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Abstract

Despite their popularity as proxies of expected returns, the implied cost of capital's (ICC) measurement error properties are relatively unknown. Through an in-depth analysis of a popular implementation of ICCs by Gebhardt, Lee, and Swaminathan (2001) (GLS), I show that ICC measurement errors can be not only nonrandom and persistent, but can also be associated with firms' risk or growth characteristics, implying that ICC regressions are likely confounded by spurious correlations. Moreover, I document that biases in GLS' measurement errors are driven not only by analysts' systematic forecast errors but also by functional form assumptions, so that correcting for the former – a primary focus of the ICC literature – is insufficient by itself. From these findings, I argue that the choice between ICCs and realized returns involves a tradeoff between bias and efficiency, and suggest that realized returns should be used in conjunction with ICCs to make more robust inferences about expected returns.

Keywords: Expected returns, implied cost of capital, measurement errors.
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1 Introduction

The implied cost of equity capital (ICC), defined as the internal rate of return that equates the current stock price to discounted expected future dividends, is an increasingly popular class of proxies for the expected rate of equity returns in accounting and finance.[1] ICCs have intuitive appeal in that they are anchored on the discounted-cash-flow valuation model. Moreover, ICCs have two distinct advantages over alternatives such as ex post realized returns. First, ICCs are forward-looking and utilize forecasts of a firm's future fundamentals (e.g., consensus analyst forecasts of future earnings). Second, ex post realized returns are noisy estimates of expected returns, as evidenced by Campbell (1991) and Vuolteenaho (2002). These advantages promulgated a growing body of literature that uses ICCs to study the cross-sectional variations in expected returns, where inferences are made from regressions of ICCs on firm characteristics or regulatory events of interest.[2]

While ICCs are likely more precise than alternatives like realized returns, the properties of their measurement errors—the differences between the firm's ICC and (unobserved) true expected returns—are not fully understood, and these properties can have significant implications. If ICC measurement errors are systematically correlated with firm characteristics, researchers' inferences may be confounded by spurious correlations with measurement errors. If so, researchers face a bias-efficiency tradeoff when choosing between ICCs and realized returns. On the other hand, if ICC measurement errors are uncorrelated with the regressors, e.g., if they are "classical" or random noise, then in

[1]That is, ICCs are the $\widehat{er}_{i,t}$ that solves

$$P_{i,t} = \sum_{n=1}^{\infty} \frac{\mathbb{E}_t\left[D_{i,t+n}\right]}{\left(1+\widehat{er}_{i,t}\right)^n},$$

where $P_{i,t}$ is firm i's price at time t, and $\mathbb{E}_t\left[D_{i,t+n}\right]$ is the time-t expectation of the firm's dividends in period $t+n$.

[2]For example, Botosan (1997) studies the impact of corporate disclosure requirements; Chen, Chen, and Wei (2009) and Chen, Chen, Lobo, and Wang (2011) examine the impact of different dimensions of corporate governance; Daske (2006) examines the effect of adopting IFRS or US GAAP; Dhaliwal, Krull, Li, and Moser (2005) examines the effects of dividend taxes; Francis, LaFond, Olsson, and Schipper (2004) study the effects of earnings attributes; Francis, Khurana, and Pereira (2005) study the effects of firms' incentives for voluntary disclosure; Hail and Leuz (2006) examine the effect of legal institutions and regulatory regimes; and Hribar and Jenkins (2004) examine the effect of accounting restatements.

large sample the estimated regression coefficients converge to the true associations between firm characteristics and expected returns. If so, ICCs should be unambiguously preferred over realized returns.

Why might one expect ICCs to have nonrandom measurement errors? Measurement errors in ICCs can arise from two potential sources, each of which has the potential to be nonrandom and systematically associated with firm characteristics. The first source of ICC measurement errors is forecast errors of future fundamentals (e.g., cash flows or earnings). To the extent that such forecasts are systematically biased toward certain types of firms, the resulting ICCs can be expected to contain measurement errors that are correlated with the characteristics of such firms. For example, La Porta (1996); Dechow and Sloan (1997); Frankel and Lee (1998); and Guay, Kothari, and Shu (2011) show that consensus analyst EPS (as well as long-term growth) forecasts tend to be more optimistic for growth firms. Thus, all else equal, ICCs constructed using these analyst forecasts could produce measurement errors that are systematically more positive for growth firms than for value firms. Moreover, this source of measurement errors could be persistent if analysts' optimism is persistent, perhaps due to heuristic biases (e.g., Lys and Sohn, 1990; Elliot, Philbrick, and Wiedman, 1995).

A second source of ICC measurement errors is model misspecification, which results from erroneous assumptions embodied in the functional form that maps information and prices to expected returns. Model misspecification, by its nature, produces persistent errors; moreover, if the extent of misspecification varies with firm type, ICC measurement errors can be expected to be correlated with firm characteristics even if forecasts of future earnings are unbiased. For example, ICCs implicitly assume constant expected returns, despite the growing body of literature on time-varying expected returns (e.g., Cochrane, 2011; Ang and Liu, 2004; Fama and French, 2002; Jagannathan, McGrattan, and Scherbina, 2001). ICC is a measure of yield and can be viewed as a "weighted average" of expected future returns which can overstate (understate) the true expected returns over the next period if the term structure is upward-sloping (downward-sloping). If the term structure of expected returns varies with certain firm characteristics, they could generate nonrandom ICC measurement errors.[3] Consistent with this, the theoretical work of Hughes, Liu, and Liu (2009) show that when expected returns are stochastic but ICCs implicitly assume constant expected returns, ICCs differ from expected returns and ICC measurement errors can be correlated with firms' risk and growth profiles, even if forecasts of future cash flows are perfectly rational. As a consequence, despite a concerted effort to understand and mitigate the impact of systematic forecast biases on ICC measurement errors (e.g., Easton and Sommers, 2007; Hou, Van Dijk, and Zhang, 2012; Guay et al., 2011; Mohanram and Gode, 2012), it is still possible for ICCs to produce measurement errors—resulting from model misspecification—that are systematically correlated with firm characteristics and can be persistent.

Though the above provides some intuition behind nonrandomness in ICC measurement errors, the relations between firm characteristics and ICC measurement errors are ex ante ambiguous. In particular, it is unclear how a given firm characteristic would interact with the two potential sources of measurement errors. Therefore, these properties of ICC measurement errors are ultimately open empirical questions that have significant implications for empirical research using ICCs. In a discussion paper on the implied cost of capital, Lambert (2009) commented that "[there are likely] biases and spurious correlations in estimates of implied cost of capital." Echoing such sentiments, Easton (2009) concluded in his survey of ICC methodologies that "as long as measurement error remains the *Achilles' Heel* in estimating the expected rate of returns, it should be one of the focuses of future research on these estimates." (p.78)

This paper provides a first study on the persistence and cross-sectional properties of ICC measurement errors. In particular, I seek to examine whether ICC measurement errors are random in nature, or whether there is evidence of systematic associations with firm characteristics. A finding that measurement errors are random would support their use as dependent variables in regression settings. On the other hand, documenting nonrandom measurement errors that are associated with firm characteristics raises concerns about spurious correlations, because regressions of ICCs on firm characteristics could

[3] Lyle and Wang (2014) show that the slope of the term structure is higher for growth firms (those with low book-to-market multiple).

reflect associations with measurement errors rather than expected returns.

To address these questions, I develop a methodology for estimating the persistence of ICC measurement errors and their cross-sectional associations with firm characteristics. The methodology anchors on two main sets of assumptions. First, I assume that unexpected returns, the difference between future realized returns and expected returns, are uncorrelated with ex ante publicly available information. On an intuitive level, this assumption says "news is news"—by definition news cannot be systematically predictable.[4] Second, I model expected returns and ICC measurement errors as AR(1) processes to allow for the possibility of time-varying and persistent expected returns and ICC measurement errors, respectively. A finding of an AR(1) persistence parameter value of 0 for ICC measurement errors would suggest that they are random measurement errors.

Based on these two sets of assumptions, I derive methodologies for estimating the persistence parameters of expected returns and measurement errors based on the autocovariances of ICCs and the covariances between realized returns and ICCs. Moreover, utilizing the AR(1) structures in expected returns and ICC measurement errors, I show that a transformation of ICCs—a linear combination of ICC values and the persistence parameters—produces a "well-behaved" proxy for ICC measurement errors in the form of the sum of a firm-specific mean, the ICC measurement error, and random noise. Because this proxy takes a form akin to the classic errors-in-dependent-variable set up, I show that valid inferences on the associations between ICC measurement errors and firm characteristics can be made using fixed effects regressions.

I apply these methodologies to a popular implementation of ICCs, colloquially known as "GLS" in recognition of its creators (Gebhardt, Lee, and Swaminathan, 2001), and document three main findings that contribute to the ICC literature. First, I present the first direct evidence that ICC measurement errors can be nonrandom and quite persistent: GLS measurement errors have an average (median) persistence parameter of 0.46 (0.48). Second, I show that ICC measurement errors can be systematically associated

[4]Note that this condition is implied by the stronger assumption that realized returns are bias free, an assumption that has been questioned by the ICC literature (e.g., Easton and Monahan, 2005; Botosan, Plumlee, and Wen, 2011). However, the paper's methodology applies so long as biases in expected returns are constant and unexpected returns are not predictable.

with firm characteristics: GLS measurement errors are cross-sectionally associated with firm risk and growth characteristics, such as market capitalization, book-to-market ratio, 3-month momentum, analyst coverage, and analyst long-term growth forecasts. Third, I find that the associations between GLS measurement errors and firm characteristics persist even after controlling for analyst forecast biases, consistent with GLS measurement errors driven by both functional form misspecification and by analyst forecast biases—the primary source of measurement errors focused on by the empirical ICC literature (e.g., Easton and Sommers, 2007; Guay et al., 2011; Hou et al., 2012). In particular, I document that GLS measurement errors are positively associated with the slope of the term structure in expected returns.

To provide comfort in the paper's methodologies and inferences about GLS measurement errors, I conduct a further construct validity test. The logic of this test rests on the observation that if the paper's methods yield valid inferences about GLS measurement errors, they also produce valid inferences about expected returns.[5] In other words, this methodology should produce estimates that better capture the systematic associations between expected returns and firm characteristics compared to regressions that use GLS. To test these implications, I compare the performance of expected-return proxies constructed using historically-estimated regression coefficients estimated based on the paper's methodology and based on GLS. I find that regression coefficients estimated using the paper's methodology generate expected-return proxies that exhibit substantially better ability in sorting average future returns, providing confidence in the paper's methodology for making inferences about GLS measurement errors.

Based on the evidence documented in this paper, I draw several conclusions that are important for the ICC literature. First, empirical results involving cross-sectional regressions of ICCs on firm characteristics are likely confounded by spurious correlations between ICC measurement errors and firm characteristics. Second, methodologies for mitigating ICC measurement errors such as portfolio grouping and instrumental vari-

[5]Specifically, subtracting the model-derived proxy for ICC measurement errors from ICC produces a "well-behaved" proxy for expected returns, again one that takes a form similar to the classic errors-in-dependent-variables set up that allows for valid inferences on the associations between expected returns and firm characteristics using fixed effects regressions.

ables are limited in effectiveness since common grouping variables or instruments (e.g., market capitalization and book-to-market ratio) are likely correlated with the measurement errors, as is the case of GLS. Third, correcting for systematic analyst forecast errors alone is unlikely to be adequate in fully addressing ICC measurement errors, since the latter are also driven by errors arising from model misspecification (e.g., the implicit assumption of constant expected returns).

Nevertheless, researchers need not abandon ICCs entirely. After all, making inferences about unobserved expected returns is a difficult task and no proxy of expected returns is perfect or strictly dominates all alternatives. I argue that a researcher's choice between ICCs and realized returns involves a tradeoff between bias and efficiency. ICCs have the distinct advantage of having less noisy or more precise measurement errors, but their nonrandomness can bias regression estimates and confound inferences. On the other hand, realized returns have the distinct advantage of having unpredictable errors (i.e., "news"), which allow for consistent estimation of regression coefficients, but the noisiness in these errors make the regression estimates imprecise. The use of realized returns as a proxy of expected returns, therefore, yields low-powered and conservative tests. Based on this observation, in the last section of the paper I suggest a conservative approach for how realized returns and ICCs can be used together to provide more robust inferences about expected returns.

The remainder of the paper is organized as follows. Section 2 of the paper describes the theoretical model and lays out the estimation procedures. Section 3 presents the empirical results. Section 4 discusses the implications of the paper's findings, and offers some practical recommendations for researchers.

2 Theoretical Model and Empirical Methodology

In this section I motivate the potential concerns with inferences about expected returns that arise from regressions using ICCs. In particular, when ICC measurement errors are nonrandom, regressions of ICCs on firm characteristics could reflect spurious correlations with measurement errors. I then develop empirical methodologies for studying the properties of ICC measurement errors that provides answers to two primary research questions of interest. Are ICC measurement errors nonrandom? If so, are they associated with firm characteristics?

2.1 Motivation

Researchers are often interested in understanding the association between a particular firm characteristic (e.g., earnings quality, corporate governance) and a firm's (unobserved) expected rate of returns,

$$er_{i,t} \equiv \mathbb{E}_t\left(r_{i,t+1}\right). \tag{1}$$

To examine these questions empirically, a regression framework is typically employed using a proxy of expected returns ($\widehat{er}_{i,t}$) as a dependent variable, where

$$\widehat{er}_{i,t} = er_{i,t} + w_{i,t} \tag{2}$$

and $w_{i,t}$ is the proxy's measurement error.

The standard approach assumes that expected returns are linear in firm characteristics (3) with standard OLS assumptions on residuals. Assume also that measurement errors are linear in certain firm characteristics with standard assumptions on residuals (4). A univariate case is presented here for simplicity and without loss of generality.

$$\begin{aligned} er_{i,t} &= \delta_0 + \delta \cdot z_{i,t} + \varepsilon^{er}_{i,t} && (3) \\ w_{i,t} &= \beta_0 + \beta \cdot x_{i,t} + \varepsilon^{w}_{i,t} && (4) \\ &\quad \text{where } \left(\varepsilon^{w}_{i,t}, \varepsilon^{er}_{i,t}\right) \sim_{iid} (0,0)\,; \\ &\quad \mathbb{E}\left(\varepsilon^{w}_{i,t} | z_{i,t}, x_{i,t}\right) = \mathbb{E}\left(\varepsilon^{er}_{i,t} | z_{i,t}, x_{i,t}\right) = 0 \end{aligned}$$

The last condition, that the residuals are mean independent of the firm characteristics, implies that the residuals and firm characteristics are uncorrelated, which allow for the consistent estimation of the slope coefficients.

Because the dependent variable is measured with error, the estimated slope coefficient of interest could be biased to the extent that measurement errors are associated with the regressor. On the other hand, to the extent that measurement errors are "classical" or random noise, the estimated slope coefficient continues to be consistent.

I present a simple illustrative example for intuition. Without loss of generality, suppose that $z_{i,t} = x_{i,t} = Size_{i,t}$, where $Size_{i,t}$ is firm i's log of market capitalization at the beginning of period t.[6] Then, equations (2), (3), and (4) imply the following relation between the expected-return proxy and *Size*.

$$\widehat{er}_{i,t} = (\delta_0 + \beta_0) + (\delta + \beta)\, Size_{i,t} + \left(\varepsilon^{er}_{i,t} + \varepsilon^{w}_{i,t}\right)$$

If measurement errors are random (i.e., $\beta = 0$), then a regression of the expected-return proxy on $Size$ produces valid estimates of δ. If measurement errors are nonrandom and associated with *Size* (i.e., $\beta \neq 0$), then such a regression produces a biased estimate of δ. This bias results from the (spurious) correlation between *Size* and the measurement error (β) and confounds the researcher's inferences on expected returns.

2.2 Empirical Methodology

The preceding example provides intuition behind why nonrandom ICC measurement errors can critically affect inferences about unobserved expected returns. But studying the nonrandomness of ICC measurement errors empirically is not easy, given that firms' true expected returns are unobserved. To help address these questions, I develop methodologies below for estimating the persistence in ICC measurement errors and their cross-sectional associations with firm characteristics under the linearity assumptions of (3) and (4).

[6] This simplifying assumption, made here for illustration only, is unnecessary for the remainder of the paper.

