Panel data estimation in finance: parameter consistency and the standard error sideshow*

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ABSTRACT

We examine the empirical validity of a key assumption that is necessary for common panel data estimation techniques to generate consistent parameter estimates. In a large set of canonical empirical finance models, we show that this assumption of strict exogeneity is almost always violated. This implies that many studies are not estimating the true parameter of interest, and larger samples or adjustments to the standard errors cannot ever hope to solve the problem. We discuss reasons why strict exogeneity is unlikely to hold in many common finance settings, and we offer evidence on the possible economic magnitude of the resulting estimation errors. While we cannot offer a comprehensive solution to this problem, we suggest an approach that exploits industry-year variation in the explanatory variable of interest. We provide some initial evidence on the usefulness of this approach in a specific setting.

1. Introduction

The use of panel data is extremely common in finance research. An important benefit of the panel structure is that it allows researchers to control for omitted unit-level factors that do not vary over time but may be arbitrarily correlated with the explanatory variables of interest. In many of these settings, the firm-year is the unit of observation and panel data estimation techniques are intended to control for the presence of time-invariant firm fixed effects. The most common panel data estimator in the recent finance literature is the fixed-effects estimator. However, other cousins of this estimator are also sometimes employed.

An important stream of recent research highlights common errors that appear in the finance literature when the fixed-effects estimator is applied to panel data. First, as has been highlighted by Gormley and Matsa (2014), many researchers often do not calculate the fixed-effects estimator correctly because of errors in the process of transforming the variables that enter the regression equation. Second, as highlighted by Petersen (2009) and Thompson (2011), researchers often use standard errors that do not adequately adjust for the types of error variance and covariance structures that are common in finance settings. There are standard solutions/approaches to these problems that are well known in the econometrics literature (see Wooldridge (2010)). These solutions can be implemented by transforming the data in a way that is consistent with the underlying model of interest and by using the appropriate estimation commands and options in standard microeoconometric software packages such as Stata (see Cameron and Trivedi (2010)). In some cases, for example fixed effects along multiple dimensions, certain data transformations and programming steps may be necessary to yield the desired estimates (see Gormley and Matsa (2014)).

While this recent literature makes many useful and important points, it does not emphasize a fundamental assumption that must be true for the fixed-effects (FE) estimator, or its cousin, the first-difference (FD) estimator, to have any hope of consistently estimating the coefficients of interest. Consistency of these common estimators requires reliance on a strict exogeneity assumption. This is a much stronger requirement than the typical notion of contemporaneous exogeneity, which (loosely) only requires a lack of contemporaneous correlation between the error term and the explanatory variables. In particular, as articulated by Wooldridge (2010), strict exogeneity effectively requires there to be no feedback from the dependent variable to future values of the independent variable. Even a cursory consideration of the variables used in finance research suggests that this assumption will often be violated. Many of the dependent variables of interest to financial economists, for example firm performance/returns, leverage, and compensation are almost surely related to the subsequent evolution of the explanatory variables of interest such as firm size, risk, or governance characteristics. In fact, many dynamic theoretical models posit exactly this type of feedback.[1]

In this paper we examine the strict exogeneity assumption in a set of canonical panel-data regression models selected from the existing finance literature. For each of these models we: (a) formally test whether the strict exogeneity assumption holds, and (b) explore whether failures in the strict exogeneity assumption are likely to lead to substantive inconsistencies in common estimators. We present overwhelming evidence that the strict exogeneity assumption is, in fact, quite commonly violated. In fact, when we use large samples, we can reject the validity of the strict exogeneity assumption in virtually all of the canonical regression models we consider.

[1] For representative models with an implicit or explicit focus on dynamics and feedback between variables, see for example, Fischer, Heinkel, and Zechner (1989), Hermalin and Weisbach (1998), Hennessy and Whited (2005), and Strebulaev and Whited (2012).

Thus, there is little hope that the common FE (or FD) estimates that appear in much of the associated finance literature are consistently estimating the parameters of interest. If the estimates cannot be expected to converge to their true values when the number of cross-sectional units (N) grows without bound, any concerns about the nuances of the standard error calculation would appear to be a relative side-show.

It is difficult to make strong statements on the size of the resulting parameter estimation inconsistencies without knowing more about the underlying structural dynamics. However, we do uncover several facts that suggest that this problem can have a significant effect on economic inferences in finance settings. First, we note that the problem of inconsistency in the FE estimator is known to be on the order of 1/T, where T is the number of time periods, suggesting that the problem may become small when T is large (see Nickell (1981)). Unfortunately, this result depends on the presence of stable (i.e., time-invariant) fixed effects. As we demonstrate, in common finance panel settings, unit-level fixed effects do appear to change over time, perhaps because of occasional discrete changes to the management, ownership, or governance of firms. Thus, in the minority of settings in which a large number of time periods are even available, it appears unlikely that the 1/T result will solve the problem.

To gauge the possible magnitude of inference errors, we consider the relative variation in the FE and FD estimates. Under strict exogeneity, these two estimates asymptotically converge to the same true underlying parameter value. If strict exogeneity is violated, as frequently appears to be the case, these estimators have different probability limits, neither of which is the true parameter value of interest. In the settings we examine, we find that the difference between the FE and FD estimators can be quite large, with differences on the order of 50% or more being quite common. Moreover, there are some cases in which these estimators are significant and of opposite sign. These pathological cases are on the order of 12 times more prevalent than would be suggested by chance even under the most conservative assumptions. Thus, taken as a whole, our evidence suggests that a large portion of prior research uses estimation methods that lead to inconsistent estimates and these inconsistencies can be substantive. Thus, even if a researcher carefully follows the recommendation of the related recent literature and has access to a dataset of almost infinite size, in many cases they will likely estimate something that differs substantively from the true parameter of interest.

Our findings offer a major challenge to empirical finance research as they indicate that simple FE or FD panel data estimators are in many cases not the correct tools to use in settings that include the presence of unit-level fixed effects. At the very least, our evidence suggests that one should test the strict exogeneity assumption in <u>all settings</u> before proceeding with these estimators. If these tests reject, as appears to commonly be the case, one can either settle for an inconsistent estimator, an unappealing option, or turn to alternative estimators using either the data at hand (the internal option) and/or techniques that exploit additional outside information (the external option).

With regard to the internal option, there are a variety of different estimators that have been suggested, mostly of the GMM variety. While it is beyond the scope of our paper to comment on these specific estimators, in the spirit of our analysis it is worth noting that common GMM estimators utilized in finance panel settings (e.g., Arellano Bond (1991), Blundell and Bond (1998)) also rely on testable assumptions related to the suitability of those methods. In particular, those methods usually maintain an assumption of no serial correlation, an assumption that can be tested. A recent paper by Dang, Kim, and Shin (2015) demonstrates that these tests quite frequently reject, at least in the context of dynamic capital structure research. This suggests that the GMM internal option may at times, unfortunately, offer little or no improvement over the more traditional alternatives.[2]

The external option where new information is brought into a panel to identify the effect of interest is, of course, usually the most desired course of action. The challenge is in identifying good instruments that both satisfy the exclusionary restriction and the relevancy condition. While some notable successes along these lines have appeared in the finance literature, there is little of generality that can be said about this approach as it usually relies on special (often one-time) events such as exogenous shocks to a firms' economic, legal, regulatory, or tax environment.[3] In many contexts, shocks/instruments of this type are not readily apparent.

