The Ratio Problem*

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Abstract

We use the term "ratio problem" to describe the omitted variable and measurement error bias that can arise when a ratio is the dependent variable in a statistical model. First, we show how bias can arise from the omission of two classes of variables based on a ratio's denominator. Second, we show how measurement error in the ratio's denominator can produce bias. We use an important finding in economics (the "inverse U" relationship between managerial ownership and Tobin's Q) as an example to show how results can be reversed when omitted variables are included. We show that this risk of bias is pervasive across disciplines, including in public health, where we demonstrate that the relationship between control measures in China and the incidence of COVID-19 is reversed when the ratio problem is addressed. We provide empirical tests and solutions to the ratio problem, and urge caution when applied researchers use ratios as dependent variables.

Keywords: Omitted Variable Bias, Measurement Error, Tobin's Q, Corporate Governance

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1 Introduction

Statistical models in a range of fields, from economics to public health, can be specified using ratios, such as $Y/n = f(\cdot)$, where Y represents an output, n is a scale factor, such as population or the replacement value of assets, and Y/n is a function of various inputs represented by $(\cdot)$, such as economic factors or responses to a pandemic. Researchers studying models with inputs and outputs often use ratios as outputs.

In this paper, we examine empirical challenges that applied researchers should consider when a researcher estimates the parameters of such a model. Specifically, we show that estimating $Y/n = f(\cdot)$ using linear regression risks obtaining biased estimates simply because Y is divided by n. In theory, the denominator is not necessarily problematic; in practice, however, there are statistical concerns whenever the output is a ratio.

We use the term "ratio problem" to describe two challenges – omitted variable and measurement error bias – that arise anytime a researcher uses linear regression with a ratio output. Intuitively, the central problem is bias that arises when a right-side input of interest is correlated with either (i) the reciprocal of the scale factor, $1/n$, or (ii) the scaled version of other right-side input variables. Such correlation can arise either because the input variable of interest is also scaled by n, or because it is not scaled, but is otherwise correlated with $1/n$ or the interaction of $1/n$ with other right-side input variables. Bias can alternatively arise when the denominator of the ratio is measured with error.

Our key insight is that a researcher examining a ratio as an output variable should test whether it is necessary to include as regressors $1/n$ and both scaled and unscaled input variables, as we discuss below. Some research has elided the ratio problem in the past by using workarounds: winsorizing, examining only size-based slices of their samples, or including fixed effects. We show precisely why these workarounds can fail, by demonstrating the general, underlying problem they purport to solve. Although many researchers have used sophisticated techniques to provide ad hoc fixes when specific omitted variable or measurement challenges arise, there is no overarching treatment in the literature of the more general problem. We provide that treatment.

We illustrate the ratio problem by focusing on the large literature in law, economics, and finance investigating whether Tobin's Q, the market value of a firm's securities scaled by their replacement values, is a function of a firm's corporate governance.[1] We also point to research on COVID-19, where correcting for the ratio problem reverses the sign of every coefficient in one prominent study finding a relationship between control measures such as transportation and entertainment restrictions and the incidence of COVID-19.

We focus on the Tobin's Q literature in corporate governance as an example for scholars in all disciplines, because it is so clear in this literature that the denominator of the outcome variable suffers from considerable measurement error (Bartlett and Partnoy, 2020), thus risking biased regression estimates (see, e.g., Erickson and Whited, 2012; Erickson and Whited, 2006). This literature also illustrates the importance of distinguishing between numerator measurement error and denominator measurement error. Numerator measurement error might merely inflate residuals and standard errors, making inference more difficult, yet not causing bias (Gompers, Ishii, and Metrick, 2009, p. 1068). But denominator measurement error is a different matter entirely. As we show, the widely-cited "inverse U" relationship between managerial ownership and Tobin's Q is reversed when we account for error in the denominator.

We contribute to the literature by comprehensively documenting the bias that can arise in any empirical estimation of a production function that models an output as a ratio. We provide empirical tests of the degree of bias and assess the conditions under which a logarithmic transformation of a ratio can avoid some of the problems posed by the use of a dependent variable that is a ratio. (Of course, whether a logarithmic specification is theoretically appropriate is a separate question.)

Concern about the use of ratios in empirical research has a haphazard pedigree, a line that winds through various disciplines for more than a century. Pearson (1897) observed that deflating two uncorrelated variables by a common deflator can result in spurious correlation between the variables. Many researchers have cited this observation as important given the widespread use of common deflators to address scale effects and heteroskedasticity. For in-

[1] Examples include Bebchuk, Cohen, and Ferrell (2008); Gompers, Ishii, and Metrick (2003); Gompers, Ishii, and Metrick (2009); Duchin, Goldberg, and Sosyura (2016); Schoar and Zuo (2017); and Fabisik et al. (2018).

stance, Neyman (1952) provocatively showed how the correlation between storks-per-women and babies-per-women could suggest that storks influence birth rates.

During subsequent decades, a handful of statisticians confronted various aspects of spurious correlation arising from deflation. Kuh and Meyer (1955), Madansky (1964), Kunreuther (1966), and Belsley (1972) explored how spurious correlation with a common deflator arises when the relationship between the deflated variable and the deflator is not linear through the origin. Casson (1973) relatedly showed that spurious correlation arises when measurement error affects the common deflator. More recently, some researchers in various disciplines have similarly addressed aspects of the statistical challenges of using a common deflator to address scale effects. For example, sociologists and political scientists have debated whether deflation is an acceptable method to control for scale effects due to the risk of spurious correlation from a common deflator (See, e.g., Firebaugh and Gibbs, 1985). Similar debates have occurred in zoology (Atchley, Gaskins, and Anderson, 1976), ecology (Beaupre and Dunham, 1995), and biology (Packard and Boardman, 1988).

Although some researchers have addressed spurious correlation arising from deflation generally, the literature has not focused on how bias from a ratio regression can arise not only when inputs are scaled with the same deflator, but also when inputs are not scaled. Kronmal (1993), while focusing primarily on the challenge of using a common deflator, briefly discussed problems that might arise when the dependent variable is the only ratio in a specification (in Section 3.1, with two examples). However, his focus was on diagnosing the problems associated with a mis-specified, deflated regression model where the theoretical production function at issue maps right-side inputs to an unscaled output variable.[2] Accordingly, Kronmal (1993) did not consider the distinct bias that can arise in the setting, common in finance and economics, when a researcher estimates the parameters of a production function where the theoretical output variable is itself a ratio that represents an important construct such as firm performance.

[2]In particular, Kronmal's analysis focused on a setting where a researcher seeks β_x in $y = \beta_0+\beta_x x+\beta_z z+\epsilon$ but does so by estimating α_x in $\frac{y}{z} = \alpha_0 + \alpha_x x + \upsilon$. As a result, Kronmal's concern was on the conditions when α_x would yield an unbiased estimate of the relationship between x and y. As we show, even when a researcher is interested in the relationship between x and $\frac{y}{z}$ (for instance, in estimating how x predicts return on assets or Tobin's Q), α_x may be biased absent the adjustments we recommend.

Unfortunately, both Kronmal (1993) and the earlier research cited above on spurious correlation arising from the use of a common deflator have largely been ignored in practice. Although some researchers have acknowledged the potential problems of using ratios as outcome variables, most have not grappled with the scope of the ratio problem that we document or how to address it. To the contrary, some researchers have cited Kronmal (1993) as the basis for avoiding ratios as dependent variables altogether, complicating the ability to compare results across prior studies (see, e.g., Knight and Rosa, 2011). Similarly, writing within the strategy literature, Wiseman (2009) noted that a regression of Y/Z on X estimates an interaction effect between X and Z (a point we discuss below), but did not examine how this specification also risks omitted variable bias. Additionally, neither Kronmal (1993) nor Wiseman (2009) examined how measurement error in Z might further complicate the use of ratios as dependent variables or whether the risk of spurious correlation persists with a log transformation of a ratio. We address each of these challenges.

In short, researchers have not described the combination of omitted variable and measurement error challenges that result when a researcher estimates the parameters of a production function that uses a ratio as an output variable. Nor have they set forth tests of the extent of coefficient bias that can result from the use of ratios in such a context. Our contribution here is to do both.

