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Beyond Congestion: Zombie Firm Prevalence and the Regime-Dependent Profitability Sensitivity of Corporate Investment in Vietnam's Equity Markets

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Abstract

This study examines how the prevalence of zombie firms in an industry relates to the investment of financially healthy peers in Vietnam's equity markets. Two channels are contrasted: congestion, under which zombie density lowers the average investment of peers, and credit rationing, under which it changes how investment responds to profitability. Zombie status is identified through persistent interest coverage shortfalls, and industry-year density is measured on a lagged, leave-one-out basis for firms listed in Ho Chi Minh City and Hanoi between 2016 and 2025. Linear specifications with firm and year fixed effects give no evidence of congestion. A threshold analysis instead shows that the sensitivity of capital expenditure growth to return on assets is considerably higher in industry-years with zombie peers than in those without, while investment is lower at modest profitability. Inference relies on wild cluster bootstrap, randomisation tests, and a threshold test that re-estimates the cutoff in every draw. The slope differential holds across most alternative definitions, outcomes, and subsamples, but not all, and evidence for a discrete threshold is borderline. The findings are consistent with credit rationing, although firm-level data cannot identify the mechanism.

Keywords: zombie firms, investment sensitivity, emerging markets, Vietnam, corporate finance

JEL Classification: G32 , G21 , O16 , C23 , D22

1 Introduction

Firms that stay in business although their operating profit cannot cover interest payments, commonly called zombie firms, have attracted sustained attention in corporate finance and macro-financial research since Caballero et al. (2008) linked them to the prolonged stagnation of Japan after the asset-price collapse (Hoshi and Kashyap 2004). The argument rests on a soft budget constraint (Kornai et al. 2003). Banks that face capital shortfalls roll over loans to insolvent borrowers instead of recognising losses (Peek and Rosengren 2005), and the surviving borrowers keep absorbing customers, labour, and credit. Healthy competitors then face lower profits and weaker incentives to invest (Caballero et al. 2008; Ahearne and Shinada 2005). Later studies extended the congestion argument to OECD economies (Adalet McGowan et al. 2018), to listed firms across advanced economies (Banerjee and Hofmann 2022), and to China (Tan et al. 2016; Wang and Zhu 2021), and a parallel literature documents credit misallocation toward weak borrowers in Europe (Acharya et al. 2019; Schivardi et al. 2022).

The empirical case for spillovers is nonetheless less settled than the headline narrative suggests. Zombie status is defined from the same accounting information that determines the profitability and growth of the firms it is compared with, so industries with many zombies can differ mechanically from industries with few, even when no lending or competition channel operates (Schivardi et al. 2020). In addition, nearly all studies model peer outcomes as a linear function of zombie prevalence. A linear coefficient on density can be close to zero while the way investment responds to firm fundamentals changes with the local credit environment, and it cannot distinguish a shift in the average level of investment from a shift in its sensitivity to profitability.

Vietnam provides a useful setting for this question. Corporate financing on the Ho Chi Minh City Stock Exchange and the Hanoi Stock Exchange remains heavily bank-intermediated, a feature common to emerging markets in which weak external capital markets and institutional voids lead firms to rely on intermediated and group-based financing (Khanna and Yafeh 2007). The sample period covers the aftermath of a domestic bad-debt resolution process, the COVID-19 shock with its forbearance measures, and the subsequent tightening of credit. Existing work on zombie firms in Vietnam focuses on the characteristics and performance of the distressed firms themselves (Nguyen et al. 2025) or on the relation between distress and firm performance more generally (Vo 2023). Whether zombie prevalence in an industry changes how the *other*, financially healthy firms in the same industry invest has not been examined for this market.

This paper studies that question for 4,853 firm-year observations on 642 healthy firms over 2016 to 2025. Zombie status is based on an interest coverage ratio below one in two consecutive years, and industry-year zombie density is computed on a lagged, leave-one-out basis. Two rival hypotheses are set against each other. Under congestion, density lowers the average level of peer investment. Under credit rationing, density changes the sensitivity of peer investment to profitability, because lenders in a riskier borrower pool place more weight on observable performance. With firm and year fixed effects, the linear relation between density and the growth of capital expenditure is statistically indistinguishable from zero, so the data do not support congestion in its

simplest form. A threshold model shows, however, that the slope of investment growth on return on assets differs sharply between industry-years with and without zombie peers. The estimated cutoff is at the boundary of zero density, so the relevant regime is the presence or absence of zombie peers and not a particular share. The same split is associated with a lower level of investment at low and moderate profitability, and the net effect turns non-negative only in the upper tail of profitability.

The evidence is interpreted with explicit attention to its limits. Inference accounts for the small number of industry clusters, for the estimated cutoff, and for the possibility that the regime partly reflects the number of firms in each industry-year. The slope differential remains in most alternative definitions of zombie status, outcome measures, subsamples, and timing placebos, but it weakens under stricter or looser persistence rules and under finer industry definitions with sector-year effects, and the statistical support for a discrete threshold is borderline. The credit-rationing mechanism cannot be tested directly with firm-level financial statements, so the results are described as consistent with it and not as proof.

The study contributes in three ways. First, it separates a level channel from a sensitivity channel and states them as rival hypotheses, which allows a null average effect to coexist with a change in how investment responds to profitability. Second, it applies inference suited to this design: threshold tests in which the cutoff is re-estimated in each bootstrap draw, wild cluster bootstrap with few clusters, randomisation tests, and a simulation of test size. Procedures that treat the estimated cutoff as known or resample at the observation level reject a true linear model far too often in simulation, which matters for threshold applications in corporate finance more generally. Third, it provides evidence for an emerging market that has been largely absent from the spillover literature, together with a transparent account of which conclusions survive a broad set of variants and which do not.

Section 2 surveys related work and states the hypotheses. Section 3 describes the data. Section 4 sets out the empirical strategy. Section 5 reports the main results, Section 6 the robustness analysis, Section 7 discusses interpretation and limitations, and Section 8 concludes. Additional material is in the Appendix.

2 Related Literature

2.1 Zombie firms and spillovers

The modern literature begins with Japan in the 1990s, where banks constrained by capital regulation continued to lend to insolvent borrowers (Peek and Rosengren 2005; Hoshi and Kashyap 2004). Caballero et al. (2008) classify firms as zombies when they receive interest payments below the lowest rate paid by the best borrowers and show that zombie-dominated industries display weaker job creation, lower productivity, and depressed investment and employment among non-zombie firms. Ahearne and Shinada (2005) reach a related conclusion with an interest-payment-based classification. Adalet McGowan et al. (2018) replace the subsidised-interest criterion with an interest coverage ratio below one for three consecutive years among firms at least ten years old, and find that a higher share of capital sunk in zombie firms is associated with weaker investment and employment growth of non-zombie incumbents across OECD

countries. Banerjee and Hofmann (2022) study listed firms in advanced economies and relate the long-run rise in zombie prevalence to declining interest rates and to the life cycle of firms. In China, Tan et al. (2016) and Wang and Zhu (2021) report that zombie presence crowds out private investment and financing of healthy firms, with stronger effects among non-state and more market-oriented firms. El Ghoul et al. (2021) document across seventy-nine countries that zombie firms follow distinctive dividend, investment, and leverage policies and that debt enforcement efficiency lowers their prevalence.

Evidence on the credit channel behind these patterns is mixed. Acharya et al. (2019) show that unconventional monetary policy in Europe was followed by lending to weak firms that did not invest or hire more, while Schivardi et al. (2022) trace misallocation of credit toward low-quality firms during the sovereign debt crisis using loan-level data. The common feature of the spillover studies is that peer outcomes are modelled as a monotone and, in practice, linear function of zombie prevalence.