In an effort to offer some constructive guidance to finance researchers given the econometric challenges we highlight, we suggest a quasi-external approach to identification that exploits industry-year variation in the explanatory variable of interest as an instrument for firm-level variation. Recent research has emphasized industry-year variation as either a potential nuisance factor to be controlled for (Gormley and Matsa (2014)), or as a source of variation to test whether theoretically irrelevant factors may affect economic decisions (e.g., Jenter and Kanaan (2014)). We suggest that in some cases industry-year variation is not a nuisance, but rather can be viewed as useful and theoretically relevant if exploited as an instrument for the underlying firm-level explanatory variable of interest. Of course the usefulness of this approach will depend on the specific context, an issue we discuss below.

[2] The finite sample properties of panel GMM estimators can also be quite poor (see Wooldridge (2010)), adding to the recommendation for caution in exploiting this internal approach. See also the comments by Roberts and Whited (2012) on the use and limitations of GMM techniques in different finance settings.

[3] For some interesting recent successes in applying this strategy, see Matsa (2010) and Agrawal and Matsa (2013).

To illustrate the potential of this approach, we consider the role of firm risk in determining a firm's level of managerial ownership. This is a context in which we find evidence casting doubt on the strict exogeneity assumption, and even the weaker requirement of contemporaneous exogeneity is questionable given the possible contemporaneous feedback effect from ownership to risk taking (see Tufano (1996)). As we would expect if firm risk is partially driven by industry shocks, we find a strong positive relation between innovations in industry risk and firm risk, where the firm is excluded in the industry calculation. Thus, industry-risk innovations certainly appear to satisfy the relevancy condition. We argue that the exclusionary restriction is also very likely to hold in this context, as feedback from innovations in firm ownership structure to industry risk would appear to be negligible, particularly when we exclude the largest firms from the sample.

When we proceed to instrument for firm-risk innovations with industry-risk innovations, our evidence indicates a significant negative role for risk in the determination of managerial ownership levels. While this result is interesting in its own right, for our purposes the more important point is that it suggests that exploiting industry-level innovations in explanatory variables of interest to achieve convincing identification in panel data contexts in finance may be a productive strategy. Given our evidence that many of the widely used panel-data approaches rely on assumptions that are rejected by the data, this would appear to be a particularly useful strategy to exploit in some settings, particularly when the other identification approaches discussed by Roberts and Whited (2012) are not feasible or appropriate.

The rest of this paper is organized as follows. In Section 2 we outline the basic econometric issues, survey the existing literature, and characterize current practice in the finance literature. In section 3 we outline our specific tests and apply these tests to a large set of models and model permutations selected from the recent finance literature. In section 4 we discuss estimation when the strict exogeneity assumption is rejected and illustrate the potential use of exploiting industry-year variation some of these cases. Section 5 concludes.

2. Prior literature and empirical strategy

2.1 Outlining the problem

While the strict (also called strong) exogeneity assumption is discussed in textbook treatments of panel data, with a couple of notable exceptions, this assumption is almost never acknowledged or addressed in finance panel-data applications. Given this lack of familiarity to finance audiences, we briefly outline the issue here with a specific eye towards finance applications. The reader is referred to textbook treatments for more of the technical details (e.g., Cameron and Trivedi (2005), Wooldridge (2010)).

We consider a simple regression model with a dependent variable y, a single independent variable of interest x, and an assumed model in which $y_{it} = \alpha_i + \beta x_{it} + \epsilon_{it}$, where i denotes an arbitrary cross-sectional unit (from 1 to N) and t denotes an arbitrary time period (from 1 to T). Since in panel finance applications N is usually much larger than T, all asymptotics are for N approaching infinity. Following Wooldridge (2010), we will refer to the assumption $E(\epsilon_{it} \mid x_{it}, \alpha_i) = 0$ as the contemporaneous exogeneity assumption and $E(\epsilon_{it} \mid x_{is}, \alpha_i) = 0$ for all t and s as the strict exogeneity assumption. Assuming contemporaneous exogeneity holds, and recognizing that lagged explanatory variables can always be added to the model, we are concerned primarily with violations of strict exogeneity in which $E(\epsilon_{it} \mid x_{is}, \alpha_i) \neq 0$ for $s > t$. To see how this assumption may be violated, consider the case in which high realizations of the dependent variable at time t (say firm performance) have a positive effect on subsequent levels of the explanatory variable (say managerial ownership). In this case, strict exogeneity would be violated because $E(\epsilon_{it} x_{i(t+1)} \mid \alpha_i) > 0$; higher values of this period's performance are associated with higher levels of next period's ownership.

To understand the resulting bias in the FE and FD estimator, consider the simple case in which we have two time periods (call them 1 and 2), perhaps several years apart, so that the FE and FD estimators are numerically identical and the resulting parameter estimate for β is derived from a simple linear regression of changes in y (in our example performance) on changes in x (in our example ownership). Suppose also that the true β is 0 in which case there is no causal effect of x on y. If we regress ($\Delta y = y_2\text{-}y_1$) on ($\Delta x = x_2\text{-}x_1$), the only systematic variation in the data will arise from high (low) y_1 values tending to feed back to high (low) x_2 values. This will result in an apparent negative correlation between Δy and Δx and will yield (asymptotically with probability 1) a spurious negative estimated β coefficient. Extensions of this argument apply to upward and downward inconsistencies in parameter estimates that depend on the sign of the actual coefficient (when $\beta \neq 0$) and the sign of the dynamic feedback from the dependent variable to subsequent values of the explanatory variable(s). Clearly multivariate models and multiple time periods make it more difficult to sign and understand the resulting bias.

Given the potential seriousness of this issue, it is not surprising that Wooldridge (2010) and other econometric treatments emphasize the importance of testing for strict exogeneity before relying on FE or FD estimation procedures. However, as we show below, researchers in finance relying on FE or FD estimation almost never test for strict exogeneity. Unfortunately, we show that these tests will very commonly reject the strict exogeneity assumption, in which case the reported estimates will be inconsistent and should therefore be viewed with suspicion.

2.2 Recent/current practice

To characterize current practice, we search through every issue of the *Journal of Finance*, *Journal of Financial Economics*, and *Review of Financial Studies* from 2006 to 2013 for the mention of the word fixed effect (or a synonym).[4] We quickly scan each flagged article to determine whether the paper features an empirical model with unit-level (e.g., firm, bank, person) fixed effects rather than solely time (e.g., year, quarter, etc.) effects. We placed each paper into non-mutually exclusive categories based on whether the authors report (a) traditional FE estimates, (b) traditional FD estimates, and/or (c) some version of a dynamic panel GMM estimate. We do not categorize models that rely on external instruments or natural experiments, as our focus is on evaluating models in which this type of external information is not exploited.

