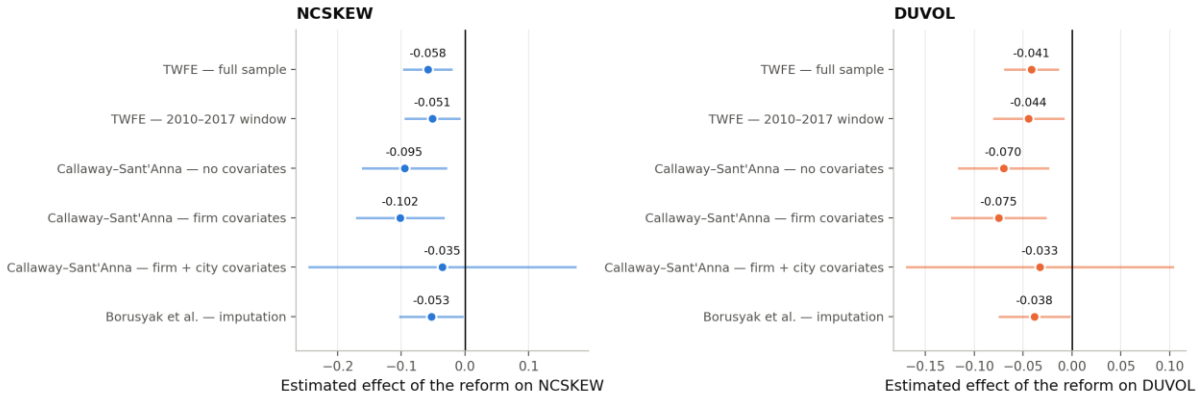
Supplementary Information

Judicial recentralization and stock price crash risk: evidence from China

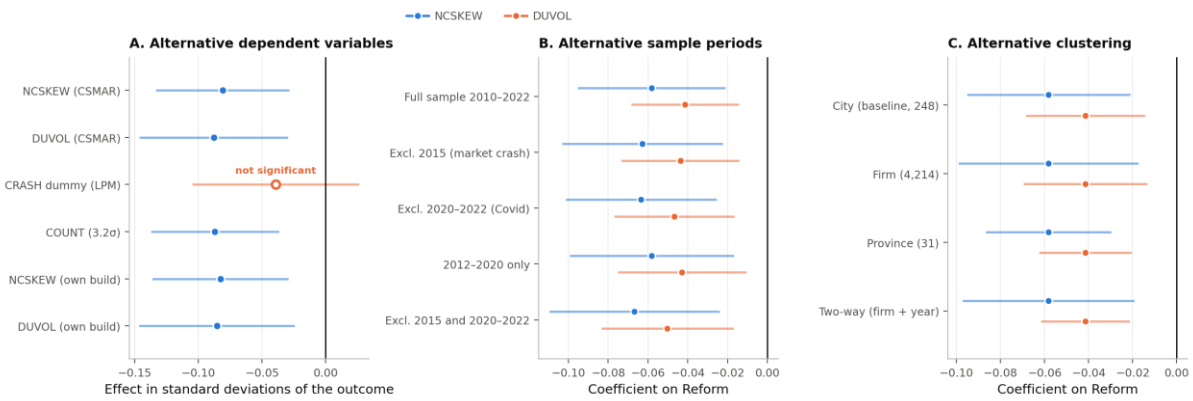
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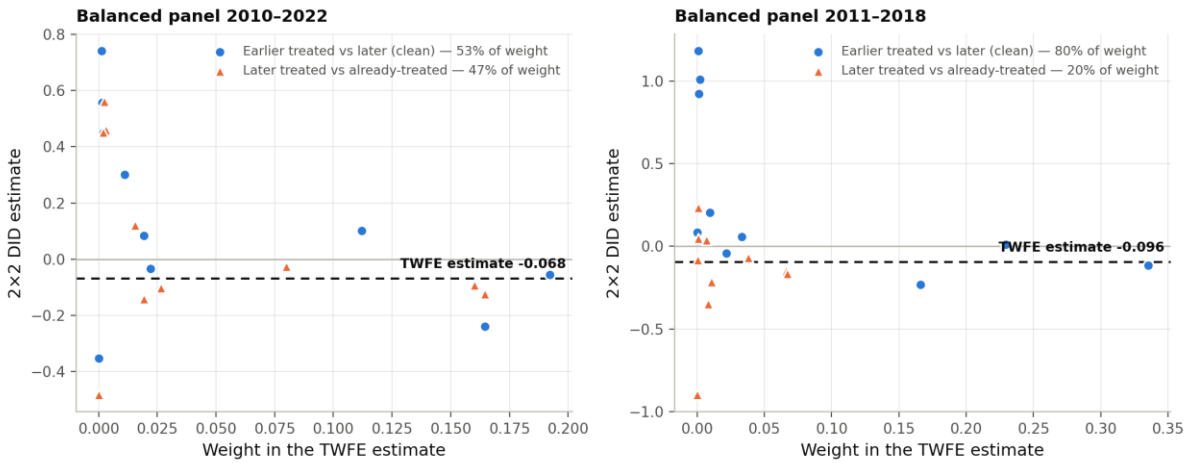
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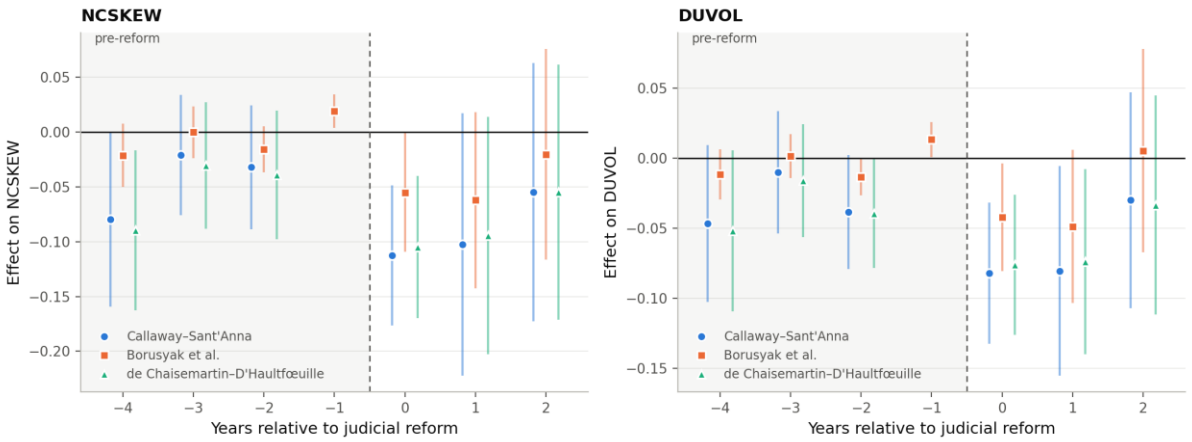
Supplementary Fig. S1 Aggregate treatment effect across estimators. Bars are 95% confidence intervals; those for the robust estimators are based on 300 block bootstrap replications resampling prefectures. The second row restricts the fixed-effects sample to 2010–2017 so that it is directly comparable to the identification window available to the robust estimators.



