...and the Cross-Section of Expected Returns

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Abstract

Hundreds of papers and hundreds of factors attempt to explain the cross-section of expected returns. Given this extensive data mining, it does not make sense to use the usual criteria for establishing significance. What hurdle should be used for current research? Our paper introduces a new multiple testing framework and provides historical cutoffs from the first empirical tests in 1967 to today. A new factor needs to clear a much higher hurdle, with a t-statistic greater than 3.0. We argue that most claimed research findings in financial economics are likely false.

Keywords: Risk factors, Multiple tests, Beta, HML, SMB, 3-factor model, Momentum, Volatility, Skewness, Idiosyncratic volatility, Liquidity, Bonferroni, Factor zoo.

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* Current Version: April 20, 2015. First posted to SSRN: April 11, 2013. Send correspondence to: Campbell R. Harvey, Fuqua School of Business, Duke University, Durham, NC 27708. Phone: +1 919.660.7768, E-mail: cam.harvey@duke.edu . We would like to thank the Editor (Andrew Karolyi) and three anonymous referees for their detailed and thoughtful comments. We would also like to thank Viral Acharya, Jawad Addoum, Tobias Adrian, Andrew Ang, Ravi Bansal, Mehmet Beceren, Itzhak Ben-David, Bernard Black, Jules van Binsbergen, Oliver Boguth, Tim Bollerslev, Alon Brav, Ian Dew-Becker, Robert Dittmar, Jennifer Conrad, Michael Cooper, Andres Donangelo, Gene Fama, Wayne Ferson, Ken French, Simon Gervais, Bing Han, John Hand, Abby Yeon Kyeong Kim, Lars-Alexander Kuehn, Sophia Li, Harry Markowitz, Kyle Matoba, David McLean, Marcelo Ochoa, Peter Park, Lubos Pastor, Andrew Patton, Lasse Pedersen, Tapio Pekkala, Jeff Pontiff, Ryan Pratt, Tarun Ramadorai, Alexandru Rosoiu, Tim Simin, Avanidhar Subrahmanyam, Ivo Welch, Basil Williams, Yuhang Xing, Josef Zechner and Xiaofei Zhao as well as seminar participants at the 2014 New Frontiers in Finance Conference at Vanderbilt University, the 2014 Inquire Europe-UK meeting in Vienna, the 2014 WFA meetings, and seminars at Duke University, Texas A&M University, Baylor University, University of Utah, and Penn State University.
Note: Our data are available for download and resorting. The main table includes full citations as well as hyperlinks to each of the cited articles.
See http://faculty.fuqua.duke.edu/~charvey/Factor-List.xlsx.

1 Introduction

Over forty years ago, one of the first tests of the Capital Asset Pricing Model (CAPM) found that the market beta was a significant explanator of the cross-section of expected returns. The reported t-statistic of 2.57 in Fama and MacBeth (1973, Table III) comfortably exceeded the usual cutoff of 2.0. However, since that time, hundreds of papers have tried to explain the cross-section of expected returns. Given the known number of factors that have been tried and the reasonable assumption that many more factors have been tried but did not make it to publication, the usual cutoff levels for statistical significance may not be appropriate. We present a new framework that allows for multiple tests and derive recommended statistical significance levels for current research in asset pricing.

We begin with 313 papers that study cross-sectional return patterns published in a selection of journals. We provide recommended test thresholds from the first empirical tests in 1967 through to present day. We also project minimum t-statistics through 2032 assuming the rate of "factor production" remains the same as the last ten years. We present a taxonomy of historical factors as well as definitions.[1]

Our research is related to a recent paper by McLean and Pontiff (2014) who argue that certain stock market anomalies are less anomalous after being published.[2] Their paper tests the statistical biases emphasized in Leamer (1978), Ross (1989), Lo and MacKinlay (1990), Fama (1991) and Schwert (2003).

Our paper also adds to the recent literature on biases and inefficiencies in cross-sectional regression studies. Lewellen, Nagel and Shanken (2010) critique the usual practice of using cross-sectional R^2s and pricing errors to judge success and show that the explanatory power of many previously documented factors are spurious. Our work focuses on evaluating the statistical significance of a factor given the previous tests on other factors. Our goal is to use a multiple testing framework to both re-evaluate past research and to provide a new benchmark for current and future research.

We tackle multiple hypothesis testing from the frequentist perspective. Bayesian approaches to multiple testing and variable selection also exist.[3] However, the high dimensionality of the problem combined with the fact that we do not observe all the factors that have been tried poses a big challenge for Bayesian methods. While we propose a frequentist approach to overcome this missing data issue, it is unclear how

[1]We also provide a link to a file with full references and hyperlinks to the original articles: *http://faculty.fuqua.duke.edu/~charvey/Factor-List.xlsx.*

[2]Other recent papers that systematically study the cross-sectional return patterns include Subrahmanyam (2010), Green, Hand and Zhang (2012, 2013). Other papers that study anomaly discoveries and investor actions include Edelen, Ince and Kadlec (2014) and Liu et al. (2014).

[3]See Jefferys and Berger (1992), Scott and Berger (2006) and Scott (2009).

to do this in the Bayesian framework. Nonetheless, we provide a detailed discussion of Bayesian methods in the paper.

Multiple testing has only recently gained traction in the finance literature. For the literature on multiple testing corrections for data snooping biases, see Sullivan, Timmermann and White (1999, 2001) and White (2000). For research on data snooping and variable selection in predictive regressions, see Foster, Smith and Whaley (1997), Cooper and Gulen (2006) and Lynch and Vital-Ahuja (2012). For applications of multiple testing approach in the finance literature, see, for example, Shanken (1990), Ferson and Harvey (1999), Boudoukh et al. (2007) and Patton and Timmermann (2010). More recently, a multiple testing connection has been used to study technical trading and mutual fund performance, see for example Barras, Scaillet and Wermers (2010), Bajgrowicz and Scaillet (2012) and Kosowski, Timmermann, White and Wermers (2006). Conrad, Cooper and Kaul (2003) point out that data snooping accounts for a large proportion of the return differential between equity portfolios that are sorted by firm characteristics. Bajgrowicz, Scaillet and Treccani (2013) show that multiple testing methods help eliminate a large proportion of spurious jumps detected using conventional test statistics for high-frequency data. Holland, Basu and Sun (2010) emphasize the importance of multiple testing in accounting research. Our paper is consistent with the theme of this literature.

There are limitations to our framework. First, should all factor discoveries be treated equally? We think no. A factor derived from a theory should have a lower hurdle than a factor discovered from a purely empirical exercise. Economic theories are based on a few economic principles and, as a result, there is less room for data mining. Nevertheless, whether suggested by theory or empirical work, a t-statistic of 2.0 is too low. Second, our tests focus on unconditional tests. While the unconditional test might consider the factor marginal, it is possible that this factor is very important in certain economic environments and not important in other environments. These two caveats need to be taken into account when using our recommended significance levels for current asset pricing research.

While our focus is on the cross-section of equity returns, our message applies to many different areas of finance. For instance, Frank and Goyal (2009) investigate around 30 variables that have been documented to explain capital structure decisions of public firms. Welch and Goyal (2008) examine the performance of a dozen variables that have been shown to predict market excess returns. Novy-Marx (2014) proposes unconventional variables to predict anomaly returns. These three applications are ideal settings to employ multiple testing methods.

2 The Search Process

Our goal is not to catalogue every asset pricing paper ever published. We narrow the focus to papers that propose and test new factors. For example, Sharpe (1964), Lintner (1965) and Mossin (1966) all theoretically proposed (at roughly the same time), a single factor model — the Capital Asset Pricing Model (CAPM). Following Fama and MacBeth (1973), there are hundreds of papers that test the CAPM. We include the theoretical papers as well as the first paper to provide test statistics. We do not include the hundreds of papers that test the CAPM in different contexts, e.g., various international markets, different time periods. We do, however, include papers, such as Kraus and Litzenberger (1976) which test the market factor as well as one additional risk factor linked to the market factor.

Sometimes different papers propose different empirical proxies for the same type of economic risk. Although they may look similar from a theoretical standpoint, we still include them. An example is the empirical proxies for idiosyncratic financial constraints risk. While Lamont, Polk and Saa-Requejo (2001) use the Kaplan and Zingales (1997) index to proxy for firm-level financial constraints, Whited and Wu (2006) estimate their own constraint index based on the first-order conditions of firms' optimization problem. We include both even though they are likely highly correlated.

Since our focus is on factors that can broadly explain return patterns, we omit papers that focus on a small group of stocks or for a short period of time. This will, for example, exclude a substantial amount of empirical corporate finance research that studies event-driven return movements.[4]

Certain theoretical models lack immediate empirical content. Although they could be empirically relevant once suitable proxies are constructed, we choose to exclude them.

With these rules in mind, we narrow our search to generally the top journals in finance, economics and accounting. To include the most recent research, we search for working papers on the Social Science Research Network (SSRN). Working papers pose a challenge because there are thousands of them and they have not been subjected to peer review. We choose a subset of papers that we suspect are in review at top journals or have been presented at top conferences or are due to be presented at top conferences. We end up using 63 working papers. In total, we focus on 313 articles, among which are 250 published articles. We catalogue 316 different factors.[5]

Our collection of 316 factors likely under-represents the factor population. First, we generally only consider top journals. Second, we are selective in choosing only a

[4]See Kothari and Warner (2006) for a survey on event studies. More specifically, three criteria help differentiate our risk factors from event signals in corporate finance. First, while we are generally considering returns realized at the monthly or lower frequency intervals for risk factors, it is routine for event studies to consider daily or even higher frequency returns. Second, portfolio sorts based on risk factors typically cover the entire cross-section of stocks whereas event studies usually focus on a much smaller group of securities that are affected by the event signal. Finally, portfolio sorts based on risk factors are usually repeated at a fixed time interval whereas events may happen sporadically.

[5]As already mentioned, some of these factors are highly correlated. For example, we include four versions of idiosyncratic volatility, i.e., Fama and MacBeth (1973), Ali, Hwang and Trombley (2003), Ang, Hodrick, Xing and Zhang (2006), and Fu (2009).

handful of working papers. Third, sometimes there are many variants of the same characteristic and we usually only include the most representative ones. Fourth, and perhaps most importantly, we should be measuring the number of factors tested (which is unobservable) — that is, we do not observe the factors that were tested but failed to pass the usual significance levels and were never published (see Fama, 1991). Our multiple testing framework tries to account for this possibility.

3 Factor Taxonomy

To facilitate our analysis, we group the factors into different categories. We start with two broad categories: "common" and individual firm "characteristics". "Common" means the factor can be viewed as a proxy for a common source of risk. Risk exposure to this factor or its innovations is supposed to help explain cross-sectional return patterns. "Characteristics" means the factor is specific to the security or portfolio. A good example is Fama and MacBeth (1973). While the beta against the market return is systematic (exposure to a common risk factor), the standard deviation of the market model residual is not based on a common factor — it is a property of the individual firm, i.e., it is an idiosyncratic characteristic.

Strictly speaking, a risk factor should be a variable that has unpredictable variations through time. Moreover, assets' risk exposures to this factor need to be able to explain the cross-sectional return patterns. Based on these criteria, individual firm characteristics should not qualify as risk factors because characteristics are pre-known and have limited time-series variation. However, we interpret firm characteristics in a broader sense. If a certain firm characteristic is found to be correlated with the cross-section of expected returns, a long-short portfolio can usually be constructed to proxy for the underlying unknown risk factor. It is this unknown risk factor that we have in mind when we classify particular firm characteristics as risk factors.

Based on the unique properties of the proposed factors, we further divide the "common" and "characteristics" groups into finer categories. In particular, we divide "common" into: "financial", "macro", "microstructure", "behavioral", "accounting" and "other". We divide "characteristics" into the same categories — except we omit the "macro" classification, which is common, by definition. The following table provides further details on the definitions of these sub-categories and gives examples for each.

Table 1: **Factor Classification**

Risk type		Description	Examples
Common (113)	**Financial** (46)	Proxy for aggregate financial market movement, including market portfolio returns, volatility, squared market returns, etc.	Sharpe (1964): market returns; Kraus and Litzenberger (1976): squared market returns
	Macro (40)	Proxy for movement in macroeconomic fundamentals, including consumption, investment, inflation, etc.	Breeden (1979): consumption growth; Cochrane (1991): investment returns
	Microstructure (11)	Proxy for aggregate movements in market microstructure or financial market frictions, including liquidity, transaction costs, etc.	Pastor and Stambaugh (2003): market liquidity; Lo and Wang (2006): market trading volume
	Behavioral (3)	Proxy for aggregate movements in investor behavior, sentiment or behavior-driven systematic mispricing	Baker and Wurgler (2006): investor sentiment; Hirshleifer and Jiang (2010): market mispricing
	Accounting (8)	Proxy for aggregate movement in firm-level accounting variables, including payout yield, cash flow, etc.	Fama and French (1992): size and book-to-market; Da and Warachka (2009): cash flow
	Other (5)	Proxy for aggregate movements that do not fall into the above categories, including momentum, investors' beliefs, etc.	Carhart (1997): return momentum; Ozoguz (2008): investors' beliefs
Characteristics (202)	**Financial** (61)	Proxy for firm-level idiosyncratic financial risks, including volatility, extreme returns, etc.	Ang, Hodrick, Xing and Zhang (2006): idiosyncratic volatility; Bali, Cakici and Whitelaw (2011): extreme stock returns
	Microstructure (28)	Proxy for firm-level financial market frictions, including short sale restrictions, transaction costs, etc.	Jarrow (1980): short sale restrictions; Mayshar (1981): transaction costs
	Behavioral (3)	Proxy for firm-level behavioral biases, including analyst dispersion, media coverage, etc.	Diether, Malloy and Scherbina (2002): analyst dispersion; Fang and Peress (2009): media coverage
	Accounting (87)	Proxy for firm-level accounting variables, including PE ratio, debt to equity ratio, etc.	Basu (1977): PE ratio; Bhandari (1988): debt to equity ratio
	Other (24)	Proxy for firm-level variables that do not fall into the above categories, including political campaign contributions, ranking-related firm intangibles, etc.	Cooper, Gulen and Ovtchinnikov (2010): political campaign contributions; Edmans (2011): intangibles

Numbers in parentheses represent the number of factors identified. See Table 6 for details.

4 Adjusted T-statistics in Multiple Testing

4.1 Why Multiple Testing?

Given so many papers have attempted to explain the same cross-section of expected returns, statistical inference should not be based on a "single" test perspective. Our goal is to provide guidance as to the appropriate significance level using a multiple testing framework. When just one hypothesis is tested, we use the term "individual test", "single test" and "independent test" interchangeably.[6]

Strictly speaking, different papers study different sample periods and hence focus on different cross-sections of expected returns. However, the bulk of the papers we consider have substantial overlapping sample periods. Also, if one believes that cross-sectional return patterns are stationary, then these papers are studying roughly the same cross-section of expected returns.

We want to emphasize that there are many forces that make our guidance lenient, that is, a credible case can be made for an even higher threshold for discovery. We have already mentioned that we only sample a subset of research papers and the "publication bias/hidden tests" issue (i.e. it is difficult to publish a non-result).[7] However, there is another publication bias that is more subtle. In many scientific fields, replication studies routinely appear in top journals. That is, a factor is discovered, and others try to replicate it. In finance and economics, it is very difficult to publish replication studies. Hence, there is a bias towards publishing "new" factors rather than rigorously verifying the existence of discovered factors.

There are two ways to deal with the bias introduced by multiple testing: out-of-sample validation and using a statistical framework that allows for multiple testing.[8] When feasible, out-of-sample testing is the cleanest way to rule out spurious factors. In their study of anomalies, McLean and Pontiff (2014) take the out-of-sample approach. Their results show a degradation of performance of identified anomalies after publication which is consistent with the statistical bias. It is possible that this degradation is larger than they document. In particular, they drop 12 of their 97 anomalies because they could not replicate the in-sample performance of published studies. Given these non-replicable anomalies were not even able to survive routine data revisions, they are likely to be insignificant strategies, either in-sample or out-of-sample. The degradation from the original published "alpha" is 100% for these strategies — which would lead to a higher average rate of degradation for their strategies.

[6]The last term should not be confused with any sort of stochastic independence.

[7]See Rosenthal (1979) for one of the earliest and most influential works on publication bias.

[8]Another approach to test factor robustness is to look across multiple asset classes. This approach has been followed in several recent papers, e.g., Frazzini and Pedersen (2012) and Koijen, Moskowitz, Pedersen and Vrugt (2012).

While the out-of-sample approach has many strengths, it has one important drawback: it cannot be used in real time. To make real time assessment in the out-of-sample approach, it is common to hold out some data. However, this is not genuine out-of-sample testing as all the data are observable to researchers. A real out-of-sample test requires data in the future. In contrast to many tests in the physical sciences (where new data can be created for an experiment), we often need years of data to do an out-of-sample test. We pursue the multiple testing framework because it yields immediate guidance on whether a discovered factor is real.

4.2 A Multiple Testing Framework

In statistics, multiple testing refers to simultaneous testing of more than one hypothesis. The statistics literature was aware of this multiplicity problem at least 60 years ago.[9] Early generations of multiple testing procedures focus on the control of the family-wise error rate (see Section 4.3.1). More recently, increasing interest in multiple testing from the medical literature has spurred the development of methods that control the false-discovery rate (see Section 4.3.2). Multiple testing is an active research area in both the statistics and the medical literature.[10]

Despite the rapid development of multiple testing methods, they have not attracted much attention in the finance literature. Moreover, most of the research that does involve multiple testing focuses on the Bonferroni adjustment,[11] which is known to be too stringent. Our paper aims to fill this gap.

First, we introduce a hypothetical example to motivate a more general framework. In Table 2, we categorize the possible outcomes of a multiple testing exercise. Panel A displays an example of what the literature could have discovered and Panel B notationalizes Panel A to ease our subsequent definition of the general Type I error rate — the chance of making at least one false discovery or the expected fraction of false discoveries.

Our example in Panel A assumes 100 published factors (denoted as R). Among these factors, suppose 50 are false discoveries and the rest are real ones. In addition, researchers have tried 600 other factors but none of them were found to be significant. Among them, 500 are truly insignificant but the other 100 are true factors. The total number of tests (M) is 700. Two types of mistakes are made in this process: 50 factors are falsely discovered to be true (Type I error or false positive) while 100 true factors are buried in unpublished work (Type II error or false negative). Usual statistical control in a multiple testing context aims at reducing "50" or "50/100",

[9]For early research on multiple testing, see Tukey (1951, 1953) for Tukey's range test and Scheffé (1959) for Scheffé's method on adjusting significance levels in a multiple regression context.

[10]See Shaffer (1995) for a review of multiple testing procedures that control for the *family-wise error rate*. See Farcomeni (2008) for a review that focuses on procedures that control the *false-discovery rate*.

[11]See Shanken (1990), Ferson and Harvey (1999), Boudoukh et al. (2007).

Table 2: **Contingency Table in Testing M Hypotheses.**

Panel A shows a hypothetical example for factor testing. Panel B presents the corresponding notation in a standard multiple testing framework.

Panel A: An Example

	Unpublished	Published	Total
Truly insignificant	500	50	550
Truly significant	100	50	150
Total	600	100(R)	700(M)

Panel B: The Testing Framework

	H_0 not rejected	H_0 rejected	Total
H_0 True	$N_{0\|a}$	$N_{0\|r}$	M_0
H_0 False	$N_{1\|a}$	$N_{1\|r}$	M_1
Total	$M - R$	R	M

the absolute or proportionate occurrence of false discoveries, respectively. Of course, we only observe published factors because factors that are tried and found to be insignificant rarely make it to publication.[12] This poses a challenge since the usual statistical techniques only handle the case where all test results are observable.

Panel B defines the corresponding terms in a formal statistical testing framework. In a factor testing exercise, the typical null hypothesis is that a factor is not significant. Therefore, a factor is insignificant means the null hypothesis is "true". Using "0" ("1") to indicate the null is true (false) and "a" ("r") to indicate "not reject" ("reject"), we can easily summarize Panel A. For instance, $N_{0|r}$ measures the number of rejections when the null is true (i.e. the number of false discoveries) and $N_{1|a}$ measures the number of failed rejections when the null is false (i.e. the number of missed discoveries). To avoid confusion, we try not to use standard statistical language in describing our notation but rather words unique to our factor testing context. The generic notation in Panel B is convenient for us to formally define different types of errors and describe adjustment procedures in subsequent sections.

[12]Examples of publication of unsuccessful factors include Fama and MacBeth (1973) and Ferson and Harvey (1993). Fama and MacBeth (1973) show that squared beta and standard deviation of the market model residual have an insignificant role in explaining the cross-section of expected returns. However, the inclusion of these two variables was a result of a falsification experiment rather than a search for new factors. Overall, it is rare to publish "non-results" and all instances of published non-results are coupled with significant results for other factors.

4.3 Type I and Type II Errors

For a single hypothesis test, a value α is used to control Type I error rate: the probability of finding a factor to be significant when it is not. The α is sometimes called the 'level of significance'. In a multiple testing framework, restricting each individual test's Type I error rate at α is not enough to control the overall probability of false discoveries. The intuition is that, under the null that all factors are insignificant, it is very likely for an event with α probability to occur when many factors are tested. In multiple hypothesis testing, we need measures of the Type I error that help us simultaneously evaluate the outcomes of many individual tests.

To gain some intuition on plausible measures of Type I error, we return to Panel B of Table 2. $N_{0|r}$ and $N_{1|a}$ count the total number of the two types of errors: $N_{0|r}$ counts false discoveries while $N_{1|a}$ counts missed discoveries. As generalized from single hypothesis testing, the Type I error in multiple hypothesis testing is also related to false discoveries — concluding a factor is "significant" when it is not. But, by definition, we must draw several conclusions in multiple hypothesis testing, and there is a possible false discovery for each. Therefore, plausible definitions of the Type I error should take into account the joint occurrence of false discoveries.

The literature has adopted at least two ways of summarizing the "joint occurrence". One approach is to count the total number of false discoveries $N_{0|r}$. $N_{0|r}$ greater than zero suggests incorrect statistical inference for the overall multiple testing problem — the occurrence of which we should limit. Therefore, the probability of event $N_{0|r} > 0$ the should be a meaningful quantity for us to control. Indeed, this is the intuition behind the *family-wise error rate* introduced later. On the other hand, when the total number of discoveries R is large, one or even a few false discoveries may be tolerable. In this case, $N_{0|r}$ is no longer a suitable measure; a certain *false discovery proportion* may be more desirable. Unsurprisingly, the expected value of $N_{0|r}/R$ is the focus of *false discovery rate*, the second type of control.

4.3.1 Family-wise Error Rate

The two aforementioned measures are the most widely used in the statistics literature. Moreover, many other techniques can be viewed as extensions of these measures. Holm (1979) is the first to formally define the *family-wise error rate*. Benjamini and Hochberg (1995) define and study the *false discovery rate*. Alternative definitions of error rate include *per comparison error rate* (Saville, 1990), *positive false discovery rate* (Storey, 2003) and *generalized false discovery rate* (Sarkar and Guo, 2009). We now describe the two leading approaches in detail.

The *family-wise error rate* (FWER) is the probability of at least one Type I error:

$$\text{FWER} = Pr(N_{0|r} \geq 1).$$

FWER measures the probability of even a single false discovery, irrespective of the total number of tests. For instance, researchers might test 100 factors; FWER measures the probability of incorrectly identifying one or more factors to be significant. Given significance or threshold level α, we explore two existing methods (Bonferroni and Holm's adjustment) to ensure FWER does not exceed α. Even as the number of trials increases, FWER still measures the probability of at least one false discovery. This absolute control is in contrast to the proportionate control afforded by the *false discovery rate* (FDR), defined below.

4.3.2 False Discovery Rate

The *false discovery proportion* (FDP) is the proportion of Type I errors:

$$\text{FDP} = \begin{cases} \dfrac{N_{0|r}}{R} & \text{if } R > 0, \\ \\ 0 & \text{if } R = 0. \end{cases}$$

The *false discovery rate* (FDR) is defined as:

$$\text{FDR} = E[FDP].$$

FDR measures the expected proportion of false discoveries among all discoveries. It is less stringent (i.e., leads to more discoveries) than FWER and usually much less so when many tests are performed.[13] Intuitively, this is because FDR allows $N_{0|r}$ to grow in proportion to R whereas FWER measures the probability of making even a single Type I error.

