DRIVER BEHAVIOR & PERFORMANCE

TECHNICAL REPORT

FOUNDATION FOR TRAFFIC SAFETY

Traffic Safety Impacts of Reduced Police–motorist Interactions

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Title

Traffic Safety Impacts of Reduced Police–motorist Interactions

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Foreword

The original scope of this work was intended to examine the relationship between changes in police traffic enforcement—particularly reductions in traffic stops—and roadway safety outcomes across multiple jurisdictions using a case-control design. Due to limitations in the availability of and trends in relevant data across sites, findings of the case studies presented in this report reflect only observed associations within specific contexts of available information rather than definitive causal effects. Results reported in this document are influenced by variations in data quality, external factors affecting driving behavior and enforcement practices, and other unmeasured confounding variables.

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About the Sponsor

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Executive Summary

In the wake of changes to the way many police departments enforce traffic laws, there are concerns about potential impacts to road safety. The primary objective of this study was to examine measurable effects of reduced police–motorist interactions on key traffic safety outcomes. Jurisdictions have increasingly adopted policies that limit traffic stop interactions between law enforcement and motorists, typically involving limiting or prohibiting pretextual stops or stops for minor, non-safety-related violations that are used to investigate more serious crimes. A policy to deprioritize pretextual stops is typically associated with an increased focus on public safety, by prioritizing violations associated with dangerous driving behaviors such as speeding, distracted driving, or impaired driving. Existing studies examining the impact of policy change on safety outcomes are limited in number, scope, and quality, but the data that are available suggest that policies reducing police–motorist interactions do not have a detrimental impact on public safety. However, most policy changes are fairly recent, and therefore the full impact of the changes may not be apparent yet. In addition, many jurisdictions have seen reduced police–motorist interactions due to a variety of factors—in the absence of a policy change.

The current study sought to examine the relationship between reduced police–motorist interactions and safety outcomes. Six jurisdictions were selected for study. Three sites (Nashville, TN; Philadelphia, PA; and Minneapolis, MN) were selected because of a large and sustained decline in police interactions with motorists. In contrast, the other three locations (Charlotte, NC; Chicago, IL; and Saint Paul, MN) were selected because they exhibited less change in traffic stops.

Data on crashes of various severity were obtained for each site. In this study, an interrupted time series analysis was used to evaluate variations in crash trends and severity levels after a change in the number of police interactions with motorists. Monthly crash counts were analyzed using ARIMA models, which account for patterns like seasonal cycles and long-term trends across different crash types, including fatal and serious injury crashes, known-injury crashes, and all police-reported crashes. The impact of the reduction in police stops on speed-related crashes at all crash-severity levels was also analyzed. Lastly, rate ratios were calculated to quantify the effect size of the changes in traffic stops based on the interrupted time series models.

Results & Conclusion

Results on the relationship between police–motorist stops and safety outcomes were inconsistent. For two of the three locations with the biggest and most sustained declines in police–motorist stops (Philadelphia and Minneapolis), there was some indication of increased crashes, particularly among more severe or speed-related crashes. Minneapolis exhibited significant rises in fatal and serious injury crashes and

known-injury crashes; results also suggest there may have been a significant increase in speed-related crashes. Philadelphia demonstrated significant increases in fatal and serious injury crashes and speed-related crashes, but also saw slight reductions in known-injury and total police-reported crashes. The third location (Nashville), however, did not see significant increases in fatal and serious injury, known-injury, or speed-related crashes; there was a small increase in total police-reported crashes. Notably, of the three sites with steeper declines in enforcement, Nashville was the only jurisdiction whose entire study period pre-dated the COVID-19 period.

The jurisdictions with more modest changes in enforcement also did not show a consistent pattern on crash outcomes. Charlotte showed a small but statistically significant increase in speed-related crashes and a small but statistically significant decrease in police-reported crashes. Patterns in Chicago indicated a significant increase in fatal and serious injury crashes and a decrease in all police-reported crashes. Saint Paul exhibited a significant increase in fatal and serious injury crashes, but a small but statistically significant decrease in known injury crashes and speed-related crashes.

Many methodological challenges exist in evaluating the impact of changes in police–motorist interactions on road safety, and the case-study approach used had some notable limitations. It is possible that with declining numbers of officers, some non-injury crashes may not be reported, and therefore were not captured in the data. The case-study approach was unable to use control groups, due to a lack of available data from comparable cities without reductions in police stops in the relevant time periods. Therefore, it was not possible to completely disentangle the impact of changes in enforcement levels from the impacts of other factors that transpired around the same time (such as the COVID-19 pandemic).

Given the difficulty of isolating the effects of changes in police–motorist interactions from other factors with variable and potentially large influence on safety outcomes, the long-term impacts of these types of changes are difficult to assess. This study's mixed results across the selected sites do not demonstrate conclusively that reductions in police–motorist interactions impact crash rates. However, the results suggest that increases in more serious and speed-related crashes coincided with the timing of reductions in police–motorist interactions in some case-study cities.

Introduction

Traffic law enforcement has long been a key component among efforts to reduce crashes, deaths, and injuries associated with high-risk driving behaviors, but somewhat recently, a growing number of policy makers are revising their policies regarding the priority of traffic stops for pretextual, regulatory, and equipment-related reasons. The rationale for a change in policy and procedures is to increase the safety of drivers (and officers) by limiting the frequency of contacts between law enforcement and motorists for these "minor" violations. These are perceived to be less directly related to traffic safety than higher-risk violations such as speeding, alcohol-impaired driving, failure to wear a seat belt, or distracted driving. Some police departments have also reduced traffic enforcement and general contact with motorists for other reasons. Since the early part of 2020, police staffing, and sometimes willingness to conduct traffic stops have also led to a reduction in traffic law enforcement without necessarily having a policy driving the change (Lum et al., 2023; Police Executive Research Forum, 2021). The impact of the reduction in traffic enforcement on public safety has not been well documented, whether the reductions were a function of policing reform and policies or other reasons (e.g., staffing).

The primary objective of the study was to examine measurable effects of reduced police–motorist interaction on key traffic safety outcomes using a case-study approach. Six jurisdictions were selected to examine the relationship between police traffic stops and motor vehicle crashes.

Interventions that Reduce Interactions between Police and Motorists

Several types of interventions have the potential to limit the number of police–motorist interactions. Some are relatively simple, involving a change in enforcement priorities or use of technology, whereas others are more involved and rely on road surveys, data collection/analysis, and more global changes in traffic safety procedures. Police–motorist interactions may also be influenced by other factors, such as funding availability, political or social pressure, and societal events like the COVID-19 pandemic.

Changes in Policy

Change in enforcement policy typically involves limiting or prohibiting pretextual stops or stops for minor non-safety-related violations. These minor offenses typically fall into one of the following categories: tinted windows, lighting violation, improper license plate/registration display, vehicle defects, rearview mirror/window obstruction, expired/invalid registration, or expired/invalid inspection. A policy to deprioritize pretextual investigatory stops is typically associated with an increased focus on public safety by prioritizing violations associated with dangerous driving such as speeding, distracted driving, or impaired driving. As of 2023, more than a dozen jurisdictions have

implemented such a reform, including the states of Virginia, Washington, New Jersey, and Oregon and the cities of Washington, DC; Philadelphia and Pittsburgh, PA; Memphis, TN; Ann Arbor, MI; Fayetteville, NC; Berkeley, CA; Brooklyn Center, MN; and New York City, NY (Rau et al., 2023). Note that this list is constantly changing and reflects the state of affairs at the time of this review.

While automated enforcement of speeding, red-light running, and other dangerous driving behaviors may reduce police–motorist interactions, the impacts of implementation of automated enforcement was considered out of scope for the project.

Documented Safety Outcomes of Reductions Associated with Policy Change

Los Angeles, CA: In March 2022, the City of Los Angeles implemented a policy that limits pretextual stops to safety-related incidents. The City Working Group, comprised of city representatives from various departments (e.g., police, transportation, attorney's office), conducted a study with help from consultant firms looking at the early impact of that policy change (City of Los Angeles, 2023). Analyses were conducted using LAPD traffic stop data comparing the years 2019–2021 to 2022. The results showed a clear decline in yearly traffic stops from approximately 712,000 in 2019 to 331,000 in 2022, a 54% decrease. Data from the pandemic years suggest a progressive decrease in enforcement activity with 521,000 stops in 2020 and 429,000 stops in 2021. The data suggest a decrease in racial disparity in stops in 2022 compared to prior years. Moreover, the proportion of traffic stops related to moving violations increased from 52% in 2021 to 71% in 2022.

Although the report by the City of Los Angeles did not include data on injury or fatality outcomes, a separate publication by the non-profit Streets Are for Everyone reported an increase in traffic fatalities concurrent with the decrease in traffic stops (Kevitt, 2024). Fatalities in the City of Los Angeles increased from 246 in 2019 to 312 in 2022, a 27% increase. Although traffic fatalities dropped to 231 in 2020, they increased to 294 in 2021, 312 in 2022, and 336 in 2023.

However, whether the changes observed since 2022 are the result of the policy and/or part of an existing trend (or other factors) cannot be determined with the information available in these reports (due to lack of an adequate control group).

Fayetteville, NC: The Fayetteville Police Department implemented traffic enforcement reform from 2013 through 2016. This example of a successful reprioritization effort shifted focus toward safety stops rather than investigatory stops. This study did not result in a reduction of police interactions but is included in the review because it is one of the few studies to include a pre–post investigation with an adequate control group comparison, providing "difference in differences" evidence of the impact of the policy on safety outcomes. The study compared safety outcomes

between Fayetteville and a control group, and revealed positive safety outcomes associated with the intervention (Fliss et al., 2020).

Over the course of the intervention, there was a 13% decrease in total crashes relative to the control group, a 23% decrease in injury crashes, and a 28% decrease in road fatalities relative to the control group. The traffic stops data show that the shift in focus was successful with more than 80% of all traffic stops for safety-related reasons (compared to 30% before the intervention). However, as mentioned above, the Fayetteville intervention did not reduce police interactions with motorists. Indeed, the data presented in the report suggests that although the yearly average number of discretionary stops decreased from 1,776 to 1,562 (a 12% decrease) from pre-intervention to the end of intervention, the total number of traffic stops increased during the intervention. The yearly average number of safety related stops increased from 6,119 to 23,786 (nearly 300% increase) from pre- to post-intervention; the yearly average number of regulatory and equipment stops increased from 6,073 to 9,583 (~60% increase). Thus, overall, the total number of stops increased as a result of the intervention (Fliss et al., 2020).

St. Paul, MN: The St. Paul Police Department followed a policy implemented by the Ramsey County Attorney's Office (RCAO) that directed law enforcement to focus on safety-related violations rather than minor equipment violations. The motivation behind the policy was that pretextual stops have minimal impact on public safety while putting undue burden on communities of color. Along with the enforcement shift in focus, the policy indicated that the RCAO would not prosecute most felonies resulting from pretextual traffic stops (unless a danger to public safety was apparent).

The Justice Innovation Lab examined the potential impact of the policy on public safety, and the data indicate that the policy was effective in reducing interactions between police and motorists. The number of pretextual stops decreased from a monthly average of 487 before the policy change to just 49 after implementation (Naddeo & Pulvino, 2023). The time series analysis conducted to examine the impact of policy changes on traffic incidents, crime, and gun seizures revealed no significant changes in number of traffic crashes or fatalities, monthly gun seizures, or response time to 911 calls.

Summary: Although studies looking at the impact of policy change on safety outcomes are limited in number, scope, and quality, the data that are available suggest that policies reducing police–motorist interactions do not have a detrimental impact on public safety. However, since the policy changes are fairly recent, the full impact of the changes may not be apparent yet.

Current Study

This study seeks to add to the current body of knowledge by exploring the relationship between reduced police–motorist interactions and safety outcomes using a case-study approach. Six jurisdictions were selected for study: three with clear decreases in traffic stops, and three with more stable stop numbers over time. Data on crashes of various severity were obtained for each site. The impact of different trends in stops on safety outcomes are compared using time series analyses.

Method

An ideal approach to examine associations between enforcement policies and traffic safety outcomes is a controlled interrupted time series approach, to assess temporal trends in crash outcomes in cities with changes to enforcement policies, and associated decreases in police stops, compared to trends in cities where policies did not change. However, as the research team began to gather and appraise candidate data sources, it became clear that this approach was not feasible for several reasons:

- First and foremost, there were virtually no sufficiently large cities without policy changes and in which patterns of police stops did not change at least somewhat substantially (typically declining) with recent and available data.
- Second, cities with Vision Zero programs were avoided, as those crash reduction efforts may confound the relationship between police stops and crashes.
- Finding control cities to pair with those with changes in enforcement policies was further complicated by the difficulty of finding sufficiently similar cities—in terms of location in the same state, population size and density, racial makeup, and household income, among other characteristics—where police stops remained stable.
- Lastly, the necessary data was not available from all cities considered for study.

This study instead employed a case-study design, which allowed for an in-depth examination of trends in crashes before and after changes in police traffic stops, in cities both with and without changes in enforcement policies.

Site Selection

Six case-study locations were selected to examine the impact of reduced traffic stops on traffic safety outcomes. Nashville, TN; Philadelphia, PA; and Minneapolis, MN, were selected because of a measurable decline in police interactions with motorists. In contrast, Charlotte, NC; Chicago, IL; and Saint Paul, MN, were selected because they showed less distinct declines in police–motorist interactions, temporary declines, or increases in stops.

Analysis of Crashes Relative to Decreases in Police Stops

For each location, an interrupted time series analysis was used to evaluate how crash trends and levels changed before and after a reduction in police interactions with motorists (hereafter referred to as an "interruption"). Monthly crash counts were analyzed using ARIMA models, which account for patterns like seasonal cycles and long-term trends. The crash severity types analyzed were as follows:

- Fatal and serious (incapacitating) injury crashes
- Any known injury (fatal, incapacitating, or visible non-incapacitating, but not crashes listed as possible injury crashes), herein referred to as injury crashes
- All police-reported crashes regardless of severity

The impact of the reduction in police stops on speed-related crashes was also examined. A crash was considered speed-related if the investigating officer included speed-related factors (speeding, too fast for conditions, racing, etc.) in their crash report. Speed-related crashes were of particular interest, as research has shown that speeding is one of the driver behaviors affected by enforcement as well as one of the most strongly linked to serious crashes (Kirley et al., 2023).

It was hypothesized that if there was a large enough, sustained decrease in traffic stops then crashes would increase, in general or for certain crash severities. It was also hypothesized that injury crashes would increase as risky drivers were likely those most deterred by enforcement (Chaudhary et al., 2004) and risky drivers would tend to have more injurious crashes (Shyhalla, 2014). Additionally, it was expected that drivers would likely speed more frequently with a lower perceived risk of being ticketed.

In each model, terms for both a level change (a sudden shift in crash counts at the time of the interruption) and a slope change (a shift in the ongoing trend after the interruption) were included. This gave the model the flexibility to detect both immediate and gradual effects. However, including both terms can lead to a more conservative test. When both level and slope changes are modeled together, they share the explanatory power, which can reduce the likelihood that either one appears statistically significant on its own, even if one might have reached significance had it been tested without the other. However, running them separately violates some statistical norms; specifically, the same effect (explanatory power) has the potential of being repeated in each analysis.

To ensure consistency and objectivity across all locations and crash types, the auto.arima() function in the *forecast R* package (Hyndman et al., 2025) was used to select model parameters. This automated method identifies the best-fitting model based on statistical criteria. *R* uses stepwise selection to find the "best fit" model. *R* also evaluated the quality of each model by examining the residuals—the differences between observed and predicted crash counts. Results were examined for signs of leftover patterns that the model did not explain. In a few cases, residual autocorrelations remained, indicating that

some of the patterns in the data may not have been fully captured. These are noted in the results for each relevant location. Nonetheless, this approach provided a systematic, transparent way to compare trends across locations and helped minimize subjective decision-making in model selection. Included in each analysis is the Ljung-Box probability, which indicates whether the autocorrelations were adequately controlled ($p \geq 0.05$) or whether there are still some uncontrolled autocorrelations ($p < 0.05$). Model results with a Ljung-Box probability lower than 0.05 suggest that values are correlated across time in a way that has not been fully accounted for in the model, and thus results should be interpreted with caution.

