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Terrorism, Dread Risk and Bicycle Accidents.

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Abstract

Following the airplane attacks of September 11th, 2001 it is claimed that many Americans, dreading a repeat of these events, drove instead of flying, and that, consequently, there were extra car accidents, increasing the number of fatalities directly caused by the attacks by 1,500. After the Madrid train bombings of March 11th, 2004, Spaniards, like Americans, avoided the attacked mode of travel, but no increase in car travel or fatal accidents resulted. Here we analyze behavioral concomitants of the July 7th 2005 bomb attacks on public transport in London. We find reduced underground train travel and an increase in rates of bicycling and, over the 6 months following the attacks, 214 additional bicyclist road casualties - a 15.4% increase. Nevertheless we found no detectable increase in car accidents. We conclude that, while fear caused by terrorism may initiate potentially dangerous behaviors, understanding the secondary effects of terrorism requires consideration of the environmental variables that enable fear to manifest in dangerous behaviors.

Keywords: public transport, decision making, risk perception, road accidents

Gigerenzer (2004, 2006) published evidence that the attacks in the USA on September 11th 2001 caused substantially greater loss of life than had been noticed via a secondary psychological *dread risk* effect. People dread, and are especially averse to, situations where many may be harmed or killed at one point in time, compared to situations in which a similar or even greater number may be harmed or killed, but distributed over a longer period (Slovic, 1987). Gigerenzer, observing a decrease in air travel in the wake of the attacks, and a simultaneous increase in road traffic claimed that some people, seeking to avoid a dread risk, switched from flying to driving. Calamitously, because driving is more dangerous than flying (Myers, 2001), the switch resulted in more fatalities than would have occurred if people had continued with their previous pattern of travel. Gigerenzer (2004) estimated the increase in road deaths in the three months following September 11th 2001 was 353 - exceeding the total number of passengers killed on the four hijacked airplanes; subsequently Gigerenzer (2006) reported an increase of 1,595 in traffic fatalities over the twelve months following the attacks and attributed this increase to a dread risk effect.

Gigerenzer's (2004 & 2006) analyses compared the number of road fatalities for the months following the attacks with the numbers for the five preceding years, but did not report any tests of statistical significance. Evidence that travel by road increased was limited to comparison with the figures for the period eight months before and twelve months after the attacks – but again no tests of statistical significance were reported. Motivated in part by the lack of tests of statistical significance in Gigerenzer's analyses, Su, Tran, Wirtz, Langteau & Rothman (2009) conducted more rigorous statistical analyses of the data.

Consistent with Gigerenzer's (2004) descriptive findings, one way ANOVAs revealed that while the number of pre-September passenger flying miles did not vary by year, the number of U.S. domestic passenger flying miles in the post-September months was significantly lower in 2001 than in 1999 and 2000. However Su et al. found no evidence to

support the second element of the dread risk effect - a significant increase in miles driven or of a significant increase in traffic fatalities after September 11th.

Gigerenzer (2004) reported a 2.9% increase in total U.S. driving miles in the post-September months of 2001, compared with the post-September months of 2000, and contrasted this increase with the 0.9% increase in total driving miles during the pre-September months of 2001, compared with the same months in 2000. However, Su et al. noted the presence of historical trends in U.S. driving such that, from 1970 through 2004, for a given month relative to that same month in the previous year, driving miles increase on average by 2.91%; hence the 2.9% increase in total driving miles between October and December of 2001 reported by Gigerenzer (2004) was quite close to expectations. Moreover, as total U.S. driving miles in the post-September months of 2001 did not differ significantly from total U.S. driving miles in the same months in 1999 and 2000, Su et al. rejected Gigerenzer's claim that there was a notable increase in driving miles in the United States after the September 11 attacks. Consequently any change in the number of driving fatalities could not be attributed to the dread risk effect – that is a switch from flying to driving.

Su et al.'s statistical analyses also revealed that while the number of fatal traffic accidents in the United States was somewhat elevated during the last 3 months of 2001, there was no significant overall increase in traffic fatalities. Su, et al. also reported statistical analyses to test for a regional increase in traffic fatalities in the post-September months of 2001; citing survey evidence that proximity to the attacks was associated with a greater stress reaction (Schlenger et al., 2002) they hypothesized that stress induced by geographic proximity to the attacks would decrease driving quality.

