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Predicting Returns with Managerial Decision Variables: Is There a Small-Sample Bias?

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ABSTRACT

Many studies find that aggregate managerial decision variables, such as aggregate equity issuance, predict stock or bond market returns. Recent research argues that these findings may be driven by an aggregate time-series version of Schultz's (2003, *Journal of Finance* 58, 483–517) pseudo market-timing bias. Using standard simulation techniques, we find that the bias is much too small to account for the observed predictive power of the equity share in new issues, corporate investment plans, insider trading, dividend initiations, or the maturity of corporate debt issues.

EQUITY MARKET TIMING IS THE TENDENCY OF FIRMS to issue equity before low equity market returns. In contrast, *pseudo* market timing, as recently defined by Schultz (2003), is the tendency of firms to issue equity *following high* returns. In small samples, pseudo market timing can give the appearance of genuine market timing. Consider an extreme example of pure pseudo market timing with only two returns. If the first return is high, equity issues rise; if the first return is low, equity issues fall. The first return can be mechanically explained ex post: Relatively low equity issues precede a high first return and relatively high equity issues precede a low first return. Even though the returns are random, equity issues "predict" in-sample returns more often than not.

In a provocative article, Butler, Grullon, and Weston (2005) argue that an aggregate version of the pseudo market-timing bias explains why the variable studied in Baker and Wurgler (2000), namely, the equity share in new equity and debt issues, predicts stock market returns in-sample. While Butler et al. focus their critique on a particular link between financing patterns and stock returns, their general argument—that the pseudo market-timing bias extends to time-series predictive regressions—is of considerably broader interest, because a number of aggregate managerial decision variables that have been used in predictive regressions, not just equity issuance, are correlated with returns in the direction that induces a bias.

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Consider the following examples. Seyhun (1992, 1998) and Lakonishok and Lee (2001) find that high aggregate insider buying appears to predict high stock market returns. However, aggregate insider buying increases as stock prices fall, raising the possibility that their result is driven by the bias. Baker, Greenwood, and Wurgler (2003) find that a high ratio of long-term to total debt issuance appears to predict lower excess bond returns. However, if long-term debt issuance increases as the term spread narrows (and thus as excess bond returns rise), the potential for bias emerges. Lamont (2000) finds that corporate investment plans forecast lower stock market returns, yet planned investment increases with stock prices. Finally, Baker and Wurgler (2004) find that the aggregate rate of dividend initiation is inversely related to the future returns on dividend payers over non-payers. Again, however, the initiation rate increases with the relative returns on payers. Thus, in each of these papers, in addition to those involving equity issuance, the bias is a potentially serious concern. The size of the bias must therefore be empirically pinned down before any of the above results can be accepted (or rejected) with confidence.[1]

In this paper, we empirically estimate the aggregate pseudo market-timing bias that affects predictive regressions based on managerial decision variables. We start by observing that none of this is a fundamentally new question in asset pricing or time-series econometrics. While Butler et al. (2005) do not make the connection, aggregate pseudo market timing is simply a new name for the small-sample bias studied by Stambaugh (1986, 1999), Mankiw and Shapiro (1986), Nelson and Kim (1993), Elliott and Stock (1994), Kothari and Shanken (1997), Amihud and Hurvich (2004), Lewellen (2004), Polk, Thompson, and Vuolteenaho (2004), Campbell and Yogo (2005), and others. These studies focus on valuation ratios, such as aggregate dividend yield or market-to-book, which exhibit an extreme and mechanical form of pseudo market timing—for example, when the market crashes, the dividend yield automatically rises. Our predictors are different, but the bias is the same, and it can be estimated using the same standard methods.

Specifically, when the predictor variable is stationary (all of the managerial decision predictors we consider are theoretically stationary by construction), it is straightforward to run simulations and assess the magnitude of the small-sample bias induced by aggregate pseudo market timing. In these simulations, we impose the null hypothesis of no genuine market timing and varying degrees of pseudo market timing, thereby mechanically tying the equity share and other candidate predictor variables to contemporaneous returns.

Our simulation results are, in a sense, a big letdown. Contrary to the conclusions of Butler, Grullon, and Weston (2004, 2005), the aggregate pseudo market-timing bias is only a minor consideration for every variable we consider. The

[1] It is important to note that the aggregate pseudo market-timing bias discussed in this paper, and in Butler et al. (2004, 2005), is a purely time-series phenomenon. It is not the bias emphasized by Schultz (2003), who discusses the potential for bias in "event time" studies of abnormal IPO returns. There, the problem arises when the *number* of firms going public increases following high abnormal returns on previous IPOs. For studies of that conceptually distinct type of bias, see Schultz (2003, 2004), Ang, Gu, and Hochberg (2004), Dahlquist and de Jong (2004), and Viswanathan and Wei (2004). For a general discussion, see Ritter (2003).

results can be described in terms of the theoretical determinants of the bias. As shown in Stambaugh (1986, 1999), the bias is most severe when the sample is small, the predictor is persistent, and the predictor's innovations are highly correlated with returns. It turns out that empirically relevant values for these parameters are unable to generate a significant bias.

For example, aggregate pseudo market timing of the degree observed in-sample has less than a 1% chance of reproducing the predictive power of the equity share variable. A reasonable point estimate is that less than 2% of that variable's ordinary least squares (OLS) coefficient is due to pseudo market timing. Even when we impose, counterfactually, pure pseudo market timing, forcing the correlation between innovations in the equity share and returns to one, approximately 88% of the OLS coefficient remains unexplained. When we further increase the autocorrelation of the equity share by three standard deviations from its actual level, over 80% of the OLS coefficient still remains unexplained, and pseudo market timing of this sort still has less than a 1% chance of equaling the actual predictive coefficient. The bottom line is that in a sample of 75 years, small-sample bias is quite modest compared to the equity share's actual coefficient. Results for other predictors are qualitatively similar. In no case we consider is the bias large enough to cast doubt on OLS-based inferences about predictive power.

Because previous research makes aggregate pseudo market-timing bias arguments in the context of "regime changes" or "large shocks," at least informally, we also consider simulations that formally allow for regime shifts in the return series. Similar inferences obtain from this set of simulations. If anything, adding regime changes tends to reduce the bias somewhat. We conclude, in summary, that the aggregate pseudo market-timing bias is a minor concern for the predictive regressions based on managerial decision variables that appear in the literature.