Our procedure flags 251 articles that report unit-level FE (222) or FD (47) estimates, and 17 report GMM estimates. If we exclude models that include lagged dependent variables, the corresponding numbers are 240, 216, 44, and 6 respectively. Clearly these figures indicate that FE is the most popular estimation procedure. In all of the papers that report solely FE estimates, only 3 mention the word strict or strong exogeneity, and of these only a single paper actually tests the strict exogeneity assumption.[5] Clearly the field has either not widely recognized this issue, or perhaps the field is collectively hopeful that any resulting inconsistencies are not large enough in magnitude to substantively change the economic inferences of interest.

2.3 Prior work in finance that accounts for violations of strict exogeneity

[4] We used some articles for early 2014, with the cutoff for each journal depending on our access to online issues via our institutions' library access at the time of the data collection.

[5] Interestingly in the 11 papers that utilize the Arellano and Bond (1991) and Blundell and Bond (1998) panel data GMM procedures, only 4 actually test the serial correlation assumption that is necessary for these estimators to yield consistent estimates.

As is well known, any panel data analysis that includes a lagged dependent variable as a control variable must violate the strict exogeneity assumption (i.e., there is no need to test for strict exogeneity, it is structurally violated in the underlying the model). The most prominent area in finance in which this is recognized is in the dynamic capital structure literature, as current leverage is usually assumed to be partially governed by past leverage. Since conventional FE and FD estimators are inconsistent in this context, a relatively recent literature has exploited variants of the dynamic GMM approach with different identifying assumptions to estimate the parameters of interest in this setting, with much debate on the merits of different approaches.[6] We have little to offer here, except to note that testing the underlying assumptions in a GMM estimation is also called for whenever feasible. Recent evidence by Dang, Kim, and Shin (2015) suggest that these assumptions are often rejected in the dynamic capital structure framework.

As we discuss earlier, the strict exogeneity issue is almost entirely unacknowledged in panel data finance models that do not include a lagged dependent variable. The one notable exception is the important work of Wintoki, Linck, and Netter (2013). Those authors highlight the importance of the strict exogeneity issue in one specific setting, namely the effect of board structure on firm performance. They test for strict exogeneity in this context and reject the validity of this assumption, leading them to question prior work on this issue that relies on (inconsistent) FE or FD estimators. They proceed to use a dynamic panel GMM framework and show that the assumptions underlying this GMM estimation are not rejected in standard tests, although they do caution the reader with regard to test power and other possible untestable limitations.

[6] For a comprehensive analysis of the features of different estimators in finance models with lagged dependent variables, see Flannery and Hankins (2013). For a skeptical view of popular GMM estimators in a dynamic capital structure context, see Iliev and Welch (2011).

Our paper is similar in many ways to Wintoki, Linck, and Netter (2013), but they restrict attention to one specific research context. The distinguishing feature of our study is that we highlight that this issue applies to a large set of empirical models in finance and show that the strict exogeneity assumption is routinely rejected in finance panel data models even when there is no lagged dependent variable. Thus, the concern raised by Wintoki, Linck, and Netter (2013) turns out to be only the tip in the iceberg. We also offer evidence on the potential magnitudes of the inconsistencies in FE and FD estimators when the strict exogeneity issue is ignored. In addition, we suggest an alternative systematic approach to external identification in large panels which has the potential in some cases to be more convincing than internal identification via GMM, and more widely applicable than the "magic bullet" strategy of hoping to find a unique economic, tax, legal, or regulatory event that perturbs the explanatory variable of interest.

3. Testing for strict exogeneity

3.1 Identifying a set of canonical regression

In order to explore these issues in an informative set of common contexts, we identify a set of canonical panel-data regression models from the recent finance literature. To do this, we assign each fixed-effects-panel-data study flagged in our earlier literature search into one of a broad set of mutually exclusive categories based on the main dependent variable of interest. The five largest categories have dependent variables of the following type: (a) a firm's investment choice, (b) a firm's leverage/capital structure, (c) a CEO's compensation level or ownership position, (d) a firm's annual fundraising choice, and (e) a measure of firm performance (stock returns, accounting performance, Q, etc.). In total, 60% of the published panel studies with unit-level fixed effects in the elite set of three journals searched can be placed (using some subjective judgment) into one of these categories.

For each of these five dependent variable categories, we identify a small set of specific variables (both dependent and independent) that are used most frequently in the regression models identified in our literature search, subject to the constraint that the variables can be constructed from standard data sets. Our first choice is to construct variables and models that correspond to the choices by Gormley and Matsa (2014), as they offer some thoughtful off-the-shelf specifications that are informed by the literature. For variables/models not included in their study, we use variable definitions that appear to be most common in the literature, with some subjective judgement on our part in grouping similar variables together. For all five dependent variable categories we identify two common dependent variable constructions. We then model these dependent variables as a function of the independent variables appearing in the associated model of Gormley and Matsa (2014), or, when a Gormley and Matsa (2014) model is not available, independent variables that appear in at least 20% of all associated published studies identified in our literature search.

For ease of exposition, we will refer to the selected dependent variables as depvar1 through depvar5. These capture, in order, measures of leverage, investment, incentives, fundraising activities, and firm performance. We add the letters a and b to the end of the depvar notation to indicate the two different dependent variables selected for that category, for example book leverage (depvar1a) and market leverage (depvar1b).

Rather than discuss each of the independent variables in detail, we report in Table 1 a summary of the dependent variables and associated independent variables in models predicting each dependent variable. The actual construction procedure/technical definition of each of these variables is relegated to the appendix. Our hope is that none of our choices are controversial. We are simply trying to collect and characterize a large and varied literature in a reasonable and succinct way. The number of independent variables varies depending on the dependent variable, with a maximum number of six. At times we refer to these as indvar1 through indvar6, where the mapping in Table 1 can be used to recover the actual economic variable in question.

3.2 Testing for strict exogeneity

For each dependent variable, we select a single associated explanatory variable and conduct the strict exogeneity tests outlined by Wooldridge (2010), one based on the FE transformation and the other based on the FD transformation. Each test is for a model in which the dependent variable is a linear function of the unit-level (i.e., firm) fixed effect, year dummies, and the selected explanatory variable. We also conduct corresponding tests for a model in which all of the independent variables for a given dependent variable are included together in the model. Test statistics are calculated with standard errors clustered at the firm-level to allow for arbitrary serial correlation and heteroskedasticity. The Wooldridge (2010) strict exogeneity tests add one-period ahead future values of the independent variable(s) to the regression model and test whether the associated coefficients are 0, as should be the case if strict exogeneity holds. Thus, evidence of a non-zero coefficient (or a joint test of non-zero coefficients in the case of multiple explanatory variables) is taken as evidence against the strict exogeneity assumption.

The p-values for these tests using the entire universe of available Compustat data (excluding financials and utilities) from 1965 to 2012 are reported in Table 2.[7] The dependent variable for each model is listed in the left column, and the other column headings indicate the

[7] We start our sample with 1965 as this is the first year that all of our independent variables are available in Compustat.

test that is conducted, with FE1 (FD1) for example indicating the fixed effects (first difference) version of the Wooldridge (2010) test for strict exogeneity in a model with indvar1 as the sole independent variable (in addition to the year and firm effects). The FEJ and FDJ test designations are for the pooled version of these tests derived from models that include all of the selected explanatory variables jointly together.

