Testimony in support of the STEP Act by Scott Schneider, Chair, Progressive Neighbors Steering Committee

My name is Scott Schneider. I have been a resident of Montgomery County for almost 40 years. I am a graduate of the Citizen's Police Academy. I was also a member of the County Executive's Task Force of Reimagining Public Safety. I am testifying today in support of the STEP act (Bill 12-23) on behalf of Progressive Neighbors, a local political group advocating for the election of more progressive local legislators.

Stops

Several years ago I testified before the Council to support eliminating "pretext stops" by police, where police use any minor infraction as an excuse to stop and often search a vehicle. This was a major recommendation of the Reimagining Task Force. I pointed out that driving to the Council meeting every one of the council members likely violated some section of the traffic code and could have been pulled over. The traffic code is so broad and gives police officers so much discretion that it invariably leaves wide latitude for racially disparate policing. And to no one's surprise what is exactly what the data tell us. Black and Latinx residents are pulled over at a disproportionate rate. A comprehensive analysis by the NY Times in 2021 showed how police officers often pull drivers over for minor offenses (disproportionately Black and Latinx men) and the situations can spiral out of control resulting in a fatality(Why So Many Police Traffic Stops Turn Deadly, Kirkpatrick, et. al, NYT Nov. 30, 2021) https://www.nytimes.com/2021/10/31/us/police-traffic-stops-killings.html).

Stops that are due to serious safety violations, like speeding and reckless driving, of course need to continue. But stopping someone because they forgot to use their turn signal or made a rolling stop at a stop sign is unnecessary and a waste of police resources. Many of these low level offenses (like expired registrations) could be handled by photo-enforcement. The County should also sponsor clinics (as DC does) for people to get broken taillights fixed. Such low level traffic enforcement undermines trust and confidence in the police and can negatively impact residents by decreasing the likelihood of cooperation with law enforcement and impacting their mental and emotional health. (Cadoff et. al. Oct. 2020, Misdemeanor Enforcement Across Seven U.S. Jurisdictions, John Jay College of Criminal Justice).

Searches

Officers will argue that stops are valuable in that they sometimes will result in searches that find illegal weapons or dangerous drugs like fentanyl. As I testified previously, only 2.6% of all stops result in searches, but that figure is higher for Black males (4.9%) and Latino males (4.1%). About half (45%) of searches were for probable cause. When people are stopped it should be required that the police inform people of their rights to refuse a search ("You have the right to refuse a search and if you do it won't be held against you.") similar to a Miranda warning. The Reimagining Public Safety Task Force (2021) recommended moving much of the traffic enforcement to automated enforcement and a pilot program for the elimination of "pretext stops" for minor offenses.

Danger

Officers are trained to believe that traffic stops are inherently dangerous for them and to anticipate the need for force or danger. But the data do not support this view. A recent study estimated that an officer fatality occurred at a rate of 1 fatality per 6.5 million stops. Assaults on officers during traffic stops resulting in serious injuries only occurred once in every 361,111 stops (Jordan Woods, 117 Mich. Law Review 635, 2019). But stops are dangerous for drivers. Each year nationwide, police officers kill about 1,000 civilians. About 100 police officers die nationwide each year on the job and about half of those die in traffic accidents.

Staffing

The Montgomery County Police Department is understaffed. This bill would help by reducing the considerable time spent doing traffic stops for minor violations. They can then spend their limited time on more serious public safety issues.

Thank you for your time and I urge you to support this bill to help eliminate racial disparities in policing and creating a more effective police force.

Why Many Police Traffic Stops Turn Deadly

Officers, trained to presume danger, have reacted with outsize aggression. For hundreds of unarmed drivers, the consequences have been fatal.

By David D. Kirkpatrick, Steve Eder, Kim Barker and Julie Tate
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https://www.nytimes.com/2021/10/31/us/police-traffic-stops-killings.html

"Open the door now, you are going to get shot!" an officer in Rock Falls, Ill., shouted at Nathaniel Edwards after a car chase.

"Hands out the window now or you will be shot!" yelled a patrolman in Bakersfield, Calif., as Marvin Urbina wrestled with inflated airbags after a pursuit ended in a crash.

"I am going to shoot you — what part of that don't you understand?" threatened an officer in Little Rock, Ark., adding a profanity, as she tried to pry James Hartsfield from his car.

The police officers who issued those warnings had stopped the motorists for common offenses: swerving across double yellow lines, speeding recklessly, carrying an open beer bottle. None of the men were armed. Yet within moments of pulling them over, officers fatally shot all three.

The deaths are among a series of seemingly avoidable killings across the United States. Over the past five years, a New York Times investigation found, police officers have killed more than 400 drivers or passengers who were not wielding a gun or a knife, or under pursuit for a violent crime — a rate of more than one a week.

Most of the officers did so with impunity. Only five have been convicted of crimes in those killings, according to a review of the publicly reported cases. Yet local governments paid at least $125 million to resolve about 40 wrongful-death lawsuits and other claims. Many stops began with common traffic violations like broken taillights or running a red light; relative to the population, Black drivers were overrepresented among those killed.

The recurrence of such cases and the rarity of convictions both follow from an overstatement, ingrained in court precedents and police culture, of the danger that vehicle stops pose to officers. Claiming a sense of mortal peril — whether genuine in the moment or only asserted later — has often shielded officers from accountability for using deadly force.

"We get into what I would call anticipatory killings," said Sim Gill, the district attorney for Salt Lake County, Utah. "We can't give carte blanche to that."

In case after case, officers said they had feared for their lives. And in case after case, prosecutors declared the killings of unarmed motorists legally justifiable. But The Times reviewed video and audio recordings, prosecutor statements and court documents, finding patterns of questionable police conduct that went beyond recent high-profile deaths of unarmed drivers. Evidence often contradicted the accounts of law enforcement officers.

Dozens of encounters appeared to turn on what criminologists describe as officer-created jeopardy: Officers regularly — and unnecessarily — placed themselves in danger by standing in front of fleeing vehicles or reaching inside car windows, then fired their weapons in what they later said was self-defense. Frequently, officers also appeared to exaggerate the threat.

In many cases, local police officers, state troopers or sheriff's deputies responded with outsize aggression to disrespect or disobedience — a driver talking back, revving an engine or refusing to get out of a car, what officers sometimes call "contempt of cop."

In dashboard- and body-camera footage, officers could be seen shooting at cars driving away, or threatening deadly force in their first words to motorists, or surrounding sleeping drivers with a ring of gun barrels — then shooting them when, startled awake, they tried to take off. More than three-quarters of the unarmed motorists were killed while attempting to flee.

"We have got to take him out," an Oklahoma state trooper declared over the radio in 2019 to patrolmen chasing a man in McAlestersuspected of shoplifting a bottle of vodka. The officers used their cars to force his S.U.V. from the road, opened a door as it rolled slowly past and shot from both sides, killing the driver, dashcam footage shows.

A Tennessee sheriff ordered his deputies to fire at a motorist with a suspended license in 2017: "Don't ram him, shoot him!" he later recounted saying, according to a body-camera recording. Knocking the man off the highway might "tear my cars up!"

Struggling to subdue a driver a few months later, a patrolman in Moundridge, Kan., warned that the man might be reaching for a police sidearm; an officer shot him, another struck his head with the butt of a shotgun and a third pummeled his body with a baton — killing him though he never touched a gun, video records show.And last year a body camera recorded an officer in Las Cruces, N.M., warning a motorist that he would "choke you out, bro," then pinning him in a headlock. "A good little scrap," the officer called it, before realizing the man had died.

Some families of the drivers said that their relatives were not blameless. "I don't have my head buried in the sand," said Deborah Lilly, whose 29-year-old son, Tyler Hays, had drugs in his car and tried to run away when he was pulled over for tinted windows last year by a sheriff's deputy in Hamilton County, Tenn. "I am just saying he did not deserve to get shot in the back." (Over the next three months, the deputy shot at two other unarmed drivers, wounding one.)

Almost all of the officers involved in these cases declined to comment or could not be reached. Advocates for the police argue that the dangers of stopping cars require readiness to use deadly force. "I have watched enough videos of an officer who is not on edge enough and his dashcam films his own death," said Larry James, general counsel of the National Fraternal Order of Police. "What are you going to do? Are you going to be indicted, or are you going to be buried?"

Traffic stops are by far the most common police encounters with civilians, and officers have reason to be wary in their approach: They don't know who is inside a car or whether there are weapons. Ten officers have been killed this year in such interactions, including a Chicago officer who was shot in August by a passenger during a traffic stop for an expired registration.

But some police chiefs and criminologists said that alarmist training about vehicle stops has made officers too quick to shoot at times, resulting in needless killings. Academies and commanding officers often rely on misleading statistics, gory cop-killing videos and simulated worst-case scenarios to instill hypervigilance. Many officers are trained to place a hand on the trunk of the car as they approach, to leave fingerprints as evidence if ambushed by the driver.

"All you've heard are horror stories about what could happen," said Sarah Mooney, assistant police chief in West Palm Beach. "It is very difficult to try to train that out of somebody."

The overemphasis on danger has fostered tolerance for police misconduct at vehicle stops, some argue.

“Prosecutors and courts give more leeway to officers’ decisions to use force at vehicle stops, as a result of the exaggerated concern about the potential for officers getting hurt,” said Michael Gennaco, a consultant to police departments on officer accountability and a former Justice Department prosecutor. “Officers would likely kill fewer drivers if there were deterrence.”

‘The Most Dangerous Thing’

Three sheriff’s deputies surrounded a beat-up Mercedes with a broken taillight in Clark County, Wash., in February. The tools strewn across the passenger seat worried them immediately, they later told investigators.

“That right there can hurt someone,” said Deputy Holly Troupe.

The driver’s retorts set off more alarms. “You need to chill out!” she recalled him parroting back to her.

To help force him out of the car, Deputy Sean Boyle punched the driver in the nose. Deputy Troupe grabbed him below the jaw in what she called “pain compliance.” But the driver, Jenoah Donald, a 30-year-old mechanic who had autism and struggled with drug addiction, started the car with one hand and clutched Deputy Boyle’s ballistic vest with the other, the officer later said.

Deputy Boyle, though he had 70 pounds on the driver, told investigators he had feared he might be stuck half-inside a moving car: “I was convinced, ‘This is how you are going to die,’” he later told investigators. So he shot Mr. Donald in the head.

Prosecutors questioned whether the stop would have ended differently if the officers had explained to the driver why they were ordering him to leave the car. But Deputy Boyle, with two decades on the job, had fired “in good faith,” the prosecutors concluded.

“I know from the academy that they tell you traffic stops and D.V.s” — domestic violence cases — “are the most dangerous thing we’ll do,” Deputy Troupe, a rookie, told investigators. “I thought, ‘This is why they tell us that.’”

Some officers involved in fatalities at vehicle stops cite their training, which for decades has stressed the perils of those interactions.

In many departments, police academy lessons and daily briefings include a steady diet of body-worn camera videos that depict easygoing officers being gunned down by drivers who whipped out overlooked firearms.

Seemingly every officer in America has watched the 1998 dashcam footage of Deputy Kyle Dinkheller’s murder on the Georgia roadside where he pulled over a veteran with a semiautomatic rifle in his pickup. Roll call briefings often feature fresher reminders, like

the images of an officer shot in March outside a Nashville store by a driver who kept a handgun in her purse.

Trainers and tactical guides typically emphasize that vehicle stops account for more killings of officers than almost any other type of interaction.

Of the roughly 280 officers killed on duty since late 2016, about 60 died — mostly by gunfire — at the hands of motorists who had been pulled over, a Times analysis showed. (About 170 other officers died in accidents on the job.) But the assertions about the heightened danger ignore the context: Vehicle stops far outnumber every other kind of police dealings with civilians.

In fact, because the police pull over so many cars and trucks — tens of millions each year — an officer's chances of being killed at any vehicle stop are less than 1 in 3.6 million, excluding accidents, two studies have shown. At stops for common traffic infractions, the odds are as low as 1 in 6.5 million, according to a 2019 study by Jordan Blair Woods, a law professor at the University of Arkansas.

"The risk is statistically negligible, but nonetheless it is existentially amplified," said Mr. Gill, the Salt Lake County district attorney and an outspoken proponent of increased police accountability.

State laws generally prohibit police officers from using lethal force unless they reasonably believe it necessary to prevent imminent death or serious injury. Under pressure from street protests over the 2014 killing of Michael Brown, an unarmed Black teenager in Ferguson, Mo., and the more recent Black Lives Matter marches, many police departments have made de-escalation their watchword. They often advise officers to defuse conflict with motorists, for example by listening attentively instead of just barking orders.

"The last thing I need to try to do is exert my authority, like 'You're going to do what I tell you to do because I said so,'" said Jon Blum, a former police officer who now writes training materials for police agencies and the International Association of Chiefs of Police. "What the officer has to do is sell the person."

Departments have increasingly instructed officers to let suspected lawbreakers drive away and find them later, avoiding the risks of potential confrontation or a high-speed pursuit. "You have the guy's car license plate and you know where he lives," said Scott Bieber, the chief of police in Walla Walla, Wash. "You go get him in 45 minutes at his house and add a charge of eluding."

But some veteran officers say the emphasis on avoiding conflict can embolden criminals.

"I've actually heard people say, 'You're not supposed to chase me, you're not supposed to pursue,'" said Sgt. Sanford Swanson Jr., a patrolman who is also an instructor for Pro Train, which has taught vehicle-stop tactics to trainers in 38 states. "Sometimes walking away can still pose dangers."

A Line in the Sand

Genevive Dawes, a 21-year-old mother of two, was asleep with her boyfriend in a Dodge Journey outside a Dallas apartment building before dawn on Jan. 18, 2017.

Someone had reported a suspicious vehicle in the parking lot, and body-camera footage shows six police officers surrounding the car with bright lights and raised guns. “Hands up!” one shouted, video footage shows. “Show your hands! Don’t move!”

Ms. Dawes, awakened, slowly backed up the S.U.V. until a patrol car moved to block her. Then she edged forward and tried to reverse again.

Shouting at her to stop, two officers fired 13 bullets through the passenger-side window that passed over Ms. Dawes’s crouching boyfriend and struck her in the neck, chest and arms. As she collapsed, an officer continued yelling, commanding her boyfriend to reach through the shattered window to open the door so that his hand stayed visible.

“Step out! Get on your knees!” the officer, Christopher Hess, ordered. “Walk on your knees towards me!”

Then, body camera footage shows, he falsely announced into his radio, “They rammed the squad car twice.”

The officers later said they had feared the Dodge might run them down, but in a rare departure, skeptical prosecutors persuaded a grand jury to indict Officer Hess for aggravated assault.

At trial, his lawyers attacked Ms. Dawes’s character — she had heroin and methamphetamines in her system, the Dodge had been stolen before she bought it and a handgun was later found on the back floorboard. Christopher Hess, by then fired from the police force, was acquitted.

Many of the fatal vehicle stops reviewed by The Times unfolded in a similar way: Officers acted as if their lives were in constant peril, and killed drivers who failed to obey orders.

“The fear is excessive,” said Grant Fredericks, an authority on the forensic analysis of dash- and body-camera footage and a former officer who has examined scores of police shootings at vehicle stops. “The more fear officers feel, the more aggressive they become.”

But no degree of fright, he said, explained the approach of some officers, who often threatened or used deadly force in response to mere defiance.

“The reaction sometimes seems to be, ‘How dare you?” Mr. Fredericks said. “‘How dare you not do what you’re told to do?’”

Officers have killed more than 5,000 civilians since Sept. 30, 2016, according to data on police killings collected by The Washington Post and the research groups Mapping Police Violence and Fatal Encounters. Many died during felonies in progress, home invasions, domestic violence calls or shootouts in the streets. At least 1,500 were killed by officers pulling over suspected carjackers, during chases and at other types of vehicle stops.

From that data, The Times identified the more than 400 unarmed drivers and passengers who were not under pursuit for a violent crime. All of the deaths were reported by local news organizations, and a small number made national headlines.

The Times examined video or audio from more than 180 of those encounters; interviewed dozens of chiefs, officers, trainers and prosecutors; submitted scores of open-records requests to obtain investigative files; and reviewed civil claims from more than 150 cases.

More than 75 of the drivers were suspected of car theft, either because of registration issues or stolen vehicle reports. Nearly 60 motorists were stopped for reckless driving, including many who turned out to be drunk or high. Others were pulled over for questioning about nonviolent offenses like shoplifting.

The police say there is no such thing as a routine stop; the driver's behavior can turn it into a high-risk encounter, calling for drawn weapons and other measures. In The Times's review, motorists were often resistant or evasive. Some had been hiding illegal drugs or weapons; others had had outstanding warrants for failing to pay a fine or missing a court date.

Among those killed, some became icons of the Black Lives Matter movement, including Daunte Wright (shot in Brooklyn Center, Minn., after being pulled over for expired registration tags); Rayshard Brooks (shot running from officers in a Wendy's parking lot in Atlanta); and Jordan Edwards (a 15-year-old passenger shot leaving a house party in Balch Springs, Texas). But relatives of many others also questioned whether race played a role in their deaths.

In 2017, a white officer in Kent, Wash., told investigators that he had stopped a Honda Accord in part because its young Black occupants seemed afraid of him; one "had a scared look on his face."

The officer pulled over the car for a canceled registration, and the driver, Giovonn Joseph-McDade, a 20-year-old community college student, sped off. A second officer shot him. Although prosecutors deemed the shooting justified, a civil court judge questioned whether the officers had faced any real threat, and the city of Kent this year paid the driver's family $4.4 million to settle a wrongful-death suit.

"My son never would have been pulled over had he not been Black," said his mother, Sonia Joseph. Police officials declined to comment.

Image

Kalfani Ture, a criminologist at Mount St. Mary's University in Maryland and a former Georgia police officer who is Black, said overstating the risks compounded racial bias. "Police think 'vehicle stops are dangerous' and 'Black people are dangerous,' and the combination is volatile," he said.

The problem is especially acute at so-called pretextual stops, he argued, where officers seek out minor violations — expired registration, a dangling air freshener, tinted windows — to search a car they consider suspicious.

"We fish," Dr. Ture said, recalling his past work as a policeman. "If I follow a car for five minutes, I can always find one or two moving violations."

Officers in about four dozen of the deadly cases shot unarmed drivers because they had appeared to reach for something or held an object that the police took for a weapon — including several cellphones, two butane torch lighters, a cigarette, an electric toothbrush case, a bottle of antifreeze and a bag of sandwiches.

Body-camera footage showed an officer in Evansville, Ind., in 2019 pleading with a drunken motorist to stop reaching below his seat: "Whoa, whoa, whoa, let me see your hands!"

When the man didn't comply, Officer Mario Reid shot him — then discovered that he had been grabbing a hammer, not a gun.

"That is the worst day of my life," Officer Reid said in an interview.

But he defended meeting disobedience with deadly force. "If an officer is giving commands repeatedly and they are not being followed and the officer hesitates a bit — there are plenty of those officers who are no longer living or were seriously injured," he said. "I understand the risks involved in doing what I do, and I have to get up every day and face that."

In other cases, officers were carried away by the momentum of a chase. "Police are trained and driven to satisfy their curiosity," said Chief Kenton Buckner of Syracuse. "Sometimes that gets the best of them — why is the car running from me when I stopped them for a taillight?"

On Christmas Day in 2018, Officer Marco Mercado in San Jose, Calif., heard a tip over police radio about a white car that may have been used in a drive-by shooting. He spotted a white Toyota Camry with a license plate that had been reported stolen. When the driver did not pull over, he suspected it was the car linked to the shooting, he later told investigators.

The fleeing Toyota crashed into a chain-link fence. Boxed in by patrol cars, the driver edged forward and back 11 times in an attempt to free the vehicle. “I’m going to shoot you if you don’t stop,” Officer Mercado threatened, according to body camera footage. Moments later, as the Toyota bumped into a patrol car blocking its path, he and three other officers fired 37 shots at the driver, 24-year-old Jennifer Vasquez, killing her.

The officers told investigators that she was reaching for something, that she might have tried to run them down, that her eyes looked “scary,” according to a prosecutor’s report. But Officer Mercado also told investigators that he had decided “to draw a line in the sand” if Ms. Vasquez did not stop driving.

He later learned he had followed the wrong car. The police concluded that the stolen Toyota, which Ms. Vasquez had borrowed from a friend, had not been involved in the drive-by shooting.

‘Get-Out-of-Jail-Free Card’

“Can you prosecute a police officer for a killing at a vehicle stop?” asked Mr. Gill, the Salt Lake County prosecutor. “Theoretically, you can. But practically it becomes virtually impossible.”

The legal standard, he said, “overwhelmingly errs on the side of sheltering police misconduct.”

Although protests since the killing of George Floyd in Minneapolis last year appear to have spurred a modest uptick in criminal charges against officers, the police continue to claim special allowances for the use of force at vehicle stops.

In the more than 400 killings of unarmed drivers, The Times identified charges brought against officers in 32 cases. Among the five officers who were convicted, one got probation, another served seven months, one is awaiting sentencing and a fourth will soon have his appeal heard by the Texas Supreme Court.

The fifth conviction was for murdering George Floyd, who had been pulled from a car on suspicion of passing a fake $20 bill at a Minneapolis convenience store.

Nearly two dozen criminal cases are pending. The New Mexico officer who threatened to choke out a motorist is facing murder charges; the city of Las Cruces paid $6.5 million to settle a wrongful-death suit. That officer was also fired, one of more than two dozen who were dismissed or resigned.

A series of U.S. Supreme Court rulings have expanded the powers and protections of officers pulling over cars, including a 1997 decision holding that the police “must routinely exercise unquestioned command of the situation” because of the unpredictable dangers, and a 2014 decision allowing the police to shoot at moving cars.

“You watch the movies about bank robberies, you know, it happens all the time,” Justice Antonin Scalia said during oral arguments, asserting the practice was standard. “Are these movies unrealistic?”

Even in instances of officer-created jeopardy — the police putting their lives at risk and then citing that risk to justify killing a driver — half the federal appeals courts tell judges and juries to look only at the final moment when a trigger is pulled, ignoring officers’ earlier choices, said Cynthia Lee, a law professor at George Washington University. The results are “arbitrary and inconsistent,” she said.

Police advocates say that even if officers step into the path of a car or reach into a window, a tactical error should not cost them their right to self-defense.

“That doesn’t give somebody a green light to run them over and try to kill them,” said David Mastagni, a California lawyer for police officers and unions. “It doesn’t take away the officers’ justification to use deadly force.”

In more than 150 formal statements or public comments declining to bring charges, some prosecutors emphasized that the legal standard tied their hands, regardless of whether a killing was avoidable. Many others focused on the faults of the drivers, such as their criminal records or drug use.

After the Tennessee sheriff ordered deputies to shoot at a fleeing pickup to avoid damaging patrol cars, for example, the district attorney noted that the driver had taken methamphetamines and had veered all over the road to try to evade his pursuers. He was “a dangerous and unstable subject,” District Attorney Bryant Dunaway wrote.

In other cases, officers faced no charges even when evidence appeared to undermine their explanations.

A Georgia state trooper told investigators that, after forcing a Nissan Sentra with a broken taillight into a ditch, he had felt threatened by its engine “revving” and wheels “wrenching” toward him. A state inquiry found that the battery had been disconnected, the engine disabled and the wheels pointed away from the officer. But a grand jury this spring declined to indict the trooper, who is white, for killing Julian Lewis, a Black 60-year-old carpenter, with a bullet to the head. The possibility of racial bias “is hard to ignore,” said his son, Brook Bacon.

Claiming to fear for their lives “is a get-out-of-jail-free card for the police,” said Sheila Albers, a former middle school principal in Overland Park, Kan., whose 17-year-old son, John, was killed by the police.

After friends reported John as a suicide risk, officers found him backing the family minivan out of the driveway, and one fired more than a dozen shots into the vehicle. Prosecutors accepted the officer’s explanation that the boy had driven “in an extremely aggressive manner.”

But exhibits submitted in a wrongful-death lawsuit indicated that the minivan had been moving at about three miles per hour and that the officer was not in its path when he started shooting. The city paid the family $2.3 million to settle.

Some shootings were commended. In January 2019, Deputy Jason Hanratty of Pueblo County, Colo., stepped out of his car to confront the driver of a GMC Yukon with a broken taillight that had spun out on a lawn after a chase.

When the S.U.V. lurched toward the officer, he pushed off againstthe driver's side hood with his hand and got out of the way, previously unreported body-cam footage shows.

But, Deputy Hanratty later told investigators, he nonetheless feared the S.U.V. would hit him, and he was frightened by the driver, Alicia Martinez, who was 20 and pregnant: She was "ghost-white" and "looking through me, like I was not even there." He fired three shots through her side window as the car passed, seriously injuring her and killing her 18-year-old passenger, Amiliano Apodaca.

A year later, the sheriff awarded a medal of valor to the officer, who by then had made sergeant, praising his actions that night as "truly heroic."

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SPECIAL REPORT

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Police Behavior during Traffic and Street Stops, 2011

Lynn Langton, Ph.D., and Matthew Durose, *BJS Statisticians*

In 2011, over 62.9 million U.S. residents age 16 or older, or 26% of the population, had one or more contacts with police during the prior 12 months **(figure 1)**. For about half (49%) of persons experiencing contact with police, the most recent contact was involuntary or police-initiated. In 2011, 86% of persons involved in traffic stops during their most recent contact with police and 66% of persons involved in street stops (i.e., stopped in public but not in a moving vehicle) believed that the police both behaved properly and treated them with respect during the contact. A greater percentage of persons involved in street stops (25%) than those pulled over in traffic stops (10%) believed the police had not behaved properly. Regardless of the reason for the stop, less than 5% of persons who believed the police had not behaved properly filed a complaint.

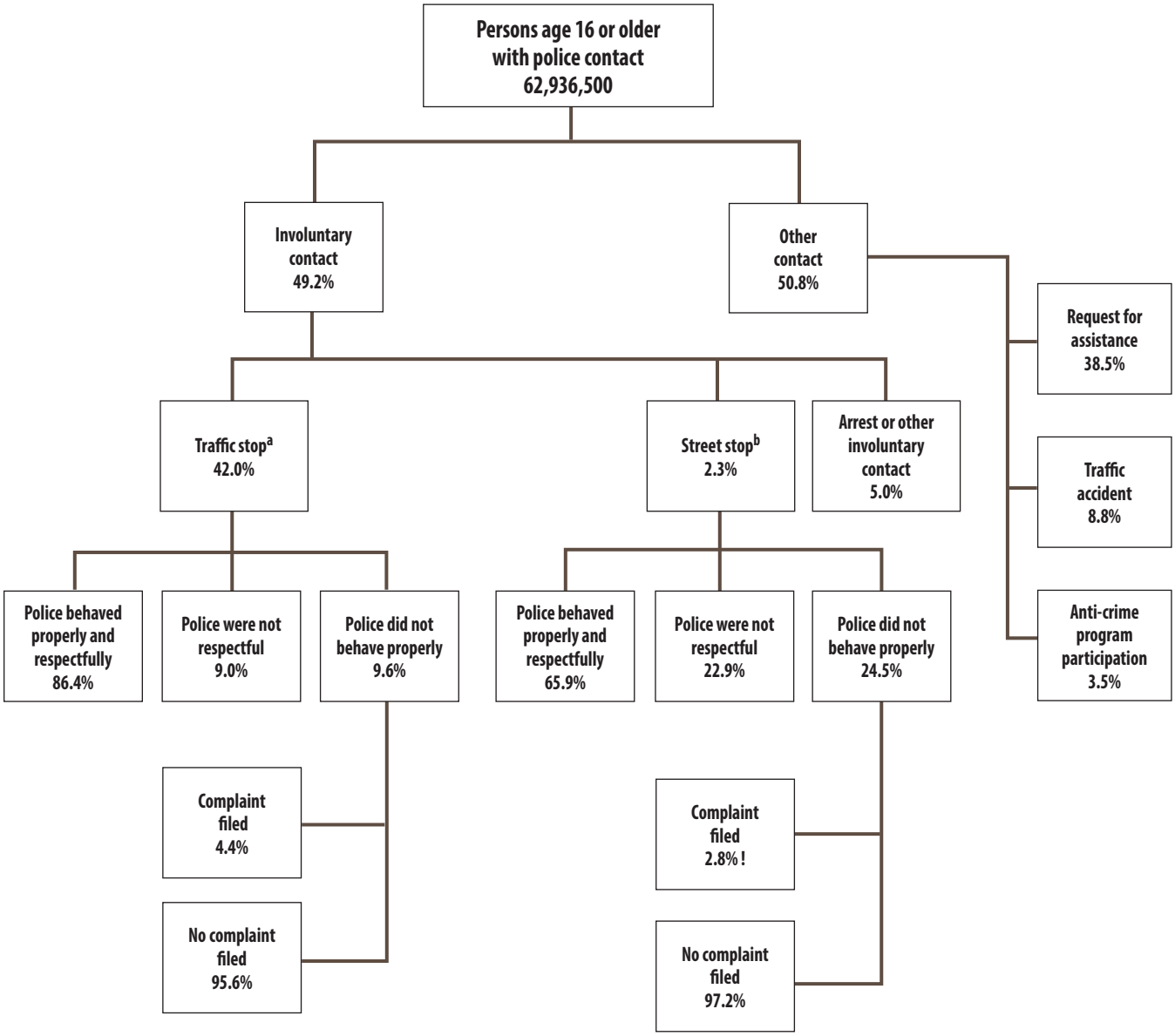
The data in this report were drawn from the Bureau of Justice Statistics' (BJS) 2011 Police-Public Contact Survey (PPCS), a supplement to the National Crime Victimization Survey (NCVS), which collects information from a nationally representative sample of persons in U.S. households. The PPCS collects information on contact with police during a 12-month period. This report examines involuntary contacts with police, specifically those that occurred when the person was the driver of a motor vehicle (i.e., traffic stops) or when the person was stopped by the police while in a public place but not in a moving vehicle (i.e., street stops). It describes variations in perceptions of police behavior and police legitimacy during traffic and street stops. (For more information on how perceptions of police behavior and legitimacy were measured in this report, see survey questions on page 12.) All findings in this report are based on persons for whom the most recent contact in 2011 was in a street stop or as the driver in a traffic stop. For information on voluntary contacts with police, see *Requests for Police Assistance, 2011*, NCJ 242938, BJS website, September 2013.

HIGHLIGHTS

- Relatively more black drivers (13%) than white (10%) and Hispanic (10%) drivers were pulled over in a traffic stop during their most recent contact with police. There were no statistical differences in the race or Hispanic origin of persons involved in street stops.
- Persons involved in street stops were less likely (71%) than drivers in traffic stops (88%) to believe that the police behaved properly.
- Of those involved in traffic and street stops, a smaller percentage of blacks than whites believed the police behaved properly during the stop.
- Drivers pulled over by an officer of the same race or ethnicity were more likely (83%) than drivers pulled over by an officer of a different race or ethnicity (74%) to believe that the reason for the traffic stop was legitimate.
- White drivers were both ticketed and searched at lower rates than black and Hispanic drivers.
- Across race and Hispanic origin, persons who were searched during traffic stops were less likely than persons who were not searched to believe the police behaved properly during the stop.
- About 1% of drivers pulled over in traffic stops had physical force used against them by police. Of these drivers, 55% believed the police behaved properly during the stop.
- About 6 in 10 persons age 16 or older involved in street stops believed they were stopped for a legitimate reason.
- About 19% of persons involved in street stops were searched or frisked by police. The majority of persons who were searched or frisked did not believe the police had a legitimate reason for the search.

FIGURE 1

Perceptions that police behaved properly and respectfully during most recent contact with persons age 16 or older, by type of contact, 2011



Note: Based on the most recent contact with police during the past 12 months. Detail may not sum to 100% due to missing data and multiple responses. See appendix table 1 for estimates and standard errors.

[a]Includes being stopped by police as either a driver or a passenger in a motor vehicle. All other tables focus on the driver of the motor vehicle in a traffic stop.

[b]Includes being stopped by police in a public place, not a moving vehicle.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

About 71% of persons involved in streets stops thought the police behaved properly, compared to 88% of drivers pulled over in traffic stops

In 2011, less than 1% of the 241.4 million U.S. residents age 16 or older were involved in a street stop during their most recent contact with police (**table 1**; appendix table 2). A greater percentage of males (1%) than females (less than 1%) were involved in street stops during 2011. Persons ages 16 to 24 were more likely than persons age 35 or older to be involved in street stops. While no differences were observed in the percentage of non-Hispanic white, non-Hispanic black, and Hispanic populations age 16 or older involved in a street stop, among those who were stopped, a smaller percentage of blacks (38%) than Hispanics (63%) or whites (78%) felt the police behaved properly during the stop.

Traffic stops were a more common form of police contact than street stops in 2011. About 10% of the 212.3 million U.S. drivers age 16 or older were stopped while operating a motor vehicle during their most recent contact with police.[1] As with street stops, a greater percentage of male drivers (12%) than female drivers (8%) were pulled over in traffic stops. Across age groups, the highest percentage of stopped drivers was among drivers ages 18 to 24 (18%). A higher percentage of black drivers (13%) than white (10%) and Hispanic (10%) drivers age 16 or older were pulled over in a traffic stop during their most recent contact with police.

A higher percentage of drivers in traffic stops (88%) than persons involved in street stops (71%) believed the police behaved properly during the stop. White drivers pulled over by police (89%) were more likely than black drivers (83%) to think that the police behaved properly, while no difference was observed between the percentages of stopped white drivers and Hispanic drivers who thought that the police behaved properly. There was also no statistical difference in the percentages of black and Hispanic stopped drivers who believed the police behaved properly.

[1]The driving population includes persons age 16 or older who reported driving a few or more times during the year or who were stopped as the driver in a traffic stop during 2011.

TABLE 1

Involuntary contact with police among persons age 16 or older, by demographic characteristics and type of contact, 2011

	Street stops[a]			Traffic stops[b]		
		Percent of stopped persons			Percent of stopped drivers	
Demographic characteristics	Percent of all persons	Total	Police behaved properly[d]	Percent of all drivers[c]	Total	Police behaved properly[d]
Total	0.6%	100%	70.7%	10.2%	100%	88.2%
Sex						
Male	0.8%	67.5%	69.8%	11.9%	58.8%	86.9%
Female	0.4	32.5	72.7	8.4	41.2	89.9
Race/Hispanic origin						
White[e]	0.6%	65.2%	77.6%	9.8%	69.3%	89.4%
Black/African American[e]	0.6	12.4	37.7 !	12.8	12.6	82.7
Hispanic/Latino	0.7	15.3	62.9	10.4	12.2	86.5
American Indian/Alaska Native[e]	0.5 !	0.4 !	100 !	15.0	0.6	74.2
Asian/Native Hawaiian/other Pacific Islander[e]	0.4 !	3.6 !	85.0 !	9.4	4.0	89.5
Two or more races[e]	1.8 !	3.1 !	76.6 !	13.4	1.3	94.8
Age						
16–17	1.5%	8.5%	67.4%	9.0%	1.8%	92.3%
18–24	1.6	31.7	72.1	17.8	19.5	85.1
25–34	0.9	27.1	64.4	12.7	22.4	88.1
35–44	0.4	10.6	81.6	11.3	19.8	87.9
45–54	0.4	10.9	79.7	9.4	17.9	88.7
55–64	0.2	5.5	62.2 !	7.1	11.4	89.7
65 or older	0.2	5.7	68.8 !	4.8	7.2	92.3

Note: See appendix table 2 for estimates of the U.S. population and driving population age 16 or older and appendix table 3 for standard errors.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

[a]Includes persons stopped by police during the past 12 months for whom the most recent contact involved being stopped by police on the street or in public, but not in a moving motor vehicle.

[b]Includes persons stopped by police during the past 12 months for whom the most recent contact was as a driver in a traffic stop.

[c]Percents based on the driving population age 16 or older, which includes PPCS respondents who reported driving a few times a year or more or were the driver in a traffic stop.

[d]Denominator includes approximately 2% of respondents who did not know or did not report whether police behaved properly.

[e]Excludes persons of Hispanic or Latino origin.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

Traffic stops

Regardless of the reason for the traffic stop, black (67%) and Hispanic (74%) drivers were less likely than white drivers (84%) to believe the reason for the stop was legitimate

In 2011, a greater percentage of white drivers (84%) than Hispanic (74%) or black drivers (67%) who were stopped by police believed they were pulled over for a legitimate reason (table 2). Across all races and Hispanic origin, drivers stopped for speeding were among the most likely to perceive that the reason for the traffic stop was legitimate (90% of white, 83% of Hispanic, and 73% of black drivers). In general, drivers who were pulled over and not given a reason for the traffic stop were the least likely to think the traffic stop was legitimate. For example, 51% of white drivers who were stopped without the police giving a reason believed the stop was legitimate, whereas 84% who were given a reason believed that the stop was legitimate.

Among other reasons for traffic stops that were associated with comparatively lower perceptions that the stop was legitimate, less than 70% of white (69%), black (69%), and Hispanic (64%) drivers who were pulled over for a stop light or stop sign violation believed the police had a legitimate reason for stopping them. Less than 70% of black drivers stopped due to a vehicle defect (69%), a seatbelt or cell phone violation (64%), or an illegal turn or lane change violation (65%) thought the police had a legitimate reason for stopping them.

TABLE 2

Perception that reason for traffic stop was legitimate among drivers age 16 or older, by race or Hispanic origin of driver and reason for stop, 2011

	Percent of stopped drivers				
Reason for traffic stop	All	White[a]	Black/AfricanAmerican[a]	Hispanic/Latino	Other[a,b]
Any reasons	100%	100%	100%	100%	100%
Police gave reason for the stop					
Speeding	46.5	50.1	37.7	39.2	37.3
Vehicle defect	14.1	12.7	19.0	16.5	14.6
Record check	9.7	9.0	14.0	9.7	9.9
Roadside sobriety check	1.3	1.6	0.4 !	1.0 !	1.0 !
Seatbelt or cell phone violation	6.6	6.6	6.5	6.5	7.4
Illegal turn or lane change	7.0	6.6	7.0	7.1	10.8
Stop sign/light violation	6.7	6.1	5.5	9.9	9.4
Other reason[c]	5.1	4.7	5.3	6.8	5.2
Police did not give reason for the stop	3.1	2.6	4.7	3.3	4.2 !

	Percent reporting reason for stop was legitimate[d]				
Reason for traffic stop	All	White[a]	Black/African American[a]	Hispanic/Latino	Other[a,b]
Any reasons	80.0%	83.6%	67.5%	73.6%	78.4%
Police gave reason for the stop					
Speeding	87.1	89.6	72.8	83.1	87.3
Vehicle defect	81.2	86.4	69.0	74.4	79.3
Record check	80.0	80.9	83.0	70.7	81.2
Roadside sobriety check	79.4	86.0	-- !	56.6 !	68.1 !
Seatbelt or cell phone violation	79.7	84.0	63.8	77.3	69.0 !
Illegal turn or lane change	73.0	75.4	65.0	72.6	67.1
Stop sign/light violation	68.4	68.8	69.2	63.6	74.6
Other reason[c]	59.1	65.2	21.6 !	61.9	67.8 !
Police did not give reason for the stop	44.6	51.0	36.6 !	18.3 !	59.8 !

Note: Based on persons for whom the most recent contact with police was as a driver in a traffic stop. See appendix table 4 for standard errors.

! Interpret with caution. Estimate based on 10 or fewer cases or the coefficient of variation is greater than 50%.

-- Less than 0.05%.

[a]Excludes persons of Hispanic or Latino origin.

[b]Includes persons identifying as American Indian, Alaska Native, Asian, Native Hawaiian, or other Pacific Islander, and persons of two or more races.

[c]Denominator includes approximately 3% of white, 6% of black, 3% of Hispanic, and 4% of other race drivers who did not know or did not report whether the reason for the stop was legitimate.

[d]Includes reasons such as reckless driving, littering, failure to yield, following too closely, obstructed license plate, and noise violations.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

A greater percentage of drivers pulled over by an officer of the same race or ethnicity (83%) than drivers stopped by an officer of a different race or ethnicity (74%) believed the reason for the traffic stop was legitimate

About 83% of drivers pulled over by an officer of the same race or Hispanic origin thought the reason for the traffic stop was legitimate, compared to 74% of drivers pulled over by an officer of a different race or Hispanic origin (table 3).[2] However, drivers' perceptions of traffic stop legitimacy varied somewhat by the reason for the stop and whether the driver and officer were the same race or Hispanic origin. When the reason for the stop was speeding, a vehicle defect, a roadside sobriety check, or a seatbelt or cell phone violation, drivers pulled over by an officer of a different race or ethnicity were less likely than drivers pulled over by an officer of the same race or ethnicity to perceive the reason for the traffic stop to be legitimate. In comparison, a similar percentage of drivers stopped for a record check, an illegal turn or lane change, or a stop light or stop sign violation perceived the stop to be legitimate, regardless of whether the officer was the same race or ethnicity as the driver or a different race or ethnicity. Whether the driver and officer were intraracial (41%) or interracial (42%), the officer's failure to give a reason for the stop resulted in less than half of stopped drivers believing the stop was legitimate.

[2]Data on officer race or Hispanic origin are based on respondent's perception.

TABLE 3

Perception that reason for traffic stop was legitimate among drivers age 16 or older, by reason for stop and whether driver and officer were intra- or interracial, 2011

	Intraracial driver and officer		Interracial driver and officer	
Reason for traffic stop	Total stopped drivers	Reason for stop was legitimate[a]	Total stopped drivers	Reason for stop was legitimate[a]
Any reasons	100%	83.3%	100%	74.4%
Police gave reason for the stop				
Speeding	51.1	89.4	42.3	83.6
Vehicle defect	13.0	84.4	16.7	74.6
Record check	8.7	79.3	9.6	80.8
Roadside sobriety check	1.4	83.4	0.4 !	38.5 !
Seatbelt or cell phone violation	6.1	86.2	7.2	70.4
Illegal turn or lane change	6.6	75.2	7.3	67.7
Stop sign/light violation	6.0	70.9	7.5	62.3
Other reason[b]	4.6	63.6	5.8	45.6
Police did not give reason for the stop	2.4	46.8	3.1	41.8

Note: Based on persons for whom the most recent contact with police was as a driver in a traffic stop. Information on the race or Hispanic origin of the officer is based on respondent's perception. Excludes drivers who were stopped by two or more officers of different races or Hispanic origin and officers whose race or Hispanic origin were unknown to the driver. See appendix table 5 for standard errors.

[a]Denominator includes approximately 3% of respondents who did not know or did not report whether the reason for the stop was legitimate.

[b]Includes reasons such as reckless driving, littering, failure to yield, following too closely, obstructed license plate, and noise violations.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

While the majority of drivers pulled over in a traffic stop were stopped by white officers, a larger percentage of black drivers (14%) than white (4%) or Hispanic (3%) drivers were stopped by black officers (table 4). Similarly, a greater percentage of Hispanic drivers was stopped by Hispanic officers (17%) than were white (3%) or black (6%) drivers.

A similar percentage of white drivers believed the reason for the stop was legitimate, regardless of whether they were stopped by white, Hispanic, or black officers. While black drivers had similar perceptions of police legitimacy when pulled over by white (70%) or black (71%) officers, a lower percentage of black drivers stopped by Hispanic officers perceived the stop to be legitimate (47%). Among Hispanic drivers, no differences were observed in perceptions of traffic stop legitimacy, regardless of the race or Hispanic origin of the officer.

Among drivers who thought the reason for the stop was not legitimate, 65% believed the police behaved properly, compared to 94% among drivers who thought the stop was legitimate

When the reason for the traffic stop was not seen as legitimate, a smaller percentage of white, black, and Hispanic drivers believed the police behaved properly during the stop than when the reason for the stop was legitimate. Whether the driver and officer were intra- or inter- racial, relatively fewer whites, blacks, and Hispanics thought the police behaved properly when the reason for the stop was perceived to be illegitimate. Regardless of the race or Hispanic of the officer, over 90% of white, black, and Hispanic drivers who believed the stop was legitimate also thought that the police behaved properly. Among white and Hispanic drivers who believed the police had no legitimate reason for the stop, the percentage who also believed that the police behaved properly did not vary, regardless of whether the officer was white, black, or Hispanic. Among black drivers who believed the officer had no legitimate reason for the traffic stop, a higher percentage thought the police behaved properly when the officer was black (87%) than when the officer was white (58%) or Hispanic (55%).

TABLE 4

Perception that reason for traffic stop was legitimate among drivers age 16 or older, by race and ethnicity of driver and officer and driver's perception that police behaved properly, 2011

		Percent of stopped drivers			
				Police behaved properly[c]	
Race and ethnicity of driver and officer	Percent of all drivers	Total[a]	Reason for stop was legitimate[b]	Reason for stop was legitimate	Reason for stop was not legitimate
Total	10.2%	100%	80.0%	93.9%	65.0%
White driver[d,e]	9.8%	100%	83.6%	93.9%	64.5%
White officer[d]	7.9	81.0	84.0	93.8	67.2
Black/African American officer[d]	0.4	4.3	82.3	96.6	60.3
Hispanic/Latino officer	0.3	3.3	76.5	98.0	63.5
Black/African American driver[d,e]	12.8%	100%	67.5%	94.2%	58.7%
White officer[d]	8.3	65.3	70.2	93.6	58.3
Black/African American officer[d]	1.8	13.8	70.7	91.6	87.1
Hispanic/Latino officer	0.7	5.7	46.8	100	55.4 !
Hispanic/Latino driver[e]	10.4%	100%	73.6%	94.7%	60.1%
White officer[d]	6.7	64.9	74.3	94.2	64.1
Black/African American officer[d]	0.3	3.2	74.1	94.2 !	64.3 !
Hispanic/Latino officer	1.7	16.7	77.4	93.8	54.4 !
Other driver[d,e]	10.5%	100%	78.4%	91.9%	78.7%
White officer[d]	7.6	72.3	80.3	93.1	75.0
Black/African American officer[d]	0.3 !	3.1 !	52.7 !	51.2 !	79.4 !
Hispanic/Latino officer	0.6 !	5.8 !	76.9 !	100 !	100 !

Note: Based on persons for whom the most recent contact with police was as a driver in a traffic stop. Information on the race or Hispanic origin of the officer is based on driver's perception. See appendix table 6 for standard errors.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation was greater than 50%.

-- Less than 0.05%.

[a]Percentages do not sum to 100% due to 11% of white drivers, 15% of black drivers, 15% of Hispanic drivers, and 19% of other race drivers who were stopped by officers identified as American Indian, Alaska Native, Asian, Native Hawaiian, other Pacific Islander, or two or more races; groups of officers of different races and Hispanic origin; and officers whose race or Hispanic origin was unknown to the driver.

[b]Denominator includes approximately 3% of respondents who did not know or did not report whether the reason for the stop was legitimate.

[c]Denominator includes approximately 1% of drivers who thought the stop was legitimate and 6% of drivers who did not think it was legitimate who did not know or did not report whether the police behaved properly.

[d]Excludes persons of Hispanic or Latino origin.

[e]Includes officers identified as American Indian, Alaska Native, Asian, Native Hawaiian, or other Pacific Islander, and persons of two or more races.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

In 2011, there were small racial differences in the percentage of drivers who were ticketed

About 5% of the 212.3 million drivers age 16 or older were pulled over and ticketed in the most recent contact with police (table 5). Another 3% of all drivers were pulled over and given a verbal or written warning and 1% were allowed to proceed with no enforcement action after being stopped.

A greater percentage of male drivers (6%) were ticketed than female drivers (4%), and a greater percentage of black (7%) and Hispanic (6%) drivers were ticketed than white drivers (5%). A greater percentage of black drivers (2%) were stopped and allowed to proceed with a no enforcement action than white (1%) and Hispanic (1%) drivers. A greater percentage of drivers ages 18 to 24 (10%) were ticketed than drivers in any other age group.

The majority (93%) of stopped drivers who were issued a warning believed that the police behaved properly during the stop. Regardless of the demographic characteristics of the driver, 87% to 95% of drivers who were issued a warning after being stopped believed the police behaved properly. Among drivers of all sexes, ages, races and Hispanic origin ticketed by police, the percentage who believed the police behaved properly ranged from 81% to 90%.

Across most demographic characteristics examined, stopped drivers who were allowed to proceed without any enforcement action were less likely than drivers who were issued a warning to believe the police behaved properly. Since previous findings in this report suggest an association between perceptions of traffic stop legitimacy and perceptions that police behaved properly, this may suggest that drivers were less likely to believe the reason for the stop was legitimate when no enforcement action occurred as a result of the stop.

In 2011, 1% of stopped drivers were arrested during the stop. The majority of arrested drivers also received a ticket or a warning during the stop. Among stopped drivers who were arrested, 76% believed the police behaved properly (not shown in table).

TABLE 5

Enforcement actions taken by police against drivers age 16 or older, by driver's demographic characteristics and perception that police behaved properly, 2011

	Ticketed			Warned			Allowed to proceed with no enforcement action		
		Percent of stopped drivers			Percent of stopped drivers			Percent of stopped drivers	
Race of driver	Percent of all drivers	Ticketed drivers	Police behaved properly[a]	Percent of all drivers	Warned drivers	Police behaved properly[a]	Percent of all drivers	Drivers with no enforcement	Police behaved properly[a]
All drivers	5.3%	100%	86.6%	3.4%	100%	93.3%	1.4%	100%	82.5%
Sex									
Male	6.2%	58.5%	86.2%	3.9%	57.8%	92.4%	1.7%	61.7%	78.6%
Female	4.4	41.5	87.2	2.9	42.2	94.5	1.1	38.3	88.9
Race/Hispanic origin									
White[b]	4.8%	65.5%	87.5%	3.6%	75.5%	94.2%	1.4%	69.2%	84.5%
Black/African American[b]	7.0	13.2	81.1	3.5	10.5	87.4	2.1	14.7	78.9
Hispanic/Latino	6.2	14.0	86.6	2.8	9.9	91.7	1.3	11.0	74.7
Other[b,c]	6.7	7.2	89.1	2.5	4.1	94.6	1.3	5.1	83.9
Age									
16–17	3.9%	1.5%	88.2%	4.2%	2.5%	94.5%	0.9% !	1.3%	100 % !
18–24	9.8	20.5	84.4	5.2	17.1	91.5	2.7	21.2	76.9
25–34	7.2	24.1	85.4	4.1	21.6	95.0	1.4	17.5	81.5
35–44	6.1	20.3	86.6	3.9	20.2	92.7	1.3	16.9	79.8
45–54	4.7	17.2	88.3	3.1	17.9	91.8	1.5	20.9	83.5
55–64	3.4	10.6	89.1	2.4	11.7	94.2	1.2	13.4	87.6
65 or older	2.0	5.7	89.9	2.0	9.0	95.4	0.8	8.8	90.8

Note: Based on persons for whom the most recent contact with police was as a driver in a traffic stop. Excludes a small percentage of drivers who were arrested without any other enforcement action (0.4%). See appendix table 7 for standard errors.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

[a]Denominator includes approximately 3% of respondents who did not know or did not report whether police behaved properly.

[b]Excludes persons of Hispanic or Latino origin.

[c]Includes persons identifying as American Indian, Alaska Native, Asian, Native Hawaiian, or other Pacific Islander, and persons of two or more races.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

Among black and Hispanic stopped drivers, a similar percentage of ticketed and not ticketed drivers believed the police behaved properly during the traffic stop

Among drivers stopped in traffic stops, there was no statistical difference in the percentage of white (50%) and black (55%) stopped drivers were ticketed in 2011. Hispanic stopped drivers (60%) were more likely than white stopped drivers to receive a ticket. A greater percentage of white drivers were ticketed when stopped by black officers (64%) than white officers (49%) **(table 6)**. However, for black and Hispanic drivers stopped by police, the percentage issued a ticket did not vary by the race or Hispanic origin of the officer. These differences and similarities in enforcement practices by race or Hispanic origin of the driver and officer may be related to the reason for the traffic stop or other factors and do not necessarily reflect biased or unbiased treatment.

Among white, black, and Hispanic drivers who were stopped and ticketed, the percentage who believed the police behaved properly did not vary regardless of whether the officer was white, black, or Hispanic. Overall, for most racial and ethnic groups, the majority of stopped drivers believed the police behaved properly whether a ticket was issued or not.

TABLE 6

Stopped drivers age 16 or older who were ticketed, by race of officer and driver and driver's perception that police behaved properly, 2011

		Percent of stopped drivers[a]		
			Police behaved properly	
Race of driver and officer	Percent of all drivers issued a ticket	Ticketed	Ticketed drivers	Drivers not ticketed[b]
White driver[c,d]	4.8%	49.5%	87.5%	91.2%
White officer[c]	3.9	49.2	87.5	91.6
Black/African American officer[c]	0.3	63.6	86.5	96.7
Hispanic/Latino officer	0.2	50.2	88.3	91.5
Black/African American driver[c,d]	7.0%	55.1%	81.1%	84.6%
White officer[c]	4.7	56.5	80.5	86.3
Black/African American officer[c]	1.1	63.2	88.9	92.6
Hispanic/Latino officer	0.4	53.4	74.4 !	78.4 !
Hispanic/Latino driver[d]	6.2%	60.2%	86.6%	86.4%
White officer[c]	3.9	58.7	85.0	88.3
Black/African American officer[c]	0.2 !	52.9 !	91.9 !	80.4 !
Hispanic/Latino officer	1.0	59.1	88.7	79.4
Other driver[c,d]	6.7%	64.0%	89.1%	89.3%
White officer[c]	4.8	63.5	89.5	89.5
Black/African American officer[c]	0.3	89.1	60.2 !	100 !
Hispanic/Latino officer	0.6 !	100 !	100 !	-- !

Note: Based on persons for whom the most recent contact with police was as a driver in a traffic stop. Information on the race or Hispanic origin of the officer is based on driver's perception. Excludes drivers who were stopped by officers identified as American Indian, Alaska Native, Asian, Native Hawaiian, other Pacific Islander, or two or more races; drivers stopped by groups of officers of different races and Hispanic origin; and officers whose race or Hispanic origin was unknown to the driver. See appendix table 8 for standard errors.

-- Less than 0.05%.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

[a]Denominator includes about 2% of respondents who did not know or did not report whether police behaved properly.

[b]Includes drivers who were given a verbal or written warning or allowed to proceed without any enforcement action.

[c]Excludes persons of Hispanic or Latino origin.

[d]Includes officers identified as American Indians, Alaska Natives, Asians, Native Hawaiians, or other Pacific Islanders, and persons of two or more races.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

Stopped drivers who were searched were less likely than drivers who were not searched to believe that the police behaved properly

In 2011, 3% of drivers pulled over by police in a traffic stop had their person or vehicle searched (table 7). A greater percentage of male drivers (4%) than female drivers (2%) were searched during traffic stops. Male drivers accounted for 76% of searches conducted among stopped drivers. A lower percentage of white drivers stopped by police were searched (2%) than black (6%) or Hispanic (7%) drivers. A greater percentage of stopped drivers ages 18 to 34 (5%) than those age 55 or older (1%) was searched during traffic stops.

Across all demographic groups examined, a smaller percentage of drivers who had their person or vehicle searched by police during a traffic stop than drivers who were not searched believed the police behaved properly. Overall, 61% of searched drivers believed the police behaved properly, compared to 89% of drivers who were stopped but not searched. The percentage of searched drivers who believed the police behaved properly did not vary by sex, race or Hispanic origin, or age.

TABLE 7

Stopped drivers age 16 or older who were searched by police, by driver's demographic characteristics and perception that police behaved properly, 2011

Demographic characteristics	Percent of all stopped drivers searched by police	Percent of stopped drivers[a]		
		Searched	Police behaved properly	
			Searched drivers	Drivers not searched
Total	3.5%	100%	61.3%	89.1%
Sex				
Male	4.5%	75.7%	61.0%	88.1%
Female	2.1	24.3	62.2	90.5
Race/Hispanic origin				
White[b]	2.3%	46.6%	62.4%	90.0%
Black/African American[b]	6.3	22.8	61.6	84.1
Hispanic/Latino	6.6	23.1	64.8	88.1
Other[b,c]	4.4 !	7.4 !	42.5 !	91.3
Age				
16–17	1.4% !	0.7% !	--% !	93.5%
18–34	4.8	58.0	58.7	88.1
35–54	3.1	33.8	69.8	88.9
55 or older	1.4 !	7.5 !	49.0 !	91.3

Note: Includes respondents for whom the most recent contact with police was as a driver in a traffic stop. See appendix table 9 for standard errors.

-- Less than 0.05%.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

[a]Denominator includes about 6% of searched drivers and 2% of other stopped drivers who did not know or did not report whether the police behaved properly.

[b]Excludes persons of Hispanic or Latino origin.

[c]Includes persons identifying as Native American, Alaska Native, Asian, Native Hawaiian, or other Pacific Islander, and persons of two or more races.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

When the police did not ask permission to conduct a search, less than half of searched drivers thought the officers behaved properly during the traffic stop

Less than half (46%) of drivers believed the police behaved properly when a person or vehicle search was conducted without the police first asking permission to conduct the search or without the police having a perceived legitimate reason to conduct the search **(table 8)**. When the police asked permission before conducting a search during a traffic stop, a greater percentage of drivers believed the police behaved properly (72%). Similarly, a greater percentage of drivers thought the police behaved properly when they believed the police had a legitimate reason for conducting the search (86%) than when the reason for the search was not seen as legitimate (46%). About 6% of searched drivers reported that the police uncovered illegal items during the search (not shown in table).

More than half of drivers who experienced police use of physical force or verbal threats thought police behaved properly

In 2011, 6% of drivers pulled over in traffic stops experienced some type of force used against them, from shouting and cursing, to verbal threats of force or other action, to physical force, including hitting, handcuffing, and pointing a gun **(table 9)**. Of the 1% of stopped drivers who experienced physical force during the traffic stop, more than half (55%) believed the police behaved properly during the contact. A similar percentage of drivers who experienced verbal threats of force believed the police behaved properly (56%).

Three in 4 (75%) stopped drivers who experienced any type of force believed the police actions were unnecessary. About two-thirds (65%) of drivers who experienced police use of force did not think the force was excessive. Among stopped drivers who experienced any type of verbal or physical force, 83% who believed the force used or threatened against them was necessary also thought police behaved properly, compared to 38% of those who did not believe the use of force was necessary.

TABLE 8

Stopped drivers age 16 or older who had their person or vehicle searched by police, by perception that police behaved properly, 2011

	Percent of stopped drivers	
Stop characteristics	Total	Police behaved properly[a]
All stops	100%	88.2%
Police searched driver or vehicle		
No	96.4%	89.3%
Yes	3.5	61.4
	Percent of searched drivers	
Police asked permission to search[b]		
No	40.2%	46.0%
Yes	59.8	71.8
Driver thought search was legitimate[b]		
No	61.4%	46.1%
Yes	38.6	85.8

Note: Based on persons for whom the most recent contact with police was as a driver in a traffic stop. See appendix table 10 for standard errors.

[a]Denominator includes about 6% of searched and 2% of other stopped drivers not searched who did not know or did not report whether the police behaved properly.

[b]Based on the number of drivers who experienced a personal or vehicular search.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

TABLE 9

Type of force used or threatened by police against stopped drivers age 16 or older, by perception that police behaved properly, 2011

	Percent of stopped drivers	
Type of force	Total	Police behaved properly[a]
All stops	100%	88.2%
Force used		
Shouting or cursing[b]	1.2%	22.0% !
Verbal threats[c]	3.4	56.4
Physical force[d]	1.5	55.0
	Percent of drivers who experienced force	
Driver thought use of force was necessary[e]		
No	74.7%	38.4%
Yes	19.1	83.3
Driver thought use of force was excessive[e]		
No	64.6%	68.3%
Yes	33.3	12.4 !

Note: Includes persons for whom the most recent contact with police was as a driver in a traffic stop. See appendix table 11 for standard errors.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

[a]Denominator includes about 3% of respondents who did not know or did not report whether the police behaved properly.

[b]Excludes stopped drivers who experienced verbal threats or physical force.

[c]Includes threats of arrest, ticketing, or use of force. Excludes stopped drivers who experienced physical force used against them.

[d]Includes pushing, grabbing, hitting, kicking, handcuffing, using chemical or pepper spray, using an electroshock weapon, or pointing a gun.

[e]Based on stopped drivers who had force (shouting or cursing, verbal threats, or physcial force) used against them by police. Percent of stopped drivers does not sum to 100% due to persons who did not know whether the use of force was necessary or excessive.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

Street stops

About 6 in 10 persons involved in street stops believed they were stopped for a legitimate reason

In 2011, less than 1% of persons age 16 or older were stopped by the police while in a public place **(table 10)**. Among persons stopped by the police in a street stop during their most recent police contact, at least 41% were stopped because the police suspected them of something or they matched the description of someone for whom the police were looking. At least 16% of persons involved in street stops said the police did not provide a reason for the stop or the police were seeking information about another person or investigating a crime, and at least 7% were stopped because the police were providing a service. About 20% of persons involved in street stops did not report a reason for the stop.

Overall, 64% of persons involved in street stops believed the police stopped them for a legitimate reason. Among persons who were stopped because the police suspected them of something, 61% thought the reason for the stop was legitimate. The percentage of persons who thought the reason for the stop was legitimate was higher among those who were stopped because the police were providing a service (91%) or seeking information or investigating a crime (92%). Similarly, compared to those who were stopped because the police were investigating a crime (90%) or were providing assistance (96%), a lower percentage of persons stopped because they were suspected of something believed the police behaved properly (68%).

