Comments on: "The Trouble with Instruments: Re-Examining Shock-Based IV Designs" by Atanasov and Black

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ABSTRACT

Atanasov and Black (2015) (AB) analyzes potential limitations of empirical studies that use shock-based IV designs, focusing specifically on our article that studies the effect of board independence on firm value (Duchin et al., 2010). With regard to our study, AB raises three concerns with our analysis. This note presents our reaction to AB's analysis. We agree with two of the concerns in the abstract; it turns out they do not matter for the substance of our analysis. We disagree on the critical issue concerning selection of covariates. As a guide to future research, we highlight the nature of the disagreement, and explain why we believe covariates should be motivated by theory, and why an atheoretical approach to selecting covariates can result in failure to identify effects that actually exist. An important lesson from the analysis is that researchers should exercise caution when including ad-hoc covariates in empirical specifications. We offer concluding thoughts about empirical research and causal inference.

1. Overview

Atanasov and Black have done a service to the research community by summarizing, replicating, and exploring the strengths and weaknesses of an increasingly popular method for producing causal estimates, what they call "shock-based IV designs" (Atanasov and Black, 2015, 2016 (forthcoming)). Their careful reading yields valuable insights about the state of the literature and potential limitations of previous research, and offers a road map of how to conduct research using these methods.

Our purpose in this note is to extend the discussion initiated by Atanasov and Black with specific reference to our paper (Duchin et al., 2010; henceforth DMO) that they analyze in depth in Atanasov and Black (2015; henceforth AB). We provide a detailed reaction to the specific concerns they raise. Critically, we believe their argument to include covariates without theoretical justification could cause more problems than it solves, and we illustrate that problem in the context of our paper. We also offer some additions to Atanasov and Black's road map of how to conduct research with shock-based IV designs. In particular, we call attention to the necessity of carefully selecting covariates. Finally, we offer some thoughts on the substantive issues examined in our paper regarding board independence and firm performance.

2. Detailed Reaction to Analysis of Original DMO Article

A. Original DMO Article

The purpose of DMO was to estimate the effect of board independence on corporate value and performance. There was a large preexisting literature on this question that generally concluded that board independence did not affect firm value. This conclusion was based primarily on regressions of firm performance on board independence. Because there are compelling anecdotal and theoretical reasons to expect board independence to respond to firm performance, the coefficients produced by the literature's regressions were difficult to accept as causal estimates. The main idea of DMO was to use the introduction of new rules and regulations that forced some firms to add independent directors as a quasi-natural experiment to study the effectiveness of outside directors. DMO sorts firms into treatment and control groups based on their ex ante noncompliance and compliance with the new regulations, respectively. The article's main specification uses the requirement of a fully independent audit committee

mandated by the Sarbanes-Oxley Act of 2002 (SOX); similar results were obtained using noncompliance with other regulations, including NYSE and Nasdaq requirements that the board of directors be comprised of a majority of independent directors.

The main specification of DMO estimates a two stage least squares (2SLS) model. In the first stage, the change in the percentage of independent directors on the board from 2000 to 2005 is predicted using noncompliance with the requirement of a fully independent audit committee. In the second stage, the predicted change in board independence is used to explain changes in ROA, Q, and annual stock returns. Thus, DMO provides difference-in-difference estimates, where the first difference is between compliant and noncompliant firms and the second difference is between 2000 and 2005. The sample is reduced to one before-versus-after observation per firm to avoid the serial correlation problem that arises when using long time-series in differences-in-differences estimation.

DMO reports two main findings: (1) The effect of board independence on firm value is quantitatively small at the point estimate, and statistically indistinguishable from zero. (2) When the effect of board independence is allowed to depend on the cost of information acquisition, as suggested by theory, an increase in board independence improves performance and value in low information-cost firms, and hurts performance and value in high information-cost firms.

Two points about the research design are worth noting. First, DMO uses a quasi-experimental design that lacks random assignment into treatment and control groups. Instead, firms are assigned into treatment and control groups based on noncompliance with the new board independence regulations prior to their introduction. Second, the article studies a period of time (2000-2005) during which a number of different rules concerning board independence were put in place. These include NYSE and Nasdaq requirements that the audit committee consist entirely of independent directors, later written into law by SOX, as well as later listing regulations implemented by NYSE and Nasdaq that required a majority of independent directors on the board, and minimal representation of outside directors on the compensation and nominating committees. We return to these features below.

