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The Influence of COVID-19 Restrictions and Rhetoric in the U.S.: A Multi-Definition and Multi-Method Approach

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ABSTRACT

Xenophobic extremism directed towards Asian persons, as operationalized in a multitude of ways, was found to be especially heightened during the COVID-19 pandemic. The emotion-threat and emboldenment frameworks suggest that such extremism may have been the result of pandemic-related restrictions and rhetoric, which led to an increase in negative emotions directed toward the "outgroup," as further defined by political leaders. Informed by these perspectives, this study examines whether COVID-19 pandemic restrictions and rhetoric affected hate crime in the U.S. Using a multi-definition and multi-method approach, this research finds that both COVID-19 restrictions and rhetoric were related to state-level increases in hate crimes against Asian persons. However, subsequent analyses revealed that individual cities saw both reductions in hate crimes and null impacts, while quarterly analyses were unable to replicate findings that were observable using monthly data. This suite of results highlights the need for a triangulation strategy when examining questions related to hate crime.

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Hate crime; quantitative; COVID-19; anxiety-threat; emboldenment

Introduction

The COVID-19 pandemic resulted in heightened xenophobic extremism targeting Asian persons,[1] evident across a wide variety of outcomes (Han et al., 2023; Hartman et al., 2020; He et al., 2020; Jeung et al., 2021; Lantz & Wenger, 2023; Wenger & Lantz, 2022). Within the growing literature seeking to understand what drove these increases, and although the specific phrasing varies, the "emotion-threat" and emboldenment perspectives have emerged as two of the most well-supported explanations. The former posits that affective states brought on by macro-level events like that of the COVID-19 pandemic can lead to prejudice against outgroup members (Bodenhausen,

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[1]We use the term "Asian" throughout the manuscript in lieu of "AAPI" as the latter is specific to naturalized immigrants of Asian origin (or their descendants).

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1993; Brooks et al., 2020; Faulkner et al., 2004; Kessler et al., 2010; Ren & Feagin, 2021; Stephan & Stephan, 2000). Here, the mechanism tying heightened COVID-related negative emotions like that of anxiety and fear (Chae et al., 2021; Kan et al., 2021) to that of hate-motivated behaviors is individual-level (mis)perceptions associated with whom is most likely to be vulnerable to the disease (Faulkner et al., 2004; Lantz et al., 2023). The latter perspective, that of the emboldenment framework, hypothesizes that these (mis)perceptions can be exacerbated by the rhetoric of state leaders (Darling-Hammond et al., 2020; Dugan & Chenoweth, 2020; Lantz et al., 2023; Su et al., 2020), who can further dehumanize underrepresented populations and legitimize the use of violence against them as the dominant group becomes empowered (Fisher et al., 2019; Hodwitz & Massingale, 2023; Piazza, 2020). As hate crimes can be opportunistic (Leander et al., 2020; Tewksbury et al., 1999), such dehumanizing rhetoric essentially creates an environment with a "broadened latitude for hate" (Cunningham, 2018, p. 507).

While recent contributions have been vital to understanding COVID-19's influence, particularly on the Asian community, there remain important gaps in the literature. First, the impact of specific pandemic restrictions coupled with political rhetoric is unclear. Second, and especially concerning given the often disconnect between hate-related attitudes and behaviors (Khalil et al., 2022; Nivette et al., 2022), there is less known about how specific elements of the response to the COVID-19 pandemic affected the incidence of hate-motivated behaviors. Third, and despite important calls for a "multi-definition, "multi-method" methodology when examining hate crime (Ansari & He, 2015; Holder, 2022), there is a lack of research utilizing this type of approach.

This study seeks to fill these gaps in three main ways. First, we examine the effects of *specific* pandemic-related mitigation strategies and statements, allowing this study to more directly assess the influence of these events. Second, we evaluate hate-motivated behaviors using a *"multi-definition" approach* by examining three separate outcomes as measured at distinct units of analysis and as acquired from different sources of data. Third, we also provide a *"multi-method" plan* through utilizing three separate analytic strategies to examine whether findings are dependent upon analytic assumptions. The context of Asian victimization during the pandemic makes this phenomenon even more important to understand, particularly given the lack of research historically focused on this community (Johnson & Betsinger, 2009), coupled with the policy implications that could result from these data (Lantz et al., 2023). *Concordantly, this is the first known study that investigates the effects of both COVID-19 restrictions and rhetoric on hate crime in the U.S. using a triangulation approach.* This investigation finds that both COVID-19 restrictions and rhetoric were related to state-level increases in hate crimes against Asian persons, but not for all cases. Indeed, supplemental analyses indicate that individual cities saw subsequent reductions in overall hate crime and null impacts. Further, this study finds that quarterly analyses were unable to replicate findings that were observable using monthly data. This suite of findings highlights the continued need for a "multi-definition" and "multi-method" approach when analyzing impacts on hate crime.

Literature Review

Pandemics, Negative Emotions, Prejudicial Misperceptions, and Hate-Motivated Behaviors

Pandemics have the capacity to promote negative emotional states like that of anxiety and fear (Kwong et al., 2021; Stephan & Stephan, 2000), which can then lead to the creation of prejudicial attitudes. Indeed, the COVID-19 pandemic was related to a 3-fold escalation in anxiety, with these types of disorders increasing by 25.6% globally (Daly & Robinson, 2022). In turn, perceived risk of vulnerability can motivate negative reactions toward racial and foreign outgroups (Faulkner et al., 2004; Hartman et al., 2020; Kessler et al., 2010), which can then result in the activation of beliefs grounded in prejudicial stereotypes that guide risk-avoidance strategies (Bodenhausen, 1993; Brewer, 1999; Lantz et al., 2023). Consistent with the work of Brewer (1999) and Bodenhausen (1993), studies reported increased receptiveness to nationalistic, anti-immigrant, and prejudicial opinions in response to COVID-related anxiety and fear (Hartman et al., 2020; Tabri et al., 2020). These attitudes were inordinately directed towards Asian persons during the COVID-19 pandemic (Darling-Hammond et al., 2020; Huang et al., 2023; Lantz et al., 2023; Lu & Sheng, 2020), due to mischaracterizations regarding whom was most likely to be carriers of the disease (Lantz et al., 2023) and thus, framing members of this community as an "outgroup threat." Mask wearing became symbolic of how this outgroup was further defined through the framing of Asian persons as "diseased" and the "source of the pandemic" (Ren & Feagin, 2021, p. 749). Relatedly, shelter-in-place orders created radical changes to routine activities, further contributing to fear and anxiety that became weaponized against this outgroup (Lantz & Wenger, 2023; Ren & Feagin, 2021; Wenger & Lantz, 2022). Indeed, such orders exemplified disruption to daily routines through loss of employment, decreased social support, increased financial stress, and school closures (Piquero et al., 2020). As evidence, increases in calls to mental health and suicide prevention hotlines coincided with implementation of stay-at-home orders in the U.S. and abroad (Goodman, 2020; Jackson, 2020). In this way, these orders may have served "negative interventions" associated with increases in crime (Piquero et al., 2020, p. 607).

