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Aggression and Sleep: A Daylight Saving Time Natural Experiment on the Effect of Sleep Loss and Gain on Assaults

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Aggression and Sleep: a Daylight Saving Time Natural Experiment on the Effect of Mild Sleep Loss and Gain on Assaults

ABSTRACT

Objectives. The purpose of this study was to test the effect of a mild, short-term sleep loss/gain on assault rates.

Methods. Using National Incidence Based Reporting System data and city-reported data from Chicago, New York, Philadelphia, and Los Angeles, we calculated the difference in assault rates on the Monday immediately following Daylight Saving Time (DST) as compared to the Monday a week later using a Poisson Quasi Maximum Likelihood Estimator model. The same analyses were performed to examine effects of the return to standard time in the fall. We employed several falsification checks.

Results. There were 2.9% fewer (95% CI: -4.2%, -1.6%, $p < 0.0001$) assaults immediately following DST, when we lose an hour, as compared to a week later. In contrast, there was a 2.8% rise in assaults immediately following the return to standard time, when an hour is gained, as compared to a week later (95% CI: 1.5%, 4.2%, $p < 0.0001$). Multiple falsification analyses suggest the spring findings to be robust, while the evidence to support the fall findings is weaker.

Conclusion. This study suggests that mild and short-term changes in sleep do significantly affect rates of assault. Specifically, there is support for the theory that mild sleepiness possibly associated with an hour loss of sleep results in reduced assaults. This contradicts the simple inverse relationship currently suggested by most of the correlational literature. This study and the mixed findings presented by experimental studies indicate that measurement variability of both sleep and aggression may result in conflicting findings.

Key words. aggression, antisocial behavior, assault, Daylight Saving Time, sleep

INTRODUCTION

Low quantity and quality of sleep is increasingly thought to be a causal factor in antisocial behavior and aggression (Kamphuis et al. 2012). The evidence surrounding the relationship between sleep and antisocial behavior has been derived primarily from small samples in experimental settings involving significant sleep deprivation (Cote et al. 2013; Cote et al. 2014), and via self-reports of sleep, affect, and antisocial behavior (e.g., Raine and Venables 2017). Although there are exceptions, including studies on children using teacher and parent reporters (Chervin et al. 2003; Fallone et al. 2005). This quasi-experimental study uses an exogenous shock to sleep to measure the effects of mild sleep reductions and increases on quantifiable measures of antisocial behavior (i.e. assaults), using a larger and more representative sample than previously studied. We exploit the switch to Daylight Saving Time (DST) in the spring and the return to standard time in the fall as a mild exogenous shock to sleep to test whether even one night of a mild change in sleep affects rates of assault on the Monday after a switch. As noted in more detail below, although the switch occurs early Sunday morning, the literature generally agrees that effects are most acutely felt on Monday, when inflexible business and school hours enforce a specific wake time. As a secondary issue, cross-disciplinary research has called into question the energy-saving premise of the DST policy and revealed significant unintended effects of DST, both negative and positive. Consequently, there is ongoing legislation in several states surrounding the policy (Victor 2016). This study aims to provide more knowledge about a costly outcome (assault), which would be of relevance to policy makers, researchers involved in policing and corrections, and the general public.

Background

Prior research indicates that DST affects quantity and quality of sleep and that changes in sleep can alter physiological pathways that, in turn, affect aggression.

