Gun Victimization in the Line of Duty: Fatal and Non-Fatal Firearm Assaults on Police Officers in the United States, 2014-2019

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Research Summary
Using open-source data from the Gun Violence Archive (GVA), we analyze national- and state-level trends in fatal and non-fatal firearm assaults of U.S. police officers from 2014 to 2019 (N = 1,467). Results show (1) the majority of firearm assaults are non-fatal, (2) there is no compelling evidence that the national rate of firearm assault on police has substantially increased over the last six years, and (3) there is substantial state-level variation in rates of firearm assault on police officers.

Policy Implications
GVA has decided strengths relative to existing data sources on police victimization and danger in policing. We consider the promises and pitfalls of this and other open-source datasets in policing research and recommend that recent state-level improvements in use-of-force data collection be replicated and expanded to include data on violence against police.

Keywords
policing; gun violence; firearm assault; danger

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Introduction

After more than 50 years of social science research on policing in the United States, the danger of police work remains a salient feature of police officers' occupational environment (Loftus, 2010; Marenin, 2016; Sierra-Arévalo, 2019). Scholarly attention to the danger of policing has been renewed by recent discussion of a "war on cops" that began after the 2014 police killing of Michael Brown in Ferguson, MO. Proponents of this hypothesized war posit that the contemporary political climate has resulted in widespread distrust and even disdain of police on the part of public officials, academics, and the news media; in turn, the public has become increasingly "anti-police" and emboldened to question, resist, and violently attack police officers on U.S. streets (Mac Donald, 2016). However, despite widespread concern among police administrators (Nix et al., 2018), empirical research on the most dire implication of a war on cops—violence against police—finds no significant increases in fatal or non-fatal violence against police in recent years (Maguire et al., 2017; Shjarback & Maguire, 2019). Nonetheless, the issue of violence against police remains highly salient to U.S. politics and policy, including the rise of the Blue Lives Matter movement and the growth in laws seeking enhanced penalties for killing police officers (Craven, 2017).[1]

Despite the rich history of research on danger in police work, however, there are several longstanding limitations to this body of scholarship. First, researchers' operationalization of "danger" tends toward the rarest, most extreme measure of danger in police work: felonious line-of-duty deaths that are driven by firearm assaults (see White et al., 2019, p. 14). This focus on felonious deaths underestimates the total scope of the danger police confront by ignoring non-fatal violence against officers (c.f. Bierie, 2017; Bierie et al., 2016), including non-fatal firearm assaults that, even though they do not result in a line-of-duty death, represent cases of deadly force directed at police. Second, analyses that attend to *all* assaults on police officers better capture less-than-lethal violence (e.g. punches, kicks) but do not differentiate such cases from especially lethal threats like firearm assaults (Shjarback & Maguire, 2019; Tiesman et al., 2018; c.f. Bierie et al., 2016). Third, data sources that rely on voluntary reporting by police (e.g. LEOKA, NIBRS) are limited by a lack of consistent reporting by law enforcement agencies and marked lag times in the release of said data, frustrating timely, confident estimates of a pressing public safety and policy issue (Kuhns et al., 2016, p. 6; Nix et al., 2019, p. 6; Shjarback & Maguire, 2019).

Because of its inattention to cases in which officers are shot but not killed, existing research tends to provide either an underestimate of gun violence directed at officers or eschew specificity in favor of an estimate of assault broadly defined. This, in combination with the data quality and timeliness issues that affect datasets commonly used to examine violence against police, prevents accurate estimates of total firearm assaults on officers that are of longstanding salience to the issue of officer safety in the United States (Cell, 2019; The President's Commission on Law Enforcement and Administration of Justice, 1967, p. 239).[2] Given the decided gravity of the

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[1] An analysis of emergency room data by Tiesman et al. (2018) departs from other studies of officer victimization and finds a short term increase in non-fatal assaults. However, this trend was found only for 2007 to 2011; the trend then decreased through 2014 to pre-2007 levels.

[2] The threat of being shot on duty looms large in the United States where the supply of civilian-owned firearms was recently estimated at approximately 265 million (Azrael et al., 2017). At least in part because of the large supply of

problem at hand, there is a clear and urgent need for researchers to bring new, more timely data to bear.

This article addresses these issues with open-source data provided by the Gun Violence Archive (GVA), a non-profit organization that collects and constantly updates data on firearm assaults of police officers across the United States. Because GVA records both fatal and non-fatal firearm assaults on police, we are able to provide an estimate of firearm assaults on police officers that includes (and differentiates) fatal and non-fatal shootings.[3] We use these data to provide national- and state-level estimates of fatal and non-fatal firearm assaults against police officers in the United States from 2014 to 2019. We conclude with consideration of future directions for this research as well as the promises and limitations of data like those collected by GVA in research on violence against and by police. We also provide concrete policy recommendations for improving the quality and timeliness of data on violence against police to better support police agencies, researchers, and policy makers.

Literature Review

Social science research on the danger of police work in the U.S. can trace its roots back more than half a century to foundational ethnographic studies of life on patrol. Early single-site studies (Westley, 1953, 1970) and comparative studies (Banton, 1964) note officers' tangible preoccupation with danger and violence in the line of duty. Decades of subsequent scholarship confirm the enduring importance placed by officers, supervisors, and the police organization on the reality of violence in policing (Brown, 1988; Moskos, 2009; Sierra-Arévalo, 2016; Skolnick, 1966), especially when that violence proves deadly (Manning, 1977, pp. 7–8; Sierra-Arévalo, 2019).

Such qualitative research began to be complemented by quantitative analyses of line-of-duty danger beginning in the 1970s. In 1971, a group of law enforcement executives—in response to sharp increases in felonious officer deaths throughout the 1960s—called for an expansion of the FBI's data collection efforts on violence directed at police (Rabe-Hemp, 2017, pp. 61–62). Beginning in 1972, the FBI began collecting more detailed information on both officers killed and officers assaulted in the line of duty, eventually combining these data in 1982 into what is now commonly known as LEOKA, or Law Enforcement Officers Killed and Assaulted (FBI, 2019a). Researchers quickly took advantage of this new data source to quantitatively assess the landscape of violence against police.

The earliest analyses of LEOKA data concentrate on felonious officer deaths, specifically in cities, and uncovered a positive relationship with structural factors such as the percentage of the city population that is Black, city crime rate, and the proportion of a city living in poverty (Lester, 1977, 1984). Later city-level analyses examine the relationship between political, city-

firearms, officers in the United States are victimized by firearms at rates far greater than in European nations like Germany or England (Zimring, 2017, pp. 79–80, 86).

[3] We recognize that there are other forms of deadly (i.e. lethal) force that seriously injure or kill police in the line of duty, such as assaults with a knife. Nonetheless, firearms stand are far and away the weapons used most often in fatal attacks on officers. For example, the most recently recorded fatal stabbing in the Officer Down Memorial Page's data occurred in 2017. By comparison, there were 144 fatal firearm assaults between 2017 and 2019 (ODMP, 2020).

level factors like Black representation in city council and Black mayorship on felonious police deaths (Jacobs & Carmichael, 2002; Kaminski & Stucky, 2009; Kent, 2010). LEOKA has also been used to examine felonious officer deaths at the national (Swedler et al., 2015), regional (Fridell & Pate, 1995), and county level (Kaminski, 2008). Finally, other scholars have moved beyond LEOKA and turned to the National Violent Death Reporting System (NVDRS) (Blair et al., 2016), the National Incident Based Reporting System (NIBRS) (Bierie, 2017; Bierie et al., 2016; Willits, 2014) or data collected by non-profit organizations like the National Law Enforcement Officers Memorial Fund (NLEOMF) or the Officer Down Memorial Page (Kaminski & Marvell, 2002; Maguire et al., 2017; White et al., 2019) to explore patterns in felonious police deaths.

Scholars have noted for some time, however, that analyses focused on felonious line-of-duty deaths systematically underestimate the full scope of danger that officers face by excluding non-fatal assaults (see Brandl, 1996). Accordingly, other research has analyzed non-fatal assaults, specifically (Shjarback & Maguire, 2019; Tiesman et al., 2018), both fatal and non-fatal assaults (Crifasi et al., 2016; Fridell et al., 2009), or some combination of fatal assaults, non-fatal assaults, and line-of-duty accidents (Brandl, 1996; White et al., 2019). These related streams of research provide invaluable insight but, of course, also come with important limitations.

With regard to studies that focus on non-fatal assaults or which examine both fatal and non-fatal assaults, the clearest benefit of such research is its ability to describe the most common type of violence directed at police. Estimates from the most recently available LEOKA statistics illustrate this point: in comparison to the 55 officers feloniously killed in 2018 (51 by firearm), nearly 59,000 were non-fatally assaulted (2,116 by firearm) (FBI, 2019b). This practical benefit notwithstanding, special attention to non-fatal assaults often obfuscates the particular phenomenon of assaults which, even if non-fatal, constitute a use of deadly force against police. For example, Shjarback and Maguire's (2019) time-series analysis of LEOKA data to investigate trends in violence directed at police, though able to provide cautious estimates of national-level trends in non-fatal assaults, does not analytically distinguish an injury caused by a fist or a bullet. Tiesman et al.'s (2018) analysis of injurious assaults treated in U.S. emergency rooms and analyses employing NIBRS data have the same limitation (Bierie, 2017; Willits, 2014).[4]

There are several studies that disaggregate fatal and non-fatal firearm assaults on police across the U.S. Bierie and colleagues' (2016) analysis of gun violence against police includes both fatal and non-fatal firearm assault estimates drawn from NIBRS, improving on past research that either focuses on fatal assaults alone or which conflates firearm assault with assault more generally. However, though NIBRS collects data from multiple states and thousands of law enforcement agencies, it is affected by data issues not unlike those that affect LEOKA (Kuhns et al., 2016, p. 6). In 2010, the most recent year of NIBRS available to Bierie et al. (2016), approximately 5,400 agencies from 37 states were represented in NIBRS, capturing only 37% of agencies and oversampling on small- and medium-sized agencies (2016, p. 506). In the same vein, though Crifasi et al. (2016) differentiate fatal from non-fatal firearm assaults in their study

[4] Other studies, though able to differentiate firearm assaults from simple assaults, are unable to speak to national-level trends because their data are limited to a single city (Brandl, 1996; Brandl & Stroshine, 2003, 2012; Gibbs et al., 2018).

of assault lethality, their reliance on LEOKA data raises concerns about the reliability of their firearm assault estimates similar to other studies employing this dataset.

