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**SUPERIOR COURT OF THE STATE OF CALIFORNIA**  
**COUNTY OF LOS ANGELES**

ROBIN CREST, EARL DE VRIES, and  
JUDY DE VRIES,

Plaintiffs,

v.

ALEX PADILLA,<sup>1</sup> in his official capacity as  
Secretary of State of the State of California.

Defendant.

Case No. 19STCV27561

**DECLARATION OF JONATHAN  
KLICK IN SUPPORT OF PLAINTIFFS'  
OPPOSITION TO DEFENDANT'S  
MOTION FOR SUMMARY JUDGMENT**

**Reservation No. 667418313333**

Hearing: September 21, 2021  
Time: 9:30 a.m.  
Place: Dept. 38  
Judge: Hon. Maureen Duffy-Lewis

Action Filed: August 6, 2019  
FAC Filed: September 20, 2019  
Trial Date: October 25, 2021

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<sup>1</sup> Dr. Shirley N. Weber is the current Secretary of State.

1 **DECLARATION OF JONATHAN KLICK, Ph.D., J.D.**

2 I, JONATHAN KLICK, declare:

3 1. I am over the age of 18 years and a U.S. citizen. I know the following facts based on my  
4 own personal knowledge, and, if called as a witness, I could and would testify to these facts.

5 **Background and Experience**

6 2. I earned a Ph.D. in economics from George Mason University in 2002 and a J.D. from  
7 the George Mason University School of Law in 2003. I was hired as an assistant professor of law in  
8 2004 and was named the Jeffrey A. Stoops Professor of Law and Economics in 2005. I was hired as a  
9 full professor of law at the University of Pennsylvania in 2008 and continue in that role to the present. I  
10 have also held the Erasmus Chair of Empirical Legal Studies at Erasmus University Rotterdam since  
11 2009, where I teach courses in empirical research design and causal inference to masters and doctoral  
12 students in law and economics.

13 3. I was appointed the Maurice R. Greenberg Visiting Professor at the Yale Law School for  
14 the Fall 2013 semester, and I have held other visiting professor positions at the following law schools in  
15 the U.S.: Columbia University, Northwestern University, and the University of Southern California. I  
16 was appointed the inaugural Dean's Distinguished Fellow at the Villanova University Charles Widger  
17 School of Law in 2017.

18 4. I have been a visiting professor of economics at the University of Canterbury in  
19 Christchurch New Zealand, where I taught graduate-level econometrics. I have taught as a visiting  
20 professor in the economics departments or law schools at the following foreign universities: Waseda  
21 University (Tokyo, Japan), University of Ljubljana (Ljubljana, Slovenia), Bar-Ilan University (Ramat  
22 Gan, Israel), Goethe-Universität Frankfurt(Frankfurt, Germany), and the University of Hamburg  
23 (Hamburg, Germany). I have taught courses on empirical methods and causal inference at the Max  
24 Planck Institute (Hamburg and Jena, Germany) and at the Swiss National Bank's Study Center  
25 (Gerzensee, Switzerland).

26 5. In addition to classes on econometrics, statistics, and causal inference, of relevance to this  
27 declaration, I have taught corporate law at Florida State University, the University of Pennsylvania, and  
28 Waseda University. I have also taught expert and scientific evidence law at the University of

1 Pennsylvania, as well as to federal and state judges through workshops sponsored by centers at George  
2 Mason University.

3 6. I previously served as a senior economist for the Rand Corporation (Santa Monica, CA),  
4 and I have been the co-editor of the International Review of Law and Economics since 2012. I have  
5 taught empirical methods and causal inference to international, federal, and state judges, as well as  
6 federal and state regulators and law professors at least annually for the Law and Economics Center of  
7 George Mason University since 2009. I have also taught empirical methods and causal inference  
8 regularly to international regulators for the Global Antitrust Institute.

9 7. My research includes work in applied econometrics and focuses on using statistical tools  
10 to identify the causal effects of laws and regulations on individual behavior. My work has been  
11 published in numerous peer-reviewed journals in economics, law, psychology, and a host of other fields.  
12 I was chosen to write the chapter on empirical law and economics for the Oxford Handbook of Law and  
13 Economics (2017).<sup>2</sup> Also of relevance to this declaration, I have published methodological papers on  
14 using statistical methods to draw causal inferences in the event study framework used extensively in  
15 corporate finance. I have also published articles<sup>3</sup> on the reliability of work using the implicit association  
16 test (used to identify unconscious bias) in one of the American Psychological Association's journals.

17 8. Additional details about my background are provided in my CV which is attached as  
18 Exhibit 1.

### 19 **Introduction**

20 9. Declarations offered in support of Secretary Padilla's motion for summary judgment  
21 assert that women are underrepresented on the boards of directors of firms headquartered in California,  
22 and this underrepresentation is due to discrimination.<sup>4</sup> These declarations also claim that research  
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24 <sup>2</sup> "Empirical Law and Economics," (with Jonah Gelbach) Oxford Handbook of Law and  
Economics (Oxford University Press, 2017).

25 <sup>3</sup> Hart Blanton, James Jaccard, Jonathan Klick, Barbara Mellers, Gregory Mitchell, and Philip  
26 Tetlock (2009), "Strong Claims and Weak Evidence: Reassessing the Predictive Ability of the IAT,"  
Journal of Applied Psychology, 94(3): 567-582. Hart Blanton, James Jaccard, Jonathan Klick, Barbara  
27 Mellers, Gregory Mitchell, and Philip Tetlock (2009), "Transparency Should Trump Trust," Journal of  
Applied Psychology, 94(3): 598-603.

28 <sup>4</sup> See, for example, DECLARATION OF ALISON KONRAD, PH.D., IN SUPPORT OF  
SECRETARY OF STATE'S MOTION FOR SUMMARY JUDGMENT, at paragraphs 10 and 11.

1 demonstrates that women have a positive effect on many different aspects of corporate performance.<sup>5</sup>

2 10. In my opinion, the evidence offered in these declarations supporting Secretary Padilla's  
3 motion for each of these points (underrepresentation of women on boards, discrimination as the cause of  
4 this underrepresentation, and that research shows a differential benefit of appointing women, as opposed  
5 to men, in terms of firm performance) is deficient and unreliable.

6 11. In the sections that follow, I analyze the arguments and the evidence provided in three of  
7 the declarations provided in support of Secretary Padilla's motion (Alison Konrad, Cindy Schipani, and  
8 Jessica Grounds) in detail. Before I separately address those declarations, I provide a general overview  
9 of my analysis of the underrepresentation claim, the discrimination claim, and the beneficial effects  
10 claim. I also provide a basic primer in statistics and causal inference to aid in understanding the  
11 evidence provided in the declarations. After analyzing the declarations, I then provide a general  
12 conclusion. After my report, I provide my CV and I include a review I wrote regarding a similar  
13 proposal by the Nasdaq stock exchange to require firms to increase the number of women on the boards  
14 of firms traded on the exchange.

15 **The Claim that Women are Underrepresented on Boards**

16 12. All three of the declarations, Konrad, Schipani, and Grounds, share the assertion that  
17 there are too few women on corporate boards. However, they never explicitly answer the question of  
18 how appropriate representation would be determined. Implicitly, it appears as though their shared  
19 assumption is 50-50 parity. This comes through most clearly in Schipani who uses Bureau of Labor  
20 Statistics data (from the Current Population Survey) to note that women make up more than 50 percent  
21 of management, professional, and related occupations and represent similar proportions of people  
22 entering law and medical schools and more than half of those receiving doctoral degrees and nearly half  
23 of all master's degrees in business. However, contrasted with this, she notes sources indicating that  
24 board seats are not distributed equally between men and women.

25 13. As I detail below in my section on Schipani's declaration, the 50 percent numbers she  
26 cites for the education metrics are very recent. For example, it is only this year that the University of

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28 <sup>5</sup> See, for example, DECLARATION OF ALISON KONRAD, PH.D., IN SUPPORT OF SECRETARY OF STATE'S MOTION FOR SUMMARY JUDGMENT, at paragraph 12.

1 Pennsylvania's Wharton School became the first of the elite business schools to enroll an MBA cohort  
2 with more women than men. These elite business schools are the kind of places that train future board  
3 members. If they are only now achieving parity, it will be a number of years until the available pool of  
4 elite MBAs with the requisite job experience generally necessary to be considered for a board position  
5 will reach anything close to parity. The same is true for some of the other common educational  
6 pathways (lawyers and medical doctors) and is very far from parity even at the graduation stage for  
7 science and health PhD's, which is one of the other common routes to a corporate board.

8 14. Other than asserting that the male/female split should be closer to 50/50, the declarations  
9 in support of Secretary Padilla's motion do not provide a compelling determination of what appropriate  
10 representation would be. Instead, they merely assert that women are underrepresented.

11 **Discrimination as Cause of "Underrepresentation" of Women on Corporate Boards**

12 15. As mentioned earlier, one of the two key assertions made in the Konrad declaration is  
13 that the supposed underrepresentation of women on boards is due to discrimination. As noted earlier,  
14 whether the current number of women represents underrepresentation is far from given, but even if it is  
15 assumed, Konrad's evidence of discrimination is not compelling. First, most of the studies she cites  
16 regarding discrimination or bias toward women, even if they demonstrate exactly what she claims, have  
17 nothing to do with estimating discrimination or bias in the context of choosing corporate directors. The  
18 majority of the studies she cites examine bias in the general population or, at best, bias in the workplace  
19 by her own description. While I do not generally find these studies compelling for methodological  
20 reasons, they are not relevant to demonstrating discrimination in the choice of corporate directors.  
21 Konrad's discussion of these studies is irrelevant at best and possibly misleading.

22 16. Of the handful of studies that examine anything related to board members, Arjun Mitra,  
23 Corinne Post, Steve Sauerwald (2021) most directly examines potential bias in director appointments.  
24 The authors look at the fraction of voters who vote against or abstain from voting for board supported  
25 candidates. To identify causality, they implement an instrumental variables model. This method is  
26 described more in depth below, but essentially this method requires the researcher to find some variables  
27 that predict the variable of interest (in this paper: whether the candidate is a woman or not) but is  
28 otherwise unrelated to the outcome studied (votes against the candidate). This article uses for its

1 instruments the number of female directors serving on other corporate boards in the geographic area  
2 around a given firm's headquarters and the number of connections between male board members and  
3 females on other boards those men sit on. As discussed below, a number of papers use these  
4 instruments, and they are likely invalid instruments.<sup>6</sup>

5 17. However, even if one takes the paper's results as given, it appears that Konrad does not  
6 tell the whole story about whether this paper supports the notion that women face discrimination in  
7 board elections. Konrad focuses on the result that women in certain circumstances appear to be treated  
8 worse than men (e.g., when the director attends fewer board meetings). While it is true this interaction  
9 (female candidate interacted with attendance issues) generates more negative votes (no votes or  
10 abstentions), this paper also shows that being a woman board member actually leads to fewer negative  
11 votes in general. That is, the primary effect identified in this paper is that female directors actually are  
12 treated more positively than male directors, and the effect is statistically significant in every  
13 specification estimated (Table 3). This is not supportive of the idea that women face discrimination  
14 when board personnel decisions are made; it actually shows the opposite.

15 18. The interaction effect Konrad focuses on is problematic. Even if one accepts the authors'  
16 identification strategy, they only instrument for (that is, account for omitted variable bias) the female  
17 variable. They do not instrument for the variables that are interacted with the female variable. Even if  
18 one believes the primary effect of the female variable (i.e., treated more favorably) because the  
19 instrumental variables approach is accepted, there is no reason to believe that the interaction effects  
20 represent credible causal estimates.

21 19. The Benton (in press) article examines the number of boards female board members  
22 serve on, which could be a metric of discrimination, if access to additional boards is blocked due to the  
23

24  
25 <sup>6</sup> Briefly, both of these instruments likely affect firm performance directly and therefore are not  
26 valid instruments. If it is true that firms with female directors are better performing companies (as  
27 Konrad asserts), firms located in areas where most neighbor firms have many women directors would  
28 benefit from general agglomeration effects from being near well-performing firms. The other  
instrument can be problematic since multiple board seats held by men will often be in similar firms. If,  
as Konrad asserts, women on boards improve firm performance, this means that a firm's competitors  
will be better performing leading to fiercer competition in the industry which would affect firm behavior  
and performance.

1 board member's sex. While this paper's statistical approach is not credible,<sup>7</sup> even if the results are taken  
2 as given, the paper shows a number of results showing that women are treated more favorably in terms  
3 of board appointments. For example, of the four specifications shown in Table 3, two show that being a  
4 woman has a statistically significant positive effect on the number of boards the director sits on, and two  
5 show a statistically significant negative effect. In Table 4, one specification shows a statistically  
6 significant positive effect and two show a statistically significant negative effect, with the fourth  
7 estimate being positive but not statistically significant. Thus, this paper shows mixed results regarding  
8 the primary effect of being a woman on gaining additional board seats.

9         20. McDonald and Westphal (2013) is cited by Konrad for the proposition that women board  
10 members receive less mentoring which inhibits their ability to secure additional board seats. This paper  
11 does nothing to isolate causality and likely suffers from omitted variables bias, but even if its results are  
12 taken as given, the paper finds that being a woman has zero statistical effect on board appointments  
13 (Table 4). While the paper does attempt to estimate the joint effect of being a woman and not receiving  
14 mentoring, finding a jointly negative effect, at best, this paper provides mixed evidence on the claim that  
15 women are discriminated against in board appointment decisions.

16         21. Schipani likewise presents very little evidence of discrimination in the board appointment  
17 context, as opposed to assertions of general bias against women. The closest she comes to a directly  
18 relevant study is the Tinsley et al (2017) study discussed in more depth below, which purports to find  
19 that firms tend to replace departing male board members with males and departing female board  
20 members with females, which Schipani declares generates a bias against women.<sup>8</sup> However, as noted  
21 below, even if one takes the paper's results at face value, the estimates suggest that the tendency to  
22 match is stronger for a departing female candidate than it is for a departing male candidate, which leads  
23 to a net positive effect for women which will reduce the male-female differential over time.

24         22. In sum, the Konrad and Schipani declarations present very little evidence that is even  
25 relevant to the claim that women face discrimination in board appointments. Further, their presentation

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26 <sup>7</sup> The models use random effects which only yield causal estimate if the effects of variables that  
27 are not accounted for in the model are unrelated to the variables that are included. This is a very strict  
28 and highly unlikely assumption. Further, the models include the lagged dependent variable as a control  
variable which is known to generate statistical bias.

<sup>8</sup> Schipani declaration at paragraph 37.

1 of the direct evidence on this issue is misleading since each of the papers they rely on has mixed  
2 evidence and some of the papers actually indicate that women are treated favorably in the board  
3 appointment context.

4 **The Claim that Increased Female Board Participation Would Improve Firm Outcomes**

5 23. I examine this claim extensively in Klick (2021),<sup>9</sup> finding that the empirical evidence is  
6 mixed at best, and a conservative evaluation would indicate that there are not statistically significant  
7 effects of female board participation on most firm outcomes. In my earlier analysis, I found that  
8 proponents of regulations requiring the appointment of more women tend to ignore papers finding no  
9 effects or even negative effects of female board members on firm outcomes. Further, I find that the  
10 proponents are willing to credit studies that are methodologically unreliable if they purport to find a  
11 positive effect of female board members on outcomes. Similar conclusions were reached by Harvard  
12 corporate law professor Jesse Fried.<sup>10</sup> These conclusions track with an earlier assessment by noted  
13 feminist scholar Deborah Rhode and Amanda K. Packel: “After exploring the strengths and limitations  
14 of various methodological approaches and survey findings, [we conclude] that the relationship between  
15 diversity and financial performance has not been convincingly established.<sup>11</sup>” More systematic meta-  
16 analyses of this literature likewise tend to find little evidence of a systematic effect of female board  
17 participation on corporate outcomes or behavior.<sup>12</sup>

18 24. The cherry-picking of studies by proponents of board diversity mandates and the  
19 declarations of Konrad and Schipani is exemplified by them ignoring the substantial literature  
20 examining the “natural experiment” provided by Norway’s 2003 law which required boards to appoint  
21 women to 40 percent of their seats. A number of papers, using sophisticated methodologies find that

22 \_\_\_\_\_  
23 <sup>9</sup> Jonathan Klick (2021), Review of the Literature on Diversity on Corporate Boards, American  
Enterprise Institute Monograph (included as an appendix).

24 <sup>10</sup> Fried, Jesse M., Will Nasdaq's Diversity Rules Harm Investors? (March 31, 2021). European  
Corporate Governance Institute - Law Working Paper No. 579/2021, Available at SSRN:  
25 <https://ssrn.com/abstract=3812642> or <http://dx.doi.org/10.2139/ssrn.3812642>, but see Painter, Richard  
26 W., Board Diversity: A Response to Professor Fried (April 11, 2021). Available at SSRN:  
<https://ssrn.com/abstract=3824245> for a contrary take on Fried’s evidence.

