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Commentary

Board Diversity: Should We Trust Research to Inform Policy?[1]

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Research on board diversity has received much attention recently. This is a natural consequence of the fact that female representation on corporate boards has risen to the top of policy agendas. It is, however, far from obvious whether and how such research is useful for policy discussions. This is our own fault. We, the researchers, often do not explain what we are trying to achieve. We are not always clear about the limitations of our work, and we often tolerate misinterpretation of our work by policymakers and the media.

In this short piece, I summarize what we have learned from the board diversity literature, and what is useful (and not useful) for policy debates and design. I organize my comments around three questions.

WHAT CAN WE LEARN ABOUT DIVERSITY FROM STUDIES OF BOARD DIVERSITY?

The answer is: not a lot. It is very hard to disentangle "diversity effects" from the effects of other individual and group characteristics that correlate with measures of diversity. Board directors, as a group of people, are highly non-representative of the general population. For example, there is no reason to believe that female board members have the same personality traits as those observed in the general population. Thus, findings of "gender effects" on boards are unlikely to be generalizable beyond the board. This problem is an example of what some researchers call *lack of external validity*.

External validity issues notwithstanding, we can still learn something. In particular, we may learn about what kind of behavior survives many rounds of selection and self-selection. As an example, consider the evidence that, in the general population, women are more risk-averse than men (see, e.g., Sapienza, Zingales, & Maestripieri, 2009). Because of selection, it does not follow that female directors should also be more risk-averse than male directors. In fact, in a sample of Swedish directors, Adams and Funk (2012) find that female directors are more risk-loving than male directors.

WHAT CAN WE LEARN ABOUT CORPORATE GOVERNANCE FROM STUDIES OF BOARD DIVERSITY?

Here I think we can be more optimistic. As I have argued elsewhere (Ferreira, 2010), there are six broad conclusions we can draw from the empirical literature on board diversity: (1) Firms appear to choose directors for their characteristics, and different types of firms choose different levels of director heterogeneity; (2) firms choose directors as a means to deal with the external environment; (3) CEOs and top executives appear to prefer directors who are similar to themselves; (4) social networks and commonality of backgrounds appear to affect director appointments and the dynamics of the board; (5) directors from minority groups perceive their minority status as a hindrance to their work as a director; (6) minority directors may serve interests other than those of shareholders.

My work with Renée Adams (Adams & Ferreira, 2009) reveals some of the interactions between gender and governance. We find evidence that female directors are more independent (from management) than male directors. Crucially, this is true for both nominal and *de facto* independence. We find that women have better attendance at board meetings, are more likely to sit on monitoring committees, and are more likely to force CEO departures after poor stock price performance. In short, female directors are more likely to be tough monitors of CEOs.

Our interpretation is not that independence is a female trait. We cannot rule out the possibility that the observed independence of female directors is explained by other unobserved characteristics, such as different social and busi-

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ness networks of female and male directors. It is important to keep this in mind, especially when interpreting findings that show an association between board gender diversity and firm performance. Because we cannot disentangle "gender effects" from the effects of independence, we interpret our findings as suggesting that board gender diversity might be a better proxy for board independence than conventional measures, which are based on regulatory definitions of independence.

As we have argued elsewhere (Adams & Ferreira, 2007), independence comes with costs and benefits, and there is thus no reason to expect the average firm to benefit from adding more women to its board. In fact, our evidence also indicates that the link between firm performance and board gender diversity is tenuous. The estimated effects of board diversity vary substantially across empirical specifications and methods. Such effects are also heterogeneous, i.e., they depend on firm characteristics. Some firms appear to benefit from adding women to the board, while others would probably experience a decline in performance. This should come as no surprise, given that gender diversity is strongly related to board independence. The literature has taught us that board independence has no obvious impact on firm performance (see the survey by Adams, Hermalin, & Weisbach, 2010).

