An Opportunity for Convergence?
Understanding the Prevalence and Risk of Distracted Driving Through the Use of Crash Databases, Crash Investigations, and Other Approaches

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ABSTRACT – A variety of methodologies for understanding the prevalence of distracted driving, its risk, and other aspects of driver secondary activity, have been used in the last 15 years. Although the current trend is toward naturalistic driving studies, each methodology contributes certain elements to a better understanding that could emerge from a convergence of these efforts. However, if differing methods are to contribute to a common and robust understanding of driver distraction, it is critical to understand the strengths and limitations of each method. This paper reviews several of the non-naturalistic methods. It suggests that "convergence science" – a more concerted and rigorous effort to bring different approaches together into an integrative whole – may offer benefits for identification and definition of issues and countermeasure development to improve driving safety.

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INTRODUCTION

"The small man... said: 'Where does a wise man hide a leaf?' And the other answered: 'In the forest.'
- Gilbert K. Chesterton (1874-1936),
Innocence of Father Brown

In the scientific effort to understand driver distraction over the last two decades, varying research paradigms have been used in an attempt to understand driver activities and their association with distraction. Key questions are: Which activities lead to distraction? What are their prevalences during driving? Which elevate crash risk? What are the mechanisms through which some activities interfere with driving? The approaches to these questions have ranged from epidemiological analyses of crash data; experiments conducted in laboratories, simulators, tracks, and roadways; naturalistic driving studies (NDS); and a variety of other methods. Unfortunately, efforts to integrate the data and results from different approaches into an overall picture have been relatively few – even though the use of converging operations has long been advocated within science as a means of achieving robust conclusions.

Indeed, convergence of different fields (and sub-disciplines within a field), has recently been

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identified as an important scientific trend – one that involves more than simply bringing together "converging operations," and one that involves more than simply bringing together experts from different disciplines. It involves an exchange of mindsets and a much more fundamental integration of approaches that were originally viewed as separate and distinct. "Convergence is a broad rethinking of how… scientific research can be conducted . . . so that we capitalize on a range of knowledge bases...," according to Phillip Sharp, Nobel Laureate, and one of the authors of an MIT expert-panel report on scientific convergence (Sharp et al., 2011).

However, there are many reasons for which studies of driver distraction within different traditions have not been more effectively integrated. Perhaps central among them is that convergence of methods is a difficult undertaking, particularly when the subject matter spans the full complexities of human behavior and human choice that are reflected during driving within the context of traffic and the larger dynamic roadway environment. Further, drivers, vehicles, and roads change over time, sometimes substantially.

When different paradigms are brought together it can be quite easy to misunderstand small methodological details that are essential for reaching appropriate conclusions (letting the leaves get hidden in the forest), and it can be similarly easy to overlook large patterns emerging from clusters of different findings (failing to see the forest that is rendered by the leaves). Such problems arise from the difficulty of finding a common vantage point from which to view

data emerging from different methods. Developing conclusions from previously separate or even divergent techniques requires careful and systematic analysis of the similarities and differences between the methods and data – and sometimes requires the development of new integrative frameworks within which to place the data and approaches.

Moreover, it has been pointed out that successful application of a convergent-operations approach typically requires careful and conscientious communication among scientists of different areas of the field(s) studied, and overcoming the fact that "researchers in different sub-disciplines often structure their research questions differently, use different language for discussing similar phenomena, have different types of controls and measures, and draw from different literatures, even when investigating similar topics" (Lickliter, 2000).

Nonetheless, within the topic of driver distraction, this paper explores whether it is feasible to take steps toward building a better understanding of the prevalence and risk of distracted driving by harnessing findings across methods and data sources of different types (crash databases, case histories, driver performance studies in laboratory and simulator, etc.). In particular, it strives to do two things: (1) explore those methods of study which can be described as "non-naturalistic" (given that another article in this issue is focused on describing "naturalistic research"), and (2) describe what would be involved in using a "convergence science" approach to integrate these "non-naturalistic" methods with "naturalistic driving studies" (and other approaches) towards a deeper understanding of driver distraction. In that effort, this paper briefly reviews different non-naturalistic sources of data and methods that are available, identifies some of their strengths and limitations, as well as the convergence opportunities that they may offer. It concludes with a description of what a convergence approach might require in this area of study.

Note, however, that convergence science is an approach that is applied to problems of such complexity that advances lie beyond the reach of any one field, let alone any one individual. One example of where it has been successful is in the new field of computational biology (which emerged from the convergence of computer science, engineering, physics, and molecular and cellular biology). In new "convergence fields" such as this one, extensive rethinking and innovation occur when the contributing fields are "joined" – and it is this fusion

of ideas and knowledge that leads to entirely new perspectives and outcomes. Therefore, given that the very notion of convergence science is to allow a *combination* of fields to *synthesize* new conceptual structures – there is no new specific framework for distracted driving that is proposed here. Rather, the argument made in this paper is that such an integrated perspective can only emerge from the joint work of many scientists bringing together differing approaches. Thus, this paper concludes by offering ideas for how a convergence effort on distracted driving might be structured, and discussing what may be required in order for forward progress to occur.

Key Concepts

Since different types of methods and data related to driver distraction will be examined in this paper, and an attempt made to discern common patterns, it is essential to clarify terminology at the outset in order to have a foundation on which to discuss findings.

Defining "Distraction." Many studies that have been done on distraction (regardless of approach) have used different definitions of driver distraction. A recent effort to develop a common definition found 55 different definitions in the published literature (Foley et al., 2013).

Regan et al. (2011, p. 1776) developed a widely-adopted definition:

> *Driver distraction is the diversion of attention away from activities critical for safe driving toward a competing activity, which may result in insufficient or no attention to activities critical for safe driving.*

While this definition is excellent at a high level, it requires further operationalization for application to the coding and analysis of data related to driving and crashes (see Foley et al., 2013). Important in this regard are questions about whether to include, as part of distraction: (a) sleepiness and fatigue, (b) driver error, (c) driving-related diversions of attention, (d) "looked but did not see," and (e) "improper lookout." The inclusion or exclusion of these areas as part of distraction can have large effects on the reported prevalence and risk of distraction. All of these can affect the magnitude of the "distraction" issue that studies report – and, importantly, can affect efforts to compare studies using different definitions.

Terminology

Some of the key terms relevant for understanding crash risk are clarified below. They come from the

field of epidemiology, as defined in the epidemiological dictionary of Porta (2008), but have been adapted for traffic safety application as noted.

Cases. In epidemiology, a *case* is defined as "a particular disease, health disorder, or condition under investigation found in an individual or within a population or study group" (Porta, 2008, p. 30). When applying this term within the context of traffic safety, contracting a "disease" can be thought of as having a "crash." Cases are thus individual drivers who crashed. (In some studies, cases are video clips of a driver and the surroundings just before a crash.)

Controls. In epidemiology, the word *control* used as a noun in the expression *case-control study* means "person(s) in a group that is used for reference in comparison to a case group." (Porta, 2008, p. 52). It refers to individuals without the disease being studied (or, for purposes of this paper, to drivers who did not crash, or video clip samples of driver behavior in baseline driving without a safety-critical incident).

Exposure refers to "the variable whose causal effect is to be estimated" (Porta, 2008, p. 89). In epidemiological studies, these typically include "environmental and lifestyle factors, socioeconomic and working conditions, medical conditions, and genetic traits." Note that exposures may be harmful or beneficial -- or even both. Exposure can also be defined in terms of the amount of a factor to which a group or individual was exposed. In traffic safety, exposure denotes a risk factor that could cause a crash. A comparison of exposures in cases and controls is often used to establish whether the exposure was a causative factor in an outcome, such as a disease or crash.

