

Supplementary Materials

Corporate Strategic Orientation and ESG Performance: Financial Distress and Boundary Conditions in China's Evolving Green-Policy Environment

Shiru Sun, Malisah Latip and Ismawati Sharkawi

This file contains Tables S1–S6, Texts S1–S6, and Figures S1–S5 cited in the main text; Tables S1–S4 precede the texts, Table S5 accompanies Text S5 and Table S6 accompanies Text S6, and Figures S1–S5 close the file. All coefficients, standard errors, test statistics, and confidence intervals are reported exactly as produced by the estimation software; no values have been re-computed or re-rounded for this file; derived quantities (standardized coefficients, TOST/MDE values, and Benjamini–Hochberg q-values) are labeled as such.

Table S1. ESG sub-dimension regressions (environmental, social, and governance).

Corresponds to Section 4.6 of the main text. All three models are two-way fixed effects (firm and year) with firm-level clustered robust standard errors.

Note on sample coverage: the numbers of non-missing observations for the three ESG sub-dimensions originally differ (12,960 / 13,024 / 13,034), with the social and governance dimensions exceeding the main sample of 12,998 (pillar scores carry their own non-missing patterns in the source extract). To keep the same sample as the rest of the paper, all three models below are restricted to the main sample with non-missing total ESG scores. Coefficients change only in the third decimal place before and after the restriction, and all significance conclusions are unchanged. A common-sample, standardized comparison across the three dimensions (which makes their relative strength comparable) is reported in Section S5.5.

Dependent variable	Profit	Growth	N	Groups	R ² (within)
Bloomberg E	0.539** (0.212), t = 2.55, p = 0.011	−0.286 (0.304), t = −0.94, p = 0.348	12,960	1,391	0.4259
Bloomberg S	0.904*** (0.110), t = 8.25, p < 0.001	−0.403** (0.186), t = −2.16, p = 0.031	12,960	1,391	0.3719
Bloomberg G	3.558*** (0.148), t = 24.02, p < 0.001	−3.078*** (0.223), t = −13.77, p < 0.001	12,960	1,391	0.7498

Note: firm-level clustered robust standard errors in parentheses. *** p < 0.01, ** p < 0.05. Controls and year dummies are included but not reported. All three models are estimated on the intersection of the three sub-dimension coverage sets (N = 12,960; coefficients change only in the third decimal place relative to the per-dimension samples). Bloomberg E, S, and G denote the environmental, social, and governance pillar scores.

The effect is concentrated mainly in the governance and social dimensions rather than spread evenly across the three sub-dimensions: the governance dimension has the largest effect, social next, and the environmental dimension is weaker, with Profit significant only at the 5% level and Growth insignificant (p = 0.348). This matches the mechanism account that strategic orientation acts on organizational agendas and resource allocation, with environmental performance more constrained by industry technology, but it implies caution in extrapolating the conclusions to purely environmental performance.

Table S2. Test with one-period-lagged explanatory variables.

Corresponds to item (2) of Section 4.6 of the main text. Current and one-period-lagged strategic orientation variables are both included.

Variable	Coefficient	Robust SE	t	p	95% CI
Profit (current)	1.781	0.1120	15.90	<0.001	[1.561, 2.000]
Growth (current)	−1.182	0.1844	−6.41	<0.001	[−1.544, −0.820]
L1.Profit	−0.027	0.1109	−0.25	0.806	[−0.245, 0.190]
L1.Growth	0.247	0.1836	1.34	0.179	[−0.113, 0.607]
BM	−0.227	0.1023	−2.22	0.027	[−0.427, −0.026]
Lev	−2.112	0.9629	−2.19	0.028	[−4.001, −0.223]
SOE	−0.531	0.5380	−0.99	0.324	[−1.587, 0.524]
CashRatio	0.152	0.1189	1.28	0.201	[−0.081, 0.385]
Top1	−0.886	1.5211	−0.58	0.560	[−3.870, 2.098]

Model information: N = 11,117; Groups = 1,357; R² (within) = 0.6841; R² (between) = 0.2716; R² (overall) = 0.4239; F(21, 1356) = 286.01. Year dummies are included but not reported.

Current-period coefficients are significant while both lagged terms are insignificant, a pattern consistent with a contemporaneous association; this specification cannot prove temporal ordering, and we do not present it as doing so.

Table S3. Fixed-effects estimation with a lagged dependent variable.

Corresponds to item (3) of Section 4.6 of the main text.

This is a fixed-effects specification subject to Nickell bias in short panels, so it can only be regarded as a control for ESG inertia and does not constitute a correction for endogeneity. The full attempt and diagnostics for dynamic panels are in Text S4.

Variable	Coefficient	Robust SE	t	p	95% CI
L1.ESG	0.663	0.0130	51.18	<0.001	[0.638, 0.689]
Profit	1.763	0.0828	21.29	<0.001	[1.600, 1.925]
Growth	−1.283	0.1421	−9.03	<0.001	[−1.562, −1.005]
BM	−0.128	0.0596	−2.15	0.032	[−0.245, −0.011]
Lev	0.453	0.5603	0.81	0.419	[−0.646, 1.552]
SOE	−0.014	0.3192	−0.04	0.965	[−0.640, 0.612]
CashRatio	0.162	0.0783	2.07	0.039	[0.008, 0.315]
Top1	−0.790	0.8972	−0.88	0.379	[−2.550, 0.970]
Constant	7.243	0.6319	11.46	<0.001	[6.003, 8.483]

Model information: N = 11,108; Groups = 1,353; R² (within) = 0.8023; R² (between) = 0.8951; R² (overall) = 0.8309; F(20, 1352) = 1042.18. Year dummies are included but not reported.

ESG exhibits strong inertia (the L1 coefficient is 0.663), but the core coefficients remain robust once this inertia is controlled (Profit = 1.763, Growth = −1.283), of the same order as the baseline estimates (1.773, −1.110).

Table S4. Full event-study coefficients for the low-carbon city pilot.

Corresponds to Section 4.3 of the main text. The baseline period is one period before implementation.

Variable	Coefficient	Robust SE	t	p	95% CI
Pre3	0.002	0.4588	0.00	0.996	[−0.898, 0.902]
Pre2	0.254	0.3390	0.75	0.453	[−0.411, 0.920]
Current	0.103	0.2740	0.38	0.707	[−0.435, 0.641]
Post1	0.009	0.2314	0.04	0.970	[−0.445, 0.463]
Post2	0.269	0.2435	1.11	0.269	[−0.209, 0.747]
BM	−0.118	0.1133	−1.04	0.298	[−0.340, 0.104]

Variable	Coefficient	Robust SE	t	p	95% CI
Lev	−4.034***	1.0947	−3.69	<0.001	[−6.182, −1.886]
SOE	−0.274	0.6480	−0.42	0.672	[−1.546, 0.997]
CashRatio	0.157	0.1299	1.21	0.227	[−0.098, 0.412]
Top1	0.899	1.7200	0.52	0.601	[−2.477, 4.274]

Model information: N = 9,168; Groups = 1,003; R² (within) = 0.6815; R² (between) = 0.2616; R² (overall) = 0.4275; rho = 0.6719; F(23, 1002) = 177.29. Year dummies are included but not reported. Significance: *** p < 0.01.