2.2.1 Assumption 1: AR(1) Structures

To make the analysis of measurement errors tractable, I begin by modeling the time-series behavior of expected returns and measurement errors to as AR(1) processes, with persistence parameters of ϕ_i and ψ_i and with innovations $u_{i,t+1}$ and $v_{i,t+1}$, respectively:

$$er_{i,t+1} = \mu_{ui} + \phi_i \cdot er_{i,t} + u_{i,t+1}; \tag{5}$$

$$w_{i,t+1} = \mu_{vi} + \psi_i \cdot w_{i,t} + v_{i,t+1}; \tag{6}$$

$$\text{where } (u_{i,t}, v_{i,t})' \sim {}_{iid} \left((0,0)', \ \Sigma_{uv} \right), \Sigma_{uv} \text{ invertible}; \tag{7}$$

$$|\phi_i|, |\psi_i| < 1; \text{ and} \tag{8}$$

$$\phi_i \neq \psi_i. \tag{9}$$

In this setup, both AR(1) parameters are assumed to be constant across time; moreover, while the persistence parameter of expected returns (ϕ_i) is firm-specific, the persistence of expected-returns-proxy measurement errors (ψ_i) is implicitly firm- and model-specific (i.e., dependent on the model that generates the ICC). Finally, I make the regularity assumption that the two processes are stationary (8), and the identifying assumption that, for each firm, the AR(1) parameters are not equal to each other (9).

The AR(1) assumption on expected returns (5) captures the possibility that expected returns are persistent and time-varying.[7] This modeling choice is common in the asset pricing literature (e.g., Conrad and Kaul, 1988; Poterba and Summers, 1988; Campbell, 1991; Pástor, Sinha, and Swaminathan, 2008; Binsbergen and Koijen, 2010; Pástor and Stambaugh, 2012; Lyle and Wang, 2014), and is consistent with the growing literature on time-varying (e.g., Cochrane, 2011; Ang and Liu, 2004; Fama and French, 2002; Jagannathan et al., 2001) and persistent (e.g., Fama and French, 1988; Campbell and Cochrane, 1999; Pástor and Stambaugh, 2009) expected returns.

The AR(1) assumption about measurement errors captures the possibility that mea-

[7]As noted in Campbell (1990) and Campbell (1991), the AR(1) assumption on expected returns need not restrict the size of the market's information set, and in particular does not assume that the market's information set contains only past realized returns. The AR(1) assumption merely restricts the way in which consecutive periods' forecasts relate to each other, and it is quite possible that each period's forecast is made using a large set of variables.

surement errors could also be persistent and time-varying. Unlike the assumption on expected returns, this assumption is new—to my knowledge there are no existing estimates of the persistence of ICC measurement errors. Note that this modeling choice does not impose persistence by assumption, as a persistence parameter of 0 is possible.

There is, however, rationale for why ICC measurement errors could be nonrandom and persistent. One source of measurement errors in ICC stems from analyst forecast errors, which can be potentially persistent. For example, analysts may be slow to incorporate new information (e.g., Lys and Sohn, 1990; Elliot et al., 1995) and update their forecasts sluggishly due to heuristic biases in how new information is weighed relative to old information. Elliot et al. (1995) argue that analysts are conservative in incorporating new information into their forecasts, consistent with the belief adjustment model of Hogarth and Einhorn (1992) in which analysts (overly) anchor to old information about the firm and (under) adjust their priors based on new information. This type of behavioral bias could lead to persistence in forecast errors, and, consequently, persistence in ICC measurement errors. The second source of ICC measurement errors, model misspecification, could also give rise to persistent measurement errors, to the extent that the misspecification is persistent. For example, the assumption of constant expected returns that is implicit (and persistent) in ICC models could give rise to persistent ICC measurement errors.

Ultimately the persistence in ICC measurement errors is an open empirical question. A finding that ICC measurement errors have a persistence parameter of 0 would be significant, as such a result would imply that measurement errors are random, mitigating concerns about spurious inferences in regression settings. In this case, ICCs should be unambiguously preferred over realized returns as a proxy of expected returns.

2.2.2 Assumption 2: News is News

In order to estimate the AR(1) parameters of the model, I make a second assumption that unexpected returns, or $news_{i,t+1}$ defined from the identity

$$r_{i,t+1} = er_{i,t} + news_{i,t+1}, \tag{10}$$

is uncorrelated with ex ante publicly available information. This assumption is a statement that "news is news," or that news cannot be, by definition, systematically predictable. If "news" is anticipated, it is part of expectations and not news.

More formally, this assumption is implied by the stronger assumption that realized returns are bias free. Though this stronger assumption follows from the definition of conditional expectations,[8] it is an assumption that has been questioned by the ICC literature (e.g., Easton and Monahan, 2005; Botosan et al., 2011). Thus this paper makes the weaker assumption that unexpected returns are uncorrelated with ex ante publicly available information, which allows for biased expected returns so long as the biases are constant.

Note that the notation and set up used here is similar to that of Lee, So, and Wang (2014). Like this paper, Lee et al. (2014) studies properties of measurement errors of ex ante proxies of expected returns by making the "news is news" assumption. There are some important differences nonetheless. Lee et al. (2014) is primarily interested in the variance in measurement errors, and derives a methodology for ranking ex ante measures

[8] Realized returns ($r_{i,t+1}$) is the sum of expected returns and news,

$$r_{i,t+1} = \mathbb{E}[r_{i,t+1}|\chi_t] + \delta_{i,t},$$

where χ_t is the publicly available information at time t. Taking conditional expectations on both sides and substituting $\mathbb{E}[r_{i,t+1}|\chi_t] = \mathbb{E}[\mathbb{E}(r_{i,t+1}|\chi_t)|\chi_t]$ yields

$$\mathbb{E}[\delta_{i,t}|\chi_t] = 0,$$

or that realized returns are unbiased.

Note that this paper's "news is news" assumption is implied by this definition. The conditional mean independence condition above implies that news is uncorrelated with publicly available information, i.e.,

$$\mathbb{E}[\delta_{i,t}x_t|\chi_t] = x_t\mathbb{E}[\delta_{i,t}|\chi_t] = 0$$

for any $x_t \in \chi_t$. Thus, this definition of expected returns implies that unexpected returns cannot be systematically predictable based on ex ante information.

of expected returns on the basis of their measurement error variances. This paper focuses on the nonrandomness of these measurement errors, in particular their associations with firm characteristics. To make this paper's methodology tractable requires additional assumptions, not required in Lee et al. (2014), on the stochastic process [i.e., AR(1)] describing measurement errors and expected returns.

2.3 Estimating AR(1) Parameters

Under the AR(1) structures and the "news is news" assumption, I show in Appendix A that the persistence parameters of expected returns and ICC measurement errors can be estimated. To summarize, I derive a) the autocovariance function for the expected-return proxy and b) the covariance function between realized returns and expected returns. I then show that the persistence parameters can be identified by relating these covariance functions.

2.4 Estimating Measurement Error Associations with Firm Characteristics

The above modeling set up also allows for the estimation of the associations between ICC measurement errors and firm characteristics. In particular, the AR(1) structures above yield a proxy for ICC measurement errors with desirable properties. Substitution of (5) and (6) into (2) and some simple algebraic manipulations produce $\widehat{w}_{i,t}$:[9]

$$\underbrace{\frac{\widehat{er}_{i,t+1}-\phi_i\widehat{er}_{i,t}}{\psi_i-\phi_i}}_{\widehat{w}_{i,t}(\psi_i,\phi_i)} = \underbrace{\left(\frac{\mu_{ui}+\mu_{v_i}}{\psi_i-\phi_i}\right)}_{\alpha_i}+w_{i,t}+\frac{u_{i,t+1}+v_{i,t+1}}{\psi_i-\phi_i} \tag{11}$$

$$= \beta_0+\beta\cdot x_{i,t}+\alpha_i+\left(\varepsilon^{\omega}_{i,t}+\frac{u_{i,t+1}+v_{i,t+1}}{\psi_i-\phi_i}\right) \tag{12}$$

by the linearity assumption of (4).

This proxy for ICC measurement errors, by equation (11), is a sum of three components: (1) a firm-specific constant (α_i); (2) the unobserved measurement error ($w_{i,t}$); and (3) iid mean 0 innovations. This proxy takes a form akin to the classical errors-in-variables structure, i.e., the proxy is the sum of the target variable of interest and iid mean 0 noise. The difference here is that the measurement error ($\widehat{w}_{i,t}-w_{i,t}$), while iid, has a non-zero mean. In particular, under the linearity assumption relating ICC measurement errors to firm characteristics (4), the measurement-error proxy can be written in the form of a standard fixed effects model (12). Thus, under this model one can estimate β, the associations between measurement errors and firm characteristics ($x_{i,t}$), through a fixed-effects regression of $\widehat{w}_{i,t}(\psi_i,\phi_i)$ on $x_{i,t}$.[10]

This setup also allows for inferences about the association between expected rate of returns and firm characteristics—the researcher's ultimate goal. In particular, a well-behaved measurement error structure can by obtained from a simple modification of the

[9] To show the algebraic steps:

$$\begin{aligned}
\widehat{er}_{i,t+1} &= er_{i,t+1}+w_{i,t+1} \text{ by definition of expected-return proxy}\\
&= (\mu_{ui}+\phi_i er_{i,t}+u_{i,t+1})+(\mu_{v_i}+\psi_i w_{i,t}+v_{i,t+1}) \text{ by AR(1) assumptions}\\
&= (\mu_{ui}+\mu_{v_i})+\phi_i er_{i,t}+\psi_i w_{i,t}+(u_{i,t+1}+v_{i,t+1})\\
&= (\mu_{ui}+\mu_{v_i})+\phi_i\widehat{er}_{i,t}+(\psi_i-\phi_i)\,w_{i,t}+(u_{i,t+1}+v_{i,t+1})\\
\text{Thus } \widehat{er}_{i,t+1}-\phi_i\widehat{er}_{i,t} &= (\mu_{ui}+\mu_{v_i})+(\psi_i-\phi_i)\,w_{i,t}+(u_{i,t+1}+v_{i,t+1})
\end{aligned}$$

To arrive at the expression for $\widehat{w}_{i,t}(\psi_i,\phi_i)$ requires the identifying assumption of (9): $\phi_i\neq\psi_i$.

[10] Alternatively, if the fixed effects can be assumed to be uncorrelated with firm characteristics, then β can be estimated by a standard OLS regression of $\widehat{w}_{i,t}$ on $x_{i,t}$.

ICC: subtract $\widehat{w}_{i,t}(\psi_i, \phi_i)$ from the expected-return proxy.

$$\begin{aligned}
\widehat{er}_{i,t} - \widehat{w}_{i,t}(\psi_i, \phi_i) &= er_{i,t} + w_{i,t} - \alpha_i - w_{i,t} - \frac{u_{i,t+1} + v_{i,t+1}}{\psi_i - \phi_i} \text{ by eqns (2), (11)} \\
&= -\alpha_i + er_{i,t} + \frac{u_{i,t+1} + v_{i,t+1}}{\phi_i - \psi_i} \qquad (13) \\
&= \delta_0 + \delta \cdot z_{i,t} - \alpha_i + \left(\varepsilon^{er}_{i,t} + \frac{u_{i,t+1} + v_{i,t+1}}{\phi_i - \psi_i} \right) \qquad (14) \\
& \quad \text{by linearity assumption of eqn (3)}
\end{aligned}$$

Similar to before, equation (13) shows that the modified expected-return proxy ($\widehat{er}_{i,t} - \widehat{w}_{i,t}$) is the sum of three components: (1) a firm specific constant ($-\alpha_i$); (2) the unobserved expected returns ($er_{i,t}$); and (3) iid mean 0 AR(1) innovations. Compared to the definition of an expected-return proxy (2), the key feature in this modification is the absence of the measurement-error term ($\omega_{i,t}$) in equation (13).

As before, this proxy takes a form akin to the classical errors-in-variables structure. Viewed differently, this modification of ICCs replaces the original (potentially "bad") measurement errors with well-behaved ones. Under the linearity assumption relating expected returns to firm characteristics (3), the modified expected-return proxy can be expressed (14) in the form of a standard fixed-effects model. The slope coefficients (δ) of interest, therefore, can be estimated by fixed-effects regressions of $\widehat{er}_{i,t} - \widehat{w}_{i,t}$ on $z_{i,t}$. Alternatively, this methodology can be viewed as a way to "control" for the measurement error in a regression setting. In particular, it is equivalent to regressing $\widehat{er}_{i,t}$ on firm characteristics and controlling for $\widehat{w}_{i,t}$, but constraining the slope coefficient to be 1.

The above procedures for estimating the associations of firm characteristics with ICC measurement errors and with expected returns require the AR(1) parameters, which need to be estimated. As summarized in Section 2.2.2 and detailed in Appendix A, this paper develops an estimation procedure for these AR(1) parameters under the setup of the model.

3 Empirical Results

In this section I apply the above methodologies to the popular GLS (Gebhardt et al., 2001) model in order to assess empirically whether ICCs measurement errors could be nonrandom and whether they could be associated with firm characteristics. I also provide evidence for the validity of this methodological approach in explaining GLS measurement errors.

3.1 The Expected-Return Proxy: GLS

GLS is a practical implementation of the residual income valuation model[11] with a specific forecast methodology, forecast period, and terminal value assumption. Appendix B details the derivation of GLS from the residual income model. To summarize, the time-t GLS expected-return proxy for firm i is the $\widehat{er}_{i,t}^{gls}$ that solves

$$P_{i,t} = B_{i,t} + \sum_{n=1}^{11} \frac{\frac{\mathbb{E}_t[NI_{i,t+n}]}{\mathbb{E}_t[B_{i,t+n-1}]} - \widehat{er}_{i,t}^{gls}}{\left(1+\widehat{er}_{i,t}^{gls}\right)^n} \mathbb{E}_t\left[B_{i,t+n-1}\right] + \frac{\frac{\mathbb{E}_t[NI_{i,t+12}]}{\mathbb{E}_t[B_{i,t+11}]} - \widehat{er}_{i,t}^{gls}}{\widehat{er}_{i,t}^{gls}\left(1+\widehat{er}_{i,t}^{gls}\right)^{11}} \mathbb{E}_t\left[B_{i,t+11}\right], \quad (15)$$

where $\mathbb{E}_t\left[NI_{i,t+1}\right]$ and $\mathbb{E}_t\left[NI_{i,t+2}\right]$ are estimated using median analyst FY1 and FY2 EPS forecasts ($FEPS_{i,t+1}$ and $FEPS_{i,t+2}$) from the Institutional Brokers' Estimate System (I/B/E/S), and where $\mathbb{E}_t\left[NI_{i,t+3}\right]$ ($FEPS_{i,t+3}$) is estimated as the median FY2 analyst EPS forecast times the median analyst gross long-term growth-rate forecast from I/B/E/S. For those firms with no long-term growth forecasts, GLS uses the growth rate implied by the one- and two-year-ahead analyst EPS forecasts—i.e., $FEPS_{i,t+3} = FEPS_{i,t+2}\left(1 + FEPS_{i,t+2}/FEPS_{i,t+1}\right)$. In estimating the book value per share, GLS relies on the clean surplus relation and applies the most recent fiscal year's dividend-payout ratio (k) to all future expected earnings to obtain forecasts of expected future dividends—i.e., $\mathbb{E}_t D_{t+n+1} = \mathbb{E}_t NI_{t+n+1} \times k$. GLS uses the trailing 10-year industry me-

[11] Also known as the Edwards-Bell-Ohlson model, the residual income model simply re-expresses the dividend discount model by assuming that book value forecasts satisfy the clean surplus relation, $\mathbb{E}_t B_{i,t+n+1} = \mathbb{E}_t B_{i,t+n} + \mathbb{E}_t NI_{i,t+n+1} - \mathbb{E}_t D_{i,t+n+1}$, where $\mathbb{E}_t B_{i,t+n}$, $\mathbb{E}_t NI_{i,t+n}$, and $\mathbb{E}_t D_{i,t+n}$, are the time-t expectation of book values, net income, and dividends in $t+n$.

dian ROE to proxy for $\frac{\mathbb{E}_t[NI_{i,t+12}]}{\mathbb{E}_t[B_{i,t+11}]}$.[12] Finally, for years 4–12, each firm's forecasted ratio of expected net income over expected beginning book value is linearly interpolated to the trailing 10-year industry median ROE.

I use GLS to study the properties of ICC measurement errors for two primary reasons. First, it is one of the most widely used implementations of ICCs in studying expected return variation. Table 1 reports that since the work of Botosan (1997) that spawned the literature, 69% of the papers that study expected return variation using ICCs employ GLS.[13] A second reason for choosing GLS is that the model contains several interesting features, e.g., the roles of three-year-ahead forecasts and industry median ROE, that can contribute to measurement errors. These features are useful from a validation standpoint, because they provide some of the intuitions against which the efficacy of this paper's empirical methodology for explaining GLS measurement errors can be checked.

I compute GLS for all U.S. firms (excluding ADRs and those in the "Miscellaneous" category in the Fama-French 48-industry classification scheme) from 1976 to 2010, combining price and total-shares data from CRSP, annual financial-statements data from Compustat, and data on analysts' median EPS and long-term growth forecasts from I/B/E/S. GLS is computed as of the last trading day in June of each year, resulting in a sample of 75,055 firm-year observations.

In Table 2, summary statistics on GLS in my sample are reported and contrasted with realized returns, an ex post proxy for ex ante expected returns. Panel A reports annual cross-sectional summary statistics, including the total number of firms, the mean

[12]The use of "expected" long-run ROE to proxy for $\frac{\mathbb{E}_t[NI_{i,t+12}]}{\mathbb{E}_t[B_{i,t+11}]}$ can be viewed as a functional form assumption that contributes to measurement error, by Jensen's inequality.

