Still unanswered questions (and new ones) to Bebchuk, Brav and Jiang

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(Opinions expressed herein are strictly those of the authors)
Abstract

Bebchuk et al. have produced a new version of their paper *The Long-Term Effects of Hedge Fund Activism* (December 2014) to be published in the June 2015 issue of the *Columbia Law Review*. In this revised text, the authors struggle valiantly to cope with the challenging questions a number of critics have raised about their original paper. We, among many, have taken issues with their original paper. While the revised draft by Bebchuk *et al* has the merit of clarifying some aspects of their study, many issues remain unresolved and new ones creep in. In this paper, we provide examples of the latter while reiterating the fundamental questions that remain unanswered.
Bebchuk et al. have produced a new version of their paper *The Long-Term Effects of Hedge Fund Activism* (December 2014) to be published in the June 2015 issue of the *Columbia Law Review*. In this revised text, the authors struggle valiantly to cope with the challenging questions a number of critics have raised about their original paper.

We, among many, have taken issues with their original paper in a piece titled “Activist hedge funds: creators of lasting wealth? What do the empirical studies really say?” (July 2014). As Professor Bebchuk purported to respond to our criticisms on the Harvard Law School Forum on Corporate Governance and Financial Regulation, we reiterated the questions (most of them) left unanswered (“Hedge fund activism and their long-term consequences: Unanswered questions to Bebchuk, Brav and Jiang”, August 2014).

**New issues**

While the revised draft by Bebchuk et al. has the merit of clarifying some aspects of their study, many issues remain unresolved and new ones creep in.

For an example of the latter, comparing their table 3 from the July 2013 version of their paper with the same table 3 in the December 2014 version, one finds that the number of observations is identical (except for year t+5 as we now learn that the fifth-year data for the interventions of 2007 were not available for the first version of the paper) but the Tobin's Qs are very different as shown in the following tables:
### Table 1
Comparison from July 2013 and December 2014 versions

**Industry-Adjusted Operating Performance**
*(July 2013)*

<table>
<thead>
<tr>
<th></th>
<th>t: Event Year</th>
<th>t+1</th>
<th>t+2</th>
<th>t+3</th>
<th>t+4</th>
<th>t+5</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Average</strong></td>
<td>-1.507</td>
<td>-1.369</td>
<td>-1.377</td>
<td>-1.329</td>
<td>-0.984</td>
<td>-0.935</td>
</tr>
<tr>
<td><strong>Median</strong></td>
<td>-0.748</td>
<td>-0.614</td>
<td>-0.540</td>
<td>-0.547</td>
<td>-0.470</td>
<td>-0.420</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>1,611</td>
<td>1,384</td>
<td>1,206</td>
<td>1,076</td>
<td>942</td>
<td>710</td>
</tr>
</tbody>
</table>

*Source: Excerpt from Table 3 of Bebchuk, Brav and Jiang, 2013, p.9.*

**Industry-Adjusted Operating Performance over Time**
*(December 2014)*

<table>
<thead>
<tr>
<th></th>
<th>t: Event Year</th>
<th>t+1</th>
<th>t+2</th>
<th>t+3</th>
<th>t+4</th>
<th>t+5</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Average</strong></td>
<td>-0.469</td>
<td>-0.414</td>
<td>-0.335</td>
<td>-0.279</td>
<td>-0.194</td>
<td>-0.137</td>
</tr>
<tr>
<td><strong>Median</strong></td>
<td>-0.661</td>
<td>-0.526</td>
<td>-0.471</td>
<td>-0.492</td>
<td>-0.425</td>
<td>-0.399</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>1,611</td>
<td>1,384</td>
<td>1,206</td>
<td>1,076</td>
<td>942</td>
<td>831</td>
</tr>
</tbody>
</table>

*Source: Excerpt from Table 3 of Bebchuk, Brav and Jiang, 2014, p.25.*

For instance, the average industry-adjusted Tobin's Q at t=event year was -1.507 for the 1,611 observations in the sample in July 2013 and -0.469 for the same 1,611 observations in December 2014 and so on (and why are the median numbers not adjusted by the median of the relevant industry instead of the average?).