Su et al.'s analyses divided the United States into three geographic regions and, consistent with their hypothesis, found that traffic fatalities increased in the post-September months of 2001 but only in the north-eastern portion of the United States – the region where

the attacks occurred. As there was no corresponding significant increase in the amount of driving in the Northeast during that period, and also as the number of alcohol- or drug-related traffic citations increased during the last 3 months of 2001, again only in the Northeast of the USA, Su et al. argued for a different causal account of the increased traffic fatalities: the traumatic effect of the attacks increased psychological stress, which in turn impaired driving ability and predisposed individuals to fatal traffic accidents.

Critically for the dread risk hypothesis this interpretation places the locus of the behavioral consequences of the USA attacks on involuntary stress rather than on volitional choice. Although Su et al.'s account of the increased fatalities attributes them to dangerous behaviors induced by fear, it does not imply any misguided attempt to avoid risk through voluntary decisions – specifically a switch in mode of travel. This matters because, while interventions to reduce these effects would of course be just as important, different sorts of intervention may be appropriate to counter stress rather than dread risk.

Gaissmaier and Gigerenzer (2012) responded to Su et al.'s (2009) paper by conducting a more fine-grained analysis of driving data. Analysing data across each of the 50 states plus Washington, D.C., for the years 1996 to 2001 they found that the average monthly increase in miles driven per inhabitant in the post-September months of 2001 (27.2 miles) was substantially (and significantly) larger than the average increase observed in the previous 5 years in those same months (9.9 miles); while for the pre-September months, the average monthly changes in miles driven in 2001 was non-significantly lower than the average monthly changes in the 5 previous years. Moreover while increased driving was weakly associated with proximity to New York City, where stress reactions to the attacks were previously shown to be greatest (Schuster et al., 2001), it was much more strongly associated with driving opportunity, operationalized either as number of highway miles per inhabitant or as number of car registrations per inhabitant. Increased driving, in turn, was the

best predictor of increased fatalities; Gaissmaier and Gigerenzer's analysis of state-by-state variations in changes in fatalities after the attacks showed that fatalities increased with changes in miles driven, but not with proximity to New York City. Although traffic fatalities increased in the Northeast, substantial increases also occurred in many states more distant from New York City, which, by virtue of their number of highway miles per inhabitant and/or number of car registrations per inhabitant, afforded greater opportunity for increased driving¹. Thus, Gaissmaier and Gigerenzer (2012) claimed a secondary impact of the terrorist attacks - which increased fatalities - was attributable to both varying fear of dread risks and variance in the environmental conditions conducive to increased driving instead of flying.

López-Rousseau (2005) investigated whether any similar effect occurred following the March 11th 2004 bomb attacks on four trains in Spain which killed 191 people and injured 1,755. The effect was smaller and shorter than the American reaction following September 11th 2001; train travel reduced for just two months following the bombing. However, there was no corresponding increase in road traffic; in fact, there was a decrease. Accordingly traffic fatalities also decreased and no secondary dread risk effect was observed.

Why were reactions to the attacks on the civilian populations of the two countries different? One suggestion is the socio-political differences between the countries: López-Rousseau points out that, unlike the USA, Spain had been exposed to decades of bomb attacks on public targets so one attack may produce a smaller alarm reaction and less of a dread risk. A possible reason why any - albeit reduced - reaction did not result in secondary deaths is that Spain has less of a car culture than the USA; Spaniards may not be so willing or able to transfer from train to car when cars are less available and public transport is more available.

¹ Baron's (2004) analysis showing evidence for a stronger upturn in fatal accidents involving out of state drivers, compared to fatal accidents involving in-state drivers, is also consistent with the notion that there was a switch from flying (commonly involving interstate travel) to driving.

In light of these observations we conducted an analysis of the effects of the 2005 bomb attacks in London. On July 7th 2005 52 people were killed and 700 were injured in four separate explosions caused by bomb attacks on public transportation in London. Bombs exploded on three underground trains and on one bus. We focused our analysis of secondary effects on London – the location of the bomb attacks.