B. Concerns Raised in AB

AB raises three potential concerns with the analysis in DMO. First, AB argues that the second-stage results in DMO would be more convincing with two separate first stage regressions, one for board independence and one for board independence interacted with information cost, instead of the single one for board independence that is used. This strikes us as a reasonable point, although it happens not to be materially important for the particular findings of DMO: when AB reestimates the DMO regressions with two separate first stage regressions instead of one, significance levels change marginally, but the coefficients are similar and the changes in significance levels do not push the estimates into the zone of "statistical insignificance."

Second, AB argues that DMO's information cost variable that is interacted with board independence ought to be included directly (not just interacted) in the regression. DMO in fact report a specification of this form (Table 8), and AB's replication produces similar results. Again, there are some minor changes to significance levels and coefficients, but the overall conclusion is unchanged.

AB's third argument, and most far-reaching for the DMO study, concerns covariate balance between the treatment and control groups. AB expresses a preference for a specification that trims about 20 percent of the sample (in order to increase overlap between treated and control firms) and further includes pre-shock board independence ("*%Indep*") as a predictor in the first and second stage regressions. Under this specification, the signs of the coefficients of interest do not change, and the magnitudes are similar, but the standard errors rise so that coefficients are not different from zero at conventional levels of statistical significance. AB arrives at its preferred specification by observing (Table DMO-1; also Table 2 in DMO) that before the shock, treated and control firms are similar on the outcome variables (ROA, Q, stock return) as well as on other firm characteristics such as leverage, intangible assets, and number of business segments, but that there is an imbalance in *%Indep* in the base year 2000. In effect, AB suggests that the DMO regressions are misspecified because of an omitted variable, *%Indep* in 2000.

C. What Control Variables to Include

AB's discussion raises an important issue concerning how to select control variables. AB prefers an expansive set of covariates to balance the treatment and control groups, with little attention paid to theoretical justification. While we generally agree with AB about the importance of a balanced sample, there are risks associated with including ad hoc balancing covariates. In particular, inclusion of covariates that are part of the economic mechanisms that the researcher is trying to investigate would over-control the specification by absorbing the treatment effect that is of interest. We believe this a serious concern with the preferred specification in AB.

The DMO identification strategy is to exploit forced increases in board independence; noncompliance with the requirement to have a fully independent *audit* committee is used as an instrument to identify forced increases in *board* independence. AB argues for including the pre-shock *%Indep* as a control variable, based entirely on the observation that its mean value is significantly different between the treatment and control groups. However, proceeding in this way essentially assumes that audit committee independence and board independence are two completely unconnected firm decisions, and that one can reasonably think of choosing one holding constant the other. In reality, there are good reasons to view the two choices as intertwined; specifically, it is possible to create an independent audit committee with little or no impact on board independence if the board already has a high percentage of independent directors. Achieving compliance with the requirement of a fully independent audit committee affects board independence to the extent that a company lacks enough independent directors to create an independent audit committee. Firms with few independent directors have to add independent directors because committee memberships tend to have limited overlap. Different directors tend to sit on different committees because different board committees typically hold their meetings at the same time, just before the general board meeting. Thus, controlling for pre-shock *%Indep*, as AB suggests, likely controls for forced increases in board independence to achieve compliance with the audit committee requirement that we use as an instrument for identification. Put differently, this control weakens the instrument, but in a mechanical rather than

substantive way.[1] (As an aside, we note that in Panel G of Table 5 of DMO, we estimated regressions using compliance status with majority board independence – which depends on AB's suggested control variable – as an instrument, and found similar results.)

D. Additional Empirical Analysis

In this section, we offer additional evidence to illustrate our concerns with AB's preferred approach. First, in Figure 1 we report scatter plots of pre-shock board independence and change in board independence. We consider both raw and instrumented changes in board independence. The figure shows that there is in fact a close relation between pre-shock board independence and change in board independence from 2000-2005, as one would expect for mechanical reasons. The figure helps explain why including pre-shock *%Indep* as a covariate would reduce the ability to predict changes in board independence after the shock: firms with a high proportion of independent directors initially did not need to change their board composition to comply with the audit committee requirement, while firms with a low proportion of independent directors had to increase board independence. *%Indep* is highly correlated with the very mechanism (changes in regulatory requirements) that the model is using to identify forced changes.

