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ABSTRACT

Death on the Job: The Great Recession and Work-Related Traffic Fatalities

In light of recent discussions about shifting employees from traditional workplaces to virtual employment, we are motivated by the question of whether this phenomenon will end up saving lives even in the absence of an infectious disease outbreak. Motor vehicle incidents are the leading cause of work-related fatalities in the US, killing more than 1,200 workers each year, which make up about a quarter of all work-related deaths. Not only are motor vehicle crashes the top killer at work, but economic expansions can further increase occupational and traffic deaths as they both tend to be procyclical. In this paper, we examine the effects of business cycles on traffic fatalities in the US with a special focus on work-related deaths. Specifically, we implement a longitudinal design across all 50 states by compiling quarterly data for 2004-2012 and consider macroeconomic fluctuations around the Great Recession. Our findings show that traffic deaths during prosperous times are not solely due to an increase in risky behaviors such as drunk driving, but directly related to work. Given the highly preventable nature of traffic crashes, policy makers, public health advocates, and employers can develop effective strategies, including remote work arrangements, to improve both occupational and traffic safety.

JEL Classification: E32, I12, I18

Keywords: traffic fatalities, occupational health, business cycles, unemployment, Great Recession

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Introduction

The recent COVID-19 pandemic has put a bright spotlight on the relationship between employment and health, especially the tradeoff between the two. As the infectious disease outbreak led to shelter-in-place restrictions, the economic burden of social distancing measures became almost as concerning as the disease itself (e.g., Cutler and Summers, 2020). This is because reductions in economic activity itself can adversely impact a variety of health outcomes, including both physical and mental health status (e.g., French et al., 2020; Ruhm, 2020). Mortality as a health outcome typically attracts significant attention not only because it is the most extreme consequence, but it is also easier to measure compared to various other outcomes. Having to work under the threat of COVID-19 has led to the death of many people, especially health care workers and first responders (Babb et al., 2020). However, losing a job can also pose a death threat in the form of suicides, substance overdoses, cardiovascular events, and other health shocks (e.g., Classen and Dunn, 2012).

While the pandemic illuminated compromises between population health and the economy, we face similar balancing acts every day—even in the absence of any infectious disease outbreaks. In this paper, we highlight one such compromise—work-related traffic fatalities. Motor vehicle crashes are the leading cause of occupational deaths in the US (Centers for Disease Control and Prevention [CDC], 2011; 2016a; US Bureau of Labor Statistics [BLS], 2019). In 2013, the latest year for which cost reports are available, more than 1,600 fatalities and almost 300,000 injuries resulted from work-related traffic crashes (Network of Employers for Traffic Safety [NETS], 2016). The same year, economic costs of on-the-job traffic crashes for employers—including medical care expenses, property damage, lost work time, workplace disruption, liability costs, and legal expenses—were estimated to be more than $25 billion (in 2013 dollars), without even taking into account any societal costs such as travel delays (NETS, 2016). An earlier estimate by Zaloshnja and Miller (2006) for the period of 1998-2001 puts the total annual cost of on-the-job traffic crashes for employers at about $40 billion. From an occupational safety point of view, this suggests substantial potential savings for workers, employers, and society at large.

What makes traffic crashes a particularly interesting occupational policy issue for workers is the fact that traffic mortality is a health outcome that is both preventable and also highly responsive to changes in economic activity. Indeed, the largest effects of business cycles on mortality is through motor vehicle crashes (e.g., Gerdtham and Ruhm, 2006; Miller et al., 2009; Ruhm, 2000, 2015). Many studies show that traffic safety tends to display a procyclical pattern such that economic downturns lead to significantly fewer fatalities. Using US data from 1972-1991, Ruhm (2000) addresses the question of whether recessions can be good for health in his seminal paper. He estimates that a one percentage point increase in the unemployment rate decreases total mortality by about 0.6% or around 11,000 deaths in a given year. His estimated mortality elasticity associated with traffic crashes is 3.0%—over five times larger than for overall mortality.

Similarly, Boone and van Ours (2006) report that the occupational accident rate displays a procyclical pattern for 16 OECD countries during 1976-2001. Hence, we would expect work-related traffic fatalities to respond to economic fluctuations much more quickly than other causes of mortality (e.g., cancer or cardiovascular disease). He (2016) provides empirical evidence that traffic patterns related to economic activity lead to a higher risk of motor vehicle fatality when the economy improves. The author focuses on the Great Recession period in the US and uses state-level panel data for 2003-2013. She shows that the procyclical pattern of fatalities is more pronounced for crashes that involve large trucks, which can directly be tied to changes in commercial and economic activity that occur during macroeconomic fluctuations.

As noted earlier, the COVID-19 pandemic has sparked a renewed interest in improved traffic safety as an unintended positive consequence of an economic downturn. First, the pandemic combined with the associated deep recession led to significant reductions in commercial and personal driving. According to US Department of Transportation data, travel on all roads in the US in 2020 decreased to the same level of driving as back in 1999—a 19% drop (Coren, 2020). Second, recent estimates suggest that more than 35% of Americans who were working prior to the pandemic switched from commuting to working remotely (Brynjolfsson et al., 2020). As a result, American workers ended up saving an estimated $758 million per day since the onset of the pandemic (Tanzi, 2020). This includes not only the direct costs of commuting (e.g., gas and vehicle maintenance), but also indirect (e.g., time spent driving) and externality (e.g., injuries, deaths, and property damage due to traffic crashes, pollution) costs.

In this paper, we examine the effects of business cycles on traffic fatalities in the US with a special focus on *work-related* (WR) motor vehicle crashes. Specifically, we consider macroeconomic fluctuations around the recent Great Recession to discern and characterize the effects of business cycles on WR traffic crashes, defined as those involving at least one vehicle that is registered as a business, company, or government vehicle. Starting at the end of 2007 and lasting until mid-2009, real gross domestic product dropped at an unprecedented rate not seen since the Great Depression and almost nine million Americans lost their jobs (Economic Report of the President, 2010). Bankruptcies and home foreclosures quickly followed the crashing of both the stock and housing markets, all of which made this a very severe economic and financial crisis. As such, the Great Recession provides a particularly salient case study to examine WR traffic fatalities (WRTFs). We implement a longitudinal design by assembling state-by-quarter US data (2004-2012) from the Fatality Analysis Reporting System (FARS).

We significantly extend the exiting literature on the relationship between economic activity and mortality by paying particular attention to WRTFs. While He (2016) analyzes motor vehicle safety in general, we directly distinguish between WR versus non-WR (NWR) traffic fatalities. The novelty here is the incorporation of certain FARS data elements to capture WR/NWR status based on vehicle registrations and also on another indicator categorizing whether at the time of the crash a deceased individual was "at work." One of our main contributions to the extant literature is to highlight a specific type of externality—that associated with WR activities as opposed to those externalities associated with driving under the influence of alcohol. The latter has been frequently examined in the existing literature. Here, we show that WRTFs are just as responsive to business cycles as alcohol-impaired traffic fatalities, underscoring the everyday compromises between population health and the economy. Our estimates reveal that a one percentage point decline in the state unemployment rate is associated with a 4.7% increase in WRTFs. To provide further context and insights, we conduct a series of comparisons, such as workweek versus weekend and daytime versus nighttime crashes.