While the rise in prejudicial attitudes against Asian persons during the COVID-19 pandemic has been well-documented, less work has focused on how such attitudes may have graduated to hate-motivated behaviors. Theoretically, beliefs can translate into actions through a process of vicarious retribution, whereby members of an ingroup exhibit aggression towards members of an outgroup in response to a perceived threat for which the targeted outgroup members have no direct responsibility (Brewer, 1999; Lantz et al., 2023). However, research has demonstrated the tenuous link between extremist attitudes and violent behavior as the former are frequently stable or declining through adolescent years and not connected to the latter (Nivette et al., 2022). In addition, extremist attitudes and behavior are also often conflated, leading to incorrect conclusions regarding exit programming impacts (Khalil et al., 2022). Of the studies that have evaluated behavior, results suggest notable increases in anti-Asian victimization during the pandemic. More specifically, survey-based research has found that Asian respondents disproportionately experienced an incident

of bias or knew of someone who had (He et al., 2020; Lantz et al., 2023). Fewer studies have utilized official data sources (for exceptions, see Center for the Study of Hate & Extremism, 2021; Han et al., 2023), likely due to the numerous issues impacting these data (Mills et al., 2024; Pezzella et al., 2019). Nonetheless, this limited trajectory of research denotes similar increases. No known study has employed nationwide victimization data in the U.S. to evaluate this phenomenon.

While the importance of pandemic-related negative emotions in creating prejudicial attitudes cannot be understated, hate-motivated behaviors require opportunities to commit them (Tewksbury et al., 1999). More broadly, while threat alone may be sufficient to drive prejudicial attitudes, such behaviors are affected by the environment. As Lantz et al. (2023) argue, and in the context of COVID-19, President Trump and other major political officials may have established such an environment by identifying Asian persons as the source of the pandemic.

Rhetoric Surrounding a Pandemic: Emboldenment and Political Legitimization

In the lead up to the outbreak in the U.S., news media outlets consistently used terms such as the "Wuhan virus" and "China's coronavirus" to describe COVID-19 (Darling-Hammond et al., 2020; Su et al., 2020). This continued until February 11th, 2020, when the World Health Organization issued an explicit request to instead employ "coronavirus" and "COVID-19" to avoid stigmatization (World Health Organization, 2020). As noted by Darling-Hammond et al. (2020), most mainstream media outlets complied with this request and the use of stigmatizing terminology in news media reports decreased to negligible levels by March 6th, 2020. However, on March 7th, 2020, former Secretary of State Mike Pompeo used the term "Chinese virus" on both Fox News and CNBC. President Donald Trump also began employing similar language during this time, including "China virus" and "Chinese virus," on March 18th and May 25th, 2020 respectively. The former date represents the press conference when President Trump defended the term,[2] while the second date is the first time President Trump used the phrase "China virus" on Twitter (Reese, 2021).

Building upon Hewitt's (2003, p. 42) prediction that presidential rhetoric "sympathetic" to violent extremist beliefs is "likely to result in violence," there is now growing evidence connecting such rhetoric to behavior, particularly in relation to hate crimes and terrorism (Chyzh et al., 2019; Dugan & Chenoweth, 2020; Fisher et al., 2019; Hodwitz & Massingale, 2023; Muller & Schwarz, 2020a, 2020b). The language adopted by U.S. Presidents and other leaders frequently shapes popular understanding of events and potentially legitimizes violence against marginalized individuals (Piazza, 2020; Pilecki et al., 2014). From both the closely related emboldenment and political legitimization perspectives (Lantz et al., 2023), marginalized groups may experience violent backlash in response to their political gains. Members of the dominant entity

[2]"It's not racist at all, no, not at all. It comes from China, that's why. I want to be accurate. … I have great love for all of the people from our country, but as you know China tried to say at one point … that it was caused by American soldiers. That can't happen, it's not gonna happen, not as long as I'm president. It comes from China."

may also be emboldened to victimize marginalized persons through the words of leaders (Dugan & Chenoweth, 2020). This impact can be exacerbated in times of crisis and political polarization (Levin & Reitzel, 2018). For example, inflammatory rhetoric used by the Bush administration following 9/11 preceded increased hate crimes against Arab and Muslims, particularly in the first several weeks following the attacks (see Hanes & Machin, 2014; Swahn et al., 2003). And of particular interest to the current investigation, Han et al.' (2023) found short-term spikes of anti-Asian hate crime within four U.S. cities after the terminology "Kung fu" or "Chinese virus" were used publicly.

The aforementioned research demonstrates how factors related to emboldenment can drive an increase in hate crimes. However, and despite both this important work coupled with long-standing predictions (Hewitt, 2003), the extent to which nationwide hate crimes were fueled by this rhetoric remains an outstanding empirical question. Consequently, the next section details our efforts to explore the influence of these factors coupled with COVID-19 restrictions using a multi-definition and multi-method approach.

