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Impaired Credit Dynamism and the Innovation Slowdown

Masami Imai
Koji Sakai
Michiru Sawada

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TOKYO CENTER FOR ECONOMIC RESEARCH
1-7-10-703 Iidabashi, Chiyoda-ku, Tokyo 102-0072, Japan

Abstract

Distortions in credit allocation can slow technological progress by sustaining unproductive firms and generating congestion that crowds out innovation from otherwise healthy firms. We study this mechanism using Japan’s banking crisis of the 1990s, linking firm-level borrowing data to the universe of patent applications with more than fifteen years of historical citation outcomes. Innovation declines more in technology fields facing greater credit distortion, with effects substantially larger for forward citations than for patent counts. Firm-level evidence reveals persistently low innovation by zombie firms and reduced innovation by healthy firms operating in zombie-intensive industries, consistent with congestion effects.

Masami Imai
TCER
and
Wesleyan University
Economics
238 Church Street Middletown, CT 06459-0007, USA
mimai@wesleyan.edu

Koji Sakai
Kyoto Sangyo University
Economics
Motoyama, Kamigamo, Kita-ku, Kyoto 603-8555, Japan
ksakai@cc.kyoto-su.ac.jp

Michiru Sawada
Nihon University
Economics
1-3-2 Kanda-Misakicho, Chiyoda-ku, Tokyo 101-8360,Japan
sawada.michiru@nihon-u.ac.jp

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Masami Imai (Wesleyan University)

Koji Sakai (Kyoto Sangyo University)

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Abstract

Distortions in credit allocation can slow technological progress by sustaining unproductive firms and generating congestion that crowds out innovation from otherwise healthy firms. We study this mechanism using Japan's banking crisis of the 1990s, linking firm-level borrowing data to the universe of patent applications with more than fifteen years of historical citation outcomes. Innovation declines more in technology fields facing greater credit distortion, with effects substantially larger for forward citations than for patent counts. Firm-level evidence reveals persistently low innovation by zombie firms and reduced innovation by healthy firms operating in zombie-intensive industries, consistent with congestion effects.

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"The banker makes possible the carrying out of new combinations, authorizes people, in the name of society as it were, to form them. He is the ephor of the exchange economy."
— *Joseph A. Schumpeter*, *The Theory of Economic Development* (1934)

"Financial markets essentially involve the allocation of resources. They can be thought of as the 'brain' of the entire economic system, the central locus of decision-making: if they fail, not only will the sector's profits be lower than they otherwise would have been, but the performance of the entire economic system may be impaired."
— *Joseph E. Stiglitz* (1993)

1. Introduction

Two Josephs—Joseph Schumpeter and Joseph Stiglitz—writing six decades apart and from distinct intellectual traditions, both underscored the central role of the financial system in directing innovation and shaping broader economic outcomes. Yet in many economies, credit does not flow to its most productive uses. In particular, underdeveloped financial systems, often dominated by state-owned banks or institutions influenced by industrial elites, frequently channel preferential credit to insiders, connected incumbents, or otherwise unviable firms.[1] Such privileged access to finance gives incumbents a competitive edge in product markets. Having secured their dominant positions, they often act to resist further financial development (Rajan and Zingales 2003; Benmelech and Moskowitz 2010).

There are two main channels through which persistent credit misallocation undermines technological progress. First, when financial systems favor unproductive firms, those with transformative ideas may be starved of the liquidity required to develop them. Second, in

[1] Credit misallocation has been a recurrent feature of financial systems across diverse institutional and historical contexts. For example, in the United States, the Savings and Loan crisis of the 1980s illustrated how regulatory forbearance allowed insolvent thrifts to continue channeling funds into low-quality real estate and speculative assets, ultimately amplifying eventual losses (Kane 1989). In South Korea, preferential access to finance under implicit state guarantees enabled large business groups (*chaebols*) to accumulate unsustainable debts, with the collapse of Daewoo in 1999 providing a salient example (Krueger and Yoo 2002; Minetti and Yun 2015). Similarly, in Mexico, related-party lending during the 1990s directed bank credit toward politically connected or insider-owned firms, many of which subsequently defaulted (La Porta, López-de-Silanes, and Zamarripa 2003). See also Claessens et al. (2008), Faccio (2006), Carvalho (2014), and Morck et al. (2011).

Schumpeterian models of creative destruction, innovation is driven by the prospect of displacing incumbents and earning temporary monopoly rents (Aghion and Howitt 1992). When subsidized incumbents that should otherwise exit remain in the market, they congest product markets, depress prices, and erode the market share and profitability of more dynamic rivals, thereby reducing the expected returns to innovation.[2]

While an extensive empirical literature documents the detrimental impact of financial underdevelopment on real economies,[3] our understanding of credit dynamism and its consequences for technological progress remain incomplete. What are the implications of sustained impairment of credit dynamism for innovation? We seek to answer this question through examination of Japan's 1990s episode of widespread credit misallocation, commonly referred to as zombie lending, as a natural experiment (Hoshi 2006; Peek and Rosengren 2005; Caballero, Hoshi and Kashyap 2008).

Why focus on Japan's experience from more than thirty years ago, especially when zombie lending has also afflicted many economies since the Global Financial Crisis of 2008? While Japan's institutional context is in some respects unique, the mechanisms we study, namely, delayed balance-sheet repair and the resulting credit subsidies to unproductive firms appear to be common across post-crisis banking systems (Baron et al. 2026). More importantly, Japan's prolonged episode of financial zombification offers a rare empirical setting that helps overcome two central measurement challenges. First, detailed firm-level borrowing data allow us to directly identify impaired credit reallocation. Second, the long time horizon since the crisis enables the use of patent-based measures of innovation quality, particularly forward citation data, without the truncation bias that complicates analyses of more recent episodes.

A first methodological challenge is that credit misallocation is difficult to observe and quantify systematically. Impaired credit dynamism typically involves lenders extending

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[2] In effect, a large presence of financially protected incumbents can push the economy into a range where intensified competition weakens innovation incentives by compressing margins and lowering the payoff to creative destruction—a concern long emphasized in classical Schumpeterian theory (Schumpeter 1942). Moreover, when credit distortions deter entry by new firms, the diminished threat of entry further weakens incumbents' incentives to innovate, consistent with the Arrow replacement effect (Arrow 1962). See Aghion et al. (2005) and Aghion et al. (2019) for subsequent work showing that both product market competition (Aghion et al. 2005) and access to finance (Aghion et al. 2019) affect innovation and productivity in a non-monotonic way.

[3] See Levine (2005) for reviews of the literature.

subsidized loans to unviable borrowers, but the contractual terms of such loans (e.g., interest rates, collateral requirements, or renegotiation agreements) are rarely disclosed. Building on Caballero, Hoshi, and Kashyap (2008), we infer credit subsidies indirectly from firms' financial statements by comparing their effective borrowing costs to benchmark rates for the highest-credit-quality borrowers. This approach provides a systematic measure of zombie lending and allows us to trace the consequences of impaired credit reallocation during Japan's lost decade.

A second methodological challenge concerns the measurement of technological progress. Identifying mechanisms of creative destruction requires distinguishing between marginal improvements and transformative inventions that have the potential to disrupt entire industries. Patent statistics provide detailed, time-stamped information on inventive activity, but because patent quality is highly heterogeneous, forward citations are widely used as a proxy for innovation quality (Griliches 1990; Trajtenberg 1990; Hall, Jaffe, and Trajtenberg 2005; Harhoff et al. 1999; Moser et al. 2016). A well-known limitation of citation-based measures, however, is truncation bias: more recent patents mechanically receive fewer citations because they have had less time to be cited (Hall, Jaffe, and Trajtenberg 2001). Recent work shows that this bias is non-random and can materially affect inference if not handled carefully (Dass, Nanda, and Xiao 2017; Lerner and Seru 2022). By focusing on patented inventions from the 1990s and observing more than fifteen years of subsequent citations, Japan's early zombification episode allows us to credibly measure innovation quality—rather than patent counts alone—while minimizing concerns about truncation bias.[4]

We construct two complementary patent datasets. The first covers the universe of patented inventions from 1992–2002, aggregated by technological field, with forward citation counts observed through 2018. The second links a subset of these patents to publicly listed

[4] Complementary measures of innovation quality do not rely on the accumulation of citations over time. Kelly et al. (2021) use textual analysis of patent documents to construct measures of technological novelty and impact that are immediately observable at the time of patenting. Kogan et al. (2017) infer innovation quality from stock market reactions to patent announcements, capturing investors' assessments of economic value in real time. Despite their very different constructions, both measures exhibit highly skewed distributions, reflecting the empirical regularity that a small fraction of inventions accounts for a disproportionate share of subsequent innovation and economic value. Moreover, both are strongly correlated with forward citations, reinforcing the interpretation of citations as capturing the economic significance of new ideas. Unlike citation-based measures, however, these approaches do not require the passage of time to assess patent quality, as market reactions and patent documents are immediately observable, whereas forward citations can take decades to fully accumulate.

firms in the Nikkei NEEDS Financial Database. This firm-level linkage allows us not only to control for firm characteristics, but also to identify the mechanisms through which impaired credit reallocation affects innovation—distinguishing between mechanical effects driven by the low innovative activity of zombie firms themselves and equilibrium effects operating through congestion and competitive pressures within industries.

Our analysis yields three main results. First, zombie lending is disproportionately concentrated in technological fields that were already less innovative prior to the rise of zombification, highlighting the importance of accounting for non-random selection across technologies. Separately, differences in baseline innovative intensity generate differential truncation in patent statistics: more innovative, low-zombie fields produce larger volumes of patents and citations and therefore experience more severe mechanical truncation for later application cohorts, whereas truncation is substantially less pronounced in technologically stagnant, high-zombie fields. Second, exploiting within-field variation over time, we find that greater exposure to zombie lending is associated with economically large declines in innovation. Notably, citation-weighted measures of innovation respond roughly twice as strongly to credit misallocation as simple patent counts, reinforcing the interpretation that impaired credit dynamism disproportionately undermines the creation of high-quality, potentially disruptive inventions central to creative destruction. Third, firm-level analyses reveal two reinforcing channels through which impaired credit reallocation depresses innovation. Zombie firms innovate substantially less than healthy firms, and—consistent with congestion effects—the innovative advantage of healthy firms erodes with increased zombie prevalence. Quantitatively, the estimated interaction effects imply that the innovation gap between non-zombie and zombie firms vanishes once the zombie share reaches roughly 35 percent—a level well within the observed range in many industries during the late 1990s.

We address three methodological concerns. First, how zombie lending is defined. Following Hoshi (2000) and Caballero, Hoshi, and Kashyap (2008), we identify zombie firms based on implicit credit subsidies inferred from borrowing costs rather than ex post performance. This approach focuses on distortions in credit allocation itself, rather than mechanically linking financial distress status to poor outcomes. To guard against potential misclassification, we also consider a stricter definition that combines credit subsidies with balance-sheet indicators of financial distress, following Acharya et al. (2024). Second, because zombie lending is disproportionately concentrated in technological fields that were already less innovative, selection poses a serious concern. We address this issue using within-technology variation over time, extensive controls, and placebo tests based on non-corporate patenting activity, which responds to common technology shocks but is plausibly insulated from bank lending behavior.

Finally, we confront concerns about truncation bias in patent citations. Because Japan's zombification episode occurred earlier than post-GFC episodes studied elsewhere, our data allow us to observe more than fifteen years of forward citations for patents filed in the 1990s. We exploit this long horizon to conduct extensive robustness checks that vary citation windows and sample definitions, following the recommendations of Dass, Nanda, and Xiao (2017) and Lerner and Seru (2022). In addition, to ensure that our results are not driven by differential truncation associated with surges in patenting, we re-estimate our main specifications after excluding industries and technology classes that experienced unusually rapid growth in patenting activity. Beyond addressing truncation concerns, this restriction also mitigates selection issues by avoiding comparisons between rapidly innovating fields—where zombie lending is rare—and chronically low-innovation technologies. These exercises confirm that our findings are not artifacts of right-censoring, unequal citation exposure, or non-random selection across technologies.

Related Literature

Our paper contributes to several strands of the literature on finance, credit allocation, and innovation. Foundational work on finance and economic growth emphasizes that financial development promotes growth primarily by improving capital allocation and total factor productivity rather than by expanding the quantity of investment (Beck, Levine, and Loayza 2000; Wurgler 2000). Conventional measures of financial development, however, largely capture the scale of intermediation rather than the quality or dynamism of credit allocation.

Closely related studies examine settings in which policy-induced changes improved the efficiency of credit reallocation. A prominent example is the literature on US banking deregulation, which shows that the removal of state branching restrictions accelerated credit flows from low- to high-quality firms and facilitated Schumpeterian creative destruction, leading to productivity growth (Jayaratne and Strahan 1996; Black and Strahan 2002; Cetorelli and Strahan 2006; Kerr and Nanda 2009; Bai, Carvalho, and Phillips 2018; Herrera, Minetti, Schaffer 2025).[5] While this literature highlights the importance of credit reallocation, isolating its effects on innovation is challenging because banking deregulation are multifacted and affects financial markets through multiple channels simultaneously, including competition, firm entry, and organizational structure.[6]

Our paper is most closely related to the literature on zombie lending and productivity slowdowns. Caballero, Hoshi, and Kashyap (2008) show that subsidized lending to distressed firms during Japan's 1990s stagnation distorted resource allocation and depressed productivity growth by impeding reallocation and exit.[7] We build on this framework by studying innovation as a distinct and complementary channel through which impaired credit dynamism affected Japan's long-run economic performance.[8]

Recent and concurrent work examines the effects of post–Global Financial Crisis zombification on innovation in Europe. Schmidt et al. (2024) and Ascani and Balachandran Nair (2025) study Spain and Italy, respectively, documenting declines in patenting activity following the rise of zombie firms. Our approach differs in two important respects. First, we follow Caballero, Hoshi, and Kashyap (2008) in defining zombie firms based on implicit credit subsidies

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[5] Others also examine financial liberalization or banking reform in other countries as a quasi-experimental setting to identify credit allocation efficiency (e.g., Bertrand, Schoar, and Thesmar 2007, Varela 2018).

[6] The effects of banking deregulation on innovation are nuanced. Amore, Schneider, and Žaldokas (2013) find that interstate banking deregulation increases both the quantity and quality of innovation by public manufacturing firms. In contrast, Cornaggia et al. (2015) show that while deregulation reduces innovation by public firms at the state level, it increases innovation by private firms, as greater bank competition improves credit access and allows small, innovative firms to remain independent. Chava et al. (2013) distinguish between intrastate and interstate branching deregulation, finding that the former reduces, while the latter increases, the level and riskiness of innovation by young and private firms. Hombert and Matray (2017) further document that intrastate deregulation dampens innovation among small firms by weakening lending relationships and limiting inventor mobility with little effect on large firms.

[7] Others build on the seminal work of Caballero, Hoshi and Kashyap (2008) to examine Japan's zombification episode (e.g., Kwon, Narita, and Narita (2015), Imai (2016), Cheung and Imai (2024), and Sakai and Uesugi (2024).

[8] Related work applying the Caballero–Hoshi–Kashyap framework to post-GFC Europe includes Banerjee and Hofmann (2022), McGowan, Andrews, and Millot (2017), and Acharya et al. (2024). See Acharya et al. (2022) for a review of the literature.

rather than balance-sheet distress, allowing us to capture distortions in credit allocation rather than poor performance per se. Second and more importantly, Japan's earlier zombification episode provides a much longer horizon, enabling us to measure innovation quality—rather than patent counts alone—using unusually long forward citation data. This distinction is crucial for identifying creative destruction, as simple patent counts conflate trivial inventions with transformative innovations that drive industry disruption.

The remainder of the paper is organized as follows. Section 2 describes the institutional background of credit misallocation in Japan and outlines our measure of zombie lending. Section 3 presents the patent data and discusses their construction and key measurement issues. Section 4 links credit misallocation to patent outcomes by technology class and describes the key features of the linked data. Section 5 formally presents aggregate evidence on the innovation effects of impaired credit dynamism using technology-field–level analyses. Section 6 provides firm-level evidence on congestion externalities and heterogeneous responses to zombie lending. Section 7 concludes.