*We are flummoxed by these differences between those two tables based on the same observations. Should the authors have flagged these differences and offer some explanation? If errors of some sort can mar these simple tables, how can we rely on results produced by tricky and complex statistical manipulations?*
**Holding period**

Another example of unreconciled differences arises from the data on the length of holding period by activist hedge funds.

The latest Bebchuk *et al* version of the paper claims that the median holding period reaches 539 days. But, in another study based on the same data set, the authors Brav, Jiang and Kim (2013) estimate this median holding period at 348 days. The same authors had estimated in 2009 the median holding period at 266 days.

Another researcher, Gantchev, has found that for 1,164 interventions by hedge funds between 2000 and 2007 the median holding period was 9 months (or about 270 days).

**TABLE 2**

**Length of holding period**

<table>
<thead>
<tr>
<th>Authors</th>
<th>Sample</th>
<th>Median</th>
<th>Average</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Using the same dataset...</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Brav, Jiang and Kim (2009) <em>Hedge Fund Activism: A Review</em></td>
<td>1,172 events, from 2001 to 2007</td>
<td>266 days</td>
<td>376 days</td>
</tr>
<tr>
<td>Brav, Jiang, Partnoy and Thomas (2008) <em>Hedge Fund Activism, Corporate Governance, and Firm Performance</em></td>
<td>1,059 hedge fund-target pairs, from 2001 to 2006</td>
<td>369 days</td>
<td>N/A</td>
</tr>
<tr>
<td>Brav, Jiang and Kim (2013) <em>Hedge fund Activism Updated tables and figures</em></td>
<td>2,624 fund-target firm pairs, from 1994-2011</td>
<td>348 days</td>
<td>581 days</td>
</tr>
<tr>
<td>Bechuk, Brav and Jiang (December 2014) <em>The Long-Term Effects of Hedge Fund Activism</em></td>
<td>2,040 Schedule 13D filings, from 1994-2007</td>
<td>539 days</td>
<td>811 days</td>
</tr>
<tr>
<td><strong>Other authors</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Gantchev (2013) <em>The costs of shareholder activism: Evidence from a sequential decision model</em></td>
<td>1,164 campaigns, from 2000-2007</td>
<td>9 months</td>
<td>14.66 months</td>
</tr>
<tr>
<td>Becht, Franks, Grant and Wagner (2014) <em>The Returns to Hedge Fund Activism: An International Study</em></td>
<td>1,187 engagements (North America), from 2000-2010</td>
<td>N/A</td>
<td>515 days</td>
</tr>
</tbody>
</table>
What explains the significant difference observed between the Brav, Jiang and Kim 2013 results (median 348 days) and the Bebchuk, Brav and Jiang December 2014 results (median 539 days) when their samples greatly overlap?

Again, these differences raise the issue of selection, definition and termination date of activist intervention.

The number of different hedge funds
Bebchuk et al do not inform us about the number of distinct hedge funds in their dataset or the number of different targeted companies. Other, more transparent, studies show evidence of large variations in number of hedge funds as, again, the issue of definition creeps up.

<table>
<thead>
<tr>
<th>TABLE 3</th>
<th>Number of hedge funds and targeted companies</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Period</td>
</tr>
<tr>
<td>Brav, Jiang, Partnoy and Thomas (2008)</td>
<td>2001-2006</td>
</tr>
<tr>
<td>Xu and Li (2011)</td>
<td>1994-2008</td>
</tr>
<tr>
<td>Clifford (2008)</td>
<td>1998-2005</td>
</tr>
<tr>
<td>Boyson and Mooradian (2011)</td>
<td>1994-2005</td>
</tr>
<tr>
<td>Greenwood and Schor (2009)</td>
<td>1994-2006</td>
</tr>
<tr>
<td>Gantchev (2013)</td>
<td>2000-2007</td>
</tr>
</tbody>
</table>

Such wide variations in the numbers across studies are bound to influence the statistical results obtained and the conclusions reached.