[13]Table 1 summarizes the proxies of expected returns employed by papers published in top tier accounting and finance journals published since 1997. These journals are *The Accounting Review*, *Journal of Accounting and Economics*, *Journal of Accounting Research*, *Review of Accounting Studies*, *Contemporary Accounting Research*, *Accounting Horizons*, *Journal of Finance*, *Journal of Financial Economics*, *Review of Financial Studies*, and *Journal of Corporate Finance*. By combing through ABI-ProQuest and Business Source Complete as well as through the historical archives of the journals, I identified 54 papers that use ICCs as a measure of expected returns and, in particular, as a dependent variable in cross-sectional regression settings. These do not include the theoretically or methodologically oriented papers on ICCs, such as Gebhardt et al. (2001) or Easton and Monahan (2005). From this set of papers, 54% use the model of Claus and Thomas (2001), 69% use GLS, 61% use the related models of Gode and Mohanram (2003) and Ohlson and Juettner-Nauroth (2005), and 70% use the related models of PEG or MPEG (Easton, 2004); in contrast, future realized returns is the least popular, used in only 24% of the papers.

and standard deviation of GLS and 12-month-ahead realized returns, the risk-free rate, and the implied and ex post risk premiums.[14] Panel B reports summaries of the Panel A data by five-year sub-periods and for the entire sample period. For example, columns 2-7 of Panel B report the averages of the annual median and standard deviation of GLS, the averages of the annual mean and standard deviation in realized returns, the average of the annual risk-free rate, and the average of the annual implied risk-premium over the relevant sub-periods.

Overall, the patterns and magnitudes shown in Table 2 are consistent with prior implementations of GLS (e.g., Gebhardt et al., 2001). Critically, these patterns illustrate an important difference between ICC and realized return as expected-return proxies. Consistent with prior work (e.g., Campbell, 1991; Vuolteenaho, 2002), these summary statistics suggest that GLS is much more precise (i.e., they have lower measurement error variance). Unlike realized returns, whose average cross-sectional standard deviation is 47.67%, GLS exhibits far less variation, with an average cross-sectional standard deviation of 4.34%. Therefore, a finding that GLS measurement errors are random noise would support the view that ICCs should be unambiguously preferred over realized returns in regression settings.

3.2 Randomness of GLS Measurement Errors

To address whether GLS measurement errors are random, AR(1) parameters of GLS measurement errors are estimated following the methodology outlined in Appendix A. I also estimate the AR(1) parameter for the expected returns process.

Appendix A shows that the GLS measurement-error persistence parameter for a firm (ψ_i^{gls}) is identified by the equation $c_i(s) - cr_i(s+1) = \psi_i \times [c_i(s-1) - cr_i(s)]$, where $c_i(s) \equiv Cov\left(\widehat{er}_{i,t+s}^{gls}, \widehat{er}_{i,t}^{gls}\right)$ is the s-th order sample autocovariance of the firm's GLS. The measurement-error persistence parameter can be estimated from the slope coefficient of an OLS regression of $\{\widehat{c}_i(s) - \widehat{cr}_i(s+1)\}_{s\geq 1}^{T}$ on $\{\widehat{c}_i(s-1) - \widehat{cr}_i(s)\}_{s\geq 1}^{T}$, where $\widehat{c}_i(s)$ is

[14] Risk-free rates are the one-year Treasury constant maturity rate on the last trading day in June of each year, obtained from the website of the Federal Reserve Bank of St. Louis: http://research.stlouisfed.org/fred2/series/DGS1/

the sample analog of $c_i(s)$. Similarly, the expected-returns persistence parameter for a firm (ϕ_i), under the model dynamics, is identified by the equation $cr_i(s+1) = \phi_i \times cr_i(s)$, where $cr_i(s) \equiv Cov\left(r_{i,t+s}, \widehat{er}^{gls}_{i,t}\right)$ is the covariance between firm i's realized annual returns from $t+s-1$ to $t+s$ and GLS in period t. The expected-returns AR(1) parameter can be estimated from the slope coefficient of an OLS regression of $\{\widehat{cr}_i(s+1)\}^T_{s\geq 1}$ on $\{\widehat{cr}_i(s)\}^T_{s\geq 1}$, where $\widehat{cr}_i(s)$ is the sample analog of $cr_i(s)$.

For tractability, I assume persistence parameters are industry-specific and report in Table 3 the estimates based on the Fama and French (1997) 48-industry classification.[15] Panel B of Table 3 reports the estimated persistence parameters, the t-statistics, and R^2 for each of the 48 Fama-French industries (excluding the "Miscellaneous" category), and Panel A reports summary statistics across all industries.

In every industry the estimated persistence parameters for expected returns are positive and bounded between 0 and 1. Across the 47 industries in the sample, the mean (median) industry AR(1) parameter for expected returns is 0.55 (0.56), with a standard deviation of 0.21, mean (median) t-statistics of 3.82 (3.35), and mean (median) R^2 from the linear fit of 36.39% (34.88%).

Centrally, Table 3 reports the first estimates, to my knowledge, of ICC measurement-error persistence in the literature. These estimates suggest that GLS measurement errors are persistent. Though on average less persistent than expected returns, the estimated coefficients are significant. The mean (median) industry AR(1) parameter for GLS measurement errors is 0.47 (0.48), with a standard deviation of 0.18, mean (median) t-statistics of 3.05 (3.03), and mean (median) R^2 from the linear fit of 29.23% (28.93%). In an untabulated t-test test, I find an overall t-statistic of 17.61, rejecting the null that the mean persistence in GLS measurement errors is no different from 0 at the 1% level.

To summarize, these AR(1) estimates suggest that GLS measurement errors are non-random. They also raise the possibility that GLS measurement errors could induce biases

[15] These estimates are produced using sample industry-specific covariances and autocovariances for up to 19 lags. For each industry l and for lags $s = 1, ..., 19$, I estimate $\widehat{cr}_l(s) \equiv \widehat{Cov}\left(r_{i,t+s}, \widehat{er}^{gls}_{i,t}\right)$ $\forall i \in l$ and $\widehat{c}_l(s) \equiv \widehat{Cov}\left(\widehat{er}^{gls}_{i,t+s}, \widehat{er}^{gls}_{i,t}\right)$ $\forall i \in l$. These estimated covariances, $\{\widehat{cr}_l(s)\}^{19}_{s\geq 1}$ and $\{\widehat{cr}_l(s)\}^{19}_{s\geq 1}$, are then used to estimate the industry-specific expected-returns and GLS measurement-error persistence parameters.

in regression settings, as motivated and discussed in Section 2. I examine this possibility empirically next.

3.3 Cross-Sectional Variation in GLS Measurement Errors

To assess the cross-sectional associations between GLS measurement errors and firm characteristics, I construct the GLS measurement-error proxy using the estimated industry-based AR(1) parameters estimates:

$$\widehat{w}_{i,t}^{gls}\left(\widehat{\psi}_i^{gls},\widehat{\phi}_i\right)\equiv\frac{\widehat{er}_{i,t+1}^{gls}-\widehat{\phi}_i\widehat{er}_{i,t}^{gls}}{\widehat{\psi}_i^{gls}-\widehat{\phi}_i}. \tag{16}$$

Following the methodology developed in Section 2, these associations of interest can be estimated in a regression of $\widehat{w}_{i,t}^{gls}$ on firm characteristics and industry fixed effects.

3.3.1 GLS Measurement Errors and Firm Characteristics

Table 5 reports results from a pooled fixed-effects regression of the GLS measurement-error proxy ($\widehat{w}_{i,t}^{gls}$) on ten firm characteristics that are commonly hypothesized (or have been shown) to explain the cross-sectional variation in expected returns and that have been widely used as explanatory variables in the ICC literature: *Size*, defined as the log of market capitalization (in \$millions); *BTM*, defined as the log ratio of book value of equity to market value of equity; *3-Month Momentum*, defined as a firm's realized returns in the three months prior to June 30 of the current year; *DTM*, defined as the log of 1 + the ratio of long-term debt to market capitalization; *Market Beta*, defined as the CAPM beta and estimated for each firm on June 30 of each year by regressing the firm's stock returns on the CRSP value-weighted index using data from 10 to 210 trading days prior to June 30; *Standard Deviation of Daily Returns*, defined as the standard deviation of a firm's daily stock returns using returns data from July 1 of the previous year through June 30 of the current year; *Trailing Industry ROE*, defined as the industry median return-on-equity using data from the most recent 10 fiscal years (minimum 5 years and excluding loss firms) and using the Fama-French 48-industry definitions; *Analyst Coverage*, defined as the log of the total number of analysts covering the firm; *Analyst Dispersion*, defined as the log of 1 + the standard deviation of FY1 analyst EPS forecasts; and *Analyst LTG*, defined as the median analyst projection of long-term earnings growth. All analyst-based data are reported by I/B/E/S, as of the prior date closest to June 30 of each year. Summary statistics of the main dependent and independent variables are reported in Table 4.[16]

Industry fixed effects are included throughout, following the estimation methodology (12), and year dummies are also included to account for time fixed effects. The computation of standard errors also warrants explanation, as it is extensive and requires two steps. First, I account for within-industry and within-year clustering of residuals by computing two-way cluster robust standard errors (see Petersen, 2009; Gow, Ormazabal, and Taylor, 2010), clustering by industry and year. Second, since the AR(1) parameters are estimated, I account for the additional source of variation arising from the first-stage estimation following the bootstrap procedure of Petrin and Train (2003).[17] All coefficients and standard errors have been multiplied by 100 for ease of reporting, so that each coefficient can be interpreted as the expected percentage point change in GLS measurement errors associated with a 1 unit change in the covariate.

Table 5 reports empirical evidence that GLS measurement errors are significantly associated with characteristics relevant to the firm's risk and growth profile (e.g., *Size*, *BTM*, and *Analyst LTG*) and with characteristics relevant to the firm's information environment (e.g., *Analyst Coverage* and *Analyst Dispersion*). Columns 1 and 2 report a positive (negative) association between *Size* (*BTM* and *3-Month Momentum*) and GLS measurement errors, but no significant associations exist with *DTM*, *Market Beta*, *Standard Deviation of Daily Returns*, or *Trailing Industry ROE*. Column 3 considers only analysts-based variables, and finds a negative (positive) association between *Analyst Dispersion* (*Ana-*

[16]Note that the mean value for the measurement error proxies cannot be interpreted as the average measurement error in GLS. This is because, by equation (11), this proxy contains a fixed effects term.

[17]The methodology adds an additional term—the incremental variance due to the first-stage estimation—to the variance of the parameters obtained from treating $\left(\widehat{\phi}_i, \widehat{\psi}_i^{gls}\right)$ as the true $\left(\phi_i, \psi_i^{gls}\right)$. Specifically, I generate 1000 bootstrap samples from which to estimate 1000 bootstrap AR(1) parameters. I then re-estimate the regressions using the bootstrapped AR(1) parameters (i.e., using the 1000 new bootstrap dependent variables). Finally, the variance in regression parameter estimates from the 1000 bootstraps is added to the original (two-way cluster robust) variance estimates (which are appropriate when ϕ and ψ are observed without error). These total standard errors are reported in Table 5.

lyst Coverage and *Analyst LTG*) and GLS measurement errors. When combining analyst and non-analyst regressors (i.e., columns 4 and 5), *BTM*, *3-Month Momentum*, *Analyst Coverage*, and *Analyst LTG* are significantly associated with GLS measurement errors. The coefficients on *Size* and their statistical significance attenuate in these specifications, compared to specifications that do not include *Analyst Coverage* (i.e., columns 1 and 2), likely due to their relatively high correlation (72%). Interpreting the specification in column 5, I find that, all else equal, a 1 unit increase in the firm's *BTM* (*3-Month Momentum*) is associated with an expected 2.24 (8.20) percentage point decrease in GLS measurement errors, with significance at the 10% (10%) level, and a 1 unit increase in a firm's *Analyst Coverage* (*Analyst LTG*) is associated with an expected 1.97 (2.25) percentage point increase in GLS measurement errors, with significance at the 5% (5%) level. The adjusted R^2s are high across the board, around 80% for each specification. However, this is a byproduct of the empirical strategy and driven by the industry fixed effects.[18]

To provide some intuition for these results, the estimates of Table 5 are consistent with the findings in the literature on the biases in analysts' forecasts. For example, the empirical findings that analysts tend to issue overly optimistic forecasts for growth firms (e.g., Dechow and Sloan, 1997; Frankel and Lee, 1998; Guay et al., 2011) imply that growth (lower *BTM*) firms tend to have higher ICCs and, all else equal, should produce more positive ICC measurement errors—consistent with the negative coefficients on *BTM* in Table 5. The empirical literature also finds that high long-term-growth estimates may capture analysts' degree of optimism (La Porta, 1996), implying that firms with high long-term-growth projections tend to have higher ICCs and, all else equal, should produce more positive ICC measurement errors—consistent with the positive coefficients on *Analyst LTG* in Table 5.

Overall, this evidence suggests that GLS measurement errors lead to spurious correlations in regression settings. For example, inferences on firm characteristics such as *BTM* based on GLS regressions are biased due to correlations with measurement errors. It is worth noting that not all of the risk proxies included in this analysis exhibit a sig-

[18]The identification of the coefficients requires industry fixed effects. Moreover, by construction, there is substantial across industry variation in ($\widehat{w}_{i,t}^{gls}$) which uses industry-specific persistence parameters.

nificant association with GLS measurement errors (e.g., *DTM* and *Market Beta*). An implication is that researchers can continue to make valid inferences on the coefficients of these "good" risk proxies (z), i.e., that are not correlated with GLS measurement errors, so long as they are uncorrelated with those risk proxies (x) found to be correlated with GLS measurement errors, in a regression of GLS on x and z. If x and z are correlated, however, then the coefficient on z would also be biased.

3.3.2 GLS Measurement Errors, Analyst Forecast Optimism, and Term Structure

Though biases in analysts' forecasts provide intuition for the results of Table 5, they may not be the only drivers of GLS measurement errors. Firm characteristics (e.g., *Size* and *BTM*) can also influence measurement errors through functional form misspecification, for example through the implicit ICC assumption of constant expected returns.[19] This section examines whether both sources of ICC measurement errors—analyst forecast errors and functional form misspecification—lead to biases.

The existing empirical and methodological literature on ICCs have focused solely on the role of forecast biases as a source of measurement errors (e.g., Easton and Sommers,

[19]The relations between firm characteristics and ICC measurement errors are, therefore, ex ante ambiguous, because it is unclear how a given firm characteristic would interact with the two potential sources of measurement errors. To illustrate, let

$$w(x) = \widehat{f}\left(p, \widehat{E}(x), x\right) - f(p, E, x)$$

where $\widehat{f}$ is a function mapping prices and forecasts of earnings to an ICC, f is the function mapping prices and "true" expectations of earnings to "true" expected returns, and w is the measurement error. Let x be some firm characteristic that is relevant in determining expected returns, ICCs, and expected earnings, and that also affects the degree of optimism in earnings forecasts $\widehat{E}$.

A simple first-order Taylor approximation of w around $x = 0$ yields the following expression

$$w \approx \left[\widehat{f}\left(p, \widehat{E}(0), 0\right) - f(p, E, 0)\right] + \left[\widehat{f}_E\left(p, \widehat{E}(0), 0\right)\widehat{E}_x(0) + \widehat{f}_x\left(p, \widehat{E}(0), 0\right) - f_x(p, E, 0)\right] x,$$

so that the marginal effect of the firm characteristic x on measurement errors is approximated by:

$$w' \approx \widehat{f}_E\left(p, \widehat{E}(0), 0\right)\widehat{E}_x(0) + \left[\widehat{f}_x\left(p, \widehat{E}(0), 0\right) - f_x(p, E, 0)\right].$$

This expression says that a change in the firm characteristic x affects ICC measurement errors in two ways: through its effect on the forecast of earnings and through a functional form effect.

This expression suggests that it is, in general, difficult to sign w' for some arbitrary characteristic x. While $\widehat{f}_E$ is positive, the signs of $\widehat{E}_x$, $\widehat{f}_x$, and f_x are ambiguous. For any arbitrary firm characteristic, therefore, there is no clear prediction on how it will be associated with ICC measurement errors.

2007; Guay et al., 2011; Hou et al., 2012). Therefore, examining whether analysts' forecast biases are the sole driver of ICC measurement error biases is important because of the implications for the efficacy of improving forecasts as a solution to improving ICCs.

