

Extended evaluation of the effects of Utah's 0.05% BAC per se law on crashes: An update with additional years and regional contrasts

October 2026

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Abstract

The National Transportation Safety Board has recommended that states lower the *per se* blood alcohol concentration (BAC) limit from 0.08% to 0.05% in a national effort to reduce alcohol-impaired driving. Most industrialized nations have established a 0.05% or lower BAC limit for driving. In December 2018, Utah became the first U.S. state to establish a 0.05% limit, and early research showed some evidence that the law reduced alcohol-involved crashes in its first full year of implementation. Examining 4 full years of the law's impact, this study shows it had continued benefit, both independently and in contrast to neighboring states, despite the unusual changes in drinking-driving behavior resulting from COVID-19 pandemic disruptions. The uniqueness of the alcohol environment in Utah and the confounding effects of the pandemic prevent us from determining the size of the effect.

Keywords: moderate AOD use, drinking and driving, AOD impairment, BAC, DWI laws, sanction, AOD availability, traffic accident, COVID-19

Introduction

In the 1970s, the National Highway Traffic Safety Administration (NHTSA) began raising awareness about *per se* laws, which made it a crime to drive with a blood alcohol concentration (BAC) over a certain limit regardless of whether the driver appeared visibly intoxicated. In the U.S., BAC *per se* laws were initially set at 0.15% BAC or higher, and by the beginning of the 1980s, *per se* laws moved from 0.15% or 0.12% to become nearly universal at 0.10% BAC. However, early research examining the relationship between BAC and impaired driving had already begun to establish significant impairment at BACs lower than 0.10%.

A case-control study conducted in Grand Rapids, Michigan, in the 1960s found that BACs over 0.04% were associated with increasing risk of crash involvement, with risk increasing more rapidly beginning at 0.08% (Borkenstein et al., 1964). Subsequent case-control studies (e.g., Compton et al., 2002; Voas et al., 2012) have confirmed the association between BAC and crash risk. These case-control studies are consistent with the dose-dependent relationship between alcohol and performance found in experimental research (Moskowitz et al., 2000). A review of 109 experimental studies reported that more than half found impairment in one or more driving-related behaviors at BACs of 0.05% or lower, and 94% found impairment at BACs of 0.08% or lower (Moskowitz & Fiorentino, 2000).

The movement toward the current 0.08% BAC standard gained momentum in the mid-1980s, after Utah and Oregon became the first two states to lower their *per se* limits to 0.08% in 1983 (Rodriguez-Iglesias et al., 2001). By 1999, 17 states and the District of Columbia had enacted 0.08% laws. Under the Department of Transportation's reauthorization of the Transportation Equity Act for the 21st Century (TEA-21, 2000), states were required to adopt a 0.08% limit by 2004 or risk losing significant federal highway construction funds. By 2005, all 50 states had complied. The move from 0.10% to 0.08% was associated with a median decrease of 7% in alcohol-related fatalities (Shults et al., 2001).

In 2013, the National Transportation Safety Board (NTSB) recommended a further reduction to a 0.05% BAC *per se* limit, citing evidence that critical driving skills—such as depth perception and reaction time—are significantly degraded at 0.05%, and noting that other industrialized nations with limits of 0.05% or lower saw immediate drops in fatal collisions (NTSB, 2013) when they were adopted. A subsequent meta-analysis predicted that fatal alcohol-related crashes would be reduced by about 11% in U.S. states that reduced their BAC *per se* limit to 0.05% (Fell & Scherer, 2017).

On December 30, 2018, Utah became the first state to implement a 0.05% *per se* law, although the law was passed (and highly publicized) in March 2017. The initial evaluation of the Utah law (Thomas et al., 2022) reported a 19.8% reduction in fatal crash rates per vehicle mile traveled (VMT) for 2019 relative to 2016. Fatal crash counts in Utah were too small to support a more powerful time series analysis. However, monthly time series analyses of overall crash rates (i.e., including nonfatal crashes) found an 11.5% reduction during the 21 months between passage and implementation of the law and a further 9.6% reduction during the 12 months after implementation. Results based on alcohol-involved crashes were inconclusive. The initial evaluation reported a slight increase in arrests of drivers at BACs between 0.05% and 0.079% during 2019, but overall arrests for driving under the influence (DUI) were relatively stable. Additional years of data suggest that arrests for DUI increased slightly above expected levels after the law was changed (Utah Commission on Criminal and Juvenile Justice, 2025). Using a difference-in-differences approach, another evaluation found that Utah experienced markedly greater reductions in alcohol-positive fatal crashes in 2019 versus 2016, compared with contiguous states (Li et al., 2026).

An evaluation of the Utah 0.05% law by different researchers reported a statistically significant reduction in overall crash rates per capita, but the reduction was mostly limited to crashes involving no injuries, and the effect was stronger during the day compared with night (Portillo et al., 2024). Typically, alcohol-related crashes are more likely to occur at night, and they are more likely to involve injuries.

Despite the inconsistencies in results for alcohol-related crashes, the positive results in Utah have sparked renewed legislative debates in other states, and several states are currently considering legislation or have already introduced legislation that is under debate in early 2026.

With several more years of crash data maturation, the current study is a replication of the initial Utah evaluation (Thomas et al., 2022), with the following enhancements:

1. The earlier study only had 1 year of post-implementation data available (2019); the current study extends on this with 3 additional years of data, for a total of 48 months.
2. The earlier study contrasted Utah's fatal crash experience with national-level fatal crash rates; we gathered state-level crash data from similar neighboring comparison states in the Mountain West region, as these states are economically and demographically more similar to Utah than the nation as a whole.
3. The earlier study used the data element of police-reported involvement of alcohol in crashes, regardless of the presence or absence of actual BAC results, which can be posted to the file much later and can be positive even when police did not 'report' nor 'suspect' alcohol on the form. We used a three-way composite measure that includes any of the following: actual alcohol-positive results; police-reported alcohol (when test results are missing/pending); or BAC test refusals, which legally are treated as synonymous with DUI due to implied-consent laws. The earlier study did not count test refusals as implicitly alcohol-involved.
4. Rates of BAC testing and recording vary from state to state, making official counts of alcohol involvement incomplete to some unknown degree. Other studies have established that single-vehicle nighttime (SVN) crashes are an indicator of alcohol involvement in the aggregate (Heeren et al., 1985; Voas et al., 2009). We evaluated changes in the percentage of crashes that were SVN crashes as a verification check on the designated alcohol-involvement measures described above.
5. Roughly 55%–70% of crashes that meet states' criteria for reporting are property damage only crashes that do not involve injury or fatality. The subset of crashes involving injury or fatality are generally much more likely to involve alcohol; therefore, we examined the rates of alcohol involvement and rates of SVN crashes within the subset of injury-involved crashes.

This results in four outcomes for each state, namely:

- (1a.) Percentage of all crashes that involved alcohol;
- (1b.) Percentage of injury crashes that involved alcohol;
- (2a.) Percentage of all crashes that are SVN;
- (2b.) Percentage of injury crashes that are SVN.

Method

The statistical approach utilizes comparative time series via ARIMA Intervention analysis (Box & Jenkins, 1970) of Utah's 0.05% BAC law (effective December 30, 2018), with similar analyses for each of the three neighboring comparison states (Arizona, Colorado, and Idaho). These states are selected based on similar demographic growth, shared geographic characteristics, and the fact that they maintained a 0.08% BAC limit throughout the study period.

These crash data were obtained from the department of public safety or state police of each state, depending on which agency serves as the repository for those crash data in that state. The funding sponsor (IIHS) regularly acquires these files from many states for research purposes.

We tested several potential regressor series as a control for exogenous factors, if necessary. These include:

- Monthly VMT to account for fluctuations in driving exposure and seasonality (e.g., higher crash rates during winter months in mountainous regions). These were obtained from the Federal Highway Administration.
- Economic measures to account for exogenous factors, as other research has demonstrated that these are sometimes correlated with crash rates and alcohol involvement, although typically more so as *between-states* factors, rather than within-states over time. We obtained state-specific monthly series of unemployment rates (namely, the UE-3 measure) from the Bureau of Labor Statistics and the similar labor force participation rates (LFP) from the Federal Reserve Economic Data (FRED).
- Additionally, other data on inflation, gas prices, and higher-level unemployment (UE-4, UE-5, UE-6) are not available monthly by state, but we included national-level monthly data series for each of these (also from the BLS and the FRED).

The crash data obtained from the states come from multilevel relational databases with crash-level records, driver/vehicle-level records, and person-level records. Each state has its own system for recording the various elements of alcohol involvement, sometimes at more than one level. These

structural formats can sometimes change from year to year within a state, so care is required to ensure that a measure can be tracked consistently across time.

We used the person-level data for tracking whether a particular crash was alcohol-involved or had a test refusal. In aggregating up to the crash level from these, we designated a crash as alcohol-involved if any driver in that crash was alcohol-positive. Crashes were aggregated into two groups: the monthly numbers of total reported crashes and the monthly numbers of alcohol-involved crashes. From these we computed the monthly percentages of crashes that were alcohol-involved as our first outcome measure and compiled these into separate time series for each state. The additional outcome measures below were computed in a similar manner.

For SVN crashes, we counted crashes that occurred within the overnight time window from 9:00 p.m. to 4:59 a.m. (8 hours overnight), extending the weekend hours on Saturday and Sunday mornings to 6:59 a.m. (10 hours overnight).

For Utah, we used data from January 2010 through December 2022, for a total of 13 years or 156 monthly data points. For the comparison states, these time series began in 2012, and Colorado had an additional year available post-implementation (2023). The lengths of the time series were 132 months in Arizona and Idaho and 144 months in Colorado. This provided a baseline period (pre-implementation) of 84 months for the comparison states and 108 months for Utah.

