Do $t$-Statistic Hurdles Need to be Raised?

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April 2024

Abstract

Many scholars have called for raising statistical hurdles to guard against false discoveries in academic publications. I show these calls may be difficult to justify empirically. Published data exhibit bias: results that fail to meet existing hurdles are often unobserved. These unobserved results must be extrapolated, which can lead to weak identification of revised hurdles. In contrast, statistics that can target only published findings (e.g. empirical Bayes shrinkage and the FDR) can be strongly identified, as data on published findings is plentiful. I demonstrate these results theoretically and in an empirical analysis of the cross-sectional return predictability literature.

JEL Classification: G0, G1, C1

Keywords: stock market predictability, stock market anomalies, p-hacking, multiple testing

First posted to SSRN: September 25, 2018. I thank Antonio Gil de Rubio, Preston Harry, Jack McCoy, and Nelson Rayl for excellent research assistance, Rebecca Wasyk for excellent scientific programming, and Dino Palazzo and Fabian Winkler for many valuable discussions. I also thank three anonymous referees, an anonymous associate editor, Christine Dobridge, Bjorn Eraker, Cam Harvey, Laura Liu, Alan Moreira (discussant), Nelson Rayl, Alessio Saretto (discussant), Ivan Shaliastovich, Mihail Velikov, and seminar participants at the Federal Reserve Board, George Mason University, University of Cologne, and the University of Wisconsin for helpful comments. The views expressed herein are those of the authors and do not necessarily reflect the position of the Board of Governors of the Federal Reserve or the Federal Reserve System.
1 Introduction

Suppose a researcher proposes a “factor” behind a phenomenon. How do we determine if this factor is worth noting? At least since Fisher (1925), researchers have used the following procedure: (1) construct a statistic that is Student’s $t$-distributed in the case that the factor is false, and (2) declare the factor a discovery if this $t$-statistic exceeds 1.96 in absolute value. More recently, several papers have called for raising this $t$-statistic hurdle, or “$t$-hurdle” to guard against false discoveries (Harvey, Liu and Zhu (2016); Chordia et al. (2020)), including a paper with dozens of co-authors (Benjamin et al. (2018)).

In this paper, I examine whether these calls can be empirically justified. An empirical justification is critical, as prior beliefs regarding cutting edge research are sure to vary across scholars. Moreover, an empirical justification seems possible, as multiple testing statistics provide methods for estimating hurdles that control the false discovery rate (FDR) (Benjamini and Hochberg (1995)). Indeed, FDR methods are central to Harvey, Liu and Zhu (2016)’s argument and the concept is also used in Benjamin et al. (2018).

I find that raising the $t$-hurdle may be difficult to justify empirically. My results differ from previous studies because I acknowledge weak identification: the problem that likelihood functions may depend little on certain model parameters (Canova and Sala (2009)). This problem is important because the data on academic discoveries exhibit publication bias: results that fail to meet the existing $t$-hurdle are often unobserved. Unobserved results need to be extrapolated, which can lead to weak identification of the key determinants of $t$-hurdles. I characterize this problem in a theoretical analysis that extends Benjamini and Hochberg (1995) to a setting with publication bias (as in Hedges (1992)). In an empirical analysis, I bootstrap $t$-hurdle estimates using a rich dataset of published cross-sectional stock return predictors (Chen and Zimmermann (2022)). Consistent with the theory, the empirical estimates say little about whether hurdles should be raised, stay the same, or even be lowered.

At the same time, I find other multiple testing statistics are more strongly identified. Empirical Bayes shrinkage and the FDR among published factors (Chen and Zimmermann (2020); Chen (2021b)) focus on the right tail of $t$-stats,

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1 In the traditional language, a “false factor” is called a “true null hypothesis” while a “true factor” is called a “false null hypothesis.” Some readers may find the traditional language confusing, hence my choice of terminology.

2 For contrasting prior beliefs regarding asset pricing, see Cochrane (2017) and Barberis (2018).
and this portion of the distribution tends to be well-observed, in spite of publication bias. For cross-sectional predictors, I find that published t-stats are biased upward by at most 28% and that the FDR for published predictors is at most 22%, with 95% confidence. This strong identification, as well as the weak identification of t-hurdles, is also found across 10 alternative model specifications. Robustness is intuitive, as the theoretical results impose few functional form assumptions. Readers may differ on whether a worst-case FDR of 22% is satisfactory, but at least some will interpret these results as implying that t-hurdles need not be raised for the cross-sectional predictability literature.

How can t-hurdles not be raised? This idea seems to fly in the face of multiple testing logic. If one tests 1,000 factors, 5% will meet the 1.96 hurdle on average, even if all 1,000 factors are false. This scenario is visualized in Panel (a) of Figure 1, which depicts the distribution of absolute t-stats implied by a standard normal. By luck, 50 of the 1,000 false factors meet the classical hurdle. But since all factors are false, the FDR is 50/50 = 100%. Clearly, this scenario implies that the classical hurdle needs to be raised.

**Figure 1: Why t-hurdles may not need to be raised.** Panels illustrate two possible scenarios. Bars are stacked. Panel (a) shows a scenario with 1,000 factors, all of which are false. Panel (b) shows 1,000 false factors and 1,000 true factors, with the distribution of true factors selected to match data on published t-statistics in McCrary, Christensen and Fanelli (2016).

The problem with this logic is that it ignores the information obtained from

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3Here, the FDR is the number of false and significant factors, divided by the number of significant factors, in expectation (Benjamini and Hochberg (1995)).
multiple testing. Each additional test is another data point, which brings news about the veracity of factors overall. This news can in general be positive or negative. More data does not necessarily mean more bad news.

The data on published factors looks more like Panel (b). In this panel, there are once again 1,000 false factors (red), so 50 false factors sneak past the 1.96 hurdle. But now there are an additional 1,000 true factors (blue), and among all factors, 1,000 have t-stats that exceed 1.96. Thus, the 50 false factors represent only 5% of the 1,000 factors that meet the classical hurdle. Supposing that an FDR of 5% is sufficient, as it is in a variety of fields from genomics to functional imaging (Benjamini (2020)), the classical hurdle need not be raised at all.

Panel (b) illustrates how multiple testing provides more information. With a single test, asking the question “what share of t-stats exceed 1.96?” is impossible to answer. But with multiple testing, answering this question is a straightforward counting exercise. With many tests one can also estimate the share of factors that are false (e.g. Storey (2002)). If this share is small, then a t-stat of 1.5 may be due to bad luck, in which case the classical hurdle can be lowered (Benjamini and Hochberg (2000)).

My theoretical results characterize exactly when FDR methods imply a raising of statistical hurdles. The theory revolves around $\pi_F$, the share of false factors among all factors under consideration. Assuming that an FDR of 5% is sufficient, the t-hurdle should be raised if and only if $\pi_F$ exceeds the share of t-stats larger than 1.96. Panel (b) of Figure 1 shows the knife edge case in which both of these objects are equal to 50%, and thus the classical t-hurdle is sufficient.

The key to empirically justifying a higher t-hurdle, then, is to empirically justify a large $\pi_F$. Unfortunately, my theoretical results suggest that $\pi_F$ is weakly identified under publication bias. I show that, if false and true factors are in a sense distinct, then values of $\pi_F$ ranging from 0 to 2/3 imply almost identical distributions in the region in which data is well-observed. As a result, empirical studies may say little about the proper t-hurdle.

To quantify this problem, I specify a parametric model of biased publication and fit it to the Chen and Zimmermann (2022) dataset using quasi-maximum likelihood. The model includes the key parameter $\pi_F$, as well as additional pa-

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4McCrary, Christensen and Fanelli (2016) shows the distribution of t-stats from meta-studies in political science, psychology, economics, and all fields of science. Of t-stats that exceed 1.96, roughly half also exceed 3.0. In Figure 1, Panel (b), parameters are selected to match this moment. A similar distribution is seen in cross-sectional asset pricing (Chen and Zimmermann (2020)).
rameters that fully describe the distribution of t-stats. The estimated parameters, in turn, provide consistent formulas for revised t-hurdles. Bootstrapped estimates show that $\pi_F$ is weakly identified: I re-sample the data, re-estimate the model, and find that the estimated $\pi_F$ ranges from 0% to 70% (90% confidence interval). t-hurdles that control the FDR at 5% range from 0 to 3.0 (90% C.I.).

More positively, I find that multiple testing statistics that target published findings are strongly identified, even in the presence of publication bias. The shrinkage and FDR for published t-stats tend to be determined by the properties of true factors. And under the same conditions that imply weak identification of $\pi_F$, the properties of true factors are strongly identified. Intuitively, if $\pi_F$ does not affect the observed distribution, it must be the properties of true factors that determine the data. Empirical estimates confirm strong identification for the Chen-Zimmermann dataset. These results are robust to a cluster-bootstrap that closely mimics correlations in the empirical data.

An alternative method for handling publication bias is to algorithmically generate a complete set of factors (Yan and Zheng (2017); Chordia et al. (2020)). Standard FDR methods can then be applied and $\pi_F$ estimated without the identification problems introduced by publication bias. This $\pi_F$, however, should be interpreted as an upper bound on the $\pi_F$ of the literature, as expert researchers should be able to find true factors at a higher rate than a simple algorithmic procedure (Chen (2021b)).

Code to replicate all figures and tables can be found at https://github.com/chenandrewy/qml-pub-bias.

1.1 Related Literature

Many papers examine multiple testing effects in cross-sectional asset pricing. Among these papers, Harvey et al. (2016); Chordia et al. (2020); and Harvey and Liu (2021) focus on t-hurdle corrections. To these papers, I add a framework for understanding when t-hurdles need to be raised. The framework shows that most estimates in Harvey et al. (2016) and Chordia et al. (2020) assume that t-stat hurdles need to be raised, and thus they cannot answer the question of whether t-hurdles need to be raised. It also highlights how the multiple testing algorithm

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5 These papers include Harvey et al. (2016); Yan and Zheng (2017); Chordia et al. (2020); Chen and Zimmermann (2020); Jacobs and Müller (2020); Chen (2021a,b); Harvey and Liu (2021); and Jensen et al. (Forthcoming).
perhaps most common in finance (Benjamini and Yekutieli (2001) Theorem 1.3) is likely too conservative.

I also add to Harvey et al. (2016); Chordia et al. (2020); and Harvey and Liu (2021) by examining the identification problems that come with publication bias (Copas (1999); Hedges and Vevea (2005)). These problems would be seen in standard errors on t-hurdles, which are not provided by Harvey et al. (2016) or Harvey and Liu (2021). I revisit their estimates and find that the standard errors are so wide they say little about whether t-stat hurdles should be raised or even be lowered.

In a contemporaneous paper, Harvey and Liu (2021) critique three assumptions that have been used in this literature: (1) cross-factor correlations are all equal, (2) the publication probability is a strict t-stat cutoff, and (3) the selected parametric models may be too restrictive. I find that none of these issues has a significant effect on the small shrinkage and FDR estimates found by Chen and Zimmermann (2020) and Jensen et al. (Forthcoming). I find similar estimates assuming (1) weak dependence across factors, (2) a smoothly increasing publication probability, and (3) many distinct parametric forms for latent effect sizes. Harvey and Liu propose instead to model latent effect sizes by randomly drawing from empirical data on published and data-mined factors. This method deviates strongly from the literature (Efron (2012); Andrews and Kasy (2019)) and it has not been shown to recover effect sizes either theoretically or in simulations.