Current Study

The extant literature has indicated how (1) critical events can lead to increased negative emotions, which in turn can be exhibited through the heightening of hate-motivated behaviors targeting the outgroup, and (2) rhetoric from political leaders can legitimize the use of violence against this same outgroup as the dominant entity becomes emboldened. However, there remains a question as to how these effects impacted hate-motivated *behaviors* after *specific* COVID-19 pandemic-related mitigation efforts were implemented *coupled with* xenophobic statements by public figures. While previous studies have treated the beginning of the COVID-19 pandemic as an exogenous shock (see Gray & Hansen, 2021) or analyzed reflective data that was collected cross-sectionally (Kim et al., 2022), the present study instead examines specific elements of the response. Rather than viewing the COVID-19 pandemic as a monolithic entity, this approach is designed to give specific policy insights that can be used as an evidence base for tailoring future pandemic responses. In addition, there is no known study that has utilized triangulation to assess these potentially important effects in the context of the COVID-19 pandemic. Consequently, this study asks, *Have COVID-19 pandemic restrictions and rhetoric affected hate crime in the U.S.?*

Informed by the aforementioned emotion-threat framework, we anticipate that COVID-19 restrictions will be related to increases in hate crimes against Asian persons. The pandemic represents a critical event that led to heightened negative emotions directed toward towards the perceived "outgroup" based on misperceptions surrounding who was believed to be the most vulnerable to infection. Such prejudice was then manifested through attacks against Asian persons. As such, we posit:

> **H1a:** COVID-19 pandemic *restrictions* will be related to an increase in hate crimes against Asian persons.[3]

[3]It should be noted that heightened negative emotions are treated as a proposed unobserved mechanism in this study. This is in line with the approach proposed by Wikström and Kroneberg (2022, p. 183), where-

This study also examines the impact of COVID-19 policy restrictions in association with *total* hate crimes for the three main reasons. First, research on the anxiety-threat framework also suggests that negative emotions could lead to a diffusion effect of sorts, whereas the definition of the outgroup becomes more generalized or misidentified (Chakraborti, 2010; Huang et al., 2023). Thus, while we expect more robust impacts for whom was specifically othered during the pandemic, attacks again non-Asian persons may also be influenced by the overall affective state. Second and relatedly, the offender may have believed their target was Asian when this was not the case or when the incident was not officially classified as an anti-Asian hate crime (Huang et al., 2023). Finally, as two data sources for the dependent variable are police-generated data, only employing anti-Asian hate crimes also assumes that such data were coded correctly by the agency which is not always the case (Gladfelter et al., 2017). Consequently, H1b states that:

H1b: COVID-19 pandemic *restrictions* will be related to an increase in total hate crimes.

Informed by the research linking political rhetoric to behavior (Dugan & Chenoweth, 2020; Piazza, 2020), this study additionally hypothesizes that problematic language used by political leaders will be related to increases in hate crimes. As noted, this language can dehumanize groups while at the same time legitimizing violence against the outgroup through the emboldenment of the dominant entity. Again, we expect that Asian persons will be differentially affected by such racially stigmatizing language given its initial target. Thus, we posit:

H2a: COVID-19 pandemic *rhetoric* will be related to an increase in hate crimes against Asian persons.

As with H1, we analyze this construct on all hate crimes based on similar justifications (see Hewitt, 2003; Hodwitz & Massingale, 2023; Fisher et al., 2019;). The pandemic may have emboldened hate crime against all perceived outgroup members as other groups experienced similar dehumanization coupled with the legitimization of violence directed towards them (Chakraborti, 2010). Additionally, based on the issues discussed in H1b regarding the reliable identification of Asian victims in police data, the inclusion of a more wholistic outcome ensures all possible impacts are assessed. Thus, we hypothesize:

by "both observable entities and unobservable processes (mechanisms) [should] be included in scientific investigation." As Wikström and Kroneberg (2022, p. 138) further note, this is common within criminology as explanations for, "human action are commonly unobservable or only partially observable" and omitting these theoretically necessary considerations from theoretical discussion would fail to yield plausible explanatory mechanisms (see also Popper, 1956). Particularly due to the covert nature of hate crimes and other forms of political violence, this is often necessary, but goes undiscussed in existing empirical studies (Fisher & Dugan, 2019). Consequently, this study was written to avoid these common pitfalls in the existing literature by explicitly noting the unobservable theoretical mechanism connecting the operationalized independent and dependent variables in each analysis.

H2b: COVID-19 pandemic *rhetoric* will be related to an increase in total hate crimes.

All in all, we examine the effect of COVID-19 pandemic restrictions and rhetoric on hate crimes, both aggregated and disaggregated to those where an Asian person is specifically identified. The next section details our efforts to examine this important potential relationship.

Overview of Analysis Plan

We utilize three sources of data, units of analyses, and analytic methods to triangulate findings across a series of assumptions and collection methods. The first of these approaches employs a *fixed effects analysis of monthly, state-level Uniform Crime Report (UCR) data for the years 2019–2021*. The second strategy involves an *ARIMA (autoregressive moving average with exogenous inputs) analysis of quarterly data from the National Crime Victimization Survey (NCVS) for the years 2010–2022*. The third effort applies *series hazard analysis to data obtained from an open records request on 18 cities from January 1st, 2019, through 30 September 2020*. Appendix 1 provides a comparison of these analyses, with their data source, unit of analysis, time frame, advantages, and disadvantages specified (supplementary material). Triangulation across analytic procedures and measurement approaches serves as a promising way to assess key criminological hypotheses (Sullivan & McGloin, 2014) and to examine whether findings are consistent across statistical assumptions that are made over analytic approaches (Wikström & Kroneberg, 2022). Specifically, this study employs fixed effects modeling to control for time-stable differences across U.S. states that are unaccounted for in UCR data. Second, as this first method is unable to account for error dependency across adjacent temporal units, this research then uses ARIMA to analyze NCVS data. Finally, given that hate crimes may influence subsequent incidents, this study also employs series hazard models. Each of these approaches examines different elements of the potential relationship between pandemic restrictions and rhetoric on hate crime that would not be assessable using a single analytic strategy.

Data and Samples

Overview

As noted, the issues with official hate crime data are well-documented (Pezzella et al., 2019), particularly regarding underreporting amongst Asian victims (Lantz & Wenger, 2022). While agency participation has steadily increased since the Hate Crime Statistics Act was passed in 1990, several departments still report zero annual hate crimes to the UCR, with entire states declaring that they had no bias-motivated offenses within an entire year (Mills et al., 2024). Research has uncovered the influence of both department (Martin, 1996; McDevitt et al., 2001?) and structural-level factors (King, 2007; McVeigh et al., 2003; Scheuerman et al., 2020; Stacey, 2015), which ultimately lead to convergence issues (Holder, 2022), making it imperative that at least one of our sources was obtained from victimization data. Also as noted, studies suggest that additional filtering of hate crime data occurs between the reporting of the incident

and its formal accounting within the Uniform Crime Report (hereafter, "UCR"), thus reinforcing the need for multiple sources within police data (Gladfelter et al., 2017). Thus, we employ three main sources: the UCR, the National Crime Victimization Survey Hate Crime Data (hereafter, "NCVS"), and U.S. City-Level Hate Crime Data.