2 Omitted Variable Bias

Consider the following regression specification that estimates Tobin's Q as a function of x, a corporate governance measure of interest (such as managerial ownership or corporate governance indices):

$$\frac{MV_i}{BV_i} = \beta_0 + \beta_1 x_i + \epsilon_i \tag{1}$$

where MV/BV is a proxy for Tobin's Q, MV represents the market value of firm i, and BV is the book value of assets of firm i. Equation (1) typically includes a vector of control variables, which we omit for now without loss of generality.

We show in this section why Equation (1) omits two potentially important variables, and therefore risks bias in β_1. We obtain Equation (2) below by adding two terms to the right side of Equation (1). We use C to denote a scaled "constant" omitted variable, and ME to denote a scaled "main effect" omitted variable:

$$\frac{MV_i}{BV_i} = \beta_0 + \beta_1 x_i + \beta_c \frac{1}{BV_i} + \beta_{ME} \frac{x_i}{BV_i} + \upsilon_i \tag{2}$$

where β_c estimates the relationship between Tobin's Q and the reciprocal of BV, β_{ME} estimates a main effect for x scaled by BV, and υ is an error term, where $E[\upsilon|\, x, BV] = 0$. Note that Equation (2) includes as regressors both a scaled and unscaled variable of interest (x), along with $\frac{1}{BV_i}$.

It is standard in empirical research involving the use of a ratio as an outcome variable to omit the third and fourth terms on the right side of Equation (2). But omitting these two terms reflects two potentially important assumptions that arise whenever a ratio is a dependent variable in a linear regression. Both assumptions involve the ratio's denominator.

2.1 Two Key Assumptions about Ratios as Dependent Variables

First, omitting $\beta_c \frac{1}{BV_i}$ reflects an assumption that MV approaches zero as BV approaches zero. We call $\frac{1}{BV_i}$ a "constant" variable because including it in Equation (2) allows for MV to have a non-zero "constant" intercept value as BV approaches zero. In other words, although both Equations (1) and (2) assume a linear relationship between MV and BV, Equation (1) requires that this linear relationship between MV and BV pass through the origin, whereas Equation (2) permits the intercept to vary.

Second, omitting $\beta_{ME} \frac{x_i}{BV_i}$ reflects an assumption that MV is not related to the main effect, x, except through the interaction of x with BV. We call $\frac{x_i}{BV_i}$ a "main effect" variable because including it in Equation (2) allows for MV to be associated directly with x as a main effect, separately from the association of x and BV. In other words, although both Equations (1) and (2) estimate a linear relationship between MV/BV and x, Equation (1) requires that this relationship involve BV, whereas Equation (2) permits a relationship between MV and x, independent of BV.

The intuition supporting the above two points is apparent if we simply re-write Equation (1) as the following, mathematically identical equation:

$$MV_i = \beta_0 BV_i + \beta_1 x_i BV_i + \epsilon_i BV_i \tag{3}$$

Note that Equation (3) is missing a constant term. Accordingly, it reflects an assumption that the linear relationship between MV and BV passes through the origin. Unlike Equation (1), and equivalently Equation (3), Equation (2) allows for the possibility that the linear relationship between MV and BV has a non-zero intercept. (Recall that the omitted constant variable in Equation (1) was a scaled constant; the omitted variable in Equation (3) is simply a constant, unscaled.)

Note further that Equation (3) is missing a main effect term for x. Accordingly, it reflects an assumption that MV is not related to x except through the interaction of x with BV. Unlike Equation (1), and equivalently Equation (3), Equation (2) allows for the possibility that MV is related to x apart from the relationship between x and BV. (Recall that the omitted variable in Equation (1) was a scaled main effect; the omitted variable in Equation (3) is simply a main effect, unscaled.)

Are the assumptions in Equation (1) reasonable? Or does omitting the "constant" and "main effect" variables lead to bias in β_1? We answer this question formally below. But first we want to illustrate the potential seriousness of the ratio problem with an example, to show how omitting the constant and main effect terms matters to an important result in the finance literature.

2.2 An Example: Managerial Ownership and Tobin's Q

Consider the well-known finding of a non-linear relationship between managerial ownership and Tobin's Q, first established in Morck, Shleifer, and Vishny (1988) and confirmed by McConnell and Servaes (1990). The "inverse U" relationship – Tobin's Q rises as the percentage of the company's equity owned by management increases, but then declines after a peak at roughly 40% – has been widely replicated and cited. It is now standard to include both managerial ownership and its square as control variables in a wide range of studies.

The "inverse U" papers use a version of Equation (1), with both managerial ownership and managerial ownership squared as variables of interest. We present the standard regression specification in Equation (4), where Own and Own^2 represent the percentage of outstanding shares owned by managers and that percentage squared:

$$\frac{MV_i}{BV_i} = \beta_0 + \beta_1 Own_i + \beta_2 Own_i^2 + \epsilon_i \tag{4}$$

We replicate the "inverse U" findings using Equation (4) and updated data. In particular, we estimate Equation (4) for all firms appearing in Standard and Poor's Compustat database from 2010 to 2018 where a firm can be matched with managerial ownership data from Execucomp.[3] Table 1 presents the results.

Table 1

	(1) $\frac{MV}{BV}$
Own	0.0199***
	(0.00374)
Own^2	-0.000260**
	(0.0000960)
Constant	1.830***
	(0.0139)
Observations	15,672

Robust standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The positive coefficient on Own and the negative coefficient on Own^2 are consistent with past findings of an "inverse U" relationship, as is shown in the plot of the fitted values in Figure 1. In unreported results, windsorizing managerial ownership at 1% and 99% within the data produces identically signed coefficient estimates for Own and Own^2. The relationship between managerial ownership and Tobin's Q might be linear and positively sloped at low percentage ownership levels, but the influential finding in the literature is the inflection point

[3]As is typical in this literature, we use Compustat data to estimate Tobin's Q as the market value of assets divided by the book value of assets, where the market value of assets is computed as the book value of assets plus the market value of common stock less the sum of the book value of common stock and balance sheet deferred taxes (see, e.g.,Gompers, Ishii, and Metrick, 2009).

at higher ownership percentages, causing both management ownership and its square to be widely used as regressors in empirical finance (see, e.g., Bebchuk, Cohen, and Ferrell, 2008).[4]

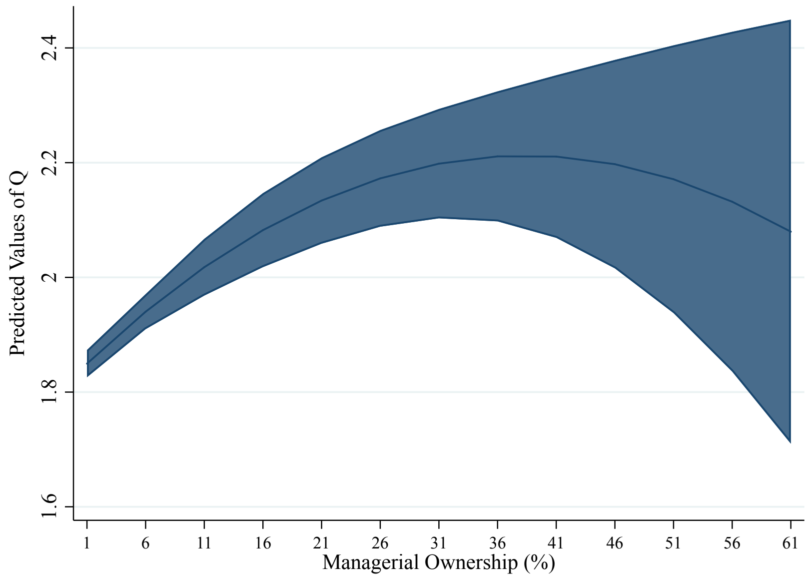

Figure 1: Predicted Values of Q As a Function of Managerial Ownership - Equation (4). Shaded region represents 95% confidence intervals.

