Emotional Cues, Election Outcomes, and Intimate Partner Violence

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Abstract
This paper investigates the relationship between intimate partner violence (IPV) and U.S. presidential election outcomes. Using elections from 2000 to 2016, this paper makes two contributions to the existing literature. First, it expands the emotional cue literature following Card and Dahl (Q J Econ 126:103–143, 2011) by examining a novel emotional trigger: election outcomes. Second, it provides an economic investigation of election stresses found in the psychology and political science literature. By interacting with the margin of victory for each state, I find large and significant decreases in IPV in states that vote for the winning candidate.

Keywords Emotional cues · Elections · Intimate partner violence

JEL Classification K14 · J12

Introduction

Intimate partner violence (IPV) is a major problem globally. Not only is the issue widespread, but it can also be triggered by emotional cues like financial, economic, or emotional stressors. Political stressors, in particular those generated by elections, have been studied, but little research has focused on elections before the 2016 U.S. presidential election. In addition, there has been little effort to link election-based emotional cues to the most common outcome variable used in the economic analysis of emotional cues literature: IPV.

This paper examines the effect of presidential election outcomes on intimate partner violence. The results in the literature on emotional cues and IPV find that intimate partner violence increases after unexpected sports team losses (Card and Dahl

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2011; Cardazzi et al. 2024). However, many other possible emotional triggers exist, many of which have not been analyzed. Presidential election outcomes represent one unexamined trigger source. Similar to how individuals identify with certain sports teams, some people may also identify with specific political parties, or candidates. As a result, an unexpected election defeat may trigger emotional outbursts. This paper examines the incidence of domestic violence after presidential elections, with a specific focus on the party makeup in the state of residence and the unexpectedness of the election results.

Economic research on emotional cues is widespread, dating back to Card and Dahl (2011). This seminal paper concluded that the risk of intimate partner violence is affected by "gain-loss" utility generated by National Football League (NFL) game outcomes relative to some rationally determined reference point. In the case of Card and Dahl (2011), the authors examine an outcome that may see the highest rates of "loss of control", family violence, and how it responds to shocks to an individual's gain-loss utility function caused by unexpected NFL game losses. Shocks to gain-loss utility will be called "triggers" in this paper. Several recent studies have extended this literature in two directions: expansion of outcome variables and inclusion of additional "triggers" that can influence gain-loss utility. This paper uses the standard outcome variable, reported IPV incidents, but contributes to the literature by investigating the impact of a novel trigger, presidential election outcomes.

Election outcomes are understudied as potential emotional triggers. Much like sports teams, individuals tend to align themselves with specific organizations, in this case, political parties and candidates, and may react violently if their "team" loses an election. However, there are two unique differences between elections and sporting events. First, election outcomes may have much greater outcome uncertainty than sporting events. Betting markets for sporting events are well-known as accurate predictors of game outcomes, and, while betting markets for presidential elections exist (Rhode and Strumpf 2004), they are not particularly well-known to the average voter. Most members of the public rely on polling for information about expected election outcomes, which may not be as accurate of a predictor (Prosser and Mellon 2018). This could increase uncertainty about election outcomes, leading to a larger "shock" from an unfavorable election outcome, which in turn could generate a greater loss of control as pointed out by Straus et al. (2017), leading to higher rates of intimate partner violence. Second, because presidential elections occur every four years, they may be perceived to have much higher stakes than sporting events, which occur daily or weekly.

There has been much discussion of the increased divisiveness and polarization in American politics following the 2016, 2020, and 2024 U.S. presidential elections. Discussions range from those based on anecdotal evidence (see, for example, Balz 2016 and Levin et al. 2021), and evidence from surveys (Dimock and Wike 2020), to more formal empirical analyses (see, for example, Boxell 2020). Greater divisiveness may lead more Americans to align more closely with specific political "teams", and thus, increase the likelihood of loss of control following elections.

I find that incidents of IPV increase following negative presidential election outcomes, identifying a novel trigger for emotional responses. Based on estimates from my preferred model specification that includes state margin of victory, a state that

voted for the losing candidate in the presidential election by a 1 point margin experienced a 0.7% increase in IPV incidents on election day. The impact is found for losing candidates of both political parties and appears to exist regardless of election expectations. This paper also contributes to the literature by undertaking a more rigorous empirical test of the impact of politically-driven triggers than previously existed.

Literature Review

While postulated in earlier research by Rees and Schnepel (2009), the "emotional cue" literature is often attributed to Card and Dahl (2011), who reported increases in family violence immediately following unexpected NFL team losses. An upset loss by the local NFL team was associated with roughly a 10% increase in male-on-female domestic violence incidents. This is consistent with loss aversion, proposed by Tversky and Kahneman (1991), and suggests that fans derive gain-loss utility from game outcomes relative to an expected reference point (Kőszegi and Rabin 2006). Kirby et al. (2014), Montolio and Planells-Struse (2016), Cardazzi et al. (2024), Ala'Alrababa'h et al. (2021), and Ivandić et al. (2024) all expand on the link between sporting events and crime. The magnitudes of possible effects of unexpected outcomes on crime can vary, as can the direction of the effect, as demonstrated by Lindo et al. (2018) finding that sexual assaults increase after upset wins, but not after losses. However, it is clear from the literature that sporting events, specifically unexpected sporting event outcomes, can generate negative externalities in the form of increased crime.

The application of upsets in sports contests, described by Munyo and Rossi (2013) as "Euphoria" following upset wins and "Frustration" following upset losses, has expanded well beyond crime into other outcomes. Ala'Alrababa'h et al. (2021) find that the rise of Liverpool FC football (soccer) star Mohamed Salah led to a decrease in Islamophobic tweets. Ge (2018) finds that taxi tips in New York City tended to increase after an unexpected close win by the New York Knicks. Ge (2018) found no evidence of loss aversion and proposes that it may not be the outcome (positive or negative) that is driving emotional responses, but rather the "surprise" element of an unexpected win or loss.

Using life satisfaction data from the Behavioral Risk Factor Surveillance System (BRFSS), Janhuba (2019) finds that unexpected local college football victories increase the life satisfaction of residents. Leung et al. (2017) find that Wikipedia contributions by members who identify as fans of specific NFL teams decline after their team suffers an unexpected loss. In an analysis of juvenile court cases, Eren and Mocan (2018) find that juvenile offenders in Louisiana receive approximately 30 days to their sentences after an unexpected Louisiana State University (LSU) football loss, conditional on the judge being an LSU alum. NFL victories can also play a role in judicial decisions, as pointed out by Chen and Loecher (2025).

Though the majority of the emotional cue literature finds negative effects of possible "triggers", positive responses have been found as well. As previously stated, Ge (2018) finds positive increases in taxi tipping after unexpected basketball wins.

Janhuba (2019) finds increases in life satisfaction after upset wins. Depetris-Chauvin et al. (2020) find that unexpected national team victories in African countries reduce civil conflict. Munyo and Rossi (2013) find small and brief effects of "euphoria" (an upset win) on violent crime, as does a similar study by Montolio and Planells-Struse (2016). Finally, Dilmaghani and Hu (2024) find increases in fertility in response to unexpected NHL victories in Canada. These positive effects need to be regarded as well, as it is possible that changes to IPV could be caused by an increase in IPV from "losing" sides or a reduction in IPV among "winning" sides.

A growing recent literature has taken to examining other possible "triggers" that may generate emotional responses. In a comprehensive review, Hsiang et al. (2013) conclude that interpersonal violence can increase as a result of changes in weather, in particular temperature rises[1]. Economic shocks can cause increases in IPV, as demonstrated by Anderberg et al. (2016), who found a relationship between unemployment rates and physical abuse, but the effects vary by gender. An increase in female unemployment leads to an increased likelihood of physical abuse, while an increase in male unemployment leads to declines in the likelihood of abuse. Other studies, such as Leslie and Wilson (2020), Arenas-Arroyo et al. (2021), and Gibbons et al. (2021) have explored various aspects of COVID-19 lockdowns on IPV.