Identification under publication bias is also studied in Andrews and Kasy (2019), who prove identification of a non-parametric model. Their proof obtains the latent distributions by solving a system of ODEs, effectively assuming an unlimited sample of published tests. My analysis is less restrictive about data availability. Also unlike Andrews and Kasy, I connect to the literature on false discovery rates and empirical Bayes shrinkage. More broadly, my paper helps bridge the literatures on publication bias (Hedges and Vevea (2005); McShane et al. (2016)) and multiple testing (Efron (2012)) and provides guidance on which multiple testing corrections should be applied under publication bias.

There are other arguments against raising t-hurdles. Raising t-hurdles would lead to published data that is in a sense more distorted. This reasoning motivates, in part, the push for pre-analysis plans, which lower t-hurdles provided that the analysis is rigorously pre-specified (Olken (2015); Kasy and Spiess (2023)). Raising t-hurdles also fails to address common misinterpretations of statistical
significance and artificial dichotimization introduced by hypothesis testing (McShane et al. (2019); Chen and Zimmermann (2023)).

2 Theoretical Results

I describe a general model (Section 2.1), define multiple testing statistics (Section 2.2), and then prove results that illustrate weak and strong identification (Sections 2.3-2.4). Section 2.5 explains why some commonly-used FDR methods cannot answer the question of whether t-stat hurdles should be raised. Section 2.6 discusses false negative rates.

2.1 A Model of Multiple Testing and Publication Bias

A literature is generated in two steps. In the first step, researchers generate an “unbiased” set of $N$ factors that obey the assumptions in Benjamini and Hochberg (1995). Factor $i$ has a t-stat $t_i$ that follows

$$t_i | \mu_i \sim \text{Normal}(\mu_i, 1)$$ (1)

where $\mu_i$ is the latent effect size of factor $i$ (e.g. the expected return) divided by its standard error. $\mu_i$ can also be thought of as the “corrected” t-stat, since it corrects $t_i$ for sampling error. $\mu_i$ depends on whether factor $i$ is false ($F_i$) or true ($T_i$)

$$\mu_i | F_i = 0.$$ (2)

$$\mu_i | T_i \sim g(\cdot | \lambda),$$ (3)

where $g(\cdot | \lambda)$ is an arbitrary probability distribution and $\lambda$ is a vector of parameters. Last, and most importantly, factor $i$ is false with probability $\pi_F$.\(^6\)

I describe the factors from the first step as “unbiased” because $t_i | F_i \sim \text{Normal}(0, 1)$, and thus the theory of Fisher (1925) and Benjamini and Hochberg (1995) applies. However, the second step of literature generation creates a bias.

In the second step, factor $i$ is published depending on its statistical signifi-

\(^6\)Benjamini and Hochberg (1995) leave open the possibility of other distributional assumptions for Equation (1), but the standard normal is commonly used in practice (Efron (2012); Harvey et al. (2016)). Chen (2021a) shows that the standard normal assumption holds for long-short portfolios from the asset pricing literature.
where \( \text{pub}_i \) is the event that factor \( i \) is published, \( s(|t_i|) \) is a function with values in \([0,1]\), and \( \tilde{s} \) and \( t_{\text{good}} \) are constants. Conceptually, \( s(|t_i|) \) captures the prevailing t-stat hurdles that were applied in the generation of the published data.

This second step implies \( t_i \sim F_{i,\text{pub}_i} \sim \text{Normal}(0,1) \), violating the assumptions of both Fisher (1925) and Benjamini and Hochberg (1995). Thus, to estimate the FDR and related statistics, one needs to recover the properties of the unbiased factors generated in the first step.

Equations (4)-(6) describe a “satisficing” model of publication bias. They say that a higher t-stat implies a higher probability of publication but once the t-stat is high enough the community is satisfied and there are no additional gains. A satisficing model is consistent with the view that researchers are primarily incentivized to uncover convincing and interesting mechanisms and that statistical significance plays a secondary role. This assumption nests the functional forms used in previous estimates of publication bias (Harvey et al. (2016); Andrews and Kasy (2019); Chen and Zimmermann (2020)).

I refer to t-stats that exceed \( t_{\text{good}} \) as “well-observed.” \( t_{\text{good}} \) is typically 1.96 (Andrews and Kasy (2019)) or a number between 1.96 and 3.0 (Harvey et al. (2016); Chen and Zimmermann (2020)). Relatively few t-stats below \( t_{\text{good}} \) are observed and the data moments in this region are distorted. In contrast, the likelihood conditional on \( |t_i| > t_{\text{good}} \) is not distorted, so one can make inferences about \( \pi_F \) and \( \lambda \) directly from the conditional moments in this region.

In reality, the probability of publication depends not only on \( |t_i| \) but also on supporting evidence. However, data on supporting evidence is rarely available, so the publication bias literature focuses on Equation (4), which ignores supporting evidence. The net effect of this misspecification is unclear. Omitting supporting evidence tends to bias estimates of \( \pi_F \) and \( \lambda \) in a way that implies stronger latent effects, as these parameters must absorb the larger \( |t_i| \) values induced by the supporting evidence. On the other hand, omitting supporting evidence tends to imply weaker latent effects, as the supporting evidence is by definition a signal
of strong latent effects. Appendix A formalizes these issues and provides some simulation evidence suggesting that the net bias is small.

The model abstracts from dynamic issues like out-of-sample decay in effect size. More generally, the effect size depends on whether it is measured in the original sample ($\mu_i$) or out-of-sample ($\mu_i^{OOS}$). While researchers typically aim to find factors with stable effect sizes, empirical evidence in cross-sectional asset pricing finds $\mu_i^{OOS} \approx 0.50 \mu_i$ (McLean and Pontiff (2016); Chen and Zimmermann (2020)). Thus, finding that $\mu_i$ is close to $t_i$ does not necessarily imply out-of-sample robustness.

### 2.2 Hurdles that Control the False Discovery Rate

Calls for raising statistical hurdles often come down to controlling the false discovery rate with a t-stat hurdle:

$$\text{hurdle}(5\%) \equiv \min_{h \in \mathbb{R}_+} \{h : \text{Fdr}(h) \leq 5\%\}. \quad (7)$$

where $\text{Fdr}(h)$ is the Bayesian formulation from Efron (2008):

$$\text{Fdr}(h) \equiv \Pr(F_i \mid |t_i| > h). \quad (8)$$

(see also Efron et al. (2001) and Storey (2002)). Equation (7) finds the lowest t-stat hurdle that results in less than 5% of factors being false, where 5% is chosen for ease of exposition (the main results hold for other choices). $\text{Fdr}(h)$ is used in both Ioannidis (2005) and Benjamin et al. (2018). Jensen et al. (Forthcoming) estimates an object similar to $\text{Fdr}(1.96)$ for cross-sectional return predictors. The (2018) version of Chen and Zimmermann (2020) estimates both hurdle (5%) and $\text{Fdr}(h)$.

As shown by Storey (2002) and Storey et al. (2004), the Bayesian formulation is equivalent to the Benjamini-Hochberg (1995; 2000) approach under weak dependence. To see this, define the Benjamini-Hochberg FDR:

$$\text{FDR}_{BH}(h) \equiv \mathbb{E} \left[ \frac{\sum_{i=1}^{N} I(F_i \cap |t_i| > h)}{\sum_{i=1}^{N} I(|t_i| > h)} \right], \quad (9)$$

where $I(\cdot)$ is an indicator function and I assume $\sum_{i=1}^{N} I(|t_i| > h) > 0$. Then sup-
pose the following weak law of large numbers hold: For any $h \in \mathbb{R}_+$,

$$\frac{1}{N} \sum_{i=1}^{N} I(|t_i| > h) \xrightarrow{p} \Pr(|t_1| > h) \tag{10}$$

$$\frac{1}{\pi F N} \sum_{i=1}^{N} I(F_i \cap |t_i| > h) \xrightarrow{p} \Pr(|t_1| > h | F_1) \tag{11}$$

These expressions say that if you keep counting the shares of factors that exceed a hurdle, you’ll eventually find the probability that a factor exceeds a hurdle. Rearranging Equation (9) and plugging in Equations (10)-(11) leads to Equation (8):

$$\mathbb{E} \left[ \frac{\sum_{i=1}^{N} I(F_i \cap |t_i| > h)}{\sum_{i=1}^{N} I(|t_i| > h)} \right] = \mathbb{E} \left[ \frac{\sum_{i=1}^{N} I(F_i \cap |t_i| > h) / (\pi F N) \pi F N}{\sum_{i=1}^{N} I(|t_i| > h) / N N} \right] \xrightarrow{p} \frac{\Pr(|t_1| > h | F_1) \Pr(F_1)}{\Pr(|t_1| > h)} = \Pr(F_i | | t_i | > h), \tag{12}$$

where the last equality applies Bayes rule. Harvey et al. (2016)’s Section 4 uses Equation (9) in the constraint of Equation (7) to argue that t-stat hurdles need to be raised.

### 2.3 Weak Identification of t-stat Hurdles

As discussed in Benjamini and Hochberg (2000) (Remark 2), Equations (7) and (8) imply that hurdles need not be raised and may even be lowered. The following proposition characterizes exactly when this occurs:

**Proposition 1.** If $\Pr(F_i || t_i | > h)$ is strictly decreasing in $h$, then

- $\text{hurdle}(5\%) > 1.96$ if and only if $\pi F > \Pr(||t_i| > 1.96)$ \hspace{1cm} (13)
- $\text{hurdle}(5\%) = 1.96$ if and only if $\pi F = \Pr(||t_i| > 1.96)$ \hspace{1cm} (14)
- $\text{hurdle}(5\%) < 1.96$ if and only if $\pi F < \Pr(||t_i| > 1.96)$ \hspace{1cm} (15)

The proof is in Appendix B.1.

Proposition 1 says the t-stat hurdle needs to be raised if and only if $\pi F$ is larger than $\Pr(||t_i| > 1.96)$. This comparison is visualized in Panel (b) of Figure 1. $\pi F$ is the share of false factors (red) relative to the total mass, while $\Pr(||t_i| > 1.96)$ is the mass to the right of the vertical line. In this illustration, these probabilities are equal, so the multiple testing hurdle is equal to the classical one. In contrast, Panel (a) shows a setting in which the share of the red mass (100%) is much larger.
than the mass to the right of the vertical line (5%), implying that hurdle needs to be raised.

Some readers may have the intuition that $\pi_F > \Pr(|t_i| > 1.96)$ must be the “right” case of Proposition 1. This intuition is perhaps natural, as statistics is a conservative institution. However, there is no logical reason for why we must have $\pi_F > \Pr(|t_i| > 1.96)$. Assuming that this expression holds amounts to placing additional restrictions on the model of Section 2.1 and it is unclear how to motivate these restrictions. Thus, it is the role of data to tell us which case of Proposition 1 is the right one. For example, Benjamini et al. (2006) reviews several methods for estimating $\pi_F$.

The methods in Benjamini et al. (2006), however, assume that all factors are observed. Under publication bias (Equations (4)-(6)) it may be difficult to identify $\pi_F$. Indeed, the meta-study literature on publication bias runs into flat likelihood functions and estimation problems for other parameters (Copas (1999) and Hedges and Vevea (2005)). The following proposition shows publication bias leads to identification problems for $\pi_F$:

**Proposition 2. (Weak Identification)** If there exists $\epsilon > 0$ such that for all $\bar{t} > t_{\text{good}}$,

\[
\frac{\Pr(|t_i| \leq \bar{t}|F_i)}{\Pr(|t_i| \leq \bar{t}|T_i)} < \epsilon
\]

then for any $\pi \in [0, 2/3]$, $\pi' \in [0, 2/3]$, and $\bar{t} > t_{\text{good}}$,

\[
\left| \frac{\Pr(|t_i| \leq \bar{t}|t_i > t_{\text{good}}|F_i)}{\Pr(|t_i| \leq \bar{t}|t_i > t_{\text{good}}|T_i)} - 1 \right| < 2\epsilon + O(\epsilon^2),
\]

where $\Pr(|t_i| \leq \bar{t}|t_i > t_{\text{good}}|F_i)_{\pi_F = \pi}$ is

\[
\Pr(|t_i| \leq \bar{t}|t_i > t_{\text{good}}) = \frac{\pi_F \Pr(|t_i| \leq \bar{t}|F_i) + (1 - \pi_F) \Pr(|t_i| > \bar{t}|T_i)}{\pi_F \Pr(|t_i| > t_{\text{good}}|F_i) + (1 - \pi_F) \Pr(|t_i| > t_{\text{good}}|T_i)}.
\]

evaluated at $\pi_F = \pi$.

