Losing Sleep at the Market: Comment

By J. Michael Pinegar*

In a recent provocative paper in this journal, Mark J. Kamstra et al. (2000) test and reject the hypothesis that the mean weekend return following changes in daylight saving time equals the mean weekend return throughout the rest of the year. The authors report that the average Friday-to-Monday return on daylight-saving weekends is 200–500 percent larger than the average negative return for the other weekends of the year. The finding appears to hold not only in the United States and Canada where daylight-saving date patterns are similar, but also in the United Kingdom, whose patterns ostensibly differ from those in North America. The results also appear robust to alternative statistical methods based on time-varying conditional heteroscedasticity and/or bootstrapping.

This paper provides further robustness tests of the results reported by Kamstra et al. I show that the difference between mean weekend returns for daylight-saving and non-daylight-saving weekends is significant only for fall changes in daylight saving time and that the fall difference is driven by two outliers associated with international stock market crises. Two separate adjustments for the heteroscedasticity these outliers induce cause the significance of the fall difference to vanish. The total sample (spring plus fall) difference remains marginally significant for *some* indexes after heteroscedasticity adjustments with classical fixed-level hypothesis tests. However, Bayesian sample-size adjustments produce posterior odds ratios that consistently favor the null hypothesis of no daylight-saving anomaly over the alternative that the anomaly exists. I also fail to reject the hypothesis that daylight-saving and non-daylight-saving weekend returns have equal distributions. For these reasons, I question the robustness of the findings reported by Kamstra et al. (2000).

Section I presents more details of my tests, and Section II summarizes my findings and discusses their interpretation.

I. The Impact of Changes in Daylight Saving Time

Congress officially passed a daylight-saving act in 1967. Until 1986, the spring change in the United States occurred on the last Sunday in April. Since then, it has occurred on the first Sunday in April. The fall change has always occurred on the last Sunday in October. I examine the impact of daylight-saving-time changes from 1 January 1967 through 31 December 1998, one year longer than the period used in Kamstra et al. The one-year difference has only a trivial impact on the results.

Table 1 reports means for daylight-saving and non-daylight-saving weekend returns for the equally weighted and value-weighted NYSE and AMEX indexes and for the S&P 500. Results are reported separately for the spring and fall changes in daylight-saving and for other weekends. Since weekend returns that follow changes in daylight saving time always occur on Mondays, my "other weekend" benchmark includes only those weekends for which markets are open on the following Monday. Besides mean weekend returns, the table reports three test statistics that include (1) an ordinary least-squares (OLS) t statistic, (2) a t statistic based on Halbert L. White (1980) corrections for heteroscedasticity of unknown form; and (3) a t statistic based on Lawrence R. Glosten et al. (1993; henceforth GJR) corrections for time-varying conditional heteroscedasticity with asymmetric volatility responses to news. Each statistic tests the equality of mean daylight-saving and non-daylight-saving weekend returns. An asterisk (dagger) indicates rejection at the 0.05 (0.10) level of the null hypotheses that mean weekend returns following spring, fall, or combined spring and fall changes in

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TABLE 1—TESTS OF THE INDIVIDUAL IMPACT OF SPRING AND FALL CHANGES IN DAYLIGHT SAVING ON VARIOUS U.S. STOCK INDEXES (1 JANUARY 1967–31 DECEMBER 1998)

	Mean weekend returns			t Statistics[a]		
Portfolio	Spring daylight saving (N = 31)	Fall daylight saving (N = 31)	Other weekends (N = 1,486)	Spring	Fall	Both
NYSE (equally weighted)	−0.00179	−0.00582	−0.00078	−0.72 (−0.77) [−0.93]	−3.60* (−1.55) [−1.63]	−3.03* (−1.71†) [−1.74†]
NYSE (value weighted)	−0.00128	−0.00505	−0.00034	−0.60 (−0.70) [−1.00]	−3.02* (−1.39) [−1.42]	−2.54* (−1.53) [−1.65†]
AMEX (equally weighted)	−0.00216	−0.00613	−0.00100	−0.82 (−0.78) [−1.36]	−3.61* (−1.56) [−1.11]	−3.10* (−1.73†) [−1.76†]
AMEX (value weighted)	−0.00196	−0.00616	−0.00151	−0.29 (−0.31) [−0.51]	−2.99* (−1.29) [−0.93]	−2.30* (−1.30) [−1.27]
S&P 500	−0.00142	−0.00525	−0.00042	−0.60 (−0.72) [−0.81]	−2.91* (−1.36) [−1.34]	−2.46* (−1.51) [−1.28]

Source: All data are from Center for Research in Security Prices.

