THE "WAR ON COPS," RETALIATORY VIOLENCE,
AND THE MURDER OF GEORGE FLOYD

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The police murder of George Floyd sparked nationwide protests in the summer of 2020 and revived claims that public outcry over such high-profile police killings perpetuated a violent "war on cops." Using data collected by the Gun Violence Archive (GVA) on firearm assaults of U.S. police officers, we use Bayesian structural time series (BSTS) modeling to empirically assess if and how patterns of firearm assault on police officers in the United States were influenced by the police murder of George Floyd. Our analysis finds that the murder of George Floyd was associated with a 3-week spike in firearm assaults on police, after which the trend in firearms assaults dropped to levels only slightly above that which were predicted by pre-Floyd data. We discuss potential explanations for these findings and consider their relevance to the contemporary discussion of a "war on cops," violence, and officer safety.

Introduction

Since the popularization of the “war on cops” narrative after the 2014 police killing of Michael Brown in Ferguson, MO, (DeMarche, 2015; Martinelli, 2015; Swan, 2015), no research has found evidence that this killing led to a significant increase in violence against police (Maguire et al., 2017; Shjarback & Maguire, 2021; Sierra-Arévalo & Nix, 2020; White, 2020; White et al., 2019). But, while there continues to be no evidence of a sustained “war on cops” that began with the killing of Michael Brown, it is conceivable that other high-profile police killings might lead to a concentrated increase in the more general phenomenon of “retaliatory violence” (Bejan et al., 2018). The police murder of George Floyd in May 2020 and the widespread public outcry which followed raises precisely this possibility. At present, however, the question of whether Floyd’s murder prompted an increase in retaliatory violence against police remains empirically untested.

Answering this question is complicated by the ongoing COVID-19 pandemic during which Floyd’s murder and subsequent protests occurred. At the onset of the pandemic, many police departments instructed their officers to initiate fewer stops to reduce transmission of the COVID-19 virus (Lum et al., 2022). By temporarily decreasing the frequency of police-public interactions, this shift in police activity plausibly reduced officers’ exposure to the risk of being assaulted in the line of duty. Following the lifting of stay-at-home orders in the Spring of 2020, homicides surged in cities and towns across the U.S. (Asher, 2021), as did firearm violence linked to the COVID-19 pandemic (Arthur & Asher, 2021; Kim & Phillips, 2021; Lang & Lang, 2020). Given these intersecting phenomena and their plausible link to violence directed at police, any assessment of retaliatory violence against police following Floyd’s murder must address changes in violence linked to the onset of COVID-19.

In this paper, we empirically test whether the police murder of George Floyd led to an increase in retaliatory violence against police. We leverage open-source data from the Gun Violence Archive (GVA) on the most serious subset of violence against police: firearm assaults. We use these data to construct a Bayesian structural time series (BSTS) model that incorporates firearm assault data in the months prior to Floyd's murder (both before and after the onset of COVID-19) to isolate the effect of Floyd's murder on firearm assault of police. We find evidence that Floyd's murder was associated with an approximately 3-week spike in firearm assaults on police that dropped back to levels slightly above that predicted by pre-Floyd data. We discuss potential explanations for these findings and consider their relevance to the contemporary discussion of a "war on cops," officer safety, and violence.

Literature Review

The "War on Cops": Theory and Research

Building on a long line of research that investigates the incidence and determinants of violence against police (Bierie, 2017, 2017; Fridell & Pate, 1995; Kaminski & Stucky, 2009; Lester, 1984; Sierra-Arévalo & Nix, 2020; Tiesman et al., 2020), recent research has focused on claims that policing is in the midst of a violent "war on cops" that began after the 2014 police killing of Michael Brown in Ferguson, MO. According to prominent proponents of this theory within media, policing, and government (Barr, 2019; Lahren, 2020; Sowell, 2016), critical scrutiny of police by the public, politicians, and academics led to widespread and sustained violence against police officers (FBI, 2017; Mac Donald, 2016).

The dominant mechanism underlying the alleged "war on cops" hinges on damaged police legitimacy caused by high-profile incidents of police violence against racial minorities

(Nix & Pickett, 2017). Lower legitimacy predicts reduced likelihood of public cooperation and compliance with police (Peyton et al., 2019; Tyler, 2004), in turn increasing the likelihood of resistance and escalation of police-public interactions into physical violence (Sunshine & Tyler, 2003, p. 520; Terrill, 2003). This mechanism is especially salient to explicitly anti-police attacks or ambushes which are hypothesized to become more common as perceptions of police degrade and antagonism increases (White, 2020). As summarized by the Federal Bureau of Investigation (2017, p. 3):

> […] the Michael Brown shooting in Ferguson, MO, in 2014, and the social disturbances that followed, initiated a movement that some perceived made it socially acceptable to challenge and discredit the actions of police. […] this change in social mores allows assailants to become more emboldened to question, resist, and fight law enforcement […] assailants developed a distrust of law enforcement, and felt emboldened and justified in using violence against police.

Though there is no agreed upon definition of what level of violence would constitute a "war" on police, researchers have operationalized "war" as a significant increase in the frequency of violence against police following the killing of Michael Brown. No studies to date have found such an increase, regardless of the data source, the operational definition of violence employed, or the length of the "post-Ferguson" period being examined. Using data from the Officer Down Memorial Page, Maguire et al. (2017) found no significant change in felonious officer deaths after the police killing of Michael Brown. Drawing on the same data source but restricting their analysis to ambush killings—the sub-type of violence most closely aligned with explicitly anti-police killings—White (2020) found no significant increase since 2014. Drawing on Law Enforcement Officers Killed an Assaulted (LEOKA) data that captures non-fatal assaults on police, Shjarback and Maguire (2021) found no evidence of a significant increase in injurious or noninjurious assaults on police following the killing of Michael Brown.

Rhetoric, Retaliatory Violence, and George Floyd

Given the lack of research which supports the "war on cops" hypothesis, it is clear that continued claims of a "war on cops" are made without concern for empirical evidence that shows no significant increase in violence against police since the 2014 police killing of Michael Brown. Instead, they point to heinous attacks on police or short-term, year-to-date increases as proof of a sustained "war" on police. As a result, the "war on cops" can be invoked after any incident of violence against police, even though such claims fail to consider the duration or intensity of fluctuations in violence against police. Rather than operate as a falsifiable hypothesis, continued claims of a "war on cops" constitutes overtly political rhetoric that is unresponsive to empirical data.

Our critique of the imprecision and unempirical usage of the "war on cops" should not be equated to minimization of violence against police. Nor is it an effort to summarily dismiss officers' preoccupation with being violently victimized. Firearm assaults and violence more generally harms officers, their colleagues, and their families (Sierra-Arévalo, 2019b), and the real possibility of being shot in the line of duty continues to structure how officers' think and behave (Carlson, 2020; Nix et al., 2020; Sierra-Arévalo, 2019a, 2021). Violence against police is an important social problem that demands rigorous study. We make no claims to the contrary. Instead, we argue that continued argument over the "war on cops" hypothesis, regardless of how many years elapse since the purported origin of said "war," reifies the viability of a claim that has consistently been refuted by empirical research. It is our view that continued study of violence against police is best served by eschewing the premise and rhetoric of “war.”

To that end, it is useful to consider the more general process of “retaliatory violence” and the specific question of whether the police murder of George Floyd preceded an increase in violence against police. As described by Bejan et. al. (2018), “retaliatory violence” speaks to a more general process of reciprocal conflict. Similar to the aforementioned legitimacy-based theory of violence against police, Bejan et al. argue that widespread knowledge of police killings can cause “emotional contagion, spreading fear, anger and other negative effects” that, in turn, can “catalyze negative behaviors” like violence against police (2018, pp. 3–4). Importantly, “retaliatory violence” also encompasses police violence against the public in response to the on-duty murder of a police officer.[1] Their analysis found no evidence of lethal retaliatory violence against police following police killings between January 1, 2015 and September 30, 2016. In contrast, they found that killings of police were associated with a significant, same-day increase in racial minorities fatally shot by police over the same period.

On the one hand, this study supports prior research which finds that high-profile police killings have not preceded significant increases in violence against police. On the other, it shares the necessary limitation of prior research in that its analysis is constrained to a particular moment in time. This leaves open the possibility that other, more recent cases of high-profile police violence could precede an increase in retaliatory violence against police. The police murder of George Floyd is one such case.

[1]Legewie (2016)2/17/2023 3:08:00 PMfound that the murder of an NYPD officer by a Black suspect was followed by a period of increased stops and uses of force against Black suspects that lasted between 3 and 10 days. No such effects were detected after the shooting or murder of an officer by a Hispanic or White suspect.

In May 2020, Floyd's murder by Derek Chauvin of the Minneapolis Department was recorded via smartphone and uploaded to social media, where it quickly went viral. Within days, protests erupted across the U.S. and continued for weeks. Though past police killings have resulted in significant media coverage and public demonstrations, the scale of public outcry following Floyd's murder sets this case apart from prior police killings. The more than 11,000 protests across all 50 states since Floyd's death are estimated to be the largest social movement in U.S. history (ACLED, 2021; Buchanan et al., 2020). These protests were also accompanied by sharp declines in favorable public opinion of police (Reny & Newman, 2021). Given the magnitude of public outcry and shifts in public opinion, the sociopolitical environment following Floyd's murder would, presumably, be one in which retaliatory violence was especially likely to occur. At present, however, whether and to what degree this historic police killing affected patterns of violence against police remains unclear.

COVID-19 and Violence

Assessment of whether Floyd's murder prefaced an increase in retaliatory violence against police is complicated by its co-occurrence with the global COVID-19 pandemic. When social distancing orders and mask mandates took effect in March 2020, social life was sharply reordered at a mass scale in ways that plausibly affected patterns of violence against police.[2] First, the decline in social activity caused by COVID-19 affected the routine activities of "motivated offenders…suitable targets…and capable guardians" (Cohen & Felson, 1979, p. 589). The COVID-related reduction in criminal activity as measured by calls for service, by

[2] We use "social distancing" in place of the full range of terminology that describes policies or interventions intended to reduce individual mobility and COVID-19 transmission, including "stay-at-home orders", "lockdowns", and others.

extension, also depressed the frequency of police-public interaction (Ashby, 2020; Boman & Gallupe, 2020). Additionally, COVID-19 affected police operations. A survey of almost 1,000 law enforcement agencies found that 77 percent reported instructing officers to reduce arrests for minor offenses, and 62 percent reported they had enacted policies to limit the use of proactive traffic or pedestrian stops (Lum et al., 2022, pp. 11–12). In conjunction, the decrease in calls for service and a reduction in proactive police activity could decrease violence against police by reducing the overall number of police-public interactions in which violence could occur.

However, while there was an overall reduction in calls for service following the onset of COVID-19, other data suggest that some patterns in contact may have remained constant or even increased. Ashby's (2020) analysis of call-for-service data in 10 cities, for example, revealed that call reductions were most consistently related to specific call-types, like traffic-related calls (see also Nivette et al., 2021). By comparison, burglary calls increased and robbery calls remained static across 9 of the 10 cities, while assault calls were static in 7 of 10 cities. Most salient to the issue of violence against police, firearm assaults and homicides also spiked during 2020 (Rosenfeld et al., 2020). An increase in firearm violence could plausibly affect patterns of firearm assault on police who respond to this violence in the wider population and whose rate of firearm assault victimization is nearly 3 times larger than the public's (Sierra-Arévalo et al., 2022, p. 386).