Sleep and Antisocial Behavior

A relationship has been proposed between poor sleep quality and anger, short-temperedness, delinquency, and impulsive aggression (Catrett and Gaultney 2009; Kamphuis et al. 2012; Lindberg et al. 2003). The research thus far has focused largely on antisocial or psychiatric populations (Ireland and Culpin 2006; Lindberg et al. 2003) and healthy individuals with consistent sleep disturbances (Coulombe et al. 2011; Granö et al. 2008; Gregory and O'Connor 2002) or short-term extreme sleep deprivation (multiple hours) (Christian and Ellis 2011; Cote et al. 2013; Vohs et al. 2010). Despite the varied methodology, subject pools, and operationalization of both sleep and antisocial behavior, a relationship between poor sleep and antisocial behavior has been found in cross-sectional (Kamphuis et al. 2012) and longitudinal studies (Gregory and O'Connor 2002; Raine and Venables 2017), and studies of both adults (Granö et al. 2008; Shin et al. 2005; Taub 1977; Vaughn et al. 2015), and children and adolescents (Backman et al. 2015; Becker, Langberg, and Evans 2015; Catrett and Gaultney 2009; Chervin et al. 2003; Clinkinbeard et al. 2011; Kamphuis et al. 2012; Meijer et al. 2010). It is important to note that many of these studies considered the consequences of long-term poor sleep and were reliant on self-report measures.

Of particular relevance to this study, and as noted by a narrative review (Krizan and Herlache 2016), direct tests of the results of short-term significant sleep deprivation on a number of antisocial outcomes using experimental methods have reported mixed findings. One study (Kahn-Greene et al. 2006) deprived subjects of sleep for 55 hours and then measured aggression and hostility using fill-in-the-response vignettes. They found that subjects were more likely than controls to direct blame and/or hostility towards others, and to be unwilling to alleviate a conflict by accepting the blame (Kahn-Greene et al. 2006). A contradictory study (Cote et al. 2013) observed *reduced* reactive aggression (as measured by a Point Subtraction Aggression Paradigm task) in sleep-deprived men as compared to controls, and no relationship between sleep deprivation and aggression in women after 33 hours of sleep deprivation. A study by Vohs et al. (2010) examined the effects of sleep deprivation on reactive aggression as measured by volume of noise chosen by the participant to be blasted at their opponent in a game. In reporting null results, they were unable to show support for effects of sleep deprivation on aggression in either direction. Finally, Haack and Mullington (2005) assessed affect every day for 12 days in an experimentally sleep deprived group and a control group, and found that sleep restriction did result in higher self-reported anger/aggression, but effects were not seen until a few days of consistent sleep restriction. Moreover, effects were reversed upon a single day of sleep recovery. These experimental tests suggest that the consequences of short-term sleep deprivation are still poorly understood, and results may vary significantly by methodology. Because of these experimental findings, we propose competing hypotheses as to the effects of DST and standard time switches (and by extension, a single hour of sleep loss or gain) on rates of assault. That is, for each switch, assault rates on the following Monday could plausibly increase *or* decrease.

Possible Pathways Between Sleep and Aggression

Less sleep, more aggression?

One plausible pathway through which poor sleep may lead to increased antisocial behavior could be through impaired neurocognitive functioning or sleepiness and subsequent low self-control. There is supporting evidence for each of these relationships and for some of the relationships in sequence, even if the entire sequential pathway has not been explicitly tested.

First, studies involving both sleep disordered (Fulda and Schulz 2001) and healthy populations (e.g. Kronholm et al. 2009; Nilsson et al. 2005; Sadeh, Gruber, and Raviv 2002; Touchette et al. 2007) have linked poor sleep quality to impairment in a number of cognitive functions. Some studies identified regions of interest a priori, and found sleep quality to be associated with cognitive functions which draw specifically from emotion-processing (e.g. Baum et al. 2014; van der Helm, Gujar, and Walker 2010) and executive functioning parts of the brain (e.g. Cote et al. 2014; Rossa et al. 2014; Williamson and Feyer 2000).

