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EFFECTS OF DAYLIGHT-SAVING TIME CHANGES ON STOCK MARKET RETURNS AND STOCK MARKET VOLATILITY: REBUTTAL[1, 2]

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Summary.—In a 2011 reply to our 2010 comment in this journal, Berument and Dogen maintained their challenge to the existence of the negative daylight-saving effect in stock returns reported by Kamstra, Kramer, and Levi in 2000. Unfortunately, in their reply, Berument and Dogen ignored all of the points raised in the comment, failing even to cite the Kamstra, *et al.* comment. Berument and Dogen continued to use inappropriate estimation techniques, over-parameterized models, and low-power tests and perhaps most surprisingly even failed to replicate results they themselves reported in their previous paper, written by Berument, Dogen, and Onar in 2010. The findings reported by Berument and Dogen, as well as by Berument, Dogen, and Onar, are neither well-supported nor well-reasoned. We maintain our original objections to their analysis, highlight new serious empirical and theoretical problems, and emphasize that there remains statistically significant evidence of an economically large negative daylight-saving effect in U.S. stock returns. The issues raised in this rebuttal extend beyond the daylight-saving effect itself, touching on methodological points that arise more generally when deciding how to model financial returns data.

Kamstra, Kramer, and Levi (2000) documented an economically large and statistically significant negative effect in stock returns on the Mondays following daylight-saving time changes in four countries: the U.S., Canada, the U.K., and Germany. The effect was found to be 200 to 500% of the regular average Monday negative returns, implying, for instance, an average one-day loss of 31 billion dollars on the United States stock exchanges every time the clocks are changed for daylight-saving purposes.

Berument, Dogen, and Onar (2010), henceforth BDO, challenged that finding (examining data for the U.S. only), claiming, "Returns on the first business day following daylight savings time changes were not lower nor was the volatility higher" (p. 632). Kamstra, Kramer, and Levi (2010) commented on BDO's findings, highlighting serious methodological flaws in

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their analysis and demonstrating that the daylight-saving effect indeed remains evident in U.S. stock returns. (It is worth noting that Kamstra, *et al.* (2000) did not investigate volatility.) Berument and Dogen (2011) replied to the comment, failing to address any of the serious methodological flaws highlighted by the Kamstra, *et al.* (2010) comment, and in fact neglecting even to mention or cite the comment. Further, Berument and Dogen's analysis (still employing the flawed methods that are prone to lead to parameter instability) produced results that contradicted even the results reported by BDO, which the authors apparently did not recognize, owing to confusion in interpreting their own results. Furthermore, Berument and Dogen purported to estimate a risk-return relationship based on the capital asset pricing model but violated several important conditions of the model, rendering their inference invalid and their conclusions suspect. Additionally, Berument and Dogen mischaracterized the psychology literature in claiming support for their findings. We elaborate on all of these points below and conclude, again, that the evidence in support of a statistically significant and economically large negative daylight-saving effect remains intact in U.S. stock return data.

Methodological Issues

Several of the methodological issues we note in this section extend beyond the context of the daylight-saving effect in that they may apply when modeling stock returns data in general. BDO relied on a model with 15 lags of the dependent variable as the basis for their empirical analysis. The use of a model with 15 lags of the dependent variable, aside from being non-standard in finance, severely compromises one's ability to detect the daylight-saving effect. Accordingly, Kamstra, *et al.* (2010) noted that when one includes 15 lags of the dependent variable, the daylight-saving coefficient estimate does not capture the full effect of the daylight-saving time change; one would need an adjustment that takes account of all 15 lagged dependent variable coefficients to capture the effect. Berument and Dogen (2011) did not respond to this point, and indeed, continued to employ up to 15 lags of the dependent variable with no adjustment.