As the figures in Table 2 indicate, the vast majority of the p-values are below .01, indicating that in most cases strict exogeneity can be rejected with a high degree of confidence. In fact, of the 108 tests conducted on individual explanatory variables, we can reject the strict exogeneity assumption at the 1% level in more than 80% of the reported models. Moreover, in the joint tests that include all explanatory variables, the strict exogeneity assumption is rejected at the 1% level in all models.[8] Clearly this indicates that violations of strict exogeneity are extremely common, and the findings of Wintoki, Linck, and Netter (2013) would appear to extend much more broadly to finance panel data studies. This is not surprising if one believes that financial choices/outcomes, performance, and incentives (the dependent variables) often have an effect on the future determinants of these choices (the explanatory variables), for example a firm's asset, growth, or governance characteristics.

To further explore these results, we break the sample into shorter time periods, or alternatively, into industry-based subsamples. In particular, we first conduct the preceding analysis for the entire sample of firms restricted to 10 year sample sub-periods, and next we conduct an analysis for all years but with subsamples restricted to a single industry using a firm's

[8] It is worth noting that the sign on the leading coefficients in the Table 2 tests are the same (different) as the sign of the estimated contemporaneous effect in approximately half of all cases, so simple statements about the direction of the parameter inconsistency are difficult to make. We also note that the ratio of the magnitude (absolute value) of the leading coefficient to the corresponding contemporaneous coefficient has a median across all models of .48, indicating that the magnitudes of the violations of strict exogeneity are substantial in a relative sense.

1-digit SIC code. Since we now have multiple p-values on test statistics for each dependent variable-independent variable combination (one for each subsample), we tabulate the median p-value for the associate set of test statistics. These figures are reported in Table 3, with panel A reporting the time-period subsample results and panel B reporting the industry-based subsample results.

As we would expect given the smaller sample sizes involved in these tests, the p-values in both panels of Table 3 are generally somewhat higher than for the larger samples incorporated into Table 2. However, it is quite remarkable that the median p-values are below .05 for more than half of the reported models, suggesting that even in these smaller subsamples there is substantial evidence against a maintained assumption of strict exogeneity. As the final column in Table 3 indicates, in the joint tests that include all explanatory variables the tests reject strict exogeneity at the 5% level or better in the vast majority (over 90%) of the models. If we couple these observations with strong a priori theoretical reasons to believe that strict exogeneity will be violated, the general case for suspecting that most FE and FD estimates in these types of canonical models are inconsistent seems quite compelling.

3.3 Insights on magnitudes

3.3.1 Comparing FE and FD estimates

The fact that FE and FD estimates will generally be inconsistent in many or most panel data finance settings is concerning. However, if the inconsistency is small, it is possible that conclusions regarding the magnitude of a coefficient of interest or a test of whether the coefficient is different from zero may be at least approximately valid. It is difficult to make more precise statements without understanding the dynamic structure of the underlying variables.

However, some potentially useful information can be inferred by comparing the FE and FD estimates, as large differences between these estimates would suggest a problem of substantial magnitude.

To investigate, we compare the magnitudes and signs of the corresponding FE and FD coefficient estimates for the models in Tables 2 and 3. We collect all of coefficient pairs and calculate the percentage of pairs in which the FE and FD coefficient estimates are of opposite sign, and also the percentage of cases in which both coefficients are significant at the 10% level or better and also of opposite sign. We also calculate the median ratio of the larger of the two coefficients in magnitude to the smaller coefficient in the subset of cases in which both coefficients have the same sign. These figures are reported in Table 4.

The figures in panel A indicate that FE and FD estimates are not infrequently of opposite sign. This is concerning, since we would expect two estimators that are cousins of one another and are applied to the same data to usually be of the same sign. As Wooldridge (2010) notes, substantial differences between FE and FD estimators are often an indication of a violation of strict exogeneity.

It would be particularly concerning if these two estimators yield significant coefficients of opposite sign.[9] If a given coefficient is 0, the likelihood of observing two coefficients that are significant at the 10% level and of opposite sign is .5% under the extreme assumption of independence of the two estimators. In general we would expect the estimators to be positively correlated, and also the true coefficient to differ from 0. Both of these considerations will tend to lower the likelihood of observing significant coefficients of opposite signs under standard

[9] It is worth noting that the standard errors we compute for these estimates are robust to arbitrary serial correlation. Thus, while the serial correlation structure may tend to favor one of these estimators (FE or FD) over the other based on efficiency grounds, both are consistent and provide informative asymptotic inferences when strict exogeneity holds.

distributional assumptions. However, the figures in panel B of Table 4 indicate that this behavior is not nearly as rare as would be expected, with rates in many cases far above the .5% threshold. If we pool across all coefficient pairs included in this panel, we arrive at a rate of 6.2% which is more than 12 times the expected rate under our extremely conservative assumptions. We interpret this as additional evidence of potentially misleading inferences being drawn from FE and FD estimates, either because of a failure of strict exogeneity or other model misspecification.

Turning to the subset of cases in which the FE and FD estimates are at least of the same sign, we report in panel C that the median ratio of the larger (magnitude) estimate to the smaller is frequently quite large. Pooling across all dependent variables, the median of these medians is 1.51, indicating that differences of magnitude on the order of 52% are quite common. This is of course after already excluding the substantial number of cases where the point estimates have opposite signs. Certainly this does not inspire much confidence in the economic content of FE or FD estimates when strict exogeneity is violated in common finance panel settings.

3.3.2 Hoping for a 1/T save

While the FE and FD estimators are both inconsistent when strict exogeneity fails, the degree of inconsistency of the FE estimator may be smaller in a long panel because the FE estimator effectively differences variables from their means while the FD estimator takes differences from adjacent periods. Intuition suggests that feedback effects will be more influential when directly comparing adjacent periods, and this notion is formally captured by the fact that the inconsistency of the FE estimator is on the order of 1/T while the inconsistency of the FD estimator is independent of T (see Nickell (1981)). Thus, one might hope that a long panel, when it is available, would render the FE coefficients to be relatively informative.[10] For this 1/T result to be useful, a firm's fixed effect for the dependent variable of interest needs to be stable over an entire sample period. Unfortunately, given the occasional changes that occur over time to a typical firm's management, ownership, and asset base (via mergers/acquisitions/divestitures), the assumption of a stable unit-level fixed effect over a long sample period may not be valid.

To investigate, we calculate correlations in estimated firm fixed effect coefficients derived from models estimated over different subperiods. In particular, for each dependent variable, we estimate a FE model using the entire set of associated independent variables for non-overlapping 10 year subperiods (5 year periods in the case of the incentive variables as they have a shorter data series) starting with the most recent observation year and rolling backwards.[11] We then collect the estimated fixed effects and correlate these estimates for different lags.

