The Effect of Gender Diversity Mandates on Firm Performance: Evidence from India

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- The amendment in Indian Companies Act 2013 (henceforth, ICA 2013) offers a suitable context to explore this research question

Research Hypotheses

- The inclusion of women on corporate boards improves firm performance
  - Gender diversity at the board level fosters diversity of thought and ideas, which can translate to more creative and innovative decision-making improving the firms performance and strategic prospects
  - A counterargument is that increased diversity could be dysfunctional as it may lead to greater conflicts, slower decision-making, and disrupt organizational agility in a rapidly changing fast-moving business environment
  - Therefore, the relationship between gender diversity and firm performance remains puzzling. I hypothesize a positive association because it seems an inverse-U-shaped relationship exists and the increased levels of board diversity should heighten firm performance in the initial period before the positive impact tapers off

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  3. Every Public Company having a minimum turnover of INR 3 billion or more
- It is important to note, that treatment assignment, even though exogenous, is non-random. It is based on pre-specified thresholds and thus correlated with firm size. I will revisit this fact later in the supplementary analysis section to account for non-compliance as well as differential rates of compliance across treated and untreated groups

Research Context

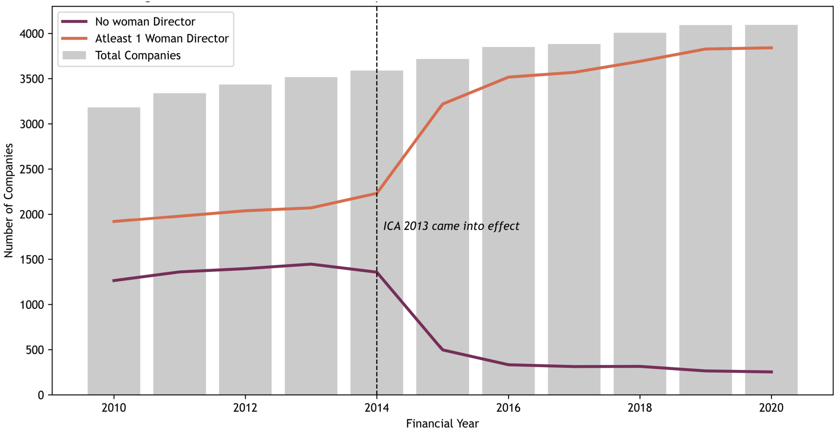

Figure 1: Number of CMIE Prowess Indexed Indian Companies with at least one woman director from FY 2010 to 2020

Data and Variables

- Data Source: Consistent with the prior research in the Indian context, I obtain financial information for listed Indian firms using CMIE Prowess[1]. I further segregate firms as family and non-family using the classificatory scheme of Thomas Schmidheiny Centre for Family Enterprise
  - This yields a strongly-balanced raw longitudinal dataset of 40,707 firm-year observations from 2011 to 2015[2]
- Dependent Variable: Return on Assets (π) defined as Profit after Tax net of the prior period and extraordinary transactions expressed as the percentage of the total assets of the company at the end of the financial year

[1] Bertrand et al., 2002; Mani & Moody, 2014; Mishra & Suar, 2010
[2] A total of 4,210 firms for each of the 5 financial years

Data and Variables

- Independent Variables:
  - Treat: Binary indicator based on whether the firm is targeted or not, i.e., required by the law to appoint woman director[3]
  - Post: For the baseline model, I consider a fixed time window of two years immediately before and after the law came into effect. FY 2014 is coded as 0 and FY 2015 as 1
  - Other Covariates: Not included in the baseline model but included in additional specifications for supplementary analysis
    1. Firm Size
    2. Firm Age
    3. Research Intensity
    4. Industry Code
    5. Family

[3] This is different from firm actually appointing women directors (i.e., $D \neq Z$)

Identification Strategy

1. Change in the expected value of π for control firms immediately before and after the year of treatment offers a credible counterfactual for the change in the expected value of π for treated firms.