Statistical methodology: ARIMA intervention time series analysis

The study utilizes the Box–Jenkins three-stage approach (Box & Jenkins, 1970) for modeling each state, beginning with baseline modeling (Utah: 2010–2018, comparisons: 2012–2018). This is for development of a seasonal ARIMA model for each state to capture existing trends and cyclic patterns. The three stages are:

1. Identification: Use plots of auto-correlation functions (ACF) and partial auto-correlation functions (PACF) to diagnose series stationarity (an important assumption for ARIMA). If the series is nonstationary, apply either first-order differencing (d) or seasonal [annual] differencing, depending on the form of the ACF. (Utah was stationary; Arizona and Idaho required seasonal differencing; Colorado required first-order differencing.)
2. Estimation: Determine optimal parameters (p, d, q) for the nonseasonal and seasonal autoregressive (AR) and moving average (MA) components. As these are monthly series, the periodicity is 12, which corresponds to the seasonal components (SAR, SMA).
3. Diagnostic testing: Perform residual analysis using the Ljung-Box Q-statistic to ensure residuals behave as white noise, indicating no remaining autocorrelation. Also assess model fit using the usual metrics for time series, namely RMSE, MAPE, AIC, BIC, std error of model, etc.

In addition, many time series benefit from using a natural log transform (a) to account for heteroskedasticity (variance is correlated with level) and (b) to interpret the dummy variables that are used to model interventions as a relative percentage change. A log transform also makes it straightforward to contrast the impacts in the comparison states relative to Utah, as all the states have different baseline levels of alcohol involvement as a percentage of their total crashes. All the series analyzed for this study required this log transform.

An intervention analysis assesses the impact of the 0.05% law change via the inclusion of transfer functions. These are binary (dummy) variables added to the ARIMA model as regressor series, starting with a value of zero during the baseline and increasing to a value of one at the point of implementation (January 2019).

The evaluation of the post-law period was complicated by an extreme disruption caused by the COVID-19 lockdown period that began in March 2020. This lockdown caused many social phenomena, including travel patterns, unemployment, and crime, to change drastically. Particularly in 2020–2021, there was a nationwide surge in alcohol-related fatalities per VMT; all of our four states experienced huge spikes in the percentage of crashes that were alcohol-involved, even as traffic volume plummeted, suggesting the safer drivers stayed home and/or the unsafe drivers took greater risks than usual. Many of the covariate series (e.g., VMT, unemployment) did not correlate similarly with the outcome measures as they had before, making their inclusion dubious. The contamination of the COVID effect made model diagnosis challenging.

For this reason, the law intervention models were first tested on the truncated series for the 14-month period preceding the COVID lockdown disruption. After diagnosing the uncontaminated model parameters, these could be extended through the remaining period, with the COVID effect modeled explicitly with a step function of its own, paired with potential decay functions (linear decay, exponential decay, log decay) to model statistically the shape of the pulse that the pandemic caused in the data series.

The earlier study (Thomas et al., 2022) also included interviews and surveys with people in Utah, which found that many drivers mistakenly believed that the 0.05% BAC law went into effect when the legislature passed it in March 2017, perhaps due to the enormous amount of publicity generated by the debate around the law (Utah Department of Public Safety, n.d.; Lighthouse Research & Development, 2018). That study found evidence that an anticipation effect may have lowered alcohol-involved driving in the 21 months between passage and implementation, although the results were not statistically significant. We also tested that effect with a dummy variable similar to the implementation effect, using a 21-month binary pulse as a regressor.

Finally, we integrated the Utah results with the comparison states' results using the comparison technique of a difference-in-differences (DiD) framework (Lechner, 2011; Wang et al., 2024; Roth et al., 2023). This technique contrasts the experimental state parameters with those for the presumably null

comparison states' outcomes, which estimates the net marginal effect above and beyond that seen in comparison states.

Results

Figures 1–4 display the time series graphs for the first outcome, the percentage of all crashes that were alcohol-involved. Time series graphs for additional outcomes are shown in the Appendix. All figures display the three segments of the study period, including the

1. Baseline period, up until March 2017 when the law is *passed* (but not yet in effect);
2. Anticipation period of 21 months, when some Utahns mistakenly believed the BAC limit was 0.05%;
3. Implementation period of 48 months (60 months for Colorado), including the COVID period with its marked jump only 14 months after the law went into effect.

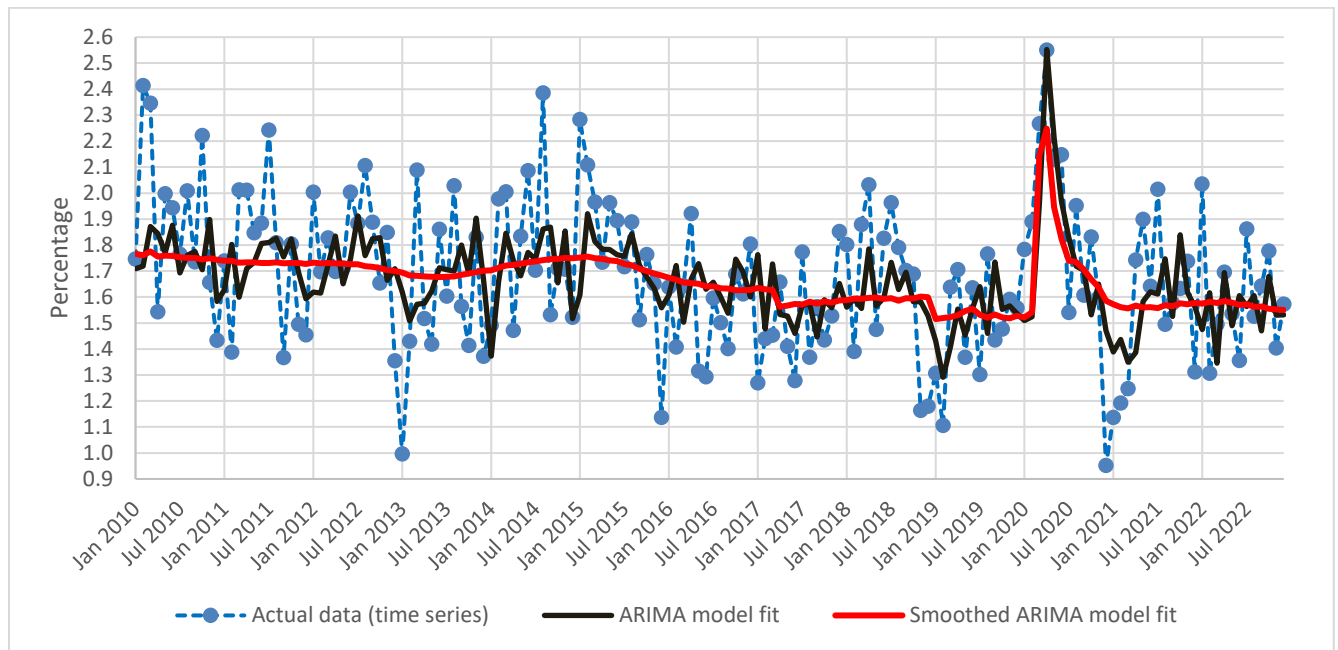
Each graph displays the actual data of the time series (dashed blue line), the ARIMA model fit (black line), and the smoothed, seasonally adjusted ARIMA model fit (red line). The smoothed line is based on the average of multiple yearly moving average windows, with discrete breaks at the two intervention points and the COVID-19 spike in 2020, to facilitate visual interpretation.

Utah

Utah has always had one of the lowest rates of alcohol involvement in crashes in the United States and began the baseline period at extremely low levels, averaging alcohol-involvement rates between 1.3% and 2.0% during the baseline years, as seen in Figure 1.

Figure 1

Percentage of crashes that involved alcohol in Utah, 2010–2022



At the point that the 0.05% BAC law goes into effect, the level drops suddenly, showing a significant decrease of 8.8% ($p = 0.045$) relative to baseline (Table 1). Although it briefly spikes with the COVID lockdowns, going up by 46% during March to May 2020, the increase dissipates rather quickly, returning to normal in about 10 months ($p = 0.010$). The *average* monthly impact for the 34-month COVID period is 6.4% per month (most of that occurring in the first 6 months).

Table 1

ARIMA output for percent of alcohol-involved crashes in Utah

		Estimate	SE	<i>t</i>	<i>p</i>	Effect
Constant		0.53551	0.02676	20.011	0.000	
Non-seasonal lags	AR1	0.26872	0.08012	3.354	0.001	
	AR10	−0.18516	0.07857	−2.357	0.020	
	MA3	−0.21400	0.08153	−2.625	0.010	
Seasonal lags	Seasonal AR1	0.18161	0.08574	2.118	0.036	
Regression coefficients	Law passed	−0.07910	0.05680	−1.392	0.166	−7.6%
	Law in effect	−0.09201	0.04550	−2.022	0.045	−8.8%
	COVID pulse	0.38141	0.14625	2.608	0.010	46.4%

Note. $df = 148$

This estimate of an 8.8% decrease due to the 0.05% law is in addition to the impact of the COVID disruption. When the COVID period is excluded, analysis of the truncated series finds a similar implementation effect of -9.7% , although that is not significant ($p = 0.168$) with just 14 months post-law.

Table 2 displays the ARIMA intervention results of all four Utah outcome measures. Three of the four measures suggest a beneficial impact. The alcohol percentage of injury crashes shows an even stronger effect (-12.3%) than that for all crashes, and the SVN as a percentage of injury crashes shows a similar (though nonsignificant) effect size of -8.6% , while the SVN as a percentage of all crashes suggests an increase (nonsignificant).