[a] The statistics are an OLS t statistic, a t statistic based on White (1980) corrections for heteroscedasticity of unknown form (in parentheses), and a t statistic based on Glosten et al. (1993) corrections for time-varying conditional heteroscedasticity (in square brackets). Each statistic tests the equality of mean daylight-saving and non-daylight-saving weekend returns. An asterisk (*) indicates rejection of the null hypothesis at the 0.05 level; a dagger (†) indicates rejection at the 0.10 level.

daylight saving time equal mean returns for non-daylight-saving weekends.

The t statistics are based on the following regressions:

$$R_{p,t} = \alpha_p + \delta_M \times \text{Mon}_t + \delta_s \times \text{sdlight}_t + \delta_f \times \text{fdlight}_t + \varepsilon_{p,t}$$

and

$$R_{p,t} = \alpha_p + \delta_M \times \text{Mon}_t + \delta_d \times \text{dlight}_t + \varepsilon^*_{p,t}$$

in which $R_{p,t}$ is the return on portfolio p at time t, α_p is a constant, Mon_t is a dummy variable that equals 1 on Mondays and 0 otherwise, sdlight_t and fdlight_t are dummy variables that equal 1 on Mondays following the respective spring and fall changes in daylight saving time, dlight_t is a dummy variable that equals 1 on Mondays following fall *and* spring changes in daylight saving time, and $\varepsilon_{p,t}$ and $\varepsilon^*_{p,t}$ are error terms.

The equality of mean daylight-saving and non-daylight-saving weekend returns implies that δ_s, δ_f, and δ_d in the above regressions equal zero. The OLS t statistics in Table 1 allow rejection of that hypothesis for δ_f and δ_d, but not for δ_s. Kamstra et al. (2000) present test statistics for the second regression, but not for the first.[1] Thus, the first new information in Table 1 is that, on average, Monday returns that follow spring changes in daylight saving time are not abnormally low compared with Monday returns that follow other weekends. This finding holds for all three sets of t statistics. Since spring and fall changes associated with daylight saving time occur with equal frequency, differences in the t statistics for δ_s and δ_f cannot be attributed to differences in sample size.

However, the heteroscedasticity adjustments do offer useful insights into differences in the t statistics for these coefficients. Neither adjust-

[1] Eliminating data for 1998 and running the second regression for each of the indexes in Table 1 produces t statistics for δ_d that are virtually identical to the t statistics Kamstra et al. (2000) report.

ment impacts the t statistics for δ_s in a meaningful way, but the impact is dramatic on the t statistics for δ_f and δ_d. The *least* significant OLS t statistic for δ_f in Table 1 is -2.91 (for the S&P 500 index), but the *most* significant heteroscedasticity-consistent t statistic is only -1.63 (for the NYSE equally weighted index). Thus, I reject the hypothesis that δ_f equals zero at the 0.01 level for every index in Table 1 with OLS regressions, but I fail to reject that hypothesis at the 0.10 level with the White (1980) and GJR adjustments.

Kamstra et al. also adjust for time-varying conditional heteroscedasticity, but to increase the power of their tests they only examine δ_d, not δ_s and δ_f. They report (in footnote 12) that δ_d "is typically significant at the 10-percent level." My results in Table 1 are consistent with that finding for the NYSE equally weighted and value-weighted indexes and for the AMEX equally weighted index. However, δ_d is not significant at the 0.10 level in the GJR regressions for the AMEX value-weighted index or for the S&P 500. White (1980) corrections render δ_d insignificant for these two indexes and for the NYSE value-weighted index. Thus, the significance of δ_d depends on the index I use and on the type of heteroscedasticity adjustment I impose.[2]

Of course, the most significant heteroscedasticity-consistent t statistic for δ_d (-1.76) in Table 1 still marginally supports the daylight-saving anomaly based on classical fixed-level hypothesis tests. However, given the large number of daily returns in the regressions (8,057), fixed-level hypothesis tests may be inappropriate. Therefore, I also employ Bayesian sample-size adjustments. With diffuse priors, the critical absolute value for the t statistic that equates the posterior odds for the null and alternative hypotheses given the data is approximately 3.00. The t statistic of -1.76 suggests that the null is over 364 times more likely than the alternative. Thus, Bayesian sample-size adjustments weaken the evidence favoring the daylight-saving anomaly dramatically.[3]