Background

In 2018, about a quarter of all WR fatalities in the US (1,276 deaths out of a total of 5,250) occurred in roadway incidents (BLS, 2019). The occupation with the most fatal WR injuries was drivers, particularly truck drivers. While the fatal injury rate (the number of fatal occupational injuries per 100,000 full-time equivalent workers) was about 3.5 for all American workers, the rate is 26.0 for drivers in general and 28.3 for truckers. Limited and fragmented data sources do not allow for detailed analyses of WRTFs in the US, but researchers elsewhere report patterns that are noteworthy. For example, drivers of company vehicles in Australia were found to be 50% more likely to experience traffic crashes compared to drivers who use their own vehicles even after controlling for demographics and other factors such as annual miles driven (Lynn and Lockwood, 1998), suggesting some form of moral hazard. Interestingly, many factors that typically play a significant role in traffic safety, such as alcohol use and speeding, are less likely to influence WR motor vehicle crashes (CDC, 2011; Mitchell et al., 2014).

As explained in detail below, work-relatedness of traffic crashes in the US is not easy to discern with existing data sources. For example, the National Household Travel Survey (NHTS) is a nationally representative survey of travel and commuting behavior of Americans. It provides various details about trips including the mode of transportation and whether they were for work/business or for personal/recreational purposes. However, these surveys were conducted in 2001 and then again in 2009, rendering them unusable for the purpose of examining the effects of the Great Recession. Nevertheless, the NHTS provides insightful statistics on the extent of travel and commuting patterns in the US. For example, on average, 28% of the miles traveled in 2009 were for work/business purposes (Santos et al., 2011). Of these WR miles traveled, more than 91% was by private vehicles and only about 2.2% was using public transit (with the remainder using other means such as bicycles, walking, etc.). Thus, a significant portion of the motor vehicle travel in the US is WR and an overwhelming majority of that takes place in private vehicles.

In this paper, we use data from the FARS, which allows us to address a prominent feature of WR traffic collisions—the fact that a great majority of fatalities pertain to non-workers or bystanders. According to Drummond et al. (2017), bystanders may be pedestrians or occupants of other vehicles who are not identified as working themselves, but are involved in a crash with another driver who is working. Employers are financially responsible for injuries sustained by their employees, but they may also have to compensate for harms caused by their employees to any bystanders (Zaloshnja and Miller, 2006). Recently, this situation was highlighted in the media due to an increase in crashes involving bystanders caused by delivery drivers of the online retail giant Amazon. Critics claim that Amazon's speedy delivery promise has come at a high human cost and that the company should bear some of the legal and liability consequences associated with faster shipping (Callahan, 2019). Using the Drummond et al. (2017) definition of WRTFs allows us to consider the total human toll by taking into account not only worker-specific deaths, but also other fatalities that result from a WR traffic crash.

Most research studies on occupational health have focused on non-fatal injuries since these are far more numerous compared to fatal injuries at work. For example, Boone and van Ours (2006) report that only about 0.01% of all WR accidents in the US were fatal. However, non-fatal injuries are typically subject to measurement error as claims reporting can mask the true injury rates at work. This is particularly problematic during recessions, as workers may be reluctant to report injuries when unemployment rates are on the rise. Using US data from 1992-2009, Butler (2011) examines the relationship between workplace safety and business cycles by considering both fatal and non-fatal injuries. Using workplace safety data, he shows that both injury rates are procyclical. Moreover, he finds that occupational fatality rates are more responsive to business cycles compared to non-fatal injury rates. Unfortunately, reliable data on non-fatal traffic injuries are not consistently available for all 50 states over the time period of our analysis, so we focus on fatal crashes only. Nevertheless, Butler (2011) goes on to explain that fatal injuries are a more reliable measure of workplace safety compared to non-fatal injuries, because the former are not subject to measurement errors in reporting propensity.

Theoretical Framework

Several explanations exist for why WRTFs may be sensitive to business cycles. First, increased economic activity presents higher crash risks simply due to more vehicle miles traveled. Second, it may also pose higher risks in the form of more hazardous work and/or driving conditions. This could be because working longer shifts increases exposure to rush hour traffic and crashes in general, and WR crashes in particular. It could also be related to elevated job-related stress, simply due to higher work intensity even in the absence of additional overtime or driving, which could lead to more fatigued, distracted, and aggressive driving. A third mechanism could relate to the opportunity cost of time increasing during economic upturns. Time pressure may lead people to engage in more risky driving behaviors when the economy is strong such as speeding, texting while driving, drowsy driving, etc. The first two mechanisms above reflect an external *spillover effect* that is independent from whether a worker personally experiences job loss or financial setback. Each or all of these mechanisms could explain the observed procyclical nature of WRTFs. One of the reasons why traffic crashes may display a countercyclical pattern is if recessions reduce the resources local governments direct to traffic law enforcement, which would in turn pose higher traffic risks.

Economic expansions can also impact traffic safety if higher incomes lead to more alcohol consumption and thus more drunk driving. However, reductions in income- and employment-related stress during economic booms may decrease "self-medication" with alcohol or drugs, which in turn might curb impaired driving. Ruhm and Black (2002) provide evidence that the former effect dominates the latter. Ruhm (1995) attributes higher motor vehicle fatality rates during good times mainly to the rise in alcohol use and drunk driving. Similarly, Cotti and Tefft (2011) find that fatal crashes involving alcohol are more responsive to business cycles than are overall fatal crashes. With the exception of increased traffic congestion, which poses only "external" risks, the rest of the possible mechanisms can also operate though "internal" effects as they can be influenced by individual behaviors too.

The reason why occupational mortality in particular can be procyclical is because health is an input into the production of goods and services (Ruhm, 2000). During economic expansions, workers may be pushed to their limits due to longer work hours or increased work intensity even if their work hours remain the same. As a result, we would expect commuting time and WR driving to increase relatively more compared to leisure-related driving, which would increase occupational health risks including WR relative to NWR motor vehicle crashes. However, a relative reduction in leisure-related driving during recessions may significantly lower alcohol-impaired driving and mostly NWR crashes. Other factors, such as occupational stress, can impact WR and NWR traffic safety during both good times and bad times albeit for different reasons—stress due to increased overtime or workload versus anxiety due to job and financial losses.

To the extent that increasing unemployment rates lead to changes in the composition of the workforce by reducing the proportion of inexperienced workers as reported by de la Fuente et al. (2014), the remaining more experienced and typically older workers will presumably be less prone to WR accidents in general and WR traffic crashes in particular (Grabowski and Morrisey, 2001). This would then suggest a relatively greater impact of recessions on WRTFs relative to NWRTFs. Finally, some factors may have similar effects on WR versus NWR traffic safety, such as increased traffic congestion during macroeconomic expansions or reductions in public health spending and traffic safety enforcement during recessions. Ultimately, whether WR traffic mortality is more procyclical than NWR traffic mortality is an empirical question. Based on this theoretical background, we pose a series of hypotheses.

Hypothesis 1 (H1): WRTFs are more sensitive to business cycles than NWRTFs.

Given that WR driving or commuting to workplaces in general happens more during the workweek as opposed to the weekend, and during daytime as opposed to nighttime, we have the following expectations:

Hypothesis 2a (H2a): Any difference in the sensitivity of WRTFs versus NWRTFs to business cycles (expressed in *H1*) is greater during the workweek than the weekend.

Hypothesis 2b (H2b): Any difference in the sensitivity of WRTFs versus NWRTFs to business cycles (expressed in *H1*) is greater during daytime hours than nighttime hours.

To the extent WR driving may respond more to business cycles via changes in overtime hours rather than during regular workweek/daytime hours, then the opposite of *H2a* and *H2b* could be expected. However, we would expect recreational/leisure driving (just like spending) to be more responsive to changes in economic activity compared to WR driving since the latter is a necessity. In addition, recessions could force some workers to accept distant jobs that require longer commutes (Maheshri and Winston, 2016). Finally, if workers in denser urban areas are more likely to be exposed to more congested roads and heavy rush-hour traffic than their counterparts in rural areas (Sameem and Sylwester, 2017), we expect the following:

Hypothesis 2c (H2c): Any difference in the sensitivity of WRTFs versus NWRTFs to business cycles (expressed in *H1*) is greater in urban areas than rural areas.