BDO and Berument and Dogen (2011) relied on a version of Nelson's (1991) exponential generalized autoregressive conditional heteroskedastic (EGARCH) model for their empirical analysis. Kamstra, *et al.* (2010) noted that the use of EGARCH can produce biased estimates of the mean equation including, importantly, the magnitude of the daylight-saving effect in the model BDO estimated (and in the model Berument and Dogen subsequently estimated). Researchers should take care using this technique when the variable of interest is in the mean equation, as is the case in Equation (1) specified by BDO and Berument and Dogen (2011). Note that the daylight-saving coefficient estimates BDO reported for their EGARCH

estimation are *orders of magnitude smaller* than those they reported in their appendix based on ordinary least squares (OLS). Kamstra, *et al.* (2010) noted that such extreme changes in coefficient estimates across different estimation techniques can be a signal of estimation instability. Berument and Dogen (2011) remarked that EGARCH can produce unstable parameter estimates in their footnote 7, yet they continued to employ EGARCH without qualification in estimating the daylight-saving effect. They *did* turn to a new technique to estimate their model in a manner robust to outliers, developed by Carnero, Pena, and Ruiz (2007). The Carnero, *et al.* model is designed to give more reliable estimates of the time-series parameters of the *variance* parameters, not the parameters of the mean equation (which are the parameters of interest to Berument and Dogen). Further, Carnero, *et al.* did not describe how to implement their outlier-robust technique in the context of an EGARCH model (they focused on simpler GARCH models), making it impossible to verify any of Berument and Dogen's (2011) estimates.

It is worth emphasizing that even Berument and Dogen (2011) were unable to replicate BDO's findings based on EGARCH, just as we were unable. Whereas BDO claimed to have found an insignificant daylight-saving effect on stock returns in their paper (and failed to mention the evidence of a significantly negative daylight-saving effect evident in tables in their own appendix), Berument and Dogen (2011) reported a significant *positive* effect on stock returns. At no point have they commented on the fact that their own results were different in their various papers or explained why that might be the case. We stand by the results reported by Kamstra, *et al.* (2000) and Kamstra, *et al.* (2010), namely that there is a statistically significant economically large negative daylight-saving effect in U.S. stock returns.

Berument and Dogen (2011) added a further complication to their already-problematic EGARCH model by including in the mean regression equation a set of volatility-in-mean terms. (Berument and Dogen produced these volatility-in-mean terms by interacting the parameters of the mean equation, including the daylight-saving variable, denoted DST_t, with the EGARCH estimate of conditional volatility, denoted h^2_t, yielding an interacted daylight-saving/volatility-in-mean term, denoted $DST_t * h_t^2$.) In estimating this model, Berument and Dogen found positive (albeit insignificant) coefficient estimates on the DST_t term and claimed this overturned the findings of Kamstra, *et al.* (2000), writing, "This result is the reverse of that of Kamstra, *et al.* (2000)" (p. 871). Their interpretation of the results that emerged from estimating this model was perplexing in light of the previously mentioned facts that they make no adjustments to account for the inclusion of 15 lags of the dependent variable and that EGARCH can

produce unstable parameter estimates and in light of several additional serious problems we now consider.

First, Berument and Dogen (2011) argued that the daylight-saving effect arises due to *reduced* aversion to risk, as implied by a negative coefficient on their interaction term $DST_{t^*} * h_t^2$, but their interpretation is not based on sound financial theory. The capital asset pricing model (see Merton, 1980) implies that the market return is linearly related to volatility (Berument & Dogen, 2011, do include the $DST_t * h_t^2$ term in their model in an additive manner), but the risk-return relationship that emerges from this model relies on the regression model meeting two important conditions. The model must employ returns *in excess of the risk free rate of return*, however, Berument and Dogen (2011) used raw returns. Further, the model must contain no other linear terms, namely *no intercept term, and no variables such as the daylight-saving term*. Yet Berument and Dogen included an intercept, lags of the dependent variable, and variables related to daylight saving in their model. Thus, Berument and Dogen (2011) specified a model without any clear connection to financial theory or the risk-return relationship posited in financial theory. Their conclusion that the negative coefficient on the volatility-in-mean term $DST_t * h_t^2$ implies lower risk aversion following daylight-saving time changes is therefore without merit.