For each site, model results (Autoregression, Differencing, Moving Average) describe the model's attempts to control for the existing trends in crash outcomes. See the Appendix for a brief description of the models and the significance of each term used in the analysis as well as the significance of changes in slope and level associated with the change in police–motorist interactions. A positive estimate for the level indicates an immediate "step" increase in the outcome measure (i.e., number of crashes) after the interruption. Similarly, a positive change in slope would indicate an increase in the slope of the outcome after the interruption. Note this does not necessarily imply an increase in crash outcomes. If the crash rate had been decreasing before the interruption, a positive change in slope could represent a slowing rate of decrease of the number of monthly crashes, or it could represent a reversal from decreasing to increasing. In the case of monthly crashes that had been trending upward before the interruption, a positive change in slope would represent an accelerating rate of increase. If the slope had been flat before the interruption, then a positive change in slope would simply indicate that the crash rate began increasing. Each location had individual ARIMAs run for different levels of crash severity.

Rate ratios were calculated to quantify the aggregate effect of the interruption on the outcome measure over the post-interruption period examined in a single measure. Rate ratios were calculated by comparing the fitted crash counts from the statistical model to the counterfactual crash counts that would have been expected if no changes in police–motorist interactions had taken place (Travis-Lumer et al., 2022). In practice, this means that the fitted series represents what actually happened given observed conditions, while the counterfactual series projects what would have happened if the interruption had not occurred. The rate ratio was derived as the ratio of these two values. A rate ratio equal to 1 means there was no difference between observed and expected outcomes. A rate ratio less than 1 indicates a reduction relative to the counterfactual, while a rate ratio greater than 1 indicates an increase. The model aimed to minimize but was unable to fully account for other factors that may have affected police–motorist interactions and/or crashes, such as the COVID-19 pandemic and accompanying changes in travel patterns, or changes in the use of automated traffic enforcement or other road safety measures.

Crash plots for each site are also presented. Plots in the figures include the actual number of crashes, the fitted line (what the crash data show after removal of autocorrelation effects), and the model counterfactual line indicating the model's predicted crash numbers had there been no interruption point.

Interruption Dates

Determining the dates that changes to police–motorist interactions took place is challenging for several reasons. There may be delay between the date a policy is enacted and when and how implementation and behavior change occurs among departments and officers, as well as when and how drivers change their behaviors. In addition, implementation may vary across jurisdictions, work shifts, or even individual officers, resulting in the lack of a clear starting point of changes in stops. Further, changes to policies may also impact the recording of stops in administrative data, potentially resulting in lags between changes in officer behavior and when it is reflected in the data. In cities without formal policy changes, identifying the time point where police–motorist interactions change is also difficult as there are many factors that may influence such trends, including the onset of the COVID-19 pandemic, which was encompassed in four of the six study sites' data, high-profile incidents involving police across the country, court rulings, and public scrutiny, which may have affected the number of police–motorist interactions. To best approximate an interruption date, this study based the "interruption" month using each site's stop data. Selection of the interruption timing was determined from examination of stop data graphs for a peak immediately before a decline in monthly stops. Because the analyses consider both level and slope, the interruption was treated as sudden and permanent. Results using this type of interruption are interpreted as immediate and lasting changes, with level shifts indicating abrupt jumps at the interruption point and slope shifts indicating changes in the longer-term trend thereafter. See Table 1 and the figures under each location's analyses.

Table 1. Study Periods and Interruption Dates Used for Models

Location	Dates of Data Modeled	Interruption Date
Nashville	Jan 2015–Feb 2020	Aug 2018
Minneapolis	Jan 2018–Dec 2023	May 2020
Philadelphia	Jan 2017–Dec 2022	May 2020
Saint Paul	Jan 2018–Dec 2023	Sep 2021
Chicago	Jan 2018–Dec 2022	Jan 2020
Charlotte	Jan 2014–Feb 2020	Jul 2016

In all locations, the average monthly number of stops prior to the interruption was higher than post-interruption stops (Table 2). Averages of pre- and post-interruption

stops do not appropriately address pre-existing slopes in the data but provide a starting point from which to judge change. The three locations that were selected for obvious and consistent decreases in police stops—Minneapolis, Nashville, and Philadelphia—had the greatest percentage declines in stops.

Table 2. Decrease in Average Monthly Police Stops from Pre- to Post-Interruption

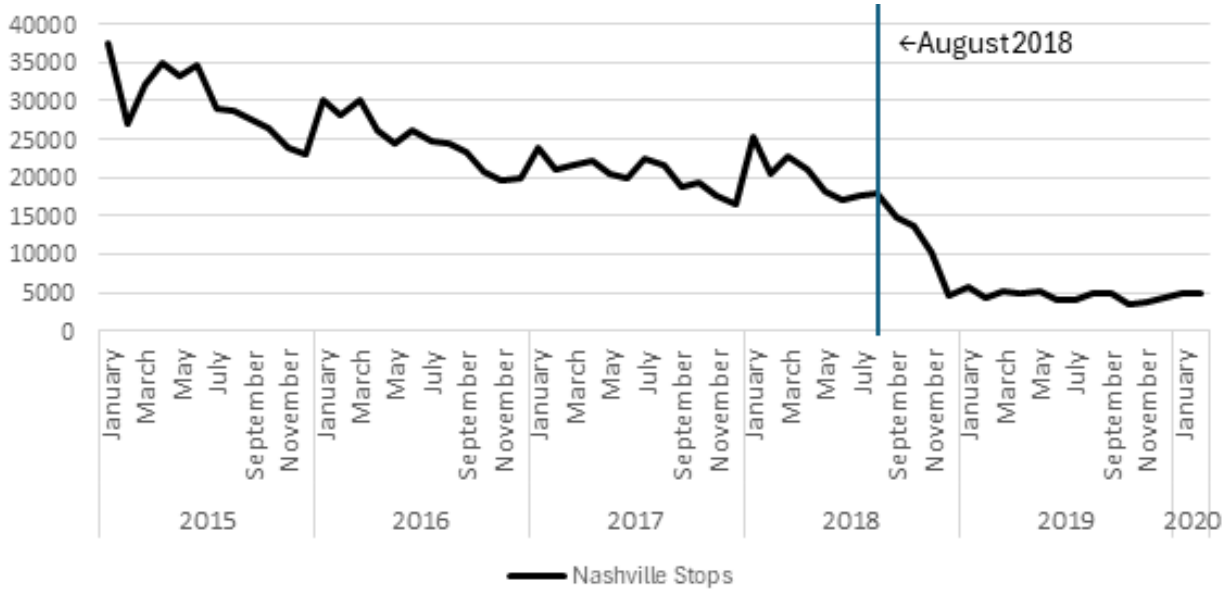
Location	Average Pre	Average Post	% Change
Nashville	23037	2858	−88%
Minneapolis	1650	303	−82%
Philadelphia	26945	10254	−62%
Saint Paul	2458	1563	−36%
Chicago	42226	32595	−23%
Charlotte	9881	7719	−22%

Results

Nashville, Tennessee

Following reports of racial disparity in relation to Nashville law enforcement (Gideon's Army, 2016), a study was conducted in 2018 with findings that suggested Metropolitan Nashville Police Department (MNPD) traffic stops were not effective in reducing crime (Chohlas-Wood et al., 2018). These findings sparked MNPD training/policy changes. MNPD supervisors instructed officers to scale back discretionary stops and emphasized community policing strategies. In 2018, leadership altered enforcement priorities and operational behavior, including the reduction of discretionary stops and a pull back from stops for non-moving violations (e.g., broken taillights, expired tags). Commanding officers also directed traffic enforcement to focus on more immediately harmful violations (e.g., reckless driving, DUIs) rather than saturation patrols in high-crime areas. MNPD showed an overall decline in the number of traffic stops leading up to COVID-19 (Figure 1). Nashville police stops were headed in a downward direction prior to the interruption date but then dropped suddenly and continued at a low consistent rate.

Figure 1. Monthly Police Stops, Nashville, January 2015–February 2020



All Crash Severities

As indicated in Table 3, the best-fitting ARIMA model did not detect any change in level or slope in monthly fatal and serious injury crashes in association with reduced police interaction with motorists; however, the model does not fully account for the existing autocorrelations (Ljung Box $p < 0.05$), rendering the results questionable. The results of the analysis show that the monthly number of injury crashes increased following the interruption date, though this was offset somewhat by a decrease in slope that did not quite reach statistical significance. Neither the level nor the slope of total police-reported crashes was significantly affected by the interruption.

Table 3. ARIMA Results of Analyses for Monthly Crashes by Severity, Nashville, January 2015–February 2020

Outcome	Term	Estimate	Std_Error	t_value	p_value	Ljung_Box_p	Model [12]
Fatal and serious injury crashes	**ar1**	0.33	0.139	2.37	0.018	0.046	(1,0,0)(1,0,0)
	sar1	0.295	0.157	1.884	0.06		
	intercept	56.02	3.085	18.162	<0.001		
	level1	21.094	31.735	0.665	0.506		
	slope1	−0.662	0.576	−1.149	0.251		
Injury crashes	**ar1**	−0.382	0.136	−2.809	0.005	0.226	(1,1,0)(1,1,1)
	sar1	−0.149	0.294	−0.507	0.612		
	sma1	−0.737	0.595	−1.24	0.215		
	level1	194.652	85.874	2.267	0.023		
	slope1	−3.045	1.636	−1.861	0.063		
Total police-reported crashes	**ma1**	−0.721	0.104	−6.944	<0.001	0.267	(0,1,1)(0,1,1)
	sma1	−0.481	0.215	−2.242	0.025		
	level1	266.070	347.459	0.766	0.444		
	slope1	−3.826	6.262	−0.611	0.541		

Note: ar = autoregressive; ma = moving average; sar = seasonal autoregressive; sma = seasonal moving average

In aggregate, neither the total numbers of fatal and serious injury crashes nor those of injury crashes over the follow-up period differed significantly from the numbers that would have been expected under the counterfactual (i.e., no change in traffic enforcement stops). However, there were 3% more total police-reported crashes after the interruption than expected under the counterfactual (Table 4).

Table 4. Rate Ratios by Crash Severity, Nashville, August 2018–February 2020

Outcome	Rate Ratio	CI_Lower	CI_Upper	P_Value
Fatal and serious injury crashes	0.92	0.80	1.06	0.25
Injury crashes	1.01	0.97	1.06	0.57
Total police-reported crashes	1.03	1.02	1.04	**<0.001**

Note: CI = confidence interval

Figures 2 through 4 show the actual numbers of crashes per category pre- and post-interruption, as well as the estimated numbers of crashes based on the model (fitted), and the estimated crashes that would have been expected had the interruption not occurred (counterfactual) for the various crash severities.

Figure 2. Monthly Fatal and Serious Injury Crashes, Nashville, January 2015–February 2020

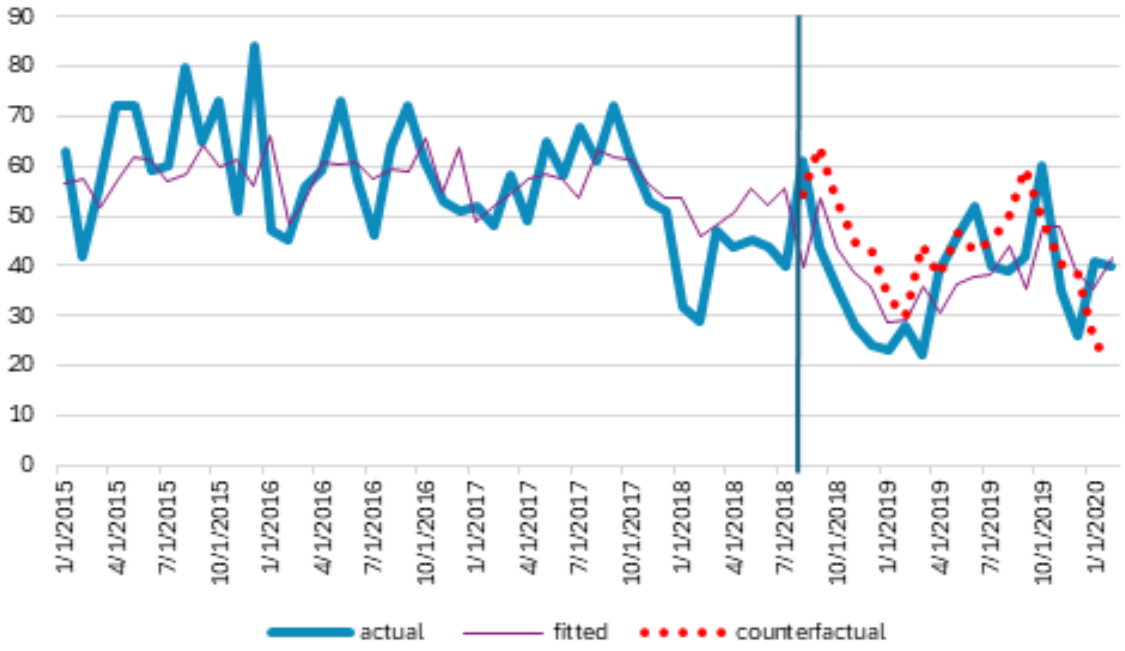


Figure 3. Monthly Injury Crashes, Nashville, January 2015–February 2020

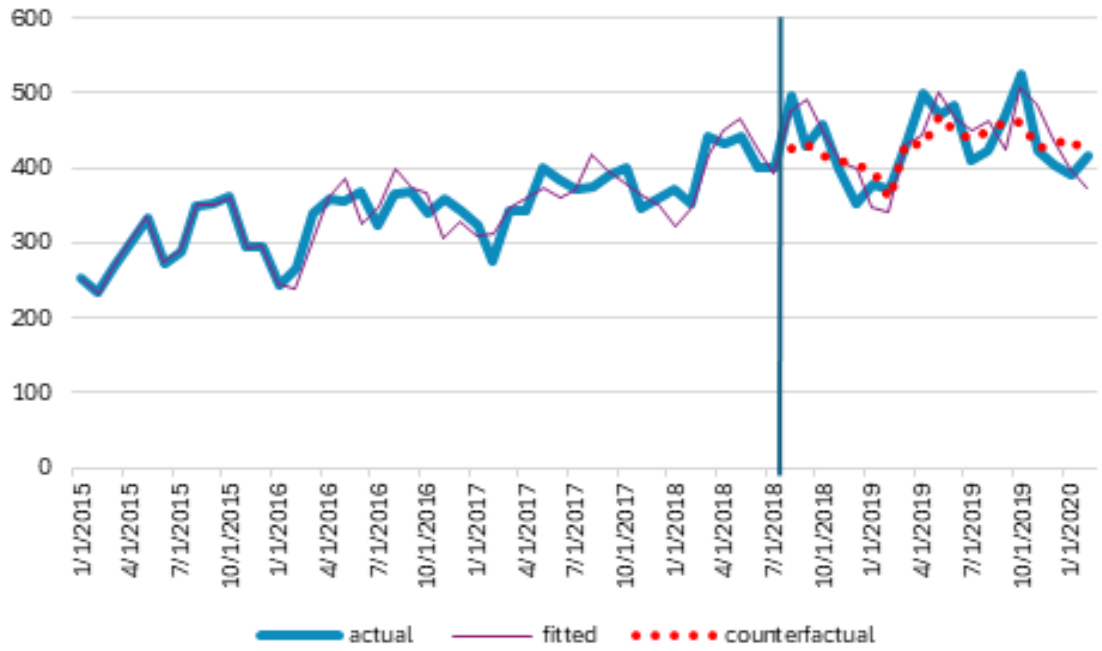
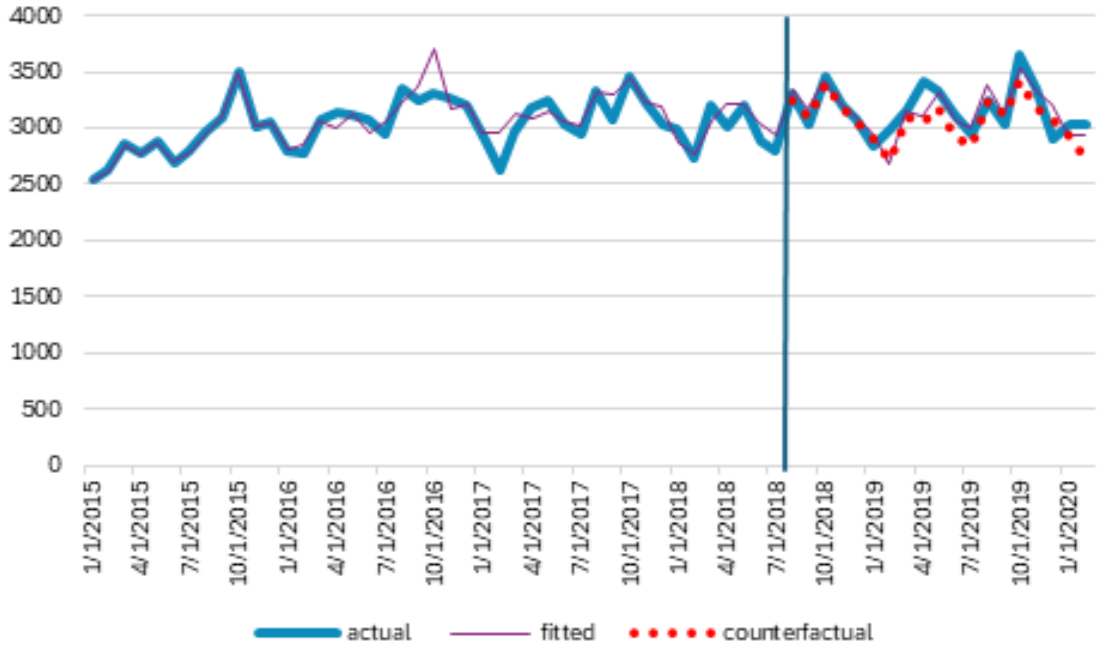


