

**ARTICLE**

# The public salience of crime, 1960–2014: Age-period-cohort and time-series analyses\*

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**Abstract**

The public salience of crime has wide-ranging political and social implications; it influences public trust in the government and citizens' everyday routines and interactions, and it may affect policy responsiveness to punitive attitudes. Identifying the sources of crime salience is thus important. Two competing theoretical models exist: the objectivist model and the social constructionist model. According to the first, crime salience is a function of the crime rate. According to the second, crime salience is a function of media coverage and political rhetoric, and trends in crime salience differ across population subgroups as a result of differences in their responsiveness to elite initiatives. In both theories, period-level effects predominate. Variation in crime salience, however, may also reflect age and cohort effects. Using data from 422,504 respondents interviewed between 1960 and 2014, we first examine the nature of crime salience using hierarchical age-period-cohort (HAPC) models and then analyze period-level predictors using first differences. We find that 1) crime salience varies mostly at the period level; 2) crime salience trends are parallel (cointegrated) across demographic, socioeconomic, and partisan groups; and 3) crime salience trends within every population subgroup are most consistent with the constructionist model. The crime rate does not exert a significant effect in any subgroup.

**KEY WORDS**

crime, media, most important problem, parallel publics, public opinion

Understanding how the salience of crime to the public varies over time—that is, when and why members of the public come to view crime as a pressing social problem—is important for at least three reasons. First, shifts in the percentage of the public identifying crime as the country’s most important problem (MIP) contribute to subsequent changes in aggregate support for punitive crime policies (Ramirez, 2013), which in turn exert large effects on incarceration rates and criminal justice expenditures, at both the state and federal levels (Enns, 2014, 2016). Put simply, crime salience indirectly affects crime policy by shaping policy attitudes. Second, because issue salience interacts with aggregate policy preferences to affect policy outcomes (Burstein, 2006; Lax & Phillips, 2012; Monroe, 1998), punitive attitudes are likely to have larger effects on policymaking when the salience of crime to the public is high (Zimring & Johnson, 2006). Third, beyond its policy implications, crime salience has significant social, cultural, and political repercussions (Garland, 2001; Simon, 2007). Elevated crime salience undermines trust in the government (Chanley, Rudolph, & Rahn, 2000). It also motivates various forms of adaptive reactions—avoidance, precautionary, and defensive behaviors—by private citizens to enhance their personal safety and protect family members, which in turn affect their everyday interactions and social relationships (Warr, 2009; Warr & Ellison, 2000).

Two theoretical perspectives explain changes in the public salience of crime over time. According to the first, the objectivist model, trends in crime salience reflect changes in actual rates of criminal offending (Pickett, 2019). According to the second, the constructionist model, crime salience varies because of changes in elite initiatives—specifically, changes in media crime coverage and political rhetoric about crime (Beckett, 1997; Beckett & Sasson, 2004).<sup>1</sup> Both models indicate that crime salience mainly varies at the period level, but their similarities stop there. Adjudicating between the models is important because trends in crime salience play a central role in many theories of culture, social structure, and crime control in late modern society (Beckett & Sasson, 2004; Garland, 2001; Savelberg, 1994; Simon, 2007).

Unfortunately, the few empirical studies that included aggregate data to analyze changes over time in the public salience of crime had notable limitations. Either the researchers only reported bivariate findings (Miller, 2013, 2016), evaluated short time periods using data from only one or two polling organizations (Beckett, 1997; Lowry, Nio, & Leitner, 2003), or overlooked the implications of nonstationarity for time-series analysis (Hill, Oliver, & Marion, 2010; Oliver, 1998, 2002). None, as far as we know, used multilevel data to examine simultaneously the independent effects of age, birth cohort, and time period on perceived crime salience in the United States. This is important because “individual aging, historical contexts, and generational membership are simultaneously related to the passage of time but have separate effects” (Gray, Grasso, Farrall, Jennings, & Hay, 2018, p. 2). As Anderson, Lytle, and Schwadel (2017, p. 835) noted, “[S]ome social attitudes change across generations or birth cohorts rather than across time periods.”

Additionally, according to the constructionist approach, elite initiatives are race coded and should have larger effects among individuals who harbor anti-minority sentiments that increase their sensitivity to racial “dog whistles” (Drakulich, 2015); crime salience trends should thus differ across racial and political groups (Beckett & Sasson, 2004; Tonry, 2011). Crime salience should also diverge along socioeconomic cleavages. According to Garland (2001, p. 152), for example, the “professional middle classes” were “the dog that did not bark” during the U.S. imprisonment binge because they experienced especially large increases in crime salience. According to the objectivist approach, on the other hand, crime salience trends are parallel across different racial, political, and socioeconomic groups because they all respond to the crime rate (Page & Shapiro, 1992; Pickett, 2019).

<sup>1</sup>As used herein, the term “elite initiatives” refers to agenda-setting activities by either media or political elites (Beckett & Sasson, 2004; Chomsky & Herman, 1988).

To advance the literature and shed light on the validity of existing theoretical accounts, we examine changes over time in the public salience of crime. To do so, we use the Most Important Problem Dataset (MIPD; Heffington, Park, & Williams, 2017), which includes individual-level MIP responses and respondent data from 686 nationally representative surveys that have asked the question about the nation's most important problem between 1938 and 2015. The MIPD provides a large analytic sample ( $N = 422,504$ ), which permits us to estimate hierarchical age-period-cohort (HAPC) models to determine whether changes over time in crime salience mostly reflect period effects, as criminological theories suggest, or the effects of age or birth cohort. The data also allow for us to estimate time-series models to examine 1) whether trends in crime salience are parallel across different demographic, partisan, and socioeconomic groups, and 2) how the crime rate, media coverage, and political rhetoric affect crime salience in the population as a whole and in population subgroups.

## 1 | THEORETICAL AND EMPIRICAL BACKGROUND

Social scientists have identified two competing theoretical models to explain changes over time in the public salience of crime (Pickett, 2019). The first is the “objectivist model” (Beckett, 1994, p. 427), also known as the “democracy-at-work thesis” (Beckett, 1997, p. 15). It hinges on a straightforward theoretical prediction: Changes in the public salience of crime should reflect changes in objective reality (Miller, 2016; Wilson, 1975). As Beckett (1994, p. 428) put it, “objectivists argue that the increased incidence of ‘street crime’ and drug use has led to increased public concern about those issues.” The second, the “constructionist model” (Beckett, 1994, p. 428), hinges on the premise that because claims-makers socially construct and frame social problems (Baranauskas & Drakulich, 2018; Drakulich, 2015), the salience of crime to the public is likely to fluctuate mainly in response to media coverage and political rhetoric (Beckett, 1997; Beckett & Sasson, 2004; Weaver, 2007).

Crucially, what distinguishes the objectivist and constructionist models is their predictions about the relationship (or lack thereof) between crime rates and public opinion. According to the objectivist model, this relationship should be sizable and positive; it may be direct, reflecting changes in citizens’ victimization experience (personal or vicarious), or indirect via *fact-based* changes in media coverage or political rhetoric (Miller, 2013, 2016). By contrast, according to the constructionist model, crime rates and public opinion should be unrelated or, at most, weakly related. The reason is twofold. First, media coverage and political rhetoric should shape public opinion. Second, what should drive the media and politicians is market incentives and partisan interests rather than changes in objective social conditions (Beale, 2006; Beckett & Sasson, 2004). Constructionists thus assume manipulative rather than educative effects of elite influence on public opinion (Beckett, 1997; Loo & Grimes, 2004; Weaver, 2007). They assume, elites *construct* reality rather than *report* it, hence, the name of the model.

Unfortunately, in most previous studies of crime concern, researchers have analyzed cross-sectional samples of individuals to examine personal crime fear or perceived risk. As a result, they have reported mixed findings about whether the local crime rate influences these variables (Drakulich, 2013; Quillian & Pager, 2001; Taylor & Hale, 1986). Some researchers have found that local crime has a positive effect, but it is weak (Markowitz, Bellair, Liska, & Liu, 2001; Pickett, Chiricos, Golden, & Gertz, 2012; Rountree & Land, 1996) or varies based on individual characteristics (e.g., race) of the respondents (Brunton-Smith & Sturgis, 2011; Liska, Lawrence, & Sanchirico, 1982).

There are several difficulties, however, with using these studies to draw conclusions about changes over time in aggregate concern about crime. One is that concern about the crime problem is different than personal fear or perceived risk (Ferraro, 1995). Crime concern is likely to reflect altruistic fear for others’ safety, not just for one’s own (see Warr & Ellison, 2000). Another difficulty is that that local

crime rates may anchor individuals' level of concern, anxiety, or perceived risk, without explaining changes over time in these variables. Crime *rates* vary tremendously across areas, but crime *trends* are mostly parallel across places (Enns, 2016). As Baumer and Wolff (2014, p. 13) explained, "there is compelling evidence that a significant portion of the observed sub-national trends (e.g., state, city, and county-level trends) reflect broader *national influences*" (emphasis in original). Indeed, McDowall and Loftin (2009, p. 318) found that "in an average year roughly two-thirds of the cities and their residents are subject to the national trend." Therefore, what is most theoretically germane to aggregate attitudes in the United States is national crime trends, not local rates.