Besides the lack of representativeness that characterizes LEOKA and NIBRS data, the issue of significant lag times in the release of these data creates marked challenges in providing timely, accurate analyses of deadly force against police. Though open-source data like those provided by the National Law Enforcement Officer Memorial Fund (NLEOMF) and the Officer Down Memorial Page (ODMP) provide practically real-time data on officers accidentally and feloniously killed in the line of duty, they do not record information on non-fatal assaults. As a result, researchers interested in non-fatal assaults are largely restricted to data that are anywhere from 18 to 24 months old (Kuhns et al., 2016, p. 6).[5] This is, of course, neither the fault of researchers nor, to our knowledge, the result of willful tardiness on the part of government—collecting, cleaning, and collating data from thousands of independent law enforcement agencies is a monumental undertaking. Nonetheless, the persistent limitations of existing data create clear need for new, national-level data sources that can enable more timely investigation of firearm violence against police and support the decision making of law enforcement agencies and policy makers.

Data and Method

Data Source

This analysis uses data collected by the Gun Violence Archive (GVA), an independent, non-profit organization whose mission is to "provide free online public access to accurate information about gun-related violence in the United States" (GVA, 2020a). The GVA's definition of gun-related violence is expansive and tracks firearm homicides, suicides, and injuries, as well as accidental shootings, defensive firearm uses, mass shootings, officer-involved shootings, and more. To gather this data, GVA researchers monitor approximately 7,500 news media, law enforcement, and governmental sources from across the United States for cases of firearm violence. Additionally, GVA researchers manually sweep social media accounts (e.g. Facebook, Twitter) and websites to gather relevant cases. For each incident, GVA records date, geocoded location, city/county, state, available victim- and perpetrator-level information (e.g. name, age, sex), incident type (e.g. "Shot – Wounded/Injured", "Shot – Dead"), and URL links to online sources that document each incident (GVA, 2020b).

Notably, while GVA collects data on officer-involved shootings of the public tracked by other open-source efforts,[6] GVA stands apart in that it also records firearm violence directed at law enforcement officers. Additionally, GVA includes and differentiates fatal and non-fatal firearm

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[5] As Kuhn et al. (2016) discuss, though a LEOKA summary report on a given year is released nearly 12 months after that year's conclusion (e.g. the 2013 LEOKA summary report was released at the end of November 2014), the detailed data necessary for more than summary statistics are released 16-18 months after year's end. As a result, the most recent LEOKA data available for Kuhn et al.'s report, written in 2015 and published in 2016, was for 2012.

[6] Other open-source data sources that draw on media reports, submissions from the public, and public records requests include: *The Counted* (Swaine et al., 2016), *Mapping Police Violence* (Sinyangwe et al., 2020), and *Fatal Encounters* (Burghart, 2017).

assaults, allowing for more complete and fine-grained estimation of the firearm violence that results in the death and non-fatal injury of police officers.

Case Selection and Analytic Strategy

All cases in the GVA's larger dataset in which law enforcement officers were shot (fatally and non-fatally) were provided by GVA for the period between January 1, 2014 and December 31, 2019.[7] We restrict our analytic sample in several ways.

First, we include active, sworn local and state law enforcement officers who are members of agencies that respond to calls for service; this sample is composed largely of officers employed by local departments at the city or county-level, sheriff's departments, and state police agencies. Additionally, our sample includes special jurisdiction officers such as transit or university police, tribal police, and specialized state agencies like wildlife or park police whose patrol and enforcement activities are reasonably similar to local and state departments. We exclude federal law enforcement officers, parole and probation agents, and court officers who, though sworn, do not engage in routine investigatory, patrol, or enforcement activity.

Second, our analytic sample is restricted to cases involving (a) on-duty officers, (b) whose person or equipment (excluding vehicles) was shot,[8] (c) with a firearm, (d) by someone who is not a police officer (including while struggling with a suspect over a firearm).[9] These criteria exclude off-duty firearm injuries, injuries caused by means other than a pistol, rifle, or shotgun (e.g. shrapnel from an explosion, pellet gun), self-inflicted firearm injuries whether accidental (e.g. training accident) or intentional (e.g. suicides or suicide attempts), and "blue on blue" shootings in which one officer accidentally shot another officer.[10] Additionally, these criteria exclude cases in which a suspect fired at but did not strike an officer, as well as those in which a suspect pointed a firearm at an officer but did not fire.

To select this sample from the raw data provided by GVA, the authors (and a research assistant directly supervised by the first author) independently checked each case (n=1962). The case-by-case check was accomplished by following the online sources recorded by GVA for every

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[7] The latter half of November 2019 and all of December 2019 were provided to the authors by GVA after the beginning of 2020. An additional data pull from the GVA database was performed on January 10, 2020 to check for additional cases from November and December 2019 that had not yet been found. No additional cases in those months were identified by GVA in this final data pull.

[8] We include cases in which officers were grazed by a bullet, shot in their ballistic vest or other protective equipment (e.g. ballistic shield, ballistic helmet), and cases in which officers' radios, duty belts, boots, secondary weapons, etc., were hit by bullets, bullet fragments, or shrapnel (e.g. glass, metal shards). We include such cases because they are the outcome of suspects firing rounds at officers where the difference between minimal and significant injury is exceedingly slim.

[9] In cases of a struggle over a firearm, we included cases of officers being shot in a struggle over a firearm even if it was unclear whether it was the suspect or the officer who pulled the trigger of the firearm. However, in cases where the trigger-puller was unclear, we then considered whether other officers opened fire. If other officers opened fire and it was not explicitly stated the an officer was hit by a round fired from the firearm over which a struggle occurred, we excluded the case on the grounds that we could not rule out a "blue-on-blue" /"friendly fire" incident.

[10] We focus on on-duty officers to more accurately estimate the prevalence of non-fatal firearm injury to officers in the course of normal policing activities as opposed to cases in which off-duty officers happen to be victims (e.g. victim of a robbery) or those in which they intervene in situations outside their official duties.

individual listed in the dataset. Because URLs for online media reports were sometimes inactive, Internet searches using the incident date, incident location, and available officer names were used to find other sources to verify the incident. In the interest of providing a conservative estimate of non-fatal firearm injury, cases for which media sources listed an officer as "wounded," "injured," or "hurt" but did not specifically stipulate a gunshot injury from a bullet, shot (e.g. shotgun ammunition), bullet fragments, or shrapnel were excluded. Similarly, cases in which it was unclear if an officer shot themselves, was shot by a suspect, or was shot by another officer were excluded to err towards a conservative estimate. Cases which coders were uncertain how to code were flagged and reviewed by the authors to arrive at a final coding decision. Our inclusion criteria and coding process produced an analytic sample of 1467 cases for our descriptive analysis of fatal and non-fatal firearm assaults on police officers to provide estimates at the national and state level (see Table 1).[11]

[TABLE 1 here]

To calculate national rates of firearm assault per 100,000 officers (or at the state level, per 1,000 officers), we use estimates of the number of sworn local and state officers from the FBI's Police Employee data (PE), which document the number of sworn officers at the agency level.[12] As mentioned in our discussion of past work, PE data (and the FBI'S UCR data more broadly) have

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[11] These 1,467 cases represent individual officers assaulted by gunfire during 1,185 incidents (1.24 officers per incident). 510 cases were removed from our analytic sample based on manual verification and our inclusion criteria. 15 cases of an officer being shot that were not present in GVA data were found during independent verification of cases (11 of which were retained). 6 cases were coded as "unverifiable" and excluded from our sample when no online sources could be found to verify GVA-listed information. 8 cases were coded as "unclear" and excluded from our sample because the available news sources did not provide sufficient information to definitively discern who shot an officer. One such case described an officer who was non-fatally shot in the hand while making entry into a residence to serve a search warrant. News sources indicate a suspect pointed a firearm at officers and was shot by officers but do not stipulate the suspect ever fired their weapon. Another case involved an officer who was shot in the foot while struggling with a suspect in a bar. A second officer present at the scene fired multiple rounds and the suspect's firearm was discharged once. News sources covering the incident did not state whether the officer was struck by a round from the suspect's weapon or that of the other officer. See Appendix A for a frequency table of reasons for case exclusion. Note that these frequencies do not sum to 510 because cases could be excluded for multiple reasons (e.g. a federal law enforcement officer who accidentally shot themselves, an off-duty deputy who committed suicide).

