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

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May 9, 2024

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2. Research Hypotheses
3. Research Design
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Research Motivation

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- In the context of corporate boards, there is ample research that analyzes the impact of gender balance on firm performance. But a major chunk of it happens to be correlational with mixed findings and therefore empirically indeterminate
- Therefore, both theoretically and empirically, there is an opportunity to systematically tease out the causal impact of gender quotas on firm performance

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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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  1. Every Listed Company
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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 is arbitrary but non-random as it is based on pre-specified thresholds and thus significantly correlated with firm size. We shall revisit this fact later for additional analysis to mitigate against bias arising due to non-compliance as well as differential rates of compliance across treated and untreated groups

Research Context

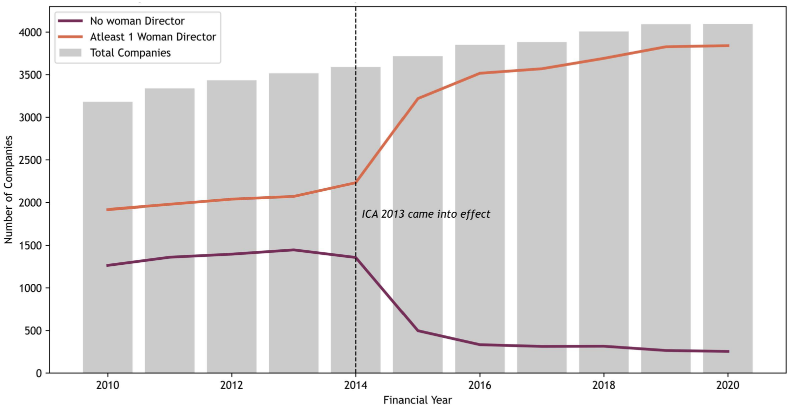

Figure 1: Number of CMIE Prowess Indexed Indian Companies with at least one woman director from FY 2010 to 2020

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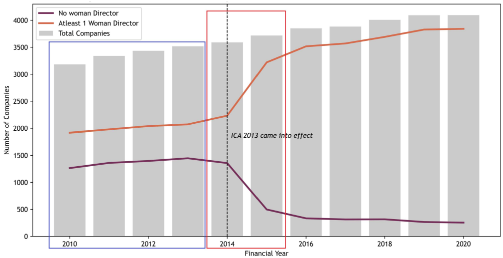
Data and Variables

- 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 21,070 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 appoints women director or not[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: To be specified separately for each model

[3] This is different from whether the firm is required by law to appoint women directors (i.e., $D \neq Z$)

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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. This assumption is commonly known as the parallel trends assumption

$$\mathbb{E}(\pi_{post,treated}) - \mathbb{E}(\pi_{pre,treated}) = \mathbb{E}(\pi_{post,control}) - \mathbb{E}(\pi_{pre,control})$$

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2. There are no compliance issues, i.e., I assume $D_i = Z_i$. That is, I categorize firms as treated based on treatment uptake and not treatment assignment.
3. Treatment effect is homogeneous and contemporaneous
4. There is no unobserved time-varying confounding

Tests for Assumptions

- To evaluate the plausibility of the parallel trend assumption, I test for the equality of means in ROA for treated and untreated firms in the pre-treatment time period, i.e., from 2011 to 2014, using the e placebo regressions
- Assumptions 2 - 4 are surely quite restrictive. I relax each of these assumptions, one at a time, by estimating a different model with a slightly different estimand to test the robustness of my baseline estimates.

Assumptions	Robustness Checks
2	Fuzzy DiD
3	Staggered DiD with dynamic treatment effects
4	Matched DiD

Basic Descriptives

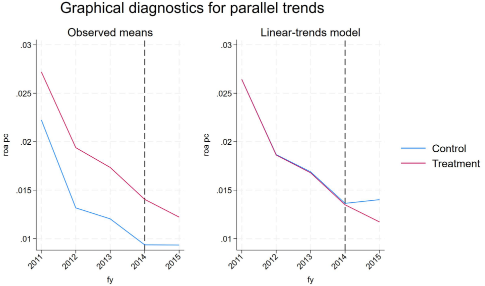
- Treated: 3711 firms; Control: 497 firms
- Covariate Balance for Treated vs Untreated

Variable	Type	SMD	Mean Balance	Var Ratio	Var Balance
π	Contin.	0.0810	Balanced, <0.1	1.0306	Balanced, <2
DE Ratio	Contin.	0.0626	Balanced, <0.1	1.0733	Balanced, <2
Age	Contin.	0.3072	Not Balanced, >0.1	0.8344	Balanced, <2
Size	Contin.	0.3969	Not Balanced, >0.1	1.0473	Balanced, <2
Research Intensity	Contin.	0.0402	Balanced, <0.1	1.0836	Balanced, <2
Family	Binary	0.0261	Balanced, <0.1	.	.

Table 1: Summary of balance measures

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

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TWFE Results

- Firms and Year Fixed Effects
- Error term clustered at the firm-level

Table 2: Difference-in-differences regression

	Coefficient	std. err.	t	P>|t|
ATET				
DiD (1 vs 0)	-0.00190	0.00234	-0.81	0.418

Note: Std. err. adjusted for 4,208 clusters in co_code. Number of Observations = 8,382. Data type: Longitudinal. ATET estimate adjusted for panel effects and time effects.