The proof is in Appendix B.2.

Proposition 2 says that, under certain assumptions, $\pi_F$ has little effect on model predictions. In particular, Equation (17) says the distribution of t-stats in the well-observed region changes by at most a factor of $2\epsilon$ across values of $\pi_F$. 

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ranging from 0 to 2/3. The bound of 2/3 is chosen for illustrative purposes. More
generally, a bound of $\tilde{\pi}$ for $\pi_F$ implies an upper bound of $\tilde{\pi}/(1 - \tilde{\pi})\varepsilon$ on the RHS
of Equation (17) (see proof).

The key assumption is Equation (16), which formalizes the implicit assumption
in hypotheses testing that true and factors are distinct. If true and false fac-
tors are not distinct, then the pursuit of separating true from false factors is in a
sense misguided. In my setting, false factors by definition have $|t_i|$ close to zero,
so distinctiveness can be thought of as saying that true factors are more likely to
have $|t_i| > t_{\text{good}}$. Equation (16) says that true factors are $1/\varepsilon$ times more likely to
be found in this region than false factors, where $\varepsilon$ is presumably small.

Figure 2 illustrates Proposition 2 by overlaying alternative parameterizations
of Harvey, Liu and Zhu (2016)’s parametric model. HLZ’s baseline estimate im-
plies $\pi_F = 0.444$ and $g(\cdot|\lambda)$ is an exponential distribution with mean of about 2.
This estimate implies that raising t-hurdles is necessary. According to HLZ’s Ta-
ble 5, the classical t-hurdle of 1.96 needs to be raised to 2.27 to control the FDR
at 5%. The distribution of t-stats implied by HLZ’s baseline estimate is shown in
the blue bars of Figure 2.

[Figure 2 about here]

But now consider an alternative model, that uses all the same parameters as
HLZ’s baseline but simply changes $\pi_F$ to zero. With no false factors, the alterna-
tive model implies that t-hurdles can be lowered, all the way to 0. The predicted
distribution of t-stats is shown in the red bars of Figure 2.

These two models differ markedly in their densities of t-stats near zero. How-
ever, these t-stats are difficult to observe and must be extrapolated. Indeed, HLZ
assume $t_{\text{good}} = 2.57$ (vertical line), and for $|t_i| > t_{\text{good}}$ the two distributions are
nearly identical. Thus, it is unlikely that the data can tell us whether t-stats
should be raised, stay the same, or even be lowered.

This identification problem should show up in standard error estimates. They
imply that small perturbations to the data imply very different t-stat hurdles, and
thus large standard errors. HLZ do not provide standard errors for their hurdle
estimates. I fill this gap in Section 3.
2.4 Strongly-Identified Multiple Testing Statistics

Revised hurdles are not the only way to deal with a many-factor setting. Chapter 1 of Efron (2012)’s textbook on large scale inference examines empirical Bayes shrinkage:

\[
\text{shrinkage}(\hat{t}) \equiv \frac{|\hat{t}| - E(\mu_i | t_i = \hat{t})}{|\hat{t}|} \quad (19)
\]

where \( \hat{t} \in \mathbb{R}_+ \). Equation (19) measures how much you should shrink an observed t-stat (\(|t_i|\)) toward zero in order to recover its unbiased counterpart \( E(\mu_i | t_i = \hat{t}) \). For example, shrinkage (4.0) = 0.25 means that a t-stat of 4.0 should be shrunk by 25% and that the unbiased t-stat is 4.0 \times (1 - 0.25) = 3.0. Since the sample mean return is proportional to \( t_i \), the same shrinkage adjustment applies to the sample mean return. Asset pricing papers that study empirical Bayes shrinkage include Chen and Zimmermann (2020), Chinco et al. (2021), Chen and Velikov (2023), and Jensen et al. (Forthcoming).

Chapter 2 of Efron (2012) examines the local FDR:

\[
\text{fdr}(\hat{t}) \equiv \Pr(F_i | t_i = \hat{t}). \quad (20)
\]

\( \text{fdr}(\hat{t}) \) is just the probability that a factor with a t-stat of \( \hat{t} \) is false. Integrating \( \text{fdr}(\hat{t}) \) from \( h \) to infinity leads to Equation (8).

In contrast to hurdle (5%), empirical Bayes shrinkage and the local fdr can be used to target only published findings (by selecting \( \hat{t} \) to be equal to the t-stats of published factors). And in the published region, shrinkage and the local fdr depend more so on the parameter vector that governs true factors (\( \lambda \)). To see this, note that the key term in Equation (19) can be written as

\[
E(\mu_i | t_i = \hat{t}) = E(\mu_i | t_i = \hat{t}, T_i) \left[ \frac{1}{1 + \Delta} \right] \quad (21)
\]

\[
\Delta \equiv \left( \frac{\pi_F}{1 - \pi_F} \right) \left[ \frac{f_{|t_i|F}(\hat{t})}{f_{|t_i|T}(\hat{t})} \right] \quad (22)
\]

where \( f_{|t_i|F}(\hat{t}) \) and \( f_{|t_i|T}(\hat{t}) \) are the densities of \(|t_i|\) given that \( i \) is false or true, respectively. Intuitively, shrinkage of the full model is the same as shrinkage assuming \( \pi_F = 0 \), but with a correction that increases in \( \pi_F \). A similar argument can be made for the local FDR. Appendix B.3 provides the derivations.
For empirically relevant \( \tilde{t} \) and \( \lambda \), \( \Delta \) is often small. For example, the HLZ dataset has a mean \(|t_i|\) of around 4.0, corresponding to \( f_{|t| F}(4.0) = 0.00027 \). Meanwhile, their estimates imply \( f_{|t| T}(4.0) = 0.076 \), as true factors are much more likely to have large t-stats. So even if \( \pi_F \) is as large as 0.9, \( \Delta \) is only 0.03. Similarly large t-stats are commonly found in replications of cross-sectional predictability papers (Chen and Zimmermann (2022)). More broadly, Brodeur et al. find that t-stats of 4.0 are quite common in the main hypotheses tests reported in top economics journals.

This analysis rests critically on \( \lambda \), which must be estimated with published data and its associated identification problems. Fortunately, the following proposition shows that \( \lambda \) can be strongly identified

**Proposition 3. (Strongly Identified Parameters)** If there exists \( \varepsilon \) such that for all \( \tilde{t} > t_{good} \),

\[
\frac{\Pr(|t_i| \in [t_{good}, \tilde{t}]|F_i)}{\Pr(|t_i| \in [t_{good}, \tilde{t}]|T_i)} < \varepsilon
\]

and \( \pi_F < 2/3 \), then

\[
\left| \frac{P(|t_i| < \tilde{t}||t_i| > t_{good})}{P(|t_i| < \tilde{t}||t_i| > t_{good}, T_i)} - 1 \right| \leq 2\varepsilon + O(\varepsilon^2).
\]

The proof is in Appendix B.4.

Proposition 3 says that, under the same condition that lead to weak identification of t-hurdles (Equation (16)), the model’s predictions about well-observed data (\( P(|t_i| < \tilde{t}||t_i| > t_{good}) \)) are almost the same as the predictions that come from assuming all factors are true (\( P(|t_i| < \tilde{t}||t_i| > t_{good}, T_i) \)). As in the Proposition 2, this proposition uses the \( \pi_F < 2/3 \) for illustrative purposes. The more general case in which \( \pi_F \) is bounded by \( \bar{\pi} \) implies an upper bound of \( \bar{\pi}/(1 - \bar{\pi})\varepsilon \) on the RHS of Equation (24) (see proof).

An implication of Proposition 3 is that a reasonable estimate of \( \lambda \) could potentially be found by estimating the model while assuming \( \pi_F = 0 \). This result can be seen in Figure 2. The red bars are essentially the model that one would estimate assuming \( \pi_F = 0 \). Thus, assuming \( \pi_F = 0 \) would lead to roughly the same \( \lambda \) as HLZ’s baseline estimate, in which \( \pi_F = 0.44 \). Intuitively, if \( \pi_F \) is not at all responsible for fitting the t-stats to the right of 1.96, it must be the other parameters in HLZ’s estimate that accomplish this.
Taken with Equation (21), Proposition 3 implies that the bias in large t-stats could potentially be identified with just published data. This result is seen in the small standard errors in Chen and Zimmermann (2020)’s shrinkage estimates as well as the robustness of their estimates to alternative modeling assumptions. Indeed, Chen and Zimmermann’s headline bias estimate of 12% is not far from Harvey and Liu (2021)’s median estimate of 19%, even though the two papers use very different modeling assumptions and estimation methods. Similar estimates are also found in Chinco et al. (2021) and Jensen et al. (Forthcoming), who use yet another set of methods (see discussion in Chen and Zimmermann (2023)). Section 3 provides additional evidence that these bias estimates are strongly identified for the cross-sectional predictability literature.

The key to strong identification is flexibility. Unlike, hurdle(5%), shrinkage(\(\bar{t}\)) and fdr(\(\tilde{t}\)) allow one to focus on the portion of the data that is well-observed. If the data are not informative about shrinkage (1.0), one can instead examine shrinkage (4.0). The cost of this flexibility is that shrinkage (\(\bar{t}\)) and fdr(\(\tilde{t}\)) do not provide a precise answer to the question, “do t-hurdles need to be raised?” They provide important evidence (e.g. published t-stats are biased upward by 12%). But ultimately an additional framework is required to convert this evidence into a new t-hurdle.

Also unlike hurdle(5%), shrinkage (\(\bar{t}\)) provides economic magnitudes. Thus, it avoids the potentially artificial dichotomization that can come from hypothesis testing (McShane et al. (2019)). This gain, however, comes at additional analytical costs, namely that shrinkage (\(\bar{t}\)) requires selecting \(g(\cdot|\lambda)\) and estimating \(\lambda\).

An important caveat of this analysis is that it relies on \(\pi_F\) being not too close to 1.0. For \(\pi_F = 0.9\) or higher, the \(2\varepsilon\) bound becomes a \(9\varepsilon\) bound (or higher), and Proposition 3 may have little bite. Intuitively, if almost all factors are false, then even the extreme right tail of the distribution will be largely determined by false factors. However, we will see that empirical evidence suggests \(\pi_F \geq 0.9\) is unlikely cross-sectional return predictors (Section 3).

\[\text{Under publication bias, the local FDR can be bounded without estimating } \lambda \text{ by using a kind of worst case scenario (Chen (2021b)).}\]
2.5 Conservative FDR Controls

Some FDR methods avoid estimation of $\pi_F$ by effectively assuming $\pi_F \geq 1.0$. As a result, these conservative methods assume that t-hurdles need to be raised, and thus cannot answer the question of whether t-hurdles need to be raised.