Second, employing self-control (for example, to inhibit emotional responses stemming from the limbic system) draws on a limited pool of mental resources (namely executive control functions centered in the prefrontal cortex), and when that pool is lessened, for example by sleep, self-control may be impaired until the pool can be replenished (Hagger et al. 2010). Finally, the last pathway, between low self-control and increased antisocial behavior, has been formalized in the well-supported, seminal Self-Control Theory (Evans et al. 1997; Gottfredson and Hirschi 1990). An inability to regulate one's impulses and immediate desires has been associated with antisocial behavior and delinquency (Hay 2001), with convergent findings across a number of samples and methodologies (Cheung and Cheung 2008; Gibbs and Giever 1995; Vazsonyi et al. 2001). In short, both psychological and criminological literatures have provided supporting evidence for a causal chain that could link poor sleep quantity/quality and antisocial behavior.

Less sleep, less aggression?

In addition to correlational studies suggesting a simple inverse relationship between sleep and aggression, there is some experimental research documenting the opposite relationship—

more sleep deprivation resulting in reduced reactive aggression (Cote et al. 2013). Cote et al. (2013) used a sample of 49 undergraduate students and the Point Subtraction Aggression Paradigm to look at the effect of 33 hours of sleep deprivation on aggression. Predictably, the sleep deprivation subjects self-reported more negative mood than the controls. However when it came to actual behavioral aggression, sleep deprived women stole at the same rate as the controls, and sleep deprived men stole less than the controls. Cote et al. (2013) proposed that lessened sleep may result in increased physiological and cognitive responsiveness, but decreased behavioral aggression. Additionally, as sleep deprivation disrupted the relationship between testosterone and reactive aggression in the male subjects, Cote et al., (2013) hypothesized that the reduced testosterone in the sleep deprived subjects may play a role in the observed reduced reactive aggression.

Vohs et al. (2010) also used undergraduate students to examine the effects of 24 hours of sleep deprivation on reactive aggression (operationalized as level of volume blasted at an imaginary opponent as punishment). In explaining unexpected null findings, Vohs et al. (2010) concluded that rather than sleep deprivation causing aggression, it might be methodological issues and the oft-associated self-regulation depletion driving the general consensus in the correlational literature. They also noted the benefits of measuring behavior as opposed to intention, as was seen in earlier experimental studies (Kahn-Greene et al. 2006). It could be that when presented with hypothetical situations, individuals will rely on lay beliefs as to how they, a sleep deprived person, should and would respond. Indeed, when looking at how healthy individuals deal with a mild decrease in sleep quantity/quality, it may be that, despite an increase in irritability and negative affect, sleepiness and lethargy reduces the likelihood of them acting on their aggressive impulses.

Daylight Saving Time, Sleep, and Cognitive Functioning

Anticipating significant differences in assault counts as a result of the reduced sleep caused by DST requires first that the shift to DST results in decreased sleep. Previous research conducted on DST supports this core assumption (Barnes and Wagner 2009). Using a large sample from the American Time Use Survey conducted by the Bureau of Labor Statistics, Barnes and Wagner (2009) found DST to be associated with a self-reported decrease of 40 minutes of sleep from Sunday to Monday. Proposing DST effects on assault also assumes that the amount of sleep lost is enough to affect cognitive functioning and energy. Research surrounding DST does indicate that even the loss of one hour can result in sluggish cognitive functioning as measured by performance on the SAT (Gaski and Sagarin 2011). Employing methods so as to focus on the loss of sleep, specific connections have been drawn between DST and a resulting rise in car accidents (Harrison 2013), stock market losses (Kamstra, Kramer, and Levi 2000; Pinegar 2002), work-place injuries (Barnes and Wagner 2009), the workplace use of the internet for personal reasons or "cyberloafing" (Wagner et al. 2012), reduced test scores (Gaski and Sagarin 2011), crime due to changes in ambient lighting (Calandrillo and Buehler 2008; Doleac and Sanders 2015), and even suicide rates (Berk et al. 2008). These studies have demonstrated that the sleep effects of DST are sufficiently strong for these researchers to have statistical power to detect relationships between sleep and a variety of outcome measures. We note that many of these studies have focused on the transition to DST without examining the switch back to standard time. As a result, while the literature suggests that DST (and a loss of an hour of sleep) has an acute negative effect on cognitive functioning, there is less evidence to support the inverse.