2. Credit Misallocation in Japan

2.1. Institutional Background

The phenomenon of zombie lending emerged after the burst of Japan's asset-price bubble in 1990–91, which triggered a sharp decline in land and stock prices. Because real estate served as collateral for much of bank lending—and because banks themselves held large equity and property positions—the collapse destroyed bank capital and rendered many firms insolvent. Loan growth and investment contracted sharply (Peek and Rosengren 1998, 2000; Imai and Takarabe 2011; Amiti and Weinstein 2011, 2018; Gan 2007a, 2007b). At the same time, under-capitalized banks, reluctant to recognize losses, rolled over loans to failing borrowers to keep them nominally current, allowing "zombie firms" to survive not through profitability but through banks' willingness to continue lending on favorable terms (Peek and Rosengren 2005; Caballero, Hoshi, and Kashyap 2008).[9]

[9] The *main bank* system—with its long-term relationships, cross-shareholdings, and managerial ties—reinforced these incentives by making banks hesitant to abandon long-standing clients (Hoshi, Kashyap, and Scharfstein 1990; Morck and Nakamura 1999; Aoki and Patrick 1994).

As Imai (2019) emphasizes, zombie lending reflected not only weak bank balance sheets but also a regulatory and political environment that suppressed market discipline and favored forbearance to avoid widespread bankruptcies and unemployment.[10] The government's reluctance to force recognition of loan losses reflected broader efforts to contain the political and fiscal costs of full-scale restructuring. Only with the Takenaka Plan (2002–2005) did the government impose stricter accounting standards and begin to compel banks to recognize losses. Japan's experience thus illustrates how regulatory forbearance and moral hazard—amplified by political incentives to conceal losses—can transform a temporary asset-price collapse into a decade-long episode of capital misallocation.[11]

2.2. Measuring Credit Misallocation

How should one identify credit misallocation? By nature, misallocation is rarely transparent to outside observers with banks rolling over or restructuring loans to otherwise insolvent borrowers. This opacity is compounded by the fact that banks possess private information about borrower viability and may deliberately obscure the extent of evergreening, making it difficult to distinguish between efficient liquidity support and inefficient forbearance. Moreover, assessing its macroeconomic consequences requires disentangling these effects from contemporaneous shocks to productivity, demand, or financial conditions. These challenges underscore the need for detailed micro-level data and creative empirical strategies.

One can identify zombie lending based on the prevalence of financially distressed or insolvent firms.[12] While convenient, such performance-based definitions risk circularity: by

[10] The government's 1996 blanket deposit guarantee shielded creditors from losses, weakening depositor monitoring (Imai 2006). Regulators tolerated "regulatory capital arbitrage," allowing banks to meet Basel standards with deferred tax assets and subordinated debt rather than genuine equity (Hoshi and Kashyap 2004). Political connections and *amakudari* appointments of former officials to bank boards further delayed corrective action (Horiuchi and Shimizu 2001).

[11] Across countries, a common mechanism behind delayed bank resolution is the incentive of regulators—and ultimately politicians—to avoid upfront fiscal costs. Closing insolvent banks or recapitalizing healthy ones requires immediate cash outlays to repay depositors or inject public capital, actions that make losses explicit to taxpayers. A large body of work shows that these fiscal and political incentives encourage regulatory forbearance, allowing weak banks to evergreen loans to distressed borrowers and creating the conditions for zombie lending. This pattern is documented in the U.S. thrift crisis (Kroszner and Strahan 1996) and the European banking sector (Acharya et al. 2020).

[12] "Zombie firms" are frequently identified as mature firms exhibiting persistently weak financial performance—often measured using the interest coverage ratio (ICR), Tobin's q, leverage, or sales growth. An ICR below one over multiple years signals chronic weakness and dependence on forbearance.

construction, they equate poor performance with zombie status. This endogeneity motivates identifying distortions in lending behavior directly, as in Caballero, Hoshi, and Kashyap (2008).

Caballero, Hoshi, and Kashyap (2008) detect subsidized credit in two steps. First, they establish a conservative lower bound for required interest payments by combining prevailing short- and long-term prime rates with the lowest coupon rates on convertible bonds observed in the preceding five years—benchmarks attainable only by the most creditworthy firms. They then compare actual interest payments to this benchmark, normalizing by total borrowing to construct an "interest rate gap." Firms whose observed payments fall below this bound are classified as zombies, indicating bank subsidization through evergreening or restructuring. Because this approach relies on loan terms rather than ex post firm performance, it isolates distortions in credit allocation without mechanically conflating them with low profitability or productivity. Our analysis uses their approach.[13]

Our primary data source, Nikkei Financial QUEST, provides detailed financial statements for all firms listed on Japanese stock exchanges, including the over-the-counter market. This coverage is considerably broader than that of Caballero, Hoshi, and Kashyap (2008), who restricted their sample to firms listed on the first and second sections of the Tokyo Stock Exchange. Thus, we provide a more comprehensive measure of credit misallocation. From this database, we obtain information on each firm's short-term bank loans (maturity of less than one year), long-term bank loans (maturity exceeding one year), and total bonds outstanding, including both convertible bonds (CBs) and warrant-attached bonds. To measure the degree of credit subsidization, we complement these data with prime lending rates published by the Bank of Japan (https://www.boj.or.jp/statistics/dl/loan/prime/primeold2.htm) and subscriber yields for convertible bonds collected from various issues of *Kin'yu Nenpo* (Annual Report on Finance) published by the Ministry of Finance.

Using these data, we construct annual measures of the share of zombie firms in Japan's corporate sector. Figure 1 replicates the original analysis of Caballero, Hoshi, and Kashyap (2008, Figure 1, p. 1945) using our expanded firm coverage and updated data and reveals a

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[13] See Online Appendix for a detailed description of the zombie-firm identification method developed by Caballero, Hoshi, and Kashyap (2008).

remarkably similar pattern. After the collapse of the asset-price bubble in the early 1990s and the sharp recession of 1992–93, the share of zombie firms rises sharply beginning around 1993. In the top panel of Figure 1, where firms are weighted equally, the zombie share fluctuates between 5 and 15 percent during the late 1980s and early 1990s, then accelerates through the mid-1990s, exceeding 25 percent and reaching roughly 30 percent by the early 2000s. Notably, much of this increase occurs during a period of economic stabilization and partial recovery beginning in 1994–95, indicating that the expansion of zombie lending was not merely a transitory response to recession but persisted well into the recovery phase. This persistence underscores the enduring nature of Japan's credit misallocation problem and the slow pace of financial restructuring.

From the standpoint of congestion spillovers, however, a size-weighted measure is arguably more relevant. The bottom panel of Figure 1, which weights firms by total assets, displays a similar time pattern but at lower levels, indicating that approximately 15 percent of corporate assets were tied up in zombie firms during the late 1990s. Both measures are substantially lower throughout the 1980s and the early phase of the post-bubble downturn.

When we disaggregate the analysis by industry (Figure 2), our results continue to closely mirror those reported by Caballero, Hoshi, and Kashyap (2008). Following their approach, we group firms into six broad sectors—manufacturing, construction, real estate, trade, services, and all firms combined—and compute the asset-weighted share of zombie firms within each sector. The resulting industry-level series closely replicate the time-series patterns in their Figure 3 (p. 1951): zombie prevalence rises sharply in the early 1990s and remains elevated across all sectors for the remainder of the sample. Consistent with their findings, the problem is more pronounced in nonmanufacturing industries—particularly construction, real estate, and services—where zombie asset shares increase substantially following the bubble's collapse. In contrast, the manufacturing sector exhibits a more modest rise, likely reflecting greater exposure to international competition and fewer opportunities for banks to sustain unviable borrowers through domestic protection. The purpose of replicating these figures is twofold purposes: to validate the consistency of our data with earlier studies and to establish a benchmark for interpreting our subsequent analysis.[14]

3. Patent Data and the Measurement of Innovation

Measuring technological knowledge is inherently difficult, but patent data provide one of the few standardized and widely used sources for observing innovative activity (Griliches 1990; Hall, Jaffe, and Trajtenberg 2001). Patent records contain rich, time-stamped information on the technological content and ownership of inventions, including application and grant dates, technology classifications, and references to prior patents and non-patent literature. At the same time, the informativeness of patent statistics depends critically on the institutional environment in which patents are examined and recorded.

This section first describes truncation bias in the widely used NBER patent database for the United States as a benchmark, then explains the construction and institutional features of the Japanese patent data, and finally highlights key features of the data that are central for interpreting patent-based measures of innovation and assessing truncation bias across settings.

3.1. Truncation Bias in the NBER Patent Database

Truncation bias in the NBER patent database has been extensively documented (Hall, Jaffe, and Trajtenberg 2001, 2005; Harhoff et al. 1999; Dass, Nanda, and Xiao 2017; Lerner and Seru 2022). The issue arises because the NBER database contains only granted patents. While inventive activity is often dated by application year to better capture the timing of innovation, applications still pending at the end of the sample are unobserved, generating truncation that becomes more severe for later cohorts.

A related problem arises in citation data. Because citations can be observed only from patents granted within the sample window, even older patents have incomplete citation counts, with missing citations becoming increasingly severe for patents closer to the cutoff

[14] Using the same data and zombie definitions, we also replicate the central empirical findings of Caballero, Hoshi, and Kashyap (2008) regarding the real effects of zombie lending. In particular, we confirm that otherwise healthy firms exhibit lower employment and investment in industries with higher zombie prevalence, consistent with congestion and competitive spillovers operating through product and factor markets (results not shown to conserve space). These findings closely mirror those reported in Caballero, Hoshi, and Kashyap (2008, Table 3) and provide further validation that our data and measurement strategy capture the same underlying phenomenon documented in their study.

year. As a result, patents near the end of the sample mechanically appear less influential, not because they are of lower quality, but because future citing patents fall outside the dataset. These features generate systematic truncation bias in citation-based measures of innovation quality.

Recent work demonstrates that these biases are large, non-random, and consequential. Dass, Nanda, and Xiao (2017) show that truncation in the NBER-2006 data leads to severe understatement of both patenting activity and citations for cohorts in the early 2000s, even after applying standard corrections based on historical grant-lag distributions. Lerner and Seru (2022) extend this analysis using a longer observation window and show that both patent and citation truncation vary systematically across technologies, firms, and regions. Importantly, they document that commonly used adjustment methods correct only a small fraction of the bias and that the resulting measurement error is correlated with firm characteristics such as size, R&D intensity, and financial constraints. As a result, empirical studies may spuriously attribute patterns in patenting or citations to economic mechanisms when they instead reflect truncation-induced measurement error.

3.2. Institutional Features of the Japanese Patent System

A defining institutional feature of the Japanese patent system is pre-grant publication. All patent applications are automatically disclosed eighteen months after filing, regardless of whether they are subsequently examined or registered. As a result, researchers can observe application activity directly, in principle avoiding truncation associated with grant lags. In practice, however, application data alone are not a reliable measure of realized innovative output.

This limitation arises from Japan's request-for-examination system, introduced in 1971. Unlike the U.S. system, in which all patent applications are examined automatically, applicants in Japan must affirmatively request substantive examination within a specified period after filing—currently within three years, and up to seven years prior to 2001. Because filing fees are relatively low, firms may file applications strategically before the economic value or novelty of an invention is fully established and subsequently choose not to pursue examination. As a consequence, only a subset of applications is examined, and an even smaller subset is ultimately registered as patents.

For this reason, patent grants—rather than applications—remain the appropriate unit for measuring realized inventive activity, paralleling standard practice in the NBER-based U.S. patent literature. At the same time, the ability to observe applications in the Japanese data is valuable for diagnosing truncation patterns and for selecting sample endpoints that ensure sufficient post-application observation.

The request-for-examination system also implies that the lag between application and registration is both long and highly variable. Median application-to-registration lags are on the order of three to four years, and a nontrivial share of patents experience lags of five to eight years or longer, particularly in technologically crowded fields (Goto and Motohashi 2007; Nagaoka, Motohashi, and Goto 2010). These institutional features play a central role in shaping truncation patterns in Japanese patent data and motivate careful choices regarding innovation timing and sample windows in empirical analysis.[15]

3.3. Institute of Intellectual Property (IIP) Patent Database

Our patent data come from the Institute of Intellectual Property (IIP) Patent Database, developed under the leadership of Akira Goto. The IIP Patent Database was designed to provide a structured, research-ready version of Japanese patent records based on the Japan Patent Office's standardized administrative data ("Seiri-Hyojunka Data") to facilitate empirical research on innovation (Goto and Motohashi 2007; Nagaoka, Motohashi, and Goto 2010). While the underlying JPO records extend back to the early twentieth century, the IIP database focuses on applications from 1964 onward, reflecting the availability of consistent bibliographic fields and International Patent Classification (IPC) codes. The first public release, covering 1964–2004 and approximately nine million applications, became available in 2005; subsequent releases extend

[15] The request-for-examination system, and the longer median application-to-registration lags that result, are not unique to Japan. For example, under the European Patent Convention, European patent applications are typically published at 18 months after filing or priority, and applicants must file a request for examination and pay an examination fee within a fixed period after publication of the European search report for the application to proceed toward grant. These parallels underline that truncation and variation in application-to-grant timing are common features in multiple advanced patent systems, reinforcing the importance of carefully aligning timing and measurement when using patent data to study innovation.

coverage forward in time. We use the 2020 release, whose documentation is the most complete and which covers all published patent applications and registered patents through September 16, 2019.

The IIP database reflects key institutional features of the Japanese patent system by recording information separately for applications, examination requests, and registrations. It includes application dates, IPC-based technology classifications, applicant and inventor information, examination request dates, and registration dates for granted patents. Citation information in the IIP Patent Database is compiled from search reports prepared by patent examiners rather than by applicants themselves, mitigating concerns about strategic citation behavior—a feature shared with the European patent system. Substantial effort was devoted to cleaning and harmonizing applicant identities, as the original JPO data do not contain stable applicant identifiers and include numerous name variants—an issue analogous to that documented for raw USPTO data (Goto and Motohashi 2007; Lerner and Seru 2022).

Do the IIP patent data exhibit truncation patterns similar to those documented for the NBER patent database? To address this question, we replicate the diagnostic exercises proposed by Lerner and Seru (2022). Their analysis of the NBER-2006 patent database plots: (i) the number of patents by grant year, (ii) the number of successful patent applications—defined as applications that eventually result in grants—by application year, (iii) the total number of applications (successful plus unsuccessful), and (iv) forward citations per patent by application year. Together, these panels provide a clear visual diagnosis of truncation arising from grant lags and finite citation windows.

Figure 3 replicates these diagnostics using the IIP Patent Database. When patents are organized by grant (registration) year, patent counts evolve smoothly over time, with no evidence of a sharp end-of-sample decline (Panel A). By contrast, when granted patents are reorganized by application year, the series declines sharply toward the end of the sample, approaching zero (Panel B). This decline begins around 2015—approximately three years before the end of the observation window—consistent with median application-to-registration lags of three to four years and substantially longer lags for a nontrivial share of patents under Japan’s request-for-examination system.

Citation-based measures exhibit an even earlier and more pronounced truncation pattern. Forward citations per patent, when plotted by application year, peak for relatively early cohorts and then decline steadily, with citation intensity for recent cohorts falling close to zero (Panel D). This decline begins around 2001—roughly eighteen years before the end of the citation window—reflecting the compounded lags associated with the registration of cited patents, the subsequent grant of citing patents, and the finite observation window for citations.

Panel C plots the total number of patent applications, defined as the sum of successful and unsuccessful applications. In contrast to the U.S. data, where total applications continue to rise through the end of the sample, the IIP data exhibit a sharp decline beginning in 2018. This decline is fully mechanical and reflects Japan's eighteen-month pre-grant publication rule: because the IIP database used in this study was compiled in September 2019, applications filed after early 2018 had not yet reached the publication stage and are therefore unobserved (Goto and Motohashi 2007).

To further illustrate citation truncation, Figure 4 plots the distribution of forward citations by citation lag for patents applied for in different years. Earlier cohorts (e.g., 1990 and 1995) exhibit long citation tails, with meaningful citation activity extending beyond twenty years after application. By contrast, for more recent cohorts (notably 2005 and 2010), citations drop sharply after relatively short lags and approach zero within ten years. This pattern reflects a finite observation window rather than faster technological obsolescence. Accordingly, our baseline analysis restricts attention to patent applications filed no later than 2002, ensuring sufficiently long post-application horizons for measuring innovation quality.[16]

4. Creation of Linked Patent–Zombie Datasets

To examine how credit misallocation affects innovative activity, we construct two complementary datasets that link patent outcomes to measures of zombie lending. Zombie lending is measured at the industry-year level following Caballero, Hoshi, and Kashyap (2008), while innovation outcomes are observed at the firm and technology levels. Because these objects are defined at different levels of aggregation, empirical analysis requires mapping

[16] Robustness to alternative application and citation windows is later examined.

industry-level credit distortions into the domains where innovation is observed. This mapping underlies both datasets used in our analysis.