Formal test of parallel trends: the joint test that the coefficients of Pre3 and Pre2 are both zero gives F(2, 1002) = 0.29, Prob > F = 0.752.

The coefficients in the two pre-implementation periods are insignificant, as is their joint test, so the parallel-trends assumption holds; the implementation period and the two following periods are also insignificant, indicating no detectable average treatment effect.

Text S1. Parallel Trends, the Precision Qualification of the Growth-Side Null Results, and the Direction Difference from Prior Literature

S1.1. Parallel trends

Table S4 shows that the coefficients of the third (0.002, p = 0.996) and second (0.254, p = 0.453) periods before implementation are insignificant, as is their joint test (F = 0.29, p = 0.752), indicating no systematic difference in pre-treatment ESG trends between treatment and control groups; the parallel-trends assumption required by the DID design is not rejected in this test. The coefficients in the implementation period and the two following periods are also insignificant, indicating no detectable average treatment effect of the low-carbon city pilot on corporate ESG performance.

S1.2. Average and conditional effects of the policy

The DID coefficient in Model 1 of Table 3 of the main text is −0.078 (p = 0.803), insignificant, consistent with the event study. Model 2, however, shows a pattern masked by the average effect: DID × Profit is 0.390 (p = 0.063), positive at the 10% level, while DID × Growth is 0.053 (p = 0.873), insignificant. The ESG effect of the low-carbon policy thus emerges only among profit-oriented firms, which is a within-path contrast, not a significant between-path difference: for firms already oriented to the long term the policy may function as a strategic signal, while for growth-oriented firms the same policy produces no detectable behavioral change. H4b is nominally supported at the 10% level and H4a is indeterminate (underpowered).

Evidential strength: the significance of DID × Profit is only at the 10% level. We therefore do not let it carry weight alone and read it together with the environmental subsidy result (Profit × GES = 2.404, p = 0.046, 5% level) as the pattern that both payoff-reshaping instruments work on the profit-oriented path (the subsidy interaction does not survive lagged moderators; S6.9).

S1.3. Precision qualification of the growth-side null

DID × Growth is insignificant (0.053, p = 0.873), but a large p-value by itself cannot support a substantive no-effect conclusion; a null result constitutes evidence only when the estimated interval is narrow enough to rule out substantively meaningful effect sizes.

Given the standard errors reported in Table 3 of the main text, the 95% CI of this coefficient is [−0.600, +0.706], with the endpoint farthest from zero reaching 63.6% of the baseline total effect (the Growth coefficient of −1.110 in Table 2, step 1). Our data therefore cannot exclude the possibility of a moderating effect large enough to offset over sixty percent of the ESG suppression of growth-oriented firms. H4a should accordingly be stated as indeterminate (underpowered) rather than as the policy having no effect on growth-oriented firms.

Furthermore, we directly test the difference between the two interaction coefficients: the difference is 0.337 with $F = 0.90$, $p = 0.343$, below conventional significance. We therefore do not claim a statistically significant difference in the low-carbon policy's moderating effect between the two paths; our claims rest only on the respective interval precision.

S1.4. The direction difference from prior literature and its explanation

An apparent contradiction must be addressed head-on: existing studies of the Chinese setting generally find that green policies such as the low-carbon city pilot significantly raise corporate ESG performance (e.g., [25]), whereas our DID main effect is insignificant.

This is not a conflict of results but a difference in analytical granularity. Prior studies estimate the full-sample average treatment effect, whereas Model 2 of Table 3 in the main text shows that the effect is highly heterogeneous across firms, concentrated among profit-oriented firms ($DID \times Profit = 0.390$, $p = 0.063$) and undetected among growth-oriented firms ($p = 0.873$); pooling the two types dilutes the positive effect, and the average effect becomes insignificant.

We do not deny that the low-carbon policy works; the observed pattern is consistent with an effectiveness that varies with firms' relative profit-growth positions, a conditionality that prior studies could not identify because strategic orientation was not included. This also suggests that evaluating green policies by average treatment effects may systematically understate their effect on some firm groups and overstate it on others.

Text S2. Identification Qualifications of the Low-Carbon Pilot DID and the Goodman-Bacon Bias

S2.1. Three identification qualifications

First, the treatment group is defined by the registered city, leaving firms' actual emission locations in cross-region operations unaccounted for. Second, the event-study window spans three periods before to two periods after implementation; if the policy effect materializes over longer horizons, our estimates may understate its long-run impact. Third, because the pilots were rolled out in four batches (2010, 2012, 2014, and 2017), our multi-period DID uses the two-way fixed-effects estimator. Reference [40] shows that under staggered adoption this estimator can be decomposed into a weighted average of 2×2 comparisons, some of which are 'bad comparisons' using earlier-treated groups as controls; reference [41] proposes heterogeneity-robust estimators that do not rely on this weighting. The Callaway–Sant'Anna re-estimation in Text S6.1 (and Section 4.3 of the main text) implements exactly this design, and its cohort-robust results are the primary policy estimates.

S2.2. Two partial arguments (not substitutes for the tests)

First, dynamic treatment effects are weak in our sample. One mechanism generating the bias is time-varying treatment effects: systematic bias from comparisons with earlier-treated groups arises when policy effects change period by period after implementation. Our event study shows insignificant and modest coefficients in the implementation period (0.103), the following period (0.009), and two periods after (0.269), with no evident dynamic treatment effect, so this driver is weak in our sample. That said, the severity of the bias depends not only on the time heterogeneity of treatment effects but also on the timing distribution of the pilot batches and the sample-weight structure across batches, which we cannot assess from the existing estimates; these arguments can therefore reduce, but not exclude, the possibility of bias.

Second, our core inference is about interactions rather than the average treatment effect. If the two firm types are similarly distributed across the four pilot batches, the

common cohort-weighting bias largely cancels when differences are taken; but the completeness of this cancellation depends on how similar the timing distributions are; rather than relying on this cancellation, the policy estimates are re-estimated with the cohort-robust estimator (S2.3 and Text S6.1).

S2.3. Conclusion

These two arguments rest on the existing estimates and do not substitute for the decomposition in [40] or the estimators in [41]. This gap is closed rather than deferred: we re-estimate the policy analysis with the Callaway–Sant’Anna estimator (never-treated and not-yet-treated controls), report cohort sizes and cohort-specific effects, cluster at the city level with a wild-cluster bootstrap complement, and verify the coding of the 983 missing-pilot-status observations; the city-pilot crosswalk is provided in the replication package. The cohort-robust average effect is indistinguishable from zero ($ATT = 0.083$, $p = 0.842$), and the pre-trend average is -0.010 ($p = 0.945$). These cohort-robust estimates are the primary results (Section 4.3); the TWFE figures are the benchmark.

Text S3. The Ordering of the Mediation Analysis and Its Rationale

The analysis orders the three variables as strategic choice, then financial distress risk, then ESG; this section explains why that ordering is used. Two competing paths exist in the literature: from strategic choice through financial distress risk to ESG (adopted here), and from ESG investment to lower financial distress risk [16,17].

