Publication Bias in Reused Natural Experiments*

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ABSTRACT

A growing literature considers multiple-testing issues in financial economics where empirical settings are reused. However, standard multiple-testing procedures assume all statistical tests are observed, an assumption violated by selective reporting of statistically significant results. We study publication bias and multiple testing in extensively studied natural experiments by fitting a structural model to the observed distributions of t-statistics in order to infer the underlying distributions of all tests. Calibrating the analysis to empirical environments typical of corporate finance, we find that estimated shrinkage factors, local false discovery rates, and minimum t-statistic hurdles are larger than those documented for cross-sectional asset pricing, reinforcing prior calls to raise conventional significance thresholds in heavily reused natural experiments.

JEL: G1, G10

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Natural experiments play a central role in empirical economics because they allow researchers to identify causal relations in the absence of controlled laboratory experiments. Since natural and policy-driven events that approximate random assignment are scarce, these settings are often reused to study different outcome variables. Heath, Ringgenberg, Samadi, and Werner (2023) document that just two settings, the staggered introduction of business combination laws in the U.S. and the Securities Exchange Commission's (SEC) Regulation SHO pilot program, have been used to examine more than 100 and 400 distinct outcome variables, respectively. Similarly, at least 176 different outcomes have been studied using rainfall as an instrumental variable (Mellon (2021)), more than 80 studies exploit the Russell index reconstitution, and at least 60 examine the U.S. Tick Size Pilot. The multiple testing problem suggests that evaluating statistical significance using conventional thresholds in settings that have been reused many times can lead to incorrect inferences. For a discussion of multiple testing issues across the various areas of financial economics, see Harvey, Liu, and Saretto (2020).

At the same time, a growing literature documents that published results often exhibit publication bias, as statistically non-significant findings are less likely to appear in leading journals (Harvey (2017); Brodeur, Lé, Sangnier, and Zylberberg (2016); Andrews and Kasy (2019); Askarov, Doucouliagos, Doucouliagos, and Stanley (2024)). As a consequence, non-significant results are also less likely to be written up and reported by researchers in the first place (Rosenthal (1979); Nature (2025)). Because standard multiple-testing procedures assume that all statistical tests conducted are observed, the presence of publication bias may distort these procedures and still lead to incorrect inference. Several recent papers attempt to quantify publication bias effects in cross-sectional asset pricing by inferring the distribution of unreported findings from observed test statistics (Harvey, Liu, and Zhu (2016); Chen and

Zimmermann (2020); Harvey and Liu (2021); Chen (2024)).[1]

In this paper, we examine how publication bias interacts with multiple-testing concerns when natural experiments are reused. To quantify multiple-testing effects under publication bias, we fit a structural model of biased publication (Chen (2024)) to the distribution of reported t-statistics observed for two extensively studied empirical settings: business combination laws and the Regulation SHO pilot.[2] We explicitly account for publication bias by extrapolating the distribution of t-statistics using empirical data from the well-observed region of reported tests that are statistically significant under single hypothesis testing. With this approach, we consider multiple testing statistics that can directly target reported findings and are robust to publication bias. These statistics have been widely applied in cross-sectional asset pricing using various approaches. The measures we consider include empirical Bayes shrinkage (Chen and Zimmermann (2020); Chinco, Neuhierl, and Weber (2021); Harvey and Liu (2021); Jensen, Kelly, and Pedersen (2023); Chen (2024)) and the local false discovery rate (Chen and Zimmermann (2017); Chen (2024)). The structural estimation also provides estimates of statistical thresholds required to control false discoveries at levels previously considered in the literature when reporting is selective.

Under a range of assumptions about the underlying distribution of true effects and the publication process previously employed in the literature, our baseline results indicate false discovery rates (FDRs) of up to 51% and 53% for tests using business combination laws and the Regulation SHO pilot, respectively. For reference, comparable FDR estimates in cross-

[1] Other approaches address publication bias by algorithmically generating test statistics to provide upper bounds on multiple-testing concerns (Yan and Zheng (2017); Chordia, Goyal, and Saretto (2020); and an earlier draft of Heath et al. (2023)).

[2] We focus on these two settings because they span the two dominant identification designs underlying most heavily reused natural experiments (i.e., staggered policy adoption and randomized treatment assignment) and because, with more than 100 and 400 documented outcome variables respectively, they are some of the most extensively reused settings in the literature.

sectional asset pricing have been as low as $\approx 1\%$ (Chen and Zimmermann (2017)). Empirical Bayes estimates indicate that a sizeable proportion of the variation in reported t-statistics is due to noise and that reported t-statistics should be shrunk by as much as 53% in the business combination laws setting and 67% in the Regulation SHO pilot setting, depending on the assumed distribution and publication process. These baseline shrinkage estimates exceed the headline shrinkage estimates reported by Chen and Zimmermann (2020); Harvey and Liu (2021); and Jensen et al. (2023) for cross-sectional asset pricing ranging from $10\% - 19\%$. The FDR-based t-statistic thresholds we estimate are also comparable to, or larger than, those obtained by applying FDR corrections to all reported tests without accounting for publication bias (as in Heath et al. (2023)). Our baseline results provide evidence that publication bias is potentially considerable in these settings, and confirm initial evidence that conventional statistical thresholds should be raised to better control false discoveries in extensively reused natural experiments.

To account for concerns about estimation uncertainty under publication bias raised in the literature (Chen (2024)), we also bootstrap estimates using two distinct approaches designed to reflect biased reporting. As corporate finance theory suggests that firms optimize along multiple margins, accounting for correlations across tests is also important in the business combination laws and Regulation SHO pilot settings. The first bootstrap we use is a simple, non-parametric bootstrap that draws from the observed distributions of t-statistics with replacement. The second bootstrap we apply is a semi-parametric bootstrap developed by Chen (2024), which we adapt to our empirical settings while explicitly accounting for correlations across outcomes in commonly used corporate finance data. The bootstrapped estimates further reinforce our baseline results that publication bias is important in these settings. Bootstrapped local FDR and shrinkage estimates are generally larger than those

previously documented for cross-sectional asset pricing across a variety of assumed distributions and publication functions. Bootstrapped confidence intervals in our settings often exceed the "worst case" shrinkage and FDR estimates reported by Chen (2024): 28% and 22%, respectively. Upper confidence limits of our bootstrapped confidence intervals indicate a worst-case FDR as large as 67% for business combination laws and 79% for the Regulation SHO pilot. Corresponding confidence intervals for bootstrapped shrinkage estimates indicate that reported results should be shrunk by as much as 67% for business combination laws and 83% for the Regulation SHO pilot. Given that bootstrapped estimates for the share of false results in our structural estimation are large relative to prior work in cross-sectional asset pricing, our bootstrapped minimum t-hurdles also reinforce previous suggestions to raise t-statistic hurdles to better control false discoveries in these settings. Bootstrapped confidence intervals for minimum t-hurdles to control the FDR are larger than the conventional hurdle of 1.96.

Our paper builds on the results in Heath et al. (2023), which studies the same two settings (business combination laws and the Regulation SHO pilot) and applies family-wise error rate and FDR corrections to account for multiple testing. The application of these corrections (also applied by Harvey et al. (2016) and Chordia et al. (2020)) implicitly assumes that every test conducted by researchers is observed by the econometrician. As discussed above, this is unlikely to be true because statistically insignificant results are less likely to be written up or published in the first place (Rosenthal (1979); Brodeur et al. (2016)). We instead ask a different question: rather than correcting inference conditional on the sample of reported t-statistics, we explicitly model the publication process that generated that sample using the structural approach of Chen (2024). The result is a direct empirical test of the assumption that all tests are observed. Importantly, we show that correcting for the violation of this

assumption implies significance thresholds comparable to, and in some cases larger than, those obtained by applying FDR corrections without accounting for publication bias.

The remainder of the paper proceeds as follows: Section I describes the data collection procedure for reported t-statistics in the business combination laws and Regulation SHO pilot settings and the data used for the semi-parametric bootstrap procedure. Section II details the estimation of the model of biased publication and the two bootstrap procedures used to account for estimation uncertainty. Section III reports point estimates, then bootstrapped results for the estimation. We discuss the practical implications of our results in Section IV. Section V provides concluding remarks.

I. Data

In this section, we describe our sample of manually collected t-statistics reported in papers using the business combination laws and Regulation SHO pilot settings. We also describe our approach to generate outcome variables from CRSP and Compustat meant to capture dependence structures for corporate finance outcomes for our semi-parametric bootstrap.

A. Reported t-statistics

Our empirical data consist of manually collected t-statistics associated with the various outcome variables examined in studies using the staggered implementation of business combination laws and the Regulation SHO pilot program as sources of exogenous variation.[3] U.S. states have enacted business combination laws at different points in time. The enactment of business combination laws has been used as a source of exogenous variation in the threat of a corporate takeover. These state-level changes have been used to examine a wide variety

[3]Heath et al. (2023) provide further description of each empirical setting.

of outcome variables including takeover premiums, wages, corporate investment, corporate innovation, board size, dividends, secondary market liquidity, workplace safety, and CEO aging. The Regulation SHO pilot program assigned firms into treated and control groups and suspended a specific short sale restriction, Rule 10a-1, "the uptick rule," for firms in the treatment group. The pilot has also been reused to examine a wide variety of outcome variables including corporate investment, innovation, payout policies, workplace safety, analysts' rounding of forecasts, and banks' loss recognition.

To construct the database, a list of papers that use business combination laws or the Regulation SHO pilot as a source of exogenous variation was compiled as of March 31, 2021. Unique outcome variables using methodologies that could be represented as difference-in-differences regressions of the following form are included:

$$y_{i,t} = \alpha + \beta_1 \cdot Treatment_{i,t} + \beta_2 \cdot Post + \beta_3 \cdot Treatment_{i,t} \times Post + \epsilon_{i,t}, \tag{1}$$

where $y_{i,t}$ is an outcome variable for firm i in year t, $Treatment_{i,t}$ is either the staggered introduction of state-specific business combination laws or an indicator variable indicating the Regulation SHO pilot stocks, and $Post$ is an indicator variable indicating the period after the beginning of the treatment. Papers using various combinations of fixed effects and/or control variables are included, as long as they examine the β_3 coefficient in a model of the form shown above.

t-statistics are selected based on the following conditions: If there are multiple models examining the same dependent variable, we keep the β_3 coefficient from the model with the most controls and/or fixed effects; models that include additional terms interacted with the main treatment effect, and models used in sub-sample analysis are excluded. Following

Harvey et al. (2016) and part of Chen and Zimmermann (2020), we take reported t-statistics as given, assuming that data errors, coding errors, misalignment, heteroskedasticity, autocorrelation, clustering, outliers, and other issues are all accounted for.[4] Our raw sample of reported t-statistics consists of 114 outcomes for the business combination laws and 434 outcomes for the Regulation SHO pilot. The full lists of outcome variables examined using business combination laws and the Regulation SHO pilot are reported in Appendix Table A.I. We further exclude two reported t-statistics associated with the Regulation SHO pilot that were greater than 40 in magnitude. These two statistics are implausible and the various assumed underlying distributions of tests that we examine cannot fit them. We also exclude results for two papers that only report the level of statistical significance (i.e., 1%, 5%, 10%).

The respective distributions of all reported t-statistics for business combination laws (Panel A) and the Regulation SHO pilot (Panel B) are reported in Figure 1. These distributions exhibit features that are inconsistent with unbiased reporting. Both panels exhibit pronounced discontinuities at $|t| = 1.96$, the asymptotic threshold for statistical significance at the conventional 5% level. The distributions exhibit sharp peaks slightly above the 1.96 threshold, with frequency counts higher than would be expected from a smooth underlying distribution. Furthermore, both distributions exhibit notably sparse regions immediately below $|t| = 1.96$. In Panel A, the frequency drops sharply for t-statistics in the range of 1.5 to 2.[5] These patterns are somewhat more pronounced for business combination laws,

[4]Reported and replicated t-statistics used in cross-sectional asset pricing studies often are not adjusted for implementation costs (see Novy-Marx and Velikov (2016); Patton and Weller (2020); Chen and Velikov (2023); Muravyev, Pearson, and Pollet (2025)). Similarly, for discussions of other potential empirical issues in the business combination laws setting aside from multiple testing, see Karpoff and Wittry (2018) and Hu and Spamann (2019). For discussions of other potential issues in the Regulation SHO pilot setting, see Boehmer, Jones, and Zhang (2019) and Black, Desai, Litvak, Yoo, and Yu (2024).

[5]Brodeur et al. (2016) attribute these discontinuities in part to t-statistic "inflation" by researchers. Similarly, Menkveld et al. (2024) document significant variation in methodological choices across researchers when testing the same hypothesis. Specification issues would further reinforce the need for multiple testing controls.

potentially because early studies of the Regulation SHO pilot sought to empirically rule out hypothesized costs of removing the uptick rule permanently. In contrast to the distribution of t-statistics observed in cross-sectional asset pricing (Harvey et al. (2016); Chen and Zimmermann (2020); Chen (2024)) and in other fields (Brodeur et al. (2016)) where very large t-statistics are common, we observe considerable clustering near the conventional t-statistic hurdle. In particular, Chen and Zimmermann (2020) document that approximately half of the t-statistics they examine are larger than 4.0, a magnitude observed far less frequently in our settings.

[INSERT FIGURE 1 ABOUT HERE]

Because the approaches of Harvey et al. (2016), Harvey and Liu (2021), and Chen (2024) rely on the well-observed region of tests conducted by researchers in the presence of publication bias, we follow this literature and remove outcomes with reported t-statistics smaller than 1.96 in magnitude. Harvey et al. (2016) argue that in addition to non-reporting of non-significant cross-sectional factors, significant factors may also not be reported because of incentives in the asset-management industry. Such non-reporting of significant results is less likely in our setting of reused natural and regulatory experiments. Removing t-statistics smaller than 1.96 in magnitude results in a sample of 73 and 180 outcome variables for business combination laws and the Regulation SHO pilot, respectively. For comparison, Chen and Zimmermann (2020) examine a sample of 77 reported statistics as part of their analysis.

B. CRSP/Compustat Variables

Samples of CRSP and Compustat outcome variables are constructed to capture dependence structures in the original business combination laws and Regulation SHO pilot samples

for the semi-parametric bootstrap detailed in Section II.A. For business combination laws, we follow the sample construction procedure of Karpoff and Wittry (2018).[6] This sample consists of annual Compustat data for fiscal years ending from 1976 through 1995, excluding financial firms, utilities, and observations with missing/negative sales or total assets. As in Karpoff and Wittry (2018), our final sample consists of 10,213 firms and 88,648 firm-year observations.

For Regulation SHO, we examine the sample of treatment and control firms in Diether, Lee, and Werner (2009).[7] This sample excludes stocks that were added to the Russell 3000 index over the period June 2004 through June 2005. Stocks are also excluded if they underwent corporate events such as mergers, bankruptcies, etc., were added or eliminated in the June 2005 index reconstitution, underwent ticker changes, were listed on Nasdaq's small-cap market, changed their listing venue, or were acquired, merged, or privatized. Stocks with an average price above $100 or average quoted spread exceeding $1.00 are also excluded. We further require the availability of annual Compustat data for fiscal years ending during 2002 through 2009, excluding observations with missing/negative sales or total assets. The final sample consists of 1,708 (576 pilot, 1,132 control) firms and 12,284 firm-year observations.