Returning to Example A, Panel A shows that a false discovery event has occurred under FWER since $N_{0|r} = 50 \geq 1$ and the realized *FDP* is high, $50/100 = 50\%$. This suggests that the *probability* of false discoveries (FWER) and the *expected* proportion of false discoveries (FDR) may both be high.[14] The remedy, as suggested by many

[13] There is a natural ordering between FDR and FWER. Theoretically, FDR is always bounded above by FWER, i.e., $FDR \leq FWER$. To see this, by definition,

$$\begin{aligned} \text{FDR} &= E[\frac{N_{0|r}}{R}|R > 0]Pr(R > 0) \\ &\leq E[I_{(N_{0|r} \geq 1)}|R > 0]Pr(R > 0) \\ &= Pr((N_{0|r} \geq 1) \cap (R > 0)) \\ &\leq Pr(N_{0|r} \geq 1) = \text{FWER}, \end{aligned}$$

where $I_{(N_{0|r} \geq 1)}$ is an indicator function of event $N_{0|r} \geq 1$. This implies that procedures that control FWER under a certain significance level automatically control FDR under the same significance level. In our context, a factor discovery criterion that controls FWER at α also controls FDR at α.

[14] Panel A only shows one realization of the testing outcome for a certain testing procedure (e.g., single tests). To evaluate FWER and FDR, both of which are expectations and hence depend on the underlying joint distribution of the testing statistics, we need to know the population of the testing

FWER and FDR adjustment procedures, would be to lower p-value thresholds for these hypotheses (p-value, as defined in our context, is the single test probability of having a t-statistic that is at least as large as the observed one under the null hypothesis). In terms of Panel A, this would turn some of the 50 false discoveries insignificant and push them into the "Unpublished" category. Hopefully the 50 true discoveries would survive this change in p-value threshold and remain significant, which is only possible if their p-values are relatively small.

On the other hand, Type II errors — the mistake of missing true factors — are also important in multiple hypothesis testing. Similar to Type I errors, both the total number of missed discoveries $N_{1|a}$ and the fraction of missed discoveries among all abandoned tests $N_{1|a}/(M-R)$ are frequently used to measure the severity of Type II errors.[15] Ideally, one would like to simultaneously minimize the chance of committing a Type I error and that of committing a Type II error. In our context, we would like to include as few insignificant factors (i.e., as low a Type I error rate) as possible and simultaneously as many significant ones (i.e., as low a Type II error rate) as possible. Unfortunately, this is not feasible: as in single hypothesis testing, a decrease in the Type I error rate often leads to an increase in the Type II error rate and vice versa. We therefore seek a balance between the two types of errors. A standard approach is to specify a significance level α for the Type I error rate and derive testing procedures that aim to minimize the Type II error rate, i.e., maximize power, among the class of tests with Type I error rate at most α.

When comparing two testing procedures that can both achieve a significance level α, it seems reasonable to use their Type II error rates. However, when we have multiple tests, the exact Type II error rate typically depends on a set of unknown parameters and is therefore difficult to assess.[16] To overcome this difficulty, researchers frequently use distance of the actual Type I error rate to some pre-specified significance level as the measure for a procedure's efficiency. Intuitively, if a procedure's actual Type I error rate is strictly below α, we can probably push this error rate closer to α by making the testing procedure less stringent, i.e., higher p-value threshold so there will be more discoveries. In doing so, the Type II error rate is presumably low-

outcomes. To give an example that is compatible with Panel A, we assume that the t-statistics for the 700 hypotheses are independent. Moreover, we assume the t-statistic for a false factor follows a normal distribution with mean of zero and variance of 1.0, i.e., $\mathcal{N}(0,1)$; for a true factor, we assume its t-statistic follows a normal distribution with mean of 2.0 and variance of 1.0, i.e., $\mathcal{N}(2,1)$. Under these assumptions about the joint distribution of the test statistics, we find via simulations that FWER is 100% and FDR is 26%, both exceeding 5%.

[15]See Simes (1986) for one example of Type II error in simulation studies and Farcomeni (2008) for another example in medical experiments.

[16]In single hypothesis testing, the Type II error is a function of the unknown true parameter value — in our context, the population factor mean return — under the alternative hypothesis. By tracing out all possible values under the alternative hypothesis, we obtain the Type II error function. The situation is more complicated in multiple hypothesis testing because the Type II error depends on multiple parameters that correspond to the collection of alternative hypotheses for all the tests. Hence, the Type II error function is multivariate when there are multiple tests. See Zehetmayer and Posch (2010) for power estimation in large-scale multiple testing problems.

ered given the inverse relation between the two types of error rates. Therefore, once a procedure's actual Type I error rate falls below a pre-specified significance level, we want it to be as close as possible to that significance level in order to achieve the smallest Type II error rate. Ideally, we would like a procedure's actual Type I error rate to be exactly the same as the given significance level.[17]

Both FWER and FDR are important concepts that are widely applied in many scientific fields. However, based on specific applications, one may be preferred over the other. When the number of tests is very large (e.g., a million), FWER controlling procedures tend to become very tough as they control for the occurrence of even a single false discovery among one million tests. As a result, they often lead to a very limited number of discoveries, if any. Conversely, FWER control is more desirable when the number of tests is relatively small, in which case more discoveries can be achieved and at the same time trusted. In the context of our paper, we are sure that many tests have been tried in the finance literature. Although there are around 300 published ones, hundreds or even thousands of factors might have been constructed and tested. However, it is not clear whether this number should be considered "large" compared to the number of tests conducted in, say, medical research.[18] This creates difficulty in choosing between FWER and FDR. Given this difficulty, we do not take a stand on the relative appropriateness of these two measures but instead provide adjusted p-values for both. Researchers can compare their p-values with these benchmarks to see whether FDR or even FWER is satisfied.

4.4 *P*-value Adjustment: Three Approaches

The statistics literature has developed many methods to control both FWER and FDR.[19] We choose to present the three most well-known adjustments: Bonferroni, Holm, and Benjamini, Hochberg and Yekutieli (BHY). Both Bonferroni and Holm control FWER, and BHY controls FDR. Depending on how the adjustment is implemented, they can be categorized into two general types of corrections: a "single step" correction equally adjusts each p-value and a "sequential" correction is an adaptive procedure that depends on the entire distribution of p-values. Bonferroni is a

[17]In our framework, individual p-values are sufficient statistics for us to make adjustment for multiple tests. Each individual p-value represents the probability of having a t-statistic that is at least as large as the observed one under the null hypothesis. What happens under the alternative hypotheses (i.e., Type II error rate) does not directly come into play because hypothesis testing in the frequentist framework has a primary focus on the Type I error rate. When we deviate from the frequentist framework and consider Bayesian methods, the Type II error rate becomes more important because Bayesian odds ratios put the Type I and Type II error rates on the same footing.

[18]For instance, tens of thousands of tests are performed in the analysis of DNA microarrays. See Farcomeni (2008) for more applications of multiple testing in medical research.

[19]Methods that control FWER include Holm (1979), Hochberg (1988) and Hommel (1988). Methods that control FDR include Benjamini and Hochberg(1995), Benjamini and Liu (1999) and Benjamini and Yekutieli (2001).

single-step procedure whereas Holm and BHY are sequential procedures. Table 3 summarizes the two properties of the three methods.

Table 3: **A Summary of *p*-value Adjustments**

Adjustment type	Single/Sequential	Multiple test
Bonferroni	Single	Family-wise Error Rate
Holm	Sequential	Family-wise Error Rate
Benjamini, Hochberg and Yekutieli (BHY)	Sequential	False Discovery Rate

Suppose there are in total M tests and we choose to set FWER at α_w and FDR at α_d. In particular, we consider an example with the total number of tests $M = 10$ to illustrate how different adjustment procedures work. For our main results, we set both α_w and α_d at 5%. Table 4, Panel A lists the t-statistics and the corresponding p-values for 10 hypothetical tests. The numbers in the table are broadly consistent with the magnitude of t-statistics that researchers report for factor significance. Note that all 10 factors will be "discovered" if we test one hypothesis at a time. Multiple testing adjustments will usually generate different results.[20]

4.4.1 Bonferroni's Adjustment

Bonferroni's adjustment is as follows:

- Reject any hypothesis with p-value $\leq \frac{\alpha_w}{M}$:

$$p_i^{Bonferroni} = \min[M \times p_i, 1].$$

Bonferroni applies the same adjustment to each test. It inflates the original p-value by the number of tests M; the adjusted p-value is compared with the threshold value α_w.

Example 4.4.1 To apply Bonferroni's adjustment to the example in Table 4, we simply multiply all the p-values by 10 and compare the new p-values with $\alpha_w =$ 5%. Equivalently, we can look at the original p-values and consider the cutoff of $0.5\%(= \alpha_w/10)$. This leaves the t-statistic of tests 4,7 and 8 as significant, which are highlighted in Panel B.

Using the notation in Panel B of Table 2 and assuming M_0 of the M null hypotheses are true, Bonferroni operates as a single step procedure that can be shown

[20] Readers who are already familiar with the three multiple testing adjustment procedures can skip to Section 4.5 for our main results.

Table 4: **An Example of Multiple Testing**

The table displays 10 t-statistics and their associated p-values for a hypothetical example. Panel A and B highlight the significant factors under single tests and Bonferroni's procedure, respectively. Panel C and D explain Holm's and BHY's adjustment procedure, respectively. Bold numbers in each panel are associated with significant factors under the specific adjustment procedure of that panel. M represents the total number of tests (M = 10) and $c(M) = \sum_{j=1}^{M} 1/j$. b is the order of p-values from lowest to highest. α_w is the significance level for Bonferroni's and Holm's procedure and α_d is the significance level for BHY's procedure. Both numbers are set at 5%. The cutoff p-value for Bonferroni is 0.5%, for Holm 0.60% and for BHY 0.85%.

Panel A: Single tests and "significant" factors											# of discoveries
Test →	1	2	3	4	5	6	7	8	9	10	
t-statistic	1.99	2.63	2.21	3.43	2.17	2.64	4.56	5.34	2.75	2.49	10
p-value (%)	**4.66**	**0.85**	**2.71**	**0.05**	**3.00**	**0.84**	**0.00**	**0.00**	**0.60**	**1.28**	
Panel B: Bonferroni "significant" factors											
Test →	1	2	3	4	5	6	7	8	9	10	
t-statistic	1.99	2.63	2.21	3.43	2.17	2.64	4.56	5.34	2.75	2.49	3
p-value (%)	4.66	0.85	2.71	**0.05**	3.00	0.84	**0.00**	**0.00**	0.60	1.28	
Panel C: Holm adjusted p-values and "significant" factors											
Reordered tests b	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	
Old order	8	7	4	9	6	2	10	3	5	1	4
p-value (%)	**0.00**	**0.00**	**0.05**	**0.60**	0.84	0.85	1.28	2.71	3.00	4.66	
$\alpha_w/(M+1-b)$ $\alpha_w = 5\%$	0.50	0.56	0.63	0.71	0.83	1.00	1.25	1.67	2.50	5.00	
Panel D: BHY adjusted p-values and "significant" factors											
Reordered tests b	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	
Old order	8	7	4	9	6	2	10	3	5	1	
p-value (%)	**0.00**	**0.00**	**0.05**	**0.60**	**0.84**	**0.85**	1.28	2.71	3.00	4.66	6
$(b \cdot \alpha_d)/(M \times c(M))$ $\alpha_d = 5\%$	0.17	0.34	0.51	0.68	0.85	1.02	1.19	1.37	1.54	1.71	

to restrict FWER at levels less than or equal to $(M_0 \times \alpha_w)/M$, without any assumption on the dependence structure of the p-values. Since $M_0 \leq M$, Bonferroni also controls FWER at level α_w.[21]

[21] The number of true nulls M_0 is unknown, so we usually cannot make Bonferroni more powerful by increasing α_w to $\hat{\alpha} = M\alpha_w/M_0$ (note that $M_0\hat{\alpha}/M = \alpha_w$). However some papers, including Schweder and Spjotvoll (1982) and Hochberg and Benjamini (1990), try to improve the power of Bonferroni by estimating M_0. We try to achieve the same goal by using either Holm's procedure which also controls FWER or procedures that control FDR, an alternative definition of Type I error rate.

4.4.2 Holm's Adjustment

Sequential methods have been proposed to adjust p-values in multiple hypothesis testing.[22] They are motivated by a seminal paper by Schweder and Spjotvoll (1982), who suggest a graphical presentation of the multiple testing p-values. In particular, using N_p to denote the number of tests that have a p-value exceeding p, Schweder and Spjotvoll (1982) suggest plotting N_p against $(1-p)$. When p is not very small (e.g., $p > 0.2$), it is very likely that the associated test is from the null hypothesis. In this case, the p-value for a null test can be shown to be uniformly distributed between 0 and 1. It then follows that for a large p and under independence among tests, the expected number of tests with a p-value exceeding p equals $T_0(1-p)$, where T_0 is the number of null hypotheses, i.e., $E(N_p) = T_0(1-p)$. By plotting N_p against $(1-p)$, the graph should be approximately linear with slope T_0 for large p-values. Points on the graph that substantially deviate from this linear pattern should correspond to non-null hypotheses, i.e., discoveries. The gist of this argument — large and small p-values should be treated differently — have been distilled into many variations of sequential adjustment methods, among which we will introduce Holm's method that controls FWER and BHY's method that controls FDR.

Holm's adjustment is as follows:

- Order the original p-values such that $p_{(1)} \leq p_{(2)} \leq \cdots p_{(b)} \leq \cdots \leq p_{(M)}$ and let associated null hypotheses be $H_{(1)}, H_{(2)}, \cdots H_{(b)} \cdots, H_{(M)}$.
- Let k be the minimum index such that $p_{(b)} > \frac{\alpha_w}{M+1-b}$.
- Reject null hypotheses $H_{(1)} \cdots H_{(k-1)}$ (i.e., declare these factors significant) but not $H_{(k)} \cdots H_{(M)}$.

The equivalent adjusted p-value is therefore

$$p_{(i)}^{Holm} = \min[\max_{j \leq i}\{(M-j+1)p_{(j)}\}, 1].$$

Holm's adjustment is a step-down procedure: for the ordered p-values, we start from the smallest p-value and go down to the largest one.[23] If k is the smallest index that satisfies $p_{(b)} > \frac{\alpha_w}{M+1-b}$, we will reject all tests whose ordered index is below k.

To explore how Holm's adjustment procedure works, suppose k is the smallest index such that $p_{(b)} > \frac{\alpha_w}{M+1-b}$. This means that for $b < k$, $p_{(b)} \leq \frac{\alpha_w}{M+1-b}$. In particular, for $b = 1$, Bonferroni equals Holm, i.e., $\frac{\alpha_w}{M} = \frac{\alpha_w}{M+1-(b=1)}$; for $b = 2$, $\frac{\alpha_w}{M} < \frac{\alpha_w}{M+1-(b=2)}$,

[22] Here "sequential" refers to the fact that we adjust the ordered p-values sequentially. It does not mean that the individual tests arrive sequentially.

[23] Viewing small p-values as "up" and large p-values as "down", Holm's procedure is a "step-down" procedure in that it goes from small p-values to large ones. This terminology is consistent with the statistics literature. Of course, small p-values are associated with "large" values of the test statistics.

so Holm is less stringent than Bonferroni. Since less stringent hurdles are applied to the second to the $(k-1)$th p-values, more discoveries are generated under Holm's than Bonferroni's adjustment.

Example 4.4.2 To apply Holm's adjustment to the example in Table 4, we first order the p-values in ascending order and try to locate the smallest index k that makes $p_{(b)} > \frac{\alpha_w}{M+1-b}$. Table 4, Panel C shows the ordered p-values and the associated $\frac{\alpha_w}{M+1-b}$'s. Starting from the smallest p-value and going up, we see that $p_{(b)}$ is below $\frac{\alpha_w}{M+1-b}$ until $b=5$, at which $p_{(5)}$ is above $\frac{\alpha_w}{10+1-5}$. Therefore, the smallest b that satisfies $p_{(b)} > \frac{\alpha_w}{M+1-b}$ is 5 and we reject the null hypothesis for the first four ordered tests (we discover four factors) and fail to reject the null for the remaining six tests. The original labels for the rejected tests are in the second row in Panel C. Compared to Bonferroni, one more factor (test 9) is discovered, that is, four factors rather than three are significant. In general, Holm's approach leads to more discoveries and all discoveries under Bonferroni are also discoveries under Holm's criteria.

Like Bonferroni, Holm also restricts FWER at α_w without any requirement on the dependence structure of p-values. It can also be shown that Holm is uniformly more powerful than Bonferroni in that tests rejected (factors discovered) under Bonferroni are always rejected under Holm but not vice versa. In other words, Holm leads to at least as many discoveries as Bonferroni. Given the dominance of Holm over Bonferroni, one might opt to only use Holm. We include Bonferroni because it is the most widely used adjustment and a simple single-step procedure.

4.4.3 Benjamini, Hochberg and Yekutieli's Adjustment

Benjamini, Hochberg and Yekutieli (BHY)'s adjustment is as follows:

- As with Holm's procedure, order the original p-values such that $p_{(1)} \leq p_{(2)} \leq \cdots p_{(b)} \leq \cdots \leq p_{(M)}$ and let associated null hypotheses be $H_{(1)}, H_{(2)}, \cdots H_{(b)} \cdots, H_{(M)}$.
- Let k be the maximum index such that $p_{(b)} \leq \frac{b}{M \times c(M)} \alpha_d$.
- Reject null hypotheses $H_{(1)} \cdots H_{(k)}$ but not $H_{(k+1)} \cdots H_{(M)}$.

The equivalent adjusted p-value is defined sequentially as:

$$p_{(i)}^{BHY} = \begin{cases} p_{(M)} & \text{if } i = M, \\ \min[p_{(i+1)}^{BHY}, \frac{M \times c(M)}{i} p_{(i)}] & \text{if } i \leq M-1. \end{cases}$$

where, $c(M)$ is a function of the total number of tests M and controls for the generality of the test. The larger $c(M)$ is, the more stringent the test is and hence, the more general the test is in guarding against dependency among the test statistics. In particular, Benjamini and Yekutieli (2001) show that setting $c(M)$ at

$$c(M) = \sum_{j=1}^{M} \frac{1}{j}, \tag{1}$$

makes sure that the procedure works under arbitrary dependency among the test statistics. We focus on this specification due to its generality but will discuss what happens under alternative specifications of $c(M)$.

In contrast to Holm's, BHY's method starts with the largest p-value and goes up to the smallest one. If k is the largest index that satisfies $p_{(b)} \leq \frac{b}{M \times c(M)}\alpha_d$, we will reject tests (discover factors) whose ordered index is below or equal to k. Also, note that α_d (significance level for FDR) is chosen to be the same as α_w (significance level for FWER). Significance level is subjective in nature. Here we choose the same significance level to make an apples-to-apples comparison between FDR and FWER adjustment procedures. We discuss this choice in more detail in Section 4.6.

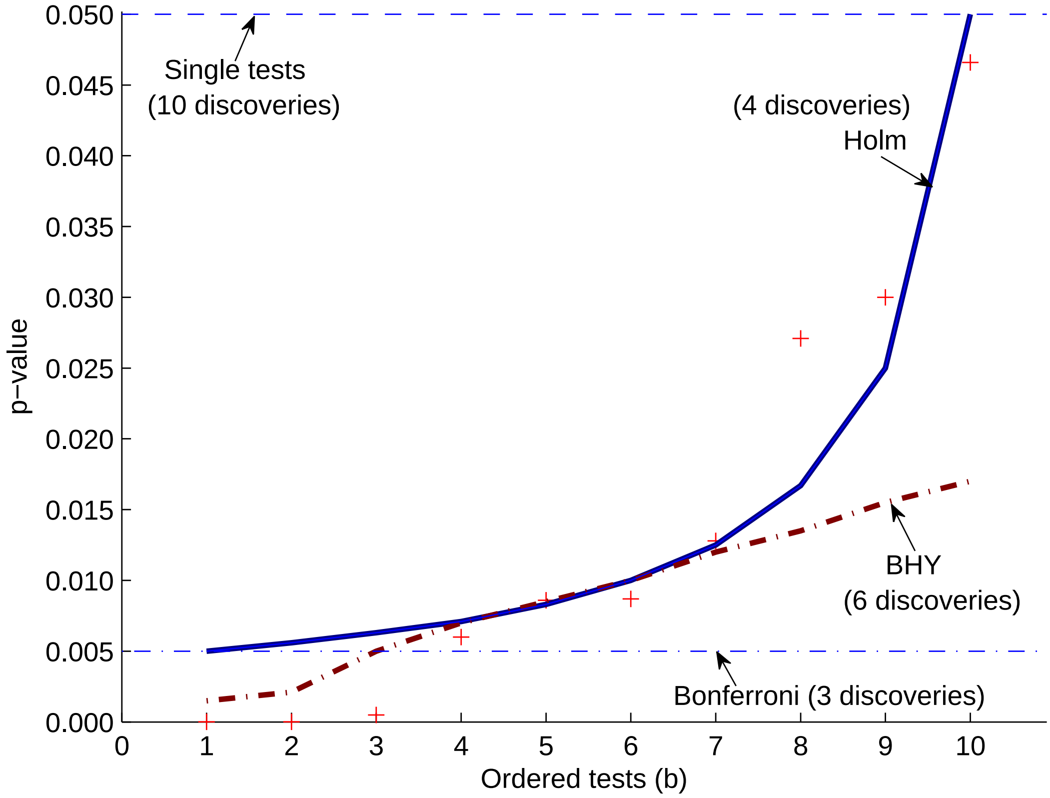
To explore how BHY works, let k be the largest index such that $p_{(b)} \leq \frac{b}{M \times c(M)}\alpha_d$. This means that for $b > k$, $p_{(b)} > \frac{b}{M \times c(M)}\alpha_d$. In particular, we have $p_{(k+1)} > \frac{(k+1)}{M \times c(M)}\alpha_d$, $p_{(k+2)} > \frac{(k+2)}{M \times c(M)}\alpha_d$, $\ldots$, $p_{(M)} > \frac{M}{M \times c(M)}\alpha_d$. We see that the $(k+1)$th to the last null hypotheses, not rejected, are compared to numbers smaller than α_d, the usual significance level in single hypothesis testing. By being stricter than single hypothesis tests, BHY guarantees that the *false discovery rate*, which depends on the joint distribution of all the test statistics, is below the pre-specified significance level. See Benjamini and Yekutieli (2001) for details on the proof.

Example 4.4.3 To apply BHY's adjustment to the example in Table 4, we first order the p-values in ascending order and try to locate the largest index k that satisfies $p_{(b)} \leq \frac{b}{M \times c(M)}\alpha_d$. Table 4, Panel D shows the ordered p-values and the associated $\frac{b}{M \times c(M)}\alpha_d$'s. Starting from the largest p-value and going down, we see that $p_{(b)}$ is above $\frac{b}{M \times c(M)}\alpha_d$ until $b = 6$, at which $p_{(6)}$ is below $\frac{6}{10 \times 2.93}\alpha_d$. Therefore, the smallest b that satisfies $p_{(b)} \leq \frac{b}{M \times c(M)}\alpha_d$ is 6 and we reject the null hypothesis for the first six ordered tests and fail to reject for the remaining four tests. In the end, BHY leads to six significant factors (tests 8,7,4,9,6 and 2), three more than Bonferroni and two more than Holm.

In summary, for single tests, using the usual 5% cutoff, 10 out of 10 are discovered. Allowing for multiple tests, the cutoffs are far smaller, with BHY at 0.85%, Holm at 0.60% and Bonferroni at 0.5%.

The choice of $c(M)$ determines the generality of BHY's procedure. Intuitively, the larger $c(M)$ is, the more difficult it is to satisfy the inequality $p_{(b)} \leq \frac{b}{M \times c(M)}\alpha_d$ and hence there will be fewer discoveries. This makes it easier to restrict *the false discovery rate* below a given significance level since fewer discoveries are made. In the original work that develops the concept of *false discovery rate* and related testing procedures, $c(M)$ is set equal to one. It turns out that under this choice, BHY is only valid when the test statistics are independent or positively dependent. With our choice of $c(M)$ (i.e., $c(M) = \sum_{j=1}^{M} \frac{1}{j}$), BHY is valid under any form of dependence among the p-values.[24] Note with $c(M) > 1$, this reduces the size of $\frac{b}{M \times c(M)}\alpha_d$ and it is tougher to satisfy the inequality $p_{(b)} \leq \frac{b}{M \times c(M)}\alpha_d$. That is, there will be fewer factors found to be significant.

Figure 1: **Multiple Test Thresholds for Example A**



The 10 p-values for Example in Table 4 and the adjusted p-value lines for various adjustment procedures. All 10 factors are discovered using the standard criteria for single tests, three under Bonferroni, four under Holm and six under BHY. The significance level is set at 5% for each adjustment method.

Figure 1 summarizes our example. It plots the original p-values (single tests) as well as adjusted p-value lines for various multiple testing adjustment procedures. We see the stark difference in outcomes between multiple and single hypothesis testing.