Findings from prior cross-sectional studies show that news viewership is a consistent predictor of fear of crime and perceived risk (Roche, Pickett, & Gertz, 2016).<sup>2</sup> Consuming television news, especially local TV news, is positively related to these variables (Barankauskas & Drakulich, 2016; Chiricos, Padgett, & Gertz, 2000; Eschholz, Chiricos, & Gertz, 2003). Attention paid to crime news is also a strong predictor (Graber, 1980; O'Keefe & Reid-Nash, 1987; Shi, 2018). The explanation for these effects is that the media exaggerate the frequency and severity of crime events, which lead audience members to believe that crime is on the rise and increasingly violent (Gilliam & Iyengar, 2000; Roberts, Stalans, Indermaur, & Hough, 2003). Again, it is hard to know whether the media effects observed in individual-level studies will generalize to changes in aggregate attitudes over time. First, the if-it-bleeds-it-leads media values that motivate exaggerated crime coverage may be stable over time, so that the *amount* of crime coverage, sensationalized as it is, still varies over time in response to actual changes in the crime rate (Enns, 2016; Miller, 2016). Second, studying individual differences in news consumption is different than examining changes in the nature of the news itself. Even if people's viewing habits remain unchanged, their attitudes still may change if the media's coverage of crime increases.

The few scholars that have examined aggregate public opinion about crime in the United States have analyzed survey responses about whether crime is the country's most important problem (MIP; Beckett, 1994, 1997; Miller, 2013, 2016; Oliver, 1998, 2002). Although it has limitations (Jennings & Wlezien, 2011, 2015; Wlezien, 2005), the MIP question is the standard indicator of the public salience of social problems (Heffington et al., 2017; Smith, 1980). Across disciplines, researchers have used it to examine the perceived importance of different social issues (Hill, 1998; Smith, 1980), as well as to explore agenda-setting processes (Althaus & Tewksbury, 2002; Erbring, Goldenberg, & Miller, 1980) and policy responsiveness to public opinion (Canes-Wrone & Shotts, 2004; Monroe, 1998; Nicholson-Crotty & Meier, 2003).

To date, the evidence from studies of changes in MIP responses is inconclusive about the validity of either the objectivist or the constructionist model. In some studies, scholars have found that media crime coverage is positively associated with crime salience (Beckett, 1994, 1997; Lowry et al., 2003), but others have found the opposite (Oliver, 1998). Political rhetoric has been positively associated with crime salience in some studies (Beckett, 1994, 1997; Oliver, 1998, 2002) but not in others (Hill et al., 2010). Although some researchers have found a positive association between the crime rate and crime salience (Lowry et al., 2003; Oliver, 1998), especially those examining bivariate correlations (Miller, 2013, 2016), others have reported mixed findings (Oliver, 2002), or even a negative (albeit nonsignificant) association between the two (Beckett, 1994, 1997).

Unfortunately, prior studies of crime salience in the United States have had several substantive and methodological limitations. To start with, researchers have overlooked a fundamental prediction of both models, objectivist and constructionist—namely, that crime salience varies mostly at the period level.

<sup>2</sup>We are aware of one longitudinal study of individuals' crime perceptions (Shi, Roche, & McKenna, 2018). In it, researchers found no evidence that changes in consumption result in changes in perceptions.

Testing this prediction requires estimating age–period–cohort models and is important because period-level characteristics represent only one possible source of temporal variation. For example, Gray et al. (2018) found sizable cohort effects on perceptions of antisocial behavior (e.g., teenage loitering) and fear of crime in Britain between 1982 and 2010. Analyzing death penalty support in the United States, Anderson et al. (2017) found significant age effects, with support peaking in midlife. These authors suggested that the period-level effects found in prior crime salience studies may be confounded as a result of the collinearity between age, period, and cohort (see, e.g., Schwadel & Ellison, 2017). Even still, according to both the objectivist and constructionist models, most over-time variation in crime salience is at the period level. Thus, our first research question is as follows:

*Research Question #1: Do variations in crime salience over time mostly reflect age, period, or cohort effects?*

Additionally, in many prior studies, researchers have only analyzed short time periods (e.g., Beckett, 1994, 1997; Hill et al., 2010; Lowry et al., 2003), and/or they have included the crime rate and media crime coverage in the same model (e.g., Beckett, 1994, 1997; Oliver, 1998, 2002), which would lead to overcontrol bias if the crime rate affects the number of news stories about crime (Elwert & Winship, 2014), as some evidence indicates it does (Enns, 2016; Miller, 2016). In many previous studies, scholars have also excluded period-level controls for theoretically relevant factors, such as economic conditions, that may confound the relationship between the crime rate and crime salience (e.g., Beckett, 1994, 1997; Lowry et al., 2003; Miller, 2013, 2016). Consequently, the models estimated in these studies may have been misspecified.

Perhaps most notably, however, prior researchers have not tested (as far as we can tell) the stationarity of crime salience (e.g., Miller, 2013, 2016; Oliver, 1998, 2002). This is important because “most time-series methods are only valid if the underlying time-series is stationary” (Levendis, 2018, p. 81). Nonstationary series do not equilibrate (revert to a given mean). Because their means and variances change over time, any two nonstationary series often will be strongly correlated by chance (Enders, 2014). These “spurious regressions” (Granger & Newbold, 1974, p. 111)—or “nonsense correlations” (Yule, 1926, p. 1)—happen simply because the series are trending simultaneously, not because of any causal relationship between them, and they invalidate test statistics (Levendis, 2018). Absent cointegration, nonstationary series must be analyzed in differences, to render them stationary (Grant & Lebo, 2016), but differencing a stationary regressand is problematic (Enders, 2014). Consequently, the first step in any time-series analysis should be to test the stationarity of the dependent variable (Philips, 2018).

There are strong reasons to expect crime salience will be nonstationary. For example, a key theoretical predictor, the U.S. crime rate, is nonstationary (Greenberg, 2001). In fact, nonstationarity may explain some of the strongest evidence for the objectivist model. Miller (2016) found that between 1950 and 2010, the correlation between the U.S. homicide rate and crime coverage by *The New York Times* was .80. She also examined the correlation between states’ homicide rates and state and local newspaper coverage of crime, which was almost as strong ( $r = .63$ ). Additionally, Miller showed that the correlation between the U.S. homicide rate and fear of crime was astonishingly large ( $r = .88$ ). Despite their size, however, all of these correlations may be meaningless if the variables are nonstationary (Enders, 2014). Unfortunately, it does not seem that Miller (2013, 2016) tested their stationarity.

To address all of these limitations, we use data for a long time period, test for stationarity, and estimate a series of time-series models to re-examine the relationship between crime salience and its theoretical predictors:

Research Question #2: *Is the period-level variation in crime salience a function of the crime rate, media coverage, or political rhetoric?*

We contribute to the literature in another way: We provide the first time-series analysis of how crime salience trends and the predictors of those trends vary across population subgroups. In individual studies of crime concern, fear, and perceived risk, researchers have found that local crime and media consumption have different effects depending on viewers' characteristics (e.g., race, gender; Brunton-Smith & Sturgis, 2011; Chiricos, Eschholz, & Gertz, 1997; Eschholz et al., 2003; Roche et al., 2016) or residential context (Barankauskas & Drakulich, 2018; Weitzer & Kubrin, 2004). Again, it is difficult to generalize these findings to aggregate public opinion. Most criminological theorizing on the public salience of crime, however, also anticipates that both the sources of and trends in crime salience will vary across population subgroups.

Garland (2001), for example, posited variation across socioeconomic groups, arguing that the increase in crime salience since the mid-1900s occurred mostly in the middle classes. He explained that "social and spatial changes" during the middle decades of the twentieth century "transformed the middle-class experience of crime" and created "new middle class concerns about crime" (pp. 152–154). In his words, "within a single generation," crime went "from being a problem that mostly affected the poor" to "a prominent fact of life not only for the urban middle classes, but for many middle-class suburbanites as well" (p. 152). Brown (2006) tested Garland's (2001) argument but did not find significant differences in punitive attitudes between the middle class and the rest of the population.

The most widely accepted account about divergent group dynamics is the social constructionist argument that post-1965 increases in crime salience reflect Republican legislators' use of anticrime rhetoric to realign the electorate (the Southern Strategy) and attack both the civil rights movement and the welfare state (Beckett, 1997; Tonry, 2011; Weaver, 2007). According to this account, "conservative politicians ... worked for decades to alter popular perceptions of crime," and although public responsiveness was not universal, "some members of the public did respond favorably"—namely, "those opposed to social and racial reform" (Beckett & Sasson, 2004, pp. 46–49). From this perspective, then, the effects of political rhetoric about crime should be largest among Whites and Republicans, and trends in crime salience should vary by race and political party identification.