The timing of “intervention”
The research question that Bebchuk et al seek to address is a fairly simple one: what happens to the performance of targeted companies in the years following an activist hedge fund intervention? In the short term, as the
authors are pretty pleased to point out, companies targeted by activist hedge funds show an abnormal return as a result of their 13D filing.

But von Lilienfeld-Toal and Jan Schnitzler have shown that this stock behaviour is observed for all 13-D filings, as illustrated in Figure 1.

**FIGURE 1**  
Abnormal returns for different 13-D filers  


As for the longer term performance associated to hedge fund activism, the apparent simplicity of the question vanishes. As one digs into the arcane details of these “interventions”, a number of issues come to the fore.

For instance, what is meant by date of intervention? In Bebchuk et al, the “intervention” is assumed to take place at the time of a 13D filing. Every “improvement” of performance after that date is attributed to the ministrations of activist hedge funds. In some cases (how many?) there were no 13D filing, so it appears Bebchuk et al used the time at which the hedge fund made public its holding and its intentions.
In reality, it is not so simple. Take the case of Nelson Peltz’s Trian Fund and PepsiCo. In July 2013, Nelson Peltz, the activist hedge fund manager, announced that his fund beneficially owned in excess of $1.3bn of PepsiCo shares, which represented far less than the 5% threshold for a 13D filing (actually around 1%).

Typically, Peltz then issued a 59-page white paper making the point that “the status quo is unsustainable” and proposing that PepsiCo be split up to create two stock-listed companies. PepsiCo rejected this proposal. As PepsiCo’s performance has been stellar over the last two years, Peltz realized he would not win (for now) his case for a split-up; so he settled, on January 16th 2015, for the consolation prize offered by the company of a Peltz nominee joining the board of PepsiCo on March 23, 2015.

Now when does that intervention begin? There was no 13D filing. So is July 2013 the “event date”? But PepsiCo having refused to implement any of the hedge fund’s proposals, how could any improvement in PepsiCo’s performance be attributed to Trian’s intervention? Perhaps then the date of the intervention should be March 2015 when a Trian nominee joins the board of PepsiCo.

This issue is singularly important as a poor choice of intervention date, such as July 2013, would falsely credit the hedge fund for the improved performance of PepsiCo. In fact, the management of PepsiCo took vigorous measures to improve performance after the July 2011 announcement by the company that it would not meet its EPS guidance for 2011; nothing to do with any hedge fund agitation.

Here is another instance reported in Brav et al. (2008) to illustrate hedge fund activism. The activist fund MLF Investment targeted Alloy Inc. The timeline of the events is presented in Table 2 below. The initial 13D was filed in November 2003 (which would be considered as t=event year in the Bebchuk et al research), but it took another year before the activist was granted a seat on the board (should September 2004 be considered t=event year instead?). Only two years later did the activist investor achieve its desired outcome (a spinoff). It could be argued that performance
improvement resulting from this activist intervention should be measured
from that point on; thus, December 2005 would be considered as t=event
year.

**TABLE 4**
**Timeline for the case of MLF Investments and Alloy, Inc.**
(Example used in *Hedge Fund Activism, Corporate Governance, and Firm
Performance* [2008] by Brav, Jiang, Partnoy and Thomas)

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>MLF Investments LLC filed a Schedule 13D indicating that it owned 5.8% of Alloy, Inc.</td>
<td>Matthew Feshbach, the founder and managing partner of MLF Investments, appointed to Alloy’s board</td>
<td>Alloy announced plans to spin off its merchandise business</td>
<td>Alloy, Inc. distributed to its stockholders 100% of the outstanding shares of common stock of dELiA*s, Inc.</td>
<td>MLF Investments executed a smooth exit from its investment.</td>
</tr>
</tbody>
</table>

Such issues quickly overwhelm the crude tool kit of econometrics, which is unable to capture the manifold complexity and nuances of social phenomena.