Table 2

Utah outcomes

	Estimate	SE	<i>t</i>	<i>p</i>	Effect
Outcomes for implementation (December 30, 2018)					
Alcohol % of all crashes	-0.09201	0.04550	-2.022	0.045	-8.8%
Alcohol % of injury crashes	-0.13099	0.04868	-2.691	0.008	-12.3%
SVN % of all crashes	0.05768	0.03202	1.802	0.074	5.9%
SVN % of injury crashes	-0.09031	0.05538	-1.631	0.105	-8.6%
Outcomes for passage (March 23, 2017)					
Alcohol % of all crashes	-0.07910	0.05680	-1.392	0.166	-7.6%
Alcohol % of injury crashes	-0.05001	0.03535	-1.415	0.159	-4.9%
SVN % of all crashes	0.04933	0.02468	1.999	0.048	5.1%
SVN % of injury crashes	-0.06444	0.05004	-1.288	0.200	-6.2%

Note. SVN = single-vehicle nighttime.

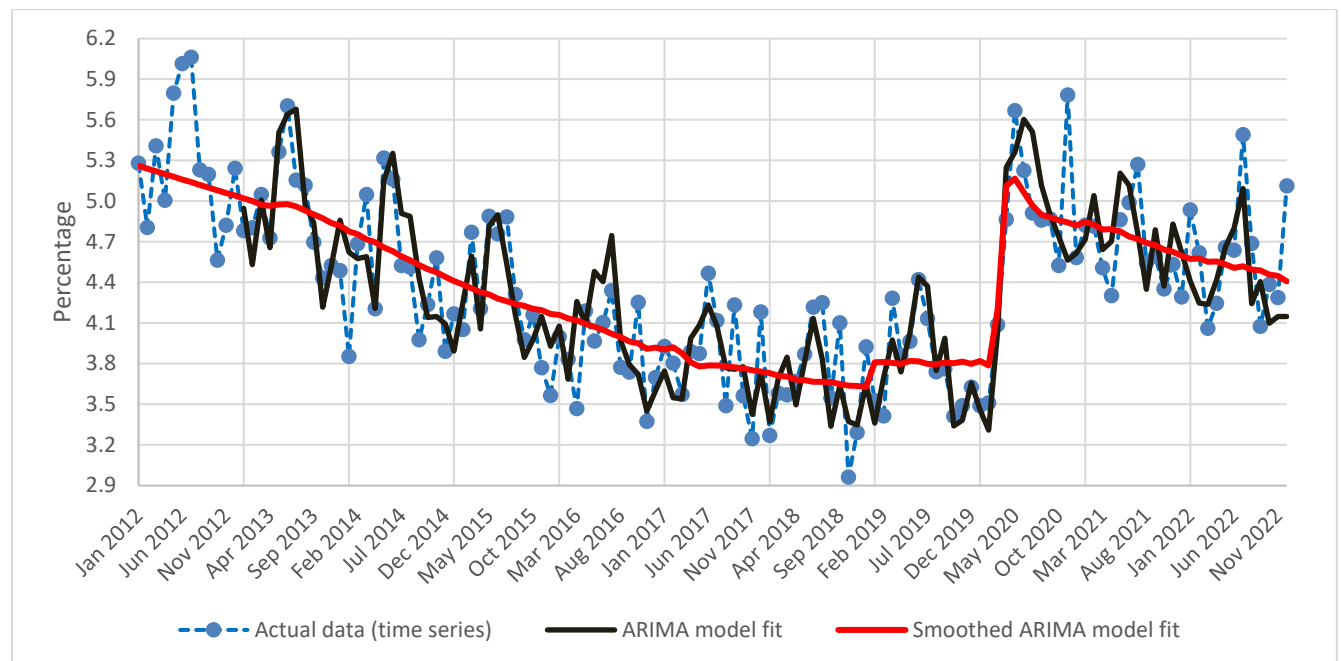
As in the earlier study, there is slight evidence suggesting a possible anticipation effect after the law is passed, but any potential decrease of between -5% and -7.6% is not significant ($p = 0.166$). However, the 5.1% estimated increase in SVN crashes is statistically significant ($p = 0.048$).

Arizona

Arizona begins the baseline period at a rate of about 5.5% alcohol involvement in all crashes and demonstrates a visually obvious steady and steep decline over time despite the wide seasonal fluctuations, as seen in Figure 2. By the time of the Utah law implementation, Arizona's alcohol-involvement rate has reached an average level of about 3.6%, but then the downward trend suddenly reverses and the rate increases by a significant 15.6% for the remaining 48 months ($p < 0.001$). (Note that all these ARIMA analyses utilize a log transform, so the floor effect of 0% does not artificially constrain the parameter estimates, as those are relative to level.)

Figure 2

Percentage of crashes that involved alcohol in Arizona, 2012–2022



Arizona's trendline jumps upward by 43% ($p < 0.001$) at the COVID point. Then, rather than dropping back equally rapidly, the prior downward trend of the baseline period merely resumes, and by the end of 2022 average alcohol-involvement rates only reach about 4.6% , a rate that is still well above

the pre-COVID and pre-Utah law change levels. The average monthly impact for the 34-month COVID period in Arizona is 42.2%, much higher than the 6.4% monthly impact in Utah.

Alcohol-involvement rates for injury crashes in Arizona increase by a similar magnitude as for all crashes, rising 14.7%, as shown in Table 3. No change is seen in Arizona's two SVN outcome series, but there is evidence suggesting the SVN alcohol-involvement rates may have increased briefly during the 21-month period between the passage and implementation of Utah's 0.05% BAC law. These are quasi-comparison series for the Utah series during that anticipation period, for contrasting Utah's effects with surrounding states.

Table 3

Arizona outcomes

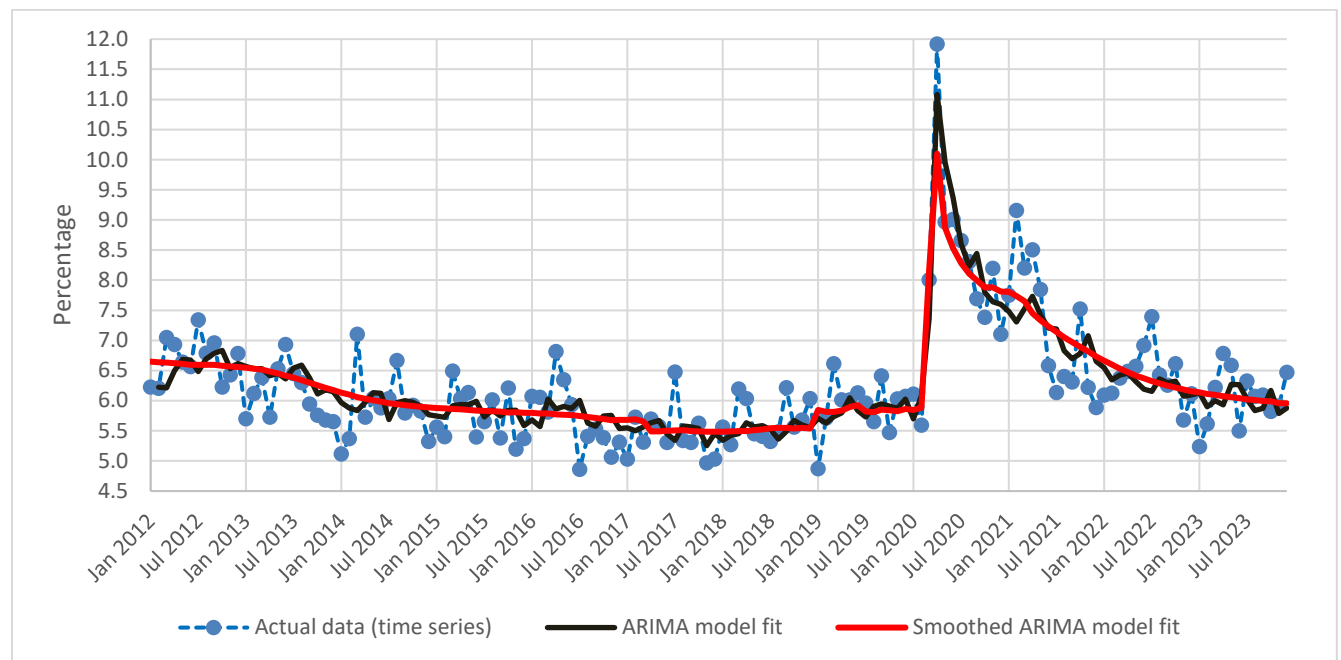
	Estimate	SE	<i>t</i>	<i>p</i>	Effect
Outcomes for comparison to implementation (December 30, 2018)					
Alcohol % of all crashes	0.14463	0.03199	4.522	0.000	15.6%
Alcohol % of injury crashes	0.13692	0.01627	8.417	0.000	14.7%
SVN % of all crashes	0.00258	0.04081	0.063	0.950	0.3%
SVN % of injury crashes	0.03243	0.05984	0.542	0.589	3.3%
Outcomes for comparison to passage (March 23, 2017)					
Alcohol % of all crashes	0.03633	0.02319	1.567	0.120	3.7%
Alcohol % of injury crashes	0.08157	0.01347	6.056	0.000	8.5%
SVN % of all crashes	-0.00639	0.02895	-0.221	0.826	-0.6%
SVN % of injury crashes	0.02383	0.04547	0.524	0.601	2.4%

Colorado

Colorado begins the baseline period with an alcohol-involvement rate of about 6.5% that is trending slightly downward (captured by the monthly differencing instead of the seasonal differencing necessary in Arizona and Idaho). As seen in Figure 3, Colorado's rate bottoms out around 5.5% during the middle period (between March 2017 and December 2018) and begins to increase slightly around the time Utah's law is implemented. This upturn produces an estimated implementation effect of 3.2%, although it is not significant ($p = 0.619$).