Prompted by press reports of large increases in bicycle sales immediately following the London attacks which suggest that some switching from public transport to bicycles may have occurred (Muir, 2005) we examined variations in bicycle travel and bicycle accidents. People switching from underground trains to bicycles could cause a secondary effect: just as car travel is riskier than plane travel (Myers, 2001), bicycling is more dangerous than traveling by underground train. Between 1997-2006 an annual average of 16 fatalities and 3,452 injuries were recorded for London cyclists. Despite far more traveling by underground than bicycle (1.1 million underground passengers and 18,000 bicyclists travel daily into central London), between 2000-2004 there was an annual average of six fatalities and 119 injuries on the underground. However note that most underground fatalities are attributable to suicides or follow deliberate trespass on the line (Transport for London, 2007). Only five underground train accidents causing passenger deaths have occurred since it opened in 1863. Litman (2005) reports that, for London in 2004, the number of deaths per billion trips was Underground (9.44); Bus (4.89) and Bicycle (152.7).

We also analyzed London car accident data but couldn't detect any increase in car accidents following the London attacks. Like Spain, the UK has less of a car culture than the USA - particularly in London. Of those working in central London, 80% travel to work by public transport (bus, rail or underground); only 11% travel by car - compared with 76% in the rest of Great Britain (Transport for London, 2007). For many Londoners driving is not feasible: cars were charged (£8 - US\$14 - at the time of the attacks) - for entering central

London during weekdays and parking is scarce and costly. Nonetheless bicycling may be one option for some public transport users.

Method And Results

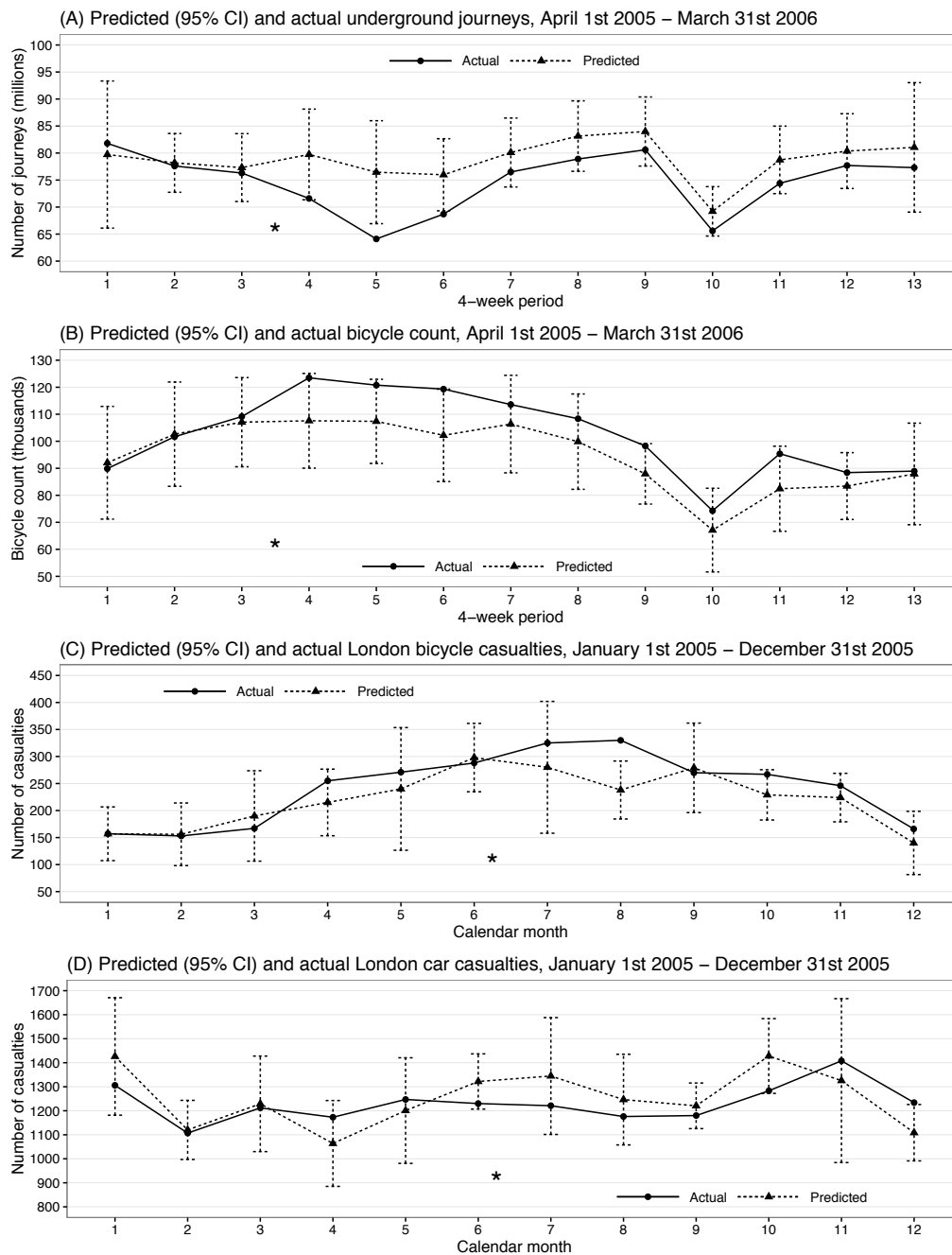
To test for a secondary effect we analyzed we analyzed four data sets: recorded London underground passenger journeys, a London bicycle survey, London car accident casualties and London pedal-cyclist casualties. The underground passenger data and bicycle survey were supplied to the authors on request by Transport for London - the organization responsible for public transport in London. The road casualties datasets, compiled from reports made by police attending accidents, are available from the UK government website (<https://data.gov.uk/>).

