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Reconstructing Sober Driving Baselines: Strategy for Evaluating Automated Driving Systems Beyond the Status Quo

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ABSTRACT

Introduction: The status quo of driving risk in the US is skewed by a small minority of the population driving with alcohol intoxication. This study evaluates this premise by quantifying the distinct crash rates of both impaired and unimpaired driver cohorts, shedding light on the true safety burden of alcohol on the *status quo*. To evaluate Automated Driving Systems (ADS) against a normative expectation of how humans ought to drive, localized, expectations-based benchmarks are required.

Method: This study utilized exposure reconstruction across the 50 most populous U.S. urban areas, disaggregating vehicle miles traveled (VMT) and crash rates temporally (day versus night, weekday versus weekend) using INRIX telematics. By pairing U.S. Fatality Analysis Reporting System (FARS) crash outcomes with established, continuous relative risk curves, this study back-calculated alcohol impaired exposure, which was then used to estimate an unimpaired baseline rate. The reconstructed exposure was then carried downstream to scale county-level *police-reported* crash rates in active SAE Level 4 deployment counties.

Results: When compared to the all drivers (the *status quo*), Removing alcohol-involved drivers (the *sober* cohort; BAC = 0.00) reduced the average fatal crash involvement rate by 23% (from 1.42 to 1.10 incidents per 100 million miles), varying geographically from 14% in Minneapolis to 32% in Houston. For these sober drivers, nighttime fatal crash risk remained 3.1 to 3.9 times higher than daytime risk, which demonstrates that elevated nighttime risk is likely heavily influenced by other known risk factors such as reduced visibility, fatigue, speeding, and responding to alcohol-impaired road users. For lower-severity police-reported crashes, the *sober* baseline rate was 6.0% to 11.0% lower than the *status quo*, reflecting structurally lower relative risk multipliers than in fatal crashes.

Conclusions: Isolating and removing alcohol impairment established a localized, expectations-based safety benchmark. Using flat national averages introduces extreme spatial bias, underestimating local safety burdens in high-exposure markets. These findings demonstrate that while comparing against the *status quo* is essential for measuring the net safety impact of replacing active drivers, evaluating rule-compliant ADS fleets against a *sober* aggregate provides an expectations-based target to actively raise the roadway safety bar.

Practical Applications: Municipal planners can cheaply monitor localized impairment exposure using fatal crash proportions and continuous relative risk models. ADS developers can use these *sober* human baselines to establish geographically aligned, expectations-based safety targets.

1. INTRODUCTION

1.1 The Status Quo

The *status quo* of roadway fatalities is heavily influenced by alcohol-impaired drivers, a contributor that renders current safety levels unacceptable. *Legally-impaired*, defined herein as blood alcohol concentrations (BAC) of 0.08 grams per deciliter (g/dL) or higher, drivers were involved in approximately 30% of the 40,901 U.S. roadway fatalities reported in 2023 (NCSA 2025a, 2025b). While this aggregate statistic encompasses all roadway actors, the specific burden on passenger vehicle operations is reflected in a fatal crash involvement rate of 1.57 per 100 million vehicle miles traveled (VMT) across the US (Scanlon 2026c). Notably, the consequences of alcohol impairment are often borne by those within the impaired vehicle; 73% of alcohol-involved fatalities are the impaired driver (60%) and their occupants (13%) (NCSA 2025a). The remaining 27% of the traffic safety burden in these collisions was composed of other vehicle occupants (16%) or non-occupants, such as pedestrians and cyclists (11%) (NCSA 2025a).

The Vision Zero framework has the foundational principle that roadway fatalities are inherently unacceptable (Larsson et al., 2010; Lie, 2001; Tingvall, 1997, 2022). Achieving this safety state necessitates the systematic elimination of established risk contributors with the Safe System Approach identifying "Safer People" as a critical pillar for harm reduction (U.S. DOT, 2022). From this perspective, aggregate human driving baselines are fundamentally distorted because they accept systemic safety failures as a baseline condition. Recent analysis by Campolettano et al. (2026) examined the passenger vehicle traffic safety burden through this lens, demonstrating that driver impairment (43%), speeding (28%), and restraint nonuse (53%) were collectively present in over three-fourths (77%) of fatal crash involvements.

Consequently, this study focuses specifically on the safety burden of alcohol on the status quo of roadway safety by developing crash involvement rate benchmarks independently for alcohol-impaired and -nonimpaired drivers. The elevated crash risk associated with alcohol has been extensively documented over several decades, consistently demonstrating a disproportionate risk relative to *sober* (BAC = 0.0) drivers (Borkenstein et al., 1974; Zador, 1991; Zador et al., 2000; Blomberg et al., 2005; Voas et al., 2012; Lacey et al., 2016; Romano et al., 2018). While historical relative risk curves have been evaluated relative to a *sober* baseline, real-world population assessments require evaluating the risk of entire driving cohorts. By applying the mathematical risk models of Voas et al. (2012) and Lacey et al. (2016) to the empirical BAC exposure distributions of active drivers, this current study utilizes composite, population-weighted cohort relative risks. Under this framework, we account for the actual BAC distribution of active drivers, including the "long-tail" of high-BAC driving where risk increases exponentially. The composite relative risk of the entire *legally-impaired* cohort is over 30 times higher for fatal crashes and over 8 times higher for police-reported crashes compared to the *sub-threshold* BAC ($0.00 \leq \text{BAC} < 0.08$) cohort.

Because crash risk is a measure of outcomes relative to exposure, this significantly heightened risk means that a small volume of impaired driving exposure accounts for a massive, disproportionate share of fatalities (Scanlon et al., 2025). Periodic roadside exposure evaluations from the National Highway Traffic Safety Administration (NHTSA) National Roadside Survey (NRS) demonstrate that *legally-impaired* drivers are present in only 0.4% of weekday daytime driving and 1.5% of weekend nighttime driving (Ramirez et al., 2016). Even during peak exposure hours in late weekend nighttime periods (Friday and Saturday nights from 1:00 to 3:00 a.m.), *legally-impaired* drivers represent only 3.3% of active driving exposure. The fact that this small exposure fraction accounts for approximately one-third of all U.S. fatal crash involvements (NCSA, 2025a) highlights the profound distortion that alcohol impairment introduces to the *status quo* human driving baseline. Isolating and removing this distortion is a prerequisite to establishing a true expectations-based safety benchmark, which reflects normative human performance and is differentiated from *status quo* evaluations in the subsequent discussion.

1.2 Evaluating Automated Driving Systems Using Status Quo and Expectations Benchmarks

The international deployment and scaling of Automated Driving Systems (ADS) are progressing at a rapid pace. Within the framework of this research, ADS are characterized as SAE Level 4 systems, which are designed to execute the entirety of the dynamic driving task (DDT) and the associated DDT fallback within a defined operational design domain (ODD) without a fallback-ready user (SAE International, 2021). While the analytical focus of this study remains centered on Level 4 systems, the resulting findings carry significant implications for safety performance evaluation across the broader spectrum of automation (SAE Levels 3–5). Although the current penetration of ADS remains relatively limited, the technology is increasingly integrating into the active transport network, necessitating a rigorous framework for evaluating its aggregate safety impact.

A robust and expanding body of literature focuses on the evaluation of ADS through safety performance assessments, also known as retrospective safety impact analyses (Scanlon et al., 2025). Safety performance assessment methodologies have evolved to provide incremental, multi-faceted safety signals as fleets scale (Scanlon et al., 2026a). To ensure valid "apples-to-apples" comparisons, researchers have developed the RAVE Checklist, which provides rigorous methodological guidance for spatial, temporal, and exposure-matched baseline corrections (Scanlon et al., 2025). Benchmarking techniques have progressed from broad national comparisons to highly localized, geographic-specific evaluations at the municipal, county, and grid-based spatial heat maps (Chen et al., 2025; Teoh and Kidd, 2017; Teoh et al., 2026; Scanlon et al., 2024). Further refinements have integrated temporal controls to adjust for risk variations (Kusano et al., 2026; Scanlon et al., 2026b) and disaggregated crash performance by conflict typology and roadway classification (Kusano et al., 2025; Scanlon et al., 2026c).

Finally, researchers have applied a third-party liability and crash responsibility lens to evaluate the rate of insurance claims relative to human drivers (Di Lillo et al., 2024, 2025).

The vast majority of past research has utilized *status quo* benchmarks of the entire population of drivers (Scanlon et al., 2026a). This baseline methodology captures the aggregate performance of the active human driving population, enabling researchers to quantify the net safety impact of a technology relative to current roadway actors. The utilization of status quo benchmarks is a well-established paradigm within the automotive industry, serving for decades as a foundational lens for evaluating the safety impact of both passive and active vehicle safety systems. The *status quo* benchmark represents the current performance of the driving population, and is the logical reference point when assessing the potential novel technology on improving traffic safety. The *status quo* benchmark represents an empirical expectation, that is, reflecting the collective beliefs regarding how roadway actors actually behave based on past experiences and observed reality (Fraade-Blanar et al., 2026). This *status quo* benchmark does not constitute a normative statement regarding the sufficiency, reasonableness, or public acceptance of a technology (Scanlon et al., 2026a), rather, it simply represents a comparison with the status quo.

While *status quo* benchmarks provide a highly interpretable measure of aggregate risk relative to active roadway actors, they necessarily group all driving behaviors into a single, unadjusted baseline. Consequently, evaluating a technology strictly against the *status quo* measures its performance against a baseline that aggregates law-abiding driving with high-risk or illegal behaviors, such as speeding, distracted driving, or alcohol impairment. This limitation highlights the critical need for benchmarks representative of expectations around driving performance (Fraade-Blanar et al., 2026). Prior research has established that setting these expectations benchmark targets, such as benchmarking against a mature, skilled, or law-abiding driver, helps promote a safer, progressive baseline for technological evaluation (Shladover and Nowakowski, 2019). Furthermore, because human drivers routinely overestimate their own driving capabilities (Svenson 1981), they may consequently expect ADS to perform at a level significantly higher than the average human driver (Nees, 2019). Survey data research has shown that the public expects an ADS to be four to five times as safe as human drivers, achieving a 75% to 80% reduction in fatalities to meet the "tolerable risk" criterion (Liu et al., 2018). Furthermore, for widespread societal acceptance, the public anticipates that an ADS should be two orders of magnitude (99%) safer than current global road traffic risks (Liu et al., 2018). To this end, expectations-based, or normative, benchmarks can be helpful in evaluating technology against a baseline that isolates these distinct risk behaviors.

Only two prior studies, to the knowledge of the authors, have applied this principle of an *expectations* benchmark to ADS evaluation. Goodall (2021) developed a “model driver” benchmark that looked to remove driver performance errors, impairment, drowsiness, and inattentiveness from *status quo* benchmarks. However, because Goodall’s (2021) model relied on relative risk models from naturalistic driving data (specifically the SHRP-2 study), it relied on visual observation of impairment rather than toxicological measurements and was limited to a

national-level aggregation estimation across all crash severities. In another study, the work by Di Lillo et al. (2025) focused on third-party claims rates for only the latest available fleet of human-driven vehicles. These model years were selected as an expectations-based benchmark because of the increasing prevalence of active safety and advanced driver assistance systems (ADAS), which, in turn, represented a higher-performing, modern human vehicle fleet. The non-impaired driver benchmarks developed as part of this study contribute to providing an additional expectations-based benchmark for ADS safety assessments.

1.3 The Data Challenge and Reconstructing Exposure

This paper establishes an expectations-based benchmark that can serve as a rigorous comparison point for Automated Driving System (ADS) safety performance assessments. Alcohol impairment exerts a massive safety burden on the status quo of roadway safety, and its effect can be mathematically isolated and removed to formulate a sober driving baseline.

