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A careful re-examination of seasonality in international stock markets: Comment on sentiment and stock returns

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ABSTRACT

In questioning Kamstra, Kramer, and Levi's (2003) finding of an economically and statistically significant seasonal affective disorder (SAD) effect, Kelly and Meschke (2010) make errors of commission and omission. They misrepresent their empirical results, claiming that the SAD effect arises due to a "mechanically induced" effect that is non-existent, labeling the SAD effect a "turn-of-year" effect (when in fact their models and ours separately control for turn-of-year effects), and ignoring coefficient-estimate patterns that strongly support the SAD effect. Our analysis of their data shows, even using their low-power statistical tests, there is significant international evidence supporting the SAD effect. Employing modern, panel/time-series statistical methods strengthens the case dramatically. Additionally, Kelly and Meschke represent the finance, psychology, and medical literatures in misleading ways, describing some findings as opposite to those reported by the researchers themselves, and choosing selective quotes that could easily lead readers to a distorted understanding of these findings.

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"The reports of my death are greatly exaggerated."
– Mark Twain

1. Introduction

It is testimony to the widespread interest in seasonality in equity returns, corresponding in strength and nature to the presence of SAD in markets at different latitudes and hemispheres, that researchers such as Kelly and Meschke (2010; hereafter KM) are drawn to investigate the phenomenon. We establish that the SAD effect survives and is even strengthened by KM's examination. We show, with KM's own data, that the SAD effect first documented by Kamstra, Kramer, and Levi (2003; henceforth KKL2003) is a robust, economically meaningful, and statistically significant feature of financial markets. We also show that in challenging KKL2003, KM take liberties with the data, the literature they cite, and the literature they choose not to cite.

Errors of commission and omission emerge on even casual inspection of their estimation techniques. Perhaps most pertinent, KM mislead readers by describing the SAD effect as a turn-of-the-year effect when in fact their model (and our model) controls explicitly for the turn of the year. KM also introduce a new specification (consisting of three variables to capture the SAD effect) and then test the significance of the three variables *one-at-a-time*, rather than performing a joint test with a (standard) *F*-test. As we show, joint tests strongly reject the null of no SAD effect, with their data and their model, but one-at-a-time tests are compromised by multicollinearity in their new three-variable specification, further misleading readers that there is no SAD effect. Further, KM do not explore joint tests of the SAD hypothesis *across their data series*. Instead they use single-series-at-a-time tests and ordinary least squares (OLS) estimation, and they ignore modern methods such as system-of-equations generalized method of moments (GMM). KM use heteroskedasticity-robust standard errors, when heteroskedasticity and autocorrelation consistent (HAC) standard errors with data-dependent window width selection techniques are appropriate. GMM and HAC standard errors, which are commonly employed, are powerful and robust techniques that allow precise estimation of parameters and standard errors even in the presence of autocorrelation and heteroskedasticity. GMM is the standard for

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performing system-of-equations estimation with equity returns data. See Hodrick and Zhang (2001), Jagannathan and Wang (2007), Bekaert et al. (2009), and Albuquerque et al. (2009). Nonetheless, we find significant evidence of the SAD effect even using OLS methods such as seemingly unrelated regression with panel/time-series estimation.

As Hirshleifer and Shumway (2003) argue persuasively, joint tests using panel data are more powerful than one-at-a-time single equation tests. We acknowledge that in KKL2003 we did not exploit the full power of systems equation estimation, joint tests, or the most powerful HAC standard error estimates available. The aim was to soundly show that the SAD effect is large and easily statistically significant, and so we took a conservative testing approach. Since KM question the very existence of a SAD effect, it is appropriate for them to give the established result the benefit of the doubt, and use the most powerful tests available. When we perform panel/time-series estimation and joint tests on KM's data, exploiting GMM and HAC, we easily reject the null of no SAD effect. Had KM paid attention to the characteristics of the data, for instance that their own coefficient estimates are almost always the sign and magnitude predicted by SAD, they would have reached different conclusions. KM's own results, as inefficient as their test procedures are, strongly support the SAD hypothesis, but this support is obscured by their reporting conventions and introduction of spuriously correlated regressors, as we detail below.