4.1 Linking Patent Applicants to Firms

We begin by linking patent records from the Institute of Intellectual Property Patent (IIP) Database to firm-level financial information from the Nikkei NEEDS Financial Quest (FQ) database. This task is nontrivial because patent applicants are not recorded using stable firm identifiers. The same firm may appear under multiple names due to variation in spelling, abbreviations, character sets (Japanese, Chinese, and Roman), typographical conventions, and organizational changes such as mergers, spin-offs, and rebranding (Yamashita and Yamauchi 2019).

To address these challenges, we rely on a company name dictionary compiled by the National Institute of Science and Technology Policy (NISTEP). The 2019 version of this dictionary assigns unique firm identifiers (NISTEP IDs) and standardized names to major patent applicants—defined as firms that have filed at least 100 patent applications since 1970—which together account for more than 90 percent of corporate patent applications in Japan. Crucially, the dictionary records historical organizational changes, allowing past firm names to be linked to current identifiers.

Using NISTEP IDs as the primary key, we match patent applicants to firms in the Nikkei FQ database. We further refine this matching using information on company names, security codes, and headquarters locations at the municipal level. Because firm names in the Nikkei FQ database can be reliably traced back only to 1997, we identify earlier name changes using the *Handbook of the Tokyo Stock Exchange*. This procedure allows us to match approximately 76 percent of patent applications filed by firms with NISTEP IDs, corresponding to about 60 percent of all patent applications in the IIP database.

4.2 Sample Period

Our baseline patent sample consists of applications filed between 1992 and 2002. The start year is chosen to avoid the unusual macroeconomic and policy environment of the early 1990s, when tight monetary policy by the Bank of Japan and regulatory restrictions on commercial real estate lending precipitated the collapse of the asset price bubble and a deep recession (Okina, Shirakawa, and Shiratsuka 2001; Sonoda and Sudo, forthcoming). Because this episode represents a sharp macroeconomic regime break rather than a stable pre-crisis baseline, earlier years do not provide an appropriate counterfactual. The end year is chosen to mitigate truncation bias in forward patent citations (Section 3).[17]

4.3 Firm-Level Dataset

Using the matched data, we first construct a firm-level dataset by restricting attention to patents whose applicants can be linked to publicly listed firms in the Nikkei FQ database. For these firms, we aggregate patent counts and forward citations to the firm-application year level and merge them with balance-sheet information and measures of zombie status. This dataset allows us to study firm-level innovation behavior and congestion externalities in section 6.

A limitation of this firm-level analysis is that it necessarily excludes patents granted to firms that are not publicly listed and therefore do not appear in the Nikkei FQ database. Because credit misallocation affects the broader corporate sector, including private firms and other entities for which firm-level financial data are unavailable, analyses restricted to listed firms may understate the aggregate impact of impaired credit reallocation on innovative activity.

4.4 Technology-Class–Level Dataset

To address this limitation, we exploit the fact that all patents in the IIP database are assigned to detailed technology classes based on their International Patent Classification (IPC) codes, regardless of the ownership or listing status of the applicant. This feature allows us to construct a technology-class–level dataset that includes the universe of patented inventions, encompassing patents by listed firms, unlisted firms, and other corporate entities (excluding patents with foreign applicants).

The primary dataset for our aggregate analysis is therefore constructed at the technology-class level. Each patent is assigned to one of 120 technology classes based on its primary IPC code. For each technology class, we aggregate patent counts and forward citations

[17] Japan experienced a severe banking crisis in 1997–98, marked by major financial institution failures and government interventions. While our baseline sample extends through 2002, our results do not hinge on including the late-1990s crisis period. In robustness checks, we re-estimate all specifications using a sample ending in 1997, prior to the banking crisis, and obtain qualitatively similar results.

by application year using the universe of patents filed between 1992 and 2002. This dataset enables us to estimate the aggregate effects of credit misallocation on innovation across technological fields, rather than only the responses of a selected subset of publicly listed firms.[18]

4.5 Mapping Industry-Level Credit Misallocation into Technology Fields

Because zombie lending is measured at the industry-year level and varies over time, while patents are classified by technology rather than industry, we translate industry-level credit misallocation into technology-level exposure using a shift–share approach. Using the firm-patent linkage described above, we identify the industries in which patenting firms operate and compute, for each technology class k and industry j, the share of patents originating from industry j, denoted w_{kj}.

These weights are computed using the full matched patent–firm sample pooled over 1991–2001—one year prior to the estimation window—and are held fixed over time, reflecting the long-run industry composition of innovation within each technology class. Fixing the weights avoids mechanically linking contemporaneous patenting outcomes to exposure measures and follows standard practice in shift–share designs.[19]

We then define technology-level zombie exposure in year t as:

$$z_{kt} = \sum_j w_{kj} \, z_{jt},$$

where z_{jt} denotes the asset-weighted share of zombie firms in industry j in year t. Under this construction, variation in technology-level exposure arises solely from changes in

[18] Nano-technology (IPC B82), one of the 120 technology classes, is only defined and available starting in 1997, but our technology-class level panel data are virtually balanced with 1,314 technology-class–year observations (119 x 11 + 5 = 1314).

[19] Online Appendix Table A1 provides transparency on the industry–technology mapping underlying the construction of our exposure measures. For each of the 120 IPC technology classes, the table reports the two industries with the largest patenting shares, w_{kj}. Patent activity within technology classes is highly concentrated: only 13 industries ever appear as the dominant patenting sector, and this set expands to just 16 industries when second-ranked sectors are included. Electric machinery is by far the most pervasive industry, appearing among the top two patenting sectors in 71 technology classes, followed by non-electric machinery and chemicals. Overall, the implied mappings are economically intuitive—for example, machinery-related technologies are primarily associated with manufacturing industries, while construction- and real-estate-related technologies draw disproportionately from nonmanufacturing sectors—confirming that the exposure measures capture meaningful industry–technology linkages rather than mechanical artifacts of aggregation.

industry-level zombie prevalence over time, while the mapping from industries to technologies remains stable.[20]

5. Zombie Lending Exposure and Innovation across Technology Classes

5.1. Descriptive Patterns

We begin by documenting several descriptive patterns in the technology class–level data that motivate our empirical strategy and underscore the importance of accounting for selection across technologies, differential truncation in citation data, and heterogeneous underlying innovation trends.

Figure 5 plots average patent grants (top panel) and average forward citations (bottom panel) over 1992–2002 against the industry-weighted average zombie exposure of each technology class, computed using one-year-lagged values over 1991–2001. Each point corresponds to one of the 120 IPC technology classes. The figure reveals a clear negative cross-sectional relationship: technology classes more exposed to zombie lending exhibit substantially lower patenting activity and fewer citations on average. At face value, this pattern suggests that innovation is weaker in technological fields associated with industries characterized by persistent credit misallocation.

However, these cross-sectional correlations also highlight an important selection issue. Figure 6 plots total patent grants (top panel) and total forward citations (bottom panel) by application year for technology classes with low versus high exposure to zombie lending, where technology classes are classified based on whether their industry-weighted average zombie share over 1991-2001 lies below or above the median. Low-zombie technology classes are substantially more innovative throughout the sample period, both in terms of patenting volume and citation intensity, mirroring the pattern in Figure 5. Importantly, these differences in innovative output were already visible well before zombie lending became a prominent feature of Japan's banking system in the early 1990s. That is, technology classes with high

[20] This approach is closely related to that used by Autor et al. (2020), who study the effects of trade shocks on innovation by weighting industry-level shocks by the industry composition of patenting within each technology, and by Hombert and Matray (2017), who map financial shocks into technology-level innovation outcomes using patent data. As in these studies, our objective is not to assign each technology to a single industry, but to translate industry-level credit distortions into technology-level exposure measures based on observed patterns of innovation.

zombie exposure are not simply those that subsequently "fell behind," but rather those that were already characterized by lower innovative dynamism. This pre-existing heterogeneity cautions against interpreting the cross-sectional patterns in Figure 5 as causal effects of zombification on innovation.

At the same time, Figure 6 reveals a closely related pattern that raises additional econometric concerns, closely connected to the non-classical measurement error and endogeneity issues emphasized by Dass, Nanda, and Xiao (2017) and Lerner and Seru (2022). Forward citations in low-zombie technology classes rise sharply during the 1990s and then decline rapidly in the early 2000s, whereas citation activity in high-zombie fields remains comparatively flat throughout. Part of this divergence reflects mechanical truncation, as discussed in Section 3: more innovative fields generate larger volumes of patents and citations and therefore experience more severe end-of-sample citation truncation. At the same time, the pronounced rise in citations during the 1990s suggests that some low-zombie technology classes might have been "hot" fields whose innovative activity surged for reasons unrelated to credit conditions—for example, due to underlying technological breakthroughs or shifts in demand.

As a result, selection, heterogeneous innovation trends in the 1990s, and subsequent citation truncation in the early 2000s interact in ways that can be misleading. Absent careful treatment, naive comparisons across technology classes or over time could lead to faulty causal interpretations—for example, that low-zombie fields performed strongly in the 1990s only to stagnate in the 2000s, suggesting that credit misallocation has only transitory effects on innovation—when, in fact, the observed pattern may simply reflect a relative surge in innovation in a subset of low-zombie technologies whose patents have yet to receive most of their citations because truncation is more severe in technologically dynamic fields.

These considerations motivate several features of our empirical design. First, we exploit within-technology variation over time while controlling for technology fixed effects, thereby accounting for time-invariant differences in innovative capacity across fields. Second, we restrict the baseline analysis to patent applications filed no later than 2002, for which citation windows are sufficiently long. Third, we construct technology-level zombie exposure measures that vary only through changes in industry-level zombie prevalence, holding the industry–technology mapping fixed. Finally, we conduct robustness checks that explicitly address the possibility that results are driven by “hot” technology fields by excluding technologies that experienced surges in patenting or citations during the 1990s. The regression analysis that follows is structured to address these intertwined concerns of selection, truncation, and latent technological trends highlighted by Figures 5 and 6.

5.2. Baseline Regression Specification

Our baseline specification relates innovation outcomes in technology class k and application year t to exposure to zombie lending as follows:

$$y_{kt} = \beta z_{kt-1} + \alpha_k + \gamma_t + \varepsilon_{kt},$$

where y_{kt} denotes an innovation outcome—such as the log number of granted patents or the log number of forward citations associated with patent applications in technology class k and year t. The key explanatory variable, z_{kt-1}, is the industry-weighted zombie share for technology class k in year $t-1$, constructed using the long-run industry composition of patenting within each technology class, as described in Section 4.5. Standard errors are clustered by the industry with the largest patenting share for each technology class (13 clusters).

The specification includes technology-class fixed effects (α_k), which absorb time-invariant differences in innovative capacity across fields. Year fixed effects (γ_t) capture aggregate trends in patenting, changes in patent-office practices, and macroeconomic shocks common to all technologies.

This within-technology design directly addresses the selection concerns highlighted in Section 4. Technology classes that are more exposed to zombie lending are systematically less innovative in the cross section, even prior to the rise of zombie lending. By comparing each technology class to itself over time, the regression framework isolates changes in innovation associated with changes in exposure to credit misallocation, rather than relying on comparisons between inherently high- and low-innovation fields. At the same time, year fixed effects ensure that identification comes from differential exposure to zombie lending across technologies within a given year, rather than from secular trends in innovative activity.

Patent grants and forward citations are inherently count variables and exhibit highly skewed distributions, with a small number of technology classes accounting for a disproportionate share of innovative activity. A common approach in the innovation literature is to estimate log-linear models using the logarithm of one plus the count of patents or citations, which allows zeros to be included while compressing the right tail of the distribution. While this transformation is convenient, it treats zero outcomes in an ad hoc manner and distorts proportional effects at low counts. We therefore complement log-linear specifications with Poisson regressions, which model the conditional mean of count outcomes directly and accommodate zeros naturally. [21]

5.3. Baseline Results

Table 1 reports baseline estimates of the baseline model relating innovation outcomes at the technology-class level to exposure to zombie lending. Panel A presents specifications that include year fixed effects but exclude technology-class fixed effects, while Panel B adds technology-class fixed effects and therefore exploits only within-technology variation over time. In each panel, we report results for patent grants and forward citations using both log-linear OLS specifications and Poisson count models.

Panel A reveals a strong negative cross-sectional relationship between zombie exposure and innovation. Technology classes that are more exposed to zombie lending exhibit substantially lower patenting and citation activity on average, regardless of whether innovation is measured using log outcomes or Poisson counts. The estimated coefficients are large in magnitude and highly statistically significant across all four specifications. These results mirror the raw patterns documented in Section 4 and confirm that zombie lending is disproportionately concentrated in technological fields characterized by persistently low innovative activity.

[21] Log-linear specifications using log(1 + y) yield qualitatively similar patterns. However, recent work shows that log transformations in the presence of zeros can distort magnitudes and inference, whereas Poisson estimators deliver more reliable semi-elasticities for count and count-like outcomes (Santos Silva and Tenreyro 2006; Cohn, Liu, and Wardlaw 2022; Chen and Roth 2024). Accordingly, we emphasize Poisson estimates when discussing economic magnitudes.

Panel B introduces technology-class fixed effects, absorbing time-invariant differences in innovative capacity across technological fields. Once these fixed effects are included, the magnitude of the estimated coefficients declines sharply but remains negative and statistically significant. This attenuation indicates that a substantial portion of the unconditional correlation between zombie exposure and innovation reflects selection: industries and technologies with chronically low innovative dynamism are more likely to experience persistent credit misallocation. At the same time, the remaining within-technology estimates suggest that increases in exposure to zombie lending within a given technological field are associated with meaningful reductions in innovative activity.

In Poisson specifications with technology-class fixed effects, the results show that a 10 percentage point increase in the zombie share faced by a technology class is associated with roughly a 10 percent reduction in patent grants and a 20 percent reduction in forward citations. Thus, the quantitative impact on citation-based measures is roughly twice as large as that on patent counts. Because forward citations proxy for the economic and technological significance of inventions, the larger effect on citations implies that impaired credit dynamism is particularly damaging to the generation of high-impact innovations that drive creative destruction.

5.4. Robustness Check

5.4.1. Alternative Measures of Zombie Lending

Our baseline measure of zombie lending, used in Table 1, follows Hoshi (2006) and Caballero, Hoshi, and Kashyap (2008) and identifies zombie firms solely on the basis of whether they appear to receive subsidized credit. This approach deliberately avoids defining zombies using profitability, productivity, or growth outcomes. As emphasized by Caballero, Hoshi and Kashyap (2008), performance-based definitions risk hard-wiring the very correlations one seeks to study: if zombies are identified by poor operating performance, then industries with many zombies will mechanically appear unproductive and slow-growing. By contrast, a subsidy-based definition permits an evaluation of whether distortions in credit allocation—rather than ex post firm outcomes—predict subsequent innovative activity.

While this definition is therefore well suited for isolating the role of credit misallocation, it may misclassify some financially healthy firms as zombies if they face legitimately low borrowing costs due to low risk. To address this concern, we implement a more stringent alternative definition based on Acharya et al. (2024). Under this definition, firms are classified as receiving zombie credit only if they both (i) appear to receive a credit subsidy and (ii) exhibit balance-sheet characteristics indicative of financial fragility—specifically, above-median leverage and below-median interest coverage. This refinement narrows attention to firms that are not only beneficiaries of unusually favorable financing terms but also unlikely to sustain those terms absent creditor forbearance. Figure A1 in the Online Appendix documents the evolution of zombie prevalence under this alternative definition.

Table 2 reports the results of re-estimating the baseline technology-class regressions with both technology-class fixed effects and application-year fixed effects using this stricter measure of zombie exposure. The estimates are qualitatively similar to those in Table 1 (Panel B) but are uniformly larger in magnitude and more precisely estimated. Across all specifications, greater exposure to zombie lending is associated with significantly lower patenting and citation activity, regardless of whether innovation is measured using log specifications or Poisson models.