[24]See Benjamini and Yekutieli (2001) for the proof.

While all 10 factors would be discovered under single hypothesis testing, only three to six factors would be discovered under a multiple hypothesis test. Although single hypothesis testing guarantees the Type I error of each test meets a given significance level, meeting the more stringent FWER or FDR bound will lead us to discard a number of factors.

4.5 Summary Statistics

Figure 2 shows the history of discovered factors and publications.[25] We observe a dramatic increase in factor discoveries during the last decade. In the early period from 1980 to 1991, only about one factor is discovered per year. This number has grown to around five in the 1991-2003 period, during which a number of papers, such as Fama and French (1992), Carhart (1997) and Pastor and Stambaugh (2003), spurred interest in studying cross-sectional return patterns. In the last nine years, the annual factor discovery rate has increased sharply to around 18. In total, 164 factors were discovered in the past nine years, roughly doubling the 84 factors discovered in all previous years. We do not include working papers in Figure 2. In our sample, there are 63 working papers covering 68 factors.

We obtain t-statistics for each of the 316 factors discovered, including the ones in working papers.[26] The overwhelming majority of t-statistics exceed the 1.96 benchmark for 5% significance.[27] The non-significant ones typically belong to papers that propose a number of factors. These likely represent only a small sub-sample of non-significant t-statistics for all tried factors. Importantly, we take published t-statistics as given. That is, we assume they are econometrically sound with respect to the usual suspects (data errors, coding errors, misalignment, heteroskedasticity, autocorrelation, clustering, outliers, etc.).

4.6 *P*-value Adjustment When All Tests Are Published ($M = R$)

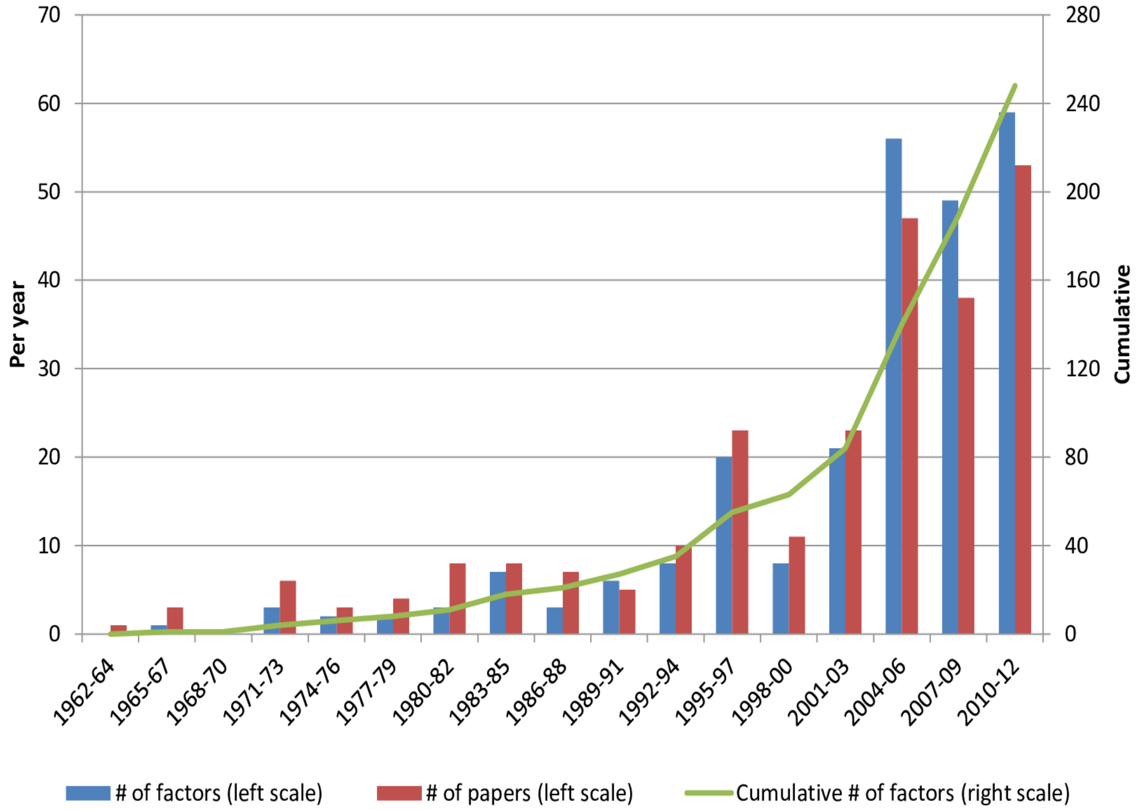
We now apply the three adjustment methods previously introduced to the observed factor tests, under the assumption that the test results of *all tried factors* are available. We know that this assumption is false since our sample under-represents all

[25] To be clear, we only count those that have t-statistics or equivalent statistics reported. Roughly 20 new factors fail to satisfy this requirement. For additional details, see factors in Table 6 marked with ‡.

[26] The sign of a t-statistic depends on the direction of the long/short strategy. We usually calculate p-values based on two-sided t-tests, so the sign does not matter. From an investment perspective, the sign of the mean return of a long/short strategy does not matter as we can always reverse the direction of the strategy. Therefore we use absolute values of these t-statistics.

[27] The multiple testing framework is robust to outliers. The procedures are based on either the total number of tests (Bonferroni) or the order statistics of t-statistics (Holm and BHY).

Figure 2: **Factors and Publications**



insignificant factors by conventional significance standards: we only observe those insignificant factors that are the results of purposeful falsification experiments. We design methods to handle this missing data issue later.

Despite some limitations, our results in this section are useful for at least two purposes. First, the benchmark t-statistic based on our incomplete sample provides a lower bound of the true t-statistic benchmark. In other words, if M(total number of tests) $>R$ (total number of discoveries), then we would accept fewer factors than when $M = R$,[28] so future t-statistics need to at least surpass our benchmark to claim significance. Second, results in this section can be rationalized within a Bayesian or hierarchical testing framework.[29] Factors in our list constitute an "elite" group: they have survived academia's scrutiny for publication. Placing a high prior on this group

[28]This is always true for Bonferroni's adjustment but not always true for the other two types of adjustments. The Bonferroni adjusted t-statistic is monotonically increasing in the number of trials so the t-statistic benchmark will only rise if there are more factors. Holm and BHY depend on the exact t-statistic distribution so more factors do not necessarily imply a higher t-statistic benchmark.

[29]See Wagenmakers and Grünwald (2006) and Storey (2003) on Bayesian interpretations of traditional hypothesis testing. See Meinshausen (2008) for a hierarchical approach on variable selection.

in a Bayesian testing framework or viewing this group as a cluster in a hierarchical testing framework, one can interpret results in this section as the first step factor selection within an a priori group.

Based on our sample of observed t-statistics of published factors,[30] we obtain three benchmark t-statistics. In particular, at each point in time, we transform the set of available t-statistics into p-values. We then apply the three adjustment methods to obtain benchmark p-values. Finally, these p-value benchmarks are transformed back into t-statistics, assuming that standard normal distribution well approximates the t-distribution. To guide future research, we extrapolate our benchmark t-statistics into the future assuming that the rate of "factor production" remains the same as the recent history, i.e., 2003-2012.

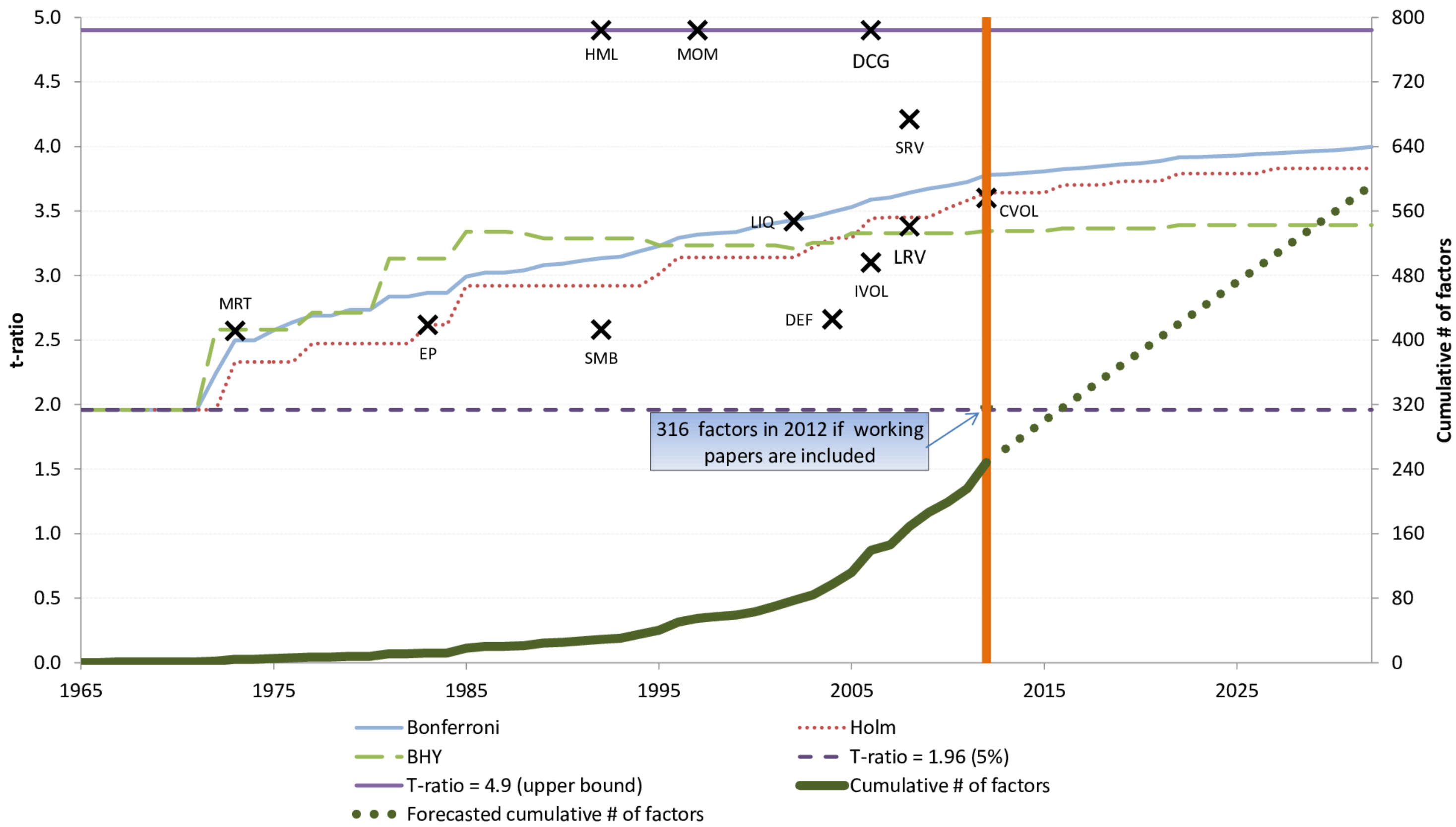
We choose to set α_w at 5% (Holm, FWER) and α_d at 1% (BHY, FDR) for our main results. Significance level is subjective, as in individual hypothesis testing where conventional significance levels are usually adopted. Since FWER is a special case of the Type I error in individual testing and 5% seems the default significance level in cross-sectional studies, we set α_w at 5%. On the other hand, FDR is a more lenient control relative to FWER. If we choose the same α_d as α_w, then by definition the BHY method will be more lenient than both Holm and Bonferroni. We set FDR at 1% but will explain what happens when α_d is increased to 5%.

Figure 3 presents the three benchmark t-statistics. Both Bonferroni and Holm adjusted benchmark t-statistics are monotonically increasing in the number of discoveries. For Bonferroni, the benchmark t-statistic starts at 1.96 and increases to 3.78 by 2012. It reaches 4.00 in 2032. The corresponding p-values (under single tests) for 3.78 and 4.00 are 0.02% and 0.01% respectively, much lower than the starting level of 5%. Holm implied t-statistics always fall below Bonferroni t-statistics, consistent with the fact that Bonferroni always results in fewer discoveries than Holm. However, Holm tracks Bonferroni closely and their differences are small. BHY implied benchmarks, on the other hand, are not monotonic. They fluctuate before year 2000 and stabilize at 3.39 (p-value = 0.07%) after 2010. This stationarity feature of BHY implied t-statistics, inherent in the definition of FDR, is in contrast to Bonferroni and Holm. Intuitively, at any fixed significance level α, the Law of Large Numbers forces the false discovery rate (FDR) to converge to a constant. If we change α_d to 5%, the corresponding BHY implied benchmark t-statistic is 2.78 (p-value = 0.54%) in 2012 and 2.81 (p-value = 0.50%) in 2032, still much higher than the 1.96 starting value. In sum, taking into account of testing multiplicity, we believe the minimum threshold t-statistic for 5% significance is about 2.8, which corresponds to a p-value (if a single test) of 0.5%.

To see how the new t-statistic benchmarks better reveal the statistical significance of factors, in Figure 3 we mark the t-statistics of a few prominent factors. Among these factors, HML, MOM, DCG, SRV and MRT are significant across all types of

[30]See Appendix A for details on our sampling procedure.

Figure 3: **Adjusted t-statistics, 1965-2032**



Bonferroni and Holm are multiple testing adjustment procedures that control the *family-wise error rate* (FWER) and are described in Section 4.4.1 and 4.4.2, respectively. BHY is a multiple testing adjustment procedure that controls the *false discovery rate* and is described in Section 4.4.3. The green solid curve shows the historical cumulative number of factors discovered, excluding those from working papers. Forecasts (dotted green line) are based on a linear extrapolation. The dark crosses mark selected factors proposed by the literature. They are MRT (market beta; Fama and MacBeth (1973)), EP (earnings-price ratio; Basu (1983)), SMB and HML (size and book-to-market; Fama and French (1992)), MOM (momentum; Carhart (1997)), LIQ (liquidity; Pastor and Stambaugh (2003)), DEF (default likelihood; Vassalou and Xing (2004)), IVOL (idiosyncratic volatility; Ang, Hodrick, Xing and Zhang (2006)); DCG (durable consumption goods; Yogo (2006)); SRV and LRV (short-run and long-run volatility; Adrian and Rosenberg (2008)) and CVOL (consumption volatility; Boguth and Kuehn (2012)). T-statistics over 4.9 are truncated at 4.9. For detailed descriptions of these factors, see Table 6.

t-statistic adjustments, EP, LIQ and CVOL are sometimes significant and the rest are never significant.

One concern about our results is that factors are discovered at different times and tests are conducted using different methods. This heterogeneity in the time of discovery and testing methods may blur the interpretation of our results. Ideally, we want updated factor tests that are based on the most recent sample and the same testing method.[31] To alleviate this concern, we focus on the group of factors that are published no earlier than 2000 and rely on Fama-MacBeth tests. Additionally, we require that factor tests cover at least the 1970-1995 period and have at least the Fama-French three factors (Fama and French, 1993) as controls. This leaves us with 124 factors. Based on this factor group, the Bonferroni and Holm implied threshold t-statistics are 3.54 and 3.20 (5% significance), respectively, and the BHY implied thresholds are 3.23 (1% significance) and 2.67 (5% significance) by 2012. Not surprisingly, these statistics are smaller than the corresponding thresholds based on the full sample. However, the general message that we need a much higher t-statistic threshold when multiple testing is taken into account is unchanged.

4.7 Robustness

4.7.1 Test statistics dependence

There is a caveat for all three methods that we have considered so far. In the context of multiple testing, any type of adjustment procedure can become too stringent when there is a certain dependence structure in the data. This is because these procedures are primarily designed to guard against Type I errors. Under a certain correlation structure, they may penalize Type I errors too harshly and lead to a high Type II error rate.

In theory, under independence, Bonferroni and Holm approximately achieve the pre-specified significance level α when the number of tests is large. On the other hand, both procedures tend to generate fewer discoveries than desired when there is a certain degree of dependence among the tests. Intuitively, in the extreme case where all tests are the same (i.e., correlation = 1.0), we do not need to adjust at all: FWER is the same as the Type I error rate for single tests. Hence, the usual single hypothesis test is sufficient. Similarly, BHY may generate too few discoveries when tests are independent or positively correlated.

[31] We want to stress that the three types of adjustments in our paper are robust to the heterogeneity in the time of discovery and testing methods among individual studies. That is, despite the varying degrees of sample overlap and the differences in the testing methods, our adjustment procedures guarantee that the Type I errors (however they are defined) are controlled under their pre-specified levels. Therefore, from a technical point of view, neither non-simultaneity nor differences in testing methods invalidate our results.

Having discussed assumptions for the testing methods to work efficiently, we now try to think of scenarios that can potentially violate these assumptions. First, factors that proxy for the same type of risk may be dependent. Moreover, returns of long-short portfolios designed to achieve exposure to a particular type of factor may be correlated. For example, there are a number of factors with price in the denominator which are naturally correlated. We also count four different idiosyncratic volatility factors. If this type of positive dependence exists among test statistics, all three methods would likely to generate fewer significant factors than desired. On the other hand, most often factors need to "stand their ground" to be publishable. In the end, if you think we are overcounting at 316, consider taking a haircut to 113 factors (the number of "common" factors in Table 1). Figure 3 shows that our main conclusions do not materially change. For example, the Holm at 113 factors is 3.29 (p-value = 0.10%) while Holm at 316 factors is 3.64 (p-value = 0.03%).

Second, research studying the same factor but based on different samples will generate highly dependent test statistics. Examples include the sequence of papers studying the size effect. We try to minimize this concern by including, with a few exceptions, only the original paper that proposes the factor. To the extent that our list includes few such duplicate factors, our method greatly reduces the dependence that would be introduced by including all papers studying the same factor but for different sample periods.

Finally, when dependence among test statistics can be captured by Pearson correlations among contemporaneous strategy returns, we present a new model in Section 5 to systematically incorporate the information in test correlations.

4.7.2 The Case When $M > R$

To deal with the hidden tests issue when $M > R$, we propose in Appendix B a simulation framework to estimate benchmark t-statistics. The idea is to first back out the underlying distribution for the t-statistics of all tried factors; then, to generate benchmark t-statistic estimates, apply the three adjustment procedures to simulated t-statistics samples.[32]

Based on our estimates, 71% of all tried factors are missing. Using this information, the new benchmark t-statistics for Bonferroni and Holm are estimated to be 4.01 and 3.96, respectively; both slightly higher than when $M = R$. This is as expected because more factors are tried under this framework. The BHY implied t-statistic increases from 3.39 to 3.68 at 1% significance and from 2.78 to 3.18 at 5% significance. In sum, across various scenarios, we think the minimum threshold t-statistic is 3.18,

[32]The underlying assumption for the model in Appendix B is the independence among t-statistics, which may not be plausible given our previous discussions on test dependence. In that case, our structural model in Section 5 proposes a more realistic data generating process for the cross-section of test statistics.

corresponding to BHY's adjustment for $M > R$ at 5% significance. Alternative cases all result in even higher benchmark t-statistics.

One concern about BHY is that our specification of $c(M)$ results in an overly stringent threshold for FDR. We therefore try the more lenient choice (i.e., $c(M) \equiv 1$) as in Benjamini and Hochberg (1995). Based on our estimate that 71% of tried factors are missing and by simulating the missing tests as in Appendix B, we find that the BHY implied threshold equals 3.05 at 5% significance and 3.17 at 1% significance. Indeed, these numbers are smaller than the numbers under our default specification of $c(M)$ (i.e., $c(M) = \sum_{j=1}^{M} \frac{1}{j}$). However, they are above 3.0 and therefore are consistent with our overall message.

4.7.3 A Bayesian Hypothesis Testing Framework

We can also study multiple hypothesis testing within a Bayesian framework. One major obstacle of applying Bayesian methods in our context is the unobservability of all tried factors. While we propose new frequentist methods to handle this missing data problem, it is not clear how to structure the Bayesian framework in this context. In addition, the high dimensionality of the problem raises concerns on both the accuracy and the computational burden of Bayesian methods.

Nevertheless, ignoring the missing data issue, we outline a standard Bayesian multiple hypothesis testing framework in Appendix C and explain how it relates to our multiple testing framework. We discuss in detail the pros and cons of the Bayesian approach. In contrast to the frequentist approach, which uses generalized Type I error rates to guide multiple testing, the Bayesian approach relies on the posterior likelihood function and thus contains a natural penalty term for multiplicity. However, this simplicity comes at the expense of having a restrictive hierarchical model structure and independence assumptions that may not be realistic for our factor testing problem. Although extensions incorporating certain forms of dependence are possible, it is unclear what precisely we should do for the 316 factors in our list. In addition, even for the Bayesian approach, final reject/accept decision still involves threshold choice. Due to these concerns, we choose not to implement the Bayesian approach. We leave extensions of the basic Bayesian framework that could possibly alleviate the above concerns to future research.

4.7.4 Methods Controlling the FDP

Instead of FDR, recent research by Lehmann and Romano (2005) develops methods to directly control the realized FDP. In particular, they propose a stepdown method to control the probability of FDP exceeding a threshold value. Since their definition of Type I error (i.e., $P(FDP > \gamma)$ where γ is the threshold FDP value) is different from either FWER or FDR, results based on their methods are not comparable to ours. However, the main conclusion is the same. For instance, when $\gamma = 0.10$ and

$\alpha = 0.05$, the benchmark t-statistic is 2.70 (p-value = 0.69%), much higher than the conventional cutoff of 1.96. Details are presented in Appendix D.

5 Correlation Among Test Statistics

Although the BHY method is robust to arbitrary dependence among test statistics, it does not use any information about the dependence structure. Such information, when appropriately incorporated, can be helpful in making the method more accurate (i.e., less stringent). We focus on the type of dependence that can be captured by Pearson correlation. As one way to generate correlation among test statistics, we focus on the correlation among factor returns. This correlation is likely driven by macroeconomic and market-wide variables. Therefore, in our context, the dependence among test statistics is equivalent to the correlation among factor returns.

Multiple testing corrections in the presence of correlation has only been considered in the recent statistics literature. Existing methods include bootstrap based permutation tests and direct statistical modeling. Permutation tests resample the entire dataset and construct an empirical distribution for the pool of test statistics.[33] Through resampling, the correlation structure in the data is taken into account and no model is needed. In contrast, direct statistical modeling makes additional distributional assumptions on the data generating process. These assumptions are usually case dependent as different kinds of correlations are more plausible under different circumstances.[34]

In addition, recent research in finance explores bootstrap procedures to assess the statistical significance of individual tests.[35] Many of these studies focus on performance evaluation and test whether fund managers exhibit skill. Our approach focuses on the joint distribution of the test statistics (both FWER and FDR depend on the cross-section of t-statistics) and evaluates the significance of each individual factor.

Unfortunately, we do not always observe the time-series of factor returns (when a t-statistic is based on long-short strategy returns) or the time-series of slopes in cross-sectional regressions (when a t-statistic is based on the slope coefficients in cross-sectional regressions). Because few researchers post their original data, often all we have is the single t-statistic that summarizes the significance of a factor. We propose a novel approach to overcome this missing data problem. It is in essence a "direct

[33] Westfall (1993) and Ge et al. (2003) are the early papers that suggest the permutation resampling approach in multiple testing. Later development of the permutation approach tries to reduce computational burden by proposing efficient alternative approaches. Examples include Lin (2005), Conneely and Boehnke (2007) and Han, Kang and Eskin (2009).

[34] See Sun and Cai (2008) and Wei et al. (2009).

[35] See Efron (1979) for the original work in the statistics literature. For recent finance applications, see Karolyi and Kho (2004), Kosowski, Timmermann, White, and Wermers (2006), Kosowski, Naik and Teo (2007), Fama and French (2010), Cao, Chen, Liang and Lo (2013) and Harvey and Liu (2014c).

modeling approach" but does not require the full information of the return series based on which the t-statistic is constructed. In addition, our approach is flexible enough to incorporate various kinds of distributional assumptions. We expect it to be a valuable addition to the multiple testing literature, especially when only test statistics are observable.