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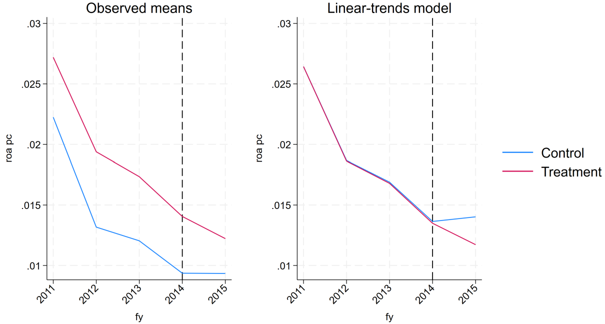
4. There is perfect compliance

$$Z_i = D_i$$

Tests for Parallel Trend Assumption

- Graphically, the parallel trend assumption holds in the pre-treatment period (2011-2014)



- I also test the validity of this assumption using 1, 2, and 3-year lagged values of π as placebo outcomes. The interaction effect is insignificant for all three models

Other Assumptions

- Just like parallel trend assumption, we cannot directly test for the remaining assumptions except for evaluating their plausibility
- Assumptions 2 - 4 are also quite strong. However, there is a way to relax each of these assumptions, one at a time, by estimating a different model with a slightly different estimand. I do this as part of my supplementary analysis section and then compare estimates against each other

Assumptions	Supplementary Analysis
No time-varying confounding	Matched DiD
No Carryover Effect	Staggered DiD
Perfect Compliance	Fuzzy DiD

TWFE Results

- Treatment = 4177; Control = 1295
- Firms and Year Fixed Effects
- Error term clustered at the firm-level

$$\pi_{it}(D) = \alpha_i + \gamma_t + \tau_{2fe} D + \epsilon_{it}$$

Table 1: Difference-in-differences regression

	Coefficient	std. err.	t	P>|t|
ATET				
DiD (1 vs 0)	-0.0018	0.0016	-1.15	0.250

Note: Std. err. adjusted for 5,472 clusters in co_code. Number of Observations = 10,295. Data type: Longitudinal. ATET estimate adjusted for panel effects and time effects.

Fuzzy DiD (de Chaisemartin and DHaultfoeuille, 2018)

- I relax the assumption that $Z_i = D_i$. In fact, $Z_i \neq D_i \implies$ treatment assignment $\neq$ treatment delivered
- Two-sided non-compliance: Some treated firms don't appoint women directors; some control firms end up appointing them
- I use fuzzy-DiD which employs the IV approach to calculate ATT among the treatment switchers (similar to LATE). Using fuzzy-DiD has a distinct set of identifying assumptions over and above assumptions already made
  - Assumes only one-sided non-compliance
  - I exclude a small number of cases where treated firms become untreated

Fuzzy DiD (de Chaisemartin and DHaultfoeuille, 2018)

For fy = 2014

	D = 0	D = 1	Sum
Z = 0	482	459	941
Z = 1	1745	2585	4330
Sum	2227	3044	5271

For fy = 2015

	D = 0	D = 1	Sum
Z = 0	345	600	945
Z = 1	702	3532	4234
Sum	1047	4132	5179

Fuzzy DiD Plug In Estimator

$$W_{\text{DID}} = \frac{E(Y_{11}) - E(Y_{10}) - \{E(Y_{01}) - E(Y_{00})\}}{E(D_{11}) - E(D_{10}) - \{E(D_{01}) - E(D_{00})\}}$$

- $E(Y_{11}) - E(Y_{10})$ Change in ROA for the treatment group between the two time periods (-0.076%)
- $E(Y_{01}) - E(Y_{00})$ Change in outcomes for the control group between the two time periods (0.31%)
- $E(D_{11}) - E(D_{10})$ Change in treatment uptake for the treatment group (23.72%)
- $E(D_{01}) - E(D_{00})$ Change in treatment uptake for the control group (14.72%)

Therefore, the plug-in estimator yields the LATE estimate of -0.0428. I also employ 2SLS to obtain standard error estimates.

Wald DiD 2SLS Estimation

Wald's DID Estimator is the coefficient of D in a 2SLS regression of Y on D with Z and T as included instruments and Z x T as the excluded instrument.

To satisfy exclusion restriction, I also consider size as an additional covariate because:

$$
\begin{tikzcd}
 & S \arrow[dl] \arrow[dr] & \\
Z \arrow[r] & D \arrow[r] & Y
\end{tikzcd}
$$

Figure 2: Directed Acyclic Graph (DAG) illustrating causal relationships among variables Treatment Assignment (Z), Treatment Receipt (D), ROA (Y), and Firm Size (S).