27 <sup>11</sup> Deborah L. Rhode and Amanda K. Packel, “Diversity on Corporate Boards: How Much  
Difference Does Difference Make?,” Delaware Journal of Corporate Law 39, no. 2 (2014): 377–426.

28 <sup>12</sup> See Jonathan Klick (2021), Review of the Literature on Diversity on Corporate Boards,  
American Enterprise Institute Monograph, pp. 16-17.



1 appointing women led to a worsening of financial performance and value, and it led a number of firms  
2 to go private to avoid the law.<sup>13</sup> Even ignoring the selective citation practices, the papers cited by  
3 Konrad and Schipani fare poorly when subjected to methodological scrutiny. As detailed below, there is  
4 very little evidence offered to support the notion that female board participation improves firm  
5 performance. When the broader literature is examined, as suggested by the meta-studies or a more  
6 detailed paper-by-paper investigation, the correct evaluation of the literature is that appointing more  
7 women to corporate boards has no systematic effect on firm behavior, performance, or value, and the  
8 Norwegian experience suggests that bad outcomes are possible as well.

### 9 **Empirical Primer**

10 25. Before diving into the empirical studies cited by Konrad and Schipani, it is useful to  
11 provide a short guide to causal inference in policy and social science studies.<sup>14</sup> A similar treatment is  
12 available in the Klick (2021) monograph appended to this declaration.

13 26. Correlations measure the degree to which two random variables move together. Often  
14 measured as a correlation coefficient, a positive correlation indicates that the two variables tend to move  
15 in the same direction on average (if one goes up, the other tends to do so as well), while a negative  
16 correlation indicates that they tend to move in opposite directions on average (one goes up, the other  
17 tends to go down). A zero correlation indicates the two variables have no systematic relationship to  
18 each other. Correlations do not necessarily imply any causal relationship. Changes in A might cause  
19 the changes in B, while it might be the other way around, or both might be caused by other variables, or  
20 the observed tendency to move together might be mere coincidence.

21 27. The possibility of mere coincidence being the source of the correlation declines as more  
22 and more data are examined. All other things equal, the likelihood of a coincidental correlation declines  
23 as a sample size grows. However, the causality of the relationship cannot be deduced merely by looking  
24 at more data or by seeing the correlation arise many different times, since each time the correlation

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25 <sup>13</sup> For details, see Jonathan Klick (2021), Review of the Literature on Diversity on Corporate  
26 Boards, American Enterprise Institute Monograph, pp. 14-15.

27 <sup>14</sup> For a textbook treatment, see Guido W. Imbens and Donald B. Rubin (2015), Causal Inference  
28 for Statistics, Social, and Biomedical Sciences: An Introduction. For a shorter treatment, see Jonah B.  
Gelbach and Jonathan Klick (2017) “Empirical Law and Economics,” in Oxford Handbook of Law and  
Economics (Oxford University Press).

1 might actually be influenced by other variables that also happen to be present each of the times the  
2 correlation between A and B is observed.

3 28. To get closer to causation, it is important to account for the other influences on the  
4 correlation. Regression analysis is the common way to do this in non-experimental settings (i.e., setting  
5 where real world data are used, as opposed to researcher-generated laboratory data). Regression allows  
6 a researcher to adjust for the influence of the other variables to see what is left over in terms of the  
7 correlation between A and B. That is, regression adjustment allows the researcher to partial out the  
8 effects of other variables. In principle, if all of the variables that affected, say A, were included in the  
9 regression, whatever regression estimate of the correlation between A and B was left over (sometimes  
10 called the regression coefficient) would be the causal effect of B on A. That is, how much of a change  
11 in A is caused by a one-unit change in B.

12 29. Unfortunately, it is not generally possible to adjust for all of the variables that affect A  
13 since oftentimes those variables are not known or at least data on them are not available to the  
14 researcher. If the researcher omits some of these variables, the omitted variables are often referred to as  
15 unobservable or at least unmodeled.

16 30. If a regression is estimated with some relevant variables omitted, the estimated causal  
17 effect of B on A is likely to suffer from what is known as omitted variables bias (also referred to  
18 alternatively as endogeneity, simultaneity, selection effects, reverse causality, and a host of other names;  
19 for our purposes, we will treat them all as being equivalent). Formally, omitted variables bias occurs  
20 when a variable that influences A (or many such variables) is left out of the regression and that omitted  
21 variable (or variables) are correlated with B. Intuitively, in this situation, the estimated correlation  
22 between A and B will include some of the effects of the omitted variables. The effect of this bias could  
23 lead the estimate to be too big, too small, or correct by accident (e.g., the bias from leaving one variable  
24 out leads the estimate to be too big, while the bias from leaving another variable out leads the estimate to  
25 be too small, with the biases cancelling each other out).

26 31. Experimental studies (like a drug trial) sidestep this omitted variable bias problem by  
27 randomizing the variable of interest (say, giving a subject the drug to be studied vs giving them a  
28 placebo). When the outcome variable is examined via regression, while there are many omitted

1 variables, the variable of interest (getting the treatment) was assigned randomly so there can be no  
2 correlation between it and the omitted variables.

3 32. Experiments are not always feasible in social science or policy analysis because it is  
4 perhaps unethical to randomly assign people to various conditions or it is unconstitutional to randomly  
5 enforce a law against some people but not others. Even when they are feasible, while they would allow  
6 us to estimate an unbiased causal effect, the potentially artificial nature of the experimental setting may  
7 lead the experimental subjects to act differently than they would in a real-world setting. This is  
8 sometimes referred to as a problem of external validity (i.e., is it possible to extrapolate the findings of  
9 the experiment to the real-world setting).

10 33. Social scientists use a number of approaches to attempt to overcome the omitted variables  
11 bias problem when experiments are not possible. Perhaps the most credible are so-called quasi-  
12 experimental methods or “natural experiments.” In these natural experiments, the researcher leverages  
13 some real world “shock” that changes a policy variable of interest for some people but not for others. In  
14 this setting, the researcher compares the behavior or outcomes for those affected by the shock before and  
15 after the shock takes place. This before/after differential is then compared to the contemporaneous  
16 before/after differential for a similar group that was not affected by the change. This comparison or  
17 control group provides the presumed counterfactual comparison (what would have happened if the  
18 shock had not occurred) against which the treatment group’s changes can be compared. If the changes  
19 are similar between the two groups, the estimated causal effect of the shock is zero since the unaffected  
20 people acted similarly to the affected people. Any difference between the groups is presumptively  
21 attributed to the shock.

22 34. Such natural experiments are judged on two dimensions: 1) was the shock really as good  
23 as random or did it happen to the treated group for some reason that might be related to their attributes  
24 (even their unobservable attributes) and 2) were the treatment and control groups sufficiently similar  
25 (even in their unobservable attributes) such that the control group provides a good counterfactual for the  
26 treatment group. To the extent that the shock is potentially not really random or that the control group is  
27 not sufficiently similar, omitted variable bias potentially remains. While these questions are generally  
28 not formally testable (since doing so would require having access to data on unobservable attributes), the

1 assumptions are subjected to qualitative or intuitive scrutiny.

2 35. Since natural experiments are not always available, researchers have developed other  
3 approaches that attempt to approximate such a design. Especially in the literature relied on by Konrad, a  
4 common technique is the instrumental variables (IV) technique (sometimes called two stage least  
5 squares). In this approach, the researcher attempts to find an instrument that is like a shock to the  
6 variable of interest. That is, an instrument is something that shifts the variable of interest but is not  
7 otherwise related to the outcome being examined. If such an instrument can be found, the researcher  
8 examines only the variation in the endogenous variable of interest that is related to the shock caused by  
9 the movement of the instrument and sees how that part of the variation affects the outcome. If the  
10 instrument is a good one, it will not be related to the outcome except through its effect on the  
11 endogenous variable and, so, will not be affected by any correlations with the omitted variables. That is,  
12 the estimate from the instrumental variable approach will lead to a causal estimate as long as the  
13 instrument is valid. To be valid, the instrument must be correlated with the endogenous variable  
14 (otherwise the instrument won't generate any variation in the endogenous variable) and it must not be  
15 otherwise related to the outcome being studied. This latter so-called exclusion restriction is not  
16 generally formally testable<sup>15</sup> (since, again, doing so would require one to examine correlations between  
17 the instrument and all omitted variables), but it is subjected to intuitive scrutiny. If one can conceive of  
18 a plausible story about how the instrument can affect the outcome variable in some way other than  
19 through its effect on the endogenous variable, there is reason to be skeptical of causal interpretations of  
20 the resulting estimates.

21 36. One particular form of instrumental variables approach that is used in the literature cited  
22 by Konrad is a generalized method of moments estimator (GMM) which essentially uses lagged (i.e.,  
23 past period) values of the endogenous variable or other control variables as instruments. The intuition is  
24 that since the variable value is from a past period it is hopefully unrelated to current omitted variables  
25 affecting the outcome. Unfortunately, this is often problematic if, for example, there are delayed effects  
26 or long-lasting effects of (for example) a firm's characteristics on future behavior, performance, or

27 <sup>15</sup> If one has multiple instruments for each endogenous variable, there is a diagnostic test called  
28 the test of overidentifying restrictions, but it is not a very powerful test and can easily lead one to think  
the exclusion restriction is satisfied when in fact it is not. This is discussed further down in this report.

1 value. In such a case, the exclusion restriction will not be satisfied. The GMM approach is sometimes  
2 used in studies examining stock price returns because, if one is willing to assume that asset returns are  
3 informationally efficient, past characteristics should be capitalized in past values and so should not show  
4 up in future returns. Even in this case though, the approach would be problematic if one does not  
5 assume that markets are efficient (e.g., if there are momentum effects in returns).

6 37. The last approach used in this literature that is worth explaining is the so-called fixed  
7 effects model. In this regression, if one has multiple observations for each entity (say, firm) over time, it  
8 is possible to pull out the effects of unobservable variables as long as those effects are constant over  
9 time without specifying all of the individual influences on these constant unobservable portions of the  
10 outcome variable. As long as the unobservable effects are indeed constant over time, the use of fixed  
11 effects models will adjust for these unobservable effects. However, if it is the case that these  
12 unobservable effects are changing over time, fixed effects models will still lead to estimates that suffer  
13 from omitted variables bias.

14 38. Before moving on to the studies themselves, it is worth defining the term statistical  
15 significance. Informally, if an estimate is referred to as statistically significant, it means that an estimate  
16 of the given size is unlikely to be observed by random chance alone (i.e., in reality even though the  
17 correlation between A and B is zero, random variation in the data makes it appear as though there is a  
18 correlation). A statement of statistical significance is often accompanied by a level, such as this result is  
19 statistically significant at the 5 percent level which means, if there really were no correlation between A  
20 and B, one would observe the estimated correlation (or larger in magnitude) in less than 5 percent of all  
21 random samples of the phenomenon. A conventional statistical significance level is 5 percent.  
22 Statistical significance does not mean causal, nor does it mean important. Instead, it merely gives a  
23 sense of how likely it is such a result arise by mere chance. A statistically insignificant estimate cannot  
24 be distinguished from mere random variation.

### 25 **Konrad Declaration**

26 39. In her declaration, Professor Alison Konrad argues that there is voluminous evidence that  
27 including women on corporate boards, especially a critical mass of women, improves firm performance  
28 in a host of ways. She often invokes the phrase “clear and convincing” when describing this evidence

1 and suggests that the studies she relies upon are methodologically rigorous. Additionally, she cites her  
2 own work and that of others that provide qualitative evidence (e.g., verbal responses in interviews) that  
3 women appear to value different things than men in terms of corporate goals and decision-making, and  
4 they perceive corporate leadership differently based on factors related to gender. Qualitative evidence  
5 such as that arising from interviews is of questionable reliability due to general problems having to do  
6 with the inability to control for correlated factors, sample compositions, and the like. For example,  
7 when Konrad suggests that her interview-based research found different perceived experiences of  
8 women board members when they were solo members vs, say, members of boards with three or more  
9 women, it is not possible to account for other differences across firms with different board compositions.

10 40. Additionally, Konrad invokes Harvard’s Project Implicit as part of her argument that  
11 “there is clear and convincing evidence that SB 826 is needed in order to spur firms toward gender  
12 equity on their boards of directors,” indicating that the research on implicit attitudes “has documented  
13 strong unconscious biases linking men to careers and women to families, and research shows consistent  
14 evidence of anti-female gender bias in the workplace.” To the extent that research on implicit attitudes  
15 provides a more reliable basis given the ability of researchers to examine attitudes using randomized  
16 experiments, it might be the start of an evidentiary basis used to support real-world policy changes.  
17 Unfortunately, the implicit attitudes measure and body of research using it has not fared well when  
18 subjected to replication, methodological scrutiny, and extrapolation to real world settings.<sup>16</sup> Most of the  
19 critical literature, admittedly, focuses on race and the IAT and implicit bias reasoning, but that is  
20 because there are relatively few papers demonstrating the relationship between these implicit measures  
21 and sex bias.<sup>17</sup>

22 <sup>16</sup> For my own contribution to this literature, albeit on the use of the implicit association test in  
23 the context of race, see Hart Blanton, James Jaccard, Jonathan Klick, Barbara Mellers, Gregory  
24 Mitchell, and Philip E Tetlock (2009), “Strong claims and weak evidence: reassessing the predictive  
25 validity of the IAT,” *Journal of Applied Psychology*, 94(3): 567-582, showing that a number of well-  
26 cited papers that assess the ability of the IAT to predict decisions are not robust to small modeling  
27 changes. For a meta-analysis of the IAT and race literature that shows the IAT literature reveals that  
28 “IATs were poor predictors of every criterion category other than brain activity” (categories examined:  
interpersonal behavior, person perception, policy preference, microbehavior, response time, and brain  
activity), see Frederick L Oswald, Gregory Mitchell, Hart Blanton, James Jaccard, Philip E. Tetlock  
(2013), “Predicting ethnic and racial discrimination: a meta-analysis of IAT criterion studies,” *Journal of  
Personality and Social Psychology*, 105(2): 171-192.

<sup>17</sup> On this point, see Greg Mitchell and Philip E. Tetlock (2017), “Popularity as a poor proxy for

1           41. For these reasons, it is more useful to consider quantitative studies of actual firm  
2 experiences and how they differ across various degrees of board gender diversity. There do exist a  
3 number of quantitative studies using real-world data that attempt to isolate the causal effects of  
4 appointing more women to corporate boards on important business and social outcomes. The degree to  
5 which these studies are successful in isolating causality varies considerably. Proponents of laws and  
6 regulations that mandate increased sex diversity on corporate boards often choose selectively from the  
7 existing literature and overlook methodological flaws of the papers whose results support their favored  
8 outcomes.<sup>18</sup>

9           42. Konrad’s analysis exhibits a similar tendency to pick and choose selectively based on  
10 supportive conclusions, while ignoring the methodological failures. Konrad’s conclusion that “The  
11 evidence clearly shows that women directors, and particularly a critical mass of women benefits  
12 companies, communities and society by reducing unethical behavior by firms, such as environmental  
13 damage, the sale of dangerous products, and bank misconduct,” is belied by the failure of these studies  
14 to use compelling and rigorous identification strategies, as well as other methodological flaws as  
15 documented extensively below. Konrad also ignores a number of studies, many of which are  
16 methodologically better, that point in other directions.<sup>19</sup>

17           43. I examine each of the studies cited in Konrad’s exhibits B-H individually that she notes  
18 as having examined the critical mass hypothesis. As for the other cited studies in these exhibits, I  
19 specifically address a sampling of them below, but the main points apply virtually without exception to  
20

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21 utility: The case of implicit prejudice,” in S. O. Lilienfeld & I. D. Waldman, eds., *Psychological science  
22 under scrutiny: Recent challenges and proposed solutions* (pp. 164–195) at pp. 182-184.

23 <sup>18</sup> For an extensive documentation of this in one particular policy setting, see Jonathan Klick  
(2021), review of the Literature on Diversity on Corporate Boards, American Enterprise Institute  
24 Monograph.

25 <sup>19</sup> As just one example, Kenneth R. Ahern and Amy K. Dittmar (2012), “The Changing of the  
26 Boards: The Impact on Firm Valuation of Mandated Female Board Representation,” *Quarterly Journal  
27 of Economics*, 127: 137–97 shows that when firms were required to increase the gender diversity of  
28 their boards in Norway, there was a statistically significant reduction in firm value. This paper examines  
a natural experiment with sophisticated econometric tools, was published in one of the top five  
economics journals, has been cited more than 1,500 times, and was clearly available to Konrad. This  
cannot be explained by Konrad finding the Norwegian experience not to be relevant since she  
extensively cites studies of non-U.S. firms in her declaration. More examples of contrary studies can be  
found in Jonathan Klick (2021), review of the Literature on Diversity on Corporate Boards, American  
Enterprise Institute Monograph, none of which apparently merited attention from Konrad.