Whether I use a classical or Bayesian approach, the impact of the heteroscedasticity adjustments on the t statistics for δ_f and δ_d suggests the presence of outliers in the sample. Figure 1 illustrates the size of those outliers by plotting weekend returns of the NYSE equally weighted index following spring and fall changes in daylight saving time. Conspicuous in the figure are the two large negative returns in the fall of 1987 (-0.08735) and 1997 (-0.04966). Both outliers are associated with international events. The 1987 outlier occurred on 26 October 1987, a few weeks following the international stock market crash. An excerpt from the *Washington Post* on 27 October 1987 states that "Nervous investors around the world dumped shares yesterday [Monday] in a massive selloff that pushed the U.S. stock market into its second-biggest one-day plunge, surpassed only by 'Black Monday' ... ." An article in the *Los Angeles Times* on Tuesday, 28 October 1997 explains the market decline for the second outlier in these words: "Stock prices here and around the world plummeted Monday [in response to the Asian crisis] in a decline so

[2] I also examine the significance of δ_d in two other ways. First, I use Whitney K. Newey and Kenneth D. West (1987) corrections for heteroscedasticity *and* serial correlation through lag 5 for the portfolios in Table 1. These adjustments produce results that differ only trivially from the White (1980) results. Second, I examine size-ranked deciles of NYSE and AMEX stocks. White (1980) and Newey and West (1987) corrections yield five (six) t values that are insignificant at the 0.10 level for the NYSE (AMEX) decile portfolios. GJR corrections yield three (six) insignificant t values for the NYSE (AMEX) portfolios. For both the NYSE and AMEX portfolios, large-cap firms (those in deciles 9 and 10) have insignificant t values for δ_d with all heteroscedasticity adjustments. Thus, if the anomaly is robust, it is robust only among small firms. However, why changes in daylight saving time would affect traders in small- but not large-cap stocks is not immediately obvious.

[3] When I compare daylight-saving to non-daylight-saving weekend returns without including returns for other days of the week, the sample size drops to 1,548. The t statistic that equates the posterior odds of the null and alternative hypotheses in that specification is 2.80. The most significant OLS t statistic in regressions that include only weekend returns is -2.61. The Bayesian posterior odds ratio for that t statistic in that sample is 1.64:1. The posterior odds ratio for the most significant t value (-1.73) from the White (1980) regressions that use only weekend returns is 11.05:1. Thus, with or without heteroscedasticity adjustments and with or without other (non-weekend) daily returns, the Bayesian posterior odds ratio consistently favors the null hypothesis that no daylight-saving anomaly exists over the alternative hypothesis that it does exist in this sample. For applications of Bayesian sample-size adjustments with other anomalies, see Robert A. Connolly (1989, 1991) and Eric C. Chang et al. (1993, 1998).

