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ARTICLE

Threat, emboldenment, or both? The effects of political power on violent hate crimes*

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Abstract

How do expressions of support or opposition by the U.S. federal government, influence violent hate crimes against specific racial and ethnic minorities? In this article, we test two hypotheses derived from Blalock's (1967) conceptualization of intergroup power contests. The political threat hypothesis predicts that positive government attention toward specific groups would lead to more hateful violence directed against them. The emboldenment hypothesis predicts that negative government attention toward specific groups would also lead to more hateful violence directed against them. Using combined data on U.S. government actions and federal hate crime statistics from 1992 through 2012, vector autoregression models provide support for both hypotheses, depending on the protected group involved. We conclude that during this period, African Americans were more vulnerable to hate crimes motivated by political threat, and Latinx persons were more vulnerable to hate crimes motivated by emboldenment.

KEYWORDS
emboldenment, government actions, political threat, violent hate crime

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1 | INTRODUCTION

Since the election of Donald Trump as President of the United States, the media has reported an increase in the number of hate crimes against marginalized groups defined by their race, ethnicity, sexual orientation/identity, and religion (Burch, 2017; Goldman, 2017; Hauslohner, 2018; Potok, 2017).[1] In fact, the Center for the Study of Hate and Extremism found that hate crimes reported to the police rose 12.5 percent from 923 in 2016 to 1,038 in 2017 in the ten largest U.S. cities, with the most common motivations being anti-Black, anti-Semitic, anti-gay, and anti-Latino (Levin & Reitzel, 2018). Although hate crime rates have typically spiked during election years in the United States, 2017 was the first year in which they continued to rise once the presidential election was over (Levin & Reitzel, 2018). Indeed, according to the FBI's annual Hate Crime Statistics report, hate crimes were 17 percent higher in 2017 than in 2016 (7,175 and 6,125, respectively).[2]

Scholarly analyses have claimed that President Trump's hostile rhetoric toward specific groups has emboldened individuals to perpetrate violence against those he mentions (Müller & Schwarz, 2018). Although this may be true, we argue that the rise and fall of violence against marginalized groups have long been sensitive to gains and losses of political power for those groups (e.g., Cunningham, 2012; McVeigh, Cunningham, & Farrell, 2014; Van Dyke & Soule, 2002), and that the emboldenment of perpetrators toward violence is just one possible dynamic. When we consider Blalock's (1967) power-threat theory, which explicitly links political power to hostility against

[1] We use the words "minority," "marginalized," and "protected group" interchangeably to refer to nondominant groups.

[2] The reports are found exclusively online, last accessed May 25, 2020 (https://www.fbi.gov/services/cjis/ucr/publications#Hate-Crime%20Statistics).

marginalized groups, we also anticipate that when marginalized groups gain political power, the established dominant group will become more hostile (Van Dyke & Soule, 2002). For instance, it was immediately after Emancipation in 1865 that the Ku Klux Klan formed as a means to preserve white supremacy (Levin, 2002). Similarly, when members of the Black community[3] gained economic status in various U.S. cities, the communal backlash against them was often severe. In Tulsa, Oklahoma, for instance, White mobs rioting against the Black community left up to 300 dead and tens of thousands homeless (Ellsworth, 1992; Oklahoma Commission to Study the Race Riot of 1921, 2001). Countless other examples in U.S. history demonstrate this pattern.

Prior research typically analyzes the association between political threat and various forms of nonviolent reactionary mobilization, such as social movement formation (Cunningham, 2012; Van Dyke & Soule, 2002) or dominant group voting behavior (McVeigh et al., 2014). But our study turns to Blalock's (1967) original conceptualization of intergroup power contests—and what became known as "power-threat theory"—to articulate political threat and emboldenment hypotheses that explain changes in patterns of violent hate crimes against specific groups in the United States since the early 1990s. Although all hate crimes are inherently violent, we explicitly focus on violent hate crimes using the Federal Bureau of Investigation (FBI) definition because of the extremity of the acts, because of the enduring relevance of direct violence as a tool by which dominant groups intimidate and control marginalized groups (Müller & Schwarz, 2018; Tolnay & Beck, 1992), and because measures of violent crimes are less subject to underreporting bias than nonviolent crimes (Hart & Rennison, 2003).

The *political threat* hypothesis predicts that violent backlash against specific groups is triggered by political gains made by those groups (e.g., new civil rights protections, meaningful changes in economic and social status, or support among powerful political elites; Taylor, 2016). This interpretation would characterize violent hate crime as a mechanism to reinforce the dominant group's dominion in the power equilibrium across groups (Blalock, 1967). In this way, perpetrators associated with dominant groups use violent hate crimes to try to reestablish social dominance when marginalized groups begin to achieve more power (Eitle, D'Alessio, & Stolzenberg, 2002). If political threat contributes to the variation in violent hate crimes, we would expect to see increases in violent hate crimes against groups that recently gained political power, signaling a potential threat to status quo hierarchies.

A complementary approach, which we call the *emboldenment hypothesis*, predicts that increases in violent hate crimes against certain populations are triggered by government elites who signal supremacy over those groups, emboldening some members of the dominant group to commit violent action. Within the context of intergroup power, we interpret explicit anti-group acts by the government as signals to some members of the dominant group that they can combat the perceived threat of minority power growth without consequence. In other words, some may perceive that the government is giving them a license to act on their anger. Indeed, scholars have tied hate speech—particularly by those in power—to discriminatory action, retaliatory violence, and hate crimes (Gagnon, 1995; Kalmoe, 2014; Kteily, Bruneau, Waytz, & Cotterill, 2015; Müller & Schwarz, 2018). As Sara Lipton argued, "[A] heightening of rhetoric against a certain group can incite violence against that group, even when no violence is called for. When a group is labeled hostile and brutal, its members are more likely to be treated with hostility and brutality" (2015, p. 1). For instance, for many decades after the end of *de jure* slavery in the United States, politicians and law enforcement officials condoned the lynching of Black Americans, signaling impunity and approval for those who would engage in such acts (Bouie, 2017; Tolnay & Beck, 1995).

[3] We use the terms Black and African American interchangeably in this article.

Most existing studies of political threat have been focused on the expansion of wealth, voting, population growth, or political representation among marginalized groups as the main source of political threat (Blalock, 1967; Eitle et al., 2002; Taylor, 2016; Van Dyke & Soule, 2002). But we argue that focused attention from politicians—through their speech and their policy proposals—can induce backlash or emboldenment effects without necessarily creating material benefits or committing material harm to marginalized communities. Even without altering *de facto* economic opportunities or political power, favorable actions by the federal government may signal the government's commitment to expanding economic and agenda-setting power for African Americans or other marginalized groups. This expectation may result in *anticipatory* mobilization, meaning that backlash could occur without any favorable policies coming to pass. Conversely, focused negative government attention—through officials' discriminatory or racist speech and policy proposals—might similarly embolden violent hate crimes against the relevant group. Indeed, prior research on emboldenment has often been focused on the ways in which official rhetoric—such as racist or exclusionary language—can propel violence against marginalized communities (Gagnon, 1995; Kalmoe, 2014; Kteily et al., 2015; Lipton, 2017; Müller & Schwarz, 2018). Thus, both policies and rhetoric regarding a protected group can produce political threat, emboldenment, or both effects.

Our study therefore assesses these two compatible components of power threat theory, which predict an increase in violent hate crimes both during times of increased political power for specific marginalized groups (*political threat*) and during times of political retrogression for such groups (*emboldenment*). We generate hypotheses and test them using vector autoregression on monthly data constructed from recorded events on U.S. federal government actions—including speech and material action—and violent hate crimes specific to Black and Latinx people between 1992 and 2012. Our aim is to assess how political threat and emboldenment relate to changes in violent hate crimes targeting those groups. Although we find different effects across each demographic group, our findings suggest that speech and actions by federal officials toward these groups affect patterns of hate crimes against them. We conclude that during this period, African Americans were more vulnerable to hate crimes motivated by political threat, and Latinx people were more vulnerable to hate crimes driven by emboldenment.

2 | INTERGROUP POWER CONTESTS—POWER THREAT THEORY

At a time of heightened racial discord in the United States, Blalock (1967) introduced what became known as "power-threat theory" in his book *Toward a Theory of Minority Group Relations*. Blalock characterized intergroup power contests by the shifts away from and toward a power equilibrium where the dominant group maintains greater power relative to the minority group (see also Blumer's 1958 explanation of group position). Although the theory is meant to be generalized to any set of groups where one dominates over the others, the book and subsequent writings on this topic were focused mostly on Anglo- and African-American majority and minority groups, respectively (e.g., Fossett & Kiecolt, 1989; Giles & Evans, 1986; Jacobs & Helms, 1999; Quillian, 1995, 1996; Taylor, 1998). Blalock (1967) argued that the dominant group in a particular setting acts to maintain a power equilibrium relative to a minority group, and that these power shifts are functions of increases in resources and mobilization efforts: "…resources seem to depend primarily on the motivation and goals of persons *over whom* power is exercised, whereas mobilization is more largely a function of the goals of the persons exerting the power" (Blalock, 1967, p. 114). In other words, as a minority group gains resources, the dominant group mobilizes its resources to reestablish its dominance within the power hierarchy (i.e., the political threat hypothesis). This dynamic has been documented by Wacquant (2006) and others, who have shown that Jim Crow laws supplanted slavery, and Western and Pettit (2010), who argued that mass incarceration of African Americans supplanted Jim Crow laws (see also Alexander, 2012). Van Dyke and Soule (2002) similarly showed how increases in perceived political, economic, and demographic threats to White supremacy led to increased numbers of White nationalist and White supremacist militias in the 1990s. Moreover, Vazquez (2010) argued that by incorporating immigration law into the U.S. criminal justice system, dominance was reestablished over Latinx people following major gains in labor and immigration rights.

Although the key variables used by scholars in this area are minority resources and dominant group mobilization, Blalock's (1967) conceptualization of relative power includes resources and mobilization for both groups. Of course, the underlying assumption here is that the equilibrium, as defined by the dominant group, is final. In actuality, systems that preserve one group's dominance over others are increasingly interpreted by some members of the public as inherently unjust and discriminatory, making them vulnerable to legal challenges and other forms of change over time (Epps, 1998).

These shifts in perspective tend to result from mobilization by marginalized groups. Such mobilization is epitomized by the abolition movement with regard to slavery; the civil rights movement in response to Jim Crow and racial segregation; and the women's and lesbian, gay, bisexual, transgendered, and queer (LGBTQ) movements in response to laws restricting the rights of women and sexual minorities (Van Dyke & Soule, 2002). Scholars have referred to periods of mobilization that result in major gains for aggrieved groups as the "rights revolutions" (Epps, 1998). We characterize minority group mobilization as a persistent effort to destabilize the dominant group's equilibrium of inequality.

Just as minority group mobilization is persistent, dominant group resources are plentiful as default narratives have historically favored members of the dominant group, which since at least the nineteenth century in the United States have been White, male, natively born, heterosexual Christians (Doane, 1997). But the dominant group is not monolithic. Instead, it is vulnerable to fragmentation as mobilization and resistance by marginalized groups begin to challenge the sustainability of the existing order (Epps, 1998). For example, Whites in the United States split over the abolition of slavery, resulting in a civil war between Union and Confederacy forces. A similar dynamic occurred in South Africa, where White business elites split over the continuance of the apartheid system (Charney, 1999; Fourie & Eloff, 2005). In the twenty-first century United States, immigration is a notably partisan issue, with Whites divided mainly along political party lines concerning appropriate approaches to immigration and immigrants (Pew Research Center, 2018).