5.1 A Model with Correlations

For each factor, suppose researchers construct a corresponding long-short trading strategy and normalize the return standard deviation to be $\sigma = 15\%$ per year, which is close to the annual volatility of the market index.[36] In particular, let the normalized strategy return in period t for the i-th discovered strategy be $X_{i,t}$. Then the t-statistic for testing the significance of this strategy is:

$$T_i = (\sum_{t=1}^{N} X_{i,t}/N)/(\sigma/\sqrt{N}).$$

Assuming joint normality and zero serial correlation for strategy returns, this t-stat has a normal distribution

$$T_i \sim N(\mu_i/(\sigma/\sqrt{N}), 1),$$

where μ_i denotes the population mean of the strategy. The μ_i's are unobservable and hypothesis testing under this framework amounts to testing $\mu_i > 0$. We assume that each μ_i is an independent draw from the following mixture distribution:

$$\mu_i \sim p_0 I_{\{\mu=0\}} + (1 - p_0)\text{Exp}(\lambda),$$

where $I_{\{\mu=0\}}$ is the distribution that has a point mass at zero, $\text{Exp}(\lambda)$ is the exponential distribution that has a mean parameter λ and p_0 is the probability of drawing from the point mass distribution. This mixture distribution assumption is the core component for Bayesian multiple testing and succinctly captures the idea of hypothesis testing in the traditional frequentist's view: while there are a range of possible values for the means of truly profitable strategies, a proportion of strategies should have a mean that is indistinguishable from zero. The exponential assumption is not essential for our model as more sophisticated distributions (e.g., a Gamma distribution featuring two free parameters) can be used. We use the exponential distribution for its simplicity[37] and perhaps more importantly, for it being consistent with the

[36] Notice that this assumption is not necessary for our approach. Fixing the standard deviations of different strategies eliminates the need to separately model them, which can be done through a joint modeling of the mean and variance of the cross-section of returns. See Harvey and Liu (2014a) for further discussions on this.

[37] As shown later, we need to estimate the parameters in the mixture model based on our t-statistics sample. An over-parameterized distribution for the continuous distribution in the mixture model, albeit flexible, may result in imprecise estimates. We therefore use the simple one-parameter exponential distribution family.

intuition that more profitable strategies are less likely to exist. An exponential distribution captures this feature by having a monotonically decreasing probability density function.

Next, we incorporate correlations into the above framework. Among the various sources of correlations, the cross-sectional correlations among contemporaneous returns are the most important for us to take into account. These correlations are likely induced by response to common macroeconomic or market shocks. Other kinds of correlations can be easily embedded into our framework as well.[38]

As a starting point, we assume that the contemporaneous correlation between two strategies' returns is ρ. The non-contemporaneous correlations are assumed to be zero. That is,

$$\begin{aligned} \mathrm{Corr}(X_{i,t}, X_{j,t}) &= \rho, \quad i \neq j, \\ \mathrm{Corr}(X_{i,t}, X_{j,s}) &= 0, \quad t \neq s. \end{aligned}$$

Finally, to incorporate the impact of hidden tests, we assume that M factors are tried but only factors that exceed a certain t-statistic threshold are published. We set the threshold t-statistic at 1.96 and focus on the sub-sample of factors that have a t-statistic larger than 1.96. However, as shown in Appendix B, factors with marginal t-statistics (i.e., t-statistics just above 1.96) are less likely to be published than those with larger t-statistics. Therefore, our sub-sample of published t-statistics only covers a fraction of t-statistics above 1.96 for tried factors. To overcome this missing data problem, we assume that our sample covers a fraction r of t-statistics in between 1.96 and 2.57 and that all t-statistics above 2.57 are covered. We augment the existing t-statistic sample to construct the full sample. For instance, when $r = 1/2$, we simply duplicate the sample of t-statistics in between 1.96 and 2.57 and maintain the sample of t-statistics above 2.57 to construct the full sample. For the baseline case, we set $r = 1/2$, consistent with the analysis in Appendix B. We try alternative values of r to determine how the results change.[39]

Given the correlation structure and the sampling distribution for the means of returns, we can fully characterize the distributional properties of the cross-section of returns. We can also determine the distribution for the cross-section of t-statistics as they are functions of returns. Based on our sample of t-statistics for published research, we match key sample statistics with their population counterparts in the model.

[38]To incorporate the serial correlation for individual strategies, we can model them as simple autoregressive processes. See Harvey and Liu (2014a) for further discussion of the kinds of correlation structures that our model is able to incorporate. See Sun and Cai (2008) for an example that models the spatial dependence among the null hypotheses.

[39]Our choice of the threshold t-statistic is smaller than the 2.57 threshold in Appendix B. This allows us to observe false discoveries that overcome the threshold more frequently than under 2.57. This is important for the estimation of p_0 in the model. For more details on the selection of the threshold t-statistic, see Harvey and Liu (2014a).

The sample statistics we choose to match are the quantiles of the sample of t-statistics and the sample size (i.e., the total number of discoveries). Two concerns motivate us to use quantiles. First, sample quantiles are less susceptible to outliers compared to means and other moment-related sample statistics. Our t-statistic sample does have a few influential observations and we expect quantiles to be more useful descriptive statistics than the mean and the standard deviation. Second, simulation studies show that quantiles in our model are more sensitive to changes in parameters than other statistics. To offer a more efficient estimation of the model, we choose to focus on quantiles.

In particular, the quantities we choose to match and their values for the baseline sample (i.e., $r = 1/2$) are given by:

$$\begin{cases} \widehat{T} = \text{Total number of discoveries} = 353, \\ \widehat{Q}_1 = \text{The 20th percentile of the sample of t-statistics} = 2.39, \\ \widehat{Q}_2 = \text{The 50th percentile of the sample of t-statistics} = 3.16, \\ \widehat{Q}_3 = \text{The 90th percentile of the sample of t-statistics} = 6.34. \end{cases}$$

These three quantiles are representative of the spectrum of quantiles and can be shown to be most sensitive to parameter changes in our model. Fixing the model parameters, we can also obtain the model implied sample statistics T, Q_1, Q_2, and Q_3 through simulations.[40] The estimation works by seeking to find the set of parameters that minimizes the following objective function:

$$D(\lambda, p_0, M, \rho) = w_0(T - \widehat{T})^2 + \sum_{i=1}^{3} w_i(Q_i - \widehat{Q}_i)^2,$$

where w_0 and $\{w_i\}_{i=1}^3$ are the weights associated with the squared distances. Motivated by the optimal weighting for the Generalized Method of Moments (GMM) estimators, we set these weights at $w_0 = 1$ and $w_1 = w_2 = w_3 = 10,000$. They can be shown to have the same magnitude as the inverses of the variances of the corresponding model implied sample statistics across a wide range of parameter values and should help improve estimation efficiency.[41]

We estimate the three parameters (λ, p_0, and M) in the model and choose to calibrate the correlation coefficient ρ. In particular, for a given level of correlation ρ,

[40]Model implied quantiles are difficult (and most likely infeasible) to calculate analytically. We obtain them through simulations. In particular, for a fixed set of parameters, we simulate 5,000 independent samples of t-statistics. For each sample, we calculate the four summary statistics. The median of these summary statistics across the 5,000 simulations are taken as the model implied statistics.

[41]We do not pursue a likelihood-based estimation. Given that we have more than a thousand factors and each of them is associated with an indicator variable that is missing, the likelihood function involves high-dimensional integrals and is difficult to optimize. This leads us to a GMM-based approach.

we numerically search for the model parameters (λ, p_0, M) that minimize the objective function $D(\lambda, p_0, M, \rho)$.

We choose to calibrate the amount of correlation because the correlation coefficient is likely to be weakly identified in this framework. Ideally, to have a better identification of ρ, we would like to have t-statistics that are generated from samples that have varying degrees of overlap.[42] We do not allow heterogeneity in sample periods in either our estimation framework (i.e., all t-statistics are generated from samples that cover the same period) or our data (we do not record the specific period for which the t-statistic is generated). As a result, our results are best interpreted as the estimated t-statistic thresholds for a hypothetical level of correlation.

To investigate how correlation affects multiple testing, we follow an intuitive simulation procedure. In particular, fixing λ, p_0 and M at their estimates, we know the data generating process for the cross-section of returns. Through simulations, we are able to calculate the previously defined Type I error rates (i.e., FWER and FDR) for any given threshold t-statistic. We search for the optimal threshold t-statistic that exactly achieves a pre-specified error rate.

5.2 Results

Our estimation framework assumes a balanced panel with M factors and N periods of returns. We need to assign a value to N. Published papers usually cover a period ranging from twenty to fifty years. In our framework, the choice of N does not affect the distribution of T_i under the null hypothesis (i.e., $\mu_i = 0$) but will affect T_i under the alternative hypothesis (i.e., $\mu_i > 0$). When μ_i is different from zero, T_i has a mean of $\mu_i/(\sigma/\sqrt{N})$. A larger N reduces the noise in returns and makes it more likely for T_i to be significant. To be conservative (i.e., less likely to generate significant t-statistics under the alternative hypotheses), we set N at 240 (i.e., twenty years). Other specifications of N change the estimate of λ but leave the other parameters almost intact. In particular, the threshold t-statistics are little changed for alternative values of N.

The results are presented in Table 5. Across different correlation levels, λ (the mean parameter for the exponential distribution that represents the mean returns for true factors) is consistently estimated at 0.55% per month. This corresponds to an annual factor return of 6.6%. Therefore, we estimate the average mean returns for truly significant factors to be 6.6% per annum. Given that we standardize factor

[42]Intuitively, t-statistics that are based on similar sample periods are more correlated than t-statistics that are based on distinct sample periods. Therefore, the degree of overlap in sample period helps identify the correlation coefficient. See Ferson and Chen (2013) for a similar argument on measuring the correlations among fund returns.

returns by an annual volatility of 15%, the average annual Sharpe ratio for these factors is 0.44 (or monthly Sharpe ratio of 0.13).[43]

Table 5: **Estimation Results: A Model with Correlations**

We estimate the model with correlations. r is the assumed proportion of missing factors with a t-statistic in between 1.96 and 2.57. Panel A shows the results for the baseline case when $r = 1/2$ and Panel B shows the results for the case when $r = 2/3$. ρ is the correlation coefficient between two strategy returns in the same period. p_0 is the probability of having a strategy that has a mean of zero. λ is the mean parameter of the exponential distribution for the monthly means of the true factors. M is the total number of trials.

				t-statistic			
ρ	p_0	$\lambda(\%)$	M	FWER(5%)	FWER(1%)	FDR(5%)	FDR(1%)
				Panel A: $r = 1/2$ (Baseline)			
0	0.396	0.550	1,297	3.89	4.28	2.16	2.88
0.2	0.444	0.555	1,378	3.91	4.30	2.27	2.95
0.4	0.485	0.554	1,477	3.81	4.23	2.34	3.05
0.6	0.601	0.555	1,775	3.67	4.15	2.43	3.09
0.8	0.840	0.560	3,110	3.35	3.89	2.59	3.25
				Panel B: $r = 2/3$ (more unobserved tests)			
0	0.683	0.550	2,458	4.17	4.55	2.69	3.30
0.2	0.722	0.551	2,696	4.15	4.54	2.76	3.38
0.4	0.773	0.552	3,031	4.06	4.45	2.80	3.40
0.6	0.885	0.562	4,339	3.86	4.29	2.91	3.55
0.8	0.922	0.532	5,392	3.44	4.00	2.75	3.39

For the other parameter estimates, both p_0 and M are increasing in ρ. Focusing on the baseline case in Panel A and at $\rho = 0$, we estimate that researchers have tried $M = 1297$ factors and 60.4% $(= 1 - 0.396)$ are true discoveries. When ρ is increased

[43]Our estimates are robust to the sample percentiles that we choose to match. For instance, fixing the level of correlation at 0.2, when we use the 10th together with the 50th and 90th percentiles of the sample of t-statistics, our parameter estimate is $(p_0, \lambda, M) = (0.390, 0.548, 1287)$. Alternatively, when we use the 80th together with the 20th and 50th percentiles of the sample of t-statistics, our parameter estimate is $(p_0, \lambda, M) = (0.514, 0.579, 1493)$. Both estimates are in the neighborhood of our baseline model estimates.

to 0.60, we estimate that a total of $M = 1775$ factors have been tried and around 39.9% ($= 1 - 0.601$) are true factors.

Turning to the estimates of threshold t-statistics and focusing on FWER, we see that they are not monotonic in the level of correlation. Intuitively, two forces are at work in driving these threshold t-statistics. On the one hand, both p_0 and M are increasing in the level of correlation. Therefore, more factors — both in absolute value and in proportion — are drawn from the null hypothesis. To control the occurrences of false discoveries based on these factors, we need a higher threshold t-statistic. On the other hand, a higher correlation among test statistics reduces the required threshold t-statistic. In the extreme case when all test statistics are perfectly correlated, we do not need multiple testing adjustment at all. These two forces work against each other and result in the non-monotonic pattern for the threshold t-statistics under FWER. For FDR, it appears that the impact of larger p_0 and M dominates so that the threshold t-statistics are increasing in the level of correlation.

Across various correlation specifications, our estimates show that in general a t-statistic of 3.9 and 3.0 is needed to control FWER at 5% and FDR at 1%, respectively.[44] Notice that these numbers are not far away from our previous estimates of 3.78 (Holm adjustment that controls FWER at 5%) and 3.38 (BHY adjustment that controls FDR at 1%). However, these similar numbers are generated through different mechanisms. Our current estimate assumes a certain level of correlation among returns and relies on an estimate of more than 1,300 for the total number of factor tests. On the other hand, our previous calculation assumes that the 316 published factors are all the factors that have been tried but does not specify a correlation structure.

5.3 How Large Is ρ?

Our sample has limitations in making a direct inference on the level of correlation. To give some guidance, we provide indirect evidence on the plausible levels of ρ.

First, the value of the optimized objective function sheds light on the level of ρ. Intuitively, a value of ρ that is more consistent with the data generating process should result in a lower optimized objective function. Across the various specifications of ρ in Table 5, we find that the optimized objective function reaches its lowest point when $\rho = 0.2$. Therefore, our t-statistic sample suggests a low level of correlation. However, this evidence is only suggestive given the weak identification of ρ in our model.

Second, we draw on external data source to provide inference. In particular, we analyze the S&P CAPITAL IQ database, which includes detailed information on the time-series of returns of over 400 factors for the U.S. equity market. We estimate

[44] To save space, we choose not to discuss the performance of our estimation method. Harvey and Liu (2014a) provide a detailed simulation study of our model.

the average pairwise correlation among these factors to be 0.15 over the 1985-2014 period.

Finally, existing studies in the literature provide guidance on the level of correlation. McLean and Pontiff (2014) estimate the correlation among anomaly returns to be around 0.05. Green, Hand and Zhang (2012) focus on accounting-based factors and find the average correlation to be between 0.06 and 0.20. Focusing on mutual fund returns, Barras, Scaillet and Wermers (2010) argue for a correlation of zero among fund returns (i.e., excess returns against benchmark factors) while Ferson and Chen (2013) calibrate this number to be between 0.04 and 0.09.

Overall, we believe that the average correlation among factor returns is in the neighborhood of 0.20.

5.4 How Many True Factors Are There?

The number of true discoveries using our method seems high given that most of us have a prior that there are only a handful of true systematic risk factors. However, many of these factors that our method deems statistically true have tiny Sharpe Ratios. For example, around 70% of them have a Sharpe Ratio that is less than 0.5 per annum. From a modeling perspective, we impose a monotonic exponential density for the mean returns of true factors. Hence, by assumption the number of discoveries will be decreasing in the mean return.

Overall, statistical evidence can only get us so far in reducing the number of false discoveries. This is a limitation not only to our framework but probably any statistical framework that relies on individual p-values. To see this, suppose the smallest t-statistic among true risk factors is 3.0 and assume our sample covers 50 risk factors that all have a t-statistic above 3.0. Then based on statistical evidence only, it is impossible to rule out any of these 50 factors from the list of true risk factors.

We agree that a further scrutiny of the factor universe is a valuable exercise. There are at least two routes we can take. One route is to introduce additional testable assumptions that a systematic risk factor has to satisfy to claim significance. Pukthuanthong and Roll (2014) use the principle components of the cross-section of realized returns to impose such assumptions. The other route is to incrementally increase the factor list by allowing different factors to crowd each other out. Harvey and Liu (2014c) provide such a framework. We expect both lines of research to help in culling the number of factors.

6 Conclusion

At least 316 factors have been tested to explain the cross-section of expected returns. Most of these factors have been proposed over the last ten years. Indeed, Cochrane (2011) refers to this as "a zoo of new factors". Our paper argues that it is a serious mistake to use the usual statistical significance cutoffs (e.g., a t-statistic exceeding 2.0) in asset pricing tests. Given the plethora of factors and the inevitable data mining, many of the historically discovered factors would be deemed "significant" by chance.

There is an important philosophical issue embedded in our approach. Our threshold cutoffs increase through time as more factors are data mined. However, data mining is not new. Why should we have a higher threshold for today's data mining than for data mining in the 1980s? We believe there are three reasons for a tougher criteria today. First, the low-hanging fruit has already been picked. That is, the rate of discovering a true factor has likely decreased. Second, there is a limited amount of data. Indeed, there is only so much you can do with the CRSP database. In contrast, in particle physics, it is routine to create trillions of new observations in an experiment. We do not have that luxury in finance. Third, the cost of data mining has dramatically decreased. In the past, data collection and estimation were time intensive so it was more likely that only factors with the highest priors — potentially based on economic first principles — were tried.

Our paper presents three conventional multiple testing frameworks and proposes a new one that particularly suits research in financial economics. While these frameworks differ in their assumptions, they are consistent in their conclusions. We argue that a newly discovered factor today should have a t-statistic that exceeds 3.0. We provide a time-series of recommended "cutoffs" from the first empirical test in 1967 through to present day. Many published factors fail to exceed our recommended cutoffs.

While a t-statistic of 3.0 (which corresponds to a p-value of 0.27%) seems like a very high hurdle, we also argue that there are good reasons to expect that 3.0 is too low. First, we only count factors that are published in prominent journals and we sample only a small fraction of the working papers. Second, there are surely many factors that were tried by empiricists, failed, and never made it to publication or even a working paper. Indeed, the culture in financial economics is to focus on the discovery of new factors. In contrast to other fields such as medical science, it is rare to publish replication studies just focusing on existing factors. Given that our count of 316 tested factors is surely too low, this means the t-statistic cutoff is likely even higher.[45]

[45]In astronomy and physics, even higher threshold t-statistics are often used to control for testing multiplicity. For instance, the high profile discovery of Higgs Boson has a t-statistic of more than 5 (p-value less than 0.0001%). See ATLAS Collaboration (2012), CMS Collaboration (2012), and Harvey and Liu (2014b).

Should a t-statistic of 3.0 be used for every factor proposed in the future? Probably not. A case can be made that a factor developed from first principles should have a lower threshold t-statistic than a factor that is discovered as a purely empirical exercise. Nevertheless, a t-statistic of 2.0 is no longer appropriate — even for factors that are derived from theory.

In medical research, the recognition of the multiple testing problem has led to the disturbing conclusion that "most claimed research findings are false" (Ioannidis (2005)). Our analysis of factor discoveries leads to the same conclusion – many of the factors discovered in the field of finance are likely false discoveries: of the 296 published significant factors, 158 would be considered false discoveries under Bonferonni, 142 under Holm, 132 under BHY (1%) and 80 under BHY (5%). In addition, the idea that there are so many factors is inconsistent with the principal component analysis, where, perhaps there are five "statistical" common factors driving time-series variation in equity returns (Ahn, Horenstein and Wang (2012)).

The assumption that researchers follow the rules of classical statistics (e.g., randomization, unbiased reporting, etc.) is at odds with the notion of individual incentives which, ironically, is one of the fundamental premises in economics. Importantly, the optimal amount of data mining is not zero since some data mining produces knowledge. The key, as argued by Glaeser (2008), is to design appropriate statistical methods to adjust for biases, not to eliminate research initiatives. The multiple testing framework detailed in our paper is true to this advice.

Our research quantifies the warnings of both Fama (1991) and Schwert (2003). We attempt to navigate the zoo and establish new benchmarks to guide empirical asset pricing tests.

Table 6: **Factor List: Factors Sorted by Year**

An augmented version of this table is available for download and resorting. The main table includes full citations as well as hyperlinks to each of the cited articles. See http://faculty.fuqua.duke.edu/˜charvey/Factor-List.xlsx.
Many of the working papers we cite have been published but given our method depends on point in time, we cite only the working paper version.

Year	**Common #**	**Char. #**	**Factor**	**Formation**	**Type**	**Journal**	**Short reference**
1964			Market return	THEORY	Common financial	*Journal of Finance*	Sharpe (1964)
1965			Market return	THEORY	Common financial	*Journal of Finance*	Lintner (1965)
1966			Market return	THEORY	Common financial	*Econometrica*	Mossin (1966)
1967		1	Total volatility	Individual stock return volatility	Char. financial	*Yale Economic Essays*	Douglas (1967)
1972			Market return	THEORY	Common financial	*Journal of Finance*	Heckerman (1972)
			Relative prices of consumption goods	THEORY	Common macro		
1972			Market return	Equity index return	Common financial	*Studies in the Theory of Capital Markets*	Black, Jensen and Scholes (1972)^a
1972			Market return	THEORY	Common financial	*Journal of Business*	Black (1972)
1973			State variables representing future investment opportunity	THEORY	Common financial and macro	*Econometrica*	Merton (1973)
1973	1		Market return	Equity index return	Common financial	*Journal of Political Economy*	Fama and MacBeth (1973)
	2		Beta squared*	Square of market beta	Common financial		
		2	Idiosyncratic volatility*	Residual stock volatility from CAPM	Char. financial		
1973			High order market return	THEORY	Common financial	*Journal of Financial and Quantitative Analysis*	Rubinstein (1973)
1974			World market return	THEORY	Common financial	*Journal of Economic Theory*	Solnik (1974)
1974			Individual investor resources	THEORY	Common financial	*Journal of Financial Economics*	Rubinstein (1974)
1975		3	Earnings growth expectations	Projecting firm earnings growth based on market beta, firm size, dividend payout ratio, leverage and earnings variability	Char. accounting	*Journal of Finance*	Gupta and Ofer (1975)
1976			Market return†	Equity index return	Common financial	*Journal of Finance*	Kraus and Litzenberger (1976)
	3		Squared market return*	Square of equity index return	Common financial		
1977		4	PE ratio	Firm price-to-earnings ratio	Char. accounting	*Journal of Finance*	Basu (1977)
1978			Marginal rate of substitution	THEORY	Common macro	*Econometrica*	Lucas (1978)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
1979		5	Dividend yield	Dividend per share divided by share price	Char. accounting	*Journal of Financial Economics*	Litzenberger and Ramaswamy (1979)
			Market return†	Equity index return	Common financial		
1979			Aggregate real consumption growth	THEORY	Common macro	*Journal of Financial Economics*	Breeden (1979)
1980			Short sale restrictions	THEORY	Char. microstructure	*Journal of Finance*	Jarrow (1980)
1981			Market return†‡	Equity index return	Common financial	*Journal of Finance*	Fogler, John and Tipton (1981)[b]
			Treasury bond return‡	3-month US Treasury bill return	Common financial		
			Corporate bond return‡	Index of long-term Aa utility bonds with deferred calls returns	Common financial		
1981	4		Treasury bill return	Principal components extracted from returns of Treasury bills	Common financial	*Journal of Finance*	Oldfield and Rogalski (1981)
1981			World consumption	THEORY	Common macro	*Journal of Financial Economics*	Stulz (1981)
1981			Transaction costs	THEORY	Char. microstructure	*Journal of Finance*	Mayshar (1981)
1981		6	Firm size	Market value of firm stocks	Char. financial	*Journal of Financial Economics*	Banz (1981)
1981		7	Short interest	Equity short interest	Char. microstructure	*Journal of Financial and Quantitative Analysis*	Figlewski (1981)
1982			Individual consumer's wealth	THEORY	Common financial	*Journal of Business*	Constantinides (1982)
1983		8	EP ratio	Firm earnings-to-price ratio	Char. accounting	*Journal of Financial Economics*	Basu (1983)
1983			Foreign exchange rate change	THEORY	Common financial	*Journal of Finance*	Adler and Dumas (1983)
1983			Institutional holding‡	Institutional concentration rankings from Standard and Poor's	Char. other	*Financial Analyst Journal*	Arbel, Carvell and Strebel (1983)
1984			Earnings expectations‡	Consensus earnings expectations	Char. accounting	*Financial Analyst Journal*	Hawkins, Chamberlin and Daniel (1984)
1984			New listings announcement‡	Announcement that a company has filed a formal application to list on the NYSE	Char. accounting	*Financial Analyst Journal*	McConnell and Sanger (1984)
1985			Market return†	Equity index return	Common financial	*Journal of Financial Economics*	Chan, Chen and Hsieh (1985)
	5		Industrial production growth	Seasonally adjusted monthly growth rate of industrial production	Common macro		
	6		Change in expected inflation*	Change in expected inflation as defined in Fama and Gibbons (1984)	Common macro		