Fall Shift to Standard Time and Sleep

The popularly held belief is that we lose an hour of sleep in the switch to DST, but gain an hour in the return to standard time in the fall. At least one study using a nationally representative sample partially supports this hypothesis; Barnes and Wagner (2009) found a lesser but still highly significant loss of 40 minutes associated with the advent of DST. On the other hand, Harrison (2013) notes in a review that there is no strong evidence that the fall shift results in gaining an hour of sleep. This is also supported by Barnes and Wagner (2009), who found a nonsignificant difference in sleep duration after the fall shift. Furthermore, there is evidence that the adjustment back to standard time is easier and completed faster than its DST counterpart (Kantermann et al. 2007). In accordance with a general lack of consensus and literature, we allow for competing hypotheses as to the effect of the switch back to standard time on assaults.

This Study

This study uses the exogenous mild shocks of DST to assess the effect on crime of a short-term, and mild, reduction in sleep time. The advantages of this methodology as compared to earlier sleep/antisocial research is that it uses a representative and large sample of the United States and looks directly at a real world outcome of interest, assaults, as opposed to self-reported aggression or simulated antisocial behavior in an experimental setting. Considering that most adults have adjusted their bed and rise times within one week post-switch (Harrison 2013) we expect to see significant effects of the policy on crime isolated on the Monday immediately following switches (as compared to one week post-switch). We note that by focusing on a one-time loss or gain of a single hour, our study diverges from the existing sleep literature. Additionally, our outcome (assaults severe enough to warrant police involvement), is a highly valid measure of aggression, as opposed to laboratory tests employed in previous studies. Thus, while we do look at a similar cause, sleep, it is difficult to ascertain how our findings will compare to previous findings.

Broadly, our research question is: Does a small change in sleep duration result in increased aggression? Specifically, we will assess whether 1) the rate of assaults is affected by a potential one hour loss of sleep on the Monday following the transition to DST, as compared to the Monday one week after, and whether 2) the rate of assaults is affected by a potential gain of one hour of sleep on the Monday following the return to standard time, as compared to the Monday one week later.

METHODS

Data

Due to a need to examine data at a daily level, we used publically available data from the National Incident-Based Reporting System (NIBRS) from 2001 to 2014, the only national crime database with a daily level of detail. NIBRS is an incidence-based reporting system used by local, state, and federal law enforcement agencies. Small jurisdictions are disproportionately represented in NIBRS. To provide a more representative view of the effects of DST, we combined these data with city-reported publicly available data from the cities of Chicago, Los Angeles, New York, and Philadelphia. These cities provide data on an incident-level, with the same requisite variables (date, incident type) as the NIBRS data, however they do not participate in NIBRS. The NIBRS database provides detailed crime data in the form of incident counts for 22 Group “A” offenses, including aggravated assault and simple assault. For each analysis, we summed both types of assault to provide an overall measure of assault (using all assaults that occurred during the 24 hours). Using the combined NIBRS and city-reported data, the number of observed assaults used in the main analyses in the spring and fall is n = 60,333 and n = 62,546, respectively.

While the switches to and from DST actually go into effect at 2 a.m. Sunday morning, the aforementioned literature around DST suggests that the effects are not felt until Monday, when traditional business/school hours preclude sleeping in to compensate (Varughese and Allen 2001). Accordingly, we compared the Monday after the transitions to and from DST with the Monday a week later (rather than the Monday before) because it best controls for a) daylight, b) day effects, and c) any possible seasonal effects. We essentially expected those two Mondays to be, on average, very similar in terms of weather and lighting (as opposed to the Monday prior to the switch, when there is an additional hour of sunlight or darkness during typical activity hours). This methodology has been used in prior Daylight Saving Time literature (Wagner et al. 2012). Like Doleac and Sanders (2015), we discarded data for years when holidays fell on one of the days of interest. For example in Fall 2005, Halloween, which may artificially inflate crime rates, coincided with the Monday directly following DST. Likewise, we discarded the data for Spring 2008 and 2014, when St. Patrick's Day fell on the comparison Monday (the Monday one week following). Because NIBRS includes information on the age and gender of the offender, we also looked at potential moderating effects of age and gender through the use of interaction terms. Counts of assaults per year are presented in Table 1.