Ucr. Since 1990, the Federal Bureau of Investigation (FBI) has been collecting incidents from thousands of agencies on those incidents motivated by bias against a given survivor's race, religion, sexual orientation, ethnicity, disability, and gender or gender identify (Federal Bureau of Investigation, 2021). This supplementary effort, known as the Hate Crime Statistics Program, includes specific information on the type of bias that allows for a separate examination of anti-Asian incidents. These data were downloaded from the FBI's website, organized by state, and then aggregated to monthly counts of total hate crime incidents and anti-Asian hate crime incidents.

Ncvs. While not as detailed nor as long-standing as UCR hate crime data (hate crime-specific data has been collected since 2003), the NCVS addresses the main reporting issue intrinsic to official sources. Indeed, and because the NCVS is administered directly to a representative sample of U.S. households, it does not require a crime to be reported to the police. After a participant identifies themselves as a victim of a criminal incident, they are then asked if they have, *"any reason to suspect the incident just discussed was a hate crime or crime of prejudice or bigotry"* (NCVS Codebook, 2021). Unfortunately, the NCVS does not include specific bias motivations beyond identifying attacks as racially-motivated (as ascertained from the question, *"do you suspect the offender(s) targeted you because of your race?"* (NCVS Codebook, 2021). However, the respondent's race is documented, whereas an Asian victim can be assumed. Thus, we measure this outcome through two operationalizations: (1) number of racially-based hate crimes, and (2) number of racially-motivated hate crimes with an Asian respondent. From these data, we then created quarterly counts of total hate crime, racially-motivated hate crime, and racially-motivated hate crime with an Asian respondent. While this approach does limit direct generalizability due to the differences between self-identified and ascribed race (Laniyonu & Donahue, 2023), it also provides an important point of comparability across the analyses.

U.S. City-Level Hate Crime Data

For the city-level data, and with initial intentions to create as representative of a sample as possible, we first started with a sampling frame of the 33 incorporated areas in the U.S. with a population of 500,000 or above based on 2019 Census estimates (Annual Estimates of the Resident Population for Incorporated Places of 50,000 or More, n.d.). We chose this cut point so that we would incorporate cities in the top three states in terms of: (1) the largest concentration of Asian persons (United States Census Bureau, 2010) and, (2) the highest number of coronavirus cases (National Public Radio, 2021); namely California, New York, and Texas.

We submitted a series of open record requests to the primary agency representing these cities in October 2020. The request asked for the date, offense type, bias motivation, and victim race/ethnicity for all "hate crime incidents" from January 1st, 2019,

through 30 September 2020. We also asked for the aggregate monthly counts of arrests for this same period. When early responses indicated that departments were overwhelmed due to pandemic-related resource limitations, we expanded our sample to cities with populations of 400,000 or more and their associated agencies. Appendix 2 shows the full universe of city-level departments, whether they provided the full scope of data in our analysis timeframe and consequently, how we obtained our final sample of 18 cities (supplementary material). Cities within our three states of interest are bolded, demonstrating a majority of population coverage for those states.

COVID-19 U.S. State Policy Database

For measures related to COVID-19 restrictions, we used the COVID U.S. State Policy Database (CUSP). These data, collected by researchers at Boston University, include state-wide directives on "closures, shelter-in-place orders, housing protections, changes to Medicaid and SNAP, physical distancing closures, and reopening dates" (Raifman et al., 2021).

Reese Data

For the variables operationalizing the past dates of President Trump's tweets, we employed Austin Reese' web scraping database, which covers 2009-June 2020.

Analyses

Fixed Effects Negative Binomial Models

To examine the UCR monthly count data as organized by state, we employed negative binomial models. These types of models are ideal for count data and were more appropriate than their Poisson counterpart given the means and variances of our dependent variables were not equivalent (Fisher & Lee, 2019). The addition of fixed effects also allows for the researcher to investigate changes in the independent variables (here, pandemic rhetoric and restrictions) on changes in the dependent variables (here, hate crime) independent of time-stable unobserved heterogeneity across states (Allison, 2009; Wolfowicz et al., 2023). The unit of analysis for the fixed-effects regression models is the state-month, which is used to account for unmeasured time stable variation across U.S. states. While it is possible that the impacts are fixed upon in other ways, the method used here is consistent with the statistical approach in other notable studies examining terrorism and bias crimes (e.g. Fisher & Lee, 2019; Wolfowicz et al., 2023). Recent research from Mills et al. (2024) also raises two major data issues regarding the use of UCR hate crime data related to: (1) the impact of historical context, and 2) the presence of ceremonial compliance which results in, "the consistent and invariable reporting of zero hate crimes in a particular jurisdiction" (pg. 381). First, Mills et al. (2024) notes that the localized history of racial oppression can affect the nature of the approach to the reporting of hate crime data echoing earlier findings (see King, 2007). Fixed-effects models remove the impact of time-stable heterogeneity like those noted in Mills et al. (2024), such as historical lynchings and the former Confederate South, which are then differenced out in the following analyses employing UCR data. As such, while important for other estimation procedures, these

influential historical impacts are effectively controlled for in the following fixed-effects models. Secondly, it should be noted that this estimation procedure does not address the biases stemming from ceremonial compliance. As noted by Dugan and Chenoweth (2020, p. 723), the inclusion of data from "agencies erroneously report[ing] no or low incidents of hate crimes" results in biases toward zero in estimations, raising the likelihood of Type II errors in these models (especially for fixed-effects models). Consequently, the ability of these models to detect statistically significant influences is diminished and the resultant findings should be seen as conservative estimates of the likely actual impacts (Cronin et al., 2007). To further account for potential serial and cross-sectional dependence in the regressors and error terms across these models, along with potential heteroskedasticity, additional sensitivity analyses were also conducted using a range of bootstrapping procedures (see Gonçalves, 2011). In all cases, this process produced substantively identical findings. Results were also robust to the inclusion and exclusion of data from 2021.