There have also been more novel innovations in this literature with regards to both potential outcomes and emotional "shocks". For example, television shows have been found to generate behavioral responses, as demonstrated by Kearney and Levine (2015), who find declines in teen pregnancy during the peak popularity of the television show 16 and Pregnant and La Ferrara et al. (2012), who find declines in fertility in areas of Brazil that saw expanded coverage of soap operas, called Globo telenovelas. Music has also been shown to affect behavior, as demonstrated by Zullow (1991)'s and Edmans et al. (2021)'s analyses of consumer optimism and stock market returns, respectively. Examining U.S. Top 40 song lyrics for "ruminations about bad events and pessimistic explanatory style", Zullow (1991) finds increases in exposure to depressive music associated with increased pessimism in economic surveys. Edmans et al. (2021) employ valence measures of the top 200 songs streamed daily on Spotify in a sample of 40 countries to determine that increased popularity of higher-valence (cheerful, positive) music is associated with above-normal same-week returns and negative next-week returns.

While not examined as a trigger, elections have been analyzed as a possible outcome variable. Perhaps the most famous of these is Bagues and Esteve-Volart (2016), which use a novel dataset of Spanish Christmas Lottery winners to suggest that incumbent local politicians are much more likely to win re-election in areas that have high populations of lottery winners. These findings are bolstered by Liberini et al. (2017), which find that respondents who are more satisfied with life are more likely to vote for incumbents. However, Liberini et al. (2019), use the Understanding Society Panel Survey (UNDSOC) to find little evidence that general unhappiness played a role in Brexit voting, but do conclude that voting was influenced heavily by "feelings about their incomes". Similarly, Meier et al. (2019) find that rain during election day tends to increase votes for the incumbent in Swiss municipal elections. In the politi-

[1] See also, Burke et al. (2015), Ranson (2014), and Otrachshenko et al. (2021).

cal science literature, there is rather mixed literature on how negative shocks, such as sporting event outcomes, can affect vote share for incumbents[2].

A great deal of literature covering multiple disciplines specifically focuses on the effects of the 2016 U.S. presidential election. This literature can be broken roughly into three categories: first, the primarily psychological research on stress attributed during and after the 2016 presidential election; second, the criminology, economics, and law literature on crime rates immediately after the 2016 election; and finally, the more broad research of political identity that has blossomed after the 2016 election.

There is evidence that the 2016 presidential election was associated with increased stress. In their annual *Stress in America* survey, the American Psychological Association reported that large portions of the U.S. population were affected by "political stress" in 2017 (American Psychological Association 2017). In a study of cortisol responses among college students, Hoyt et al. (2018) finds that individuals with a negative view of Donald Trump saw higher stress levels in the week leading up to the election, a phenomenon that Baum-Baicker (2020) later called the "Trump stress effect". Hoyt et al. (2018) reported recoveries to normal stress levels for most groups after the election but also found some evidence that election stress may be more long-term. Hagan et al. (2020) also find evidence that stress related to the 2016 election may have had recurring effects until as late as February 2017. This suggests that possible election-related shocks to gain-loss utility may be much more long-lasting than other "triggers" that have been examined.

However, this and other papers (including Cepeda et al. (2018) and Tashjian and Galván (2018) have primarily analyzed self-reported health outcomes, and as a result, may be overstating possible impacts. Finally, many of these studies focus on students or racial minorities: individuals more likely to be on the losing side of the 2016 election, which could bias or overstate results. Nevertheless, this literature suggests that mental health outcomes resulting from election stress may spread over to other sectors, such as crime.

Without directly addressing the emotional cue aspect of election outcomes, there is a literature that examined changes in criminal and antisocial behavior before, during, and after the 2016 election. Most notably, Edwards and Rushin (2018) finds a large and statistically significant increase in reported hate crimes after the election of President Trump. The effect is most prominent in areas that see the highest percentages of votes for Trump, suggesting an "emboldening" effect as opposed to an emotional cue. These results are supported by Feinberg et al. (2022) and Warren-Gordon and Rhineberger (2021). Bursztyn et al. (2017), among others[3], propose that the 2016 election undermined existing social norms and may have normalized xenophobic views and actions. Giani and Méon (2021) even find evidence that the 2016 election may have fostered a contagion effect, increasing xenophobic biases outside of the United States. Finally, the 2016 election has generated significant literature on the nature of political identity. As pointed out by Gift and Miner (2017), partisanship is a fundamental component of politics. Sports fandom is generated through a

[2] See, for example. Busby et al. (2017), Busby and Druckman (2018), Healy et al. (2010), and Fowler and Montagnes (2015).

[3] See, for example, Müller and Schwarz (2020) and Crandall et al. (2018).

similar process. Like party affiliation, sports fandom is often influenced by upbringing or geography and is capable of uniting groups of disparate incomes, education levels, ethnicity, and religion. Given these similarities, it would not be surprising to see Card and Dahl (2011) reactions when faced with an electoral loss, especially as American politics grow increasingly polarized. A possibly exacerbating factor is the belief among partisans that their specific group contains a level of exceptionalism unappreciated by those in the outgroup, a phenomenon Golec De Zavala et al. (2009) describe as "collective narcissism". According to Golec De Zavala et al. (2009), collective narcissists are united by the ingroup's perceived superiority and tend to behave aggressively toward outgroups because of their perceived disrespect, making it a more general categorization than nationalism. In a review article, Golec de Zavala et al. (2019) propose that the link between collective narcissism and intergroup violence can be strengthened by negative emotional triggers, but to my knowledge, this has not been empirically tested. However, this theoretical literature does make it possible that negative election outcomes can possibly generate even greater effects than those associated with negative sporting outcomes.

In perhaps the best research on partisans' responses to election losses, Pierce et al. (2016) use a series of surveys compiled by CivScience to analyze the effects of the 2012 U.S. presidential election on self-reported happiness. Using a regression discontinuity approach, the authors find significant declines in happiness of self-reported Republicans in the aftermath of the 2012 election (which was won by Democrat Barack Obama). Self-reported Democrats saw no changes in the happiness level, consistent with loss aversion (although not described by Pierce et al. 2016 as such). Curiously, the declines in self-reported happiness associated with the 2012 election are much larger in magnitude than other potentially traumatizing events the authors' study, such as the Newtown school shooting and the Boston Marathon bombing.

One potential issue with using elections as a possible trigger is their difficulty to develop expectations for. Sporting events, for example, happen concurrently and regularly, allowing fans to develop and update their expectations gradually. Because presidential elections happen only once every four years, there is little time for updating among partisans. In addition, sporting events have widely-available betting odds to serve as accurate predictors of game outcomes. While election betting markets exist, they are fairly unknown among the general public. Partisans can use election polls to update their expectations. However, pollsters have their own issues with accuracy.

Other possible sources of election information can come from television, the internet, and neighbors and peers. However, these can be even more biased than polls. There has been a long literature on the increasing polarization of mainstream news (see Prior 2013). In addition, Chopra et al. (2024), use a series of experiments to conclude that both Democrats and Republicans demand biased news- but only if those biases do not conflict with their respective worldviews. In an analysis of Reddit communities, Gaudette et al. (2021) suggest that internet forums can be used to validate extreme views and further vilify outgroups.

Even neighbors and peers can form biased beliefs about elections. In a groundbreaking study of 180 million American voters, Brown and Enos (2021) conclude that the United States is heavily politically segregated, even at the neighborhood

level. The effect is so pronounced that "[a] large proportion of voters live with virtually no exposure to voters from the other party in their residential environment (Brown and Enos 2021 p. 998)[4]. All of these factors could create a "feedback loop", where partisan supporters, who may only interact with fellow partisans and chose to consume information that follows their partisan biases, will always expect their candidates to win elections. This would make any unfavorable election outcome an unexpected one.

In summary, there is a robust literature on emotional cues, but primarily focused on sporting results as a trigger. There is a growing psychological and medical literature on psychological stress caused by elections, but these are often theoretical or suffer from limited observations. Finally, election externalities have been examined in the political science and criminology spheres, but have primarily focused on the 2016 election. This combination of rather disjointed research leaves much to be desired if U.S. presidential elections (in general) produce externalities as emotional cues, which in turn can generate crime.