A very different picture emerges if we add the omitted "constant" and "main effect" terms to Equation (4). Note that Equation (4) omits $\beta_c \frac{1}{BV}$, and instead assumes that the linear relationship between MV and BV has a zero intercept. Equation (4) also omits two main effect terms, and instead assumes that MV is not associated with Own and Own^2, except through an interaction with BV. Equation (5) adds these terms:

$$\frac{MV_i}{BV_i} = \beta_0 + \beta_1 Own_i + \beta_2 Own_i^2 + \beta_c \frac{1}{BV_i} + \beta_{ME1} \frac{Own_i}{BV_i} + \beta_{ME2} \frac{Own_i^2}{BV_i} + \epsilon_i \tag{5}$$

Using the same data as in Table 1, Column (2) of Table 2 presents coefficient estimates based on Equation (5). The contrast to the previous estimates from Table 1, set forth here in Column (1), is stark. Note that in Column (2), the coefficient estimates for both $\frac{1}{BV_i}$ and $\frac{Own_i^2}{BV_i}$ are positive, while the coefficient estimate for $\frac{Own_i}{BV_i}$ is negative. Moreover, the

[4]The original papers finding an "inverse U" relationship did not incorporate firm fixed effects (as is common in more contemporary corporate governance studies). We discuss the use of firm fixed effects below in note 7.

coefficient estimate for Own^2, the variable driving the "inverse U" result in Column (1), is smaller in Column (2).

Table 2

	(1) $\frac{MV}{BV}$	(2) $\frac{MV}{BV}$
Own	0.0199***	0.0165***
	(0.00374)	(0.00398)
Own^2	-0.000260**	-0.000162
	(0.0000960)	(0.000104)
$\frac{1}{BV}$		83.01***
		(4.757)
$\frac{Own}{BV}$		-5.632***
		(0.878)
$\frac{Own^2}{BV}$		0.0931***
		(0.0237)
Constant	1.830***	1.759***
	(0.0139)	(0.0144)
Observations	15,672	15,672

Robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In Figure 2, we use the new estimates in Column (2) to re-plot the fitted values of Tobin's Q against management ownership. The "inverse U" disappears.

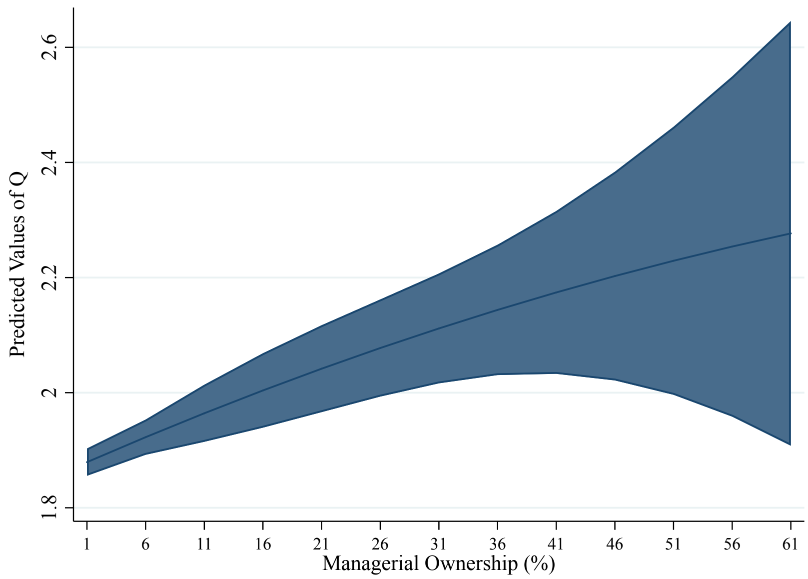

Figure 2: Predicted Values of Q As a Function of Managerial Ownership - Equation (5). Shaded region represents 95% confidence intervals.

We demonstrate formally below why Equation (4) produces biased estimates for Own and Own^2. Intuitively, the omitted variables added to Equation (5) reduce the statistical bias in Equation (4) in two ways. First, adding the "constant" term reduces the coefficient bias that arises from the assumption that MV approaches zero as BV approaches zero. In Equation (5), MV is permitted to be non-zero as BV approaches zero, and the coefficient for $\frac{1}{BV_i}$ indicates that MV indeed is positive as BV nears zero. In contrast, because Equation (4) omits this "constant" term, its coefficient estimates for Own and Own^2 reflect the extent to which those variables are correlated with $\frac{1}{BV_i}$. That correlation is a source of bias.

Second, adding the "main effect" variables (both first-order and squared terms) reduces the coefficient bias that arises from the assumption that MV can be related to Own and Own^2 only through the interaction of BV with those variables. In Equation (5), MV is permitted to be related directly to Own and Own^2, and the coefficients for $\frac{Own_i}{BV_i}$ and $\frac{Own_i^2}{BV_i}$ indeed reflect MV being negatively associated with Own and positively associated with Own^2 (without any interaction with BV). In contrast, because Equation (4) omits the "main effect" terms, its coefficient estimates for Own and Own^2 reflect the extent to which those variables are correlated with $\frac{Own_i}{BV_i}$ and $\frac{Own_i^2}{BV_i}$, respectively. That correlation also is a source of bias.

In sum, the net impact of the omitted variable bias in this example is as follows. β_1, the coefficient for Own in Equation (4), is biased upward. The coefficient for Own declines when the omitted variables are included, in Equation (5), and the coefficient for $\frac{Own_i}{BV_i}$ is large and negative. In contrast, β_2, the coefficient for Own^2 in Equation (4), is biased downward when the variables are omitted. Most strikingly, when the omitted variables are included, in Equation (5), the coefficient for the second-order "main effect" variable, $\frac{Own_i^2}{BV_i}$, not only is large, but has the opposite sign (positive) as the negative coefficient for Own^2 in Equation (4).

As we show in Section 4 below, when we use a logarithmic transformation, the "inverse U" becomes an "actual U." Putting aside the question of whether a logarithmic transformation is theoretically appropriate, we show empirically that the ratio problem with respect to this example is so serious that, when the omitted variables are included, the well-documented relationship between managerial ownership and firm value turns out to be the opposite of the one that researchers have widely assumed.

Of course, omitting the "constant" and "main effect" variables does not always bias the coefficients in a regression that uses a ratio as the dependent variable. Moreover, even if omitting these variables leads to bias, the bias may not be economically meaningful. We turn next to this question: when is it reasonable for a researcher to omit these variables?

2.3 Tests for Omitted Variable Bias

We now present tests to assess when it is reasonable to omit the "constant" and "main effect" variables from a regression with a ratio as the dependent variable. We begin by examining the conditions under which omitting these variables does not lead to any bias. Second, we describe a test for the magnitude of bias. Finally, we consider a more general matrix approach. We conclude this section with an illustration of our general matrix test based on the example of the relationship between managerial ownership and Tobin's Q.

2.3.1 A Test for Zero Bias

Omitted variable bias depends on how omitted variables affect the error term in Equation (1). Obtaining an unbiased estimate for β_1 in Equation (1) requires cov(x,ϵ) = 0.

Combining Equations (1) and (2), we observe that $\epsilon_i = \beta_c \frac{1}{BV_i} + \beta_{ME} \frac{x_i}{BV_i} + \upsilon_i$. Accordingly, in order to obtain an unbiased estimate for β_1 in Equation (1), the following must be true:[5]

$$\begin{aligned} cov(x, \epsilon) &= 0 \\ cov(x, \beta_c \frac{1}{BV} + \beta_{ME} \frac{x}{BV} + \upsilon) &= 0 \\ \beta_c cov(x, \frac{1}{BV}) + \beta_{ME} cov(x, \frac{x}{BV}) &= 0 \end{aligned} \tag{6}$$

Thus, Equation (1) provides an unbiased estimate for β_1 only when Equation (6) holds, meaning that one of the following must be true:

$$\beta_c = 0 \text{ and } \beta_{ME} = 0, \text{ or} \tag{6a}$$

$$\beta_c cov(x, \frac{1}{BV}) = -\beta_{ME} cov(x, \frac{x}{BV}), \text{ or} \tag{6b}$$

$$cov(x, \frac{1}{BV}) = 0 \tag{6c}$$

Equation (6) highlights the narrow set of conditions when omitting the "constant" and "main effect" variables poses no risk of bias in estimating for β_1 in Equation (1). Although the circumstances under which condition (6b) is likely to hold are not intuitive, there may be theoretical and empirical reasons to believe that either condition (6a) or (6c) apply in a particular research setting. For instance, x and $\frac{1}{BV}$ may simply be orthogonal. Likewise, there are settings where it is plausible that both β_c and β_{ME} equal zero.