We argue that the constant threat of fragmentation tends to tame overt expressions of hostility toward marginalized groups, as the dominant group publicly acclimatizes to a growing sympathy among the broader population. Despite having ample resources to reestablish dominance, threatened Whites feel regular pressure from minority group resistance and other Whites to accept new equilibria, tempering the capacity to mobilize all available resources to reestablish social dominance. It is under this tension that any overt hostile act by an elite member of the dominant group could motivate violence against marginalized groups (i.e., the emboldenment hypothesis).

In this article, we focus on Black and Latinx individuals in the United States. We argue that key metrics of federal power for any minority group include material changes implemented at the federal level, as well as public support expressed by federal officials—like the Voting Rights and Civil Rights Acts, which rendered Jim Crow laws illegal; or the DREAM Act, which expressed support for deportation immunity to immigrants who arrived in the United States as children. We argue that such changes only result from minority group mobilization and create new resources for ongoing mobilization efforts. For instance, pressure from the Civil Rights movement was mainly responsible for the eradication of legalized segregation and Jim Crow, even as key leadership of the movement negotiated with federal officials regarding the terms of the Civil Rights and Voting Rights Acts. And the Dreamer movement, which mobilized hundreds of thousands of undocumented immigrants to defer deportation and allow undocumented students to attend college in the United States, was responsible for the passage of Deferred Action for Childhood Arrivals (DACA) and for increasing support for immunity for illegal immigrants (Pew Research Center, 2018).

Yet, such accommodations can provoke backlash as well, as others fear disempowerment by the perceived power growth of minority groups. Bouie (2017) argued that expressions of political racism have been cyclical as African Americans experienced different levels of political gain and loss across different time periods. For instance, the landmark case *Brown v. Board of Education* (1954) ruled that racial segregation of children in schools was unconstitutional and mandated that they be integrated. Initially hailed as a major step toward justice, the ruling also fueled backlash from pro-segregationists, with some governors defying federal orders to integrate (Fobanjong, 2003). More recently, after the passage of DACA, the Obama administration ramped up deportation of Latin American immigrants, and the Trump campaign tapped into anti-immigrant sentiment by referring to Mexicans as “drug dealers,” “criminals,” and “rapists” and promised to build a border wall to end illegal immigration (Phillips, 2017), rhetoric that he has maintained as president.

Therefore, publicly expressed disdain toward minority groups by members of the federal government provides the dominant group a new resource that it can use in its efforts to return to a status quo equilibrium, including an expectation of impunity for crimes committed against the group.

Combined, we hypothesize that increases in expressed federal support for specific marginalized groups puts members of those groups at risk for violent hate crime victimization (*political threat*), and that increases in federal opposition to progress for specific marginalized groups also puts their members at risk of violent hate crimes (*emboldenment*). The following sections further delineate the causal mechanisms linking U.S. federal actions to violent hate crime for each hypothesis.

2.1 | Political threat hypothesis

Blalock (1967) explained that as minority resources increase and are mobilized, they increase political power for marginalized groups and thereby threaten the dominant group’s position of power. This leads the dominant group to mobilize, which in turn reestablishes the power balance preferred by the dominant group. In the same vein, we would expect that evidence of increases in minority mobilization would also raise concerns of possible lost relative power for the dominant group.

Empirical evidence has primarily supported this relationship through various operationalizations of minority resources and dominant group mobilization. A commonly used metric of minority resources is the percentage of the population that is Black, which has been found to be associated higher levels of anti-Black prejudice (Quillian, 1995; Taylor, 1998), support for harmful racial policies (Fossett & Kiecolt, 1989; Giles & Evans, 1986; Quillian, 1996), increase in Republican voter registrations in Louisiana (Giles & Hertz, 1994), votes for Wallace in 1968 (Wright, 1977), spending on corrections (Jacobs & Helms, 1999), spending on police (Jackson & Carroll, 1981), support for the death penalty (Baumer, Messner, & Rosenfeld, 2003), and support for other tough-on-crime policies (King & Wheelock, 2007). Of particular relevance to the current research is King's (2007) conclusion that law enforcement in jurisdictions with large Black populations is less likely to comply with federal hate crime law (i.e., classify crimes as hatefully biased). We will return to this observation later as it has implications for the dependent variable in this study.

Other scholars measure racial threat through mobilization, such as rioting or protest, and find it associated with spending on corrections (Jacobs & Helms, 1999), support for establishing "law and order" (Wasow, 2020), and police expenditures (Jackson & Carroll, 1981). Behrens, Uggen, and Manza (2003) measured minority threat by the percentage of the size of the prison population, based on the logic that dominant group mobilization is measured by felon disenfranchisement laws. In other words, as people leave prisons, they will be unable to vote in elections, which disproportionately diminishes Black political power because of the disproportionate representation of Black people in prison. Racial threat hypotheses have also been tested using a percentage of the population that is immigrant, finding an effect on the arrest rate for drunkenness (Brown & Warner, 1992) and prejudice against immigrants (Quillian, 1995). Jacobs and Carmichael (2001) found that Black and Hispanic population growth is associated with the growth of the prison population for each group. Finally, Green, Strolovitch, and Wong (1998) found that when minorities (Asians, Latinos, and African Americans) moved into New York neighborhoods with White strongholds, hate crimes against these groups increased. This last study most closely aligns with the current research as we also measure dominant group mobilization as a rise in hate crimes targeting specific groups.

2.2 | Emboldenment

To the best of our knowledge, no other scholarship has framed emboldenment within the context of power-threat theory or intergroup power contests. The long-running struggles between the dominant group and different racial, ethnic, religious, and sexual minorities in the United States, however, suggest that mobilization by marginalized groups deserves attention in any study of intergroup power contests. Although dominant group resources remain plentiful, they are also restrained by the risk that their overuse will draw backlash from other dominant group members, inadvertently empowering targeted minority groups (Dugan, Huang, LaFree, & McCauley, 2008). In other words, if dominant group members openly attack minorities, others might distance themselves from the dominant group and could even support the mobilization efforts by minorities. Therefore, dominant resources are more than just the ability to reestablish dominance, but they also entail the ability to do so without alienating other dominant group members.

Thus, continuous pressure of minority mobilization combined with the tempered dominant group resources creates a potential increase of dominant group mobilization when a revered member of the dominant group legitimizes minority group repression. Thus, the emboldenment hypothesis suggests that public action by an established authority against a specific minority group raises the level of dominant group resources enough for some members of the dominant group to counter-mobilize against persistent minority group mobilization.

Indeed, Müller and Schwarz (2018) found a rise in anti-Muslim hate crimes after President Trump's tweets on Islamic-related topics—tweets that have typically been hostile toward Muslims—suggesting that the violence was emboldened by Trump's rhetoric. Kalmoe (2014) tested this relationship more directly by exposing subjects to a political ad that conjured either violent metaphors or nonviolent synonyms. Those with aggressive traits were more likely to support political violence when exposed to the violent metaphors. This provides ostensible support for the emboldenment hypothesis for those members of the dominant group who might already feel heightening threats from marginalized groups. If true, the emboldenment hypothesis suggests that political elites have the power to exacerbate tensions across groups. In fact, Gagnon (1995) argued that elites are aware of their power and that they intentionally provoke violent conflict between the dominant group and others because such conflict tends to focus attention exclusively on ethnic identity and away from other politically charged topics that could compromise their power. This argument is also made by Tolnay and Beck (1995), who claimed that the White elites of the Antebellum South benefited from the antagonism between African Americans and poor Whites as it kept them from forming a coalition of laborers. By fostering this antagonism, White elites allowed White mob violence that then led to thousands of lynched African Americans. Indeed, the authors hypothesized that the eventual decline in U.S. lynchings was a result of the discouragement of the White elite, who were trying to entice low-wage Black laborers to remain in the South during the Great Migration (Tolnay & Beck, 1995).

In sum, we measure minority and dominant group resources as the number of pro- and anti-group actions by the U.S. federal government each month from 1992 through 2012, respectively. We focus this research on African Americans and Latinx persons because there was significant public attention to movements against racial discrimination and immigration policy, especially regarding immigration from Latin America, during this period.

3 | HYPOTHESES

The political threat hypothesis expects that as political elites take actions to further empower marginalized groups, violent hate crimes against them would increase. We generalize this approach by considering whether the growth in power of African American and Latinx populations elicits the same response from the dominant group. Combined, the following hypothesis comprise our expectations according to political threat theory.

Hypothesis 1. Federal government actions that *favor* Black or Latinx persons are related to *increases* in violent hate crimes targeting Black and Latinx persons, respectively.

The emboldenment hypothesis expects that as federal officials take actions that undermine marginalized groups, a subsequent increase in violent hate crimes against those groups follows. Once again, we generalize this approach by considering whether federal government actions opposing African American and Latinx persons elicits increases in hate crimes from the dominant group. Combined emboldenment leads to the following hypothesis:

Hypothesis 2. Federal government actions that *oppose* Black or Latinx persons are related to *increases* in violent hate crimes targeting Black and Latinx persons, respectively.

4 | DATA AND METHOD

4.1 | Data

The primary data come from two different sources that chronicle individual events and can therefore be aggregated to any temporal unit. The dependent variables measure violent hate crimes in the United States, which were compiled by the FBI's Uniform Crime Reporting (UCR) program for its Hate Crime Database under the Hate Crime Statistics Act of 1990 (HCSA). The primary independent variables come from the Government Actions in Terror Environments-United States (GATE-USA) data. All events are aggregated to the month and combined for analysis beginning in January 1992, the first month that the UCR hate crime data are available, and ending in December 2012, the last month that GATE-USA data are available, totaling 252 months. Other data come from public sources that provide the congressional sessions when each party held the majority in both houses, the number of tax-exempt civil rights or advocacy organizations for each year, and other UCR data. Also, supplemental analyses in appendix E in the online supporting information use data from the National Crime Victimization Survey (NCVS).[4]

4.1.1 | Violent hate crime rate

The HCSA of 1990 authorized the FBI's UCR program to collect data on "crimes that manifest evidence of prejudice based on race, religion, sexual orientation, or ethnicity" (U.S. Department of Justice, 2009, p. 4). Data collection follows a similar protocol as other UCR data sets, which relies on voluntary reporting of each law enforcement agency to a centralized state repository. However, instead of submitting reports every month, agencies either submit reports every quarter that list details on each incident through an incident report or submit a zero report verifying that no hate crimes were reported to the agency over the last quarter. NIBRS-compliant agencies can forgo these reports by simply marking an indicator that the documented event was a hate biased crime. Despite the similarities in reporting for crimes with and without hateful bias, without federal money allocated to help local agencies with their additional administrative and investigatory burdens, participating has been tenuous—especially in the beginning (McVeigh, Welch, & Bjarnason, 2003). In 1991, the population represented by participating agencies was less than 50 percent; however, that number rose to 85 percent by 2002 (King, 2007).

The absence of additional investigatory support is especially detrimental to compliance. To report a bias in the HCSA, law enforcement must investigate the reasons behind a crime rather than just uncovering the material actions that unfolded (Boyd, Berk, & Hamner, 1996). Without resources for specialized investigations, many departments simply report no hate crimes (Jenness & Grattet, 2001; McVeigh et al., 2003). Even when motives are apparent without additional inquiry, different officers interpret similar circumstances differently (Martin, 1995). Furthermore, the controversial nature of assigning bias to a perpetrator sometimes makes both victims and officers reluctant to report a crime as hate motivated (Cronin, McDevitt, Farrell, & Nolan, 2007; McVeigh et al., 2003). In fact, because hate crimes are often the result of intergroup conflict, labeling or even failing to label a crime as hate motivated can be interpreted as law enforcement choosing a side, possibly making them vulnerable to protest (Bell, 2002; McVeigh et al., 2003).