By contrast, according to the objectivist model, changes in the crime rate should cause all population subgroups to update their crime salience perceptions accordingly and at about the same time. Although not examining crime salience, scholarship in political science indicates that changes in aggregate attitudes tend to be similar in direction and magnitude across population groups (Enns & Kellstedt, 2008; Erikson, Mackuen, & Stimson, 2002; Soroka & Wlezien, 2010). Page and Shapiro (1992, p. 291), for example, analyzed more than 3,000 subgroup trends in public opinion and found that "there were substantial differences in trends less than 6% of the time." They explained that the most likely explanation for parallel public opinion change is that "most Americans are exposed to the same news—at least the same big news" (p. 33), which they argued mediates the effects of social and economic trends (see pp. 353–354).

Therefore, Page and Shapiro's (1992) argument is most consistent with an indirect objectivist model, where crime trends indirectly affect all subgroups' crime salience perceptions *through* media coverage. It hinges, of course, on the tenuous assumption that the crime rate influences the number of news stories about crime. Importantly, however, parallel publics—similar crime salience trends across population subgroups—could also exist under the social constructionist model if all Americans receive the same crime news and respond to it in a similar way, but trends in news coverage do not follow crime trends. Thus, interpreting the theoretical implications of parallel trends requires having evidence about their sources as well.

Evidence from political science reveals that the effect of social trends on media coverage is complex (Soroka, Stecula, & Wlezien, 2015), and potentially asymmetric, such that worsening conditions have larger effects than improving conditions (Soroka, 2006). To our knowledge, however, researchers have not examined the determinants of media crime coverage. Yet, there is some evidence that trends in punitive policy preferences are mostly parallel across population subgroups, which seemingly lends support to the objectivist model (Enns, 2016; Ramirez, 2013). Given the inconclusive evidence on racial/political divergence versus parallel publics, we examine a third research question:

*Research Question #3: Are crime salience trends and predictors parallel or divergent across subgroups?*

## 2 | DATA AND MEASURES

To answer our research questions, we use the MIPD, which includes responses to open-ended MIP questions asked in 686 nationally representative surveys conducted by 26 different survey houses (e.g., NORC, Gallup, ANES) between 1939 and 2015 (Heffington et al., 2017). This data set has two strengths. First, it is the most extensive available, including the most respondents from the most years. Second, and particularly important for our purposes, it includes individual-level data on respondents' characteristics (demographic, socioeconomic, and partisan) for most surveys. It "can thus be used ... to assess whether the notion of 'parallel publics' extends to issue importance" (Heffington et al., 2017, p. 3), as well as to analyze both the individual- and period-level predictors of perceptions.

The original data set includes 686 surveys with a total of 935,172 cases. We drop 272 surveys (397,918 respondents) that do not include necessary variables, like age, reducing the sample size to 537,254. We then drop item missing data (cases with missing values on the variables used in the analysis) in the remaining 414 surveys, leaving 422,504 cases in the final sample. In supplementary analyses provided in the online supporting information, we re-estimate the models after imputing the item missing data and find similar results (available by request). The MIPD data set also provides population weights, which we apply in the analysis.<sup>3</sup>

### 2.1 | Dependent variable

The outcome variable in our analysis, crime salience, is a dichotomous indicator of whether the respondent's open-ended response identified crime as the country's MIP (no = 0, yes = 1). In constructing this measure, we use the Singers (2011) classification system, which is one of the established coding schemes provided in the MIPD data set. The crime issue is defined as including open-ended MIP responses that focus on crime, violence, school violence, gangs, drugs, guns, and/or gun control.<sup>4</sup> On average, across all survey years, approximately 11 percent of respondents identified the crime issue as the country's MIP. Descriptive statistics for all variables included in our analyses are provided in the online supporting information (appendix tables A1 and A2).

<sup>3</sup>We also estimate unweighted HAPC models as a robustness check (not shown). The main findings are unchanged. Additional supporting information can be found in the full text tab for this article in the Wiley Online Library at <http://onlinelibrary.wiley.com/doi/10.1111/crim.2020.58.issue-3/issuetoc>.

<sup>4</sup>The MIPD also provides MIP coding schemes from the Manifesto Research on Political Representation (MARPOR; Volkens, Bara, Budge, McDonald, & Klingemann, 2013) and the Comparative Agendas Project (CAP; Baumgartner & Jones, 2002). Unfortunately, the MARPOR scheme has no crime category, and the CAP scheme defines the crime issue too broadly, including responses focused on protests, family values, parenting, child abuse, and the generational divide.

## 2.2 | Individual-level predictors

In the first part of our analysis, the key individual-level predictors are respondents' demographic and socioeconomic characteristics, partisan identification, and residential location. Age is measured continuously in years and is grand-mean-centered.<sup>5</sup> We also include a polynomial term for age (age-squared) to account for possible curvilinear age effects (Anderson et al., 2017; Brunton-Smith & Sturgis, 2011). To measure household income consistently across time, we use an ordinal indicator that divides respondents into the four quartiles of the income distribution for their survey year (0–25th percentile = 1, and 75–100th percentile = 4). The other individual-level predictors are binary variables, which we code as follows: gender (male = 0, female = 1), race (non-White = 0, White = 1), education (no college degree = 0, college or above = 1), partisan identification (Democrat/Independent = 0, Republican = 1), urbanicity (rural/suburban = 0, major cities = 1), and census region (non-South = 0, South = 1).

## 2.3 | Period-level predictors

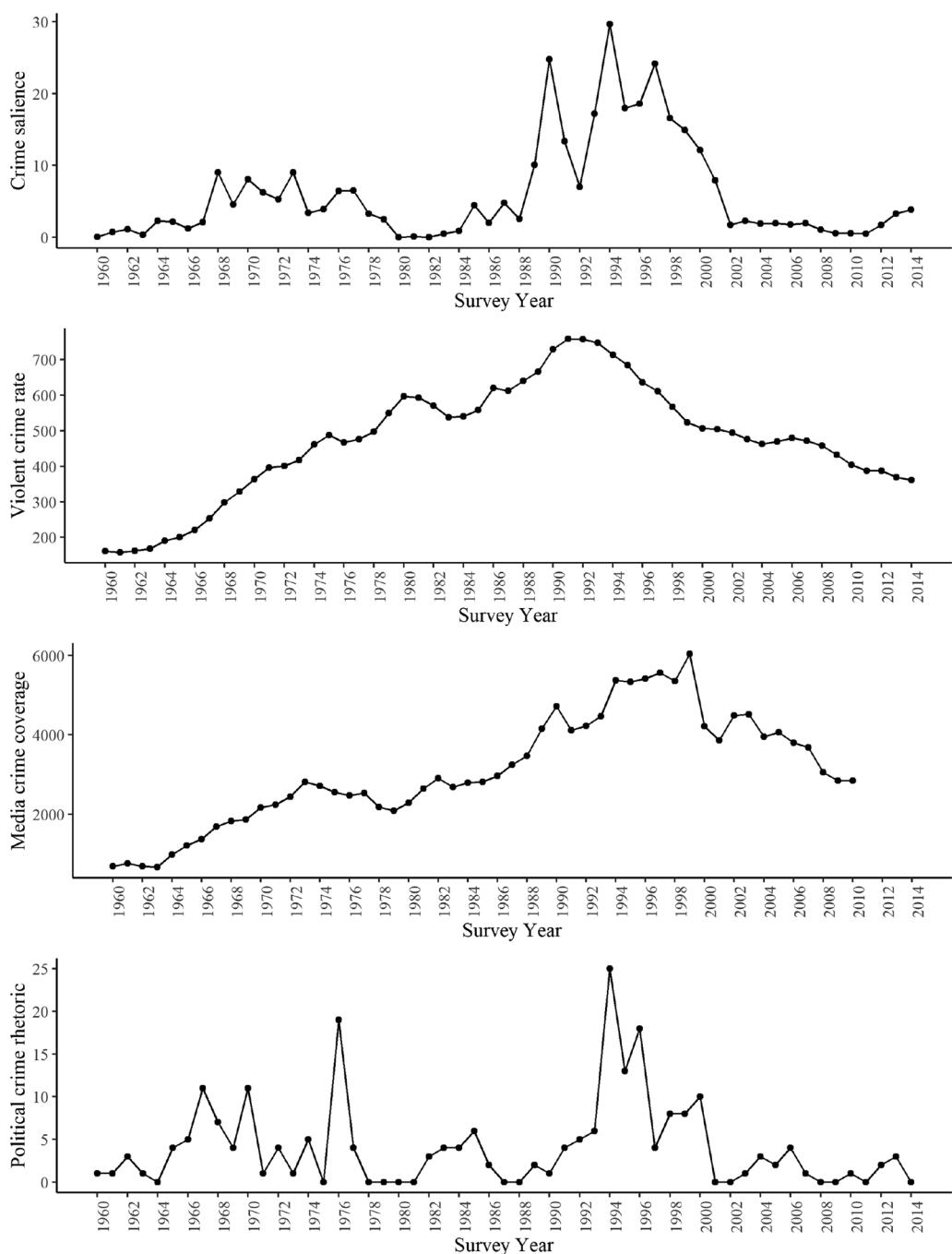
We code each of the 55 survey years as a separate time period and use seven period-level predictors from external data sources in the time-series analyses. Because our outcome variable is national in scope—Americans' perceptions of the country's MIP—we also measure our period-level predictors at the national level. The first, our core period-level predictor, is the U.S. crime rate. Similar to Anderson et al. (2017), we use data from Uniform Crime Reports (UCR) on the annual number of violent crimes in the United States per 100,000 residents.