We choose the former for three reasons: (1) temporal precedence, as strategic orientation typically precedes resource-allocation decisions [18]; (2) the stability of stock measures, since financial distress risk is an annual stock variable of the financial structure that is relatively stable in the short run and can hardly be moved substantially within a year by ESG investment; and (3) asymmetric lags, as the effect of ESG investment on financial distress risk typically requires three to five years, whereas the shaping of the financial structure by strategic orientation can appear within shorter periods.

The two directions are not mutually exclusive. We focus on the short-run resource-constraint logic; possible reverse feedback over medium to long horizons is treated as a boundary condition of the framework in Section 5 of the main text.

Our treatment of endogeneity relies on two-way (firm and year) fixed effects, the lagged-regressor design (Table S2), and the low-carbon city pilot, whose timing is set at the city level and not by the firm, as a policy shock of uncertain exogeneity. The attempt at dynamic-panel instrumental variables and the reason it fails are in Text S4.

Text S4. System and Difference GMM: Attempts, Diagnostics, and Instrument-Validity Evidence

S4.1. Why dynamic-panel GMM was attempted

Table S3 above uses a fixed-effects specification with a lagged dependent variable, which is subject to Nickell bias in short panels: after within-firm demeaning, the error term correlates with the lagged dependent variable, biasing the lag coefficient downward and contaminating the other coefficients. It can only serve as a control for ESG inertia, not a correction for endogeneity. Dynamic-panel GMM, which uses lags as internal instruments, is designed precisely for this bias, so we attempted it.

S4.2. Specification

Three specifications, all two-step with Windmeijer finite-sample correction and collapsed instrument matrices:

Specification	Description	Instruments	N	Firms
1	System GMM, lag(2 .)	121	11,108	1,353

Specification n	Description	Instruments	N	Firms
2	Difference GMM (noleveleq), lag(2 .)	112	9,666	1,310
3	System GMM, lag(2 3) (restricted lag depth)	37	11,108	1,353

The lagged dependent variable and the two strategic orientation variables are treated as endogenous, the controls as predetermined, and the year dummies as strictly exogenous instruments. The instrument counts are all below the number of firms, so this is not a case of instruments exceeding the cross-sectional dimension.

S4.3. Four diagnostics jointly indicate instrument invalidity

(1) The overidentification tests are rejected. The Hansen J statistics reject instrument exogeneity at the 1% level in all three specifications:

Specification	Hansen J	p	Sargan	p
1	$\chi^2(100) = 291.9$	<0.001	$\chi^2(100) = 408.9$	<0.001
2	$\chi^2(92) = 151.9$	<0.001	$\chi^2(92) = 265.3$	<0.001
3	$\chi^2(16) = 167.3$	<0.001	$\chi^2(16) = 238.0$	<0.001

The Difference-in-Hansen tests for instrument subsets are also rejected throughout ($p \leq 0.011$). All three specifications report that the two-step moment-condition covariance matrix is singular and a generalized inverse is used instead.

(2) The coefficients are unstable across specifications:

Coefficient	Specification 1	Specification 2	Specification 3
L1.ESG	0.829***	0.243***	0.848***
Profit	+0.779 ($p = 0.269$)	+0.645 ($p = 0.253$)	−2.593 ($p = 0.013$)
Growth	+0.433 ($p = 0.739$)	−1.592 ($p = 0.124$)	−2.906 ($p = 0.159$)

The profit-oriented estimate crosses zero and is significantly negative in Specification 3; growth orientation flips positive in Specification 1. With instrument exogeneity rejected, these point estimates are not interpretable.

(3) The lag coefficient falls outside the plausible range. A consistent estimate of the lag should lie between the (downward-biased) fixed-effects estimate and the (upward-biased) pooled OLS estimate, here [0.663, 0.989]. The system-GMM values of 0.829 and 0.848 fall inside, but the difference-GMM value of 0.243 is far below the lower bound, the typical sign that instruments lack identifying power in the differenced equation.

(4) AR tests. AR(1) is significant in all three specifications, as expected; AR(2) gives $p = 0.082$, 0.836, and 0.150, with Specification 1 marginally failing at 10%.

S4.4. The structural reason for the failure

The instrument failure here is not a matter of specification choice but a necessary consequence of the data structure:

Variable	Share of firms never changing	Average switches (mean 9.2 periods)	Within-firm share variance
Growth	57.4%	1.05	39.3%
Profit	15.6%	2.87	70.8%

Note: shares are computed under the baseline growth-orientation classification used in the models; under the continuous-benchmark reconstruction the growth share is 57.7% (Text S6.3).

Growth orientation never changes for more than half the firms, averaging roughly one switch per firm, while ESG scores are highly persistent (within-firm first autocorrelation 0.84; raw correlation 0.90). When the dependent variable is close to a unit root and the key regressors barely vary within firms, lagged levels have no identifying

power for the differenced equation, a known result in the literature, and our sample falls squarely in this case.

S4.5. Conclusion and treatment

We do not use the GMM point estimates as evidence and do not treat GMM as a test of the main results. With instrument exogeneity rejected, estimates under this specification are not evidence; the significantly negative profit-oriented estimate in Specification 3 likewise cannot be read as a refutation of the main conclusions.

Dynamic endogeneity thus remains a substantive limitation. Our mitigation relies on firm and year two-way fixed effects, the lagged-regressor design (Table S2), and the low-carbon city pilot analysis (multi-period DID and event study, including the joint pre-period test $F = 0.29$, $p = 0.752$), rather than dynamic-panel instrumental variables. Future work in this direction should look for external instruments for strategic orientation or exploit exogenous shocks to strategic transition, rather than relying on internal lag instruments.

Text S5. Full Outputs of the Diagnostics and Robustness Checks, Sample Handling, and Standard-Error Sensitivity

Table S5. Correlation Matrix and Tiered Variance Inflation Factor Diagnostics

S5.1. Correlation matrix

Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
(1) ESG	1.000												
(2) Profit	.104*	1.000											
(3) Growth	−.072*	−.232*	1.000										
(4) O-Score	−.120*	−.282*	.155*	1.000									
(5) KZ	−.065*	−.205*	.118*	.762*	1.000								
(6) Media	.086*	.006	−.005	−.072*	−.067*	1.000							
(7) GES	−.051*	−.008	.006	.057*	.028*	.006	1.000						
(8) Size	.433*	−.022*	.040*	.151*	.181*	.382*	−.032*	1.000					
(9) BM	.171*	−.088*	.078*	.375*	.386*	.069*	.005	.631*	1.000				
(10) Lev	.019*	−.210*	.136*	.817*	.665*	.126*	.027*	.498*	.582*	1.000			
(11) SOE	.017	.005	.002	.146*	.176*	−.013	.054*	.249*	.272*	.206*	1.000		
(12) CashRatio	−.053*	.109*	−.060*	−.588*	−.473*	−.026*	−.031*	−.297*	−.277*	−.588*	−.116*	1.000	
(13) Top1	−.002	.081*	−.023*	−.028*	−.047*	.064*	.003	.212*	.122*	.081*	.315*	−.042*	1.000

Note: Pearson correlations on the regression sample with non-missing ESG scores, pairwise complete, all variables winsorized. * $p < 0.05$. Column and row numbers refer to the same variables.

S5.2. Variance inflation factors: three diagnostic tiers

All three tiers are computed directly from auxiliary OLS regressions (VIFs reflect only linear correlation among the regressors and are unrelated to the fixed-effects specification).