1 the papers that form Konrad’s “clear and convincing” evidentiary base. Despite Konrad claiming that  
2 all of these studies “tested/adjusted for endogeneity,” it is not even nominally true for many of the  
3 studies, and for virtually all of the rest, the attempt to account for omitted variable bias is not credible.  
4 In some instances, noted below, a paper’s findings do not even match Konrad’s description. In all, even  
5 ignoring the large number of papers Konrad ignores that undercut her conclusions, Konrad’s reliance on  
6 this literature is unwarranted.

7 **Konrad-cited studies**

8 **Exhibit B: Impact on Ethical Behavior**

9 44. Professor Konrad states “I examined six panel studies examining the impact of women  
10 directors on firm ethical behavior, and they are described in Exhibit B. Several of these studies looked at  
11 corporations located both in the U.S. and in other countries. All six studies showed statistically  
12 significant beneficial effects . . .<sup>20</sup>”. As detailed below, none of these studies is credible from a causal  
13 inference standpoint and most also had other technical problems yielding them to be unreliable. In the  
14 note to her exhibit B, she states “All listed studies analyzed panel data and tested/adjusted for  
15 endogeneity.” While the note is nominally true, as detailed below, none of the studies correctly  
16 tested/adjusted for endogeneity (a synonym for the omitted variable bias problem described above). In  
17 all cases, the instrumental variable approaches used rely on invalid instruments that do not satisfy the  
18 necessary exclusion restriction (i.e., the instrument itself likely has effects on the outcomes studied  
19 through channels other than the endogenous variable to which the instrument is applied).

20 **Studies**

21 *Arnaboldi, F., Casu, B., Gallo, A., Kalotychou, E., & Srkisyan, A. (in press). Gender diversity*  
22 *and bank misconduct. Journal of Corporate Finance.*

23 45. This paper, which purports to show a negative relationship between WOCB, and fines  
24 levied by U.S. regulators against publicly listed banks in 28 EU countries, uses panel data methods to  
25 attempt to isolate a causal relationship between female board participation and fines. There are a  
26 number of problems with this paper methodologically. First, the regressions generally include lagged  
27 dependent variables in the empirical specification. It is well-known that the inclusion of lagged

28 <sup>20</sup> Konrad declaration, p. 19.



1 dependent variables in panel data models leads to statistical bias.<sup>21</sup> For this reason alone, the estimates  
2 in the paper are not reliable. The estimation approach has a host of other reliability problems. For  
3 example, the paper only includes country fixed effects, as opposed to bank fixed effects. The latter  
4 would be a more credible approach to address omitted variable bias problems. The authors'  
5 instrumental variables approach is not very credible either, as they use as an instrument the education  
6 levels of females (relative to males) in a bank's country as the predictor of female presence on the board.  
7 Because female education levels do not vary substantially year to year in a country, this instrument is  
8 essentially duplicative of the country fixed effects that are already in the model. Notably, the authors do  
9 not present any evidence that this instrument is strong conditional on all of the other controls in the  
10 model. Beyond this problem, if female education level is correlated with any other cultural or  
11 sociological variables absent from the model that might affect bank behavior, the instrument will not  
12 satisfy the exogeneity requirement for valid instruments. The authors' exogeneity test does not  
13 demonstrate that the exclusion restriction is satisfied.<sup>22</sup>

14 *Capezio, A., & Mavisakalyan, A. (2016). Women in the boardroom and fraud: Evidence*  
15 *from Australia. Australian Journal of Management, 41(4), 719-734.*

16 46. Despite recognizing the endogeneity problems involved in simply regressing an outcome  
17 variable (in this case a fraud metric from KPMG survey data) on a variable capturing female board  
18 presence,<sup>23</sup> the article uses a wholly unreliable instrumenting approach that relies on the assumption that  
19 firms with CEOs with more feminine names will tend to have more female board members. First, there  
20 is no evidence that this is true generally. Second, the paper provides no evidence that it was true in their  
21 sample (e.g., not even t statistics). Thirdly, there is evidence that men with more feminine names tend to

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24 <sup>21</sup> See, for example, Paul D. Allison, Richard Williams, and Enrique Moral-Benito (2017),  
25 "Maximum Likelihood for Cross-lagged Panel Models with Fixed Effects," *Socius*, 3: 1-17 or any  
textbook treatment such as Manuel Arellano (2003), *Panel Data Econometrics*.

26 <sup>22</sup> See, for example, Jeffrey M. Wooldridge (2009), *Introductory Econometrics: A Modern*  
27 *Approach*, 4<sup>th</sup> ed., p. 529 ("In the context of the simple IV estimator, we noted that the exogeneity  
28 requirement cannot be tested.")

<sup>23</sup> The article notes that female presence may be incidentally correlated with other uncontrolled  
for firm governance characteristics, and it notes that firms may choose female directors to look good  
when they know fraud has occurred.

1 act more recklessly in school in U.S. data.<sup>24</sup> While this relationship might not extend to adulthood and  
2 might not exist in the Australian context studied in Capezio and Mavisakalyan, it should cast doubt on  
3 what was already a dubious identification strategy. Lastly, after the authors quickly skim over their  
4 unreliable estimation approach, they suggest that any bias would have to be quite large to overturn their  
5 results. This is total speculation. Specifically, they write (p. 729) “the correlations between %FEMALE  
6 and FRAUD with the unobserved confounding variables each need to exceed 0.5428 (in absolute terms)  
7 to turn the estimated effect of %FEMALE insignificant. This is rather large effect in the social  
8 sciences.” By definition, unobserved confounding variables are not observed, which means it is not  
9 possible to reliably say anything about their correlation with anything else.

10 *Liu, C. (2018). Are women greener? Corporate gender diversity and environmental*  
11 *violations. Journal of Corporate Finance, 52, 118-142.*

12 47. While this paper purports to find that firms with a higher fraction of women on their  
13 boards face fewer environmental lawsuits, the claimed attempts to overcome endogeneity are unreliable.  
14 First, the author claims that using lagged values of the board composition effect overcomes endogeneity  
15 when this is obviously not true. The implicit claim is that past characteristics of the firm cannot be  
16 correlated with current unobservable characteristics. This is silly for at least two reasons. First, while  
17 the paper examines lawsuits faced in a given year, the conduct generating the litigation could easily have  
18 occurred during the past three years, in which case the board composition in the prior years would be  
19 contemporaneous with the actual actions of the firm. Second, lagging the composition metric does  
20 nothing to handle issues involving firms with certain characteristics that are also coincidentally related  
21 to firm behavior being more likely to choose female directors. The author’s second attempt to handle  
22 endogeneity is to use propensity score matching. As is well known in social science and statistics,  
23 “propensity score matching does not solve the problem of omitted variable bias.<sup>25</sup>” Propensity score  
24 matching allows one to make comparisons among entities that are similar on observable characteristics  
25 (that are used to do the matching) but do nothing to ensure the comparisons are similar on other

26 <sup>24</sup> Figlio, David N. "Boys Named Sue: Disruptive Children and Their Peers." *Education Finance*  
27 *and Policy* 2, 4 (Fall 2007): 376-94.

28 <sup>25</sup> Ben Grunwald and John MacDonald (2014), “Propensity Score Matching,” in *Encyclopedia of*  
*Criminology and Criminal Justice*, edited by Gerben Bruinsma and David Weisburd, p. 4060 available at  
[https://doi.org/10.1007/978-1-4614-5690-2\\_46](https://doi.org/10.1007/978-1-4614-5690-2_46).

1 dimensions. This paper is not reliable.

2 ***Owen, A. L., & Temesvary, J. (2018). The performance effects of gender diversity on bank***  
3 ***boards. Journal of Banking & Finance, 90, 50-63.***

4 48. This paper uses lagged endogenous variables among its instruments, and the other  
5 instrument is the share of independent directors. There are at least two problems with these instruments.  
6 First, many commentators have argued that independent directors will affect firm behavior and  
7 performance.<sup>26</sup> Second, if the lagged variable is itself correlated with unobservable firm characteristics,  
8 it too is unsuitable as an instrument. Thus, neither of the authors' instruments are valid which, in turn,  
9 makes the authors' "Sargan Test" invalid as a test of the necessary exclusion restriction for the  
10 instrumental variables approach to address omitted variable bias concerns.<sup>27</sup> Beyond these fatal  
11 concerns, the paper also overstates the precision of its results since it uses only heteroskedasticity robust  
12 standard errors rather than the appropriate clustered standard errors. Therefore, any claims of statistical  
13 significance in this paper are questionable.<sup>28</sup>

14 <sup>26</sup> For a high-quality study that uses a credible natural experiment to identify the effect of  
15 independent directors on firm value, see Bang Dang Nguyen and Kasper Meisner Nielsen (2010), "The  
16 value of independent directors: Evidence from sudden deaths," *Journal of Financial Economics*, 98(3):  
17 550-567 which finds that an unexpected death of an independent director leads to an 85 basis point  
18 decline in the firm's stock return on average (in a 1994-2007 sample of U.S. firms) and the degree of  
19 this effect is also a function of how independent the board is overall.

20 <sup>27</sup> The Sargan Test is a test of overidentifying restrictions which, essentially, tests whether the  
21 regression leads to statistically comparable results regardless of which subset of instruments is used in  
22 the IV procedure. The intuition is that if all of the instruments are valid (i.e., sufficiently predictive of  
23 the endogenous regressor and otherwise unrelated to the outcome variable), they should all generate the  
24 same regression coefficients, whereas if they are not valid, they likely will generate different  
25 coefficients. Of course, another possibility remains – the various instruments can be invalid in similar  
26 ways. So, for example, if independent directors are associated with more female directors and,  
27 independently, better firm performance and better performing firms are also more likely to have had  
28 more female directors in past years, it is very likely that the same omitted variable bias works through  
29 both channels. In such a case, the Sargan Test will be "passed" because all of the instruments suffer  
30 from similar biases. For a more formal treatment of this issue, see Jeffrey M. Wooldridge (2009),  
31 *Introductory Econometrics: A Modern Approach*, 4<sup>th</sup> ed., p. 529-531 "Therefore, the two IV estimates  
32 may be similar even though each is inconsistent. In effect, because the IVs in this example are chosen  
33 using similar reasoning, their separate use in IV procedures may very well lead to similar estimates that  
34 are nevertheless both inconsistent."

35 <sup>28</sup> There is a large literature on this. See, for example, A. Colin Cameron and Douglas L. Miller  
36 (2015), "A Practitioner's Guide to Cluster-Robust Inference," *Journal of Human Resources*, 50(2): 317-  
37 372. Intuitively, this issue arises from having repeat observations for the same entity (firm, in this case).  
38 While this aspect of the data is helpful to allow one to isolate causal effects (since one can see what  
39 happens to a given firm when a particular aspect changes, assuming that all other unmodeled  
40 characteristics remain fixed), it does mean that observations are not statistically independent from one  
41 another. That is, a firm in time t is not independent from the same firm in time t-1 which means it is

1            *Kaitlin D. Wowak , George P. Ball , Corinne Post , David J. Ketchen Jr. (2021) The*  
2            *Influence of Female Directors on Product Recall Decisions. Manufacturing & Service*  
3            *Operations Management 23(4):895-913.*

4            49.     This paper has a faulty instrumental variables approach in that it instruments its female  
5            board metric using a measure of the fraction of male board members who sit on other firms' boards  
6            where there are female directors. The idea is that if male board members are exposed to other settings  
7            where females are on boards, they are more likely to invite or advocate for females on their other boards.  
8            The authors present evidence that the instrument is strong, but they admit that there is no way to test  
9            whether the exclusion restriction is satisfied (p. 903). They instead simply note that this approach has  
10           been used in other work and assert that this measure will not be related to firm performance except  
11           through the choice to appoint female board members. As noted in my previous work (Klick 2021), this  
12           approach is questionable by the very logic used by those advocating for greater female board  
13           participation. That is, it is claimed that women directors improve firm performance. Given that firms  
14           compete with each other for customers, capital, and labor, a better performing competitor can directly  
15           lead to worse performance for a firm. Thus, the instrument fails the exclusion restriction in that setting.  
16           As I noted previously, this concern might be dampened if the various firms are not related to each other  
17           (as competitors, in labor markets, etc.),<sup>29</sup> but the Wowak et al paper focuses exclusively on firms in the  
18           medical product space, which is a fairly narrow market and almost ensures that the firms with common  
19           directors compete.<sup>30</sup> Beyond this failure to reliably account for the omitted variable bias problem, this  
20           paper is not credible in many technical respects. For example, in their time to recall regressions, they  
21           use basic OLS regressions when duration/hazard models should have been used given the censoring  
22           problem in the dataset (i.e., some firms might eventually recall a product after the end of their sample  
23           period, but such recalls will not be observed by the researchers), and they use standard errors that do not

24           \_\_\_\_\_  
25           inappropriate to treat these two observations as independent data points. Clustering represents the  
26           standard way to handle this issue and often (though not always) leads standard error estimates to  
27           increase in size which, in turn, affects statistical significance determinations.

28           <sup>29</sup> Jonathan Klick (2021), Review of the Literature on Diversity on Corporate Boards, American  
Enterprise Institute Monograph.

<sup>30</sup> For example, Ye Cai and Merih Sevilir (2012), "Board connections and M&A transactions,"  
Journal of Financial Economics, 103: 327-349 notes that interlocking boards often lead to or result from  
M&A activity, and they find that healthcare-related industries are very active in M&A terms.

1 account for the clustering issue described above, or even other basic issues such as heteroskedasticity.  
2 This suggests that they likely overstate the precision of their estimates and therefore the effects they  
3 declare to be statistically significant are unlikely to survive when proper standard error estimates are  
4 used.

5 *Christopher Godfrey, Andreas G. F. Hoepner, Ming-Tsung Lin, and Ser-Huang Poon (*  
6 *2020), “Women on boards and corporate social irresponsibility: evidence from a Granger*  
7 *style reverse causality minimisation procedure,” The European Journal of Finance.*

8 50. This paper uses “Granger causality” methods to purportedly identify the causal effect of  
9 female board representation on the number of negative news items about a firm related to corporate  
10 social responsibility (Corporate Social Irresponsibility, they call it) and subsequent stock market effects,  
11 claiming to find that firms with more women on their boards have fewer negative corporate social  
12 responsibility incidents. First, it should be noted that before they apply their Granger-based approach,  
13 they find that female representation adversely affects (i.e., reduces) annual stock returns of their firms to  
14 a statistically significant degree and appears to increase the negative stock market impact of corporate  
15 social irresponsibility (Table 4, models 2 and 3) and to increase return volatility (Table 4, models 5 and  
16 6), albeit not in a statistically significant way. Thus, making any positive claim about female  
17 participation in their data hinges on the validity of their Granger-based approach. Unfortunately, it is  
18 well known that Granger type approaches do not actually isolate causality.<sup>31</sup> Essentially what Godfrey  
19 et al do is regress their endogenous variable on their controls and use the left-over variation in their  
20 female board metric to create what they claim to be an unconfounded version of the female board metric  
21 and use that as the explanatory variable in their regression. That is, they are basically using an  
22 instrumental variables approach with no instrument. Such an approach does nothing to solve the omitted  
23 variable bias problem (if it did, literally everyone would use this approach since it is obviously easier  
24 than using instrumental variables where one must identify a valid instrument), leaving all of the results  
25 in this paper unreliable and non-credible.

26  
27  
28 <sup>31</sup> See any textbook treatment (e.g., Jeffrey M. Wooldridge (2009), *Introductory Econometrics: A Modern Approach*, 4<sup>th</sup> ed., p. 650).

1                    **Exhibit C: Impact on Earnings Management**

2                    51.        Konrad declares that the 23 studies she examined that looked at the relationship between  
3 female directors and earnings management provide “clear and convincing evidence that women directors  
4 benefit boards by reducing nefarious earnings management activities intended to mislead investors.<sup>32</sup>”  
5 Unfortunately, as was the case above, none of the studies she cites provide reliable evidence of a causal  
6 effect of female directors on earnings management indicators. While once again in the footnote to the  
7 exhibit she writes “All listed studies analyzed panel data and tested/adjusted for endogeneity,” this time  
8 it is not even nominally true in that a number of the studies she invokes do not even pretend to address  
9 endogeneity, as detailed below.