2.2 Measurement and inference

Schivardi et al. (2020) argue that comparisons of healthy-firm outcomes across industries with different zombie shares can be biased by construction, because the classification is derived from firm performance and therefore correlates mechanically with the performance distribution of the surrounding industry. Once the bias is addressed in Italian data, the estimated real effects of zombie lending are small. The concern is relevant to every reduced-form design of the type used here, and it motivates three safeguards in the analysis below: lagged and leave-one-out density, inclusion of only healthy firms in the estimation sample, and explicit tests that distinguish genuine dependence on lagged density from contemporaneous or future density and from the number of firms in each industry-year.

Inference raises separate issues. Threshold and regime-switching models are designed to detect a break in a coefficient at an unknown point (Hansen 1999, 2000), and the supremum-type test statistic has a non-standard distribution because the cutoff is unidentified under the null (Andrews 1993). Treating an estimated cutoff as known inflates rejection rates. When regressors vary only at the industry level and the number of industries is small, conventional clustered standard errors are also unreliable, and wild cluster bootstrap methods are recommended (Cameron et al. 2008; MacKinnon et al. 2023). Randomisation inference provides an additional finite-sample check that does not rely on asymptotic approximations (Young 2019). Smooth-transition models and the associated linearity tests (Teräsvirta 1994; Fok et al. 2005) offer an alternative to a hard cutoff. Standard errors follow the guidance for finance panels in Petersen (2009), and the multiplicity of robustness variants is handled with the false discovery rate procedure of Benjamini and Hochberg (1995).

2.3 Credit rationing and investment sensitivity

The notion that firms' investment responds to internal funds more strongly when external finance is costly or rationed is central to the financing-constraints literature, although the interpretation of investment-cash-flow sensitivities is debated (Kaplan

and Zingales 1997). Models in which intermediary capital is scarce predict that a deterioration in the quality or capitalisation of the lending pool hurts poorly capitalised borrowers most (Holmström and Tirole 1997). If a larger presence of impaired borrowers makes banks more cautious, or ties up their lending capacity, healthy firms with weak internal profitability should be rationed first, while firms with strong profitability can still obtain funds. Investment then becomes more closely tied to profitability, and investment at low profitability falls. This reasoning implies a change in the slope of investment on profitability, and a lower level of investment where profitability is low, without necessarily implying a lower average across all firms.

2.4 Vietnam

Research on zombie firms in Vietnam is sparse. Nguyen et al. (2025) compare zombie and healthy manufacturing firms and find lower liquidity, capital efficiency, and profitability among the former, which is a within-firm comparison and not a test of spillovers. Vo (2023) uses interest coverage to measure financial distress and a threshold regression to relate market risk and distress to a firm's own performance. The question of whether zombie prevalence alters the investment behaviour of other firms in the same industry remains open.

2.5 Hypotheses

Three hypotheses organise the empirical analysis.

H1 (Congestion). Industry zombie density is negatively related to the capital expenditure growth of healthy peers, so that the linear coefficient on lagged density is negative.
H2 (Regime-dependent sensitivity). The sensitivity of capital expenditure growth to return on assets differs across zombie-density regimes and is higher where zombie peers are present.
H3 (Net effect). Given H2, the net association between the presence of zombie peers and investment is negative at low and moderate profitability and weakens or reverses at high profitability.

H1 and H2 are not mutually exclusive, but a null result for H1 would not rule out H2. The cutoff that defines the regimes is treated as unknown, and one purpose of the estimation is to learn whether it lies at a positive density or at the boundary where zombie peers are absent.

3 Data

The sample combines firm-level financial statement data for firms listed on the Ho Chi Minh City Stock Exchange and the Hanoi Stock Exchange for fiscal years 2012 to 2025. The source data are organised in twenty-four variable groups, merged on firm identifier and fiscal year, which yields an unbalanced panel of 698 firms and 217 variables after columns with more than 65 percent missing values are removed. Industry classification follows an eleven-sector scheme. Fiscal years before 2016 serve only to build the interest

coverage history and the lagged density measure, so the estimation window is 2016 to 2025. Consecutive observations of the same firm are always one calendar year apart, so lags based on the position in the panel coincide with calendar lags.

Zombie status is assigned using the interest coverage ratio, defined as earnings before interest and taxes divided by interest expense, for firm-years in which interest expense exceeds a minimal threshold that prevents spurious ratios from near-zero denominators. A firm-year is classified as a zombie if the ratio falls below one in the current and the preceding year and the firm has at least three years of listed history, following the persistence logic of Caballero et al. (2008) and Adalet McGowan et al. (2018). Firm-years with insufficient history are treated as missing and not as healthy, because assigning healthy status by default would tilt the healthy sample toward newly listed firms. Financial firms are retained and classified with the same criterion, because interest income and expense are part of the ordinary business of a bank; they account for 5.1 percent of the estimation sample, and results without them are reported in Section 6. The classification of financial firms should nevertheless be read with more caution than that of non-financial firms.

Industry-year zombie density is the share of zombie firms among listed peers in the same sector and year, excluding the focal firm and lagged by one year relative to the outcome. Sector-years with fewer than five listed firms are dropped. Table 1 reports the market-wide zombie share by fiscal year, and Figure 1 plots it together with the mean sector density and the share of sector-years in which no zombie peer is present. Years before 2016 are omitted from the table because fewer than three years of history are available to evaluate the persistence criterion, and 2026 is omitted because it is an incomplete reporting year.

Table 1 Market-wide zombie firm share by fiscal year (firm-years with sufficient interest coverage history)

Year	2016	2017	2018	2019	2020	2021	2022	2023	2024	2025
Zombie share	5.5%	6.4%	6.7%	6.9%	8.4%	11.1%	9.8%	11.3%	15.1%	12.6%

The market-wide share rises from about five percent in 2016 to a peak near fifteen percent in 2024, with the largest single-year increase between 2020 and 2021 and a partial retracement afterwards. This upward drift is consistent with the long-run increase documented for advanced economies (Banerjee and Hofmann 2022). A consequence for the empirical design is that industry-years without any zombie peer become less frequent over time, which is taken up in Section 6.

The estimation sample retains only firm-years in which the firm is not itself classified as a zombie, so that the estimates describe how zombie prevalence in the surrounding industry relates to the investment of healthy incumbents and do not mechanically capture the weak investment of zombie firms themselves. After requiring non-missing values of the outcome, the lagged density, and all controls, the sample contains 4,853 firm-year observations on 642 firms, distributed across sectors as shown

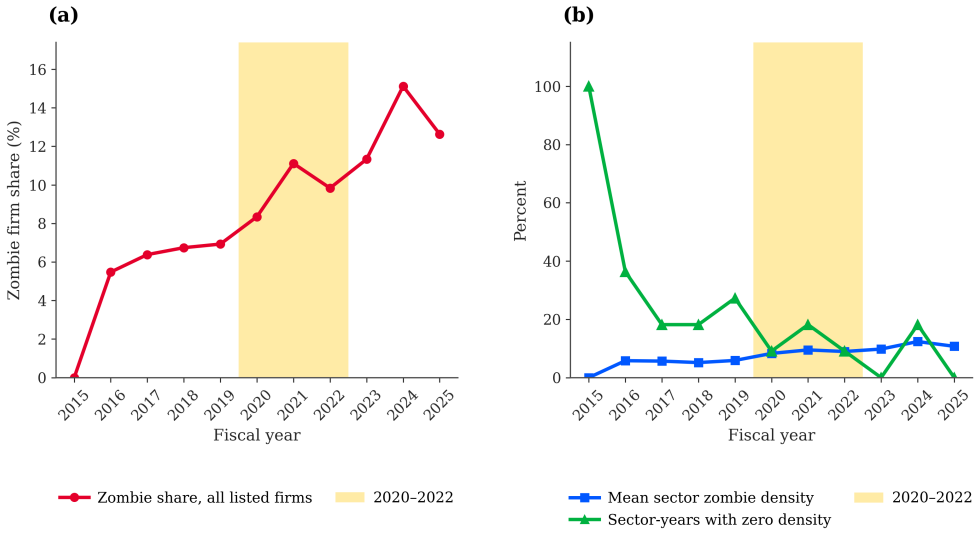


Fig. 1 Zombie prevalence over time. Panel (a): share of listed firms classified as zombie firms by fiscal year. Panel (b): mean sector-level zombie density and the percentage of sector-years with zero zombie density. The shaded band marks 2020 to 2022.

in Table 2. In 799 of these observations (16.5 percent) no firm in the same sector was a zombie in the preceding year.

