A. *Web Appendix II: Regression Discontinuity Analysis*

In this appendix I conduct a regression discontinuity analysis using the S&P/Barra data. It is an exact replication of a section previously included in the paper.

Although index membership is not randomly determined and fundamental risk characteristics are not likely to be independent of index assignment, a comparison of comovement among observations just above and just below the cutoff is a quasi-randomized experiment since fundamentals should, on average, be similar for such firms. This empirical approach is referred to in the statistics and econometrics literature as a "regression discontinuity analysis". Hahn et al. (1999) and Jacob and Lefgren (2002) are examples from recent literature that use the technique.

Two conditions must be satisfied for identification. First, experimental units must be separated into two groups, one that receives some treatment and another that does not, according to a single sorting variable, z_i, where group membership is completely defined by a cut-off point, z_0. Second, for some outcome variable y_i, the conditional expectation of y_i given z_i must be continuous at z_0 under the null of no treatment effect. If these two conditions are met, the treatment effect is identified by estimating the expected difference in the outcome variable for observations just above and just below the cut-off. In this paper, the sorting variable is the BM ratio, the cut-off is the BM ratio that defines which index a stock is included, and the treatment effect is index inclusion.

Although index membership is known with certainty, a drawback of the S&P/Barra index data is that the actual book-values used to rebalance the indices and the cut-off point are not observed. Consequently, I measure book values and create my own cut-off following the methodology used by Barra. Barra's web site does not indicate how book-values are computed, but states that market values "used at the time of rebalancing are the equity's position at the close of trading one month prior (i.e., November 30 and May 31)."[1] I measure market value in the same manner and measure book value as common equity reported in Compustat at the end of the latest fiscal quarter at least five months prior to the end of May or November. For each

[1] The information was obtained from www.barra.com. MSCI acquired Barra in 2004 and information regarding the S&P/Barra indices is no longer available at this site.

rebalancing month, I define the cut-off point to separate stocks in the S&P 500 into two groups with equal market capitalization.

On average across rebalancing periods, the calculated BM ratio and cut-off correctly predict index assignment about 94 percent of the time in both the test and control samples. A concern however, is that in estimating the treatment effect,an unobserved variable helps determine index assignment, namely, the difference between observed BM and the actual BM used by S&P/Barra, that could also be correlated with the outcome variable. In this case, OLS estimates of the treatment effect on the outcome variable would be biased.[2]

The solution to this endogeneity problem proposed by Hahn et al. (1999) and Jacob and Lefgren (2002) is to estimate the effect of treatment using an instrumental variables approach. The first step is to define a treatment dummy, T_i, where $T_i = 1$ if stock i is in the value index and zero otherwise, and to regress the treatment dummy on observable characteristics including a dummy, D_i, which equals 1 if the observed BM ratio is above the estimated cut-off and zero otherwise. The second step is to use only the variation in treatment correlated with observable characteristics in estimating the treatment effect on comovement. Point estimates of this effect should be unaffected by the correlation between treatment and unobservable characteristics.

Formally, the first stage estimates the regression

$$T_i = \lambda_0 + \delta_1 ln(BM_i) + \delta_2 D_i + \delta_3 ln(ME_i) + e_i, \tag{1}$$

where T_i is the treatment dummy, BM_i is the book-to-market ratio of stock i, D_i is the cut-off dummy, and ME_i is the market value of stock i. All explanatory variables are observed just before the rebalancing month during which T_i is determined. The second stage is then to estimate

$$y_i = \lambda_0 + \lambda_1 ln(BM_i)_i + \lambda_2 E[T_i] + \lambda_3 ln(ME_i) + v_i, \tag{2}$$

where $E[T_i]$ is taken from the first stage, and y_i is one of the estimated slope coefficients from (**??**) over the post-rebalance window, either β^*_{iG} or β^*_{iV}. The approach amounts to modeling the baseline relationship between BM ratios and comovement.

[2]Simple OLS estimates are very similar to those reported in the paper however.