The resulting correlations are reported in Table 5 for each of the estimated models. As the figure indicates, for all of the models there is strong evidence of a monotonic decline in correlation of the fixed effects estimates as the estimation time periods get further apart. For the furthest lags, most of the correlations (8 of 10) are below .50, and in some cases they are quite small (under .20). Certainly this does not offer strong support for the stability of the underlying unit-level fixed effects coefficients. When we make the corresponding calculations for 5-year estimation periods, the same basic story emerges. Correlations in the estimated fixed effects tend to drop monotonically as the time periods grow more distant, and the correlations for the most

[10] Monte Carlo results presented by Judson and Owen (1999) suggest that inconsistency in the FE and FD estimators may be significant even when T is reasonably large.

[11] Results are similar if we examine models that use single explanatory variables one at a time, rather than a model with all variables included together.

distant 5-year period lags are generally smaller than for the 10-year periods (figures not tabulated). Thus, it appears that as panels get longer, the implicit assumption of a constant unit-level fixed-effect becomes more questionable. Consequently, placing faith in the 1/T save to restore confidence in the inconsistent FE estimates when strict exogeneity is violated would appear to be unwarranted.

3.4 Robustness and extensions

The models we present in the tables are intended to be boilerplate and uncontroversial. The basic points that emerges are: (a) there are good theoretical reasons to expect that future values of many common finance independent variables are correlated with the dependent variable even after partialing out contemporaneous control variables, (b) formal tests confirm this with a high degree of confidence, (c) standard FE and FD estimates are inconsistent when this strict exogeneity assumption is violated, and (d) there are reasons to suspect that this inconsistency can at times be large in magnitude.

While the evidence we present seems quite strong, one may be concerned that some peculiarity in our modeling or sampling choices may be driving the results. To investigate, we have experimented with (a) a more aggressive winsorization at the 5% tails rather than 1%, (b) trimming (dropping) the 1% and 5% tail observations rather than winsorizing, and (c) completely eliminating all winsorization of the data. We have also experimented with restricting the sample to the post 1980 and post 1990 time periods, and we have also estimated the models on only the industries that were excluded from the initial sample (utilities and financials). Finally, in all cases in which we could identify a common alternative definition/construction of an independent variable, we have substituted this alternative into the estimated models (list of these alternatives available from the authors). In all cases the results with these model or sample alterations are substantively unchanged from what we report in Table 2. Thus, it seems that the evidence against strict exogeneity is, perhaps not surprisingly, quite robust.

The evidence against strict exogeneity seems so strong that one might question whether a test on a large sample would ever not reject. To investigate, we select as an independent variable a measure that is very hard to predict – a firm's stock return – and as a dependent variable, a seemingly innocuous construct that is likely to depend on the independent variable and also to have a firm-specific component – the ratio of a firm's receivables to payables. If we conduct the Table 2 tests in a model using this dependent variable/independent variable combination, the p-value of the FE (FD) test for strict exogeneity is .77 (.91).[12] Thus, it is not the case that the strict exogeneity test always rejects. It would thus appear that it rejects in most of the models we consider earlier because of some economic relation between the dependent and future values of the independent variables commonly used in finance panel data regressions.

4. Using industry-year variation for identification in certain settings

If conventional FE and FD estimators are inconsistent in a given setting, a researcher is left with a substantial challenge in their goal of identifying a parameter of interest. An internal identification approach using a variant of GMM may be suitable in some, but certainly not all, settings. External identification exploiting exogenous changes in various economic parameters of interest is of course always desirable, but in many (perhaps most) settings, is not feasible.

[12] Stock returns do (weakly) predict the contemporaneous ratio of receivables to payables, as the estimated coefficient of -.018 is statistically significant at the 10% level.

In some cases, we suggest that time varying industry shocks may be suitable as instruments for firm-level innovations in the explanatory variable of interest.[13] It will almost always be the case that industry innovations in a variable of interest will be correlated with firm-level innovations, so the relevancy condition using this strategy will generally be easy to establish. If industry shocks capture inputs into firm shocks that are not driven by firm decisions, the exclusionary restriction will also in many cases be defensible. In measuring industry shocks the firm's own characteristics should be excluded in the calculation, so as to purge any endogeneity arising from purely firm-level variation. In addition, if a firm is large or an industry is concentrated, the exclusionary restriction may be less likely to hold, as there may be feedback from a firm's choices to other industry participants.

To explore the potential usefulness of this approach in settings in which traditional FE and FD estimators are likely inconsistent, we consider the relation between a firm's risk and its level of inside ownership. This is an issue that has attracted attention dating back to the work of Demsetz and Lehn (1985). From a pure-risk sharing perspective, higher risk should be associated with less concentrated ownership, as insiders seek to diversify. However, as highlighted by Demsetz and Lehn (1985), agency problems may be greater in high risk environments, in which case the optimal level of inside ownership may in fact be elevated. Not only is the sign of this relation theoretically ambiguous, the direction of causality is also unclear, as inside ownership may affect current or future risk rather than the other way around. Even if one was willing to accept an assumption of contemporaneous exogeneity, our evidence above

[13] The approach of using industry variation for identification in a cross-section has occasionally been employed in the prior finance literature. However, the approach we suggest here is more general as it can be exploited in a panel setting in which unit-level fixed effects may be correlated with the explanatory variables of interest.

indicates that strict exogeneity is likely violated in this context, so traditional FE or FD estimates will be inconsistent.

To investigate, we calculate the first difference of a firm's logged inside ownership and also the change in the standard deviation of daily stock returns (referred to hereafter as the change in firm risk) measured over the course of the observation year. While a regression of the change in ownership against the change in firm risk will remove any firm specific determinants of ownership, even in a best case scenario the strict exogeneity issue still remains. As a measure of changes in industry risk, we calculate the median change in firm risk for all other firms in the same 4-digit industry.[14] As we report in column 1 of Table 6, a simple regression of changes in firm risk against changes in industry risk plus a full set of year dummies yields a highly significant relationship with a coefficient of .473 and a t-statistic of 49.16. Not surprisingly, innovations in industry risk are closely related to contemporaneous innovations in firm risk. We repeat the same regression in column 2 applied to the small subset of observations with high levels of inside ownership (greater than 20%), and the coefficient even on this much smaller subsample is similar in magnitude (.390) and highly significant (t=8.49).

We next explore the relation between ownership and firm risk by using changes in industry risk as an instrument for changes in firm risk in a 2SLS estimation applied to the first differences. These estimates, reported in column 3 of Table 6, hint at a negative relation, but the coefficient is not quite significant at conventional levels (t=1.62). This may not be surprising, as many firms have very low levels of inside ownership in which most of the ownership variation may be driven by equity compensation grants, rather than deliberate decisions by insiders to manage their equity positions. Thus, we estimate in column 4 the same model, but with a

[14] We have also experimented with using mean industry changes in risk rather median changes. In general, median changes have larger coefficients and more explanatory power in explaining changes in firm risk, and thus we select these median changes for our instrument.

restriction to firms with start of period (i.e., time t-1) high inside ownership, defined as a minimum of 20%. The column 4 models reveals a significant negative relation between firm risk and inside ownership (t=1.97). Thus, it appears that our approach can identify an underlying effect, and the effect in this context is negative in sign. If we instead estimate a simple OLS first difference model of changes in ownership against changes in firm risk, the resulting coefficients corresponding to columns 3 and 4 of Table 6 are very small in magnitude and insignificant (coefficients not tabulated). This suggests that our approach has the potential to identify a causal relation, even when no simple obvious correlation between the endogenous variables is evident.