Arima. For the quarterly NCVS data, we employed ARIMA. ARIMA can effectively assess the influence of an intervention after adjusting for the error dependency found between connected units of time (McDowall et al., 2019). More specifically, ARIMA can determine whether a particular series (here, counts of total, racially-motivated, and racially-motivated hate crimes with an Asian respondent) changes after an intervention (pandemic restrictions and rhetoric). Please see Appendix 3 for additional detail regarding our white noise models (supplementary material).

Series Hazard Modeling

For the city-level data, we employed series hazard modeling (hereafter "SHM"), a method previously used to evaluate macro-level changes related to aerial hijackings (Dugan et al., 2005), terrorism in Northern Ireland (LaFree et al., 2009), and eco-terrorism (Carson, 2014). This method is an extension of the Cox proportional method (see Dugan, 2011 for a full description), where the unit of analysis is the failure of the event (as compared to the subject as in traditional hazard analyses). In sum, SHM can be used to estimate the change in an incident's hazard rate as it relates to an intervention; in this context, the change in the hazard of a hate crime as it relates to the dates of important pandemic restrictions and rhetoric.

SHM can address many of the issues intrinsic to other, more commonly used methods like that of event studies (see Kothari & Warner, 2007 for a full review of this alternative method, with an inclusion of their specific shortcomings) or multi-level modeling (which would require a larger sample size). Instead, SHM allows for the consideration of how events influence one another by including such considerations as covariates; or in this case, how one hate crime can affect subsequent hate crimes. For example, a well-publicized incident has the potential to embolden other offenders. Thus, we predict the pandemic's effect on the hazard of another hate crime (and those solely perpetrated against Asian persons) after controlling for other factors that would influence this same hazard.

The series hazed model estimates the number of days until the next attempt (Y) as a function of an unspecified hazard baseline hazard function (k0) and other

independent variables (interventions, event context, and trends over time) measured at the time of the current attempt (Dugan et al., 2005). Equation (1) represents the model.

$$h(Y) = \lambda_0 \exp(\beta_1 Restrictions + \beta_2 Rhetoric + \beta_3 Context + \beta_4 Trends) \quad (1)$$

Dependent Variables

UCR Analysis

For the UCR Monthly Data, we examined two dependent measures: (1) counts of total monthly hate crime, and (2) counts of monthly hate crimes motivated by Anti-Asian bias (as ascertained from bias motivation). The descriptive statistics are shown in Appendix 4 (supplementary material).

NCVS Analysis

For the NCVS quarterly data, we assessed three measures: (1) counts of quarterly hate crime, (2) counts of quarterly racially-motivated hate crime (as determined from the previously identified racial bias question), and (3) counts of racially-motivated hate crime with an Asian respondent. Again, the descriptive statistics for these outcomes are shown in Appendix 4 (supplementary material).

City-Level Analysis

From the U.S. City-Level Hate Crime data, we created two measures: (1) days until the next hate crime and (2) days until the next hate crime against an Asian person. For crimes against Asian persons, we employed the "bias motivation" ("anti-Asian") and "victim race/ethnicity" ("Asian") fields reported by agencies to separate out these offenses. As noted, the measure of "time until" can be superior to the outcome of frequency given the relative rareness of hate crime, where several days in a city-level series will have no incidents.

Independent Variables

The COVID-19 pandemic has been marked through several national-level dates (e.g. when China first alerted the World Health Organization to an increasing number of cases in Wuhan on December 12th, 2019, the first reported COVID case in the U.S. on January 19th, 2020 (CDC, n.d.)). Due to their aforementioned significance in both symbolically defining the outgroup and increasing negative emotions, we choose to measure the pandemic through face mask mandates for all individuals in public spaces and stay-at-home/shelter-in-place orders. These dummy variables were operationalized through the date in which the order was enacted and if relevant, when it was removed (0 = before and 1 = after), as gathered from the CUSP data. For the fixed effects models, this date varied based on the state and was set to 1 during the first month for which the restrictions were implemented (and 0 if it was removed). For the ARIMA models, this date was the first quarter in which these events occurred (quarter 41 for

stay-at-home orders and 42 for mask mandates). For the series hazard model, such dates varied based on the state in which the city was located.

In addition, and as our secondary hypotheses were related to rhetoric, we selected three key dates in this timeline. First, we include the day that former Secretary Pompeo departed from WHO guidance by using the term "Chinese virus" on Fox News and CNBC (March 7th, 2020) as obtained from Darling-Hammond et al.'s (2020) "Time Line of Tone of COVID-19 Coverage" (pg. 871). Second, we utilized the aforementioned days representing former President Trump's defense at a press conference and own use of "China/ese" virus *via* Twitter from the Reese data: March 18th, 2020, and May 25th, 2020, respectively. Given the findings regarding increases in stigmatizing language and self-reported implicit bias against Asian person amongst the public after this date, this measure should be especially important to analyze. These variables were again operationalized through dummy indicators, where 1 = after the rhetoric has occurred. Since both the Pompeo interview and the Trump press conference occurred in the same month and quarter respectively, they are collapsed in both the UCR and NCVS analysis. Similarly, the restrictions overlapped with these rhetoric interventions in the NCVS analysis and thus are tested as a singular influence.

Control Variables[4]

For the city-level analyses, we were able to utilize a series of controls specific to our method that have been incorporated in similar investigations (Carson, 2014; Dugan et al., 2005; LaFree et al., 2009). First, we included a measure for the last event attempt (number of days since the last hate crime or hate crime against an Asian person) based on the premise that crimes that occur close together would likely influence subsequent incidents. In addition, we employ a monthly count of city-level crimes as primarily measured through arrests to control for any unrelated trends in the hate crime hazard. Based on previous investigations (Dugan & Yang, 2012; LaFree et al., 2009), we also tested for interactions with the rhetoric variables to see if the slope of these interventions differed from the overall trend. In other words, it may be problematic to assume a constant effect that lasts until the end of the series (Dugan & Yang, 2012). However, and as noted below, very few of these interactions overall yielded statistically significant findings.

Results

UCR Results

Table 1 displays results for the models examining UCR monthly hate crime data. Both COVID-19 restrictions (supporting H1a) and rhetoric (supporting H2a and H2b) yielded

[4]While we would have liked to include additional control variables (e.g., city-level demographics), the short temporal window examined by this study reduces statistical power and increases the likelihood of Type II errors. The inclusion of additional control variables would further increase the likelihood of Type II errors for multiple reasons.

Table 1. Negative binomial fixed effect results, UCR data.