Numerous mechanisms have been proposed as drivers of the observed spike in firearm violence that coincided with COVID-19. Most broadly, violence may have been tied to COVID-related "strain," or an increase in "physical, mental, emotional, and financial" pressure on already at-risk individuals and the institutions (e.g. police, courts, hospitals, social services) tasked with addressing violence (Rosenfeld et al., 2020, p. 20). In addition to increasing the incidence of violence to which police responded, widespread strain could have also changed the interpersonal conditions under which police and the public interacted. For example, pandemic-related job loss, damaged mental health, and increased alcohol consumption could have strained police-interactions such that they were, on average, more likely than pre-COVID interactions to escalate into violence (Ansell & Mullins, 2021; Grossman et al., 2020; T. Wu et al., 2021). Similarly, increased firearm sales during 2020 have been hypothesized as a contributor to the COVID-era spike in firearm violence. While one national-level analysis found no relationship between firearm purchases and firearm violence between April and July 2020 (Schleimer et al., 2021), it is still unclear if other firearm-related changes related to increased firearm violence, such as the expansion of right-to-carry legislation and firearm carrying, influenced firearm violence after the onset of COVID-19 (Donohue et al., 2019, 2022).

It remains an open question as to which (if any) of these mechanisms had a direct effect on violence against police in 2020. Nonetheless, these plausible changes could have altered the average call to which officers responded to following the onset of COVID-19, as well as the behavior of community members and officers who interacted with one another. This could, in turn, affect the probability that a single police-public interaction would result in an officer being assaulted with a firearm.

The Present Study

Any analysis that hopes to assess whether the murder of George Floyd led to an increase in retaliatory violence against police must consider COVID-19's complex and wide-ranging effects on crime, violence, and policing. To do so, we draw on Gun Violence Archive (GVA) that captures fatal and non-fatal firearms assaults on police officers across the United States.

Using these data, we undertake a Bayesian structural time series (BSTS) analysis which allows us to construct a synthetic counterfactual based on pre-Floyd patterns in firearm assault on police. Crucially, this Bayesian counterfactual incorporates historical variation in firearm assault on police, including that which coincides with social distancing that began prior to Floyd's murder. By comparing this counterfactual to the observed patterns of firearm assault after Floyd's murder, we can assess the following question: did patterns of firearm assault on police after the murder of George Floyd differ significantly from patterns in firearm assault that we would expect based on data from before Floyd's murder, including when COVID-19 social distancing measures were in effect? We turn now to a description of our data, methodology, and their respective utility for assessing this important question.

Data and Method

Data Sources

Firearm assault data was drawn from GVA, a non-partisan, non-profit organization that seeks to "collect and check for accuracy, comprehensive information about gun-related violence in the U.S." (GVA, 2020a). GVA gathers information based on a wide-reaching definition of firearm violence intended to include all incidents in which someone was fatally or non-fatally shot with a firearm, including "OIS [officer-involved shootings], accidental, children shooting themselves, murders, armed robberies, familicide, mass shootings, DGU, Home Invasions, drivebys and everything else" (GVA, 2020b).

In addition to firearm violence perpetrated by police officers captured in other open-source datasets (Burghart, 2017; Sinyangwe et al., 2020; Swaine et al., 2016), GVA is unique in that it also captures both fatal and non-fatal firearm assaults on police. To gather these data, GVA staff use a mixture of automated and manual techniques to check more than 7,500 sources that include local and state law enforcement agencies, social media (e.g. Twitter and Facebook), local and national news agencies, and governmental sources. Each case includes the date an incident occurred, geocoded location, whether a victim was injured or killed, and URL hyperlinks to the sources from which incident information was drawn.

GVA data provides for notable improvements on prior research. First, the bulk of prior research on violence against police examined patterns in violence against police via multi-week, monthly, or yearly time series (Maguire et al., 2017; Shjarback & Maguire, 2021; Sierra-Arévalo & Nix, 2020; White, 2020; cf. Bejan et al., 2018). Our analysis makes use of GVA's daily-level data, enabling identification of potentially shorter temporal fluctuations in firearm violence directed at officers that would not be detectable with coarser time-series.

Second, because the GVA data that we use are updated daily and are drawn from across the entire United States, we mitigate the significant time delays and reporting biases of datasets used in prior research on violence against police. Namely, the FBI's LEOKA data is released with delays of usually 16 to 18 months (Kuhns et al., 2016, p. 6; Shjarback & Maguire, 2021, p. 9). National Incident-Based Reporting System (NIBRS) data used in prior analyses (e.g. Bierie et al., 2016) relied on data drawn from only 37 percent of agencies nationwide. Despite improvements in NIBRS uptake, only 53 percent of agencies used NIBRS in 2021 (Congressional Research Service, 2022).

Third, while other data such as that provided by the Officer Down Memorial Page also cover the entire United States and are released in close-to-real-time, these data capture only lethal violence against police. In the case of firearm violence, analysis of only fatal firearm assaults omits, on average, 83 percent of total firearm assaults on police (Sierra-Arévalo et al., 2022, p. 1051). GVA, by comparison, captures both fatal and non-fatal firearm assaults on police, providing a more accurate estimate of firearm violence directed at police.

Case Selection

The data used in this analysis were provided directly by GVA for all cases of an officer injured or killed by gunfire between January 1, 2014 and December 31, 2020. Following the case selection and coding strategy of GVA data used by Sierra-Arévalo and Nix (2020), we restrict our sample in several ways.

Our analytic sample includes active, sworn law enforcement officers employed by a local or state agency that responds to calls for service. This includes officers employed by special jurisdictions and agencies such as transit police, school/university police, tribal police, and wildlife or park police. We also include officers who are part-time or reserve officers that, while on-duty, perform the same function as a full-time local or state officer. Though we include local and state officers who are shot while cooperating with federal agencies in specialized task force operations, we exclude federal law enforcement officers employed by agencies such as the U.S. Marshals, FBI, DEA, ATF, and others, as they do not engage in routine patrol or respond to community calls for service.

We further restrict our analytic sample to local and state officers who a) were on-duty at the time of being shot, b) had their person or on-person equipment (e.g., radio, ballistic vest, ballistic shield, or duty belt) struck by a bullet or bullet fragment, c) shot from a firearm, d) by someone who is not an on-duty police officer. We exclude injuries sustained by explosives or pellet guns, self-inflicted injuries and deaths (e.g. accidental discharges and suicides), and "blue on blue" incidents in which one on-duty officer accidentally shot another. Because of inconsistent reporting, our analytic sample also excludes cases in which a suspect pointed a firearm at officers without firing, as well as cases in which shots were fired but no officer was hit.

Firearm Assault on Police, 2014-2020

Table 1 provides descriptive statistics on firearm assaults of police from 2014 to 2020, including yearly total firearm assaults, yearly fatal firearm assaults, and yearly non-fatal firearm assaults. Similar to prior research which finds that non-fatal firearm assaults on police account for the vast majority of firearm assaults in a given year (Sierra-Arévalo et al., 2022; Sierra-Arévalo & Nix, 2020), we find that non-fatal firearm assaults far outpace fatal firearm assaults between 2014 and 2020. On average, non-fatal firearm assaults account for 83.9 percent of total firearm assaults on U.S. police. The proportion of non-fatal to fatal firearm assaults varied over the observation period from a low of 79.5 percent non-fatal to a high of 88.1 percent non-fatal in 2020.

[**TABLE 1 HERE]**

To further contextualize longitudinal trends in firearm assault on U.S. police, Table 1 also provides yearly estimates for the rate of firearm assaults using three distinct denominators. The first calculates a rate per 10,000 officers using yearly estimates of the population of sworn local and state police across the entire United States. The second calculates a rate per 100,000 arrests using Uniform Crime Report (UCR) arrest data. We follow Kaplan's (2022) suggestion to restrict our constructed estimates of yearly arrests to those agencies that submitted complete data to the UCR every year from 2014 to 2020.[3] The third denominator calculates a similar rate but does so using only arrests for violent offenses (murder and nonnegligent manslaughter, robbery, and aggravated assault) and weapons-related offenses (e.g., possession, carrying, concealing). Each of these denominators and the data used to construct them come with important assumptions and limitations.

Beginning with our estimates of firearm assault rates based on officer population, we construct this denominator using the FBI's Police Employee (PE) Data for 2014-2020. These data include local and state law enforcement officers "who ordinarily carry a firearm and a badge, have full arrest powers, and are paid from governmental funds set aside specifically for sworn law enforcement representatives" (FBI, 2019, para. 1). A strength of this denominator is that a population-based rate is suited to estimate the *average* risk of firearm assault among sworn state and local police officers without making any assumptions about the type of interaction that preceded a firearm assault. That is, whether a firearm assault occurred during an ambush, a traffic stop, a trespassing call, or any other circumstance, its inclusion accords with our interest in all firearm assaults against any sworn local or state law enforcement officer. By contrast, the lack of officer assignment information in the PE dataset requires that all officers be treated as interchangeable, obscuring variation in the relative risk of firearm assault faced by officers with different assignments (e.g., administrative, patrol, SWAT). Nor does a population-based denominator capture changes in the frequency of police-public interactions during which firearm assaults occur.

[3] This methodological decision resulted in dropping 5-10% of reported arrests each year. However, it was necessary because including agencies with partial data would introduce uncertainty as to whether year-to-year changes in arrests reflected shifts in officer behavior or non-random variation in agency reporting across years (see Kaplan, 2022, Chapter 5.1). To check for bias in our estimates driven by this inclusion restriction, we compared longitudinal patterns in all arrests and violent/firearms-related arrests for 12-month-reporting agencies against all reporting agencies (see Appendix B). Our estimates of all arrests and violent/weapons-related arrests based on 12-month-reporting agencies tracks closely with estimates based on all reporting agencies.

The second denominator used to estimate yearly rates of firearm assault on police is constructed with UCR arrest data. An arrest-based denominator provides for some sense of the rate of firearm assault relative to police-public interactions. Unlike a population-based denominator, an arrest-based denominator can capture change in officer behavior that is a proxy for interactional exposure to the possibility of assault (i.e., a decrease in arrests could be interpreted as a decrease in total exposure). This benefit of an arrest-based denominator is especially notable in the context of early 2020, during which many departments reported decreased levels of interaction with the public (Lum et al., 2022). However, an arrest-based denominator requires the conceptual assumption that firearm assaults occur predominately during arrests, which is out of step with LEOKA data that show only 12 percent of firearm assaults between 2014 and 2019 occurred during arrests (FBI, 2020).[4] A denominator constructed from the universe of total police-public interactions (most of which do not result in arrest) would sidestep this conceptual disjuncture. Unfortunately, “police-public contact” data are only released every 3 to 4 years (Bureau of Justice Statistics, 2020). Recognizing that arrest data are the best available for estimating police-public contact over time, it is prudent to keep in mind that an arrest-based denominator inevitably overestimates the rate of firearm assault per police-public interaction because it relies on a subset of total interactions.