There is a fascination in the management and economics literature with estimating the impact of female directors on firm performance and profitability. This is understandable. But this literature is often too quick to jump to strong conclusions on the basis of the flimsiest of evidence. Establishing causality requires strong assumptions. In many academic studies and policy pieces, the usual disclaimer that "correlation does not imply causation" often comes in small print. Econometric black boxes do not solve this problem either. State-of-the-art identification strategies often look superficially convincing, but they rarely deliver the goods.

To illustrate this point, I now turn to the literature on mandatory gender quotas. The introduction of a law establishing a 40 percent quota for female directors in Norwegian firms is often seen as the "Holy Grail" of econometric identification. The most successful papers in this literature are Ahern and Dittmar (2012) and Matsa and Miller (2013) – henceforth AD and MM, respectively. I will focus only on these two papers because they are the best known and most influential ones. These works use the introduction of the quota as a "natural experiment," so that an increase in board gender diversity can be seen as an exogenous event. Notwithstanding some differences in focus and methodology, both papers reach a common conclusion: The introduction of board gender quotas in Norway had a negative effect on firm performance.

This conclusion is certainly plausible. Any regulation that forces firms to do things that they currently do not do is likely to impose some costs. In this particular case, firms affected by the quota are forced to hire new female directors to comply with the law, and there is no presumption that these new directors will be as qualified as the incumbent ones. Indeed, such a mandatory quota system is expected to reduce profitability, unless (1) managerial talent is in excess supply, or (2) most firms engage in Beckerian taste-based discrimination (Becker, 1957), and are thus willing to sacrifice profits just to avoid employing women. Assumption (1) does not seem compatible with the observed levels of pay for top executives. Assumption (2) is certainly possible in some cases, but highly unlikely to be the norm in today's competitive landscape.

Although I believe in the plausibility of this conclusion, I am less convinced about the actual evidence in its favor. Without getting into the specific details of each paper, here I discuss five difficulties that are common to all papers that use the Norway quota as a natural experiment to identify the effect of female directors on performance and profits.

The first issue is the timing problem. The "natural experiment" is not well defined. The exact date of the "quota shock" is subject to interpretation, and could be selected *ex post* to reverse-engineer empirical findings (I am not implying that findings were indeed reverse-engineered). The gender quota was first discussed in 1999. A law suggesting a 40 percent quota passed in 2003, but there was no penalty for noncompliance. In 2005, liquidation was established as the penalty for noncompliance. Firms were given two years to adjust. Until the beginning of 2008, a significant number of firms still had not complied. Full compliance was only achieved in 2008. There is thus too much freedom to define the shock. The proportion of female directors on the boards of Norwegian companies increased (roughly) monotonically from 2001 to 2009. AD choose 2003 as their event date. MM choose 2006. Nygaard (2011) chooses 2005 as the event date and finds significantly different results.

The second issue is the choice of control group. Because the gender balance law applies to all Norwegian firms that have a specific organizational form (public limited liability companies), there is no natural control group to which the "treated" firms should be compared. An ingenious solution is to use firms with higher pre-shock board diversity as controls. However, firms choose when to adjust board composition, and such a decision could be affected by past or expected firm performance. Alternatively, one could use non-public limited liability companies as controls. However, firms with different organizational forms are likely to be different in other (unobserved) characteristics as well. Organizational form is also an endogenous choice and may be changed to avoid regulation.

Using firms from other Nordic countries also yields a very imperfect control group. These firms operate in different legal environments, under different currencies and macroeconomic conditions. There are too many reforms and other things happening at the same time. Spillover effects are also possible: Non-Norwegian firms might be affected (directly or indirectly) by what happens in Norway. Such firms may react to anticipated changes in law, public opinion, and business practices in their own countries. Adams and Kirchmaier (2013) show that female board participation in Finland and Sweden increased dramatically just before 2006, mirroring the trend in Norway. In contrast, in Denmark female board representation remained flat at 11–13 percent from 2003 until 2010. I just find it hard to believe that firms in these countries are good counterfactuals for Norwegian firms. In a hypothetical world in which Norwegian quotas did not exist, other Nordic firms would likely behave differently from what they do now.