Specifically, I test roles of analyst forecast optimism and the implicit assumption of a constant expected return (Hughes et al., 2009) in driving GLS measurement errors. Ex ante, I expect ICC measurement errors (w) to be increasing with the degree of earnings-forecast optimism, $\widehat{E} - E$. The intuition comes from the dividend discount model: holding prices and fundamentals (i.e., true expected returns) fixed, an increase in forecasted cash flows (the numerator) in some future period mechanically increases the implied cost of capital (the denominator), thereby making the measurement errors—the difference between the ICC and the underlying expectation of returns—more positive. I also expect ICC measurement errors to be increasing in the slope of the term structure in expected returns, or the difference between expected one-period returns in the long-run from expected returns over the next period. Because an ICC represents a yield, or implicitly assumes a flat term structure in expected returns, it can be viewed as a weighted average of expected future returns $[\sum_{j=1}^{\infty} \omega_j \mathbb{E}_t (r_{t+j})]$. This average can overstate (understate) the true expected returns over the next period $[\mathbb{E}_t (r_{t+1})]$ if the term structure is upward-sloping (downward-sloping), since long-run expected returns are relatively high (low). Thus, all else equal, ICC measurement errors are more positive for firms with more positive-sloping term structures in expected returns.

I begin by testing the relation between GLS measurement errors and the degree of optimism in analyst forecasts; doing so requires unbiased forecasts for earnings expectations. For this purpose I adopt the mechanical earnings-forecast model of Hou et al. (2012), which produces benchmark earnings forecasts in a two-step process: first, estimate historical relations between realized earnings and firm characteristics by running historical pooled cross-sectional regressions; second, apply the historically estimated coefficients on current firm characteristics to compute the model-implied expectation of future earnings.[20]

[20] Appendix B explains my implementation and estimation of Hou et al. (2012)'s mechanical forecast model.

This characteristic-based mechanical forecast model is a useful benchmark for studying analyst forecast optimism. Hou et al. (2012) show that these mechanical earnings forecasts closely match the consensus analyst forecasts in terms of forecast accuracy, but exhibit lower levels of forecast bias. Moreover, they show that these forecasts produce higher levels of earnings response coefficients compared to consensus analyst forecasts, and argue that their mechanical forecasts are closer to the true expectations of earnings.[21]

Denoting Hou et al.'s time-t mechanical forecasts of $FY_{t+\tau}$ EPS as $\widehat{E}_{j,t+\tau}$, I define the following analyst optimism variables: for $\tau = 1, 2, 3$, FY_τ *Forecast Optimism* is the difference between the analyst FY_τ median EPS forecast and $\widehat{E}_{j,t+\tau}$. A benchmark for a firm's average long-run earnings is also necessary to obtain empirical measures for the level of optimism in the terminal earnings forecast in GLS. I use the average of FY3, FY4 and FY5 mechanical forecasts [i.e., $(\widehat{E}_{j,t+3} + \widehat{E}_{j,t+4} + \widehat{E}_{j,t+\tau})/3$] as the long-run benchmark, and define *Terminal Forecast Optimism* as the difference between the implied FY12 earnings and the long-run benchmark.[22] Finally, following the literature, I also create scaled versions of the optimism variables, scaling by total assets and by the standard deviation in analyst FY1 earnings forecasts.

It is worth highlighting several features of GLS that yield some intuitions about the expected relations between GLS measurement errors and analyst forecast optimism and that facilitate the assessments of my empirical methodology and results. The first such feature is the important role of the FY3 earnings forecast. GLS forecasts the ratio of expected net income to expected book value from FY4 to FY11 by linearly interpolating from the forecasted FY3 ratio to the trailing industry median ROE. Holding constant the accuracy of the terminal forecast, to the extent that FY3 earnings forecasts are overly optimistic, the subsequent years' forecasts will also be upwardly biased. Therefore, the

[21]These authors define forecast bias as realized earnings minus forecast earnings (standardized by market capitalization for model-based forecasts and by price for I/B/E/S forecasts); they define forecast accuracy as the absolute value of forecast bias.

[22]The use of the average of FY3, FY4, and FY5 as a benchmark need not follow from the assumption that such an average represents a good levels forecast of the firm's long-run earnings. Under the assumption that the difference between the GLS terminal EPS forecast and the long-run benchmark is proportional to the difference between the GLS terminal EPS forecast and the true but unobserved expected long-run EPS, variations in *Terminal Forecast Optimism* may still be informative about the degree of terminal forecast optimism.

degree of optimism in FY3 forecasts is expected to play an especially important role in explaining GLS measurement errors. A more obvious feature of GLS is the important role of the terminal value assumption. All else equal, GLS measurement errors are expected to be positively associated with the degree of optimism in the terminal earnings forecast.

Table 6 reports results from a pooled fixed-effects regression of GLS measurement-error proxy ($\widehat{w}_{i,t}^{gls}$) on *FY1*, *FY2*, and *FY3 Forecast Optimism* and *Terminal Forecast Optimism*. Year and industry fixed effects are included throughout, and the computation of standard errors as well as the reporting conventions are identical to Table 5. Columns 1-3 use the unscaled optimism variables, and columns 4-6 (7-9) use the scaled optimism variables, scaling by total assets (standard deviation of FY1 analyst forecasts).

Consistent with intuition, GLS measurement errors are associated positively and significantly (at the 1% level) with *FY3 Forecast Optimism* (columns 1, 4, and 7), and positively and significantly (at the 5% level) with *Terminal Forecast Optimism* (columns 2, 5, and 8), regardless of scaling.[23] In specifications that include all optimism variables (columns 3, 6, and 9), *FY3 Forecast Optimism* appears to be more important in explaining measurement errors, as its coefficient remains associated positively and significantly (at the 5% level) with GLS measurement errors, while the coefficient on *Terminal Forecast Optimism* is attenuated and no longer statistically significant at conventional levels.[24]

Interpreting the coefficients in column 3, I find that a one dollar increase in analysts' *FY3 Forecast Optimism* is associated with an expected 1.14 percentage-point increase in GLS measurement errors, with statistical significance at the 5% level. A one dollar increase in *Terminal Forecast Optimism* is associated with an expected 22 basis-point increase in GLS measurement errors, but the coefficient is not statistically significant at the conventional levels. Measures of *FY1* and *FY2 Forecast Optimism* are not significant in any of the specifications in Table 6, which is unsurprising in that for GLS the bias in FY3 earnings forecasts has disproportionate influence on GLS measurement errors. Overall, these results are consistent with GLS' unique modeling features, and provide comfort

[23]In untabulated results, I find that scaling forecasts by price yields qualitatively identical results to those of Table 6.

[24]This may be due to the possibility that earnings forecast optimism can be measured with greater precision in the short run than in the long run.

that the methodology developed in this paper are useful for explaining the variations in GLS measurement errors.

I now turn to consider jointly the influence of analyst forecast optimism and the implicit assumption of constant expected returns on GLS measurement errors. In particular, I use a proxy from the work of Lyle and Wang (2014), who develop a methodology for estimating the term structure of expected returns at the firm level based on two firm fundamentals: *BTM* and *ROE*. Their model assumes that the expected quarterly-returns and the expected quarterly-*ROE* revert to a long-run mean following AR(1) processes, and produces empirical estimates of a firm's expected returns over all future quarters. I approximate the slope of the term structure (*Term*) as the difference between the long-run expected (quarterly) returns from the expected one-quarter-ahead returns following the model of Lyle and Wang (2014).

Table 7 replicates the fixed-effects regressions of Table 6, but includes as additional controls *Size*, *BTM*, *3-Month Momentum*, and *Term*. Qualitatively the results with respect to analyst forecast optimism remain unchanged, but the coefficients and their statistical significance attenuate slightly relative to Table 5. The attenuation is likely a result of the correlation between analyst optimism and the controls—for example, the aforementioned empirical observation that analysts are overly optimistic about higher-growth (e.g., lower *BTM*) firms.

Of particular importance is the consistent finding that the constant term structure assumption is important in driving GLS measurement errors. In all specifications, the steeper the slope in the term structure of expected returns, the more positive are GLS measurement errors, consistent with expectations, with all coefficients on *Term* being statistically significant at the 5% level. Moreover, the coefficients on *Size* and *3-Month Momentum* remain statistically significant in nearly all of the specifications, even after controlling for biases in analyst forecasts. These empirical findings support the view that analyst forecast biases do not, by themselves, drive the biases in GLS errors, and that functional form misspecification incrementally and significantly contributes to these biases also. Finally, to my knowledge, the empirical results of Tables 5–7 are the first direct empirical evidence broadly in support of the theoretical results of Hughes et al. (2009).

3.3.3 Validity Test

The findings of Tables 6 and 7 are consistent with intuitions about the sources of GLS measurement errors, providing some comfort and validity to the methodologies developed in this paper. In this section I conduct further validity tests to speak to the informativeness of this paper's methodologies about GLS measurement errors. The logic of this validity test relies on expected returns as a statistical construct—the conditional expectation of future returns—and is consistent with the view that realized returns are unbiased for expected returns.[25] Under this construct, good proxies of expected returns should, on average, sort future realized returns (e.g., Guay et al., 2011; Hou et al., 2012; Lyle and Wang, 2014; Lewellen, 2015).

Section 2.4 shows that if $\widehat{w}_{i,t}^{gls}$ indeed captures GLS measurement errors' cross-sectional associations with firm characteristics, then a modified version of GLS ($\widehat{er}_{i,t}^{mgls} \equiv \widehat{er}_{i,t}^{gls} - \widehat{w}_{i,t}^{gls}$) captures the cross-sectional association between expected returns and firm characteristics. In other words, if the paper's model is valid for GLS, then a fixed-effects regression of Modified GLS (ModGLS) on firm characteristics should, compared to GLS, produce regression coefficients that better capture the systematic associations between expected returns and firm characteristics. If so, these regression coefficients can be used to form proxies of expected returns that exhibit superior ability in sorting future returns.

Following this intuition, I construct proxies of expected returns using historically estimated regression coefficients on firm characteristics estimated using ModGLS, and compare them with similarly estimated expected-return proxies but estimated using GLS. A finding that those proxies constructed from historically estimated associations between ModGLS and firm characteristics exhibit superior ability in sorting future returns is consistent with the paper's empirical methodologies being informative about GLS mea-

[25] An alternative that has been relied on in the ICC literature is a model-based construct (e.g., Botosan and Plumlee, 2005; Botosan et al., 2011), in which expected returns are expected to exhibit certain associations with measures of risk, as predicted by theoretical models.

surement errors.

I follow a two step procedure to create expected-return proxies using historically estimated associations between ModGLS and firm characteristics. First, in each year (t) I regress ModGLS on firm characteristics using three years' data from $t-1$ to $t-3$ (with year and industry fixed effects), and obtain estimated coefficients $\widehat{\delta}_{t-1}$.[26] Second, I apply the coefficients $\widehat{\delta}_{t-1}$ on current values of covariates X_t to obtain expected returns (Fitted ModGLS) over the next year.

I consider three sets of covariates (corresponding to the significant covariates in the three regression specifications of Table 10 presented in the next section). *Model 1* is a three-factor model with

$$X_t = \{Size_t,\ BTM_t,\ Momentum_t\}\,;$$

Model 2 is a five-factor model with

$$X_t = \{Size_t,\ BTM_t,\ Momentum_t,\ DTM_t,\ StdRet_t\}\,;$$

and Model 3 is an seven-factor model with

$$X_t = \{Size_t, BTM_t, Momentum_t, DTM_t, StdRet_t, AnalystDispersion_t, AnalystLTG_t\}.^{27}$$

After estimating the Fitted ModGLS using this procedure, I sort them into decile portfolios and summarize the average realized 12-month-ahead returns within each decile. I compare these average returns to those produced by decile portfolios formed by GLS (i.e., by decile ranking $\widehat{er}^{gls}_{i,t}$) and Fitted GLS, which is created following the above two-step procedure but using GLS as the dependent variable. Again, if the regression coefficients estimated using ModGLS better capture the systematic relations between expected re-

[26]The regression requires a 1-year lag since the dependent variable, $\widehat{er}^{mgls}_{i,t} \equiv \widehat{er}^{gls}_{i,t} - \widehat{w}_{i,t}\left(\widehat{\psi}^{gls}_i, \widehat{\phi}\right)$, requires $\widehat{er}^{mgls}_{i,t+1}$. Recall that $\widehat{w}_{i,t}\left(\widehat{\psi}^{gls}_i, \widehat{\phi}\right) = \frac{\widehat{er}^{gls}_{i,t+1} - \widehat{\phi}_i \widehat{er}^{gls}_{i,t}}{\widehat{\psi}^{gls}_i - \widehat{\phi}_i}$.

[27]Because of the high degree of correlation between *Size* and *Analyst Coverage*, I use only the former even though in Table 10 the coefficients on *Analyst Coverage* are significant.

turns and firm characteristics, then I expect Fitted ModGLS to sort future returns better than does Fitted GLS. As a performance metric, I compare the average decile spread—the average difference in the realized 12-month-ahead returns between the top and bottom decile portfolios—over the period from June 30, 1979 to June 30, 2010.[28]

Table 8 Panel A (B) compares the realized 12-month-ahead market-adjusted (size-adjusted) returns between GLS, Fitted GLS, and Fitted ModGLS decile portfolios, which are formed annually.[29] The Fitted ModGLS sorts future returns best, producing substantially larger decile spreads (reported in row 1) than either GLS or Fitted GLS. Panel A (B) finds the average market-adjusted (size-adjusted) annual decile spread for GLS to be 1.4% (-0.30%), with time-series t-statistic of 0.43 (-0.95), suggesting that those firms with the highest values of GLS do not on average have realized returns that are statistically different from those with the lowest values of GLS.[30] Similarly, in none of the three models does Fitted GLS exhibit significant ability to sort future market- or size-adjusted returns.

In contrast, Fitted ModGLS exhibits economically and statistically significant ability to sort future returns in each of the three models. Fitted ModGLS estimated using *Model 1*, *2*, and *3* produces average decile spreads in market-adjusted (size-adjusted) returns of 11.16% (9.23%), 9.37% (7.58%), and 8.51% (6.69%), with all spreads statistically different from 0% at the conventional levels. Finally, tests of the hypotheses that the decile spreads produced by Fitted ModGLS are no different from those produced by GLS (reported in row 3) or Fitted GLS (reported in row 4) are rejected at the conventional levels in all cases, whether using a standard t-test or the Wilcoxon signed-rank test, suggesting that Fitted ModGLS exhibits superior return-sorting ability.

Table 9 repeats the exercise presented in Table 8, but considers decile portfolios formed within each year and each Fama-French industry. In other words, Table 8 compares the relative performance of GLS, Fitted GLS, and Fitted ModGLS in sorting future returns

[28]The first year for which I obtain Fitted ModGLS estimates is 1979, since our overall sample begins in 1976 and obtaining Fitted ModGLS estimates requires data from 1976 to 1978.

[29]Market adjustment is performed using the value-weighted CRSP market index; size adjustments are performed using CRSP size deciles, formed at the beginning of each calendar year.

[30]Time-series t-statistics are computed using the time-series standard deviation of annual decile spreads.

for the cross section of stocks, while Table 9 compares how they sort within-industry returns. Overall, the results of Table 9 are consistent with those of Table 8. GLS and Fitted GLS exhibit no economically or statistically significant within-industry return-sorting ability. In contrast, Fitted ModGLS exhibits significant within-industry return-sorting ability in each of the three models, with decile spreads that are economically and statistically significant and that are statistically different from those produced by GLS or Fitted GLS.

In summary, Tables 8 and 9 are evidence that the methodology developed in this paper is informative about GLS measurement errors. These tables also lend further credence to the results of Tables 5–7 and suggest that regressions using Modified GLS produce coefficients that better capture the systematic relations between expected returns and firm characteristics.

3.4 Expected Returns and Firm Characteristics

Having established the efficacy of this paper's methodology in explaining GLS measurement errors, I turn to the assessment of regression inferences using GLS.

3.4.1 Comparing Inferences from GLS and ModGLS

In Table 10 Panels A, B, and C, I estimate fixed-effects regressions of expected-return proxies on firm characteristics widely hypothesized to be associated with the expected rate of returns. For ease of interpretation, I follow Gebhardt et al. (2001) and standardize each explanatory variable by its cross-sectional annual mean and standard deviation. Year and industry fixed effects are included in each regression and the reporting conventions are as specified in Table 5.

Columns 1 and 2 of each panel report fixed-effects regression coefficients estimated using GLS and ModGLS, respectively. Panel A considers *Size*, *BTM*, and *3-Month Momentum* as covariates, as in *Model 1* of Tables 8 and 9. Consistent with the prior literature, regressions using GLS in Panel A, column 1 suggest a a negative (positive) and significant association between expected returns and *Size* (*BTM*). The association between GLS and *3-Month Momentum* is negative and statistically significant at the 1% level, which is inconsistent with the well-documented momentum effect (e.g., Jegadeesh and Titman, 1993; Chan, Jegadeesh, and Lakonishok, 1996) that higher momentum firms are expected to have higher future returns. One explanation is that this negative association is an artifact of how GLS (and ICCs more generally) is constructed. Since price and $\widehat{er}_{i,t}^{gls}$ are inversely related by construction (15), holding expectations of future fundamentals fixed, firms with greater recent price appreciation may also tend to have lower values of GLS.