Wald DiD 2SLS Estimation

- Turns out Z x T is a strong instrument; relevance criterion satisfied (significant and F-statistic > 250)

Table 2: First-Order Regression with D as Outcome

	Coefficient	Std. err.	z	P>|z|
Z x Post	0.0927	0.0244	3.79	0.000
Z	0.0542	0.0183	2.95	0.003
Post	0.1430	0.0226	6.32	0.000
Size	0.0297	0.0021	13.81	0.000
Constant	0.3392	0.0194	17.41	0.000

Number of obs: 10,423 **F(4, 10418):** 288.03
Prob > chi2: 0.0000 **Root MSE:** .4422

Wald DiD 2SLS Estimation

Table 3: Reduced-form Regression with π as Outcome

	Coefficient	Std. err.	z	P>|z|
$\hat{D}$	-0.0341	0.0273	-1.25	0.211
Size	0.0067	0.0008	7.87	0.000
Z	-0.0118	0.0030	-3.87	0.000
Post	0.0070	0.0060	1.18	0.238
Constant	-0.0019	0.0084	-0.23	0.821

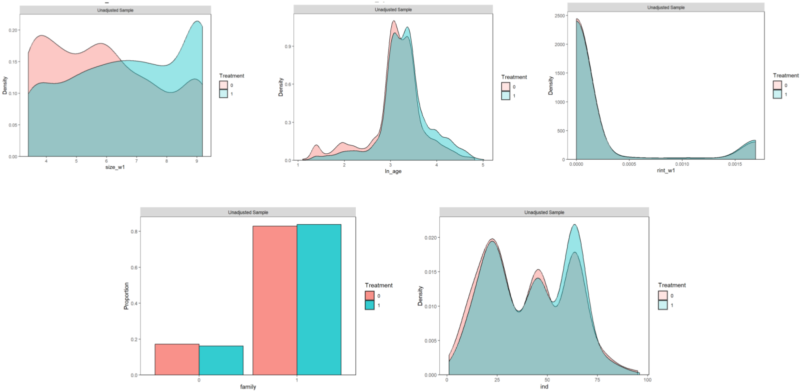
Number of obs: 10,295 **F(4, 10290):** 121.01
Prob $>$ chi2: 0.0000 **Root MSE:** .0515

- LATE offers a relatively more numerically substantive and statistically significant estimate
- Important to note if I don't include size as an additional control, 2SLS yields te exact estimate as plug-in estimator

Matched DiD

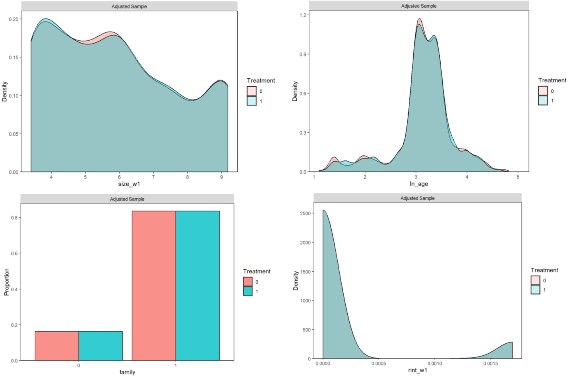
- Control firms are younger in age and smaller in size compared to treated firms. There is a possibility of time-varying confoundedness which may differentially impact the bigger and older firms differently as opposed to smaller and younger firms
- To alleviate some of these concerns, I implemented a matched DID using coarsened exact matching (CEM) on dimensions such as firm age, size, and family ownership. I also make the exact match on industry types
- The underlying intuition is that matching on pre-treatment outcomes partially balances unobserved confounders, which can subsequently mitigate some bias resulting from unobserved time-varying confounding

Covariate Balance: Before



Covariate Balance: After

- Treated: 471 Firms; Control: 471 Firms



Matched DiD

- Unfortunately, the parallel trend assumption does not seem to hold due to sharp divergences in the pre-treatment trends.

