Exogenous Stress in Auditing

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Abstract We examine whether audit staff's exogenous, non-work stress affects audit quality. Empirically studying the effect of stress on the audit process is challenging because stress is an unobservable mental state. We exploit traffic accidents as a natural exogenous, non-work stressor to overcome this challenge. Psychology literature on commuting suggests that exogenous commuting stress affects work performance through depleted cognitive resources. It differs fundamentally from work-related stressors, especially auditing-inherent time pressure. Using a rich dataset of all accidents in the U.S. from 2016 to 2021, we find evidence consistent with an association of traffic-induced stress among audit staff and audit quality (i.e., higher discretionary accruals, higher restatement likelihood, and higher likelihood of receiving a comment letter). We provide a battery of placebo, robustness, and cross-sectional analyses to substantiate the validity of our assumption and results. Our findings contribute to understanding audit quality threats originating outside of audit tasks.

JEL classification: M41, M42, R41

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Introduction

People are the most important input factor into the audit process and ensure a high-quality audit (Francis 2011; IAASB 2014). Yet, over the past decade, evidence has been mounting that stress threatens auditors' well-being (Lyon 2023; Collins and Killough 1992; Deveau 2023). Auditors, from staff to partners, agree that stress is one of the most damaging aspects of being an auditor (Hermanson, Houston, Stefaniak, and Wilkins 2016). However, while anecdotal evidence suggests that stress impacts auditors' performance (Flood 2023; Peart 2019; World Health Organization 2020), empirical evidence of the impact of stress on audit outcomes is limited and has so far focused on task-related stress. In this study, we investigate the relationship between audit staff's exposure to exogenous, non-work stressors and audit outcomes. Specifically, we examine whether traffic-induced stress weakens audit quality.

A primary challenge in empirically examining audit staff's stress is that stress is an unobservable mental state. Psychological studies frequently use laboratory and, to a lesser extent, natural stressors to induce stress and study stress responses (Starcke and Brand 2012).[1] Finding a suitable natural exogenous stressor is difficult, particularly in the audit setting. Thus far, the audit literature has primarily examined endogenous, task-related stressors, especially time pressure (e.g., Bennett and Hatfield 2017; Lambert, Jones, Brazel, and Showalter 2017).[2] The results from this literature establish an important relation of task-related stressors with audit outcomes. Our empirical approach complements this literature by adding stressors originating from outside work and by addressing two limitations it faces.

Firstly, although time pressure may cause stress, 'in scientific terms, stress is not equivalent to time pressure' (Lupien, Maheu, Tu, Fiocco, and Schramek 2007, 210). A situation induces stress when it is novel, unpredictable, or uncontrollable (Koolhaas et al. 2011; Lupien

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[1] We define stress as a mental strain caused by a challenging situation (World Health Organization 2023). A stressor is the external cause of the mental strain (Hermans, Henckens, Joëls, and Fernández 2014).
[2] A notable exception is early survey research that studies the effects of auditors' self-assessed job-related stress and task-related mental stress (Choo 1986, 1995; Low and Tan 2011).

et al. 2007). Yet, heavy workloads and deadline pressures are constant companions for auditors (e.g., Bennett and Hatfield 2017; Christensen, Newton, and Wilkins 2021; Heo, Kwon, and Tan 2021).[3] Annelin and Svanström (2022) report that some auditors even thrive under this planned time pressure. Hence, endogenous potential stressors in auditing may not always cause significant stress. On the other hand, time pressure can also be unpredictable and uncontrollable and cause significant stress, e.g., due to unexpected client demands. However, and secondly, expected and unexpected elements of time pressure are naturally entangled and correlate with auditors' planning decisions, making it difficult to cleanly identify the relation between stress and audit outcomes. To overcome these limitations, we exploit traffic accidents on audit staff's daily commutes during audit fieldwork as a largely unpredictable exogenous stressor. In additional tests, we also control for recurring local traffic congestion to further disentangle more predictable from unpredictable non-work stressors.

Commuting to work is one of the most widespread sources of stress for working adults. Every day, 129 million U.S. workers (83.8%) commute to work by car, spending an average of 26.9 minutes on their one-way commute (U.S. Census Bureau 2020). An extensive body of research has established that traffic-induced stress affects workers' quality of life (e.g., Novaco, Stokols, and Milanesi 1990; Stokols, Novaco, Stokols, and Campbell 1978; Wiese, Chaillié, Noppeney, and Stertz 2020). Accidents, in particular, cause stress among commuters because their occurrence is unpredictable and their consequences uncontrollable (Novaco et al. 1990; Higgins, Sweet, and Kanaroglou 2018). Such stressors during the commute deplete commuters' cognitive and self-regulation resources, potentially having a lasting and likely unconscious impact on work performance beyond the commute (e.g., Zhou et al. 2017).[4] Thus, variation in

[3] Other papers that address time and budget pressure include Beardsley, Lassila, and Omer (2019), Choo (1995), Czerney, Jang, and Omer (2019), Ettredge, Fuerherm, and Li (2014), Glover (1997), Kelley and Margheim (1990), Lambert, Jones, Brazel, and Showalter (2017), López and Peters (2012), Low and Tan (2011), and McDaniel (1990). For a detailed review of some of the time pressure literature, see Pietsch and Messier (2017).
[4] While accidents may mechanically delay the start of the workday, time pressure plays a subordinate role in creating traffic-induced stress compared to commuting quality and unpredictability (Wiener et al. 2024). We observe patterns

traffic congestion from accidents has two desirable features. First, exposure to this potential stressor likely causes a strong stress response because individual accidents are novel, unpredictable, and uncontrollable events.[5] Second, it provides us with plausibly exogenous variation in audit staff's stress exposure, helping us to cleanly identify the association of non-work stressors with audit quality.

We predict a negative effect of traffic-induced stress on audit quality. Our prediction follows from the audit staff's vulnerability and exposure to traffic-induced stressors. Their vulnerability arises because auditing involves complex judgment and decision-making (Nelson and Tan 2005) and relies on teamwork (Bamber 1983). Next to generally depleting cognitive resources, stress impairs judgment and decision-making through hurried decisions, increased risk-taking, and impaired executive functions (Starcke and Brand 2016).[6] Teamwork deteriorates under stress due to impaired information exchange (Ellis 2006; Dietz et al. 2017). Time pressure of audits further exacerbates this vulnerability because it reinforces the stress response to exogenous stressors (Hennessy 2008). Audit staff's exposure to traffic is high because they commute regularly and longer distances than the average U.S. worker (Hermanson, Hill, and Ivancevich 2009). To summarise, traffic-induced stress is an important stress source in the auditing setting, and audit staff may react to such stress by adapting their behavior, for instance, by working less productive long hours or cutting corners (Heo et al. 2021). Ultimately, those reactions to traffic-induced stressors likely threaten audit quality.

In our primary analysis, we measure traffic-induced stress as the mean daily number of severe accidents in a 40-km radius around a client's headquarters in a 40-day window before

that are consistent with this argument. Back-of-the-envelope calculations in Appendix D suggest that accidents plausibly deplete audit teams' cognitive and self-regulation resources for around 18 percent of fieldwork days, while reducing working time by just 1.35 percent. Cross-sectional tests in Table 6, Panel B, suggest that the variability of accidents, affecting resource depletion but not lost time, drives our results. Finally, Table 7, Panel A, shows that expectable time delays from traffic (*DELAYHOURS*) are not associated with our audit quality proxies. Hence, we conclude that lost time is unlikely to be a substantial factor in our empirical setting.

[5] We cannot observe this stress response, so our causal chain from stressors to outcomes of being stressed is necessarily indirect.

[6] Executive functions are mental processes that govern, e.g., focus or thinking before acting (Diamond 2013).

the auditor's signature.[7] Furthermore, we require that audit offices are no more than 100 km away from client headquarters, at least 50 percent of commuters in the headquarters' county commute by car, and audits are signed before March 31, 2020, when COVID-19 lead to primarily remote audits.

Our primary results suggest a negative association of our traffic-induced stress measure with audit quality. We triangulate our results across three proxies for audit quality. First, absolute discretionary accruals as a continuous measure of clients' earnings quality allow us to observe more nuanced quality differences than binary audit quality proxies. The frequency of traffic accidents around the client's headquarters is associated with larger absolute discretionary accruals. Second, we use restatements, a clear but rare signal of an audit failure, and find that the frequency of traffic accidents around the client's headquarters is associated with an increased restatement likelihood. Finally, we estimate the association of traffic-induced stress with receiving a securities and exchange commission (SEC) comment letter because comment letters likely reflect erroneous or imprecise audited financial statements. They are another binary proxy for less egregious audit failures. Again, we find that the frequency of traffic accidents around the client's headquarters is associated with a higher likelihood of receiving an SEC comment letter. Overall, these results are consistent with our prediction that the audit staff's stress weakens audit quality and hold in a battery of robustness tests.

One possible concern of our stress measure is that it might also capture the traffic-induced stress of client personnel. Audits are produced in collaborative financial reporting ecosystems (Knechel, Thomas, and Driskill 2020). Other participants in a financial reporting ecosystem, especially client personnel, may also suffer from traffic-induced stress. We perform a placebo

[7] Accident broadcasters directly supply severity scores, so we do not know their exact definition. They combine congestion length, expected delay, and duration of cleaning the accident scene (see https://www.kaggle.com/datasets/sobhanmoosavi/us-accidents/discussion/286160). We use scores 3 and 4 out of 4, which cover 20 percent of all accidents. Even if severity scores are noisy, we expect them to improve the identification of significant traffic congestion. Appendix C shows that we find weaker but similar results when using all accidents or when using self-defined severity based on congestion length or duration.

test to alleviate this concern and isolate the association of audit staff's exposure to traffic-induced stress with audit quality as a joint outcome of this ecosystem. Specifically, we repeat our primary analysis on a sample of clients audited by audit offices more than 100 km away from the client headquarters or located outside the U.S. In these cases, audit staff are unlikely to live within commuting distance to the client. However, client personnel will still suffer from the traffic-induced stress our measure covers. We do not find significant effects of our traffic measure in these placebo tests, giving us confidence that the association of traffic-induced stress with audit quality is primarily driven by the commute of audit staff, not client personnel.

We next conduct several additional tests to substantiate the validity of several assumptions behind our theoretical prediction and its operationalization. A key assumption we make is that accidents cause stress in individuals and do not cause other disruptions that eventually correlate with our audit quality proxies. To empirically examine this assumption, we analyze two situations where the link between accidents and auditor commuting is absent, while keeping as many environmental conditions as possible similar to our primary setting. Specifically, we use two placebo tests that repeat our primary analysis on alternative samples for which we can construct our measure, but car commuting is less relevant for audits. First, we use a sample of clients headquartered in counties with less than 50 percent car commuters. In this sample, we expect most audit staff to use other means of transport, thus making them less susceptible to stress from road congestion. The second test uses audits signed after March 31, 2020, when commuting became largely irrelevant due to remote auditing during the COVID-19 pandemic. In both placebo tests, we find no significant association of our accident measure with our audit quality proxies, consistent with our assumption that audit staff's stress response is the critical link between our accident measure and audit quality. Hence, any possible alternative explanation would have to plausibly link accidents to audit quality only for counties with more than 50 percent car commuters and only without pandemic-related remote audits.

A second assumption we make is that audit staff's stress response substantially affects their work, and that professionalism and internal quality control are insufficient to alleviate possible consequences of stress-impaired procedures.[8] We conduct a cross-sectional analysis to substantiate the validity of this assumption. Specifically, we split clients by whether their fiscal year ends in December, the busy season during which audit staff faces the most deadline pressure. Our theory predicts exogenous stress to be exacerbated when time pressure is high. Therefore, substantial stress responses to exogenous stressors are more likely in the busy season. Moreover, the high deadline pressure and accumulation of parallel engagements make it more plausible that internal quality control cannot fully correct for stress effects on audit quality. Our evidence is consistent with our theory because our results only hold for December year-end clients. Hence, possible alternative explanations would have to plausibly hold only for busy season audits.

Thirdly, we assume that audit staff do not internalise the occurrence of accidents to the point where they become essentially expected events. We address this assumption with a second and third cross-sectional analysis. Specifically, we split our sample by the median of the standard deviation of the daily number of severe accidents we observe and by more versus less urban areas. We conjecture that accidents that happen with higher day-to-day variation are less predictable and that auditors in less urban areas are less likely to be used to constant traffic congestion, both reducing the likelihood that audit staff internalise their occurrence. Results substantiate the validity of our assumption. Hence, a possible alternative explanation would have to be moderated by the local variability of accidents and local urbanicity.