Second, Berument and Dogen's interpretation of their coefficient estimates was confused and misleading. If investors did indeed become *less* averse to risk on daylight-saving Mondays, as Berument and Dogen (2011) assert, then the price of risky assets would *rise* on the Monday, reflecting investors' increased tolerance for holding risky assets and increased interest in holding risky assets. This would naturally lead to *higher* returns on the Monday following the time change, not the lower returns Berument and Dogen themselves documented. That is, the negative daylight-saving coefficient estimate they documented is inconsistent with their explanation.

Third, Berument and Dogen's inability to replicate the significant negative sign on the DST_t term reported by Kamstra, *et al.* (2000) and in BDO's appendix (leading Berument and Dogen to assert that they reversed the findings of Kamstra, *et al.*, 2000), comes about as a result of Berument and Dogen having included two variables in the mean equation that are strongly positively correlated, the daylight-saving dummy variable DST_t and DST_t interacted with the conditional variable estimate, $DST_t * h_t^2$. Berument and Dogen reported that their positive DST_t coefficient estimates were typically insignificant, and the negative $DST_t * h_t^2$ coefficients were typically significant, again consistent with the Kamstra, *et al.* (2000) and Kamstra, *et al.* (2010) findings that there is a significant negative daylight-saving effect. We stand by the conclusion that there is a statistically sig-

TABLE 1
MEAN OF DAILY RAW RETURNS DATA WITH THE KAMSTRA, *ET AL.* (2000) TIME SPAN, AS REPORTED IN THE BDO APPENDIX

Index	Weighting	Other Days	Weekend	Spring	Fall	Joint *t* test
NYSE	Equal	.001179	−.000741	−.001948	−.006444	−2.8747***
		(6,187)	(1,558)	(30)	(30)	
NYSE	Value	.000750	−.000372	−.001354	−.005322	−2.21806***
		(6,187)	(1,558)	(30)	(30)	
AMEX	Equal	.001471	−.000848	−.002103	−.006623	−2.90287***
		(6,187)	(1,558)	(30)	(30)	
AMEX	Value	.000931	−.001416	−.001833	−.006695	−2.22585***
		(6,187)	(1,558)	(30)	(30)	
NASDAQ	Equal	.001494	−.000991	−.001636	−.007496	−3.47863***
		(5,022)	(1,259)	(24)	(24)	
NASDAQ	Value	.001005	−.001292	−.001516	−.008161	−2.46075***
		(5,022)	(1,259)	(24)	(24)	
S&P500	Equal	.000902	−.000403	−.001299	−.005838	−2.29031***
		(6,187)	(1,558)	(30)	(30)	
S&P500	Value	.000715	−.000177	−.001201	−.005111	−2.10667***
		(6,187)	(1,558)	(30)	(30)	

Note.—Numbers in parentheses appear to be number of observations (BDO omitted the description). ***$p < .01$.

nificant, economically large negative daylight-saving effect in U.S. stock returns. This conclusion emerges from sound analysis of the data, even when employing severely over-parameterized models (such as those used to produce results in BDO's appendix).

Related to the previous point, it is imperative that we place on the formal record the strong results documented in BDO's previously unpublished appendix where they found strong, statistically significant evidence of a negative daylight-saving effect.[3] We provide these results in Tables 1 and 2. We begin with Table 1, which contains mean daily raw returns for "Other days" (which are all trading days other than those that follow a weekend), "Weekend" days (which are the trading days immediately following a weekend, typically Mondays, but excluding trading days that immediately follow a daylight-saving time change), and trading days following the "Spring" and "Fall" daylight-saving time changes. The final column contains a joint *t* test on the fall and spring mean returns, testing for significant difference from zero. Note in Table 1 that *in all cases*, the average return on the trading day following a fall or spring daylight-saving time change is negative. Additionally, *in all cases*, the magnitude of the average return on the trading day following a fall and spring daylight-saving time is larger in magnitude than the average return on a trading day

[3]BDO mentioned in Footnote 6 of their paper that interested readers could obtain the appendix from the publisher.

following a regular weekend; in many cases, the negative daylight-saving returns are an order of magnitude larger than the negative regular weekend returns. Further, the joint t tests in the final column of Table 1 indicate the average spring and fall daylight-saving returns are significantly negative (at the 1% level or better). That is, BDO reported in their appendix statistically significant evidence of an economically large negative daylight-saving effect in stock returns. We are concerned by these authors' failure to explain the discrepancy of these results relative to those reported by Berument and Dogen (2011).