Our third denominator, composed of only arrests for violent crimes and weapons-related offenses, still suffers from the conceptual limitation of using arrests to calculate a rate for firearm assaults which largely occur outside arrest situations. That said, it improves on a denominator that uses all arrests by focusing on offenses which, because they involve violence and/or

[4] LEOKA tabulations do not allow for reliable differentiation between assaults which occurred *while* an officer was making an arrest, assaults which occurred prior to a *subsequent* arrest, and assaults which did not precede an arrest. This further underscores the methodological limitations of an arrest-based denominator for calculating the rate of firearm assault on police.

weapons, are a closer approximation to interactions in which a firearm assault of police is most likely. Additionally, while departments reported declines in overall arrests and contacts with the public, it is plausible that these reductions were driven by a decrease in the most discretionary police interactions. Interactions stemming from police response to violence and weapons, by comparison, are likely to be more stable than low-level offenses which departments chose to curtail in efforts to curtail the spread of COVID-19.

With these assumptions and limitations in mind, Figure 1 plots the rates of firearm assaults per 10,000 officers, per 100,000 arrests, and per 10,000 violence or weapons-related arrests, respectively, from 2014 to 2020. The trend over this period varies from a low of 2.91 gun assaults per 10,000 officers in 2014 to a high of 4.34 per 10,000 officers in 2016 and 2020.[5] Across 2015, 2017, 2018, and 2019, the rate of firearm assault per 10,000 officers remains essentially the same. Turning to arrest-based denominators, a different pattern emerges. Owing to a 25 percent reduction in arrests as compared to 2019, 2020 saw 5.82 firearm assaults per 100,000 arrests.[6] This represents a 56 percent year-over-year increase from the 3.74 firearm assaults per 100,000 arrests observed in 2019. Finally, if we restrict the denominator to arrests for violence and weapons-related offenses, we observe a less pronounced spike in the 2020 firearm assault rate. That year, there were approximately 7 firearm assaults per 10,000 serious arrests, a 22% increase from the 5.74 gun assaults per 10,000 serious arrests in 2019.

Regardless of the denominator used, longitudinal changes in the rate of firearm assault on police must be interpreted with consideration of the relative rarity of firearm assault on police

[5] We echo the caution raised by Sierra-Arévalo and Nix (2020, p. 1052) with regard to the estimate of firearm assaults of police in 2014. GVA data collection began in 2014, and it is a reasonable possibility that reporting and detection of firearm violence against police increased over time along with media attention to violence by and against police.
[6] The 2020 decrease in arrests is even more pronounced when compared to the average number of arrests across multiple years. Arrests in 2020 were more than 37 percent lower than the average number of arrests from 2014 to 2019.

and the limited number of years in the time series. Because firearm assault on police is a rare phenomenon, small changes in the number of assaults will have an outsized effect on the rate of firearm assaults when aggregating at the yearly level. The issue of temporal aggregation is all the more salient given the small number of years in the time series, exacerbating what is already “a small-sample statistical problem” (Müller & Watson, 2019, p. 3). A lack of granularity in time-series data not only risks biased estimates of longitudinal trends (Müller & Watson, 2008), but obscures the relationship between smaller time units and any outcome of interest.

[**FIGURE 1 HERE]**

Thankfully, GVA data are recorded at the daily level, allowing for a detailed visualization of daily patterns in firearm assault on police from 2014 to 2020. These trends and a 7-day rolling average trend line for each year are plotted in Figure 2.[7]

[FIGURE 2 HERE]

Daily patterns of firearm assault on police highlight the statistical rarity of such violence and its longitudinal stability throughout this period. Over the observation period, there was an average of 0.7 firearm assaults on police per day, with a maximum 7-day average of 3.1 and a minimum of 0.0. Our study period spans 2,554 days, and there were zero firearm assaults on officers on 58% (n=1500) of those days.

[7] See Appendix A for separate visualizations of daily patterns in fatal and non-fatal firearm assaults from 2014 to 2020.

Nonetheless, Figure 2 does show short-term increases in the 7-day average of daily firearm assaults. Two of these increases concentrate around particular days, denoted with vertical lines in Figure 2, which saw an abnormally high number of firearm assaults on police. The first of these is July 7, 2016, which corresponds to the ambush on police at a Black Lives Matter rally in Dallas, TX. Of the 14 officers injured or killed by firearms on that day, 13 correspond to the Dallas shooting; the remaining incident was a non-fatal firearm assault which occurred in Bristol, TN.

The second corresponds to June 1, 2020, the Monday after protests of police brutality spurred by George Floyd's murder erupted across the United States. In contrast to the single mass shooting event which accounted for 92 percent of the firearm assaults on July 7, 2016, the 9 officers shot on June 1, 2020, were spread across 5 incidents in disparate locations: Las Vegas, NV, St. Louis, MO, Richmond, VA, Los Angeles, CA, and Davenport, IA. Of those, the firearm assault of four officers in St. Louis—3 non-fatal and 1 fatal—accounted for less than half of the total shootings on that day. The remainder of our analysis focuses on this period and examines whether and to what degree there was significant change in the daily count of firearm assaults on police following the murder of George Floyd.

BSTS Modeling Strategy

To assess whether the police murder of George Floyd was associated with a significant change in the frequency of firearm assaults on police, we perform a Bayesian structural time series (BSTS) analysis. This method allows for an approximation of causal effects using time series data by estimating a treatment effect through comparison of observed outcomes with a synthetic counterfactual that approximates a trend that would have likely occurred in the absence of any intervention (Brodersen et al., 2015).

Prior research has generally approximated such a counterfactual in one of two ways. In the first, a counterfactual is constructed using time series data on an outcome before and after the intervention, in combination with “synthetic control” series that are predictive of that outcome of interest before the introduction of treatment. Often, this synthetic control is drawn from “untreated” units, such as a geographic unit which did not experience a particular legal or policy change (e.g. Bartos & Kubrin, 2018; Crifasi et al., 2015; Donohue et al., 2019; Ratcliffe et al., 2017; G. Wu et al., 2021). The observed outcome can then be compared to the counterfactual constructed with this synthetic control. This method assumes the relationship between the treatment and outcome that existed prior to the intervention would continue if the treatment had no effect and the control series variables did not receive the intervention (Brodersen et al., 2015, pp. 248–249).

The second method for constructing a counterfactual is especially useful when there is no clear comparison unit from which to draw a synthetic control series. In such cases, generation of a synthetic counterfactual can be constructed using the pre-intervention relationship between an outcome of interest and a control variable that is unaffected by the intervention of interest. The observed trend in the dependent variable can then be compared to the constructed counterfactual to assess the effect of a given intervention. Such an approach was recently used to assess the effect of a mandatory sexual assault kit (SAK) testing policy on arrests for rape. To do so, Mourtgos et al.’s (2021) counterfactual was constructed using a synthetic control of arrests for violent crimes that would not be affected by a new SAK testing policy (e.g. aggravated assault, domestic violence aggravated assault). Using the same technique, Mourtgos and Adams (2021) leveraged local and state vaccination rates as synthetic controls to estimate the causal effect of a COVID-19 vaccine mandate in the Salt Lake City Police Department on vaccination rates among officers.

Unsurprisingly, there are sometimes barriers to clear identification of synthetic controls for construction of a counterfactual. In the case of the present study, the U.S.'s exceptionally high levels of firearm availability and firearm violence against police make selection of a control country decidedly problematic (Nagin, 2020; Zimring, 2017). Because of the U.S.'s unique firearm and violence landscape, there is no country which can be reasonably treated as comparable to the United States in terms of firearm assault in order to test for the unique event of Floyd's murder by police. BSTS models are helpful in this scenario because they can use historical time series data on an outcome of interest to forecast a synthetic counterfactual of that outcome's post-intervention trend while transparently integrating the uncertainty of such forecasting (Richards et al., 2021).

Compared to ARIMA models which can also be used to forecast post-intervention trends from observed data, BSTS has clear benefits. First, ARIMA models may be inappropriate for low-density count data on serious crimes like firearm assaults on police (Greenberg & Roush, 2009, pp. 12–13; Ratcliffe et al., 2017, p. 448). And whereas ARIMA and other frequentist methods assume that there is a single and fixed population parameter within a given population, a Bayesian framework conceptualizes every parameter as a probability that said parameter is contained within a distribution of possible values (Mourtgos, Adams, & Mastracci, 2021, p. 5; Mourtgos & Adams, 2021, p. 3n3). This framework allows outcomes to be discussed in terms of probabilities that explicitly acknowledge and incorporate uncertainty into the modeling strategy.

We employ BSTS modeling in this way to produce probabilistic estimations of what firearm assault on police in the U.S. would have been in 2020 based on patterns in firearm assault that precede the police murder of George Floyd. We then compare these synthetic counterfactual trends to the observed trend in firearm assault on police following this event. Crucially, because BSTS models produce predictions based on prior observations (i.e., the observed trend's current state, t=k, informs estimates of trends next state, t = k + 1), the resulting counterfactual incorporates unobserved data generation processes reflected in the observed, historical data. In our case, this includes not only stochastic variation in firearm assaults on police but also the potential effect of changes in firearm sales, police-public contact, or other unobserved factors that might affect firearm assault on police.

Assuming that these variables exerted a causal effect on the frequency of firearm assault of police prior to Floyd's murder on May 24, 2020, this effect is captured in the BSTS estimation of the unobserved "latent state" in patterns of firearm assault prior to that date. Barring a sudden and persistent change in the relationship between unobserved variables and firearm assault on police, we can be confident that the estimated counterfactual accounts for possible unobserved data generation processes that occurred at the onset of COVID-19. Of course, there is inherent uncertainty in accounting for every possible unobserved covariate that may influence the amount of firearm assaults on police. Unlike other forecast models which produce point estimates, the posterior probability distributions produced by a BSTS approach transparently present the uncertainty of our models. Thus, the means of the posterior probability distributions which we present can be contextualized with information about the entire probability distribution of the posterior inference such that readers can assess for themselves their level confidence in our results.

BSTS models are best described as observation equations that link observed data with an unobserved latent state. A transition equation describes the latent state's development over time. The observation equation is defined as:

$$y_t = Z_t^T \alpha_t + \varepsilon_t$$

where y_t is a scalar observation, Z_t is an output vector, and α_t is the unobserved latent state. The transition equation is defined as

$$\alpha_{t+1} = T_t \alpha_t + R_t \eta_t$$

where α_t is the unobserved latent state, T_t is the transition matrix, and R_t is the control matrix. $R_t \eta_t$ accounts for seasonality in firearm assault on police. The error terms (ε_t and η_t) are Gaussian and independent (Brodersen et al., 2015).