However, an inherent challenge exists in formulating any expectations benchmark from empirical crash and exposure data. Under ideal circumstances, expectations benchmarks would be computed directly from available databases. In the case of alcohol consumption, subselecting drinking drivers from crash and VMT exposure data would enable a direct crash rate calculation. Two fundamental data limitations prevent this direct approach:

(1) While U.S. crash databases typically encode alcohol involvement, the recording of alcohol is highly inconsistent across jurisdictions. State-level databases often rely on subjective officer indications or citation records, whereas national databases report BAC and utilize post-hoc imputation methods for missing toxicological data (NCSA, 2025c). Any geographically localized benchmark ideally features objective blood alcohol concentration (BAC) measurements to ensure valid comparisons; relying instead on subjective police crash reports introduces inter-jurisdictional reporting and potentially fails to isolate low-level, sub-threshold impairment (e.g., BAC<0.08).
(2) More critically, traffic exposure databases do not track or disaggregate VMT data by driver impairment level. While federal agencies publish aggregate VMT estimates, there is no real-world mechanism that actively measures how many miles are driven by alcohol-consuming drivers within a specific municipality or temporal window.

This study bridges this data deficit by leveraging the established scientific concept of "exposure reconstruction," wherein unobserved historical exposure is mathematically back-calculated from observed outcomes using known exposure-response relationships (Esmen, 1991). By utilizing the detailed fatal crash census from the Fatality Analysis Reporting System (FARS), which contains imputed BAC estimates as a measure of the fatal crash outcome "response" (NCSA, 2025c), and pairing these outcomes with established, continuous relative risk models, this study directly calculates the proportion of VMT (f_i) driven at various BAC thresholds. This reconstructed

exposure is then mapped to localized, urban VMT baselines to generate high-resolution, geographically specific non-alcohol-consuming benchmarks.

1.4 Objective

The goal of the current study was to compute both *police-reported* and *fatal* crash involvement rate benchmarks for passenger vehicle drivers at various levels of impairment. *Fatal* crash rates for passenger vehicles were examined across the 50 most populated U.S. urban areas. *Police-reported* crash rates were examined in a subset of those areas with active ADS deployments. Many research questions were addressed:

(1) What alcohol consumption is occurring prior to driver fatal crash involvements?
(2) What is the current prevalence of driving following alcohol consumption?
(3) How much does alcohol consumption influence overall driving population crash involvement rates?
(4) How do these effects depend on geographic area?
(5) How do these crash involvement rates vary by time of day?

2 METHODOLOGY

2.1 Driver Impairment Groupings

Drivers were categorized into four distinct, toxicological cohorts (listed in Table 1). The subgroups were intentionally paired to enable downstream analysis. The "Alcohol Presence" comparison group was motivated by the relative risk literature indicating that any alcohol consumption affects your crash risk (Lacey et al., 2016, Voas et al., 2012). Secondarily, in the US, a BAC of 0.08% is commonly (but not universally) used as the *Legal Threshold* for alcohol impairment.

Table 1: Definition of Alcohol Impairment Subgroups based on alcohol BAC. Crash rates were computed for each alcohol impairment subgroup. A BAC of 0.01 is the lowest possible, non-zero recording across all data sources.

Alcohol Impairment Group	Alcohol Impairment Subgroup	BAC
Overall (all drivers)	*Status Quo*	Any value
Alcohol Presence	*Sober*	= 0
	Any Alcohol	≥ 0.01
Legal Threshold	*Sub-Threshold*	< 0.08
	Legally Impaired	≥ 0.08

2.2 Analysis Overview

The analysis was executed in two distinct, sequential phases to transform unadjusted, *status quo* human driving crash rates into benchmarks that are localized and disaggregated by alcohol impairment status. This methodological structure, conceptualized in Figure 1 and summarized in the two subsequent sections, directly replicated and extends the spatial, temporal, and exposure-matched processing frameworks established by Scanlon et al. (2026c) for fatal crash involvements and Scanlon et al. (2026a) for police-reported crash involvements. By leveraging these pre-established baselines, the current study demonstrates how aggregate roadway crash rates can be systematically disaggregated to isolate the unique safety contributions of alcohol impairment driver cohorts.

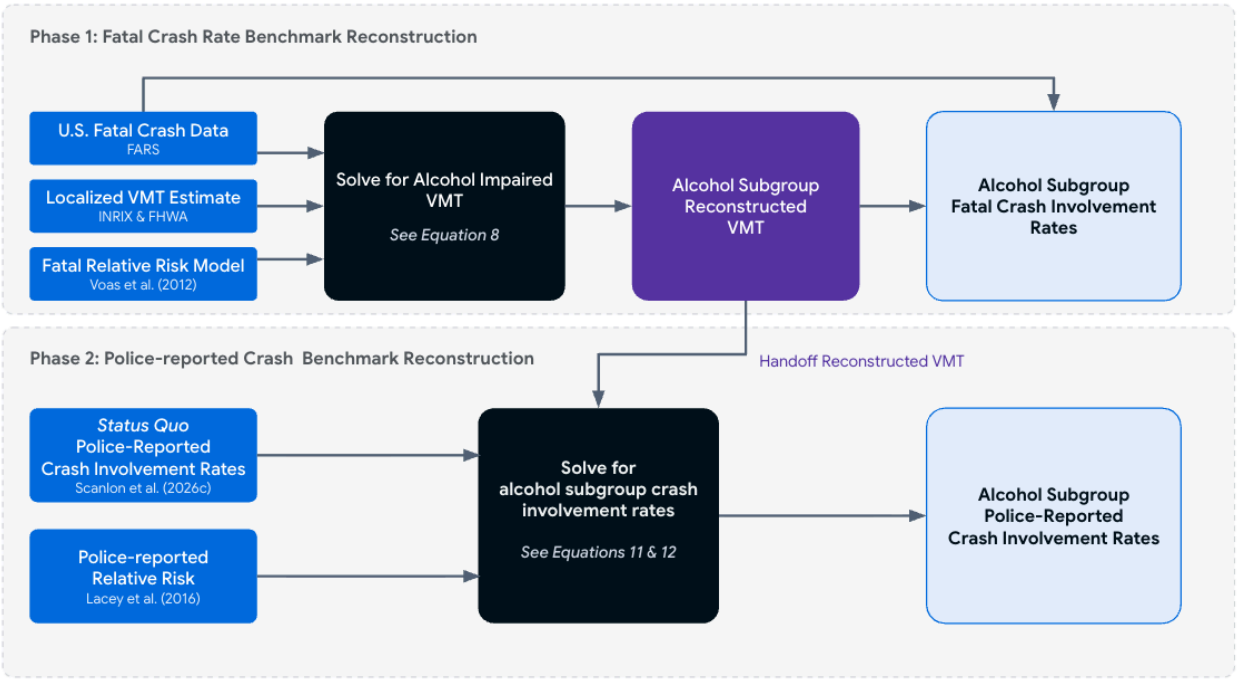


Figure 1. Conceptual overview of the methodology for reconstructing alcohol subgroup VMT and computing resultant crash involvement rates.

2.2.1 Phase 1: Fatal Crash Rate Benchmark Reconstruction

The first phase of the methodology estimates the fatal crash rates by alcohol impairment subgroups. BAC estimates are available in US fatal crash data but not in the available driving VMT data. The methodological process for estimating VMT data for various alcohol subgroups and the resulting fatal crashes are summarized as follows:

(1) The proportion of drivers involved in fatal crashes above the established BAC threshold for the impairment group (P_i; see equation 3) was calculated from historical crash records.

(2) A composite relative risk value (RR_{fatal}) was estimated to represent the driving population in each alcohol impairment subgroup. Integrating the continuous risk curves over the representative driving population enabled a single composite relative risk value representing the opposing impairment subgroups (Section 2.3 describes this methodology).
(3) These two values, P_i and RR_{fatal}, were used to solve the closed-form exposure reconstruction equation (f_i, the proportion of VMT driven by the "impaired" subgroup; see Equation 8).
(4) Combining f_i with localized overall population VMT estimates enabled the estimation of VMT for each alcohol subgroup.
(5) Final fatal crash involvement rates for each alcohol subgroup were then estimated from the VMT estimates and the observed fatal crash data (Equations 9 and 10).

2.2.2 Phase 2: Police-reported Crash Benchmark Reconstruction

The second phase of the analysis extended the benchmark methodology to *police-reported* crash rates, where BAC measurements for involved crash participants are generally not available. The methodological process is summarized below:

(1) The reconstructed exposure proportions (f_i) for alcohol subgroups calculated in Phase 1 were carried downstream to the police-reported analysis. Each urban area with reconstructed impaired VMT was associated with the respective US county presented (e.g., Austin, TX was associated with Travis County, TX).
(2) Similar to the methods in phase 1, a composite relative risk value specific to police-reported crashes (RR_{PR}) was computed.
(3) Third, the unadjusted, *status quo* police-reported crash rates were mathematically scaled using the reconstructed exposure proportions and the composite relative risk value (Equations 11 and 12).

2.3 Exposure Reconstruction Mathematical Formulation

2.3.1 Foundational Equations

The mathematical approach for the current study is established through seven foundational relationships (equations 1-7) that are algebraically solved to represent the downstream governing equations.

The mathematical approach divides all crashes and VMT data into two buckets of drivers, those below (unimpaired, *u*) and above (impaired, *i*) the BAC threshold for the respective alcohol subgroups. In principle, the current methodology could be extended to any BAC cutoff threshold. Total crash involvements (C_{total}) are the aggregate sum of unimpaired (C_u) and impaired (C_i) involvements (Equation 1). Total exposure (VMT_{total}) is the summation of unimpaired (VMT_u) and impaired (VMT_i) travel (Equation 2).

[1] $$\mathrm{C}_{total} = \mathrm{C}_u + \mathrm{C}_i$$

[2] $$\mathbf{VMT}_{total} = \mathbf{VMT}_u + \mathbf{VMT}_i$$

Two new terms are added to represent the proportion of data attributable to the impaired subgroup. The proportion of alcohol-impaired *fatal* crash involvements relative to the total population is defined as P_i (Equation 3). The proportion of VMT attributable to alcohol-impaired drivers is derived as f_i (Equation 4).

[3] $$\mathrm{P}_i = \frac{\mathrm{C}_i}{\mathrm{C}_{total}}$$

[4] $$\mathrm{f}_i = \frac{\mathrm{VMT}_i}{\mathrm{VMT}_{total}}$$

Lastly, this study set out to estimate crash involvement rates, which are computed at the vehicle-level and represented as the ratio of crash involvement frequency to total driving VMT. The crash involvement rate for the impaired population (CIR_i) and unimpaired (CIR_u) are represented by the quotients in Equation 5 and 6. Relative Risk (RR) is the ratio of impaired to unimpaired risk (Equation 7).

[5] $$\mathbf{CIR}_i = \frac{\mathrm{C}_i}{\mathrm{VMT}_i}$$

[6] $$\mathrm{CIR}_u = \frac{\mathrm{C}_u}{\mathrm{VMT}_u}$$

[7] $$\mathrm{RR} = \frac{\mathrm{CIR}_i}{\mathrm{CIR}_u}$$

2.3.2 Derivations for implementing Phase 1 *fatal* crash rates

As detailed in section 2.2.1, Phase 1 first required determining the amount of VMT with impairment. The foundational equations can be solved to represent f_i as a function of both RR_{fatal} and $P_{i,fatal}$. This equation can simply be interpreted as: if one knows the proportion of fatal crash involvements with impairment and the relative risk of driving with and without impairment, then one can determine the proportion of impaired driving VMT that must have occurred to have created those fatal crash outcomes.