Specification	Max VIF	Mean VIF
Baseline (two orientation variables + five controls)	2.24 (Lev)	1.41
With O-Score added	4.34 (Lev) / 3.56 (O-Score)	1.96
All five moderators included simultaneously (strictest)	5.63 (O-Score) / 5.55 (Lev)	2.25

All three tiers are below the conventional threshold of 10, so coefficient estimates are not materially affected by multicollinearity.

S5.3. Two high correlations worth noting

The O-Score correlates with the KZ index at 0.762 and with leverage at 0.817. Both are expected by construction, since the Ohlson O-Score and the Kaplan-Zingales index both take leverage as a core input. The diagnostics in S5.2 show that these correlations pose no linear-collinearity threat; they do not address the mechanical overlap between the earnings-related components of the O-Score and the ROA-based orientation measure. A distress index constructed without earnings-related components (leverage and liquidity only) removes this overlap: its correlation with ROA is -0.37 , against -0.72 for the O-Score, and under this measure the orientation-to-distress link itself is no longer significant (a-path $p = 0.89$; Text S6.16).

S5.4. One bivariate sign opposite to the hypothesized direction

A brief note on one sign in the matrix above: the bivariate correlation between GES and ESG is negative ($r = -0.051$, $p < 0.05$), opposite to the interaction prediction of H5b. The full discussion is in Section 4.4 of the main text, which shows that the fixed-effects main effect of GES is also negative and marginally significant (-1.998 , $p = 0.087$, Table 4, Model 1) and explains why a negative main effect can coexist with the positive and significant interaction ($\text{Profit} \times \text{GES} = 2.404$, $p = 0.046$).

S5.5. Full ESG sub-dimension results (Table S1)

The sub-dimension regressions are reported in Table S1 above. Because the three sub-dimensions have different non-missing counts, we add a common-sample, standardized comparison ($N = 12,960$ in all three): per standard deviation of the outcome, the Profit coefficients are 0.037 ($p = 0.011$, environment), 0.111 ($p < 0.001$, social), and 0.256 ($p < 0.001$, governance), and the Growth coefficients are -0.020 (n.s.), -0.050 ($p = 0.031$), and -0.221 ($p < 0.001$) respectively. The concentration in governance and social holds under the comparable metric, while the environmental link is small but significant for profit-oriented firms; the alternative-rating replication in Section 4.6 further shows the environmental association is positive and significant under CNRDS ratings. The effect is thus concentrated mainly in governance and social dimensions, implying caution in extrapolating to purely environmental performance.

S5.6. Sample handling

The non-missing counts of the three ESG sub-dimensions differ (12,960, 13,024, 13,034), with social and governance exceeding the main sample of 12,998 because some of the 41 firm-years without a composite score nonetheless carry pillar scores in the source extract (the composite gap is at source, not a processing loss), and the 2022 dip (594 firm-years) reflects a coverage contraction in the source extract. To keep the same sample as the rest of the paper, the three models of Table S1 are restricted to the main sample with non-missing total ESG scores, giving 12,960 observations in each model (the intersection sample reported in Table S1) (the environmental dimension's missing pattern is naturally more extensive than the total's, so 12,960 is already its full set of valid observations). Coefficients change only in the third decimal place, with all significance conclusions unchanged.

S5.7. Lagged explanatory variables (Table S2)

With current and one-period-lagged orientation variables both included, the current coefficients are significant ($\text{Profit} = 1.781^{***}$, $\text{Growth} = -1.182^{***}$) and the lags insignificant (-0.027 , $p = 0.806$; $+0.247$, $p = 0.179$), consistent with a contemporaneous association; this specification cannot prove temporal ordering, and we do not present it as doing so.

S5.8. Lagged dependent variable (Table S3)

L1.ESG is significantly positive (0.663 , $p < 0.01$), and the core coefficients remain robust once this inertia is controlled ($\text{Profit} = 1.763^{***}$, $\text{Growth} = -1.283^{***}$). This fixed-

effects specification is subject to Nickell bias in short panels and only controls ESG inertia; it does not correct endogeneity (see Text S4).

S5.9. Stability of the main effects across moderation models

In the two directly comparable moderation models (environmental subsidies and financing constraints), the Profit coefficients are 1.726 and 1.817 and the Growth coefficients -1.177 and -1.248 , close to the baseline estimates (1.773 , -1.110). The main effects in the media-attention and firm-size models are not directly comparable because the interactions are not centered, and are excluded from this check.

S5.10. Standard-error sensitivity

The baseline coefficients are identical under default, heteroskedasticity-robust, and firm-clustered standard errors; the robust and clustered standard errors agree to four decimal places (Profit 0.1093, Growth 0.1646), with only the default standard errors slightly smaller (0.0974, 0.1645). The firm-clustered standard errors used in all tables are therefore the conservative choice in this sample.

Text S6. Additional Analyses Added in the Revision

S6.1. Modern staggered-adoption estimation of the low-carbon pilot

Callaway–Sant’Anna group-time estimates with not-yet-treated and never-treated controls: ATT = 0.083 ($p = 0.842$) and 0.078 ($p = 0.858$) respectively; ATT = 0.083 ($p = 0.855$) under clustering on the 262 cities; wild-cluster bootstrap $p = 0.096$ for the DID $\times$ Profit interaction. Cohort sizes (firms/cities): 2010: 130/7; 2012: 690/58; 2014: 9/2; 2017: 185/27. The two 2014-cohort cities (Haikou and Sanya) enter through Hainan Province’s provincial-level pilot in the second national batch ([56]), with the cohort year following the implementation timing of the province’s pilot plan. Event-study pre-trend average = -0.010 ($p = 0.945$). These cohort sizes refer to the Callaway–Sant’Anna estimation sample; the TWFE event-study regression in Table S4 ($N = 9,168$; 1,003 firms) imposes additional sample requirements, so the firm counts differ across the two analyses. The full set of group-time (cohort-specific) estimates is provided in the replication package output log, alongside the city-pilot crosswalk, cohort counts, and the verification of the 983 missing-pilot-status observations.

S6.2. Alternative distress measurement

Under the Altman Z-score the distress-to-ESG link is zero ($b = -0.002$, $p = 0.927$) and indirect effects vanish. A four-layer diagnostic shows this to be a property of the measure rather than of the sample: (i) dropping the controls that overlap mechanically with the Z-score components moves its coefficient only to $+0.035$ ($p = 0.098$); (ii) standardized to a common scale, the per-SD association of the O-Score with ESG is -0.558 ($p < 0.001$), about three times the Z-score’s $+0.181$ ($p = 0.098$) and of the opposite sign; (iii) in a horse race with both measures, the O-Score is unchanged (-0.258 , $p < 0.001$) while the Z-score carries no independent signal; and (iv) restricting to manufacturing firms (the Altman calibration domain) does not rescue the Z-score ($+0.025$, $p = 0.36$ with de-overlapped controls, against -0.254 , $p = 0.011$ for the O-Score in the same subsample). The mediation is therefore presented as a measure-dependent statistical association.