Enns (2016) examined crime coverage in six major newspapers based in different cities (e.g., *Los Angeles Times*, *Boston Globe*). He found that, in the same way that local crime trends follow a national trend, “crime reporting in all six newspapers follows a common trajectory.” He thus developed and validated a national-level indicator of the amount of newspaper crime coverage: the frequency of articles in these six prominent newspapers mentioning the word “crime.” We use his measure in our analysis. Enns (2016) demonstrated the reliability of his word-coding methodology by constructing news measures for other issues (unemployment, inflation) and comparing them with objective data.

To measure political rhetoric, we follow previous studies and use presidents’ State of the Union (SOTU) addresses (Cohen, 1995; Oliver, 2002). Presidents are the most visible political figures (Manza & Cook, 2002). Their annual SOTU address is highly publicized, carefully thought out, and of key importance to their agenda-setting efforts (Cohen, 1995)—it is “the central battleground for presidential priorities” (Light, 1982, p. 160). In our main analysis, we thus use an indicator of the intensity of political rhetoric about crime that is equal to the total number of times the president mentioned the words “crime,” “criminal,” or “offender” in that year’s SOTU address.<sup>6</sup> Figure 1 shows the trends in our core period-level predictors and in crime salience perceptions.

<sup>5</sup>We use grand-mean centering to address the problems with estimation of intercept in original metric and make the intercept more interpretable. With the age variable grand-mean-centered, the intercept is no longer the expected value of crime salience when a respondent is at age 0 but, instead, when the respondent is at the mean age of the population. With the current grand-mean-centering approach, the age coefficient can be interpreted as the expected change in the outcome when the respondent is one year older/younger than the population’s mean age.

<sup>6</sup>In 1981, President Carter gave the SOTU address on January 16, and days later, on January 20, President Reagan was sworn into the office. President Reagan did not give his first SOTU address until the next year in 1982. This tradition that the newly inaugurated presidents do not give SOTU addresses in their first years has continued since then. Therefore, in 5 years (1981, 1989, 1993, 2001, and 2009), we use the presidents’ first major address to the Congress as a proxy of the SOTU address. Although those addresses were not officially SOTU addresses, they were highly publicized and served the same function as the SOTU addresses for the newly inaugurated presidents.



**FIGURE 1** Trends in crime salience, violent crime, media coverage, and political rhetoric

Note: For crime salience, violent crime rate, and political rhetoric,  $N = 55$ ; for media coverage,  $N = 51$ .

The changing problem status of the economy and national security—two social issues that are always important but only sometimes problematic—may influence the public salience of crime, as measured with MIP responses (Jennings & Wlezien, 2015; Wlezien, 2005). Accordingly, we include controls for the problem status of these social issues. Following Wlezien (2005), our first measure

of the state of the economy is the average annual value of the leading economic indicators (LEI) index, which comes from the Commerce Department and is based on nine objective indicators and one subjective indicator of the condition of the U.S. economy.<sup>7</sup> Higher values of the LEI index indicate better economic conditions. We also control for the inflation rate as calculated with the current consumer price index published by the Bureau of Labor Statistics. Controlling for economic conditions is important because they are a cause of crime trends and, thus, may confound the relationships we examine (Rosenfeld, 2018). To control for the state of national security and foreign policy, we use two binary indicators. The first indicates whether the survey was conducted during a Cold War year (no = 0, yes = 1), and the second indicates whether the United States was engaged in the Vietnam War, the Gulf War, or the Iraq/Afghanistan War during the survey year (no = 0, yes = 1).

### 3 | ANALYTIC STRATEGY

#### 3.1 | HAPC modeling: Variance decomposition

In the first part of our analysis, we focus on our first research question, and test the prediction common to both the objectivist and constructionist models that over time changes in crime salience mostly reflect period effects. To do this, we estimate age-period-cohort models. Increasingly, scholars are using these models to examine social change, aging, and population processes (Anderson et al., 2017; Gray et al., 2018; O'Brien, 2015; Yang & Land, 2016). The models have limitations, however. The most notable of which is the identification problem that results from the linear dependency between age, period, and cohort ( $\text{cohort} = \text{period} - \text{age}$ ). HAPC models circumvent the identification issue by not assuming additive age, period, and cohort effects.

The starting point for HAPC modeling is to conduct model specification tests to identify whether age, period, and cohort factors each uniquely explain variation in crime salience. Here, we compare a full fixed-effects APC logistic regression model with several partial models (Yang, Fu, & Land, 2004). We then estimate cross-classified HAPC models to tease out the crime salience variation that is uniquely attributed to cohort and period effects when individual-level characteristics are held constant. HAPC modeling deals with the identification problem of APC analysis by treating repeated-cross sectional survey data as having a hierarchical structure where respondents are nested in, and cross-classified by, two higher level units of social context—period and birth cohort (Yang & Land, 2006). In HAPC models, age and other individual-level characteristics are level-1 variables, whereas periods and cohorts are cross-classified level-2 units.<sup>8</sup> In the HAPC models we estimate, the individual-level equation is

<sup>7</sup>The nine objective indicators include 1) average weekly hours, 2) average weekly initial claims for unemployment insurance, 3) manufacturers' new orders for consumer goods and materials, 4) ISM Index of New Orders, 5) manufacturers' new orders for nondefense capital goods excluding aircraft orders, 6) building permits for new private housing units, 7) Standard & Poor's 500 stock index, 8) interest rate spread between 10-year Treasury bonds and federal funds, and 9) Leading Credit Index. The subjective indicator is the index of consumer expectation released by the University of Michigan.

<sup>8</sup>HAPC modeling has several limitations (see reviews by Bell & Jones, 2015; Luo & Hodges, 2016; O'Brien, 2017). Therefore, we provide a series of supplemental analyses where we implement several alternative APC models to examine the age, period, and cohort effects on crime salience. Some of these alternative analyses are presented in appendix tables G1–G3 in the online supporting information; other supplementary analyses are available by request.

as follows:

$$\begin{aligned} \text{Logit}(Crime \ Salience)_{ijk} = & \beta_{0jk} + \beta_1 \text{Age}_{ijk} + \beta_2 \text{Age}_{ijk}^2 + \beta_3 \text{Gender}_{ijk} + \beta_4 \text{Race}_{ijk} \\ & + \beta_5 \text{Republican}_{ijk} + \beta_6 \text{Income}_{ijk} + \beta_7 \text{College}_{ijk} + \beta_8 \text{Urban}_{ijk} \\ & + \beta_9 \text{South}_{ijk} + e_{ijk} \end{aligned}$$

A logit link function accounts for the dichotomous outcome variable (1 = choose crime as the MIP, 0 = choose other issues as the MIP). Individual  $i$  is nested in, and cross-classified by, a specific period ( $k$ ) and a specific birth cohort ( $j$ ).  $\beta_{0jk}$  is the intercept for respondents in cohort  $j$  and period  $k$ ,  $e_{ijk}$  is the individual-level error term, and  $\beta_1$  to  $\beta_9$  represent the fixed effects of individual-level predictors. The level-2 model is as follows:

$$\beta_{0jk} = \gamma_0 + \mu_{0j} + \vartheta_{0k}$$

In this equation,  $\gamma_0$  is the model intercept, which is the overall mean of crime salience;  $\mu_{0j}$  represents random effects for cohort  $j$  and  $\vartheta_{0k}$  represents random effects for period  $k$ . These two components of the level-2 equation are used to estimate period effects (when cohort effects and all the individual-level factors are held constant) and cohort effects (when period effects and all the individual-level factors are held constant). In the HAPC models, the period categories are the individual survey years ( $n = 55$ ) and the generational cohorts are 5-year groupings of birth years ( $n = 18$ ). Unfortunately, HAPC modeling is inappropriate for examining period-level predictors when there is autocorrelation and/or nonstationarity, and thus, we use a different modeling strategy to test our period-level research questions.

### 3.2 | ARMAX modeling in first differences: Explaining period effects

In the second part of the analysis, we focus on our second research question, and examine the relationship between crime salience, crime rates, and political rhetoric and media coverage. The regressand in the period-level analysis is the percentage of the U.S. population that selected crime as the MIP in the specific year (averaged across surveys). In the subgroup analysis, it is the percentage of the U.S. population subgroup (e.g., males and females) that did so. Unfortunately, Enns's (2016) media measure is only available from 1960 to 2010, and thus this portion of our analysis is focused on that time period ( $n = 50$ ). We follow Philips's (2018, p. 233) ARDL-bounds procedure to determine the appropriate method for the time-series analyses.