The strengthening of the estimates under the stricter definition admits two, not mutually exclusive, interpretations. On the one hand, incorporating balance-sheet indicators may sharpen the measurement of zombie lending by excluding genuinely healthy firms that happen to face low borrowing costs, thereby reducing attenuation bias in the baseline estimates. On the other hand, because the alternative definition conditions on financial distress, it may reintroduce some degree of endogeneity: sectors with more fragile firms may both receive forbearance lending and experience weaker innovative performance for reasons unrelated to credit allocation per se. In this sense, the larger coefficients in Table 2 may reflect a combination of credit distortions and underlying sectoral distress.

For this reason, we view the subsidy-based measure used in Table 1 as our preferred specification for causal interpretation. The results in Table 2 nonetheless serve an important robustness role: they demonstrate that the negative relationship between zombie exposure and innovation is not driven by the inclusion of financially healthy firms among zombies and, if anything, becomes stronger when attention is restricted to firms that are both subsidized and financially fragile.

5.4.2 Addressing Endogeneity Concerns

A central concern in interpreting the baseline results is that exposure to zombie lending may be correlated with latent differences in innovative potential across technological fields. For example, industries with weaker growth prospects or lower-quality firms may both attract greater creditor forbearance and exhibit persistently lower innovation, generating spurious correlations between zombie exposure and patenting outcomes. While no single test can fully resolve such endogeneity concerns, we provide several complementary pieces of evidence suggesting that our results are not driven by omitted variables related to business conditions or average firm quality.

Table 3 augments the baseline technology-class regressions with additional controls commonly used in the zombie-lending literature. Following Caballero, Hoshi, and Kashyap (2008), we include industry-weighted sales growth to proxy for differences in business opportunities across sectors. We also control for the industry-weighted share of financially fragile firms—defined as firms with above-median leverage and below-median interest coverage ratios—following Acharya et al. (2022, 2024), to account for variation in the underlying quality of firms operating within a technology class.

Across all specifications in both Panel A (patent grants) and Panel B (forward citations), exposure to zombie lending remains negatively and statistically significantly associated with innovative activity. Importantly, the magnitude of the zombie exposure coefficient changes little when these additional controls are included. This stability indicates that the baseline relationship is not driven solely by differences in contemporaneous business conditions or by the concentration of low-quality firms in particular technological fields.

The control variables themselves behave as expected. Greater exposure to financially fragile firms is associated with lower patenting and citation activity, consistent with weaker innovative capacity in sectors dominated by distressed firms. In contrast, sales growth has limited explanatory power once technology-class and year fixed effects are included. Echoing the findings of Caballero, Hoshi, and Kashyap (2008) that variation in business opportunities alone cannot account for patterns of zombie lending. Taken together, these results suggest that the negative association between zombie lending exposure and innovation reflects more than simple selection on firm quality or demand conditions. Instead, they are consistent with the view that credit misallocation—over and above underlying sectoral weakness—plays an independent role in shaping innovative outcomes.

A remaining concern is that the negative association between zombie exposure and innovation reflects unobserved, technology-specific shocks that simultaneously depress patenting activity and financial conditions, rather than a causal effect of zombie lending on corporate innovation. To address this possibility, we conduct a placebo test using patenting activity by non-corporate applicants.

We classify patents into corporate and non-corporate applicants, where the latter include individuals, academic institutions, and public research entities. These applicants are unlikely to be directly affected by bank forbearance, creditor incentives, or the congestion effects emphasized in the zombie-lending literature. At the same time, non-corporate patenting is plausibly exposed to the same underlying technological opportunities and scientific advances that shape corporate innovation.[22]

We then replicate the baseline technology-level regressions using non-corporate patent outcomes as dependent variables. Table 4 reports the results. While zombie exposure is weakly negatively associated with non-corporate patenting in specifications without technology-class fixed effects, this relationship disappears once fixed effects are included. Across all specifications—with innovation measured using either log outcomes or Poisson models—the estimated coefficients are statistically indistinguishable from zero. This absence of a systematic relationship stands in sharp contrast to the robust negative effects observed for corporate patents. Taken together, these placebo tests strengthen the interpretation that zombie lending

[22] We verify that patent grants and citations by corporate and non-corporate applicants are strongly and positively correlated across technology classes, even after controlling for technology-class and year fixed effects. This pattern indicates that both types of patenting respond to common technology shocks, validating the use of non-corporate patents as a meaningful placebo. See Table A2 in Online Appendix.

causally impedes corporate innovation rather than merely reflecting unobserved technology-level shocks or latent differences in technological opportunity across fields.

5.4.3. Truncation Bias

5.4.3.1. Excluding "hot" technology fields

The presence of rapidly innovating ("hot") technology fields raises two distinct concerns. First, these fields may generate spurious correlations if low zombie exposure simply reflects strong underlying technological opportunities and economic viability, rather than differences in credit allocation. Second, and more subtly, technology classes experiencing rapid innovation are precisely those for which forward citations are most severely truncated near the end of the sample (Lerner and Seru 2022). In such fields, patenting activity accelerates sharply, while observed citation counts decline mechanically toward the end of the sample because a substantial share of citations arrives with long delays. This asymmetry is particularly relevant in our setting because hot technology fields tend to exhibit relatively low exposure to zombie lending.

As a result, observed citation counts may substantially understate the true quality of innovations in low-zombie technology classes, especially during periods when credit misallocation intensifies elsewhere. Importantly, this pattern implies that truncation bias is more likely to attenuate the estimated negative relationship between zombie exposure and innovation than to generate it spuriously.

To address these concerns, we conduct a robustness check that excludes the ten technology classes exhibiting the fastest growth in patent grants over the 1992–2002 period and re-estimate our baseline specifications using both log-linear OLS and Poisson models. If the baseline relationship were driven primarily by unobserved heterogeneity in technological opportunity correlated with zombie exposure, or by differential truncation in rapidly growing fields, the estimated effects should weaken once these classes are excluded.

Table 5 reports the results. Across all specifications, including log-linear models for patent grants, the coefficient on zombie exposure remains negative and statistically significant. Moreover, relative to the baseline estimates reported in Table 3, the magnitude of the coefficient generally increases. This pattern is consistent with the interpretation that truncation bias in high-growth technology fields attenuates the estimated effect of zombie lending on innovation, and that excluding these fields reduces this bias rather than driving the results.

5.4.3.2. Robustness to Alternative Sample Periods and Citation Windows

To further assess whether truncation bias remains a concern despite our use of long (15-plus-year) citation windows, we follow the recommendations of Lerner and Seru (2022) and conduct a series of robustness checks that vary both the patent application period and the length of citation exposure. The underlying logic is simple: if truncation bias were driving our results, then altering the timing of patent cohorts or restricting citation windows should materially weaken the estimated relationship between zombie lending and innovation.

We conduct three sets of exercises. First, we vary the application window—shortening the sample to 1992–1998 or extending it to 1992–2007—while continuing to measure forward citations through 2018 (Table 6). Second, holding the application period fixed at 1992–2002, we truncate the citation data earlier—ending in 2010 or 2015—thereby mechanically increasing the severity of right-censoring (Table 7, Panels A and B).[23] Third, we impose a fixed 15-year citation window for all patents (Table 7, Panel C), ensuring equal citation exposure across application cohorts.

Across all specifications in Tables 6 and 7, zombie exposure is consistently associated with significantly lower forward citations. Notably, as shown in Table 7, the magnitude and statistical significance of the estimates are remarkably stable when we vary citation windows, including under aggressive truncation that limits citation exposure to as little as seven years. In sum, across technologies and over time, increases in exposure to zombie lending predict meaningful declines in both the quantity and the quality of innovation, and these patterns are robust to extensive checks for selection and truncation.

6. Firm-Level Analysis

[23] Truncating the citation data at 2010 (or 2015) means that any citations received after that year are not counted, even if the patent continues to be cited thereafter. This construction mechanically increases the degree of right-censoring in measured forward citations, particularly for patents applied for later in the sample period.

The technology-class analysis establishes a robust aggregate relationship: greater exposure to zombie lending is associated with slower innovation, particularly in citation-weighted measures of quality. By construction, however, these reduced-form results cannot distinguish whether innovation declines because zombie firms themselves innovate less, or because zombie lending undermines creative destruction by distorting competition and weakening innovation incentives for otherwise healthy firms. Distinguishing between these channels requires moving from aggregate technological fields to firm-level behavior.

We therefore turn to a firm-level dataset that links patent outcomes to publicly listed firms with detailed balance-sheet information from the Nikkei Financial QUEST database. Although this analysis necessarily focuses on a subset of patent applicants that can be reliably matched to listed firms, it offers a key advantage: it allows us to directly identify zombie and non-zombie firms and to examine how healthy firms respond when they operate in industries with high zombie prevalence. This granularity enables us to separate a direct effect—zombie firms innovate less than comparable non-zombies—from a congestion effect, whereby non-zombie firms reduce innovation in response to distorted competition. The firm-level evidence thus provides the mechanism-level link between impaired credit dynamism and slower creative destruction.

6.1. Baseline Specification

To examine the spillover effects of zombie lending on the innovative activity of healthy firms, we follow the empirical framework of Caballero, Hoshi, and Kashyap (2008) and estimate the following firm-level specification: [24]

$$y_{ijt} = \beta_1 \, \mathit{NonZombie}_{ijt-1} + \beta_2 \, \mathit{NonZombie}_{ijt-1} \times \mathit{ZombieShare}_{jt-1} + \theta s_{ijt} + \eta_{jt} + \varepsilon_{ijt},$$

where y_{ijt} denotes an innovation outcome for firm i operating in industry j in year t. Our primary outcomes are measures of inventive activity based on patent data, including the number of patent grants and the number of forward citations associated with patent applications filed by firm i in year t. To mitigate concerns about truncation bias in forward

[24] Acharya et al. (2022, 2024) follow the same regression model to examine congestion effects in the US and European data, respectively.

citation data, the baseline firm-level analysis focuses on patent applications filed no later than 2002, mirroring the technology-class–level analysis in Section 5.

The indicator variable *NonZombie*$_{ijt-1}$ equals one if firm i is not classified as a zombie in year $t-1$ and zero otherwise. The variable *ZombieShare*$_{jt-1}$ measures the asset-weighted share of zombie firms in industry j at time $t-1$. Industry–year fixed effects η_{jt} absorb all industry-specific shocks and time-varying demand or productivity conditions common to firms within the same industry each year. As a consequence, the specification does not identify the aggregate effect of changes in zombie prevalence at the industry level. Instead, identification comes entirely from differential innovation responses of zombie and non-zombie firms within the same industry–year, allowing us to isolate congestion effects while abstracting from industry-wide movements in innovation. Accordingly, the firm-level estimates should be interpreted as capturing within-industry reallocative effects rather than aggregate innovation losses. [25]

The coefficient β_1 captures the baseline innovation advantage of non-zombie firms relative to zombie firms when industry-level zombie prevalence is negligible. A positive estimate of β_1 indicates that, in the absence of severe congestion from zombies, financially healthy firms are more innovative than their zombie counterparts. The interaction coefficient β_2 captures how this innovation advantage varies with the prevalence of zombie firms in the industry. A negative $\beta_2$implies that as zombie lending becomes more widespread, the innovation gap between non-zombie and zombie firms narrows, consistent with congestion effects that depress innovation incentives for otherwise healthy firms.

A potential concern in interpreting the interaction coefficient β_2 as evidence of congestion effects is that industry-level zombie prevalence may proxy for underlying industry conditions. In particular, β_2 could be negative if non-zombie firms are more sensitive than zombies to adverse industry shocks. Caballero, Hoshi, and Kashyap (2008) address this concern

[25] Moreover, because the firm-level analysis is restricted to publicly listed firms that can be matched to patent records, the estimates should not be interpreted as capturing aggregate innovation effects for the corporate sector as a whole, but rather as mechanism-level evidence on how impaired credit dynamism distorts innovation incentives among observable incumbent firms.

by controlling for firm-level sales growth as a proxy for business opportunities, while acknowledging that such controls may attenuate estimates if congestion operates precisely by reducing healthy firms' growth prospects. Following their approach, we include sales growth s_{ijt} in selected specifications as a robustness check; its coefficient θ is expected to be positive.

Standard errors are clustered at the industry level to account for correlated shocks and common exposure to industry-wide financial conditions.

6.2. Firm-Level Results: Innovation Gaps and Congestion Effects

Tables 8 and 9 report firm-level estimates of the impact of zombie lending on innovation, using the subsidy-based zombie definition of Caballero, Hoshi, and Kashyap (2008) and a stricter definition that additionally incorporates balance-sheet weakness following Acharya et al. (2019). All specifications include industry–year fixed effects, so identification comes from comparisons between zombie and non-zombie firms operating within the same industry and year.

Across all specifications, non-zombie firms are significantly more innovative than zombie firms. This result holds for both patent grants and forward citations and is robust across functional forms and zombie definitions. In Poisson specifications without interaction terms, the estimated coefficient on the non-zombie indicator implies that healthy firms produce roughly 150 percent more patents and citations than zombies on average—an innovation gap that is substantial even when averaged over heterogeneous competitive environments.

Allowing the innovation gap to vary with industry-level zombie prevalence yields two further insights. First, once the interaction between non-zombie status and zombie share is included, the coefficient on the non-zombie indicator increases markedly. In the Poisson specifications, this coefficient rises to approximately 2.5, implying that in industries with negligible zombie presence, healthy firms are 250 percent more innovative than zombies. This contrast highlights that unconditional estimates mask considerable heterogeneity: the innovation advantage of healthy firms is largest precisely where credit allocation is least distorted.

Second, and central to our mechanism, the interaction between non-zombie status and zombie share is negative and precisely estimated across all specifications. As zombie prevalence rises, the innovation advantage of healthy firms erodes, consistent with congestion effects whereby subsidized incumbents weaken the incentives and returns to innovation for otherwise productive competitors. Formally, the expected innovation gap between non-zombie and zombie firms is

$$E[y \mid \mathit{NonZombie} = 1] - E[y \mid \mathit{NonZombie} = 0] = \beta_1 + \beta_2 \times \mathit{ZombieShare}.$$

Using the Poisson estimates, this gap closes at zombie-share levels of approximately 35 percent ($-\frac{\beta_1}{\beta_2} = \frac{2.5}{7} = .35$), well within the range observed in some industries in Japan during the 1990s as seen in Figure 2. In sufficiently distorted industries, healthy firms thus become as non-innovative as zombies despite retaining access to unsubsidized credit.

Sales growth enters positively in the Poisson specifications, consistent with stronger business conditions supporting innovative activity. Importantly, including sales growth has little effect on the magnitude or significance of the interaction term, indicating that the estimated congestion effects are not driven by differential exposure to industry-wide shocks affecting healthy firms. The firm-level evidence, thus, confirms that impaired credit dynamism depresses innovation not only because zombie firms innovate less, but because their continued presence undermines the incentives of otherwise productive competitors.

6.3. Truncation and Hot-Sector Robustness

Just as in the technology-class analysis, firm-level patent outcomes may be affected by non-uniform truncation and by the presence of technologically dynamic sectors that experience rapid growth in patenting and citations and subsequent undercounting in forward citations. We therefore conduct a parallel set of robustness checks to assess whether the firm-level congestion results are driven by (i) dominant “hot” sectors or (ii) the timing of patent application cohorts and citation windows.

6.3.1. Excluding Rapid-Growth Technologies

We first address the concern that the baseline firm-level results may be disproportionately influenced by a small number of technologically dominant sectors. In the Japanese data, electrical machinery stands out as the single largest contributor to patenting and citation growth during the 1990s and early 2000s. This sector accounts for roughly 10 percent of firm-level observations and experienced the sharpest increase in patenting activity, making it a natural candidate for both sector-specific technological shocks and more severe citation truncation.

Table 10 reports firm-level regressions excluding all firms in the electrical machinery sector. Reassuringly, removing this technologically dynamic sector leaves the key results essentially unchanged. Across both patent grants and forward citations, the coefficient on the interaction between non-zombie status and industry-level zombie share remains negative, precisely estimated, and similar in magnitude to the baseline estimates. The innovation advantage of non-zombie firms persists, and the erosion of this advantage as zombie prevalence rises is virtually identical to that observed in the full sample.

These findings reinforce the mechanism-based interpretation of the firm-level results. The estimated congestion effects do not hinge on the behavior of a single high-innovation sector, nor do they reflect sector-specific technological booms. Instead, they are consistent with a broader pattern in which impaired credit dynamism weakens innovation incentives for healthy firms through distorted competition. Moreover, because electrical machinery is also the sector most exposed to potential differential citation truncation, its exclusion provides an especially stringent robustness check.