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Year	#	#	Factor	Formation	Type	Journal	Short reference
	7		Unanticipated inflation	Realized minus expected inflation	Common macro		
	8		Credit premium	Risk premium measured as difference in return between "under Baa" bond portfolio and long-term government bond portfolio	Common financial		
	9		Term structure*	Yield curve slope measured as difference in return between long-term government bond and 1-month Treasury bill	Common financial		
1985		9	Long-term return reversal	Long-term past abnormal return	Char. other	*Journal of Finance*	Bondt and Thaler (1985)
1985			Investment opportunity change	THEORY	Common financial	*Econometrica*	Cox, Ingersoll and Ross (1985)
1986			Transaction costs	THEORY	Common microstructure	*Journal of Financial Economics*	Amihud and Mendelson (1986)
1986			Transaction costs	THEORY	Common microstructure	*Journal of Political Economy*	Constantinides (1986)
1986			Expected inflation	THEORY	Common macro	*Journal of Finance*	Stulz (1986)
1986	10		Long-term interest rate	Change in the yield of long-term government bonds	Common financial	*Journal of Finance*	Sweeney and Warga (1986)
1986			Industrial production growth†	Seasonally adjusted monthly growth rate of industrial production	Common macro	*Journal of Business*	Chen, Roll and Ross (1986)
			Credit premium†	Risk premium measured as difference in return between "under Baa" bond portfolio and long-term government bond portfolio	Common financial		
			Term structure†	Yield curve slope measured as difference in return between long-term government bond and 1-month Treasury bill	Common financial		
			Unanticipated inflation†	Realized minus expected inflation	Common macro		
			Change in expected inflation†	Changes in expected inflation as defined in Fama and Gibbons (1984)	Common macro		
	11		Change in oil prices*	Growth rate in oil prices	Common macro		
1988		10	Debt to equity ratio	Non-common equity liabilities to equity	Char. accounting	*Journal of Finance*	Bhandari (1988)
1988			Long-term growth forecasts‡	Long-term growth forecasts proxied by the five-year earnings per share growth rate forecasts	Char. accounting	*Financial Analyst Journal*	Bauman and Dowen (1988)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
1989	12		Consumption growth	Per capita real consumption growth	Common macro	*Journal of Finance*	Breeden, Gibbons and Litzenberger (1989)
1989		11	Illiquidity	Illiquidity proxied by bid-ask spread	Char. microstructure	*Journal of Finance*	Amihud and Mendelson (1989)
1989		12	Predicted earnings change	Predicted earnings change in one year based on a financial statement analysis that combines a large set of financial statement items	Char. accounting	*Journal of Accounting & Economics*	Ou and Penman (1989)
1990		13	Return predictability	Short-term (one month) and long-term (twelve months) serial correlations in returns	Char. financial	*Journal of Finance*	Jegadeesh (1990)
1991			Market return†	Equity index return	Common financial	*Journal of Political Economy*	Ferson and Harvey (1991)
			Consumption growth†	Real per capita growth of personal consumption expenditures for non-durables & services	Common macro		
			Credit spread†	Baa corporate bond return less monthly long-term government bond return	Common financial		
	13		Change in the slope of the yield curve	Change in the difference between a 10-year Treasury bond yield and a 3-month Treasury bill yield	Common financial		
			Unexpected inflation†	Difference between actual and time-series forecasts of inflation rate	Common macro		
	14		Real short rate	One-month Treasury bill return less inflation rate	Common financial		
1992	15		Size	Return on a zero-investment portfolio long in small stocks and short in large stocks	Common accounting	*Journal of Finance*	Fama and French (1992)^c
	16		Value	Return on a zero-investment portfolio long in growth stocks and short in value stocks	Common accounting		
1992			Return momentum‡	Size and beta adjusted mean prior five-year returns	Char. financial	*Journal of Financial Economics*	Chopra, Lakonishok and Ritter (1992)
1992			Predicted return signs‡	Return signs predicted by a logit model using financial ratios	Char. accounting	*Journal of Accounting & Economics*	Holthausen and Larcker (1992)
1993		14	Return momentum	Past stock returns	Char. other	*Journal of Finance*	Jegadeesh and Titman (1993)
1993			Returns on S&P stocks‡	Returns on S&P stocks	Common financial	*Review of Financial Studies*	Elton, Gruber, Das and Hlavka (1993)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
			Returns on non-S&P stocks‡	Returns on non-S&P stocks	Common financial		
1993			High order market and bond return‡	High order equity index returns and bond returns	Common financial	*Journal of Finance*	Bansal and Viswanathan (1993)[d]
1993			Market return†	Equity index return	Common financial	*Journal of Financial Economics*	Fama and French (1993)
			Size†	Return on a zero-investment portfolio long in small stocks and short in large stocks	Common accounting		
			Value†	Return on a zero-investment portfolio long in growth stocks and short in value stocks	Common accounting		
			Term structure†	Difference in return between long-term government bond and one-month Treasury bill	Common financial		
			Credit risk†	Difference in return between long-term corporate bond and long-term government bond	Common financial		
1993			World equity return‡	US dollar return of the MSCI world equity market in excess of a short-term interest rate	Common financial	*Review of Financial Studies*	Ferson and Harvey (1993)[e]
			Change in weighted exchange rate‡	Log first difference of the trade-weighted US dollar price of ten industrialized countries' currencies	Common financial		
			Change in long-term inflationary expectations‡	Change in long-term inflationary expectations	Common macro		
			Weighted real short-term interest rate‡	GDP weighted average of short-term interest rates in G-7 countries	Common financial		
			Change in oil price†‡	Change in the monthly average US dollar price per barrel of crude oil	Common macro		
			Change in the Eurodollar-Treasury yield spread‡	First difference of the spread between the 90-day Eurodollar yield and the 90-day Treasury-bill yield	Common financial		
			Change in G-7 industrial production‡	Change in G-7 industrial production	Common macro		
			Unexpected inflation for the G-7 countries‡	Unexpected inflation based on a time-series model on an aggregate G-7 inflation rate	Common macro		
1994	17		World equity return	US dollar return of the MSCI world equity market in excess of a short-term interest rate	Common financial	*Journal of Banking and Finance*	Ferson and Harvey (1994)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
	18		Change in weighted exchange rate*	Log first difference of the trade-weighted US dollar price of ten industrialized countries' currencies	Common financial		
	19		Change in long-term inflationary expectations*	Change in long-term inflationary expectations	Common macro		
			Change in oil price*†	Change in the monthly average US dollar price per barrel of crude oil	Common macro		
1994	20		Tax rate for capital gains	Short-term capital gains tax rate	Common accounting	*Journal of Finance*	Bossaerts and Dammon (1994)
	21		Tax rate for dividend	Dividend tax rate	Common accounting		
1995	22		Change in expected inflation	Change in expectation from economic surveys	Common macro	*Journal of Finance*	Elton, Gruber and Blake (1995)
	23		Change in expected GNP	Change in expectation from economic surveys	Common macro		
1995		15	New public stock issuance	New public stock issuance	Char. accounting	*Journal of Finance*	Loughran and Ritter (1995)
1995		16	Dividend initiations	Initiations of cash dividend payments	Char. financial	*Journal of Finance*	Michaely, Thaler and Womack (1995)
		17	Dividend omissions	Omissions of cash dividend payments	Char. financial		
1995			Seasoned equity offerings‡	Whether a firm makes seasoned equity offerings	Char. financial	*Journal of Financial Economics*	Spiess and Affleck-Graves (1995)
1996	24		Money growth	M2 or M3 minus currency, divided by total population	Common macro	*Journal of Finance*	Chan, Foresi and Lang (1996)
1996	25		Returns on physical investment	Inferred from investment data via a production function	Common macro	*Journal of Political Economy*	Cochrane (1996)
1996			Market return†	Equity index return	Common financial	*Journal of Political Economy*	Campbell (1996)
	26		Labor income	Real labor income growth rate	Common macro		
			Dividend yield†	Dividend yield on value-weighted index	Common financial		
			Interest rate†	Treasury bill rate less 1-year moving average	Common financial		
			Term structure†	Long-short government bond yield spread	Common financial		
1996			Market return†	Equity index return	Common financial	*Journal of Finance*	Jagannathan and Wang (1996)
			Slope of yield curve†	Long-short government bond yield spread	Common financial		

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Year	#	#	Factor	Formation	Type	Journal	Short reference
			Labor income†	Real labor income growth rate	Common macro		
1996		18	Earnings forecasts	Errors in analysts' forecasts on earnings growth	Char. accounting	*Journal of Finance*	Porta (1996)
1996		19	R&D capital	R&D capital over total assets	Char. accounting	*Journal of Accounting & Economics*	Lev and Sougiannis (1996)
1996		20	Accruals	Accruals defined by the change in non-cash current assets, less the change in current liabilities, less depreciation expense, all divided by average total assets	Char. accounting	*Accounting Review*	Sloan (1996)
1996		21	Buy recommendations	Buy recommendations from security analysts	Char. financial	*Journal of Finance*	Womack (1996)
		22	Sell recommendations	Sell recommendations from security analysts	Char. financial		
1996		23	Credit rating	Institutional investor country credit rating from semi-annual survey	Char. other	*Journal of Portfolio Management*	Erb, Harvey and Viskanta (1996)
1996		24	Illiquidity	Derivative transaction price with respect to signed trade size	Char. microstructure	*Journal of Financial Economics*	Brennan and Subrahmanyam (1996)
1997			Nonlinear functions of consumption growth‡	Low order orthonormal polynomials of current and future consumption growth	Common macro	*Journal of Finance*	Chapman (1997)[f]
1997			Opportunistic strategy return‡	Return for hedge funds that follow an opportunistic strategy	Common financial	*Review of Financial Studies*	Fung and Hsieh (1997)[g]
			Global/macro strategy return‡	Return for hedge funds that follow a global/macro strategy	Common financial		
			Value strategy return‡	Return for hedge funds that follow a value strategy	Common financial		
			Trend following strategy return‡	Return for hedge funds that follow a trend following strategy	Common financial		
			Distressed investment strategy return‡	Return for hedge funds that follow a distressed investment strategy	Common financial		
1997			Size†	Return on a zero-investment portfolio long in small stocks and short in large stocks	Common accounting	*Journal of Finance*	Carhart (1997)
			Value†	Return on a zero-investment portfolio long in growth stocks and short in value stocks	Common accounting		
			Market return†	Equity index return	Common financial		

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Year	#	#	Factor	Formation	Type	Journal	Short reference
	27		Momentum	Return on a zero-investment portfolio long in past winners and short in past losers	Common other		
1997			Size†	Market value of equity	Char. accounting	*Journal of Financial Economics*	Brennan, Chordia and Subrahmanyam (1997)
			Book-to-market ratio†	Book value of equity plus deferred taxes to market value of equity	Char. accounting		
			Momentum†	Past cumulative stock return	Char. financial		
		25	Trading volume	Dollar volume traded per month	Char. microstructure		
1997		26	Disclosure level	Voluntary disclosure level of manufacturing firms' annual reports	Char. accounting	*Accounting Review*	Botosan (1997)
1997		27	Earnings forecast uncertainty	Standard deviation of earnings forecasts	Char. accounting	*Journal of Financial Research*	Ackert and Athanassakos (1997)
1997			Size†	Market value of equity	Char. accounting	*Journal of Finance*	Daniel and Titman (1997)
			Value†	Book value of equity plus deferred taxes to market value of equity	Char. accounting		
1997			Earnings management likelihood‡	Earnings management likelihood obtained by regressiong realized violators of Generally Accepted Accounting Principles on firm characteristics	Char. accounting	*Journal of Accounting and Public Policy*	Beneish (1997)
1997		28	Corporate acquisitions	Difference between stock mergers and cash tender offers for corporate acquisitions	Char. financial	*Journal of Finance*	Loughran and Vijh (1997)
1998			Fundamental analysis‡	Investment signals constructed using a collection of variables that relate to contemporaneous changes in inventories, accounts receivables, gross margins, selling expenses, capital expenditures, effective tax rates, inventory methods, audit qualifications, and labor force sales productivity	Char. accounting	*Accounting Review*	Abarbanell and Bushee (1998)
1998			Firm fundamental value‡	Firms' fundamental values estimated from I/B/E/S consensus forecasts and a residual income model	Char. accounting	*Journal of Accounting and Economics*	Frankel and Lee (1998)
1998		29	Bankruptcy risk	The probability of bankruptcy from Altman (1968)	Char. financial	*Journal of Finance*	Ilia (1998)
1998		30	Illiquidity	Liquidity proxied by the turnover rate: number of shares traded as a fraction of the number of shares outstanding	Char. microstructure	*Journal of Financial Markets*	Datar, Naik and Radcliffe (1998)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
1999	28		Fitted return based on predictive regressions	Expected portfolio return obtained by projecting historical returns on lagged macro instruments, including term spreads, dividend yield, credit spread and short-term Treasury bill	Common financial	*Journal of Finance*	Ferson and Harvey (1999)
1999		31	Industry momentum	Industry-wide momentum returns	Char. other	*Journal of Finance*	Moskowitz and Grinblatt (1999)
1999			Debt offerings‡	Whether a firm makes straight and convertible debt offerings	Char. financial	*Journal of Financial Economics*	Spiess and Affleck-Graves (1999)
2000	29		Entrepreneur income	Proprietary income of entrepreneurs	Common financial	*Journal of Finance*	Heaton and Lucas (2000)
2000	30		Coskewness	Excess return on a portfolio which long stocks with low past coskewness	Common financial	*Journal of Finance*	Harvey and Siddique (2000)
2000		32	Trading volume	Past trading volume	Char. microstructure	*Journal of Finance*	Lee and Swaminathan (2000)
2000		33	Within-industry size	Difference between firm size and average firm size within the industry	Char. financial	*Working Paper*	Asness, Porter and Stevens (2000)
		34	Within-industry value	Difference between firm book-to-market ratio and average book-to-market ratio within the industry	Char. accounting		
		35	Within-industry cashflow to price ratio	Difference between firm cashflow to price ratio and average cashflow to price ratio within the industry	Char. accounting		
		36	Within-industry percent change in employees	Difference between firm percent change in employees and average percent change in employees within the industry	Char. accounting		
		37	Within-industry momentum	Difference between firm past stock prices and average past stock prices within the industry	Char. financial		
2000		38	Financial statement information	A composite score based on historical financial statement that separates winners from losers	Char. accounting	*Journal of Accounting Research*	Piotroski (2000)
2001			Consumption growth†	Per capita real consumption growth rate	Common macro	*Journal of Political Economy*	Lettau and Ludvigson (2001)
	31		Consumption-wealth ratio	Proxied by a weighted average of human and nonhuman wealth	Common macro		
2001		39	Level of liquidity	Level of dollar trading volume and share turnover	Char. microstructure	*Journal of Financial Economics*	Chordia, Subrahmanyam and Anshuman (2001)
		40	Variability of liquidity	Volatility of dollar trading volume and share turnover	Char. microstructure		

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Year	#	#	Factor	Formation	Type	Journal	Short reference
2001		41	Financial constraints	Measure financial constraints with Kaplan and Zingales (1997) index	Char. financial	*Review of Financial Studies*	Lamont, Polk and Saa-Requejo (2001)
2001			Straddle return‡	Lookback straddles' returns constructed based on option prices	Common financial	*Review of Financial Studies*	Fung and Hsieh (2001)
2001			Consensus recommendations*	Consensus recommendations measured by the average analyst recommendations	Char. accounting	*Journal of Finance*	Barber, Lehavy, McNichols and Trueman (2001)
2001		42	Bond rating changes	Moody's bond ratings changes	Char. financial	*Journal of Finance*	Dichev and Piotroski (2001)
2001		43	Analysts' forecasts	Financial analysts' forecasts of annual earnings	Char. accounting	*Accounting Review*	Pieter, Lo and Pfeiffer (2001)
2001		44	Institutional ownership	Institutional holdings of firm assets	Char. accounting	*Quarterly Journal of Economics*	Gompers and Metrick (2001)
2002			Market return†	Equity index return	Common financial	*Journal of Finance*	Dittmar (2002)
			Squared market return†	Squared equity index return	Common financial		
			Labor income growth†	Smoothed labor income growth rate	Common financial		
	32		Squared labor income growth	Squared smoothed labor income growth rate	Common financial		
2002		45	Distress risk	Distress risk as proxied by Ohlson's O-score	Char. financial	*Journal of Finance*	Griffin and Lemmon (2002)
2002		46	Analyst dispersion	Dispersion in analysts' earnings forecasts	Char. behavioral	*Journal of Finance*	Diether, Malloy and Scherbina (2002)
2002		47	Breadth of ownership	Ratio of the number of mutual funds holding long positions in the stock to total number of mutual funds	Char. microstructure	*Journal of Financial Economics*	Chen, Hong and Stein (2002)
2002		48	Information risk	Probability of information-based trading for individual stock	Char. microstructure	*Journal of Finance*	Easley, Hvidkjaer and O'Hara (2002)
2002		49	Short-sale constraints	Shorting costs for NYSE stocks	Char. microstructure	*Journal of Financial Economics*	Jones and Lamont (2002)
2002		50	Earnings sustainability	A summary score based on firm fundamentals that informs about the sustainability of earning	Char. accounting	*Working Paper*	Penman and Zhang (2002)
2002	33		Market illiquidity	Average over the year of the daily ratio of the stock's absolute return to its dollar trading volume	Common microstructure	*Journal of Financial Markets*	Amihud (2002)
2003	34		GDP growth news	GDP growth news obtained from predictive regressions on lagged equity and fixed-income portfolios	Common macro	*Journal of Financial Economics*	Vassalou (2003)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
2003	35		Market liquidity	Aggregated liquidity based on firm future excess stock return regressed on current signed excess return times trading volume	Common microstructure	*Journal of Political Economy*	Pastor and Stambaugh (2003)
2003			Idiosyncratic return volatility†	Residual variance obtained by regressing daily stock returns on market index return	Char. financial	*Journal of Financial Economics*	Ali, Hwang and Trombley (2003)
			Transaction costs†	Bid-ask spread, volume, etc.	Char. microstructure		
			Investor sophistication†	Number of analysts or institutional owners	Char. accounting		
2003		51	Shareholder rights	Shareholder rights as proxied by an index using 24 governance rules	Char. accounting	*Quarterly Journal of Economics*	Gompers, Ishii and Metrick (2003)
2003		52	Excluded expenses	Excluded expenses in firm's earnings reports	Char. accounting	*Review of Accounting Studies*	Jeffrey, Lundholm and Soliman (2003)
2003		53	Growth in long-term net operating assets	Growth in long-term net operating assets	Char. accounting	*Accounting Review*	Fairfield, Whisenant and Yohn (2003)
2003		54	Order backlog	Order backlog divided by average total assets, transformed to a scaled-decile variable	Char. accounting	*Review of Accounting Studies*	Rajgopal, Shevlin and Venkatachalam (2003)
2003		55	Return consistency	Consecutive returns with the same sign	Char. financial	*Journal of Behavioral Finance*	Watkins (2003)
2004	36		Idiosyncratic consumption	Cross-sectional consumption growth variance	Common macro	*Journal of Finance*	Jacobs and Wang (2004)
2004	37		Cash flow news	News about future market cash flow	Common financial	*American Economic Review*	Campbell and Vuolteenaho (2004)
	38		Discount rate news	News about future market discount rate	Common financial		
2004			Market return†	Equity index return	Common financial	*Review of Financial Studies*	Vanden (2004)^h
	39		Index option returns	Return on S&P 500 index option	Common financial		
2004	40		Default risk	Firm default likelihood using Merton's option pricing model	Common financial	*Journal of Finance*	Vassalou and Xing (2004)
2004	41		Real interest rate	Real interest rates extracted from a time-series model of bond yields and expected inflation	Common financial	*Journal of Finance*	Brennan, Wang and Xia (2004)
	42		Maximum Sharpe ratio portfolio	Maximum Sharpe ratio portfolio extracted from a time-series model of bond yields and expected inflation	Common financial		