Data and Methods

This study utilizes data for all 50 US states from 2004 to 2012. A distinct challenge when studying WRTFs is that work-relatedness of traffic crashes is not easy to identify. In the US, data sources on motor vehicle fatalities are not linked with data on WR injuries to allow research on the intersection of these two phenomena. Fatal work injury data in the US are produced by the Census of Fatal Occupational Injuries (CFOI), part of the BLS's Occupational Safety and Health Statistics (OSHS) program. The CFOI uses a combination of federal, state, and independent data sources to identify and describe fatal work injuries. While the CFOI allows verification of whether the incidents are job-related, it does not provide information on non-workers who may have been injured or died in fatal work incidents. The number of deaths recorded by the BLS, therefore, does not reflect the full extent of human loss.

CFOI data are compiled by patching together information from multiple sources, including death certificates, police reports, and workers' compensation claims. Even though this method involves reviews of tens of thousands of unique documents, it leaves out an obvious group of affected individuals—*bystanders* who were not working at the time of the crash. Data from workers' compensation claims lack crash-related information as well as who else may be involved in a traffic crash (McCall and Horwitz, 2005). While police traffic crash records cover all parties involved in a crash, they are less likely to identify fatalities as WR (CDC, 2016a; Thygerson et al., 2011). Even if a crash is deemed WR, they do not offer any information on the worker(s) or work characteristics. Finally, inconsistencies in hospital records make it difficult to define work-relatedness according to whether workers' compensation was the primary payer.

Given these limitations, no US database is able to identify fatal WR traffic crashes and at the same time provide detailed characteristics on the crash, including all involved vehicles and individuals. To alleviate this problem, efforts are ongoing by the National Institute for Occupational Safety and Health (NIOSH), the National Highway Traffic Safety Administration (NHTSA), and the BLS (Byler et al., 2016). The lack of complete data and standard definitions of work-relatedness pose significant challenges to study WRTFs not only in the US but also in other countries (e.g., Adminaite et al., 2017; Byler et al., 2016; Drummond et al., 2017; Thomas et al., 2012). We address this challenge by utilizing data from the Fatality Analysis Reporting System (FARS).

FARS is a surveillance system administered by NHTSA. It is not based on a sample but a census in that it includes detailed information on every traffic crash in the US that occurs on public roads as long as it results in at least one fatality within 30 days after the crash. Unfortunately, work-relatedness is not easy to discern with FARS data either. Nevertheless, the FARS database has a major advantage in that it captures some of the harm (i.e., fatal) to bystanders who make up the great majority of fatalities in WR traffic collisions (Drummond et al., 2017). Considering all deaths that result from a WR traffic crash allows us to better gauge the full extent of externalities associated with WR activities.

Sample and measures

The Great Recession began in December 2007 and ended in June 2009. Our sample includes quarterly data on traffic fatalities from 2004-2012 for all 50 states (*N*=1,800). Total fatalities refer to the number of occupants (i.e., drivers and passengers) as well as non-occupants (e.g., pedestrians, bicyclists) killed in traffic collisions. We divide total fatalities into two groups—WRTFs and NWRTFs—to provide a side-by-side comparison. We define WRTFs as those that occur in any crash that involved at least one vehicle that is registered as a business, company, or government vehicle—henceforth, "company vehicle." It is possible that such vehicles are used for NWR purposes, which would be a type I measurement error. Our classification may suffer from a type II error as well if we fail to assign WR status to a crash simply because no company vehicles were involved. Despite these shortcomings, our definition of WRTFs has a major advantage—we count not only the fatalities among occupants of company vehicles but also any other bystander deaths that result from the same crash. This broader definition can yield more than four times higher counts.

While we label some fatalities as bystanders, they may also be at work when the crash occurred even though they were not driving a company vehicle (i.e., type II error). Many working individuals tend to drive their personal vehicles (or carpool), take public transportation, or walk to work. According to an estimate from Australia, 75% of driver casualties occur not during the course of work but during commuting (Boufous and Williamson, 2006). Driscoll et al. (2005) report that overall WR fatalities are underestimated by about 30% in the US with WRTFs being subject to a greater degree of measurement error. Similarly, the European Transport Safety Council (Adminaite et al., 2017) estimates that up to 40% of all fatalities may be WR. Hence, even with the inclusion of bystanders, our total count of WRTFs is certainly a lower bound of the true figure. Consequently, our results that follow are likely to constitute a conservative lower bound estimate of the true effect of business cycles. As part of our robustness checks, we experiment with alternative definitions of WRTFs as explained further below.

Table 1 presents descriptive statistics for our sample. Considering all fatalities, the average fatality rate (per 100,000 people) is 3.55 in a given quarter and state in our sample. The average WRTF rate is 0.55. Therefore, about 15.5% of all fatalities are identified as WR. This proportion is close to, but still lower than, the estimate of Drummond et al. (2017) who report that 23% of all fatalities in Ireland were WR over the period of 2008-2011. Considering that the Drummond et al. estimate excludes commuters, the true proportion of WRTFs is actually higher. Their figure does not include commuters simply because workers' compensation typically does not pay for injuries to commuters. However, many European Union countries recognize commuter deaths as WR (Drummond et al., 2017). Overall, our fatality rates significantly underrepresent the true extent of WR traffic deaths, but unlike workers' compensation data, they capture the fatal injuries to bystanders. A WRTF rate of 0.55 per quarter and state translates into a total of 6,666 WRTFs in a given year across the entire US. The BLS (2019) figure of 1,276 WR deaths in roadway incidents would correspond to only 19% of this total, which is similar to the reports of Drummond et al. (2017) that only about 15% of all fatalities in WR traffic collisions are workers themselves, while the remaining 85% are bystanders.

[INSERT TABLE 1 HERE]

Given the focus on alcohol use as a major factor in prior studies, we also analyze the responsiveness of alcohol-impaired fatalities to business cycles, which provides a direct comparison to WRTFs. A motor vehicle crash is identified as alcohol-impaired if the blood alcohol content (BAC) of any driver involved in the crash was above the legal BAC limit of .08g/dL. The average alcohol-impaired fatality rate is 1.06 in a given quarter and state in our sample (i.e., 30% of all traffic fatalities), suggesting that driving while intoxicated is a major concern. However, on average, only about 2.1% of all fatalities (and 0.8% of single-vehicle fatalities) are characterized as both WR and alcohol-impaired. Therefore, the overlap between the two categories is extremely limited and alcohol-impairment can essentially be ignored as a factor in WR motor vehicle collisions. This is consistent with earlier research that reports a significantly lower likelihood of alcohol involvement in WR traffic incidents compared to NWR ones (e.g., Mitchell et al., 2014; Thygerson et al., 2011).

The outcomes listed in the top panel of Table 1 include both WRTFs and NWRTFs broken down by day of the week, time of the day, and urban versus rural areas. As this table reveals, the majority of WRTFs tend to occur during the workweek as opposed to weekend, during the daytime as opposed to nighttime, and in rural areas as opposed to urban areas. These averages reflect the amount of WR driving as well as the motor vehicle crash collision risks in each grouping. In what follows, we estimate a series of linear regression models for fatality rates that include state, year, and quarter fixed-effects along with a rich set of time-varying factors at the state level. Several traffic and alcohol policies, including beer taxes, BAC limits, zero tolerance laws, and graduated licensing laws, were left out of the estimation because they display negligible or no within-state variation during 2004-2012. The bottom panel of Table 1 reports descriptive statistics for all control variables included in the regressions. Appendix Table A1 includes full definitions of the variables along with a detailed list of data sources. All data were obtained from publicly available sources, and as a result, institutional review board review was not sought nor necessary.