Finally, we perform several robustness tests. First, Hao and Pham (2024), using three measures from the Texas A&M Urban Mobility Report, suggest that regular rush-hour

[8] Any on-site quality control has the same exogenous stress exposure, theoretically leading to impaired performance. Moreover, the impairment largely happens subconsciously. Hence, individuals performing quality control procedures may not be aware of their possibly reduced control effectiveness.

congestion affects audit fees as a client business risk determinant. Importantly, one of their additional tests suggests that regular rush-hour congestion is also associated with audit quality proxies. Our research fundamentally differs from Hao and Pham (2024) conceptually and empirically. They use relatively time-invariant and expectable traffic measures to proxy for a client's local economic conditions, whereas we use a more volatile, unforeseeable measure to capture variations in exogenous stressors. Our research design aims to hold local economic conditions constant by using granular geographic fixed effects. Consistent with the fundamental conceptual and empirical differences in our and Hao and Pham's (2024) approach, we find that our results hold after controlling for their variables of interest, which are not incrementally associated with audit quality proxies. Our results are also robust to client fixed effects, audit office-by-busy season fixed effects, census data controls, and several subsamples.

We make the following contributions to the literature. First, we introduce exogenous, non-work stress as a prevalent potential audit quality threat to the auditing literature. Thus far, the literature has primarily been concerned with workload and time or fee pressure (Christensen et al. 2021; Heo et al. 2021; e.g., Bennett and Hatfield 2017; Ettredge, Fuerherm, and Li 2014). We complement this research on task-inherent stressors with evidence that stressors originating from outside the task are likewise associated with audit outcomes. Moreover, workload and time pressure can be both planned and acute. As our empirical approach can disentangle acute from recurring non-work stressors, it helps triangulate established negative associations between task-inherent stressors and audit quality by allowing for a more explicit investigation of acute and unpredictable stressors versus planned or expected stressors. Relatedly, Starcke and Brand (2012) emphasise that acute stress can have markedly different effects on decision-making compared to time pressure. Hence, our results introduce exogenous, non-work stressors as a potential complementary threat to the audit process.

These contributions come with an important caveat. Our theory relies on a long causal chain from traffic accidents as stressors, to auditors developing stress that carries over to their tasks, to the stress affecting their judgment, to judgment errors passing several layers of review and manifesting as an observable audit quality weakness. While this causal chain is based on strong theoretical arguments and we collect corroborating evidence on the validity of many of our assumptions, we ultimately only observe its very first and very last step, making our collective evidence indirect.

Second, we contribute to nascent auditing literature on the effects of individual auditor behavior on the audit process using society-level data exogenous to the economic incentives of auditing. This literature conjectures that, even if the individual audit team members are equally capable and motivated to do their job, they are not immune to environmental influences restricting their ability to realise their full potential. A recent example is Morris and Hoitash (2023), who show that more significant influenza outbreaks in U.S. states are associated with lower audit quality, longer delays, and higher audit fees. They propose working sick as the underlying mechanism. Similarly, Chen, Michas, Russomanno, and Zhuang (2024) find that air pollution at client headquarters increases restatements. The authors attribute this effect to the health of client personnel and audit staff. We focus on immediate and largely unpredictable exogenous shocks to the audit process. Severe accidents are less predictable than time pressure and influenza outbreaks. In a study conceptually related to ours, Seavey and Thevenot (2022) use the number of (or people involved in) fatal accidents in an audit office's county-year to infer the traffic risks auditors face. They show that traffic risk increases audit fees and audit delays. As with the influenza season, they infer effects on individual audit staff from measures at an audit-office-year level. In our case, accidents around client sites in a focal 40-day period constitute engagement-level variation.[9]

[9] Chen, Michas, Russomanno, and Zhuang (2024) similarly leverage engagement-level variation.

Third, we contribute to the broader literature on the economic impacts of traffic congestion and the effects of stress on judgment and decision-making. For the former, we provide novel evidence of spillovers of traffic congestion to the workplace in a setting with a theoretically high propensity of being affected by stress. This evidence suggests a potentially underappreciated negative welfare outcome of congested roads, as financial reporting and auditing are critical for the functioning of financial markets. Therefore, detriments to the financial reporting network may have substantial adverse welfare effects. For the latter, we provide initial evidence from the field consistent with experimentally established mechanisms between stress and decision-making tasks manifesting on a large scale in a highly regulated and highly professionalised working environment.

Understanding exogenous, non-work stressors has important practical implications, as clients and auditors should share an interest in mitigating potential harm that originates outside of work and spills over to audit tasks. Auditors may have some control over managing exogenous, non-work stressors because they can consider staff's individual situations when managing audit teams.[10] On the contrary, they cannot remove fundamental task-inherent features of year-end audits, such as time and budget constraints, and have little control over unexpected client demands contributing to more acute time pressure. Regarding traffic-induced stress, the auditing profession specifically debates to what degree audit staff can work remotely (Ovaska and Murphy 2022; Taylor 2021). Avoiding traffic-related stress could be one aspect when weighing costs and benefits of remote work. Other options to address exogenous stressors could include following calls from health scientists for organisations to allow alternative work arrangements or flextime, helping employees reduce stressful commutes (Wiese et al. 2024, De Reuver and Biron 2024).

Background and Hypothesis Development

[10] Next to commuting, other non-work sources of stress could include family issues, mental health issues, or social and urban stress. Qualitative evidence in Annelin and Svanström (2022) suggest that auditors consider these as relevant sources of stress.

Following the World Health Organization (2023), we define stress as a mental strain caused by a challenging situation. A situation induces stress when it is novel, unpredictable, or uncontrollable (Koolhaas et al. 2011; Lupien et al. 2007). Commuting research has established a strong relationship between commuting and stress (e.g., Stokols et al. 1978; Novaco et al. 1990; Wiese et al. 2020). Stressors during a commute can cause physiological and psychological strain that plausibly affect performance and conduct at work (see the models in Koslowsky et al. 1995 and Koslowsky 1997). The spillover from unresolved strain during the commute to work persists after the commute, even when unconscious, and accumulates when interacting with other non-work or work-related stressors, such as time pressure (Hennesy and Jakubowski 2008; Calderwood and Mitropoulos 2021).[11] A key psychological mechanism behind this spillover is that dealing with stressors during the commute depletes commuters of energy and self-regulation resources (e.g., Zhou et al. 2017). Several papers empirically substantiate this mechanism by showing that commuting leads to physiological stress responses, such as elevated blood pressure, and worse performance in cognitively demanding tasks post commute and actual work performance (Schaeffer et al. 1988; Hennessy and Jakubowski 2008; Ma and Ye 2019; Nepal et al. 2021).

Significant contributors to commuting stress include traffic congestion, unpredictability, uncontrollability, and variability (Novaco et al. 1990; Higgins et al. 2018; Kluger 1998). Hence, unforeseeable short-term variation in traffic congestion due to severe accidents likely induces elevated stress levels.[12] Moreover, this traffic-induced stress is relatively acute due to its short-term and unforeseen nature compared to, for instance, daily rush hours. Empirical

[11] Commuting stress spilling over to the workplace is consistent with theoretical frameworks on job strain: the job demands-control model (Karasek, 1979); the conservation of resources theory (Hobfoll 1989); the effort-recovery model (Meijman and Mulder 1998); and theory on self-regulation (Muraven and Baumeister 2000).

[12] Consider an audit team member commuting to a client's headquarters for audit fieldwork. If she drives into severe traffic congestion following an accident, her commute will be *more congested*; she can *neither predict nor control* when she will arrive at the client. Her commute will be *more variable* if this happens multiple times during the audit engagement. Hence, the situation fulfills all elements of elevated traffic-induced stress.

evidence from diary studies suggests that such acute traffic-induced stress matters for work performance. Those studies analyze self-reported commuting experiences and find that work performance suffers from commutes with unusual interruptions, shifting from routine and automated behaviors to cognitive strain. This mental strain results from additional decision-making (e.g., considering alternative routes, rescheduling) or self-control (e.g., abstaining from speeding; Zhou et al. 2017; Wiese et al. 2020; McClean et al. 2021; Gerpott et al. 2022).

Four core characteristics of audit fieldwork make auditing a setting with particular vulnerability to traffic-induced stress,[13] beyond the general vulnerability of work performance to depleted energy and self-control.[14] First, many audit tasks require complex judgment and decision-making. The psychology literature identifies three mechanisms that yield adverse effects of acute stress on decision-making. Stress can (a) increase risk-taking and reward-seeking, (b) prompt hurried and unsystematic decision-making, and (c) impair executive functions (Starcke and Brand 2016). Second, high-quality audits rely on the audit staff's professional skepticism. Stressed individuals may become more obedient to the opinions of others. Obedience to the client's opinion threatens audit quality because it undermines professional skepticism when evaluating client explanations. Third, teamwork characterises many audit tasks, and team performance suffers under acute stress, for instance, due to impaired information exchange (Ellis 2006; Dietz et al. 2017). Moreover, a single stressed individual can affect the stress levels of the entire team because stress is contagious (Erkens, Nater, Hennig, and Häusser 2019; see also Annelin and Svanström 2022 for team stress dynamics in auditing). Fourth, audit staff under constant time pressure may experience traffic-induced stress

[13] These characteristics also have drawbacks because complex decision-making, skepticism, and team dynamics introduce many complex drivers of audit quality that are difficult to control for empirically. However, throughout the paper, we use a battery of controls, fixed effects, and additional tests to reduce this concern as much as possible and discuss how an omitted variable would have to correlate with accidents to explain our results.

[14] To the extent that audit staff interacts with client personnel to collect audit evidence, we consider client personnel's stress as part of audit quality because it directly affects the audit process (Knechel, Thomas, and Driskill 2020; Friedrich, Knechel, Sofla, and Zuiddam 2023). Our stress measure primarily covers days after fiscal year-end, when most of the work preparing the financial statements should be finished. Hence, a substantial proportion of client personnel's stress it may measure in addition to audit staff's stress captures our concept of interest.

particularly strongly because time pressure and stress reinforce each other (Hennessy 2008). Taken together, traffic-induced stress likely reduces the quality of affected audit teams' fieldwork.[15] Therefore, we posit the following hypothesis:

H1: Traffic-induced stress negatively affects audit quality.

Data and Models

Sample Selection

We start our sample selection with 22,588 non-financial industry observations from the intersection of Compustat and the Audit Analytics Opinion data set with signature dates at least 40 days after August 1, 2016, and ending after the fiscal year 2021. The availability of our traffic-induced stress variables (see next section) restricts our starting date, and 2021 is the last year with necessary census data on commuting behavior. We exclude 2,615 observations with missing headquarters location data[16] and 7,429 observations with missing data for control variables. We only keep observations with financial year-end and signature date between 0 and 250 days apart (minus 39 observations), with more than one audit firm per city and SIC 2-digit industry combination (minus 3 observations) and with positive sales (minus 85 observations). Our traffic data only covers the contiguous United States (minus 51 observations). We further restrict our sample to counties with more than 50 percent car commuters (minus 769 observations) and audit offices in the U.S. that are no more than 100 km away from the client's headquarters (minus 2,550 observations) to increase the likelihood that audit staff commutes by car. We remove 2,997 firms with signature dates after March 31, 2020, from our primary sample because the 40-day window to construct our stress variables for those firms overlaps with stay-at-home orders of the COVID-19 pandemic introduced in most U.S. states in March

[15] Elevated time constraints are a complementary mechanism by which traffic-induced stress could affect audit quality. However, as we discuss in more detail in Appendix D, this complementary mechanism of lost time due to traffic-induced stress likely plays a second-order role in our empirical setting.

[16] We take headquarter locations from the University of Notre Dame's Software Repository for Accounting and Finance available at https://sraf.nd.edu/sec-edgar-data/.

2020. Finally, we remove 10 singletons from our primary specifications. Our final sample size is 6,040 (5,713 when requiring additional data to calculate discretionary accruals as our dependent variable). Table 1 summarises our sample selection.[17]

Stress Variables

To construct a measure of traffic-induced stress, we use U.S. traffic event data from August 2016 to March 2023 collected by Moosavi, Samavatian, Parthasarathy, and Ramnath (2019) and Moosavi, Samavatian, Parthasarathy, Teodorescu, and Ramnath (2019).[18] The authors collected the data from multiple APIs that broadcast traffic events reported by various entities such as U.S. state departments of transportation, law enforcement agencies, and traffic cameras. The dataset covers 7.7 million accidents in the contiguous United States. For each accident, the dataset contains exact start and end time stamps, location data, and information on the severity of resulting traffic congestion.