Prevalence is the measure of the number of individuals who have an attribute at a particular time or period, divided by the population at risk of having that attribute midway through the period (Porta, 2008, p, 191). An applied example would be the prevalence of a secondary task while driving, as in, "at any one time, the prevalence of U.S. drivers exposed to talking on a cell phone is estimated to be 10%." Prevalence is a measure taken at a particular moment or brief period of time; it is a "snapshot." Note that causal factors cannot be determined from prevalence alone; a comparison to an appropriate baseline control is required, from which a relative risk metric can then be estimated. For example, assume that 10% of crashes occur when the driver is exposed to talking on a cell phone (called "Talk" for short). If the prevalence of crashes during baseline periods of driving (when drivers are not exposed to

"Talk") is higher than 10%, then Talk has a "protective" effect and is correctly interpreted as preventing crashes (in the absence of any bias). If the baseline prevalence for Talk is near 10%, then Talk has no causal effect on crashes; that is, Talk and crashes are only apparently associated by chance. If the baseline prevalence of crashes for Talk is lower than 10%, then Talk is correctly interpreted as a cause in crashes. (Again, this example assumes all sources of possible bias have been eliminated.) That is, the prevalence of a risk factor just before a crash cannot be assigned either a causal or preventive role in a crash without comparison to an appropriate baseline.

Incidence is the number of instances of illness (or crashes) commencing during a given period in a specified population. More generally, it is the number of new safety-critical events in a defined population within a specified period of time. Incidence can be measured as a frequency count, a rate or a proportion (Porta, 2008, p. 124). For example, incidence is the number of drivers who have newly crashed while distracted in a specified population in a given time period. An example of an *incidence rate* is the number of fatalities in the U.S. driving population over one year.

Risk. The probability that an event will occur.

Relative Risk (RR). This term can refer to either a risk ratio or a rate ratio. The *risk ratio* is the ratio of the risk (or probability) of an event among the exposed to the risk (probability) among the unexposed. It can be calculated from the entries in a 2 x 2 table (e.g. Table 1).

Table 1. Table of Data for Computing Relative Risk

		Crash	
Risk		**Present**	**Absent**
Present (Exposed) Drinking Water		*a*	*b*
Absent (Not Exposed) NOT Drinking Water		*c*	*d*

The RR is estimated from the formula: $a/(a+b)$ / $c/(c+d)$. An example of a relative risk ratio would be the ratio of: the risk of crash among drivers drinking water while driving to the risk of crash among drivers not drinking water while driving. The *rate ratio* is the ratio of the incidence rate in the exposed to the incidence rate in the unexposed, and closely approximates the risk ratio.[1]

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[1] Individual studies may apply these definitions slightly differently. For example, the "unexposed" category may be defined in a non-standard way by an individual researcher as "not exposed

Odds Ratio (OR). The OR is not synonymous with the rate ratio or risk ratio, nor is it even a direct measure of relative risk. The exposure OR, is computed from "odds" and is commonly used in case control studies. It is the ratio of the odds of exposure among the cases to the odds of exposure among the non-cases (Porta, 2008, p. 175). "Odds" are not the same as "probabilities," though they both describe the likelihood of an event. (However, they do it in slightly different ways). For rare or infrequent events, the OR and the risk ratio can approximate one another. For more common events, the approximation does not hold. In a case-control study with incident cases, unbiased subject selection, and a rare disease, the OR is an approximate estimate of the risk ratio. With incident cases, unbiased subject selection, and density sampling of controls, the OR is an approximate estimate of the ratio of the rates in the exposed and unexposed, without a rarity assumption (see Young, 2013 for applications of the OR to traffic safety).

Rate of Use or *Frequency of Engagement.* These terms are not formal epidemiological terms – but often have been used to describe driver behavior in traffic safety studies. They refer to how often a driver (or group of drivers) may use a device – or engage in an activity – over a period of time during their natural interactions in the vehicle. Often, such measures are expressed as "rates" (e.g., usage or engagement over a time period, such as: "Drivers tuned the radio 10 times per hour, on average, during their trips in this study.") These terms (and their associated measures) would be called *incidence rates* in epidemiology. Thus rate of use -- or frequency of engagement -- is expressed as a count or frequency of activity by drivers within a given population over a time period (e.g., "drivers in the study performed grooming behaviors at the rate of 2 times per week").

Clarifying Important Concepts

Relative Risk of a Crash as a Concept. Relative risk is a risk with respect to something else. It is not the same thing as absolute risk. Relative risk can be high, and absolute risk can be low. For example, assume

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to any secondary activity – not to drinking water, nor to passenger activity, nor to device use, nor to daydreaming, nor to anything attention-diverting," although this definition causes errors in the prevalence and the population attributable risk estimates of the exposure (Young, 2013). Thus, the use of these definitions becomes critical to understanding findings that are reported, their biases, and to their correct comparison across studies.

the relative risk of a crash is 10,000 from exposure to a risk factor X. But the absolute risk is low if X is lightning striking the road directly in front of your car. Many common study designs, such as case-control studies, can only estimate relative risk; cohort studies can estimate absolute risk as well as relative risk.

Prevalence is Not Causal. As Young (2013) has pointed out:

> The science of epidemiology cautions that cause cannot be determined from prevalence alone; a comparison to an appropriate baseline control is required, from which a relative risk metric can be estimated . . . the prevalence of a risk factor cannot be assigned a causal role in a crash without comparison to an appropriate baseline.

For example, to estimate a causal relation, the number of crashes which occur while attention is diverted by doing a specific activity (e.g., eating a sandwich) must be compared to a baseline (e.g., the proportion of crashes which occur while attention is not diverted by that activity (proportion of crashes while not eating a sandwich). Even after a comparison to a control, a causal relation can still never be "proved" with the degree of absolute certainty that accompanies the proof of a mathematical theorem (Rothman et al., 2008, p. 25).

In contrast, when prevalence alone is considered (e.g., x% of drivers eat sandwiches while driving) – or even y% of crash-involved drivers were eating sandwiches), it tells nothing about whether eating sandwiches causes crashes. Eating sandwiches may be equally prevalent for drivers who do not crash as for those who do crash. Thus, prevalence alone does not inform cause. In order to know whether eating prior to a crash is causal, eating must be shown to elevate crash risk by comparing the rate of crashing-while-eating to the rate of crashing-while-not-eating. Only in this way can relative risk (and causal effects) be estimated. This concept is critical when using studies based solely on crash data, because often studies (even many published studies) imply or implicitly assume (incorrectly) that prevalence indicates a causal relationship. Estimating causality by epidemiological methods also requires the elimination of bias in the selection of the cases and their comparison to a baseline (and the appropriate choice of that baseline). Therefore, those who use a prevalence metric -- often the sole measure in survey methods for example, or the only measure available in national crash databases -- must be aware of its limitations.

METHODS FOR ASSESSING RELATIVE RISK

Estimation Methods from Epidemiology

Methods for estimating relative risk have been carefully developed in the field of epidemiology (risk ratios, rate ratios, risk differences, rate differences, etc.). The odds ratio (OR), commonly used in naturalistic driving studies, is a surrogate metric, which produces a valid estimate of a relative risk under certain conditions. An appropriate study design is needed (e.g., case-control, case-cohort, case-crossover, etc.) with careful and appropriate selection of control conditions that fit that particular design.

The relative and absolute risk of a crash in response to an exposure (such as a secondary task or drowsiness) can be directly assessed with a cohort design (Young and Schreiner, 2009). But it requires a large, expensive, and time-consuming study because all those in the population under study must be evaluated, whether they have crashed or not. A case-control design is faster and easier because the baseline data are selected only from among those who have had a safety-critical event such as a crash or near-crash. But a case-control study can only approximate relative risk, and provides no information about absolute risk. It is also subject to bias in the selection of controls (Young, 2013). The case-crossover method eliminates bias from demographic and even other factors, but it too is subject to bias in crash studies if, for example, the amount of driving in case and control periods is not matched (Young, 2012).

Describing these epidemiological methods further is beyond the scope of this paper, but interested readers are referred to any introductory epidemiology textbook (e.g. Rothman, 2012), or to Young (2013) for descriptions of some of the methods that have been used in a traffic safety context. Suffice it to say that these methods must be rigorously applied, or relative risk estimates can be biased either high or low from their true population value (i.e., they can be invalid).