Column 2 of Panel A estimates fixed-effects regression coefficients of ModGLS on *Size*, *BTM*, and *3-Month Momentum*. The coefficients on *Size* and *BTM* remain negative and positive, respectively, similar to the column 1 results using GLS, though the estimated magnitudes differ. In contrast, the coefficient on *3-Month Momentum* reverses in sign: it is positive and statistically significant at the 10% level, consistent with the momentum phenomenon. The empirical evidence in Table 5, GLS measurement errors are more negative for higher momentum firms, suggest that the negative and significant coefficients on *3-Month Momentum* in column 1 reflects *Momentum*'s associations with GLS measurement errors.

Panel B of the table considers four additional firm characteristics as covariates: *Market Beta*, *DTM*, *StdDev of Daily Returns*, and *Trailing Industry ROE*. In column 1, using GLS as the dependent variable, the coefficients on *Size*, *BTM*, and *3-Month Momentum* are very similar to those reported in Panel A, column 1 in terms of both magnitudes and statistical significance. Moreover, GLS is associated negatively and significantly (at the 1% level) with *Market Beta*, and positively and significantly with *DTM*, *StdDev of Daily Returns*, and *Trailing Industry ROE* (all at the 1% level). The results on *Market Beta* and *Trailing Industry ROE* are unexpected and likely a result of spurious correlations. If the capital asset pricing model explains the cross section of expected returns, the relation between expected returns and *Beta* should be positive. If CAPM does not work, or if the estimation of *Beta* is too noisy, there should be no association with expected returns. It is also unclear whether a positive association should exist between a firm's expected returns and its *Trailing Industry ROE*. A potential explanation is that this again is a mechanical artifact of the way GLS is constructed. Since GLS uses the *Trailing Industry ROE* in its terminal value assumptions, higher *Trailing Industry ROE* mechanically yields higher values of GLS, all else equal.

Panel B, column 2, which uses ModGLS as the dependent variable, also shows that the inclusion of the four additional variables has little impact on the coefficients on *Size*, *BTM*, and *3-Month Momentum*: all three coefficients remain very similar to those reported in Panel A, column 2, in terms of both magnitudes and statistical significance. As in Panel A, the coefficient on *3-Month Momentum* reverses in sign, from negative and significant in column 1 to positive and significant in column 2. Moreover, Panel B, column 2, reports coefficients on *DTM* and *StdDev of Daily Returns* that are positive and significant, consistent both with expectations and with column 1. Unlike in column 1, the coefficient on *Market Beta* is no longer statistically different from 0, though its magnitude is larger; nor is the coefficient on *Trailing Industry ROE* any longer statistically different from 0, with magnitudes that are substantially attenuated toward zero. This evidence, combined with the results of Table 5 are consistent with the associations of GLS with *Beta* and *Trailing Industry ROE* reflecting measurement errors in GLS.

Panel C adds to the covariates in Panel B three analyst-based variables: *Analyst Coverage*, *Analyst Dispersion*, and *Analyst LTG*. The addition of these variables does not substantially change the magnitudes or significance of the coefficients on the non-analyst variables in column (1) compared to Panel A. Moreover, I find that GLS is positively and significantly (at the 1% level) associated with *Analyst Dispersion* and *Analyst LTG*. The coefficient on *Analyst Coverage* is negative, but not statistically different from 0, probably due to its high correlation with *Size*. The positive association between GLS and *Analyst LTG* is inconsistent with the empirical observation that firms with high LTG estimates tend on average to have lower returns (e.g., La Porta, 1996) and is likely a mechanical artifact of how GLS is computed. Recall that GLS uses median analyst forecasts of FY1, FY2, and FY3 EPS; however, the FY3 forecast is imputed by applying *Analyst LTG* projections to the median FY2 EPS forecast. To the extent that larger values of

Analyst LTG tend to be too extreme, as argued by La Porta (1996), GLS' forecasts of FY3 earnings will also be too optimistic. In other words, the positive association between GLS and *Analyst LTG* probably reflects the degree of optimism in FY3 forecasts.[31] With the exception of *Size*, the addition of analyst variables in Panel C does not substantially change the magnitudes or significance of the coefficients on the non-analyst variables in column 2 relative to those in Panel A. The attenuation in the coefficient and significance of *Size* is not surprising, given the relatively high correlation (72%) between *Analyst Coverage* and *Size*. Consistent with column 1, I find ModGLS to be associated positively and significantly (at the 1% level) with *Analyst Dispersion*; however, unlike column 1 and consistent with expectations, the coefficient on *Analyst LTG* reverses in sign and becomes negative and statistically significant at the 5% level. This evidence, combined with the results of Table 5, suggest that the associations between GLS and *Analyst LTG* are likely influenced by systematic measurement errors in GLS.

3.4.2 Inferences After Removing Forecast Biases

A natural question arising from the above results—that GLS likely suffers from spurious correlations through dependent-variable measurement errors—is whether mitigating earnings-forecast biases could improve regression inferences. Complementing the results of Tables 6 and 7 suggest that earnings-forecast optimism is not the sole driver of GLS measurement errors, column 3 of each panel addresses this question by considering "MechGLS" as an alternative dependent variable in Table 10. This proxy of expected returns is an implementation of GLS but uses the benchmark earnings forecasts of Hou et al. (2012).

The regression coefficients using MechGLS are, in general, directionally similar to those estimated using GLS, but the magnitudes and statistical significance differ. For example, in all three panels the coefficients on *Size* and *BTM* are substantially larger in magnitude than those estimated using GLS, and generally closer to the coefficients estimated using ModGLS (reported in column 2). However, many of the surprising

[31] In untabulated results, I find that the measures of *FY3 Forecast Optimism* used in this paper are positively and significantly associated with *Analyst LTG*.

coefficients estimated using GLS persist in regressions using MechGLS. In particular, *3-Month Momentum* and *Market Beta* remain negative and significant (both at the 1% level), while *Trailing Industry ROE* remains positive and significant (at the 1% level) in all relevant panels. As discussed above, because the associations with *Momentum* and *Trailing Industry ROE* are likely a mechanical artifact of GLS' functional form, it is not surprising that correcting for forecast biases do not have an effect on these coefficients. In contrast, it is interesting to note that removing systematic biases in analyst forecasts explains away the puzzling negative association between GLS and *Analyst LTG*, though the coefficient is insignificant statistically.

In summary, MechGLS appears to resolves some puzzling associations between GLS and firm characteristics. Many of the unexpected associations persist, however, consistent with the view that the spurious correlations between firm characteristics and GLS measurement errors arise from *both* analysts' earnings-forecast errors and from functional form misspecification.

3.4.3 Inferences from Ex Post Realized Returns

Realized returns is defined as the sum of expected returns and unexpected returns, or news (see, e.g., Campbell, 1991; Vuolteenaho, 2002). Under the assumption that "news is news," that is unexpected returns cannot be systematically correlated with ex ante information, regressions using realized returns lead to consistent estimates of the associations between expected returns and firm characteristics. Despite this advantage, however, the noisiness of returns [e.g., Table 2 column (7)] implies that their use in regression settings can be expected to reduce the precision with which researchers can estimate associations between expected returns and firm characteristics.[32]

[32]To see these more formally, in estimating the slope coefficient (β) from

$$y = \beta_0 + \beta \cdot x + \varepsilon,$$

the critical condition for consistency is the uncorrelatedness between the residual (ε) and the regressor (x). Combining equations (10) and (1), the relation between realized returns and firm characteristics can be written as

$$r_{i,t+1} = \delta_0 + \delta \cdot z_{i,t} + (\varepsilon^{er}_{i,t} + news_{i,t}).$$

The properties of the residual ($\varepsilon \equiv \varepsilon^{er}_{i,t} + news_{i,t}$) imply uncorrelatedness with the regressor $z_{i,t}$, meaning that OLS yields consistent estimates of δ.

Column 4 of each panel in Table 10 reports regressions using 12-month-ahead realized returns as an ex post proxy for expected returns. Comparing these regression coefficients to those estimated using the alternative proxies of expected returns, I find that the coefficients in column 4 align most closely with those in column 2, estimated using ModGLS. Like column 2, column 4 reports a positive and significant coefficient on *3-Month Momentum* across all three panels, and no statistical significance in the coefficients on *Market Beta* and *Trailing Industry ROE.* However, consistent with a lack of precision, regressions of realized returns in Panels B and C do not obtain statistical significance in any additional covariates considered, in contrast to ModGLS.

Comparisons of column 4 coefficients on Size, *BTM*, and *Momentum* further bolster the hypothesis that regression coefficients estimated using GLS (or MechGLS) are influenced by spurious correlations with the dependent variable's measurement errors, and that the problem is unlikely to be fully resolved by accounting for systematic earnings-forecast biases. In particular, whereas column 4 regressions suggest that higher *Momentum* is positively and significantly associated with higher expected rates of returns, regressions using GLS and MechGLS lead to the opposite conclusion.

4 Implications, Recommendations, and Conclusion

This paper documents the first direct empirical evidence that ICC measurement errors can be nonrandom and correlated with firm characteristics. Applying methodologies developed in this paper to GLS, a popular implementation of ICCs, I show that GLS' measurement errors are on average quite persistent, with a median persistence parameter of 0.48. Moreover, I show that GLS measurement errors are associated with firm characteristics commonly associated with risk and growth profiles, and that these patterns are driven not only by systematic biases in analyst forecasts but also by functional form

Recall also that the OLS coefficients have the following sampling distribution (under homoscedasticity):

$$\sqrt{N}(\hat{\beta}-\beta) \sim N(0, \sigma_{\varepsilon}^2 \mathbb{E}(x_i x_i')),$$

where $x_{i,t}' = [1, x_{1i,t}, x_{2i,t}, ..., x_{ki,t}]$. The noisiness in realized returns increases the noisiness in the residual and the sampling variation in OLS coefficients, reducing precision.

assumptions embedded in GLS.

These findings not only imply that regression coefficients estimated using ICCs may capture or be influenced by spurious associations with dependent-variable measurement errors, but also that the standard methods for addressing measurement errors, namely portfolio grouping and instrumental variables, are unlikely to be effective. The idea behind grouping is to form portfolios of firms with similar expected returns, so that measurement errors (presumed to be random) cancel out on average at the portfolio level. Ideally, groups should be formed to minimize the within-group variation and maximize the across-group variation in expected returns. In practice, since true expected returns are not observed, the formation of grouping portfolios often involves the use of firm characteristics such as *Size* and *BTM* (e.g., Easton and Monahan, 2005), which are assumed to be correlated with expected returns but not with measurement errors. Clearly, the usefulness of this methodology is limited by the extent to which the grouping variables are systematically associated with measurement errors, or the extent to which measurement errors fail to cancel out in portfolios. In the case of GLS (i.e., Table 5), since average measurement errors are systematically different for firms of different *Size* and *BTM*, differences in average GLS values across portfolios formed on these variables are likely confounded by the portfolio differences in average measurement errors, raising doubts about the efficacy of such grouping methods.

Based on similar rationale, the instrumental variables (IV) approach is also unlikely to be effective. The idea behind the IV approach is to fit ICCs with a set of variables, the instruments, that are correlated with expected returns but not measurement errors. The usefulness of this approach depends on the validity of the instruments. Firm characteristics like *Size* and *BTM* are commonly-used instruments for ICCs (e.g., Gebhardt et al., 2001; Easton and Monahan, 2005), but again the evidence in Table 5 suggests that these variables (among others) violate the exclusion restriction (i.e., uncorrelatedness with measurement errors) in the case of GLS, raising doubts about the usefulness of the IV approach.

Making inferences on unobserved variables is a notoriously difficult task. In studying the properties of unobserved expected returns and in choosing among proxies of expected returns, researchers face important trade offs in the proxies' measurement error properties. The choice between any ICC and realized returns is a tradeoff between bias and efficiency. The measurement errors of realized returns—unexpected returns or news—are noisy but cannot, by definition of news, be systematically predictable over time. On the other hand, ICCs such as GLS may be less noisy, but their measurement errors can be systematically associated with firm characteristics. In other words, whereas ICCs are more precise, they have biases that, as illustrated in this paper, can confound regression inferences. Realized returns, on the other hand, yield consistent regression estimators in large sample, but are noisy and imprecise.

Given these tradeoffs, how should researchers make inferences about expected returns? In general, I argue that, to convincingly establish an association between expected returns and firm characteristics using ICCs, researchers should complement ICC regressions with regressions using realized returns. Caution should be applied in particular when ICC regressions and realized returns regressions produce statistically significant regression coefficients with opposite signs (e.g., the coefficient on *Momentum* in columns 1, 3, and 4 of Table 10), as these likely indicate evidence of spurious correlations with dependent variable measurement errors. However, in the 54 ICC papers I surveyed for this study, reported in Table 1, only 24% used realized returns as an alternative benchmark.

A practical and conservative approach for researchers is to begin with realized returns. Because realized returns are imprecise, their use as proxies of expected returns yields low-powered and conservative tests. If researchers are able to obtain statistical significance using realized returns, it would be sufficient for inference. In the absence of obtaining significant coefficients using realized returns, researchers can proceed to ICCs, which are more precise. However, researchers ought to acknowledge, discuss, and examine the possibility that the regression coefficients may be driven by spurious correlations with measurement errors. Finding that realized returns produce coefficients of similar sign and magnitude, but lack the significance that is obtained in regressions that use ICCs, can assuage these concerns. Another possibility is to demonstrate that different ICC models, in particular models with relatively low correlation, yield consistent results.[33] Of course, one possibility is to also implement GLS and the modified GLS approach proposed in this paper.

ICCs are an intuitively appealing class of expected-return proxies with the potential to help researchers better understand the cross-sectional variation in expected returns, but, echoing the sentiments of Easton (2009) and Lambert (2009), much remains unknown about their measurement errors and how to correct for them. Thus the use of ICCs in regression settings should be interpreted with caution. In the mean time, we should not hastily give up on realized returns as a proxy of expected returns.

[33] For example, Botosan et al. (2011) and Hou et al. (2012) report relatively low correlations between GLS and the PEG (or MPEG) models of Easton (2004). On the other hand, there's a relatively a high degree of correlation between GLS and ICC models proposed by Claus and Thomas (2001) and implementations of Gordon and Gordon (1997).

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Appendix

A Estimating AR(1) Parameters

To estimate the AR(1) parameters, recall the relation between future realized returns to ex ante conditional expected returns in Eqn (10): $r_{i,t+1} = er_{i,t} + news_{i,t+1}$. I assume that unexpected returns, *news*, cannot be systematically predictable. I argue that this property must be true by definition of "news," but it is also implied by the definition of conditional expectations. Conditional expectations are optimal in the sense of minimizing mean squared errors and its "Decomposition Property" (Angrist and Pischke, 2008, pp.32-33) provides the necessary structure on unexpected returns: it cannot be systematically predictable based on ex ante information. Under this "news is news" assumption the AR(1) parameters can be identified through the time-series autocovariance functions of expected-return proxies and the time-series covariance between realized returns and expected-return proxies.

A.1 Time-Series Expected-Return Proxy Autocovariance

Under the AR(1) structure and the "news is news" assumption, the s^{th} order autocovariance function for a firm i is given by

$$\begin{aligned} c_i(s) &\equiv Cov\left(\widehat{er}_{i,t+s}, \widehat{er}_{i,t}\right) \\ &= \phi_i^s \left[Var\left(er_{i,t}\right) + Cov\left(er_{i,t}, w_{i,t}\right)\right] + \psi_i^s \left[Var\left(w_{i,t}\right) + Cov\left(er_{i,t}, w_{i,t}\right)\right], \end{aligned} \tag{A1}$$

which is consistent with a covariance-stationary process.

A.2 Time-Series Realized Returns—Expected-Return Proxy Covariance

To derive the covariance between realized returns s periods ahead and current expected returns, I turn to the returns decomposition of Eqn (10). Substituting in the definition of expected-returns proxies from Eqn (2), s-year-ahead realized returns can be related to the current period's expected returns as

$$r_{i,t+s} = \phi_i^{s-1} er_{i,t} + \sum_{n=0}^{s-2} \phi_i^n u_{i,t+n+1} + \delta_{t+s}.$$

Using this decomposition, the k^{th} order return-proxy covariance for a firm i is given by

$$\begin{aligned} cr_i(s) &\equiv Cov\left(r_{i,t+s}, \widehat{er}_{i,t}\right) \\ &= \phi_i^{s-1} \left[Var\left(er_{i,t}\right) + Cov\left(er_{i,t}, w_{i,t}\right)\right]. \end{aligned} \tag{A2}$$

A.3 Identifying AR(1) Parameters

Combining the above functions $c_i(s)$ and $cr_i(s)$, the following relations are obtained:

$$c_i(s) - cr_i(s+1) = \psi_i \times (c_i(s-1) - cr_i(s)), \text{ and} \tag{A3}$$

$$cr_i(s+1) = \phi_i \times cr_i(s) \qquad \text{for } s \geq 1. \tag{A4}$$

Thus, using sample estimates $\widehat{c}_i(s)$ and $\widehat{cr}_i(s)$, ψ_i can be estimated from a time-series regression of $\{\widehat{c}_i(s) - \widehat{cr}_i(s+1)\}_{s\geq 1}^{T}$ on $\{\widehat{c}_i(s-1) - \widehat{cr}_i(s)\}_{s\geq 1}^{T}$; similarly, ϕ_i can be estimated from a time-series regression of $\{\widehat{cr}_i(s+1)\}_{s\geq 1}^{T}$ on $\{\widehat{cr}_i(s)\}_{s\geq 1}^{T}$.