Future research on the effects of activism should consider alternative research methods to econometrics: for instance extensive case studies of a significant number of hedge fund interventions, interviews with managers and board members of targeted firms, and so on.

“Matching with peer companies”

Then, of course, in order to attribute any performance improvement to activist intervention, the results must be adjusted for the performance of other firms in the “industry” or to a “matched” group of companies.

So the authors write that they have matched every targeted company with firms with the same three-digit SIC level. But in their footnote 53, they inform us: *When using three-digit industry classification resulted in less than five firms,*
we used two-digit SIC levels or, if using two-digit SIC industry classification also provided less than five firms, one-digit industry classification.

But they never inform us as the frequency of these adjustments: in how many cases were the comparisons done with two-digit or one-digit SIC enterprises. That is very important as no one should consider a one-digit industry as an “industry” match¹.

**Mean-reversion**
Again, in their attempt to show that their results do not merely capture the common phenomenon of mean reversion, the authors set out to compare the performance of targeted companies with those of a “matching group”.

But it seems that they faced the same issue of finding comparable companies; so “for each target, we identify five matched firms from the same SIC two-digit industry that are closest in the performance to the target at the time of intervention. If there are fewer than 20 firms in an SIC two-digit industry, we expand the pool for matches to the SIC one-digit industry.” (p.35, emphasis added)

As they do not inform us of the frequency of this “expansion to the SIC one-digit” or of the validity of using two-digit SIC firms to compare performances, their demonstration that there is no regression to the mean is unpersuasive. Furthermore, their demonstration would have been more credible, had they included results obtained at t-3, t-2 and t-1 showing a trend line supportive of their claim.

**SIC vs. NAICS**
Finally, as we have already flagged the well-known problems with the SIC classification. We stressed the superiority of the NAICS: “NAICS codes provide a greater level of detail about a firm’s activity than SIC codes...There are 358 new

¹ Eliminating the finance industry, public administration and unclassified companies, there are but eight very broad one-digit industries, unsuitable for any sort of “match”: Agriculture, forestry, and fishing; Mining; Construction; Manufacturing; Transportation, communications, and utilities; Wholesale trade; Retail trade; Services.
industries recognized in NAICS, 250 of which are services producing industries". We still have not read any comment from Bebchuk et al about this issue.

Other unanswered questions

✓ The characteristics of the activist interventions

We still do not know much about their sample of activist interventions:

- They have identified 2,040 cases but “event year” statistics are based on 1,611 cases; what explains this difference?

- What is the distribution of the demands of activist hedge funds? How many “interventions” merely listed as the reason “General undervaluation/Investment purposes”? How many demanded board representation? How many failed to get board representation? Are all these interventions, whether they have failed or not, included in the sample? This information is crucial as demonstrated by some of the results obtained by Gow, Shin and Srinivasan (2014) who surveyed 1,969 cases of activist interventions between the years 2004 and 2012. In roughly 45% of their cases, the activists asked for board representation but were rebuffed some 50% of the time. (See Table 5).