We turn now to Table 2 which contains results BDO reported in their appendix based on estimating the daylight-saving effect in stock returns using OLS. In all cases, the spring and fall coefficient estimates are negative, with the fall estimates statistically negative in all cases, and with a joint test on the fall and spring values significant in all cases. Note the significance of the estimates is particularly striking in light of the fact that the model appears to include 15 lags of the dependent variable. (The appendix does not indicate the precise model specification employed, so one is left to assume BDO utilized Equation 1 from BDO, which included 15 lags of the dependent variable.) Again, these are results extracted directly from BDO's appendix, providing striking evidence in support of a daylight-saving effect in U.S. stock returns. Yet BDO and Berument and Dogen (2011) did not mention this support, and in fact Berument and Dogen perplexingly claimed to find evidence of a significant *positive* daylight-saving effect in U.S. stock returns, in stark contrast to the results reported

TABLE 2
OLS ESTIMATION WITH THE KAMSTRA, *ET AL.* (2000) TIME SPAN, AS REPORTED IN BDO APPENDIX

Index	Weighting	Spring	Fall	Fall/Spring
NYSE	Equal	−.094082	−.477508***	−.284962***
		(−.725)	(−3.6643)	(−3.070)
NYSE	Value	−.101393	−.486617***	−.293855***
		(−.664)	(−3.186)	(−2.696)
AMEX	Equal	−.088044	−.452909***	−.269590***
		(−.675)	(−3.456)	(−2.889)
AMEX	Value	−.017077	−.445013***	−.230188**
		(−.114)	(−2.976)	(−2.161)
NASDAQ	Equal	−.044863	−.471056***	−.256540***
		(−.409)	(−4.267)	(−3.266)
NASDAQ	Value	−.008856	−.575737***	−.291120**
		(−.053)	(−3.452)	(−2.451)
S&P500	Equal	−.075897	−.497941***	−.286237***
		(−.487)	(−3.188)	(−2.570)
S&P500	Value	−.108744	−.480298***	−.294023***
		(−.665)	(−2.930)	(−2.514)

Note.—t statistics are reported in parentheses. **$p < .05$, ***$p < .01$.

in BDO's appendix and in contrast to the fact that they reported negative mean returns on the trading day following daylight-saving time returns in all cases in Table 1 of Berument and Dogen (see their p. 868). Their conflicting evidence sheds doubt on the reliability of their analysis.

In addition to Berument and Dogen (2011) having reported results about the impact of daylight-saving time changes on *mean stock returns* that conflict with results BDO previously reported, Berument and Dogen (2011) also reported results about the impact of daylight-saving time changes on *stock return volatility* that conflict with results BDO previously reported. To wit, Berument and Dogen (2011) reported the effect of daylight-saving time changes "is more pronounced during volatile periods" (p. 875). Yet BDO previously reported "Returns on the first business day following daylight savings time changes were not lower nor was the volatility higher" (p. 632) and "In the present study, the estimated coefficients of the daylight savings time dummies in the volatility specification were mostly negative … but were not statistically significant at the 10% level" (p. 636). We are puzzled by these authors' informal analytic approach. If they believe one set of results is more valid than another, they ought to comment on the factors that led to the different findings and explain why the reader should have more confidence in one set of results over another. In absence of such a discussion, we have no reason to doubt the strong evidence in support of a statistically significant and economically large negative daylight saving effect in U.S. stock returns, as reported by Kamstra, *et al.* (2000), Kamstra, *et al.* (2010), and elsewhere.