Analyses and Results

Figure 1 depicts percentage changes in fatality rates over the same quarter of the previous year. This illustration accounts for seasonal forces that tend to influence driving patterns as well as crashes. Clearly, the WRTF rate displays a bigger drop during the recession and also a faster increase during the following recovery period compared to both the NWRTF and alcohol-impaired fatality rates. The WRTF rate seems to also show a more sustained drop even prior to the official beginning of the Great Recession—presumably due to the early stages of the economic slowdown. Besides the seasonal trends, there is also a downward trend in fatalities over the years (not shown in this figure) as traffic deaths decline over time mostly due to improvements in motor vehicle safety. In our empirical analysis, we account for these time and seasonal trends by including both year and quarter fixed-effects in addition to state fixed-effects that capture state-specific idiosyncrasies.

[INSERT FIGURE 1 HERE]

Our empirical specification takes the following form:

(1) $$lnF_{stq} = \beta_0 + \beta_1 UR_{stq} + \boldsymbol{X}_{stq}\beta_2 + \lambda_t + \delta_s + \gamma_q + \varepsilon_{stq}$$

where lnF_{stq} is the natural logarithm of fatalities per 100,000 people in state *s*, year *t*, and quarter *q*. UR_{stq} is unemployment rate, which proxies for business cycles specific to each state, quarter, and year. This variable is the main focus of our analysis. The vector $\boldsymbol{X}$ contains state-, year-, and quarter-specific characteristics, also listed in Table 1, which could potentially influence traffic safety such as traffic policies. Year fixed-effects (λ_t) account for annual secular nationwide trends in traffic safety. State fixed-effects (δ_s) account for any time-invariant factors in traffic safety across states. Finally, quarter fixed-effects (γ_q) capture any seasonal variations in traffic crashes. In the rare instances when zero fatalities appear in any given specification, we set the outcome measure equal to 0.1 fatalities per 100,000 people to ensure that the logarithm is defined.

The main estimation results are presented in Tables 2-4. All of the estimated coefficients are based on least squares regressions weighted by state population. In all regressions, we clustered the standard errors to allow for non-independence of observations within each state. Coefficient estimates for fixed-effects (and in some tables for other variables as well) are omitted for brevity. Full estimation results can be obtained from the authors upon request. The two variables that have typically been the focus in prior studies are the total fatality rate in column (1) and alcohol-impaired fatality rate in column (2). However, the last two columns are of greater interest for us—WRTF versus NWRTF rates in columns (3) and (4), respectively. Finally, each table reports the mean of the dependent variable to facilitate interpretation of the estimates. The log-linear specification implies that a one percentage point change in the unemployment rate is associated with an estimated exp(β_1)-1 percentage change in the fatality rate.

In column (1) of Table 2, results show a one percentage point increase in the state unemployment rate is associated with a 2.9% (i.e., exp(-0.029)-1=2.86%) decrease in the overall fatality rate (p<.01), which is similar to the estimated reductions reported in extant studies—3.0% (Ruhm, 2000) and 2.9% (He, 2016; Miller et al., 2009) for the US and 2.2% for Europe (Tapia Granados, et al., 2017). Also consistent with the existing literature, the alcohol-impaired traffic fatality rate is particularly sensitive to business cycles (3.6%; p<.01). More importantly, the WRTF rate is more responsive to changes in economic activity than the NWRTF rate—4.7% (p<.01) versus 2.7% (p<.01), respectively. A formal post-estimation test rejects the equality of the estimated unemployment rate coefficients between WRTF and NWRTF rates (p<.10). Hence, WRTFs are particularly responsive to economic downturns providing empirical support for *H1*. These findings suggest that the adverse effects of economic upturns on traffic safety work not only through an increase in drunk driving, but WR factors as well. Therefore, any traffic safety measures that are designed to prevent drunk driving alone will not be sufficient to fully address traffic deaths during economic expansions.

[INSERT TABLE 2 HERE]

Although the control variables are of minor importance, they display a few interesting patterns. For example, while speed limits are not significantly associated with overall, alcohol-impaired, or NWRTF rates, they are significantly associated with reductions in WRTF rates (p<.01). The proportion of light trucks among all registered motor vehicles is significantly associated with an increase in the overall motor vehicle fatality rate (p<.01) and also with an increase in the NWRTF rate (p<.10). As expected, traffic volumes captured by VMT per licensed driver has a significant impact on all fatality rates, albeit smaller for the WRTF rate. This pattern is even more prominent when we consider the effect of alcohol consumption per capita on fatality rates. These findings are largely consistent with those reported in the existing literature (e.g., Noland and Zhou, 2017).

To provide further insights, we conduct a series of comparisons by disaggregating fatalities according to day, time, and location of the crash. These results can be found in Table 3. First, as seen in column (1) of this table, the effect of business cycles is not just isolated to workweek, daytime, or urban areas. Second, in each panel of this table, the WRTF rate (column (3)) is typically more responsive to changes in economic activity than the NWRTF rate (column (4)). The differences in responsiveness, however, depend on crash characteristics. For example, in panel 3a, the gap in the sensitivity of WRTF versus NWRTF rates to business cycles is statistically significant for workweek crashes (p<.01) but not for weekend crashes. Similarly, the difference in the sensitivity of WRTF versus NWRTF rates to business cycles is statistically significant for daytime crashes (p<.05) but not for nighttime crashes.

[INSERT TABLE 3 HERE]

Further examination of column (3) of panel 3c reveals that the WRTF rate in urban crashes is much more responsive to business cycles (6.8%, p<.05) compared to the NWRTF rate (4.2%, p<.01). However, the estimated difference is not statistically different from zero. When we consider rural crashes, neither WRTF nor NWRTF rates are responsive to changes in economic activity. Even the estimate for the overall fatality rate in rural crashes in column (1) is small in magnitude and marginally significant. This finding is consistent with that of Sameem and Sylwester (2017) who report that motor vehicle crashes are more procyclical in urban counties compared to rural ones. The explanation may be related to the fact that working individuals in rural areas are more likely to be engaged in agriculture, which is less sensitive to fluctuations in the aggregate economy (e.g., Da-

Rocha and Restuccia, 2006). Considering these results collectively, the findings presented in Table 3 provide support for *H2a* and *H2b*, but not for *H2c*.

Table 4 reports the results of various sensitivity checks, which reveal that our findings are robust across various specifications, estimation techniques, and alternative definition of WRTFs. At the top of Table 4 (panel 4a), we repeat the baseline estimation results from Table 2 for comparison purposes. Whether using unweighted regressions (panel 4b), replacing unemployment rate with real personal income per capita as a proxy for business cycles (panel 4c), excluding the Great Recession period between 2007Q1 and 2009Q2 (panel 4d), including state-specific time trends in addition to fixed-effects (panel 4e), or replacing year and quarter fixed-effects with year-by-quarter fixed-effects (panel 4f), these extensions do not alter our main conclusions.

