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ORIGINAL ARTICLE

Sample size bias in the empirical assessment of the acute risks associated with daylight saving time transitions

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ABSTRACT
The assessment of the acute impact of daylight saving time (DST) transitions is a question of great interest for an understanding of the benefits and inconveniences of a practice that is now under public scrutiny in Europe and America. Here, we report a thorough analysis of a record of 13 well-known research studies that reported increased risks associated with DST transitions in health issues – acute myocardial infarction, ischemic strokes and trauma admissions – and in societal issues – accidents, traffic accidents and fatal motor vehicle accidents. We found that a five percent increase of the risks suffices to understand the reported increased risks associated with the spring transition. Reported values above this threshold are impacted by the sample size of the study. In the case of the autumn transition, no increase in the risks is found.

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Sample size is the best-known study limitation of any research based on a sampled observation. Any finite sample size translates in a limited knowledge of the parent population that may have originated the observation. This limitation is essentially contained in the size of the confidence interval associated with the reported point estimate (Amrhein et al. 2019). Researchers push hard to study large samples in the hope of a better determination of the effect. Nonetheless, it is the case that the same phenomenon is analyzed by vividly different sample sized studies, which gives rise to a distribution of sampled results.

Because every research study is analyzed on a solo basis, it is infrequent that the sample of reported point estimates is analyzed consistently in relation to the distribution of the sample sizes.

We bring here a comprehensive analysis of the research studies on the acute effects that daylight saving time (DST) transitions may cause on public health and on societal issues. DST is the seasonal biannual changing of the clocks, a long-standing practice by which modern, extratropical societies – chiefly in America and Europe – adapt the phase of their daily rhythms to the early summer sunrises and to the late winter sunrises without altering daily routines as observed by the clock (Martín-Olalla 2019; Sani et al. 2015).

The current criticism against the practice of DST focuses the disruption on human circadian rhythms it brings. The spring transition – when clocks are shifted forward – charges with the burden of the proof because the advance in the phase of human social rhythms is accompanied by a sleep deprivation which, eventually, may give rise to the increase in the incidence of acute diseases and accidents (Meira e Cruz et al. 2019; Harrison 2013).

DST transitions set a natural experiment in which every individual participates. Yet, when it comes to a research study that assesses the correlation between transition dates and societal issues, things are further limited: it is only one health or one societal issue that is analyzed in a limited region – a country, some region or some hospital – and during a limited period of time – one year, a few years, one or two decades. All these factors impact the sample size of the study that can vary by some orders of magnitude, whereas the point estimate of the effect ranges from a factor 0.8 to a factor 1.9 – where no effect is associated with 1. Eventually, and in view of these results, governments in Europe and America are considering discontinuing this practice in the fear that its impact on human health may be more severe than previously thought (Parliament, European, Directorate-General for Parliamentary Research Services, and Irmgard Anglmayer 2017). In this call, the role of the largest reported point estimates may have been overrated, and the role of the sample size underrated.

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We analyze a set of 13 well-known research studies that associated the incidence of acute myocardial infarction (AMI), ischemic strokes or traffic accidents with DST transition dates. Many of them were retrieved from review reports (Harrison 2013; Manfredini et al. 2018). They are frequently cited in review literature to support the discontinuity of DST practice (Roenneberg et al. 2019; Watson 2019). They are also cited in ex-post impact assessment reports from the European Parliament (Parliament, European, Directorate-General for Parliamentary Research Services, and Irmgard Anglmayer 2017) or from the German Parliament (Caviezel et al. 2016) and in technical reports (Kearney et al. 2014). We show that the parent Poissonian population that may have originated the distribution of incidence ratios (IRs) in this set of 13 well-known research studies shows an increase of the risk around 5% in the spring transition. Any reported IR above this threshold is likely impacted by the sample size of the study.

Methods

The point estimate of the acute impact of DST transitions on human health is primordially assessed by the IR = O / N: the ratio of the observed counts O in a societal issue after a transition – also known as the study group – to the expected counts N in the same societal issue – the control group. The window of time to compute acute effects is the week after the transition. Research studies report either the week IR or the day IR in the first 7 days following a transition, or both.

We included in our analysis research studies which, in addition to IR, reported the total counts in the control group N or this number could be deduced from the reading of the manuscript.