[12] Law enforcement officers are defined by the FBI as "individuals 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, 2019a). Our estimates exclude agencies that report they are "covered by" another law enforcement agency in order to avoid double-counting officers. Our chosen denominator—population of local and state officers—has strengths and limitations. To the former, this denominator is well-suited to estimate the *average* risk of firearm assault faced by sworn state and local police officers. Because it does not require assumptions about the type of interaction (if any) that precedes a firearm assault, we are able to include the greatest possible number of firearm assaults in our analysis. Conversely, our denominator does not differentiate officers based on their assignment (e.g., administrative versus patrol versus SWAT), obfuscating variation in officers' exposure to the risk of firearm assault and preventing estimation of assignment-specific risk profiles. Other denominators (all with their own assumptions and limitations) could be used, including the rate of firearm assault vis-à-vis violent crime, firearm crime, or arrest. An arrest-based denominator might, for instance, be preferred for estimation of the risk faced by patrol officers since they are responsible for most arrests. On the other hand, appropriate use of an arrest-based denominator would necessitate excluding firearm assaults that did not occur during an arrest—a problematic choice since, between 2014 and 2018, only 5 percent of felonious officer deaths occurred during an arrest (FBI, 2019d).

well-documented issues with incomplete reporting/missing data (King et al., 2011; Lynch & Jarvis, 2008). We also note that 2019 PE data is currently unavailable at the time of this article's writing, further underscoring our critique of the lag time in the release of governmentally-produced policing data. To address these two issues, we follow the suggestions of past research (King et al., 2011, p. 450; Stucky, 2005) and use multiple years of PE data to impute missing estimates of sworn state and local officers. Specifically, we calculate a quadratic regression function for each state's officer population for 2013-2018, then use the regression coefficients for *year* and $year^2$ to estimate missing state-years. We use this approach to impute a total of 53 values, 51 of which are 2019 imputations (50 states plus Washington D.C.) and two of which correspond to a single year of missing data for Alaska in 2015 and West Virginia in 2014.[13]

Despite our use of multiple years of data to impute 2019 values for each state and mitigate the unreliability of any single year estimate, there are some states in the PE data that show reporting problems across several years. According to FBI UCR records (FBI, 2019c), three states—Mississippi, Indiana, and West Virginia—had less than 75 percent of agencies in metropolitan statistical areas (MSAs), cities outside MSAs, or nonmetropolitan counties report data to the FBI for every year between 2013 and 2018.[14] We denote these three states in all our analyses of state-level trends in firearm assault on police officers with "*" and discuss the broader implications of such data quality issues in our Discussion.

Results

From 2014 to 2019, 249 police officers were fatally shot by suspects and 1,218 were struck or non-fatally wounded by suspect gunfire (see Table 1). The total number of firearm assaults during this period has shifted between a low of 189 in 2014 to a high of 288 in 2016. Over the full 6-year period, an average of 245 officers a year were shot by suspects in the line of duty. Of those shot, an average of 42 per year (17 percent) were killed; only 14 to 21 percent of firearm assaults on officers each year result in fatalities, underscoring the importance of collecting and analyzing data on non-fatal firearm assaults alongside those on fatal firearm assaults.

Figure 1 presents monthly frequencies of fatal and non-fatal firearm assault on officers from 2014 to 2019. On average, 20 officers were assaulted with firearms each month. The number of monthly firearm assaults ranged from a low of 10 in February 2014 to a high of 46 in February 2016. Interestingly, there is no clear evidence of seasonality in firearm assaults on officers overall or when looking at non-fatal firearm assault, running counter to seasonal patterns found by some research for violence and crime more generally (McDowall et al., 2012; McDowall &

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[13] We selected a quadratic imputation function after benchmarking it against two other imputation models. The first, a lagged value imputation, assumes no year-to-year variation in the state's officer population and duplicates the 2019 value for the prior year of data (e.g. state-year $population^{2019}$ = state-year $population^{2018}$). The second, a linear regression imputation, calculates a predicted state-year population value for every state-year by regressing state officer population on *year*. The quadratic imputation is the same as the linear imputation save for the addition of a $year^2$ term that allows for variation in the slope of the regression function over time. For each of these imputation methods, we then compared the predicted population estimates to the original values present in the PE data and calculated the root mean squared error (RMSE) for each model to assess the magnitude of the error between estimated values and the PB population estimates. Of our three tested imputation approaches, the quadratic regression imputation model had the smallest RMSE values. See Appendix B for more information.

[14] See Appendix C for an illustration of this data quality issue using 2018 UCR data.

Curtis, 2015). Turning to longitudinal trends, though the trend in the monthly frequency of fatal firearm assaults on officers is quite flat from 2014 to 2019, there does appear to be a slight, upward trend in the monthly frequency of firearm assaults on officers, overall. This overall trend is driven by the parallel trendline in monthly non-fatal firearm assaults. Without accurate estimates of the population of officers in the United States per month, however, it is not possible to calculate a monthly rate. Though, to our knowledge, no such monthly estimates exist, we can aggregate monthly counts of firearm assaults into yearly counts and use yearly estimates of the population of U.S. police officers to calculate annual rates.

[FIGURE 1 here]

Figure 2 does exactly this and plots the national rate of firearm assault on police officers (per 100,000 officers) from 2014 to 2019 and also disaggregates this overall rate into separate trend lines for fatal and non-fatal firearm assaults. Across the time series, the national rate of firearm assault on police was lowest in 2014 (29.92 per 100,000 officers) and highest in 2016 (44.11 per 100,000 officers). Overall, the national rate shows a slight upward trend between 2014 and 2019 ($B = .750$).

Turning to the disaggregated trend lines for fatal and non-fatal firearm assaults, two notable patterns emerge. First, we find that year-to-year changes in the fatal and non-fatal firearm assault rates do not consistently track one another over time. The rates of fatal and non-fatal firearm assault diverge from 2014 to 2015, move in parallel between 2015 and 2017, and diverge again from 2018 to 2019. Note also that 2017 to 2018 is the only period in which the rate of non-fatal firearm assault on officers decreases while the rate of fatal assault increases. Overall, these longitudinal patterns reinforce that trends in the national rate of firearm assault on police are largely driven by changes in the rate of non-fatal firearm assault.

Similar to the frequency trends shown in Figure 1, the trend in the national fatal firearm assault rate is quite flat from 2014 to 2019 ($B = -.025$) while the fitted linear trends for total firearm assaults and non-fatal firearm assaults shows a slight increase ($B = .750, .772$). Of course, sober interpretation of this increase is merited given that the slope of both of these trend lines is quite small and represents a relatively small yearly increase in the number of officers non-fatally assaulted with firearms. To illustrate this, let us assume a static number of officers drawn from 2018 UCR estimates of the number of full-time, sworn police officers in the United States: 686,665 (FBI, 2019c). Using this as a population baseline, we then look to the slope of the fitted non-fatal firearm assault trend knowing that trends in total firearm assault are driven by changes in non-fatal assault. The slope of the fitted trend for the rate of non-fatal firearm assault suggests that, on average, an additional 5.3 officers were victims of non-fatal firearm assault every year between 2014 and 2019.

[FIGURE 2 here]

Though certainly informative to study national trends, such analyses are likely to be affected by aggregation bias wherein heterogeneity across smaller ecological units is masked (Kaminski, 2008; Kaminski & Marvell, 2002; Kent, 2010; Peterson & Bailey, 1988). That is, by combining data from across the U.S. to produce national-level estimates, we risk losing sight of important variation at smaller units of analysis. To address this, we first provide a state-level view of the frequency of firearm assaults on police for 2014 to 2019 (see Figure 3). Over this 6-year period, states experienced an average of 28.77 firearm assaults, or 4.80 firearm assaults per year. Texas (n=143) and California (n=112) had the highest number of firearm assault incidents over this period, averaging 23.83 and 18.67 firearm assaults per year, respectively. In contrast, GVA data indicate Delaware and Montana each experienced only two firearm assaults on officers over this period.

[FIGURE 3 here]

Next, we calculate 6-year average firearm assault rates for each state to account for variation in state-level officer populations (see Figure 4). Our results show substantial variation across the United States. Officers in Mississippi, New Mexico, and Alaska experienced the greatest risk of being assaulted with firearms over the last six years. Both Mississippi's and New Mexico's average firearm assault rate from 2014 to 2019 were more than two standard deviations greater than the national mean (.47 firearm assaults per 1,000 officers); Alaska's rate was more than 1.5 standard deviations greater.[15]

[FIGURE 4 here]

At the other extreme, some geographically-clustered states showed markedly lower rates of firearm assault on officers over this time period. For example, the 6-year average firearm assault rate in New York and New Jersey was between .5 and 1 standard deviation below the national mean, and Connecticut was the only state with a 6-year average rate more than 1 standard deviation below the national mean. However, other geographic regions show more apparent variation between states, such as in the southeastern U.S. where Florida—compared to Alabama, Georgia, and South Carolina—appears to have been safer for police officers. In the southwest, New Mexico stands out as considerably more dangerous than its neighboring states as measured by its average firearm assault rate over the past six years.

Figure 5 displays the 6-year average firearm assault rate for each state and the District of Columbia, and illustrates each state's relative position to the U.S. mean over the same time period. Here, we note that although 46 states and the District of Columbia fell within one standard deviation of the mean, there is still meaningful variation among these states. Consider Idaho, which had a six-year average firearm assault rate of 0.71 per 1,000 officers. Officers working in Idaho were 1.9 times more likely to be assaulted with a firearm than officers in

[15] As we have noted, Mississippi's rate was deemed unreliable and its rate should be interpreted with this in mind. Appendix D shows new estimates for Mississippi and two other states using more reliable (but dated) estimates from the Census of State and Local Law Enforcement Agencies (CSSLEA) (Reaves, 2011). Even using a larger, more reliable denominator for Mississippi only caused its ranking to fall from 1st to 3rd in terms of its average rate of firearm assault on police. As such, we can be reasonably certain that Mississippi is one of the more dangerous states in which officers work.

neighboring Wyoming (0.38 per 1,000), and 2.5 times more likely to be assaulted with a firearm than officers just south of them in Utah (0.28 per 1,000).

[FIGURE 5 here]

Even in the northeast, which we have noted is comparatively safer for officers than the rest of the United States, we find notable variation across directly neighboring states, like New Jersey, New York, and Pennsylvania. Though geographically proximate, New Jersey's average rate (.11) and New York's average rate (.10) are *3 and 3.3 times smaller* than Pennsylvania's (.33). There appears to be even more variation when considering the states surrounding outliers like New Mexico, a state with an especially high average firearm assault rate between 2014 and 2019. New Mexico's rate (1.56) is *between 2.0 and 5.6 times higher* than its directly neighboring states: Oklahoma (.77), Colorado (.74), Arizona (.62), Texas (.54), and Utah (.28).