TABLE 10

Reason for street stops involving persons age 16 or older, by perceptions that stop was legitimate and police behaved properly, 2011

Reason for street stop	Percent of all persons	Percent of stopped persons		
		Total	Reason for stop was legitimate[a]	Police behaved properly[b]
Any reasons	0.6%	100%	64.1%	70.7%
Suspected of something or matched description of someone police were looking for[c]	0.2	40.7	60.8	68.5
Police were seeking information about another person or investigating a crime	0.1	15.5	92.1	89.8
Police were providing a service	--	6.9	90.8	95.9
No reason given by police	0.1	16.5	29.7	49.0
Unknown[d]	0.1	20.4	68.1	69.8

Note: Based on persons for whom the most recent contact with police involved being stopped by police in public or on the street, not in a moving vehicle. See appendix table 12 for standard errors.

-- Less than 0.05%.

[a]Denominator includes less than 1% of respondents who did know or did not report whether police had a legitimate reason for the stop.

[b]Denominator includes about 3% of respondents who did know or did not report whether police behaved properly.

[c]Includes street stops in which the respondent was with someone who the police suspected of something or who matched the description of someone for whom they were looking.

[d]No reason reported.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

Regardless of the reason for the stop, a slightly higher percentage of persons involved in a street stop with an officer of the same race or ethnicity believed that the police behaved properly (79%) than persons stopped by an officer of a different race or ethnicity (62%) **(table 11)**. Persons least likely to believe the police behaved properly during street stops were those stopped for reasons they did not believe were legitimate (38%), persons who were searched without a perceived legitimate reason (29%), and persons who had force used against them (30%).[3]

[3]Due to small sample sizes, the percentage of persons who had illegal items uncovered in the search could not be calculated.

TABLE 11

Characteristics of persons age 16 or older involved in street stops and outcomes of the stop, by perceptions that police behaved properly, 2011

	Percent of stopped persons	
Stop characteristics	Total	Police behaved properly[a]
All stops	100%	70.7%
Officer and respondent were the same race or Hispanic origin[b]		
No	22.4%	62.3%
Yes	54.5	78.8
Unknown[c]	23.1	59.8
Reason for stop was legitimate		
No	35.6%	37.5%
Yes	64.1	89.5
Searched or frisked		
No	78.9%	76.6%
Yes	19.1	53.8
Person thought search was legitimate		
No	11.1%	29.5%
Yes	8.0	87.9
Force used		
No	74.6%	84.5%
Yes	25.4	30.3
Person thought force was excessive		
No	17.3%	44.5%
Yes	7.0	-- !

Note: Based on persons for whom the most recent contact with police involved being stopped by police in public or on the street, not in a moving vehicle. See appendix table 13 for standard errors.

-- Less than 0.05%.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

[a]Denominator includes about 4% of respondents who did know or did not report whether police behaved properly.

[b]Information on the race or Hispanic origin of the officer is based on the person's perception.

[c]Includes person who were stopped by two or more officers of different races or Hispanic origin and officers whose race or Hispanic origin was unknown to the person.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011

Police–Public Contact Survey questions pertaining to perceptions of police behavior and legitimacy of police actions

Perceptions of police behavior

Q. Looking back in this contact, do you feel the police behaved properly?

Perceptions of legitimacy of stop

Q. Would you say that the police officer(s) had a legitimate reason for stopping you?

Perceptions of legitimacy of search

Q. Do you think the police officers had a legitimate reason to search the vehicle (asked of drivers in traffic stops only)?

Q. Do you think that police officers had a legitimate reason to search you, frisk you, or pat you down?

Perceptions of police use of force

Q. Did you feel that this/these action(s) [used by police against you] was/were necessary?

Q. Did you feel any of the force used or force threatened against you was excessive?

Methodology

Data collection

The Police-Public Contact Survey (PPCS) is a supplement to the National Crime Victimization Survey (NCVS). The NCVS annually collects data on crime reported and not reported to the police against persons age 12 or older from a nationally representative sample of U.S. households. The sample includes persons living in group quarters (such as dormitories, rooming houses, and religious group dwellings) and excludes persons living in military barracks and institutional settings (such as correctional or hospital facilities) and the homeless. (For more information, see the *Survey Methodology* in *Criminal Victimization in the United States, 2008,* NCJ 231173, BJS website, May 2011.)

Since 1999, the PPCS has been administered every 3 years at the end of the NCVS interview to persons age 16 or older within households sampled for the NCVS. Proxy responders and those who complete the NCVS interview in a language other than English were not eligible to receive the PPCS.

The U.S. Census Bureau administered the 2011 PPCS questionnaire between July 1, 2011, and December 31, 2011, and processed the survey data. Respondents were provided a list of specific reasons for having contact with police and were asked if they had experienced any of those types of contacts during the prior 12 months. For example, persons interviewed in July 2011 were asked about contacts that occurred between August 2010 and July 2011. Persons who said they had a contact during 2011 were asked to describe the nature of the contact, and those who had more than one contact were asked about only their most recent contact during the period. To simplify the discussion of the findings, this report describes all contacts reported during the 12 months prior to the interviews as 2011 contacts.

PPCS nonrespondents consisted of persons whose household did not respond to the NCVS (NCVS household nonresponse), persons within an interviewed NCVS household who did not respond to the NCVS (NCVS person nonresponse), and persons who responded to the NCVS but did not complete the PPCS (PPCS person nonresponse). The NCVS household response rate was 89% and the person response rate was 88%. In 2011, PPCS interviews were obtained from 49,246 of the 62,280 individuals age 16 or older in the NCVS sample (79%). A total of 13,034 nonrespondents were excluded from the 2011 PPCS as noninterviews or as proxy interviews. Noninterviews (10,907) included respondents who were not available for the interview, those who refused to participate, and non-English-speaking respondents. (Unlike the NCVS interviews, PPCS interviews were conducted only in English.) The remaining 2,127 were proxy interviews representing household members who were unable to participate for physical, mental, or other reasons.

To produce national estimates on police-public contacts, sample weights were applied to the survey data so that the respondents represented the entire population, including the nonrespondents. After adjustment for nonresponse, the sample cases in 2011 were weighted to produce a national population estimate of 241,404,142 persons age 16 or older.

Despite the nonresponse adjustments, low overall response rates and response rates to particular survey items can still increase variance in these estimates and produce bias when the nonrespondents have characteristics that differ from the respondents. The Office of Management and Budget guidelines require a nonresponse bias study when the overall response rate is below 80%. The Bureau of Justice Statistics (BJS) and the Census Bureau compared the distributions of respondents as well as nonrespondents and nonresponse estimates for various household and demographic characteristics, and examined their impact on the national estimates produced for the 2011 PPCS. The study looked at household-level and person-level response rates and found some evidence of bias in the rates among blacks and persons of Hispanic origin. Blacks accounted for 12% of the U.S. population in 2011 but about 11% of PPCS respondents after weighting adjustments. Hispanics accounted for 14% of the U.S. population but about 12% of the PPCS respondents after weighting adjustments. Because the largest bias in person nonresponse was observed in the Hispanic origin characteristics, future iterations of the PPCS will address this issue by administering the survey in languages other than English and including Hispanic origin as a factor in the noninterview adjustment. Item nonresponse statistics were also computed for key survey questions from the PPCS, and no evidence of bias was found during the analysis.

Changes to the 2011 PPCS

Since its inception in 1996, the PPCS has captured information about in-person (i.e., face-to-face) contacts between police and the public. Telephone contacts were previously not included. The survey also excluded face-to-face interactions in which persons approached an officer or an officer initiated contact with them in a social setting or because their work brought them into regular contact. In March 2010, BJS hosted a series of meetings with subject-matter experts in the area of policing and police legitimacy to initiate discussion and work on substantive changes to the PPCS questionnaire. In 2011, based in part on these meetings, the PPCS was revised to expand the scope of the survey and to better capture contacts with police.

First, to determine if contact occurred and to enhance individuals' recollections about their interactions with police over a 12-month period, BJS implemented new screening procedures in the 2011 PPCS that describe a broad range of situations known to bring people in contact with police. Second, the scope of the PPCS was expanded to collect information about interactions that people had with the police that did not result in a face-to-face contact

(e.g., reporting a crime to the police by phone or email). Additionally, a new set of questions was added to the instrument to collect detailed information about requests for police assistance (e.g., reporting a crime or noncrime emergency) and contacts in which the police stopped someone in a public place or on the street but not in a motor vehicle (known as street stops).

These revisions, which included adding new questions and reordering existing questions, were significant when compared to the 2008 version of the questionnaire. To assess the impact of the survey redesign on trends in rates and types of contact, BJS administered a split-sample design in which a subset of the sample was interviewed using the 2008 version of the questionnaire, and the remaining sample was interviewed using the 2011 version. Based on the evaluation, it was determined that a 15/85 split would provide sufficient power to measure a 15% change in contact rate. In other words, about 85% of the 2011 sample was randomly assigned the revised questionnaire and the other 15% received the questionnaire designed for the 2008 survey. The Census Bureau completed interviews for 41,408 (79%) of the 52,529 residents who received the revised questionnaire.

The findings in this report are based on data collected from the revised questionnaire. An evaluation of the impact of the changes to the 2011 PPCS instrument on trends in contacts between the police and the public is underway, and the results of that assessment will be made available through the BJS website.

Standard error computations

When national estimates are derived from a sample, as is the case with the PPCS, caution must be taken when comparing one estimate to another estimate. Although one estimate may be larger than another, estimates based on a sample have some degree of sampling error. The sampling error of an estimate depends on several factors, including the amount of variation in the responses, the size of the sample, and the size of the subgroup for which the estimate is computed. When the sampling error around the estimates is taken into consideration, the estimates that appear different may, in fact, not be statistically different.

One measure of the sampling error associated with an estimate is the standard error. The standard error can vary from one estimate to the next. In general, for a given metric, an estimate with a smaller standard error provides a more reliable approximation of the true value than an estimate with a larger standard error. Estimates with relatively large standard errors are associated with less precision and reliability and should be interpreted with caution.

In order to generate standard errors around estimates from the PPCS, the Census Bureau produces generalized variance function (GVF) parameters for BJS. The GVFs take into account aspects of the NCVS complex sample design and represent the curve fitted to a selection of individual standard errors based on the Jackknife Repeated Replication technique. The GVF parameters were used to generate standard errors for each point estimate (i.e., numbers or percentages) in the report.

In this report, BJS conducted tests to determine whether differences in estimated numbers and percentages were statistically significant once sampling error was taken into account. Using statistical programs developed specifically for the NCVS, all comparisons in the text were tested for significance. The primary test procedure used was Student's t-statistic, which tests the difference between two sample estimates. To ensure that the observed differences between estimates were larger than might be expected due to sampling variation, the significance level was set at the 95% confidence level.

Data users can use the estimates and the standard errors of the estimates provided in this report to generate a confidence interval around the estimate as a measure of the margin of error. The following example illustrates how standard errors can be used to generate confidence intervals:

> According to the NCVS, in 2011, an estimated 88.2% of drivers stopped by police in traffic stops believed that the police behaved properly during the contact (see table 1). Using the GVFs, BJS determined that the estimate has a standard error of 1.13 (see appendix table 3). A confidence interval around the estimate was generated by multiplying the standard errors by ±1.96 (the t-score of a normal, two-tailed distribution that excludes 2.5% at either end of the distribution). Thus, the confidence interval around the estimate is 88.2 ± (1.13 X 1.96) or 86.1 to 90.4. In other words, if different samples using the same procedures were taken from the U.S. population in 2011, 95% of the time the percentage of stopped drivers who believed the police behaved properly would be between 86% and 90%.

In this report, BJS also calculated a coefficient of variation (CV) for all estimates, representing the ratio of the standard error to the estimate. CVs provide a measure of reliability and a means to compare the precision of estimates across measures with differing levels or metrics. In cases where the CV was greater than 50%, or the unweighted sample had 10 or fewer cases, the estimate was noted with a "!" symbol (interpret data with caution; estimate is based on 10 or fewer sample cases, or the coefficient of variation exceeds 50%).

Many of the variables examined in this report may be related to one another and to other variables not included in the analyses. Complex relationships among variables were not fully explored in this report and warrant more extensive analysis. Readers are cautioned not to draw causal inferences based on the results presented.

APPENDIX TABLE 1

Standard errors and estimates for figure 1: Perceptions that police behaved properly and respectfully during most recent contact with persons age 16 or older, by type of contact, 2011

Stop characteristic	Number of persons 16 or older	Standard error	
		Number	Percent
Any contact	62,936,500	1,581,523	~%
Involuntary contact	30,954,800	1,079,805	1.2%
Traffic stop	26,404,200	982,622	1.1
Driver thought police did not behave properly	2,547,600	218,913	0.7
Compliant filed			
Yes	110,900	31,184	1.2
No	2,436,700	212,649	1.5
Driver thought police were not respectful	2,371,700	208,937	0.7
Driver thought police were proper and respectful	22,808,700	899,273	1.1
Street stop	1,433,300	150,720	0.5
Person thought police did not behave properly	351,800	62,186	3.5
Compliant filed			
Yes	10,000	8,183	2.3
No	341,800	61,097	2.6
Person thought police were not respectful	327,700	59,542	3.4
Person thought police were proper and respectful	944,600	115,368	4.1
Arrest or other involuntary contact	3,117,300	249,752	0.7
Other contact	31,981,800	1,100,593	1.2%
Voluntary	24,227,400	932,916	1.3
Traffic accident	5,533,100	363,573	1.0
Anti-crime program participation	2,221,300	200,205	0.6

Note: Detail based on the most recent contact during the past 12 months. Detail may not sum to total due to missing data or categories that are not mutually exclusive. Estimates rounded to the nearest hundred.

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 2

Population of persons age 16 or older and driving population age 16 or older, by demographic characteristics, 2011

Demographic characteristics	Population age 16 or older	Driving population age 16 or older
Total	241,404,142	212,298,850
Sex		
Male	118,267,679	106,632,822
Female	123,136,463	105,666,027
Race/Hispanic origin		
White*	167,364,010	153,358,921
Black/African American*	27,763,474	21,322,976
Hispanic/Latino	31,240,097	25,495,436
American Indian/Alaska Native*	1,058,592	845,043
Asian/Native Hawaiian/ other Pacific Islander*	11,447,990	9,168,427
Two or more races*	2,529,979	2,108,046
Age		
16–17	8,060,403	4,323,648
18–24	28,743,383	23,714,718
25–34	41,829,412	38,016,545
35–44	40,680,390	37,756,084
45–54	44,353,446	41,172,146
55–64	37,837,219	34,884,444
65 or older	39,899,889	32,431,265

Note: See appendix table 3 for standard errors.

*Excludes persons of Hispanic or Latino origin.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 3

Standard errors for table 1: Involuntary contact with police among persons age 16 or older, by demographic characteristics and type of contact, 2011

	Street stops				Traffic stops			
			Percent of stopped persons				Percent of stopped drivers	
Demographic characteristics	Population age 16 or older	Percent of all persons	Total	Police behaved properly	Driving population age 16 or older	Percent of all drivers	Total	Police behaved properly
Total	22,320	0.1%	~%	3.9%	1,380,721	0.4%	~%	1.1%
Sex								
Male	1,968,304	0.1%	4.1%	4.6%	1,928,128	0.5%	1.6%	1.4%
Female	1,978,921	0.1	3.9	6.1	1,923,848	0.4	1.6	1.4
Race/Hispanic origin								
White	1,899,202	0.1%	4.1%	4.3%	1,960,731	0.4%	1.6%	1.2%
Black/African American	1,012,560	0.1	2.6	9.8 !	862,900	0.9	0.9	2.6
Hispanic/Latino	1,085,620	0.1	2.8	9.0	962,142	0.8	0.9	2.4
American Indian/Alaska Native	124,069	0.6 !	0.4 !	~ !	107,481	3.5	0.2	10.5
Asian/Native Hawaiian/other Pacific Islander	582,997	0.2 !	1.3 !	13.0 !	505,194	1.1	0.5	3.3
Two or more races	217,923	0.7 !	1.3 !	16.4 !	193,506	2.3	0.2	3.8
Age								
16–17	464,692	0.4%	2.1%	11.4%	309,377	1.4%	0.3%	4.0%
18–24	1,033,650	0.2	3.8	6.2	920,877	1.1	1.2	2.1
25–34	1,281,640	0.2	3.6	7.0	1,215,222	0.8	1.2	1.8
35–44	1,262,079	0.1	2.4	8.6	1,210,524	0.7	1.2	1.9
45–54	1,323,296	0.1	2.4	8.8	1,270,498	0.6	1.1	1.9
55–64	1,211,990	0.1	1.7	14.4 !	1,157,274	0.6	0.9	2.2
65 or older	1,248,572	0.1	1.7	13.6 !	1,109,568	0.5	0.7	2.3

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 4

Standard errors for table 2: Perception that reason for traffic stop was legitimate among drivers age 16 or older, by race or Hispanic origin of driver and reason for stop, 2011

	Percent of stopped drivers				
Reason for traffic stop	All	White	Black/AfricanAmerican	Hispanic/Latino	Other
Any reasons	~%	~%	~%	~%	~%
Police gave reason for the stop					
Speeding	1.6	1.8	3.2	0.5	4.2
Vehicle defect	1.0	1.1	2.4	0.3	2.9
Record check	0.8	0.9	2.1	0.2	2.4
Roadside sobriety check	0.2	0.3	0.3 !	0.1 !	0.7 !
Seatbelt or cell phone violation	0.6	0.7	1.4	0.2	2.1
Illegal turn or lane change	0.7	0.7	1.5	0.2	2.5
Stop sign/light violation	0.6	0.7	1.3	0.2	2.3
Other reason	0.6	0.6	1.3	0.2	1.7
Police did not give reason for the stop	0.4	0.4	1.2	0.1	1.6 !

	Percent reporting reason for stop was legitimate				
Reason for traffic stop	All	White	Black/African American	Hispanic/Latino	Other
Any reasons	1.4%	1.4%	3.2%	3.1%	3.8%
Police gave reason for the stop					
Speeding	1.5	1.5	4.4	3.7	4.5
Vehicle defect	2.6	2.7	6.0	6.1	8.2
Record check	3.0	3.5	5.6	8.0	9.4
Roadside sobriety check	6.8	6.4	~ !	24.6 !	32.5 !
Seatbelt or cell phone violation	3.5	3.7	9.9	8.8	12.7 !
Illegal turn or lane change	3.8	4.3	9.5	9.0	10.8
Stop sign/light violation	4.0	4.8	10.3	8.4	10.7
Other reason	4.7	5.4	9.0 !	9.9	15.0 !
Police did not give reason for the stop	5.7	7.2	11.3 !	10.6 !	17.3 !

! Interpret with caution. Estimate based on 10 or fewer cases or the coefficient of variation is greater than 50%.

~ Not applicable.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 5

Standard errors for table 3: Perception that reason for traffic stop was legitimate among drivers age 16 or older, by reason for stop and whether driver and officer were intra- or interracial, 2011

	Intraracial driver and officer		Interracial driver and officer	
Reason for traffic stop	Total stopped drivers	Reason for stop was legitimate	Total stopped drivers	Reason for stop was legitimate
All reasons	~%	1.5%	~%	2.3%
Police gave reason for the stop				
Speeding	1.9	1.6	2.5	2.6
Vehicle defect	1.1	3.0	1.7	4.4
Record check	0.9	3.9	1.3	5.0
Roadside sobriety check	0.3	7.7	0.2 !	25.8 !
Seatbelt or cell phone violation	0.7	3.8	1.1	6.5
Illegal turn or lane change	0.8	4.6	1.1	6.6
Stop sign/light violation	0.7	5.0	1.1	6.7
Other reason	0.6	5.9	1.0	7.6
Police did not give reason for the stop	0.4	7.9	0.7	9.9

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 6

Standard errors for table 4: Perception that reason for traffic stop was legitimate among drivers age 16 or older, by race and ethnicity of driver and officer and driver's perception that police behaved properly, 2011

		Percent of stopped drivers			
				Police behaved properly	
Race and ethnicity of driver and officer	Percent of all drivers	Total	Reason for stop was legitimate	Reason for stop was legitimate	Reason for stop was not legitimate
Total	0.4%	~%	1.4%	0.9%	2.7%
White driver	0.4%	~%	1.4%	1.0%	3.4%
White officer	0.4	1.5	1.5	1.1	3.6
Black/African American officer	0.1	0.6	4.6	2.4	12.2
Hispanic/Latino officer	0.1	0.5	5.6	2.1	11.9
Black/African American driver	0.9%	~%	3.2%	1.9%	5.1%
White officer	0.7	3.2	3.7	2.3	6.3
Black/African American officer	0.3	2.1	6.8	4.8	8.6
Hispanic/Latino officer	0.2	1.3	10.8	~	14.4 !
Hispanic/Latino driver	0.8%	~%	3.0%	1.8%	5.6%
White officer	0.6	3.3	3.6	2.2	6.7
Black/African American officer	0.1	1.0	12.7	7.8 !	26.0 !
Hispanic/Latino officer	0.3	2.3	5.8	3.8	13.2
Other driver	1.0%	~%	3.8%	2.8%	7.0%
White officer	0.9	4.1	4.1	2.9	8.9 !
Black/African American officer	0.1 !	1.3 !	20.3 !	27.5 !	23.6 !
Hispanic/Latino officer	0.2 !	1.8 !	12.9 !	~ !	~ !

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation was greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 7

Standard errors for table 5: Enforcement actions taken by police against drivers age 16 or older, by driver's demographic characteristics and perception that police behaved properly, 2011

	Ticketed			Warned			Allowed to proceed with no enforcement action		
		Percent of stopped drivers			Percent of stopped drivers			Percent of stopped drivers	
Race of driver	Percent of all drivers	Ticketed drivers	Police behaved properly	Percent of all drivers	Warned drivers	Police behaved properly	Percent of all drivers	Drivers with no enforcement	Police behaved properly
All drivers	0.3%	~%	1.5%	0.2%	~%	1.2%	0.1%	~%	2.5%
Sex									
Male	0.4%	2.0%	1.8%	0.3%	2.4%	1.6%	0.2%	3.2%	3.2%
Female	0.3	2.0	1.9	0.2	2.3	1.5	0.1	3.1	3.0
Race/Hispanic origin									
White	0.3%	2.0%	1.6%	0.2%	2.1%	1.3%	0.1%	3.0%	2.8%
Black/African American	0.7	1.2	3.4	0.4	1.2	3.7	0.3	2.1	5.7
Hispanic/Latino	0.6	1.2	2.9	0.4	1.2	3.2	0.2	1.8	6.8
Other	0.8	0.9	3.4	0.4	0.7	3.7	0.3	1.2	8.2
Age									
16–17	0.9%	0.3%	6.8%	0.9%	0.6%	4.7%	0.4% !	0.6%	~% !
18–24	0.8	1.5	2.7	0.5	1.6	2.6	0.4	2.5	5.1
25–34	0.6	1.6	2.4	0.4	1.8	1.9	0.2	2.2	5.1
35–44	0.5	1.5	2.5	0.4	1.7	2.3	0.2	2.2	5.3
45–54	0.4	1.4	2.5	0.3	1.6	2.5	0.2	2.4	4.5
55–64	0.4	1.1	2.9	0.3	1.3	2.5	0.2	2.0	4.8
65 or older	0.3	0.7	3.6	0.3	1.1	2.5	0.2	1.6	5.1

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 8

Standard errors for table 6: Stopped drivers age 16 or older who were ticketed, by race of officer and driver and driver's perception that police behaved properly, 2011

		Percent of stopped drivers		
			Police behaved properly	
Race of driver and officer	Percent of all drivers issued a ticket	Ticketed	Ticketed drivers	Drivers not ticketed
White driver	0.3%	1.8%	1.6%	1.4%
White officer	0.2	2.0	1.7	1.5
Black/African American officer	0.0	5.7	4.9	3.3
Hispanic/Latino officer	0.0	6.5	5.7	5.0
Black/African American driver	0.7%	3.3%	3.4%	3.4%
White officer	0.5	3.9	4.0	3.8
Black/African American officer	0.2	7.1	5.7	6.1
Hispanic/Latino officer	0.1	10.8	12.7 !	12.7 !
Hispanic/Latino driver	0.6%	3.3%	2.9%	3.4%
White officer	0.4	3.9	3.6	3.7
Black/African American officer	0.1 !	14.3 !	10.6 !	16.3 !
Hispanic/Latino officer	0.2	6.8	5.6	8.3
Other driver	0.8%	4.3%	3.4%	4.3%
White officer	0.7	4.9	3.8	4.8
Black/African American officer	0.1	12.8	21.1 !	~ !
Hispanic/Latino officer	0.2 !	~ !	~ !	~ !

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 9

Standard errors for table 7: Stopped drivers who were searched by police, by driver's demographic characteristics and perception that police behaved properly, 2011

Demographic characteristics	Percent of all drivers searched by police	Percent of stopped drivers		
		Searched	Police behaved properly	
			Searched drivers	Drivers not searched
Total	0.4%	~%	5.8%	1.2%
Sex				
Male	0.6%	4.8%	6.5%	1.4%
Female	0.5	4.5	10.6	1.4
Race/Hispanic origin				
White	0.4%	5.5%	7.9%	1.2%
Black/African American	1.4	4.4	10.9	2.8
Hispanic/Latino	1.4	4.4	10.7	2.5
Other	1.6	2.6	18.4 !	2.8
Age				
16–17	1.5% !	0.8% !	~% !	3.9%
18–34	0.7	5.5	7.3	1.6
35–54	0.6	5.1	8.7	1.6
55 or older	0.5	2.6	18.6 !	1.8

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 10

Standard errors for table 8: Stopped drivers age 16 or older who had their person or vehicle searched by police, by driver's perception that police behaved properly, 2011

Stop characteristics	Percent of stopped drivers	
	Total	Police behaved properly
All stops	~%	1.1%
Police searched driver or vehicle		
No	0.7%	1.1%
Yes	0.4	5.4
	Percent of searched drivers	
Police asked permission to search		
No	5.3%	8.0%
Yes	5.4	6.2
Driver thought search was legitimate		
No	5.4%	6.7%
Yes	5.3	5.8

~ Not applicable.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 11

Standard errors for table 9: Type of force used or threatened by police against stopped drivers, by driver's perception that police behaved properly, 2011

Type of force	Percent of stopped drivers	
	Total	Police behaved properly
All stops	~%	1.1%
Force used		
Shouting or cursing	0.2%	6.8% !
Verbal threats	0.4	5.6
Physical force	0.3	7.7
	Percent of drivers who experienced force	
Driver thought use of force was necessary		
No	3.9%	4.7%
Yes	3.2	6.6
Driver thought use of force was excessive		
No	4.2%	4.9%
Yes	4.0	4.2 !

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 12

Standard errors for table 10: Reason for street stops involving persons age 16 or older, by perceptions that stop was legitimate and police behaved properly, 2011

Reason for street stop	Percent of all persons in a street stop	Percent of stopped persons		
		Total	Reason for stop was legitimate	Police behaved properly
Any reasons	0.1%	~%	4.1%	3.9%
Suspected of something or matched description of someone police were looking for	0.0	4.1	6.0	5.8
Police were seeking information about another person or investigating a crime	0.0	2.8	5.1	5.7
Police were providing a service	~	1.9	7.8	5.3
No reason given by police	0.0	2.9	8.1 !	9.0
Unknown	0.0	3.2	7.7	7.6

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

APPENDIX TABLE 13

Standard errors for table 11: Characteristics of persons age 16 or older involved in street stops and outcomes of the stop, by perceptions that police behaved properly, 2011

Stop characteristics	Percent of stopped persons	
	Total	Police behaved properly
All stops	~%	3.9%
Person thought officer and respondent were the same race or Hispanic origin		
No	3.4%	7.7%
Yes	4.2	4.5
Unknown	3.4	7.7
Person thought reason for stop was legitimate		
No	4.0%	6.2%
Yes	4.1	3.2
Searched or frisked		
No	3.6%	4.0%
Yes	3.1	8.4
Person thought search was legitimate		
No	5.0%	9.6%
Yes	4.3	8.2
Force used		
No	3.8%	3.6%
Yes	3.5	6.7
Person thought force was excessive		
No	5.4%	8.7%
Yes	3.5	0.0 !

~ Not applicable.

! Interpret with caution. Estimate based on 10 or fewer sample cases or the coefficient of variation is greater than 50%.

Source: Bureau of Justice Statistics, National Crime Victimization Survey, Police-Public Contact Survey, 2011.

The Bureau of Justice Statistics, located in the Office of Justice Programs, U.S. Department of Justice, collects, analyses, and disseminates statistical information on crime, criminal offenders, victims of crime, and the operation of justice systems at all levels of government. William J. Sabol is acting director.

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Policing, Danger Narratives, and Routine Traffic Stops

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POLICING, DANGER NARRATIVES, AND ROUTINE TRAFFIC STOPS

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This Article presents findings from the largest and most comprehensive study to date on violence against the police during traffic stops. Every year, police officers conduct tens of millions of traffic stops. Many of these stops are entirely unremarkable—so much so that they may be fairly described as routine. Nonetheless, the narrative that routine traffic stops are fraught with grave and unpredictable danger to the police permeates police training and animates Fourth Amendment doctrine. This Article challenges this dominant danger narrative and its centrality within key institutions that regulate the police.

The presented study is the first to offer an estimate for the danger rates of routine traffic stops to law enforcement officers. I reviewed a comprehensive dataset of thousands of traffic stops that resulted in violence against officers across more than 200 law enforcement agencies in Florida over a 10-year period. The findings reveal that violence against officers was rare and that incidents that do involve violence are typically low risk and do not involve weapons. Under a conservative estimate, the rate for a felonious killing of an officer during a routine traffic stop was only 1 in every 6.5 million stops, the rate for an assault resulting in serious injury to an officer was only 1 in every 361,111 stops, and the rate for an assault against officers (whether it results in injury or not) was only 1 in every 6,959 stops.

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This Article is also the first to offer a comprehensive typology of violence against the police during traffic stops. The typology indicates that a narrow set of observable contextual factors precedes most of this violence—most commonly, signs of flight or intoxication. The typology further reveals important qualitative differences regarding violence during traffic stops initiated for only traffic enforcement versus criminal enforcement.

The study has significant implications for law enforcement agencies and courts. The findings and typology have the potential to inform police training and prompt questions about whether greater invocation of police authority during routine stops for traffic violations undermines, rather than advances, both officer and civilian safety. The findings also lay an early empirical foundation for rethinking fundamental assumptions about officer safety and routine traffic stops in Fourth Amendment doctrine. This Article ultimately urges institutional actors that regulate the police to abandon oversimplified danger narratives surrounding routine traffic stops in favor of context-rich archetypes that more accurately reflect the risks and costs of policing during these stops.

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INTRODUCTION

"Any vehicle encounter has the potential to be dangerous, so mitigate your risks on every stop."[1]

The traffic stop is the most common form of civilian interaction with the police.[2] Every year, police conduct tens of millions of traffic stops.[3] The dominant narrative in policing is that each one of these stops is not just highly dangerous but also potentially fatal.[4] Each stop is unique, and "there is no such thing as a routine traffic stop."[5]

Over the past few decades, police authority to question drivers and passengers, order them out of cars, and conduct various searches and seizures

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1. Amaury Murgado, *How to Approach Traffic Stops*, POLICE MAG. (Nov. 26, 2012), http://www.policemag.com/channel/careers-training/articles/2012/11/traffic-stops.aspx [https://perma.cc/MR8H-QJ6F] (offering guidance on best practices on how officers should approach routine traffic stops).

2. *Traffic Stops*, BUREAU JUST. STAT., http://www.bjs.gov/index.cfm?ty=tp&tid=702 [https://perma.cc/C98Y-UNM5] (noting that "[t]he most common reason for contact with the police is being a driver in a traffic stop").

3. ROBERT C. LAFOUNTAIN ET AL., NAT'L CTR. FOR ST. CTS., EXAMINING THE WORK OF STATE COURTS: AN ANALYSIS OF 2010 STATE COURT CASELOADS (2012), http://www.courtstatistics.org/~/media/Microsites/Files/CSP/DATA%20PDF/CSP_DEC.ashx. [https://perma.cc/79FQ-SF9Y]; *2015 Statewide Traffic Case Loads and Rates – Trial Courts*, NAT'L CTR. FOR ST. CTS., http://www.ncsc.org/Sitecore/Content/Microsites/PopUp/Home/CSP/CSP_Traffic [https://perma.cc/49U4-TS3D] (showing that states had at least 43 million incoming traffic cases in 2016).

4. *See, e.g.,* Murgado, *supra* note 1 ("Any vehicle encounter has the potential to be dangerous, so mitigate your risks on every stop."); Chelsea Whitaker, *The Routine Traffic Stop*, LAWOFFICER (Nov. 21, 2016), http://lawofficer.com/special-assignment-teams/officer-safety/the-routine-traffic-stop/ [https://perma.cc/X2TE-K4FQ] ("There isn't a more dangerous aspect of policing than traffic stops."); John Wills, *Routine Traffic Stops*, OFFICER.COM (June 3, 2013), https://www.officer.com/on-the-street/body-armor-protection/article/10952972/routine-traffic-stops [https://perma.cc/CC9C-YYGT] ("Pulling over a vehicle is inherently dangerous.").

5. *See, e.g.,* Craig McMorris, *Police: No Such Thing as a Routine Traffic Stop*, FOX CAROLINA (Mar. 23, 2012, 11:26 AM) (updated Apr. 20, 2012, 11:32 AM), http://www.foxcarolina.com/story/17239535/police-say-no-such-thing-as-routine-traffic-stop (on file with the *Michigan Law Review*) (quoting one police sergeant who supervises road patrol officers as saying "officers are taught from day one that there is no such thing as a routine traffic stop"); Whitaker, *supra* note 4 ("There is no such thing as a routine traffic stop."); Wills, *supra* note 4 ("There is no such thing as a routine traffic stop.").

has expanded significantly.[6] The idea that routine traffic stops[7] are fraught with grave and unpredictable danger to the police has animated this expansion. For instance, in several Fourth Amendment cases involving traffic stops, the U.S. Supreme Court has deferred to law enforcement based on officer safety concerns, stressing that officers must be empowered during these stops to take "unquestioned command of the situation."[8]

The idea that routine traffic stops pose grave and unpredictable danger to the police also influences how officers are trained to approach and act during these stops. Police academies regularly show officer trainees videos of the most extreme cases of violence against officers during routine traffic stops[9] in order to stress that mundane police work can quickly turn into a deadly situation if they become complacent on the scene or hesitate to use force.[10] With technological advances, these violent examples are also included as scenarios in virtual simulation programs that train officers in how to protect themselves during routine traffic stops.[11] Video clips and simulations

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6. *See generally* Lewis R. Katz, *"Lonesome Road": Driving Without the Fourth Amendment*, 36 SEATTLE U. L. REV. 1413 (2013); Wayne R. LaFave, *The "Routine Traffic Stop" from Start to Finish: Too Much "Routine," Not Enough Fourth Amendment*, 102 MICH. L. REV. 1843 (2004).

7. In this Article, I use the term "routine traffic stops" to refer to motor vehicle stops for purposes of only enforcing traffic violations and not enforcing the criminal law beyond a traffic violation.

8. *See, e.g.*, Arizona v. Johnson, 555 U.S. 323, 330 (2009) (quoting Maryland v. Wilson, 519 U.S. 408, 414 (1997)); *see also* Michigan v. Long, 463 U.S. 1032, 1048 (1983) (recognizing the Court's "view of the danger presented to police officers in 'traffic stop' and automobile situations"). For a broader discussion of courts' deference to law enforcement and police expertise, see generally Anna Lvovsky, *The Judicial Presumption of Police Expertise*, 130 HARV. L. REV. 1995 (2017). *See also* Alice Ristroph, *The Constitution of Police Violence*, 64 UCLA L. REV. 1182, 1210 (2017) ("[C]ourts defer almost invariably to officers' later accounts of their perceptions of danger or resistance.").

9. Thomas Lake, *The Endless Death of Kyle Dinkheller*, CNN: STATE (Aug. 2017), http://www.cnn.com/interactive/2017/politics/state/kyle-dinkheller-police-video/ [https://perma.cc/4HKJ-MU74]; Leon Neyfakh, *How Police Learn When to Shoot*, SLATE (May 21, 2015, 2:43 AM), http://www.slate.com/articles/news_and_politics/crime/2015/05/police_shootings_the_grim_videos_cops_watch_of_their_colleagues_being_killed.html [https://perma.cc/J5SZ-S4ZN]; Peter Robison, *Inside the School Teaching Cops When It's OK to Kill*, BLOOMBERG (July 1, 2015, 2:39 AM), https://www.bloomberg.com/news/articles/2015-07-01/the-policeman-s-id [https://perma.cc/6NKD-U6SV].

10. Seth Stoughton, *Law Enforcement's "Warrior" Problem*, 128 HARV. L. REV. F. 225, 227 (2015); Seth W. Stoughton, *Police Body-Worn Cameras*, 96 N.C. L. REV. 1363, 1397–98 (2018) [hereinafter Stoughton, *Police Body-Worn Cameras*] (discussing "officer survival" videos "which attempt to remind officers of the dangers of complacency by showing officers being brutally attacked, disarmed, or killed"); *id.* at 1297–98 nn.137–44 (providing examples of "officer survival videos"); Cammy Clark, *Training Helps Keys Cops Make Better Split-Second Decisions About When to Shoot*, MIAMI HERALD (Nov. 23, 2014, 1:20 PM), http://www.miamiherald.com/news/local/community/florida-keys/article4077525.html [https://perma.cc/7MYF-4QWA].

11. Clark, *supra* note 10; *see, e.g.*, Sharon E. Crawford, *GSP Using Simulator to Train Officers for Lethal Encounters*, MACON TELEGRAPH, May 17, 2002, at B3.

make these extreme cases of violence all the more real for officers and define how they come to perceive the dangers of the routine traffic stops that they eventually conduct.[12]

The narrative that routine traffic stops are fraught with danger to the police is longstanding.[13] But as this Article explains, this narrative finds little support in existing studies or data.[14] One key shortcoming of leading sources is that they are largely devoid of context.[15] These sources provide little to no insight into the sequences, patterns, or trends connected to this violence.[16] They also offer no information on the contextual factors that precede this violence or the points of the traffic stop in which violence tends to occur. Given how little we know, it is not surprising that the most violent and extreme cases come to define the narrative surrounding routine traffic stops within key institutions that regulate the police (for instance, law enforcement agencies, courts, and legislatures).

To narrow this knowledge gap, I undertook the largest and most comprehensive study to date on violence against the police during routine traffic stops: defined in this Article as motor vehicle stops initiated only to enforce traffic violations. The study is the only qualitative study that systematically examines sequences, patterns, and trends surrounding this violence. Drawing on methods from the field of criminology, I gathered and analyzed incident narratives from a comprehensive sample of over 4,200 cases of violence against officers during traffic stops across more than 220 law enforcement agencies in the state of Florida over a 10-year period.[17] This study is the first to offer an informed estimate of the danger rate that police officers actually face during routine traffic stops for traffic violations.

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12. Neyfakh, *supra* note 9. Relevant to this point is the "availability heuristic" concept. *See* Christine Jolls, Cass R. Sunstein & Richard Thaler, *A Behavioral Approach to Law and Economics*, 50 STAN. L. REV. 1471, 1477 (1998) (defining the availability heuristic as a rule of thumb "in which the frequency of some event is estimated by judging how easy it is to recall other instances of this type" and noting that the heuristic leads "us to erroneous conclusions"); Andrew E. Taslitz, *Police Are People Too: Cognitive Obstacles to, and Opportunities for, Police Getting the Individualized Suspicion Judgment Right*, 8 OHIO ST. J. CRIM. L. 7, 44 (2010) ("Police may also fall victim to the 'availability heuristic,' judging an event's probability based on what images or data are most readily available in the individual's memory."). *See generally* Amos Tversky & Daniel Kahneman, *Availability: A Heuristic for Judging Frequency and Probability*, in JUDGMENT UNDER UNCERTAINTY: HEURISTICS AND BIASES 163 (Daniel Kahneman et al. eds., 1982) (discussing the availability heuristic).

13. *See, e.g.*, Long, 463 U.S. at 1048 (1983) (recognizing the Court's view "of the danger presented to police officers in 'traffic stop' and automobile situations"); CHARLES REMSBERG, THE TACTICAL EDGE 271 (1986) ("The fact is that of officers who die making vehicle stops, *MOST die making so-called LOW-RISK stops for MISDEMEANOR violations*."); *see also* sources cited *supra* notes 9–10 (discussing how the danger narrative surrounding traffic stops persists today).

14. *See infra* Part I.

15. *See infra* Part I.

16. *See infra* Part I.

17. I will provide a more detailed description of the study methodology *infra* in Part II.

To summarize, the findings do not support the dominant danger narrative surrounding routine traffic stops. Based on a conservative estimate, I found that the rate for a felonious killing of an officer during a routine traffic stop for a traffic violation was only 1 in every 6.5 million stops.[18] The rate for an assault that results in serious injury[19] to an officer was only 1 in every 361,111 stops. Finally, the rate for an assault (whether it results in officer injury or not) was only 1 in every 6,959 stops. Less conservative estimates suggest that these rates may be much lower.[20]

In addition, the vast majority (over 98%) of the evaluated cases in the study resulted in no or minor injuries to the officers. Further, only a very small percentage of cases (about 3%) involved violence against officers in which a gun or knife was used or found at the scene, and the overwhelming majority of those cases resulted in no or minor injuries to an officer.[21] Less than 1% of the evaluated cases involved guns or knives and resulted in serious injury to or the felonious killing of an officer.[22]

The study also identified that routine traffic stops have a different risk profile than criminal enforcement stops: defined in this Article as stops initiated to investigate or enforce the criminal law beyond a traffic violation.[23] The study is the first to systematically examine how violence against the police may differ within these stop categories. I found that the most common weapons used to assault officers during routine traffic stops were "personal weapons"—namely, a driver's or passenger's hands, fists, or feet.[24] Converse-

18. *See infra* Section III.C (presenting danger ratios).

19. The Florida Department of Law Enforcement's *Uniform Crime Reports Guide Manual* defines "serious injury" as "injury so severe that it results in disablement or disfigurement. . . . Examples of serious injury include broken bones, loss of teeth, lacerations so severe that stiches are needed, internal injuries, injuries resulting in paralysis or the deprivation of a limb/body part, loss of consciousness, etc." FLA. DEP'T OF LAW ENF'T, UNIFORM CRIME REPORTS GUIDE MANUAL 27 (2017), https://www.fdle.state.fl.us/FSAC/UCR/UCRGuideManual.aspx [https://perma.cc/BUA4-3BQG].

20. *See infra* Section III.C (presenting danger ratios).

21. *See infra* Section III.B.

22. *See infra* Section III.B.

23. Here, I acknowledge that the line between traffic stops and criminal enforcement stops is not always clear. In fact, in 1974, Florida decriminalized the bulk of minor traffic offenses as civil violations. FLA. STAT. ANN. § 316.655 (West 2014) (noting that the bulk of traffic violations are civil infractions); *see also* Jordan Blair Woods, *Decriminalization, Police Authority, and Routine Traffic Stops*, 62 UCLA L. REV 672, 698 (2015). Florida, however, left a small group of more serious traffic violations classified as criminal traffic violations. Examples include driving while under the influence, reckless driving, leaving the scene of an accident, fleeing from a police officer, racing, and not having a valid license or registration. FLA. STAT. ANN. §§ 316.027, .191–.193; .1935, 322.34 (West 2014).

24. *See infra* Section III.B. For the purposes of the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting Program, "personal weapons" are defined as "hands, fists, feet, arms, teeth, etc."). U.S. FED. BUREAU OF INVESTIGATION, CRIMINAL JUSTICE INFO. SERVICES DIV., UNIF. CRIME REPORTING PROGRAM, 2019 NATIONAL INCIDENT-BASED REPORTING

ly, the most common weapon used to assault officers during criminal enforcement stops was the motor vehicle itself (for instance, using the car to run over an officer).

To enhance our contextual understanding of this violence, this Article also draws on qualitative methods to offer the first comprehensive typology of major traffic stop scenarios that escalate into violence against the police.[25] In short, four variables preceded the violence in most (just under 94%) of the evaluated cases: (1) the encounter resulted from a criminal enforcement stop rather than a routine traffic stop; (2) the driver refused to submit to the encounter, either by refusing to pull over or by fleeing, on foot or in the vehicle, after initially pulling over; (3) the officer reported noticing clear signs of intoxication upon initial contact with the driver or passenger; or (4) the officer invoked their authority during the stop in some way beyond asking for basic information, requesting documentation, or running a records check—for instance, ordering drivers out of the car or placing their hands on the drivers.[26] Notably, only a very small percentage of violence against the police (just over 3%) involved violence that was random or unprovoked and was not preceded by one of these variables.[27] Only a handful of those cases involved guns or knives.

In enhancing our contextual understanding of this violence, this Article contributes to several discussions about policing in legal scholarship. To begin, the study provides further empirical support for the idea that we lack sufficient context-rich information to effectively regulate the police.[28] In so doing, it further establishes that this knowledge gap fosters inconsistencies between how key institutional actors that regulate the police perceive everyday police work and how everyday police work unfolds on the ground.[29] As Rachel Harmon explains, evaluating the true costs of policing requires us to

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SYSTEM USER MANUAL 92 (2018), https://ucr.fbi.gov/nibrs/nibrs-user-manual [https://perma.cc/74ZH-243L].

25. The typology is structured around a hierarchy of observable contextual factors that preceded the violence in the evaluated cases, as the routine traffic stop unfolded into its major stages (its inception, during its course, and its conclusion). *See infra* Part IV.

26. *See infra* Part IV.

27. As discussed *infra* in Part IV, the remaining 3% of cases were situations involving bystander perpetrators of violence or situations in which the violence against officers occurred after the drivers or passengers had been apprehended by officers (for instance, the violence occurred at the police station or DUI testing center).

28. *See* Rachel Harmon, *Why Do We (Still) Lack Data on Policing?*, 96 MARQ. L. REV. 1119, 1119–20 (2013); *see also* Joanna C. Schwartz, *How Governments Pay: Lawsuits, Budgets, and Police Reform*, 63 UCLA L. REV. 1144, 1196–97 (2016) ("[F]or decades, commentators have noted with concern the lack of data collected about officer uses of force, civilian complaints, and other evidence of misconduct.").

29. *See generally, e.g.*, Brandon Garrett & Seth Stoughton, *A Tactical Fourth Amendment*, 103 VA. L. REV. 211 (2017) (advocating for a "tactical Fourth Amendment"); Seth W. Stoughton, *Policing Facts*, 88 TUL. L. REV. 847 (2014) (discussing problems when the U.S. Supreme Court grounds its justifications for constitutional norms on inaccurate factual or evidentiary grounds).

narrow these knowledge gaps, and that, in turn, requires new types of data collection.[30] It is especially important to narrow the knowledge gap involving violence against the police during routine traffic stops because of the serious consequences that stopped drivers and passengers may face (e.g., officer force, arrest, conviction, or incarceration) for even the most minor assault against an officer (e.g., nudging or slapping an officer's hand).

The study presented in this Article embodies this type of needed research in at least three important ways. First, the study shows that the dominant danger narrative surrounding routine traffic stops has many potential costs. Specifically, it facilitates a mindset of fear that can undermine the ability of police to develop effective de-escalation measures in the specific situations in which the findings suggest that most of this violence occurs.[31] In addition, this danger narrative may instigate avoidable and unnecessary conflicts during routine traffic stops that undermine both officer and civilian safety.[32]

Second, the study illustrates how the dominant danger narrative is a vastly oversimplified archetype and that there is a need for new theories and archetypes to accurately account for violence against the police during routine traffic stops. On a broader level, violence against the police—both during routine traffic stops and in other policing contexts—remains underexplored and undertheorized. Rather, existing scholarship largely focuses on *civilians* as the *targets* of police violence.[33] To the extent that policing law and policy rest on nonempirical assumptions and myths about officer safety, it is necessary to pay greater attention to how assumptions about police dangerousness shape the legal and desirable scope of police powers. The findings

30. Harmon, *supra* note 28, at 1120–21.

31. *See infra* Section V.A.

32. *See infra* Section V.A.

33. *Cf.* Illya Lichtenberg, *The Dangers of Warrant Execution in a Suspect's Home: Does an Empirical Justification Exist for the Protective Sweep Doctrine?*, 54 SANTA CLARA L. REV. 623, 630 (2014) ("[T]he application of research toward the violent victimization of the police in a context specific to the Fourth Amendment has only recently been examined."). Several criminological studies in the past three decades, however, have broadly examined violence against police officers, especially with regard to the felonious killings of officers. *See generally, e.g.*, JODI M. BROWN & PATRICK A. LANGAN, U.S. DEPT. OF JUST., POLICING AND HOMICIDE, 1976-98: JUSTIFIABLE HOMICIDE BY POLICE, POLICE OFFICERS MURDERED BY FELONS (2001); ROBERT J. KAMINSKI, A DESCRIPTIVE ANALYSIS OF FOOT PURSUITS IN THE LOS ANGELES COUNTY SHERIFF'S DEPARTMENT (2010); Shannon Bohrer et al., *Establishing a Foot Pursuit Policy*, FBI L. ENFORCEMENT BULL., May 2000, at 10; Steven G. Brandl, *In the Line of Duty: A Descriptive Analysis of Police Assaults and Accidents*, 24 J. CRIM. JUST. 255 (1996); Michele W. Covington et al., *Battered Police: Risk Factors for Violence Against Law Enforcement Officers*, 29 VIOLENCE & VICTIMS 34 (2014); Robert E. Crew, Jr., *An Effective Strategy for Hot Pursuit: Some Evidence from Houston*, 11 AM. J. POLICE, no. 3, 1992, at 89; Robert J. Kaminski, *Police Foot Pursuits and Officer Safety*, LAW ENFORCEMENT EXECUTIVE F., Mar. 2007, at 59; ROBERT J. KAMINSKI, THE MURDER OF POLICE OFFICERS (Marilyn McShane & Frank P. Williams III eds., 2004); Rebecca Reviere & Vernetta D. Young, *Mortality of Police Officers: Comparisons by Length of Time on the Force*, 13 AM. J. POLICE, no. 1, 1994, at 51; William Wilbanks, *Cops Killed and Cop-Killers: An Historical Perspective*, 13 AM. J. POLICE, no. 1, 1994, at 31.

and provisional typology move the conversation in this direction and lay an early empirical approach and foundation for generating new theories and testable hypotheses about this violence against the police during routine traffic stops as well as other policing contexts.[34] Put another way, the study findings and provisional typology broaden avenues for critiquing police power by considering *police officers* as the *targets* of civilian violence.

Both of these contributions are pertinent in the current political moment involving policing as well as for police reform moving ahead. On one hand, a long line of legal scholarship describes how pervasive police practices of racial profiling during routine traffic stops have taken, and continue to take, a harsh toll on minority drivers and passengers.[35] Several recently publicized cases and public protests have called attention to situations in which routine traffic stops escalate into fatal police shootings of unarmed people of color.[36] On the other hand, the former Obama Administration's more ag-

34. *See infra* Section V.C.

35. *See* CHARLES R. EPP ET AL., PULLED OVER: HOW POLICE STOPS DEFINE RACE AND CITIZENSHIP (2014); Mario L. Barnes & Robert S. Chang, *Analyzing Stops, Citations, and Searches in Washington and Beyond*, 35 SEATTLE U. L. REV. 673 (2012); Devon W. Carbado, *(E)Racing the Fourth Amendment*, 100 MICH. L. REV. 946 (2002); Devon W. Carbado, *From Stopping Black People to Killing Black People: The Fourth Amendment Pathways to Police Violence*, 105 CALIF. L. REV. 125, 149–62 (2017) [hereinafter Carbado, *From Stopping Black People to Killing Black People*]; Devon W. Carbado & Cheryl I. Harris, *Undocumented Criminal Procedure*, 58 UCLA L. REV. 1543 (2011); Angela J. Davis, *Race, Cops, and Traffic Stops*, 51 U. MIAMI L. REV. 425 (1997); Samuel R. Gross & Katherine Y. Barnes, *Road Work: Racial Profiling and Drug Interdiction on the Highway*, 101 MICH. L. REV. 651 (2002); David A. Harris, Essay, *"Driving While Black" and All Other Traffic Offenses: The Supreme Court and Pretextual Stops*, 87 J. CRIM. L. & CRIMINOLOGY 544 (1997) [hereinafter Harris, *"Driving While Black"*]; Elizabeth E. Joh, Essay, *Discretionless Policing: Technology and the Fourth Amendment*, 95 CALIF. L. REV. 199, 209 (2007); Kevin R. Johnson, Essay, *How Racial Profiling in America Became the Law of the Land:* United States v. Brignoni-Ponce *and* Whren v. United States *and the Need for Truly Rebellious Lawyering*, 98 GEO. L.J. 1005, 1009–45 (2010); Nancy Leong, *The Open Road and the Traffic Stop: Narratives and Counter-Narratives of the American Dream*, 64 FLA. L. REV. 305, 308 (2012); Tracey Maclin, *Race and the Fourth Amendment*, 51 VAND. L. REV. 333 (1998); L. Song Richardson, Response, *Implicit Racial Bias and the Perpetrator Perspective: A Response to* Reasonable but Unconstitutional, 83 GEO. WASH. L. REV. 1008 (2015); David A. Sklansky, *Traffic Stops, Minority Motorists, and the Future of the Fourth Amendment*, 1997 SUP. CT. REV. 271, 274 n.13; Nirej Sekhon, *Blue on Black: An Empirical Assessment of Police Shootings*, 54 AM. CRIM. L. REV. 189 (2017).

36. *See* Eric. J. Miller, *Encountering Resistance: Contesting Policing and Procedural Justice*, 2016 U. CHI. LEGAL F. 295, 296; *see also* Jess Bidgood, *No Third Trial for Ex-Officer Who Killed Cincinnati Driver*, N.Y. TIMES (July 18, 2017), https://www.nytimes.com/2017/07/18/us/police-shooting-ray-tensing-cincinnati.html (on file with the *Michigan Law Review*) (discussing the fatal police shooting of Sam DuBose); Bill Kirkos & Ralph Ellis, *Officer Who Shot Philando Castile: "I Thought I Was Going to Die,"* CNN (June 9, 2017, 10:50 PM), http://www.cnn.com/2017/06/09/us/philando-castile-officer-trial/index.html [https://perma.cc/X385-5HMB?type=image] (discussing the fatal police shooting of Philando Castile); Hayden Mitman, *David Jones Shooting Protesters Issue Demands to Police, Mayor*, METRO (July 28, 2017), http://www.metro.us/news/local-news/philadelphia/david-jones-shooting-protesters-issue-demands-to-police-mayor [https://perma.cc/66S4-J45C?type=image] (discussing the fatal police shooting of David Jones).

gressive stance toward police misconduct and abuse has led some commentators to argue that there is currently a "war on cops" that undermines both officer and civilian safety.[37] Consistent with this idea, Former Attorney General Jeff Sessions ordered the Department of Justice to conduct a sweeping review of DOJ-initiated reform agreements with troubled police forces nationwide.[38] This move is part of the Trump Administration's broader agenda to promote "officer safety and morale while fighting serious crime."[39] This Article intervenes in this debate by showing how laws, policies, and doctrine that rest on nonempirical assumptions about officer safety do not help to mediate these tensions and may even exacerbate them.

Third and finally, this Article informs scholarly discussions about which institutional actors are in the best position to regulate the police and to use data when doing so. Some scholars have discussed the shortcomings of courts and the limitations of contemporary constitutional frameworks to effectively regulate the police.[40] They have also expressed skepticism about the institutional competency of courts to create and use data in ways that appropriately regulate the police.[41] Other scholars have advocated for courts to make greater use of empirical data when deciding cases involving police conduct and adopt a more favorable outlook on the institutional capacity of courts to accomplish this task.[42] From this perspective, improving how

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These cases often go unprosecuted. *See* Kate Levine, *How We Prosecute the Police*, 104 GEO. L.J. 745, 763 (2016) ("Prosecutors decline to charge officers who kill (often unarmed) suspects at an extremely high rate."). *See generally* Kate Levine, *Police Suspects*, 116 COLUM. L. REV. 1197 (2016) (describing the ways in which criminal procedure rules advantage police suspects).

37. *See generally, e.g.*, HEATHER MAC DONALD, THE WAR ON COPS (2016); Bianca Padró Ocasio, *Police Group Director: Obama Caused a "War on Cops,"* POLITICO (July 8, 2016, 11:19 AM), http://www.politico.com/story/2016/07/obama-war-on-cops-police-advocacy-group-225291 [https://perma.cc/S65Q-LYX4]. Relatedly, some critics have argued that heightened scrutiny of the police increases crime rates by causing police officers to be less aggressive in enforcing the law. *See* Stephen Rushin & Griffin Edwards, *De-Policing*, 102 CORNELL L. REV. 721, 731–37 (2017) (discussing different versions of the "de-policing" hypothesis).

38. Sari Horwitz et al., *Sessions Orders Justice Department to Review All Police Reform Agreements*, WASH. POST (Apr. 3, 2017), https://www.washingtonpost.com/world/national-security/sessions-orders-justice-department-to-review-all-police-reform-agreements/2017/04/03/ba934058-18bd-11e7-9887-1a5314b56a08_story.html?utm_term=.7f27707d9b39 [https://perma.cc/2S94-KJCK].

39. *Id.*

40. *See, e.g.*, Barry Friedman & Maria Ponomarenko, *Democratic Policing*, 90 N.Y.U. L. REV. 1827, 1882–83 (2015); Rachel A. Harmon, *The Problem of Policing*, 110 MICH. L. REV. 761, 762–63 (2012); Eric J. Miller, *Role-Based Policing: Restraining Police Conduct "Outside the Legitimate Investigative Sphere,"* 94 CALIF. L. REV. 617, 621 (2006); John Rappaport, *Second-Order Regulation of Law Enforcement*, 103 CALIF. L. REV. 205 (2015). In prior work, I have shared this skepticism about relying too much on constitutional law interventions to regulate the police. Woods, *supra* note 23, at 709–10.

41. *See, e.g.*, Harmon, *supra* note 40, at 773–74.

42. *See, e.g.*, Andrew Manuel Crespo, *Systemic Facts: Toward Institutional Awareness in Criminal Courts*, 129 HARV. L. REV. 2049, 2055–56 (2016); Tracey L. Meares, *Three Objections to the Use of Empiricism in Criminal Law and Procedure—And Three Answers*, 2002 U. ILL. L. REV. 851, 873; Tracey L. Meares & Bernard E. Harcourt, *Foreword: Transparent Adjudication*

courts regulate the police requires new forms of partnerships to equip judges with the type of empirical expertise that can inform judges' decisions.[43]

The findings presented in this Article have much to offer all perspectives in this scholarly conversation. With regard to nonconstitutional interventions, the greater contextual understanding of violence against the police that the study offers can inform how law enforcement agencies train officers to approach and act during routine traffic stops.[44] Of course, the work police officers do can involve risks to their safety. But certain exercises of police authority during routine traffic stops (for instance, grabbing drivers or telling them to exit the car) may not be desirable, even if legal or constitutional, if they instigate escalation in ways that harm officers or civilians. Regarding constitutional interventions, the study prompts novel questions about longstanding Fourth Amendment doctrine that invokes officer safety as a justification to allow officers to conduct various searches and seizures during routine traffic stops, regardless of the basis or context of those stops. As this Article will discuss, the findings open avenues to fundamentally rethink assumptions engrained in Fourth Amendment doctrine in at least two areas: pretextual traffic stops[45] and the routine ordering of drivers and passengers out of vehicles.[46]

This Article proceeds as follows. Part I explains the limitations of leading sources of information on violence against the police during routine traffic stops, which underscores the need for the study that I conducted. Part II explains the methodology of the study. Part III presents key statistical findings of the study. Part IV draws on qualitative methods to create a typology of major traffic stop scenarios that escalate into violence against the police. Finally, Part V examines the broader implications of the study for law enforcement agencies, courts, and future research on policing.

I. LIMITATIONS OF EXISTING DATA SOURCES

This Part evaluates existing data sources on violence against the police during routine traffic stops and identifies key limitations. The analysis shows that extant information is largely devoid of context. Specifically, existing data tell us very little about the patterns, sequences, and trends surrounding this violence against officers. As this Article will later discuss, this contextual knowledge is essential for institutional actors that regulate the police to accu-

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and Social Science Research in Constitutional Criminal Procedure, 90 J. CRIM. L. & CRIMINOLOGY 733, 736–37 (2000).

43. Crespo, *supra* note 42, at 2105.

44. *See infra* Section V.A.

45. Joh, *supra* note 35, at 209 (defining pretextual traffic stops as "occasions when the justification offered for the detention is legally sufficient, but is not the actual reason for the stop" (footnote omitted)). In *Whren v. United States*, 517 U.S. 806, 819 (1996), the U.S. Supreme Court held that the subjective reasons why officers conduct traffic stops are irrelevant so long as the officers have probable cause of a traffic violation.

46. *See infra* Section V.B.

rately identify, measure, and mitigate the risks and costs of policing during routine traffic stops.