Our conclusions differ markedly from earlier work on aggregate pseudo market-timing bias, in particular Butler et al. (2004). The reason is that these authors do not present any direct estimates of the bias, but instead build a case from several indirect exercises. The central approach in both papers can be boiled down to a strategic process of removing data that are identified ex post as most consistent with genuine market timing. In the first paper, the authors crudely remove from the analysis only crash years that are preceded by a high equity share. In the second paper, the procedure is more insidious. The authors remove the effect of a "regime change" in 1982 that is identified ex post with data through 2001. This process is exactly equivalent to searching for an indicator variable that removes as much of the variation in bond returns as is mathematically possible. These manipulations that have no a priori justification are not trivial for time series regressions that involve fewer than 75 data points and an R-squared of less than 25 percent. Not surprisingly, in both cases, the predictive power of the managerial decision variable falls. It is worth noting that the central question is whether the issuance of relatively more equity and long-term debt reliably and genuinely *preceded* low stock market and long-term bond returns, respectively. The strategic removal of data makes for an interesting analysis of robustness, perhaps, but it has nothing to do with pseudo market timing, and the answer to the central question remains an emphatic yes. In the working paper version of this article (Baker, Taliaferro, and Wurgler (2004)), we provide a detailed critique of their approach; however, we omit further discussion here because it is not relevant to the central issue of small-sample bias.

The remainder of the paper is organized as follows. Section I reviews the small-sample bias known as the aggregate pseudo market-timing bias and places it within an empirical framework that can be used to run simulations. Section II describes the data. Section III reports simulation results. Section IV concludes.

I. Estimating the Aggregate Pseudo Market-Timing Bias

A. Empirical Framework

An important conceptual point is that the aggregate pseudo market-timing bias—in other words, the pseudo market-timing bias in the context of time-series predictive regressions—is just a different name for an issue that is well understood in the financial econometrics literature. A common empirical framework is the system used by Mankiw and Shapiro (1986), Stambaugh (1986), and subsequent authors:

$$r_t = a + bX_{t-1} + u_t, \quad u_t \sim \text{i.i.d.}\left(0, \sigma_u^2\right) \tag{1}$$

$$X_t = c + dX_{t-1} + v_t, \quad v_t \sim \text{i.i.d.}\left(0, \sigma_v^2\right), \tag{2}$$

where r denotes returns on the stock market, for example, and X is a candidate predictor variable such as aggregate equity issuance. Equation (1) is the predictive regression, while equation (2) describes the evolution of the predictor. The contemporaneous covariance between the disturbances is σ_{uv}. We assume that the predictor is stationary, so that $|d| < 1$.

To connect this framework to aggregate pseudo market timing, we adapt and (very closely) paraphrase the following discussion from Stambaugh (1999, p. 379), who illustrates why the OLS estimate $\hat{b}$ is biased in its simplest possible setting. Consider repeated samples of only two observations, (r_1, X_0) and (r_2, X_1), so that $\hat{b}$ in each sample is just the slope of the line connecting these points, $\hat{b} = \frac{r_2 - r_1}{X_1 - X_0}$. Suppose $b = 0$, meaning that managers do not have genuine market timing ability; $d \approx 1$, so that innovations to equity issuance are highly persistent; and $\sigma_{uv} > 0$, meaning that managers are pseudo market timers, increasing equity issuance as stock prices rise. Consider those samples in which the first return is relatively low, $(r_2 - r_1) > 0$, or $u_2 > u_1$ (since $b = 0$). On average, in this case, u_1 is negative, and because $\sigma_{uv} > 0$, v_1 is also negative, so is $(X_1 - X_0)$. Thus, $\hat{b} < 0$ on average. Now consider samples in which the first return is relatively high. On average, in this case, u_1 is positive, hence v_1 is also positive, and therefore $(X_1 - X_0)$ is positive. Again, $\hat{b} < 0$ on average. Thus, on average across all samples, one should see a negative relation between equity issuance and subsequent returns, even though no timing ability exists. (In settings in which $\sigma_{uv} < 0$, it follows that $\hat{b} > 0$ on average.) This is the aggregate pseudo market-timing bias.

This two-period example highlights the three main determinants of the bias. First, as the pseudo market-timing covariance σ_{uv} goes to zero, the bias disappears because the signs of u_1 and v_1 are no longer connected. Second, as the persistence of the predictor d goes to zero, the bias shrinks because the sign of $(X_1 - X_0)$ is less tightly linked to the sign of v_1 and thus to the sign of $(r_2 - r_1)$. But even when $d = 0$, there is still some correlation and thus some bias. Third, as the number of observations T increases, the bias approaches zero because (with $b = 0$) the scatter of points becomes a horizontal cloud of these two-point clusters.

In the T-period case, Stambaugh (1986, 1999) shows that the size of the bias in $\hat{b}$ when u and v are normally distributed in the system above is

$$E[\hat{b} - b] = \frac{\sigma_{uv}}{\sigma_v^2} E[\hat{d} - d]. \tag{3}$$

Kendall (1954) shows that the downward small-sample bias in the OLS estimate of d is approximately $-(1 + 3d)/T$. Mentally substituting this expression into equation (3), one sees that the pseudo market-timing correlation, the predictor's persistence, and the sample size remain the key determinants of bias in the T-period case.

B. Simulation Procedure

As mentioned in the introduction, a large literature considers the bias in $\hat{b}$ when X is a scaled-price variable such as the aggregate dividend yield or book-to-market. Because dividends and book values are persistent, innovations in the dividend yield and the aggregate market-to-book ratio are highly correlated with contemporaneous returns, and thus an extreme, mechanical pseudo market-timing correlation arises. While our predictors are different from those usually considered in this literature, the nature of the underlying bias is identical and it can be estimated using the same empirical techniques developed in, for example, Nelson and Kim (1993) and Kothari and Shanken (1997).

In particular, when the predictor is stationary, it is straightforward to simulate equations (1) and (2) to determine the magnitude of the bias. The predictor variables we consider are theoretically stationary by construction (although in any given small sample, of course, one might not be able to reject a unit root). In the simulations, we impose the null hypothesis of no predictability ($b = 0$, so the predictive term in equation (1) drops out under the null) and vary the pseudo market-timing correlation, ρ_{uv}, and the other key parameters, d and T, to see whether a significant bias in $\hat{b}$ obtains for empirically relevant parameters.