Discussion

In spite of the marked declines in line-of-duty deaths among police officers over the past 50 years (White et al., 2019), violence against police remains a problem that affects public perception, police practice, and policy in the United States (Moule, 2019; Nix et al., 2018; Sierra-Arévalo, 2016; Stoughton, 2016). And though scholars have done well to leverage available data to empirically assess violence against police, longstanding definitional and data quality issues continue to hamper the timely and precise estimation of the most lethal threat officers face: firearm assault. With the help of open-source data on fatal and non-fatal firearm assault on police officers gathered daily by the GVA, our analysis provides a fresh assessment of this longstanding public safety and public policy concern.

With regard to national trends in firearm assault, our findings highlight the necessity of including non-fatal firearm assault in discussions of danger in U.S. policing. As we have shown, the lion's share of firearm assaults on officers in the U.S. are non-fatal. Between 79 and 86 percent of a given year's firearm assaults on police do not result in a line of duty death. By extension, conclusions stemming from analyses that employ data on fatal firearm assaults alone will be derived from, on average, only 17 percent of total firearm assault cases. Indeed, given that the difference between a fatal and non-fatal firearm can be a matter of luck—weapon caliber, wound location, and total number of gunshot wounds all affect firearm assault lethality (Altheimer et al., 2019; Braga & Cook, 2018)—it is vital to recognize that sole focus on fatal firearm assault will inevitably and sorely underestimate the incidence of deadly force against police officers.

Additionally, our state-level analyses show marked variation across states and raise important questions about what underlying structural conditions might explain these patterns. We believe that GVA provides the means for researchers to revisit questions about violence against police with a more nuanced operationalization of the firearm assaults that drive patterns in felonious police deaths. For example, GVA data might be used to reexamine the relationship between violence against police and demographic or structural factors that include racial and ethnic composition, crime rates, poverty, or local political arrangements (Batton & Wilson, 2016; Kaminski, 2008; Kaminski & Marvell, 2002; Kaminski & Stucky, 2009; Kent, 2010).

Of particular interest is the relationship of state-level firearm ownership and firearm laws to firearm assault on police. Though past work indicates that states with stronger firearm laws have lower rates of firearm homicide, overall (Lee et al., 2017), and that lower rates of civilian firearm ownership are associated with lower rates of police homicide, specifically (Swedler et al., 2015), our results suggest heterogeneity in these factors is unlikely to fully explain state-level variation in rates of firearm assault on police. For example, while Arizona, New Mexico, Utah, and Colorado are all "shall issue" concealed carry permit states with above average levels of firearm ownership (CDC, 2019; Siegel et al., 2017), they vary markedly in their rates of firearm assault on police. Further insight might be gleaned by investigating whether firearm laws and firearm supply differentially affect rates of firearm assault on police depending on other features of states' legal and criminal justice systems. For example, easily accessible firearms might combine with punitive sentencing laws to increase the likelihood of a suspect using deadly force to avoid arrest and incarceration (e.g. Kovandzic et al., 2002).

GVA might also be used to sharpen and better integrate research on violence *by* police with that on violence directed *at* police (see Fridel et al., 2019). For example, Legewie's (2016) quasi-experimental analysis of NYPD Stop, Question, and Frisk (SQF) data finds that the murder of a police officer by a Black suspect is related to increased use of force against Black New Yorkers—no such effects were found for Whites or Hispanics. Using a regression-based approach, Bejan et al. (2018) find that, across the U.S., an increase in felonious police deaths is related to a same-day increase in police killings of minority individuals and a decrease in the killing of White individuals; an increase in minority civilian deaths was related to a decrease in police deaths, while an increase in White, non-Hispanic deaths was associated with an increase in police deaths. Neither of these studies speaks to *non-fatal* firearm assaults on police. Future work on the cyclical, potentially retaliatory nature of violence between police and public can be improved by incorporating the non-fatal firearm assaults captured in GVA. These data would allow better measurement of deadly force targeting both police and the public and, by providing a greater number of data points across states, might reveal localized variation in police-public violence that can explain the state-level variation in firearm assault we find in our descriptive analyses.

Besides state-level differences in the prevalence of firearm assault on police, GVA data might also be leveraged to investigate if and to what degree the lethality of firearm assaults varies based on technology, training, and policy. Though research shows that ballistic body armor significantly reduces the likelihood of an officer dying after being shot in the torso (Liu & Taylor, 2017), there is significant variation in the strength of agencies' body armor policies (e.g. whether the agency has a "mandatory wear" policy) (Liu & Taylor, 2017). Though researchers will have to contend with likely issues of small sample sizes driven by the relative rarity of firearm assault on police, the combination of GVA data with LEMAS data on body armor policies could be used to better understand how agency- and state-level variation in such policies affect the likelihood of officers dying by way of firearm assault. Additional factors to consider include whether agencies distribute tourniquets or other trauma care technology to officers, the amount of training that officers receive on the use of this equipment, and the distance of firearm assault incidents from a trauma care facility (Circo, 2019; Crandall et al., 2013).

The insights and promise of these data notwithstanding, care should be taken to not extrapolate too strongly from the slight, upward trend found in non-fatal firearm assaults recorded by GVA from 2014 to 2019. In particular, it is prudent to consider that 2014 marked a shift in public and political attention to policing following a string of highly-publicized police killings, including that of Michael Brown in Ferguson, MO, 12-year old Tamir Rice in Cleveland, OH, and Eric Garner in New York City, NY (Cobbina, 2019; Weitzer, 2015). It is possible that the lower rate of firearm assaults on police in 2014 may be an artifact of measurement error born of either (a) differential attention paid by news media to incidents of police victimization in 2014 relative to later years, or (b) a change in the likelihood that police organizations notify news media of incidents in which officers were shot. Bearing in mind that GVA only extends back to 2014, we cannot rule out the possibility that GVA records more incidents of violence against police precisely because of increased attention to policing and police violence. By the same token, police departments and their administrators may be increasingly likely to notify the news media of violence against their officers as a means to manage public perception or build public sympathy in a tense political climate (Chermak & Weiss, 2005; Surette, 2001).[16]

Despite these potential measurement issues, GVA provides decided benefits for studying firearm assault on police relative to data sources like LEOKA and NIBRS. Perhaps the clearest of these is the speed with which GVA is released to the public. In contrast to the years-long lags between the collection and dissemination of LEOKA and NIBRS data, GVA is updated on a daily basis. Additionally, each case is uploaded with source URLs that allow for independent verification of each case and which can be mined for other information such as the time of day a shooting took place, the type of call to which officers were responding, what kind of weapon(s) were used, whether bystanders were also wounded or killed, etc. Finally, GVA gathers data from across the United States and uses sources from independent news organizations instead of police-generated reports, sidestepping some of the reporting issues long known to affect LEOKA and NIBRS.

These notable benefits aside, we also emphasize that open-source data like GVA is not a cure-all for the data quality issues of policing data writ large, and there are considerations when using GVA that merit careful attention. Data sources on violence against police (or any police-related data) cannot in and of themselves guarantee better estimates of social phenomena. This is because no matter how good our estimates of the frequency of a given phenomenon, any estimate of the *rate* of that phenomenon is dependent on the accuracy of the denominator—in the case of firearm assault on police, this means the number of officers working in a particular geographic area (see Tregle et al., 2019, pp. 2–3). Case in point, Mississippi's firearm assault rate (2.29 per 1000 officers) must be considered alongside the fact that officer population estimates for Mississippi are highly unreliable. In 2018, for example, scarcely more than 75 percent of the agencies in Mississippi's metropolitan statistical areas reported data to the FBI; the percentage of agencies reporting drops to 37.9 percent when considering cities in non-metropolitan areas and bottoms out at 20.7 percent in nonmetropolitan counties. Similar but less grave concerns exist for Indiana and West Virginia (see Appendix C). All told, no measure of firearm assault on police

[16] Potential measurement bias for 2014 should also be considered alongside the fact that estimated trends for a limited time frame are sensitive to the inclusion or exclusion of individual years. If, for example, 2014 is dropped and a new linear trend line is imposed on estimates for non-fatal firearm assaults from 2015 to 2019, the trend is negative (B= -.573), indicating that, on average, 3.9 *fewer* officers per year were non-fatally assaulted with a firearm during this period (see Appendix E.)

officers, regardless of its precision and accuracy, can make up for unreliability in officer population estimates when trying to calculate a state- or national-level rate.

Turning to GVA itself, two issues stand out. First, though the media-based reports collected by GVA circumvent some of the reporting shortcomings of existing police-generated data, it remains an open question as to the completeness of these reports and whether there is significant variation across states in reporting of violence against police. Relatedly, just as it is unclear how many cases present in other datasets are not present in GVA, it is unclear how many cases recorded in GVA are not accounted for in existing datasets—a problem compounded by GVA's relative recency and the lag time associated with the release of LEOKA and NIBRS data. Future research would do well to assess the differences between GVA and existing data with an eye towards how much of these differences can be explained by operational definitions of violence against police or reporting error.

Second, it is imperative for researchers to recognize the irregularity of these data and to carefully and clearly operationalize the phenomena they hope to measure when cleaning and coding them. Our own data cleaning and coding process resulted in 510 cases—nearly 26 percent of the total dataset provided by GVA—being excluded from our analytic sample based on our inclusion criteria. Additionally, the scope of GVA's definition of gun violence also demands specificity in researchers' operational definitions. Our definition of fatal and non-fatal firearm assaults on police officers allows for precise exclusion of a variety of other instances in which officers were shot, including on-duty firearm suicides, cases of officers accidentally shooting themselves, and "blue on blue" cases in which an officer was accidentally shot by another officer. Importantly, our operational definition of firearm assault was informed by existing research on the dangers that officers emphasize on patrol and which drive felonious line of duty deaths, as well as the authors' domain-specific expertise on the untidy, often unclear circumstances in which police work takes place.