Recorded underground passenger journeys were listed for each of 13 four-week periods for 11 years (starting April 1st, ending March 31st) for the years 1995-1996 to 2005-6. We computed thirteen regression models – one for each of the 13 four-week periods – regressing passenger count onto numerical year using data for the 10 years prior to the attacks (1995-1996 to 2004-5). Each model fitted well (mean $R^2 = 0.75$) to the historical counts of underground passengers. Together these regression analyses modelled intra- and inter-annual variance and, by substituting the year of the attacks into each of the obtained regression equations, generated predictions for the year of the attacks that we could compare with the observed counts of underground passengers for the year of the attacks (April 1st 2005 – March 31st 2006). Predicted and observed counts are shown in the upper panel of figure 1. We performed a similar analysis on the London bicycle survey data. Whilst this survey does not purport to be representative of London-wide bicycle levels it does represent bicycle levels on the “red route network” - 580 km of strategic road (5% of total road length in London carrying 33% of traffic by volume) managed by Transport for London. As such, fluctuations in this survey could be seen as indicative of fluctuations in bicycling levels in London.

Regression models fitted well to historical counts of bicyclists for each of 13 four-week periods (mean $R^2=0.92$) for the years from period 13 of 1999-200 to period 8 of 2008-2009 excluding the year of the attacks (2005-2006); this gave us models based on eight years for periods 1-8 and period 13 and seven years data for the remaining periods. We included data for years following the year of the attacks due to the relatively small number of years of data available for the years preceding the attacks and in order to get good fits to the regression models. Note that, while the years following the attacks could conceivably reflect any impact of the attacks on levels of bicycling, any upturn in those years would make any upturn in the observed levels of bicycling in the year 2005-2006 harder to detect as a disparity between predicted and observed.

As with the counts of underground passengers, the regression models modeled intra- and inter-annual variance and provided predictions for the year of the attacks that we could compare with the observed counts of bicyclists for the year of the attacks (April 1st 2005 – March 31st 2006); predicted and observed counts are depicted in the second panel of figure 1.

The July 7th attacks occurred nearly half way through period 4 of 2005-6. The top panel of figure 1 shows that the observed number of underground journeys was close to the predicted number for the first three periods of the year (April 1st – June 24th) but below the predicted number from period 4 of 2005-6 through to period 13 (June 25th 2005 – March 31st 2006); for period 6 (August 21-September 17 2005) observed journeys are outside the 95% interval. The underground system operated a reduced service for a period after the attacks but was fully operational by August 4th 2005 (Transport for London Press Release, 2005) so disruption to service cannot account for this observation. Simultaneously, observed numbers of cyclists (second panel figure 1) closely match predictions for the first three periods but are noticeably above predictions from period 4 and for the remainder of 2005-6; for period 6 the observed count is above the 95% confidence interval.



* marks the temporal location of July 7th 2005

Note: The predicted points for each period (month or 4-week period) are based on separate linear regression models – one for each period - regressing the time series of historic annual data onto numerical year to infer the slope and intercept in the historic data so as to extrapolate a prediction for the predicted year.

Figure 1: Predicted and observed numbers of underground train travelers, cyclists, bicycle accident casualties and car accident casualties in London.