Theoretical Motivation

This section motivates the empirical analysis by applying the reference-dependent preference model of the relationship between emotional cues generated by NFL game outcomes on the likelihood that IPV occurs in a couple developed in Card and Dahl (2011) to the case of election outcomes. It also explores a special case of this model where the potential abuser in a couple gets all election-related information from other partisans and biased news sources, which affects the potential abuser's reference point for the election.

Following Card and Dahl (2011), consider a couple prone to conflict, with some risk $h \geq 0$ of a violent interaction occurring. The election outcome y functions as a possible trigger source where ($y = 1$ denotes a win for the abuser's party or candidate in the election, and $y = 0$ a loss). Based on the Card and Dahl (2011) model, gain-loss utility μ, is a function of the election outcome and $p = E(y)$:

$$h = h^0 - \mu\ (y - p) \tag{1}$$

where μ is a piece-wise linear function defined as

$$\mu\ (y - p) = \begin{cases} \alpha\ (y - p) & if\ y - p < 0 \\ \beta\ (y - p) & if\ y - p > 0 \end{cases} \tag{2}$$

The parameter α reflects the impact of a negative emotional cue on the probability a violent interaction occurs and β reflects the impact of a positive emotional cue on this likelihood. The presence of loss aversion implies that $\alpha > \beta$. In other words, negative shocks are more harmful than positive shocks are beneficial. This makes the implied probabilities of loss of control occurring

[4] See also Kaplan et al. (2022), which confirms this result.

$$h = \begin{cases} h^0 + \alpha\, p & if\ \ y = 0 \\ h^0 - \beta\ (1-p) & if\ \ y = 1 \end{cases} \tag{3}$$

Now consider the case where the abuser in the couple lives in a more partisan environment, where they interact with other partisan supporters and consumes biased election information, as documented by Brown and Enos (2021). If an individual receives biased information and interacts only with fellow partisans, they may become more certain their candidate will win, regardless of the predicted outcome based on polling or betting odds. In this situation $p = 1$, and the likelihood of a violent interaction becomes

$$h = \begin{cases} h^0 + \alpha & if\ y = 0 \\ h^0 & if\ y = 1 \end{cases} \tag{4}$$

Notice that the impact of negative outcomes (losses) on the probability of a loss of control is largest in this case because losses are *always unexpected*. In addition, victories produce the baseline probability of harm (h^0). Finally, in the case of victories by preferred parties or candidates, partisans and non-partisans respond the same way, while only partisans respond negatively to an election loss.

A second possible explanation is that a "euphoria" effect is driving results. Instead of a negative effect of losses (described by Munyo and Rossi 2013 as "frustration"), there could a strong positive effect of "winning" an election. From the perspective of the "winning" side, information is limited and likely biased. Winning an election could bolster existing world views and confirm existing biases about in-group exceptionalism. This sense of fulfillment could lead to a reduction in IPV.

Empirical Analysis

The goal of this paper is to analyze the impact of triggers generated by U.S. presidential election results on IPV. The choice of IPV as an outcome variable follows Card and Dahl (2011), who note that IPV appears to be the most sensitive crime to emotional triggers as predicted by the "loss of control" mechanism described by Baumeister and Heatherton (1996). Much like the literature on emotional cues generated by outcomes in sporting events, the empirical approach assumes individuals align with specific political parties or candidates, and may react violently in the event of an election loss by their preferred party or candidate.

In general, this paper employs a Poisson estimation approach following Card and Dahl (2011) because of the presence of many zeros in the outcome variable, the number of reported IPV incidents in a state on a specific day. The models exploit the exact timing of elections by including an indicator variable identifying election day as well as indicator variables identifying one day and one week before and after elections. Some models also include indicator variables for the month after the election to test for the presence of long-term effects like those reported by Hagan et al. (2020). As a measure of the number of partisans in each state, I interact the time dummy variables with the margin of victory in each election in each state. In theory, a state with a

higher margin of victory should contain a larger number of partisan residents than a swing state with a lower margin of victory.

Data

Following Card and Dahl (2011), I use data from the FBI's National Incident-Based Reporting System (NIBRS) to track daily incidents of IPV in each state. NIBRS tracks crime reports by reporting agency, state, and day. Between 2000 and 2019 (my sample period), 42 states plus the District of Columbia participated in the NIBRS program for at least one year. The key advantage of NIBRs is that it can measure daily criminal incidence counts, as opposed to monthly or weekly counts available in other crime data sources like the National Crime Victimization Survey.

Following a procedure similar to Kaplan (2024), I pull all crimes reported by each state participating in NIBRS for each year from 1999 to 2019. NIBRS does not have a specific designation for IPV, meaning one must be created. This involves merging NIBRS datasets for incident, victim, arrestee, and victim-offender relationship, and aggregating total incidents of IPV for each state, day, and year[5]. I categorize IPV as any reported incident of assault, battery, or assault and intimidation perpetrated by the victim's wife or husband, boyfriend/girlfriend, ex-boyfriend/ex-girlfriend, ex-husband/ex-wife, common-law spouse, or homosexual partner. Note that these criteria are more general than Card and Dahl (2011), who only analyzed male-on-female IPV. Finally, IPV incidents are aggregated at the state level for each day.

Another potential problem with NIBRS data stems from the sporadic participation of different police agencies. Because NIBRS is voluntary, not all police agencies in a state provide reports every year in the sample. In 2019, roughly 50% of all police agencies submitted reports to NIBRS, up from 30% in 2012 and only 4% in 1995. In addition, participation can vary substantially across states. While virtually all Michigan agencies submit reports to NIBRS, only one police agency in the state of Alabama participates. For this reason, state-year fixed effects are necessary to account for differences in participation across states and times. These fixed effects should also account for other characteristics. Of the sample, 19 states report at least some data to NIBRS in 2000, 29 in 2004, 35 in 2008, and 36 in 2012 and 2016; with some precincts in Colorado, Connecticut, Idaho, Iowa, Kansas, Kentucky, Massachusetts, Michigan, Nebraska, North Dakota, Ohio, South Carolina, South Dakota, Tennessee, Texas, Utah, Vermont, Virginia, and West Virginia reporting throughout the entire sample.

There may also be issues with the data structure and classification of certain NIBRS variables. Bibel (2015) points out that local police agency reporting can be subjective, and certain NIBERS criminal codes may function as "catch-words" that can vary across departments and states. The victim-offender relationship tables found in NIBRS, the tool arguably most vital for measuring IPV, can also be subject to misreporting. It is not required to list the nearest or most important victim-offender relationship, only a relationship. This may cause, for example, a "stranger" interac-

[5] Detailed information about navigating and cleaning NIBRS data can be found in Akiyama and Nolan (1999).

tion to be classified as an "ex-boyfriend" interaction, or vice versa. In addition, as pointed out by Chilton and Regoeczi (2007), nation- or state-wide changes in crime trends can be explained by the participation (or lack thereof) of certain agencies, in particular, those of large cities. I attempt to control changes in participation rate through state-year-fixed effects.

This forms an original sample of 210,457 state-day observations. However, elections only likely impact IPV for the fall months, I cut the sample to only include state-day observations that occur between September 1 and December 31 of each year. Furthermore, because presidential elections occur every four years, in practice I am only looking at five different years: 2000, 2004, 2008, 2012, and 2016. This leaves a sample to 18,680 state-day observations.[6] Figure 1 displays a histogram of the number of IPV incidents that occur in each state each day. Notice that almost 25% of the sample are state-days where no recorded IPV incidents occur. This high proportion of zeros makes a Poisson regression model preferred to a standard OLS regression model. The long right tail of the histogram may also cause some concern, as there are 485 state-days where more than 60 incidents of IPV occurred. However, these cells make up less than 0.01% of the sample. Most of these outliers occur in Tennessee, a state that sees both relatively high NIBRS participation and a very high rate of IPV (see, for example, Ortiz 2018). These outliers are included in most of the regression models but are removed as a robustness check in the appendix.