A. *LEOKA Statistics*

The leading source of official statistics on violence against the police, including violence during routine traffic stops, is the Federal Bureau of Investigation's "Law Enforcement Officers Killed and Assaulted" (LEOKA) program.[47] LEOKA statistics are gathered as follows: Officers who are assaulted by a civilian provide information about the incident to their respective departments by filling out an incident report.[48] Incident reports are not standardized; they can vary from state to state or even from agency to agency. To increase uniformity in reporting, however, the LEOKA program uses standardized offense definitions by which law enforcement agencies submit crime data without regard to local or state definitions of crime.[49] Each participating agency voluntarily collects, organizes, and sends assault data to the FBI. The FBI then compiles this information and uses it to generate and release a comprehensive annual report on violence against law enforcement officers across the United States.[50]

The FBI has historically gathered and released LEOKA statistics on two categories of violence against the police that are relevant to this Article. First, the FBI has gathered and released annual national statistics on law enforcement officers *killed* in the line of duty since 1937.[51] Second, annual national statistics involving *assaults* against officers were added in 1960.[52] LEOKA reports are supposed to be based on actual assaults or homicides, and not mere

47. Brandl, *supra* note 33, at 256 (1996) ("Much of what is known regarding the felonious assault and murder of police officers comes from FBI annual data."). The LEOKA program is part of the FBI's annual Uniform Crime Reporting (UCR), which began in 1930 and is one of the main sources of official crime statistics in the United States. LARRY J. SIEGEL & JOHN L. WORRALL, ESSENTIALS OF CRIMINAL JUSTICE 31–32 (11th ed. 2017); *Uniform Crime Reporting*, FBI, https://ucr.fbi.gov/ [https://perma.cc/33NB-8L2W].

48. *See, e.g.*, ARK. CRIME INFO. CTR., LEOKA: LAW ENFORCEMENT OFFICER KILLED OR ASSAULTED, http://www.acic.org/Websites/acic/images/pdfs/2016_7NIBRS_LEOKA.pdf [https://perma.cc/8256-NYTC] (explaining LEOKA reporting procedures in Arkansas).

49. GENNARO F. VITO & JEFFREY R. MAAHS, CRIMINOLOGY 29 (4th ed. 2017) (noting that the Uniform Crime Report "features standardized definitions of crime").

50. *LEOKA Resources*, FBI, https://ucr.fbi.gov/leoka-resources [https://perma.cc/T9EU-GPL9]. The most recent report was based on statistics collected from just over 12,400 law enforcement agencies. According to the FBI, these agencies employed 586,466 officers and provided service to more than 268.2 million people in the United States (which covers approximately 83% of the U.S. population). *See 2016 Law Enforcement Officers Killed & Assaulted*, FBI (2016), https://ucr.fbi.gov/leoka/2016/officers-assaulted/assaults_topic_page_-2016 [https://perma.cc/4Q53-G7G6].

51. *About Law Enforcement Officers Killed and Assaulted*, FBI, https://ucr.fbi.gov/leoka/2016/about-leoka/about_leoka_-2016 [https://perma.cc/2PVE-8FAE].

52. *Id.*

threats or simple resistance without violence.[53] Importantly, the FBI's intended purpose in gathering these statistics is to assist law enforcement agencies in developing policies that enhance officer safety.[54] Several courts have also referenced or relied on LEOKA statistics in their opinions when discussing the safety risks that routine traffic stops pose to law enforcement officers.[55]

Until 2012, LEOKA statistics lumped together cases of violence against officers during *any* vehicle stop under a single category: "traffic pursuits and stops."[56] Critically, this category captured routine traffic stops, criminal enforcement stops, and felony vehicle stops for non-traffic-based offenses. This overinclusive classification makes it impossible to tell how many cases involve vehicle stops related to traffic enforcement, criminal enforcement, or both.

The problem with this overinclusive classification is that it fails to reflect important distinctions. Consider two hypotheticals. In the first, a police officer engages in a high-speed vehicle pursuit of an armed bank robber who fled the crime scene in a car. The robber eventually stops, and the officer approaches the car with her gun drawn. The robber puts the car into reverse and intentionally drives at the officer, who then falls back and suffers minor injuries from hitting the ground. In the second, a deputy sheriff pulls over a

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53. *Id.* As I discuss later, a small subset of cases in my study were situations that involved mere threats against officers, and not actual or attempted violence against the officers.

54. *LEOKA Resources*, *supra* note 50.

55. *See, e.g.*, Maryland v. Wilson, 519 U.S. 408, 413 (1997); Pennsylvania v. Mimms, 434 U.S. 106, 110 (1977) (per curiam) (citing United States v. Robinson, 414 U.S. 218, 234 n.5 (1973)); United States v. Robinson, 414 U.S. 218, 234 n.5 (1973); *see also, e.g.*, United States v. Robinson, 846 F.3d 694, 699 (4th Cir. 2017) (en banc); United States v. Rochin, 662 F.3d 1272, 1273 (10th Cir. 2011) (Gorsuch, J.) (citing 2010 LEOKA statistics); United States v. Bullock, 510 F.3d 342, 349 (D.C. Cir. 2007) (Kavanaugh, J.); United States v. Holmes, 385 F.3d 786, 791 (D.C. Cir. 2004) (Roberts, J.) (citing 2002 LEOKA statistics); United States v. Pecina, No. 2:13–cr–00146, 2014 WL 3849847, at *9 (N.D. Ind. Aug. 4, 2014) (citing 2012 LEOKA statistics); United States v. Dulaney, No. 2:06–cr–00281–HDM–LRL, 2007 WL 680785, at *3 (D. Nev. Mar. 1, 2007) (quoting *Wilson*, 519 U.S. at 413) (citing 1994 LEOKA statistics), *aff'd*, 299 F. App'x 622 (9th Cir. 2008); Aguiar v. State, 199 So. 3d 920, 924 (Fla. Dist. Ct. App. 2016) (en banc) (quoting *Wilson*, 519 U.S. at 413–14) (citing 1994 LEOKA statistics); Hiibel v. Sixth Judicial Dist. Ct. *ex rel.* County of Humboldt, 59 P.3d 1201, 1205 n.20 (Nev. 2002) (citing 2000 LEOKA statistics), *aff'd*, 542 U.S. 177 (2004); State v. Sloane, 939 A.2d 796, 802 (N.J. 2008) (citing 2005 LEOKA statistics).

56. Prior to 1972, the LEOKA program only used the term "traffic stop" when classifying violence against police officers involving *any* vehicle stop. Although this term was never specifically defined in the pre-1972 LEOKA reports, the 1972 report abandons the term "traffic stops" in favor of "traffic pursuits and stops." After this change, the LEOKA report reframes the pre-1972 statistics as involving *both* "traffic pursuits and stops." The 1972 LEOKA report included the following statistics: (1) from 1963 to 1967, 11 of the 298 officer killings (or 3.7%) involved "traffic pursuits and stops," (2) from 1968 to 72, 49 of the 488 officer killings (or 10%) involved "traffic pursuits and stops," and (3) in 1972, 3,523 (approximately 10.5%) of all total assaults of police officers involved "traffic pursuits and stops." *See* FBI, U.S. DEP'T OF JUSTICE, UNIFORM CRIME REPORTS FOR THE UNITED STATES 46, 168 (1972) (containing the 1972 LEOKA report).

woman for failure to wear a seatbelt, orders her out of the car, and starts to frisk her for weapons. Feeling personally violated as the deputy runs the edge of his hands down her chest, the driver lightly slaps the officer's hand.

LEOKA statistics would have classified both of these cases together under the "traffic pursuits and stops" category, even though a traffic violation was only central in the second case.[57] This lack of a distinction is problematic. Criminal enforcement stops for suspected felonies (for instance, a bank robbery) arguably pose greater risks to officer safety on the whole than do routine traffic stops for minor traffic infractions (for instance, failure to wear a seatbelt).[58] The grave and persistent dangers of felony vehicle stops is why police training has traditionally categorized such stops as "high-risk."[59]

Therefore, institutional actors that relied on pre-2013 LEOKA statistics to inform their decisions about appropriate policing during routine traffic stops did so with statistics that included a much broader swath of vehicle stops than routine traffic stops. In 1977, for example, the U.S. Supreme Court announced its decision in *Pennsylvania v. Mimms*, which held that officers may, at their discretion, routinely order drivers out of vehicles as a safety precaution.[60] To reach its holding, the Court relied on LEOKA statistics in the "traffic pursuits and stops" category to support its view that routine traffic stops are especially fraught with danger to the police.[61] Twenty years later, in *Maryland v. Wilson*, the Court extended *Mimms* to hold that officers also have unbridled discretion to routinely order passengers out of vehicles as a safety precaution.[62] The Court stressed the need for officers to "routinely exercise unquestioned command of the situation,"[63] again relying

57. We can assume that the fleeing bank robber likely committed a range of traffic violations, but those were incidental, at best, to the reason for the stop.

58. *See, e.g.*, Duane Wolfe, *5 Felony Traffic Stop Tactical Tips for Police Officers*, POLICEONE.COM: THE WARRIOR'S PATH (Apr. 20, 2015), https://www.policeone.com/police-products/vehicles/articles/8529241-5-felony-traffic-stop-tactical-tips-for-police-officers/ [https://perma.cc/ET2P-Q7A4] (describing the felony stop as "one of the most-common [sic] high-risk" policing situations).

59. *See, e.g.*, FLA. HIGHWAY PATROL, POLICY MANUAL § 17.21.06(C) (2015), https://www.flhsmv.gov/fhp/Manuals/1721.pdf [https://perma.cc/4UGU-BQBA]; GA. DEP'T OF PUB. SAFETY, POLICY MANUAL § 11.03.3(E) (2011), https://dps.georgia.gov/sites/dps.georgia.gov/files/Policies/Chapter11/11.03%20Traffic%20Stops%20revision%20March%204%2C%202011.pdf [http://perma.cc/ZQG3-MBLV] (discussing protocol on felony/high risk traffic stops); N.M. DEP'T OF PUB. SAFETY, POLICIES AND PROCEDURES: TRAFFIC CONTROL AND ENFORCEMENT OPERATIONS § 6.0(G)(3) (2015), https://www.dps.nm.gov/templates/g5_hydrogen/custom/PDFs/AB&MOPR.41_Traffic_Control_Enforcement.pdf [https://perma.cc/V8FU-TYJY] (discussing protocol on felony/high risk stops).

60. 434 U.S. 106, 109–11 (1977).

61. *Mimms*, 434 U.S. at 110 (citing Allen P. Bristow, *Police Officer Shootings—A Tactical Evaluation* 54 J. CRIM. L. CRIMINOLOGY & POLICE SCI. 93, 93 (1963)) (relying on data from the 1961 LEOKA report that found 32% of shooting incidents occurred in the course of vehicle stops).

62. Maryland v. Wilson, 519 U.S. 408 (1997).

63. *Id.* at 414 (quoting Michigan v. Summers, 452 U.S. 692, 703 (1981)).

on overinclusive LEOKA statistics to reaffirm its view that routine traffic stops are especially dangerous settings for officers.[64]

Even with this overinclusivity problem, LEOKA statistics from 2012, presented *infra* in Table 1, contradict the dominant danger narrative. Those statistics report that cases in the "traffic pursuits and stops" category accounted for only 8.41% of officers who reported assaults that year. Contrary to the dominant danger narrative, "traffic pursuits and stops" fell outside the top four major policing scenarios in which officers reported being victims of civilian-perpetrated assaults. Those scenarios included disturbance calls; attempted arrests on suspects other than burglary or robbery suspects; the handling, transporting, and custody of prisoners; and investigating suspicious persons/circumstances.

TABLE 1

Law Enforcement Officers Assaulted by Circumstance at Scene of Incident, 2012[65]		
Circumstance	***Freq.***	***% Dist.***
Disturbance call	17,205	32.52
Attempting other arrest	8,057	15.23
Handling, transporting, custody of prisoner	7,173	13.56
Investigating suspicious person/circumstance	4,915	9.29
Traffic pursuit/stop	4,450	8.41
Handling person with mental illness	1,353	2.56
Burglary in progress/pursuing burglary suspect	760	1.44
Civil disorder (mass disobedience, riot, etc.)	722	1.36
Robbery in progress/pursuing robbery suspect	463	0.88
Ambush situation	267	0.50
All other	7,536	14.25
Total number of victim officers	**52,901**	**100.00**

64. *Id.*

65. This table was modified from FBI, LAW ENFORCEMENT OFFICERS KILLED AND ASSAULTED tbl.73 (2012), https://ucr.fbi.gov/leoka/2012/tables/table_73_leos_asltd_circum_at_scene_of_incident_by_type_of_weapon_and_percent_distribution_2012.xls [https://perma.cc/76E5-2UR8].

In 2013, the FBI made a significant change to the LEOKA program that is relevant to this Article: it separated the "traffic pursuits and stops" category into two subcategories: (1) "traffic violation stops" and (2) "felony vehicle stops." These two new subcategories are now used to track the felonious killings of law enforcement officers as well as assaults against officers who are injured with "firearms or knives/other cutting instruments."[66]

These improved LEOKA statistics provide important insight into the problems with treating vehicle stops as a monolithic category. First, consider the improved statistics that separate incidents involving the felonious killings of officers during "traffic violation stops" versus "felony vehicle stops." According to the most recent LEOKA statistics, presented *infra* in Table 2, a total of 509 officers were feloniously killed between 2007 and 2016;[67] 79 officers were feloniously killed during "traffic pursuits and stops."[68] The total was more than the number of officers killed in any other single category except "arrest situations." When "traffic pursuits and stops" are viewed as a monolithic category, then institutional actors could easily extrapolate from these LEOKA statistics that routine traffic stops are one of the deadliest settings for law enforcement officers.

66. FBI, LAW ENFORCEMENT OFFICERS KILLED AND ASSAULTED tbl.101 (2016), https://ucr.fbi.gov/leoka/2016/detailed-assault-topic-page-summaries/tables/table-101.xls [https://perma.cc/56Y4-CH5U].

67. FBI, LAW ENFORCEMENT OFFICERS KILLED AND ASSAULTED tbl.23 (2016), https://ucr.fbi.gov/leoka/2016/tables/table-23.xls [https://perma.cc/M5MN-XA5H].

68. *Id.*

TABLE 2

Law Enforcement Officers Feloniously Killed[69]	
Circumstance at Scene of Incident, 2007–2016	
Arrest situation	89
Robbery in progress/pursuing robbery suspect	33
Burglary in progress/pursuing burglary suspect	10
Drug-related matter	9
Attempting other arrest	37
Traffic pursuit/stop	79
Traffic violation stop	48
Felony vehicle stop	31
Investigating suspicious person/circumstance	73
Disturbance call	63
Disturbance (bar fight, person with firearm, etc.)	37
Domestic disturbance (family quarrel, etc.)	26
Ambush (entrapment/premeditation)	55
Tactical situation (barricaded offender, hostage taking, high-risk entry, etc.)	54
Unprovoked attack	50
Investigative activity (surveillance, search, interview, etc.)	27
Handling, transporting, custody of prisoner	11
Handling person with mental illness	8
Civil disorder (mass disobedience, riot, etc.)	0
Total number of victim officers	**509**

Critically, a very different picture of the dangerousness of routine traffic stops emerges when "traffic violation stops" are separated from "felony vehicle stops." These improved statistics show that of the 79 total officer killings during "traffic pursuits and stops," 48 killings (or approximately 60%) involved "traffic violation stops," and 31 killings (or approximately 40%) involved "felony vehicle stops."[70] Contrary to the dominant danger narrative, these more nuanced statistics show that "traffic violation stops" accounted for fewer felonious killings of officers than six other major policing scenarios: disturbance calls, arrest situations, investigating suspicious persons/circumstances, ambushes, unprovoked attacks, and tactical situations. This more nuanced breakdown is also reflected *supra* in Table 2.

While "traffic violation stops" still account for a majority of the killings in the "traffic pursuits and stops" category, the 40% involving "felony vehicle stops" is far from insignificant—especially given that felony vehicle stops are conducted much less frequently than traffic violation stops.[71] Put simply,

69. This table appears in FBI, LAW ENFORCEMENT OFFICERS KILLED AND ASSAULTED tbl.23 (2016), https://ucr.fbi.gov/leoka/2016/tables/table-23.xls [https://perma.cc/M5MN-XA5H].

70. *Id.*

71. *See* sources cited *supra* notes 2–3.

these more refined statistics show how categorizing felony vehicle stops and routine traffic stops for only minor traffic violations together provides an inflated metric of the number of officers that are feloniously killed during routine traffic stops. Several courts—including the U.S. Supreme Court—have relied on these overinclusive LEOKA statistics in cases involving police authority and routine traffic stops.[72]

Second, consider the improved LEOKA statistics that separate incidents involving officers who are assaulted and injured with "firearms or knives/other cutting instruments" during "traffic violation stops" versus "felony vehicle stops."[73] According to the most recent LEOKA statistics, presented *infra* in Table 3, a total of 522 officers reported assaults and injuries with firearms or knives/other cutting instruments between 2012 and 2016;[74] 47 officers reported assaults and injuries during "traffic pursuits and stops."[75] This is not an insubstantial number, but it also does not rank as the most dangerous aspect of policing based on this metric: overinclusive "traffic pursuits and stops" accounted for fewer officer assaults and injuries with firearms or knives/other cutting instruments than four other major policing scenarios (disturbance calls, arrest situations, investigating suspicious persons or circumstances, and tactical situations).

72. *See* decisions cited *supra* note 55; *see also infra* Section V.B.2.

73. FBI, tbl.101, *supra* note 66.

74. *Id.*

75. *Id.*

TABLE 3

Law Enforcement Officers Assaulted and Injured with Firearms or Knives/Other Cutting Instruments[76]	
Circumstance at Scene of Incident, 2012–2016	
Disturbance call	119
Disturbance (bar fight, person with firearm, etc.)	69
Domestic disturbance (family quarrel, etc.)	50
Tactical situation (barricaded offender, hostage taking, high-risk entry, etc.)	93
Investigating suspicious person/circumstance	78
Arrest situation	65
Robbery in progress/pursuing robbery suspect	15
Drug-related matter	13
Burglary in progress/pursuing burglary suspect	6
Attempting other arrest	31
Traffic pursuit/stop	47
Traffic violation stop	33
Felony vehicle stop	14
Handling person with mental illness	39
Investigative activity (surveillance, search, interview, etc.)	34
Ambush (entrapment/premeditation)	26
Unprovoked attack	15
Handling, transporting, custody of prisoner	4
Civil disorder (mass disobedience, riot, etc.)	2
Total number of victim officers	**522**

More relevantly, though, the more refined statistics that separate "traffic violation stops" from "felony vehicle stops" further undermine the dominant danger narrative. Of the 47 officers who reported assaults and injuries with firearms or knives/other cutting instruments, 33 incidents involved "traffic violation stops," whereas 14 incidents involved "felony vehicle stops."[77] As Table 3 shows above, "traffic violation stops" accounted for fewer assaults and injuries with a firearm or knife/cutting instrument than six other major policing scenarios (disturbance calls, arrest situations, investigating suspicious persons or circumstances, tactical situations, investigative activities, and handling individuals with mental illness).

The newer and improved LEOKA statistics cast doubt on whether routine traffic stops are truly exceptional with regard to the safety risks that they pose to law enforcement officers. These improved statistics also show the importance of separating vehicle stops for traffic enforcement from vehicle stops for felony criminal enforcement in order to accurately measure the

76. This table appears in FBI, LAW ENFORCEMENT OFFICERS KILLED AND ASSAULTED tbl.101 (2016), https://ucr.fbi.gov/leoka/2016/detailed-assault-topic-page-summaries/tables/table-101.xls [https://perma.cc/56Y4-CH5U].

77. *Id.*

dangerousness of routine traffic stops to officers. As explained later, my study expanded on this approach.[78]

Although these improved LEOKA statistics offer important insights, they also have significant limitations. To begin, these improved statistics only involve a very small subset of cases involving violence against the police during routine traffic stops—namely, the most violent ones: encounters that result in an officer being injured by a deadly weapon or feloniously killed. As discussed later in this Article, the overwhelming majority of violence against the police during routine traffic stops does not involve felonious killings of officers or assaults with guns or knives that lead to injury.[79] Rather, the bulk of violence involves unarmed assaults that cause no injury or minor injuries to officers.[80] Therefore, the improvements to the LEOKA statistics do not address the overinclusivity problems.

Similarly, both the old and newly improved LEOKA statistics present small subsets of police–civilian encounters. They do not capture the fact that tens of millions of routine traffic stops occur every year.[81] Traffic stops are the most common interaction between police and civilians,[82] so the number of incidents that result in violence against the police is only a drop in the bucket compared to the total number of encounters.[83] Further, neither the old nor the newly improved LEOKA statistics provide any contextual information on the bases of the stops or the sequence of events that escalated into violence against the officers during those stops.

As a result, LEOKA statistics do not help to evaluate whether certain exercises of police power during routine traffic stops—such as ordering drivers or passengers out of cars—create avoidable and unnecessary conflicts that may undermine both officer and civilian safety. Further, these statistics do not tell us whether cases of violence against the police during felony vehicle stops follow different patterns or sequences than do cases of violence involving routine traffic stops for only minor traffic violations. These questions are essential to evaluate whether and when it is appropriate for institutional actors to rely on officer safety as a justification to permit certain invocations of authority during routine traffic stops.[84]

B. *The Bristow Study*

Allen Bristow's study on civilian shootings of police officers ("the Bristow study") is a second major source of information on violence against the

78. *See infra* Part II (discussing the methodology of the study).

79. *See infra* Section III.B.

80. *See infra* Section III.B.

81. *See supra* note 3 and accompanying text.

82. *See supra* note 2 and accompanying text.

83. A more thorough discussion of this point will be provided *infra* Sections I.C and III.C.

84. *See infra* Sections V.A., V.B.

police during routine traffic stops.[85] Published in 1963, the Bristow study is important because courts have relied on, and still reference, the study when emphasizing the dangers of routine traffic stops to the police.[86] Moreover, the Bristow study has shaped a common statistic within law enforcement circles: one-third of all officer killings involve a routine traffic stop.[87]

The continuing influence of the Bristow study warrants scrutiny, given that it was published over 50 years ago, in an era when routine traffic stops served a much different law enforcement purpose. Between the 1920s and 1970s, law enforcement agencies largely followed a reactive philosophy that was geared toward responding to civilian complaints of crime.[88] The Bristow study therefore applies to a time when reactive philosophies were the policing norm.

Dominant policing philosophies have shifted since then. During the 1960s and 1970s, rising crime rates and growing civil unrest generated skepticism over the effectiveness of reactive policing.[89] Proactive policing strategies emerged as the norm soon after in the 1980s.[90] These strategies shifted

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85. Allen P. Bristow, *Police Officer Shootings—A Tactical Evaluation*, 54 J. CRIM. L. CRIMINOLOGY & POLICE SCI. 93, 93 (1963).

86. *See, e.g.*, Michigan v. Long, 463 U.S. 1032, 1049 n.3 (1983); Pennsylvania v. Mimms, 434 U.S. 106, 110 (1977); United States v. Robinson, 414 U.S. 218, 234 n.5 (1973); Adams v. Williams, 407 U.S. 143, 147–48, 148 n.3 (1972); *see also, e.g.*, United States v. Washington, 490 F.3d 765, 771 (9th Cir. 2007) (quoting *Adams*, 407 U.S. at 148 n.3); United States v. Sakyi, 160 F.3d 164, 168 (4th Cir. 1998) (citing *Mimms*, 434 U.S. at 110, and *Long*, 463 U.S. at 1048 n.13); United States v. Stanfield, 109 F.3d 976, 981 (4th Cir. 1997) (quoting *Mimms*, 434 U.S. at 110); Ruvalcaba v. City of Los Angeles, 64 F.3d 1323, 1327 (9th Cir. 1995) (quoting *Long*, 463 U.S. at 1047); United States v. Pajari, 715 F.2d 1378, 1383 (8th Cir. 1983) (quoting *Adams*, 407 U.S. at 148 n.3); Flanegan v. O'Leary, No. 14–1379, 2015 WL 5311271, at *3 (W.D. Pa. Sept. 11, 2015) (quoting *Mimms*, 434 U.S. at 110); Hall v. Raech, 677 F. Supp. 2d 784, 801 n.22 (E.D. Pa. 2010) (citing *Mimms*, 434 U.S. at 110); Merring v. Town of Tuxedo, No. 07–CV–10381 (CS), 2009 WL 849752, at *7 (S.D.N.Y. Mar. 31, 2009) (quoting *Adams*, 407 U.S. at148 n.3); United States v. Williams, No. CRIM.A. H-05-68, 2005 WL 2171877, at *2 (S.D. Tex. Sept. 2, 2005) (citing *Mimms*, 434 U.S. at 110); United States v. Garcia, 279 F. Supp. 2d 294, 302 n.1 (S.D.N.Y. 2003) (citing *Mimms*, 434 U.S. at 110); United States. v. Younger, No. 86–0036–F, 1986 WL 8790, at *5 (D. Mass. Aug. 11, 1986) (citing *Adams*, 407 U.S. at 148 n.3); United States v. Balsamo, 468 F. Supp. 1363, 1384 n.30 (D. Me. 1979) (quoting *Adams*, 407 U.S. at 148 n.3). In addition, David Harris has discussed that the U.S. Supreme Court has misused the Bristow study to support the proposition that "stops of cars pose such danger to officers that they necessitate broad police powers to conduct vehicle searches," when the study does not support this conclusion. David A. Harris, *Frisking Every Suspect: The Withering of* Terry, 28 U.C. DAVIS L. REV. 1, 41 n.181 (1994).

87. This point is based on my conversations with several former law enforcement officers from different policing jurisdictions.

88. George L. Kelling & Mark H. Moore, *The Evolving Strategy of Policing*, PERSP. ON POLICING, Nov. 1988, at 4–5.

89. Bruce A. Green & Alafair S. Burke, *The Community Prosecutor: Questions of Professional Discretion*, 47 WAKE FOREST L. REV. 285, 287 (2012).

90. Debra Livingston, *Police Discretion and the Quality of Life in Public Places: Courts, Communities, and the New Policing*, 97 COLUM. L. REV. 551, 565–78 (1997) (discussing the historical development of community policing and problem-oriented policing).

the focus of policing away from individuals who committed crime toward individuals who had not yet committed crime, as well as the circumstances that might encourage them to offend.[91]

In this new policing climate, routine traffic stops assumed a greater role than mere traffic enforcement. Rather, law enforcement agencies came to view these stops as cost-effective tools for officers to stop and search "suspicious" drivers and passengers who the officers believed may be involved in nontraffic crime.[92] As routine traffic stops became increasingly entwined with criminal enforcement, the protective tools that officers had at their disposal also improved (for instance, bulletproof vests[93] and improved tactics[94]).

These important differences cast doubt on the applicability of the Bristow study to contemporary policing situations.[95] Beyond applicability concerns, however, the Bristow study also has several methodological shortcomings that undermine its ability to provide insight into the dangers of routine traffic stops to law enforcement. To begin, there are sampling problems with the study. The study examined a sample of 110 civilian shootings that resulted in injury or death involving 150 officers.[96] This sample was based on an informal collection of cases over a two-year period.[97] Bristow himself warned that the study was only intended to be a pilot study and that its findings should be viewed with caution.[98]

Putting aside sampling limitations, the Bristow study found that 32% (or 35 of the 110 police shootings) occurred while police officers were attempting to "investigate, control, or pursue suspects who were in automobiles."[99] Of those 35 cases, 7% of the officers were shot during a vehicle pursuit; 28% were shot while sitting in their patrol cars before exiting; 22% were shot while exiting from their vehicles or approaching a suspect's vehicle; and 43% were shot after initial contact with the suspect while questioning, issuing a

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91. FAIRNESS AND EFFECTIVENESS IN POLICING: THE EVIDENCE 85–88 (Welsey Skogan & Kathleen Frydl eds., 2004).

92. *See* Gross & Barnes, *supra* note 35, at 660 (discussing the use of traffic stops and racial profiling "to increase the probability of finding large hauls of drugs").

93. OSHA GRAY DAVIDSON, UNDER FIRE 87 (1998); Ronnie Garrett, *Body Armor: Protecting Those Who Serve*, POLICE MAG., (July 17, 2012), http://www.policemag.com/channel/patrol/articles/2012/07/protecting-those-who-serve.aspx [https://perma.cc/3Z79-DGAK] (noting that the National Institute of Justice (NIJ) "began testing and developing body armor and performance standards for ballistic and stab resistance" in the mid-1970s).

94. *See* Garrett & Stoughton, *supra* note 29, at 244–49 (discussing the "revolution" in police tactics in the 1960s and 1970s).

95. Relevant to this idea, Illya Lichtenberg recently attempted to replicate the Bristow study using more recent LEOKA statistics and could not replicate its results. *See generally "Police Officer Shootings—A Tactical Evaluation": A Replication of the 1963 Bristow Study*, 54 WILLAMETTE L. REV. 79 (2017).

96. Bristow, *supra* note 85, at 93.

97. *Id.*

98. *Id.*

99. *Id.*

citation, or requesting a record check on the suspect.[100] Critically, the study did not separate these 35 vehicle stop cases in terms of their underlying basis—the same shortcoming with most available LEOKA statistics.[101] It is unclear how many of the 35 cases involved criminal enforcement stops as opposed to routine traffic stops for traffic violations. Because of the overinclusive nature of the findings, the Bristow study cannot provide insight into the dangers that routine traffic stops specifically pose to the police.[102]

At the same time, the findings of the Bristow study are underinclusive in important ways. The Bristow study is limited to civilian *shootings* of police officers. But available LEOKA statistics show, and the findings presented in this Article further indicate, that only a very small percentage of violence against the police during routine traffic stops involves guns.[103] Therefore, because of its narrow focus, the Bristow study does little to enhance our understanding of the more common situations in which violence against the police during routine traffic stops occurs.

C. *Lichtenberg and Smith's Study*

The only other major published study that focuses on the dangerousness of routine traffic stops to law enforcement officers is Illya D. Lichtenberg and Alisa Smith's study.[104] Published over fifteen years ago, Lichtenberg and Smith examined 10 years of LEOKA statistics in the "traffic stops and pursuits" category between 1988 and 1997.[105] To measure the dangerousness of routine traffic stops to law enforcement officers, Lichtenberg and Smith used a metric called the "danger ratio."[106] This ratio, which researchers have also

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100. *Id.*

101. *See supra* Section I.A.

102. Some U.S. Supreme Court justices stressed these problems with the Bristow study in their dissents. *See* Pennsylvania v. Mimms, 434 U.S. 106, 118 (1977) (Stevens, J., dissenting) ("These figures tell us very little about the risk associated with the routine traffic stop" and emphasizing that "the Court has based its legal ruling on a factual assumption about police safety that is dubious at best.").

103. *See infra* Section III.B (discussing the study findings on the nature of the violence). The most recent LEOKA statistics report that only 6.3% of assaults during "traffic pursuits [or] stops" involve firearms, and only 0.9% involve knives/other cutting instruments. FBI, LAW ENFORCEMENT OFFICERS KILLED AND ASSAULTED tbl.78 (2016), https://ucr.fbi.gov/leoka/2016/officersassaulted/tables/table-78.xls [https://perma.cc/MU7W-LKV6]. As discussed *infra* Section III.B, the percentage for routine traffic stops is likely less because the overinclusive "traffic pursuits [or] stops" category includes both routine traffic stops and felony vehicle stops. I also found many cases of assaults against an officer that were categorized as involving guns or knives/cutting instruments, but those weapons were merely found at the scene and were not the cause of the injuries the officer suffered.

104. Illya D. Lichtenberg & Alisa Smith, *How Dangerous Are Routine Police–Citizen Traffic Stops? A Research Note*, 29 J. CRIM. JUST. 419 (2001).

105. *Id.* at 421.

106. *Id.*

applied to measure the dangerousness of domestic disturbance calls to officers,[107] is calculated by dividing the total number of relevant harmful incidents to the police by the total number of relevant police responses.[108]

Lichtenberg and Smith used LEOKA statistics from the overinclusive "traffic pursuits and stops" category to calculate the numerator of the danger ratio. According to these statistics, 89 law enforcement officers were feloniously killed in the line of duty during "traffic pursuits and stops" between 1988 and 1997,[109] which accounted for 12.9% of the total civilian killings of officers during that period.[110] The LEOKA statistics also reported 58,502 incidents involving assaults against officers during "traffic pursuits and stops," which accounted for 9.4% of the total assaults against officers during the same period.[111]

As discussed above, the major shortcoming of using these LEOKA statistics to calculate the numerator of the danger ratio is that the statistics do not distinguish between routine traffic stops and criminal enforcement stops. The previous analysis of post-2013 LEOKA statistics that separate "traffic violation stops" from "felony vehicle stops" showed how this overinclusive classification offers an inflated vision of the dangerousness of routine traffic stops for only traffic violations. As explained later, the study presented in this Article analyzed the underlying incident narratives to estimate the extent to which the overinclusive "traffic pursuits and stops" category captures routine traffic stops versus criminal enforcement stops.

Further, with regard to the denominator of the danger ratio, one difficulty in calculating the total number of routine traffic stops is that many stops occur that do not result in a citation.[112] To reduce the effects of possible missing cases, Lichtenberg and Smith calculated the denominator based on three different frequency estimates of the total number of routine traffic stops each year that officers initiate nationwide: (1) a low-end estimate (60 million stops) based on the number of annual reported traffic filings in 1991 as reported by the National Center of State Courts; (2) a mid-range estimate (120 million stops) based on a scholarly estimate that only one-half of stopped drivers ever receive a traffic citation;[113] and (3) a high-end estimate (180 million stops) based on a different scholarly estimate that only one-

107. *Id.* at 420 (citing Joel Garner & Elizabeth Clemmer, *Danger to Police in Domestic Disturbances—A New Look*, NAT'L INST. JUST. RES. BRIEF, Nov. 1986, at 1, 2).

108. *Id.*

109. *Id.* at 423–24.

110. *Id.* at 424.

111. *Id.*

112. *Id.* at 423. Issues surrounding how police exercise their discretion to issues citations, and how many, during routine traffic stops fit into broader debates about the role of underenforcement in shaping the fairness and legitimacy of the criminal justice system. For a broader discussion of under-enforcement in the criminal justice system see generally, Alexandra Natapoff, *Underenforcement*, 75 FORDHAM L. REV. 1715 (2006).

113. Lichtenberg & Smith, *supra* note 104, at 423 (citing DAVID H. BAYLEY, POLICE FOR THE FUTURE 30 (1994)).

third of stopped drivers ever receive a traffic citation.[114] Table 4 presents Lichtenberg and Smith's danger ratios for each estimate.[115]

TABLE 4

Lichtenberg and Smith (2011) Danger Ratios		
	Killings	***Assaults***
Low-End	1 in 6.7M*	1 in 10,256*
Mid-Range	1 in 13.4M*	1 in 20,512*
High-End	1 in 20.1M*	1 in 30,768*
		** Number of traffic stops*

As Table 4 reflects, even the most conservative low-end ratio reflects how uncommonly violence against the police during routine traffic stops occurs.[116] That ratio suggests that based on nationwide LEOKA statistics, officers are at most feloniously killed by civilians in only 1 in every 6.7 million stops and assaulted—regardless of injury—in only 1 in every 10,256 stops. Based on these ratios, Lichtenberg and Smith concluded that the dangerousness of traffic stops to officers might not be as great as the U.S. Supreme Court has assumed in its Fourth Amendment jurisprudence.[117]

Although Lichtenberg and Smith's danger ratios provide a better estimate of violence against officers, these ratios cannot tell us about violence against the police during routine traffic stops specifically because they rely on overinclusive LEOKA data. This metric of dangerousness also does not offer any contextual information about the circumstances, or the sequences, patterns, or trends, surrounding this violence against officers. Relatedly, danger ratios do not help to evaluate whether specific invocations of police authority (for instance, ordering drivers and passengers out of vehicles) instigate escalation in ways that compromise both officer and civilian safety. Danger ratios can thus offer insight into aggregate probabilities of violence against the police during routine traffic stops, but they do not help to evaluate how much of this violence was avoidable or unnecessary in the first place.

* * *

To summarize, leading sources of information on violence against the police during routine traffic stops have a common and significant shortcoming: they are largely devoid of context. The need for context-rich infor-

114. *Id.* (citing MICHAEL K. BROWN, WORKING THE STREET 227 (1981)).

115. *Id.* at 424–25.

116. In this regard, the danger ratio is predicated on what *actually* happened and not what *could* have happened during the traffic stop.

117. Lichtenberg & Smith, *supra* note 104, at 419.

mation on violence against the police during routine traffic stops informed the design of my study, which the Article will now describe.

II. METHODOLOGY

This Part explains the methodology of the study. It first explains the study design, data collection, and data analysis process. It then describes the limitations of the study.

The primary goal of the study was to further our contextual understanding of violence against the police during routine traffic stops.[118] The study mostly relied on qualitative methods because the nature of the research was inductive and not hypothesis driven.[119] I was also concerned with examining the ways in which violence against the police occurred in natural police settings as opposed to under experimental conditions.[120] A grounded-theory approach[121] served as the methodological framework for data collection and analysis, allowing for the generation of better-informed theories and propositions about when violence against the police during routine traffic stops occurs.

I chose to gather the underlying data from law enforcement agencies in Florida. Florida has one of the most comprehensive public records laws in the United States.[122] This level of access resulted in a nearly perfect response rate from the agencies that I contacted, although as explained further below,

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118. CATHERINE MARSHALL & GRETCHEN B. ROSSMAN, DESIGNING QUALITATIVE RESEARCH 75 (6th ed. 2016) (noting that "[h]istorically, qualitative methodologists have described three major purposes for research: to *explore*, *explain*, or *describe* a phenomenon").

119. MICHAEL QUINN PATTON, QUALITATIVE EVALUATION AND RESEARCH METHODS 44 (2d ed. 1990) ("Inductive analysis contrasts with hypothetical-deductive approach of experimental designs that requires the specification of main variables and the statement of specific research hypotheses *before* data collection begins."); ANSELM STRAUSS & JULIET CORBIN, BASICS OF QUALITATIVE RESEARCH 11 (2d ed. 1998) (noting that "[i]n speaking about qualitative analysis, we are referring not to the quantifying of qualitative data but rather to a nonmathematical process of interpretation, carried out for the purpose of discovering concepts and relationships in raw data and then organizing these into a theoretical explanatory scheme"); *id*. at 136 (discussing how the concept of induction is often applied to qualitative research and that "[a]lthough statements of relationship or hypotheses do evolve from the data (we go from the specific case to the general), whenever we conceptualize data or develop hypotheses, we are interpreting to some degree").

120. *See* EMMA WINCUP, CRIMINOLOGICAL RESEARCH: UNDERSTANDING QUALITATIVE METHODS 13 (2d ed. 2017) (noting that "qualitative techniques offer[] the opportunity to make a distinct contribution by elucidating the context in which offending takes place and the meanings attached to such behaviour").

121. STRAUSS & CORBIN, *supra* note 119, at 12 (explaining "grounded theory" as the process by which theory is derived from data).

122. FLA. STAT. ANN. § 119.011–.19 (West 2014); FLA. GOV'T FIN. OFFICERS ASS'N, BASIC GOVERNMENT RESOURCE MANUAL 64 (2017) (noting that Florida has some of "the most comprehensive open government laws in the country," which includes its public records law); Keith W. Rizzardi, *Sunburned: How Misuse of the Public Records Laws Creates an Overburdened, More Expensive, and Less Transparent Government*, 44 STETSON L. REV. 425, 425 (2015) (noting that Florida's public records law "has been praised as a model of open government").

those responding agencies did not necessarily have all the relevant cases on file.[123]

Demographic factors also make Florida a prime location for the study. Florida is one of the most populous and diverse states with a mix of major urban, suburban, and rural areas.[124] Millions of traffic citations are issued in the state every year.[125]

Moreover, in 1974, Florida decriminalized the bulk of minor traffic offenses by removing criminal penalties and reclassifying those offenses as civil violations.[126] Those decriminalization reforms were part of a broader wave of traffic decriminalization that swept across over twenty states in the 1970s and 1980s.[127] As I have discussed in prior work, this wave of traffic decriminalization was "largely [a] product[] of legislative and public judgments that [minor] traffic violations: (1) do not pose a serious enough threat to warrant the significant penalty of the criminal law; [and] (2) pose too great of a burden on courts when handled inside the criminal framework . . . ."[128] Importantly, these judgments are in tension with the dominant danger narrative surrounding routine traffic stops.

The study drew on two original sources of data. The first source (what I call "the Florida LEOKA Database") was a comprehensive Excel database that I obtained through a public records request from the Florida Department of Law Enforcement.[129] The Florida LEOKA Database included all incidents of violence against officers during "traffic pursuits and stops" in Florida that resulted in a LEOKA report during the 10-year period of 2005 to

123. As discussed later, I did not receive responses from law enforcement agencies that had disbanded and, therefore, no longer existed when conducting the study. Several other agencies had been incorporated into a different Florida law enforcement agency, and I was able to obtain the relevant narratives by contacting the new agency if it held onto those records.

124. *Florida Passes New York to Become the Nation's Third Most Populous State, Census Bureau Reports*, U.S. CENSUS BUREAU (Dec. 23, 2014), http://www.census.gov/newsroom/press-releases/2014/cb14-232.html [https://perma.cc/GYC8-LXCS].

125. This number has wavered between 2.8 million and 5.2 million during the past decade. *Crash and Citation Reports & Statistics*, FLA. DEP'T OF HIGHWAY SAFETY & MOTOR VEHICLES, http://www.flhsmv.gov/resources/crash-citation-reports/ [https://perma.cc/D46N-YALQ].

126. *See* FLA. STAT. ANN. § 316.655 (West 2014) (noting that the bulk of traffic violations are civil infractions). Examples of criminal traffic violations include driving while under the influence, reckless driving, leaving the scene of an accident, fleeing from a police officer, racing, not having a valid license or registration, and having no or an expired tag. *Id.*; *id.* §§ 316.655; 318.17.

127. Woods, *supra* note 23, at 698.

128. *Id.* at 734–35.

129. The Florida Department of Law Enforcement is the primary entity that gathers data for the FBI's Uniform Crime Report (UCR) from participating law enforcement agencies across the entire state of Florida. *See Uniform Crime Reports (UCR)*, FLA. DEP'T L. ENF'T, http://www.fdle.state.fl.us/FSAC/UCR-Reports [https://perma.cc/SXK2-DHLX].

2014.[130] For each incident, the Florida LEOKA Database reported the year; case number; affiliated law enforcement agency; assignment description (whether the officer was alone or assisted, as well as the type of vehicle the officer was driving); the extent of officer injury (felonious killing, serious, major, none); whether a civilian assailant used a weapon and, if so, what type of weapon; the incident time; the officer's age; and the number of years of officer experience.[131] The database included 6,903 total cases—9 involving felonious killings and 6,894 involving assaults against officers—from 288 law enforcement agencies across Florida.[132] Almost all of the agencies were police departments or sheriff's offices.[133]

Although the Florida LEOKA Database provided useful information, it did not describe the bases of the stops or the sequences of events leading to violence against the officers.[134] For these reasons, the centerpiece of the study was the second source—a large sample of incident narratives (and other supplemental information) from the underlying incidents in the Florida LEOKA Database. To create this large sample, I submitted public records requests to 221 of the 288 law enforcement agencies represented in the Florida LEOKA Database. For every case that an agency was affiliated with in the Florida LEOKA Database, I requested the pages of the incident report, arrest record, or probable-cause affidavit that included the officer's narrative of what occurred. Between March 2016 and May 2018, I collected 4,255 narratives that fit the inclusion criteria for the assault cases.[135]

The sampling strategy, described in more detail below, guided which law enforcement agencies I decided to submit public records requests to. The

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130. Fla. Dep't of Law Enf't, Law Enforcement Officers Killed or Assaulted, Florida Uniform Crime Report 2005–2014 [Computer Program] (2014) (on file with author) [hereinafter Florida LEOKA Database]. Some readers may wonder why the study stopped at 2014. When I began data collection in early 2016, some police departments had not yet compiled all of their relevant data for 2015 (especially during the later months of the year). In order to accurately calculate the danger rates of routine traffic stops, it was necessary to have the full universe of LEOKA "traffic pursuits and stops" cases over a particular year.

131. This Article only presents certain portions of this information, saving the rest for future scholarship.

132. Not included in these 6,903 cases were seven cases that I excluded from the Florida LEOKA Database involving duplicate incidents and five cases involving the accidental killings of officers. In addition, for simplicity purposes, I categorized cases coming from different station locations of the Florida Highway Patrol together as coming from a single agency—Florida Highway Patrol. The same applied to different station locations of the Florida Fish and Wildlife Conservation Commission.

133. Some exceptions included tribal police forces, station locations of the Florida Highway Patrol, and station locations of the Florida Fish and Wildlife Conservation Commission.

134. Harmon, *supra* note 28, at 1136 (discussing the FBI's Uniform Crime Reports and noting that "the federal government does not collect or analyze most of the extensive data produced by police departments through incident and arrest reports").

135. I excluded 164 narratives that I could not read because of illegible handwriting or that had insufficient information to determine whether the case involved a vehicle stop. A total of 598 narratives were unavailable from the agencies that I contacted because they did not keep records dating back to 2005.

most common reasons why I did not submit public records requests to the other 67 of the 288 law enforcement agencies represented in the Florida LEOKA Database included (1) the agencies had disbanded, (2) the agencies had been subsumed by a different agency that did not have the prior records on file; (3) the agency reported fewer than 5 incidents in the database, and I had a sufficient number of agencies that matched that agency in terms of geography, size, and agency type; and (4) the public records request was too costly because the agency did not store the relevant records electronically or in a way that was easy to access or retrieve.

Having a detailed narrative allowed me to go beyond any previous study by separating the LEOKA cases that involved routine traffic stops for traffic violations from felony vehicle stops for nontraffic crime. To the best of my ability, I was also able to identify and separate pretextual stops, relying on officers' descriptions of using a traffic violation to pull over a vehicle that they suspected was engaged in crime or that appeared otherwise suspicious.[136] After categorizing the different types of vehicle stops, I then explored relevant sequences, patterns, and trends. To do this, I coded and analyzed the narratives (and other provided supplemental information) in the qualitative data analysis software ATLAS.ti.

The length and detail of the information included in the reports varied. In order to qualify for the study, the report at a minimum had to include the officer's narrative of the routine traffic stop. Most narratives typically included the following information: (1) the officer's basis for initiating the stop, (2) the sequence of events during the stop that resulted in violence against the officer, (3) the presence and severity of any injuries the officer sustained from the violence, (4) the crimes for which the driver or passenger was arrested, and (5) the ways in which police invoked their authority during the stop and whether that invocation resulted in the discovery of any illegal drugs, weapons, or other contraband.

On balance, the narratives typically included two to three pages of typed text, although lengths ranged from one paragraph to over one hundred pages. The longer reports usually involved assaults that resulted in serious injuries or death to either an officer or a driver or passenger. Many cases also included narratives from multiple officers who were involved in the stop. These supplemental reports could have been written by officers who were on patrol with the assaulted officer or by officers who responded as backup or became involved in the stop as a result of a BOLO ("be on the look-out") for a driver or passenger fleeing the scene. Multiple reports made it possible to compare and contrast different versions of the events for consistency and, to some extent, accuracy.

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136. In identifying pretextual stops, I looked for specific indicators in the officers' descriptions that suggested that before the officers pulled a vehicle over for a traffic violation, they were suspicious that the vehicle or its occupants were involved in crime beyond that traffic violation. I did not categorize cases as pretextual stops simply because the officers discovered evidence of crime beyond a traffic violation after it pulled over the vehicle during a routine traffic stop.

Because the narratives were almost always specific sections from the incident or arrest reports, many agencies sent entire copies of the reports. Additional sections in the reports often included the same information that was listed in the Florida LEOKA Database, including the extent of the injury the officer suffered and the type of weapon used during the incident. I was then able to compare and contrast the information from both sources to check for consistency and, to some extent, accuracy.

I gathered as many narratives as possible and prioritized which narratives to obtain based on a purposeful sampling strategy known as maximum variation sampling.[137] This strategy is a common qualitative strategy when a random sample cannot be drawn,[138] and it aims to capture and describe central themes and patterns that cut across study sites with a great deal of variation.[139] To maximize variation in the sample, I chose a large sample of law enforcement agencies that varied based on agency size, geographic area (urban, rural, suburban), and agency type (police departments and sheriff's offices[140]).[141]

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137. *See generally* PATTON, *supra* note 119, at 172 (providing an overview of maximum variation sampling). In this regard, the results of the study are not derived from a random nonprobability sample, which undercuts the generalizability of the findings. At the same time, the study adopted a maximum-variation sampling strategy with this limitation in mind.

138. STRAUSS & CORBIN, *supra* note 119, at 211 (explaining that "the ideal form of theoretical sampling might be difficult to carry out if a researcher does not have unlimited access to persons or sites" and that "[r]ealistically, the researcher might have to sample on the basis of what is available"). Because the study is based on a nonprobability sample, I did not use confidence intervals to validate my results. At the same time, the study adopted a maximum variation sampling strategy and a robust coding process with this limitation in mind.

139. PATTON, *supra* note 119, at 172 (noting that maximum-variation strategy sampling "aims at capturing and describing the central themes or principal outcomes that cut across a great deal of participant or program variation").

140. The distinction between police departments versus sheriff's offices was important for the study because 66 "of Florida's 67 counties have elected sheriffs as their chief law enforcement officers": only one county has an appointed sheriff as its chief law enforcement officer. *Florida Sheriff Directory: Majority Elected*, FLA. SHERIFF'S ASS'N, https://www.flsheriffs.org/sheriffs/directory [https://perma.cc/KGV2-7JUF]. Conversely, police chiefs in Florida are appointed officials. *See, e.g.*, Frank Maradiaga, *Boynton Beach Has Appointed a New Police Chief*, CBS 12 (Dec. 8, 2017), http://cbs12.com/news/local/boynton-beach-has-appointed-a-new-police-chief [https://perma.cc/ZHN6-REYZ] (describing appointment of Boynton Beach police chief); Linda Trischitta, *Miramar Appoints Longtime City Police Leader as Chief*, SUNSENTINEL (Mar. 18, 2016, 4:21 PM), http://www.sun-sentinel.com/local/broward/fl-miramar-police-chief-20160318-story.html (on file with the *Michigan Law Review*) (describing appointment of Miramar police chief). Distinguishing between police departments and sheriff's officers in methodological terms is also important given the tendency in the literature to think of these entities as the same. *See, e.g.*, James Tomberlin, Note, *"Don't Elect Me": Sheriffs and the Need for Reform in County Law Enforcement*, 104 VA. L. REV. 113, 116 (2018) (noting that "within policing scholarship, the county sheriff does not have an identity separate and distinct from other local law enforcement officers").

141. Kathleen M.T. Collins, *Advanced Sampling Designs in Mixed Research: Current Practices and Emerging Trends in the Social and Behavioral Sciences*, *in* SAGE HANDBOOK OF MIXED METHODS IN SOCIAL & BEHAVIORAL RESEARCH 353, 361 (Abbas Tashakkori & Charles Teddlie eds., 2d ed. 2010) (noting that "if the researcher's goal is to assess a range of variability

I adopted this sampling strategy because I expected that it would not be possible to collect all the narratives from the 6,903 total cases in the Florida LEOKA Database.[142] As explained above, the most common obstacle I encountered during data collection was that many law enforcement agencies did not keep records dating as far back as 2005. A few law enforcement agencies did not electronically store their case files, which made it difficult for custodians to retrieve the records. In addition, several departments had disbanded by the time of the study.

At the same time, it is important to emphasize that the rules regarding sample size differ for qualitative and quantitative methods.[143] In quantitative research, large sample sizes are typically needed for statistical tests that use deductive reasoning to generalize from a sample to an underlying population.[144] Large sample sizes for quantitative research thus serve the purpose of reducing sampling error.[145] In contrast, the inductive nature of qualitative research means that the validity and insights generated from qualitative data have "more to do with the information-richness of the cases selected" than with the size of the sample itself.[146] Decisions about sample size are largely guided by whether increasing the sample would provide new information that is not already forthcoming from the sampled units.[147]

Consistent with a major tenet of qualitative analysis, I did not come to the data in advance with presuppositions or hypotheses.[148] Rather, data analysis unfolded in three stages.[149] During the first stage—deconstruction (*open coding*)—I perused the data with an open mind to look for topics and concepts of interest. I then created subcategories in order to operationalize those

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within a sample" then "[w]hen using a maximum variation sampling scheme, the researcher likely would conduct a cross-case analysis involving a relatively large sample").

142. *See generally* JENNIFER MASON, QUALITATIVE RESEARCHING 91 (1996) ("[T]he pursuit of representativeness often requires the construction of very large samples which make the use of qualitative data generation methods very time consuming and costly and in many instances therefore impossible to achieve.").

143. David L. Morgan, *Sample*, *in* 2 THE SAGE ENCYCLOPEDIA OF QUALITATIVE RESEARCH METHODS 797 (Lisa M. Given ed., 2008) (noting that "qualitative and quantitative research emphasize different sample selection procedures that are specifically adapted to the purposes and goals that guide each kind of research").

144. *Id.*

145. JOHN W. CRESWELL, A CONCISE INTRODUCTION TO MIXED RESEARCH METHODS RESEARCH 76 (2015) (noting that for quantitative sampling "[i]t is important to select as large a sample as possible, because with a large sample there is less room for error in how well the sample reflects the characteristics of the population").

146. PATTON, *supra* note 119, at 185 (emphasis omitted).

147. *Id.* at 185–86 (quoting YVONNA S. LINCOLN & EGON G. GUBA, NATURALISTIC INQUIRY 202 (1985)).

148. *Id.* at 44 ("The qualitative methodologist attempts to understand the multiple interrelationships among dimensions that emerge from the data without making prior assumptions or specifying hypotheses about the linear or correlative relationships among narrowly defined, operationalized variables.").

149. These distinctions were only analytical, and the stages interwove in practice.

topics and concepts. During the second stage—construction (*axial coding*)—I went beyond labeling and categorizing the data to look for connections and relationships within and among the categories. I then reassembled the topics and concepts developed in the deconstruction phase into new patterns that reflected these connections and relationships. I also explored interesting patterns, whether the data related to what one might expect based on previous research or common sense, and whether there were any contradictions in the data. During the last phase—confirmation (*selective coding*)—I triangulated the data in order to confirm the validity of the study findings and conclusions.[150] I triangulated the data in several ways, including comparing data from law enforcement agencies based on agency size, geographic area (urban, rural, suburban), and agency type (police departments versus sheriff's offices).

To maximize the success of the data analysis process, I conducted a pilot phase of coding on a smaller subsample of narratives. This pilot phase assisted in identifying relevant codes for the broader sample. I also coded each narrative twice at different times of the data analysis process. In addition, to maximize the reliability of the coding process and the findings, four research assistants independently verified my codes for each narrative.[151]

The study, like all empirical studies, has limitations. The Florida LEOKA Database and the collected narratives derive from the FBI's LEOKA program. Because agencies voluntarily submit data to the LEOKA program, there are uncertainties about the extent to which law enforcement agencies in Florida participate in the LEOKA program. At the same time, it is important to keep in mind that LEOKA statistics are still viewed as the best official source of statistics on violence against officers in the United States, in spite of the voluntary reporting limitation.[152]

Nonetheless, I cross-checked the law enforcement agencies represented in the Florida LEOKA Database with a comprehensive list of law enforcement agencies in Florida and discovered 9 sheriff's offices and 60 police departments that were not represented in the Florida LEOKA Database. Most of these unrepresented agencies served smaller areas in terms of population.[153] It is impossible to know whether these agencies are not represented in the Florida LEOKA Database because they did not participate in the LEOKA program or, because of their smaller size, they had no incidents to

150. STRAUSS & CORBIN, *supra* note 119, at 230 (noting that selective coding "denotes the final step in the analysis—the integration of concepts around a core category and the filling in of categories in need of further development and refinement").

151. DAVID SILVERMAN, INTERPRETING QUALITATIVE DATA: METHODS FOR ANALYZING TALK, TEXT AND INTERACTION 148 (1993) (noting that inter-rater reliability "involves giving the same data to a number of analysts (or raters) and asking them to analyze it according to an agreed set of categories").

152. *See* Brandl, *supra* note 33, at 256.

153. As mentioned previously, in executing the maximum-variation sampling strategy, I distinguished agencies by their size. Therefore, smaller policing jurisdictions were sufficiently represented in the study.

report involving violence against officers during "traffic pursuits and stops" between 2005 and 2014.

Further, because arrest reports and probable cause affidavits tell versions of the events from the officer's perspective, perhaps the greatest methodological limitation of the study involves the limits of the arrest reports and probable cause affidavits themselves. For this reason, the decision to focus on the incident narratives warrants explanation. Arrest reports and probable cause affidavits often contain valuable information that the FBI does not collect or analyze under the LEOKA program.[154] In this study, almost all of the drivers and passengers in the evaluated cases were not only cited for traffic infractions, but also arrested for the crime of assault or battery on a law enforcement officer. Most narratives included detailed information about the incident, which was likely motivated by the officer's desire for the arrest and subsequent criminal charges to hold up in court.

Another benefit of these narratives is that their content often provided a lens into what the officers believed and saw, at what point of the routine traffic stop those beliefs and observations were made, and the time and events between those beliefs and observations and the purported violence against the officers. An understanding of these elements is critical given that institutional actors commonly prioritize officer perceptions in making determinations about the reasonableness of police activity and are hesitant to substitute their own judgment for that of trained officers.[155]

In addition, a sample based on the officers' perspectives should in theory provide the set of cases with versions of the events that are most favorable to affording deference to the police. That is especially the case for this study given that the officers had an incentive to include detail beyond boilerplate language in the incident description so that the arrest and subsequent charge for assaulting an officer could hold up in court. Accordingly, if the study findings from this set of cases casts doubt on officer safety as a justification for expansive police powers during routine traffic stops, then future research based on interviews of the stopped drivers and passengers would likely cast even further doubt.

At the same time, I fully recognize that officer and civilian descriptions of police encounters can and often do differ in meaningful ways. The rise of police body cameras and "copwatching" via cell phone recording has brought many discrepancies and questionable versions of police events into

154. Harmon, *supra* note 28, at 1136.

155. *See* Lvovsky, *supra* note 8 (discussing judicial presumptions involving police expertise); *see also* Graham v. Connor, 490 U.S. 386, 396 (1989) (noting that the reasonableness of police use of force under the Fourth Amendment "must be judged from the perspective of a reasonable officer on the scene, rather than with the 20/20 vision of hindsight"); Ristroph, *supra* note 8, at 1210 (noting that courts almost invariably defer to officers' perceptions of danger and resistance).

the public view.[156] Therefore, the narratives provide only one of many possible lenses to study violence against the police during routine traffic stops.

III. STATISTICAL FINDINGS

This Part presents key statistical findings of the study. In short, the findings do not support the dominant danger narrative surrounding routine traffic stops. The findings are organized into three Sections. First, I found that the bases of the stops in approximately one in every three of the assault narratives involved more than traffic enforcement or did not involve traffic enforcement at all. Second, the bulk of violence against officers was relatively minor—both in terms of the degree of officers' injuries and the weapons used against them. At the same time, I discovered that a very different picture of assaults against officers emerged when routine traffic stops were separated from criminal enforcement stops. This finding underscores the need to avoid adopting a monolithic conceptualization of routine traffic stops: the very error that enables oversimplified danger narratives. Third, the danger ratios involving routine traffic stops were low.

156. *See generally* David A. Harris, *Picture This: Body-Worn Video Devices (Head Cams) as Tools for Ensuring Fourth Amendment Compliance by Police*, 43 TEX. TECH. L. REV. 357, 364 (2010) (noting that police body cams will sometimes support citizen complaints over the officer's version of the events); Laurent Sacharoff & Sarah Lustbader, *Who Should Own Police Body Camera Videos?*, 95 WASH. U. L. REV. 269 (2017) (discussing police body cams and their connection to police accountability); Jocelyn Simonson, *Copwatching*, 104 CALIF. L. REV. 391 (2016) (describing the phenomenon of organized copwatching and its connection to police accountability); Stoughton, *Police Body-Worn Cameras*, *supra* note 10 (discussing the arguments for and against police body-worn cameras); Molly Hennessy-Fiske, *In Ferguson, Volunteers Have Electronic Eyes on Police*, L.A. TIMES (Oct. 3, 2014, 8:00 AM), http://latimes.com/nation/la-na-copwatch-20141003-story.html [https://perma.cc/DVG5-AWYP]. Advocates of body cameras within law enforcement circles have also argued that body cameras may assist in reducing false complaints of police misconduct. Mary D. Fan, *Justice Visualized: Courts and the Body Camera Revolution*, 50 U.C. DAVIS L. REV. 897, 926 & n.163 (2017).

A. *The Bases of the Stops*

Analysis of the 4,255 assault narratives that met the inclusion criteria of this study revealed four major types of stops in the sample. First, "routine traffic stops," which include motor vehicle stops based only on a traffic violation[157] or a checkpoint. Second, "criminal enforcement stops," which include motor vehicle stops initiated for the purpose of criminal enforcement beyond a traffic violation. These stops could also be, but are not necessarily, based on a traffic violation. Examples include felony vehicle stops, suspicious vehicle stops, pretextual stops,[158] hit-and-run cases, and motor vehicle stops for outstanding warrants. Third, "indeterminate stops," which include stops that involved a traffic violation, but it was unclear whether they fell into the routine traffic stop or criminal enforcement stop category. Fourth, "other stops," which include stops that did not involve motor vehicles (and were often erroneously classified as "traffic pursuits or stops") and encounters arising from motor vehicle accidents or crashes.