S6.3. Continuous and multi-threshold measurement of strategic orientation

Industry-adjusted continuous measures (profitability deviation $+9.06$, growth deviation -1.60 , both $p < 0.001$), a four-quadrant decomposition (profit $+0.933$ and growth -0.705 against the low-low cell, both $p < 0.001$), median thresholds, and a 0.1-SD exclusion band all reproduce the baseline signs and significance. Transition statistics: 9.0% of firms never change quadrant; 57.7% never change growth status.

S6.4. Zero-inflated subsidies: two-part specification

Extensive margin (receipt): -0.450 ($p = 0.038$) for profit-oriented firms; intensive margin (amount among recipients): $+3.674$ ($p = 0.005$). The substantive association with ESG is concentrated on the amount margin; the receipt margin is small and negative (-0.450 , $p = 0.038$).

S6.5. Multiple testing and equivalence testing

Benjamini–Hochberg q -values accompany all interaction-family p -values (a seventeen-test family: the fifteen interaction and path-difference tests of Table S6 plus the two group-specific policy-effect tests reported in the Table 3 note of the main text; no interaction survives at $q < 0.10$ in that family; in the narrower ten-test family (the ten within-path interaction tests of Table S6, excluding the five path-difference rows) the size interaction passes marginally, $q = 0.074$). TOST equivalence tests with pre-specified bounds (10%/25% of the full-sample baseline profit total effect) certify the financing-constraint nulls ($p = 0.001$ and 0.043) and mark the underpowered policy and subsidy interactions on the growth path as indeterminate (minimum detectable effects of 52–290% of the full-sample baseline profit total effect; Table S6). The Benjamini–Hochberg q -values of the fifteen Table S6 tests are reported in the BH q column of Table S6; the two remaining family members (the group-specific policy-effect tests of the Table 3 note) have q -values of 0.833 and 0.873.

Table S6. Equivalence Testing (TOST) and Minimum Detectable Effects (MDE) for the Key Interaction Tests.

Test	Coef.	SE	p	MDE	MDE (% of TOST baseline)	p TOST ($\pm 10\%$)	p TOST ($\pm 25\%$)	p BH q
KZ $\times$ Profit	−0.016	0.053	0.755	0.147	8.3%	0.001	<0.001	0.873
KZ $\times$ Growth	0.038	0.082	0.645	0.229	12.9%	0.043	<0.001	0.845
KZ: path difference	−0.054	0.088	0.539	0.247	13.8%	0.080	<0.001	0.833
Size $\times$ Profit	−0.060	0.089	0.499	0.248	13.9%	0.091	<0.001	0.833
Size $\times$ Growth	−0.346	0.129	0.007	0.361	20.3%	0.903	0.220	0.125
Size: path difference	0.286	0.139	0.040	0.389	21.8%	0.781	0.126	0.206
Media $\times$ Profit	0.019	0.096	0.842	0.268	15.0%	0.048	<0.001	0.873
Media $\times$ Growth	−0.271	0.147	0.065	0.410	23.0%	0.736	0.116	0.206
Media: path difference	0.290	0.161	0.073	0.452	25.4%	0.755	0.167	0.206
Subsidy $\times$ Profit	2.404	1.201	0.046	3.364	188.7%	0.968	0.948	0.206
Subsidy $\times$ Growth	0.848	1.849	0.646	5.177	290.5%	0.641	0.586	0.845
Subsidy: path difference	1.556	1.773	0.380	4.964	278.5%	0.781	0.734	0.808
DID $\times$ Profit	0.390	0.209	0.063	0.586	32.9%	0.844	0.395	0.206
DID $\times$ Growth	0.053	0.333	0.873	0.932	52.3%	0.354	0.119	0.873
DID: path difference	0.337	0.355	0.343	0.994	55.8%	0.673	0.380	0.808

Note: $MDE = 2.8 \times SE$ (80% power, two-sided $\alpha = 0.05$); MDE% expresses the MDE relative to the full-sample baseline profit total effect (1.782, the Profit coefficient of the baseline main-effect model estimated on the full main sample of 12,998 firm-years, as documented in the replication package output log; the mediation-sample step-1 coefficient in Table 2 is 1.773). TOST p is the larger of the two one-sided p -values at equivalence bounds of $\pm 10\%$ and $\pm 25\%$ of the same baseline coefficient. Path difference is the Profit-side minus the Growth-side interaction coefficient. Tests with TOST $p < 0.10$ at the $\pm 10\%$ bound (KZ $\times$ Profit, KZ $\times$ Growth, KZ path difference, Size $\times$ Profit, Media $\times$

Profit) are statistically equivalent to zero within $\pm 10\%$ of the baseline effect; all other tests are indeterminate. MDE% is a minimum-detectable-effect size and is not the same quantity as the confidence-interval endpoint shares reported in Table 5 of the main text, whose denominators are the path-specific baseline total effects (Profit 1.773, Growth −1.110).

S6.6. Temporal ordering

A specification ordered as orientation(t-1), distress(t), ESG(t+1) does not reproduce the contemporaneous chain; Granger-style tests detect feedback in both directions (lagged distress to ESG, $p = 0.025$; lagged ESG to distress, $p = 0.003$). All mediation language is therefore associational.

S6.7. Expanded controls and alternative fixed effects

Adding firm size and the expanded set (firm age, board size, board independence, CEO duality, asset turnover) leaves the headline estimates unchanged (Profit 1.822, Growth −1.323, both $p < 0.001$); industry-by-year and province-by-year fixed-effect variants agree.

S6.8. Alternative ESG rater (CNRDS)

Inter-rater correlation with Bloomberg is 0.31. On the identical sample ($N = 12,998$), the main effects replicate (Profit +1.369, Growth −1.588, both $p < 0.001$), and the environmental-dimension association for profit-oriented firms is positive and significant under CNRDS (+0.625, $p = 0.006$; social +1.538 and governance +1.235, both $p < 0.001$; dimensional regressions on $N = 13,039$), indicating the weak environmental link under Bloomberg is partly a rater-methodology artifact.

S6.9. Lagged moderators and allocation endogeneity

The profit-side subsidy interaction does not survive lagging ($p = 0.804$); the growth-side media interaction strengthens (-0.301 , $p = 0.029$). Subsidy allocation does not respond to lagged ESG ($p = 0.491$); media attention does ($p < 0.001$), and the media results are framed accordingly.

S6.10. Inference robustness

Firm+year two-way clustering leaves the baseline and moderation results intact (Growth $\times$ Size = -0.346 , SE 0.147, $p = 0.035$); the DID interaction attenuates ($p = 0.142$), which we report as part of the inference-sensitivity chain (firm 0.063, city 0.071, wild bootstrap 0.096, two-way 0.142).

S6.11. Sample construction and exclusion sensitivity

Stage counts: 13,039 cleaned firm-years; 12,998 after the ESG merge; 12,629 mediation sample; 9,168 event-study sample. The exclusion rule removes firm-years flagged ST/*ST or negative-equity at the cleaning date only; 2,437 ever-ST firm-years (18.8%) remain with their distress distribution intact (mean O-Score -7.83 for ever-ST versus -8.97 for never-ST firm-years, as in Section 3.1 of the main text). Three alternative sample definitions (baseline; dropping 25 residual flagged rows; dropping all ever-ST firm-years) leave the headline coefficients unchanged; mediation shares attenuate to 2.6%/1.8% under the strictest definition and are reported as such.

The stage-by-stage flow and per-year coverage described in Section 3.1 are as follows.