[INSERT TABLE 4 HERE]

In panel 4g, we consider only those crashes that involved a single motor vehicle (i.e., crashes where a motor vehicle had a collision by itself or with non-motorists such as bicyclists or pedestrians). This is to identify the driver who is at fault, which is very challenging when multiple vehicles are involved. The driver in a single-vehicle crash is almost certainly at fault as the bystanders are rarely the cause of a crash (e.g., jaywalking). If this driver was in a company vehicle, then both the crash and the fatality(s) can be more confidently defined as WR, whereas in multiple vehicle crashes it is not clear who is at fault. Similarly, in column (2), we focus attention on those crashes where only one motor vehicle was involved and its driver was alcohol-impaired. Even though these subsets of crashes limit the number of fatalities examined, and thus reduce the precision of the estimates, estimated coefficients turn out to be almost identical to our baseline findings (panel 4a).

In panel 4h, we consider all crashes that involve at least one large truck (i.e., trucks with a gross vehicle weight rating greater than 10,000 pounds). This is because large trucks are almost exclusively used for work purposes (He, 2016) and they tend to be very dangerous in a crash due to their sheer size. CDC (2011) reports that the overwhelming majority (67%) of WR highway transportation fatalities occurred among truck occupants. According to a NHTSA (2019) report, an overwhelming majority of fatal crashes that involved at least one large truck in 2017 occurred during weekdays (78%) and daytime (72%). Moreover, only a very small portion of large truck drivers was alcohol-impaired at the time of the crash (3%). A comparison of the estimates in column (1), between panels 4a and 4h, indicate that the fatality rate in crashes involving large trucks is far more responsive to business cycles than the overall fatality rate. As expected, also in panel 4h, business cycles affect traffic safety mainly through their impact on WRTF rather than on NWRTF rates. It is important to note that large trucks have been responsible for the highest number of other vehicle occupants killed in every year between 2008 and 2017. As such, large trucks can pose significant negative externalities in terms of traffic safety, which is exacerbated during economic booms.

An additional question that may arise is whether business cycles affect the likelihood of a WR traffic crash or whether it instead increases the number of people riding in each vehicle, which would then translate into higher fatality rates even if the crash risk were unchanged. To investigate this question, we consider the rate of fatal traffic *crashes* (panel 4i) as opposed to the rate of *fatalities* (panel 4a). The estimates (both WR and NWR) reveal that business cycles primarily affect motor vehicle crash risk rather than simply the number of people who end up being involved in crashes.

An alternative method of estimation in the traffic safety literature is the conditional fixed-effects Poisson regression, which allows for modeling the count of fatalities rather than a fatality rate by conditioning on state, year, and quarter fixed-effects. To provide estimation results comparable to our fatality rate (i.e., per 100,000 people) estimates in least squares models, we include the natural logarithm of state population as an offset in the count data models. Panel 4j presents results of the Poisson models, which once again are consistent with our baseline estimates in panel 4a.

In general, conditional fixed-effects Poisson models work well when the outcome variable takes on mostly small integer values, as is the case in panel 4k. In this last panel, we consider an alternative definition of WRTFs that is based on a FARS data element indicating whether at the time of the crash a deceased individual was "at work" or not. Using this classification, we construct the number of fatalities in crashes that include at least one driver who was *at work* when the crash occurred. Unfortunately, this information is available only for those drivers who died in the crash and not for others who suffered non-fatal injuries or were not hurt. Thus, this alternative *at work* fatality count is also subject to measurement error, leading to a substantial undercount of all WRTFs. Nevertheless it is consistent with our default WRTF count since they are highly correlated (r=0.74). More importantly, this alternative *at work* definition yields very similar estimation results to our baseline estimates. Hence, we infer that our default measure does a relatively good job of capturing the concept of WRTFs despite its noted shortcomings.

Finally, we investigate whether WRTF rates are procyclical simply due to higher traffic volumes experienced during economic booms. All forms of congestion on roadways—industrial, commercial, commuting, and leisure—fall during recessions, albeit to different degrees. Alternatively, even in the absence of any changes in traffic congestion, a procyclical pattern of WRTF rates can emerge if business cycles impact crash risk, conditional on vehicle miles driven, due to other reasons such as a higher proportion of larger vehicles on the roads or more aggressive or stressful driving. He (2016) shows that the procyclical pattern of overall traffic fatality rates in the US during the Great Recession can be explained mostly by an increase in collision risk rather than exposure to greater amounts of driving.

In Table 5, we examine traffic volume as the dependent variable measured by the logarithm of VMT. Regardless of how traffic volume is defined in each of the three columns, we find that a one percentage point increase in the state unemployment rate is associated with a 1.0-1.3% decrease in VMT. Comparing these figures to the estimated effects presented in Table 2, we conclude that the reduction in traffic congestion during recessions explains much of the drop in the NWRTF rate, but a much smaller portion of the decline in the WRTF rate. The residual effect could possibly be due to other factors related to risky driving behaviors such as a rise in aggressive, distracted, or stressful driving.

[INSERT TABLE 5 HERE]

Discussion and Conclusions

In this paper, we show that WR traffic deaths during prosperous times can be directly related to increased economic activity. Our core estimates indicate that a one percentage point decline in the state unemployment rate is associated with a 4.7% increase in WRTFs. The average WRTF rate is 0.55 in our state-by-quarter sample (see Table 2), which translates into about 313 lives lost per year across the US. Given that we substantially undercount WRTFs due to data availability and limitations described earlier, this projection is most likely a conservative estimate of the potential lives lost, not to mention other economic consequences including medical care expenses, property damage, etc. During the Great Recession, the average state unemployment rate went up by 4.24 percentage points. Applying our findings suggests that the WRTF rate decreased by 20.0% over these seven quarters of economic decline—representing one of the most profound unintended positive effects of the Great Recession.

Considering earlier studies that focus on alcohol-impaired motor vehicle crashes during economic expansions, WR deaths cannot be blamed on drunk driving. Instead, mechanisms such as higher traffic volumes, increased freight movement, higher proportion of larger vehicles, and more stressful driving conditions can play a larger role in WRTFs. While our analysis sheds some light on traffic crashes as the leading cause of occupational deaths, we are unable to provide direct evidence of the underlying potential mechanisms due to the data issues discussed earlier. For example, a study from Norway reveals that high speed and failure to use a seat belt are the most important risk factors in fatal accidents triggered by drivers at work (Nævestad et al., 2015). Another study from Australia reports that alcohol, fatigue, and speed are less likely to be important factors in WR crashes compared to NWR crashes (Mitchell et al., 2014). Maheshri and Winston (2016) use detailed data on driving behaviors of individuals from State Farm Mutual Automobile Insurance Company to show that the Great Recession may have improved traffic safety partly by reducing VMT for observably risky drivers (e.g., seniors) and instead increasing VMT for safer drivers (e.g., middle-aged adults). A more nuanced examination of such contributing factors is a potential avenue for future research as the US data sources improve.

As discussed in the Data and Methods section, our classification of work-relatedness may be subject to measurement error. This is largely due to limited and fragmented data sources that is acknowledged by the NIOSH. Given the substantial human and economic costs of WRTFs, reliable databases that provide detailed information on such crashes, involved vehicles, and individuals are needed before we can develop effective prevention strategies. Given that current data sources significantly undercount the number of WRTFs, it is not presently feasible to conduct finer-level analyses (e.g., by industry or worker versus bystander), which may be possible with improved data in the future. A critical piece of information to capture would be the amount of time or actual mileage workers accrue in a given month or year, to better account for crash risk exposure. It would then be possible to identify whether recessions reduce WRTFs by reducing congestion in general or by reducing time spent on the roads for workers in particular.