[8] $$\mathrm{f}_i = \frac{\mathrm{P}_{i,fatal}}{\mathrm{RR}_{fatal} - \mathrm{P}_{i,fatal} \cdot (\mathrm{RR}_{fatal} - 1)}$$

Fatal crash involvement rates for the impaired ($CIR_{i,\ fatal}$) and unimpaired ($CIR_{u,\ fatal}$) subgroups can then be directly computed by solving for equations 9 and 10.

[9] $$\mathrm{CIR}_{u,fatal} = \frac{\mathrm{C}_{u,fatal}}{\mathrm{VMT}_u} = \frac{\mathrm{C}_{u,fatal}}{\mathrm{VMT}_{total} \cdot (1 - \mathrm{f}_i)}$$

[10] $$\mathrm{CIR}_{i,fatal} = \frac{\mathrm{C}_{i,fatal}}{\mathrm{VMT}_i} = \frac{\mathrm{C}_{i,fatal}}{\mathrm{VMT}_{total} \cdot \mathrm{f}_i}$$

2.3.3 Derivations for implementing Phase 2 *police-reported* crash rates

The second phase computed the *police-reported* crash involvement rates by alcohol subgroups. This derivation is in Equations 11 and 12 for both the unimpaired and impaired subgroups, respectively. Alcohol subgroup crash involvement rates are a function of *status quo*, *police-reported* rates (CIR_{PR}), the previously computed f_i, and the police-reported composite relative risk estimate (RR_{PR}).

[11] $$\mathrm{CIR}_{u,PR} = \frac{\mathrm{C}_{total,PR}}{\mathrm{VMT}_{total} \cdot [1 + f_i(RR_{PR} - 1)]} = \frac{\mathrm{CIR}_{PR,total}}{[1 + f_i(RR_{PR} - 1)]}$$

[12] $$\mathrm{CIR}_{i,PR} = \mathrm{CIR}_{u,PR} \cdot RR_{PR}$$

2.4 Composite relative risk estimation

Mathematically reconstructing the unobserved exposure required using the observed outcomes and known exposure-response curves (i.e., relative risk, RR).

2.4.1 Alcohol-impaired Crash Risk Model Selection

This study selected two primary relative risk models representative of distinct crash severity thresholds: Lacey et al. (2016) for *police-reported* crashes (RR_{PR}) and Voas et al. (2012) for *fatal* crashes (RR_{fatal}). Candidate models were strictly restricted to case-controlled studies conducted within the United States to limit any geographic effects. Alternative candidate models considered, but ultimately excluded, are discussed in detail with a comparative assessment in Appendix A.

To model relative risk at the police-reported level, this study utilized the conditional logistic regression model (crash involvement risk as a function of BAC) from Lacey et al. (2016). This study is the most recent of a lineage of large-scale, police-reported case control studies

examining the effects of alcohol-impairment on crash risk (Borkenstein et al., 1964; Blomberg et al., 2005). The Lacey et al. (2016) study was a NHTSA sponsored study conducted in Virginia Beach, Virginia over a 20-month period from approximately 2010 to 2012. Over 3,000 drivers from crashed vehicles were investigated at the scene of the crash, where biological samples (breath, oral fluid, and blood) were obtained to estimate BAC. This study exclusively looked at non-freeway crashes to limit investigator on-scene risk. Exactly one week after the crash, two additional control drivers, per crash-involved driver, were investigated at the same crash location at the same time of day and travel direction. Because the Lacey et al. (2016) report only publishes a static lookup table of discrete, rounded relative risk estimates rather than the underlying regression coefficients, this study utilized the raw, participant-level data published by NHTSA to directly refit the Lacey et al. (2016) "Model 1", which is the conditional logistic regression of alcohol-related relative risk adjusted for age and gender using the maximum-power sample of 10,221 drivers. Two alternative models, Model 2 (oral fluid only) and Model 3 (drug negative), were not selected to minimize non-participation bias from oral fluid collection and preserve real-world *status quo* baseline representativeness rather than isolating an artificial, drug-negative cohort. Refitting this model from the raw dataset was also methodologically necessary to extract the parameter covariance matrix and model statistical uncertainty downstream. The model implementation was verified using the published relative risk estimates in the Lacey et al. (2016) report.

To model fatal crash involvement risk, the current study utilized a national-level case control study conducted by Voas et al. (2012), which was one of a series of studies conducted that all used similar methodology (Zador, 1991; Zador et al., 2000; Romano et al., 2018). Voas et al. (2012) examined fatal crash involvements throughout the US from 2006 and 2007. Driving exposure data by BAC was obtained in this study from the 2007 National Roadside Survey (NRS), which surveys breath-test sampled drivers from a representative US sample.

Voas et al. (2012) derived multiple sets of fatal crash involvement risk models. This current study specifically relied on two of those models predicting crash involvement risk (in any crash type) that resulted in (a) the driver being fatally injured or (b) the driver not being fatally injured (but some other person was fatally injured). These two models are predicting mutually exclusive events, and, for this current study, were combined (detailed in the following section) to formulate a single model predicting crash involvement for *any* person being fatally injured.

2.4.2 Two-Stage Composite Relative Risk Estimation

Four unique composite relative risk estimates (two alcohol-impairment comparison groups and two severity levels) were needed. Each composite relative risk estimation represents the relative probability of a crash involvement between each opposing alcohol subgroups (see table 2). For example, the fatal crash involving relative risk (RR_{fatal}) for the "alcohol presence" group compared *any alcohol* drivers to *sober* drivers. Importantly, the impaired subgroups have a long-tail of higher alcohol impairment at high BACs, where risk of a crash involvement

increases exponentially. The composite average approach enabled representation of the distribution of drivers within this impaired cohort.

The estimation process was executed in two distinct stages. The first step necessitated the characterization of the population BAC exposure distribution, which was essential for establishing representative BAC profiles within the alcohol subgroups. This study utilized downloadable raw data from the 2013-2014 NRS (Berning et al., 2015) to construct these distributions, which is a nationally-representative dataset of BAC-levels for the driving population. NRS data is sampled data from specific locations throughout the US. Geographic-specific distributions were not readily available. The imputed BAC metric ("imputed_BAC2") served as the primary measure for BAC concentration. Furthermore, to preserve temporal specificity, BAC distributions were stratified into binary temporal cohorts via the "data_session_n" variable, identifying *daytime* (06:00–18:00) and *nighttime* (18:00–06:00) periods. Driver age and gender, both key predictors in the *fatal* and *police-reported* risk model, were extracted through the "DriverAgeYears" and "Gender" variables. Sample weighting ("NEW_FINAL_weights") was also applied in the analysis for national representativity.

The second step estimated a relative risk value for the opposing alcohol subgroupings (e.g., *sober* versus *any alcohol*). Consistent with best practices in this literature, odds ratios were used as a direct estimation of relative risk given the relative infrequency of fatal crashes relative to exposure (Agresti, 2002; Zador et al., 2000; Voas et al., 2012; Lacey et al., 2016). Baseline odds were established using a 0.00 BAC driver. Every person within the respective relative BAC exposure distribution (from the prior step with NRS data) was assigned odds based on their BAC. Again, the Voas et al. (2012) fatal risk paper relied upon two mutually-exclusive models needed to estimate crash involvement relative risk: (1) driver involved and fatally injured and (2) driver involved and not fatally injured. For the Voas et al. (2012) model, those odds were summed to represent the total odds. A risk score (with respect to the baseline) using the ratio of driver odds to baseline odds were then computed for each driver. A composite average of those risk scores were then computed using the aforementioned case weightings. Finally, the ratio of alcohol subgroups risk scores were combined to form a final relative risk value.

2.5 Crash and VMT Data

Crash involvement rates for 2023 were used because this was the latest year of data available across all datasets at the time of this analysis. This methodology directly replicated and leveraged findings from two recent studies to characterize the aggregate *status quo*. A summary table of the data sources and dimensions relied upon are provided in Table 2 below. First, Scanlon et al. (2026b) quantified baseline *fatal* crash involvement rates for the 50 most populous U.S. urban areas utilizing FARS data. As a comprehensive national database, FARS provides a systematic accounting of all crash events, vehicles, and persons involved in fatal collisions (NCSA, 2025c). Second, Scanlon et al. (2026c) was used for *police-reported* crash involvement rates. This study established county-specific crash involvement rates across multiple severity

levels for urban jurisdictions with active ADS deployments. This latter effort relied on state-compiled crash and VMT data. State crash data has varying levels of data quality but is generally of lower data granularity than FARS data (e.g., fewer data fields). Although both studies examined a variety of dimensions, including road type and crash types, this study focused on higher-level crash aggregations to demonstrate the associated methodology and alcohol effect sizes.

Table 2. Summary of Data Sources and Dimensions for Status Quo Crash Rate Characterization

	Urban Area Fatal Crash Involvements *Scanlon et al. (2026b)*	**County-level Police-reported Crash Involvements** *Scanlon et al. (2026c)*
Data Source: Crash	FARS	Respective State Agencies
Data Source: VMT	FHWA + INRIX (for temporal adjustment)	Respective State Agencies + FHWA
Geographic Areas	US top 50 most populated urban areas	Select US counties with current or imminent ADS deployments
Vehicle Type	Selected in-transport, passenger vehicles (49 CFR § 565.15)	
Road Type	All roads combined	
Severity Levels	*Any fatal*: at least one person was fatally injured.	*Police-reported*: the crash was reported by police within the database.
Temporal Data	Four temporal cohorts: Daytime (6 AM–6 PM) vs. Nighttime (6 PM–6 AM) and Weekdays (Mon 6 AM–Fri 6 PM) vs. Weekends (Fri 6 PM–Mon 6 AM).	N/A; no temporal analysis was performed.

This study methodology required *fatal* crash-involved driver BAC estimations, which was outside the scope of Scanlon et al. (2026c). To supplement the aggregations with these alcohol BAC levels, the FARS imputed alcohol files (Rubin et al., 1998; Subramanian, 2002) were relied upon. The "MIPER" data file contains 10 imputed driver-level BAC values. The proportion of

imputed driver BAC values that fell within each of the alcohol subgroups were then used as weighting values for the respective subgroup.

2.6 Calculating P_i using three years of crash data

When analyzing fatal crash involvement rates, this study reconstructed alcohol subgroup VMT separately for each of the four temporal slices listed in Table 2. The *status quo* VMT from Scanlon et al. (2026c) incorporated spatial traffic volume data from INRIX to redistribute geographic aggregate VMT data by time-of-day and day-of-week dimensions (INRIX, 2024). Attributing the VMT to alcohol subgroups required fatal crash involvement observations by BAC within the temporal groups (i.e., the computation of P_i; the proportion of fatal crash involvement with an impaired driver). For some urban areas, slicing the data by narrow temporal and impairment groupings resulted in very few crash involvement observations and high associated variance. The three most recent years of crash data (2021 to 2023) were used to estimate P_i to improve the precision of the reconstructed VMT estimations. The estimation of P_i was the only factor to use multiple years of data. The remaining estimates were done using only 2023 data.

2.7 Modeling Statistical Uncertainty

There were several sources of uncertainty throughout each step of this analysis. To robustly propagate each source of uncertainty down to the final crash involvement rates, we employed monte-carlo simulation with 1,000 iterations. Uncertainty from two primary components are being modeled. First, the composite relative risk estimation representing each alcohol subgroup cohort. Secondly, statistical uncertainty inherent in the frequencies of both *fatal* and *police-reported* crash involvements was characterized using a Poisson distribution.

For the composite relative risk estimation, resampling with replacement was performed to create new representations of the NRS data. There was also uncertainty in the underlying risk models relied upon. For both the Lacey et al. (2016) and Voas et al. (2012) models, model coefficients were resampled in each simulation. In Lacey et al. (2016), the model was recreated in R, and coefficients were resampled from a multivariate normal distribution. In Voas et al. (2012), only the standard error for each coefficient was available, and each coefficient was resampled by assuming normal distribution and independence of those coefficients. In each of the resamplings, the aforementioned relative risk calculations were applied, and the resulting distribution was stored for use in the subsequent rates analysis.