We estimate our BSTS model using daily counts of firearm assault data from January 1, 2014, through May 24, 2020 (the day before George Floyd's murder) – a time series 2,336 data points in length. We use daily counts rather than weekly or monthly aggregations to produce maximally precise estimates of our synthetic counterfactual and the effect of Floyd's murder on firearm assault of police.[8] The synthetic counterfactual model was estimated with data from January 1, 2014, through May 24, 2020, which incorporates random variation in the frequency of firearm assault on police, as well as variation associated with the COVID-19 pandemic and other

[8] Daily counts of firearm assaults were used for several reasons. First, more granular data enhances the precision of an estimated counterfactual. In Appendix C, we show that that whether assessed by one-step-ahead prediction errors or Widely Applicable Information Criteria (WAIC), the daily count model is a better fit than models using weekly or monthly firearm assault counts. Second, because we do not observe officer or arrest counts at the daily level, we are unable to calculate a reliable daily rate of firearm assaults on police. Third, even if such rate calculations were possible, the exceedingly small number of daily firearm assaults on police would generate commensurately small daily rate estimates which are less reliable and more difficult to interpret.

unobserved covariates prior to Floyd's murder.[9] We then estimated the synthetic counterfactual from May 25, 2020, through December 31, 2020. This is the counterfactual trend against which observed patterns of firearm assault on police was compared.[10] The post-Floyd difference between this synthetic counterfactual trend and the observed trend represents the effect of the police murder of George Floyd on the frequency of firearm assaults of police.[11]

Results

Bayesian Structural Time Series (BSTS) Analysis

To construct our BSTS model, we first estimated a 221-day forecast from the day of Floyd's murder, May 25, 2020, through December 31, 2020. Figure 3 plots the observed data alongside the synthetic counterfactual with an intervention date of May 24, 2020. Panel A shows the entire time series and marks the date of Floyd's murder. Zooming into the pre-intervention period (Panel B), the synthetic counterfactual tracks the observed data extremely well. Panel C shows the post-intervention period and an approximately 3-week spike (May 25, 2020, through June 13, 2020, or 20 days) in the observed data that aligns with the timing of Floyd's murder.

We aggregate daily-level estimates from our BSTS model to the weekly level for ease of interpretation. Prior to the spike, the weekly mean frequency of firearm assaults on police was

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[9] As a robustness check, we compared the predictive ability of our BSTS model with models that included all arrests, violence/weapons-related arrests, firearm sales, homicides, or all these predictors simultaneously. The addition of any or all these predictors did produce models with better predictive ability than our chosen BSTS specification. See Appendix E for more detail and a comparison of the cumulative one step prediction error across all tested models.

[10] Ten thousand Markov Chain Monte Carlo (MCMC) iterations were simulated to fit the model. The correlation of residuals was not significantly different from zero, indicating no autocorrelation; the posterior distribution of residuals was normally distributed.

[11] Crucially, we do not estimate the effect of Floyd's murder in a counterfactual world in which COVID-19 never occurred. Instead, we compare post-Floyd patterns in firearm assault on police to a counterfactual which incorporates potential pre-Floyd COVID effects on firearm assault of police. Our estimated effect, then, captures the effect of Floyd's murder during COVID-19 relative to a counterfactual in which there was COVID-19 but no murder of George Floyd. See Appendix D for description of a supplementary BSTS model that examines firearm assault of police relative to the onset of COVID-19, specifically.

4.7. The synthetic counterfactual trend from May 25, 2020, through June 13, 2020, estimated a weekly mean of 4.33 firearm assaults, or 12.38 firearm assaults during that same period. The observed trend over this period shows a weekly mean of 13.29—a value 3 times as large as the expected weekly mean—for a total of 38 officers shot in the line of duty during the post-Floyd spike. The difference of 25.62 firearm assaults between the observed and counterfactual values represents the increase in firearm assaults during the 3-week spike relative to what would be expected given pre-Floyd trends in firearm assault. This spike quickly dissipates and the observed weekly average frequency of firearm assault on police falls to 5.85. This observed average is slightly above the synthetic counterfactual's predicted post-Floyd weekly average of 5.01.

[FIGURE 3 HERE]

To quantify the aggregate effect of Floyd's murder on the frequency of firearm assault of police for the entire post-Floyd period, we evaluated the difference in means between the observed data and synthetic counterfactual at the weekly level for the entire post-intervention period using Bayesian estimation. Here, too, we aggregate daily-level data to the weekly level for ease of interpretation and to allow for the approximation of normal distributions necessary for accurate mean comparisons between the two parameters (i.e., observed and counterfactual).[12] If we denote the set of observed data as *D*, consisting of all observed and synthetic counterfactual values, a descriptive model has the following form, where μ is the mean of the observed and synthetic counterfactual values, respectively; σ is the standard deviation of the observed and synthetic

[12] Posterior predictive checks were plotted for both parameters following MCMC estimation. If a model is a good fit, synthesized data from that model should be similar to the observed data. Fifty simulated datasets were drawn from the posterior model parameters and plotted against the observed data. The simulated data for the weekly model strongly predict the observed data and do so substantially better than a daily model.

counterfactual values, respectively; and ν (nu) is a normality parameter shared by the two measures.[13]

$$\frac{p(D|\mu_{obs},\sigma_{obs},\mu_{syn},\sigma_{syn},\nu)\ \times\ p(\mu_{obs},\sigma_{obs},\mu_{syn},\sigma_{syn},\nu)}{p(D)} = p(\mu_{obs},\sigma_{obs},\mu_{syn},\sigma_{syn},\nu|D)$$

Because the integral is impossible to compute in many Bayesian models, the posterior distribution is estimated by generating a large representative sample from it by using MCMC methods. One hundred thousand MCMC samples were generated using weakly informative priors. Using weakly informative priors helps constrain parameters to reasonable ranges, and unless one truly believes that all possible events have an equal probability, weakly-informative priors are recommended in Bayesian analysis (Kruschke, 2013; McElreath, 2020). Moreover, when using weakly informative priors, even a modest amount of data will quickly overwhelm the prior, regardless of the value that prior takes (Kruschke, 2013; McElreath, 2020; Mourtgos, Adams, & Mastracci, 2021). This difference in means method yields complete distributional information about the parameters of both measures, with μ_{obs} - μ_{syn} being the causal effect of interest (Angrist & Pischke, 2009; Kruschke, 2013).

Table 2 provides parameter values, credible intervals, and probability values for our BSTS model estimating the effect of Floyd's murder on firearm assaults of police and indicates that there were 1.178 more weekly firearm assaults on police following Floyd's murder than we

[13] A traditional *t* test uses normal distributions to describe the data in two groups. However, to accommodate outliers, the *t* distribution is often used, as it has thicker tails than the normal distribution. The normality parameter in this analysis is often referred to as the "degrees of freedom," however, since we are not using the *t* distribution in this context, "normality parameter" is more appropriate. The normality parameter can range continuously from 1 to infinity. When the normality parameter is small, the *t* distribution has heavy tails. When the normality parameter is large, the *t* distribution is nearly normal. See Kruschke (2013) for further explanation.

would have expected if Floyd had not been killed by police.[14] The Floyd-related increase equates to 37.19 additional firearm assaults on police between May 25, 2020, and December 31, 2020. The probability value for the difference in means is .98, indicating a 98 percent probability that the observed number of firearm assaults on U.S. police following the murder of George Floyd departs from the estimated counterfactual of a world in which Floyd was not murdered by police. Importantly, this estimated increase corresponds to the entire post-Floyd period, which includes the 3-week spike immediately following Floyd's murder. Recalling that this spike equates to 25.62 additional firearm assaults on police in the 3-weeks following Floyd's death, this means that a substantial portion of the post-Floyd increase in firearm assaults on police was concentrated in the three weeks following Floyd's murder.

[Table 2 Here]

Discussion and Conclusion

The police murder of George Floyd in May 2020 sparked what experts have described as the largest social movement in history (Buchanan et al., 2020). Floyd's murder coincided with a once-in-a-century pandemic that sharply shifted the behavior of individuals and social institutions. This unique confluence of events presents challenges to assessment of whether Floyd's murder by police had a significant effect on violence against police. To address these important considerations, we used a Bayesian structural time series (BSTS) analysis to assess

[14] The frequentist analog to a credible interval is a confidence interval, yet the two concepts are statistically different. In a frequentist paradigm, confidence intervals are based on repeated sampling theory. A 95% credible interval indicates that if the same experiment is repeated *ad infinitum*, the unknown but fixed coefficient will fall within it. A credible interval can be interpreted as the probability that the population parameter is between the upper and lower bounds of the credible interval, based on available information (Mourtgos, Adams, & Nix, 2021).

whether the police murder of George Floyd led to a temporally specific change in the frequency of firearm assault on police. This technique allowed us to estimate a counterfactual model which incorporated data from before COVID-19 and after the onset of COVID-19 but prior to Floyd's murder. The comparison of observed post-Floyd firearm assault frequency against our synthetic counterfactual revealed evidence of a significant increase in retaliatory violence: Floyd's murder immediately preceded a sharp, 3-week spike in firearm assault on police, estimated at 25.62 additional firearm assaults relative to our synthetic counterfactual. This spike in retaliatory violence quickly subsided to levels only slightly above what would be expected in the absence of Floyd's murder. Overall, Floyd's murder led to approximately 37 additional firearm assaults on police between May 25, 2020, and December 31, 2020.

It is important to underscore that while our results show a significant, short-term increase in firearm assaults on police following George Floyd's murder, the shooting of a police officer remains a statistically rare event. To be clear: violence against police is a matter of grave concern that merits concerted attention from researchers. However rare, violence against police has very real costs. Though the post-Floyd spike in officers assaulted with a firearm is small in absolute terms, every case of firearm assault in our dataset represents injury and death of human beings. This says nothing of the pain and suffering of officers' friends, family, and communities, nor the danger that such violence poses to the function of democratic governance. What's more, we emphasize that the rarity of firearm assault on police does not mean officers' preoccupation with being shot is irrational. Indeed, given that U.S. police are assaulted with firearms more often than the U.S. public or police in other industrialized nations (Sierra-Arévalo et al., 2022, p. 1; Zimring, 2017, pp. 78–80, 86), firearm violence is a decidedly reasonable concern for officers on patrol. Nonetheless, we maintain that the post-Floyd spike in firearm assaults on police should be interpreted in light of its rarity within the context of a policing system comprised of more than 780,000 officers who engage in more than 61 million interactions with the public every year (Gardner, 2022; Harrell, 2020).

Our findings are notable in their departure from prior research on retaliatory violence against police. While past research found no evidence of retaliatory violence against police following an incident of police violence (Bejan et al., 2018), our analysis provides suggestive evidence that the police murder of George Floyd led to a significant, short-term increase in firearm assaults against police. In conjunction with the sharp decline in favorable attitudes toward police following Floyd's murder (Reny & Newman, 2021), this 3-week, post-Floyd spike in firearm assaults on police aligns with theories of legitimacy which predict increased violence during police-public interactions under conditions of damaged police legitimacy (Sunshine & Tyler, 2003, pp. 293–295). To be sure, the unprecedented scale of the protests following Floyd's murder necessitates caution in extrapolating from our analytic case to all incidents of highly publicized police violence. This caution is all the more merited when considering the unique context of the COVID-19 pandemic during which Floyd's murder and the subsequent spike in firearm assault against police occurred. Future research might investigate whether new high-profile police killings precede short-term spikes in retaliatory violence against police and how any such increase compares to that observed following Floyd's murder.

Our data and analytic approach come with important limitations. First, as described in prior research that uses GVA to explore violence against police (Sierra-Arévalo & Nix, 2020, p. 1054), GVA data is limited in that they exclude less injurious but far more common simple assaults against police. Though these data are useful in that they capture the most lethal subset of violence against police, the exclusion of simple assaults prevents confident generalization from our findings to *all* violence against police. It remains unknown if patterns in less injurious violence against police also changed because of Floyd's murder. Future research would do well to expand its scope and investigate these possibilities with high-quality national, longitudinal data that measure the full gamut of violence against police.