An example illustrates the basic procedure. In our benchmark simulations, we use the empirically relevant parameter set: the bias-adjusted estimate of d ($\hat{d} + \frac{1+3\hat{d}}{T}$); the empirical distribution of, and hence the correlation between, OLS estimates of u and v, where u is obtained under the null of $b = 0$ and v is obtained with the bias-adjusted d; and the number of observations actually available for the given predictor as T. We then simulate $100 + T$ values for r and X, starting with the actual X_0 and drawing with replacement from the empirical joint distribution of u and v. We throw away the first 100 values, leaving a sample size of T, which we use to compute a simulated OLS estimate, $\hat{b}$. We repeat this procedure 50,000 times to plot the distribution of simulated OLS estimates, and we locate the actual estimate in this distribution. We then vary one or more of the parameters, generate a new simulated distribution, again locate the actual OLS estimate, and so forth.

An alternative approach is to compute reduced-bias p-values directly with the recently developed methods of Amihud and Hurvich (2004) and Polk et al. (2004). Because these two procedures lead to virtually identical inferences, we focus primarily on the simulation results, which allow us to consider situations in which the degree of pseudo market timing and the level of persistence in equity issues are counterfactually high.

II. Data

A. Predictor Variables

We focus on six aggregate managerial decision variables. Five have previously been examined in a predictive regression context, and all six, based on the a priori considerations outlined in the introduction, are likely to be subject to at least some degree of aggregate pseudo market-timing bias. We replicate the methodology of the original studies where possible. When we face a choice about the return prediction horizon, however, theory is no guide, in which case we use the horizons that are "strongest" or most emphasized in the original studies.

The first four predictor variables are used to forecast 1-year-ahead real stock market returns. The equity share in new issues, that is, the ratio of aggregate gross equity issuance to aggregate gross equity plus debt issuance, is derived from the *Federal Reserve Bulletin* data and is discussed in Baker and Wurgler (2000) and Butler et al. (2005). Henderson, Jegadeesh, and Weisbach (2004) study the equity share variable using international data. By construction, this variable isolates the security choice decision from the level-of-external-finance decision. In the *Bulletin* data, equity issues include common and preferred and debt issues include public and private. The annual series covers 1927 through 2001.

Detrended equity issuance is also based on the *Bulletin* annual gross equity issuance series. We take the log difference of aggregate gross equity issuance in year t and the average gross equity issuance over the previous 5 years ($t - 1$ through $t - 6$). This annual series covers 1932 though 2001. We are not aware of a prior study that uses exactly this variable.[2] It gives a different perspective on aggregate equity issuance than the equity share variable and will prove useful in our discussion of that variable below.

[2] See Lamont (2002) on the predictive power of net new lists and Dichev (2004) on net equity capital flows.

Lamont (2000) studies planned investment growth. This series is based on a Commerce Department survey of firms' planned capital expenditure in the coming year. Lamont defines real planned investment growth as planned capital investment in year t divided by actual capital investment in $t - 1$ all minus the growth in the national income accounts' nonresidential fixed investment deflator. As Lamont notes, investment plans for year t are reported as of February of t. Our annual series of real planned investment growth for 1947 through 1992 comes from Lamont's website.

Seyhun (1992, 1998) and Lakonishok and Lee (2001) study aggregate insider buying. Seyhun shared with us his monthly series, derived from the SEC's *Ownership Reporting System* file, on the fraction of publicly traded firms with net insider buying, as plotted in Seyhun (1998, p. 117). We average this series across months to construct an annual insider buying series from 1975 through 1994.

Baker et al. (2003) use the long-term share in debt issues, that is, the ratio of aggregate long-term debt issuance to aggregate short- plus long-term debt issuance, to forecast cumulative 3-year excess returns on long-term government bonds. The debt issuance data are from the Federal Reserve *Flow of Funds*.[3] Short-term debt is primarily bank loans, with issuance defined as the level of short-term credit market debt outstanding. Long-term debt is primarily corporate bonds. Under the assumption that one tenth of long-term debt outstanding matures each year, long-term issuance is defined as the annual change in the level of long-term debt outstanding plus one tenth the lagged level. The long-term share series is annual from 1953 through 2000.

The aggregate rate of dividend initiation, that is, the percentage of the previous year's surviving non-payers that paid positive dividends this year, is used by Baker and Wurgler (2004) to forecast 3-year cumulative excess returns of dividend payers over non-payers. They derive aggregate dividend payment series from aggregations of COMPUSTAT data. The dividend initiation series is annual from 1963 through 2000.

We refer the reader to the corresponding papers for more details on these variables. We omit summary statistics, because all of our analysis uses a standardized version of each predictor that has zero mean and unit variance across the full sample period.

B. Returns

Real annual stock market returns are based on the CRSP NYSE/AMEX/Nasdaq value-weighted and equal-weighted return series, converted to real terms using the Consumer Price Index from Ibbotson Associates. The mean value-weighted (equal-weighted) real stock market return from 1928 through 2002 is 8.10% (13.30%) and the standard deviation is 20.47% (30.91%). Because Lamont's (2000) planned investment variable is dated the end of February, we match it to subsequent March–February stock market returns.

[3] These data come from corporate balance sheets and include a broader range of liabilities. They therefore do not match the totals from the *Federal Reserve Bulletin* used in computing the equity share in new issues.

Excess returns on long-term Treasury bonds over bills are from Ibbotson Associates; their government bond returns series uses data from the *Wall Street Journal* for 1977 through 2000 and the CRSP Government Bond File for 1976 and earlier. The mean 3-year cumulative excess return for years starting with 1954 through 1998 (and ending with 1956 through 2000) is 2.06% with a standard deviation of 16.71%.

Finally, excess returns on dividend payers over non-payers are based on the book-value-weighted return indexes of payers and non-payers derived from CRSP and COMPUSTAT and described in Baker and Wurgler (2004). The mean 3-year cumulative excess return on payers over non-payers for years starting with 1964 through 1998 (and ending with 1966 through 2000) is 3.09% with a standard deviation of 37.99%.

We use r_t to denote the return in year t and R_{t+k} to denote the k-year cumulative return that starts with the year t return.[4]

III. Simulation Results

A. Predictive Regressions Based on Equity Issuance

Table I shows simulation results for predictive regressions using aggregate equity issuance variables for 1-year-ahead stock market returns. Panel A reports simulations for the equity share and Panel B considers detrended equity issuance. The left side of the table contains the simulation inputs, which are always based on the null of no predictability ($b = 0$). The right side reports the simulated distribution of the predictive regression coefficient and locates the actual OLS coefficient in this distribution. Figure 1 presents some of the interesting cases graphically. The basic approach is to start with empirically relevant parameter values, in order to determine the size of the bias in practice, and then to examine progressively more extreme counterfactual parameter values to get a sense of what would be required for pseudo market timing to explain observed predictive coefficients.

