

CRIME AND (ELECTORAL) PUNISHMENT:
VIOLENCE AND VOTING IN THE UNITED STATES

by

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CONFLICT OF INTEREST STATEMENT

While writing this thesis, I was an employee of the United States Department of Justice. My professional affiliation neither enhanced nor restricted my ability to complete this research. All parts of this thesis are unclassified and freely available through open-source searches. All views expressed in this thesis are mine; this thesis is not to be taken as official policies or positions of the United States' government.

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SECTION ONE

Introduction

“While the harm victims of crime suffer is very real, our understanding of the meaning and causes of this harm depends upon the way in which the crime issue is apprehended in political discourse. ... The Bush campaign's manipulation of the ‘Willie Horton’ incident, for example, can be understood as an attempt to invoke the image of ‘the black rapist’ (with all its historical and cultural significance) in order to generate support for law and order policies – and for the candidate who was, presumably, more capable of implementing them.”

– Katherine Beckett (1997, pp. 5–6)

During the 1988 American presidential election, crime took center stage. Per a 1972 law supported by Massachusetts Governor Michael Dukakis – sitting Vice President George H. W. Bush’s challenger for the presidency – Massachusetts had a furlough program for prisoners, and during his tenth weekend away from prison, convicted murderer William “Willie” Horton escaped to Maryland, abducted a couple in their home, violently assaulted the man, and raped and stabbed the woman (Egelko, 2008; Mendelberg, 1997, 2001; Newburn & Jones, 2005; Schwartzapfel & Keller, 2015; *Willie Horton Case*, 1988). Recognizing Dukakis’ support for the program as an opportunity to paint his opponent as soft on crime, Bush structured part of his campaign around the idea only he could keep the country safe. His supporters picked up this message, most notably culminating in a Bush-affiliated political action committee running “perhaps the most famous negative ad of recent times – the Willie Horton spot” (Geer, 2006, p. 46). The “Weekend Passes” ad suggested Dukakis was personally responsible for Horton’s crimes, and political observers “later concluded that it was perhaps the single most significant and persuasive piece of television advertising used in the entire 1988 presidential campaign” (Haney, 2011).

In this thesis, I study the 1988 presidential election (a race marked by its focus on crime) in the context of the 1984 and 1992 elections (races with their sights set on other issues); this analysis is an ideal way to gauge the impacts of crime, one slice of broader lines of research about constituents' responses to elite cues, their perceptions of the world, and their voting behavior. Though existing scholarship connects these threads of political behavior, none examine local crime through its implications for presidential elections, marking this paper as a novel contribution to the literature. Furthermore, this project yields insights into constituents' issue attribution and voting behavior, processes of interest to politicians, political strategists, and other on-the-ground political careerists.

To perform my analysis, I examine data from the 1984, 1988, and 1992 waves of the American National Election Study (W. E. Miller & National Election Studies, 1999a, 1999c, 1999b), the Federal Bureau of Investigation's Uniform Crime Reporting (UCR) Program (Kaplan, 2022a, 2022b), the U.S. Bureau of Economic Analysis (n.d.), the World Bank & Organisation for Economic Co-operation and Development (n.d.), and the U.S. Bureau of the Census (1993, 1998). Overall, I find an inverse, statistically significant, and unique relationship between incidences and perceptions of crime in 1988, no similar relationships for 1984 or 1992, and limited interactive effects on perceptions of crime from local crime rates and constituents' attention to elite cues about crime. In addition, I find no significant relationships between incidences or perceptions of crime and presidential vote choice in any of the three elections. Altogether, this evidence strongly suggests crime does not align with traditionally-conceived theories of public opinion and retrospective voting.

This paper proceeds as follows: in the second section, I build a theoretical framework by surveying the literatures on retrospective voting, elite cueing, constituents' issue attribution,

crime, and the three elections in which I contextualize my study. This discussion yields a set of testable hypotheses I outline in the third section, and in the fourth section, I describe the data I use to test them. In the fifth section, I describe, report, and discuss my empirical analysis, and in the sixth section, I conclude my paper by discussing the implications of my findings and proposing directions for future research.

SECTION TWO

Theoretical Framework

Public Opinion, Retrospective Voting, and Crime

Per the retrospective voting model, constituents choose for whom to vote by reflecting on and evaluating the performances and policies of incumbents (e.g., Achen & Bartels, 2017; Fiorina, 1978, 1981; Healy & Malhotra, 2013). According to the model, constituents vote for representatives who improve their welfare and against representatives who do not. Many of the model's tests focus on economic issues, potentially because – as Cook et al. (1994) propose – “it is difficult to incorporate [noneconomic] variables into aggregate models.” Brody (1991) writes, “a fuller explanation of the dynamics of public support needs to apply this model to noneconomic policy areas,” such as was done by Cook et al. (1994), Howell & Sims (2016), and Leal (2006) when studying how governors' support varied based on their positions on abortion, their states' crime rates, and how well the students in their states performed on standardized tests (also see Healy & Malhotra, 2013).

The retrospective voting model's key notion is the idea voters recognize changes to their welfare (the “retrospective” piece) and reward or punish representatives for them (the “voting” piece), in theory holding elites accountable only for the things they control or influence directly (Hibbing & Alford, 1981; Kiewiet, 1983; Peffley & Williams, 1985; Pitkin, 1967; Plescia, 2017). That ideal may not always be fulfilled, however: notably, Achen & Bartels (2017, p. 128) find evidence of “blind retrospection,” that “voters consistently and systematically punish incumbents for conditions beyond their control.” Specifically, they show constituents from New Jersey's beach areas voted against incumbent president Woodrow Wilson in 1916 potentially due to a

string of random shark attacks, something which worsened their welfare but was entirely outside Wilson's control. Mendelberg (2018) rebuts this conclusion, instead arguing beach constituents voted against Wilson not because of the shark attacks themselves but because Wilson did not make even a symbolic effort to prevent them. Supporting Achen & Bartels and pushing back on Mendelberg, additional research has shown impacts on elections from other politically-irrelevant factors, such as sporting events (Busby et al., 2016; A. Fowler & Montagnes, 2015; Healy et al., 2010) and constituents simply being in good moods on Election Day (Bower, 1981; Forgas, 1995; Isen et al., 1978; Samuelson & Zeckhauser, 1988; Yen & Chuang, 2008). Clearly, constituents' issue attribution to elites is not always perfect.

Unlike the shark attacks, sporting events, and unique attitudes of voters, not everything that affects vote choice is politically irrelevant, but though something may be relevant to politics, it is not necessarily relevant to the election in question. For example, the national economy can affect non-national elections (e.g., Carsey & Wright, 1998), and local economies can affect non-local elections (Cottrell et al., 2018; de Benedictis-Kessner & Warshaw, 2020; Healy & Lenz, 2017; Hill et al., 2010; Niemi et al., 1995). These effects may be somewhat unwarranted given non-national representatives have little influence over the national economy and non-local representatives have limited influence over local economies.

Connecting with all this, studying the impacts of crime during the 1984, 1988, and 1992 election cycles is a valid way to test the retrospective voting model. First, crime is a politically-relevant issue, playing out on both the local and national levels (Finckenauer, 1978; Herzik, 1983; Tonry, 2000) and affecting all Americans (Lerman & Weaver, 2014). For example, many constituents feel the effects of crime and the U.S.' carceral state either directly (e.g., being victimized) or indirectly (e.g., going through security checkpoints to access public spaces).

Second, existing research shows crime influences electoral decision-making in non-presidential races and falls on the agenda of non-presidential candidates (Dometrius, 1999; Leal, 2006). Though the onus of local law enforcement lies with state and local officials more than federal representatives (e.g., Richman, 2006; Zimring & Hawkins, 1996), presidents nonetheless have increased their attention to crime since 1965 (Marion, 1994a, 1994b; Oliver, 2002) and national representatives continue to pursue policies to reduce local crime (Oliver & Barlow, 2016).

Finally, per Healy & Malhotra (2013), “events and outcomes ... [which] are temporally isolated and can be clearly attributed to a particular incumbent” have the potential to swing votes – e.g., crime in a state can be “a very clear indicator” of a governor’s performance (Cummins, 2009). Similarly, Popkin (1991, p. 111) writes the following:

many Americans picture state governors as directly responsible for all pardons and furloughs handed out to prisoners ... The release of Willie Horton on furlough was effective against Michael Dukakis in 1988 because furloughs, pardons, and death penalties are to governors what hostages are to presidents.

Hence, incidents of crime – such as Horton’s – may impact vote choice, but to do so, they first must be salient to voters (Campbell et al., 1960; Converse, 1964; J. M. Miller et al., 2017). The literatures on heuristics (Gilens & Murakawa, 2002; Kuklinski & Quirk, 2000; Popkin, 1991, 1993) and agenda-setting (Erbring et al., 1980; Gilens & Murakawa, 2002; MacKuen, 1984a, 1984b, 1981; McCombs & Reynolds, 2002; J. M. Miller et al., 2017; R. E. Miller & Wanta, 1996) show issue salience can stem in large part from campaigns, the media, and how much attention constituents pay to them, so the literature suggests elite signals about crime may trickle down to constituents, affecting both their perceptions of objective crime rates and ultimately their vote choices.

Moving back to the general theory of the retrospective voting model, it is not only objective events that can influence constituents' vote choices, however – perceptions of events can also affect them. While describing retrospective voters, Fiorina (1981, p. 5, emphasis in the original) writes they “need *not* know the precise ... policies of the incumbent administration in order to see or feel the *results* of those policies. ... In order to ascertain whether the incumbents have performed poorly or well, citizens need only calculate the changes in their own welfare.” Constituents' perceptions of the world are not always accurate, however, and the model complexifies when their subjective perceptions differ from their objective realities. For example, leading up to the 1992 election, the media's coverage of the economy was overwhelmingly negative (T. E. Patterson, 1993), leading voters to think the economy was worse than it really was and potentially contributing to Bush's electoral loss (Hetherington, 1996). Similarly, when examining gubernatorial races, Svoboda (2016) finds though “aggregate measures of the state's economy ... [are] objective, these figures may not be consistent with ... the electorates' perceptions of the economy which determines their voting patterns.” Moreover, Campbell & Converse (1972, p. 8) note the “detailed interplay between objective situations and the way people assess them subjectively” – in the end, individuals' “pseudo-environments” (Lippmann, 1929) are not necessarily truthful or accurate indicators of the world at large (also see Gay, 2004). Bringing this all together, though constituents “live in the same world, ... they think and feel in different ones” (Lippmann, 1929, p. 20).

These patterns manifest with the issue of crime. First, constituents' perceptions of it tend to differ from reality, as they frequently overestimate crime rates (Baumer, 1985; Beckett, 1994; Hipp, 2013; Indermaur & Roberts, 2009; Moore, 1998; Warr, 1995). These overestimates can diminish their perceived qualities of life and motivate them to search for solutions at the ballot

box (Garofalo, 1981; Garofalo & Laub, 1978; Lewis, 2017), thus connecting objective crime conditions, crime perceptions, and vote choice. In all, then, though objective conditions may matter to vote choice, their influence seems conditional on constituents' perceptions of them, and this line of reasoning appears applicable to the issue of crime.

Crime as a Raced Issue

In the prior sub-section, I surveyed the literatures on public opinion and retrospective voting, and I briefly outlined how crime may fit in with them. In this sub-section, I delve deeper into the literature on crime specifically, juxtaposing ways the issue may and may not align with those aforedescribed theories of American political behavior.

One way crime may fit with the retrospective voting model is through its impact on constituents' perceptions of the world and their vote choices. Specifically, violent crimes are “signal crimes” (Couttenier et al., 2021; Innes & Fielding, 2002), defined as “incident[s] ... disproportionately influential in terms of causing a person or persons to perceive themselves to be at risk in some sense.” Furthermore, when analyzing violent crimes in relation to property crimes, Romer et al. (2006) find the former receive more media attention than the latter, potentially explaining Cummins' (2009) finding that “voters hold the incumbent [gubernatorial] party responsible for violent crimes, but not property crime.” Violent crime, then, can affect both constituents' perceptions of their safety as well as their vote choices.

Overall, however, crime in the United States is a raced issue, marking it as different from other, lesser-raced issues – e.g., abortion deals primarily with gender, privacy, and reproductive rights (Bouie, 2023; Garrow, 2015; Ginsburg, 1985; cf. O'Brian, 2020). First, the very creation of the U.S.' carceral system was a “well-documented turn on the part of white people to the

coercive apparatus of the state to fortify a threatened racial hierarchy [which had been upset by emancipation and Reconstruction]” (Blackmon, 2009; Burghardt & du Bois, 1985; Eubank & Fresh, 2022; LeFlouria, 2015; Lichtenstein, 1996; Muhammad, 2019; Oshinsky, 1997).

Additionally, being Black increases the likelihood constituents are stopped and frisked by law enforcement, arrested after being stopped, or subjected to force (Coviello & Persico, 2015; Levchak, 2021; Milner et al., 2016). Furthermore, the U.S.’ prisons historically have incarcerated disproportionately-large shares of Black people (Nellis, 2021; Sabol et al., 2020). Relative to white constituents, all this leaves Black constituents less trusting of law enforcement and the U.S.’ criminal justice system overall (Bobo & Johnson, 2004; Prowse et al., 2019; Russell, 1998; Toch & Maguire, 2014; Weitzer & Tuch, 1999).

Altogether, given this “criminalization” of being Black in America (Alexander, 2012; Hinton & Cook, 2021; Jackson, 2022), constituents – and especially those who are Black – may not necessarily respond to crime by voting for harder-on-crime candidates. Such votes would not exclusively be in favor of reducing crime; they also would be in favor of maintaining the U.S.’ racially-discriminating carceral state, a set of institutions which some have argued is the country’s “new Jim Crow” (e.g., Alexander, 2012, Chapter 5; López, 2010). As Forman (2012, secs. 4–5) points out, however, this is not necessarily a perfect analogy:

in [Washington, D.C.,] this majority-black city with substantial local control over who goes to prison and for how long ... [there still exist racially-disparate incarceration rates which] mirror the rates of other cities where African Americans have substantially less control over sentencing policy. ... These data indicate the limits of the Jim Crow analogy, which attributes mass incarceration entirely to the animus or indifference of white voters and public officials toward black communities. ...

[Furthermore,] the New Jim Crow writers ... focus almost exclusively on the War on Drugs. This approach ... makes less sense for more recent proponents of the analogy, who attack the broader phenomenon of mass incarceration but restrict their attention to punishments for drug offenders. Other crimes – especially violent crimes – are rarely mentioned.

Regardless, this literature on crime and law enforcement suggests the issue's raced nature may ultimately prevent it from serving as the baseline of retrospective voting as traditionally conceived. Those effects may not manifest for everyone, however, as some constituents' desires for safety and security could outweigh their preferences for non-discriminatory policies, leading them to support hard-on-crime candidates who promise hard-on-crime policies. To examine the extent to which constituents are caught between the two sides of this potential trade-off, I now turn to the topic of public opinion and elite influence regarding crime.