Table 1. Number of assaults on Mondays immediately after and one week after transitions to and from DST

	Spring		Fall	
Year	Monday immediately following start of DST	Second Monday after start of DST	Monday immediately following end of DST	Second Monday after end of DST
2001	1797	1827	1673	1593
2002	1652	2015	1671	1625
2003	1695	2032	1732	1951
2004	3597	3512	4019	3458
2005	2396	2355	Not Included Due to Halloween	
2006	2624	2766	2645	2430
2007	2681	2556	2397	2432
2008	Not Included Due to St. Patrick's Day		2639	2215
2009	2780	2740	2525	2642
2010	2618	2623	2480	2481
2011	2574	2836	2551	2750
2012	2799	2882	2473	2420
2013	2406	2570	2442	2259
2014	Not Included Due to St. Patrick's Day		2501	2542
Total	60,333		62,546	

Statistical Model

We estimated the effect of DST using a Poisson Quasi Maximum Likelihood Estimator (QMLE) regression model, fit separately to the spring data and the fall data. We preferred a Poisson QMLE model over a negative binomial model because it is, on balance, more efficient and robust (Wooldridge 2010).

$$y_{itd} \sim \text{Poisson}(\lambda_{itd}) \text{ and } \log(\lambda_{itd}) = \beta d + \alpha_i + \xi_t$$

where y_{itd} is the number of assaults reported in city *i*, in year *t*, on Monday *d* (where *d* = 1 is the Monday immediately following the switch and *d* = 0 is the Monday one week after that). α_i represents a city fixed effect and ξ_t represents a year fixed effect. Of primary interest is β, where $e^{\beta} - 1$ is the estimated fraction increase in crime immediately following the change to/from DST relative to the Monday one week later. This method of comparing the Mondays is consistent with previous literature on the effects of DST (Coren 1996; Wagner et al. 2012), although we note that we did not make any comparisons using the Monday prior to DST, so as to best isolate the effects of sleep as opposed to changes in daylight hours and ambient lighting.

In order to look at gender and age of the offender as potential moderators, we calculated interaction terms using dichotomous and categorical variables, respectively. The discretized age into three categories, 14-25, 26-40, and 41 and older.

Robustness Checks

We tested the robustness of our findings using four falsification checks. First, we took advantage of the 2007 switch in the advent and end of DST legislated in the federal Energy Policy Act of 2005. From 2007 on, DST was extended by one month, by shifting the start from the first Sunday in April up to the second Sunday of March, and shifting the end from the last Sunday of October to the first Sunday of November. We reversed the definitions, coding days before 2006 using the post-2007 definition and days after 2007 using the pre-2007 DST definition. We expect no effect since no DST changes occurred under this flipped coding definition. If the Monday coefficient is nonsignificant, this robustness check supports the theory that sleep, as opposed to weather or another non-DST factor, is driving the effects. Second, we compared the Monday one week post-switch to the Monday two weeks post switch, expecting to see no significant difference between the two, as people seem to adjust their sleep patterns by that point (Harrison 2013). Our final two robustness checks compared other weekdays (i.e. Wednesday and Thursday) immediately following the switch to their corresponding day the following week. Wednesday and Thursday best balance our interest of looking at weekdays and looking at days in which DST would have lessened effects as compared to Monday. We expected to see at least weakened if not null effects, as it seems plausible that people will not have adjusted to the switch entirely by those days, but will be less affected than on Monday. We wished to include in our falsification checks an analysis of Arizona, which does not observed DST, but we could not compile a sufficient number of assaults from NIBRS or localities.

