More on the Performance of Higher Order Moment Estimators in Investment Equations*

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Abstract

Almeida, Campello, and Galvao (2010) [ACG] use Monte Carlo simulations and real data to assess the performance of estimators that deal with measurement errors in investment models. ACG are the first to provide an independent assessment of alternative methods, showing when they work properly and discussing the assumptions embedded in them. Erickson and Whited (2010) review ACG's study focusing exclusively on tests involving the Erickson and Whited (2000, 2002) [EW] estimator. While casting doubt on the usefulness of the ACG analysis, Erickson and Whited (2010) develop a number of ex-post fixes for the problems uncovered by ACG. The authors argue that the ACG tests would place the EW estimator in a more positive light had they used those fixes. This paper evaluates the new fixes proposed by Erickson and Whited and clarifies their implications for the debate about measurement error. The analysis provides further support for ACG's main conclusion: the presence of measurement error does not justify the use of the EW estimator in lieu of more robust, simpler alternatives.

JEL Classification Numbers: G31, C23.
Keywords: Investment equations, measurement error, Monte Carlo simulations, instrumental variables, GMM.

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1 Introduction

Data mismeasurement is a problem affecting statistical inference in every applied science. In economics and finance, the problem assumes special characteristics as empiricists often use proxies to assess the effects of variables they cannot observe. Under those circumstances, it is hard to argue that a particular variable is measured without errors, or even suggest that one proxy is less mismeasured than another. Researchers have to make difficult choices when dealing with real data and a debate on data mismeasurement should have the ultimate goal of helping empiricists with those choices.

A recent paper by Almeida, Campello, and Galvao (2010) compares a number of alternative estimators to assess their usefulness and reliability in dealing with measurement errors. Prior to their study, the choice of estimation procedure alone led to different conclusions when researchers examined similar issues (e.g., the sensitivity of firm investment to cash flow). The ACG analysis is extensive and shows that under special circumstances the estimator proposed by Erickson and Whited (2000, 2002) performs as well as other estimators, such as the popular Arellano and Bond (1991) estimator. The ACG study also shows, however, that the performance of the EW estimator drops dramatically when one encounters patterns that arise in real-world panel data, such as fixed effects and error heteroskedasticity. The performance of other estimators, in contrast, are less affected by those data issues. ACG further show that the EW estimator is very sensitive to distributional assumptions involving higher-order moments of the measurement error process.

Erickson and Whited (2010) post a strong-worded response to the ACG study. The authors ignore the constructive debate proposed by ACG and instead provide a one-sided defense of the EW estimator. Their strategy is two-fold. On one front, they alter and reinterpret the tests of the EW estimator that are performed by ACG. At every turn, Erickson and Whited claim to have found errors and inconsistencies in the ACG study, labeling the ACG conclusions as incorrect. On the other front, Erickson and Whited propose new patches for those same problems uncovered by ACG. They argue that the performance of the EW estimator would be substantially improved in the ACG study if that study used these new, ex-post developed patches.

The ACG study points to problems afflicting each of the estimators used in the literature. Regarding the EW estimator, ACG conclude that it is too specialized to be of broad interest. In short, the EW estimator is cumbersome and unreliable. Erickson and Whited (2010) propose additional procedures and tests aiming at enhancing the performance of the EW estimator. This, however, misses the point of the ACG study. Adding more steps and assumptions to the EW estimator does not make it a more practical method. The new procedures and tests proposed by Erickson and Whited would represent a contribution for the specialized econometrician if they made the EW estimator more reliable. In this paper, we show that this is not the case. Those additional tests and steps do not strengthen the case for the EW approach.

Let us illustrate how Erickson and Whited (2010) proceed in their critique of the ACG study (the technical details are provided below). The authors argue that much of the poor performance of the EW estimator in the ACG analysis is explained by the fact that ACG use "poor starting values" when implementing the EW estimator. Erickson and Whited recognize that ACG use the correct starting values most of the time (i.e., in most of the yearly cross-sections that comprise a panel dataset), but because ACG might have not used it in a few cross-sections, the EW estimation went haywire. Erickson and Whited's arguments about the importance of starting values and their proposed fixes are new to the literature on the EW estimator. They are an ex-post rationalization of a problem uncovered by the ACG analysis — the study the authors try to discredit.[1] Even if warranted, the new argument about starting values does not make the EW method more appealing. At a basic level, the need to optimize starting values only increases the burden on the researcher who is considering whether to use the EW estimator. Most troubling, what does it say about the usefulness and reliability of an estimator when its performance hinges entirely on "optimal choices of starting values"?

We show that Erickson and Whited's (2010) starting value argument is flawed and inconsistent. First, the starting value problem affects only a subset of the EW estimators. In particular, as recognized by Erickson and Whited, the discussion of good and bad starting values does not apply to the EW-GMM3 estimator. Yet ACG show that the EW-GMM3 estimator is also problematic in dealing with measurement errors. Second, as one can see from Erickson and Whited's (2010) own results, changing initial conditions alone does not help the EW-GMM4 and EW-GMM5 estimators in situations where identification is likely to fail (e.g., in the absence of high skewness).[2] Third, if starting value choices were a key problem, one would expect to see those choices affecting estimation outcomes

[1] The initialization approach used by ACG follows exactly Erickson and Whited (2000, 2002). Skepticism about the conclusions in ACG should extend to previous papers employing the EW estimator.

[2] Table 5 in Erickson and Whited shows that EW estimates for mismeasured regressors remain biased even after the authors work with "better" starting values.

even under high data skewness (the ideal setting for the EW method). However, ACG show that in these cases the GMM estimator always performs well, irrespective of starting values. Fourth, it is easy to demonstrate that the usefulness of the new procedures suggested by Erickson and Whited (the construction of "trimmed GMM estimates" using "Gini intervals") depend on the measurement error process itself. Critically, moderate degrees of mismeasurement can render these procedures useless. In particular, as we discuss below, the "reverse regressions" that are required to construct Gini intervals yield extremely large intervals if mismeasurement is sufficiently high, rendering the procedure ineffective. These intervals also become very large when the value of the coefficient of the mismeasured variable is close to zero, again making the procedure ineffective. These are crucial problems for model estimation since the measurement error process is unobservable and can be severe in corporate data. Our results show that discussions about starting values affecting the implementation of the EW estimator are a distraction away from the central issue of model identification.

The original EW estimator was designed for cross-sectional data and can be inconsistent in the presence of fixed effects that arise in panel data. A potential remedy to this problem is to difference the data in order to eliminate fixed effect components (cf. Riddick and Whited (2009)). However, as shown in ACG and confirmed by Erickson and Whited's (2010) own results, when one treats the data for fixed effects one may reduce skewness, harming identification under the EW framework.[3] The performance of the EW estimator is sensitive to the presence of fixed effects, and its usefulness ultimately hinges on whether individual-specific effects are prevalent in the data.

Most researchers would be hard-pressed to dismiss the importance of fixed effects in corporate finance data. Nevertheless, Erickson and Whited (2010) argue that fixed effects are not an important issue when estimating empirical investment equations. They do this by criticizing the standard Hausman test used by ACG (also used in Erickson and Whited (2000) and Whited (2001)) on the basis that it may not distinguish between fixed effects in the measurement error and fixed effects in the regression error. The authors propose a new test for fixed effects that relies on the EW estimator. As we explain below, there are two important difficulties with this new test. First, the test confounds lack of skewness with the presence of fixed effects. As a result, it has poor finite-sample statistical properties, such as extremely high "test size." Second, the implementation of their test is challenging

[3] In Table 6 of Erickson and Whited (2010) the within transformation reduces the degree of skewness, biasing estimates of mismeasured regressors under the EW method (biases in EW-GMM4 and EW-GMM5 exceed 30%).

in panel data due to the aggregation problem embedded in its design. To wit, the usual Hausman test for fixed effects compares coefficients from two estimations (with and without treating fixed effects) performed over the entire panel. The procedure produces a single test statistic that the researcher can use following standard inference theory. The proposed Erickson and Whited (2010) test, in contrast, is performed period-by-period, producing statistics for every individual cross-section of the data. After performing those tests, researchers must then use *ad hoc* rules in determining whether fixed effects are sufficiently relevant in their data — inferences are no longer rooted in standard theory.

As we show below, one can mitigate the problems associated with the Erickson and Whited (2010) test and, at the same time, isolate fixed effects in the error term from fixed effects in the mismeasured regressor. To do this, one can use the Hausman test to compare the estimates produced from the OLS-IV procedure in ACG with and without fixed effects adjustments. Using the original COMPUSTAT sample from ACG and a sample constructed following Erickson and Whited, we perform this measurement-error consistent Hausman test and find that the null of fixed effects irrelevance is overwhelmingly rejected. Our tests simply confirm ordinary belief that fixed effects matter in corporate data. They also stress that researchers should be cautious about estimators that have a difficult time handling fixed effects in panel data.

Another example of Erickson and Whited's (2010) unbalanced approach is the review of the heteroskedasticity issue highlighted by the ACG analysis. Erickson and Whited argue that the way ACG introduce heteroskedasticity in the data also reduces R^2. The authors argue that this latter effect is a key contributor to the poor performance of the EW estimator in the ACG study. Unfortunately, in the real world, the two effects go hand-in-hand and it is easy to see that the artifice used by Erickson and Whited — where heteroskedasticity is disjoint of changes in R^2 — has little practical value. The ACG heteroskedasticity model follows established econometric practice. Importantly, ACG show that in contrast to the poor performance of the EW estimator, the OLS-IV estimator performs well under high degrees of heteroskedasticity, even if the variance of the estimator increases (i.e., even if R^2 drops dramatically). Erickson and Whited (2010) largely focus on the merits of diagnostic tests for heteroskedasticity, instead of discussing the consequences of the problem. The upshot of Erickson and Whited's re-examination of the ACG work is the conclusion that heteroskedasticity is a problem the EW estimator cannot easily handle.

Erickson and Whited (2010) also claim that some of the skewed distributions examined by ACG (namely, the F and the Chi-square distributions) are "equivalent to a Normal distribution." The authors argue that ACG mislead readers by overstating the differences between these distributions and the standard Normal. Erickson and Whited's arguments are inaccurate. In their analysis, ACG simply compare the very skewed distribution proposed by Erickson and Whited (2000, 2002) — the Lognormal distribution — with alternative, less skewed distributions. As shown in ACG, the performance of the EW estimator is sensitive to the degree of skewness in these distributions. Erickson and Whited, however, take issue with this constructive observation. While they are unable to formally dispute it, they take the route of redrawing the ACG distributions so as to make them "look like" Normal distributions (see their Figure 1).

The way Erickson and Whited make the skewed distributions in ACG look like Normal distributions involves shifting and re-scaling graphs with the purpose of changing their visual appearance. Notably, their "modified Normals" are not used in the ACG study. Our analysis clarifies the distraction created by the "graphing exercises" in Erickson and Whited (2010). More formally, we reconstruct the artificial Normal distributions in Erickson and Whited's paper and test their properties against the distributions they mean to mimic from the ACG paper. Kolmogorov-Smirnov distributional tests and Jarque-Bera tests of skewness overwhelmingly reject the null hypothesis that the F, Chi-square, and Lognormal distributions of ACG are similar to those modified Normals. Erickson and Whited's arguments are a distraction around ACG's point that the EW estimator is sensitive to departures from extremely high skewness, while other estimators are insensitive to such departures.

The ACG study adheres to the standard literature when performing empirical tests. For example, when examining the sensitivity of investment to cash flow, ACG follow the current literature in their approach to sample selection and variable construction. Erickson and Whited (2010) conjecture that the performance of the EW estimator is improved in their analysis, relative to ACG, because they use a "superior, less mismeasured" proxy for q (the mismeasured regressor in investment equations). It is straightforward to show that their argument is flawed.