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Year	#	#	Factor	Formation	Type	Journal	Short reference
2004	43		Return reversals at the style level	Zero-investment portfolios sorted based on past return performance at the style level	Common other	*Journal of Financial Economics*	Teo and Woo (2004)
2004		56	Unexpected change in R&D	Unexpected change in firm research and expenditures	Char. accounting	*Journal of Finance*	Allan, Maxwell and Siddique (2004)
2004		57	52-week high	Nearness to the 52-week high price	Char. financial	*Journal of Finance*	George and Hwang (2004)
2004		58	Analysts' recommendations	Consensus analysts' recommendations from sell-side firms	Char. accounting	*Journal of Finance*	Jegadeesh, Kim, Krische and Lee (2004)
2004		59	Put-call parity	Violations of put-call parity	Char. financial	*Journal of Financial Economics*	Ofek, Richardson and Whitelaw (2004)
2004		60	Abnormal capital investment	Past year capital expenditures scaled by average capital expenditures for previous three years	Char. accounting	*Journal of Financial and Quantitative Analysis*	Titman, Wei and Xie (2004)
2004		61	Balance sheet optimism	Net operating assets scaled by total assets	Char. accounting	*Journal of Accounting and Economics*	Hirshleifer, Hou, Teoh and Zhang (2004)
2005	44		Long-horizon consumption growth	Three-year consumption growth rate	Common macro	*Journal of Political Economy*	Parker and Julliard (2005)
2005	45		Long-run consumption	Cash flow risk measured by cointegration residual with aggregate consumption	Common macro	*Journal of Finance*	Bansal, Dittmar and Lundblad (2005)
2005	46		Housing price ratio	Ratio of housing to human wealth	Common financial	*Journal of Finance*	Lustig and Nieuwerburgh (2005)
2005		62	External corporate governance	Proxies for corporate control	Char. accounting	*Journal of Finance*	Cremers and Nair (2005)
		63	Internal corporate governance	Proxies for share-holder activism	Char. accounting		
2005			Market return[†]	Equity index return	Common financial	*Journal of Financial Economics*	Acharya and Pedersen (2005)[i]
	47		Market liquidity*	Value-weighted individual stock illiquidity as defined in Amihud (2002)	Common microstructure		
		64	Individual stock liquidity	Individual stock illiquidity as defined in Amihud (2002)	Char. microstructure		
2005		65	Price delay	Delay in a stock price's response to information	Char. microstructure	*Review of Financial Studies*	Hou and Moskowitz (2005)
2005		66	Heterogeneous beliefs	Factors constructed from disagreement among analysts about expected short- and long-term earnings	Char. financial	*Review of Financial Studies*	Anderson, Ghysels and Juergens (2005)
2005		67	Short-sale constraints	Short-sale constraint proxied by Institutional ownership	Char. microstructure	*Journal of Financial Economics*	Nagel (2005)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
2005		68	Short-sale constraints	Short-sale constraint proxied by short interest and institutional ownership	Char. microstructure	*Journal of Financial Economics*	Asquith, Pathak and Ritter (2005)
2005		69	Patent citation	Change of patent citation impact deflated by average total assets	Char. other	*Journal of Accounting, Auditing & Finance*	Gu (2005)
2005		70	Information uncertainty	Information uncertainty proxied by firm age, return volatility, trading volume or cash flow duration	Char. financial	*Review of Accounting Studies*	Jiang, Lee and Zhang (2005)
2005		71	Adjusted R&D	Adjusted R&D that incorporates capitalization and amortization	Char. accounting	*Working Paper*	Lev, Nissim and Thomas (2005)
2005		72	R&D reporting biases	R&D reporting biases proxied by the difference between R&D growth and earnings growth	Char. accounting	*Contemporary Accounting Research*	Lev, Sarath and Sougiannis (2005)
2005		73	Growth index	A combined index constructed based on earnings, cash flows, earnings stability, growth stability and intensity of R&D, capital expenditure and advertising	Char. accounting	Review of Accounting Studies	Mohanram (2005)
2006			Market return†	Equity index return and its square	Common financial	*Review of Financial Studies*	Vanden (2006)^j
			Index option return†	Index option return and its square	Common financial		
			Interaction between index and option return‡	Product of market and option returns	Common financial		
2006	48		Financing frictions	Default premium	Common financial	*Review of Financial Studies*	Gomes, Yaron and Zhang (2006)
2006	49		Investment growth by households*	Household investment growth	Common macro	*Journal of Business*	Li, Vassalou and Xing (2006)
	50		Investment growth by nonfarm nonfinancial corporate firms	Nonfarm nonfinancial corporate firms investment growth	Common macro		
	51		Investment growth by nonfarm noncorporate business	Nonfarm noncorporate business investment growth	Common macro		
	52		Investment growth by financial firms	Financial firms investment growth	Common macro		
2006			Third to tenth power of market return‡	Third to tenth power of market return	Common financial	*Journal of Business*	Chung, Johnson and Schill (2006)^k
2006		74	Financial constraints	Constraint index estimated from a firm's investment Euler equation	Char. financial	*Review of Financial Studies*	Whited and Wu (2006)
2006	53		Downside risk	Correlation with index return conditional on index return being below a threshold value	Common financial	*Review of Financial Studies*	Ang, Chen and Xing (2006)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
2006	54		Systematic volatility	Aggregate volatility relative to Fama and French (1992) three-factor model	Common financial	*Journal of Finance*	Ang, Hodrick, Xing and Zhang (2006)
		75	Idiosyncratic volatility	Idiosyncratic volatility relative to Fama and French (1992) three-factor model	Char. financial		
2006	55		Investor sentiment	Composite sentiment index based on various sentiment measures	Common behavioral	*Journal of Finance*	Baker and Wurgler (2006)
2006	56		Retail investor sentiment	Systematic retail trading based on transaction data	Common behavioral	*Journal of Finance*	Kumar and Lee (2006)
2006	57		Durable and nondurable consumption growth	Durable and nondurable consumption growth	Common macro	*Journal of Finance*	Yogo (2006)
2006			Market return†	Equity index return	Common financial	*Journal of Finance*	Lo and Wang (2006)
	58		Trading volume	Return on a hedge portfolio constructed using trading volume and market returns	Common microstructure		
2006	59		Liquidity	Market-wide liquidity constructed first by decomposing firm-level liquidity into variable and fixed price effects then averaging the variable component	Common microstructure	*Journal of Financial Economics*	Sadka (2006)
2006	60		Earnings	Return on a zero-investment portfolio long in stocks with high earnings surprises and short in stocks with low earnings surprises	Common accounting	*Journal of Financial Economics*	Chordia and Shivakumar (2006)
2006	61		Liquidity	Turnover-adjusted number of days with zero trading over the prior 12 months	Common microstructure	*Journal of Financial Economics*	Liu (2006)
2006		76	Capital investment	Capital expenditure growth	Char. accounting	*Journal of Finance*	Anderson and Garcia-Feijoo (2006)
2006		77	Industry concentration	Industry concentration as proxied by the Herfindahl index	Char. accounting	*Journal of Finance*	Hou and Robinson (2006)
2006		78	Environment indicator*	A composite index measuring a firm's environmental responsibility	Char. other	*Financial Management*	Brammer, Brooks and Pavelin (2006)
		79	Employment indicator*	A composite index measuring employee responsibility	Char. other		
		80	Community indicator*	A composite index measuring community responsiveness	Char. other		
2006		81	Intangible information	Residuals from cross-sectional regression of firm returns on fundamental growth measures	Char. accounting	*Journal of Finance*	Daniel and Titman (2006)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
2006		82	Profitability	Expected earnings growth	Char. accounting	*Journal of Financial Economics*	Fama and French (2006)
		83	Investment*	Expected growth in book equity	Char. accounting		
			Book-to-market†	Book value of equity plus deferred taxes to market value of equity	Char. accounting		
2006		84	Net financing	Net amount of cash flow received from external financing	Char. accounting	*Journal of Accounting and Economics*	Bradshaw, Richardson and Sloan (2006)
2006		85	Forecasted earnings per share	Analysts' forecasted earnings per share	Char. accounting	*Working Paper*	Cen, Wei and Zhang (2006)
2006		86	Pension plan funding	Pension plan funding status calculated as the difference between the fair value of plan assets and the projected benefit obligation, divided by market capitalization	Char. accounting	*Journal of Finance*	Franzoni and Marin (2006)
2006		87	Acceleration	Firm's ranking on change in six-month momentum relative to the cross-section of other firms	Char. financial	*Working Paper*	Gettleman and Marks (2006)
2006		88	Unexpected earnings' autocorrelations	Standardized unexpected earnings' autocorrelations via the sign of the most recent earnings realization	Char. accounting	*Journal of Accounting Research*	Narayanamoorthy (2006)
2007	62		Payout yield	Return on a zero-investment portfolio long in high-yield stocks and short in low-yield stocks	Common accounting	*Journal of Finance*	Boudoukh, Michaely, Richardson and Roberts (2007)
2007	63		Productivity	Productivity level as in King and Rebelo (2000)	Common macro	*Journal of Financial Economics*	Balvers and Huang (2007)
	64		Capital stock	Quarterly capital stock interpolated from annual data	Common macro		
2007	65		Fourth-quarter to fourth-quarter consumption growth	Fourth-quarter to fourth-quarter consumption growth rate	Common macro	*Journal of Finance*	Jagannathan and Wang (2007)
2007		89	Credit rating	S&P firm credit rating	Char. financial	*Journal of Finance*	Avramov, Chordia, Jostova and Philipov (2007)
2007		90	Trader composition	Fraction of total trading volume of a stock from institutional trading	Char. microstructure	*Working Paper*	Shu (2007)
2007		91	Change in order backlog	Change in order backlog	Char. accounting	*Seoul Journal of Business*	Baik and Ahn (2007)
2007		92	Firm productivity	Firm productivity measured by returns on invested capital	Char. accounting	*Working Paper*	Brown and Rowe (2007)
2007		93	Insider forecasts of firm volatility	Future firm volatility obtained from executive stock options	Char. financial	*Working Paper*	James, Fodor and Peterson (2007)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
2007		94	Ticker symbol	Creativity in stocks' ticker symbols	Char. other	*Quarterly Review of Economics & Finance*	Head, Smith and Wilson (2007)
2007	66		Earnings cyclicality	Sensitivity of earnings to changes in aggregate total factor productivity	Common macro	*Working Paper*	Gourio (2007)
2008	67		Market volatility innovation	Difference in monthly average of squared daily return differences	Common financial	*Review of Financial Studies*	Kumar, Sorescu, Boehme and Danielsen (2008)
		95	Firm age	Firm's public listing age	Char. accounting		
			Market return†	Equity index return	Common financial		
		96	Interaction between market volatility and firm age	Product of market volatility and firm age	Char. accounting		
2008	68		Short-run market volatility	High frequency volatility extracted from a time-series model of market returns	Common financial	*Journal of Finance*	Adrian and Rosenberg (2008)
	69		Long-run market volatility	Low frequency volatility extracted from a time-series model of market returns	Common financial		
2008	70		Investment growth	Return on a zero-investment portfolio long in low investment growth firms and short in high investment growth firms	Common financial	*Review of Financial Studies*	Xing (2008)
2008	71		Mean consumption growth	Across-state mean consumption growth rate	Common macro	*Review of Financial Studies*	Korniotis (2008)
	72		Variance of consumption growth*	Across-state consumption growth variance	Common macro		
	73		Mean habit growth	Across-state mean habit growth rate	Common macro		
	74		Variance of habit growth	Across-state habit growth variance	Common macro		
2008	75		Liquidity	Systematic liquidity extracted from eight empirical liquidity measures	Common microstructure	*Journal of Financial Economics*	Korajczyk and Sadka (2008)
2008		97	Country-level idiosyncratic volatility	Weighted average of variances and auto-covariances of firm-level idiosyncratic return shocks	Char. financial	*Review of Financial Studies*	Guo and Savickas (2008)
2008		98	Distress	Distressed firm failure probability estimated based on a dynamic logit model	Char. financial	*Journal of Finance*	Campbell, Hilscher and Szilagyi (2008)
2008		99	Shareholder advantage	Benefits from renegotiation upon default	Char. accounting	*Review of Financial Studies*	Garlappi, Shu and Yan (2008)
		100	Interaction between shareholder advantage and implied market value of assets	Implied market value of assets provided by Moody's KMV	Char. accounting		

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Year	#	#	Factor	Formation	Type	Journal	Short reference
2008		101	Asset growth	Year-on-year percentage change in total assets	Char. accounting	*Journal of Finance*	Cooper, Gulen and Schill (2008)
2008		102	Share issuance	Annual share issuance based on adjusted shares	Char. accounting	*Journal of Finance*	Pontiff and Woodgate (2008)
2008			Earnings announcement return[‡]	Earnings announcement return capturing the market reaction to unexpected information contained in the firm's earnings release	Char. financial	*Working Paper*	Brandt, Kishore, Santa-Clara and Venkatachalam (2008)
2008		103	Firm economic links	Economic links proxied by return of a portfolio of its major customers	Char. financial	*Journal of Finance*	Cohen and Frazzini (2008)
2008		104	Sin stock	Stocks in the industry of adult services, alcohol, defense, gaming, medical and tobacco	Char. other	*Financial Analyst Journal*	Frank, Ma and Oliphant (2008)
2008		105	Goodwill impairment	Buyers' overpriced shares at acquisition	Char. accounting	*Accounting Review*	Gu and Lev (2008)
2008		106	Information in order backlog	Changes in order backlog on future profitability	Char. accounting	*Working Paper*	Gu, Wang and Ye (2008)
2008		107	Investor recognition	Investor recognition proxied by the change in the breadth of institutional ownership	Char. other	*Review of Accounting Studies*	Lehavy and Sloan (2008)
2008		108	DuPont analysis	Sales over net operating assets in DuPont analysis	Char. accounting	*Accounting Review*	Soliman (2008)
2008		109	Small trades	Volume arising from small trades	Char. microstructure	*Review of Financial Studies*	Hvidkjaer (2008)
2008	76		Idiosyncratic component of S&P 500 return	Residual of the linear projection of the S&P 500 return onto the CRSP value weighted index return	Common financial	*Working Paper*	Brennan and Li (2008)
2009	77		Cash flow covariance with aggregate consumption	Cash flow covariance with aggregate consumption	Common macro	*Journal of Finance*	Da (2009)
	78		Cash flow duration	Cash flow duration sensitivity to aggregate consumption	Common macro		
2009			Financial constraints	THEORY	Common financial and macro	*Journal of Finance*	Livdan, Sapriza and Zhang (2009)
2009	79		Long-run stockholder consumption growth	Aggregated microlevel stockholder consumption	Common macro	*Journal of Finance*	Malloy, Moskowitz and Vissing-Jorgensen (2009)
2009	80		Takeover likelihood	Estimated via a logit model of regressing ex-post acquisition indicator on various firm- and industry-level accounting variables	Common financial	*Review of Financial Studies*	Cremers, Nair and John (2009)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
2009	81		Illiquidity	Estimated using structural formula in line with Kyle's (1985) lambda	Common microstructure	*Review of Financial Studies*	Chordia, Huh and Subrahmanyam (2009)
2009	82		Cash flow	Aggregate earnings based on revisions to analyst earnings forecasts	Common accounting	*Journal of Financial Economics*	Da and Warachka (2009)
2009	83		Investors' beliefs*	Belief extracted from a two-state regime-switching model of aggregate market return and aggregate output	Common other	*Review of Financial Studies*	Ozoguz (2008)
	84		Investors' uncertainty	Uncertainty extracted from a two-state regime-switching model of aggregate market return and aggregate output	Common other		
2009		110	Media coverage	Firm mass media coverage	Char. behavioral	*Journal of Finance*	Fang and Peress (2009)
2009		111	Financial distress	Credit rating downgrades	Char. accounting	*Journal of Financial Economics*	Avramov, Chordia, Jostova and Philipov (2009)
2009		112	Idiosyncratic volatility	Conditional expected idiosyncratic volatility estimated from a GARCH model	Char. accounting	*Journal of Financial Economics*	Fu (2009)
2009		113	Debt capacity	Firm tangibility as in Almeida and Campello (2007)	Char. accounting	*Journal of Finance*	Hahn and Lee (2009)
2009		114	Realized-implied volatility spread	Difference between past realized volatility and the average of call and put implied volatility	Char. financial	*Management Science*	Bali and Hovakimian (2009)
		115	Call-put implied volatility spread	Difference between call and put implied volatility	Char. financial		
2009		116	Productivity of cash	Net present value of all the firm's present and future projects generated per dollar of cash holdings	Char. accounting	*Working Paper*	Chandrashekar and Rao (2009)
2009		117	Advertising	Change in expenditures on advertising	Char. accounting	*Working Paper*	Chemmanur and Yan (2009)
2009		118	Analyst forecasts optimism	Relative optimism and pessimism proxied by the difference between long-term and short-term analyst forecast of earnings growth	Char. financial	*Journal of Financial Markets*	Da and Warachka (2009)
2009		119	Information revelation	Monthly estimate of the daily correlation between absolute returns and dollar volume	Char. microstructure	*Working Paper*	Gokcen (2009)
2009		120	Earnings volatility	Earnings volatility	Char. accounting	*Working Paper*	Gow and Taylor (2009)
2009		121	Cash flow volatility	Rolling standard deviation of the standardized cashflow over the past sixteen quarters	Char. accounting	*Journal of Empirical Finance*	Huang (2009)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
2009		122	Local unemployment	Relative state unemployment	Char. other	*Working Paper*	Korniotis and Kumar (2009)
		123	Local housing collateral	State-level housing collateral	Char. other		
2009		124	Efficiency score	Firm efficiency/inefficiency identified from the residual of the projection of firm market-to-book ratio onto various firm financial and accounting variables	Char. financial	*Journal of Financial and Quantitative Analysis*	Nguyen and Swanson (2009)
2009		125	Order imbalance	Difference between buyer- and seller-initiated trades	Char. microstructure	*Review of Financial Studies*	Barber, Odean and Zhu (2009)
2010	85		Market volatility and jumps	Estimated based on S&P index option returns	Common financial	*Working Paper*	Cremers, Halling and Weinbaum (2010)
2010	86		Market mispricing	Zero-investment portfolio constructed from repurchasing and issuing firms	Common behavioral	*Review of Financial Studies*	Hirshleifer and Jiang (2010)
2010		126	Idiosyncratic skewness	Skewness forecasted using firm level predictive variables	Char. financial	*Review of Financial Studies*	Boyer, Mitton and Vorkink (2010)
2010		127	Political campaign contributions	Firm contributions to US political campaigns	Char. other	*Journal of Finance*	Cooper, Gulen and Ovtchinnikov (2010)
2010		128	Real estate holdings	Real estate to total property, plant and equipment	Char. accounting	*Review of Financial Studies*	Tuzel (2010)
2010		129	Realized skewness	Realized skewness obtained from high-frequency intraday prices	Char. financial	*Working Paper*	Amaya, Christoffersen, Jacobs and Vasquez (2011)
		130	Realized kurtosis	Realized kurtosis obtained from high-frequency intraday prices	Char. financial		
2010		131	Excess multiple	Excess multiple calculated as the difference between the accounting multiple and the warranted multiple obtained by regressing the cross-section of firm multiples on accounting variables	Char. accounting	*Journal of Accounting, Auditing & Finance*	An, Bhojraj and Ng (2010)
2010		132	Firm information quality	Firm information quality proxied by analyst forecasts, idiosyncratic volatility and standard errors of beta estimates	Char. financial and accounting	*Working Paper*	Armstrong, Banerjee and Corona (2010)
2010		133	Long-run idiosyncratic volatility	Long-run idiosyncratic volatility filtered from idiosyncratic volatility using HP filters	Char. financial	*Working Paper*	Cao and Xu (2010)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
2010	87		Private information	Return on a zero-investment portfolio long in high PIN stocks and short in low PIN stocks; PIN (private information) is the probability of information-based trade	Common microstructure	*Journal of Financial and Quantitative Analysis*	David, Hvidkjaer and O'Hara (2010)
2010		134	Intra-industry return reversals	Intra-industry return reversals captured by the return difference between loser stocks and winners stocks based on relative monthly performance within the industry	Char. financial	*Working Paper*	Hameed, Huang and Mian (2010)
2010		135	Related industry returns	Stock returns from economically related supplier and customer industries	Char. financial	*Journal of Finance*	Menzly and Ozbas (2010)
2010		136	Earnings distributed to equity holders	Earnings distributed to equity holders	Char. accounting	*Review of Accounting & Finance*	Papanastasopoulos, Thomakos and Wang (2010)
		137	Net cash distributed to equity holders	Dividends minus stock issues	Char. accounting		
2010		138	Excess cash	Most recently available ratio of cash to total assets	Char. accounting	*Financial Management*	Simutin (2010)
2010		139	Extreme downside risk	Extreme downside risk proxied by the left tail index in the classical generalized extreme value distribution	Char. financial	*Journal of Banking and Finance*	Huang, Liu, Rhee and Wu (2010)
2010		140	Volatility smirk	Steepness in individual option volatility smirk	Char. financial	*Journal of Financial and Quantitative Analysis*	Xing, Zhang and Zhao (2010)
2010			Exposure to financial distress costs	THEORY	Char. financial	*Journal of Financial Economics*	George and Hwang (2010)
2011	88		Rare disasters	Disaster index based on international political crises	Common financial	*Journal of Financial Economics*	Berkman, Jacobsen and Lee (2011)
2011			Distress risk‡	Aggregate distress risk obtained by projecting future business failure growth rates on a set of basis assets	Common financial	*Journal of Financial Economics*	Kapadia (2011)[l]
2011			Momentum†	Factor-mimicking portfolios based on momentum of international equity returns	Common other	*Review of Financial Studies*	Hou, Karolyi and Kho (2011)
	89		Cash flow-to-price	Factor-mimicking portfolios based on cash flow-to-price of international equity returns	Common accounting		
2011		141	R&D investment	Firm's investment in research and development	Char. accounting	*Review of Financial Studies*	Li (2011)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
			Financial constraints†	Kaplan and Zingales (1997) financial constraint index	Char. financial		
2011		142	Extreme stock returns	Portfolios sorted based on extreme past returns	Char. financial	*Journal of Financial Economics*	Bali, Cakici and Whitelaw (2011)
2011		143	Jumps in individual stock returns	Average jump size proxied by slope of option implied volatility smile	Char. financial	*Journal of Financial Economics*	Yan (2011)
2011		144	Intangibles	Employee satisfaction proxied by the list of "100 Best Companies to Work for in America"	Char. other	*Journal of Financial Economics*	Edmans (2011)
2011			Market return†	Equity index return	Common financial	*Working Paper*	Chen, Novy-Marx and Zhang (2011)^m
	90		Investment portfolio return	Difference between returns of portfolios with low and high investment-to-asset ratio	Common financial		
	91		Return-on-equity portfolio return	Difference between returns of portfolios with high and low return on equity	Common financial		
2011		145	Volatility of liquidity	Measured by the price impact of trade as in Amihud (2002)	Char. microstructure	*Working Paper*	Akbas, Armstrong and Petkova (2011)
2011		146	Dispersion in beliefs	Revealed through active holdings of fund managers	Char. behavioral	*Working Paper*	Jiang and Sun (2011)
2011		147	Credit default swap spreads	Five-year spread less one-year spread	Char. financial	*Working Paper*	Han and Zhou (2011)
2011		148	Organizational capital	Directly measured using Selling, General and Administrative expenditures	Char. accounting	*Working Paper*	Eisfeldt and Papanikolaou (2011)
2011		149	Residual income	Firm residual income growth extracted from firm earnings growth	Char. accounting	*Review of Accounting Studies*	Balachandran and Mohanram (2011)
2011		150	Accrual volatility	Firm accrual volatility measured by the standard deviation of the ratio of accruals to sales	Char. accounting	*Working Paper*	Bandyopadhyay, Huang and Wirjanto (2011)
2011		151	Implied cost of capital	Implied cost of capital estimated using option contracts	Char. financial	*Working Paper*	Callen and Lyle (2011)
2011		152	Non-accounting information quality	Average delay with which non-accounting information is impounded into stock price	Char. financial	*Contemporary Accounting Research*	Callen, Khan and Lu (2011)
		153	Accounting information quality	Average delay with which accounting information is impounded into stock price	Char. financial		