As Table 5 below shows, approximately one in every three of the 4,255 assault narratives did not involve a routine traffic stop. Rather, the stops had to do with criminal enforcement (for instance, a pretextual stop) or nothing to do with motor vehicle traffic enforcement (for instance, motor vehicle accidents or crashes). Put simply, many "traffic pursuits and stops" cases in the Florida LEOKA Database did not involve routine traffic stops, illustrating the breadth of this LEOKA category.

157. Although some traffic violations were more common than others, a broad range of traffic violations were represented in the sample, and many cases involved more than one observed traffic violation. Of the 2,911 cases in the "routine traffic stops" category, the most common reasons for pulling a car over were speeding (526 cases); erratic or reckless driving (378 cases); failing to stop at a stop sign (258 cases); no, improper, or expired tags or registration (231 cases); having no lights on or a broken headlight or taillight (224 cases); failure to maintain a single lane (126 cases); illegal window tints (123 cases); not wearing a seatbelt (118 cases); failure to stop at a red light (118 cases); suspicion of driving under the influence (109 cases); no tag light (108 cases); and knowledge of driving with no or an invalid or suspended license (87 cases). In 381 cases, the narratives simply stated "traffic infraction(s)" or "traffic violations." (Data on file with the author.)

158. Here, I acknowledge that the number of pretextual stops could be even higher in the sample for narratives that did not mention that the officers were suspicious that a vehicle or its occupants were engaged in crime beyond a traffic violation.

TABLE 5

Frequencies of Type of Stop for Assaults Against Officers	
Routine traffic stops	2,911
Traffic violation	2,897
Checkpoint	14
Criminal enforcement stops	804
Pretextual stop	256
Stolen vehicle/tag	251
Felony vehicle stop	141
Hit and run	54
Outstanding warrant	45
Suspicious vehicle stop	44
Previously eluded police	13
Indeterminate stops	64
Unclear whether traffic or criminal	64
Other stops	476
Not a vehicle stop	208
Bicycle case	142
Crash or accident	89
Golf cart/ATV	17
Pedestrian	19
Gas station drive off	1
Total number of narratives	**4,255**

In my analysis, I separated the 9 cases involving the felonious killings of officers from the 4,255 narratives involving assaults against officers. A similar diversity emerged in the 9 felonious killings of officers. Five cases involved routine traffic stops: those stops were based on speeding, not stopping at a stop sign, no visible license plate, careless driving, and an unidentified minor traffic violation. Three cases involved criminal enforcement stops: one of those stops was initiated to investigate robbery suspects, another involved an officer who was checking the license plate of a parked car to see if the car was stolen, and the other involved a pretextual stop of a suspicious vehicle in a high-crime area. In the remaining case, the basis of the vehicle stop was unclear from the available information.[159]

B. *Nature of the Violence: Officer Injury, Weapon Type, and Time of Day*

Going beyond what previous studies have done, the stop classification scheme described above allowed me to examine whether the nature of the

159. At the same time, the low number of felonious killings of officers makes it difficult, if not impossible, to apply common statistical methods to identify relationships beyond these basic qualitative trends. *See supra* notes 141–148 (discussing the differences between the different uses and purposes of large sample sizes in qualitative and quantitative research).

violence against officers differed across major stop types. Here, I was particularly concerned with routine traffic stops and criminal enforcement stops. I then compared this more granular view within both stop categories with the overall totals in the Florida LEOKA Database.[160]

Consistent with the LEOKA reporting scheme, officer injuries were separated into three categories: no injury, minor injury, or serious injury.[161] Table 6 below shows that, contrary to the dominant danger narrative, most assault cases in the routine traffic stop and criminal enforcement stop categories as well as in the Florida LEOKA Database involved no injury or minor injuries to officers. Incidents that resulted in serious injuries to officers accounted for less than 2% of the cases in both major stop categories as well as in the Florida LEOKA Database overall.

TABLE 6

Frequencies of Injury by Type of Stop for Assault Cases[162]						
	Routine Traffic Stops		***Criminal Enforcement Stops***		***Florida LEOKA Database***	
Injury	*Freq.*	*% Dist.*	*Freq.*	*% Dist.*	*Freq.*	*% Dist.*
None	2,253	77.40	671	83.46	5,299	76.86
Minor	615	21.13	118	14.68	1,464	21.24
Serious	43	1.48	15	1.87	131	1.90
Total	**2,911**	**100**	**804**	**100**	**6,894**	**100**

With regard to weapons, my analysis of the narratives revealed that many cases categorized under the "gun" or "knife/cutting object" category in the Florida LEOKA Database did not actually involve a gun or a knife that caused injury to an officer. Rather, a gun or knife had been found at the scene, and to the extent that an officer suffered an injury, it derived from anoth-

160. As discussed *supra* in Part II, extent of the injury and weapon used were codes in the Florida LEOKA Database. To reiterate, this database includes the total number of LEOKA "traffic pursuits and stops" cases during the relevant years (whether I was able to obtain a narrative or not).

161. As noted previously, the Florida Department of Law Enforcement's *Uniform Crime Reports Guide Manual* defines "serious injury" as "injury so severe that it results in disablement or disfigurement." FLA. DEP'T OF LAW ENF'T, *supra* note 19, at 27. "Examples of serious injury include broken bones, loss of teeth, lacerations so severe that stiches are needed, internal injuries, injuries resulting in paralysis or the deprivation of a limb/body part, loss of consciousness, etc." *Id.*

162. Although not presented in this table, I also calculated these figures for the indeterminate stops and other stops. For the 64 indeterminate stops, 53 (82.81%) involved no injury, 10 (15.63%) involved a minor officer injury, and 1 (1.56%) involved a serious officer injury. For the 476 "other" stops, 345 (72.48%) involved no injury, 116 (24.37%) involved a minor injury, and 15 (3.15%) involved a serious injury.

er weapon source. Table 7 below presents the findings regarding the type of weapon for the assault cases. Cases in which guns or knives were used or found accounted for approximately 2.5% to 4% of cases in both major stop type categories as well as in the Florida LEOKA Database overall. Although more sophisticated quantitative tools are necessary to determine whether the differences are significant, a higher percentage of evaluative narratives in criminal enforcement stops (3.36%) involved guns than in routine traffic stops (1.99%) or the Florida LEOKA Database overall (2.48%).

Further analysis also revealed that, contrary to the dominant danger narrative, cases involving officers who were seriously injured or feloniously killed with guns or knives were even more rare and comprised less than 1% of the cases in both major stop categories as well as in the Florida LEOKA Database overall. Specifically, these incidents comprised 0.31% of the routine traffic stops, 0.50% of the criminal enforcement stops, and 0.29% of the cases in the Florida LEOKA Database.[163]

163. These percentages were calculated as follows: *routine traffic stops* [(4 felonious killing cases involving guns or knives during routine traffic stops + 5 serious injury cases involving guns or knives during routine traffic stops)/(5 felonious killings involving routine traffic stops + 2,911 assault cases involving routine traffic stops)]; *criminal enforcement stops* [(3 felonious killings involving guns or knives during criminal enforcement stops + 1 serious injury case involving guns or knives during criminal enforcement stops)/(3 felonious killings involving criminal enforcement stops + 804 assault cases involving criminal enforcement stops)]; *Florida LEOKA Database* [(9 felonious killings involving "traffic pursuits and stops" + 11 serious injury cases involving guns or knives during "traffic pursuits and stops")/(9 felonious killings involving "traffic pursuits and stops" + 6,894 assault cases involving "traffic pursuits and stops")].

TABLE 7

Frequencies of Weapons by Type of Stop for Assault Cases[164]						
	Routine Traffic Stops		***Criminal Enforcement Stops***		***Florida LEOKA Database***	
Weapon	*Freq.*	*% Dist.*	*Freq.*	*% Dist.*	*Freq.*	*% Dist.*
Hands/Fists/Feet	1,761	60.49	262	32.59	3,898	56.54
Other	998	34.28	474	58.96	2,564	37.19
Blunt Object	60	2.06	27	3.36	157	2.28
Gun[165]	58	1.99	27	3.36	171	2.48
Knife/Cutting Object	17	0.58	4	0.50	40	0.58
Unknown	14	0.48	10	1.24	58	0.84
Fire/Incendiary	3	0.10	0	0	6	0.09
Total	**2,911**	**100**	**804**	**100**	**6,894**	**100**

Moreover, Table 7 above shows that two very different pictures of violence emerged under the routine traffic stop and criminal enforcement stop categories. For routine traffic stops, the most commonly used weapon against officers was "personal weapons"—namely, the driver's or passenger's hands, fists, or feet. For the criminal enforcement stops, "other" weapons were the most commonly used weapon against officers. From the narratives, I discovered that "other" weapons usually involved the use of the motor vehicle as a weapon against the officer while it was moving (for instance, attempting to use a car to hit an officer who then gets injured while trying to get out of the way) or parked (for instance, opening and hitting an officer with a car door).[166] As Table 7 shows, approximately three in every five routine traffic stops in which an assault on an officer occurred involved an assault with a driver' or passenger's hands, fists, or feet, whereas approximately

164. Although not presented in this table, I also calculated these figures for the indeterminate stops and "other" stops. For the 64 indeterminate stops, 35 cases (54.69%) involved "other," 24 cases (37.50%) involved hands/fists/feet; 3 cases (4.69%) involved a blunt object, and 2 cases (3.13%) involved a gun. For the 476 "other" stops, 344 cases (72.27%) involved hands/fists/feet, 105 cases (22.06%) involved "other," 10 cases (2.10%) involved a knife/cutting object, 8 cases (1.68%) involved a gun, 6 cases (1.26%) involved a blunt object, 2 cases (0.42%) involved unknown weapons, and 1 case (0.21%) involved fire/incendiary.

165. This category combines incidents involving firearms, handguns, rifles, and shotguns—each of which have their own codes under the LEOKA program.

166. More specifically, I discovered from the narratives that for routine traffic stops, 904 of the 998 "other" weapon cases (90.58%) involved vehicles used as weapons against officers. Moreover, 44 of the 60 "blunt object" cases (73.33%) and 7 of the 14 "unknown" weapon cases (50.00%) also involved vehicles used as weapons against officers. Common "other" weapons besides vehicles included spit, dogs, and car keys. For criminal enforcement stops, 459 of the 474 "other" weapon cases (96.84%) involved vehicles used as weapons against officers. Moreover, 25 of the 27 "blunt object" cases (92.59%) and 6 of the 10 "unknown" weapon cases (60.00%) also involved vehicles used as weapons against officers.

one in every three assaults involved the car itself as a weapon. These ratios flipped for criminal enforcement stops.

This contrasting picture of violence held steady when separating the smaller subset of cases that resulted in serious injury to officers by weapon type. Table 8 presents these findings.

TABLE 8

Frequencies of Weapons by Type of Stop for Assault Cases that Resulted in Serious Injury to an Officer[167]						
	Routine Traffic Stops		***Criminal Enforcement Stops***		***Florida LEOKA Database***	
Weapon	*Freq.*	*% Dist.*	*Freq.*	*% Dist.*	*Freq.*	*% Dist.*
Hands/Fists/Feet	27	62.79	4	26.67	78	59.54
Other	11	25.58	10	66.67	42	32.06
Gun	5	11.63	1	6.67	10	7.63
Knife/Cutting Object	0	0	0	0	1	0.76
Total	**43**	**100**	**15**	**100**	**131**	**100**

As Table 8 shows, hands/fists/feet were the most common weapon used during routine traffic stops that resulted in serious injury to an officer. In addition, hands/fists/feet accounted for 62.79% of routine traffic stops that resulted in serious injury to an officer, whereas "other" weapons (mostly, the motor vehicle)[168] accounted for 25.58%. These percentages flipped for criminal enforcement stops. "Other" weapons accounted for 66.67% of the criminal enforcement stops that resulted in serious injury to officers, whereas hands/fists/feet accounted for 26.67%.[169]

167. Although not presented in this table, I also calculated these figures for the indeterminate stops and "other" stops. Only 1 of the 64 indeterminate stops resulted in serious injury to an officer and that case involved an "other" weapon. Of the 476 "other" stops, 15 cases involved a serious injury to officers. Of those cases, 10 (66.67%) involved hands/fists/feet and 5 (33.37%) involved "other" weapons.

168. Motor vehicles were used to assault officers in 9 of the 11 routine traffic stop cases involving "other" weapons that resulted in serious injury to an officer.

169. The narratives revealed that all 10 of the criminal enforcement stops involving "other" weapons that resulted in serious injury to an officer involved motor vehicles used as weapons against officers. In addition, different patterns regarding gun violence emerged within the routine traffic stop and criminal enforcement stop categories. On one hand, Table 7, *supra*, shows that guns were found or used in a higher proportion of cases for criminal enforcement stops compared to routine traffic stops (3.36% versus 1.99%). On the other hand, Table 8, *supra*, shows that guns were found or used in a higher proportion of cases that resulted in serious injuries to officers during routine traffic stops compared to criminal enforcement stops (11.63% versus 6.67%). The number of cases in the gun violence category, however, was small and could be driving these patterns. Therefore, one caveat that is important to stress is that

Table 8 above further shows that the proportion of assaults that involve hands/fists/feet versus "other" weapons in the Florida LEOKA Database was more consistent with the routine traffic stops than the criminal enforcement stops. Put another way, the overall picture of violence that emerges from the official LEOKA statistics did not map onto all stop types. In this regard, the findings indicate that the overinclusive "traffic pursuits and stops" LEOKA category can obscure differences in the nature of the violence between different stop types. As discussed later in this Article, this point has significant implications for police training and Fourth Amendment doctrine, and it underscores the need to avoid conceptualizing routine traffic stops through monolithic classifications and oversimplified danger narratives.[170]

Finally, common trends emerged regarding time of day that applied to both routine traffic stops and criminal enforcement stops. As Table 9 below shows, in both stop categories assaults against officers started to increase in the late afternoon to early evening hours, hit their peak during the late night hours, and began to decline in the early morning hours.

TABLE 9

Frequencies of Assaults by Time of Day						
	Routine Traffic Stops		***Criminal Enforcement Stops***		***Florida LEOKA Database***	
	Freq.	*% Dist.*	*Freq.*	*% Dist.*	*Freq.*	*% Dist.*
12:00am–2:59am	700	24.05	167	20.77	1,535	22.27
3:00am–5:59am	236	8.11	64	7.96	605	8.78
6:00am–8:59am	94	3.23	29	3.61	252	3.66
9:00am–11:59am	177	6.08	76	9.45	462	6.70
12:00pm–2:59pm	228	7.83	72	8.96	588	8.53
3:00pm–5:59pm	322	11.06	115	14.30	843	12.23
6:00pm–8:59pm	456	15.66	138	17.16	1,115	16.17
9:00pm–11:59pm	698	23.98	143	17.79	1,494	21.67
Total	**2,911**	**100**	**804**	**100**	**6,894**	**100**

C. *Danger Ratios*

This Section presents two levels of danger ratios[171] from the sample. First, to provide a point of comparison with Lichtenberg and Smith's study, it presents danger ratios for the "traffic pursuits and stops" cases in the Flor-

more nuanced statistics are needed to make sense of these patterns and to determine whether they apply on a more general scale.

170. *See infra* Part V.

171. To reiterate, danger ratios are a metric that captures the dangerousness of police encounters to officers. These ratios are calculated by dividing the total number of relevant harmful incidents by the total number of relevant police responses. *See* Lichtenberg & Smith, *supra* note 104, at 422.

ida LEOKA Database. Second, going beyond what previous studies have been able to do, this Section then provides the first estimate of danger ratios for routine traffic stops that only involve traffic violations. Consistent with prior studies, both levels of danger ratios were low—providing additional empirical evidence that undermines the dominant danger narrative surrounding routine traffic stops.

Beginning with the denominator of the danger ratio, the State of Florida keeps records of the total number of citations issued for traffic violations each year, but not the total number of conducted stops. Just under 46 million traffic citations were issued in Florida between 2005 and 2014, which translates into a yearly average of just under 4.6 million citations.[172] Two uncertainties, however, arise from citation data: (1) the number of traffic stops that are conducted that do not result in a citation; and (2) the number of traffic stops that result in multiple citations.

To account for the number of stops that are conducted that do not result in a citation, I followed the approach from Lichtenberg and Smith's study to calculate low-end (4.6 million stops), mid-range (9.2 million stops), and high-end (13.8 million stops) frequency estimates. The low-end estimate assumes that the number of citations is equivalent to the number of traffic stops. The mid-range estimate is based on one scholarly approximation that only 1 in every 2 traffic stops results in a citation.[173] The high-range estimate is based on an alternative scholarly estimate that only 1 in every 3 traffic stops results in a citation.[174]

There is a dearth of research on how many traffic stops involve multiple citations and how many citations are issued during those stops. It is important to attempt to account for multiple-citation stops, however, because of the disparities that are involved in those stops. Research shows that people of color are more likely to be cited multiple times in an individual traffic stop than white individuals.[175] To account for multiple-citation stops, I relied on

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172. *See Crash and Citation Reports & Statistics*, FLA. HIGHWAY SAFETY & MOTOR VEHICLES, http://www.flhsmv.gov/resources/crash-citation-reports/ [https://perma.cc/D46N-YALQ]. The minimum total number of annual citations was in 2014 (approximately 3.6 million) and the maximum was in 2007 (approximately 5.3 million). *Id.*

173. Lichtenberg & Smith, *supra* note 104, at 421 (citing DAVID H. BAYLEY, POLICE FOR THE FUTURE 30 (1994)).

174. *Id.* (citing MICHAEL K. BROWN, WORKING THE STREET 227 (1981)).

175. *See* Barnes & Chang, *supra* note 35, at 687 (critically evaluating one study by stressing that "[w]e question why the researchers find the one recorded violation condition so helpful in a data set where the most concerning racially disparate result has to do with multiple citations and citation seriousness"). The report from the U.S Department of Justice on the Ferguson Police Department documented such evidence of racial disparities in multiple citations for individual traffic stops. U.S. DEP'T OF JUSTICE, CIVIL RIGHTS DIV., INVESTIGATION OF THE FERGUSON POLICE DEPARTMENT 66 (2015), https://www.justice.gov/sites/default/files/opa/press-releases/attachments/2015/03/04/ferguson_police_department_report.pdf [https://perma.cc/X5Q6-2JRB]. The report stressed that in 2013, more than 50% of all African Americans received multiple citations with a single encounter with the Ferguson Police De-

two studies that offer different estimates of the percentage of traffic stops that involve multiple citations. I then used both approximations to split the low-end, mid-range, and high-end estimates derived from the approach taken in Lichtenberg and Smith's study into three additional frequency tiers.

For the high frequency estimate in each tier, I assumed that each individual traffic citation corresponded to an individual traffic stop. In this regard, the low-end, mid-range, and high-end estimates using the approach in Lichtenberg and Smith's study became the high frequency estimate in each of these three new frequency tiers (high-high-end; high-mid-range; and high-low-end). I adopted the following formula to calculate a middle frequency estimate in each tier (mid-high-end; mid-mid-range; and mid-low-end): 88.5% of stops result in 1 citation; 10% result in 2 citations; and 1.5% result in 3 or more citations.[176] Finally, I adopted the following formula to calculate a low frequency estimate in each tier (low-high-end; low-mid-range; low-low-end): 71.75% of stops result in 1 citation; 18.75% result in 2 citations; 6.5% result in 3 citations; 2% result in 4 citations; and 1% result in 5 or more citations.[177] These three approaches yielded nine separate danger

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partment, whereas only 26% of non–African Americans did. *Id.* The racial disparities were even greater for incidents that resulted in more than two citations. *Id.* Some jurisdictions use traffic fines as a means of revenue and target low-income communities and communities of color to do so. *See* Beth A. Colgan, *The Excessive Fines Clause: Challenging the Modern Debtors' Prison*, 65 UCLA L. REV. 2, 23 & n.23, 31, 34 (2018).

176. To calculate this mid-frequency range estimate, I looked to one recent study of a major police department in Virginia that tracked over 75,000 traffic stops over a 4-year period and found that 88.6% of the traffic stops resulted in only a single citation; 9.85% resulted in 2 citations; and 1.55% involved 3 or more citations. *See* CYNTHIA LUM & XIAOYUN WU, BASIC ANALYSIS OF TRAFFIC CITATION DATA FOR THE ALEXANDRIA POLICE DEPARTMENT (2011–2015) 22 (2017), https://www.alexandriava.gov/uploadedFiles/police/info/AlexandriaPDTrafficCitationAnalysisReportFINAL.pdf [https://perma.cc/428B-CVSH].

177. A different 3-year statewide study on traffic stops in Arizona tracked the number of citations issued in traffic stops conducted by officers each year. The study reported data for 2008, 2007, and 2006, but only the 2007 and 2006 data reported detailed data on the percentages of individual traffic stops that resulted in 3 or more citations. Therefore, I only used the 2007 and 2006 data to approximate the percentage of stops that result in 3 or more citations.

For 2007, the study reported 485,183 recorded traffic stops initiated by police officers. ROBIN S. ENGEL ET AL., TRAFFIC STOP DATA ANALYSIS STUDY: YEAR 2 FINAL REPORT x (2008), http://www.azdps.gov/sites/default/files/media/Traffic_Stop_Data_Report_2008.pdf [https://perma.cc/FSL9-U8KV]. The number of traffic citations issued during an individual traffic stop ranged from 0 to 6 citations. *Id.* at 71. Regarding number of citations, 54.8% of stops did not result in a citation, 32.8% resulted in 1 citation, 8.4% resulted in 2 citations, 2.9% resulted in 3 citations, 0.8% resulted in 4 citations, 0.4% resulted in 5 citations; and less than 0.001% resulted in 6 citations. *Id.* Excluding the stops that did not result in a citation yields the following estimates for the number of stops that resulted in a citation for 2007: 159,140 stops (1 citation), 40,755 stops (2 citations), 13,585 stops (3 citations), 3,881 stops (4 citations), 1,940 stops (5 or more citations).

For 2006, the study reported 460,545 recorded traffic stops initiated by police officers. ROBIN S. ENGEL ET AL., TRAFFIC STOP DATA ANALYSIS STUDY: YEAR 1 FINAL REPORT xii (2007), http://www.azdps.gov/sites/default/files/media/Traffic_Stop_Data_Report_2007.pdf [https://perma.cc/F8SL-63N6]. The number of citations issued during an individual traffic stop

ratios for each examined type of violence against officers that ranged from most to least conservative.

Table 10 presents and compares the danger ratios from the Florida LEOKA Database with the danger ratios from Lichtenberg and Smith's study. Overall, the danger ratios from the Florida LEOKA Database were higher than the danger ratios from Lichtenberg and Smith's study for both officer killings and assaults.[178]

ranged from 0 to 6. *Id.* at 122. Regarding number of citations, 54% of the stops did not result in a citation, 32.7% resulted in 1 citation, 8.7% resulted in 2 citations, 3.2% resulted in 3 citations, 1.0% resulted in 4 citations, 0.4% resulted in 5 citations, and 0.001% resulted in 6 citations. *Id.* Excluding the stops that did not result in a citation yields the following estimates for the number of stops that resulted in a citation for 2006: 150,598 stops (1 citation), 40,067 stops (2 citations), 14,737 stops (3 citations), 4,605 stops (4 citations), 1,842 stops (5 or more citations).

Averaging these estimates from the 2007 and 2006 data yielded the following results: 71.84% (1 citation), 18.74% (2 citations), 6.57% (3 citations), 1.97% (4 citations), .88% (5 or more citations). This formed the basis of the final low frequency tier estimates for the percentage of stops that result in a citation, excluding the stops that did not result in a citation: 71.75% (1 citation), 18.75% (2 citations), 6.5% (3 citations), 2% (4 citations), 1% (5 or more citations).

178. There are many ways to interpret these differences. One takeaway is that danger ratios may vary within and across geographic areas. Lichtenberg and Smith's ratios were based on national estimates, suggesting that the stops from the Florida LEOKA Database were on average more dangerous than the national averages captured in Lichtenberg and Smith's study.

TABLE 10

Danger Ratio Comparisons[179]					
Lichtenberg & Smith (2001)			***Florida LEOKA Database***		
	Killings	*Assaults*		*Killings*	*Assaults*
Low-End	1 in 6.7 M	1 in 10,256	*Low-Low-End*	1 in 3.61M*	1 in 4,714*
			Mid-Low-End	1 in 4.52M*	1 in 5,904*
			High-Low-End	1 in 5.11M*	1 in 6,672*
Mid-Range	1 in 13.4 M	1 in 20,512	*Low-Mid-Range*	1 in 7.22M*	1 in 9,428*
			Mid-Mid-Range	1 in 9.04M*	1 in 11,807*
			High-Mid-Range	1 in 10.22M*	1 in 13,345*
High-End	1 in 20.1 M	1 in 30,768	*Low-High-End*	1 in 10.83M*	1 in 14,143*
			Mid-High-End	1 in 13.57M*	1 in 17,711*
			High-High-End	1 in 15.33M*	1 in 20,017*
** Number of "traffic pursuits [or] stops"*					

At the same time, the danger ratios from the Florida LEOKA Database were still very low. Taking the most conservative ratio (the low-low-end estimate), the rate of a felonious killing of an officer was only 1 in every 3.6 million stops. Moreover, the rate for an assault (whether it resulted in injury or not to an officer) was only 1 in every 4,714 stops. The least conservative estimate suggests that the rate of violence for felonious killings and assaults is much lower: 1 in 15.3 million stops for felonious killings and 1 in 20,017 stops for assaults.

To provide an idea of how these danger ratios compare to other policing contexts, consider Rose Mary Stanford and Bonney Lee Mowry's study, which calculated the danger ratios for domestic and general disturbance calls.[180] Notably, Stanford and Mowry's study was based on a large dataset

179. The danger ratios involving felonious killings and assaults in the Florida LEOKA Database were calculated as follows. For the denominator for both felonious killings and assaults, I first calculated the three frequency tiers for the low-end estimate. The high-low-end estimate is based on the assumption that each individual stop produces a single traffic citation. On average, 4.6 million traffic citations were issued year, rendering a high-low-end estimate of 4.6 million stops.

The mid-range estimate is based on the approximation described above that 88.5% of stops result in 1 citation; 10% result in 2 citations; and 1.5% result in 3 or more citations. To simplify the calculation, I limited 3 or more citations to only 3. Based on these figures, every 200 stops

from the Tampa Police Department,[181] which is one of the police agencies included in my study. Stanford and Mowry estimated that the rate for assault against an officer while handling a domestic disturbance call was 1 in every 385 calls,[182] which is over 12 times higher than the most conservative low-end danger ratio for assaults in the Florida LEOKA Database. In addition, Stanford and Mowry estimated that the rate for assault against an officer while handling a general disturbance call was 1 in every 323 calls,[183] which is over 14 times higher than the most conservative low-end danger ratio for assaults in the Florida LEOKA Database.

The findings presented in Section III.B revealed that only a very small percentage (less than 2%) of the total 6,894 "traffic pursuits and stops" assault cases in the Florida LEOKA Database resulted in serious injury to officers. With more detailed information on the extent of officer injury, I was able to go beyond Lichtenberg & Smith's study to calculate the danger ratios for assault cases based on injury. As Table 11 shows, the danger ratios for assaults resulting in serious injury to officers were dramatically lower than assaults that resulted in no or minor injury to officers.[184] Taking the most con-

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will yield 226 citations: 177 stops will involve 1 citation, 20 stops will involve 2 citations, 3 stops will involve 3 citations. Applying these figures, 4.6 million citations equate to mid-low-range estimate of approximately 4.07 million stops: 3.6 million stops will involve 1 citation, 0.41 million stops will involve 2 citations, and 0.06 million stops will involve 3 citations.

The high-range estimate is based on the approximation that 71.75% of stops result in 1 citation; 18.75% result in 2 citations; 6.5% result in 3 citations; 2% result in 4 citations; and 1% result in 5 or more citations. To simplify the calculation, I limited 5 or more citations to only 5. Based on these figures, every 400 stops will yield 567 citations: 287 stops will involve 1 citation, 75 stops will involve 2 citations, 26 stops will involve 3 citations, 8 stops will involve 4 citations, and 4 stops will involve 5 citations. Applying these figures, 4.6 million citations equate to a high-low-end estimate of approximately 3.25 million stops: 2.34 million will involve 1 citation, 0.61 million will involve 2 citations, 0.21 million will involve 3 citations, 0.06 million will involve 4 citations, and 0.03 million will involve 5 or more citations.

For the numerator for felonious killings, I divided the total number of felonious killings involving "traffic pursuits and stops" (9 felonious killings) by 10 to obtain the average number of felonious killings of officers per year. I then took the reciprocal of the denominator divided by the numerator to obtain the appropriate ratio. For the numerator for assaults, I divided the total number of assaults involving "traffic pursuits and stops" (6,894 assaulted cases) by 10 to obtain the average number of assaults against officers per year. I then took the reciprocal of the denominator divided by the numerator to obtain the appropriate ratio.

Once I had the three frequency tier estimates for the low-end estimates, I could then divide those estimates by two and three to obtain the three frequency tier estimates for the mid-range and high-range estimates.

180. Rose Mary Stanford & Bonney Lee Mowry, *Domestic Disturbance Danger Rate*, 17 J. POLICE SCI. & ADMIN. 244 (1990).

181. *Id.* at 245.

182. *See id.* at 276 (reporting that "the potential for assault while handling a domestic disturbance call is estimated at 2.6 per 1000 calls").

183. *See id.* (reporting that the rate of assault against an officer while handling general disturbance calls is 3.1 assaults per 1000 calls).

184. Here, I acknowledge that the line between no injury, minor injury, and serious injury is arguably fortuitous. An assault that ended in no injury very well could have resulted in a

servative estimate (low-low-end ratio), the rate for an assault that resulted in serious injury to an officer (regardless of weapon type) was only 1 in every 248,092 "traffic pursuits [or] stops." The least conservative estimate suggests that the rate of violence that results in serious injury to an officer is much less: 1 in 1.05 million stops.

TABLE 11

Danger Ratio Comparisons for the Florida LEOKA Database,[185] Assaults Based on Officer Injury			
	No Injury	*Minor Injury*	*Serious Injury*
Low-Low-End	1 in 6,133*	1 in 22,199*	1 in 248,092*
Mid-Low-End	1 in 7,681*	1 in 27,801*	1 in 310,687*
High-Low-End	1 in 8.681*	1 in 31,421*	1 in 351,145*
Low-Mid-Range	1 in 12,266*	1 in 44,399*	1 in 496,183*
Mid-Mid-Range	1 in 15,361*	1 in 55,601*	1 in 621,374*
High-Mid-Range	1 in 17,362*	1 in 62,842*	1 in 702,290*
Low-High-End	1 in 18,400*	1 in 66,598*	1 in 744,275*
Mid-High-End	1 in 23,042*	1 in 83,402*	1 in 932,061*
High-High-End	1 in 26,043*	1 in 94,262*	1 in 1.05M*
			** Number of "traffic pursuits [or] stops"*

Although the danger ratios that emerge from the Florida LEOKA Database provide useful insight, even these estimates are too high because of the overinclusivity problems with available LEOKA statistics. In analyzing the incident narratives of the underlying LEOKA cases, I was able to provide the first estimates of danger ratios for routine traffic stops that only involve traffic violations. These danger ratios are a major contribution given the methodological shortcomings of available LEOKA statistics and the tendency for institutional actors to rely on LEOKA statistics when evaluating the dangerousness of routine traffic stops based on traffic violations.[186]

For routine traffic stops, I calculated moderate and maximum estimates for three sets of danger ratios: (1) felonious officer killings; (2) assaults resulting in serious injuries to officers; and (3) assaults against officers (whether resulting in officer injury or not). The moderate estimate captures where the direction of the data from the sample indicates that the danger ratios

serious injury. To reiterate, the danger ratio is predicated on what *actually* happened and not what *could* have happened during the traffic stop.

185. To calculate these danger ratios, I used the same method discussed in *supra* note 179 to calculate the denominator. The value of the numerator, however, changed. As Table 6 shows, 5,299 of the 6,894 total assault cases resulted in no injury; 1,464 cases resulted in minor injury; and 131 cases resulted in serious injury. I divided those values by 10 to obtain the average number of cases each year that resulted in no injury, minor injury, or serious injury to an officer.

186. *See supra* Part I; *see also infra* Section V.B.2.

most likely fall, whereas the maximum estimate captures the mathematically highest possibility for the danger ratios. At the outset, it is important to recognize that several methodological assumptions shaped the calculations. These assumptions took different approaches to account for the narratives that I was unable to obtain and thus analyze from the Florida LEOKA Database.[187] Table 12 presents the danger ratios for the routine traffic stops.

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187. To explain those methodological assumptions in greater detail, first, for the danger ratios involving felonious killings, my analysis revealed that of the 9 cases involving felonious killings, 5 involved routine traffic stops, 3 involved criminal enforcement stops, and 1 involved indeterminate stops. For the moderate estimate, I assumed that the 1 indeterminate stop did not involve a routine traffic stop. For the maximum estimate, I assumed that the indeterminate stop involved a routine traffic stop, bringing the total to 6 felonious killings. Although the basis of the indeterminate stop is not entirely clear, the record indicates that the officer who conducted the vehicle stop was part of an agency operation in a high-crime neighborhood that attempted to catch thieves looking to steal recently bought Christmas gifts from homes or cars.

For the danger ratios involving assaults generally, my analysis revealed that 2,911 of the 4,255 assault narratives that met the inclusion criteria fell under the routine traffic stop category and 1,344 assault cases did not involve routine traffic stops (804 criminal enforcement stops, 476 "other" stops, and 64 cases indeterminate stops). *See supra* Section III.A, Table 5. This left 2,639 assault narratives to account for. For the moderate estimate, I calculated the total number of assaults for routine traffic stops as 4,670. Here, I applied my finding that two in every three cases in the Florida LEOKA Database fell into the routine traffic stop category in order to approximate that 1,759 (or two-thirds) of the 2,639 narratives that I could not obtain, read, or that had insufficient information involved a routine traffic stop. I then added this 1,759 value to the 2,911 cases that fell under the routine traffic stop category from the evaluated assault narratives to arrive at a total moderate estimate of 4,670. For the maximum estimate, I calculated the total number of assaults for routine traffic stops as 5,550. Here, I assumed that each of the 2,639 cases for which I was unable to obtain, read, or that had insufficient information fell under the routine traffic stops category and added that value to the 2,911 cases that fell under the routine traffic stop category from the evaluated assault narratives.

For the danger ratios that involved assaults resulting in serious injury to officers, three findings shaped my assumptions in calculating the moderate and maximum estimates. First, my analysis of the narratives that met the inclusion criteria revealed 43 cases that involved routine traffic stops that resulted in serious injury to officers and 31 cases that did not involve routine traffic stops (15 criminal enforcement stops, 15 "other" stops, and 1 indeterminate stop). *See supra* Part III.B, Table 6 & note 160. Second, from the information in the Florida LEOKA Database, I also knew that 131 of the 6,894 assault cases in the database involved serious injuries against officers. Therefore, there were 57 remaining cases involving serious injury to officers to account for. *See supra* Section III.B, Table 6. Third, less than 2% of all stops under each major stop category in which the basis was described (routine traffic stops, criminal enforcement stops, and "other" stops) as well as in the total number of "traffic pursuits and stops" cases in the Florida LEOKA database involved serious injuries to officers. *See supra* Section III.B, Table 6 & note 160. Thus, for the moderate estimate, I estimated the total number of routine traffic stops that resulted in serious injury to officers by averaging two estimates: (1) two-thirds of 131 = 87 (based on my estimate that two-thirds of the total cases in the Florida LEOKA Database involved routine traffic stops), and (2) 2% of 4,670 = 93 (which combined: (1) my findings that just under 2% of the cases in each major stop category resulted in serious injuries to officers and (2) my moderate estimate above that 4,670 cases in the Florida LEOKA Database involved routine traffic stops). The average of those two calculations came to a moderate estimate of 90 cases that involved routine traffic stops that resulted in serious injury to an officer. For the maximum estimate, I assumed that all of the 57 narratives involving serious officer injuries

TABLE 12

Danger Ratios for Routine Traffic Stops						
	Moderate Estimate			***Maximum Estimate***		
	Killings	*Assaults*	*Serious Injury*	*Killings*	*Assaults*	*Serious Injury*
Low-Low-End	1 in 6.5M*	1 in 6,959*	1 in 361,111*	1 in 5.42M*	1 in 5,856*	1 in 325,000*
Mid-Low-End	1 in 8.14M*	1 in 8,715*	1 in 452,222*	1 in 6.78M*	1 in 7,333*	1 in 407,000*
High-Low-End	1 in 9.2M*	1 in 9,850*	1 in 511,111*	1 in 7.67M*	1 in 8,288*	1 in 460,000*
Low-Mid-Range	1 in 13M*	1 in 13,919*	1 in 722,222*	1 in 10.83M*	1 in 11,712*	1 in 650,000*
Mid-Mid-Range	1 in 16.28M*	1 in 17,430*	1 in 904,444*	1 in 13.57M*	1 in 14,667*	1 in 814,000*
High-Mid-Range	1 in 18.4M*	1 in 19,700*	1 in 1.02M*	1 in 15.33M*	1 in 16,577*	1 in 920,000*
Low-High-End	1 in 19.5M*	1 in 20,878*	1 in 1.08M*	1 in 16.25M*	1 in 17,568*	1 in 975,000*
Mid-High-End	1 in 24.42M*	1 in 26,146*	1 in 1.36M*	1 in 20.35M*	1 in 22,000*	1 in 1.22M*
High-High-End	1 in 27.6M*	1 in 29,550*	1 in 1.53M*	1 in 23M*	1 in 24,865*	1 in 1.38M*
						** Number of routine traffic stops*

As Table 12 shows, the danger ratios are even lower in the routine traffic stop category than the danger ratios based on official statistics in the Florida LEOKA Database. Under the most conservative moderate estimate, the rate for a felonious killing of an officer during a routine traffic stop was 1 in every 6.5 million stops. The rate for an assault that results in serious injury to an officer was only 1 in every 361,111 stops. Finally, the rate for an assault (whether it results in officer injury or not) was only 1 in every 6,959 stops. The least conservative moderate estimate suggests that the rates are much less: 1 in every 27.6 million stops for a killing, 1 in every 1.53 million stops involving an assault that results in serious injury to an officer, and 1 in every 29,550 stops for an assault (whether it resulted in officer injury or not).

* * *

In sum, the dominant danger narrative suggests that routine traffic stops are highly dangerous settings for police because officers are more frequently injured or killed during them compared to other police settings. The statistical findings, however, do not support this narrative. The bulk of violence

that I was unable to obtain, read, or that had insufficient information fell under the routine traffic stop category. That resulted in a maximum estimate of 100 cases.

against the officers in the evaluated cases was relatively minor—both in terms of the extent of the officer injuries and the weapons used against them. The danger ratios were also low, especially for cases that resulted in serious injury to officers. Having presented these findings, the next Part shifts gears to the typology and offers a more contextual interpretation of the circumstances under which violence against the police during routine traffic stops occurs.

IV. TYPOLOGY

This Part draws on qualitative methods to develop a typology of major traffic stop scenarios that escalate into violence against the police. I was specifically interested in exploring the context surrounding the cases in the routine traffic stop and criminal enforcement stop categories, and whether overall trends of violence in the Florida LEOKA Database were more consistent with one or both categories.[188] Notably, over 99% of the 3,715 evaluated narratives involving routine traffic stops or criminal enforcement stops fit within the typology.[189] For simplicity purposes, in the Sections below I label the sum of the routine traffic and criminal enforcement stops as "total evaluated stops."[190]

Before presenting the typology, it is useful to explain its structure. The typology is organized around a hierarchy of mutually exclusive and observable contextual factors that preceded the violence in the evaluated cases and as the stop unfolded along its major phases: at inception, during its course, and after its conclusion.[191] I divided "during its course" into two separate stages related to officers' invocation of authority: First, violence that occurred *before* officers invoked their authority beyond that authority incidental to the traffic stop (i.e., asking for basic information, requesting documentation, or running a records check). Second, violence that occurred *after* officers invoked some additional authority such as orders to exit the vehicle, touching or handcuffing drivers or passengers, reaching inside the vehicle, telling drivers or passengers that they were under arrest, or asking for permission to search or searching the vehicle or its occupants.

At the outset, it is important to note a caveat concerning the typology. The typology is intended to be descriptive, not predictive. It enhances our

188. Therefore, for the purposes of the typology, I omitted cases in the indeterminate stop and "other" stop categories.

189. There were 2,911 cases in the routine traffic stop category and 804 cases in the criminal enforcement stop category. *See supra* Section III.A, Table 5.

190. In less than 1% of the total evaluated stops (34 cases), the violence was unclear, it appeared as though there was no violence and only threats or simple resistance, or the situations did not neatly fit into the major traffic stop scenarios. Of those cases, 20 involved routine traffic stops and 14 involved criminal enforcement stops.

191. Under the hierarchy, once a case satisfied a set of contextual factors, it was accounted for in the typology. In this regard, multiple sets of contextual factors might have applied to a specific case if the typology were organized in a nonhierarchical fashion.

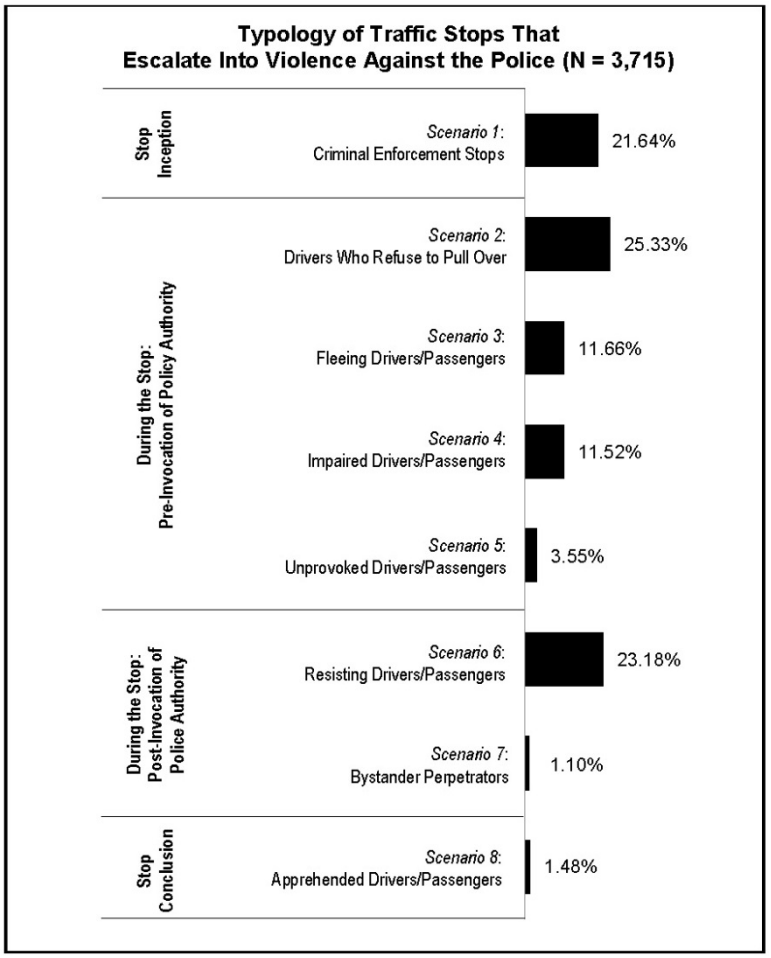
contextual understanding of violence against the police during routine traffic stops through a heuristic that organizes and attempts to make sense of the many different contextual factors surrounding this violence. It does not establish causal relationships between the identified contextual factors and the violence. In addition, similar to many other typologies that have been introduced in the criminological realm, future research is necessary to continue to test the typology and explore its broader applicability.[192]

My analysis revealed eight major traffic stop scenarios, which are organized in the typology in hierarchical order as follows: (1) criminal enforcement stops; (2) drivers who refused to pull over; (3) drivers or passengers who attempted to flee after stopping but before the violence occurred against the officers; (4) drivers or passengers who officers noticed were possibly impaired immediately or soon after initiating the stop; (5) drivers or passengers who engaged in violence before the officers invoked their authority during the stop; (6) drivers or passengers who violently resisted after the officers invoked their authority during the stop; (7) bystander perpetrators (usually bystanders who assaulted the officers after the officers invoked their authority during the stop); and (8) drivers or passengers who assaulted an officer after they were already apprehended (for instance, at the police station after the stop concluded).

Figure 1 below presents the typology and the proportion of the total evaluated stops that fell under each scenario based on this hierarchy.

192. *See, e.g.*, Lallen T. Johnson, *Drug Markets, Travel Distance, and Violence: Testing a Typology*, 62 CRIME & DELINQ. 1465 (2016) (testing Reuter and MacCoun's typology of drug market violence); Nickie D. Phillips, *The Prosecution of Hate Crimes: The Limitations of the Hate Crime Typology*, 24 J. INTERPERSONAL VIOLENCE 883 (2009) (testing Levin and McDevitt's hate crime typology).

FIGURE 1



As Figure 1 shows, qualitative analysis revealed that four sets of observable contextual factors preceded the violence in just under 94% of the total evaluated stops: (1) the encounter resulted from a criminal enforcement stop rather than a routine traffic stop (*scenario 1*); (2) the driver refused to submit to the encounter, either by refusing to pull over or by fleeing, on foot or in the vehicle, after initially pulling over (*scenarios 2 and 3*); (3) the officer reported noticing clear signs of intoxication upon initial contact with the driver or passenger (*scenario 4*); or (4) the officer invoked his or her authority during the stop in some way beyond asking for basic information, requesting documentation, or running a records check—for instance, ordering drivers out of the car or placing his or her hands on the drivers (*scenario 6*). Contrary to the dominant danger narrative, only a very small percentage of cases (just over 3%) involved violence against officers that did not precede one of these four sets of contextual factors and appeared unprovoked. Only a hand-

ful of those cases involved guns or knives. The remaining 3% were situations involving bystander perpetrators of violence or situations in which the violence against officers occurred after the drivers or passengers had been apprehended by officers (for instance, the violence occurred at the police station or DUI testing center).

The Sections below explain the findings within each of the eight major scenarios in greater detail.

A. *Inception of the Stop*

Scenario 1: Criminal Enforcement Stops

The first major scenario in the typology involves stops that are initiated for the purpose of criminal enforcement to some degree. The stops under this scenario accounted for almost 21.64% (804 cases) of the total evaluated stops. During these stops, officers have at least some suspicion at the very inception of the stop that a vehicle, driver, or passenger is involved in crime beyond a traffic violation. As Table 5 (presented *supra* in Section III.A) reflects, the categories of stops that fall under this scenario include felony vehicle stops, suspicious vehicle stops, pretextual stops, stolen vehicles, hit-and-run suspects, suspects with outstanding warrants, and re-engagement with drivers who had previously eluded the police.

At this early point of the typology, all the criminal enforcement stops have been accounted for, leaving only the routine traffic stops to examine. In Section IV.D, I will provide a more refined analysis of how the picture of violence that emerges from the criminal enforcement stops differs from the routine traffic stops.

B. *During the Stop*

The next group of scenarios involves contextual factors that appear during the course of the routine traffic stop. Given that this stage of the routine traffic stop is when officers have the most contact with drivers and passengers, it is not surprising that most evaluated cases fell under this phase of the stop. As noted previously, I separated this phase of the routine traffic stop into two distinct stages: contextual factors that appeared *before* and *after* officers invoked police authority beyond asking for basic information, requesting documentation, and conducting a records check. To be clear, the violence against officers did not necessarily occur during this part of the encounter, although it could have—the key point is that it is at this stage of the routine traffic stop when the first relevant factor is observed.

1. During the Stop: Pre-Invocation of Police Authority

Scenario 2: Drivers Who Refuse to Pull Over

This second scenario involves situations in which officers initiated traffic stops for traffic violations, and the officers noticed clear signs that drivers were refusing to pull over or accelerating to elude them. Another 25.33% of the total evaluated stops (941 cases) fell under this scenario. In many of the cases, a vehicle pursuit ensued, and often, drivers used their vehicles as weapons to hit (or attempt to hit) the officers or their patrol cars. In some cases, the drivers successfully eluded the officers. In situations when the drivers eventually stopped, violence against the police fell into two patterns. First, violence occurred when officers attempted to apprehend the drivers or passengers outside of the car. Second, after fleeing on foot, drivers or passengers assaulted the officers to get away or to physically resist detention or arrest.

Scenario 3: Fleeing Drivers/Passengers

Another 11.66% of the total evaluated stops (433 cases) fell under this third scenario, which involves drivers or passengers who complied with an officer's initial orders to pull over and then fled or attempted to flee before assaulting the officer. To clarify, under this scenario the evidence of flight had to occur after the vehicle initially pulled over but before the officer invoked police authority beyond asking for basic information, requesting documentation, or conducting a records check. These cases followed two patterns. First, in approximately two-thirds of these cases, the drivers pulled over and drove away as the officers were approaching or soon after the officers made contact. Second, in approximately one-third of these cases, the drivers pulled over, and drivers or passengers fled via foot as the officers approached or soon after the officers made contact.

Scenario 4: Impaired Drivers/Passengers

An additional 11.52% of the total evaluated stops (428 cases) fell under this fourth scenario, which involves impaired drivers and passengers. Here, the officers initiated traffic stops for traffic violations (most commonly DUI suspicion or erratic, reckless, or aggressive driving), and upon making contact with the drivers or passengers, the officers observed clear signs of intoxication. Common cues included a smell of alcohol emanating from inside the vehicle or the driver or passenger's breath, slurred speech, glassy eyes, uncoordinated conduct, or drivers who were unconscious.

Interestingly, although this contextual factor came into play prior to an officer's invocation of authority, the evaluated cases in this category were quite diverse in terms of when the violence ultimately occurred during the stop. In one-fourth of the cases, the violence occurred before the drivers or passengers were searched or apprehended; in about half, the violence occurred while the drivers or passengers were being searched or apprehended;

and in one-fourth, the violence occurred after the drivers or passengers were already apprehended and were being placed into patrol vehicles, tested at the DUI centers, or processed at the police station or jail.[193] Moreover, drivers or passengers who appeared intoxicated were often belligerent before assaulting the officers. This belligerent behavior, however, did not always begin upon immediate contact with officers. In many instances, this behavior started after the officers asked the intoxicated drivers or passengers whether they had been drinking, ordered them out of the vehicles to perform field sobriety exercises, or told them that they were under arrest for driving under the influence.

Scenario 5: Unprovoked Drivers/Passengers

This fifth scenario, which accounted for an additional 3.55% (132 cases) of the total evaluated stops, involved what I call "unprovoked drivers/passengers." This scenario includes cases of violence that occurred after the officers made contact with the drivers or passengers and before the officers invoked any additional police authority beyond initiating the stop, asking for documentation, or running a records check. Importantly, this scenario captures the prototypical cases of apparently random and unprovoked violence that animate the dominant danger narrative surrounding routine traffic stops. Therefore, institutional actors that are especially concerned with random or unprovoked violence against the police would likely be most interested in this scenario.

In general, the small proportion of cases that fell under this scenario followed two patterns. The first pattern includes cases that are apparently random violence. In some cases, the vehicle pulled over, and the driver or passenger immediately exited and charged at the officer. In other cases, the officers approached the driver or passenger window, and the driver or passenger opened the car door to hit the officer with it. A handful of cases involved serious violence in which the drivers or passengers pulled over and brandished a gun or knife, or shot at or cut the officers.

In the second pattern, the violence immediately preceded a driver or passenger's attempt to flee the scene. Put another way, the violence was intended as an aggressive attack on the officer as opposed to violence incidental to escape. To avoid confusion, what distinguishes these cases of violence from Scenario 3 ("fleeing drivers/passengers") is that the violence here occurred before, not after, the drivers or passengers fled or attempted to flee. Usually, this violence occurred after the drivers or passengers realized that the officers had discovered that they were driving with no or an invalid license, had outstanding warrants, or saw illegal drugs, drug paraphernalia, or weapons in plain view. In some of the plain view cases, the officers saw drugs, drug paraphernalia, or weapons through the driver or passenger win-

193. Future research is necessary to make better sense of these cases, but one possibility is that the diversity of the violence could be a reflection of people's unpredictable behavior while they are intoxicated. These questions, however, are beyond the scope of this Article.

dow. In other cases, drugs, drug paraphernalia, or weapons fell out of the drivers' or passengers' clothing after they exited the vehicle on their own initiative, without any prior orders from the officers to exit.

2. During the Stop: Post-Invocation of Police Authority

Scenario 6: Resisting Drivers/Passengers

Another 23.18% of the total evaluated stops (861 cases) fell under this sixth scenario, which involved officers who were assaulted after they invoked their authority beyond asking for basic information, requesting documentation, or running a records check. In general, the cases under this scenario followed three patterns.

The first pattern was the most common and involved drivers or passengers who resisted apprehension or arrest.[194] Here, officers pulled over a vehicle for a traffic violation. The encounter either started civilly or the drivers or passengers expressed frustration or disagreement with being pulled over. The situation then quickly escalated once the officer attempted to restrain or apprehend the driver or passenger for safety purposes, to prevent them from ingesting drugs, or to conduct an arrest. Examples of relevant police conduct include pulling drivers or passengers out of vehicles, telling drivers or passengers that they were under arrest, or attempting to apply handcuffs on the drivers or passengers. The most common reasons why police invoked or attempted to invoke these forms of authority were to subdue angry drivers and passengers; arrest them for having an outstanding warrant or not having a valid drivers' license or registration; arrest them for obstruction of justice after not complying with an officer's request during the stop; or arrest them after discovering contraband (most often, drugs) after a pat down, search of their person or vehicles, or discovering contraband in plain view.

The second pattern, which was less common,[195] involved drivers or passengers who resisted during a pat down of their outer clothing, search of their person or vehicles, or during a dog sniff of the vehicles. In many cases, the officers discovered contraband (usually drugs), but that discovery did not precipitate the violence—the request to conduct the pat down or search did.

The third pattern, which was also less common,[196] involved drivers or passengers who resisted exercises of police authority that did not involve a pat down of their clothing, search, or arrest. Examples include officers telling the driver to turn off the car, ordering a driver or passenger out of the car, grabbing the driver or passenger's cell phone, reaching inside the car, or touching the driver or passenger. These cases also involved drivers who refused to sign citations and the situation escalated when the officers told them that they had to sign.

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194. This first pattern accounted for 14.10% of the total evaluated stops (524 cases).

195. This second pattern accounted for 2.64% of the total evaluated stops (98 cases).

196. This third pattern accounted for 5.95% of the total evaluated stops (221 cases).

Scenario 7: Bystander Perpetrators

The seventh scenario accounted for 1.10% of the total evaluated stops (41 cases) and involved third parties who arrived at the scene of the traffic stop and assaulted the officers. Most often, the third parties were relatives of the drivers or passengers, or community members who were attempting to defend the drivers or passengers during the police encounter. More often than not, the officers dismissed or abruptly told the bystanders to leave the scene or risk being arrested themselves, after which the situation escalated.

C. *Conclusion of the Stop*

Scenario 8: Apprehended Drivers/Passengers

This last scenario involves drivers and passengers who assaulted officers after they were already apprehended. This scenario accounted for an additional 1.48% of the total evaluated stops (55 cases). The cases under this scenario fell into three patterns. Under the first pattern, drivers or passengers who were already handcuffed assaulted officers while they were being escorted to, or placed into, patrol cars. Under the second pattern, the drivers or passengers were already secured in the back of patrol cars and either spat at the officers or kicked officers while they were standing beside the patrol car. Under the third pattern, drivers or passengers assaulted officers outside of the setting where the traffic stop occurred (for instance, at the police station, jail, DUI testing center, or hospital).

D. *Comparing Routine Traffic Stops and Criminal Enforcement Stops*

My study is the first to offer a more nuanced view of how violence within different vehicle stop types captured in official LEOKA data may vary. This Section shows that when the typology is applied separately to criminal enforcement and routine traffic stops, a very different picture of violence emerges. This discovery underscores a need to avoid conceptualizing traffic stops as a monolithic category when assessing their dangerousness to police officers—a point that I will discuss later in this Article in greater detail.[197]

Figure 2 below shows how the typology separately maps onto criminal enforcement and routine traffic stops, as well as how that mapping compares to the overall proportions of violence when classified together.[198]

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197. *See infra* Part V.

198. To explain the frequencies that support Figure 2, there were 2,911 routine traffic stops in the "total evaluated stops." In 20 routine traffic stops, the violence was unclear, it appeared as though there was no violence and only threats or simple resistance, or the situations did not neatly fit into the major traffic stop scenarios. As shown in Figure 2, 941 stops fell under Scenario 2, 433 stops fell under Scenario 3, 428 stops fell under Scenario 4, 132 stops fell under Scenario 5, 861 stops fell under Scenario 6, 41 stops fell under Scenario 7, and 55 stops fell under Scenario 8.

FIGURE 2

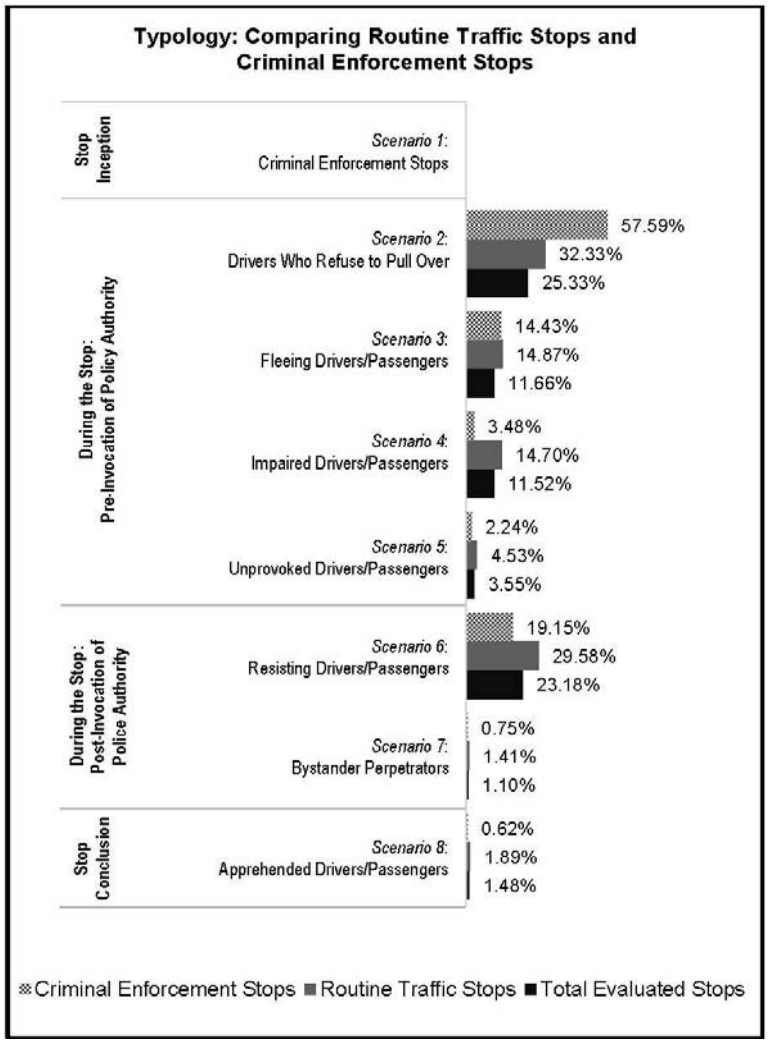

Figure 2[199] reveals two meaningful differences. First, a much higher proportion of criminal enforcement stops fell under Scenario 2, which includes

There were 804 criminal enforcement stops in the "total evaluated stops." In 14 criminal enforcement stops, the violence was unclear, it appeared as though there was no violence and only threats or simple resistance, or the situations did not neatly fit into the major traffic stop scenarios. 463 stops fell under Scenario 2, 116 fell under Scenario 3, 28 fell under Scenario 4, 18 stops fell under Scenario 5, 154 stops fell under Scenario 6, 6 stops fell under Scenario 7, and 5 stops fell under Scenario 8.

199. Because criminal enforcement stops defined Scenario 1 in the typology, I excluded Scenario 1 from Figure 2 in order to compare and contrast the proportions of violence in the criminal enforcement and routine traffic stop categories. In addition, the percentages do not

cases in which drivers refused to pull over (57.59% versus 32.33%). Strikingly, qualitative analysis revealed that in more than 70% of criminal enforcement stops, the driver refused to pull over, or, the driver stopped and immediately or soon after stopping the driver or a passenger fled via vehicle or foot (*scenarios 2 and 3*). Figure 2 also shows that this difference was overshadowed in the typology when criminal enforcement and routine traffic stops were considered together—as they are in official LEOKA statistics.

Second, a lower proportion of the criminal enforcement stops fell under Scenario 6, which includes cases in which the violence occurred after officers exercised authority beyond asking for basic information, requesting documentation, or running a records check (19.15% versus 29.58%). At the same time, the proportion of criminal enforcement stops that fell under this scenario is not trivial. As the next Part discusses in more detail, this finding raises questions about the extent to which greater invocation of police power during routine traffic stops—especially for only traffic violations—creates avoidable and unnecessary conflicts that undermine both officer and civilian safety.