Even with our efforts to craft a precise set of inclusion criteria, some cases forced imperfect choices. For example, Sergeant Ron Helus of the Ventura County Sheriff's Department was shot six times while responding to an active shooter situation. Though five of those shots were fired by the suspect, a coroner's report found that the sixth gunshot wound was caused by a rifle round fired by another officer. This round struck Helus in the heart and was ruled to be the cause of the deputy's death (Berman, 2018). We elected to retain this case as an example of a *non-fatal* shooting by a suspect because, even though we could confirm the deputy was shot by a suspect, we could not conclusively determine if the deputy would have died had another officer not shot him through the heart. Other researchers coding the same case might very well have made a different coding decision. We present this example not to argue that our definitional choices are perfect but to lay bare and emphasize the ambiguities intrinsic to reducing complex social realities into even seemingly clear-cut variables like fatal/non-fatal or suspect-/officer-inflicted.

Such considerations are especially important in light of the growing use of media- or crowd-sourced data in research on police. At the end of 2019, two high-profile articles—one employing data from *Fatal Encounters,* the other from *Mapping Police Violence*—came under scrutiny for coding errors and debatable coding decisions. Besides clear errors wherein a suspect shot by police who was coded as "unarmed" was verifiably armed, cases in which a suspect was in possession of a toy/replica firearm or in which a suspect crashed and died while fleeing from officers were coded the same as cases in which officers shot a suspect armed with a real firearm. Once coding errors were amended or cases like those involving toy guns or non-traditional weapons (e.g. screwdriver) were coded as "armed," the reported effect of exposure to police shooting unarmed Black suspects on the birthweight of Black infants or on the mental health of Black Americans was reduced to statistical non-significance (AAAS, 2019; Lozada & Nix, 2019; Nix & Lozada, 2019). We chose to manually check each case in the GVA dataset to avoid such issues and, in our opinion, such steps are necessary when using any media- or crowd-sourced data. Thankfully, the richness of the GVA data and the inclusion of online sources with each case allows for researchers to verify cases independently and, perhaps more importantly, to finely tune and apply their inclusion criteria to enhance the precision of their measurements.

Even with careful verification of cases, of course, any analysis built on GVA will be limited to estimates of *gun* violence. We maintain that studying gun violence against police is critically important given its lethality and its centrality to police training, culture, and operations (Carlson, 2019; Sierra-Arévalo, 2016, 2019). However, it bears repeating that studies of gun assaults cannot speak to the far more common cases of simple assault on police (FBI, 2019b). Even studies that capture the larger universe of assaults on police do not accurately measure the more general "resistance" to police examined in prior research (Terrill, 2003). We suggest that these phenomena can both be considered examples of what might be more broadly conceptualized as police-public conflict. Though there are significant challenges to integrating data on violent assault with that on disrespect or non-violent resistance (especially for multiple agencies), it would behoove researchers to consider how to integrate measures of police-public conflict that can range from lethal violence to low-level disrespect. Such integration would provide valuable insight into not only how lower-level police-public conflict predicts serious violence against police but also how the range of police-public conflict shapes officers' perceptions of the public and their use of coercive force (Nix et al., 2018, 2020).

With these benefits, limitations, and cautions regarding GVA in mind, we conclude with some concrete suggestions as to how current data collection policies and practices might be amended to improve data used to assess the danger of police work. The longstanding calls for better data on force used *by* police and the decades of largely insufficient attempts to remedy the lack of a national database on police use-of-force are instructive in this regard. First, efforts to understand and address issues of violence by and against police are characterized by a common problem: data that prevents accurate measurement of the phenomenon at hand (Shane, 2018, pp. 128–129). As we have already described, and as discussed at length by others (Hickman & Poore, 2016; Klinger et al., 2016; Nix et al., 2017), existing data are insufficient for making precise, reliable estimates of police use-of-force in the United States. Data on violence against police are drawn from the same or similarly flawed data sources (e.g. UCR, NIBRS), and officer employment data required to quantify the scope of violence by and against police are often drawn from these same sources. Even officer population statistics drawn from the Census of State and Local Law Enforcement Agencies (CSLLEA) or Law Enforcement Management and Administrative Statistics (LEMAS) data are of limited utility for timely analyses given that the most recent

CSLLEA is over a decade old and LEMAS, which is administered every 3 years, takes between 2 and 3 years to be released once data are collected (Reaves, 2011; Banks et al., 2016). [17]

We believe that the parallel problems that plague data on police use-of-force and violence against police can be addressed through common mechanisms, leveraging (and expanding) efforts to improve shortcomings in the former to also improve the latter. Recognizing that the federal government has historically struggled to compile timely and reliable national-level criminal justice data (Alpert, 2016; Alpert, 1948; Zimring, 2017), we see more promise in efforts to compile such data at the state level. These state-level data could then be aggregated to generate regional and national estimates. To date, six states—California, Colorado, Connecticut, North Carolina, Oregon, and Texas—have begun collecting and publishing data on police use-of-force (National Conference of State Legislatures, 2018; Shjarback, 2019).

Such efforts, though discussed primarily as a means to gather data on violence by police, could and should be used to gather higher quality data on violence directed at police. California stands apart as a state that is already moving their data collection efforts in this direction. Currently, the California Attorney General compiles state data on use of force incidents resulting in serious injury or death, including fatal and non-fatal firearm assault as well as non-injurious firearm discharges for its annual URSUS report. Importantly, these data capture firearm violence that includes incidents wherein officers shoot or shoot at civilians *and* incidents in which an officer is shot or shot at by a civilian. Thus, California's data on deadly force captures not only fatal shootings but also non-fatal shootings and non-injurious shootings, both of which far outpace fatalities and which are necessary to provide holistic understanding of the danger associated with policing in the state (Fyfe, 1988; Nix et al., 2017).

In 2018, URSUS recorded 630 incidents, detailing not only demographic information on officers and members of the public involved, but also the circumstances that preceded each incident, the number of officers and agencies present, level of civilian resistance, indicators of civilian mental status, and more. That the most populous state in the U.S. can compile such rich data on hundreds of cases every year suggests that similar efforts can and should be replicated by all states. And though the rollout of new data systems will always come with implementation challenges, the advent of open-source, cloud-based options of the kind that undergird the URSUS system can drastically reduce development costs and sidestep the need for individual agencies to build out their own data collection infrastructure (Williams, 2016). The more states that take the necessary steps to improve their policing data, the better positioned police departments, researchers, policy makers, and the public will be to measure, understand, and address the deeply intertwined issues of violence by and against police.

[17] Garner et al. (2018) is one exception that uses LEMAS and UCR data to provide a national estimate of police force for 2012. However, because their analytic sample is composed of only 1,646 agencies, their national estimate requires significant imputation that cannot provide reliable agency- or state-level estimates of force. What's more, such techniques do not address the fact that timely estimates of police use-of-force with these data are impossible given that LEMAS is released every three years.

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Figure 1. Monthly Firearm Assaults on U.S. Police, 2014-2019