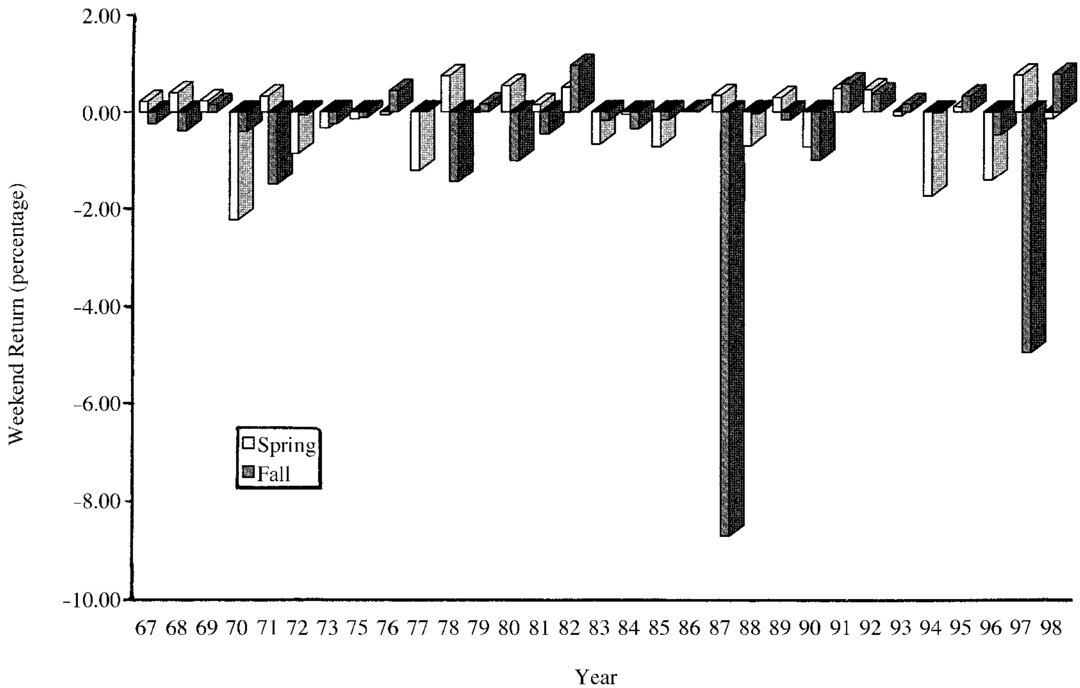

FIGURE 1. WEEKEND RETURNS OF THE NYSE EQUALLY WEIGHTED INDEX FOLLOWING FALL AND SPRING CHANGES IN DAYLIGHT SAVING TIME (1967–1998)

unrelenting it triggered two extraordinary trading halts on the New York Stock Exchange, including one that led to an early close ... ."

The two large declines that account for the significant difference between daylight-saving weekend returns and other weekend returns were precipitated by international events that could hardly have been caused by the switch to daylight saving time, although that switch might have worsened their impact. Removing these outliers would mean that the difference is no longer significant, but one would then have no statistical basis for assessing their impact. To put these two outliers in a broader context, I now use the Kolmogorov-Smirnov test of the equality of the unknown distribution functions of U.S. daylight-saving and non-daylight-saving weekend returns. The cumulative frequency distributions of daylight-saving and non-daylight-saving weekend returns are depicted for the NYSE equally weighted index in Figure 2. The Kolmogorov-Smirnov test examines the maximum absolute distance between those distributions. If that distance is too large, the hypothesis of equal distributions is rejected. The large-sample approximate critical value for rejection of the null at the 0.20 level with a two-tail test and unequal sample sizes is given in Wayne W. Daniel (1978) by

$$\mathrm{KS} = 1.07 \times \sqrt{\frac{N_{\text{dlight}} + N_{\text{other}}}{N_{\text{dlight}} \times N_{\text{other}}}}$$

where the N's are sizes of the respective samples. The critical value of KS in my sample is 0.1387. The largest absolute distance between the cumulative frequency distributions in Figure 2 is 0.1162.[4] Thus, I cannot reject the equality of the distributions of daylight-saving and non-daylight-saving weekend returns for the NYSE

[4] The largest negative return for the NYSE equally weighted index (−0.15000) occurs on the "regular" Monday of the international stock market crash on 19 October 1987.

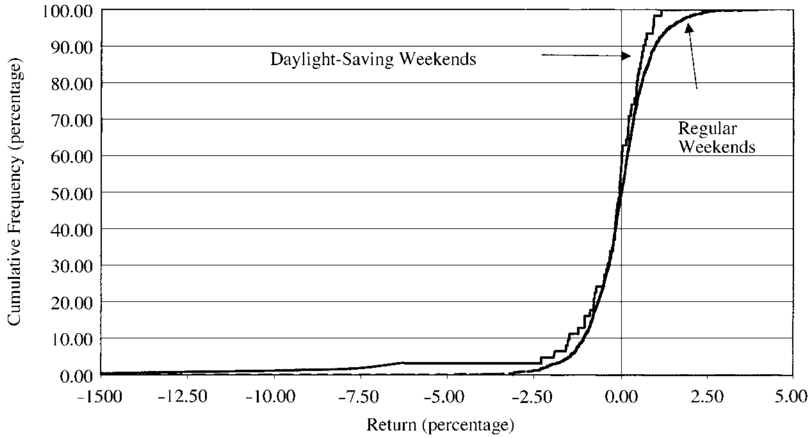

FIGURE 2. CUMULATIVE FREQUENCY DISTRIBUTIONS OF RETURNS FOLLOWING CHANGES IN DAYLIGHT SAVING TIME AND REGULAR WEEKENDS (NYSE EQUALLY WEIGHTED INDEX)

equally weighted index even at the 0.20 level. In unreported tests, I also fail to reject that equality for the other U.S. indexes at the 0.20 level.[5]