RESULTS

Spring Analyses: Potential One Hour Loss of Sleep

We examined whether we find an effect of the potential loss of an hour of sleep in the spring transition to Daylight Saving Time. Table 2 shows that, on average, the potential one hour loss of sleep was associated with a decrease in assaults. There were 3% (95% CI: -4.3%, -1.6%, $p < 0.001$) fewer assaults on the Monday immediately following DST as compared to the Monday one week later. There was no moderating effect of gender ($p = 0.48$) or age ($p = 0.56$ and $p = 0.34$).

Table 2. Estimates of the Effects of Spring Changes to Daylight Saving Time on Assault, including Falsification Tests

Primary Hypotheses	
% increase in assault (95% CI)	-2.9%(-4.2%, -1.6%)
p-values	< 0.001
n	60,333
Falsification Analyses	
Reverse Coding	
% increase in assault (95% CI)	0.6% (-0.7%, 2.0%)
p-values	0.37
n	58,547
Monday one week and two weeks after DST	
% increase in assault (95% CI)	0.3% (-1.2%, 1.7%)
p-values	0.71
n	59,443
Wednesday immediately following and one week after DST	
% increase in assault (95% CI)	-0.3% (-1.5%, 1.0%)
p-values	0.62
n	67,997
Thursday immediately following and one week after DST	
% increase in assault (95% CI)	-1.2% (-2.4%, 0.0%)
p-values	0.05
n	68,658

The spring robustness checks (also shown in Table 2) generally resulted in null findings. The reverse coding check, reversing the changes stated in the Energy Policy Act of 2005, was nonsignificant (p = 0.39). Comparing the Mondays one week and two weeks after DST when any effect of DST should wear off was also not significant (p = 0.66). We also found no effect when analyzing Wednesdays (p = 0.62), though we found a marginal effect when looking at Thursdays (p = 0.05), though this finding is not significant if adjusted for the number of falsification tests. These non-significant falsification tests are in striking contrast to the strong finding from the analysis of the correct labeling of Mondays.

Fall Analyses: Potential One Hour Gain of Sleep

The return to standard time in the fall and potential gain of an hour of sleep was associated with an increase in assaults as shown in Table 3. Indeed, we found what amounted to almost a mirror image of our spring findings, as there were 2.8% (95% CI: 1.5%, 4.2%, $p < 0.001$) more assaults immediately following the switch as compared to the following week. There was no moderating effect of gender ($p = 0.33$) or age ($p = 0.19$ and $p = 0.63$).

Table 3. Estimates of the Effects of Fall Changes to Standard Time on Assault, including Falsification Tests

	Primary Hypotheses	
	% increase in assault	2.8% (1.5%, 4.2%)
	p-values	< 0.001
	n	62,546
	Falsification Analyses	
Reverse Coding		
	% increase in assault (95% CI)	0.8% (-0.6%, 2.2%)
	p-values	0.28
	n	58,966
Monday one week and two weeks after DST end		
	% increase in assault (95% CI)	2.1% (0.7%, 3.4%)
	p-values	< 0.005
	n	60,740
Wednesday immediately following and one week after DST end		
	% increase in assault (95% CI)	4.5% (3.1%, 5.8%)
	p-values	< 0.001
	n	62,875
Thursday immediately following and one week after DST end		
	% increase in assault (95% CI)	2.1% (0.8%, 3.5%)
	p-values	< 0.005
	n	59,051

Reversing the Energy Policy Act of 2005 coding indicated no effect ($p = 0.28$). Unlike our analysis in spring, analyses of other days following the return to standard time showed effects on the same scale as the primary analysis. The comparison of the Mondays one week and two weeks after the return to standard time, the comparison of the two Thursdays following the return to standard time, and the comparison of the two Wednesdays following the return to standard time showed significant increases in assaults (in all cases $p < 0.005$).