Evidence from a suite of unit root tests—Augmented Dickey–Fuller, Phillips–Perron, Dickey–Fuller GLS, and Kwiatkowski–Phillips–Schmidt–Shin—shows that the regressand is nonstationary both for the U.S. population and for the subgroups examined. Several regressors, including the violent crime rate and news coverage, are also nonstationary, which may explain the large bivariate correlations between the levels of these variables observed in prior work (Miller, 2013, 2016). Unless these variables are cointegrated, however, these level relationships may be spurious (Enders, 2014; Grant & Lebo, 2016). The results of an ARDL bounds test and a supplementary Johansen test both show that the nonstationary regressand and regressors are *not* cointegrated, indicating that the data must be analyzed in first differences (Grant & Lebo, 2016; Philips, 2018). The findings from an examination of the errors from the first-difference model using the autocorrelation function, partial autocorrelation function, and Breusch–Godfrey test indicate that they follow a second-order autoregressive process. To account

for the AR(2) disturbances, we use Stata's "arima" command to estimate ARMAX models in first differences.<sup>9</sup> Test results revealed that this produces white noise residuals.

### 3.3 | Correlation and cointegration: Parallel publics

To answer our third research question about whether subgroup trends are parallel or divergent, we examine the bivariate correlations between the trends. Nonstationarity is less important when the goal is to describe rather than explain trends (McDowall & Loftin, 2009). Nonetheless, we examine the correlations between subgroup trends in both levels and differences. We also conduct Engle–Granger cointegration tests for the mutually exclusive subgroups (e.g., Whites vs. non-Whites and males vs. females), normalizing on both trends (Enders, 2014). A finding of cointegration implies that the two subgroups share a common trend—specifically, that there is a stable, long-term relationship between the subgroups' crime salience perceptions in levels (Enders, 2014; Stock & Watson, 1988).

## 4 | RESULTS

We begin with the APC model specification tests. Appendix B1 in the online supporting information presents the comparisons of model fit for the full APC fixed-effects model and several reduced models. It shows the model fit statistics for each model and the likelihood ratio tests between the models. The results indicate that the full APC model provides the best model fit.

Next, we estimate HAPC models to disentangle age, period, and cohort effects on crime salience, while examining the effects of various individual-level predictors. Table 1 presents the results of two different HAPC models of crime salience. The first model is the baseline HAPC model. It includes only the individual-level age variables, using both linear and quadratic terms, and estimates period and cohort effects as cross-classified level-2 random intercepts. The second model includes the other individual-level variables. Appendix B2 in the online supporting information plots the predicted probabilities from these models for the age, period, and cohort effects.

As shown in table 1, age has a statistically significant but small curvilinear effect on crime salience, which is positive with a decelerating slope. This finding is unchanged in the second model, in which we introduce the other individual-level variables. The first panel in appendix B2 plots the predicted probabilities by age. The curve reveals that Americans in their 50s and 60s are most likely to say crime is the country's MIP. The flatness of curve shows that the amount of the variation in crime salience that is a result of the aging process is very small, much smaller than the amount of variation that results from period effects. As with age, the amount of variation resulting from cohort effects is small, much smaller than that resulting from period effects (.106 vs. 3.519). Including individual-level predictors in model 2 further reduces this variation by ~13 percent (from .106 to .092), although it remains significant. The third panel of appendix B2 plots the predicted probabilities by cohort and shows that pre-1910 and post-1970 cohorts are the most likely to choose crime as the MIP.

The findings in model 2 of table 1 show that females, non-Whites, non-Republicans, the less educated, those with higher incomes, urban dwellers, and southerners are all significantly more likely to perceive crime as the country's MIP. The main takeaway for our first research question from the results in table 1 and from the predicted probabilities in appendix B2, however, is that most variation over

<sup>9</sup>Specifically, the models are estimated using maximum likelihood with the Kalman filter and include two lags of the outcome variable ( $\Delta$  crime salience). We use robust standard errors to account for heteroskedasticity.

**TABLE 1** Hierarchical logistic age-period-cohort models of crime salience

Fixed and Random Effects	Model 1		Model 2	
	b	(SE)	b	(SE)
Individual-Level Predictors				
Intercept	-3.495***	(.061)	-3.342***	(.073)
Age	.025***	(.000)	.029***	(.000)
Age-squared <sup>a</sup>	-.002***	(.000)	-.002***	(.000)
White	—	—	-.488***	(.001)
Female	—	—	.267***	(.001)
Republican	—	—	-.009***	(.001)
Income	—	—	.005***	(.000)
College degree	—	—	-.214***	(.001)
Urban	—	—	.304***	(.001)
South	—	—	.162***	(.001)
Variance Components				
Period	3.519***		3.562***	
Birth cohort	.106***		.092***	
N (Individual)	422,504		422,504	
N (Period)	55		55	
N (Cohort)	18		18	

Notes: Omitted reference categories are non-White, male, Democrat/Independent, no college degree, rural/suburban, other regions.

Abbreviation: SE = standard error.

<sup>a</sup>Coefficients and standard error of age-squared are multiplied by 10.

\**p* < .05; \*\**p* < .01; \*\*\**p* < .001 (two-tailed).

**TABLE 2** Period-level predictors of crime salience for full population: ARMAX (2,0,0) models of first differences (*N* = 50)

Variables	Model 1		Model 2		Model 3	
	Coef.	(SE)	Coef.	(SE)	Coef.	(SE)
Violent crime rate	.022	(.020)	.026	(.018)	.007	(.011)
News coverage	—	—	—	—	.005***	(.001)
Political rhetoric	—	—	—	—	.135*	(.053)
Inflation	—	—	-.200	(.265)	-.031	(.260)
LEI index	—	—	.158	(.101)	-.013	(.103)
Active War	—	—	2.337	(3.245)	1.501	(2.380)
Cold War	—	—	-4.684	(8.438)	-1.558	(3.393)

Abbreviations: Coef. = coefficient; SE = standard error.

\**p* < .05; \*\**p* < .01; \*\*\**p* < .001 (two-tailed).

time in crime salience is at the period level. This finding provides support for a fundamental tenet of both the objectivist and construction models.

Attention now turns to explaining the observed period-level variation (the second research question). Table 2 presents the results of three ARMAX (2, 0, 0) models predicting crime salience for the full population. Model 1 only includes the violent crime rate, model 2 adds the controls, and model 3 incorporates news coverage and political rhetoric. The results in table 2 are striking. Even when news

coverage, political rhetoric, and the controls are excluded, changes in the violent crime rate are not significantly associated with changes in crime salience (model 1). The results remain unchanged in the full model (model 3). Instead, what the results show is that the only significant predictors of changes in crime salience are changes in news coverage and political rhetoric.

To explore the possibility that crime may exert an indirect effect through news coverage, we examine whether changes in violent crime are associated with changes in news coverage (not shown). Although the bivariate correlation between these variables in levels is large ( $r = .78$ ), it is spurious. Both variables are nonstationary, and their correlation in differences is small and nonsignificant ( $r = .11, p = .444$ ). Regressing crime coverage on violent crime in first differences (with period-level controls) shows that changes in crime are not significantly associated with changes in the news (not shown). In sum, we find no support for the objectivist model. Our findings are thus entirely consistent with the social constructionist model.

An important question is whether the period-level predictors of crime salience differ for different population subgroups. To answer this question, we estimate models 2 and 3 from table 2 separately for 14 population subgroups (e.g., male vs. female). The subgroup findings are shown in table 3. Changes in violent crime are not significantly associated with changes in crime salience in any subgroup. Changes in news coverage, however, are positively and significantly associated with changes in crime salience in every subgroup. Political rhetoric also has a significant and positive effect for most (12/14) subgroups, although its standardized effect is only about half that of media coverage (not shown).

These findings lend preliminary support to the parallel public hypothesis. Media coverage has approximately the same effect size ( $b = .005$  to  $.006$ ) in every population subgroup, and there is no evidence that the effect of political rhetoric varies markedly across racial or political groups. The respective coefficients among Whites and non-Whites are similar ( $b = .134$  vs.  $.151$ ), as are those among Republicans and non-Republicans ( $b = .114$  vs.  $.152$ ), and in neither case do they differ significantly, per Paternoster, Brame, Mazerolle, and Piquero (1998, p. 862) slope-difference test ( $Z = .203, p = .839$  and  $Z = .502, p = .616$ , respectively). In fact, the positive effect of political rhetoric only fails to reach significance in two subgroups, females and those with low income, and only the gender difference is significant.

In the last portion of the analysis, we explore whether long-term trends in crime salience are parallel or divergent across population subgroups (the third research question). First, we examine the descriptive trends in crime salience between 1960 and 2014 for different age groups and generations/birth cohorts (figure 2). For illustrative purposes, respondents are classified into four age groups (18–29, 30–44, 45–59, and 60+) and four broad cohorts, which correspond to the Lost and Greatest generations (pre-1924), Silent Generation (1925–1944), Baby Boom Generation (1945–1965), and generations X and Y (post-1966). The trend lines are mostly parallel across all age and cohort groups. In each group, the percentage of respondents identifying crime as the MIP increases from the late-1960s to mid-1970s falls in the late 1970s, rises steeply again from the late-1980s to a peak in the mid-1990s, and then declines later.

Figure 3 shows the descriptive trends in crime salience for other population subgroups—demographic, socioeconomic, and partisan. The figure has seven panels, which separately graph changes over time in crime salience by gender, race, education, income, partisan identification, urbanicity, and region. Here, too, the trends lines are mostly parallel across groups. In each group, the salience of crime increases from the late-1960s to the mid-1970s falls in the late 1970s, rises steeply beginning in the late-1980s, peaks in the mid-1990s, and then declines after that.