B Residual Income Model and GLS

This paper's estimation of a firm's expected rate of equity returns follows the methodology of Gebhardt et al. (2001) (GLS), a valuation model based on the residual-income model that re-expresses the dividend-discount model:

$$P_{i,t} = \sum_{n=1}^{\infty} \frac{\mathbb{E}_t\left[D_{i,t+n}\right]}{\left(1+\widehat{er}_{i,t}\right)^n}.$$

By assuming that forecasts of book values satisfy clean surplus relation, i.e.,

$$\mathbb{E}_t B_{i,t+n+1} = \mathbb{E}_t B_{i,t+n} + \mathbb{E}_t NI_{i,t+n+1} - \mathbb{E}_t D_{i,t+n+1},$$

where $\mathbb{E}_t B_{i,t+n}$, $\mathbb{E}_t NI_{i,t+n}$, and $\mathbb{E}_t D_{i,t+n}$, are the time-t expectation of book values, net income, and dividends in $t+n$, the dividend-discount model can be rewritten as

$$\begin{aligned} P_{i,t} &= B_{i,t} + \sum_{n=1}^{\infty} \frac{\mathbb{E}_t\left[NI_{i,t+n}\right] - \widehat{er}_{i,t}\mathbb{E}_t\left[B_{i,t+n-1}\right]}{\left(1+\widehat{er}_{i,t}\right)^n}. \\ &= B_{i,t} + \sum_{n=1}^{\infty} \frac{\frac{\mathbb{E}_t[NI_{i,t+n}]}{\mathbb{E}_t[B_{i,t+n-1}]} - \widehat{er}_{i,t}}{\left(1+\widehat{er}_{i,t}\right)^n}\mathbb{E}_t\left[B_{i,t+n-1}\right]. \end{aligned}$$

Practical implementation of RIM requires explicit forecasts and a terminal-value estimate. GLS forecasts future earnings and book values for 12 years and makes a terminal-value assumption based on the trailing industry median ROE. GLS is the $\widehat{er}^{gls}_{i,t}$ that solves

$$P_{i,t} = B_{i,t} + \sum_{n=1}^{11} \frac{\frac{\mathbb{E}_t[NI_{i,t+n}]}{\mathbb{E}_t[B_{i,t+n-1}]} - \widehat{er}^{gls}_{i,t}}{\left(1+\widehat{er}^{gls}_{i,t}\right)^n}\mathbb{E}_t\left[B_{i,t+n-1}\right] + \frac{\frac{\mathbb{E}_t[NI_{i,t+12}]}{\mathbb{E}_t[B_{i,t+11}]} - \widehat{er}^{gls}_{i,t}}{\widehat{er}^{gls}_{i,t}\left(1+\widehat{er}^{gls}_{i,t}\right)^{11}}\mathbb{E}_t\left[B_{i,t+11}\right],$$

where $\mathbb{E}_t\left[NI_{i,t+1}\right]$ and $\mathbb{E}_t\left[NI_{i,t+2}\right]$ are estimated using median I/B/E/S analyst FY1 and FY2 EPS forecasts ($FEPS_{i,t+1}$ and $FEPS_{i,t+2}$) and where $\mathbb{E}_t\left[NI_{i,t+3}\right]$ ($FEPS_{i,t+3}$) is estimated as the median FY2 analyst EPS forecast times the median analyst gross long-term growth-rate forecast. For those firms with no long-term growth-rate forecasts, GLS uses the growth rate implied by the one- and two-year-ahead analyst EPS forecasts—i.e., $FEPS_{i,t+3} = FEPS_{i,t+2}\left(1 + FEPS_{i,t+2}/FEPS_{i,t+1}\right)$. In estimating the book value per share, GLS relies on the clean surplus relation and applies the most recent fiscal year's dividend-payout ratio (k) to all future expected earnings to obtain forecasts of expected future dividends: i.e., $\mathbb{E}_t D_{t+n+1} = \mathbb{E}_t NI_{t+n+1} \times k$. GLS uses the trailing 10-year industry median ROE to proxy for $\frac{\mathbb{E}_t[NI_{i,t+12}]}{\mathbb{E}_t[B_{i,t+11}]}$. Finally, for years 4–12, each firm's forecasted ratio of expected net income over expected beginning book value is linearly interpolated to the trailing 10-year industry median ROE.

C Mechanical Forecast Model Coefficients

This table reports the average regression coefficients and their time-series t-statistics from annual pooled regressions of one-year-ahead through five-year-ahead earnings on a set of variables that are hypothesized to capture differences in expected earnings across firms. Specifically, for each year t between 1970 and 2010, I estimate the following pooled cross-sectional regression using the previous ten years (six years minimum) of data:

$$E_{j,t+\tau} = \beta_0 + \beta_1 EV_{j,t} + \beta_2 TA_{j,t} + \beta_3 DIV_{j,t} + \beta_4 DD_{j,t} + \beta_5 E_{j,t} + \beta_6 NEGE_{j,t} + \beta_7 ACC_{j,t} + \varepsilon_{j,t+\tau}$$

where $E_{j,t+\tau}$ ($\tau = 1, 2, 3, 4$, or 5) denotes the earnings before extraordinary items of firm j in year $t + j$, and all explanatory variables are measured at the end of the year t; $EV_{j,t}$ is the enterprise value of the firm (defined as the sum of total assets and market value of equity minus the book value of equity); $TA_{j,t}$ is total assets; $DIV_{j,t}$ is the dividend payment; $DD_{j,t}$ is a dummy variable that equals 0 for dividend payers and 1 for non-payers; $NEGE_{j,t}$ is a dummy variable that equals 1 for firms with negative earnings and 0 otherwise; and $ACC_{j,t}$ is total accruals scaled by total assets, where total accruals are calculated as the change in current assets plus the change in debt in current liabilities minus the change in cash and short-term investments and minus the change in current liabilities. R^2 is the time-series average R-squared from annual regressions.

Yrs	*Cons*	*EV*	*TA*	*DIV*	*DD*	*E*	*NEGE*	*ACC*	R^2
1	2.097	0.010	-0.008	0.327	-2.251	0.756	0.963	-0.017	0.855
	(5.36)	(44.83)	-(33.65)	(37.83)	-(3.47)	(162.04)	(2.27)	-(8.93)	
2	3.502	0.013	-0.009	0.487	-3.191	0.680	3.143	-0.019	0.798
	(6.51)	(40.52)	-(27.40)	(39.45)	-(3.68)	(98.27)	(2.73)	-(7.68)	
3	14.855	-0.001	0.002	0.610	-10.001	0.337	1.397	0.010	0.466
	(23.05)	(5.65)	(0.30)	(42.83)	-(9.48)	(50.71)	(0.61)	(0.60)	
4	21.346	0.000	0.002	0.503	-13.631	0.231	-0.713	0.008	0.336
	(29.45)	(4.78)	(0.07)	(36.95)	-(11.70)	(36.41)	-(0.76)	(2.11)	
5	26.535	-0.001	0.003	0.445	-16.003	0.173	-3.038	0.008	0.261
	(33.44)	-(3.16)	(6.44)	(33.51)	-(12.76)	(27.59)	-(2.13)	(1.43)	

Table 1. Use of ICCs in Academic Literature

Table 1 reports the proxies of expected returns used in papers published since 1997 in the following accounting and finance journals: *The Accounting Review* (TAR), *Journal of Accounting and Economics* (JAE), *Journal of Accounting Research* (JAR), *Review of Accounting Studies* (RAST), *Contemporary Accounting Research*, *Accounting Horizons*, *Journal of Finance*, *Journal of Financial Economics*, *Review of Financial Studies*, and *Journal of Corporate Finance*. These articles are collated by searching for keywords and citations. Keyword searching involves searching full-text and abstracts in ABI-Proquest, Business Source Complete, and the historical archives of each journal for combinations of the terms "implied" and "ex ante" with variations on the term "cost of capital" (e.g., "cost of equity capital," "equity cost of capital," "risk premium"). Citation searching uses Google Scholar to find papers in the leading journals that cite the following methodological papers in the implied cost of capital literature: Botosan (1997), Claus and Thomas (2001), Easton (2004), Gebhardt et al. (2001), Gode and Mohanram (2003), Ohlson and Juettner-Nauroth (2005). Finally, in tallying the table's statistics, I counted papers that use expected-return proxies as a dependent variable in regression settings and exclude papers that are theoretic or methodological in nature. Panel A Reports the number of such articles found from each journal. Panel B reports the distribution of the ICC measures used in the literature. "CT" refers to the model of Claus and Thomas (2001), "GLS" refers to the model of Gebhardt et al. (2001), "OJ/GM" refers to the related models of Ohlson and Juettner-Nauroth (2005) and Gode and Mohanram (2003), and "PEG/MPEG" refers to the related models in Easton (2004). "Composite" indicates the use of composite ICC measures (typically involving taking the average of the four measures), while "Realized Returns" indicates the use of ex post returns.

Panel A: Number of Papers by Journal

Total	TAR	JAE	JAR	RAST	Other
54	17	4	11	7	15

Panel B: Expected-Return Proxies Used

CT	GLS	OJ / GM	PEG / MPEG	Realized Returns	Composite
54%	69%	61%	70%	24%	46%

Table 2. Summary Statistics on Expected-Return Proxies

Table 2, Panel A, reports, for all firm-year observations at the end of June of each year from 1976 to 2010, (1) the total number of observations, (2) the annual mean value of GLS, (3) the standard deviation of GLS, (4) the 12-month risk-free rate, (5) the implied risk premium, calculated as the difference between mean GLS and the risk-free rate, (6) the mean 12-month-ahead realized returns, (7) the standard deviation of 12-month-ahead realized returns, and (8) the ex post risk premium, calculated as the difference between mean returns and the risk-free rate. Risk-free rates as of the last trading day in June each year are obtained from the Federal Reserve Bank of St. Louis' one-year Treasury constant-maturity-rate series (`http://research.stlouisfed.org/fred2/data/DGS1.txt`). Each column of Panel B reports, for each five year interval from 1976 to 2010, the time-series averages for the respective columns of Panel A.

Panel A: Summary Statistics, by Year

Year	(1) Obs	(2) Mean GLS	(3) StdDev GLS	(4) Implied Premium	(5) RF Rate	(6) Mean Returns	(7) StdDev Returns	(8) Ex Post Premium
1976	529	12.02%	3.85%	6.46%	5.56%	9.80%	28.47%	3.34%
1977	655	12.49%	3.55%	5.72%	6.77%	18.95%	32.50%	13.23%
1978	792	12.57%	2.80%	8.38%	4.19%	15.88%	29.34%	7.50%
1979	1,069	13.57%	5.45%	9.40%	4.17%	17.64%	37.59%	8.24%
1980	1,091	14.38%	6.70%	8.49%	5.89%	43.34%	45.60%	34.85%
1981	1,137	13.91%	12.32%	14.87%	-0.96%	-12.60%	29.05%	-27.47%
1982	1,189	14.91%	7.22%	14.34%	0.57%	92.69%	75.36%	78.35%
1983	1,249	10.65%	3.93%	9.70%	0.95%	-8.65%	29.54%	-18.35%
1984	1,503	12.57%	3.30%	12.30%	0.27%	24.54%	42.38%	12.24%
1985	1,508	11.49%	3.75%	7.71%	3.78%	29.99%	47.26%	22.28%
1986	1,543	9.97%	3.24%	6.41%	3.56%	11.74%	37.66%	5.33%
1987	1,641	9.97%	3.46%	6.77%	3.20%	-4.60%	32.74%	-11.37%
1988	1,661	11.15%	3.82%	7.50%	3.65%	14.59%	41.58%	7.09%
1989	1,707	10.85%	4.30%	8.12%	2.73%	7.60%	44.24%	-0.52%
1990	1,746	11.01%	3.88%	8.05%	2.96%	4.40%	39.83%	-3.65%
1991	1,776	10.64%	4.04%	6.32%	4.32%	16.13%	46.90%	9.81%
1992	1,883	10.11%	4.37%	4.05%	6.06%	26.96%	58.02%	22.91%
1993	2,097	9.25%	3.35%	3.45%	5.80%	5.83%	37.05%	2.38%
1994	2,567	9.87%	3.11%	5.51%	4.36%	25.09%	54.05%	19.58%
1995	2,774	9.77%	3.87%	5.65%	4.12%	25.58%	57.87%	19.93%
1996	3,046	9.27%	3.28%	5.70%	3.57%	19.70%	49.88%	14.00%
1997	3,284	9.11%	3.66%	5.67%	3.44%	20.76%	53.11%	15.09%
1998	3,401	9.03%	3.29%	5.38%	3.65%	1.10%	68.40%	-4.28%
1999	3,277	9.75%	3.99%	5.07%	4.68%	19.25%	119.70%	14.18%
2000	3,006	10.51%	5.24%	6.08%	4.43%	17.76%	63.93%	11.68%
2001	2,714	9.68%	4.63%	3.72%	5.96%	4.17%	51.12%	0.45%
2002	2,606	9.11%	3.65%	2.06%	7.05%	5.54%	54.12%	3.48%
2003	2,674	9.09%	3.57%	1.09%	8.00%	38.26%	60.68%	37.17%
2004	2,842	8.40%	2.76%	2.09%	6.31%	12.31%	38.14%	10.22%
2005	2,975	8.49%	3.23%	3.45%	5.04%	16.64%	42.71%	13.19%
2006	3,092	8.53%	3.37%	5.21%	3.32%	18.35%	37.67%	13.14%
2007	3,104	8.14%	3.02%	4.91%	3.23%	-17.56%	40.70%	-22.47%
2008	3,071	10.39%	6.31%	2.36%	8.03%	-24.43%	37.57%	-26.79%
2009	2,855	10.92%	6.63%	0.56%	10.36%	27.88%	55.59%	27.32%
2010	2,991	10.32%	4.90%	0.32%	10.00%	34.18%	48.21%	33.86%

Table 2. Continued

Panel B: Summary Statistics, by 5-Year Intervals

Year	(1) **Obs**	(2) **Mean GLS**	(3) **StdDev GLS**	(4) **Implied Premium**	(5) **RF Rate**	(6) **Mean Returns**	(7) **StdDev Returns**	(8) **Ex Post Premium**
1976-1980	4,136	13.01%	4.47%	7.69%	5.32%	21.12%	34.70%	13.43%
1981-1985	6,586	12.71%	6.10%	11.78%	0.92%	25.19%	44.72%	13.41%
1986-1990	8,298	10.59%	3.74%	7.37%	3.22%	6.75%	39.21%	-0.62%
1991-1995	11,097	9.93%	3.75%	5.00%	4.93%	19.92%	50.78%	14.92%
1996-2000	16,014	9.54%	3.89%	5.58%	3.96%	15.71%	71.00%	10.13%
2001-2005	13,811	8.95%	3.57%	2.48%	6.47%	15.38%	49.35%	12.90%
2006-2010	15,113	9.66%	4.85%	2.67%	6.99%	7.68%	43.95%	5.01%
All	75,055	10.63%	4.34%	6.08%	4.54%	15.97%	47.67%	9.88%

Table 3. AR(1) Parameters

Table 3, Panel A, reports summary statistics on the expected-returns (ϕ) and GLS measurement-error (ψ) AR(1) parameters of equations (5) and (6), estimated by OLS regressions of equations (A3) and (A4) by Fama-French 48 industry. $T(\phi)$ and $R^2(\phi)$ [$T(\psi)$ and $R^2(\psi)$] are the White-robust t-statistics and R^2 from the estimation of (A4) [(A3)]. $\psi - \phi$ and $|\psi - \phi|$ are the difference and the absolute value of the difference between the GLS measurement error and expected return persistence parameters. Panel B reports the AR(1) parameter estimates for each of the Fama-French industries.