With regard to the latter, recall that Equation (1) assumes the relationship between the numerator and the denominator of the ratio is a linear one that goes through the origin; in other words, the assumption is that $\beta_c = 0$. Many ratios, such as percentages and rates, are

[5] In deriving Equation (6), note that $cov(x, \upsilon) = 0$ given the assumption that $E[\upsilon|x, BV] = 0$. More precisely, $cov(x, \upsilon) = E[x\upsilon] - E[x]E[\upsilon]$. Focusing on the second half of this equation and using the law of iterated expectations, $E[\upsilon] = E_x[E_\upsilon[\upsilon|x]]$. Because $[\upsilon|x, BV] = 0$, it is also true that $[\upsilon|x] = 0$ (Wooldridge, 2010, p. 18); therefore, $E_x[E_\upsilon[\upsilon|x]] = 0$. Likewise, by the law of iterated expectations, $E[x\upsilon]$ is equal to $E_x[E_{x\upsilon}[x\upsilon|x]]$, which is equivalent to $[E_x[xE_\upsilon[\upsilon|x]]$. Because the inner term is zero, $[E_x[xE_\upsilon[\upsilon|x]] = 0$.

likely to satisfy the first part of condition (6a), because the numerator will approach zero as the denominator approaches zero. Moreover, the second part of condition (6a) is satisfied if $\beta_{ME} = 0$, which holds when x has no association with the numerator that is independent from the denominator.

Consider an experiment that examines the percentage of questions answered correctly by students who were treated with a tutorial program relative to students who were not. When a student answers zero questions, it is obviously impossible for the student to have any correct answers (meaning $\beta_c = 0$). It is also impossible for the tutorial treatment to affect the number of correct answers that is separate from the number of questions that a treated student answers; the treatment effect, if any, must work through its interaction with the number of questions answered (meaning $\beta_{ME} = 0$). Accordingly, condition 6(a) would hold.

However, condition (6a) is unlikely to be satisfied in many other contexts. For instance, $\beta_c \neq 0$ is common in economics. Tobin's Q is an obvious example: as Bartlett and Partnoy (2020) describe, firms often have positive MV even when BV is near zero. Other examples of ratios whose numerator is often not zero as the denominator approaches zero are return on assets, debt-to-equity, and working capital (current assets/current liabilities). Firms often have positive net income but near-zero book value of assets, debt with zero (or negative) book value of equity, or positive current assets but zero or near-zero current liabilities. Moreover, economic and financial ratios commonly describe abstract concepts such as firm performance, profitability, or credit risk, where there is no clear mathematical relationship between the numerator and denominator, linear or not.

Likewise, $\beta_{ME} \neq 0$ can hold even if $\beta_c = 0$. Consider a study examining the effect of the size of a city's police force on the number of homicides per capita. When the population is zero, so too will be the size of the police force, suggesting $\beta_c = 0$. However, β_{ME} might be non-zero. For instance, a change in the size of a city's police force could affect the number of homicides, even after controlling for city population (see, e.g., Marvell and Moody, 1996).[6]

[6] We discuss in Section 4 why using a log transformation of a ratio also solves some of the above problems.

Our point here is to set forth a formal test for demonstrating that the zero bias conditions for ratios as dependent variables are satisfied. The straightforward solution: use Equation (6) to test whether these conditions hold.

2.3.2 A Test for the Magnitude of Bias

Second, we use the standard omitted variables framework to show how a researcher can test the economic magnitude of any bias that results from omitting $\beta_c \frac{1}{BV_i}$ or $\beta_{ME} \frac{x_i}{BV_i}$. In the case of a bivariate regression such as Equation (1), β_1 can be estimated by the following:

$$\hat{\beta}_1 = \frac{cov(x, \frac{MV}{BV})}{var(x)} \tag{7}$$

where $\hat{\beta}_1$ represents the estimate of β_1 in Equation (1).

Assuming that conditions (6a), (6b), and (6c) of the preceding test do not hold, we know that Equation (2) should be used to estimate β_1, which we now denote as β_1^T for its "true" value. To calculate the bias in $\hat{\beta}_1$, we substitute the right-side of Equation (2) for $\frac{MV}{BV}$ in Equation (7):

$$\begin{aligned}\hat{\beta}_1 &= \frac{cov(x, \beta_0 + \beta_1^T x + \beta_c \frac{1}{BV} + \beta_{ME} \frac{x}{BV} + v)}{var(x)} \\ &= \beta_1^T + \beta_c \left(\frac{cov(x, \frac{1}{BV})}{var(x)} \right) + \beta_{ME} \left(\frac{cov(x, \frac{x}{BV})}{var(x)} \right)\end{aligned} \tag{8}$$

According to Equation (8), the bias in $\hat{\beta}_1$ will equal the sum of the second and third terms above: a "constant" bias term plus a "main effect" bias term. The "constant" bias term is equivalent to a linear projection of $\frac{1}{BV_i}$ on x, multiplied by the coefficient β_c in Equation (2). Likewise, the "main effect" term is equivalent to a linear projection of $\frac{x}{BV_i}$ on x, multiplied by the coefficient β_{ME} in Equation (2).

In other words, the magnitude of the bias arising from the omission of the "constant" and "main effect" variables depends on the linear relationship between each of these variables and x, scaled by the size of their coefficients in Equation (2). Note that the net bias depends on the sign of each term: same signs magnify the bias; opposite signs offset.[7]

We now have both a test and an intuitive explanation of the degree of bias that arises from the use of a ratio as a dependent variable. Bias is greatest where: (a) both $\frac{1}{BV_i}$ and $\frac{x}{BV_i}$ are correlated with the dependent variable, (b) x is correlated with both $\frac{1}{BV_i}$ and $\frac{x}{BV_i}$, and (c) the resulting "constant" term and "main effect" terms have the same sign.

2.3.3 A More General Matrix Approach

Finally, we generalize from the above bivariate framework so that we can test for bias when there is more than one regressor of interest, as with the example of the relationship among Tobin's Q and both managerial ownership and its square. Equation (9) describes this more general version of Equation (1):

$$Y = X\beta + \upsilon \tag{9}$$

[7] This same approach also illustrates why using the common fixed effects estimator does not necessarily eliminate bias in $\hat{\beta}_1$. Consider, for instance, Equation (1) with the addition of firm fixed effects:

$$\frac{MV_{it}}{BV_{it}} = \beta_0 + \beta_i + \beta_1 x_{it} + \epsilon_{it} \tag{F1}$$

where β_i represents a firm-specific intercept. As is well known, we can obtained an estimate of β_1 in the fixed effects model through de-meaning the variables in Equation (F1):

$$\begin{aligned} \frac{MV_{it}}{BV_{it}} - \frac{\overline{MV}_i}{BV_i} &= (\beta_0 - \bar{\beta}_0) + (\beta_i - \bar{\beta}_i) + \beta_1(x_{it} - \bar{x}_i) + (\epsilon_{it} - \bar{\epsilon}_i) \\ \frac{M\ddot{V}_{it}}{BV_{it}} &= \beta_1 \ddot{x}_{it} + \ddot{\epsilon}_{it} \end{aligned} \tag{F2}$$

where $\frac{M\ddot{V}_{it}}{BV_{it}}$, $\ddot{x}_{it}$, and $\ddot{\epsilon}_{it}$ represent de-meaned variables for each firm i. Similarly, adding firm fixed effects to Equation (2) and de-meaning variables within firm yields:

$$\frac{M\ddot{V}_{it}}{BV_{it}} = \beta_1^T \ddot{x}_{it} + \beta_c \frac{\ddot{1}}{BV_{it}} + \beta_{ME} \frac{\ddot{x}_{it}}{BV_{it}} + \ddot{\upsilon}_{it} \tag{F3}$$

where β_1^T is the true, unbiased estimate for β_1 in Equation (F1). Applying the same procedure utilized in Equations (7) and (8), the bias in $\hat{\beta}_1$ is thus equal to:

$$\begin{aligned} \hat{\beta}_1 &= \frac{cov(\ddot{x}_{it}, \beta_1^T \ddot{x}_{it} + \beta_c \frac{\ddot{1}}{BV_{it}} + \beta_{ME} \frac{\ddot{x}_{it}}{BV_{it}} + \ddot{\upsilon}_{it})}{var(\ddot{x}_{it})} \\ &= \beta_1^T + \beta_c \left(\frac{cov(\ddot{x}_{it}, \frac{\ddot{1}}{BV_{it}})}{var(\ddot{x}_{it})} \right) + \beta_{ME} \left(\frac{cov(\ddot{x}_{it}, \frac{\ddot{x}_{it}}{BV_{it}})}{var(\ddot{x}_{it})} \right) \end{aligned} \tag{F4}$$

In short, as in Equation (8), the bias in $\hat{\beta}_1$ will likewise equal the sum of a "constant" bias term plus a "main effect" bias term.

where Y represents a vector of outcomes, X represents a vector of regressors (including a constant) that omits $\frac{1}{BV_i}$ and $\frac{x}{BV_i}$, and β represents a vector of coefficient estimates for the variables in X.