6.3.2. Robustness to Alternative Sample Periods and Citation Windows

We next examine whether the firm-level congestion effects are sensitive to the timing of patent application cohorts or the length of citation exposure, following the same logic as in Section 5.4.3.2. If truncation bias were driving the results, altering application windows or shortening citation horizons should materially weaken the estimated interaction effects.

Table 11 varies the patent application window while continuing to measure forward citations through 2018. Panel A restricts the sample to applications filed between 1992 and 1998, while Panel B extends the window to 1992–2007. Despite substantial changes in sample size and effective citation horizons, the results remain qualitatively unchanged. The interaction between non-zombie status and zombie share remains negative and statistically significant, indicating that healthy firms' innovation advantage erodes as zombie prevalence rises. As expected, the shorter application window yields larger standard errors due to fewer observations, but the estimated magnitudes remain comparable to the baseline.

Table 12 directly varies citation exposure while holding the application period fixed at 1992–2002. Panels A and B truncate citations in 2010 and 2015, respectively, thereby mechanically increasing the severity of right-censoring, especially for later cohorts. Even under these more aggressive truncation schemes, the estimated congestion effects are remarkably stable. We impose a fixed 15-year citation window for all patents (Panel C), ensuring equal citation exposure across application cohorts. The interaction coefficients retain both their sign and economic magnitude across all specifications.

Taken together, Tables 10–12 demonstrate that the firm-level results are not artifacts of dominant technology sectors, application timing, or unequal citation exposure. As in the technology-class analysis, Japan's early zombification episode provides sufficiently long citation horizons to verify directly that the key mechanism—erosion of healthy firms' innovation incentives in zombie-dominated industries—persists even when truncation concerns are deliberately exacerbated.

7. Conclusion

This paper studies how impaired credit dynamism—the failure to reallocate capital from unproductive incumbents to innovative firms—affects technological progress. Using Japan's experience with widespread zombie lending in the 1990s, we combine detailed firm-level financial data with the universe of patented inventions and unusually long citation horizons to examine both aggregate and micro-level innovation outcomes.

We document three main findings. First, credit misallocation is systematically concentrated in technological fields that were already less innovative prior to the rise of zombie lending, highlighting the importance of accounting for selection across technologies. Second, exploiting within-technology variation over time, we show that increases in exposure to zombie lending are associated with economically meaningful declines in patenting activity, especially when measured with forward citations. Third, firm-level evidence reveals the mechanism underlying these aggregate patterns: zombie firms innovate less, and crucially, healthy firms innovate less when they are forced to compete in industries dominated by zombies. As zombie prevalence rises, the innovation advantage of healthy firms erodes and can vanish entirely.

Our results underscore that the costs of zombie lending extend beyond the misallocation of capital toward unproductive firms. By distorting competition and weakening the rewards to innovation, impaired credit dynamism slows creative destruction and depresses the quality of technological progress economy-wide. These findings complement existing work on productivity and reallocation by highlighting innovation as a distinct and powerful channel through which financial frictions shape long-run economic performance.

More broadly, the paper illustrates the value of combining detailed micro-level financial data with long-horizon patent information. While zombie lending has reemerged in many economies following recent financial crises, Japan's early experience provides a uniquely informative historical setting in which truncation and selection issues can be addressed directly. The evidence suggests that policies delaying restructuring and sustaining unviable firms may impose long-lasting costs on innovation, even after macroeconomic conditions stabilize.

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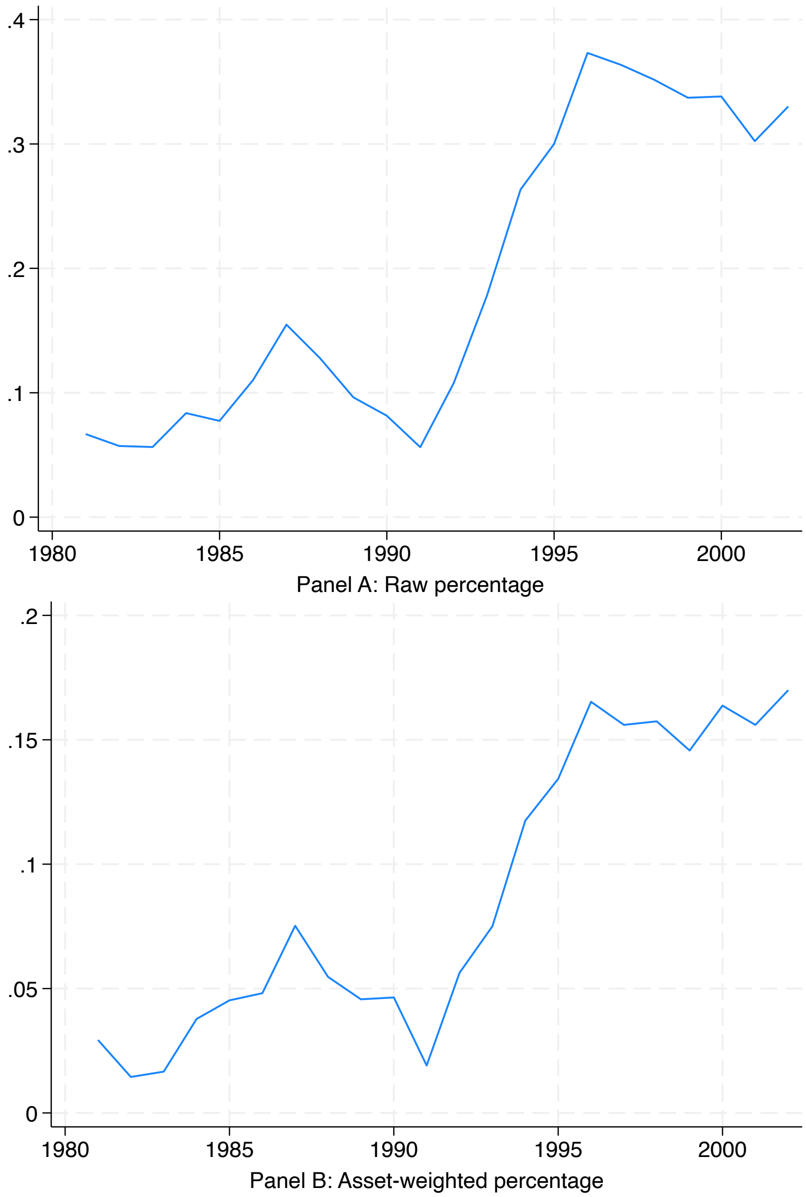
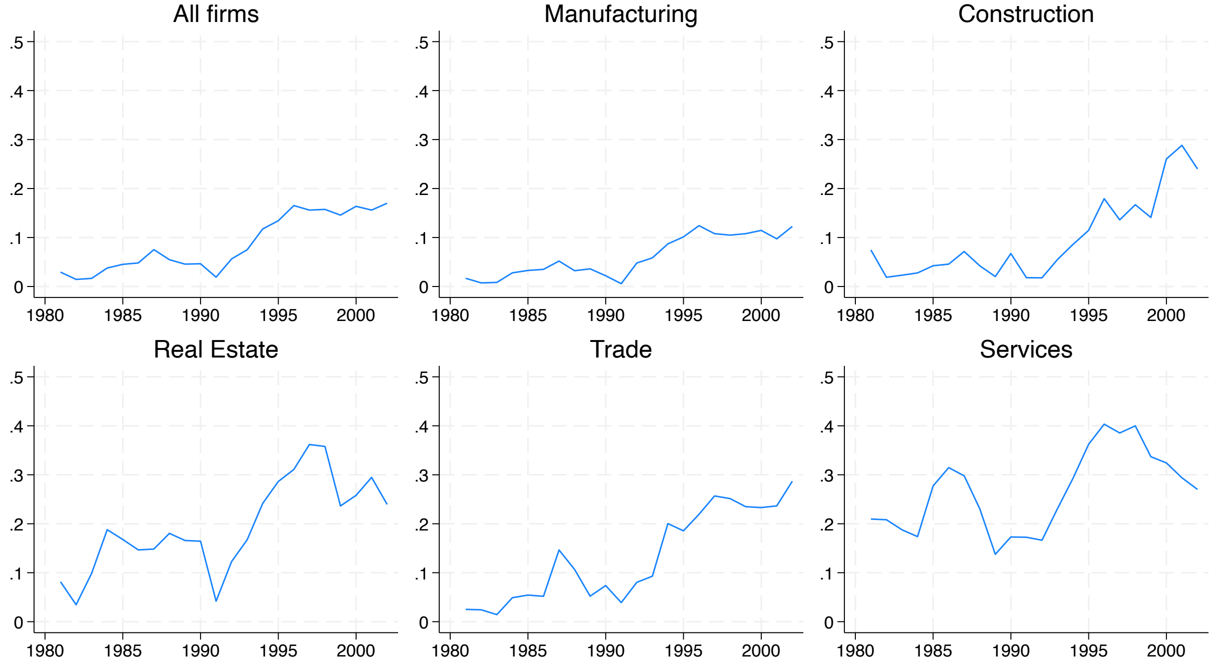
Figure 1: Prevalence of Firms Receiving Subsidized Loans in Japan



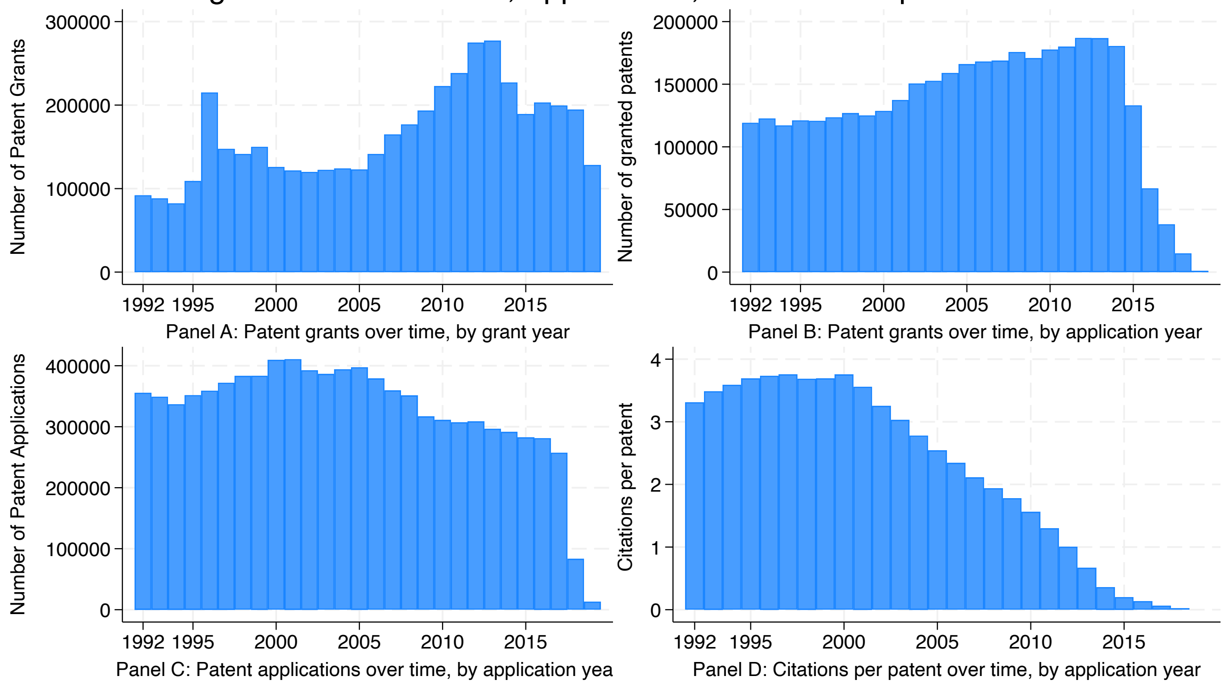
This figure reports the share of listed firms classified as zombies—firms receiving subsidized loans—constructed following Caballero, Hoshi, and Kashyap (2008). Panel A shows the unweighted fraction of zombie firms, treating each firm equally. Panel B shows the asset-weighted fraction, weighting firms by total assets. Zombie prevalence rises sharply in the early 1990s and remains elevated thereafter, indicating the persistence of subsidized lending well beyond the initial post-bubble recession.

Figure 2: Cross-Industry Incidence of Asset Weighted Zombie Percentage



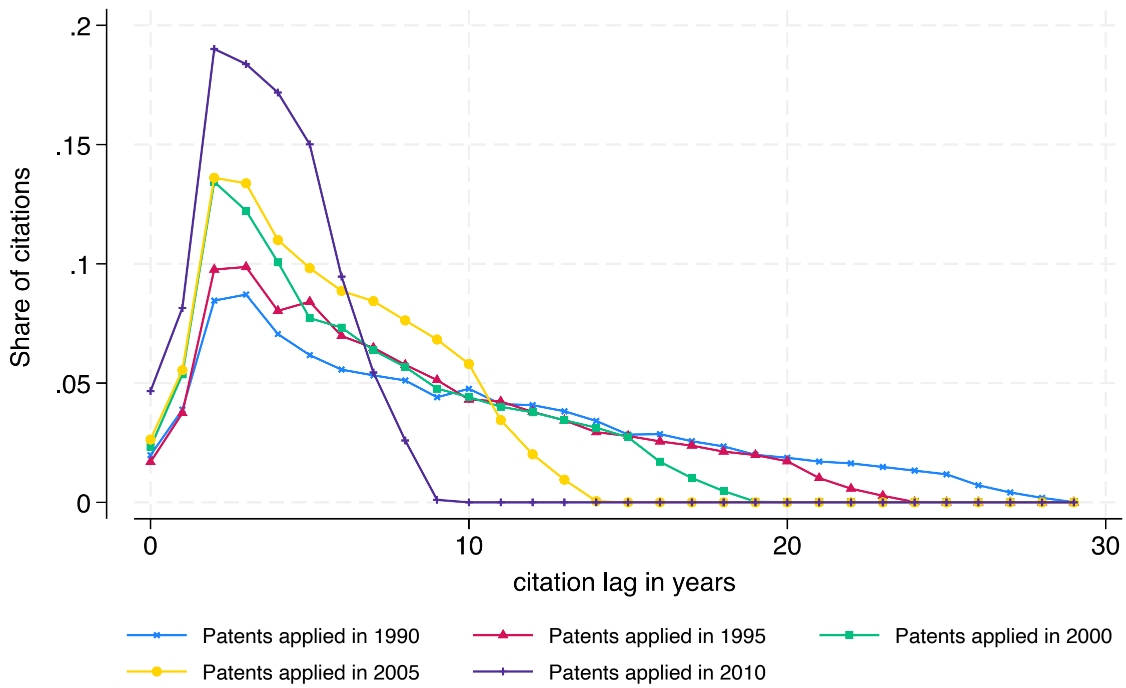
This figure reports the asset-weighted share of zombie firms by industry, constructed following Caballero, Hoshi, and Kashyap (2008). Firms are grouped into six sectors: manufacturing, construction, real estate, trade, services, and all firms combined. Zombie prevalence rises sharply in the early 1990s and remains elevated thereafter, particularly in nonmanufacturing sectors such as construction, real estate, and services.