Given the relation is modeled correctly, λ_2 measures the effect of treatment (inclusion in the value index) on the outcome variable for observations arbitrarily close to the BM cutoff, conditional on log size.

The IV estimates and IV parameter covariance matrices are obtained for each rebalancing period. The cross-sectional regression results are aggregated using an approach similar to Fama and McBeth (1973). Mean parameter estimates across rebalancing periods are calculated. I then assume parameter estimates are independent across time and estimate the variance of the mean as the sum of the variances divided by N^2, where N is the total number of rebalancing periods, and variances are obtained from the diagonals of the IV parameter covariance matrices.

The estimated coefficients of (1) and (2) are used to conduct the following test:

Test 2 *Under the null fundamental hypothesis, inclusion in the value index should not decrease β^*_{iG} and should not increase β^*_{iV}.*

(T2-A) $H_0 : \overline{\lambda}_2 \geq 0$ *using β^*_{iG} as the outcome variable*

(T2-B) $H_0 : \overline{\lambda}_2 \leq 0$ *using β^*_{iV} as the outcome variable*

Proper estimates of λ_2 rely on a linear relationship between log BM ratios and the measures of comovement, β^*_{iG} and β^*_{iV}. Any non-linearities between these variables can be played out through λ_2. Most regression discontinuity designs validate the baseline model by examining the relationship between the sorting variable and the outcome variable over a period in which no treatment occurred (e.g., Jacob and Lefgren, (2002)). With the S&P/Barra data, I not only observe these variables over the control sample during which no treatment occurred, but I also know exactly which variables *would* have received treatment had treatment been given. If the coefficient on the treatment dummy representing index inclusion is not significant over the control sample, results over the test sample are not likely to be caused by non-linearities.

Test 2 is conducted using daily data and weekly data. The estimation window to obtain β^*_{iG} and β^*_{iV} using daily data is the five month period following the rebalancing month in which BM and ME are observed. For weekly data, it is the eleven month period following the rebalancing month. To be included in the cross sectional regression for any given rebalancing month, a stock must be in the same S&P/Barra index over the entire estimation window.

Regression discontinuity designs commonly use data within some narrow range around the cut-off point. A tradeoff exists since using a narrower range helps mitigate effects of non-linearities, while a wider range provides more degrees of freedom to estimate the baseline relationship. For each cross-sectional regression, I only exclude stocks whose log BM ratio is more than two standard deviations away from the mean log BM ratio across stocks for that rebalancing month. This eliminates the effects of large outliers possibly caused by mismeasured book values, but also provides a sample sufficient to reliably estimate the baseline relationship between BM ratios and comovement.

Results for Test 2 are reported in Table IV. Panel A reports results using daily data and Panel B reports results using weekly data. Panel A offers substantial evidence in favor of rejecting the null hypotheses of Test 2. Over the test sample the average value of λ_2 when β^*_{iG} is the dependent variable is -0.358. When β^*_{iV} is the dependent variable the average value of λ_2 is 0.326. Both results are significant at the 5 percent level. Slightly stronger results are found over the period from 1998 though 2002 in Panel A. However, none of these results are significant over the control sample. Sentiment for value and growth appears to be generating covariation among stocks within the test sample.

Panel B of Table IV also offers evidence in favor of rejecting the null hypotheses of Test 2 using weekly data. Over the test sample the average value of λ_2 when β^*_{iG} is the dependent variable is -0.395. When β^*_{iV} is the dependent variable the average value of λ_2 is 0.334. Again, both results are significant at the 5 percent level. Over the control sample, the average value of λ_2 is barely significant at the 10 percent level when using β^*_{iG} as the dependent variable. Using weekly data, non-linearities may be driving some of the results. However, the same cannot be said for results using β^*_{iV} as the dependent variable. Over the control sample in Panel B, the estimate of λ_2 is not significant.