Of course, these results relate only to the U.S. daylight-saving anomaly—or do they? Kamstra et al. (2000) also examine the daylight-saving anomaly in Canada, Germany, and the United Kingdom. In Germany, where the daylight-saving date patterns differ from those in the United States, Kamstra et al. find no anomaly. In Canada and the United Kingdom, they find an effect similar to the one they report for the United States. Unfortunately, the international events that taint the evidence in U.S. markets also confound the interpretations for Canada and the United Kingdom. The returns on the TSE 300 index in Canada following fall changes in daylight saving time in 1987 and 1997 were −0.07563 and −0.06174. Corresponding returns for the Total Share index in the United Kingdom were −0.06189 and −0.02606. The problem is that the international crises that induced such large negative returns in the United States also induced these large negative returns because fall changes in daylight saving time in Canada and the United Kingdom coincide with the fall changes in daylight saving time in the United States.[6] Thus, separating the *incremental* impact of sleep desynchronosis from the effects of the crises remains difficult. In the extreme case, when sleep desynchronosis has no effect in Canadian and U.K. markets, removing the outliers from the sample dramatically weakens the hoped-for independent corroboration of the U.S. findings.[7] Even if the daylight-saving anomaly in Canada and the United Kingdom proves robust, my tests show that the anomaly is not robust in the United States. Thus, the U.S. data do not corroborate the *potential* robustness of the anomaly in the foreign markets.

II. Conclusion

The recent finding that mean weekend returns are significantly lower following changes in daylight-saving vis-à-vis other weekends is not robust. The mean difference is significant only for fall changes in daylight saving time, and the

[5] Figure 2 suggests that the medians for the daylight-saving and non-daylight-saving weekend returns are approximately equal. Using a Wilcoxon sign test, I fail to reject the equal medians hypothesis at the 0.10 level for every index in my sample.

[6] According to Kamstra et al. (2000), Canada shares with the United States a largely common daylight-saving date pattern, and the U.K. fall daylight-saving change has occurred on the last Sunday in October since 1985.

[7] According to Kamstra et al., the estimates of the mean fall daylight-saving weekend returns for Canada and the United Kingdom are −0.0037031 and −0.0043035, respectively. Removing the 1987 and 1997 fall daylight-saving weekend returns from these samples leaves the estimates at 0.00111 for Canada and at −0.00125 for the United Kingdom.

fall difference is significant only when no adjustments are made for heteroscedasticity caused by outliers associated with international events. In the total sample (spring plus fall), the difference remains marginally significant for some indexes under classical fixed-level hypothesis tests. However, Bayesian sample-size adjustments produce posterior odds ratios that, by wide margins, favor the null hypothesis of no daylight-saving anomaly over the alternative hypothesis that the anomaly exists. I also fail to reject the hypothesis that the distributions of daylight-saving and non-daylight-saving weekend returns are equal. Though my analysis focuses on U.S. markets, the international events that create the illusion of significance in the United States also influence markets in the United Kingdom and Canada whose fall daylight-saving changes correspond to changes in the United States. Thus, the apparently corroborating evidence from these other markets should be interpreted with caution.

Of course, caution is also important in interpreting my findings. They do not imply that sleep desynchronosis could not or does not amplify the impact of negative news. Interviews with market participants conducted by Robert J. Shiller (1991) show little evidence that fundamentals account for the severity of the international stock market crash in 1987. Perhaps sleep desynchronosis also worsened the effects of the crash in the United States. However, the crash occurred on a *non-daylight-saving* Monday. Indeed, the change in sleeping patterns from weekdays to weekends occurs with much greater frequency and is very plausibly more pronounced than the change in sleeping patterns between daylight-saving and non-daylight-saving weekends. Thus, sleep desynchronosis may contribute to the so-called "day-of-the-week" effect on non-daylight-saving Mondays also.[8] To my knowledge, that conjecture has not been tested. However, my results show that the incremental effect of the change in daylight saving time on the "day-of-the-week effect" (whatever its cause) is not robust. Therefore, I propose that we not lose more sleep over the recently advanced daylight-saving anomaly.

[8] Based on the evidence in Ko Wang et al. (1997), if macroeconomic news arrives in random intervals, explaining the day-of-the-week effect based on sleep desynchronosis would require that sleep desynchronosis be a more serious problem at the end, rather than at the beginning, of the month.

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