Figure 3. Patent Grants, Applications, and Citations per Patent



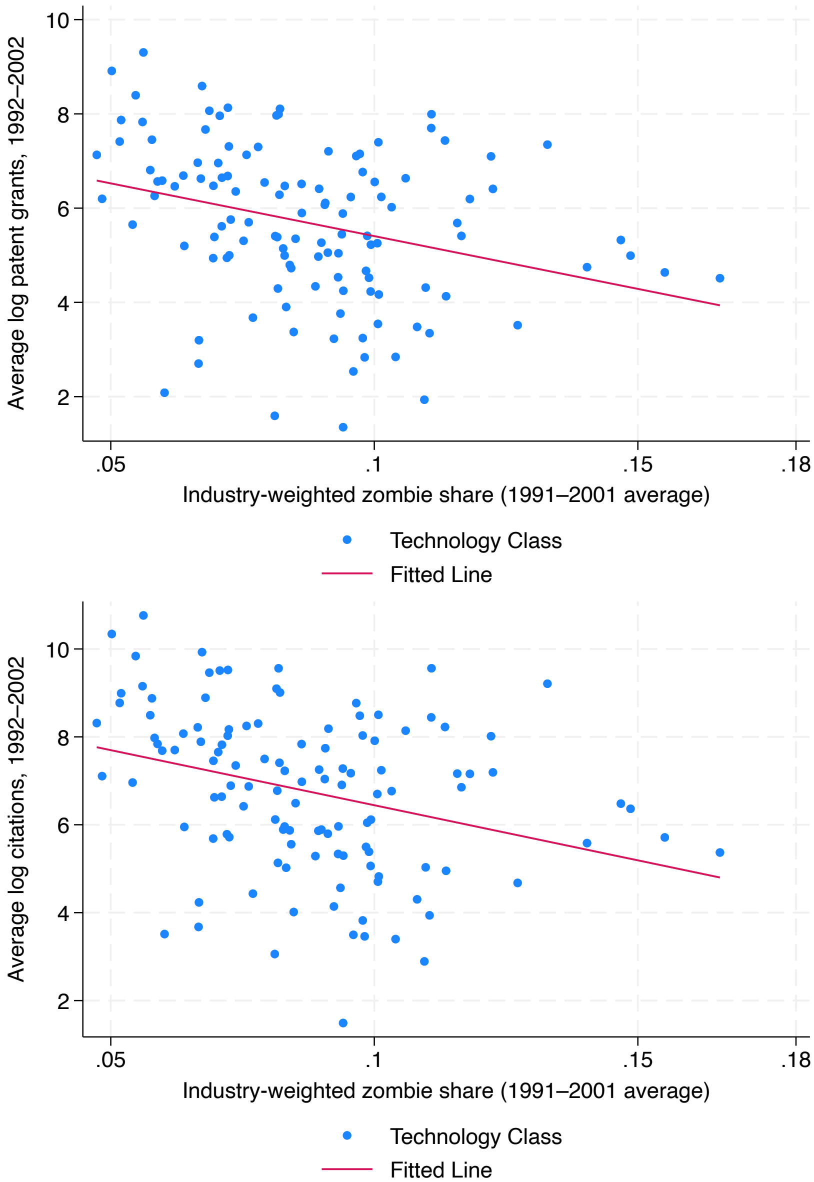
Panel A reports the number of registered patents by grant (registration) year. Panel B reports the number of registered patents by application year. Panel C reports the total number of published patent applications, defined as the sum of successful and unsuccessful applications. Panel D reports forward citations per patent by application year. The sharp end-of-sample declines in Panels B and D reflect truncation arising from long and variable application-to-registration lags and finite citation windows. The decline in Panel C beginning in 2018 reflects Japan's 18-month pre-grant publication rule and the September 2019 data cutoff.

Figure 4: Distribution of Forward Citation Lags for Selected Cohorts: 1990, 1995, 2000, 2005 and 2010



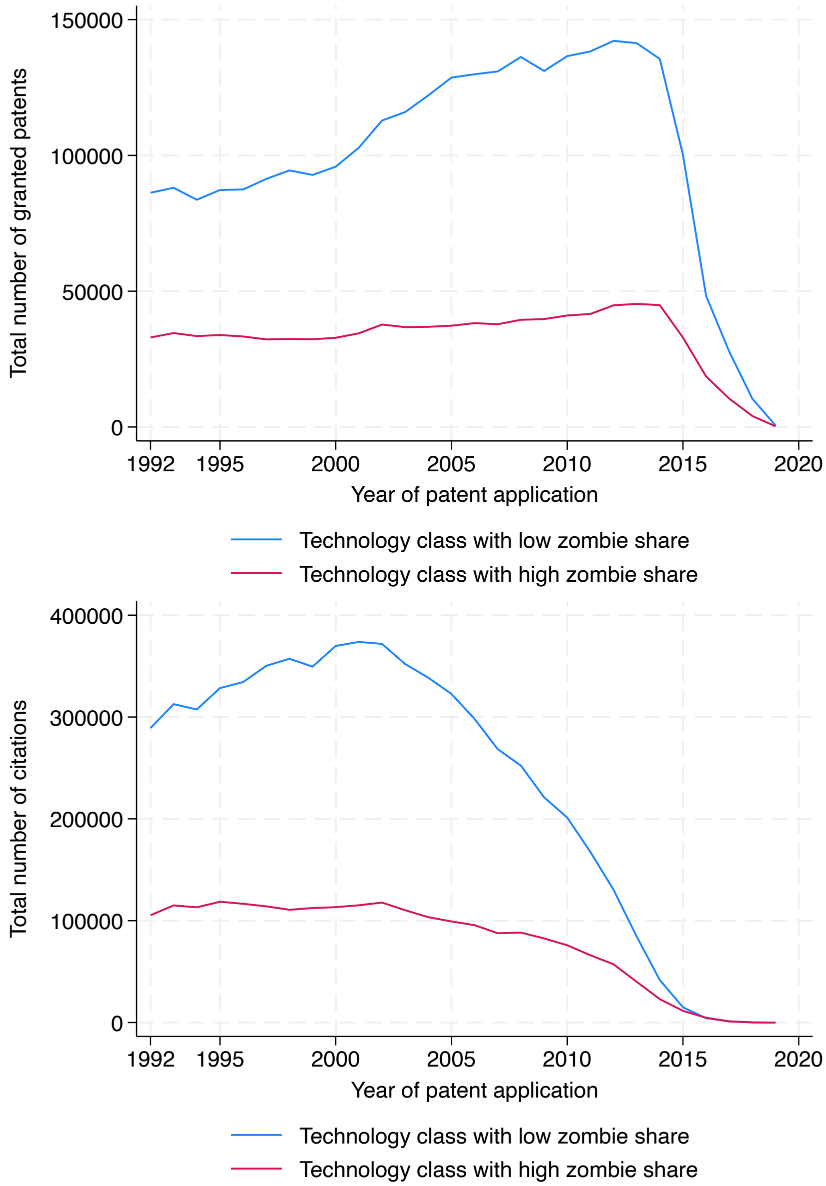
The figure plots the distribution of forward citations by citation lag (in years) for patents applied for in different years. Earlier cohorts display long citation tails, while more recent cohorts exhibit mechanically truncated distributions due to the finite observation window.

Figure 5. Technology-Level Innovation and Exposure to Zombie Lending



This figure plots average patent grants (top panel) and average forward citations (bottom panel) over 1992–2002 against industry-weighted average zombie lending exposure for each of the 120 IPC technology classes. Zombie exposure is constructed as a weighted average of industry-level zombie shares over 1991–2001, using the long-run industry composition of patenting within each technology class. Each point represents one technology class. Technology classes more exposed to zombie lending exhibit substantially lower patenting and citation activity on average. These cross-sectional correlations motivate the analysis but, as discussed in the text, reflect substantial pre-existing heterogeneity in innovative intensity across technological fields rather than causal effects of zombification.

Figure 6: Innovation Dynamics in Technology Classes with High and Low Zombie Exposure



Notes: This figure plots total patent grants (top panel) and total forward citations (bottom panel) by application year for technology classes with low versus high exposure to zombie lending. Technology classes are classified based on whether their industry-weighted average zombie share over 1991–2001 is below or above the median. Low-zombie technology classes are substantially more innovative throughout the sample period, including prior to the rise of zombie lending in the early 1990s. The sharp post-2000 decline—especially pronounced for citations in low-zombie fields—reflects mechanical truncation arising from finite citation windows rather than real declines in inventive activity. The figure illustrates how truncation interacts with pre-existing differences in innovative intensity across technologies, motivating our focus on early application cohorts and within-technology variation in the regression analysis.

Table 1. Zombie Exposure and Innovation Across Technology Classes

Panel A: Without technology class fixed effects

	(1)	(2)	(3)	(4)
Dependent variable	ln(1+Grants)	Grants	ln(1+Citations)	Citations
Estimation method	OLS	Poisson	OLS	Poisson
Zombie exposure	-14.11**	-22.01***	-16.09**	-24.73***
	(4.693)	(7.582)	(5.777)	(9.335)
Technology class fixed effects	No	No	No	No
Year fixed effects	Yes	Yes	Yes	Yes
Observations	1,314	1,314	1,314	1,314

Panel B: With technology class fixed effects

	(1)	(2)	(3)	(4)
Dependent variable	ln(1+Grants)	Grants	ln(1+Citations)	Citations
Estimation method	OLS	Poisson	OLS	Poisson
Zombie exposure	-0.911**	-1.198*	-1.606**	-2.082***
	(0.350)	(0.622)	(0.542)	(0.492)
Technology class fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	1,314	1,314	1,314	1,314

This table reports technology-class–level regressions of innovative activity on exposure to zombie lending. The unit of observation is a technology class–year, based on 120 IPC technology classes observed over the period 1992–2002. Panel A presents specifications without technology-class fixed effects, while Panel B includes technology-class fixed effects. The dependent variable is the number of granted patents or forward citations, measured using ln(1 + y) in log-linear OLS specifications or in levels using Poisson regressions, as indicated in the column headers.

Zombie exposure is the industry-weighted zombie share for technology class k in year t-1, constructed using the long-run industry composition of patenting within each technology class, as described in Section 4.5. All specifications include application-year fixed effects; specifications with technology-class fixed effects absorb time-invariant differences in innovative capacity across fields. Standard errors are clustered by the industry with the largest patenting share for each technology class (13 clusters) and are reported in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 2. Zombie Exposure and Innovation Using a Stricter Zombie Definition (Credit Subsidy + Balance-Sheet Weakness)

	(1)	(2)	(3)	(4)
Dependent variable	ln(1+Grants)	Grants	ln(1+Citations)	Citations
Estimation method	OLS	Poisson	OLS	Poisson
Zombie exposure (credit subsidy + balance-sheet weakness)	-1.500**	-2.734***	-1.769**	-3.267***
	(0.557)	(0.488)	(0.692)	(0.705)
Technology class fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	1,314	1,314	1,314	1,314

This table reproduces the specifications in Panel B of Table 1 using an alternative, more stringent definition of zombie firms. The unit of observation is a technology class–year, based on 120 IPC technology classes observed over the period 1992–2002.

The dependent variable is the number of granted patents or forward citations, measured using ln(1 + y) in log-linear OLS specifications (columns (1) and (3)) or in levels using Poisson regressions (columns (2) and (4)). Zombie exposure is constructed following Acharya et al. (2024) and classifies firms as zombies only if they both receive subsidized credit and exhibit balance-sheet indicators of financial fragility, defined as above-median leverage and below-median interest coverage.

All specifications include technology-class fixed effects and application-year fixed effects. Standard errors are clustered by the industry with the largest patenting share for each technology class (13 clusters) and are reported in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 3. Zombie Exposure and Innovation: Controlling for Firm Quality and Business Conditions

Panel A: Patent Grants

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Grants)	ln(1+Grants)	ln(1+Grants)	Grants	Grants	Grants
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Zombie exposure	-0.911**	-1.036***	-0.977***	-1.198*	-1.235***	-1.122**
	(0.350)	(0.277)	(0.313)	(0.622)	(0.444)	(0.482)
Low-quality firm share		-1.017***	-1.000***		-1.020***	-0.983***
		(0.226)	(0.220)		(0.305)	(0.345)
Sales growth			0.362			0.444
			(0.433)			(0.585)
Technology class fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Observations	1,314	1,314	1,314	1,314	1,314	1,314

Panel B: Forward Citations

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Citations)	ln(1+Citations)	ln(1+Citations)	Citations	Citations	Citations
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Zombie exposure	-1.606**	-1.720***	-1.650***	-2.082***	-2.229***	-2.030***
	(0.542)	(0.512)	(0.538)	(0.492)	(0.408)	(0.411)
Low-quality firm share		-0.928**	-0.908**		-0.853**	-0.785**
		(0.352)	(0.317)		(0.363)	(0.388)
Sales growth			0.432			0.891
			(0.638)			(0.572)
Technology class fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Observations	1,314	1,314	1,314	1,314	1,314	1,314

This table augments the baseline technology-class–level regressions in Panel B of Table 1 by adding controls for firm quality and business conditions. The unit of observation is a technology class–year, based on 120 IPC technology classes observed over the period 1992–2002. Panel A reports results for patent grants, and Panel B reports results for forward citations. Low-quality firm share is the industry-weighted fraction of firms with above-median leverage and below-median interest coverage, following Acharya et al. (2022, 2024), and captures average financial fragility within a technology class. Sales growth is the industry-weighted sales growth rate and proxies for differences in business opportunities, following Caballero, Hoshi, and Kashyap (2008).

Zombie exposure is defined as in the baseline analysis. The dependent variable is measured using ln(1 + y) in log-linear OLS specifications or in levels using Poisson regressions, as indicated in the column headers. All specifications include technology-class fixed effects and application-year fixed effects. Standard errors are clustered by the industry with the largest patenting share for each technology class (13 clusters) and are reported in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 4. Placebo Test Using Non-Corporate Patents

Panel A: Without technology class fixed effects

	(1)	(2)	(3)	(4)
Dependent variable	ln(1+Grants)	Grants	ln(1+Citations)	Citations
Estimation method	OLS	Poisson	OLS	Poisson
Zombie exposure	-3.555	-2.009	-6.611*	-7.244
	(2.539)	(3.262)	(3.310)	(4.785)
Technology class fixed effects	No	No	No	No
Year fixed effects	Yes	Yes	Yes	Yes
Observations	1,314	1,314	1,314	1,314

Panel B: With technology class fixed effects

	(1)	(2)	(3)	(4)
Dependent variable	ln(1+Grants)	Grants	ln(1+Citations)	Citations
Estimation method	OLS	Poisson	OLS	Poisson
Zombie exposure	0.0937	-2.207	-0.481	-3.799
	(1.060)	(1.901)	(1.365)	(2.563)
Technology class fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	1,314	1,314	1,314	1,314

This table reports placebo regressions that replicate the baseline technology-class–level specification (Table 1) using patenting outcomes by non-corporate applicants, including individuals, universities, and public research institutions. The unit of observation is a technology class–year. Panel A reports specifications without technology-class fixed effects, while Panel B includes technology-class fixed effects.

Zombie exposure is defined as in the baseline analysis. The dependent variable is measured using ln(1 + y) in log-linear OLS specifications or in levels using Poisson regressions, as indicated in the column headers. Standard errors are clustered by the industry with the largest patenting share for each technology class (13 clusters) and are reported in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 5. Zombie Exposure and Innovation: Excluding Rapid-Growth Technology Classes

Panel A: Patent Grants

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Grants)	ln(1+Grants)	ln(1+Grants)	Grants	Grants	Grants
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Zombie exposure	-1.161***	-1.264***	-1.240**	-1.436***	-1.419***	-1.320***
	(0.351)	(0.393)	(0.419)	(0.487)	(0.400)	(0.392)
Low-quality firm share		-0.903***	-0.897***		-1.189***	-1.157***
		(0.229)	(0.231)		(0.280)	(0.317)
Sales growth			0.141			0.377
			(0.432)			(0.536)
Technology class fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Observations	1,204	1,204	1,204	1,204	1,204	1,204

Panel B: Forward Citations

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Citations)	ln(1+Citations)	ln(1+Citations)	Citations	Citations	Citations
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Zombie exposure	-1.793***	-1.872***	-1.875***	-2.413***	-2.514***	-2.345***
	(0.543)	(0.562)	(0.599)	(0.381)	(0.338)	(0.243)
Low-quality firm share		-0.696**	-0.697**		-1.135***	-1.077***
		(0.252)	(0.236)		(0.275)	(0.310)
Sales growth			-0.0170			0.740
			(0.616)			(0.515)
Technology class fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Observations	1,204	1,204	1,204	1,204	1,204	1,204

This table replicates the technology-class–level regressions in Table 3 after excluding the ten technology classes that experienced the largest growth in patenting activity between 1992 and 2002.