Next, to illustrate that AB's balancing covariate *%Indep* is half instrument half control, we construct two modified measures of board independence. The first measure, *Distance from 50% independence (if < 50%)*, is an instrumental variable that equals zero for firms compliant with the requirement of a majority independent board, and equals the difference between 50% and the percentage of independent directors for firms without a majority independent board. The second measure, *%Indep (if > 50%)*, is a control variable that equals the percentage of independent directors for firms compliant with the requirement of a majority independent board, and equals 50% for all firms without a majority independent board. While the two measures perfectly span *%Indep* (*%Indep (if > 50%)* minus *Distance from 50% independence (if < 50%)* equals *%Indep*), the first measure identifies forced increases in board independence for firms

[1] As another example, consider the inclusion of firm size in a study of the effect of agency problems on firm value. While its inclusion seems innocuous at first, to the extent that firm size captures "empire-building" behavior, its inclusion is in fact incorrect and may absorb or bias the estimated effect of agency problems on firm value.

below the 50% threshold whereas the second measure controls for board independence for firms above the 50% threshold.

In Table 1, we estimate regression models similar to those in Table DMO-5 in AB, replacing the percentage of independent directors with the two measures introduced above. As columns 1 and 2 of Table 2 show, the first stage models now yield significant F-statistics of 36.9 and 34.2, suggesting that the instruments are effective. Further, as shown in columns 3-5, the interaction term *Instrumented δIndep * Info Cost* is negative and economically similar to the original DMO results (see columns 5-7 of Table 3 in the original paper) across all measures of performance (ROA, Q, stock return). The effects are also statistically significant at the 10% level or better across all measures.

AB treat the full independence of the audit committee and the majority independence of the board as two completely separate board composition issues. Because they are linked, as shown in Figure 1, AB finds that few firms whose board independence is 25% or lower have an independent audit committee, and few firms whose board independence is 80% or higher do not have an independent audit committee. AB then truncates the sample by excluding firms whose board independence is lower than 25% or higher than 80%. In Table 2, we repeat our analysis from Table 1 in AB's truncated sample although the truncation is unwarranted. As can be seen, the results are not driven by the observations that AB truncated.

In particular, columns 1 and 2 of Table 2 show that the first stage models continue to yield significant F-statistics of 17.4 and 12.5, suggesting that our instruments are not "weak" even in the truncated sample. Further, as shown in columns 3-5, the interaction term *Instrumented δIndep * Info Cost* remains negative and economically similar to the original results across all measures of performance (ROA, Q, stock return). The effects are statistically significant at the 5% level for Q and stock return.

3. Lessons

One of the valuable contributions of AB is compilation of issues that might arise when estimating causal effects using shock-based IV designs. The list of potential problems and solutions in AB should provide a useful road map for future research. In the spirit of AB, we would like to extend the list. One insight that emerges from AB and this paper is that researchers should be careful when including covariates. An atheoretical approach

of simply including every variable at hand runs the risk that one of the covariates may absorb the effect under investigation if the covariate is mechanically linked to the variable of interest. There is no purely statistical way to choose covariates, so we believe that researchers should rely on theory where possible, and even common-sense intuition can be useful. A "kitchen sink" approach of including all variables at hand stands a real chance of failing to identify actual effects.

Another contribution of AB is pointing out that there are many ways that a shock-based IV design could produce misleading results. One obvious lesson is for researchers to be aware of and try to address as many inference challenges as possible. At the same time, we do not believe that researchers should avoid shock-based IV designs simply because they cannot perfectly address every possible challenge to inference. No research method is without limits. Even fully randomized controlled experimental designs, the "gold standard" in the eyes of some researchers, suffer from limitations such as unknown external validity and finite sample sizes (Leamer, 2010). In our opinion, research is best viewed not as "correct" versus "flawed" but as being located along a continuum with varying degrees of persuasiveness, recognizing that no study will be 100 percent conclusive. In this vein, we see value in shock-based IV designs so long as they offer a material improvement over previous research, even if not every inference problem can be completely resolved. The good should not be sacrificed for the perfect.

In terms of the substantive findings regarding board independence and corporate value, we believe our study ought to be viewed as one piece in assembling an overall picture. This overall picture of how boards affect firm performance has to be based on a wide array of studies using different data sets, different time periods, and different methods; and it has to be informed by and consistent with theory and practical observation. We believe DMO represents a useful step beyond the previously existing evidence that was based on simple regressions and correlations. For this reason, we think the finding of minor effects at the mean adds significant support to the prevailing view, and the finding of effects conditional on information conditions opens up a new direction for research. At the same time, our evidence is based on a single shock that took place among a general concern with corporate governance, and we would like to see evidence from less contentious periods before reaching conclusions about generalizability.