Accordingly, in the first row of Panel A, we start with a benchmark parameter set that includes a bias-adjusted estimate of the equity share's autocorrelation, the empirical correlation of u and v (under $b = 0$ and the bias-adjusted d), and a sample size of 75. The simulations indicate that the aggregate pseudo market-timing bias is small, if not negligible, for this empirically relevant parameter set. The actual OLS coefficient is -6.44 (and the unreported OLS heteroskedasticity robust $t = -3.33$). In contrast, the average simulated coefficient under the null of no predictability is only -0.11. Thus, as a point estimate, the bias accounts for 1.73% of the equity share's actual coefficient on value-weighted returns. Using the analytic expression for the bias in equation (3) leads to

[4] We measure the excess returns on long-term bonds ($R_{GLt+3} - R_{GSt+3}$) and dividend payers ($R_{Dt+3} - R_{NDt+3}$) in logs and sum across overlapping 3-year periods starting with year t:

$$R_{At+3} - R_{Bt+3} = \sum_{s=1}^{3} \log\left(\frac{1 + r_{At+s}}{1 + r_{Bt+s}}\right).$$

Table I

Pseudo Market Timing and Predictive Regressions Based on Equity Issuance

We simulate 50,000 estimates from the following system of equations:

$$r_{mt} = a + u_t, \quad E_t = c + dE_{t-1} + v_t, \text{ with } b = \frac{\text{cov}(r_{mt}, E_{t-1})}{\text{var}(E_{t-1})},$$

where r is the aggregate market return, either equal- or value-weighted, and E is one of two measures of equity issues. The equity share in new issues is the ratio of equity issues to total equity and debt issues. Detrended equity issues are the log difference between the level of equity issues and the average level of equity issues in the previous 5 years. Both are standardized to have zero mean and unit variance. We simulate $100 + T$ values for r and E starting with the actual E_0 and drawing with replacement from the empirical joint distribution of u and v. We throw away the first 100 values, leaving us with a sample size of T. We use OLS estimates of b for 50,000 separate samples, reporting the average and locating the actual OLS estimate in this simulated distribution with one- and two-tailed p-values. As implied in the return equation above, we impose the null hypothesis of no predictability ($b = 0$) in all cases. The parameters are as follows. The first row in each panel uses the in-sample bias-adjusted OLS estimate for d and the OLS estimate for ρ_{uv}. The second row uses the in-sample OLS estimate for d and sets ρ_{uv} equal to one, leaving the other distributional properties of u and v unchanged. The third row increases the bias-adjusted OLS estimate for d by three OLS standard deviations. The fourth row reduces the sample size to 10. The fifth through eighth rows repeat this procedure for equal-weighted returns.

	Parameter Inputs			Simulation versus Actual Results				
	d	ρ_{uv}	T	Average b	Actual b	Average/ Actual (%)	One-Tail p-value	Two-Tail p-value
Panel A: Equity Share in New Issues = EI/(EI + DI)								
VW	0.507	0.142	75	−0.11	−6.44	1.73	[0.006]	[0.010]
VW	0.507	1.000	75	−0.79	−6.44	12.19	[0.014]	[0.014]
VW	0.819	1.000	75	−1.10	−6.44	17.13	[0.009]	[0.009]
VW	0.819	1.000	10	−7.76	−6.44	120.53	[0.528]	[0.536]
EW	0.507	0.136	75	−0.15	−11.92	1.25	[0.001]	[0.002]
EW	0.507	1.000	75	−1.14	−11.92	9.59	[0.003]	[0.003]
EW	0.819	1.000	75	−1.66	−11.92	13.89	[0.002]	[0.002]
EW	0.819	1.000	10	−11.67	−11.92	97.90	[0.449]	[0.455]
Panel B: Detrended Equity Issues = log (5 · EI/(EI_{-1} + EI_{-2} + EI_{-3} + EI_{-4} + EI_{-5}))								
VW	0.581	0.209	70	−0.30	−7.83	3.77	[0.015]	[0.023]
VW	0.581	1.000	70	−1.27	−7.83	16.19	[0.033]	[0.033]
VW	0.801	1.000	70	−1.61	−7.83	20.58	[0.025]	[0.025]
VW	0.801	1.000	10	−10.70	−7.83	136.73	[0.581]	[0.595]
EW	0.581	0.201	70	−0.46	−16.50	2.80	[0.003]	[0.003]
EW	0.581	1.000	70	−1.90	−16.50	11.54	[0.005]	[0.005]
EW	0.801	1.000	70	−2.49	−16.50	15.07	[0.004]	[0.004]
EW	0.801	1.000	10	−16.86	−16.50	102.21	[0.466]	[0.471]

similar results, with $b^{bias\text{-}adjusted} = -6.34$.[5] The one-tail p-value shows that there is only a 0.6% probability that the bias would lead to a coefficient as negative as the actual coefficient.[6] These results are presented graphically in Panel A of Figure 1. The actual coefficient, marked with a diamond, falls in the left tail of the simulated distribution.

In the second set of parameter values, we consider the counterfactual case of *pure* pseudo market timing, setting the correlation of u and v to one. To do this, we create a new v that is equal to u but is standardized so that it has the same standard deviation as the empirical v series (i.e., $v_t = u_t \cdot \frac{s_v}{s_u}$, where s denotes sample standard deviation). The second row of Table I shows that even pure pseudo market timing generates only a small bias given empirical values of d and T. In particular, the mean simulated coefficient is only 12.19% as large as the actual coefficient, and there is only a 1.4% probability that extreme pseudo market timing of this type would lead to a coefficient as low as the actual coefficient. This case is plotted in Panel B of Figure 1.

In the third parameter set, we again assume pure market timing and we try to further increase the bias by increasing d to three standard errors above its bias-adjusted estimate to 0.819. Even when one tilts these two parameters as far as possible toward pseudo market timing, the average simulated coefficient is still only 17.13% of the actual coefficient in a sample of 75 observations. The simulated distribution is in Panel C of Figure 1.

In the fourth and most extreme parameter set, we simultaneously consider pure market timing, a counterfactually high predictor autocorrelation, and a counterfactually low 10 observations. Only in this extreme case is the aggregate pseudo market-timing bias on the order of the actual coefficient. The simulated distribution is plotted in Panel D of Figure 1. The actual coefficient falls near the middle. This case highlights the fact that the bias is fundamentally a (very) small-sample problem.