Zombie exposure is defined as in the baseline analysis. The dependent variable is measured using ln(1 + y) in log-linear OLS specifications or in levels using Poisson regressions, as indicated in the column headers. All specifications include technology-class fixed effects and application-year fixed effects. Standard errors are clustered by the industry with the largest patenting share for each technology class (13 clusters) and are reported in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 6. Zombie Exposure and Forward Citations: Alternative Application Windows

Panel A: Short Application Window (1992–1998)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Citations)	ln(1+Citations)	ln(1+Citations)	Citations	Citations	Citations
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Zombie exposure	-1.961***	-1.900**	-2.035**	-1.868***	-1.953***	-2.093***
	(0.624)	(0.650)	(0.707)	(0.614)	(0.592)	(0.632)
Low-quality firm share		0.403	0.363		-0.370	-0.375
		(0.461)	(0.428)		(0.659)	(0.600)
Sales growth			-0.672			-0.519
			(0.518)			(0.416)
Technology class fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Observations	833	833	833	833	833	833

Panel B: Extended Application Window (1992–2007)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Citations)	ln(1+Citations)	ln(1+Citations)	Citations	Citations	Citations
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Zombie exposure	-0.870**	-0.928**	-0.907**	-1.646***	-1.623***	-1.523***
	(0.379)	(0.366)	(0.375)	(0.508)	(0.520)	(0.455)
Low-quality firm share		-0.165	-0.130		0.130	0.233
		(0.255)	(0.293)		(0.160)	(0.188)
Sales growth			0.332			0.831
			(0.542)			(0.540)
Technology class fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Observations	1,914	1,914	1,914	1,914	1,914	1,914

This table replicates the forward-citation specifications in Panel B of Table 3 using alternative patent application windows. Panel A restricts the sample to patent applications filed between 1992 and 1998, while Panel B extends the application window to 1992–2007. In both panels, forward citations are measured through 2018.

Zombie exposure is defined as in the baseline analysis. The dependent variable is measured using ln(1 + y) in log-linear OLS specifications or in levels using Poisson regressions, as indicated in the column headers. All specifications include technology-class fixed effects and application-year fixed effects. Standard errors are clustered by the industry with the largest patenting share for each technology class (13 clusters) and are reported in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 7. Zombie Exposure and Forward Citations: Alternative Citation Windows

Panel A: Citations Ending in 2010 (≈7–18 years of exposure)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Citations)	ln(1+Citations)	ln(1+Citations)	Citations	Citations	Citations
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Zombie exposure	-1.657***	-1.761***	-1.694***	-2.209***	-2.395***	-2.192***
	(0.458)	(0.436)	(0.441)	(0.521)	(0.447)	(0.433)
Low-quality firm share		-0.844*	-0.825*		-0.996**	-0.937**
		(0.417)	(0.383)		(0.399)	(0.428)
Sales growth			0.412			0.866
			(0.735)			(0.613)
Technology class fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Observations	1314	1314	1314	1314	1314	1314

Panel B: Citations Ending in 2015 (≈12–23 years of exposure)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Citations)	ln(1+Citations)	ln(1+Citations)	Citations	Citations	Citations
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Zombie exposure	-1.641***	-1.757***	-1.691***	-2.112***	-2.264***	-2.068***
	(0.524)	(0.490)	(0.511)	(0.498)	(0.411)	(0.410)
Low-quality firm share		-0.948**	-0.929**		-0.884**	-0.817**
		(0.350)	(0.316)		(0.348)	(0.373)
Sales growth			0.409			0.872
			(0.618)			(0.560)
Technology class fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Observations	1314	1314	1314	1314	1314	1314

Panel C: Fixed 15-Year Citation Window

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Citations)	ln(1+Citations)	ln(1+Citations)	Citations	Citations	Citations
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Zombie exposure	-1.600***	-1.704***	-1.632***	-2.004***	-2.145***	-1.989***
	(0.469)	(0.436)	(0.449)	(0.471)	(0.358)	(0.378)
Low-quality firm share		-0.853**	-0.832***		-0.765**	-0.706*
		(0.302)	(0.266)		(0.367)	(0.400)
Sales growth			0.443			0.724
			(0.624)			(0.567)
Technology class fixed effects	YES	YES	YES	YES	YES	YES
Year fixed effects	YES	YES	YES	YES	YES	YES
Observations	1314	1314	1314	1314	1314	1314

This table replicates the forward-citation specifications in Panel B of Table 3 using alternative citation windows, while holding the patent application period fixed at 1992–2002 throughout. Panel A truncates citation data in 2010. Panel B truncates citation data in 2015. Panel C imposes a fixed 15-year citation window for all patents, ensuring equal citation exposure across application cohorts. Zombie exposure is defined as in the baseline analysis. The dependent variable is measured using $\ln(1 + y)$ in log-linear OLS specifications or in levels using Poisson regressions, as indicated in the column headers. All specifications include technology-class fixed effects and application-year fixed effects. Standard errors are clustered by the industry with the largest patenting share for each technology class (13 clusters) and are reported in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 8. Zombie Lending and Firm-Level Innovation

Panel A: Patent Grants

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Grants)	ln(1+Grants)	ln(1+Grants)	Grants	Grants	Grants
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Non-zombie dummy	0.514***	1.069***	1.069***	1.607***	2.329***	2.326***
	(0.122)	(0.129)	(0.129)	(0.252)	(0.199)	(0.198)
Non-zombie x zombie percentage		-3.239***	-3.239***		-6.629***	-6.614***
		(0.540)	(0.540)		(0.941)	(0.934)
Sales growth			0.00994			0.248***
			(0.0777)			(0.0946)
Industry-year fixed effects	YES	YES	YES	YES	YES	YES
Observations	23,433	23,433	23,433	23,433	23,433	23,433

Panel B: Forward citations

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Citations)	ln(1+Citations)	ln(1+Citations)	Citations	Citations	Citations
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Non-zombie dummy	0.646***	1.334***	1.334***	1.832***	2.598***	2.594***
	(0.149)	(0.154)	(0.154)	(0.286)	(0.248)	(0.247)
Non-zombie x zombie percentage		-4.013***	-4.013***		-7.022***	-7.006***
		(0.686)	(0.684)		(1.085)	(1.077)
Sales growth			0.0857			0.345***
			(0.128)			(0.110)
Industry-year fixed effects	YES	YES	YES	YES	YES	YES
Observations	23,433	23,433	23,433	23,433	23,433	23,433

This table reports firm-level regressions of patent grants and forward citations on a non-zombie indicator and its interaction with industry-level zombie prevalence, measured by the asset-weighted share of zombie firms in the industry. Zombie firms are identified using the subsidy-based definition of Caballero, Hoshi, and Kashyap (2008).

All specifications include industry–year fixed effects, so identification comes from differences between zombie and non-zombie firms operating within the same industry and year. The dependent variable is measured using ln(1 + y) in log-linear OLS specifications or in levels using Poisson regressions, as indicated in the column headers. Standard errors are clustered at the industry level. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table 9. Zombie Lending and Firm-Level Innovation: Stricter Zombie Definition (Credit Subsidy + Balance-Sheet Weakness)

Panel A: Patent Grants

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Grants)	ln(1+Grants)	ln(1+Grants)	Grants	Grants	Grants
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Non-zombie dummy	0.406***	0.763***	0.764***	1.365***	1.876***	1.870***
	(0.0999)	(0.116)	(0.117)	(0.188)	(0.137)	(0.137)
Non-zombie x zombie percentage		-4.014***	-4.015***		-9.268***	-9.244***
		(0.883)	(0.884)		(1.870)	(1.866)
Sales growth			-0.0235			0.199*
			(0.0756)			(0.115)
Industry-year fixed effects	YES	YES	YES	YES	YES	YES
Observations	23,423	23,423	23,423	23,423	23,423	23,423

Panel B: Forward citations

	(2)	(3)	(4)	(6)	(7)	(8)
Dependent variable	ln(1+Citations)	ln(1+Citations)	ln(1+Citations)	Citations	Citations	Citations
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Non-zombie dummy	0.533***	0.986***	0.985***	1.581***	2.204***	2.194***
	(0.129)	(0.142)	(0.143)	(0.251)	(0.176)	(0.175)
Non-zombie x zombie percentage		-5.100***	-5.098***		-10.89***	-10.86***
		(1.119)	(1.121)		(2.320)	(2.316)
Sales growth			0.0424			0.297**
			(0.123)			(0.121)
Industry-year fixed effects	YES	YES	YES	YES	YES	YES
Observations	23,423	23,423	23,423	23,423	23,423	23,423

This table replicates the firm-level regressions in Table 8 using a stricter definition of zombie firms. Zombie firms are identified following Acharya et al. (2019), requiring both subsidized credit and balance-sheet weakness, defined as above-median leverage and below-median interest coverage.

All specifications include industry–year fixed effects. The dependent variable is measured using ln(1 + y) in log-linear OLS specifications or in levels using Poisson regressions, as indicated in the column headers. Standard errors are clustered at the industry level. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table 10. Zombie Lending and Firm-Level Innovation: Excluding Rapid-Growth Technology (Electrical Machinery)

Panel A: Patent Grants

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Grants)	ln(1+Grants)	ln(1+Grants)	Grants	Grants	Grants
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Non-zombie dummy	0.469***	1.041***	1.041***	1.327***	2.057***	2.056***
	(0.120)	(0.160)	(0.160)	(0.206)	(0.233)	(0.233)
Non-zombie x zombie percentage		-3.128***	-3.128***		-5.483***	-5.480***
		(0.632)	(0.633)		(0.907)	(0.908)
Sales growth			-0.0194			0.131
			(0.0753)			(0.145)
Industry-year fixed effects	YES	YES	YES	YES	YES	YES
Observations	20,960	20,960	20,960	20,960	20,960	20,960

Panel B: Forward citations

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Citations)	ln(1+Citations)	ln(1+Citations)	Citations	Citations	Citations
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Non-zombie dummy	0.590***	1.298***	1.298***	1.489***	2.240***	2.240***
	(0.146)	(0.191)	(0.191)	(0.217)	(0.252)	(0.252)
Non-zombie x zombie percentage		-3.869***	-3.870***		-5.599***	-5.595***
		(0.787)	(0.787)		(1.014)	(1.011)
Sales growth			0.0369			0.289
			(0.122)			(0.214)
Industry-year fixed effects	YES	YES	YES	YES	YES	YES
Observations	20,960	20,960	20,960	20,960	20,960	20,960

This table replicates the firm-level regressions in Table 8 after excluding firms in the electrical machinery industry, the most technologically dynamic sector in the sample.

All specifications include industry–year fixed effects. The dependent variable is measured using ln(1 + y) in log-linear OLS specifications or in levels using Poisson regressions, as indicated in the column headers. Standard errors are clustered at the industry level. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table 11. Zombie Lending and Firm-Level Forward Citations: Alternative Application Windows

Panel A: Short Application Window (1992–1998)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Citations)	ln(1+Citations)	ln(1+Citations)	Citations	Citations	Citations
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Non-zombie dummy	0.725***	1.287***	1.287***	1.768***	2.534***	2.533***
	(0.164)	(0.175)	(0.175)	(0.343)	(0.314)	(0.310)
Non-zombie x zombie percentage		-3.656***	-3.654***		-7.434***	-7.432***
		(0.839)	(0.838)		(1.555)	(1.543)
Sales growth			0.138			0.640***
			(0.158)			(0.135)
Industry-year fixed effects	YES	YES	YES	YES	YES	YES
Observations	14,096	14,096	14,096	14,096	14,096	14,096

Panel B: Extended Application Window (1992–2007)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Citations)	ln(1+Citations)	ln(1+Citations)	Citations	Citations	Citations
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Non-zombie dummy	0.449***	1.242***	1.242***	1.390***	2.248***	2.248***
	(0.113)	(0.136)	(0.136)	(0.139)	(0.208)	(0.207)
Non-zombie x zombie percentage		-3.952***	-3.956***		-6.259***	-6.255***
		(0.563)	(0.564)		(1.434)	(1.429)
Sales growth			0.0615			0.159***
			(0.0537)			(0.0483)
Industry-year fixed effects	YES	YES	YES	YES	YES	YES
Observations	37,072	37,072	37,072	37,072	37,072	37,072

This table replicates the firm-level forward-citation regressions in Table 8 using alternative patent application windows. Panel A restricts the sample to patent applications filed between 1992 and 1998, while Panel B extends the application window to 1992–2007. In both panels, forward citations are measured through 2018.

All specifications include industry–year fixed effects. The dependent variable is forward citations associated with patent applications filed by firm i in year t, measured using ln(1 + y) in log-linear OLS specifications or in levels using Poisson regressions, as indicated in the column headers. Standard errors are clustered at the industry level. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Table 12. Zombie Lending and Firm-Level Forward Citations: Alternative Citation Windows

Panel A: Citations Ending in 2010 (≈7–18 years of exposure)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Citations)	ln(1+Citations)	ln(1+Citations)	Citations	Citations	Citations
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Non-zombie dummy	0.631***	1.303***	1.303***	1.846***	2.597***	2.593***
	(0.146)	(0.151)	(0.150)	(0.292)	(0.252)	(0.251)
Non-zombie x zombie percentage		-3.922***	-3.922***		-7.003***	-6.986***
		(0.670)	(0.667)		(1.093)	(1.083)
Sales growth			0.101			0.377***
			(0.126)			(0.109)
Industry-year fixed effects	YES	YES	YES	YES	YES	YES
Observations	23,433	23,433	23,433	23,433	23,433	23,433

Panel B: Citations Ending in 2015 (≈12–23 years of exposure)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Citations)	ln(1+Citations)	ln(1+Citations)	Citations	Citations	Citations
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Non-zombie dummy	0.643***	1.329***	1.329***	1.833***	2.599***	2.595***
	(0.148)	(0.153)	(0.153)	(0.287)	(0.249)	(0.248)
Non-zombie x zombie percentage		-4.000***	-4.000***		-7.035***	-7.018***
		(0.685)	(0.683)		(1.087)	(1.078)
Sales growth			0.0897			0.350***
			(0.128)			(0.109)
Industry-year fixed effects	YES	YES	YES	YES	YES	YES
Observations	23,433	23,433	23,433	23,433	23,433	23,433

Panel C: Fixed 15-Year Citation Window

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable	ln(1+Citations)	ln(1+Citations)	ln(1+Citations)	Citations	Citations	Citations
Estimation method	OLS	OLS	OLS	Poisson	Poisson	Poisson
Non-zombie dummy	0.638***	1.316***	1.316***	1.839***	2.609***	2.606***
	(0.147)	(0.152)	(0.152)	(0.285)	(0.246)	(0.244)
Non-zombie x zombie percentage		-3.959***	-3.959***		-7.034***	-7.017***
		(0.677)	(0.675)		(1.077)	(1.068)
Sales growth			0.0925			0.338***
			(0.128)			(0.107)
Industry-year fixed effects	YES	YES	YES	YES	YES	YES
Observations	23,433	23,433	23,433	23,433	23,433	23,433

This table replicates the firm-level forward-citation regressions in Table 8 using alternative citation windows, while holding the patent application period fixed at 1992–2002 throughout. Panel A truncates citations in 2010, and Panel B truncates citations in 2015.
All specifications include industry–year fixed effects. The dependent variable is forward citations associated with patent applications filed by firm i in year t, measured using ln(1 + y) in log-linear OLS specifications or in levels using Poisson regressions, as indicated in the column headers. Standard errors are clustered at the industry level. *, **, and *** denote statistical significance at the 10, 5, and 1 percent levels, respectively.

Appendix A. Detecting Zombie Lending Following Caballero, Hoshi, and Kashyap (2008)

To identify zombie lending, we replicate the approach of Caballero, Hoshi, and Kashyap (2008), who compute for each firm *i* in year *t* a minimum required interest payment ($R^*_{i,t}$) representing the cost the firm would face if it borrowed at the most favorable market rates available to creditworthy firms. The lower bound for required interest payments is defined as:

$$R^*_{i,t} = rs_{t-1}BS_{i,t-1} + (\frac{1}{5}\sum_{j=1}^{5} rl_{t-j})BL_{i,t-1} + rcb_{min}Bonds_{i,t-1}$$

where:

$BS_{i,t-1}$: short-term bank loans (maturity less than one year)

$BL_{i,t-1}$: long-term bank loans (maturity greater than one year)

$Bonds_{i,t-1}$: total bonds outstanding (including convertible and warrant-attached bonds)

rs_{t-1}: average short-term prime rate

rl_{t-j}: average long-term prime rate

rcb_{min}: minimum observed coupon rate on any convertible bond issued during the previous five years

This specification constructs a conservative lower bound—meaning the implied interest rate is lower than that faced by most borrowers—because it assumes that even bond financing occurs at the most favorable historical rates available. Each firm's actual interest payments, $R_{i,t}$, are compared to this benchmark. The difference, normalized by total borrowing at the start of the year, gives the interest rate gap:

$$x_{i,t} = \frac{R_{i,t} - R^*_{i,t}}{B_{i,t-1}}$$

where $B_{i,t-1}$ is the amount of total borrowing at the beginning of the period ($B_{i,t-1} = BS_{i,t-1} + BL_{i,t-1} + Bonds_{i,t-1} + CP_{i,t-1}$) with $CP_{i,t-1}$ denoting commercial paper outstanding. A negative gap ($x_{i,t} < 0$) indicates that a firm's observed interest payments ($R_{i,t}$) fall below the most favorable market rate implied by $R^*_{i,t}$, suggesting the firm is receiving subsidized credit or loan evergreening from its banks. Accordingly, Caballero, Hoshi, and Kashyap (2008) classify a firm as a zombie firm in year t when $x_{i,t} < 0$.