Or focus in this exercise is to illustrate the potential usefulness of industry-year innovations in some settings in which our earlier observations suggest that researchers have little hope of consistently estimating the desired parameters of interest using their reported estimation method. For completeness, we consider the robustness of our findings by adding to the column 4 model variables measuring (a) changes in firm size, (b) a firm's stock returns, and (c) the start of period level of (logged) inside ownership, as we suspect these variables may be related to ownership dynamics. As we report in column 5, the coefficients on these added variables are insignificant, and their inclusion has no substantive effect on the firm risk coefficient. Because of concerns about potential feedback from firm risk to industry risk, we have also experimented with excluding observations in which a firm's sales account for more than 10% of the industry total. The column 4 results with this restriction are substantively unchanged. We have also experimented with using market model residuals to measure risk rather than unadjusted returns. The results with this modification are similar to what we report, but the coefficient on firm risk in the column 4 model falls slightly in significance with this modification to the 10% level (t=1.72).

5. Conclusion

Several recent articles in the finance literature have investigated issues related to properly constructing conventional panel-data estimators and their associated standard errors given the types of databases that are commonly used in finance research. In this study we ask the preliminary question of whether these conventional estimators (i.e., fixed effects (FE) or first difference (FD) estimates) are likely to be informative in the sense that they will yield consistent estimates. We highlight the fact that consistency of FE and FD estimates relies on a strict exogeneity assumption that is both much stronger than the typical notion of contemporaneous exogeneity and is also testable.

We proceed to conduct these tests in a set of standard panel-data finance models identified from the recent literature. Perhaps not surprisingly given various dynamic theories of financial choices, we show that strict exogeneity can be rejected in essentially all of these models. In our view this evidence indicates that conventional FE and FD estimates, which we show are quite commonly used in finance research, are, in most cases, inconsistent estimators of the parameter of interest. At the very least, our evidence suggests that researchers should address the strict exogeneity assumption from a theoretical perspective, and also test this assumption, before they even consider using conventional estimators.

In an effort to gauge how misleading conventional panel data estimators may be in typical finance research settings, we examine differences between FE and FD estimates derived for the same models over the same sample. We show that these estimates frequently diverge substantially, suggesting that the magnitudes of inconsistencies arising from these estimation procedures can be, at least in some cases, substantial. We also caution that reliance on a long panel to minimize the inconsistency in the FE estimator may not be particularly useful, as unit level fixed-effects in typical finance research settings do not appear to be stable over long sample periods.

Our results are challenging as they suggest that finance researchers frequently must turn to less conventional approaches to estimate parameters of interest. GMM estimators, which have been increasing in popularity, are one such approach. However, in cases in which (a) theory suggests that the assumptions underlying GMM are violated, (b) the testable assumptions underlying GMM are rejected, or (c) the finite sample properties of GMM are poorly behaved, this is unlikely to be a suitable alternative.

Given these limitations to GMM, we suggest the possibility that in some settings the presence of industry-year shocks to explanatory variable may be a useful identification strategy. In particular, if there is an time-varying industry component to an explanatory variable of interest that satisfies the exclusionary restriction conditional on unit-level fixed effects and sample-wide year effects, this variation could be quite informative and could lead to a systematic approach to (quasi) external identification. We demonstrate the potential usefulness of this approach by using industry innovations in risk as an instrument for firm risk in an examination of the effect of risk on inside ownership. In this specific context our identification approach leads to estimates that differ substantively from naïve estimates that ignore endogeneity in the panel. While exploratory in nature, this preliminary evidence appears quite promising in terms of recommending this as a strategy to be considered in other contexts.

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Table 1: Variables and Models Selected for Tests of Strict Exogeneity

Dependent Variable	Indvar1	Indvar2	Indvar3	Indvar4	Indvar5	Indvar6
Depvar1a = Book leverage	Q_t	$logsales_t$	ROA_t	$zscore_t$	$logmarketcap_t$	$tangibility_t$
Depvar1b = Market leverage	Q_t	$logsales_t$	ROA_t	$zscore_t$	$logmarketcap_t$	$tangibility_t$
Depvar2a = Debt issuance	Q_{t-1}	$logsales_t$	ROA_t	$zscore_{t-1}$	$logmarketcap_{t-1}$	$tangibility_{t-1}$
Depvar2b = Equity issuance	Q_{t-1}	$logsales_t$	ROA_t	$zscore_{t-1}$	$logmarketcap_{t-1}$	$tangibility_{t-1}$
Depvar3a = Capex/Assets	Q_{t-1}	$logassets_{t-1}$	$zscore_{t-1}$	$Cashflow_t$	$Cash_{t-1}$	$tangibility_{t-1}$
Depvar3b = R&D/Assets	Q_{t-1}	$logasset_{t-1}$	$zscore_{t-1}$	$Cashflow_t$	$Cash_{t-1}$	$tangibility_{t-1}$
Depvar4a = Managerial Ownership	Q_t	ROA_t	$Stockreturn_t$	$logassets_t$	$volatility_t$	$tangibility_{t-1}$
Depvar4b = CEO compensation	Q_{t-1}	ROA_t	$Stockreturn_t$	$logassets_{t-1}$	$volatility_t$	$tangibility_{t-1}$
Depvar5a = Tobin's Q	$R\&D/Assets_{t-1}$	$logsales_t$	ROA_t			
Depvar5b = Return on Assets	Q_{t-1}	$logsales_t$	$R\&D/Assets_{t-1}$			

Note.- All dependent variables are measured as of time t where stock variables are measured as of the end of fiscal year t and flow variables are measured over the course of year t. All variables are constructed from Compustat, CRSP, or Execucomp data. Normalizations of flow (stock) dependent variables are divided by start (end) of period assets. All variable definitions, constructions, and timing conventions are detailed in an appendix. The dependent variables are selected based on our survey of all papers published in elite finance journals over a recent time period in which the authors(s) use unit-level fixed effects. The independent variables are selected from corresponding models in Gormley and Matsa (2014) or, when no such model exists, the variables that appear most commonly in the associated literature based on our literature survey.