The remaining rows in Panel A of Table I repeat these exercises for the case of equal-weighted market returns. The equity share has an actual coefficient of -11.92 (unreported OLS $t = -3.49$ and $b^{bias\text{-}adjusted} = -11.78$) for equal-weighted returns. We modify the parameters as before, starting with the empirically relevant case and proceeding to more extreme values. The biases here

[5] Amihud and Hurvich (2004) propose an estimator for the standard error of the bias-adjusted estimate. Because the distribution of b is not always symmetric, we present one- and two-tailed p-values from the simulations instead. None of the inferences in Tables I and II is materially affected. For example, the Amihud and Hurvich one-tailed p-value is 0.0036 for the case of value-weighted returns on the equity share in new issues and 0.0004 for the corresponding case with equal-weighted returns.

[6] Note that the simulations presume knowledge of the parameters d and σ_{uv}. We input the bias-adjusted estimate of d and use the empirical distribution of the OLS residuals u and v. Thus, the p-values in Table I and II are not exact. Polk et al. (2004) use a neural network to implement the theoretical result in Jansson and Moreira (2003), developing a conditional t-statistic and a set of critical values that do not require knowledge of d and σ_{uv}. Like the Amihud and Hurvich standard errors, this procedure produces almost identical inferences. For example, one-tailed p-values are 0.0044 for the case of value-weighted returns on the equity share in new issues and 0.0004 for the corresponding case with equal-weighted returns.

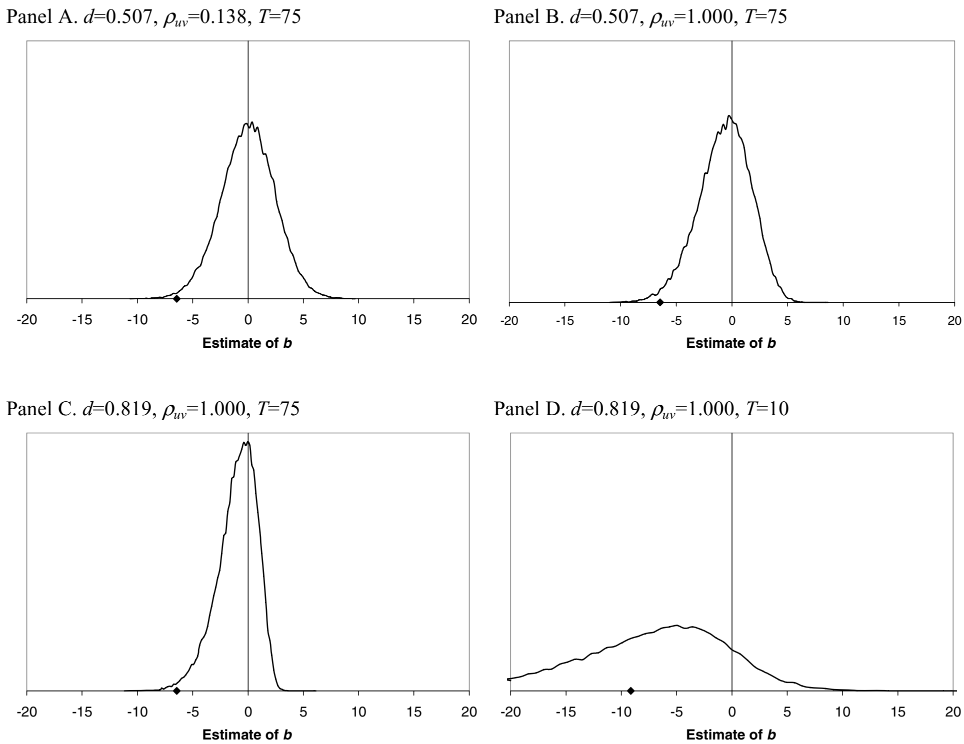

Figure 1. Pseudo market timing and predictive regressions based on equity issuance. We simulate 50,000 estimates from the following system of equations:

$$r_{mt} = a + u_t, \quad E_t = c + dE_{t-1} + v_t, \text{ with } b = \frac{\operatorname{cov}(r_{mt}, E_{t-1})}{\operatorname{var}(E_{t-1})},$$

where r is the value-weighted stock market return and E is the ratio of equity issues to total equity and debt issues. See Table I for details. The graph is a histogram of realizations of a regression of returns r_{mt} on E_{t-1}. The diamond shows the location of the actual OLS estimate on the X-axis. The parameters vary as follows. Panel A uses the in-sample bias-adjusted OLS estimate for d and the OLS estimate for ρ_{uv}. Panel B uses the in-sample OLS estimate for d and sets ρ_{uv} equal to one, leaving the standard deviation of u and v unchanged. Panel C increases the bias-adjusted OLS estimate for d by three OLS standard deviations. Panel D reduces the sample size to 10.

are also very small; under empirically relevant parameters, the point estimate suggests that the bias accounts for 1.25% of the actual coefficient. Panel B considers another measure of aggregate equity issuance, the log deviation from a 5-year moving average, which has an actual predictive coefficient of −7.83 (unreported OLS $t = -4.05$ and $b^{bias\text{-}adjusted} = -7.61$) for value-weighted returns and −16.50 (unreported OLS $t = -3.79$ and $b^{bias\text{-}adjusted} = -16.19$) for equal-weighted returns. The pattern of results is similar for this variable, indicating that it is also unaffected by the bias.

In summary, the aggregate pseudo market-timing bias has only a very small effect on predictive regressions based on aggregate equity issuance variables. The results overwhelmingly reject the conclusion of Butler et al. (2005) that the equity share's actual predictive coefficient is due to pseudo market timing. In fact, only an extremely counterfactual parameter set can generate a bias that approaches the observed coefficient.

B. Predictive Regressions Based on Other Managerial Decisions

Table II considers the effect of the aggregate pseudo market-timing bias on a range of other predictors derived from managerial decisions. As suggested in the introduction, many such predictors are likely to have, or are already known to have, the correlation structure that induces a bias. It is an empirical question whether the bias is sufficiently large to be of concern. We report simulations using value-weighted returns in the first two panels of Table II; results for equal-weighted returns are similar.

Panel A considers the real planned investment growth variable examined in Lamont (2000). The actual OLS coefficient for standardized real planned investment growth is -7.15 (unreported OLS $t = -3.74$ and $b^{bias\text{-}adjusted} = -7.11$). We vary the parameter set as usual, starting with the empirically relevant case. The correlation between innovations in investment growth is positive but not particularly high, and the persistence of this predictor is low. The upshot is no bias.