Following Heath et al. (2023), we collect variables from Compustat and CRSP, and include commonly used transformations of each variable to use as outcomes for the semi-parametric bootstrap detailed in II.A. To arrive at a set of Compustat variables, we collect raw variables from financial statements that are not missing for at least 70% of observations in a sample between January 1970 and June 2019. For Compustat outcomes, we use the raw variable, raw variable scaled by total assets, and the percentage change in the raw variable scaled by total assets. This approach results in 96 raw Compustat variables, generating

[6]We thank Michael Wittry for sharing the data set.
[7]We thank Ingrid Werner for sharing the data set.

288 Compustat outcomes in total. We also use monthly CRSP stock data to calculate firm-year average trading volume, average share turnover, cumulative returns, average dollar bid-ask spread, and average percentage bid-ask spread using firms' fiscal years for temporal aggregation. The resulting sample consists of 293 variables. Appendix Table A.II lists the raw CRSP/Compustat outcomes used for the semi-parametric bootstrap. All variables are winsorized at the 2.5% and 97.5% levels.

II. Estimation

We apply a structural model of biased publication developed in Chen (2024) to the business combination laws and Regulation SHO pilot settings using quasi-maximum likelihood. In contrast to other meta-analyses in cross-sectional asset pricing, the model can be directly applied to reported t-statistics, making it applicable to other empirical settings. In the model, a literature is generated in two steps. In the first step, researchers generate an "unbiased" set of results characterized by μ_i, a t-statistic that corrects for sampling error. For true results, μ_i follows an arbitrary probability distribution with positive mean λ_μ and standard deviation λ_σ. For false results, μ_i equals 0. Result i is false with probability π_F. In the second step of the model, result i is reported based on its statistical significance, where the probability of reporting is weakly increasing in the magnitude of the t-statistic up to a certain point beyond which the probability of reporting is fixed at an arbitrary upper bound.

In our estimation, we allow for a variety of assumptions about the underlying probability distribution and publication function for observed t-statistics previously employed in the literature. We assume that true results are log-normal as in the baseline specification of Chen (2024). We also assume that true results are exponentially distributed (Harvey et al. (2016)). Finally, we assume that true results follow a scaled t-distribution (Andrews and

Kasy (2019)).[8]

For the publication function, we adopt an assumption used by Harvey et al. (2016) and Chen (2024), assuming the following staircase function:

$$\Pr(\text{pub}_i||t_i|) = \begin{cases} \eta\bar{s} & \text{if } |t_i| \in (1.96, 2.58] \\ \bar{s} & \text{if } |t_i| > 2.58. \end{cases} \tag{2}$$

This functional form captures both the standard t-statistic thresholds in economics, and the fact that a higher t-statistic implies a higher probability of reporting. As in Chen (2024), we allow η to fall within an interval $\eta \in [\frac{1}{3}, \frac{2}{3}]$, allowing for some proportion of marginally-significant results to go unreported. While maintaining the assumption that true results are log-normally distributed, we also restrict η to equal 0.5 as in Harvey et al. (2016).

We estimate the parameters ($\pi_F, \lambda_\mu, \lambda_\sigma$, and depending on the assumed functional form of the publication function, η) with quasi-maximum likelihood, choosing parameters that maximize the mean log marginal likelihood of $|t_i|$ conditional on i being reported. Chen (2024) finds that quasi-maximum likelihood is consistent under pairwise correlations as high as 0.90 and it is essentially unbiased in relatively small samples for π_F.[9] For the numerical integration used to compute log-likelihoods and multiple testing statistics, we also assume $\pi_F \in [0.01, 0.99]$ for technical reasons. To account for estimation uncertainty, we additionally bootstrap estimates using two approaches detailed in the following section.

[8]Other distributional assumptions examined for further robustness by Chen (2024) do not fit our empirical data well.

[9]This consistency result is established for correlation among tests conducted simultaneously within a single study. Our setting instead involves correlation *across* separate studies examining different outcomes with the same underlying treatment and control assignment, a channel we are not aware of a corresponding theoretical guarantee for. This is one motivation for the semi-parametric bootstrap we use in Section II.A that estimates this correlation structure directly rather than relying on it.

A. Bootstraps

We account for estimation uncertainty in our baseline results using two bootstraps. In our empirical settings, it is important for the bootstraps to reflect biased publication. Furthermore, as firms optimize along multiple margins, accounting for correlations across tests is also important in corporate finance settings. The first bootstrap we apply is a simple non-parametric bootstrap where we draw reported t-statistics with replacement. Since these t-statistics are drawn from the empirical data, they will also reflect biased publication. To explicitly account for correlations in the business combination laws and Regulation SHO pilot samples with commonly used corporate finance data, we also use a semi-parametric bootstrap. Specifically, we use the 293 CRSP/Compustat outcome variables discussed in Section I.B and reported in Appendix Table A.II. This bootstrap adapts the semi-parametric bootstrap developed by Chen (2024) to our respective empirical settings and is designed to explicitly mimic the correlation structure in each setting while also ensuring that the samples reflect biased reporting in observed t-statistics. For the semi-parametric bootstrap in the business combination laws setting, we first estimate panel regressions following Karpoff and Wittry (2018) and Heath et al. (2023) of the form:

$$y_{i,j,l,s,t} = \alpha_i + \alpha_{l,t} + \alpha_{j,t} + \beta \cdot BC_{s,t} + \theta' \mathbf{x_{i,t}} + \delta' \mathbf{L_{i,s,t}} + \epsilon_{i,j,l,s,t}, \tag{3}$$

where $y_{i,j,l,s,t}$ is the outcome variable of interest for firm i in year t in industry j, located in state l, and incorporated in state s, BC is an indicator variable equal to one if business combination laws had been adopted in state s by year t and zero otherwise. $\mathbf{x_{i,t}}$ includes size, size squared, age, and age squared, and $\mathbf{L_{i,s,t}}$ includes the full set of Karpoff and Wittry (2018) controls for institutional and legal context. Standard errors are clustered at the firm

level.

For the Regulation SHO pilot, we follow Heath et al. (2023) and estimate panel regressions of the form

$$y_{i,t} = \alpha_i + \alpha_t + \beta \cdot Treatment_{i,t} + \theta' \mathbf{x_{i,t}} + \epsilon_{i,t}, \tag{4}$$

where $y_{i,t}$ is the outcome variable of interest for firm i in year t, $Treatment_{i,t}$ is an indicator variable equal to one if the firm is in the pilot group and the fiscal year ends on July 31, 2005 or later, or if the firm is in the control group and the firm's fiscal year ends on July 31, 2008 or later, and equal to zero otherwise. This ensures that pilot firms' entire fiscal year is after the pilot announcement date on July 28, 2004 and that control firms' entire fiscal year is after the repeal of the Regulation SHO price tests for all firms on July 6, 2007. The vector $\mathbf{x_{i,t}}$ contains control variables including the natural log of book value of assets (size), size squared, firm age, and firm age squared. We include firm and year fixed effects and standard errors are clustered at the firm level.

For the purposes of generating residuals for the semi-parametric bootstrap, we center the actual data before resampling it by subtracting the fitted values from each observation. We then bootstrap the centered data using an approach similar to Heath et al. (2023). We bootstrap 1,000 samples by randomly sampling firms with replacement from each sample. To preserve the time series properties of the raw data, we draw all dates for each drawn firm. For business combination laws, we stratify the draws by state of incorporation to reflect the original structure of the data. For the Regulation SHO pilot, we similarly stratify draws by treatment status. After drawing firms, we generate a new firm index to preserve the correct degrees of freedom when absorbing fixed effects.

For each of the 1,000 replicates, we construct residuals by sampling five of the 293 outcomes with replacement and estimate treatment effects with the centered data, resulting in

5,000 residuals ε_i in total. Following Chen (2024), we add this sampling noise to simulated t-statistics using the point estimate in our estimations. Specifically, we simulate 5,000 μ_i using the point estimate, calculate $t_i = \mu_i + \varepsilon_i$, and then apply Equation 32 of Chen (2024) to obtain reported t-statistics once again using the point estimate. Importantly, this bootstrap explicitly captures correlations in the empirical data, while also maintaining publication bias.

III. Results

Quasi-maximum likelihood provides parameter estimates for the share of false results, π_F, the mean and standard deviation parameters that govern the distribution of true results (λ_μ and λ_σ), and depending on the assumed functional form of the publication function, the parameter corresponding to the publication probability of marginally significant results, η. With estimates of π_F, λ_μ, and λ_σ, we obtain multiple testing statistics.

Our main specification assumes true results are log-normally distributed and estimates the publication probability of marginally significant results, η, within the interval [1/3, 2/3] rather than fixing it at a single value. This corresponds to the baseline distributional assumption in Chen (2024) and is the specification underlying Figures 2 - 4; it lets the degree of under-reporting of marginally significant results be inferred from the data rather than imposed; and, as we show below, it is often the most conservative of the specifications we consider, yielding lower estimates of shrinkage and the FDR than the alternative distributional and publication-function assumptions.

A. Multiple Testing Statistics

While family-wise error rate corrections used in Harvey et al. (2016), Chordia et al. (2020), and Heath et al. (2023) are designed to maintain control of the probability of any false

positives at the usual 5% level in economics, the profession may be willing to tolerate more false positives as the number of tests in a given setting increases. False discovery rate corrections are instead designed to control the expected fraction of "discoveries" that are false positives. The first statistic is the minimum hurdle required to control the FDR at the 5% level, which depends more on the false results parameter π_F in the estimation. The 5% level for the FDR has been proposed by several papers on multiple testing in finance (see, Harvey et al. (2016); Chordia et al. (2020); Heath et al. (2023); Chen (2024); among others).

$$h(5\%) \equiv \min_{h \in \mathbb{R}_+} \{h : \text{FDR}(h) \leq 5\%\} \tag{5}$$

We also compute empirical Bayes shrinkage, which has been shown to be robust to publication bias (see Chen and Zimmermann (2020) for discussion). Empirical Bayes shrinkage reflects how much researchers should shrink observed t-statistics $|\bar{t}|$ toward zero in order to obtain unbiased t-statistics.[10] This multiple testing statistic depends more on the estimated mean and standard deviation parameters that govern true results in the estimation, λ_μ and λ_σ. Empirical Bayes shrinkage is examined in Chen and Zimmermann (2020), Chinco et al. (2021), Harvey and Liu (2021), Jensen et al. (2023), and Chen (2024), using various approaches.

$$\text{shrinkage}(\bar{t}) \equiv \frac{|\bar{t}| - \mathbb{E}(\mu_i \,|\, |t_i| = \bar{t})}{|\bar{t}|} \tag{6}$$

Finally, we compute local FDRs (Chen and Zimmermann (2017); Chen (2024)). This

[10]This shrinkage factor is computed on the t-statistic because μ_i, the bias-corrected quantity the model estimates, is itself defined as a t-statistic. Publication selection likely operates on the point estimate rather than its precision, in which case a given percentage shrinkage of the t-statistic corresponds to an equal percentage shrinkage of the underlying economic magnitude of a reported effect, holding its standard error fixed.

approach is designed to target statistically significant statistics and is also robust to publication bias. FDR($\bar{t}$) represents the probability that a result with a t-statistic of $|\bar{t}|$ is false. The local FDR also depends more on the parameters that govern true results, as for empirical Bayes shrinkage. Among FDR procedures, Chen (2022) provides simulation evidence suggesting that the Benjamini-Yekutieli (BHY; Benjamini and Yekutieli (2001)) correction used by Harvey et al. (2016), Chordia et al. (2020), and Heath et al. (2023) may also be overly restrictive, further motivating our use of this local FDR measure.

$$\text{FDR}(\bar{t}) \equiv \Pr(F_i \,|\, |t_i| = \bar{t}) \tag{7}$$

FDR($\bar{t}$) can be integrated from the minimum hurdle to control the FDR at the 5% level, h, to infinity to obtain a Bayesian formulation of the FDR.

B. Baseline Estimates

B.1. Business combination laws

We report results for selected baseline estimates and multiple testing statistics for business combination laws in Table I. Across the distributional and functional form assumptions, estimates for the share of false results π_F are larger than documented in previous work in cross-sectional asset pricing. Under our main specification, we estimate a share of false results of $\pi_F = 0.82$, a minimum t-hurdle of 3.0 to control the FDR at the 5% level, empirical Bayes shrinkage of 39.90%, and a local FDR of 29.28%.

Across the specifications, parameter estimates for π_F range from 0.82 to 0.96. These estimates are close to $\pi_F = 1$, the value implicitly assumed by the Benjamini and Hochberg (1995) (BH) correction, which controls the FDR without estimating π_F. These estimates

for the business combination laws setting are also larger in magnitude than the baseline estimates of 0.44 from Harvey et al. (2016) and 0.01 from Chen (2024) in cross-sectional asset pricing.

[INSERT TABLE I ABOUT HERE]

As our point estimates for π_F are large, so too are the minimum t-statistic hurdles to control the FDR at the 5% level in our empirical data. FDR t-statistic hurdles range from 3.0 to 3.3 for business combination laws. For comparison, the t-hurdle obtained by applying the BH correction to all reported t-statistics in the business combination laws setting without accounting for publication bias is 2.2. Furthermore, the corresponding BHY t-hurdle reported by Heath et al. (2023) is 3.0.

Estimates for average empirical Bayes shrinkage for business combination laws range from 39.90% to 52.74%. These estimates imply that a reported t-statistic of 5 should be shrunk to an unbiased t-statistic of 3 for the estimate of 39.90% and a t-statistic of 2.36 for the estimate of 52.74%. These shrinkage estimates are also larger than comparable headline estimates in cross-sectional asset pricing: 12% from Chen and Zimmermann (2020), 19% from Harvey and Liu (2021) and similar estimates from Chinco et al. (2021) and Jensen et al. (2023).

Similarly, our local FDR estimates range from 29% to 51% for business combination laws, larger in magnitude than a similar estimate implied by Harvey et al. (2016) and discussed in Chen (2024): 6%. The FDRs in the business combination laws setting are also larger than the corresponding FDR estimate of $\approx 1\%$ from Chen and Zimmermann (2017).

B.2. Regulation SHO pilot

Results for the Regulation SHO pilot are reported in Table II. Results are qualitatively similar across the distributional and functional form assumptions. Under our main specification, we estimate a share of false results of $\pi_F = 0.77$, a minimum t-hurdle of 3.4 to control the FDR at the 5%, empirical Bayes shrinkage of 58.05%, and a local FDR of 36.81%.

Across the specifications, parameter estimates for π_F range from 0.77 to 0.89. The minimum t-statistic hurdles to control the FDR at the 5% level in our empirical data range from 3.4 to 3.6 for the Regulation SHO pilot. For comparison, the t-hurdle obtained by applying the BH correction to all reported t-statistics in the Regulation SHO pilot setting is 2.4, a hurdle lower than that obtained after explicitly accounting for publication bias. The corresponding BHY t-hurdle for all reported tests using the Regulation SHO pilot reported by Heath et al. (2023) of 3.5 is comparable to ours.