Concern about biases in the HCSA collection has led scholars to scrutinize the variation in hate crime reporting across jurisdictions. Hate crime data were found less likely to be collected in jurisdictions with larger Black populations (King, 2007) and in the South or Midwest (McVeigh et al., 2003). In fact, McVeigh et al. (2003) interpreted the counties with high numbers of hate crimes as those that have higher compliance (i.e., more support for minorities) rather than simply having more hate crimes. Following this logic, they argued that counties with successful social

[4] Additional supporting information can be found in the full text tab for this article in the Wiley Online Library at http://onlinelibrary.wiley.com/doi/10.1111/crim.2020.58.issue-4/issuetoc.

movements would have more advocates lobbying for bias crimes to be recorded in the HCSA. Furthermore, King (2007) used minority threat theory—the same theory used in this article—to argue that the more "threatening" jurisdictions are less likely to participate in HCSA. Indeed, he found that jurisdictions in places with large Black populations are less likely to comply with the hate crime law. Given these and other conclusions from years of scrutiny, variation in counts of U.S. hate crimes is likely driven by a mixture of actual events and the ability and willingness of victims and local agencies to report events as hate crimes.

Combined, these concerns raise considerable doubt about the validity of any research that relies on the HCSA data as a dependent variable, especially for time series beginning in 1992, like that which is used in the current research. We recognize this challenge and have included several features of this research that are designed to mitigate this problem. Additionally, we conduct supplementary analysis using victim reports of hate crimes and subsets of the HCSA agencies that participated for all or most of the years from 1992 through 2012.

First, the main analyses use data aggregated across all jurisdictions to the monthly unit, thus, absorbing all geographic variation and relying entirely on temporal variation in the violent hate crime rates for our estimations. By aggregating across jurisdictions, variation in compliance is less of a concern. This strategy would only mitigate all concerns, however, if compliance were constant over the two decades of analysis; and we know that compliance has increased over this period (Cronin et al., 2007; King, 2007; McVeigh et al., 2003). As crime rates are constructed by calculating the frequency of events in the numerator and the total population at risk in the denominator, erroneously assuming constant compliance over the series leads to systematic bias. To demonstrate, if the true rate was flat over the series, but compliance increased, then by using the total U.S. population in the denominator, the rate would appear to increase over the series. To reduce this type of bias, we generate violent hate crime rates using the populations represented only by those agencies that participated in the HCSA during the quarter associated with each month.

We identify those agencies that participate in the HCSA following the practice of King (2007) by using data from the batch header files to identify agencies that turned in either an incident or a zero report that quarter (UCR, 2017). These files also include the population represented by the reporting agencies, regardless of whether they participated.[5] We anticipate that this overall strategy improves the accuracy of the rates, yet we also recognize that they are still likely biased downward, as some agencies erroneously report no or low incidents of hate crimes, and therefore, the dependent variables are still measured with error (Cronin et al., 2007). Furthermore, by limiting the population represented in the denominator to only a subset of the total population, generalizability to the entire nation is compromised. We know that generalizability, however, is already compromised by using data that rely on selective compliance. By using the reporting agency population in the denominator, the variation over time in the newly constructed dependent variables more closely mimics the variation in the underlying actual violent hate crime rates than when we use the total population in the denominator. The results presented here use the agency population in the denominator. A second set of results, however, that uses the total population in the denominator is reported in appendix D in the online supporting information. The findings are nearly identical.

[5] Some participating agencies are recorded as having zero population even when their population group reports inhabitants (e.g., under 10,000). Therefore, when the agency population is listed as zero, but the population group indicates a positive population, the maximum population value was used as the population for that agency. Although this likely inflates the population to some degree, we view it as the more conservative strategy as it deflates the violent hate crime rate.

Furthermore, because of the unavoidable selective compliance, we dedicate appendix B in the online supporting information to assessing the spatial variation of compliant agencies across states. This analysis shows that the distribution of agency participation by state is uneven. For example, the largest proportion of participating agencies is in Texas, whereas agencies in Hawaii, Alaska, Georgia, and Alabama show the least participation.

Finally, three variables are added to the models to address potential bias in the estimates. We first include the total number of agencies (measured in the thousands) that submitted either an incident or a zero report that quarter. As the denominators of the dependent variables are calculated using only the population from these reporting agencies, that denominator is a function of the participating agencies. Furthermore, an agency's decision to participate in HCSA could be influenced by attention given to protected groups by the federal government, which would make the number of participating agencies an important control to mitigate possible bias. The second control variable is included because only those participating agencies that submit incident reports (rather than zero reports) can influence the numerator in the violent hate crime rates. Furthermore, it is unclear whether zero reports reflect true zeros or the agency's reluctance to report biased crimes, which could also be influenced by the tone set by the federal government. Thus, we include the percentage of participating agencies that submitted incident reports as a second control. Third, we follow the lead of McVeigh et al. (2003) by adding the number of civil rights organizations (measured in thousands) from the National Center for Charitable Statistics Core Files data sets for all years, which was calculated for each year using the same protocol as McVeigh and colleagues (NCCS, n.d.). This variable approximates efforts by civil rights organizations to lobby law enforcement to comply with the HCSA and report biased crimes as such so they can be prosecuted accordingly. Furthermore, because the number of civil rights organizations also approximates minority mobilization, this variable is also theoretically relevant to the hypotheses. Thus, we expect that some of its variation relates to hate crime reporting (McVeigh et al., 2003) and that some of its variation relates to hate crime perpetration as the number of civil rights organizations approximate minority group mobilization. In each case, we would anticipate that the number of civil rights organizations would be positively related to violent hate crimes.

Although we expect that these efforts improve the validity of the findings, we recognize that the data are inherently problematic as they systematically ignore hate crimes unreported and/or unrecognized by law enforcement. Furthermore, it is possible that victims' and agencies' motivations to report crime as biased are influenced by changes in the federal attention paid to specific groups, above and beyond the controls described earlier. If local agencies are reluctant to report crimes as hatefully biased when the federal government is giving the targeted group either positive or negative attention, then our results will be biased downward.

Unfortunately, these data are the best available for our purposes. The National Crime Victimization Survey (NCVS), however, did begin collecting data on hate crimes as reported by the victims in 2003. Unsurprisingly, the estimated number of hate crimes generated from victim reports is substantially higher than that reported by police agencies. In fact, between 2005 and 2015, the NCVS estimates around 250,000 hate crimes each year, whereas the HCSA only reported a little more than 7,000 biased crimes in 2015 (Office of Justice Programs, 2018). Although the estimates from the NCVS data are undoubtedly closer to the actual numbers, limitations in the NCVS data set preclude it from being used in the current analysis. For example, because it relies on sampling, not all months have an episode of violent hate crimes against Black or Latinx persons. This generates many missing values at the monthly level. Also, the rates will exhibit high variation, making estimation at that level difficult. Because the NCVS and the HCSA are both measuring the same underlying dynamic, however, a comparison between the two might help validate the HCSA data.

Supplemental analyses in appendix E in the online supporting information describe in detail the similarities and differences between the two data sources, estimates their trend and first differenced correlation, and re-estimates the main models with the HCSA data, the NCVS hate events known to the police, and the NCVS hate events unknown to the police.

Finally, as another check on the validity of these data, we repeat the main analyses using rates generated from only the 741 agencies that participated for all four quarters in all 21 years in the series and the 1,973 agencies that participated in all four quarters for 20 of the 21 years. As a point of contrast, the main analyses rely on data generated from 22,213 different agencies that participated in at least one quarter during the 21 years. Violent hate crime trends against Black and Latinx persons from all three samples are compared prior to analyses. Appendix B in the online supporting information describes the spatial distribution of those agencies in both subsets, as well as in the main dataset.

To test the hypotheses, we rely on the details provided in the HCSA data. Each event includes information on the race of the offender (if known), type of victims, bias motivation, offense type, and location type. Because we are only interested in violent hate crimes as defined by the FBI, we retain only murder, non-negligible manslaughter, negligible manslaughter, kidnapping or abduction, forcible rape, forcible sodomy, sexual assault with an object, forcible fondling, robbery, aggravated assault, simple assault, and intimidation. Only those hate crimes that were perpetrated by White or unknown offenders were retained as they are members of the dominant group. Finally, only crimes with biases that were anti-Black, anti-multiracial, and anti-Hispanic were retained for the specific analyses. After applying these criteria, we retain 39,599 violent hate crimes against Black people (including anti-multiracial), aggregate them to the month, and calculate the rates per 1,000,000 persons living in participating jurisdictions. Data used for the Latinx models include only violent anti-Hispanic hate crimes resulting in 8,004 violent crimes to calculate monthly rates per 1,000,000 persons. We use natural logarithms of these rates to improve normality.

4.1.2 | Federal government actions relevant to specific groups

These data come from a larger GATE project that documents all actions by the federal U.S. government that are relevant to political extremists and their and their constituencies' grievances (e.g., the far-right; see Dugan & Chenoweth, 2017). The original source of the GATE-USA data collection is all Reuters news articles that mention key federal U.S. government actors between 1987 and 2012 (see Dugan & Chenoweth, 2017, for the complete list of actors). Lead sentences were extracted and coded using Textual Analysis by Augmented Replacement Instructions (TABARI), which searches them and identifies stories that match the criteria of an extensive set of dictionaries designed to capture political activity (Schrodt, 2012) and supplemented with names of known extremists in the United States (Freilich, Chermak, Belli, Gruenewald, & Parkin, 2014; Smith & Damphousse, 2001; START, 2016). Final cases were coded by research assistants and cleaned by the investigators. As such, each action was reviewed by at least two persons, one of which was a principal investigator to assure consistency across all cases.[6]

[6] Agreement statistics were not calculated because disagreement in earlier parts of the coding protocol led to greater disagreement elsewhere. To assure validity and reliability, the coders and investigators had weekly meetings throughout the project to discuss ambiguous cases. All coding decisions were recorded and applied across all cases.

For the current project, we kept only actions—which include both speech and material acts—by U.S. federal actors that were relevant to Black and/or Latinx persons (including all immigration -relevant actions) that occurred between January 1992 and December 2012. These cases were collected in the original GATE data because of their relevance to grievances by far-right extremists. The final data includes 148 non-neutral actions that were relevant to Black people; 116 that politically benefited them (e.g., prosecuting White supremacists and promoting racial equality) and 32 that disadvantaged them (e.g., challenging Affirmative Action). We also selected only those events that were relevant to Latinx persons specifically or immigration more generally, resulting in 220 non-neutral actions with 109 supporting Latinx or immigration rights and 111 against such rights. We retained immigration-relevant actions that were specific to non-Latinx persons because those events are set within the larger context of immigration, as the United States public tends to conflate immigration policy with Latinx-specific immigration (Lopez, Morin, & Taylor, 2010).[7] We aggregated all events to the month and according to whether they favored or disadvantaged the protected groups and used their square roots for analysis to mitigate non-normality.

When aggregated to the month, the GATE data are intended to capture the amount of positive and negative attention given to specific marginalized groups. This differs from efforts to delineate relevant legislation because it relates more to the public conversation than to the legislative process. Nevertheless, legislation is captured indirectly because more attention is given to the group during periods when relevant legislation is introduced and voted on (e.g., during efforts to remove Affirmative Action with the introduction of the 1995 Equal Opportunity Act into the U.S. Senate). Also, GATE captures some of the legislative process, as coders were directed to capture all events that reported 1) the promotion of a policy idea prior to proposal, 2) a Bill proposal, 3) a congressional subcommittee vote, 4) a vote by the full House or Senate, 5) the signing of a Bill, 6) the veto of a Bill, 7) the override of a veto, 8) an executive order, or 9) responses to a new policy, law, or act. Appendix A in the online supporting information describes the key patterns of federal attention given to each group in the GATE data.