These conservative algorithms include Benjamini and Yekutieli (2001)’s (BY’s) Theorem 1.3, which is emphasized in HLZ, and is popular in the finance literature. As shown by Storey (2002), BY’s Theorem 1.3 is equivalent to replacing the constraint in Equation (7) with an estimator:\(^8\)

$$h_{BY1.3}(5\%) \equiv \min_{h \in \mathbb{R}_+} \{ h : \widehat{\text{FDR}}(h) \leq 5\% \} \quad (25)$$

where

$$\widehat{\text{FDR}}(h) \equiv \frac{\Pr(|t_i| > h | F_i)}{\widehat{\Pr}(|t_i| > h)} \left( \sum_{i=1}^{N} \frac{1}{i} \right)$$

$$\widehat{\Pr}(|t_i| > h) \equiv \frac{N^{-1} \sum_{i=1}^{N} I(|t_i| > h)}{N - \sum_{i=1}^{N} I(|t_i| > h)} \quad (26)$$

Compare Equation (26) to the result of applying Bayes rule to $\Pr(F_i | |t_i| > h)$

$$\Pr(F_i | |t_i| > h) = \frac{\Pr(|t_i| > h | F_i)}{\Pr(|t_i| > h)} \pi_F$$

Thus, $h_{BY1.3}(5\%)$ effectively assumes that $\pi_F = \sum_{i=1}^{N} \frac{1}{i}$. In HLZ’s case, where $N \approx 300$, we have $\pi_F = 6.3$. So this algorithm implies that, not only is $\pi_F$ large, but it is six times larger than 1.0. Since $\pi_F > 1.0$ is physically impossible, it is hard to justify why such a severe penalty is necessary. Indeed, $h_{BY1.3}(5\%)$ is described as “a severe penalty” and “not really necessary” (Efron (2012)). Even the original Benjamini and Yekutieli (2001) paper says this algorithm is “very often unneeded, and yields too conservative of a procedure.”

An implication of this conservatism is that BY’s Theorem 1.3 always implies that t-hurdles need to be raised. Assuming $\pi_F > 1$, only one case of Proposition 1 is possible:

**Corollary 1.** If $\widehat{\text{FDR}}(\cdot)$ is strictly decreasing, then $h_{BY1.3}(5\%) > 1.96$.  

\(^8\)This is seen Equation (13) of Storey (2002). See also the Equivalence Theorem of Efron and Tibshirani (2002). Strictly speaking, these papers show this equivalence for the Benjamini and Hochberg (1995) algorithm, however, the BY algorithm simply modifies the Benjamini and Hochberg (1995) algorithm with a constant factor.
The proof is in Appendix B.5.

Similarly, the seminal Benjamini and Hochberg (1995) (BH95) algorithm is equivalent to replacing \( \sum_{i=1}^{N} \frac{1}{T} \) with 1.0 in Equation (26). Thus, BH95 is isomorphic to using \( \pi_F = 1.0 \) in Equation (7), and Proposition 1 implies that BH95 raises t-hurdles in all cases except for the extreme case in which \( \sum_{i=1}^{N} I(|t_i| > 1.96) = N \).

The benefit of imposing \( \pi_F = 1.0 \) is simplicity. The BH95 hurdle can be computed for an arbitrary dataset without any additional specifications. The cost, however, is that BH95 discards all information in the data that can inform us about \( \pi_F \). So while BH95 is an excellent first pass for controlling for multiple testing, it cannot tell us which case of Proposition 1 holds, and cannot tell us whether t-hurdles must be raised.\(^9\)

In their first draft, Benjamini and Hochberg (1995) did \textit{not} assume that t-stat hurdles should be raised. As described in Benjamini (2010), the 1989 draft recommended a graphical method for estimating \( \pi_F \). After many years of rejections, the authors shifted to the conservative formulation that became the seminal BH95 algorithm. The original algorithm, along with the result that t-stat hurdles could possibly be lowered, was eventually published in Benjamini and Hochberg (2000), and many statisticians went on to propose additional estimators for \( \pi_F \) (Efron et al. (2001); Allison et al. (2002); Storey (2002); Genovese and Wasserman (2004); Benjamini et al. (2006), among others).

\section{2.6 False Negative Rates}

A natural question is how publication bias affects identification of false negative rates (FNRs), also known as the type II error rate. This question can be analyzed using a Bayesian formulation of the FNR:

\[
\text{Fnr}(h) \equiv \Pr(T_i \mid |t_i| \leq h)
\]  

(see Efron (2012) Chapter 4.3). Fnr\((h)\) is the probability a factor is true, given that the factor fails to meet the hurdle \( h \). One can alternatively define the FNR using factor counts as in Equation (9) (see Genovese and Wasserman (2002)) but these

\(^9\)Unlike the finance literature, the statistics literature tends to favor the BH95 algorithm over BY’s Theorem 1.3 (Benjamini (2020)). HLZ argue for using BY’s Theorem 1.3, as it controls the FDR under arbitrary dependence. But BH95 controls the FDR under weak dependence (Storey et al. (2004)) and simulations suggest FDR control under arbitrary dependence if the tests in question use z-statistics (Reiner-Benaim (2007)).
definitions are equivalent under weak dependence.

Applying Bayes rule shows how the FNR runs into identification issues:

\[
Fnr(h) = 1 - \frac{\Pr(|t_i| \leq h | F_i)}{\Pr(|t_i| \leq h)} \pi_F. \tag{30}
\]

In the presence of publication bias, \(\pi_F\) may be weakly identified (Proposition 2). Moreover, the very definition of publication bias (Equations (4)-(6)) implies that the data that bears on \(\Pr(|t_i| \leq h)\) is limited. Intuitively, to estimate a false negative rate one needs to know the number of insignificant t-stats, and publication bias means that insignificant t-stats are poorly observed.

An alternative way to address the FNR is to use the FDR control that comes closest to achieving the constraint in Equation (7). Achieving this constraint would typically involve forming a point estimate of \(\pi_F\) (rather than assuming an upper bound), as is pursued in this paper.

3 Empirical Estimates of t-Stat Hurdles

The theoretical results illustrate weak and strong identification, but a precise description of these issues requires empirical estimates of sampling uncertainty. This section provides one such estimate by bootstrapping estimates using a dataset constructed from asset pricing publications.

3.1 Data

My data begins with 207 published cross-sectional stock return predictors from the Chen and Zimmermann (2022) (CZ) dataset (March 2022 release). I focus on their original predictor portfolios, which consists of long-short portfolios constructed following the procedures in the original studies.

This dataset has several advantages as a setting for studying publication bias. CZ show that the replicated t-stats closely match the originals, which rules out coding errors, fraud, or other more nefarious sources of bias. CZ also show that the pairwise correlations cluster around zero (see also Chen (2021b); Bessembinder, Burt and Hrdlicka (2021)), suggesting that the weak dependence assumptions underlying standard estimation methods are valid (Wooldridge (1994)), and that modeling the correlations will add relatively little to estimation efficiency.
Moreover, the monthly returns in this dataset can be used to account for correlations in my bootstrapped standard errors. Chen (2021a) shows the distribution of t-stats is quite similar to the distribution found in HLZ, which eases comparison. Last, this dataset is publicly available at www.openassetpricing.com.

To simplify the baseline model, I drop predictors with in-sample t-stats that fall below 1.96, leading to a sample of 183 predictors. The approach of dropping t-stats < 1.96 is also used in HLZ. Carefully modeling the 24 predictors with smaller t-stats requires a relatively complicated publication probability function and is likely to introduce more identification problems (Copas (1999)). In the robustness section, I include t-stats < 1.96 and find broadly similar results (Section 4.1).

Since all of CZ’s factors are cross-sectional return predictors, I refer to them as “predictors” in what follows.

### 3.2 Structural Model and Estimation

The structural model adds three assumptions to the model of Section 2.1. The additional structure can be thought of as restrictions that help identify the key model objects (e.g. \( \pi_F, \Pr(|t_i| > 1.96) \)).

The first is the unbiased t-stat \( \mu_i \) is log-normal for true factors

\[
\mu_i | T_i \sim \text{Log-normal}(\lambda_{\mu}, \lambda_{\sigma}).
\]  

(31)

where \( \lambda_{\mu} \) and \( \lambda_{\sigma} \) are mean and standard deviation of \( \log(\mu_i) | T_i \), respectively. This form is arguably the simplest assumption one can make about \( \mu_i | T_i \) while ensuring that true predictors have an expected return that is distinct from 0. Section 4.4 shows that alternative distributonal assumptions have little effect on the results.

The second assumption is a functional form for the publication probability (Equation (4)). I assume a staircase function

\[
\Pr\{\text{pub}_i | |t_i|\} = \begin{cases} 
\eta \tilde{s} & \text{if } |t_i| \in (1.96, 2.58] \\
\tilde{s} & \text{if } |t_i| > 2.58.
\end{cases}
\]

(32)

where the t-stat cutoffs of 1.96 and 2.58 correspond to the traditional 5% and 1% significance cutoffs, respectively, and \( \eta \) represents a “haircut” for marginally-
significant predictors. One can think of this function as the simplest intuitive model of selective publication for marginally-significant predictors. This functional form is also assumed in HLZ. Section 4.3 shows that a logistic form (as in Chen and Zimmermann (2020) and Harvey and Liu (2021)) leads to similar results.

The last assumption is that $\eta$ lies within an interval obtained from intuition

$$\eta \in [1/3, 2/3].$$

Equation (33) says that at least some marginally-significant predictors are missing ($\eta \leq 2/3$), but at least a significant minority are reported ($\eta \geq 1/3$). HLZ uses the more restrictive assumption that $\eta = 1/2$, though they also examine $\eta = 1/3$. Section 4.3 shows that relaxing this assumption or restricting this assumption has little effect on the main results.

I also assume $\pi_F \in [0.01, 0.99]$ for technical reasons. I use numerical integration to compute the log-likelihoods and multiple testing statistics, and I find that these methods can become poorly behaved for $\pi_F$ that is extremely close to 0 or 1.0. One can increase the range of $\pi_F$ with more complicated numerical methods but this complication is unlikely to affect the main results.

I estimate $\theta = \left(\pi_F, \lambda_\mu, \lambda_\sigma\right)$ with quasi-maximum likelihood (QML). I choose $\hat{\theta}$ to maximize the mean log marginal likelihood of $|t_i|$ conditional on $i$ being published. I do not estimate $\bar{s}$, as it is not identified (it is cancelled out in the conditional likelihood). Under technical conditions, this QML estimator is consistent (Wooldridge (1994); Liu, Moon and Schorfheide (2020)).

Intuitively, the model implies that the mean derivatives of the marginal likelihoods are zero when evaluated at the true parameters, and this implication can be used as moment conditions to estimate the model.

I confirm that QML is consistent under pairwise correlations as high as 0.90 in Appendix C.1. Indeed, in simulated samples of only 200 t-stats, I find QML is essentially unbiased for $\pi_F$ and produces standard errors of around 0.10 to 0.15. These simulations assume no publication bias and demonstrate that the large standard errors I find for $\pi_F$ are not due to QML.

I measure estimation uncertainty using two bootstraps. My baseline bootstrap is simple and non-parametric: I draw a full set of t-stats from the empirical

$^{10}$The key technical assumption is that the log marginal likelihood of a single observation satisfies the uniform weak law of large numbers.
data with replacement and re-run QML. This bootstrap is transparent and also ensures that the bootstrapped data displays a similar selection bias and distributional properties as the original dataset. Moreover, cross-sectional predictor correlations are typically close to zero (McLean and Pontiff (2016); Chen and Zimmermann (2022)). These mild correlations suggest that this simple bootstrap will lead to similar results as one that accounts for correlations.