Table 4 provides the bivariate correlations between subgroup crime salience trends in both levels and differences. There are boxes around the correlations for mutually exclusive subgroups. All

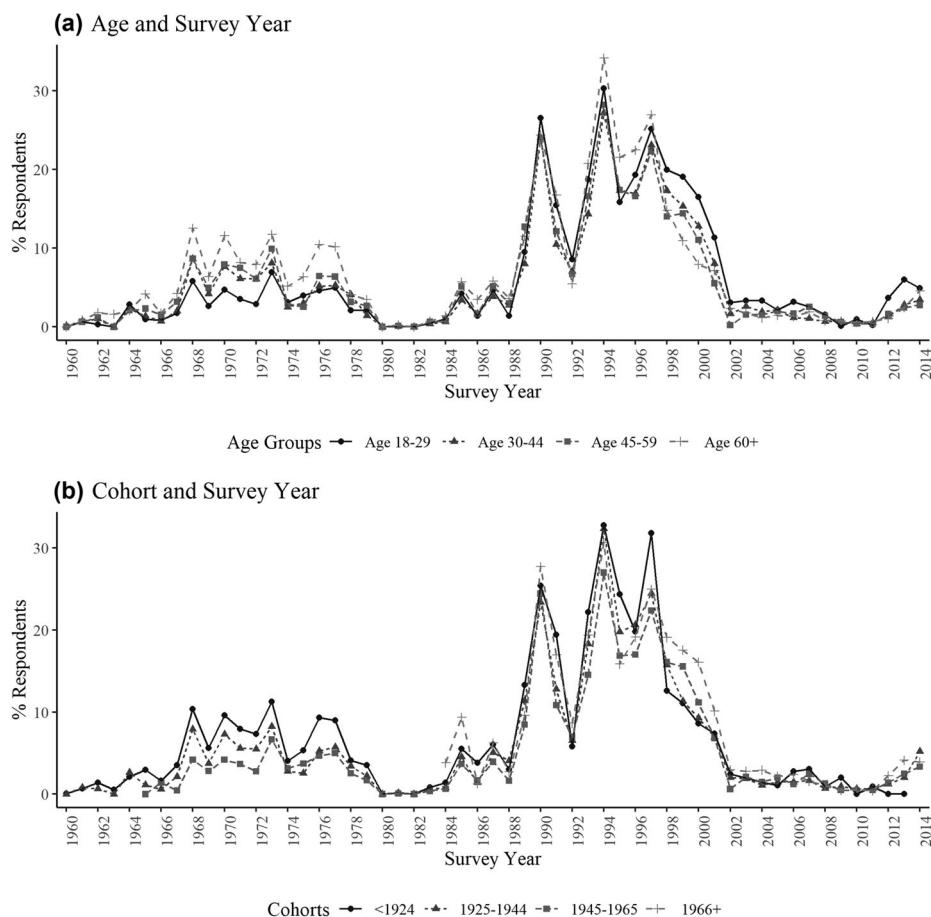
**T A B L E 3** Subgroup analyses: ARMAX (2, 0) models of first differences

Variables	Male		Female	
	Model 1	Model 2	Model 3	Model 4
Violent crime rate	.028 (.018)	.011 (.011)	.026 (.019)	.002 (.013)
News coverage	—	.005 (.001)***	—	.006 (.001)***
Political rhetoric	—	.231 (.058)***	—	.045 (.054)
Variables	White		Non-White	
	Model 5	Model 6	Model 7	Model 8
Violent crime rate	.025 (.018)	.005 (.011)	.035 (.020)	.017 (.013)
News coverage	—	.005 (.001)***	—	.006 (.002)**
Political rhetoric	—	.134 (.055)*	—	.151 (.063)*
Variables	Republican		Democrat/Independent	
	Model 9	Model 10	Model 11	Model 12
Violent crime rate	.030 (.018)	.010 (.012)	.024 (.019)	.004 (.012)
News coverage	—	.005 (.001)***	—	.006 (.001)***
Political rhetoric	—	.114 (.051)*	—	.152 (.056)**
Variables	Low Income		High Income	
	Model 13	Model 14	Model 15	Model 16
Violent crime rate	.025 (.019)	.006 (.012)	.027 (.019)	.006 (.011)
News coverage	—	.006 (.001)***	—	.006 (.001)***
Political rhetoric	—	.113 (.063)	—	.160 (.055)**
Variables	No College Degree		College Degree or Above	
	Model 17	Model 18	Model 19	Model 20
Violent crime rate	.027 (.018)	.008 (.012)	.023 (.019)	.002 (.011)
News coverage	—	.005 (.001)***	—	.005 (.001)***
Political rhetoric	—	.134 (.058)*	—	.141 (.038)**
Variables	Urban		Rural/Suburban	
	Model 21	Model 22	Model 23	Model 24
Violent crime rate	.026 (.021)	.005 (.014)	.027 (.019)	.006 (.011)
News coverage	—	.006 (.002)***	—	.006 (.001)***
Political rhetoric	—	.210 (.066)**	—	.129 (.055)*
Variables	South		Non-South	
	Model 25	Model 26	Model 27	Model 28
Violent crime rate	.034 (.020)	.016 (.014)	.023 (.018)	.003 (.011)
News coverage	—	.005 (.001)***	—	.005 (.001)***
Political rhetoric	—	.152 (.068)*	—	.130 (.052)*

Notes: Presented are unstandardized coefficients (standard errors in parentheses). All models include period-level controls. N = 50.

\*p < .05; \*\*p < .01; \*\*\*p < .001 (two-tailed).

correlations are very large, indicating the trends are parallel. Even the correlations between the first-differenced trends for mutually exclusive subgroups are very large ( $r > .90$ ). The findings from formal Engle–Granger cointegration tests (appendix C) reveal that the crime salience trends for each of the mutually exclusive subgroups are cointegrated. Therefore, there is no evidence that trends in crime salience differ markedly for Whites versus non-Whites, or for Republicans vs. non-Republicans, as is indicated by the constructionist model and scholarship on the racialized politicization of the crime issue (Beckett & Sasson, 2004; Weaver, 2007). Instead, the picture that emerges is one of



**FIGURE 2** Crime salience trends, by age groups and generations ( $n = 422,504$ )

Note: Weighted prevalence estimates are shown.

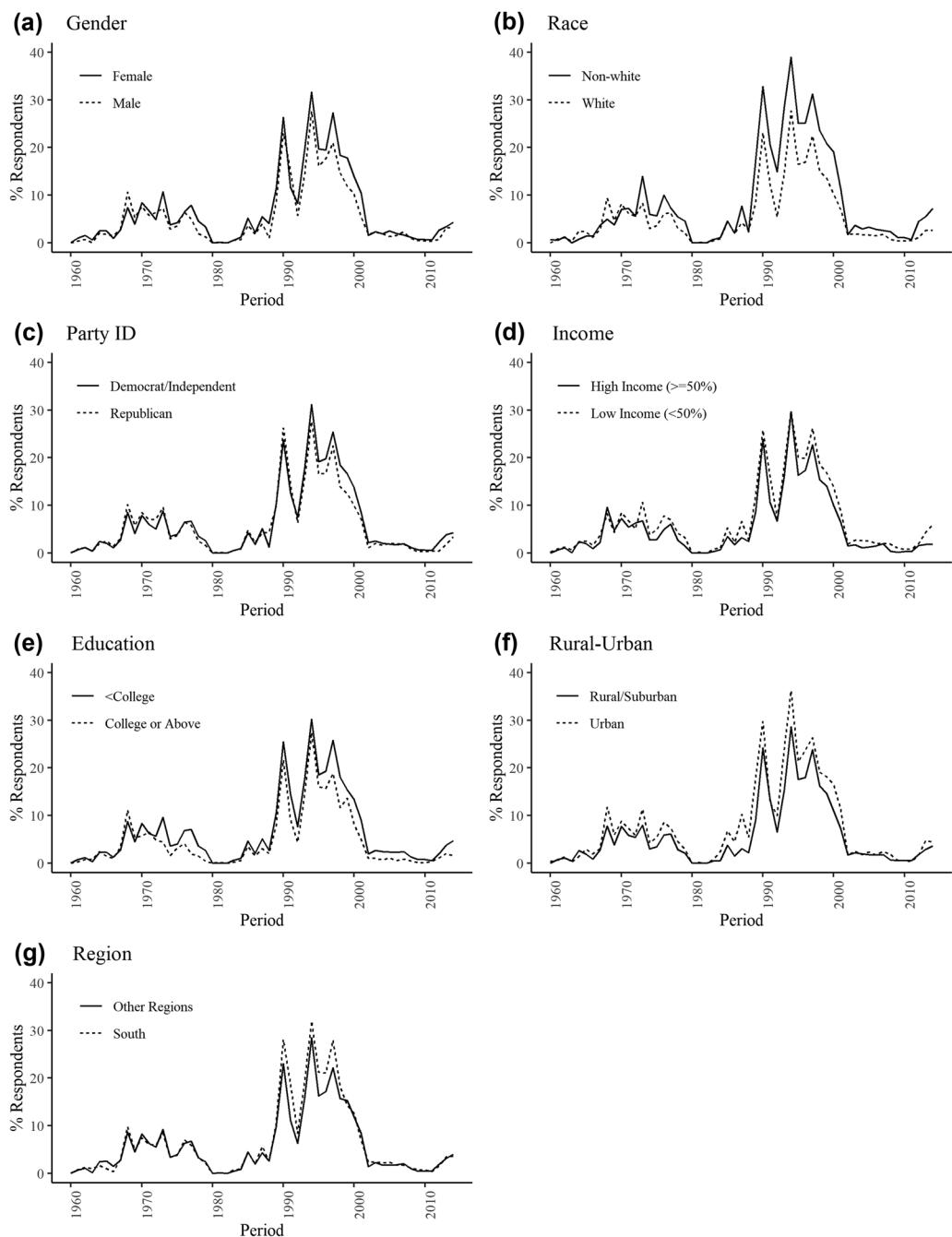
parallel publics (Page & Shapiro, 1992), which is consistent with past findings for tends in punitive attitudes (Enns, 2016; Ramirez, 2013).