In such a generalized setting, the multivariate analog to Equation (2) is Equation (10):

$$Y = X\beta + Z\gamma + \upsilon \tag{10}$$

where Z represents an additional vector of omitted variables and γ represents a vector of coefficient estimates for the variables in Z. For present purposes, we confine Z to be a vector of variables that includes $\frac{1}{BV_i}$ and $\frac{x}{BV_i}$. We assume $E[\upsilon|X, Z] = 0$.

We can estimate the vector β in Equation (9) as $\hat{\beta} = (X'X)^{-1}X'Y$. To estimate the bias that arises from the omission of Z from Equation (9), we substitute the right-side of Equation (10) for Y and denote β^T as the "true" β from Equation (10), so that:

$$\begin{aligned} \hat{\beta} &= (X'X)^{-1}X'(X\beta^T + Z\gamma + \upsilon) \\ &= \beta^T + (X'X)^{-1}X'Z\gamma \end{aligned} \tag{11}$$

Accordingly, the vector $\hat{\beta}$ will be biased in the amount of $(X'X)^{-1}X'Z\gamma$.[8] We can now test the amount of bias by calculating this amount for each coefficient estimate in $\hat{\beta}$.

Assume that δ is a $K \times M$ matrix where (i) K represents the number of included variables in X and (ii) M represents the vector of coefficients for the k-th included variable from all linear projections of the m omitted variables in Z on the full set of included regressors in X. Recall from Equation (10) that γ denotes the $(M \times 1)$ vector of coefficients associated with the omitted variables in Z. Therefore, the bias in the coefficient of the k-th included regressor is:

$$Bias_k = \gamma_1\delta_{k,1} + \gamma_2\delta_{k,2} + ... + \gamma_M\delta_{k,M} = \sum_{j=1}^{M} \gamma_m\delta_{k,m} \tag{12}$$

[8] That $(X'X)^{-1}X'\upsilon = 0$ follows from the assumption that $E[\upsilon|X, Z] = 0$.

Finally, we apply this generalized test in Equation (12) to the "inverse U" example, in which the regression specification omits both a "constant" variable, $\frac{1}{BV_i}$, and two "main effect" variables, $\frac{Own}{BV_i}$ and $\frac{Own^2}{BV_i}$. Recall that including these three variables significantly reduced the coefficient estimate for Own^2, the variable responsible for the "inverse U" relationship. In our generalized framework, these three variables are the vector of variables in Z, and the coefficient estimates for these variables are γ_1, γ_2, and γ_3, respectively, in the vector γ.

In Table 3, we present the coefficient estimates from the linear projections of each variable in Z on the full set of regressors in X, which includes Own and Own^2.

Table 3

	(1) $\frac{1}{BV}$	(2) $\frac{Own}{BV}$	(3) $\frac{Own^2}{BV}$
Own	0.000238***	0.00350***	0.0379***
	(0.0000113)	(0.000213)	(0.00681)
Own^2	-0.00000401***	-0.00000968	0.00191***
	(0.000000293)	(0.00000550)	(0.000176)
Observations	16,778	16,778	16,778

Robust standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

We then use these data to construct δ, as shown in Table 4.

Table 4

	(1) m_1	(2) m_2	(3) m_3
k_1=Own	0.000238	0.00350	0.0379
k_2=Own^2	-0.00000401	-0.00000968	0.00191

Note that the coefficients in Table 4 are relatively small. Accordingly, the bias should be small so long the coefficients for γ_1, γ_2, and γ_3 are small. However, recall from Table 2 that γ_1 (the coefficient estimate for $\frac{1}{BV_i}$, or β_c) was estimated to be approximately 83.0. Consequently, the upward bias in Own is driven primarily by $\gamma_1\delta_{1,1}$ and the downward bias in Own^2 is driven by $\gamma_1\delta_{2,1}$. The full estimates for bias using the test in Equation (12) are as follows:

$$BiasOwn = 0.0002383(83.009) + 0.0035015(-5.63) + 0.0379096(0.093) \approx 0.0035$$
$$BiasOwn^2 = -0.00000401(83.009) - 0.00000968(-5.63) + 0.0019109(0.093) \approx -0.0001$$

Accordingly, a researcher who used the above tests to calculate bias would see immediately that a regression examining the relationship between managerial ownership and Tobin's Q should include the omitted "constant" and "main effect" variables in order to avoid significant coefficient bias. The source of the bias is primarily the failure to include the "constant" term, $\beta_c\frac{1}{BV_i}$.

3 Measurement Error

Next we turn to measurement error. Measurement error presents special problems in a linear regression that uses a mismeasured ratio as the dependent variable. Indeed, as we show below, measurement error in a ratio that is a dependent variable poses a kind of omitted variable problem, but one that is thornier than the omitted variable problem discussed above.

Our central insight in approaching measurement error in this context is to distinguish sharply between error in the numerator and error in the denominator of a ratio that is used as an outcome variable. Error in the numerator can be addressed by standard techniques. Error in the denominator cannot.

Moreover, the problems associated with measurement error in the denominator of a ratio that is used as a dependent variable apply even if the omitted variable problems discussed in Section 2 do not. Accordingly, measurement error in the denominator of a ratio that is used as a dependent variable merits special attention.

We are not the first to address the special problems of measurement error implicated by the use of ratios. For instance, a large literature within the Q-theory of investment examines the bias that can arise because a mismeasured proxy for Q is used as a regressor (see, e.g., Erickson and Whited, 2012; Erickson and Whited, 2006). Researchers, however, have not examined the bias that can arise when these same proxies for Q are used as outcome variables, as is common in corporate governance research, on the assumption that measurement error in an outcome variable will not bias any slope coefficients (see, e.g., Gompers, Ishii, and Metrick, 2009). Likewise, those researchers who have examined the problem of a mismeasured ratio that is an outcome variable have focused on settings where a researcher deflates all variables in a regression by a common deflator that is measured with error. For instance, Briggs (1962) examines how measurement error in a common deflating variable relates to the problem of spurious correlation between variables that share the common deflator. Casson (1973) similarly examines how measurement error in a common deflator might lead to inconsistent estimates.

Our approach is more general. We illustrate why the nature of bias that results from measurement error in a ratio that is used as an outcome variable—and relatedly, the proper mode for addressing it—differs depending on whether the measurement error appears in the numerator or the denominator. Although empirical researchers often address potential bias arising from measurement error in the numerator of a ratio that is an outcome variable, we are unaware of any papers that address the distinctive bias arising from measurement error in the denominator.[9]

3.1 Measurement Error in the Numerator of a Ratio

When measurement error affects the numerator of a ratio that is an outcome variable, one can adopt the standard approach to addressing measurement error in an outcome variable, which assumes that measurement error is additive. Under this approach, a researcher concerned that measurement error is correlated with a regressor of interest can address this concern by finding an acceptable proxy for this error and adding it as a control.

[9]In their innovative paper examining measurement error in Tobin's Q, Erickson and Whited (2006) likewise examine how measurement error in the numerator of Q may pose a quantitatively different risk of bias than measurement error in its denominator. However, their research focused on the use of Q in empirical tests of the Q-theory of investment, in which a proxy for Q is used as a regressor, implicating the risk that measurement error in the proxy will bias coefficient estimates towards zero. In contrast, we examine the bias that arises from measurement error in the numerator and denominator of Q when it is used as a dependent variable – a research setting that Erickson and Whited do not investigate given that they assume that "measurement error [in a dependent variable] does not bias any slope coefficients" (Erickson and Whited, 2006, p. 28).