Election results from each state come from the official Federal Election Commission Report for each election. National polling data for all elections come from Gallup, one of the most trusted pollsters. For an additional specification, state polls for the 2008, 2012, and 2016 elections are used from FiveThirtyEight, a well-known pollster and poll aggregator. Summary statistics can be found in Table 1. Note that the disparity in the number of observations included is caused by the fact that IPV totals are calculated from each participating state on each day from January 1, 2000, to

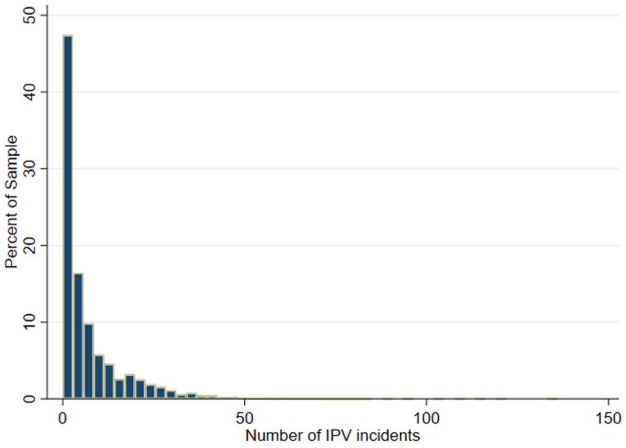

Fig. 1 Histogram of IPV Incidents

[6] A larger sample of 70,487 observations is used for the regression used for Table 1 as well as the placebo tests used in the Appendix.

Table 1 Summary statistics

Variable	Obs	Mean	Std Dev	Min	Max
Election Day	70,487	0.002	0.047	0	1
Week Before Election	70,487	0.015	0.122	0	1
Day Before Election	70,487	0.002	0.047	0	1
Day After Election	70,487	0.002	0.047	0	1
Week After Election	70,487	0.015	0.122	0	1
Month After Election	70,487	0.065	0.247	0	1
Democratic Vote Share	18,680	46.56	12.03	24.75	92.46
Republican Vote Share	18,680	50.19	11.80	4.09	72.79
IPV	70,487	6.702	9.416	0	136
Margin of Victory (MOV)	18,680	3.864	23.63	−86.77	85.93
Loss	18,680	0.417	0.493	0	1

December 31, 2018, while the election-related variables only appear in years where there is a presidential election. The average number of IPV incidents each day average around 7.2, but there is also a large amount of zero-weighting, which will warrant a nonlinear estimation. The sample contains slightly more Republican-voting states and slightly more "winning" states (states that vote for the winning candidate). Also of note is the margin of victory. While there are heavily-voting outliers (most notably Washington D.C. and Idaho), the average margin of victory is less than four points in each state.

Estimation

I estimate reduced form models explaining observed variation in the incidence of IPV following elections. The inclusion and removal of expected outcome proxy variables (polling and betting odds) permit a test of the predictions of both the standard Card and Dahl (2011) gain-loss utility model and the "feedback loop" version of the model discussed above. A linearized approximation of the estimation employed is shown below for simplicity.

$$\begin{aligned} IPV_{it} =& \alpha_0 + \beta_1 MOV_i x ElectionDay_t + \beta_2 MOV_i x WeekBeforeElection_t \\ &+ \beta_3 MOV_i x DayBeforeElection_t + \beta_4 MOV_i x DayAfterElection_t \\ &+ \beta_5 MOV_i x WeekAfterElection_t + \beta_6 MOV_i x MonthAfter\ Election_t \\ &+ \sum \zeta_{1t} DOW_t + \sum \zeta_{2t} Month_t + \sum \zeta_{3t} StatexYear_{it} + \varepsilon_{it} \end{aligned} \quad (5)$$

Note that the above model specification reflects the adaptation of the Card and Dahl (2011) model described in Eq. 4. This model does not take into account expectations, operating under the assumption that, through news exposure or peers, partisans' will always expect their candidate to win.

Analyses using sporting events as a trigger will often use IPV rates in a given team's city or state as variables of interest, assuming that this area sees a higher percentage of fans than anywhere else. With this framework, there are two "teams": each state will have some percentage of Democratic-leaning partisans and some percentage of Republican-leaning partisans. Therefore, margin of victory is used to identify

the percentage of each state who align with the winning and losing candidates. If the loss aversion hypothesis holds, a state that votes more for the losing candidate should see a higher concentration of losing partisans, and more losses of control than a state that votes more for a winning candidate. To further clarify, using MOV is not necessarily measuring expectations, it simply to track the ratio of partisans in each state.

Due to the large number of zeros found in IPV data, I employ a Poisson count model for the estimation of the parameters in Eq. 5. This approach allows for the inclusion of state fixed effects without creating an incidental parameters problem. This is important, as the participation rate for state agencies can vary substantially, and thus, fixed effects are necessary to account for these differences.

To measure the presence of partisans in a state, I include a variable reflecting the election margin of victory achieved in each state (MOV). This is calculated from taking the percentage of votes for the winning (national) candidate (scaled 0–100) and subtracting the percentage of votes for the (national) losing candidate. Therefore, a state that voted for the winning candidate will exhibit an MOV greater than 0, while a state that votes for the losing candidate will exhibit an MOV less than zero. States where the margin of victory is high (in other words, dominated by one candidate/party) should contain the highest percentage of partisans, and therefore will experience the highest likelihood of "loss of control" incidences. An additional variation using a margin of victory squared term is also employed, as there is a possibility that residents of "swing states" may respond more violently. MOV is measured by the percentage of votes won by the winning candidate subtracted by the percentage of votes won by the losing candidate, with the key assumption being that states with a greater MOV have a larger concentration of partisans[7]. As an additional specification, I also use a dummy variable equal to one if the majority of the state voted for the losing candidate (and thus, "lost" the election). This is used because it may simply be the outcome of the election that matters rather than the margin of victory for some partisans.

I also incorporate a series of dummy variables in the regression models to identify time periods before and after election day. It may be the case that partisans (or the general populace) experience a period of high-stress "election anxiety" before presidential election day, leading to higher rates of IPV in these periods. I also include indicator variables for the day and week after an election to see if any persistent gain-loss utility shocks exist. The day after the election may present a large effect as the results of the election usually are confirmed very late on Election Day, if not later. Finally, I include a variable identifying the month after a presidential election to test the hypotheses of Hagan et al. (2020) that election-related stress could be particularly long-lasting. All these time period indicator variables are interacted with the margin of victory in some empirical models.

[7] It may be the case that Democrats and Republicans have different propensities for loss of control. This is possible. However, the implication of party-specific effects would be extremely difficult due to the unbalanced nature of the panel (more Republican-leaning states, three Republican winners compared to two Democratic winners in the sample), as well as the sparse number of elections studied. As a way to indirectly investigate any heterogenous effect, I analyze each presidential election individually.

Results

Baseline results from models with no interaction terms appear in Table 2. All tables of regression results report standard errors clustered at the state-year level in parentheses. In addition, all regression models include day of the week, month, and state-year fixed effects, which are not reported for brevity. Election Days are included in the regression, as are the day and week before the election date, as well as the day, week, and month after Election Day. A month after the election is included because of some of the long-term election-related stress effects found in laboratory experiments. No significant results are found regarding changes in IPV related to the dates of presidential elections. These results could argue that elections themselves do not inherently produce changes in IPV. Instead, any results must be associated with election *outcomes*.