	Model 1 β(SE) All Hate Crime	Model 2 β(SE) Anti-Asian Hate Crime
Mask Mandate	−0.19*** (.05)	−0.07 (.13)
Stay-at-Home Orders	.04 (.09)	.53** (.05)
Pompeo Interviews/Trump Press Conference	−0.14 (.12)	.49* (.24)
Trump Tweet	.46*** (.19)	−0.07 (.22)

***Significant at .001, **Significant at .01, *Significant at .05

Table 2. ARIMA results, NCVS data, all hate crime (square root).

	Type of Effect	Coefficient of Intervention	*SE*	*z*	*p*
Stay-at-Home Orders/ Pompeo Interviews/ Trump Press Conference	Gradual, Permanent	.14	.19	.75	.45
Mask Mandates/Trump Tweet	Gradual, Permanent	.20	.20	1.02	.31

statistically significant impacts on that of hate crime. Net of time-stable heterogeneity across states, state-level stay-at-home orders produced statistically significant estimates for anti-Asian hate crime. It should also be noted that introductions in state-level mask mandate policies were related to *decreases* in the count of monthly total hate crimes, in contrast to the expected direction of our hypotheses. In addition, and during the month that both former Secretary Pompeo and President Trump employed "China virus," there was a statistically significant increase in the count of hate crimes against Asian victims supportive of H2a. Also consistent with our predictions (H2b), Trump's tweet was associated with a statistically significant increase in total counts of monthly hate crimes. However, no detectable impact was observed on anti-Asian hate crime specifically.

NCVS Results

Tables 2–4 show the ARIMA results for all quarterly hate crimes, racially-biased hate crimes, and racially-biased hate crimes with an asian respondent. As demonstrated, none of the interventions yielded statistically significant impacts on these outcomes. In other words, and in terms of survivor-based data, pandemic restrictions and rhetoric did not produce statistically significantly increases in the quarterly counts of hate crimes and these findings are concordantly null of hypotheses 1 (both a and b) and 2 (both a and b).

City-Level Results

Table 5 displays the results for all hate crimes at the city-level, with hazard ratios and standard errors reported. Hazard ratios greater than 1 indicate an increased "risk," while those less than 1 are representative of a decrease. The city-level models yielded a range of statistically significant impacts, producing mixed support for H1a and H1b.

Table 3. ARIMA results, NCVS data, racially-biased (square root).

	Type of Effect	Coefficient of Intervention	*SE*	*z*	*p*
Stay-at-Home Orders/ NCVS Pompeo Interviews/Trump Press Conference	Gradual, Permanent	.07	.14	.49	.63
Mask Mandates/Trump Tweet	Gradual, Permanent	.12	.15	.81	.42

Table 4. ARIMA results, NCVS data, racially-biased with asian respondent (square root).

	Type of Effect	Coefficient of Intervention	*SE*	*z*	*p*
Stay-at-Home Orders/ Pompeo Interviews/ Trump Press Conference	Temporary	.11	1.75^{e}+07	.00	1.00
Mask Mandates/Trump Tweet	Temporary	.09	2.03^{e}+09	.00	1.00

Importantly, mask mandates were not observed to influence total hate crime in any of the locations that were examined. The stay-at-home orders also yielded mixed findings, indicating a statistically significant *decrease* in the hazard of hate crimes in Houston, New York, and Washington (in contrast to our hypothesized direction), while at the same time *increasing* this same hazard in Kansas City. However, it should be again noted that Missouri (affecting Kansas City) and Tennessee (affecting Memphis) never had a state-wide mask order and that impacts were not generated for Long Beach, Louisville, and Mesa due to collinearity.

Overall, the rhetoric variables are generally more supportive of both hypotheses as outlined above (H2a and H2b). Pompeo's interviews were related to an increase in the total hazard of hate crimes in Houston, coupled with a similar rise in Austin. President Trump's tweet was correlated with an increase in the total hate crime hazard in Houston, Kansas City, and Louisville. Nonetheless, and unsupportive of H2a and b, President Trump's press conference did not have a statistically significant impact on the hazard of subsequent hate crimes. The exception was New York City and Dallas, where its effect was *opposite* to the hypothesized direction.

Table 6 shows the results for hate crimes against Asian persons only. Of the four cities with enough hate crime involving Asian persons to support a statistical model (Boston, Los Angeles, New York, and San Francisco), none of the interventions of interest yielded statistically significant findings.

Discussion and Conclusions

This research is the first of its kind to employ a *triangulation approach* to gauge the effect of *specific* COVID restrictions and rhetoric simultaneously in relation to hate-motivated *behaviors*. Appendix 5 provides a summary of the results. Despite notable cases at the city-level where the number of recorded hate crimes were too few or the selected policies of interest were not implemented, this study demonstrates that the impacts of COVID-19 policy and political rhetoric was not monolithic and there was meaningful variation in their impacts on hate-motivated behaviors across

Table 5. Hazard ratios and standard errors for series hazard models by city, all hate crimes.