Figure 4. Monthly Total Police-Reported Crashes, Nashville, January 2015–February 2020



Speed-Related Crashes

The change in police stops was not associated with any statistically significant change in the level or slope of speed-related crashes according to the interrupted time series analysis. Table 5 shows the model and results for these analyses. However, although the decreases in both the level and slope were not statistically significant individually, the aggregate total number of speeding-related crashes after the decrease in police stops was 10% lower than would have been expected under the counterfactual condition in which there was no change in police stops (Table 6, Figure 5).

Table 5. ARIMA Results of Analyses for Monthly Speed-Related Crashes, Nashville, January 2015–February 2020

Term	Estimate	Std_Error	t_value	p_value	Ljung_Box_p	Model [12]
sar1	0.303	0.127	2.377	0.017	0.265	(0,0,0)(1,0,0)
intercept	122.676	6.969	17.604	<0.001		
level1	−4.337	14.18	−0.306	0.76		
slope1	−0.576	0.757	−0.761	0.447		

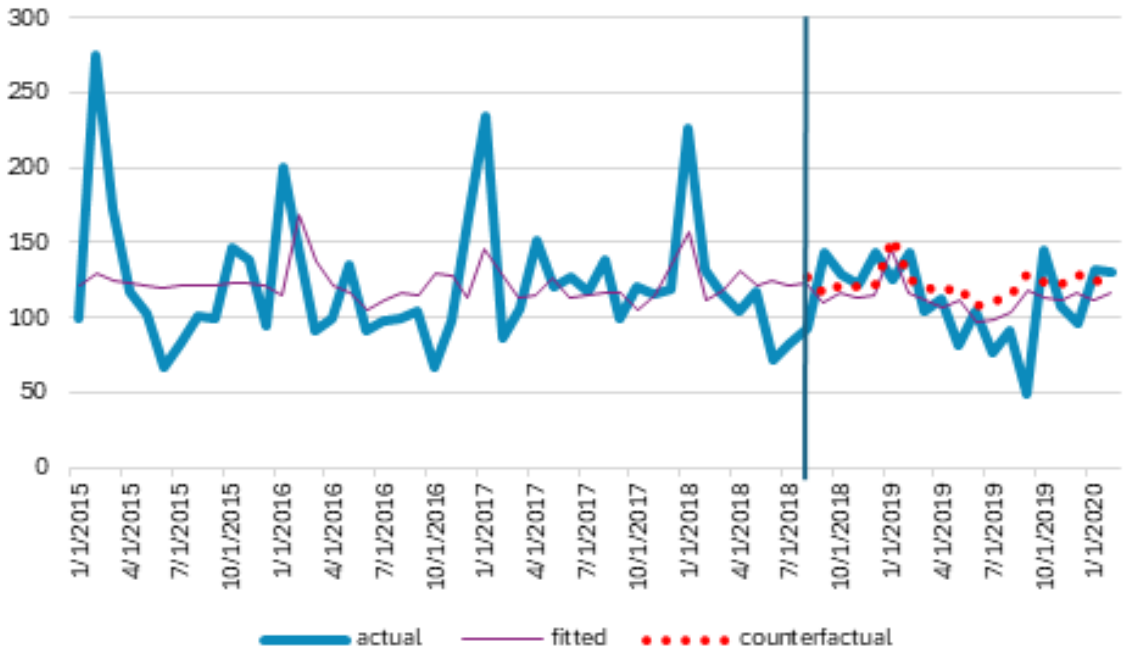
Note: sar = seasonal autoregressive

Table 6. Rate Ratio for Speed-Related Crashes, Nashville, August 2018–February 2020

Rate Ratio	CI_Lower	CI_Upper	P_Value
0.90	0.88	0.93	0.001

Note: CI = confidence interval

Figure 5. Monthly Speed-Related Crashes, Nashville, January 2015–February 2020

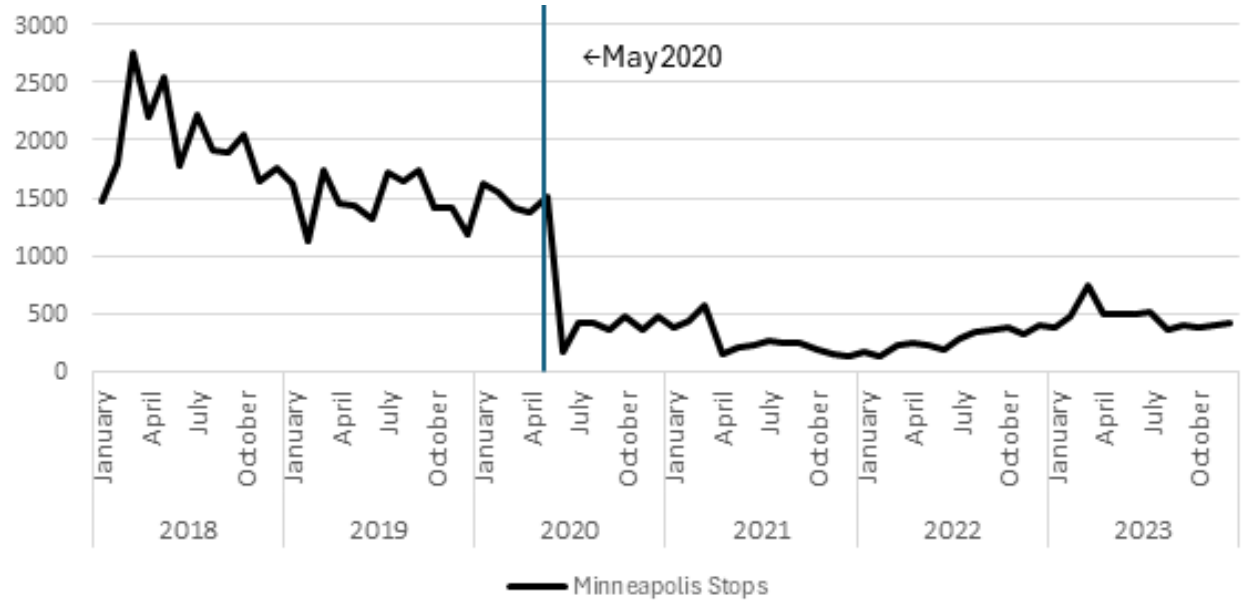


Minneapolis, Minnesota

The Minneapolis Police Department (MPD) has seen an 82% drop in traffic stops since its pre-pandemic peak (Figure 6). This decline is partly due to reform efforts and a staffing loss (the department went from 900 to 583 officers) and never recovered post-COVID-19 (Ziezulewicz, 2025). MPD has also made changes to its traffic enforcement priorities in more recent years. In 2021, MPD announced officers would stop conducting traffic stops for minor violations like expired tags, items hanging from mirrors, and broken license plate lights. One year later, state investigators found that MPD officers were more likely to use force during traffic stops involving Black drivers. As a result of a settlement agreement, traffic officers were forbidden from stopping drivers for certain low-level violations (CBS Minnesota, 2021). However, as can be seen in Figure 6, these reform efforts did not substantially impact the numbers of stops relative to those typical leading up to the pandemic.

Minneapolis stop data (Figure 6) show a gradual downward trend in stops up until June 2020 when stops dropped and stayed lower than every point in the pre-interruption period.

Figure 6. Monthly Police Stops, Minneapolis, January 2018–December 2023



All Crash Severities

The slope of monthly fatal and serious injury crashes, and injury crashes, increased significantly following the decline in police stops, while the model did not detect any changes in the respective levels of monthly crashes. The level of monthly total police-reported crashes, however, showed a decline immediately after the interruption date according to the model in the absence of a change in slope (Table 7).

Table 7. ARIMA Results for Analyses for Monthly Crashes by Severity, Minneapolis, January 2018–December 2023

Outcome	Term	Estimate	Std_Error	t_value	p_value	Ljung_Box_p	Model [12]
Fatal and serious injury crashes	ar1	0.344	0.12	2.864	0.004	0.431	(1,0,0)(0,1,1)
	sma1	-0.839	0.37	−2.27	0.023		
	level	0.16	2.071	0.077	0.938		
	slope	0.177	0.069	2.551	0.011		
Injury crashes	sar1	-1.049	0.135	−7.786	<0.001	0.173	(0,0,0)(2,1,0)
	sar2	-0.429	0.143	−3.005	0.003		
	drift	-0.618	0.229	−2.699	0.007		
	level	-7.833	4.452	−1.759	0.079		
	slope	0.684	0.227	3.021	0.003		
Total police-reported crashes	ma1	0.683	0.087	7.875	<0.001	0.47	(0,0,1)(0,0,1)
	sma1	0.355	0.123	2.894	0.004		
	intercept	788.473	31.137	25.323	<0.001		
	level	-260.314	47.806	−5.445	<0.001		
	slope	0.485	1.701	0.285	0.776		

Note: ar = autoregressive; ma = moving average; sar = seasonal autoregressive; sma = seasonal moving average

In aggregate, the total numbers of fatal and serious injury crashes and injury crashes were each 15% higher than would have been expected under the counterfactual, while the total number of police-reported crashes was 18% lower than expected under the counterfactual (Table 8, Figures 7–9).

Table 8. Rate Ratios by Crash Severity, Minneapolis, May 2020–December 2023

Outcome	Rate Ratio	CI_Lower	CI_Upper	P_Value
Fatal and serious injury crashes	1.15	1.11	1.19	0.001
Injury crashes	1.15	1.06	1.26	0.001
Total police-reported crashes	0.82	0.79	0.84	0.001

Note: CI = confidence interval

Figure 7. Monthly Fatal and Serious Injury Crashes, Minneapolis, January 2018–December 2023