We found seven AMI studies that met these requirements: six reported O in the first 7 days following the spring and autumn transitions and deduced N from various estimates (Čulić 2013; Janszky et al. 2012; Janszky and Ljung 2009; Jiddou et al. 2013; Sandhu et al. 2014; Sipilä et al. 2015); one reported the IR for the first 7 days after a transition (Kirchberger et al. 2015) and N could be deduced from the total number of AMI in their catalogue. We also considered one ischemic stroke study (Sipilä et al. 2016) reporting IR in the first 7 days after a transition.

We also considered a series of four accident-related studies: accidental deaths (Coren, 1996a), traffic accidents (Coren 1996b), road accidents (Robb and Barnes 2018) and fatal motor vehicle accidents (Fritz et al. 2020). The first two studies reported O and N; the last two studies reported IR and N could be deduced from their catalogue.

Finally, we considered one study on trauma admissions in Austria, Germany and Switzerland (Nohl et al. 2021) which reported O and N for the week after and prior to the transition, excluding the transition date. This study did not report IR.

In addition to day IR and week IR, we also analyzed a set of stratified IR associated with patient characteristics and codiagnoses – among others: gender, age group and cholesterol in the first week (Janszky et al. 2012; Jiddou et al. 2013; Kirchberger et al. 2015). We also considered the breakdown by accident type – accident, motorcycle accident, pedestrian, among others – in the trauma study (Nohl et al. 2021).

Each of these studies was natural experiments with little design; in this way, they are all comparable. The authors collected a dataset – such as the Swedish registry of AMIs (Janszky and Ljung 2009), the National Highway Traffic Safety Administration registry of motor vehicle accidents (Fritz et al. 2020) or the TraumaRegister DGU of the German Trauma Society – and crunched the numbers to get IR.

With the study group determined by the natural experiment, the control group – a contrafactual assessment of the events that would have been collected if the clock changing had not occurred – is the key parameter that gives rise to the reported IR. Studies differ in the way the control group is counted. Older, seminal studies take N as the number of events just before the transition (Coren 1996b) or an average of event just before and well after the transition (Janszky and Ljung 2009). Modern approaches consider sophisticated models that take into account seasonal and societal variations (Fritz et al. 2020; Robb and Barnes 2018). We do not distinguish the ones from the others and assume every author choice of N and, therefore, IR is a solid point estimate.

In our analysis, we identify the total counts in the control group with the sample size of the study. Eventually, the number comes from the population size of the region analyzed, the number of events – strokes or accidents – that this population produces every year and the number of years in the catalogue. Table 1 summarizes the main characteristics of every study.

Every study reported the 95 confidence interval (CI) associated with their point estimate of IR. Our goal in this meta-analysis is to test the distribution of reported IR and sample sizes N within the Poisson statistics. For this purpose, we test parent populations with IR_t and retrieve exact Poissonian confidence intervals from the quantiles of the χ^2 distribution for $2N{\cdot}\mathrm{IR}_t$ degrees of freedom (low bound) and $2{\cdot}(N{\cdot}\mathrm{IR}_t + 1)$ degrees of freedom (high bound) (Sun et al. 1996). As IR_t is an adjustable parameter, we search for the value of IR_t whose

Table 1. The sample size N – the total number of events in the control week, the expected total number of events from a model or the average number of events in a week – and the incidence ratio (IR) associated with the week after the spring transition and the autumn transition. Values were obtained for the week after a transition, except when marked with †, in which case, the largest IR reported in the first week after a transition is shown. Studies marked with ‡ did not provide N, we deduced them from an average of the record. Studies marked with ¶ did not report IR and their 95%CI. Bold annotates p – values below the standard level of significance $\alpha = 0.05$. According to their N, the reported values can be located inside the 95%CI for $IR_t = 1.05$ (spring) and $IR_t = 0.99$ (autumn), see Figure 1 and Table 2.