As a starting point, Beckett (1994) discusses two competing explanations for public concern about crime. First, the objectivist model purports "knowledge of objective conditions is a necessary and largely sufficient condition for the identification of a social problem" – i.e., there will be a positive relationship between incidences of crime and constituents' perceptions of crime as an important problem (also see Mayer, 1992; Niemi et al., 1989). In comparison, the constructionist model purports "the public's assessment of the nature of [social] problems will be shaped by their popular representation" – i.e., there will be a positive relationship between elite cues or public policy about crime and constituents considering it an important problem (also see Chambliss & Sbarbaro, 2018; Graber, 1980). Both these theories connect with traditional models of public opinion: constituents examine objective conditions to form their subjective understandings of the world – their "pseudo-environments" (Lippmann, 1929) – and elite cues may moderate that reality-perception relationship (though they do not necessarily construct the environments single-handedly).

As it turns out, research testing these theories of public opinion formation is mixed. Beckett (1994) finds support for the constructionist model: "media and state initiative are positively associated with increased public concern about crime, while the crime rate does not

appear to have had such an impact” (also see Beckett, 1997; Beckett & Sasson, 2004; Ramirez, 2013; V. M. Weaver, 2007). In comparison, Enns (2014) shows public opinion regarding crime has led elite cues on crime and been “a fundamental determinant” of increases in the U.S.’ incarceration rate (also see Curie, 1998; Enns, 2016; Kaminer, 1995, p. 6; J. Q. Wilson, 1975). Connecting with this, Smith (2004) shows “the explosive increase in prison populations since the mid-1970s is largely a product of the most basic political environment[:] ... partisan control of state government, gubernatorial election cycles, selected policy decisions, and race,” and he later writes, “the social fault lines built over race are still strong enough to trigger the use of prisons as a means of social control.” In the end, there does not appear to be a clear resolution to this question of the relationships between public opinion, elite action, and crime policy, though it once again appears crime’s connection with race may lie at the center of understanding how constituents react to it. After all, given the topic’s unique, raced nature, constituents may not interpret elite cues about it similarly to how they interpret cues about other issues.

The literature on framing yields insight into how constituents make sense of crime-focused cueing. Per Converse (1964, sec. 6), “visible social groupings” – e.g., race – anchor constituents’ belief systems. Nelson & Kinder (1996, emphasis in the original) continue this line of reasoning, purporting “[p]ublic opinion on matters of government policy is *group-centric*: shaped in powerful ways by the attitudes citizens possess toward the social groups they see as the principal beneficiaries (or victims) of the policy.” Constituents, then, make sense of the world by relying on “primary frameworks,” structures which allow “user[s] to locate, perceive, identify, and label ... concrete occurrences ... [without necessarily] be[ing] aware [of such influence]” (Goffman, 1986, p. 21).

A wide range of evidence shows constituents' attitudes toward groups relate to their opinions regarding social policy, and this is hardly surprising given many policies are justified and criticized on the bases of their being group-centric in the first place (e.g., Bentley, 1908; Dahl, 1963; Glazer & Moynihan, 1975; Weir et al., 1988). For example, white constituents' support for affirmative action connects with their attitudes toward Black constituents (Kinder & Sanders, 1996; Kluegel & Smith, 1983; cf. Sniderman, 1993), constituents' willingness to be politically tolerant of certain groups connects with their "gut-level" reactions to those other groups (Kuklinski et al., 1991; Sullivan et al., 1993), and constituents' opposition to welfare programs connects with their negative attitudes toward poor constituents in general (Feldman, 1983; Kluegel & Smith, 2017) and – especially for white constituents given the raced nature of welfare – Black constituents in specific (Gilens, 1996a, 1996b, 1999; Schram et al., 2010).

How does all this connect with crime? Similar to some of those aforescribed issues, crime-focused discussions frequently activate constituents' attitudes toward Black people, such as through race-coded, "dog whistle" rhetoric (Beckett, 1997; Haney-López, 2014; V. M. Weaver, 2007). The Horton ad, for example, depicted Horton as "the black rapist" (with all its historical and cultural significance)" (Beckett, 1997, p. 6), thereby seeing Bush's campaign make an appeal originally framed implicitly by race. The appeal activated white constituents' racial resentment yet did not have strong, positive effects on their perceptions of crime (e.g., Mendelberg, 1997, 2001), providing evidence at odds with Beckett's (1994) constructionist model of public opinion. Ultimately, though, given racial attitudes can be sufficient to affect vote choice (Carmines & Stimson, 1990; Huckfeldt & Kohfeld, 1989; White & Laird, 2020), Bush did not necessarily need to make constituents more concerned about crime to benefit from the ad; simply stoking racial resentment might have been enough to win electoral support.

Situating a Test of Crime-Focused Retrospection

Gilens (1996b) writes, “[w]hen crime or welfare become vehicles with which to mobilize and stimulate antiblack sentiments among the white electorate, we face an insidious politics of racial division,” though as described, the amount that politics aligns with extant theories of public opinion and retrospective voting remains somewhat unclear. I seek to fill some of this gap in the literature by testing the extent to which crime aligns with traditional conceptions of American political behavior, and the 1984, 1988, and 1992 elections provide an optimal context in which to do so.

First, as I have discussed, the 1988 election focused on crime (and the other two elections focused on other topics), providing an ideal case study (and quasi-counterfactuals) for the effects of crime when elections are (and are not) referenda on the topic. The 1984 election centered on the economy (Abramson et al., 1986; Hershey, 1994; T. M. Holbrook, 1994; Leighley & Nagler, 1992; Raines, 1984; Sigelman & Gant, 1989). The 1992 election also focused on the economy, though it also centered on the candidates’ personalities, abortion attitudes, and foreign policy positions (Abramowitz, 1995; Alvarez & Nagler, 1995; Dodson, 1993; Doherty & Gimpel, 1997; Levy, 2022; Lipset, 1993; Ornstein, 1992). Additionally, studying the 1984 and 1992 elections in relation to the 1988 race makes sense given they were the two races closest in time to 1988, marking them as likely to be similar to it (Baker & Scheiner, 2004; Budge, 1994; J. Fowler, 2005; Somer-Topcu, 2009).

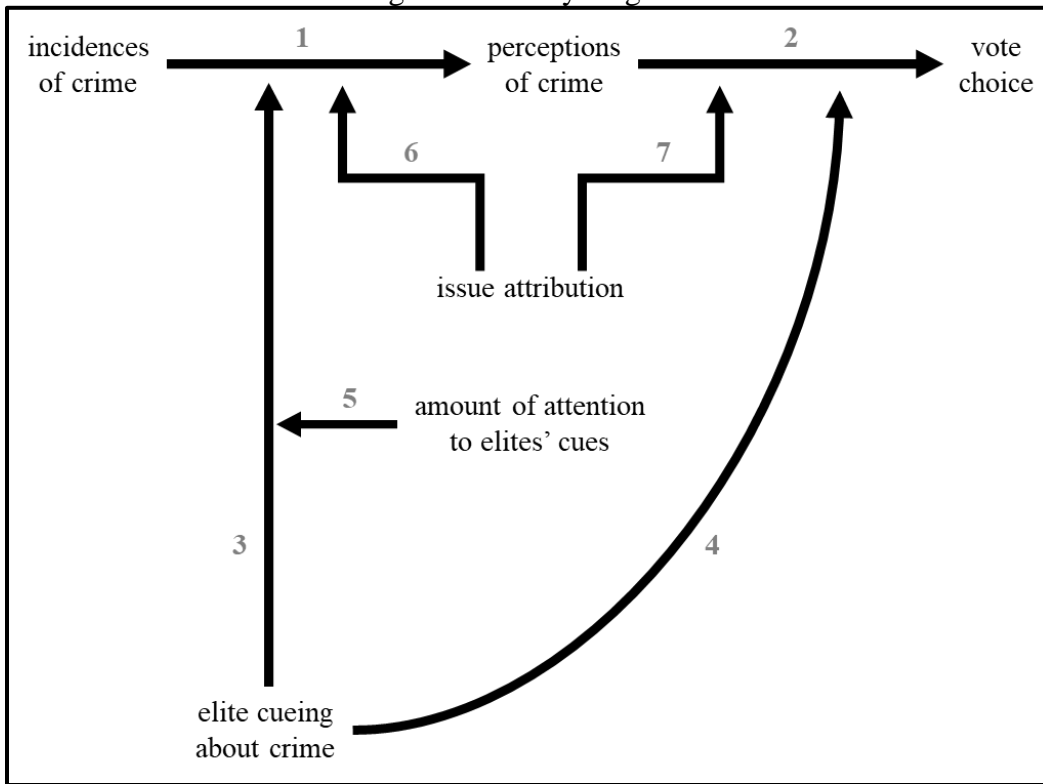
Second, all three years were included in a period of “crime rates [rising] in the late 1980s and early 1990s, but later declin[ing] in the late 1990s ... therefore provid[ing] a good period in which to gauge the effect of crime when it was at both high and low levels,”(Cummins, 2009;

also see Garland, 2001, p. 90; LaFree, 2018, Chapter 2; Ruth & Reitz, 2003, Chapter 3; Saad, 2007). In fact, in 1988, twenty percent of voters reported punishing criminals was among the issues they considered most important (Dionne, 1988), and that percentage is likely a lower bound of the percentage of constituents who considered crime overall to be salient given punishing criminals is only one slice of the issue as a whole. Furthermore, that twenty percent of constituents supported Bush by a rate of approximately two-to-one; there appeared to be a connection between voting for Bush and considering crime (or at least that part of the issue) to be salient.

In comparison, in 1987, the percentage of respondents who considered anything related to crime to be one of the most important issues facing the country was lower by anywhere from at least five percentage points (U.S. Council for Energy Awareness, 1987) to seventeen percentage points (ABC News/ Washington Post, 1987; CBS News/ New York Times, 1988a, 1988b). This difference suggests some change between 1987 and 1988 increased the salience of crime, and it seems possible that difference could have been a cueing effect from Bush's campaign.

This context, then, will provide an easy test to find evidence crime functions similarly to lesser-raced issues when it comes to public opinion and retrospective voting. In all, I sum up this theory in the following diagram:

Figure 1. Theory diagram.



If crime comports with traditional theories of American political behavior, perceptions of crime will mediate the relationship between incidences of crime and constituents' vote choices (arrows one and two in Figure 1). The presence of elite cues about crime (arrows three and four) will moderate those incidence-perception and perception-vote relationships, and the amount of attention constituents pay to the cues also will moderate the incidence-perception relationship (arrow five). Finally, constituents' issue attribution of crime – whether they perceive it as a district- or country-level problem – will moderate both relationships as well (arrows six and seven).¹

¹ Though I do not test for moderation from issue attribution the way researchers traditionally have tested for moderation (with interaction effects), I nonetheless theorize these incidence-perception and perception-vote relationships may differ depending on whether constituents consider crime a local problem or a national problem.

SECTION THREE

Hypotheses

In this section, I outline a number of testable hypotheses to facilitate an examination of the extent to which crime fits into the traditional conceptions of public opinion and retrospective voting. I begin by discussing the relationships between incidences and perceptions of crime. First, because crime is undesirable – e.g., it makes people feel unsafe and therefore worsens their psychological wellbeing (Brantingham & Brantingham, 1995; Garofalo, 1981) – I expect increased local crime rates to increase the chances constituents considered crime an important local problem. Given elite cues can increase issue salience (e.g., Gilens & Murakawa, 2002; Kuklinski & Quirk, 2000; Popkin, 1991), however, I expect that relationship to vary from election to election, and given 1988 had the most cues about crime, I expect this incidence-perception relationship to have been strongest then. Hence, I make my first hypothesis as follows:

H1. Local incidences of crime increased the chances constituents considered crime to be an important local problem (H1a), and this relationship was most pronounced in 1988 (H1b).

This hypothesis applies only to perceptions of crime as a local problem.

Given only the 1988 election centered on a depiction of crime as a national issue, in line with my previous logic, I expect there to have been a positive relationship between incidences and perceptions of crime as a national issue that year alone. As a result, I make my second hypothesis as follows:

H2. In 1988, local incidences of crime increased the chances constituents considered crime to be an important national problem (H2a); this relationship did not exist in 1984 (H2b) or 1992 (H2c).

In sum, these hypotheses will allow me to examine constituents' perceptions of crime and how they may vary across levels of issue attribution, elite cues, and crime rates.

Moving one level deeper into my theory, I expect the amount of attention constituents paid to elite cues about crime to have further moderated the incidence-perception relationships. Put another way, for 1988, I expect there to be positive and significant effects on constituents' perceptions of crime from the interaction of their local crime rates and the amounts of attention they paid to the presidential campaigns. Given the 1984 and 1992 elections did not focus on crime, I do not expect there to be similar effects during those years. I therefore make my third hypothesis as follows:

H3. In 1988, there were positive multiplicative effects of local crime and constituents' campaign attention on their perceptions of crime as an important local problem (H3a) and national problem (H3b). There were no similar effects in 1984 (H3c for the country level) or 1992 (H3d for the local level and H3f for the country level).²

This hypothesis will allow me to test the extent to which constituents' individual levels of attention to the campaigns mattered for their perceptions of crime.

Having hypothesized about public opinion on crime, I now turn to vote choice. Applying the retrospective voting model to crime and the 1988 election, constituents could have voted against Dukakis because of Horton's escape and crimes – e.g., Jamieson (1992, p. 33) quotes a summary of pro-Bush voters' positions as “vot[ing] for George Bush because I can't vote for a man who lets murderers out of jail.”

As for 1984 and 1992, the literature on “issue ownership” suggests how constituents may have responded to crime despite those races not focusing as singularly on the issue. Given the Republican party has traditionally “owned” crime and constituents frequently vote for the party

² Given I do not have data to study constituents' perceptions of crime as a district-level issue in 1984 (see section four), I do not outline a hypothesis for multiplicative effects on perceptions of crime as a local issue. By the same logic guiding H3d, I would expect there not to be an interactive effect for 1984.

which “owns” the issues they deem salient (Bélanger & Meguid, 2008; Canes-Wrone et al., 2011; Holian, 2004; Petrocik, 1996), those in higher-crime areas may have voted Republican due simply to the party’s reputation as being hard on crime. Moreover, this possibility could have had a similar impact on constituents in 1988. Ansolabehere & Iyengar (1994) write, “news coverage of ‘owned’ issues ... shift[s] voting preference toward the ‘owner,’” so 1988’s focus on and coverage of crime could have bolstered support for Bush due solely to his being a Republican. Bringing all this together, I make my fourth hypothesis as follows:

H4. Incidences of crime led constituents to vote Republican in all three years (H4a), though the relationship was stronger in 1988 than in 1984 and 1992 (H4b).

In other words, I expect the 1988 race’s cueing on crime to have accentuated a pre-existing relationship between crime and voting Republican, and I expect that pre-existing relationship to have manifested in 1984 and 1992.