Statistical uncertainty surrounding the observed *fatal* and *police-reported* crash involvement counts were modeled assuming that crash involvements followed a Poisson distribution. The 95% confidence limits were derived using the exact relationship between the Poisson and chi-squared distributions, where the lower and upper bounds were determined by the 0.025 and 0.975 percentiles of the chi-squared distribution, respectively.

3 RESULTS

3.1 Computed Composite Relative Risk Values

The derived composite relative risk estimates are presented with associated confidence intervals in Table 3. The estimates demonstrated that alcohol impairment exerts a disproportionately greater influence on *fatal* crash involvement risk relative to *police-reported* outcomes. This observation reinforces the finding that the safety burden of alcohol consumption scales with crash severity. Because crash risk follows an exponential trajectory relative to blood alcohol concentration, the composite relative risk for the *alcohol presence* comparison is inherently lower than that of the *legal threshold* comparison.

Table 3: Relative risk estimations by Driver Impairment group and crash severity.

Comparison Groups (BAC Level)	Crash Involvement Severity Level	Relative-Risk Value Estimate (95% CI)
Any Alcohol (≥ 0.01) vs. Sober (= 0)	*Police-reported* ($RR_{pr,\ any}$)	2.80 (1.95, 4.20)
	Fatal ($RR_{fatal,\ any}$)	8.55 (5.03, 16.8)
Legally Impaired (≥ 0.08) vs. Sub-Threshold (< 0.08)	*Police-reported* ($RR_{pr,\ legal}$)	8.10 (4.96, 13.2)
	Fatal ($RR_{fatal,\ legal}$)	30.5 (16.6, 64.9)

3.2 Summary Statistics Comparing Alcohol Impairment Groups Fatal Crash Involvement Rates

Table 4 provides a breakdown of fatal crash involvement rates for both the *alcohol presence* and *legal threshold* comparison groupings. First, substantial crash involvement rate differences were observed between the *legally-impaired* drivers and *any alcohol* drivers. *Legally-impaired* drivers were found to have a *fatal* crash involvement rate of 32.4 incidents per 100 million miles (IP100MM), while drivers consuming *any alcohol* (a superset of the *legally-impaired* group) had a fatal crash involvement rate of 8.85 IP100MM. Interestingly, despite these large differences in the impaired subgroups, there was very little difference between *sub-threshold* and *sober* consumption subgroups. *Sub-threshold* and *sober* drivers had a fatal crash involvement rate of 1.13 and 1.10 IP100MM, respectively, which was less than a 3% difference.

Table 4. Average data year 2023 estimate across the 50 most populated US urban areas for various measures analyzed. The 95% confidence interval values shown are the mean values for the computed 95th percent confidence interval lower and upper bounds for each urban area.

Measure and Subgroup		Day (Weekday)	Night (Weekday)	Day (Weekend)	Night (Weekend)	All Day/Time Combinations
% Fatal Crash Involvements w/ Alcohol Impairment ($P_{i, fatal}$)	Any Alc.	10.7% (7.1%, 15.6%)	33.3% (26.8%, 40.5%)	16.3% (9.5%, 26.2%)	40.9% (34.4%, 47.6%)	27.1% (23.8%, 30.5%)
	Leg. Imp.	8.5% (5.3%, 12.9%)	27.2% (21.1%, 33.9%)	13.0% (7.1%, 22.3%)	34.0% (27.8%, 40.6%)	22.2% (19.2%, 25.5%)
% VMT w/ Alcohol Impairment (f_i)	Any Alc.	1.4% (0.6%, 2.7%)	5.7% (2.7%, 10.0%)	2.3% (0.9%, 4.9%)	7.7% (3.8%, 13.1%)	4.2% (2.1%, 7.1%)
	Leg. Imp.	0.3% (0.1%, 0.7%)	1.2% (0.5%, 2.4%)	0.5% (0.2%, 1.2%)	1.7% (0.8%, 3.3%)	0.9% (0.4%, 1.8%)
Fatal CIR Status Quo		0.71 (0.55, 0.91)	2.73 (2.10, 3.51)	0.88 (0.58, 1.31)	4.38 (3.46, 5.50)	1.42 (1.25, 1.62)
Fatal CIR(unimpaired)	Sober	0.65 (0.50, 0.83)	1.99 (1.48, 2.64)	0.76 (0.50, 1.13)	2.89 (2.18, 3.79)	1.10 (0.95, 1.27)
	Sub-Thresh.	0.65 (0.51, 0.84)	2.07 (1.55, 2.72)	0.77 (0.51, 1.14)	3.02 (2.30, 3.93)	1.13 (0.98, 1.30)
Fatal CIR (Impaired)	Any Alc.	5.00 (2.58, 11.2)	15.2 (8.68, 31.1)	6.12 (2.98, 14.3)	23.2 (13.7, 46.5)	8.85 (5.30, 17.6)
	Leg. Imp.	17.8 (8.28, 43.6)	56.5 (29.0, 128.9)	21.6 (9.62, 55.8)	86.7 (45.9, 193.5)	32.42 (17.5, 70.7)

The minimal differences between the *sober* and *sub-threshold* cohorts are primarily attributable to the sparse representation of fatal crash-involved drivers operating within the intermediate BAC range ($0.0 < BAC < 0.08$). Because the *no-alcohol* subgroup makes up roughly 95.8% of all VMT, It makes up the vast majority of drivers in both the *sober* and *sub-threshold* cohorts. The additional drinking drivers in the *sub-threshold* cohort below the 0.08 BAC threshold are only, on average, 3.3% of all drivers. Consequently, *sober* miles heavily dilute the modest risk of these low-level drinking miles, causing the crash involvement rates of the *sober* and *sub-threshold* cohorts to converge.

3.3 Reconstructed VMT by Alcohol Consumption

Figure 2 illustrates the estimated proportion of VMT attributable to the *any alcohol* cohort (f_i) across the 50 urban areas. Given that the exposure reconstruction methodology relies on a direct mathematical relationship between f_i and the proportion of fatal crash involvements with alcohol ($P_{i, fatal}$), both metrics exhibit similar geographic and temporal trends (see Appendix B for the corresponding $P_{i, fatal}$ distributions).

The proportion of *any alcohol* VMT was generally higher at nighttime compared to daytime (e.g., 3.3-times higher on weekend nights versus weekend days) and, to a lesser extent, during the weekend compared to the weekday (e.g., 1.4-times higher on weekend nights versus weekday nights). There were also considerable, significant differences (means outside of the respective

confidence intervals) in the proportion of *any alcohol* VMT between urban areas analyzed. During weekend nights, Cincinnati, Austin, and Cleveland had the three highest proportions of VMT with *any alcohol* consumption at 14.0%, 13.0%, and 12.4%, respectively. Miami / Fort Lauderdale, Tampa / St. Petersburg, and Milwaukee had the three lowest weekend night *any alcohol* VMT proportions with 3.7%, 4.4%, and 4.7%, respectively.

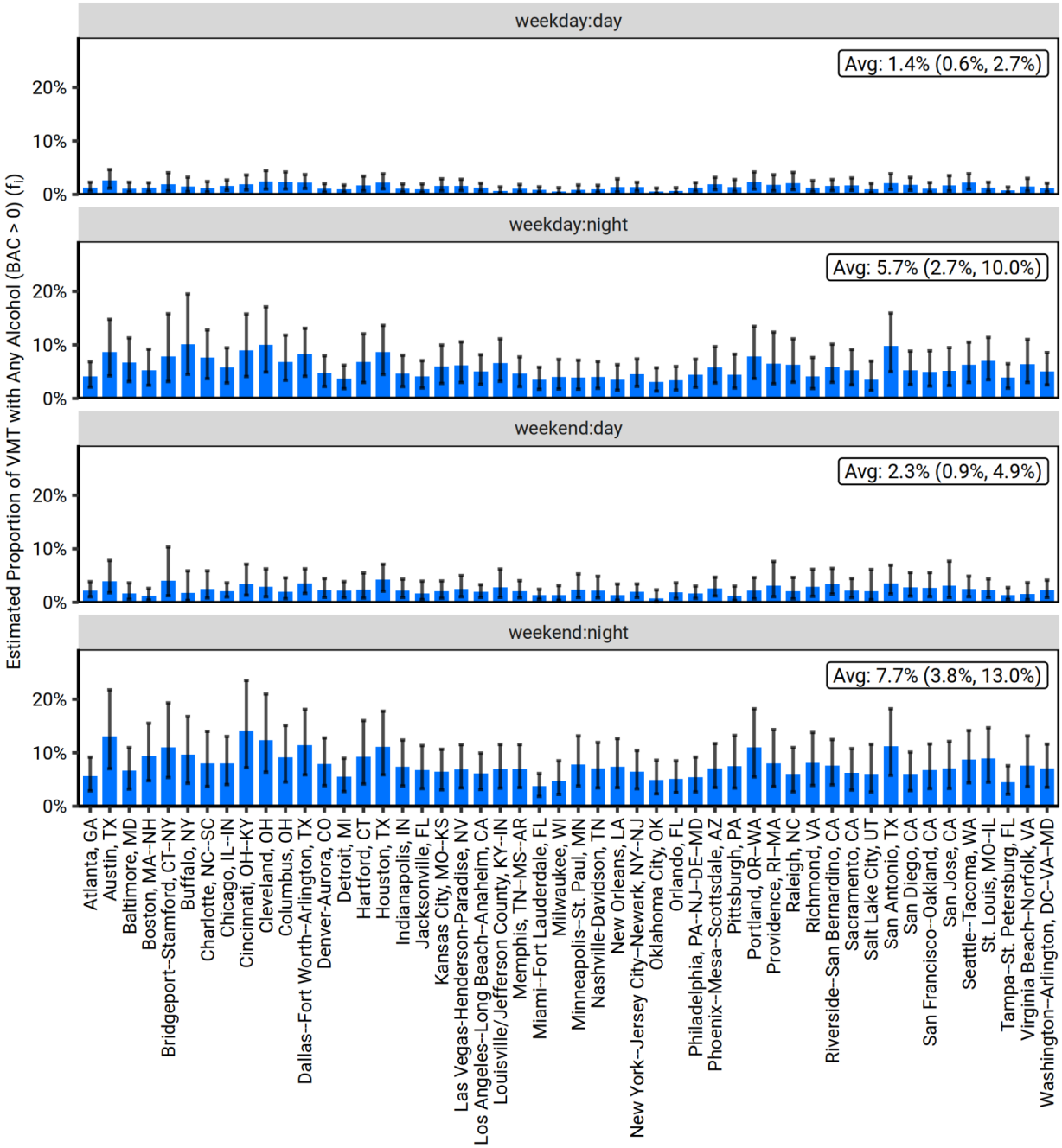


Figure 2. Proportion of VMT with Any Alcohol Consumption (f_i) across the 50 most populated US urban areas, stratified by temporal groupings. The displayed CIs are the

mean values for the computed 95th percent confidence interval lower and upper bounds for each urban area. These proportions were estimated using fatal crash data from 2021 through 2023.

3.4 Isolating Sober Drivers from the Status Quo Fatal Crash Benchmarks

Fundamental to this study is the question of how much alcohol impairment is affecting *status quo* crash involvement rates. Figure 3 shows the crash involvement rates for the *sober* subgroup compared against *status quo* driving.