	Austin $n=36$	Boston $n=301$	Columbus $n=182$	Dallas $n=178$	Detroit $n=186$	Fort Worth $n=143$	Houston $n=95$	Indianapolis $n=123$	Kansas City $n=208$	Long Beach $n=30$	Los Angeles $n=720$	Louisville $n=48$	Memphis $n=11$	Mesa $n=9$	New York $n=608$	San Diego $n=47$	San Francisco $n=120$	Washington $n=294$
Mask Mandate	.85	1.42	.90	.99	1.76	.54	.81	.81	–	–	1.08	–	–	–	1.47	1.46	1.11	2.00
	(.80)	(.85)	(.25)	(.46)	(1.14)	(.19	(.62)	(.45)			(.35)				(.52)	(1.26)	(.55)	(1.67)
Stay-at-Home	.10	.41	1.31	.53	1.44	.46	.13***	1.44	9.57***	–	–	–	.11	–	.54**	–	–	.16*
Orders	(.13)	(.27)	(.84)	(.70)	(.69)	(.26)	(.05)	(1.07)	(5.43)				(.23)		(.14)			(.14)
Pompeo	23.54**	1.51	1.19	1.71	.77	–	3.99*	–	.29	–	1.53	–	15.52	–	1.06	–	.82	1.58
Interviews	(29.04)	(.60)	(.62)	(.89)	(.56)		(2.17)		(.21)		(.81)		(24.63)		(.29)		(.61)	(1.15)
Trump Press	.57	.38	.75	.05**	.54	1.16	.65	.55	1.78	.14	2.66	1.92	1.74	527,807.9[5]	.40*	1.58	.42	.69
Conference	(.50)	(.28)	(.58)	(.05)	(.51)	(.40)	(.42)	(.33)	(1.45)	(.14)	(3.27)	(1.66)	(3.42)	(.41)	(.18)	(1.15)	(.32)	(.56)
Press	–	–	–	–	–	–	–	–	–	–	.99	–	–	–	–	–	–	–
Conference Interaction											(.00)							
Trump Tweet	.04	13.44	1.58	5.11	2.60	1.88	6.57***	1.87	5.72***	3.97	1.65	21.04**	17.63	.10	.07	.84	2.54	.35
	(.09)	(20.45)	(.98)	(4.47)	(1.56)	(.77)	(3.80)	(1.36)	(2.41)	(4.88)	(.59)	(20.56)	(42.21)	(.25)	(.13)	(.76)	(1.49)	(.33)
Tweet	–	.99	–	–	–	–	–	–	–	–	–	–	–	–	1.00	–	–	
Interaction		(.01)													(.00)			
Last Attempt	1.04**	.98	.99	1.04	.99	1.07***	1.03**	.99	1.05**	1.02*	.97	1.01	1.01	1.06	1.12**	.99	.99	.95
	(.02)	(.02)	(.02)	(.02)	(.01)	(.02)	(.01)	(.01)	(.00)	(.01)	(.05)	(.01)	(.02)	(.03)	(.04)	(.01)	(.01)	(.03)
Monthly Crime	.97	.99	1.00	1.00**	1.00	.99	.99	1.00	1.00	1.00	1.00**	.99**	1.03	1.01	1.00	1.00	1.00	1.00*
	(.02)	(.01)	(.00)	(.00)	(.01)	(.00)	(.00)	(.01)	(.01)	(.00)	(.00)	(.00)	(.02)	(.00)	(.00)	(.00)	(.00)	(.00)

***Significant at .001, **Significant at .01, *Significant at .05.

[5]This inflated hazard ratio indicates this particular model is overfit.

Table 6. Hazard ratios and standard errors for series hazard models by city, asian survivors.

	Boston $n = 16$	Los Angeles[6] $n = 20$	New York $n = 23$	San Francisco $n = 12$
Press Conference	.3.37	–	.80	.03
	(5.55)		(.90)	(.06)
Tweet	.05	.63	.41	3.70
	(.09)	(.48)	(.56)	(7.32)
Pompeo	3.38	1.27	–	1.88
	(5.24)	(1.53)		(2.81)
Mask Mandate	.42	–	.42	–
	(.67)		(.27)	
Stay-at-Home Orders	.05	2.13	.78	–
	(.09)	(2.42)	(.87)	
Last Attempt	1.00	.99	1.03	.99
	(.01)	(.01)	(.04)	(.01)
Total Monthly Crime	.99	1.00	1.00	.99
	(.00)	(.00)	(.00)	(.00)

***Significant at .001, **Significant at .01, *Significant at .05.

the U.S. Our findings reveal that the impact of the COVID-19 pandemic on hate crime was not uniform over time, was not limited to a single exogenous shock at the beginning of the pandemic (Gray & Hansen, 2021), and had measurable impacts on hate crime well after the initial stages of the pandemic (Han, Riddell, & Piquero, 2023; Lantz & Wenger, 2023). This study further demonstrates that specific elements of the response to the pandemic, including political rhetoric, yielded measurable impacts on total hate crime and those specifically targeting Asian persons. As such, our findings add to the growing literature regarding the effects of COVID-related restrictions and political rhetoric on criminal outcomes.

This suite of results provides mixed support for restriction-based interventions, indicating that there were diverse and heterogenous impacts on *reported* hate crimes during the COVID-19 pandemic. These sometimes-divergent findings are consistent with the diverse response to the pandemic across the U.S. Counter to expectations stemming from the emotion-threat hypothesis, the implementation of state-level mask mandates was related to a statistically significant *decrease* in the count of monthly hate crimes as ascertained from nationwide official data. While follow-up research is required to further examine why stay-at-home orders led to *increased* reported hate crimes against the Asian community, while mask mandates were also related to *decreased* overall reported hate crimes, these findings collectively demonstrate that Asian persons were specifically targeted above other marginalized groups during the observation period. Further, as stay-at-home orders reduced opportunities for committing hate crimes in public spaces, this observed increase in hate crimes against Asian persons is particularly stark. Consistent with emotion-threat, the Asian community was differentially targeted following stay-at-home orders, likely based on mis(perceptions) regarding infection vulnerability. In comparison to mask mandates, shelter-in-place orders created much more extreme changes to routine activities that extended to loss of employment, social isolation, and school closure (Piquero et al., 2020). As noted by Lantz and Wenger (2023) and Wenger and Lantz (2022), such

[6]The interaction for Pompeo was significant but produced an inflated hazard ratio indicative of an overfit model (as with the Mesa model). Therefore, we report the intervention without the interaction here.

drastic disruptions to daily life would be expected to produce exponential increases in fear and anxiety that were weaponized against the perceived outgroup threat: here, Asian persons. However, the importance of geographic scope was also shown in the combined findings, with state-level, stay-at-home orders related to a *decrease* in the hazard of hate crimes in Houston, New York, and Washington and *increases* in the hazard of all hate crimes in Kansas City. Consequently, while it is vital to note that stay-at-home orders were related to nationwide counts of hate crimes involving Asian persons, this impact was experienced differently across the U.S.