10                  Ahmed & Ali (2017) does not appear in Konrad’s reference list, but it appears to be:

11                  *Ammad Ahmed and Searat Ali (2017), “Boardroom gender diversity and stock liquidity:  
12 Evidence from Australia,” Journal of Contemporary Accounting and Economics, 13(2):  
13 148-165.*

14                  52.        This article proposes both an instrumental variables approach and a propensity score  
15 matching approach to overcome the omitted variables bias issue. Unfortunately, their instrument is not  
16 really an instrument. Namely, they use the passage of gender diversity laws that effect all of the firms in  
17 their sample since the reforms were nationwide and, therefore (as they note), they can no longer include  
18 their year fixed effects in the IV model. Because of this, there is no way to account for general  
19 background trends that affect all firms independent from any effect of increased board diversity. This is  
20 another IV paper that essentially has no instrument (since the IV model actually accounts for less than  
21 the original year fixed effects model accounts for) and, therefore, is unreliable. Also, as discussed  
22 above, propensity score matching does nothing to account for omitted variables since the matching only  
23 uses included variables (implicitly assuming that any effect of omitted variables is fully captured by the  
24 included variables).

25                  Evgeniou & Vermaelen (2017) also does not appear in the reference list, but it appears to be:

26                  *Theodoros Evgeniou and Theo Vermaelen (2017), “Share buybacks and gender diversity,”  
27 Journal of Corporate Finance, 45(C): 669-686.*

28                  <sup>32</sup> Konrad declaration, p. 20.

1           53. This paper uses the instrument of the fraction of male board members who are on other  
2 companies' boards with female representatives, as covered above in some of the other papers.<sup>33</sup> As  
3 discussed before, this approach requires that there is no competition between the interconnected firms;  
4 otherwise, there are other channels through which this instrument can affect firm performance which  
5 would render the instrument invalid. Beyond that, this paper does not actually use its instrument  
6 approach in examining the main outcome of the paper (i.e., the relationship between female  
7 representation and stock buybacks); instead, it only examines the instrumented effect of females on the  
8 ultimate stock market reaction to the buybacks. Given that, even if we ignore the concerns about the  
9 instrument used here, the authors provide no evidence that having more women affects buybacks in any  
10 way. We are only left with the results that in some time frames, having women on the board doesn't  
11 affect stock market reactions to buybacks in a statistically significant way over the 1- and 2-year time  
12 horizons, but it might over the 3- and 4-year time horizons.

13           Fan et al (2019) does not appear in the reference list, but it appears to be:

14           ***Yaoyao Fan, Yuxiang Jiang, Xuezhi Zhang, and Yue Zhou (2019), "Women on boards and***  
15           ***bank earnings management: From zero to hero," Journal of Banking & Finance, 107(C):***  
16           ***105607.***

17           54. Purporting to find a non-linear effect of female board membership on earnings  
18 manipulation (indicating, according to the authors, that once a critical mass of woman is present on a  
19 board, a bank is less likely to engage in manipulation), this paper, similar to many others already  
20 covered, uses the degree to which male board members are involved with boards in other companies  
21 populated by women as an instrument. This paper makes the point I have made above that if firms  
22 compete, this instrument is invalid, but they note that the bank setting may be helpful in this regard since  
23 banks are largely prohibited from having interlocking boards, so any experience male directors have on  
24 boards of other companies will be with firms outside of the banking industry. This does make this  
25 instrument more plausible than in the other papers that use it. However, the instrument is still not  
26 beyond criticism. If certain kinds of companies are more likely to appoint female directors (younger  
27

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28           <sup>33</sup> Note, the paper describes the approach as a Heckman selection model which is, in effect, an  
instrumental variables model.

1 firms, firms with better governance, firms from particular industries, etc.) which is well-recognized in  
2 the literature, the male board members of those firms may be better directors as well on a whole host of  
3 dimensions (e.g., better governed firms not only appoint more women, but the men they appoint are also  
4 better in various ways); those higher quality male directors may also affect the performance of the banks  
5 whose boards with which they are involved. Note that this concern applies to the other papers using this  
6 instrument too, but at least Fan et al are able to be confident that direct competitive effects will not  
7 invalidate their instrument.

8         55. While this paper is certainly one of the better papers methodologically speaking, there are  
9 some other concerns beyond the one raised above about the instrument. First, Fan et al's results are  
10 primarily about non-linear effects of female participation (i.e., sort of testing the critical mass idea) so  
11 the endogenous regressor is the female representation metric and its square. The Fan results suggest at  
12 low levels of female participation, the effect of adding women to the board is to actually increase  
13 earnings management but at higher levels this effect turns into one where additional women reduce  
14 earnings management (e.g., the Table 4 regression shows that when female representation is included  
15 linearly, increasing the percentage of women increases the earnings management metric [DLLP] by a  
16 statistically significant 0.0042, whereas when it is included as a quadratic, a U shape is estimated with  
17 additional women increasing earnings management at low levels and decreasing it after about 25 percent  
18 of the board is female). However, the authors appear to just use their instrument to correct for  
19 endogeneity in the linear metric of female representation and assume it is straightforward to square the  
20 corrected linear metric when estimating the quadratic relationship. This approach is humorously  
21 referred to as the **forbidden regression** in a standard graduate econometrics textbook, and it is noted  
22 that since both the endogenous variable and its square are both endogenous, one at least needs two  
23 separate instruments for any IV approach to be valid.<sup>34</sup> Thus, even if one puts aside the concerns raised  
24 above about the instrument, at best, only the specifications that include female board participation as a  
25 linear term are valid, and those specifications show that adding additional women to a board actually  
26 increases earnings management by banks.

27  
28 <sup>34</sup> See Jeffrey M. Wooldridge (2001), *Econometric Analysis of Cross Section and Panel Data*, 1<sup>st</sup>  
ed., pp. 235-237.



1 Gul, Srinidhi, and Ng (2011) does not appear in the reference list, but it appears to be:

2 ***Ferdinand Gul, Bin Srinidhi, and and Anthony C. Ng (2011), “Does board gender diversity***  
3 ***improve the informativeness of stock prices?” Journal of Accounting and Economics, 51(3):***  
4 ***314-338***

5 56. This paper, after claiming that women on boards led to more informative stock prices,  
6 notes the common omitted variables bias problem that is ubiquitous (in this literature and all non-  
7 experimental literatures) and claims to solve it by 1) examining the natural experiment provided by  
8 Norway’s requirement that firms achieve 40 percent female representation on their boards by the end of  
9 2008; and 2) by collapsing the female board data into categories that represent increases or decreases in  
10 female representativeness on boards. As for the first approach, the authors do not show their work,  
11 instead merely asserting that when they examine the Norwegian experiment increasing female  
12 representation led to more informative stock prices. It suffices to say that the quick presentation elides  
13 numerous potential concerns with their approach.<sup>35</sup> Perhaps more important, they do not note that many  
14 papers examining the Norway example have shown that a large number of firms went private to avoid  
15 the regulation<sup>36</sup> (thus creating a sample selection problem) and for the firms that were induced to  
16 appoint more female directors, performance in general deteriorated.<sup>37</sup> The second approach, simply  
17 collapsing the female participation variable into those firms which added females versus those that did  
18 not, does nothing to address omitted variable bias (since the same potentially omitted factors that change  
19 the level of female participation also affect its change).

20 Kyaw, Olugbode, and Petracci (2015) does not appear in the references, but it appears to be:

21 ***Khine Kyaw, Mojisola Olugbode and Barbara Petracci (2015), “Does gender diverse board***  
22 ***mean less earnings management?” Finance Research Letters, 14(C): 135-141.***

23 57. This short paper does nothing to address endogeneity in its regressions relating gender

24 <sup>35</sup> For example, they do not discuss how they calculate standard errors, nor do they discuss what  
25 happens if firm fixed effects and other controls are included. They also do not present any attempt to  
26 make a comparison with firms outside of Norway that were unaffected by the law but doing so would be  
useful to account for general background trends in stock price informativeness.

27 <sup>36</sup> Øyvind Bøhren and Siv Staubo (2014), “Does Mandatory Gender Balance Work? Changing  
Organizational Form to Avoid Board Upheaval,” *Journal of Corporate Finance*, 28: 152–68

28 <sup>37</sup> See, for example, David A. Matsa and Amalia R. Miller (2013), “A Female Style in Corporate  
Leadership? Evidence from Quotas,” *American Economic Journal: Applied Economics*, 5(3): 136–69.

1 metrics and earnings management beyond including firm fixed effects. While such an approach will  
2 account for omitted variables that are constant over the sample period, it does not address time varying  
3 omitted variable effects. As most of the covered literature admits, firms might appoint women to boards  
4 in particular times as a kind of window dressing trying to deflect attention from other problems, leading  
5 to a time-varying omitted variable bias. This paper does not purport to address endogeneity, despite  
6 Konrad’s exhibit C note that “All listed studies analyzed panel data and tested/adjusted for  
7 endogeneity.”

8 Lakhal et al (2015) is not included in the reference list, but it appears to be:

9 ***Faten Lakhal, Aguir Amel, Nadia Lakhal, and Adnane Malek (2015), “Do Women On Boards  
10 And In Top Management Reduce Earnings Management? Evidence In France,” Journal of  
11 Applied Business Research, 31(3): 1107-1118.***

12 58. This paper does not purport to address endogeneity, despite Konrad’s exhibit C note that  
13 “All listed studies analyzed panel data and tested/adjusted for endogeneity.” It merely regresses  
14 measures of discretionary accruals on female board metrics. It does not even include firm fixed effects.  
15 This paper is totally unsophisticated and completely non-credible.

16 Radu and Smaili (in press) is not included in the reference list, but it appears to be:

17 ***Camélia Radu and Nadia Smaili (2021), “Board Gender Diversity and Corporate Response  
18 to Cyber Risk: Evidence from Cybersecurity Related Disclosure,” Journal of Business Ethics.***

19 59. This paper does not purport to address endogeneity, despite Konrad’s exhibit C note that  
20 “All listed studies analyzed panel data and tested/adjusted for endogeneity.” It merely regresses  
21 measures of discretionary accruals on female board metrics, although it does include firm fixed effects  
22 which, as above, will not account for time-varying omitted variable bias effects. Beyond the  
23 methodological unreliability of this study, it is interesting that the presence of a cybersecurity disclosure  
24 (or the number of words or paragraphs in the disclosures) is taken as a positive outcome, whereas it  
25 could also be interpreted that firms with more women on the board have more things to disclose. I have  
26 no evidence that this is the case, but neither do the authors that it is not the case. Also, this paper, as do  
27 many of the others cited by Konrad that attempt to isolate the critical mass effect includes variables for  
28 one woman, two women, and three or more women and finds (as do many of the papers) that the first

1 two variables generate a negative effect, while the third generates a positive effect, but none of the  
2 papers goes further and examines four women, five women, and so on. It is as if the authors find some  
3 evidence of what they were looking for and then stop examining the data. For all we know, just as the  
4 effect of one or two women differs from the effect of three women, there may be more non-linearities  
5 that go in all sorts of ways, casting doubt on whether there really is a generalized critical mass effect.

6 Seebeck and Vetter (in press) is not included in the reference list, but it appears to be:

7 ***Andreas Seebeck and Julia Vetter (2021), "Not Just a Gender Numbers Game: How Board***  
8 ***Gender Diversity Affects Corporate Risk Disclosure," Journal of Business Ethics.***

9 60. This paper purports to examine an exogenous shock, namely the UK's decision to leave  
10 the EU, to examine the effect of female board members on risk disclosure. The implicit idea is that  
11 since Brexit was a surprise, firms did not somehow change their boards or behavior in reaction to the  
12 shock, keeping unobservables fixed, mitigating omitted variables bias concerns. This is misleading,  
13 however, since the firms that have more women, as repeatedly noted in this literature, might be different  
14 in all sorts of unmodeled ways (better governance, different strategies, etc.) and the necessary "shock" in  
15 a natural experiment is to the policy variable of interest. In this case, the variable of interest is the  
16 presence/fraction of women on boards which is unaffected by the Brexit shock. The authors argue that  
17 Brexit created new risks, and so the choice to disclose them is unrelated to having more women because  
18 a firm knew it was risky. This argument only addresses the endogeneity concern that somehow women  
19 are appointed in firms that have underlying risks they are worried about (it does nothing with the  
20 endogeneity concern that better/different firms are more likely to appoint women) and only then if it is  
21 truly the case that Brexit was wholly unanticipated by firms. The authors do use an instrumental  
22 variables approach, but their instruments are totally non-credible. They use a firm's size and the  
23 presence of a mandatory retirement policy for a firm's board as their instruments. Firm size could be  
24 related to all kinds of unobservable characteristics of a firm and, therefore is an invalid instrument.  
25 Likewise, firm policies are likely correlated in many ways, so any relationship that exists between the  
26 firm's retirement policies and other unmodeled firm policies that are related to firm behavior will  
27 invalidate the retirement policy instrument.

28 Srinidhi, Gul, and Tsui (2011) is not in the reference list, but it appears to be:

1            ***Bin Srinidhi, Ferdinand A. Gul, and Judy Tsui (2011), “Female Directors and Earnings***  
2            ***Quality,” Contemporary Accounting Research, 28(5): 1610-1644.***

3            61.        This paper uses an instrumental variables approach (via a Heckman correction) to  
4            account for the potential endogeneity of the female board participation metrics, but the authors’  
5            instruments are not credible in the least. They use variables that measure sales, growth, stock volatility,  
6            and diversification, as well as a host of other firm characteristics. The closest they get to an exogenous  
7            instrument is the percentage of women employed in an industry, but this instrument clearly can have  
8            direct effects on firms if women affect firm performance both directly by influencing their own  
9            employer’s performance and indirectly by affecting competition in the industry. This approach is not  
10           reliable.

11           Strydom, Yong, and Rankin (2017) is not in the reference list, but it appears to be:

12           ***Maria Strydom, Hue Hwa Au Yong, and Michaela Rankin (2017), “A few good (wo)men?***  
13           ***Gender diversity on Australian boards,” Australian Journal of Management, 42(3): 404-***  
14           ***427.***

15           62.        This paper too uses a number of firm characteristics and the percentage of women  
16           employed in the industry. For the reasons given above, these are not valid instruments and so do not  
17           address the endogeneity concern the authors themselves note.

18           Wahid (2019) is not in the reference list, but it appears to be:

19           ***Aida Sijamic Wahid(2019), “The Effects and the Mechanisms of Board Gender Diversity:***  
20           ***Evidence from Financial Manipulation,” Journal of Business Ethics, 159:705–725.***

21           63.        This paper uses an instrumental variables approach with the population of women in the  
22           area around the firm’s headquarters as an instrument and the longitude of a firm’s headquarters as  
23           another instrument. Because these instruments do not change for a given firm (or do not change much  
24           for the population variable), Wahid must drop the firm fixed effects in the IV specification, leaving any  
25           constant unobservable effects unaccounted for. If firms’ headquarters location decisions have any  
26           relationship with firm behavior (e.g., politically savvy firms locating near Washington, D.C., or forward-  
27           looking technology firms locating on the West Coast, etc.) these instruments will be invalid. It seems  
28           unlikely that such decisions are wholly random with respect to firm behavior and quality.

1 Ye et al (2019) is missing from the reference list, but appears to be:

2 ***Dezhu Ye, Jie Deng, Yi Liu, Samuel H. Szewczyk, and Xiao Chen (2019), “Does board gender***  
3 ***diversity increase dividend payouts? Analysis of global evidence,” Journal of Corporate***  
4 ***Finance, 58: 1-26.***

5 64. To examine the effect of female directors on dividend policy, Ye et al implement an IV  
6 analysis where their instrument for a given firm’s number of female board members is the average  
7 number of female board members in the firm’s industry in the same country. As discussed above, if  
8 female directors purportedly affect firm decisions and performance, then being in a market where a  
9 firm’s competitors have more female board members exerts competitive effects on the firm itself,  
10 implying that this instrument does not satisfy the exclusion restriction and, therefore, is invalid.

11 65. While I will not go into detail regarding the papers Konrad lists as “Did not Test Critical  
12 Mass” in Exhibit C, the same problems follow through to these papers. For example, Abad et al  
13 (2017)<sup>38</sup> uses a GMM approach to identify causal effects which is comparable to an instrumental  
14 variables approach but uses lagged control variables (or first differenced control variables) as the  
15 instruments. As Wooldridge points out, this kind of estimator requires “all equations in the system must  
16 be properly specified, which means their instruments must be exogenous.<sup>39</sup>” This means that if there are  
17 any lagged effects of the control variables (e.g., if institutional investors have long term effects on firm  
18 behavior, or the effect of outside directors is long-lived, or even if there is temporal dependence such as  
19 autoregressive components to firm outcomes, etc.) on the outcome variable, the GMM approach will not  
20 generate unbiased estimates. In cases where the outcome is a firm’s return, this assumption might make  
21 sense if one were willing to assume that markets were informationally efficient. However, this article  
22 examines informational asymmetries, so the authors clearly do not believe informational efficiency  
23 holds. Again, these papers do not use reliable methods to estimate causal effects.