Table 2 Estimation-sample composition by sector

Sector	Firm-years	Share of sample
Industrials	1,522	31.4%
Materials	857	17.7%
Real Estate	453	9.3%
Consumer Staples	427	8.8%
Consumer Discretionary	425	8.8%
Utilities	314	6.5%
Financials	248	5.1%
Energy	246	5.1%
Health Care	173	3.6%
Information Technology	108	2.2%
Communication Services	80	1.6%
Total	4,853	100.0%

The outcome is capital expenditure growth measured as a symmetric growth rate, $(x_t - x_{t-1})/(0.5|x_t| + 0.5|x_{t-1}|)$, in the manner of Davis and Haltiwanger (1992). This measure stays defined when expenditure is zero and is bounded between -2 and 2; 5.4 percent of observations lie at the bounds, and an alternative outcome that excludes them is analysed in Section 6. Continuous controls are winsorised at the first and ninety-ninth percentiles. The controls are return on assets, the debt-to-assets ratio,

the natural logarithm of total assets, and operating cash flow scaled by assets. Table 3 reports summary statistics and Table 4 the pairwise correlations.

Table 3 Descriptive statistics, estimation sample ($N = 4{,}853$)

Variable	Mean	SD	Min	P25	Median	P75	Max
Capex growth (symmetric)	−0.008	1.067	−2.000	−0.773	0.001	0.773	2.000
Zombie density (lag-1)	0.082	0.058	0.000	0.040	0.080	0.127	0.254
Return on assets	0.059	0.059	−0.110	0.019	0.045	0.083	0.316
Debt-to-assets	0.238	0.179	0.000	0.085	0.212	0.370	0.798
Ln(total assets)	28.010	1.611	24.334	26.865	27.961	29.003	33.834
CFO-to-assets	0.059	0.134	−0.383	−0.011	0.060	0.139	0.465

Table 4 Pairwise Pearson correlations, estimation sample

	(1)	(2)	(3)	(4)	(5)	(6)
(1) Capex growth	1.000					
(2) Zombie density (lag-1)	0.039	1.000				
(3) Return on assets	0.052	−0.122	1.000			
(4) Debt-to-assets	−0.002	−0.049	−0.361	1.000		
(5) Ln(assets)	0.047	0.135	−0.064	0.277	1.000	
(6) CFO-to-assets	0.006	−0.113	0.388	−0.302	−0.082	1.000

4 Empirical Strategy

4.1 Linear baseline

The baseline relates the capital expenditure growth of healthy firm i in sector $s(i)$ at year t to lagged sector zombie density $q_{s(i),t-1}$ and firm-level controls,

$$\begin{aligned} y_{it} = \mu_i + \lambda_t + \delta\, q_{s(i),t-1} + \beta_1\, \mathrm{ROA}_{it} + \beta_2\, \mathrm{Leverage}_{it} \\ + \beta_3\, \ln(\mathrm{Assets})_{it} + \beta_4\, \mathrm{CFO/Assets}_{it} + \varepsilon_{it}, \end{aligned} \tag{1}$$

where μ_i and λ_t are firm and year fixed effects. The two sets of effects are removed jointly by alternating projections suited to unbalanced panels, and the implementation is checked against a standard two-way fixed-effects estimator. H1 corresponds to $\delta < 0$. Because density varies at the sector-year level, interaction between density and profitability is also examined, together with a bootstrap test of whether the relation between investment and the product of density and profitability is adequately described by a linear interaction, using a third-order Taylor expansion in the manner of smooth-transition linearity tests (Teräsvirta 1994; Fok et al. 2005).

4.2 Regime model

To allow density to change the sensitivity of investment to profitability, observations are split into two regimes according to $H_{it} = \mathbf{1}\{q_{s(i),t-1} > \gamma\}$, where γ is a cutoff to be estimated (Hansen 1999, 2000). The specification is

$$\begin{aligned} y_{it} = \mu_i + \lambda_t + \beta\,\mathrm{ROA}_{it} + \phi\,H_{it}\,\mathrm{ROA}_{it} + \theta\,H_{it} \\ + \delta\,q_{s(i),t-1} + \mathbf{z}_{it}'\boldsymbol{\kappa} + \varepsilon_{it}, \end{aligned} \tag{2}$$

where $\mathbf{z}_{it}$ contains leverage, size, and cash flow with common coefficients. The return-on-assets slope is β in the low-density regime and $\beta + \phi$ in the high-density regime, so ϕ is the slope differential tested by H2, and θ is the level shift. The net association of the high-density regime with investment at profitability r is

$$M(r) = \theta + \phi\,r, \tag{3}$$

which changes sign at $r^* = -\theta/\phi$ when θ and ϕ have opposite signs. Standard errors for $M(r)$ and r^* use the delta method. H3 implies $M(r) < 0$ at low and moderate r.

The cutoff is chosen by least squares over a grid of observed density values, subject to a trimming rule that requires at least 15 percent of observations in each regime. The threshold test is a supremum-type F statistic for the joint restriction $\theta = \phi = 0$ across the grid. Its null distribution is non-standard (Andrews 1993), so critical values and p-values come from a wild cluster bootstrap in which the cutoff is re-estimated in every draw. A likelihood-ratio confidence set for γ is also reported, and sensitivity to the trimming share is examined in the Appendix.

4.3 Inference

Density varies only across sector-years, and the panel has eleven sectors, so asymptotic cluster-robust inference at the sector level is unreliable. Results are therefore reported with firm, sector-year, and sector clustering, with p-values for the key coefficients from a wild cluster bootstrap-t procedure (Cameron et al. 2008; MacKinnon et al. 2023). With eleven clusters, Webb six-point weights are used. Bootstrap p-values are simulation-based and vary slightly in the second or third decimal across runs. As an additional check, randomisation inference permutes the density series across sectors within each year and recomputes the threshold statistic (Young 2019). The size of the threshold test is evaluated in a simulation described in the Appendix.

4.4 Economic importance and mechanical bias

Economic importance is assessed through the standardised slope differential, the change in within-firm R^2, information criteria, and the net effect in equation (3). Four design features address the mechanical-bias concern of Schivardi et al. (2020). Density is lagged and leave-one-out. Only healthy firms are estimated. The number of peers in each sector-year is held fixed through sample restrictions and controls, because a

sector-year with few firms is more likely to have zero density by chance. And the lag-one density is compared with contemporaneous, two-year-lagged, and future density in horse-race specifications.

4.5 Variants and multiple testing

The slope differential is re-estimated under a set of variants covering the definition of zombie status, the measurement of density, the fixed-effects structure, and the outcome variable. Because many variants are evaluated, p-values are also adjusted across the family using the Holm procedure and the Benjamini-Hochberg false discovery rate (Benjamini and Hochberg 1995). A conclusion is labelled robust only when the differential keeps its sign and is significant at the ten percent level in at least three quarters of the relevant checks.