We measure our stress variables at the client-year level and consider all severe accidents (severity score 3 or 4 out of 4) in the 40-day window before the audit opinion's signature date for a given client. We focus on accidents within a 40-km radius around the client headquarters. We measure traffic-induced stress as the daily mean of the number of accidents that fulfil our selection criteria (*ACCIDENTS*). Our analysis uses the natural logarithm of 1+*ACCIDENTS* (*LN_ACCIDENTS*) because *ACCIDENTS* is highly skewed. However, our results are robust to this choice (see Appendix C).

[17] In untabulated analysis, we validate that our sample is representative of the population of U.S. public firms, except for New York because we exclude New York City for its public transport system. It is also representative in terms of industries, except for chemical and allied products (SIC code 28), which make up 14.3 percent of our sample compared to 21.1 percent of all U.S. public firm years in our sample period. We also find that firm years in our sample compared to all U.S. public firm years are substantially larger (6.851 versus 5.823 billion USD total assets), more profitable (35.5 versus 52.4 percent loss years), and concentrated among Big 4 clients (71.4 versus 59.4 percent), which likely results from requiring data for all control variables. Crucially, restatement rates do not substantially differ between our sample and all U.S. public firm years.

[18] We thank Sobhan Moosavi for making the data publicly available on https://www.kaggle.com/datasets/sobhanmoosavi/us-accidents. Data coverage starts in February 2016. We do not use the data from February to July 2016 because there is an apparent ramp-up in the number of covered accidents that does not follow the seasonal patterns in later years.

We focus on the client headquarters because we assume that many audit engagement team members commute to the client headquarters by car. Survey evidence suggests that U.S. staff auditors work about 80 percent of the time in their home region and commute about 40 minutes on average (Hermanson et al. 2009). As discussed in our sample selection, we exclude all counties from our analysis where U.S. census data suggests that less than 50 percent of workers commute by car, thus increasing the likelihood of auditors using a car for their commute.[19] We focus on severe accidents because many minor accidents may have no or only little impact on traffic congestion, e.g., because damaged cars can move to the shoulder of the road. They would add significant noise to our variable. We rely on expert judgments on accident severity included in the dataset that reflect the severity of the delay and disruption caused by the accident on a 4-point scale. While these ratings are judgmental and we have no further information to judge their quality, they have distinct advantages over creating an own definition of severity. For the latter, we only have start and end locations and timestamps, but end locations are only available for a subset and do not contain information on the severity of the congestion. Timestamps do not reflect whether and when exactly there was a congestion. In Appendix C, we further discuss these choices and explore results when ignoring the severity or using accident timing and congestion length to infer severity.

We use a 40-km radius to ensure that the affected roads are part of the commute of a sufficiently large proportion of audit staff but avoid capturing irrelevant traffic. Survey evidence indicates that auditors within one standard deviation of mean commute time travel between 20 and 60 minutes (Hermanson et al. 2009). The midpoint, 40 minutes, allows covering a distance of 40 kilometers at an average speed of 60 kilometers an hour (or 37.3 miles per hour, similar to average speed estimates; Ride to Work 2020). Hence, choosing less

[19] Note that we do not require all audit staff to be affected by traffic-induced stress. Instead, the stress of some team members likely already decreases team performance. Although home location and commuting times among team members differ, our data allow us to capture a given audit team's average traffic congestion exposure relative to other audit teams.

than 40 kilometers is likely to miss substantial portions of relevant accidents. Appendix C contains results when we choose a 100-km radius instead.

Finally, we calculate our variable of interest for all weekdays in the 40-day window before the signing date of each auditor's reports. We chose a 40-day window because we expect the highest likelihood that audit staff will conduct fieldwork at the client's headquarters during this period. Descriptive evidence from Heo et al. (2021) suggests that end-of-year fieldwork is completed within no more than 40 days. Although their sample is from Korea, it is reasonable to expect that most U.S. audits will not require considerably more end-of-year fieldwork than the maximum of the firms studied in Heo et al. (2021). We choose the maximum to encompass as much end-of-year fieldwork as possible and, if end-of-year fieldwork is shorter, include some interim fieldwork instead. Including all hours of every day avoids assumptions about when auditors commute.[20] Appendix C presents evidence for restricting the time window to the period between the fiscal year-end and the signature date (thus making sure all end-of-year fieldwork is fully included, on a client-individual level), using a 20-day window, and removing night times. We choose a uniform 40-days window over the fiscal year-end to signature date window in our primary specification to smooth possibly bumpy distributions of accidents over time when time windows are of heterogeneous length and may include very short windows.

Models

We estimate equation (1) below, where our dependent variable is either *DA*, performance-adjusted modified Jones (1991) discretionary accruals (Kothari, Leone, and Wasley 2005, see Appendix B for details), *RESTATE*, a dummy representing whether the financial statements were later restated, or *CL*, a dummy representing whether the client's 10-K later received a comment letter from the SEC. Survey evidence suggests that auditors perceive comment letters as the

[20] As Figure 2 in Moosavi, Samavatian, Parthasarathy, Teodorescu, and Ramnath (2019) shows, the pattern of accidents during the day follows rush hours. Hence, although we avoid assumptions on when auditors commute, the data naturally weights typical commute times more highly than off-peak hours.

second-best observable audit quality indicator directly after restatements (Christensen, Glover, Omer, and Shelley 2016). In our setting specifically, comment letters might be advantageous because they are a much more frequent event than restatements, increasing the identifiability of more nuanced audit quality differences. Moreover, Baugh and Schmardebeck (2023) find evidence that comment letters are associated with auditors' decision errors, and stress may increase the prevalence of common decision errors.

$$DA/RESTATE/CL_{i,t} = \alpha_{i,t} + \gamma_i + \delta_i + \tau_t + \beta_1 LN\_ACCIDENTS_{i,t} + \beta_2 SIZE_{i,t} + \beta_3 FOREIGN_{i,t} + \beta_4 BUSSEG_{i,t} + \beta_5 GEOSEG_{i,t} + \beta_6 STDCFO_{i,t} + \beta_7 STDREV_{i,t} + \beta_8 GROWTH_{i,t} + \beta_9 RESTRUCTURE_{i,t} + \beta_{10} CFO_{i,t} + \beta_{11} MW_{i,t} + \beta_{12} LATE_{i,t} + \beta_{13} RESTATE_{i,t} + \beta_{14} QUICK_{i,t} + \beta_{15} LEV_{i,t} + \beta_{16} LOSS_{i,t} + \beta_{17} ZSCORE_{i,t} + \beta_{18} GC_{i,t} + \beta_{19} RESTATING_{i,t} + \beta_{20} LN\_BTM_{i,t} + \beta_{21} LIT_{i,t} + \beta_{22} BIG4_{i,t} + \beta_{23} INDSPEC_{i,t} + \beta_{24} AUDCHANGE_{i,t} + \beta_{25} AUDDIST_{i,t} + \beta_{26} DYE_{i,t} + \beta_{27} LACC_{i,t} + \beta_{28} NACC_{i,t} + \varepsilon_{i,t} \quad (1)$$

Where i indicates a client and t indicates a fiscal year, $\alpha_{i,t}$, γ_i, δ_i, τ_t, are fixed effects for the auditor's commuting zone,[21] the client's commuting zone, the client's industry, and the fiscal year (based on Audit Analytics' definition), respectively, and $\varepsilon_{i,t}$ are the residuals. We control for the following client characteristics (e.g., Francis and Yu 2009; Ashbaugh-Skaife, Collins, Kinney, and LaFond 2008). Client *SIZE* is typically positively associated with financial reporting quality. *FOREIGN* operations and many business (*BUSSEG*) or geographical (*GEOSEG*) segments measure the diversification and complexity of a client's business. More diversified operations may be less volatile, resulting in higher financial reporting quality, while complexity may contribute to lower financial reporting quality. To

[21] We use the 2000 Commuting Zones from https://www.ers.usda.gov/media/5688/2000-commuting-zones.xls?v=99988. Commuting zones reflect local labor markets, increasing the likelihood that workers live in the commuting zone of their employer because "county boundaries are not always adequate confines for a local economy and often reflect political boundaries rather than meaningful boundaries for economic activity" (U.S. Department of Agriculture 2019). Hence, audit staff are more likely to commute through their audit office's commuting zone, and client personnel are more likely to commute through a client headquarters' commuting zone. Commuting zone fixed effects also control for urbanicity as a determinant of accident frequency. In Table 7, Panel B, we use client fixed effects to control for any local determinants of accident frequency. In Appendix C, we show the robustness of our results to using counties instead of commuting zones.

control for the volatility of operations directly, we include the five-year rolling standard deviation of cash flows from operation (*STD_CFO*) and revenue (*STD_REV*). Firms may experience lower financial reporting quality during rapid growth, which we capture by the percentage change in revenue (*GROWTH*) or when they *RESTRUCTURE* their operations. High operating cash flows (*CFO*) may reduce the use of accruals and increase financial reporting quality. Financial reporting quality is likely to be lower when firms have material weaknesses in their internal controls (*MW*), when they miss the filing deadline (*LATE*), and when 10-Ks are restated in later periods (*RESTATE*). Firms experiencing liquidity issues or financial distress may have lower financial reporting quality. We measure short-term liquidity with the *QUICK* ratio and the pressure of external financing with debt to total assets (*LEV*). *LOSS* years, low *Z_SCORE* values, and a modified going concern opinion (*GC*) are different signs of financial distress. The financial reporting quality of a firm may also systematically differ in years when restatements of prior periods are announced (*RESTATING*) because firms correct prior financial reporting errors in those years. As a market-based measure for growth opportunities and risk, we use the natural logarithm of the book-to-market ratio (*LN_BTM*), where higher values represent lower risk. Finally, litigation risk (*LIT*) could be associated with lower financial reporting quality.

To account for different auditor characteristics possibly changing audit quality irrespective of auditor exposure to exogenous stress, we control for auditor size (*BIG4*), industry specialist auditors (*INDSPEC*), first-year audit engagements (*AUDCHANGE*), and the physical distance between the audit office and the client's headquarters (*AUDDIST*). We also control for auditors' time pressure, specifically for December financial year-ends (*DYE*), large accelerated filer status (*LACC*), and non-accelerated filer status (*NACC*). *DYE* also controls for seasonal variation of accident rates in the winter. Note that when *RESTATE* is our independent variable, we must remove *RESTATE* from our control variables.

All client data are from Compustat. Data on audits, restatements, and internal control opinions are from Audit Analytics. We construct our comment letter data by downloading all comment letters from the SEC's Electronic Data Gathering, Analysis, and Retrieval (EDGAR) system (File Type = UPLOAD). We use regular expressions to detect whether comment letters mention an Item 10-K or 10-K/A and identify the fiscal year of the referenced 10-K or 10-K/A.

We estimate all our models with ordinary least squares (OLS) regressions. When our dependent variable is a dummy, using OLS as a linear probability model avoids the potential incidental parameter problem of non-linear models.[22] We use heteroskedasticity-robust standard errors in all analyses and cluster them by client firm.[23]

Results

Descriptive Statistics

Table 2 presents summary statistics for our variable of interest, dependent variables, and controls. There are almost 10 severe accidents on an average day around client headquarters in the 40-day window before the signature date. We observe substantial variation, where some client headquarters have no accidents fulfilling our selection criteria, the bottom 25 percent of the sample have less than about 4 severe accidents per day, and the top 25 percent have more than 14 severe accidents. The maximum is over 37 severe accidents, and the standard deviation of just below 8 confirms the considerable variation in our variable of interest. Figure 1 provides additional insights into this variation. It plots the daily mean of severe accidents that survived our selection criteria (*ACCIDENTS*) based on the counties where our sample firms are headquartered. For county-years with multiple firms in our sample, we plot the average of *ACCIDENTS* of all sample observations belonging to that county-year. Due to the geographical

[22] Our inferences do not change if we use logistic regressions instead of OLS.

[23] We consider alternative clusters to account for possible dependence resulting from fixed effects, dependence within industries, and for local dependence of traffic phenomena within commuting zones in a given year (Breuer and deHaan 2024). Specifically, we rerun our primary tests from Table 4 with standard errors clustered by industry, industry-year, client commuting zone, client commuting zone-year, and two ways by client commuting zone-year and industry. Our inferences remain the same.

distribution of the U.S. economy and some of our sample selection criteria (e.g., excluding counties with predominant public transport like New York County), our sample covers only 262 of all 3,143 U.S. counties. However, Figure 1 shows significant geographic and longitudinal variation in *ACCIDENTS* for the represented county years. For instance, consider the included counties on Florida's Atlantic coast. The northeastern counties (around Jacksonville) had the most *ACCIDENTS* in 2017 and 2018, whereas the southeastern counties (around Miami) had the most *ACCIDENTS* in 2016 and 2019. *ACCIDENTS* levels were relatively similar in 2017 and 2018, whereas the southeast had substantially more *ACCIDENTS* in 2016 and 2019. However, Figure 1 also shows a systematic geographic variation of yearly averages of *ACCIDENTS*, underscoring the importance of controlling for geographic fixed effects.