A wide range of geographic-specific differences were observed. The largest difference was observed in Houston, where 6.8% of drivers were estimated to have consumed some alcohol prior to driving, and the *sober* crash involvement rate was found to be 32% lower than the *status quo* crash involvement rate. At its lowest difference, Minneapolis / St. Paul (3% of all VMT was estimated to be in the *any alcohol* consumption cohort) *sober* drivers had a crash involvement rate 13.5% lower than the *status quo.*

Another interesting comparison point enabled by the current study was the analysis of unimpaired crash rates, by themselves. One logical theory follows that *status quo* crash involvement rates were extremely elevated at night (Scanlon et al., 2026b) due to the prevalence of alcohol impairment. By isolating and examining the *sober* subgroup independently, the effects of driving at night without alcohol impairment can be examined. Figure 4 shows *sober* fatal crash involvement rates by time of day and day of week. *Sober* fatal crash involvement rates were significantly higher at nighttime compared to daytime. On weekdays and weekends, nighttime driving crash involvement rates were 3.1-times and 3.8-times higher, respectively. Daytime unimpaired fatal crash involvement rates had similar magnitudes comparing between weekends and weekdays. At nighttime, fatal crash involvement rates were significantly higher (1.45-times) on weekends (2.89 IP100MM) compared to weekdays (1.99 IPM100MM).

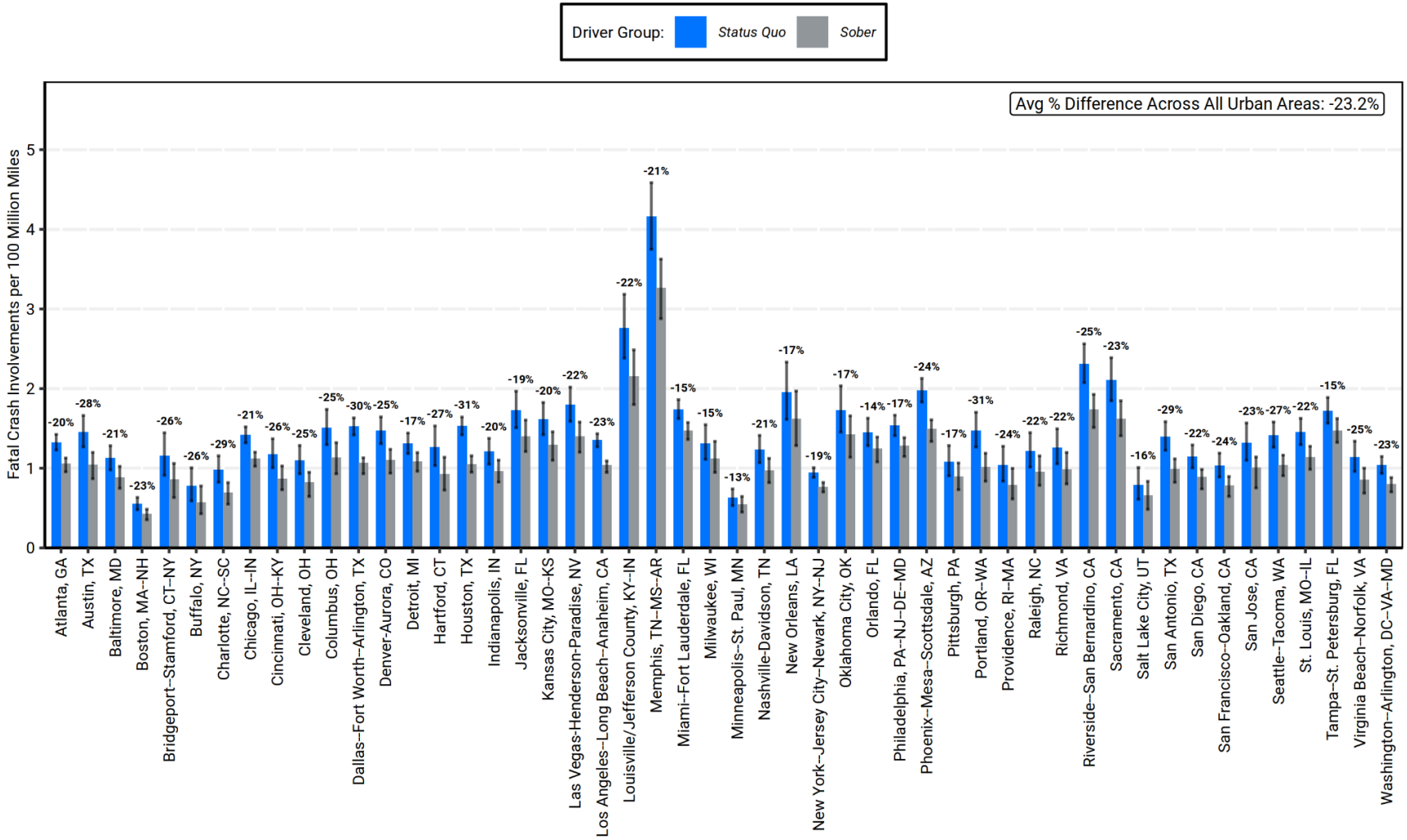


Figure 3. Fatal crash involvement rates for the *sober* subgroup compared against *status quo* driving across the 50 most populated US urban areas using 2023 data. A simple average of the percent differences across all 50 urban areas is shown in the text box.

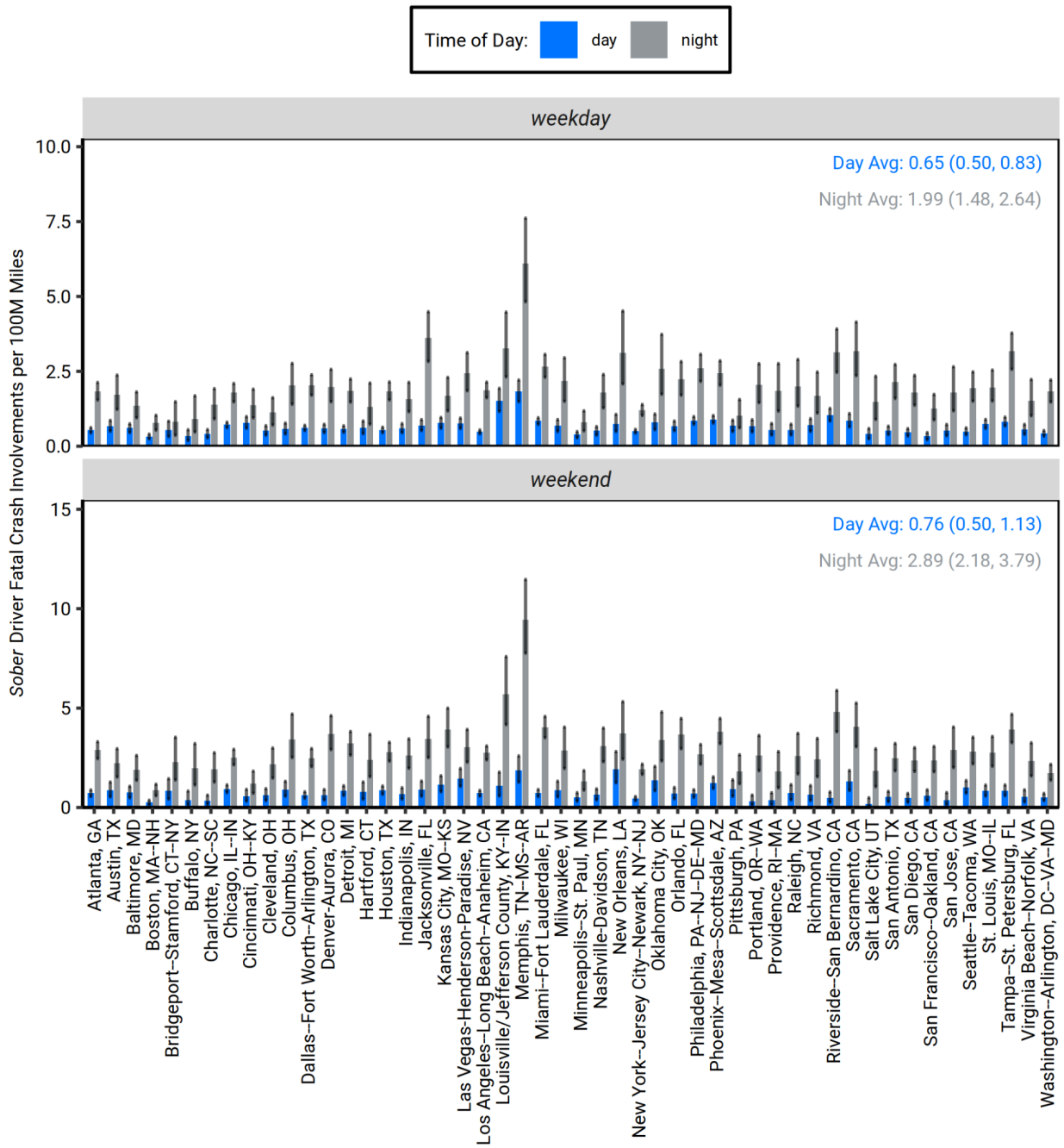


Figure 4. *Sober* driver cohort fatal crash involvement rates for 2023 by time of day and day of week for the 50 most populated US urban areas. The displayed CIs are the mean values for the computed 95th percent confidence interval lower and upper bounds for each urban area.

3.5 Police-reported Crash Involvement Rates

The analysis of *police-reported* crash involvement rates across the various alcohol cohorts yielded the estimates summarized in Tables 5 and 6. Figure 5 provides a geographic distribution of these outcomes, illustrating the variance between the *status quo* benchmarks established by Scanlon et al. (2026) and the derived *sober* driver population for each respective US county.

Attributable to the attenuated relative risk magnitudes associated with *police-reported* events (see section 3.1), the crash involvement rates for unimpaired (*sober* and *sub-threshold*) and impaired (*any alcohol* and *legally impaired*) drivers exhibited a closer convergence with the *status quo* than the corresponding *fatal* involvement metrics. Naturally, the difference between these cohorts was defined by the specific relative risk values utilized for the computation. The *sober* subgroup yielded a crash involvement rate with less than a 1% difference from the *sub-threshold* cohort; this finding is once again ascribed to the sparse volume of VMT generated by drivers maintaining a BAC below the 0.08 threshold. The resulting *sober* involvement rate was only marginally lower than the *status quo*, reflecting a reduction range of 6% to 11% across the targeted jurisdictions.

Table 5. Mean and 95% confidence interval 2023 estimates of the proportion of *police-reported* crash involvements and VMT with impaired driving for the 50 targeted US urban areas. The mean and confidence interval bounds were computed by taking a simple average across all urban area values.

Measure	***Any Alcohol*** **BAC ≥ 0.01**	***Legally Impaired*** **BAC ≥ 0.08**
Proportion of Police-reported Crash Involvements (%)	12.0% (2.5%, 26.1%)	7.9% (1.4%, 19.0%)
Proportion of VMT (%)	4.7% (2.4%, 7.7%)	1.1% (0.5%, 2.0%)

Table 6. Mean and 95% confidence interval 2023 police-reported crash involvement rates for each driver cohort for the 50 targeted US urban areas. The mean and confidence interval bounds were computed by taking a simple average across all urban area values.