For robustness, I also examine a semi-parametric bootstrap that carefully accounts for correlations. In short, this second approach combines a cluster bootstrap with a parametric bootstrap. The cluster bootstrap ensures that the samples closely mimic the correlation structure in the data while the parametric bootstrap ensures that the samples mimic the distribution of observed t-stats. Details on the semi-parametric bootstrap are found in Appendix C.2

3.3 Parameter Estimates and Intuition

Table 1 shows the resulting parameter estimates. The point estimate finds that essentially all factors are true ($\hat{\pi}_F = 0.01$), but the 90% confidence interval is huge, ranging from 0.01 to about 0.90. Even the 50% C.I. is quite large, ranging from 0.01 to about 0.70. There results are consistent with Proposition 2 and suggest that t-hurdles are weakly identified.

[Table 1 about here]

Huge uncertainty about $\pi_F$ obtains regardless of whether I use the simple non-parametric bootstrap (shown in the table with no parentheses) or the semi-parametric cluster bootstrap (shown with parentheses). For all parameters in Table 1, accounting for correlations has almost no effect on estimation uncertainty. This result is intuitive, as the typical predictor correlation is close to zero (Chen and Zimmermann (2022), see also Figure A.3 in the Appendix).

In contrast to $\pi_F$, the parameters that govern true factors ($\lambda_\mu$ and $\lambda_\sigma$) are strongly identified. Table 1 converts these parameters into the expected unbiased t-stat $E(\mu_i|T_i)$ and standard deviation of the unbiased t-stat SD($\mu_i|T_i$) for ease of interpretation.\textsuperscript{11} The bootstrapped estimates imply that $E(\mu_i|T_i)$ is be-

\textsuperscript{11} The lognormal distribution implies

\begin{align*}
E(\mu_i|T_i) &= \exp\left(\lambda_\mu + \sigma_\mu^2/2\right) \\
SD(SE_i) &= \sqrt{\exp(\lambda_\mu^2) - 1} \exp(2\lambda_\mu + \lambda_\sigma^2).
\end{align*}
tween 2.0 and 3.8 while SD(μ_i | T_i) is between 1.9 and 2.7, with 90% confidence. These results are consistent with Proposition 3.

Figure 3 provides the intuition behind these results. It compares the predictions of the point estimate with an alternative model that also uses QML, but the alternative model fixes π_F at 2/3. Panel (a) shows that both models fit data to the right of 2.58 very well. Recall that this is the subsample of the data that is well-observed (Equation (32)). Thus, even a slight perturbation to the observed data can move the point estimate from π_F = 0.01 to π_F = 2/3.

The models differ in their predictions about t-stats between 1.96 and 2.58, suggesting the data in this region can help identify π_F. However, many t-stats in this region may be missing, and it is difficult know a-priori just how many. Both QML estimates restrict uncertainty by assuming that the fraction missing is between 1/3 and 2/3 (Equation (33)). Nevertheless, this restriction is unable to identify π_F. Section 4.3 shows that even assuming the missing fraction is 0.5 cannot identify π_F

Thus, the key to identification is the distribution of t-stats below 1.96. As seen in Figure 3, the two models have very different predictions for this region. The data tell us very little about which is the right story, as there are only 24 observations in this region. Indeed, a close read of Chen and Zimmermann (2022) shows that even these 24 observations are poorly defined, as it is unclear if these observations should even be called “predictors.”

This ambiguity leads me to drop t-stats below 1.96 in the estimation (though they are still shown in Figure 3). Nevertheless, Section 4 shows that taking on this additional data still implies substantial uncertainty. Intuitively, only 7 t-stats below 1.5 are observed, and it is very hard to say how representative these t-stats are. This very small sample problem suggests that adding the standard errors of predictor mean returns to the estimation, which in principle can non-parametrically identify the publication probability (Andrews and Kasy (2019)), will still result in weak identification of π_F. Indeed, previous versions of this paper did include these standard errors in the estimation and found similar results.12

12Code for the previous versions can be found at https://github.com/chenandrewy/t-hurdles.
Panel (b) of Figure 3 illustrates strong identification. It examines the distribution of true predictors’ t-stats \( t_i|T_i \) in the point estimate and the alternative model. Despite very different implications about \( \pi_F \), both models imply similar distributions of \( t_i|T_i \). Intuitively, a highly dispersed \( t_i|T_i \) is required to fit the long right tail in observed t-stats, regardless of \( \pi_F \). Since \( t_i|T_i \) is just \( \mu_i|T_i \) plus standard normal noise, this result implies that the parameters governing \( \mu_i|T_i \) are strongly identified, leading to the relatively narrow confidence bounds in Table 1.

Returning to Table 1, the probability of publishing a marginally-significant t-stat \( \eta \) tends to be larger than 50% in its bootstrapped distribution, implying that the asset pricing literature does not strongly discriminate against predictors with t-stats between 1.96 and 2.58. This result is intuitive, as the asset pricing literature contains relatively little discussion of marginal significance, and instead focuses on economic mechanisms.

Table 1 shows that the bootstrapped \( \eta \) often bumps against the upper bound of 2/3 imposed by the model (Equation (33)). This restriction was imposed to exclude the possibility that weak identification is due to an excessively general model. Section 4.3 shows that relaxing these bounds does not change the main results.

As very similar estimates obtain using either the non-parametric or semi-parametric bootstrap, for the remainder of the paper I discuss only the non-parametric bootstrap.

### 3.4 Weak Identification of t-stat Hurdles for Cross-Sectional Predictors

With bootstrapped parameters in hand, I can finally quantify weak and strong identification of multiple testing statistics.

Figure 4 plots the bootstrapped distribution of t-hurdles (Equation (7)). Panel (a) examines the more common FDR upper bound of 5%, which results in a highly dispersed t-stat hurdle of between 0 and 3.0. In other words, the data say little about how to adjust t-hurdles for multiple testing. Indeed, the classical 5% counterpart of 1.96 lies right in the middle of the distribution, implying substantial uncertainty about whether the hurdle should be raised or lowered.
The estimates imply a large mode in t-hurdles close to 0, reflective of the large mode in bootstrapped estimates of \( \pi_F \) close to zero that is implicit in Table 1. This mode is intuitive given the shape of the empirical distribution (Figure 3): This unimodal shape provides little evidence in support of a bimodal distribution in \( \mu_i \), and thus simply assuming \( \mu_i \) is lognormal (\( \pi_F = 0 \)) provides a strong fit to the data. This result is also consistent with Proposition 3 and Figure 2, which show that the observed distribution of the full model is approximately the same as a model with \( \pi_F = 0 \).

Some readers may find \( \pi_F = 0 \) to be implausible, but Figure 4 suggests that this mode is immaterial. Excluding the mode at 0, the distribution of t-hurdle estimates is still highly dispersed, with values ranging from 1.0 to 3.0. Additionally, Section 4.2 shows that restricting \( \pi_F \geq 0.20 \) still results in substantial uncertainty about the right t-hurdle.

Panel (b) shows that weak identification is also seen if one selects the unusually restrictive FDR \( \leq 1\% \) t-hurdle. The resulting distribution of t-hurdles is, once again, highly dispersed, ranging from 0 to 3.5. The classical 1% hurdle of 2.58 lies well-within the interior of this distribution.

### 3.5 Strong Identification of Shrinkage and the Local FDR

Figure 5 shows that the data are informative about shrinkage and the local FDR for published predictors. Panel (a) examines shrinkage by evaluating Equation (19) using bootstrapped data and then taking the mean across published t-stats within each bootstrap. The panel shows the distribution of this mean across bootstraps. The vast majority of the distribution lies below 26%, which is the upper bound on statistical bias estimated in McLean and Pontiff (2016)'s out-of-sample tests. This result implies that published sample mean returns are at least 74% due to true expected returns rather than publication bias, multiple testing, or other related statistical effects.

[Figure 5 about here]

The mode of the shrinkage distribution is close to 12%, which is the point estimate found by Chen and Zimmermann (2020). This similarity obtains despite the fact that I assume a bi-modal distribution in t-stats, providing a counterexample to Harvey and Liu (2021)'s claim that Chen and Zimmermann's estimate is
sensitive to their unimodal assumption. Indeed, Section 4 shows Chen and Zimmermann’s estimate is robust to 10 alternative models, all of which use bi-modal distributions.

Panel (b) shows similar results for the mean local FDR. Panel (b) computes the mean local FDR by evaluating Equation (20) at bootstrapped parameter values and taking the mean across published t-stats within the bootstrap. Quantitatively similar to Panel (a), Panel (b) finds that published factors are at least 75% true, with high confidence. The figure displays a large mode near 0, due to the unimodal shape of the empirical data (see Section 3.4), but even if this mode were excluded the estimates still imply that the published predictors are largely true with high confidence.

For comparison, Figure 5 also plots the mean local FDR for published predictors implied by HLZ’s baseline estimates. HLZ’s estimate implies that only 6% of published findings are false, close to the middle of the bootstrapped distribution. This result is surprising, given HLZ’s verbal statement that “most claimed research findings in financial economics are likely false.” However, this estimate is natural given their numerical estimates.

To understand HLZ’s numerical estimates, note that the mean local FDR for published factors is approximately the Bayesian expression

$$
\mathbb{E}\left[ \text{fdr}(t_i) \middle| \text{pub}_i \right] \approx \Pr \left( F_i \middle| |t_i| > 1.96 \right) = \frac{\Pr \left( |t_i| > 1.96 \middle| F_i \right)}{\Pr \left( |t_i| > 1.96 \right)} \pi_F. \quad (36)
$$

HLZ find that $\pi_F = 0.444$ (Table 2) and that of 1,378 total t-stats (also Table 2), 353 t-stats that exceed 1.96 (page 28). Plugging these numbers into Equation (36) leads to

$$
\mathbb{E}\left[ \text{fdr}(t_i) \middle| \text{pub}_i \right] \approx \frac{\frac{5\%}{353/1378}}{0.444} = 8.6\%.
$$

This approximation is a touch higher than the 6% found by applying Equation (20) to their model simulation because the simulation assumes a more stringent publication criteria (only half of t-stats between 1.96 and 2.58 are published). Chen (2021b) finds that even HLZ’s conservative estimates imply the FDR is rather small.
4 Robustness

Figures 4 and 5 depend on functional form and distributional assumptions. However, Propositions 2 and 3 do not, suggesting that similar results may obtain under a wide variety of assumptions.

This subsection confirms this robustness for 10 alternative sets of assumptions. Table 2 shows the 5th and 95th percentile of multiple testing statistics, computed across 500 bootstrapped estimates for each set of assumptions. Most of the 90% confidence intervals for t-hurdles span 0 and 2.9, implying substantial uncertainty about the proper correction to the classical hurdle of 1.96. In contrast, all specifications imply shrinkage is at most 29%, and the local FDR is at most 25%, with 95% confidence.

[Table 2 about here]

Indeed, previous versions of this paper found similar results for additional estimation and modeling assumptions, including estimations that account for standard errors and t-stats separately, and estimations that modeled the full distribution of correlations. Additional robustness can be found using the github code ([a github site]) or using the github code for the previous draft ([a github site])

The remainder of this section briefly describes each set of assumptions and their motivations.

4.1 Robustness to Using Small Chen-Zimmerman t-stats

Section 3.1 simplifies modeling by dropping the 24 t-stats that fall below 1.96. Specifications (2) and (3) include these small t-stats—as long as they exceed 0.50. The three t-stats that fall below 0.50 are 0.06, 0.09, and 0.40. Including these tiny t-stats leads to shrinkage estimates that divide by tiny numbers (Equation (19)), leading to extreme outliers that drive the means.