Table 3 presents correlations of the variables of our primary regressions. Correlations are significant and positive between *LN_ACCIDENTS* and *DA*, insignificant between *LN_ACCIDENTS* and *RESTATE*, and significant and positive between *LN_ACCIDENTS* and *CL*. Most correlations are small or moderate, with coefficients below 0.50. Sizeable correlations are between *SIZE, BIG4, LACC,* and *NACC*, which is unsurprising given that *SIZE*, *LACC*, and *NACC* all represent client size, and large clients tend to hire Big 4 auditors. Multicollinearity does not seem to be an issue as variance inflation factors (VIFs) in our main models are below 5, except for *SIZE,* which is below 10.

Multiple Regression Analysis

Table 4 presents the results from OLS regressions of our audit quality models in equation (1). The dependent variables are performance-adjusted modified Jones (1991) discretionary accruals in Model 1, restatements in Model 2, and comment letters in Model 3. In all models, *LN_ACCIDENTS* is significant and positive. To assess economic effects, we use the within-fixed effects standard deviation of *LN_ACCIDENTS*, which is 0.497. In Model 1, increasing

LN_ACCIDENTS by one within-group standard deviation increases *DA* by 0.005 (= 0.010 × 0.497), about 5 percent of average accruals. Hence, we find indications of a slight decrease in accruals-based audit quality when auditors become more stressed. In Model 2, an increase of *LN_ACCIDENTS* by one within-group standard deviation increases the likelihood of a restatement by 0.9 percentage points (= 0.018 × 0.497), which is about an 11 percent increase compared to the unconditional restatement likelihood of 8 percent. This result is consistent with stressed auditors being substantially more likely to miss a misstatement. Finally, the coefficient of 0.023 in Model 3 suggests a 1.1 percentage point (= 0.023 × 0.497) increase in comment letter likelihood following a one within-group standard deviation increase of *LN_ACCIDENTS*, which is about a 7 percent increase compared to the unconditional likelihood of 15.8 percent. Overall, these results are consistent with our hypothesis that the audit staff's traffic-induced stress has a negative effect on audit quality.

Testing the Validity of Key Assumptions

Audit Staff (Not Client Personnel) Causing Audit Quality Effects

To rule out that traffic-induced stress of client personnel (and not audit staff) drives our results, we identify auditor-client pairings where audit staff are less likely to commute to the client office from their homes, but client personnel is equally likely to commute to their work as in our primary sample. Specifically, we focus on auditor-client pairings where client headquarters and audit offices are more than 100 km apart, or audit offices are outside the U.S. Most likely, audit staff for those engagements would stay at a hotel during fieldwork at the client headquarters. We do not expect auditor-client distance to affect the commuting behavior of client personnel. Census data provides some evidence for this expectation. In our primary sample with auditor-client distances below 100 km, census data suggests that 75.1 percent of workers work in the county of their employer. In our placebo sample with auditor-client distances above 100 km, the ratio of in-county workers is 78.1 percent. While this difference is significant (t = -7.239), its magnitude is small. Hence, we expect that the commuting behavior of client personnel in our placebo sample is comparable to that of our primary sample. Table 5, Panel A presents the results of repeating the analysis from Table 4 with this placebo sample. For brevity, we suppress the results of control variables. As expected, there are no significant effects on audit quality. Hence, there is no indication that client personnel's traffic-induced stress drives our primary results.[24]

Accidents Cause Stress Affecting Professional Performance

Next, we conduct two placebo tests to substantiate the validity of our assumption that accidents cause stress in individuals that affects work performance. A concern could be that our accident measure instead reflects or correlates with other characteristics related to the client's headquarters location that correlate with audit quality even if auditors do not develop commuting stress or their stress does not spill over to audit tasks. To address this concern, we create two more samples for which we expect our traffic measure not to capture any traffic-induced stress, neither for audit staff nor client personnel. Hence, if our proposed mechanism holds true, we should not observe significant effects in these samples. The first sample consists of only counties where less than 50 percent of the workforce commutes by car. Therefore, most workers will not be affected by accidents on car roads. However, if our traffic measure would spuriously capture other factors related to audit quality and severe accidents, such as local social, cultural, or economic characteristics, it would still capture those characteristics in this placebo sample if they were unrelated to the commuting mode. Results in Table 5, Panel B, reduce the concern of such a spurious relation because *LN_ACCIDENTS* is insignificant in all Models.

In a second placebo test, we use all observations we previously excluded because they fall into the period after March 2020 that was affected by COVID-19 stay-at-home orders, with many audit staff (and client personnel) working from home. As there is no commuting when

[24] All results in Table 5 are robust to calculating *LN_ACCIDENTS* based on days between the financial year end and the auditor signature date instead of a 40-day window before the auditor signature date (untabulated).

working from home, we again expect that our measure stops capturing stress while still capturing other local characteristics that are less or less swiftly affected by the COVID-19 pandemic. Coefficients on *LN_ACCIDENTS* in Table 5, Panel C, are all insignificant. Hence, we find no evidence that variables correlating with our stress measure but less affected by the COVID-19 pandemic drive our primary results. Overall, these two placebo tests are consistent with our proposed mechanism and reduce concerns that the accidents we capture cause or correlate with something other than auditors' commuting stress.

Professionalism and Quality Control do not Always Compensate Stress Responses

We conduct a cross-sectional analysis to substantiate the validity of our assumption that audit staff's stress response substantially affects their work, and that the audit team's internal quality control is insufficient to alleviate possible consequences of stress-impaired procedures. Our primary results provide evidence consistent with a stress response inducing performance effects, even after internal quality control. If our assumption holds that exogenous stress in auditing matters despite auditing's professionalism and internal quality control, our associations should vary with plausible variations of auditors' capacity for professionalism and quality control. Therefore, we split our sample based on whether a client's fiscal year ends in December (i.e., falls in the busy season). Auditors are more resource-constraint in the busy season, plausibly reducing their capacity for quality control and compensating quality deficiencies. Panel A of Table 6 shows insignificant coefficients on *LN_ACCIDENTS* in our three Models outside of the busy season. Conversely, *LN_ACCIDENTS* is significant and positive in all three Models for the subsample of busy-season audits. These results are consistent with our proposed mechanism and make alternative explanations for our primary results less likely.

Audit Staff Do Not Get Used to Accidents

Two more cross-sectional analyses address possible concerns that audit staff might internalise the occurrence of accidents to the extent that they become essentially expected events. Our hypothesis and contribution to the literature depend on our assumption that unpredictability is a central contributor to traffic-induced stress. Therefore, we use plausible variation in the unpredictability of accidents in our sample to substantiate this assumption. As Figure 1 shows, local infrastructure characteristics make accidents more common in certain regions. While individual accidents remain unpredictable, locals possibly get used to unexpected traffic congestion, reducing their susceptibility to traffic-induced stress. A high daily standard deviation of accident events is a plausible proxy for the unpredictability of these events. Therefore, we build two subsamples based on a median split of the standard deviation of daily accidents corresponding to the daily mean of accidents (*ACCIDENTS*) we used to construct our variable *LN_ACCIDENTS*. Another plausible proxy for predictability is urbanicity because the residents of the most urban areas are more likely to be constantly exposed to traffic congestions. Therefore, we build two subsamples based on the larger versus smaller cities hosting the audit office. Consistent with our proposed mechanism, Panel B (for standard deviation of accidents) and Panel C (for urbanicity of audit offices) of Table 6 show that our results are concentrated in the subsamples where traffic congestions from accidents are plausibly more surprising. Therefore, any alternative explanation for our results must also exhibit variation correlated with the standard deviation of accident occurrence and urbanicity of audit offices. Moreover, while the variation in accident predictability affects the strength of stress responses predicted by our theory, it does not affect the commuting time added by those accidents. Hence, this analysis also corroborates our conjecture that minor losses of time caused by accidents are unlikely to drive the effects we observe.

Robustness Tests

We conduct six robustness tests to control for possible omitted variables correlated with our traffic measure and measurement error. The first four robustness tests address possible concerns that the likelihood of traffic incidents may systematically vary with local economic, infrastructural, and cultural characteristics. In their paper primarily studying whether auditors price local rush hour congestion as a business risk factor, Hao and Pham (2024) show in an additional test in their Table 6 that, without controlling for local, time-invariant fixed effects, local rush hour congestion is negatively associated with audit quality. While their measure is also traffic congestion-related, it does not have the necessary characteristics of an exogenous stressor based on the theory we use in this paper. Specifically, rush hour congestion is predictable and, to some degree, controllable, which accidents are not. Consistent with these arguments, we find that in our sample, industry and year fixed effects can only explain around 10 percent of the variation in both our accident measure and the rush hour measures used by Hao and Pham (2024), but our primary fixed effects structure containing granular local fixed effects can explain around 85 percent of the variation in rush hour measures, but only 67 percent of the variation in our accident measure, leaving more than twice as much variation unexplained and, therefore, unpredictable (untabulated).

To test the robustness of our results to the remaining variation in rush hour congestion, in our first robustness test, we include rush-hour data from Texas A&M's Urban Mobility Report.[25] Following Hao and Pham (2024), we use (1) their Travel Time Index (*TTI*), which measures the time it takes to pass a road section during peak hours scaled by the time it takes to pass the same road section during low-volume hours; (2) the natural logarithm of the additional time the average commuter is estimated to spend being stuck in traffic per year (*DELAYHOURS*); and (3) the natural logarithm of the estimated cost from rush hour congestion per commuter (*CONGESTCOST*). The data is available at the urban area-by-calendar year

[25] Data accessed through https://mobility.tamu.edu/umr/data-and-trends/.

level, with urban areas from the Highway Performance Monitoring System. We match urban areas to our data based on the U.S. Census Beaureau's 2010 urban area to county relationship file.[26] Counties can span multiple urban areas, and urban areas can span multiple counties. Therefore, for each county, we take the average of rush hour measures for all urban areas it spans.[27]

Panel A of Table 7 presents our results when adding *TTI*, *DELAYHOURS*, and *CONGESTCOST* as additional controls to our primary analysis. For brevity, we only report coefficients on *LN_ACCIDENTS* and *TTI*, *DELAYHOURS*, and *CONGESTCOST*. Our results for *LN_ACCIDENTS* are almost identical economically and statistically compared to our primary specification throughout all three Models. Moreover, *TTI*, *DELAYHOURS*, and *CONGESTCOST* are insignificant and economically small in all three Models. Thus, we find no evidence for an association of rush-hour congestion with audit quality. These results suggest that our measure captures an incremental association of traffic with audit quality beyond usual congestion levels in urban areas. Moreover, the results further support the conclusion from our cross-sectional tests in Table 6, Panels B and C, that our measure captures events with the necessary novelty and unpredictability to trigger a stress response different from responses to everyday stressors.

Second, we add client fixed effects to our main regression models. Hence, we rely only on within-client variation in traffic-induced stress of audit staff, keeping all time-invariant local and cultural characteristics of a client and its headquarters fixed. We drop client commuting zone and industry fixed effects in these regressions because they are already covered by the client fixed effects. Panel B of Table 7 presents the results of repeating our primary analysis from Table 4 with client fixed effects. The coefficient on *LN_ACCIDENTS* is significant and

[26] See https://www.census.gov/geographies/reference-files/2010/geo/relationship-files.html.
[27] We lose 53 observations when *DA* is our dependent variable and 61 observations when *RESTATE* or *CL* is our dependent variable because some rural counties have no corresponding urban area.

positive in all three Models.[28] Third, to control for workload differences between audit offices and between the busy and off seasons, we replace auditor commuting zone with audit office-by-busy season fixed effects and, again, find robust results in Panel C of Table 7. Fourth, in Panel D of Table 7, we add several community and traffic-related controls from the U.S. census, all available at the county level. *IN_COUNTY* is the percentage of county residents working in their county. *COMM_TIME* is the average time in minutes county residents commute to work. *PERC_CAR* is the percentage of county residents commuting to work by car. *POP_DENS* is a county's population density. Moreover, we add *N_ROADS*, which is the number of roads in the 40-km radius used to construct *LN_ACCIDENTS*, to capture the density of the local road infrastructure. Our primary results remain statistically significant. None of the added controls is significant in any of the three Models, suggesting that our commuting zone fixed effects already cover community and infrastructure characteristics. Overall, these robustness tests alleviate concerns that omitted variables drive our primary results. While we cannot fully exclude omitted variable bias, we cannot think of an omitted variable varying within our fixed effects, beyond our controls, and creating all patterns we previously showed in our placebo and cross-sectional tests that would be more likely to explain our results than our theory-backed test variable.