#### 4.1 | Supplemental analyses

We estimate several supplemental models to test the robustness of our findings.<sup>10</sup> First, we replace the UCR violent crime rate with four alternative measures of crime: 1) the UCR homicide rate, 2) the Centers for Drug Control and Prevention homicide mortality rate, 3) the UCR index crime rate (minus arson), and 4) the National Crime Victimization Survey serious violent crime rate.<sup>11</sup> The models using

<sup>10</sup>In addition to the appendices, we also estimate HAPC models 1) using different coding schemes for age, periods, and cohorts, as well as different specifications; 2) using equal intervals for categorizing age, periods, and cohorts, per Luo and Hodges (2016); 3) treating age and periods as fixed effects and cohorts as random effects; and 4) treating age and cohort as fixed effects and period as random effects. In all of these analyses, the main takeaway is the same as in our main models. For illustration purposes, we presented some of these models in appendix tables G1 to G3 in the online supporting information. The results of other supplementary models are available by request.

<sup>11</sup>To measure NCVS violence, we use Lauritsen, Rezey, and Heimer (2016) estimates, which account for the 1992 change in NCVS methodology and include series victimizations. We thank the editor for sharing this data with us.

**FIGURE 3** Crime salience trends, by population subgroup ( $n = 422,504$ )

Note: Weighted prevalence estimates are shown.

**TABLE 4** Period-level correlations for subgroup crime salience trends, in levels and differences

<b>Levels</b> (N = 55)	<b>1</b>	<b>2</b>	<b>3</b>	<b>4</b>	<b>5</b>	<b>6</b>	<b>7</b>	<b>8</b>	<b>9</b>	<b>10</b>	<b>11</b>	<b>12</b>	<b>13</b>
1. Male	1.00												
2. Female	.973*	1.00											
3. White	.993*	.990*	1.00										
4. Non-White	.962*	.983*	.967*	1.00									
5. Republican	.992*	.980*	.994*	.961*	1.00								
6. Non-Repub.	.985*	.995*	.993*	.982*	.980*	1.00							
7. Low Income	.985*	.993*	.992*	.986*	.985*	.995*	1.00						
8. High Income	.990*	.988*	.996*	.965*	.990*	.992*	.984*	1.00					
9. No Degree	.990*	.995*	.996*	.983*	.989*	.998*	.998*	.992*	1.00				
10. Degree	.982*	.968*	.984*	.942*	.979*	.975*	.967*	.988*	.973*	1.00			
11. Urban	.972*	.986*	.981*	.977*	.977*	.984*	.983*	.980*	.984*	.970*	1.00		
12. Non-Urban	.991*	.992*	.997*	.977*	.990*	.996*	.994*	.995*	.998*	.979*	.977*	1.00	
13. South	.993*	.983*	.991*	.979*	.989*	.989*	.992*	.988*	.994*	.974*	.975*	.994*	1.00
14. Non-South	.987*	.996*	.997*	.976*	.989*	.996*	.993*	.994*	.997*	.979*	.987*	.996*	.986*
<b>Differences</b> (N = 54)	<b>1</b>	<b>2</b>	<b>3</b>	<b>4</b>	<b>5</b>	<b>6</b>	<b>7</b>	<b>8</b>	<b>9</b>	<b>10</b>	<b>11</b>	<b>12</b>	<b>13</b>
1. Male	1.00												
2. Female	.909*	1.00											
3. White	.976*	.972*	1.00										
4. Non-White	.886*	.939*	.907*	1.00									
5. Republican	.965*	.967*	.991*	.901*	1.00								
6. Non-Repub.	.966*	.977*	.988*	.950*	.970*	1.00							
7. Low Income	.954*	.965*	.973*	.959*	.966*	.982*	1.00						
8. High Income	.957*	.962*	.985*	.882*	.975*	.975*	.932*	1.00					
9. No Degree	.971*	.978*	.993*	.943*	.986*	.994*	.987*	.973*	1.00				
10. Degree	.937*	.925*	.956*	.853*	.949*	.943*	.910*	.966*	.933*	1.00			
11. Urban	.914*	.949*	.937*	.940*	.923*	.965*	.942*	.933*	.950*	.916*	1.00		
12. Non-Urban	.971*	.973*	.996*	.916*	.989*	.986*	.972*	.983*	.992*	.951*	.926*	1.00	
13. South	.980*	.937*	.977*	.917*	.975*	.970*	.974*	.950*	.981*	.929*	.907*	.976*	1.00
14. Non-South	.956*	.986*	.991*	.932*	.981*	.992*	.973*	.982*	.991*	.950*	.963*	.989*	.955*

Notes: Trends in the percentage of each subgroup selecting crime as the MIP from 1960 to 2014. Mutually exclusive subgroups are included within boxes.

\*p < .05 (two-tailed).

these alternative measures of crime all produce the same findings: 1) Crime does not have a significant effect, but 2) news coverage and political rhetoric do (appendix tables D1–D8 in the online supporting information). Given the generality of our outcome variable, we also estimated models using the CDC unintentional drug overdose death rate, which is negatively related to crime salience; the other findings are the same (appendix tables D9–D10).

Next, we estimate supplementary models using two alternative measures of political rhetoric (appendix E). The first alternative measure is based on SOTU addresses, like our main indicator, but

also counts the number of times the president mentions the word “drug” in the context of illegal drugs. The findings using this measure are the same. The second alternative indicator comes from Ramirez (2013). He analyzed all of the Public Papers of the President, not just SOTU addresses, and coded every sentence that mentioned the words “crime” or “criminal behavior” as either a permissive (pro-punitive) frame, a treatment frame, or neither. He then created a measure of net punitive tone by subtracting the number of treatment statements from the number of pro-punitive statements. When we use Ramirez’s (2013) measure, the effects of political rhetoric are slightly less robust.

We also estimate supplementary models after replacing newspaper crime coverage with TV crime coverage (appendix F). The TV crime coverage variable comes from the *Vanderbilt Television News Archive* and is equal to the average number of times the word “crime” was mentioned in ABC, CBS, and NBC news from 1969 to 2014 ( $\alpha = .948$ ). In prior studies, other scholars have also used this data source (e.g., Hill et al., 2012). Just like newspaper crime coverage, TV news coverage has a positive ( $b = .112$  to  $.153$ ) and highly significant effect ( $p < .01$ ) on crime salience for the full population and for every population subgroup. The effects of political rhetoric, however, are mostly nonsignificant in these supplementary models.

## 5 | DISCUSSION AND CONCLUSION

Our study is the most comprehensive analysis of the public salience of crime to date. We analyzed 422,504 responses to 414 surveys administered to probability samples of the U.S. public between 1960 and 2014. We learned six things. First, a fundamental prediction common to all leading criminological theories of crime salience is correct. Most variation over time in public concern about crime is at the period level. In comparison, the effects of age and birth cohort are very small. Second, the effect of age is nonlinear, such that crime is most salient in midlife when Americans are in their 50s and 60s. Both of these findings—that period effects dominate those of age and birth cohort, and that age has a nonlinear effect—parallel those from Anderson et al. (2017) recent study of Americans’ support for capital punishment. Therefore, they may reflect something about the broader nature of public opinion on criminal justice, rather than anything specific to crime concern or death penalty attitudes.

Third, neither changes in the violent crime rate overall nor changes in the homicide rate specifically are associated significantly with changes in the public salience of crime. In fact, even at the bivariate level, changes in the violent crime rate and changes in the homicide rate both have small, nonsignificant associations with changes in crime salience ( $r = .08$ ,  $p = .550$  and  $r = .11$ ,  $p = .430$ , respectively). Roberts et al. (2003, p. 22) have argued that “responses to [the MIP question] … reveal the dissociation between public views and crime statistics.” Weaver (2007, p. 233) has argued likewise: “[T]here is virtually no empirical correlation … between the incidence of crime and public concern with crime.” But other scholars have disagreed (e.g., Miller, 2013, 2016). The source of the confusion seems to be the nonstationarity of the respective variables, which results in large spurious correlations between their *levels*. For example, the bivariate correlation between violent crime and crime salience in *levels* is large, positive, and highly significant ( $r = .543$ ,  $p < .001$ ). When analyzed appropriately (in first differences), however, these variables are weakly and nonsignificantly related, which strongly supports Roberts et al. (2003) and Weaver’s (2007) arguments, and casts serious doubt on objectivist theory.