To further test how the numbers of underground journeys and numbers of bicycle journeys changed at the time of the attacks we tested the relative accuracy of the forecasts of the regression models for the time periods before and after the date of the attacks. For underground journeys the discrepancy between predicted and actual was significantly greater for the nine periods after the attacks than the three periods before (Before: predicted = 235.2 million; before observed = 235.7 million; After predicted = 788.8 million; after observed = 733.2 million) $\chi^2(1) = 504,247$, $p < 0.001$. The models *over*-predicted underground journeys after the attacks; across the nine four-week periods after the attacks there were 55.6 million (7.1%) fewer underground journeys than expected. For bicycle journeys the models *under*-predicted bicycle journeys after the attacks; across the nine four-week periods after the attacks there were 98,638 (10.6%) more than expected. The discrepancy between predicted and actual was significantly greater for the nine periods after the attacks than the three periods before (Before predicted = 301,743; before observed = 300,682; after predicted = 932,168; after observed = 1,030,806) $\chi^2(1) = 1,249$, $p < 0.001$.

We analyzed bicycle casualties by again using regression models for historical counts for the ten calendar years 1995-2004 to generate predictions for the year of the attacks. As the bicycle casualty data were collated by calendar month per calendar year rather than by the thirteen four week periods used in the other data sets, we analyzed the data accordingly. The third panel of figure 1 shows that bicycle *casualties* (injuries including fatalities) increased in July, and were above the levels predicted by the regression models we fitted for each of the twelve calendar months (mean $R^2 = 0.75$) for all but one of the months following July (September was a marginal exception); August casualties exceeded the model's 95% confidence interval. As the July 7th attacks occurred almost exactly half way through the year we compared the differences between predicted and actual numbers of bicycle casualties in each half of the year; casualties surpassed predictions significantly more during the second

half of 2005 (predicted = 1390; observed = 1604) than the first half (predicted = 1256; observed = 1291), $\chi^2(1) = 4.597$, $p = 0.032$, indicating 214 (15.4%) additional casualties.

To investigate the link between changes in bicycling frequency and changes in bicycle accident frequency we computed the correlation between the historical variations in our measures of bicycle journeys and bicycle accidents. Across the 88 consecutive four-weekly periods from March 2nd, 2000 to June 9th 2007 the correlation was only modest - $r(86) = 0.24$, $p = 0.026$. However, over that same time period, bicycle journeys increased substantially yet, despite this, bicycle accidents declined somewhat: while bicycle journeys correlated positively with time measured in four-week periods, $r(86) = 0.83$, $p < 0.001$; bicycle accidents correlated negatively albeit non-significantly, $r(86) = -0.20$, $p = 0.06$. Presumably some factor(s) exogenous to the relationship between bicycle journey frequency and bicycle accident frequency affected the bicycle accident rate over time as their partial correlation, controlling for the time variable, revealed that the underlying relationship between bicycle journeys and bicycle accidents was much stronger, $r(85) = 0.66$, $p < 0.001$.

Our regression analyses of car accident casualties are depicted in the bottom panel of figure 1; regression models for each calendar month fitted well to historical data for the five years from 2000-2004 (mean $R^2 = 0.86$) and provided predictions for 2005. As is evident from the figure, observed car accident casualties do not show any noticeable increase over predicted levels; indeed, levels of car accident casualties appear somewhat *below* the predicted levels for the second half of the year. Nonetheless, and in contrast with the pattern for bicycle casualties, the discrepancy between predicted and observed car accident casualties in the second six months of the year (predicted = 7674; observed = 7502) was not significantly greater than the discrepancy observed in the first six months of the year (predicted=7362; observed = 7275), $\chi^2(1) = 0.21$, $p = 0.65$.

As a final point for this section we note that while our method of using separate regression models of historic time series data to generate predictions for each period (month or 4-week period) in the datasets analyzed here might seem unduly elaborate, it offers clear advantages over simpler methods suggested by one reviewer of this paper such as taking the average of previous years. As noted above, along with the seasonal variations within each year, there are also long term trends over the years that would generate mispredictions if a simple averaging approach was used. For example, the annual count of bicycle accidents over ten years in the data we analyzed shows a long term downward trend – for each month there is a strong negative correlation of number of accidents with increasing numerical year – (Pearson's r between -0.758 and -0.967, mean = -0.86). Our analyses show that, for each month, there is a strong downward linear trend, and no evidence for a deviation from linearity (see supplementary materials) which also confutes the possibility raised by an anonymous reviewer that nonlinear trends in the historic data for bicycle accidents might account for our observations.