The main results obtained from interacting the Election Day indicator with the margin of victory from each state are found in Table 3. Column 1 uses margin of victory or defeat (percentage of votes for the winning candidate minus percentage of votes for the losing candidate), column 2 uses a squared margin of victory term to examine if swing states behave differently than "secure" states, and column 3 instead uses a dummy variable equal to one if the state "lost" the election (voted for the losing candidate). There are no effects on MOV on IPV the day and week before the election, which should not be surprising given the election outcome is not known. There are significant results for Election Day. The sign of the coefficient on the MOV term is negative, which supports the loss aversion hypothesis. States that voted for the losing candidates see increases in IPV on Election Day. The magnitude suggests that a state that voted for the losing candidate by a 1-point margin experience a 0.9% increase in IPV incidents on election day. In another interpretation, to produce the 10% IPV effect Card and Dahl (2011) found associated with upset NFL losses, a state would need to vote for the losing candidate by a margin of 11.1 points to produce this effect. However, it could also be the case that this effect is driven by decreases in IPV

Table 2 Election day results

Variable	IPV (1)
Week Before Election	0.045
	(0.028)
Day Before Election	0.032
	(0.049)
Election Day	−0.013
	(0.059)
Day After Election	0.026
	(0.054)
Week After Election	−0.015
	(0.026)
Month After Election	−0.015
	(0.029)
Constant	−21.03***
	(1.001)
Observations	70,487

Standard errors clustered at the state-year level reported in parenthesis. *, **, and *** corresponds to 0.1, 0.05, and 0.01 significance level, respectively. Day of week, month, and state-year fixed effects included in regression but not shown

Table 3 Results interacting MOV and election loss

Variable	MOV (1)	MOV^2 (2)	Lost Election (3)
Week Before Election*MOV	−0.001	−0.003	
	(0.001)	(0.001)	
Day Before Election*MOV	−0.001	−0.0009	
	(0.003)	(0.003)	
Election Day*MOV	−0.009**	−0.009***	
	(0.004)	(0.004)	
Day After Election*MOV	0.001	0.002	
	(0.003)	(0.003)	
Week After Election*MOV	−0.001	−0.001	
	(0.002)	(0.002)	
Month After Election*MOV	−0.003	−0.004	
	(0.001)	(0.001)	
Week Before Election*MOV^2		−0.000003	
		(0.00005)	
Day Before Election*MOV^2		−0.00002	
		(0.00009)	
Election Day*MOV^2		0.0002	
		(0.0001)	
Day After Election*MOV^2		−0.0001	
		(0.0001)	
Week After Election*MOV^2		−0.00002	
		(0.00006)	
Month After Election*MOV^2		−0.00005	
		(0.00005)	
Week Before Election*Loss			0.065*
			(0.037)
Day Before Election*Loss			0.089
			(0.077)
Election Day*Loss			0.205*
			(0.109)
Day After Election*Loss			−0.025
			(0.081)
Week After Election*Loss			0.0007
			(0.045)
Month After Election*Loss			0.099
			(0.052)
Constant	−20.08***	−20.08***	−20.07***
	(1.004)	(1.004)	(1.003)
Observations	18,680	18,680	18,680

Standard errors clustered at the state-year level reported in parenthesis. *, **, and *** corresponds to 0.1, 0.05, and 0.01 significance level, respectively. Day of week, month, and state-year fixed effects included in regression but not shown

in states that win the election. The effects appear to be extremely short-term; there appears to be no effects for days after the election.

There is a potential that partisans in swing states may be more likely to exhibit losses of control. For that reason, a squared margin of victory term needs to be added. Column 2 of Table 3 shows the results from this specification. The Election Day interaction with MOV matches the magnitude and significance found in column 1. There also is no evidence that swing states see higher rates of IPV. Not only are all of the MOV squared terms insignificant, the magnitude of these coefficients is very close to zero. IPV and margin of victory appear to have a linear relationship.

Table 4 IPV results separating "Winning" and "Losing" States

Variable	Won Election		Lost Election	
	(1)	(2)	(3)	(4)
Week Before Election*MOV	−0.0005	0.012*	−0.003	0.0004
	(0.002)	(0.007)	(0.002)	(0.005)
Day Before Election*MOV	−0.0007	0.012	−0.001	−0.011
	(0.004)	(0.012)	(0.004)	(0.013)
Election Day*MOV	−0.004	−0.002	−0.014**	0.0007
	(0.005)	(0.011)	(0.007)	(0.012)
Day After Election*MOV	0.00005	0.007	0.004	−0.007
	(0.004)	(0.012)	(0.005)	(0.012)
Week After Election*MOV	−0.002	−0.002	−0.0008	0.004
	(0.002)	(0.005)	(0.003)	(0.006)
Month After Election*MOV	−0.001	0.004	−0.006	−0.003
	(0.002)	(0.006)	(0.003)	(0.005)
Week Before Election*MOV^2		−0.0005**		0.0001
		(0.0002)		(0.0002)
Day Before Election*MOV^2		−0.0004		−0.0004
		(0.0004)		(0.0005)
Election Day*MOV^2		−0.00003		0.0005*
		(0.0004)		(0.0003)
Day After Election*MOV^2		−0.0003		−0.0005
		(0.0004)		(0.0005)
Week After Election*MOV^2		0.000008		0.0002
		(0.0002)		(0.0002)
Month After Election*MOV^2		−0.0002		−0.00008
		(0.0002)		(0.0002)
Constant	−19.82***	−19.81***	−19.95***	−19.95***
	(1.006)	(1.006)	(1.010)	(1.010)
Observations	10,890	10,890	7,790	7,790

Standard errors clustered at the state-year level reported in parenthesis. *, **, and *** corresponds to 0.1, 0.05, and 0.01 significance level, respectively. Day of week, month, and state-year fixed effects included in regression but not shown

Another possibility is that the outcome of the state plays a greater role as a possible trigger. Partisans in states that vote for the winner become part of the "winning team", while states that vote for the opposing candidate become the "losing team". To test this, I use a dummy variable for whether the state "lost" the election, meaning they did not vote for the winning candidate[8]. Column 3 of Table 3 shows the results of this regression. Results are generally insignificant in this specification.

Though the main specification finds a significant result, there are practical questions as to the direction of the result. The negative coefficient could be caused by increases in IPV in states that lose the election, consistent with the loss aversion framework. However, it is also possible that the result is driven by decreases in IPV in winning states, consistent with the "euphoria" framework of Munyo and Rossi (2013). As a practical test of the direction of this effect, I split the sample into states that win the election and states that lose the election and run the same regressions found in Table 5. Though perhaps crude, this can help to explain the possible direction of the effect discovered. Results from the split regression can be found in Table 4,

[8] Two states, Maine and Nebraska, separate some of their electoral votes by congressional district, making it possible for multiple candidates to win portions of the state. To control for this, I use the aggregated vote totals for each candidate statewide, with the candidate with the most votes marked as the winner.

which shows state-years that voted with the winner (and thus, "won" the election) on the left side and state-years that voted against the winner (and thus "lost" the election) on the right. We see relatively large changes in IPV with states who voted for the losing candidate, strong in magnitude than previous versions of the model. States that vote more towards the losing candidate see larger spikes in IPV on Election Day. This reflects much of the known emotional cue literature. I find no evidence of a reduction in IPV or "euphoria" effect in states that vote for the winning candidate. Though election-related emotional responses, measured in terms of, for example political violence, has gained considerable attention in recent years, these results suggest that emotional election violence is not a new phenomenon.

I also conduct a series of robustness checks, which are found in the appendix of this paper. These include examining each election in the sample separately, incorporating polling (in the form of Gallup national and FiveThirtyEight state predictions) and measuring the effects of election upsets, examining the *Bush v. Gore* Supreme Court decision, omitting the state of Tennessee (which sees one of the highest rates of IPV), and including placebo election dates using odd-numbered years. In general, these extensions do not yield significant or contesting results.

Conclusion

This paper examines the effect of United States presidential elections on rates of intimate partner violence. This paper also tries to synthesize a large and widespread psychology, political science, and criminology literature into the emotional cue space of behavioral economics. This paper hopes to establish a standardization framework for further studies on the relationship between emotional cues and the political process.

I find evidence that IPV is vulnerable to presidential election results. By interacting Election Day with the margin of victory of presidential candidates in each state, I find significant negative results during or immediately following the election. This result appears robust when adjusting for outliers and performing placebo tests. A crude splitting of the sample suggests that the effect may not be driven by loss aversion in losing states, but by declines in IPV among winning states. I find no evidence that swing states produce higher rates of IPV.

The magnitude of this effect needs examination with regard to Card and Dahl (2011). I find that a state voting for a losing candidate is associated with a 20% change in IPV, an effect approximately twice in magnitude than what Card and Dahl (2011) find with unexpected NFL outcomes. Interacting with the margin of victory, an approximately 11-point difference in the margin of victory can generate the 10% change found by Card and Dahl (2011), an effect that appears reasonable in magnitude.