Fourth, media coverage does not follow the crime rate. Neither changes in the violent crime rate, nor changes in the homicide rate, are significantly associated with changes in the amount of news devoted to crime. Pickett (2019, p. 422) recently argued that “despite persistent biases in how the traditional media cover crime … the amount of coverage closely follows the crime rate.” Enns (2016) examined descriptive trends in news and crime and came to a similar conclusion: “[N]ewspaper coverage of crime

did indeed track the rising crime rate. In fact, the two series move virtually in tandem.” Likewise, Miller (2016) found that both the violent crime rate and the homicide rate have large positive correlations with newspaper crime coverage ( $r = .60$  to  $.80$ ). Again, the mistake has been to overlook the implications of nonstationarity. The U.S. crime rate and newspaper crime coverage are both nonstationary. Although they are strongly correlated in levels in our data ( $r = .780$ ,  $p < .001$ )—as nonstationary variables often are (Granger & Newbold, 1974)—they are not when examined in first differences ( $r = .111$ ,  $p = .444$ ). The same is true in the case of television crime news. Therefore, the hypothesis from objectivist theory that crime trends indirectly affect public opinion via changes in media crime coverage does not withstand empirical scrutiny.

Fifth, changes in newspaper coverage and changes in television news are both positively associated with changes in the public salience of crime. These relationships are large and highly significant for the full population and for every subpopulation. Additionally, the magnitude of the media effect is roughly identical in every subpopulation. Thus, even though media crime coverage does not follow the crime rate, it still strongly influences public opinion. Political rhetoric also has a statistically significant, positive effect on crime salience for the full population and for most population subgroups. There is thus strong support for the social constructionist model’s prediction that the framing of social problems by the media and political figures shapes how the public views these problems (Beckett, 1994, 1997). The only hypothesis from social constructionist theory that is not supported pertains to the moderating effects of race and political party. Media crime coverage and political rhetoric both seem to have similar effects on Whites and non-Whites, as well as on Republicans and non-Republicans.

Sixth, trends in crime salience are nearly identical across demographic, socioeconomic, and partisan groups. The correlations between subgroup trends are very large, and the trends are cointegrated—they exhibit an equilibrium relationship, moving together in the long run. This finding contrasts theoretical accounts, like Garland’s (2001), that indicate that the salience of crime increased in recent decades for some population subgroups (e.g., the middle classes) but not for others. Our findings show that at least since 1960, when the salience of crime increased, it did so for all major population subgroups at about the same time. Crime salience also peaked for all subgroups in the mid-1990s and then fell steadily thereafter. When added to the existing evidence about trends in punitive attitudes (Anderson et al., 2017; Enns, 2016; Ramirez, 2013), the takeaway is that for aggregate views about both crime and punishment, parallel opinion change is the norm. Therefore, any valid theory of aggregate attitudes toward criminal justice must be able to account for parallel opinion change. Page and Shapiro (1992) theorized that what explains the existence of parallel publics is national media coverage, which transmits similar information about changes in objective realities to all groups. The findings herein are only partially consistent with their theory. All subpopulations seem to receive similar information about crime from the media, but that information does not reflect changes in objective reality.

What are the policy and research implications of our findings? There is strong evidence that aggregate punitiveness influences court decision-making, capital punishment policy, execution rates, incarceration rates, and criminal justice expenditures (Pickett, 2019). Both criminological theory (Zimring & Johnson, 2006) and evidence from other disciplines (Burstein, 2006; Lax & Phillips, 2012; Monroe, 1998), however, indicate that aggregate punitiveness should have a larger effect on policy when the salience of social problems is high. To our knowledge, scholars have not yet tested this possibility in the criminal justice context specifically. Therefore, an important direction for future research is to examine whether trends in crime salience and punitiveness have an interactive effect on criminal justice policy and practice. It may well be that objectivist theory best explains trends in punitiveness (Pickett, 2019), whereas social constructionist theory best explains trends in crime salience, and that the opinion-policy relationship thus reflects a nuanced mixture of the two theoretical models.

Second, our finding that media crime coverage does not follow the crime rate raises the question: What does it follow? We are not aware of any previous studies in which scholars have examined the determinants of changes over time in news stories about crime. In other disciplines, researchers have explored the determinants of media coverage of different issues, like the economy, and have found a complex relationship with objective conditions (Soroka et al., 2015). Soroka (2006), for example, found that increases in unemployment and inflation have much larger effects on news coverage than decreases. It would be useful to explore whether this is also true for crime trends and crime coverage. More generally, there is a need for additional studies aimed at both examining changes over time in the amount and nature of news stories about crime and exploring the antecedents of those changes.

Third, the relationship observed herein between media coverage and public opinion mostly predates the rise of Internet news. It is possible that in the Internet news era, “media portrayals of crime and the criminal justice system may be different than in the past” (Enns, 2016, p. 164). Perhaps the public’s attention to crime news will weaken as the Internet gives users more control over the content consumed. Although beyond the scope of our study, there is evidence in our data that the relationship between media coverage and public opinion weakened after the rise of the Internet age. The World Wide Web went public in mid-1991. The correlation between changes in newspaper crime coverage and changes in crime salience is smaller in the years afterward ( $r = .294, p = .222$ ) than in the years before ( $r = .687, p < .001$ ). There is also growing evidence that at the individual level, Internet news consumption, unlike traditional news consumption, is either unrelated or negatively related to fear of crime and punitiveness (Baranauskas & Drakulich, 2018; Roche et al., 2016). An important direction for future research, then, is to examine these associations at the aggregate level and over time.

Subsequent studies should also be aimed at exploring the cohort-level predictors of crime salience perceptions. The findings from our analysis revealed small but significant cohort effects, but identifying the explanations for these effects was beyond the scope of our analysis. Theoretically, cohort effects emerge when people form key social and political worldviews during early life (Mannheim, 1952) that remain stable thereafter and influence how they react to social and political events over the life course (Alwin & McCammon, 2007; Ryder, 1965). As new cohorts replace old ones, social and political attitudes can change over time (Alwin, 1990; Alwin & Krosnick, 1991). Cohort membership ties each birth cohort to a fixed temporal place in historical processes because people born around the same time are exposed to the same major events during the formative period (Jennings & Niemi, 1981; Sears & Funk, 1999). And cohort characteristics can influence this process. For example, larger cohorts may experience greater job competition, fewer employment opportunities, and weaker social controls (O’Brien, 1989; Steffensmeier, Streifel, & Shihadeh, 1992). It would thus be useful for researchers seeking to build on our study to evaluate whether factors like cohort size may explain the cohort effects we observed.

Our study has several limitations, which provide opportunities for additional research. First, following previous work (Beckett, 1994, 1997; Oliver, 1998), we focused on public opinion about the country’s MIP. Future research should be aimed at exploring trends in residents’ perceptions of the MIP in their local community, and how these perceptions relate to local crime trends and trends in local news coverage. Second, because of data limitations, we were unable to examine trends for specific non-White subgroups, like Hispanics and Blacks. Researchers should attempt to do so in the future. Third, the measure of political rhetoric in our main analysis, and the two alternative measures in our supplementary analyses, all were limited to presidential speeches or statements. Compared with other politicians, the president should have the most influence on public opinion (Enns, 2016; Manza & Cook, 2002). Nonetheless, researchers building on our work should examine whether different findings emerge with non-presidential measures of political rhetoric. Fourth, there is an ongoing debate about how best to deal with the identification problem in age-period-cohort models (Bell & Jones,

2015; Fienberg, 2013; Luo, 2013; Yang & Land, 2013). Scholars should thus explore whether similar findings emerge using alternative modeling approaches.

Another limitation of our analysis is the measurement of the outcome variable. Because of data constraints, we measured crime salience using Singer's (2011) coding of open-ended MIP responses, which lumped together responses focusing on different crime-related phenomena (e.g., crime, school violence, drugs, and gangs). It is possible that different crime rates may be related to some specific MIP responses but not to others. Researchers should thus examine whether the results of our study replicate using different measures of crime (e.g., school violence rate) and more specific crime-related MIP responses (e.g., school violence as MIP).

To conclude, our findings are most consistent with the social constructionist model of crime salience, but they also strongly support the notion of parallel publics. Changes in the U.S. crime rate have little effect on crime salience perceptions. This is true for the population as a whole and for specific population subgroups (e.g., Whites and non-Whites). Changes in the U.S. crime rate also have little effect on the number of news stories about crime. Nonetheless, when there are increases in media crime coverage and political rhetoric about crime, different demographic, socioeconomic, and partisan groups all become more concerned about crime, and they do so at about the same time and to about the same extent. Whether the effects of media coverage will change in the Internet news era remains to be seen. We hope that future studies will build on our analysis by exploring this and related questions.

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## SUPPORTING INFORMATION

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

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