Panel A: Summary of Industry-Based AR(1) Parameters

	Exp Ret AR(1) Parameter			Meas Error AR(1) Parameter			Diff	
Statistic	ϕ	$T(\phi)$	$R^2(\phi)$	ψ	$T(\psi)$	$R^2(\psi)$	$\psi - \phi$	$\|\psi - \phi\|$
Min	0.0411	0.2295	0.0033	0.0091	0.0391	0.0001	-0.3435	0.0050
P25	0.3432	2.1741	0.1484	0.3625	1.7783	0.1687	-0.1435	0.0482
Mean	0.5296	3.8999	0.3517	0.4583	3.1198	0.2900	-0.0651	0.1241
Median	0.5609	3.3547	0.3488	0.4759	3.0334	0.2923	-0.0669	0.1017
P75	0.6993	4.6066	0.5060	0.6115	4.3487	0.4124	-0.0145	0.1824
Max	0.8828	10.9994	0.8041	0.7902	6.4917	0.6107	0.3198	0.3435
Std Dev	0.2288	2.7517	0.2372	0.2046	1.6772	0.1724	0.1496	0.0928

Panel B: Parameters by Industry

	Exp Ret AR(1) Parameter			Meas Error AR(1) Parameter			Diff	
Statistic	ϕ	$T(\phi)$	$R^2(\phi)$	ψ	$T(\psi)$	$R^2(\psi)$	$\psi - \phi$	$\|\psi - \phi\|$
Aero	0.4924	1.4347	0.2087	0.4465	1.3351	0.1742	-0.0460	0.0460
Agric	0.4711	2.2308	0.2099	0.3711	3.3389	0.2767	-0.1000	0.1000
Autos	0.5151	2.7750	0.3128	0.4329	1.8321	0.1815	-0.0822	0.0822
Banks	0.8167	7.0113	0.6573	0.6819	3.9986	0.5143	-0.1349	0.1349
Beer	0.7508	5.0177	0.6177	0.5939	2.6149	0.3526	-0.1568	0.1568
BldMt	0.5090	5.0311	0.3621	0.4769	2.4830	0.2333	-0.0321	0.0321
Books	0.7444	5.4945	0.6688	0.6006	3.9898	0.5084	-0.1438	0.1438
Boxes	0.7399	6.6686	0.6952	0.6693	5.1139	0.4931	-0.0705	0.0705
BusSv	0.4505	2.7653	0.2999	0.1070	0.4482	0.0120	-0.3435	0.3435
Chems	0.5949	4.6537	0.4504	0.4576	2.0130	0.1955	-0.1373	0.1373
Chips	0.3223	2.2446	0.1202	0.2906	1.3173	0.0744	-0.0317	0.0317
Clths	0.6328	4.3476	0.4461	0.4865	3.0449	0.2184	-0.1464	0.1464
Cnstr	0.4620	2.0148	0.2367	0.3237	2.0405	0.2055	-0.1383	0.1383
Coal	0.7656	4.5033	0.5877	0.7273	4.5135	0.5504	-0.0383	0.0383
Comps	0.3781	1.8807	0.1395	0.6979	4.4980	0.4603	0.3198	0.3198
Drugs	0.7062	4.5281	0.4996	0.5782	4.3736	0.4578	-0.1280	0.1280
ElcEq	0.1028	0.3467	0.0105	0.2784	1.2560	0.0907	0.1757	0.1757
Enrgy	0.3852	2.1174	0.1484	0.5676	4.3891	0.4124	0.1824	0.1824
FabPr	0.3432	2.3309	0.1261	0.2000	1.4481	0.0452	-0.1433	0.1433
Fin	0.5364	3.7767	0.3488	0.6508	3.1820	0.3935	0.1144	0.1144
Food	0.8191	9.8812	0.8041	0.5600	3.7785	0.2949	-0.2591	0.2591
Fun	0.6958	6.1236	0.4761	0.6289	5.0342	0.4673	-0.0669	0.0669

Table 3. Continued

Panel B (Continued): Parameters by Industry

	Exp Ret AR(1) Parameter			Meas Error AR(1) Parameter			Diff	
Statistic	ϕ	$T(\phi)$	$R^2(\phi)$	ψ	$T(\psi)$	$R^2(\psi)$	$\psi - \phi$	$\lvert\psi - \phi\rvert$
Gold	0.2417	1.3176	0.0619	0.1617	0.9536	0.0262	-0.0800	0.0800
Guns	0.7118	4.6066	0.5096	0.6753	4.3238	0.4557	-0.0365	0.0365
Hlth	0.6247	3.3483	0.3939	0.6300	3.3628	0.4000	0.0053	0.0053
Hshld	0.8828	7.3571	0.7210	0.7902	4.5180	0.5166	-0.0926	0.0926
Insur	0.4990	2.2594	0.2495	0.2249	1.3071	0.0723	-0.2741	0.2741
LabEq	0.0411	0.2295	0.0033	0.0091	0.0391	0.0001	-0.0320	0.0320
Mach	0.5561	2.9528	0.3136	0.5416	2.9643	0.3066	-0.0145	0.0145
Meals	0.5621	3.1310	0.3261	0.3805	1.7783	0.2140	-0.1816	0.1816
MedEq	0.6993	4.3783	0.5025	0.3891	2.5130	0.2037	-0.3102	0.3102
Mines	0.3395	1.4655	0.1184	0.2224	1.6248	0.1092	-0.1171	0.1171
Paper	0.6955	5.1242	0.5705	0.4237	2.0416	0.1787	-0.2717	0.2717
PerSv	0.5376	4.2492	0.2867	0.3737	2.0180	0.1679	-0.1639	0.1639
RlEst	0.3084	1.2982	0.1005	0.2589	1.2772	0.0865	-0.0495	0.0495
Rtail	0.5306	3.3547	0.2879	0.4425	2.4520	0.2276	-0.0881	0.0881
Rubbe	0.1609	0.5405	0.0213	0.3625	5.4105	0.3819	0.2016	0.2016
Ships	0.5609	4.5336	0.3134	0.5659	4.8124	0.3657	0.0050	0.0050
Smoke	0.7008	2.8130	0.5060	0.4462	1.7586	0.2806	-0.2546	0.2546
Soda	0.7843	7.8869	0.7112	0.7321	6.1354	0.5750	-0.0522	0.0522
Steel	0.5933	3.1018	0.3580	0.5280	4.2941	0.4547	-0.0653	0.0653
Telcm	0.8814	10.9994	0.7755	0.7797	4.5611	0.6107	-0.1017	0.1017
Toys	0.2426	0.7131	0.0166	0.2895	3.1156	0.0894	0.0469	0.0469
Trans	0.2845	1.2631	0.0800	0.4759	6.4917	0.4690	0.1914	0.1914
Txtls	0.6671	4.4126	0.4218	0.6223	3.8049	0.3311	-0.0447	0.0447
Util	0.7431	6.4281	0.6762	0.5424	2.7154	0.2893	-0.2007	0.2007
Whlsl	0.6088	4.5118	0.3513	0.5696	3.0334	0.3120	-0.0392	0.0392

Table 4. Sample Summary Statistics

Table 4 reports sample distributional statistics for the primary independent and dependent variables used in this study. Market Capitalization (*Size*) is (log of) the market capitalization (in $millions); Book-to-Market Multiple (*BTM*) is (log of) the ratio of book value of equity to market value of equity; Debt-to-Market Multiple (*DTM*) is (log of 1 +) the ratio of long-term debt to market capitalization; *Market Beta* is estimated for each firm on June 30 of each year by regressing the firm's stock returns on the CRSP value-weighted index using data from 10–210 trading days prior to June 30; *Standard Deviation of Daily Returns* is the standard deviation of a firm's daily stock returns using returns data from July 1 of the previous year until July 30 of the current year; *3-Month Momentum* is a firm's realized returns in the three months prior to June 30 of the year in question; *Trailing Industry ROE* is the industry median ROE using data from the most recently available ten fiscal years (as of June 30 of each year) and Fama-French industry definitions; Number of Estimates (*Analyst Coverage*) is (log of 1 +) the number of sell-side analysts covering the firm (as reported in I/B/E/S); StdDev of Estimates (*Analyst Dispersion*) is (log of 1 +) the standard deviation of analyst FY1 forecasts (as reported in I/B/E/S); *Analyst LTG* is the (gross) analyst long-term growth estimate (reported in I/B/E/S) or, for firms without such forecasts and with positive FY1 forecasts, the implied (gross) growth rate from the analyst median FY1 EPS forecast to the analyst median FY2 EPS forecast. *FY1 (FY2) [FY3] Forecast Optimism* is the difference between I/B/E/S median analyst forecasted FY1 (FY2) [FY3] per-share earnings and the projections of the mechanical forecast model; *FY1 (FY2) [FY3] Forecast Optimism / Assets* is *FY1 (FY2) [FY3] Forecast Optimism* divided by total assets per share using total assets from the most recently available data (as of June 30); *FY1 (FY2) [FY3] Forecast Optimism / Analyst StdDev* is *FY1 (FY2) [FY3] Forecast Optimism* divided by the standard deviation of analyst forecasts of FY1 EPS. *Term* is the difference between the long-run expected return and the one-quarter ahead expected return, following Lyle and Wang (2014). $\widehat{w}_{i,t}^{gls}$ is the measurement-error proxy, the primary dependent variable of interest, computed as

$$\widehat{w}_{i,t}^{gls} \equiv \left(\widehat{er}_{i,t+1}^{gls} - \widehat{\phi}_i \widehat{er}_{i,t}^{gls}\right) / \left(\widehat{\psi}_i^{gls} - \widehat{\phi}_i\right)$$

where the AR(1) parameters are estimated as described in Table 3.

Variable	5^{th} Pctile	25^{th} Pctile	Mean	Median	75^{th} Pctile	95^{th} Pctile	StdDev	N
Market Capitalization ($ Mil)	26.47	105.19	2,467.77	334.06	1,181.76	9,140.99	11,827.43	75,055
Size	3.2761	4.6557	5.9415	5.8113	7.0748	9.1205	1.7780	75,055
Book-to-Market Multiple	0.1071	0.3012	0.6616	0.5120	0.8129	1.6199	0.7790	75,055
BTM	-2.2338	-1.2001	-0.7473	-0.6695	-0.2071	0.4824	0.8644	75,055
Debt-to-Market Multiple	0.0000	0.0124	0.4654	0.1496	0.4638	1.6492	1.9763	75,055
DTM	-0.2800	-0.0763	0.0578	0.0322	0.1573	0.4651	0.2546	75,039
Market Beta	-0.0083	0.4025	0.8447	0.7746	1.2074	1.9764	0.6060	71,422
StdDev of Daily Returns	0.0119	0.0182	0.0291	0.0253	0.0358	0.0593	0.0156	75,055
3-Month Momentum	0.0000	0.0124	0.2691	0.1394	0.3810	0.9743	0.3686	75,055
Trailing Industry ROE	0.0981	0.1168	0.1270	0.1279	0.1378	0.1526	0.0171	75,055
Number of Estimates	1.0000	2.0000	6.9799	5.0000	10.0000	22.0000	6.7776	75,037
Analyst Coverage	0.0000	0.6931	1.4937	1.6094	2.3026	3.0910	0.9811	75,037
StdDev in Estimates	0.0000	0.0100	0.1006	0.0400	0.1100	0.3800	0.1954	75,055
Analyst Dispersion	0.0000	0.0100	0.0861	0.0392	0.1044	0.3221	0.1276	75,055
Analyst LTG	1.0446	1.1050	1.2995	1.1500	1.2250	1.5390	1.8850	75,055
FY1 Optimism	-1.4025	-0.2435	0.1588	0.1592	0.5403	1.6385	1.4908	73,884
FY2 Optimism	-1.5886	-0.1737	0.3191	0.3417	0.8256	2.0683	1.6173	73,884
FY3 Optimism	-3.0071	-0.2973	0.3979	0.4828	1.2166	3.1773	3.7569	73,884
FY1 Optimism / Assets	-0.0672	-0.0063	0.0128	0.0059	0.0304	0.1101	0.1875	73,835
FY2 Optimism / Assets	-0.0610	-0.0035	0.0251	0.0147	0.0490	0.1514	0.2308	73,835
FY3 Optimism / Assets	-0.1404	-0.0076	0.0315	0.0205	0.0653	0.1961	0.7334	73,835
FY1 Optimism / Analyst StdDev	-23.5343	-2.2687	4.3612	2.3362	10.1292	39.4040	29.9899	59,799
FY2 Optimism / Analyst StdDev	-25.6145	-1.0086	7.9980	4.8920	15.7399	55.0904	35.0595	59,799
FY3 Optimism / Analyst StdDev	-40.2376	-0.8458	10.7405	8.0338	23.8887	72.5512	53.2109	59,799
Terminal Optimism	-2.8945	0.6249	3.1654	2.4330	4.8766	11.5870	5.1368	73,884
Terminal Optimism / Assets	-0.1227	0.0132	0.2671	0.1102	0.2573	0.7681	2.1229	73,835
Terminal Optimism / Analyst StdDev	-26.1256	10.2174	75.8682	39.1776	100.6350	309.4016	143.9744	59,799
Term	-0.3098	-0.0019	0.0268	0.0540	0.1177	0.2462	0.2481	60,750
$\widehat{w}_{i,t}^{gls}$	-2.5627	-0.5520	-0.3444	-0.1776	-0.0366	0.5013	1.6785	62,208

Table 5. ICC Measurement Errors and Firm Risk and Growth Characteristics

Table 5 reports OLS regressions of GLS measurement-error proxy on firm characteristics. All variables are as defined in Table 4. Year and FF48 industry fixed effects are included throughout. Two-way cluster robust standard errors, clustered by FF48 industry and by year and adjusted for first-stage estimation noise, appear in parentheses immediately below the coefficient estimate. All coefficients and standard errors are multiplied by 100. Levels of significance are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

	(1)		(2)		(3)		(4)		(5)	
Size	1.6057	**	1.3286	*			0.9039		0.6252	
	(0.738)		(0.724)				(0.658)		(0.669)	
BTM	-2.4508	*	-2.3049	*			-2.3409	*	-2.2417	*
	(1.457)		(1.395)				(1.362)		(1.314)	
3-Month Momentum	-8.9592	**	-8.8014	*			-8.4182	*	-8.2024	*
	(4.464)		(4.633)				(4.314)		(4.510)	
DTM			-1.7967						-1.7186	
			(2.086)						(2.052)	
Market Beta			1.6927						1.4739	
			(1.340)						(1.302)	
StdDev of Daily Returns			-42.0994						-59.7358	
			(58.880)						(58.829)	
Trailing Industry ROE			43.8398						47.2509	
			(148.404)						(150.234)	
Analyst Coverage					3.7488	***	2.0898	**	1.9687	**
					(1.499)		(1.032)		(0.862)	
Analyst Dispersion					-7.6161	*	-5.2338		-4.7855	
					(4.438)		(3.788)		(3.427)	
Analyst LTG					2.3011	***	2.3023	***	2.2532	**
					(0.896)		(0.892)		(0.906)	
Observations	61,040		58,588		61,044		61,034		58,582	
Adj. R^2	0.8032		0.8055		0.8038		0.8045		0.8068	

Table 6. ICC Measurement Errors and Analyst Earnings Forecast Optimism

Table 6 reports OLS regressions of the GLS measurement-error proxy on various measures of analyst *FY1*, *FY2*, and *FY3 Forecast Optimism* as defined in Table 4. Year and FF48 industry fixed effects are included throughout. Two-way cluster robust standard errors, clustered by FF48 industry and by year and adjusted for first-stage estimation noise, appear immediately below the coefficient estimate in parentheses. All coefficients and standard errors are multiplied by 100. Levels of significance are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

	Unscaled Optimism			Scaled Optimism, by Assets			Scaled Optimism, by Std of Forecast		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Terminal Forecast Optimism		0.4524 ***	0.2197		3.9529 **	2.0636		0.0139 ***	0.0073
		(0.137)	(0.202)		(1.837)	(2.002)		(0.005)	(0.006)
FY1 Forecast Optimism	-0.8076		-0.6360	-28.2524		-22.5523	-0.0449		-0.0297
	(0.695)		(0.663)	(17.949)		(17.163)	(0.031)		(0.035)
FY2 Forecast Optimism	-0.9536		-1.1331	-3.4460		-8.7684	-0.0621		-0.0750
	(0.996)		(0.995)	(14.722)		(15.062)	(0.052)		(0.057)
FY3 Forecast Optimism	1.3751 ***		1.1387 **	19.3636 ***		15.6922 **	0.0814 ***		0.0685 **
	(0.493)		(0.522)	(0.068)		(6.282)	(0.030)		(0.029)
Observations	60,026	60,026	60,026	59,786	59,786	59,786	50,593	50,593	50,593
Adj. R^2	0.8048	0.8041	0.8048	0.8067	0.8060	0.8069	0.8094	0.8088	0.8094

Table 7. ICC Measurement Errors, Analyst Earnings Forecast Optimism, and Term Structure

Table 7 reports OLS regressions of the GLS measurement-error proxy on various measures of analyst *FY1*, *FY2*, *FY3*, and *Terminal Forecast Optimism* as defined in Table 4. Panel B includes *Size*, *BTM*, *3-Month Momentum*, and *Term* as controls. Year and FF48 industry fixed effects are included throughout. Two-way cluster robust standard errors, clustered by FF48 industry and by year and adjusted for first-stage estimation noise, appear immediately below the coefficient estimate in parentheses. All coefficients and standard errors are multiplied by 100. Levels of significance are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