Figure 2 also illustrates an important similarity in how the typology maps onto the criminal enforcement and routine traffic stops. Specifically, the proportion of cases in Scenario 5—which captures random and unprovoked violence against officers—was very small for both criminal enforcement and routine traffic stops. As emphasized above, this scenario captures the stereotypical cases of random and unprovoked violence that animate the dominant danger narrative surrounding routine traffic stops.

* * *

In sum, the presented typology provides a better contextual understanding of violence against the police during routine traffic stops. Contrary to the dominant danger narrative, only a very small proportion of cases in the sample involved apparently random or unprovoked violence that did not precede evidence of flight or intoxication. Further, applications of the typology suggest that monolithic conceptions of routine traffic stops can overshadow important contextual differences surrounding violence against officers during traffic stops, particularly when officers execute stops for only traffic enforcement and not criminal enforcement purposes. As the next Part discusses, these insights illustrate that an accurate evaluation of the risks and costs of policing during routine traffic stops requires abandoning the oversimplified danger narrative in favor of more sophisticated archetypes.

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add up to 100 percent in light of the small subset of narratives that did not fit into the typology because the basis of the violence was unclear, because the incident involved only threats of violence, or because the situation did not fit under any of the major scenarios.

V. IMPLICATIONS

At a conceptual level, the findings and typology illustrate that oversimplified archetypes of police encounters—such as the dominant danger narrative surrounding routine traffic stops—can distort how institutional actors, the public, and researchers evaluate the risks and costs of policing. Put differently, oversimplified archetypes frustrate the ability to achieve what Rachel Harmon has coined "harm-efficient policing"—that is, "policing that imposes harms only when, all things considered, the benefits for law, order, fear reduction, and officer safety outweigh the costs of those harms."[200] In this regard, the findings and typology offer empirical support for the idea that more accurate evaluations of the costs and harms of policing require context-rich information and more sophisticated archetypes.

In the routine traffic stop context more specifically, the findings undermine prevailing assumptions about the dangers of routine traffic stops within key institutions that regulate the police. Official LEOKA statistics reflect a tendency among institutional actors to think of all traffic stops alike, regardless of their basis or context. This monolithic view of the routine traffic stop overshadows the various contextual circumstances surrounding violence against officers during these stops and enables sensationalized danger narratives to shape how these stops are viewed within key institutions that regulate the police.

This Part examines the above points in greater detail. First, it discusses how the findings and typology can inform law enforcement policy and police training on routine traffic stops. Second, it explains how the findings and typology are relevant to Fourth Amendment doctrine involving police authority during routine traffic stops. Finally, it discusses the implications of the findings and typology for future research on policing.

A. *Law Enforcement*

The findings and typology can inform law enforcement policy and police training in several respects.

The presented danger ratios suggest that routine traffic stops are not as dangerous as conventionally assumed in the law enforcement domain.[201] The findings further suggest that a considerable amount of violence against the police during routine traffic stops occurs when the stops escalate after officers invoke their authority in a substantial way during the stop (for instance, ordering drivers or passengers out of cars, touching drivers or passengers, or searching them or their vehicles).[202] In this regard, the exact things that officers may do to protect their safety—which, critically, the dominant danger

200. Harmon, *supra* note 40, at 792.

201. *See supra* Section III.C.

202. *See supra* Section IV.B.

narrative suggests that they should do, and which may be perfectly legal and constitutional—may be the exact things that officers should not do.

Currently, police academies regularly show officer trainees videos of the most extreme cases of violence against officers during routine traffic stops in order to stress that everyday police work can quickly turn into a deadly situation if they become complacent or hesitate to use force.[203] When police training places primacy on the worst-case scenario, it should be expected that officers may have misguided perceptions of danger during the routine traffic stops that they conduct.[204] It should be further expected that officers may respond to perceptions of danger during routine traffic stops in hyper-aggressive ways that instigate escalation, and as a result, potentially undermine both officer and civilian safety. Explicit and implicit racial biases can further shape these perceptions and dangers.[205] Given how rare random and deadly violence with guns or knives against officers during routine traffic stops appears to occur, the study lays an early empirical foundation for critically examining whether, or the extent to which, police training should emphasize the worst-case scenario. On balance, showing videos of the most violent routine traffic stops may create avoidable and unnecessary problems during the stops that trainees will eventually conduct.

The findings also prompt important questions about whether the interplay between monolithic conceptions of routine traffic stops and police practices of pretextual traffic stops may exacerbate these problems. Today, pretextual traffic stops are a pervasive law enforcement practice.[206] As the next Section will discuss in further detail, Fourth Amendment doctrine has created a regime under which the subjective reasons why officers conduct traffic stops are irrelevant for the purposes of evaluating the reasonableness of the stop, so long as officers have probable cause of a traffic violation.[207] Legal scholars have critiqued this regime on the grounds that it enables harmful practices of racial profiling by obscuring when the real reason why officers

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203. Stoughton, *Police Body-Worn Cameras*, *supra* note 10, at 1397–98 (discussing "officer survival" videos "which attempt to remind officers of the dangers of complacency by showing officers being brutally attacked, disarmed, or killed"); *id.* at *1997–98 nn.137–44 (providing examples of "officer survival videos").

204. Here, the concept of the availability heuristic is relevant. *See* sources cited *supra* note 12.

205. L. Song Richardson, *Arrest Efficiency and the Fourth Amendment*, 95 MINN L. REV. 2035, 2045 (2011) (discussing how subconscious racial biases of police officers can result in officers unconsciously associating people of color with danger); Andrew E. Taslitz, *Racial Profiling, Terrorism, and Time*, 109 PENN ST. L. REV. 1181, 1196 (2005) (discussing connections between subconscious racial stereotypes and officer perceptions of danger).

206. Carbado, *From Stopping Black People to Killing Black People*, *supra* note 35, at 155–56 (noting how law enforcement agencies are "very much aware of the on-the-ground implications" of *Whren v. United States*).

207. *See infra* Section V.B; Whren v. United States, 517 U.S. 806, 819 (1996) (holding that the subjective reasons why officers conduct traffic stops are irrelevant so long as the officers have probable cause of a traffic violation).

conduct stops is the race or ethnicity of drivers or passengers.[208] As explained below, the findings open new avenues for critique by illuminating how obscuring whether stops are based on traffic enforcement or criminal enforcement can undermine officer safety. In this regard, pretextual traffic stops are not only bad for over-policed minority communities but are also bad for the police.

When the distinction between whether officers initiate traffic stops for only traffic enforcement versus criminal enforcement is legally and constitutionally irrelevant, the distinction also becomes obscured in law enforcement policy. Consider the policies and standards from one police department in Florida included in the study. The policies and standards provide guidelines for how police officers should conduct themselves during two types of stops: (1) felony vehicle stops[209] and (2) stops for traffic violations.[210] By its very definition, the latter category includes both traffic stops for only traffic enforcement and pretextual stops that also involve criminal enforcement. A similar distinction appears in the policy manual of the Florida Highway Patrol. The policy manual includes guidance on procedures for "traffic stops" and "felony/high risk traffic stops."[211]

This point is engrained at the highest level in the FBI's LEOKA statistics. As discussed previously, the newly improved post-2013 LEOKA statistics separate vehicle stops into two categories: "felony vehicle stops" and "traffic violation stops." [212] Everything in between "felony vehicle stops" and "traffic violation stops"—including pretextual stops—is obscured.

When the distinction between whether officers initiate traffic stops for only traffic enforcement versus criminal enforcement is obscured in law enforcement policy and is legally and constitutionally irrelevant, then the distinction also becomes obscured in everyday police work. Put another way, once a vehicle stop becomes about traffic, officers come to see the stop as a traffic stop. This applies to pretextual traffic stops. Officers come to view pretextual traffic stops as traffic stops when in reality they are criminal enforcement stops.

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208. *See, e.g.*, Carbado, *From Stopping Black People to Killing Black People*, *supra* note 35, at 152–56 (discussing the racialized consequences of pretextual traffic stops); Gabriel J. Chin & Charles J. Vernon, *Reasonable but Unconstitutional: Racial Profiling and the Radical Objectivity of* Whren v. United States, 83 GEO. WASH. L. REV. 882, 886 (2015) ("[S[cholars have been overwhelmingly critical of *Whren*."); Harris, *"Driving While Black"*, *supra* note 35, at 546; Joh, *supra* note 35, at 209.

209. PALM BEACH GARDENS POLICE DEP'T, "FELONY VEHICLE STOPS," POLICY AND PROCEDURE 4.2.1.26, https://egov.pbgfl.com/cp/data/pdpolicies/4.2.1.26.pdf [https://perma.cc/ST9H-SFA8].

210. PALM BEACH GARDENS POLICE DEP'T, "STOPPING AND APPROACHING TRAFFIC VIOLATORS," POLICY AND PROCEDURE 4.2.3.8, https://egov.pbgfl.com/cp/data/pdpolicies/4.2.3.8.pdf [https://perma.cc/CYB9-8HVR].

211. FLA. HIGHWAY PATROL, POLICY MANUAL, "TRAFFIC STOPS" 17.21, https://www.flhsmv.gov/fhp/Manuals/1721.pdf [https://perma.cc/JAU7-WHE3].

212. *See supra* Section I.A.

Importantly, the findings suggest that when pretext obscures the subjective basis of a traffic stop in law enforcement policy and everyday police work, the dangers of the stop itself can become obscured. The findings and typology indicate that traffic stops based only on traffic violations and traffic stops based on criminal enforcement may have different risks to law enforcement, both qualitatively and quantitatively.[213] Therefore, even if legally and constitutionally irrelevant, distinctions between the subjective bases of traffic stops may have practical significance to officer safety on the ground. For instance, criminal enforcement stops in the study were much more likely to involve assaults with motor vehicles against officers rather than assaults with a driver or passenger's hands, fists, or feet.[214] Although more research is necessary, the findings illustrate that conceptualizing traffic stops in monolithic terms (such as under the LEOKA "traffic pursuits and stops" category) hides this important contextual difference.[215]

Therefore, one potential takeaway from the findings is that law enforcement agencies should stop conceptualizing pretextual traffic stops as traffic stops; instead, they should start thinking of those stops in terms of what they are—criminal enforcement stops. With greater context-rich information—like the findings and typology in the study—law enforcement agencies can then direct officers and officer trainees to approach routine traffic stops and traffic stops for criminal enforcement purposes differently. First, consider criminal enforcement stops. The findings and typology suggest that law enforcement should be most concerned about drivers or passengers who attempt to flee or elude the police and that they should take steps to minimize the risk of vehicles being used as weapons against them.[216]

The focus on danger involving flight and vehicles in current law enforcement policy, however, narrowly centers on high-speed vehicle pursuits. Calls for law enforcement agencies to adopt policy restrictions on high-speed vehicle chases go back decades[217] and persist today.[218] Two concerns animat-

213. *See supra* Section III.B, Part IV.

214. *See supra* Section III.B.

215. *See supra* Section III.B.

216. *See supra* Sections III.B, IV.B; *see also* Jeffrey J. Noble & Geoffrey P. Alpert, *State-Created Danger: Should Police Officers Be Accountable for Reckless Tactical Decision Making?*, *in* CRITICAL ISSUES IN POLICING 567, 569 (Roger G. Dunham & Geoffrey P. Alpert eds., 7th ed. 2015) ("Officer-created jeopardy often results when dealing with suspects inside vehicles.").

217. *See, e.g.*, HUGH NUGENT ET AL., U.S. DEP'T OF JUSTICE, NAT'L INST. OF JUSTICE, RESTRICTIVE POLICIES FOR HIGH-SPEED POLICE PURSUITS (1990), https://www.ncjrs.gov/pdffiles1/Digitization/122025NCJRS.pdf [https://perma.cc/2BK4-SNAK].

218. Recent incidents of serious injuries and fatalities during high-speed pursuits have prompted critical discussions about high-speed pursuit policies in several policing jurisdictions. *See, e.g.*, Corey Jones, *With Three Fatal Crashes Since May, OHP Pursuit Policy Remains Confidential*, TULSAWORLD (Dec. 25, 2016), http://www.tulsaworld.com/news/local/with-three-fatal-crashes-since-may-ohp-pursuit-policy-remains/article_4a38ba2b-7740-581a-aec6-f62364bc42a1.html [https://perma.cc/CB9P-R756]; Sharon Ko, *Bexar County Sheriff's Office Changing Pursuit Policy to Protect Citizens*, KENS5 (July 19, 2017, 11:27 PM);

ed these calls. First, Geoffrey Alpert's groundbreaking research in the 1980s called attention to the fact that most high-speed vehicle pursuits result from an observed traffic violation, not a serious crime.[219] Second, evidence suggested that high-speed vehicle pursuits posed great risk of serious injury and death to officers, drivers, passengers, and innocent bystanders.[220] In 1990, the U.S. Department of Justice described these pursuits as "the most dangerous of all ordinary police activities."[221] Since then, many law enforcement agencies have adopted policies that restrict when officers can engage in high-speed vehicle pursuits.[222]

The findings reveal that this focus on high-speed vehicle pursuits in law enforcement policy is far too narrow. Specifically, it does not capture the breadth of criminal enforcement stops in which vehicles are commonly used as weapons against the police. In the study, many criminal enforcement stops that involved vehicles used as weapons against officers did not involve "high-speed" pursuits. Rather, those cases involved drivers who hit or ran patrol cars off the road (or attempted to) while driving under or near the speed limit. In other cases, violence against an officer with a motor vehicle occurred before a vehicle pursuit, or in cases that did not involve a vehicle pursuit at all. For instance, some drivers hit (or attempted to hit) stationary patrol cars while the officers were inside or outside of the patrol cars, or tried to hit the officers as they approached the stopped vehicle. Other motorists used the car doors to hit officers or rolled up the windows while the officers reached inside. Those cases did not necessarily unfold into a high-speed vehicle pursuit.

Next, consider routine traffic stops. The findings and typology suggest that law enforcement should be especially concerned about drivers and passengers who use their hands, fists, or feet to assault officers after they exer-

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http://www.kens5.com/news/local/bexar-county-sheriffs-office-pursuit-policy-changes-da-reviewing-draft/458079966 [https://perma.cc/E72J-HY2K]; Jacob Tierney, *Allegheny County Police Departments Revisit High-Speed Chase Policies*, TRIBLIVE (Jan. 11, 2017, 12:16 AM), http://triblive.com/local/valleynewsdispatch/11757116-74/police-pursuit-policy [https://perma.cc/JWK9-J5S9]. In addition, a recently released study that received nationwide media coverage found that between 1979 and 2013, more than 5,000 passengers and innocent bystanders were killed, and tens of thousands were injured, during high-speed vehicle pursuits. In that same time frame, 139 police officers and 6,300 suspects died during these pursuits. Thomas Frank, *High-Speed Police Chases Have Killed Thousands of Innocent Bystanders*, USA TODAY (July 30, 2015, 12:05 PM), https://www.usatoday.com/story/news/2015/07/30/police-pursuits-fatal-injuries/30187827/ [https://perma.cc/63RY-NZ5N].

219. *See, e.g.*, Geoffrey P. Alpert & Patrick R. Anderson, *The Most Deadly Force: Police Pursuits*, 3 JUST. Q. 1, 10 (1986).

220. *See id.*

221. Patrick Oliver & Samuel Kirchhoff, *Managing High-Speed Pursuits*, POLICE MAG. (June 15, 2017), http://www.policemag.com/channel/vehicles/articles/2017/06/managing-high-speed-pursuits.aspx [https://perma.cc/9T98-TD76].

222. John Rappaport, *How Private Insurers Regulate Public Police*, 130 HARV. L. REV. 1539, 1596 (2017) (noting that "insurers typically require police agencies to maintain adequate policies on vehicle pursuits"); Oliver & Kirchhoff, *supra* note 221.

cise authority beyond asking for basic information, requesting documentation, or running a records check.[223] This insight prompts questions about whether and when officers should exercise more authority than necessary to accomplish those tasks.

Accordingly, law enforcement policy might discourage officers from routinely ordering drivers and passengers out of vehicles during traffic stops based only on traffic violations, in the absence of convincing grounds that the drivers or passengers pose a threat. Or, with greater contextual information that a motorist's hands, fists, or feet are the most common threat during stops for only traffic violations, law enforcement agencies might discourage officers from responding to minor civilian resistance or aggression (for instance, cursing, pulling away, or pushing) with force in order to reduce possibilities for escalation. Some law enforcement policies, however, do the exact opposite.[224]

In certain instances, these greater invocations of police authority embody hyperaggressive officer responses to perceptions of danger. In other instances, however, these exercises of authority may be connected to authoritarian or hypermasculine officer personalities.[225] The findings and typology

223. *See supra* Section III.B.

224. For instance, consider the Tampa Police Department's policy on the use of tasers. On one hand, the policy states that a taser may not be used on a subject fleeing a traffic infraction stop when the only known criminal offense is obstruction based solely on the flight from the stop. On the other hand, the policy provides that officers may use the taser if the traffic violator engages in *any* physical resistance other than flight. Specific examples listed include pushing, pulling away, or striking. TAMPA POLICE DEP'T, STANDARD OPERATING PROCEDURES § 521.2. V.H.4(c) (2016), https://www.tampagov.net/sites/default/files/police/files/tpd-sop-2016-09-01.pdf [https://perma.cc/JB6Z-YUVN] (section on "Electronic Control Device (Tasers)"); *see also* Seth W. Stoughton, *Principled Policing: Warrior Cops and Guardian Officers*, 51 WAKE FOREST L. REV. 611, 652–58 (2016) (discussing how officers are taught to have a "command presence," which could increase the potential for conflict between police and civilians).

225. For a discussion of authoritarian officer personalities and policing and masculinity see generally Frank Rudy Cooper, *"Who's the Man?": Masculinities Studies,* Terry *Stops, and Police Training*, 18 COLUM. J. GENDER & L. 671 (2009) (discussing masculinity, policing, and *Terry* stops); Eric J. Miller, *Detective Fiction: Race, Authority, and the Fourth Amendment*, 44 ARIZ. ST. L.J. 213 (2012) (discussing authoritarian personalities and policing). *See also* Richard Delgado & Jean Stefancic, *Critical Perspectives on Police, Policing, and Mass Incarceration*, 104 GEO. L.J. 1531, 1542–50 (2016) (discussing hyperaggressive policing and authoritarian officer personalities); Angela P. Harris, *Gender, Violence, Race, and Criminal Justice*, 52 STAN. L. REV. 777, 781–83 (2000) (discussing masculinity and policing). Focus on authoritarian personalities in police work goes back several decades. *See* David Alan Sklansky, *Police and Democracy*, 103 MICH. L. REV. 1699, 1733–34 (2005) (discussing police sociology in the 1960s and 1970s); *see also* Jonathan Simon, *Speaking Truth and Power*, 36 LAW & SOC'Y REV. 37, 39 (2002) (same). Relevant here is Seth Stoughton's explanation of the "[w]arrior mindset" in policing, which he describes as in part defined by an officer's resolve and willingness to engage in righteous violence on the job. Stoughton, *supra* note 224, at 638–39. Stoughton describes four elements to this "Warrior mindset": honor, duty, resolve, and a willingness to engage in righteous violence. *Id.* at 668.

also shed light on how police training might address officers who conduct routine traffic stops who fit this personality type.[226]

In the routine traffic stop context, drivers or passengers may not immediately comply with an officer's request or may directly challenge that officer's authority during the stop. Officers who fit this personality type may be more likely to lose their calm and assert their authority in more aggressive ways.[227] The findings suggest that such responses can facilitate escalation in ways that undermine both officer and civilian safety.

On this point, consider the facts surrounding the Sandra Bland case. Bland—a black woman—was pulled over in the middle of the day by a white male Texas state trooper for failing to signal during a lane change.[228] The trooper asked Bland for her driver's license and registration and walked to his patrol car with the documents. Several minutes later, the trooper—intending to give Bland a warning—approached the driver's window. Sensing that Bland was irritated, the trooper asked if she was okay. Bland responded that she was unhappy about being pulled over. After Bland explained why she was upset, the trooper asked, "are you done," and then requested she put out her cigarette. Bland responded, "I'm in my car, why do I have to put out my cigarette?"

The encounter then took a turn for the worst. The trooper, irritated that Bland would not put out her cigarette, ordered her out of the car. Bland refused, expressing that she did not have to step out. The trooper opened the driver's door and tried to pull Bland from the car. Bland refused to get out of the car and did not want to talk to the cop other than to identify herself for the purposes of the traffic ticket. The officer then grabbed Bland, after which she screamed "Don't touch me, I'm not under arrest." The trooper then yelled that she was under arrest. Bland asked, "For what?" The trooper continued to order her out of the car, yelling "I will light you up!" while pointing a Taser. Bland yelled, "You're doing all of this for a failure to signal?" After exiting the car, the trooper put her hands behind her back, handcuffed her, slammed her head on the ground, and told her that she was being arrested for failure to comply.[229] The trooper told Bland that he was initially going to

226. Other legal scholars have recommended that police training address how officers with authoritarian personalities should engage with members of over-policed communities. *See, e.g.*, Mary Fan, *Street Diversion and Decarceration*, 50 AM. CRIM. L. REV. 165, 204 (2013) (stressing that officers should "be trained to guard against aggravating mistrust among minority communities by adopting more authoritarian communication styles with people of color").

227. *See, e.g.*, Josh Bowers, *Annoy No Cop*, 166 U. PA. L. REV. 129, 183–85 (2017) (discussing how disrespect of police led to state intrusions in the Sandra Bland case). *See generally* Phillip Atiba Goff et al., *Illegitimacy is Dangerous: How Authorities Experience and React to Illegitimacy*, 4 PSYCHOLOGY 340 (2013).

228. The facts to follow were captured on a released dashcam video. *Dashcam Footage of Sandra Bland's Arrest During a Traffic Stop Before Her Death in Police Custody – Video*, GUARDIAN (July 21, 2015, 9:40 PM), https://www.theguardian.com/us-news/videos/2015/Jul/22/dash-cam-sandra-bland-arrest-video [https://perma.cc/2WAB-4RJG].

229. *See id.*

give her a warning, but was now throwing her in jail. Three days later, Bland was found hanging from a plastic bag in her cell in an apparent suicide.[230]

These troubling facts call attention to how issues concerning implicit racial bias must inform policy and practice surrounding how officers respond to minor civilian resistance or aggression during routine traffic stops. In her work, Professor L. Song Richardson has thoroughly discussed how police officers' implicit racial stereotypes and racial anxieties[231] can shape interactions with civilians in ways that facilitate escalation.[232] Studies have found that officers who perceive civilians as not respecting them are also more likely to view those civilians as more dangerous.[233] Richardson stresses that these safety concerns are especially relevant to officer interactions with black individuals.[234] Critically, misguided assumptions about the dangers of routine traffic stops only boost the conditions for implicit racial stereotypes and racial anxieties to escalate encounters during routine traffic stops in ways that result in violence against the police (as well as civilians).

Of course, the analysis above does not discount the fact that there are times when it will be appropriate for officers to exercise police authority during routine traffic stops beyond asking for basic information, requesting documentation, and conducting a records check. For instance, perhaps an officer sees evidence of crime in plain view while conducting a traffic stop. When the cases simply involve a traffic violation, however, the key inquiry becomes whether the safety risks surrounding the power to detain, apprehend, and arrest for both officers and civilians is worth the perceived safety or authority benefits. To the extent that these exercises of authority create avoidable and unnecessary conflicts during routine traffic stops, they might not be.[235]

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230. Katie Rogers, *The Death of Sandra Bland: Questions and Answers*, N.Y. TIMES (July 23, 2015), https://www.nytimes.com/interactive/2015/07/23/us/23blandlisty.html (on file with the *Michigan Law Review*).

231. L. Song Richardson, *Implicit Racial Bias and Racial Anxiety: Implications for Stops and Frisks*, 15 OHIO ST. J. CRIM L. 73, 79 (2017) (defining "racial anxiety" as "the worry that they will be perceived as racist by the civilians they encounter" (footnote omitted)). Richardson explains that racial anxiety is associated with a variety of physiological responses, including "sweating, increased heart rate, facial twitches, fidgeting, and avoiding eye contact." *Id.* (footnotes omitted).

232. *Id.*

233. *Id.* at 80–81 (citing Goff et al., *supra* note 227, at 343).

234. *Id.* at 80 (describing that connections between racial anxiety and safety concerns are especially relevant to police interactions with Black individuals because "the stereotype of police racism will be more salient").

235. *Cf.* Rachel A. Harmon, *Why Arrest?*, 115 MICH. L. REV. 307 (2016) (providing a comprehensive critique of the police power to arrest and arguing that this power should be curtailed).

B. *Courts*

In addition to law enforcement, the findings and typology are also relevant to courts. This Section demonstrates how the findings and typology lay an early empirical foundation for rethinking fundamental assumptions in two important areas of Fourth Amendment doctrine on police authority and routine traffic stops: (1) pretextual traffic stops and (2) the routine ordering of drivers and passengers out of vehicles. Although this Section focuses on the U.S. Supreme Court's Fourth Amendment jurisprudence, insights from this study could apply to lower federal or state courts in assessing the reasonableness of police conduct during routine traffic stops under either the federal or state constitutions.

1. Pretextual Stops

Whren v. United States is the U.S. Supreme Court's foundational case on pretextual traffic stops.[236] *Whren* involved a pretextual stop: a stop initiated on the basis of an observed traffic violation but that was really a criminal enforcement stop. The officers were patrolling a "high drug area" of Washington D.C. in an unmarked car.[237] The officers became suspicious when they passed a truck that had temporary license plates waiting at a stop sign.[238] The truck had youthful occupants, and the officers noticed the driver looking down into the lap of the passenger.[239] The truck remained at the stop sign for 20 seconds, which seemed like an unusually long time to the officers.[240] When the police car made a U-turn to head back to the truck, the truck then turned without signaling and sped off at what the officers deemed an "unreasonable" speed.[241] The officers initiated a traffic stop and, upon approaching the driver's window, noticed Whren in the passenger's seat holding two plastic bags of what appeared to be crack cocaine.[242] After arresting the vehicle occupants, the officers found several illegal drugs in the truck.[243]

The defendants challenged the pretextual traffic stop on Fourth Amendment grounds. Specifically, they argued for the Court to adopt a test that evaluated whether a reasonable officer in the same circumstances would have made the stop for the reasons given.[244] The Court specifically rejected the idea that a police officer's subjective motivation for conducting a traffic

236. 517 U.S. 806 (1996). *See generally* Harris, *"Driving While Black," supra* note 35 (exploring the consequences of *Whren* in pretexual traffic stops).

237. *Id.* at 808.

238. *Id.*

239. *Id.*

240. *Id.*

241. *Id.*

242. *Id.* at 808–09.

243. *Id.* at 809.

244. *Id.* at 810.

stop (for instance, criminal enforcement) is relevant under the Fourth Amendment so long as the officer has valid objective grounds for stopping the vehicle (namely, probable cause of a traffic violation).[245] Based on this idea, the Court held that traffic stops are reasonable under the Fourth Amendment when police officers have probable cause that a traffic violation occurred.[246]

The defendants' challenge emphasized the racial consequences of pretextual traffic stops.[247] They argued that total compliance with traffic laws is nearly impossible, and as a result, officers would almost always be able to pull over almost any driver for a technical violation.[248] They claimed that this breadth creates opportunities for law enforcement to use probable cause of a traffic violation as a pretext to stop drivers and passengers—and in particular, minority drivers and passengers—for suspicion of crimes when officers do not have probable cause of those crimes.[249] The Court, however, concluded that these concerns about racial inequality were irrelevant for the purposes of the Fourth Amendment when officers have probable cause of a traffic violation.[250] It viewed those concerns as relevant to equal protection under the Fourteenth Amendment instead.[251]

In deeming the subjective basis of a traffic stop irrelevant under the Fourth Amendment, the Court in *Whren* did not just strip the stop at issue of its racial context.[252] Rather, it also stripped the stop of its criminal enforcement context. The officers were on patrol in a high crime area and were suspicious that the youthful vehicle occupants were involved in crime.[253] Nonetheless, the message that emerges from the Court's reasoning is that a vehicle stop based on a traffic violation is a traffic stop, regardless of whether officers have ulterior criminal enforcement motives.

The Court never mentions officer safety in *Whren*. As discussed in Section V.A, however, the findings and typology illustrate that obscuring the subjective bases of traffic stops can also obfuscate the potentially different danger risks that criminal enforcement and routine traffic stops pose to law enforcement. Nonetheless, *Whren* constitutionalized pretextual traffic stops under the Fourth Amendment and inspired a regime in which pretextual stops have become an institutionalized law enforcement practice to further

245. *Id.* at 813, 819.

246. *Id.* at 819.

247. *Id.* at 810.

248. *Id.*

249. *Id.*

250. *Id.* at 813.

251. *Id.*

252. Maclin, *supra* note 35, at 336–38 (discussing how the Court in *Whren* rendered police motives based on racial stereotypes or bias irrelevant for Fourth Amendment purposes).

253. *Whren*, 517 U.S. at 808.

criminal enforcement goals.[254] As explained below, this regime has significant implications for police safety from at least two perspectives.

First, from the perspective of law enforcement, this regime encourages a race to the bottom in terms of the quantum of evidence of crime that officers need to pull over traffic violators whom they suspect of crime. This low floor then shapes how much evidence officers obtain in everyday police work before initiating the vehicle stops. As scholars have described, officers use probable cause of the traffic violation as a means to sniff for criminal activity before they have reasonable suspicion or probable cause of crime beyond the traffic violation.[255] The defendants in *Whren* stressed this point[256]—although the implications for officer safety were never considered.

This race to the bottom, however, encourages uncertainty in the sense that officers have less and arguably insufficient information to assess whether the situation they are about to enter is really just about traffic or more serious crime. In some situations, this uncertainty might cause officers to overestimate the risks during pretextual stops and respond in hyperreactive and overaggressive ways that increase the likelihood of escalation. In other situations, this uncertainty might cause officers to underestimate danger risks when they have insufficient information to evaluate whether the traffic violators they approach during pretextual stops are engaged in more serious and potentially violent crime.

In these latter situations, an odd paradox arises. Pretextual traffic stops are a key tool of modern proactive policing.[257] From an officer safety perspective, however, pretextual traffic stops actually put officers in a reactive position. In obscuring the dangers of the stop at hand, officers can be caught off guard when a violent threat emerges during the stop, and that surprise can lead them to respond in aggressive ways that facilitate escalation and violence during the encounter. With greater knowledge about whether they are initiating encounters with criminal suspects, officers could proactively take control of the situation by using the minimum amount of force necessary.

Second, from the perspective of drivers and passengers, pretextual traffic stops have become so commonplace that it is difficult for civilians to discern when they are being pulled over for just a traffic violation or something

254. Carbado, *From Stopping Black People to Killing Black People*, *supra* note 35, at 155–56.

255. Tracey Maclin, *Let Sleeping Dogs Lie: Why the Supreme Court Should Leave Fourth Amendment History Unabridged*, 82 B.U. L. REV. 895, 943–944 (2002) ("[O]fficers who initiate these intrusions may well lack the requisite level of suspicion required to stop and search for criminal activity, and these minor traffic offenses may be used as a pretext to facilitate the discovery of illegal narcotics or other criminal evidence." (footnote omitted)).

256. *Whren*, 517 U.S. at 810.

257. Steven Maynard-Moody & Michael Musheno, *Social Equities and Inequities in Practice: Street-Level Workers as Agents and Pragmatists*, 72 PUB. ADMIN. REV. S16, S21 (2012) ("[O]ne of the primary and most institutionalized 'crime-fighting' tools of modern proactive policing is the investigatory stop of drivers and pedestrians.").

else—like their race.[258] Put differently, *Whren* not only obscures the subjective basis of the traffic stop in doctrine, but it also encourages a regime in which the basis of a traffic stop is obfuscated for drivers and passengers—and in particular, overpoliced minority drivers and passengers, who are more commonly subjected to pretextual traffic stops.[259] As a result, drivers and passengers may view traffic stops as instances of profiling (perhaps correctly in many cases), and that anxiety can encourage escalation during the encounters that unfolds into violence against officers. The findings suggest that this violence is not necessarily major or serious and can stem from simple traffic violators using their hands, fists, and feet to assault officers.[260]

To make these points more concrete, consider the following narrative from the study.[261] At 7:30 AM, an officer stopped a black mother with her three children in the car for driving 28 miles per hour in a school zone that had a posted speed limit of 15 miles per hour. According to the officer, the driver refused to sign the citation and exhibited "a belligerent, sarcastic, and insulting manner of speech and demeanor" by demanding that she see the digital read out of the radar unit. The officer told the driver that she would be arrested if she refused to sign the citation. The driver refused the citation and stated "[g]o ahead arrest me. That's what you want to do anyway. I can see how this works. All you are is a redneck hick, who only targets young black women with out of state tags." When the officer then radioed for backup, she mumbled "[o]h just give it to me," grabbed the citation book, and signed the citation while repeatedly stating "I know my rights, I know my rights you redneck hick." After signing the citation, the driver aggressively tore off the citation and threw the citation book at the officer, striking him on the hand and chest. The officer then forcibly removed her from the car and placed her into handcuffs. Her vehicle was towed and an officer transported her three children to the police station, where she was processed in the main county jail for assaulting an officer.

As will become clearer in the next Section, the state interest in officer safety has animated Fourth Amendment jurisprudence in situations other than pretextual stops.[262] The findings reveal that if the Court is truly committed to officer safety in assessing the constitutional reasonableness of police conduct during routine traffic stops, then Fourth Amendment doctrine should not ignore the subjective bases of those stops. Rather, Fourth Amendment rules should encourage a regime in which officers have more, not less, information about the context surrounding the stops that they initiate.

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258. *Cf.* Richardson, *supra* note 231, at 80 (discussing how racial anxiety can contribute to "Black individuals approaching police interactions with heightened suspicion and anxiety").

259. Maclin, *supra* note 35, at 336.

260. *See supra* Sections III.B, IV.B.

261. Narrative No. 1221 (on file with author). All quotes in the following paragraph are contained within this narrative. Narative No. 1221 (on file with the author).

262. *See infra* Section V.B.2.

These critiques are salient in the current moment. *Whren* was unanimously decided in 1996.[263] Last term, however, the Court decided *District of Columbia v. Wesby*.[264] In *Wesby*, the Court held that officers had probable cause to arrest partygoers for unlawful entry and emphasized that the probable cause inquiry was an objective one.[265] Justice Ginsburg opened her concurrence by stating that the case led her to "question whether this Court, in assessing probable cause, should continue to ignore why police in fact acted."[266] Justice Ginsburg further stressed that a number of commentators had criticized the path that the Court had taken in *Whren*, and that ignoring the subjective basis of police conduct may weigh too heavily in favor of law enforcement to the detriment of Fourth Amendment protection.[267] Although it is unclear whether the Court will ultimately reexamine this issue, the findings and typology indicate that the path the Court took in *Whren* may also have dangerous consequences for law enforcement.

2. Orders to Exit Vehicles

As discussed below, officer safety is a major theme in the U.S. Supreme Court's Fourth Amendment jurisprudence on the routine ordering of drivers and passengers out of vehicles. Contrary to the image of routine traffic stops as mundane and unexceptional in *Whren*, these decisions emphasize that routine traffic stops pose grave danger to officers. The findings and typology call into question whether the Court's assumptions about the dangers of routine traffic stops are correct.

In its 1977 decision in *Pennsylvania v. Mimms*, the Court held that officers may routinely order drivers out of vehicles as a safety precaution without violating the Fourth Amendment.[268] *Mimms* involved a routine traffic stop in which officers discovered a gun—which, critically, my findings suggest is rarely the weapon used to assault officers during these stops.[269] In *Mimms*, two officers conducted a traffic stop on a car with an expired license plate.[270] One of the officers approached the car and asked the driver to step out and produce his driver's license and registration.[271] When the driver stepped out, the officer noticed a large bulge under the driver's sports jacket.[272] Fearing

263. Whren v. United States, 517 U.S. 806 (1996).

264. 138 S. Ct. 577 (2018).

265. *Id*. at 589.

266. *Id*. at 593 (Ginsburg, J., concurring in judgment in part).

267. *Id*. at 594.

268. 434 U.S. 106, 107 (1977).

269. *See supra* Section III.B.

270. *Mimms*, 434 U.S. at 107.

271. *Id.*

272. *Id.*

that the driver may possess a weapon, the officer frisked the driver and discovered a gun.[273]

The driver argued that the officer's request to exit the car violated the Fourth Amendment.[274] To reach its holding, the Court balanced the asserted governmental interests against the extent of the individual intrusion. The Court considered the intrusion that stemmed from the officer's request to exit the vehicle as "*de minimis*" and "a mere inconvenience,"[275] especially because the vehicle was already lawfully stopped. On the other side of the balancing scale, the Court considered the government's interest in protecting officer safety.[276]

Importantly, before the officer requested that the driver exit the car, there were no facts to suggest that the driver posed a safety risk or that the driver was involved in crime.[277] The government conceded this point, but argued that the state interest in officer safety weighed in favor of granting officers authority to routinely order drivers out of stopped vehicles.[278] The Court agreed with the government, stressing that it was "too plain for argument" that the government's interest in officer safety was "both legitimate and weighty."[279] Based on this idea, the Court concluded that the officer's command did not violate the Fourth Amendment.[280]

The Court in *Mimms* rooted its intuitions about the dangers of routine traffic stops in empirical data. It first cited the finding of the Bristow study that approximately 30% of shootings of police officers occurred when they "approached a suspect seated in an automobile."[281] The Bristow study, however, never separated those shootings in terms of their underlying basis (as explained in the previous Parts).[282] It is unclear how many of those shootings involved traffic stops, and specifically traffic stops based only on traffic violations.[283] In addition, the Court in *Mimms* relied on LEOKA statistics to conclude that "a significant percentage" of civilian killings of officers occurs during traffic stops.[284] Those statistics, however, include a much broader swath of vehicle stops than routine traffic stops, including felony vehicle stops (as also explained in the previous Parts).[285]

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273. *Id.*

274. *Id.* at 108.

275. *Id.* at 109–11.

276. *Id.* at 110–11.

277. *Id.* at 109.

278. *Id.* at 109–10.

279. *Id.* at 110.

280. *Id.* at 111.

281. *Id.* at 110 (citing Bristow, *supra* note 85, at 93).

282. *See supra* Section I.B.

283. *See supra* Section I.B.

284. *Mimms*, 434 U.S. at 110 (quoting United States v. Robinson, 414 U.S. 218, 234 n.5 (1973)).

285. *See supra* Section I.A.

The Court in *Mimms* acknowledged that not all shootings of officers during routine traffic stops occur when officers issue traffic citations.[286] Nonetheless, it declined to view the circumstances under which officers may be at greater risk of harm during routine traffic stops as relevant to its Fourth Amendment analysis.[287] It further rejected the idea that routine traffic stops involve less danger to officers than other types of police encounters.[288] The danger ratios presented in this Article, however, support this very point.[289] The Court's refusal to consider these important contextual differences in its analysis contributes to the idea that all routine traffic stops are potentially dangerous, no matter what their basis or context. In this regard, the anticontextualist approach of the Court in *Mimms* laid the groundwork for the dominant danger narrative to shape how routine traffic stops are perceived in Fourth Amendment doctrine.[290]

The findings and typology prompt questions about whether this anticontextualist approach fosters Fourth Amendment rules that undermine officer safety on the whole. For instance, the findings suggest that the most common weapons used against officers during routine traffic stops for only traffic violations involve drivers' hands, fists, or feet.[291] In many cases, violence occurred after the officers invoked their authority in a substantial way during the stop—including ordering drivers to exit the car—after which the situation soon escalated.[292] Some narratives clearly reflected that the drivers perceived the officer requests to exit the car as illegitimate, especially since they were being pulled over for only a minor traffic infraction.[293] A constitutional rule that authorizes officers to routinely order drivers out of stopped vehicles only increases opportunities for drivers to use their hands, fists, or feet to assault officers when those stops escalate.

Justice Stevens' dissent in *Mimms* offers a glimpse into how greater contextual information—like the findings and typology presented in this Arti-

286. *Mimms*, 434 U.S. at 110.

287. *See id.* at 110–11.

288. *Id.* at 110 (citing United States v. Robinson, 414 U.S. 218, 234 (1973)).

289. *See supra* Section III.C.

290. Related to this point, some scholars have recently critiqued the ways in which Fourth Amendment doctrine should recognize tactical training. This perspective calls attention to other ways in which Fourth Amendment rules operate in ways that are divorced from context during police work on the ground. *See generally* Garrett & Stoughton, *supra* note 29 (advocating for a "tactical Fourth Amendment").

291. *See supra* Section III.B.

292. *See supra* Section IV.

293. Although future research is necessary to more fully theorize these findings, Monica Bell's work discusses how experiences in which individuals feel unfairly treated by law enforcement (what she labels as "procedural injustice") are an underlying feature of a new concept she advances called "legal estrangement." Bell stresses that "[l]egal estrangement is a theory of detachment and eventual alienation from the law's enforcers, and it reflects the intuition among many people in poor communities of color that the law operates to exclude them from society." Monica C. Bell, Essay, *Police Reform and the Dismantling of Legal Estrangement*, 126 YALE L.J. 2054, 2054 (2017).

cle—might inform Fourth Amendment doctrine. Justice Stevens critiqued the majority's conclusion that routine traffic stops pose serious dangers to law enforcement.[294] He emphasized that the majority made no attempt to distinguish among the different situations in which an officer may approach a person seated in a car, and the different safety risks to officers that those situations might pose.[295] Justice Stevens noted that the Bristow study never distinguished traffic stops for common traffic offenses versus other incidents involving civilians in vehicles who killed police officers.[296]

In this regard, Justice Stevens' dissent challenges a monolithic conception of the routine traffic stop. Unlike the majority, his analysis rejects the idea that all vehicle-stop types pose similar dangers to law enforcement. With better context-rich information, Justice Stevens might have successfully advanced an alternative Fourth Amendment rule that restricted officers from ordering drivers out of vehicles without specific facts to suggest that the officers are in danger.

Twenty years later, in *Maryland v. Wilson*,[297] the Court extended *Mimms* to hold that officers may also routinely order passengers to exit a vehicle during a traffic stop.[298] The Court's analysis includes different assumptions about police danger and routine traffic stops that are worthy of scrutiny. In considering the governmental interests, the Court affirmed its view in *Mimms* that it is "too plain for argument" that the government's interest in officer safety is "both legitimate and weighty."[299] The Court again rooted its intuition about the risks of routine traffic stops to officer safety in empirical data. This time it cited more recent, yet still overinclusive, LEOKA statistics that reported 5,762 assaults against officers and 11 felonious killings during "traffic pursuits and stops" in 1994.[300]

The Court in *Wilson* also explained its view as to why passengers contributed to the danger that officers face during routine traffic stops. It stressed that "the fact that there is more than one occupant of the vehicle increases the possible sources of harm to the officer."[301] It further explained that outside the car, passengers would not have access to any weapons concealed in the interior of the passenger compartment.[302] Finally, it noted that "the possibility of a violent encounter stems . . . from the fact that evidence of

294. Pennsylvania v. Mimms, 434 U.S. 106, 117 (1977). (Stevens, J., dissenting) ("[T]he Court has based its legal ruling on a factual assumption about police safety that is dubious at best.").

295. *Id.* at 117–18

296. *Id.* at 118 n.8.

297. 519 U.S. 408 (1997).

298. *Id.* at 411.

299. *Id.* at 412 (quoting *Mimms*, 434 U.S. at 110).

300. *Id.* at 413.

301. *Id.*

302. *Id.* at 414.

a more serious crime might be uncovered during the stop."[303] The Court viewed the passenger's motivation to use violence as a means to prevent officers from discovering evidence of crime as just as strong as that of the driver.[304]

This discussion in *Wilson* is significant because it reveals that criminal opportunity theory is the theoretical frame through which the Court is evaluating the dangers of routine traffic stops to officers. Criminal opportunity theory assumes that offenders are rational actors and asserts that there are three essential elements to crime: (1) a motivated offender, (2) a suitable target, and (3) the absence of a capable guardian.[305] Under this theory, one could conceptualize the routine traffic stop as a situation in which motivated offenders (the drivers and passengers who are stopped and involved in crime) come into regular contact with suitable targets (officers who initiate and conduct the traffic stops) in relatively unguarded locations (streets, roads, and highways).

The findings suggest that this theoretical frame is far too narrow and too simplistic to account for violence against the police during routine traffic stops. In particular, this frame ignores the racialized consequences of how traffic enforcement has become intertwined with criminal enforcement.[306] The institutionalization of pretextual traffic stops and concentrated police surveillance in certain communities can lead not only drivers, but also passengers, who are innocent of non-traffic-based crime to resist officers with minor violence when officers invoke greater authority than necessary during the stops. That greater authority includes the routine ordering of drivers and passengers out of vehicles. For these reasons, the findings and typology prompt questions about whether the rule announced in *Wilson* is both empirically and theoretically unsound.

C. *Toward a New Research Agenda*

The presented findings and provisional typology are just a beginning. They open avenues and lay a foundation for a new research agenda on how to conceptualize and evaluate dangerousness in everyday police work, including routine traffic stops. This new research agenda is consistent with broader calls in legal scholarship for better contextual data on policing to inform laws, policy, and doctrine.[307] My study focused on the routine traffic stop because it is the most common interaction that civilians have with the

303. *Id.*

304. *Id.*

305. Ronald V. Clarke & Marcus Felson, *Introduction: Criminology, Routine Activity, and Rational Choice*, *in* 5 ROUTINE ACTIVITY, AND RATIONAL CHOICE (Ronald V. Clarke & Marcus Felson eds., 1993).

306. Woods, *supra* note 23, at 709.

307. Harmon, *supra* note 40, at 773–74.

police.[308] Future research, however, can shed insight into whether oversimplified archetypes and misguided perceptions of "dangerousness" are also distorting evaluations of policing risks and costs in other policing situations.

In addition, there are still many significant questions about violence against the police during routine traffic stops and other policing contexts that remain unexplored and undertheorized.[309] Scholarship has largely centered on *civilians* as the *targets* of police violence.[310] In the traffic stop context, these critiques have described how expansive police powers to conduct various searches and seizures during traffic stops create multiple pathways to officers victimizing minority drivers and passengers.[311] Although these critiques have done important work to expose and explain injustices in the traffic stop setting, these critiques are limited in their potential to challenge the dominant danger narrative because they focus on civilians as the targets of police violence.

In this regard, the study findings and provisional typology broaden avenues in legal scholarship for critiquing police powers, including the vast powers that officers now have in traffic stop settings, by considering *police officers* as the *targets* of civilian violence. To the extent that doctrine, law, and policy rest on non-empirically-based assumptions and myths about officer safety—such as the dominant danger narrative in the routine traffic stop context—it is necessary to pay greater scholarly and empirical attention to connections between assumptions about police dangerousness and the legal and desirable scope of police power. Although more research and theorization are necessary, the findings and provisional typology push the scholarly conversation in this direction.

With regard to the provisional typology, two major areas are especially ripe for exploration. First, future research is necessary to provide a more fine-grained understanding of violence against the police under each of the eight major traffic-stop scenarios. Second, the study did not focus on the question of *why* civilians use violence against the police at these different points of the routine traffic stop. Having an understanding of these motivations can inform whether it is desirable for institutional actors to grant officers vast police powers under one or more of the major scenarios in the interest of officer safety. This is especially the case given that the findings and proposed typology raise questions about whether greater invocations of police power during routine traffic stops foster escalation in ways that may undermine both officer and civilian safety.

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308. *See Traffic Stops*, *supra* note 2.

309. In future work, I intend to examine the questions prompted in this Section through additional findings from the study as well as data from new sources.

310. See sources cited *supra* note 35.

311. *See, e.g.*, Carbado, *From Stopping Black People to Killing Black People*, *supra* note 35, at 149–62.

CONCLUSION

Although more research is necessary, this Article makes an important first step in enhancing our contextual understanding of violence against the police during routine traffic stops. This Article has called attention to how the dominant danger narrative surrounding routine traffic stops is an oversimplified archetype that hinders key institutional actors from accurately evaluating the risks and costs of policing during these stops. The presented findings and typology raise significant questions about the validity of laws, policies, and doctrine that rely on this oversimplified archetype.

At the same time, leading sources of information on violence against the police during routine traffic stops provide information that is largely devoid of context. Only by equipping relevant institutional actors with appropriate context-rich information can they move beyond the dominant danger narrative when defining the legitimate and desirable scope of police power in routine traffic-stop settings. The research presented in this Article moves us further in that direction.

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Re-prioritizing traffic stops to reduce motor vehicle crash outcomes and racial disparities

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Abstract

Background: Law enforcement traffic stops are one of the most common entryways to the US justice system. Conventional frameworks suggest traffic stops promote public safety by reducing dangerous driving practices and non-vehicular crime. Law enforcement agencies have wide latitude in enforcement, including prioritization of stop types: (1) safety (e.g. moving violation) stops, (2) investigatory stops, or (3) economic (regulatory and equipment) stops. In order to prevent traffic crash fatalities and reduce racial disparities, the police department of Fayetteville, North Carolina significantly re-prioritized safety stops.

Methods: Annual traffic stop, motor vehicle crash, and crime data from 2002 to 2016 were combined to examine intervention (2013–2016) effects. Fayetteville was compared against synthetic control agencies built from 8 similar North Carolina agencies by weighted matching on pre-intervention period trends and comparison against post-intervention trends.

Results: On average over the intervention period as compared to synthetic controls, Fayetteville increased both the number of safety stops + 121% (95% confidence interval + 17%, + 318%) and the relative proportion of safety stops (+ 47%). Traffic crash and injury outcomes were reduced, including traffic fatalities − 28% (− 64%, + 43%), injurious crashes − 23% (− 49%, + 16%), and total crashes − 13% (− 48%, + 21%). Disparity measures were reduced, including Black percent of traffic stops − 7% (− 9%, − 5%) and Black vs. White traffic stop rate ratio − 21% (− 29%, − 13%). In contrast to the Ferguson Effect hypothesis, the relative de-prioritization of investigatory stops was not associated with an increase in non-traffic crime outcomes, which were reduced or unchanged, including index crimes − 10% (− 25%, + 8%) and violent crimes − 2% (− 33%, + 43%). Confidence intervals were estimated using a different technique and, given small samples, may be asymmetrical.

Conclusions: The re-prioritization of traffic stop types by law enforcement agencies may have positive public health consequences both for motor vehicle injury and racial disparity outcomes while having little impact on non-traffic crime.

Keywords: Traffic stop, Motor vehicle crash, Disparity, Crime, Synthetic control, Law enforcement, Policing, Race, Public health critical race praxis

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Background

Law enforcement traffic stops are one of the most common first contacts with the US justice system (Davis 2018). Community-led movements (American Civil Liberties Union of Illinois 2014), national press (LaFraniere and Lehren 2015), peer-reviewed research (Baumgartner et al. 2018a, 2018b) and the Department of Justice (US Department of Justice, Civil Rights Division 2015) have all suggested that traffic stops are most burdensome to low-income and racial-ethnic minority drivers and their communities. In this paper we provide a brief background on law enforcement traffic stops through conventional and critical public health lens and evaluate an intervention designed to reduce racial-ethnic disparities in traffic stops while reducing traffic crash injury outcomes.

Conventional frameworks suggest traffic stops promote public safety by reducing dangerous driving practices and non-vehicular crimes. Assumptions of criminal justice deterrence theory (Becker 1968) underlie these conventional frameworks, treating dangerous driving and non-vehicular crimes as events where each actor rationally weighs the certainty of being caught, the celerity (speed) of that consequence, and the severity of punishment against the immediate positive consequences of their action. This conventional framework implies an objective world of traffic stop rationale where some have chosen to break the law, others have not, and traffic stops of all kinds have a wholly positive effect on public safety. These frameworks either ignore traffic stop disparities entirely or justify them as negative collateral consequences to otherwise legal and rational public safety interventions. In either case, conventional frameworks suggest these disparities merit little attention and action under an objective enforcement of the law.

Law enforcement discretion, priorities, and disparities

In contrast to these conventional frameworks, public health authorities have called for analyses that center disparities and for engagement in anti-racist action (Jones 2001). The American Public Health Association (APHA) recently launched a National Campaign Against Racism (Jones 2018) that suggests going beyond an individual focus (e.g. who is or isn't racist) to ask, "how is racism operating here?" within structures, policies, practices, norms and values (Jones 2018). One mechanism for how racism operates in the application of justice is through individual and agency discretion. In contrast to conventional objective frameworks, law enforcement agencies have wide, subjective discretion in the selective enforcement of traffic stops. Supreme court cases in 1968 and 1996 (Chief Justice Warren 1968; Justice Scalia 1996) enabled US law enforcement, under any reasonable suspicion and the loosest definitions of crime profiles, to escalate any minor traffic violation into a traffic stop (Meares 2014). Nearly all driving trips include actions interpretable as infractions, whether small wavering within lanes or movement over or under posted speed limits (Baumgartner et al. 2018a, 2018b; Meares 2014). Taken together, these rulings legally permit law enforcement nearly complete discretion over traffic stop enforcement, even if the public views those stops as unfair (Meares et al. 2015).

These enforcement and patrol priorities differentially expose populations to different patrol densities and thresholds of interaction based on neighborhood-level factors. Neighborhood-level segregation by race-ethnicity and income, coupled with institutional policies prioritizing certain spaces and incident types, operates alongside any additional disparities caused by interpersonal bias based on perceived race-ethnicity phenotypes. Indeed, previous studies have quantitatively refuted the idea that individual outlier officers (e.g. the "bad apple" hypothesis) sufficiently explain the large racial-ethnic disparities found in traffic stops (Baumgartner et al. 2018a, 2018b). Still, all individual officers exercise subjective discretion in their traffic stop enforcement, and all do so partly informed by their race-ethnicity, gender, and socio-economic position personal biases, both implicit and explicit (Schafer et al. 2006). In addition, individual officers do not operate within a vacuum. Officers operate within, or at least influenced by, the implicit norms and explicit policies of their agencies (Schafer et al. 2006). Those formal and informal policies include neighborhood-specific patrol deployments and the relative emphasis of public safety and control priorities.

The Public Health Critical Race Praxis (PHCRP), based on Critical Race Theory (Richard Delgado 2016) provides a standardized framework to investigate these traffic stop dynamics (Ford and Airhihenbuwa 2018, 2010) and critique conventional frameworks (Muhammad et al. 2018). PHCRP principles explicitly acknowledge the social construction of knowledge, structural determinism, critical analysis, and disciplinary self-critique (Ford and Airhihenbuwa 2010). In keeping with these principles, and in contrast to conventional frameworks, we recognize law enforcement agency priorities and exercise of discretion are constructed over time, malleable in the present and future, influence officers and communities beyond individual interactions, and deserving of critical analysis.

Considering the relative and absolute frequency of traffic stops by the type of stop is one method of understanding an agency's implicit and explicit priorities. For the purpose of this discussion, we divide traffic stops into three categories. (1) "Safety stops" include violations of speed limits, stop lights, driving while impaired, and safe movement. (2) "Investigatory stops" include explicit

investigation, unspecified rationales, and discretionary seatbelt enforcement, since in prior studies seatbelt stops are most similar to investigatory stops in their disparate application (Baumgartner et al. 2018a, 2018b) and may have mixed evidence as primary stop rationale (Harper 2019). Lastly, (3) "economic stops" are disproportionately consequences of economic circumstances, including not carrying insurance, expired motor vehicle registrations, or equipment malfunctions. Under conventional frameworks these three stop types may be associated with public safety injury and crime outcomes. For instance, safety stops ostensibly reduce motor vehicle and pedestrian crashes. Similarly, investigatory stops may be designed to reduce non-traffic crime, discover and detain individuals after having committed certain crimes, or reduce traffic injury severity by increasing seatbelt use. Finally, economic stops could be framed conventionally as reducing traffic crashes because of equipment failures. Because of their link to public safety outcomes, the relative and absolute frequency of these traffic stop types represent a set of often implicit public health prioritizations.

However, disparities in traffic stops may also differ by these stop types: For instance, Black and Hispanic drivers constitute a larger proportion of investigatory and economic stops than safety related stops in the North Carolina, and are disproportionately over-represented in all stop types relative to the North Carolina population (Baumgartner et al. 2018a, 2018b). In contrast with conventional frameworks that conceive economic stops as protective and unbiased, critical intersectional frameworks acknowledge the link between race-ethnicity and income disparities. Since Black and Hispanic individuals are often disproportionally represented in low-income and low-wealth populations, they may also be disproportionally at risk of economic stops. Due to segregation, they may be more likely to live in lower-resourced areas where investigatory stops are more prevalent, creating multi-level disparity dynamics. These higher-disparity stops are not infrequent: statewide, previous analysis of the North Carolina traffic stop dataset statewide (Baumgartner et al. 2018a, 2018b) demonstrates that economic and investigatory stops make up nearly half of all traffic stops. These disparities by traffic stop type suggest that an agency's relative traffic stop type priorities, whether implicit or explicit, represent not only prioritizations of public safety outcomes but also potentially disparate population targeting.

When agency and officer enforcement priorities differ from community priorities, this violates principles of community self-determination and consequently threatens community trust and perceived legitimacy of law enforcement (Fontaine et al. 2017; Hamm et al. 2017). Trust may also be challenged within agencies, such as when new agency priorities differ from individual officer priorities (Kramer 1999). Law enforcement agencies or individual officers may respond to community mistrust and calls for increased community accountability by scaling back their public safety services (such as certain traffic stops) believed to be essential for violent crime control. This dynamic, named the Ferguson Effect (Gross and Mann 2017), is therefore observable (and testable) in two parts: after increased public scrutiny or reprioritization of public safety activities, there will be a (1) drop in law enforcement activities and (2) an increase in the negative outcomes (e.g. violent crime) those activities were meant to protect against. Studies have shown evidence of Ferguson Effects in the attitudes and actions of officers (drops in productivity, reduced motivation, belief crime will rise as officers "de-police"), though this effect was moderated by their belief in whether communities afford legitimacy to policing (Nix and Wolfe 2018). In contrast, the evidence for increases in negative crime outcomes after de-policing is mixed, confounded by income inequality and racial segregation (Gross and Mann 2017). A recent Missouri study found no effect on crime outcomes at all when traffic stops, searches, and arrests are reduced specifically (Shjarback et al. 2017). Because the intervention considered just such a reprioritization within an agency after community members challenged police legitimacy, we acknowledge this Ferguson Effect as a relevant dynamic for consideration and evaluate it as a secondary aim.

Fayetteville intervention

Given finite budget and staffing realities, law enforcement administrators may choose to direct agency traffic stop programs to target certain public safety outcomes by prioritizing traffic stops by type or directing patrol patterns to maximize traffic stop efficiency. In keeping with this opportunity, city leaders in Fayetteville, North Carolina were called to respond to the city's consistently high motor vehicle crash rate (Barksdale 2013). Simultaneously, tensions between community groups and police came to a head as city council intervened to halt searches that disproportionately targeted Black residents. Soon after, the police chief and second-in-command stepped down (Top two Fayetteville police officials leave amid controversy n.d.).

After newly being appointed in 2013 and faced with issues of motor vehicle crashes and eroded community trust, Chief Harold Medlock voluntarily requested a review of his department practices and policies by the US Department of Justice Office of Community Oriented Policing Services' (COPS Office) (COPS Office: Ethics and Integrity Training 2019) Collaborative Reform Initiative for Technical Assistance (CRI-TA) (Rodriguez

et al. 2015). That report provided preliminary evidence of racial disparities in traffic stops compared to Fayetteville's residential data, though also documented the beginnings of a reduction starting with his tenure in 2013. The report also documented that Fayetteville began to require officers collect Global Positioning System (GPS) data on all traffic stops, an element still not required on the state form; this is corroborated in Fayetteville's written policies for traffic stops, where failure to record this data are grounds for negative performance review (Fayetteville Police Department Administrative Bureau 2015). Those data could then be used alongside its Crash Analysis Reduction Strategy (CARS) program, where ten intersections with the most crashes were used for targeted traffic stop enforcement each week (Fayetteville Police Department 2019). To increase transparency and accountability, press releases were disseminated each week detailing these locations, with three intersections targeted each day. The press releases also detailed the written warnings and state citations issued the prior week.

Because of Chief Medlock's focus on traffic crash reductions and improving community trust exacerbated by racial disparities in traffic stops and other outcomes, he gave guidance to highly prioritize safety stops in order to prevent traffic crash fatalities and reduce racial disparities during his tenure from 2013 to 2016 (Barksdale 2016). We hereafter refer to this collection of changes to agency traffic stop activities, associated policies, workflows, staffing changes, and required organizational change work as the Fayetteville intervention. Notably this intervention included mechanisms we are not measuring in this analysis, including both quantifiable changes (e.g. increased spatial clustering of safety traffic stops around high crash locations) and changes more difficult to quantify (e.g. changing internal organization culture and norms). Therefore, though we track four quantitative measures describing their traffic stop prioritization profile to gauge intervention implementation over the study period, they are best seen as representative indicators of the intervention, not the full substance or mechanism of the intervention.

The purpose of this paper was to evaluate this Fayetteville intervention alongside a broader examination of the relationship of law enforcement traffic stops and public health outcomes.

Methods

The intervention impact was assessed by comparing traffic stop, motor vehicle crash, and crime measures from Fayetteville Police Department to a composite control agency built by a weighted combination of data from eight similarly large North Carolina police departments that did not enact Fayetteville's reprioritization intervention.