		Spring			Autumn		
Study	Region	Years	N	IR [95%CI]	Years	N	IR [95%CI]
Acute myocardial infarction							
Janszky and Ljung (2009)	Sweden	15	10251.0	**1.05** [1.03, 1.07]	20	13492.0	0.99 [0.97, 1.00]
Janszky et al. (2012)	Sweden	10	3115.5	**1.04** [1.00, 1.08]	13	4095.5	0.99 [0.96, 1.03]
Jiddou et al. (2013)	Two hospitals (Michigan, USA)	6	145.4	**1.17** [1.00, 1.36]	6	158.3	0.99 [0.85, 1.16]
Čulić (2013)	One hospital (Croatia)	6	45.9	1.15 [0.86, 1.51]	6	45.9	1.20 [0.90, 1.56]
Sandhu et al. (2014)	Michigan (USA)	4	849.0	1.02 [0.95, 1.09]	4	658.0	0.98 [0.90, 1.06]
Sipilä et al. (2015)	Finland	7	1257.2	1.01 [0.96 ,1.07]	9	1633.2	0.99 [0.94, 1.04]
Kirchberger et al. (2015)‡	Ausburg (Germany)	26	491.9	1.08 [0.97, 1.25]	25	473.0	1.02 [0.93, 1.13]
Ischemic strokes							
Sipilä et al. (2016)†‡	Finland	1	210.7	**1.14** [1.00, 1.30]	1	210.7	1.11 [0.98, 1.25]
Accidents							
Coren (1996a)	USA	1	2750.0	**1.07** [1.03, 1.10]	1	2938.5	0.98 [0.95, 1.02]
Coren (1996b)†	Canada	1	2594.0	**1.08** [1.04, 1.12]	1	4111.0	**0.92** [0.89, 0.95]
Robb and Barnes (2018)†	New Zealand	11	910.0	**1.16** [1.02, 1.28]	12	1346.0	1.07 [0.94, 1.16]
Fritz et al. (2020)‡	USA	22	14044.8	**1.06** [1.03, 1.09]	22	14044.8	1.00 [0.98, 1.03]
Trauma admissions							
Nohl et al. (2021)¶	Austria, Germany, and Switzerland	15	3456.0	**1.08** [1.05, 1.12]	15	3755.0	**0.94** [0.91, 0.97]

95%CI contains the 95% of the reported IR. In this way, the null hypothesis IR = IR_t sustains. In simple words, we will contextualize the difference between IR = 11,000/10,000 = 1.10 and IR = 110/100 = 1.10 within the Poisson statistics.

We did not include in our meta-analysis research studies on road traffic accidents where IR was not reported (Lahti et al. 2010) (Finland) or N was unavailable (Molina et al. 2022) (Florida, USA). Likewise, we did not consider research studies where the window of time was longer than 1 week (Rodríguez-Cortés et al. 2022; Singh et al. 2022) since they do not necessarily account for acute effects.

We understand that the sample of 13 research studies here analyzed is representative of the literature on the field. The sample produces 64-day IRs, 10-week IRs and 85 stratified IRs which will all be analyzed separately. As a limitation of our analysis, we do merge results from AMI, ischemic strokes, accidents and trauma admissions.

Also, reported IRs and, therefore, our meta-analysis are limited to the specific setting of transition dates under which societal issues were evaluated. Spring transition comes in Europe and New Zealand 2 weeks after the spring Equinox. In America, it used to come 3 weeks after the Equinox, but since the year 2007, it was advanced to 1 week before the Equinox. This comes in the middle of the window of time analyzed in Fritz et al. (2020), while Coren (1996a, 1996b) refer to the scenario prior to the year 2007. The autumn transition comes in New Zealand 3 weeks after the Equinox. In Europe, it used to come 2 weeks after the Equinox, but since the year 1995, it does in the last Sunday of October. Kirchberger et al. (2015) are impacted by this change. In America, the last Sunday of October was also the autumn transition date until the year 2007, when it was moved 1 week later.

Results

Figure 1 shows the reported IRs versus the sample size of the study for the spring transition (left) and the autumn transition (right). Generally speaking, day IRs (intermediate ink) have smaller sample sizes than week IRs (darkest ink) and show a much larger variability.