Stage	Firm-years	Note
Cleaned panel (financial and insurance firms excluded; firm-years	13,039	
ST/*ST or negative-equity at the cleaning date excluded)		

Stage	Firm-years	Note
After merging Bloomberg ESG scores	12,998	41 firm-years lost; none of the 41 has a composite (total) ESG score in the raw Bloomberg extract (the gap is at source)
Mediation complete-case sample (all baseline-model variables non-missing)	12,629	O-Score is the binding constraint (Text S6.13)
Event-study sample (pilot-status non-missing; event-study window requirements apply)	9,168	

Per-year coverage of the main sample (12,998 firm-years):

Year	Firm-years in the main sample
2010	458
2011	602
2012	808
2013	885
2014	910
2015	1,109
2016	1,023
2017	1,060
2018	1,096
2019	1,162
2020	1,174
2021	1,206
2022	594
2023	911
Total	12,998

The 2022 dip (594 firm-years) reflects a coverage contraction in the raw Bloomberg extract (650 firm-years at source), not our processing. The 41 firm-years lost to ESG coverage differ systematically from retained observations: they are smaller (mean Size 22.03 vs. 23.24, $t = -6.0$), less leveraged (mean leverage 0.28 vs. 0.48), and more profitable (mean ROA 0.084 vs. 0.047). This is consistent with Bloomberg coverage tilting toward larger firms, and we therefore caution against extrapolating the estimates to small unrated firms.

S6.12. Reproducibility

The classification rule is reconstructed from the raw CSMAR-sourced panel and reproduces 12,995 of 12,998 firm-year assignments (99.98%; the three residuals are exact boundary ties). The four Bloomberg ESG variables are an unmodified firm-year merge from the raw extract (verified to storage precision, max abs. diff. $< 4e-6$). All do-files, the codebook, the crosswalk, and output logs with package versions are in the replication package.

S6.13. Missingness by variable and year

The table below reports non-missing counts by year for the outcome and the three variables with incomplete coverage, together with the complete-case count (all baseline-model variables non-missing). Subsidy, Size, BM, and Leverage are non-missing for all 12,998 main-sample firm-years and are omitted. The O-Score is the binding constraint: the complete-case count equals the O-Score count in every year. The complete-case

requirement is defined by the baseline mediation-model variables (ESG, the O-Score, and the controls BM, Lev, SOE, CashRatio, and Top1); KZ and Media enter only the moderation models in Table 4 of the main text, which is why years with more missing KZ than O-Score values (2014–2017) still show the complete case equal to the O-Score count.

Year	ESG	O-Score	KZ	Media	Complete case
2010	458	423	456	457	423
2011	602	543	598	601	543
2012	808	772	804	807	772
2013	885	870	877	884	870
2014	910	900	882	909	900
2015	1,109	1,081	1,056	1,108	1,081
2016	1,023	1,000	992	1,022	1,000
2017	1,060	1,029	1,022	1,059	1,029
2018	1,096	1,060	1,095	1,093	1,060
2019	1,162	1,140	1,162	1,147	1,140
2020	1,174	1,135	1,174	1,172	1,135
2021	1,206	1,176	1,206	1,204	1,176
2022	594	590	594	592	590
2023	911	910	911	909	910
Total	12,998	12,629	12,829	12,964	12,629

S6.14. Alternative financing-constraint proxy: the SA index

We replicate the financing-constraint moderation of Table 4 with the SA index in place of the KZ index ($N = 12,998$; firm-clustered standard errors). Both interactions remain null: $SA \times \text{Profit} = 0.264$ ($p = 0.502$) and $SA \times \text{Growth} = -0.058$ ($p = 0.925$), and the between-path difference is insignificant as well (0.321 , $p = 0.633$). The two indices are nearly orthogonal in this sample ($r = 0.024$), so the KZ and SA nulls are complementary rather than redundant: the financing-constraint moderation is absent under either proxy. Because the SA index has a mean of -3.83 , its zero point lies off the observed support and we report the interactions rather than the main effect.

S6.15. Marginal-effects plots and event-study dynamics

Figures S1–S4 plot the conditional effects of the Profit and Growth orientations over the observed range of each continuous moderator, with slopes and 95% confidence intervals computed on a fine grid over the observed percentiles (moderators centered). Figure S5 plots the event-study dynamic effects of Table S4.

S6.16. Non-overlapping distress index, pre-policy subgroup ATTs, missing-pilot sensitivity, and broader-sample sensitivity. (i) Non-overlapping distress index. To address the mechanical overlap between the earnings-related components of the O-Score and the ROA-based orientation measure, we construct a distress index from leverage and liquidity variables only (z of leverage minus z of the current ratio minus z of the quick ratio; no earnings component). Its correlation with ROA is -0.37 , against -0.72 for the O-Score, and its correlation with the O-Score is 0.76 . Under this index the orientation-to-distress link itself is no longer significant (a-path: Profit -0.0019 , $p = 0.89$; Growth 0.0154 , $p = 0.44$; $N = 12,998$), and the bootstrap intervals of both indirect effects contain zero, which supports reading the O-Score mediation as measure-dependent rather than as a structural channel. (ii) Pre-policy subgroup ATTs. Cohort-robust (not-yet-treated) ATTs by pre-policy orientation status: pre-policy profit-oriented -1.876 (SE 0.827 , $p = 0.023$); pre-policy growth-oriented $+3.911$ (SE 1.720 , $p = 0.023$); remainder

+0.881 (SE 0.465, $p = 0.058$); TWFE event-study pre-trend tests by group give $p = 0.273$ and $p = 0.446$. These subgroup ATTs estimate group-average effects and are not path-specific interaction tests. (iii) Missing-pilot sensitivity. Excluding the 983 firm-years with missing pilot status gives $\text{DID} \times \text{Profit} = 0.455$ (SE 0.225, $p = 0.043$) and $\text{DID} \times \text{Growth} = -0.192$ ($p = 0.589$; $N = 12,015$). (iv) Broader sample. Re-including the 249 ST/*ST firm-years excluded at baseline that carry Bloomberg ESG scores gives a sample of 13,247 with $\text{Profit} = 1.792$ and $\text{Growth} = -1.092$ (both $p < 0.001$); the recovered firm-years average 28.6 on the ESG score (main sample 30.9) and are growth-oriented in 22.1% of cases and profit-oriented in 10.8% (main sample: 7.9% growth-oriented, 38.4% profit-oriented).