[INSERT TABLE II ABOUT HERE]

Empirical Bayes shrinkage estimates range from 48.25% to 66.63% for the Regulation SHO pilot setting. These estimates imply that a reported t-statistic of 5 should be shrunk to an unbiased t-statistic of 2.59 for the estimate of 48.25% and a t-statistic of 1.67 for the estimate of 66.63%. Similar to our results for business combination laws, the local FDR estimates range from 37% to 53% for Regulation SHO. These shrinkage and FDR estimates are larger than the comparable estimates obtained in cross-sectional asset pricing discussed above.

Overall, these baseline results highlight the importance of biased publication and multiple-testing issues in heavily reused natural experiments. Importantly, our estimated shrinkage factors, local FDRs, and minimum t-hurdles to control the FDR appear larger than com-

parable estimates from cross-sectional asset pricing. These results also collectively provide reinforcing evidence for the calls of Heath et al. (2023) to raise t-statistic thresholds in these settings, even in the presence of biased reporting. In the following section, we consider the robustness of the baseline results to bootstrapping using two distinct approaches in conjunction with the various distributional and publication function assumptions made above.

C. Bootstrapped Estimates

While our point estimates of the share of false results and multiple testing statistics are markedly different from corresponding results in cross-sectional asset pricing, these results may be subject to significant estimation uncertainty. Using data on reported cross-sectional asset pricing factor t-statistics, Chen (2024) indicates that multiple testing statistics, particularly those that depend more on the poorly observed region of the t-statistic distribution under biased reporting, can be subject to significant estimation uncertainty.

Consequently, we also bootstrap our results using two approaches detailed in Section II.A. For each assumed distribution and publication function reported in Tables I and II, we bootstrap 1,000 replicates of t-statistics. The two bootstraps we use are a simple bootstrap and a semi-parametric bootstrap. Both of these bootstraps are designed to reflect the reporting bias in the original data and the semi-parametric bootstrap takes an alternative approach of accounting for dependence across tests by explicitly accounting for correlations using actual CRSP and Compustat data in these settings.

C.1. Business combination laws

Bootstrapped results are reported for business combination laws in Table III. Results for the simple bootstrap are reported in Panel A, while results for the semi-parametric bootstrap are

reported in Panel B. We report bootstrapped estimates for the share of false results parameter π_F estimated using quasi-maximum likelihood, and for the various multiple testing statistics including: the minimum t-statistic hurdle required to control the FDR at the 5% level, average empirical Bayes shrinkage, and the local FDR. We report the 5th, 25th, 50th, 75th, and 95th percentiles of the bootstrapped estimates and multiple testing statistics to span the confidence intervals reported by Chen (2024) for cross-sectional asset pricing.

[INSERT TABLE III ABOUT HERE]

While we do observe variation in bootstrapped estimates for the parameter reflecting the share of false results, the bootstrapped confidence intervals are generally tighter than previously documented for cross-sectional asset pricing t-statistics. The smallest lower confidence limit across specifications and bootstraps is for the 0.50-0.98 confidence interval when assuming that t-statistics are log-normally distributed and that the publication function follows the staircase function of Equation 2 with $\eta \in [\frac{1}{3}, \frac{2}{3}]$ for the semi-parametric bootstrap. For reference, the comparable bootstrapped confidence interval for the share of false results parameter in the baseline estimation of Chen (2024) is 0.01-0.89. Furthermore, many of the reported percentiles of π_F are close to the assumed $\pi_F = 1$ of the BH correction. This concentration of large bootstrapped estimates for π_F is in contrast to comparable bootstrapped estimates for cross-sectional asset pricing factors. The π_F parameter primarily governs t-statistic hurdles, which we discuss next.

The large bootstrapped estimates for π_F imply that the minimum t-statistic hurdle to control the FDR at the 5% level is larger than the conventional hurdle. Minimum t-statistic hurdles to control the FDR at the 5% level range from 2.6 to 3.5 across reported percentiles for both the simple and semi-parametric bootstraps. While these statistics do vary across bootstrap replicates, they collectively provide further evidence that t-statistic hurdles should

be raised for this empirical setting in order to control the FDR at the standard level. The full distribution of bootstrapped hurdles assuming that t-statistics are log-normally distributed and that the publication function follows the staircase function of Equation 2 with $\eta \in [\frac{1}{3}, \frac{2}{3}]$ is reported in Panel A of Figure 2. Hurdle statistics are shaded corresponding to estimates for the share of false results parameter, π_F in a given estimation.

[INSERT FIGURE 2 ABOUT HERE]

As the majority of share of false results estimates are large, all of the bootstrapped confidence intervals for t-statistic hurdles are larger than the conventional asymptotic hurdle of 1.96. These bootstrapped hurdles are in stark contrast to analyses in cross-sectional asset pricing. Chen (2024) finds significant variation across bootstrapped estimates, with a confidence interval of 0.0-3.0. Notably, bootstrapped hurdle estimates in cross-sectional asset pricing are characterized by a large mode of bootstrapped t-hurdles also close to zero, raising questions regarding whether cross-sectional asset pricing t-statistic hurdles should even be raised as proposed in Harvey et al. (2016). For business combination laws, we instead observe a large cluster of bootstrapped t-statistic hurdles between 3 and 3.5, even larger than the t-hurdle obtained by simply applying the BH FDR correction to all reported t-statistics without accounting for biased reporting.

Results for multiple testing statistics that rely more on the estimated mean and standard deviation parameters of true result t-statistics are similar. The results suggest that reported t-statistics in the business combination laws setting should be shrunk by up to 67% to obtain their unbiased counterparts. Results are similar for the semi-parametric bootstrap, with shrinkage estimates as large as 66% for reported percentiles. Even our best-case estimates from lower confidence limits for the bootstraps range from 25% to 39%, with several of these

estimates exceeding the worst-case estimate of 28% from Chen (2024) for cross-sectional asset pricing.

The full distribution of bootstrapped shrinkage statistics for the simple bootstrap assuming that t-statistics are log-normally distributed and that the publication function follows the staircase function of Equation 2 with $\eta \in [\frac{1}{3}, \frac{2}{3}]$ is reported in Panel A of Figure 3.

[INSERT FIGURE 3 ABOUT HERE]

We find more variation across bootstrapped statistics for the local FDR than for shrinkage depending on the specification. For our reported percentiles, bootstrapped local FDR statistics are as large as 67% for the business combination laws setting. Furthermore, half of our lower confidence limits are comparable to the worst-case FDR of 22% from Chen (2024) for cross-sectional asset pricing.

The full distribution of bootstrapped local FDR statistics for the simple bootstrap assuming that t-statistics are log-normally distributed and that the publication function follows the staircase function of Equation 2 with $\eta \in [\frac{1}{3}, \frac{2}{3}]$ is reported in Panel A of Figure 4.

[INSERT FIGURE 4 ABOUT HERE]

C.2. Regulation SHO pilot

Bootstrapped results are reported for the Regulation SHO pilot in Table IV. As for business combination laws, results for the simple bootstrap are reported in Panel A, while results for the semi-parametric bootstrap are reported in Panel B.

[INSERT TABLE IV ABOUT HERE]

Results for the share of false results parameter are qualitatively similar to those for business combination laws. The smallest lower confidence limit across specifications and bootstraps is for the 0.22-0.98 confidence interval when assuming that t-statistics are exponentially distributed and that the publication function follows the staircase function of Equation 2 with $\eta \in [\frac{1}{3}, \frac{2}{3}]$ for the simple bootstrap. However, most bootstrapped confidence intervals are tighter in other specifications, particularly for the semi-parametric bootstrap. The π_F parameter primarily governs t-statistic hurdles, which we discuss next.

Even as the lower confidence limits for π_F are smaller in certain cases compared to comparable estimates for business combination laws, the Regulation SHO pilot estimates still imply that the minimum t-statistic hurdle to control the FDR at the 5% level is larger than the conventional hurdle. Minimum t-statistic hurdles to control the FDR at the 5% level range from 2.5 to 3.8 across reported percentiles for both the simple and semi-parametric bootstraps. These estimates again provide further evidence that t-statistic hurdles should be raised for this empirical setting in order to control the FDR at the standard level. The full distribution of bootstrapped hurdles assuming that t-statistics are log-normally distributed and that the publication function follows the staircase function of Equation 2 with $\eta \in [\frac{1}{3}, \frac{2}{3}]$ is reported in Panel B of Figure 2. Hurdle statistics are shaded corresponding to estimates for the share of false results parameter, π_F in a given estimation. We observe a large cluster of bootstrapped t-statistic hurdles between 3 and 3.5, as for business combination laws.

Bootstrapped estimates for shrinkage in the Regulation SHO pilot are qualitatively similar. The results suggest that reported t-statistics in the Regulation SHO pilot setting should be shrunk by up to 83% to obtain their unbiased counterparts. Results are similar for the semi-parametric bootstrap, with shrinkage estimates as large as 79% for reported percentiles. As for business combination laws, even the best-case estimates implied by lower confidence

limits for the bootstraps range from 41% to 57%, with all of the reported percentiles exceeding the worst-case estimate of 28% from Chen (2024).

The full distribution of bootstrapped shrinkage statistics for the simple bootstrap assuming that t-statistics are log-normally distributed and that the publication function follows the staircase function of Equation 2 with $\eta \in [\frac{1}{3}, \frac{2}{3}]$ is reported in Panel B of Figure 3. These estimates are less dispersed than for business combination laws.

We again find more variation across bootstrapped statistics for the local FDR depending on the specification. For our reported percentiles, bootstrapped local FDR statistics range between 7.60% and 79.34%. However, nearly all of the bootstrapped estimates are larger than the worst-case FDR of 22% from Chen (2024) for cross-sectional asset pricing.

The full distribution of bootstrapped local FDR statistics for the simple bootstrap assuming that t-statistics are log-normally distributed and that the publication function follows the staircase function of Equation 2 with $\eta \in [\frac{1}{3}, \frac{2}{3}]$ is reported in Panel B of Figure 4. Conclusions are qualitatively similar to those for the shrinkage estimates in the Regulation SHO pilot setting.

To summarize our bootstrapped results: reported results in the business combination laws setting should be shrunk by up to 67%, while reported results in the Regulation SHO pilot setting should be shrunk by up to 83%. More generally, bootstrapped confidence intervals for shrinkage and local false discovery rates in our empirical settings often exceed the worst-case shrinkage and FDRs of 28% and 22% from Chen (2024), respectively. Furthermore, our bootstrapped quasi-maximum likelihood estimates further suggest a need to raise t-statistic hurdles from the conventional level to control the FDR at the 5% level.

IV. Discussion

We discuss the practical implications of our results in this section. Our empirical findings indicate that publication bias is significant in the extensively reused settings that we examine. Ideally, researchers would report null results while journals would publish these null results. Abadie (2020) provides a discussion of the potential benefits of publishing null results while a recent *Journal of Corporate Finance* special issue is entirely devoted to non-significant results (Ringgenberg (2025)). To help track all reported outcomes tested in reused regulatory and natural experiments, we created a research registry at www.reusingnaturalexperiments.com.

To the extent that outcomes tested in a given setting are not reported, we provide heuristics for researchers reusing a given setting. As discussed above, while the family-wise error rate t-statistic hurdles proposed by Heath et al. (2023) are designed to maintain control of the probability of any false positives at the usual 5% level in economics, the profession may be willing to tolerate more false positives as the number of tests in a given setting increases. False discovery rate corrections are generally more permissive than family-wise error rate corrections while maintaining some control of the frequency of false positives. Our estimates suggest two practical heuristics for researchers and reviewers evaluating findings from heavily reused natural experiments. First, consumers of research should treat a t-statistic of at least 3 (rather than the conventional 1.96) as the effective bar for statistical significance; our main-specification hurdles cluster in this range, and rise further once estimation uncertainty is incorporated. Second, consumers of research should discount the economic magnitude of a newly reported result by roughly 40% to 65%, consistent with our empirical Bayes shrinkage estimates.

We acknowledge several limitations of our estimates. While we explore a range of distri-

butions and publication functions from the extant literature in our analysis, reused natural experiments could potentially follow yet a different publication process. Our analysis is also limited to two settings. Future work could extend our analysis to other heavily reused natural experiments such as the Russell index reconstitution, the Tick Size Pilot, or rainfall-based instruments. Finally, our structural estimates are extrapolated from a relatively small number of well-observed test statistics (73 for business combination laws and 180 for the Regulation SHO pilot), and the corresponding bootstrapped confidence intervals we report indicate that, while directionally consistent, these estimates carry some estimation uncertainty.

V. Conclusion

This paper provides the first empirical evidence of how publication bias interacts with multiple-testing concerns in heavily reused natural experiments. Natural experiments that plausibly identify causal effects are difficult to find, so once one is established, researchers tend to reuse it across a large number of subsequent studies (Mellon (2021); Heath et al. (2023)). Separately, there is a well-documented tendency for authors and journals to publish statistically significant results over null results (Rosenthal (1979); Brodeur et al. (2016)). These two forces interact in ways that further complicate inference, and this paper quantifies that interaction directly in two of the most extensively reused settings in corporate finance: business combination laws and the Regulation SHO pilot.

Using a structural model of the publication process (Chen (2024)), we extrapolate the unreported tests underlying the observed distribution of reported t-statistics in these two settings. Under our main specification, we estimate that the share of false results is 82% for business combination laws and 77% for the Regulation SHO pilot, implying local false discovery rates of 29% and 37%, empirical Bayes shrinkage of 40% and 58%, and minimum

t-statistic hurdles of 3.0 and 3.4 to control the FDR at the conventional 5% level. Two independent bootstrap procedures, including one that explicitly accounts for correlation across outcomes studied with common corporate finance data, reinforce these baseline estimates and in several specifications imply even larger bias, with worst-case shrinkage as high as 67% and 83% and worst-case false discovery rates as high as 67% and 79% for business combination laws and the Regulation SHO pilot, respectively.

In sum, our findings indicate that publication bias is sizable in these settings and reinforce initial evidence that conventional statistical thresholds should be raised to better control false discoveries in extensively reused natural experiments. We provide a research registry to encourage the reporting of tested outcomes. To the extent that tested outcomes are not being reported, we provide heuristics for researchers and consumers of research to use.

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Table I
Estimates, Business Combination Laws

This table reports multiple testing statistics calculated from quasi-maximum likelihood estimates using the estimation approach detailed in Section II for the business combination laws setting. We report results separately for various assumed distributions and publication functions. Results are reported for the share of false results parameter, π_F, the minimum t-statistic hurdle required to control the false discovery rate (FDR) at the 5% level, empirical Bayes shrinkage, and the local FDR.

Assumptions		Estimates			
Distribution	Publication Function	Probability False π_F	t-hurdle, FDR=5%	Shrinkage	FDR
Log-normal	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.82	3.0	39.90	29.28
Log-normal	$\eta = 0.5$	0.96	3.3	52.74	51.15
Exponential	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.85	3.1	43.61	34.67
Scaled t	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.91	3.2	42.19	40.78

Table II
Estimates, Regulation SHO

This table reports multiple testing statistics calculated from quasi-maximum likelihood estimates using the estimation approach detailed in Section II for the Regulation SHO pilot setting. We report results separately for various assumed distributions and publication functions. Results are reported for the share of false results parameter, π_F, the minimum t-statistic hurdle required to control the false discovery rate (FDR) at the 5% level, empirical Bayes shrinkage, and the local FDR.