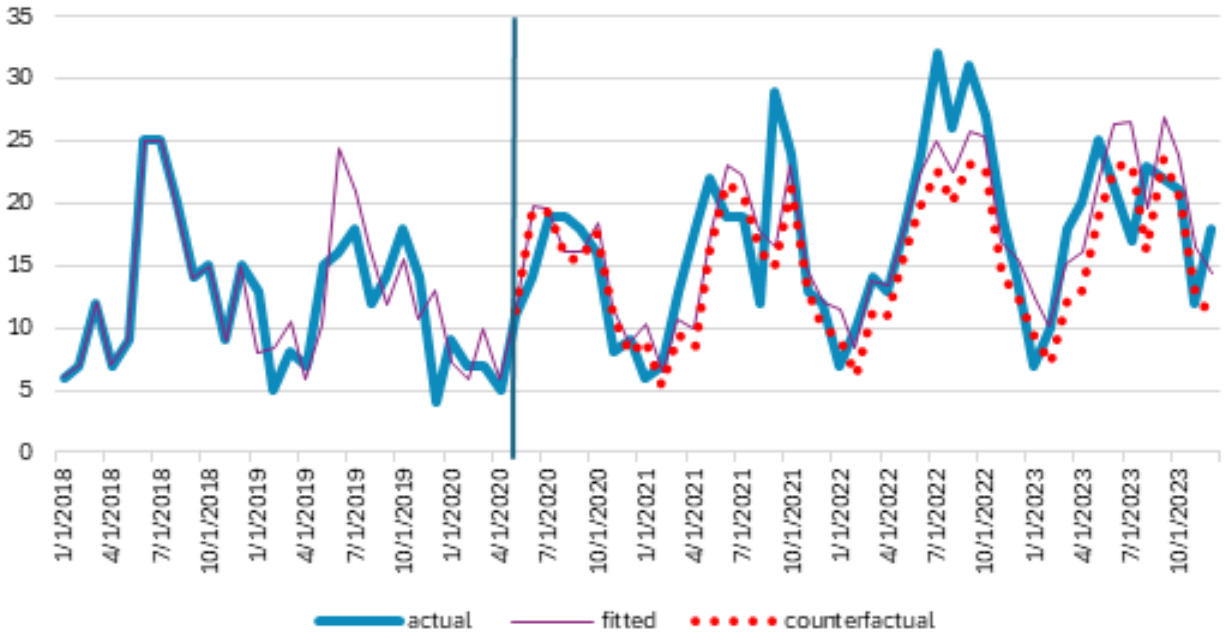


Figure 8. Monthly Injury Crashes, Minneapolis, January 2018–December 2023

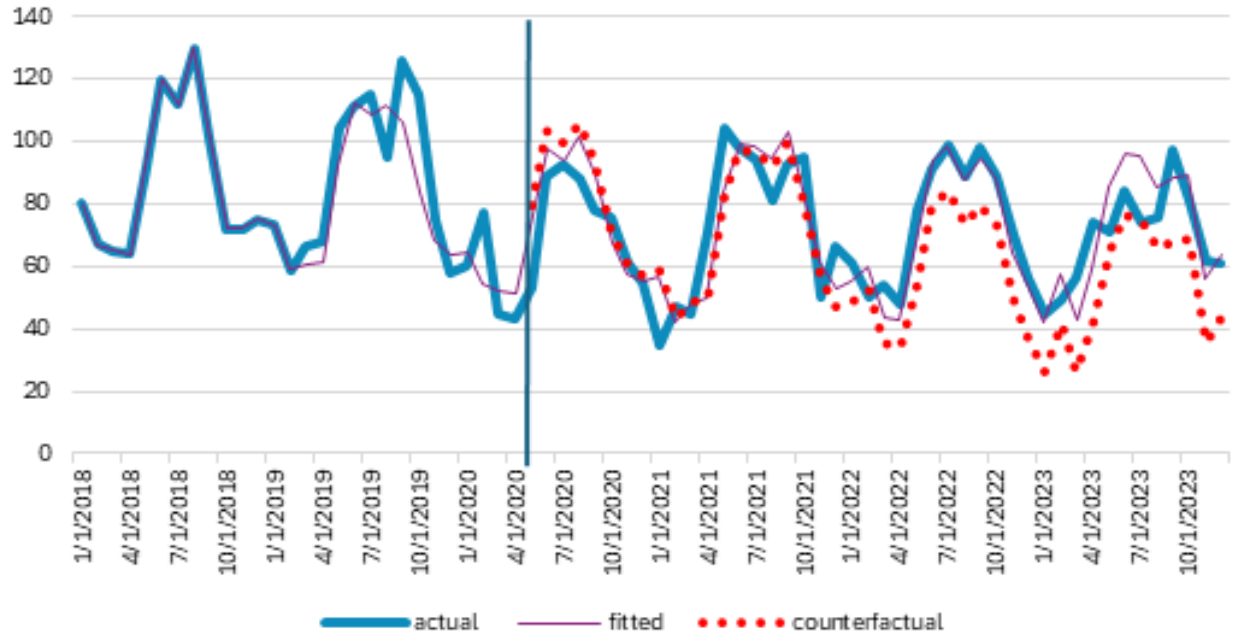
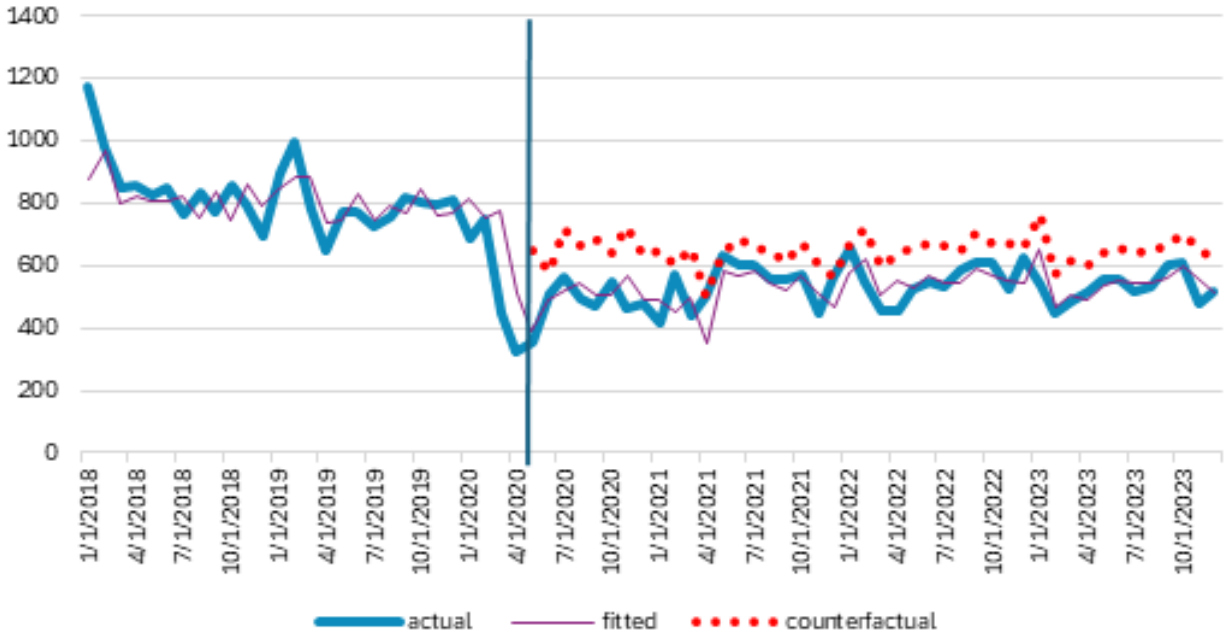


Figure 9. Monthly Total Police-Reported Crashes, Minneapolis, January 2018–December 2023



Speed-Related Crashes

Interrupted time series analyses indicated that there were significant increases in both the level and slope of speed-related crashes following the decrease in police stops (Table 9). The increase in speed-related crashes relative to the model-based counterfactual was also significant in aggregate (Table 10). This result, however, should be interpreted with caution, as the model-based counterfactual predicts a continuation of the very steep decreasing trend in speed-related crashes prior to the interruption (Figure 10), which seems improbable.[1]

[1] The type of model that was used can sometimes predict negative numbers of crashes, for example, under the counterfactual condition where a steep decreasing trend is predicted to continue. Because the actual number of crashes cannot be negative, predicted negative numbers of crashes were replaced with zeros.

Table 9. ARIMA Results for Monthly Speed-Related Crashes, Minneapolis, January 2018–December 2023

Term	Estimate	Std_Error	t_value	p_value	Ljung_Box_p	Model [12]
sma1	−0.886	0.214	−4.145	<0.001		
sma2	0.65	0.324	2.005	0.045		
drift	−1.98	0.386	−5.135	<0.001	0.119	(0,0,0)(0,1,2)
level	18.603	7.243	2.568	0.01		
slope	2.249	0.365	6.168	<0.001		

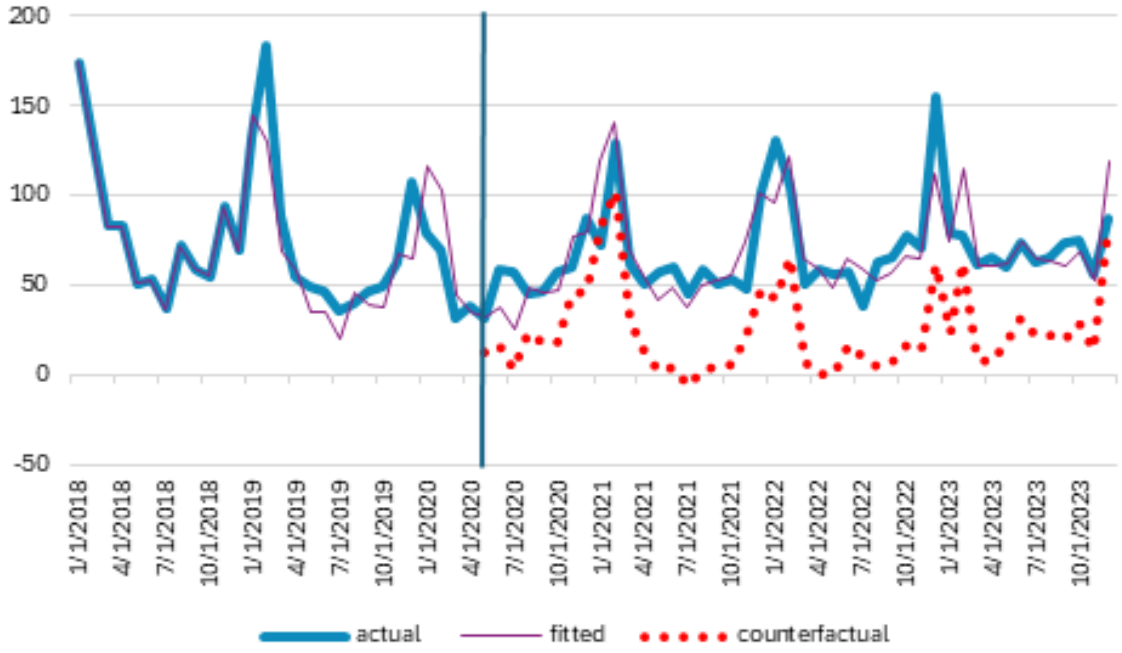
Note: sma = seasonal moving average

Table 10. Rate Ratio for Speed-Related Crashes, Minneapolis, May 2020–December 2023

Rate Ratio	CI_Lower	CI_Upper	P_Value
4.24	2.79	6.44	0.001

Note: CI = confidence interval

Figure 10. Monthly Speed-Related Crashes, Minneapolis, January 2018–December 2023

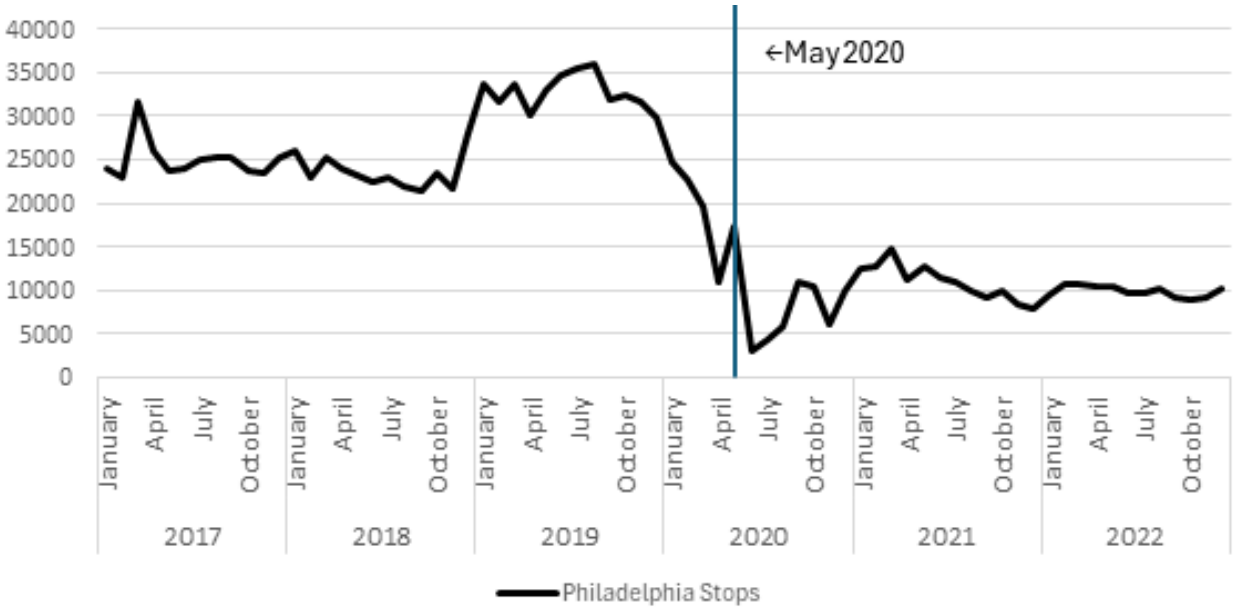


Philadelphia, Pennsylvania

Philadelphia police stops were fairly flat until 2019 when they peaked prior to a sudden decrease starting at the interruption point, coinciding with the onset of COVID-19. The number of stops continued at a low level following the interruption (Figure 11). Subsequently, in March 2022, the *Driving Equity Act* was enacted and aimed

to reduce racial disparity in traffic stops. The law also placed increased emphasis on more safety-related stops and on crime (Philadelphia City Council, 2022). The model did not include sufficient time after the policy took effect to examine any lasting impacts on police stops.

Figure 11. Monthly Police Stops, Philadelphia, January 2017–December 2022



All Crash Severities

Analyses of Philadelphia crash data indicate that following the decrease in police motorist stops, the level of fatal and serious injury crashes increased significantly, while there was no change detected in the slope. There were no significant changes in the level or slope of injury crashes. The decrease in slope for total police-reported crashes after the interruption appears marginally significant (Table 11).

Table 11. ARIMA Results for Monthly Crashes by Severity, Philadelphia, January 2017–December 2022

Outcome	Term	Estimate	Std_Error	t_value	p_value	Ljung_Box_p	Model [12]
Fatal and serious injury crashes	ar1	0.427	0.106	4.029	<0.001	0.077	(1,0,0)(0,0,0)
	intercept	31.591	2.535	12.464	<0.001		
	level	18.061	5.873	3.075	**0.002**		
	slope	−0.248	0.288	−0.86	0.39		
Injury crashes	ma1	−0.364	0.117	−3.12	0.002	0.465	(0,1,3)(2,0,0)
	ma2	0.018	0.123	0.143	0.886		
	ma3	−0.29	0.1132	−2.558	0.011		
	sar1	0.073	0.1	0.733	0.463		
	sar2	0.515	0.124	4.142	<0.001		
	level	−30.15	35.57	−0.848	0.397		
	slope	−4.352	3.743	−1.163	0.245		
Total police-reported crashes	ar1	0.238	0.12	1.985	0.047	0.079	(1,0,0)(2,0,0)
	sar1	0.057	0.107	0.535	0.593		
	sar2	0.468	0.127	3.673	<0.001		
	intercept	918.811	27.115	33.886	<0.001		
	level	−35.972	40.346	−0.892	0.373		
	slope	−3.743	1.952	−1.917	0.055		

Note: ar = autoregressive; ma = moving average; sar = seasonal autoregressive

The aggregate total number of fatal and serious injury crashes increased by 22% after the interruption date relative to the counterfactual. While the interrupted time series analysis did not indicate any significant changes in level or slope for injury crashes or total police-report crashes, the aggregate total numbers were 3% and 8% lower, respectively, than would have been expected without the interruption (Table 12, Figures 12–14).

Table 12. Rate Ratios by Crash Severity, Philadelphia, May 2020–December 2022

Outcome	Rate Ratio	CI_Lower	CI_Upper	P_Value
Fatal and serious injury crashes	1.22	1.18	1.25	0.001
Injury crashes	0.97	0.96	0.98	0.001
Total police-reported crashes	0.92	0.91	0.94	0.001

Note: CI = confidence interval

Figure 12. Monthly Fatal and Serious Injury Crashes, Philadelphia, January 2017–December 2022