Data Sources

There are several sources of data (other than naturalistic driving data) that can be used toward understanding distraction and crash risk. Each of these sources is associated with its own unique strengths and limitations. Below, a number of key sources are listed and described at a summary level. Then, some of them are discussed in more depth (to define their unique positive and negative features).

1. *Governmental Crash Databases (Other than from Naturalistic Data)*
   a. U.S. National Crash Databases (e.g., FARS, NASS-CDS)
   b. State Crash Databases
   c. International Crash Databases (Sweden, Germany, Japan, and elsewhere)

2. *Event-Based Data Sources*[2]

3. *Fixed Video-Observation of Crashes* (e.g., cameras mounted at intersections, etc.)

4. *Crash Investigation of Individual Cases* (e.g., National Transportation Safety Board, NHTSA's Special Crash Investigations Database)

5. *Insurance Claims Databases* (e.g., Highway Loss Data Institute)

6. *Driving Performance Data which Do Not Include Crashes* (e.g., many experimental studies done in simulators, on tracks, or roads)

7. *Survey and Questionnaire-Based Data which Do Not Include Crashes* (e.g., many studies of prevalence of activities done while driving are based upon self-reported questionnaires or survey responses).

8. *Naturalistic Driving Data* (While these data sources and methods are not covered within this paper, interested readers may find the following references useful in addition to the article on naturalistic data in this issue: (a) the 100-Car Data Set (publicly available to researchers), VTTI (2014), (b) the SHRP2 Dataset (which will be publicly available to researchers), (Transportation Research Board, 2014), and (c) an FHWA Workshop: Utilizing Various Data Sources for Surface Transportation Human Factors Research (Yang, 2013).

Some of the data sources above are described in more depth below. This is not an exhaustive compilation, and is limited due to space constraints.

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[2] Event-based data sources may be considered by some to be naturalistic driving study methods. However, they are described here because they are not covered in the article on "naturalistic research" that appears elsewhere in this issue.

U.S. Government Crash Databases

In the United States, the National Highway Traffic Safety Administration (NHTSA) acquires data on motor vehicle crashes, primarily from police reports, but also crash investigations. Data are compiled, de-identified, and made available through various public databases, including the National Automotive Sampling System (NASS) General Estimates System (GES), which began in 1988, and the Fatality Analysis Reporting System (FARS), which began in 1975, among others.[3] Recently, efforts from 2006 to 2011 have integrated FARS and NASS GES into a single data entry system, using standardized forms.

NASS GES. According to NHTSA documentation in the NASS GES manual (*http://www-nrd.nhtsa.dot.gov)*:

> *The NASS GES obtains its data from a nationally representative probability sample selected from the more than 5 million police-reported crashes which occur annually. These crashes include those that result in a fatality or injury and those involving major property damage. . . To be eligible for the NASS GES sample, a Police Accident Report (PAR) must be completed for the crash, and the crash must involve at least one motor vehicle traveling on a trafficway and must result in property damage, injury, or death.*

Data for NASS GES are submitted from 60 collection sites across the US. NASS GES data collectors visit approximately 400 police agencies within the 60 sites on a weekly, biweekly, or monthly basis. They randomly sample about 50,000 police accident reports (PARs) per year. They compile a list of all qualifying crashes reported since their last visit and then select a sample from these qualifying crashes. The PARs are then sent to trained personnel who interpret and code data into electronic data files. Personally-identifying information is not coded into the system.

FARS. The Fatal Analysis Reporting System (FARS) began in 1975 as a census of fatal traffic crashes within the 50 United States, the District of Columbia and Puerto Rico. To be included in FARS, a crash must involve a motor vehicle traveling on a trafficway customarily open to the public and result

in the death of a person (occupant or non-occupant of a vehicle) within 30 days of the crash. Coded data elements cover over 100 different items that characterize the crash, the vehicle, and the people involved. Each state provides data in a standard format to NHTSA.

Among the key changes starting in 2010 were the introduction of a form for pre-crash information coding in FARS (that had already been available in NASS GES), and a change to case structure (i.e., how the groups of related data elements are organized). Of particular relevance to driver distraction research is an item added to the pre-crash form for FARS, which allows coding of sources/types of distraction prior to the crash (if the reporting police officer can discern that such a distraction was present and its type). The distraction coding elements are shown in Table 2.[4] The final updates to FARS moved the two databases into a single, unified data entry system, making them compatible with the Model Minimum Uniform Crash Criteria (MMUCC), the guideline used by nearly all U.S. states to develop and revise their crash forms and databases.

Table 2. New Distraction Coding in FARS as of 2011
Coding Elements for New PreCrash Form Item16 (PC16) – Driver Distracted By:
00 Not Distracted
01 Looked But Did Not See
03 By Other Occupant(s)
04 By Moving Object in Vehicle
05 While Talking or Listening to Cellular Phone
06 While Dialing Cellular Phone
07 Adjusting Audio and/or Climate Controls
09 While Using Other Device/Controls Integral to Vehicle
10 While Using or Reaching For Device/Object Brought Into Vehicle
12 Distracted by Outside Person, Object, or Event
13 Eating or Drinking
14 Smoking Related
15 Other Cellular Phone Related
16 No Driver Present
92 Distraction/Inattention, Details Unknown
96 Not Reported
97 Inattentive or Lost in Thought
98 Other Distraction
99 Unknown if Distracted
• Format – select all that apply
• Added new remarks

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[3] The NASS Crashworthiness Data System (CDS) is an additional subpart of NASS which is specifically intended to support the investigation of injury mechanisms toward improved vehicle design. The General Estimates Systems (GES) part of NASS is focused more on providing the big picture of crashes, for use in problem assessment and trends analysis.

[4] These codes represent a large change, and one that precludes straightforward mapping onto codes used in prior years. A number of codes were added and some codes were changed or removed. Of particular note, the code for "Cellular telephone present in vehicle" was eliminated in 2010.

Strengths

- Broad and representative coverage of fatal and serious injury crashes. There are several reasons why analysis of higher-severity crashes is important. One is that for any crash-causing factors that are correlated with crash severity level (such as drowsiness), having data on fatal and severe crashes would enable such relationships to be discovered (Young, 2013). Studies containing mainly crashes of low severity may not allow such factors or relationships to be found. Crashes at the highest severity levels are rare, and so are underrepresented in naturalistic driving studies, but are well documented (particularly fatal crashes), in the government crash databases.

- Representative sampling/census. The U.S. national databases, given their breadth of coverage (and the representative nature of their sampling – or the fact that they are a complete census of fatal crashes in the FARS database), can provide a particularly robust view of the prevalence of crash types, and their association with the prevalence of types of distraction –if police are able to detect and code that such distraction occurred from interviews with the driver and witnesses after a crash has occurred.

- Coded information. There is much data coded in the multiple forms about the conditions of the crash that can be useful for generating hypotheses about contributing causes of crashes, that can then be tested by comparison to appropriate baseline data collected, for example, in an NDS (see Young 2013).