Literature Issues

We turn now to the way Berument and Dogen (2011) cited, and in some cases perhaps mischaracterized, the psychology literature. Recall that Kamstra, *et al.* (2000) posited that a *one-hour* disruption of sleep on daylight-saving weekends may lead to anxiety that may be associated with lower risk tolerance. In contrast, the studies Berument and Dogen (2011) cited consider correlation between the likelihood people undertake activities such as smoking and their tendency to experience sleep problems.

These studies variously focused on a particular segment of the population (e.g., adolescents' typical sleep habits), behavior under very extreme conditions (such as 49 hours of sleep deprivation), or subjects' driving performance during a lengthy exercise, and hence bear little relationship to the *financial* implications of adults losing or gaining of an hour of sleep following a daylight-saving time change. We shall now go through some of the papers Berument and Dogen (2011) cited regarding sleep to highlight additional problems.

Berument and Dogen (2011) wrote, "O'Brien and Mindell (2005) re-

ported that adolescents with more sleeping problems display significantly more risk-taking behavior and that higher risk-taking behavior increases immediate threats, such as a higher incidence of traffic accidents, unplanned pregnancies, and infectious diseases" (p. 865). In fact, O'Brien and Mindell did not consider accidents, pregnancies, or diseases. O'Brien and Mindell measured risk-taking by asking adolescents to report the frequency with which they engaged in certain behaviors, mostly within the past 30 days. The questions pertained to behaviors in seven domains: safety (e.g., wearing a bicycle helmet), violence (e.g., carrying a gun), tobacco, alcohol, marijuana, other drugs, and sex. Further, the results reported by O'Brien and Mindell (2005) were not predictive in any sense. O'Brien and Mindell (2005) found tendency to engage in risky behavior in the past 30 days was associated with average sleep patterns over the past 2 weeks. [Obviously recent sleep patterns cannot cause previous risky behavior. In any case, implying temporal causation based on these results is ill-advised, especially since O'Brien and Mindell themselves noted, "Perhaps those students who engage in risk-taking have a decreased need for sleep or have a lower threshold for what amounts to a pattern of sleep deprivation" (p. 130).] Additionally, the relationship O'Brien and Mindell found between typical weekend sleep desynchronosis and risk taking over the previous 30 days was limited to sleep desynchronosis much greater than that associated with daylight-saving time changes; O'Brien and Mindell wrote "students with a weekend [sleep] delay of 2 hr or more had significantly higher risk-taking scores compared to students with a weekend delay of 1 hr or less" (p. 125). Daylight-saving time changes, of course, are always limited to 1 hour, thus the findings with respect to sleep disruptions of 2 or more hours are not relevant, especially since O'Brien and Mindell's analysis of disruptions of 2 or more hours were performed *relative* to disruptions of 1 hour or less. Finally, O'Brien and Mindell were careful to note that their findings apply to adolescents, and they themselves noted that this limits generalizability (p. 131).[4]

A study Berument and Dogen (2011) cited that considers risk taking in a *financial* context is by Killgore, Balkin, & Wesensten (2006), who hypothesized that "participants [sleep deprived for more than two days] would show deficits analogous to those seen in patients with lesions to the ventromedial prefrontal lobes. The present data support this hypoth-

[4]Note that it is widely accepted that adolescents take more risks than adults. A recent CBC news article quoted Ian Manion, a psychologist at the Ontario Centre of Excellence for Child and Youth Mental Health: "Research and studies suggest adolescents engage in risky and dangerous behaviour because it's a part of normal brain development. Teenagers seek out risk-taking behaviour because of the complex brain systems involved in decision-making." Additionally, Sandeep Mishra, a post-doctoral researcher at the University of Guelph, said, "The reality is that teenagers tend to make riskier decisions. The bulk of the evidence suggests that risk-taking in teenage years is both normal and typical" (Waliji, 2012).

esis" (p. 11). That is, the behavior of the participants in this study was so extreme that they were compared to people with brain damage. The relevance of such a study to the context of daylight-saving time changes seems tenuous.