Does the sample of Bebchuk et al. contain a similar number of cases where activists demanded board representation and were rebuffed? We are not told. It is noteworthy that the best stock market performance 36 months after the intervention was posted by those very companies that rejected the hedge fund’s call for board representation. How do Bebchuk et al. square their results with those of Gow, Shin and Srinivasan?
TABLE 5
Stock Returns by Category of Activism, from Activism Announcement (Month T to Month T+36)

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Non-board related activism events</td>
<td>1,089</td>
<td>67.6%</td>
<td>51.8%</td>
<td>45.9%</td>
<td>1.0%</td>
<td>-0.9%</td>
</tr>
<tr>
<td>Activists demanded, but did not win, board seats</td>
<td>456</td>
<td>135.2%</td>
<td>115.0%</td>
<td>100.1%</td>
<td>6.4%</td>
<td>4.8%</td>
</tr>
<tr>
<td>Activists were granted one or more board seats</td>
<td>424</td>
<td>24.1%</td>
<td>9.8%</td>
<td>0.1%</td>
<td>-3.1%</td>
<td>-5.1%</td>
</tr>
</tbody>
</table>


- What is the distribution of targeted companies by industry? By size of the hedge fund stake? By market value of the targets? Why are no results provided for years t=-3, t=-2, t=-1? In how many cases have the operating performance actually deteriorated for the years t+3, t+4 and t+5? This last question is especially relevant before concluding that all the interventions produce beneficial long-term effects (or do not prove detrimental).

There are partial answers to these questions coming from other sources: a group of researchers\(^2\) who had access to the same dataset used by Bebchuk et al. (but restricted to the period between 2001 and 2007) reported that 29.6% of the activists’ targets were high-tech firms (defined by 3-digit SIC code). If close to a third of the sample is made up of high tech firms, what are the consequences of

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such an over-representation, given the well-known fact that traditional accounting metrics do not capture the operating performance of these firms?

We also previously noted that subsequent tables prepared by two of the paper co-authors for 2,624 hedge fund “interventions” between 1994 and 2011 showed that:

- The median market value of targeted companies was $134.6 million and on average $835.3 million;
- A so-called group of “matched firms” had average market value of $2.741 billion, a very significant difference with targeted firms!
- The median original investment by hedge funds was $13.5 million; in only the top 5% did the hedge fund investment reach a median value of $185.1 million

It appears that the Bebchuk, Brav, and Jiang study is heavily weighted with small companies and relatively small investments by hedge funds. Do the results published by Bebchuk et al. hold for larger targeted companies and more recent interventions?

✓ How are the ROA and Tobin’s Q “improved” in their study?
Apart from a statement about these measures being commonly used in financial research, the authors do not assess the validity of these metrics to determine the contribution of hedge fund activism to the performance of targeted companies.

Indeed, these two measures may be fine indicators of performance but the issue we are raising is that we do not know how these two metrics were improved (if they really were). Econometric analysis cannot capture the myriad ways of improving these measures, some of which are not an indication of improved performance.
For instance, these activist hedge funds frequently urge targeted companies to use all cash on their balance sheet to buy-back shares. Implementing such a policy will raise both ROA and Q; but that is merely an accounting artifact which does not indicate any real improvement in performance.

A higher Tobin's Q may or may not be a demonstration of improved performance (Dybvig and Warachka, 2012). Write-downs of assets, of goodwill, reductions in capital investments and R&D that have no near-term impact on stock price also boost Tobin's Q but are not demonstrations of real performance enhancement. Selling assets/divisions with low ROA but perhaps high expected growth in profit will also raise ROA, in the short term.

The latest version of Bebchuk et al.’s paper does not enlighten in the least about these issues.

✔ **The econometric cauldron**

To cope with the difficulty of capturing the dynamic complexity of real-life events, the authors chose to pour into an econometric cauldron all possible data, stir and select results they deem significant. In order to include all plausible control variables, econometric models require a large number of observations. As a result of the huge computational power of computers, it has become conventional to include large sets of data in econometric studies. However, in the case of Bebchuk et al, it seemed particularly egregious to mix in all publicly listed companies in the Compustat data bank with the companies which have been the subject of activist interventions.

So, for the purpose of running regressions, this operation would result, say, for year 1995 when 37 companies were targeted by activists, in mixing these 37 companies with 10,000+ listed U.S. companies in the Compustat databank for 1995, companies where no intervention whatsoever occurred, and so on year after year. That's
how they got to some 130,000 “observations” and tens of thousands of “dummy variables” in some of the regressions.