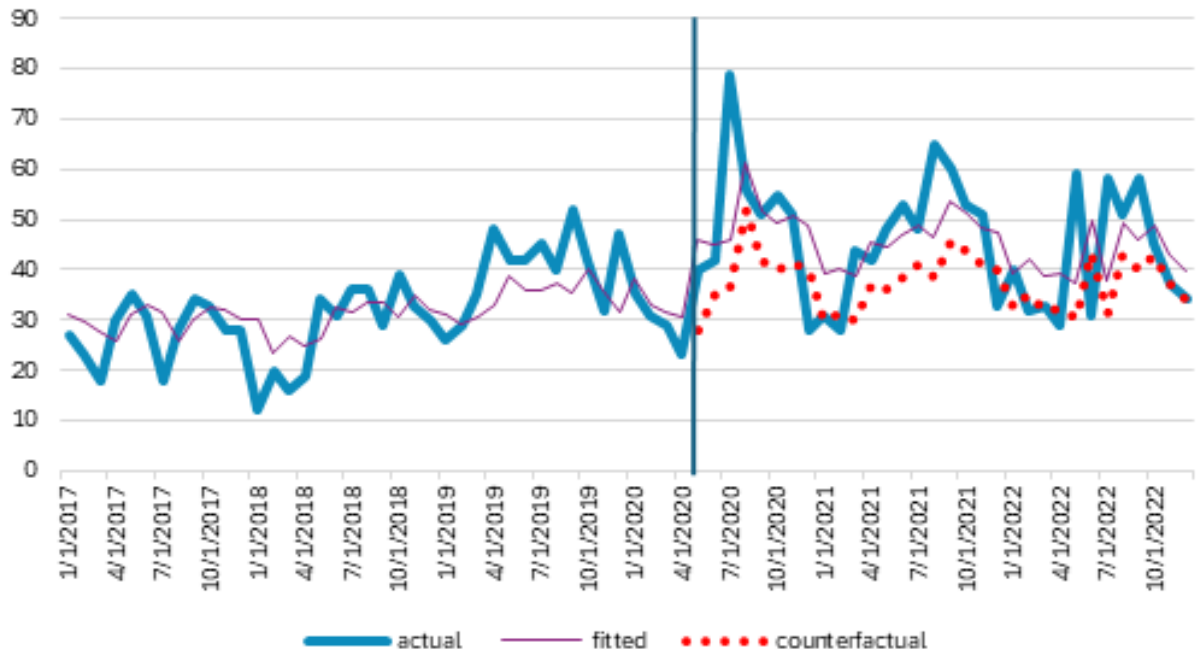


Figure 13. Monthly Injury Crashes, Philadelphia, January 2017–December 2022

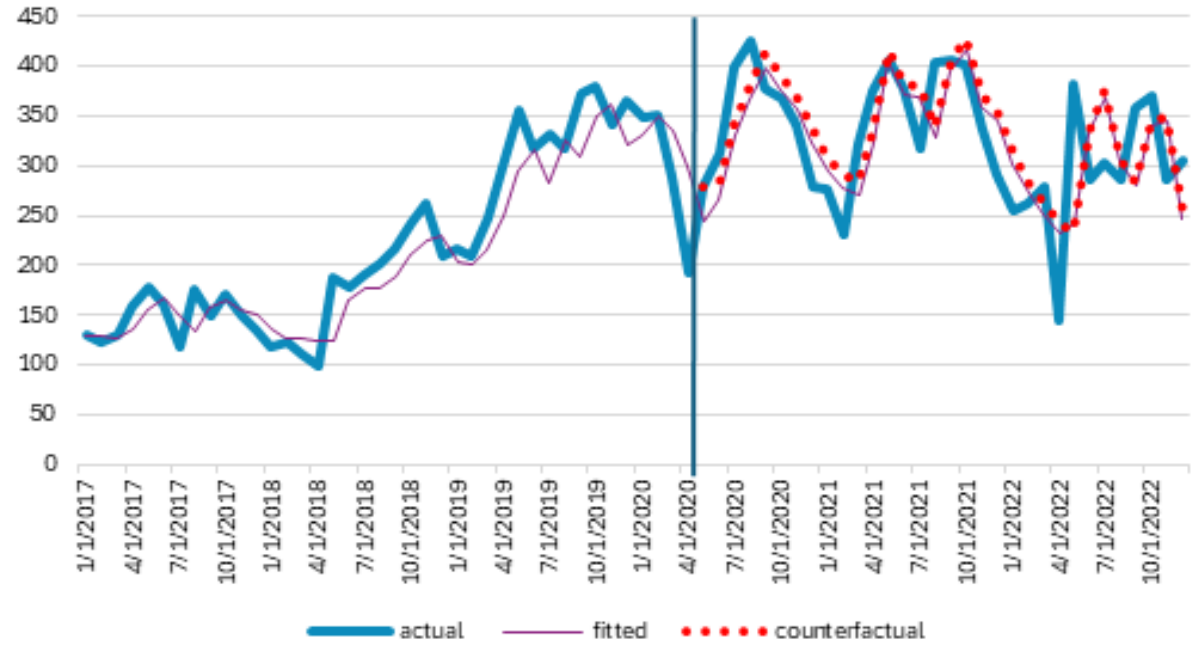
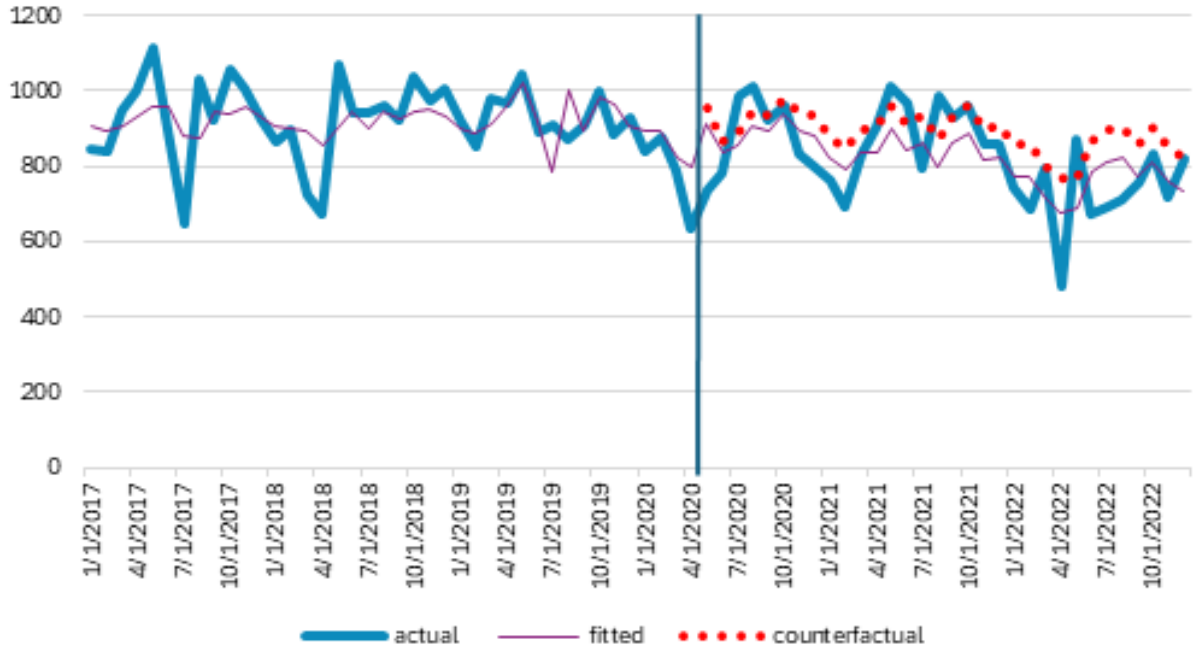


Figure 14. Monthly Total Police-Reported Crashes, Philadelphia, January 2017–December 2022



Speed-Related Crashes

Following the decrease in traffic stops, the level of speed-related crashes increased significantly, while the slope shifted downwards significantly (Table 13). In aggregate, speed-related crashes increased by 6% after the interruption (Table 14, Figure 15).

Table 13. ARIMA Results for Monthly Speed-Related Crashes, Philadelphia, January 2017–December 2022

Term	Estimate	Std_Error	t_value	p_value	Ljung_Box_p	Model [12]
sma1	0.204	0.122	1.679	0.093	0.218	(0,0,0)(0,0,1)
intercept	141.72	3.808	37.214	<0.001		
level1	30.746	7.692	3.997	**<0.001**		
slope1	−0.873	0.262	−3.326	**<0.001**		

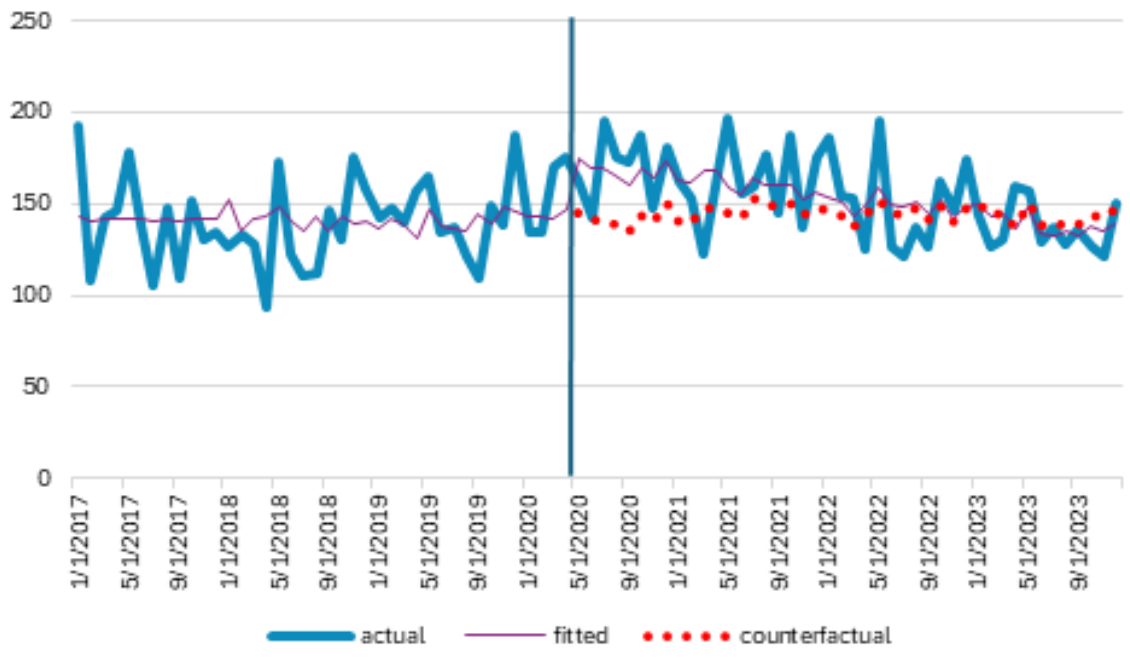
Note: sma = seasonal moving average

Table 14. Rate Ratio for Speed-Related Crashes, Philadelphia, May 2020–December 2022

Rate Ratio	CI_Lower	CI_Upper	P_Value
1.06	1.02	1.10	**0.006**

Note: CI = confidence interval

Figure 15. Monthly Speed-Related Crashes, Philadelphia, January 2017–December 2022

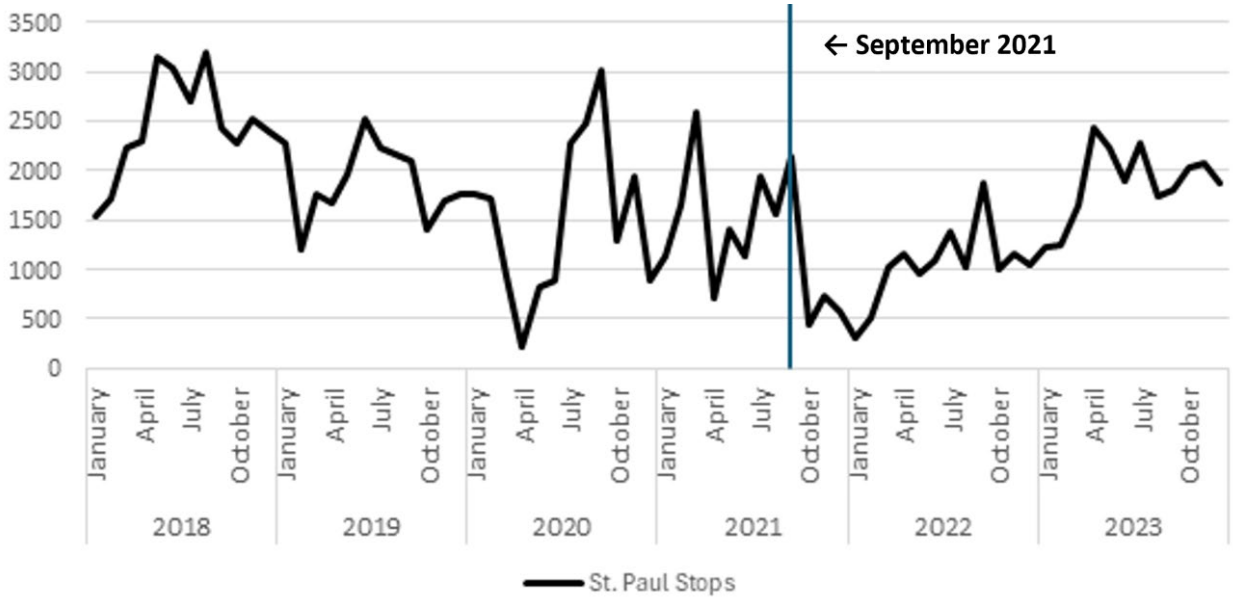


Saint Paul, Minnesota

Since September 2021, the St. Paul Police Department declared it would reduce stops for minor vehicle violations like broken taillights and dark window tints, while increasing stops for speeding, reckless driving, and DUI offenses. An analysis of four years of data (April 2018–April 2022), looking at 200,000 traffic stops, found the department did reduce stops for minor vehicle violations by more than 20%. At the same time, officers increased stops for more serious offenses such as speeding and DUI, effectively keeping stops relatively consistent except for a drop in 2020 lasting a short period (Figure 16).

Monthly police stops in Saint Paul were somewhat erratic with many ups and downs. A single interruption point was selected despite there being two distinct drops in police stops. February 2020 presented a potential interruption point, but preference was given to a point that was after the COVID-19 timeframe to explore any effects. Two interruption points are possible, but the dates of the data made that option not advisable as there would be limited data before and after each interruption. Thus, September 2021 was chosen as the interruption point (i.e., at the second drop in police stops).

Figure 16. Monthly Police Stops, Saint Paul, January 2018–December 2023



All Crash Severities

The level of fatal and serious injury crashes increased significantly after the decrease in police stops, but with no change in the slope. No significant changes were detected in the levels or slopes for injury crashes or total police-reported crashes (Table 15).

Table 15. ARIMA Results for Monthly Crashes by Severity, Saint Paul, January 2018–December 2023

Outcome	Term	Estimate	Std_Error	t_value	p_value	Ljung_Box_p	Model [12]
Fatal and serious injury crashes	ar1	1.609	0.14	11.52	<0.001	0.947	(2,0,3)(0,0,0)
	ar2	−0.861	0.132	−6.539	<0.001		
	ma1	−1.693	0.175	−9.684	<0.001		
	ma2	1.178	0.19	6.186	<0.001		
	ma3	−0.285	0.124	−2.306	0.021		
	intercept	5.603	0.273	20.545	<0.001		
	level	2.062	0.956	2.156	**0.031**		
	slope	−0.068	0.054	−1.246	0.213		
Injury crashes	ma1	0.347	0.139	2.49	0.013	0.553	(0,0,3)(0,1,1)
	ma2	0.148	0.147	1.006	0.314		
	ma3	0.517	0.144	3.58	<0.001		
	sma1	−0.669	0.196	−3.415	<0.001		
	level	−0.976	4.273	−0.228	0.819		
	slope	−0.091	0.239	−0.383	0.702		
Total police-reported crashes	ar1	0.441	0.162	2.718	0.007	0.675	(1,1,1)(1,0,0)
	ma1	−0.886	0.098	−9.045	<0.001		
	sar1	0.259	0.12	2.162	0.031		
	level	35.363	39.711	0.891	0.373		
	slope	−2.836	2.955	−0.96	0.337		

Note: ar = autoregressive; ma = moving average; sma = seasonal moving average; sar = seasonal autoregressive

In aggregate, consistent with the interrupted time series analysis results for the level of fatal and serious injury crashes, the number of these crashes post-interruption was 25% more than would have been expected without the interruption (Table 16, Figures 17–19). However, while the interrupted time series analysis did not indicate a change in the level of injury crashes or police-reported crashes, the respective rate ratios suggest that both decreased in aggregate following the interruption, but only the decrease in injury crashes was significant.