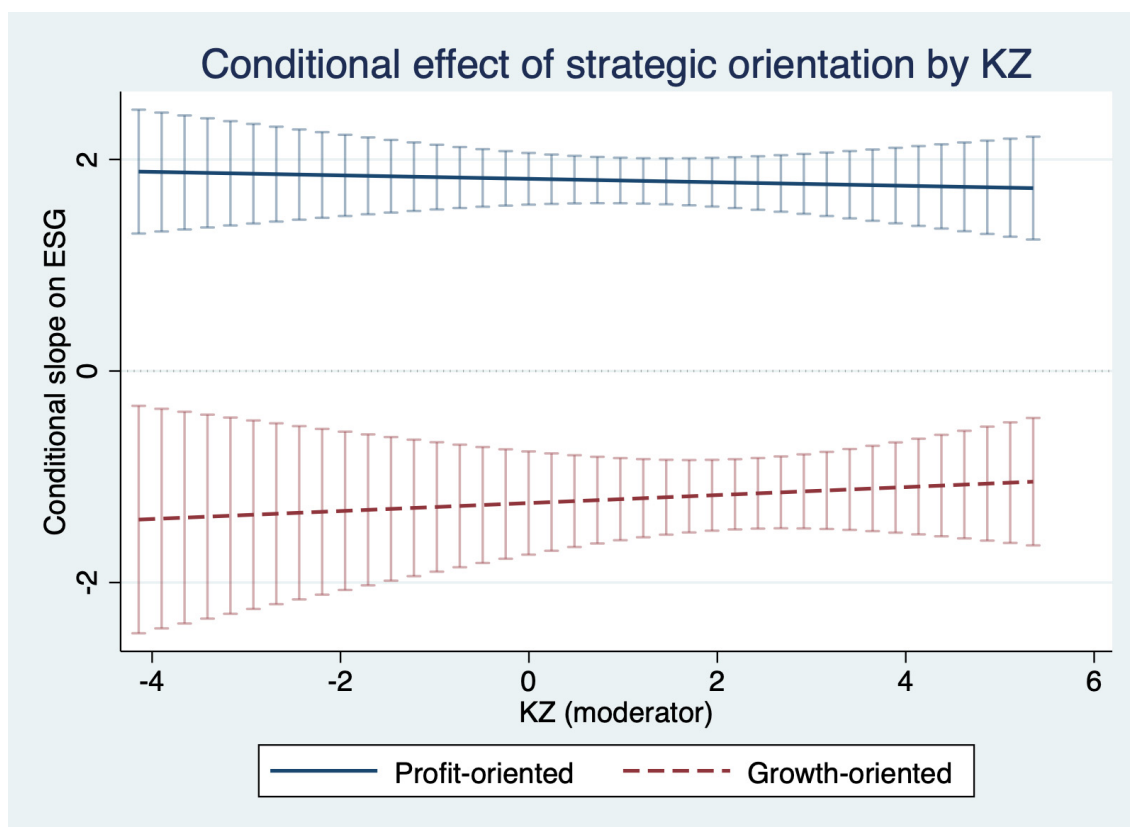


Figure S1. Conditional effects of strategic orientation across the KZ index (financing constraints): marginal effects of Profit and Growth orientation with 95% confidence intervals.

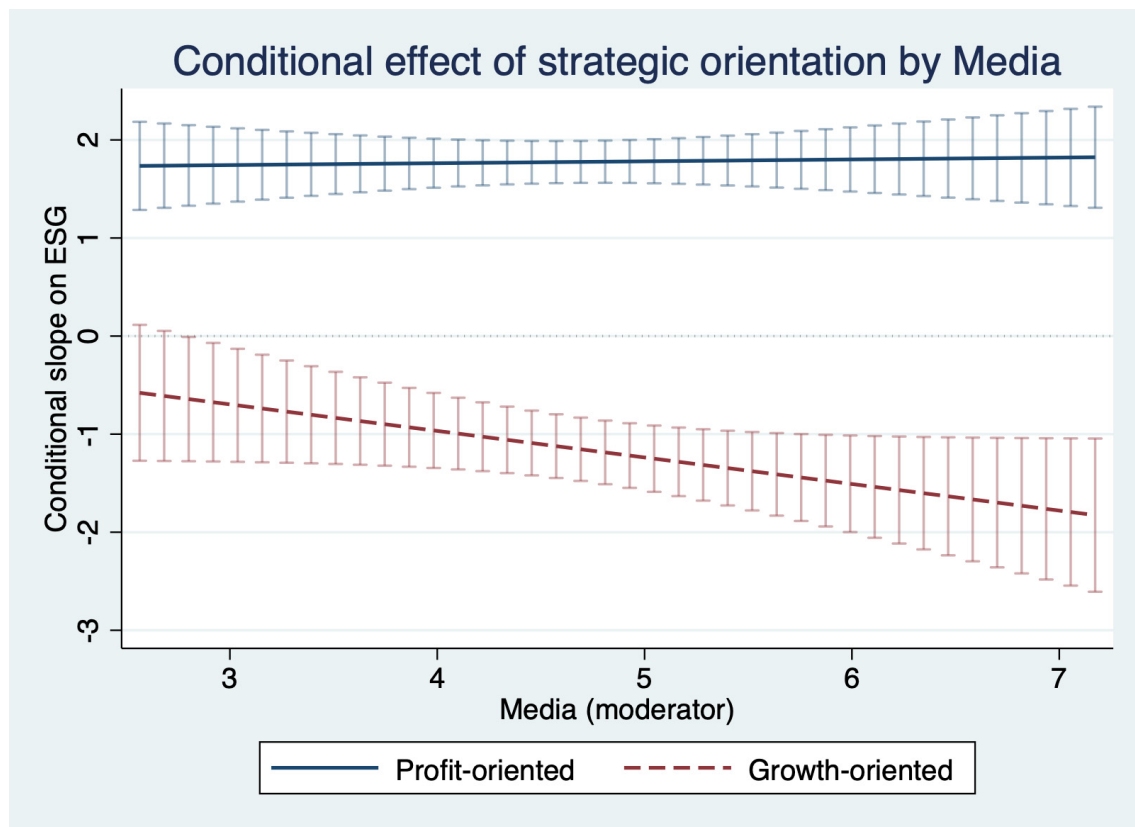


Figure S2. Conditional effects of strategic orientation across media coverage (log positive-report counts): marginal effects of Profit and Growth orientation with 95% confidence intervals.