Given the absence of clear description of their data and as we could not believe that they would use such a process, we charitably concluded that they must have used quarterly data instead of yearly data to boost their number of observations. Bebchuk *et al* now clarify that they have opted for this conventional course to increase their number of observations.

That approach to statistical analysis, no matter that it has become the norm for econometric publications, illustrates the limits of the econometric tool kit. When it does try to cope with complex social phenomena, econometric treatment of this kind too often produces results that are slippery, fragile, and remote from the observations on which these computations are supposedly based.

✓ **Interpreting the statistical results of multiple regressions**

We wrote earlier but it bears repeating: statistical analyses of the type used by Bebchuk *et al* do not provide proof of any causal relationships between company performance and hedge fund “intervention”. [Actually, no econometric study ever does.]

But Bebchuk *et al* make broad claims, which may be interpreted by some as a demonstration of causality:

> [t]he results are **consistent with the view** that operating performance improves steadily during the years following the intervention relative to the intervention time. Indeed, in each of the four regressions, each of the coefficients for the dummy variables representing the years $t+1$, $t+2$, $t+3$, $t+4$ and $t+5$ coefficients is higher than the coefficient for the event year. (p.30, emphasis added)

This assertiveness of the authors is surprising considering that half of the coefficients of their four key regressions are not statistically
significant, as shown by the grey cells in the following table. [In line with a long-standing practice, we consider as statistically significant the 1% and 5% levels; stretching beyond those levels smacks of a desperate attempt at salvaging unsatisfactory results]

**TABLE 6**
Regression analysis in Table 4 of Bebchuk et al (December 2014)
(Grey cells replace coefficients that are not statistically significant at the 1% or 5% level)

<table>
<thead>
<tr>
<th>Variable</th>
<th>Regression 1</th>
<th>Regression 2</th>
<th>Regression 3</th>
<th>Regression 4</th>
</tr>
</thead>
<tbody>
<tr>
<td>t: Event year</td>
<td>-0.010 (5%)</td>
<td>-0.014 (1%)</td>
<td>-0.342 (1%)</td>
<td></td>
</tr>
<tr>
<td>t+1</td>
<td></td>
<td></td>
<td>-0.260 (1%)</td>
<td></td>
</tr>
<tr>
<td>t+2</td>
<td></td>
<td>-0.179 (1%)</td>
<td>0.156 (5%)</td>
<td></td>
</tr>
<tr>
<td>t+3</td>
<td>0.015 (1%)</td>
<td></td>
<td>0.239 (1%)</td>
<td></td>
</tr>
<tr>
<td>t+4</td>
<td></td>
<td></td>
<td>0.283 (1%)</td>
<td>0.302 (1%)</td>
</tr>
<tr>
<td>t+5</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Source: adapted from Table 4, in Bebchuk et al, December 2014

“If a coefficient's t-statistic is not significant, don't interpret it at all. You can't be sure that the value of the corresponding parameter in the underlying regression model isn't really zero."  

**Age of company and its performance**

In their regression analysis, the authors use the natural logarithm of the age of the firm as control variable. In the econometric literature, this variable is frequently used to control for the well-known fact that as firms grow older, their performance tends to decrease.

In Bebchuk et al's regressions the coefficient of Ln(Age) is negative and significant as expected when Tobin's Q is the dependent variable but **positive** and statistically very significant for all the regressions where ROA is the dependent variable, a result at variance with their results for Tobin's Q and completely opposite of what previous studies have established. The authors give no explanation for this surprising

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3 A reference may be found in DeVeaux, Velleman, and Bock (2012), *Stats: Data and Models*, 3rd edition, Addison-Wesley, p.801.
result, which may be an indication of a common, but serious, econometric pathology called “multicollinearity”, making the interpretation of all coefficients subject to great caution.