	Unscaled Optimism			Scaled Optimism, by Assets			Scaled Optimism, by Std of Forecast		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Terminal Forecast Optimism		0.2314	0.0644		3.7920 *	1.8764		0.0084 *	0.0040
		(0.159)	(0.217)		(1.974)	(2.237)		(0.005)	(0.006)
FY1 Forecast Optimism	0.2265		0.2613	-9.4353		-6.1555	-0.0115		-0.0043
	(0.868)		(0.867)	(13.399)		(14.476)	(0.030)		(0.034)
FY2 Forecast Optimism	-2.6903		-2.7037	-19.5308		-21.1550	-0.0787		-0.0849
	(1.702)		(1.700)	(16.756)		(17.728)	(0.057)		(0.060)
FY3 Forecast Optimism	1.923 **		1.8263 **	22.3786 **		17.6782 *	0.0687 **		0.0613 **
	(0.801)		(0.873)	(9.361)		(9.106)	(0.029)		(0.028)
Size	1.0926	1.397 *	1.0721	1.2580 *	1.6800 ***	1.3566 *	1.2064 *	1.4928 **	1.2173 *
	(0.761)	(0.834)	(0.784)	(0.697)	(0.665)	(0.700)	(0.635)	(0.699)	(0.637)
BTM	-1.9595	-2.076	-1.9603	-2.0939	-1.4452	-1.7278	-2.1485	-2.1738	-2.0637
	(1.554)	(1.597)	(1.552)	(1.466)	(1.122)	(1.398)	(1.489)	(1.539)	(1.513)
3-Month Momentum	-8.3062 *	-8.7471 *	-8.3113 *	-8.4864 *	-8.6387 **	-8.3624 *	-9.1427 *	-9.4116 *	-9.1148 *
	(4.499)	(4.631)	(4.499)	(4.610)	(3.687)	(4.493)	(5.099)	(5.162)	(5.093)
Term	9.4083 **	9.3665 **	9.3959 **	8.8732 **	8.7997 **	8.8106 **	9.3856 **	9.4133 **	9.4552 **
	(4.194)	(4.209)	(4.206)	(3.911)	(3.913)	(3.901)	(4.134)	(4.175)	(4.180)
Observations	48,460	48,460	48,460	48,287	48,287	48,287	41,179	41,179	41,179
Adj. R^2	0.8068	0.8062	0.8069	0.8086	0.8081	0.8087	0.8106	0.8101	0.8106

Table 8. Cross-Sectional Sorting of Future Returns – by Year

Table 8, Panel A (B), reports average annual 12-month-ahead realized market-adjusted (size-adjusted) returns for each decile portfolio, formed annually using GLS, Fitted GLS, and Fitted Modified GLS. Fitted GLS [Modified GLS] in year t is obtained in a two-step process: (1) regress GLS [Modified GLS] on a set of firm characteristics using the previous three years' data, from $t-3$ to $t-1$, where t ranges from 1979 to 2010; (2) apply the estimated coefficients on the covariates at t. Model 1 includes three covariates: *Size*, *BTM*, and *3-Month Momentum*; Model 2 adds *DTM* and StdRet to Model 1; and Model 3 adds *Analyst Dispersion* and *Analyst LTG* to Model 2. All variables are as defined in Table 4. In each panel, row 1 reports the average annual spread in realized 12-month-ahead returns between the 10^{th} and 1^{st} deciles of expected-return proxies; row 2 reports the time-series t-statistics in the annual spread of row 1; row 3 reports the t-statistics from a t-test (Wilcoxon signed-rank test) of the null hypothesis that the average annual decile spread produced by Fitted Modified GLS deciles is equal to the average annual decile spread produced by GLS deciles; row 4 reports the t-statistics from a t-test (Wilcoxon signed-rank test) of the null hypothesis that the average annual decile spread produced by Fitted Modified GLS deciles is equal to the average annual decile spread produced by Fitted GLS deciles.

Panel A: Market-Adjusted Returns

		Model 1		*Model 2*		*Model 3*	
Decile	GLS	Fitted GLS	Fitted ModGLS	Fitted GLS	Fitted ModGLS	Fitted Fitted GLS	Fitted ModGLS
1	0.0151	0.0248	-0.0102	0.0294	-0.0043	0.0265	-0.0060
2	0.0169	0.0125	0.0068	0.0153	0.0099	0.0178	0.0165
3	0.0252	0.0250	0.0230	0.0249	0.0219	0.0205	0.0186
4	0.0262	0.0383	0.0280	0.0260	0.0206	0.0276	0.0206
5	0.0381	0.0306	0.0239	0.0345	0.0372	0.0420	0.0383
6	0.0395	0.0378	0.0349	0.0472	0.0308	0.0480	0.0337
7	0.0478	0.0457	0.0539	0.0441	0.0528	0.0422	0.0445
8	0.0606	0.0533	0.0453	0.0466	0.0421	0.0451	0.0517
9	0.0591	0.0476	0.0512	0.0532	0.0575	0.0606	0.0609
10	0.0291	0.0419	0.1014	0.0363	0.0894	0.0272	0.0792
(1) Decile 10 − 1	**0.0140**	**0.0171**	**0.1116**	**0.0068**	**0.0937**	**0.0007**	**0.0851**
(2) T-Statistic	**0.4252**	**0.4847**	**3.3692**	**0.1952**	**2.8020**	**0.0201**	**2.5754**
(3) H_0: Fitted ModGLS=GLS			3.12 (2.94)		3.16 (3.09)		2.83 (2.64)
(4) H_0: Fitted ModGLS=FittedGLS			3.31 (2.95)		3.25 (2.62)		3.03 (2.49)

Panel B: *Size*-Adjusted Returns

		Model 1		*Model 2*		*Model 3*	
Decile	GLS	Fitted GLS	Fitted ModGLS	Fitted GLS	Fitted ModGLS	Fitted GLS	Fitted ModGLS
1	0.0076	0.0211	-0.0158	0.0259	-0.0107	0.0226	-0.0127
2	0.0105	0.0070	0.0014	0.0091	0.0029	0.0111	0.0093
3	0.0187	0.0172	0.0146	0.0176	0.0149	0.0138	0.0122
4	0.0189	0.0309	0.0192	0.0158	0.0116	0.0183	0.0112
5	0.0286	0.0218	0.0137	0.0246	0.0284	0.0330	0.0290
6	0.0306	0.0277	0.0250	0.0373	0.0206	0.0377	0.0239
7	0.0345	0.0339	0.0432	0.0342	0.0417	0.0316	0.0337
8	0.0471	0.0395	0.0334	0.0325	0.0291	0.0312	0.0387
9	0.0434	0.0302	0.0343	0.0380	0.0415	0.0434	0.0458
10	0.0046	0.0156	0.0764	0.0098	0.0651	0.0020	0.0542
(1) Decile 10 − 1	**-0.0030**	**-0.0055**	**0.0923**	**-0.0161**	**0.0758**	**-0.0206**	**0.0669**
(2) T-Statistic	**-0.0951**	**-0.1693**	**3.6639**	**-0.5146**	**2.8973**	**-0.6564**	**2.4756**
(3) H_0: Fitted ModGLS=GLS			3.29 (2.97)		3.51 (3.05)		3.02 (2.75)
(4) H_0: Fitted ModGLS=FittedGLS			3.57 (3.12)		3.57 (2.99)		3.24 (2.90)

Table 9. Cross-Sectional Sorting of Future Returns – By Year and Industry

Table 9, Panel A (B), reports average annual 12-month-ahead realized market-adjusted (size-adjusted) returns for each decile portfolio, formed annually and within each FF48 industry using GLS, Fitted GLS, and Fitted Modified GLS. Fitted GLS [Modified GLS] in year t is obtained in a two-step process: (1) regress GLS [Modified GLS] on a set of firm characteristics using the previous three years' data, from $t-3$ to $t-1$, where t ranges from 1979 to 2010; (2) apply the estimated coefficients on the covariates at t. Model 1 includes three covariates: *Size*, *BTM*, and *3-Month Momentum*; Model 2 adds *DTM* and StdRet to Model 1; and Model 3 adds *Analyst Dispersion* and *Analyst LTG* to Model 2. All variables are as defined in Table 4. In each panel, row 1 reports the average annual spread in realized 12-month-ahead returns between the 10^{th} and 1^{st} deciles of expected-return proxies; row 2 reports the time-series t-statistics in the annual spread of row 1; row 3 reports the t-statistics from a t-test (Wilcoxon signed-rank test) of the null hypothesis that the average annual decile spread produced by Fitted Modified GLS deciles is equal to the average annual decile spread produced by GLS deciles; row 4 reports the t-statistics from a t-test (Wilcoxon signed-rank test) of the null hypothesis that the average annual decile spread produced by Fitted Modified GLS deciles is equal to the average annual decile spread produced by Fitted GLS deciles.

Panel A: Market-Adjusted Returns

Decile	GLS	*Model 1* Fitted GLS	*Model 1* Fitted ModGLS	*Model 2* Fitted GLS	*Model 2* Fitted ModGLS	*Model 3* Fitted GLS	*Model 3* Fitted ModGLS
1	0.0126	0.0263	0.0036	0.0308	0.0043	0.0268	0.0025
2	0.0186	0.0211	0.0137	0.0178	0.0257	0.0211	0.0249
3	0.0243	0.0200	0.0209	0.0230	0.0226	0.0226	0.0185
4	0.0233	0.0244	0.0311	0.0235	0.0268	0.0266	0.0352
5	0.0456	0.0324	0.0277	0.0280	0.0279	0.0267	0.0256
6	0.0268	0.0445	0.0329	0.0329	0.0315	0.0437	0.0323
7	0.0662	0.0492	0.0416	0.0545	0.0439	0.0470	0.0443
8	0.0576	0.0398	0.0442	0.0488	0.0433	0.0488	0.0413
9	0.0494	0.0451	0.0612	0.0551	0.0544	0.0572	0.0592
10	0.0332	0.0572	0.0888	0.0441	0.0850	0.0366	0.0827
(1) Decile $10-1$	**0.0206**	**0.0309**	**0.0852**	**0.0133**	**0.0807**	**0.0098**	**0.0802**
(2) T-Statistic	**0.8208**	**1.2115**	**3.5691**	**0.4586**	**3.1609**	**0.3451**	**3.3088**
(3) H_0: Fitted ModGLS=GLS			2.97 (2.90)		3.10 (2.97)		3.17 (2.86)
(4) H_0: Fitted ModGLS=FittedGLS			2.75 (2.69)		3.63 (3.09)		3.69 (3.25)

Panel B: *Size*-Adjusted Returns

Decile	GLS	*Model 1* Fitted GLS	*Model 1* Fitted ModGLS	*Model 2* Fitted GLS	*Model 2* Fitted ModGLS	*Model 3* Fitted GLS	*Model 3* Fitted ModGLS
1	0.0067	0.0214	-0.0018	0.0260	-0.0017	0.0218	-0.0039
2	0.0118	0.0142	0.0064	0.0105	0.0194	0.0137	0.0186
3	0.0172	0.0120	0.0119	0.0144	0.0131	0.0147	0.0095
4	0.0132	0.0166	0.0219	0.0156	0.0183	0.0186	0.0270
5	0.0363	0.0237	0.0184	0.0186	0.0181	0.0170	0.0164
6	0.0166	0.0343	0.0220	0.0235	0.0209	0.0335	0.0216
7	0.0544	0.0373	0.0307	0.0426	0.0325	0.0363	0.0327
8	0.0452	0.0269	0.0322	0.0341	0.0303	0.0346	0.0288
9	0.0340	0.0280	0.0450	0.0401	0.0379	0.0407	0.0423
10	0.0094	0.0305	0.0644	0.0179	0.0622	0.0112	0.0594
(1) Decile $10-1$	**0.0027**	**0.0091**	**0.0662**	**-0.0081**	**0.0638**	**-0.0106**	**0.0633**
(2) T-Statistic	**0.1167**	**0.3935**	**3.4726**	**-0.3195**	**3.3975**	**-0.4185**	**3.4402**
(3) H_0: Fitted ModGLS=GLS			2.84 (2.58)		3.19 (2.66)		3.11 (2.58)
(4) H_0: Fitted ModGLS=FittedGLS			3.02 (2.99)		4.03 (3.27)		3.76 (3.31)

Table 10. Expected Returns and Firm Characteristics

Table 10 reports OLS regressions of proxies of expected returns on various measures of characteristics associated with a firm's risk profile or information environment. Columns $1-4$ use GLS, Modified GLS (ModGLS), GLS formed using Mechanical Forecasts (MechGLS), and realized returns over the next 12 months (Returns) as the proxy of expected returns. Panels A, B, and C differ by the firm characteristics considered. Each explanatory variable is standardized by its annual average and standard deviation. Year and FF48 industry fixed effects are included throughout. Two-way cluster robust standard errors, clustered by FF48 industry and by year and adjusted for first-stage estimation noise, appear immediately below the coefficient estimate in parentheses. All coefficients and standard errors are multiplied by 100. Levels of significance are indicated by *, **, and *** for 10%, 5%, and 1%, respectively.

Panel A

	Expected Sign	(1) GLS		(2) ModGLS		(3) MechGLS		(4) Returns	
Size	(−)	-0.6957	***	-3.4004	***	-2.0823	***	-2.5561	***
		(0.116)		(1.288)		(0.267)		(0.978)	
BTM	(+)	1.2995	***	3.3184	***	1.7430	***	2.7889	***
		(0.161)		(1.253)		(0.191)		(1.100)	
3-Month Momentum	(+)	-0.3481	***	1.8951	*	-0.3295	***	2.2666	***
		(0.044)		(1.065)		(0.104)		(0.799)	
Observations		61,027		61,027		55,786		61,027	
Adj. R^2		0.4128		0.8046		0.2822		0.1166	

Panel B

	Expected Sign	(1) GLS		(2) ModGLS		(3) MechGLS		(4) Returns	
Size	(−)	-0.4725	***	-2.5426	**	-1.7987	***	-2.3925	***
		(0.112)		(1.213)		(0.313)		(0.955)	
BTM	(+)	1.1624	***	3.0511	**	1.5379	***	2.2843	***
		(0.160)		(1.242)		(0.184)		(0.915)	
3-Month Momentum	(+)	-0.3950	***	1.8053	*	-0.3745	***	2.2743	***
		(0.030)		(1.081)		(0.092)		(0.746)	
Market Beta	(+ or 0)	-0.1120	***	-1.2381		-0.4855	***	-0.1655	
		(0.041)		(0.867)		(0.102)		(0.757)	
DTM	(+)	0.5665	***	1.2474	*	0.5601	***	0.8990	
		(0.102)		(0.639)		(0.135)		(0.814)	
StdDev of Daily Returns	(+)	0.5445	***	1.4080	**	0.5127		0.1152	
		(0.088)		(0.695)		(0.351)		(1.343)	
Trailing Industry ROE	(0)	0.8931	***	0.2497		0.8417	***	-2.0227	
		(0.101)		(2.418)		(0.055)		(1.308)	
Observations		58,576		58,576		54,063		58,576	
Adj. R^2		0.4722		0.8069		0.3029		0.1152	

Table 10. Continued

Panel C

	Expected Sign	(1) GLS		(2) ModGLS		(3) MechGLS		(4) Returns	
Size	(−)	-0.4227	***	-1.3344		-1.6508	***	-2.6033	**
		(0.103)		(1.136)		(0.316)		(1.318)	
BTM	(+)	1.1669	***	2.9224	**	1.5467	***	2.2025	**
		(0.148)		(1.213)		(0.177)		(0.926)	
3-Month Momentum	(+)	-0.3926	***	1.6488	*	-0.3894	***	2.2865	***
		(0.033)		(0.973)		(0.092)		(0.746)	
Market Beta	(+ or 0)	-0.0981	**	-1.1466		-0.4706	***	-0.1898	
		(0.040)		(0.805)		(0.102)		(0.758)	
DTM	(+)	0.5290	***	1.1866	*	0.5615	***	0.8601	
		(0.098)		(0.622)		(0.137)		(0.815)	
StdDev of Daily Returns	(+)	0.4037	***	1.7487	**	0.5247		0.1026	
		(0.086)		(0.712)		(0.349)		(1.341)	
Trailing Industry ROE	(0)	0.8850	***	0.1867		0.8352	***	-2.0216	
		(0.103)		(2.428)		(0.056)		(1.311)	
Analyst Coverage	(−)	-0.0659		-2.0284	**	-0.1874		0.1151	
		(0.078)		(0.887)		(0.122)		(0.725)	
Analyst Dispersion	(?)	0.2070	***	0.7824	*	-0.0066		0.3719	
		(0.038)		(0.422)		(0.071)		(0.465)	
Analyst LTG	(−)	1.3463	***	-3.2639	**	-0.0285		-0.1616	
		(0.072)		(1.585)		(0.052)		(0.248)	
Observations		58,570		58,570		54,063		58,570	
Adj. R^2		0.5669		0.8079		0.3032		0.1152	