5 Results

5.1 Linear specification

Table 5 reports equation (1). With firm effects alone, lagged zombie density has a positive and significant coefficient. The sign is opposite to that predicted by H1. The coefficient falls by roughly two thirds and loses significance once year effects are added, which is expected because density and aggregate investment both trend over a period that includes a credit expansion, the COVID-19 shock, and its aftermath. Under the preferred two-way specification, the coefficient is not distinguishable from zero with firm clustering and has a wild cluster bootstrap p-value of 0.58 at the sector level and 0.57 at the sector-year level. The interaction between density and profitability is also not significant (coefficient 4.17, bootstrap $p = 0.62$). H1 therefore receives no support in either its congestion form or its reallocation form.

Table 5 Baseline linear specification. Cluster-robust standard errors by firm in parentheses. Significance: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

	Model 1: Firm FE only	Model 2: Firm + Year FE
Zombie density (lag-1)	0.879^{***} (0.256)	0.322 (0.485)
Return on assets	2.312^{***} (0.235)	2.116^{***} (0.267)
Debt-to-assets	0.137 (0.145)	0.115 (0.153)
Ln(assets)	0.005 (0.041)	−0.019 (0.055)
CFO-to-assets	0.030 (0.147)	0.053 (0.155)
Observations	4,853	4,853
Firms	642	642
Fixed effects	Firm	Firm + Year

The absence of a linear association does not imply that density is irrelevant. A bootstrap test for nonlinearity in the density-profitability relation, based on a Taylor expansion in the density, rejects linearity at conventional levels ($p = 0.004$ with sector clustering and $p = 0.006$ with sector-year clustering), although the linear interaction

itself is insignificant. This pattern points to a dependence of the slope on density that is not monotone, which is examined next.

5.2 Threshold and regime

Figure 2 summarises the threshold analysis. Panel (a) plots the F statistic over the cutoff grid. It is highest at a cutoff of zero, where the high-density regime consists of all sector-years with at least one zombie peer in the previous year. The maximum is 13.86, compared with bootstrap 5 percent critical values of 13.88 (sector-year clustering) and 14.85 (sector clustering). The corresponding bootstrap p-values are 0.052 and 0.068, so the evidence for a threshold is borderline. The 95 percent likelihood-ratio confidence set for γ runs from 0 to 0.052, which means that the data cannot rule out a cutoff near five percent but that the point estimate lies at the boundary. The cutoff of five percent that would arise in a conventional grid search is thus not supported as a distinct threshold. Randomisation inference, which permutes density across sectors within years, gives a p-value of 0.028 for the threshold statistic.

Panel (b) of Figure 2 compares the profitability slope without zombie peers with slopes across terciles of positive density. The slope is close to zero (0.04) in the 799 observations with zero density, rises to 2.85 and 3.32 in the first two terciles, and falls to 1.59 in the third. The slopes are jointly different ($p = 0.004$ with sector-year clustering and $p = 0.018$ with sector clustering). The profile resembles a step at zero density followed by a plateau and a partial decline, and not a gradual increase with density. This is why the regime is characterised below as the presence or absence of zombie peers.

5.3 Regime estimates

Table 6 reports equation (2) with the cutoff at zero. Without zombie peers, investment growth shows essentially no relation to return on assets (slope -0.05). With zombie peers, the slope is 2.54. The differential of 2.58 is significant under every clustering choice: the wild cluster bootstrap p-value is 0.03 with sector clustering and 0.006 with sector-year clustering, and the 95 percent confidence interval based on sector clusters is 0.74 to 4.43. Observations in the high-density regime also show a lower level of investment growth by 0.29 (bootstrap $p = 0.03$ at the sector level and 0.03 at the sector-year level). H2 is supported, and the level shift is in line with the premise of H3.

Figure 3 illustrates the result. Panel (a) shows binned observations of capital expenditure growth against return on assets after removing fixed effects and controls, with a flat fitted line in the absence of zombie peers and a steep line where they are present.

5.4 Net effect and economic importance

Because the regime changes both the slope and the level, the net association depends on profitability. Table 7 and panel (b) of Figure 3 report $M(r)$ at selected percentiles of return on assets. The net association is negative at the 5th and 25th percentiles and its 95 percent interval excludes zero there. At the median it is -0.18 with an interval that just includes zero. At the 75th percentile it is -0.08 and statistically indistinguishable from zero, and at the 95th percentile it is positive but imprecise. The

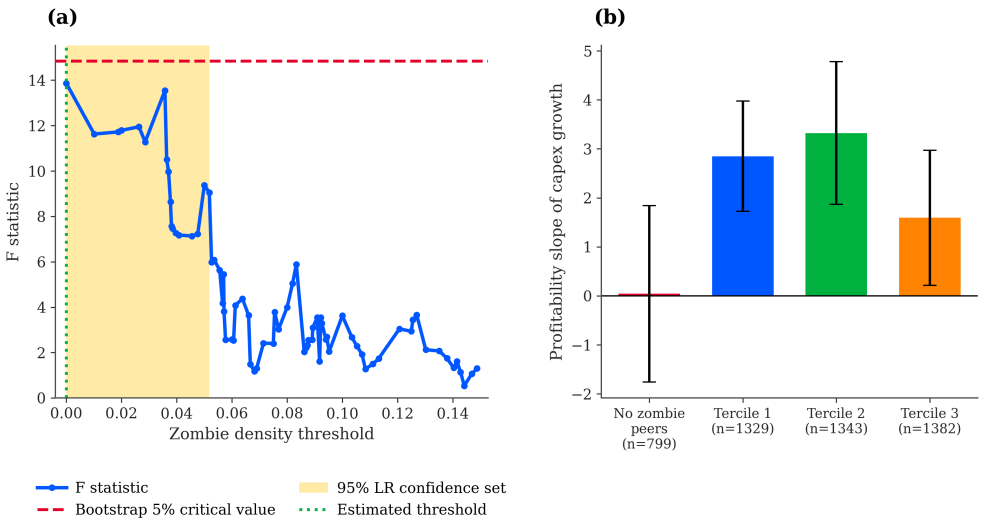


Fig. 2 Threshold test and profitability slopes. Panel (a): F statistic for a regime split at each candidate cutoff of lagged zombie density, with the bootstrap 5 percent critical value, the estimated cutoff, and the 95 percent likelihood-ratio confidence set. Panel (b): slope of capital expenditure growth on return on assets for observations without zombie peers and for terciles of positive density, with 95 percent confidence intervals based on sector-year clustering. Sample sizes are shown below each bar.

Table 6 Regime model with the cutoff at zero density. Dependent variable: capital expenditure growth. SE (F), SE (SY), and SE (S) are standard errors clustered by firm, sector-year, and sector. The p (S) and p (SY) columns are wild cluster bootstrap-t p-values with Webb weights. The low-density regime contains 799 observations and the high-density regime 4,054. The standardised differential is 0.088 standard deviations of the outcome per standard deviation of within-firm ROA, and the 95 percent confidence interval for ϕ with sector clustering is 0.74 to 4.43.

	Estimate	SE (F)	SE (SY)	SE (S)	p (S)	p (SY)
ROA slope, no zombie peers	−0.048	0.861	0.917	0.986		
ROA slope, zombie peers	2.536	0.541	0.457	0.342		
Slope differential (ϕ)	2.584	0.801	0.842	0.827	0.032	0.006
Level shift (θ)	−0.291	0.102	0.112	0.080	0.030	0.033

estimated sign reversal occurs at a return on assets of 0.113 (delta-method standard error 0.044), located at the 86th percentile of the sample. The pattern agrees with H3: firms with weak profitability invest less where zombie peers are present, and firms with strong profitability do not.