4.1.3 | Sources for other key variables

As mentioned earlier, the number of civil rights organizations came from The National Center for Charitable Statistics (NCCS, n.d.), which derives its files from databases maintained by the Internal Revenue Service (IRS) on tax-exempt nonprofit organizations. The number of organizations from the NCCS core files with National Taxonomy of Exempt Entities (NTEE) core code classifications that indicate civil rights or advocacy organizations (codes R20 through R30). Because these data are only measured once each year, all tallies for months in the same year are the same, consequently deflating estimated standard errors and risking false significance. In fact, this variable only has 21 unique values out of the total number of 252 months. For this reason, we consider the estimated effects civil rights organizations to be informative rather than definitive.

We also include controls for whether the White House, House of Representatives, and Senate are all controlled by the Democrats or Republicans as the political climate likely affects both government actions and violent hate crimes. Data on the composition of Senate and House of

[7] Nevertheless, earlier models that used an immigration variable that excluded actions that were specific to non-Latinx persons produced substantively similar findings, although the broader measure was more strongly related to violence against Latinx persons specifically.

TABLE 1 All variables included in model with their operationalizations

Variables	Operationalization
Dependent Variables	
Ln Violent Hate	$\ln\left(\frac{\text{freq of violent bias crimes}}{\text{n persons living under reporting agency}} \times 1{,}000{,}000\right)$
Government Actions	
Sq. rt. Pro-Group GA	The square root of the number of U.S. federal government actions that give *positive* attention to specific group in current month
Sq. rt. Anti-Group GA	The square root of the number of U.S. federal government actions that give *negative* attention to specific group in current month
Control Variables	
Civil Rights Groups	The number of civil rights or advocacy groups, measured in thousands, registered with the IRS as tax exempt
Democratic Controlled	A dummy variable (1/0) indicating months when the presidency, Senate and House of Representatives are all controlled by the Democratic party
Republican Controlled	A dummy variable (1/0) indicating months when the presidency, Senate and House of Representatives are all controlled by the Republican party
Violent Crime Rate	$\frac{\text{freq of violent crimes}}{\text{n persons living in US}} \times 1{,}000{,}000$
Total Participating Agencies (PA)	The total number of UCR participating agencies, measured in thousands, that submitted either an HCSA incident or zero report to the FBI in the quarter associated with the current month
Percent PA with Incid Reports	The percent of total participating agencies that submitted an incident report (rather than a zero report) in the quarter associated with the current month
January—November	Dummy indicators (1/0) for each month from January through November, using December as the reference month

Representative members were compiled from several sources accessed in the fall of 2017.[8] Furthermore, because violent crime rates are likely to be driven by violent crime in general, we control for the monthly violent crime rate using the FBI's UCR data. Finally, to absorb variation caused by seasonal affects, we include dummy variables for each month using December as reference. See table 1 for a list of all variables and their operationalization.

4.2 | Methods

Because of the concern for how representative the HCSA data are, we begin the analysis by examining the distribution of participating agencies by state. We do this for the entire data set over the 21 years, and then we examine subsets of agencies that more fully participated over the entire period.

[8] These include https://www.senate.gov/history/partydiv.htm; https://www.senate.gov/artandhistory/history/common/briefing/senators_changed_parties.htm; https://www.brookings.edu/multi-chapter-report/vital-statistics-on-congress/; http://history.house.gov/Institution/Party-Divisions/Party-Divisions/; http://history.house.gov/Congressional-Overview/Profiles/1st/; and https://www.brookings.edu/multi-chapter-report/vital-statistics-on-congress/.

We next compare the yearly trends for different measures of violent hate crime rates against Black and Latinx persons to assess which rates better measure the underlying pattern of violent hate crimes in the United States. We then compare the yearly trends of the two types of government actions with their corresponding violent hate crime rate. In this section, we also summarize all key variables to show the average level of each over the entire period.

To test our hypotheses, we combine the monthly data from each source that are specific to each protected group to run two distinct models. Monthly units are used to capture close to real-time reactions to government action, while reducing sparseness in the data. Close to real-time reactions are preferred as hate crime offenders are often impulsive (Levin & McDevitt, 2002). Also, King and Sutton (2013), who relied on daily time series of hate crime events following antecedent events, discovered that hate crime spikes a few days after each event and then decays over the next several days. As the current research assesses the effects of cumulative lower level events that we characterize as positive or negative attention, as opposed to single antecedent events (e.g., the O.J. Simpson verdict), we anticipate that their effects on hate crimes will still be detectable during the following month as attention is ongoing.

Furthermore, we recognize that government actions could also be influenced by preceding violent hate crimes, which could, in turn, bias our findings through simultaneity. In fact, the GATE-USA data show that after the burning of several Black churches, President Clinton began a national campaign to improve race relations, demonstrating that violent hate crimes affect government actions relevant to the victims of those crimes. Therefore, to assure that the specified results estimate the effects of government actions on violent hate crimes, and are not biased as a result of this type of endogeneity, we conduct three-equation reduced form vector autoregression (VAR) analyses with exogenous control variables (shown in equation 1) for Black and Latinx persons using monthly data. We see that in these series of equations, VAR simultaneously estimates the effects of lagged pro-group government actions (*PGAs*) and lagged anti-group government actions (*AGAs*) on the violent hate crime rate against the specified group (*VHCR*) while estimating the effects of the same series of lagged variables on each type of government action. In these series, **X** includes the control variables listed in table 1, the number of civil rights groups, indicators of whether the federal government was controlled by the Democrats or Republicans, the violent crime rate, the total number of participating agencies that quarter, the percentage of participating agencies that report at least one incident that quarter, and indicators for each month, excluding December. Granger causality tests are conducted to assess the directionality of the relationship, and impulse response functions (IRFs) are generated to show how the VHCR responds to the impulses of *PGA* and *AGA*, as well as how *PGA* and *AGA* respond to the impulse of the *VHCR*.

$$\begin{aligned}
VHCR_t &= \sum_{j=1}^{J} \beta_{1j} VHCR_{t-j} + \sum_{j=1}^{J} \beta_{2j} PGA_{t-j} + \sum_{j=1}^{J} \beta_{3j} AGA_{t-j} + \boldsymbol{\beta_4} \mathbf{X} \\
PGA_t &= \sum_{j=1}^{J} \alpha_{1j} PGA_{t-j} + \sum_{j=1}^{J} \alpha_{2j} VHCR_{t-j} + \sum_{j=1}^{J} \alpha_{3j} AGA_{t-j} + \boldsymbol{\alpha_4} \mathbf{X} \\
AGA_t &= \sum_{j=1}^{J} \gamma_{1j} AGA_{t-j} + \sum_{j=1}^{J} \gamma_{2j} VHCR_{t-j} + \sum_{j=1}^{J} \gamma_{3j} PGA_{t-j} + \boldsymbol{\gamma_4} \mathbf{X}
\end{aligned} \tag{1}$$

Because the *VHCR*s were generated from a changing mixture of agencies that participated in the HCSA, we replicate the analyses shown in equation 1 using only those agencies that participated in all four quarters for all 21 years and for at least 20 of the 21 years.

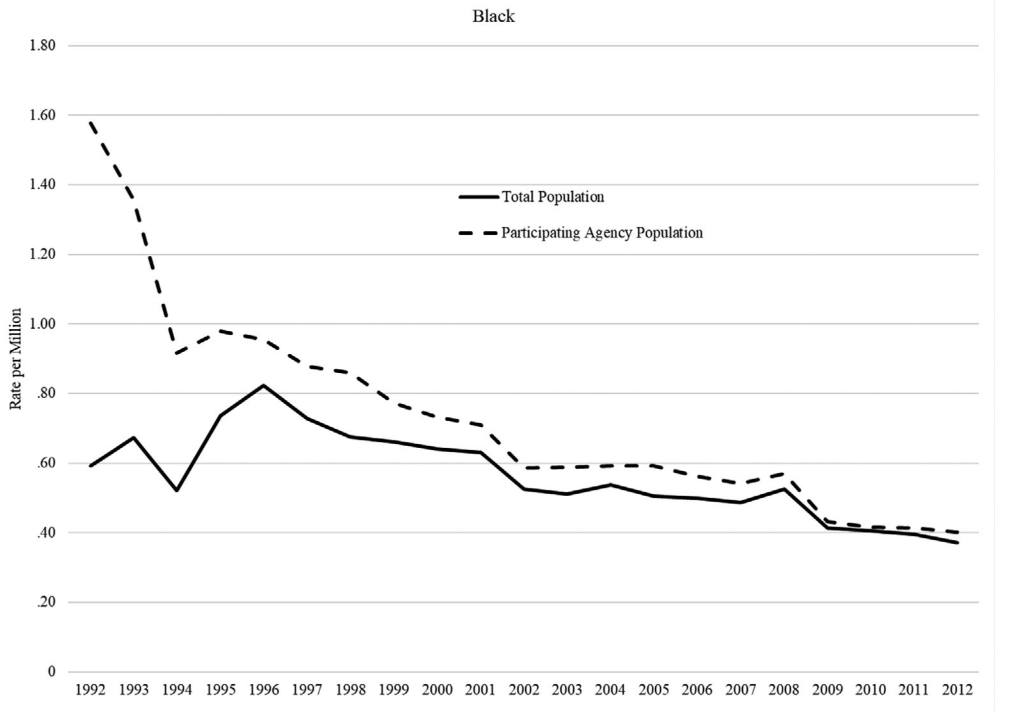
Additional sensitivity analyses are conducted and reported in detail in appendix D in the online supporting information to further assess whether the findings hold if we use the entire U.S. population in the denominator instead of only counting those persons represented by the participating agencies. Also, to better validate the HCSA, we construct total hate crime data from the HCSA and the NCVS and compare the rates from the HCSA with those in the NCVS that were known and unknown to the police in appendix E in the online supporting information. We also run the VAR models shown in equation 1 and compare the IRFs across all three models. We expect that the results generated from the HCSA will be more similar to the NCVS police known hate crimes than to the police unknown hate crimes.