First, as we demonstrate below, Erickson and Whited's preferred proxy has measurement problems of its own. As such, it is not unambiguously better than alternative proxies, such as the standard market-to-book ratio (used by, among others, Kaplan and Zingales (1997) and Rauh (2006)). Second, given that it is impossible to show theoretically that one proxy is better than another, the key issue is whether inferences under a particular estimator are robust to the use of alternative proxies. In our data analysis, we show that the OLS-IV estimator produces reasonable estimates for all of the proxies that we consider (including the proxy proposed by Erickson and Whited). The EW estimator, in contrast, performs poorly even when we use the proxy preferred by Erickson and Whited. In particular, the year-by-year coefficients on q that are produced under the EW estimator continue to be unstable and unreliable, similarly to evidence in presented in ACG. When we combine these coefficients using the Fama-MacBeth procedure of Riddick and Whited (2009), we obtain q coefficients that are economically nonsensical and statistically insignificant. We conclude that the improved performance of the EW estimator in Erickson and Whited (2010) has nothing to do with the proxy for q, but is rather explained by their use of a new minimum distance procedure to combine the year-by-year coefficients into a single estimate. Unfortunately, since Erickson and Whited do not share their data and programs, we are unable to investigate this possibility in more depth.[4]

ACG also follow the literature when comparing alternative methodologies. Erickson and Whited (2002) provide the foundations of the EW estimator. Accordingly, ACG use the simulation settings proposed in Erickson and Whited (2002) to compare the EW estimator with those proposed by Arellano and Bond (1991), Blundell et al. (1992), and Biorn (2000). Erickson and Whited (2010) now label the Erickson and Whited (2002) setting as "data irrelevant" as a way to criticize ACG. While seemingly appealing, this is a moot argument. The simulations carried out in ACG are *not* specifically designed to be "COMPUSTAT relevant," but instead designed to reveal and compare root differences among estimators. This is common practice in econometrics. The idea behind using simulations is that of maintaining the simplest setup so that the reader can identify the source of potential problems and differences as the experiment is changed. We demonstrate the fragility of Erickson and Whited's argument by simulating data whose moments match an arbitrary benchmark, yet make it impossible to identify the simplest OLS-IV model. Matching data moments *per se* is an irrelevant exercise for an informed debate about the properties of robust estimators.

Finally, notice that Erickson and Whited (2010) present no evidence refuting ACG's conclusion that OLS-IV estimators perform well in investment equations. Rather, in their data analysis section the authors say "We limit our efforts to the EW estimators because their OLS-IV analysis is not

[4] In an independent study, Agca and Mozumdar (2010) use the original Erickson and Whited (2000) sample (with corrections and extensions) and find that the proxy used for q in Erickson and Whited produces negative values in a significant number of cases. They also find that the Erickson and Whited identification tests fail in the majority of cases analyzed. Agca and Mozumdar, too, cast doubt on the practical use of the EW methodology and on Erickson and Whited's conclusions about the insignificance of cash flow in investment equations.

problematic". Erickson and Whited's new Monte Carlo results confirm ACG's conclusion that the OLS-IV estimators deliver unbiased estimates on average (see their Table 6). Thus, even if one accepts all their patches and ex-post fixes at face value, the best that Erickson and Whited achieve is a performance that mirrors that of the OLS-IV estimators in ACG. Since the setting adopted by Erickson and Whited (2010) is specifically designed to favor the EW estimator, it is hard to see how their new results would convince researchers to use the EW estimator in lieu of more robust, economically intuitive alternatives.

In all, one should find it disconcerting that Erickson and Whited (2010) respond disproportionately to a study they should find useful for their own work. The authors go to great lengths to criticize ACG, and in the process cast doubt on their own research agenda. As an example, the main contribution of the Erickson and Whited (2000) paper is to show that investment is not sensitive to cash flow once one accounts for measurement errors in q. In the process of trying to address the weaknesses of the EW estimator, Erickson and Whited (2010) now find that investment *is* sensitive to cash flow.[5] These contradictory findings happen despite the fact that the authors use the same data sources and methods in their two studies. ACG's main goal was exactly that of promoting a better understanding of the strengths and weaknesses of methods such as the EW estimator.

The next section studies the ex-post patches proposed by Erickson and Whited (2010). It also clarifies a number of misconceptions contained in their analysis. Section 3 offers concluding remarks.

2 Patching Up the EW Estimator

This section examines the claim that ex-post fixes to the original EW estimator would have put that estimator in a more positive light in the ACG study. At first blush, these fixes simply add more complexity to a cumbersome estimator. More importantly, ACG have already revealed hidden weaknesses in the original EW estimator. It is logical that these fixes be put under the microscope as well. We do this in this section. The section also discuss other features of the Erickson and Whited (2010) paper.

2.1 The Starting Value Problem of the EW Estimator

Erickson and Whited (2010) state that "The poor performance of the EW estimators in the ACG Monte Carlos is also an artifact of an uninformed choice of starting values for GMM4 and GMM5."

[5] Erickson and Whited (2010) conjecture that the positive cash flow coefficient may be due to the use of net capital stock as deflator for the q proxy. However, their own results suggest that this is not the case — the cash flow coefficient remains positive after using their preferred denominator for empirical q.

The authors also say: "The GMM3 estimator is based on 5 equations in 5 unknowns and is therefore exactly identified. Results obtained from this estimator cannot, therefore, depend on starting values for a minimization routine." Their contention is that the results for the EW-GMM4 and EW-GMM5 in the ACG study were spurious due to poor choices of starting values in the non-linear optimization algorithm that underlies the EW estimator — the objective function of that estimator is only locally (not globally) concave. At the same time, the authors admit that ACG's tests of EW-GMM3 are immune to the poor starting value problem.

The starting value argument of Erickson and Whited (2010) is limited and internally inconsistent. The panel data results presented in ACG show that the biases associated with the EW-GMM3 estimator are *as severe* as those of the EW-GMM4 and EW-GMM5 estimators. Since the large biases in the EW-GMM3 is independent of the starting value issue, there is no reason to believe that biases in the EW-GMM4 and EW-GMM5 estimators are due ACG's choices of starting values. Moreover, as shown in Table 5 of Erickson and Whited (2010), changing the initial condition alone *does not* help EW-GMM4 and EW-GMM5 estimators. Specifically, results in columns 2 and 3 of that table show that estimates for the mismeasured regressors remain biased after the authors work with "better" starting values (the bias associated with the EW-GMM4 estimator, for example, is about 24%).

Another inconsistency in the argument about starting values is transparent from Table 2 in ACG as well as previous results in Erickson and Whited (2000, 2002). If starting values for the optimization algorithm for high order moments were a key problem, then one would expect to observe problems with initial condition values even in the highly skewed case (e.g., the Lognormal distribution case). However, it is well known that for Lognormal distributions the GMM estimator performs well, irrespective of initial conditions. One is left with the conclusion that the identification problem that arises from lack of skewness is responsible for less than well-behaved objective functions with local minima that compromise the EW estimation procedure.[6] In other words, the key issue is skewness — as originally pointed out by ACG — and not starting values. Given the conclusion that better starting value choices cannot do much to improve the performance of the EW estimators, we turn our attention to another fix proposed by Erickson and Whited (2010): the use of "trimmed estimators."

Erickson and Whited (2010) propose a new approach to the implementation of the original EW

[6] Confirming this argument, we show below that the bias in the EW estimator is further reduced when one uses a more strongly skewed distribution (a Normal to the fourth power).

that is based on the use of Gini intervals (cf. Gini (1921)). The new, add-on procedure is designed to reduce the bias in the EW estimator by eliminating outlying coefficients that arise from the period-by-period estimations embedded in the EW method for panel data. The procedure imposes that the end values of the EW objective function optimization must belong to an interval determined by the "direct" and "reverse" regressions of interest (explained shortly). The underlying idea is that in case estimates are too extreme, the EW estimator will at least approximate the results from a simple OLS.

Careful examination shows that this artifice does not generally reduce bias in the EW estimator. To demonstrate this, we conduct a simulation where we study the length of the Gini intervals proposed by Erickson and Whited (2010). Using the design of ACG, we simulate data from the following model with measurement error:

$$y_i = \alpha_0 + \chi_i \beta + z_{i1}\alpha_1 + z_{i2}\alpha_2 + z_{i3}\alpha_3 + u_i, \tag{1}$$

where the observable variable x_i is given by

$$x_i = \chi_i + e_i. \tag{2}$$

As in ACG's study, the vector $(\chi_i, z_{i1}, z_{i2}, z_{i3})$ is generated (alternatively) from four different distributions: Normal, F(10,40), Chi-square (5), and Lognormal. We draw a 1000 samples of u_i and e_i from Lognormal distributions and set $(\alpha_1, \alpha_2, \alpha_3) = (-1, 1, -1)$, with the number of repetitions equal to 2000. To study the sensitivity of the results, we also vary the coefficient that controls for the mismeasured variable as $\beta = \{0, 0.01, 0.05, 0.1, 0.5, 1, 2, 10\}$.

To compute the Gini intervals as in Erickson and Whited (2010) one must estimate two OLS regressions. The first is the "direct regression:"

$$y_i = \theta_0 + x_i\theta_1 + z_{i1}\theta_2 + z_{i2}\theta_3 + z_{i3}\theta_4 + u_i. \tag{3}$$

The second is the "reverse regression:"

$$x_i = \gamma_0 + y_i\gamma_1 + z_{i1}\gamma_2 + z_{i2}\gamma_3 + z_{i3}\gamma_4 + u_i. \tag{4}$$

Using the results from these estimations one can compute the bounds for the coefficient associated with mismeasured regressor in Eq. (1). In particular, the Gini interval for β in Eq. (1) is given by $\left(\theta_1, \frac{1}{\gamma_1}\right)$, which are taken from Eqs. (3) and (4). The bounds for the exogenous covariates are computed similarly and we omit them to save space. The results from our simulation are reported in Table 1.

Table 1: Lower and Upper Bounds for Simulated Gini Intervals

This table shows results from Monte Carlo simulations for the Gini interval of the mismeasured regressor. The lower bounds (Lower B.) and upper bounds (Upper B.) are computed from the 'direct' and the 'reverse' regressions. The data is generated from a cross-section model, where we draw the regressors from four different distributions (Normal, Lognormal, F(10,40), and Chi-square(5)) and vary the coefficient of the mismeasured variable.

	Normal		Lognormal		$F(10,40)$		Chi-square(5)	
β	Lower B.	Upper B.	Lower B.	Upper B.	Lower B.	Upper B.	Lower B.	Upper B.
0	0.00	21.47	-90.85	0.00	-26.77	0.00	0.00	26.23
0.01	-73.88	0.00	0.00	68.77	0.00	55.69	0.01	14.03
0.05	0.01	37.56	0.02	42.34	-428.74	0.00	0.03	27.21
0.1	-24.21	0.00	0.04	5.61	0.00	159.07	0.06	8.79
0.5	0.06	7.59	0.19	4.00	0.02	102.18	0.29	2.03
1	0.12	9.29	0.39	2.71	0.04	25.49	0.58	1.75
2	0.25	5.83	0.77	2.85	0.08	24.95	1.15	2.38
10	1.24	10.80	3.88	10.19	0.40	13.09	5.77	10.08

Table 1 has two salient features. First, across different distributional assumptions, the true coefficient β (in the first column) is almost always inside the Gini intervals obtained from the regression pairs Eqs. (3) and (4). This should not be surprising. However, a closer examination reveals that the length of these intervals are extremely large, which makes them uninformative. Another salient feature of Table 1 is that for values of β close to zero, the Gini interval is also extremely large. For instance, for the Lognormal distribution with $\beta = 0.05$ the interval ranges from 0.02 to 42.34, while for $\beta = 0.05$ under the F-distribution the interval goes from -428.74 to 0. This result is important and points to a critical identification problem. In many of the real-world applications afflicted by measurement errors, the coefficient associated with the mismeasured regressor is small (e.g., investment equations). As a result, one of the bounds of the Gini interval will go to infinity, rendering the approach ineffective.

Clearly, the difficulty in dealing with measurement errors is that they can be severe, yet the severity of the problem might be unknown to the researcher. In this case, one should look for estimators that are robust to the scale of the error measurement process. Unfortunately, the Gini interval fix proposed by Erickson and Whited (2010) does not meet this standard. To see this, consider simulation tests where we study the sensitivity of the Gini intervals to the addition of noise to the mismeasured variable. We use the same setup described above, but for simplicity now concentrate on the normal distribution case, fix $\beta = 1$, and replace Eq. (2) with

$$x_i = \chi_i + \sigma \times e_i, \tag{5}$$

where $\sigma = [0, 2]$ controls the amount of noise in the model. We can study the sensitivity of the bounds

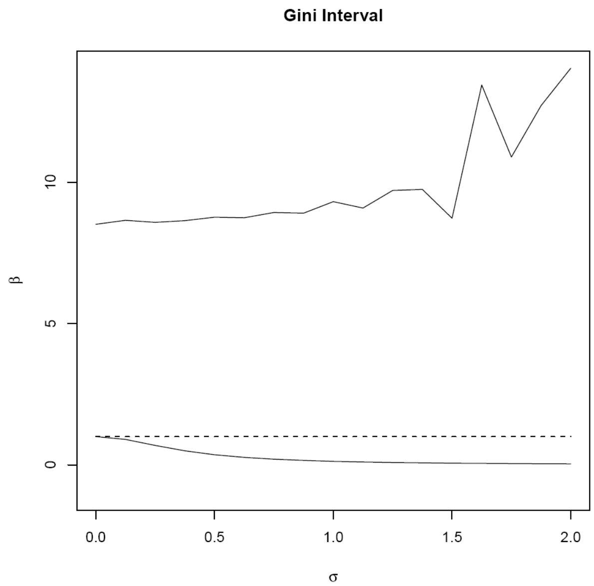

Figure 1: This figure shows the bounds of Erickson and Whited's (2010) proposed Gini intervals as a function of the noise parameter σ.

of the Gini to noise in the mismeasured regressor by modulating the parameter σ.