Four domain areas were chosen to assess the intervention's impact. Traffic stop prioritization profile measures were chosen to provide evidence the intervention was not only designed and publicized but implemented. Traffic stop disparity measures were chosen to assess questions of improved equity. Motor vehicle crash measures were chosen to assess crashes averted and lives saved. Crime measures were chosen in order to explore the possibility of a Ferguson Effect, the possibility that a de-prioritization of investigatory and economic stops was associated with an increase in crime.

Thirteen measures were chosen from those four domain areas to assess these questions in more detail. Traffic stop prioritization profile measures included (1) number of safety-related traffic stops, (2) percent of safety-related stops, (3) percent of regulatory and equipment stops, and (4) percent of investigatory stops. Measures of traffic stop disparities included (5) percent Black non-Hispanic stops and (6) the traffic stop rate ratio (TSRR) of Black non-Hispanic to White non-Hispanic stops. Motor vehicle crash measures included (7) total crashes, (8) crashes with injuries, and (9) crash-related fatalities. Lastly, crime-related measures included violent crime (10) counts and (11) rates and index crime (12) counts and (13) rates. Notably, Black non-Hispanic traffic stop disparities against White non-Hispanic referent, though only one of a number of useful disparities to consider by race, ethnicity, gender, and age (Baumgartner et al. 2018a, 2018b), were chosen because of previously documented disparities, the specific history of anti-Black racism in the United States, and the explicit focus in Fayetteville around those disparities.

When considering causal questions involving race-ethnicity, individual race-ethnicity comes to simultaneously represent a range of interrelated, but separate constructs (e.g. phenotype, self-identified race, socially assigned race, experiences of discrimination, structural racism, historical trauma, etc.) that have unique causal relationships (VanderWeele and Robinson 2014). We acknowledge this, but do not in this study divide the construct into its many components or bring in accessory datasets to improve its contextualization and construct precision.

Data sources

Traffic stop data were obtained from the North Carolina State Bureau of Investigation (SBI) database, including over 20 million police traffic stops from 2002 to 2018, representing 308 of the 518 state, county, municipal, campus, and place-specific (e.g. state fairgrounds, capital building) police departments (NC State Bureau of Investigation 2019). By 2002, reporting was mandated by

most North Carolina agencies, including all sheriff departments, state agencies, and municipal agencies with jurisdictions above 10,000 population, making it one of the oldest and most complete traffic stop databases in the nation (Baumgartner et al. 2018a, 2018b). Though it does not include all agencies, it represents the policing jurisdictions of 99% of the state population, excluding only the smallest cities and place-specific agencies. All traffic stop measures were derived solely or in part from this dataset.

One evaluation measure, the rate ratio of Black non-Hispanic vs. White non-Hispanic driver traffic stops, required accessory datasets to calculate. Per previous literature (Fliss 2019; US Department of Justice, Civil Rights Division 2015; Withrow and Williams 2015), commonly used, residential-based rates for traffic stops are fundamentally flawed since traffic stops are inherently tied to travel patterns. A supplemental dataset, the 2017 National Household Travel Survey, was used previously to produce NC-specific estimates of vehicle access and vehicle miles traveled by race-ethnicity group (Fliss 2019). Since NC elected to additionally fund the survey as an add-on partner for supplemental sampling (Dai and Roth 2017), survey results could be made representative of the state by multiplying by the pre-calculated weight factors specific to households, people, or trips to account for nuanced sampling strategies and non-response adjustments. Statewide estimates of vehicle access and total annual VMT (see Additional file 2: Table S2) were used as an adjustment factor to city- and year-specific residential data to derive city-year-specific estimates of drivers and total VMT by race-ethnicity (Fliss 2019). Specifically, 64.2% of Black non-Hispanic residents of Fayetteville were estimated to have access to a vehicle, contributing approximately 9775 VMT per year per driver on average. These driving adjustment factors were 82.2% and 10,819 VMT for White non-Hispanics, respectively.

Population demographic data for race-ethnicity-specific rate calculations were obtained from the United States American Communities Survey (ACS) and United States census, interpolating years 2002 to 2009 using 2000 and 2010 census data when ACS estimates were unavailable. Data on North Carolina motor vehicle crashes since 2002 were obtained from the University of North Carolina Highway Safety Research Center (HSRC) (UNC Highway Safety Research Center n.d.), and data on North Carolina crime counts and rates since 2002 were also obtained from the North Carolina SBI (NC State Bureau of Investigation 2019).

Synthetic control

Authors have recently advocated for synthetic control's utility to epidemiology (Rehkopf and Basu 2018) and it has been used specifically in assessing policy effects in both justice (Gius 2019; Muhammad et al. 2018) and public health (Abadie et al. 2010) contexts. In contrast to difference-in-difference (DiD) modeling, which can be conceived of a special case of synthetic control (Xu 2017), the synthetic control techniques compare measures from one or more intervention units over time (in this case, Fayetteville Police Department is the single unit) against measures derived from the weighted combination of 1 or more units from a pool of control units (Abadie et al. 2010). Synthetic control therefore has benefits over DiD in maximizing similarity to controls, loosening the parallel trends assumption, and a statistical basis for control selection (Robbins and Davenport 2018).

In this study, Fayetteville Police Department was the single intervention unit and eight similarly large cities in North Carolina served as the pool of potential controls (see Table 1). In this case and with small intervention ($N = 1$) and potential control pool numbers, the synthetic control technique finds 1 or more control agencies that, in linear weighted combination, generate a synthetic agency for each outcome measure with a pre-intervention trend that maximizes similarity against the intervention agency (or units, in larger studies) on for each measure. These same linear combinations of agency weights, determined by maximizing the pre-intervention period (2002–2012) matching, are then applied to the same agencies in the post-intervention period (2013–2016). The intervention agency can then be compared to the synthetic control agencies for each measure to generate an estimator of the difference between the Fayetteville with the intervention applied and a counterfactual Fayetteville as if it did not receive the intervention. Synthetic control methods, as a method of weighted matching, have the benefit of controlling for some unmeasured confounders (Abadie et al. 2010; Gius 2019) and can optionally be matched on one or more known time-varying or time-unvarying confounders besides pre-intervention outcome measures, though this was not done here for reasons described in the Discussion section. See Table 1 for the list of cities and summary measures from the pre-intervention period.

In this case, the synthetic control method was chosen to control for known global time trends (e.g. statewide changes in driving frequency) that a single-unit difference-in-difference analysis would have left uncontrolled for. As example, driving frequency may have changed statewide, or at least in multiple cities in this analysis, over the intervention period as a function of changes in employment due to the recession and its recovery. Comparing Fayetteville's pre-intervention trend to only its own post-intervention trend would erroneously conflate any reduction in crashes of Fayetteville's

Table 1 Fayetteville and control agency demographics, traffic stops, crashes, and crime

	Demographic Measures			Traffic Stop Measures				Crash Measures			Crime Measures			
	Population	% Black	Median household income	Average annual safety stops	Safety stops (%)	Black driver stops (%)	Traffic stop rate ratio[a]	All crashes	Crashes with injuries	Fatalities from crashes	Index crimes	Index crime rate	Violent crime count	Violent crime rate
Intervention City														
Fayetteville	203,670	41%	$43,882	13,968	43.8	56.8	2.5	5298	1886	62	13,367	7848.1	1224	730.5
Control Cities														
Cary	155,822	8%	$94,617	9179	56.5	18.3	3.8	2355	615	9	2145	1663.8	115	88.9
Charlotte	808,834	35%	$55,599	47,177	43.4	50.4	2.7	22,943	8241	168	45,840	6219.8	6243	845.2
Durham	251,761	39%	$52,115	9329	48.7	57.0	2.8	7284	1979	38	13,233	6121.4	1758	806.2
Greensboro	282,177	41%	$42,802	21,043	55.6	50.9	2.1	7374	2930	53	14,873	5976.1	1767	708.4
High Point	108,982	33%	$43,322	9919	47.9	40.8	1.9	2327	908	23	5719	5805.5	653	659.8
Raleigh	441,326	28%	$58,641	26,374	44.6	45.0	2.9	13,675	3608	80	14,687	4063.9	1914	530.8
Wilmington	113,724	18%	$43,855	6674	52.6	25.7	1.9	3454	1298	32	6679	6707.7	774	773.5
Winston-Salem	238,474	34%	$40,898	13,616	46.1	45.0	2.1	5811	1798	42	15,026	7004.1	1690	786.6

[a]Traffic stop rate ratio is White non-Hispanic to Black non-Hispanic drivers adjusted to travel denominators instead of residential denominators. Average annual data from pre-intervention period (2002–2012). Abbreviations: *MHHI* Median household income

intervention to the reduction in crashes due to global changes in statewide driving. Synthetic control provides some control of this kind of confounding. Because the specific causal relationships of the intervention and its covariates are largely unmapped and because of the relatively small number of observations (acknowledging an intervention $n = 1$), no attempt was made to control for other specific time-varying or time-unvarying confounders between agencies beyond the confounding control that weighted matching on pre-intervention period provides for these global and potentially time-varying confounders. Independent synthetic control agencies were created for each measure for this same reason; simultaneous matching against all measures implies shared confounders between them, which was not known (and was not expected by authors) to be the case.

The post-intervention synthetic control annual average, annual difference between intervention and control, percent change with confidence interval, placebo test permutation p-value (calculated by assigning intervention status to each control agency and recalculating the post-intervention model), and linear trend p-value were calculated for each reprioritization, crash, disparity, and crime measure. 95% confidence intervals were estimated using Taylor series linearization as having relatively few units limit resampling- and placebo permutation-based methods. Given the number of units, these point estimates may not exactly match those derived from the synthetic control weighting-based method and therefore confidence intervals may be unsymmetrical. The statistical package R (R Core Team 2018) and key libraries (Pebesma 2018; Robbins and Davenport 2018; Wickham 2017) were used for analysis.

Results

Synthetic control generated measure-specific weight vectors using between 1 and 5 control agencies (see Additional file 1: Table S1), with the model average of 3.0 agencies. Table 2 presents annual averages, differences, and percent change comparing post-intervention Fayetteville to the post-intervention control agency for thirteen intervention-related measures. At the end of the intervention period over 80% of Fayetteville's traffic stops were safety stops, up from a low of 30% in 2010. The Fayetteville intervention was associated with a 47% average increase in the proportion of safety stops and a striking 121.3% (17.3%, 318.1%) average increase in the number of safety stops. From a low of just over 9000 safety stops in 2006, at the end of the intervention period Fayetteville completed nearly 60,000 safety stops in 2016.

Both measures of Black non-Hispanic traffic stop disparities were reduced in Fayetteville as compared to the synthetic control agencies: the percent of traffic stops reduced 7.0% and the driving-adjusted traffic stop rate ratio was reduced 21%. Linearization estimates were similar and associated confidence intervals were relatively small.

All three measures of negative traffic outcomes were also reduced relative to synthetic controls: total crashes were reduced 13% (765 fewer each year), injurious crashes were reduced 23% (479 fewer each year), and

Table 2 Treatment vs. synthetic control: stop profile, crash outcome, and crime outcomes

	Fayetteville Police Department		Synthetic Control	Difference between Fayetteville and Synthetic Control			
	Pre-intervention annual average	Post-intervention annual average	Post-intervention annual average	Annual Difference	Percent Change and 95% CI (%)	Linear test *p*-value	Permutation test *p*-value
Traffic Stop Profile							
Total Safety Stops	13,968 (100%)	34,930 (100%)	15,786 (100%)	+ 19,144	+ 121.3 (+ 17.1, + 318.1)	0.0055	< 0.0001
% Safety Stops	6119 (43.8%)	23,786 (68.1%)	7296 (46.2%)	+ 21.9%	+ 47.3 (+ 20.0, + 80.9)	0.0001	< 0.0001
% Regulatory & Equip. Stops	6073 (43.5%)	9583 (27.4%)	6951 (44%)	−16.6%	−37.7 (−54.6, − 14.5)	0.0012	< 0.0001
% Discretionary	1776 (12.7%)	1562 (4.5%)	1367 (8.7%)	−4.2%	−48.4 (−55.5, − 40.1)	< 0.0001	< 0.0001
Measures of Traffic Stop Disparity							
% Black non-Hispanic Stops	56.8%	54.7%	58.8%	−4.1%	−7.0 (−8.9, −5.0)	< 0.0001	0.250
Black non-Hispanic TSRR	2.5	2.2	2.8	n/a	−21.3 (−28.5, −13.3)	< 0.0001	0.125
Motor Vehicle Crash Outcomes							
Crashes (all)	5298 (100%)	5160 (100%)	5925 (100%)	−765.0	−12.9 (−37.5, + 21.3)	0.4439	0.125
Crashes (w/ injuries)	1886 (35.6%)	1639 (31.8%)	2118 (41%)	−479.3	−22.6 (−48.5, + 16.3)	0.2763	0.125
Traffic Fatalities	62.3	48.8	68.0	−19.3	−28.3 (−64.1, + 43.2)	0.4146	0.125
Crime Outcomes							
Violent Crimes	1223.6	1233.5	1257.3	−23.8	−1.9 (−32.8, + 43.2)	0.9218	> 0.99
Violent Crime Rate (per 1000)	730.5	596.9	582.4	+ 14.5	+ 2.5 (−14.0, + 22.2)	0.7815	0.750
Index Crimes	13,367.4	11,658.0	12,896.4	− 1238.4	−9.6 (−24.5, + 8.2)	0.2923	0.500
Index Crime Rate (per 1000)	7848.1	5637.3	5933.4	−296.1	−5.0 (−12.8, + 3.5)	0.2482	0.750

Table includes both annual averages pre-intervention (2002–2012) and post-intervention (2013–2016). Note: confidence intervals are not symmetrical around point estimates because different methods were used to produce each and small numbers further limited convergence

traffic fatalities were reduced 28% (representing 19 fewer fatalities each year). The percent change in metrics associated with motor vehicle crashes were large but had wider confidence intervals and moderate agreement with Taylor linearization estimates.

Non-traffic crime outcomes showed little change. Index crime counts and rates were reduced 10% and 5% respectively, though confidence intervals were high. The Fayetteville violent crime count and rate were effectively indistinguishable from the control, with small estimates, wide relative confidence intervals, permutation test p-value > 0.99 and linear p-test of 0.96. Because of this, synthetic control estimates poorly matched the Taylor linearization estimates and small counts and rates disagreed in direction of association.

Figure 1 shows the trend of nine of these measures. The respective synthetic control agencies closely matched Fayetteville's pre-intervention trends for most measures. Relatively small numbers of traffic fatalities among many agencies created more variation in the pre-intervention match for that measure. Divergence in the intervention period (in grey) demonstrates the intervention's modeled effect.

Figure 2 shows the estimated effect (Treatment – control) for these same nine measures, as well as permutation tests of non-intervention agencies modeled under the same synthetic control framework with a placebo intervention. These placebo trends are graphical representations of the summary measure placebo tests presented in Table 2. Post-intervention clustering of the placebo trends, clustering of the pre-intervention trend around zero, and a sharp direction change of the intervention unit post-intervention represent stronger model fit.

Discussion

Traffic stop profile measures confirmed the implementation of the intervention strategy. Both the relative percent of safety stops and the absolute number of safety stops completed marked increased in Fayetteville in comparison to the measure-specific synthetic control agencies. This increase in the percent of safety stops was matched with a corresponding relative reduction in economic and investigatory stops.

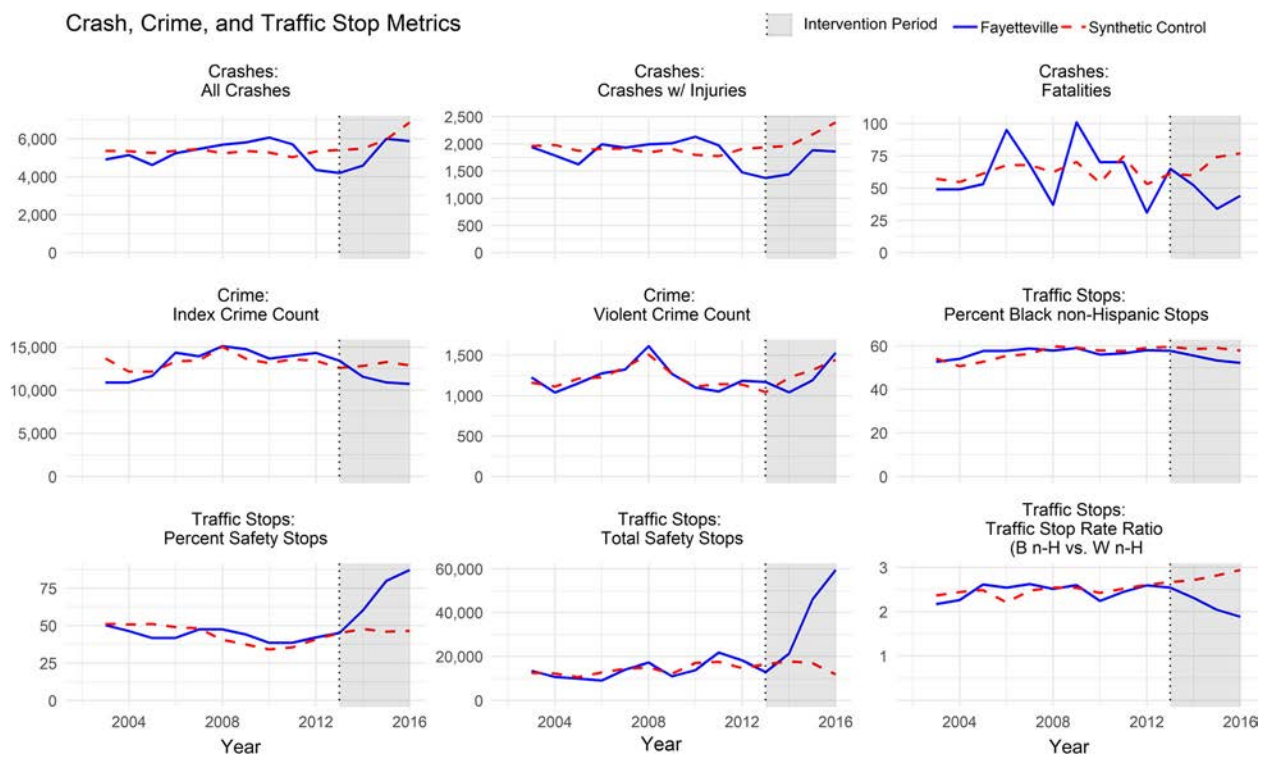

Fig. 1 Crash, Crime, and Traffic Stop Metrics pre- and post-intervention period. Fayetteville Police Department is compared to a synthetic control department built by the 8 most similarly urban, high population, North Carolina police departments best matched for the specific metric during the pre-intervention period, from 2002-2012. Differences between the synthetic control (e.g. counter factual Fayetteville) and Fayetteville during the post-intervention period (i.e. 2013-2016) represent the modeled effect of the intervention

Motor vehicle crash outcomes were all reduced, though confidence intervals were relatively wider. Measures of traffic stop disparities were also reduced, suggesting a focus on safety stops (and relative de-prioritization of investigatory and economic stops) was a viable strategy to reduce Black non-Hispanic disparities in their traffic stop program.

Neither index crimes nor violent crimes changed appreciably during the intervention relative to the synthetic control agencies: three measure point estimates saw small reductions and one saw a small increase, but these nominal changes were much smaller than their associated confidence intervals. The disagreement in direction of the small change violent crime counts (decrease) and rates (increase) demonstrates that the measure was largely unchanged; small variation in population denominators explain the metric direction disagreement and the intervention effect on violent crime was effectively indistinguishable. This study does not provide any evidence of a negative effect on crime for de-prioritizing investigatory and economic stops. However, a more detailed view of the trend of the reduction in the total number of stops during the transition into the intervention suggests the first half of the Ferguson Effect, a reduction in output by some officers in response to community outcry and public attention, may have occurred in the first intervention year. Staffing changes as agency culture changed may also have occurred during the intervention roll-out period, producing or contributing to this reduction in output before the subsequent large increase in safety stops.

These results suggest redesigning a traffic stop program for public health impact may reduce negative motor vehicle crash outcomes, simultaneously reduce some negative consequences of traffic stop programs (e.g. race-ethnic disparities, reduced economic stop burden on communities), and the relative de-prioritization may not have an significant impact on crime rates. Safety traffic stops, especially when directed at high crash areas using regular review and traffic stop GPS data for evaluation, may be a more effective public safety tool than economic or investigatory stops. If investigatory stops can be de-prioritized with little impact on crime, but carry with them negative consequences to community trust, those traffic programs may be de-emphasized even without a relative prioritization of safety stops.

However, these apparent public health wins can be fleeting, as transitions in administrators may bring entirely new or adjusted priorities. Since Chief Medlock's retirement in 2016, the percent of safety-related stops has dropped and the percent of Black drivers stopped has increased (Open Data Policing 2019). Future

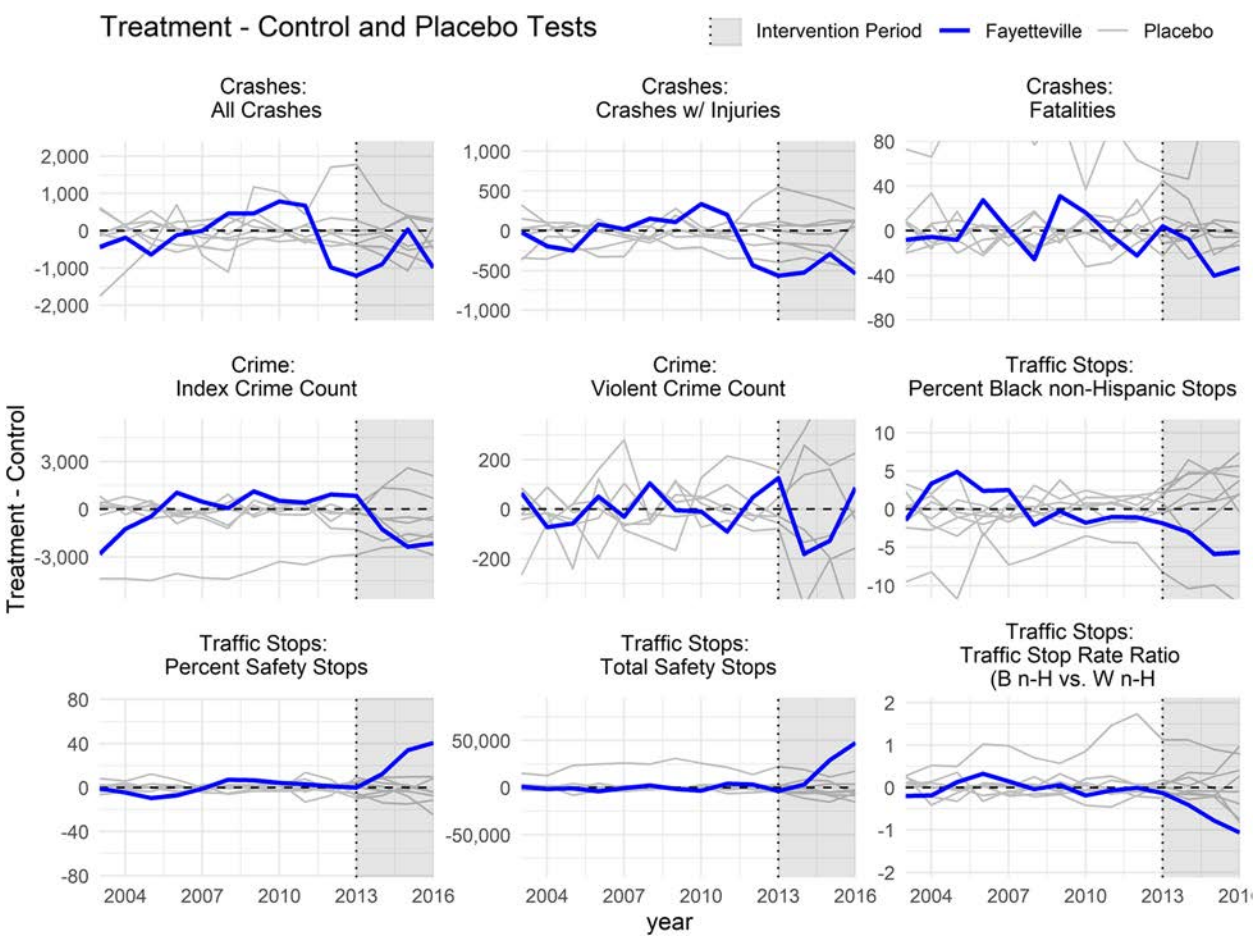

Fig. 2 Treatment – Control Trends and Placebo Tests, pre- and post-intervention period. All permutations of non-intervention law enforcement agencies were treated as if they had enacted the intervention during the period, even though they hadn't (placebo treatment), then likewise matched on pre-intervention period metrics using the same synthetic control process. The estimated change (treatment – control, ideally zero during the pre-intervention period) is graphed for both actually treated and placebo treatments. Some placebo comparisons produce outlier trend lines because the control pool was selected for Fayetteville, and placebos may be inadequately matched

analyses may explore whether these new changes are associated with increases, decreases, or neither in crash, injury, and crime measures. Adherence to consistent public health priorities, especially when those relative priorities and implicit logics are made explicit, may help administrators transition while keeping interventions consistent.

Negative consequences of traffic stops

This study posits a relationship between certain stop types and public health outcomes under a conventional framework. However, that conventional framework ignores or downplays the real, negative consequences of traffic stop enforcement in practice. Regulatory and equipment stops, and their associated fines, are a direct form of criminalizing individual and community economic poverty. Beyond the immediate impacts, the harm of economic stops creates a negative spiral operating within communities collectively and individuals specifically, extracting wealth and people's bodies from low-income communities as the inability to pay mounting traffic tickets escalate into denied registration and warrants for arrest. The United State Justice Department Civil Rights Division cited this extreme and racialized extraction of wealth through traffic stops in its review of the Ferguson Police Department (US Department of Justice, Civil Rights Division 2015). When used unaccountably (e.g. recording no GPS data, as is the norm in NC), moving and safety violation stops can be enforced in an area with few motor vehicle crashes to justify them. Lastly, investigatory stops may have strikingly low contraband hit rates or racialized application (Baumgartner et al. 2018a, 2018b), which subject some to antagonistic law enforcement interactions over years (Peralta and Corley 2016) without contraband to show for the interaction.

Beyond the serious financial and carceral consequences, at their most severe, traffic stops can have fatal consequences for motorists, even when unarmed. Sandra Bland, an unarmed Black woman who died in jail after a routine traffic stop, had multiple other unpaid traffic tickets at the time of her arrest, including for operating a vehicle without a license and lack of insurance (Katy Smyser 2015). Walter Scott, an unarmed Black man, was shot to death, in the back, by a South Carolina police officer after a traffic stop for a non-functioning brake light (Blinder 2017). Philando Castile was pulled over forty times, for reasons including speeding, driving without a muffler and not wearing a seat belt, in the years running up to his fatal shooting during a traffic stop (Peralta and Corley 2016). An uncritical increase in traffic stop enforcement means increased interactions with law enforcement, creating more opportunities for escalated and fatal encounters that may disproportionately impact low-income people and people of color given structural disparities and both implicit and explicit bias. The associated loss of community trust has real public health consequences, including fewer calls for timely emergency services (Desmond et al. 2016). Beyond the negative consequences acknowledged to be more objective, public safety interventions driven by traffic stops should acknowledge the disparate, subjective, emotional experience drivers of color experience. Recent studies now document how these disparities in chronic stress get biologically embedded (i.e. "get under the skin") and have measurable and negative consequences for individual health (Hertzman and Boyce 2010; Krieger et al. 2015; Nuru-Jeter et al. 2009), including specifically symptoms of post-traumatic stress disorder associated with increased interactions with police (Hirschtick et al. 2019).

Program effectiveness, program efficiency

Central to this discussion are questions of absolute and relative intervention efficacy and efficiency. In Fayetteville's case, their safety stop program was likely more efficient because of its use of crash data to inform prioritization of intersections and the geocoded stop data to ensure intervention fidelity. However, safety related traffic stops are not the only method to reduce motor vehicle crash injuries. The efficacy of even maximally efficient traffic stop programs must be weighed against strategies from other sectors such as public education campaigns and built environment investments, which may be either or both more efficacious and cost-efficient (Centers for Disease Control and Prevention, National Center for Injury Prevention and Control 2019). Likewise, focusing on policing interventions for public safety in the absence of infrastructure improvements, given historical (e.g. redlining) and present disparities in those investments raise equity concerns (Rothstein 2017).

When considering equitable investment in communities, this intervention to reprioritize traffic stops may best be a stop gap response to immediately reduce disparities and promote traffic crash outcomes but is not an ultimate solution. Though the intervention reduced racial disparities in Fayetteville compared by 21% of what they could have been, Black drivers still experienced over twice the incidence of traffic stops per vehicle miles traveled as White non-Hispanic drivers at the end of the study period. If not considering alternative interventions that may be more efficient, efficacious, or equitable, an investment in traffic stop programs in isolation may be capable of reducing motor vehicle crashes further but may require a totalitarian police state model stopping nearly all drivers for every possible infraction. Intervention considerations should include not only comparison of the positive efficacy and financial cost of programs but should weigh the negative collateral or intentional damages done. Traffic stop programs may be intentionally phased out or scaled back alongside infrastructure investments and other interventions that carry fewer negative and inequitable consequences to remain in alignment with public safety needs.

The same principles are true when considering other public safety outcomes: though policing has seen large funding increases and expanding scope of practice (Hinton 2016), policing should not be seen as either a panacea overall or the most efficacious intervention for non-vehicular crime and injury specifically. Police do not replace mental health workers, social workers, or public health workers capable of implementing evidence-based programs at the individual and community level for substance misuse and violence-related outcomes. As law enforcement agencies are increasingly accountable to the efficacies and efficiencies of their programs, it is in their best interest to focus on programs, including carefully-designed traffic stop programs, that have fewer negative consequences, more equitable outcomes, improved efficacy, and efficient implementation when compared to interventions from other sectors.

Program priorities and the relative worth of life

In both law enforcement and public health, we implicitly and explicitly prioritize certain causes of disease, injury, and death over causes. Our prioritizations are revealed by our evidence and assumptions of efficacy and efficiency, by program funding and implementation, and ultimately by community investments enabled by political power. Even ignoring other sectors and intervention strategies besides traffic stops, police may compare the cost and efficacy of traffic stop programs in preventing injury and death by motor vehicle crash to preventing

injury or death during a burglary, assault, homicide, or suicide. When considering who is targeted by interventions, public health recommends considering the burden of traffic stop preventable injuries, the exposure to traffic stops in the form of patrols patterns and priorities, and distributions of both exposure and outcome across population subgroups (Ward et al. 2019) alongside efficacy and cost. Because of unequal distribution of outcomes, exposure to interventions, differences in intervention effectiveness and efficiency, these priorities come to represent the relative value of lives by race-ethnicity and socio-economic position. As example, if community investment (including through law enforcement and traffic stop patrol programs) in preventing deaths by assault grossly outweighs investment in prevention of deaths by motor vehicle crashes, overdose, or heart disease, and especially when the underlying burden of assault injuries and mortality is comparably low, we implicitly prioritize the health and lives of populations seeking to prevent assault over other public health priorities and other populations.

These prioritization dynamics operate at multiple levels within and above agencies: within agencies as individual officer, patrol team, and precincts patterns; and above as clusters of agencies, statewide, nationwide, and between countries. At the national level we see these prioritizations in the focus on criminalizing drug use and addiction in urban, Black communities in the 1980s that lead to disproportionate incarceration of Black people at a level rarely seen anywhere else in the world (Hinton 2016). In contrast, the multiple phases of the opioid epidemic since 2000, hitting more (but not exclusively) rural and white communities, has been comparably treated as a public health crisis rather than a criminal justice one (Bailey et al. 2017; Netherland and Hansen 2017). Though this intervention analysis provided some contextual factors at the agency level, future research should not be limited to either implicit bias at the individual or policy effects at the agency level, but instead should continue to focus on questions or program priorities and implicit worth of human life at multiple and interacting levels.

Whether legally defensible or not, traffic stop programs may still be considered unjust and burdensome. They may ignore racial disparities in financial hardships, erode community trust, embody community stress, and trade injury and loss of life outcomes in some communities to promote or appear to promote the well-being of other communities. Even within the same community, for example, a seatbelt program that extracts large amounts of financial resources may cause serious harm to individual and community health and may outweigh the injury prevention benefit. Co-designing traffic stop programs along with impacted communities may alleviate some of these negative outcomes, though likely not all given the multiple underlying dynamics at play (Smith and Holmes 2014). It is precisely these implicit disparities in the value of people's experiences, and ultimately their bodies and lives, that drives associated policy platforms calling for the end of criminalization and dehumanization of Black and low-income communities (The Movement for Black Lives 2019).

Accountability

We argue that public health has a fundamental interest in detailed traffic stop data given associated public safety outcomes and equity considerations under both conventional and anti-racist frameworks (Ford and Airhihenbuwa 2010). However, not all states maintain active traffic stop databases like North Carolina's. Further, most active traffic stop databases that do exist were started recently. When contrasted with many other public health surveillance systems, limited data on traffic stops suggest a relatively limited oversight of law enforcement activities. Public health has already acknowledged that data on deaths caused by officers are public health data that can and should be maintained (Feldman et al. 2019; Krieger et al. 2015), and that collecting law enforcement data in general is fundamental to accountability and trust (McGregor 2015). Data collection on traffic stops should also include some within-agency spatial component, as Fayetteville has elected to collect, such as spatial coordinates or an address or intersection that could be retroactively geocoded. Besides promoting accountability and transparency, such detailed data on traffic stop programs also benefits police agencies. Spatially-referenced traffic stop data can inform prediction and intervention models of public safety events like crashes and violent assaults and also ensure accountability within the agency and to community priorities. GPS tools for spatial referencing are increasingly low-cost, included in most cell phones, and retrospective geocoding are inexpensive. Recognizing the decreasing cost and increasing utility, the National Institute of Justice (NIJ) and the Bureau of Justice Assistance collaborated with the National Highway Traffic Safety Administration (NHTSA) to promote the Data-Driven Approaches to Crime and Traffic Safety (DDACTS) (Crime Mapping for DDACTS - Crime Mapping and Analysis NewsCrime Mapping and Analysis News n.d.) program. Agencies that capture detailed traffic stop data would be following these NIJ best practices.

As an example of the equity implications of public safety interventions, NHTSA put out a manual for state highway safety offices that included evidence of law enforcement traffic stop activities by types of traffic stop (Goodwin et al. 2015). This document informed updates of CDC guidelines around motor vehicle safety

interventions (CDC Injury Center Motor Vehicle Safety 2019). Included as an evidence-based intervention are "a saturation patrol (also called a blanket patrol, 'wolf pack,' or dedicated DWI patrol)" (Goodwin et al. 2015). Likewise, movement from secondary to primary enforcement of seatbelt laws (e.g. allowing seatbelt ticketing when no other infraction is present) is associated with more seatbelt usage and reduced traffic crash fatalities. But when public health advocates for saturation approaches do not acknowledge and measure disparities, these approaches may disproportionately burden under-resourced communities with the negative consequences of traffic stops. And, without some within-jurisdiction accountability, agencies are free to use their discretion to distribute DWI and seatbelt patrols into neighborhoods for other reasons. Those neighborhoods may not have the political and economic capital to fight in court, may not equitably weather the negative effects of such saturation interventions, and may not have the associated needs or see the consequent benefits to their public health outcomes.

Limitations

This study has multiple limitations. Since only one agency enacted the intervention, our findings are suggestive but limited by sample size in many ways. For instance, in Fig. 2, because placebo tests are limited to the control pool of 8 non-intervention agencies, permutation *p*-values could only be in multiples of 0.125. Moreover, the relatively small control pool was only selected to provide adequate comparison to Fayetteville, i.e. by ensuring a spread of most metric around Fayetteville. Therefore, in some cases, some placebo trends and related tests were unstable for some metrics when no linear combination of other control agencies could remotely match the placebo agency. As example, no linear combination (weights adding to 100%) of smaller agencies can effectively model Charlotte, the largest agency with twice the population, twice the traffic stops, and three times the index crime count; if Charlotte were the agency of interest, it would require a different control pool.

Even in the case of Fayetteville, though the control pool provided adequate coverage for most metrics, one metric (the percent of Black non-Hispanic traffic stops) was best represented by a 100% weighted match against a single city agency in Durham, North Carolina. This effectively reduces the more nuanced synthetic control method to a simpler difference-in-difference model comparing a single intervention city to a single control city. In this case, Durham may be well suited as a control city to Fayetteville on most metrics (see Table 2) in this case, including closely matching this metric (e.g. 57% of traffic stop drivers are black in both cities in the pre-intervention period). However, this single control city analysis is not as robust to city-specific variation. If a group of agencies were to adopt this prioritization formally or smaller variations in these metrics were considered in a national study, results may be more robust. If a group of agencies were to adopt this prioritization formally or smaller variations in these metrics were considered in a national study, results may be more robust.

We do hypothesize that the synthetic control method improved confounding control compared to a simpler difference-in-difference model. However, an approach that incorporated data on more agencies and more covariates under a more detailed confounding control scheme would likely produce more accurate results than our approach of matching on the pre-intervention period. In this case, because of both small numbers of units and a lack of clarity on whether potential covariates were mediators or confounders of the intervention effect on each specific measure, we did not additionally adjust for metric-specific known confounders beyond the confounding control that metric-specific matching on the pre-intervention period provides. For example, while local economic changes associated with changes in a given metric (say, crime) across multiple cities would be adjusted for by comparison to the synthetic control built from cities matched on that crime metric, if Fayetteville had city-specific economic changes unrelated to those otherwise matched cities this analysis would not detect it. However, including time-unvarying or time-varying covariates requires the synthetic control to attempt to match both pre-intervention trends and covariates simultaneously; in sparse models with small sample sizes, this effectively deprioritizes unknown confounder control for (supposedly) known confounder control, should those covariates truly be confounders (and not mediators, etc.). While we did not have that causal clarity on covariates (or sample size) here to make that trade-off, other synthetic control studies with sufficient sample size and covariate clarity should include carefully chosen covariates to better control for local confounding otherwise uncontrolled for by pre-intervention matching. That said, particularly when there is a scarcity of implementation sites and promising interventions, documentation of aspiring anti-racist interventions is worthwhile in the face of these limitations (Jones et al. 2019).

Further, the capture of race-ethnicity in administrative datasets has known limitations (Knox and Lowe 2019). Race-ethnicity is a powerful social construct associated with many associated health disparities (Tsai and Venkataramani 2016), so many we that require dedicated frameworks to harmonize them (Duran and Pérez-Stable 2019). Because of its social construction (Ford and Airhihenbuwa 2018), the meaning of race-ethnicity changes over place and time and can vary person to person even within the same time and place. Health research

acknowledges that self-identification may differ from social-identification (Jones et al. 2008). Even in the same person, conceptions of race-ethnicity change over the life course (Mihoko Doyle and Kao 2007). Concretely in this study, the self-identification options in justice databases are limited and may not match driver's self-identity. Stopping officers may not refer to driver-specified race-ethnicity, notably incomplete in NC driver's license records (Richard Stradling 2018), but instead fill out form SBI-122 based on their own ascription of the race of the driver. Indeed, there is documentation that in some regions law enforcement officers may knowingly misidentify race-ethnicity in response to scrutiny under new racial profiling laws and accountability that databases would seek to provide (Friberg et al. 2015).

Conclusions

Reprioritizing traffic stops for public health can reduce negative crash outcomes, reduce disparities, and may not have negative impacts on crime. More generally, a public health anti-racist approach requires, for example and at least, that injury prevention researchers who design interventions that will be enacted by law enforcement (e.g., seatbelt traffic stop campaigns) to consider the reality that some agencies and officers may intentionally or unintentionally target populations in racially disparate ways. The collateral damage of even well-intentioned public safety interventions may outweigh their benefits. These damages may be disparately born by low-income and communities of color. Public safety and public health are intimately related endeavors, as evidenced by their relationship to traffic stops. When engaged with public safety issues, public health should adopt a critical view of policing at the same time both fields must critically interrogate their own historical and present-day practices. Conventional logics, such as the Ferguson Effect belief that de-prioritizing investigatory stops is associated with increases in violent crime, may not hold up to critical scrutiny.

Public health has outlined an explicit call to anti-racist practice and principles. Law enforcement organizations, individual law enforcement agencies and officers, city councils, county boards, and community groups may elect to take up that call to guide their own activities. When co-designing traffic stop programs, these groups should consider goals of equity and maximizing public health impact alongside effects on community trust. But regardless of law enforcement agency action or non-action, public health advocates can use traffic stop datasets to both ensure their efficacy for public safety goals and document and act on any racially disparate impacts of these programs.

Supplementary information

Supplementary information accompanies this paper at https://doi.org/10.1186/s40621-019-0227-6.

Additional file 1: Table S1. Synthetic control weight vectors for each measure.

Additional file 2: Table S2. NC representativeness, access, and volume by race-ethnicity.

Abbreviations

ACS: American Communities Survey; APHA: American Public Health Association; CARS: Crash Analysis Reduction Strategy; CDC: Center for Disease Control; COPS: Community Oriented Policing Service; CRI-TA: Collaborative Reform Initiative for Technical Assistance; CRT: Critical Race Theory; DDACTS: Data-Driven Approaches to Crime and Traffic Safety; DiD: Difference-in-difference; DWI: Driving while intoxicated; GPS: Global Positioning System; HSRC: Highway Safety Research Center; NHTS: National Household Travel Survey; NHTSA: National Highway Traffic Safety Administration; PHCRP: Public Health Critical Race Praxis; SBI: State Bureau of Investigation; TSRR: Traffic stop rate ratio; US: United States; VMT: Vehicle Miles Traveled

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Authors' contributions

MDF completed this work as part of his PhD dissertation in epidemiology, including project conception, all analysis, and primary manuscript authorship. All co-authors read, provided edits, and approved the final manuscript.

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Availability of data and materials

The data that support the findings of this study are available from either publicly (e.g. US Census for demographic data, UNC HSRC for motor vehicle crash data at http://nccrashdata.hsrc.unc.edu/) or on request (e.g. NC SBI for crime or traffic stop data). Restrictions for datasets available on request, which were used under license for the current study, may apply, and so are available only through their requesting channels.

Ethics approval and consent to participate

This study uses only de-identified, administrative, secondary data collection and was exempted under UNC IRB #18–2186.

Consent for publication

This manuscript includes no data from individuals.

Competing interests

The authors declare that they have no competing interests.

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Publisher's Note

Misdemeanor Enforcement Trends Across Seven U.S. Jurisdictions

October 2020

Becca Cadoff, M.P.A., Preeti Chauhan, PhD, Erica Bond, J.D.

DATA COLLABORATIVE FOR JUSTICE AT JOHN JAY COLLEGE
STRENGTH IN NUMBERS

JOHN JAY COLLEGE OF CRIMINAL JUSTICE

Table of Contents

The Data Collaborative for Justice (DCJ) at John Jay College of Criminal Justice houses a group of research initiatives that raise important questions and share critical research about the criminal legal system and its role in creating safe, just, and equitable communities. DCJ conducts data analysis and research on enforcement in the community, the adjudication of cases in the courts, and the use of confinement in jails and prisons. DCJ's work has informed policy reforms, facilitated partnerships between researchers and government agencies across the country, spurred new scholarly research on lower-level enforcement, and has been cited extensively in the press. For more information about the Data Collaborative for Justice please visit: https://datacollaborativeforjustice.org/

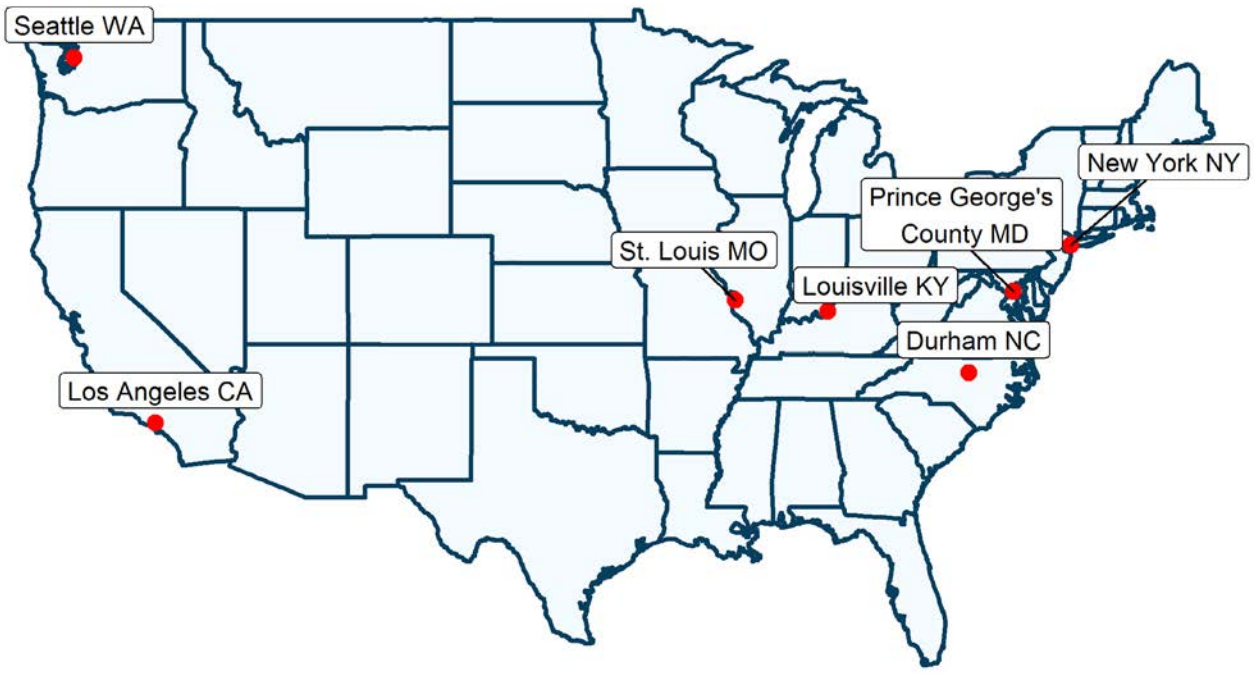
Misdemeanor Enforcement Trends Across Seven U.S. Jurisdictions

October 2020

The Research Network on Misdemeanor Justice

In 2016, the Data Collaborative for Justice (DCJ) at John Jay College launched the Research Network on Misdemeanor Justice ("the Research Network") with the goal of analyzing data and producing research to inform policy conversations and reforms related to lower-level enforcement, particularly misdemeanor arrests. After receiving applications from almost 40 jurisdictions, DCJ selected eight jurisdictions to join the Research Network: Durham, NC; Los Angeles, CA; Louisville, KY; New York City, NY; Prince George's County; MD; Seattle, WA; and St. Louis, MO.[1] In each jurisdiction, local researchers partnered with criminal justice practitioners and policymakers to produce reports on long-term trends in lower-level enforcement modeled on DCJ's prior reports, Trends in Misdemeanor Arrests in New York and Tracking Enforcement Rates in New York City.[2]

Figure 1. Map of Research Network Sites



[1] Meridian, MS is also a Research Network partner site. However, this site was unable to produce a report due to data collection challenges and instead utilized their Research Network resources to improve the jurisdiction's data infrastructure.

[2] Researchers in each jurisdiction were encouraged to modify their analytic approaches based on data availability, and to conduct analyses relevant to local concerns around criminal justice policy. A list of the reports can be found in Appendix A, List of Research Network Reports.

Since the Research Network's creation, much has changed in the larger world. In the months since the individual jurisdictions published their localized research, two factors have radically shifted misdemeanor enforcement practices and the national dialogue regarding the purpose and impact of misdemeanor enforcement. First, a global pandemic hit – COVID-19 – forcing jurisdictions across the U.S. to rethink existing law enforcement practices as the virus spread rapidly across the country, with jails and prisons hit particularly hard. Second, in the wake of high-profile police killings, including the deaths of George Floyd and Breonna Taylor, the criminal legal system has faced a moment of reckoning at a scale not seen in decades. Nationwide protests called out a system marred by racial disparities and at odds with the desires of many in the communities most impacted by it.

This project started almost five years ago to provide insights into how police have been interacting with communities around misdemeanor crimes – insights that have even greater meaning and consequence given the current moment. Historically, little effort had been made to gather data on misdemeanors even though they represent the vast majority of enforcement interactions between police and communities. Misdemeanors can also result in significant jail time and a permanent criminal record — both of which have a ripple effect on individuals' lives and their communities. **While the Research Network has found that enforcement of misdemeanors has decreased in recent years, in all of the years covered by this research, the people behind the data are disproportionately people of color**. Thus, these data tell a story about the communities of color, particularly Black communities, whose relationships with police have been shaped by years of high levels of misdemeanor enforcement.

As policymakers look to reform the criminal legal system, misdemeanors are an area ripe for continued discussion. This report, which combines and analyzes findings across seven jurisdictions, can be used as a basis for evidence-based policy that impacts individuals' lives and the well-being of whole communities. It can also point to the areas where further research is critically needed if we are to achieve the healthy and safe communities that we all want and deserve.

This report highlights cross-jurisdictional trends in misdemeanor arrests, drawing both from analyses published in the original Research Network reports and from updated data provided by the jurisdictions to DCJ for use in an interactive online dashboard. Despite certain data limitations (see Appendix B, Data Definitions & Limitations), several patterns emerged across the jurisdictions that provide important insights into law enforcement practices more broadly. These patterns include: (1) a general decline in misdemeanor enforcement from the late-2000s to the most recent year of reported data; (2) consistency with regards to the demographic groups experiencing the highest rates of enforcement (i.e., Black people, younger age groups, and males relative to White people, oldest age groups, and females); and (3) similar trends in the types of crimes being enforced – "person-related" charges (those with an identifiable victim) increased as a proportion of arrests while drug-related charges decreased over time. Notably, fluctuating rates of misdemeanor enforcement in each of the Network Sites did not appear to influence crime rates, particularly violent crime rates, which either remained stable (St. Louis, Seattle), increased slightly (Louisville), or decreased substantially (New York, Prince George's County, Los Angeles). This is consistent with other research that indicates there is a not a direct relationship between misdemeanor enforcement and the prevention of more serious crime (Piza, 2018; Sullivan & O'Keeffe, 2017).

KEY FINDINGS

- Misdemeanor Arrest Rates: The misdemeanor arrest rates in all Research Network jurisdictions decreased in recent years. These declines often followed a period of significant increases in misdemeanor enforcement.

- Misdemeanor Arrests by Race: Black people were arrested at the highest rates of any racial/ethnic group for all jurisdictions across the entire study period. **Racial disparities between Black people and White people existed in all jurisdictions**, and these disparities persisted despite the recent overall declines in arrest rates. However, the magnitude of the disparities varied by jurisdiction and over time -- ranging from approximately three to seven arrests of Black people for one arrest of a White person.

- Misdemeanor Arrests by Age: Arrest rates were highest for younger age groups (i.e., 18-20-year-olds and 21-24-year-olds) at the beginning of the study period. At the same time, arrest rates were generally much lower for the oldest age group (i.e., 35-65-year-olds). Over time, **arrest rates for the younger age groups fell the most, sometimes to rates lower than 25-34-year-olds.**

- Misdemeanor Arrests by Sex: **Males were arrested at higher rates than females** in all jurisdictions across the study period. Although the arrest rates for males fell more than for females, this gender gap in arrest rates persisted over the study period.

- Misdemeanor Arrests by Charge: Within the context of fluctuating misdemeanor arrests, the composition of misdemeanor charges changed over time across most sites. **Cross-jurisdiction trends indicate a move away from more discretionary, drug-related charges and an increase in the share of charges where there is an identifiable complainant or victim ("person-related" offenses)**.

- Additional Research on Misdemeanor Arrests: The extensive analyses presented in this report provide critical insights into misdemeanor enforcement across seven geographically diverse jurisdictions but also raise many more questions. **Future research should examine the impact of misdemeanor arrests on individuals and communities, including how changing rates of enforcement impact community safety, trust and confidence in the police, jail populations, and the work of prosecutors, defense attorneys, and the courts**. Further, additional research is needed to understand trends in misdemeanor enforcement in non-urban areas (the Research Network jurisdictions are generally in or close to large, urban areas). Finally, **research should also focus on the relationship between misdemeanor arrests and issues of public health, community safety, and racial equity that have been highlighted by the COVID-19 pandemic and recent nationwide protests against police brutality**.

Why Do Misdemeanor Arrests Matter?

Misdemeanor offenses make up the majority of criminal cases nationwide, with estimates ranging from 75% to 80% each year (Natapoff, 2018; Stevenson & Mayson, 2018). Although misdemeanor arrests are considered to be less serious than felony offenses (e.g., homicide, grand larceny), they can still result in significant jail time and a permanent criminal record – both of which have been shown to negatively impact individuals' lives (Kohler-Hausmann, 2018; Natapoff, 2018). Further, enforcement of state misdemeanor statutes does not reflect the full spectrum of lower-level contacts that police have with community members[3] – local police may also enforce local, ordinance, or municipal violations through arrests, citations, and summonses, as well as conduct other kinds of stops (traffic and pedestrian).

What drives misdemeanor enforcement?

Although misdemeanor enforcement may be driven by crime (Hughes et al., 2020) and community calls for service (Glazener et al., 2020), research has also shown that this activity may be the result of other influences. Changes in lower-level enforcement may be driven by factors that include:

- Police department policies and priorities (Lum & Vovak, 2017),
- Local government reliance on the fines and fees generated by a large number of convictions of misdemeanor arrests (Martin, 2018; United States Department of Justice, 2015),
- Individual officer actions in areas of concentrated economic disadvantage (Smith, 1986; Sun et al., 2008),
- Budgetary allocations and grants (Slocum et al., 2018).

What impact does misdemeanor and other lower-level enforcement have on individuals and communities?

There is evidence that suggests misdemeanor arrests and other lower-level enforcement activities do not always maximize public safety[4] and, in some cases, can undermine trust and confidence in the police (Schuck, 2020). Contact with the criminal legal system can also harm an individual's health and well-being (Sundaresh et al., 2020; Vergano, 2019). Lower-level arrests, and even police stops, can negatively impact individuals and the communities they come from in a variety of ways, by:

- Decreasing the likelihood of cooperation with law enforcement in the future (Schuck, 2010; Tankebe, 2013),
- Reducing opportunities related to education, employment, and housing (Roberts, 2011),
- Increasing the likelihood that an individual is stopped or arrested again as a consequence of reduced access to education, employment, and housing (Malcolm & Siebler, 2017).

Ultimately, understanding trends in misdemeanor enforcement is vital for a number of reasons. First, it can help communities and policymakers determine whether the types of misdemeanor crimes that police are enforcing are a priority for those communities and/or whether other resources are needed to address persistent social problems (e.g., related to substance misuse). Second, it can help communities assess whether disparities in enforcement by race, age, gender, or neighborhood require reforms to ensure that the criminal legal process is not reinforcing or exacerbating inequities in society. Finally, with information about local trends in misdemeanor enforcement, the public, government leaders, and advocates are better positioned to weigh community safety concerns against the potential harms of misdemeanor arrests.

[3] For instance, in New York City, in 2014, there were 256,754 misdemeanor arrests, an additional 43,495 pedestrian stops, and 369,058 criminal summonses issued (Chauhan et al., 2015). In St Louis, in 2017, there were 2,103 misdemeanor arrests, 19,921 traffic stops, 5,102 criminal summonses and 2,437 arrests for ordinance violations (Slocum et al., 2018).

[4] Guardian actions, such as citizen contacts and business checks, may have more public safety benefits than enforcement actions such as arrests and field interrogations (Piza, 2018). Further, at least two New York City-based studies that investigated the relationship between low-level enforcement and major crime found that there is no direct relationship between the two (Sullivan & O'Keeffe, 2017; Rosenfeld et al., 2007).

Misdemeanor Arrests and COVID-19

While this report was being finalized, the COVID-19 pandemic rippled around the globe and continues to infect large numbers of people around the nation. In many parts of the country, the pandemic has forced changes to the operations of the criminal legal system, which in turn have impacted the health and safety of people who come into contact with the system. COVID-19 has altered law enforcement policies and practices, forced courts to pause or limit the processing of cases and/or establish remote operations, and required jails and prisons to respond to large scale outbreaks (Equal Justice Initiative, 2020). The pandemic has also raised concerns about whether and how police should respond to the social and economic fallout from COVID-19 at a time when many are calling for a reduced law enforcement footprint in communities.

Law enforcement practices have changed in direct response to COVID-19 in at least two ways. First, police have been called upon to enforce "stay-at-home orders" and orders from government officials to wear masks in public. Initial reports documented racial disparities in arrests for COVID-19 related violations. In some places, this type of enforcement has been directed disproportionately at people of color (Bates, 2020; Jabali, 2020). Second, arrests for lower-level offenses appear to have fallen dramatically. This is likely the result of fewer people being out in public due to "stay-at-home orders", which in turn decreases opportunities for crime, victimization, and enforcement (Elinson & Chapman, 2020; Kamana, 2020). Further, some police departments have directed officers to avoid making lower-level arrests in order to contain the spread of the virus (Elinson & Chapman, 2020). It remains to be seen whether misdemeanor enforcement will increase as people become more active outside their homes again.

The pandemic has not only directly impacted law enforcement practices but has also produced social crises that police may be called on to address. The economic and mental health distress created by COVID-19 may result in more calls for service related to persons in a mental health crisis, poverty-related offenses (e.g., trespassing and shoplifting due to housing and food insecurity), and/or quality of life offenses. It remains to be seen whether calls for service will increase, whether enforcement will increase in tandem, or if jurisdictions will expand social services such as mental health programs, affordable housing, and job programs that can prevent and address engagement with the criminal legal system.

Future research on misdemeanor arrests should rigorously examine the intersections between the criminal legal system and COVID-19. Researchers should explore ways that the pandemic has affected arrest practices, incarceration policies, and public safety.[5] Particularly, as gun crimes and homicides have increased in some places (DeBruyn, 2020), which could prompt greater police enforcement across the board. Ultimately, COVID-19 raises critical questions about whether and what kinds of lower-level enforcement make sense in a world in which a deadly disease is spreading rapidly. These questions are particularly important for Black and Latinx communities, many of which are already burdened by the economic and social costs imposed by higher rates of lower-level enforcement and are now contending with higher rates of COVID-19 infections (Artiga et al., 2020; Oppel et al., 2020; The New York Times, 2020).

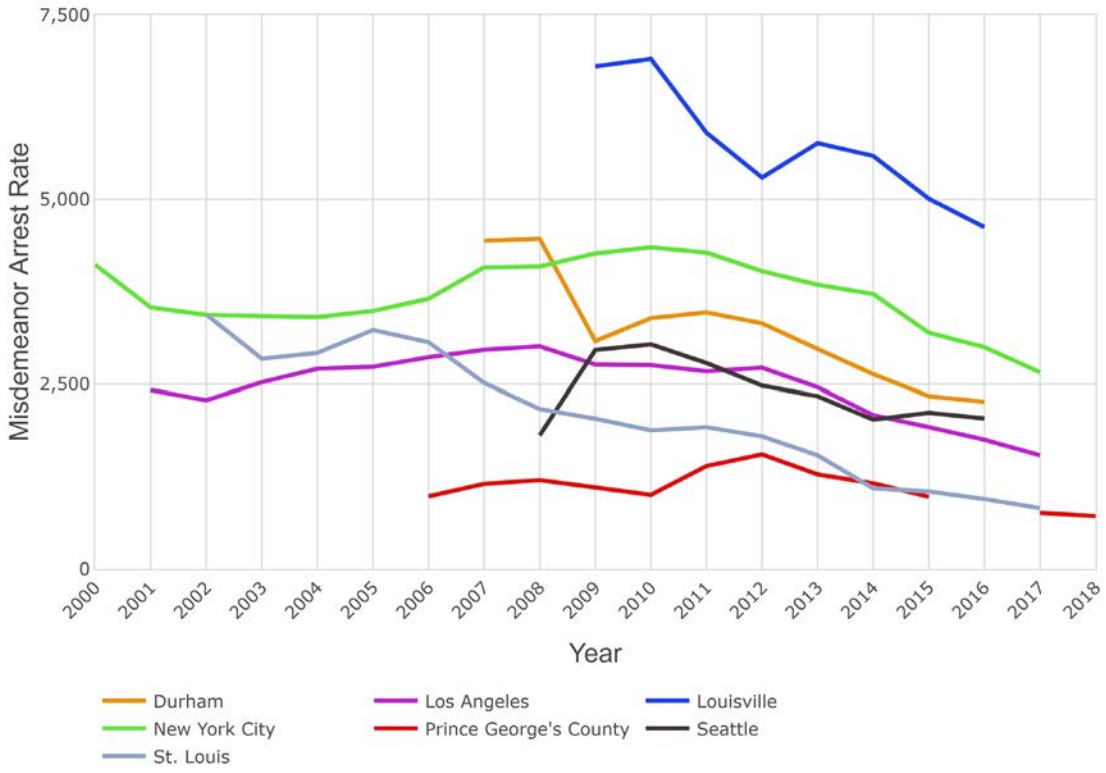
[5] Organizations like the the Prison Policy Institute, the Marshall Project, and the Brennan Center publish crucial, frequently-updated information and resources regarding COVID-19 and its impact on both law enforcement responses and communities.