Table 2: P-values for Test of Strict Exogeneity

	Model Used For Test						
	FE1/FD1	FE2/FD2	FE3/FD3	FE4/FD4	FE5/FD5	FE6/FD6	FEJ/FDJ
Depvar1a = Book leverage	.000/.001	.000/.000	.006/.000	.000/.000	.000/.004	.000/.977	.000/.000
Depvar1b = Market leverage	.000/.000	.000/.000	.000/.000	.000/.000	.000/.000	.294/.177	.000/.000
Depvar2a = Debt issuance	.000/.000	.000/.000	.000/.000	.000/.000	.000/.000	.000/.000	.000/.000
Depvar2b = Equity issuance	.000/.000	.000/.000	.000/.759	.000/.000	.000/.000	.000/.000	.000/.000
Depvar3a = Capex/Assets	.000/.000	.000/.000	.000/.000	.984/.001	.000/.000	.000/.000	.000/.000
Depvar3b = R&D/Assets	.000/.000	.000/.001	.187/.841	.000/.008	.000/.001	.000/.000	.000/.000
Depvar4a = Managerial Ownership	.000/.000	.050/.000	.000/.000	.237/.000	.000/.000		.000/.000
Depvar4b = CEO compensation	.000/.843	.111/.000	.000/.000	.000/.040	.000/.841		.000/.000
Depvar5a = Tobin's Q	.000/.000	.000/.000	.002/.000				.000/.000
Depvar5b = Return on Assets	.882/.000	.000/.008	.000/.001				.000/.000

Note.- Each cell of the table indicates the P-values for two tests of the null of strict exogeneity. The first (second) number in each cell is the p-value for the Fixed Effects (First Difference) regression test outlined by Wooldridge (2010) in which leading values of an explanatory variable(s) are included in the regression equation. All standard errors in the derivation of these p-values are robust to arbitrary hetereoskedasticity and serial correlation. The rows indicate the dependent variable in the models that are tested and the columns indicate the independent variable where the number following the FE/FD designation indicates the sole explanatory variable that is included in the regression equation using the mapping in Table 1 (in addition to year dummies). In these test the p-value is based on the t-statistic for the coefficient on the leading term in the regression equation. The FEJ/FDJ test is for a model in which all explanatory are included together and the p-values in this case are for an F-test of joint significance the entire set of leading coefficient terms. The sample includes all Compustat firms with available data from 1965 to 2012 with the exception of financials and utilities.

Table 3: Subsample Tests of Strict Exogeneity

	Model Used For Test						
Panel A: 10 Year Subsample	FE1/FD1	FE2/FD2	FE3/FD3	FE4/FD4	FE5/FD5	FE6/FD6	FEJ/FDJ
Depvar1a = Book leverage	.068/.126	.267/.000	.144/.000	.000/.000	.001/.130	.110/.166	.000/.000
Depvar1b = Market leverage	.185/.002	.001/.000	.002/.000	.000/.003	.000/.000	.333/.161	.000/.000
Depvar2a = Debt issuance	.000/.000	.000/.060	.327/.000	.000/.000	.001/.169	.331/.013	.000/.000
Depvar2b = Equity issuance	.236/.023	.000/.199	.101/.156	.000/.000	.000/.000	.000/.001	.000/.000
Depvar3a = Capex/Assets	.379/.388	.000/.026	.415/.001	.573/.267	.000/.000	.000/.000	.000/.000
Depvar3b = R&D/Assets	.020/.089	.006/.215	.367/.385	.126/.215	.288/.248	.185/.045	.000/.000
Depvar4a = Managerial Ownership	.244/.233	.009/.163	.571/.278	.060/.200	.444/.120		.000/.000
Depvar4b = CEO compensation	.001/.040	.000/.067	.042/.237	.000/.382	.170/.389		.000/.000
Depvar5a = Tobin's Q	.000/.011	.000/.000	.099/.011				.000/.000
Depvar5b = Return on Assets	.002/.000	.006/.128	.099/.002				.000/.040
Panel B: 1-Digit Industry Subsamples	FE1/FD1	FE2/FD2	FE3/FD3	FE4/FD4	FE5/FD5	FE6/FD6	FEJ/FDJ
Depvar1a = Book leverage	.056/.170	.153/.000	.133/.000	.000/.000	.000/.192	.049/.105	.000/.001
Depvar1b = Market leverage	.191/.001	.000/.000	.001/.000	.000/.008	.000/.000	.289/.134	.000/.000
Depvar2a = Debt issuance	.000/.000	.000/.040	.221/.000	.000/.000	.001/.104	.208/.013	.000/.000
Depvar2b = Equity issuance	.322/.012	.000/.187	.188/.156	.000/.000	.000/.000	.000/.001	.000/.000
Depvar3a = Capex/Assets	.411/.372	.000/.022	.542/.000	.503/.258	.000/.000	.000/.000	.000/.000
Depvar3b = R&D/Assets	.022/.052	.004/.275	.425/.260	.127/.201	.308/.256	.157/.041	.025/.116
Depvar4a = Managerial Ownership	.018/.003	.617/.342	.006/.031	.468/.016	.187/.149		.199/.182
Depvar4b = CEO compensation	.031/.425	.495/.083	.007/.000	.000/.365	.049/.424		.001/.003
Depvar5a = Tobin's Q	.242/.271	.000/.290	.019/.202				.031/.035
Depvar5b = Return on Assets	.004/.289	.006/.128	.628/.279				.000/.004

Note.- Each cell of the table indicates the median p-values for two tests of strict exogeneity where each test is conducted over the indicated subsample. The first (second) number in each cell is the median p-value for the Fixed Effects (First Difference) regression test outlined by Wooldridge (2010) in which leading values of an explanatory variable(s) are included in the regression equation with the median calculated over the set of p-values derived from the indicated set of subsamples. All standard errors in the derivation of these p-values are robust to arbitrary hetereoskedasticity and serial correlation. The rows indicate the dependent variable in the models that are tested and the columns indicate the independent variable where the number following the FE/FD designation indicates the sole explanatory variable that is included in the regression equation using the mapping in Table 1 (in addition to year dummies).

Table 4: Comparing FE and FD Estimates

Panel A: Proportion of Cases in which FD and FE estimates Differ in Sign

	Indvar1	Indvar2	Indvar3	Indvar4	Indvar5	Indvar6
Depvar1a	.000	.278	.000	.000	.000	.000
Depvar1b	.000	.111	.000	.000	.000	.000
Depvar2a	.000	.056	.056	.000	.000	.222
Depvar2b	.000	.556	.000	.500	.278	.167
Depvar3a	.000	.000	.056	.000	.000	.389
Depvar3b	.0000	.000	.000	.389	.389	.222
Depvar4a	.333	.389	.222	.000	.056	
Depva4b	.167	.000	.000	.389	.167	
Depvar5a	.167	.000	.056			
Depvar5b	.167	.000	.000			

Panel B: Proportion of Cases in which FD and FE estimates Differ in Sign and Both Significant at 10%

	Indvar1	Indvar2	Indvar3	Indvar4	Indvar5	Indvar6
Depvar1a	.000	.056	.000	.000	.000	.000
Depvar1b	.000	.111	.000	.000	.000	.000
Depvar2a	.000	.056	.000	.000	.000	.111
Depvar2b	.000	.556	.000	.333	.222	.000
Depvar3a	.000	.000	.056	.000	.000	.389
Depvar3b	.000	.000	.000	.056	.167	.056
Depvar4a	.167	.167	.167	.000	.000	
Depva4b	.111	.000	.000	.389	.000	
Depvar5a	.056	.000	.000			
Depvar5b	.000	.000	.000			

Panel C: Median ratio of larger magnitude FE/FD coefficient to smaller FE/FD coefficient if same signs