24 **Exhibit D**

25 66. Konrad suggests that the studies she reviewed provide support for the idea that having

26 <sup>38</sup> David Abad, María Encarnación Lucas-Pérez, Antonio Minguez-Vera, José Yagüe (2017),  
27 “Does Gender Diversity on Corporate Boards Reduce Information Asymmetry in Equity Markets?,”  
BRQ Business Research Quarterly, 20(3): 192–205.

28 <sup>39</sup> See Jeffrey M. Wooldridge (2001), *Econometric Analysis of Cross Section and Panel Data*, 1<sup>st</sup>  
ed., p. 199.

1 more female board members constrains CEO pay or rebalances CEO pay in favor of more incentive  
2 compensation. Many of the studies she cites favorably are decidedly mixed in their actual conclusions,  
3 and none of the papers does a credible job of identifying causal relationships.

4 Owen & Temesvary (2019) is not included in the reference list, but it appears to be:

5 ***Ann Owen and Judit Temesvary (2019), "CEO compensation, pay inequality, and the gender***  
6 ***diversity of bank board of directors," Finance Research Letters, 30(C): 276-279.***

7 67. This paper uses only firm fixed effects to make any progress on the omitted variable bias  
8 issue. As noted before, this approach only works if one assumes that all effects arising from omitted  
9 variables bias are constant. Moreover, even if one were willing to make this assumption, four of the  
10 seven specifications indicate that firms with more females on their boards unambiguously pay CEOs  
11 more. Of the other three specifications, while there is one (the only one that allows for it) that nominally  
12 shows the critical mass effect espoused by Konrad, the quadratic component of the diversity metric (that  
13 shows the reduction in compensation at higher levels of female representation) is only statistically  
14 significant at the  $p < 0.10$  level, while the other component (that shows that increasing female  
15 representation increases CEO compensation) is statistically significant at the  $p < 0.01$  level. Further,  
16 because the authors do not account for the clustered standard errors issue raised above, their standard  
17 errors are likely understated, which means even the 10 percent statistical significance claim is likely  
18 overly optimistic. This paper does nothing more to account for endogeneity, so it would not be credible  
19 to infer from this paper that firms with greater female board representation pay CEOs more (even though  
20 that is what a majority of the specifications indicate), but neither does it provide reliable evidence that  
21 female board members somehow constrain CEO pay.

22 Usman et al (2019) is not included in the reference list, but it appears to be:

23 ***Muhammad Usman, Muhammad Umar Farooq, Junrui Zhang, Nanyan Dong, and***  
24 ***Muhammad Abdul Majid Makki (2019), "Women on boards and CEO pay-performance link,"***  
25 ***International Journal of Manpower, 40(7): 1171-1200.***

26 68. This paper uses two instrumental variables approaches (a Heckman approach and their  
27 2SLS approach) but neither uses plausible instruments. In their Heckman selection model, they use a  
28 number of firm characteristics (firm size, board size, etc.) that surely affect CEO compensation

1 directly.<sup>40</sup> Given this, the instruments are not valid. In their 2SLS approach uses the number of female  
2 directors in a firm’s industry. As I have repeated many times in this declaration, this only works as an  
3 instrument if it is the case that female directors have no effect on the performance of a firm’s  
4 competitors, which presumably undercuts the entire claim made by the paper. They also provide a  
5 propensity score matching approach which does not account for omitted variable bias (since matching is  
6 done on the basis of observed/included variables).

7 69. Once again, I will not go into detail for the papers listed as “Did Not Test Critical Mass,”  
8 but the papers in this grouping are no more reliable. The Adams and Ferreira paper noted by Konrad to  
9 have been highly cited merely relies on firm fixed effects in terms of attempting to isolate causality. As  
10 discussed, many times earlier, this only works if the effects of omitted variables are constant.  
11 Furthermore, Adams and Ferreira make the odd decision to not cluster their standard errors in their  
12 models that include firm fixed effects, making their determinations of statistical significance unreliable.  
13 In any event, at best, this paper provides very mixed evidence regarding the effect of women on boards  
14 on CEO compensation, sometimes finding that more women lead to more incentive pay and sometimes  
15 less. There is a similar lack of robustness with respect to overall compensation. To suggest that this  
16 paper shows that women on boards lead to beneficial effects on CEOs (as Konrad does in her exhibit D)  
17 is misleading at best.

### 18 **Exhibit E**

19 70. Konrad declares, on the basis of the articles detailed in exhibit E, “The findings from this  
20 set of studies indicate a clear and convincing picture of the beneficial effects of women directors on a  
21 corporation’s social responsibility.” However, as detailed below, virtually none of the studies she  
22 reviewed does a plausible job accounting for omitted variable bias, and many do not even attempt to  
23 account for this bias despite Konrad characterizing the studies as all testing/adjusting for endogeneity in  
24 the note to exhibit E. This failure to adequately address this bias leaves the literature far from being

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25 <sup>40</sup> For a famous presentation of this with respect to firm size, see Xavier Gabaix and Augustin  
26 Landier (2018), “Why Has CEO Pay Increased So Much,” *Quarterly Journal of Economics*, 123(1): 49–  
27 100 which concludes “the sixfold increase of U.S. CEO pay between 1980 and 2003 can be fully  
28 attributed to the sixfold increase in market capitalization of large companies during that period.” They  
find similar results looking at a more recent period in Xavier Gabaix, Augustin Landier, and Julien  
Sauvagnat (2014), “CEO Pay and Firm Size: An Update After the Crisis,” *Economic Journal*, 124(574):  
F40–F59.

1 clear and convincing about anything regarding the effect of female board participation and corporate  
2 social responsibility (CSR) metrics. I detail the problems with these papers below.

3 ***Muhammad Atif, Mohammed Hossain, Md Samsul Alam, and Marc Goergen (2021), “Does***  
4 ***board gender diversity affect renewable energy consumption?” Journal of Corporate Finance,***  
5 ***66: 101665.***

6 71. This paper’s identification strategy involves using the ratio of females to males in the  
7 workforce of a firm’s state as an instrument for female board members. This analysis does not use firm  
8 fixed effects when modeling the firm’s use of renewable energy. As mentioned before, such an  
9 approach requires that nothing about a firm’s location affects its performance, otherwise the instrument  
10 will have direct effects on firm behavior, leaving the instrument invalid. The paper also uses propensity  
11 score matching, which likewise only yields credible causal estimates under very strong assumptions  
12 (i.e., that all omitted variables are completely accounted for by the included variables). This paper does,  
13 however, offer a third approach that is potentially more plausible by focusing on the replacement of a  
14 male director with a female director, compared to matched firms that replace a departing male director  
15 with another male director around the same time. In a difference-in-difference framework, if the  
16 replacement decision is taken to be a shock (i.e., random with respect to unmodeled firm  
17 characteristics), such an approach would yield causal estimates. While this is admittedly a questionable  
18 assumption, it at least represents a somewhat novel approach in this literature and provides a more well-  
19 defined identification assumption. Unfortunately, the authors do not implement the difference-in-  
20 difference test appropriately since they do not include firm fixed effects (instead relying on industry  
21 fixed effects). The paper does not reveal what would occur if the correct, more rigorous approach were  
22 used, leaving no clue to what a sensible causal estimate would be.

23 ***Walid Ben-Amar, Millicent Chang, and Philip McIlkenny (2017), “Board Gender Diversity***  
24 ***and Corporate Response to Sustainability Initiatives: Evidence from the Carbon Disclosure***  
25 ***Project,” Journal of Business Ethics, 142: 369-383.***

26 72. This paper uses a firm’s board size and whether it has a mandatory retirement policy for  
27 directors to instrument for female board participation. As noted with respect to other papers, if these  
28 firm policy decisions have any other effects on firm behavior or performance, these instruments will not



1 be valid. This concern is heightened in this case since the regressions do not include firm fixed effects  
2 (which would otherwise at least adjust for any constant effects arising from omitted variables). It is also  
3 interesting that these instruments are not actually statistically significant predictors of the authors'  
4 chosen female board metric, casting doubt on whether their instrumenting approach is valid even if  
5 exogeneity of the instruments is assumed. Another interesting aspect of this paper is that its evidence  
6 for the critical mass effect is weak at best since the paper estimates fairly constant effects of female  
7 board participation on carbon disclosure likelihood regardless of whether a firm has one, two, or three  
8 women board members, whereas the critical mass effect would require that the estimated effect grows in  
9 size as more women are appointed to boards. Despite this, Konrad declares that this paper supports the  
10 critical mass hypothesis (see "Critical Mass" column in Exhibit E). To be fair, however, these results  
11 are unreliable since they do not account for endogeneity through instruments or even through firm fixed  
12 effects, so we are actually left with no credible estimate of the critical mass effect one way or the other.

13 Boulouta (2013) is not included in the reference list, but it appears to be:

14 ***Ioanna Boulouta (2013), "Hidden Connections: The Link Between Board Gender Diversity***  
15 ***and Corporate Social Performance," Journal of Business Ethics, 113(2): 185-197.***

16 73. This paper uses a GMM approach to instrument its female board metric, which relies on  
17 using the lags of the predictor variables as instruments. As discussed before, this approach only works if  
18 one believes there are no lagged effects of firm characteristics on firm behavior. This paper also fails to  
19 account for firm fixed effects. Bizarrely, the author claims that fixed effects are not appropriate because  
20 simultaneity/reverse causality issues might be present (p. 191) and so she uses the instrumental variables  
21 approach. As indicated before, rigorous modeling would involve using both fixed effects and  
22 instrumental variables so as to better account for omitted variable bias (of which endogeneity,  
23 simultaneity, and reverse causality are all examples). In any event, even if all of these concerns are  
24 ignored, the effect of the author's female board metric on the various corporate social performance  
25 metrics is not generally statistically significant.

26 Charumathi and Rahman (2019) is not included in the reference list, but it appears to be:

27 ***B. Charumathi and Habeebu Rahman (2019), "Do Women on Boards Influence Climate***  
28 ***Change Disclosures to CDP? – Evidence from Large Indian Companies," Australasian***

1            *Accounting, Business, and Finance Journal, 13(2)*

2            74.      This paper is not credible at all. Not only does it not account for omitted variable bias in  
3 any way, it does not even adjust for basic covariates, instead relying on comparisons of means. It is  
4 highly questionable as to why Konrad included this paper given that she asserts that all listed studies  
5 “tested/adjusted for endogeneity.”

6            Cook and Glass (2018) is not listed in the references, but it appears to be:

7            *Alison Cook and Christy Glass (2018), “Women on corporate boards: Do they advance*  
8 *corporate social responsibility?”*

9            75.      In addition to firm fixed effects, this paper attempts to use lagged variables, including its  
10 female board metrics, to address endogeneity. Unfortunately, this approach does nothing to address the  
11 problem since just as omitted variables can confound current relationships, they also can confound  
12 lagged relationships, especially if there is persistence in any of the effects. This paper also does nothing  
13 to adjust for dependence in its standard errors, which potentially significantly overstates the estimates’  
14 precision which is particularly worrisome here since so many of the estimated effects are only borderline  
15 statistically significant.

16            *Claude Francoeur, Réal Labelle, Souha Balti, and Saloua EL Bouzaidi (2019), “To what*  
17 *extent do gender diverse boards enhance corporate social performance?” Journal of Business*  
18 *Ethics, 155(2): 343-357.*

19            76.      This article uses a GMM approach that uses lagged variables as the instruments for the  
20 female board metric in regressions explaining a firm’s corporate social responsibility characteristics. As  
21 noted above, this approach only works if lagged effects of firm characteristics on firm behavior are ruled  
22 out. Especially given the paper’s failure to even account for firm fixed effects, this assumption seems  
23 dubious.

24            Hussain, Rigoni, and Orij (2018) is not included in the reference list, but it appears to be:

25            *Nazim Hussain & Ugo Rigoni, and René P. Orij (2018), “Corporate Governance and*  
26 *Sustainability Performance: Analysis of Triple Bottom Line Performance,” Journal of*  
27 *Business Ethics, 149(2): 411-432.*

28            77.      This paper moves back and forth between fixed and random effects models, only finding

1 any support for a positive relationship between women on a firm’s board and its various corporate social  
2 responsibility metrics for one set of metrics (social sustainability) and only then when random effects  
3 models are used. Unfortunately, it is well known that random effects models generate biased estimates  
4 if there are omitted variables.<sup>41</sup>

5 ***Eunjung Hyun, Daegyung Yang, Hojin Jung, and Kihoon Hong (2016), “Women on boards and  
6 corporate social responsibility,” Sustainability, 8(4): 300.***

7 78. This paper uses fixed effects models. If omitted variable effects are constant, this  
8 approach will yield credible causal estimates. Unfortunately, this is a very strong and likely  
9 unwarranted assumption. This paper also included its lagged dependent variable as a predictor which  
10 generates bias problems as discussed above. Finally, the authors do nothing to account for dependence  
11 in their data, implying that their standard errors are incorrectly calculated.

12 ***Nooraisah Katmon, Zam Zuriyati Mohamad, Norlia Mat Norwani, and Omar Al Farooque  
13 (2019), “Comprehensive board diversity and quality of corporate social responsibility  
14 disclosure: Evidence from an emerging market,” Journal of Business Ethics, 157: 447-481.***

15 79. This paper uses lagged predictors as instruments for some of its control variables, which  
16 is inappropriate, but more important, it does not actually instrument for its board gender variable and  
17 therefore does not even attempt to account for endogeneity of the effect for which Konrad relies on the  
18 paper. Further, in the model where IV is used (inappropriately), the gender effect is not statistically  
19 significant. That Konrad includes this paper as having been one that tested/adjusted for endogeneity is  
20 misleading at best.

21 McGuinness, Vieito, and Wang (2017) is not included in the reference list, but appears to be:

22 ***Paul B. McGuinness, João Paulo Vieito and Mingzhu Wang(2017), “The role of board gender  
23 and foreign ownership in the CSR performance of Chinese listed firms,” Journal of Corporate  
24 Finance, 42(C): 75-99.***

25 80. This paper’s attempt to account for omitted variable bias is quite odd. The paper uses a  
26 Heckman selection model (a variety of an instrumental variables analysis as described above), but it

27  
28 <sup>41</sup> See Jeffrey M. Wooldridge (2001), *Econometric Analysis of Cross Section and Panel Data*, 1<sup>st</sup>  
ed., section 10.2.

1 does so without any instrument. That is, the paper merely models whether a firm has, for example,  
2 female directors as a function of the other control variables in the regression. This does nothing to  
3 account for omitted variables.<sup>42</sup> This paper's attempt to adjust for endogeneity is unreliable.

4 ***Elmagrhi, M. H., Ntim, C. G., Elamer, A. A., & Zhang, Q. (2019). A study of environmental***  
5 ***policies and regulations, governance structures, and environmental performance: The role***  
6 ***of female directors. Business Strategy & the Environment, 28(1), 206-220.***

7 81. This paper uses GMM methods (effectively using lagged control variables as  
8 instruments) which is only appropriate if one assumes there are no delayed effects of firm characteristics  
9 on firm behavior. This approach is unreliable.

10 Hollindale et al (2019) is not included in the reference list, but it appears to be:

11 ***Janice Hollindale, Pamela Kent, James Routledge, and Larelle Chapple (2019), "Women on***  
12 ***boards and greenhouse gas emission disclosures," Accounting and Finance, 59: 277-308.***

13 82. This paper does not even claim to address endogeneity, despite Konrad's indication that it  
14 does.

15 Isidro and Sobral (2015) is not included in the reference list, but it appears to be:

16 ***Helena Oliveira Isidro and Márcia Sobral (2015), "The Effects of Women on Corporate***  
17 ***Boards on Firm Value, Financial Performance, and Ethical and Social Compliance," Journal***  
18 ***of Business Ethics, 132(1): 1-19.***

19 83. This paper uses a simultaneous equations model. In principle, such a model works  
20 similarly to an instrumental variables model, but the authors do not use any instruments in their  
21 estimation. The results of such a modeling approach are not reliable.

22 ***Riadh Manita, Maria Giuseppina Bruna, Rey Dang, L'Hocine Houanti (2018), "Board gender***  
23 ***diversity and ESG disclosure: Evidence from the USA," Journal of Applied Accounting***  
24 ***Research, 19(2): 206-224.***

25 84. This paper, which Konrad notes does not find evidence of a positive effect of female  
26 board participation on firm disclosure scores, uses only lagged control variables and firm fixed effects to

27 <sup>42</sup> For a technical discussion of how a Heckman type model works using only the inverse Mills  
28 ratio for identification, see Jeffrey M. Wooldridge (2001), *Econometric Analysis of Cross Section and Panel Data*, 1<sup>st</sup> ed., 564.

1 account for endogeneity. As discussed before, the use of firm fixed effects only accounts for omitted  
2 variables whose effects are constant throughout the sample period, and lagged variables do not account  
3 for omitted variable bias at all. This approach is not generally reliable.

