Comments on "Investment Cash Flow Sensitivity: Fact or Fiction?"
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In Cummins, Hassett, and Oliner (2006), we assessed the influence of cash flow on investment when controlling explicitly for measurement error in Tobin's Q. We showed that the cash-flow effects often found in other studies disappeared after controlling for this measurement error. We also showed that a measure of Q based on analyst forecasts of firm-level earnings does a better job of capturing the fundamentals that drive investment spending than does the usual measure of Q based on stock prices.

Agca and Mozumdar (2012), henceforth AM, assert that errors in our data and econometric methodology are responsible for both of our key findings. AM identify three alleged problems with our work: (a) that we may have misdated the cash-flow series in our dataset, (b) that the set of instruments we use is too restrictive, and (c) that our analyst-based measure of Q is flawed.

As discussed below, we do not find AM’s critique to be compelling. First, AM provide no convincing evidence to support the claim that our cash-flow series was misdated. Second, even when AM modify our data to correct this alleged problem, use their preferred instruments, and substitute a different analyst-based measure of Q, their results do not overturn the central findings in our paper. We cannot emphasize this point strongly enough: When AM use our dataset, modified as they see fit, they do not reverse our results. Only when they construct their own dataset do their results differ substantially from those in our paper. Those results may be of interest in their own right, but they do not constitute a rebuttal of our findings.

Cash-Flow Data

AM assert that we may have misdated the cash-flow series used in our regression analysis. To explore this issue, ideally one would compare our cash-flow data for individual firms to the source data in Compustat and determine whether we used cash flow for year $t+1$...
when we had intended to use cash flow for year $t$, as AM allege. This path, however, is not available. The dataset we posted on the *American Economic Review* website omitted firm identifiers in order to protect the confidentiality of the proprietary I/B/E/S data that we used to create the analyst-based measure of $Q$. And unfortunately we did not save the internal files that contain this information. Hence, it is not possible to identify the individual firms in our dataset.

Given this limitation, AM present the results of a cruder matching test that we suggested to them. This test searches Compustat for matches between the cash-flow/investment ratio in our dataset and all possible observations of $\frac{CF_{t+1}}{I_t}$ and $\frac{CF_t}{I_t}$ in Compustat, declaring a match if the difference is less than a pre-specified threshold. AM interpret the results of this test as being inconclusive. However, the proper interpretation is that the test provides (a) no evidence that we misdated the cash-flow series and (b) some limited evidence that the data are properly timed.

Three points are important for interpreting the test results. First, our cash-flow and investment data were deflated to be in real terms, with a base year of 1992, while the Compustat data are in current dollars. Thus, the only valid comparison to the Compustat data is for 1992. For all other years, the comparison is between a ratio in real terms and one in current-dollar terms, and the only "matches" are noise matches that occur because the difference between our real cash-flow/investment ratio and the current-dollar ratio for some random Compustat firm is smaller than the pre-specified threshold. This means that the match rate for years other than 1992 is just a baseline control and that true matches will show up as a match rate for 1992 that is greater than the baseline. Second, we pulled our data from the industrial, full coverage, and research files that existed on Compustat tapes at the Federal Reserve Board as of the year 2000. We no longer have access to those tapes and instead provided AM with a download of data from a Compustat CD for late 2000. The firms on the CD, which is updated weekly, could well be
different than the firms included on the annual tapes. Consequently, one wouldn't necessarily expect the match rate to be close to 100 percent even in 1992. Third, there is nothing magical about the pre-specified thresholds for declaring a match. Looser thresholds would generate more matches, while tighter ones would generate fewer matches. This point implies that the absolute level of the match rate is essentially arbitrary. The power of the test comes strictly from the relative match rates across years and across the two different ratios constructed in Compustat ($\frac{CF_{t+1}}{I_t}$ and $\frac{CF_t}{I_t}$).

The results of the matching test are presented in AM's table A2. (We will focus on the results using the year 2000 Compustat CD, but the results are the same when AM use more recent Compustat data.) Under the looser of the two thresholds, the match rate between our cash-flow/investment ratio and $\frac{CF_{t+1}}{I_t}$ in Compustat ranges from 21 to 28 percent for years other than 1992. Recall these are just noise matches. The absolutely crucial result is that the match rate for 1992 is within this range. This means that the test provides no evidence of true matches between our data and a deliberately misdated cash-flow/investment ratio from Compustat. The conclusion is the same using the tighter of the two thresholds. However, when the test is run with the correctly dated ratio from Compustat ($\frac{CF_t}{I_t}$), the contrast in the results is striking. The rate of noise matches under the looser threshold ranges from 25 to 34 percent in years other than 1992, but the match rate rises to 44 percent in 1992. This comparison provides some evidence of true matches between our data and the properly dated cash-flow/investment ratio from Compustat. Overall, these results cast doubt on AM's claim that we used misdated cash-flow data.
Instruments

For the GMM estimation in our paper, we used an instrument set that included the ratio of cash flow to capital and the ratio of investment to capital lagged three and four years.\(^1\) Since we required both the third and fourth lags to be in the instrument set for every observation, we ended up using the first four years of the dataset (1982-85) to construct the instruments.