Table 16. Rate Ratios by Crash Severity, Saint Paul, September 2021–December 2023

Outcome	Rate Ratio	CI_Lower	CI_Upper	P_Value
Fatal and serious injury crashes	1.25	1.17	1.33	**0.001**
Injury crashes	0.97	0.97	0.98	**0.001**
Police-reported crashes	0.98	0.97	1.00	0.10

Note: CI = confidence interval

Figure 17. Monthly Fatal and Serious Injury Crashes, Saint Paul, January 2018–December 2023

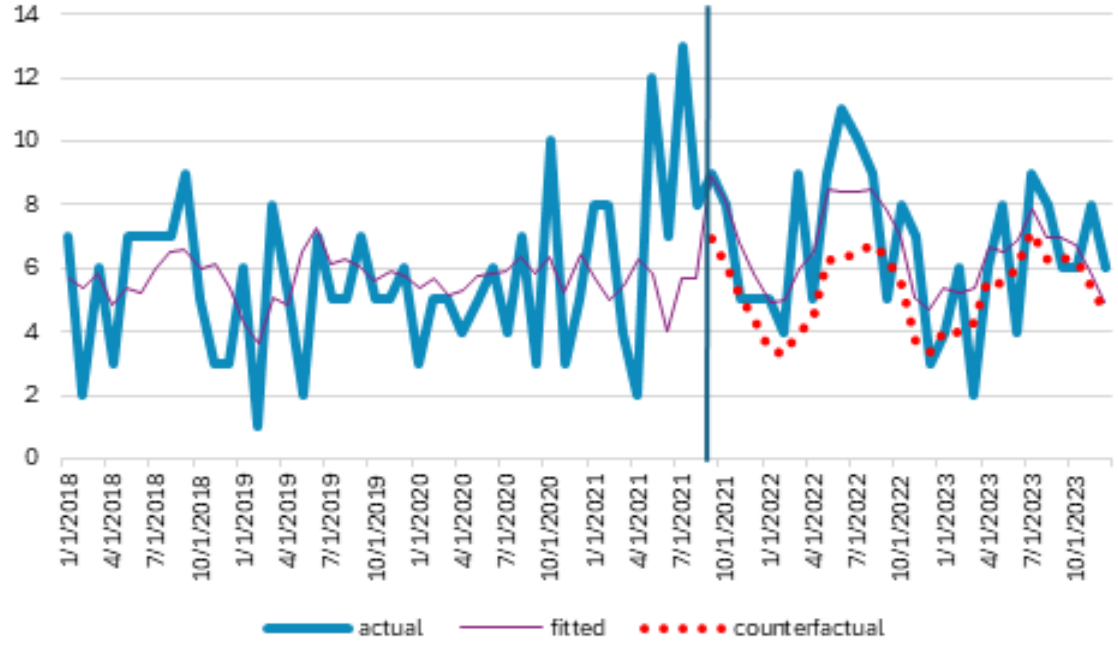


Figure 18. Monthly Injury Crashes, Saint Paul, January 2018–December 2023

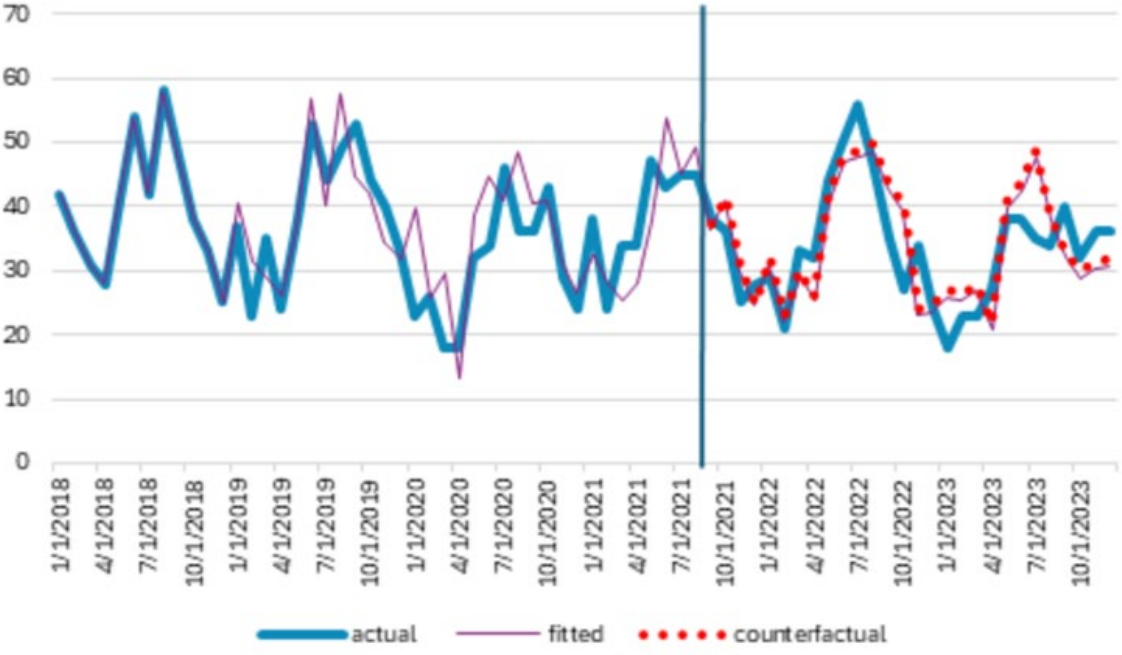
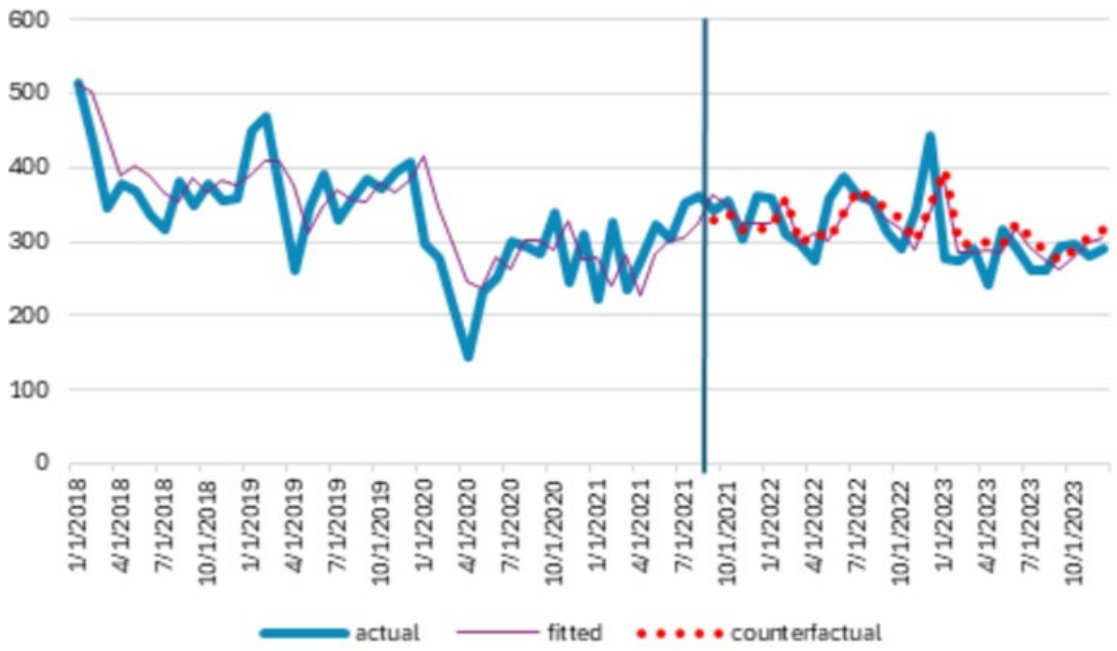


Figure 19. Monthly Total Police-Reported Crashes, Saint Paul, January 2018–December 2023



Speed-Related Crashes

Saint Paul speed-related crash analyses showed that there was a decrease in slope after the interruption date (Table 17). In aggregate, the number of speed-related crashes after the interruption was 12% less than the counterfactual (Table 18, Figure 20).

Table 17. ARIMA Results for Monthly Speed-Related Crashes, Saint Paul, January 2018–December 2023

Term	Estimate	Std_Error	t_value	p_value	Ljung_Box_p	Model [12]
ma1	0.334	0.107	3.125	0.002	0.25	(0,0,1)(1,0,1)
sar1	0.845	0.168	5.028	<0.001		
sma1	−0.544	0.279	−1.95	0.052		
intercept	54.606	5.89	9.271	<0.001		
level	9.725	8.944	1.087	0.277		
slope	−1.123	0.497	−2.261	0.024		

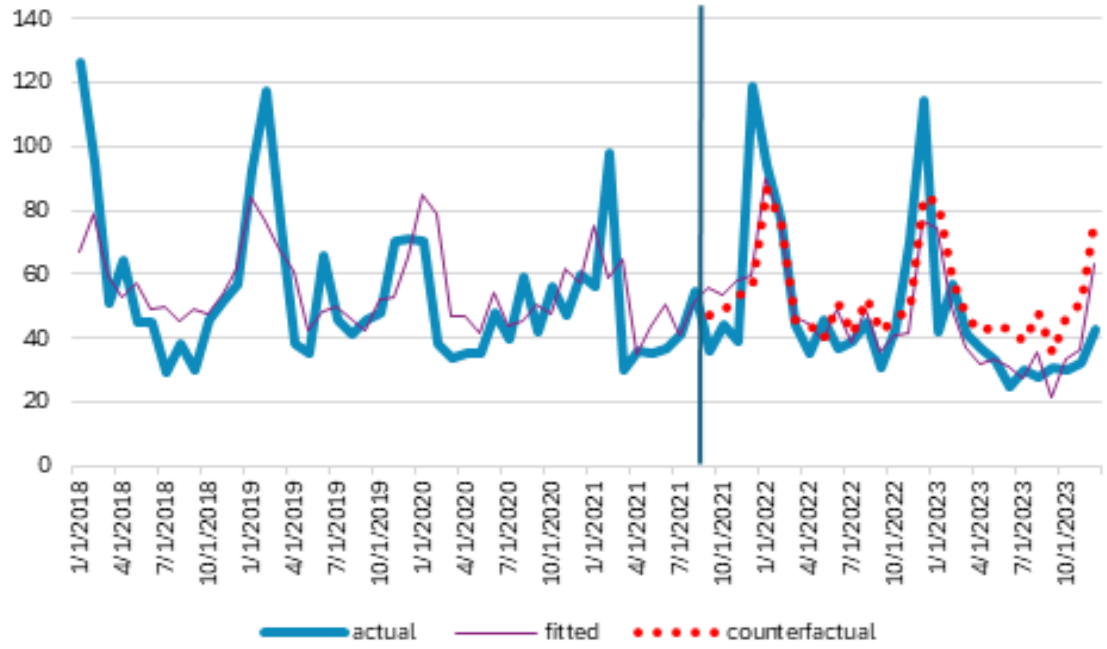
Note: ma = moving average; sar = seasonal autoregressive; sma = seasonal moving average

Table 18. Rate Ratio for Speed-Related Crashes, Saint Paul, September 2021–December 2023

Rate Ratio	CI_Lower	CI_Upper	P_Value
0.88	0.79	0.98	0.023

Note: CI = confidence interval

Figure 20. Monthly Speed-Related Crashes, Saint Paul, January 2018–December 2023



Chicago, Illinois

Chicago was selected due to its unique pattern of traffic stops. Chicago's stops went down in 2020, as did many other locations, but recovered very quickly (Figure 21). It is of some interest to see if the relatively brief decline, during a time period when there were very few drivers, impacted the number of crashes following the initial decline. Illinois Uniform Traffic Citation (UTC) data indicate that traffic stops in Chicago significantly decreased by a large amount immediately after the onset of the COVID-19 pandemic. After that decline, the UTC data show increases for the years 2021, 2022, and 2023. UTC data show that Chicago PD recorded approximately 53,500 traffic stops in 2023, returning to a number closer to pre-COVID-19 stop levels.

For Chicago, the stop data range from January 2018 to December 2022 with the interruption placed at January 2020 (Figure 21). The pre-interruption period showed slightly increasing stops. Post-interruption, the number of stops suddenly drop, followed by a gradual increase.

Figure 21. Monthly Police Stops, Chicago, January 2018–December 2022

All Crash Severities

The results of the interrupted time series analysis for the initial drop in police traffic stops in Chicago (which was followed by a gradual increase in stops) indicated a rise in the level of fatal and serious injury crashes and an increasing slope of those crashes. However, the Ljung Box score indicates that the autocorrelations were not fully controlled, rendering this result uncertain. There was no indication of an impact of the change in police interactions on the level or slope of injury crashes, and the autocorrelations in the injury crash data were adequately controlled. The model for total police-reported crashes indicates a decrease in the level with an increasing slope post-interruption (Table 19).

Table 19. ARIMA Results for Monthly Crashes by Severity, Chicago, January 2018–December 2022

Outcome	Term	Estimate	Std_Error	t_value	p_value	Ljung_Box_p	Model [12]
Fatal and serious injury crashes	ma1	0.833	0.157	5.294	**<0.001**	**0.017**	(0,0,2)(1,1,0)
	ma2	0.501	0.179	2.802	**0.005**		
	sar1	−0.582	0.121	−4.789	**<0.001**		
	drift	−2.675	1.04	−2.572	**0.01**		
	Level	35.893	16.597	2.163	**0.031**		
	Slope	2.357	1.061	2.222	**0.026**		
Injury crashes	ar1	0.691	0.142	4.865	**<0.001**	0.123	(2,0,0)(1,1,0)
	ar2	−0.333	0.139	−2.4	**0.016**		
	sar1	−0.463	0.137	−3.39	**<0.001**		
	Level	−80.555	46.898	−1.718	0.086		
	Slope	3.929	2.0459	1.92	0.0548		
Total police-reported crashes	ma1	0.735	0.112	6.538	**<0.001**	0.79	(0,0,2)(1,0,0)
	ma2	0.433	0.13	3.343	**<0.001**		
	sar1	0.265	0.125	2.113	**0.035**		
	intercept	9214.365	319.99	28.796	**<0.001**		
	Level	−1892.8	466.571	−4.057	**<0.001**		
	Slope	43.355	20.828	2.082	**0.037**		

Note: ar = autoregressive; ma = moving average; sar = seasonal autoregressive

The rate ratio for fatal and serious injury crashes indicates the aggregate number of these crashes post-interruption was 16% higher than expected; however, as noted previously, this result should be treated with caution because the Ljung-Box score indicated that the model did not control for all of the autocorrelations (Table 20, Figure 22). Injury crashes did not diverge from what the model predicted post-interruption (Figure 23). While the interrupted time series indicated a decrease in the level but an increase in slope of police-reported crashes, the aggregate number of these crashes was 5% lower than expected post-interruption. Figure 24 shows that the monthly number of crashes was lower than expected throughout 2020 and early 2021; however, the monthly crash rate converged with the counterfactual by mid- to late-2021.

Table 20. Rate Ratios by Severity, Chicago, January 2020–December 2022

Outcome	Rate Ratio	CI_Lower	CI_Upper	P_Value
Fatal and serious injury crashes	1.16	1.14	1.18	**0.001**
Injury crashes	1.00	0.98	1.03	0.554
Total police-reported crashes	0.95	0.93	0.97	**0.001**

Note: CI = confidence interval

Figure 22. Monthly Fatal and Serious Injury Crashes, Chicago, January 2018–December 2022