The magnitude is moderate. Moving return on assets from the 25th to the 75th percentile (a change of 0.064) raises capital expenditure growth by about 0.17 more in the regime with zombie peers than in the regime without, which equals roughly 16 percent of the within-firm standard deviation of the outcome. The within-firm R^2 rises from 0.0052 in the linear model to 0.0080 in the regime model. The Akaike criterion

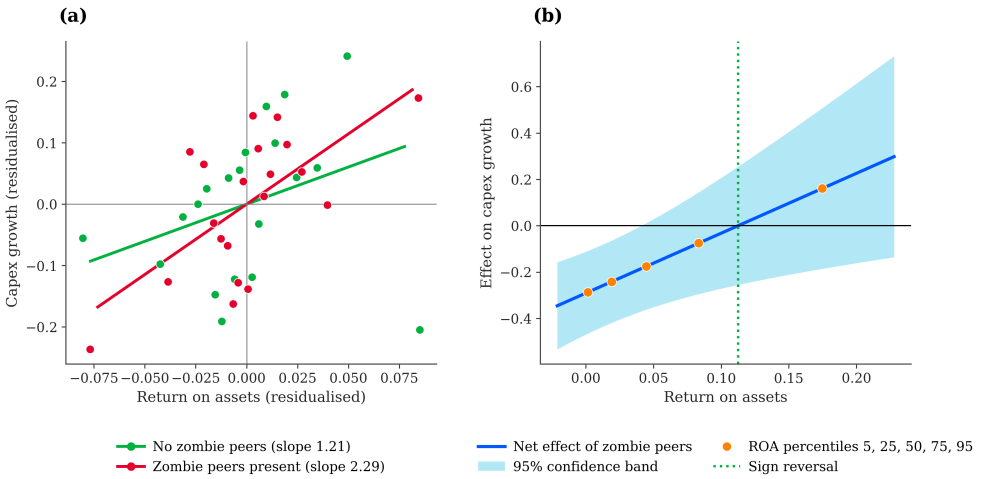


Fig. 3 Regime-dependent profitability sensitivity. Panel (a): binned scatter of capital expenditure growth against return on assets, both residualised on firm effects, year effects, and controls, separately for observations without and with zombie peers. Panel (b): net association of the zombie-peer regime with capital expenditure growth as a function of return on assets, with a 95 percent confidence band, the sample percentiles of return on assets, and the point at which the sign reverses.

Table 7 Net association of the zombie-peer regime with capital expenditure growth, by return on assets

Percentile of ROA	ROA	Net effect	SE	Lower 95%	Upper 95%
5th	0.001	−0.287	0.080	−0.466	−0.109
25th	0.019	−0.242	0.080	−0.419	−0.065
50th	0.045	−0.176	0.084	−0.362	0.011
75th	0.083	−0.076	0.099	−0.295	0.144
95th	0.175	0.161	0.156	−0.186	0.507
Sign reversal	0.113	SE 0.044; 85.7th percentile			

favours the regime model (226.5 against 236.4) and the Bayesian criterion favours the linear model (268.8 against 271.9), as expected given the additional parameters. The regime result is therefore best seen as a statistically identifiable but economically modest change in the way investment responds to profitability, and not as a large change in investment behaviour.

5.5 Alternative cutoffs

Table 8 repeats the regime estimation with cutoffs set at percentiles of the density distribution and at five percent. The slope differential is positive and significant at the ten percent level when the low regime contains the lowest quarter or third of observations or when the cutoff is five percent, but it falls and loses significance at the median and turns negative at higher cutoffs. The level shift is significant only at the zero cutoff and, marginally, at the 33rd percentile. The balanced splits at the median

and above therefore do not reproduce the result, which is consistent with a step at zero density and not with a smooth dependence of the slope on density.

Table 8 Regime estimates under alternative cutoffs of lagged zombie density. The cutoff value is in parentheses. p (S) and p (SY) are wild cluster bootstrap p-values for the slope differential with sector and sector-year clustering, and the last column is the sector-clustered bootstrap p-value for the level shift.

Cutoff	Low share	Slope low	Slope high	Differential	p (S)	p (SY)	Level shift	p level
No zombie peers (0.000)	16.5%	−0.048	2.536	2.584	0.032	0.006	−0.291	0.032
25th percentile (0.040)	26.6%	0.944	2.568	1.625	0.034	0.010	−0.080	0.332
33rd percentile (0.057)	34.5%	1.346	2.589	1.243	0.060	0.072	−0.084	0.074
Median (0.080)	50.2%	1.881	2.459	0.577	0.564	0.374	0.090	0.166
67th percentile (0.105)	67.1%	2.371	1.577	−0.794	0.108	0.246	0.096	0.128
75th percentile (0.127)	75.9%	2.392	1.416	−0.976	0.198	0.244	−0.045	0.604
Fixed cutoff (0.050)	28.3%	0.888	2.620	1.731	0.018	0.010	−0.054	0.410

6 Robustness

6.1 Definitions, identification, and outcomes

Tables 9 and 10 report the slope differential, the level shift, and the threshold test under variants of the zombie definition, the density measure, the fixed-effects structure, and the outcome. Variants of the definition include calendar-exact lags, a three-year and a one-year persistence rule, a criterion that adds a borrowing rate below the sector-year median in the spirit of subsidised credit (Caballero et al. 2008), a minimum interest burden of 0.2 percent of assets, a rank for listing history based on the full panel, exclusion of financial firms, a minimum of ten peers per sector-year, and a density weighted by interest-bearing debt. Identification variants add sector-year effects, which absorb density at the sector level so that only the differential is identified, allow sector-specific profitability slopes, combine several strict choices, and replace the sector with a finer industry classification for density.

Figure 4 plots the standardised differentials with intervals based on sector clusters. The differential is positive in all nineteen variants. Among the fourteen variants that change the definition, density measure, or identification, eleven are significant at the ten percent level with sector-clustered bootstrap, and the three exceptions are the three-year and one-year persistence rules and the finer industry classification with sector-year effects. Introducing sector-year effects or sector-specific slopes does not weaken the differential in the pooled sector classification, which indicates that it is not driven by permanent differences in profitability slopes across sectors. Among the five outcome variants, the differential is significant for asset growth, the rank of capex growth, the positive growth indicator, and, at the ten percent level, capex growth excluding the bounds, but not for capital expenditure scaled by assets. Among the fourteen definition and identification variants, the level shift is identified in twelve (it

Table 9 Slope differential under alternative designs. The differential is in outcome units, and the standardised differential is in standard deviations of the outcome per standard deviation of within-firm ROA. p (S) and p (SY) are wild cluster bootstrap p-values with sector and sector-year clustering (Webb weights), and q_{BH} is the Benjamini-Hochberg adjusted p-value across all nineteen variants. The bootstrap runs are independent of those in Table 6, so values for the baseline may differ slightly.

Variant	N	Differential	Standardised	p (S)	p (SY)	q_{BH}
Panel A: Definition of zombie status and density						
Baseline	4,853	2.584	0.088	0.024	0.006	0.057
Calendar-exact lags	4,853	2.584	0.088	0.030	0.006	0.057
Three-year persistence	4,462	1.362	0.046	0.172	0.138	0.192
One-year persistence	5,029	0.872	0.028	0.108	0.202	0.128
Subsidised borrowing rate	5,130	1.388	0.048	0.072	0.024	0.098
Interest $\geq$ 0.2% of assets	3,576	2.280	0.078	0.046	0.014	0.073
Listing history rank	4,853	2.584	0.088	0.024	0.008	0.057
Non-financial firms only	4,605	2.637	0.089	0.020	0.006	0.057
At least 10 peers per cell	4,851	2.584	0.088	0.030	0.004	0.057
Debt-weighted density	4,853	2.074	0.070	0.018	0.010	0.057
Combined strict design	3,406	2.330	0.080	0.060	0.016	0.088
Panel B: Identification						
Firm and sector-year effects	4,853	2.391	0.080	0.010	0.016	0.057
Sector-specific ROA slopes	4,853	2.619	0.089	0.006	0.010	0.057
Industry density, sector-year effects	4,823	0.958	0.032	0.458	0.336	0.458
Panel C: Outcome variable						
Capital expenditure to assets	4,911	0.056	0.044	0.360	0.280	0.380
Asset growth	5,053	0.596	0.084	0.008	0.008	0.057
Capex growth excluding bounds	4,628	2.037	0.072	0.078	0.010	0.099
Rank of capex growth	4,853	0.666	0.084	0.022	0.006	0.057
Positive growth indicator	4,853	0.882	0.064	0.040	0.026	0.069