4 *María Consuelo Pucheta-Martínez, Inmaculada Bel-Oms, and Gustau Olcina-Sempere*  
5 *(2019), “Commitment of independent and institutional women directors to corporate social*  
6 *responsibility reporting,” Business Ethics: A European Review, 28: 290-304.*

7 85. This paper does nothing to account for endogeneity, despite Konrad’s statement that all  
8 of the papers in her Exhibit E “tested/adjusted for endogeneity.” Although this makes the paper  
9 unreliable, it is interesting that while Konrad includes it as supporting the critical mass idea because the  
10 paper estimates a non-linear effect of women board members on corporate social responsibility  
11 reporting, the nonlinear effect is actually the opposite of what the critical mass idea predicts. That is, in  
12 this paper, at low levels, additional women on the board improve reporting but after a point, additional  
13 women reduce reporting. Again, the paper is unreliable, so there is no reason that this paper should  
14 influence anyone’s views on whether there is a critical mass effect or not, but, arguably, Konrad’s use of  
15 this paper is misleading.

16 *María del Carmen Valls Martínez, Pedro Antonio Martín Cervantes, and Salvador Cruz*  
17 *Rambaud (2020), “Women on corporate boards and sustainable development in the American*  
18 *and European markets: Is there a limit to gender policies?” Corporate Social Responsibility &*  
19 *Environmental Management, 27: 2642-2656.*

20 86. This paper uses a fixed effects model, which accounts for constant effects of omitted  
21 variables at the firm level. Unfortunately, this paper includes the lagged dependent variable as an  
22 explanatory variable which, as discussed above, generates bias in the estimates. The paper does not  
23 provide the fixed effects estimates without including the lagged outcome variable as a control, leaving  
24 the reader unable to assess the reliability of the paper’s estimates, even if one is willing to assume that  
25 any omitted variable effects are constant.

26 87. I omit discussion of the papers Konrad includes as not testing the critical mass idea, but  
27 they too universally share the identification problems discussed above. For example, Beji et al (2021)<sup>43</sup>

28 <sup>43</sup> Rania Beji, Ouidad Yousfi, Nadia Loukil, and Abdelwahed Omri (2021), “Board Diversity and

1 uses a GMM approach which, as discussed above, essentially requires that there be no lagged effects of  
2 firm characteristics on the firm’s behavior since lagged control variables serve as the instruments in  
3 GMM. Likewise, Dang et al (2021)<sup>44</sup> use a “control function approach” which also requires an  
4 exogenous instrument. Their instrument is whether the firm is included in the S&P 100 index. If this  
5 instrument does indeed meet the exclusion restriction, their estimate of the effect of women on a firm’s  
6 board on the firm’s ESG disclosures is positive but only marginally statistically significant (i.e.,  
7 significant at the 10 percent level but not the 5 percent level). While this paper provides slightly more  
8 technical justification for its approach than most papers in this literature, reliability boils down to  
9 whether one believes that inclusion in the S&P 100 is unrelated to a firm’s behavior other than through  
10 its appointment of women to its corporate board. It has long been known that inclusion in the S&P 500  
11 affects firms in various ways, including providing capital inflows and increased investor scrutiny.<sup>45</sup> I  
12 am unaware of any similar research on effects of inclusion in the S&P 100. Intuitively, one might  
13 assume that the S&P 500 inclusion is substantively different given the prominence of that marker in the  
14 public’s perception, in which case maybe the instrument is legitimate. On the other hand, perhaps being  
15 included in the more elite group carries proportionately more attention. Further, S&P’s choices  
16 themselves presumably involve a host of firm characteristics which may be directly related to firm  
17 behavior. While this uncertainty about the instrument’s validity and the borderline statistical  
18 significance could reasonably lead one to be skeptical of the paper’s results, this paper at least attempts  
19 to address the omitted variable bias problem more rigorously than does most of this literature.

#### 20 **Exhibit F**

21 88. Konrad analyzes a set of studies that get to the heart of the notion that corporate sex  
22 diversity mandates will lead to more gender equity in Exhibit F. On the basis of what she describes as  
23 “9 rigorous panel studies that examined the impact of women directors on gender equity in firms,” she

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Corporate Social Responsibility: Empirical Evidence from France,” *Journal of Business Ethics*, 173,  
25 pages 133–155.

26 <sup>44</sup> Rey Dang, L'Hocine Houanti, Jean-Michel Sahut, and Michel Simioni (2021), “Do women on  
27 corporate boards influence corporate social performance? A control function approach,” *Finance  
28 Research Letters*, 39: 101645.

<sup>45</sup> See, for example, William B. Elliott, Bonnie F. Van Ness, Mark D. Walker, and Richard S.  
Warr (2008), “What Drives the S&P 500 Inclusion Effect? An Analytical Survey,” *Financial  
Management*, 35(4): 31-48.

1 concludes, “Overall, the set of results shown in Exhibit F provide clear and convincing evidence that  
2 WOCB enhance gender equity in firms.” Unfortunately, as describes below in detail, none of these  
3 studies is actually rigorous, and despite Konrad again asserting that all of the exhibit studies  
4 “tested/adjusted for endogeneity,” none does so adequately and a number of them do not even claim to  
5 do so. Because this literature sits so firmly in the core of what gender diversity mandates are meant to  
6 do, I examine all of the studies (not just those that examine the critical mass hypothesis, as I do in the  
7 rest of this report). This set of studies does not provide reliable evidence regarding the effect of female  
8 board participation on gender equity.

9 *Philipp Geiler and Luc Renneboog (2015), “Are female top managers really paid less?”*  
10 *Journal of Corporate Finance, 35: 345-369.*

11 89. This paper uses matching methods in its investigation of female pay disparities for  
12 executives and CEOs but uses Tobit regression (without any matching) to examine pay differentials  
13 between male and female executives and how they are affected by female board members. There is no  
14 attempt to control for endogeneity in any of these regressions beyond simply adding control variables.  
15 Despite Konrad indicating all of the papers listed in Appendix F have tested/adjusted for endogeneity,  
16 this is not the case, and these estimates are not reliable.

17 *Christina Quintana-García and Marta Elvira (2017), “The effect of the external labor market*  
18 *on the gender pay gap among executives,” Industrial & Labor Relations Review, 70(1): 132-*  
19 *159.*

20 90. This paper does not even claim to account for endogeneity in any way, as it merely uses  
21 control variables in its wage regressions. Beyond this approach being entirely unreliable due to the  
22 standard issue of omitted variables bias, the results in the paper do not actually even nominally indicate  
23 that “% [women on corporate board] reduce the executive gender pay gap” as claimed in Konrad’s  
24 Exhibit F entry for this paper (pointing to Tables 5 and 6). Table 6 does not include male pay at all in its  
25 analysis making it impossible to say anything about gender gaps. Table 5 does indicate that women  
26 executives earn less, though the effect is not statistically significant, and that firms with greater female  
27 representation on their boards pay executives more regardless of their sex. The Table 5 specifications  
28 do not involve the interaction between the female board metric and the female executive indicator that

1 would be necessary to examine any such differential effect of the female board metric by executive sex.  
2 The specification does include interactions between the female board metric and the female executive  
3 indicator only for those hired through the external labor market. Only including this interaction leaves  
4 unclear what the effect is on internally promoted executives, and what is the effect on males hired  
5 through the external market. Even if these issues are ignored, the included interaction effect, while  
6 positive is not statistically significant in any event. These results are unreliable, and they do not provide  
7 the support Konrad claims even if they were reliable.

8 *Yoshio Yanadori, Jill A. Gould, and Carol T. Kulik (2018). "A fair go? The gender pay gap*  
9 *among corporate executives in Australian firms," International Journal of Human Resource*  
10 *Management, 29(9): 1636-1660.*

11 91. This paper likewise does not even claim to account for endogeneity. Not only does the  
12 paper not include even firm fixed effects (which, as noted above, under certain assumption could yield  
13 some confidence regarding causality), it does not include even industry controls. This paper, however,  
14 does correctly include the female X female board representation interaction that is necessary to examine  
15 whether female board representation has any effect on the male-female executive pay gap. The results  
16 from this interaction are only marginally statistically significant for total pay ( $0.10 > p > 0.05$ ) and are  
17 not statistically significant for financial incentives. In any event, the failure to account for omitted  
18 variables bias in any way leaves these results unreliable.

19 Bozhinov, Joecks, and Scharfenkamp (in press) is not included in the reference list, but it appears  
20 to be:

21 *Viktor Bozhinov, Jasmin Joecks, and Katrin Scharfenkamp (2021), "Gender spillovers from*  
22 *supervisory boards to managementboards," Managerial and Decision Economics, 42(5):*  
23 *1317-1331.*

24 92. This paper's claimed methodological innovation (using correlated random effects) is  
25 somewhat odd in that correlated random error models are generally used in situations where a researcher  
26 wishes to estimate the effects of variables that do not vary within an individual entity in a panel (in this  
27 case within a firm), making firm fixed effects not feasible (since fixed effects absorb all non-time  
28 varying effects). The correlated random effects model weakens the assumptions for the standard



1 random effects model (which requires that the control variables are not correlated with constant  
2 unobservable effects) which is why it is preferable to the standard random effects model (though still  
3 imposing stronger assumptions than the fixed effects model).<sup>46</sup> In the Bozhinov et al paper, however,  
4 there is variation over time in their variable of interest (various versions of whether there were women  
5 on a firm’s corporate board in a previous period), making it possible to estimate fixed effects models  
6 (which, again, rely on weaker identifying assumptions than the correlated random effects assumption).  
7 A possible explanation for using correlated random effects models in this setting where fixed effects  
8 models are possible could be on efficiency grounds (i.e., the correlated random effects estimator might  
9 provide lower standard errors because it allows the estimator to gain information even from those  
10 entities where the variable of interest does not vary at the firm level over the researchers 2009-2016 time  
11 period), but even in that case, it would be important to provide both the correlated random effects  
12 estimates and the fixed effects estimates to discern whether they differed substantially (which would be  
13 indicative of bias in the correlated random effects estimates). That the authors do not do this is  
14 perplexing and should limit confidence in the reliability of the paper’s conclusions. Beyond this  
15 concern, as with fixed effects models, if any effects of omitted variables are changing over the 17-year  
16 period examined by the authors, the estimates would be biased and unreliable.

17 Cook and Glass (2016) does not appear in the reference list, but appears to be:

18 *Alison Cook and Christy Glass (2016), “Do women advance equity? The effect of gender*  
19 *leadership composition on LGBT-friendly policies in American firms,” Human Relations, 69(7):*  
20 *1431-1456.*

21 93. This paper has a number of problems and concerns. First, although the paper claims to  
22 control for firm fixed effects, it also indicates it controls for industry fixed effects, which would not be  
23 possible since a firm’s industrial classification is generally fixed, leaving a reader to question whether  
24 the models actually include firm fixed effects. This concern is important since firm fixed effects is the  
25 paper’s only attempt to address omitted variable bias. Secondly, the female board effect goes away  
26 (becomes statistically insignificant and the sign of the effect becomes inconsistent) in Table 5 which

27 <sup>46</sup> See, for example, Reinhard Schunck (2013), “Within and between estimates in random-effects  
28 models: Advantages and drawbacks of correlated random effects and hybrid models,” *Stata Journal*,  
13(1): 65–76.

1 limits the sample to firms with female CEOs. Since this is a small fraction of the sample, it might not be  
2 enough to totally extinguish the female board member effect, but it is driving at least some of the  
3 estimated effect; the authors never provide results controlling both for female CEO and the woman  
4 board member metric in the full sample to allow the reader to see how much of the general women  
5 board member effect is specific to firms with female CEOs. Along similar lines, Table 4 shows that  
6 firms with women board directors who are on other firms' boards ("interlinks") are drivers of the LGBT  
7 policies. However, the authors never present a regression that jointly includes the female board member  
8 metric, the female interlink measure, and a general interlink measure. It might be the case that it is  
9 interlinked boards in general that are driving the claimed female board and female interlink effects. It is  
10 standard practice when estimating interaction effects to include the main effects as well to avoid these  
11 biases. All that said, especially if the paper does not actually include firm fixed effects, this paper does  
12 not do much to account for endogeneity. Finally, this paper does nothing to account for the dependence  
13 issue that arises from using panel data, and therefore its standard errors are unreliably calculated, leading  
14 to reliability issues with its determination of statistical significance.

15 Corwin, Loncarich, and Ridge (in press) is not listed in the references, but it appears to be:

16 ***Emily Corwin, Holly Loncarich, and Jason W. Ridge (2021), "What's It Like Inside the Hive?  
17 Managerial Discretion Drives TMT Gender Diversity of Women-Led Firms, forthcoming.***

18 94. This paper does nothing to account for endogeneity in female board representation, not  
19 even fixed effects. This paper does not provide reliable estimates of the effect of female board  
20 representation on the degree to which a firm appoints women to its top management team.

21 Furlotti et al (2019) does not appear in the reference list, but it appears to be:

22 ***Katia Furlotti, Tatiana Mazza, Veronica Tibiletti, and Silvia Triani (2019), "Women in top  
23 positions on boards of directors: Gender policies disclosed in Italian sustainability reporting,"  
24 Corporate Social Responsibility and Environmental Management, 26(1): 57-70.***

25 95. This paper does not examine female board members generally, only the effect of having a  
26 female chairperson, indicating it is not relevant to the general issue of the effect of appointing women to  
27 corporate boards. Beyond this relevance issue, the paper does nothing to account for endogeneity,  
28 despite Konrad's indication that "all listed studies analyzed panel data and tested/adjusted for

1 endogeneity.”

2 Gould, Kulik, and Sardeshmukh (2018) is not listed in the references, but it appears to be:

3 ***Jill A. Gould, Carol T. Kulik, and Shruti R. Sardeshmukh (2018), “Trickle-down effect: The***  
4 ***impact of female board members on executive gender diversity,” *Human Resource****  
5 ***Management, 57(4): 931-945.***

6 96. This paper undertakes an instrumental variables analysis to account for endogeneity. As  
7 with any instrumental variables analysis, the reliability of the approach depends on the quality of the  
8 instrument. The authors’ instruments include a metric of how many of a firm’s male directors are on  
9 other boards with female directors. This instrument has been criticized above since the presence of  
10 female directors on a competitor’s boards may have competitive effects which would violate the  
11 exclusion restriction. That said, this paper does perform a test of over-identifying restrictions, so if its  
12 other instruments are good (or at least not directly related to the firm’s behavior in the same way as the  
13 other instruments), it can validate the instrumental variable estimates. Unfortunately, one of the other  
14 instruments, the lagged outcome variable would, by construction, be endogenous in the same way since  
15 it is the same variable merely lagged (so any persistence in behavior would generate similar omitted  
16 variable bias). The authors purport to use a third instrument, the presence of ASX recommendations for  
17 disclosures, but since this instrument affects all firm in a given year, it is duplicative of a background  
18 year fixed effect (which the models do not include) and will therefore merely pick up background trends  
19 in female board representation which will obviously affect the first instrument, leaving all of the  
20 instruments to likely be endogenous in exactly the same way. Thus, the fact that the estimates pass the  
21 test of overidentifying restrictions is not a reliable guide to whether the estimates are indeed reliable  
22 themselves.

23 Matsa and Miller (2011) does not appear in the reference list, but it is likely:

24 ***David A. Matsa and Amalia R. Miller (2011), “Chipping away at the Glass Ceiling: Gender***  
25 ***Spillovers in Corporate Leadership,” *American Economic Review: Papers and Proceedings,****  
26 ***101(3): 635-639.***

27 97. This paper is included in the non-peer reviewed papers and proceedings issue of the  
28 American Economic Review, which publishes short versions of presentations made at the American

1 Economic Association’s Annual Meeting. The short papers are often descriptive or preliminary papers  
2 that, after receiving commentary and engaging in subsequent revisions often are published elsewhere or  
3 never generate a future publication. The authors never published a follow up version of the paper.  
4 While this short 5-page paper presents interesting descriptive results indicating that firms with higher  
5 proportions of females on their boards are more likely to have females among their top five executives  
6 in the following year, it is hard to claim causality. Of the six specifications they present, two include  
7 lagged dependent variables which can generate biases as discussed above. Of the remaining four  
8 specifications, only one includes firm fixed effects, and that estimate is anywhere from 25 to 50 percent  
9 as large as the other estimates, strongly suggesting that there is substantial scope for omitted variable  
10 bias in the authors’ estimates. While the authors do argue that their specification which includes leading  
11 (i.e., future) values of their female board metric and they generate statistically insignificant coefficients,  
12 leading them to suggest maybe there is a causal relationship between female board representation and  
13 the appointment of women among a firm’s top five executives, they do not do this exercise with firm  
14 fixed effects (or even with industry fixed effects). This leaves open the strong possibility that their  
15 timing argument would not survive strong attempts to account for endogeneity. The concluding remarks  
16 of Matsa and Miller (“If these estimates capture the causal effects of changing board composition, what  
17 do they imply about the nature of the glass ceiling and its potential remedies?” p. 639) indicate the  
18 speculative nature of their analysis. Such speculation is completely appropriate in the context of a  
19 conference proceedings where the idea is to solicit commentary and criticism, as well as to spur future  
20 research, but it does not constitute reliable evidence. The fact that Matsa and Miller (or anyone else in  
21 the almost 400 articles citing Matsa and Miller) have not published even a working paper shoring up  
22 these conference results in the intervening 10 years is indicative that the preliminary results have not led  
23 to reliable subsequent estimates.