DISCUSSION

This study leveraged the exogenous short-term shock to sleep provided by the Daylight Saving Time policy to test whether a potential loss or gain of an hour of sleep results in increased aggression. The springtime results were strong and robust and showed that a mild and short-term reduction in sleep quantity results in *fewer* assaults (as compared to a week later). This finding supports lack of motivation/energy due to sleepiness rather than reduced self-control as the effect of mild short-term sleep loss. Collectively these tests indicate that there is something particularly special about the Monday following DST that causes the observed decrease in assaults, suggesting that the 3% decline in assault is likely due to DST and its effect on sleep.

On the other hand, while the fall finding showed a corresponding finding—a mild and short-term sleep gain results in increased assaults—findings are not robust to the falsification analyses as demonstrated by significant findings in almost all of those tests. This weaker finding is consistent with the lack of evidence that the fall switch results in any discernable sleep gain (Harrison 2013), which may explain the previous studies that have examined solely the springtime shift, and ignored the fall switch (Kotchen and Grant 2011; Wagner et al. 2012). It is is possible that the results are simply more long-lasting in the fall, but it seems unlikely considering most of the DST literature suggests less of a significant change in sleep in the fall than in the spring (it is easier for people to phase delay than phase advance) (Kantermann et al. 2007). Indeed, it could also be that people are getting more sleep, but increased motivation/energy alone is not a risk factor for increased aggression.

Sleep loss has wide-ranging effects on the body including physiological (Van Cauter et al. 2008), cognitive (e.g., Sadeh, Gruber, and Raviv 2002) and mood (Cote et al. 2013) alterations. This study suggests it also has distinct behavioral consequences. Unfortunately, our methodology restricts our ability to provide a causal explanation for the relationship between mild sleep loss and assaults; thus, we cannot provide insight into the biological pathways through which one hour of sleep might cause the observed reduction in assaults. It could be that the lethargy induced by DST results in a reduction in motivated behavior, particularly that which requires significant physical outlay (e.g., assaults). Nevertheless, recent longitudinal research indicates that daytime drowsiness at age 15 is associated with increased crime at age 23 (Raine and Venables 2017). This contrary finding further suggests the need to differentiate the effects of limited sleep loss on short-term violence from the effect of more systemic sleep disruption and drowsiness on longer-term offending.

These findings and similar mixed findings in the sleep deprivation experimental studies coalesce to suggest that there is more nuance to the sleep-antisocial literature than those suggested by correlational studies. Neither the correlational nor the few experimental studies in the sleep literature are easily comparable to a one-time loss or gain of a single hour, which we aimed to operationalize here. Our findings suggest that a mild loss of sleep may result in lethargy or reduced motivation and thus reduced assaults. The methodology of this study contributes in a novel way to the sleep-antisocial behavior literature.

Findings have potentially important implications. This study demonstrates that the relationship between quantity/quality sleep and antisocial behavior may be more nuanced than suggested by the current literature (Kamphuis et al. 2012). Indeed, the mixed findings of the experimental literature suggest the importance of future research carefully considering how to measure sleep and aggression (Cote et al. 2013; Haack and Mullington 2005). It seems plausible that one hour loss as opposed to 24 hours, or 33 hours, or multiple hours over multiple days, will have differential effects on behavior. Moreover, Vohs et al. (2010) note that differential findings in the experimental literature may be attributable to the variable methods that have been used to measure aggression, e.g., attributing blame in a vignette vs. a computerized "game." By using a significant and criminal level of aggression that requires physical exertion and some degree of motivation, this study supports the suggestion put forth by Cote et al. (2013) regarding the possibility that sleep loss may influence behavioral responsiveness very differently than it does cognitive or physiological responsiveness. The advent of DST could result in a reduction in motivation levels, accounting for a decrease in assaults despite an increase in negative mood.