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Fuzzy DiD (de Chaisemartin and DHaultfoeuille, 2018)

- I relax the assumption that $D_i = Z_i$. In fact, $D_i \neq Z_i \implies$ 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

Fuzzy DiD (de Chaisemartin and DHaultfoeuille, 2018)

For fy = 2014

	D = 0	D = 1	Sum
Z = 0	286	287	573
Z = 1	1413	2150	3563
Sum	1699	2437	4136

For fy = 2015

	D = 0	D = 1	Sum
Z = 0	134	439	573
Z = 1	475	3088	3563
Sum	609	3527	4136

Fuzzy DiD (de Chaisemartin and DHaultfoeuille, 2018)

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.

- Turns out Z x T as an instrument lacks relevance (non-significant even though F-value > 10)
- This is because the Wald DiD uses the difference in the differences in treatment adoption rates for treated and control subsamples – which is close to 0 – making estimates highly unreliable

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

- Change in treatment take-up rates: Treated $=$ Control $= 26\%$
- Inflated LATE

Fuzzy DiD (de Chaisemartin and DHaultfoeuille, 2018)

Table 3: Instrumental variables 2SLS regression

	Coefficient	Std. err.	z	P>|z|
D	0.171	2.580	0.07	0.947
Z	-0.019	0.265	-0.07	0.943
Post	-0.046	0.677	-0.07	0.945
Constant	-0.071	1.297	-0.06	0.956

Number of obs: 8228 **Wald chi2(3):** 0.86
Prob > chi2: 0.8342 **Root MSE:** .09077

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 – firm age, size, and family ownership. I also make the exact match on 2-digit industry codes

  Important to note that I match firms on pre-treatment values to safeguard estimates against post-treatment bias
- 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

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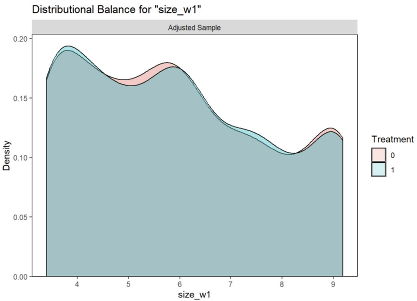
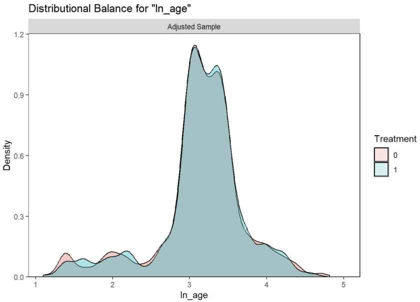
Matched DiD

- Treated: 471 Firms; Control: 471 Firms





Matched DiD

- Unfortunately, the parallel trend assumption does not hold strongly during the pre-treatment horizon.

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.

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Staggered DiD (Callaway and SantAnna, 2021)

- Comparison Group: Never Treated
- Treatment Effect: Based on Event Study and Dynamic Aggregation

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

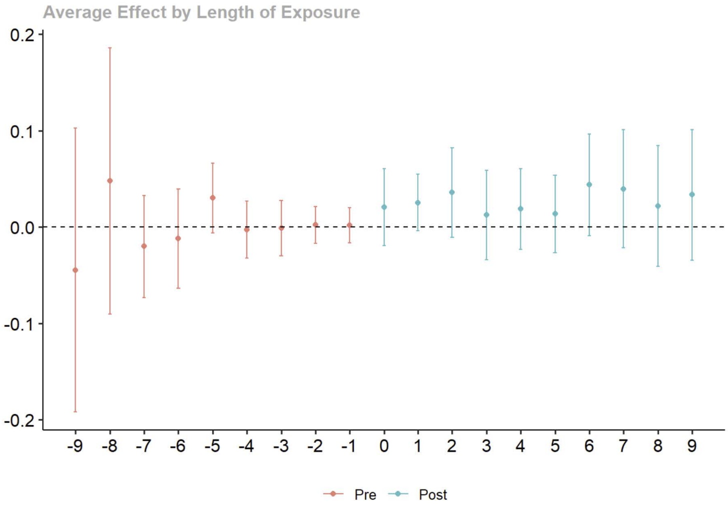
	Coefficient	**std. err.**	**t**	**P>|t|**
ATET				
did (1 vs 0)	0.0264	0.0116	2.27	0.0232

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)



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- The magnitude and direction of causal association fluctuate depending on the tweaks made to the underlying DiD framework
- The results are non-significant throughout for non-staggered DiD with a two-year window. It is positive and significant at 5% LOS if we consider staggered DiD.
- Non-significant results for non-staggered DiD could mean two things – first, there is no significant causal linkage between board diversity and immediate firm performance; second - the underlying data is scarce leading to noisy estimates
  - The gender of board directors is inferred using genderize.io API. The API does a good job at classifying English names, so there is a possibility of Hindi names being misclassified
  - Overwhelmingly small control sample. As these are unlisted firms much smaller in size and age, there are no other alternate market-based performance benchmarks other than accounting-based performance variables that can be used as DV for additional robustness checks

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