This paper also presents a strange puzzle, in the fact that Election Day spikes in IPV are found, but no effect is found the day after the election. In all five elections in the sample, the presidential election was called fairly late in the night (and in the case of the 2000 presidential election, in the early hours of the next day). If the election is not known until the final hours of the day, how is the outcome of the election generating IPV spikes that occur over the entire day? One possible explanation could

be that partisans are concluding the outcome of the election before it is "official" in the same way that sports partisans will conclude the game's outcome before the game official ends. Perhaps partisans find another trigger, such as media coverage of the election (which may violate the partisan's in-group exceptionalism) or an individual state being called for the opposing candidate. This research should provide a starting point for further studies examining topics such as interplay between news coverage and emotional cues.

This also demonstrates the need for further research into emotional cues. While various researchers have identified stresses resulting from the 2016 presidential elections, I find that election-related effects in the form of intimate partner violence have existed long before the election of Donald Trump. Though this paper lacks any sort of policy recommendations, this paper should demonstrate the need for further studies and analyses of emotional responses to political and cultural events.

There are several opportunities for further research on the intersections of emotional cues and political life. One avenue could be to investigate the potential substitutability of IPV and other crimes, such as property crime. Another option is to examine if effects are seen in lower-level elections or the Congressional mid-term elections. Finally, it may be novel to tie election IPV with news viewership or social media activities such as hate speech.

Appendix

Additional Models and Tables

Examining Individual Elections

Given the changes to political polarization over the 16-year sample period, it may be the case that different elections have different effects on IPV. To measure possible changes in IPV across election cycles, I split the sample by election years. Following the consensus on increasing political polarization over time, post-election IPV may be increasing for more recent elections. Table 5 shows the results examining each election year individually. Similar to Table 3, the odd-numbered columns use margin of victory, while the even-numbered columns use a dummy variable if the state "lost" the election 9voted for the losing candidate. Columns 1 and 2 cut the sample to the year 2000, using the U.S. presidential election (Bush/Gore) as a trigger. Columns 3 and 4 cut the sample to just the year 2004, focusing on the 2004 presidential election (Bush/Kerry). Columns 5 and 6 use only 2008, focusing on the 2008 presidential election (Obama/McCain). Columns 7 and 8 use 2012, and the 2012 presidential election (Obama/Romney). Columns 9 and 10 cut the sample to 2016 and focus on the 2016 election (Trump/Clinton).

More dramatic results appear when examining elections individually. For the 2000 election, a highly contended and close U.S. election, there is a clear increase in IPV on Election Day in states that voted heavily for Al Gore (the Democratic

Table 5 Results by election

Variable	2000 (Bush/Gore)		2004 (Bush/Kerry)		2008 (Obama/McCain)		2012 (Obama/Romney)		2016 (Trump/Clinton)	
	MOV (1)	Lost Election (2)	MOV (3)	Lost Election (4)	MOV (5)	Lost Election (6)	MOV (7)	Lost Election (8)	MOV (9)	Lost Election (10)
Week Before Election*MOV	0.0008		−0.009		0.0006		0.002		−0.002	
	(0.006)		(0.005)		(0.003)		(0.002)		(0.001)	
Day Before Election*MOV	−0.008		0.012*		−0.010		−0.004		0.003	
	(0.008)		(0.007)		(0.009)		(0.005)		(0.003)	
Election Day*MOV	−0.033***		−0.016**		−0.002		−0.009		0.0008	
	(0.012)		(0.007)		(0.009)		(0.006)		(0.005)	
Day After Election*MOV	−0.009		−0.003		0.011*		−0.002		−0.0002	
	(0.009)		(0.008)		(0.006)		(0.008)		(0.004)	
Week After Election*MOV	0.002		0.0001		0.0002		−0.004*		−0.003*	
	(0.006)		(0.005)		(0.004)		(0.003)		(0.002)	
Month After Election*MOV	−0.003		−0.003		−0.005		−0.003		−0.002	
	(0.006)		(0.003)		(0.004)		(0.002)		(0.001)	
Week Before Election*Loss		0.166		0.161**		−0.050		−0.009		0.097**
		(0.150)		(0.077)		(0.063)		(0.080)		(0.043)
Day Before Election*Loss		−0.071		0.226		0.344		0.004		−0.082
		(0.060)		(0.216)		(0.207)		(0.157)		(0.072)
Election Day*Loss		0.450		0.266		0.066		0.397**		0.0006
		(0.420)		(0.255)		(0.212)		(0.173)		(0.130)
Day After Election*Loss		0.403***		−0.110		−0.113		−0.212		0.119
		(0.126)		(0.244)		(0.136)		(0.210)		(0.094)
Week After Election*Loss		−0.118		0.079		−0.070		0.129*		−0.050
		(0.187)		(0.090)		(0.099)		(0.078)		(0.039)
Month After Election*Loss		−0.044		0.137**		0.158		0.109		0.061
		(0.300)		(0.067)		(0.127)		(0.080)		(0.044)
Constant	−17.79***	−3.555***	−17.85***	−17.84***	−18.14***	−18.13***	−19.59***	−19.59***	−19.89***	−19.83***
	(1.029)	(0.080)	(1.021)	(1.021)	(1.018)	(1.017)	(1.016)	(0.112)	(1.015)	(1.015)
Observations	2,352	2,352	3,520	3,520	4,148	4,148	4,268	4,268	4,392	4,392

Standard errors clustered at the state level reported in parenthesis. *, **, and *** corresponds to 0.1, 0.05, and 0.01 significance level, respectively. State, day of week, and month fixed effects included in regression but not shown

Table 6 Results by election including MOV^2

Variable	2000 (Bush/Gore) (1)	2004 (Bush/Kerry) (2)	2008 (Obama/McCain) (3)	2012 (Obama/Romney) (4)	2016 (Trump/Clinton) (5)
Week Before Election*MOV	−0.0003	−0.007	−0.0004	0.002	−0.002
	(0.006)	(0.005)	(0.003)	(0.003)	(0.001)
Day Before Election*MOV	−0.007	0.014	−0.010	−0.007	0.004
	(0.008)	(0.009)	(0.008)	(0.005)	(0.003)
Election Day*MOV	−0.029***	−0.019***	−0.003	−0.004	0.004
	(0.007)	(0.004)	(0.007)	(0.009)	(0.004)
Day After Election*MOV	−0.013**	−0.0003	0.012*	−0.004	−0.001
	(0.007)	(0.009)	(0.007)	(0.002)	(0.004)
Week After Election*MOV	0.007*	−0.004	0.0002	−0.004	−0.001
	(0.003)	(0.004)	(0.004)	(0.002)	(0.001)
Month After Election*MOV	−0.004	−0.002	−0.006	−0.002	−0.002
	(0.004)	(0.0002)	(0.004)	(0.002)	(0.001)
Week Before Election*MOV^2	0.0001	−0.0001	0.0001	0.0001	−0.000002
	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.00005)
Day Before Election*MOV^2	−0.0002	−0.0001	0.0004	−0.0004*	−0.0001
	(0.0002)	(0.0003)	(0.0003)	(0.0002)	(0.0001)
Election Day*MOV^2	0.0005*	0.0002	0.0004	0.0003*	−0.0002
	(0.0003)	(0.0002)	(0.0003)	(0.0002)	(0.0002)
Day After Election*MOV^2	0.0005**	−0.0002	−0.0002	−0.0005*	0.00005
	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0001)
Week After Election*MOV^2	−0.00005***	0.0002*	−0.000001	0.00004	−0.0001***
	(0.00001)	(0.0001)	(0.0002)	(0.00007)	(0.00004)
Month After Election*MOV^2	0.0002	−0.00009	0.0001	0.0001	−0.000001
	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.00005)
Constant	−17.78***	−17.86***	−18.15***	−19.59***	−19.88***
	(1.029)	(1.021)	(1.018)	(1.016)	(1.014)
Observations	2,352	3,520	4,148	4,268	4,392

Standard errors clustered at the state level reported in parenthesis. *, **, and *** corresponds to 0.1, 0.05, and 0.01 significance level, respectively. State, day of week, and month fixed effects included in regression but not shown

candidate). States where Gore won larger percentages of the vote see an approximate 3.6% increase in IPV incidents for every percent increase in Democratic vote share. This is clear evidence of an emotional cue tied to a contested election result. There is a dramatically large increase in IPV found in losing states using the loss model for 2000. However, as a caution, the sample size is limited for the year 2000, and the total number of observations is relatively low. For 2004, which saw a close election that was presumed by pollsters as a dead heat, there is still evidence of increased IPV on Election day in states that voted for Democratic candidate John Kerry. However, this effect is roughly half the size of that found in 2000.