The third issue is sample selection. Regardless of how control and treatment groups are defined, firms self-select into both treatment and control groups, plausibly to avoid treatment. Bøhren and Staubo (2014) show that roughly 50 percent of exposed firms changed their organizational form after the introduction of the quota. They also argue that some firms consciously avoided adopting the organizational form exposed to regulation.

Fourth, there is a multitude of confounding effects. There are other governance-related reforms contemporaneous to the introduction of gender quotas. This is especially problematic because the potential "event window" (2003–2008) is so wide. For example, the Norwegian Code of Practice for Corporate Governance was implemented in fiscal year 2005 (with some small changes in 2006). Norway also adopted IFRS accounting rules in 2005. How can we be sure that these are not behind the observed changes in performance?

A final issue is the mechanism. AD and MM offer different explanations for the effect of quotas on firm performance. AD argue that newly appointed women are younger and less experienced than current directors. MM explicitly disagree with AD, and suggest instead that the effects of the quota are explained by a "female leadership style." In their words, "our paper is unique in its examination of how the gender quota changed the style of corporate leadership, and shows that these changes in corporate strategy cannot be explained by board member age or experience" (p. 139).

I find it surprising that no one mentions board independence. Bøhren and Staubo (2013) document that the introduction of the quota had a significant impact on board independence: Average independence increased from 46 percent in 2003 to 67 percent in 2008. This is explained by the fact that 84 percent of female directors in Norway are independent, while only 50 percent of male directors are independent.

To summarize, there are too many problems with the "causal" evidence on the effect of quotas on performance. It's fair to say that we don't really know whether and how quotas affect the financial performance of firms.

IS RESEARCH ON BOARD DIVERSITY USEFUL FOR POLICY DISCUSSIONS?

Proponents of board gender balance regulations often make a "business case" for female board representation. Such a case can take many shapes and forms, but it usually amounts to citing some study (academic or not) showing that female board representation is positively related to firm value or profits.

I am skeptical of such a strategy. First, reliance on a business case can easily backfire. As I have argued elsewhere (Ferreira, 2010), making a business case for women in the boardroom on the basis of statistical evidence linking women to profits logically creates the possibility of a business case against women should the evidence suggest that women reduce profits. I am not sure that this is the discussion we want to have.

Second, perhaps a different and more convincing business case can be made at the occupational choice stage, that is, when young women start their careers. A larger pool of qualified professionals in business may benefit society as whole. It is, however, not obvious that board quotas would help to achieve this goal. By design, board quotas force firms to hire female directors from the current pool of businesswomen. Such a policy does not immediately change the composition of the pool of director candidates.

We currently have no evidence on the effect of board quotas on early career decisions of women. It is conceivable that, because of a more balanced gender composition at the top, young women will become more likely to pursue business-related degrees and keener to consider careers in areas currently dominated by men, such as business and finance. Employers could also become keener to recruit women. But even if such things happen, there could be unintended consequences. What would happen to the high-skilled sectors where women are relatively well represented (such as law, medicine, education, and the public sector)? These sectors are the most likely to suffer if governments implement policies that provide incentives for their young professionals to choose alternative careers.

Finally, it has never been clear to me why the board is the place to start with policies that aim to promote better female representation in business. Most proponents of board quotas believe that, if we smash the glass ceiling at the board level, we will also reduce discrimination at lower levels. I am not sure that this conclusion follows. If a group is more likely to be promoted to the top, perhaps employers will become more demanding when first recruiting from this group. I would like to see more empirical and theoretical research on this issue.

To conclude, current research does not really support a business case for board gender quotas. But it does not provide a case against quotas either. There is little hope that any (credible) research will ever do so. Causal effects will always be too hard to estimate, unless governments unintentionally help us with badly designed policies that randomly assign quotas to some firms and not to others.

I do not think that the lack of evidence that female board representation improves profitability is a problem. The business case is a bad idea anyway. When discussing policies that promote women in business, it is better to focus on potential benefits to society that go far beyond narrow measures of firm profitability.

NOTE

1. This comment is based on a keynote address given at the Diversity in Boards Workshop at De Nederlandsche Bank in Amsterdam, December 2013.

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