Assumptions		Estimates			
Distribution	Publication Function	Probability False π_F	t-hurdle, FDR=5%	Shrinkage	FDR
Log-normal	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.77	3.4	58.05	36.81
Log-normal	$\eta = 0.5$	0.89	3.6	66.63	52.69
Exponential	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.86	3.5	62.62	50.08
Scaled t	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.83	3.4	48.25	38.99

Table III
Bootstrapped Multiple Testing Statistics, Business Combination Laws

This table reports multiple testing statistics for business combination laws calculated from bootstrapped quasi-maximum likelihood estimates using the estimation approach detailed in Section II. Results are reported for a simple bootstrap (Panel A), and a semi-parametric bootstrap (Panel B) described in Section II.A. We report results for various assumed distributions and publication functions. Results are reported for the share of false results parameter, π_F, the minimum t-statistic hurdle required to control the false discovery rate (FDR) at the 5% level, empirical Bayes shrinkage, and the local FDR.

Panel A: Business Combination Laws, Simple Bootstrap

Assumptions		Probability False π_F					t-hurdle, FDR=5%				
Distribution	Publication Function	P05	P25	P50	P75	P95	P05	P25	P50	P75	P95
Log-normal	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.54	0.85	0.94	0.96	0.98	2.6	3.0	3.2	3.3	3.4
Log-normal	$\eta = 0.5$	0.80	0.95	0.96	0.97	0.98	3.0	3.2	3.3	3.4	3.5
Exponential	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.57	0.77	0.84	0.89	0.93	2.6	3.0	3.1	3.2	3.3
Scaled t	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.75	0.89	0.93	0.96	0.98	2.9	3.1	3.2	3.3	3.5

		Shrinkage					FDR				
Distribution	Publication Function	P05	P25	P50	P75	P95	P05	P25	P50	P75	P95
Log-normal	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	27.38	38.72	46.78	54.43	64.57	11.84	29.29	43.83	53.34	64.60
Log-normal	$\eta = 0.5$	36.38	45.83	51.78	57.81	66.57	25.83	42.74	49.61	57.10	66.54
Exponential	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	27.06	37.03	43.03	49.57	57.36	12.57	26.39	33.70	42.11	51.77
Scaled t	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	24.83	35.26	42.82	49.92	59.85	21.91	36.33	44.83	53.07	63.92

Panel B: Business Combination Laws, Semi-Parametric Bootstrap

Assumptions		Probability False π_F					t-hurdle, FDR=5%				
Distribution	Publication Function	P05	P25	P50	P75	P95	P05	P25	P50	P75	P95
Log-normal	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.50	0.77	0.88	0.94	0.98	2.6	2.9	3.1	3.3	3.4
Log-normal	$\eta = 0.5$	0.85	0.94	0.96	0.98	0.98	3.1	3.2	3.3	3.4	3.5
Exponential	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.70	0.82	0.86	0.90	0.95	2.8	3.0	3.1	3.2	3.4
Scaled t	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.81	0.91	0.94	0.96	0.99	2.9	3.2	3.3	3.4	3.5

Assumptions		Shrinkage					FDR				
Distribution	Publication Function	P05	P25	P50	P75	P95	P05	P25	P50	P75	P95
Log-normal	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	27.82	35.21	42.27	49.57	59.87	10.88	23.49	34.00	45.70	59.31
Log-normal	$\eta = 0.5$	38.51	46.48	52.99	59.71	66.00	31.21	43.33	50.69	58.37	66.57
Exponential	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	26.60	38.36	45.29	50.90	59.10	19.56	29.80	36.19	43.90	52.58
Scaled t	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	28.26	38.25	44.92	50.02	61.61	24.69	38.63	47.35	54.34	65.17

Table IV
Bootstrapped Multiple Testing Statistics, Regulation SHO

This table reports multiple testing statistics for the Regulation SHO pilot calculated from bootstrapped quasi-maximum likelihood estimates using the estimation approach detailed in Section II. Results are reported for a simple bootstrap (Panel A), and a semi-parametric bootstrap (Panel B) described in Section II.A. We report results for various assumed distributions and publication functions. Results are reported for the share of false results parameter, π_F, the minimum t-statistic hurdle required to control the false discovery rate (FDR) at the 5% level, empirical Bayes shrinkage, and the local FDR.

Panel A: Regulation SHO, Simple Bootstrap

Assumptions		Probability False π_F					t-hurdle, FDR=5%				
Distribution	Publication Function	P05	P25	P50	P75	P95	P05	P25	P50	P75	P95
Log-normal	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.44	0.70	0.77	0.85	0.90	2.8	3.2	3.4	3.5	3.6
Log-normal	$\eta = 0.5$	0.80	0.87	0.90	0.93	0.97	3.4	3.5	3.6	3.7	3.8
Exponential	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.22	0.73	0.87	0.93	0.98	2.5	3.3	3.5	3.6	3.8
Scaled t	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.58	0.82	0.90	0.94	0.98	3.0	3.4	3.5	3.6	3.8

		Shrinkage					FDR				
Distribution	Publication Function	P05	P25	P50	P75	P95	P05	P25	P50	P75	P95
Log-normal	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	46.84	53.98	58.50	64.04	70.95	13.24	28.66	36.81	47.35	58.09
Log-normal	$\eta = 0.5$	56.71	63.09	67.11	70.53	75.99	36.55	47.78	54.72	60.84	70.06
Exponential	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	49.49	57.56	63.77	70.45	82.64	7.60	35.91	50.98	62.07	79.34
Scaled t	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	41.37	47.09	51.68	58.15	70.95	18.84	37.66	48.74	59.20	74.44

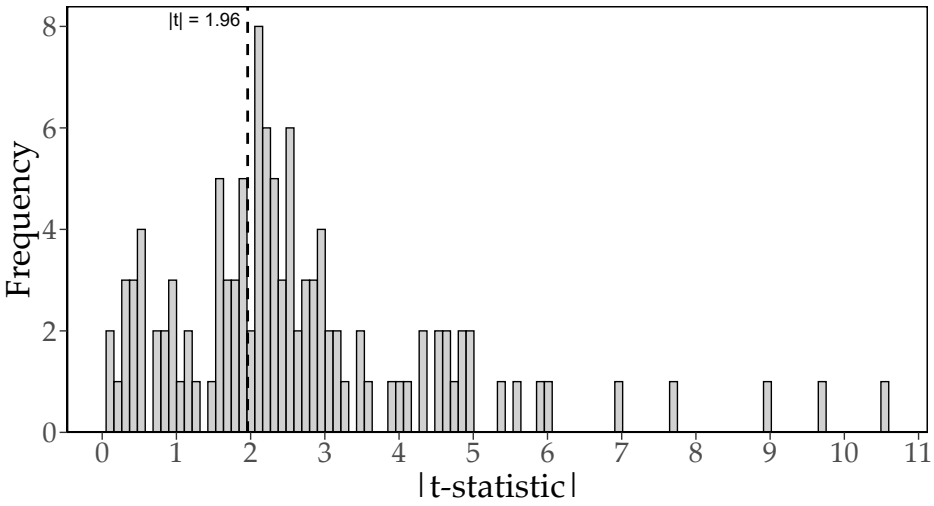
Panel B: Regulation SHO, Semi-Parametric Bootstrap

Assumptions		Probability False π_F					t-hurdle, FDR=5%				
Distribution	Publication Function	P05	P25	P50	P75	P95	P05	P25	P50	P75	P95
Log-normal	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.71	0.81	0.88	0.95	0.99	3.2	3.4	3.5	3.6	3.8
Log-normal	$\eta = 0.5$	0.79	0.87	0.91	0.96	0.99	3.4	3.5	3.6	3.7	3.8
Exponential	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.71	0.83	0.88	0.91	0.95	3.3	3.4	3.5	3.6	3.7
Scaled t	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	0.67	0.88	0.93	0.97	0.99	3.2	3.5	3.6	3.7	3.8

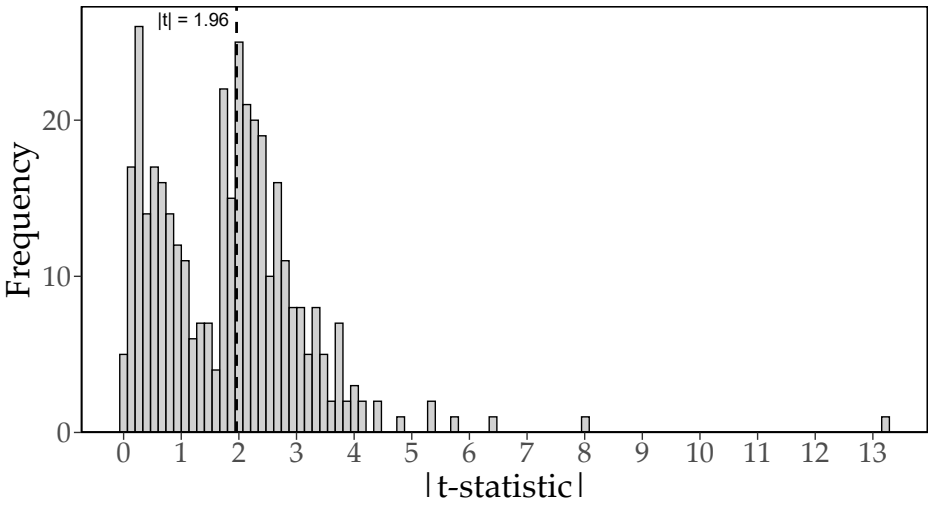
Assumptions		Shrinkage					FDR				
Distribution	Publication Function	P05	P25	P50	P75	P95	P05	P25	P50	P75	P95
Log-normal	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	51.50	57.39	62.37	68.45	78.26	28.28	37.96	48.16	59.88	76.84
Log-normal	$\eta = 0.5$	55.22	62.07	66.98	70.95	79.44	35.22	47.87	55.57	63.91	78.69
Exponential	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	54.80	60.21	64.86	68.33	74.99	32.06	44.09	52.05	58.51	68.98
Scaled t	$\eta \in [\frac{1}{3}, \frac{2}{3}]$	41.40	49.10	55.62	63.83	74.64	24.81	43.89	56.55	67.54	78.63

Figure 1 Distribution of Reported *t*-Statistics

Panel A: Business Combination Laws Sample



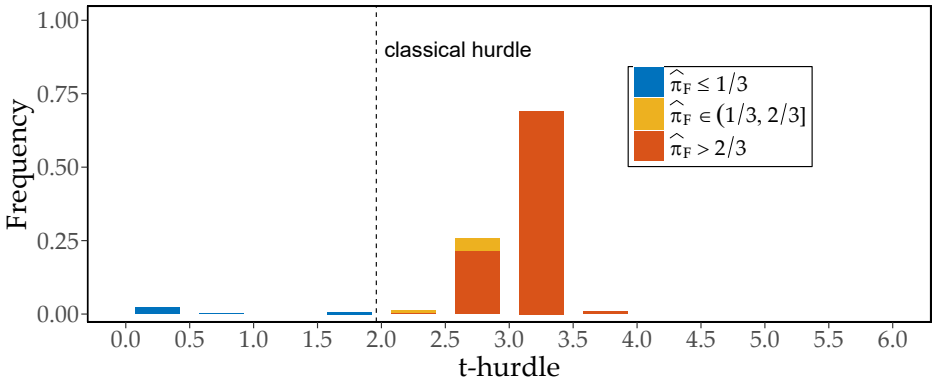
Panel B: Regulation SHO Pilot Sample



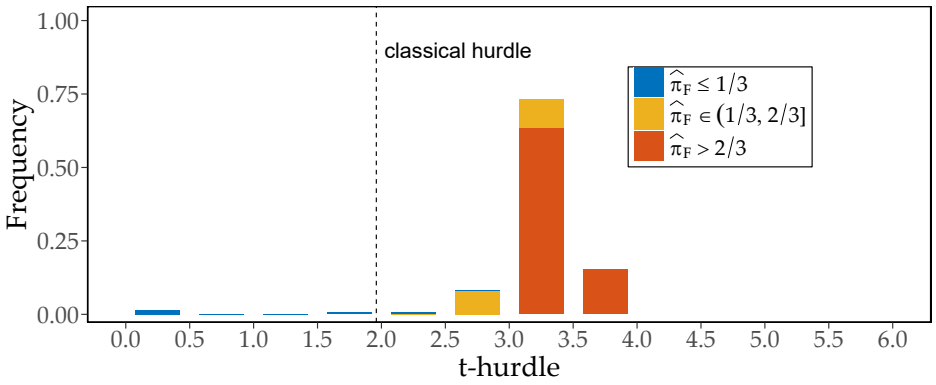
Note: This figure reports the distribution of reported *t*-statistics for business combination laws (Panel A) and for the Regulation SHO pilot (Panel B). *t*-statistics are manually collected and associated with the various outcome variables examined in these empirical settings using the approach detailed in Section I.A. The vertical dashed line in the figure represents the conventional asymptotic *t*-statistic hurdle of 1.96. The list of outcome variables for which *t*-statistics are collected is reported in Table A.I.

Figure 2 Bootstrapped t-Hurdles

Panel A: Business Combination Laws Sample



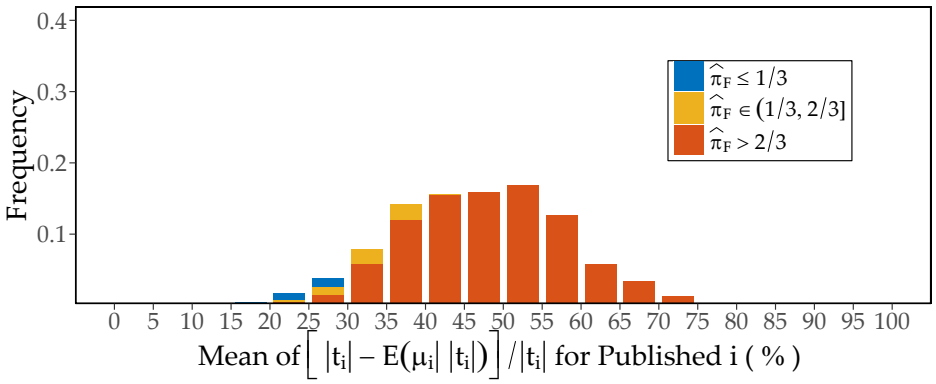
Panel B: Regulation SHO Pilot Sample



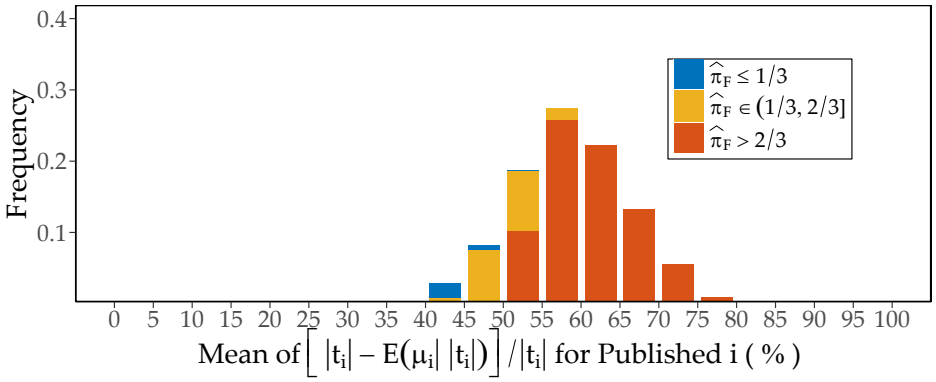
Note: This figure reports statistics on the minimum t-statistic hurdle required to control the false discovery rate (FDR) at the 5% level. Statistics are calculated from bootstrapped quasi-maximum likelihood estimates using the estimation approach detailed in Section II. Results in this figure are reported for the simple bootstrap described in Section II.A, assuming that t-statistics are log-normally distributed and that the publication function follows the staircase function of Equation 2 with $\eta \in [\frac{1}{3}, \frac{2}{3}]$. Hurdle statistics are shaded corresponding to estimates for the share of false results parameter, π_F in a given estimation. Results are reported for business combination laws in Panel A and for the Regulation SHO pilot in Panel B.