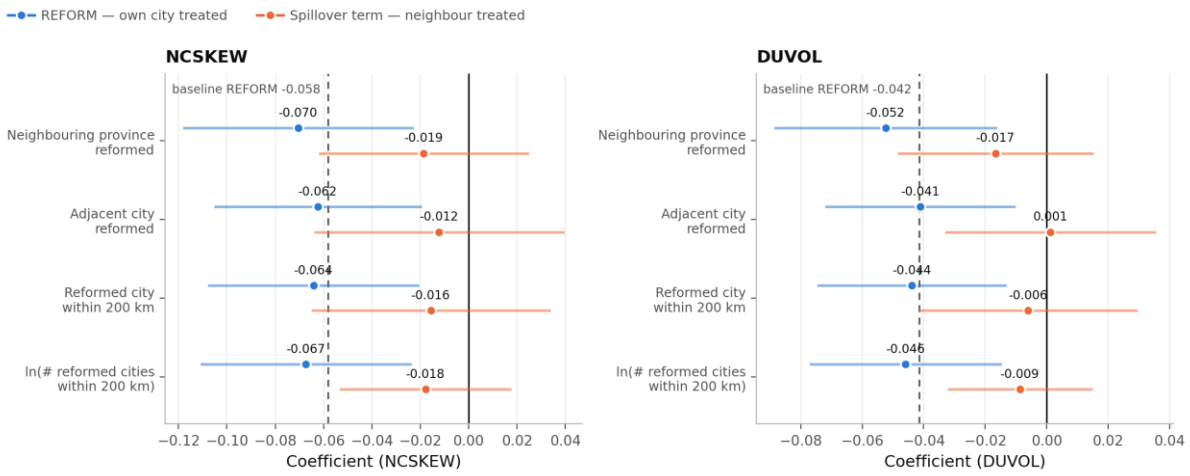
Supplementary Fig. S2 Robustness across outcomes, sample periods and clustering levels. Panel A rescales each coefficient by the standard deviation of its own dependent variable so that magnitudes are comparable across measures; hollow markers denote estimates that are not statistically significant. Panels B and C plot coefficients with 95% confidence intervals. All specifications include firm and year fixed effects and the full set of firm- and city-level controls.