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Year	#	#	Factor	Formation	Type	Journal	Short reference
2011		154	Labor unions	Labor force unionization measured by the percentage of employed workers in a firm's primary Census industry Classification industry covered by unions in collective bargaining with employers	Char. other	*Journal of Financial and Quantitative Analysis*	Chen, Kacperczyk and Ortiz-Molina (2011)
2011		155	Overreaction to nonfundamental price changes	Overreaction to within-industry discount rate shocks as captured by decomposing the short-term reversal into across-industry return momentum, within-industry variation in expected returns, under-reaction to within-industry cash flow news and overreaction to within-industry discount rate news	Char. other	*Working Paper*	Da, Liu and Schaumburg (2011)
2011		156	Short interest	Short interest from short sellers	Char. financial	*Accounting Review*	Michael and Rees (2011)
2011		157	Percent total accrual	Firm accruals scaled by earnings	Char. accounting	*Accounting Review*	Hafzalla, Lundholm and Van Winkle (2007)
2011			Projected earnings accuracy‡	Skilled analysts identified by both past earnings forecasts accuracy and skills	Char. accounting	*Working Paper*	Hess, Kreutzmann and Pucker (2011)
2011		158	Firm productivity	Firm level total factor productivity estimated from firm value added, employment and capital	Char. accounting	*Working Paper*	Imrohoroglu and Tuzel (2011)
2011		159	Really dirty surplus	Really dirty surplus that happens when a firm issues or reacquires its own shares in a transaction that does not record the shares at fair market value	Char. accounting	*Accounting Review*	Landsman, Miller, Peasnell and Shu (2011)
2011		160	Earnings forecast	Earnings forecast based on firm fundamentals	Char. accounting	*Review of Accounting Studies*	Li (2011)
2011		161	Asset growth	Yearly percentage change in total balance sheet assets	Char. accounting	*Working Paper*	Nyberg and Poyry (2011)
2011		162	Real asset liquidity	Number of potential buyers for a firm's assets from within the industry	Char. microstructure	*Working Paper*	Ortiz-Molina and Phillips (2011)
2011		163	Customer-base concentration	Annual change in customer-base concentration	Char. other	*Working Paper*	Patatoukas (2011)
2011		164	Tax expense surprises	Seasonally differenced quarterly tax expense	Char. accounting	*Journal of Accounting Research*	Thomas and Zhang (2011)
2011			Predicted earnings increase score‡	Predicted earnings increase score based on financial statement information	Char. accounting	*Review of Accounting Studies*	Wahlen and Wieland (2011)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
2011			Shareholder recovery	THEORY	Common financial	*Journal of Finance*	Garlappi and Yan (2011)
2011	92		Garbage growth	Realized annual garbage growth	Common macro	Journal of Finance	Savov (2011)
2012	93		Financial intermediary's wealth	Intermediary's marginal value of wealth proxied by shocks to leverage of securities broker-dealers	Common financial	*Journal of Finance*	Adrian, Etula and Muir (2012)
2012	94		Stochastic volatility*	Estimated from a heteroscedastic VAR based on market and macro variables	Common financial	*Working Paper*	Campbell, Giglio, Polk and Turley (2012)
2012	95		Average variance of equity returns	Decomposition of market variance into an average correlation component and an average variance component	Common financial	*Review of Financial Studies*	Chen and Petkova (2012)
2012	96		Income growth for goods producing industries	Income growth for goods producing industries	Common macro	*Journal of Finance*	Eiling (2012)
	97		Income growth for manufacturing industries	Income growth for manufacturing industries	Common macro		
	98		Income growth for distributive industries	Income growth for distributive industries	Common macro		
	99		Income growth for service industries*	Income growth for service industries	Common macro		
	100		Income growth for government*	Income growth for government	Common macro		
2012	101		Consumption volatility	Filtered consumption growth volatility from a Markov regime-switching model based on historical consumption data	Common macro	*Journal of Finance*	Boguth and Kuehn (2012)
2012	102		Market skewness	Higher moments of market returns estimated from daily index options	Common financial	*Journal of Financial Economics*	Chang, Christoffersen and Jacobs (2012)
2012	103		Learning*	Learning estimated from an investor's optimization problem under Knightian uncertainty	Common financial	*Working Paper*	Viale, Garcia-Feijoo and Giannetti (2011)
	104		Knightian uncertainty	Knightian uncertainty estimated from an investor's optimization problem under Knightian uncertainty	Common financial		
2012	105		Market uncertainty	Proxied by variance risk premium	Common financial	*Working Paper*	Bali and Zhou (2012)
2012			Labor income‡	Labor income at the census division level	Common macro	*Working Paper*	Gomez, Priestley and Zapatero (2012)^{n}
2012		165	Product price change	Cumulative product price changes since an industry enters the producer price index program	Char. financial	*Working Paper*	Van Binsbergen (2012)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
2012	106		Future growth in the opportunity cost of money	Opportunity cost of money as proxied by 3-month Treasury bill rate or effective Federal Funds rate	Common macro	*Working Paper*	Lioui and Maio (2012)
2012			Inter-cohort consumption differences	THEORY	Common macro	*Journal of Financial Economics*	Garleanu, Kogan and Panageas (2012)
2012	107		Market-wide liquidity	Proxied by "noise" in Treasury prices	Common microstructure	*Working Paper*	Hu, Pan and Wang (2012)
2012		166	Stock skewness	Ex ante stock risk-neutral skewness implied by option prices	Char. financial	*Journal of Finance*	Conrad, Dittmar and Ghysels (2012)
2012		167	Expected return uncertainty	Proxied by the volatility of option-implied volatility	Char. financial	*Working Paper*	Baltussen, Van Bekkum and Van der Grient (2012)
2012		168	Information intensity	Proxied by monthly frequency of current report filings	Char. microstructure	*Working Paper*	Zhao (2012)
2012		169	Credit risk premia	Market implied credit risk premia based on the term structure of CDS spreads	Char. financial	*Working Paper*	Friewald, Wagner and Zechner (2012)
2012		170	Geographic dispersion	Number of states in which a firm has business operations	Char. other	*Journal of Financial Economics*	Garcia and Norli (2012)
2012		171	Political geography	Political proximity measured by political alignment index of each state's leading politicians with the ruling presidential party	Char. other	*Journal of Financial Economics*	Kim, Pantzalis and Park (2012)
2012		172	Option to stock volume ratio	Option volume divided by stock volume	Char. microstructure	*Journal of Financial Economics*	Johnson and So (2012)
2012		173	Cash holdings	Firm cash holdings	Char. accounting	*Journal of Financial Economics*	Palazzo (2012)
2012		174	Labor mobility	Labor mobility based on average occupational dispersion of employees in an industry	Char. accounting	*Working Paper*	Donangelo (2012)
2012		175	Debt covenant protection	Firm-level covenant index constructed based on 30 covenant categories	Char. accounting	*Working Paper*	Wang (2012)
2012		176	Stock-cash flow sensitivity	Stock-cash flow sensitivity estimated from a structural one-factor contingent-claim model	Char. financial	*Working Paper*	Chen and Strebulaev (2012)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
2012	108		Jump beta	Discontinuous jump beta based on Todorov and Bollerslev (2010)	Common financial	*Working Paper*	Sophia Zhengzi Li (2012)
2012			Long-run consumption growth‡	Long-run consumption growth rate identified from the risk-free rate and market price-dividend ratio based on Bansal and Yaron (2005)'s long-run risk model	Common macro	*Journal of Financial Economics*	Ferson, Nallareddy and Xie (2012)^o
			Short-run consumption growth‡	Short-run consumption growth rate identified from the risk-free rate and market price-dividend ratio based on Bansal and Yaron (2005)'s long-run risk model	Common macro		
			Consumption growth volatility‡	Consumption growth volatility shocks identified from the risk-free rate and market price-dividend ratio based on Bansal and Yaron (2005)'s long-run risk model	Common macro		
2012		177	Change in call implied volatility	Change in call implied volatility	Char. financial	*Working Paper*	Ang, Bali and Cakici (2012)
		178	Change in put implied volatility	Change in put implied volatility	Char. financial		
2012		179	Firm hiring rate	Firm hiring rate measured by the change in the number of employees over the average number of employees	Char. other	*Working Paper*	Bazdresch, Belo and Lin (2012)
2012		180	Information processing complexity	Past return for paired pseudo-conglomerates	Char. financial	*Journal of Financial Economics*	Cohen and Lou (2012)
2012		181	Opportunistic buy	Prior month buy indicator for opportunistic traders who do not trade routinely	Char. microstructure	*Journal of Finance*	Cohen, Malloy and Pomorski (2012)
		182	Opportunistic sell	Prior month sell indicator for opportunistic traders who do not trade routinely	Char. microstructure		
2012		183	Innovative efficiency	Patents/citations scaled by research and development expenditures	Char. other	*Journal of Financial Economics*	Hirshleifer, Hsu and Li (2012)
2012		184	Abnormal operating cash flows	Abnormal operating cash flows	Char. accounting	*Working Paper*	Li (2012)
		185	Abnormal production costs	Abnormal production costs	Char. accounting		
2012		186	Deferred revenues	Changes in the current deferred revenue liability	Char. accounting	*Contemporary Accounting Research*	Prakash and Sinha (2012)
2012		187	Earnings conference calls	Sentiment of conference call wording	Char. other	*Journal of Banking and Finance*	Price, Doran, Peterson and Bliss (2012)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
2012		188	Earnings forecast optimism	Difference between characteristic forecasts and analyst forecasts	Char. accounting	*Working Paper*	So (2012)
2012	109		Commodity index	Open interest-weighted total index that aggregates 33 commodities	Common financial	*Working Paper*	Boons, Roon and Szymanowska (2012)
2012		189	Time-series momentum	Time-series momentum strategy based on autocorrelations of scaled returns	Char. financial	*Journal of Financial Economics*	Moskowitz, Ooi and Pedersen (2012)
2012		190	Carry	Expected return minus expected price appreciation	Char. financial	*Working Paper*	Koijen, Moskowitz, Pedersen and Vrugt (2012)
2012		191	Expected return proxy	Logistic transformation of the fit (R^2) from a regression of returns on past prices	Char. financial	*Journal of Financial Economics*	Burlacu, Fontaine, Jimenez-Garces and Seasholes (2012)
2012		192	Fraud probability	Probability of manipulation based on accounting variables	Char. accounting	*Financial Analysts Journal*	Beneish, Lee and Nichols (2013)
2012		193	Buy orders	Sensitivity of price changes to sell orders	Char. microstructure	*Working Paper*	Brennan, Chordia, Subrahmanyam and Tong (2012)
		194	Sell orders	Sensitivity of price changes to buy orders	Char. microstructure		
2013	110		Expected dividend level	Expected dividend level based on a macro time-series model	Common financial	*Working Paper*	Doskov, Pekkala and Ribeiro (2013)
	111		Expected dividend growth	Expected dividend growth based on a macro time-series model	Common financial		
2013		195	Firm's ability to innovate	Rolling firm-by-firm regressions of firm-level sales growth on lagged R&D	Char. accounting	*Review of Financial Studies*	Cohen, Diether and Malloy (2013)
2013		196	Board centrality	Board centrality measured by four basic dimensions of well-connectedness	Char. other	*Journal of Accounting and Economics*	Larcker, So and Wang (2013)
2013		197	Gross profitability	Gross profits to assets	Char. accounting	*Journal of Financial Economics*	Novy-Marx (2013)
2013		198	Betting-against-beta	Long leveraged low-beta assets and short high-beta assets	Char. financial	*Working Paper*	Frazzini and Pedersen (2013)
2013		199	Secured debt	Proportion of secured to total debt	Char. accounting	*Working Paper*	Valta (2013)
		200	Convertible debt	Proportion of convertible to total debt	Char. accounting		
		201	Convertible debt indicator	Dummy variable indicating whether a firm has convertible debt outstanding	Char. accounting		
2013	112		Cross-sectional pricing inefficiency	Pricing inefficiency proxied by returns to simulated trading strategies that capture momentum, profitability, value, earnings and reversal	Common microstructure	*Working Paper*	Akbas, Armstrong, Sorescu and Subrahmanyam (2013)

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Year	#	#	Factor	Formation	Type	Journal	Short reference
2013		202	Attenuated returns	Composite trading strategy returns where the weights are based on averaging percentile rank scores of various characteristics for each stock on portfolios	Char. financial	*Working Paper*	Chordia, Subrahmanyam and Tong (2013)
2013		203	Bad private information	Decomposing the PIN measure of Easley, Hvidkjaer and O'Hara (2002) into two elements that reflect informed trading on good news and bad news	Char. microstructure	*Working Paper*	Brennan, Huh and Subrahmanyam (2013)
2013	113		Trend signal	Return on a zero-investment portfolio long in past winners and short in past losers based on short-term, intermediate-term and long-term stock price trends	Common other	*Working Paper*	Han and Zhou (2013)

Notes to Table

This table contains a summary of risk factors that explain the cross-section of expected returns. The column "Char.(#)"("Common(#)") reports the cumulative number of empirical factors that are classified as characteristics (common risk factors).

*: insignificant; †: duplicated; ‡: missing p-value.

a: Black, Jensen and Scholes (1972) first tested the market factor. However, they focus on industry portfolios and thus present a less powerful test compared to Fama and MacBeth (1973). We therefore use the test statistics in Fama and MacBeth (1973) for the market factor.

b: No p-values reported for their factors constructed from principal component analysis.

c: Fama and French (1992) create zero-investment portfolios to test size and book-to-market effects. This is different from the testing approach in Banz (1981). We therefore count Fama and French (1992)'s test on size effect as a separate one.

d: No p-values reported for their high order equity index return factors.

e: No p-values reported for their eight risk factors that explain international equity returns.

f: No p-values reported for his high order return factors.

g: No p-values reported for their five hedge fund style return factors.

h: Vanden (2004) reports a t-statistic for each Fama-French 25 size and book-to-market sorted stock portfolios. We average these 25 t-statistics.

i: Acharya and Pedersen (2005) consider the illiquidity measure in Amihud (2002). This is different from the liquidity measure in Pastor and Stambaugh (2003). We therefore count their factor as a separate one.

j: No p-values reported for the interactions between market return and option returns.

k: No p-values reported for their co-moment betas.

l: No p-values reported for his distress tracking factor.

m: Gomez, Priestley and Zapatero (2012) study census division level labor income. However, most of the division level labor income have a non-significant t-statistic. We do not count their factors.

n: No p-values reported for their factors estimated from the long-run risk model.

o: The paper is replaced by Hou, Xue and Zhang (2014).

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A Sampling Procedure

There are at least two ways to generate t-statistics for a risk factor. One way is to show that factor related sorting results in cross-sectional return patterns that are not explained by standard risk factors. The t-statistic for the intercept of the long/short strategy returns regressed on common risk factors is usually reported. The other way is to use factor loadings as explanatory variables and show that they are related to the cross-section of expected returns after controlling for standard risk factors. Individual stocks or stylized portfolios (e.g., Fama-French 25 portfolios) are used as dependent variables. The t-statistic for the factor risk premium is taken as the t-statistic for the factor. In sum, depending on where the new risk factor or factor returns enter the regressions, the first way can be thought of as the left hand side (LHS) approach and the second the right hand side (RHS) approach. For our data collection, we choose to use the RHS t-statistics. When they are not available, we use the LHS t-statistics or simply the t-statistics for the average returns of long/short strategies if the authors do not control for other risk factors.

B Multiple Testing When the Number of Tests (M) is Unknown

The empirical difficulty in applying standard p-value adjustments is that we do not observe factors that have been tried, found to be insignificant and then discarded. We attempt to overcome this difficulty using a simulation framework. The idea is first simulate the empirical distribution of p-values for all experiments (published and unpublished) and then adjust p-values based on these simulated samples.

First, we assume the test statistic (t-statistic, for instance) for any experiment follows a certain distribution D (e.g., exponential distribution) and the set of published works is a truncated D distribution. Based on the estimation framework for truncated distributions,[46] we estimate parameters of distribution D and total number of trials M. Next we simulate many sequences of p-values, each corresponding to a plausible set of p-value realizations of all trials. To account for the uncertainty in parameter estimates of D and M, we simulate p-value sequences based on the distribution of estimated D and M. Finally, for each p-value, we calculate the adjusted p-value based on a sequence of simulated p-values. The median is taken as the final adjusted p-value.

[46]See Heckman (1979) and Greene (2008), Chapter 24.

B.1 Using Truncated Exponential Distribution to Model the t-statistic Sample

Truncated distributions have been used to study hidden tests (i.e., publication bias) in medical research.[47] The idea is that studies reporting significant results are more likely to get published. Assuming a threshold significance level or t-statistic, researchers can to some extent infer the results of unpublished works and gain understanding of the overall effect of a drug or treatment. However, in medical research, insignificant results are still viewed as an indispensable part of the overall statistical evidence and are given much more prominence than in the financial economics research. As a result, medical publications tend to report more insignificant results. This makes applying the truncated distribution framework to medical studies difficult as there is no clear-cut threshold value.[48] In this sense, the truncated distributional framework suits our study better — 1.96 is the obvious hurdle that research needs to overcome to get published.

On the other hand, not all tried factors with p-value above 1.96 are reported. In the quantitative asset management industry significant results are not published — they are considered "trade secrets". For the academic literature, factors with "borderline" t-statistics are difficult to get published. Thus, our sample is likely missing a number of factors that have t-statistics just over the bar of 1.96. To make our inference robust, for our baseline result, we assume all tried factors with t-statistics above 2.57 are observed and ignore those with t-statistics in the range of (1.96, 2.57). We experiment with alternative ways to handle t-statistics in this range.

Many distributions can be used to model the t-statistic sample. One restriction that we think any of these distributions should satisfy is the monotonicity of the density curve. Intuitively, it should be easier to find factors with small t-statistics than large ones.[49] We choose to use the simplest distribution that incorporates this monotonicity condition: the exponential distribution.

Panel A of Figure B.1 presents the histogram of the baseline t-statistic sample and the fitted truncated exponential curve.[50] The fitted density closely tracks the histogram and has a population mean of 2.07.[51] Panel B is a histogram of the original t-statistic sample which, as we discussed before, is likely to under-represent the sample

[47] See Begg and Berlin (1988) and Thornton and Lee (2000).

[48] When the threshold value is unknown, it must be estimated from the likelihood function. However, such estimation usually incurs large estimation errors.

[49] This basic scarcity assumption is also the key ingredient in our model in Section 5.

[50] There are a few very large t-statistics in our sample. We fit the truncated exponential model without dropping any large t-statistics. In contrast to the usual normal density, exponential distribution is better at modeling extreme observations. In addition, extreme values are pivotal statistics for heavy-tailed distributions and are key for model estimation. While extreme observations are included for model estimation, we exclude them in Figure B.1 to better focus on the main part of the t-statistic range.

[51] Our truncated exponential distribution framework allows a simple analytical estimate for the population mean of the exponential distribution. In particular, let c be the truncation point and the

with a t-statistic in the range of (1.96, 2.57). Panel C is the augmented t-statistic sample with the ad hoc assumption that our sample covers only half of all factors with t-statistics between 1.96 and 2.57. The population mean estimate is 2.22 in Panel B and 1.93 in Panel C. As expected, the under-representation of relatively small t-statistics results in a higher mean estimate for the t-statistic population. We think the baseline model is the best among all three models as it not only overcomes the missing data problem for the original sample, but also avoids guessing the fraction of missing observations in the 1.96-2.57 range. We use this model estimates for the follow-up analysis.

Using the baseline model, we calculate other interesting population characteristics that are key to multiple hypothesis testing. Assuming independence, we model observed t-statistics as draws from an exponential distribution with mean parameter $\hat{\lambda}$ and a known cutoff point of 2.57. The proportion of unobserved factors is then estimated as:

$$P(\text{unobserved}) = \Phi(2.57; \hat{\lambda}) = 1 - \exp(-2.57/\hat{\lambda}) = 71.1\%, \tag{B.1}$$

where $\Phi(c; \lambda)$ is the cumulative distribution function evaluated at c for a exponential distribution with mean λ. Our estimates indicate that the mean absolute value of the t-statistic for the underlying factor population is 2.07 and about 71.1% of tried factors are discarded. Given that 238 out of the original 316 factors have a t-statistic exceeding 2.57, the total number of factor tests is estimated to be 824 ($= 238/(1 - 71.1\%)$) and the number of factors with a t-statistic between 1.96 and 2.57 is estimated to be 82.[52] Since our t-statistic sample covers only 57 such factors, roughly 30% (=(82-57)/82) of t-statistics between 1.96 and 2.57 are hidden.

B.2 Simulated Benchmark t-statistics Under Independence

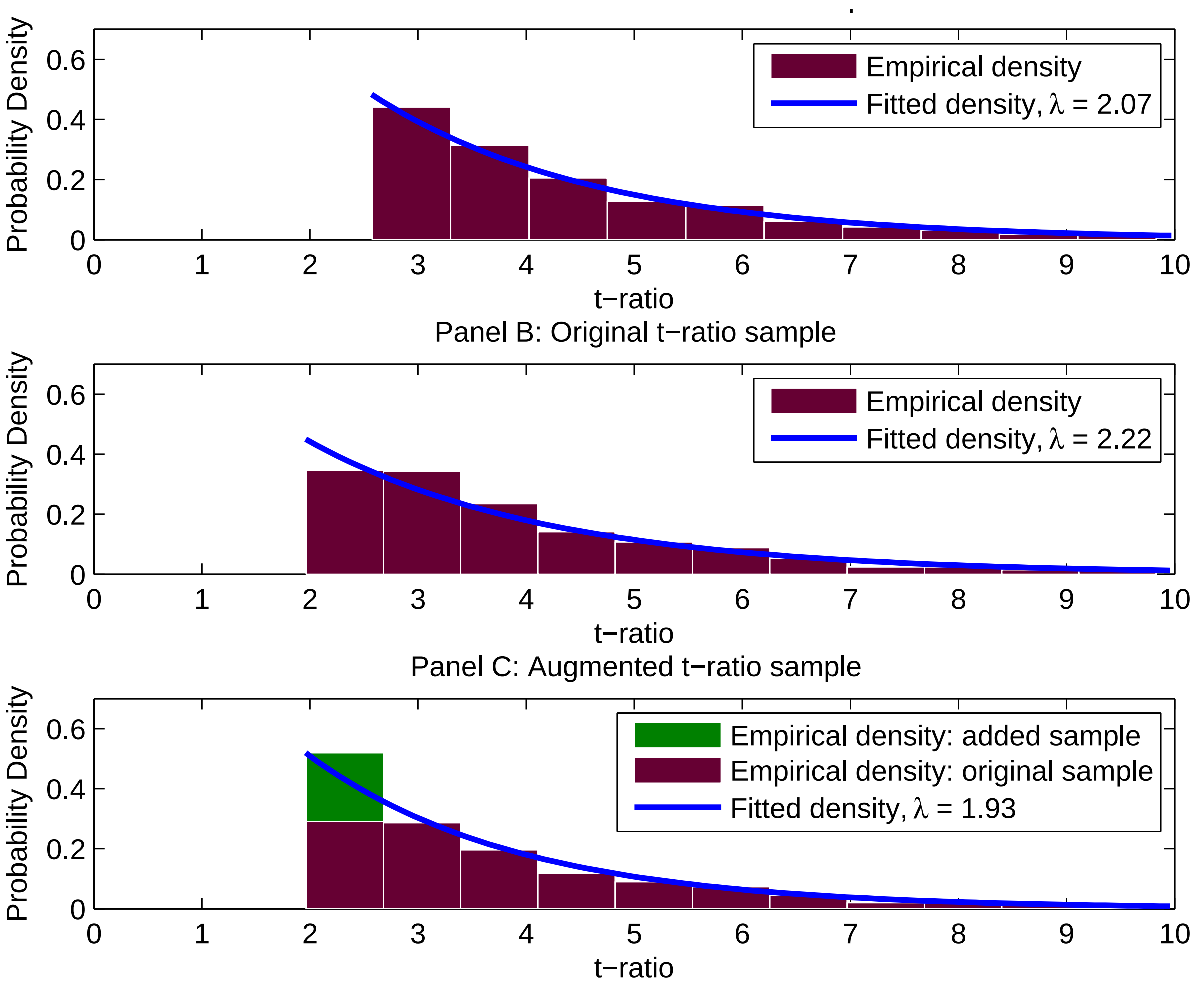
The truncated exponential distribution framework helps us approximate the distribution of t-statistics for all factors, published and unpublished. We can then apply the aforementioned adjustment techniques to this distribution to generate new t-statistic benchmarks. However, there are two sources of sampling and estimation uncertainty that affect our results. First, our t-statistic sample may under-represent all factors with t-statistics exceeding 2.57.[53] Hence, our estimates of total trials are biased (too low), which affects our calculation of the benchmarks. Second, estimation error for the truncated exponential distribution can affect our benchmark t-statistics. Although

t-statistic sample be $\{t_i\}_{i=1}^N$. The mean estimate is given by $\hat{\lambda} = 1/(\bar{t} - c)$, where $\bar{t} = (\sum_{i=1}^N t_i)/N$ is the sample mean.

[52]Directly applying our estimate framework to the original sample that includes all t-statistics above 1.96, the estimated total number of factor tests would be 713. Alternatively, assuming our sample only covers half of the factors with t-statistics between 1.96 and 2.57, the estimated number of factors is 971.

[53]This will happen if we miss factors published by the academic literature or we do not have access to the "trade secrets" by industry practitioners.

Figure B.1: **Density Plots for t-statistic**



Empirical density and fitted exponential density curves based on three different samples. Panel A is based on the baseline sample that includes all t-statistics above 2.57. Panel B is based on the original sample with all t-statistics above 1.96. Panel C is based on the augmented sample that adds the sub-sample of observations that fall in between 1.96 and 2.57 to the original t-statistic sample. It doubles the number of observations within the range of 1.96 and 2.57 in the original sample. λ is the single parameter for the exponential curve. It gives the population mean for the unrestricted (i.e., non-truncated) distribution.

we can approximate the estimation error through the usual asymptotic distribution theory for MLE, it is unclear how this error affects our benchmark t-statistics. This is because t-statistic adjustment procedures usually depend on the entire t-statistic distribution and so standard transformational techniques (e.g., the delta method) do not apply. Moreover, we are not sure whether our sample is large enough to trust the accuracy of asymptotic approximations.

Given these concerns, we propose a simulation framework that incorporates these uncertainties. We divide it into four steps:

Step I Estimate λ and M based on a new t-statistic sample with size $r \times R$.

Suppose our current t-statistic sample size is R and it only covers a fraction of $1/r$ of all factors. We sample $r \times R$ t-statistics (with replacement) from the original t-statistic sample. Based on this new t-statistic sample, we apply the above truncated exponential distribution framework to the t-statistics and obtain the parameter estimates λ for the exponential distribution. The truncation probability is calculated as $\hat{P} = \Phi(2.57; \hat{\lambda})$. We can then estimate the total number of trials by

$$\hat{M} = \frac{rR}{1 - \hat{P}}$$

Step II Calculate the benchmark t-statistic based on a random sample generated from $\hat{\lambda}$ and $\hat{M}$.

Based on the previous step estimate of $\hat{\lambda}$ and $\hat{M}$, we generate a random sample of t-statistics for all tried factors. We then calculate the appropriate benchmark t-statistic based on this generated sample.

Step III Repeat Step II 10,000 times to get the median benchmark t-statistic.

Repeat Step II (based on the same $\hat{\lambda}$ and $\hat{M}$) 10,000 times to generate a collection of benchmark t-statistics. We take the median as the final benchmark t-statistic corresponding to the parameter estimate $(\hat{\lambda}, \hat{M})$.

Step IV Repeat Step I-III 10,000 times to generate a distribution of benchmark t-statistics.

Repeat Step I-III 10,000 times, each time with a newly generated t-statistic sample as in Step I. For each repetition, we obtain a benchmark t-statistic t_i corresponding to the parameter estimates $(\hat{\lambda}_i, \hat{M}_i)$. In the end, we have a collection of benchmark t-statistics $\{t_i\}_{i=1}^{10000}$.

To see how our procedure works, notice that Steps II-III calculate the theoretical benchmark t-statistic for a t-statistic distribution characterized by $(\hat{\lambda}, \hat{M})$. As a result, the outcome is simply one number and there is no uncertainty around it. Uncertainties are incorporated in Steps I and IV. In particular, by sampling repeatedly from the original t-statistic sample and re-estimating λ and M each time, we take into account estimation error of the truncated exponential distribution. Also, under the assumption that neglected significant t-statistics follow the empirical distribution of our t-statistic sample, by varying r, we can assess how this under-representation of our t-statistic sample affects results.

Table B.2 shows estimates of M and benchmark t-statistics. When $r = 1$, the median estimate for the total number of trials is 817,[54] almost the same as our previous estimate of 820 based on the original sample. Unsurprisingly, Bonferroni implied benchmark t-statistic (4.01) is larger than 3.78, which is what we get ignoring unpublished works. Holm implied t-statistic (3.96), while not necessarily increasing in the number of trials, is also higher than before (3.64). BHY implied t-statistic increases from 3.39 to 3.68 at 1% significance and from 2.78 to 3.18 at 5% significance. As r increases, sample size M and benchmark t-statistics for all four types of adjustments increase. When r doubles, the estimate of M also approximately doubles and Bonferroni and Holm implied t-statistics increase by about 0.2, whereas BHY implied t-statistics increase by around 0.03 (under both significance levels).

Table B.2: **Benchmark t-statistics When M is Estimated**

Estimated total number of factors tried (M) and benchmark t-statistic percentiles based on a truncated exponential distribution framework. Our estimation is based on the original t-statistic sample truncated at 2.57. The sampling ratio is the assumed ratio of the true population size of t-statistics exceeding 2.57 over our current sample size. Both Bonferroni and Holm have a significance level of 5%.