Figure 3 Bootstrapped Shrinkage Estimates

Panel A: Business Combination Laws Sample



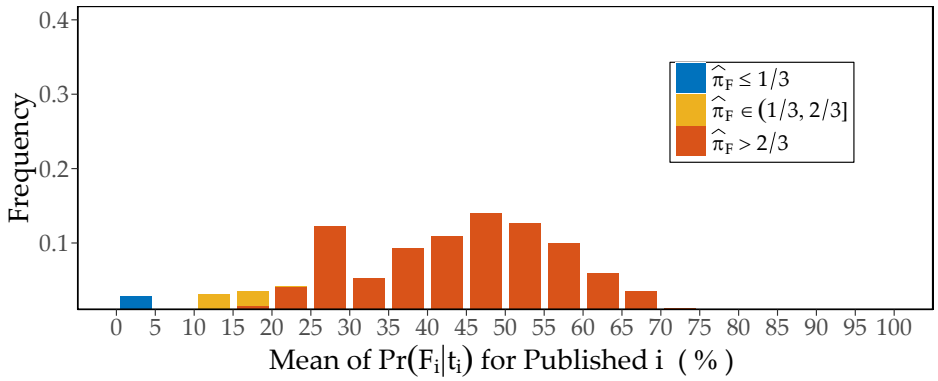
Panel B: Regulation SHO Pilot Sample



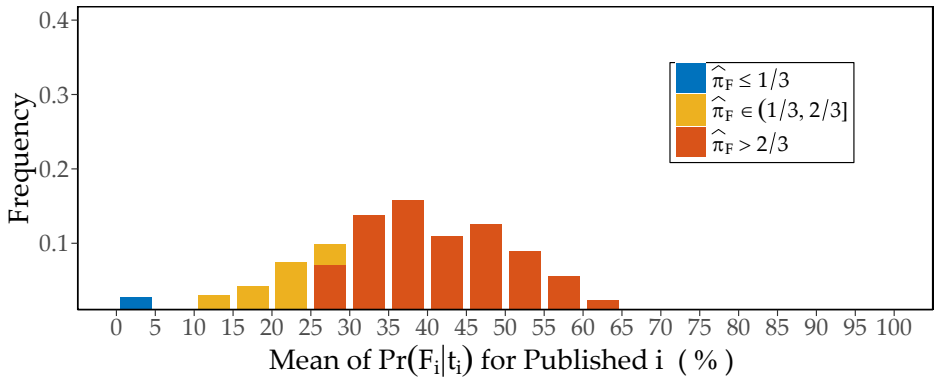
Note: This figure reports empirical Bayes shrinkage statistics that are calculated from bootstrapped quasi-maximum likelihood estimates using the estimation approach detailed in Section II. Results in this figure are reported for the simple bootstrap described in Section II.A, assuming that t-statistics are log-normally distributed and that the publication function follows the staircase function of Equation 2 with $\eta \in [\frac{1}{3}, \frac{2}{3}]$. Shrinkage statistics are shaded corresponding to estimates for the share of false results parameter, π_F in a given estimation. Results are reported for business combination laws in Panel A and for the Regulation SHO pilot in Panel B.

Figure 4 Bootstrapped FDRs

Panel A: Business Combination Laws Sample



Panel B: Regulation SHO Pilot Sample



Note: This figure reports local false discovery rate (FDR) statistics that are calculated from bootstrapped quasi-maximum likelihood estimates using the estimation approach detailed in Section II. Results in this figure are reported for the simple bootstrap described in Section II.A, assuming that t-statistics are log-normally distributed and that the publication function follows the staircase function of Equation 2 with $\eta \in [\frac{1}{3}, \frac{2}{3}]$. FDR statistics are shaded corresponding to estimates for the share of false results parameter, π_F in a given estimation. Results are reported for business combination laws in Panel A and for the Regulation SHO pilot in Panel B.

Appendix

1. Table A.I lists the outcome variables for which we collect reported t-statistics.

2. Table A.II provides data definitions for the 293 variables obtained from Compustat and CRSP.

Table A.I
Reported Outcome Variables

This table lists the outcomes for which we collect t-statistics. Reported outcomes are collected using the approach detailed in Section I.A. Panel A lists outcomes examined in relation to business combination laws. Panel B lists outcomes examined in relation to Regulation SHO.

Panel A: Business Combination Laws

First Draft	Draft used	Paper	Outcome	Location
10/1/1999	10/1/1999	Bertrand and Mullainathan (1999a)	Log Deflated Wage	Tab 3, Col 2
11/29/1999	11/29/1999	Bertrand and Mullainathan (1999)	Log of Total CEO Compensation	Tab 3, Col 1
10/1/2003	10/1/2003	Bertrand and Mullainathan (2003)	Log of Production Worker Wages	Tab 3, Col 4
10/1/2003	10/1/2003	Bertrand and Mullainathan (2003)	Log of White Collar Worker Wages	Tab 4, Col 2
10/1/2003	10/1/2003	Bertrand and Mullainathan (2003)	Log of White Collar Worker Employment	Tab 4, Col 4
10/1/2003	10/1/2003	Bertrand and Mullainathan (2003)	Plant Death Dummy	Tab 5, Col 4
10/1/2003	10/1/2003	Bertrand and Mullainathan (2003)	Plant Birth Dummy	Tab 6, Col 3
10/1/2003	10/1/2003	Bertrand and Mullainathan (2003)	Log of Plant-Level Investment	Tab 7, Col 3
10/1/2003	10/1/2003	Bertrand and Mullainathan (2003)	Log of the number of plants	Tab 8, Col 1
10/1/2003	10/1/2003	Bertrand and Mullainathan (2003)	Log of Capital Stock	Tab 8, Col 2
10/1/2003	10/1/2003	Bertrand and Mullainathan (2003)	Log of Employment	Tab 8, Col 3
10/1/2003	10/1/2003	Bertrand and Mullainathan (2003)	Log of Total Value of Shipment	Tab 8, Col 4
10/1/2003	10/1/2003	Bertrand and Mullainathan (2003)	TFP Percentile	Tab 9, Col 4
10/1/2003	10/1/2003	Bertrand and Mullainathan (2003)	Return on Capital	Tab 9, Col 9
3/26/2004	3/26/2004	Cheng, Nagar, and Rajan (2005)	ln[1+director/officer stock holdings] (%)	Pg 658 Discussion of Robustness
3/2/2007	1/30/2013	Atanassov (2013)	$\text{Ln}(1+\text{Pat})_{t+4}$	Tab 3, Col 2
3/2/2007	1/30/2013	Atanassov (2013)	$\text{Ln}(1+\text{Cit/Pat})_{t+4}$	Tab 3, Col 5
3/2/2007	1/30/2013	Atanassov (2013)	$\text{Ln}(1+\text{Pat})_{t+3}$	Tab 3, Col 1
3/2/2007	1/30/2013	Atanassov (2013)	$\text{Ln}(1+\text{Cit/Pat})_{t+3}$	Tab 3, Col 4
3/2/2007	1/30/2013	Atanassov (2013)	$\text{Ln}(\text{Q})_{t+1}$	Tab 8, Col 1
3/2/2007	1/30/2013	Atanassov (2013)	$\text{Ln}(\text{Q})_{t+2}$	Tab 8, Col 2
3/2/2007	1/30/2013	Atanassov (2013)	$\text{Ln}(\text{Q})_{t+3}$	Tab 8, Col 3
8/9/2007	10/31/2009	Giroud and Mueller (2010)	ROA	Tab 3, Col 1
8/9/2007	10/31/2009	Giroud and Mueller (2010)	Likelihood of being acquired	Tab 6, Col 1
7/16/2008	5/29/2009	Qiu and Yu (2009)	Credit Spread	Tab 4, Col 2
11/24/2008	11/24/2008	Juks (2008)	ln(1+S+W+R)	Tab 4, Col 4
11/24/2008	11/24/2008	Juks (2008)	ln(1+SP+WP+RP)	Tab 5, Col 4
11/24/2008	11/24/2008	Juks (2008)	Accidents	Tab 6, Col 4
11/24/2008	11/24/2008	Juks (2008)	ln(1+S)	Tab 7, Col 4
11/24/2008	11/24/2008	Juks (2008)	ln(1+W)	Tab 8, Col 4
11/24/2008	11/24/2008	Juks (2008)	ln(1+R)	Tab 9, Col 4
3/3/2009	3/3/2009	Zhao and Chen (2009)	Magnitude of Abnormal Accruals	Tab 5A, Col 2
3/22/2009	3/22/2009	Huang and Zhao (2009)	Turnover	Tab 3, Col 4
12/31/2009	3/21/2011	Francis et al. (2011)	Dividend Payout Ratio	Tab 4, Col 8
12/31/2009	3/21/2011	Francis et al. (2011)	Dividend Dummy	Tab 5, Col 9
12/31/2009	3/21/2011	Francis et al. (2011)	Total Payout Ratio	Tab 7, Col 2
12/31/2009	3/21/2011	Francis et al. (2011)	Cash Holding	Tab 9, Col 2
12/31/2009	3/21/2011	Francis et al. (2011)	Dividend Payout Ratio II	Tab 12, Col 2
12/31/2009	3/21/2011	Francis et al. (2011)	Dividend Payout Ratio III	Tab 12, Col 4
1/24/2010	12/7/2015	Cen, Dasgupta, and Sen (2016)	Firm-level num PC	Tab 6, Col 1
1/24/2010	12/7/2015	Cen, Dasgupta, and Sen (2016)	Firm-level PC percentage	Tab 6, Col 2
1/24/2010	12/7/2015	Cen, Dasgupta, and Sen (2016)	Relationship-level PC percentage	Tab 6, Col 3
6/27/2010	3/20/2014	Callen, Guan, and Qiu (2014)	CSCORE	Tab 5, Col 2
6/27/2010	3/20/2014	Callen, Guan, and Qiu (2014)	Special Items scaled by total assets	Tab 9, Col 3
8/30/2010	7/5/2012	Huang and Peyer (2012)	Percentage of outside director	Tab 4, Col 1
8/30/2010	7/5/2012	Huang and Peyer (2012)	Director retainer fee	Tab 4, Col 2
8/30/2010	7/5/2012	Huang and Peyer (2012)	Director incentive pay	Tab 4, Col 3
8/30/2010	7/5/2012	Huang and Peyer (2012)	Classified Board	Tab 4, Col 4
8/30/2010	7/5/2012	Huang and Peyer (2012)	Largest block	Tab 4, Col 5
8/30/2010	7/5/2012	Huang and Peyer (2012)	Block at 5%	Tab 4, Col 6
8/30/2010	7/5/2012	Huang and Peyer (2012)	Debt/Value	Tab 4, Col 7
8/30/2010	7/5/2012	Huang and Peyer (2012)	CEO Salary and Bonus	Tab 4, Col 8
8/30/2010	7/5/2012	Huang and Peyer (2012)	CEO Stock ownership	Tab 4, Col 9
8/30/2010	7/5/2012	Huang and Peyer (2012)	CEO long term incentive pay	Tab 4, Col 10
4/27/2011	6/27/2013	Kim and Purnanandam (2013)	SEO CAR	Tab 2, Col 2
2/13/2013	11/17/2015	Amore and Bennedsen (2016)	ln(1+green patent cites)	Tab 3, Col 1
2/13/2013	11/17/2015	Amore and Bennedsen (2016)	At least one green patent	Tab 3, Col 2
2/13/2013	11/17/2015	Amore and Bennedsen (2016)	Green Patents to total patents	Tab 3, Col 4
2/13/2013	11/17/2015	Amore and Bennedsen (2016)	Green cites to total cites	Tab 3, Col 5
2/13/2013	11/17/2015	Amore and Bennedsen (2016)	Complex Patents	Tab 8, Col 1
2/13/2013	11/17/2015	Amore and Bennedsen (2016)	Non-complex patents	Tab 8, Col 2