Table IV also provides estimates of mean coefficients of the first-stage regression and contains two interesting results. First, R^2 measures for the first stage regressions are all in the range of 0.75 to 0.83, indicating the observed variables used in the first stage regression effectively predict the treatment dummy, T_i. Second, the panel suggests a substantial amount of variation in expected treatment is driven by whether the observed BM ratio is above the estimated cutoff. For instance, over the test sample in Panel A, the estimated value of δ_2 is 0.702 and is significant at the one percent level.

In summary, the results of Table IV provide evidence in favor of rejecting the null hypotheses of Test 2. Over the test sample, stocks whose BM ratios are just above (below) the cutoff covary significantly more with the value (growth) index than stocks whose BM ratios are just below (above) the cutoff. These same results are not found among stocks over the control sample, suggesting the effects identified over the test sample are not driven by non-linearities and that sentiment for value and growth generates covariation among stock returns.

REFERENCES

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Table IV
Regression Discontinuity Analysis - Test 2

At the end of each rebalancing month, the following system of equations is estimated using all stocks in the S&P 500:

$$T_i = \delta_0 + \delta_1 ln(BM_i) + \delta_2 D_i + \delta_3 ln(ME_i) + e_i$$

$$y_i = \lambda_0 + \lambda_1 ln(BM_i) + \lambda_2 E[Treat_i] + \lambda_3 ln(ME_i) + v_i$$

where T_i equals 1 if stock i is in the value index and 0 if it is in the growth index, BM_i and ME_i are the book-to-market ratio and size of stock i, D_i is a dummy that equals 1 if BM_i is above the cut-off, and y_i is the covariation of the return for stock i with either the growth or value index measured over the post-rebalance window in a bivariate regression of stock i on both indices. All right hand variables are observed at the end of the rebalancing month. Average coefficient estimates across rebalance months are presented along with t-statistics. None of the estimated average values of λ_3 or δ_3 are significant, and are not reported in the interest of clarity. Also estimated, but not reported are the regression intercepts. Results for the control sample exclude the October 1987 crash. Significance of the one-tailed tests described in the paper at the 1%, 5%, and 10% levels is indicated respectively by ***, **, and *.

Dependent Variable is β^*_{iG} T2-A H$_0$: $\bar{\lambda}_2 \geq 0$		Dependent Variable is β^*_{iV} T2-B H$_0$: $\bar{\lambda}_2 \leq 0$		First Stage		
$\bar{\lambda}_1$	$\bar{\lambda}_2$	$\bar{\lambda}_1$	$\bar{\lambda}_2$	$\bar{\delta}_1$	$\bar{\delta}_2$	$\bar{R}^2$
Panel A. Daily Data						
1992-2004						
-0.283 ***	-0.358 **	0.253 ***	0.326 **	0.163 ***	0.702 ***	0.762
-(2.58)	-(2.10)	(2.40)	(1.98)	(5.92)	(18.80)	
1998-2002						
-0.200 **	-0.451 ***	0.178 **	0.360 **	0.122 ***	0.747 ***	0.791
-(2.27)	-(2.81)	(2.05)	(2.28)	(5.77)	(21.58)	
1981-1991 (Control)						
-0.422 ***	-0.212	0.425 ***	0.167	0.188 ***	0.683 ***	0.754
-(3.08)	-(1.16)	(2.91)	(0.85)	(5.41)	(17.23)	
Panel B. Weekly Data						
1992-2004						
-0.284 **	-0.395 **	0.233 **	0.334 **	0.141 ***	0.750 ***	0.812
-(2.17)	-(1.92)	(1.81)	(1.66)	(5.61)	(20.93)	
1998-2002						
-0.220 **	-0.440 **	0.217 **	0.243	0.126 ***	0.760 ***	0.828
-(2.16)	-(2.26)	(2.15)	(1.26)	(6.96)	(22.73)	
1981-1991 (Control)						
-0.429 ***	-0.327 *	0.454 ***	0.259	0.156 ***	0.746 ***	0.823
-(2.85)	-(1.64)	(2.69)	(1.15)	(4.94)	(19.91)	