Discussion

Our analyses establish a behavioral pattern consistent with the dread risk effect. Although the pattern is for all casualties not just fatalities, evidence of a secondary adverse effect of the London attacks is discernible – though not in terms of an increase in car accident casualties. Immediately following the attacks of July 7th, Londoners simultaneously reduced their travel on underground trains by 7.1% and over the same period increased their travel on bicycles by 10.6% resulting in an increase in the number of bicycle road accident casualties; in the second half of 2005 bicycle accident casualties were 15.4% higher than predicted. While the usual caveats of correlational data apply, this pattern of change is consistent with a dread risk effect – that, in the wake of the attacks, some people switched their travel from

underground trains to bicycles and that, as a consequence, the London bomb attacks caused a mediated secondary casualty toll estimated at 214 additional casualties.

A question arises as to why the measures of secondary effects following attacks on public targets are as variable as they appear to be. To wit, no secondary effect on casualties was measurable in Spain following the 2004 Madrid attacks (López-Rousseau, 2005) but an effect was reported following the 2001 attacks in the USA (Gaissmaier & Gigerenzer, 2012) and a discernible effect of the London attacks is reported here for bicycle casualties but not car casualties. Doubtless many factors could potentially account for varying reactions to different attacks. As mentioned it is possible that experience of attacks may reduce the dread risk reaction (López-Rousseau, 2005; Yechiam, Barron & Erev, 2005). Familiarity with such attacks may result in reduced dread risk, however the study into the effects of the attacks in Spain did not report any measures of dread (such as a survey of public attitudes) and other factors could account for the differences. For example, if economic, social and geographical circumstances are such that air travel is more discretionary and substitutable by driving for American airplane passengers than train travel is by driving for Spanish train travelers, we need not assume that experience of attacks reduces dread. Like Spain, the UK (including London) had experienced decades of bomb attacks and we were able to detect a behavioural pattern consistent with a secondary effect in London – albeit not in terms of an increase in car accident casualties. This nuanced pattern, while hardly probative, fits Gaissmaier & Gigerenzer's, (2012) notion that an environmental structure that enables fear to manifest in dangerous behavior is critical for dread risk effects.

Yechiam et al. (2005) investigated the impact of the prolonged wave of attacks in Israel known as Al-Aqsa Intifada where attacks were targeted toward specific civilian targets in each month that led to fatalities between September 2000 and October 2003. These authors suggested that while attacks create phobia-like responses, experience with the environment

could provide a therapeutic-like process that reduces the long-term effects of terrorism.

Yechiam et al. reasoned that, during the Intifada, Israelis had to decide between a safe alternative (e.g. “stay at home”) and a riskier alternative that exposed the decision maker to a small probability of being attacked. Yechiam et al. argued that most occasions when local residents went to a public place (walking in the street or sitting in a café), implying selection of the risky alternative, usually resulted in a “good” outcome – no attack – which would reinforce the tendency to participate in similar activities in the future.

Consistent with this notion Yechiam et al. found that Israeli hotel visits by tourists from abroad sharply decreased but that hotel visits by native Israelis hardly reduced during the Intifada. This might reflect, as they suggest, reduced dread in those exposed to risk without suffering bad outcomes compared to those tourists from abroad who avoided exposure to the risk and therefore did not obtain any positive experience from surviving it. However Yechiam et al. also had no direct measures of dread and, as they note, the cost of avoiding a risky area is much higher for local residents than to potential visitors from abroad who, presumably, in many cases, can simply select a different vacation location. For many Israelis the risks of a hotel visit when they already live in a threatened area were very marginal. Accordingly it is possible that dread was not diminished in the manner suggested.

Critically for understanding their implications, the observational studies in the USA, Spain, Israel and London are essentially correlational; lacking experimental controls they do not exclude alternative explanations for the observed associations between attacks and public behavior. Consequently caution is needed before inferring any particular causal account. For example, it is possible that the observed reduction in plane travel observed in the USA in the wake of the September 11th 2001 attacks was not due to dread at all but to aversion to the

inconvenience that the increased security arrangements² brought to air travel (see Blalock, Kadiyali, and Simon, 2007). Although in the present case it is difficult to see any rival causal account for the observed pattern - for example, in the wake of the London attacks there was no corollary inconvenience caused by increases in security for traveling on the London underground - we cannot establish conclusively that dread effects were responsible for a switch – or even, strictly, that any people at all actually “switched” travelling from underground to bicycle.