Finally, given objective conditions alone do not determine vote choice – constituents’ perceptions of objective conditions also matter – I expect perceptions of crime to mediate the relationship between incidences of crime and vote choice. Combining this with the aforescribed depictions of crime as a national issue in 1988 alone, I make my fifth hypothesis as follows:

H5. Perceptions of crime mediated the relationship between incidences of crime and vote choice in 1984 (H5a for perceptions as a national problem), in 1988 (H5b for perceptions as a local problem and H5c for perceptions as a national problem), and in 1992 (H5d for the local level and H5e for the national level).³

In all, these hypotheses outline a path to examine the effects of crime on elections.⁴

³ Similar to H3 and for the same reason, I do not outline a hypothesis about perceptions of crime as a local problem in 1984.

⁴ Similar to earlier research into public opinion and retrospective voting, my hypotheses focus on the American citizenry as a whole. As I described in section two, however, Black constituents have unique attitudes toward crime and the U.S.’ carceral state, so they may react to incidences of crime differently than non-Black constituents. Nonetheless, given my primary interest in this thesis is broad, I do not perform any race-based subgroup analyses.

SECTION FOUR

Data

To test my hypotheses, I analyze data from five sources: American National Election Study (ANES) public opinion polls (W. E. Miller & National Election Studies, 1999a, 1999b, 1999c), the Federal Bureau of Investigation's Uniform Crime Reporting (UCR) Program (Kaplan, 2022a, 2022b), the U.S. Bureau of Economic Analysis (n.d.), the World Bank & Organisation for Economic Co-operation and Development (n.d.), and the U.S. Bureau of the Census (1993, 1998). In the following paragraphs, I describe these sources in greater detail and outline how I prepared the data for my analysis.

My first source, the ANES data from 1984, 1988, and 1992, are individual-level, nationally-representative stratified cluster samples of voting-age Americans in the continental United States (W. E. Miller & National Election Studies, 1999a, 1999b, 1999c). Each dataset contains information about constituents' demographics as well as their political behaviors, affiliations, and opinions (e.g., race, gender, voting status, partisanship, ideology, issue views). Most of interest for my analysis is the information the data contain about constituents' vote choices in each respective year's presidential election, whether constituents considered crime to be one of the most important problems facing their districts and the country, the amount of attention constituents paid to each year's presidential campaigns, and constituents' demographics.

When studying perceptions of crime (H1 – H3), I include in my analysis all ANES respondents who provided complete information to their interviewers, and when studying vote choice (H4 – H5), I include the subset of those constituents who also reported a clear candidate

preference. Notably, this means I do not drop non-validated voters, and this ultimately makes my thesis an easier test to find evidence in favor of traditional theories of public opinion and retrospective voting. Given political knowledge increases the probability of voting and decreases the probability of following elite cues (e.g., Delli Carpini & Keeter, 1996; Gilens & Murakawa, 2002), including nonvoters in my analysis allows me to study constituents with less political knowledge, ultimately making it more likely I will find evidence of elite cue-taking. In many cases, constituents follow cues when they lack the political knowledge or ability to create issue positions themselves or when they receive cues from credible-seeming elites (Berelson et al., 1954, pt. 3; Campbell et al., 1960, sec. 3; Converse, 1964; Gilens & Murakawa, 2002; Kuklinski & Quirk, 2000; Popkin, 1991, 1993; Tversky & Kahneman, 1974; Zaller, 1992, Chapters 3–4); these conditions will be more likely to manifest in my study given I include nonvoters. Additionally, political knowledge increases the chances constituents have preferences on salient issues – as I have argued crime was in 1988 – and makes those preferences ideologically consistent, stable over time, and resistant to new or misleading information (Delli Carpini & Keeter, 1996; Feldman, 1989; Kinder & Sanders, 1990; Krosnick & Milburn, 1990; Lanoue, 1992; Stimson, 1975), effects which would decrease the likelihood I would find evidence of cue-taking in an analysis of validated voters. In all, then, analyzing constituents with lower levels of political knowledge will increase the chances I find effects of elite cueing, and not finding those effects (which, as it turns out, I do not) will provide strong evidence that crime does not function like lesser-raced issues.

I clean all three years of data the same way so my analyses will be comparable across years. First, I code vote choice as an indicator variable to capture whether a voter supported that

year's Republican candidate (coded one if the respondent reported an intent to vote Republican or zero otherwise).

Second, following Mendelberg (2001), I code two indicator variables to operationalize constituents' perceptions of crime: when asked what they considered to be the three most important problems in their districts and in the country, if respondents mentioned anything related to crime (e.g., lack of law and order, gangs, terrorism), I code the variables one, and if respondents did not mention anything related to crime, I code the variables zero.

The 1984 ANES did not ask respondents what they considered to be the most important problems in their districts, however, so I have information about whether respondents perceived crime to be an important district-level problem only for 1988 and 1992. All three ANES waves asked about important problems for the U.S. as a whole, so I have information from all three years about whether constituents considered crime to be an important national problem.

Specifically, the ANES asked respondents the following questions and allowed them to provide three responses to each. To capture constituents' perceptions of district-level issues, the ANES asked, "In the campaign in this district for the U.S. House of Representatives, what would you say was the single most important issue to you?" To capture constituents' perceptions of national-level issues, the ANES asked, "What do you think are the most important problems facing this country?" When answering, constituents were able to select issues from an expansive list of options, and as the ANES asked the district-focused question before the country-focused question (in 1988 and 1992 when it asked both), it is unlikely the survey's organization primed respondents to consider national-level problems to be problems in their districts as well.

Third, I create a variable to capture constituents' attention to elite cues on crime. When discussing crime-focused cue-givers, Enns (2016, p. 51) writes, "the public is most likely to

notice national-level political discourse, [so a] focus on prominent national politicians offers a best case scenario for observing elite influence.” Following that idea and given crime was a centerpiece of the 1988 presidential election, I operationalize attention to elite cues about crime by looking to the attention constituents paid to the three years’ presidential campaigns.

The ANES asked respondents to rate how much attention they paid to the campaigns on television, in newspapers, on the radio, and in magazines; I scale their responses to each question from zero to one then select the maximum value of all four. I do this for general campaign attention – i.e., I do not party-code the measure. For example, a constituent who reported paying the most attention possible on one medium and no attention on the others would receive a value of one, and a constituent who paid the median level of attention on one medium and less attention on the others would receive a value equal to that median value. This measure is preferable to an equally-weighted index of the four values given it does not assume constituents need to have paid attention to more than one medium to be cued on crime.⁵

Finally, I code variables for constituents’ demographics, partisanship, and ideologies.⁶ Other than for respondents’ ages and family incomes, I code the variables as indicators taking the value one if the respondent falls into that category or zero otherwise. I code age as a continuous variable, and I code a variable for respondents’ family incomes as continuous (ranging from one

⁵ As I describe in footnote 14, as a robustness check, I repeat my analysis of campaign attention using an equally-weighted index rather than this maximum value variable, and I draw the same conclusions.

⁶ My demographic controls include indicator variables for respondents’ levels of education (base category is having a college degree), race (base category is being white), occupations (base category is having a job other than homemaking, doing clerical work, or being another form of professional), religion (base category is not knowing one’s religion), ethnicity (coded as one if the respondent is Spanish or Hispanic), gender (coded as one if the respondent is female), and multiple more-straightforward indicator variables coded as one when constituents fall into the respective categories: married or living with a partner, owning a home, being retired, being a union member or living in the same household as a union member, and living in a rural area. For partisanship, the base category is when constituents reported being Independents, apolitical, or members of another political party (relative to being weak or strong Democrats or Republicans). For ideology, the base category is being moderate or not having thought much about ideology (relative to being extremely or somewhat liberal or conservative).

to twenty-two) and taking ordinal values to reflect the bins into which each respondent's income falls per the 1984 ANES income scale. For example, a respondent making under \$3,000 is coded as one, a respondent making between \$3,000 and \$4,999 is coded as two, and so on, up to a respondent making \$75,000 or more being coded as twenty-two.

My second data source is the FBI's UCR Program (Kaplan, 2022a), and I use its Offenses Known and Clearances by Arrest dataset, "the main measure of crime in the United States ... [because it] includes the monthly number of crimes reported to the police or otherwise known to the police" (Kaplan, 2022b, sec. 2.3.1). Local law enforcement agencies voluntarily send crime reports to the Federal Bureau of Investigation, and the FBI concatenates and disseminates this overall dataset. The unit of observation is agency-month: for each month, the data reports the number of violent crimes known to various law enforcement agencies as well as the size of the population under each agency's purview.⁷ Each row, then, pertains to one agency for one month, yet given the finest resolution I have for ANES respondents' locations is their counties, I aggregate these reported crime incidents by the counties in which they took place. Theoretically, looking for effects of crime within individuals' counties makes sense given local events are likely to be salient to constituents (Enos et al., 2019; Gillion, 2020; Healy et al., 2010; Margalit, 2011).

I calculate county-level per capita violent crime rates during the six months before each of the three presidential elections (May through October of each respective year). I look across

⁷ These crime statistics are not straightforward counts of incidents of crime: if a crime is reported then later discovered to be unfounded, the data includes a negative number at some point after it counted the first report of the crime (Kaplan, 2022b, sec. 2.2.1). For example, if the only crime reported in August of some year were someone thinking his wallet were stolen, the number of crimes in the UCR data for August would be one. If the man were to find his wallet in September and no other crimes were reported, the number of crimes reported in the data for September would be negative one. Together, these values would sum to zero, indicating no crimes were committed over August and September of that year. Therefore, averaged over a long enough period of time – sometimes months, sometimes even multiple years – there should be no negative numbers in the UCR data. The crime rates I calculate all are at least zero, indicating I do not face that issue in my analysis.

these months given recent events are more salient to constituents than older events (Achen & Bartels, 2004, 2017; Fair, 1978; Kramer, 1971; Tufte, 1978), so looking at crimes committed more recently are most likely to show impacts on constituents' perceptions of crime and vote choices. I calculate these crime rates beginning with two sums by county for those six months before each election. First, I sum all incidents of reported violent crime.⁸ Second, I sum the number of people under each agency's purview.⁹ I divide that population summation by six to make it the county's average population across those six pre-election months, then I divide the summation of all criminal incidents in a county by that county's average population across those six months. This yields county-level per capita violent crime rates averaged across the six months before each of the three presidential races. I match these crime rates to ANES respondents by year and the respondents' counties.

An important clarification is that these crime rates are not measures of actual incidences of crime – they instead measure reported crime, and the two are not always identical. First, police do not find out about every crime, such as victims not contacting law enforcement if they do not trust the police to fix the harm done or if the crimes were of certain limited magnitudes or outcomes (Finkelhor & Ormrod, 2001; Gutierrez & Kirk, 2017; Myers, 1980; Pezzella et al., 2019; Skogan, 1976; Sparks et al., 1977; U.S. National Advisory Commission on Criminal Justice Standards and Goals, 1973).

On the other hand, for crimes brought to the attention of law enforcement, police may see acts as suspect yet not necessarily consider them illegal. For example, Stump (2011) shows officers sometimes create and follow their own definitions of hate crimes, resulting in some hate

⁸ I count any incident of homicide, rape, rape, robbery, aggravated assault, burglary, theft (both of items other than motor vehicles and of motor vehicles themselves), and simple assault (Kaplan, 2022b, sec. 2.1.1).

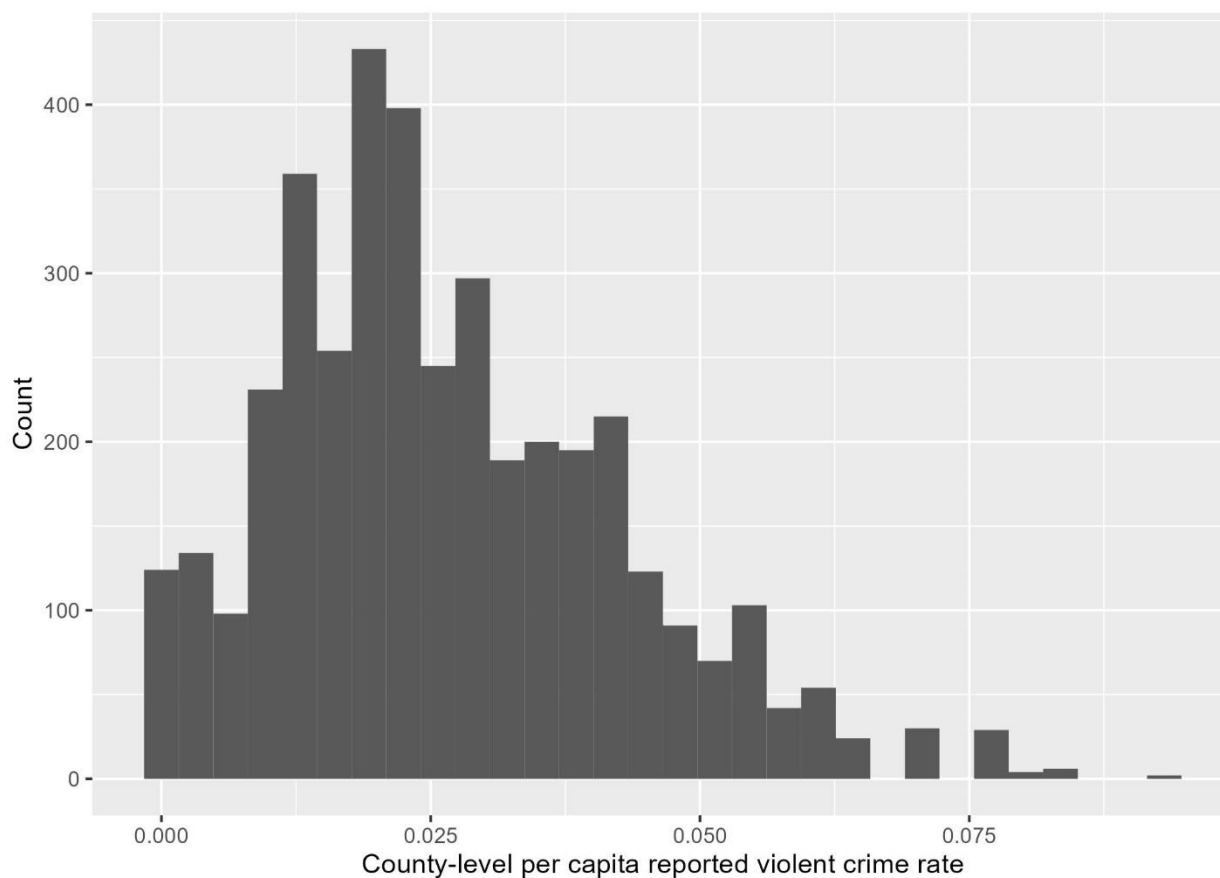
⁹ To find counties' populations, I use the UCR data – as compared to another source like the decennial Census – given the UCR data is updated monthly.

crimes (those not in accordance with officers' definitions) going uninvestigated and potentially being left out of the UCR data as a result. Finally, police may know crimes have been committed yet be unable to pursue the criminals; this could result from their being busy with other cases and not having the resources to mount investigations (Goodman, 1996) or because they recognize pursuing the criminals would infringe on other citizens' personal liberties (O. W. Wilson, 1963).

Above all else, local law enforcement agencies are not strictly required to submit information to the FBI – and may choose not to do so out of a desire to avoid appearing unsuccessful at their jobs – so the UCR data reflects only a subset of all crimes (S. J. Cook & Fortunato, 2023; Kaplan, 2022b). Hence, the data should be taken as a lower bound of actual crime incidents, thereby making my calculated crime rates lower bounds of actual crime rates. Though the UCR data is imperfect, it nonetheless provides “the best available over-time indication of violent ... crime in the United States ... [while being] particularly well suited for evaluating long-term patterns [in crime]” (Enns, 2016, pp. 11–12). More broadly, “recorded crime rates have a special significance insofar as they are the rates most often discussed in the media [and presented to the public]” (Garland, 2001, n. 4.40).

The following figure plots the distribution of these county-level per capita crime rates for each constituent:

Figure 2. Distribution of crime rates for all constituents.



Though the distribution is not perfectly normal, taking the logarithm of these crime rates is not a strong improvement to the data's normality, so I use this level distribution for my analysis.

My third and fourth data sources are the U.S. Bureau of Economic Analysis (n.d.) and the World Bank & Organisation for Economic Co-operation and Development (n.d.). I take the BEA's data on counties' nominal per capita incomes, convert them to real dollars using the GDP deflators reported by the World Bank & OECD (the base year is 2015), then calculate the change in each county's real per capita income between the year prior to the presidential election and the

election year itself.¹⁰ This provides a measure of the change in each county's economy leading up to each election, a useful control in any study of vote choice. I match this data to ANES respondents by year and the respondents' counties.

Fifth and finally, I use data from the U.S. Census Bureau (1993, 1998) and the UCR Program to calculate the percentage of each county who is Black. The Census Bureau's data reports estimates for the number of constituents in each county who were Black as of July 1 of each year, and I divide those estimates by each county's population for July (I calculated the county-level populations using the UCR data as described above).¹¹ I will use these calculations as controls in my analysis, and I describe why in the next section. Finally, I match these percentages to ANES respondents by year and the respondents' counties.

All this results in three final datasets: ANES polling data from 1984, 1988, and 1992 with data about county-level per capita reported violent crime rates, changes to county-level per capita real incomes, and the percentages of each county who are Black calculated from other sources and matched to ANES respondents by year and county. Each of these datasets takes individual ANES respondents as its unit of observation.

¹⁰ I use these two sources because they allow me to calculate these statistics by county going back to the 1980s. Other sources (e.g., Economic Census, American Community Survey, Community Population Survey) do not provide data with the same level of specificity or for the years I study.

¹¹ I use this data from the Census Bureau because these counts updated every year, and other sources (e.g., the decennial Census) do not update as frequently.

SECTION FIVE

Empirical Analysis and Discussion

Basic Empirical Strategy

To test my hypotheses, I will estimate numerous models across all three election years, and as I coded each year's data the same way, any differences in my results will correlate with differences between the years. Each model has the same basic structure of explanatory variables: I regress some dependent variable on continuous variables for constituents' ages and the squares of their ages, my variable for constituents' family incomes, indicator variables for constituents' other demographics, partisanship, and ideologies (see footnote 6); my two county-level controls, and – most frequently of interest – my variable for county-level per capita reported violent crime rates.

I include these controls for respondents' demographics, partisanship, ideologies, and county characteristics given extant research shows constituents' political behavior varies systematically by demographic, partisan, and ideological groups (e.g., Burns et al., 2001; Conway, 2000; Rosenstone & Hansen, 1993; Teixeira, 2011; Verba et al., 1995; Walsh, 2012). I include the change in a county's income from the prior year given "diffuse anxieties" about economic and social deterioration can influence constituents' attitudes toward crime (Britto, 2013; Costelloe et al., 2009; Dowler, 2003; Garland, 2001; Gottschalk, 2014; Hollway & Jefferson, 1997). I include the percent of each county who is Black given the number of Black people nearby can influence how much other constituents fear crime (Chiricos et al., 2001; Liska et al., 1982; Pickett et al., 2012; Taylor & Covington, 1993). Furthermore, I include in each model state-level fixed effects given no two states are the same and there may be unobservable,

state-level factors that influence constituents' political behavior – e.g., states' political cultures or crime policies (Elazar, 1966; Gunther, 1997; Johnson, 1976; Key & Heard, 1949; Lieske, 2010; S. C. Patterson, 1968; Sharkansky, 1969).

Finally, for all my analyses, I will report HC1 robust standard errors clustered at the county level and weight my models when possible. The 1984 ANES dataset does not include weights given the ANES staff found calculating them to be more work than the benefit gained from using them, so I do not weight my analyses of that year.¹² I weight my 1988 analyses by the number of eligible adults in each respondent's household and my 1992 analyses using the full sample weights included in that year's dataset.

Analyzing Perceptions of Crime

To test my first three hypotheses, I build off my basic empirical strategy, with each model taking as its dependent variable one of my indicator variables for whether constituents considered crime to be an important problem. Given I have two measures of crime perceptions – one for crime as a district-level issue and one for crime as a national issue – I run my models twice, allowing me to estimate the relationships between the same explanatory variables and both dependent variables.

For each year and each dependent variable, then, I begin by regressing the most important problem (MIP) indicators on my basic model structure as described. Second, I add to my models my scaled variable for constituents' campaign attention and the interaction between that variable

¹² From the 1984 ANES' introduction (W. E. Miller & National Election Studies, 1999a, p. 8): "Technically, respondents ought to be weighted in all analyses by the inverse of their selection probability. NES staff has traditionally compared frequencies run weighted and unweighted on variables of interest and concluded that the differences in distributions (typically very minor) do not justify the added complexities of weighting. This is the case for 1984 as well." See Traugott (1985) for those weighted, unweighted, and approximately-identical analyses.

and my variable for crime rates; this will allow me to observe whether the correlations between crime rates and perceptions of crime vary across levels of attention to the presidential campaigns. In the following tables, I report excerpts from my estimates relevant to my respective hypothesis tests, and I report the full outputs in Appendix A.

The results from my models with district-level perceptions of crime as the dependent variable are as follows:

Table 1. Crime as a district MIP (excerpt).

	<i>Dependent variable:</i>			
	Crime as a district MIP			
	1988		1992	
	(1)	(2)	(3)	(4)
Crime rate	-1.704** (0.670)	-2.295*** (0.722)	0.085 (0.306)	0.087 (0.459)
Campaign attention		0.001 (0.018)		-0.004 (0.010)
Crime rate * campaign attention		0.956 (0.650)		0.008 (0.530)
Constant	-0.045 (0.047)	-0.038 (0.049)	-0.027 (0.034)	-0.026 (0.031)
Controls?	Yes	Yes	Yes	Yes
Observations	1,250	1,250	1,345	1,345
R ²	0.101	0.103	0.095	0.095

Note:

* p<0.1; ** p<0.05; *** p<0.01

All models contain state-level fixed effects, and standard errors are robust (HC1) and clustered at the county level. See Table 5 in Appendix A for full regression results.

The first two models are for 1988, and the second two are for 1992.

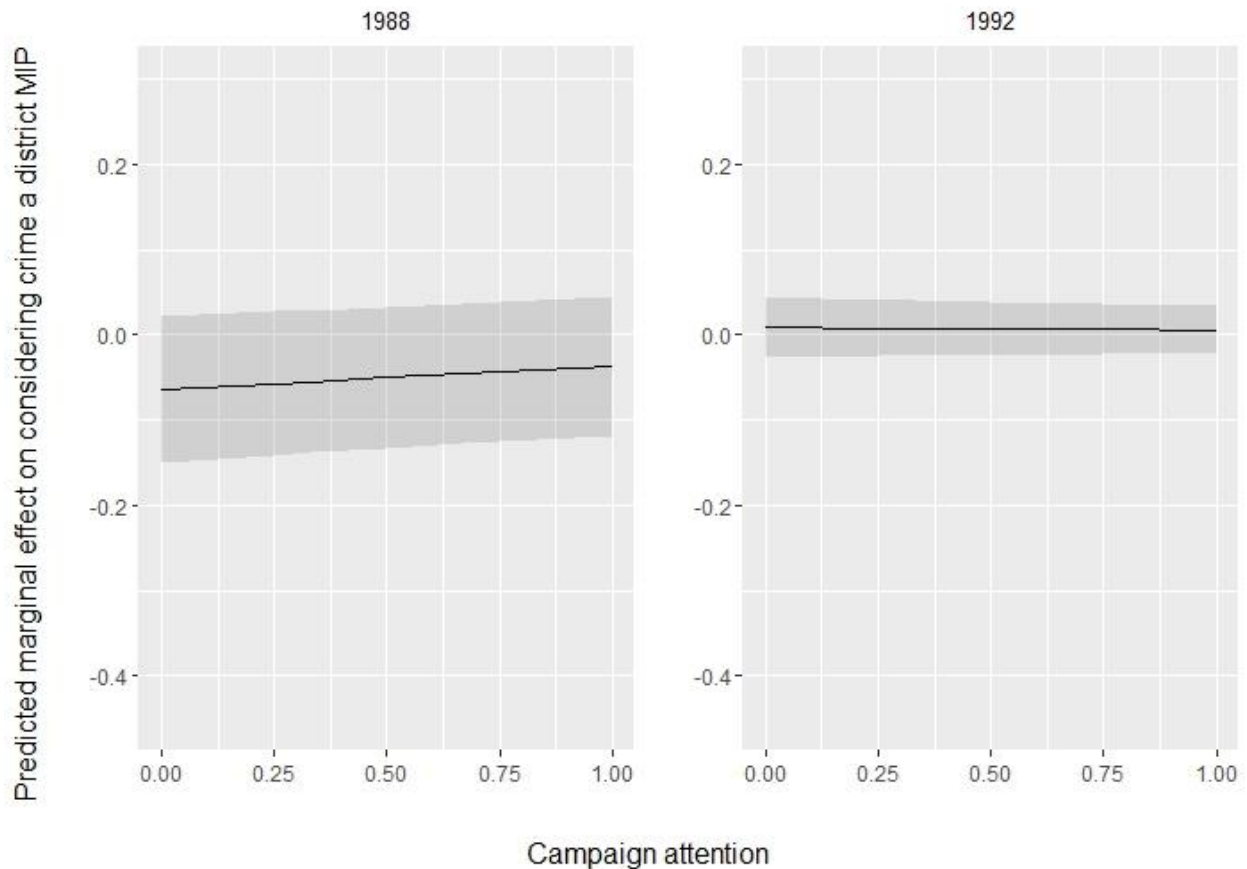
I begin by examining my first and third models to test H1a (that local crime led constituents to consider crime an important district-level problem). My results reject the hypothesis. Per my first model, for 1988, an increase in crime correlates with a significant decrease in the chance constituents considered crime an important district-level problem. Per my third model, for 1992, an increase in crime correlates with a small insignificant increase in the chance constituents considered crime an important district-level problem.

To test H1b (that the relationship between incidences and perceptions of crime was unique in 1988), I conduct a Z-test of whether the corresponding *Crime rate* coefficients in my first and third models are distinct. Following Clogg et al. (1995), I calculate this Z-score by dividing the difference between the two estimates by the square root of the sum of each estimate's standard error squared. Ultimately, I fail to reject the null hypothesis that the difference between the estimates is insignificant, providing further evidence against H1 and the idea crime aligns with other, lesser-raced issues studied in the traditional public opinion paradigm. I discuss these results in greater detail after testing H2 and H3.

I continue my examination of perceptions of crime on the district level by testing H3a (that there were positive and multiplicative effects of crime and campaign attention on constituents' perceptions of crime as an important district-level problem in 1988) and H3d (that those effects did not exist in 1992). Given the estimates of the interaction terms for both years are statistically insignificant, I reject H3a and fail to reject H3f. The point estimate for the 1988 interaction term is positive and substantively significant, however, lending some small level of support to H3a. The corresponding estimate for 1992 is substantively insignificant, lending support to H3d.

To further my analysis, in Figure 3, I plot the predicted marginal effects of campaign attention on district-level perceptions of crime (holding all continuous variables at their means or examining constituents fitting into the base categories for categorical variables). I also plot the effects' 95% confidence intervals.¹³

Figure 3. Predicted marginal effect of campaign attention on crime as a district MIP.



This figure illustrates an interesting contrast between the two years: though the predicted effects in both years are insignificant across their entire domains, the predicted probabilities for 1992 are

¹³ I use 95% confidence intervals given I intend this plot to illustrate the predicted marginal effects for each year separately, not to facilitate an analysis of whether there is overlap between the years' confidence intervals (see Maghsoodloo & Huang, 2010). I do the same for Figure 4.

centered around zero and are nearly identical (i.e., the line appears flat), whereas the predicted probabilities for 1988 are not centered as closely around zero and increase monotonically.

What explains these insignificant multiplicative effects? One rationale could be about the foci of each election. For 1988, it is possible the media environment’s uber-saturation with crime led to all constituents being “treated” regardless of their levels of campaign attention.

Additionally, for 1992, given the election was not about crime, it would not make much sense for there to have been this form of multiplicative effect in the first place. I discuss the remainder of this first batch of findings in greater detail after testing H2 and the rest of H3.

Having examined crime perceptions on the district level, I transition to an analysis of crime perceptions on the national level. To test my hypotheses about perceptions of crime as a national problem, I estimate slight variations of the models I reported in Table 1. Now, I use as my dependent variable my indicator for whether constituents mentioned crime as one of the most important problems on the national level. I leave my explanatory variables unchanged. My results are as follows:

Table 2. Crime as a country MIP (excerpt).

	<i>Dependent variable:</i>					
	Crime as a country MIP					
	1984	1988		1992		
(1)	(2)	(3)	(4)	(5)	(6)	
Crime rate	-0.148 (0.644)	2.974** (1.441)	-4.968*** (1.734)	-2.789 (2.065)	0.138 (0.875)	0.689 (1.409)
Campaign attention		0.074* (0.040)		0.138** (0.055)		-0.003 (0.047)
Crime rate * campaign attention		-3.806** (1.583)		-3.271* (1.872)		-0.774 (1.684)

Constant	-0.041 (0.041)	-0.108* (0.055)	-0.017 (0.139)	-0.091 (0.145)	0.080 (0.098)	0.073 (0.100)
Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,306	1,306	1,250	1,250	1,345	1,345
R ²	0.058	0.064	0.094	0.096	0.086	0.086

Note: *p<0.1; **p<0.05; ***p<0.01
All models contain state-level fixed effects, and standard errors are robust (HC1) and clustered at the county level. See Table 6 in Appendix A for full regression results.

The first two models are for 1984, the middle two models are for 1988, and the last two models are for 1992.

I begin by testing H2 (that local crime led constituents to consider crime an important national problem in 1988 alone), starting with the relationship between crime and its perceptions in 1988. In my third model, which captures the overall relationship between crime rates and whether constituents perceived crime as a national problem, the estimate for *Crime rate* is negative, substantively significant, and statistically significant, indicating constituents living in higher-crime areas were less likely to report crime as being an important national problem. This rejects H2a. In my first and fifth models (for 1984 and 1992, respectively), I find insignificant estimates for this correlation, failing to reject H2b and H2c.

To test whether this estimate for 1988 is different from the corresponding 1984 and 1992 estimates, I conduct Z-tests for *Crime rate* similar to the ones described earlier, and each test rejects the null hypothesis of an insignificant difference between the 1988 and 1984 or 1992 estimates – i.e., my tests show the estimate for 1988 is statistically different from the corresponding 1984 and 1992 estimates. Given the 1988 correlation between incidences and

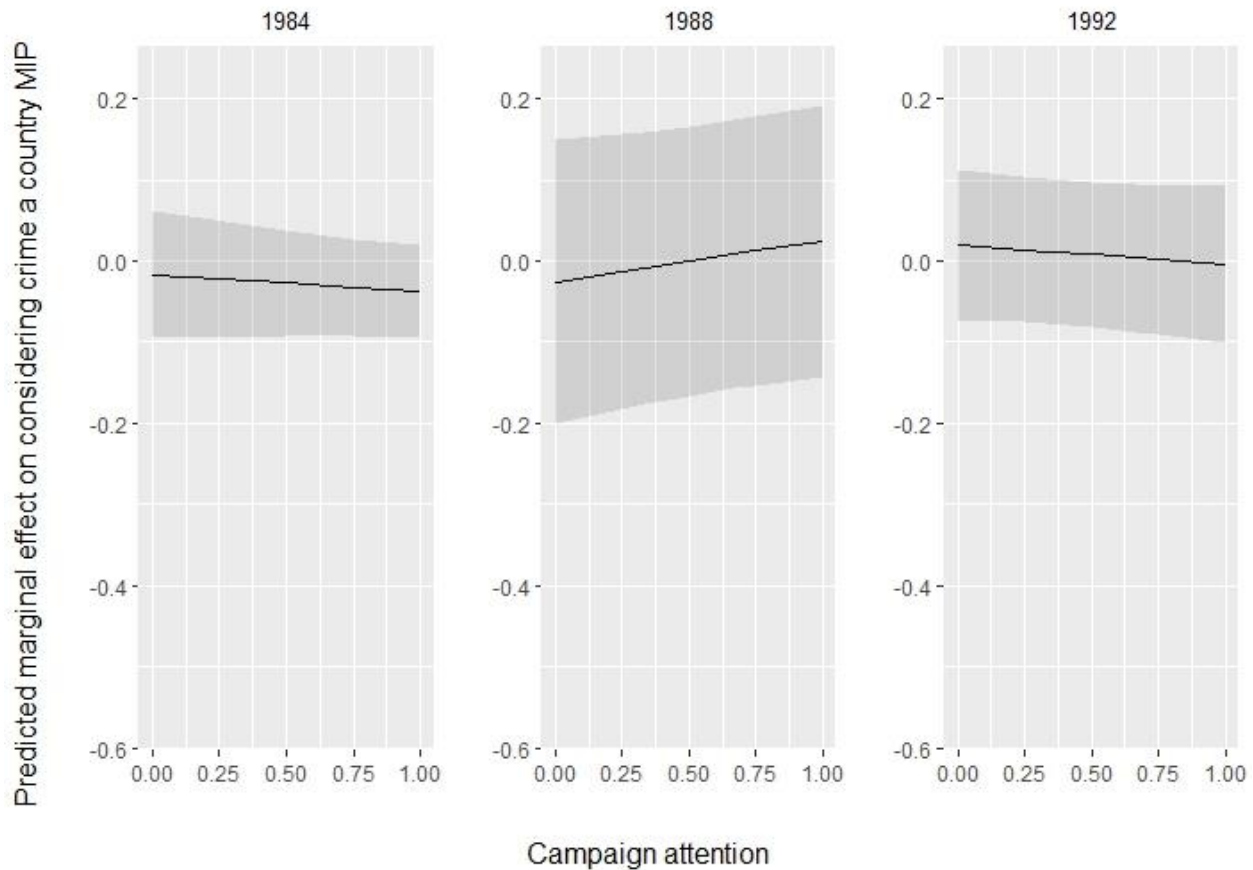
national perceptions of crime is negative, however, this significant difference does not lend much support to my theory. I discuss these results in greater depth after finishing my test of H3.

I now turn to H3b, H3c, and H3f (that there were positive multiplicative effects of local crime and campaign attention on constituents' perceptions of crime as a country-level problem in 1988 alone). I do not find statistically significant multiplicative effects of crime rates and campaign attention on crime perceptions in 1988 or 1992, respectively rejecting H3b and failing to reject H3f. A Z-test suggests an insignificant difference between the interaction effects in 1988 and 1992.

Interestingly, the estimate for the interaction effect in 1984 is negative, statistically significant, and substantively significant, rejecting H3c. Similar to the test on the estimates for 1988 and 1992, however, a Z-test of the difference between the estimates for the interaction effects in 1984 and 1992 suggests an insignificant difference, indicating this finding about 1984 is not as theory-shattering as it first appears.

Like for my analyses of H3a and H3d, in Figure 4, I plot the predicted marginal effects of campaign attention for each year and with 95% confidence intervals.

Figure 4. Predicted marginal effect of campaign attention on crime as a country MIP.



Similar to Figure 3, my analysis of 1988 is the only one to yield a positively-sloped line of marginal effects with respect to campaign attention, suggesting some small level of potential alignment with my theory about attention to crime-focused elite cues increasing the salience of crime to constituents. Qualifying this, the main interactive estimate’s insignificance (see Table 2) nonetheless rejected H3b.

Two factors may explain these multiplicative effects. First, like I proposed earlier, it is possible all constituents were “treated” by the 1988 election’s uber-saturation with crime rhetoric, leaving no room for additional effects from campaign attention. Second, given crime is an issue ultimately delegated more so to state and local governments than the federal government

(e.g., Richman, 2006; Zimring & Hawkins, 1996), it is possible constituents were less likely to consider crime an important national problem regardless of the amount of attention they paid to the campaigns. My results for 1992 may align with this theory; I discuss these ideas in greater detail in the following paragraphs.

In summary, my tests of H1 – H3 yield the following conclusions:

- When analyzing the U.S. as a whole, I do not find evidence of positive and significant correlations between incidences and perceptions of crime in 1984 and 1992. I find negative and significant correlations between incidences and perceptions of crime in 1988.
 - For both levels of issue attribution, I find these 1988 correlations to be statistically different from the corresponding 1984 and 1992 correlations. Given the signs of the 1988 correlations are negative, however, this uniqueness does not do much to bolster the credibility of my theory.
- When analyzing constituents who paid nonzero amounts of attention to the presidential campaigns, for all my models except for perceptions of crime as a country-level problem in 1984, I find no significant multiplicative effects of crime rates and campaign attention on perceptions of crime as an important problem. That one significant multiplicative effect is negative and statistically indistinguishable from the corresponding correlation in 1988, so it does not provide strong evidence against my theory.

Interestingly, the majority of these findings go against my theory.¹⁴

¹⁴ To test the robustness of my conclusions thus far (and those of my forthcoming tests of the effects of crime on vote choice, H4 and H5), I re-run my analyses under a variety of other specifications, each time drawing conclusions largely in line with what I report in the main body of this thesis.

First, people can become desensitized to crime (Di Tella et al., 2019; McCart et al., 2007; Scharrer, 2008), and that desensitization might lead constituents to stop considering high levels of crime an important problem. To counter this possibility and given crime is seasonal across years (e.g., Albanese, 1990; Rock et al., 2003), I calculate crime rates for May through October of the year before the election year, take the difference between both crime rates, and re-estimate my models with these differenced rates. The results align with my main findings.

Second, given the raced nature of crime, white constituents were most likely to be positively affected by Bush's hard-on-crime rhetoric (e.g., Mendelberg, 1997), so I filter out all non-white constituents and re-run my analyses. So doing allows me to sharpen my results by eliminating any effects of Bush's hard-on-crime rhetoric; this should create an easier test for traditional conceptions of public opinion and retrospective voting. The results align with my main findings.

Third, as I discussed in section four, UCR data – and therefore my calculated crime rates – are biased downward, frequently from law enforcement agencies failing to report any crime statistics and therefore having their areas of responsibility coded as having had zero criminal incidents (Kaplan, 2022b). Cook & Fortunato (2023) study when agencies report data to the UCR Program and find state legislative capacity is positively related to data reporting behavior, so I re-run my analyses on constituents whose states' legislative capacities were above the median for each respective election year (I use Cook & Fortunato's quantification of states' legislative capacities). The results align with my main findings. It also is worth noting the data reported by higher-capacity, UCR-complying states still may be (and likely are) underreports of criminal incidents, however – Cook & Fortunato show having a higher state legislative capacity increases the chances that state's agencies comply with UCR data requests, not that those compliances are necessarily more transparent.

What explains these results? I suggest four potential rationales, each revolving around the conditions under which constituents follow elite cues. First, certain issues – like crime – are “easy” (Carmines & Stimson, 1980), and constituents are better able to formulate preferences for these kinds of issues (Carmines & Kuklinski, 1990; Cobb & Kuklinski, 1997; Lupia, 1994; Ratneshwar & Chaiken, 1991). Despite that ability, however, they still can have issues made salient by elite cues, so this might explain why my results for 1988 are statistically different than those from 1984 and 1992 for the most part, but it does not explain the significant correlations’ signs.

A second potential explanation stems from the idea elite cues can spark backlash from constituents of the opposite party (Berinsky, 2009; Goren et al., 2009; Levendusky, 2010; Merkley & Stecula, 2018, 2021; Nicholson, 2012). Given Democrats tend to live in urban areas (Mummolo & Nall, 2017; Nall, 2015) and urban areas have higher crime rates than rural areas (Boggs, 1965; Danziger, 1976; Glaeser & Sacerdote, 1999), it is possible Bush’s hard-on-crime rhetoric in 1988 pushed Democrats away from reporting it as a most important problem; this could explain the negative correlations between incidences and perceptions of crime from my analyses of all constituents. A way to test this revised theory would be to estimate my models

Fourth, it is possible constituents who paid attention to the presidential campaigns across more media were more cued than constituents who paid lots of attention only to one medium, so I re-test H3 using an equally-weighted index of constituents’ campaign attention across all four media. The results align with my main findings.

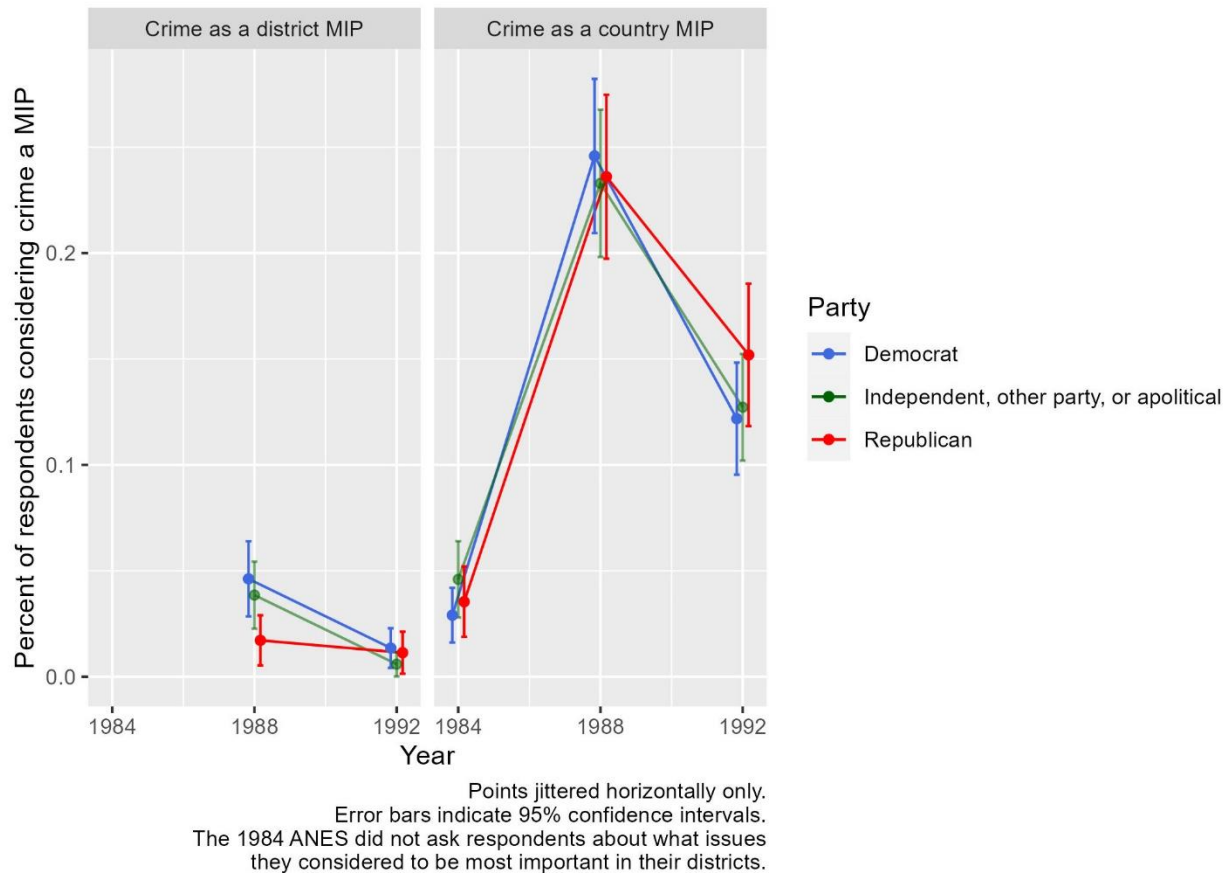
Fifth, though I proposed a theoretical reason in section two for analyzing all constituents rather than validated voters only, I nonetheless re-run my analyses exclusively on voters. For 1984 and 1988, I filter out constituents whose voting records the ANES staff was unable to find and verify. For 1992, however, the ANES staff did not validate turnout, so I exclude constituents who did not self-report voting in the election. The results align with my main findings.

Sixth, given how many individual-level controls I include in my analyses, it seems possible some of my null results may come not from truly-null relationships between my independent and dependent variables but rather from my using so many degrees of freedom to estimate my controls. As a result, I re-run my analyses without controlling for individuals’ demographic, partisan, or ideological groups. The results align with my main findings.

separately on Democrats and Republicans, but I am unable to do so (this analysis yields mostly null results).

A second-best way to analyze the groups separately is to compute the percentages of constituents of each party who considered crime an important problem in each year. If a lower percentage of Democrats considered crime an important problem in 1988, the results will lend support to this explanation, yet as this essentially will be a naïve comparison of means, these percentages could be biased by other factors (e.g., I am unsure if I would find the same results if I were to control for crime rates and constituents' races as well). Therefore, if a higher percentage of Democrats considered crime an important problem in 1988 – suggesting a potentially-greater cueing effect on Democrats than Republicans – the results would not necessarily provide evidence against this backlash explanation. Put another way, this comparison is a straw-in-the-wind test (Bennett, 2010), as it will provide neither necessary nor sufficient evidence to reject or fail to reject this explanation. I illustrate the percentages of respondents who considered crime an important problem in each year (and those percentages' 95% confidence intervals) in Figure 5.

Figure 5. Percentages of respondents considering crime a MIP by year and MIP level.



In 1988, a higher percentage of Democrats than Republicans cited crime as an important problem on both the district and country levels. To examine whether these percentages are statistically distinct, I conduct Z-tests of them by dividing the difference in percentages of Democrats and Republicans who considered crime an important problem by the square root of the sum of each party's variance divided by its sample size. Unsurprisingly given the figure, only the 1988 percentages of Democrats and Republicans considering crime an important district-level problem were distinct from one another. As a result, backlash to Bush's cues is unlikely to be the cause of the negative correlations I find, though I cannot draw this conclusion definitively.

An alternate explanation for the negative correlations has to do with constituents learning about crime as they began to consider it more salient. Relative to being indirectly affected by an

issue, being directly affected by it results in constituents forming stronger attitudes and seeking additional information about it (Dennison, 2019; e.g., Fazio & Zanna, 1981; Fortner et al., 2010; J. M. Miller et al., 2017; Weaver, 1991), so relative to constituents who – for example – thought about crime only because of the presidential campaigns, constituents who lived in high-crime areas may have thought about crime in a unique way. Hence, it is possible those living in high-crime areas – regardless of partisan affiliation – became less likely to consider crime a national-level problem because they learned crime-fighting is delegated mainly to state and local governments (e.g., Richman, 2006; Zimring & Hawkins, 1996). In all, then increasing incidences of crime could cause constituents to attribute less responsibility for crime to the federal government.

My results do not exactly comport with this explanation, however. Given this would be a time-invariant, cue-independent learning effect, I would expect a positive and significant correlation between crime rates and perceptions of crime as a district-level problem in 1992 (the estimate for *Crime rate* in my third model in Table 1) and negative and significant correlations between crime rates and perceptions of crime as a national problem in 1984 and 1992 (the estimates for *Crime rate* in my first and fifth models in Table 2). I do find a positive – albeit insignificant – correlation between crime rates and perceptions of crime as a district-level problem in 1992 and a negative – albeit also insignificant – correlation between crime rates and perceptions of crime as a national problem in 1984, partially aligning with this explanation. In 1992, however, I find a positive – though still insignificant – correlation between crime rates and perceptions of crime as a national problem, countering this explanation. It is possible 1992 saw leftover effects from Bush’s 1988 rhetoric painting local crime as an issue concerning the president and therefore the entire country; if this were the case, the positive – though

insignificant – correlation for national perceptions of crime in 1992 would make sense. It could be the case constituents still were likely to consider crime a district-level issue in 1992 – explaining that positive point estimate – despite the 1988 election having added a focus of crime as a national issue to constituents’ minds. Overall, the two positive correlations are not necessarily at odds with one another.

How plausible is this explanation? To answer this question, I conduct something of a difference-in-differences in spirit, though the evidence it provides is circumstantial at best. Holding constant the year and varying the level of issue attribution, the 1992 point estimate for the correlation between crime rates and the chance constituents considered crime an important *country-level* problem is approximately 1.6 times that of the 1992 point estimate for considerations of crime as a *district-level* problem. In comparison, varying the year and holding constant the level of attribution, the 1992 point estimate for the correlation between crime rates and the chance constituents considered crime an important country-level problem is only approximately 0.7 times the 1984 point estimate for considerations of crime as a country-level problem. Given the change in scale across levels of issue attribution is larger than the change in scale across years (each time holding the other variable constant), this brief analysis suggests post-1988 issue attribution for crime was not directed more strongly at the country overall (relative to individuals’ districts).

How does this apply to 1988 specifically? It is possible Bush’s rhetoric shifted perceptions of crime away from the district level – this would explain the unique negative correlation I find between crime rates and perceptions of crime as a district-level problem – though it does not address why there is also a negative correlation between crime rates and perceptions of crime as a national problem. Perhaps the resolution lies somewhere in connection

with the learning effect I described earlier: it is possible the 1988 election simultaneously shifted perceptions of the onus of handling crime away from the district level while a simultaneous and competing learning effect led constituents not to consider crime a national problem. If this were the case, Bush's rhetoric could explain why the 1988 correlation between crime rates and perceptions of crime as a national problem was distinct from the 1984 and 1992 correlations but could not explain its sign and significance alone. All this is a preliminary theory, and a more-robust analysis may produce findings different from the ones I speculate there would be.

A final examination of my results revolves around the idea the Horton ad and 1988 election's overall focus, both about crime at face value, actually were little more than veiled racial appeals – i.e., these seemingly-counterintuitive results could stem from the fact crime does not function like lesser-raced issues. Mendelberg (1997, 2001), for example, advances this view, finding the main impact of the Horton ad was on racial resentment, not the salience of crime.

Mendelberg's work is not sufficient to explain my results, though our findings are not necessarily at odds with one another. I find a negative and significant correlation between local incidences of crime and concerns about crime as a national problem in 1988, and I show it to be statistically different than the corresponding correlations in 1984 and 1992, suggesting something unique to 1988 impacted the relationship between incidences and perceptions of crime as a national problem. Setting aside the correlation being negative, if the differences being significant were not due to the Horton ad, what could explain them? Mendelberg (2001, p. 180) does show "worry about crime is ... almost entirely unassociated ... [with] racial resentment," so these results likely were not driven by racial resentment; if they were, I would have expected the

correlations to be positive.¹⁵ So, I again presume the 1988 election's focus on crime had something to do with these results.

Finally, Mendelberg did show the Horton ad did not increase crime's salience, however, though two key differences distinguish our work. First, when Mendelberg (1997, n. 10) studied its effect through a lab experiment, she did so during the spring of 1992, so it is possible her subjects had been "treated" by the ad when it first ran on television. This could have changed the baseline salience of crime in her subjects' minds, so the treatment group's seeing the ad a second time could have had no discernable effect given her control group could have been treated years prior. This explanation also would align with my finding of null multiplicative effects of campaign attention and local crime rates on constituents' perceptions of crime in 1988 – seeing the ad may not have done anything new given constituents may already have been cued. Second, when Mendelberg (2001, Chapter 6) performed an analysis of observational data (which my analysis echoes), she analyzed the 1988 ANES data across three different periods of the Horton ad's presence: low exposure and implicit messaging, high exposure and implicit messaging, high exposure and explicit messaging. It is possible her segmenting of the data and my lumping it together could explain our seemingly-different conclusions about the nature of the Horton ad and 1988 election.

Analyzing Vote Choice

To test my two hypotheses about the relationship between crime and constituents' vote choices, I first estimate models of constituents' vote choices as functions of the explanatory

¹⁵ As can be seen in my full regression outputs in Appendix A, in 1988 alone, an increase in the percent of a constituent's county who was Black had a positive and significant correlation with constituents considering crime to be an important problem on both the district and national levels. These results potentially align with Mendelberg's findings that the Horton ad stoked racial resentment against Black people.

variables in my basic model structure. Second, I re-estimate those models two times, each time adding one of my indicator variables for perceptions of crime.

Building up these models will allow me to study whether perceptions of crime may mediate the relationship between incidences of crime and vote choice, yet one objection to these specifications may stem from the timing of the “most important problem” questions in the ANES polls. The questions were not asked until the surveys’ post-election waves, so constituents could have voted before – not after – being surveyed and reporting their views and attributions of crime. As a result, constituents’ vote choices – which they expressed before the post-election ANES waves – could have altered their perceptions of crime, similar to how engaging in other acts of political participation may affect those same individuals’ issue views (McAdam, 1989; Pop-Eleches et al., 2021).

Even if the data on constituents’ perceptions of local and national problems had been collected in the ANES’ pre-election waves, however, constituents still would have been thinking about for whom to vote, so changing the timing of the “most important problem” questions would not itself alleviate concerns of endogeneity between issue considerations and the act of voting. Furthermore, this potential endogeneity poses an easy test to find a significant relationship between perceptions of crime and vote choice given vote choice may have influenced perceptions of crime in the first place. If I do not find a significant correlation between the two (which, as it turns out, I do not), I will show (1) a lack of significant endogeneity between perceptions of crime and the act of voting in a crime-focused election and (2) evidence that constituents do not engage in crime-based retrospective voting. Though these factors do not eliminate endogeneity concerns, they nonetheless show the challenges to doing so and bolster support for my empirical designs and regression results.

Similar to my tests for my first three hypotheses, I report relevant excerpts from my estimated models in the main body of my analysis (the following two tables) and the entire regression outputs in Appendix A. Compared to the prior sub-section, I first present my models with national perceptions of crime (though this time the perceptions are explanatory variables):

Table 3. Vote choice by crime rate and crime as a country MIP (excerpt).

	<i>Dependent variable:</i>					
	Vote Republican					
	1984		1988		1992	
	(1)	(2)	(3)	(4)	(5)	(6)
Crime rate	1.066 (1.602)	1.058 (1.608)	-0.440 (1.782)	-0.290 (1.804)	1.565 (1.525)	1.529 (1.542)
Crime as country MIP		0.012 (0.066)		0.021 (0.035)		0.040 (0.039)
Constant	0.538*** (0.128)	0.539*** (0.130)	0.548*** (0.161)	0.552*** (0.160)	0.190 (0.135)	0.191 (0.136)
Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	923	923	750	750	1,007	1,007
R ²	0.571	0.571	0.535	0.535	0.427	0.428

Note: *p<0.1; **p<0.05; ***p<0.01
 All models contain state-level fixed effects (estimates not shown), and standard errors are robust (HC1) and clustered at the county level.
 See Table 7 in Appendix A for full regression results.

The first two models are for 1984, second two for 1988, and last two for 1992.

I begin by testing H4 (that constituents in higher-crime areas vote Republican), starting with an analysis of models one, three, and five. Those three models examine the relationship between crime rates, perceptions of crime as an important country-level problem, and reported vote choice for all constituents. Interestingly, none of the correlations between crime rates and

vote choice were significant, rejecting H4a. Furthermore, the magnitude of the coefficient for *Crime rate* in my model for 1988 is the smallest in magnitude, rejecting H4b. Nonetheless, I conduct Z-tests on the estimates, and each time, I fail to reject the null hypothesis of an insignificant difference between the 1988 estimate for *Crime rate* and the 1984 or 1992 estimate. I reject H4, and I will discuss these results in greater detail after testing H5.

I now test H5a, H5c, and H5e (that perceptions of crime as an important national problem mediated a relationship between crime rates and vote choice in each of the three election years). First of note is that these perceptions do not have statistically or substantively significant correlations with vote choice in any of the models, marking my results as rejecting these hypotheses; having a significant estimate for crime perceptions (which I do not) is a necessary condition to show potential mediation. Given the perception terms' standard errors are either larger than the estimates themselves (models two and four) or approximately the same size as the estimate (model six), I still do not rule out the possibility perceptions of crime as a national problem could correlate positively with vote choice, though these results certainly do not provide strong support for that theory. Finally, I calculate Z-scores to test whether the 1988 estimate for *Crime as country MIP* is statistically distinct from the corresponding 1984 and 1992 estimates. Each test fails to reject the null hypothesis of an insignificant difference between the estimates, aligning with my earlier rejections of H5a, H5c, and H5e.

I now transition to an analysis of perceptions of crime on the district level. Table 4 reports the estimates of my models of vote choice as functions of – among other things – crime rates and whether constituents perceived crime to be an important district-level problem.

Table 4. Vote choice by crime rate and crime as a district MIP (excerpt).

	<i>Dependent variable:</i>			
	Vote Republican			
	1988		1992	
	(1)	(2)	(3)	(4)
Crime rate	-0.440 (1.782)	-0.222 (1.738)	1.565 (1.525)	1.557 (1.527)
Crime as district MIP		0.085 (0.089)		0.078 (0.070)
Constant	0.548*** (0.161)	0.553*** (0.159)	0.190 (0.135)	0.195 (0.135)
Controls?	Yes	Yes	Yes	Yes
Observations	750	750	1,007	1,007
R ²	0.535	0.535	0.427	0.427

Note:

*p<0.1; **p<0.05; ***p<0.01

All models contain state-level fixed effects (estimates not shown), and standard errors are robust (HC1) and clustered at the county level.

Note the first and third models are repeated from Table 3.

See Table 8 in Appendix A for full regression results.

I now test H5b and H5d (that perceptions of crime as an important local problem mediated a relationship between crime rates and vote choice in both election years). In models two and four, the correlations between perceptions of crime on the district level and vote choice are insignificant and have large standard errors, and my Z-test between the 1988 estimate for *Crime as a district MIP* and the corresponding 1992 estimate fails to reject the null hypothesis of an insignificant difference between the terms. I reject H5b and H5d.

Overall, in light of my finding insignificant correlations between crime rates and presidential vote choices, it is not surprising I do not find evidence suggesting potential mediation from constituents' perceptions of crime on either level; after all, subjective

perceptions of an objective condition cannot mediate the effect of the condition itself if the condition has no significant relationship with vote choice in the first place. How come there appears to be no relationship between crime and vote choice at all, though?

Similar to my earlier discussions, one explanation is that constituents recognize local crime is not an inherently-federal issue (e.g., Richman, 2006; Zimring & Hawkins, 1996), so the lack of significant correlations between crime rates and presidential vote choice appears to support the idea of voters being rational and not attributing local crime to the president, someone not responsible for handling it.

At first blush, however, this explanation may appear in contention with the aforescribed literature on blind retrospection and the broader evidence that constituents' votes can be swayed by events removed from the elections in question. As Achen & Bartels (2018) write, this incorrect issue attribution is most likely to occur and affect vote choice in contexts with “substantial ... losses” to constituents' livelihoods which stem from the issue in question, widely-publicized negative shocks to constituents' expectations that the issue won't cause them any harm (i.e., constituents being surprised to realize the issues they once did not find salient will harm them after all), and easy opportunities to blame political leaders for these negative shocks. In contrast to the shark attacks on which Achen & Bartels (2017) base their theory of blind retrospection, crime is an inherently-political issue, so as I discussed in section two, given crime in 1988 did not need to jump through many hoops to become relevant to politics and a specific candidate – it already was relevant to politics and Dukakis – that election should have provided an easy test to find evidence of constituents' far-from-perfect issue attribution. Why explains why my results do not show evidence of it?

One potential explanation revolves around the fact crime does not necessarily decrease constituents' welfare, with Achen & Bartels' first condition for blind retrospection being that especially issues that harm constituents can motivate this irrational political behavior. Taylor (1995), Case & Mayer (1996), Gibbons (2004), and Lynch & Rasmussen (2010), for example, all find crime may contribute to increasing property values – something in constituents' interests – and more generally, one vein of microeconomics views crime as an interpersonal welfare transfer with the possibility to increase society's overall wellbeing (Lewin & Trumbull, 1990; McChesney, 1993). Counterintuitively, then, crime may not always lead to substantial losses to constituents' livelihoods, so it may make sense for increasing crime rates in voters' neighborhoods not to incentivize them to support Bush all else equal and on average.