5 | RESULTS

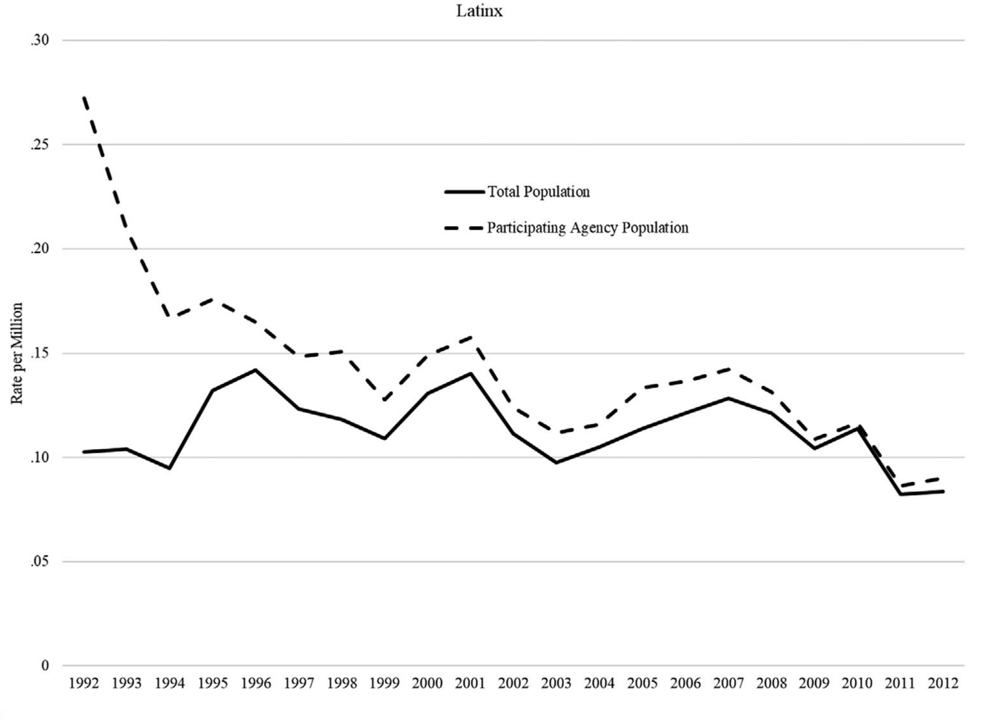
Before turning to our analysis, we assess how well the distribution of participating agencies represents the entire nation. Detailed analyses are found in appendix B in the online supporting information. The UCR HCSA files used here include information from 22,213 law enforcement agencies from 1992 to 2012. Only 3.3 percent of those agencies (743) participated (i.e., submitted a report) for every quarter during the 21-year period. A larger subset, 8.9 percent or 1,972 agencies, participated in 20 of the 21 years. The states that have the highest average number of participating years per agency are Texas (17.2), Iowa (16.2), and New York (15.9), whereas agencies in Hawaii (0), Alaska (.47), and Georgia (1.95) have the lowest participation. The rates generated from the subsets of participating agencies are dominated by Texas agencies, which account for 36 percent of those that participated for at least 20 years and 66 percent of those that participated for all 21. The main analyses are replicated below using violent hate crime rates generated from these two data sets to assess the robustness of the findings.

Figure 1 presents the annual trends of violent hate crime rates against Black (a) and Latinx (b) persons with both the total population and the participating agency population used in the denominators. In each case, the rate using the limited population is larger by a factor that peaks at 2.66 in 1992 and is as small as 1.02 in 2010 for both groups. The dashed line in both cases shows a nearly monotonic decreasing trend from 1992 to 2012. Yet, when we look at the rates that use the entire population in the denominator, we see that they both increase in the beginning of the series, peak in 1996, and then follow a similar trend as the one that uses the limited population in the denominator. The divergence in the beginning of the series mimics what would be expected if agency compliance started low and then grew. The convergence later in the series suggests more of an equilibrium, indicating that the findings generated by each rate might converge. All forthcoming analyses use the rates generated from the agency populations in the denominators.

Figures 2a and 2b shows the yearly trends for the specific types of government actions and their corresponding violent hate crime (*VHCR*) rate per million using the agency populations as denominators. The *VHCR* is measured on the left axis, and the total number of monthly government actions is measured on the right axis. A comparison of government actions shows that that U.S. federal government acted more frequently in support of and against Latinx persons or immigration compared with when African American issues were addressed. More often than not, when federal officials directly addressed issues relevant to Black persons during this period, they did so in a way that supported them. The left axes show that the peak in the *VHCR* against



a).



b).

FIGURE 1 Violent hate crime rate per protected group using different populations in the denominator ($n = 21$)

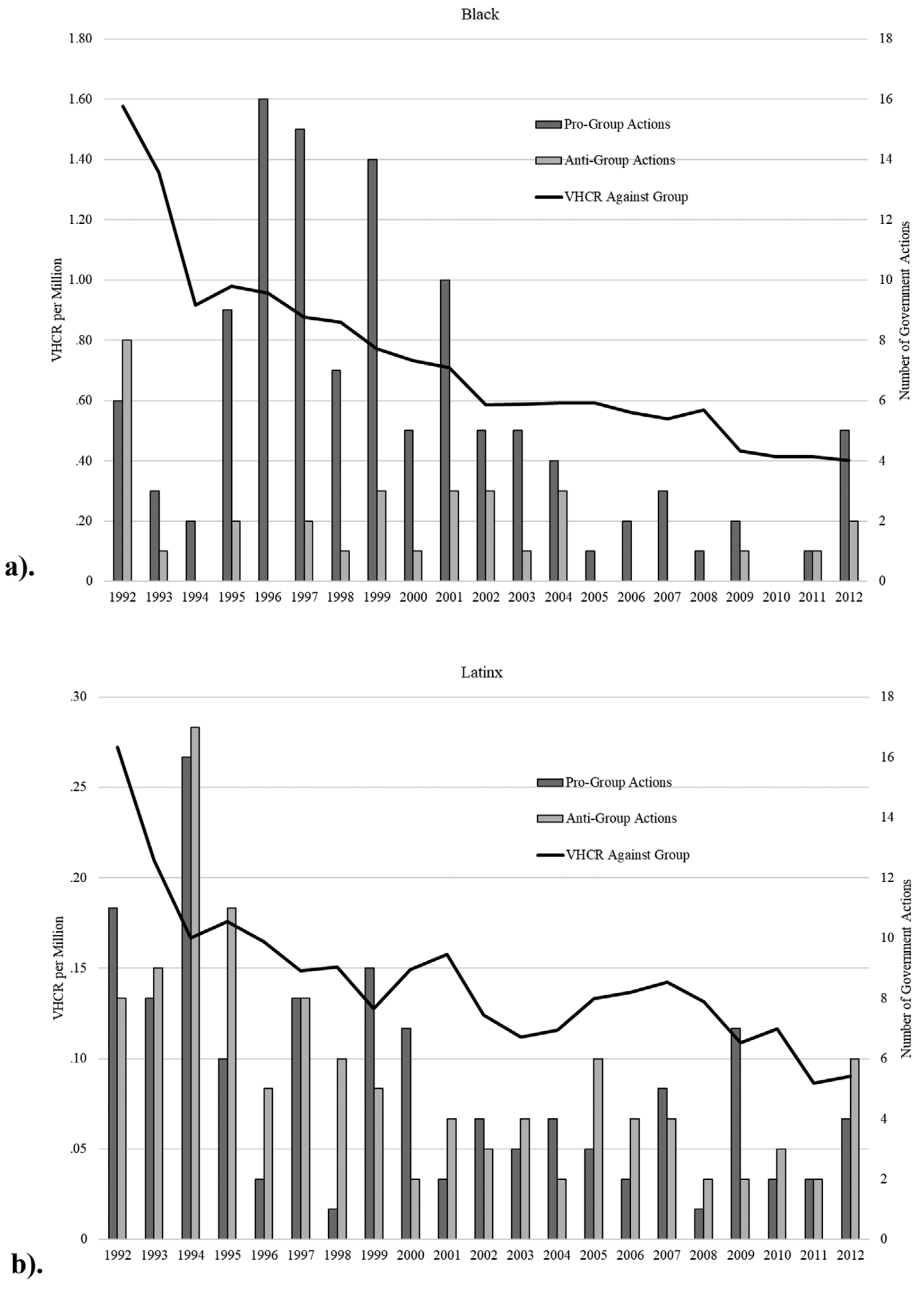


FIGURE 2 Government actions and VHCRs relevant to each group ($n = 21$)

TABLE 2 Descriptive statistics for key variables (n = 252)

Variables	Mean	St. Dev	Min	Max	*p (zero)*
Anti-Black VHCR	.735	.324	.267	.122	.000
Anti-Latinx VHCR	.144	.053	.035	.328	.000
Pro-Black GA	.460	.898	.000	6.000	.702
Anti-Black GA	.127	.480	.000	6.000	.893
Pro-Latinx/Immigrant GA	.425	.787	.000	5.000	.694
Anti-Latinx/Immigrant GA	.448	.758	.000	4.000	.663

Black people is nearly six times higher than that for Latinx persons. In general, the trends show a greater number of relevant federal actions in the earlier years, which seems to correspond to the downward trends of the *VHCR*. The first 2 to 3 years of the series, however, show lower numbers of government actions.

Next, we summarize the number of government actions and violent hate crimes relevant to Black and Latinx persons.

5.1 | Summary statistics

Table 2 lists the descriptive statistics for the key dependent and independent variables. Turning first to the dependent variables, the average *VHCR* is highest against Black people (.74 per million), whereas that for Latinx persons averages about .14 per million.

The average number of government actions (*GA*) relevant to these groups is less than one per month. Furthermore, the government more often gives positive attention to these groups than negative attention. We see that most actions are relevant to Latinx persons or immigration, with nearly the same average number of actions each month (.43 favor and .45 against). On average there were .46 favorable federal actions per month for African Americans (or one every 2.17 months), and adverse actions once every 7.7 months. The last column shows the proportion of months where no government actions were reported for each protected group. Here we see that in most months, members of the U.S. federal government engage in no actions against these groups. Furthermore, federal officials expressed or implemented favorable actions toward Black and Latinx persons or immigrants in about 30 percent of the months.

5.2 | Vector autoregression models

Dickey-Fuller tests that allow four lags find that all six variables used in the VAR regression are stationary. The test for the VHCR for Black persons, however, is only significant at the .10 level.[9,10] The optimal number of lag lengths is selected by running the VAR models with different lag lengths and comparing the likelihood ratio test statistic, the Akaike's information criterion, and

[9] Because of this and the downward annual trends shown in figure 2, additional tests were run on the two VHCRs. A modified Dickey-Fuller test that applies a GLS transformation shows that both series are stationary in early lags but non-stationary with later lags. The Phillips-Perron test concludes that both are stationary.

[10] As a test for robustness, the primary VAR model was rerun using the first differenced anti-Black VHCR; the results remained substantively similar.

TABLE 3 Granger causality wald tests (n = 250)

	Black	Latinx
VHCR		
Pro-Group Action	21.313**	.871
Anti-Group Action	7.112**	5.980*
Pro-Group Action		
VHCR	3.077	2.955
Anti-Group Action	4.056	.968
Anti-Group Action		
VHCR	1.433	.330
Pro-Group Action	4.191	4.376

*$p \leq .05$; **$p \leq .01$.

the Hannan and Quinn's information criterion. For both groups, the two-lagged model is selected. Tests on the residuals show no autocorrelation, confirming the adequacy of the choice of the two-lagged model for each group. Finally, all eigenvalues generated from the companion matrices fall within the unit root, allowing us to conclude that both VAR models are stationary. Tests for normality show that both *VHCR*s are normally distributed but the government actions are not. This is unsurprising, given the high proportion of zeros and the square-rooted counts. Findings that use the government actions as the response variable will be interpreted with caution. Their results, however, do correspond to unpublished structural equation models of a similar specification that were estimated using the negative binomial distribution.

Table 3 reports the Wald chi-square statistics for tests of Granger causality in both VAR models. Here we find that only the hypothesized relationships show any evidence of Granger causality. Government actions seem to influence the *VHCR*, but the *VHCR* does not seem to influence government actions. The tests indicate that both pro- and anti-African-American actions relate to the *VHCR* against Black people, whereas only anti-immigration/Latinx actions relate to the *VHCR* against Latinx persons. When we consider these results within the context of the hypotheses, Black people might be vulnerable to both political threat and emboldenment and Latinx persons might only be vulnerable to emboldenment. As the Granger causality tests cannot speak to directionality, we turn now to the IRFs to more specifically assess the hypotheses.