Persistent data limitations, however, present challenges to realizing such research. At the national level, individual agencies submit incident-level assault information to NIBRS about whether and what type of weapon were used, whether an officer was injured, and basic description of circumstances when an assault occurred. Unfortunately, low response rates frustrate confident use of these data—in 2020 and 2021, NIBRS did not reach the 60 percent reporting minimum set by the FBI (Congressional Research Service, 2022). At the state level, California collects and publicly posts rich incident-level data on assaults of police. However, it remains the only the state to do so, preventing combination of assault data across states for a national-level analysis. Considering these continued data quandaries, we believe that GVA remains useful for the study of police violence and the preferred option for studying the firearm assaults that drive felonious officer deaths.

Additionally, while our analysis provides suggestive evidence in support of a legitimacy-based mechanism for retaliatory violence following the murder of George Floyd, we did not measure police legitimacy nor directly test if changes in legitimacy affected firearm assault of police. As a result, we are unable to confidently discount the possibility that some of the observed Floyd-related spike in firearm assaults is explained by changes in police behavior and their perceptions of the public (Hoffman et al., 2021; Nix et al., 2017, 2020). Case in point, indiscriminate and excessively violent police tactics were documented at Floyd protests across the U.S. (Buford et al., 2020). Rather than ease tensions or deter violent escalation, indiscriminate violence “places officers at greater risk by increasing the number of people who are hostile toward the police and who view the use of force against police as justifiable” (Maguire & Oakley, 2020, p. 76). Similarly, Floyd’s murder and concerns over officer safety might have affected officer behavior during non-protest interactions in ways that also increased the probability of violence. Without more detailed data on the situational and interactional context which preceded each assault in our dataset, we are unable to conclusively confirm or refute this competing mechanism for the post-Floyd increase in firearm assaults on police.

In closing, we elaborate on our preference for the concept of “retaliatory violence” in research on violence against police by discussing the concrete dangers of the “war on cops” narrative. As we have argued, “retaliatory violence” parsimoniously captures a process of reciprocal conflict between police and the public. The “war on cops,” by comparison, is more amorphous and politically charged than a defined claim about increases in violence against police after a high-profile incident of police violence. It is invoked not only to draw attention to such violence, but also to discredit police critics, impede police reform, and solidify police power.

One retired officer called the “war on cops” a “national virus” and accused “subversive groups” like Black Lives Matter of perpetuating “narratives of hate” that exacerbate deadly violence against police (Martinelli, 2016). Another retired police officer lamented the “blood of police officers and sheriff’s deputies […] running in the streets” before drawing parallels between Black Lives Matter and the Ku Klux Klan (Sutton, 2015). In response to NAACP claims of racially biased policing by the Tulsa Police Department, one TPD officer wrote to his police colleagues, “This is an attack on you. It’s an attack on our profession and it’s done to sway public opinion against you” (Frank, 2018). Police and politicians resist calls to enhance accountability and transparency on the grounds that releasing police data poses unacceptable risks to officer safety (Moran & Hodge, 2020). “Blue Lives Matter” laws—a reference to the countermovement that emerged in response to Black Lives Matter—seek to treat assaults of police as federal hate crimes (Thusi, 2020).

These efforts are not historically novel. The rhetoric of police besieged in a “war” dates back more than half a century (McCormick, 1970), and the past 30 years provide ample evidence of this rhetoric’s evergreen political utility. In 1992, Phil Caruso of the NYPD’s Police Benevolent Association accused Mayor David N. Dinkins of fueling violent protests by treating a man killed by the NYPD as a “martyr.” Not coincidentally, the accusations came after Dinkins moved to establish an all-civilian police review board and independent panel to investigate police corruption (Dao, 1992). Two years later, President Bill Clinton’s State of the Union address invoked the death of a Washington, D.C., officer to advocate for the Violent Crime Control and Law Enforcement Act that would eventually be signed into law (Clinton, 2016). In the wake of the terrorist attacks of September 11, 2001, domestic policing swollen by funding for the War on Crime and the War on Drugs was further expanded and militarized to ensure national security and officer safety in the domestic War on Terror (Forman, 2009; Katzenstein, 2020). Following Floyd’s murder, Senator Chuck Grassley (R-IA) urged the Senate to move away from calls to reform and “defund” the police that he linked to rising crime and the murder of police officers (Grassley, 2021).

In addition to its role in resisting reform and expanding police power, “war” rhetoric also stands to exacerbate critical policing challenges. First, continued claims of a “war” are apt to amplify a “'we-them' siege mentality” which pits police against the public (Crank, 1994, p. 330). Insofar as this mentality is linked to police misconduct that harms public wellbeing and police legitimacy, the bellicose language of the “war on cops” is likely to only make matters worse. This rhetoric also has implications for current difficulties in the recruitment and retention of police officers (Mourtgos, Adams, & Nix, 2021; PERF, 2019). As suggested by police advocates and professional associations (IACP, 2020, p. 4; National Police Association, 2022), the perception of mounting violence at the hands of a hostile public is likely a contributing factor in depressed recruitment and increased resignations. By reducing departmental staffing levels, “war on cops” rhetoric may be reducing police effectiveness and response time in ways that, ironically, decrease officer safety.

Finally, “war” rhetoric also impedes a unified push for interventions that would simultaneously improve officer and public safety. At present, discussion of officer safety revolves around tactical and technological considerations that can be implemented by officers at the street-level. De-escalation, for example, aims to reduce the likelihood that an interaction results in violence (Engel et al., 2020). Body armor and tourniquets are designed to reduce injury and mortality when a shooting occurs (Sierra-Arévalo et al., 2022). As reasonable and useful as these efforts are, however, they reflect a conception of officer safety which views violence against police as a problem distinct from the community violence to which police respond.

Rather than perpetuate discussion of a “war on cops” and treat violence against police as a problem best addressed with an ever-expanding repertoire of training and equipment, such violence must be understood as a product of the same environmental conditions that give rise to violence more generally. As noted by the director of the Police Executive Research Forum with regard to increases in violence since 2020, “When homicides go up, more shootings go up, and it contributes to an overall increase in violence and police officers find themselves in the middle of that environment” (Tucker & Krishnakumar, 2022). Rather than continue to amplify empirically unsupported notions of a “war on cops,” police executives and policy makers interested in reducing violence should implement strategies which can reduce firearm violence through a mixture of enforcement and non-enforcement intervention (Abt, 2019). To do otherwise is to double down on political rhetoric instead of investing in evidence-based solutions that can simultaneously enhance public and police safety.

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Table 1. Fatal and Non-Fatal Firearm Assaults, 2014-2020

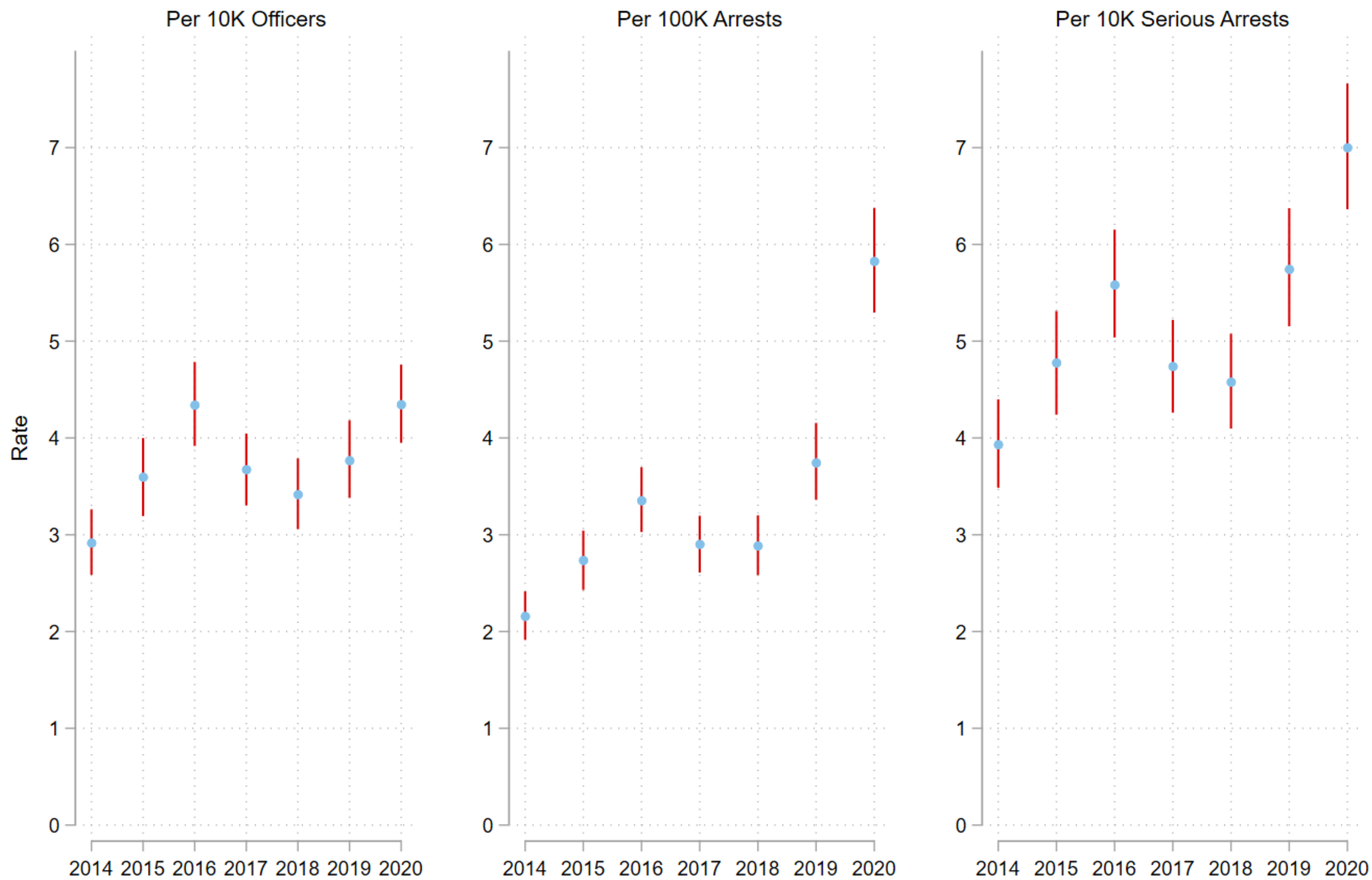
Year		Fatal	Nonfatal	Total	Total Assaults per 10K Officers
2014	N	37	152	189	2.91
	%	19.6	80.4	100.0	
2015	N	32	202	234	3.59
	%	13.7	86.3	100.0	
2016	N	59	229	288	4.34
	%	20.5	79.5	100.0	
2017	N	37	211	248	3.68
	%	14.9	85.1	100.0	
2018	N	45	195	240	3.41
	%	18.8	81.2	100.0	
2019	N	39	229	268	3.76
	%	14.6	85.4	100.0	
2020	N	37	275	312	4.34
	%	11.9	88.1	100.0	
Total	N	286	1,493	1,779	—
	%	16.1	83.9	100.0	

Table 2. BSTS Model of Floyd Murder on Firearm Assaults on Police – Weekly Estimates

	Mean	SD	Lower 95% CI	Upper 95% CI
Parameter				
μ_{obs}	6.078	.586	4.965	7.258
μ_{syn}	4.900	.127	4.652	5.152
σ_{obs}	2.840	.583	1.712	3.994
σ_{syn}	.655	.103	.464	.863
ν	20.976	23.559	1.265	67.502