Following the literature, we measure business cycle fluctuations using within-state over-time variation in unemployment rates. However, unemployment rates may not capture the entire layoff risk and associated stressors recessions pose for workers. Strully (2009) points out that "job churn," described as high rates of job displacement and reemployment, instead of high rates of unemployment per se, can also adversely affect population health. Given that the Great Recession led many workers to quit the labor market altogether, and to the extent that job losses can have sustained impacts on health over time, our estimates would underestimate the full effect of business cycles on WRTFs.

Although fatalities are the most costly consequences of WR motor vehicle collisions, non-fatal injuries are far more numerous. Chen (2009) reports that for every WRTF in 2002, about 50 non-fatal WR traffic injuries were treated in emergency departments. While we lack data to examine non-fatal injuries, this topic would be an important extension in the future. Finally, our fixed-effects estimation techniques account for unobservable time-invariant state characteristics and observable time-varying characteristics, but they do not account for unobservable factors to the extent they may be varying over time. For example, if business cycles differentially affect the patterns of commuting or modes of transportation, such factors would not be captured by our models.

Overall, traffic crash mortality makes up one of the leading causes of death for Americans. In 2017, traffic crashes killed more than 100 people every day and about 7,000 people per day are treated in emergency departments for traffic-related injuries (CDC, 2020). Sadly, these stark numbers also mean that we have become somewhat callous to traffic crashes even though, or precisely because, it is one of the top causes of death in the US. The US has the highest rate of motor vehicle crash fatalities compared to other high-income countries, along with the lowest reduction in traffic crash deaths over time (CDC, 2016b). Our findings suggest that policies to address the risks associated with drunk driving will not be sufficient to mitigate the traffic fatality tolls of economic expansions—especially not those that are WR. Considering the highly preventable nature of traffic crashes, this is a public health issue that can be approached through actions taken by drivers, policymakers, and employers. Back in the early 2000s, the CDC (2004) recommended that employers develop and implement programs to purchase safe vehicles and encourage safer driving among its employees. At the present time, employers can take this recommendation a step further by introducing autonomous vehicles or shifting some of their workforce to virtual offices, which may be far more effective in reducing occupational death and injuries by taking many workers off the roads. It will be interesting to observe whether workplace cultures and policies forced by the COVID-19 pandemic result in long-lasting changes. If so, it is likely that future macroeconomic fluctuations will have a much smaller impact on occupational traffic fatalities.

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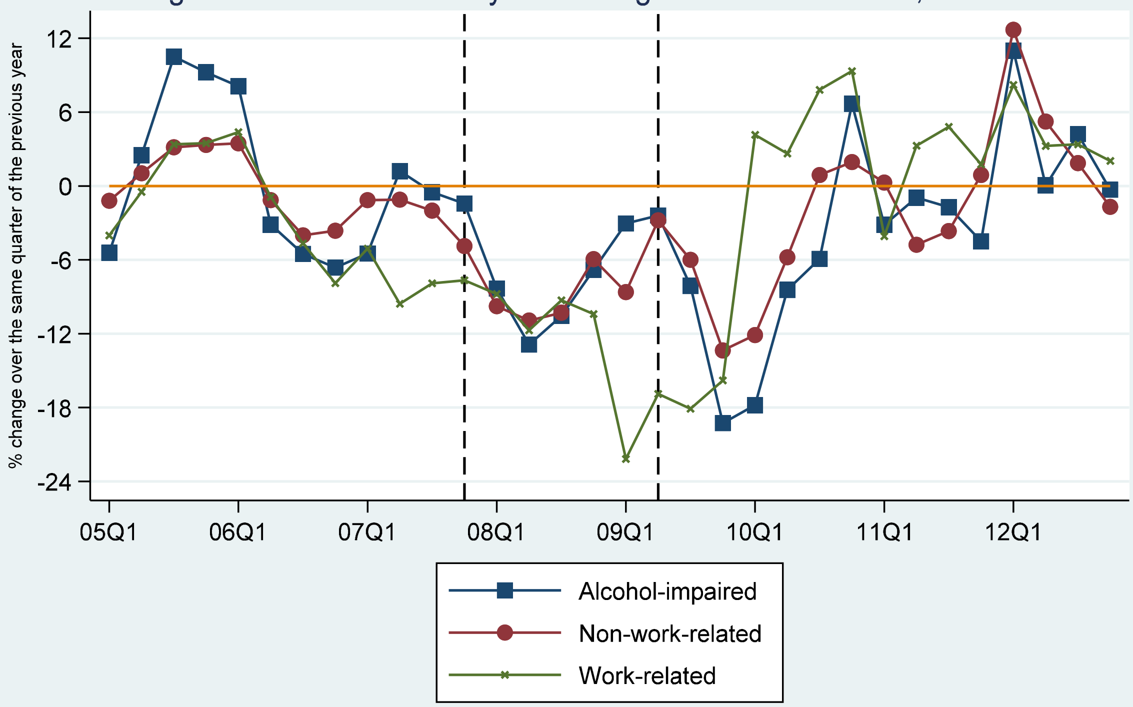
Figure 1. Economic activity and changes in traffic fatalities, 2004-2012



Source: Quarterly civilian unemployment rates come from the US Bureau of Labor Statistics and traffic fatalities come from the FARS.
Note: Dashed lines represent the beginning (December 2007) and the end (June 2009) of the Great Recession.

Table 1: Descriptive statistics, 2004Q1-2012Q4 (N=1,800)

Dependent variables (all expressed per 100,000 people)	Mean	Std. Dev.
<u>Traffic fatality rate</u>[1]	3.55	1.59
Alcohol-impaired (BAC≥.08) fatality rate	1.06	0.59
<u>Work-related traffic fatality rate</u>[1]	0.55	0.36
Workweek (Monday-Friday 8am-6pm)	0.26	0.19
Weekend (between Friday 6pm and Monday 6am)	0.15	0.15
Daytime (between 6am and 6pm)	0.39	0.28
Nighttime (after 6pm and before 6am)	0.16	0.14
Urban	0.17	0.12
Rural	0.39	0.34
<u>Non-work-related traffic fatality rate</u>[1]	2.87	1.28
Workweek	0.79	0.41
Weekend	1.27	0.62
Daytime	1.53	0.73
Nighttime	1.31	0.63
Urban	0.97	0.48
Rural	1.89	1.27
Independent variables		
Unemployment rate (%)	6.32	2.32
Speed limit on rural interstates≥70mph[2]	0.62	0.49
Seat belt law - Primary enforcement[3]	0.61	0.49
Handheld ban - Primary enforcement[4]	0.08	0.26
Texting ban - Primary enforcement[4]	0.19	0.39
Light trucks as a proportion of all registered motor vehicles (%, annual)[5]	48.67	8.14
Proportion of young drivers (% age 24 and under, annual)	13.39	1.97
Vehicle miles traveled (VMT) per licensed driver (1,000s)[6]	3.69	0.71
Alcohol consumption per capita (annual)[7]	2.73	0.55
Real personal income per capita (in constant 2012 $1,000)	42.04	6.54
Population (1,000s, annual)	6,064	6,672

Notes: Data are quarterly unless specified otherwise. Data on fatality measures come from the Fatality Analysis Reporting System (FARS). See Appendix Table A1 for all other data sources. [1]Total fatalities may be larger than the sum of work-related (if the crash involved at least one vehicle that is registered as business, company, or government vehicle) and non-work-related (if the crash did not involve any business, company, or government registered vehicle) traffic fatalities since the motor vehicle registration information is missing or not-applicable for about 4.2% of our observations. [2]Equals one if a state had a speed limit of 70 mph or greater on rural interstates in a given quarter and zero otherwise. Takes on fractional values for the quarters in which laws changed. [3]Equals one if a state had a primary enforcement of seat belt law in a given quarter and zero otherwise. Takes on fractional values for the quarters in which laws changed. Primary enforcement allows law enforcement officers to issue a ticket to drivers or passengers for not wearing a seat belt, without any other traffic offense taking place. [4]Equals one if a state had a primary enforcement of handheld/texting bans in a given quarter and zero otherwise. Takes on fractional values for the quarters in which laws changed. [5]Proportion of light trucks among all registered motor vehicles. Light trucks include pickups, panels, delivery vans, personal passenger vans, passenger minivans, and utility-type vehicles. [6]Vehicle miles traveled (VMT) on public roads in 1,000s. [7]Annual gallons of ethanol content of alcohol consumption per capita age 21 and older.