We also note the selective choice of studies KM cite and their incomplete description of the large and growing body of research on the SAD effect. First, they paint a one-sided picture of the SAD literature in finance. An even-handed exposition would cite not only the papers that contest the SAD hypothesis, but also the growing list of supportive papers. They write, "While there is a large and growing literature that uses KKL2003 to motivate their research, several other studies are critical of the SAD hypothesis" (p. 1309). There is no mention or analysis of the particular papers that find support for the SAD hypothesis, in spite of the fact that in some cases those papers use virtually the same data KM consider but come to very different conclusions. Second, there are multiple instances in which KM mischaracterize several established results in the psychology literature. For instance, they claim there is "mixed" evidence that depression is associated with increased risk aversion when in fact the evidence is overwhelmingly supportive on this point. And third, they misrepresent several papers in the finance literature, for example implying that Goetzmann and Zhu (2005) overturn the relationship between length of day and investor behavior when in fact Goetzmann and Zhu do not study length of day (nor do they claim to). We elaborate on all of these shortcomings below.

We describe the statistical and econometric problems inherent in KM's analysis in Section 2. In Section 3 we highlight the errors and bias KM reveal in their discussion of the finance literature. In Section 4 we describe KM's errors in citing the psychology and medical literatures. In Section 5 we revisit the empirical analysis using methods that do not exhibit the econometric problems of KM's analysis; we report results based on various model specifications, including single-equation OLS as well as several panel/time-series models that exploit cross-market correlation. Finally, in our Appendix A we describe the problems inherent in KM's Appendix A.

2. Statistical/econometric problems

In this section we describe statistical problems inherent in KM's analysis. Because KM employ single-equation estimation techniques, our discussion in this section mostly refers to results based on these methods. In Section 5 we report on more powerful system-of-equations methods appropriate for the analysis of cross-correlated series such as we have here.

2.1. Mechanical inducement of statistical significance

In describing their concern with the model specification KKL2003 employ, KM write:

> To illustrate, consider if returns were quite large during winter but in fall no different from spring and summer. In a specification with a fall and a fall–winter dummy, the fall–winter dummy would capture the positive winter returns and implicitly attribute them to the entire period from fall to winter, ... Hence, the overlap between the two dummies would mechanically induce statistical significance where a properly specified model would find none. (p. 1309)

There are many problems with KM's illustration. First, while we challenge the validity of KM's illustration of a "mechanical effect" below, even if we accept the validity of their illustration, this "mechanical effect" disappears when one controls for the large winter returns (i.e., when one controls for a turn-of-the-year effect). That is, their illustration is based on a misspecified model we do not estimate, with two *overlapping dummy variables* and *no control for a turn-of-the-year effec*t. In our analyses, we always control for a turn-of-the-year effect (and we do not employ overlapping dummy variables). In extended analysis we describe below, we find strong evidence supporting the SAD effect, even when controlling for a turn-of-the-year effect in multiple ways. That is, evidence in support of the SAD effect is not an artifact of failing to control for a turn-of-the-year effect. KM's suggestion to the contrary is simply incorrect, as we show.

Second, properly specified *tests* are just as important as properly specified regression models. After controlling for a turn-of-the-year effect, a careful test of KKL2003's SAD hypothesis would explore the joint significance of the SAD variables, namely the fall dummy and the length of night variable. *F*-tests are appropriate when one has a joint hypothesis on coefficients in a regression, in particular a regression that controls separately for, in this case, a turn-of-the-year effect. *F*-tests can also have much greater power than one-at-a-time *t*-tests when the individual variables (the length of night variable and the fall dummy variable in this case) are overlapping and correlated – features of these variables that KM enthusiastically highlight. But KM employ one-at-a-time *t*-tests on these variables, never discussing the joint significance of the variables intended to capture the SAD effect. Although we did not report these joint tests in KKL2003, we did perform such tests (in the context of a model that properly controlled for the turn-of-the-year effect) and the tests indicate the individually negative significant fall dummy variable and positive significant length of night variable are strongly *jointly* significant. (Note that we provide these tests below.) That is, the individual significance of each variable is not "mechanically" induced by ignoring large positive returns around the turn of the year. Rather than performing such joint tests when they propose a re-examination of the SAD effect in their Section 6.1, KM instead introduce a new, more disaggregated specification of KKL2003's SAD model and perform one-at-a-time *t*-tests on this new, less parsimonious specification. They remark:

> In this section we show that the SAD interaction term does not differ materially from a fall–winter dummy and that the SAD effect is mechanically driven by a *de facto* overlapping dummy-variable specification and higher returns around the turn of the year. ...A simple way to test whether the overlap of the SAD and fall variables drives the significant results on the fall dummy is to split the SAD variable into fallSAD and

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