One putative defence of the rationality of a switch from public transport to bicycling is that cycling confers health benefits that outweigh the risks from higher exposure to air pollution and traffic accidents. While there is evidence that this may be true in the Netherlands (de Hartog, Boogaard, Nijland & Hoek, 2010), there is no evidence on the equivalent position in London where both air pollution (see de Hartog et al, 2010) and traffic accident risk (Pucher & Buehler, 2008) are considerably higher. Nevertheless, irrespective of any cost benefit analysis, any *switch* to bicycling triggered by the bomb attacks would somewhat undermine the health benefits defence of the rationality of the human response to risks: why wait for a terror attack before evaluating the options for getting fit?

Another reservation that deserves consideration is the idea that although switching to riskier forms of transport appears maladaptive, this evaluation relies on the benefit of hindsight inasmuch as the attacks might have heralded a change in the actuarial landscape; if attacks had subsequently recurred to a significant extent then such transport switches might not have been so ill advised. Indeed, 14 weeks after the September 11th 2001 attacks in the USA, there was a failed attempt to blow up an American passenger airliner in midair and two

² Regulatory arrangements are one way in which dread risk could influence institutional policies that stimulate maladaptive switching in individuals – for example, following the Hatfield rail crash in October 2000, the UK rail regulator introduced speed restrictions on trains causing significant delays on many routes which some claimed led to frustrated rail passengers switching to riskier road travel causing additional deaths (The Economist, 2000).

weeks after the July 7th attacks in London there were four failed attempted bomb attacks, on one day, in London on public transport that mirrored the pattern of the July 7th attacks in that three were on underground trains and one was on a bus. However, as it turned out, in both cases, there were no repeat events to cause a sea change in the statistical risks.

The question of how to respond to unusual events has parallels with the question posed by Paul Meehl in his paper “When shall we use our heads instead of the formula?” (Meehl, 1957). Acknowledging that there will be cases where so-called “broken leg cues”³ can invalidate actuarially based predictions, Meehl argued that, nevertheless, the occasions when people should use judgment to overrule statistics should be “very very seldom” (p. 273). Yet, and contrary to Meehl’s prescription, abundant evidence indicates that people very readily eschew statistics in favor of judgment based on more individuated features of events. Exemplary cases include, the strong tendency to discount historical data when forecasting (e.g. Kahneman & Lovallo, 1993; Buehler, Griffin & Ross, 2002); the curiously incurious interest in the statistical probabilities of winning shown by even experienced gamblers (e.g. Wagenaar, 1989; Clotfelter & Cook, 1990) and people’s widespread ignorance of statistical risks - for instance, Gigerenzer (2006) argued that few people will be aware that the probability of losing one’s life is about the same for driving 12 miles by car as for a nonstop flight from Boston to Los Angeles (Sivak & Flannagan, 2003) - will all usually be to the detriment of decisions under risk.

Claims that, in response to the risk of terror attacks, the vast sums spent by governments and other institutions on aviation security and other counter-measures could have been spent far more effectively (cf. Linos, Linos & Colditz, 2007; Stewart & Mueller,

³ Meehl (1954) explained that no sensible sociologist with a successfully cross validated statistical model, predicting a 90% likelihood that Professor X would visit the cinema on a particular night, would stick with the model if he learned that Professor X had just broken his leg.

2008; 2013) suggest that dread risk may also adversely drive institutional policies to the detriment of many individuals. For example, Stewart & Mueller's (2008) cost-benefit analyses of the increased US homeland security expenditure following the 9/11 attacks estimated that the annual cost per life saved ranged from \$64 million to \$600 million, far above the regulatory safety goal (societal willingness to pay to save a life) of \$1–\$10 million per life saved.

Finally we note that, while the evidence for dread risk effects in the wake of terrorism is correlational, experimental studies of decisions under risk show that risky choices depend on affective reactions (Loewenstein, Weber, Hsee, & Welch, 2001). Specifically consistent with dread risk effects, Rottenstreich and Hsee (2001) show, for “affect-rich” rather than “affect-poor” events, more pronounced overweighting of small probabilities and less sensitivity to probability variation (though see Klein et al., (2018) for a contrary finding). Assuming terrorism evokes emotion, this pattern of “probability neglect” (Sunstein, 2002) could at least contribute to dread risk reactions to terror attacks. Further analyses of behavioral consequences of the discretionary choices people are free to make in reaction to dreaded events may reveal other secondary effects.

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