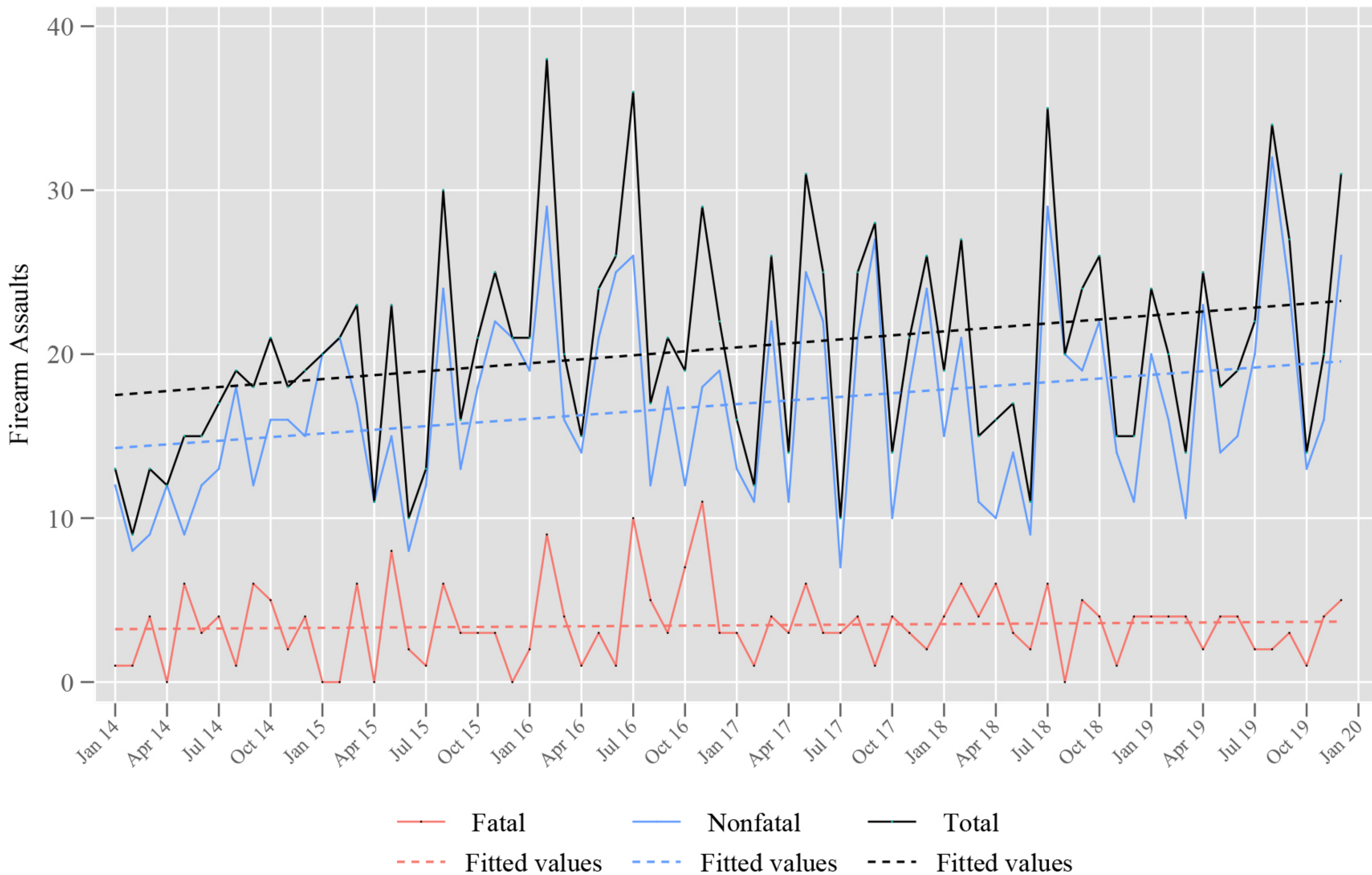


Figure 2. National Rate of Firearm Assault on Police, 2014-2019

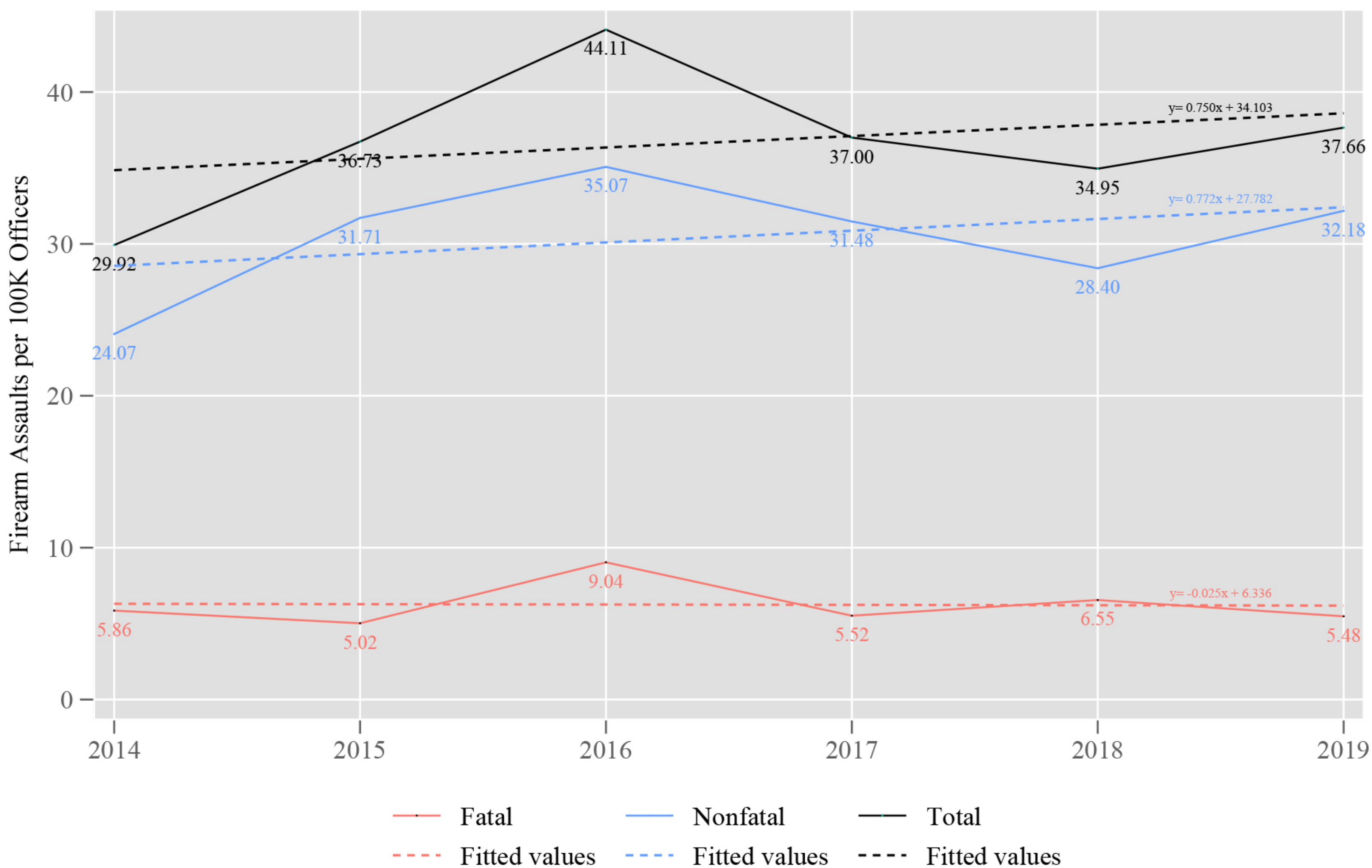


Figure 3. Total Firearm Assaults on Police by State, 2014-2019

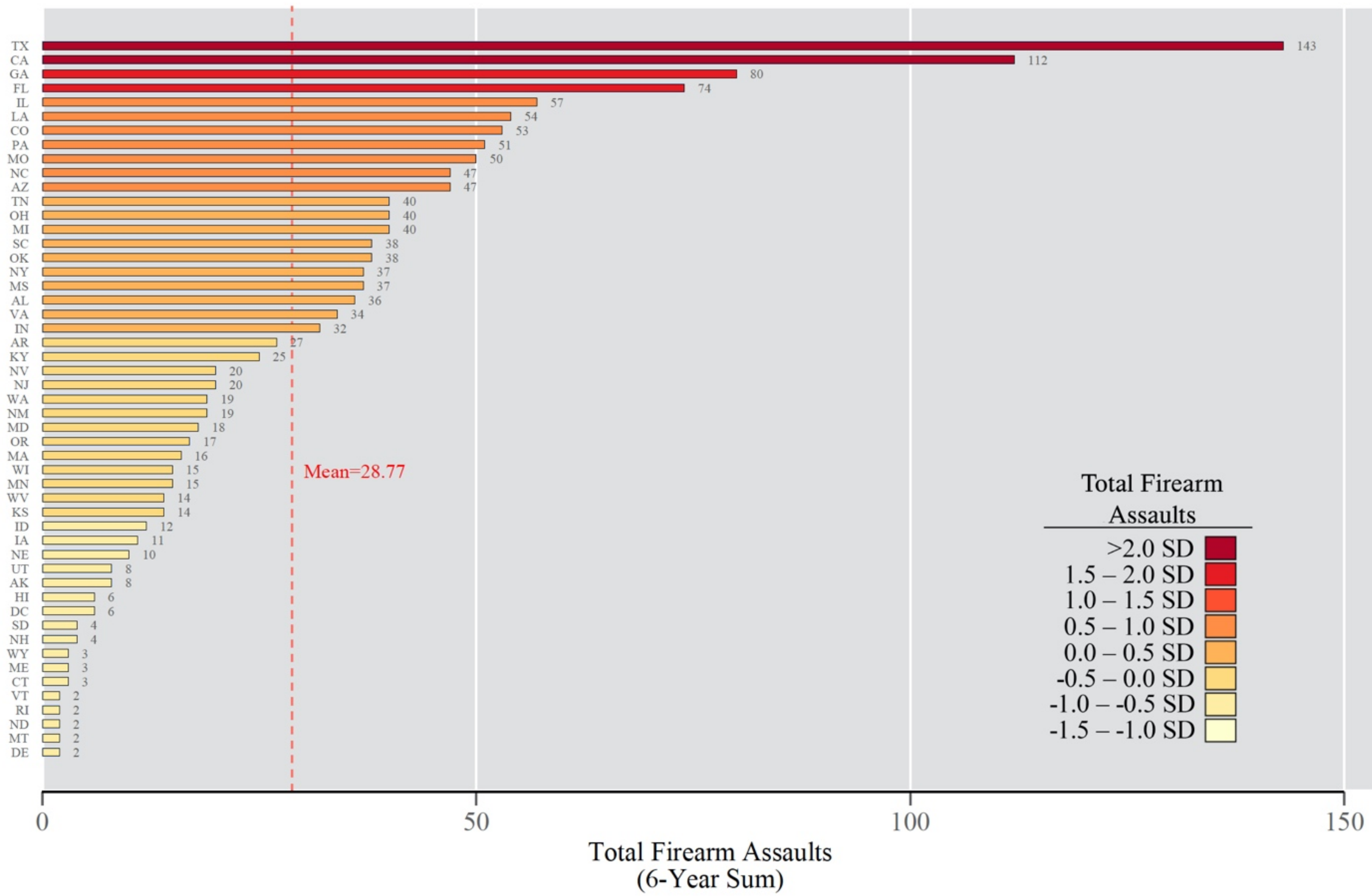


Figure 4. 6-Year Average Rate of Firearm Assault on Police by State, 2014-2019

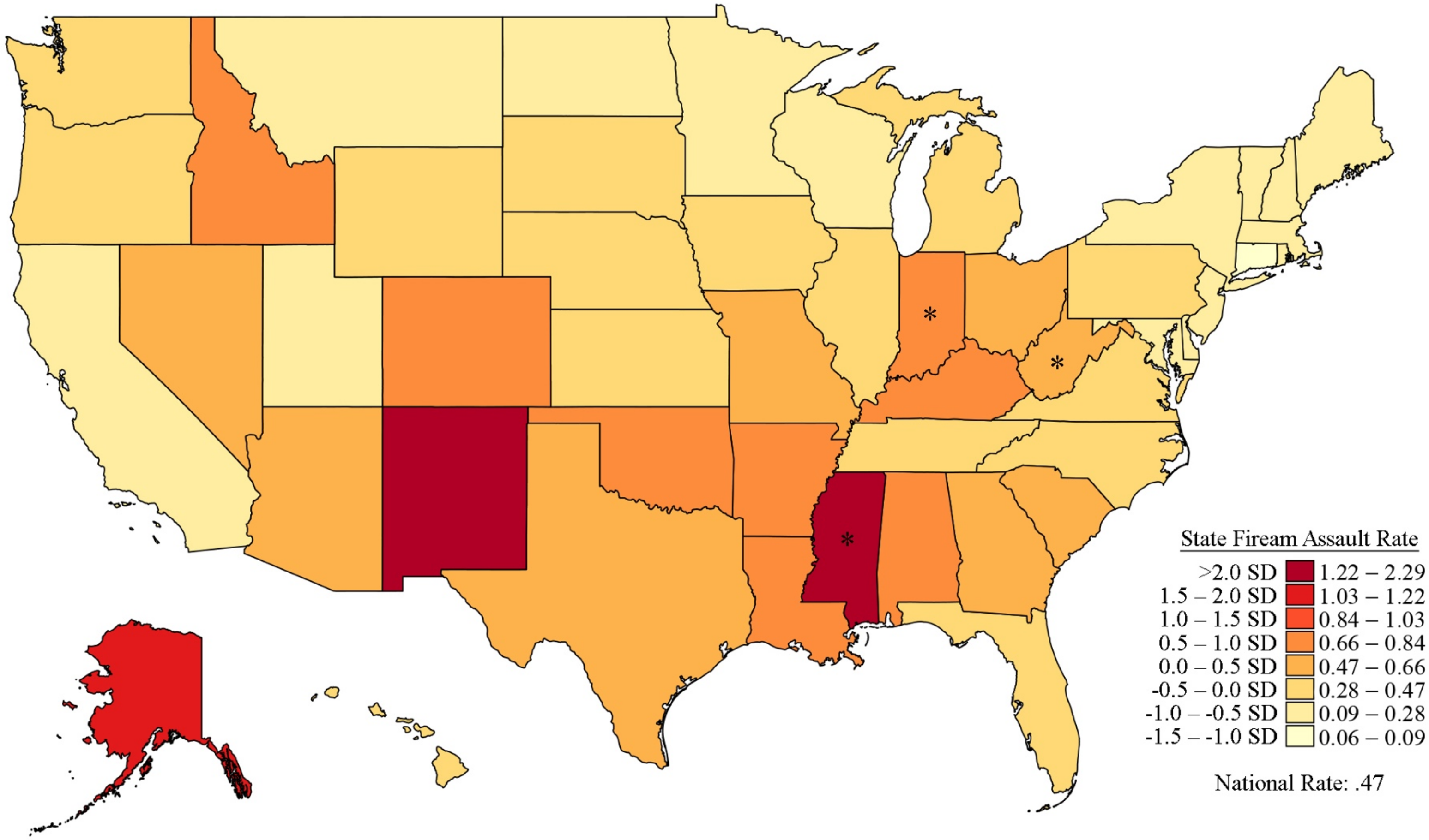
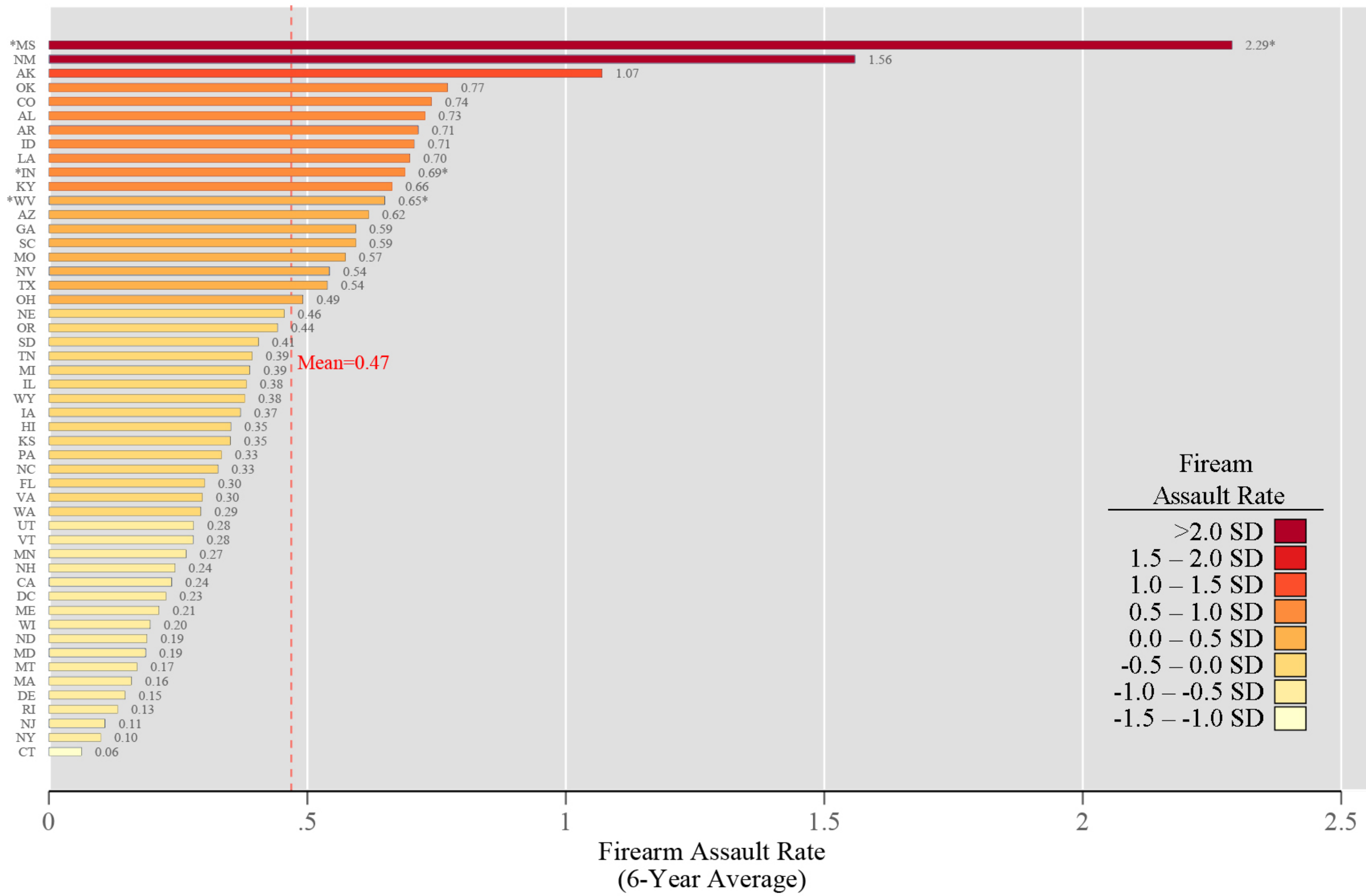


Figure 5. 6-Year Average Rate of Firearm Assault on Police by State, 2014-2019



Appendix A: Frequency Table of Reasons for Case Exclusion

Reason for Case Exclusion	Cases
Not Active Duty State or Local Law Enforcement	129
Federal Employees	60
Not a Law Enforcement Officer	24
Retired or Off-Duty	102
Self-Inflicted	188
Suicide	45
Blue on Blue	101
Officer Lied or Committed Crime	7
Duplicate Case	18
Unverifiable Case	6
Unclear Circumstances	8

Note: Case total does not sum to number of excluded cases (n= 510) because cases could be excluded for multiple reasons. For example, 22 of the 60 federal employees would have been excluded for at least one of the other reasons listed above.

Appendix B: Police Employee Data, 2013 – 2018 and Two Imputed Estimates for 2019.

State	2013	2014	2015	2016	2017	2018[a]	2019[b]	2019[c]
Alabama	9492	7698	8801	6978	5235	10661	11926.8	7807.5
Alaska	1312	1258	1227.4*	1219	1247	1286	1357.7	1243.8
Arizona	11955	12663	12807	12474	12862	12756	12497.4	13013.1
Arkansas	5893	5917	5860	5943	5976	6743	7185.5	6506.3
California	76912	77190	77402	77849	78740	79141	80051.7	79496.5
Colorado	11981	10478	11835	11972	11402	12512	13145.9	12253.1
Connecticut	8517	8634	7918	7944	7849	7792	7782.6	7513.6