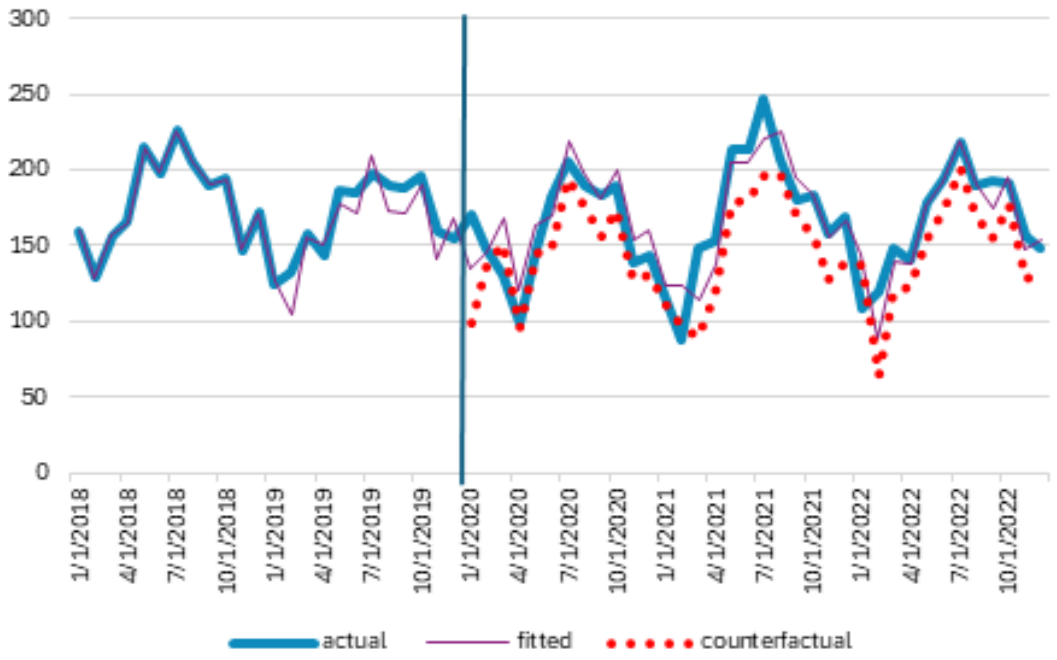


Figure 23. Monthly Injury Crashes, Chicago, January 2018–December 2022

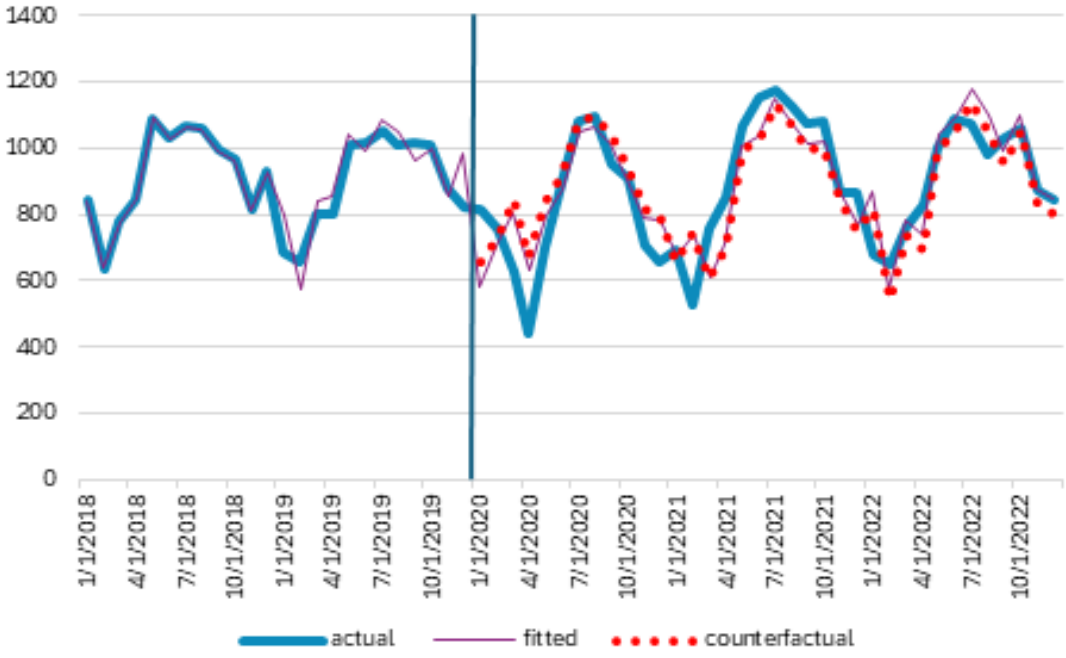
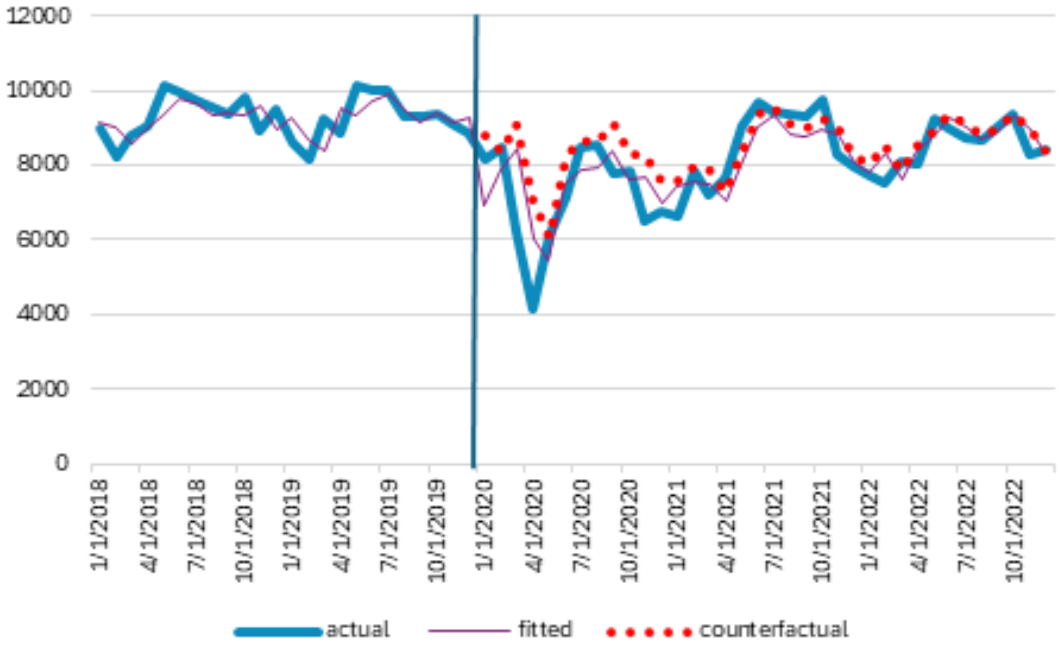


Figure 24. Monthly Police-Reported Crashes, Chicago, January 2018–December 2022



Speed-Related Crashes

ARIMA models were also run for speed-related crashes. A significant decrease in the level of speed-related crashes followed the interruption point, but no change was detected in slope (Table 21). In aggregate, speed-related crashes were 14% lower post-interruption than expected had the interruption not occurred (Table 22, Figure 25).

Table 21. ARIMA Results for Monthly Speed-Related Crashes, Chicago, January 2018–December 2022

Term	Estimate	Std_Error	t_value	p_value	Ljung_Box_p	Model [12]
sar1	−0.505	0.142	−3.567	<0.001		
level1	−51.314	17.99	−2.852	0.004	0.877	(0,0,0)(1,1,0)
slope1	−0.681	0.811	−0.839	0.402		

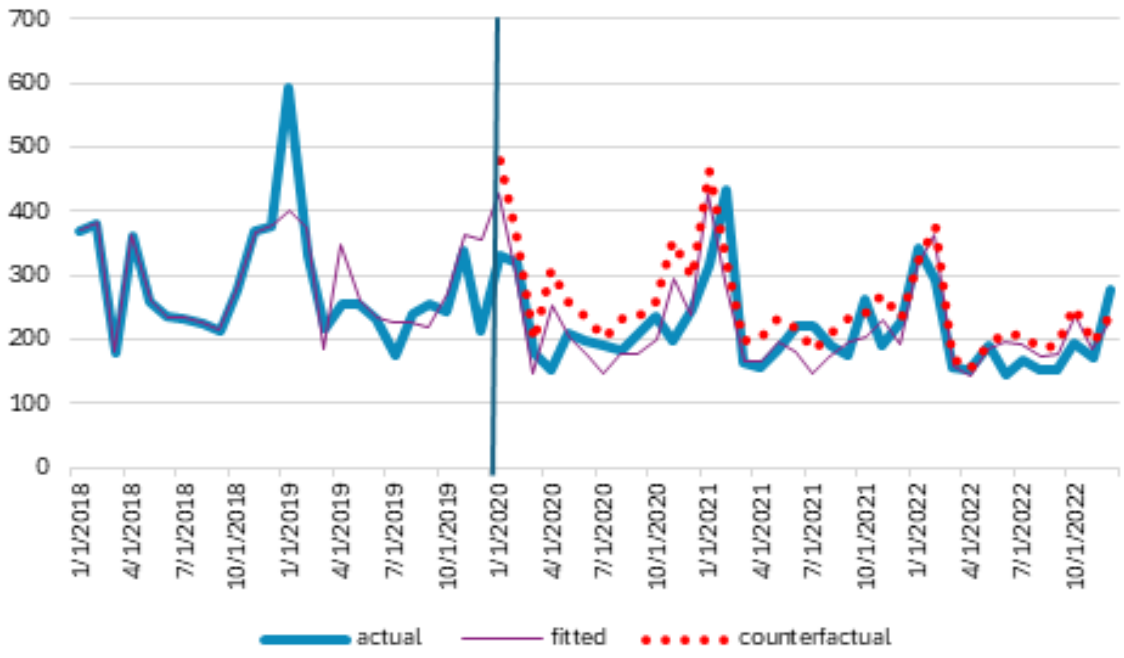
Note: sar = seasonal autoregressive

Table 22. Rate Ratio for Speed-Related Crashes, Chicago, January 2020–December 2022

Rate Ratio	CI_Lower	CI_Upper	P_Value
0.86	0.82	0.90	0.001

Note: CI = confidence interval

Figure 25. Monthly Speed-Related Crashes, Chicago, January 2018–December 2022

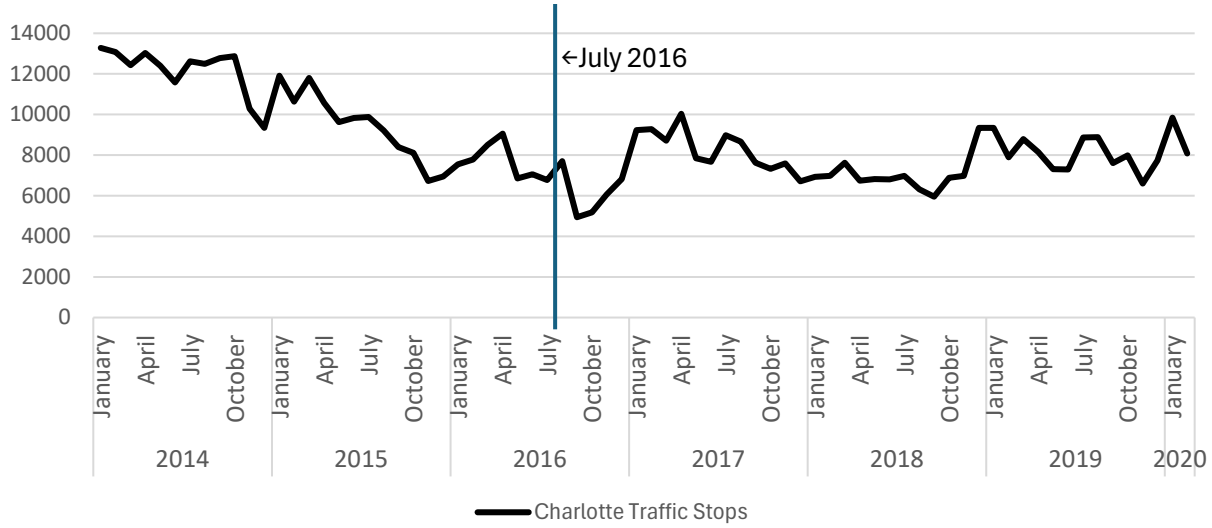


Charlotte, North Carolina

Charlotte was selected because the number of traffic stops remained relatively consistent from 2016 to 2020 (see Figure 26). Over the past decade, traffic stops conducted by the Charlotte-Mecklenburg Police Department trended downward, but not to the extent of the decrease seen in some other locations up until the COVID-19 pandemic. Charlotte was a location that showed no evident reduction in traffic stops and could be examined with data exclusively prior to 2020. From 2016 to 2020, the rate of stops was somewhat consistent with a short-lived decline at the start of the pandemic. The data show an overall downward trend, but that trend was not severe.

Charlotte data did not have an obvious point at which the stop data declined precipitously. There was a preexisting downward trend in stops prior to the interruption (and other adjacent interruption points would yield similar modeling outcomes). Following the July 2016 interruption point, there was a brief increase in stops followed by a fairly stable rate of stops.

Figure 26. Monthly Police Stops, Charlotte, January 2014–February 2020



All Crash Severities

The results from the ARIMA on Charlotte indicate that there were no changes detected in level or slope of fatal and serious injury crashes or injury crashes after the moderate change in stops by police (Table 23). The data show a significant decrease in the level but not the slope of total police-reported crashes after the interruption date.

Table 23. ARIMA Results for Monthly Crashes by Severity, Charlotte, January 2014–February 2020

Outcome	Term	Estimate	Std_Error	t_value	p_value	Ljung_Box_p	Model [12]
Fatal and serious injury crashes	intercept	13.267	0.752	17.641	<0.001	0.224	(0,0,0)(0,0,0)
	level	2.067	1.47	1.406	0.16		
	slope	−0.095	0.049	−1.935	0.053		
Injury crashes	ar1	0.445	0.11	4.045	<0.001	0.61	(1,0,0)(1,0,0)
	sar1	0.39	0.137	2.847	0.004		
	intercept	167.03	8.478	19.701	<0.001		
	level	8.642	12.307	0.702	0.483		
	slope	−0.247	0.438	−0.564	0.573		
Total police-reported crashes	ma1	−0.642	0.128	−5.009	<0.001	0.096	(0,1,1)(0,1,2)
	sma1	−0.835	0.195	−4.278	<0.001		
	sma2	0.109	0.227	0.478	0.633		
	level	−245.557	121.361	−2.023	**0.043**		
	slope	−23.16	13.181	−1.757	0.079		

Note: ar = autoregressive; sar = seasonal autoregressive; sma = seasonal moving average

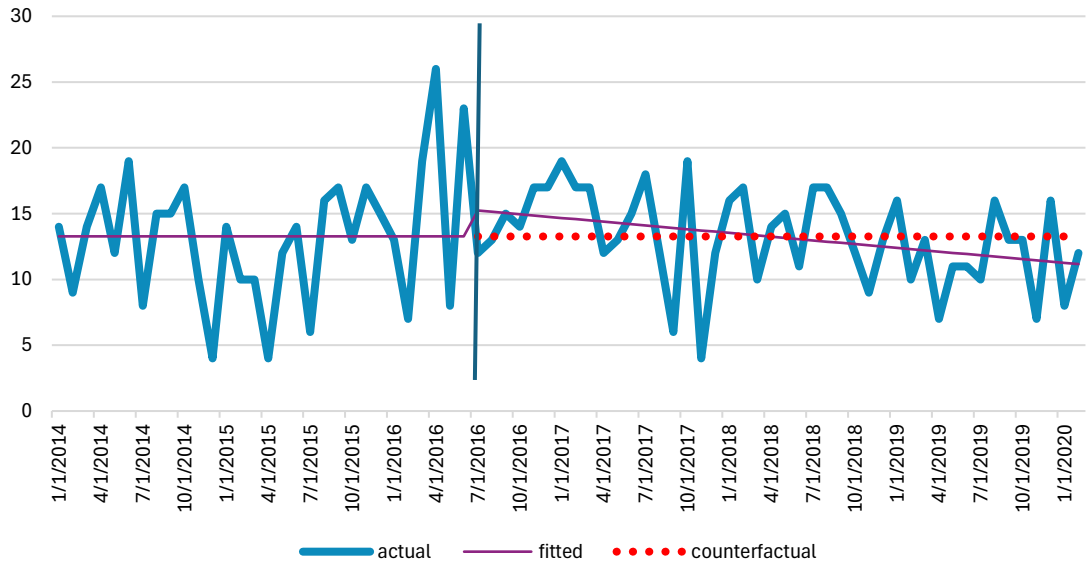
Similarly, only total police-reported crashes diverged from the counterfactual following the interruption, decreasing by 2% (Table 24, Figures 27–29).

Table 24. Rate Ratios by Crash Severity, Charlotte, July 2016–February 2020

Outcome	Rate Ratio	CI_Lower	CI_Upper	P_Value
Fatal and serious injury crashes	0.99	0.94	1.04	0.741
Injury crashes	1.01	1.00	1.01	0.102
Total police-reported crashes	0.98	0.97	0.99	**0.001**

Note: CI = confidence interval

Figure 27. Monthly Fatal and Serious Injury Crashes, Charlotte, January 2014–February 2020



Note: When there are a minimal number of crashes ARIMA modeling can have a hard time getting detailed estimates due to floor effects and limited variability.