Driver Cohort	Police-reported Crash Involvement Rate (IPMM)
Status Quo	4.88 (4.84, 4.93)
Sober **(BAC = 0)**	4.51 (4.01, 4.82)
Sub-Threshold (BAC < 0.08)	4.56 (4.07, 4.84)
Any alcohol (BAC ≥ 0.01)	12.66 (9.06, 18.6)

Legally Impaired (BAC ≥ 0.08)	36.9 (23.2, 58.7)

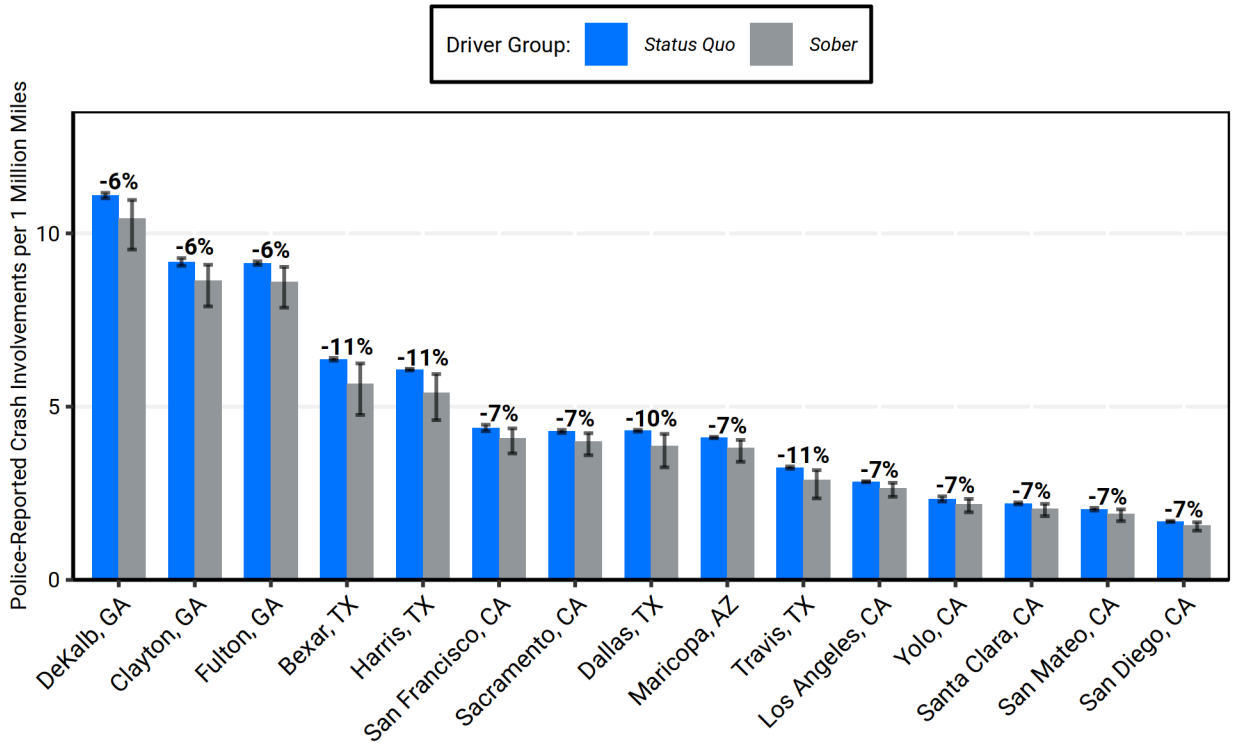


Figure 5. *Police-reported* crash involvement rates comparing *sober* drivers against *status quo* drivers (established in Scanlon et al., 2026c) using 2023 data.

4. DISCUSSION

4.1 Implications for Quantifying Localized Alcohol-impaired Drivers

The exposure reconstruction methodology demonstrated in this study offers a powerful, scalable framework for quantifying the localized prevalence and safety impact of alcohol impairment on public roadways. Historically, traffic safety researchers have relied on roadside surveys to estimate the prevalence of active drinking drivers on public roads (e.g., Ramirez et al., 2016). By mathematically pairing annually compiled census data from the Fatality Analysis Reporting System (FARS) with established, continuous epidemiological relative risk models, this approach allows for high-resolution, geographically specific back-calculations of active driving exposure that can be estimated year-over-year.

The results aligned well with more recent (2013–2014) NRS data (Ramirez et al., 2016). The Voas et al. (2012) model was built using the older 2007 NRS (Lacey et al., 2009). The exposure

fractions back-calculated in the current study across the U.S. urban areas were similar to the empirical observations in the 2013-2014 NRS sample, providing some validation to the current study's methodology. In the NRS observed data, legally impaired drivers (BAC ≥ 0.08 g/dL) accounted for approximately 0.4% of weekday daytime drivers and 1.5% of weekend nighttime drivers. In comparison, our exposure reconstruction model back-calculated that legally impaired drivers accounted for 0.3% of weekday daytime VMT and 1.7% of weekend nighttime VMT across the 50 most populous U.S. urban areas in 2023. Minor variations are expected given that our analysis isolates the dense urban boundaries of the top 50 metropolitan areas rather than a nationwide average. Also, the exposure reconstruction was performed for a more recent crash year (2023), so there is expected to be some natural deviations.

As another comparison point, Miller et al. (2001) in a foundational health economics study estimated that approximately 1 in 140 miles driven in the United States (0.71% of total national VMT) were alcohol *legally-impaired* drivers. Miller's approach extrapolated NRS data using trip data for the purposes of computing an economic cost per mile. Like the current study, this method also relied on NRS data (different years), but it is another validation point that these differing transformations are creating comparable proportions.

A key takeaway from this study is the utility of the proportion of fatal crash involvements with legally-impaired drivers, which is readily available in annual FARS data or localized crash records (e.g., NCSA 2025a). By leveraging this proportion alongside the composite relative risk values in Table 3, the prevalence of alcohol-impaired VMT can be reconstructed for specific temporal and spatial domains via Equation 8. This provides a scalable, trackable signal for monitoring localized intoxication exposure, provided researchers account for statistical uncertainty and the associated methodological limitations.

4.2 Implications for ADS Safety Performance Benchmarking

To date, safety performance assessments of ADS have predominately compared crash rates against a *status quo* human baseline. While the status quo remains a vital benchmark because it represents the actual, aggregate risk profile currently present on public roadways, it mixes law-abiding driving with high-risk, illegal behaviors like speeding, distraction, and biological impairment. To evaluate an ADS against a normative expectation of how a driver ought to perform, expectations-based benchmarks can be useful.

By mathematically isolating and subtracting the safety burden of alcohol-impaired driving, this study establishes a localized, expectations-based benchmark that reflects completely sober driving. These localized results demonstrated that using a single national average (of non-drinking drivers) to evaluate highly geographically constrained ADS deployments would introduce significant analytical bias. For example, the average percent reduction in the fatal crash rate reduction between drivers with any alcohol and sober across the 50 urban areas was 23%. However, in Houston, Texas, where the prevalence of drivers with any alcohol was highest, the

sober human fatal crash involvement rate benchmark was 32% lower than the status quo baseline. Conversely, in Minneapolis, Minnesota, the *sober* benchmark was only 13.5% lower. Applying a flat 23% national average adjustment to an ADS deployment in Houston would substantially underestimate the local safety burden of human impairment, leading to an artificially deflated safety target with the opposite effect being true in Minneapolis.

Furthermore, isolating *sober* driving risk reveals critical insights into traffic safety dynamics that are independent of intoxication. While a common traffic safety hypothesis suggests that elevated nighttime crash rates are primarily driven by alcohol impairment, the current results showed that even after mathematically removing all alcohol-involved driving, nighttime fatal crash risk remains 3.1 to 3.9 times higher than daytime risk for weekdays and weekends, respectively. The residual risk is driven by other critical safety factors such as reduced visibility (especially for pedestrians) (Wood, 2020), inadequate headlighting (Brumbelow, 2022), the presence of other legally-impaired drivers, fatigue (NCSA, 2017; Meyerson et al., 2024), drug use (Kelley-Baker et al., 2017), speeding (NCSA, 2023), and responding to other alcohol-impaired road users. By isolating these non-alcohol-related risk contributors, our framework provides a robust baseline that can be incrementally expanded to establish new, progressive safety targets for ADS as more refined relative risk models become available and crash data granularity permits.

To the knowledge of the authors, Goodall (2021) is the only other study to have removed undesirable driving from *status quo* crash rates. The current study's benchmarks can be constructively compared to the "model driver" benchmark proposed by Goodall (2021), which estimated a 33% overall crash rate reduction from the *status quo* by combining several low-risk behaviors (sober, rested, attentive, and cautious) into a single baseline. However, if we isolate the effect of alcohol alone, a notable difference emerges: Goodall (2021) estimated that removing alcohol impairment ("Sober" versus "All driving") yielded a minor 0.5% crash rate reduction (2,010 vs. 2,020 crashes per 100 million miles) at the lower-severity levels (combination of police reported and non-reported crashes) predominately captured by naturalistic driving data. In contrast, the current study demonstrated that removing any alcohol-involved driving yielded a substantial 6% to 11% rate reduction at the *police-reported* severity level. This divergence highlights a fundamental difference in both data design and crash severity. First, Goodall (2021) relied on naturalistic driving data from the SHRP-2 study (Dingus et al., 2016), which identified alcohol impairment through visual and behavioral observation by video analysts. While highly valuable for capturing active driver behavior, visual observation naturally underrepresents the prevalence of low-to-moderate BAC levels that do not produce obvious, visible physical cues, reflected in SHRP-2's low baseline alcohol prevalence of 0.1% VMT. In contrast, the current study relied on data from FARS (field measurements of BAC combined with imputation) and the NRS (active blood and breath tests), allowing us to capture the entire spectrum of alcohol impairment. Second, the safety impact of alcohol is more pronounced in higher-severity collisions rather than the lower severity crashes (often non-police-reported) that dominate naturalistic databases. Lastly, while Goodall (2021) provided a valuable national-level aggregate

estimate, the current study complements Goodall's cumulative "model driver" baseline by providing the localized, high-severity resolution necessary for evaluating contemporary ADS deployments.

The current study's benchmark also complements work done by Di Lillo et al. (2025) that was introduced in Section 1.2. Di Lillo et al. (2025) controlled for vehicle model year in an effort to reflect maturing safety technology in the latest model-year vehicle fleets equipped with advanced active safety systems. Data permitting, combining vehicle and human factors for generating new benchmarks would reflect an even higher bar for safety. The work by Goodall, notably, combined multiple human factors in developing their "model driver," which was possible because the available data contained that information. Researchers should consider novel approaches for setting expectations-based benchmarks, including how to integrate multiple approaches for a more comprehensive, multi-dimensional target.

4.3 Epidemiological Interpretation of Cohort Dynamics

An essential epidemiological finding of this study is the magnitude similarity of the *sober* and *sub-threshold* crash rates. Across both *fatal* and *police-reported* severities, removing the *sub-threshold* drinking population (which becomes the *sober* population) yields a negligible rate reduction of less than 3%. This cohort behavior is explained by the mathematical interaction between an exponential risk function and a highly skewed population exposure distribution. Because relative crash risk rises exponentially with alcohol concentration, drivers in the *sub-threshold* range experience a modest risk increase compared to *sober* driving cohort. In contrast, the composite fatal relative risk for the *legally impaired* drivers had a relative involvement risk 30.5 times higher than the sub-threshold cohort. On the exposure side, *sober* drivers represent nearly 96% of all active roadway VMT, leaving a small fraction of travel driven under *sub-threshold* BAC levels. Consequently, because *sub-threshold* drinking drivers contribute to very few crashes and represent a minimal exposure fraction, their elimination has virtually no impact on the unimpaired safety baseline.

Instead, the massive traffic safety benefits realized by moving beyond the *status quo* are almost entirely driven by the elimination of the high-BAC exponential tail, where legally impaired drivers account for over 22% of fatal crash involvements despite operating less than 1% of total VMT. This dynamic explains why the proportion of fatal crash involvements involving legally impaired drivers (P_i in this study) serves as a mathematical approximation (slight underestimate) for the percent rate reduction of the sub-legal cohort from the *status quo* baseline. Because legally impaired VMT is so small, removing this cohort leaves the baseline exposure denominator virtually unchanged, allowing the rate reduction to scale almost 1-to-1 with the eliminated crashes.