is absorbed by sector-year effects in the other two); it is negative in all twelve and significant at the ten percent level in nine. It is essentially zero under the subsidised-rate criterion and weaker with one-year persistence and debt weighting. After adjustment for the family of nineteen variants, the false discovery rate is 0.057 for the baseline and for nine other variants, so the adjusted evidence is suggestive at the ten percent level and not decisive at the five percent level. Holm-adjusted values are larger. The linear coefficient on density has a p-value below 0.10 in none of the variants. The threshold test, which is the most demanding, has a bootstrap p-value below 0.10 with sector-year clustering in eight of the fourteen definition and identification variants.

6.2 Sectors and years

Dropping one sector at a time leaves the differential positive in every case, with estimates between 1.66 (without Utilities) and 2.81 (without Consumer Staples) and sector-level bootstrap p-values between 0.027 and 0.053. The estimate falls most when Utilities are excluded, which is notable because that sector contributes 214 of the 799 observations without zombie peers. The level shift keeps a negative sign in all eleven exclusions and is significant at the ten percent level in nine. Dropping one year at a time gives differentials between 2.05 and 4.39, with sector-level p-values below 0.10 in

Table 10 Level shift and threshold test under alternative designs. The level shift is in outcome units, and p level is the sector-clustered wild cluster bootstrap p-value. The last column gives the bootstrap p-value of the threshold test with sector-year clustering. The level shift is absorbed by sector-year effects in the two designs marked n.a.

Variant	Level shift	p level	Threshold p
Panel A: Definition of zombie status and density			
Baseline	−0.291	0.026	0.050
Calendar-exact lags	−0.291	0.042	0.044
Three-year persistence	−0.142	0.038	0.882
One-year persistence	−0.086	0.362	0.346
Subsidised borrowing rate	−0.010	0.912	0.078
Interest $\geq$ 0.2% of assets	−0.313	0.002	0.136
Listing history rank	−0.291	0.044	0.054
Non-financial firms only	−0.286	0.066	0.042
At least 10 peers per cell	−0.291	0.040	0.052
Debt-weighted density	−0.160	0.106	0.184
Combined strict design	−0.312	0.004	0.160
Panel B: Identification			
Firm and sector-year effects	n.a.	n.a.	0.070
Sector-specific ROA slopes	−0.303	0.040	0.044
Industry density, sector-year effects	n.a.	n.a.	0.438
Panel C: Outcome variable			
Capital expenditure to assets	−0.008	0.422	0.728
Asset growth	−0.069	0.026	0.170
Capex growth excluding bounds	−0.220	0.164	0.288
Rank of capex growth	−0.077	0.026	0.072
Positive growth indicator	−0.086	0.048	0.346

nine of the ten exclusions; the exception is the exclusion of 2017 ($p = 0.11$). Figure C1 in the Appendix displays both exercises.

Subperiod estimates are less precise. The differential is positive in the periods up to 2019 (1.29), from 2020 onward (5.38), and in 2020 to 2022 (13.70), but the sector-clustered bootstrap p-values are 0.12, 0.27, and 0.04, respectively. The low-density regime contains 608 observations before 2020 and only 191 afterwards, so identification of the regime contrast rests mainly on the earlier years. The large point estimates after 2020 are based on few observations and should not be interpreted quantitatively.

6.3 Peer counts, timing, and mechanical bias

Zero density is more likely in sector-years with few firms (Table 11, panel A). The share of sector-years with zero density is 46 percent among cells with 10 to 19 firms and 29 percent among cells with 20 to 39 firms, against 8 percent among cells with 40 to 79 firms and 14 percent among the largest cells. The regime thus partly reflects the number of firms in the sector-year. The differential remains positive when the sample is restricted to sector-years with at least 20 or at least 30 peers (2.34 and 2.84) and when the logarithm of the number of peers and its interaction with return on assets are added (2.56, bootstrap $p = 0.017$ at the sector level and 0.020 at the sector-year

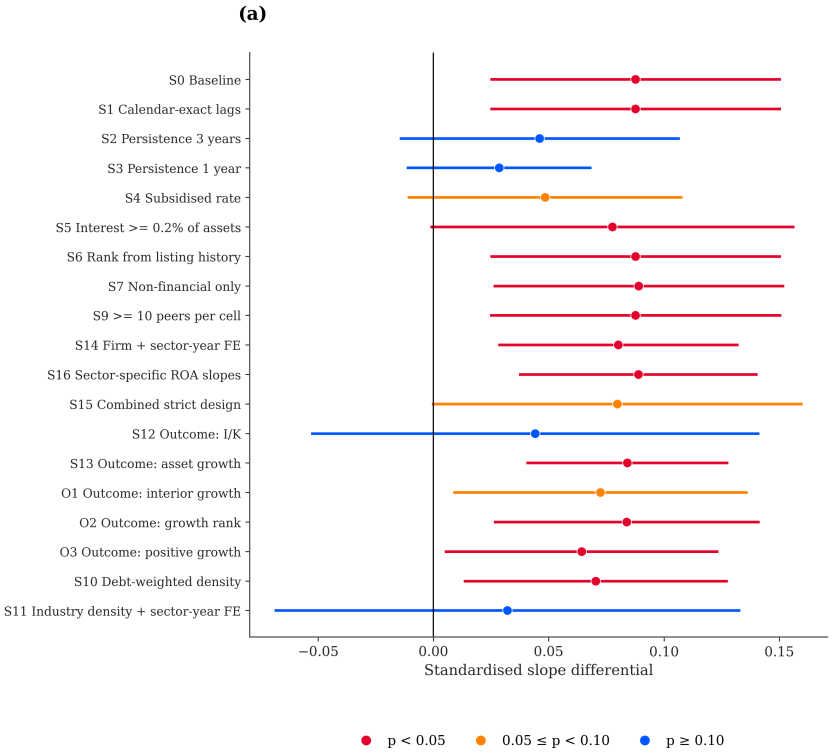


Fig. 4 Standardised slope differential (high-density minus low-density regime) under alternative designs, with 95 percent confidence intervals based on sector clustering. Colour indicates the wild cluster bootstrap p-value at the sector level.

level). The results are therefore not driven by the correlation between peer counts and the regime.

Table 12 compares lag-one density with alternative timings in horse-race specifications. A regime defined by future density (one year ahead) has no relation to the slope when it enters alone (differential 1.01, $p = 0.72$) or together with the lag-one regime (-0.24, $p = 0.69$), whereas the lag-one regime keeps a differential of about 3.0 ($p = 0.04$). Contemporaneous density likewise has no incremental explanatory power (-0.87, $p = 0.63$) when lag-one density is included, while the lag-one regime remains significant ($p = 0.013$). The two-year lag, which is positively correlated with the one-year lag, shows a differential of 1.26 ($p = 0.10$) when entered alone and 0.08 ($p = 0.92$) when both are included. Lag-one density therefore dominates the other timings, and the absence of an effect of future density argues against an explanation based on persistent unobserved industry conditions that move with both density and the slope. The correlation between the regime indicators is between 0.36 and 0.53, so these comparisons have limited power to separate adjacent years.

Table 11 Peer counts and the zero-density regime. Panel A: zero-density sector-years by number of firms in the cell. Panel B: slope differential after accounting for peer counts, with wild cluster bootstrap p-values for sector (S) and sector-year (SY) clustering.