An objection to this line of reasoning could be that constituents do not consciously think of local crime as increasing their wellbeing. Pursuant to the aforescribed literature outlining how constituents' perceptions of the world influence their political behavior, however, it seems possible they do not necessarily need to know what specific occurrences affect their welfare to refrain from blind retrospection; simply “calculat[ing] the changes in their own welfare” (Fiorina, 1981, p. 5) may have connected voters' wellbeing with crime at its current levels, or at least not made crime appear a salient factor diminishing constituents' wellbeing and therefore leading them to support Bush.

Finally, it is possible crime's raced nature prevents it from aligning with the traditional conceptions of retrospective voting for lesser-raced issues and through issue misattribution (e.g., Achen & Bartels, 2017, Chapter 5). Though I do not focus on racial resentment in this thesis, it nonetheless seems possible the election's cues on crime did not lead to crime-focused

retrospective voting; stoking racial resentment as Bush's campaign did (e.g., Mendelberg, 1997, 2001) may have been somewhat mutually exclusive with facilitating retrospective voting.

Regardless of the exact reason for these null relationships, they nonetheless boost the credibility of my findings vis-à-vis the potential endogeneity between constituents' (post-election) perceptions of crime and their reported vote choices. Large amounts of endogeneity would have increased the chances of significant correlations between perceptions of crime and vote choice, so given all my estimates are null, it seems unlikely perceptions of crime heavily depend on vote choice. Thus, my analysis passes this conservative test to detect endogeneity, though similar to my earlier analysis of Figure 5, this is a straw-in-the-wind test and does not provide strong evidence of exogeneity.

SECTION SIX

Conclusion

Overall, my results suggest crime does not align with traditional conceptions of public opinion and retrospective voting, and despite not pinpointing the specific reason, I suggest a leading contender may be crime's double-barrelled nature. Though about safety and security at face value, the issue really is a vehicle for "the addition of fear and distrust into the law and politics of punishment" (Zimring & Johnson, 2006) – crime is not solely an issue of governmental policy. Given crime and elite cues about it change constituents' emotional attitudes, it makes sense why I find incidences of crime do not steer whether constituents perceive crime to be an important political problem in contexts without heavy elite cueing about the topic.

Moreover, my findings relate to work on the "social construction of reality" (e.g., Berger & Luckmann, 1991). That literature suggests individuals see three worlds, starting with objective social realities comprised of unbiased conditions (e.g., crime rates). Second, individuals see symbolic social realities comprised of non-literal representations of their objective realities (e.g., elite cues and media coverage about crime). Third, they see subjective social realities comprised of their understandings of the other two realities (e.g., to use Lippmann's (1929) terminology, crime-focused "pseudo-environments"). My findings suggest constituents' symbolic realities can change how they make sense of their objective realities while constructing their subjective realities – the presence of heavy elite cueing on crime corresponds with a unique and negative relationship between incidences and perceptions of crime. Future work may examine in greater

depth the exact conditions under which increased elite cueing about an issue can decrease its salience.

Finally, both the American politics literature (Arnold, 1990; Fenno, 1978; Lazarus, 2013; Mayhew, 1974) and the comparative politics literature (Dannevig & Hovelsrud, 2016; Franklin & Wlezien, 1997; Givens & Luedtke, 2005; Spendzharova & Versluis, 2013) show issue salience can signal constituents' policy preferences and affect policy outcomes. In light of my finding that crime rates do not significantly correlate with constituents' vote choices, future research may examine whether and how constituents communicate their preferences about crime policy to elites. Though a "blunt instrument," constituents can use the vote to signal preferences (Fearon, 1999; Franko, 2013; Griffin & Newman, 2005), but I do not find evidence of crime leading constituents to vote one way or another with any discernible regularity.

APPENDIX A

Full Regression Results

A.1 Crime as a district MIP

Table 5. Crime as a district MIP (full).

	<i>Dependent variable:</i>			
	Crime as a district MIP			
	1988	1988	1992	1992
	(1)	(2)	(3)	(4)
Crime rate	-1.704** (0.670)	-2.295*** (0.722)	0.085 (0.306)	0.087 (0.459)
Campaign attention		0.001 (0.018)		-0.004 (0.010)
Crime rate * campaign attention		0.956 (0.650)		0.008 (0.530)
Age	-0.001 (0.002)	-0.001 (0.002)	0.0004 (0.001)	0.0005 (0.001)
Age squared	0.00001 (0.00002)	0.00001 (0.00002)	-0.00001 (0.00001)	-0.00001 (0.00001)
Education: Up to and including HS diploma	0.010 (0.020)	0.013 (0.020)	0.003 (0.006)	0.003 (0.007)
Education: Some college	-0.008 (0.015)	-0.007 (0.015)	0.008 (0.008)	0.008 (0.007)
Education: Advanced degree	-0.019 (0.019)	-0.021 (0.019)	-0.006 (0.006)	-0.006 (0.006)
Family income	0.0001 (0.001)	-0.0001 (0.001)	0.0003 (0.0004)	0.0003 (0.0004)
Race: Black	0.005 (0.018)	0.005 (0.017)	0.007 (0.011)	0.007 (0.011)
Race: American Indian or Alaskan native	0.004 (0.010)	0.001 (0.010)	0.049* (0.026)	0.048* (0.026)
Race: Asian or Pacific Islander	0.001 (0.024)	-0.001 (0.024)	0.049 (0.048)	0.049 (0.048)

Race: Other	-0.023 (0.021)	-0.023 (0.022)		
Spanish or Hispanic	-0.026 (0.021)	-0.020 (0.023)	0.005 (0.010)	0.006 (0.010)
Female	-0.015 (0.011)	-0.014 (0.011)	0.003 (0.006)	0.003 (0.006)
Married or living with partner	-0.007 (0.013)	-0.006 (0.013)	-0.016* (0.009)	-0.016* (0.009)
Homeowner	-0.002 (0.011)	-0.002 (0.011)	0.004 (0.007)	0.004 (0.007)
Retired	-0.030 (0.024)	-0.031 (0.025)	0.004 (0.011)	0.004 (0.011)
Occupation: Homemaker	-0.030** (0.014)	-0.032** (0.014)	-0.004 (0.005)	-0.004 (0.005)
Occupation: Clerical	0.046* (0.025)	0.047* (0.025)	-0.004 (0.004)	-0.004 (0.004)
Occupation: Professional	0.006 (0.012)	0.005 (0.012)	0.008 (0.007)	0.008 (0.007)
Union member in household	0.009 (0.012)	0.008 (0.012)	-0.007 (0.006)	-0.007 (0.006)
Rural	0.005 (0.014)	0.006 (0.014)	-0.005 (0.006)	-0.005 (0.006)
Religion: Protestant	0.004 (0.034)	0.002 (0.033)	0.005 (0.012)	0.006 (0.012)
Religion: Catholic	-0.007 (0.039)	-0.007 (0.039)	-0.007 (0.007)	-0.007 (0.007)
Religion: Jewish	0.042 (0.094)	0.041 (0.092)	-0.003 (0.010)	-0.003 (0.010)
Religion: Other Christian	-0.032 (0.030)	-0.034 (0.031)	-0.002 (0.006)	-0.002 (0.006)
Religion: Other	-0.018 (0.026)	-0.018 (0.027)	0.043 (0.048)	0.044 (0.048)
PID: Strong Democrat	0.025 (0.018)	0.022 (0.018)	0.026* (0.014)	0.027* (0.014)
PID: Weak Democrat	0.001 (0.016)	-0.0005 (0.016)	-0.002 (0.002)	-0.002 (0.002)
PID: Weak Republican	-0.011	-0.012	0.001	0.001

	(0.011)	(0.011)	(0.003)	(0.003)
PID: Strong Republican	-0.017	-0.019	0.003	0.003
	(0.012)	(0.012)	(0.006)	(0.006)
Ideology: Extremely liberal	-0.060***	-0.064***	0.064	0.064
	(0.017)	(0.017)	(0.056)	(0.056)
Ideology: Liberal	-0.038**	-0.041***	-0.008	-0.008
	(0.015)	(0.016)	(0.011)	(0.011)
Ideology: Slightly liberal	-0.001	-0.003	-0.004	-0.003
	(0.018)	(0.018)	(0.006)	(0.007)
Ideology: Slightly conservative	-0.022*	-0.023*	-0.002	-0.001
	(0.013)	(0.013)	(0.004)	(0.004)
Ideology: Conservative	0.007	0.005	0.005	0.005
	(0.017)	(0.017)	(0.008)	(0.008)
Ideology: Extremely conservative	0.047	0.044	-0.001	-0.001
	(0.046)	(0.046)	(0.005)	(0.005)
Change in county per capita real income	0.00001	0.00001	0.00002	0.00002
	(0.00001)	(0.00001)	(0.00002)	(0.00002)
Percent of county who's Black	0.611***	0.613***	0.063	0.063
	(0.112)	(0.113)	(0.040)	(0.040)
Constant	-0.045	-0.038	-0.027	-0.026
	(0.047)	(0.049)	(0.034)	(0.031)
Controls?	Yes	Yes	Yes	Yes
Observations	1,250	1,250	1,345	1,345
R ²	0.101	0.103	0.095	0.095

Note:

* p<0.1; ** p<0.05; *** p<0.01

All models contain state-level fixed effects (estimates not shown), and standard errors are robust (HC1) and clustered at the county level.

A.2 Crime as a country MIP

Table 6. Crime as a country MIP (full).

	<i>Dependent variable:</i>					
	Crime as a country MIP					
	1984		1988		1992	
	(1)	(2)	(3)	(4)	(5)	(6)
Crime rate	-0.148 (0.644)	2.974** (1.441)	-4.968*** (1.734)	-2.789 (2.065)	0.138 (0.875)	0.689 (1.409)
Campaign attention		0.074* (0.040)		0.138** (0.055)		-0.003 (0.047)
Crime rate * campaign attention		-3.806** (1.583)		-3.271* (1.872)		-0.774 (1.684)
Age	0.001 (0.002)	0.002 (0.002)	-0.002 (0.004)	-0.002 (0.005)	0.001 (0.003)	0.001 (0.003)
Age squared	-0.00001 (0.00002)	-0.00001 (0.00002)	0.00001 (0.00005)	0.00001 (0.00005)	-0.00001 (0.00003)	-0.00001 (0.00003)
Education: Up to and including HS diploma	0.003 (0.021)	0.002 (0.021)	0.078* (0.043)	0.084* (0.044)	0.010 (0.025)	0.009 (0.025)
Education: Some college	-0.003 (0.018)	-0.004 (0.017)	0.044 (0.050)	0.044 (0.051)	0.014 (0.029)	0.015 (0.029)
Education: Advanced degree	0.015 (0.032)	0.017 (0.032)	0.141* (0.080)	0.143* (0.080)	0.083* (0.051)	0.083* (0.050)
Family income	-0.0002 (0.001)	-0.0004 (0.001)	0.003 (0.003)	0.003 (0.003)	-0.003 (0.002)	-0.003 (0.002)
Race: Black	0.023 (0.027)	0.024 (0.025)	0.074* (0.044)	0.073* (0.044)	0.083** (0.042)	0.084** (0.042)
Race: American Indian or Alaskan native	0.110*** (0.040)	0.113*** (0.040)	0.201 (0.128)	0.200 (0.127)	0.168* (0.091)	0.162* (0.092)
Race: Asian or Pacific Islander	0.383 (0.370)	0.385 (0.365)	0.161 (0.301)	0.160 (0.310)	-0.019 (0.110)	-0.021 (0.111)
Race: Other			0.356 (0.278)	0.356 (0.281)		
Spanish or Hispanic	0.004 (0.022)	0.004 (0.021)	0.073 (0.099)	0.080 (0.105)	0.185 (0.187)	0.187 (0.189)
Female	0.019	0.017	0.032	0.034	-0.002	-0.003

	(0.014)	(0.014)	(0.031)	(0.030)	(0.023)	(0.023)
Married or living with partner	0.002	0.004	0.023	0.024	-0.020	-0.020
	(0.010)	(0.010)	(0.029)	(0.029)	(0.019)	(0.019)
Homeowner	0.007	0.007	0.010	0.012	0.001	0.001
	(0.011)	(0.011)	(0.032)	(0.032)	(0.024)	(0.024)
Retired	-0.014	-0.012	0.059	0.055	-0.014	-0.013
	(0.028)	(0.028)	(0.052)	(0.052)	(0.034)	(0.035)
Occupation: Homemaker	0.009	0.012	-0.027	-0.030	-0.026	-0.025
	(0.024)	(0.025)	(0.036)	(0.037)	(0.029)	(0.029)
Occupation: Clerical	-0.006	-0.005	0.038	0.040	0.045	0.046
	(0.018)	(0.018)	(0.052)	(0.052)	(0.044)	(0.044)
Occupation: Professional	0.003	0.004	0.011	0.008	-0.0003	0.001
	(0.016)	(0.016)	(0.035)	(0.035)	(0.024)	(0.025)
Union member in household	-0.025**	-0.025**	-0.043	-0.044	-0.018	-0.018
	(0.010)	(0.010)	(0.033)	(0.034)	(0.019)	(0.019)
Rural	-0.007	-0.007	-0.005	-0.003	-0.068***	-0.069***
	(0.015)	(0.014)	(0.030)	(0.029)	(0.019)	(0.019)
Religion: Protestant	0.010	0.006	0.104**	0.103**	0.096*	0.098*
	(0.014)	(0.014)	(0.044)	(0.043)	(0.050)	(0.050)
Religion: Catholic	0.020	0.016	0.118***	0.115***	-0.027	-0.026
	(0.017)	(0.017)	(0.044)	(0.043)	(0.032)	(0.032)
Religion: Jewish	-0.033*	-0.033*	0.258**	0.260**	0.170*	0.171*
	(0.019)	(0.019)	(0.102)	(0.102)	(0.091)	(0.091)
Religion: Other Christian	0.013	0.006	0.060	0.060	-0.0003	0.00003
	(0.024)	(0.023)	(0.092)	(0.091)	(0.030)	(0.030)
Religion: Other	0.100	0.097	0.057	0.056	0.002	0.004
	(0.089)	(0.089)	(0.149)	(0.150)	(0.125)	(0.126)
PID: Strong Democrat	-0.033**	-0.031**	-0.022	-0.027	-0.053	-0.051
	(0.016)	(0.015)	(0.039)	(0.039)	(0.034)	(0.034)
PID: Weak Democrat	-0.017	-0.017	0.006	0.006	-0.012	-0.012
	(0.018)	(0.018)	(0.040)	(0.040)	(0.025)	(0.025)
PID: Weak Republican	-0.030**	-0.029**	0.037	0.036	0.025	0.026
	(0.015)	(0.015)	(0.042)	(0.043)	(0.032)	(0.032)
PID: Strong Republican	-0.005	-0.003	0.002	-0.004	0.017	0.020
	(0.022)	(0.022)	(0.038)	(0.039)	(0.035)	(0.036)
Ideology: Extremely liberal	0.021	0.016	-0.099	-0.101	0.054	0.056
	(0.065)	(0.067)	(0.111)	(0.108)	(0.066)	(0.066)

Ideology: Liberal	0.003 (0.023)	0.001 (0.022)	-0.108* (0.056)	-0.113** (0.056)	0.011 (0.043)	0.014 (0.043)
Ideology: Slightly liberal	0.016 (0.022)	0.016 (0.022)	0.110** (0.052)	0.105** (0.052)	0.019 (0.031)	0.020 (0.031)
Ideology: Slightly conservative	0.014 (0.016)	0.014 (0.016)	0.003 (0.038)	-0.002 (0.037)	0.026 (0.026)	0.027 (0.027)
Ideology: Conservative	0.017 (0.016)	0.016 (0.016)	0.036 (0.043)	0.032 (0.043)	0.041 (0.032)	0.042 (0.032)
Ideology: Extremely conservative	-0.041*** (0.012)	-0.039*** (0.012)	0.056 (0.071)	0.054 (0.071)	0.018 (0.066)	0.019 (0.066)
Change in county per capita real income	-0.00003*** (0.00001)	-0.00003*** (0.00001)	0.00005*** (0.00002)	0.00005*** (0.00002)	0.00001 (0.00003)	0.00001 (0.00003)
Percent of county who's Black	0.037 (0.109)	0.034 (0.101)	0.818*** (0.226)	0.803*** (0.223)	-0.119 (0.128)	-0.120 (0.128)
Constant	-0.041 (0.041)	-0.108* (0.055)	-0.017 (0.139)	-0.091 (0.145)	0.080 (0.098)	0.073 (0.100)
Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,306	1,306	1,250	1,250	1,345	1,345
R ²	0.058	0.064	0.094	0.096	0.086	0.086

Note:

*p<0.1; **p<0.05; ***p<0.01

All models contain state-level fixed effects (estimates not shown), and standard errors are robust (HC1) and clustered at the county level.