Table 4: Difference-in-differences regression

	Coefficient	std. err.	t	P>|t|
ATET				
did (1 vs 0)	0.00057	0.00310	0.19	0.853

Note: Std. err. adjusted for 942 clusters in co_code. Number of Observations = 1,882. Data type: Longitudinal. ATET estimate adjusted for panel effects and time effects.

Staggered DiD (Callaway and SantAnna, 2021)

- Assuming no treatment reversals
- Comparison Group: Never Treated
  - The conditional parallel trend assumption holds after conditioning on industry and state (both time-invariant, so no post-treatment bias) at 10% LOS
  - For not yet treated as comparison group, conditional parallel trend assumption not satisfied
- Treatment Effect: Based on Event Study and Dynamic Aggregation

Table 5: ATT's based on event-study/dynamic aggregation:

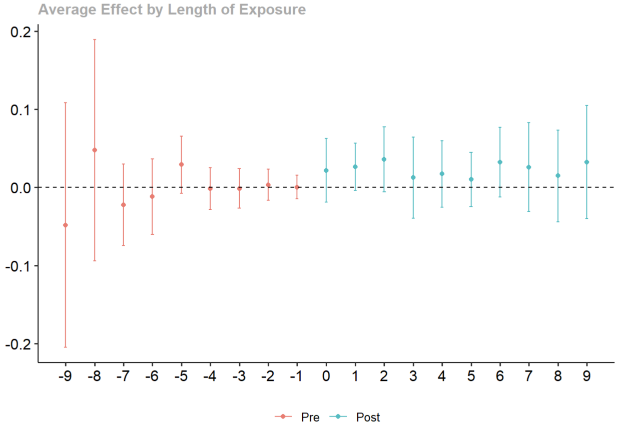
	Coefficient	**Std. Err.**	**t**	**P>|t|**
ATET				
DiD (1 vs 0)	0.0231	0.0115	2.01	0.0445

Staggered DiD (Callaway and SantAnna, 2021)

Table 6: Dynamic Effects

Event time	Estimate	Std. Error	95% Simult. Conf. Band		Sig.
-9	-0.0446	0.0551	-0.1921	0.1029	
-8	0.0477	0.0516	-0.0904	0.1857	
-7	-0.0202	0.0197	-0.0729	0.0325	
-6	-0.0119	0.0192	-0.0632	0.0395	
-5	0.0299	0.0136	-0.0066	0.0663	**
-4	-0.0028	0.0110	-0.0321	0.0265	
-3	-0.0012	0.0106	-0.0296	0.0273	
-2	0.0021	0.0071	-0.0170	0.0212	
-1	0.0016	0.0068	-0.0166	0.0197	
0	0.0203	0.0150	-0.0197	0.0603	
1	0.0252	0.0110	-0.0043	0.0547	**
2	0.0356	0.0173	-0.0107	0.0819	**
3	0.0125	0.0172	-0.0336	0.0586	
4	0.0185	0.0157	-0.0234	0.0604	
5	0.0133	0.0151	-0.0271	0.0537	
6	0.0436	0.0197	-0.0090	0.0962	**
7	0.0395	0.0229	-0.0217	0.1006	
8	0.0219	0.0233	-0.0404	0.0841	
9	0.0333	0.0252	-0.0342	0.1008	

Staggered DiD (Callaway and SantAnna, 2021)



- The results are non-significant throughout for non-staggered DiD with a two-year window. It is positive and significant at a 5% level of significance if we consider staggered DiD, suggesting one-shot treatment effect may not be very telling of the positive impact that gender diversity creates.
- Moreover, the non-significant results for non-staggered DiD could mean two things – *first*, there is no significant causal linkage between board diversity and immediate or short-term firm performance; and *second*, the underlying data is scarce leading to noisy estimates. Both seem to be partly true.
- The number of never-treated firms is exceptionally small. Moreover, as almost all of these firms are unlisted, they are relatively much smaller in size and age.

- Theoretically speaking, firm performance is a multi-dimensional construct. My analysis uses ROA, an accounting-based measure to assess firm performance. However, the use of ROA could be limiting for being a backward-looking accounting-based computational artifact.
- Another important concern that could be raised is about the generalizability of my findings. I acknowledge while this focus allows for a detailed analysis of a specific legislative change, it might not apply to other regulatory environments or cultural contexts.

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