4.4 Limitations

There are a number of limitations to consider in this study. First, this study used data from a range of years and different populations; the 2013-2014 NRS study (Ramirez et al., 2016), a fatal crash risk model built from 2006–2007 data (Voas et al., 2012), and a police-reported crash risk model built from 2010-2012 data (Lacey et al., 2016). The Lacey et al. (2016) model was also built from a limited geographic area (Virginia Beach, VA). With that being said, each of these models and data sources showed consistency in their relative risk magnitudes when compared to prior studies of the same design. These models and data are all the most recent sources of information that contained the requisite components.

Second, there are many sources of statistical uncertainty in generating crash rates using exposure reconstruction. This study employed a robust bootstrapping technique to handle these sources. However, one area where uncertainty could not be incorporated was with the total geographic VMT estimates, where the underlying data sources and estimation techniques were not readily available. While adding this VMT uncertainty would widen the absolute confidence intervals of the crash rates, this limitation is mitigated by the structure of the exposure reconstruction. Because the unobserved exposure fraction is solved exclusively as a function of the crash proportions and the relative risk parameters, it is completely independent of the absolute VMT magnitude. Furthermore, because absolute VMT acts as a common scaling denominator in both the Status Quo and Unimpaired rate formulations, it cancels out entirely when computing relative rate reductions. Consequently, any unmodeled variance in the total VMT has no mathematical bearing on the reconstructed impaired exposure estimates or the reported percentage differences, ensuring that the core comparative conclusions of this study remain highly stable.

Third, this study specifically focuses on the effects of alcohol on crash risk. There are, of course, other undesirable driving behaviors, such as drug use and driving distraction, that could also affect crash involvement rates. This study focused on alcohol impairment because epidemiological studies that evaluate both alcohol and drug relative risks within the same cohort and design framework consistently demonstrate that alcohol is the predominant driver of crash risk and represents a substantially higher relative risk than other substances. For example, in the Lacey et al. (2016) NHTSA study, the age- and gender-adjusted crash risk at a BAC of 0.08 g/dL was 3.93 times higher than that of unimpaired drivers. Yet, within that same cohort, none of the tested drug classes (including cannabis/THC) exhibited a statistically significant independent crash risk. However, impairment effects are highly substance-dependent. In the European Union's DRUID project (Schulze et al., 2012), researchers evaluated substance-related risks for serious and fatal injuries, demonstrating that while individual drugs carry varying risks, multiple drug combinations result in a highly elevated relative risk of 5 to 30 times. Crucially, even this severe poly-drug risk is eclipsed by high-level alcohol impairment within the same project framework, where a BAC of 0.12 results in an extremely elevated relative risk of 20 to 200 times.

Fourth, the proportion of legally-impaired fatal crash involvements was estimated from a multi-year window (2021–2023) to reduce statistical variance in low-frequency spatial-temporal strata, while single-year data (2023) was used for the actual rate computations. A detailed sensitivity analysis of this temporal aggregation is provided in Appendix C. Although this multi-year approach stabilizes the baseline, it may smooth out localized year-to-year trends in alcohol-involved driving; a more elegant solution for future implementations may be to conditionally bring in additional years of data only when specific sample size thresholds require it.

Lastly, as with the prior literature on fatal crash involvement risk, this study relied on imputed BAC measurements in FARS. The results suggest that 1 out of every 5 drivers who had consumed alcohol was over the legal limit. It is likely that many drivers with low BAC were not tested, or any effects of alcohol had worn off during the elapsed time following the crash. So, in both the reconstructed VMT and observed crash involvements, it is possible that some drivers in the *sober* benchmark could have had a low BAC.

5. Conclusions

This study established crash rate benchmarks for both alcohol-impaired and non-alcohol-impaired drivers. A non-alcohol-impaired benchmark serves as a potential expectations-based target for evaluating ADS safety performance, moving safety performance assessments beyond the traditional *status quo* baseline. By mathematically isolating and subtracting the safety burden of alcohol-impaired driving through exposure reconstruction, this research provides a transparent, localized framework for evaluating how ADS performs relative to a non-alcohol-impaired human driving population.

The empirical results of this study underscore the disproportionate and severe safety burden that alcohol-impaired drivers place upon the U.S. roadway system, a toll that is shared by both impaired and unimpaired roadway actors. Across the 50 most populous U.S. urban areas, the sober subgroup had an average fatal crash involvement rate of 1.10 incidents per 100 million miles, representing a 23% reduction from the status quo baseline of 1.42 incidents per 100 million miles (Scanlon et al., 2026b). Crucially, because traffic collisions frequently involve multiple vehicles, the safety hazards of alcohol impairment are not confined to the impaired driver; alcohol-involved crashes directly impact sober drivers, passengers, and vulnerable road users who are involved in crashes with alcohol-impaired drivers. Indeed, national statistics demonstrate that over one-fourth (27%) of those killed in alcohol-impaired crashes are occupants of other vehicles or non-occupants such as pedestrians and cyclists (NCSA, 2025a). Furthermore, isolating the *legally impaired* cohort reveals that drivers operating above the legal limit account for only 0.9% of active VMT on public roads, yet they are involved in over 22% of fatal crash involvements (NCSA, 2025a; Scanlon et al., 2026b). These stark disparities highlight how a tiny fraction of high-risk driving exposure inflates the status quo baseline for all roadway users. While comparing against the status quo remains essential for measuring the net safety impact of

replacing active human drivers, evaluating ADS fleets against a non-alcohol-impaired aggregate provides a potentially useful expectations-based target to actively raise the roadway safety bar.

The localized findings also demonstrated that applying a single national-level adjustment factor to evaluate highly geographically constrained ADS deployments introduces significant analytical bias. The difference in fatal crash rate between the *status quo* and *sober* crash involvement rates varied dramatically by urban market, spanning from a 13.5% reduction in Minneapolis to a 32.0% reduction in Houston. Utilizing a flat national adjustment would systematically underestimate the local human safety burden in high-exposure municipalities like Houston, leading to artificially deflated safety targets for local ADS fleets.

Finally, even after separating *sober* driving from the baseline, nighttime fatal crash risk remains 3.1 to 3.9 times higher than daytime risk. This persistent disparity demonstrates that removing human impairment is not a panacea for traffic fatalities. These findings demonstrate that Safe System interventions must continue to treat remaining factors, such as roadway visibility, advanced vehicle lighting, driver fatigue, and speed management as parallel priorities. Furthermore, members of the *sober* cohort must still interact with impaired drivers on shared roadways. This exposure, which is more pronounced during nighttime periods, may likely depress the observed performance of *sober* drivers compared to roadways without any drinking drivers.

This study provides an important framework for ADS safety benchmarks that are immediately applicable. Research should continue in this area, seeking to extend the methodology to include other risk-contributing areas. These benchmarks should not only help evolve our expectations for ADS but also our understanding of the degree to which the status quo is plagued by unacceptable driving behavior.

6. PRACTICAL APPLICATIONS

The findings and methodology developed in this study have immediate, practical applications for municipal traffic safety planners, Automated Driving System (ADS) developers, and regulatory agencies working to achieve Vision Zero (USDOT, 2022):

1. Scalable Exposure Monitoring for Municipal Planners: This study establishes "exposure reconstruction" as a practical tool for municipal traffic engineers and regional safety planners. Traditionally, tracking the prevalence of active drinking drivers on public roadways required funded checkpoint operations or decennial roadside surveys (Ramirez et al., 2016). Our methodology demonstrated that because the composite relative fatal risk of impaired driving is so high (30.5), the proportion of local fatal crash involvements involving legally impaired drivers (readily available in annual FARS or state crash registries) serves as a mathematical proxy for the localized percentage rate reduction. By leveraging this proportion alongside the composite relative risk values provided in Table 3, municipal planners can utilize Equation 8 to directly back-calculate and monitor

localized alcohol-impaired VMT exposure fractions across specific spatial and temporal domains, providing a continuous, low-cost safety signal to track the efficacy of local countermeasure programs.

2. Unbiased Expectation Benchmarking for ADS Developers: For ADS developers and safety evaluators, this study provided a mathematically defensible methodology to construct localized, expectations-based human benchmarks that conform to RAVE Checklist best practices (Scanlon et al., 2025). By segmenting benchmarks into distinct geographic, roadway, and temporal strata, and subtracting the safety burden of alcohol-impaired driving, evaluators can compare ADS fleets against a non-alcohol-impaired human benchmark (Scanlon et al., 2026a).
3. Target-Setting for Policymakers: By demonstrating that nighttime fatal crash risk remains 3.1 to 3.9 times higher than daytime risk even for completely sober drivers, this study demonstrated how alcohol-effects can be removed to study other factors driving higher risk at night. For federal and state policymakers, this finding practically demonstrates that the Safe System "Safer People" and "Safer Roads" pillars must be pursued as independent, parallel priorities (USDOT, 2022). It highlights that even for sober drivers, substantial nighttime safety gains can only be achieved by proactively targeting independent variables such as pedestrian visibility (Wood, 2020), advanced vehicle headlighting performance (Brumbelow, 2022), and structural speed management (NCSA, 2023).

7. DECLARATION OF GENERATIVE AI AND AI-ASSISTED TECHNOLOGIES IN THE MANUSCRIPT PREPARATION PROCESS

During the preparation of this work the author(s) used Gemini and Notebook LM in order to assist with coding and support the literature review. After using this tool/service, the author(s) reviewed and edited the content as needed and take(s) full responsibility for the content of the published article.

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APPENDIX

Appendix A: Comparative Assessment of Relative Risk Models

To explain the selection of the primary relative risk models used in this study's exposure reconstruction methodology, an evaluation of alternative U.S. relative risk models was conducted. Relative risk studies taking place outside of the US were not considered to minimize representativeness issues (Schulze et al., 2012; Lardelli-Claret et al., 2006; Lenguerrand et al., 2008; McLean et al., 1980; Keall et al., 2004). Table A1 summarizes the primary candidate studies evaluated. The candidate models not selected are described in subsequent paragraphs.

Table A1. Studies Considered for Determining Relative Crash Risk between Impaired and Unimpaired Driving

	Source	Study Period	Geographic Area	Severity Level	Control Methodology	Primary Limitations	Selection Status in Current Study
Includes Non-Fatal Crashes	**Borkenstein et al. (1964)**	1962-1963	Grand Rapids, MI	police-reported	Local roadside match	Dated; single mid-sized city	Alternative Candidate (Unselected)
	Blomberg et al. (2005)	1997-1999	Long Beach, CA, and Fort Lauderdale, FL	police-reported	Local roadside match	Completed over 25 years ago; excludes freeways	Alternative Candidate (Unselected)
	Lacey et al. (2016)	2010-2012	Virginia Beach, VA	police-reported	Local roadside match	Excludes freeways	**Primary Model (Selected)**
	Dingus et al. (2016)	2010-2013	Six US Cities	NDS-reported	Continuous camera monitoring	Relies on visual observation of impairment; no objective toxicological BAC.	Alternative Candidate (Unselected)
Fatal Crashes Only	**Zador (1991)**	1986	Entire US	Fatal	NRS Checkpoints	Dated (later updated)	Alternative Candidate (Unselected)
	Zador et al. (2000)	1995-1996	Entire US	Fatal	NRS Checkpoints	Dated (later updated)	Alternative Candidate (Unselected)
	Voas et al. (2012)	2006-2007	Entire US	Fatal	NRS Checkpoints	Relies on database linkage	**Primary Model (Selected)**
	Romano et al. (2018)	2013-2014	Entire US	Fatal	NRS Checkpoints	Models individual driver fatality risk rather than involvement risk	Alternative Candidate (Unselected)
	Levitt & Porter (1999)	1983-1993	Entire US	Fatal	Two-vehicle crash mixing model	Indirect relative risk measure	Alternative Candidate (Unselected)

The foundational case-control methodology for assessing relative risk was established by Borkenstein et al. (1964, 1974) in the Grand Rapids Study. By comparing blood alcohol concentrations (BAC) of crash-involved drivers against random roadside controls matched by time and location, Borkenstein et al. documented the classic exponential risk curve at higher BACs, as well as the "Grand Rapids Dip" at very low BACs ($0.01\% \leq BAC < 0.04\%$). Subsequent studies have re-analyzed and extended those analyses (Allsop et al., 1966; Hurst et al., 1994).