	Indvar1	Indvar2	Indvar3	Indvar4	Indvar5	Indvar6
Depvar1a	1.496	2.180	1.315	1.296	1.137	1.161
Depvar1b	1.278	1.340	1.387	1.414	1.144	1.174
Depvar2a	1.268	1.984	1.718	1.587	1.244	2.236
Depvar2b	1.098	2.670	1.081	1.522	5.435	3.648
Depvar3a	1.120	2.775	1.928	1.063	1.417	1.839
Depvar3b	1.237	2.173	1.286	1.404	1.971	1.781
Depvar4a	7.277	2.290	3.022	1.228	4.371	
Depvar4b	1.648	1.624	1.387	3.977	3.769	
Depvar5a	1.750	1.356	1.113			
Depvar5b	4.198	1.252	2.211			

Note-. In this table we compare pairs of FE and FD estimates for the same explanatory variable for the set of subsamples included in Table 3 based on either year subsamples or industry subsamples. In models explaining the indicated dependent variable as a function of the indicated independent variable (plus year effects), we identify the fraction of all cases in which the FE and FD estimates differ in sign (in Panel A) of differ in sign and are both significant at the 10% level or higher (in Panel B). In Panel C we restrict attention to cases in which the FE and FD estimates on the independent variable have the same sign and report the median value for the ratio of the larger magnitude (absolute value) FE or FD coefficient to the smaller magnitude coefficient in the pair over the set of all subsamples.

Table 5 – Correlation of Fixed Effects For Different Time Periods

Book Levg..	P1	P2	P3	P4	P5
P1	1	0.7072	0.3711	0.2179	0.1145
P2	0.7072	1	0.5441	0.2971	0.1032
P3	0.3711	0.5441	1	0.5984	0.3186
P4	0.2179	0.2971	0.5984	1	0.6403
P5	0.1145	0.1032	0.3186	0.6403	1

Mkt. Levg.	P1	P2	P3	P4	P5
P1	1	0.694	0.4346	0.3302	0.3176
P2	0.694	1	0.5795	0.3811	0.3319
P3	0.4346	0.5795	1	0.6429	0.4305
P4	0.3302	0.3811	0.6429	1	0.707
P5	0.3176	0.3319	0.4305	0.707	1

Debt Issuance	P1	P2	P3	P4	P5
P1	1	0.3959	0.2369	0.2618	0.279
P2	0.3959	1	0.4757	0.4251	0.3243
P3	0.2369	0.4757	1	0.3456	0.2408
P4	0.2618	0.4251	0.3456	1	0.1463
P5	0.279	0.3243	0.2408	0.1463	1

Equity Issuance	P1	P2	P3	P4	P5
P1	1	0.4135	0.0279	-0.0783	-0.1024
P2	0.4135	1	0.0147	-0.1339	-0.1692
P3	-0.0279	-0.0147	1	0.3847	0.3404
P4	-0.0783	-0.1339	0.3847	1	0.3822
P5	-0.1024	-0.1692	0.3404	0.3822	1

Capex	P1	P2	P3	P4	P5
P1	1	0.7476	0.7422	0.6923	0.6701
P2	0.7476	1	0.5864	0.5796	0.6374
P3	0.7422	0.5864	1	0.7677	0.705
P4	0.6923	0.5796	0.7677	1	0.8183
P5	0.6701	0.6374	0.705	0.8183	1

R&D	P1	P2	P3	P4	P5
P1	1	0.7714	0.666	0.5187	0.4878
P2	0.7714	1	0.8375	0.6464	0.5948
P3	0.666	0.8375	1	0.8019	0.6949
P4	0.5187	0.6464	0.8019	1	0.8657
P5	0.4878	0.5948	0.6949	0.8657	1

Ownership	P1	P2	P3	P4
P1	1.000	0.873	0.638	0.464
P2	0.873	1.000	0.803	0.558
P3	0.638	0.803	1.000	0.783
P4	0.464	0.558	0.783	1.000

Compensation	P1	P2	P3	P4
P1	1.000	0.642	0.516	0.454
P2	0.642	1.000	0.647	0.513
P3	0.516	0.647	1.000	0.733
P4	0.454	0.513	0.733	1.000

ROA	P1	P2	P3	P4	P5
P1	1	0.7384	0.6783	0.5641	0.5732
P2	0.7384	1	0.6702	0.5944	0.647
P3	0.6783	0.6702	1	0.6165	0.5901
P4	0.5641	0.5944	0.6165	1	0.5342
P5	0.5732	0.647	0.5901	0.5342	1

Q	P1	P2	P3	P4	P5
P1	1	0.6482	0.4267	0.388	0.3706
P2	0.6482	1	0.4181	0.2612	0.2755
P3	0.4267	0.4181	1	0.5872	0.425
P4	0.388	0.2612	0.5872	1	0.5204
P5	0.3706	0.2755	0.425	0.5204	1

Note.- For each dependent variable we estimate a fixed effects model regressing the dependent variable against all of the independent variables outlined in Table 1 (plus year effects) for 10-year non-overlapping periods starting at the end of the sample and working backwards. In the case of the ownership and compensation variables we use 5-year periods. For each model and time period we estimate firm fixed effects coefficients and report in each cell the simple correlation across different subperiods where P1 is the most recent period and P5 (or P4) is the most distant time period.

Table 6
Using Industry Year Innovations to Identify the Role of Risk in Inside Ownership

	Dependent Variable Δ in Firm Risk		Dependent Variable Δ in Log Ownership		
	(1)	(2)	(3)	(4)	(5)
Δ in Industry Risk	.473*** (.010)	.390*** (.046)			
Δ in Firm Risk			-3.15 (1.94)	-28.62** (14.54)	-29.37** (14.90)
Δ in Firm Size					.015 (.130)
Firm Stock Return					-.050 (.041)
Lagged log ownership					.005 (.088)
Which Observations	All	High Ownership	All	High Ownership	High Ownership
Estimation	OLS	OLS	IV-2SLS	IV-2SLS	IV-2SLS
Number of Observations	57,492	1,268	18,735	1,244	1,244

Note.- The dependent variables in columns 1-2 is the annual change in the standard deviation of a firm's daily stock returns measured over the fiscal year. The dependent variable in columns 3-5 is the change in the log of the sum of all managerial ownership in Execucomp, in percent units with the number 1 added before taking logs. The change in industry risk for any firm is measured as the change in the median value of the firm risk measure for all firms in the same 4-digit industry with the observation firm excluded in this calculation. The firm's stock return is the buy-and-hold return for the fiscal year over which the change in ownership is measured. Change in firm size is measured contemporaneous with ownership and size is measured using the log of a firm's sales. Lagged log ownership is the ownership level at the start of the fiscal year over which the change in ownership is measured. The columns indicating all observations include all sample observations with non-missing data while the high ownership observations add the restriction that ownership is at the 20% level or above as of the start of the year over which any annual change is measured. All models include a full set of year dummy variables (coefficients not reported) and robust standard errors clustered at the firm level. Changes in years in which the CEO is replaced are excluded from all models. The IV estimates are 2SLS estimates in which the annual change in industry risk is used as an instrument for the annual change in firm risk. Standard errors are reported in parentheses under each coefficient estimate. ***Significant at the 1% level. **Significant at the 5% level. *Significant at the 10% level.