Figure 3

Percentage of crashes that involved alcohol in Colorado, 2012–2023



Colorado experiences an upward spike of 84% ($p < 0.001$) when the COVID disruption hits, rising from 6.0% alcohol involvement to a model-estimated 10.5%, though the actual data point for that month is 12%, double what it was just a few months prior. As with Utah's COVID experience, Colorado's rate dissipates fairly quickly with a logarithmic pattern, but takes longer to reach its pre-COVID levels, not returning to those rates until about the end of 2023 (note Colorado has an extra year in its series). The average monthly impact for the 34-month Covid period (excluding 2023, since we do not have that year for other states) is 23.3%.

Table 4 shows the results for Colorado's comparison series. None of the changes are significant, and they range in size from 0% for all SVN crashes to 10% and 7% for injury crashes and SVN injury crashes.

Table 4*Colorado outcomes*

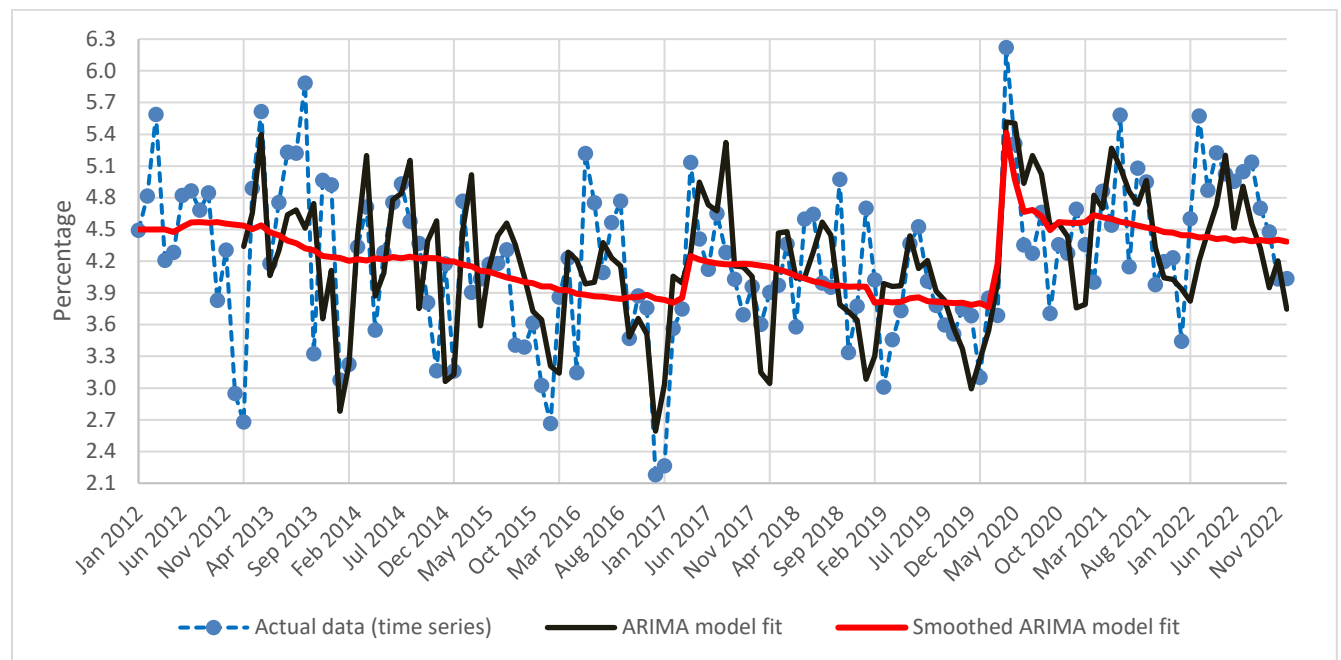
	Estimate	SE	<i>t</i>	<i>p</i>	Effect
Outcomes for comparison to implementation (December 30, 2018)					
Alcohol % of all crashes	0.03126	0.06274	0.498	0.619	3.2%
Alcohol % of injury crashes	0.09843	0.06843	1.438	0.153	10.3%
SVN % of all crashes	−0.00300	0.08033	−0.037	0.970	−0.3%
SVN % of injury crashes	0.06697	0.10367	0.646	0.519	6.9%
Outcomes for comparison to passage (March 23, 2017)					
Alcohol % of all crashes	0.00939	0.04484	0.209	0.834	0.9%
Alcohol % of injury crashes	0.07732	0.05529	1.399	0.164	8.0%
SVN % of all crashes	0.02250	0.05526	0.407	0.685	2.3%
SVN % of injury crashes	0.08938	0.07139	1.252	0.213	9.3%

Idaho

Idaho begins the baseline period at a rate of about 5.0% alcohol involvement, with highly volatile series. That volatility may be due in part to the much smaller number of crashes, which makes Idaho more sensitive to sampling errors, but it may also stem from the state's more northerly latitude and the associated weather patterns. Idaho also shows a small but significant annual trend requiring seasonal differencing, like Arizona. Idaho reaches a rate of about 4.0% at the point that Utah passes its 0.05% law, when Idaho's alcohol-involvement rate seems to shift upward by 14.7% ($p = 0.008$) before resuming its prior downward trend, as seen in Figure 4.

Figure 4

Percentage of crashes that involved alcohol in Idaho, 2012–2022



While Idaho shows little sign of change at the 0.05% law implementation point, the ARIMA model estimates the parameter at that point as a nonsignificant 10.1% relative increase in trend ($p = 0.155$). As in Arizona, the COVID disruption results in a shift upward in Idaho, where the alcohol-involvement rate spikes by 30.3% ($p < 0.001$). This effect also does not dissipate immediately; rather, alcohol-involvement rates resume their prior downward trend. The average monthly impact for Idaho's 34-month COVID period is thus that full 32.5%.

Idaho's outcomes are much more volatile than the others, likely due to factors such as state population size, which results in a much smaller number of crashes). Table 5 shows the ARIMA intervention results for all four of Idaho's comparison series.

Table 5*Idaho outcomes*

	Estimate	SE	<i>t</i>	<i>p</i>	Effect
Outcomes for comparison to implementation (December 30, 2018)					
Alcohol % of all crashes	0.09644	0.06743	1.430	0.155	10.1%
Alcohol % of injury crashes	-0.01172	0.07226	-0.162	0.871	-1.2%
SVN % of all crashes	-0.01047	0.02367	-0.442	0.659	-1.0%
SVN % of injury crashes	-0.18304	0.05901	-3.102	0.002	-16.7%
Outcomes for comparison to passage (March 23, 2017)					
Alcohol % of all crashes	0.13669	0.05083	2.689	0.008	14.6%
Alcohol % of injury crashes	0.08490	0.06411	1.324	0.188	8.9%
SVN % of all crashes	-0.01366	0.02004	-0.682	0.497	-1.4%
SVN % of injury crashes	-0.05912	0.05311	-1.113	0.268	-5.7%

Net marginal comparative effect (DiD method)

The results of Utah's ARIMA Intervention model parameters can be contrasted with those of each comparison state by subtracting Utah's intervention parameter from the corresponding parameter for the comparison state for the net change amount. The magnitude of this net effect is then divided by the pooled estimate of the standard errors, producing a t-statistic, with the degrees of freedom adjusted for the sum of both ARIMA models being contrasted.

Table 6 shows the contrasts for the implementation effect on the alcohol-involved percentage of all crashes. Note that Utah's raw effect of an 8.8% decrease is enhanced when seen relative to the *increases* in each of the three comparison states; the cumulative (or marginal) effect becomes a decrease of 21.1% relative to Arizona ($p < 0.001$), a nonsignificant decrease of 11.6% relative to Colorado ($p = 0.115$), and a decrease of 17.2% relative to Idaho ($p = 0.022$).

Table 6*Contrasting states' alcohol crash percentages, 48 months post-implementation*

Implementation	<i>b</i>	Relative change	<i>SE</i>	<i>t</i>	<i>p</i>	<i>df</i>	Average monthly COVID impact	<i>t</i>	<i>p</i>
Utah	-0.09201	-8.8%	0.04550	-2.022	0.045	148	6.37%	2.608	0.010
Arizona	0.14463	15.6%	0.03199	4.521	0.000	111	42.20%	15.600	0.000
Colorado	0.03126	3.2%	0.06274	0.498	0.619	137	23.31%	10.632	0.000
Idaho	0.09644	10.1%	0.06743	1.430	0.155	113	32.45%	5.509	0.000

Contrasted models	Difference (<i>b</i>)	Cumulative effect	<i>SE</i>	<i>t</i>	<i>p</i>	<i>df</i>
Utah vs Arizona	-0.23664	-21.1%	0.05562	-4.255	0.000	115
Utah vs Colorado	-0.12327	-11.6%	0.07750	-1.591	0.115	98
Utah vs Idaho	-0.18845	-17.2%	0.08135	-2.317	0.022	106

As we have already noted, Utah weathered the COVID-19 pandemic far better and returned to normalcy far quicker than the other three states. While this nationwide disruption was an inconvenient complicator to our longitudinal experiment in evaluating the 0.05% law, we can still estimate the net marginal effect by restricting the implementation effect to only the 14 months before the pandemic, as seen in Table 7.