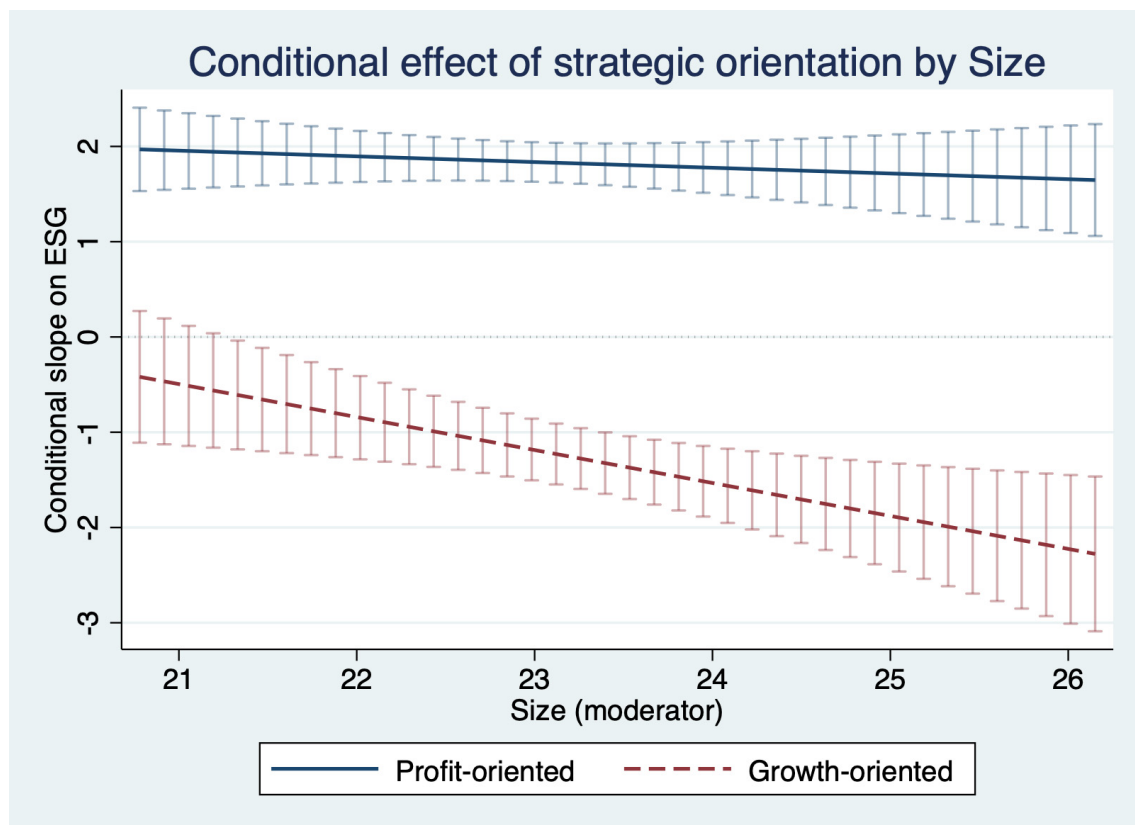


Figure S3. Conditional effects of strategic orientation across firm size: marginal effects of Profit and Growth orientation with 95% confidence intervals.

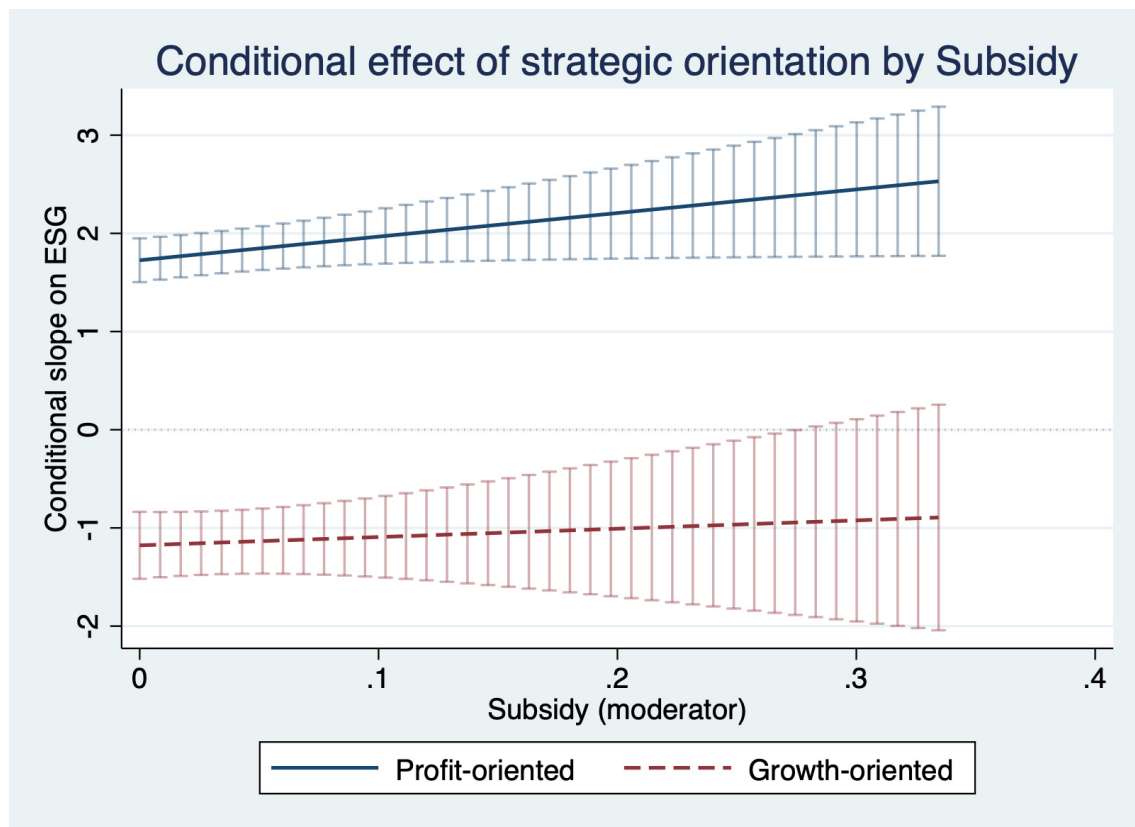


Figure S4. Conditional effects of strategic orientation across government environmental subsidies: marginal effects of Profit and Growth orientation with 95% confidence intervals.

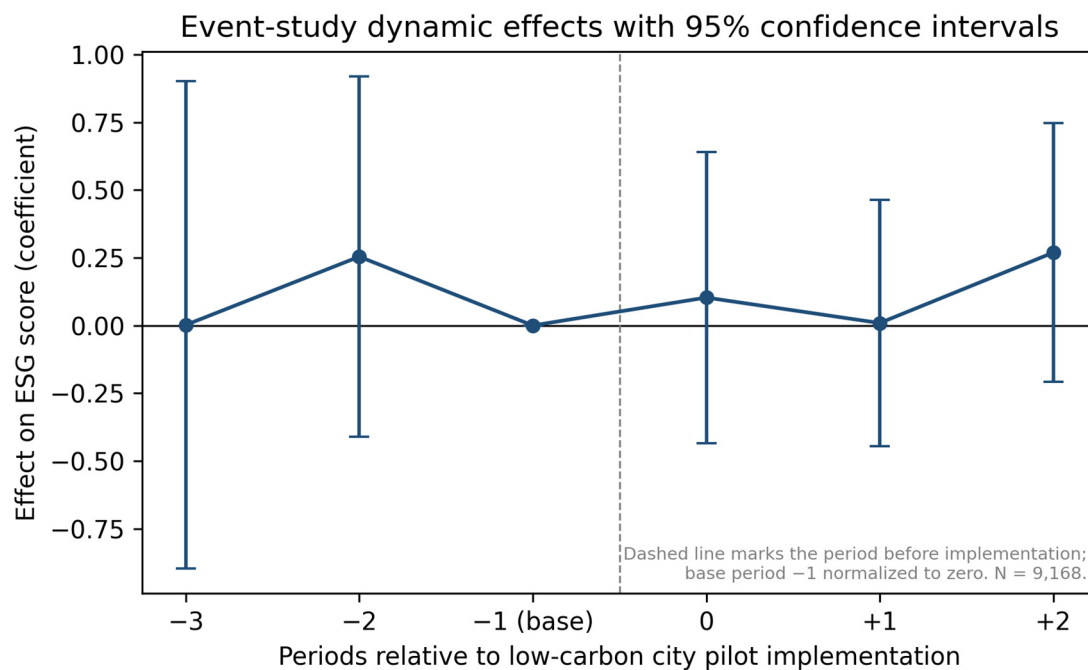


Figure S5. Event-study dynamic effects of the low-carbon city pilot with 95% confidence intervals (coefficients from Table S4; base period -1 normalized to zero).