Table 2: Estimates of linear fixed-effects models for traffic fatality rates, 2004Q1-2012Q4 (N=1,800)

	Total	Alcohol-impaired (BAC≥.08)	Work-related	Non-work-related
	(1)	(2)	(3)	(4)
Unemployment rate	-0.029***	-0.037***	-0.048***	-0.027***
	(0.004)	(0.008)	(0.012)	(0.005)
Speed limit on rural interstates≥70mph	0.037	-0.005	0.110***	0.040
	(0.040)	(0.095)	(0.035)	(0.052)
Seat belt law - Primary enforcement	-0.007	-0.019	-0.048	-0.017
	(0.018)	(0.032)	(0.041)	(0.020)
Handheld ban - Primary enforcement	-0.032	-0.027	-0.013	-0.030
	(0.022)	(0.033)	(0.051)	(0.027)
Texting ban - Primary enforcement	0.002	-0.029	-0.016	0.009
	(0.015)	(0.027)	(0.046)	(0.019)
Light trucks as a proportion of all	0.003***	0.002	0.003	0.002*
registered motor vehicles	(0.001)	(0.002)	(0.003)	(0.001)
Proportion of young drivers	0.006	-0.001	-0.012	0.012
	(0.010)	(0.017)	(0.011)	(0.012)
Ln(VMT per licensed driver)	0.543***	0.613***	0.454**	0.559***
	(0.175)	(0.206)	(0.184)	(0.193)
Ln(Alcohol consumption per capita)	0.596**	0.633*	0.366	0.533**
	(0.234)	(0.317)	(0.460)	(0.244)
Mean of the dependent variable	3.55	1.06	0.55	2.87
p-value for the test of differences in estimated coefficients for unemployment rate in specification (3) vs (4)			0.098*	

Notes: Dependent variable is the natural logarithm of fatalities per capita (per 100,000 people). Each linear regression model also includes year, quarter, and state fixed-effects and all regressions are weighted by population. Standard errors are in parentheses and are clustered to allow for non-independence of observations within each state. *, **, *** Significance at the 10, 5, and 1 percent level, respectively.

Table 3: Estimates of linear fixed-effects models for traffic fatality rates by day, time, and location of the crash, 2004Q1-2012Q4 (N=1,800)

	Total	Alcohol-impaired (BAC≥.08)	Work-related	Non-work-related	*p*-value for the test of differences in estimated coefficients for unemployment rate in specification (3) vs (4)
	(1)	(2)	(3)	(4)	
Panel 3a: Workweek (Monday-Friday 8am-6pm) versus weekend (between Friday 6pm and Monday 6am) crashes					
Workweek					
Unemployment rate	-0.028***	-0.021	-0.056***	-0.022***	0.009
	(0.007)	(0.023)	(0.013)	(0.008)	***
Mean of the dependent variable	1.08	0.10	0.26	0.79	
Weekend					
Unemployment rate	-0.033***	-0.046***	-0.056***	-0.033***	0.271
	(0.005)	(0.007)	(0.020)	(0.006)	
Mean of the dependent variable	1.48	0.60	0.15	1.27	
Panel 3b: Daytime (between 6am and 6pm) versus nighttime (after 6pm and before 6am) crashes					
Daytime					
Unemployment rate	-0.029***	-0.023*	-0.054***	-0.024***	0.010
	(0.005)	(0.013)	(0.011)	(0.006)	**
Mean of the dependent variable	1.98	0.26	0.39	1.53	
Nighttime					
Unemployment rate	-0.030***	-0.038***	-0.042*	-0.031***	0.657
	(0.007)	(0.010)	(0.025)	(0.007)	
Mean of the dependent variable	1.54	0.78	0.16	1.31	
Panel 3c: Urban versus rural crashes					
Urban					
Unemployment rate	-0.045***	-0.047***	-0.070**	-0.043***	0.232
	(0.010)	(0.015)	(0.026)	(0.009)	
Mean of the dependent variable	1.19	0.35	0.17	0.97	
Rural					
Unemployment rate	-0.014*	-0.036***	-0.015	-0.011	0.821
	(0.007)	(0.011)	(0.019)	(0.008)	
Mean of the dependent variable	2.35	0.71	0.39	1.89	

Notes: Dependent variable is the natural logarithm of fatalities per capita (per 100,000 people). Each cell is from a separate linear regression with year, quarter, and state fixed-effects as well as all the other variables listed in Table 2. All regressions are weighted by population. Standard errors are in parentheses and are clustered to allow for non-independence of observations within each state. *, **, *** Significance at the 10, 5, and 1 percent level, respectively.

Table 4: Robustness checks, 2004-2012 (N=1,800 unless noted otherwise)

	Total	Alcohol-impaired (BAC≥.08)	Work-related	Non-work-related
	(1)	(2)	(3)	(4)
Panel 4a: Baseline results (from Table 2)				
Unemployment rate	-0.029***	-0.037***	-0.048***	-0.027***
	(0.004)	(0.008)	(0.012)	(0.005)
Mean of the dependent variable	3.55	1.06	0.55	2.87
Panel 4b: Unweighted regressions				
Unemployment rate	-0.030***	-0.038***	-0.066***	-0.027***
	(0.007)	(0.012)	(0.018)	(0.009)
Panel 4c: Replacing unemployment rate with real personal income per capita				
Ln(Real personal income per capita)	0.522***	0.444	1.642***	0.389**
	(0.173)	(0.278)	(0.369)	(0.191)
Panel 4d: Excluding the Great Recession period of 2007Q4-2009Q2 (N=1,450)				
Unemployment rate	-0.030***	-0.042***	-0.047***	-0.028***
	(0.006)	(0.009)	(0.014)	(0.006)
Mean of the dependent variable	3.59	1.07	0.57	2.90
Panel 4e: Including state-specific quadratic time trends				
Unemployment rate	-0.031***	-0.040***	-0.042**	-0.033***
	(0.003)	(0.006)	(0.017)	(0.004)
Panel 4f: Replacing year and quarter fixed-effects with year-by-quarter fixed-effects				
Unemployment rate	-0.030***	-0.039***	-0.049***	-0.028***
	(0.005)	(0.009)	(0.013)	(0.005)
Panel 4g: Fatality rate in single motor vehicle crashes				
Unemployment rate	-0.029***	-0.037***	-0.049**	-0.028***
	(0.005)	(0.009)	(0.021)	(0.005)
Mean of the dependent variable	1.99	0.73	0.14	1.82
Panel 4h: Fatality rate in motor vehicle crashes that involve at least one large truck (i.e. vehicles with gross vehicle weight rating greater than 10,000 lbs)				
Unemployment rate	-0.063***	-0.069**	-0.078***	-0.042
	(0.018)	(0.034)	(0.021)	(0.032)
Mean of the dependent variable	0.45	0.05	0.37	0.08
Panel 4i: Fatal traffic crash rate as the outcome measure				
Unemployment rate	-0.029***	-0.034***	-0.049***	-0.027***
	(0.004)	(0.008)	(0.012)	(0.004)
Mean of the dependent variable	3.21	0.96	0.48	2.61
Panel 4j: Conditional fixed-effects Poisson regressions				
Unemployment rate	-0.029***	-0.039***	-0.041***	-0.027***
	(0.004)	(0.008)	(0.009)	(0.005)
Mean of the dependent variable	189.21	55.41	28.21	153.05
Panel 4k: Conditional fixed-effects Poisson regressions using the alternative "at work" definition of work-related fatalities				
Unemployment rate			-0.059**	-0.015
			(0.026)	(0.016)
Mean of the dependent variable			3.81	168.03