The study provides more consistent support for our emboldenment hypotheses in terms of rhetoric-related measures, albeit still with small geographic variations and limited again to *reported* crime. The use of stigmatizing language by former Secretary Pompeo and President Trump in March of 2020 was related to increases in hate crime counts involving Asian victims and the hazard of total hate crimes within Houston and Austin (but, decreases in New York and Dallas). In addition, the term "Chinese virus" in a tweet by former President Trump some two months later led to increases in total hate crime counts and the hazard of total hate crimes in Houston, Kansas City, and Louisville. These findings are largely in line with extant literature that has consistently described the deleterious impact of stigmatizing and prejudiced language by political leaders on racial, ethnic, and religious minorities in other settings. While President Trump later clarified his remarks and condemned attacks against the Asian community (Gover et al., 2020), our findings display that a degree of damage was already done in line with previous studies suggesting that hate crimes can rise rapidly after a previous triggering event (King & Sutton, 2013). Our results move us to strongly encourage both political leaders and media outlets to be mindful of the potential unintended consequences of their messaging and closely adhere to the recommendations offered by the WHO (2020) regarding the use of stigmatizing language. Such efforts should also be coupled with public awareness campaigns designed to both disseminate facts and dispel misperceptions. To do otherwise may only embolden individuals of an extremist disposition and provide what is tantamount to tacit approval to target certain communities.

However, and perhaps even more critical to this trajectory of research going forward, our findings were contingent on how hate crimes were measured, the analytic procedures used, and the geographic scope of the analysis. Indeed, the lack of a universal finding throughout all three approaches highlights the contingent nature of these results and demonstrates the importance of triangulation to better situate existing findings within the hate crime literature. Most notably, those analyses with longer temporal units (quarterly) and focused specifically on cities were most likely to yield null findings. In contrast, state-month models were able to generate robust statistically significant results. This is both consistent and inconsistent with the Weisburd et al. (1993),[7] as more confined temporal units were better able to detect statistically significant impacts; however, this was also the case for wider geographic

[7]The Weisburd paradox as it is more commonly known, postulates increasing sample size can diminish statistical power by increasing the frame of reference to include more observations that are less likely to be impacted by the intervention (see also Nelson, Wooditch, and Dario, 2015).

units. Collectively, these patterns suggest that longer temporal units may miss important relationships for opportunistic and short-lived impacts on hate crimes (Hanes & Machin, 2014; Leander et al., 2020). In addition, our results document important heterogeneity in the experience of hate crimes across the U.S., whereby restrictions and rhetoric can yield null or negative impacts in some, but not all cities, while also increasing the overall likelihood of hate crime for specifically marginalized populations within a state. Beyond highlighting the importance of including hate crimes committed outside of large cities in future studies, these findings reveal that robust results at the state-level can miss policy-vital insights in more local jurisdictions.

It is also important to acknowledge the limitations we encountered across the three analytic approaches employed by this research, including those intrinsic to assessing a phenomenon as complicated as hate crime during the COVID-19 pandemic. As previously discussed, validity and reliability issues with hate crime data are well-documented, including the variety of department and structural factors affecting hate crime reporting coupled with those consistently reporting zero hate crimes as a form of ceremonial compliance (Mills et al., 2024). More specifically, recording practices may have improved and/or diminished during the COVID-19 pandemic within jurisdictions. In addition, the underreporting of crime by Asian victims (Allport, 1993), a phenomenon that extends to hate crimes amid COVID-19 (Gover et al., 2020; Lantz & Wenger, 2022) is of particular salience here. However, it is of special note that despite these issues, we still found statistically significant results at two distinct units of analysis despite the increased likelihood of Type II errors. Given the known biases in hate crime data, this suggests that the extent of this problem is even larger than established here. Relatedly, no investigation into this type of research question can eliminate all possible third variable explanations. While the fixed effects models can control for time-stable differences across geographic units, there were several events that occurred during March 2020 that may also be responsible for results in some of these models. Although these limitations stem from the organic and reactionary nature of the interventions examined, future studies should specifically seek to collect data and use analytic techniques that are able to distinguish the impacts of specific interventions. While this study was able to partially address this by triangulating findings across multiple methods and data sources, it is vital to note that while fixed effects regression is advantageous for accounting for time-stable heterogeneity across geographic units, this can come at the cost of being unable to distinguish between multiple interventions that occur within a singular temporal unit.

All in all, these findings highlight the importance of triangulating findings to produce policy-relevant research.[8] While this would be moot if all findings produced by this study were consistent, the variation in results across data sources, temporal units,

[8]Historically, triangulation has rarely been employed within criminal justice and criminological journals, even among those in the upper tiers (Anderson et al., 2011). However, it has become more common, particularly in qualitative and mixed-methods studies, especially considered to be beneficial when findings are consistent (Maruna, 2009). Nonetheless, triangulation and mixed methods research in general, "provide additional value because triangulation allows for space to examine both divergent and convergent findings across these different domains" (Wilkes et al. 2022, p. 530).

and geographic units were able to more accurately identify policy usable findings for those within specific cities, at the state-level, and for federal governments responding to pandemics. Concordantly, those examining hate crime-related research questions with the data we are currently constrained to using should employ multiple sources, units of analysis, and analytic methods to triangulate findings. Not only does this provide additional credence to the robustness of observed relationships when findings are consistent across these multiple empirical approaches, but this approach can reveal much about what would have otherwise been obscured by generating a single result based upon an average from a heterogeneous phenomenon.

Informed by this discussion, we also appeal for a meaningful change in the collection and reporting of data related to hate crimes. Timeliness of hate crime data reporting is an issue less frequently discussed. Updated hate crime data for 2020 were not released through the UCR Hate Crime Statistics Program until late 2021. In line with the predictions of Gover et al. (2020), it was not until 2022 that researchers could begin to fully discern the impact of COVID-19 on hate crimes targeting the Asian community—a full 2 years after the beginning of the U.S. outbreak and more than 1 year after mass distribution of vaccines. In other words, by the time data were available to social scientists, it was too late to use them for anything beyond academic purposes. Mirroring this concern, of the agencies to whom we submitted open records requests, several failed to process our requests within a 6-month window. Because of these limitations in data access, researchers have been hamstrung to such a degree that we cannot effectively diagnose or suggest remedy in terms of policy or practice.

We therefore call for the development and implementation of a centralized, real-time hate crime reporting database managed by the federal government (Johnson, 2021). Not unlike the COVID-19 data reporting systems managed by the Centers for Disease Control and Prevention, such an initiative would allow for rapid and continual analysis, as well as deployment of federal resources toward state and local jurisdictions who may be understandably overwhelmed—just as many have been in the wake of the pandemic and recent states of social unrest (Edmondson, 2021). Such efforts would also likely lead to the development of early intervention protocols that can be adapted to the circumstances of future critical incidents ranging from pandemics to terrorist attacks, each of which might result in the misperception of marginalized groups as existential threats (Hartman et al., 2020; Kessler et al., 2010).

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