To illustrate, a researcher considering the effect of x on MV/BV, where MV is measured with error, μ, might estimate the relationship between x and observable MV^*/BV, where $MV = MV^* + \mu$, as follows:

$$\begin{aligned}\frac{MV_i^* + \mu_i}{BV_i} &= \beta_0 + \beta_1 x_i + \upsilon_i \\ \frac{MV_i^*}{BV_i} &= \beta_0 + \beta_1 x_i + \upsilon_i - \frac{\mu_i}{BV_i}\end{aligned} \tag{13}$$

Is the additive measurement error $(\upsilon_i - \frac{\mu_i}{BV_i})$ in Equation (13) a concern? The answer is typically no. Any additive measurement error will lead to bias in β_1 only if x is correlated with $\frac{\mu_i}{BV_i}$. A researcher can correct any such bias by including as a regressor an observable proxy for μ (or $\mu^{'}$), scaled by BV, as in Equation (14):

$$\frac{MV_i^*}{BV_i} = \beta_0 + \beta_1 x_i + \beta_2 \frac{\mu_i^{'}}{BV_i} + \upsilon_i \tag{14}$$

where $E[\upsilon|\, x, BV, \mu^{'}] = 0$

We can estimate the bias in β_1 that arises from omitting $\beta_2 \frac{\mu_i^{'}}{BV_i}$ using the same omitted variables technique we used in Section 2: assume $\hat{\beta}_1$ is the estimate of β_1 in Equation (14) that lacks an estimate for $\beta_2 \frac{\mu_i^{'}}{BV_i}$ (i.e., assume a simple bivariate regression of $\frac{MV_i^*}{BV_i}$ on x) and then substitute the full right side of Equation (14) for $\frac{MV_i^*}{BV_i}$. Denoting β_1^T as the "true" estimate for β_1 in Equation (14) we find that:

$$\begin{aligned}\hat{\beta}_1 &= \frac{cov\left(x, \beta_0 + \beta_1^T x + \beta_2 \frac{\mu^{'}}{BV} + \upsilon\right)}{var\,(x)} \\ &= \beta_1^T + \beta_2 \frac{cov\left(x, \frac{\mu^{'}}{BV}\right)}{var\,(x)}\end{aligned} \tag{15}$$

As Equation (15) shows, additive measurement error simply requires the standard "correction vector" for a single omitted variable, as in Greene (2008). The estimate $\hat{\beta}_1$ must be reduced by the coefficient for β_2 in Equation (14) multiplied by the linear projection of $\frac{\mu^{'}}{BV}$ on x. Adding $\frac{\mu_i^{'}}{BV_i}$ as a covariate in Equation (14) eliminates bias in β_1 when x is correlated with $\frac{\mu^{'}}{BV}$.

Measurement error in the numerator of a dependent variable ratio can be addressed using the above approach if the denominator is measured precisely, or is simply a constant, because measurement error is additive. However, different problems arise when there is measurement error in the denominator.

3.2 Measurement Error in the Denominator of a Ratio

We illustrate the challenges posed by measurement error in the denominator of a ratio that is used as an outcome variable by returning to Tobin's Q. We use MV/BV as an illustrative example for clarity, but the same analysis applies equally to any ratio.

As Bartlett and Partnoy (2020) describe, Tobin's Q, as conceived in Tobin (1969), had the replacement value of assets, RV, in its denominator. The original rationale for using RV was powerful and intuitive: investment arose in Tobin's macroeconomic model when the market value of a firm's assets was greater than their replacement value. However, because RV was difficult and costly to observe, researchers began shifting in the 1980s from attempting to estimate RV directly to using BV as a rough approximation. Researchers also began to use this proxy for Tobin's Q as an outcome variable for studying the effect of corporate governance on firm value. Indeed, Gompers, Ishii, and Metrick (2009) call Q the "workhorse" of large-sample valuation studies and follow the convention of estimating it as MV/BV.

The shift from RV to BV in the denominator of Tobin's Q was problematic because book value is a poor measure of replacement value. For example, book value does not include many intangible assets. Additionally, the accounting assumptions that are associated with book value, including the requirement to record many assets at cost, cause book value to diverge from replacement value. Most problematically, the reasons why BV can differ from RV are generally endogenous to a firm; Bartlett and Partnoy (2019) demonstrate that measurement error in using BV rather than RV is correlated with common regressors used in corporate governance research.

When using the market-to-book proxy for Q as an outcome variable, researchers have attempted to correct for any bias arising from measurement error in BV by turning to the standard approach discussed in Section 3.1. For example, Wernerfelt and Montgomery (1988,

p. 247) use a variation of Equation (1) and note that the proxy for Tobin's Q "leaves intangible assets out of the denominator, thus overstating the relative performance of firms with large investments in intangibles." As a "partial correction," they include in their regression specifications a control for "estimates of a firm's current marketing and RD expenditures, divided by the replacement cost of physical assets." Such a control variable would be appropriate if the measurement error were in the numerator of the proxy for Tobin's Q.

Unfortunately, the standard approach does not work when measurement error is in the denominator. The contrast between approaches is apparent when we substitute BV measured with error for RV in the denominator of Tobin's Q. We start with a specification that resembles Equation (1), but with RV, the replacement value of a firm's assets, in the denominator.

$$\frac{MV_i}{RV_i} = \beta_0 + \beta_1 x_i + \upsilon_i \tag{16}$$

We assume that Equation (16) poses none of the omitted variable biases discussed in Section 2. For example, suppose from that framework that $\beta_c = 0$ and $\beta_{ME} = 0$. The problems we are about to describe arise exclusively from measurement error in the denominator alone.

Assume BV measures RV with error μ, such that $RV = BV + \mu$. Substituting for RV in Equation (16), we obtain:

$$\begin{aligned} \frac{MV_i}{BV_i + \mu_i} &= \beta_0 + \beta_1 x_i + \upsilon_i \\ \frac{MV_i}{BV_i} &= \beta_0 + \beta_0 \frac{\mu_i}{BV_i} + \beta_1 x_i + \beta_1 \frac{x_i \mu_i}{BV_i} + \upsilon_i + \upsilon_i \frac{\mu_i}{BV_i} \end{aligned} \tag{17}$$

Note the complications that arise when a researcher is concerned that measurement error in the denominator is correlated with a regressor of interest, as in Wernerfelt and Montgomery (1988). Obtaining an unbiased estimate for β_1 requires including in the model not only a proxy for $\frac{\mu_i}{BV_i}$ but also for $\frac{x_i \mu}{BV_i}$. With this additional term, β_0 estimates both the intercept and the coefficient for the proxy for $\frac{\mu_i}{BV_i}$. Likewise, β_1 estimates both the coefficient for x_i and for the proxy for $\frac{x_i \mu_i}{BV_i}$. Finally, the error term is now $\upsilon_i + \upsilon_i \frac{\mu_i}{BV_i}$.

These complications arise because, as suggested by Equation (3), β_0 in Equation (16) estimates the linear relationship between MV and RV, and the coefficient β_1 estimates the linear relationship between MV and the interaction of x and RV. By substituting $BV_i + \mu_i$ for RV and then using MV/BV as our outcome variable, recovering an unbiased estimate for β_0 and β_1 requires including the terms $\frac{\mu_i}{BV_i}$ and $\frac{x_i \mu_i}{BV_i}$ whenever μ is correlated with x.

We can calculate the bias in $\hat{\beta}_1$ when $\frac{\mu_i}{BV_i}$ and $\frac{x_i \mu_i}{BV_i}$ are omitted from Equation (16). As above, we define β_1^T to be the true coefficient for β_1 if these variables were included; β_1^T will also be the true estimate for β_1 in Equation (16) if a researcher could observe RV. Assuming $E[\upsilon | x, BV, \mu] = 0$, the bias in the estimate of $\hat{\beta}_1$ would therefore be as follows:

$$\begin{aligned}\hat{\beta}_1 &= \frac{cov\left(x, \beta_0 + \beta_0 \frac{\mu}{BV} + \beta_1^T x + \beta_1^T \frac{x\mu}{BV} + \upsilon + \upsilon \frac{\mu}{BV}\right)}{var\left(x\right)} \\ &= \beta_0 \frac{cov\left(x, \frac{\mu}{BV}\right)}{var(x)} + \beta_1^T \left(1 + \frac{cov\left(x, \frac{x\mu}{BV}\right)}{var(x)}\right) \end{aligned} \tag{18}$$

The correction vector for $\hat{\beta}_1$ is thus a function of the covariance of x with two omitted variables, $\frac{\mu}{BV}$ and $\frac{x\mu}{BV}$, rather than simply one, $\frac{\mu}{BV}$.[10]

In short, measurement error in the denominator of a ratio that is an outcome variable requires adjustments that are more challenging than simply adding a single right-hand side proxy for this measurement error. Given the complexity of measurement error in this context, we next investigate the logarithmic transformation as an alternative.