The other study Berument and Dogen (2011) cited that considers risk taking in a financial context is by Killgore (2007). In that study, too, participants experienced extreme sleep deprivation: 23 hours or more. Additionally, the focus of study in the Killgore (2007) study was the different impact sleep deprivation has on people characterized as morning-type versus evening-type. Past research shows morning-type people are less risk tolerant, and this study aimed to test the hypothesis that extreme sleep deprivation could make risk-averse morning-type people more willing to take risks. Strikingly, Killgore found some results that contradict Berument and Dogen's description of his findings: "[S]leep deprivation significantly *reduces* self-reported and behaviorally demonstrated willingness to engage in high-risk and sensational activities under conditions of uncertainty" (p. 613; emphasis added). That is, while Berument and Dogen claim that Killgore (2007) found "sleep problems are associated with a higher risk tolerance" (p. 864), Killgore reports the exact opposite. Killgore further notes his finding that sleep deprivation led to reduced risk taking was contradictory to both his initial hypothesis and the Killgore, *et al.* (2006) findings. Clearly, to resolve that discrepancy, more research would need to be done to determine the impact of extreme sleep deprivation on risk taking.

The final paper Berument and Dogen (2011) cited in the context of relating sleep disruptions to risk taking is by Brown, Tickner, and Simmonds (1970). Berument and Dogen wrote "sleep deprivation due to extended driving increases risky decisions, such as passing with low visibility or forcing other drivers to adjust their speed to permit the tired driver to pass" (p. 865). We note several features of the Brown, *et al.* study which limit its applicability to the current context. First, it was a case study of six participants, a small sample size. Second, the study aimed to compare the riskiness of participants' driving during the first versus last 3-hour segments of a 12-hour driving spell based on the experimenter's evaluation. Brown, *et al.* themselves emphasize that the experimenter's evaluations were not blind and hence he may have been "biased by his expectancy that prolonged driving would affect skill adversely" (p. 241). Consistent with this possibility, we note that Brown, *et al.* found drivers actually drove more *slowly* in the last 3-hour segment than in the first, and their response time was *faster* in the last 3-hour segment than in the first. Nonetheless, the experimenter still evaluated their driving to be more risky in the last three hour segment, which is surprising. Finally, we note that a study focused on driving performance after 12 hours in constrained seating would seem

to bear little relevance to the question of whether sleep desynchronosis arising from daylight-saving time changes leads to financial risk aversion.

Conclusion

In conclusion, Kamstra, Kramer, and Levi (2010) made four main points. First, the results reported by BDO were not replicable. Second, BDO used an unconventional, highly over-parameterized model for returns that can lead to multicollinearity and hence low power for tests, in particular making it difficult to reject the null of no daylight-saving effect in stock returns, even if the true data-generating model incorporated a daylight-saving effect. Third, BDO relied on estimation methods that are known to produce biased estimates of the parameters of the mean (the parameters of interest here). Fourth, estimation of a conventional model specification with an unbiased estimation technique reaffirms the evidence for a daylight-saving effect in returns.

Berument and Dogen (2011) ignored all of these points, and failed even to mention Kamstra, *et al.* (2010), which is surprising since Berument and Dogen's paper was labeled as a *reply* to Kamstra, *et al.* (2010). The new estimation results reported by Berument and Dogen contradict the estimation results reported by BDO. BDO's appendix provided statistically significant evidence of an economically large negative daylight-saving effect in U.S. stock returns, consistent with Kamstra, *et al.* (2000) and Kamstra, *et al.* (2010). We remain unchanged in our view that the evidence supporting the existence of a daylight-saving effect in U.S. stock returns is strong. As for the evidence on the influence of daylight-saving time changes on U.S. stock return volatility, the implementation of EGARCH employed by BDO and Berument and Dogen (2010) is rife with problems (leading, for instance, to unstable parameter estimates), and we encourage readers to be skeptical of results based on those analyses.

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