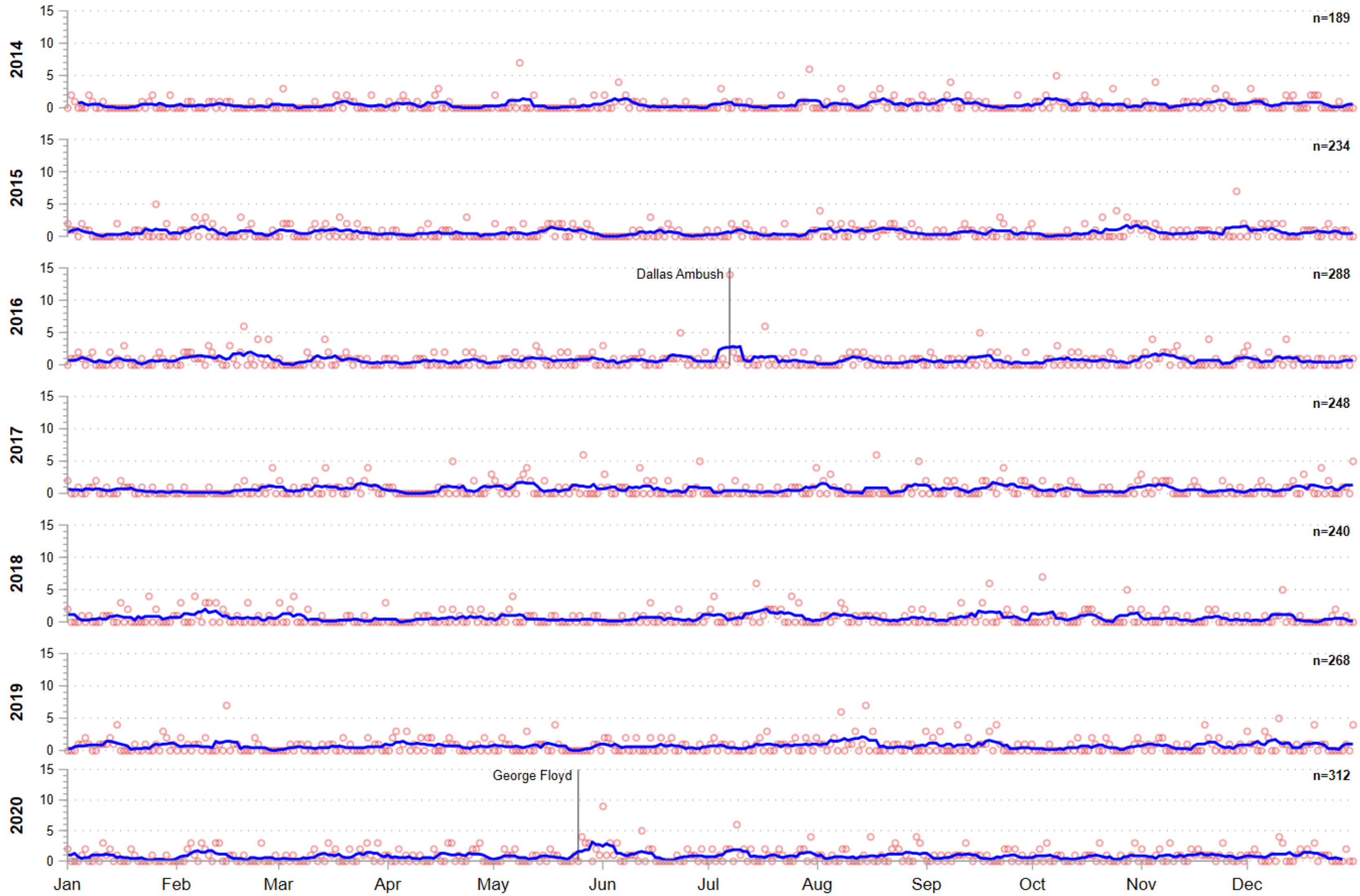
Mean Difference = 1.178 [.017, 2.380]
Probability of Different Means = .98

Figure 1. 3 Benchmarks for Yearly Rate of Firearm Assaults on Police, 2014-2020.



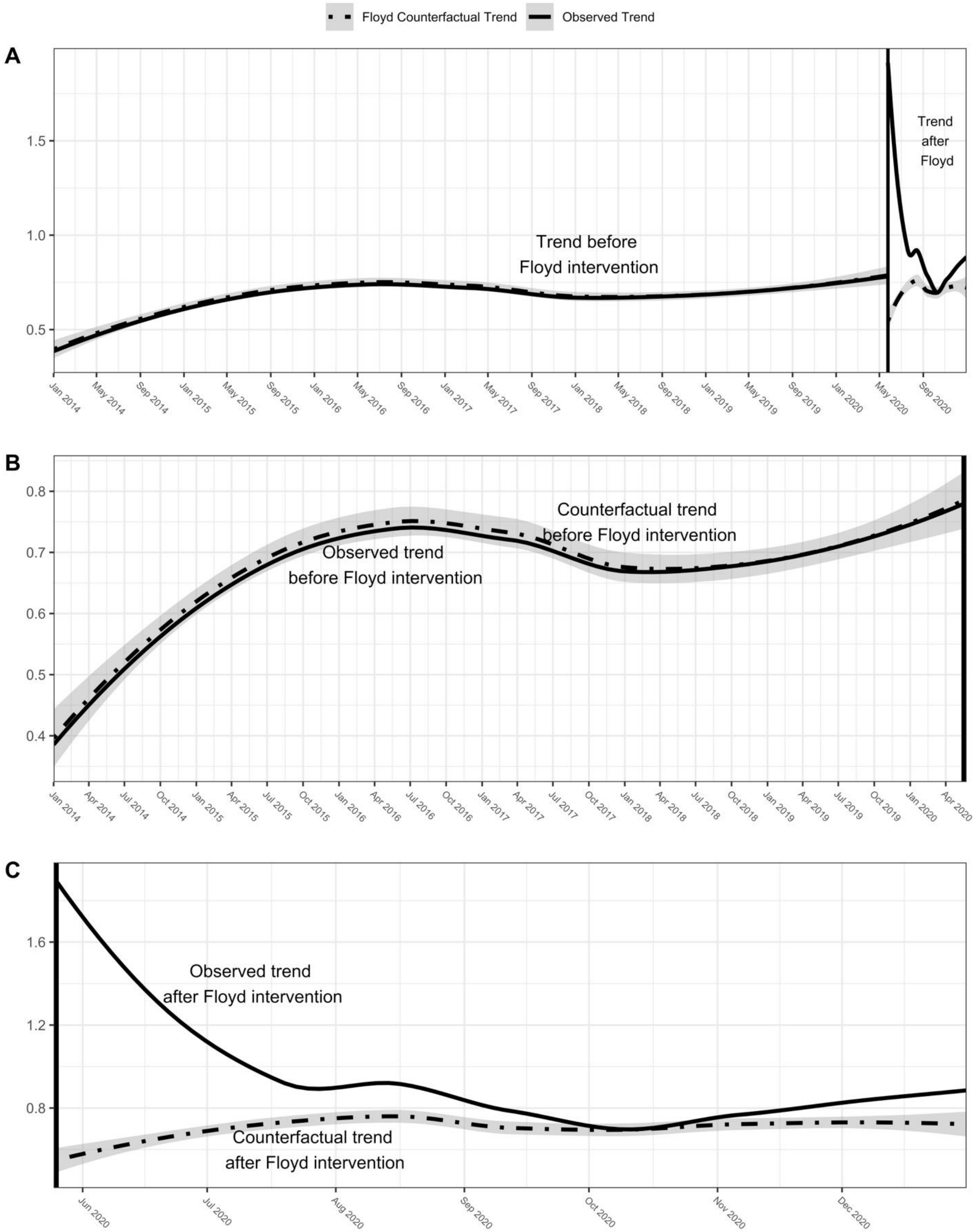
Note: Confidence intervals calculated with a yearly bootstrap using 1000 random simulations. A monthly bootstrap did not substantively alter the confidence intervals.

Figure 2. Daily firearm assaults on police officers, 2014-2020.



NOTE: Blue line is the rolling 7-day mean number of firearm assaults.

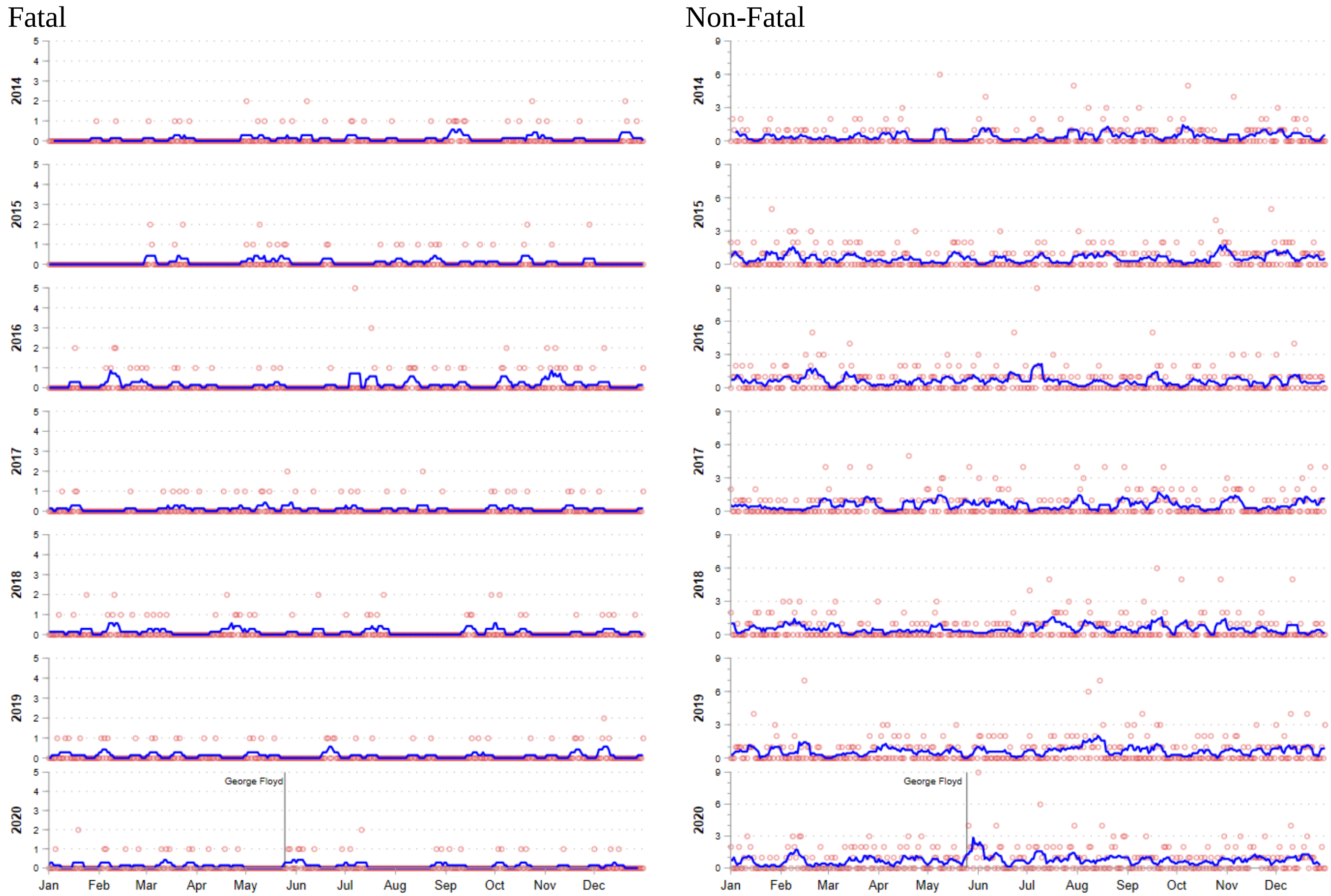
Figure 3. Observed and Counterfactual Trends in Firearm Assault on Police Before and After the Murder of George Floyd



Appendix A

As we describe on pg. 3, our analysis combines fatal and non-fatal firearm assaults to provide a more complete measure of firearm violence against police than what is captured when using only firearm fatalities.