4 Logarithmic Transformations of Ratios

Researchers commonly use logarithmic transformations when ratios are dependent variables, citing outliers as one justification. For example, financial ratios such as the market-to-book proxy for Tobin's Q often have skewed distributions because many firms have very low measures of book value (Erickson and Whited, 2012). Accordingly, it is common for researchers to use the natural log of MV/BV (see, e.g., Gompers, Ishii, and Metrick, 2009).

[10] As in note 4, because $E[\upsilon | x, BV, \mu] = 0$, the law of iterated expectations dictates that $cov(x, \upsilon) = 0$ and $cov(x, \upsilon \frac{\mu}{BV}) = 0$.

Equation (19) is a representative specification in the finance and economics literature; it is the same as Equation (1), except that the dependent variable is expressed in logarithmic terms:

$$\ln\left(\frac{MV_i}{BV_i}\right) = \beta_0 + \beta_1 x_i + \epsilon_i \tag{19}$$

Equation (19) does more than simply address non-normality. It also avoids many of the problems discussed in Sections 2 and 3. The reason is obvious: the ratio $\ln(MV/BV)$ is mathematically equivalent to $\ln(MV)$– $\ln(BV)$, and Equation (19) is therefore equivalent to the following:

$$\ln(MV) = \beta_0 + \beta_1 x_i + \ln(BV) + \epsilon_i \tag{20}$$

Note that Equations (19) and (20) avoid some, but not all, of the problems posed by Equation (1). First, they avoid the "main effect" problems. Recall that in Equation (1) MV was assumed not to be related to x except when interacted with BV. In Equations (19) and (20), $\ln(MV)$ is assumed to be related to x independent of any interaction with $\ln(BV)$. As a result, β_1 is a true main effect coefficient, and the omitted variable problem described in Section 2 disappears.

Note that Equations (19) and (20) also do not assume a linear relationship between $\ln(MV)$ and $\ln(BV)$ passing through the origin. Therefore, they also avoid the "constant" problem associated with Equation (1). The additive nature of the logarithmic specification avoids some of the ratio problem.

Finally, Equations (19) and (20) avoid the denominator measurement error problem discussed in Section 3. Instead, they present only the standard measurement error problems associated with a non-ratio dependent variable (or, equivalently, the numerator of a ratio).[11]

However, Equations (19) and (20) also impose a new and problematic assumption: that there is a one-to-one linear relationship between $\ln(MV)$ and $\ln(BV)$. Although researchers often overlook this important assumption, it is apparent from Equation (20), where the

[11]As in Section 3, assume that $RV = BV + \mu$, where μ represents measurement error in using BV as a proxy for RV. MV/RV is therefore equivalent to $MV/(BV + \mu)$. If we further define ϕ to be $BV/(BV + \mu)$,

coefficient of $\ln(BV)$ is fixed at 1.0. In other words, Equations (19) and (20) assume that the elasticity of MV with respect to BV is exactly one. We suspect most researchers would find it surprising if a 1% increase in a firm's book value always corresponded to an expected 1% increase in market value. In any event, it is possible to test this assumption empirically, and also to calculate the extent to which this assumption creates bias.

We test this assumption and examine its implications with an example, returning to the "inverse U" relationship between managerial ownership and Tobin's Q. First, we replicate the results of McConnell and Servaes (1990) using the same updated data from Table 1, but with a logarithmic transformation of the dependent variable. Equation (21) describes the specification.

$$\ln\left(\frac{MV_i}{BV_i}\right) = \beta_0 + \beta_1 Own_i + \beta_2 Own_i^2 + \epsilon_i \tag{21}$$

Table 5 presents the coefficient estimates.

Table 5

	(1) $\ln\left(\frac{MV}{BV}\right)$
Own	0.00652***
	(0.00138)
Own^2	-0.0000609
	(0.0000353)
Constant	0.462***
	(0.00513)
Observations	15,672

Robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

MV/RV would also be equivalent to $\phi(MV/BV)$. Thus, measurement error affects Equation (19) as follows:

$$\ln\left(\phi_i \frac{MV_i}{BV_i}\right) = \beta_0 + \beta_1 x_i + \epsilon_i$$
$$\ln\left(\frac{MV_i}{BV_i}\right) = \beta_0 + \beta_1 x_i + \epsilon_i - \ln(\phi)$$

In short, measurement error is additive and poses the ordinary challenge of classical measurement error. That is, if ϕ is correlated with x, adding a right-hand side regressor to proxy for measurement error can address any bias in β_1.

Note that the positive coefficient for Own and the negative coefficient for Own^2 remain consistent with the original finding of an "inverse U" relationship between managerial ownership and Tobin's Q. Figure 3 illustrates this conclusion by presenting a plot of the fitted values from these regression estimates against firm-year levels of ownership.

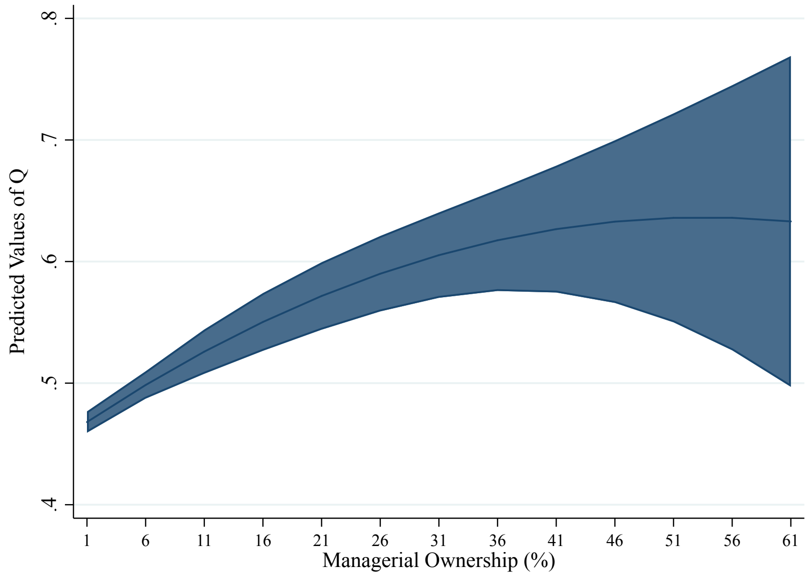

Figure 3: Predicted Values of Ln(Q) As a Function of Managerial Ownership (Elasticity = 1.0). Shaded region represents 95% confidence intervals.

Now we relax the assumption that the elasticity of MV with respect to BV must be one. In Equation (22) we allow the elasticity to depart from 1.0 by estimating it directly:

$$\ln\left(\frac{MV_i}{BV_i}\right) = \beta_0 + \beta_1 Own_i + \beta_2 Own_i^2 + \beta_3 \ln(BV_i) + \epsilon_i \tag{22}$$

Note that Equation (22) is mathematically equivalent to a specification with $\ln(MV)$ alone as the dependent variable and a coefficient for $\ln(BV_i)$ of $(1 + \beta_3)$. Either specification will generate the same coefficient estimates for Own and Own^2.

Table 6 shows how dramatically the results have changed.

Table 6

	(1) $\ln\left(\frac{MV}{BV}\right)$	(2) $\ln\left(\frac{MV}{BV}\right)$
Own	0.00652***	-0.00806***
	(0.00138)	(0.00140)
Own^2	-0.0000609	0.000200***
	(0.0000353)	(0.0000350)
Ln(BV)		-0.0799***
		(0.00236)
Constant	0.462***	1.135***
	(0.00513)	(0.0205)
Observations	15,672	15,672

Robust standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

As Column (2) shows, the coefficients of Own and Own^2 have changed signs. Figure 4 plots the fitted values against Own.