Table 7*Contrasting states' alcohol percentages, prepandemic (14 months) truncated*

Implementation	<i>b</i>	Relative change	<i>SE</i>	<i>t</i>	<i>p</i>	<i>df</i>
Utah	-0.10166	-9.7%	0.07324	-1.388	0.168	115
Arizona	0.17218	18.8%	0.02655	6.485	0.000	77
Colorado	0.01641	1.7%	0.06555	0.250	0.803	92
Idaho	0.12293	13.1%	0.07258	1.694	0.094	80

Contrasted models	Difference (<i>b</i>)	Cumulative effect	<i>SE</i>	<i>t</i>	<i>p</i>	<i>df</i>
Utah vs Arizona	-0.27384	-24.0%	0.07790	-3.515	0.001	82
Utah vs Colorado	-0.11807	-11.1%	0.09829	-1.201	0.235	53
Utah vs Idaho	-0.22459	-20.1%	0.10311	-2.178	0.033	62

Utah's implementation effect of -9.7% was not significant by itself for the truncated period ($p = 0.168$). However, when contrasted with the increases in the other three states, the cumulative effects are significant decreases of 24.0% and 20.1% relative to Arizona ($p = 0.001$) and Idaho ($p = 0.033$) and a nonsignificant decrease of 11.1% relative to Colorado ($p = 0.235$). Note the pattern of results is consistent with those shown above in Table 6 for the full implementation period that includes the COVID disruption. This correspondence between the sizes of the intervention effects for the truncated (14-month) period and the full 48-month period was consistent across all ARIMA results (as should be the case if the model accurately partials out the COVID disruption effect).

Anticipation Effect at Utah's Passage Date

The cumulative anticipation effect for the 21-month passage period (immediately prior to the actual implementation date) is a significant net decrease of 19.4% only when contrasted with Idaho ($p = 0.006$), as shown in Table 8. The comparative results for the other states are not significant.

Table 8

Contrasting states' anticipation effects on alcohol crash percentages during 21 months prior to implementation

Passage	<i>b</i>	Relative change	<i>SE</i>	<i>t</i>	<i>p</i>	<i>df</i>
Utah	-0.07910	-7.6%	0.05680	-1.393	0.166	148
Arizona	0.03633	3.7%	0.02319	1.567	0.120	111
Colorado	0.00939	0.9%	0.04484	0.209	0.834	137
Idaho	0.13669	14.6%	0.05083	2.689	0.008	113
Contrasted models	Difference (<i>b</i>)	Cumulative effect	<i>SE</i>	<i>t</i>	<i>p</i>	<i>df</i>
Utah vs Arizona	-0.11543	-10.9%	0.06135	-1.881	0.062	115
Utah vs Colorado	-0.08849	-8.5%	0.07237	-1.223	0.224	98
Utah vs Idaho	-0.21579	-19.4%	0.07622	-2.831	0.006	106

Results for alcohol-involved percentage in injury crashes

As noted in the results for Utah, the impact on alcohol percentage in injury crashes (−12.3%) is slightly larger than in all crashes. It also contrasts significantly with two of the three comparison states, as shown in Table 9. The contrast here is large again relative to Arizona (−23.5% net), but this time it is also significant relative to Colorado (−20.5%), while not for Idaho (−11.2%).

Table 9

Contrasting states' injury crash alcohol percentage, 48 months post-implementation

Implementation	<i>b</i>	Relative change	<i>SE</i>	<i>t</i>	<i>p</i>	<i>df</i>
Utah	−0.13099	−12.3%	0.04868	−2.691	0.008	148
Arizona	0.13692	14.7%	0.01627	8.415	0.000	110
Colorado	0.09843	10.3%	0.06843	1.438	0.153	135
Idaho	−0.01172	−1.2%	0.07226	−0.162	0.871	125

Contrasted models	Difference (<i>b</i>)	Cumulative effect	<i>SE</i>	<i>t</i>	<i>p</i>	<i>df</i>
Utah vs Arizona	−0.26791	−23.5%	0.05133	−5.220	0.000	114
Utah vs Colorado	−0.22942	−20.5%	0.08398	−2.732	0.008	95
Utah vs Idaho	−0.11927	−11.2%	0.08713	−1.369	0.174	116

Results for single-vehicle nighttime (SVN) crashes

Analyses of the impacts of the Utah law on SVN crashes were not consistent with the alcohol-involved results. While Utah showed a marginal increase of about 6% for SVN as a percentage of all crashes (Table 1), the three comparison states showed no change (Tables 3–5). This meant that the cumulative or net differences for Utah were nonsignificant increases of 6%–7% (not shown in tables). When the SVN outcomes were restricted to injury crashes, Utah showed patterns of change in the same direction and magnitude (−8.6%) as the alcohol-involved outcome series, though the result was not significant. Contrasted with Arizona and Colorado, this resulted in cumulative net effects of −11.6% and −14.6% (not shown in tables), but those numbers are not significant.

Although SVN is a less specific, less sensitive surrogate outcome measure than alcohol involvement, it is worth noting that there could be testing and/or reporting deficiencies in the police-reported alcohol data. Incompleteness of the record almost guarantees an underestimate with regard to the first two outcome measures, and there could be a bias in the series if these testing rates change over time, particularly near the points of intervention.

In Utah, for example, we do have a large portion of cases in which an officer suspected alcohol and/or requested a BAC test, but the BAC test result is listed as ‘pending,’ and the crash records for these cases may never get updated.

We performed ARIMA time series tests of those Utah crashes with a ‘pending’ or ‘missing’ alcohol test result to see if there was evidence of incomplete alcohol results changing over time. We found that the percentage of crashes with pending test results stayed relatively consistent and did not significantly change at the law’s implementation point (4.4%, $p = 0.588$) when we looked at the full COVID-19 period or at the truncated pre-COVID series (-3.7%, $p = 0.647$).

We then analyzed the alcohol-involved series for all crashes again, making the extreme assumption that all pending tests would have come back positive, thereby positing the worst-case scenario for Utah’s underreported testing data. In that scenario, alcohol positive &/or pending tests as a percentage of all crashes showed a nonsignificant decrease of 2.9% ($p = 0.778$).

Discussion

One challenge of public policy evaluation is controlling for extraneous factors that can confound the results. Time series intervention models can only go so far in assessing longitudinal impacts when other factors such as economics, weather, or spillover effects from other jurisdictions cannot be fully accounted for. By using comparison states as contrasts, we hoped to account for much of that.

For the first two outcome series—alcohol-involved percentages in all crashes and injury crashes—our main findings after adjusting for the COVID effect are supported by results of similar magnitudes in the truncated 14-month findings, even as statistical power is lost due to fewer data points in the time series.

However, the two surrogate measures utilizing SVN were not as consistent as the alcohol-involved series, raising some doubts. The SVN results for injury crashes were similar to the alcohol-involved results, with net differences of -12% to -15% (relative to Arizona and Colorado), but the large standard errors kept those effect sizes from reaching significance.

It was difficult to statistically account for each state's different experience with the disruptions that occurred during the early stages of the COVID pandemic. It is possible that some of the differences in the impact of COVID on drunk driving crashes in Utah compared with the other states could be due to its 0.05% law, but there is no way to ascertain that directly.

A visual inspection of all the series demonstrates huge differences favoring Utah, but we cannot relate those benefits to the 0.05% law conclusively. The enormity of the COVID effect in the comparison states and the rapidity of Utah's COVID-impact decrease suggest that something in Utah created a robust protective effect against the disruption that increased drinking and driving elsewhere. Whether that was wholly cultural or in part due to the 0.05% law is indeterminable, but the phenomenon is encouraging.

Some of the outcomes (especially those limiting crashes to the 30%–40% that had an injury) were more volatile than others due to smaller monthly pools of crashes, which produced greater error terms and

made ARIMA modelling difficult. (Idaho, as a smaller state, had inherently much more volatility in its series.) This volatility is separate from periodic and seasonal patterns, which are obvious in most of these series.

The various regressor series for economics and traffic volume (VMT, unemployment, labor force participation, inflation indices, etc.) ultimately did not serve as useful covariate series. While many of these correlated with portions of the series before COVID, many relationships fell apart or even reversed beginning in 2020. Fortunately, ARIMA model parameters usually accounted for those seasonal, trend/drift, and nonperiodic dynamics, rendering the potential covariate series unnecessary.

Alcohol use patterns are unusual in Utah because of the high percentage of residents who identify as members of the Church of Jesus Christ of Latter-day Saints (LDS). The church discourages alcohol consumption, and Utah has by far the lowest per capita alcohol consumption in the nation at just 1.21 gallons of ethanol per adult, compared with 1.71 gallons per adult for the next-lowest state and a national average of 2.48 gallons per adult (Slater & Alpert, 2025). Thus, the results of Utah's implementation of a 0.05% BAC law may not translate directly to another state lacking those strong cultural influences.

It should be noted, however, that the LDS influence in Utah has declined in recent decades. Historically much higher, the percentage of residents who identify themselves as Mormons has fallen from a little more than 50% in 2001 to 42% in 2022 (Cragun et al., 2023).

Because Utah started at very low levels of alcohol-involved driving before the law's implementation, one could argue that this could have stacked the deck against finding a significant result by constraining the room for further reductions. Finding a significant reduction from such low levels is encouraging for other states that have higher levels of alcohol involvement and thus more room for improvement from implementing a similar law.

Conclusion and policy implications

The results from these extended analyses echo the findings from an earlier study (Thomas et al., 2022) and suggest that Utah's 0.05% law has been beneficial. However, the uniqueness of the alcohol environment in Utah and the confounding effects of the pandemic prevent us from determining the size of the effect.