Limitations

There are several general limitations of the data contained in these databases, which depend upon information provided in PARs. Some of these limitations (drawn in part from Tijerina et al., 2003) include:

- Lack of baseline data. Within these databases there are only data on crashes, not on driving without crashes. Baseline data are needed to provide data on exposure to risks which did not lead to crash, as well as data on "not-exposed" conditions (as shown in Table 1) in order to estimate odds ratios or relative risk ratios in the effort to examine potential causes of crashes. As a result, the only epidemiological measure that can be derived is "prevalence" -- unless some method external to the government databases can

be used to obtain the baseline data that are needed to estimate the denominator of the relative risk.[5]

- Constraints on coding distraction and/or an inability to detect distraction (given that reporting occurs post-crash). There are three such constraints, described below:

  Inability to determine driver attention status. Despite the in-depth nature of crash reporting, the attention status of the driver just prior to a crash is often reported as unknown (or as "no driver present"). For example, Stutts et al. (2001) found that the reporting of "unknown status" or "no driver present" characterized a large proportion of the vehicles in their study (41.5%). For studies based on FARS, this percentage can be as high as 73% unknown (and this may in part be due to the fact that the driver may be fatally injured, or key witnesses such as passengers may be among the fatalities). This inability to determine the attention status of a driver prior to the crash still remains a major problem for assessing the prevalence of secondary tasks or drowsiness in non-NDS crash databases (except

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[5] It is for this reason that Redelmeier and Tibshirani (1997) created comparison periods with which to compare crashes preceded by cell phone use in one of the earliest studies on the relative crash risk of cell phone use. However, the information to which they had access contained only known driving for the crash periods, and no objective driving data for the baseline periods. Therefore, for comparison to the "calling and crashing period" they created a baseline based on self-reported information from drivers by defining a time period during which drivers remembered driving, but which occurred on the day or week before the crash, and on which the cell phone record said they used their cell phones. However, it is critical that the individuals were also driving at these times, and the determination of whether a person drove during the control period (and for how much of the control period) was based on the subject's self-reported memory of whether they were driving at a clock time on the control day that matched the clock time of the crash on the "crash day." Note, however, that the assumptions made by the researchers was that drivers were driving the entire time in these self-reported baseline periods. These assumptions have been challenged due to the inaccuracies of self-reported memory-based data. A subsequent study (Young, 2012) offered corrections for this bias, and found that the risk ratio for cell phone conversation is near 1, not 4 as reported by Redelmeier and Tibshirani. The differences in these estimates underscores the critical importance of having objective data sources for all cells in Table 1 that are needed for computing Relative Risk ratios, not just for cells *a* and *c*.

for blood alcohol content or drug use, which can be assessed in an individual after death). It has proven possible to infer the drowsiness state of drivers in the FARS database through the method of multiple imputation (Tefft, 2012), but it remains to be investigated whether such methods might prove useful for inferring driver distraction as well.

Crash-reporting forms offer a limited number of elements from which to choose coding of driver secondary activities (see Table 1). In part this is because there is only a small range of pre-crash behavior that can potentially be inferred after a crash has occurred (since there is no video record of what the driver was doing, as there is in naturalistic studies) – and in part it is necessary to keep coding efficient and accurate. The new coding options, while representing a major step forward, nonetheless group many types of technological distraction sources together, and omit many sources of non-device distraction. The consequence is that many attention-diverting activities cannot be recorded even if they were known. In addition, coding changed between years, with the effect that some types of distraction appear to "drop" when in fact only coding changed. This limits the safety field's ability to form a clear picture of the full range of attention-diverting activities in which drivers engage based on these types of coded data alone.

Detection of a distracted state is difficult post-crash. Even if a distraction source can be coded, it may be difficult to detect its pre-crash engagement after a crash has occurred, even for professional crash investigators. This difficulty arises for several reasons. It is only very infrequently that drivers may admit to pre-crash distracting activity, for fear of self-incrimination (Tessmer, 2000). Therefore, unless something is readily in view (triggering a question by an investigator), or unless a witness steps forward, it is difficult to establish that attention-diverting activity occurred prior to crash. The only other possibilities are even more remote: corollary evidence (such as an independent source of phone records) (Bents, 2000) or, rarely, some physical evidence of pre-crash activity that remains at the scene of crash.

- Time of Attention-Diversion vs. Time of Crash. Even if an attention-diversion can be detected and coded, its timing relative to the time of crash is usually unknown, and the exact time of the crash itself is also uncertain. Even if investigating officers determine that an epoch of distraction occurred, it is usually not possible for the officers to determine if that epoch (such as the placing of a cell phone call) – occurred before the crash, at the time of the crash, or after the crash. This timing information – which is critical to questions of causation and contributing factors – is simply not observable by investigators at the time of their arrival on the scene post-crash.[6]

These strengths and limitations are important to keep in mind when using data from state and national crash databases.

Convergent-Use Opportunities
Opportunities for using these government crash databases together with other sources of data include:

- Using the information on fatal and serious injury crashes to discover important patterns with respect to environmental, vehicle, and driver demographics, since these more serious crash types have (fortunately) been missing from naturalistic driving studies to date. Because of their rarity, these severe levels are unlikely ever to be available in sufficient quantity in NDS studies to achieve sufficient statistical power.

- Using the coded information on the various forms (Crash, Vehicle, Driver and Person) as a source for hypothesis-generation about contributing causes (and/or consequences) in distraction-related crashes (for further study and testing using other methods).

- Augmenting data from crash databases with other types of data that can assist in completing

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[6] Some studies utilize cell phone billing records in conjunction with data from crash databases. To use such records accurately requires in-depth knowledge of cell phone and telematics system operation. Unfortunately, a misunderstanding of when timing signals are recorded has, in some instances, led to inaccurate beliefs that billing records indicate information about dialing (and when dialing has occurred), though the signal-timing in the billing records instead reflects a later point, when talking is underway. This misunderstanding has been reflected in claims of some billing record studies about what their reported RRs reflect, and has led to misunderstanding of findings in some cases. See Young (2014a, b) for a more complete description.

the pre-crash timeline, such as obtaining phone billing records, and expertise on their use.

Data from Event-Based Recording Systems

Sensor-based technologies and vehicle telemetry enabled the development of systems which can be triggered to record data based upon states that occur during driving (e.g., events or forces associated with crash). These systems can thus record data useful for understanding driver behavior, including distraction and crashes. Some of these systems are embedded as telematics systems within the original equipment of vehicles (e.g., OnStar), while others are offered for after-market installation as part of research or training programs (e.g., DriveCam®, GreenRoad, and others). The capabilities of these systems vary. Embedded telematics systems may automatically send crash data to a central location if a vehicle is involved in a crash (this capability became available starting in about 1996 with OnStar, for example). In addition to automatically sending notifications of crashes, other features may be available as well. A driver may be allowed to place calls (e.g., to OnStar advisors for assistance in navigation and route following, as well as to the driver's personal call recipients) using an embedded hands-free system. When these systems are used, time-stamps are automatically placed by the cellular company (not OnStar) on all call events as well as on crash-notification calls. The time-stamps for the crash-notification calls only are also separately recorded and stored along with any additional crash information at the OnStar database. By comparing the times of the calls and crashes in these databases, accurately synchronized times are thus known for all such calls and crash notifications in the database. Hence, for OnStar, for example, a census of all calls and AACN crashes is available, and there is no need for statistical sampling. Several studies have been published which investigated the relationship between airbag-deployment crashes and in-vehicle personal calls using the OnStar embedded cellular hands-free personal calling system by comparing time-stamps for OnStar calls with time-stamps for airbag deployment calls.

After-market systems (such as DriveCam®) which are intended for research, training, or other applications, offer a different suite of capabilities. These systems may record video (as well as vehicle dynamics and driver performance measures) over a short time window of a triggered threshold (e.g., when a threshold for hard-braking is reached). The data may be stored locally – and/or transmitted for storage and subsequent access and analysis.

Strengths

- Size. The size of event-based databases can be very large – limited only by the number of vehicles equipped with the systems. For example, the OnStar database is extremely large because the system is offered on all General Motors vehicles, and thus data on crashes have been collected over a very large number of vehicle miles traveled. See Young and Schreiner (2009) for illustrative results from this database. However, if event-data recorders are installed for research projects in a field study, there may be a small sample of vehicles equipped, with each contributing data over an extended period of time (also for a large number of vehicle miles traveled). See McGehee et al. (2007) for an example of this kind of work.

- Time-stamping of crash information. Event-triggered recorders can provide accurate time-stamps for the events that they record. For example, OnStar gives exact time-stamps for crashes, which combined with cellular billing record call timing information from the cellular company, can be used to make a valid estimate of the absolute and relative risk of conversation while driving.