3/14/2013	3/14/2013	Santos (2013)	R&D Expenditures	Tab 4, Col 4
10/1/2013	10/1/2013	Sauvagnat (2013)	Equity Issues	Tab 10, Col 1
10/1/2013	10/1/2013	Sauvagnat (2013)	Net Change in Equity	Tab 10, Col 3
10/1/2013	10/1/2013	Sauvagnat (2013)	Debt Issues	Tab 10, Col 5
10/1/2013	10/1/2013	Sauvagnat (2013)	Net Change in Debt	Tab 10, Col 7
10/1/2013	10/1/2013	Sauvagnat (2013)	Sales Growth	Tab 11, Col 1
10/1/2013	10/1/2013	Sauvagnat (2013)	Asset Growth (last three years)	Tab 11, Col 3
2/7/2014	4/1/2015	Cardella, Fairhurst, and Klasa (2015)	STI	Tab 11, Pan A, Col 8
3/21/2014	3/21/2014	Zeng (2014)	Ln(1+Adj_Patent)	Tab 3, Col 1
3/21/2014	3/21/2014	Zeng (2014)	Ln(1+Adj_Citation)	Tab 3, Col 3
7/14/2014	8/23/2016	Gormley and Matsa (2016)	Stock Volatility	Tab 2, Col 1
7/14/2014	8/23/2016	Gormley and Matsa (2016)	Performance-related exit	Tab 2, Col 2
7/14/2014	8/23/2016	Gormley and Matsa (2016)	Operating asset volatility	Tab 2, Col 3
7/14/2014	8/23/2016	Gormley and Matsa (2016)	Cash flow volatility	Tab 2, Col 4
7/14/2014	8/23/2016	Gormley and Matsa (2016)	ln(Cash) (Firm, CH)	Tab 2, Col 5
7/14/2014	8/23/2016	Gormley and Matsa (2016)	Number of acquisitions	Tab 3, Col 1
7/14/2014	8/23/2016	Gormley and Matsa (2016)	Any Acquisition Indicator	Tab 3, Col 2
7/14/2014	8/23/2016	Gormley and Matsa (2016)	Deal value/Acquirer assets in t-1	Tab 3, Col 3
7/14/2014	8/23/2016	Gormley and Matsa (2016)	Number of Diversifying acquisitions	Tab 3, Col 4
7/14/2014	8/23/2016	Gormley and Matsa (2016)	Correlation of Stock Returns	Tab 4, Col 1
7/14/2014	8/23/2016	Gormley and Matsa (2016)	Altman Z-score	Tab 4, Col 2
7/14/2014	8/23/2016	Gormley and Matsa (2016)	Correlation of operating asset returns	Tab 4, Col 3
7/14/2014	8/23/2016	Gormley and Matsa (2016)	Correlation of cash flow to assets	Tab 4, Col 4
7/14/2014	8/23/2016	Gormley and Matsa (2016)	ln(Cash) (Target, CH)	Tab 4, Col 5
7/14/2014	8/23/2016	Gormley and Matsa (2016)	Three-year asset CAGR	Tab 4, Col 6
7/14/2014	8/23/2016	Gormley and Matsa (2016)	Cash flow/Assets	Tab 4, Col 7
7/14/2014	8/23/2016	Gormley and Matsa (2016)	Payout/Assets	Tab 4, Col 8
7/14/2014	8/23/2016	Gormley and Matsa (2016)	Percent Equity	Tab 5, Col 1
7/14/2014	8/23/2016	Gormley and Matsa (2016)	Acquirer announcement CAR [-1, 1]	Tab 5, Col 2
9/11/2014	12/15/2017	Karpoff and Wittry (2017)	Capex	Tab 4, Col 2
9/11/2014	12/15/2017	Karpoff and Wittry (2017)	PPE Growth	Tab 4, Col 3
9/11/2014	12/15/2017	Karpoff and Wittry (2017)	Asset Growth	Tab 4, Col 4
9/11/2014	12/15/2017	Karpoff and Wittry (2017)	SGA Expense	Tab 4, Col 6
9/11/2014	12/15/2017	Karpoff and Wittry (2017)	Leverage	Tab 4, Col 7
11/2/2014	4/12/2017	Cain, Mckeon, and Solomon (2017)	Acquired	Tab 4, Col 2
11/2/2014	4/12/2017	Cain, Mckeon, and Solomon (2017)	Hostile Hazard	Tab 4, Col 4
11/2/2014	4/12/2017	Cain, Mckeon, and Solomon (2017)	Hostile Single-Period	Tab 4, Col 6
9/6/2015	7/9/2019	Pasquariello (2019)	ILLIQ	Tab 5, Col 4
11/15/2015	8/25/2016	Bhargava, Faircloth, and Zeng (2017)	$\text{Count}_{(t+1)}$	Tab 2, Col 1
11/15/2015	8/25/2016	Bhargava, Faircloth, and Zeng (2017)	$\text{NCSKEW}_{(t+1)}$	Tab 2, Col 2
11/15/2015	8/25/2016	Bhargava, Faircloth, and Zeng (2017)	$\text{DUVOL}_{(t+1)}$	Tab 2, Col 3
12/2/2015	8/21/2016	Castellaneta and Conti (2017)	IRR	Tab 4, Col 1
3/16/2016	3/16/2018	Fich, Harford, and Yore (2019)	ln(Cash) (CHE)	Tab 6, Col 2
5/20/2016	6/4/2019	Bharath and Hertzel (2019)	Loan Issue	Tab 3, Pan A, Col 7
5/20/2016	6/4/2019	Bharath and Hertzel (2019)	Loan Covenant strictness	Tab 5, Col 3
10/1/2016	1/1/2018	Chen, Cheng, Lo, and Wang (2016)	CEO Contractual Protection	Tab B-1
12/3/2016	8/27/2020	Gao, Li, and Ma (2020)	Loan Spread	Tab 4, Col 2
11/20/2017	11/11/2019	Phillips and Zhdanov (2019)	Scaled VC deals	Tab A12, Pan A, Col 5
11/20/2017	11/11/2019	Phillips and Zhdanov (2019)	Scaled unique VC deals	Tab A12, Pan A, Col 6
1/1/2020	1/1/2020	Gao, Jia, and Kim (2020)	CashETR(t)	Tab 5, Pan E, Col 1
1/1/2020	1/1/2020	Gao, Jia, and Kim (2020)	CashETR3(t)	Tab 5, Pan E, Col 3
6/29/2020	6/29/2020	Borgschulte, Guenzel, Liu, and Malmendier (2020)	CEO Pay	Tab 5, Pan B, Col 3
6/29/2020	6/29/2020	Borgschulte, Guenzel, Liu, and Malmendier (2020)	Death	Tab 3, Col 6

Panel B: Regulation SHO

First Draft	Draft used	Paper	Outcome	Location
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Volume	Tab 2, Pan A, Col 7, Row 1
3/15/2006	6/16/2007	Alexander and Peterson (2008)	No. Trades	Tab 2, Pan A, Col 7, Row 2
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Short/Long	Tab 2, Pan A, Col 7, Row 4
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Trade Size (Short Sale)	Tab 2, Pan A, Col 7, Row 3
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Quoted Spread	Tab 3, Pan A, Col 7, Row 1
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Effective Spread (trade weighted)	Tab 3, Pan A, Col 7, Row 2
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Bid Size	Tab 3, Pan A, Col 7, Row 3
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Ask Size	Tab 3, Pan A, Col 7, Row 4
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Bsize/Asize	Tab 3, Pan A, Col 7, Row 5
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Effective Spread of short sales	Tab 4, Pan A, Col 7, Row 1
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Trades < Midpoint	Tab 4, Pan A, Col 7, Row 2
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Price Location	Tab 4, Pan A, Col 7, Row 5
3/15/2006	6/16/2007	Alexander and Peterson (2008)	ES(P<MP)	Tab 4, Pan A, Col 7, Row 3
3/15/2006	6/16/2007	Alexander and Peterson (2008)	ES(P>MP)	Tab 4, Pan A, Col 7, Row 4
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Pr(P<MP)	Tab 4, Pan A, Col 7, Row 6
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Pr(P>MP)	Tab 4, Pan A, Col 7, Row 7
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Price Impact	Tab 5, Pan A, Col 7, Row 1
3/15/2006	6/16/2007	Alexander and Peterson (2008)	PI (P<MP)	Tab 5, Pan A, Col 7, Row 2
3/15/2006	6/16/2007	Alexander and Peterson (2008)	PI (P>MP)	Tab 5, Pan A, Col 7, Row 3
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Autocorr (5 min)	Tab 6, Pan A, Col 7, Row 1
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Autocorr (30 min)	Tab 6, Pan A, Col 7, Row 2
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Autocorr (60 min)	Tab 6, Pan A, Col 7, Row 3
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Up-Down R^2	Tab 6, Pan A, Col 7, Row 4
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Realized Variance	Tab 7, Pan A, Col 7, Row 1
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Semi-Variance	Tab 7, Pan A, Col 7, Row 2
3/15/2006	6/16/2007	Alexander and Peterson (2008)	MAD	Tab 7, Pan A, Col 7, Row 3
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Price Range	Tab 7, Pan A, Col 7, Row 4
3/15/2006	6/16/2007	Alexander and Peterson (2008)	Implied Volatility	Tab 7, Pan A, Col 7, Row 5
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Price (NYSE)	Tab 2, Pan A, Col 7, Row 1
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Number of Trades (NYSE)	Tab 2, Pan A, Col 7, Row 2
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Trade size (NYSE)	Tab 2, Pan A, Col 7, Row 3
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Volume (NYSE)	Tab 2, Pan A, Col 7, Row 4
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Short interest (NYSE)	Tab 3, Pan A, Col 7, Row 5
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Daily quote range	Tab 5, Pan A, Col 7, Row 1
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Volatility (close-close, TAQ, NYSE)	Tab 5, Pan A, Col 7, Row 2
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Volatility (open-close, TAQ, NYSE)	Tab 5, Pan A, Col 7, Row 3

7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Daily traded price range (NYSE)	Tab 5, Pan A, Col 7, Row 4
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Volatility (close-close, CRSP, NYSE)	Tab 5, Pan A, Col 7, Row 5
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Semivariance (Positive)	Tab 5, Pan A, Col 7, Row 6
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Semivariance (Negative)	Tab 5, Pan A, Col 7, Row 7
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Relative bid depth (NYSE)	Tab 6, Pan A, Col 7, Row 3
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Buy imbalance (NYSE)	Tab 6, Pan A, Col 7, Row 4
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Buy imbalance/volume (NYSE)	Tab 6, Pan A, Col 7, Row 5
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Quoted spread (NYSE, cents)	Tab 6, Pan B, Col 7, Row 1
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Quoted spread (NYSE, bps)	Tab 6, Pan B, Col 7, Row 2
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Effective Spread (NYSE, cents)	Tab 6, Pan B, Col 7, Row 3
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Effective Spread (NYSE, bps)	Tab 6, Pan B, Col 7, Row 4
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Realized Spread (NYSE, cents)	Tab 6, Pan B, Col 7, Row 5
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Realized Spread (NYSE, bps)	Tab 6, Pan B, Col 7, Row 6
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Trade-based Volatility (NYSE)	Tab 8, Pan A, Col 7, Row 1
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Trade-based Semivariance (Positive, NYSE)	Tab 8, Pan A, Col 7, Row 2
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Trade-based Semivariance (Negative, NYSE)	Tab 8, Pan A, Col 7, Row 3
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Midquote-Based Volatility (5-min, NYSE)	Tab 8, Pan A, Col 7, Row 4
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Midquote-Based Volatility (15-min, NYSE)	Tab 8, Pan A, Col 7, Row 5
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Midquote-Based Volatility (30-min, NYSE)	Tab 8, Pan A, Col 7, Row 6
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Variance Ratio (NYSE, Five minutes vs 15 minutes)	Tab 10, Pan A, Col 7, Row 1
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Variance Ratio (NYSE, Five minutes vs 30 minutes)	Tab 10, Pan A, Col 7, Row 2
7/22/2006	1/23/2009	Diether, Lee, and Werner (2009)	Variance Ratio (NYSE, Thirty minutes vs open-close)	Tab 10, Pan A, Col 7, Row 3
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11/16/2011	12/2/2015	Grullon, Michenaud, and Weston (2015)	Change in Total Assets	Tab 6, Pan A, Col 3
11/16/2011	12/2/2015	Grullon, Michenaud, and Weston (2015)	CAPX R&D	Tab 6, Pan A, Col 5
11/16/2011	12/2/2015	Grullon, Michenaud, and Weston (2015)	Equity Issues	Tab 6, Pan B, Col 1
11/16/2011	12/2/2015	Grullon, Michenaud, and Weston (2015)	Debt Issues	Tab 6, Pan B, Col 3
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11/4/2013	12/13/2018	Chang, Lin and Ma (2020)	AcqCAR(âĹŠ2, 2)	Tab 6, Col 1
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1/18/2014	9/16/2016	He and Tian (2016)	$\text{Value}_{(t+1)}$	Tab 3, Pan A, Col 4
1/18/2014	9/16/2016	He and Tian (2016)	$\text{Originality}_{(t+1)}$	Tab 3, Pan A, Col 6
1/18/2014	9/16/2016	He and Tian (2016)	$\text{Generality}_{(t+1)}$	Tab 3, Pan A, Col 8
1/18/2014	9/16/2016	He and Tian (2016)	$\text{Patent}_{(t+1)}$	Tab 3, Pan C, Col 2
1/18/2014	9/16/2016	He and Tian (2016)	$\text{LitigationRisk_SS}_{(t+1)}$	Tab 5, Col 2
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1/18/2014	9/16/2016	He and Tian (2016)	$\text{FracPress}_{(t+1)}$	Tab 8, Col 2
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3/19/2014	3/6/2018	Wang (2018)	Cash Ratio	Tab 4, Col 1

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8/15/2014	9/8/2019	Chen, Chen, and Chou (2015)	Put Open Interest	Tab 3, Pan B, Col 1
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11/12/2014	11/12/2014	Li and Zhang (2015)	MFSURP, Rogers and Van Buskirk News Measure	Tab 6, Col 2
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12/1/2015	5/21/2020	Chu, Hirshleifer, and Ma (2020)	Asset growth Long Leg Portfolio Return	Tab 4, Pan B, Row 3
12/1/2015	5/21/2020	Chu, Hirshleifer, and Ma (2020)	Investment to Assets Long Leg Portfolio Return	Tab 4, Pan B, Row 4
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12/1/2015	5/21/2020	Chu, Hirshleifer, and Ma (2020)	O-Score Long Leg Portfolio Return	Tab 4, Pan B, Row 11
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12/1/2015	5/21/2020	Chu, Hirshleifer, and Ma (2020)	Asset growth Short Leg Portfolio Return	Tab 4, Pan C, Row 3
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8/29/2016	12/28/2018	Gao, He, and Wu (2018)	EnvScore	Tab 8, Col 5
8/29/2016	12/28/2018	Gao, He, and Wu (2018)	ProScore	Tab 8, Col 6
9/1/2016	6/11/2020	Brockman, Luo, and Xu (2020)	Employee Treatment Index	Tab 4, Col 3
9/1/2016	6/11/2020	Brockman, Luo, and Xu (2020)	Employee Treatment Concern Index	Tab 4, Col 6
9/1/2016	6/11/2020	Brockman, Luo, and Xu (2020)	Employee Treatment Strength Index	Tab 4, Col 9
9/1/2016	6/11/2020	Brockman, Luo, and Xu (2020)	Union Relation Concern Index	Tab 5, Col 1
9/1/2016	6/11/2020	Brockman, Luo, and Xu (2020)	Health and Safety Concern Index	Tab 5, Col 2
9/1/2016	6/11/2020	Brockman, Luo, and Xu (2020)	Workforce Reduction Concern Index	Tab 5, Col 3
9/1/2016	6/11/2020	Brockman, Luo, and Xu (2020)	Retirement Benefit Concern Index	Tab 5, Col 4
9/1/2016	6/11/2020	Brockman, Luo, and Xu (2020)	Other Concern Index	Tab 5, Col 5
9/1/2016	6/11/2020	Brockman, Luo, and Xu (2020)	SG&A Expense Over Sales	Tab 10, Col 2
10/28/2016	12/4/2017	Chen, Zhu, and Chang (2017)	Div	Tab 4, Col 5
10/28/2016	12/4/2017	Chen, Zhu, and Chang (2017)	Repo	Tab 5, Col 5
10/28/2016	12/4/2017	Chen, Zhu, and Chang (2017)	Cash Holding	Tab 6, Pan A, Row 4
10/28/2016	12/4/2017	Chen, Zhu, and Chang (2017)	Total Payout	Tab 7, Col 5
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	LogDelta (Habib and Ljungqvist Expected Volatility)	Tab 2, Pan A1, Col 2
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	LogVega (Habib and Ljungqvist Expected Volatility)	Tab 2, Pan A1, Col 4
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	Vega/Delta (Habib and Ljungqvist Expected Volatility)	Tab 2, Pan A1, Col 6
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	LogDelta (36 months volatility in 2003)	Tab 2, Pan B1, Col 2
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	LogVega (36 months volatility in 2003)	Tab 2, Pan B1, Col 4
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	Vega/Delta (36 months volatility in 2003)	Tab 2, Pan B1, Col 6
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	OPT/EQ ($)	Tab 3, Col 3
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	OPT/EQ (#)	Tab 3, Col 4
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	LogTDC	Tab 4, Col 2
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	LogEQ	Tab 4, Col 4
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	LogOption ($)	Tab 5, Pan A, Col 2
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	LogStock ($)	Tab 5, Pan A, Col 4
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	LogOption (#)	Tab 5, Pan A, Col 6
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	LogStock (#)	Tab 5, Pan A, Col 8
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	LogBonus ($)	Tab 5, Pan A, Col 10
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	LogSalary ($)	Tab 5, Pan A, Col 12
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	Eindex	Tab 6, Col 2
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	1-R2	Tab 7, Col 1
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	PIN	Tab 7, Col 2
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	delay	Tab 7, Col 3
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	sigma1	Tab 7, Col 4
2/6/2017	2/6/2017	De Angelis, Grullon, and Michenaud (2017)	ar30	Tab 7, Col 5
2/27/2017	4/1/2019	Albertus, Bird, Karolyi, and Ruchti (2019)	SD(Change in Investment) (capital expenditures)	Tab 4, Col 1
2/27/2017	4/1/2019	Albertus, Bird, Karolyi, and Ruchti (2019)	SD(Change in Investment) (employee count)	Tab 4, Col 2
2/27/2017	4/1/2019	Albertus, Bird, Karolyi, and Ruchti (2019)	Investment	Tab 3, Col 1
2/27/2017	4/1/2019	Albertus, Bird, Karolyi, and Ruchti (2019)	Employment	Tab 3, Col 2
6/2/2017	10/21/2017	Choi (2018)	Rounding	Tab 2, Col 4
6/2/2017	10/21/2017	Choi (2018)	Cumulative Abnormal Returns	Tab 3, Col 3
6/19/2017	9/17/2020	Wu and Xu (2020)	Ownership Breadth Long Portfolio, Raw Returns	Tab 6, Pan A, Col 1
6/19/2017	9/17/2020	Wu and Xu (2020)	Ownership Breadth Short Portfolio, Raw Returns	Tab 6, Pan A, Col 2
6/19/2017	9/17/2020	Wu and Xu (2020)	Ownership Breadth Long-Short Portfolio, Raw Returns	Tab 6, Pan A, Col 3
6/19/2017	9/17/2020	Wu and Xu (2020)	Ownership Breadth Long-Short Portfolio, FF Risk-adjusted Return	Tab 6, Pan A, Col 4