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Appendix

Figure A1

Percentage of injury crashes that involved alcohol in Utah, 2010–2022

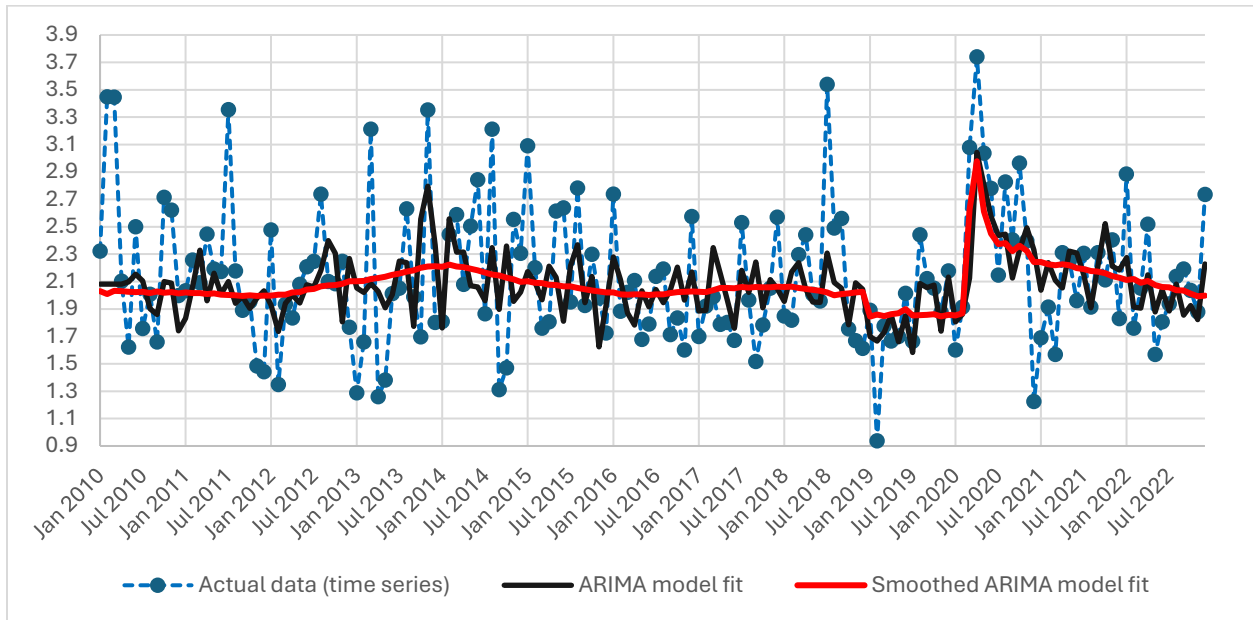


Figure A2

Percentage of injury crashes that involved alcohol in Arizona, 2012–2022

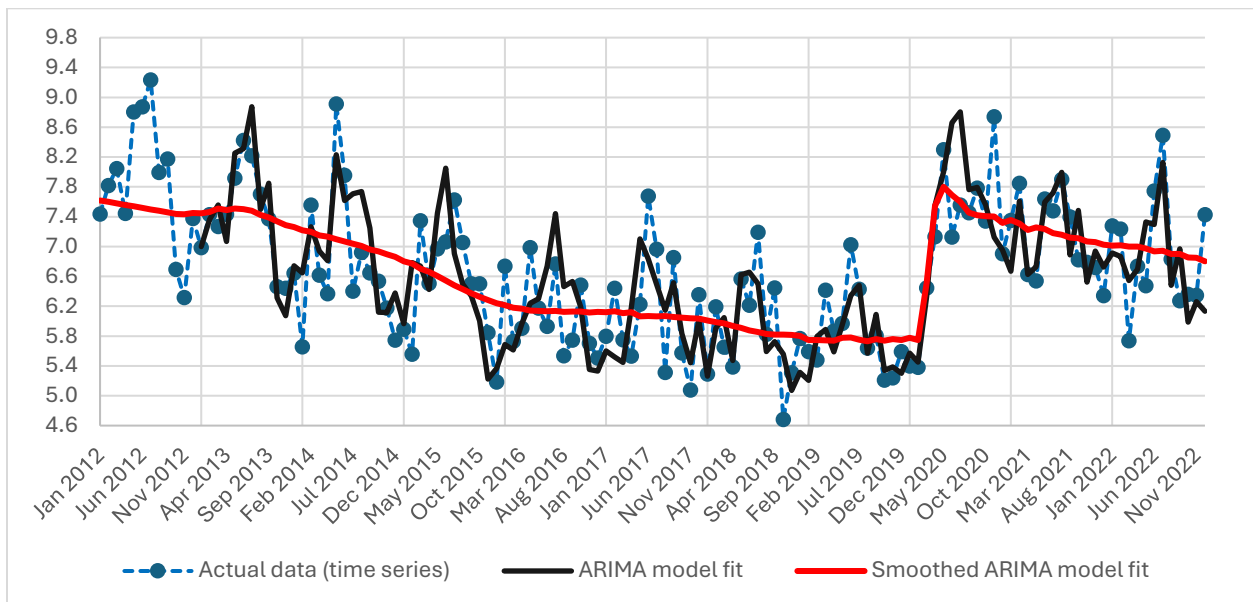


Figure A3

Percentage of injury crashes that involved alcohol in Colorado, 2012–2023

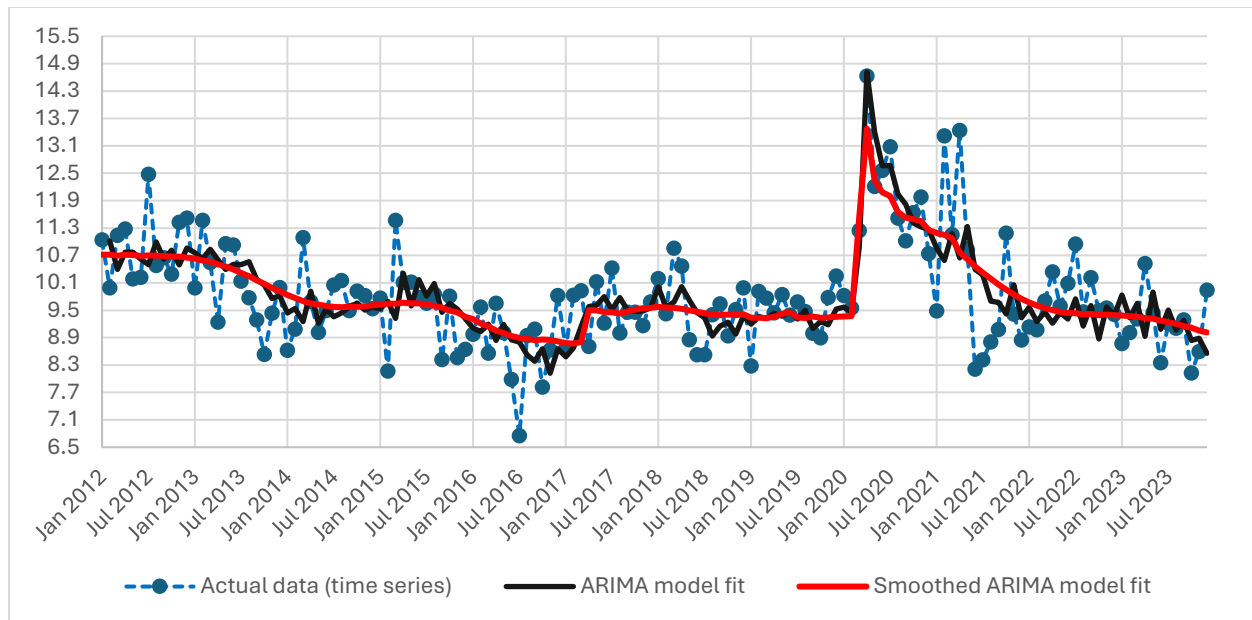


Figure A4

Percentage of injury crashes that involved alcohol in Idaho, 2012–2022

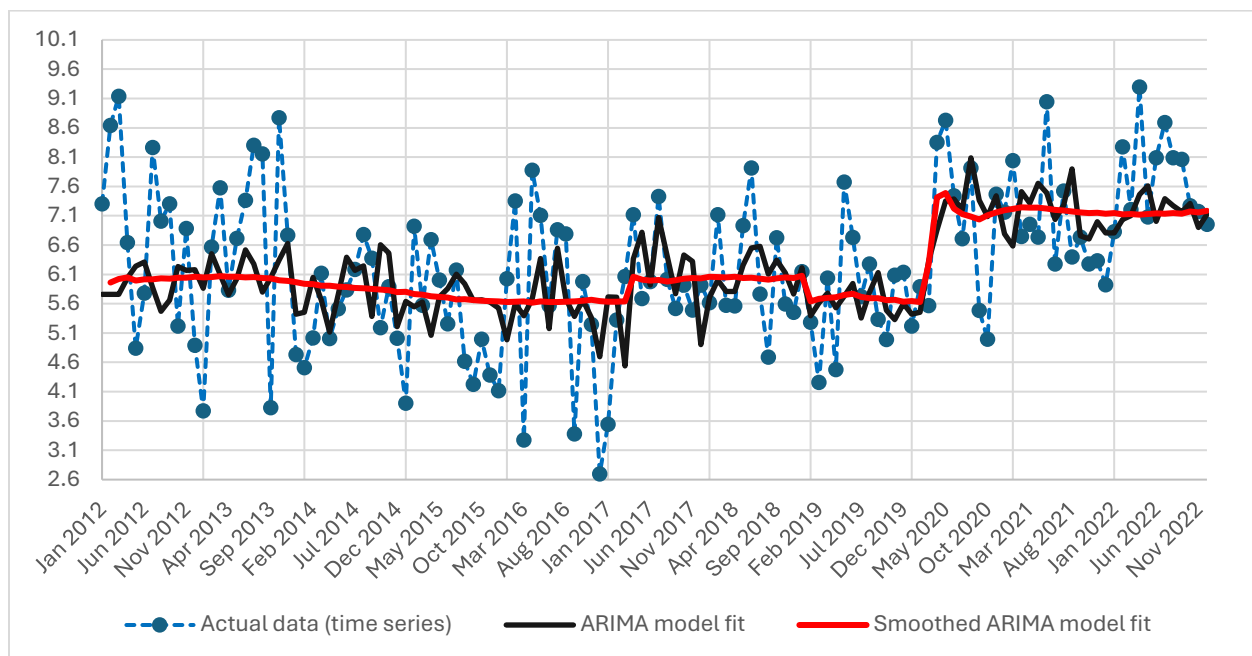


Figure A5

Percentage of crashes that were single-vehicle nighttime crashes in Utah, 2010–2022