- Potential for cross-linking of variables stored in the database with crash information. Different types of information can be stored in the database (e.g., certain types of vehicle information, GPS location of the crash, and selected de-identified demographic data, subject to stringent privacy-protection policies). Such information may offer the opportunity for inter-correlations to add depth of understanding to key issues.

Limitations

- Not all event-based data recording systems contain video (some do, some do not) -- nor many of the other variables acquired through NDS instrumentation -- so the details of driver behavior at the time of the crash, or immediately preceding crash – may be less complete than in NDS. (Some systems do, however, provide very complete pre-crash records of behavior).

- Event-based recording systems only record epochs that are related to the triggers which are pre-identified in the systems (events or forces tied to crashes, or to vehicle dynamics which often precede crashes or near-crashes). This means that if data on baseline driving is needed

(for use in forming the denominator of the risk ratio), it must be gathered in another way.

- Event-based recorders may not provide data on the full range of secondary activities (but only those that lead drivers to exceed the trigger thresholds that initiate data recording).

Convergent-Use Opportunities

Databases generated by event-based data recorders may be privately owned.[7] Due to the sensitive nature of the data acquired by event-based recording systems, the databases may be protected, or accessible only by special arrangement through the researchers who are responsible for overseeing confidentiality and participant rights. Therefore, research and convergence opportunities with these databases may need to be explored one-by-one with careful attention to permissions and access-agreements, de-identification, and presentation of aggregate data only.

Crash Data from Fixed Video-Observation (e.g., cameras mounted at intersections, etc.)

A variety of studies in the United States and in other countries have been done using fixed observation of crashes (or driver behaviors) – often captured using video cameras installed above or near the traffic flow (e.g., on light poles, on overpasses, at roadside, etc.). In this technique, observation is done at one or more fixed locations, and all crashes (or all activities that can be seen) can be recorded. Typically, recording is done at a few carefully selected locations which offer situations of interest (e.g., high crash rates, certain crash types, or certain driver behaviors) – with the intent to study one location in depth – or to use a sample of locations to represent crashes or activity-use generally. Often, such studies generate prevalence measures for the locale or locales studied. Two (of many) examples include:

- Johnson et al. (2004) gathered information using photographs of drivers traveling on the New Jersey Turnpike. The resulting over 40,000 high quality digital photographs were examined. The presence of cell phones and other distracting behaviors was coded. Johnson et al. (2004)

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[7] The OnStar crash and demographic databases, for example, are privately owned. They are not subject to public examination, and their contents are considered proprietary to GM/OnStar, because privacy protections are required for subscribers.

reported that cell phones were the most frequent secondary behavior observed in the study.

- NHTSA utilizes observations of cell phone use as a part of its annual National Occupant Protection Use Surveys (NOPUS). For the 2004 survey, 5% of drivers were observed using hand-held cell phones, up from 3% from the 2 years prior (Glassbrenner, 2005). More recent observations (2006-2008) have placed the value at around 6% (ranging between 5% – 7%). However, although NOPUS utilizes a nationally stratified sample, its observations were made strictly during daylight hours and only when drivers were stopped at intersections (Flannagan and Sayer, 2010).

Strengths

- Set-Up. Fixed-observation studies can be relatively fast and easy to set up.

- Rapid source of insight for testing with other methods. They can rapidly generate a source of insights for hypothesis generation.

Limitations

- Limited coverage. Often, only certain locations can be covered, making it more difficult to generalize to a broader, more representative scope. Selection of locations can be subject to bias, since the behavior-of-interest may be correlated with the location observed (or the conditions which are associated with it). For example, Flannagan and Sayer (2010) point out that by observing phone use only by drivers stopped at intersections, NOPUS usage estimates may have been biased high, because of a possible driver strategy of waiting to use cell-phones until the vehicle is stopped at a stop sign or stoplight. Thus, a percentage of 6% or 7% when stopped at an intersection may reflect a (relatively) higher percentage of usage at that location, due to driver strategy -- but one that may not be representative of a (lower) usage rate characteristic of periods of actual driving.

- Restricted view of driver activity. The view of driver activities may be restricted from the vantage points available from a fixed observation point (and important information may not be visible), thus biasing the observations. Certainly a view from outside vehicles is generally more constrained than camera views inside the vehicle, limiting amount, types, and accuracy of information that can be obtained.

- Limited range of metrics. Applications may be primarily restricted to prevalence at the fixed observation points.

Convergent-Use Opportunities
- These types of studies may provide prevalence information on narrowly-scoped issues, easily observed issues, or very localized issues – if the fixed observation points can be selected without bias.

- These studies may serve as a source of hypothesis generation (that can be followed up by testing with other methods).

Case Studies: In-Depth Crash Investigation & Modeling

The National Transportation Safety Board and certain other organizations perform in-depth investigations of selected crashes, including distraction-related crashes. These detailed investigations provide case studies of crashes of particular types. The richness of detail in these case studies can provide a source of insight and hypothesis-generation on which more structured studies may follow-up when conducted on a broader scale (using other data sources and methods). Similarly, NHTSA's Special Crash Investigations Database archives in-depth investigations of crashes of certain types (e.g., backover crashes) – but at this time does not include distraction crashes.

Strengths. Crash investigations result in a rich record of data about individual crashes on a wide array of variables.

Limitations. Individual crashes may not comprise a representative sample and cannot be used to infer prevalence or be generalized to a nationally representative sample as can national crash databases. No scientific statements can be made, because these require generalizations across hundreds of cases and NTSB methods look only at single crashes. No baseline data is available so relative risk and actual crash causation cannot be determined using epidemiological methods. Possible and probable causes for a single case can be hypothesized by expert opinion, but not tested as a generalizable cause through epidemiological means.

Convergent-Use Opportunities
- Records from crash investigation cases support the generation of hypotheses about crash-causation factors.

- These cases can be an excellent source of data for modeling (using techniques such as DREAM, cf. Wallén et al., 2008; Talbot et al., 2012; Ljung, 2002).

Driving Performance Experiments (without Actual Crashes)

In the arena of driver distraction, much research has been devoted to measuring how demanding a secondary task is of a driver's resources, and whether these demands interfere in a measurable way with driving. This effort is important, and the experimental method offers several important strengths. However, there are critical considerations that must be borne in mind when considering the findings of such work, particularly if there is interest in using them toward an understanding of crash risk.

Strengths of Experimental Methods
- Careful control
- Repeatability across test participants
- Useful for measuring effects of task demand
- Source of insight into underlying mechanisms through which driving interference arises (explanatory power, theory-building)

Limitations of Experimental Methods. Such methods necessarily "induce" task performance to occur under conditions of desired measurement, and as a result, do not reflect the natural strategies, tactics, and choices that drivers make on the road regarding whether to engage in a task, when to engage (under what conditions), and so forth. As such, experimental methods cannot themselves be used to assess prevalence or risk. These methods are associated with some additional limitations, as follows.

Studying a single task versus understanding the range of tasks. When a study examines a single task in-depth (or type of task, such as cell phone conversation) and focuses exclusively on measuring the demand of that single task or task type – conclusions are often drawn about any observed performance decrements without comparing that task's effects to other tasks of different types. A narrow focus on a single task or type of task can lead to over-interpretation of its effects – even though the driving performance decrements arising from that task may be small in magnitude when compared to a range of other task effects found in other studies. Thus, focusing too narrowly on single tasks or task types may result in conclusions which differ from those that are reached from understanding them in the context of a broader range of tasks that are done during driving – and may lead to the overlooking of

broader findings and understanding that can only come from a comparison across many tasks of varying types.