In 2008 and 2012, two relatively predictable elections, I find limited evidence of IPV increases. States that voted for 2008 election loser John McCain show little change in IPV. There is a small increase in IPV for states that voted for election winner Barack Obama in 2008 the day after Election Day, but this is of borderline significance. The 2012 election, the most predictable election in the sample, yields insignificant results. The small increase in IPV recorded the week before the election

Table 7 IPV using upsets and upset losses

Variable	(1)	(2)
Week Before Election*Gallup Upset	0.051	0.031
	(0.032)	(0.043)
Day Before Election*Gallup Upset	−0.033	0.014
	(0.058)	(0.074)
Election Day*Gallup Upset	−0.011	0.006
	(0.087)	(0.115)
Day After Election*Gallup Upset	0.088	0.071
	(0.071)	(0.103)
Week After Election*Gallup Upset	−0.070**	−0.079
	(0.035)	(0.054)
Month After Election*Gallup Upset	0.021	0.001
	(0.034)	(0.040)
Week Before Election*Gallup Upset*Loss		0.049
		(0.050)
Day Before Election*Gallup Upset*Loss		−0.117
		(0.101)
Election Day*Gallup Upset*Loss		−0.044
		(0.161)
Day After Election*Gallup Upset*Loss		0.040
		(0.138)
Week After Election*Gallup Upset*Loss		0.023
		(0.066)
Month After Election*Gallup Upset*Loss		0.051
		(0.042)
Constant	−20.05***	−20.05***
	(1.004)	(0.050)
Observations	18,680	18,680

Standard errors clustered at the state-year level reported in parenthesis. *, **, and *** corresponds to 0.1, 0.05, and 0.01 significance level, respectively. Day of week, month, and state-year fixed effects included in regression but not shown

in 2012 is likely caused by this time period overlapping with Halloween, which often sees and increase in IPV.

Interestingly, counter to claims in the psychology literature, I find no evidence of an increase in IPV during the 2016 election. However, there is some limited evidence that the Trump election saw a long-term increase in IPV, albeit a small one. States with a high concentration of Trump voters see a small increase in IPV up to a month after the election. The coefficient is relatively small, but there may be some evidence in support of the long-term election stress phenomenon found in the psychology literature, and perhaps warrants further study.

Table 6 shows the results of separating the data by election, similar to Table 4, only including margin of victory (MOV) squared. Column 1 refers to the 2000 election (Bush/Gore), column 2 the 2004 election (Bush/Kerry), column 3 the 2008 election (Obama/McCain), column 4 the 2012 election (Obama/Romney), and column 5 the 2016 election (Trump/Clinton). Examining MOV, we find little change in most coefficients compared to Table 4. Like that table, there is a strange result where states that lost the 2016 election saw increases in IPV the week before the election began. This could be perhaps explained by a more severe case of "election anxiety" for states that voted heavily for the opposing candidate (Clinton). Also present is the small increase in IPV the month after the election for states that voted for the losing can-

Table 8 IPV including state polling

Variable	(1)	(2)
Week Before Election*538 Upset Loss	0.168***	
	(0.211)	
Day Before Election*538 Upset Loss	0.120***	
	(0.041)	
Election Day*538 Upset Loss	0.170***	
	(0.040)	
Day After Election*538 Upset Loss	0.349***	
	(0.058)	
Week After Election*538 Upset Loss	0.149***	
	(0.042)	
Month After Election*538 Upset Loss	0.084**	
	(0.033)	
Week Before Election*538 MOV difference		−0.0004
		(0.001)
Day Before Election*538 MOV difference		−0.001
		(0.002)
Election Day*538 MOV difference		−0.004
		(0.003)
Day After Election*538 MOV difference		0.003
		(0.003)
Week After Election*538 MOV difference		−0.001
		(0.001)
Month After Election*538 MOV difference		−0.003**
		(0.001)
Constant	−19.63***	−19.66***
	(1.005)	(1.005)
Observations	12,808	12,808

Standard errors clustered at the state-year level reported in parenthesis. *, **, and *** corresponds to 0.1, 0.05, and 0.01 significance level, respectively. Day of week, month, and state-year fixed effects included in regression but not shown

didate. Again, this effect is somewhat unexplained, but perhaps could lend support from the laboratory experiments of Hagan et al. (2020) and others.

Though some coefficients for the MOV squared terms are significant, they are very small in value. We find little evidence that much of the increase in IPV is driven by swing states. The larger the concentration of partisans seems to be a strong determining factor in the relationship between elections and IPV. With partisan sorting increasing Brown and Enos (2021), this may be of concern for future policymakers.

Incorporating Polling

Additional specifications are included in the incorporating polling data in some model specifications to account for potential election outcome shocks relative to expectations. Including polling data in the empirical models complicates the analysis. Presidential polls and polling methods are almost countless, which makes finding a poll to use in this analysis difficult. The quality of the poll (such as the polling technique, the sample size, or its accuracy to the true election outcome), the visibility of the poll (did the poll come from a widely-known source?), and the timing of the poll (ideally, the closer to the election day, the more accurate the information) must all be taken into account. In addition, it could be the case that partisans are just as concerned with the outcome of their state as they are with the nationwide election results. For that

Table 9 IPV and Bush v. Gore SCOTUS decision

Variable	(1)
Week Before *Bush v. Gore*	0.132**
	(0.045)
Day Before *Bush v. Gore*	0.048
	(0.105)
Bush v. Gore Decision	0.205*
	(0.118)
Day After *Bush v. Gore*	0.024
	(0.082)
Week After *Bush v. Gore*	0.104
	(0.086)
Constant	−17.79***
	(1.029)
Observations	2,352

Standard errors clustered at the state level reported in parenthesis. *, **, and *** corresponds to 0.1, 0.05, and 0.01 significance level, respectively. State, day of week, and month fixed effects included in regression but not shown

reason, polling from each state may be useful as well, especially in the case of swing states. To simplify matters, I use national polling from Gallup, taking the national poll closest to Election Day. I also include dummy variables for upset election results (the 2016 election), whether the state voted for the winning candidate, and whether the winning candidate lost the popular vote (2000 and 2016).

Table 7 shows results using Gallup polls to determine upset outcomes. Gallup, considered to be the most trusted pollster, only incorrectly predicted one election in the sample (2016)[9]. We denote the 2016 as an "upset" election, where Gallup (and most pollsters for that matter) incorrectly predicted the election, creating a true "surprise". We mark only the 2016 election as an upset in Column 1, with Column 2 including states that voted for the losing candidate in 2016 (Clinton). These dummy variable regressions provide a simplistic view on the effects of a truly surprising election result and how pollsters- at least one well known pollster can affect views. Seemingly in support of Eq. 4, polled upsets are not associated with dramatic changes in IPV. In fact, results seem particularly muted and insignificant. There does appear to be a slight increase in IPV in the week before the election in states that voted for Clinton, suggesting a case of "election anxiety". But overall, a shock result does not seem to warrant larger effects on IPV.

Given the limited number of true election upsets, voters may be interested in the outcome of their state. Having your state "win" or "lose" the election (voting for the winning or losing candidate, respectively) may also generate losses of control. We measure predicted state outcomes from pollster FiveThirtyEight. A well-trusted pollster for the 2008, 2012, and 2016 elections, they predict the expected outcome for each state. We record state upsets, when FiveThirtyEight incorrectly polled a candidate to win the election. A state upset could generate even stronger emotional responses, as they may be tied with existing expectations (consistent with Eq. 3 proposed by Card and Dahl 2011), guilt (if an abuser did not vote for example), or an erosion of ingroup exceptionalism. Table 8 show these results.

Column 1 of Table 8 uses a dummy variable of upsets (when FiveThirtyEight incorrectly predicted how the state would vote), while Column 2 examines the dif-

[9] The 2004 election was marked by Gallup as a dead heat in their final poll.