Delaware	2339	2194	2239	2283	2325	2315	2424.7	2314.2
District of Columbia	4579	4535	4264	4174	4228	4393	4558.9	4168.1
Florida	42346	36054	36287	34436	44593	42880	52508.1	42076.3
Georgia	21498	19881	19690	20223	24044	27250	32969.7	26275.9
Hawaii	2886	3006	2939	2995	2951	2844	2731.1	2904.9
Idaho	2707	2725	2766	2770	2851	2894	2964.7	2917.2
Illinois	9420	24358	13295	23897	25215	26744	27547.3	30467.5
Indiana	7054	8763	8169	5539	7794	9349	10142.8	8371.8
Iowa	5035	5003	5183	5241	5252	4716	4460.0	4992.7
Kansas	6174	6804	5864	6559	6874	6575	6828.5	6766.0
Kentucky	7092	7101	6321	3709	7659	7386	9516.2	6597.9
Louisiana	8228	10037	9118	15274	15418	13960	15087.9	17101.7
Maine	2243	2285	2280	2311	2322	2349	2363.7	2365.5
Maryland	17312	15893	17379	17158	15535	15410	14373.0	15367.3
Massachusetts	16293	16644	17033	16686	16511	16717	16287.9	16784.7
Michigan	16909	17028	16996	17142	17081	17229	17259.5	17254.7
Minnesota	8743	8879	9211	9413	9689	9927	10213.2	10165.5
Mississippi	2903	2524	2415	2506	2884	2700	3158.1	2670.9
Missouri	14400	14458	14347	14572	14688	14428	14474.0	14587.7
Montana	1684	1435	1853	1612	1917	1839	1988.5	1921.3
Nebraska	3497	3644	3722	3523	3345	3797	3711.9	3628.4
Nevada	5359	5501	6299	6341	6789	6273	6159.6	6941.3
New Hampshire	2612	2583	2632	2699	2764	2838	2958.5	2862.0
New Jersey	24460	29668	30272	30811	31341	31817	29969.8	33962.5
New Mexico	353	3341	977	3176	2933	2710	2262.5	3524.3
New York	58528	58370	60821	62484	62433	62327	62487.2	64111.9
North Carolina	23258	23980	23775	24006	24070	24004	23794.6	24271.9
North Dakota	1471	1593	1698	1761	1749	1776	1723.1	1880.3
Ohio	13147	9616	13256	12765	13460	14976	17131.1	14888.6
Oklahoma	7841	6897	7517	8561	8828	8893	9904.7	9299.2