Notes: Dependent variable is the natural logarithm of fatalities per capita (per 100,000 people) in panels a-h, the natural logarithm of fatal traffic crashes per capita (per 100,000 people) in panel i, and fatality counts in panels j and k. Each cell is from a separate linear regression with year, quarter, and state fixed-effects as well as all the other variables listed in Table 2. Estimated coefficients are based on regressions weighted by population in panels a and c-i. Estimated coefficients are based on Poisson regressions that condition on year, quarter, and state fixed-effects in panels j and k. Standard errors are in parentheses and are clustered to allow for non-independence of observations within each state. *, **, *** Significance at the 10, 5, and 1 percent level, respectively.

Table 5: Estimates of linear fixed-effects models for vehicle miles traveled (VMT), 2004Q1-2012Q4 (N=1,800)

Dependent variable:	**Ln(VMT)**	**Ln(VMT per capita)**	**Ln(VMT per licensed driver)**
	(1)	(2)	(3)
Unemployment rate	-0.012***	-0.010***	-0.013***
	(0.003)	(0.003)	(0.003)
Speed limit on rural interstates≥70mph	0.006	0.007	-0.032
	(0.012)	(0.009)	(0.028)
Seat belt law - Primary enforcement	0.005	0.003	0.020
	(0.015)	(0.012)	(0.014)
Handheld ban - Primary enforcement	0.006	0.004	0.013
	(0.011)	(0.012)	(0.013)
Texting ban - Primary enforcement	0.008	0.017	0.009
	(0.012)	(0.012)	(0.013)
Light trucks as a proportion of all registered motor vehicles	-0.001	-0.001	-0.001*
	(0.001)	(0.001)	(0.001)
Proportion of young drivers	-0.003	-0.000	0.009***
	(0.004)	(0.005)	(0.003)
Ln(Alcohol consumption per capita)	-0.253*	0.036	0.048
	(0.127)	(0.119)	(0.150)

Notes: Dependent variable is the natural logarithm of vehicle miles traveled (VMT) in specification (1), the natural logarithm of VMT per capita (per 100,000 people) in specification (2), and the natural logarithm of VMT per licensed driver in specification (3). Each linear regression model also includes year, quarter, and state fixed-effects and all regressions are weighted by population. Standard errors are in parentheses and are clustered to allow for non-independence of observations within each state. *, **, *** Significance at the 10, 5, and 1 percent level, respectively.

Appendix Table A1: Variable definitions and data sources

Variable	Definition	Sources
Dependent variables		
1 Traffic fatalities	Quarterly fatalities in motor vehicle traffic crashes including drivers/operators as well as passengers and non-motorists such as pedestrians.	U.S. Department of Transportation, National Highway Traffic Safety Administration (NHTSA), Fatality Analysis Reporting System (FARS)*
2 No-alcohol-involved fatalities	Quarterly fatalities in traffic crashes that involve no drivers with positive blood alcohol concentration (BAC=.00).	
3 Alcohol-involved fatalities	Quarterly fatalities in traffic crashes that involve at least one driver with positive blood alcohol concentration (BAC≥.01).	
4 Alcohol-impaired fatalities	Quarterly fatalities in traffic crashes that involve at least one driver with blood alcohol concentration at or over the legal limit (BAC≥.08).	
5 Work-related fatalities	Quarterly fatalities in traffic crashes that involve at least one motor vehicle whose registered owner is either a business, company, or government. (Alternative definition: Quarterly fatalities in traffic crashes that involve at least one driver who was "at work" at the time of the crash.)	
6 Non-work-related fatalities	Quarterly fatalities in traffic crashes that did not involve any motor vehicles registered as business, company, or government vehicle. (Alternative definition: Quarterly fatalities in traffic crashes that did not involve any driver who was "at work" at the time of the crash.)	
Independent variables		
7 Unemployment rate (%)	Quarterly state unemployment rate as a percent of the labor force.	U.S. Bureau of Labor Statistics
8 Speed limit on rural interstates≥70mph	Equals one if a state had a speed limit of 70 mph or greater on rural interstates in a given quarter and zero otherwise. Takes on fractional values for the quarters in which laws changed.	Insurance Institute for Highway Safety (IIHS)
9 Seat belt law - Primary enforcement	Equals one if a state had a primary enforcement of seat belt law in a given quarter and zero otherwise. Takes on fractional values for the quarters in which laws changed. Primary enforcement allows law enforcement officers to issue a ticket to drivers or passengers for not wearing a seat belt, without any other traffic offense taking place.	

Appendix Table A1, continued

	Variable	Definition	Sources
10	Handheld ban - Primary enforcement	Equals one if a state had a primary enforcement of handheld ban in a given quarter and zero otherwise. Takes on fractional values for the quarters in which laws changed.	French and Gumus (2018)
11	Texting ban - Primary enforcement	Equals one if a state had a primary enforcement of texting ban in a given quarter and zero otherwise. Takes on fractional values for the quarters in which laws changed.	French and Gumus (2018)
12	Light trucks as a proportion of all registered motor vehicles (%)	Annual proportion of light trucks among all registered motor vehicles. Light trucks include pickups, panels, delivery vans, personal passenger vans, passenger minivans, and utility-type vehicles.	U.S. Bureau of Economic Analysis
13	Proportion of young drivers (%)	Annual number of young drivers (age 24 and under) as a percent of the total number of drivers.	U.S. Department of Transportation, Federal Highway Administration (FHWA)
14	Vehicle miles traveled per licensed driver	Quarterly total vehicle miles traveled (VMT) per licensed driver (1,000s).	U.S. Department of Transportation, Federal Highway Administration (FHWA)
15	Alcohol consumption per capita	Annual gallons of ethanol content of alcohol consumption per capita age 21 and older.	Haughwout, LaVallee, and Castle (2016)
16	Real personal income per capita	Quarterly personal income per capita seasonally adjusted at annual rates in constant 2012 $1,000.	U.S. Bureau of Economic Analysis
17	Population	Annual estimates of the July 1 state resident population in 1,000s.	U.S. Census Bureau

Notes: *FARS also captures information on whether the death certificate indicated a person was "at work" at the time of the crash. However, this data element was recorded only if this person died and not if it was a non-fatal injury. Therefore, at work status is missing for the majority of individuals (about 60%) and only about 1% of people are deemed at work. As such, at work status is severely underreported and thus we decided not to utilize this data element.

Sources: Michael T. French and Gulcin Gumus. 2018. “Watch for Motorcycles! The Effects of Texting and Handheld Bans on Motorcyclist Fatalities,” *Social Science & Medicine* , 216, pp.81-87.

Sarah P. Haughwout, Robin A. LaVallee, I-Jen P. Castle. 2016. "Apparent Per Capita Alcohol Consumption: National, State, and Regional Trends, 1977–2014," Surveillance Report #104. Bethesda, MD: NIAAA, Division of Epidemiology and Prevention Research, Alcohol Epidemiologic Data System.