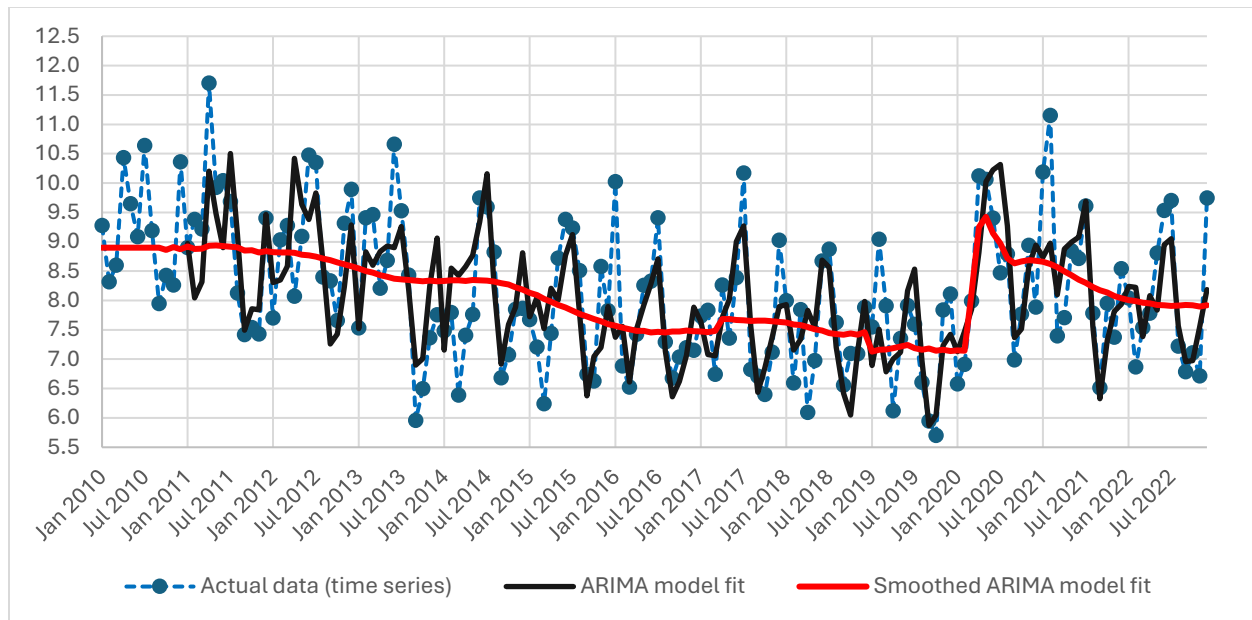


Figure A6

Percentage of crashes that were single-vehicle nighttime crashes in Arizona, 2012–2022

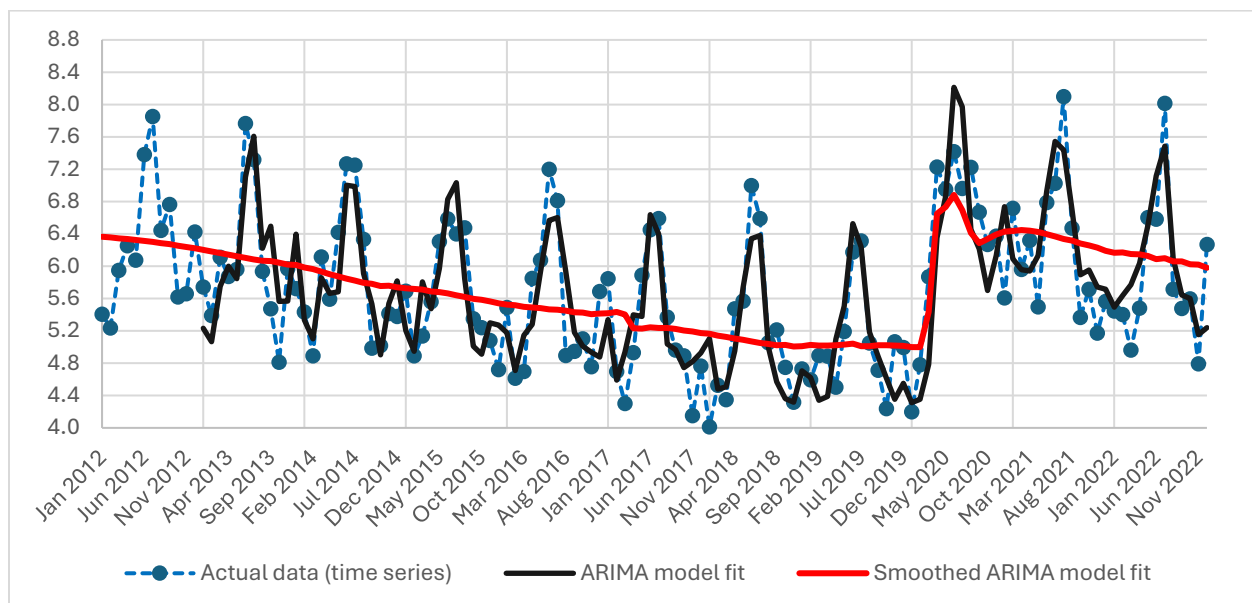


Figure A7

Percentage of crashes that were single-vehicle nighttime crashes in Colorado, 2012–2023

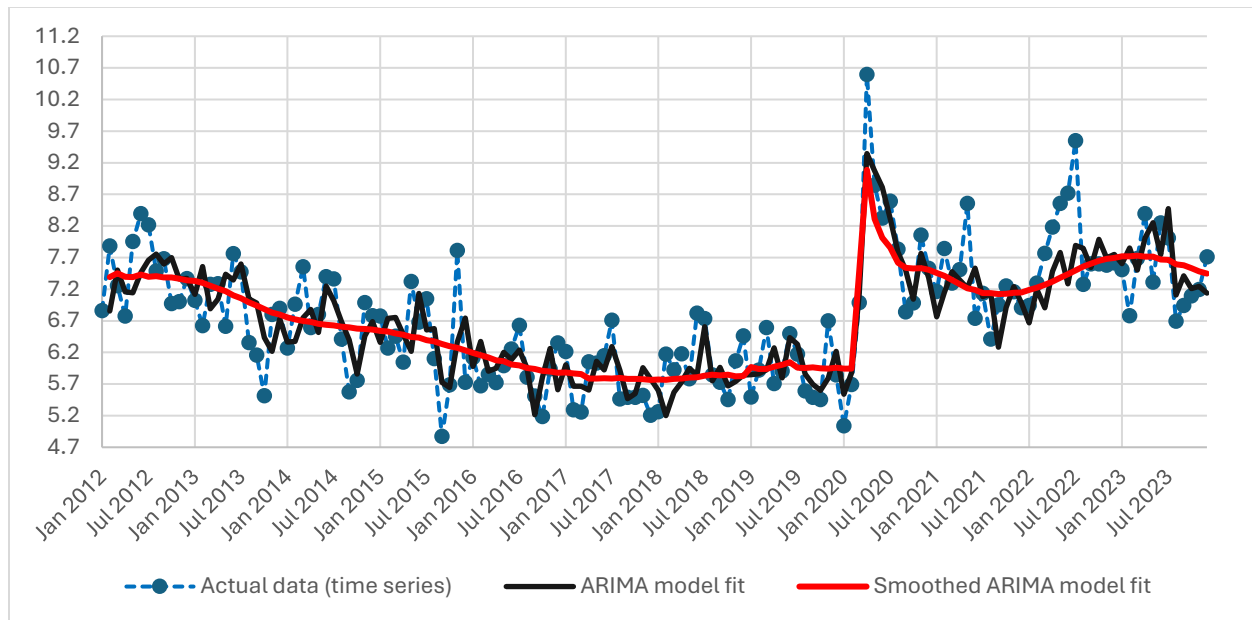


Figure A8

Percentage of crashes that were single-vehicle nighttime crashes in Idaho, 2012–2022