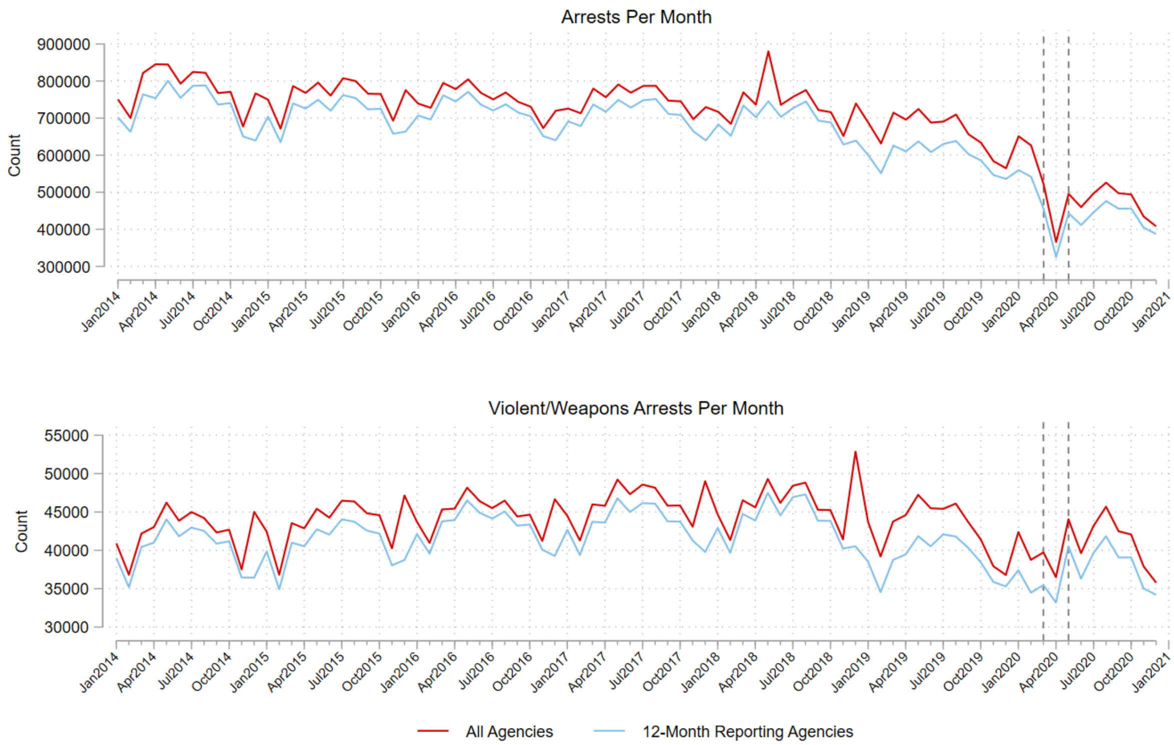
Appendix Figure A. Daily trends in fatal and non-fatal firearm assault of police, 2014-2020



Note: Trend line in each panel is a 7-day rolling average.

Appendix B

Appendix Figure B. Comparison of All Monthly Arrests and Monthly Violent/Weapons Arrests for All Agencies and 12-month Reporting Agencies, 2014 – 2020



Appendix C

Model Fit of Daily, Weekly, or Monthly Firearm Assault Data

Competing models were estimated using daily, weekly, and monthly counts of firearm assaults on police. The daily count model used 2,336 data points to train the BSTS model, the weekly model used 335 data points, and the monthly model used 77 data points.

All three models incorporated a semi-local linear trend component and seasonal parameter. Ten thousand MCMC iterations were simulated to fit each model.

Upon estimation, model fit was estimated for each of the three models in two ways. First, mean one-step-ahead prediction errors for each model were calculated. A time series has t observed time points $(x_1 \ldots, x_t)$. Predictions are made for the model at times $t+1, t+2$, etc. If we denote the k steps ahead forecast of x_{t+k} with data $(x_1 \ldots, x_t)$ by $x_t(k)$, then $x_t(1)$ is the prediction of x_{t+1} based on the data up to and including time t. The one-step-ahead prediction error, then, is described as $e_t(k) = x_{t+k} - x_t(k)$ and is the amount by which the model differs from the actual value once it becomes available. The mean one-step-ahead prediction errors for the daily model, weekly model, and monthly model are -.007, .016, and .104, respectively, indicating less error in the daily model than in the weekly or monthly models.

The Widely Applicable Information Criteria (WAIC) for each model's final state component was also evaluated. The Widely Applicable Information Criteria (WAIC) process compares models' predictive capabilities by estimating the relative out-of-sample Kullback-Leibler (KL) divergence. WAIC is advantageous because it makes no assumptions about the shape of the posterior distribution and provides an approximation of the out-of-sample deviance that converges to the cross-validation approximation in a large sample. WAIC accomplishes this by taking the log-posterior-predictive-density and attaching a penalty proportional to the variance in the posterior predictions, thus controlling for model overfitting risk (McElreath, 2020). A lower criterion value indicates a better fit. The WAIC scores for the daily, weekly, and monthly models are 1.1, 48.5, and 15.3, respectively, indicating a better model fit with the daily values.

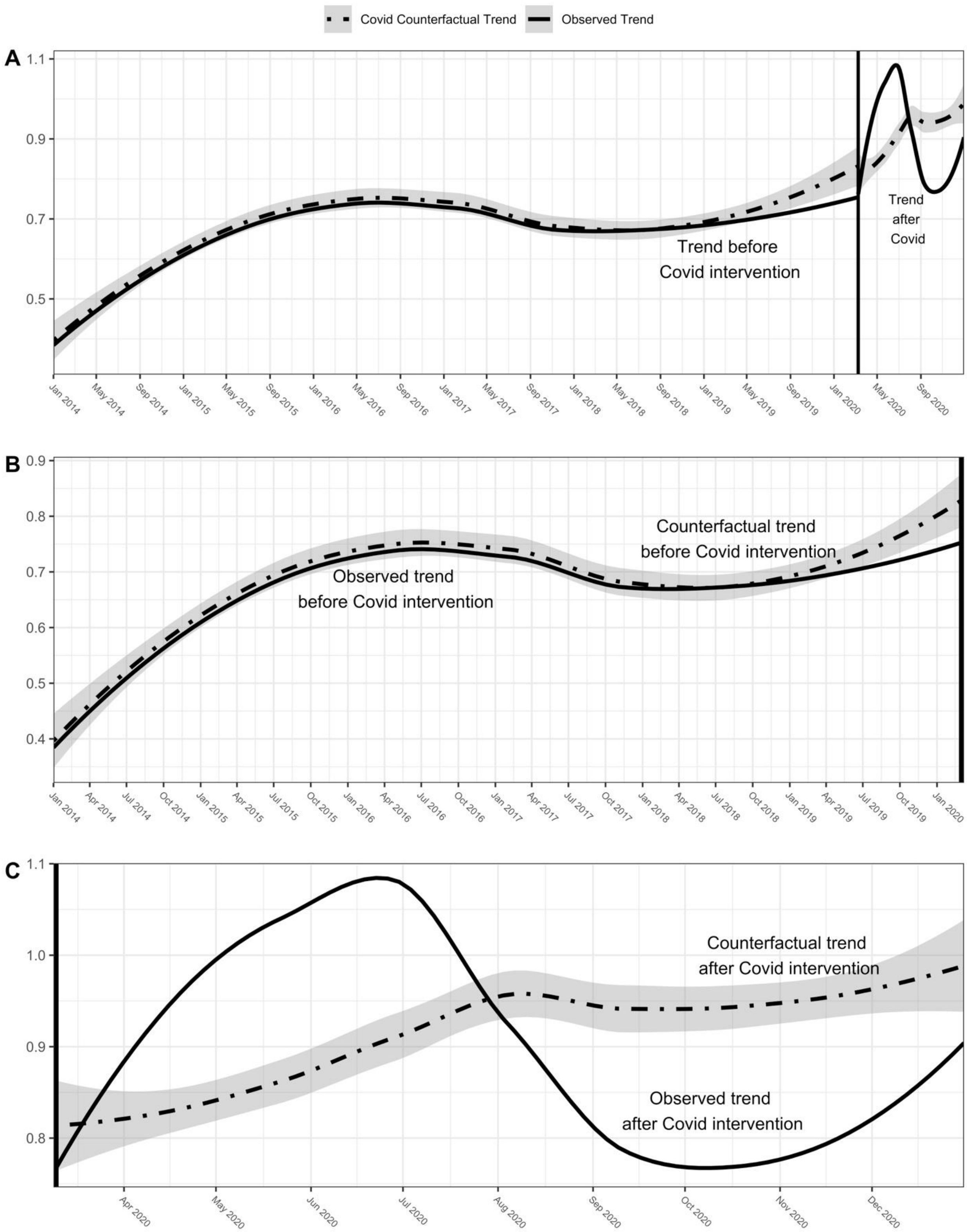
Appendix D

While initially developing the analytic strategy for this manuscript, we wrestled extensively with how to account for the onset of the COVID-19 pandemic. At one point, we ran separate BSTS models to estimate two effects: 1) the effect of COVID-19 on firearm assaults of police, and 2) the effect of George Floyd's murder (which occurred after the onset of COVID-19) on firearm assaults of police. Our original intent was to use this model to approximate a counterfactual world in which COVID-19 occurred (but not the Floyd murder), then compare it to the observed and counterfactual trends estimated in our Floyd model. However, because Floyd's murder occurred during COVID-19, we believe that such a comparison creates unintuitive counterfactual comparisons that are of limited practical use, in no small part because the COVID-19 model required naïve interpretation of a notable spike in firearm assaults in May 2020 that aligned with Floyd's murder. Inclusion of the COVID-19 model does not alter our conclusion about the Floyd-specific effect on firearm assaults of police. For these reasons, we circumscribed our analysis to focus specifically on whether Floyd's murder preceded a significant increase in firearm assaults of police and incorporated data into a single model from prior to COVID-19 and after COVID-19 onset but prior to Floyd's murder. For transparency, we provide description of how we constructed the COVID-19 BSTS model, visualization of its results, and interpretation.

Our strategy for constructing the COVID model was based on the timeline of the COVID-19 response in the United States. Leading up to widespread social distancing in the U.S. beginning March 9, 2020 (Leslie & Wilson, 2020), individuals' actions and local and state policies regarding pandemic response varied greatly. The World Health Organization's (WHO) timeline of the early days of the COVID-19 pandemic highlights the dynamic nature of information dissemination and varied governmental responses (World Health Organization, 2021). The WHO began supplying COVID-19 information and guidance in January 2020; in the context of natural disasters, this early, uncertain period of information gathering about an impending crisis is known as the "alarm" stage (Helsloot & Ruitenberg, 2004). In the following weeks, a host of largely uncoordinated policy responses across cities and states (e.g. stay-at-home orders, remote work and schooling) coalesced into widespread social distancing by March 9, 2020 (Leslie & Wilson, 2020). This date reasonably corresponds to the start of the "acute" stage of a disaster, when active measures to aid victims and save life are in widespread effect (Helsloot & Ruitenberg, 2004).