A.3 Vote choice (with crime as a country MIP)

Table 7. Vote choice by crime rate and crime as a country MIP (full).

	<i>Dependent variable:</i>					
	Vote Republican					
	1984		1988		1992	
	(1)	(2)	(3)	(4)	(5)	(6)
Crime rate	1.066 (1.602)	1.058 (1.608)	-0.440 (1.782)	-0.290 (1.804)	1.565 (1.525)	1.529 (1.542)
Crime as a country MIP		0.012 (0.066)		0.021 (0.035)		0.040 (0.039)
Age	0.003 (0.006)	0.003 (0.006)	0.005 (0.006)	0.005 (0.006)	-0.003 (0.004)	-0.003 (0.004)
Age squared	-0.00003 (0.0001)	-0.00003 (0.0001)	-0.0001 (0.0001)	-0.0001 (0.0001)	0.00003 (0.00004)	0.00003 (0.00004)
Education: Up to and including HS diploma	0.095*** (0.035)	0.095*** (0.035)	0.014 (0.038)	0.011 (0.039)	-0.077* (0.041)	-0.077* (0.041)
Education: Some college	0.048 (0.036)	0.048 (0.036)	0.026 (0.036)	0.025 (0.036)	-0.126*** (0.036)	-0.126*** (0.036)
Education: Advanced degree	-0.070 (0.056)	-0.070 (0.057)	0.046 (0.054)	0.043 (0.054)	-0.093 (0.058)	-0.096* (0.058)
Family income	0.009*** (0.003)	0.009*** (0.002)	0.003 (0.003)	0.003 (0.003)	0.002 (0.003)	0.002 (0.003)
Race: Black	-0.242*** (0.052)	-0.242*** (0.052)	-0.249*** (0.051)	-0.251*** (0.052)	-0.158*** (0.044)	-0.160*** (0.043)
Race: American Indian or Alaskan native	-0.051 (0.037)	-0.052 (0.038)	-0.408*** (0.049)	-0.408*** (0.049)	-0.170*** (0.048)	-0.168*** (0.049)
Race: Asian or Pacific Islander	-0.070 (0.195)	-0.074 (0.193)	-0.079 (0.067)	-0.086 (0.067)	0.265** (0.123)	0.270** (0.122)
Race: Other			-0.302*** (0.098)	-0.316*** (0.102)		
Spanish or Hispanic	-0.051 (0.070)	-0.051 (0.070)	-0.027 (0.210)	-0.032 (0.213)	-0.182 (0.115)	-0.193 (0.121)
Female	-0.081*** (0.029)	-0.082*** (0.029)	0.052 (0.040)	0.051 (0.039)	0.048 (0.031)	0.048 (0.031)
Married or living with partner	-0.017	-0.017	0.030	0.030	0.047	0.048

	(0.024)	(0.024)	(0.034)	(0.034)	(0.037)	(0.037)
Homeowner	0.007	0.007	-0.030	-0.030	0.051*	0.050
	(0.026)	(0.026)	(0.037)	(0.037)	(0.031)	(0.031)
Retired	-0.068	-0.068	-0.010	-0.009	0.053	0.052
	(0.046)	(0.046)	(0.064)	(0.064)	(0.052)	(0.052)
Occupation: Homemaker	0.125***	0.126***	-0.138**	-0.138**	-0.003	-0.001
	(0.038)	(0.038)	(0.058)	(0.058)	(0.052)	(0.052)
Occupation: Clerical	0.111**	0.112**	-0.089*	-0.090*	0.039	0.038
	(0.046)	(0.046)	(0.049)	(0.049)	(0.047)	(0.047)
Occupation: Professional	0.047	0.047	-0.043	-0.043	-0.023	-0.022
	(0.035)	(0.035)	(0.041)	(0.042)	(0.036)	(0.036)
Union member in household	-0.142***	-0.141***	-0.080**	-0.080**	-0.012	-0.011
	(0.041)	(0.041)	(0.035)	(0.035)	(0.037)	(0.037)
Rural	-0.018	-0.018	-0.039	-0.038	0.108***	0.110***
	(0.027)	(0.027)	(0.036)	(0.037)	(0.036)	(0.037)
Religion: Protestant	0.093	0.093	0.037	0.037	0.211***	0.207***
	(0.058)	(0.058)	(0.054)	(0.054)	(0.056)	(0.056)
Religion: Catholic	0.072	0.072	0.023	0.022	0.144***	0.145***
	(0.064)	(0.064)	(0.052)	(0.052)	(0.055)	(0.055)
Religion: Jewish	-0.092	-0.092	-0.102	-0.104	0.005	-0.002
	(0.119)	(0.119)	(0.133)	(0.131)	(0.074)	(0.074)
Religion: Other Christian	0.113	0.112	0.096	0.097	0.100**	0.100**
	(0.104)	(0.104)	(0.103)	(0.104)	(0.042)	(0.042)
Religion: Other	0.117	0.116	-0.015	-0.012	0.121	0.119
	(0.087)	(0.087)	(0.181)	(0.182)	(0.135)	(0.136)
PID: Strong Democrat	-0.498***	-0.498***	-0.377***	-0.378***	-0.223***	-0.221***
	(0.035)	(0.036)	(0.043)	(0.043)	(0.035)	(0.035)
PID: Weak Democrat	-0.321***	-0.320***	-0.247***	-0.250***	-0.164***	-0.165***
	(0.043)	(0.043)	(0.065)	(0.065)	(0.040)	(0.040)
PID: Weak Republican	0.202***	0.202***	0.231***	0.230***	0.213***	0.213***
	(0.040)	(0.039)	(0.049)	(0.049)	(0.045)	(0.046)
PID: Strong Republican	0.234***	0.234***	0.346***	0.346***	0.429***	0.429***
	(0.038)	(0.038)	(0.046)	(0.047)	(0.051)	(0.051)
Ideology: Extremely liberal	-0.240***	-0.240***	-0.179***	-0.173***	-0.084*	-0.085*
	(0.089)	(0.090)	(0.065)	(0.063)	(0.048)	(0.048)
Ideology: Liberal	-0.070*	-0.070*	-0.265***	-0.263***	-0.100***	-0.101***
	(0.040)	(0.040)	(0.043)	(0.044)	(0.032)	(0.032)

Ideology: Slightly liberal	-0.092** (0.046)	-0.092** (0.046)	-0.140*** (0.047)	-0.139*** (0.046)	-0.097*** (0.032)	-0.098*** (0.032)
Ideology: Slightly conservative	0.092*** (0.030)	0.092*** (0.030)	0.046 (0.038)	0.045 (0.038)	0.090** (0.042)	0.089** (0.041)
Ideology: Conservative	0.090** (0.037)	0.090** (0.037)	0.095** (0.041)	0.093** (0.041)	0.215*** (0.049)	0.213*** (0.049)
Ideology: Extremely conservative	0.006 (0.081)	0.007 (0.081)	0.020 (0.085)	0.018 (0.085)	0.130 (0.104)	0.128 (0.104)
Change in county per capita real income	-0.00002 (0.00002)	-0.00002 (0.00002)	0.00001 (0.00002)	0.00001 (0.00002)	0.00004 (0.00002)	0.00003 (0.00002)
Percent of county who's Black	-0.037 (0.240)	-0.036 (0.240)	0.138 (0.281)	0.115 (0.281)	0.121 (0.143)	0.124 (0.145)
Constant	0.538*** (0.128)	0.539*** (0.130)	0.548*** (0.161)	0.552*** (0.160)	0.190 (0.135)	0.191 (0.136)
Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	923	923	750	750	1,007	1,007
R ²	0.571	0.571	0.535	0.535	0.427	0.428

Note: * p<0.1; ** p<0.05; *** p<0.01
All models contain state-level fixed effects (estimates not shown),
and
standard errors are robust (HC1) and clustered at the county level.

A.4 Vote choice (with crime as a district MIP)

Table 8. Vote choice by crime rate and crime as a district MIP (full).

	<i>Dependent variable:</i>			
	Vote Republican			
	1988		1992	
	(1)	(2)	(3)	(4)
Crime rate	-0.440 (1.782)	-0.222 (1.738)	1.565 (1.525)	1.557 (1.527)
Crime as district MIP		0.085 (0.089)		0.078 (0.070)
Age	0.005 (0.006)	0.005 (0.006)	-0.003 (0.004)	-0.003 (0.004)
Age squared	-0.0001 (0.0001)	-0.0001 (0.0001)	0.00003 (0.00004)	0.00003 (0.00004)
Education: Up to and including HS diploma	0.014 (0.038)	0.012 (0.038)	-0.077* (0.041)	-0.077* (0.041)
Education: Some college	0.026 (0.036)	0.027 (0.036)	-0.126*** (0.036)	-0.126*** (0.036)
Education: Advanced degree	0.046 (0.054)	0.047 (0.054)	-0.093 (0.058)	-0.093 (0.058)
Family income	0.003 (0.003)	0.003 (0.003)	0.002 (0.003)	0.002 (0.003)
Race: Black	-0.249*** (0.051)	-0.254*** (0.053)	-0.158*** (0.044)	-0.159*** (0.043)
Race: American Indian or Alaskan native	-0.408*** (0.049)	-0.408*** (0.049)	-0.170*** (0.048)	-0.189*** (0.054)
Race: Asian or Pacific Islander	-0.079 (0.067)	-0.077 (0.068)	0.265** (0.123)	0.257** (0.125)
Race: Other	-0.302*** (0.098)	-0.300*** (0.099)		
Spanish or Hispanic	-0.027 (0.210)	-0.026 (0.212)	-0.182 (0.115)	-0.183 (0.115)
Female	0.052 (0.040)	0.053 (0.040)	0.048 (0.031)	0.048 (0.031)
Married or living with partner	0.030	0.031	0.047	0.049

	(0.034)	(0.034)	(0.037)	(0.037)
Homeowner	-0.030	-0.029	0.051*	0.051*
	(0.037)	(0.037)	(0.031)	(0.031)
Retired	-0.010	-0.006	0.053	0.052
	(0.064)	(0.065)	(0.052)	(0.052)
Occupation: Homemaker	-0.138**	-0.135**	-0.003	-0.003
	(0.058)	(0.058)	(0.052)	(0.052)
Occupation: Clerical	-0.089*	-0.092*	0.039	0.039
	(0.049)	(0.049)	(0.047)	(0.047)
Occupation: Professional	-0.043	-0.043	-0.023	-0.023
	(0.041)	(0.041)	(0.036)	(0.036)
Union member in household	-0.080**	-0.080**	-0.012	-0.011
	(0.035)	(0.035)	(0.037)	(0.037)
Rural	-0.039	-0.039	0.108***	0.109***
	(0.036)	(0.036)	(0.036)	(0.036)
Religion: Protestant	0.037	0.037	0.211***	0.210***
	(0.054)	(0.054)	(0.056)	(0.056)
Religion: Catholic	0.023	0.024	0.144***	0.145***
	(0.052)	(0.053)	(0.055)	(0.055)
Religion: Jewish	-0.102	-0.097	0.005	0.006
	(0.133)	(0.134)	(0.074)	(0.074)
Religion: Other Christian	0.096	0.099	0.100**	0.100**
	(0.103)	(0.105)	(0.042)	(0.042)
Religion: Other	-0.015	-0.015	0.121	0.116
	(0.181)	(0.183)	(0.135)	(0.136)
PID: Strong Democrat	-0.377***	-0.377***	-0.223***	-0.224***
	(0.043)	(0.043)	(0.035)	(0.036)
PID: Weak Democrat	-0.247***	-0.247***	-0.164***	-0.163***
	(0.065)	(0.064)	(0.040)	(0.040)
PID: Weak Republican	0.231***	0.232***	0.213***	0.213***
	(0.049)	(0.049)	(0.045)	(0.045)
PID: Strong Republican	0.346***	0.347***	0.429***	0.429***
	(0.046)	(0.047)	(0.051)	(0.051)
Ideology: Extremely liberal	-0.179***	-0.173***	-0.084*	-0.091*
	(0.065)	(0.065)	(0.048)	(0.047)
Ideology: Liberal	-0.265***	-0.262***	-0.100***	-0.099***
	(0.043)	(0.044)	(0.032)	(0.032)

Ideology: Slightly liberal	-0.140*** (0.047)	-0.142*** (0.047)	-0.097*** (0.032)	-0.097*** (0.032)
Ideology: Slightly conservative	0.046 (0.038)	0.048 (0.038)	0.090** (0.042)	0.089** (0.042)
Ideology: Conservative	0.095** (0.041)	0.094** (0.041)	0.215*** (0.049)	0.214*** (0.049)
Ideology: Extremely conservative	0.020 (0.085)	0.014 (0.085)	0.130 (0.104)	0.130 (0.104)
Change in county per capita real income	0.00001 (0.00002)	0.00001 (0.00002)	0.00004 (0.00002)	0.00003 (0.00002)
Percent of county who's Black	0.138 (0.281)	0.072 (0.262)	0.121 (0.143)	0.116 (0.144)
Constant	0.548*** (0.161)	0.553*** (0.159)	0.190 (0.135)	0.195 (0.135)
Controls?	Yes	Yes	Yes	Yes
Observations	750	750	1,007	1,007
R ²	0.535	0.535	0.427	0.427

Note: * p<0.1; ** p<0.05; *** p<0.01
All models contain state-level fixed effects (estimates not shown), and standard errors are robust (HC1) and clustered at the county level.
Note the first and third models are repeated from Table #.

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