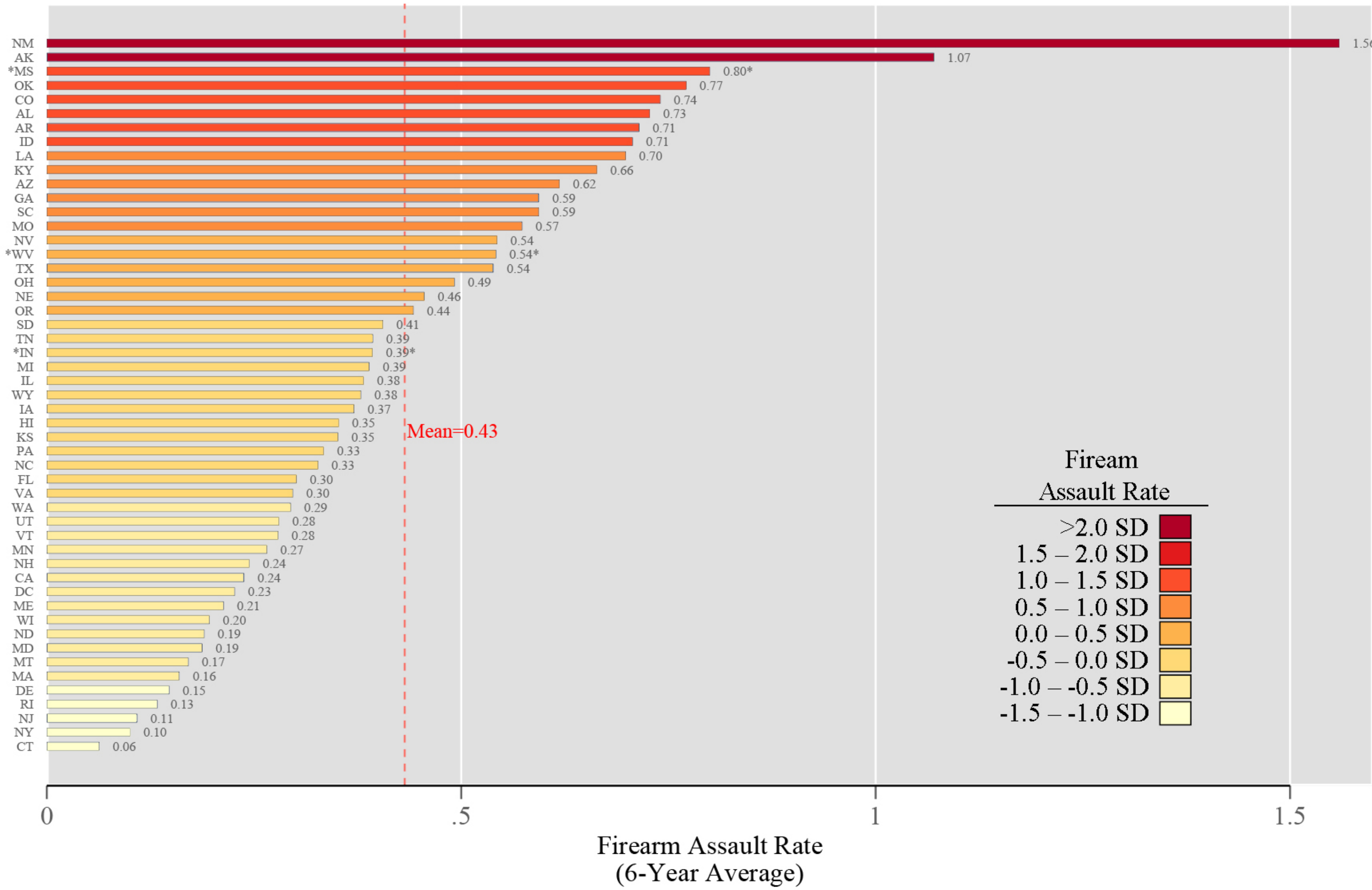
State	2013	2014	2015	2016	2017	2018[a]	2019[b]	2019[c]
Oregon	5959	6356	6538	6237	6767	6553	6514.7	6791.9
Pennsylvania	25278	24611	25525	25191	25839	25505	25873.5	25773.3
Rhode Island	2434	2497	2441	2469	2504	2494	2507.9	2508.1
South Carolina	11521	9552	11470	11792	8380	11553	11139.6	10407.9
South Dakota	1529	1669	1501	1659	1780	1771	1890.1	1821.6
Tennessee	16595	16774	16613	16766	16736	17363	17656.4	17195.7
Texas	46059	38512	45451	46521	43699	44857	45992.1	45245.3
Utah	3236	4879	4865	4902	4988	5106	4429.9	5634.1
Vermont	1158	1194	1149	1155	1196	1392	1516.2	1325.5
Virginia	18756	18858	18769	18953	19036	19258	19475.8	19261.1
Washington	10341	10311	10341	10507	10718	10820	11115.2	10884.5
West Virginia	3534	3671.6*	3806	3679	3669	3592	3399.0	3677.8
Wisconsin	12504	12776	12772	12846	12870	12704	12534.6	12880.9
Wyoming	1605	1322	1294	1253	1243	1490	1736.2	1282.5

[a] Note: we also tested the fit of a model that assumed no year-over year change between 2018 and 2019 (RMSE = 2015.18). Though not displayed here, its imputed values for 2019 equal those listed in the "2018" column for each state.
[b] Imputed values calculated using the coefficients for *year* and $year^2$ from a quadratic regression model used to estimate officer population at the state level (RMSE = 948.78). The values in this column are the values selected as our final imputation values for 2019.
[c] Imputed values calculated using the coefficient for *year* from a linear regression model used to estimate officer population at the state level (RMSE = 1105.73).
* Entries for WV (2014) and AK (2015) reflect imputed estimates derived from a quadratic regression equation regressing officer population on *year* and $year^2$.

Appendix C: Percentage of Agencies in Each State that Submitted UCR Data in 2018

State	Metropolitan statistical areas	Cities outside metropolitan areas	Nonmetropolitan counties
Alabama	94.3	93.1	91.9
Alaska	100.0	97.1	100.0
Arizona	99.3	96.3	100.0
Arkansas	99.7	91.1	84.9
California	99.9	100.0	100.0
Colorado	95.1	94.9	91.5
Connecticut	100.0	100.0	100.0
Delaware	100.0	n/a	n/a
District of Columbia	100.0	n/a	n/a
Florida	99.9	95.0	100.0
Georgia	96.6	88.7	93.5
Hawaii	100.0	n/a	100.0
Idaho	100.0	95.7	98.3
Illinois	92.1	83.2	89.7
Indiana	84.4	**59.0**	**51.4**
Iowa	90.7	100.0	97.0
Kansas	83.8	95.4	96.6
Kentucky	99.7	97.5	100.0
Louisiana	97.2	80.8	96.7
Maine	100.0	100.0	100.0
Maryland	100.0	100.0	100.0
Massachusetts	97.5	100.0	98.8
Michigan	98.8	98.0	99.2
Minnesota	98.6	99.7	100.0
Mississippi	**75.9**	**37.9**	**20.7**
Missouri	99.8	98.5	100.0
Montana	100.0	100.0	99.3
Nebraska	99.8	92.9	84.6
Nevada	100.0	100.0	100.0
New Hampshire	99.2	93.6	93.4
New Jersey	100.0	n/a	n/a
New Mexico	92.5	96.0	97.4
New York	99.7	97.4	100.0
North Carolina	89.1	86.2	95.3
North Dakota	100.0	99.8	100.0
Ohio	90.1	80.9	86.0
Oklahoma	99.9	99.3	97.9
Oregon	99.3	95.0	84.3
Pennsylvania	99.7	98.9	100.0
Rhode Island	100.0	n/a	n/a
South Carolina	98.8	97.0	96.3
South Dakota	99.1	96.0	82.8
Tennessee	99.9	100.0	100.0
Texas	98.2	93.4	98.4
Utah	99.4	89.6	90.9
Vermont	100.0	100.0	100.0
Virginia	99.9	97.8	100.0
Washington	98.6	92.8	100.0
West Virginia	81.9	**54.3**	86.7
Wisconsin	98.2	99.5	100.0
Wyoming	81.3	91.9	89.1

Appendix D: 6-Year Average Rate of Firearm Assault on Police by State, 2014-2019.



Appendix E. National Rate of Firearm Assault on Police, 2015-2019