24 **Exhibit G**

25 98. Konrad states that the studies she examines in exhibit G “clearly show that firms with  
26 more women directors take fewer extreme risks that push the firm into crisis and failure.” When the  
27 studies are examined closely, however, this is far from clear. The first two studies she lists (the only  
28 ones examining the critical mass effect) have mixed results. Even putting aside the methodological

1 problems with these studies, which are similar to those already reviewed, it is misleading to say that this  
2 literature clearly shows anything about the relationship between female board participation and risk.  
3 The more accurate assessment, even taking the studies' reliability as given (which would be a mistake),  
4 is that it depends on what firms one considers, which implies it is imprudent to force a one-size-fits-all  
5 mandate on all firms.

6 Birindelli, Chiappini, and Savioli (2020) does not appear in the reference list, but it is likely:  
7 ***Giuliana Birindelli , Helen Chiappini , and Marco Savioli (2020), "When do women on board***  
8 ***of directors reduce bank risk?" Corporate Governance, 20(7): 1307-1327.***

9 99. This paper uses fixed effects models, which can reliably isolate causal effects under the  
10 assumption that the effects of unmodeled characteristics of the bank are constant over time. While this  
11 assumption can be questioned, this paper represents a bigger issue with respect to its conclusion in  
12 Konrad's report. First, when the authors break the sample into sound and unsound banks, the effects of  
13 female participation on the board are directly opposite each other (negative relationship with risk for  
14 sound banks; positive for unsound banks). This, even if one completely accepts the results of the paper,  
15 suggests that a one-size-fits-all mandate for board diversity is inappropriate. To put the issue more  
16 pointedly, presumably risk management efforts are even more important for unsound banks, in which  
17 case these results suggest a mandate to increase female board participation would be harmful to the  
18 banks most at risk. Second, although Konrad suggests that this paper supports the critical mass  
19 hypothesis, many of the specifications show a non-linear relationship that shows while low levels of  
20 women on boards can be helpful in mitigating risk for some banks, a higher level is detrimental. That is,  
21 the paper's mixed results hardly support the general conclusion that reaching a critical mass of women  
22 on bank boards reduces risk, even if the paper is taken as completely reliable.

23 Dowling and Aribi (2013) is absent from the reference list, but it is probably:  
24 ***Michael Dowling and Zakaria Ali Aribi(2013), "Female directors and UK company***  
25 ***acquisitiveness," International Review of Financial Analysis, 29(C): 79-86.***

26 100. Putting aside concerns about endogeneity (of which there are many; the only attempt the  
27 paper makes to account for endogeneity is to include basic covariates), this paper, while finding a  
28 relationship between females on corporate boards and acquisition activity of the firm, provides no way

1 to assess whether these acquisitions improved or worsened risk. That is, there is no a priori effect of an  
2 acquisition on risk. An acquisition could worsen risk if the acquired firm is risky itself or if the financial  
3 details of the acquisition generate financial risk. On the other hand, an acquisition could generate  
4 diversification opportunities across markets, as well as provide improvements to supply chains and the  
5 like. Nothing in this paper provides any insight into which acquisitions improved risk or made it worse.  
6 It would be wrong to suggest that this paper reliably supports the claim that greater female board  
7 participation improves a firm's risk.

8 **Exhibit H**

9 *Niccolò Gordini and Elisa Rancati (2017), "Gender diversity in the Italian boardroom and*  
10 *firm financial Performance," Management Research Review, 40(1), 75-94.*

11 101. This paper purports to run an instrumental variables regression (2SLS), but it literally has  
12 zero discussion of what instruments it used to account for the endogeneity of the female board metrics.  
13 Without any way to assess the validity of the instrument used, it is not prudent to rely on this study.

14 Isidro and Sobral (2015) is not included in the reference list, but it appears to be:

15 *Helena Isidro and Márcia Sobral (2015), "The Effects of Women on Corporate Boards on*  
16 *Firm Value, Financial Performance, and Ethical and Social Compliance," Journal of Business*  
17 *Ethics, 132(1): 1-19.*

18 102. Although this paper estimates a simultaneous equation model in an attempt to address  
19 endogeneity concerns, they do not claim to include any exogenous instruments in their system, which  
20 means the entire system is unidentified. Their approach is not reliable, and their estimates likely suffer  
21 from omitted variables bias.

22 Joecks, Pull, and Vetter (2013) is not included in the reference list, but it is likely:

23 *Jasmin Joecks, Kerstin Pull and Karin Vetter (2013), "Gender Diversity in the Boardroom and*  
24 *Firm Performance: What Exactly Constitutes a "Critical Mass?"* *Journal of Business*  
25 *Ethics, 118(1): 61-72.*

26 103. The only attempt to address endogeneity in this paper is to implement random effects,  
27 which only addresses omitted variable bias if the unmodeled effects are uncorrelated with the modeled  
28 effects, which is a highly restrictive assumption. The results are therefore unreliable.

1            **Owen, A. L., & Temesvary, J. (2018). *The performance effects of gender diversity on bank***  
2            ***boards. Journal of Banking & Finance, 90, 50-63.***

3            104. See comments on Section B above.

4            Nguyen, Locke, and Reddy (2015) is not included in the reference list, but it is likely:

5            ***Tuan Nguyen, Stuart Locke, and Krishna Reddy (2015), “Does boardroom gender diversity***  
6            ***matter? Evidence from a transitional economy,” International Review of Economics &***  
7            ***Finance, 37(C), pages 184-202.***

8            105. This paper relies on a systems GMM approach. For this approach to be valid, it must be  
9            the case that the lagged control variables are indeed unrelated to the outcome variable. That is, one must  
10           believe that things like the presence of outside directors or the existence of block shareholders only have  
11           immediate but not longer run effects on firm value (Tobin’s Q, which is used as the outcome variable in  
12           this study). If, instead, one believes that these variables can have enduring effects on firm value (or the  
13           choices and behavior that affect that value), the GMM approach will not provide reliable causal  
14           estimates.

15           Ramly et al (2017) is not contained in the reference list, but it appears to be:

16           ***Zulkuflly Ramly, Sok-Gee Chan, Mohd Zulkhairi Mustapha, and Noor Sharoja Sapiei (2017),***  
17           ***“Women on boards and bank efficiency in ASEAN-5: the moderating role of the independent***  
18           ***directors,” Review of Managerial Science, 11(1): 225-250.***

19           106. This paper likewise uses GMM methods, so the preceding comment applies.

20           ***Jun Xie, Wataru Nozawa, and Shunsuke Managi, (2020), “The role of women on boards in***  
21           ***corporate environmental strategy and financial performance: A global outlook,” Corporate Social***  
22           ***Responsibility & Environmental Management, 27: 2044-2059.***

23           107. This paper does nothing to address endogeneity. Its regressions account for only sector  
24           and country fixed effects, not firm fixed effects, so any unobservable characteristics of the firm (even  
25           constant ones) that are correlated with the presence of female directors (and recall that Konrad’s entire  
26           report is an argument that female directors are related to almost every important aspect of firm behavior  
27           and performance), will generate a bias in the estimation of the relationship between female board  
28           participation and financial performance. Interestingly enough, if one ignores this concern, some of the

1 results in this paper show a negative relationship between women on a firm’s board and financial  
2 performance (e.g., return on assets in Table 7). However, there is no reason to believe the results from  
3 this paper.

#### 4 **Conclusion**

5 108. As stated before, Konrad relies selectively on the literatures relevant to claims she makes.  
6 However, even if this is ignored, the papers she cites are generally not credible and their conclusions are  
7 unreliable. Perhaps anticipating this criticism, Konrad provided a supplemental declaration in which she  
8 attempts to claim that although it is hard to make causal inferences, “Such top-tier studies in this field  
9 show amazing consistency in replicating findings showing that board diversity has beneficial effects on  
10 firm outcomes, and such replication is a critically important piece of the evidence scientists use to build  
11 theoretical consensus regarding causal inferences. As such, these studies are as valid as they can  
12 possibly be, and the consistency of their findings is an extremely strong indicator supporting  
13 causality.<sup>47</sup>” One reason that the results Konrad cites exhibit “amazing consistency” is because she  
14 either ignores or is unaware of the large number of studies reaching different conclusions. Further, as  
15 argued here, many of the papers she cites duplicate each other’s errors (e.g., using invalid instruments;  
16 not accounting for endogeneity at all, failure to appropriately account for dependence in the data, etc.),  
17 so their consistency of results is not generally a source of confidence. Konrad’s conclusions are not  
18 reliable.

#### 19 **Schipani declaration**

20 109. Professor Cindy Schipani offers a number of conclusions in her declaration that are not  
21 well-supported by reliable research. For many of her claims, Schipani cites to media reports or  
22 consulting reports, none of which is subjected to peer review or any other form of methodological  
23 evaluation. As I have indicated elsewhere (Klick 2021), consulting reports in general (including specific  
24 discussion of many of the reports cited by Schipani) use data from non-representative samples, engage  
25 in limited attempts to adjust or control for differences across firms when discussing gender  
26 representation and its effects on firm behavior, and rarely discuss their methods in a way that allows for

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27 <sup>47</sup> SUPPLEMENTAL DECLARATION OF ALISON KONRAD, PH.D, IN SUPPORT OF  
28 SECRETARY OF STATE’S OPPOSITION TO PLAINTIFFS’ MOTION FOR SUMMARY  
JUDGMENT Reservation No. 978453166871, paragraph 16.



1 transparent assessment of the reliability of the claims made in such studies.<sup>48</sup> These sources are  
2 unreliable from a social scientific standpoint. Schipani also heavily relies on sources published in  
3 student-edited law reviews, which likewise are not subjected to any methodological scrutiny and should  
4 be viewed with caution in terms of reliability of the methods used to draw conclusions, at least when it  
5 comes to statistical analyses or any claims regarding the causality link between women in board  
6 positions and firm performance.

7 110. Luckily, Schipani also draws upon potentially more reliable quantitative research  
8 published in peer-reviewed journals. Unfortunately, peer review alone is no guarantee of  
9 methodological rigor, as detailed below. As a general matter, the work Schipani invokes does not  
10 provide a reliable evidentiary basis for most of her claims. In the sections that follow, I walk through  
11 the claims and offered evidence.

### 12 **Underrepresentation of Women on Boards of Directors**

13 111. In this section, where Schipani claims that women are underrepresented on corporate  
14 boards as compared to men, Schipani invokes mostly consulting, media, and law review sources to make  
15 her argument. Beyond these sources, Schipani uses Bureau of Labor Statistics data (from the Current  
16 Population Survey) to note that women make up more than 50 percent of management, professional, and  
17 related occupations and represent similar proportions of people entering law and medical schools and  
18 more than half of those receiving doctoral degrees and nearly half of all master's degrees in business.  
19 However, contrasted with this, she notes sources indicating that board seats are not distributed equally  
20 between men and women. The implication is that if the pool of educated people is on parity between  
21 men and women, boards should reflect parity as well. Presumably, however, board membership is  
22 related to educational flows at a lag. Parity (and beyond) of women in these educational categories is a  
23 relatively recent phenomenon.<sup>49</sup>

24 112. Further, in the kinds of educational programs most likely to generate board members (in  
25

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26 <sup>48</sup> For details regarding the studies Schipani relies on, including reports by Catalyst, MSCI,  
27 McKinsey, Credit Suisse, and others, see Jonathan Klick (2021), Literature Review on Diversity on  
28 Corporate Boards, American Enterprise Institute Monograph.

<sup>49</sup> See, for example, the data presented in National Center for Education Statistics, Projections of  
Education Statistics to 2028, Post Secondary Degrees Conferred available at  
<https://nces.ed.gov/programs/PES/section-6.asp>.

1 the declaration of Jessica Grounds, the top four advanced degrees among female board members added  
2 to all-male board in California in 2019 were 1) MBA, 2) PhD in science or health-related field, 3) JD,  
3 and 4) MD), parity was reached only very recently, especially in the elite segment of that educational  
4 market<sup>50</sup> if it has been reached at all.<sup>51</sup> It should not be surprising, given this recency and that even the  
5 most elite professional school graduates cannot ascend to corporate boards upon graduation, that parity  
6 on boards has not arrived. Accounting for this issue makes the gender-gap much less striking. For  
7 example, the existing stock (i.e., not just the in-flow of graduates) of those with a research doctoral  
8 degree in science, engineering, or health is split 65 percent/ 35 percent in favor of men without even  
9 taking program prestige into account.<sup>52</sup> However, the stock differentials decline with age, suggesting  
10 that the pools will grow ever closer together, naturally leading to greater parity in board composition.

11 113. When saying that women are under-represented, Schipani is implicitly suggesting that  
12 boards should mirror the unconditional population in the U.S. Board members, however, are not drawn  
13 from the unconditional population. Instead, board members exhibit backgrounds that differ from the  
14 general population with respect to their educational backgrounds in terms of both the kind, quality, and  
15 prestige of that education. Comparisons that do not take these issues into account are not reasonable.

### 16 **Factors Explaining “Underrepresentation” of Women on Boards**

17 114. In discussing the factors that lead to the claimed underrepresentation of women on

18 <sup>50</sup> Women still do not outnumber men in MBA programs, and the University of Pennsylvania  
19 (see Jason Armesto, “Why women are the MBA minority and how that can change,” *Fortune*: July 20,  
20 2021 available at [https://fortune.com/education/business/articles/2021/07/20/why-women-are-the-mba-  
21 minority-and-how-that-could-change/](https://fortune.com/education/business/articles/2021/07/20/why-women-are-the-mba-minority-and-how-that-could-change/)) and the University of Pennsylvania’s Wharton School was the  
22 first “elite” business school to see women outnumber men, and that did not happen until Fall 2021 (see  
23 Patrick Thomas, “Wharton Is First Elite M.B.A. Program to Enroll More Women Than Men,” *Wall  
24 Street Journal*: July 28, 2021. Women first outnumbered men in terms of law school enrollment in 2016  
25 and lagged far behind until at least the 1990s (see time series data at  
26 [https://www.americanbar.org/content/dam/aba/administrative/legal\\_education\\_and\\_admissions\\_to\\_the  
27 bar/statistics/enrollment\\_degrees\\_awarded.xls](https://www.americanbar.org/content/dam/aba/administrative/legal_education_and_admissions_to_the_bar/statistics/enrollment_degrees_awarded.xls)). Women did not represent a majority of medical school  
28 students until 2019 (see Linda Searing, “The Big Number: Women now outnumber men in medical  
schools,” *Washington Post* December 23, 2019).

<sup>51</sup> While women have received as many or more PhDs as men have for over a decade, they are  
still severely under-represented in science and health-related fields, especially at more prestigious  
programs (see Kim A. Weeden,

Sarah Thébaud, and Dafna Gelbgiser (2017), “Degrees of Difference: Gender Segregation of  
U.S. Doctorates by Field and Program Prestige,” *Sociological Science*, 4: 123-150).

<sup>52</sup> Daniel J. Foley, Lance A. Selfa, and Karen H. Grigorian (2019), “Number of Women with  
U.S. Doctorates in Science, Engineering, or Health Employed in the United States More Than Doubles  
since 1997,” *Info-Brief National Center for Science and Engineering Statistics*, NSF 19-307.

1 boards, Schipani invokes interviews and consulting materials that attribute the low number of women to  
2 firms not prioritizing the recruitment of women, few women in the traditional pipeline, low board  
3 turnover, and a tendency of men to prefer individuals who are like them. She also notes a study  
4 claiming that firms prefer gender matching when replacing departing board members which creates an  
5 inertia favoring male board members.<sup>53</sup> In the cited study’s analysis using real world data (there are a  
6 number of analyses using data from experiments with undergraduate students; these analyses are  
7 seemingly not relevant given that undergraduate students do not choose corporate board members),  
8 while it is true that firms appear to be more likely to appoint a woman board member when there are  
9 more female board members exiting and less likely to appoint a female when there are more male  
10 directors exiting, the marginal effect relating to female exit is almost 3 times as large as the effect  
11 relating to male director exit. This suggests a natural reduction in the net gap between male and female  
12 directors if directors of both sexes exhibit comparable tenures. That Schipani emphasizes the inertial  
13 effect (leading to continued disparities) over the net effect (leading to reduced disparities) is somewhat  
14 misleading.