Supplementary Fig. S3 Goodman-Bacon decomposition of the two-way fixed-effects estimate. Each point is one two-by-two comparison embedded in the two-way fixed-effects estimate, plotted against the weight it receives. The weighted average of all points equals the fixed-effects coefficient exactly.



Supplementary Fig. S4 Dynamic effects under heterogeneity-robust estimators. Event-study estimates from the Callaway–Sant'Anna, Borusyak et al., and de Chaisemartin–D'Haultfœuille estimators, with 95% confidence intervals from 300 block bootstrap replications. The first two normalise event year −1 to zero; the imputation estimator imputes from all pre-treatment years jointly and therefore also reports a coefficient at event year −1.



Supplementary Fig. S5 Spillover tests across four exposure measures. Each row adds one spillover measure to the augmented specification. Bars are 95% confidence intervals based on city-clustered standard errors. The dashed vertical line marks the baseline Reform coefficient reported in Table 2.

Supplementary Table S1 Controlling-shareholder expropriation: robustness.

Specification	On impact (t, t+1)	Delayed (t+2 onward)	Obs.
Firm + year FE (baseline)	−0.00072	−0.00285***	27,399
		(0.00101)	
Firm + industry-by-year FE	−0.00089	−0.00298***	27,387
		(0.00103)	
Firm + province-by-year FE	−0.00159	−0.00227	27,397
		(0.00241)	
Firm + year FE, clustered by province	−0.00072	−0.00285**	27,397
		(0.00123)	
Firm + year FE, clustered by firm	−0.00072	−0.00285***	27,399
		(0.00094)	

The dependent variable is other receivables scaled by total assets, winsorized at the 1st and 99th percentiles. All specifications include the full set of firm- and city-level controls. Standard errors for the delayed coefficient appear in parentheses. *** P < 0.01, ** P < 0.05, * P < 0.10.

Supplementary Table S2 Event-window artefact in the modified audit opinion.

Specification	t−4	t−3	t−2	t+0	t+2	t+4
Raw event-time means (no regression)	0.0228	0.0265	0.0253	0.0308	0.0357	0.0261
No fixed effects	0.0044	0.0025	0.0012	0.0157	0.0268	0.0181
Firm fixed effects only	−0.0145	−0.0102	−0.0046	0.0150	0.0364	0.0373
Year fixed effects only	−0.0062	0.0015	0.0041	0.0026	0.0077	0.0041
Firm and year fixed effects	−0.2090	−0.1340	−0.0646	0.0708	0.2085	0.3378

The dependent variable is an indicator for a modified audit opinion (sample mean 0.027). The first row reports raw means of the variable by event time within the restricted window; the remaining rows report coefficients on event-time indicators with t−1 omitted, estimated on the sample restricted to event times −4 through +4, with the full set of controls and standard errors clustered by city. Coefficients in the final row span a range of 55 percentage points for a variable bounded between zero and one.

Supplementary Note 1. Construction of the channel variables

The channel variables are constructed as follows. Creditor governance is measured by the share of long-term borrowing and bonds payable in total interest-bearing debt (short-term borrowing, the current portion of non-current liabilities, long-term borrowing and bonds payable) and by the cost of debt, defined as interest expense over average interest-bearing debt; financial expense is used where interest expense is not reported separately, and ratios of 50% or more are treated as data errors. Auditor monitoring is measured by an indicator for a modified audit opinion. Controlling-shareholder expropriation is measured by other receivables scaled by total assets (Jiang et al., 2010) and, as an alternative, by related-party transaction volume scaled by total assets. Earnings management is measured by the absolute value of discretionary accruals from the modified Jones model (Dechow et al., 1995), estimated by industry and year on cells containing at least fifteen firms, with industries defined by the CSRC classification (two-digit codes for manufacturing and one-letter sections otherwise), and by financial opacity, the three-year rolling sum of those absolute discretionary accruals (Hutton et al., 2009). Judicial behaviour is measured from the CSMAR violation and litigation files by indicators for whether a firm committed a regulatory violation in a given year, whether it was sanctioned in that year, the lag between violation and sanction, whether it was party to litigation, and the share of its cases heard by a court in its own prefecture. We also construct stock price synchronicity as the logistic transform of the market-model R-squared (Morck et al., 2000).