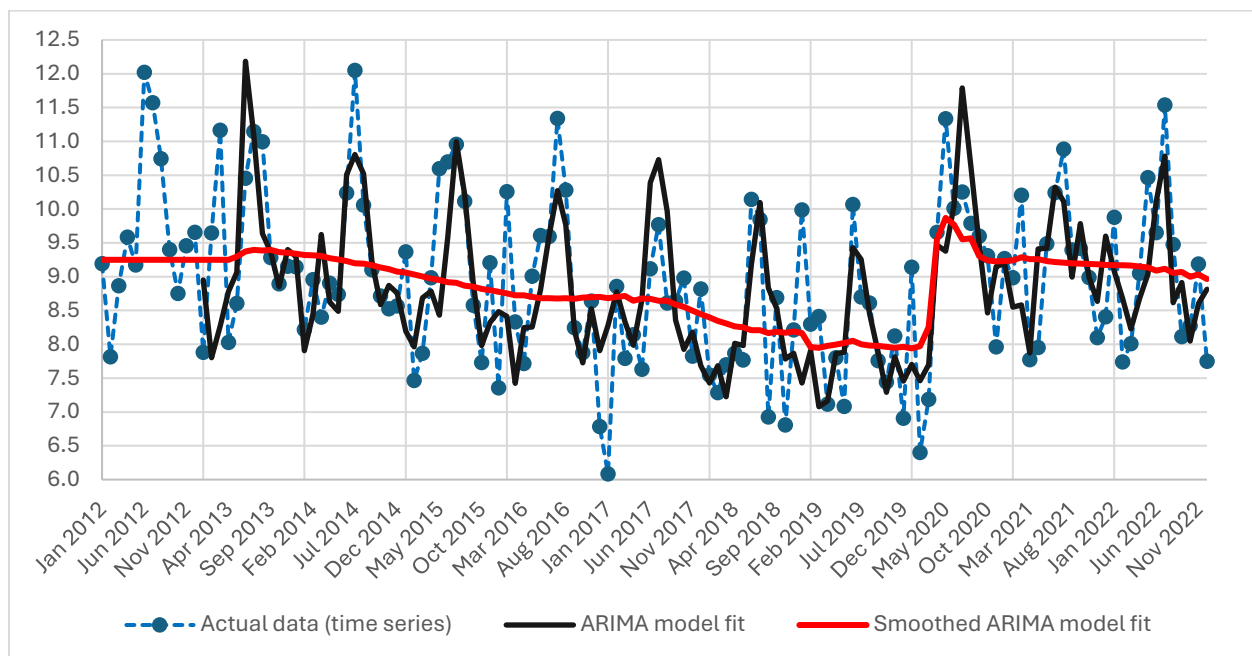


Figure A9

Percentage of injury crashes that were single-vehicle nighttime crashes in Utah, 2010–2022

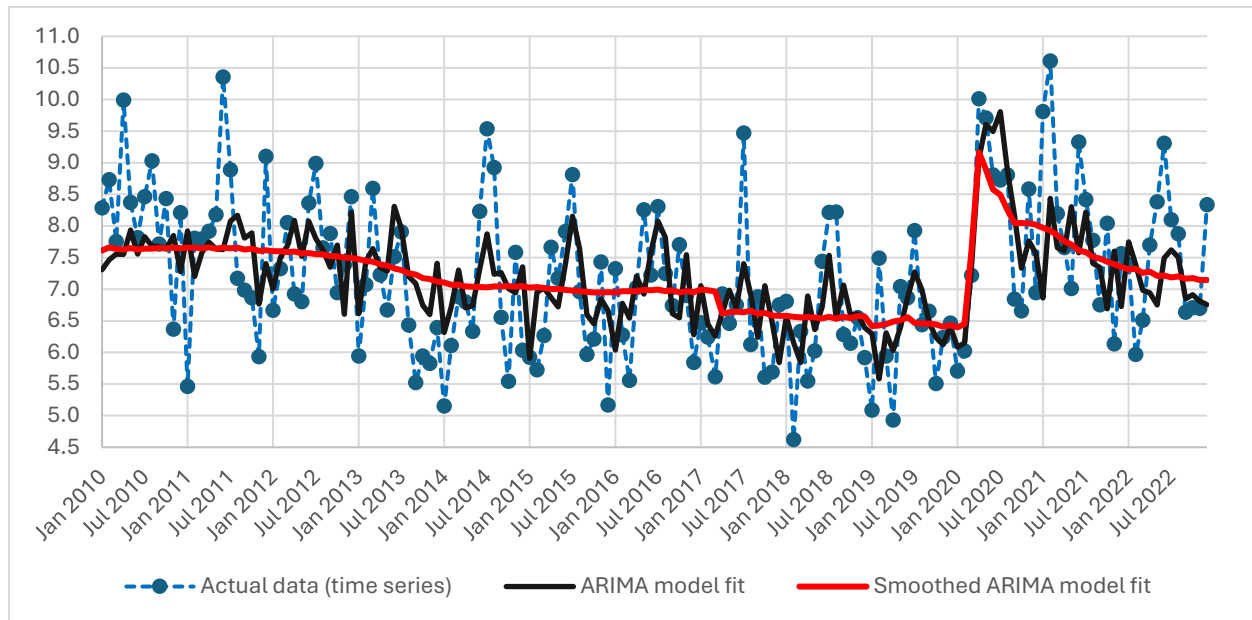


Figure A10

Percentage of injury crashes that were single-vehicle nighttime crashes in Arizona, 2012–2022

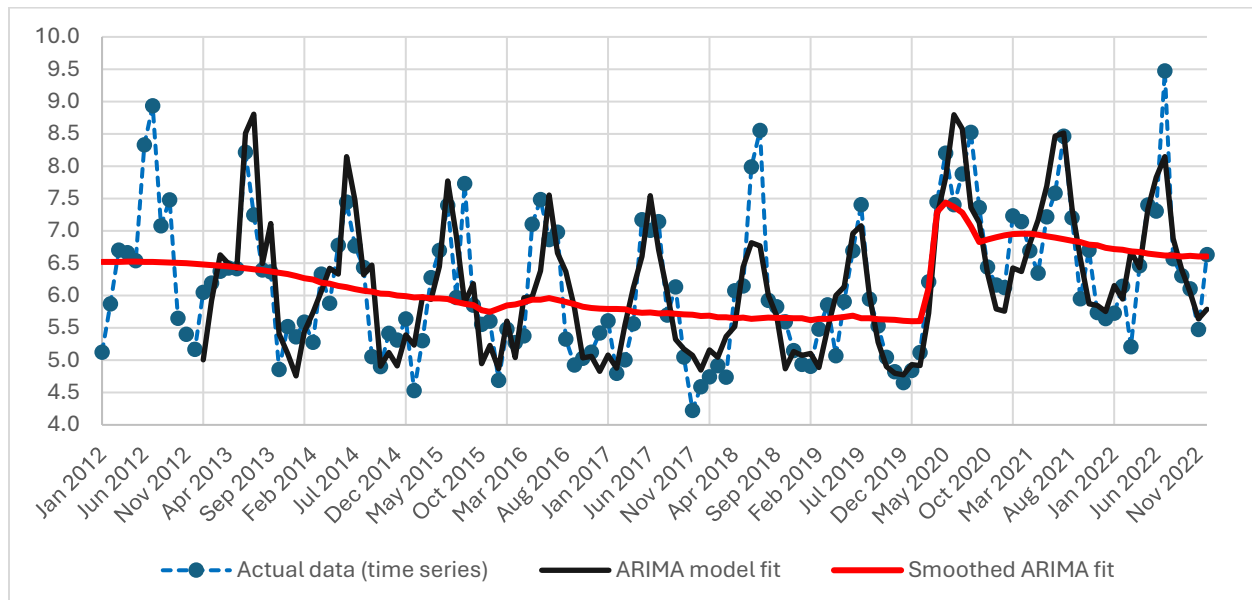


Figure A11

Percentage of injury crashes that were single-vehicle nighttime crashes in Colorado, 2012–2023

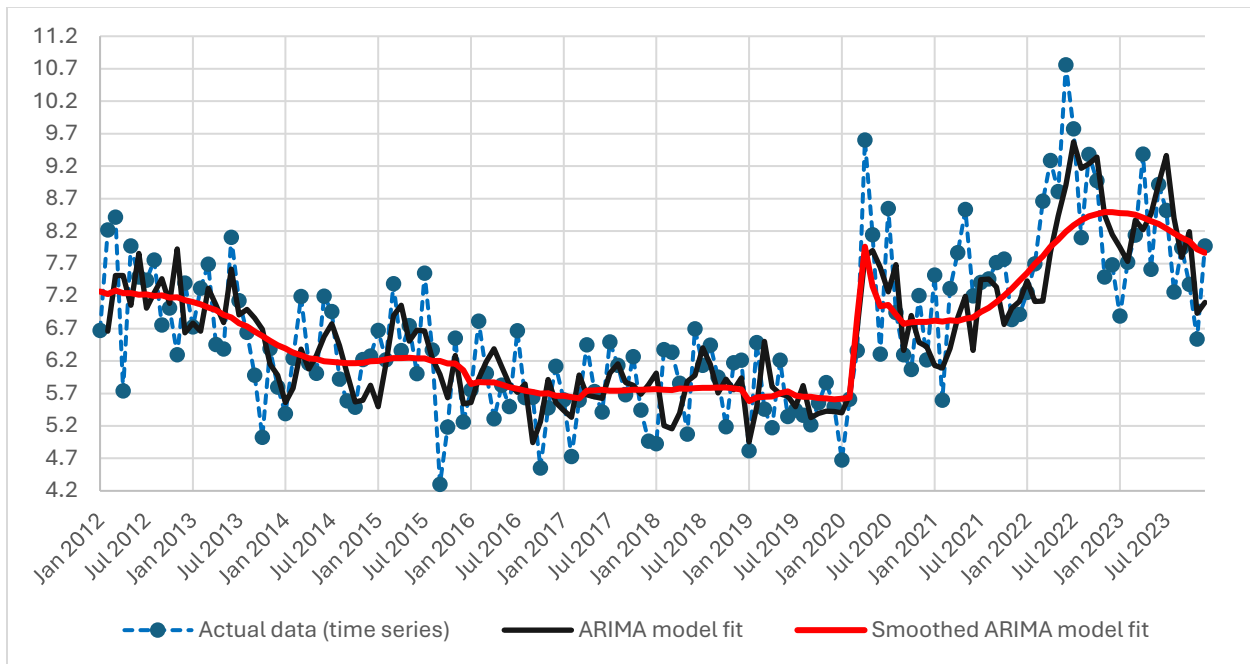


Figure A12

Percentage of injury crashes that were single-vehicle nighttime crashes in Idaho, 2012–2022