Sampling ratio	M	Bonferroni	Holm	BHY(1%)	BHY(5%)
(r)	[10% 90%]	[10% 90%]	[10% 90%]	[10% 90%]	[10% 90%]
1	817	4.01	3.96	3.68	3.17
	[731 947]	[3.98 4.04]	[3.92 4.00]	[3.63 3.74]	[3.12 3.24]
1.5	1234	4.11	4.06	3.70	3.20
	[1128 1358]	[4.08 4.13]	[4.03 4.09]	[3.66 3.74]	[3.16 3.24]
2	1646	4.17	4.13	3.71	3.21
	[1531 1786]	[4.15 4.19]	[4.11 4.15]	[3.67 3.75]	[3.18 3.25]

[54]Our previous estimate of 820 is a one-shot estimate based on the truncated sample. The results in Table B.2 are based on repeated estimates based on re-sampled data: we re-sample many times and 817 is the *median* of all these estimates. It is close to the one-shot estimate.

C A Simple Bayesian Framework

The following framework is adopted from Scott and Berger (2006). It highlights the key issues in Bayesian multiple hypothesis testing.[55] More sophisticated generalizations modify the basic model but are unlikely to change the fundamental hierarchical testing structure.[56] We use this framework to explain the pros and cons of performing multiple testing in a Bayesian framework.

The hierarchical model is as follows:

H1. $(X_i|\mu_i, \sigma^2, \gamma_i) \overset{iid}{\sim} N(\gamma_i\mu_i, \sigma^2)$,

H2. $\mu_i|\tau^2 \overset{iid}{\sim} N(0, \tau^2), \gamma_i|p_0 \overset{iid}{\sim} Ber(1-p_0)$,

H3. $(\tau^2, \sigma^2) \sim \pi_1(\tau^2, \sigma^2), p_0 \sim \pi_2(p_0)$.

We explain each step in detail as well as the notation:

H1. X_i denotes the average return generated from a long-short trading strategy based on a certain factor; μ_i is the unknown mean return; σ^2 is the common variance for returns and γ_i is an indicator function, with $\gamma_i = 0$ indicating a zero factor mean. γ_i is the counterpart of the reject/accept decision in the usual (frequentists') hypothesis testing framework.

H1 therefore says that factor returns are independent conditional on mean $\gamma_i\mu_i$ and common variance σ^2, with $\gamma_i = 0$ indicating that the factor is spurious. The common variance assumption may look restrictive but we can always scale factor returns by changing the dollar investment in the long-short strategy. The crucial assumption is conditional independence of average strategy returns. Certain form of conditional independence is unavoidable for Bayesian hierarchical modeling[57] — probably unrealistic for our application. We can easily think of scenarios where average returns of different strategies are correlated, even when population means are known. For example, it is well known that two of the most popular factors, the Fama and French (1992) HML and SMB are correlated.

[55]We choose to present the full Bayes approach. An alternative approach — the empirical-Bayes approach — is closely related to the BHY method that controls the *false-discovery rate* (FDR). See Storey (2003) and Efron and Tibshirani (2002) for the empirical-Bayes interpretation of FDR. For details on the empirical-Bayes method, see Efron, Tibshirani, Storey and Tusher (2001), Efron (2004) and Efron (2006). For an in-depth investigation of the differences between the full Bayes and the empirical-Bayes approach, see Scott and Berger (2010). For an application of the empirical-Bayes method in finance, see Markowitz and Xu (1994).

[56]See Meng and Dempster (1987) and Whittemore (2007) for more works on the Bayesian approach in hypothesis testing.

[57]Conditional independence is crucial for the Bayesian framework and the construction of posterior likelihoods. Although it can be extended to incorporate special dependence structures, there is no consensus on how to systematically handle dependence. See Brown et al. (2012) for a discussion of independence in Bayesian multiple testing. They also propose a spatial dependence structure into a Bayesian testing framework.

H2. The first step population parameters μ_i's and γ_i's are assumed to be generated from two other parametric distributions: μ_i's are independently generated from a normal distribution and γ_i's are simply generated from a Bernoulli distribution, i.e., $\gamma_i = 0$ with probability p_0.

The normality assumption for the μ_i's requires the reported X_i's to randomly represent either long/short or short/long strategy returns. If researchers have a tendency to report positive abnormal returns, we need to randomly assign to these returns plus/minus signs. The normality assumptions in both H1 and H2 are important as they are necessary to guarantee the properness of the posterior distributions.

H3. Finally, the two variance variables τ^2 and σ^2 follow a joint prior distribution π_1 and the probability p_0 follows a prior distribution π_2.

Objective or "neutral" priors for π_1 and π_2 can be specified as:

$$\begin{aligned} \pi_1(\tau^2, \sigma^2) &\propto (\tau^2 + \sigma^2)^{-2}, \\ \pi_2(p_0) &= \text{Uniform}(0, 1). \end{aligned}$$

Under this framework, the joint conditional likelihood function for X_i's is simply a product of individual normal likelihood functions and the posterior probability that $\gamma_i = 1$ (discovery) can be calculated by applying Bayes' law. When the number of trials is large, to calculate the posterior probability we need efficient methods such as importance sampling, which involves high dimensional integrals.

One benefit of a Bayesian framework for multiple testing is that the multiplicity penalty term is already embedded. In the frequentists' framework, this is done by introducing FWER or FDR. In a Bayesian framework, the so-called "Ockham's razor effect"[58] automatically adjusts the posterior probabilities when more factors are simultaneously tested.[59] Simulation studies in Scott and Berger (2006) show how the discovery probabilities for a few initial signals increase when more noise are added to the original sample.

However, there are several shortcomings for the Bayesian approach. Some of them are specific to the context of our application and the others are generic to the Bayesian multiple testing framework.

At least two issues arise when applying the Bayesian approach to our factor selection problem. First, we do not observe all tried factors. While we back out the distribution of hidden factors parametrically under the frequentist framework, it is not clear how the missing data and the multiple testing problems can be simultaneously solved under the Bayesian framework. Second, the hierarchical testing framework

[58] See Jefferys and Berger (1992).

[59] Intuitively, more complex models are penalized because extra parameters involve additional sources of uncertainty. Simplicity is rewarded in a Bayesian framework as simple models produce sharp predictions. See the discussions in Scott (2009).

may be overly restrictive. Both independence as well as normality assumptions can have a large impact on the posterior distributions. Although normality can be somewhat relaxed by using alternative distributions, the scope of alternative distributions is limited as there are only a few distributions that can guarantee the properness of the posterior distributions. Independence, as we previously discussed, is likely to be violated in our context. In contrast, the three adjustment procedures under the frequentists' framework are able to handle complex data structures since they rely on only fundamental probability inequalities to restrict their objective function — the Type I error rate.

There are a few general concerns about the Bayesian multiple testing framework. First, it is not clear what to do after obtaining the posterior probabilities for individual hypotheses. Presumably, we should find a cutoff probability P and reject all hypotheses that have a posterior discovery probability larger than P. But then we come back to the initial problem of finding an appropriate cutoff p-value, which is not at all a clear task. Scott and Berger (2006) suggest a decision-theoretic approach that chooses the cutoff P by minimizing a loss-function. The parameters of the loss-function, however, are again subjective. Second, the Bayesian posterior distributions are computationally challenging. We document three hundred factors but there are potentially many more if missing factors are taken into account. When M gets large, importance sampling is a necessity. However, results of importance sampling rely on simulations and subjective choices of the centers of the probability distributions for random variables. Consequently, two researchers trying to calculate the same quantity might get very different results. Moreover, in multiple testing, the curse of dimensionality generates additional risks for Bayesian statistical inference.[60] These technical issues create additional hurdles for the application of the Bayesian approach.

[60]See Liang and Kelemen (2008) for a discussion on the computational issues in Bayesian multiple testing.

D Method for Controlling the FDP

Related to the false discovery rate, research by Lehmann and Romano (2005) tries to control the probability of the realized FDP exceeding a certain threshold value, i.e., $P(FDP > \gamma) \leq \alpha$, where γ is the threshold FDP value and α is the significance level.[61] Instead of the expected FDP (i.e., the FDR), Lehmann and Romano's method allows one to make a statement concerning the realized FDP, which might be more desirable in certain applications. For example, targeting the realized FDP is a loss control method and seems more appropriate for risk management or insurance. For our asset pricing applications, we choose to focus on the FDR. In addition, it is difficult to tell whether controlling the realized FDP at $\gamma = 0.1$ with a significance level of $\alpha = 0.05$ is more stringent than controlling FDP at $\gamma = 0.2$ with a significance level of $\alpha = 0.01$. While we use the FDR in our main application, we provide some details on the FDP methods here.

We apply the methods developed in Lehmann and Romano (2005) to control the realized FDP. The objective is $P(FDP > \gamma) \leq \alpha$, where γ is the threshold FDP value and α is the significance level. Fixing γ and α, we order the individual p-values from the smallest to the largest (i.e., $p_{(1)} \leq p_{(2)} \leq \cdots \leq p_{(M)}$) and let the corresponding hypotheses be $H_{(1)}, H_{(2)}, \cdots, H_{(M)}$. We then reject the i-th hypothesis if $p_{(i)} \leq \alpha_i / C_{\lfloor \gamma M \rfloor + 1}$, where

$$\alpha_i = \frac{(\lfloor \gamma i \rfloor + 1)\alpha}{M + \lfloor \gamma i \rfloor + 1 - i},$$

$$C_k = \sum_{j=1}^{k} \frac{1}{j}.$$

Here, for a real number x, $\lfloor x \rfloor$ denotes the greatest integer that is no greater than x. Similar to $c(M)$ in BHY's adjustment, $C_{\lfloor \gamma M \rfloor + 1}$ allow one to control the FDP under arbitrary dependence structure of the p-values.

Table D.1 shows the benchmark t-statistics based on our sample of 316 factors for different levels of FDP thresholds and significance. The benchmark t-statistics are higher when the FDP thresholds are tougher (i.e., γ is lower) or when the significance levels are lower (i.e., α is lower). For typical values of γ and α, the benchmark t-statistics are significantly lower than conventional values, consistent with previous results based on the FWER or FDR methods. For instance, when $\gamma = 0.10$ and $\alpha = 0.05$, the benchmark t-statistic is 2.70 (p-value = 0.69%), much higher than the conventional cutoff of 1.96.

[61] Also see Romano and Shaikh (2006) and Romano, Shaikh and Wolf (2008).

Table D.1: **Benchmark t-statistics for Lehmann and Romano (2005)**

Estimated benchmark t-ratios based on Lehmann and Romano (2005). The objective is $P(FDP > \gamma) \leq \alpha$.

	$\gamma = 0.05$	$\gamma = 0.10$	$\gamma = 0.20$
$\alpha = 0.01$	3.70	3.46	3.25
$\alpha = 0.05$	3.04	2.70	2.38
$\alpha = 0.10$	2.38	2.16	2.16

E FAQ

E.1 Specific Questions

- *Why is FWER called "rate" when it is a single number? (Section 4.3)*

  FWER has been used by the statistics literature a long time ago, even before 1979. However, Holm (1979) seems to be the first one that formally defines the *family-wise error rate*. Terms used in Holm (1979) are different from our current presentation. Our "*family-wise*" terminology is likely first mentioned in Cox (1982) and later formally defined in Hochberg and Tamhane (1987). "Rate" is the standard terminology nowadays, though we are not sure of the historical reason for calling it "rate" instead of probability. But we notice people using "Type I error rate" instead of "Type I error probability" in single hypothesis testing. We think that "probability" can be used interchangeably with "rate" since "probability" is "rate" in frequentists' view.

- *How can we tell in real time the errors? (Section 4.3.2)*

  We never know the "true" errors. Even with out-of-sample testing, all we can tell is how likely it is for a factor to be "real" for one particular *realization* of historical returns.

- *Is it possible to set the actual Type I error rate to be exactly at the pre-specified level? (Section 4.3.2)*

  Any adjustment procedure has a theoretically implied upper bound on the Type I error rate. This bound is the "actual Type I error rate" (as opposed to the realized Type I error rate for a particular outcome of a multiple test) and usually achievable under specific distributional assumptions (e.g.,negative dependence among p-values as in BHY). We usually use the distance between this bound and the pre-specified significance level to measure the goodness of a procedure. In reality, for a particular sequence of p-value realizations, e.g., 316 p-values for our 316 factors, we cannot do much. By following a specific adjustment procedure, we can say what the maximal *expected* Type I error rate is if we repeat such multiple testing many times, each time with a different p-value sequence. Comparing two procedures A and B, we want to know whether the expected Type I error rate (after integrating out the randomness in the return data) under A is closer to the significance level than it is under B. It makes little sense to compare A and B based on a particular outcome (e.g., 316 p-values) of a multiple test.

- *Why not just set a different $c(M)$ rather than setting $\alpha_d = 1\%$ for BHY? (Section 4.6)*

  Our choice of $c(M)$ makes BHY valid universally (that is, BHY is able to control the FDR at the pre-specified level regardless of the dependence structure in the data). The same is true for the other two methods: Holm and Bonferroni are also able to control the FWER at the pre-specified level regardless of test dependency. We focus on tests that are valid under general distributional assumptions. Nonetheless, we did mention what happens when $c(M) = 1$ in Section 4.7.2 as a robustness check.

- *Why doesn't the t-value go to something much larger than 3.5 after so many tests (Section 4.6)*

  We report t-statistics not p-values. Suppose you start with a cutoff p-value of 0.05. For a single test, the t-statistic needs to be 2.0 or above. Now consider a multiple testing framework. For simplicity, consider the Bonferroni test. If there are two tests, appropriate cutoff is a p-value of 0.025. For 10 tests, the p-value drops to 0.005. The table below shows the number of multiple tests necessary for certain levels of t-statistics. For example, if we had 87,214 tests, then the Bonferroni would require the factor to have a t-statistic of 5.0 to be deemed significant (p-value of 0.00000057).

  Table E.1: **Bonferroni t-statistics and Required Number of Tests**

  Bonferroni t-statistics, cut-off p-values and the required number of tests.

t-statistic	p-value	# of Bonferroni tests
2	0.05	1
3	0.0027	19
4	0.000063	789
5	0.00000057	87,214
6	0.0000000020	25,340,000
7	2.56×10^{-12}	1.95×10^{10}
8	1.33×10^{-15}	3.75×10^{13}

- *Why is there a drop for the time-series of BHY implied t-statistics? (Section 4.6)*

  In Figure 3, there seems to be a drop for BHY implied t-statistics around 1995. Unlike Bonferroni or Holm, BHY implied benchmark t-statistics are not necessarily monotonically increasing in the number of factors. This is because *false discovery rate* (FDR) is about the proportion of false discoveries among all discoveries. Given a set of t-statistics for the years before 1995, suppose we find the BHY implied adjusted t-statistic. In year 1995, suppose we observe a set of large t-statistics. These large t-statistics will likely increase the denominator of

FDR (i.e., the number of discoveries R). At the same time, they are unlikely to increase the numerator (i.e., the number of false discoveries $N_{0|r}$). As a result, including this new set of large t-statistics into the previous t-statistic set, the new BHY implied benchmark t-statistic will likely decrease. The highly significant t-statistics for 1995 dilute the proportion of false discoveries made based on the t-statistics from previous years.

- *Is "... control their Type I error rates under arbitrary distributional assumptions" really true? Suppose we had 186 factors but they were 99% correlated — effectively just one factor. This seems to me to be a situation where independent test criterion is appropriate. (Section 4.7)*

  The statement is correct and the concern is about the Type II rather than Type I error of the testing procedure. In the example, it is true that independent criterion makes more sense. But multiple testing procedures are also able to control the Type I error rates, albeit too much in this case. For instance, Bonferroni implies a threshold t-statistic of 3.8 when there are 316 factors. If most of the factors are perfectly correlated, then the FWER under Bonferroni's criterion is effectively zero. Since zero is less than any pre-specified significance level, the tests still control what they are supposed to control — Type I error rate (FWER or FDR). Of course, the power of the test, which, as previously discussed, can be measured by the distance between the actual Type I error rate and the pre-specified level, would be too low.

- *How does incomplete coverage of "significant" factors affect our results? (Section 4.7)*

  It is likely that our sample somewhat under-represents the population of significant factors. As previously discussed, there are a number of causes for this under-coverage. First, there are some truly significant factors that were tested as insignificant and never made it to publication. Second, we are highly selective in choosing among working papers. Third, we only consider the top three finance journals in choosing among published works. This under-coverage will impact our t-value cutoffs. To quantitatively evaluate this impact, we tabulate a new set of cutoffs: those generated under different degrees of under-representation of the population of significant factors. Table E.2 reports the cutoff t-statistics for 2012. Assuming a true population size over our sample size ratio of r, we report adjusted t-statistics for our three approaches.[62] The top row corresponds

[62] Assuming this r ratio and sample size N, we obtain Bonferroni adjusted t-statistics straightforwardly based on total number of factors Nr. For Holm and BHY, we sample (with replacement) $N(r-1)$ values from the recent 10 years' t-statistics sample. Together with the original sample, we have an augmented sample of Nr t-statistics. We follow Holm or BHY to get the adjusted t-statistic benchmarks for each augmented sample. Finally, we generate $W = 1000$ such augmented samples

to a sample size ratio of one, i.e., our original sample. We see that when the true population is twice as large as our sample, Bonferroni implied benchmark t-statistic increases from 3.78 to 3.95 and Holm from 3.64 to 3.83. Relative to the percentage change in t-statistics, the corresponding change in p-values is large. For Bonferroni, p-value changes from 0.016% to 0.008%; for Holm from 0.027% to 0.013%. Both p-values drop by at least half. For BHY, however, the change is less dramatic. This is consistent with our previous discussion of the stationarity of BHY. In sum, we think a robust t-statistic range for Bonferroni and Holm is 3.6-4.0; for BHY, 3.3-3.4 when $\alpha_d = 1\%$ and 2.80-2.85 when $\alpha_d = 5\%$.

Table E.2: **Cutoff t-statistics for Alternative Sample Sizes**

Benchmark t-statistics and their associated p-values for the three multiple testing adjustments for 2012. Sample size ratio is true population size over our sample size. Both Bonferroni and Holm have a significance level of 5%.

Sample size ratio (r) for significant factors	Bonferroni [p-value]	Holm [p-value]	BHY(1%) [p-value]	BHY(5%) [p-value]
1	3.78	3.64	3.34	2.78
	[0.016%]	[0.027%]	[0.08%]	[0.544%]
2	3.95	3.83	3.39	2.81
	[0.008%]	[0.013%]	[0.070%]	[0.495%]
3	4.04	3.93	3.41	2.84
	[0.005%]	[0.008%]	[0.065%]	[0.451%]

- *Haven't there been recent advances in Bayesian literature with respect to multiple testing? (Appendix C)*

  In papers that apply the Bayesian testing method, there are many new ways that try to handle inadequacies. For instance, to relax the independence assumption, Brown et al. (2012) introduce an autoregressive dependence structure because their data are obtained sequentially. But they have to assume that noise from the autoregressive processes is independent from the rest of the system. Conditional independence is key to Bayesian modeling. There are ways to circumvent it, but most methods are data-driven and not applicable in our context. For instance, it is unclear how to model dependence among the test statistics of factors in our list. The indeterminacy of the cutoff is mentioned in Scott and Berger (2006). There are many applied works that propose ad hoc methods to try to establish a threshold. Finally, computational difficulty is a longstanding

and take the median as the final benchmark t-statistic. When Nr or $N(r-1)$ are not integers, we use the smallest integer that is greater than or equal to Nr and $N(r-1)$, respectively.

issue in Bayesian literature. People often discard Bayesian methods because they incorporate a "subjective" prior (i.e.,generate random samples around a region where researchers "believe" the parameters should be concentrated) into their posterior calculation. Multiple testing introduces dimensionality concerns, and it is well-known that posterior distributions are hard to calculate accurately when the dimensionality is high. In sum, we think the above three issues are generic to the Bayesian multiple testing framework and for which there are no simple/systematic solutions.

E.2 General Questions

- *What if the underlying data are non-stationary in that as anomalies are discovered, they are arbitraged away; some newer frictions/biases arise, they are discovered, and then arbitraged away, and so on? This seems like a possible alternative that would lead to the creation of more and more factors over time, without necessarily implying that the t-statistic ought to be raised for newer factors.*

  Our preferred view is that the factor universe is a combination of some stationary factors that cannot be arbitraged away (systematic risk) and some other transitory factors that can be arbitraged away once discovered. As time goes by and we accumulate more data, stationary factors tend to become more significant (t-statistic proportional to the square root of the number of time periods). Through our multiple testing framework, the adjusted benchmark t-statistic becomes higher. This higher bar helps screen newly discovered transitory factors. In other words, it should be harder to propose new transitory factors as longer sample is available. Without multiple testing, recent transitory factors are just as likely to be discovered as past transitory factors. This means that the discovery rate for transitory factors will remain high (if not higher) as time goes by. This is exactly the trend that we try to curb. Ideally, the finance profession should focus on systematic/stationary factors rather than transitory abnormalities.

- *What if many of the factors are highly correlated or at least within a "span" other than the common 3-4 factors like the Fama-French three factors and Momentum which are controlled for while finding new factors? That is, is it possible that the literature has just been rediscovering "new" factors but they remain spanned by other documented factors that did not become an "industry" like Fama-French three factors and Momentum factors?*

  This is possible, although as we mentioned in the paper, newly proposed factors often need to "stand their ground" against similar factors (not just Fama-French three factors and Momentum) that are previously proposed. All of our three adjustments are robust to correlations among the factors. This means that the

Type I error (rate of false discoveries) is still under control. However, high correlations make our adjustment less powerful, that is, the benchmark t-statistic is too high for a new factor to overcome. However, given the hundreds of factors proposed, we think it is time to worry more about the Type I error than the power of the tests. A recent paper by Green, Hand and Zhang (2013) show that the correlations among strategy signals are low on average. This seems to suggest that new factors proposed in the literature are somewhat independent from past ones.

- *Should the benchmark t-statistics be higher simply because the number of data points has increased through time?*

  For a single, independent test, the t-statistic threshold should remain constant through time. For a return series that has a mean and variance, it is true that its t-statistic will increase as we have more data points. However, this does not imply a higher t-value threshold for hypothesis testing. At 5% significance level, we should always use 2.0 for single test as it gives the correct Type I error rate under the null hypothesis that mean return is zero. As time goes by, truly significant factors are more likely to be identified as significant and false factors are more likely to be identified as insignificant. In other words, the power of the test is improved as we have more observations but this should not change the cutoff value.

  In fact, when the sample size is extremely large, it becomes very easy to generate large t-statistics. In this case, people often use alternative statistics (e.g., odds ratios) to summarize strategy performance.

- *How does Kelly and Pruitt (2011, "The three-pass regression filter: A new approach to forecasting using many predictors") relate to our paper?*

  Kelly and Pruitt (2011) is related to our paper in that it also tries dimension reduction when there is a large cross-section. However, their paper is fundamentally different from ours. Kelly and Pruitt (2011) try to extract a few factors from the cross-section and use them to forecast other series. Therefore, the first-step extraction needs to be done in a way that increases the forecasting power in the second step. Our paper stops at the first stage: we look to condense the factor universe that can explain the cross-section of returns.

- *Why should modern discoveries be treated more harshly than early discoveries? There has been data mining since day one.*

  Suppose the rate of finding a true anomaly is constant through time, say 10%. In early days, 10 things are tried and 1 is a true discovery and 1 is a false discovery. Suppose 8 are discarded. In later days, 1000 things are tried and 100 are true discoveries and 100 are false discoveries. 800 are discarded. The proportion of true vs. false is identical. Hence, the basis for this argument is that early discoveries should not be treated differently that later discoveries.

However, we are assuming that the success rate of finding true discoveries is decreasing through time. There are many reasons to think this might be the case. First, similar to the Berk and Green argument, true factors become more scarce through time. That is, people pick the low hanging fruit first. Given the discoveries already documented, this lowers the probability of new true discoveries. Second, we run out of theories based on first principles and need to appeal to more specialized theories (which could be constructed to fit knowledge of the data). Third, there is a limit on the number of securities that are being examined.

So if the rate of finding true anomalies decreases, then in the example above, maybe in later days there are 50 true discoveries and 150 false discoveries. You can think of our paper as saying in the early times there were 2 discoveries (one true one false). In later times, using single test methods, there would be 200 discoveries. Using multiple testing methods, we would declare only 100 discoveries. Within the 100, there might be 50 true and 50 false.

- *A different method to perform multiple testing is to assume that hypotheses can be ordered a priori. Why don't we use it?*

  The method that assumes a priori ordered hypotheses explores the dependence between different hypotheses. It is usually applied to situations when there exists a priori ordering of hypotheses. For instance, in medical research sometimes hypotheses can be ordered by their scientific importance. We do not use it in our paper because it is difficult to come up with a priori ordering for factors. For example, we are not sure as a priori whether CAPM should be declared significant before we test C-CAPM.