<u>Use of artificial tasks, or tasks of exaggerated length or difficulty.</u> Sometimes driving performance experiments utilize tasks in the simulator or on the track/road that are not the actual tasks which drivers undertake during real driving – and they may differ in important ways from tasks actually done by drivers as part of ordinary activity. For example, experimental tasks may be designed to be much longer than their counterparts in real driving, to place more intensive demand on drivers, to require different types of mental operations, or may be based on hardware or software that is below production-quality standards. Although artificial tasks have been used as an experimental technique for some time – when they are used, artificial tasks have limited generalizability to actual tasks, unless certain precautions are taken, because they may alter the profile of how a real task affects performance. Thus, it is important to distinguish between tasks that have been designed (or altered) for experimental purposes – and real tasks -- in order to prevent misinterpretation of findings and misattribution of performance effects to real tasks.

<u>IRB protections within driving performance studies</u>. Because research experiments are governed by Institutional Review Boards (IRBs), participant safety is protected – and participants are aware that they will be at minimal risk for a crash. Given the actual protections provided, participants are not allowed to experience risk that fully represents that in ordinary driving. These facts together mean that participant behavior within a study may be altered and may not reflect behavioral choices made under natural conditions where actions can result in an actual crash, injury, or fatality. This limits interpretation and generalizability of findings from experimental studies to actual driving.

<u>Driving performance decrements cannot be generalized directly to crash risk or relative risk</u>. Performance decrements do not translate in a linear way to changes in crash risk or safety. This point has been made by many researchers, among them Stutts et al., (2005), who wrote (p. 1100):

> Another important limitation of the study is that the measures of driving performance we were able to code and analyze – hands on steering wheel, direction of eye focus, and vehicle wanderings or encroachments across travel lanes – have not been directly linked to crash risk.

The relationship of performance decrements to crash risk or relative risk depends on multiple factors beyond the presence and magnitude of a performance decrement. First, driving has elements of "satisficing" in it, where there are tolerances around idealized performance. Further, humans are adaptive, and in a system with tolerances, can adopt strategies to achieve adequate driving under many conditions, including ones in which their performance may be imperfect. In such a context, small decrements in performance may or may not have an influence on actual crash risk. For example, drivers may choose to initiate tasks under conditions where they have a safety margin for a few moments; one that they expect will (and often does) absorb a small decrement in their own performance. Or, drivers may alter their driving in order to create a safety margin for themselves (e.g., by slowing down, or delaying an interaction with a secondary task until stopped). Such strategies and behaviors act to modulate links between performance decrements in the lab or simulator and crash risk as it is observed in the real world.

Second, crash risk is not solely a function of the resource demands placed by a task on the driver, nor of the decrements in driving performance that may be observed from those demands within experimental settings. Certainly task demands, and any interference with driving performance that may result, do enter as one factor into crash risk – and are a necessary component for assessing it. But they are not the only component; and they are by no means sufficient by themselves to predict crash risk or relative risk from a task.

This point is critically important to understand in the effort to evaluate how crash risk and relative risk is affected by distraction. Other intervening variables beyond task demands influence crash risk and relative risk. Some of these intervening variables include whether drivers choose to do tasks while driving, which types of drivers undertake tasks of different types (highly experienced drivers or inexperienced drivers), how often the tasks are done, and under what conditions, and whether hazards co-occur at the same time as a distraction. The implication of this point is that, in order for a source of data to be useful for understanding crash risk, it is necessary that the data reflect more than just "exposure" to a task. Exposure to an attention-diverting task must also occur in conditions where driver choice is represented and where other traffic, pedestrians, or potential safety hazards are present and co-occurring, if the attention-diverting task is to

be determinable as a causal factor in a crash. It is within this range of choices and conditions that the strategic and tactical elements of the driving task are reflected – and these elements play a role in determining how much risk is incurred. Thus, in order for data to be useful for understanding crashes, it must be gathered under conditions in which key variables, such as these, are at play.

This point is also critical because it underscores the fact that studies which do not allow such factors to be at play (controlled experiments, for example, which request that participants perform specific tasks at specific times in pre-defined scenarios) contribute different information to the understanding of driver distraction than do observations of naturalistic behavior. The findings emerging from different types of data sources can be used together in understanding and estimating crash risk or relative risk, but only if their strengths and limitations are appropriately recognized and respected.

Convergent-Use Opportunities for Performance Data from Experiments

Driving performance data from experiments offers the opportunity to measure how much workload is imposed by tasks of different types, to study whether, and how much tasks interfere with driving performance under conditions of test (when workload becomes excessive, or competes with resource demands of driving or simulated driving). Findings of this type can suggest hypotheses for studies of crash risk (hypotheses which then need to be evaluated using other data and other methods). Driving performance studies also provide a way to develop and test theoretical models related to driver attention, driver workload, and driver performance. These are all topics that can benefit from testing under controlled conditions, with repeatability across research participants.

Survey and Questionnaire-Based Data which Do Not Include Crash

Many studies concerning activities undertaken by drivers during driving (the frequency with which they are done, the conditions under which they are done, etc.) have utilized survey and questionnaire methodologies which result in self-reported data (e.g., Tison et al., 2011). While this class of methods has an important role to play in understanding *perception and belief* – which in turn play a role in drivers' willingness to engage in activities during driving, its use for establishing the prevalence of activities – especially activities that are "remembered" and self-reported – is subject to

inaccuracies and limitations. These issues are well-documented within the literature. For example, (non-driving) studies have shown that self-reported activities correlate only at the level of between 0.12 to 0.28 with activities actually recorded on videotape and coded by multiple observes using a formal coding method (Gosling et al., 1998 and others). Self-reports are, in addition, subject to the "social desirability bias" during the response period (on a questionnaire or survey) (cf. Crowne and Marlowe, 1960). Further, as per Lee, Hu, and Toh (2000), reports may vary by frequency of engagement – e.g., low-frequency of engagement respondents may overestimate their behavioral frequencies, whereas high-frequency respondents may underestimate.

Again, questionnaire and survey studies can provide important information about driver subjective opinions: perceptions, and attitudes – and this information is important because these subjective variables play a role in driver willingness to engage in activity, and choices about when to initiate tasks during driving. However, from a scientific point of view, it is critically important to recognize the limitations on other types of self-reported judgments – namely, reports of frequency and time durations. These limitations have, unfortunately, not always been acknowledged fully or appropriately when self-reported data have been applied within the context of epidemiological studies to augment objective crash data. However, if scientists knowledgeable about these findings were included in a convergence initiative to work with epidemiologists, there would be opportunities to better harness the strengths of these methods.

**RESULTS AND DISCUSSION:
WHAT CONVERGING SOURCES TELL US**

With that review of methods as a backdrop, a high-level view of studies using non-naturalistic data and methods can be described in terms of two over-arching areas: (a) Findings related to prevalence, and (b) Findings related to relative risk of crash related to distraction. The overview described below reveals that the field has covered only certain areas of distraction, while neglecting others, and is characterized by widely varying results regarding the crash risk estimates that have been generated. Further, findings remain separate in the literature, without any framework to integrate them.

Prevalence of Distraction While Driving

Prevalence studies based on 'non-naturalistic' data. These have tended to focus selectively, have used

self-reported data, and would benefit from the application of convergence methods.

Most studies of prevalence using non-naturalistic methods have focused on cell phone use. They show a prevalence ranging from 6% to 11% depending on type of phone and year (e.g., NOPUS, observational data while drivers are stopped, using a nation-wide probability sample (NHTSA, 2011)).

Prevalence studies done on types of secondary activities *other than* cell-phone use during driving using non-naturalistic data are numerous. However, most have used surveys (and thus are based on self-reported data). Illustrative examples would be Heck and Carlos (2008) and Huemer and Vollrath (2011). These types of data can provide insights into driver beliefs and perceptions about prevalence. However, generally, self-reported data are subject to important limitations (and should be interpreted with care, in light of the limitations described earlier). Thus, this area of investigation is one which may benefit from convergence with other methods of data acquisition.