Overall Trends In Misdemeanor Enforcement Rates

Key Finding: **The misdemeanor arrest rates in all Research Network jurisdictions decreased in recent years, often following a period of significant increases in misdemeanor enforcement.**

Figure 2 presents the rate of misdemeanor arrests per 100,000 residents in each jurisdiction during the study period.

Figure 2. Cross-Site Trends in Misdemeanor Arrest Rates



Although the rates varied significantly from site-to-site and over time, several cross-site trends emerged.[6]

Across the Research Network sites, with only one exception,[7] misdemeanor arrest rates peaked between 2008 and 2012. In Durham and Los Angeles, the highest arrest rates occurred in 2008; 2010 in Louisville, New York City, and Seattle; and 2012 in Prince George's County. These peak arrest rates varied widely, from 6,905 arrests per 100,000 people in Louisville to 1,552 arrests per 100,000 people in Prince George's County – some of this variation can be explained by differences in how misdemeanor arrest data are aggregated.[8]

[6]This cross-site report analyzes trends based on the "beginning" and "end" year of each jurisdiction's study period, with the understanding that each milestone may be represented by a different year for each site. We also examined the "peak" year in each jurisdiction, meaning the year during which the arrest rate was highest over the study period in that locality. This milestone may also have occured in different years for different jurisdictions.

[7]In St. Louis, the year with the highest misdemeanor arrest rate was the first year reported, 2002. Therefore, it is possible the peak year for misdemeanor arrests occurred earlier than the beginning of the study period for St. Louis.

[8]In Louisville, researchers produced misdemeanor enforcement data and rates that reflect all charges associated with a single arrest, including the many vehicle- and driving-related offenses that often co-occur with other charges. In all the other jurisdictions, researchers produced enforcement data and rates based on the number of misdemeanor arrests rather than the total number of charges associated with an individual arrest. This difference helps to explain the much higher rates of enforcement for Louisville reflected in Figure 2.

The recent trend in all of the Research Network jurisdictions was a decline in misdemeanor arrests from peak to end. The magnitude of these recent declines ranged from 33% in Louisville and Seattle to 76% in St. Louis.

Table 1. Misdemeanor Arrest Rates and Percent Change

Jurisdiction	Misdemeanor Arrest Rate			Percent Change		
	Beginning (Year)	Peak (Year)	End (Year)	Beginning to End	Beginning to Peak	Peak to End
New York City[9]	4,117 (2000)	4,351 (2010)	2,658 (2017)	35% Decline	6% Increase	39% Decline
Los Angeles	2,421 (2001)	3,008 (2008)	1,540 (2017)	36% Decline	24% Increase	49% Decline
St. Louis	3,441 (2002)	3,441 (2002)	824 (2017)	76% Decline	0% Change	76% Decline
Prince George's County[10]	984 (2006)	1,552 (2012)	716 (2018)	27% Decline	58% Increase	54% Decline
Durham	4,440 (2007)	4,466 (2008)	2,262 (2016)	49% Decline	1% Increase	49% Decline
Seattle	1,810 (2008)	3,035 (2010)	2,038 (2016)	13% Increase	68% Increase	33% Decline
Louisville	6,803 (2009)	6,905 (2010)	4,624 (2016)	32% Decline	1% Increase	33% Decline

In many jurisdictions **the recent decreases in misdemeanor arrest rates from their peaks were smaller when the full study period was considered**. For example, in Prince George's County, the misdemeanor arrest rate declined by 54% from the peak in 2012. However, the arrest rate had increased by 58% from the beginning of the study period to the peak, resulting in an overall decline of 27% from beginning to end of the study period. In Seattle, the misdemeanor arrest rates actually increased by 13% over the full study period, despite a recent decline from the peak enforcement rate.

Further, declines from recent peaks may actually mask longer-term increases in enforcement rates. For example, in New York City, where data are available as far back as 1980, misdemeanor arrest rates declined by 39% between 2010 and 2017 (the study period for this report), but actually increased by 91% between 1980 and 2017. Therefore, it is possible that, with more data and longer study periods, we might learn that misdemeanor enforcement in other jurisdictions is also well above historical lows, despite recent decreases in enforcement.

[9]The original New York City report documented trends in misdemeanor arrests from 1980 to 2017. However, this cross-site report examines New York City data starting in 2000 for better comparability to other jurisdictions since all other jurisdictions begin their study period after 2000.

[10]Due to a change in the Prince George's County Police Department's record management system, data for 2016 were not complete and therefore could not be used in this study.

Understanding Arrest Rates and Rate Ratios

An arrest rate is a way to standardize the number of arrests in a given jurisdiction to the population size of that jurisdiction. For example, 1,000 arrests in a place like Los Angeles – home to about four million people – is much different than 1,000 arrests in St. Louis, which has just over 300,000 people. A rate can account for this difference in population.

An arrest rate per 100,000 people is calculated by dividing the number of arrests that occurred by the population over the age of responsibility in that jurisdiction, and then multiplying that result (which is how many arrests you would expect for every person) by 100,000. For example:

Calculation for the Rate of Misdemeanor Arrests in St. Louis in 2017. Note that 17 was the age of criminal responsibility in St. Louis in 2017.

$$\frac{\text{St. Louis Arrests}}{\text{St. Louis 17+ Population}} = \frac{2{,}103}{255{,}220} = 0.00824 \times 100{,}000 = 824$$

An arrest rate ratio is one way to compare two groups using a single number. Here, the arrest rate for Black people is divided by the arrest rate for White people. For example:

Rate Ratio Calculation for the 2017 St. Louis Black-White Racial Disparity

$$\frac{\frac{\text{St. Louis Arrests for Black Individuals}}{\text{St. Louis 17+ Black Population}} = \frac{1{,}515}{115{,}920} \times 100{,}000 = 1{,}307}{\frac{\text{St. Louis Arrests for White Individuals}}{\text{St. Louis 17+ White Population}} = \frac{580}{127{,}750} \times 100{,}000 = 454} = 3.0$$

This calculation can also be interpreted as the relative likelihood that someone will be arrested in one group compared to a person from another group. For example, a rate ratio of three means that a Black person is three times more likely to be arrested than a White person, even when taking into account differences in the size of the population for each group.

Misdemeanor Trends By Demographics

Trends by Race and Ethnicity

Key Finding: **Black people were arrested at the highest rates of any racial/ethnic group for all jurisdictions across the entire study period**. Racial disparities between Black people and White people existed in all jurisdictions, and these disparities persisted despite the recent overall declines in arrest rates. However, the magnitude of the disparities varied by jurisdiction and over time – ranging from approximately three to seven arrests of Black people for one arrest of a White person.

In all jurisdictions, **the arrest rate for Black people was the highest of any racial/ethnic category at the study start.** Further, in nearly all sites, **the arrest rate for Black people saw the steepest decline**. Despite these large proportional declines, **the arrest rate for Black people was still the highest of any racial/ethnic group at study end, with only one exception**.[11] Conversely, in most jurisdictions at study start, the arrest rate was lowest for White individuals and remained low throughout the study period.

Across the Research Network sites, **the Black-White disparity in misdemeanor arrest rates varied widely**. In some jurisdictions, this racial disparity was much starker than in others. For example, the Black-White racial disparities in Prince George's County and Louisville ranged from 2.2 to 3.4 and 2.9 to 3.4, respectively, over the study period (see Table 2 and see *Understanding Arrest Rates and Rate Ratios* on page 8). Other jurisdictions had much higher levels of racial disparity: racial disparities in Durham ranged from 5.8 to 6.2 and in Seattle these disparities ranged from 5.9 to 7.1 over the study period.[12]

Despite declines in overall arrest rates and in arrest rates for Black people in particular, racial disparities in arrest rates did not necessarily improve. In many jurisdictions, **racial disparities increased from study beginning to peak** -- meaning that Black people accounted for a larger share of the increase in arrests for misdemeanors (New York City, Los Angeles, Prince George's County, and Durham). Some sites also experienced increases in racial disparities in misdemeanor enforcement from beginning to end of the study period (New York City, Los Angeles, and Prince George's County). Thus, even though arrest rates declined in all jurisdictions, **in some jurisdictions racial disparities have, in fact, increased over time**.

However, as with the overall arrest rates, examination of the **more recent trends (from peak to end) show some reductions in racial disparities**. For instance, in no jurisdiction did the Black-White racial disparity increase from study peak to study end; in two jurisdictions the disparity stayed the same (Los Angeles and Prince George's County), and in the remaining six, the disparity decreased somewhat.

In jurisdictions where researchers were able to examine arrest rates for Latinx people,[13] **the arrest rate for Latinx people was generally the second-highest and almost always between the rates of Black people and White people** (data on arrests of Latinx individuals was not available for St. Louis or Seattle). This reflects a broader failure to consistently collect information about Latinx people and their contacts with the criminal legal system across the country (Urban Institute, 2016).

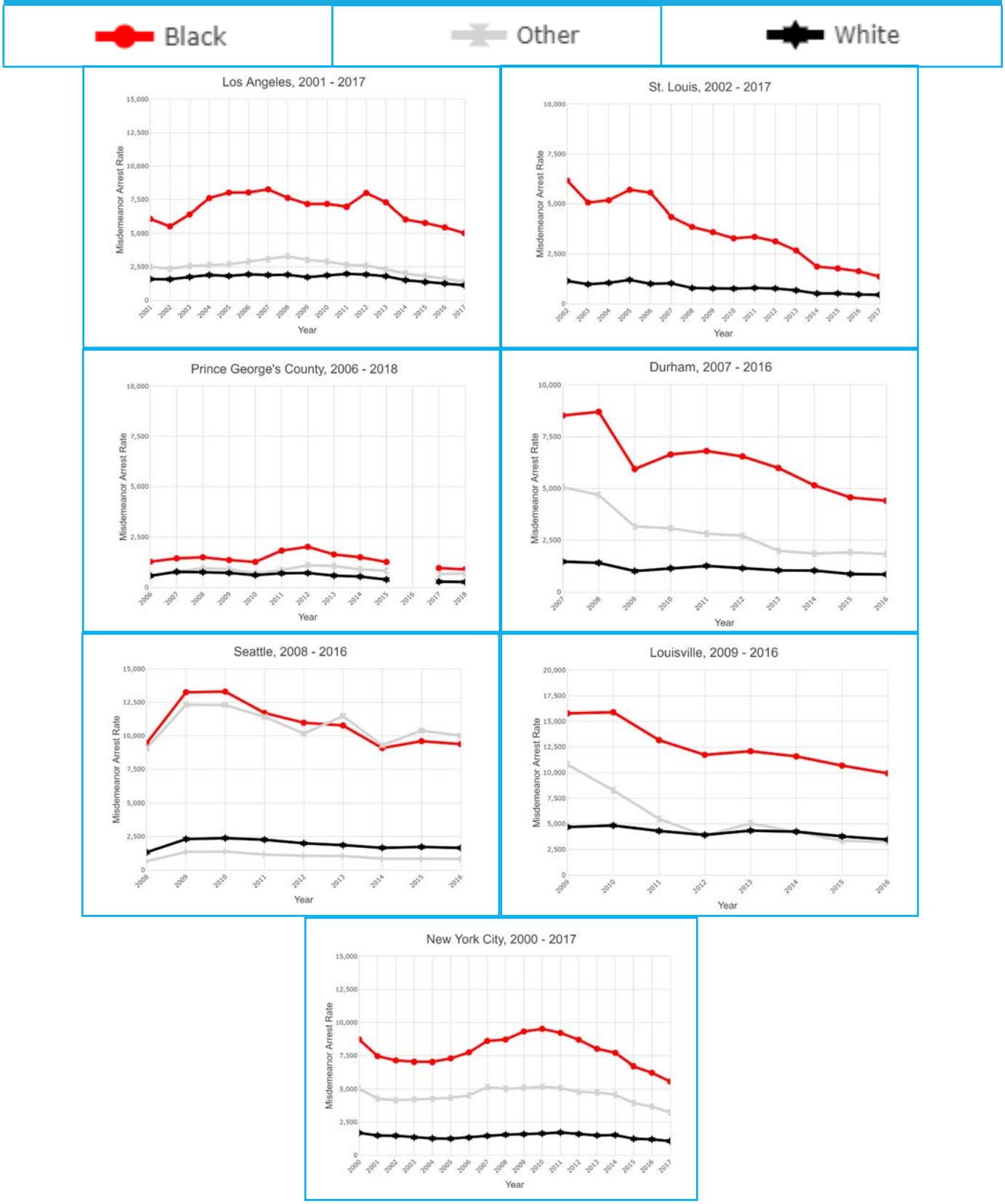
Finally, some jurisdictions analyzed arrest patterns for additional racial/ethnic categories that were important to their local communities. For example, Seattle has large Indigenous and Asian communities and therefore local researchers examined arrest rates for these populations. A number of sites include "other" as a racial/ethnic group to include individuals who did not fit into the more common categories. Arrest rates for these additional racial/ethnic breakdowns can be found in the original Research Network reports or the online data dashboard.

[11]In Seattle, the arrest rate for Indigenous people was higher, at times, than the arrest rate for Black people.

[12]DCJ cautions against direct comparisons of racial disparities between jurisdictions because of differences in data collection practices and methodologies across the Research Network sites (see Appendix B, Data Definitions & Limitations). For instance, Seattle and St. Louis include Latinx people in the categories for White and Black people, whereas other sites have separate categories for non-Latinx Black people and non-Latinx White people.

[13]The original Research Network reports used the term "Hispanic." This may be because that is the terminology used in the original data sources. However, in this report we use Latinx, which is intended to encompass the diversity of cultures, languages, and countries in Latin America (Salinas & Lozano, 2019).

Figure 3. Cross-Site Trends in Misdemeanor Arrest Rates by Race/Ethnicity



Policing and the Black Lives Matter Movement

Recent, high-profile incidents of police violence, including the deaths of Breonna Taylor in Louisville, KY, Rayshard Brooks in Atlanta, GA, and Elijah McClain in Aurora, CO have spurred a broad conversation about the role of police in communities, and especially in Black communities (Associated Press, 2019; Oppel & Taylor, 2020; Ortiz, 2020; Prentzel, 2020; Raice, 2020). By some estimates, the Black Lives Matter movement may be the largest social movement in U.S. history, with millions of people expressing their pain and unhappiness with the state of American policing (Buchanan et al., 2020). Recent demonstrations have been directed both at police violence captured on video and overpolicing in communities of color (Brunson, 2007; Gau & Brunson, 2010).

The Black Lives Matter movement has elevated a number of important issues with respect to misdemeanor enforcement and policing. It has called attention to the fact that many high profile civilian deaths at the hands of police escalated from what initially began as misdemeanor encounters — these include the deaths of George Floyd in Minneapolis, MN, Crystalline Barnes in Jackson, MS, and Eric Garner in New York City. These deaths, which reflect just a small subset of the cases where police use force during a misdemeanor encounter, call into question whether misdemeanor interactions can be characterized as "low-level" given their potential to cause significant harm to individuals and communities. Further, the Black Lives Matter movement raises important questions, not just about whether and how police should be enforcing misdemeanors and other lower-level crimes, but also about whether other kinds of resources, such as mental health clinicians or housing, should be deployed as the first-line response to significant social challenges including mental health crises, homelessness, and school safety (Neborsky, 2020).

The Black Lives Matter movement has also brought attention to how police respond to such demonstrations and social unrest. In some cities, including New York City, Los Angeles, Dallas and Philadelphia, the police arrested large numbers of protestors, many for low-level crimes related to violations of local curfews, failure to disperse, or disorderly conduct (Snow, 2020). Some departments have also been criticized for using violence in response to the protests, including as a means of crowd control and to make arrests (McCann et al., 2020). Further, there is speculation that, in some jurisdictions, police may be engaging in a "slow-down" in response to calls for policing reform (Southall, 2020; Tobin & Kachmar, 2020). Sometimes termed "the Ferguson Effect" (Byers, 2014), the full implications of these "slow-downs" in response to demonstrations have yet to be understood (Rosenfeld, 2015; Rosenfeld & Wallman, 2019).

The Black Lives Matter protests, and the police responses to them, have driven a sharp dialogue about the role that racism has played in police enforcement against Black people and the resulting harms to generations of Black people. This dialogue will benefit from continued research on misdemeanors, which constitute the majority of police enforcement activities. Researchers should contribute to the dialogue by examining the role of race in enforcement, particularly lower-level enforcement, assessing the police response to current protests demanding reforms, and measuring the impacts of reforms on racial disparities in enforcement.

Table 2. Racial Disparities in Misdemeanor Enforcement: Number of Arrests for Black People for One Arrest of a White Person			
Jurisdiction	Rate Ratio at Beginning (Year)	Rate Ratio at Peak (Year)	Rate Ratio at End (Year)
New York City	4.4 (2000)	5.9 (2010)	5.2 (2017)
Los Angeles	3.8 (2001)	4.5 (2008)	4.5 (2017)
St Louis	5.4 (2002)	5.5 (2002)	3.0 (2017)
Prince George's County	2.2 (2006)	3.4 (2012)	3.4 (2018)
Durham	5.8 (2007)	6.2 (2008)	5.2 (2016)
Seattle	7.1 (2008)	7.1 (2010)	5.7 (2016)
Louisville	3.4 (2009)	3.4 (2010)	2.9 (2016)
Note: For a detailed explanation of rate ratios, please see *Understanding Arrest Rates and Rate Ratios* on page 8.			

Enforcement and Racial Disparities

It is important to note that the higher arrest rates for Black people compared to other racial/ethnic categories **do not mean that Black people are more likely to engage in misdemeanor criminal activity than other groups**. Indeed, there is evidence that is not the case. For example, one study found that although Black people are arrested for use of illicit substances more than White people, actual rates of use are similar for both groups (Lum & Isaac, 2016).

Unfortunately, due to limitations in data collection and analysis, empirical research has yet to definitively quantify the extent to which racial disparities in lower-level enforcement may be explained by racial bias in policing versus biases that pervade U.S. institutions and policies and leave people of color at greater risk of poverty (National Academy of Sciences, Engineering, and Medicine, 2018). Some studies have demonstrated that racism plays a role in racial disparities in traffic enforcement (Chanin et al., 2018; Pierson et al., 2020). Other studies have pointed to the fact that majority Black communities tend to have higher rates of poverty, lower economic mobility, and higher rates of violent crime – and therefore a greater police presence (Braga et al., 2019; Fagan & Davies, 2000; Fagan et al., 2016; Gaston, 2019). This greater police presence increases the likelihood that police will observe criminal conduct and use their discretion to enforce lower-level crimes. Finally, a jurisdiction's demographic characteristics may play a role in racial disparities in enforcement. One study by the California Public Policy Institute showed that the California counties with the largest racial disparities in arrest tended to be wealthier, better-educated, and have lower numbers of Black residents than counties with less racial disparities (Lofstrom et al., 2019). Ultimately, racial disparities in misdemeanor enforcement are likely driven by a number of factors. Researchers should identify the biggest drivers of these disparities, including racial bias on the part of individual officers, policing policies and deployment, and broader government policies and programs, to identify promising methods of eliminating them.

Trends by Age

Key Finding: Arrest rates were highest for younger age groups (i.e., 18-20-year-olds and 21-24-year-olds) at the beginning of the study period. At the same time, arrest rates were generally much lower for the oldest age group (i.e., 35-65-year-olds).[14] **Over time, arrest rates for the younger age groups fell the most, sometimes to rates lower than 25-34-year-olds**.

Over the study period, the arrest rates for the two youngest age groups had the steepest declines. In nearly all jurisdictions, these declines brought the arrest rates for 18-20-year-olds and 21-24-year-olds down to roughly the rate for 25-34-year-olds.[15]

For example, in Louisville, the arrest rate for 18-20-year-olds was as high as 17,500 per 100,000 people while the arrest rate for 35-65-year-olds was closer to 2,500 per 100,000 people. The arrest rate for the younger age group dropped by 36% from study beginning to study end. Similarly, in St. Louis, the arrest rate for 18-20-year-olds fell by 85% over the study period, from nearly 9,000 per 100,000 people to about 1,300 per 100,000 people; the arrest rate for 35-65-year-olds also fell, but started much lower at about 1,500 per 100,000 people to about 450 per 10,000 people, a decline of about 75%.

[14]There are a few notable exceptions. First, in New York City, the arrest rate for both young adult groups, 18-20 and 21-24, were much lower at study end than at start or peak, but both were still higher than the 25-34 group. Second, in Louisville and Durham, the rate for 21-24 was higher than 25-34 at study end.

[15]In an exception to these trends, for some years in Los Angeles, the arrest rate for 25-34-year-olds was almost as low as the rate for 35-65-year-olds. Additionally, by study end, the arrest rate in Los Angeles for 18-20-year-olds was almost as low as the rate for 35-65-year-olds.

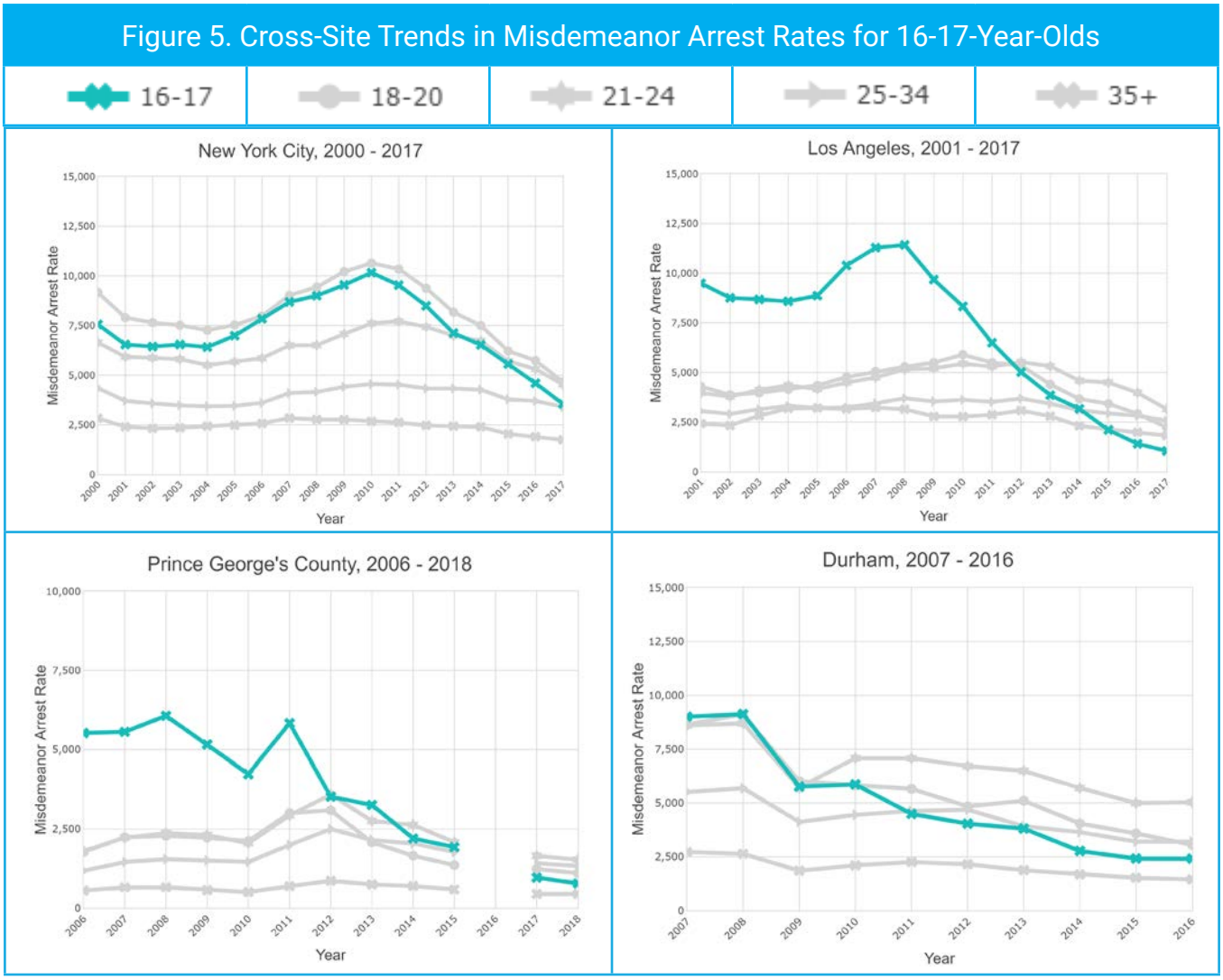
Figure 4. Cross-Site Trends in Misdemeanor Arrest Rates by Age

A CLOSER LOOK AT 16-17-YEAR-OLDS

In most U.S. states, the age of criminal responsibility is 18 (Justice Policy Institute, n.d.). This generally means that individuals who are younger than 18 at the time of arrest have their cases handled by a court system separate from "adult" criminal court, often referred to as juvenile court or family court. Typically, these specialized court systems have procedures that are designed to respond to the developmental considerations and other unique factors that young individuals' cases require (Office of Juvenile Justice and Delinquency Prevention, 1999).

However, in New York and North Carolina (which include the Research Network jurisdictions of New York City and Durham), the age of criminal responsibility was younger than 18 until very recently.[16] Therefore, in their reports, research partners in New York City and Durham included arrest rates for 16-17-year-olds because those arrests were processed in the criminal rather than juvenile systems. Further, although criminal courts in California and Maryland do not process arrests of 16-17-year-olds (which include the Research Network jurisdictions of Los Angeles and Prince George's County), these two jurisdictions had access to data on this younger age category and included them in their analyses. Therefore, DCJ was able to conduct some cross-site analyses on arrests of 16-17-year-olds.[17]

Figure 5. Cross-Site Trends in Misdemeanor Arrest Rates for 16-17-Year-Olds



[16]In New York State, the age of criminal responsibility was raised incrementally from 16 to 18: as of October 1, 2018, the age of criminal responsibility was raised to 17, and as of October 1, 2019, to 18 (Green, 2018). In North Carolina, the age of criminal responsibility was raised to 18 as of December 1, 2019 (North Carolina Department of Public Safety, n.d.).

[17]In Missouri, 17-year-olds are processed through the adult criminal court system. However, the St. Louis researchers were only able to incorporate data on arrests of individuals 18 and older into their Research Network report, so St. Louis is not included in this analysis of 16-17-year-olds. A new law that goes into effect in 2021 will raise the age of criminal responsibility in Missouri to 18 (Raise the Age Missouri, 2017).

At study start, **16-17-year-olds were often arrested at very high rates, sometimes even higher than 18-20-year-olds or 21-24-year-olds**. For example, for a majority of the study period, the arrest rate for 16-17-year-olds was significantly higher than any other age group in Prince George's County and Los Angeles. In New York City and Durham, the arrest rate for 16-17-year-olds was similar to that of 18-20 and 21-24-year-olds.

Like the other younger age groups, **the arrest rate for 16-17-year olds fell dramatically over the study period**. For example, in New York City, the arrest rate for 16-17-year-olds was on par with that of the 25-34-year-olds by study end. In Prince George's County, the rate was almost as low as that for 35-65-year-olds. In Durham, the rate was second lowest, and in Los Angeles, the rate for this age group was lower than for any other age group.

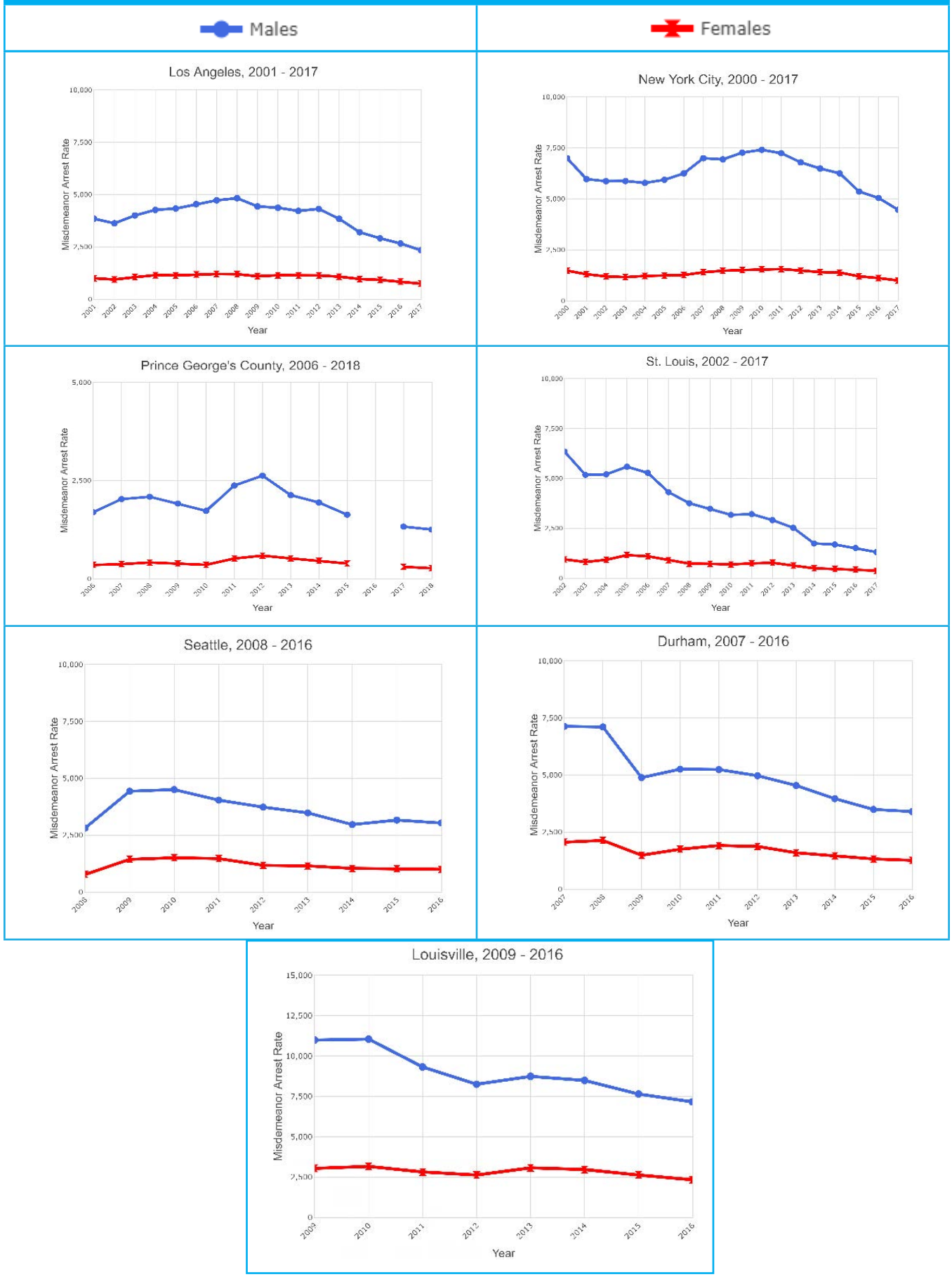
Trends by Sex

Key Finding: **Males were arrested at higher rates than females** in all jurisdictions across the study period. Although the arrest rates for males fell more than for females, this gender gap in arrest rates persisted over the study period.

As with racial trends, **the differences in arrest rates between males and females varied substantially across jurisdictions**. For example, in St. Louis at study start, males were arrested at a rate of about 6,000 per 100,000 people while females were arrested at a rate of about 1,000 per 100,000 people. However, in Seattle and Prince George's County, the arrest rates for both males and females were much lower overall, with males arrested at rates of about 2,500 per 100,000 people and females under 1,000 per 10,000 people.

Males generally saw greater declines in misdemeanor arrest rates than females. For example, in St. Louis, where the misdemeanor arrest rate declined by the largest percentage overall, the arrest rate for females declined by 60% while the arrest rate for males declined by 79% from peak to end. This indicates that **the overall reduction in the misdemeanor arrest rate impacted males more than females**. Even in jurisdictions with less dramatic declines, like Durham, the overall reduction was greater for males (52% decline) compared to females (41% decline).

Figure 6. Cross-Site Trends in Misdemeanor Arrest Rates by Sex



Misdemeanor Trends By Charge

Key Findings: Within the context of fluctuating misdemeanor arrests, the composition of misdemeanor charges changed over time across most sites. **Cross-jurisdiction trends indicate a move away from more discretionary, drug-related charges and an increase in the share of charges where there is an identifiable complainant or victim** ("person-related" offenses).

Despite the challenges of aggregating charge-related findings across jurisdictions that may have varying penal codes and thus categorize types of criminal conduct differently (see Appendix B, Data Definitions & Limitations), the Research Network reports reveal some offense-specific trends in misdemeanor arrests.[18] The jurisdictions reported on a wide variety of charges, but some of the clearest patterns emerged with respect to the proportions of arrests that were comprised of drug-related, person-related, and property-related offenses. **Drug-related offenses**, which include possession of marijuana and other controlled substances, may be considered a more discretionary law enforcement activity, particularly in the context of misdemeanor arrests (Alexander, 2010). Conversely, person-related offenses and property-related offenses include an identifiable victim or complainant and are therefore less discretionary. **Person-related offenses** include assault, harassment, and stalking, which often resulting in direct harm to known victims. **Property-related offenses** include larceny, graffiti, and forgery, which are typically related to unlawful possession or destruction of property.

The analyses presented in Table 3 focus on the proportion of arrests for a particular charge category, rather than the rate of arrest for that charge category. This provides a sense of how misdemeanor arrests are distributed across different charge categories, even as arrest rates fluctuate over time. For example, a jurisdiction might find that even while misdemeanor arrest rates have declined substantially over time, the composition of arrests may change to reflect changing patterns of crime and policing strategies (e.g., enforcement of drug crimes may represent a smaller proportion of crimes).

In **four of the six jurisdictions that examined charges, the proportion of drug-related charges decreased over time** (see Table 3). This was the case in Durham, New York City, St. Louis, and Seattle.[19] For example, drug arrests in New York City fell from 46% of all misdemeanor arrests to about a quarter of arrests at study end. In St. Louis, the proportion of drug arrests fell from 16% to 8%. However, the proportion of drug arrests in Los Angeles started and ended at roughly the same proportion (10% to 12%), and the proportion in Prince George's County increased slightly from 17% to 20%.

In nearly all jurisdictions except Prince George's County, **the proportion of arrests for person-related offenses stayed the same or increased from study beginning to study end** (see Table 3). For example, in New York City, the proportion of person-related charges increased from 17% to 28%, and in Durham, these charges increased from 20% to 32%. In Prince George's County, the proportion of person-related charges started and ended at 21% but declined from 26% at study peak. However, these beginning to end trends hide some sizeable fluctuations over the study period. In many sites, there were periods of increased proportions of person-related charges and other periods of sharp declines. For instance, in Los Angeles, person-related offenses decreased from study beginning to peak (10% to 6%) before increasing again to 10%.

[18]Louisville did not report on charge type.

[19]In some Research Network jurisdictions there were statutory changes that may have contributed to trends over time, particularly for drug-related charges. For example, in Seattle, as early as 2003, voters and law enforcement moved to deprioritize and then decriminalize marijuana possession for personal use among adults. As a result, the drug arrest rate in Seattle was much lower than in other sites. In 2014, California passed Proposition 47 which shifted certain felony drug offenses, including marijuana offenses, to misdemeanors (Judicial Branch of California, 2020a). In 2016, California passed Proposition 64 which allowed individuals over the age of 21 to use and cultivate marijuana (Judicial Branch of California, 2020b). In Missouri, Senate Bill 491 was enacted in 2014 and took effect in 2017. Further, in 2013, St. Louis passed a local ordinance (Board Bill 275) which reduced possession of small amounts of marijuana from a misdemeanor offense to an ordinance violation.

Table 3. Percentage of Misdemeanor Enforcement by Charge[20]			
Jurisdiction	(Year) Charge Types at Start	(Year) Charge Types at Peak	(Year) Charge Types at End
New York City	(2000) Drug: 46% Person: 17% Property: 10%	(2010) Drug: 34% Person: 19% Property: 15%	(2017) Drug: 24% Person: 28% Property: 19%
Los Angeles	(2001) Drug: 10% Person: 10% Property: 10%	(2008) Drug: 10% Person: 6% Property: 7%	(2017) Drug: 12% Person: 12% Property: 11%
St Louis	(2002) Drug: 16% Person: 34% Property: 10%	(2002) Drug: 16% Person: 34% Property: 10%	(2017) Drug: 8% Person: 40% Property: 7%
Prince George's County	(2006) Drug: 17% Person: 21% Property: 26%	(2012) Drug: 26% Person: 26% Property: 21%	(2018) Drug: 20% Person: 21% Property: 26%
Durham	(2007) Drug: 8% Person: 20% Property: 21%	(2008) Drug: 10% Person: 20% Property: 24%	(2016) Drug: 6% Person: 32% Property: 21%
Seattle	(2008) Drug: 3% Person: 31% Property: 29%	(2010) Drug: 2% Person: 29% Property: 34%	(2016) Drug: <1% Person: 31% Property: 25%

Trends for property-related offenses were less consistent across jurisdictions. In some places like Los Angeles, Prince George's County, and Durham, property-related offenses were about the same at study beginning and study end although for the peak year the proportion fluctuated. For example, in Prince George's County, property charges began at 26% of misdemeanor arrests, declined to 21%, and then increased back to 26%. In New York City, property charges roughly doubled as a proportion from study beginning to end: 10% to 19%. In St. Louis, property charges declined slightly from study beginning to end starting at 10% and decreasing to 7%.

Finally, while these cross-site trends emerged, **the overall composition of arrests varied significantly across sites**. For example, although drug arrests decreased in both New York City and Seattle, drug arrests in general make up a much larger share of arrests in New York City than they do in Seattle. In New York City, at the beginning of the study period, drug arrests comprised 46% of all misdemeanor arrests while drug arrests in Seattle comprised 3% of misdemeanor arrests. Similarly, arrests for person-related offenses in St. Louis comprised between 34% to 40% of misdemeanor arrests while only comprising between 6% to 12% of misdemeanor arrests in Los Angeles. These variations serve as reminders that local contexts, practices, policies, and priorities play important roles in law enforcement. As jurisdictions take up reforms, they may be starting from very different places.

[20]Other charges such as vehicle and taffic-related charges and weapons charges are not included. Therefore, these proportions do not add up to 100%.

Supplementary Analyses

In addition to analyzing trends in misdemeanor arrest rates over time, by demographic groups, and by charge, researchers in some jurisdictions were able to conduct additional analyses, including analyses of intersectional trends (e.g., 21-24-year-old Black males) and trends in prosecution, resolution, and sentencing for misdemeanor arrests.

Intersectional Demographic and Charge Trends

Most jurisdictions were able to examine misdemeanor arrest rates at the intersection of demographic categories. For example, almost all sites analyzed the arrest rates of Black males as compared to other race-sex subcategories. **Across jurisdictions, Black males were arrested at the highest rates of any group, and White females generally the lowest**. Between these extremes were Black females and White males, who were often arrested at similar rates.

Five jurisdictions (Durham, Los Angeles, New York City, Prince George's County, and Seattle) broke down demographic groups even further, adding age to their race-sex analyses. When results were available, **young, Black males were overwhelmingly the group with the highest arrest rate**. While the definitions of "young" varied from jurisdiction to jurisdiction, these were generally males aged 18-20 or 21-24.

Further, some jurisdictions highlighted **distinctive local intersectional trends**. Although these analyses do not necessarily lend themselves to cross-site interpretation, they draw attention to ways that jurisdictions might conduct future analyses. **In Los Angeles, the arrest rate for Black females increased from study beginning to end, a trend counter to the overall pattern of declining arrest rates that was realized by nearly all other groups in other jurisdictions**. The increase for Black females was particularly stark for ages 21-24, and 25-34. For the 21-24-year-old group, this was associated with an increase in arrests for prostitution-related offenses, and for the 25-34-year old group, this was associated with an increase in loitering, trespassing, and disorder-related offenses.

Trends in Case System Processing and Outcomes Following Arrest

Some jurisdictions examined outcomes following misdemeanor arrests. Although an arrest is often the first point of contact with the criminal legal system for a given case, there are multiple decision points that occur following the arrest and that impact both criminal legal system trends and people's lives. Prosecutors decide which cases get filed, and judges and juries make decisions with regard to case resolution and sentencing. Los Angeles, Seattle, and New York City were able to explore trends in prosecution (charging), case resolution (disposition), and/or sentencing.

Researchers in Los Angeles and Seattle were able to examine **trends in the prosecution of misdemeanor arrests** (i.e., the decision by prosecutors to actually bring a case to court, following an arrest by police). **In both jurisdictions, roughly six out of ten misdemeanor arrests were prosecuted**. In Los Angeles, if cases were not filed, between 7% to 19% were diverted prior to filing and another 6% to 24% were rejected for other reasons. In Seattle, between 23% to 27% of cases were declined for prosecution.

In Los Angeles, New York City, and Seattle, researchers examined **case resolution, also referred to as disposition**. In Seattle and Los Angeles, the proportions of disposition outcomes were relatively stable over time. **Specifically, in Seattle the most common outcome for misdemeanor arrests over the study period was a dismissal**, which occurred between 32% to 50% of misdemeanor cases, and the second most common outcome was a conviction which occurred between 34% to 44% of misdemeanor cases. **In Los Angeles, the most common disposition for misdemeanor arrests was a guilty verdict or plea**, which together represented roughly half of all dispositions for misdemeanor arrests. Due to the structure of reporting, however, the proportion of dismissals cannot easily be understood.

In New York City, the most common case outcome changed from study start to study end. Misdemeanor convictions declined by about half from 27% to about 15%. Adjournment in contemplation of dismissal[21] was roughly the same proportion of misdemeanor case outcomes at study beginning and end (about 27%). Dismissals increased from about 10% to a little more than 15%. The remaining outcomes include infraction convictions[22] (ranging from 30% to 27%) and declined to prosecute (5% to 8%).

Finally, New York City and Los Angeles analyzed trends in **misdemeanor sentencing**, though researchers used slightly different analytic techniques. In New York City, the most common sentences following a misdemeanor arrest were a conditional discharge (the case is considered resolved upon completion of a court-mandated program) or time served (the court sentenced the individual to the amount of time already spent in custody during the pre-disposition period). However, over the study period, the relative proportion of these case outcomes changed. The proportion of cases sentenced to a conditional discharge decreased from about 50% to just over 40%, and the proportion sentenced to time served increased from about 20% to just under 30%. The proportion of misdemeanors resulting in jail sentences, a much less common sentence, also decreased from about 20% to about 14%. The Los Angeles researchers took a different approach and analyzed the severity of sentences for various types of sentencing outcomes. In Los Angeles, those sentenced for misdemeanor arrests were often sentenced to the least severe sentencing option – more than half of fines were for $0 and 50-60% of jail sentences were zero days. **In both jurisdictions, jail sentences were not given in over 50% of cases**.

[21]In New York State, a judge may dispose of a case as an adjournment in contemplation of dismissal or an "ACD" (CPL § 160.50). In such situations, the case can be dismissed after six or twelve months (the length of time is determined based on the charge and parameters set by the judge), as long as the individual is not arrested for a new offense during that time (New York Courts, 2018a).

[22]Under New York State criminal procedure law, violation or infraction convictions are not considered part of a person's criminal record and are sealed upon completion of a case. (New York Courts, 2018b).

Future Research On Misdemeanor Enforcement

This report presents a picture of how misdemeanor enforcement has been operating across a geographically diverse set of jurisdictions in recent years. However, this analysis marks only the beginning of understanding trends in misdemeanor arrests and raises many important questions going forward. **Future research should examine the causes, correlates, and consequences of misdemeanor enforcement on individuals and communities.** Research should also continue to examine how these trends as well as more nuanced trends have evolved over time and across more jurisdictions.

Research should examine the relationship between lower-level enforcement and public safety, individual and community well-being, and racial equity. Inquiry into whether enforcement of misdemeanor offenses and related arrests bears some relationship to crime rates can provide insights into effective and efficient ways to promote safe communities. Research should also explore the impacts that misdemeanor enforcement has on other indicators of community well-being, such as health outcomes, educational attainment, employment opportunities, and housing stability.

Future research should examine the impact of misdemeanor arrests and criminal legal system involvement on people's lives. For example, studies have shown that contact with the criminal legal system is negatively associated with a person's career, education, family, housing, health, mental health, and more (Comfort, 2016; Csete, 2010; Human Rights Watch, 2010; Open Society Foundations, 2011a; 2011b; Ortiz, 2015). Further, research also suggests that frequent interactions with the police may reduce the legitimacy of the criminal legal system in the eyes of those interacting with the police and thereby decrease the likelihood of reporting crimes or cooperating with the police (Tankebe, 2013). More expansive research will enable the public and policymakers to appropriately weigh perceived public safety benefits of misdemeanor arrests against the potential harms that criminal justice involvement inflicts on individuals and their communities.

While this report documents cross-site trends in misdemeanor enforcement, DCJ does not attempt to explain *why* misdemeanor arrests rates have changed. There are a range of possible explanations for these trends, including local events (e.g., high profile cases of police brutality, news reports, etc.), departmental policy changes, changes in levels of offending, decreases in crime reporting, changes in officer use of discretion, and legislative policy changes. Research has associated changes in law enforcement with changes in the availability of local community resources,[23] and variation in rates of drug use (Gaston, 2019; Petrocelli et al., 2014), poverty (Smith, 1986; Sun et al., 2008), and homelessness (Kleinig, 1993; McNamara et al., 2013). Further, local pre-arrest diversion programs and decriminalization policies often have the impact of reducing reported misdemeanor volume (Collins et al., 2017). Likely, the trends highlighted here can be explained by many of these factors—and by others not yet validated by empirical research. **Future research should attempt to understand causal mechanisms that contribute to trends and changes in misdemeanor arrests, including those that vary from jurisdiction to jurisdiction and those that are more consistent across the nation**.

Further, the most recent data from the Research Network jurisdictions is, at minimum, two years old. Policies change quickly, and the public needs access to up-to-date information. **Analyses of misdemeanor arrest trends must continue, and jurisdictions should strive to release their most recent data at regular intervals** to allow the public and policymakers to understand misdemeanor enforcement in near real-time. These analyses should be done in concert with historical analyses, to situate current rates within a longer-term context.

Even though this report and the Research Network reports begin to fill a critical gap regarding trends in misdemeanor arrests, the variation in definitions and available data raises another important point related to misdemeanor justice: a lack of consistent publicly available data. If more data on misdemeanor arrests was publicly available, it would be possible to understand misdemeanor arrest patterns in smaller cities, as well as in suburban and rural areas. For example, the New York report documents misdemeanor arrest trends throughout New York State, not just in New York City. The patterns in New York City and other cities through-

[23]Existing research suggests that enforcement levels may be related to budgetary allocations, personnel changes, and grant awards. For example, in their original report, St. Louis provides local political context for the years included in the study, which may have impacted enforcement activities (Slocum et al., 2018).

out the state are similar, but the patterns in the rural areas of the state differ. Thus, trends in different geographic areas may be moving in different directions based on jurisdiction size and type, **and more research is needed to assess enforcement trends in suburban and rural areas**. Further, regardless of the size or type of jurisdiction, adding more data to the public sphere will shed light on overall trends in misdemeanor arrests.

Even within a jurisdiction there is heterogeneity in misdemeanor enforcement practices. For example, although not discussed in this report, within-city analyses in the Research Network jurisdictions of Los Angeles, Seattle, and Durham revealed that arrest rates varied by intra-city regions, such as police districts or other community boundaries.[24] These analyses may illustrate meaningful variation in local norms or policing practices that merit further investigation.

Jurisdictions should continue to strive to analyze intersectional data. **Future research should investigate how arrest rates vary by combinations of race, sex, age, and charge categories.** As evidenced by some of the intersectional findings that the Research Network jurisdictions have already conducted, such analyses can lead to a more nuanced understanding of arrest practices and how law enforcement resources are allocated.

Jurisdictions should seek to analyze trends in criminal justice practices following arrest, such as trends in prosecution, case disposition, sentencing, and incarceration. Beyond providing critical information about outcomes of misdemeanor arrests themselves, understanding these trends may provide additional important insights into incarceration trends. Although significant investments have been made to reduce jail populations,[25] little empirical research has been conducted on the link between arrests and trends in incarceration. Where possible, linking misdemeanor arrest records to incarceration data will provide the fullest picture of the relationship between the two.

Finally, misdemeanor arrest charges and ordinance violations, such as moving violations or municipal offenses, can result in bench warrants, which are typically issued by courts when individuals do not adhere to mandates such as appearing in court related to the original arrest charge. Although warrants are more commonly issued for local law violations rather than for misdemeanor arrests, the fact remains that warrants can lead to future adverse criminal justice outcomes, such as jail time or future arrests for the existing warrant. Therefore, **future research should explore the relationship between bench warrants and misdemeanor arrest trends**. Moreover, local court context likely also has a strong upstream influence on bench warrant arrests, making it vital to understand the manner in which court practices and policies have an impact on arrests for bench warrants.

Conclusion

In this report, DCJ has documented that local misdemeanor enforcement patterns follow similar trajectories across jurisdictions – suggesting that state laws, local police decision-making, and local crime patterns are not the sole drivers of misdemeanor enforcement. Indeed, national trends with respect to the economy, housing, health care, and racial equity may also play a significant role in the trends discussed in this report. However, additional research is needed to further unpack the misdemeanor enforcement trends presented here. For its part, DCJ will continue to work with the Research Network on Misdemeanor Justice to expand the public's understanding of the role that misdemeanor enforcement plays in community safety, health, and well-being.

[24]Although not included in their original report, researchers in St. Louis also analyzed variability across Census block groups (Slocum et al., 2020).

[25]For example, the John D. and Catherine T. MacArthur Foundation has funded over $200 million nationwide to reduce jail populations. See http://www.safetyandjusticechallenge.org/.

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Appendix A: Research Network on Misdemeanor Justice Reports List

Throughout this report, we reference the original reports produced by the partner jurisdictions of the Research Network on Misdemeanor Justice. Please refer to these original reports for more details regarding trends in each site.

Helfgott, J. B., Parkin, W., Fisher, C., Morgan, L., & Kaur, S. (2018). Trends in misdemeanor arrests, referrals, & charges in Seattle, WA.

Kozlowski-Serra, M., Smith, J., Glazener, E., Mitchell, J. & Lynch, J. (2019). Tracking enforcement rates in Prince George's County, MD, 2006-2018. College Park, MD.

Lens, M., Stoll, M., & Kuai, Y. (2019). Trends in misdemeanor arrests in Los Angeles: 2001-2017. Los Angeles, CA.

Patten, M., Hood, Q.O., Low-Weiner, C., Lu, O., Bond, E., Hatten, D., & Chauhan, P. (2018). Trends in misdemeanor arrests in New York, 1980 to 2017. New York, NY.

Schaefer, B. P., Hughes, T. W., & Jude, D. (2018). Tracking enforcement rates in Louisville 2009 - 2016. Louisville, KY.

Slocum, L. A., Huebner, B. M., Rosenfeld, R., & Greene, C. (2018). Tracking enforcement rates in the City of St. Louis, 2002-2017. St. Louis, MO.

Taylor, L. C., Moore, K. L., Brown, R. A., Troy, B. N., & Schiess, J. (2019). Misdemeanor arrest trends in the City of Durham, North Carolina, 2007-2016. Durham, NC.

Appendix B: Data Definitions & Limitations

From all corners of the country, the Research Network jurisdictions provide geographic diversity that allows for an understanding of misdemeanor arrest practices beyond a single jurisdiction or area of the nation. Despite an attempt to represent a variety of localities, the Research Network jurisdictions are mostly urban, and therefore this analysis is limited in its generalizability to non-urban areas.

Due to heterogeneity in data composition and availability, DCJ was unable to apply the same inclusion criteria to each jurisdiction's data in this cross-site analysis. Therefore, when grappling with how to synthesize the existing findings of the Research Network reports, DCJ made decisions about how to make the most accurate and valid comparisons. Below are descriptions of decisions and approaches used to standardize categories and improve comparability across sites.

Misdemeanor arrests: Based on available data, local laws, and enforcement practices, there was variety with respect to the types of enforcement activities that each jurisdiction categorized as a misdemeanor arrest. For example, the New York City report counts both custodial arrests and cite-and-release arrests (known locally as Desk Appearance Tickets), while other sites disaggregate cite-and-release arrests into a category separate from misdemeanor arrests. Further, Louisville counted all charges while others used an incident-level approach. In addition, misdemeanor arrests do not include local law violations or ordinance violations for all jurisdictions.

Time periods: Each jurisdiction used the most reliable data available for their reports. For example, New York City researchers were able to draw on data as early as the 1980s, whereas partners in Seattle had access to reliable data beginning in 2008. Additionally, some jurisdictions were able to analyze trends that included more recent years, as late as 2018, whereas other partners either released their reports earlier and showed the most recent year or were only able to access reliable data through 2015. However, the study period for each report is at least eight years.

Charges: Penal law definitions vary widely from state to state. A misdemeanor charge in one jurisdiction may be a felony charge in a second and a violation in a third. This is particularly true for drug offenses, but applies to all categories of offenses. Notably, all of these offense categories can also include charges of attempted unlawful actions.

For example, the specific dollar amounts that are considered to be misdemeanor "larceny" may vary from one jurisdiction to another (e.g., less than $750 in Seattle compared to less than $1,000 in New York City). Therefore, local researchers used local penal law codes when analyzing their data and the actual behaviors categorized as misdemeanors vary across jurisdictions.

Additionally, within a jurisdiction, statutes — and particularly drug offenses — changed over time. In some jurisdictions, as in St. Louis (Bott, 2017) and Los Angeles (Judicial Branch of California, 2020a), certain drug offenses were downgraded from felony to misdemeanor charges. In other instances, as in Seattle (Washington State Liquor and Cannabis Board, 2020) and Los Angeles (Proposition 64), statutes decriminalized certain uses of recreational marijuana. Therefore, counts and rates are as-of the year they occurred and correspond with criminal law as it existed at that time. This further complicates interpretation of changes in charge types over time.

Further, each jurisdiction conducted their charge-based analyses with varying degrees of specificity. For example, Prince George's County and Durham grouped specific penal law codes into broad categories like "person" and "property" charges, while St. Louis focused on specific charges like "assault" and "trespassing." The cross-site analysis of charges relies on the most closely related categories reported in the original reports (e.g., St. Louis's "assault" compared with Durham's "person").

This cross-site report analyzes trends based on the "beginning" and "end" year of each jurisdiction's study period, with the understanding that each milestone may be represented by a different year for each site. We also examine the "peak" year in each jurisdiction, meaning the year during which the arrest rate was highest

over the study period in that locality. This milestone also may have occurred in different years for different jurisdictions.

Race/ethnicity: Some jurisdictions were able to disaggregate race from ethnicity while others were not. Further, in some jurisdictions, researchers were able to access more nuanced race and/or ethnicity categories than in others. For instance, categories for Los Angeles were Non-Hispanic Black, Hispanic, Non-Hispanic White, and Other, while categories for Seattle were Black, White, Asian, and Indigenous.

Age: Age categories varied based on data availability and age of criminal responsibility. In New York City and Durham, the age of criminal responsibility for the study period was 16 and therefore their reports included 16- and 17-year-olds. Further, the various jurisdictions defined age categories slightly differently. For example, in their report, St. Louis's age categories were 17-20, 21-24, 25-34, 35+ while New York City's were 16-17, 18-20, 21-24, 25-34, 35-65.

For age comparisons in this report, we use the online dashboard data, rather than the original reports, because this data allows us to standardize age categories to 18-20, 21-24, 25-34, and 35+ or 35-65.

DATA COLLABORATIVE FOR JUSTICE

AT JOHN JAY COLLEGE

STRENGTH IN NUMBERS

JOHN JAY COLLEGE OF CRIMINAL JUSTICE

A large-scale analysis of racial disparities in police stops across the United States*

Emma Pierson
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Abstract

To assess racial disparities in police interactions with the public, we compiled and analyzed a dataset detailing over 60 million state patrol stops conducted in 20 U.S. states between 2011 and 2015. We find that black drivers are stopped more often than white drivers relative to their share of the driving-age population, but that Hispanic drivers are stopped less often than whites. Among stopped drivers—and after controlling for age, gender, time, and location—blacks and Hispanics are more likely to be ticketed, searched, and arrested than white drivers. These disparities may reflect differences in driving behavior, and are not necessarily the result of bias. In the case of search decisions, we explicitly test for discrimination by examining both the rate at which drivers are searched and the likelihood searches turn up contraband. We find evidence that the bar for searching black and Hispanic drivers is lower than for searching whites. Finally, we find that legalizing recreational marijuana in Washington and Colorado reduced the total number of searches and misdemeanors for all race groups, though a race gap still persists. We conclude by offering recommendations for improving data collection, analysis, and reporting by law enforcement agencies.

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1 Introduction

More than 20 million Americans are stopped each year for traffic violations, making this one of the most common ways in which the public interacts with the police (Langton and Durose, 2013). Due to a lack of comprehensive data, it has been difficult to rigorously assess the manner and extent to which race plays a role in traffic stops (Epp et al., 2014). The most widely cited national statistics come from the Police-Public Contact Survey (PPCS), which is based on a nationally representative sample of approximately 50,000 people who report having been recently stopped by the police (Bureau of Justice Statistics, 2014). In addition to such survey data, some local and state agencies have released periodic reports on traffic stops in their jurisdictions, and have also made their data available to researchers for analysis (Antonovics and Knight, 2009; Anwar and Fang, 2006; Hetey et al., 2016; Ridgeway, 2006; Ridgeway and MacDonald, 2009; Rojek et al., 2004; Ryan, 2016; Seguino and Brooks, 2017; Simoiu et al., 2017; Smith and Petrocelli, 2001; Voigt et al., 2017; Warren et al., 2006). While useful, these datasets provide only a partial picture. For example, there is concern that the PPCS, like nearly all surveys, suffers from selection bias and recall errors. Data released directly by police departments are potentially more complete, but are available only for select agencies, are typically limited in what is reported, and are inconsistent across jurisdictions.

Here we analyze a unique dataset detailing more than 60 million state patrol stops conducted in 20 states between 2011 and 2015. We compiled this dataset through a series of public records requests filed with all 50 states, and we are redistributing these records in a standardized form to facilitate future analysis. Our statistical analysis of these records proceeds in three steps. First, we quantify racial disparities in stop rates and post-stop outcomes. Adjusting for age, gender, location and year, we find that black drivers are stopped more often than white drivers relative to their share of the driving-age population, but find that Hispanic drivers are stopped less often than whites. After being stopped, black and Hispanic drivers are more likely than whites to be ticketed, searched, and arrested. Such disparities may stem from a combination of factors—including differences in driving behavior—and are not necessarily the result of racial bias. In the second phase of our analysis, we investigate the degree to which these differences may result from discrimination, focusing on search decisions. By examining both the rate at which searches occur and the success rate of these searches, we find evidence that the bar for searching black and Hispanic drivers is lower than for searching white drivers. Finally, we examine the effects of drug policy on stop outcomes. We find that legalizing recreational marijuana in Washington and Colorado reduced both search and misdemeanor rates for white, black, and Hispanic drivers, though a relative gap persists. We conclude by suggesting best-practices for data collection, analysis, and reporting by law enforcement agencies.

2 Compiling a national database of traffic stops

2.1 Data collection

To assemble a national dataset of traffic stops, we first identified which state law enforcement agencies electronically maintain traffic stop records that, at a minimum, include the race of the stopped driver. Of the 50 state agencies, 7 did not respond to our request for information or did not disclose whether any data were collected; an additional 9 agencies do not compile stop records electronically or reported that they were unable to send their data to us in electronic form; and 3 state agencies keep electronic records but do not track the race of stopped drivers (see Table A1 for details). For the remaining 31 states, we filed public records requests for detailed information

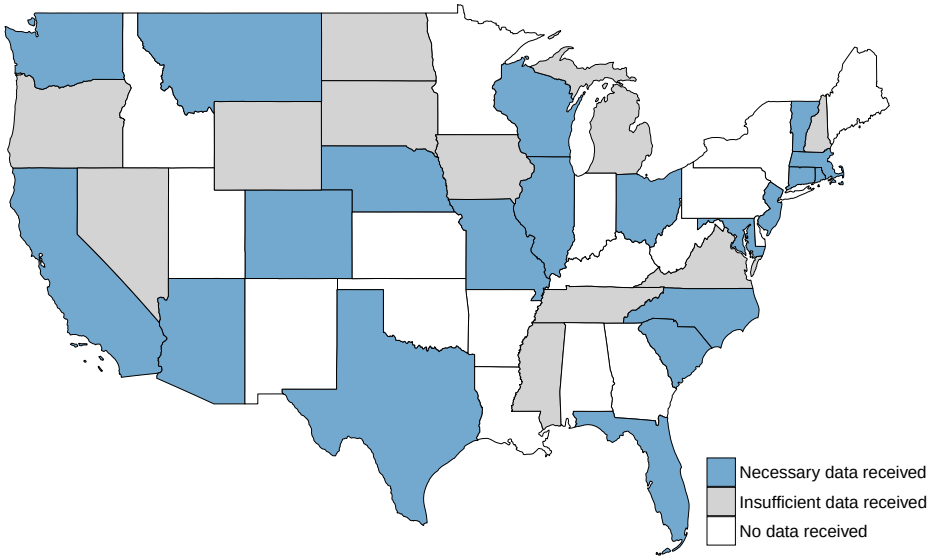

Figure 1: *We collected detailed information on over 60 million state patrol stops conducted in 20 states between 2011 and 2015. An additional 11 states provided data that are insufficient to assess racial disparities, and 19 states have not provided any data (including Hawaii and Alaska).*

on each stop conducted since 2005.