AM argue that our use of an identical set of instruments for every observation was too limiting. First, they note we could have added one year to the estimation period by using only the third lag of the instruments for the 1985 observations, while using both the third and fourth lags for later years. Alternatively, we could have kept 1986 as the first year for the estimation but enlarged the instrument set by using not only the third and fourth lags but all longer lags that were available for each subsequent year. Both of these are fair points. There is nothing sacrosanct about the instrument set we used, and it would be useful to know if our results are sensitive to alternative choices for the instruments.

Revised Estimation Results

AM's tables 1 and 2 contain the GMM estimation results after they re-date our cash flow series as they believe to be appropriate and use the two alternative instrument sets described above. These are the only results in the paper that address whether these changes overturn our original findings. In table 1, all of the estimated regressions fail standard specification tests. Thus, these results are unreliable and must be set aside. In table 2, five of the nine regressions also fail at least one of the specification tests. These results, too, must be set aside. That leaves a total of four regressions that can be used for a valid comparison to our original results. The coefficient on cash flow is insignificant in two of the four regressions and is significant at only

\(^{1}\)Although two-year lags of these variables were potentially valid instruments, our specification tests generally rejected models with those variables as instruments.
the ten-percent level in two others; no cash-flow coefficients are significant at the usual five-
percent level. In addition, the analyst-based measure of Q that we developed is significant at the
one-percent level in all four regressions, while the standard measure of Q based on stock prices
is insignificant. The only fair conclusion is that these tables provide no statistically reliable
evidence against our original results.

Analyst-based Measure of Q

AM also raise two alleged problems with our analyst-based measure of Q. First, they
note that the analyst forecasts we used do not strictly predate the period during which investment
spending is measured and thus may be endogenously determined with investment. Second, AM
assert that our formula for the present value of forecasted earnings uses a discount factor that is
off by one year; that is, the term discounted by (say) \( \rho \) really should be discounted by \( \rho^2 \), and so
on.

The logic behind these points is flawed. With regard to the dating of the analyst
forecasts, we instrumented for these forecasts with variables dated three and four years in the
past. As long as these are valid instruments, the precise dating of the earnings forecasts is not an
issue. Furthermore, the point about discounting is based on a fundamental mistake concerning
the definition of earnings. Annual earnings represent a flow over the year and are not, as AM
assert, a projected year-end value. As a flow, projected earnings for the year to come should be
discounted by roughly \( \rho^{0.5} \). Projected earnings for the subsequent year should be discounted by
roughly \( \rho^{1.5} \). Using this latter case for illustration, we discounted with \( \rho \), while AM used \( \rho^2 \).
Neither is better than the other; they are both approximations.

AM construct a revised analyst-based measure of Q that uses their preferred discounting
as well as analyst forecasts that predate the period of investment. The results they obtain with
the revised measure of Q are very similar to those obtained with our original measure. AM say this themselves on page 19. Given this finding, it is hard to understand why AM think these results bolster their case against our paper. In fact, the opposite is true. The results suggest that our method of calculating analyst-based Q, while surely imperfect, did not bias us away from finding significant cash-flow effects.

**AM's Own Dataset**

Thus far, we have focused on AM's comments that question our data and econometric methods. AM also construct a separate dataset that they use to estimate investment-cash flow regressions. They describe this dataset (see p. 15) as comparable to ours. Using this separate dataset, they generally find significant cash-flow effects.

In fact, however, their dataset in not comparable to ours in two ways. First, our dataset excluded observations with outlier values for the key variables (see p. 800 of our paper), but the dataset created by AM does not appear to have been trimmed at all. This is an important difference because the treatment of outliers can have a substantial effect on estimated cash-flow coefficients (see Erickson and Whited, 2012). Indeed, footnote 16 in Erickson and Whited (2012) states that at least some of the estimated cash-flow coefficients in an earlier version of AM's paper "are an artifact of one or two tail observations per year". Second, AM's measures of Q are not the same as ours. They measure Q as the book value of total debt plus the market value of equity divided by total assets (see their appendix A1), while we used the Salinger and Summers (1983) measure that applies to the firm's tangible fixed capital rather than its entire asset base and that incorporates a variety of tax factors. AM would need to deal with both of these data issues before they can claim to have constructed a dataset that is comparable to ours.
References