To accommodate the additional 21 t-stats that fall below 1.96, I add two ad-
ditional steps to the staircase publication probability:

\[
\Pr(\text{pub}_i) = \begin{cases} 
\eta_a \bar{s} & \text{if } |t_i| \leq 1.50 \\
\eta_b \bar{s} & \text{if } |t_i| \in (1.50, 1.96] \\
\eta_c \bar{s} & \text{if } |t_i| \in (1.96, 2.58] \\
\bar{s} & \text{if } |t_i| > 2.58.
\end{cases}
\tag{37}
\]

where the additional cutoff of 1.50 is chosen to match the cutoff used in McLean and Pontiff (2016). 1.50 is also close to the two-sided 10% hurdle of 1.64.

Specifications (2) and (3) differ in how they restrict the parameters \(\eta_a\), \(\eta_b\), and \(\eta_c\). Specification (2) makes no restrictions. Specification (3) assumes \(\eta_c \in [1/3, 2/3]\) (same as in the baseline), and imposes that the other probabilities are smaller than 1/3.

### 4.2 Robustness to Assuming \(\pi_F\) is at Least \(X\%\)

The baseline estimates show a large mode at \(\hat{\pi}_F = 0.01\) (Table 1), leading to large modes in Figures 4 and 5. Some readers may find such a small \(\pi_F\) implausible and would like to restrict \(\pi_F\) to some minimum value.

In Table 2, Specifications (4) and (5) use the restrictions \(\pi_F \geq 0.10\) and \(\pi_F \geq 0.20\), respectively.

### 4.3 Robustness to Alternative Publication Probability Functions

The baseline model restricts \(\eta \in [1/3, 2/3]\) to show that weak identification is not due to an excessively general model. Specification (6) restricts even further to \(\eta = 0.5\), which is HLZ’s baseline assumption.

Specification (7) examines a more general \(\eta \in [1/3, 1.0]\). This choice is motivated by the fact that the baseline bootstrap bumps up against the upper bound of \(\eta = 2/3\) (Table 1), suggesting that \(\eta \geq 2/3\) is preferred by the data.

Specification (8) assumes a logistic form for the publication probability

\[
\Pr(\text{pub}_i) = \frac{1}{1 + \exp(-\eta_b(|t_i| - \eta_a))},
\]

which is the same function used in Chen and Zimmermann (2020) (see also Cochrane...
4.4 Robustness to Alternative Distributional Assumptions

The baseline model assumes $\mu_i | T_i$ is log-normal, as this is the simplest assumption one can make that ensures that true predictors have a positive mean return that is distinct from 0.

In Table 2, Specification (9) assumes $\mu_i | T_i$ is exponential following HLZ and Specification (10) assumes $\mu_i | T_i$ is a scaled t-distribution, similar to Andrews and Kasy (2019) and Chen and Zimmermann (2020). Specification (11) assumes $\mu_i | T_i$ is a mixture of two normals, which can be motivated by the idea that there are two distinct types of true predictors, each with a bell-shaped distribution.

Specifications (10) and (11) often imply that $\mu_i | T_i$ can be either positive or negative, which implies that the sign of $t_i$ should be accounted for in the shrinkage estimation (Equation (19)). For these two rows, the shrinkage shown thus uses

$$\text{shrinkage}_{\text{signed}}(\bar{t}) = \frac{\bar{t} - E(\mu_i | t_i = \bar{t})}{\bar{t}}. \tag{38}$$

which implicitly assumes that the journals use theory to find the appropriate sign of predictability. Most papers in the Chen-Zimmermann dataset motivate their signs using theory.

5 Conclusion

Motivated by concerns about scientific credibility, a series of papers call for statistical hurdles to be raised. I show these calls may be difficult to justify empirically. Publication bias means that the key parameter in these arguments may be weakly identified. As a result, empirical data may say little about whether hurdles should be raised, stay the same, or even be lowered.

More positively, I find that other multiple testing statistics can be strongly identified. In particular, shrinkage and the local FDR for published factors may be able to be pinned down without knowledge of t-stats near zero. For the cross-sectional return predictability literature, these statistics imply that published findings are at least 75% true, with high confidence, across many model specifica-
What “at least 75% true” says about the proper \( t \)-hurdle is subjective, but at least some readers will argue that a literature that is mostly true is a healthy one. Still, others may argue that a literature should strive for trust, and even for achieving “no questions asked” from their readers.

A caveat about these empirical results is that they are based on the field of cross-sectional predictability. While other fields of economics also feature very large \( t \)-stats (Brodeur et al. (2016)), a rigorous analysis is required to pin down the FDR. Moreover, cross-sectional predictability is known for its clean data, standardized methods, and strong replicability (Chen and Zimmermann (2022); Jensen et al. (Forthcoming)). These features allay concerns about coding errors and the improper calculation of \( t \)-stats that may remain if shrinkage and FDR estimates were applied to other fields.

Regardless, my results demonstrate that the debate about scientific credibility should take multiple testing statistics more seriously. These statistics do not simply say that statistical hurdles should be raised, or that a large share of findings are false. Identification is important, and some multiple testing statistics are more strongly identified than others. Most important, my paper shows that the debate should focus on the more strongly identified statistics.
A Model with Supporting Evidence

This section presents a simple model for analyzing the bias that comes from ignoring supporting evidence.

As in Section 2.1, the t-stat $t_i$ provides a noisy signal of the latent effect $\mu_i$, but for simplicity assume $\mu_i$ is normal:

$$t_i | \mu_i \sim \text{Normal}(\mu_i, 1)$$
$$\mu_i \sim \text{Normal}(0, \lambda)$$

where $\lambda$ in this setting is the variance of $\mu_i$. This is the key parameter that determines shrinkage.

Add to the Section 2.1 model supporting evidence, represented by $z_i$:

$$z_i | \mu_i \sim \text{Normal}(\mu_i, 1)$$
$$\text{Cov}(t_i, z_i | \mu_i) = \rho$$

where $\rho$ allows for correlation between the noise components of $t_i$ and $z_i$. One can think of $z_i$ as representing aspects of a research paper that are not captured by the primary t-stat.

Now, extend Equation (4) to allow publication to depend on $z_i$:

$$\text{Pr}(\text{pub}_i | t_i, z_i) = \begin{cases} \hat{s} & (t_i > 2.5) \cap (z_i > z_{min}) \\ 0 & \text{otherwise} \end{cases}$$

where the strict cutoffs and the lack of absolute values on $t_i$ are chosen for simplicity. The key parameter in this expression is $z_{min}$, which captures the quality of supporting evidence required by the literature.

Let $\hat{\lambda}$ be the following estimate that ignores the supporting evidence:

$$\hat{\lambda} = \arg \min_{\lambda} \left( N^{-1} \sum_{i: \text{pub}_i} t_i - E(t_i | t_i > 2.5; \lambda) \right).$$

This expression formalizes the misspecification error. The correct model moment is $E(t_i | t_i > 2.5, z_i > z_{min}; \lambda)$, not $E(t_i | t_i > 2.5; \lambda)$.

In general, $\hat{\lambda}$ is upward biased. This happens because the target moment $E(t_i | t_i > 2.5, z_i > z_{min}; \lambda)$ is increasing in $z_{min}$, due to the fact that both $t_i$ and $z_i$
are positively related to $\mu_i$. Figure A.1 visualizes this bias. An upward-biased $\hat{\lambda}$ implies a downward biased shrinkage.

**Figure A.1: Supporting Evidence and Bias in $\hat{\lambda}$**

Model is described in Appendix A, with $\rho = 0$. Grey is $E(t_i|t_i > 2.5; \lambda)$ as a function of $\lambda$ and red is $E(t_i|t_i > 2.5, z_i > 1; \lambda)$. The “Data” line and the choice of $z_{\text{min}} = 1.0$ as “actual” are arbitrary and selected for illustration.

There is, however, is an offsetting bias. Ignoring supporting evidence in the empirical Bayes estimate of $\mu_i$ leads to

$$E(\mu_i|t_i; \lambda) < E(\mu_i|t_i, z_i; \lambda) \quad \text{for } z_i > z_{\text{min}} \quad (40)$$

since $z_i$ is positively related to $\mu_i$ and published data tend to have high $z_i$. A downward bias in estimates of $\mu_i$ leads to an upward bias in shrinkage.

Which bias dominates is not immediately clear. In a simulation analysis, I find the net bias can go either way, though the magnitudes of the bias are generally moderate. I examine many simulations with values of $\lambda$ ranging from 0.5 to 12, $\rho$ from 0 to 0.6, and $z_{\text{min}}$ from 0 to 2.

Figure A.2 presents the simulation results. In each simulation, I estimate $\hat{\lambda}$ using the potentially misspecified Equation (39), compute the potentially misspecified empirical Bayes estimate of $\mu_i$ (Equation (40)), and then compute shrinkage by taking the mean estimated $\mu_i$ for published $i$ and dividing by the mean $t_i$ for published $i$. Actual shrinkage uses the actual $\mu_i$. The actual shrinkage can be both above and below the estimated shrinkage. But generally the errors are small.
I simulate the model in Appendix A under various parameter values, estimate \( \hat{\lambda} \) ignoring the supporting evidence, and then estimate shrinkage.

Relative to the estimated shrinkage.

The errors can become noticeable for \( z_{\text{min}} = 2.0 \), suggesting that even higher levels of \( z_{\text{min}} \) can result in substantial bias. One can argue, however, that the standards for supporting evidence should not be as strict as the standards for the main evidence, and thus \( z_{\text{min}} < 2.0 \) is reasonable.

**B Proofs**

**B.1 Proof of Proposition 1**

*Proof.* In this proof, I consider a more general version of the proposition, in which the FDR-based and traditional hurdles use different critical levels. For this more general proof, we need to define a general classical hurdle:

\[
h_0(\alpha_0) \equiv \min_{h \in \mathbb{R}_+} \{ h : \Pr( |t_i| > h | F_i) \leq \alpha_0 \},
\]

where \( \alpha_0 \) is the classical significance level. Similarly, generalize Equation (7) as

\[
h_{FDR}^*(\alpha) \equiv \min_{h \in \mathbb{R}_+} \{ h : \Pr( F_i | | t_i| > h) \leq \alpha \}.
\]
Since $\Pr(F_i| | t_i > h)$ is strictly decreasing, the solution to the minimization (7) solves $\Pr(F_i| | t_i > h^{*}_{FDR}(\alpha)) = \alpha$. Rewrite this using Bayes rule:

$$\alpha = \Pr(F_i| | t_i > h^{*}_{FDR}(\alpha)) = \frac{\Pr(\|t_i > h^{*}_{FDR}(\alpha)|F_i)}{\Pr(\|t_i > h^{*}_{FDR}(\alpha))} \pi_F$$

Then Equation (41) is solved with $\Pr(\|t_i > h_0(\alpha_0)|F_i) = \alpha_0$. Multiplying both sides by Equation (42) we have

$$\Pr(\|t_i > h_0(\alpha_0)|F_i) = \Pr(\|t_i > h^{*}_{FDR}(\alpha)|F_i) \left[ \frac{\alpha_0}{\alpha} \frac{\pi_F}{\Pr(\|t_i > h^{*}_{FDR}(\alpha))} \right].$$

From here, we have three cases for the square bracket term, but the first case is symmetric with the rest.

For the first case, suppose

$$1 > \left[ \frac{\alpha}{\alpha_0} \frac{\Pr(\|t_i > h^{*}_{FDR}(\alpha))}{\pi_F} \right].$$

Note that the above expression is equivalent to the condition in Equation (13), if we use $\alpha = \alpha_0 = 5\%$. Multiply both sides of this inequality with (43) to get the equivalent expression

$$Pr(|t_i > h_0(\alpha_0)|F_i) > Pr(|t_i > h^{*}_{FDR}(\alpha)|F_i).$$

Since $Pr(|t_i > h|F_i)$ strictly decreases in $h$, the above equation is equivalent to

$$h_0(\alpha_0) < h^{*}_{FDR}(\alpha).$$

Thus we have shown the equivalence in Equation (13).