Figure 28. Monthly Injury Crashes, Charlotte, January 2014–February 2020

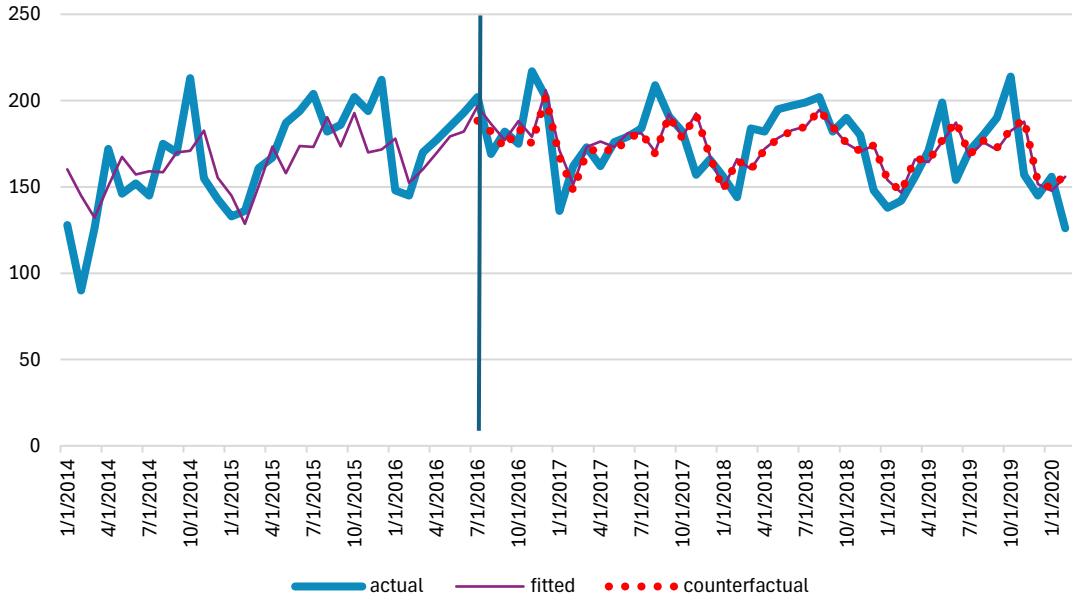
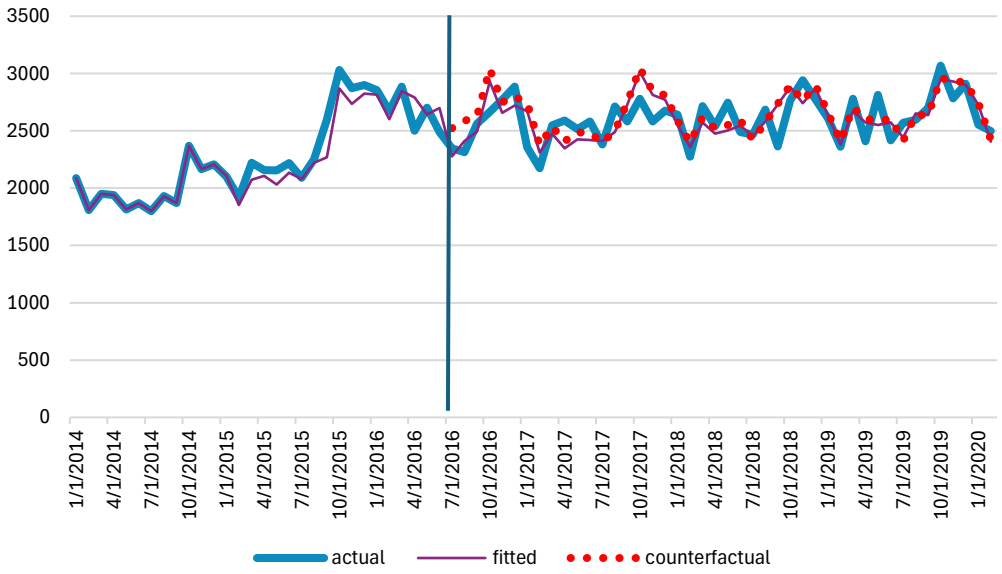


Figure 29. Monthly Total Police-Reported Crashes, Charlotte, January 2014–February 2020



Speed-Related Crashes

The interrupted time series analysis did not indicate significant change in the level or slope of Charlotte speed-related crashes post-interruption (Table 25). In aggregate, however, the number of speed-related crashes was 5% higher post-interruption than expected had the interruption not occurred (Table 26, Figure 30).

Table 25. ARIMA Results for Monthly Speed-Related Crashes, Charlotte, January 2014–February 2020

Term	Estimate	Std_Error	t_value	p_value	Ljung_Box_p	Model [12]
ar1	0.405	0.116	3.487	<0.001		
sar1	0.344	0.136	2.528	0.012		
intercept	411.099	18.7	21.984	<0.001	0.187	(1,0,0)(1,0,0)
level	21.796	29.227	0.746	0.456		
slope	1.044	0.982	1.064	0.287		

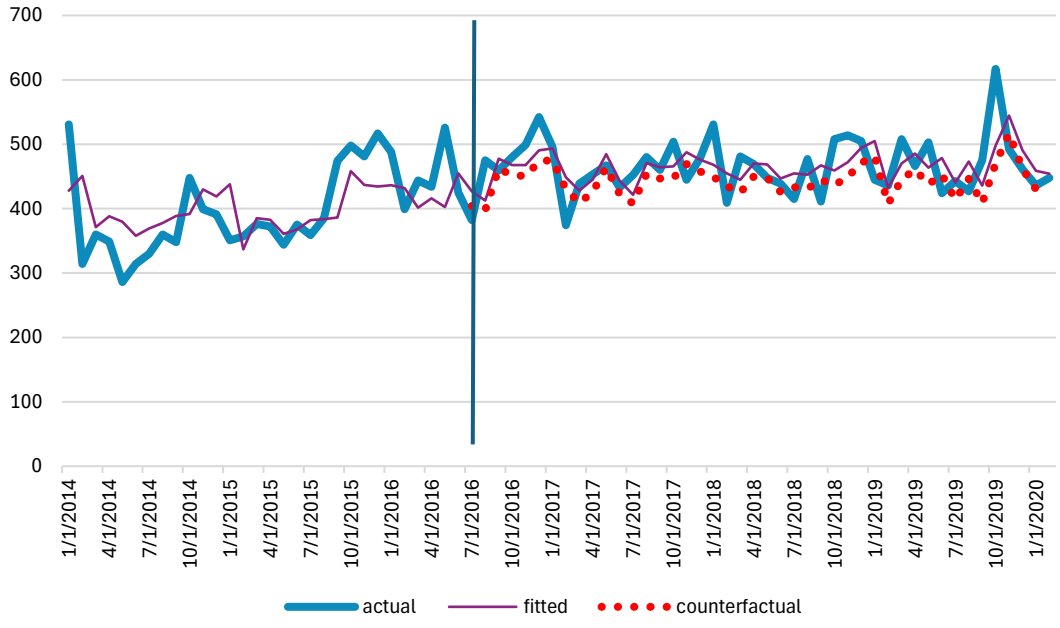
Note: ar = autoregressive; sar = seasonal autoregressive

Table 26. Rate Ratio for Speed-Related Crashes, Charlotte, July 2016–February 2020

Rate Ratio	CI_Lower	CI_Upper	P_Value
1.05	1.04	1.05	**0.001**

Note: CI = confidence interval

Figure 30. Monthly Speed-Related Crashes, Charlotte, January 2014–February 2020



Discussion & Limitations

Across the six cities examined, there was not a consistent pattern in results. Two of the three jurisdictions that experienced the most sustained reductions in police motorist stops, Minneapolis and Philadelphia, demonstrated some evidence of post-interruption increases in crashes, particularly for the crashes involving behaviors that prior research has shown to be sensitive to enforcement (i.e., speed-related crashes; Kirley et al., 2023).

In Minneapolis, both fatal and serious injury crashes and known-injury crashes began trending upward after stops declined sharply. Speed-related crashes also increased; however, caution should be used when interpreting this result. The data already showed a steep decreasing trend in speed-related crashes before the decrease in police stops, and the model predicted a continued decrease in speed-related crashes to a near-zero level, which seems unlikely. While the results indicate an increase in speed-related crashes, this was relative to that prediction, tempering the interpretation of this result. In Philadelphia, fatal and serious injury crashes increased significantly, speed-related crashes increased as well but to a lesser degree, while known-injury crashes decreased slightly but significantly. However, in Nashville, which also experienced large and sustained reductions in stops, there is little evidence that fatal and serious injury crashes or known-injury crashes increased more than if there had been no change in stops, but there was a decrease in speed-related crashes and a small increase in total police-reported crashes.

In the jurisdictions with smaller or inconsistent declines in enforcement (St. Paul, Chicago, and Charlotte), there were also mixed results. St. Paul saw a significant reduction in speed-related crashes, but also saw a significant increase in fatal and serious injury crashes. Chicago saw an increase in both fatal and serious injury crashes and speed-related crashes; however, like with speed-related crashes in Minneapolis, caution should be applied to the interpretation of these results. Charlotte saw a slight increase in total police-reported crashes and a decrease in speed-related crashes.

Assessing the impact of reductions in police–motorist interactions on road safety is methodologically challenging. Ideal approaches, such as controlled interrupted time-series methods, were not feasible due to data availability. The current case study approach allowed for an in-depth examination of six locations; however, this approach is subject to some meaningful shortcomings. Primarily, the analytic approach was unable to completely disentangle the impact of changes in enforcement levels from the impacts of other factors that transpired around the same time that may have influenced safety outcomes. In particular, the COVID-19 pandemic was associated with an increase in both speeding behavior (Wang & Cicchino, 2023) and fatal crash rates (Tefft, 2024). The interruption dates in three of the cities (Chicago, Philadelphia, and Minneapolis) coincided closely (within a few months) of the beginning of the COVID-19 pandemic, and

a fourth (Saint Paul) was later but during a period still very much influenced by the lingering impact of the pandemic. In Nashville and Charlotte, where the entire study period was prior to and thus not influenced by the pandemic, results were markedly different from those in the other four cities.

Models also did not account for the introduction of other countermeasures aimed at improving safety outcomes, including red light and speed cameras. For instance, Philadelphia began implementing speed cameras in January 2020 (data compiled from reputable data sources by the Insurance Institute for Highway Safety), shortly before the May 2020 interruption date used in the analyses.

Another limitation is that there were no control groups, due to a lack of available data from comparable cities without reductions in police stops in the relevant time periods. Examining areas that were similar yet had different changes in stops could also help to better understand if the rate of stops had a causal impact on severe crashes. Future studies may want to focus on other types of crashes, like unbelted drivers (day or night) or DUI to see if those other high-risk behaviors could be impacted by enforcement (Zwicker et al., 2007; Chaudhary et al, 2005; Solomon, et al., 2009).

Additionally, the outcome measures examined in the current study were derived from data reported by law enforcement. A previous study found an increase in crash victims treated in trauma centers in Washington, DC, despite a reduction in police-reported injury crashes, during a period when levels of enforcement were reduced (Calder et al., 2025).

Notably, four of the six cities in the current study experienced statistically significant increases in fatal and severe injury crashes alongside statistically significant decreases in total police-reported crashes during the portion of the study period that followed the reduction in stops. While this could suggest an increase in crash severity, observed decreases in total police-reported crashes might reflect decreases in the actual incidence of crashes, social forces such as those resulting from the COVID-19 pandemic or high-profile incidents (e.g., death of George Floyd) that decrease the likelihood of crash-involved drivers calling the police or the likelihood of police attending to and/or reporting minor crashes, or some combination of these factors.

Given the difficulty of isolating the effects of changes in police–motorist interactions from other factors with variable and potentially large influence on safety outcomes, the long-term impacts of these types of changes are difficult to assess. Collectively, the current results do not demonstrate conclusively that reductions in police–motorist interactions impact crash rates, but there is some evidence to suggest that more serious crashes, and speed-related crashes may increase when police reduce their stops.

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Appendix

ARIMA Overview[2]

In time series analysis, AutoRegressive Integrated Moving Average (ARIMA) models are widely used to analyze and forecast data collected over time, particularly when that data exhibits autocorrelation (i.e., the value at one point in time is correlated with values at previous time points). Autocorrelation can undermine standard statistical methods, which typically assume independence between observations, potentially leading to incorrect conclusions. ARIMA addresses this issue by modeling autocorrelation directly through three core components: **p**, **d**, and **q**. When the data shows regular, repeating patterns over fixed intervals—such as months, quarters, or seasons. A seasonal extension of the ARIMA model, also known as SARIMA (Seasonal ARIMA), adds three more terms: **P**, **D**, and **Q**, each corresponding to their non-seasonal counterparts but applied to the seasonal component of the data.

The **d (integration)** term refers to differencing the time series to remove trends and make the data *stationary*, meaning that its statistical properties—like the mean and variance—are consistent over time. For example, if crash counts steadily increase over several years, applying a first difference (d = 1) will convert the series from counts to month-to-month changes, flattening the trend. Similarly, the **D** term applies differencing to remove seasonal patterns. If crash counts tend to spike every winter, seasonal differencing (D = 1) subtracts the value from the same month in the previous year, neutralizing that pattern.

The **p (autoregressive)** term represents the number of past values used to predict the current value. For instance, if crash counts tend to remain elevated for a few months after a spike, an autoregressive term (p > 0) incorporates that persistence. The **P** term is the seasonal version of this: it uses values from the same season or month in previous years to help predict this season or month's count (e.g., last January's crash count is used to help predict this January's count).

The **q (moving average)** term models the influence of past errors—the differences between observed and predicted values—on the current value. If the model has recently underpredicted crashes, and this pattern continues, the moving average

[2] In time series models, **autoregressive (AR) terms** use past values to predict the current one (e.g., crashes this month may resemble those in recent months). **Differencing** removes long-term trends by analyzing how values change over time rather than their absolute level. **Moving average (MA) terms** account for patterns in random variation—adjusting for short-term spikes or dips by using past errors. **Seasonal terms** capture repeating patterns, such as higher crashes during winter months.

term adjusts for that. The **Q** term does the same, but for seasonal forecast errors—for example, if the model consistently underestimates summer crashes.

Together, these six parameters define the structure of an **ARIMA(p,d,q)(P,D,Q)[s]** model, where *s* is the seasonal period (e.g., 12 for monthly data with annual seasonality). For example, an **ARIMA(1,1,1)(0,1,1)[12]** model would differentiate the data once to remove trend, once more to remove seasonal cycles, and use both past values and past errors (but only seasonal errors, not seasonal past values) to model the current value. Note that the inclusion of the seasonal factors results in some referring to the models as SARIMA (Seasonal ARIMA).

The **auto.arima()** function in *R* automates the selection of all these parameters—both seasonal and non-seasonal— by testing many combinations and selecting the model that best balances goodness of fit with simplicity, typically using statistical criteria that account for both how well the model explains the data and how many parameters it uses, which is especially important when working with smaller datasets. If the **auto.arima()** function selects non-zero values for **P**, **D**, or **Q**, it indicates that seasonal structure is present and needs to be accounted for to ensure valid inference and accurate forecasting.

By modeling both regular and seasonal autocorrelation, ARIMA and SARIMA models help ensure that remaining variation in the data is not driven by inherent temporal patterns. This is particularly important in interrupted time series analyses, where the goal is to isolate the effect of a specific policy or interruption from the background rhythms and trends in the data.

Reporting of Results: Brief Description

In interrupted time series analysis using ARIMA models, the notation **(p,d,q)** describes the structure of the model used to account for patterns in the data. A model listed as **(0,0,0)** indicates that no autoregressive (AR), differencing (I), or moving average (MA) adjustments were necessary to account for autocorrelation or trends in the data. A model listed as **(0,0,1)** means that a single moving average term was included to adjust for short-term autocorrelation in the residuals. Numbers greater than 1 imply that multiple terms of that type (AR, I, or MA) were included.

A second set of parentheses describes the seasonal component of the model: **(P,D,Q)[s]**, where **s** indicates the seasonal periodicity (typically 12 for monthly data). For example, a model of **ARIMA(0,0,0)(1,0,0)[12]** indicates that seasonal autoregressive effects were present meaning that the model uses patterns that repeat every 12 months to improve the fit. All analyses in this report used a seasonal periodicity of 12, corresponding to the monthly data structure.