6/19/2017	9/17/2020	Wu and Xu (2020)	Ownership Breadth Long-Short Portfolio, FF Risk-adjusted Return	Tab 6, Pan A, Col 5
6/19/2017	9/17/2020	Wu and Xu (2020)	Ownership Breadth Long-Short Portfolio, FF Risk-adjusted Return	Tab 6, Pan A, Col 6
7/14/2017	8/2/2020	Chang, Huang, Su, and Tseng (2018)	DISPERSION	Tab 3, Row 2
7/14/2017	8/2/2020	Chang, Huang, Su, and Tseng (2018)	TURNOVER (scaled by previous 12 month average)	Tab 3, Row 3
7/14/2017	8/2/2020	Chang, Huang, Su, and Tseng (2018)	SHORT RATIO	Tab 3, Row 1
7/14/2017	8/2/2020	Chang, Huang, Su, and Tseng (2018)	CEO Compensation Duration	Tab 4, Col 3
7/14/2017	8/2/2020	Chang, Huang, Su, and Tseng (2018)	Overinvest	Tab 10, Pan A, Col 2
7/14/2017	8/2/2020	Chang, Huang, Su, and Tseng (2018)	RD	Tab 10, Pan A, Col 4
7/14/2017	8/2/2020	Chang, Huang, Su, and Tseng (2018)	HOR	Tab A4, Pan A, Col 2
7/14/2017	8/2/2020	Chang, Huang, Su, and Tseng (2018)	Log(CPD Stock)	Tab A2, Col 2
7/14/2017	8/2/2020	Chang, Huang, Su, and Tseng (2018)	Log(CPD Option)	Tab A2, Col 4
7/14/2017	8/2/2020	Chang, Huang, Su, and Tseng (2018)	ARP	Tab A4, Pan A, Col 4
7/14/2017	8/2/2020	Chang, Huang, Su, and Tseng (2018)	EM	Tab A4, Pan A, Col 6
8/8/2017	6/7/2018	Sun and Xu (2018)	Log(File Size)	Tab 3, Col 3
8/8/2017	6/7/2018	Sun and Xu (2018)	Uncertain	Tab 8, Col 3
9/19/2017	9/19/2017	Francis, Samuel, Wu (2017)	P(Dividend)	Tab 2, Pan A, Col 4
9/19/2017	9/19/2017	Francis, Samuel, Wu (2017)	P(Initiate)	Tab 2,Pan B, Col 4
9/19/2017	9/19/2017	Francis, Samuel, Wu (2017)	P(Increase)	Tab 2,Pan C, Col 4
9/19/2017	9/19/2017	Francis, Samuel, Wu (2017)	P(Repurchases)	Tab 3,Pan A, Col 4
9/19/2017	9/19/2017	Francis, Samuel, Wu (2017)	P(TotalPayout)	Tab 3,Pan B, Col 4
9/19/2017	9/19/2017	Francis, Samuel, Wu (2017)	Dividends/Lag(Assets)	Tab 4,Pan A, Col 4
9/19/2017	9/19/2017	Francis, Samuel, Wu (2017)	Repurchases/Lag(Assets)	Tab 4,Pan B, Col 4
9/19/2017	9/19/2017	Francis, Samuel, Wu (2017)	TotalPayout/Lag(Assets)	Tab 4,Pan C, Col 4
10/30/2017	2/27/2018	Maffett and Owens (2018)	CDS_Spread	Tab 2, Pan A, Col 1
10/30/2017	2/27/2018	Maffett and Owens (2018)	CDS_Flatness	Tab 2, Pan B, Col 1
3/4/2018	3/28/2020	Leung, Evans, and Mazouz (2020)	R&D Anomaly Long Equal Weighted Portfolio Returns	Tab 9, Pan B, Col 1
3/4/2018	3/28/2020	Leung, Evans, and Mazouz (2020)	R&D Anomaly Short Equal Weighted Portfolio Returns	Tab 9, Pan B, Col 2
3/4/2018	3/28/2020	Leung, Evans, and Mazouz (2020)	R&D Anomaly Long-Short Equal Weighted Portfolio Returns	Tab 9, Pan B, Col 3
3/4/2018	3/28/2020	Leung, Evans, and Mazouz (2020)	R&D Anomaly Long Equal Weighted Portfolio C-4 Adjusted Returns	Tab 9, Pan B, Col 4
3/4/2018	3/28/2020	Leung, Evans, and Mazouz (2020)	R&D Anomaly Short Equal Weighted Portfolio C-4 Adjusted Returns	Tab 9, Pan B, Col 5
3/4/2018	3/28/2020	Leung, Evans, and Mazouz (2020)	R&D Anomaly Long-Short Equal Weighted C-4 Adjusted Returns	Tab 9, Pan B, Col 6
3/4/2018	3/28/2020	Leung, Evans, and Mazouz (2020)	R&D Anomaly Long Equal Weighted Portfolio FF-5 Adjusted Returns	Tab 9, Pan B, Col 7
3/4/2018	3/28/2020	Leung, Evans, and Mazouz (2020)	R&D Anomaly Short Equal Weighted Portfolio FF-5 Adjusted Returns	Tab 9, Pan B, Col 8
3/4/2018	3/28/2020	Leung, Evans, and Mazouz (2020)	R&D Anomaly Long-Short Equal Weighted FF-5 Adjusted Returns	Tab 9, Pan B, Col 9
3/9/2018	5/14/2018	Bennett and Wang (2018)	Forced	Tab 3, Col 3
3/28/2018	3/28/2018	Brogaard, Ngo and Xia (2018)	(-1)× Price Delay	Tab 9, Pan A, Col 1
3/28/2018	3/28/2018	Brogaard, Ngo and Xia (2018)	Price Nonsynchronicity	Tab 9, Pan A, Col 2
3/28/2018	3/28/2018	Brogaard, Ngo and Xia (2018)	$(-1)\times\lvert VarianceRatio - 1\rvert$	Tab 9, Pan A, Col 3
3/28/2018	3/28/2018	Brogaard, Ngo and Xia (2018)	Lead Ownership	Tab 9, Pan B, Col 1
3/28/2018	3/28/2018	Brogaard, Ngo and Xia (2018)	Ln(Num of Lender)	Tab 9, Pan B, Col 2
3/28/2018	3/28/2018	Brogaard, Ngo and Xia (2018)	Herfindhal	Tab 9, Pan B, Col 3
4/20/2018	12/19/2018	Babenko, Choi, and Sen (2018)	All Management Proposals	Tab 3, Col 3
4/20/2018	12/19/2018	Babenko, Choi, and Sen (2018)	Compensation Proposals	Tab 3, Col 5
4/20/2018	12/19/2018	Babenko, Choi, and Sen (2018)	Governance Proposals	Tab 3, Col 7
4/20/2018	12/19/2018	Babenko, Choi, and Sen (2018)	Share Issuance Proposals	Tab 3, Col 9

5/16/2018	5/16/2018	Ben-David, Li, and Wang (2018)	Selling, Gen & Admin / Total Assets	Tab 4, Col 1
5/16/2018	5/16/2018	Ben-David, Li, and Wang (2018)	Net Equity Financing / Total Assets	Tab 5, Col 1
5/16/2018	5/16/2018	Ben-David, Li, and Wang (2018)	Net Debt Financing / Total Assets	Tab 5, Col 3
5/16/2018	5/16/2018	Ben-David, Li, and Wang (2018)	% Sales / Total Industry Sales	Tab 7, Col 1
5/16/2018	5/16/2018	Ben-David, Li, and Wang (2018)	% Profits / Total Industry Sales	Tab 7, Col 3
5/16/2018	5/16/2018	Ben-David, Li, and Wang (2018)	ROA (Earnings /Total Assets)	Tab 8, Col 1
6/1/2018	6/1/2018	Wei and Zhang (2018)	UNDER	Tab 3, Col 4
6/1/2018	6/1/2018	Wei and Zhang (2018)	DCA	Tab 5, Pan A, Col 1
6/1/2018	6/1/2018	Wei and Zhang (2018)	CAPEX	Tab 5, Pan A, Col 2
6/1/2018	6/1/2018	Wei and Zhang (2018)	BOOKLEV	Tab 5, Pan A, Col 3
6/1/2018	6/1/2018	Wei and Zhang (2018)	P(D)frailty	Tab 5, Pan B, Col 1
6/1/2018	6/1/2018	Wei and Zhang (2018)	P(D)outofsample	Tab 5, Pan B, Col 2
6/1/2018	6/1/2018	Wei and Zhang (2018)	P(D)shumway	Tab 5, Pan B, Col 3
6/1/2018	6/1/2018	Wei and Zhang (2018)	Dummy(Issuance)	Tab 6, Pan A, Col 4
6/1/2018	6/1/2018	Wei and Zhang (2018)	Offersize	Tab 6, Pan B, Col 4
6/11/2018	2/13/2020	Bai, Lee, and Zhang (2020)	Next year's total case rate (TCR)	Tab 2, Col 4
6/29/2018	9/1/2019	Cai, Guo, and Sen (2019)	RM1	Tab 2, Col 1
6/29/2018	9/1/2019	Cai, Guo, and Sen (2019)	RM2	Tab 2, Col 2
6/29/2018	9/1/2019	Cai, Guo, and Sen (2019)	RM_PROD	Tab 2, Col 3
6/29/2018	9/1/2019	Cai, Guo, and Sen (2019)	RM_CFO	Tab 2, Col 4
6/29/2018	9/1/2019	Cai, Guo, and Sen (2019)	RM_DISX	Tab 2, Col 5
10/30/2018	3/16/2019	Lin, Liu, and Sun (2019)	WPS	Tab 1, Col 2
10/30/2018	3/16/2019	Lin, Liu, and Sun (2019)	tvol	Tab 4, Col 2
10/30/2018	3/16/2019	Lin, Liu, and Sun (2019)	ivol	Tab 4, Col 4
11/14/2018	2/23/2019	Hvidkjaer, Massa, and Rzeznik (2019)	Change in Fund's Co-Illiquidity	Tab 6, Pan D, Col 8, Row 1
11/14/2018	2/23/2019	Hvidkjaer, Massa, and Rzeznik (2019)	R^2 LIQ	Tab 8, Col 4
1/1/2019	1/1/2019	Chen and Zhang (2019)	DAYAHEAD	Tab 6, Pan B, Col 2
1/1/2019	1/1/2019	Chen and Zhang (2019)	WRATE	Tab 6, Pan B, Col 4
1/1/2019	1/1/2019	Chen and Zhang (2019)	Volatility of credit rating levels	Tab 8, Col 2
1/1/2019	1/1/2019	Chen and Zhang (2019)	PP_Rating	Tab 9, Pan B, Col 2
1/1/2019	1/1/2019	Gul, Li, and Muhammad (2019)	(r(coe) - rf)	Tab 5, Col 2
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Price Informativeness (Positive news)	Tab 3, Col 4
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Price Informativeness (Negative news)	Tab 3, Col 8
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Trading Speed, Buy Trades, Positive News	Tab 5, Pan A, Col 4
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Trading Speed, Buy Trades, Negative News	Tab 5, Pan A, Col 8
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Trading Speed, Sell Trades, Positive News	Tab 5, Pan B, Col 4
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Trading Speed, Sell Trades, Negative News	Tab 5, Pan B, Col 8
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Trading Speed, Buy Trades, Positive News, Hedge Funds	Tab 11, Col 4
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Trading Speed, Sell Trades, Negative News, Hedge Funds	Tab 11, Col 8
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	High Number of Brokers, Positive News	Tab 7, Col 4
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	High Number of Brokers, Negative News	Tab 7, Col 8
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	High Number of Brokers, Positive News, Sell Trades	Tab 14, Col 3
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	High Number of Brokers, Negative News, Sell Trades	Tab 14, Col 4
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	High Number of Brokers, Positive News, After the earnings announcement	Tab 12, Col 4
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	High Number of Brokers, Negative News, after the earnings announcement	Tab 12, Col 8
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Broker centrality, Positive News	Tab 8, Col 4
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Broker centrality, Negative News	Tab 8, Col 8