“Various econometric references have indicated that collinearity increases estimates of parameter variance, yields high R square in the face of low parameter significance, and results in parameters with incorrect signs and implausible magnitudes.⁴”

Normally distributed residuals?

The authors repeatedly assert that their dependent variables are heavily skewed.

We note that Q is highly right skewed, which results in average Q exceeding median Q, and that ROA is highly left skewed... (page 22)

Industry average levels differ somewhat from industry median levels because both ROA and Q are skewed. (Footnote 54)

While dependent variables do not have to be normally distributed, there is a risk that highly skewed dependent variables will result in skewed residuals, which do have to be normally distributed. Yet, the Bebchuk et al paper is silent about this and other common issues of regression analysis.

✔ Wealth creation vs. wealth transfer

Assuming that some activist interventions create value for shareholders, where is that additional value coming from? Advocates of hedge fund activism would have us believe that it results from greater efficiency in managing the company. Yet, Brav, Jiang and Kim (2013), two of them co-authors of the Bebchuk paper and thus strong supporters of the benefits of activist hedge funds, must nevertheless acknowledge that:

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“Overall, results in this section suggest that target firm workers do not share in the improvements associated with hedge fund activism. They experience a decrease in work hours and stagnation in wages, while their productivity improves significantly. Moreover, the relative decrease in productivity-adjusted wages from above-par levels suggests that hedge fund activism facilitates a transfer of “labor rents” to shareholders which may account for part of the positive abnormal return at the announcement of hedge fund interventions”. (Brav et al, 2013, p.22)

As for transferring wealth from debt holders, Moody's observes: “Our finding that its [shareholder activism] effects on the creditworthiness of Moody's-rated issuers is almost universally negative, even if only moderately.” “As short-term shareholder activists have become more influential, we have observed numerous examples of concessions to activists that have eroded credit quality contributing to downgrades⁵.”

Klein and Zur also find a similar result: “...we find that hedge fund activism significantly reduces bondholders' wealth... Confrontational campaigns and the acquisition of at least one seat on the target's board elicit more negative bond returns. We also find an expropriation of wealth from the bondholder to the shareholder⁶”.

Finally, Aslan, H. and H. Maraachlian (2009) provide graphic evidence of the phenomenon of wealth transfer from bond holders to shareholders (See Figure 2⁷).

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⁵ **Source:** Byrd, F., D. Hambly & M. Watson. Short-Term Shareholder Activists Degrade Creditworthiness of Rated Companies, Moody's Investors Services Special Comment, June 2007.

⁶ **Source:** “The Impact of Hedge Fund Activism on the Target Firm's Existing Bondholders”, Klein and Zur (2010).

⁷ **Source:** adapted from Hadiye Aslan and Hilda Maraachlian, “Wealth effects of hedge fund activism,” Paper submitted to the European Finance Association, 36th annual conference, February 2009, Figure 2, 3 and 4; from a sample of 1,332 target firms, for the period 1996-2008.
FIGURE 2
Cumulative Abnormal Returns of Target Stocks Subject to Hedge Fund Activism

Source: adapted from Hadiye Aslan and Hilda Maraachlian, “Wealth effects of hedge fund activism,” Paper submitted to the European Finance Association, 36th annual conference, February 2009, Figure 2,3 and 4; from a sample of 1,332 target firms, for the period 1996-2008.
Conclusion

Unfortunately, even in its revised version, the paper of Bebchuk et al remains “much ado about nothing”. That would be of little consequence as this is the fate of most academic papers. But the authors did trumpet their results in the Wall Street Journal as definitive demonstration of their case for activism and did insist on giving instructions to policy makers and regulators as to the right course for them to follow.

We do believe that when researchers draw adamant policy inferences and recommendations from their research data, they should make their raw dataset available to other researchers, as Reinhart and Rogoff have done for their data on financial crises and Saez, Piketty et al for their vast database on income inequality.