Panel B looks at aggregate insider buying, as studied by Seyhun (1992, 1998) and Lakonishok and Lee (2001).[7] The actual predictive coefficient on standardized insider buying is 4.99 (unreported OLS $t = 2.14$ and $b^{bias\text{-}adjusted} = 4.04$). Insiders are contrarian, as reflected in the highly negative correlation between insider buying and contemporaneous returns. Furthermore, there are only 19 observations on this variable, further increasing the potential for small-sample bias. Indeed, although the predictor's autocorrelation is not high, this is the variable for which the bias has the most bite. Nonetheless, the first row of Panel B reports the bottom line, which is that under empirically relevant parameters, the simulated coefficient equals or exceeds the actual coefficient only 11.9% of the time. As before, it takes extreme parameters to generate a bias as large as the actual coefficient.[8]

[7] Seyhun (1998) does not report formal predictive regressions. Instead, he reports a suggestive difference in average 1-year-ahead returns conditional on aggregate "buy" signals (defined as 55% or more firms being net buyers over the prior 12 months) and aggregate "sell" signals (defined as 45% or fewer firms being net buyers). Lakonishok and Lee (2001) do present formal tests (e.g., p. 96).

[8] Increasing the autocorrelation of insider buying by three standard errors above the bias-adjusted d leads to a value near or above 1.00. The same happens with the long-term share. Such an autocorrelation can be ruled out on theoretical grounds, so we cap the counterfactually high d value at 0.99. This is still well over two standard deviations above the bias-adjusted OLS estimate of d.

Table II

Pseudo Market Timing and Predictive Regressions Based on Other Corporate Decisions

We simulate 50,000 estimates from the following system of equations:

$$r_t = a + u_t, \quad X_t = c + dX_{t-1} + v_t, \text{ with } b = \frac{\text{cov}(r_{mt}, X_{t-1})}{\text{var}(X_{t-1})},$$

where r is an aggregate market return and X is a predictor variable. We use r_t to denote the return in year t and R_{t+k} to denote the k-year cumulative return that starts with the year t return. Panel A considers value-weighted stock returns from March of t to February of $t + 1$ and, as a predictor variable, investment plans from Lamont (2000); Panel B considers value-weighted stock returns and the insider trading variable of Seyhun (1998); Panel C considers the 3-year return on long bonds over treasuries and the long-term share of debt issues from Baker et al. (2003); Panel D considers 3-year return of payers minus non-payers and the rate of dividend initiation from Baker and Wurgler (2004). The independent variables are standardized to have zero mean and unit variance. We simulate 100 + T values for r and X starting with the actual X_0 and drawing with replacement from the empirical joint distribution of u and v. We throw away the first 100 values, leaving us with a sample size of T. We use OLS estimates of b for 50,000 separate samples, reporting the average and locating the actual OLS estimate in this simulated distribution with one- and two-tailed p-values. As implied in the return equation above, we impose the null hypothesis of no predictability ($b = 0$) in all cases. The parameters in each panel are as follows. The first row uses the in-sample bias-adjusted OLS estimate for d and the OLS estimate for ρ_{uv}. The second row uses the in-sample OLS estimate for d and sets ρ_{uv} equal to one, leaving the other distributional properties of u and v unchanged. The third row increases the bias-adjusted OLS estimate for d by three OLS standard deviations or to 0.95 (2.7 standard deviations for Panel B and 2.5 standard deviations in Panel C), whichever is lower. The fourth row reduces the sample size to 10.

	Parameter Inputs			Simulation versus Actual Results				
	d	ρ_{uv}	T	Average b	Actual b	Average/ Actual %	One-Tail p-value	Two-Tail p-value
Panel A: Investment Plans, t = 1948 through 1992, from Lamont (2000)								
r_{mt}	0.088	0.118	45	−0.05	−7.15	0.76	[0.003]	[0.005]
r_{mt}	0.088	1.000	45	−0.40	−7.15	5.59	[0.000]	[0.000]
r_{mt}	0.545	1.000	45	−0.88	−7.15	12.26	[0.003]	[0.003]
r_{mt}	0.545	1.000	10	−3.69	−7.15	51.61	[0.218]	[0.224]
Panel B: Insider Trading, t = 1976 through 1994, from Seyhun (1998)								
r_{mt}	0.358	−0.746	19	1.18	4.99	23.63	[0.119]	[0.140]
r_{mt}	0.358	−1.000	19	1.52	4.99	30.44	[0.134]	[0.140]
r_{mt}	0.990	−1.000	19	3.34	4.99	66.94	[0.231]	[0.231]
r_{mt}	0.990	−1.000	10	5.81	4.99	116.39	[0.517]	[0.517]
Panel C: Long-Term Share in Debt Issues, t = 1954 through 1998, from Baker et al. (2003)								
$R_{GLt+3} - R_{GSt+3}$	0.697	0.021	45	−0.10	−9.94	1.05	[0.007]	[0.014]
$R_{GLt+3} - R_{GSt+3}$	0.697	1.000	45	−2.33	−9.94	23.46	[0.017]	[0.017]
$R_{GLt+3} - R_{GSt+3}$	0.990	1.000	45	−3.79	−9.94	38.11	[0.035]	[0.035]
$R_{GLt+3} - R_{GSt+3}$	0.990	1.000	10	−11.36	−9.94	114.30	[0.593]	[0.598]
Panel D: Dividend Initiations, t = 1964 through 1998, from Baker and Wurgler (2004)								
$R_{Dt+3} - R_{NDt+3}$	0.957	0.226	35	−3.12	−24.82	12.57	[0.036]	[0.044]
$R_{Dt+3} - R_{NDt+3}$	0.957	1.000	35	−14.11	−24.82	56.84	[0.171]	[0.171]
$R_{Dt+3} - R_{NDt+3}$	0.990	1.000	35	−14.75	−24.82	59.44	[0.179]	[0.179]
$R_{Dt+3} - R_{NDt+3}$	0.990	1.000	10	−37.11	−24.82	149.51	[0.711]	[0.727]

Panel C considers the long-term share in debt issues as a predictor of 3-year excess returns on government bonds. Interestingly, there is only a very slight positive contemporaneous correlation between innovations in the long-term share and excess bond returns, immediately suggesting that a large bias is unlikely. In the benchmark parameter set, the average simulated coefficient is only 1.05% of the actual coefficient (unreported $b^{bias\text{-}adjusted} = -9.15$).[9,10] The simulated coefficient is as negative as the OLS coefficient only 0.7% of the time. Only extreme counterfactual parameters generate an average simulated coefficient in the ballpark of the actual coefficient. Alas, these results overwhelmingly reject the conclusion of Butler et al. (2004), who argue that the long-term share's coefficient is due to aggregate pseudo market timing.