For the other two cases, simply replace $>$ with either $=$ or $<$ in the previous paragraph.
B.2 Proof of Proposition 2

Proof. For ease of notation, define

\[
\delta(X) \equiv \frac{\Pr(X | F_i)}{\Pr(X | T_i)}
\]

\[
L(\pi) \equiv \frac{\pi}{(1 - \pi)}
\]

where \(X\) is some event. Note that for \(X = |t_i| \in [t_{\text{good}}, \infty)\) we have by assumption (16)

\[
\delta(X) < \varepsilon.
\]

Use this notation to rewrite Equation (18):

\[
\Pr(|t_i| \leq \bar{t} || t_i > t_{\text{good}}) = \frac{\Pr(|t_i| \in (t_{\text{good}}, \bar{t}) | T_i)}{\Pr(|t_i| > t_{\text{good}} | T_i)} \left[ \frac{1 + L(\pi_F) \delta(|t_i| \in (t_{\text{good}}, \bar{t}))}{1 + L(\pi_F) \delta(|t_i| > t_{\text{good}})} \right]
\]

(44)

where all I did was factor \((1 - \pi_F)\) and the probabilities that condition on \(T_i\). In the above equation, the front term doesn't depend on \(\pi_F\). Thus, if we take the ratio of this expression evaluated at two different values of \(\pi_F\), the first term cancels out:

\[
\frac{\Pr(|t_i| \leq \bar{t} || t_i > t_{\text{good}}) |_{\pi_F = \pi'}}{\Pr(|t_i| \leq \bar{t} || t_i > t_{\text{good}}) |_{\pi_F = \pi}} = \frac{1 + L(\pi') \delta(|t_i| \in (t_{\text{good}}, \bar{t}))}{1 + L(\pi) \delta(|t_i| \in (t_{\text{good}}, \bar{t}))} \bigg[ \frac{1 + L(\pi) \delta(|t_i| > t_{\text{good}})}{1 + L(\pi') \delta(|t_i| > t_{\text{good}})} \bigg]
\]

(45)

\[
= 1 + \left[L(\pi') - L(\pi)\right] \left[\delta(|t_i| \in (t_{\text{good}}, \bar{t})) - \delta(|t_i| > t_{\text{good}})\right] + O(\varepsilon^2)
\]

(46)

where the last line uses a Taylor expansion around \([\delta(|t_i| \in (t_{\text{good}}, \bar{t})), \delta(|t_i| > t_{\text{good}})] = [0, 0]\) and the fact \(\delta\left([t_{\text{good}}, \bar{t}) \right) < \varepsilon\) and \(\delta\left(|t_i| > t_{\text{good}}\right) < \varepsilon\). The simplicity of this Taylor expansion comes from the fact that

\[
\frac{d}{dx} \left( \frac{1 + ax}{1 + bx} \right) \bigg|_{x=0} = \frac{a}{b} - \frac{b(a+1)}{(bx+1)^2} \bigg|_{x=0} = a - b.
\]

So the partial derivatives of Equation (45) simplify dramatically once \([\delta(|t_i| \in (t_{\text{good}}, \bar{t})), \delta(|t_i| > t_{\text{good}})] = [0, 0]\)
[0,0] is plugged in.

Subtracting 1 from both sides, taking absolute values of (46) and applying the triangle inequality

\[
\frac{\Pr(|t| \leq \tilde{t}|t| > t_{\text{good}})_{\pi_F=\pi'}}{\Pr(|t| \leq \tilde{t}|t| > t_{\text{good}})_{\pi_F=\pi}} - 1 \leq |L(\pi') - L(\pi)| \left| \delta \left(|t| \in (t_{\text{good}}, \tilde{t}) \right) - \delta \left(|t| > t_{\text{good}} \right) \right| + O(\varepsilon^2)
\]

\[
\leq |L(\pi') - L(\pi)| \varepsilon + O(\varepsilon^2)
\]

\[
\leq \frac{\tilde{\pi}}{1 - \tilde{\pi}} \varepsilon + O(\varepsilon^2).
\]

Recall $\tilde{\pi}$ is the maximum value that $\pi$ and $\pi'$ can take. The third line uses the fact that $L(\pi)$ is strictly increasing.

Finally, imposing the assumption that $\tilde{\pi} = 2/3$ finishes the proof. Note that using instead $\tilde{\pi} = 1/2$ results in a factor of 1 in front of $\varepsilon$, while $\tilde{\pi} = 9/10$ would result in a factor of 9. $\tilde{\pi} = 2/3$ was chosen as an illustrative middle ground. \qed

### B.3 Derivation of Shrinkage Expression (21)

Since $i$ is used as a subscript throughout, I drop it for ease of notation. I use the shorthand notation $p(\mu|t)$ to denote the density of $\mu$ given $|t|$, $p(\mu|T)$ to denote the density of $\mu$ given the factor is true, and similarly for other random variables.

Write down the posterior of $\mu|t$ and rewrite the denominator using the law of total probability

\[
p(\mu|t) = \frac{p(t|\mu) p(\mu|T) \pi_T + p(t|\mu) p(\mu|F) \pi_F}{p(t)}
= \frac{p(t|\mu) p(\mu|T) \pi_T + p(t|\mu) p(\mu|F) \pi_F}{p(t|T) \pi_T + p(t|F) \pi_F}
\]

Then factor out $p(t|T) \pi_T$

\[
p(\mu) = \left[ p(t|\mu) \frac{p(\mu|T)}{p(t|T)} + p(t|\mu) \frac{p(\mu|F) \pi_F}{p(t|T) \pi_T} \right]
\times \left\{ \frac{1}{1 + \frac{p(t|F) \pi_F}{p(t|T) \pi_T}} \right\}
\]

\[
= \left[ p(\mu|t, T) + p(t|\mu) \frac{p(\mu|F) \pi_F}{p(t|T) \pi_T} \right] \left\{ \frac{1}{1 + \Delta(t, \pi_F, \lambda)} \right\}
\]

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where

\[ \Delta(t, \pi_F, \lambda) \equiv \frac{p(t|F)\pi_F}{p(t|T)\pi_T}, \]

since \( p(t|\mu, T) = p(t|\mu) \).

Let \( h(\mu) \) be a function s.t. \( h(0) = 0 \). Then the posterior expected value of \( h(\mu) \) is

\[
E(h(\mu)|t, \pi_F, \lambda) = \left\{ E(h(\mu)|t, T, \lambda) + \int d\mu h(\mu) \frac{p(\mu|F)\pi_F}{p(t|T)\pi_T} \left\{ \frac{1}{1 + \Delta(t, \pi_F, \lambda)} \right\} \right\} \\
= \left\{ E(h(\mu)|t, T, \lambda) + h(0) \frac{\pi_F}{p(t|T)\pi_T} \left\{ \frac{1}{1 + \Delta(t, \pi_F, \lambda)} \right\} \right\} \\
= E(h(\mu)|t, \pi_F = 0, \lambda) \left\{ \frac{1}{1 + \Delta(t, \pi_F, \lambda)} \right\}
\]

(47)

where the second line comes from the fact that \( p(\mu|F) = \delta(\mu) \), where \( \delta(\cdot) \) is the Dirac delta, the third uses \( h(0) = 0 \), and the last line uses the fact that the model with \( \pi_F = 0 \) assumes that all factors are true. Choosing \( h(\mu) = \mu \) leads to Equation (21).

A similar argument can be made for the local FDR. The local FDR can be rewritten as

\[
\text{fdr}(t) = \frac{p(t|F)\pi_F}{p(t|T)\pi_T + p(t|F)\pi_F} \\
\leq \frac{p(t|F)}{p(t|T)\pi_T + p(t|F)\pi_F} \\
= \frac{p(t|F)}{p(t|T)} \left[ \frac{1}{1 + \frac{p(t|F)\pi_F}{p(t|T)}} \right].
\]

\( \frac{p(t|F)}{p(t|T)} \) is, in a way, a bound on the local fdr assuming that all factors are true. The square brackets is the correction term, which is just \( \Delta(t, \pi_F, \lambda) \times \pi_T \).
B.4 Proof of Proposition 3

Proof. The proposition uses the same assumptions as Proposition 2, so I can borrow this expression from its proof

$$\Pr(|t_i| \leq \tilde{t}|t_i| > t_{\text{good}}) = \frac{\Pr(|t_i| \in (t_{\text{good}}, \tilde{t})|T_i)}{\Pr(|t_i| > t_{\text{good}}|T_i)} \left[ \frac{1 + L(\pi_F) \delta(|t_i| \in (t_{\text{good}}, \tilde{t}))}{1 + L(\pi_F) \delta(|t_i| > t_{\text{good}})} \right]$$

where

$$\delta(X) \equiv \frac{\Pr(X|F_i)}{\Pr(X|T_i)}$$

$$L(\pi) \equiv \frac{\pi}{1-\pi}$$

and $X$ is some event. Rearranging,

$$\frac{\Pr(|t_i| \leq \tilde{t}|t_i| > t_{\text{good}})}{\Pr(|t_i| \in (t_{\text{good}}, \tilde{t})|T_i, t_i > t_{\text{good}})} = \frac{1 + L(\pi_F) \delta(|t_i| \in (t_{\text{good}}, \tilde{t}))}{1 + L(\pi_F) \delta(|t_i| > t_{\text{good}})} = 1 + L(\pi_F) \left[ \delta(|t_i| \in (t_{\text{good}}, \tilde{t})) - \delta(|t_i| > t_{\text{good}}) \right] + O(\varepsilon^2)$$

or

$$\frac{\Pr(|t_i| \leq \tilde{t}|t_i| > t_{\text{good}})}{\Pr(|t_i| \in (t_{\text{good}}, \tilde{t})|T_i, t_i > t_{\text{good}})} - 1 = L(\pi_F) \left[ \delta(|t_i| \in (t_{\text{good}}, \tilde{t})) - \delta(|t_i| > t_{\text{good}}) \right] + O(\varepsilon^2)$$

Taking absolute values and manipulating

$$\left| \frac{\Pr(|t_i| \leq \tilde{t}|t_i| > t_{\text{good}})}{\Pr(|t_i| \in (t_{\text{good}}, \tilde{t})|T_i, t_i > t_{\text{good}})} - 1 \right| \leq |L(\pi_F)| \left| \left[ \delta(|t_i| \in (t_{\text{good}}, \tilde{t})) - \delta(|t_i| > t_{\text{good}}) \right] \right| + O(\varepsilon^2) \leq |L(\pi_F)| \varepsilon + O(\varepsilon^2) \leq \frac{\bar{\pi}}{1-\bar{\pi}} \varepsilon + O(\varepsilon^2)$$

where $\bar{\pi}$ is the maximum value under consideration for $\pi_F$. The 2nd line comes from the fact that $\delta(|t_i| > t_{\text{good}}) > 0$ and $\delta(|t_i| \in (t_{\text{good}}, \tilde{t})) < \varepsilon$ and the third line uses the fact that $L(\pi_F)$ is strictly increasing. Plugging in $\bar{\pi} = 2/3$ completes the proof.
B.5 Proof of Corollary 1

Comparing the definition of $\text{FDR}(h)$ (Equation (26)) and Equation (28) we see that $h_{BY}$ is isomorphic to replacing $\pi_F/\Pr(|t_i| > h)$ with $(\sum_{i=1}^{N} \frac{1}{i}) / \hat{\Pr}(|t_i| > h)$ in the hurdle definition (Equation (7)). Thus, Proposition 1 implies

$$h_{BY} > 1.96 \text{ if and only if } \left( \sum_{i=1}^{N} \frac{1}{i} \right) > \hat{\Pr}(|t_i| > 1.96).$$

But since $\sum_{i=1}^{N} \frac{1}{i} > 1$ and $\hat{\Pr}(|t_i| > 1.96) \equiv \frac{\sum_{i=1}^{N} I(|t_i| > 1.96)}{N} < 1$, we have $h_{BY} > 1.96$.