However, because the data was collected in 1962–1963, the relative risk curves reflect obsolete vehicle safety technology, outdated roadway infrastructure, and different speed limits, making them less representative of modern risk profiles.

Blomberg et al. (2005, 2009) replicated the Grand Rapids methodology in Long Beach, California, and Fort Lauderdale, Florida, from 1997 to 1999. Funded by NHTSA, the study confirmed elevated relative risk starting at 0.04% BAC, with strongly accelerated risk exceeding 0.10% BAC. While methodologically sound, the Blomberg study was completed over 25 years ago and explicitly excluded freeway crashes. The current study uses Lacey et al. (2016), which is a similarly designed study, but is both more recent (2010 - 2012) and takes place in Virginia Beach, VA.

Dingus et al. (2016) utilized naturalistic driving data from the SHRP-2 safety study to calculate a relative risk score for alcohol impairment. However, because SHRP-2 relied on visual observation of impairment via in-cab cameras rather than objective toxicological measurements, it suffered from a low baseline prevalence (0.1% of baseline VMT) and likely captured only extreme cases of intoxication, resulting in an artificially inflated relative risk of 35.9-times.

A set of studies computing fatal crash involvement relative risk were considered for use in this study. In the US, a series of four distinct case-controlled studies (Zador, 1991; Zador et al., 2000; Voas et al., 2012; Romano et al., 2018) have been conducted quantifying the relative risk of alcohol impairment on crash risk by combining the Fatality Analysis Reporting System (FARS) (NCSA, 2025c) and National Roadside Survey (NRS) data (Lacey et al., 2009; Ramirez et al., 2016). While Zador (1991) and Zador et al. (2000) established the robust logistic regression framework for fatal risk, their models rely on exposure data that is over 30 years old. Romano et al. (2018) updated these risk curves using the 2013–2014 NRS data. However, Romano et al. modeled individual driver fatality risk rather than driver crash involvement.

Levitt and Porter (1999) bypassed exposure data entirely by using a mathematical "population mixing model" to analyze two-vehicle crashes in FARS. Their novel approach demonstrated that drivers over the legal limit (BAC $\geq$ 0.10 in most states at the time of this study) were over 15 times more likely to be in a fatal crash. Their model solved for a single, flat, non-continuous aggregate risk multiplier (above and below a BAC of 0.10. The current study required a continuous relative risk estimation in order to estimate VMT exposure at various BAC. Accordingly, this model was not selected.

Appendix B: Supplemental Geographic Distributions of Alcohol-Impaired Driver Fatal Crash Involvements

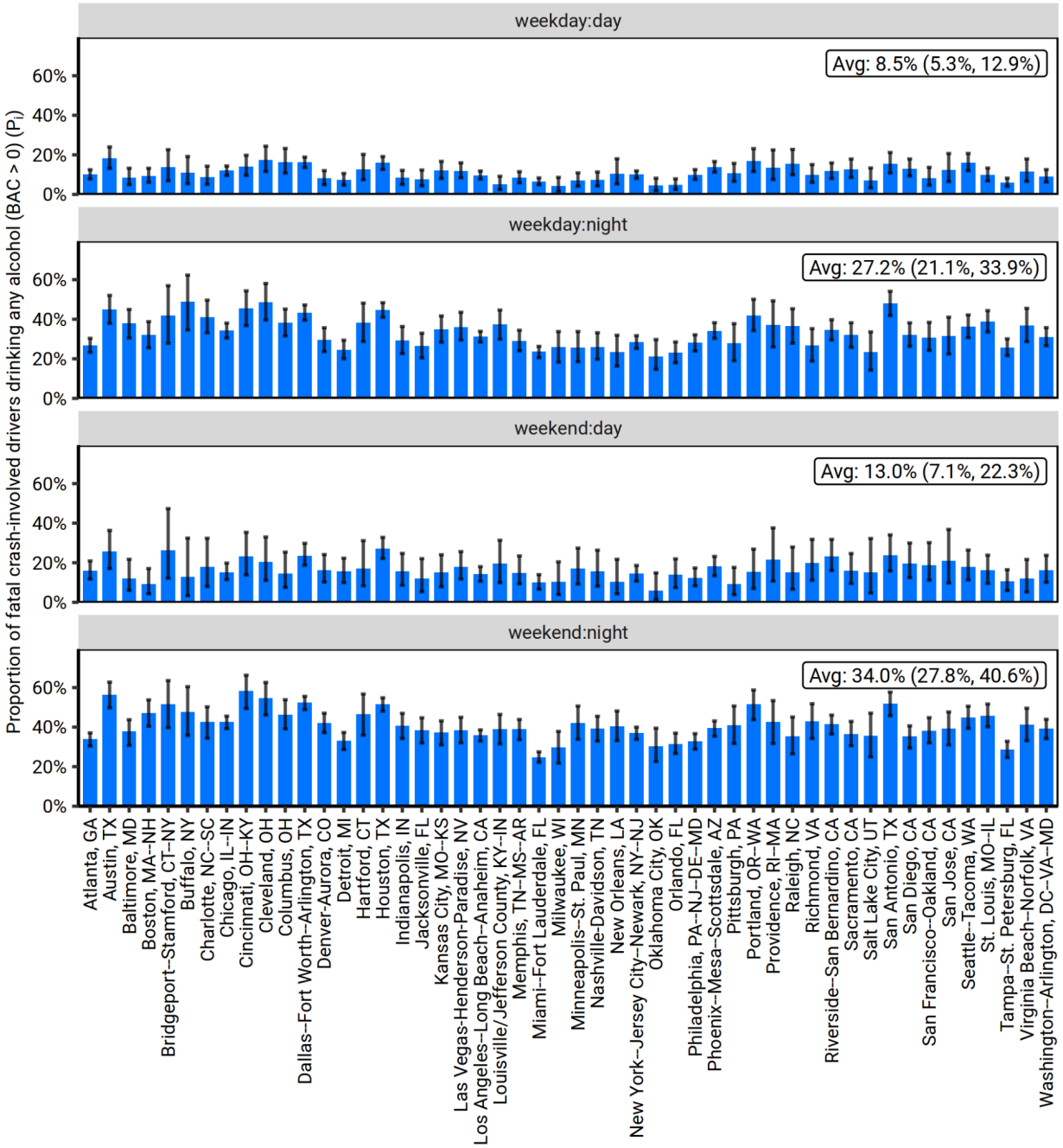


Figure A1. Proportion of fatal crash-involved drivers with any alcohol consumption (BAC ≥ 0) (P_i) across the 50 most populated US urban areas, stratified by temporal groupings. The displayed CIs are the computed 95th percent confidence interval lower and upper bounds. These proportions were estimated using fatal crash data from 2021 through 2023.

Appendix C: Sensitivity analysis for including multiple years in impaired VMT

This subsection provides a quantitative justification for the implementation of a multi-year temporal window when reconstructing alcohol-impaired exposure. As established in Equation 8, the back-calculation of f_i is mathematically dependent on both the estimated relative risk and the observed proportion of alcohol-involved fatal crashes ($P_{i,\ fatal}$), both of which are subject to statistical uncertainty. Due to the high resolution of the spatial-temporal strata utilized in this study, several cohorts possessed a limited volume of fatal crash observations. Figure A2 illustrates the reconstructed *any alcohol* VMT proportions for the 10 urban areas with the lowest fatal crash counts during the weekend daytime window. Utilizing only 2023 data, jurisdictions such as Salt Lake City (2), Buffalo (5), Providence (7), Portland (8), and San Jose (9) recorded fewer than 10 fatal involvements. Aggregating three years of fatal crash data (2021–2023) significantly bolstered these counts to 22, 19, 34, 47, and 30, respectively. The resulting impact on statistical precision was most acute in these low-frequency markets; for instance, in Salt Lake City, the 95% CI for reconstructed exposure narrowed from a range of 0.0%–17.8% (single-year) to 0.5%–6.1% (three-year), representing a nearly three-fold reduction in variance.

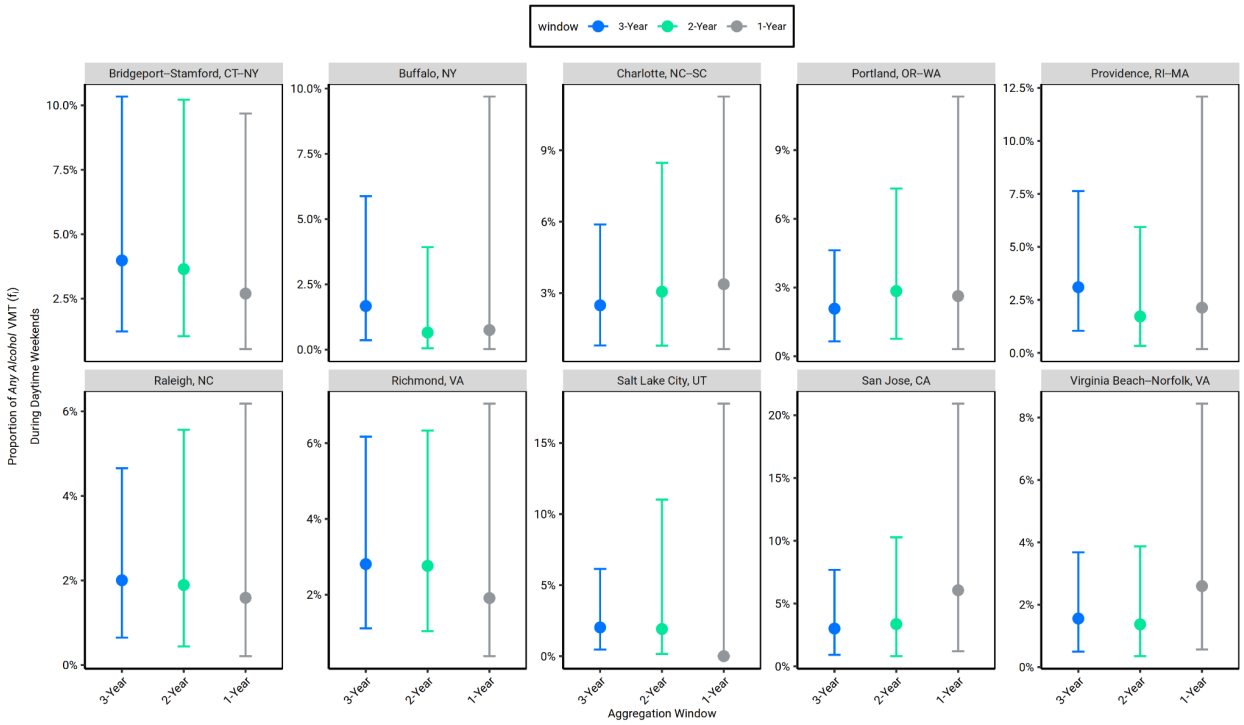


Figure A2. Sensitivity analysis reconstructed *any alcohol* VMT proportions and associated 95% confidence intervals across different temporal aggregation windows for the 10 urban areas with the lowest fatal crash counts during weekend daytime periods, demonstrating the reduction in variance and improvement in statistical precision achieved via multi-year aggregation.