Figures 3 and 4 show the IRFs (and their 95 percent confidence bounds) of anti- and pro-group government actions as the impulse on *VHCR* responses (a) and the inverse (b; *VHCR* as the impulse on government actions responses) for Black and Latinx persons, respectively. In figure 3a, we see that only those acts that give positive attention to African Americans appear to influence the *VHCR*, despite both types of actions showing Granger causality in table 3. This supports the political threat hypothesis but not the emboldenment hypothesis. The IRF peaks at .03 at the first month after a 1 standard deviation shock in pro-African American actions. It then drops to insignificance in the second month, indicating only a short-term effect. Figure 3b shows that the *VHCR* against Black people is positively related to pro-African American actions by the federal government, suggesting a simultaneous relationship, despite the Granger results reported above. Here we see that the effect is only significant in the same month as the impulse with a magnitude of .08. Forecast error variance decompositions (FEVD) show that pro-African American government actions account for approximately 8.4 percent of the variation in the *VHCR* against Black people.

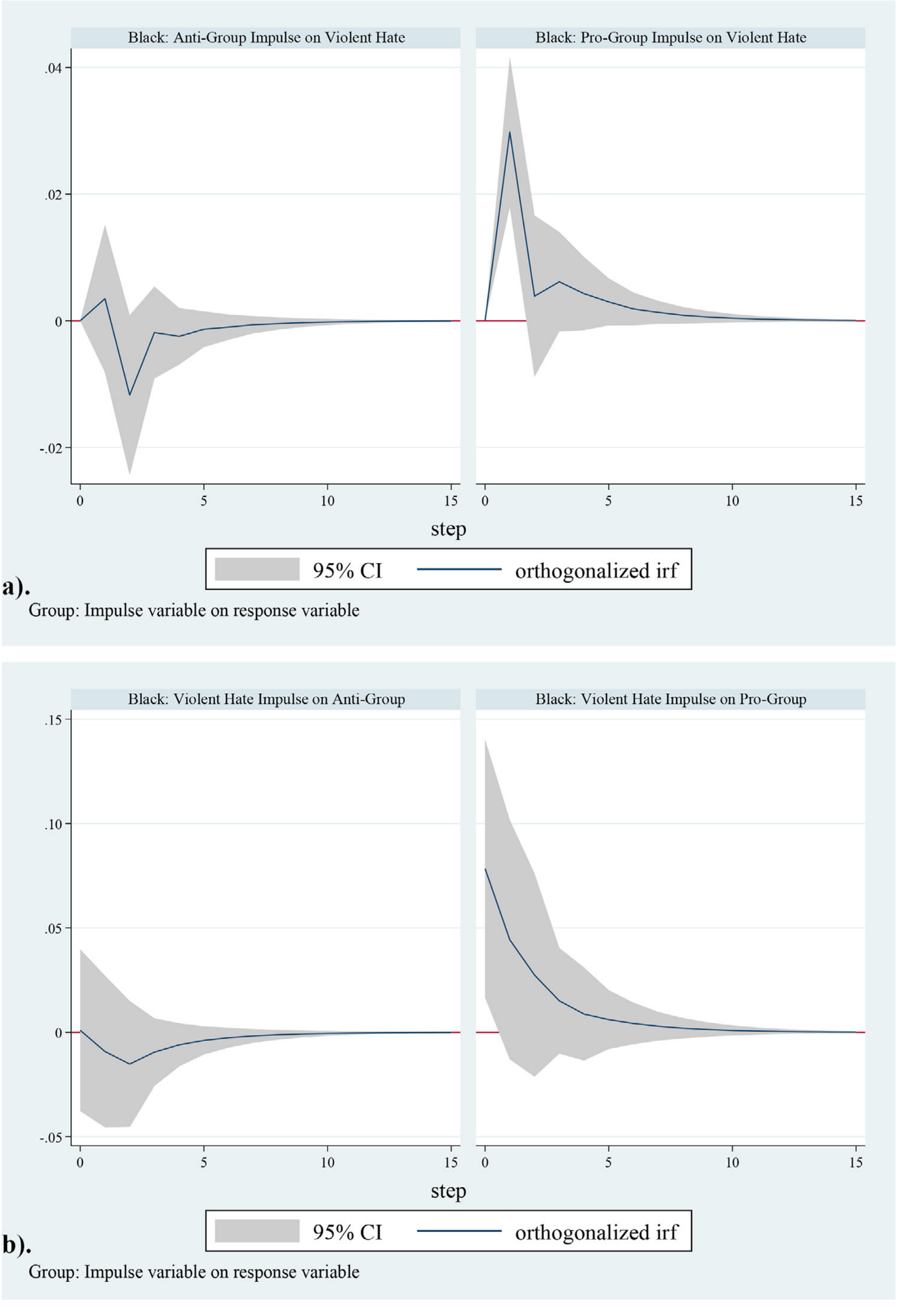


FIGURE 3 Impulse Response Functions for Black VAR (n = 250) [Color figure can be viewed at wileyonlinelibrary.com]
a). Action as Impulse on Violent Hate Crime Response
b). Violent Hate Crime as Impulse on Action Response

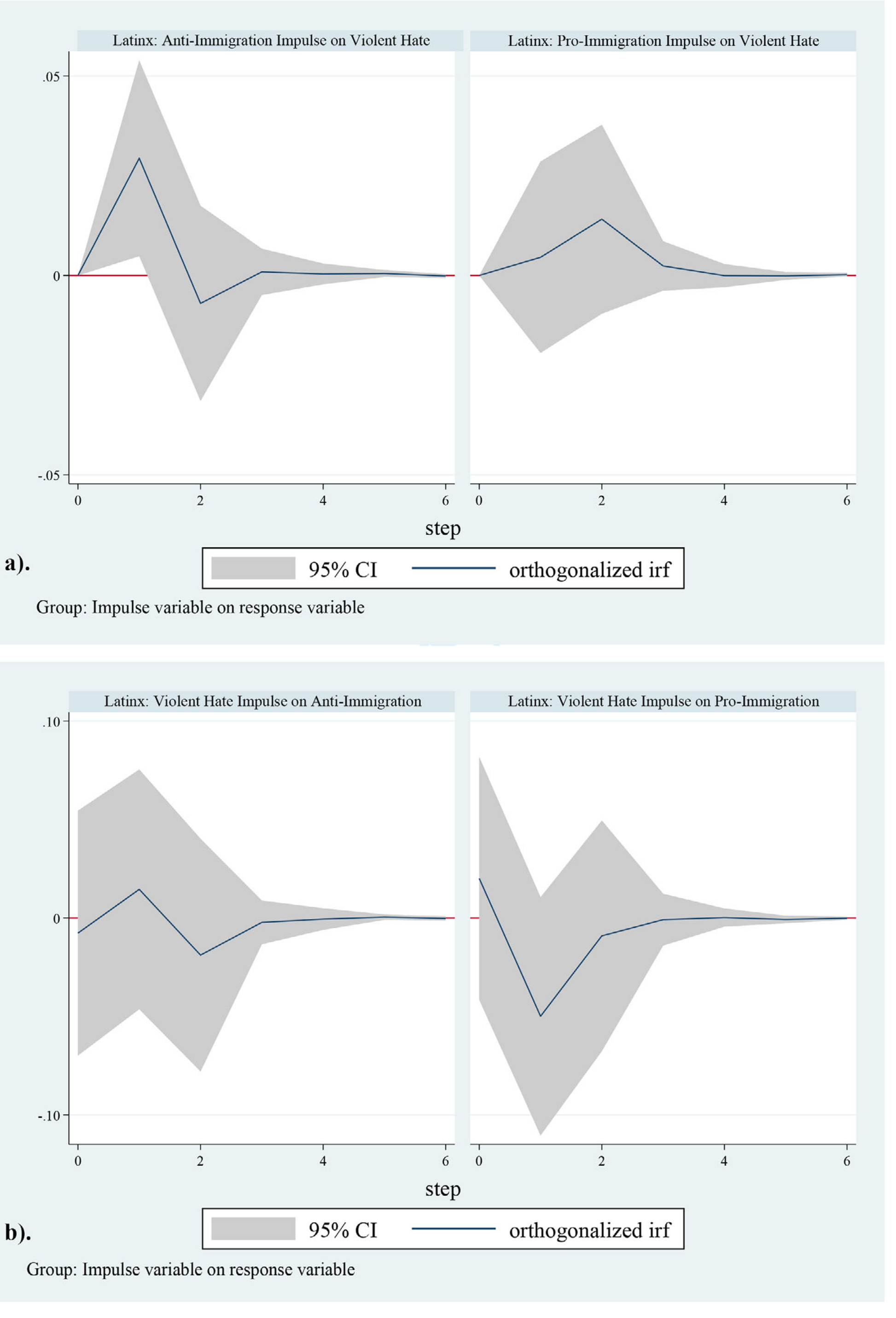


FIGURE 4 Impulse Response Functions for Latinx VAR (n = 250) [Color figure can be viewed at wileyonlinelibrary.com]
a). Action as Impulse on Violent Hate Crime Response
b). Violent Hate Crime as Impulse on Action Response

TABLE 4 VAR coefficients and standard errors of exogenous variables estimating effects on violent hate crime rates for Black and Latinx persons (n = 250)

	Black		Latinx	
Variables	**Coef**	**SE**	**Coef**	**SE**
Civil Rights Groups	−.120**	(.040)	−.101	(.073)
Democratic Controlled	−.047**	(.018)	−.011	(.038)
Republican Controlled	−.046**	(.018)	−.101**	(.037)
Violent Crime Rate	.134**	(.026)	.206**	(.051)
Total Participating Agencies	−.020**	(.008)	.023	(.016)
Percent Agencies w/Incids	.032**	(.010)	.107**	(.021)
January	.340**	(.031)	.347**	(.065)
February	.379**	(.039)	.276**	(.074)
March	.458**	(.032)	.472**	(.063)
April	.391**	(.036)	.429**	(.069)
May	.358**	(.032)	.442**	(.065)
June	.324**	(.034)	.394**	(.067)
July	.322**	(.032)	.315**	(.066)
August	.352**	(.033)	.332**	(.064)
September	.370**	(.033)	.442**	(.065)
October	.356**	(.034)	.372**	(.066)
November	.163**	(.037)	.234	(.072)

*$p \leq .05$; **$p \leq .01$ (two-tailed).

The IRFs shown in figure 4a suggest that Latinx persons are vulnerable to emboldenment rather than to political threat, as the anti-immigration function spikes in the left graph, whereas the other never clears zero. Furthermore, we find no evidence of simultaneity in figure 4b. The magnitude of a 1 standard deviation shock of anti-Latinx government actions also peaks at .03 in the month following the shock, and then it falls to insignificance, suggesting only a short-term effect. FEVD shows that this only accounts for 2.1 percent of the variance of the *VHCR* against Latinx persons.

Table 4 presents the coefficient estimates of the control variables on the *VHCR* for each group. The coefficient estimates of the control variables on the two types of government actions are reported in appendix C in the online supporting information. Here we find that the number of civil rights groups is negatively, *not positively*, related to violent hate crimes for Black people. This is surprising because both political threat and research by McVeigh et al. (2003) predict that more civil rights groups would either lead to more violence (political threat) or more reporting of hate crimes (advocacy). The negative effects suggest that the growth in civil rights groups are related to fewer violent hate crimes perhaps because they are effectively changing attitudes or preventing harmful altercations. Although the difference between this finding and McVeigh et al.'s is somewhat puzzling, the sources of variation in the two estimates differ substantially. Their analysis relied on cross-county variation within 1 year (2000), and the variation that contributed to this estimate is the yearly changes in civil rights groups over 21 years. Furthermore, this estimate might have been influenced by the changes in agency participation in the HCSA over time. Given that the different conclusions are based on different sources of the variation and that the current estimate is based on only 21 unique values over the entire series, more research is needed to assess the impact of civil rights groups on the reporting of hate crimes.