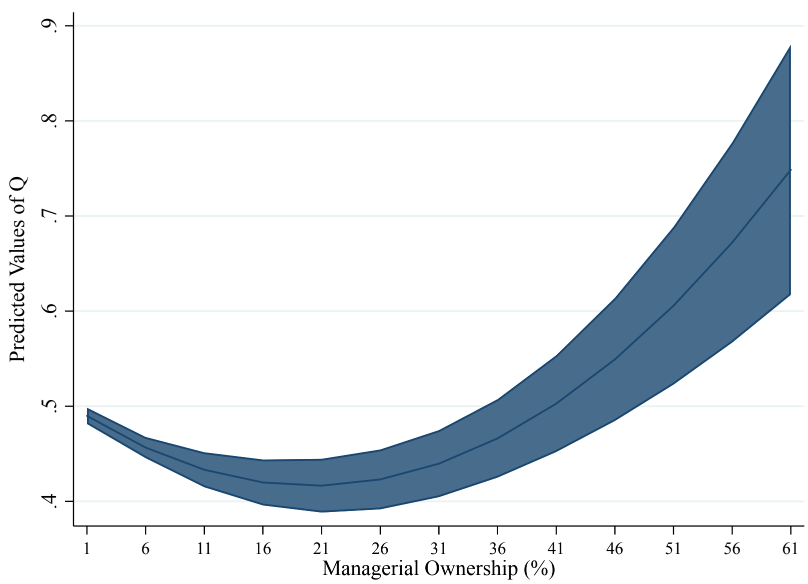

Figure 4: Predicted Values of Ln(Q) As a Function of Managerial Ownership (Elasticity Estimated). Shaded region represents 95% confidence intervals.

The relationship between $\ln(MV/BV)$ and managerial ownership is now an "actual U," not an "inverse U." (When we re-estimate the coefficients in Column (2) of Table 6 using

$\ln(MV)$ as the dependent variable, we confirm that the coefficients and standard errors for Own and Own^2 are unchanged, as expected, and that the coefficient for $\ln(BV)$ is 0.92, or -0.08+1.)

What accounts for this different result? The culprit is in the third line of Table 6, where the coefficient estimate of $\ln(BV)$ is -0.08 (or, equivalently, the elasticity of MV to BV is approximately 0.92; obviously, 0.92 is not 1.0). In this example, the elasticity assumption of 1.0 in Equation (19) was wrong.

The problem here, as in our earlier discussion, is that Own is strongly negatively associated with $\ln(BV)$ while Own^2 is strongly positively associated with $\ln(BV)$. It is the logarithmic variant of the above denominator problem. As a result of the associations between the variables of interest and the denominator of the ratio, the coefficient estimate for Own in Column (1) is biased upward, whereas the coefficient estimate for Own^2 is biased downward. As noted above, given the prevailing assumptions about the relationship between managerial ownership and Tobin's Q, this particular instance of the ratio problem has echoed throughout the literature. We suspect that this example is the tip of an iceberg of statistical bias in the economics literature, arising from the correlation of variables of interest with the denominators of ratios that are used as dependent variables in linear regressions.

Indeed, we close by referencing our findings, in a separate paper, that the results of an important COVID-19 paper are reversed when we address the ratio problem. In their 2020 study published in *Science*, Tian et al. (2020) found that the suspension of intracity public transport and closure of entertainment venues in China were associated with overall containment of COVID-19 cases, averting hundreds of thousands of cases during the first 50 days of the epidemic. Their study exploited the variation across cities in the adoption and timing of these control measures, which allowed them to study the effect of their adoption and timing on COVID-19 cases. Notably, the variable of interest in this study was the natural log of a ratio: the number of COVID-19 cases reported by each city during the first week of the epidemic, scaled by the product of a city's population and the number of individuals in the city arriving from Wuhan (in millions) between January 11 to January 23.

Tian et al. assess transmission control measures cross-sectionally using a log-linear regression and conclude that control measures are associated with a striking reduction in

COVID-19 cases, which is one reason the study has been so widely cited and influential. The study also finds timing is important: cities that acted more quickly had significantly fewer cases. However, given their regression framework, this findings rest on an assumption that both the elasticity of COVID-19 cases to a city's population and to a city's Wuhan inflows are exactly 1.0.

Using the data and code from Tian et al., we replicate their log-linear regression results identically, as shown in columns (1) in Table 7. In column (2), we relax the 1.0 elasticity assumptions by adding the natural log of population and Wuhan inflow as covariates. This modified framework estimates the elasticities of COVID-19 cases to population and Wuhan inflow rather than assuming each is 1.0.

The impact of relaxing the 1.0 elasticity assumptions is dramatic. As column (2) of Table 1 shows, the coefficient estimates are reversed *for every covariate*. Moreover, AIC and R^2 indicate significantly improved model fit. Based on these data, the timing of restrictions on intracity transit and closing entertainment venues had the opposite association: rapid implementation was associated with more COVID-19 cases, not fewer. Column (2) additionally highlights the central reason why these results are so different: the estimated coefficients for the natural log of population and Wuhan inflow are 0.484 and 0.079, respectively. (For log-linear estimates one must add 1.0 to each estimate from Table 7.) As we illustrate in our other paper, the failure of the 1.0 elasticity assumptions is critical because a city's adoption of COVID-19 control measures was correlated with both its size and its Wuhan inflows, thus biasing the estimates found by Tian et al.

Table 7

	(1) Tian Original	(2) Tien Corrected
Arrival Time	0.271***	-0.0640*
	(0.0782)	(0.0252)
Suspension of intracity public transport		
Implementation	-12.71**	6.997***
	(4.625)	(1.445)
Timing	0.463**	-0.237***
	(0.171)	(0.0532)
Closure of entertainment venues		
Implementation	-3.411**	0.802*
	(1.181)	(0.362)
Timing	1.510***	-0.286*
	(0.433)	(0.134)
Log(population)		-0.516***
		(0.0680)
Log(Wuhan inflow)		-0.921***
		(0.0169)
Constant	-1.176	3.182***
	(1.930)	(0.642)
Observations	296	296

All variables are defined in Tian et al. (2020). *Arrival Time* is the arrival time (in days) from the date of the first case in the first infected city (Wuhan) to the date of the first case in each newly infected city. *Suspension of intracity public transport - Implementation* is whether a city suspended inter-city bus service; *Suspension of intracity public transport - Timing* is the number of days with which a city suspended inter-city bus service; *Closure of entertainment venues - Implementation* is whether a city closed entertainment venues; *Closure of entertainment venues - Timing* is the speed with which a city suspended inter-city train service; *Log(population)* is the log of a city's population; and *Log(Wuhan inflow)* is the log of the number of phones tracked as moving from Wuhan to each city between January 23 and February 15. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In sum, researchers who want to use a logarithmic-transformation of a ratio as an outcome variable should include a control for the log of the denominator. Failure to do so will impose an assumption that the elasticity of the numerator with respect to the denominator is exactly

1.0. If this elasticity is not 1.0 and a covariate is correlated with the denominator, the coefficient estimates may be biased, potentially severely.

5 Conclusion

In some contexts, the use of a ratio as an outcome variable poses little risk of omitted variable bias or measurement error. For instance, a researcher might have good reason to believe that both β_c and β_{ME} are zero, and might likewise believe that the denominator of the ratio is not measured with error. Some types of ratios, including percentages, might satisfy these conditions. And of course, because the covariance of a constant with a variable is zero, dividing by a constant will also be safe (i.e., it necessarily satisfies condition (6c) discussed in Section 2.3.1).

But many ratios are problematic. In epidemiology and economics, for example, it is typical that $\beta_c \neq 0$ and $\beta_{ME} \neq 0$. Even with rates and percentages, it can be difficult to rule out the possibility that measurement error affects the denominator.

Bartlett and Partnoy (2020) address some concerns about ratios by focusing on the misuse of Tobin's Q as an outcome variable representing firm value. In this paper, we extend this work formally. We demonstrate that the previous critique was not merely historical or theoretical. Our overall message is one of caution: researchers using statistical techniques should scrutinize with care, and suspicion, regressions with ratios as dependent variables.

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