References

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Figure 1

Pre-shock Board Independence and Subsequent Independence Changes

This figure plots the proportion of board independence in 2000, before the shock, against changes in board independence from 2000 to 2005. δ*Indep* is the change in board independence from 2000 to 2005. *Instrumented* δ*Indep_1* and *Instrumented* δ*Indep_2* are the instrumented changes in board independence between 2000 and 2005. *Instrumented* δ*Indep_1* is calculated using the non-comply dummy. *Instrumented* δ*Indep_2* is calculated using both the non-comply dummy and its interaction with Info Cost.

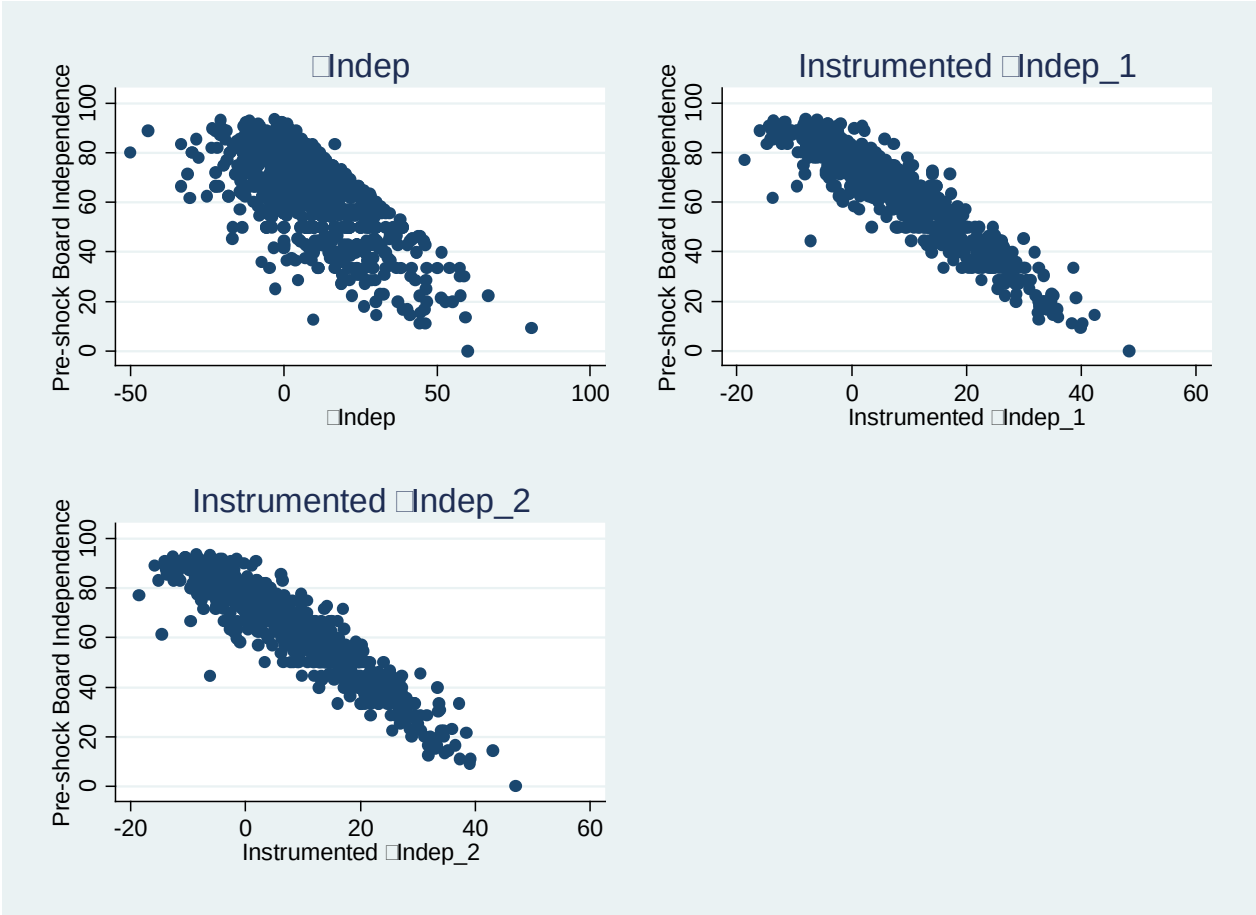

Table 1
Properly Controlling for Pct. Independent Directors: IV Results

2SLS regressions. We replace %Indep with the following two variables: (i) *Distance from 50% independence (if < 50%)*, defined as 0 for firms with an independent board, and as the difference between 50% and the actual fraction of independent directors for firms without an independent board; and (ii) %*Indep (if > 50%)*, defined as the actual percentage of independent directors for firms with an independent board and a constant 50% for firms without an independent board. Reported first-stage regressions are for sample with δQ as outcome variable. *t*-statistics, with industry clusters, are in brackets. *, **, *** indicate significance at the 10%, 5%, and 1% levels.