To address possible measurement error, we first restrict our sample to only those auditor-client pairs where client headquarters and audit offices are no more than 40 km apart. Hence, the client office is within the radius covered by our measure, increasing the likelihood that our measure covers commutes, and that larger percentages of the accidents we cover are outside of

[28] Client fixed effects only address correlated omitted variables that are time-invariant. To further address concerns about possible time-varying correlated omitted variables, we conduct a placebo test using *MW* as our dependent variable and dropping *MW* from the right-hand side of equation (1). Most internal control testing is done at interim periods, so traffic-related stress during year-end field work should only have minor effects (we thank an anonymous referee for suggesting this placebo test). We do not find a significant association of *LN_ACCIDENTS* with *MW* (coefficient = 0.013, standard error = 0.008, p = 0.103). However, as the test approaches significance at the 10% level, and as concerns are about omitted client characteristics, we repeat the test after including client fixed effects and find that the association is insignificant (coefficient = 0.006, standard error = 0.008, p = 0.494). Hence, this additional placebo test further corroborates our primary inferences.

typical commuting routes. Panel E of Table 7 corroborates our primary results, as *LN_ACCIDENTS* is significantly positive in all three Models. In our last robustness test, we exclude 55 observations where *LN_ACCIDENTS* is zero. Table 7, Panel F again shows significant and positive coefficients on *LN_ACCIDENTS* throughout all Models. Overall, those tests alleviate concerns that measurement error drives our results. As noted above, Appendix C reports the results of additional sensitivity tests when we change the construction of our traffic measure, further supporting the robustness of our inferences.

Conclusion

We use data on severe accidents in the U.S. from 2016 to 2021 to analyze the association of exogenous variation in traffic-induced stressors with audit quality. We find a robust association between the frequency of accidents around the client's headquarters and discretionary accruals, restatement likelihood, and the likelihood of receiving SEC comment letters. Further analyses suggest that these results are driven by audit staff's exposure to accidents, not client personnel's. We also present placebo tests and cross-sectional results consistent with our proposed but unobservable mechanism that accidents cause substantial stress exogenous to individual audit staff's control that eventually threatens audit quality despite auditor's professionalism and internal quality control.

Our research is subject to several caveats. Despite all efforts, the unobservable nature of the long causal chain we propose necessarily makes our evidence indirect. Second, while we collect evidence that lost time plays a subordinate role, we cannot fully exclude that it contributes to our results. However, since both mechanisms stem from stressors originating outside the audit task and could largely be alleviated by similar countermeasures that reduce the burden of commuting, we believe that our insights are relevant without being able to fully disentangle them. Future research, possibly using experimental designs, may provide cleaner and causal evidence for the associations we document. Finally, due to data limitations, we cover only a short period and cannot build a meaningful post-COVID sample. It is an empirical question whether auditors shifted their use of remote auditing substantially enough after the experiences during the COVID-19 pandemic to alleviate traffic-induced stress and its negative effects. We leave it to future research to address this question.

Despite these limitations, we contribute to the literature and audit practice. We introduce exogenous, non-work stressors as an important source of stress in the audit setting with direct practical implications, as clients and auditors should share an interest in mitigating potential harm to the audit process from its non-work environment. For traffic in particular, our COVID-19 results suggest that traffic-induced stress is no longer a factor when auditors work remotely. We also extend recent research on potential threats to the audit process that infers individual auditor behavior from 'detached' macro or society-level data (Chen et al. 2024; Morris and Hoitash 2023). Other exogenous stressors, such as family issues, personal financial distress, mental health issues, harassment, or extreme weather, could pose similar threats to auditing.

The insights are also relevant for regulators governing and assessing auditors' quality management and quality control systems. From a social sustainability perspective, our results suggest that addressing employees' individual non-work issues could be important for firm performance, both in the interest of audit firms and regulators. Finally, we show a spillover of traffic to the workplace, furthering our understanding of a potentially underappreciated negative welfare outcome of congested roads.

Data Availability Statement

Data is available from the public sources described in the text.

Declaration of Interests Statement

The authors have no relevant financial or non-financial interests to disclose.

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Appendix A

Variable Definitions

Variable	Definition
Test Variables	
ACCIDENTS	The daily mean of the number of accidents within 40km of a client's headquarters. We only count accidents with severity codes 3 or 4 in the 40 days before the signature date on the 10-K filing.
LN_ACCIDENTS	Natural logarithm of 1+*ACCIDENTS*.
Dependent Variables	
DA	Absolute performance-adjusted modified Jones (1991) discretionary accruals (Kothari et al. 2005). The absolute value of residuals from cross-sectional regressions of equation (B.1) based on SIC 2-digit industry years (see Appendix B).
RESTATE	Dummy coded as one if the 10-K is later restated. We remove restatements due to clerical errors without accounting implications, and those where the error had a downward effect on earnings.
CL	Dummy coded as one if the 10-K receives an SEC comment letter.
Control Variables	
SIZE	Natural logarithm of total assets.
FOREIGN	Dummy coded as one if the firm reports nonzero foreign currency translation.
BUSSEG	Natural logarithm of the number of business segments.
GEOSEG	Natural logarithm of the number of geographical segments.
STDCFO	Five-year rolling (t-4 to t) standard deviation of cash flows from operating activities.
STDREV	Five-year rolling (t-4 to t) standard deviation of sales revenue.
GROWTH	Current year sales minus prior year sales, scaled by prior year sales.
RESTRUCTURE	Dummy coded as one for firms that report nonzero RCP, RCA, RCEPS, or RCD.
CFO	Cash flow from operating activities scaled by lagged total assets.
MW	Dummy variable coded as 1 if the auditor identified an internal control weakness.
LATE	Dummy coded as one if the 10-K was filed after the filing deadline.
QUICK	Current assets minus inventory, scaled by current liabilities.
LEV	Leverage, measured as total debt divided by total assets.
LOSS	Dummy coded as one if net income before extraordinary items is negative.
ZSCORE	1.2 · (current assets – debt in current liabilities)/total assets + 1.4 · retained earnings/total assets + 3.3 · EBIT/total assets +

	0.6 · market value of equity/total liabilities + 0.999 · revenues/total assets.
GC	Dummy coded as one if the auditor issues a qualified going concern opinion.
RESTATING	Dummy coded as one if the company issues a restatement of a prior period in the current fiscal year.
LN_BTM	The natural logarithm of book value of equity divided by market value of equity.
LIT	Dummy coded as one if the firm belongs to one of the following SIC 4-digit industries, which are considered highly litigious industries (Francis, Philbrick, and Schipper 1994): 2833-2836, 3570-3577, 3600-3674, 5200-5961, 7370-7374, 8731-8734.
BIG4	Dummy variable coded as 1 for firms with a Big4 auditor.
INDSPEC	Dummy variable coded as 1 if the sum of all audit fees an audit office received in a SIC 2-digit industry is more than 30 percent of all audit fees paid by the same SIC 2-digit industry in the same city.
AUDCHANGE	Dummy coded as one if the current year's auditor has changed from the prior year's auditor.
AUDDIST	Distance between the client headquarters and the city of the audit office (via Google Maps API) in kilometers.
DYE	Dummy coded as one if the client's fiscal year ends in December.
LACC	Dummy coded as one if the client is a large accelerated filer.
NACC	Dummy coded as one if the client is a non-accelerated filer.

Appendix B

Accruals Models

To obtain performance-adjusted modified Jones (1991) discretionary accruals, we estimate the following model by SIC 2-digit industry-year, and require at least ten observations for each industry-year. We use all Compustat observations with positive total assets and the necessary data available and winsorize all variables at the 1st and 99th percentile:

$$TA_{i,t} = \beta_0 + \beta_1(1/Assets_{i,t-1}) + \beta_2(\Delta Sales_{i,t} - \Delta Receivables_{i,t}) + \beta_3 PPE_{i,t} + \beta_4 ROA_{i,t-1} + \varepsilon_{i,t} \tag{B.1}$$

where:

TA	=	Total accruals, calculated as net income before extraordinary items minus net cash flow from operating activities, scaled by lagged total assets
1/Assets	=	1, scaled by lagged total assets
Δ*Sales*	=	Change in total revenue, scaled by lagged total assets
Δ*Receivables*	=	Change in total receivables, scaled by lagged total assets
PPE	=	Net property, plant, and equipment, scaled by lagged total assets
ROA	=	Return on assets, calculated as net income before extraordinary items, scaled by the average of the current year and prior year total assets

The absolute values of residuals from estimating equation (B.1) represent *DA*.

Appendix C

Additional Robustness Tests

Table C1 summarises several robustness tests referenced above to show that our results are robust to design choices and assumptions we made in creating our measure *LN_ACCIDENTS*. Each line represents an excerpt of OLS regression results where we re-estimate the three Models in Table 4 and replace *LN_ACCIDENTS* with different alternative measures or change the fixed effects structure. We only report coefficients and T-statistics on the variable replacing *LN_ACCIDENTS* for brevity.

First, we show the robustness of our results by using the log transformation we used to reduce the skewness of the measure *ACCIDENTS*. The first line presents results when using the raw *ACCIDENTS* measure. The second line presents results using decile ranks of *ACCIDENTS* instead of the log transformation. Inferences remain the same in both alternatives.

Second, we explore results using all accidents instead of only accidents with severity scores of 3 or 4. The third line presents the results. Although we have no control over the assignment of severity scores and only roughly know the determinants of the scores, including severity scores 1 and 2 would likely include many accidents that did not disturb commutes substantially. Consistent with this argument, we find similar but weaker results compared to our primary specification. Specifically, we no longer find a significant association with discretionary accruals in Model 1, while results in Model 2 and Model 3 are of similar size and statistical significance as in our primary specification.

To further explore the choice of filtering on the pre-defined severity measure, we instead use the original data on the start and end locations and timestamps of the accidents to identify accidents that are likely severe. Note that relying on these data also introduces noise and researcher choice. Many accidents do not cause additional traffic congestion, e.g., because they are minor bumps and involved cars can quickly move to the shoulder of the road. However, these accidents still vary in terms of how long they last according to our data. To ensure that we only include accidents that likely disturb traffic at least to some degree, we therefore recreate our measure of interest using only accidents recorded in our data base for at least 5 minutes. As the fourth line in Table C1 shows, results disappear for discretionary accruals in Model 1, while results in Model 2 and Model 3 are of similar size and statistical significance as in our primary specification. When we instead use congestion distance to infer severity, we do not know the dynamics of the traffic congestion, e.g., when congestion started, how long it took until it fully built up, for how long it lasted, and how severely the congestion affected traffic flow. Moreover, data to calculate congestion distances is only available for a subsample, and we cannot identify whether unavailability reflects missing data or congestions of zero length. Moreover, we have to use start and end point coordinates to calculate congestion length, which is noisy because it ignores different road trajectories. When recreating our measure with only accidents above the 75th percentile of congestion length among all accidents in the fifth line, we find robust results in Models 1 and 2, while results remain of similar magnitude but become insignificant in Model 3.

Third, we relax our assumption of a 40 km radius. We use a radius of 100 km, the maximum distance we allow between client headquarters and audit offices. Thus, we increase the likelihood of covering all relevant accidents affecting audit staff's commutes. However, we also risk introducing a lot of noise because many accidents between 40 and 100 km from the client headquarters may not matter. Consistent with this argument, we find similar but weaker results (see line six) compared to our primary specification. Specifically, we no longer find a significant association with restatements in Model 2, while results in Model 1 and Model 3 are of similar size and statistical significance as in our primary specification.

Fourth, we relax several assumptions related to the timing of commutes. In the seventh line, we use a window from each client's individual fiscal year-end to the signature date of the auditor's report instead of the 40-day window ending on the auditor's report signature date. In the eighth line, we use a 20- instead of a 40-day window. Both variations reduce the likelihood of including days before year-end audit fieldwork has started. Finally, results reported in the ninth line are based on only including accidents that happen during the day, between 6 am and 10 pm, when calculating *LN_ACCIDENTS*. While this requires assumptions on the time window in which audit staff commute, it removes accidents that are outside likely commuting windows. Lines seven, eight, and nine present results that are inferentially identical to results from our primary tests.

Fifth, we replace client and auditor commuting zone fixed effects with client and auditor county fixed effects to further alleviate concerns that local characteristics drive our results. Our results in line ten are similar to our primary results, although they become statistically insignificant at conventional levels when we use restatements as an audit quality proxy.

Finally, we conduct one more placebo test based on the matching procedure when constructing *LN_ACCIDENTS*. We present results in the eleventh line. We virtually move each client's headquarters to a random city when building the 40 km radius to identify accidents. As expected, we find no significant associations of this random traffic data with our dependent variables.