Figure 1 left shows in broken lines the upper and lower bounds of the 95%CI for $IR_t^{spring} = 1.00$. Table 2 summarizes the occurrences of reported IRs inside the 95%CI. In the first row of the table, we see 64 (stratum,

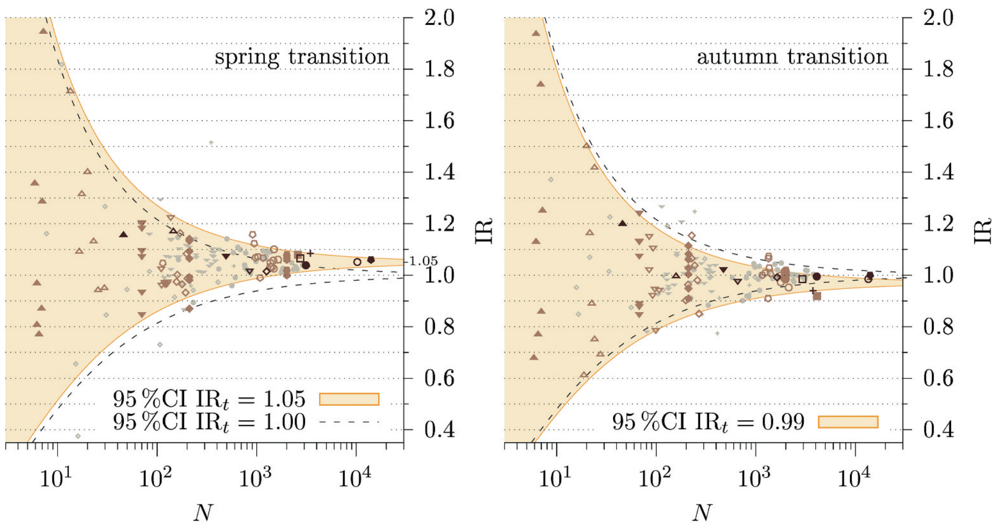

Figure 1. Scatter plot for stratified (lightest ink), day (intermediate ink) and week (darkest ink) IR associated with the spring transition (left panel) and the autumn transition (right panel). The lower and upper bounds of the 95% confidence interval for a Poissonian parent distribution with $IR_t = 1.00$ are shown by broken lines. The 95% confidence interval for a Poissonian parent distribution with $IR_t = 1.05$ (left) and $IR_t = 0.99$ (right) is shown in light shade background color. They roughly contain 95% of the observations in either panel, see Table 2. Legend: open circles (Janszky and Ljung 2009), solid circles (Janszky et al. 2012), open up triangles (Jiddou et al. 2013), solid up triangles (Čulić 2013), open down triangles (Sandhu et al. 2014), open diamonds (Sipilä et al. 2015), solid down triangles (Kirchberger et al. 2015), solid diamonds (Sipilä et al. 2016), open squares (Coren, 1996a), solid squares (Coren 1996b), open pentagons (Robb and Barnes 2018), solid pentagons (Fritz et al. 2020) and crosses (Nohl et al. 2021). See Table 2 for a breakdown of occurrences inside the 95% confidence intervals.

75.3%), 47 (day, 73.4%) and 5 (week, 50.0%) reported IRs inside the 95%CI for $IR_t^{spring} = 1.00$. Therefore, the null hypothesis $IR_t^{spring} = 1.00$ does not sustain at the standard level of significance (5%).

Results inside the 95%CI for IR_t

In contrast, the null hypothesis $IR_t^{spring} = 1.05$ – whose 95%CI is noted by a light shade background color – yields the largest occurrences inside the CI: 78 (91.8%), 60 (93.8%) and 10 (100.0%) of the stratum, day and week observations (see second row in Table 2). Therefore, the null sustains at the standard level of significance. A similar analysis for the autumn transition shows that the null hypothesis $IR_t^{autumn} = 1.00$ and $IR_t^{autumn} = 0.99$ sustains with similar scores.

As a result, we understand that for the spring transition, the IR is close to 1.05 or 5% increase in the risks, while the autumn transition poses negligible small risks. Therefore, reported IRs above $IR_t^{spring} = 1.05$ – which often raises the alarm in decision makers, the public opinion and researchers – are likely impacted by the limitation that the sample size always brings. There is only one clear outlier in the catalogue of reported IR: motorcycle accident admissions hits IR = 1.52 in the trauma study (Nohl et al. 2021). The authors explain

Table 2. Occurrences of reported IRs inside the 95% confidence interval of a Poissonian parent distribution for selected IR_t (see Figure 1 for a graphical representation). The number of occurrences relative to the total number of observations is given first; then, in parentheses, the percentage score. The null hypothesis IR = IR_t sustains at the standard level of significance for $IR_t^{spring} = 1.05$. For the autumn transition, both $IR_t^{autumn} = 1.00$ and $IR_t^{autumn} = 0.99$ sustain similar scores.