C Estimation Details

C.1 QML on Simulated Data with Correlations

This section shows that QML works well even if factors have pairwise correlations as high as 0.9 assuming no publication bias.

The simulation model begin with the model in Section 3.2 and specifies correlations to be AR1 across factor indexes:

$$\epsilon_i = t_i - \mu_i$$
$$\epsilon_i \sim N\left(\rho \epsilon_{i-1}, \sqrt{1 - \rho^2}\right)$$

This specification ensures Equation (1) holds and provides a simple way to model correlated factors.

I assume all factors are observed. One can think of this exercise as demonstrating that the weak identification I find is due to publication bias rather than QML.

I fix $\lambda_\mu$ and $\lambda_\sigma$ using the point estimate from Table 1. I vary $\pi_F$ and $\rho$ as shown in Table A.1. For each of the shown parameter values, I simulate a sample of 200 factors, apply QML, and repeat 100 times. The table shows the mean $\hat{\pi}_F$ across the 100 simulations as well as the standard deviation across simulations.

The table shows that QML is approximately unbiased for $\pi_F$ and has moderate standard errors of around 0.10 for $\rho \leq 0.5$. Standard errors increase to about 0.15 for the $\rho = 0.90$ cases, but they are still half as large as the standard error of about 0.33 in the empirical estimates. Overall Table A.1 shows that weak identi-
fication of \( \pi_F \) is not due to QML.

**Table A.1: QML on Simulated Data with Correlations**

I simulate factors with correlated t-stats that have AR1 coefficient \( \rho \) and apply QML. Results show means and standard deviations across 100 simulations using each parameter set.

| \( \pi_F \) truth | Cross-predictor AR1 coefficient \( \rho \) |
|-----------------|------------------|
|                 | 0.1   | 0.5   | 0.9   |
| mean \( \hat{\pi}_F \) | 0.13  | 0.48  | 0.90  |
| sd \( \hat{\pi}_F \)   | 0.10  | 0.15  | 0.07  |

**C.2 Details on the Semi-Parametric Bootstrap**

The cluster-bootstrap uses post-1963 panel returns from the Chen-Zimmermann data. Unlike the main results which use only in-sample data, I include data outside of the original sample periods for the cluster bootstrap. This sample selection helps keep the panel balanced and is conservative because correlations generally increase post-publication (McLean and Pontiff (2016)). Using this sample, I construct residuals by subtracting the mean return at the predictor level.

I draw residuals with a cluster bootstrap. I draw 5,000 predictors (with replacement) and 350 months (with replacement) and construct a dataset of the corresponding residuals. 350 months corresponds to the average sample size in the original papers. By construction, the distribution of correlations in the bootstrapped samples is similar that of the original data, as seen in Figure A.3.

I then construct sampling noise \( \varepsilon_i \) from these residuals by taking the mean, dividing by the standard deviation, and multiplying by the square root of the number of observations. I feed these \( \varepsilon_i \) into the point estimate. That is, I simulate 5,000 \( \mu_i \) using the point estimate, calculate \( t_i = \mu_i + \varepsilon_i \), and then obtain published \( t_i \) by applying Equation (32), once again using the point estimate. Provided that the model is well-specified, this semi-parametric bootstrap provides
valid inference (Efron and Tibshirani (1994), Chapter 6.5).

This relatively complicated bootstrap is required for capturing correlations because a simple cluster bootstrap would lead to t-statistics that are too small compared to the empirical data, as it fails to simulate publication bias. Similarly, a cluster bootstrap that simply applies a truncation would lead to t-statistics that are too large compared to empirical data, as this bootstrap effectively assumes that no publication bias in the empirical data.

**Figure A.3: Correlations in Cluster-Bootstrapped Return Residuals.**

I bootstrap from Chen-Zimmermann panel returns while ensuring that returns in the same month are always drawn together. The plot compares the distribution of correlations in the bootstrap against the correlations in the raw data.
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Figure 2: Weak Identification of t-Hurdles in Harvey, Liu, and Zhu (2016). HLZ baseline is the baseline estimate in Harvey, Liu, and Zhu’s Table 5 (2nd row), which implies that t-hurdles should be raised to 2.27 to ensure FDR ≤ 5%. The alternative model uses the same model but changes the share of false factors ($\pi_F$) to zero, implying that even a t-hurdle of 0 ensures FDR ≤ 5%. HLZ assume only t-stats > 2.57 are well-observed and both models are re-scaled to have the same density in this region. Despite having very different implications for t-hurdles, the two models are essentially observationally equivalent, consistent with Proposition 2.
Figure 3: Model Fit and Intuition. I examine predictions of the point estimate (blue) and an estimate that assumes $\pi_F = 2/3$ (red). $\pi_F$ is the fraction of all predictors that are false. Panel (a) compares model predictions for all t-stats (lines) with published t-stats in the CZ data (gray bars). Panel (b) decomposes model predictions into true predictors (bars) and false predictors (the space between lines and bars). $\pi_F = 0.01$ and $\pi_F = 2/3$ both fit the data very well, consistent with Proposition 2. Consistent with Proposition 3, $\pi_F = 0.01$ and $\pi_F = 2/3$ imply similar distributions for true predictors, suggesting $\lambda$ is strongly identified.
Figure 4: Weak Identification of t-hurdles for Cross-Sectional Predictors. I repeatedly re-sample from the CZ dataset, re-estimate the model of multiple testing with publication bias, and re-calculate the t-hurdle (Equation (7)) corresponding to an FDR of 5% (Panel (a)) and 1% (Panel (b)). $\hat{\pi}_F$ is the estimated probability of drawing a false factor. In both panels, the distribution of bootstrapped multiple-testing-adjusted t-hurdles is highly dispersed, with significant mass on both sides of the classical hurdles. Identification is so weak that the data say little about whether t-stat hurdles should be raised, stay the same, or even be lowered.
Figure 5: Strong Identification of the Shrinkage and the local FDR. I repeatedly re-sample from the CZ dataset, re-estimate the model of multiple testing with publication bias, and re-calculate shrinkage (Equation (19), Panel (a)) and the local FDR (Equation (20), Panel (b)). Shrinkage is the extent to which t-stats are biased upward due to multiple testing. The local FDR is the probability a given predictor has an expected return of zero. Within each bootstrap, shrinkage and local FDR are averaged across published predictors. The McLean and Pontiff (2016) bound is taken from their abstract. The HLZ Baseline applies Equation (20) to simulated data based on their Table 5. Both multiple testing statistics imply that published predictability is largely true with high confidence and are consistent with external point estimates.

(a) Average Shrinkage for Published t-Stats

(b) Local FDR for Published Predictors
Table 1: Estimates of a Structural Model of the Cross-Sectional Literature

I estimate a structural model of multiple testing with publication bias using quasi-maximum-likelihood (Section 3.2) on long-short portfolios from Chen and Zimmermann (2022) (Section 3.1). The bootstrapped distribution is found by either re-sampling the empirical t-stats and re-estimating (no parentheses) or using a semi-parametric cluster-bootstrap that accounts for correlations (parentheses). $E(\mu_i|T_i)$ and $SD(\mu_i|T_i)$ use textbook lognormal formulas to convert $\lambda_\mu$ and $\lambda_\sigma$ to moments. The share of false factors $\pi_F$ is weakly identified, with huge confidence bounds, consistent with Proposition 2. In contrast, $E(\mu_i|T_i)$ and $SD(\mu_i|T_i)$ are strongly identified. Accounting for correlations has very little effect on the results.

|                | Estimate | 5     | 25    | 50    | 75    | 95    |
|----------------|----------|-------|-------|-------|-------|-------|
| Probability false $\pi_F$ | 0.01     | 0.01  | 0.22  | 0.67  | 0.89  |
|                |          | (0.01)| (0.01)| (0.31)| (0.65)| (0.85)|
| True factors' unbiased t-stats ($\mu_i|T_i$) |           |       |       |       |       |       |
| $E(\mu_i|T_i)$   | 2.66     | 2.00  | 2.47  | 2.79  | 3.21  | 3.82  |
|                |          | (2.00)| (2.46)| (2.80)| (3.13)| (3.56)|
| $SD(\mu_i|T_i)$ | 2.29     | 1.88  | 2.09  | 2.25  | 2.43  | 2.70  |
|                |          | (1.89)| (2.13)| (2.26)| (2.44)| (2.67)|
| Pr(pub$_i$) if marginal ($\eta$) | 0.67     | 0.33  | 0.46  | 0.63  | 0.67  | 0.67  |
|                |          | (0.33)| (0.46)| (0.60)| (0.67)| (0.67)|
Table 2: Robustness

Each row shows the result of bootstrapping t-stat hurdles (Equation (7)), mean shrinkage for published t-stats (Equation (19)), and mean local FDR for published t-stats (Equation (20)) using a different set of modeling assumptions and/or data-inclusion requirements. Baseline uses the assumptions found throughout Section 3. “P05” and “P95” show 5th and 95th percentiles across 500 bootstrapped estimations for the robustness tests and 1000 bootstrapped estimations for the baseline. Across 10 alternative modeling assumptions, the t-hurdle is weakly identified while the shrinkage and local FDR are strongly identified.

|                  | t-hurdle |                                | Shrinkage |                                | FDR     |
|------------------|----------|---------------------------------|-----------|---------------------------------|---------|
|                  | FDR = 5% | P05  | P95  | P05  | P95  | P05  | P95  |
| (1) Baseline     |          | 0.0  | 3.0  | 8.9  | 27.9 | 0.1  | 22.6 |
| Including \(|t_i| < 1.96) |          | (2) 3-step Pr(pub_i)            | 0.0  | 1.6  | 2.6  | 17.7 | 0.1  | 4.6  |
| Extruding the \(\pi_F\) very small | (3) Restricted 3-step Pr(pub_i) |        | 0.0  | 2.4  | 7.8  | 22.6 | 0.1  | 15.7 |
| Alternative publication probability function | (4) \(\pi_F \geq 0.1\) | 0.9  | 3.0  | 9.0  | 27.8 | 0.6  | 23.3 |
|                  |          | (5) \(\pi_F \geq 0.2\)         | 1.5  | 3.0  | 9.2  | 26.6 | 1.4  | 22.0 |
| Alternative distributional assumptions | (6) \(\eta = 0.5\) | 0.0  | 2.9  | 10.9 | 26.1 | 0.1  | 20.7 |
|                  |          | (7) \(\eta \in [1/3, 1]\) | 0.0  | 3.0  | 6.9  | 26.8 | 0.1  | 22.6 |
|                  |          | (8) logistic Pr(pub_i)           | 0.0  | 2.7  | 6.6  | 21.2 | 0.1  | 17.1 |
| (9) \(\mu_i | T_i \sim \text{Exponential}\) |          | 0.0  | 2.9  | 11.3 | 28.7 | 0.1  | 19.7 |
| (10) \(\mu_i | T_i \sim \text{Student’s t}\) |          | 0.0  | 2.9  | 8.1  | 22.9 | 0.1  | 20.9 |
| (11) \(\mu_i | T_i \sim \text{Mixture-Normal}\) |          | 0.0  | 3.0  | 7.2  | 24.1 | 0.1  | 24.8 |