Panel D considers the aggregate rate of dividend initiation as a predictor of the 3-year cumulative excess return on dividend payers over non-payers, the regression emphasized in Baker and Wurgler (2004). The residual correlation indicates that when the return on payers is relatively high, non-payers initiate dividends at a high rate, suggesting at least a small bias. However, the simulations show there is only a 3.6% chance of observing the actual coefficient under the empirical parameters. Indeed, in that case, the average simulated coefficient is only 12.57% of the actual coefficient (unreported $b^{bias\text{-}adjusted} = -22.78$).

In summary, the aggregate pseudo market-timing bias is only a minor concern for the predictive regressions based on managerial decision variables that appear in the literature. Of course, although we find here no case in which inferences based on OLS estimates would be seriously misleading, good practice dictates that the potential for small-sample bias be tested on a case-by-case basis going forward.

C. Regime Shifts

The papers by Butler et al. (2004, 2005) consider aggregate pseudo market-timing arguments in connection with "large shocks" or "regime switches." Although our previous simulations draw with replacement from the empirical residuals u and v, and therefore have the same number of large shocks as the raw data, one might still ask whether the responsiveness of corporate decisions to contemporaneous returns is greater around shocks than in non-shock periods. An easy reply is that Tables I and II show the effect of setting the pseudo market-timing correlation to 1.00 *everywhere*, thereby putting an upper bound on the impact of this sort of miscalibration.

[9] For the long-term share and dividend initiations, we simulate annual returns and compare the distribution of simulated coefficients b from regressions of the 3-year overlapping returns on lagged levels of the predictor to the OLS b computed from the historical data. In contrast, the bias-adjusted estimates of b reported in the text use the correlation and autocorrelation structure of the 3-year returns and innovations in issuance. Direct simulations of 3-year returns produce similar results.

[10] We do not report the overstated OLS t-statistics from regressions with overlapping returns.

Another possibility is that the distribution of returns changes from one regime to another. Butler et al. (2004), for instance, find a structural break in excess bond returns in the early 1980s and show that the long-term share in debt issues does not have statistically significant incremental predictive power for returns when a returns-regime dummy is included. Unfortunately, this test has no a priori justification, effectively rolling genuine market timing *into the null hypothesis*.

According to Schultz (2003), pseudo market timing is simply the situation whereby managers base their decisions on current or past returns. This sort of timing typically does not change the cost of capital nor does it legitimately predict future returns. However, if one also allows managers to base their decisions on the current returns regime—not just as it is revealed in past returns, but also ex ante, as one that will persist probabilistically into the future—one is including a much broader notion of timing into the null hypothesis one that does genuinely lower the cost of capital and does legitimately predict future returns. Thus, in estimating the effect of regime shifts on small-sample bias, we need to be careful not to allow the predictor variable to shift with regimes in a fashion that is correlated with future returns, or else the test will be meaningless.

Intuitively, it is not obvious why regime shifts in returns would exacerbate bias. One might be concerned that a persistent predictor (that has a trend in it as a result) might by chance line up with a regime change. For example, suppose long-term debt issues are trending down and there is an unrelated regime change in the time series of returns in which excess long-term bond returns rise. This would lead to a spurious predictive relationship. However, this would not be a case of pseudo market-timing bias, but rather an issue of econometric power and the measurement of standard errors. In terms of bias, it would seem that as long as the regime can switch either direction, sometimes it helps and sometimes it hurts.

To verify this intuition, we run a second set of simulations. We start by examining which of our return series actually display evidence of regime shifts. We fit each return series to a two-state Markov switching model based on Hamilton (1990):

$$r_t = a(s_t) + u_t, \quad u_t \sim \text{i.i.d.}\, N\big(0, \sigma_u^2(s_t)\big),$$

$$\text{where}\, \Pr[s_t = 1 \mid s_{t-1} = 1] = p \quad \text{and} \quad \Pr[s_t = 0 \mid s_{t-1} = 0] = q. \tag{4}$$

The model is estimated using maximum likelihood. Inspection of the state vector (not reported) suggests a single shift in equal- and value-weighted stock market returns in the mid-1940s. Like Butler et al. (2004), we also find a single shift in excess bond returns in the early 1980s. Regime shifts are unlikely to exacerbate the pseudo market-timing bias in the case of insider trading and investment plans, however, since the samples for those variables do not include the mid-1940s. The relative returns of dividend payers and non-payers also display no clear regime shift in the relevant period. Hence, we exclude these variables from the subsequent simulations.

Next, we run a set of simulations in a manner closely analogous to the earlier procedure, only now we use the system given by equations (4) and (2) instead of equations (1) and (2).[11] That is, with the parameters of Hamilton's model in hand, we simulate, under the null hypothesis of no predictability, alternative patterns of returns and predictors. The goal is to see how likely it is that, with regime changes in returns, we would observe the predictive relationship that we see in the data just by chance.

Butler et al. (2004) take a different approach to statistical inference in the presence of regime changes, in including a post-1981 indicator variable directly in the predictive regression. It is tempting to think of this as an innocuous sample split, but it is not. Unlike the data on debt issuance, the regime change was not known with any certainty until well after 1982. So, the approach is equivalent to controlling for *future returns*, as best as they can be explained with a single indicator variable. Of course this lowers predictive power; it would be surprising if it did not. Like the more transparent exercise of removing crashes that were preceded by a high equity share in Butler et al. (2005), their method of analyzing long-term bond returns has nothing to do with pseudo market timing and amounts instead to another strategic process of removing variation in the data that are identified ex post as consistent with genuine market timing.

The results are presented in Table III. The parameters of the Markov model are on the left side of the table, simulation parameter inputs are in the middle, and the distribution of simulated b estimates, in comparison with the actual estimate, are on the right. In the first set of simulations, the pseudo market-timing correlation is set to zero in order to see the effect of the regime shift per se on bias. The second row sets the correlation to the empirically relevant value. The third row sets the correlation to one.