	First stage IV		**Second stage IV**		
	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	δIndep	δIndep*Info Cost	δROA	δQ	Mean return
Non-comply dummy	-0.526 [0.282]	-5.280*** [4.890]			
Non-comply dummy * Info Cost	5.02 [1.407]	13.313*** [5.017]			
Instrumented δIndep			0.218* [1.774]	1.253** [2.406]	0.040** [2.241]
Instrumented δIndep * Info Cost			**-0.529*** **[1.964]**	**-2.974***** **[2.743]**	**-0.097**** **[2.650]**
Info Cost	-5.002* [1.743]	2.964 [1.628]	-0.904 [0.325]	-25.731** [2.120]	-0.209 [0.536]
Distance from 50% independence (if < 50%)	0.736*** [9.632]	0.274*** [8.309]			
%Indep (if > 50%)	-0.556*** [13.760]	-0.280*** [10.913]	-0.04 [0.741]	-0.169 [0.949]	-0.011* [1.809]
Board Size	0.008 [0.046]	-0.056 [0.565]	-0.025 [0.169]	1.068 [1.477]	-0.012 [0.643]
Book Leverage	0.525*** [2.877]	0.206 [1.615]	0.980*** [2.833]	4.990*** [7.190]	0.044 [0.682]
Age	0.065* [1.769]	0.048*** [2.825]	0.024 [0.998]	0.661*** [4.842]	0.007 [1.634]
Market Cap	0.512** [2.670]	0.223 [1.628]	-0.454*** [2.948]	-15.261*** [6.755]	-0.376*** [7.836]
FF Industry Dummies	Yes	Yes	Yes	Yes	Yes
1st stage F-stat [p-value]	36.920 [0.000]	34.170 [0.000]			
Number of obs.	905	905	892	905	790
R^2	0.516	0.465	0.143	0.414	0.372

Table 2
Properly Controlling for Pct. Independent Directors: IV Results in Trimmed Sample

2SLS regressions. The sample is trimmed to %Indep ∈ (0.25 , 0.80]. We replace %Indep with the following two variables: (i) *Distance from 50% independence (if < 50%)*, defined as 0 for firms with an independent board, and as the difference between 50% and the percentage of independent directors for firms without an independent board; and (ii) *%Indep (if > 50%)*, defined as the percentage of independent directors for firms with an independent board and a constant 50% for firms without an independent board. Reported first-stage regressions are for sample with δQ as outcome variable. *t*-statistics, with industry clusters, are in brackets. *, **, *** indicate significance at the 10%, 5%, and 1% levels.

	First stage IV		**Second stage IV**		
	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	δIndep	δIndep*Info Cost	δROA	δQ	Mean return
Non-comply dummy	-1.273 [0.618]	-3.359*** [2.920]			
Non-comply dummy * Info Cost	7.535* [1.827]	10.103*** [3.477]			
Instrumented δIndep			0.376 [1.521]	2.828** [2.310]	0.060* [1.752]
Instrumented δIndep * Info Cost			**-0.682** **[1.449]**	**-5.842***** **[2.810]**	**-0.134**** **[2.114]**
Info Cost	-4.654 [1.381]	5.575*** [2.758]	0.857 [0.177]	12.228 [0.558]	0.280 [0.416]
Distance from 50% independence (if < 50%)	0.540*** [5.065]	0.219*** [4.340]			
%Indep (if > 50%)	-0.605*** [10.623]	-0.302*** [9.151]	0.013 [0.132]	0.031 [0.077]	-0.007 [0.571]
Board Size	0.056 [0.317]	-0.042 [0.362]	-0.042 [0.289]	1.081 [1.631]	-0.017 [0.702]
Book Leverage	0.589** [2.558]	0.287** [2.034]	1.242*** [3.732]	5.274*** [8.504]	-0.014 [0.466]
Age	0.049 [1.271]	0.045** [2.599]	0.014 [0.492]	0.744*** [5.262]	0.008* [1.838]
Market Cap	0.492 [1.660]	0.178 [1.212]	-0.443* [1.786]	-16.654*** [7.703]	-0.366*** [6.978]
FF Industry Dummies	Yes	Yes	Yes	Yes	Yes
1st stage F-stat [p-value]	17.380 [0.000]	12.540 [0.000]			
Number of obs.	719	719	707	719	628
R^2	0.385	0.392	0.171	0.435	0.370