Panel A: Zero-density sector-years		
Firms in cell	Cells	Zero share
5 to 9	1	100.0%
10 to 19	24	45.8%
20 to 39	35	28.6%
40 to 79	39	7.7%
80 or more	22	13.6%

Panel B: Slope differential					
Specification	N	Low N	Differential	p (S)	p (SY)
At least 20 peers	4,636	690	2.339	0.057	0.017
At least 30 peers	4,311	597	2.842	0.033	0.003
Peer controls	4,853	799	2.562	0.017	0.020

Table 12 Timing comparisons. Each entry is the slope differential with the sector-clustered wild cluster bootstrap p-value in parentheses. In rows marked alone, each regime enters a separate specification; in rows marked joint, both regimes enter the same specification. Corr. is the correlation between the two regime indicators.

Alternative timing	N	Corr.	Entry	Lag-1 regime	Alternative regime
Lead (density at $t+1$)	4,339	0.358	Alone	2.979 (0.037)	1.012 (0.717)
			Joint	3.012 (0.040)	−0.243 (0.687)
Lag of two years	4,300	0.533	Alone	4.128 (0.077)	1.257 (0.097)
			Joint	4.046 (0.107)	0.082 (0.923)
Contemporaneous	4,853	0.516	Alone	2.584 (0.047)	0.890 (0.777)
			Joint	2.753 (0.013)	−0.870 (0.633)

6.4 Sector heterogeneity

Whether the result differs across sectors cannot be tested formally with these data. A sector-specific estimate requires variation in the regime within the sector, and in this sample only Information Technology has at least six sector-years in each regime. For that sector the differential is positive (5.91) but very imprecise (standard error 7.95, $p = 0.49$). Other sectors have too few sector-years in one regime, as shown in the Appendix, which also reports the composition of the low-density regime. Because the low-density regime is concentrated in Utilities (68 percent of that sector's observations) and Communication Services (46 percent), while Financials contribute only two such observations, the leave-one-sector-out results above are used as the main evidence on the dependence of the result on any one sector. No claim is made about heterogeneity of the effect across sectors.

7 Discussion

7.1 Interpretation

The analysis does not find the congestion externality reported for Japan, the OECD, and China (Caballero et al. 2008; Adalet McGowan et al. 2018; Tan et al. 2016; Wang and Zhu 2021). The average association between lagged zombie density and the capital expenditure growth of healthy peers is not distinguishable from zero in a two-way fixed-effects panel. This is closer to the cautious conclusion of Schivardi et al. (2020) about the size of real spillovers than to the congestion literature, although the setting, the sample period, and the measure of investment differ.

At the same time, the way investment responds to profitability is clearly different when zombie peers are present. In their absence, investment growth is unrelated to return on assets, and where they are present it rises strongly with return on assets, while its level is lower at low and moderate profitability. This combination is what a credit-rationing account predicts: if lenders in an industry with impaired borrowers allocate scarce funds on the basis of observed profitability, weakly profitable healthy firms invest less and highly profitable firms are insulated (Holmström and Tirole 1997; Kaplan and Zingales 1997). The evidence is, however, also compatible with other explanations. Managers may revise investment plans in response to the competitive environment, demand conditions may differ systematically between industry-years with and without zombie peers, or banks may adjust lending standards in response to the same macroeconomic news that raises density. The data used here contain no loan-level or bank-level information and no direct measure of credit availability, so the credit channel is not identified. The results are therefore described as consistent with credit rationing and do not demonstrate it.

7.2 Nature of the regime

The estimated cutoff is zero density. The regimes therefore distinguish industry-years with at least one zombie peer from those with none, and they do not identify a policy-relevant share of zombie firms. The confidence set for the cutoff extends to roughly five percent, and a fixed cutoff at that level gives a similar differential (Table 8), but the estimate at the boundary, the profile across terciles in Figure 2, and the loss of the result at higher cutoffs all suggest that what matters is the presence of zombie peers and not their prevalence. A related possibility is that zero density marks industry-years with few firms or with a particular financing environment. The peer-count and timing checks reduce this concern but cannot remove it, and the concentration of the zero-density regime in Utilities and in the early years of the sample means that the contrast is identified from a limited set of sector-years.

7.3 Strength of the evidence

The strongest conclusions are the absence of a linear association and the positive slope differential, which retains its sign in every variant and is significant in most. The level shift is moderately robust. The support for a discrete threshold is borderline, with bootstrap p-values near 0.05 to 0.07 and simulated test size of about 7 to 8 percent

at the nominal 5 percent level, so the threshold itself should not be emphasised. The differential is weaker under one-year and three-year persistence rules, when density is measured at a finer industry level with sector-year effects, and when the outcome is capital expenditure scaled by assets. These failures are informative. They suggest that the result depends on the particular combination of the two-year persistence definition and the sector-level density that the analysis adopts, and that it should be interpreted as an association within that definition and not as a general feature of every plausible measure of zombie prevalence. The mechanical-bias concern of Schivardi et al. (2020) cannot be eliminated in a reduced-form design. The evidence against it consists of the lag and leave-one-out structure, the placebo timings, and the controls for peer counts, not of a design that removes the concern.

7.4 Measurement limits

Zombie density is computed from listed firms only, so unlisted firms, which may be numerous in some Vietnamese industries and are plausibly more dependent on bank credit, are not observed. The measure is also computed over eleven sectors, which are broad groupings of firms that do not necessarily compete with each other or borrow from the same lenders. Both features most likely add measurement error to the density and the regime, which would be expected to weaken estimated differences, although the direction of the bias cannot be established in general. Finer industries are available for part of the sample, but with sector-year effects the contrast loses precision. Financial firms are retained for completeness, and excluding them leaves the main estimate almost unchanged, but their classification as zombies is less informative than that of non-financial firms.

7.5 Implications

For research, the results indicate that a null linear coefficient on zombie density is not sufficient evidence that zombie prevalence is irrelevant for healthy firms, and that tests of nonlinearity in zombie-firm panels require inference that accounts for the estimated cutoff and the small number of industry clusters. In the simulation reported in the Appendix, procedures that treat the cutoff as known and resample observations reject a true linear model in more than 97 percent of cases at the 5 percent level, and a firm-clustered Wald test at the estimated cutoff rejects in 28 percent, whereas the bootstrap procedures used here reject in 7 to 8 percent. For managers and lenders in bank-based systems, the findings suggest that capital budgeting and credit assessment in industries with zombie peers lean more heavily on observed profitability, so that firms with weak reported profitability may face tighter constraints than their fundamentals alone would imply. For policy makers, monitoring how investment responds to profitability in industries with persistent distress may add information to monitoring average investment.

8 Conclusion

This study asked whether the presence of zombie firms in an industry changes the investment behaviour of financially healthy firms listed in Vietnam. Using lagged, leave-one-out sector density and a two-way fixed-effects panel of 642 firms over 2016 to 2025, the average association between zombie density and capital expenditure growth is indistinguishable from zero. The sensitivity of investment to return on assets is nevertheless much higher where zombie peers are present, and investment at low and moderate profitability is lower, with the net association turning non-negative only in the upper tail of profitability. The regime is defined by the presence of zombie peers rather than by a particular density share, and the statistical case for a discrete threshold is borderline.

The slope differential survives most alternative definitions, outcomes, subsamples, and timing checks, but it is not universal, and its economic size is modest. The results are consistent with credit rationing in industries where impaired borrowers are present, yet the firm-level financial statement data cannot identify the channel or exclude other explanations. Future work with loan-level and bank-level data, with unlisted firms, and with finer industry definitions would allow the mechanism to be tested directly and the measurement issues discussed above to be addressed. Until then, the evidence supports the view that the effect of zombie prevalence on healthy firms in a bank-based emerging market operates mainly through the allocation of investment across firms with different profitability and less through the average level of investment.