4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Broker centrality, Positive News, Sell Trades	Tab 14, Col 5
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Broker centrality, Negative News, Sell Trades	Tab 14, Col 6
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Broker familiarity, Positive News	Tab 9, Col 4
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Broker familiarity, Negative News	Tab 9, Col 8
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Broker familiarity, Positive News, Sell Trades	Tab 14, Col 7
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Broker familiarity, Negative News, Sell Trades	Tab 14, Col 8
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Log-dollar volume, Positive News	Tab 10, Col 4
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Log-dollar volume, Negative News	Tab 10, Col 8
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Log-dollar volume, Positive News, Sell trades	Tab 14, Col 1
4/5/2019	6/1/2020	Di Maggio, Franzoni, Massa and Tubaldi (2020)	Log-dollar volume, Negative News, Sell trades	Tab 14, Col 2
5/1/2019	5/1/2019	Jang and Lee (2018)	CEO Confidence	Tab 2, Col 5
6/4/2019	6/4/2019	Khurana, Pereira, Pukhuanthong, and Zhao (2019)	Audit Fees	Tab 3, Col 1
6/4/2019	6/4/2019	Khurana, Pereira, Pukhuanthong, and Zhao (2019)	Audit Report Delay	Tab 5, Col 1
6/13/2019	6/13/2019	Kum, Klassen, and Xing (2019)	REP	Tab 4, Col 2
6/13/2019	6/13/2019	Kum, Klassen, and Xing (2019)	REP%	Tab 5, Col 3
6/13/2019	6/13/2019	Kum, Klassen, and Xing (2019)	REP%_Alt	Tab 5, Col 4
10/10/2019	6/5/2020	Gong (2020)	leverage_book	Tab 2, Col 4
10/10/2019	6/5/2020	Gong (2020)	Loan Spread	Tab 4, Col 1
10/10/2019	6/5/2020	Gong (2020)	Leverage_market	Tab 7, Col 2
10/10/2019	6/5/2020	Gong (2020)	Net_Leverage_Book	Tab 7, Col 4
10/10/2019	6/5/2020	Gong (2020)	Net_Leverage_Market	Tab 7, Col 6
10/10/2019	6/5/2020	Gong (2020)	Log_debt	Tab 8, Col 1
10/10/2019	6/5/2020	Gong (2020)	Log_equity	Tab 8, Col 2
10/10/2019	6/5/2020	Gong (2020)	Debt_Issue	Tab 8, Col 3
10/10/2019	6/5/2020	Gong (2020)	Equity_Issue	Tab 8, Col 4
10/10/2019	6/5/2020	Gong (2020)	RE	Tab 8, Col 5
11/7/2019	11/7/2019	Jia, Gao, and Julian (2019)	CSR_Str	Tab 2, Pan B, Col 3
11/7/2019	11/7/2019	Jia, Gao, and Julian (2019)	CSR_Con	Tab 2, Pan B, Col 4
11/7/2019	11/7/2019	Jia, Gao, and Julian (2019)	Direct_CSR	Tab 2, Pan B, Col 5
11/7/2019	11/7/2019	Jia, Gao, and Julian (2019)	Broad_CSR	Tab 2, Pan B, Col 6
11/27/2019	11/27/2019	Kubick, Omer, and Song (2019)	FOG	Tab 4, Col 2
11/27/2019	11/27/2019	Kubick, Omer, and Song (2019)	FKGL	Tab 4, Col 4
11/27/2019	11/27/2019	Kubick, Omer, and Song (2019)	LENGTH	Tab 4, Col 6
4/7/2020	10/29/2020	Kim, Lu, and Peng (2020)	BTD	Tab 4, Col 3
4/7/2020	10/29/2020	Kim, Lu, and Peng (2020)	NETR3	Tab 4, Col 4
4/7/2020	10/29/2020	Kim, Lu, and Peng (2020)	Taxaggres	Tab 5, Col 1
4/7/2020	10/29/2020	Kim, Lu, and Peng (2020)	Detect_Taxaggres	Tab 5, Col 2
4/7/2020	10/29/2020	Kim, Lu, and Peng (2020)	Conform TA	Tab 8, Col 2
11/1/2020	11/1/2020	Maharjan, Omer, and Zhao (2020)	Shelter	Tab 3, Col 2
11/1/2020	11/1/2020	Maharjan, Omer, and Zhao (2020)	DTAX	Tab 3, Col 4
11/1/2020	11/1/2020	Maharjan, Omer, and Zhao (2020)	PERMDIFF	Tab 3, Col 6
11/1/2020	11/1/2020	Maharjan, Omer, and Zhao (2020)	Adjusted Shelter	Tab 13, Pan A, Col 1
11/1/2020	11/1/2020	Maharjan, Omer, and Zhao (2020)	Adjusted DTAX	Tab 13, Pan A, Col 2
11/1/2020	11/1/2020	Maharjan, Omer, and Zhao (2020)	Adjusted PERMDIFF	Tab 13, Pan A, Col 3
11/1/2020	11/1/2020	Maharjan, Omer, and Zhao (2020)	Predicted UTB	Tab 13, Pan B, Col 1
11/1/2020	11/1/2020	Maharjan, Omer, and Zhao (2020)	Havendum	Tab 13, Pan B, Col 2
11/1/2020	11/1/2020	Maharjan, Omer, and Zhao (2020)	Rate_thctry	Tab 13, Pan B, Col 3
1/15/2021	3/31/2021	Su, Wong, Xue, and Zhao (2021)	SV	Tab 3, Col 1
1/15/2021	3/31/2021	Su, Wong, Xue, and Zhao (2021)	10-K	Tab 3, Col 2
1/15/2021	3/31/2021	Su, Wong, Xue, and Zhao (2021)	10-Q	Tab 3, Col 3
1/15/2021	3/31/2021	Su, Wong, Xue, and Zhao (2021)	8-K	Tab 3, Col 4
1/15/2021	3/31/2021	Su, Wong, Xue, and Zhao (2021)	Insider Search Volume	Tab 3, Col 5
1/15/2021	3/31/2021	Su, Wong, Xue, and Zhao (2021)	Other types	Tab 3, Col 6
1/15/2021	3/31/2021	Su, Wong, Xue, and Zhao (2021)	CQA	Tab 3, Col 7
1/15/2021	4/1/2021	Su, Wong, Xue, and Zhao (2021)	CQAN	Tab 3, Col 8

Table A.II
Definitions of Outcome Variables for Semi-Parametric Bootstrap

This table lists CRSP/Compustat variables used to capture correlations in the semi-parametric bootstrap. We examine the raw version of each variable, as well as two possible transformations of each variable, resulting in 293 different outcome variables. *Outcome* is the name of the outcome variable. *Description* provides the description, or details the construction of the outcome variable. *Source* provides the source of the outcome variable. All outcomes are transformed into annual frequency using firms' fiscal years. In order for a Compustat variable to be included, we require that financial statement variables be non-missing for at least 70% of observations in a sample from January 1970 through June 2019. Variables for which a treatment effect could not be estimated are removed. For Compustat outcomes, we use the raw variable, raw variable scaled by total assets, and the percentage change of the raw variable scaled by total assets. Financial firms, utilities and observations with missing/negative sales or total assets are excluded. To mitigate the impact of data errors, all variables are winsorized at the 2.5% and 97.5% levels.

Outcome	Description	Source
ACO	Current Assets - Other - Total	Compustat Annual Fundamentals
ACOX	Current Assets - Other - Sundry	Compustat Annual Fundamentals
AO	Assets - Other	Compustat Annual Fundamentals
AOX	Assets - Other- Sundry	Compustat Annual Fundamentals
AP	Accounts Payable - Trade	Compustat Annual Fundamentals
AQC	Acquisitions	Compustat Annual Fundamentals
BKVLPS	Book Value Per Share	Compustat Annual Fundamentals
CAPS	Capital Surplus/Share Premium Reserve	Compustat Annual Fundamentals
CAPX	Capital Expenditures	Compustat Annual Fundamentals
CAPXV	Capital Expend Property, Plant, and Equipment Schd V	Compustat Annual Fundamentals
CEQ	Common/Ordinary Equity - Total	Compustat Annual Fundamentals
CEQL	Common Equity - Liquidation Value	Compustat Annual Fundamentals
CEQT	Common Equity - Tangible	Compustat Annual Fundamentals
CH	Cash	Compustat Annual Fundamentals
CHE	Cash and Short-Term Investments	Compustat Annual Fundamentals
COGS	Cost of Goods Sold	Compustat Annual Fundamentals
CSHO	Common Shares Outstanding	Compustat Annual Fundamentals
CSHPRI	Common Shares Used to Calculate Earnings Per Share - Basic	Compustat Annual Fundamentals
CSTK	Common/Ordinary Stock (Capital)	Compustat Annual Fundamentals
DCLO	Debt - Capitalized Lease Obligations	Compustat Annual Fundamentals
DCPSTK	Convertible Debt and Preferred Stock	Compustat Annual Fundamentals
DCVT	Debt - Convertible	Compustat Annual Fundamentals
DD1	Long-Term Debt Due in One Year	Compustat Annual Fundamentals
DLC	Debt in Current Liabilities - Total	Compustat Annual Fundamentals
DLTIS	Long-Term Debt - Issuance	Compustat Annual Fundamentals
DLTO	Other Long-term Debt	Compustat Annual Fundamentals
DLTR	Long-Term Debt - Reduction	Compustat Annual Fundamentals
DLTT	Long-Term Debt - Total	Compustat Annual Fundamentals
DP	Depreciation and Amortization	Compustat Annual Fundamentals
DPACT	Depreciation, Depletion, and Amortization (Accumulated)	Compustat Annual Fundamentals
DPC	Depreciation and Amortization (Cash Flow)	Compustat Annual Fundamentals
DV	Cash Dividends (Cash Flow)	Compustat Annual Fundamentals
DVC	Dividends Common/Ordinary	Compustat Annual Fundamentals
DVP	Dividends - Preferred/Preference	Compustat Annual Fundamentals
DVPSP_C	Dividends per Share - Pay Date - Calendar	Compustat Annual Fundamentals
DVPSP_F	Dividends per Share - Pay Date - Fiscal	Compustat Annual Fundamentals
DVPSX_C	Dividends per Share - Ex-Date - Calendar	Compustat Annual Fundamentals
DVPSX_F	Dividends per Share - Ex-Date - Fiscal	Compustat Annual Fundamentals
DVT	Dividends - Total	Compustat Annual Fundamentals
EBIT	Earnings Before Interest and Taxes	Compustat Annual Fundamentals
EBITDA	Earnings Before Interest	Compustat Annual Fundamentals
EMP	Employees	Compustat Annual Fundamentals
EPSFI	Earnings Per Share (Diluted) - Including Extraordinary Items	Compustat Annual Fundamentals
EPSFX	Earnings Per Share (Diluted) - Excluding Extraordinary Items	Compustat Annual Fundamentals
EPSPI	Earnings Per Share (Basic) - Including Extraordinary Items	Compustat Annual Fundamentals
EPSPX	Earnings Per Share (Basic) - Excluding Extraordinary Items	Compustat Annual Fundamentals
FOPO	Funds from Operations - Other	Compustat Annual Fundamentals

GP	Gross Profit (Loss)	Compustat Annual Fundamentals
IB	Income Before Extraordinary Items	Compustat Annual Fundamentals
IBADJ	Income Before Extraordinary Items - Adjusted for Common Stock Equivalents	Compustat Annual Fundamentals
IBC	Income Before Extraordinary Items (Cash Flow)	Compustat Annual Fundamentals
IBCOM	Income Before Extraordinary Items - Available for Common	Compustat Annual Fundamentals
ICAPT	Invested Capital - Total	Compustat Annual Fundamentals
INTAN	Intangible Assets - Total	Compustat Annual Fundamentals
INVT	Inventories - Total	Compustat Annual Fundamentals
IVAEQ	Investment and Advances - Equity	Compustat Annual Fundamentals
IVAO	Investment and Advances - Other	Compustat Annual Fundamentals
IVST	Short-Term Investments - Total	Compustat Annual Fundamentals
LCO	Current Liabilities - Other - Total	Compustat Annual Fundamentals
LCOX	Current Liabilities - Other - Sundry	Compustat Annual Fundamentals
LO	Liabilities - Other - Total	Compustat Annual Fundamentals
LT	Liabilities - Total	Compustat Annual Fundamentals
NI	Net Income (Loss)	Compustat Annual Fundamentals
NIADJ	Net Income Adjusted for Common/Ordinary Stock (Capital) Equivalents	Compustat Annual Fundamentals
NOPI	Nonoperating Income (Expense)	Compustat Annual Fundamentals
NOPIO	Nonoperating Income (Expense) - Other	Compustat Annual Fundamentals
NP	Notes Payable - Short-Term Borrowings	Compustat Annual Fundamentals
OIADP	Operating Income After Depreciation	Compustat Annual Fundamentals
OIBDP	Operating Income Before Depreciation	Compustat Annual Fundamentals
PI	Pretax Income	Compustat Annual Fundamentals
PPEGT	Property, Plant, and Equipment - Buildings (Net)	Compustat Annual Fundamentals
PPENT	Property, Plant, and Equipment - Total (Net)	Compustat Annual Fundamentals
PRSTKC	Purchase of Common and Preferred Stock	Compustat Annual Fundamentals
PSTK	Preferred/Preference Stock (Capital) - Total	Compustat Annual Fundamentals
PSTKL	Preferred Stock - Liquidating Value	Compustat Annual Fundamentals
PSTKN	Preferred/Preference Stock - Nonredeemable	Compustat Annual Fundamentals
PSTKRV	Preferred Stock - Redemption Value	Compustat Annual Fundamentals
RE	Retained Earnings	Compustat Annual Fundamentals
RECCO	Receivables - Current - Other	Compustat Annual Fundamentals
RECT	Receivables - Total	Compustat Annual Fundamentals
RETURN	Annual Cumulative Return	CRSP Monthly Stock File
REVT	Revenue - Total	Compustat Annual Fundamentals
SALE	Sales/Turnover (Net)	Compustat Annual Fundamentals
SEQ	Stockholders Equity - Parent	Compustat Annual Fundamentals
SPI	Special Items	Compustat Annual Fundamentals
SPREAD	Firm-Year Average Spread Between Bid and Ask	CRSP Monthly Stock File
SPREAD_PERC	Firm-Year Average Percentage Spread Between Bid and Ask	CRSP Monthly Stock File
SSTK	Sale of Common and Preferred Stock	Compustat Annual Fundamentals
TSTK	Treasury Stock - Total (All Capital)	Compustat Annual Fundamentals
TSTKC	Treasury Stock - Common	Compustat Annual Fundamentals
TURNOVER	Firm-Year Average Volume / Shares Outstanding	CRSP Monthly Stock File
TXDB	Deferred Taxes (Balance Sheet)	Compustat Annual Fundamentals
TXDC	Deferred Taxes (Cash Flow)	Compustat Annual Fundamentals
TXDITC	Deferred Taxes and Investment Tax Credit	Compustat Annual Fundamentals
TXP	Income Taxes Payable	Compustat Annual Fundamentals
TXT	Income Taxes - Total	Compustat Annual Fundamentals
VOL	Firm-Year Average Trading Volume	CRSP Monthly Stock File
XIDO	Extraordinary Items and Discontinued Operations	Compustat Annual Fundamentals
XIDOC	Extraordinary Items and Discontinued Operations (Cash Flow)	Compustat Annual Fundamentals
XINT	Interest and Related Expense - Total	Compustat Annual Fundamentals
XOPR	Operating Expenses - Total	Compustat Annual Fundamentals