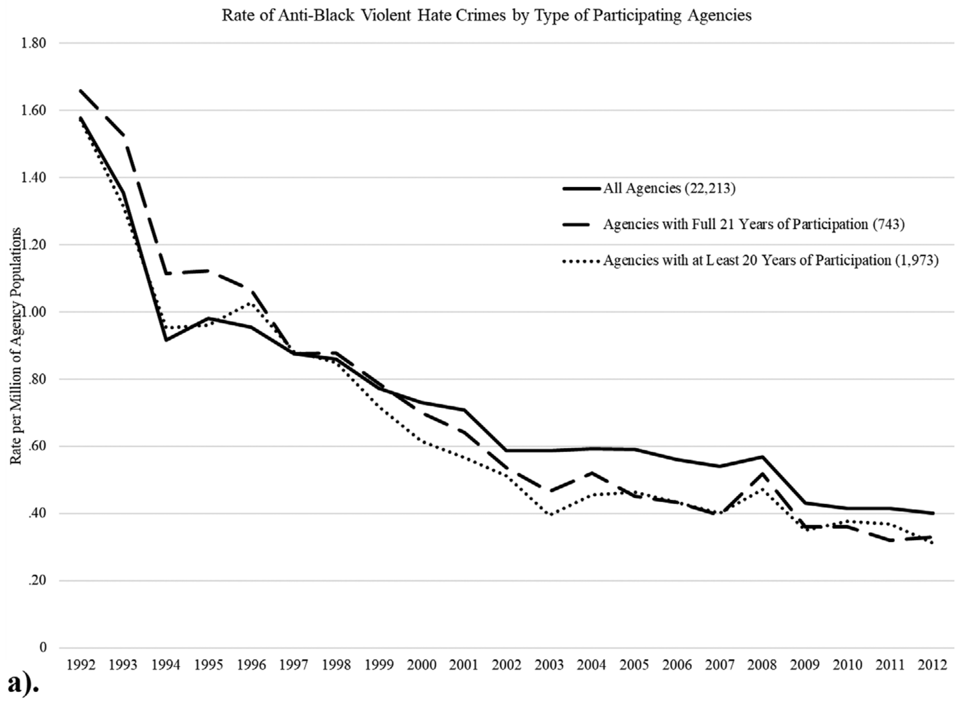
When the federal government was entirely controlled by Democrats, violent hate crimes against Black people were lower; and when Republicans were in control, violent hate crimes against both Black and Latinx persons were lower. This suggests that motivations to attack might be muted when the government is operating more smoothly, which is aligned with Young and Dugan's (2011) argument that when governments have fewer veto players, domestic terrorists perpetrate fewer attacks. Unsurprisingly, the violent hate crime rate is positively and significantly associated with the *VHCR* against Black and Latinx persons. The total number of participating agencies is included because we would expect that the more agencies that submit reports to the HCSA program, the more violent hate crimes will be reported; however, this measure is negatively related to violence against Black people and unrelated to violence against Latinx persons. As expected, we see that the percentage of participating agencies that submitted an incident report is positively related to violent hate crimes against both groups. Finally, all months are statistically significant and positive, suggesting that violent hate crimes are less common in December than in other months.

In sum, examining different patterns in hate crimes against marginalized groups, we find strong support for political threat theory for anti-Black hate crimes. We find no support for political threat theory for Latinx persons, however, and instead find evidence of emboldenment.

To assess the robustness of these findings to the distribution of participating agencies, we replicate the analysis using hate crime rates generated from only those agencies that participated consistently. First we compare the trends across samples. Figure 5 shows the *VHCR* rates against (a) Black people and (b) Latinx persons across all three sets of participating agencies. The trends appear remarkably close, especially for the anti-Black *VHCRs*. The monthly trend correlations between all agencies and the 20+-year agencies are .94 for anti-Black *VHCRs* and .79 for anti-Latinx *VHCRs*; and for the 21-year agencies, .89 and .44, respectively. The corresponding first differenced correlations are .65 and .38 for the anti-Black *VHCRs* and .58 and .35 for the anti-Latinx *VHCR*s.

The IRFs showing the impulse of anti- and pro-group actions on the *VHCR* response generated from the two subsets of participating agencies are presented in figure 6. Figures 6a and 6b show that the support for the political threat hypothesis in the African American model is robust to the composition of the participating agencies as the pro-Black IRF and its confidence bounds clear the zero line in both models with peaks that are slightly higher than that in the original model (.036 and .037). Figures 6c and 6d show that the emboldenment hypothesis in the Latinx model is also still supported in these models, with the confidence bounds of the IRF clearing zero at the peak in both models (.05 and .10).

Additional tests of robustness are found in appendices D and E in the online supporting information. Appendix D shows that when we use the total U.S. population to generate the *VHCR*, the results are nearly identical. Appendix E compares the total hate crime rates generated by the UCR HCSA data and the National Crime Victimization Survey (NCVS) data. In those analyses, we compare the UCR hate crimes with the NCVS hate crimes known to the police and unknown to the police. The UCR rates are more strongly correlated (both linearly and first differenced) with the rate of police known hate crime events generated by the NCVS than the rate of events unknown to police, although differences remain. Furthermore, a comparison of main results from all three sources shows that the IFRs generated from the HCSA data look more similar to those generated from the police known NCVS data than those unknown to the police. The appendix describes in detail the differences in the two data sources, concluding that the comparison provides some support that the data are measuring the same underlying dynamics. It is only when we consider these



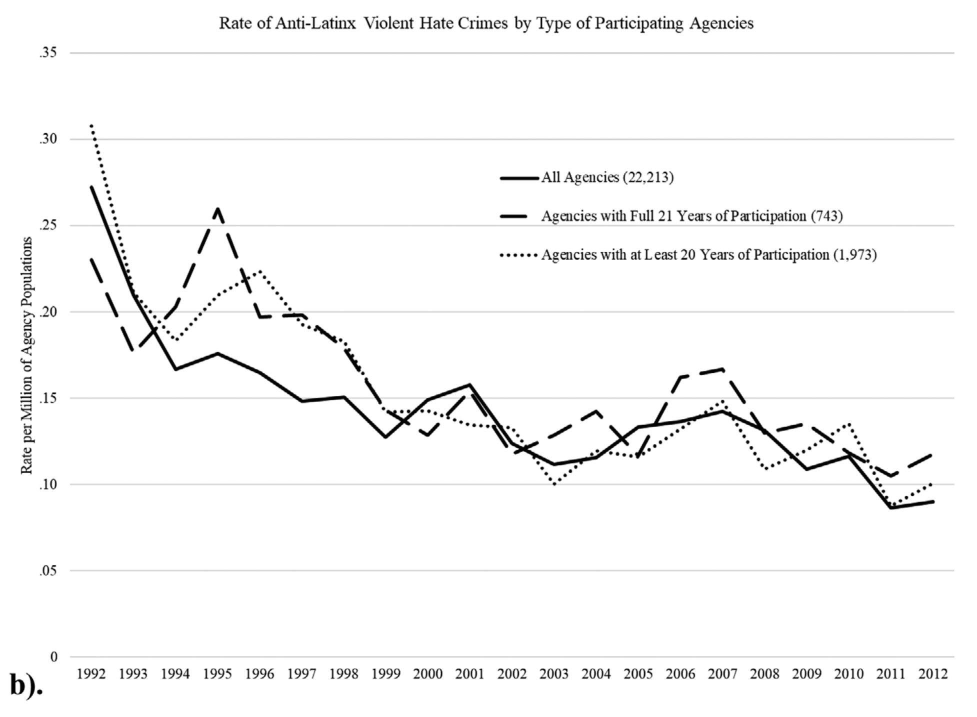


FIGURE 5 VHCR generated from different sets of participating agencies ($n = 21$)

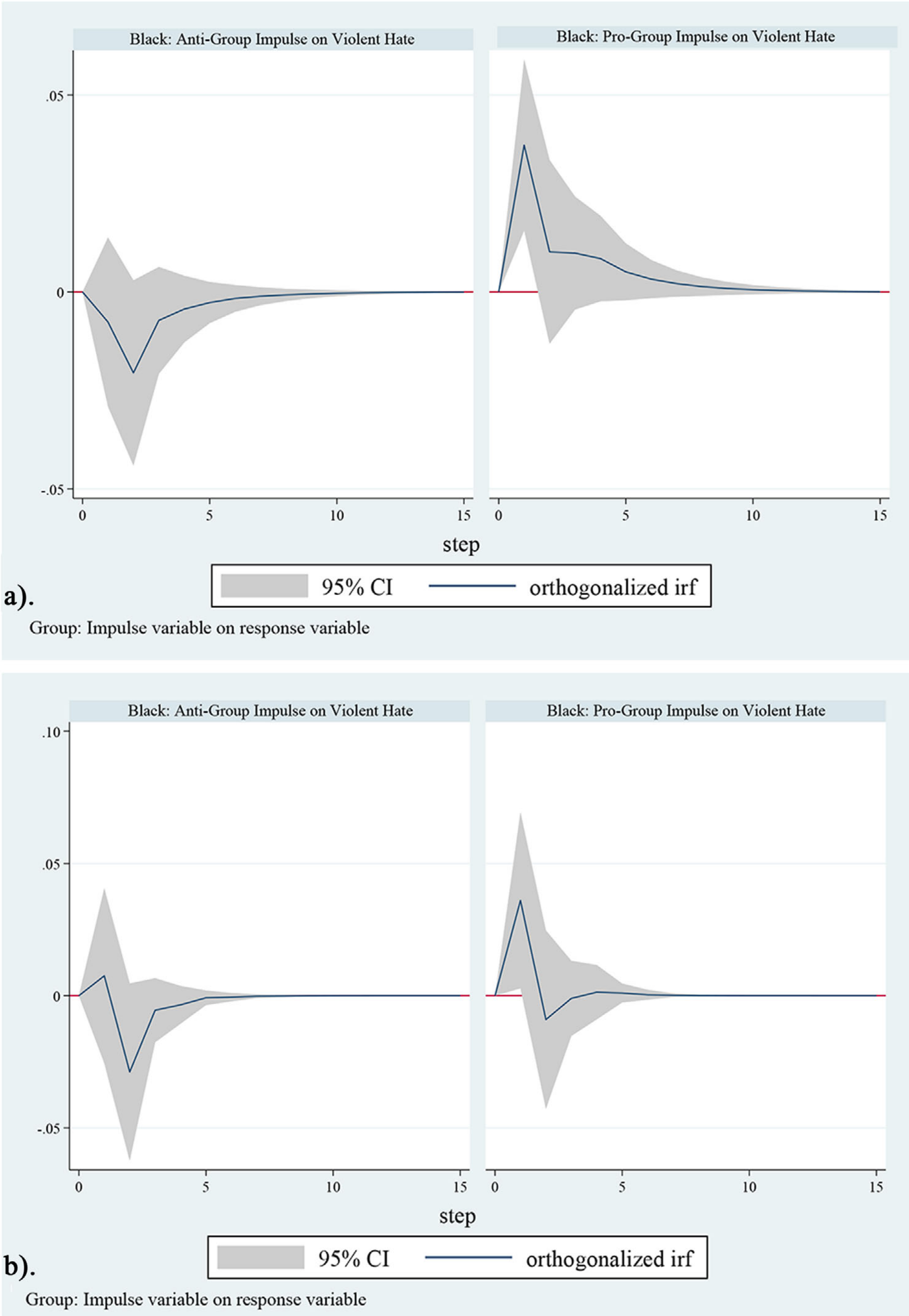
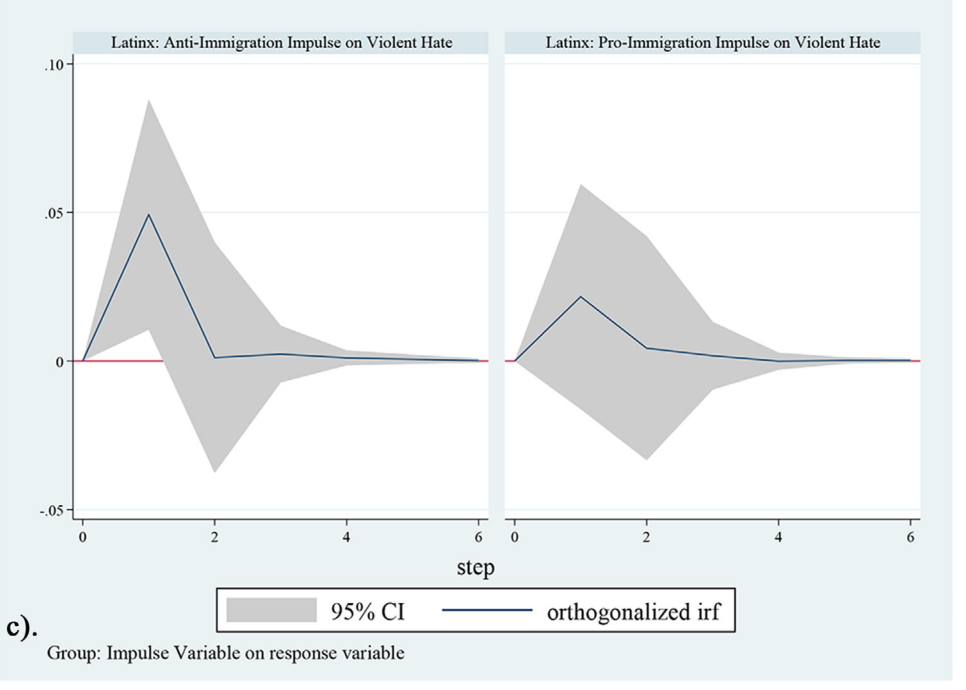