Table C1 OLS Regressions: Different Measures of Traffic-induced Stress and Audit Quality

	Model 1 DV = *DA*	Model 2 DV = *RESTATE*	Model 3 DV = *CL*
Variable	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)
ACCIDENTS	0.001*** (0.000)	0.002** (0.001)	0.002* (0.001)
Decile-rank transformation of *ACCIDENTS*	0.028*** (0.009)	0.063*** (0.021)	0.053** (0.025)
LN_ACCIDENTS when using accidents of severity 1-4	0.004 (0.003)	0.019*** (0.007)	0.017* (0.009)

LN_ACCIDENTS when using accidents lasting five minutes or more	0.003 (0.002)	0.015*** (0.005)	0.015** (0.007)
LN_ACCIDENTS when using accidents with congestion distance above the 75th percentile	0.012** (0.005)	0.018* (0.010)	0.021 (0.014)
LN_ACCIDENTS when the radius is 100 km	0.013*** (0.004)	0.012 (0.008)	0.032*** (0.011)
LN_ACCIDENTS using the period between fiscal year-end and signature date	0.009** (0.003)	0.018** (0.007)	0.025** (0.010)
LN_ACCIDENTS when using only the last 20 days before the signature	0.009*** (0.003)	0.016** (0.006)	0.019** (0.008)
LN_ACCIDENTS when using accidents happening between 6am and 10pm	0.010*** (0.003)	0.019** (0.007)	0.024** (0.010)
LN_ACCIDENTS when using client and auditor county fixed effects	0.008** (0.004)	0.013 (0.008)	0.031*** (0.011)
LN_ACCIDENTS when matching accidents from a random city	-0.001 (0.002)	0.003 (0.006)	-0.003 (0.006)

Notes: This table presents OLS regressions of discretionary accruals (Model 1), restatements (Model 2), and comment letters (Model 3). Each line represents a re-estimation of the Models in Table 4 by replacing *LN_ACCIDENTS* with the displayed variable. All other results are suppressed for brevity. The text of Appendix C describes the displayed variables. Continuous variables are winsorized at the 1st and 99th percentile. Standard errors are heteroskedasticity-robust and clustered at the client firm level. *, **, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Appendix D

Back-of-the-Envelope Calculations of the Amount of Stress Accidents Create

Although we do not have observable data on how often auditors are severely stressed after being stuck in traffic, we can combine our data with data from earlier research to assess the plausibility of our stress proxy. Our main variable *LN_ACCIDENTS* is constructed based on all weekdays in a 40 calendar-day window, typically 28 weekdays. Assuming that the 99 hours Americans spend stuck in traffic are randomly distributed across the year (Reed 2020), this window covers 28 days/365 days × 99 hours = 7.6 hours. This number is likely a lower bound because traffic is lighter on weekends, and workers drive every weekday but may not drive every weekend. If we reduce the 7.6 hours by daily rush-hour congestion of 29 percent of driving time[29] (28 days × 0.29 × (26.9 minutes/day / 60 minutes/hour) = 3.6 hours), the remaining 4 hours cover stress from accidents. For instance, 4 hours roughly equal running into three big delays of one hour and another two smaller delays of half an hour. Under these assumptions, the average commuter will be affected by an accident on her way to work five times in almost six weeks. Given that we observe an average of almost 10 severe accidents per day (we cover a 40-km radius around a client headquarter), individuals would need to run into 1.8 percent of those accidents (5/(9.896 accidents/day × 28 days)) to fulfil these assumptions.

A first rough interpretation of this data is that individuals come into an audit after a stressful commute on 18 percent (5 of 28) of fieldwork days. Given that not all individuals will be affected during the same days and that stress of team members likely affects the entire team (Annelin and Svanström 2022), the affected percentage of fieldwork days is likely higher. Hence, it is not unlikely that arousal, strain accumulation, and depleted cognitive resources and self-control from commuting stress are significant factors during audit fieldwork. Conversely,

[29] This is the average extra time for a commute in the most congested direction during peak periods in the US in 2019 according to the 2021 Urban Mobility Report of the Texas A&M Transportation Institute, see https://static.tti.tamu.edu/tti.tamu.edu/documents/umr/archive/mobility-report-2021.pdf (accessed April 3, 2025).

focusing solely on lost time as a complementary mechanism, Christensen et al. (2021) report that audit team members, on average, work 53 hours a week during final audit fieldwork. Hence, the total 4-hour reduction we calculate above equals only 1.35 percent of average working hours during final fieldwork ((4 hours/28days × 5 days/week)/53 hours/week), which is one order of magnitude smaller than the relative number of days that are likely affected under the same assumptions. We therefore conclude that associations of traffic-induced stress with audit quality are most likely to work through spillovers of strain and depleted cognitive resources, not through the complementary mechanism of lost time. This makes our setting particularly useful to study our research question because previous literature shows that the quality of the commute is generally the more likely mechanism than lost time behind associations of traffic-induced stress with work performance (Wiese et al. 2024).

Tables and Figures

Table 1 Sample Selection

Criterion	*Less*	Remaining observations
Intersection of Compustat and Audit Analytics Opinions, Audit Opinion Signatures at Least 40 Days after August 1, 2016, Fiscal Years ≤ 2021, Non-Financial Firm Years		22,588
Headquarters Location Available	(2,615)	19,973
All Variables Available	(7,429)	12,544
Audit Report Lag ≤ 250 Days	(39)	12,505
> One Audit Firm in SIC-2-Digit-City	(3)	12,502
Sales > 0	(85)	12,417
Contiguous United States Only	(51)	12,366
> 50 Percent Car Commuters	(769)	11,597
U.S. Auditor and Auditor-Client Distance ≤ 100 km	(2,550)	9,047
Audit Opinion Signature Before April 1, 2020	(2,997)	6,050
Singletons	(10)	6,040
Final Sample (Dependent Variable = *RESTATE* or *CL*)		**6,040**
Necessary Data to Calculate Discretionary Accruals	(327)	5,713
Final Sample (Dependent Variable = *DA*)		**5,713**

Table 2 Descriptive Statistics								
Variable	N	Mean	Median	Std. Dev.	Min	0.25	0.75	Max
Test Variables								
ACCIDENTS	6040	9.896	8.300	7.878	0.000	4.075	14.205	37.109
LN_ACCIDENTS	6040	2.082	2.230	0.864	0.000	1.624	2.725	3.640
Dependent Variables								
DA	5713	0.099	0.059	0.127	0.001	0.025	0.119	0.908
RESTATE	6040	0.080	0.000	0.272	0.000	0.000	0.000	1.000
CL	6040	0.158	0.000	0.365	0.000	0.000	0.000	1.000
Control Variables								
SIZE	6040	6.799	6.942	2.225	1.363	5.304	8.354	11.787
FOREIGN	6040	0.365	0.000	0.481	0.000	0.000	1.000	1.000
BUSSEG	6040	0.538	0.000	0.653	0.000	0.000	1.099	1.946
GEOSEG	6040	0.762	0.693	0.727	0.000	0.000	1.386	2.485
STDCFO	6040	159.591	33.757	418.730	0.439	9.617	108.823	3103.904
STDREV	6040	571.545	109.402	1511.078	0.202	22.232	408.198	11178.020
GROWTH	6040	0.165	0.056	0.702	-0.773	-0.023	0.161	5.999
RESTRUCTURE	6040	0.405	0.000	0.491	0.000	0.000	1.000	1.000
CFO	6040	0.036	0.081	0.222	-1.301	0.024	0.133	0.420
MW	6040	0.067	0.000	0.250	0.000	0.000	0.000	1.000
LATE	6040	0.061	0.000	0.240	0.000	0.000	0.000	1.000
RESTATING	6040	0.060	0.000	0.238	0.000	0.000	0.000	1.000
QUICK	6040	2.263	1.439	2.584	0.214	0.950	2.483	18.152
LEV	6040	0.212	0.198	0.182	0.000	0.025	0.342	0.703
LOSS	6040	0.355	0.000	0.479	0.000	0.000	1.000	1.000
ZSCORE	6040	3.662	3.194	7.163	-25.359	1.577	5.092	37.130
LN_BTM	6040	-1.040	-0.952	0.959	-4.282	-1.592	-0.412	1.241
LIT	6040	0.361	0.000	0.480	0.000	0.000	1.000	1.000
BIG4	6040	0.714	1.000	0.452	0.000	0.000	1.000	1.000
INDSPEC	6040	0.279	0.000	0.449	0.000	0.000	1.000	1.000
AUDCHANGE	6040	0.042	0.000	0.201	0.000	0.000	0.000	1.000
AUDDIST	6040	23.790	19.034	20.210	0.460	9.183	33.731	99.854
DYE	6040	0.737	1.000	0.440	0.000	0.000	1.000	1.000
LACC	6040	0.575	1.000	0.494	0.000	0.000	1.000	1.000
NACC	6040	0.156	0.000	0.363	0.000	0.000	0.000	1.000

Notes: This table presents descriptive statistics of all variables used in our main regression models. The variables are winsorized at the 1st and 99th percentile. Appendix A contains variable definitions.

Table 3 Correlations

	Variable	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1	*LN_ACCIDENTS*	0.06	0.01	0.03	-0.02	0.03	-0.07	0.02	0.05	0.04	0.05	0.00	-0.04	0.01	0.01
2	*DA*	1	0.01	-0.05	-0.31	-0.09	-0.14	-0.12	-0.06	-0.07	0.19	-0.08	-0.36	0.11	0.09
3	*RESTATE*		1	0.03	0.00	0.02	0.01	0.00	-0.02	-0.01	-0.01	0.02	0.02	0.16	0.08
4	*CL*			1	0.19	0.03	0.06	0.06	0.10	0.10	-0.02	0.08	0.08	-0.01	-0.02
5	*SIZE*				1	0.14	0.28	0.24	0.53	0.52	-0.09	0.26	0.42	-0.17	-0.17
6	*FOREIGN*					1	0.06	0.50	0.06	0.04	-0.05	0.21	0.10	0.00	-0.01
7	*BUSSEG*						1	0.15	0.08	0.12	-0.10	0.16	0.17	-0.04	-0.01
8	*GEOSEG*							1	0.14	0.12	-0.11	0.30	0.18	-0.03	-0.04
9	*STDCFO*								1	0.80	-0.03	0.08	0.14	-0.07	-0.08
10	*STDREV*									1	-0.03	0.07	0.13	-0.07	-0.07
11	*GROWTH*										1	-0.12	-0.18	0.05	0.02
12	*RESTRUCTURE*											1	0.12	-0.02	-0.05
13	*CFO*												1	-0.09	-0.08
14	*MW*													1	0.32

		16	17	18	19	20	21	22	23	24	25	26	27	28	29
1	*LN_ACCIDENTS*	-0.01	0.07	-0.07	0.07	0.02	-0.05	0.11	-0.03	0.00	0.02	-0.24	0.02	-0.02	-0.01
2	*DA*	0.02	0.10	-0.11	0.23	-0.19	-0.08	0.09	-0.22	-0.12	0.07	0.01	0.05	-0.23	0.27
3	*RESTATE*	0.34	-0.05	0.01	0.01	-0.03	0.05	-0.03	-0.05	-0.01	0.03	0.02	-0.01	-0.03	0.01
4	*CL*	0.04	-0.09	0.09	-0.06	0.01	-0.04	-0.03	0.13	0.08	-0.01	-0.03	0.03	0.17	-0.13
5	*SIZE*	0.02	-0.28	0.48	-0.41	0.13	-0.06	-0.15	0.62	0.35	-0.15	-0.13	0.05	0.74	-0.63
6	*FOREIGN*	0.02	-0.05	0.01	-0.05	0.04	-0.05	-0.03	0.09	0.07	-0.02	0.04	-0.02	0.15	-0.14
7	*BUSSEG*	0.01	-0.21	0.15	-0.20	-0.03	0.09	-0.25	0.13	0.08	-0.02	0.03	-0.02	0.20	-0.13
8	*GEOSEG*	0.00	-0.08	0.02	-0.14	0.06	-0.06	-0.06	0.15	0.13	-0.04	0.04	-0.04	0.24	-0.19
9	*STDCFO*	-0.01	-0.10	0.14	-0.14	0.01	-0.08	-0.01	0.21	0.21	-0.06	-0.09	0.03	0.29	-0.15
10	*STDREV*	0.01	-0.13	0.13	-0.16	0.02	-0.04	-0.04	0.21	0.19	-0.05	-0.07	-0.01	0.29	-0.16
11	*GROWTH*	0.01	0.14	-0.03	0.08	0.02	-0.11	0.12	-0.01	-0.05	0.01	-0.01	0.06	-0.03	0.02
12	*RESTRUCTURE*	0.04	-0.16	0.19	-0.05	-0.08	0.06	-0.06	0.21	0.11	-0.03	0.01	-0.04	0.18	-0.19