Figure 1 plots the Gini lower and upper bounds as a function of the noise parameter σ. The figure makes it clear that, for a given β, the Gini interval becomes uninformative as the severity of the measurement error increases. In other words, the newly-proposed Erickson and Whited fix is highly sensitive to the severity of the very problem the EW estimator tries to address.

Erickson and Whited (2010) argue that the tests in Tables 3 and 4 in the ACG study would put the original EW estimator in a more positive light had ACG used the Gini interval approach. To support this claim, Erickson and Whited (2010) modify various parameter and distribution assumptions of the ACG study. Since their text is very terse and the authors do not make their programs available, it is difficult to replicate their experiments. Instead, we take on the task of redoing the simulations in Tables 3 and 4 of the ACG study, differing *only* with respect to the addition of the Gini interval fix to the EW estimator. In other words, we ensure that the relative merit of the new EW fix proposed by Erickson and Whited can be isolated.

Erickson and Whited (2010) claim to succeed in improving the performance of the EW estimator under the $F(10{,}40)$ distribution, we therefore focus on this case. We redo simulations as in ACG and present the annual estimates for 10 years over 10,000 replications. The simulations are from a panel

Table 2: Gini Intervals for Table 3 in ACG

This table presents results from Table 3 of Almeida, Campello, Galvao (2010), where they simulate a panel data model with fixed effects and estimate the model in levels. The lower and upper bounds of the Gini intervals are constructed from period-by-period OLS estimates. The standard EW estimator results for GMM3, GMM4, and GMM5 are also from Almeida, Campello, Galvao. The table also shows the Trimmed EW estimates where we impose the estimated Gini intervals onto the Standard EW estimates. The associated Fama and MacBeth (FM) and MDE (Minimum Distance Estimator) aggregators that combine the period t estimates are presented at the bottom of the table. The bias of the MDE is computed with respect to the true $\beta = 1$.

	Gini Interval		Standard EW			Trimmed EW		
T	Lower B.	Upper B.	GMM3	GMM4	GMM5	GMM3	GMM4	GMM5
t=1	0.305	3.278	3.472	6.622	6.245	3.278	3.278	3.278
t=2	0.424	2.357	-0.905	2.368	2.128	0.424	2.357	2.128
t=3	0.495	2.021	-12.614	3.114	-3.466	0.495	2.021	0.495
t=4	0.531	1.884	-3.464	-2.882	-0.888	0.531	0.531	0.531
t=5	0.550	1.817	-1.942	2.517	5.757	0.550	1.817	1.817
t=6	0.551	1.816	-666.83	-1021.95	-507.28	0.551	0.551	0.551
t=7	0.533	1.875	-5.422	3.798	-7.681	0.533	1.875	0.533
t=8	0.496	2.018	-5.189	4.589	-2.368	0.496	2.018	0.496
t=9	0.425	2.356	0.799	2.931	2.947	0.799	2.356	2.356
t=10	0.306	3.269	-2.282	0.496	4.955	0.306	3.269	3.269
FM			-69.438	-99.840	-49.966	0.796	2.007	1.545
MDE			0.007	0.037	0.082	0.693	2.214	1.984
MDE Bias			-0.993	-0.963	-0.918	-0.307	1.214	0.984

data model with fixed effects. We concentrate on the bias of the mismeasured regressor in Eq. (1), which is set as $\beta = 1$. We construct the lower bound by using the estimated coefficient from OLS regressions, also year-by-year, and approximate the upper bound with the inverse of that coefficient. Minimum distance estimator (MDE) results as well as Fama-MacBeth (FM) results are presented for each of the three versions of the EW estimator.[7] The advantage of the procedure performed here is that the results are *directly* comparable to those in Tables 3 and 4 in ACG.

The results are displayed in Table 2. The true $\beta = 1$ lies inside the Gini interval for all periods, showing that our Gini interval calculations are well behaved. At the same time, notice that virtually all EW coefficients lie outside the Gini bounds. The newly-proposed "trimmed estimates" procedure of Erickson and Whited (2010) involves using one of the bounds of the estimated Gini interval when the EW estimates are "too extreme." It is clear from Table 2 that the proposed trimming will effectively drive the EW results toward OLS-based estimates. Accordingly, the bias in the modified EW will approximate that of the OLS.

The results in Table 2 make it clear that estimated trimming is not an effective tool to reduce the

[7] Since we do not have Erickson and Whited's (2010) programs, the MDE is computed as described in Appendix A of ACG with off-diagonal elements of the variance-covariance matrix set to zero.

Table 3: Gini Intervals for Table 4 in ACG

This table presents results from simulations taken from Table 4 of Almeida, Campelo, Galvao (2010), where they simulate a panel data model with fixed effects and estimate the model after applying the within transformation to the data. The lower and upper bounds of the Gini intervals are constructed from period-by-period OLS estimates. The standard EW estimator results for GMM3, GMM4, and GMM5 are also from Almeida, Campello, Galvao. The table also shows the Trimmed EW estimates where we impose the estimated Gini intervals onto the EW estimates. The associated Fama and MacBeth (FM) and MDE (Minimum Distance Estimator) aggregators that combine the period t estimates are presented at the bottom of the table. The bias of the MDE is computed with respect to the true $\beta = 1$.

	Gini Interval		Standard EW			Trimmed EW		
T	Lower B.	Upper B.	GMM3	GMM4	GMM5	GMM3	GMM4	GMM5
t=1	0.473	2.113	0.361	0.115	0.589	0.473	0.473	0.589
t=2	0.439	2.278	1.280	0.721	-0.060	1.280	0.721	0.439
t=3	0.416	2.403	-0.706	-0.755	0.297	0.416	0.416	0.416
t=4	0.402	2.486	0.008	-1.018	0.144	0.402	0.402	0.402
t=5	0.396	2.523	0.495	-0.043	0.467	0.495	0.396	0.467
t=6	0.398	2.513	-0.163	-0.972	0.592	0.398	0.398	0.592
t=7	0.403	2.479	0.422	0.056	0.241	0.422	0.403	0.403
t=8	0.417	2.400	1.201	-0.235	-0.099	1.201	0.417	0.417
t=9	0.440	2.275	0.497	-0.406	1.480	0.497	0.440	1.480
t=10	0.474	2.112	0.845	0.285	0.475	0.845	0.474	0.475
FM			0.424	-0.225	0.413	0.643	0.454	0.568
MDE			0.646	0.609	0.581	0.738	0.445	0.470
MDE Bias			-0.354	-0.391	-0.419	-0.262	-0.555	-0.530

biases of the EW estimator when a fixed effects model is estimated in level form. The results from the experiment above confirm the results from Tables 3 in ACG, which already showed that the EW estimator is unidentified when dealing with misspecification coming from fixed effects. To use concrete examples, notice that the bias under the MDE Trimmed EW-GMM3 estimator in Table 2 above is –0.307, while the equivalent figure from Table 3 in ACG is –0.354. For the Trimmed EW-GMM4 estimator the bias is 1.214 (or 221.4%), compared to –0.391 in Table 3 of ACG. These results make it clear that tests that properly introduce EW "trimmed estimates" in the ACG analysis do not improve the performance of the EW estimator.

Table 3 reports results that correspond to the simulations performed in Table 4 of the ACG paper. In those experiments, a panel data model with fixed effects is estimated after the within transformation. Again, the results from the EW estimators are taken directly from the original ACG study, with no additional modifications to their set up. The results from this new table are qualitatively similar to those of Table 2. Once again, the true $\beta = 1$ lies inside the Gini interval. However, trimming does not reduce the bias in the EW estimator, as the range of the Gini intervals is quite large and hence not very informative.

As discussed in ACG, the bias in the EW estimator for the panel data comes from the lack of skewness. This weakness of the EW estimator is irrefutable and arguments put forth in Erickson and Whited (2010) regarding the importance of "starting values" are moot. In their new study, Erickson and Whited work with a distribution that is even more skewed than the Lognormal distribution (the Normal distribution raised to the fourth power, N^4). Results in Table 6 of their paper provide further support for ACG's contention that the performance of the EW estimator hinges on strong assumptions about the distribution of an unobservable quantity. While Erickson and Whited do not share the codes needed to replicate any of their results, we study and compare the sensitivity of the EW estimator under the N^4 distribution. This examination shows that one can obtain EW-based estimates that are virtually free of any biases as one increases the skewness of the relevant distributions. This obtains in spite of the newly-introduced starting values problem that according to Erickson and Whited (2010) could affect studies such as ACG or Erickson and Whited (2000).

To validate this claim, Table 4 follows the same approach as ACG in reporting the bias and efficiency of the mismeasured regressor in Eq. (1) under various estimation approaches.[8] In particular, we compare the bias and efficiency of the standard OLS, OLS-IV, Arellano and Bond (AB-GMM), and EW estimators.[9] As in Table 4 of ACG, we generate data from a panel model with fixed effects with no heteroscedasticity. To study the impact of data skewness on those estimators, we consider data coming from the following distributions: standard Normal, $F(10, 40)$, Chi-square(5), Lognormal, N^4, and N^6. The skewness of these distributions are, respectively, 0, 1.3, 1.3, 4.5, 8, and 13.

The results in Table 4 have two salient features. First, the biases in the OLS, OLS-IV, and AB-GMM estimators are *invariant* to the degree of skewness in the distributions one assumes for the model. In particular, the biases associated with OLS-IV estimations are very low and stable. Similar inferences apply to the efficiency (RMSE) of those estimators. Second, the biases and efficiency of the EW estimators are a *monotone function* of the skewness of the distributions one chooses to use: higher skewness leads to lower biases and more efficiency. Consider, for example, the bias of the mismeasured regressor that is associated with the EW-GMM5 estimator (bottom row of Table 4).

[8] The true coefficient β is set to 1, so the biases we report correspond to deviations from that number. Efficiency is measured in terms of root mean square errors (RMSE).

[9] As in ACG, the simple OLS is computed after differencing the model. In the same fashion, the OLS-IV is computed after differencing the model and using the second lag of the observed mismeasured variable as an instrument. The AB-GMM estimates use all the orthogonality conditions, with all available lags of mismeasured variable as instruments. The EW-GMM estimates are computed after using the within transformation.

Table 4: OLS, OLS-IV, AB-GMM, and EW Estimators

This table shows the bias of the mismeasured regressor and the RMSE associated with the estimation of the model as in ACG in simulated panel data using several distributions to generate the regressors: standard Normal, F(10,40), Chi-square (5), Lognormal, Normal distribution rasied to the fourth power, Normal distribution rasied to the sixth power. The table reports results from the estimators using data after applying the within transformation. The table shows the results for the minimum distance estimator associated with EW-GMM3, EW-GMM4, and EW-GMM5. These estimators are based on the respective third, fourth, and fifth moment conditions. The table also presents results for OLS, OLS-IV, AB-GMM estimators.

		$N(0,1)$	$F(10,40)$	Chi-Square(5)	Lognormal	N^4	N^6
OLS	Bias	-0.712	-0.712	-0.713	-0.713	-0.713	-0.719
	RMSE	0.712	0.713	0.713	0.713	0.714	0.165
OLS-IV	Bias	0.006	0.007	0.006	0.006	0.002	0.005
	RMSE	0.118	0.121	0.115	0.118	0.118	0.136
AB-GMM	Bias	-0.025	-0.023	-0.023	-0.025	-0.032	-0.039
	RMSE	0.098	0.098	0.098	0.098	0.089	0.110
EW-GMM3	Bias	-0.737	-0.354	-0.381	-0.046	-0.021	-0.009
	RMSE	0.780	0.424	0.442	0.090	0.057	0.042
EW-GMM4	Bias	-0.864	-0.391	-0.389	-0.055	-0.029	-0.020
	RMSE	0.885	0.489	0.483	0.140	0.109	0.107
EW-GMM5	Bias	-0.816	-0.419	-0.413	-0.075	-0.040	-0.023
	RMSE	0.851	0.510	0.509	0.182	0.140	0.126

For the F-distribution, with skewness approximately equal to 1.3, the bias is –0.419. The Lognormal distribution has a skewness of 4.5 and the bias in the EW-GMM5 estimator drops to –0.075. When one inflates the degree of skewness in the model to about 8 using N^4, the bias goes down to –0.040. And if the skewness goes to 13 under N^6, the bias approximates 0.