15 115. Schipani also cites a study in support of the idea that gender stereo type bias affects board  
16 choices.<sup>54</sup> This study analyzes an experiment where there were no consequences for the participants,  
17 and the scenario analyzed in the experiment had nothing to do with corporate leaders. Also, there is no  
18 indication that the study participants were similar in any way to those individuals who are involved in  
19 choosing corporate board members. The experimental setting of the decisions of the participants, the  
20 different context of the decision, and the total disconnect between the study’s decisionmakers and the  
21 policy relevant decisionmakers make the relevance of this study highly questionable. There is little to  
22 tie this research to policy questions about corporate board choices.

23 116. Schipani uses another study to claim that employers favor men because they discriminate  
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25 <sup>53</sup> Catherine H. Tinsely, James B. Wade, Brian G. M Main, and Charles A. O’Reilly (2016),  
26 “Gender Diversity on U.S. Corporate Boards: Are We Running in Place?” *Industrial and Labor  
Relations Review*, 70(1): 160-189.

27 <sup>54</sup> Victoria L. Brescoll, Erica Dawson & Eric Luis Uhlmann (2010), “Hard Won and Easily Lost:  
28 The Fragile Status of Leaders in Gender-Stereotype-Incongruent Occupation,” *Psychological Science*,  
21(11): 1640-1642.

1 against mothers but not fathers.<sup>55</sup> One of the analyses in this study is a small sample laboratory  
2 experiment of undergraduates, who faced no consequences for their choices, the question involved  
3 hiring an employee, not a board member, and the undergraduates bear little resemblance to individuals  
4 who actually choose board members. This portion of the study is similarly not relevant to the policy  
5 question involved in a board diversity mandate. In the more realistic part of the study, the researchers  
6 sent out otherwise identical resumes in response to job openings where they randomized indications of  
7 the applicants' sex and parental status. While it is true that there was a statistically significant negative  
8 effect on the interaction between being a parent and being female (implying a motherhood "penalty" but  
9 no fatherhood "penalty"), the study also found a statistically significant positive effect of being a  
10 woman. This suggests an ambiguity regarding whether firms discriminate against women in total.  
11 Although the experimental manipulation provides methodological benefits in terms of causal inference  
12 and the use of real-world jobs provides a degree of external validity that is absent in the laboratory  
13 experiments, it is still highly questionable whether hiring choices for jobs advertised in a newspaper bear  
14 any resemblance to the choice of a corporate board member and provides little relevant evidence about  
15 the need for diversity mandates for corporate boards.

### 16 **Gender Diversity and Firm Performance**

17 117. The link between gender diversity and firm performance is mixed at best, and many  
18 proponents of diversity mandates cite a number of studies that are methodologically unreliable in  
19 support of their argument that gender diversity improves firm performance, while ignoring studies that  
20 do not support their arguments.<sup>56</sup> Schipani is no different in this regard, citing a number of consulting  
21 reports that have little in the way methodological rigor, as well as citing low quality studies and/or  
22 emphasizing only supportive parts of other studies, while ignoring those studies that find any negative  
23 effects of women on boards. For example, Schipani cites the positive parts of a 2015 meta-analysis,<sup>57</sup>  
24 for the proposition that the meta-analysis found a positive relationship between female representation on

25 <sup>55</sup> Shelley J. Correll, Stephen Benard, and In Paik (2007), "Getting a Job: Is there a Motherhood  
26 Penalty?" *American Journal Of Sociology*, 112: 1297-1338.

27 <sup>56</sup> See Jonathan Klick (2021), *Review of the Literature on Diversity on Corporate Boards*,  
American Enterprise Institute Monograph, as well as the earlier discussion of the Konrad declaration.

28 <sup>57</sup> Corrine Post and Kris Byron (2015), "Women on Boards and Firm Financial Performance: A  
Meta Analysis," *Academy of Management Journal*, 58(5): 1546-1571.

1 boards and accounting returns without noting that the relationship is quite small and highly variable and  
2 that the study also found no relationship between female board membership and stock returns.

3 118. Another study Schipani cites<sup>58</sup> is methodologically unsound since its only attempt to  
4 account for endogeneity is to use the lag of their control variables. As discussed above, the use of lags  
5 does not account for omitted variables bias since there is persistence in firms' behavior and  
6 performance, so if a variable is endogenous to the current period's outcomes, it is also endogenous to  
7 past period outcomes. This paper has other methodological issues as well that call its reliability into  
8 further question.<sup>59</sup>

9 119. Schipani cites another paper<sup>60</sup> for the point that women on boards and financial  
10 performance are positively correlated. However, this is a gross simplification of the paper's actual  
11 results. First, the average effect (Table 2) of women on boards is not statistically significant when firm  
12 fixed effects are accounted for (as they should be if one worries about unmodeled firm characteristics  
13 influencing firm outcomes) and, even the coefficients themselves are mixed (negative for Tobin's Q as  
14 the outcome and positive for return on assets). When firm fixed effects are not included, while the  
15 effects are statistically significant, the relationship is negative for the return on assets outcome and  
16 positive only for the Tobin's Q outcome. When the authors move to the quantile regressions, they drop  
17 the firm fixed effects which makes the estimates unreliable since they do nothing to account for omitted  
18 variables bias. But even if that is ignored, the results suggest positive statistically significant  
19 relationships only for the Tobin's Q measure of performance; for the return on assets measure, at some  
20 points in the distribution, the estimated effect is statistically significantly negative, and at the median it  
21 is not statistically significant at all. Only at high (unexplained) returns on assets is the female board  
22 effect statistically significant and positive. If one takes these results at face value (which would likely  
23 be a mistake given the identification concerns noted earlier), at best, this recommends an increase in  
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25 <sup>58</sup> Victoria Geyfman, Wade A. Cooper, and Laura M. Davis (2018), "Board Gender Diversity  
26 and Bank Performance," *Journal of Business Diversity*, 18(1): 51-67.

27 <sup>59</sup> For example, despite using panel data, the regressions do not account for serial dependence,  
28 leading it to estimate biased standard errors, which make the paper's statistical significance  
determinations unreliable.

<sup>60</sup> Martin J. Conyon and Lerong He (2017), "Firm Performance and Boardroom Gender  
Diversity: A Quantile Regression Approach," *Journal of Business Research*, 79: 198-211.

1 female representation on a firm's board only for some firms. This kind of result does not support a one-  
2 size-fits-all regulation.

3 120. Schipani cites a number of papers for the proposition that technology organizations that  
4 are gender diverse improve employee performance. Putting aside methodological concerns (few of  
5 these papers address endogeneity), many of these papers do not examine female board members.  
6 Instead, they study female managers,<sup>61</sup> gender diversity in the overall workforce,<sup>62</sup> students in a class,<sup>63</sup>  
7 and small groups completing fairly simple tasks.<sup>64</sup>

### 8 **Importance of a "Critical Mass" of Women on Corporate Boards**

9 121. Schipani cites only to consulting reports for the proposition that a critical mass of women  
10 on the corporate board are important to influence and improve firm. As indicated before, such  
11 consulting reports are not methodologically rigorous and are generally unreliable.

### 12 **Value that Women in Key Positions Add to Firms**

13 122. Schipani claims that adding women to a board of directors improves firm value in many  
14 ways. While this section relies heavily on consulting reports, it also repeatedly discusses a paper from  
15 the American Economic Review, where Schipani notes the journal by name (as opposed to just  
16 including it in a citation footnote). However, she fails to mention that it is actually the American  
17 Economic Review Papers and Proceedings,<sup>65</sup> which is a non-refereed conference proceedings issue of  
18 the journal (published annually). This oversight leaves open the possibility that readers will accord the  
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20 <sup>61</sup> Hema A. Krishnan and Daewoo Park (2005), "A Few Good Women – on Top Management  
21 Teams," *Journal of Business Research*, 58: 1712-1720.

22 <sup>62</sup> Cedrick Herring (2009), "Does Diversity Pay?" *American Sociological Review*, 74(2): 208-  
23 224.

24 <sup>63</sup> Sander Hoogendoorn, Hessel Oosterbeek, Mirjam van Praag (2013), "The Impact of Gender  
25 Diversity on the Performance of Business Teams: Evidence from a Field Experiment," *Management  
26 Science*, 59(7): 1514–1528. Although the students in a class leave this study as lacking in relevance, the  
27 use of randomization does make this study reliable at least in terms of understanding the particular  
28 context studied.

<sup>64</sup> Anita Williams Woolley, Christopher F. Chabris, Alex Pentland, Nada Hashmi, and Thomas  
W. Malone (2010), "Evidence for a Collective Intelligence Factor in the Performance of Human  
Groups," *Science*, 330(6004): 686-688. This paper uses randomization for identification, so it is reliable  
in terms of the specific setting studied, but its relevance to boards directing companies is unclear.

<sup>65</sup> Daehyun Kim and Laura T. Starks (2016), "Gender Diversity on Corporate Boards: Do  
Women Contribute Unique Skills?" *American Economic Review: Papers and Proceedings*, 106(5): 267-  
271.

1 prestige of the normal journal, not to mention its low acceptance rate and rigorous review process, to an  
2 invited submission publication that gets very little editorial attention and no peer review. Perhaps the  
3 most useful information in the article is provided in figure 1 (unmentioned by Schipani) which shows  
4 that proportional female participation has been increasing in a wide range of firms (S&P 500, S&P 1500,  
5 S&P midcap 400, S&P smallcap 600) in an unbroken linear fashion over the period 1997-2013. The  
6 paper also provides descriptive statistics indicating the reported skills female directors bring to their  
7 firms but does not analyze any relationship to firm performance. It is incorrect to suggest that this paper  
8 provides any evidence regarding the relationship between female board participation and firm  
9 performance. Schipani does cite to a working paper<sup>66</sup> by the same authors (cited in the American  
10 Economic Review: Papers and Proceedings article) suggesting that these descriptive findings about the  
11 skills brought by female directors somehow creates the link with firm performance, but it appears as  
12 though the working paper is not publicly available, leaving it impossible to examine the reliability of the  
13 authors' methodology.

14 123. For the rest of this section, Schipani includes a number of studies of questionable  
15 relevance and often only citing them as having been cited by some other commentator. One such study  
16 purports to find that gender diversity reduces the likelihood of a financial restatement.<sup>67</sup> This study  
17 makes no credible attempt to identify a causal relationship. The study in Sustainability<sup>68</sup> that examines  
18 the relationship between female board participation and corporate social responsibility scores does use  
19 fixed effects, which is a credible approach if one assumes any omitted variable bias effect is constant for  
20 each firm, but they spoil the analysis by including the lagged dependent variable as a control which  
21 generates bias as discussed above. That said, the estimated effects of female directors on CSR scores  
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23 <sup>66</sup> Daehyun Kim and Laura T. Starks (2015), "Board Heterogeneity of Expertise and Board  
24 Performance," Working Paper. A link on Starks Google Scholar page likewise does not provide the  
25 working paper  
(<https://scholar.google.com/scholar?cluster=10371427173135621814&hl=en&oi=scholarrr>).

26 <sup>67</sup> Abbott J. Lawrence (2012), "Female Board Presence and the Likelihood of Financial  
27 Restatement," Accounting  
28 Horizons, 26(4): 607-629.

<sup>68</sup> Eunjung Hyun, Daegyung Yang, Hojin Jung, and Kihoon Hong (2016), "Women on Boards and  
Corporate  
Social Responsibility," Sustainability, 8: 300-325.

1 are never statistically significant and the effect of independent female directors is as often not  
2 statistically significant as it is statistically significant. This is hardly reliable evidence that female board  
3 directors add value to firms. Schipani also cites an article as cited within this article for the proposition  
4 that female directors reduce corruption,<sup>69</sup> that uses an instrumental variables approach that includes  
5 lagged control variables as instruments. However, if there is any lagged effect of firm characteristics on  
6 firm behavior, this approach is not reliable and will continue to generate an omitted variables bias. She  
7 cites Wahid (2019) which is unreliable as discussed earlier, as are other articles cited by Schipani.<sup>70</sup>

### 8 **Conclusion**

9 124. Schipani relies primarily on the assertions made by consulting firms, media outlets, and  
10 authors in student edited law reviews to argue that female board representation improves firm outcomes.  
11 She combines this with her determination that women are under-represented on firm boards since they  
12 fall short of parity, even though the stock of individuals with the educational backgrounds common for  
13 board members are not anywhere near parity. To the extent other research is used, it is generally either  
14 unreliable or it is not relevant.

### 15 **Grounds Declaration**

16 125. Ms. Jessica Grounds provides analysis of the number of women on corporate boards prior  
17 to the mandatory gender diversity provisions of SB826 and the number of women added to corporate  
18 boards in the years since SB826 went into effect. Grounds indicates that, despite the growth she  
19 documents between 2006 and 2015, the level of women directors among public firms headquartered in  
20 California is too low and the growth rate too slow to generate sufficient sex diversity on those boards.  
21 The determination of the “right” amount of female board representation is left somewhat vague.

22 126. As I suggested above [comments on Schipani declaration], female participation in the  
23 kinds of educational programs from which directors are drawn (according to Grounds’s data, MBA,  
24 science or health-related PhD, JD, and MD) has only recently reached parity (and still lags substantially  
25 for the PhD in science or health-related fields category) and the levels of individuals with those

26 <sup>69</sup> Douglas Cumming, T. Y. Leung, and Oliver Rui (2015), “Gender Diversity and Securities  
27 Fraud,” *The Academy of Management Journal*, 58(5): 1572-593.

28 <sup>70</sup> For example, Young Zik Shin, Jeung-Yoon Chang, Keyeongmin Jeon, and Hyunpyo Kim  
(2020), “Female Directors on the Board and Investment Efficiency: Evidence from Korea,” *Asian  
Business & Management*, 19: 438–479 uses the GMM approach criticized earlier.



1 educations, especially from the prestigious programs that are relatively attractive to firms, are still highly  
2 unequal. Given this, it is necessary to think seriously about what a reasonable growth trajectory would  
3 look like, noting that individuals rarely (if ever) proceed directly from their post-graduate schooling to a  
4 seat on a corporate board. Grounds's analysis of the educational and other backgrounds of recently  
5 appointed female board members in California is not responsive to this concern since it seems entirely  
6 plausible that the firms which add women directors quickest after SB826 went into effect (or even in  
7 anticipation of it going into effect) would have the easiest time finding candidates with suitable or close  
8 to suitable credentials and backgrounds, whereas subsequent firms will need to search for candidates  
9 from a smaller relevant population (because of the educational disparities noted above) or, alternatively,  
10 rely on women who are already sitting on boards which may lead to problems of being spread too thin.

11 127. Again, Grounds showing that this has not happened yet is not surprising given that early  
12 moving firms will have a larger group from which to choose. Without rigorous analysis of the available  
13 qualified pool, which is absent from Grounds' analysis,<sup>71</sup> it is not possible to state how difficult and/or  
14 costly it will be for subsequent firms to comply with SB826. Grounds also only provides univariate  
15 descriptions (e.g., what roles the appointees performed previously, what their educational background is,  
16 etc.) as if a single characteristic proves to be a sufficient (rather than merely one of many required  
17 characteristics) in terms of being qualified to be a director. Also, conditioning on characteristics of the  
18 firms that have appointed female directors would be necessary since different firms likely have different  
19 requirements, and it is most likely important to match details like industry backgrounds and the like  
20 when determining if compliance with SB826 is as easy as Grounds's analysis implies.

21 128. Further, there is no comparison of these characteristics to those of men appointed  
22 previously, which would be informative with respect to whether the available pool is adequate.  
23 Likewise, there is no analysis of the relative qualifications of women appointed to boards later in her  
24 sample as compared to those appointed earlier, which could give a preliminary glimpse into whether the  
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26 <sup>71</sup> Grounds claims otherwise when she states "Data shared in this declaration indicates that not  
27 only is this law expanding the pool of directors for corporate board service, but many of the women are  
28 first-time corporate board directors." However, nowhere in Grounds' report is there an analysis of the  
pool, only a description of the result of searches by early moving firms; appointees being first-time  
corporate board directors indicates nothing about the characteristics of the pool from which they were  
chosen.

1 pool of female candidates is sufficient for all firms to meet SB826's requirements.

2 129. On the whole, the analysis in the declaration made by Grounds is not sufficient to judge  
3 whether the newly appointed women to California corporate boards are better, worse, or the same (in  
4 terms of qualifications) as compared to the counterfactual in which sex diversity had continued to grow  
5 organically.

6 **Conclusion**

7 130. While the declarations in support of Secretary Padilla's motion claim that (1) women are  
8 underrepresented on firm boards, (2) this underrepresentation is the result of discrimination, and (3)  
9 appointing more women to boards would improve firm outcomes, none of these assertions is supported  
10 by the evidence provided.

11  
12 I declare under penalty of perjury under the laws of the State of California that the foregoing is  
13 true and correct.

14 Executed on September 7, 2021, at Penn Valley, Pennsylvania.

15  
16   
17 JONATHAN KLICK