13	*CFO*	0.01	-0.19	0.12	-0.51	0.43	0.02	-0.17	0.20	0.13	-0.05	-0.02	-0.1	0.34	-0.30
14	*MW*	0.15	-0.04	-0.06	0.11	-0.08	0.02	0.01	-0.15	-0.06	0.13	0.01	-0.01	-0.13	0.15
15	*LATE*	0.08	-0.01	-0.05	0.11	-0.07	0.04	0.01	-0.14	-0.07	0.11	0.03	0.02	-0.14	0.11
16	*RESTATING*	1	-0.05	0.02	0.01	-0.03	0.03	-0.02	0.00	0.01	0.03	0.02	-0.01	0.00	-0.02
17	*QUICK*		1	-0.28	0.16	0.39	-0.02	0.22	-0.13	-0.13	0.04	0.04	0.04	-0.15	0.05
18	*LEV*			1	-0.11	-0.22	-0.12	-0.17	0.30	0.15	-0.06	-0.05	0.11	0.31	-0.27
19	*LOSS*				1	-0.28	0.10	0.19	-0.21	-0.15	0.07	0.00	0.09	-0.38	0.28
20	*ZSCORE*					1	-0.15	0.02	0.08	0.03	-0.05	0.01	-0.07	0.19	-0.27
21	*LN_BTM*						1	-0.17	-0.14	-0.09	0.04	0.01	-0.02	-0.24	0.17
22	*LIT*							1	0.00	-0.07	-0.01	-0.03	-0.1	-0.07	0.01
23	*BIG4*								1	0.39	-0.15	-0.12	0.05	0.54	-0.55
24	*INDSPEC*									1	-0.08	-0.05	0.00	0.28	-0.23
25	*AUDCHANGE*										1	0.00	0.00	-0.14	0.13
26	*AUDDIST*											1	-0.01	-0.07	0.07
27	*DYE*												1	0.04	-0.05
28	*LACC*													1	-0.50
29	*NACC*														1

Notes: This table presents coefficients of pair-wise correlations between our test variables and dependent variables. The variables are winsorized at the 1st and 99th percentile. Appendix A contains variable definitions. The number of observations is the smaller of the two numbers for the paired variables in column N of Table 2. Coefficients with absolute values above 0.03 are significant at the 5 percent level.

Table 4 OLS Regressions: Traffic-induced Stress and Audit Quality

	Model 1	Model 2	Model 3
	DV = *DA*	DV = *RESTATE*	DV = *CL*
Variable	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)
LN_ACCIDENTS	**0.010***** **(0.003)**	**0.018**** **(0.007)**	**0.023**** **(0.010)**
SIZE	-0.007*** (0.002)	0.013*** (0.005)	0.026*** (0.006)
FOREIGN	-0.008** (0.004)	-0.003 (0.010)	-0.010 (0.012)
BUSSEG	-0.004* (0.002)	-0.004 (0.007)	-0.006 (0.009)
GEOSEG	-0.004 (0.003)	-0.006 (0.008)	-0.003 (0.009)
STDCFO	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
STDREV	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
GROWTH	0.023*** (0.005)	-0.001 (0.004)	0.003 (0.006)
RESTRUCTURE	0.013*** (0.004)	-0.006 (0.009)	0.012 (0.011)
CFO	-0.094*** (0.026)	0.014 (0.021)	0.016 (0.025)
MW	0.014 (0.009)	0.119*** (0.021)	0.013 (0.019)
LATE	0.004 (0.009)	0.029 (0.020)	0.021 (0.019)
RESTATE	0.001 (0.006)	-	0.027 (0.02)
QUICK	0.000 (0.001)	-0.003* (0.002)	-0.004* (0.002)
LEV	-0.020 (0.015)	0.005 (0.033)	-0.046 (0.034)
LOSS	-0.009 (0.006)	0.002 (0.010)	0.031** (0.012)
ZSCORE	-0.001** (0.001)	0.000 (0.001)	-0.001 (0.001)
GC	0.047*** (0.018)	-0.021 (0.022)	0.006 (0.021)
RESTATING	0.010 (0.007)	0.335*** (0.026)	0.033 (0.022)
LN_BTM	-0.015*** (0.003)	0.006 (0.006)	-0.011* (0.006)

LIT	0.005 (0.007)	-0.003 (0.018)	0.005 (0.017)
BIG4	-0.010 (0.006)	-0.041*** (0.015)	-0.001 (0.014)
INDSPEC	0.001 (0.004)	0.005 (0.011)	-0.006 (0.013)
AUDCHANGE	0.004 (0.01)	0.002 (0.018)	0.035 (0.022)
AUDDIST	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
DYE	-0.001 (0.004)	-0.008 (0.011)	0.006 (0.012)
LACC	-0.006 (0.006)	-0.019 (0.014)	0.055*** (0.016)
NACC	0.032*** (0.009)	-0.001 (0.018)	-0.014 (0.015)
Fixed Effects	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year
N	5713	6040	6040
Adj. R^2	0.248	0.159	0.063

Notes: This table presents OLS regressions of discretionary accruals (Model 1), restatements (Model 2), and comment letters (Model 3). Continuous variables are winsorized at the 1st and 99th percentile. Bold entries represent our primary hypothesis test. Appendix A contains variable definitions. Standard errors are heteroskedasticity-robust and clustered at the client firm level. *, **, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table 5 OLS Regressions: Placebo Tests of Traffic-induced Stress and Audit Quality

Panel A Placebo Test with Sample where Client and Auditor Locations > 100 km Apart

	Model 1	Model 2	Model 3
	DV = *DA*	DV = *RESTATE*	DV = *CL*
Variable	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)
LN_ACCIDENTS	**0.012** **(0.012)**	**0.010** **(0.020)**	**-0.019** **(0.024)**
Controls	Yes	Yes	Yes
Fixed Effects	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year
N	1327	1431	1431
Adj. R^2	0.390	0.274	0.075

Panel B Placebo Test with Sample where < 50 Percent of Workers Commute by Car

	Model 1	Model 2	Model 3
	DV = *DA*	DV = *RESTATE*	DV = *CL*
Variable	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)
LN_ACCIDENTS	**0.002** **(0.027)**	**-0.008** **(0.045)**	**0.057** **(0.082)**
Controls	Yes	Yes	Yes
Fixed Effects	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year
N	693	716	716
Adj. R^2	0.352	0.152	0.048

Panel C Placebo Test with Sample After COVID-19 Stay-at-Home Orders

	Model 1	Model 2	Model 3
	DV = *DA*	DV = *RESTATE*	DV = *CL*
Variable	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)
LN_ACCIDENTS	**0.006** **(0.007)**	**-0.004** **(0.011)**	**-0.014** **(0.017)**
Controls	Yes	Yes	Yes
Fixed Effects	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year
N	2850	2992	2992

Adj. R^2	0.293	0.205	0.031

Notes: This table presents OLS regressions of discretionary accruals (Model 1), restatements (Model 2), and comment letters (Model 3). For brevity, we do not report results for control variables. Panel A uses a sample of observations where the distance between the client headquarters and audit office is more than 100 km or the audit office is outside the U.S. Panel B uses a sample of observations where client headquarters are in counties with less than 50 percent of workers commuting by car. Panel C uses all observations with auditor signatures after March 31, 2020. Continuous variables are winsorized at the 1st and 99th percentile. Bold entries represent our primary hypothesis test. Appendix A contains variable definitions. Standard errors are heteroskedasticity-robust and clustered at the client firm level. *, **, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table 6 OLS Regressions: Cross-Sectional Analyses of Traffic-induced Stress and Audit Quality

Panel A Sample Split by whether Fiscal Year Ends in December

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
	Sample: *DYE* = 0	Sample: *DYE* = 0	Sample: *DYE* = 0	Sample: *DYE* = 1	Sample: *DYE* = 1	Sample: *DYE* = 1
	DV = DA	DV = RESTATE	DV = CL	DV = DA	DV = RESTATE	DV = CL
Variable	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)
LN_ACCIDENTS	**0.004** **(0.005)**	**0.014** **(0.013)**	**0.001** **(0.019)**	**0.010**** **(0.005)**	**0.018*** **(0.009)**	**0.031***** **(0.011)**
Fixed Effects	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year
N	1503	1582	1582	4205	4452	4452
Adj. R^2	0.328	0.147	0.014	0.234	0.189	0.067

Panel B Sample Split by Median of Standard Deviation of Daily Accidents

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
	Sample: below-median Std Dev	Sample: below-median Std Dev	Sample: below-median Std Dev	Sample: above-median Std Dev	Sample: above-median Std Dev	Sample: above-median Std Dev
	DV = DA	DV = RESTATE	DV = CL	DV = DA	DV = RESTATE	DV = CL
Variable	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)
LN_ACCIDENTS	**0.009** **(0.006)**	**0.007** **(0.012)**	**0.021** **(0.018)**	**0.023**** **(0.010)**	**0.040*** **(0.022)**	**0.013** **(0.030)**
Fixed Effects	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year
N	2854	3018	3018	2853	3014	3014
Adj. R^2	0.242	0.173	0.056	0.249	0.156	0.075

Panel C Sample Split by Most Urban versus Least Urban Areas						
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
	Sample: Most Urban Areas	Sample: Most Urban Areas	Sample: Most Urban Areas	Sample: Least Urban Areas	Sample: Least Urban Areas	Sample: Least Urban Areas
	DV = DA	DV = RESTATE	DV = CL	DV = DA	DV = RESTATE	DV = CL
Variable	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)
LN_ACCIDENTS	**0.006 (0.007)**	**0.022 (0.014)**	**-0.002 (0.022)**	**0.013*** (0.004)**	**0.019** (0.010)**	**0.040*** (0.012)**
Fixed Effects	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year
N	2754	2901	2901	2958	3138	3138
Adj. R^2	0.276	0.126	0.070	0.229	0.197	0.053

Notes: This table presents OLS regressions of discretionary accruals (Models 1 and 4), restatements (Models 2 and 5), and comment letters (Models 3 and 6). For brevity, we do not report results for control variables. Panel A splits our sample into subsamples of firms with fiscal years ending in January-November (Models 1-3) and in December (Models 4-6). Panel B splits our sample into subsamples of observations where the standard deviation of the daily number of severe accidents in the 40-day window to calculate *LN_ACCIDENTS* is above (Models 1-3) or below (Models 4-6) the median. Panel C splits our sample into a subsample of the most urban areas (audit offices located in Atlanta, Austin, Boston, Chicago, Dallas, Houston, Los Angeles, New York City (where the client is, per our selection criteria, not in New York City), Philadelphia, San Diego, San Francisco, San Jose, and Seattle) in Models 1-3 and least urban areas (all other audit offices) in Models 4-6. Continuous variables are winsorized at the 1st and 99th percentile. Bold entries represent our primary hypothesis test. Appendix A contains all variable definitions. Standard errors are heteroskedasticity-robust and clustered at the client firm level. *, **, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table 7 OLS Regressions: Robustness Tests of Traffic-induced Stress and Audit Quality

Panel A Adding Control for Rush-Hour Congestion

	Model 1 DV = *DA*	Model 2 DV = *RESTATE*	Model 3 DV = *CL*
Variable	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)
LN_ACCIDENTS	**0.010***** **(0.003)**	**0.017**** **(0.007)**	**0.023**** **(0.010)**
TTI	-0.053 (0.117)	0.397 (0.257)	0.279 (0.329)
DELAYHOURS	-0.102 (0.078)	-0.065 (0.161)	-0.177 (0.227)
CONGESTCOST	0.116 (0.080)	-0.018 (0.159)	0.124 (0.234)
Other Controls	Yes	Yes	Yes
Fixed Effects	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year
N	5660	5979	5979
Adj. R^2	0.249	0.161	0.064

Panel B Adding Client Fixed Effects

	Model 1 DV = *DA*	Model 2 DV = *RESTATE*	Model 3 DV = *CL*
Variable	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)
LN_ACCIDENTS	**0.010***** **(0.004)**	**0.014*** **(0.008)**	**0.046***** **(0.013)**
Controls	Yes	Yes	Yes
Fixed Effects	Client, Auditor Commuting Zone, Year	Client, Auditor Commuting Zone, Year	Client, Auditor Commuting Zone, Year
N	5417	5741	5741
Adj. R^2	0.391	0.485	0.072