Table 10 IPV with *Bush v. Gore* interacted with MOV

Variable	MOV (1)	MOV^2 (2)	Lost Election (3)
Week Before *Bush v. Gore**MOV	0.012**	0.010*	
	(0.006)	(0.006)	
Day Before *Bush v. Gore**MOV	0.003	0.001	
	(0.006)	(0.007)	
Bush v. Gore Decision*MOV	0.015	0.009	
	(0.011)	(0.009)	
Day After *Bush v. Gore**MOV	−0.007	−0.011*	
	(0.011)	(0.006)	
Week After *Bush v. Gore**MOV	0.005	−0.003	
	(0.008)	(0.007)	
Week Before *Bush v. Gore**MOV^2		0.0001	
		(0.0001)	
Day Before *Bush v. Gore**MOV^2		0.00007	
		(0.0002)	
Bush v. Gore Decision*MOV^2		0.0004	
		(0.0003)	
Day After *Bush v. Gore**MOV^2		0.0005**	
		(0.0002)	
Week After *Bush v. Gore**MOV^2		0.0002	
		(0.0002)	
Week Before *Bush v. Gore**Loss			−0.147
			(0.269)
Day Before *Bush v. Gore**Loss			−0.283
			(0.256)
Bush v. Gore Decision*Loss			−0.332
			(0.337)
Day After *Bush v. Gore**Loss			0.271*
			(0.157)
Week After *Bush v. Gore**Loss			−0.256
			(0.238)
Constant	−17.79***	−17.78***	−17.79***
	(1.029)	(1.029)	(1.028)
Observations	2,352	2,352	2,352

Standard errors clustered at the state level reported in parenthesis. *, **, and *** corresponds to 0.1, 0.05, and 0.01 significance level, respectively. State, day of week, and month fixed effects included in regression but not shown

ference between the actual margin of victory (for the election winner) compared to FiveThirtyEight's predicted. Here, there is a considerable amount of "election anxiety" the day before the election. But the largest effects are found the day after election. A state upset can increase IPV by as much as 35%. This effect is massive and persistent. Up to a week after the election, IPV in upset losing states still see a 15% increase in IPV. A small increase in found even as much as one month after the election ends, suggesting that elections may have a longer impact than traditional emotional cues when they are unexpected. Individual nuances such as how much the state won/lost compared to poll predictions do not seem to play a role in influencing emotional responses.

Table 11 IPV results omitting Tennessee

Variable	MOV (1)	MOV^2 (2)	Lost Election (3)
Week Before Election*MOV	−0.002	−0.002	
	(0.001)	(0.001)	
Day Before Election*MOV	−0.001	−0.001	
	(0.003)	(0.003)	
Election Day*MOV	−0.010**	−0.010***	
	(0.004)	(0.004)	
Day After Election*MOV	0.001	0.002	
	(0.003)	(0.003)	
Week After Election*MOV	−0.002	−0.002	
	(0.002)	(0.002)	
Month After Election*MOV	−0.004***	−0.004***	
	(0.001)	(0.001)	
Week Before Election*MOV^2		0.00001	
		(0.00005)	
Day Before Election*MOV^2		−0.00008	
		(0.00009)	
Election Day*MOV^2		0.0002	
		(0.0001)	
Day After Election*MOV^2		−0.0001	
		(0.0001)	
Week After Election*MOV^2		−0.00001	
		(0.00006)	
Month After Election*MOV^2		−0.00005	
		(0.00006)	
Week Before Election*Loss			0.089**
			(0.036)
Day Before Election*Loss			0.074
			(0.085)
Election Day*Loss			0.254**
			(0.112)
Day After Election*Loss			−0.017
			(0.088)
Week After Election*Loss			0.018
			(0.045)
Month After Election*Loss			0.114*
			(0.059)
Constant	−20.82***	−20.82***	−20.81***
	(1.004)	(1.004)	(1.004)
Observations	18,070	18,070	18,070

Standard errors clustered at the state-year level reported in parenthesis. *, **, and *** corresponds to 0.1, 0.05, and 0.01 significance level, respectively. Day of week, month, and state-year fixed effects included in regression but not shown

Accounting for *Bush v. Gore* Decision

The 2000 presidential election warrants further discussion, as it was not officially decided on Election Day. With less than 1,000 votes deciding the state of Florida (and as a result, the presidency), the Florida Supreme Court ordered a recount of all undervotes that may have been missed by vote tabulators. This decision was challenged by the Bush campaign (who had eventually been declared winners of the state). Finally,

Table 12 IPV results using placebo dates

Variable	(1)
Week Before Placebo Election	0.085***
	(0.020)
Day Before Placebo Election	−0.025
	(0.037)
Placebo Election Day	0.032
	(0.040)
Day After Placebo Election	0.018
	(0.037)
Week After Placebo Election	0.034
	(0.024)
Constant	−21.03***
	(1.001)
Observations	70,487

Standard errors clustered at the state level reported in parenthesis. *, **, and *** corresponds to 0.1, 0.05, and 0.01 significance level, respectively. State, day of week, month, and year fixed effects included in regression but not shown

on December 12, 2000, 35 days after Election Day, the U.S. Supreme Court halted the recount, cementing George W. Bush as the winner of the election.

The *Bush v. Gore* decision was controversial and received considerable negative coverage in major newspapers[10]. With uncertainty around the outcome of the election continuing over a month after the election took place, it makes sense that we may see increased rates of "election anxiety", and, as a result, increased rates of IPV. In this sense, the timing of the Bush v. Gore decision on December 12 behaves like an additional election trigger.

Table 9 shows the timing of the *Bush v. Gore* decision on IPV. The decision itself seems to warrant a fairly large increase in IPV, however the significance is marginal. There is a fairly large increase in IPV the week before the Supreme Court decision. This could be explained by increased uncertainty and anxiety in the lead-up to the Supreme Court decision. This was also during a period of intense press coverage about the case, which may have exacerbated tension.

Table 10 interacts the timing of the *Bush v. Gore* case with margin of victory (column 1), margin of victory squared (column 2), and election loss (column 3) for each state. As a whole, we find little evidence that IPV was higher in states that voted for the losing candidate (Gore) during the time of the *Bush v. Gore* decision, suggesting that the "decision anxiety" effect was not limited to specific states. A possible reason for the limited post-decision effect could be that Bush v. Gore did not generate a "surprise" outcome, and simply confirmed the election winner.

Removing Tennessee

Table 11 replicates Table 3 but omitting observations from Tennessee. Tennessee has a high rate of reporting and some precincts (particularly around the city of Memphis) see unusually high rates of IPV. Tennessee is primarily responsible for the long right tale, which could be biasing the Poisson estimation. These corrections do little to change the main results.

[10] See, for example, Cole (2005).

Incorporating Placebo Dates

Finally, there is a possibility that, because the United States holds presidential elections at approximately the same time, the results are not caused by the elections themselves. Instead, it may be the case that the election dummy variable is "picking up" some characteristic of the data that is not picked up by the state, month, or year fixed effects. To test this possibility, I use the Tuesday after the first Monday in November (the designation for Election Day) of odd-numbered years as a placebo[11]. Odd-numbered years were chosen to avoid conflict with mid-term Congressional elections, which may also generate losses of control. While elections do take place in odd years at the state and local level, these elections tend to generate lower turnouts and have "lower stakes" than presidential elections. Therefore, we should see much smaller or non-existent results here.

Table 12 shows the results of these placebo election dates. The coefficients on Election Day, the days before and after the election, and the week after the election are insignificant. These results make it difficult to assume any multicollinearity between Election Day and any fixed effects used. However, there may be a point of concern with the week before election variable, which remains significant. This may also explain this variable's robust significance through multiple models. One probable explanation for this effect is that the variable is picking up crime increases that occur at the end of October, usually during Halloween. Though the sample size is limited further, in odd-numbered years where the placebo Election Day is held after November 7, there are not significant increases in IPV the week before the placebo election.

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Data Availability No datasets were generated or analysed during the current study.

Declarations

Conflict of interest The authors declare no competing interests.

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[11] Election Day cannot be held on November 1 st, as the holiday is described as "The Tuesday after the first Monday".

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