The results of this section make it clear that discussions about starting values affecting the EW estimator are a distraction away for the important issue of model identification. The EW estimator hinges solely on the skewness one assumes for data processes that are unobservable.

2.2 The Fixed Effects Problem of the EW Estimator

As recognized in Erickson and Whited (2010), the EW estimator is inconsistent when the data have individual-fixed effects and the model is estimated in level form (i.e., without differencing out the fixed component). Moreover, as shown in ACG and Erickson and Whited (2010), the EW estimator is not robust to the presence of fixed effects because the within transformation reduces the skewness that is required for identification (see Table 4 in ACG and Table 6 in Erickson and Whited).[10] The usefulness of the EW ultimately hinges on the question of whether individual-specific effects are prevalent in

[10] In Table 6 of Erickson and Whited (2010), the within transformation reduces the degree of skewness, biasing estimates of mismeasured regressors under the EW method (biases in EW-GMM4 and EW-GMM5 exceed 30%).

real-world data.

Most researchers would find it difficult to dismiss the importance of fixed effects in corporate finance data. The ACG study essentially calls the attention for the unusual ease with which proponents of the EW estimator have dismissed the importance of fixed effects in investment models and other finance applications. Erickson and Whited (2010), try to discredit standard Hausman tests used in, among others, Erickson and Whited (2000) as a way to distance themselves from the ACG critique. The authors in turn propose a new test for dealing with fixed effects, which we discuss in turn.

Erickson and Whited criticize the use of standard fixed-effects tests (such as the Hausman test) because they confound fixed effects in the error term and fixed effects in the mismeasured regressor. The authors propose a new test for the correlation between fixed effects and the independent variables in the presence of measurement errors. Their test compares the EW estimators before and after transforming the data, under the null hypothesis that the underlying data do not contain fixed effects. Under the null, the EW results are similar whether or not the data is differenced. The alternative is that the EW estimates differ because the data have fixed effects. The test is designed for individual cross-sections of the data (e.g., implemented year-by-year); that is, it does not consider the panel data jointly.

There are a two important difficulties with the test proposed by Erickson and Whited (2010). First, note that the test compares high order moment estimators that rely on skewness for identification. In this context, the data transformation used in the test is not innocuous. Data transformations that are designed to eliminate the fixed effect component will also reduce the skewness. As a result, the proposed test will confound lack of skewness with the presence of fixed effects. Indeed, Erickson and Whited report finite-sample simulations showing that for a nominal size of 5% their test has an actual size of 40%. The high size of the Erickson and Whited (2010) fixed effects test implies it is a problematic tool for the econometrician. It rejects the null too often and causes the researcher to use a misspecified model for estimation and inference procedures.

Second, the implementation of the proposed test is challenging in panel data due to the aggregation problem imbedded in its design. The usual Hausman test for fixed effects compares coefficients from two estimations performed over the entire panel. The procedure produces a single test statistic that the researcher can use following standard inference practice. The proposed Erickson and Whited (2010) test, in contrast, is performed period-by-period, producing statistics for every cross-section of the data. After performing the proposed test, researchers must use *ad hoc* rules in determining whether fixed effects are relevant in their data — inferences are no longer rooted in standard statistical theory. As a concrete example, consider Table 4 in Erickson and Whited (2010). The table reports the fraction of years in which their test for the presence of fixed effects produces rejections of the null that fixed effects are irrelevant. These estimates vary from 12% (under EW-GMM3) to 46% (EW-GMM5). Most researchers would deem these statistics as inconclusive with respect to the importance of fixed effects in the experiments performed. Erickson and Whited, however, take these numbers as sufficient statistics for dismissing the importance of firm-fixed effects in investment equations performed over COMPUSTAT data.

Fortunately, one can mitigate the aggregation problem in the Erickson and Whited (2010) test. To isolate fixed effects in the error term from the fixed effects in the mismeasured regressor, one can use the Hausman test to compare differences in estimates produced from the OLS-IV procedure in ACG with and without data transformation. Under the null hypothesis of no fixed effects, both estimates should be close. Under the alternative, estimates obtained from data in level form should differ considerably from estimates from differenced data. This procedure, already described in Holtz-Eakin (1988), is simple to implement and delivers a single, easy-to-interpret diagnostic statistic for the entire panel.[11] Using the original COMPUSTAT sample from ACG, we perform this measurement-error consistent Hausman test and find that the null of fixed effects irrelevance is overwhelmingly rejected. We obtain p-values that approximate zero (much lower than the standard 1% hurdle) for different choices of instruments and for different subsamples of the ACG data. These tests simply confirm ordinary belief that fixed effects matter in corporate data and researchers should be cautious before disregarding their impact on model specification and estimation. Researchers should be particularly cautious about estimators that have a difficult time handling fixed effects.

2.3 The Heteroskedasticity Problem in the EW Estimator

Erickson and Whited (2010) also criticize the introduction of heteroskedasticity in the ACG experiments. Heteroskedasticity is a common type of disturbance in real-world data. A simple, text-book result in econometrics is that the OLS estimator remains unbiased and consistent in the presence of general forms of heteroskedasticity (see, among others, Johnston and DiNardo (1996, p. 162)). It is natural to study if any given estimator — including the EW estimator — is robust to heteroskedasticity. This standard examination is performed in the ACG paper, and it turns out that lack of robustness to

[11] As is standard in Hausman-type tests, this test assumes that under the null the OLS-IV estimator in levels is more efficient than the OLS-IV in differences.

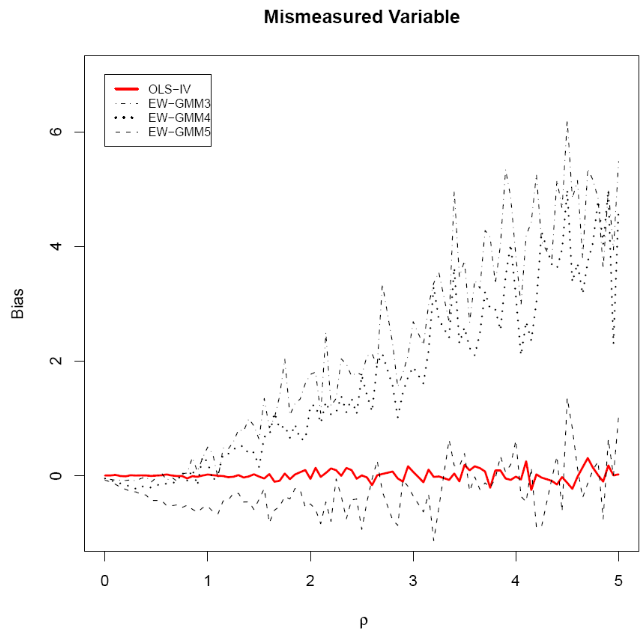

Figure 2: Bias in OLS-IV and EW estimators as a function of the heteroskedasticity parameter ρ.

heteroskedasticity is a significant shortcoming of the EW estimator. ACG show in their Figure 2 (reproduced in Figure 2 here), that while the OLS-IV estimator is approximately unbiased under pronounced heteroskedasticity the EW estimator is biased in the presence of even modest heteroskedasticity.

Erickson and Whited (2010) argue that the test ACG use to detect heteroskedasticity in investment data has a large type-I error and is likely to reject the null of homoskedasticity even when it is true. They focus on diagnostic tests, rather than discussing the consequences of the heteroskedasticity problem. Their argument revolves around whether real investment data contain heteroskedastic disturbances, and at no point the authors discuss the relative robustness of the OLS-IV and EW estimators to heteroskedasticity (ACG's central argument). Regardless of the properties of the diagnostic test, most researchers believe that data used in corporate finance applications contain heteroskedasticity. A cursory read of the literature shows that virtually all empirical papers attempt to correct standard errors for this problem. Erickson and Whited's (2010) implicit claim that ACG overstate the importance of heteroskedasticity should strike most researchers as implausible.

More importantly, Erickson and Whited (2010) argue that ACG incorrectly introduce heteroskedasticity in their simulations. In particular, they argue that the way in which ACG introduce heteroskedasticity increases the variance of the error term, driving the regression R^2 toward zero. The authors conjecture that the low regression R^2 — not heteroskedasticity — is one of the causes of the poor performance of the EW estimator in the tests reported by ACG. We discuss this argument in turn.

ACG's assumptions are very common in the econometrics literature (see, among others, Koenker (2004), Wang and Fygenson (2009), and Lamarche (2010)). It is difficult to see how objections to the way ACG study heteroskedasticity will provide sound support for the EW estimator. It is expected that the introduction of heteroskedasticity in an econometric model will lead to an increase in the variance of the coefficients, hence a lower R^2. ACG's point is that a good estimator should be robust to this possibility. Unfortunately, the EW estimator is not designed to handle heteroskedastic errors, and as result suffers with the presence of these kinds of disturbances. In contrast to the poor performance of the EW estimator, the OLS-IV estimator is largely immune to heteroskedasticity. Put differently, the "low R^2 problem" identified by Erickson and Whited (2010) dramatically affects the EW estimator and is irrelevant for OLS-IV models. Observations of this type simply corroborate ACG's conclusions about the relative merits of the EW estimator.

At a basically level, it is well known that R^2 is a problematic measure of regression fitness (see, e.g., Davidson and MacKinnon (1993)). A standard alternative is to conduct a F-test. It is useful to perform a simple experiment to compare the performance of these two diagnostic statistics under heteroskedasticity. We do this under the ACG framework, generating data using the following model:

$$y_i = \alpha_0 + \alpha_1 x_{1i} + \alpha_2 x_{2i} + (1 + \rho \times x_{1i}) \times u_i, \tag{6}$$

where variables are draw from a normal distribution and $(\alpha_0, \alpha_1, \alpha_2) = (1, -1, 1)$. We plot the p-values for the F-test and the R^2 as a function of the parameter ρ, the parameter that controls the degree of heteroskedasticity.

The results are displayed in Figure 3. The figure shows that while R^2 goes down in value as heteroskedasticity increases, the F-test's p-values remain close to zero — correctly rejecting the null of model irrelevance — even for high values of ρ. In other words, inferences from this simple model derived under the ACG simulation framework remain valid even for high degrees of heteroskedasticity; low R^2 do not compromise the quality of the estimator.

To sum up, the analysis in Erickson and Whited (2010) simply confirms the point made by ACG. The EW estimator is not robust to general forms of heteroskedasticity, while the OLS-IV estimator is robust. The one-sided analysis of Erickson and Whited is, at best, a distraction from this key point.

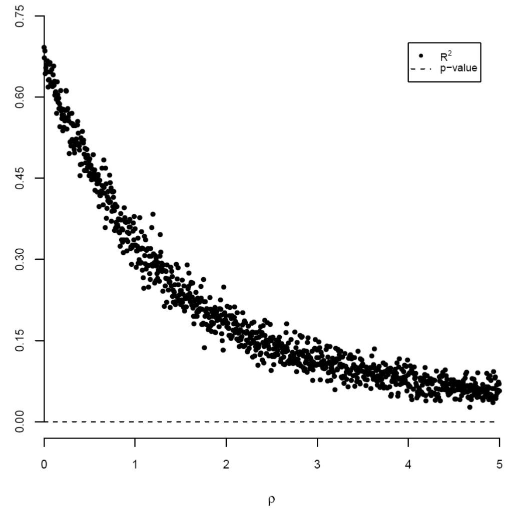

Figure 3: Regression p-values and R^2 as a function of the heteroskedasticity parameter ρ.

2.4 The Problem of Distributional Assumptions in the EW Estimator

Erickson and Whited (2010) claim that the distributions used by ACG are equivalent to a normal distribution. The authors state: "Their F distribution is nearly indistinguishable from the normal, and the chi-squared distribution exhibits only modest skewness." They also say: "ACG inadvertently misrepresent the degree of normality of the distributions they use." These and other related statements in Erickson and Whited's paper are inaccurate.

Figure 4 combines the four distributions studied in the ACG paper (standard Normal, F(10,40), Chi-square (5), and Lognormal) in one single plot. Figure 4 comes from Figure 1 in the ACG paper and shows that the distributions considered by that study are very different from one another. In particular, the F, Chi-square, and Lognormal distributions are markedly different from the Normal distribution. ACG chose distributions with varying degrees of skewness to examine the sensitivity of alternative estimators (including EW) to data skewness At the beginning of the skewness spectrum, ACG put the F-distribution and at the end they put the Lognormal distribution.