Panel C Adding Audit Office × Busy Season Fixed Effects

	Model 1 DV = *DA*	Model 2 DV = *RESTATE*	Model 3 DV = *CL*
Variable	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)
LN_ACCIDENTS	**0.008**** **(0.004)**	**0.014*** **(0.008)**	**0.031***** **(0.011)**
Controls	Yes	Yes	Yes

Fixed Effects	Client Commuting Zone, Audit Office × Busy Season, Industry, Year	Client Commuting Zone, Audit Office × Busy Season, Industry, Year	Client Commuting Zone, Audit Office × Busy Season, Industry, Year
N	5635	5961	5961
Adj. R^2	0.297	0.264	0.067

Panel D Adding Controls from Census Data

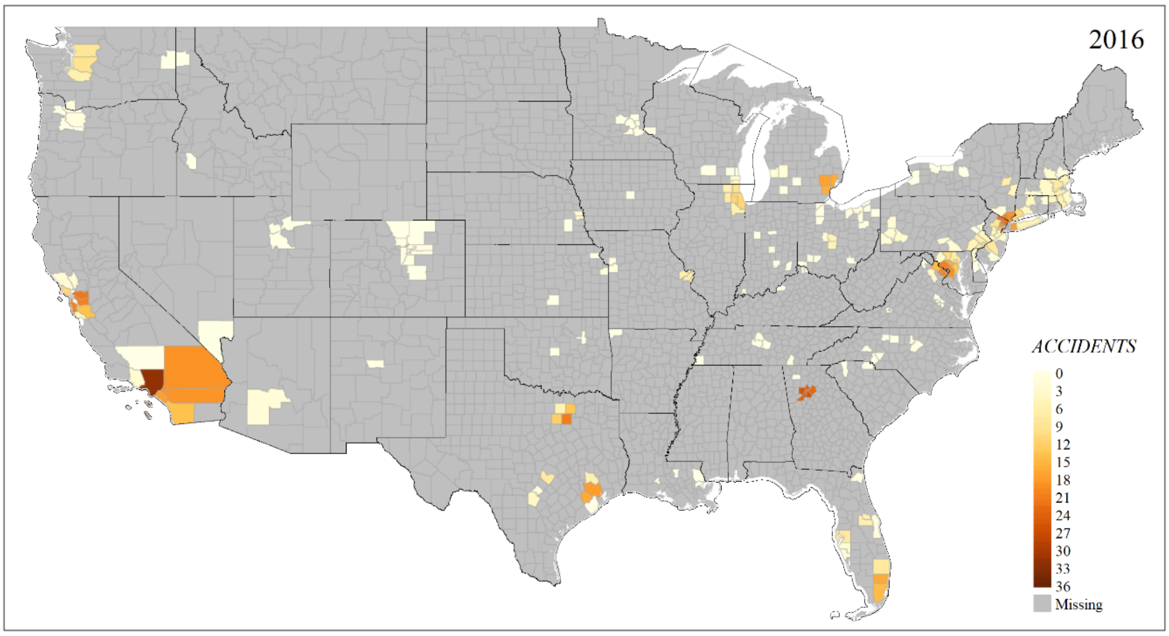
	Model 1 DV = *DA*	Model 2 DV = *RESTATE*	Model 3 DV = *CL*
Variable	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)
LN_ACCIDENTS	**0.010***** **(0.004)**	**0.018**** **(0.008)**	**0.025**** **(0.010)**
IN_COUNTY	0.000 (0.000)	0.000 (0.001)	0.000 (0.001)
COMM_TIME	-0.001 (0.001)	0.002 (0.003)	0.001 (0.004)
PERC_CAR	-0.001 (0.001)	0.001 (0.002)	-0.002 (0.002)
POP_DENS	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
N_ROADS	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Other Controls	Yes	Yes	Yes
Fixed Effects	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year
N	5677	6002	6002
Adj. R^2	0.247	0.159	0.063

Panel E Subsample where Client and Auditor Locations ≤ 40 km Apart

	Model 1 DV = *DA*	Model 2 DV = *RESTATE*	Model 3 DV = *CL*
Variable	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)
LN_ACCIDENTS	**0.012***** **(0.004)**	**0.019**** **(0.008)**	**0.037***** **(0.011)**
Controls	Yes	Yes	Yes
Fixed Effects	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year
N	4688	4967	4967
Adj. R^2	0.243	0.158	0.065

Panel F Subsample without zero Accidents			
	Model 1 DV = *DA*	Model 2 DV = *RESTATE*	Model 3 DV = *CL*
Variable	Coefficient (Std. Error)	Coefficient (Std. Error)	Coefficient (Std. Error)
LN_ACCIDENTS	**0.010***** **(0.003)**	**0.018**** **(0.007)**	**0.023*** **(0.010)**
Controls	Yes	Yes	Yes
Fixed Effects	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year	Client and Auditor Commuting Zones, Industry, Year
N	5662	5985	5985
Adj. R^2	0.245	0.163	0.064

Notes: This table presents OLS regressions of discretionary accruals (Model 1), restatements (Model 2), and comment letters (Model 3). For brevity, we do not report results for control variables. Panel A adds *TTI*, *DELAYHOURS*, and *CONGESTCOST*, as additional control variables to our original specifications. *TTI* is the travel time index from Texas A&M's Urban Mobility Report, which is the ratio of peak travel time to low-volume travel time. *DELAYHOURS* and *CONGESTCOST* are natural logarithms of 1 + Texas A&M's Urban Mobility Report's estimated additional hours and additional costs that regular peak hour congestion creates for the average commuter. Panel B adds client fixed effects. Panel C adds audit office-by-busy season fixed effects. Panel D adds the following additional controls to our original specifications: *IN_COUNTY* is the fraction of county residents that also work in the county they live in. *COMM_TIME* is the average time in minutes county residents commute to work. *PERC_CAR* is the fraction of county residents commuting to work by car. *POP_DENS* is a county's population density. *N_ROADS* is the number of roads in the 40-km radius. Panel E removes observations where the distance between the client headquarters and audit office is more than 40 km from our original sample. Panel F removes observations with a value of zero for *LN_ACCIDENTS* from our original sample. Continuous variables are winsorized at the 1st and 99th percentile. Bold entries represent our primary hypothesis test. Appendix A contains all remaining variable definitions. Standard errors are heteroskedasticity-robust and clustered at the client firm level. *, **, and *** represent significance levels of 10%, 5%, and 1%, respectively.


2016
ACCIDENTS
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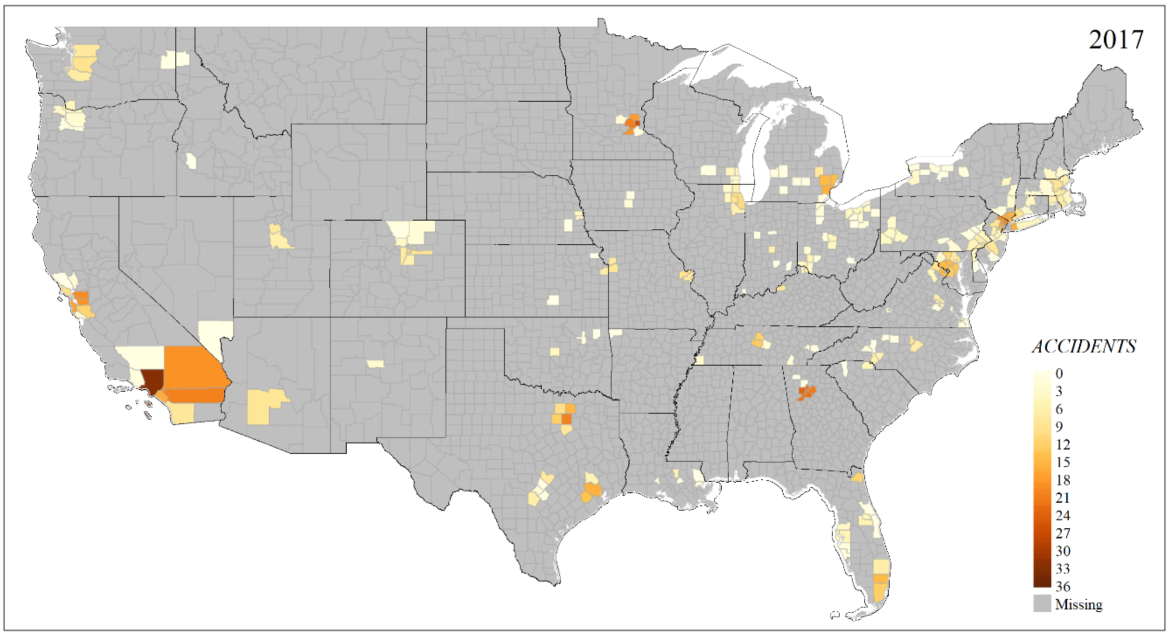
2017
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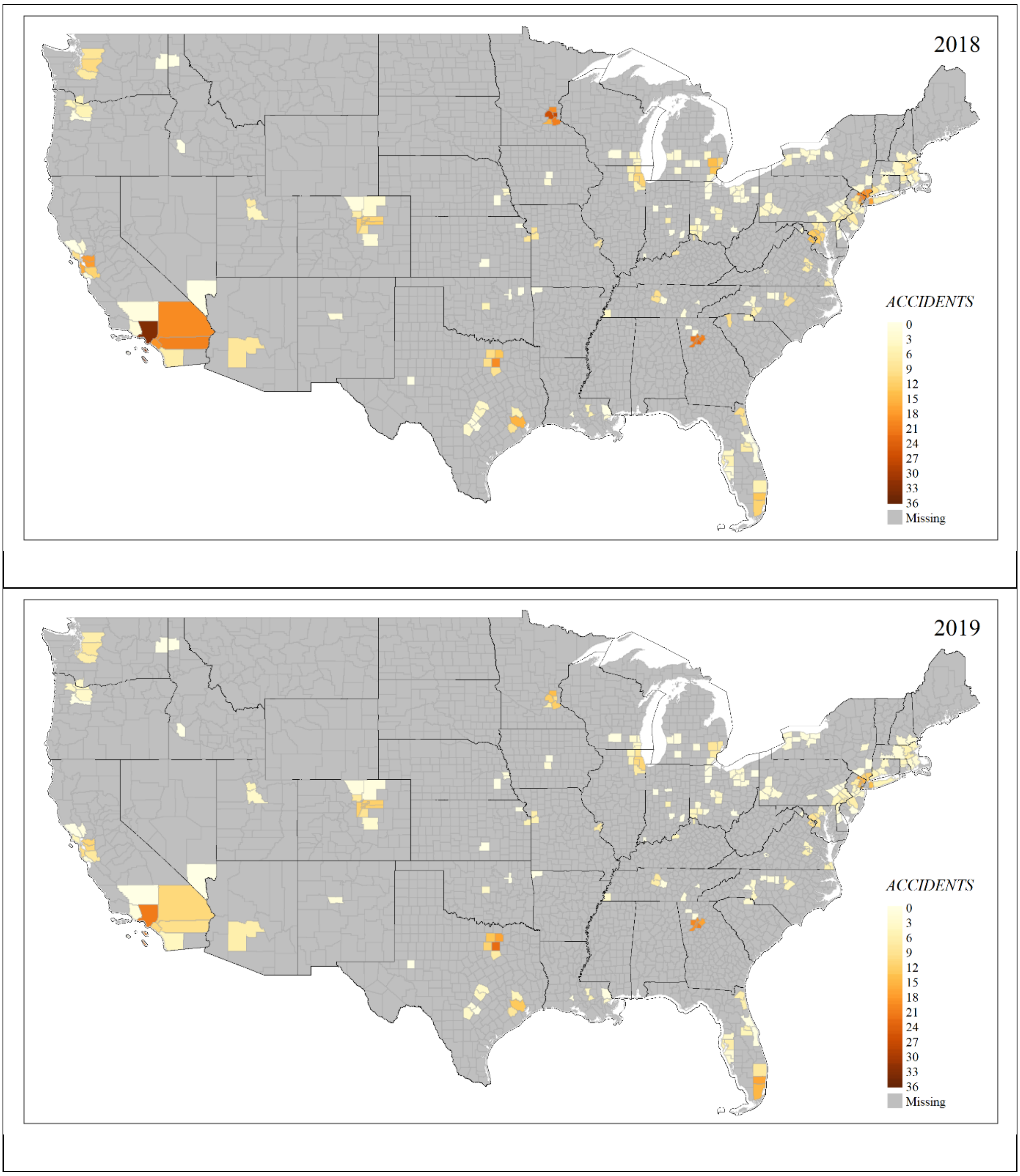

Figure 1 *ACCIDENTS* by year and by county of client headquarters from 2016-2019. When we have multiple observations for a county-year, we use the mean of *ACCIDENTS* of all county-year observations. Appendix A contains variable definitions