FIGURE 6 Impulse Response Functions for Black and Latinx persons using rates from subsets of participating agencies ($n = 250$) [Color figure can be viewed at wileyonlinelibrary.com]
a). Black IRFs using Agencies With at Least 20 Years of Participation
b). Black IRFs using Agencies With all 21 Years of Participation
c). Latinx IRFs using Agencies With at Least 20 Years of Participation
d). Latinx IRFs using Agencies With all 21 Years of Participation



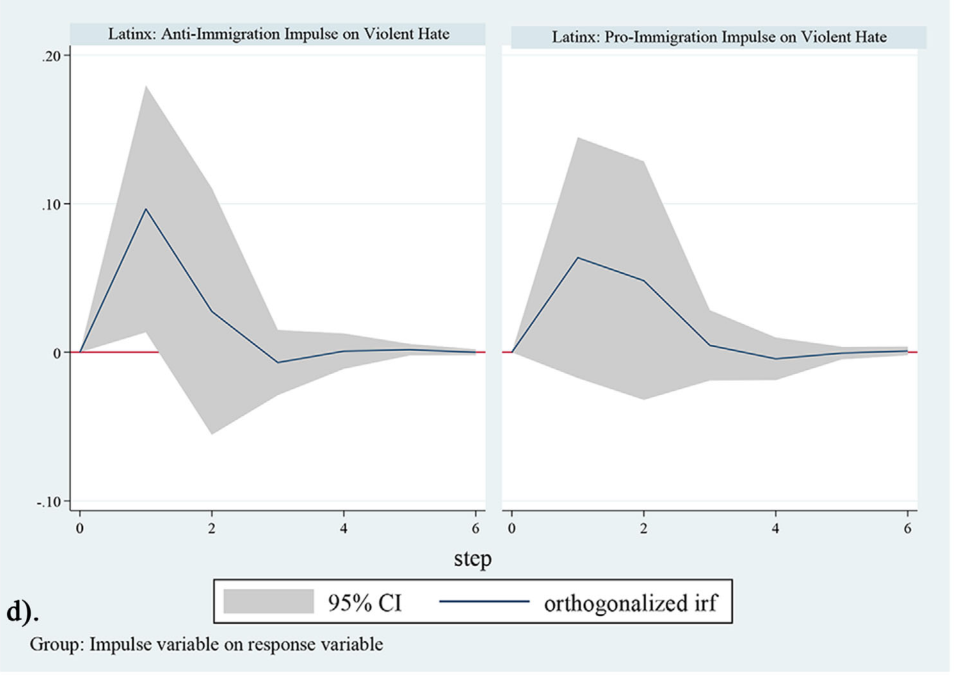


FIGURE 6 Continued

results and the other robustness tests, however, that we feel confident that we are estimating the appropriate relationships.

6 | DISCUSSION

This article began as a search for evidence about whether changes in the political power of different marginalized groups in the United States affect the amount of hate-motivated violence directed toward them. We were drawn to this research by the reported rise in hate-based violence against minority groups that drew hostile attacks from the Trump administration. We developed two compatible hypotheses predicting increased violence toward minority groups after the federal government takes either favorable or unfavorable action toward these groups. Political threat theory concludes that minority groups are at risk of backlash after expanding their political power, as dominant groups use violence to reestablish equilibrium. Indeed, we found an increase in violent anti-Black hate crimes after the U.S. government took actions that were favorable toward African Americans. We extended this theory to account for surges in dominant group resources when political elites openly express hostility toward minority groups. Support for the emboldenment hypothesis was apparent only in our findings for Latinx persons. As the federal government opposed immigrants generally, or Latinx persons specifically, violent hate crimes against Latinx persons increased.

The data used to generate these findings suffer notable limitations as surely some U.S. actions and violent hate crimes are undetected during collection. We feel confident, however, that the GATE data captured the larger patterns of attention given to these groups as the description provided in appendix A in the online supporting information characterizes most of the major events over this period. Also, the GATE data capture more of the subtle attention these groups receive as political leaders promote certain initiatives and Congress considers legislative action. Furthermore, the variation used to generate our estimates was based on month-to-month changes, which are less sensitive to undercounting of both sources. Despite this, we recognize that both anti- and pro-group government actions are measured with error, and that this type of measurement error is likely correlated, possibly causing estimation problems. Supplemental analysis not shown here, however, produce the same findings when the VAR models are run with only one type of government action. We also recognize that the estimated positive effects for African Americans could also be driven by an increased reporting by victims in those groups when the federal government expressly supports their movement. Yet, this bias might also be counterbalanced by reluctant reporting agencies that react to positive attention by dismissing hate crimes as neutrally motivated.

The findings hold in our supplemental analyses, when we use subsets of agencies that participated in the HCSA program for all or nearly all years in our analysis. Furthermore, a comparison of hate crime rates generated from both the HCSA and NCVS shows that the HCSA data align more closely to victimization events known to the police than those unknown to the police, which helps us feel more confident that the HCSA is capturing the underlying dynamic of hate crimes reported to the police. Despite the possibility that Black people might be more likely to report hate crimes when the federal government is exhibiting support for civil rights, we find it highly unlikely that Latinx victims of violent hate crime would increase their reporting to police when the political climate is hostile toward immigrants, as research suggests that some Latinx persons in the U.S. view contact with police as risky (Vidales, Day, & Powe, 2009; Xie & Baumer, 2019). While the federal hate crime statistics are far from perfect, the fact that our models—which were derived from data combined from two much different collection efforts—are consistent raises confidence that the hate crime data were sufficient for our purposes. Combined, we find support for the political threat hypothesis among hate crimes committed against Black people and for the emboldenment hypothesis among hate crimes committed against Latinx persons.

We would be remiss, however, to suggest that these findings can be generalized to all jurisdictions in the United States. The voluntary participation, and differential efforts to investigate crimes for hateful biases undeniably exclude important jurisdictions, which results in selection bias. This raises the question of how selection might affect the main results. If agencies in more biased jurisdictions are reluctant to participate, as suggested in the findings by McVeigh et al. (2003) and King (2007), then the effects from only the participating agencies might be smaller than those that would be estimated had all agencies fully participated. In other words, following King's logic, if those jurisdictions that are more "threatened" by minorities are less likely to participate, then the current set of results that rely exclusively on participating agencies are surely muted. In fact, when we reanalyze the model using data only from agencies that fully participated in the populous for the increasingly racially diverse state of Texas, the African American political threat effect increases by a factor of four (peak = .12). The Latinx emboldenment effect also increases by the same magnitude when we use data from all participating Texas agencies. If jurisdictions in Texas look more like those without agency participation than those with agency participation, then these comparisons do, indeed, suggest that the bias in the primary analyses is downward.

The dependency of the findings on the composition of participating agencies reminds us that hate crimes are local phenomena, and that if they are responsive to actions by federal leaders, then they are likely sensitive to those by nearby officials. Continued research in this area should incorporate variation in state and local government actions, including efforts to hold police accountable for violence against Black people and to remove undocumented individuals from being eligible for statewide benefits.

Taken together, what do our findings suggest about the reported increase in hate crimes since Donald Trump was elected as president? Although it is an out-of-sample case, we argue that the rise in hate crimes against Latinx people since the 2016 election appears to be a continuation of patterns that have persisted over recent decades. Since the 1990s, federal government actions targeting immigrants and Latinx persons seemed to have emboldened violent hate crimes against them. Moreover, federal efforts to support Black people are complicated by their risk of backlash, as supportive federal rhetoric, actions, and policies tend to trigger violent anti-Black hate crimes. These effects likely reflect long-simmering institutionalized and internalized racism in the United States, which is triggered when federal officials send positive cues about this demographic.

These mixed findings point to the need to disaggregate the implications of political threat and emboldenment hypotheses by the affected groups, rather than assuming these mechanisms operate identically across groups. Although our data are insufficient to identify specific mechanisms that could be driving the differences across groups, one possibility is that White perpetrators stereotype Black Americans as "insiders" and Latinx people as "outsiders." It may be that members of the dominant group commit violence against perceived insiders to punish them for increases in power, whereas members of the dominant group commit violence against perceived outsiders when the federal government signals that the threat from outsiders is growing. Qualitative data that document the reasoning of hate crime perpetrators could provide insight into the nature of perceived threat or other emotions that leads to this type of violence. Another limitation that qualitative data might uncover would be the effects, if any, federal government actions have on people who possess multiple overlapping identities, such as African Americans of Latinx heritage, or whether perpetrators of hate crime substitute one targeted group for another.

In future research, scholars should replicate this analysis with more recent data to directly assess the degree to which Trump's election and subsequent presidency has triggered an emboldening from the far-right. In fact, the daily documentation of the President's Tweets provide a rich source of information regarding the rhetoric used toward different marginalized groups. Additionally, future research should assess the role of emboldenment on mobilizing acts by organized White supremacist groups.

That said, this research, which draws on two decades of U.S. government activity, suggests that federal actions matter in important ways that could motivate violent hate crimes against protected groups—either by supporting them without protecting them from possible backlash or by criticizing and undermining them, emboldening perpetrators to commit hate crimes against them. Indeed, our results confirm evidence from other studies suggesting that hate speech among elites can motivate hate crimes among constituents (Gagnon, 1995; Müller & Schwarz, 2018). These findings have important implications for preventing hate crimes, as well as for assigning culpability when they occur.

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SUPPORTING INFORMATION

Additional supporting information may be found online in the Supporting Information section at the end of the article.

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