In Figure 1 of their paper, Erickson and Whited (2010) superimpose a separate Normal distribution onto each of the non-skewed distributions in the ACG paper. A casual reader might miss the point that the authors alter the mean and variance of each Normal depicted to match the mean and variance of the distribution in question. Erickson and Whited then argue that the skewed distributions of the ACG study are "similar" to a Normal distribution. Beyond re-centering and reshaping the distributions

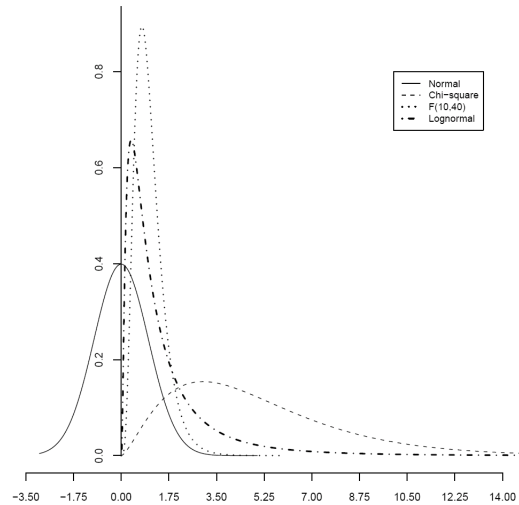

Figure 4: This figure is taken from Figure 1 in Almeida, Campello, and Galvao (2010) and depicts the distributions considered in that study.

used in ACG, Erickson and Whited use different scales in their graphs in order to achieve their desired visual effect. We reproduce in Figure 5 the plots in Figure 1 of their paper, where F, Chi-square, and Lognormal distributions appear in solid lines and the respective modified Normal distributions appear in dashed lines.

Note that ACG use only one Normal distribution — and that is the *standard* Normal — in their tests. Accordingly, the "graphing exercise" in Erickson and Whited has no resemblance to the tests performed in ACG. But the way in which Erickson and Whited portray their claim that the distributions in ACG are "near normal" begs further correction. We do this in turn.

We first correctly draw the figures Erickson and Whited (2010) meant to draw. We follow their proposed exercise of superimposing skewed and Normal distributions, but hold the exercise to some minimum standard: use *the same scale* across the distributions depicted. The results are in Figure 6, which shows the different ACG skewed distributions in solid lines and in dashed lines the respective modified Normal distributions.

Visual inspection shows that the skewed distributions used in the ACG study are quite different from any Normal curve representation once one holds a minimum standard of formality and objectivity when presenting such comparisons. The distributions studied by ACG have fatter right-tails and modes located at the left of the "modified Normals" introduced by Erickson and Whited (2010). As should be expected, these differences become more evident as one runs the skewness spectrum

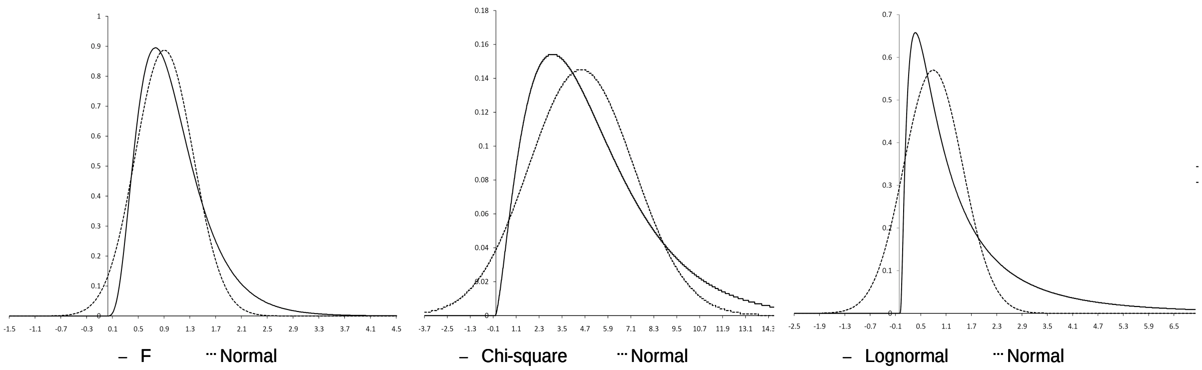

Figure 5: This figure is taken from Figure 1 from Erickson and Whted (2010). It is designed to depict the distributions used in ACG superimposed with "modified" Normal distributions.

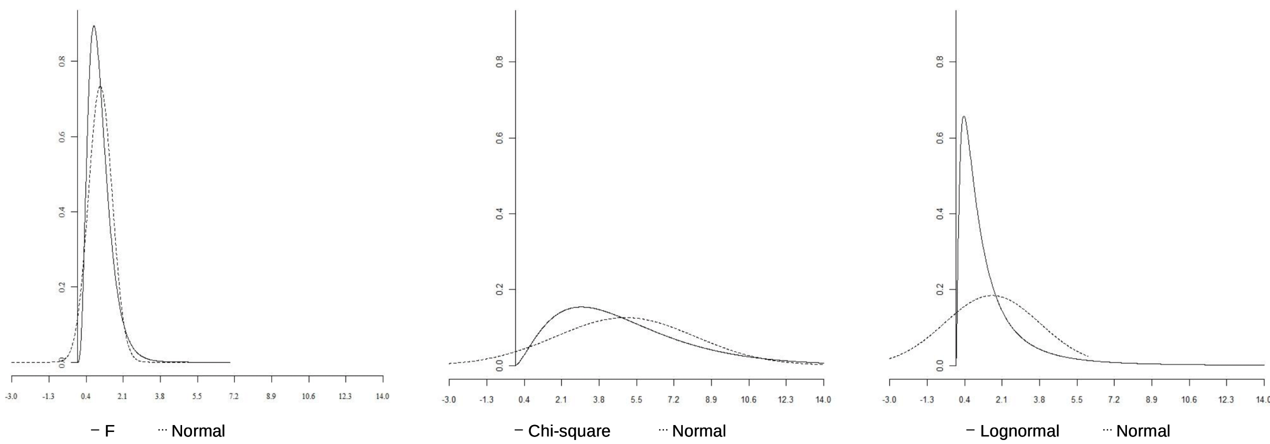

Figure 6: This figure correctly depicts the distributions used in ACG superimposed with "modified" Normal distributions.

considered by ACG, going from the F-distribution to the Lognormal distribution. Yet, they hold for every single distribution.

To show our arguments more rigorously, we perform a number of Kolmogorov-Smirnov distributional tests. These tests overwhelmingly reject the null hypothesis that the F, Chi-square, and Lognormal distributions of ACG are statistically equal to the modified Normals that are superimposed under the Erickson and Whited (2010) graphing exercise. The associated p-values are all virtually zero. We also perform Jarque-Bera tests of skewness and again overwhelmingly reject the hypothesis that the skewed distributions use in ACG have "near-Normal" skewness.

The ACG study explores the sensitivity of a number of error-robust estimators to data skewness.

In doing so, the authors work with distributions with varying degrees of skewness. They find that the EW estimator is sensitive to small departures from extremely high skewness, while other estimators are insensitive to such departures. This is a critical problem for the EW estimator, since it is designed to handle mismeasured regressors and researchers ultimately do not see the process governing the measurement error. The ACG study simply highlights and clarifies these types of limitations, similarly to studies in standard econometrics highlighting the problem of weak instruments in IV estimations.

2.5 The Problem with the Proxy for Tobin's *q*

One of the central arguments of the Erickson and Whited (2010) paper is that they use a "better proxy" for Tobin's q, a proxy that improves the performance of the EW estimator. It is straightforward to show that their argument is problematic.

First, as we demonstrate below, Erickson and Whited's preferred proxy has measurement problems of its own. Hence it is not unambiguously better than alternative proxies, such as the standard market-to-book ratio. Second, given that it is impossible to show theoretically that one proxy is better than another, the key issue is whether inferences are robust to the use of different proxies. In our data analysis, we show that the OLS-IV estimator produces reasonable estimates for the many proxies that we consider (including the proxy proposed by Erickson and Whited). In contrast, the EW estimator performs poorly even when we use the proxy Erickson and Whited prefer. In particular, the year-by-year coefficients on q and cash flow that are produced under the EW estimator continue to be unstable and unreliable. When we aggregate these coefficients using a standard Fama-MacBeth procedure, we obtain q and cash flow coefficients that are economically nonsensical and statistically insignificant. We conclude that the improved performance of the EW estimator in Erickson and Whited (2010) has little to do with their "better proxy" for q.

2.5.1 Alternative Proxies for Tobin's q

The proxy used by ACG (call it q^{MB}) is the standard market value of total assets, divided by the book value of total assets:

$$q^{MB} = \frac{\text{Book value of assets} - \text{Book value of equity} + \text{Market value of equity}}{\text{Book value of assets}}. \tag{7}$$

Erickson and Whited (2010) argue that this proxy will not isolate investment opportunities embedded in physical capital because it is influenced by other assets such as current assets. According to the authors, the ideal proxy should only measure the ratio between the market and the book (or replacement) value of physical capital. To fix the problem, they suggest the use of the following proxy (call it q^{EW}):

$$q^{EW} = \frac{\text{Market value of equity} + \text{Book value of debt} - \text{Current assets}}{\text{Book value of PPE}}. \tag{8}$$

It is easy to see that this proxy has measurement problems of its own. In order to understand these problems, consider a simplified balance sheet:

$$\text{Assets} = \text{Current assets} + \text{PPE} + \text{Other long-term assets} = \text{Other liabilities} + \text{Debt} + \text{Equity} \tag{9}$$

As in Erickson and Whited, let us assume for now that all assets other than PPE (physical capital) are correctly valued at book values. In other words, the entire difference between the market and the book value of assets can be attributed to physical capital.[12] Using this assumption and Eq. (9), q^{EW} can be written as:

$$q^{EW} = \frac{\text{Market value of PPE} + \text{Other long-term assets} - \text{Other liabilities}}{\text{Book value of PPE}}. \tag{10}$$

Even under the assumption that the difference between market and book values is due to physical capital, q^{EW} is a *biased measure* of investment opportunities in physical capital. The bias arises from the fact that the numerator (but not the denominator) of q^{EW} includes the book value of other long-term assets and is reduced by the book value of other liabilities. Other long-term assets include quantitatively important items such as intangible assets, goodwill, long-term deferred taxes, and long-term financial investment. The q^{EW} proxy essentially assumes that a firm with larger values of these other assets has greater investment opportunities in physical capital, which is conceptually incorrect. Interestingly, this very problem is highlighted in an earlier paper by Erickson and Whited (2006). It is, however, overlooked in Erickson and Whited (2010).

The second problem with q^{EW}, which is that of ignoring other liabilities, is common in corporate finance research (see Welch (2010) for a discussion). Welch shows that other liabilities such as accounts payable are sizable and important for panel capital structure regressions. In Eq. (10), firms with large values of other liabilities will have lower values of q^{EW}, giving rise to yet another source of measurement error in the model.[13]

[12] Naturally, this assumption is unlikely to hold in the real world. Below we argue that violations of this assumption make the case for q^{EW} even worse.

[13] We show below that this problem is very severe in the data, causing q^{EW} to assume negative values in 18% of the firm-years in a standard COMPUSTAT sample.

The standard proxy in the modern literature, q^{MB}, does not suffer from the problems discussed above since all assets and liabilities are accounted for both in the numerator and in the denominator of Eq. (7). This point in itself is a good reason to use q^{MB} rather than q^{EW} in empirical applications.

Noteworthy, it is possible to improve upon Erickson and Whited's (2010) proxy by deducting the (book) value of other long-term assets and adding other liabilities to the numerator of q^{EW}. Empirically, this proxy (which we call q^K) can be implemented as follows:

$$q^K = \frac{\text{Book value of PPE + Market value of equity – Book value of equity}}{\text{Book value of PPE}} \tag{11}$$

Under Erickson and Whited's assumption that all assets other than PPE are valued at book values, q^K is arguably "better" than q^{EW} as a proxy of the investment opportunities embedded in physical capital.

The assumption that all assets other than physical capital are valued at book values is essential for the Erickson and Whited's (2010) proxy construct. Unfortunately, this assumption is unlikely to hold in the real world. Intangible assets such as human capital, goodwill, and patents, for example, are not likely to be correctly valued at book values. In this case, then even q^K will be a biased measure of investment opportunities embedded in physical capital, since the numerator will contain the market value of these other long-term assets (and not only physical capital). Under this alternative scenario, it may be more reasonable to use the standard market-to-book ratio, q^{MB}, which includes the market value of all assets in the numerator and their book value in the denominator.

In sum, Erickson and Whited's (2010) claim that q^{EW} is an unambiguously superior proxy for the investment opportunities embedded in physical capital does not stand up to scrutiny. A more balanced view is that different proxies have advantages and disadvantages, and that an estimator that reliably addresses measurement error in investment models should give sensible results for all reasonable proxies. We now show that this is the case for the OLS-IV estimator, but not the case for the EW estimator.