The results show that adding regime changes has little effect on our earlier conclusions. This can be seen by comparing the simulation results in Table III to those (using the same parameter inputs) in Tables I and II.[12] Looking closely, in the case of little or no pseudo market-timing correlation, the only apparent effect of introducing regime changes is to increase standard errors slightly, as predicted above. Even allowing for the possibility of a spurious common trend in the data, it is still very unlikely that the relationship we observe in the data occurred by chance. More interestingly, consider the third row in Panel B, which looks at the case of perfect pseudo market timing and the long-term share. Here, the bias is actually somewhat *lower* than in the analogous simulation without regime changes.

To understand this result, note that there is a big difference in the mean returns across the two regimes: the data start in the low-return regime, and

[11] A minor difference is that our earlier procedure uses empirical residuals u and v. In the current procedure, we use normal residuals, to remain consistent with the assumption of normality in the estimation of the Hamilton model.

[12] For brevity, we omit regime-switching simulation results for equal-weighted returns and for detrended equity issuance. These results are very close to those presented in Table I.

Table III

Pseudo Market Timing and Predictive Regressions: Incorporating Regime Changes

We simulate 50,000 estimates from the following system of equations:

$$r_t = a(s_t) + u_t, \quad u_t \sim \text{i.i.d.}\, N\left(0, \sigma_u^2(s_t)\right), \text{where} \Pr[s_t = 1 \,|\, s_{t-1} = 1] = p \quad \text{and} \quad \Pr[s_t = 0 \,|\, s_{t-1} = 0] = q$$

$$\text{and}\, X_t = c + d X_{t-1} + v_t, \text{with}\, b = \frac{\text{cov}(r_{mt}, X_{t-1})}{\text{var}(X_{t-1})},$$

where r is an aggregate market return, X is a predictor variable, and s is a state variable that takes two discrete values. We use r_t to denote the return in year t and R_{t+k} to denote the k-year cumulative return that starts with the year t return. Panel A considers value-weighted stock market returns and the equity share in new issues from Baker and Wurgler (2000); Panel B considers the three-year return on long bonds over treasuries and the long-term share of debt issues from Baker et al. (2003). The independent variables are standardized to have zero mean and unit variance. We simulate T values for r using normal errors u_t and state $s_1 = 1$. We simulate T matched values of X starting with X_0 equal to $c/(1-d)$, the steady state value of X, and draw normal errors v_t conditional on the unconditionally demeaned r_t. We use OLS estimates of b for 50,000 separate samples, reporting the average and locating the actual OLS estimate in this simulated distribution with one- and two-tailed p-values. The parameters in each panel are as follows. The first row uses parameters from a maximum likelihood estimation of the Markov-switching model, the bias-adjusted OLS estimate for d, and the correlation between r and v set to zero. The second row sets the correlation between r and v to the empirical value. The third row sets the pseudo market-timing correlation to one.

	Markov Model						Parameter Inputs			Simulation versus Actual Results				
	$a(0)$	$\sigma_u(0)$	p	$a(1)$	$\sigma_u(1)$	q	d	ρ_{rv}	T	Average b	Actual b	Average/ Actual %	One-Tail p-value	Two-Tail p-value
Panel A: Equity Share in New Issues = EI/(EI + DI)														
r_{mt}	8.5	17.8	0.995	6.3	30.1	0.909	0.507	0.000	75	−0.01	−6.45	0.19	[0.005]	[0.010]
r_{mt}	8.5	17.8	0.995	6.3	30.1	0.909	0.507	0.142	75	−0.10	−6.45	1.55	[0.006]	[0.011]
r_{mt}	8.5	17.8	0.995	6.3	30.1	0.909	0.507	1.000	75	−0.76	−6.45	11.72	[0.019]	[0.023]
Panel B: Long-Term Share in Debt Issues, t = 1954 through 1998, from Baker et al. (2003)														
$R_{GLt+3} - R_{GSt+3}$	3.6	11.7	0.995	−1.8	6.7	0.957	0.697	0.000	45	0.02	−9.88	−0.24	[0.012]	[0.025]
$R_{GLt+3} - R_{GSt+3}$	3.6	11.7	0.995	−1.8	6.7	0.957	0.697	0.021	45	0.00	−9.88	0.00	[0.012]	[0.024]
$R_{GLt+3} - R_{GSt+3}$	3.6	11.7	0.995	−1.8	6.7	0.957	0.697	1.000	45	−0.69	−9.88	6.99	[0.013]	[0.019]

the high-return regime is an absorbing state (reached in the early 1980s). In this environment, there are two offsetting effects. The first is that of small-sample bias. By moving quickly to the high-return regime, a negative coefficient arises for the same reason it does with no regime changes. In other words, by issuing more long-term debt after high returns, the data end up suggesting, albeit only slightly in a sample this long, that a higher fraction of long-term issuance precedes lower future returns.

The second and offsetting effect is that regimes introduce persistence in returns. A high past return increases the likelihood that we are in a high-return regime. In this case, higher issuance in response to higher current returns is actually associated with *higher* future returns. This tends to create predictability in the opposite direction. For the long-term share and excess bond returns, this second effect is fairly large because both the difference in mean returns is large and regimes are persistent. Thus, the overall effect of regime switching is actually of the wrong sign. Put simply, regime changes can actually *reduce* the bias due to pseudo market timing, since issuing more based on high current or past returns is not a smart thing for a manager to do unless he is looking to *increase* his cost of capital.

In summary, pseudo market timing has, if anything, a smaller effect in the presence of regime changes in returns. To the extent that there is something special about combining these two issues, it is to introduce an offsetting effect that actually reduces the bias.

IV. Conclusion

The aggregate version of Schultz's (2003) pseudo market-timing bias is a potentially serious concern for many studies that use aggregate managerial decision variables to predict market-level returns. Butler et al. (2005) highlight the potential for this bias in the specific context of predictive regressions based on the equity share in new issues, but there are actually a number of related results that a priori may also be affected, such as predictive regressions based on planned investment growth, the maturity of new debt issues, aggregate insider buying, and the aggregate dividend initiation rate.

In this paper, we point out that "aggregate pseudo market-timing bias" is simply a new name for the small-sample bias long known to affect predictive regressions based on scaled-price predictors such as the dividend yield. Furthermore, the bias can be estimated using standard simulation methods. We run such simulations, and find that, in practice, the bias is minor for all settings we consider. While our results do not shed new light on *why* aggregate managerial decision variables predict asset returns—for example, Baker and Stein (2004) point out that successful market timing can result from passive responses to investor demand or market liquidity, not just from strategic decisions—they rigorously show that the predictability is much too strong to be attributed to small-sample bias. More research on the interpretation of predictive regressions based on aggregate managerial decision variables therefore seems warranted.

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