With this timeline in mind, we estimated our COVID model from January 1, 2014, through December 31, 2019. By using aggregated data that precedes the onset of the "alarm" stage of COVID-19 and the uncoordinated policy and behavioral changes from early 2020, we can reduce the effect of these varied and inconsistent COVID responses in our model. We then use March 9, 2020, the estimated date by which widespread social distancing was in effect, as the "intervention" date. Using this intervention point and model time periods, we estimated the COVID model with a semi-local linear trend state component and a weekly seasonal component. The time series used to build the COVID model covers January 1, 2014, through December 31, 2019, and is 2,191 data points in length. Ten thousand Markov Chain Monte Carlo (MCMC) iterations were simulated to fit the model. We found no evidence of significant autocorrelation and posterior residuals were normally distributed. The below figure plots the observed and counterfactual trends for our COVID model.

Appendix Figure D. Observed and Counterfactual Trends in Firearm Assault on Police Before and After COVID-19 Onset



Appendix Figure D shows the synthetic counterfactual trend and observed data trend for three periods. Panel A shows the entire time series and denotes the onset of widespread social distancing on March 9, 2020. Panel B focuses on the pre-intervention period and shows that the synthetic counterfactual tracks the observed data well up until January 2020. The observed trend then dips below the synthetic counterfactual before the March 9, 2020, intervention point, indicating a level of observed firearm assaults below what would be expected based on the synthetic counterfactual. At the beginning of the post-intervention period, shown in Panel C, the observed data remains below the synthetic counterfactual. However, in May and June the observed data spikes above the synthetic counterfactual before returning to a level below what would be expected without widespread social distancing.

Appendix Table D provides parameter values, credible intervals, and probability values for the COVID model. As in our main analysis, daily measures were collapsed to weekly values to allow for the approximation of normal distributions to make accurate mean comparisons between the two parameters (i.e., observed and counterfactual). Posterior predictive checks indicate good model fit when using weekly values.

Appendix Table D. BSTS Model of COVID-19 on Weekly Firearm Assaults of Police

	COVID Model			
	Mean	SD	Lower 95% CI	Upper 95% CI
Parameter				
μ_{obs}	5.867	.444	5.005	6.742
μ_{syn}	6.321	.120	6.081	6.552
σ_{obs}	2.500	.461	.1.596	3.376
σ_{syn}	.707	.092	.535	.894
ν	16.982	18.780	1.773	55.231

Mean Difference = -.454 [-1.333, .474]
Probability of Different Means = .84

Appendix Table D indicates that there were .454 fewer weekly firearm assaults on police than we would have expected without COVID-19-related SAHOs. The COVID model indicates that widespread social distancing had an inhibitory effect on firearm assaults on police that equated to 19.52 fewer firearm assaults on police between March 9, 2020 and December 31, 2020 when modeling only the effect of widespread social distancing (i.e., not considering any effect of Floyd's murder on firearm assaults). The probability value for the difference in means is .84, indicating a high probability that the distributions of the mean parameter values for the observed and synthetic counterfactual measures do not overlap. That is, there is a high probability that the observed trend in firearm assaults on police post-social distancing is a meaningful divergence from the modeled, counterfactual world in which COVID-related social distancing did not occur.

These supplementary results do not change our conclusion that Floyd's murder preceded a 3-week spike in firearm assaults of police. Instead, these results suggest that even with the Floyd-related spike in May 2020, COVID-19 very likely reduced the overall frequency of firearm assaults on police that would be predicted by patterns in firearm assault prior to March 9, 2020. Similar to our discussion of the observed Floyd-related spike, we are unable to confidently adjudicate between any number of potential mechanism(s) driving the estimated COVID-related decrease in firearm assaults of police. On the one hand, widespread social distancing and temporary stay-at-home orders reduced opportunities for crime, including offenses committed with a firearm ((Cohen & Felson, 1979; Lopez & Rosenfeld, 2021; Stickle & Felson, 2020). Simultaneously, police departments instructed officers to make fewer stops to reduce the likelihood of contracting or transmitting the virus (Lum et al., 2022). It is possible these changes in routine activities reduced the number of police-public interactions which, in turn, reduced the number of interactions that escalated into a firearm assault on police.

On the other hand, a decrease in the overall number of police-public contacts does not necessarily mean that officers faced decreased risk of violent victimization. Case in point, homicides, aggravated assaults, and gun assaults did not decrease during the early days of the pandemic. Instead, trends across 25 large cities followed the longstanding seasonal pattern of violence increasing along with ambient temperature (Rosenfeld et al., 2020; Berman et al., 2020; McDowall et al., 2012), and domestic violence-related 911 calls increased at least temporarily in many cities following the implementation of stay-at-home orders (Leslie & Wilson, 2020; Piquero et al., 2021; Richards et al., 2021). Thus, it is possible that police-public interaction declined overall during the first months of the pandemic while higher-risk interactions continued unabated or even more frequently relative to prior years. Which potential mechanisms best explain the estimated COVID-related reduction in firearm assaults of police derived from this model remains an open question for future research.

Appendix E

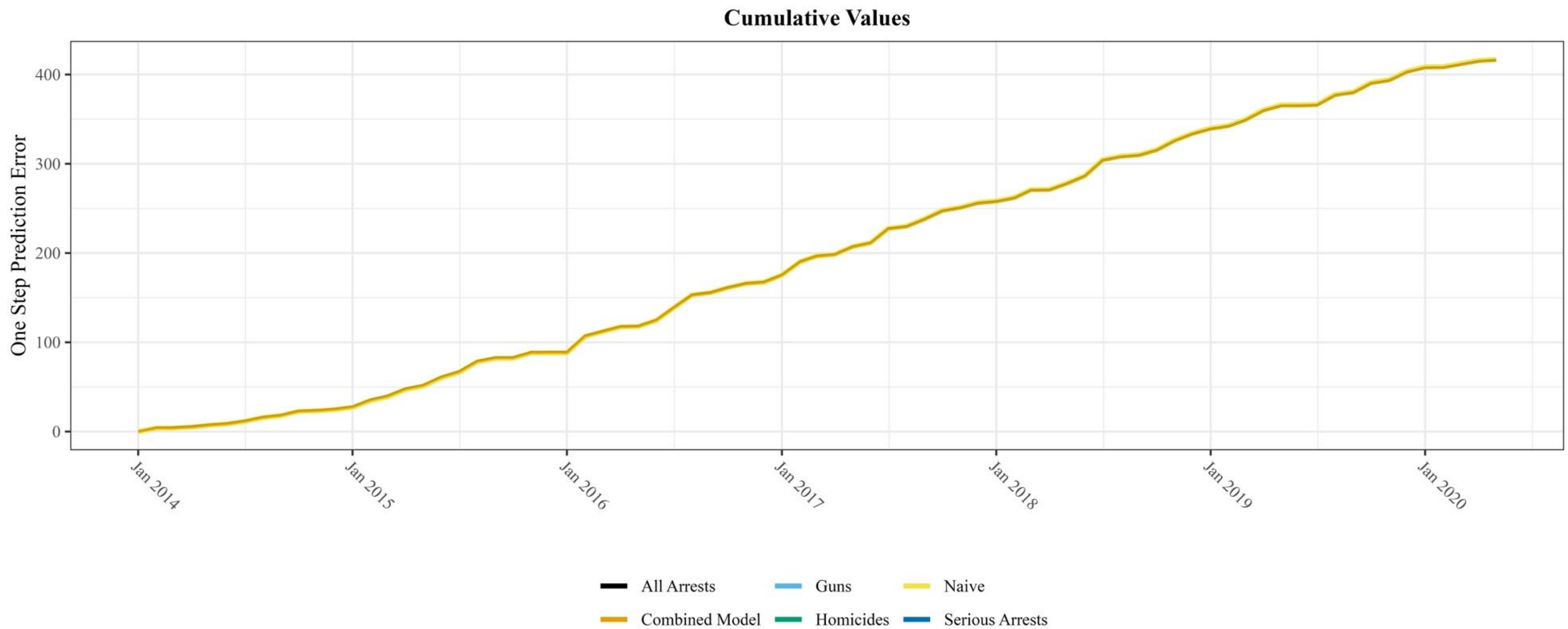
As a robustness check, we estimate a series of new BSTS models to assess whether the inclusion of regressors for homicide (a proxy for "general violence"), NICS Firearm Background Check data (a proxy for gun sales), overall arrests (a proxy for police-public contact), and violence/weapon-related arrests (a proxy for more high-risk contact) improved model fit. We did this by assessing each measure in separate models and then combining all of them into a single model. Because UCR homicide data, NICS background check data, and UCR arrest data are captured at the monthly level, we collapse our officer firearm assaults measure to the monthly level to assess the explanatory leverage gained from adding these covariates. We aggregate to the monthly level in the interest of thoroughly addressing reviewer comments but emphasize that daily-level data provide better model fit than monthly-level data (see Appendix C).

Six different models using monthly-level data were estimated using data from January 2014 through May 2020 (the same period used in our original BSTS model). The models include: 1) a model using only monthly counts of firearm assaults on officers (the 'Naïve' model); 2) a model including monthly homicides as a regressor (the "Homicides" model) 3) a model including monthly firearm sales as a regressor (the 'Guns' model); 4) a model including all monthly arrests as a regressor (the 'All Arrests' model); 5) a model using monthly violence and weapons-related arrests as a regressor (the 'Serious Arrests' model); and 6) a model incorporating monthly homicides, monthly gun sales, monthly arrests, and monthly serious arrests as regressors (the 'Combined' model).

When including regressors in BSTS models, a spike-and-slab prior is placed over coefficients, weighting predictors by averaging marginal inclusion probabilities for each regression coefficient. Semi-local linear trends and seasonal parameters were included in each model, with ten thousand MCMC iterations estimating each model. Part of the BSTS model fitting process includes generating the one-step-ahead prediction errors. The one-step-ahead prediction errors are a useful diagnostic tool for comparing models fit to the same dependent variable (i.e., counts of monthly firearm assaults).

Appendix Figure E plots each model's cumulative total of mean absolute one-step-ahead prediction errors. If a particular predictor increases a model's ability to predict future values of the monthly count of firearm assaults on police, then we would expect to see a decreased level of cumulative prediction errors relative to other models. As Appendix Figure E shows, the cumulative prediction error across all of the new models—Guns, All Arrests, Serious Arrests, Homicides, and Combined—is practically indistinguishable from our original Naïve model. These models accumulating error at the same rate indicates that the addition of monthly background checks, monthly arrests, monthly violence/weapons arrests, monthly homicides, or all these predictors combined does not add to the predictive accuracy of the naïve model which we employ in our BSTS analysis. These results, even using less precise monthly-level data, provide strong evidence that more general patterns in violence, firearm sales, and police-public contact do not account for the post-Floyd spike in firearm assaults on police.

Appendix Figure E. Cumulative One Step Prediction Errors for 6 BSTS Model Specifications



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