2.5.2 Empirical Estimations

The analysis above suggests that there is no unambiguously best proxy for Tobin's q. In this sense, Erickson and Whited's (2010) claim that the EW estimator performance varies across proxies for q does not strengthen the case for the EW estimator. Importantly, the data analysis in Erickson and Whited is incomplete since it does not compare the performance of the EW estimator with the OLS-IV estimator for their preferred proxy q^{EW}. The goal of this section is to compare the empirical performance of both estimators under different proxies for Tobin's q. This examination is central to the arguments brought up by Erickson and Whited and it is important that we clarify their claims.

ACG already compare the performance of various estimators using the standard proxy q^{MB}. Those authors find that the OLS-IV produces economically sensible results, while the EW estimator does not. However, since Erickson and Whited (2010) imply that there may be problems with the data used by ACG (see, e.g., their footnote 5), we choose a different route and redo all of our estimations using a newly constructed dataset that attempts to resemble the one used in Erickson and Whited. More importantly, our procedure also addresses Erickson and Whited's arguments about variable definitions (such as whether to deflate capital expenditures using gross or net PPE), by adopting the same definitions that they use. Unfortunately, we cannot use the exact same data used in Erickson and Whited (2010), since the authors state in their paper that they will not share their data before publication. Our data and programs, in contrast, are readily available to the interested reader.

Our variables are constructed as indicated in Section 3.2 of Erickson and Whited (2010). The only new variable is our alternative proxy, q^K. We define it as the sum of market value of equity (*prcc_f* times *csho*) with COMPUSTAT field *ppent*, minus *ceq* (the numerator), scaled by *ppegt*.[14]

Our sample is described in Table 5. The sample we use has more observations than Erickson and Whited's sample (47,735 versus 43,897 observations). The summary statistics on investment and q^{EW} are virtually identical to the figures reported by those authors. The only difference is that our q^{EW} has slightly higher skewness. Our cash flow variable has higher mean and variance than those reported by Erickson and Whited. We also report the summary statistics for q^{MB} and q^K. We note that both q^{EW} and q^K have much higher standard deviations than q^{MB} (approximately 3.5 times as high). This observation will help us interpret the coefficients that we obtain below. All of the summary statistics in Table 5 look reasonable and consistent with previous literature.

A notable point in the summary statistics refers to the number of negative observations in Erickson and Whited's preferred Tobin's q proxy, q^{EW}. This problem is also identified in independent work by Agca and Mozumdar (2010). As shown in Table 5, in 18% of the firm-years q^{EW} are negative. A negative q does not make good economic sense. The high number of negative observations is due to the problem that we identified above: Erickson and Whited's implicit and incorrect assumption that

[14] We use PPE net of accumulated depreciation in the numerator (*ppent*) since this is a proxy for the book value of the existing capital stock as it is reflected in long-term assets. As discussed by Erickson and Whited, gross PPE (*ppegt*) is used in the denominator because it is a proxy for the replacement value of the capital stock (which likely includes accumulated depreciation).

Table 5: Descriptive Statistics

This table shows the descriptive statistics for investment, cash flow, and for alternative proxies for q: q^{MB}, q^{EW} and q^{K}. The percentage of negative values is the proportion of the number of observations with negative q's to the total number of observations. The data are taken from annual COMPUSTAT industrial files from 1972 to 2005. See text for details.

Variable	Obs.	Mean	Std. Dev.	Median	Skewness	% Neg. q Values
Investment	47,735	0.128	0.113	0.099	2.966	
Cash flow	47,735	0.174	0.311	0.162	-1.256	
q^{MB}	47,735	1.436	0.990	1.149	4.173	0.0%
q^{EW}	47,735	1.522	3.601	0.598	7.232	18.3%
q^{K}	47,735	1.759	3.488	0.841	7.463	6.0%

the value of other liabilities is zero. In contrast, there are no negative observations for q^{MB}, and only 6% for q^{K}. While these negative observations may not directly impact the final estimates, they seem to underscore our point that there is no "optimal" or "better" empirical proxy for q.

Next, we present results from estimating empirical investment equations using both the EW estimators (EW-GMM3, EW-GMM4 and EW-GMM5), and OLS-IV. We also report standard OLS estimates for comparison purposes. First, we note that despite to claims the contrary in Erickson and Whited (2010), a Hausman test based on the OLS-IV procedure (introduced in Section 2.2 above) strongly rejects the null hypothesis of absence of fixed effects in the data (all the p-values are virtually equal to zero). Thus, it is incorrect to estimate this model without treating the data for fixed effects. Accordingly, the OLS-IV model is estimated using differenced values for investment, cash flow, and q. Following ACG's benchmark specification, we use the second lag of q (in level) as an instrument for current differenced q. As in ACG, the results are robust to reasonable variations in the instrument set (such as using only longer lags of q and using cash flow in addition to q). For the EW estimators, we first conduct a within transformation to address the presence of fixed effects.

While the OLS-IV estimator automatically produces a single estimate for cash flow and q coefficients in panel data, the EW estimator generates year-by-year results, which must then be aggregated into a single estimate. Fama-MacBeth is a standard procedure to aggregate year-by-year estimates in panels. This is the aggregator used by Riddick and Whited (2009) and ACG in the context of EW estimators. Erickson and Whited (2010) also use Fama-MacBeth to aggregate year-by-year estimates in some regressions, but most of their results use a newly developed minimum distance procedure. Given that they did not share their data and programs, we were unable to employ this new procedure in the current analysis. Below we focus on the Fama-MacBeth-based results. Since the original ACG paper used Fama-MacBeth, this is also a desirable way to evaluate Erickson and Whited's claim that the ACG study employed an inappropriate proxy for Tobin's q. For completeness, we also report the year-by-year EW coefficients in the appendix.

The results are presented in Table 6. We start by describing our estimates using the q^{MB} proxy, which was used by ACG and in most papers in the modern literature (e.g., Kaplan and Zingales (1997) and Rauh (2006)). The results we obtain using the Erickson and Whited (2010) sample are very similar to those reported in Table 9 of ACG. The OLS-IV estimator appears to do a good job of addressing measurement error in q, since the q coefficient increases by a factor of four relative to the benchmark OLS-FE estimates (the OLS estimates would suffer from attenuation bias in the presence of measurement errors). The cash flow coefficient decreases by a factor of three (which is expected for a well-measured regressor that is positively correlated with the mismeasured regressor). In contrast, the EW estimators fail to produce economically sensible results. The q coefficient is never statistically significant, and the point estimate is negative for the EW-GMM5 estimator. Table A.1 in the appendix shows that this pattern is due to the instability of coefficient estimates over the years. In addition, the EW estimator fails to reduce the cash flow coefficient, which actually becomes larger under the EW estimator.

The other panels in Table 6 depict the results we obtain when we experiment with the other two proxies for Tobin's q: q^{EW} and q^{K}. The first point to notice is that, contrary to the claims in Erickson and Whited, using their "better proxy" q^{EW} does nothing to improve the reliability of the EW estimates. The q coefficients remain insignificant and economically nonsensical. If anything, the results look worse than those using the standard market-to-book proxy. Second, using q^{K} also does not improve the performance of the EW estimators. As shown in the bottom portion of Table 6, the point estimates for the q coefficients are negative (though not statistically significant). These results are again caused by the lack of reliability and stability in the year-by-year coefficients obtained via the EW procedure, which can be verified in Tables A.2 and A.3 in the appendix. Third, the OLS-IV results are qualitatively identical to those reported in the top portion of Table 6. The q coefficient increases by a factor of about four, and is always significant. The cash flow coefficient, in contrast, drops significantly. These patterns are consistent with the results in ACG, and suggest that the OLS-IV does a good job of addressing measurement error irrespective of the q proxy that we choose.

In all, our results show that Erickson and Whited's (2010) claims about the relative merits of dif-

Table 6: Estimation of Investment Models

This table shows the coefficients and standard deviations that we obtain when we use OLS, OLS-FE, OLS-IV, and EW estimators in the standard investment equation. The OLS estimators use differencing transformation to treat for firm-fixed effects. The intrumentalized variable is the differenced q and the instument is the second lag of q. Robust standard erros in parentheses for OLS and EW estimators, and clustered in firms for OLS-FE and OLS-IV. Each EW coefficient is an average of the yearly coefficients reported in the Appendix tables (A.1, A.2, and A.3) and the standard error for these coefficients is a Fama-MacBeth standard error. The data are taken from the annual COMPUSTAT industrial files from 1972 to 2005. See text for details. *,** and, *** represent statistical significance at the 10%, 5%, and 1% level, respectively.

Variables	OLS	OLS-FE	OLS-IV	EW-GMM3	EW-GMM4	EW-GMM5
q^{MB}	0.025***	0.015***	0.082***	0.019	0.042	−0.051
	(0.001)	(0.001)	(0.010)	(0.054)	(0.030)	(0.093)
Cash flow	0.083***	0.097***	0.033***	0.154***	0.130***	0.177***
	(0.004)	(0.006)	(0.006)	(0.038)	(0.025)	(0.059)
Observations	47,735	47,735	40,201	47,735	47,735	47,735
q^{EW}	0.007***	0.004***	0.020***	−0.036	0.003	−0.071
	(0.000)	(0.001)	(0.003)	(0.037)	(0.009)	(0.0630)
Cash flow	0.075***	0.092***	0.018***	0.295**	0.154***	0.382**
	(0.004)	(0.006)	(0.007)	(0.127)	(0.026)	(0.179)
Observations	49,581	49,581	42,387	49,581	49,581	49,581
q^{K}	0.007***	0.004***	0.021***	−0.014	−0.026	−0.004
	(0.000)	(0.001)	(0.003)	(0.023)	(0.048)	(0.013)
Cash flow	0.074***	0.093***	0.023***	0.207***	0.245	0.172***
	(0.004)	(0.006)	(0.007)	(0.076)	(0.155)	(0.035)
Observations	49,545	49,545	42,348	49,545	49,545	49,545

ferent proxies for q are incorrect. Our conclusions are shared by recent, independent work by Agca and Mozumdar (2010). Erickson and Whited's claims are, at best, a distraction from the more substantive issues affecting empirical investment models. Both the EW and the OLS-IV estimators produce results that are insensitive to changes in proxies for q, variable definitions for cash flow and investment, and sampling procedures. How can we reconcile the results presented in Erickson and Whited's (2010) Table 4 with those presented here? We conjecture that the difference in results is due to their use of the minimum distance procedure rather than the Fama-MacBeth procedure to aggregate the yearly. Given that they do not report their yearly estimates, nor the Fama-MacBeth results for their Table 4, and more importantly, do not share their data, we are unable to verify this conjecture.

2.6 Misconception about Data Matching

ACG use Monte Carlo simulations to assess the finite sample properties of alternative estimators that account for measurement error. Simulations are useful in this context because they allow one to study different estimators in a controlled setting, where it is possible to compare and contrast elements that affect their performance. Erickson and Whited (2010) claim that ACG simulations are not data relevant because their experiments do not reproduce the moments one observes in standard data sets. While their claim could strike some as a valid concern, we show this observation has no bearing on the conclusions of the ACG study.

Monte Carlo simulations should yield useful recommendations for practitioners. Accordingly, simulation results should be obtained with respect to data generating processes (DGP) that are relevant for practical situations. In this context, dimensions such as sample size and number of replications are important in determining the accuracy of the simulations. With respect to these dimensions, ACG use representative cross-sections and time-series for generating their data, with the number of repetitions set to 10000 (which is standard). Regarding the DGP, ACG use linear models to approximate investment equations. The same linear model with linear errors in variables is used in Erickson and Whited (2000, 2002), which is standard in the investment literature.

Note that in Monte Carlo designs it is possible to re-scale variables and change distributions so as to ensure the simulated data moments will match real data moments. Indeed, when generating data for their experiments, Erickson and Whited (2010) fix the distributions of the variables of interest for the EW estimator using high degree of skewness and calibrate several other free parameters to match the moments of the data. Accordingly, they state: "To arrive at this design, we set $(\delta_\chi, \delta_z, \alpha_0)$ and the covariance matrix of $(v_{it}^\chi, v_{it}^z, v_{it}^u, v_{it}^\epsilon)$ so that the simulated vector (x_{it}, y_{it}, z_{it}) has means and a covariance matrix exactly equal to those of the actual data."

The design used by Erickson and Whited (2010) has several degrees of freedom coming from the free parameters that are chosen to match the data moments. It is possible to change the distributions and find a different set of parameters $(\delta_\chi, \delta_z, \alpha_0)$ and the covariance matrix of $(v_{it}^\chi, v_{it}^z, v_{it}^u, v_{it}^\epsilon)$ to approximate the data moments. While seemingly appealing, these parameter choices moment matching exercises are ultimately *irrelevant* for the simulations Erickson and Whited (2010) carry. Those authors' goal is to study the bias and efficiency of the EW estimator, and as ACG have shown, the important feature of their simulations is the choice of the distributions of the mismeasured and well-measured regressors (χ, z) and their associated skewness.