Prevalence studies of distraction-related *crashes* (vs. activities) using non-naturalistic data exist, but have not been updated in the U.S. for many years. However, the older ones (e.g. Wang et al., 1996) show the most prevalent source of distraction-related crashes was looking at events external to the vehicle and only a small percent of crashes were due to diversions involving in-vehicle devices (2 to 3 % when summed together). The studies that do exist are consistent with each other on this finding (and also with naturalistic studies, though naturalistic sources show some newer sources of distraction). A more recent study in Australia used individuals injured in crashes, coupled with self-report (McEvoy et al 2007) and it reports consistent findings for in-vehicle device involvement.

Thus, in the area of activity-prevalence, when non-naturalistic sources of data are considered together, consistent patterns often emerge. This can be seen in the findings and table below. These findings also tend to align well with findings from naturalistic driving studies, and serve as one illustration of the promise that a convergence effort might hold.

Table 3 (adapted in part from Tijerina et al., 2003) compares the rank orders of secondary activities that were coded prior to crash in two different crash database studies. One of these was by Wang, Knipling, and Goodman (1996) and the other by Stutts et al. (2001), (based on data from the Centers

for Disease Control from 1995-99). The studies used very different computational methods, but showed similar rank orderings of the top six types of secondary activities coded prior to crash in distraction-related crashes (see columns 3 and 5 of Table 3). (The magnitudes of percentages in each study are different due to the differing data sources and computational methods. It is thus the *ranks* that are important to compare between them, and which reveal a common pattern.) As can be seen in Table 3, the most prevalent source of distraction in both studies were things located outside the vehicle – and the only technology-related activities in 1996 and 2001 were adjusting radio/CD, using other devices carried into the vehicle, adjusting climate controls, and using/dialing the cell phone – *all* of which *together* totaled 1.6% of drivers who were crash-involved in the Wang et al. (1996) study – and 18.5% for Stutts et al. (2001) – less than the *single* top source of "outside vehicle"(2%, 29.4%).

Table 3. Prevalence in Crash-Involved Drivers

Type of Secondary Activity	% of Crash-Involved Drivers				% Clips with Drivers Who Crashed		
	Wang et al. (1996)	Rank Order	Stutts et al. (2001)	Rank Order	Type of Secondary Activity	Dingus et al. (2006)	Rank Order
Outside person, object, event	2.0	1	29.4	1	Out Left Window	9.7%	1
Adjusting radio, cassette, CD	1.2	2	11.4	2	Radio Adjustment + CD	0.0%	
Other occupant	0.9	3	10.9	3	Passenger in Adjacent Seat	6.9%	3
Moving object in vehicle	0.3	4	4.3	4	Animal or object in vehicle	6.9%	4
Other device/object brought in	0.1	6	2.9	5	Reaching for object-not-cell	2.8%	5
Adjusting vehicle/climate controls	0.2	5	2.8	6	Adjusting Climate Controls & Other integral controls	0.0%	
Eating or drinking	0.1	.	1.7	.	Eating or Drinking	1.4% 2.8%	
Using/dialing cell phone	0.1	.	1.5	.	Cell-Phone Locating, Dialing, Other	0.0%	
					Talking/Listening	8.3%	2
Smoking related	0.1	.	0.9	.	Smoking Related (reach, light, smoke, ext.)	0.0%	
					Singing + Talking	1.4%	
					Dancing	0.0%	
					Grooming	0.0%	
					Reading	0.0%	
Other distraction or Unknown	1.5+1.0	.	34.00	.	Other Activity** (daydreaming)	2.8%	6

**Coded by analysts as possibly daydreaming or lost in thought in which driver was haphazardly looking around but not at any single distraction.

It does not appear that any more recent analyses using the national crash databases have provided a stratification by distraction type in the US since the Stutts et al. (2001) study. [8] However, to examine

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[8] As mentioned previously, a more recent study was done in Australia, and showed consistent results (MeEvoy et al., 2007).

more current years than 1996 and 2001, it is interesting to look at a comparison of rank orders derived from a naturalistic driving study done by Dingus et al. (2006). It not only provides a window into how driver activities may have changed since 2001, but also allows an examination of how well findings from crash databases compare with NDS for prevalence.

This comparison is shown in the rightmost shaded columns of Table 3, and reveals that there is indeed some very interesting correspondence. In particular, the top source of pre-crash secondary activity among crash-clips continued to be diversions outside-the-vehicle. Further, passengers and animals in the vehicle remained among the top six most prevalent diversions prior to crashes (in the top six for the Dingus et al. (2006) NDS as well as for the earlier 1996 and 2001 studies based on crash databases). However, the activities of adjusting radio and climate controls did not show up at all as pre-crash activities in the 100-Car NDS crash clips. But in 2006, there was the emergence of talking and listening activities preceding crash, for crash-clips, though at a low prevalence. As a reminder, Table 3 shows prevalence only, and the most common activities in Table 3 should not be interpreted as causes of distraction-crashes based solely on their prevalence. (These same activities may or may not be similarly prevalent in baseline driving).

Finally, in the area of prevalence, studies based on crash databases offer some findings that are consistent with the notion that drivers make some strategic choices about *when* to engage in distraction-related activity. (This is based on analyses of the most prevalent *conditions under which distraction-related crashes arise and culminate as coded in crash databases*) (Tijerina et al., 2003). The most prevalent crash type for distraction crashes is the rear-end crash, and the highest prevalences for driving conditions among distraction crashes show that drivers tend to initiate tasks under roadway conditions that appear 'benign' during car following – e.g., where it does not appear that anything hazardous is going to happen for the next few moments of driving. The conditions coded in crash database studies identify that the conditions prevalent for distraction-related crashes are the following: daylight, level and straight roads, dry pavement, <45 mph. (Note, however, that most driving also occurs under those conditions, so this prevalence says nothing about relative risk of these conditions for distraction-related crashes.) Nonetheless, the findings on conditions of distraction-related crashes can be seen as consistent with findings from naturalistic

driving studies (see especially Sayer et al., 2005). In addition, Stutts et al. (2005) wrote (p. 1100, bracketed text added):

> ". . .drivers are choosing to engage in them [*secondary activities*] at 'safer' times on the roadway."

Some naturalistic and quasi-naturalistic studies have shown initiation of tasks under times of lower traffic density or other adaptive driving behavior. However, there is much more to be studied about the complexity and richness of the strategies and judgments that drivers make in this regard; much remains to be understood. Among the open questions are whether strategic choices vary with driver age, experience, and other demographic factors, as well as by region, types of driving scenarios (e.g., environmental factors, traffic and road conditions). These questions are ones for which several data sources could play complementary roles in framing new programs of research. They are important since they could have implications for mitigation and countermeasure approaches (e.g., especially in the arena of active safety systems). However, research projects explicitly designed to study strategic choices by drivers during driving are still few in number.

Crash Risk Studies Based on Non-Naturalistic Data

These studies have also tended to focus selectively on cell phone use (rather than on a full range of attention-diverting activities). Early attempts to evaluate crash risk for cell phone use generated high estimates of crash risk (ORs of 4) -- but were heavily criticized for having generated erroneously high estimates due to a number of methodological problems that became apparent after publication in the literature (e.g., Redelmeier and Tibshirani, 1997). A number of subsequent studies were done, which varied in their approaches to better utilize appropriate epidemiological controls and appropriate baseline comparisons in the estimation of relative risk for cell phone use. Some of them also delineated different relative risks for handheld dialing, and hands-free use of different types. Among the studies on cell-phone use are these: Hahn and Prieger, 2005, Young and Schreiner, 2009, Young, 2012, McEvoy et al., 2005. A number of these studies showed no elevation in crash risk from talking and listening on a cell phone. Other studies reported findings more similar to the original Redelmeier and Tbshirani (1997) finding.

However, when all of the studies on cell phone use are examined, the relative risks reported in the literature appear to vary widely and appear to be inconsistent with each other. They range from less than one -"just driving"- to "four-times-as-high" as driving. Not only are the estimates inconsistent with

each other, but the estimates at the high end of the range are also inconsistent with the number of cell-phone crashes recorded in the national crash databases (they predict more cell-phone related crashes than have actually been observed) (Farmer, Braitman, & Lund, 2010; Dingus et al, 2011.