IR_t	Stratified	Day	Week
	Spring		
1.00	64/85	47/64	5/10
	(75.3)	(73.4)	(50.0)
1.05	78/85	60/64	10/10
	(91.8)	(93.8)	(100.0)
	Autumn		
1.00	70/85	56/64	9/10
	(82.4)	(87.5)	(90.0)
0.99	71/85	54/64	9/10
	(83.5)	(84.4)	(90.0)

that motorcycle travels soar in Austria, Germany and Switzerland from March to April as many motorcyclists have summer license plates, which only operates from March to October.

Discussion

To understand the size of the spring effect, we were able to estimate the relative sample standard deviation of the record in four studies (Fritz et al. 2020; Jiddou et al. 2013; Robb and Barnes 2018; Sandhu et al. 2014). Figure 2 in Sandhu et al. (2014) shows the residuals of the model on a daily basis. Visual perception suggests that the standard error of the model is at least some 20% of the daily AMIs; the largest reported IR is in line with this number. Table 2b in Robb and Barnes (2018) provides the standard error of the model as 0.069 in the $\log_{10}$ space or $10^{0.069} = 1.17$ in the linear space: their largest reported IR is in line with this number. On the other hand, Figure 1 in Fritz et al. (2020) shows the sample standard deviation of the weekly observation in two periods of 11 years each. From this, we infer a relative sample standard deviation ~8% in the week previous to the spring transition. Their reported IR is below this threshold. Finally, Table 1 in Jiddou et al. (2013) contains N_i and O_i in the 7 years of the record, from which we found a relative standard deviation of the sample ~40% for 1 week. In contrast, Jiddou et al. (2013) reported a week IR = 1.17 or 17% increase in the week after the spring transition.

From these four studies – $N \in (150, 14000)$ – we understand that, while the spring transition shows a systematic increase of the risks, the size of the excursion is below the sample relative standard deviation of O every year. Therefore, the impact of the transitions is below the myriad of things that populates the variability of the tested quantity, which explains why the assessment of the impact of the clock transitions on health issues is elusive.

Concluding remarks

We do not conclude from our analysis that DST transitions do not impact on public health. Instead, we bring attention to the fact that its impact might be as mild as previously thought. Decision makers and researchers should understand that the assessment of the IR is only the starting point of the balance of the risks associated with the practice. The risks of the practice should be balanced out against the risks of canceling a practice.

We bring the following recent example, US Senator Marco Rubio sponsored the Sunshine Protection Act of 2021 to "lock the clock." Among other points, Rubio alleged that DST transitions caused 28 fatal motor vehicle accidents, a highlight from Fritz et al. (2020). Much to the dismay of authors' study and many others (Carter et al. 2022; Roenneberg et al. 2019), Rubio vowed for locking the clock in the DST setting – that is, making DST the new perennial Standard Time – even though the study also showed that the risks of fatal accidents doubled after DST spring transition was advanced 3 weeks in the US after the Energy Policy Act (Martín-Olalla 2020). Note that the Sunshine Protection Act of 2021 would effectively advance DST "spring transition" by 10 weeks and delay DST "autumn transition" by 8 weeks to cancel out both.

The thing to emphasize is that having locked the clock in the permanent standard time during the 20th century would have caused also a greater number of fatal accidents. In other words, the advance of clocks in spring – and, therefore, the delay of sunrise times – enforced by DST also helped to prevent risks. Had sunrise time occurred in the 40 latitude at 04:30h during the summer season, greater shares of population would have found comfortable to advance their daily activity relative to present-day scores. This choice would have translated into early winter activity, which is more prone to poor light conditions and, eventually, to greater accidental risks. Current research studies can assess the impact of the practicing transitions, as they set a natural experiment to analyze. On the contrary, we can only speculate with the impact of not having practiced DST transitions by analyzing the impact of past actions in the present day (Martín-Olalla 2022).

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