In what follows, we show that matching data moments is irrelevant for the simulations carried in studies such as ACG. We do this via a simple example. Suppose one is interested in studying the finite sample bias of the OLS-IV estimator in a cross-section setting. The model is given by

$$y = 1 + x\beta + u,$$

where x is an endogenous variable, u is the innovation term, and z is an instrument. The identification condition for β relies on $E[x'z] \neq 0$ and $E[z'u] = 0$. For simplicity, we introduce endogeneity in the model as $x = v + 0.5 \times u$, and define the instrument as $z = v$.

Given the identification condition, it is easy to see that drawing data for (u, v) from a Lognormal, Normal, or Chi-square distributions will have no effect on the object of study, which is the *bias* of the estimator. In addition, the absolute value of the parameter β will be irrelevant. We illustrate this point by drawing data from several different distributions for the variables and considering different parameters for $\beta = (1, 2, 3, 4, 5)$. First, we generate the variables (u, v) from Lognormal, Chi-square with one degree of freedom, and a Normal distribution with $\mu = 0$ and $\sigma^2 = 1$. We generate 1000 samples and compute the bias over 2000 replications. We also compute the first three moments generated by the simulated data for y and x. The results are presented in the first three columns of Table 7. They show that the bias is approximately zero for all cases, showing that neither the absolute value of the parameter β nor the choice of a particular distribution affect the bias of the OLS-IV estimator.

Now suppose the true data process for y and x arises from a Lognormal distribution. The researcher does not know the underlying data generating process, but observes data moments for y and x. To study the bias in the OLS-IV estimator via simulations, she may generate data from a distribution that yields the moments she observes for the variables y and x. Will the choice of distribution in her simulations materially affect her estimates of the bias in the OLS-IV estimator? The answer is no. The researcher could generate data from a different distribution, say from a Normal distribution, choose the parameters μ and σ to approximate the observed moments of y and x, and still correctly measure the bias in the OLS-IV estimator. To show this, we draw data from a normal distribution with $\mu = 2$ and $\sigma^2 = 4$ (Normal$(2, 4)$). The results for the simulations are presented in the last column of Table 7.

The first thing to notice from the last column of Table 7 is that the OLS-IV estimator is ap-

Table 7: Bias in the OLS-IV Estimator and Associated Data Moments

This table present simulation results for bias and first three moments of the generated variables y and x. The model is constructed with a constant, one endogenous regressor, and one instrument. The data are generated using four different distributions for the regressors and innovations: Lognormal, Normal(0,1), Chi-square(1), and Normal(2,4).

	Lognormal	Normal(0,1)	Chi-square(1)	Normal(2,4)
$\beta = 1$	-0.001	0.002	-0.003	-0.001
$\beta = 2$	-0.001	0.002	-0.003	-0.001
$\beta = 3$	-0.001	0.002	-0.003	-0.001
$\beta = 4$	-0.001	0.002	-0.003	-0.001
$\beta = 5$	-0.001	0.002	-0.003	-0.001
y's 1^{st} moment	14.82	0.68	9.40	18.21
y's 2^{nd} moment	350.46	37.92	155.60	478.15
y's 3^{rd} moment	13725.01	68.63	3631.82	14096.21
x's 1^{st} moment	2.52	-0.06	1.47	2.94
x's 2^{nd} moment	10.74	1.26	4.36	13.63
x's 3^{rd} moment	84.91	-0.29	18.52	72.83

proximately unbiased for all β. Secondly, although the distribution used to simulate the data (the Normal$(2,4)$) is very different from the true data process (Lognormal), the moments of the y and x are similar ("matched"). For example, the first moment of y is 15 for the Lognormal case and 18 for Normal$(2,4)$, while the third moments of y are 13725 and 14096 for Lognormal and Normal$(2,4)$, respectively. Likewise, the three moments of x are quite similar across the two distributions. The conclusion is that when analyzing the bias of the OLS-IV estimator, real data features such as "moment matching" or "true levels of β" are immaterial to understanding for the object of study. In this particular case, the important question is how strong is the correlation between x and z. It is this correlation that determines the model's identification.

Note that the Lognormal and Normal$(2,4)$ distributions have very different statistical properties. The first is an asymmetric distribution with high skewness while the second is a symmetric distribution (zero skewness). As we have shown, however, it is possible to find similar moments for y and x across these distributions because there are free parameters to choose (μ and σ^2) that approximate those moments. In this particular example, features of the distributions are irrelevant for the identification of the estimator. For the evaluation of biases in the EW estimator, on the other hand, these considerations are more important (as shown in ACG). The irrelevance of the "moment matching" approach is, however, still valid: one could approximate the moments for observed data from several different distributions. For instance, Erickson and Whited (2010) generate the unobserved mismeasured variable as $\chi_{it} = \delta_\chi + \phi_\chi \chi_{it-1} + v_{it}^\chi$, where v_{it}^χ is a Normal raised to the fourth power (this is done to enhance the performance of the EW estimator). However, one could have used a less skewed distribution to generate v_{it}^{χ} and adjust the free parameters of the model to match the data moments, and if necessary change also the distributions of the innovations u_{it} and ε_{it}.

This section shows that reproducing moments from the data is not a requirement for simulation studies assessing the robustness of alternative estimation methods. As ACG demonstrate, it is important to investigate the sensitive of the different estimators to different environments. We conclude that the "data irrelevant" simulations in ACG are correct and important in helping understand the differences among estimators dealing with measurement error problems.

3 Concluding Remarks

Almeida, Campello, and Galvao (2010) compare alternative measurement-error consistent estimators to assess their usefulness and reliability in practical applications. Those authors' analysis shows that, under special circumstances, the higher order moment estimator proposed by Erickson and Whited (2000, 2002) performs as well as other better-known alternatives. However, the ACG study also shows that the performance of the EW estimator drops dramatically when one has to deal with fixed effects and error heteroskedasticity. The performance of other estimators, in contrast, are less affected by those issues. ACG further show that the EW estimator is very sensitive to distributional assumptions involving the skewness of the measurement error process.

A recent paper by Erickson and Whited (2010) delivers a self-serving defense of higher order moment estimators. The authors do so by casting doubt on every aspect of the ACG analysis concerning the EW estimator, ignoring any informed discussion of alternative methods. This paper shows that the Erickson and Whited analysis is one-sided and their study fails to lend any additional credibility to the EW estimator. If anything, the Erickson and Whited paper raises additional concerns about that estimator and shows that it is dominated by alternative methods. We hope that future work on the problem of econometric inference in the presence of measurement errors focuses more on positive aspects. Researchers should debate the relevant issues in a dispassionate, less self-interested fashion bearing in mind the ultimate goal of helping empirical economists. The ACG study was conducted in a constructive, transparent manner. The results from that study should be discussed in the same way.

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Table A.1: EW coefficients for real data (q^{MB})

This table shows the coefficients and standard deviations that we obtain when we use the EW estimator in investment equation, estimated year-by-year, using q^{MB} as Tobin's q proxy. The table also shows the results for the EW estimator associated with EW-GMM3, EW-GMM4 and EW-GMM5. The table also shows the EW coefficients for the data that is treated for fixed effects via the within transformation. The data are taken from the annual COMPUSTAT industrial files over the 1972 to 2005 period. See text for details.

	q coefficient			*Cash flow* coefficient		
Year	EW-GMM3	EW-GMM4	EW-GMM5	EW-GMM3	EW-GMM4	EW-GMM5
1973	-0.036	-0.081	0.000	0.369	0.470	0.291
	(0.082)	(0.095)	(0.106)	(0.203)	(0.248)	(0.253)
1974	-0.002	0.000	0.000	0.215	0.214	0.214
	(0.071)	(0.034)	(0.185)	(0.057)	(0.041)	(0.160)
1975	0.231	0.437	0.587	0.038	-0.124	-0.242
	(0.260)	(0.372)	(0.366)	(0.209)	(0.311)	(0.311)
1976	-0.122	-0.146	-0.085	0.307	0.325	0.278
	(0.123)	(0.054)	(0.037)	(0.104)	(0.064)	(0.054)
1977	-0.195	-0.001	-0.155	0.418	0.283	0.390
	(0.213)	(0.553)	(0.110)	(0.153)	(0.390)	(0.086)
1978	-0.041	-0.215	-0.159	0.373	0.483	0.448
	(0.118)	(0.095)	(0.071)	(0.073)	(0.092)	(0.079)
1979	-0.490	0.007	-0.241	0.682	0.241	0.461
	(1.536)	(0.041)	(0.137)	(1.351)	(0.050)	(0.119)
1980	0.094	0.090	0.110	0.191	0.196	0.167
	(0.035)	(0.021)	(0.039)	(0.064)	(0.049)	(0.074)
1981	0.129	0.370	0.323	0.207	-0.070	-0.016
	(0.230)	(0.195)	(0.076)	(0.267)	(0.234)	(0.114)
1982	0.050	0.064	0.173	0.250	0.234	0.104
	(0.056)	(0.033)	(0.076)	(0.070)	(0.053)	(0.110)
1983	0.104	0.145	0.213	0.169	0.120	0.039
	(0.064)	(0.074)	(0.066)	(0.081)	(0.094)	(0.083)
1984	-0.720	-0.006	-2.841	0.590	0.159	1.869
	(2.594)	(0.026)	(2.980)	(1.626)	(0.042)	(1.907)
1985	0.009	0.196	0.205	0.171	0.041	0.035
	(0.326)	(0.176)	(0.054)	(0.224)	(0.115)	(0.051)
1986	0.136	0.120	0.133	0.114	0.125	0.116
	(0.050)	(0.015)	(0.009)	(0.048)	(0.030)	(0.028)
1987	-0.073	-0.161	-0.044	0.187	0.241	0.168
	(0.072)	(0.059)	(0.199)	(0.062)	(0.069)	(0.114)
1988	-0.145	-0.192	-0.177	0.232	0.255	0.247
	(0.057)	(0.038)	(0.012)	(0.042)	(0.048)	(0.038)
1989	0.664	0.114	0.142	-0.271	0.098	0.080
	(1.448)	(0.059)	(0.040)	(0.969)	(0.046)	(0.037)
1990	0.398	0.094	0.088	-0.144	0.041	0.045
	(0.390)	(0.052)	(0.032)	(0.242)	(0.040)	(0.033)
1991	0.019	0.036	0.115	0.082	0.063	-0.022
	(0.028)	(0.012)	(0.025)	(0.041)	(0.025)	(0.040)
1992	0.013	0.044	0.148	0.107	0.080	-0.011
	(0.050)	(0.032)	(0.055)	(0.046)	(0.037)	(0.059)
1993	0.048	0.128	0.074	0.058	0.004	0.040
	(0.058)	(0.104)	(0.024)	(0.042)	(0.068)	(0.029)
1994	0.411	0.329	0.121	-0.049	-0.013	0.079
	(3.326)	(0.178)	(0.049)	(1.472)	(0.097)	(0.032)
1995	0.208	0.191	0.139	-0.040	-0.027	0.015
	(0.155)	(0.066)	(0.040)	(0.137)	(0.065)	(0.033)
1996	0.010	0.150	0.000	0.133	0.063	0.137
	(0.078)	(0.067)	(0.050)	(0.040)	(0.038)	(0.025)
1997	-0.556	-0.270	-0.191	0.460	0.287	0.240
	(1.536)	(0.139)	(0.105)	(0.925)	(0.094)	(0.069)
1998	-0.169	-0.160	-0.320	0.202	0.195	0.311
	(2.569)	(0.090)	(0.197)	(1.867)	(0.073)	(0.154)
1999	0.068	0.037	0.067	-0.023	0.021	-0.021
	(0.026)	(0.015)	(0.024)	(0.044)	(0.031)	(0.041)
2000	0.033	0.032	0.016	0.065	0.066	0.076
	(0.024)	(0.009)	(0.003)	(0.025)	(0.018)	(0.018)
2001	-0.153	-0.020	-0.124	0.124	0.050	0.108
	(0.173)	(0.068)	(0.077)	(0.101)	(0.040)	(0.047)
2002	-0.399	-0.304	-0.273	0.267	0.212	0.193
	(0.256)	(0.094)	(0.077)	(0.161)	(0.067)	(0.055)
2003	0.872	0.171	0.069	-0.343	-0.020	0.026
	(8.471)	(0.062)	(0.015)	(3.885)	(0.036)	(0.020)
2004	0.127	0.108	0.123	-0.013	-0.003	-0.011
	(0.054)	(0.029)	(0.029)	(0.028)	(0.019)	(0.021)
2005	0.112	0.079	0.083	-0.038	-0.014	-0.017
	(0.080)	(0.015)	(0.018)	(0.057)	(0.015)	(0.017)
Fama-MacBeth	0.0192	0.0420	-0.0509	0.1542	0.1302	0.1769
Standard Error	0.0539	0.0299	0.0928	0.0380	0.0253	0.0591