To date, we have collected data on approximately 136 million state patrol stops in 31 states. Of these, we exclude 11 states from our analysis because the obtained data were insufficient to assess racial disparities (e.g., the race of the stopped driver was not regularly recorded, or only a non-representative subset of stops was provided). In the remaining 20 states that we consider, 18 provided data for each individual stop. In the other two—Missouri and Nebraska—only summary data were provided, but these summaries were sufficiently granular to allow for statistical analysis. For consistency in our analysis, we restrict to stops occurring in 2011–2015, as many states did not provide data on earlier stops. We also limit our analysis to drivers classified as white, black or Hispanic, as there are relatively few recorded stops of drivers in other race groups. Our primary dataset thus consists of 63.7 million state patrol stops from 20 states (Figure 1).

2.2 Data normalization

Each state provided the stop data in idiosyncratic formats with varying levels of specificity, and so we used a variety of automated and manual procedures to create the final dataset. For each recorded stop, we attempted to extract and normalize the date and time of the stop; the county or state patrol district in which the stop took place; the race, gender and age of the driver; the stop reason; whether a search was conducted; the legal justification for the search (e.g., "probable cause" or "consent"); whether contraband was found during a search; and the stop outcome (e.g., a citation or an arrest). We describe our procedures for normalizing each of these covariates in the Appendix. As indicated in Table 1, the availability of information varies significantly across states. We therefore restrict each of our specific analyses to the corresponding subset of states for which we have the required fields.

In many states, more than one row in the raw data appeared to refer to the same stop. For example, in several states each row referred to one *violation*, not one stop. We detected and

	State	Stops	Time Range	Stop Date	Stop Time	Stop Location	Driver Gender	Driver Age	Stop Reason	Search Conducted	Search Type	Contraband Found	Stop Outcome
1	Arizona	2,039,781	2011-2015	•	•	•	•			•		•	•
2	California	19,012,414	2011-2015	•		•	•		•	•	•		•
3	Colorado	1,674,619	2011-2015	•	•	•	•	•	•	•	•	•	
4	Connecticut	310,969	2013-2015	•	•	•	•	•	•	•	•	•	•
5	Florida	4,002,547	2011-2015	•	•	•	•	•	•	•	•		•
6	Illinois	1,528,340	2011-2015	•	•	•	•	•	•	•		•	•
7	Maryland	578,613	2011-2014				•		•	•	•	•	•
8	Massachusetts	1,773,546	2011-2015	•		•	•	•		•	•	•	•
9	Missouri	1,906,797	2011-2015							•		•	
10	Montana	547,115	2011-2015	•	•	•	•	•	•	•	•		•
11	Nebraska	840,764	2011-2014							•			
12	New Jersey	2,069,123	2011-2015	•	•	•	•		•				•
13	North Carolina	3,500,180	2011-2015	•		•	•	•	•	•	•	•	•
14	Ohio	4,660,935	2011-2015	•	•	•	•			•			
15	Rhode Island	229,691	2011-2015	•	•	•	•	•	•	•	•	•	•
16	South Carolina	3,696,801	2011-2015	•		•	•	•		•		•	•
17	Texas	10,239,721	2011-2015	•	•	•	•		•	•	•	•	•
18	Vermont	250,949	2011-2015	•	•	•	•	•	•	•	•	•	•
19	Washington	4,053,099	2011-2015	•	•	•	•	•	•	•	•	•	•
20	Wisconsin	827,028	2011-2015	•	•	•	•		•	•	•	•	•
	Total	**63,743,032**											

Table 1: *Availability of data in the 20 states comprising our primary analysis, where for each column a solid circle signifies data are available for at least 70% of the stops. For all states except Illinois, North Carolina, and Rhode Island, "stop location" refers to county; for these three states, it refers to a similarly granular location variable, as described above.*

reconciled such duplicates by inspecting columns with granular values. For example, in Colorado we counted two rows as duplicates if they had the same officer identification code, officer first and last name, driver first and last name, driver birth date, stop location (precise to the milepost marker), and stop date and time (precise to the minute).

2.3 Error correction

The raw data in many states contain errors. We ran numerous automated checks to detect and correct these where possible, although some errors likely remain due to the complex nature of the data. For example, after examining the distribution of recorded values in each state, we discovered a spurious density of stops in North Carolina listed as occurring at precisely midnight. As the value "00:00" was likely used to indicate missing information, we treated it as such.

Past work suggests that Texas state patrol officers incorrectly recorded many Hispanic drivers as white.[1] To investigate and correct for this issue, we impute Hispanic ethnicity from surnames in the three states for which we have name data: Texas, Arizona, and Colorado. To do so, we use a dataset from the U.S. Census Bureau that estimates the racial and ethnic distribution of people with a given surname, for surnames occurring at least 100 times (Word et al., 2008).[2] To increase the matching rate, we perform minor string edits to the names, including removing punctuation and suffixes (e.g., "Jr." and "II"), and consider only the longest word in multi-part surnames. Following past work (Melendres v. Arpaio, 2009; Word and Perkins, 1996), we define a name as "Hispanic-affiliated" if at least 75% of people with that name identify as Hispanic, according to the 2000 Census; we note that 90% of those with Hispanic-affiliated names identify as Hispanic. Among drivers with Hispanic-affiliated names, the proportion labeled as Hispanic in the raw data is considerably lower in Texas (37%) than in either Arizona (79%) or Colorado (70%), corroborating past results. Though imperfect, we re-categorize as "Hispanic" all drivers in Texas with Hispanic-affiliated names who were originally labeled "white" or had missing race data.

Our complete data cleaning pipeline is extensive, requiring subjective decisions and thousands of

[1] See: `http://kxan.com/investigative-story/texas-troopers-ticketing-hispanics-motorists-as-white/`

[2] `http://www.census.gov/topics/population/genealogy/data/2000_surnames.html`

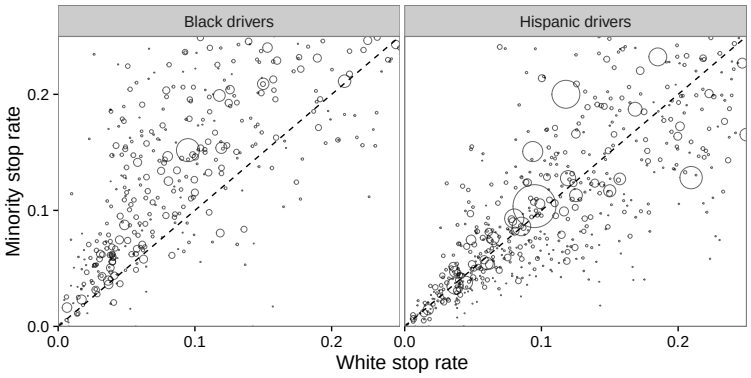

Figure 2: *Stops per person of driving age, stratified by race and location, where points are sized proportional to the number of stops. The plots cover 16 states for which we have location data. Within location, black drivers are often stopped more often than white drivers; Hispanic drivers are generally stopped at similar rates as whites.*

lines of code. For transparency and reproducibility, we have released the raw data, the standardized data, and code to clean and analyze the records at `https://openpolicing.stanford.edu`.

3 Stop rates and post-stop outcomes

We begin our analysis by examining the extent to which there are racial disparities in stop, citation, search, and arrest rates. The disparities we discuss below likely result from a combination of complex factors, and do not necessarily reflect racial bias. Regardless of the mechanism, however, we quantify these disparities in order to better understand the differential impact policing has on minority communities.

3.1 Stop rates

We first estimate the rate at which white, black, and Hispanic drivers are stopped, relative to their share of the driving-age population (Smith and Petrocelli, 2001). Although there are a variety of benchmarks one might consider (Alpert et al., 2004; Engel and Calnon, 2004; Lange et al., 2005), the driving-age population has the unique distinction of being readily available in nearly every jurisdiction, and it is accurately estimated by the U.S. Census Bureau;[3] we note, however, that this benchmark does not account for possible race-specific differences in driving behavior, including amount of time spent on the road and adherence to traffic laws.

Figure 2 shows stop rates of black and Hispanic drivers relative to whites, disaggregated by location. Each point in the plot corresponds to either the county or similar geographic unit in which the stop was made. We find that Hispanics are stopped at similar rates as whites in most jurisdictions; black drivers, however, are stopped more often than whites in over 80% of the locations we consider.

We next estimate race-specific stop rates after adjusting for driver demographics (age and gender), stop location, and stop year; age was binned into the categories 16–19, 20–29, 30–39, 40–49, and 50+ years-old. In our primary analysis, we fit a negative binomial regression, where we

[3]We use the intercensal estimates produced by the U.S. Census Bureau, available at `https://www2.census.gov/programs-surveys/popest/datasets/2010-2015/counties/asrh/cc-est2015-alldata.csv` or from our Open Policing website.

	Stop	Citation	Search	Consent search	Arrest
Black	0.37 (0.01)	0.18 (0.00)	0.73 (0.01)	0.77 (0.03)	0.65 (0.01)
Hispanic	-0.40 (0.01)	0.29 (0.00)	0.54 (0.01)	0.62 (0.02)	0.69 (0.01)
Male	0.72 (0.00)	0.08 (0.00)	0.58 (0.01)	0.86 (0.02)	0.43 (0.01)
Age 20-29	0.65 (0.01)	-0.13 (0.01)	0.13 (0.01)	-0.38 (0.03)	0.38 (0.01)
Age 30-39	0.47 (0.01)	-0.35 (0.01)	-0.06 (0.01)	-0.79 (0.03)	0.30 (0.01)
Age 40-49	0.25 (0.01)	-0.47 (0.01)	-0.37 (0.01)	-1.20 (0.04)	-0.04 (0.01)
Age 50+	-0.53 (0.01)	-0.68 (0.01)	-0.80 (0.01)	-1.82 (0.04)	-0.47 (0.01)

Table 2: *Coefficients and standard errors for stop rate and post-stop outcome models.*

benchmark to the census-estimated driving-age population:

$$y_{rag\ell y} \sim \text{NegBin}\left(n_{rag\ell y} e^{\mu+\alpha_r+\beta_a+\gamma_g+\delta_\ell+\epsilon_y}, \phi\right)$$

where $y_{rag\ell y}$ is the observed number of stops in a group defined by race, age, gender, location, and year, $n_{rag\ell y}$ is the corresponding census benchmark, and α_r are the key race coefficients (we set $\alpha_{\text{white}} = 0$). The negative binomial distribution is parameterized such that if $Y \sim \text{NegBin}(\mu, \phi)$, then $\mathbb{E}[Y] = \mu$ and $\text{Var}[Y] = \mu + \mu^2/\phi$. The parameter ϕ allows for overdispersion, and is estimated from the data.

Table 2 (first column) shows the estimated race, gender and age coefficients; we further estimate $\hat{\phi} = 3.9$. After controlling for gender, age, location, and year, we find that blacks are stopped at 1.4 times the rate at which whites are stopped ($e^{0.37} = 1.4$), and Hispanics are stopped at 0.7 times the white stop rate ($e^{-0.40} = 0.7$). To help interpret these numbers, Table 3 shows stop rates for a typical 20-29 year-old male driver: the per-capita stop rate is 0.42 for blacks, 0.29 for whites, and 0.19 for Hispanics.

As shown in Figure 2, Hispanic drivers are stopped at similar rates as whites when controlling only for location. But Hispanic drivers are more likely to be young, and young drivers are more likely to be stopped. As a result, after additionally adjusting for age (and other covariates) in the regression above, we find Hispanics are stopped at a lower rate than whites. This lower estimated rate is consistent with self-reports collected as part of the PPCS (Bureau of Justice Statistics, 2014). With the PPCS data, we used logistic regression to estimate the likelihood a respondent would report having been stopped by the police while driving, where we controlled for the respondent's race, age, gender, and size of city. We found that Hispanic respondents were less likely than white drivers to report having been stopped (odds ratio = 0.85). This result is in line with a similar analysis of the same PPCS data (Medina Jr, 2016).

To check the robustness of the observed racial disparities, we additionally fit stop rate regressions using a Poisson model with sandwich errors, and using a quasi-Poisson model (Gardner et al., 1995; Ver Hoef and Boveng, 2007). We report these results in Table A3 (first three rows). The signs of the race coefficients are the same under all three specifications, but the estimated effect sizes are somewhat larger in the negative binomial model than in the two Poisson models (both of which necessarily yield identical coefficients). We note that it is common for Poisson and negative binomial formulations to produce somewhat different effect sizes (Ver Hoef and Boveng, 2007).

3.2 Citation, search, and arrest rates

Stop rates are a natural starting point but are inherently difficult to interpret, in part because results can be sensitive to the benchmark used. (We note that there are no readily available

	White	Black	Hispanic
Stop rate	0.29	0.42	0.19
Speeding citation	72%	75%	77%
Search	1.3%	2.7%	2.3%
Consent search	0.1%	0.3%	0.3%
Arrest	2.8%	5.3%	5.5%

Table 3: *Model-estimated rates for a typical 20-29 year-old male. The "speeding citation" outcome corresponds to receiving a citation rather than a warning (or no penalty) when pulled over for speeding. Negative binomial regression is used for stop rate (first row), benchmarked to the driving-age population; logistic regression is used for all other analyses. The stop rate regression includes controls for age, gender, stop location, and stop year; all other regressions additionally include controls for stop quarter, weekday, and hour (binned into three-hour segments).*

alternatives to the driving-age population.) We thus now consider post-stop outcomes, starting with the rates at which white and minority drivers receive citations rather than warnings when pulled over for speeding.

We use logistic regression to estimate racial disparities in the probability a driver stopped for speeding is given a citation as opposed to a warning (or no penalty at all). In addition to driver age and gender, location, and year, we control for stop quarter, stop weekday, and stop hour, binned into eight 3-hour segments. (In the case of stop rates, we used negative binomial and Poisson models since we were estimating total counts; we could not control for time in that case because we lacked time-specific population benchmarks.) Table 2 (second column) shows the estimated race, gender and age coefficients. We find that black drivers have 19% higher odds of receiving a citation than white drivers, and Hispanics have 34% higher odds than whites. For typical young male drivers, Table 3 shows that 72% of whites stopped for speeding receive a citation, compared to 75% and 77% for black and Hispanic drivers, respectively.

Next, we examine search rates. After stopping a driver, officers may search both driver and vehicle for drugs, weapons, and other contraband when they suspect more serious criminal activity. Aggregating across all states for which we have search data, white drivers are searched in 2.0% of stops, compared to 3.5% of stops for black motorists and 3.8% for Hispanic motorists. Across jurisdiction, Figure 3 (top row) shows that black and Hispanic motorists are consistently searched at higher rates than white drivers. After controlling for stop location, date and time, and driver age and gender—via logistic regression, as above—we find that black and Hispanic drivers have approximately twice the odds of being searched relative to white drivers (2.1 and 1.7, respectively, as shown in Table 2).

We now consider the subset of searches conducted with consent, where officers must seek permission from drivers to search their vehicles. (In contrast, *probable cause* searches do not require consent, but legally demand a high standard of evidence.[4]) We find that minority drivers are more likely than whites to undergo consent searches in the seven states for which we have reliable data (Colorado, Florida, Massachusetts, Maryland, North Carolina, Texas, and Washington); controlling for stop location, date and time, and driver age and gender, we find that black drivers have 2.2 times the odds of whites and Hispanic drivers have 1.9 times the odds of whites of undergoing a consent search (Table 2).

[4]Officers may also conduct *protective frisks* to search for weapons, a type of search that legally requires only *reasonable suspicion*, a lower standard of evidence than probable cause. In our dataset, protective frisks occur much less frequently than probable cause and consent searches.

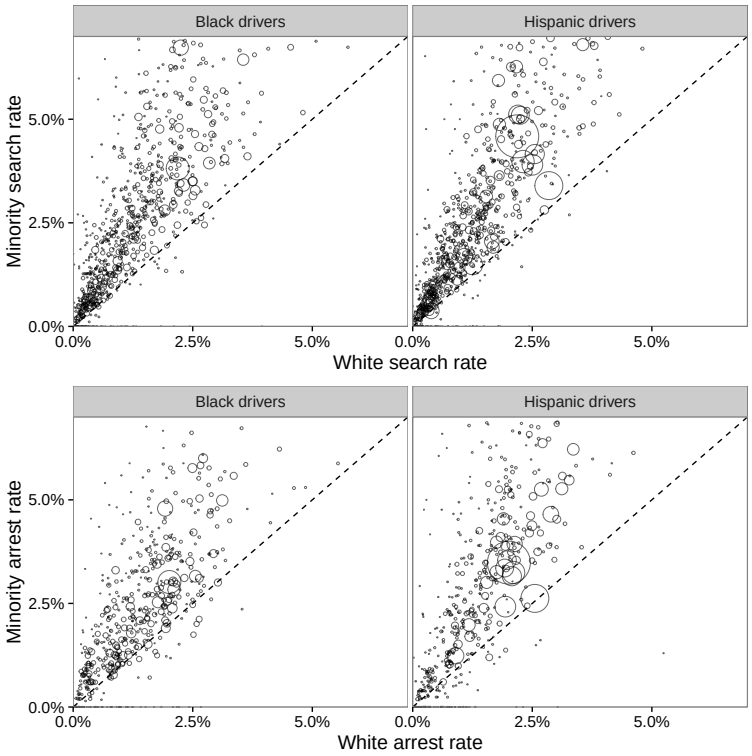

Figure 3: *Search rates (top) and arrest rates (bottom) by race and location among stopped drivers. In nearly every area, minorities are searched and arrested more often than whites. The search data cover 16 states, comprising a total of 56 million stops, and the arrest data include 40 million stops in 13 states.*

Finally, we examine arrest rates. In aggregate, black drivers are arrested in 2.8% of stops and Hispanic drivers in 3.4% of stops, compared to 1.7% for white drivers. Again controlling for driver age and gender, stop date and time, and stop location, we find that black drivers have 1.9 times the odds of being arrested, and Hispanic drivers have 2.0 times the odds of being arrested compared to white drivers (Figure 3 and Table 2).

To assess the robustness of our results on citation, search, and arrest rates, we fit logistic regression models with five different sets of control variables, as described in Table A3: (1) driver race only; (2) driver race and county; (3) driver race, age, gender, and county; (4) driver race, county, and stop time; and (5) driver race, age, gender, county, and stop time. These five models were fit on the largest set of states for which the relevant information was available. In nearly every case, the estimated race coefficients were positive and significant, indicating that black and Hispanic drivers were cited, searched, and arrested more often than white drivers. There was one exception: we found a negative coefficient (-0.11) for Hispanic drivers when estimating the likelihood of receiving a speeding citation when controlling only for race. This outlier occurs because Texas has an especially high fraction of Hispanic drivers and an especially low rate of citations. Finally, we confirmed that these racial disparities persist when we alter the set of stops analyzed: we find qualitatively similar results when we fit our models only on speeding stops; when we eliminate searches incident to arrest; and when we fit models on each state separately.

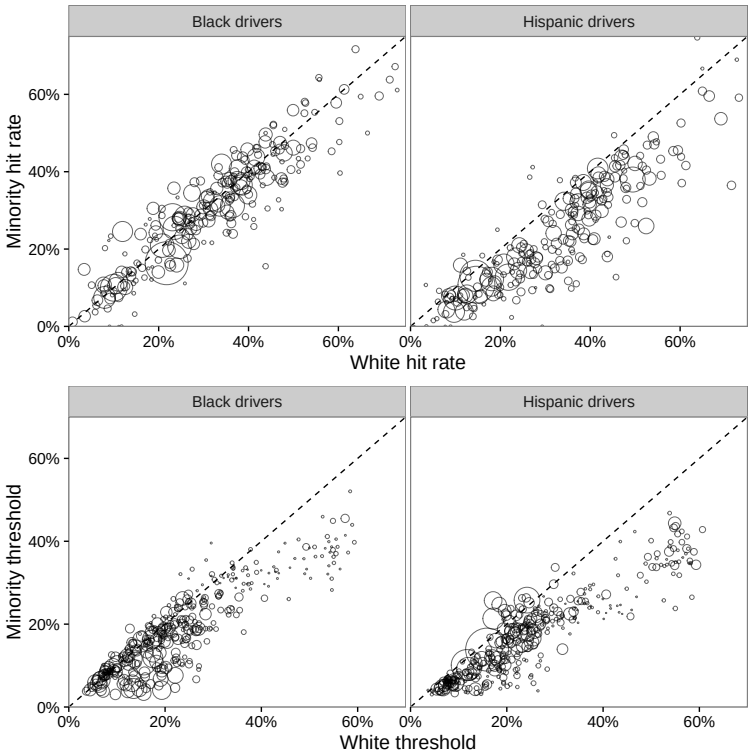

Figure 4: *Hit rates (top) and inferred search thresholds (bottom) by race and location, covering 470,000 searches in 9 states. Across locations, the inferred thresholds for searching black and Hispanic drivers are typically lower than those for searching white drivers. Despite these lower inferred search thresholds, hit rates for blacks are comparable to hit rates for whites, possibly due to the problem of* infra-marginality *in outcome tests.*

4 Testing for bias in search decisions

When stopped, black and Hispanic drivers are more likely to be issued citations, more likely to be searched, and more likely to be arrested. These disparities, however, are not necessarily the product of discrimination. Minority drivers might, for example, carry contraband at higher rates than whites, and so elevated search rates may result from routine police work. We now investigate whether bias plays a role in search decisions, a class of actions amenable to statistical analysis.

4.1 The outcome test

To start, we apply the *outcome test*, originally proposed by Becker (1957, 1993) to circumvent omitted variable bias in traditional tests of discrimination. The outcome test is based not on the search rate, but on the *hit rate*: the proportion of searches that successfully turn up contraband. Becker argued that even if minority drivers are more likely to carry contraband, absent discrimination searched minorities should still be found to have contraband at the same rate as searched whites. If searches of minorities are less often successful than searches of whites, it suggests that officers are applying a double standard, searching minorities on the basis of less evidence.

In Figure 4 (top row), we plot hit rates by race and location for the nine states (Colorado, Connecticut, Illinois, North Carolina, Rhode Island, South Carolina, Texas, Washington, and Wisconsin) for which we have the necessary information: the race of the driver, the location of the

stop, whether a search was conducted, and whether contraband was found.[5] Across jurisdictions, we consistently see that searches of Hispanic drivers are less successful than those of white drivers. However, searches of white and black drivers generally have comparable hit rates. Aggregating across location, searches of Hispanic drivers yield contraband 22% of the time, compared to 28% for searches of white and black drivers. In computing these aggregate statistics, we include Missouri and Maryland, which provide search and contraband data but not stop location, and Vermont, which has too few stops of minorities to be included in our county-level analysis in Figure 4. The outcome test thus indicates that search decisions may be biased against Hispanic drivers but not black drivers.

4.2 The threshold test

The outcome test is intuitively appealing, but it is not a perfect barometer of bias; in particular, it suffers from the problem of *infra-marginality* (Anwar and Fang, 2006; Ayres, 2002). To illustrate this shortcoming, suppose that there are two, easily distinguishable types of white drivers: those who have a 5% chance of carrying contraband, and those who have a 75% chance of carrying contraband. Likewise assume that black drivers have either a 5% or 50% chance of carrying contraband. If officers search drivers who are at least 10% likely to be carrying contraband, then searches of whites will be successful 75% of the time whereas searches of blacks will be successful only 50% of the time. Thus, although the search criterion is applied in a race-neutral manner, the hit rate for blacks is lower than the hit rate for whites, and the outcome test would (incorrectly) conclude searches are biased against black drivers. The outcome test can similarly fail to detect discrimination when it is present.

To mitigate this limitation of outcome tests, the *threshold test* has been proposed as a more robust means for detecting discrimination (Pierson et al., 2017; Simoiu et al., 2017). This test aims to estimate race-specific probability thresholds above which officers search drivers—for example, the 10% threshold in the hypothetical situation above. Even if two race groups have the same observed hit rate, the threshold test may find that one group is searched on the basis of less evidence, indicative of discrimination. To accomplish this task, the test simultaneously estimates race-specific search thresholds and risk distributions that are consistent with the observed search and hit rates across all jurisdictions. The threshold test can thus be seen as a hybrid between outcome and benchmark analysis.

Here we present a brief overview of the threshold test as applied in our setting; see Simoiu et al. (2017) for a more complete description. For each stop i, we assume that we observe: (1) the race of the driver, r_i; (2) the stop location, d_i; (3) whether the stop resulted in a search, indicated by $S_i \in \{0, 1\}$; and (4) whether the stop resulted in a hit, indicated by $H_i \in \{0, 1\}$. We applied the threshold test separately on each state having the requisite data, and limited to stop locations (e.g., counties) with at least 1,000 stops. If more than 100 locations in a state had over 1,000 stops, we considered only the 100 locations with the most stops.

The threshold test is based on a stylized model of officer behavior. During each stop, officers observe a myriad of contextual factors—including the age and gender of the driver, the stop time and location, and behavioral indicators of nervousness or evasiveness. We assume that officers distill these factors down to a single number that represents the likelihood the driver is carrying contraband, and then conduct a search if that probability exceeds a fixed race- and location-specific threshold. Since there is uncertainty in who is pulled over in any given stop, the probability of

[5] This information is also available for Vermont, but because of the state's demographic composition, very few minorities are searched in any given county, and we thus exclude it from this analysis.

finding contraband is modeled as a random draw from a race- and location-specific *signal* distribution. The threshold test jointly estimates these search thresholds and signal distributions using a hierarchical Bayesian model, as described below. Under this model, lower search thresholds for one group relative to another are interpreted as evidence of *taste-based* discrimination (Becker, 1957).

Formally, for each stop i, we assume (S_i, H_i) is stochastically generated in three steps.

1. Given the race r_i of the driver and the stop location d_i, the officer observes a signal $p_i \sim \text{beta}(\phi_{r_i d_i}, \lambda_{r_i d_i})$, where $\phi_{r_i d_i}$ and $\lambda_{r_i d_i}$ are defined by:

$$\phi_{rd} = \text{logit}^{-1}(\phi_r + \phi_d)$$

and

$$\lambda_{rd} = \exp(\lambda_r + \lambda_d).$$

The beta distribution is parameterized by its mean ϕ_{rd} and total count parameter λ_{rd}. In terms of the standard count parameters α and β of the beta distribution, $\phi = \alpha/(\alpha+\beta)$ and $\lambda = \alpha + \beta$. Thus, ϕ_{rd} is the overall probability that a stopped driver of race r in location d has contraband, and λ_{rd} characterizes the heterogeneity of guilt across stopped drivers of that race in that location. These parameters of the beta distributions are in turn functions of parameters that depend separately on race and location.

2. $S_i = 1$ (i.e., a search is conducted) if and only if $p_i \geq t_{r_i d_i}$. The thresholds t_{rd} are the key parameters of interest.

3. If $S_i = 1$, then $H_i \sim \text{Bernoulli}(p_i)$; otherwise $H_i = 0$.

This generative process is parameterized by $\{\phi_r\}$, $\{\lambda_r\}$, $\{\phi_d\}$, $\{\lambda_d\}$ and $\{t_{rd}\}$. To complete the model specification, we place weakly informative priors on ϕ_r, λ_r, ϕ_d, and λ_d, and place a weakly informative hierarchical prior on t_{rd}. The hierarchical structure allows us to make reasonable inferences even for locations with a relatively small number of stops. Finally, we compute the posterior distribution of the parameters given the data.

We estimate the posterior distribution of the parameters via Hamiltonian Monte Carlo (HMC) sampling (Duane et al., 1987; Neal, 1994), a form of Markov chain Monte Carlo sampling (Metropolis et al., 1953). We specifically use the No-U-Turn sampler (NUTS) (Hoffman and Gelman, 2014) as implemented in Stan (Carpenter et al., 2016), an open-source modeling language for full Bayesian statistical inference. To assess convergence of the algorithm, we sampled five Markov chains in parallel and computed the potential scale reduction factor $\hat{R}$ (Gelman and Rubin, 1992). We found that 2,500 warmup iterations and 2,500 sampling iterations per chain were sufficient for convergence, as indicated by $\hat{R}$ values less than 1.05 for all parameters, as well as by visual inspection of the trace plots.

We apply *posterior predictive checks* (Gelman et al., 1996, 2014) to evaluate the extent to which the fitted model yields race- and location-specific search and hit rates that are in line with the observed data. For each department and race group, we compare the observed search and hit rates to their expected values under the assumed data-generating process with parameters drawn from the inferred posterior distribution. Specifically, we compute the posterior predictive search and hit rates as follows. During model inference, our Markov chain Monte Carlo sampling procedure yields $2,500 \times 5 = 12,500$ draws from the joint posterior distribution of the parameters. For each parameter draw—consisting of $\{\phi_r^*\}$, $\{\lambda_r^*\}$, $\{\phi_d^*\}$, $\{\lambda_d^*\}$ and $\{t_{rd}^*\}$—we analytically compute the search and hit rates s_{rd}^* and h_{rd}^* for each race-location pair implied by the data-generating process with those parameters. Finally, we average these search and hit rates over all 12,500 posterior

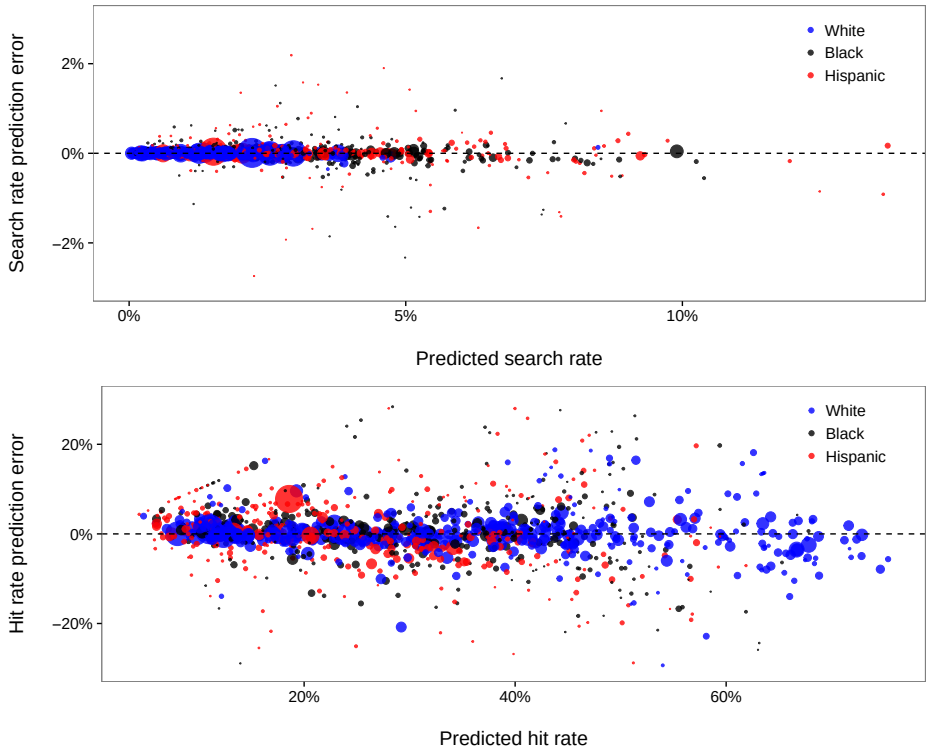

Figure 5: *Posterior predictive checks for search rates (top) and hit rates (bottom). Both plots indicate the fitted model captures key features of the data.*

draws. Figure 5 compares the model-predicted search and hit rates to the actual, observed values. Each point in the plot corresponds to a single race-location group, where groups are sized by number of stops. The fitted model recovers the observed search rates almost perfectly across races and locations. The fitted hit rates also agree with the data well, with the largest groups exhibiting almost no error. These posterior predictive checks thus indicate that the fitted model captures key features of the observed data.

We now turn to the substantive implications of our threshold analysis. As shown in Figure 4 (bottom row), the threshold test indicates that the bar for searching black and Hispanic drivers is lower than for searching white drivers in nearly every location we consider. In aggregate, the inferred threshold for white drivers is 20%, compared to 16% for blacks and 14% for Hispanics. These aggregate thresholds are computed by taking a weighted average of location-specific thresholds, where weights are proportional to the total number of stops in each location. The 95% credible intervals for the aggregate, race-specific thresholds are non-overlapping: $(19\%, 20\%)$ for white drivers, $(15\%, 17\%)$ for black drivers, and $(13\%, 14\%)$ for Hispanic drivers. Whereas the outcome test indicates discrimination only against Hispanic drivers, the threshold test suggests discrimination against both blacks and Hispanics. Consistent with past work (Simoiu et al., 2017), this difference appears to be driven by a small but disproportionate number of black drivers who have high inferred likelihood of carrying contraband. Thus, even though the threshold test finds the bar for searching black drivers is lower than for whites, these groups have similar hit rates.

The threshold test provides evidence of bias in search decisions. However, as with all tests of discrimination, there is a limit to what one can conclude from such statistical analysis alone. For example, if search policies differ not only across but also within jurisdictions, then the threshold test

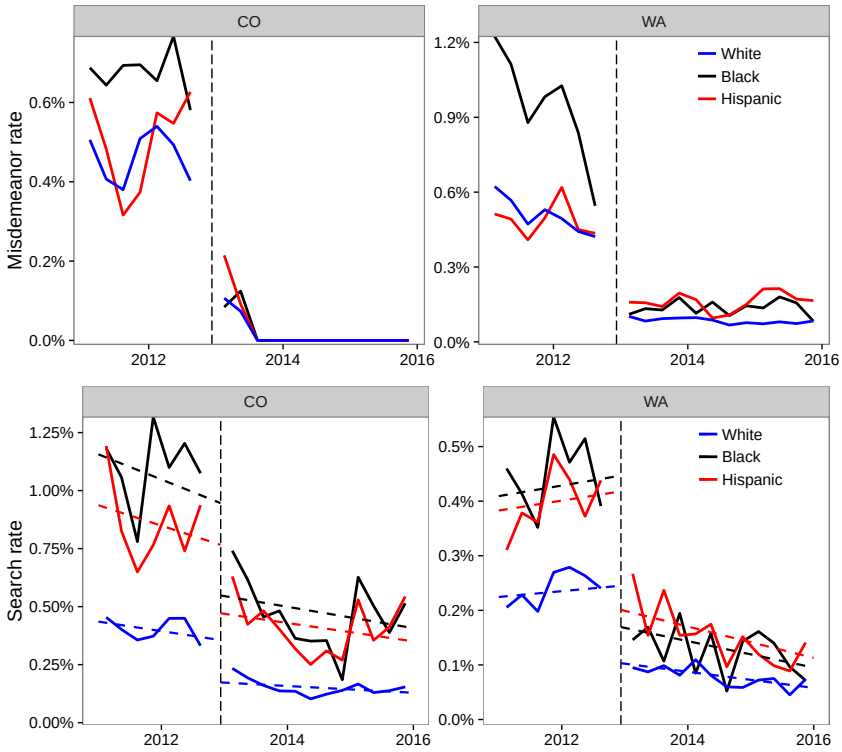

Figure 6: *The proportion of stops that result in a drug-related misdemeanor (top) or search (bottom) before and after recreational marijuana was legalized in Colorado and Washington at the end of 2012 (indicated by the vertical lines). Subsequent to legalization, there is a substantial drop in search and misdemeanor rates. The dashed lines show fitted linear trends pre- and post-legalization.*

might mistakenly indicate discrimination where there is none. Additionally, if officers disproportionately suspect more serious criminal activity when searching black and Hispanic drivers compared to whites, then lower observed thresholds may stem from non-discriminatory police practices.

5 The effects of legalizing marijuana on stop outcomes

We conclude our analysis by investigating the effects of legalizing recreational marijuana on search and misdemeanor rates. We specifically examine Colorado and Washington, the two states in which marijuana was recently legalized and for which we have detailed data. As shown in Figure 6 (top) the number of drug-related misdemeanors in both states fell substantially after marijuana was legalized at the end of 2012, in line with expectations. In Colorado, we consider only misdemeanors for marijuana possession, and so the rate necessarily drops after legalization; in Washington, we include misdemeanors for any type of drug possession as more detailed information is not available, and so there are still some recorded drug misdemeanors post-legalization. Notably, since black drivers were more likely to be charged with such offenses prior to legalization, black drivers were also disproportionately impacted by the policy change. This finding is consistent with past work showing that marijuana laws disproportionately affect minorities (Mitchell and Caudy, 2015).

Because the policy change decriminalized an entire class of behavior (i.e., possession of minor amounts of marijuana), it is not surprising that drug offenses correspondingly decreased. It is less clear, however, how the change affected officer behavior more broadly. We find that after marijuana

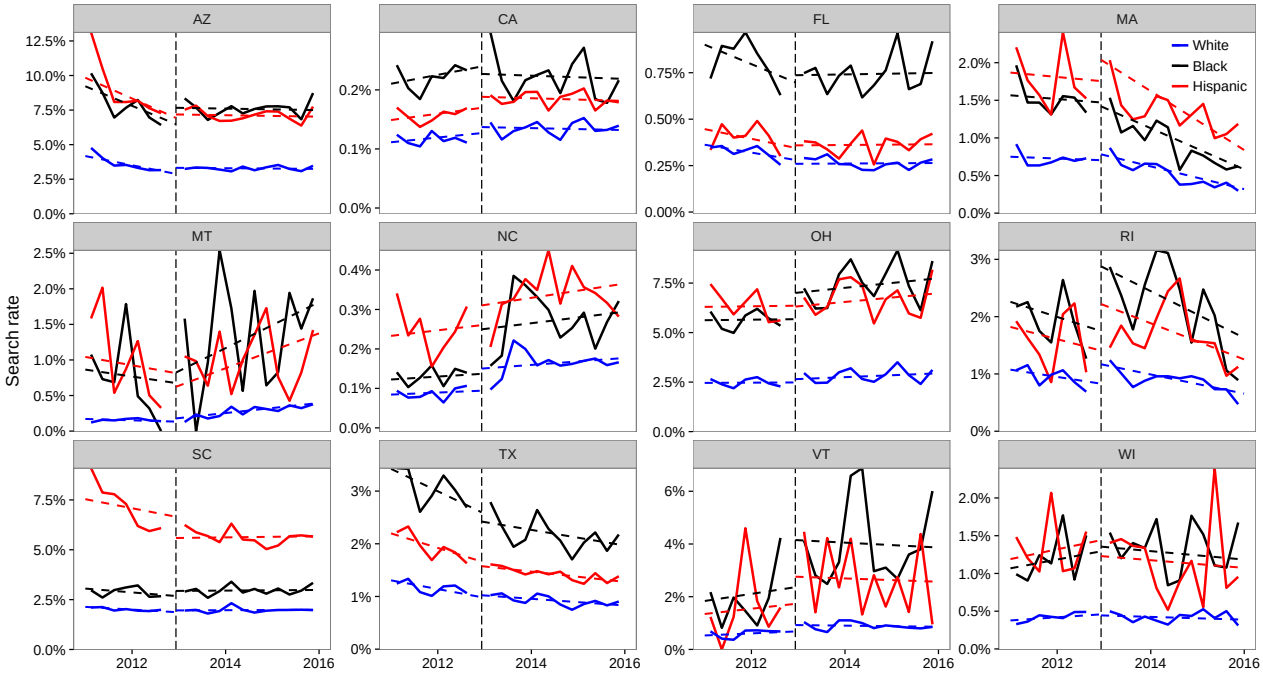

Figure 7: *In the twelve states where marijuana was not legalized, and for which we have the necessary search data, search rates do not fall at the end of 2012; this pattern further suggests that marijuana legalization caused the observed drop in search rates in Colorado and Washington.*

was legalized, the number of searches fell substantially in Colorado and Washington, (Figure 6, bottom), ostensibly because the policy change removed a common reason for conducting searches. In both states, we exclude searches incident to an arrest and other searches that are conducted as a procedural matter, irrespective of any suspicion of drug possession. Because black and Hispanic drivers were more likely to be searched prior to legalization, the policy change reduced the absolute gap in search rates between white and minority drivers; however, the relative gap persists, with minorities still more likely to be searched than whites. We further note that marijuana legalization has secondary impacts for law-abiding drivers, as fewer searches overall means fewer searches of innocent individuals. In the year after legalization in Colorado and Washington, 40% fewer drivers were searched with no contraband found than in the year before legalization.

As shown in Figure 7, in the twelve states where marijuana was not legalized—and for which we have the necessary search data—search rates did not drop significantly at the end of 2012. This pattern further suggests that the observed drop in search rates in Colorado and Washington is due to marijuana legalization. To add quantitative detail to this visual result, we compute a simple difference-in-difference estimate (Angrist and Pischke, 2008). Specifically, we fit the following search model on the set of stops in the 14 states we consider here (Colorado, Washington, and the twelve non-legalization states in Figure 7):

$$\Pr(Y = 1) = \text{logit}^{-1}\left(\sum_{s \in \text{state}} \beta_s I_s + \sum_{r \in \text{race}} \beta_r I_r + \beta_t \cdot t + \sum_{r \in \text{race}} \alpha_r I_r Z\right),$$

where Y indicates whether a search was conducted, β_s and β_r are state and race fixed effects, and β_t is a time trend, with t a continuous variable in units of years since legalization (e.g., $t = 0.5$

	Coef.	s.e.
Effect of legalization on white drivers	-0.99	0.02
Effect of legalization on black drivers	-1.01	0.06
Effect of legalization on Hispanic drivers	-0.79	0.03
Time (years)	-0.02	0.00
Black driver	0.79	0.00
Hispanic driver	0.64	0.00

Table 4: *Effects of legalizing recreational marijuana on search rates, as estimated with a difference-in-difference model. All race groups experienced a large drop in search rate.*

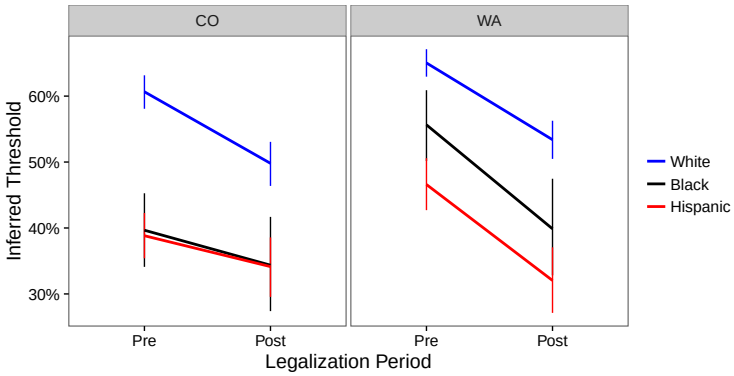

Figure 8: *Inferred average thresholds faced by drivers of different races before and after marijuana legalization. Error bars show the 95% credible intervals of the posterior thresholds. In all cases minority drivers face a lower threshold than white drivers.*

means 6 months post-legalization). The Z term indicates "treatment" status; that is, $Z_i = 1$ in Colorado and Washington for stops carried out during the post-legalization period, and $Z_i = 0$ otherwise. Thus the key parameters of interest are the race-specific treatment effects α_r. Table 4 lists coefficients for the fitted model. We find that α_r is large and negative for whites, blacks, and Hispanics, which again suggests the observed drop in searches in Colorado and Washington was due to the legalization of marijuana in those states.

Despite marijuana legalization decreasing search rates for all races, Figure 6 shows that the relative disparity between whites and minorities remains. We adapt the threshold test to assess the extent to which this disparity in search rates may reflect bias. Specifically, we estimate race-specific search thresholds pre- and post-legalization. To do so, we first divide the stops into pre- and post-legalization periods, indexed by $t \in \{\text{pre, post}\}$. The equations in Section 4.2 are modified to allow race-dependent time variation in the signal distributions and thresholds:

$$\begin{aligned}
\phi_{rdt} &= \text{logit}^{-1}(\phi_r + \phi_d + \phi_{rt}) \\
\lambda_{rdt} &= \exp(\lambda_r + \lambda_d + \lambda_{rt}) \\
t_{rdt} &= t_{rd} + t_{rt},
\end{aligned}$$

where the new parameters ϕ_{rt}, λ_{rt}, and t_{rt} are set to 0 when $t = \text{pre}$, and given a weakly informative $\text{N}(0, 1)$ prior otherwise. Inference in the model is performed separately for Colorado and Washington.

Examining the inferred thresholds (shown in Figure 8), we observe that whites drivers face

consistently higher search thresholds than minority drivers, both before and after marijuana legalization. The data thus suggest that although overall search rates drop in Washington and Colorado, bias persists in search decisions.

Figure 8 also shows that the average threshold faced by all groups decreases after legalization (though not all drops are statistically significant). There are several possible explanations for this decrease. Officers may not have fully internalized the change of policy, searching people who would have been at risk of carrying contraband before legalization, but are no longer high risk now that marijuana is legal. Alternatively, or in addition, officers may now be focused on more serious offenses (such as drug trafficking), applying a lower threshold commensurate with the increase in the severity of the suspected crime. Finally, officers may have more resources after being relieved of the task of policing marijuana possession, freeing them to make searches with a lower chance of finding contraband.

Discussion

Our investigation of over 60 million state patrol stops across the United States reveals widespread racial disparities in stop, citation, search, and arrest rates. It is important to note, however, that such differences may stem from a variety of mechanisms, and are not necessarily the result of racial bias. Moving beyond these disparities, a threshold analysis indicates that black and Hispanic drivers are searched on the basis of less evidence than white drivers, suggestive of bias in search decisions. The recent legalization of recreational marijuana in Colorado and Washington reduced the absolute gap in search rates between whites and minorities—because search rates decreased for all groups—but the relative gap remained. A threshold test further suggests that minorities continue to face bias in search decisions post-legalization. In aggregate, our results lend insight into the differential impact of policing on minority communities nationwide.

Our study provides a unique perspective on working with large-scale policing data. We conclude by offering several recommendations for data collection, release, and analysis. At minimum, we encourage states to collect individual-level stop data that include the date and time of the stop; the location of the stop; the race, gender, and age of the driver; the stop reason; whether a search was conducted; the search type (e.g., "probable cause" or "consent"); whether contraband was found during a search; the stop outcome (e.g., a citation or an arrest); and the specific violation the driver was charged with. Most states collect only a subset of this information. There are also variables that are currently rarely collected but would be useful for analysis, such as indicia of criminal behavior, an officer's rationale for conducting a search, and short narratives written by officers describing the incident. New York City's UF-250 form for pedestrian stops is an example of how such information can be efficiently collected (Goel et al., 2016a; Mummolo, 2016).

Equally important to data collection is ensuring the integrity of the recorded information. We frequently encountered missing values and errors in the data (e.g., implausible values for a driver's age and invalid racial categorizations). Automated procedures can be put in place to help detect and correct such problems. In most cases, the recorded race of the driver is based on the officer's perception, rather than a driver's self-categorization. While there are sound reasons for this practice, it increases the likelihood of errors, a problem we observed in the Texas state patrol data. To quantify and correct for this issue, police departments might regularly audit their data, possibly by comparing an officer's perception of race to a third party's judgment based on driver's license photos for a random sample of stopped drivers.

Despite the existence of public records laws, seven states failed to respond to our repeated requests for information. We hope law enforcement agencies consider taking steps to make data more accessible to external researchers and to the public. Connecticut and North Carolina are at

the forefront of opening up their data, providing online portals for anyone to download and analyze this information.

Finally, we hope that police departments start regularly analyzing their data and report the results of their findings. Such analyses might include estimates of stop, search, and hit rates, stratified by race, age, gender, and location; distribution of stop reasons by race; and trends over time. More ambitiously, departments could use their data to design statistically informed guidelines to encourage more consistent, efficient, and equitable decisions (Goel et al., 2016a,b). Many of these analyses can be automated and re-run regularly with little marginal effort. In conjunction with releasing the data underlying these analyses, we recommend the analysis code also be released to ensure reproducibility. Collecting, releasing, and analyzing police data are essential steps for increasing the effectiveness and equity of law enforcement, and for improving relations with the public through transparency.

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6 Appendix

Below we describe the procedures we used to standardize each field in our data.

1. **Stop location.** The location of stops was encoded at varying levels of granularity across states, including police beat, city or town name, intersection, address, highway number and marker, county name, county FIPS code, district code, latitude and longitude coordinates, and highway patrol trooper zone. To provide a standard location coding, we aimed to map each stop to the county in which it occurred. For example, the data provided by Washington contained the highway type, number, and closest mile marker, which we first mapped to latitude and longitude coordinates using a publicly available dataset of highway marker locations; we then mapped the coordinates to counties using public shape files. Similarly, unidentified counties in Arizona were mapped to county using the highway the stop occurred on. For Connecticut, Massachusetts and Vermont, the counties were mapped using the police department that recorded the stop. For these states we found the county corresponding to the police department using the Google Maps API. In North Carolina, Illinois, and Rhode Island, no consistent county-level information was provided for state patrol stops. Therefore, we mapped stops to similarly granular location variables: for North Carolina we used district, for Illinois we used state patrol division, and for Rhode Island we used zone code. For North Carolina and Illinois we aggregated census statistics for the counties subsumed in the region to have a usable benchmark.

2. **Driver race.** We restrict our primary analysis to white, black, and Hispanic drivers. We specifically exclude stops of Asian and Native American drivers, as these groups were not sufficiently represented in our data to allow granular analysis. Some states provided ethnicity of the driver in addition to race; drivers with Hispanic ethnicity were considered Hispanic regardless of their recorded race, consistent with previous investigations. To aid future work, we classify drivers as "Asian" and "other" where possible—though these groups are not included in our main analysis. For example, Native American and Alaskan Native drivers were classified as "other"; South Asian and Pacific Islander drivers were classified as "Asian".

3. **Driver age.** States provided either date of birth, birth year, or age of the driver. The age of the driver at the time of the stop was calculated by taking the difference between the stop date and the birth date of the driver, or stop year and birth year. If the inferred age of driver was less than 15 or greater than or equal to 100, we assumed the data were incorrect and treated age as missing in those cases.

4. **Violation.** Some states listed one violation per stop, while others provided multiple violations (e.g., there were up to twelve recorded in Washington). If multiple violation codes were provided, all were included in our standardized data. The granularity of violation codes also varied greatly, from two categories (e.g., speeding and seat belt violations in Massachusetts) to over 1,000 in Colorado. Some states provided violation data by referring to local state statute numbers, which we mapped to a text description of the violation by consulting state traffic laws. We developed a two-level hierarchy of violation categories, and standardized each violation reason using this rubric. Our violation categories are as follows: (1) license/registration (with subcategories for license, registration/plates, and paperwork); (2) speeding; (3) seat belt violations; (4) stop sign/light; (5) equipment (with a subcategory for head/taillight violations); (6) driving under the influence (DUI); (7) moving violations (with subcategories for "safe movement" and "cell phone"); and (8) truck violations. We coded violations using the

most granular category possible. For states that had hundreds of violation codes, we mapped the most common ones until 95% of stops were accounted for.

5. **Stop purpose.** Some states distinguish between violation and stop purpose—the initial reason for the stop. When stop purpose was explicitly provided, it was placed in a separate column and normalized using the same values as the violation codes.

6. **Stop outcome.** Some states provided information on the outcome of the stop: for example, verbal warning, written warning, citation, summons, or arrest. In the case of speeding stops—which we specifically analyze—a stop was classified as a warning if either a warning or no penalty was given. A few states provided multiple outcomes for each stop, and in these cases, we recorded the most severe outcome—for example, if both a citation and a warning were given, the stop outcome was coded as a citation .

7. **Search conducted.** Many states provided a binary indicator for whether a search was conducted. In other cases we had to construct this field from other information in the data. For instance, North Carolina and South Carolina provided information on whether the driver, passenger, or vehicle was searched; we coded that a search was conducted if any of these three events occurred.

8. **Search type.** We standardize search types into categories which include, for example, consent, probable cause, incident to arrest, inventory, warrant, protective frisk, and K9 searches. Most of the standardization consisted of normalizing the language (e.g., "drug dog alert" and "any K9 Used for Search" were mapped to "K9 search"). Some states had multiple search reasons, others only one. If multiple search types were given, all were included in our standardized dataset.

9. **Contraband found.** As with the "search conducted" field, states often provided a binary indicator for whether contraband was found. In other cases, it is constructed from multiple binary flags. For example, in South Carolina, we say that contraband was found if any of the "Contraband", "ContrabandDrugs," "ContrabandDrugParaphenalia,", "ContrabandWeapons", or "ContrabandDesc" fields indicate that contraband was found. In some cases, it was indicated that contraband was found but no search was conducted. It is unclear whether a search was in fact conducted but not recorded, whether contraband was incorrectly marked, or whether contraband was discovered through a process other than a search (e.g., found near the vehicle). In these instances, we set the field value to "false", and note that the choice affects only a small proportion of searches and does not qualitatively affect our results.

State	Data released	Used in analysis	Response status
Alabama			No response
Alaska			Does not collect
Arizona	•	•	Individual stop data received
Arkansas			No central database
California	•	•	Individual stop data received
Colorado	•	•	Individual stop data received
Connecticut	•	•	Individual stop data received
Delaware			Provided reports only
Florida	•	•	Individual stop data received
Georgia			Does not collect
Hawaii			No response
Idaho			Does not collect
Illinois	•	•	Individual stop data received
Indiana			No response
Iowa	•		Incomplete race data
Kansas			Request denied
Kentucky			No central database
Louisiana			Request denied
Maine			No central database
Maryland	•	•	Individual stop data received
Massachusetts	•	•	Individual stop data received
Michigan	•		Incomplete race data
Minnesota			Does not collect
Mississippi	•		Incomplete race data
Missouri	•	•	Summary data received
Montana	•	•	Individual stop data received
Nebraska	•	•	Summary data received
Nevada	•		Incomplete race data
New Hampshire	•		Incomplete race data
New Jersey	•	•	Individual stop data received
New Mexico			No response
New York			No central database
North Carolina	•	•	Individual stop data received
North Dakota	•		Provided citation data only
Ohio	•	•	Individual stop data received
Oklahoma			No response
Oregon	•		Summary data received, not usable
Pennsylvania			Request denied
Rhode Island	•	•	Individual stop data received
South Carolina	•	•	Individual stop data received
South Dakota	•		Missing race data
Tennessee	•		Provided citation data only
Texas	•	•	Individual stop data received
Utah			Request denied
Vermont	•	•	Individual stop data received
Virginia	•		Summary data received, not usable
Washington	•	•	Individual stop data received
West Virginia			No central database
Wisconsin	•	•	Individual stop data received
Wyoming	•		Provided citation data only

Table A1: *Status of responses to our public record requests, at time of writing.*

	State	Stops	Time Range	Stop Date	Stop Time	Stop Location	Driver Race	Driver Gender	Driver Age	Stop Reason	Search Conducted	Search Type	Contraband Found	Stop Outcome
1	**Arizona**	2,251,992	2009-2015	•	•	•	•	•			•		•	•
2	**California**	31,778,515	2009-2016	•		•	•	•		•	•	•		•
3	**Colorado**	2,584,744	2010-2016	•	•	•	•	•	•	•	•	•	•	
4	**Connecticut**	318,669	2013-2015	•	•	•	•	•	•	•	•	•	•	•
5	**Florida**	5,421,446	2010-2016	•	•	•	•	•	•	•	•	•		•
6	**Illinois**	4,715,031	2004-2015	•	•	•	•	•	•	•	•	•	•	•
7	Iowa	2,441,335	2006-2016	•	•					•				•
8	**Maryland**	1,113,929	2007-2014				•	•		•	•		•	•
9	**Massachusetts**	3,418,298	2005-2015	•		•	•	•	•		•	•	•	•
10	Michigan	709,699	2001-2016	•	•	•				•				•
11	Mississippi	215,304	2013-2016	•		•	•	•	•	•				
12	**Missouri**	2,292,492	2010-2015				•				•		•	
13	**Montana**	825,118	2009-2016	•	•	•	•	•	•	•	•	•		•
14	**Nebraska**	4,277,921	2002-2014				•				•			
15	Nevada	737,294	2012-2016	•			•		•	•				•
16	New Hampshire	259,822	2014-2015	•	•	•		•		•				•
17	**New Jersey**	3,845,335	2009-2016	•	•	•	•	•		•				•
18	**North Carolina**	9,558,084	2000-2015	•		•	•	•	•	•	•	•	•	•
19	North Dakota	330,063	2010-2015	•	•	•	•	•	•	•				
20	**Ohio**	6,165,997	2010-2015	•	•	•	•	•			•			
21	Oregon	1,143,017	2010-2016				•							
22	**Rhode Island**	509,681	2005-2015	•	•	•	•	•	•	•	•	•	•	•
23	**South Carolina**	8,440,934	2005-2016	•		•	•	•	•		•		•	•
24	South Dakota	281,249	2012-2015	•	•	•		•		•				•
25	Tennessee	3,829,082	1996-2016	•	•	•	•	•		•				•
26	**Texas**	23,397,249	2006-2015	•	•	•	•	•		•	•	•	•	•
27	**Vermont**	283,285	2010-2015	•	•	•	•	•	•	•	•	•	•	•
28	Virginia	5,006,725	2006-2016	•		•	•				•			
29	**Washington**	8,624,032	2009-2016	•	•	•	•	•	•	•	•	•	•	•
30	**Wisconsin**	1,059,033	2010-2016	•	•	•	•	•		•	•	•	•	•
31	Wyoming	173,455	2011-2012	•	•	•	•	•	•	•				
	Total	**136,008,830**												

Table A2: *Overview of the complete state patrol dataset. For each column a solid circle signifies data are available for at least 70% of the stops. The states used for the analysis in the paper are boldfaced. For all states except Illinois, North Carolina, and Rhode Island, "stop location" refers to county; for these three states, it refers to a similarly granular location variable, as described above.*

Outcome	Covariates	Black	Hispanic	States
Stop (negative binomial)	race, location, demo, year	0.37 (0.01)	-0.40 (0.01)	CO,CT,FL,IL,MA,MT,NC,SC,VT,WA
Stop (Poisson)	race, location, demo, year	0.27 (0.01)	-0.27 (0.01)	CO,CT,FL,IL,MA,MT,NC,SC,VT,WA
Stop (quasi-Poisson)	race, location, demo, year	0.27 (0.00)	-0.27 (0.01)	CO,CT,FL,IL,MA,MT,NC,SC,VT,WA
Citation	race	0.61 (0.00)	-0.11 (0.00)	CO,FL,IL,MT,NC,RI,TX,WI
Citation	race, location	0.22 (0.00)	0.36 (0.00)	CO,FL,IL,MT,NC,RI,TX,WI
Citation	race, location, time	0.24 (0.00)	0.36 (0.00)	CO,FL,IL,MT,RI,TX,WI
Citation	race, location, demo	0.17 (0.00)	0.32 (0.00)	CO,FL,IL,MT,NC,RI
Citation	race, location, time, demo	0.18 (0.00)	0.29 (0.00)	CO,FL,IL,MT,RI
Search	race	0.57 (0.00)	0.64 (0.00)	AZ,CA,CO,CT,FL,IL,MA,MD,MO,MT,NC,NE,OH,RI,SC,TX,VT,WA,WI
Search	race, location	0.68 (0.00)	0.74 (0.00)	AZ,CA,CO,CT,FL,IL,MA,MT,NC,OH,RI,SC,TX,VT,WA,WI
Search	race, location, time	0.75 (0.00)	0.67 (0.00)	AZ,CO,CT,FL,IL,MT,OH,RI,TX,VT,WA,WI
Search	race, location, demo	0.56 (0.00)	0.66 (0.01)	CO,CT,FL,IL,MA,MT,NC,RI,SC,VT,WA
Search	race, location, time, demo	0.73 (0.01)	0.54 (0.01)	CO,CT,FL,IL,MT,RI,VT,WA
Consent search	race	0.66 (0.01)	1.14 (0.01)	CO,FL,MA,MD,NC,TX,WA
Consent search	race, location	0.76 (0.01)	0.76 (0.01)	CO,FL,MA,NC,TX,WA
Consent search	race, location, time	0.77 (0.01)	0.76 (0.01)	CO,FL,TX,WA
Consent search	race, location, demo	0.69 (0.02)	0.70 (0.02)	CO,FL,MA,NC,WA
Consent search	race, location, time, demo	0.77 (0.03)	0.62 (0.02)	CO,FL,WA
Arrest	race	0.51 (0.00)	0.72 (0.00)	AZ,CA,CO,CT,FL,MA,MD,MT,NC,OH,RI,SC,VT,WI
Arrest	race, location	0.50 (0.00)	0.61 (0.00)	AZ,CA,CO,CT,FL,MA,MT,NC,OH,RI,SC,VT,WI
Arrest	race, location, time	0.75 (0.01)	0.75 (0.01)	AZ,CO,CT,FL,MT,OH,RI,VT,WI
Arrest	race, location, demo	0.44 (0.00)	0.75 (0.01)	CO,CT,FL,MA,MT,NC,RI,SC,VT
Arrest	race, location, time, demo	0.65 (0.01)	0.69 (0.01)	CO,CT,FL,MT,RI,VT

Table A3: *Coefficients for driver race for various regression specifications, with standard errors in parentheses. "Covariates" denotes the set of variables used in the regression; "time" indicates that stop year, stop quarter, stop weekday, and stop hour are included as covariates, and "demo" indicates that driver age and gender are included as covariates. Logistic regression is used in all cases except for estimating stop rate.*