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Declarations

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- **Conflict of interest/Competing interests:** The author declares that there are no competing interests relevant to the content of this article.
- **Ethics approval and consent to participate:** Not applicable. The study uses firm-level financial statement data of listed companies and involves no human participants or animals.
- **Consent for publication:** Not applicable.
- **Data availability:** The raw data were obtained from Refinitiv under a valid licence with full copyright permissions. The processed data are available from the corresponding author upon request.
- **Materials availability:** Not applicable.
- **Code availability:** Available upon request.
- **Author contribution:** The sole author, Quoc Lap Nguyen, was responsible for the conceptualisation, data curation, formal analysis, methodology, software, visualisation, and writing of the original draft, and approved the final manuscript.

Appendix A Dynamic panel check of the linear channel

An Anderson-Hsiao first-difference instrumental-variable estimator is used to check that the absence of a linear level effect does not reflect reverse causality at a one-year horizon (Anderson and Hsiao 1981; Nickell 1981). First-differencing removes the firm effect, the lagged difference of the outcome is instrumented with its twice-lagged level, and calendar-year effects are removed by demeaning. Table A1 shows that the coefficient on the change in zombie density is small and insignificant. The estimator addresses the linear level channel only and is not a test of the regime result, which concerns the slope and is estimated with the static fixed-effects model.

Table A1 Anderson-Hsiao dynamic panel instrumental-variable estimates. Outcome: change in capital expenditure growth. Cluster-robust standard errors by firm in parentheses.

Regressor	Coefficient	p-value
Δ(capex growth)$_{t-1}$, instrumented	−0.183 (0.023)	< 0.001
Δ(zombie density)	−0.270 (0.922)	0.770
Δ(return on assets)	1.691 (0.781)	0.030
Δ(debt-to-assets)	1.027 (0.435)	0.018
Δ(ln assets)	0.747 (0.123)	< 0.001
Δ(CFO-to-assets)	0.586 (0.224)	0.009
Observations	3,308	
Firms	573	
First-stage F-statistic	6,402.5	

Appendix B Threshold test: trimming and size

Table B2 reports the estimated cutoff and the threshold test for different trimming shares, which set the minimum share of observations in each regime, for the specification with a level shift and slope shift and for a slope-only specification. The cutoff stays at or near zero and the test statistic is stable up to a trimming share of 0.20, and the bootstrap p-values are between 0.04 and 0.07 with sector-year clustering. With trimming of 0.25 or more the low regime is forced to include sector-years with positive density, the statistic falls, and the evidence disappears. The slope-only specification gives a cutoff of five percent with p-values between 0.04 and 0.13 with sector-year clustering. These results are based on a separate bootstrap run, and values differ slightly from those in the main text.

The size of the threshold test was evaluated in a simulation with 300 firms, nine years, and eleven sectors, in which density is persistent, takes the value zero in part of the sector-years, and is unrelated to the outcome, and in which common sector-year shocks affect both profitability and investment. Table B3 reports rejection rates under this linear null in 299 replications. A threshold test with the cutoff treated as known

Table B2 Sensitivity of the threshold test to the trimming share. p (SY) and p (S) are wild cluster bootstrap p-values with sector-year and sector clustering.

Trimming	$\hat{\gamma}$	Low share	F	p (SY)	p (S)
Panel A: Slope and level shift					
0.05	0.000	0.165	13.86	0.062	0.062
0.10	0.000	0.165	13.86	0.051	0.070
0.15	0.000	0.165	13.86	0.061	0.063
0.20	0.036	0.205	13.54	0.039	0.034
0.25	0.050	0.283	9.37	0.109	0.138
0.30	0.054	0.307	6.08	0.330	0.331
Panel B: Slope shift only					
0.05	0.050	0.283	8.80	0.084	0.163
0.10	0.050	0.283	8.80	0.081	0.178
0.15	0.050	0.283	8.80	0.053	0.151
0.20	0.050	0.283	8.80	0.071	0.143
0.25	0.050	0.283	8.80	0.039	0.134
0.30	0.054	0.307	5.83	0.133	0.256

and observation-level wild bootstrap rejects in more than 97 percent of replications at the 5 percent level, and a firm-clustered Wald test for the slope differential at the estimated cutoff rejects in 28 percent. The sup-F tests with the cutoff re-estimated in every bootstrap draw and clustering at the sector-year or sector level reject in 7.0 and 7.7 percent, a modest over-rejection that is considered when interpreting p-values near 0.05.

Table B3 Rejection rates under a linear null (299 simulated panels)

Test	Nominal 5%	Nominal 10%
Cutoff treated as known, observation-level wild bootstrap	0.973	0.990
Firm-clustered Wald test at the estimated cutoff	0.281	0.361
Sup-F, sector-year wild cluster bootstrap	0.070	0.167
Sup-F, sector wild cluster bootstrap (Webb weights)	0.077	0.157

Appendix C Composition of the regimes and exclusion results

Table C4 shows how the observations without zombie peers are distributed across sectors and how many sector-years have zero density. Only Information Technology satisfies the requirement of at least thirty observations and six sector-years in each regime, so a formal test of equal differentials across sectors is not feasible. Figure C1 plots the slope differential when each sector or each year is excluded in turn.

Table C4 Regime composition by sector. The low-density regime is the set of observations with no zombie peers in the previous year. Each sector has eleven sector-years.

Sector	Low obs.	High obs.	Low share	Zero-density years
Communication Services	37	43	46.2%	5
Consumer Discretionary	36	389	8.5%	1
Consumer Staples	31	396	7.3%	1
Energy	49	197	19.9%	2
Financials	2	246	0.8%	1
Health Care	48	125	27.7%	3
Industrials	147	1,375	9.7%	1
Information Technology	43	65	39.8%	4
Materials	154	703	18.0%	2
Real Estate	38	415	8.4%	1
Utilities	214	100	68.2%	7

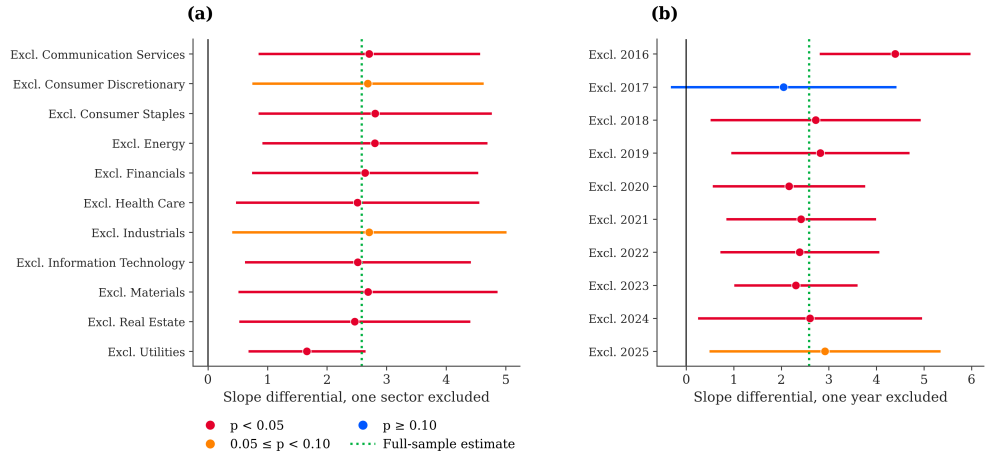


Fig. C1 Slope differential when one sector (panel a) or one year (panel b) is excluded, with 95 percent confidence intervals based on sector clustering. Colour indicates the wild cluster bootstrap p-value at the sector level, and the dotted line marks the full-sample estimate.

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