Table A.2: EW coefficients for real data (q^{EW})

This table shows the coefficients and standard deviations that we obtain when we use the EW estimator in investment equation, estimated year-by-year, using q^{EW} as Tobin's q proxy. The table also shows the results for the EW estimator associated with EW-GMM3, EW-GMM4 and EW-GMM5. The table also shows the EW coefficients for the data that is treated for fixed effects via the within transformation. The data are taken from the annual COMPUSTAT industrial files over the 1972 to 2005 period. See text for details.

	q coefficient			*Cash flow* coefficient		
Year	EW-GMM3	EW-GMM4	EW-GMM5	EW-GMM3	EW-GMM4	EW-GMM5
1973	0.009	0.026	0.002	0.230	0.111	0.276
	(0.017)	(0.008)	(0.018)	(0.140)	(0.067)	(0.149)
1974	0.000	0.022	0.015	0.215	0.171	0.186
	(0.020)	(0.012)	(0.020)	(0.050)	(0.040)	(0.051)
1975	0.044	0.114	0.154	0.123	-0.038	-0.129
	(0.065)	(0.062)	(0.067)	(0.145)	(0.162)	(0.180)
1976	-0.017	-0.055	-0.016	0.254	0.352	0.252
	(0.053)	(0.032)	(0.019)	(0.135)	(0.092)	(0.070)
1977	-0.039	0.000	-0.052	0.371	0.280	0.402
	(0.066)	(0.048)	(0.029)	(0.154)	(0.117)	(0.082)
1978	-0.052	-0.071	-0.076	0.464	0.506	0.519
	(0.047)	(0.036)	(0.041)	(0.102)	(0.096)	(0.101)
1979	0.336	0.035	0.535	-0.692	0.153	-1.250
	(0.580)	(0.049)	(0.242)	(1.640)	(0.130)	(0.667)
1980	0.057	0.077	0.065	0.078	-0.012	0.040
	(0.033)	(0.032)	(0.025)	(0.162)	(0.152)	(0.127)
1981	0.117	0.119	0.085	-0.080	-0.085	0.038
	(0.185)	(0.046)	(0.022)	(0.674)	(0.182)	(0.107)
1982	0.030	0.017	0.082	0.203	0.253	0.020
	(0.020)	(0.004)	(0.068)	(0.079)	(0.031)	(0.263)
1983	0.062	0.061	0.072	0.067	0.070	0.032
	(0.027)	(0.018)	(0.010)	(0.117)	(0.080)	(0.059)
1984	-0.133	0.000	-1.785	0.524	0.158	5.102
	(0.210)	(0.008)	(76.332)	(0.652)	(0.041)	(211.340)
1985	0.024	0.036	0.058	0.111	0.078	0.014
	(0.014)	(0.010)	(0.019)	(0.049)	(0.045)	(0.052)
1986	0.038	0.037	0.036	0.080	0.085	0.087
	(0.008)	(0.004)	(0.003)	(0.043)	(0.037)	(0.032)
1987	-0.040	-0.074	-0.014	0.256	0.351	0.180
	(0.028)	(0.029)	(0.014)	(0.096)	(0.106)	(0.055)
1988	-0.054	-0.048	-0.051	0.319	0.302	0.310
	(0.017)	(0.006)	(0.002)	(0.058)	(0.035)	(0.032)
1989	-1.082	0.042	-0.045	4.009	0.023	0.334
	(15.104)	(0.036)	(0.014)	(53.305)	(0.130)	(0.057)
1990	-0.112	-0.146	-0.221	0.419	0.517	0.732
	(0.209)	(0.127)	(0.154)	(0.627)	(0.372)	(0.520)
1991	0.001	0.000	0.000	0.094	0.099	0.099
	(0.017)	(0.006)	(0.004)	(0.086)	(0.029)	(0.023)
1992	-0.007	0.000	-0.019	0.141	0.114	0.191
	(0.013)	(0.006)	(0.012)	(0.053)	(0.024)	(0.041)
1993	0.013	0.005	0.026	0.048	0.074	0.006
	(0.011)	(0.002)	(0.015)	(0.038)	(0.020)	(0.053)
1994	-0.013	-0.014	0.000	0.160	0.161	0.132
	(0.010)	(0.006)	(0.013)	(0.023)	(0.020)	(0.034)
1995	0.025	0.033	0.020	0.017	-0.016	0.042
	(0.018)	(0.013)	(0.010)	(0.080)	(0.067)	(0.043)
1996	-0.003	0.000	-0.014	0.146	0.137	0.180
	(0.009)	(0.005)	(0.005)	(0.029)	(0.020)	(0.021)
1997	-0.215	-0.039	-0.846	0.828	0.250	2.893
	(0.580)	(0.010)	(13.361)	(1.872)	(0.043)	(43.518)
1998	-0.020	0.000	-0.048	0.144	0.082	0.234
	(0.028)	(0.031)	(0.012)	(0.089)	(0.097)	(0.050)
1999	0.009	0.004	0.004	0.015	0.047	0.048
	(0.004)	(0.001)	(0.001)	(0.032)	(0.019)	(0.017)
2000	0.012	0.012	0.004	0.032	0.029	0.067
	(0.006)	(0.005)	(0.002)	(0.034)	(0.033)	(0.018)
2001	-0.014	-0.045	-0.017	0.093	0.201	0.105
	(0.017)	(0.014)	(0.011)	(0.065)	(0.067)	(0.045)
2002	-0.049	-0.043	-0.085	0.190	0.172	0.308
	(0.023)	(0.013)	(0.030)	(0.084)	(0.051)	(0.108)
2003	0.257	0.029	0.013	-0.745	-0.033	0.018
	(1.917)	(0.009)	(0.002)	(5.995)	(0.036)	(0.019)
2004	0.009	0.021	0.034	0.012	-0.017	-0.050
	(0.011)	(0.004)	(0.008)	(0.043)	(0.020)	(0.028)
2005	0.016	0.016	0.021	-0.020	-0.016	-0.038
	(0.006)	(0.003)	(0.004)	(0.028)	(0.018)	(0.023)
Fama-MacBeth	-0.0358	0.0035	-0.0710	0.2953	0.1542	0.3817
Standard Error	0.0370	0.0092	0.0630	0.1275	0.0259	0.1787

Table A.3: EW coefficients for real data (q^K)

This table shows the coefficients and standard deviations that we obtain when we use the EW estimator in investment equation, estimated year-by-year, using q^K as Tobin's q proxy. The table also shows the results for the EW estimator associated with EW-GMM3, EW-GMM4 and EW-GMM5. The table also shows the EW coefficients for the data that is treated for fixed effects via the within transformation. The data are taken from the annual COMPUSTAT industrial files over the 1972 to 2005 period. See text for details.

	q coefficient			*Cash flow* coefficient		
Year	EW-GMM3	EW-GMM4	EW-GMM5	EW-GMM3	EW-GMM4	EW-GMM5
1973	0.009	0.024	0.002	0.231	0.120	0.279
	(0.015)	(0.008)	(0.019)	(0.128)	(0.066)	(0.157)
1974	-0.002	0.000	0.000	0.219	0.215	0.216
	(0.020)	(0.012)	(0.013)	(0.052)	(0.042)	(0.044)
1975	0.062	0.125	0.150	0.074	-0.077	-0.136
	(0.080)	(0.073)	(0.066)	(0.193)	(0.190)	(0.173)
1976	-0.028	-0.063	-0.056	0.283	0.375	0.357
	(0.065)	(0.037)	(0.029)	(0.171)	(0.101)	(0.088)
1977	-0.086	0.000	-0.064	0.503	0.280	0.446
	(0.095)	(0.034)	(0.031)	(0.245)	(0.094)	(0.095)
1978	0.002	0.000	0.000	0.343	0.348	0.348
	(0.041)	(0.327)	0.000	(0.076)	(0.652)	(0.123)
1979	0.325	0.510	0.143	-0.722	-1.274	-0.177
	(0.671)	(0.319)	(0.080)	(2.016)	(0.953)	(0.243)
1980	0.049	0.070	0.061	0.101	0.007	0.047
	(0.027)	(0.028)	(0.021)	(0.139)	(0.141)	(0.116)
1981	0.100	0.086	0.084	-0.027	0.025	0.033
	(0.316)	(0.036)	(0.017)	(1.187)	(0.153)	(0.088)
1982	0.016	0.020	0.014	0.250	0.233	0.257
	(0.016)	(0.008)	(0.003)	(0.063)	(0.044)	(0.035)
1983	0.057	0.052	0.065	0.073	0.092	0.040
	(0.026)	(0.016)	(0.008)	(0.115)	(0.074)	(0.054)
1984	-0.078	0.000	-0.015	0.366	0.157	0.197
	(0.115)	(0.022)	(0.028)	(0.365)	(0.063)	(0.096)
1985	0.032	0.040	0.041	0.084	0.061	0.057
	(0.019)	(0.014)	(0.008)	(0.066)	(0.056)	(0.045)
1986	0.038	0.035	0.036	0.074	0.084	0.081
	(0.008)	(0.004)	(0.004)	(0.044)	(0.036)	(0.035)
1987	-0.026	-0.059	-0.093	0.203	0.281	0.361
	(0.022)	(0.028)	(0.021)	(0.071)	(0.104)	(0.114)
1988	-0.061	-0.048	-0.053	0.333	0.294	0.309
	(0.022)	(0.008)	(0.003)	(0.072)	(0.036)	(0.032)
1989	-0.604	-0.050	-0.125	2.258	0.345	0.604
	(4.676)	(0.025)	(0.129)	(16.001)	(0.100)	(0.445)
1990	-0.149	-0.150	-0.262	0.515	0.518	0.833
	(0.356)	(0.121)	(0.212)	(1.038)	(0.347)	(0.675)
1991	0.000	0.000	0.000	0.097	0.099	0.099
	(0.016)	(0.005)	(0.005)	(0.077)	(0.028)	(0.024)
1992	-0.005	0.000	-0.017	0.136	0.114	0.188
	(0.014)	(0.003)	(0.010)	(0.060)	(0.019)	(0.037)
1993	0.014	0.006	0.026	0.049	0.072	0.015
	(0.011)	(0.002)	(0.012)	(0.036)	(0.022)	(0.044)
1994	-0.007	-0.014	0.000	0.146	0.159	0.132
	(0.009)	(0.004)	(0.012)	(0.023)	(0.019)	(0.033)
1995	0.022	0.031	0.021	0.038	-0.003	0.043
	(0.012)	(0.014)	(0.010)	(0.052)	(0.068)	(0.040)
1996	-0.002	0.000	-0.014	0.142	0.137	0.179
	(0.009)	(0.007)	(0.005)	(0.029)	(0.023)	(0.021)
1997	-0.161	-1.465	-0.040	0.653	4.941	0.256
	(0.301)	(71.805)	(0.003)	(0.968)	(236.009)	(0.030)
1998	-0.019	0.000	-0.049	0.140	0.082	0.229
	(0.029)	(0.034)	(0.013)	(0.087)	(0.103)	(0.050)
1999	0.010	0.005	0.005	0.012	0.047	0.048
	(0.004)	(0.001)	(0.001)	(0.032)	(0.019)	(0.017)
2000	0.010	0.008	0.004	0.048	0.054	0.071
	(0.005)	(0.003)	(0.001)	(0.026)	(0.021)	(0.018)
2001	-0.012	-0.041	-0.035	0.084	0.184	0.165
	(0.017)	(0.015)	(0.010)	(0.063)	(0.065)	(0.048)
2002	-0.109	-0.046	-0.040	0.350	0.166	0.149
	(0.165)	(0.014)	(0.012)	(0.513)	(0.052)	(0.047)
2003	0.101	0.028	0.013	-0.219	-0.020	0.023
	(0.290)	(0.009)	(0.002)	(0.801)	(0.034)	(0.019)
2004	0.013	0.020	0.032	0.003	-0.015	-0.044
	(0.008)	(0.004)	(0.007)	(0.035)	(0.021)	(0.027)
2005	0.015	0.016	0.021	-0.013	-0.016	-0.034
	(0.005)	(0.003)	(0.004)	(0.026)	(0.018)	(0.022)
Fama-MacBeth	-0.0144	-0.0261	-0.0044	0.2069	0.2450	0.1718
Standard Error	0.0234	0.0483	0.0128	0.0760	0.1547	0.0354