All channel variables are built from CSMAR: the balance sheet, income statement and direct-method cash-flow statement (consolidated annual statements, fiscal year ending 31 December); the audit-opinion file; the related-party transaction file (annual-report disclosures only, transactions between the listed firm and its related parties, renminbi amounts); the violation file, in which multi-year violations are split by violation year and the violation-to-sanction lag is the year of the sanction document minus the violation year (records without a valid sanction date are excluded); and the litigation and arbitration file, collapsed to one record per case and dated by the earliest reported event date (or the earliest announcement date where no event date is reported), in which a case is heard locally when the court and the firm's registered address are in the same prefecture-level city. Stock price synchronicity is the logistic transform of the R-squared from the expanded market model used to construct the crash-risk measures. Indicators for violations, sanctions and litigation are set to zero in firm-years with no recorded event; the violation-to-sanction lag and the local-court share are defined only for firm-years with at least one violation or case. Continuous variables are winsorized at the 1st and 99th percentiles.

Supplementary Note 2. Rank deficiency in the windowed Sun–Abraham specification

The point displayed at event year +4 is a normalization rather than an estimate. Because every prefecture is eventually assigned to a treatment cohort and the sample here is restricted to the nine-year window around each prefecture's assigned reform year, the saturated set of cohort-by-relative-period indicators is not of full rank once firm and year fixed effects are absorbed: the design matrix contains 43 columns but has rank 39. Four cells must therefore be dropped as collinear, and the

estimator drops the event-year +4 group, which is consequently plotted at zero with no confidence interval. It should not be read as evidence that the effect of the reform disappears in the fourth post-reform year.

This rank deficiency also means that the individual dynamic coefficients are not uniquely determined by the data: a different admissible choice of which four cells to drop shifts the estimated profile. We have verified that the qualitative pattern is invariant to that choice. Across 200 alternative admissible selections, every post-reform coefficient remains negative and the pre-reform coefficients remain individually insignificant, although the magnitudes vary (the event-year 0 estimate ranges from −0.07 to −0.44). We therefore read Fig. 2 of the main text as evidence on the timing and shape of the response rather than as a source of point estimates, and we rely for magnitudes on the estimators reported in Section 5.4 of the main text, which construct cohort-by-period comparisons directly and are not subject to this indeterminacy.

Supplementary Note 3. Event-window specifications for channel variables

A specification issue bears on how the channel tests in Table 9 of the main text are estimated, and we document it because it is easy to miss. The conventional way to display dynamics in a staggered design is to restrict the sample to a symmetric window around the reform year and estimate a saturated set of event-time indicators. Applied to our channel variables, that specification produces estimates that cannot be believed. For the modified audit opinion—a binary variable whose sample mean is 2.7%—the windowed specification with firm and year fixed effects yields coefficients rising monotonically from −0.209 four years before the reform to +0.338 four years after. That is a range of 55 percentage points for a variable bounded in the unit interval, and no causal effect can exceed the range of its own dependent variable.

Supplementary Table S2 traces the source. The raw event-time means of the variable vary only between 0.022 and 0.039. With no fixed effects the estimated profile is nearly flat; with firm fixed effects alone, or with year fixed effects alone, it remains small. Only when both are included together does the profile expand into a near-perfect linear trend. The reason is that the window is selected on event time itself, so that within each calendar year only cohorts at particular stages of the rollout remain in the sample. Event time, firm identity, and calendar year then become nearly collinear, and the fitted coefficients are driven along the weakly identified direction, which is a linear trend in event time. The design is not literally rank deficient—after partialling out the fixed effects, the event-time block has a smallest-to-largest singular value ratio of 0.037—but it is close enough that the estimated profile is an artefact of the window rather than a feature of the data.

We therefore estimate all dynamic specifications for channel variables on the full sample with endpoints binned at five years before and after the reform. That design is full rank and far better conditioned, with a singular value ratio of 0.12 for the event-time block. Under it, the crash-risk profile reproduces the on-impact effect documented in Section 5 of the main text, with a coefficient of −0.078 at event time zero, and the channel variables display the flat profiles summarized in Table 9 of the main text. This is the same family of problem as the rank deficiency in the Sun–Abraham specification discussed in Section 5.3 of the main text, and it reinforces the same general

point: in staggered designs, restricting the estimation sample on event time can manufacture dynamics that are not present in the underlying data.