With regard to criminal justice policy, our findings suggest that the crime-reducing effects of DST go further than the ambient lighting effects suggested by the findings of Doleac and Sanders (2015) and Calandrillo and Buehler (2008). Their findings focus on the fact that DST results in more ambient lighting at a high-crime time. We found decreased assaults immediately following DST, without robust findings suggesting a corresponding increase in the switch to standard time. Both of these findings are in agreement with regard to positive unintended effects of DST on crime. Because of the ongoing and competing bills surrounding DST in various state legislatures, it is important that legislators have as much information as possible about the unintended consequences of DST, particularly those related to crime. However, a 3% reduction in assaults one day out of the year has a negligible impact on the volume of assaults. A 3% decrease in assaults on one day per year reduces the nation's assaults by about 330, a small fraction of the 800,000 aggravated assaults and 3.2 million simple assaults reported in the 2015 National Crime Victimization Survey. Therefore, crime effects as a result of the hour of lost sleep should not factor into policy-making on DST, although we refrain from concluding the crime effects of DST in general are insubstantial (e.g., Doleac and Sanders 2015).

There are some limitations to this study. First, in four of the 12 years, the Monday immediately after the start of DST had a higher number of assaults, but these counts are consistent with our estimated year to year extra-Poisson variation. Nevertheless, unaccounted-for events on just the right dates could explain the results, though these events would need to have broad impact since NIBRS includes data on assaults from a wide geographical area. Second, while the spring findings were generally strong and robust to falsification checks, the evidence surrounding the effects of the fall switch are far weaker. It seems possible that weather effects may be more significant in the late fall, when early snow storms and falling temperatures can be expected, particularly in three of the four cities included in the analyses (i.e., New York City, Chicago, and Philadelphia). This makes some sense given the generally significant declines from week one to week two, regardless of the day, in the fall falsification analyses. Routine activities theory (Cohen and Felson 1979) would suggest that winter weather could impact assaults during both work hours (if a storm causes businesses to close) and after-work hours (if individuals choose to stay at home) by keeping possible victims and assailants from interacting. Nevertheless, the reverse coding robustness check, which is arguably the best robustness check as it uses days that could plausibly be the relevant days, found no effect.

In taking advantage of city-reported and NIBRS data, advantages of this sample include its size and broad coverage of the United States. The disadvantages of these data include our inability to examine and control for various factors, such as criminal records and sleep durations. Our assumptions around the sleep effects of the switches were therefore based on prior literature (e.g., Barnes and Wagner (2009), but could not be confirmed in this sample.

Though the evidence suggests that the loss of sleep is the likely cause of the increase in assaults, it is possible that people, unaffected by sleep quantity or quality, simply alter their activities immediately following DST in such a way that could change the likelihood of both assaulting and being assaulted (e.g., staying home instead of going to a bar), and return to their habits a week later.

The mild exogenous shock to sleep provided by the DST policy allowed us to look at a large and representative population to examine the effects of a short-term loss of sleep. The advent of DST specifically is thought to be responsible for 40 minutes of lost sleep, if not more (Barnes and Wagner 2009). DST already has shown itself to generate useful natural experiments for researchers to examine car accidents (Harrison 2013), stock market losses, work-place injuries, cyberloafing, reduced test scores, crime due to changes in ambient lighting, suicide rates, and racial profiling (Grogger and Ridgeway 2006). In this paper we have harnessed this natural experiment generator again to learn about sleep and assault on a large national dataset. While the bulk of the literature has suggested a simple inverse relationship between sleep quantity/sleep quality and aggression, our findings are more in line with the mixed findings presented by sleep deprivation experimental studies. Indeed, the measurement of sleep and aggression appear to provide the differences. It seems possible that while mild loss of sleep may indeed induce aggressive feelings or negative affect, the associated sleepiness and lethargy impedes physically acting on those feelings.

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