Reasons for inconsistencies between studies have been identified (see Young, 2012 for a summary), but a full review is beyond the scope of this paper. However, the issues leading to these inconsistencies include differences in time estimation of crashes and calls, various forms of bias between crash and control cases, and an issue that appears to have been particularly key: the use of self-reported data for establishing baseline control epochs of driving (used in the denominators of the risk ratios). Use of self-reported driving control periods appears to have led to a biased divisor in the risk ratio (one that is too small), leading to inflated risk estimates (e.g., those estimated near 4). However, when these errors and inconsistencies are corrected, the various estimates of risk converge on a similar finding. Namely, that the risk of crash while talking on a phone during driving is not elevated above the risk of crash while not talking on a phone during driving, according to Young (2013).

While there remains controversy among researchers in this area, it appears possible that when methods are applied in a correct and uniform way between studies, the corrected relative risk estimates from non-naturalistic data may be consistent with each other, and consistent with the number of cell-phone crashes in the crash databases. Young (2013) has formulated such corrected relative risk estimates for prior studies, and the RRs lie in the range of 0.56 to 0.71 (again, this is the range of values for all studies after correction for bias, according to Young, 2013). In addition, these corrected estimates are consistent with findings from a recent naturalistic study (see Fitch et al., 2013). These values indicate that risks for talking on the phone are similar to "just driving". *However*, debate continues about the methods used to arrive at these estimates, and their corrected values, and the field as a whole remains divided. A convergence-based initiative may thus be beneficial for bringing resolution.

Estimates of *relative crash risk* (not prevalence) which are based on non-naturalistic data for activities *other than* cell-phone use are not known by the present author to be in the scientific literature. Event-based data recording studies have provided some insights into prevalence of behaviors prior to near-crash situations, but have not yielded estimates of crash risk (due to the very low frequencies of crash in those studies). Beyond difficulties associated with knowing when attention-diverting activities are occurring using non-naturalistic methods, another reason for the lack of work in this area may be the absence of baseline driving data in these databases – and the fact that there are limited other evidence-based sources from which baseline driving data can be obtained in a way that is reasonably matched to crashes in the FARS NASS/GES. For this reason, NDS studies are often used for determining crash risk (since they contain both crashes and baseline data).

How Do Types of Distraction Affect Driving Performance?
Driving performance studies have revealed that visual-manual tasks have different profiles of effects on driving than do auditory-vocal cognitive tasks. In addition, visual-manual tasks induce larger decrements than auditory-vocal-cognitive tasks, based on driving performance data (e.g., Angell et al., 2006; Shutko, Mayer, Lansoo, and Tijerina, 2009). Experimental driving performance studies have also identified the importance of more in-depth research on attention-shifting, and the control of glances between device and roadway, and in active scanning – particularly as critical (and perhaps unexpected) events are developing on the road ahead.

What A Convergence Initiative on Driver Distraction Might Look Like and Entail

As pointed out in the introduction, convergence of methods is a difficult undertaking in complex scientific fields. *Convergence science* is a new approach to scientific endeavors of such complexity that advances cannot be made within a single discipline – and which require a confluence of disciplines working together – in order for understanding to be deepened. It is not a mere swapping of ideas or methods among a few people; it is a fundamentally new model for doing cross-disciplinary scientific work. The review and analysis presented here has suggested that the area of distracted driving would benefit from a convergence science approach. As the foregoing analysis has tried to illustrate, no approach for data collection or analysis is "perfect" or provides a "gold standard" for all purposes. Each method has its strengths and limitations (including naturalistic data collection). That is why bringing together multiple approaches is important and why differences that emerge between methods are essential for moving the field forward.

The study of driving distraction and safety certainly is complex – and the extent of complexity goes beyond a level at which a mere convergence of operations can achieve success. It may require a more fundamental bringing-together of multiple fields – if progress is to be made. It is not possible for this paper – or any one person – or any one field – to offer an integrative framework at this time that will move things forward in this area. At this point, progress would appear to require a fusing of now-separate knowledge domains, so that new conceptual structures can emerge, within which to organize the findings in the literature (as well as new findings that have yet to be generated). Examples of where this has happened can be found in newly-formed convergence-science fields like computational biology (made up of several disciplines, ranging from computer science to molecular and cellular biology, physics, etc.), nanotechnology (also comprised of multiple fields), and others.

The MIT Panel (Sharp, 2011) on convergence science provided a report that illustrates how such convergence initiatives can be launched, and what they require – primarily for biomedical domains. Nonetheless, it is helpful in identifying several centers that serve as models for how convergence science can be done successfully, and examples of new fields that exemplify convergence science. Importantly, it makes the point that convergence science initiatives may require special funding and infrastructure to incentivize scientific work of the type needed. This is because currently, science is organized within-disciplines, with infrastructures set up to support scientists by discipline, and salary and reward structures established to provide incentives for scientific work by discipline. Therefore, in order to encourage cross-discipline work (rather than competition and conflict), investment of dollars, support, and infrastructure change may be necessary (such as establishing a center, a lab, or an office where a cross-disciplinary team can be co-located), as well as establishing incentives for scientists to collaborate across discipline rather than compete.

Since driving distraction involves complexities of human behavior and human choice that are reflected during driving within the context of traffic and the roadway environment, and involves the infrequent outcomes of injury and crash – knowledge from many fields is required for forward progress. Among these are disciplines ranging from epidemiology and statistics, to traffic science, to telephonic and telematics engineering, to vehicle systems engineering, to human perception, cognition, attention, and neuroscience, as well as to the psychology of choice, judgment and decision-making, to name a few. These are among the disciplines that might be brought together in an initial convergence project.

As discussed earlier, when different paradigms are brought together it can be quite easy to misunderstand small methodological details that are essential for reaching appropriate conclusions (letting the leaves get hidden in the forest), and it can be similarly easy to overlook large patterns emerging from clusters of different findings (failing to see the forest that is rendered by the leaves). Such problems arise from the difficulty of finding a common vantage point from which to view data emerging from different methods. Thus, essential to any convergence effort is a common set of goals, and an openness of initial participants to collaborative interactions. *The common goal might be to* construct a "big picture" of prevalence and risk *for distracted driving*, in which findings are placed into a larger, more integrative framework that harnesses knowledge from multiple methods.

In addition, within this new framework new studies of prevalence might be defined that could be undertaken. These should cover the full range of activities that drivers do in the vehicle (not just cell phone use), and should address which types of drivers undertake them, as well as the conditions under which they tend to be initiated during driving. Further, the data should be gathered through a means of objective data recording, rather than through self-report.

In terms of crash risk, new studies would also be beneficial for advancing countermeasure development and clarifying policy refinement. However, new crash risk studies should take care to: (a) Characterize crash risks for a *broader range* of attention-diverting activities than cellphones; (b) Use precisely recorded and properly synchronized times for time-of-crash and time-of-secondary-task-engagement (rather than estimates); (c) Use objectively recorded or objectively verified baseline driving epochs as "control periods" in the denominator for computation of risk ratios (rather than self-reported or self-remembered periods of driving time that occurred hours, days, or months prior to crash), (d) Utilize a broad sample of drivers and conditions, (e) Apply epidemiological designs and techniques for controlling bias, and for exploring driver factors, condition factors, and other complexities of risk estimation.

CONCLUSION

Findings about distraction from crash databases, crash investigations, epidemiology, and related methods -- currently appear to be inconsistent and varying – but often converge when examined with care. When findings between methods appear to conflict, it is often due to methodological details having been overlooked between paradigms – or larger patterns missed, in the consideration of results – both of which are common issues in the integration of different scientific approaches. However, as research continues on driver distraction, it will take concerted effort within these related disciplines to continue to integrate findings across methodologies and data sources to achieve the goal of a deeper understanding of what is distracting and what is not, and how to mitigate distraction when it is an issue. Convergence science is recommended as a new approach that may facilitate forward progress in this area.

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