As Caballero, Hoshi, and Kashyap note, this measure cannot capture all forms of assistance—such as debt forgiveness, interest rate concessions, or off–balance-sheet support—but it provides a transparent, data-driven proxy for the extent of subsidized lending and the prevalence of zombie firms in Japan's corporate sector.

Figure A1: Prevalence of Firms Receiving Subsidized Loans in Japan Using a Stricter Zombie Definition (Credit Subsidy + Balance-Sheet Weakness)

This figure reports the share of listed firms classified as zombies—firms receiving subsidized loans and exhibiting balance sheet weakness—constructed following Acharya et al. (2024). Panel A shows the unweighted fraction of zombie firms, treating each firm equally. Panel B shows the asset-weighted fraction, weighting firms by total assets. Zombie prevalence rises sharply in the early 1990s and remains elevated thereafter, indicating the persistence of subsidized lending well beyond the initial post-bubble recession.

Table A1. Industry–Technology Mapping Based on Patent Shares (120 IPC technology classes; top two industries by patent share)

IPC class code	Technology field (IPC class level)	Top-share industry	Share (%)	Second-share industry	Share (%)
A01	Agriculture; Forestry; Animal Husbandry; Hunting; Trapping; Fishing	Machinery, non-electric	52.11	Chemicals	15.97
A21	Baking; Edible doughs	Food products	53.97	Chemicals	25.06
A22	Butchering; Meat Treatment; Processing Poultry or Fish	Food products	47.56	Machinery, non-electric	20.73
A23	Foods or Foodstuffs; Their Treatment, Not Covered by Other Classes	Food products	54.02	Chemicals	22.47
A24	Tobacco; Cigars; Cigarettes; Smokers' Requisites	Food products	73.21	Chemicals	16.03
A41	Wearing Apparel	Textile mill products	41.56	Wholesale	13.48
A42	Headwear	Motor vehicles	28.31	Chemicals	25.30
A43	Footwear	Misc manufacturing	38.84	Chemicals	21.71
A44	Haberdashery; Jewellery	Precision machinery	22.56	Misc manufacturing	18.75
A45	Hand or Travelling Articles	Electric machinery	37.36	Chemicals	34.12
A46	Brushware	Chemicals	67.80	Electric machinery	18.40
A47	Furniture (arrangements of seats for, or adaptation of seats to, vehicles B60n)	Electric machinery	46.29	Misc manufacturing	14.03
A61	Medical or Veterinary Science; Hygiene	Chemicals	24.84	Electric machinery	24.43
A62	Life-Saving; Fire-Fighting	Electric machinery	47.62	Machinery, non-electric	12.92
A63	Sports; Games; Amusements	Machinery, non-electric	42.12	Misc manufacturing	13.34
B01	Physical or Chemical Processes or Apparatus in General	Machinery, non-electric	26.64	Chemicals	22.60
B02	Crushing, Pulverising, or Disintegrating; Preparatory Treatment of Grain for Milling	Machinery, non-electric	47.36	Electric machinery	16.85
B03	Separation of Solid Materials Using Liquids	Machinery, non-electric	39.53	Electric machinery	33.21
B04	Centrifugal Apparatus or Machines for Carrying-Out Physical	Machinery, non-electric	70.47	Electric machinery	12.20
B05	Spraying or Atomising in General; Applying Liquids or Other Fluent Materials to Surfac	Chemicals	25.09	Electric machinery	20.19
B06	Generating or Transmitting Mechanical Vibrations in General	Electric machinery	56.11	Precision machinery	12.47
B07	Separating Solids from Solids; Sorting	Electric machinery	39.70	Machinery, non-electric	34.48
B08	Cleaning	Electric machinery	38.92	Machinery, non-electric	22.24
B09	Disposal of solid waste; Reclamation of contaminated soil	Electric machinery	30.76	Machinery, non-electric	24.76
B21	Mechanical Metal-Working without Essentially Removing Material; Punching Metal	Steels	36.54	Machinery, non-electric	23.71
B22	Casting; Powder Metallurgy	Steels	36.22	Machinery, non-electric	17.71
B23	Machine Tools; Metal-Working Not Otherwise Provided for	Machinery, non-electric	28.71	Electric machinery	26.68
B24	Grinding; Polishing	Machinery, non-electric	23.97	Electric machinery	22.57
B25	Hand tools; Portable power-driven tools; Handles for hand implements	Electric machinery	45.46	Machinery, non-electric	25.02
B26	Hand Cutting Tools; Cutting; Severing	Electric machinery	38.47	Machinery, non-electric	12.88
B27	Working or Preserving Wood or Similar Materials; Nailing or Stapling	Misc manufacturing	22.46	Electric machinery	20.91
B28	Working Cement, Clay, or Stone	Chemicals	17.69	Ceramics	17.16
B29	Working of Plastics; Working of Substances in a Plastic State in General	Chemicals	28.18	Machinery, non-electric	17.63
B30	Presses	Machinery, non-electric	53.62	Electric machinery	14.23
B31	Making paper articles; Working paper	Misc manufacturing	32.00	Machinery, non-electric	22.00
B32	Layered Products	Chemicals	37.22	Misc manufacturing	18.68
B41	Printing; Lining Machines; Typewriters; Stamps	Electric machinery	61.28	Chemicals	10.09
B42	Bookbinding; Albums; Files; Special Printed Matter	Misc manufacturing	58.41	Electric machinery	22.31
B43	Writing or Drawing Implements; Bureau Accessories	Misc manufacturing	63.11	Electric machinery	23.79
B44	Decorative Arts	Misc manufacturing	56.30	Chemicals	13.32
B60	Vehicles in General	Motor vehicles	58.59	Rubber products	12.61
B61	Railways	Electric machinery	55.76	Machinery, non-electric	13.27
B62	Land Vehicles for Travelling Otherwise than on Rails	Motor vehicles	65.74	Machinery, non-electric	20.93
B63	Ships or Other Waterborne Vessels; Related Equipment	Machinery, non-electric	49.28	Motor vehicles	26.02
B64	Aircraft; Aviation; Cosmonautics	Machinery, non-electric	38.98	Electric machinery	37.40
B65	Conveying; Packing; Storing; Handling Thin or Filamentary Material	Electric machinery	36.42	Machinery, non-electric	15.92
B66	Hoisting; Lifting; Hauling	Electric machinery	48.59	Machinery, non-electric	28.66
B67	Opening or closing bottles, jars or similar containers; Liquid handling	Machinery, non-electric	29.50	Motor vehicles	23.29
B68	Saddlery; Upholstery	Motor vehicles	50.00	Textile mill products	29.79
B81	Micro-Structural Technology	Electric machinery	63.84	Precision machinery	16.95
B82	Nano-Technology	Electric machinery	77.14	Chemicals	8.57
C01	Inorganic Chemistry	Chemicals	40.18	Electric machinery	18.23
C02	Treatment of Water, Waste Water, Sewage, or Sludge	Machinery, non-electric	40.93	Electric machinery	21.44
C03	Glass; Mineral or Slag Wool	Ceramics	29.35	Non-ferrous metal products	17.49
C04	Cements; Concrete; Artificial Stone; Ceramics	Ceramics	24.07	Electric machinery	22.65
C05	Opening or closing bottles, jars or similar containers; Liquid handling	Chemicals	45.47	Machinery, non-electric	19.80
C06	Explosives; Matches	Chemicals	86.06	Motor vehicles	4.53
C07	Organic Chemistry	Chemicals	62.17	Medical products	19.70
C08	Organic Macromolecular Compounds; Their Preparation	Chemicals	67.53	Textile mill products	10.20
C09	Dyes; Paints; Polishes; Natural Resins; Adhesives	Chemicals	63.27	Electric machinery	10.24
C10	Petroleum, Gas or Coke Industries; Technical Gases Containing Carbon Monoxide	Steels	22.68	Machinery, non-electric	19.33
C11	Animal or Vegetable Oils, Fats, Fatty Substances or Waxes;	Chemicals	83.02	Food products	5.23
C12	Biochemistry; Beer; Spirits; Wine; Vinegar; Microbiology; Enzymes	Chemicals	28.29	Food products	19.70
C13	Sugar Industry	Machinery, non-electric	57.14	Food products	25.71
C14	Skins; Hides; Pelts; Leather	Chemicals	45.83	Machinery, non-electric	12.50
C21	Metallurgy of Iron	Steels	82.04	Motor vehicles	4.17
C22	Metallurgy (of iron C21); Ferrous or non-ferrous alloys;	Steels	51.63	Non-ferrous metal products	21.47

C23	Coating Metallic Materials; Coating Materials with Metallic Materials	Electric machinery	30.69	Steels	24.33
C25	Electrolytic or Electrophoretic Processes; Apparatus Therefor	Electric machinery	20.91	Steels	20.44
C30	Crystal Growth	Electric machinery	34.13	Non-ferrous metal products	24.83
D01	Natural or artificial threads or fibres; Spinning	Textile mill products	46.04	Chemicals	31.56
D02	Yarns; Mechanical Finishing of Yarns; Warping or Beaming	Textile mill products	66.91	Chemicals	23.08
D03	Weaving	Machinery, non-electric	47.37	Textile mill products	33.45
D04	Braiding; Lace-making; Knitting; Trimmings; Non-woven fabrics	Textile mill products	46.94	Chemicals	20.47
D05	Sewing; Embroidering; Tufting	Electric machinery	71.22	Machinery, non-electric	16.20
D06	Treatment of Textiles or the Like; Laundering; Flexible Materials	Electric machinery	43.81	Textile mill products	23.58
D07	Ropes; Cables Other than Electrical	Non-ferrous metal products	43.33	Rubber products	36.01
D21	Paper-Making; Production of Cellulose	Paper & allied products	40.87	Chemicals	23.18
E01	Construction of Roads, Railways, or Bridges	Construction	23.49	Machinery, non-electric	19.09
E02	Hydraulic Engineering; Foundations; Soil-Shifting	Construction	41.45	Machinery, non-electric	30.65
E03	Water Supply; Sewerage	Ceramics	31.76	Electric machinery	22.66
E04	Building	Construction	36.75	Chemicals	20.77
E05	Locks; Keys; Window or Door Fittings; Safes	Motor vehicles	30.29	Electric machinery	29.15
E06	Doors, windows, shutters, or roller blinds, in general; Ladders	Non-ferrous metal products	40.80	Misc manufacturing	11.94
E21	Earth or Rock Drilling; Mining	Construction	48.88	Machinery, non-electric	25.43
F01	Machines or Engines in General; Engine Plants in General; Steam Engines	Motor vehicles	60.50	Machinery, non-electric	18.94
F02	Combustion Engines; Hot-Gas or Combustion-Product Engine Plants	Motor vehicles	68.38	Electric machinery	17.20
F03	Machines or engines for liquids (for liquids and elastic fluids	Electric machinery	52.05	Machinery, non-electric	30.97
F04	Positive-Displacement Machines for Liquids; Pumps	Electric machinery	39.67	Machinery, non-electric	39.31
F15	Fluid-Pressure Actuators; Hydraulics or Pneumatics in General	Machinery, non-electric	55.07	Motor vehicles	23.17
F16	Engineering Elements or Units; General Measures for Producing	Motor vehicles	35.26	Machinery, non-electric	25.99
F17	Storing or Distributing Gases or Liquids	Machinery, non-electric	37.49	Electric machinery	15.53
F21	Lighting	Electric machinery	56.80	Motor vehicles	27.63
F22	Steam Generation	Machinery, non-electric	70.39	Electric machinery	22.26
F23	Combustion Apparatus; Combustion Processes	Machinery, non-electric	38.80	Electric machinery	25.69
F24	Heating; Ranges; Ventilating	Electric machinery	56.27	Machinery, non-electric	17.00
F25	Refrigeration or Cooling; Combined Heating and Refrigeration Systems; Heat Pumps	Electric machinery	59.45	Machinery, non-electric	26.83
F26	Drying	Machinery, non-electric	42.83	Electric machinery	27.25
F27	Furnaces; Kilns; Ovens; Retorts	Steels	38.22	Electric machinery	18.04
F28	Heat Exchange in General	Machinery, non-electric	33.23	Electric machinery	32.10
F41	Weapons	Machinery, non-electric	47.08	Electric machinery	36.22
F42	Ammunition; Blasting	Electric machinery	34.35	Machinery, non-electric	32.06
G01	Measuring; Testing	Electric machinery	52.87	Precision machinery	10.82
G02	Optics	Electric machinery	53.71	Precision machinery	17.70
G03	Photography; Cinematography; Analogous Techniques	Electric machinery	49.13	Precision machinery	22.77
G04	Horology	Precision machinery	50.22	Electric machinery	42.78
G05	Controlling; Regulating	Electric machinery	65.36	Machinery, non-electric	16.28
G06	Computing; Calculating; Counting	Electric machinery	86.47	Misc manufacturing	2.53
G07	Checking-Devices	Electric machinery	78.58	Machinery, non-electric	15.80
G08	Signalling	Electric machinery	71.21	Motor vehicles	13.05
G09	Educating; Cryptography; Display; Advertising; Seals	Electric machinery	77.60	Misc manufacturing	5.55
G10	Musical Instruments; Acoustics	Electric machinery	49.25	Misc manufacturing	39.13
G11	Information Storage	Electric machinery	86.18	Chemicals	6.27
G12	Instrument Details	Electric machinery	60.64	Precision machinery	15.69
G21	Nuclear Physics; Nuclear Engineering	Electric machinery	60.25	Machinery, non-electric	23.82
H01	Basic Electric Elements	Electric machinery	77.21	Non-ferrous metal products	4.86
H02	Generation, Conversion, or Distribution of Electric Power	Electric machinery	76.75	Motor vehicles	5.87
H03	Basic Electronic Circuitry	Electric machinery	93.66	Precision machinery	1.42
H04	Electric Communication Technique	Electric machinery	89.64	Precision machinery	4.65
H05	Electric Techniques Not Otherwise Provided for	Electric machinery	74.95	Chemicals	7.35

This table reports, for each of the 120 IPC technology classes, the two industries with the largest shares of patenting activity. Patent shares w_{kj} are computed as the fraction of patents in technology class k that are applied for by firms in industry j. Industries are ranked within each technology class by w_{kj}, and the table lists the first- and second-ranked industries for each class.

Table A2. Correlation between Corporate and Non-Corporate Patent

Panel A: Patent Grants

	(1)	(2)	(3)
	ln(1+Grants), non-corporate	ln(1+Grants), non-corporate	ln(1+Grants), non-corporate
ln(1+Grants), corporate	0.482***	0.537***	0.481***
	(0.012)	(0.095)	(0.085)
Observations	1,314	1,314	1,314
R-squared	0.924	0.906	0.913
R-squared (within)		0.0808	0.0694
Technology class fixed effect	No	Yes	Yes
Year fixed effects	No	No	Yes

Panel B: Forward Citations

	(1)	(2)	(3)
	ln(1+Citations), non-corporate	ln(1+Citations), non-corporate	ln(1+Citations), non-corporate
ln(1+Citations), corporate	0.509***	0.456***	0.451***
	(0.011)	(0.112)	(0.106)
Observations	1,314	1,314	1,314
R-squared	0.920	0.857	0.862
R-squared (within)		0.0521	0.0516
Technology class fixed effect	No	Yes	Yes
Year fixed effects	No	No	Yes

This table examines the co-movement between corporate and non-corporate patenting activity at the technology-class level. The unit of observation is a technology class–year. The dependent variable is ln(1 + y) of non-corporate patent grants in Panel A and ln(1 + y) of forward citations to non-corporate patents in Panel B. The key explanatory variable is the corresponding ln(1 + y) measure for corporate patent grants or forward citations. Columns progressively add technology-class fixed effects and application-year fixed effects. Standard errors are clustered at the technology-class level and reported in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively.