Public Sector Exposure and Supplier Profitability

Evidence from Swedish Procurement Data

DOUGLAS DALÉN
ARAM JIMAL

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Abstract

Swedish public procurement corresponded to 18 percent of GDP in 2023, yet the profitability consequences for suppliers remain poorly understood, particularly among the private firms that win three fourths of all public tenders. We link a novel dataset of 61 million Swedish public entities procurement invoices to firm-level financial statements for the near-universe of Swedish limited liability companies, producing a baseline panel of 512,877 firm-year observations, of which 149,794 firm-years supply the public sector. Using this panel, we examine whether public-sector revenue share (PSR) is associated with firm profitability. In the full sample, the participation margin dominates, and the association between PSR and return on assets is small, negative, and statistically insignificant. A DuPont decomposition of return on assets reveals two significant offsetting forces, where PSR is negatively associated with operating margin but positively associated with asset turnover. Restricting the sample to firms with strictly positive public-sector revenue (intensity margin) reverses this result, with higher PSR significantly associated with higher return on assets and operating margin but lower asset turnover. The intensity-margin findings are consistent with the demand-uncertainty channel, as PSR is negatively associated with firm-level demand uncertainty across three-year, five-year, and seven-year rolling windows, while the investment-efficiency channel receives weaker support. A buyer-type decomposition shows that central-government and regional exposure showcase return on assets coefficients roughly two to three times those for municipal exposure. The primary contribution is the participation-versus-intensity margin decomposition, which is structurally unobservable in disclosure-based samples.

Keywords:

Public procurement, business to government, firm profitability, demand uncertainty, investment efficiency, customer concentration, participation margin, intensity margin

Authors:

Douglas Dalén (50786)
Aram Jimal (24895)

Tutor:

Henrik Nilsson, Department of Accounting

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Douglas Dalén Aram Jimal

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1. Introduction

Governments are among the largest purchasers of goods and services in most economies, yet the performance consequences for their suppliers remain poorly understood. In Sweden, public-sector procurement exceeded SEK 980 billion in 2023, corresponding to 18 percent of GDP (The National Agency for Public Procurement, 2025), and small and medium-sized enterprises win approximately three-fourths of contracted tenders (The National Agency for Public Procurement, 2026a). The aggregate spending figures include procurement by the state, regions, municipalities, and publicly owned companies, and is in this study collectively referred to as *the public sector*.

Despite this scale, the supplier-side consequences of public procurement remain comparatively underexplored. The closest body of evidence comes from the U.S. accounting and supply-chain literatures, but each strand operates under sample restrictions that limit what can be said about the broader supplier population. Cohen and Li (2020), Patatoukas (2012), and Goldman (2020) rely on SEC segment disclosures, which require reporting only when a customer accounts for at least 10 percent of revenues (U.S. Securities and Exchange Commission, 2023). Josephson et al. (2019) and Falcone et al. (2023, 2024) match federal procurement databases to Compustat/CRSP. Both routes condition on listed-firm status, and the disclosure-based studies further condition on a binary threshold. The smaller, privately held firms that account for the majority of public procurement spending in most economies are absent from these samples by construction, even though Falcone et al. (2024) identify firm size as a moderator and document that benefits accrue most strongly to larger firms in their listed-firm sample. Whether the patterns documented for U.S. listed firms also operate in private firms outside SEC monitoring is therefore an open question.

We examine whether a higher share of revenue derived from Swedish public-sector buyers is associated with higher firm profitability. We also investigate through what mechanisms any such association operates. The central construct in our analysis is *public-sector revenue share* (PSR), defined as the share of a firm's total revenues deriving from public entities in a given year. PSR captures the degree to which a firm is exposed to public-sector demand, regardless of how that revenue is distributed across individual buyers.

We construct PSR by linking a novel dataset of 61 million procurement invoices from Swedish public entities (Nylander, 2026) to the Retriever financial database (Retriever, 2026), which covers the full universe of Swedish limited liability companies (*aktiebolag*). The invoice data is drawn from records obtained under the Swedish principle of freedom of information (*offentlighetsprincipen*), capturing both formally tendered contracts and direct procurement. Our measure is continuous, threshold-free, and covers both listed firms and the private firms that account for the vast majority of Swedish employment and output (European Commission, 2025), which reveals practical consequences. Approximately 79 percent of firm-years with strictly positive public-sector revenue in our panel record an aggregated share of revenues from public buyers below 10 percent, the level at which SEC regulation and IFRS 8 require disclosure of an *individual* major customer (U.S. Securities and Exchange Commission, 2023; IASB, 2006), and the threshold floor used in previous studies.

The Swedish institutional setting is particularly suited to this question for three reasons. Public procurement in Sweden is governed by the Public Procurement Act (*Lagen om offentlig upphandling*, LOU) and related legislation, which covers formally tendered contracts above defined thresholds and permits simplified direct procurement (*direktupphandling*) below them (Riksdagen, 2016; The National Agency for Public Procurement, 2022).

First, the principle of public access to official records makes invoice-level procurement data accessible at the supplier-buyer-year level. Second, all Swedish limited liability companies are required to file annual financial statements with the Swedish Companies Registration Office, providing near-universal coverage of the private-firm population in a single comparable accounting environment. Third, the Swedish public sector is institutionally heterogeneous, spanning central agencies, regions, municipalities, publicly owned companies, and universities, allowing buyer-type heterogeneity to be examined directly at a level of granularity not available in the U.S. evidence, where fewer than one percent of sample firms report state, local, or foreign governments as major customers (Cohen and Li, 2020).

Theory offers competing predictions about the sign of the PSR–profitability association. Public-sector buyers carry minimal default risk, operate under procurement rules that provide procedural predictability and constrain opportunistic renegotiation, and generate demand that is largely uncorrelated with business-cycle fluctuations (Cohen and Li, 2020; Goldman, 2020). These features have been linked to lower demand uncertainty for suppliers, which in turn is associated with more efficient investment and lower excess-capacity costs (Guiso and Parigi, 1999; Bloom et al, 2007). An opposing prediction arises from the bargaining-power view. From that perspective, public buyers may use their scale and procedural leverage to compress supplier margins, similar to the rent extraction dynamics documented for powerful corporate buyers (Lustgarten, 1975; Hui et al, 2019).

Recent evidence from the supply-chain literature suggests the relationship may be nonlinear. Josephson et al. (2019) find that a firm's revenue dependence on government customers, a construct closely analogous to our PSR, exerts a positive, accelerating effect on firm value, as the operational efficiencies and scale benefits of government contracting increasingly outweigh the upfront compliance costs at higher levels of exposure. Falcone et al. (2024), using a different measure based on the share of government agencies in the firm's customer count, document an inverse-U relationship with ROA, where both very low and very high government customer concentration are associated with weaker performance. These contrasting findings underscore that the sign, shape, and magnitude of the association may depend on both the measure of government exposure and the performance outcome studied. Determining the pattern of the relationship in the Swedish private-firm context is an empirical question that our data is well positioned to address.

Our primary research question is: *Is public-sector revenue share associated with supplier firm profitability?* We further examine whether the association varies with industry-level demand volatility, whether it differs across public buyer types, and whether the concentration across public buyer types moderates the relationship. The formal development of these hypotheses is presented in Section 3, following the literature review.

1.1. Main Findings

In the full-sample analysis, the association between PSR and ROA is small, negative, and statistically insignificant after controlling for industry and year fixed effects. A DuPont decomposition shows that this near-zero coefficient masks two opposing forces. While PSR is negatively associated with operating margin, it is positively associated with asset turnover. This can be interpreted as public-sector suppliers operating with compressed margins but more efficient asset utilization relative to industry peers.

This full-sample result, however, is largely attributable to the *participation margin*, the compositional difference between firms that do and do not supply the public sector. Public-sector suppliers are systematically larger, older, and concentrated in different industries than non-suppliers, and these compositional differences dominate the pooled regression. The *intensity margin*, by contrast, captures variation in how much firms supply the public sector, conditional on being a supplier.

When we restrict the sample to firm-years with strictly positive public-sector revenue, the results reverse, and higher PSR is associated with significantly higher ROA and operating margin, though with lower asset turnover. This participation-versus-intensity margin decomposition constitutes the primary empirical contribution of the thesis. It shows that the null full-sample association reflects who enters the public-sector supplier population rather than what happens to profitability as firms increase their public-sector exposure. The sign and significance of the PSR–profitability association change once the comparison is restricted to firms that already participate in public procurement, a decomposition with no direct precursor in the government-customer literature, where disclosure thresholds and public-firm data constraints mean the participation margin is either unobservable or unexamined.

We find support for the demand-uncertainty mechanism proposed by Cohen and Li (2020). PSR is negatively associated with firm-level demand uncertainty, with a coefficient that is robust across three-year, five-year, and seven-year rolling windows. The investment-efficiency channel (Guiso and Parigi, 1999; Bloom et al, 2007) receives weaker support. The forward specification, which tests whether current PSR is associated with higher responsiveness of future investment to growth opportunities, is statistically insignificant, while the contemporaneous specification is significant but more susceptible to reverse causality. A size-heterogeneity analysis shows that the positive intensity-margin ROA association is concentrated among larger firms, driven by higher asset-turnover gains, while smaller firms experience negative ROA effects from public-sector exposure.

Because our baseline panel covers only two calendar years and the research design is associational, these findings document conditional correlations rather than causal effects. The identification limitations, measurement challenges, and the scope of what the data can and cannot establish are discussed in detail later.

1.2. Contributions

Our work contributes to the literature in three main ways. First, and most centrally, the thesis documents that the association between public-sector exposure and supplier profitability operates through two distinct margins that prior literature has not separated. In the full sample, the PSR–ROA association is small and statistically insignificant, as suppliers and non-suppliers differ systematically in size, age, and industry mix, and these differences in who enters the public-sector supplier population dominate the pooled estimate. Within suppliers, higher PSR is positively associated with profitability, both statistically and economically meaningful. Separating these two margins is the central empirical contribution of our thesis.

Second, by measuring public-sector exposure through invoice records rather than disclosure-based thresholds, the thesis introduces a continuous, firm-wide measure that covers the small and medium-sized private firms that account for the majority of Swedish public procurement spending (The National Agency for Public Procurement, 2026a) and that are absent from disclosure-based samples by construction. This broadens the empirical scope of the government-customer literature beyond listed firms and allows the analysis to test whether the documented associations hold across firms with fundamentally different resource bases, information environments, and accounting regimes, at the cost of introducing different measurement challenges.

Third, the analysis decomposes the PSR–profitability association by public buyer type, separating municipalities, regions, central-government agencies, publicly owned companies, universities, and university colleges. To our knowledge, this granular buyer-type decomposition has not been performed at this level of detail in the prior literature. The U.S. evidence is structurally constrained to federal procurement (Josephson et al, 2019; Falcone et al, 2023, 2024) or to firms with major federal customers (Cohen and Li, 2020), and so cannot observe heterogeneity across subnational government units that, in our setting, exhibit distinct fiscal characteristics and procurement practices.

1.3. Structure

The remainder of this study is structured as follows. Section 2 reviews our three bodies of literature, government customers as a distinct buyer type, customer concentration and demand uncertainty as a mechanism for profitability through investment efficiency. Section 3 develops four hypotheses linking PSR to profitability, industry-level demand volatility, public buyer-type heterogeneity, and within-public buyer concentration. Section 4 describes the procurement and financial data sources, the construction and validation of PSR, sample selection, variable definitions, the empirical specifications used to test each hypothesis, and the identification, measurement, and limitations of the design. Section 5 presents the baseline profitability results, the participation-versus-intensity margin decomposition, the heterogeneity and mechanism tests, and the robustness checks. Section 6 discusses the implications of the findings, situates them against the recent U.S. evidence, and identifies what the Swedish private-firm setting adds. Section 7 sets out the limitations of the analysis and outlines avenues for further research as the procurement data continue to expand.

2. Literature Review

The empirical question this thesis addresses, whether public-sector revenue exposure is associated with private-firm profitability, sits at the intersection of three bodies of research that have evolved largely independently. The government-customer literature, primarily a U.S. accounting and supply-chain literature built on SEC disclosure data and federal procurement records, isolates government buyers as a distinct customer type but does so within the listed-firm population. The customer-concentration literature, originating in industrial organization and further developed in the accounting literature, offers a general framework for how the structure of a firm's customer base affects supplier performance, but typically treats government and corporate customers as substitutable. The investment-under-uncertainty literature, drawn from real-options theory and corporate-finance empirics, provides the mechanism that links these two strands by showing how differences in demand stability map into differences in firms' investment behavior and, through investment choices, into differences in realized profitability.

Reviewing these three bodies of research together exposes a set of design choices in the prior literature, including disclosure-based sample restrictions, the dominance of U.S. listed-firm settings, limited buyer-type granularity, and the difficulty of separating the underlying mechanisms with the data available in those settings, that motivate the empirical strategy adopted by us. We return to the implications of the institutional and data features of the different settings later in this section.

The remainder of this section proceeds as follows. Section 2.1 examines the institutional features that distinguish government from corporate buyers and the corresponding evidence on profitability. Section 2.2 reviews the customer-concentration literature and the unresolved Patatoukas-Hui debate that determines what conclusions can be drawn about supplier rents under powerful buyers. Section 2.3 traces the demand-uncertainty through investment-efficiency profitability chain that supplies the principal mechanism. Section 2.4 surveys the parallel financing channel through which government customers affect supplier capital structure and credit terms. Section 2.5 synthesizes across the three bodies of literature, evaluates which findings travel to the Swedish private-firm setting, and identifies the gaps the empirical analysis is designed to address.

2.1. Government Customers as a Distinct Buyer Type

Government entities are buyers whose scale, statutory mandates, and institutional rules set them apart from private corporate counterparties. The literature identifies three recurring features along the dimensions of demand stability, monitoring requirements, and contractual risk-shifting.

First, public-sector demand is largely uncorrelated with the business cycle. Goldman (2020) documents that during the 2008 to 2009 financial crisis, federal contractors experienced 15 percent higher market capitalization, 18 percent higher capital expenditures, and 26 percent more bank credit than otherwise comparable firms, driven not by counter-cyclical stimulus but by the simple stability of recurrent procurement. Cohen and Li (2020) confirm the uncertainty

implication at the firm level, showing that suppliers with concentrated government customers exhibit lower demand uncertainty, measured by sales growth volatility, than suppliers with concentrated corporate customers.

Second, government contracting imposes monitoring and transparency requirements that exceed those typical of private supply chains. Samuels (2021) shows that U.S. government procurement requirements enhance suppliers' internal controls, with effects most pronounced for contracts entailing greater scrutiny. Cohen et al. (2022) document a parallel result on the lending side, finding that firms with major government customers face fewer restrictive loan covenants, consistent with creditors treating government oversight as a substitute for their own monitoring. The reporting and disclosure obligations attached to government contracting also affect supplier transparency more broadly. Building on these internal improvements, Samuels (2021) finds that procurement-related controls feed into the quality of contractors' external reporting. A separate body of work on industry-level information environments shows that better information environments are associated with more efficient private-firm investment, though through a different channel. Badertscher et al. (2013) document that private-firm investment is more responsive to growth opportunities in industries with greater listed-firm presence, which they attribute to information spillovers from public-firm disclosure. We draw on Badertscher et al. (2013) for our investment-efficiency test design in later sections.

Third, government contracts include clauses, specifically cost-plus reimbursement and non-commercial classifications, that shift demand and price risk from the supplier to the buyer (Cohen and Li, 2020). Hebous and Zimmermann (2021) provide complementary evidence using federal procurement contract awards as firm-level demand shocks, finding that one dollar of federal purchases increases capital investment of financially constrained firms by 10 to 13 cents over a four-quarter horizon, with no measurable effect on unconstrained firms. The mechanism here is the relaxation of borrowing constraints through pledgeable future cash flows, again distinct from Cohen and Li's (2020) demand-uncertainty channel. Government as customer can therefore operate through at least three conceptually distinct mechanisms whose empirical separation has rarely been attempted, namely uncertainty reduction, monitoring substitution, and financial-accelerator easing.

Empirically, the central finding is differential. Cohen and Li (2020) report that, for firms with major government customers, a one-standard-deviation increase in total customer concentration is associated with a 0.87 percentage-point higher ROA and a 2.13 percentage-point higher ROE, while corporate-customer concentration is associated with the opposite signs. They further show, exploiting the 1984 enactment of the Federal Acquisition Regulation (FAR) as a quasi-exogenous shock, that government suppliers experienced a sharper decline in demand uncertainty and a sharper rise in profitability after the regulation took effect. The closest Swedish counterpart is the enactment of the first version of LOU (Riksdagen, 1992).

Subsequent supply-chain work extends this evidence but reaches mixed conclusions about the functional form of the relationship. Josephson et al. (2019), measuring revenue dependence on government customers among U.S.-listed firms, find a positive and convex effect on firm

value,[1] with compliance costs dominating at low levels of government customer emphasis and learning and scale economies dominating at higher levels. Their qualitative interviews underscore that the profitability benefits emerge primarily through aggregation, with one practitioner describing government contracting as "*extremely lucrative when you start to aggregate a lot of low-margin, but big-dollar and long-term kinds of relationships*".[2] Falcone et al. (2024), using a different construct based on the share of a firm's customers that are government agencies, document an inverse-U relationship with ROA, while Falcone et al. (2023) find that government contracting is associated with higher short-term performance but lower long-term performance, suggesting the benefits attenuate or reverse over time. The contrast across these three studies reflects the fact that the construct, the outcome, and the horizon each shape the inferred relationship.

2.2. Customer Concentration: Bargaining Power Versus Efficiency Gains

The prior section treats government buyers as a distinct type. A complementary question is how the overall structure of a supplier's customer base, regardless of whether buyers are public or private, shapes performance. This is the domain of the customer-concentration literature, which begins with a pair of competing predictions.

The *bargaining-power view*, originating in industrial organization, holds that powerful customers compress supplier price-cost margins (Lustgarten, 1975). Later work shows that buyer power also operates through extended trade credit, dedicated inventory, and customer-specific investments that the supplier cannot easily redeploy (Ak and Patatoukas, 2016; Irvine et al, 2016; Campello and Gao, 2017), turning negotiation into a hold-up problem in which sunk costs cannot be fully recovered. The *collaboration view*, by contrast, drawn from work on long-term buyer-supplier relationships, holds that concentrated customer bases enable joint marketing, supply-chain co-investment, and overhead reductions, predicting the opposite sign (Patatoukas, 2012).

Empirically, the evidence has accumulated unevenly across the two views. Hui et al. (2019) demonstrate that concentration is associated with lower supplier profitability and higher major-customer profitability, consistent with rent extraction by powerful buyers, with the negative association attenuating as the supplier's own countervailing power grows. Patatoukas (2012) introduces a Herfindahl-style measure of customer-base concentration (CC) and reports a positive contemporaneous association with both ROA and asset turnover alongside lower SGA-to-sales, which he interprets as supply-chain efficiency gains. Hui et al. (2019) challenge this result, showing that Patatoukas's procedure excludes firms with negative operating margins. Reintroducing loss firms reverses the sign of the CC–ROA coefficient. Irvine et al. (2016) refine the picture temporally, finding that the early negative effect eventually attenuates, while Hui et al. (2019) argue that the attenuation never crosses zero.

[1] To measure firm value, the authors use Tobin's Q, the ratio of a firm's market value to the replacement cost of its assets.
[2] Josephson et al. (2019, p. 58), interview with a practitioner expert.

This unresolved debate has direct implications for any subsequent study built on the Patatoukas measure. While Cohen and Li (2020) inherit the CC framework and explicitly avoid the truncation bias by including loss firms, their findings remain exposed to the disclosure-threshold limitations inherent in the U.S. setting. The Swedish setting permits a direct examination of the underlying selection problem, since the procurement-invoice numerator is not constructed from disclosure thresholds and the financial-statement denominator is observed for the full universe of limited liability firms, regardless of profitability.

A related body of work documents that customer concentration affects suppliers through cost-of-capital and financing channels alongside its effects on operating margins. Dhaliwal et al. (2016) show that concentrated customer bases raise the implied cost of equity capital. Campello and Gao (2017) find that concentration increases both loan spreads and the number of restrictive covenants. They also replicate Patatoukas's positive profitability result, though Hui et al. (2019) note that this replication rests on the same truncation procedure and is therefore exposed to the same critique. Hertzel et al. (2008) document that customer bankruptcy filings transmit negative wealth effects to suppliers along the supply chain, with stronger contagion when suppliers operate in concentrated industries. Together, these studies establish that the bargaining-power view is empirically credible at least for corporate concentration, and that any positive effects must be evaluated against documented financing-cost penalties.

The tension between bargaining power and efficiency gains provides a useful lens for interpreting buyer-supplier outcomes, and the Patatoukas–Hui debate is a reminder that selection on the dependent variable can produce results that look strong but do not survive a cleaner sample, a risk that also applies to our study.

2.3. The Demand Uncertainty, Investment Efficiency, and Profitability Chain

The preceding sections identify the public buyer and the structure of the customer base as determinants of supplier outcomes, but do not always trace the pathway through which these effects operate. This section reviews the sequential mechanism that links customer-base composition to realized profitability. Customer-base composition shapes firm-level demand uncertainty, demand uncertainty shapes investment efficiency, and investment efficiency shapes profitability. Each step has independent theoretical and empirical foundations.

The first step rests on real-options theory of investment under uncertainty (Dixit and Pindyck, 1994). When investment is partially irreversible, increases in demand uncertainty raise the option value of waiting, dampening the responsiveness of investment to demand signals. Guiso and Parigi (1999) test this prediction directly using subjective probability distributions of future demand from a panel of manufacturers, finding that uncertainty significantly weakens the response of investment to expected demand, particularly for firms whose investment is difficult to reverse. Bloom et al. (2007) generalize the result theoretically and show that heightened uncertainty does not necessarily reduce the level of investment, since the higher target capital stock can offset the cautionary effect, but it robustly reduces the responsiveness of investment to growth opportunities. This responsiveness-versus-level distinction is critical empirically.

Uncertainty effects are best identified through interaction terms, specifically uncertainty multiplied by growth opportunities, rather than through level coefficients on uncertainty alone.

The second step links investment responsiveness to information environment and frictions. Biddle et al. (2009) conceptualize investment efficiency as the alignment of total investment with underlying growth opportunities, and show that information asymmetry distorts this alignment, producing both over- and underinvestment. Their measurement strategy proxies investment opportunities by sales growth and estimates the expected investment level, an approach that has been adapted across the literature to test whether better information environments steepen the investment-growth slope. Badertscher et al. (2013) apply a related approach to private U.S. firms, finding that listed-firm presence in an industry steepens the private-firm investment-growth slope, consistent with information spillovers reducing uncertainty. Their implementation demonstrates that investment-efficiency tests of this kind can be conducted using balance-sheet items and revenue data alone, without market-based inputs, which is the relevant template for the Swedish private-firm setting examined here.

The third step links investment behavior to cost structure and profitability. Banker et al. (2014) show theoretically and empirically that firms facing higher demand uncertainty adopt more rigid cost structures, choosing higher fixed-input capacity to reduce congestion costs during demand spikes, consistent with operating leverage being more pronounced under uncertainty. Building on Irvine et al.'s (2016) baseline findings, Cohen and Li (2020) present complementary evidence on the elasticity of SG&A costs to sales, noting that corporate-customer concentration significantly reduces SG&A cost elasticity, consistent with overinvestment in customer-specific assets and excess capacity. In contrast, they find that government-customer concentration is associated with a weakly negative SG&A cost elasticity, a reduction substantially smaller in magnitude compared to corporate-customer concentration. This step closes the chain, demonstrating that differences in customer-driven uncertainty propagate through cost structure to realized profitability.

Mechanism evidence in this chain requires testing each step directly rather than substituting a reduced-form profitability regression. Uncertainty effects on investment are identified through interactions with growth opportunities, since level coefficients on uncertainty alone do not capture responsiveness, and this consideration governs the specification choice. The SG&A-cost-elasticity test applied to customer concentration by Irvine et al. (2016) and Cohen and Li (2020) is unavailable in a sample reporting classification-by-nature format, since SG&A is not separately reported. The investment-efficiency test, by contrast, requires only balance-sheet items and top-line sales, making it the natural mechanism test for this thesis.

2.4. Capital Structure and the Financing Channel

A parallel body of work documents that buyer characteristics affect supplier capital structure and external financing terms, which supplies a foundation for extending the analysis from operating returns to equity returns through the leverage decomposition.

Kale and Shahrur (2007) provide the main industry-level evidence. Firms reduce leverage when their suppliers and customers undertake relationship-specific investments, since lower leverage

commits the firm to a continuation policy that protects the rents both sides have invested in. Firms raise leverage, by contrast, when they face concentrated supplier or customer industries, consistent with leverage acting as a bargaining device that shields surplus from powerful counterparties. Together, these predictions imply that capital structure is endogenous to customer-base structure, which has implications for any cross-sectional comparison of profitability that fails to control for leverage.

For government customers specifically, Cohen et al. (2022) document fewer restrictive covenants and a lower likelihood of performance-pricing provisions in loans to firms with major government customers, with no comparable effect for major corporate customers. Using within-firm changes in supplier status, they further show that loans issued after a firm becomes a government supplier contain fewer covenants than loans issued before, consistent with lenders treating government oversight as reducing the marginal value of their own monitoring. Cohen and Li (2016) provide a related precautionary-motive result, finding that firms with major government customers hold significantly less cash than otherwise comparable firms, consistent with reduced precautionary demand under more predictable revenue.

Goldman's (2020) financial-crisis evidence reinforces the financing channel from the liability side. Government contractors retained better access to credit during the 2008 to 2010 downturn, with higher probability of receiving a credit extension and larger loan volume relative to comparable firms. Hebous and Zimmermann (2021) provide the investment-side complement, showing that the investment response to government demand shocks operates predominantly through financially constrained firms, consistent with a financial-accelerator mechanism in which contracted future receivables relax borrowing constraints.

The financing channel and the demand-uncertainty channel make similar predictions for profitability and investment but different predictions for cost of debt, covenant structure, and cash holdings, and they are not always cleanly separated in the literature. The leverage decomposition in our empirical analysis is designed with this in consideration, because operating profitability measures cannot detect financing-channel effects, while the PSR–ROE association can.

2.5. Research Gaps and Contributions

Read together, the three bodies of research above point to three areas where this thesis contributes.

First, and most centrally, our data permit separating the participation margin, the comparison between firms with zero and firms with positive public-sector revenue, from the intensity margin, the variation in exposure conditional on being a public-sector supplier. This separation is structurally unobservable in the U.S. evidence. Procurement-database samples can identify non-contractors within the Compustat universe, but they strictly capture federal procurement (e.g., FPDS). Firms that supply state or local governments but lack federal contracts are therefore absorbed into the non-supplier control groups, and the broader population of private non-suppliers is unobservable altogether. The Swedish setting, with near-exhaustive coverage of both supplier and non-supplier populations across all levels of government, allows the

participation margin to be observed and the within-supplier intensity margin to be estimated separately.

Second, the dominant evidence on government customer effects is U.S.-specific and disclosure-selected. Cohen and Li (2020), Patatoukas (2012), and Goldman (2020) rely on SEC segment disclosures, which require reporting only when a customer accounts for at least 10 percent of revenues (U.S. Securities and Exchange Commission, 2023). Josephson et al. (2019) and Falcone et al. (2023, 2024) match federal procurement databases to Compustat. All six studies are therefore conditioned on listed-firm status and, in the disclosure-based studies, on a binary threshold. Cohen and Li (2020) in particular divide their sample into firms with major government customers, major corporate customers, and hybrid firms with both, but every firm in all three groups has at least one customer exceeding the 10 percent threshold. Firms whose government revenue falls below the 10 percent threshold are either classified as non-government suppliers if they have a major corporate customer or excluded from the disclosure-based sample altogether, so the design cannot resolve variation in exposure within the sub-threshold range. Whether the documented mechanisms operate beyond the listed-firm population, in firms that are smaller, privately held, more financially constrained, and outside the SEC monitoring environment, has not been tested. The Swedish setting, with near-universal coverage of private firms and a continuous, threshold-free measure of public-sector exposure, permits this.

Third, the Swedish data permit a buyer-type decomposition of public-sector exposure spanning municipalities, regions, central government agencies, publicly owned companies, universities, and university colleges, a dimension that has not been studied at this level of granularity in the prior literature. Cohen and Li (2020) confine attention to the federal government because state, local, and foreign customers appear in fewer than one percent of disclosed major-customer relationships in their sample, so the institutional heterogeneity across subnational government units in our setting is structurally absent from the disclosure-based U.S. evidence. The PSR construct itself, capturing revenue dependence on the public sector analogously to Josephson et al.'s (2019) government customer emphasis rather than HHI-weighted concentration, is suited to a setting in which most public-sector suppliers contract with several public buyers of different institutional types.

In addition, the Swedish accounting environment simultaneously constrains certain mechanism tests and represents a contribution in its own right. Most Swedish private firms report under the K2 framework (BFNAR 2016:10) or the classification-by-nature format permitted under K3 (BFNAR 2012:1) and the Annual Accounts Act (Riksdagen, 1995). The BAS chart of accounts, used by approximately 95 percent of Swedish firms (FAR, 2026), organizes costs by input type rather than by function, so cost of goods sold, gross profit, and SGA are unavailable for the majority of the sample. The SG&A cost elasticity model developed by Anderson et al. (2003), as well as the full DuPont margin decomposition applied by Cohen and Li (2020), cannot therefore be replicated. The investment-efficiency test of Biddle et al. (2009) and Badertscher et al. (2013), by contrast, requires only balance-sheet items and revenue figures and is therefore the most informative mechanism test available in this setting. ROA, OM, ROE, PM, and ATO are consistently defined across K2 and K3, preserving the core profitability

analysis. More broadly, extending the analysis to this accounting environment tests whether the associations documented for U.S. listed firms hold across a different reporting regime, ownership structure, and information environment.

Against this backdrop, the thesis examines whether higher public-sector revenue exposure is associated with higher private-firm profitability among Swedish *aktiebolag*, whether any such association is moderated by industry-level demand volatility, varies across public buyer types, and is shaped by within-public buyer concentration, and whether the demand-uncertainty and investment-efficiency mechanisms documented for U.S. listed firms with government customers also operate in the Swedish private-firm environment. The four hypotheses developed in Section 3 operationalize these questions, drawing on the bodies of work reviewed above but adapted to the constructs, sample, and accounting environment available here.

3. Hypotheses

Section 2 establishes that buyer type affects supplier performance through demand-stability, monitoring, and risk-shifting channels, while customer-base concentration introduces competing bargaining-power and efficiency-gain predictions. We draw on these channels to develop four hypotheses about whether a firm's exposure to public-sector buyers, measured as the share of total revenue derived from the public sector, is associated with profitability and how this association varies across industry conditions, buyer types, and within-public concentration.

Before presenting the hypotheses, two features of this study's setting deserve emphasis. First, our baseline panel covers only two fiscal years (2022–2023). This horizon is too short to credibly identify causal effects of public-sector exposure on profitability. It does not allow us to exploit within-firm variation across business cycles, distinguish persistent treatment effects from transitory shocks, or rule out unobserved firm-level selection into public procurement. We therefore formulate all hypotheses as predictions about *associations* rather than causal effects, and we interpret estimated coefficients accordingly throughout the empirical analysis. Second, the underlying mechanisms motivating these predictions, including demand-uncertainty reduction, bargaining power, and within-segment concentration, are drawn primarily from U.S. evidence, most directly Cohen and Li (2020), Patatoukas (2012), and Hui et al. (2019). Our empirical analysis therefore tests whether the *Swedish data exhibit patterns consistent with these U.S. mechanisms*, not whether the mechanisms themselves operate causally in Sweden. Supportive associations in our setting are suggestive of, but not dispositive evidence for, the operation of the same channels. A failure to find the predicted associations would raise questions about external validity but cannot, with this data, identify the specific reason.

3.1. Public-Sector Exposure and Profitability

The theoretical motivation for our first hypothesis draws on two competing mechanisms. The demand-uncertainty channel documented by Cohen and Li (2020) predicts a positive association. As discussed in Section 2.1, public-sector buyers carry minimal default risk, operate under procurement rules that constrain opportunistic order changes, and generate demand that is largely uncorrelated with business-cycle fluctuations (Goldman et al, 2013; Goldman, 2020). In the U.S. setting, lower demand uncertainty has been linked to more efficient customer-specific investment, lower excess capacity, and leaner cost structures, all of which are associated with higher profitability (Guiso and Parigi, 1999; Bloom et al, 2007; Banker et al, 2014). In contrast, the bargaining-power view predicts a negative association. Because LOU requires competitive tendering, public buyers may use their scale and procedural leverage to compress supplier margins, in a manner analogous to powerful corporate customers (Lustgarten, 1975; Hui et al, 2019). Because these mechanisms operate simultaneously and in opposing directions, the net direction of the association is an empirical question, and we do not predict the sign of the relationship.

The empirical relevance of these competing channels depends on the level of public-sector exposure in the sample. At low exposure levels, the bilateral bargaining dynamics emphasized in the concentration literature are unlikely to be the primary channel. The question is instead whether even modest public-sector exposure is associated with measurable differences in profitability, and whether the association strengthens as exposure increases.

The recent supply-chain evidence reviewed in Section 2.1 provides additional context for this prediction along two dimensions. First, Josephson et al. (2019), using a revenue-dependence measure analogous to PSR, find an accelerating positive effect on firm value, suggesting that the benefits of public-sector supply materialize primarily at scale. Falcone et al. (2024), using a relational-breadth measure based on the share of government agencies in the customer count, document an inverse-U with ROA. Second, Falcone et al. (2023) find that government contracting is positively associated with short-term ROA but negatively with long-term firm value, so the temporal horizon over which performance is measured may itself shape the sign. Our continuous PSR construct most closely resembles the revenue-dependence measure of Josephson et al. (2019). The linear specification captures the average marginal association across the PSR distribution. The heterogeneity panels (size, participation vs intensity margin) provide indirect evidence on whether the relationship plateaus or accelerates.

A complementary financing-channel argument suggests that the predictable cash flows from public-sector buyers may also be associated with more favorable financing terms, such as fewer restrictive covenants (Cohen et al, 2022), and altered capital structure (Kale and Shahrur, 2007; Banerjee, Dasgupta, and Kim, 2008). If this channel is at work, any operating-level association should map through the leverage identity to equity returns. We test this transmission using a leverage decomposition. As noted above, evidence in our two-year panel can document whether these patterns appear in the Swedish data, but we are not able to identify them causally.

> **Hypothesis 1 (H1):** *Public-sector revenue share is associated with supplier firm profitability, measured by return on assets (ROA), return on equity (ROE), operating margin (OM), and net profit margin (PM).*

3.2. Industry Demand Volatility as a Moderator

If the demand-uncertainty mechanism is operative, one would expect the PSR–profitability association to differ across industries with varying levels of demand volatility. In industries with structurally high demand volatility, firms face greater pressure to maintain excess capacity and to commit to costly fixed investments without assurance of future order volumes (Banker et al, 2014; Bloom et al, 2007). The stabilizing role of predictable public-sector revenue may be most valuable in these settings, generating a positive interaction.

Alternatively, if public-sector suppliers in volatile industries are disproportionately exposed to procurement-cycle shocks or face particularly aggressive competitive tendering, the interaction could be negative. In industries with low demand volatility, public and corporate demand conditions are more similar, and any differential effect of public-sector exposure is predicted to be smaller in absolute terms. We do not predict the direction of the interaction. We further caveat that this moderating pattern is a cross-sectional association rather than a causal

interaction. We cannot rule out that industry-level differences in the PSR–profitability relationship reflect unobserved industry characteristics correlated with both demand volatility and procurement participation.

Hypothesis 2 (H2): *The association between public-sector revenue share and firm profitability varies with industry-level demand volatility.*

3.3. Buyer Type and Institutional Constraints

Not all public entities are institutionally equivalent. Swedish public procurement is distributed across buyer categories that differ in scale, fiscal constraints, and procurement capacity. Central government agencies typically operate under multi-year framework agreements for defined product or service categories, corresponding to a higher degree of revenue predictability for suppliers.[3] Regional authorities manage large recurring budgets dominated by health care, a statutory responsibility under the Health and Medical Services Act (Riksdagen, 2017). Their tax bases are larger and more diversified than those of individual municipalities, which makes regional procurement spending relatively stable from year to year. Municipalities have smaller and more geographically concentrated tax bases and more discretion over spending composition, since their statutory responsibilities span housing, education, social services, and infrastructure rather than being concentrated in a single mandatory function. They are also more directly exposed to local political-cycle variation in spending priorities.

To the extent that these institutional differences correspond to differential demand characteristics for suppliers, the association between public-sector exposure and profitability may vary across buyer types. Central government and regional entities may provide more stable revenue streams than municipalities, but they may also impose more demanding compliance requirements or more competitive tendering, making the relative magnitudes an empirical question.

In the empirical implementation, we decompose PSR into six buyer-type components (municipalities, regions, central government agencies, publicly owned companies, universities, and university colleges) to test whether coefficients differ across categories. The distinction between central government, regional, and municipal exposure provides the primary test of H3, while the remaining categories are included for completeness. The estimated differences across buyer types should be interpreted as descriptive associations rather than as causal effects of buyer type per se.

Hypothesis 3 (H3): *The association between public-sector revenue share and firm profitability differs across public buyer types, such that central government, regional, and municipal exposure exhibit distinct coefficients reflecting the different institutional characteristics of each buyer category.*

[3] However, LOU contracts are typically limited to four years unless special circumstances apply (SFS 2016:1145, Ch. 7 §2), which bounds the revenue predictability that even the most stable buyer types can provide. In practice, however, many supplier relationships persist across successive procurement cycles.

3.4. Public-Buyer Concentration Within the Public Segment

Hypotheses 1 through 3 treat public-sector revenue share as a measure of how much a firm sells to the public sector. Yet two firms with the same PSR may differ in how that public revenue is distributed. One firm may sell to a single municipality, while another serves dozens of public entities. This within-public concentration, measured by the Herfindahl–Hirschman Index (HHI) of a firm's invoiced amounts across its individual public buyers relative to its total public-sector sales, introduces a dimension of supplier risk that PSR alone does not capture.

The theory offers competing predictions. On one hand, dependence on a small number of public buyers may expose suppliers to buyer-specific hold-up risk, even within the public sector. A firm that derives its entire public revenue from a single public entity faces the concentrated risk of losing that relationship through contract non-renewal, political reallocation of spending, or changes in procurement strategy. This parallels the bargaining-power channel documented in the broader customer-concentration literature, where dependence on fewer buyers is associated with compressed supplier margins (Lustgarten, 1975; Hui et al, 2019).

On the other hand, concentrated public-buyer relationships may correspond to operational efficiencies through deeper coordination, lower transaction costs, and reduced marketing effort, the same patterns that Patatoukas (2012) and Irvine et al. (2016) associate with concentrated customer bases more generally. Josephson et al. (2019) reinforce this view with both qualitative insights and empirical tests of 'government customer breadth', suggesting that the profitability benefits of government supply materialize primarily through long-term, high-volume relationships with a small number of public buyers (low breadth). In these focused relationships, economies of learning and scale dominate the upfront compliance costs. This would reinforce the demand-stabilization pattern central to H1.

Because these mechanisms point in opposing directions and no prior empirical evidence distinguishes between them in a public-buyer setting, we do not predict the direction of the association. We instead test whether the level of public-buyer concentration moderates the PSR–profitability relationship, treating H4 as exploratory.

> **Hypothesis 4 (H4)**: *Among firms with positive public-sector revenue share, the association between PSR and firm profitability varies with the concentration of public-buyer exposure, such that firms whose public revenues are concentrated among fewer buyers exhibit different profitability outcomes than firms whose public revenues are distributed across many buyers.*

4. Data and Methodology

4.1. Data Sources

The empirical analysis combines Swedish public procurement data with corporate financial data drawn from two primary sources. Invoice-level procurement data were extracted from the *kommun.jensnylander.com* database,[4] which aggregates spending records across Swedish public entities, including governmental agencies, municipalities, regions, and publicly owned companies (Nylander, 2026). Firm-level financial data were sourced from Retriever Business, a Swedish commercial database that compiles annual financial statements and company registry information for Nordic firms (Retriever, 2026).

4.2. Data Retrieval

4.2.1. Public Procurement Data

The public procurement dataset aggregates invoice-level procurement spending across Swedish public entities, including governmental agencies, municipalities, regions, universities, university colleges, and publicly owned companies (Nylander, 2026). As of the retrieval date, coverage includes 271 municipalities, 18 regions, 72 government companies, 21 universities and colleges, and 43 government agencies, with collection ongoing.[5] The dataset covers calendar years 2022, 2023, and 2024, though completeness varies across entities. Coverage is also defined per public entity by a start date and an end date published as metadata alongside the supplier-level totals. Among the 425 public entities present in the database at the retrieval date, 67 percent are covered for the full 2022–2023 calendar period, while 16 percent have a coverage window ending on or before October 2023. For example, Örebro municipality is covered through October 31, 2023, and Stockholm municipality through May 2023. This entity-level heterogeneity in the procurement dataset has consequences for PSR construction in 2023, which we address in Section 4.9.

The database is grounded in the Swedish principle of freedom of information (*offentlighetsprincipen*) in the Freedom of the Press Act (Riksdagen, 1949), which entitles any person to request and obtain official documents held by Swedish public agencies, municipalities, and regions (Riksdagen, 1949). The underlying material consists of supplier ledgers and invoice registers requested directly from public entities under this right, and the same principle underpins the external validation procedure described in Section 4.3. Each record contains the buyer's name and details, the supplier's name and organization number, account classification, and invoice amount. Because public entities operate on different financial systems, the records were ingested and standardized across heterogeneous input formats before further processing.

[4] Subsequently referred to as *the procurement dataset* or the *Nylander dataset*.
[5] This can be compared to the current 290 municipalities, 21 regions, 214 government agencies and 3,399 publicly owned organisations that are subject to the procurement laws, according to The National Agency for Public Procurement (2026b). However, approximately half of those organizations have no employees, indicating limited amounts of actual procurement magnitudes.

The database is published under a Creative Commons CC BY-NC-SA-EU license, permitting non-commercial use of the published data. However, retrieving the data programmatically presented a technical challenge because the database's API was non-functional at the time of collection and pre-aggregated supplier-level totals were unavailable for a subset of public entities. To address this, we developed a JavaScript web crawler that interfaces directly with the database's backend via two distinct query methods. For entities where supplier-level aggregation was already available, the crawler queried the suppliers (*leverantörer*) endpoint, which returned pre-aggregated supplier-level data containing transaction volumes and invoice counts by calendar year. For entities lacking pre-aggregated data, the crawler queried the invoices (*fakturor*) endpoint and downloaded raw invoice records in batches of up to 50,000, which were then aggregated by mapping each record to a composite key of vendor name and organization number, with invoice dates parsed to the corresponding calendar year. To ensure data integrity and prevent server-side rate limiting, the crawler processed entities sequentially with timed delays between requests, producing one standardized CSV file per public entity.

The raw output is a collection of entity-level CSV files, each containing supplier-level records for one public entity. These files were combined into a single supplier-buyer-year panel, where each observation represents the total invoiced amount between a given supplier and a given public entity in a given calendar year. The resulting dataset is keyed by supplier organization number, buyer identifier, and calendar year, with associated variables for invoice amounts, invoice counts, and buyer-type classifications (municipality, region, government agency, publicly owned company, university, and university college). At the merge stage (Section 4.6), this supplier-buyer-year panel is further aggregated for the main analysis to the supplier-year level by summing across all public buyers, yielding a single public-sales total per firm per year for use in PSR construction.

4.2.2. Financial Statement Data

Financial statement data was obtained from Retriever Business, a Swedish commercial data provider that aggregates company-level financial and registry information sourced from the Swedish Companies Registration Office (*Bolagsverket*) and Statistics Sweden (SCB). The database covers the full universe of Swedish limited liability companies (*aktiebolag,* abbreviated as *AB*) and provides annual income statement and balance sheet data as reported in each firm's filed annual report.

The data was downloaded from Retriever's web interface in batches, producing raw files in wide format. Firm-level attributes such as organization number, legal form, and industry codes appear as single columns, while year-varying financial variables, e.g. net sales, total assets, number of employees, are stored as separate columns with the year appended as a suffix. To construct a firm-year panel suitable for regression analysis, the batch files were merged and reshaped from wide to long format, where each row represents one firm in one fiscal year. String-coded financial variables were then parsed to numeric values.

To ensure complete population coverage, the download included all Swedish limited liability companies regardless of activity status, encompassing both currently active and inactive firms.

Restricting to active firms would introduce survivorship bias by systematically excluding companies that ceased operations during the sample window. After calendarization (Section 4.4) and group-accounts consolidation (Section 4.5), the resulting panel comprises 4,113,157 firm-years across calendar years 2022–2024, with historical data extending back to 2015 for the construction of rolling variables such as demand uncertainty, which uses up to five years of lagged sales growth.

Firms were subsequently matched to procurement records using the corporate organization number and calendar year as the common merge identifiers.

4.3. Validation of the Public Procurement Invoice Data

The procurement dataset is validated through two independent exercises that probe different aspects of the database. The first tests whether supplier-level aggregates align with the values that procuring entities themselves report. The second tests, at the invoice level, whether the Nylander data record of an entire municipality's outgoing invoice stream matches that municipality's own published records.

The first exercise relies on the Swedish principle of freedom of information (*offentlighetsprincipen*). We selected a random sample of supplier-entity pairs and submitted formal information requests to the procuring entity to confirm the total amount invoiced by each supplier during calendar years 2022, 2023, and 2024.[6] The exercise covered 36 supplier-entity pairs, yielding 108 year-level confirmations. The overall rate of exact or near-exact correspondence was high, with the majority of observations showing no deviation. Where deviations were observed, they fell into two categories. The first consists of cases showing a consistent proportional undercount, a pattern consistent with a systematic difference in VAT treatment between the invoice register and the confirmation source (see Section 4.9). The second comprises larger, isolated discrepancies with no clear pattern, most plausibly attributable to collection-stage omission errors. We interpret the proportional undercount as a non-random but level-only error, as it shifts the recorded amount for affected suppliers by a roughly constant percentage, so the ordering of suppliers by public-sector dependence is preserved even though the levels are not. The second category, by contrast, has no detectable pattern and we treat it as random measurement noise.

The second exercise exploits the fact that Örebro is, to our knowledge, the only Swedish municipality that publishes a complete open-data register of incoming supplier invoices under Swedish open data legislation (Örebro kommun, 2025), allowing an entity-wide reconciliation rather than a sample-based one. Because the Nylander data coverage of Örebro extends only through October of 2023, the 2023 reconciliation is restricted to the January–October window, while the 2022 reconciliation uses the full year.[7]

[6] Calendar year 2024 is excluded from the baseline regression sample but retained in this validation exercise.

[7] Of the 53,194 Örebro 2023 invoices absent from Nylander, 99.4% have a payment date in November or December 2023, after the declared coverage end date for Örebro (October 31, 2023). The truncation is therefore consistent with the disclosed coverage window rather than with a coverage failure within the captured period.

For calendar year 2022, the Örebro open record contains 288,748 invoices totaling SEK 4,396 million. The corresponding Nylander data record contains 288,294 invoices totaling SEK 4,383 million. Of Nylander data's 288,294 invoices, every one is present in the Örebro open record. Of Örebro's 288,748 invoices, 288,294 (99.8 percent by count, 99.7 percent by amount) are present in the Nylander data, with 454 invoices worth SEK 13 million absent. Conditional on the invoice being present in both sources, the aggregate gap across all 288,294 matched invoices is SEK 767, a difference likely owing to decimal omission in one of the databases.

Because the regression sample is restricted to limited liability companies (*aktiebolag*), the relevant validation universe is the subset of Örebro suppliers identified by Swedish corporate registration number as *aktiebolag*.[8] Among 275,785 such invoice-supplier records in 2022, the Nylander data captures 275,718. Aggregating to the supplier-year level at which our public-sales-ratio variable is constructed, the Nylander data captures 3,499 of 3,511 Örebro *aktiebolag*-year observations in 2022. On matched supplier-years, the aggregate gap between the recorded amount in the Nylander data and Örebro's is SEK 112 across SEK 3.4 billion in matched volume. Of the 454 invoices absent from Nylander altogether, 387 involve non-*aktiebolag* suppliers who fall outside our analytical sample by construction. On matched invoices, the Nylander data and Örebro record the same supplier organization number in 99.9 percent of cases. The disagreements arise from differences in legal-entity attribution.

Restricting the Örebro data to the period within the declared 2023 coverage window for the Nylander data (January through October), only 71 *aktiebolag* invoices appear in the Örebro record but not in the other, comparable to the 67 such invoices observed across the whole of 2022. Within this restricted window, the Nylander data capture rate of Örebro *aktiebolag* invoices is approximately 100 percent, indistinguishable from the 2022 baseline.

Taken together, the two validation exercises support the use of the Nylander procurement data in the analyses that follow, with the caveat that the Örebro reconciliation establishes data fidelity for one large municipality and may not generalize to other entity types whose data feeds to the procurement dataset differ.

4.4. Calendarization of Financial Statement Data

Swedish limited liability companies may choose a fiscal year ending in any calendar month. Because invoice data in the procurement dataset are recorded in calendar years (January–December), a temporal alignment procedure is needed to match financial statement variables with procurement variables.

We implement a Compustat-style calendarization rule where firms with fiscal year-end months in July through December are assigned to the corresponding calendar year, while firms with fiscal year-end months in January through June are assigned to the previous calendar year. For example, a firm whose fiscal year ends in March 2023 is assigned to calendar year 2022. The

[8] Swedish corporate registration numbers (*organisationsnummer*) follow a fixed structure in which the first digit broadly indicates legal form (Bolagsverket, 2026). Numbers beginning with '5' identify *aktiebolag* along with a small number of related corporate forms (branches of foreign firms, banks, insurance companies, and European companies). Note that some foreign registration numbers may also begin with '5'.

resulting calendar-year index is used for all lags, growth rates, rolling standard deviations, and merges throughout the analysis. This procedure ensures that the financial reporting period more closely overlaps with the calendar year from which the invoice data is drawn. For the 68 percent of firms in the baseline sample that report on a December fiscal year-end, no realignment is needed.

For firms with non-December year-ends, a residual timing mismatch remains between the numerator and denominator of PSR. Take a firm with a June 2023 fiscal year-end. Its income statement covers July 2022 through June 2023, while its invoice coverage in the procurement dataset spans January through December 2022. If this firm issued an unusually large volume of public invoices in the first half of 2022 but reported relatively low total revenues over the July 2022–June 2023 fiscal year, the resulting PSR could mechanically exceed one. The reverse, a firm with high fiscal-year revenues but low public invoicing in the overlapping calendar year, could produce an understated PSR. These boundary cases are addressed in the sample filters described in Section 4.6, but the underlying mismatch introduces measurement noise in PSR for the minority of firms with non-calendar fiscal years. This mismatch does not create a systematic directional bias, since the calendar-year and fiscal-year windows always overlap by at least six months and the direction of any residual error is unrelated to the firm's true public-sector exposure. A similar mismatch arises in any study that combines fiscal-year financial data with calendar-year transaction data, and the Compustat calendarization convention is standard practice in the empirical finance literature.

Firms with shortened or extended fiscal years, i.e., firms with financial reporting periods shorter or longer than 12 months, are retained in the sample without adjustment. Because these transitional years cover fewer or more than twelve months, the resulting financial ratios may be distorted. The number of affected firm-years is small relative to the baseline sample, and we do not believe their inclusion materially affect the results.

4.5. Allocation of Public Sector Revenue

Some firms belong to corporate groups in which a parent files consolidated group accounts (*koncernredovisning*) that combine its own activity with that of its subsidiaries into a single financial statement. As we rely on these consolidated financial statements where available, a measurement mismatch in PSR arises for those cases in which the firm public-sector revenue in the procurement dataset is attributed to individual legal entities, whereas the financial denominator resides at the group level. To resolve this mismatch, we apply an alignment procedure to both datasets before merging them.

On the financial statement data (Retriever) side, Swedish parent companies that file consolidated group accounts already report group-level revenue, assets, and profits that include all subsidiaries. To avoid double-counting, wherever a parent files consolidated accounts in a given year, we drop the subsidiary's annual report for that year since its economic activity is already captured in the parent's consolidated figures. Subsidiaries whose parents do not file

consolidated accounts are retained as standalone reporting units, because their individual financial statements are the only source of data for that entity.[9]

On the procurement dataset side, we reassign each subsidiary's invoice data to the group parent's organization number and sum them. As a result, aggregate PSR across the full sample is unchanged by this procedure. The only effect is that public procurement revenue is attributed to the correct reporting entity, ensuring that the PSR numerator (aggregated sales to the public sector) and denominator (consolidated total revenue) refer to the same economic unit.

4.6. Sample Selection

The analytical sample is constructed by merging the procurement dataset with the Retriever financial database, using the corporate organization number and calendar year as the common identifiers. All merges use the calendarized year index to align Retriever fiscal-year financial data with the calendar-year invoice totals in the procurement dataset, as described in Section 4.4. Both datasets have been consolidated to the group level prior to merge, as described in Section 4.5.

The raw procurement dataset contains 1,828,098 supplier-buyer-year records that are then aggregated to the supplier-year level, resulting in 327,552 supplier-year observations. These are merged onto the Retriever panel using a left-preserving join. All 4,113,157 Retriever firm-years are retained, with procurement dataset variables populated for the 312,089 firm-years where a matching supplier-year record exists (7.6 percent of the panel). The 15,463 supplier-year records from the procurement dataset without a Retriever match correspond primarily to non-AB entities and foreign companies that fall outside the Retriever panel. Firm-years without a match in the procurement dataset receive PSR = 0.

We then apply nine sequential filters, documented in Table 1**.** The sample is restricted to calendar years 2022 and 2023 (F1), dropping 1,317,748 firm-years. Calendar year 2024 is excluded from the baseline because the procurement dataset has incomplete coverage for that year. As of the data retrieval date, a large share of public entities had not yet reported complete 2024 invoices. The share of firm-years with positive PSR drops from 5.7 percent in 2022 to 5.1 percent in 2023 to 3.0 percent in 2024, a decline too large to reflect real economic changes and consistent with systematic non-reporting by a subset of public entities. Including 2024 would attenuate measured PSR non-randomly, since the missing entities are not a random subset of public buyers. We include 2024 in a separate robustness check (Table 10).

Two balance-sheet conditions follow. We require strictly positive total assets (F2), which drops 1,513,116 firm-years. Firms with missing or zero total assets have not filed usable financial statements, or Retriever has not read the reports correctly, and most financial ratios are

[9] Two types of edge cases remain after this procedure. (i) Firms whose ultimate parent is foreign and therefore absent in the Retriever data are retained as standalone observations. The subsidiary's own sales and procurement data remain matched at the entity level, but the denominator may include intra-group transactions. (ii) Firms that file consolidated accounts but are not classified as group parents in Retriever are classified as standalone by the procedure. Their financial-statement denominator therefore reflects group-level activity, while only invoices issued under their own organization number enter the numerator, which understates PSR.

undefined without a positive asset base. These are predominantly inactive shell companies or entities awaiting dissolution. A parallel requirement of strictly positive book equity (F3) drops a further 51,691 firm-years, since negative-equity firms are technically insolvent and financial ratios that use equity as a denominator reverse sign mechanically, creating distortions. This filter is standard in the corporate finance literature (e.g., Cohen and Li, 2020).

Table 1. Sample Selection

	Full Sample		PSR-Enriched Sample	
Step / Filter	N dropped	N after	N dropped	N after
S1: Retriever firm-year panel (2022–2024)	-	4,113,157	n.m.	n.m.
S2: Nylander supplier–buyer–year panel (2022–2024)	-	1,828,098	n.m.	n.m.
S3: Aggregate Nylander to supplier-year	1,500,546	327,552	n.m.	n.m.
S4: Merge Nylander onto Retriever	-	4,113,157	n.m.	312,089
F1: Restrict to calendar years 2022 and 2023	1,317,748	2,795,409	95,998	216,091
F2: Positive total assets	1,513,116	1,282,293	15,475	200,616
F3: Positive book equity	51,691	1,230,602	6,042	194,574
F4: Positive net sales	367,648	862,954	3,193	191,381
F5: Net sales at least SEK 250,000	164,909	698,045	12,469	178,912
F6: Non-missing demand uncertainty	175,807	522,238	25,250	153,662
F7: PSR bounded between 0 and 1	2,249	519,989	2,249	151,413
F8: Legal form: limited liability company (AB)	2	519,987	2	151,411
F9: Core regression variables non-missing	7,110	512,877	1,617	149,794

This table reports the sequential application of sample filters to the merged Retriever–procurement data panel. The starting panel comprises all Swedish *aktiebolag* with financial statements filed for fiscal years mapping to calendar years 2022–2024 (after Compustat-style calendarization and group-accounts consolidation). F1 restricts to calendar years 2022–2023 due to incomplete procurement data coverage in 2024. F5 removes micro-firms below SEK 250,000 in net sales. F6 requires non-missing demand uncertainty (DEM_UNC), which needs at least three years of valid sales growth within a five-year rolling window. F9 requires all core regression variables to be non-missing. All variables are defined in the Appendix.

The next two filters apply income-statement conditions. We require strictly positive net sales, dropping 367,648 firm-years for which firms are economically inactive or PSR is undefined (F4). A more stringent threshold of SEK 250,000 in net sales (F5) removes an additional 164,909 firm-years, eliminating micro-scale entities for which financial ratios are dominated by measurement noise rather than operating performance. The threshold is conservative relative to the literature and higher thresholds are examined in robustness analyses.

We also require a non-missing firm-level demand uncertainty measure (F6), constructed as the rolling standard deviation of log sales growth over a five-year window with at least three valid observations. This filter drops 175,807 firm-years, typically young firms with fewer than three years of sales history or firms with intermittent reporting and introduces a survivorship-like selection toward established firms.

The next filter excludes firm-years with PSR outside the unit interval [0, 1] (F7), dropping 2,249 observations. Values below zero reflect data-quality issues such as credit notes. Values above one arise from two mechanisms operating in the same direction. The first is a VAT mismatch. Invoice amounts from the procurement dataset include VAT while Retriever net

sales are recorded exclusive of VAT, inflating the numerator by up to 25 percent for VAT-liable sales at the standard rate. The second is a calendarization wedge. Invoices from the procurement dataset are recorded on a strict calendar-year basis while Retriever net sales follow the firm's fiscal year, mapped to a calendar year through the procedure in Section 4.4. For firms whose fiscal year does not coincide with the calendar year, the numerator and denominator cover overlapping but non-identical twelve-month windows, which can produce PSR values above one whenever public-sector invoicing concentrates in the non-overlapping months. Neither mechanism reflects genuine variation in public-sector dependence, justifying the exclusion.

Two further filters complete the sample. F8 confirms that all remaining observations are classified as *aktiebolag*, dropping two firm-years where the legal form recorded in the downloaded file conflicted with the Retriever online classification at the time of download. F9 requires non-missing values for all variables used in the main regression specifications (ROA, OM, ATO, ln(TA), AGE, SALE_GR, and FLEV), dropping 7,110 firm-years. The primary source of missingness at this stage is ROA and ATO, both of which require lagged total assets that are unavailable for a firm's first year in the panel. Filter F9 ensures that the baseline sample count matches the main regression observation count exactly.

The final baseline sample contains 512,877 firm-year observations covering 284,437 unique firms across calendar years 2022 and 2023, of which 149,794 firm-years have PSR coverage.

4.7. Variable Construction

This section describes the construction of the variables used in the analysis. We begin with the main independent variable, public-sector revenue share (PSR), followed by the dependent variables, control variables, and the additional variables. A complete list of all variables, their definitions and roles in the analysis is provided in Table A10 in the Appendix.

4.7.1. Public-Sector Revenue Share (PSR)

The main independent variable is the public-sector revenue share (PSR), defined as the ratio of a firm's total invoiced revenues from public-sector buyers in a given calendar year to its total net sales reported:

$$PSR_{it} = \frac{PublicSales_{it}}{Sales_{it}}, \text{ if } Sales_{it} > 0 \quad (1)$$

where $PublicSales_{it}$ is the sum of all recorded invoice amounts in the procurement dataset among public-sector buyers from firm i in calendar year t, and $Sales_{it}$ is the firm's total net sales (*nettoomsättning*) from its filed annual report, assigned to calendar year t via the calendarization procedure described in Section 4.4. Net sales is an appropriate denominator because it captures revenue from core operating activities, is comparable in nature to the invoice amounts in the procurement data numerator, and is reported under the K2, K3, and IFRS frameworks, making it available for the full sample. For firm-years without coverage in

the procurement dataset, that is, PSR is set to zero, reflecting the absence of observed public-sector revenue.

Our PSR measure differs from prior research in several respects. Studies using U.S. data identify government customers only when they exceed the 10 percent disclosure threshold required by SEC Regulation S-K for listed firms, or the corresponding major-customer disclosure requirement under IFRS (U.S. Securities and Exchange Commission, 2023; IASB, 2006). Our measure is continuous, threshold-free, and covers firms of all sizes, including both private and listed companies.

The validation exercises in Section 4.3 supports the use of the underlying procurement data which PSR is constructed from for this analysis. As discussed in Section 4.9, two known measurement issues affect PSR. The invoice amounts are typically recorded gross of VAT while the Retriever denominator is net of VAT, and the VAT treatment varies across entities. Furthermore, entity-level coverage gaps in 2023 cause the numerator to reflect partial-year public sales for some firms.

4.7.2. Dependent Variables

We examine profitability through four outcome variables. The primary dependent variable is return on assets (ROA), defined as EBIT divided by beginning-of-period total assets (TA):

$$ROA_{it} = \frac{EBIT_{it}}{TotalAssets_{i,t-1}} \tag{2}$$

We decompose ROA through the DuPont identity (Johansson and Runsten, 2014) into operating margin (OM), defined as EBIT divided by net sales, and asset turnover (ATO), defined as net sales divided by beginning-of-period total assets:

$$OM_{it} = \frac{EBIT_{it}}{Sales_{it}} \tag{3}$$

$$ATO_{it} = \frac{Sales_{it}}{TotalAssets_{i,t-1}} \tag{4}$$

Since ROA is equal to OM multiplied with ATO, this decomposition allows us to examine whether any association between PSR and profitability operates through margins or through asset efficiency. We additionally examine profit margin (PM), defined as net income divided by net sales, and return on equity (ROE):

$$PM_{it} = \frac{NetIncome_{it}}{Sales_{it}} \tag{5}$$

$$ROE_{it} = \frac{NetIncome_{it}}{Equity_{i,t-1}} \tag{6}$$

Our ROA and OM definitions use EBIT (*rörelseresultat*)[10], which is a standard and directly reported line item under both K2 and K3. Using EBIT also makes ROA and OM independent

[10] Earnings before interest and tax, commonly used interchangeably with operating income.

of firms' financing and tax positions, isolating the operating-level profitability channel most directly related to customer-base composition.

Further decomposition would be possible if gross margin and selling, general, and administrative expenses (SGA) line items were available. However, most Swedish K2 and K3 firms report income statements in the classification-by-nature format, which does not allocate costs to functional categories such as cost of goods sold or selling expenses, as discussed in Section 4.9.

4.7.3. Control Variables

The baseline specification includes four firm-level controls. Firm size is measured as the natural logarithm of total assets, $ln(TA)$. Firm age (AGE) is the number of years since the firm's formation date, with the registration date used as a fallback when the formation date is unavailable. Financial leverage ($FLEV$) is defined as beginning-of-period total assets divided by beginning-of-period book equity, measuring the equity multiplier:

$$FLEV_{it} = \frac{TotalAssets_{i,t-1}}{Equity_{i,t-1}} \tag{7}$$

Sales growth is defined as the log change in net sales:

$$SALE_GR_{it} = ln(Sales_{it}) - ln\left(Sales_{i,t-1}\right) \tag{8}$$

Sales growth is computed only for firm-years in which both current and lagged sales exceed SEK 10,000 and set to missing otherwise. This floor prevents extreme log growth rates from near-zero bases. The threshold is binding primarily for historical sales growth observations that enter the rolling DEM_UNC window (Section 4.7.4), since the baseline sample restriction to net sales of at least SEK 250,000 (F5) renders it redundant for contemporaneous sales in the estimation sample.

These controls follow the customer-concentration literature (e.g., Patatoukas, 2012; Cohen and Li, 2020). Size controls for economies of scale and reporting quality. Age captures life-cycle effects. Sales growth proxies for demand conditions and growth opportunities. Leverage controls for capital structure effects on profitability. Several controls used by Cohen and Li (2020), including market value of equity, conglomerate status, and analyst coverage, are unavailable for Swedish private firms. To the extent that these omitted variables are correlated with both PSR and profitability, our estimates may be subject to omitted variable bias, which we partially mitigate through the logarithm of total assets as a size proxy. In a robustness specification (Table A7), we replace the natural logarithm of total assets with the natural logarithm of net sales, as an alternative size proxy.

4.7.4. Demand Uncertainty

We construct firm-level demand uncertainty (DEM_UNC) as the rolling standard deviation of log sales growth over a five-year backward-looking window, requiring at least three valid observations:

$$DEM_UNC_{it} = SD\left(SALE_GR_{it}, SALE_GR_{i,t-1}, \ldots, SALE_GR_{i,t-4}\right) \quad (9)$$

This measure follows Cohen and Li (2020), who use the standard deviation of sales growth to capture demand volatility, with one modification. Cohen and Li use a symmetric five-year window centered on year *t*, while we use a strictly backward-looking window (years $t - 4$ through *t*). The backward-looking specification avoids conditioning on future information and is more appropriate for our two-year estimation sample, where a centered window would require unavailable post-sample data. For observations in calendar years 2022 and 2023, the five-year window starts at 2018 and 2019, respectively, so DEM_UNC is calculated from historical sales growth that predates the observation-year PSR. This property is desirable for the mechanism test in Section 5.5.1, where DEM_UNC is the dependent variable. The test asks whether firms with higher current PSR exhibit historically lower demand volatility. Three-year and seven-year alternative windows are also computed and discussed in Section 5.5.1.

For the H2 interaction analysis, we construct an industry-level demand uncertainty measure, DEM_UNC_IND. For each two-digit SNI industry *j* and year *t*, we first compute the cross-sectional mean of log sales across all firms in the industry-year cell, $\overline{\ln(Sales_{jt})} = \frac{1}{N_{jt}} \sum_{i \in j} ln\,(Sales_{it})$. We then take the first difference of the industry-mean log sales to obtain industry-level log sales growth, $g_{jt} = \overline{\ln(Sales_{jt})} - \overline{\ln(Sales_{j,t-1})}$. The industry-level demand uncertainty measure is the rolling standard deviation of this growth series over the same backward-looking window:

$$DEM_UNC_IND_{jt} = SD\left(g_{jt}, g_{j,t-1}, \ldots, g_{j,t-4}\right) \quad (10)$$

The two-digit SNI level (84 divisions) is chosen to balance granularity against statistical feasibility. The higher-level SNI sections provide only 21 categories, which is too coarse to capture meaningful cross-industry variation in demand conditions, while the full SNI title classification produces over 400 categories, generating excessive singletons in a two-year panel. The 84 divisions provide sufficient within-group variation in PSR while still absorbing persistent sectoral differences through the industry fixed effects. This measure captures structural differences in demand volatility across sectors and is used to test whether the association between PSR and profitability is amplified in industries with inherently more volatile demand.

4.7.5. Capital Investment

Because Swedish private firms filing under K2 and K3 are generally not required to report cash flow statements, direct capital expenditure (*Capex*) figures are unavailable. We therefore construct a balance-sheet-based capex proxy from the change in tangible fixed assets (TFA) and intangible fixed assets (IFA) plus depreciation and amortization (D&A):

$$Capex_{it} = \left(TFA_{it} - TFA_{i,t-1}\right) + \left(IFA_{it} - IFA_{i,t-1}\right) + D\&A_{it} \quad (11)$$

The baseline investment rate scales this capex proxy by beginning-of-period tangible and intangible fixed assets:

$$INVEST_{it} = \frac{Capex_{it}}{TFA_{i,t-1} + IFA_{i,t-1}} \quad (12)$$

This definition is available for 67 percent of firm-years in the baseline sample. Coverage is reduced because the measure is undefined for firms with zero beginning-of-period tangible and intangible fixed assets. The fixed-asset denominator isolates capital intensity relative to the stock of productive assets against which the investment is deployed, but differs from Cohen and Li (2020), who define their investment rate variable as $Capex + R\&D + acquisitions - sales\ of\ PP\&E$ scaled by lagged total assets. Our balance-sheet-derived proxy cannot separate acquisitive from organic investment, nor does it isolate R&D. As alternatives for robustness, we report specifications scaling the numerator by beginning-of-period total assets[11], which is closer to the Cohen and Li (2020) definition, and a variable excluding intangible assets from both numerator and denominator[12].

4.7.6. Additional Variables

Earnings volatility is defined as the rolling standard deviation of ROA over a five-year backward-looking window (minimum three observations), serving as an outcome variable for testing whether public-sector exposure reduces profitability risk:

$$ROA_VOL_{it} = SD\left(ROA_{it}, ROA_{i,t-1}, \dots, ROA_{i,t-4}\right) \quad (13)$$

Additional variables include the cash-to-assets ratio (Equation (14)), the liabilities-to-assets ratio (Equation (15)), and the natural logarithm of net sales, used in robustness specifications:

$$CASH/TA_{it} = \frac{Cash_{it}}{TotalAssets_{it}} \quad (14)$$

$$LIAB/TA_{it} = 1 - \frac{Equity_{it}}{TotalAssets_{it}} \quad (15)$$

4.7.7. Leverage Decomposition Variables

To trace how operating performance flows through the capital structure to equity returns, we construct three additional variables following Johansson and Runsten (2014). The cost of liabilities (COL) is defined as:

$$COL_{it} = \frac{InterestExpenses_{it} - InterestIncome_{it}}{(Liabilities_{i,t-1})} \quad (16)$$

where Interest expense (*externa räntekostnader*) and Interest income (*externa ränteintäkter*) are separately reported below EBIT under both K2 and K3, and liabilities denotes opening total liabilities (*skulder*), computed as the sum of short-term and long-term liabilities. COL is undefined for firm-years with zero or missing opening total liabilities (1,728 firm-years, 0.3 percent of the baseline sample).

[11] Variable *INVEST_TA*.
[12] Variable *INVEST_PPE*.

The liability-to-equity ratio (L/E) is beginning-of-period total liabilities divided by beginning-of-period book equity, maintaining consistency with the ROA denominator convention:

$$L/E_{it} = \frac{Liabilities_{i,t-1}}{Equity_{i,t-1}} \tag{17}$$

The effective tax rate (EFF_TAX) is tax expense (*skatt på årets resultat*) divided by EBT (income after net financial items, *resultat efter finansnetto*), set to missing for firm-years with non-positive pre-tax income:

$$EFF_TAX_{it} = \frac{TaxExpense_{it}}{EBT_{it}} \tag{18}$$

The leverage spread captures the residual rent earned on debt-financed assets:

$$LEV_SPREAD_{it} = ROA_{it} - COL_{it} \tag{19}$$

Pre-tax ROE is the pre-tax analogue of ROE used in the leverage decomposition:

$$PRETAX_ROE_{it} = \frac{EBT_{it}}{Equity_{i,t-1}} \tag{20}$$

We then construct the leverage-identity ROE:

$$ROE_LEVERAGE_{it} = [ROA_{it} + (ROA_{it} - COL_{it}) \times L/E_{it}] \times (1 - EFF_TAX_{it}) \tag{21}$$

COL, L/E, and EFF_TAX enter as dependent variables in separate specifications (Section 4.8.8), not as controls in the ROA or OM regressions. Conditioning on them would introduce post-treatment bias, since they are themselves outcomes of decisions PSR may influence.

4.7.8. Scope of Replicable Tests

The data setting of this study differs from that of Cohen and Li (2020) in ways that constrain the set of mechanism tests we can perform. The most consequential difference concerns the income statement format. Our sample consists predominantly of Swedish private firms reporting under the K2 and K3 frameworks of Swedish GAAP (Bokföringsnämnden, 2012, 2016). Under K2, only the classification-by-nature format (*kostnadsslagsindelad resultaträkning*) is available, while under K3, firms may choose between classification-by-nature and classification-by-function. Of the firms in our sample, the vast majority use the classification-by-nature format, which aggregates costs by input type (raw materials, personnel, depreciation, and other external expenses) rather than by function. In this format, no separate cost-of-goods-sold line exists and selling and administrative expenses are not reported as distinct categories. As a result, gross margin and SGA-to-sales ratio, two dependent variables central to the cost-structure channel tests in Cohen and Li (2020) and Patatoukas (2012), are unavailable for most firms in our sample.

Several market-based variables used by Cohen and Li, including market value of equity, Tobin's Q, analyst coverage, and stock return measures, are unavailable for unlisted firms. Most K2 firms are also not required to prepare cash flow statements, which constrains the investment variable as discussed in Section 4.7.5.

These limitations restrict the scope of mechanism tests but do not affect the core profitability analysis. ROA, OM, ROE, PM, and ATO are computed from line items, EBIT, net sales, net income, total assets, and equity, that are identically defined regardless of income statement format and consistently reported across both the K2 and K3 frameworks. The constraints on variable construction are a direct consequence of extending the analysis beyond the U.S. listed-firm environment and are discussed further in Section 4.9.

4.8. Empirical Specification

4.8.1. Baseline Specification (H1)

Our first hypothesis predicts an association between PSR and profitability. The baseline specification tests this by estimating:

$$Performance_{it} = \beta_1 PSR_{it} + \beta_2 \ln(TA_{it}) + \beta_3 AGE_{it} + \beta_4 \Delta \ln(Sales_{it}) + \beta_5 FLEV_{it} + \alpha_j + \gamma_t + \varepsilon_{it} \quad (22)$$

where $Performance_{it}$ is a profitability measure for firm $i$in calendar year t, α_j denotes industry fixed effects at the two-digit SNI division level (84 categories), and γ_t denotes year fixed effects. Standard errors are clustered at the firm level to account for within-firm serial correlation across the two sample years.

The coefficient we are interested in is β_1, which captures the conditional association between public-sector revenue share and firm profitability, controlling for size, age, growth, leverage, and absorbing time-invariant industry heterogeneity and common time shocks. Under H1, we test whether β_1 differs from zero for ROA, OM, ATO, and PM. We also estimate the specification with ATO as the dependent variable to assess the asset-efficiency channel through the DuPont decomposition. Of the 512,877 firm-years in the baseline sample, 1,669 have missing two-digit SNI codes and are absorbed by the fixed effects estimator, yielding an effective estimation sample of 511,208 observations.

We estimate the specification in four hierarchical specifications, reported in columns (1) to (4) of Table 4, progressing from PSR only (1), PSR with firm-level controls (2), adding year fixed effects (3), and adding industry fixed effects (4). This build-up shows how β_1 evolves as composition controls and FE absorb cross-sectional variation.

4.8.2. Industry Demand Uncertainty Interaction (H2)

To test whether the profitability association is stronger in volatile industries, we augment the baseline with an interaction term:

$$Performance_{it} = \beta_1 PSR_{it} + \beta_2 DEM_UNC_IND_{jt} + \beta_3 \left(PSR_{it} \times DEM_UNC_IND_{jt}\right) + X'_{it}\delta + \alpha_j + \gamma_t + \varepsilon_{it} \quad (23)$$

where DEM_UNC_IND_{jt} is the industry-level demand uncertainty for two-digit SNI industry j in year t, and X_{it} is the vector of firm-level controls. Under H2, we test whether β_3 differs from

zero, that is, whether industry-level demand volatility moderates the PSR–profitability association.

It is feasible to include both industry fixed effects and DEM_UNC_IND in the same specification, as the industry fixed effects absorb time-invariant industry means while DEM_UNC_IND varies within industry over time as a rolling standard deviation updated each year. However, with only two years in the estimation sample, the within-industry-over-time variation in DEM_UNC_IND is limited, so the interaction coefficient should be interpreted with caution.

4.8.3. Demand Uncertainty Mechanism

If the demand-uncertainty channel is effective, higher PSR should be associated with lower firm-level demand uncertainty. We test this by regressing DEM_UNC on PSR:

$$DEM_UNC_{it} = \beta_1 PSR_{it} + X'_{it}\delta + \alpha_j + \gamma_t + \varepsilon_{it} \quad (24)$$

A negative and significant β_1 would provide evidence consistent with public-sector revenue reducing the volatility of sales growth. We estimate this specification using the five-year baseline window and report the three-year and seven-year alternatives as robustness checks.

4.8.4. Investment Efficiency Mechanism

We test whether public-sector exposure improves the responsiveness of investment to growth opportunities:

$$INVEST_{i,t+1} = \beta_1 PSR_{it} + \beta_2 \Delta \ln(Sales_{it}) + \beta_3 (PSR_{it} \times \Delta \ln(Sales_{it})) + X'_{it}\delta + \alpha_j + \gamma_t + \varepsilon_{it} \quad (25)$$

The interaction coefficient β_3 captures whether firms with higher PSR adjust investment more responsively to current sales growth. Under the demand-uncertainty channel, one would expect $\beta_3 > 0$: reduced demand volatility from public buyers should make firms more willing to invest in response to positive demand signals, consistent with real options theory (Bloom et al, 2007; Guiso and Parigi, 1999). However, alternative mechanisms could produce a negative interaction if, for example, public-sector suppliers face procurement-specific rigidities that constrain investment timing.

The forward investment specification regresses $INVEST_{t+1}$ on PSR_t. For 2023 observations, this uses $INVEST_{2024}$ calculated using Retriever data as the outcome. Since Retriever's 2024 financial data is unaffected by the coverage limitations of the procurement dataset, the forward specification can be estimated on the full 2022–2023 baseline panel. We also estimate the specification using contemporaneous investment rate ($INVEST_t$). The contemporaneous specification does not test the same causal timing and is more susceptible to reverse causality, since firms that invest more may also win more public contracts in the same period.

4.8.5. Buyer Type Heterogeneity (H3)

To test whether the PSR–profitability association varies across public buyer categories, we decompose PSR into buyer-type-specific shares and estimate:

$$Performance_{it} = \sum_{k}(\beta_k PSR_{it}^{k}) + X_{it}'\delta + \alpha_j + \gamma_t + \varepsilon_{it} \quad (26)$$

where PSR_{it}^{k} denotes the share of firm i's total sales attributable to buyer type k in year t. The buyer types are municipalities (*kommun*), regions (*region*), central government agencies (*statlig myndighet*), universities (*universitet*), university colleges (*högskola*), and publicly owned companies (*kommunalt bolag*)[13]. The six components sum to PSR by construction. Under H3, we test whether the coefficients on the buyer-type components differ from each other, with Wald tests of pairwise coefficient equality providing the primary statistical test.

4.8.6. Within-Public Concentration (H4)

To test whether the concentration of a firm's public buyer base moderates the PSR–profitability association, we augment the baseline with an interaction between PSR and within-public buyer concentration:

$$Performance_{it} = \beta_1 PSR_{it} + \beta_2 PSR_HHI_{it} + \beta_3(PSR_{it} \times PSR_HHI_{it}) + X_{it}'\delta + \alpha_j + \gamma_t + \varepsilon_{it} \quad (27)$$

where PSR_HHI_{it} is the Herfindahl–Hirschman Index (HHI) of firm *i*'s invoiced amounts across its individual public buyers, computed within the firm's public-sector portfolio:

$$PSR_HHI_{it} = \sum_{b \in B_{it}} \left(\frac{PublicSales_{ibt}}{PublicSales_{it}}\right)^2 \quad (28)$$

where B_{it} is the set of public buyers from which firm *i* receives positive invoices in year *t*, and $PublicSales_{ibt}$ is the amount invoiced from firm *i* to buyer *b* in year *t*. *PSR_HHI* ranges from close to zero, when public revenue is spread evenly across many buyers, to one, when a single buyer accounts for the firm's entire public-sector revenue. We also report *PSR_LG*, the largest single buyer's share of the firm's public-sector sales:

$$PSR_LG_{it} = \max_{b \in B_{it}} \left(\frac{PublicSales_{ibt}}{PublicSales_{it}}\right) \quad (29)$$

A combined level-and-concentration measure, $CC_PUB_{it} = PSR_{it}^2 \times PSR_HHI_{it}$, scales the within-public concentration by overall public-sector exposure and is reported in the descriptive statistics for completeness.

The HHI is constructed as the sum of squared buyer shares and ranges from close to zero, when a firm's public revenue is spread evenly across many buyers, to one, when a single buyer accounts for the firm's entire public-sector revenue. Higher values therefore indicate greater

[13] Although *kommunalt bolag* literally translates to 'municipality-owned companies', the category in the procurement dataset also includes companies owned by other public-sector entities (e.g., regional or central government).

concentration of the public-buyer base. This specification is estimated on the subsample of firm-years with PSR > 0, since PSR_HHI is undefined for firms without public-sector revenue. H4 does not predict the sign of β_3.

We acknowledge that the $PSR \times PSR_HHI$ interaction could reflect both moderation and selection. Firms that concentrate their public revenue among few buyers (high HHI) likely differ from firms that diversify (low HHI) on unobservable business-model dimensions such as specialization, geographic constraint, management capacity, etc. Our specification cannot separate a true moderating effect of concentration from selection on these unobservables. We therefore interpret a significant interaction as evidence that the PSR–profitability relationship varies with concentration, without claiming that concentration causes the differential.

4.8.7. Fixed Effects Structure and Clustering

The choice of industry fixed effects at the two-digit SNI division level (84 categories) balances sectoral granularity against the risk of singleton observations. The higher-level SNI sections provide only 21 categories, which we consider too coarse to absorb meaningful industry heterogeneity. The full SNI title classification (over 400 categories) produces excessive singletons and absorbs too much of the cross-sectional variation in PSR.

With a two-year panel, firm fixed effects are not viable in the main specification. The within-firm variation in PSR over such a short window is likely insufficient to identify the association. Our industry-plus-year fixed effects specification absorbs persistent sectoral differences and common macroeconomic shocks but does not control for time-invariant unobservable firm characteristics. This restriction is a limitation of the short panel.

Standard errors are clustered at the firm level throughout. This accounts for serial correlation within firms across the two sample years and for heteroskedasticity, which is confirmed by a Breusch-Pagan test. Robustness checks report two-way clustering by firm and year and clustering at the industry level.

4.8.8. Leverage Decomposition Specification

To examine whether PSR is associated with each component of the leverage identity, we estimate the baseline specification with COL, L/E, EFF_TAX, and ROE_LEVERAGE as successive dependent variables. The specification is identical to Equation (22) except that FLEV is excluded from the control vector when COL or L/E is the dependent variable, since FLEV is a function of these outcomes. The resulting table traces the path from operating margins to after-tax equity returns, allowing any ROE difference to be decomposed into its operating, financing, and leverage components.

4.9. Identification, Measurement and Limitations

This study employs an associational research design. The coefficient β_1 in the baseline specification estimates the conditional correlation between PSR and profitability, not a causal

effect. We discuss four sources of concern for inference. The first three relate to endogeneity in the standard sense, and the fourth relates to measurement error in PSR.

4.9.1. Selection

Selection into public-sector supply is non-random. Firms that pursue public-sector contracts may differ systematically from those that do not along unobservable dimensions such as managerial quality, risk preferences, or operational efficiency that also affect profitability. If more profitable firms are more likely to win procurement contracts, the estimated association reflects selection rather than a treatment effect. Conversely, if less profitable firms are driven to seek public-sector demand as a stable revenue source, the selection bias operates in the opposite direction. The industry and year fixed effects absorb a meaningful portion of this heterogeneity, but time-varying firm-level confounders remain a concern.

4.9.2. Reverse Causality

Reverse causality is plausible. More profitable firms may have greater capacity to invest in the tendering process, yielding higher PSR. Alternatively, if public-sector contracts compress margins through competitive tendering, firms with high PSR may exhibit lower profitability. The ambiguity in the direction of reverse causality complicates straightforward interpretation of a positive β_1.

4.9.3. Omitted Variables

Omitted variable bias cannot be ruled out. Factors such as managerial networks, geographic proximity to public entities, or firm-specific cost structures may correlate with both PSR and profitability. To the extent that these factors are time-invariant and sector-specific, industry fixed effects mitigate this concern, but within-industry variation remains uncontrolled. For β_1 to be interpreted causally, PSR would need to be as good as randomly assigned conditional on observed characteristics and industry-by-year fixed effects. Given the selection concerns documented above, we view this assumption as implausible and interpret β_1 throughout as a conditional association rather than a treatment effect.

4.9.4. Measurement Error in PSR

Our PSR variable is measured with error from three identifiable sources. Only the third is non-classical and warrants extended discussion.

The first source arises from differences in VAT treatment between the numerator and denominator. Net sales (*nettoomsättning*) as reported in Swedish financial statements under both the K2 and K3 frameworks are recorded exclusive of value-added tax, following Swedish GAAP and the Annual Accounts Act (Riksdagen, 1995). The procurement dataset invoice data, by contrast, are drawn from public entities' supplier ledgers and accounts payable systems, which in most cases record the gross amount paid or payable to each supplier inclusive of VAT,

though a minority of entities record amounts exclusive of VAT. Swedish standard VAT rates are 25 percent for most goods and services, 12 percent for food and hospitality, and 6 percent for books, newspapers, and public transport. Categories such as health care and education are exempt entirely. For a firm whose entire revenue comes from VAT-liable public-sector sales at the standard rate and whose public buyers record invoices inclusive of VAT, the procurement dataset numerator would be 25 percent larger than the corresponding Retriever denominator, mechanically producing a PSR of 1.25 rather than 1. Conversely, for a firm whose public buyers record invoices exclusive of VAT, the numerator and denominator are on a more comparable footing, though a residual mismatch may remain if any VAT is embedded in the recorded amounts. Systematic VAT adjustment would require knowing the VAT category of each invoice, which is not available at the aggregated level. We address this issue by bounding PSR to the unit interval [0, 1] in the baseline sample (filter F7), excluding 2,249 firm-year observations. Because the VAT treatment varies across public entities rather than being uniformly gross or net, the mismatch introduces heterogeneous noise into PSR rather than a uniform upward bias. The cross-sectional ranking of firms by public-sector dependence is broadly preserved, and the bounding by filter F7 addresses the most extreme cases near the upper end of the distribution. Another contributor to PSR values exceeding one is the calendarization wedge described in Section 4.4. For firms whose fiscal year does not coincide with the calendar year, the numerator (calendar-year invoices) and denominator (fiscal-year sales mapped to calendar year) cover overlapping but non-identical twelve-month windows. Like the VAT mismatch, this introduces noise without a systematic directional bias and is bounded by F7.

The second source relates to firms without any coverage in the procurement dataset. These firm-years are assigned PSR of zero. For calendar years 2022 and 2023, the procurement dataset covers 271 of Sweden's 290 municipalities, 18 regions, and a share of public agencies and government-owned companies (Section 4.2.1), so the majority of zero-assigned firm-years reflect genuinely zero public-sector revenue. A residual concern remains for firms whose public buyers are among the entities not yet included in the database. We address this in two ways. First, we limit the main panel to calendar years 2022 and 2023, for which the set of covered entities is most complete. Calendar year 2024, where only 165 of the cumulative collected cohort of 271 municipalities had reported as of the data retrieval date, is reserved for a separate robustness analysis (Table 10). Second, the intensity-margin analysis (Panel B in Table 5) restricts the sample to firm-years with strictly positive PSR, eliminating the participation-margin comparison entirely and rendering the false-zero problem irrelevant for that specification.

The third source, which is the most consequential for inference, concerns the entity-specific coverage windows in the procurement dataset for 2023. Although the dataset covers calendar years 2022 and 2023 in the aggregate, individual public entities are covered through different end dates ranging from May 2023 to December 2024 or later. Sixty-seven percent of public entities in the dataset have coverage extending through at least December 31, 2023, while 16 percent have coverage ending on or before October 31, 2023. Notable cases include Örebro municipality (through October 2023) and Stockholm municipality (through May 2023). Because we do not pro-rate or window-restrict firm-level net sales, supplier firms whose public

buyers fall in the partially covered group will have a 2023 numerator that captures public sales for only a fraction of the calendar year against a full-year denominator. The resulting bias is downward (PSR is understated) and heterogeneous, with its magnitude depending on each firm's mix of public buyers and the timing of their invoices within the year. For a firm whose 2023 public sales come exclusively from Stockholm municipality, the numerator captures approximately five of twelve months. For a firm whose public sales come exclusively from a buyer covered through December 2023, the numerator is unaffected. We accept this heterogeneity in order to retain a national scale *aktiebolag* panel. The alternative, restricting the analysis to firms whose buyers are all covered through year-end, would require firm-by-firm joining of the buyer-coverage metadata to each supplier-buyer-year observation in our panel, and we leave this restriction for future work.

This third measurement error is non-classical because it is correlated with the firm's public-buyer composition. It is mitigated, but not eliminated, by industry-by-year fixed effects. Two implications follow. First, in the baseline specification, β_1 is biased toward zero on the 2023 cross-section to the extent that PSR for 2023 is understated for firms with substantial actual public-sector exposure. Second, year fixed effects absorb the average cross-firm shift but not the heterogeneous component, leaving residual within-year variance that is correlated with PSR. The direction of the resulting bias is conservative. Understated PSR attenuates the estimated association toward zero, so the baseline coefficient is, if anything, a lower bound on the magnitude of the true conditional association.[14]

We address the concerns above through several robustness checks. The intensity-margin analysis restricts the sample to firm-years with non-zero PSR, eliminating the participation-margin selection problem. Propensity score matching and entropy balancing address selection on observables. We examine coefficient stability across alternative control sets, variable definitions, and sample restrictions, and we treat the results as a conditional association rather than a causal effect.

4.10. Descriptive Statistics

4.10.1. Summary Statistics

Table 2 presents summary statistics for the baseline regression sample. The full-sample column covers all 512,877 firm-year observations. The PSR sample column covers the 149,794 firm-years with PSR coverage, and the non-PSR sample column covers the remaining 363,083 firm-years. For each variable, we report the mean, standard deviation and median. All continuous variables are reported after winsorization at the 1st and 99th percentiles. The final column reports the difference in means between the PSR and non-PSR subsamples, with significance based on two-tailed t-tests.

The baseline sample covers 284,437 unique firms across calendar years 2022 and 2023. The median firm has net sales of SEK 2.4 million, total assets of SEK 2.5 million and one employee.

[14] Because partial coverage can only understate the 2023 numerator, the resulting measurement error is one-directional and attenuates the estimated coefficient.

Mean PSR in the full sample is 0.019, with a median of zero, reflecting the fact that approximately 71 percent of firm-years have no observed coverage in the procurement dataset. Among firms in the PSR sample, mean PSR is 0.065 with a median of 0.005, indicating that most public-sector suppliers derive only a small fraction of their revenue from public buyers.

The mean-difference tests reveal systematic differences between the two groups. Public-sector suppliers are significantly larger (mean sales SEK 36.4 million vs. SEK 10.0 million), older (mean age 19.4 vs. 15.0 years) and have lower demand uncertainty (mean DEM_UNC 0.321 vs. 0.440). They also exhibit lower ROA (0.142 vs. 0.164), lower operating margin (0.078 vs. 0.130) and higher asset turnover (2.236 vs. 1.744). The negative ROA and OM mean differences indicate that public-sector suppliers are unconditionally less profitable than non-suppliers, a pattern that the regression analysis and participation-versus-intensity margin decomposition in Section 5 will decompose further.

4.10.2. Distribution of PSR

Table A1 reports the distribution of PSR among firm-years with positive public-sector revenue, separately by calendar year. The distribution is right-skewed, with the largest group (28,679 firm-years in 2022 and 27,595 in 2023) having PSR in the [0, 0.01] range, meaning that most public-sector suppliers derive less than one percent of revenue from public buyers. At the upper tail, 3,778 firm-years in 2022 and 2,871 in 2023 derive more than half of their revenue from public-sector buyers. An additional 31,362 firm-years are classified as PSR-covered at the firm level but record no invoices in the observation year. These firm-years are excluded from the PSR > 0 distribution in Table A1 but remain in the baseline PSR sample used in Table 2.

The skew has implications for the comparison with disclosure-based studies. Approximately 79 percent of PSR-positive firm-years record an aggregate PSR below 10 percent. Because SEC Regulation S-K and IFRS 8 trigger disclosure only when an *individual* customer reaches 10 percent of revenue, and because aggregate exposure can build up across many sub-threshold individual buyers, an even smaller share of our suppliers would have any single public buyer large enough to trigger disclosure on its own.

4.10.3. Industry Composition

Table A2 reports the sectoral composition of the baseline sample using SNI section codes. The three largest sectors are professional and scientific services (19.5 percent of the 2023 sample), construction (12.7 percent), and wholesale and retail trade (13.1 percent). Public-sector supplier shares vary across sectors. For instance, the PSR sample is overrepresented in accommodation and food services, education, and manufacturing, and underrepresented in professional services and real estate. These compositional differences motivate the use of industry fixed effects in the regression specifications.

4.10.4. Pairwise Correlations

Table 3 presents pairwise Pearson correlations among the main regression variables. The unconditional correlation between PSR and ROA is 0.01, small in magnitude and positive. PSR is negatively correlated with DEM_UNC ($r = -0.03$, $p < 0.01$), providing preliminary evidence for the demand-uncertainty mechanism. PSR is also positively correlated with log total assets ($r = 0.06$) and firm age ($r = 0.03$), consistent with larger and older firms being more likely to supply the public sector.

Among the control variables, no pairwise correlation exceeds 0.40 in absolute value, and variance inflation factors from the main specification range from 1.00 to 1.16, indicating that multicollinearity is not a concern.

Correlations among the DuPont components, such as the 0.55 between ROA and OM, reflect identities imposed by the DuPont decomposition rather than substantive associations, and they do not affect the regression estimates.

Table 2. Descriptive Statistics

	Full Sample (n = 512,877)			PSR Sample (n = 149,794)			Non-PSR Sample (n = 363,083)			Mean
Variables	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median	Difference
Size										
Sales	17,688	61,405	2,388	36,429	89,842	7,130	9,956	42,326	1,750	26,473***
TA	19,942	77,545	2,535	35,755	110,684	4,424	13,418	57,394	2,048	22,338***
Employees	6	17	1	12	25	4	3	11	1	9***
Public Sector Revenue Exposure										
PSR	0.019	0.081	0.000	0.065	0.140	0.005	0.000	0.000	0.000	0.065***
Dependent Variables										
ROA	0.157	0.288	0.096	0.142	0.256	0.098	0.164	0.300	0.094	-0.023***
ROE	0.301	0.813	0.174	0.273	0.768	0.169	0.313	0.830	0.176	-0.040***
ATO	1.888	1.796	1.414	2.236	1.755	1.864	1.744	1.792	1.218	0.492***
OM	0.115	0.303	0.082	0.078	0.223	0.060	0.130	0.329	0.097	-0.052***
PM	0.107	0.327	0.053	0.061	0.217	0.037	0.127	0.361	0.064	-0.066***
DEM_UNC	0.405	0.414	0.264	0.321	0.346	0.208	0.440	0.435	0.294	-0.119***
ROA_VOL	0.268	0.435	0.145	0.220	0.358	0.127	0.288	0.461	0.155	-0.068***
INVEST [(a)]	0.541	2.027	0.000	0.587	1.998	0.031	0.516	2.042	0.000	0.071***
COL [(b)]	-0.002	0.063	0.001	0.002	0.048	0.002	-0.003	0.068	0.000	0.005***
L/E	3.919	10.731	0.935	3.703	9.582	1.161	4.008	11.170	0.836	-0.305***
EFF_TAX [(c)]	0.200	0.194	0.200	0.202	0.191	0.197	0.200	0.195	0.200	0.003***
PRETAX_ROE	0.429	1.217	0.245	0.410	1.192	0.251	0.437	1.227	0.242	-0.027***
ROE_LEVERAGE [(c)]	0.559	0.994	0.276	0.550	0.972	0.279	0.562	1.002	0.275	-0.012***
LEV_SPREAD [(b)]	0.163	0.317	0.095	0.141	0.275	0.094	0.173	0.333	0.095	-0.032***

(continued on next page)

	Full Sample (n = 512,877)			PSR Sample (n = 149,794)			Non-PSR Sample (n = 363,083)			Mean
Variables	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median	Mean	Std. Dev.	Median	Difference
Control Variables										
AGE	16.304	12.828	12.000	19.427	14.186	15.000	15.015	11.989	11.000	4.412***
SALE_GR	0.050	0.433	0.036	0.058	0.350	0.045	0.047	0.463	0.032	0.012***
FLEV	5.198	10.973	2.182	5.020	9.845	2.433	5.271	11.405	2.079	-0.250***
CASH/TA	0.333	0.287	0.265	0.297	0.265	0.234	0.347	0.295	0.282	-0.050***
LIAB/TA	0.527	0.261	0.529	0.568	0.238	0.582	0.510	0.267	0.504	0.058***
Additional Analyses										
PSR_HHI	n.m.	n.m.	n.m.	0.606	0.409	0.725	n.a.	n.a.	n.a.	n.a.
CC_PUB	n.m.	n.m.	n.m.	0.021	0.087	0.000	n.a.	n.a.	n.a.	n.a.
PSR_LG	n.m.	n.m.	n.m.	0.646	0.401	0.839	n.a.	n.a.	n.a.	n.a.
PSR_LG_TOT	n.m.	n.m.	n.m.	0.053	0.132	0.004	n.a.	n.a.	n.a.	n.a.
N_BUYERS	n.m.	n.m.	n.m.	5.582	12.888	2.000	n.a.	n.a.	n.a.	n.a.

***, **, * Represent statistical significance based on two-tailed tests at the 0.01, 0.05, and 0.10 levels, respectively. This table reports summary statistics for the baseline sample of 512,877 firm-year observations (284,437 unique Swedish *aktiebolag*) over the period 2022–2023. The PSR Sample comprises firm-years with procurement data invoice coverage (N = 149,794); the Non-PSR Sample comprises firm-years without coverage (N = 363,083). The Mean Difference column reports the difference in subsample means (PSR minus Non-PSR) with significance from a two-sample t-test. Sales and total assets are in SEK thousands. AGE is measured in years since incorporation. Continuous variables are winsorized at their 1st and 99th percentiles. [(a)] N = 343,731. INVEST is undefined for firms with zero beginning tangible and intangible fixed assets. [(b)] N = 511,149. COL and LEV_SPREAD are undefined for firms with zero beginning total liabilities (1,728 firm-years, 0.3% of the sample). [(c)] N = 392,570 (EFF_TAX) and 391,284 (ROE_LEVERAGE). The effective tax rate is undefined for loss-making firms with negative earnings before tax. n.m. = not meaningful (buyer-structure variables are defined only for PSR Sample firms). n.a. = not applicable. All variables are defined in the Appendix.

Table 3. Pearson Correlation Matrix

	1	2	3	4	5	6	7	8	9	10	11	12
1. *PSR*	1.00***											
2. *ROA*	0.01***	1.00***										
3. *OM*	-0.00***	0.55***	1.00***									
4. *ATO*	0.01***	0.28***	-0.10***	1.00***								
5. *PM*	-0.02***	0.32***	0.49***	-0.14***	1.00***							
6. *ln(TA)*	0.06***	-0.09***	0.02***	-0.26***	0.07***	1.00***						
7. *AGE*	0.03***	-0.11***	-0.05***	-0.12***	0.00	0.29***	1.00***					
8. *SALE_GR*	0.00***	0.34***	0.19***	0.27***	0.06***	0.04***	-0.04***	1.00***				
9. *FLEV*	-0.00*	-0.11***	-0.01***	-0.08***	-0.10***	0.20***	-0.06***	0.03***	1.00***			
10. *DEM_UNC*	-0.03***	0.06***	-0.06***	0.00***	0.03***	-0.11***	-0.14***	0.17***	0.01***	1.00***		
11. *DEM_UNC_IND*	0.05***	0.03***	-0.04***	0.07***	-0.01***	-0.07***	-0.03***	0.05***	-0.06***	0.10***	1.00***	
12. *INVEST*	0.01***	0.09***	-0.01***	0.12***	-0.00**	0.00*	-0.03***	0.08***	-0.05***	0.03***	0.02***	1.00***

***, **, * Represent statistical significance based on two-tailed tests at the 0.01, 0.05, and 0.10 levels, respectively. This table reports pairwise Pearson correlation coefficients for the main variables used in the regression analysis. Variables are ordered by role: the explanatory variable (*PSR*), DuPont profitability components (*ROA*, *OM*, *ATO*, *PM*), control variables (ln(*TA*), *AGE*, *SALE_GR*, *FLEV*), the demand uncertainty proxy (*DEM_UNC*), the industry-level demand uncertainty moderator (*DEM_UNC_IND*), and the main investment measure (*INVEST*). The sample is the full baseline of 512,877 firm-year observations; pairwise complete observations are used where coverage differs (*INVEST*: N = 343,731; see Table 2 notes). Continuous variables are winsorized at their 1st and 99th percentiles. All variables are defined in the Appendix.

5. Results and Findings

5.1. PSR and Profitability (H1)

5.1.1. Hierarchical Specifications

Table 4 presents the baseline estimates of Equation (22) for four profitability measures: ROA, OM, ATO, and PM. In the unconditional specification (column 1), the PSR coefficient on ROA is positive and significant (0.026, t = 4.53). Adding firm-level controls strengthens this to 0.042 (t = 7.84), and adding year fixed effects leaves it virtually unchanged at 0.041 (t = 7.74). Adding industry fixed effects in the main specification (column 4) reverses the sign. The coefficient becomes small, negative, and statistically insignificant (−0.007, t = −1.30).

This sign reversal indicates that the positive unconditional association in columns (1)–(3) reflects cross-industry sorting rather than a within-industry effect of public-sector exposure. Public-sector suppliers are concentrated in sectors such as education, health care, and professional services that exhibit structurally higher profitability. Pooling firms across these sectors with firms in lower-profitability sectors produces a positive PSR–ROA correlation that is driven by industry composition, not by anything that happens within a given industry when a firm supplies the public sector. Industry fixed effects remove this composition effect by restricting the comparison to firms in the same two-digit SNI industry in the same year. The within-industry association, shown in column (4), is statistically indistinguishable from zero.

In the main specification, the DuPont decomposition shows that the near-zero ROA coefficient is the net of two offsetting movements. PSR is negatively associated with operating margin (−0.047, t = −7.85) and positively associated with asset turnover (0.630, t = 18.52). The profit margin coefficient is also negative and significant (−0.128, t = −23.08). The negative margin association is consistent with a bargaining-power view in which competitive tendering under LOU compresses supplier margins, while the positive turnover association indicates that public-sector suppliers generate more sales per unit of assets than firms in the same industry that do not supply the public sector.

Because our linear specification imposes a constant marginal association of PSR with profitability, the average ROA coefficient could mask the kind of nonlinearity Josephson et al. (2019) and Falcone et al. (2024) document for U.S. listed firms. The size-heterogeneity analysis in Section 5.5.3 and the intensity-margin specification immediately below partially address this concern by allowing the slope to differ across firm-size and exposure-level subsamples.

Table 4. PSR and Profitability — Hierarchical Specifications

	DuPont Profitability Analysis						
	ROA				OM	ATO	PM
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
PSR	0.026***	0.042***	0.041***	−0.007	−0.047***	0.630***	−0.128***
	(4.53)	(7.84)	(7.74)	(−1.30)	(−7.85)	(18.52)	(−23.08)
ln(TA)		−0.010***	−0.010***	−0.006***	0.001***	−0.208***	0.018***
		(−35.23)	(−35.25)	(−19.54)	(3.11)	(−105.24)	(37.71)
AGE		−0.002***	−0.002***	−0.001***	−0.001***	−0.008***	−0.000***
		(−60.15)	(−60.15)	(−49.91)	(−20.29)	(−36.59)	(−5.96)
SALE_GR		0.230***	0.230***	0.229***	0.128***	1.177***	0.045***
		(165.22)	(164.2)	(164.17)	(87.02)	(164.76)	(28.21)
FLEV		−0.003***	−0.003***	−0.002***	−0.001***	−0.000	−0.004***
		(−90.65)	(−90.67)	(−68.66)	(−21.68)	(−0.06)	(−62.92)
Industry FE	No	No	No	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
N	512,877	512,877	512,877	511,208	511,208	511,208	511,208
Adj. R^2	0	0.145	0.145	0.166	0.081	0.267	0.053

***, **, * Represent statistical significance based on two-tailed tests at the 0.01, 0.05, and 0.10 levels, respectively. This table presents OLS regressions of firm profitability on public-sector revenue share (PSR) for the full baseline sample of Swedish *aktiebolag* over the period 2022–2023. The main explanatory variable is PSR, defined as public procurement invoice revenue (from the procurement dataset) divided by total net sales (from Retriever). The dependent variables are: ROA (return on assets = EBIT / beginning total assets), OM (operating margin = EBIT / sales), ATO (asset turnover = sales / beginning total assets), and PM (profit margin = net income / sales). Columns (1)–(4) present a hierarchical specification for ROA: (1) PSR only, (2) PSR + controls, (3) PSR + controls + year FE, (4) PSR + controls + industry and year FE. Columns (5)–(7) present the main specification (industry + year FE) for OM, ATO, and PM. The vector of control variables includes log total assets, firm age, log sales growth, and financial leverage (FLEV). All regression models include both (2-digit SNI) industry and year fixed effects, and standard errors are clustered at the firm level. t-statistics are presented in parentheses. The reduction in N from column (3) to column (4) reflects 1,669 observations with missing (2-digit SNI) industry classification, which are absorbed by the industry fixed effects. Continuous variables are winsorized at their 1st and 99th percentiles. All variables are defined in the Appendix.

5.1.2. The Participation Versus Intensity Margin

The full-sample results in Table 4 pool 363,083 firm-years with no PSR coverage and 149,794 firm-years with PSR coverage, yielding an effective estimation sample of 511,208 after excluding 1,669 observations with missing industry codes. The PSR coefficient in this pooled specification captures a weighted average of two distinct sources of variation. The first is the participation margin, which compares public-sector suppliers to non-suppliers, and the second is the intensity margin, which compares firms with higher versus lower public-sector revenue share among firms that already supply the public sector.

To isolate the intensity margin, Panel B in Table 5 re-estimates the main specification on the subsample of 117,794 firm-years with strictly positive PSR. This is smaller than the 149,794 procurement-data-covered firm-years because 31,362 firm-years carry procurement-data coverage but record zero invoice amounts in the observation year. With this restriction, the

signs of the ROA and OM associations reverse. Among firm-years with PSR > 0, the coefficient on PSR is 0.030 for ROA (t = 5.08), 0.058 for OM (t = 8.98), and −0.721 for ATO (t = −18.61). All three coefficients are statistically significant at the 1 percent level.

Table 5. Participation Versus Intensity Margin

	Panel A: Full Sample			Panel B: Intensity Margin		
Variable	ROA	OM	ATO	ROA	OM	ATO
PSR	−0.007	−0.047***	0.630***	0.030***	0.058***	−0.721***
	(−1.30)	(−7.85)	(18.52)	(5.08)	(8.98)	(−18.61)
ln(TA)	−0.006***	0.001***	−0.208***	−0.003***	0.003***	−0.172***
	(−19.54)	(3.11)	(−105.24)	(−5.26)	(5.10)	(−46.20)
AGE	−0.001***	−0.001***	−0.008***	−0.001***	−0.000***	−0.005***
	(−49.91)	(−20.29)	(−36.59)	(−18.82)	(−5.99)	(−14.72)
SALE_GR	0.229***	0.128***	1.177***	0.237***	0.095***	1.466***
	(164.17)	(87.02)	(164.76)	(65.75)	(28.14)	(77.70)
FLEV	−0.002***	−0.001***	−0.000	−0.002***	−0.001***	0.005***
	(−68.66)	(−21.68)	(−0.06)	(−27.60)	(−12.81)	(7.46)
Industry + Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	511,208	511,208	511,208	117,794	117,794	117,794
Adj. R^2	0.166	0.081	0.267	0.138	0.064	0.239

***, **, * Represent statistical significance based on two-tailed tests at the 0.01, 0.05, and 0.10 levels, respectively. This table presents OLS regressions of firm profitability on PSR for the full baseline sample (Panel A) and for the intensity-margin subsample of procurement data-covered firms only (Panel B, PSR > 0). The intensity-margin specification isolates variation in the degree of public-sector exposure among firms that supply the public sector, removing the participation-margin comparison between public-sector suppliers and non-suppliers. The intensity-margin sample (Panel B) comprises 117,794 firm-years with positive PSR in the observation year, a subset of the 149,794 procurement data-covered firm-years; the difference reflects 31,362 firm-years that are procurement data-covered at the firm level but report zero public-sector invoices in the given year. The remaining 638 observations are fixed-effect singletons. The sign reversal relative to the full-sample results indicates that the full-sample associations are driven by the participation margin, i.e., the compositional difference between firms that do and do not supply the public sector, rather than by the intensity of public-sector exposure among suppliers. All regression models include both (2-digit SNI) industry and year fixed effects, and standard errors are clustered at the firm level. t-statistics are presented in parentheses. Continuous variables are winsorized at their 1st and 99th percentiles. All variables are defined in the Appendix.

The sign reversal in Table 5 between Panel A and Panel B is informative about the source of the full-sample associations. Panel A identifies the PSR coefficient from two sources of variation, namely the difference between firms with zero PSR and firms with any public-sector revenue, and the variation in PSR intensity among firms that already supply the public sector. Public-sector suppliers as a group have lower margins and ROA than non-suppliers, as shown in the mean-difference tests in Table 2. Even after controlling for size, age, industry, and year, this group-level difference pulls the Table 4 coefficients negative. Panel B in Table 5 removes this source of variation by conditioning on PSR > 0. The coefficient is then estimated only from the comparison among firms that already participate in public procurement. Within the set of

suppliers, more public-sector revenue is associated with higher profitability, consistent with the demand-uncertainty channel suggested by Cohen and Li (2020).

The economic magnitude is meaningful. A one-standard-deviation increase in PSR within the intensity-margin sample (approximately 0.14) is associated with a ROA increase of roughly 0.4 percentage points, around 3 percent of the intensity-margin mean ROA of 0.142. The OM effect is proportionally larger, implying that the within-supplier association operates primarily through margins once the compositional effect at the participation margin is removed. The negative ATO coefficient indicates that among public-sector suppliers, higher PSR is associated with lower turnover, a pattern that offsets part of the margin improvement and explains why the ROA coefficient, though positive, is smaller than the OM coefficient. One interpretation is that supplying a larger share of revenue to public buyers requires holding additional working capital, for example, accounts receivable tied to long payment terms, or dedicated operational capacity, reducing asset turnover even as margins improve. The negative ATO coefficient runs counter to Cohen and Li (2020), who document a positive ATO association in their U.S. listed-firm sample with major government customers.

H1 therefore finds support, with the qualification that the direction depends on the margin of comparison. The intensity-margin association is positive, while the full-sample association is dominated by selection into the supplier population.

5.2. Industry Demand Volatility as a Moderator (H2)

Our second hypothesis tests whether the PSR–profitability association varies with industry-level demand volatility. The interaction specification reports $PSR \times DEM_UNC_IND$, where DEM_UNC_IND is the rolling standard deviation of mean log sales growth within each two-digit SNI industry.

The interaction coefficients, shown in Table 6, are negative: −0.494 for ROA (t = −3.08), −0.179 for OM (t = −0.91), and −2.273 for ATO (t = −2.13). The ROA and ATO interactions are significant at the 5 percent level while the OM interaction is not. The PSR–profitability association is therefore weaker, not stronger, in industries with higher demand volatility. This is the opposite of what one would expect under the demand-uncertainty channel, where the stabilizing benefit of public-sector revenue should be largest precisely where private-sector demand is most volatile.

The specification includes both industry fixed effects and DEM_UNC_IND as a time-varying industry characteristic, so the interaction is identified from within-industry variation in demand volatility over the two sample years. With only two estimation years, the within-industry variation is limited, and DEM_UNC_IND captures total industry demand volatility rather than private-sector demand specifically. If public-sector suppliers are concentrated in industries whose volatility is itself driven by procurement cycles, the interaction may reflect those cycles rather than private-sector volatility. The negative interaction does not falsify the demand-uncertainty channel, which is examined more directly in Section 5.5.1, but it does mean that the moderation pattern one would expect under that channel is not visible in the present sample.

Table 6. Industry Demand Volatility and the PSR–Profitability Association

Variable	ROA	OM	ATO
PSR	0.018**	−0.038***	0.743***
	(1.98)	(−3.21)	(12.65)
DEM_UNC_IND	−0.228**	−0.109	−3.320***
	(−2.48)	(−1.36)	(−7.46)
PSR × DEM_UNC_IND	−0.494***	−0.179	−2.273**
	(−3.08)	(−0.91)	(−2.13)
ln(TA)	−0.006***	0.001***	−0.208***
	(−19.60)	(3.09)	(−105.21)
AGE	−0.001***	−0.001***	−0.008***
	(−49.92)	(−20.30)	(−36.59)
SALE_GR	0.229***	0.128***	1.177***
	(164.14)	(87.00)	(164.72)
FLEV	−0.002***	−0.001***	−0.000
	(−68.65)	(−21.67)	(−0.07)
Industry + Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
N	511,208	511,208	511,208
Adj. R^2	0.166	0.081	0.267

***, **, * Represent statistical significance based on two-tailed tests at the 0.01, 0.05, and 0.10 levels, respectively. This table presents OLS regressions testing whether industry-level demand uncertainty moderates the association between PSR and firm profitability (H2). The specification follows Equation (23). The main explanatory variable is PSR, defined as public procurement invoice revenue (from the procurement dataset) divided by total net sales (from Retriever). DEM_UNC_IND is the rolling standard deviation of mean log sales growth within each two-digit SNI industry over a backward five-year window (minimum three valid observations). PSR × DEM_UNC_IND is the interaction term; a negative coefficient indicates that the PSR–profitability association is attenuated in industries with higher demand volatility. The PSR main effect is evaluated at DEM_UNC_IND = 0. The vector of control variables includes log total assets, firm age, log sales growth, and financial leverage (FLEV). All regression models include both (2-digit SNI) industry and year fixed effects, and standard errors are clustered at the firm level. *t*-statistics are presented in parentheses. Continuous variables are winsorized at their 1st and 99th percentiles. All variables are defined in the Appendix.

5.3. Buyer-Type Heterogeneity (H3)

Our third hypothesis tests whether the PSR–profitability association differs across public buyer types. Table 7 decomposes PSR into six buyer-type components and reports coefficients on each.

The ROA coefficient on central-government PSR is 0.049 (t = 3.32) and on regional PSR 0.040 (t = 4.21), both larger than the municipal coefficient of 0.016 (t = 2.48). Wald tests reject equality between the municipal and regional coefficients (F = 4.07) and between the municipal and central-government coefficients (F = 4.05). The regional and central-government coefficients are not statistically different from each other (F = 0.29).

For OM, the central-government (0.053, t = 3.60) and regional (0.057, t = 5.55) coefficients are both larger than the municipal coefficient (0.035, t = 4.96), and all three are significant at the 1 percent level. However, the pairwise Wald tests for OM are statistically insignificant at the 1 and 5 percent level.

As for ATO, the coefficient on central-government PSR (–0.043, t = –0.46) is statistically weaker than the coefficients on regional PSR (–0.477, t = –8.18) and municipal (–0.339, t = –7.74). Wald tests reject equality between the central-government coefficient and the regional and municipal coefficients on the 1 percent level (F = 15.72 and F = 8.27, respectively). The ATO penalty documented in Section 5.1, as shown in the table, is significant for municipal, regional, university, and college exposure. Central-government and publicly owned company exposure show no comparable penalty.

The buyer-type ranking on ROA is consistent with the institutional differences discussed in Section 3. Buyer types with greater fiscal stability and procurement capacity exhibit larger positive associations with supplier profitability. For each unit increase of PSR, the ROA association for central-government exposure is approximately three times that for municipal exposure, and regional exposure is approximately 2.4 times the municipal coefficient.

The buyer-type ranking could, however, partly reflect firm-size composition rather than fiscal characteristics. Central-government and regional contracts tend to be larger and operate through framework agreements, which may favor larger suppliers. Municipal contracts are typically smaller and reach a broader supplier population. The regression controls for log total assets, but a level control absorbs only the additive component of size, not a non-linear interaction between firm size and buyer type. If larger firms are more profitable for reasons unrelated to their public-sector exposure and are also disproportionately represented among central-government and regional suppliers, part of the buyer-type ranking would reflect a firm-size composition effect rather than buyer-type fiscal characteristics per se. The size-heterogeneity analysis in Section 5.5.3 confirms that the PSR–ROA association is positive only for above-median-size firms, which is consistent with this concern. Disentangling the buyer-type and size channels would require a buyer-type × size interaction not currently estimated.

Table 7. Buyer-Type PSR Decomposition

Variable	ROA	OM	ATO
PSR_MUNICIPALITY	0.016**	0.035***	−0.339***
	(2.48)	(4.96)	(−7.74)
PSR_REGION	0.040***	0.057***	−0.477***
	(4.21)	(5.55)	(−8.18)
PSR_GOVERNMENT	0.049***	0.053***	−0.043
	(3.32)	(3.60)	(−0.46)
PSR_GOVT_CO	0.058***	0.032	−0.061
	(3.06)	(1.43)	(−0.50)
PSR_UNIVERSITY	0.024	0.100**	−2.029***
	(0.69)	(2.21)	(−14.58)
PSR_COLLEGE	−0.140**	0.068	−1.566***
	(−2.31)	(0.75)	(−5.17)
ln(TA)	−0.002***	0.003***	−0.177***
	(−4.39)	(6.11)	(−51.01)
AGE	−0.001***	−0.000***	−0.006***
	(−21.35)	(−6.54)	(−16.54)
SALE_GR	0.235***	0.101***	1.446***
	(75.85)	(34.07)	(89.11)
FLEV	−0.002***	−0.002***	0.004***
	(−32.19)	(−14.39)	(6.42)
Industry + Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
N	149,090	149,090	149,090
Adj. R^2	0.141	0.066	0.24
Tests: β(municipality = region)	−0.023**	−0.021*	0.138*
F-statistic	[4.07]	[2.94]	[3.64]
Tests: β(municipality = government)	−0.032**	−0.018	−0.297***
F-statistic	[4.05]	[1.23]	[8.27]
Tests: β(region = government)	−0.009	0.003	−0.435***
F-statistic	[0.29]	[0.03]	[15.72]

***, **, *Represent statistical significance based on two-tailed tests at the 0.01, 0.05, and 0.10 levels, respectively. This table decomposes PSR into buyer-type components: PSR_MUNICIPALITY (municipality; *kommun*), PSR_REGION (region; *region*), PSR_GOVERNMENT (central government agency; *statlig myndighet*), PSR_GOVT_CO (publicly-owned company; *kommunalt bolag*), PSR_UNIVERSITY (university; *universitet*), and PSR_COLLEGE (college; *högskola*). Each component equals the firm's invoice revenue from that buyer type divided by total net sales. The six components sum to PSR by construction, so PSR is replaced (not included alongside) to avoid perfect multicollinearity. The Wald tests (F-statistics in brackets) test pairwise equality of buyer-type coefficients. The sample is restricted to procurement data-covered firm-years (the PSR Sample), comprising all firm-years for which public-sector invoice data are available, regardless of whether PSR is strictly positive in the observation year (N = 149,090 after removing fixed-effect singletons). All regression models include both (2-digit SNI) industry and year fixed effects, and standard errors are clustered at the firm level. t-statistics are presented in parentheses. Continuous variables are winsorized at their 1st and 99th percentiles. All variables are defined in the Appendix.

5.4. Within-Public Buyer Concentration (H4)

Our fourth hypothesis tests whether the concentration of a firm's public buyer base moderates the PSR–profitability relationship. We do not predict the direction of the moderation. Table 8 estimates three specifications on the PSR sample. Panel A adds PSR_HHI as a level control alongside PSR; Panel B adds the largest-buyer share (PSR_LG) alongside PSR; Panel C includes the PSR × PSR_HHI interaction.

In Panel A, PSR_HHI enters with small positive coefficients (0.003 on ROA, 0.006 on OM, 0.042 on ATO), while PSR retains its intensity-margin signs (0.030, 0.053, −0.538). Wald tests reject equality between the PSR and PSR_HHI coefficients (F = 17.76 on ROA, 51.60 on OM, 209.68 on ATO), indicating that the level of public-sector exposure and the concentration of that exposure have distinct and quantitatively different associations with profitability. Panel B produces similar results using the largest-buyer share as the concentration measure.

The interaction specification in Panel C is more informative about the moderation question. The PSR × PSR_HHI interaction is small and insignificant on ROA (0.011, t = 0.68), positive and significant on OM (0.062, t = 3.86), and negative on ATO (−1.264, t = −12.54). The OM interaction indicates that the within-supplier margin association is amplified when public revenues are concentrated among fewer buyers. A firm with more of its revenue from one public buyer realizes a larger margin benefit per unit of PSR than a firm with the same PSR spread across many buyers. This pattern is consistent with operational-efficiency gains from deeper coordination, lower transaction costs, and reduced contract-management overhead associated with concentrated buyer relationships (Patatoukas, 2012; Irvine et al, 2016).

The negative ATO interaction points in the opposite direction, namely that concentrated public-buyer exposure amplifies the asset-turnover penalty. A firm that depends on a single large public buyer must hold more working capital or dedicated capacity than a firm with diffused public-sector exposure, consistent with customer-specific investment and hold-up risk (Lustgarten, 1975; Hui et al, 2019). The two interactions roughly offset on ROA, which is why the ROA interaction is insignificant. H4 finds support in the sense that buyer concentration moderates the OM and ATO channels, though the moderating effects on margins and turnover run in opposite directions and cancel on the net profitability measure.

Table 8. Buyer-Structure Analysis

	Panel A: PSR + PSR_HHI			Panel B: PSR + PSR_LG			Panel C: PSR × PSR_HHI		
Variable	ROA	OM	ATO	ROA	OM	ATO	ROA	OM	ATO
PSR	0.030***	0.053***	−0.538***	0.029***	0.053***	−0.560***	0.022*	0.011	0.310***
	(5.05)	(8.38)	(−14.31)	(4.99)	(8.40)	(−14.83)	(1.82)	(0.94)	(4.03)
PSR_HHI	0.003**	0.006***	0.042***				0.003*	0.003**	0.096***
	(2.13)	(4.03)	(4.08)				(1.77)	(2.22)	(8.92)
PSR_LG				0.003**	0.004***	0.090***			
				(2.07)	(2.73)	(8.56)			
PSR × PSR_HHI							0.011	0.062***	−1.264***
							(0.68)	(3.86)	(−12.54)
ln(TA)	−0.002***	0.003***	−0.176***	−0.002***	0.003***	−0.176***	−0.002***	0.003***	−0.179***
	(−4.24)	(6.20)	(−50.51)	(−4.30)	(6.11)	(−50.63)	(−4.18)	(6.45)	(−51.35)
AGE	−0.001***	−0.000***	−0.006***	−0.001***	−0.000***	−0.006***	−0.001***	−0.000***	−0.006***
	(−21.43)	(−6.63)	(−16.59)	(−21.45)	(−6.63)	(−16.70)	(−21.43)	(−6.63)	(−16.62)
SALE_GR	0.235***	0.101***	1.447***	0.235***	0.101***	1.446***	0.235***	0.101***	1.445***
	(75.87)	(34.07)	(89.14)	(75.86)	(34.07)	(89.13)	(75.86)	(34.09)	(89.15)
FLEV	−0.002***	−0.002***	0.004***	−0.002***	−0.002***	0.004***	−0.002***	−0.002***	0.004***
	(−32.23)	(−14.43)	(6.44)	(−32.22)	(−14.42)	(6.45)	(−32.23)	(−14.46)	(6.57)
Industry + Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	149,090	149,090	149,090	149,090	149,090	149,090	149,090	149,090	149,090
Adj. R^2	0.141	0.066	0.239	0.141	0.066	0.24	0.141	0.066	0.241
Tests: β(PSR) − β(PSR_HHI) = 0	0.026***	0.047***	−0.580***						
F-statistic	[17.76]	[51.60]	[209.68]						

***, **, * Represent statistical significance based on two-tailed tests at the 0.01, 0.05, and 0.10 levels, respectively. This table presents OLS regressions examining the role of public buyer-base structure in the PSR–profitability relationship. The sample is restricted to procurement data-covered firm-years (the PSR Sample), comprising all firm-years for which public-sector invoice data are available, regardless of whether PSR is strictly positive in the observation year (N = 149,090 after removing fixed-effect singletons). PSR_HHI is the Herfindahl index of buyer concentration computed across individual public buyers (range [0,1]; higher = more concentrated). PSR_LG is the revenue share from the single largest public buyer. Both are within-public measures (denominator is total public sales, not total sales), which are nearly orthogonal to PSR (r = 0.08 and 0.11, respectively). Panel A adds PSR_HHI alongside PSR. Panel B adds PSR_LG alongside PSR. Panel C includes the $PSR \times PSR_HHI$ interaction. The Wald test (F-statistics in brackets) tests H0: $\beta(PSR) = \beta(PSR_HHI)$, i.e., whether the level of public exposure affects profitability differently from the concentration of that exposure. All regression models include both (2-digit SNI) industry and year fixed effects, and standard errors are clustered at the firm level. t-statistics are presented in parentheses. Continuous variables are winsorized at their 1st and 99th percentiles. All variables are defined in the Appendix.

5.5. Additional Analyses

5.5.1. Mechanism: Demand Uncertainty

If the demand-uncertainty channel is the operative mechanism behind the intensity-margin results, higher PSR should be associated with lower firm-level demand volatility. Panel A in Table 9 tests this by regressing DEM_UNC (the five-year rolling standard deviation of log sales growth) on PSR with the full set of controls and fixed effects.

The PSR coefficient on DEM_UNC is economically large and significant at the 1 percent level. Firms with higher public-sector revenue share exhibit lower five-year sales-growth volatility, consistent with the demand-stabilization mechanism. A one-standard-deviation increase in PSR within the intensity-margin sample is associated with a 0.021 reduction in DEM_UNC, roughly 5 percent of the sample mean. The three-year and seven-year rolling alternatives in Table A7 produce coefficients of −0.137 and −0.158 respectively, both significant at the 1 percent level, confirming that the finding is not sensitive to the window length.

DEM_UNC is computed on a five-year backward-looking window. The result is therefore consistent with either a treatment effect, in which public-sector exposure stabilizes demand going forward, or a selection effect, in which historically stable firms are more likely to supply public buyers because operational predictability aids successful tendering. Since DEM_UNC captures historical volatility while PSR is contemporaneous, the selection reading is the more conservative of the two, as the data show that high-PSR firms have lower historical demand volatility. However, this does not establish that public-sector exposure itself causes that stability. Separating the two channels would require either forward-looking volatility measures or panel variation in PSR that is plausibly exogenous to a firm's demand history, and neither is available here. The Panel A result is therefore taken as evidence consistent with the demand-uncertainty channel, not as proof of it.

5.5.2. Mechanism: Investment Efficiency

Panel B in Table 9 tests whether PSR improves the responsiveness of capital investment to growth opportunities by estimating Equation (25). The interaction term PSR × log sales growth captures whether firms with higher public-sector revenue share adjust investment more responsively to current sales-growth signals.

The forward investment specification $INVEST_{t+1}$ tests the theoretically appropriate timing in which current PSR should improve future investment responsiveness. It yields a positive but statistically insignificant interaction coefficient of 0.226 (t = 1.50). The main effect of PSR in the forward specification is positive and significant at the 5 percent level (0.090, t = 2.05), indicating that higher PSR is associated with higher future investment levels even though the interaction is not statistically distinguishable from zero. The contemporaneous specification $INVEST_t$ produces a stronger interaction result. The PSR × log sales growth coefficient is 0.516 (t = 3.00), significant at the 1 percent level. However, the contemporaneous result tests a different causal timing and is more susceptible to reverse causality, since firms that invest more in a given year may also be more likely to win public contracts in that same year.

The investment-efficiency channel therefore receives weaker and more ambiguous support than the demand-uncertainty channel. Whether this reflects the short panel (Falcone et al, 2023, emphasize that government-contracting effects differ across short and long horizons) or a genuine absence of investment-responsiveness gains in the Swedish private-firm setting cannot be resolved with only two years of data.

Table 9. Mechanism Tests — Demand Stabilization (A) and Investment Efficiency (B)

	Panel A	Panel B	
Variable	DEM_UNC	$INVEST_{t+1}$	$INVEST_t$
PSR	−0.151***	0.090**	0.007
	(−18.98)	(2.05)	(0.16)
PSR × SALE_GR		0.226	0.516***
		(1.50)	(3.00)
SALE_GR	0.152***	0.180***	0.407***
	(64.08)	(18.23)	(33.94)
ln(TA)	−0.027***	−0.000	0.051***
	(−49.57)	(−0.22)	(22.8)
AGE	−0.003***	−0.003***	−0.005***
	(−48.64)	(−13.38)	(−23.66)
FLEV	0.001***	−0.004***	−0.006***
	(15.99)	(−20.27)	(−33.89)
Industry + Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
N	511,208	311,471	342,417
Adj. R^2	0.092	0.009	0.017

***, **, * Represent statistical significance based on two-tailed tests at the 0.01, 0.05, and 0.10 levels, respectively. Panel A regresses firm-level demand uncertainty (DEM_UNC, the rolling standard deviation of log sales growth over five years) on PSR. A negative coefficient indicates that higher public-sector exposure is associated with lower demand uncertainty, consistent with the demand-stabilization channel. Panel B regresses forward capital investment (INVEST, the change in tangible and intangible fixed assets plus depreciation, scaled by beginning-of-period assets) on the interaction $PSR \times SALE_GR$. A positive interaction coefficient indicates that PSR improves the responsiveness of future investment to current sales growth opportunities, consistent with more efficient investment under stable demand. The contemporaneous INVEST specification is reported for comparison. The forward INVEST specification $INVEST_{t+1}$ tests whether PSR improves the responsiveness of future investment to current growth signals; the contemporaneous specification $INVEST_t$ is reported for comparison but does not test the same causal timing. The PSR main effect in the $INVEST_{t+1}$ column (0.090**, t = 2.05) indicates that higher PSR is associated with higher future investment levels, even though the interaction term (investment efficiency) is not statistically significant at conventional levels. All regression models include both (2-digit SNI) industry and year fixed effects, and standard errors are clustered at the firm level. t-statistics are presented in parentheses. Continuous variables are winsorized at their 1st and 99th percentiles. All variables are defined in the Appendix.

5.5.3. Size Heterogeneity

Shown in Table 10, splitting the sample at the median of total assets produces a pronounced sign difference in the PSR–ROA association. Among small firms (below-median total assets), the PSR coefficient on ROA is −0.043 (t = −4.68). Among large firms (above-median), it is 0.034 (t = 5.53). The difference is driven primarily by ATO. Large public-sector suppliers exhibit higher asset turnover relative to their industry peers (0.938, t = 21.54), while the ATO coefficient for small public-sector suppliers is not statistically distinguishable from zero (0.073, t = 1.43). The OM coefficient is negative in both subsamples (−0.045 for small firms, −0.033 for large firms), indicating that the margin association operates across the size distribution, but large firms more than compensate through asset efficiency.

The size gradient is consistent with scale-dependent benefits from public-sector supply relationships. Larger firms may be better positioned to absorb the fixed costs of tendering and contract management, realize asset-utilization gains from recurring public-sector revenue, and negotiate payment terms that mitigate working-capital drag. Smaller firms face the margin compression without the offsetting turnover gain, leading to net negative ROA effects. The pattern is broadly consistent with the U.S. evidence in Falcone et al. (2024), which identifies firm size as a moderator of the government-customer effect, with larger firms extracting greater benefits in their listed-firm sample. Our finding extends this size gradient to a private-firm setting and shows that the smallest firms experience a negative ROA effect, a phenomenon that public-firm samples could struggle to observe because the smallest public firms are likely still larger than the median Swedish private supplier.

Table 10. Robustness Summary — PSR Coefficients Across Alternative Specifications

	ROA		OM		ATO		
Specification	PSR coef.	t-stat	PSR coef.	t-stat	PSR coef.	t-stat	N
Baseline (Table 4, column 4)	−0.007	(−1.30)	−0.047***	(−7.85)	0.630***	(18.52)	511,208
Sample and Period Checks							
Extended sample: 2022–2024	−0.011**	(−2.05)	−0.055***	(−8.90)	0.699***	(19.69)	747,830
Firm size: below-median assets	−0.043***	(−4.68)	−0.045***	(−5.48)	0.073	(1.43)	256,304
Firm size: above-median assets	0.034***	(5.53)	−0.033***	(−3.93)	0.938***	(21.54)	254,904
Selection on Observables (see Table 12)							
PSM matched sample (Table 12, Panel A)	0.014***	(2.67)	−0.025***	(−4.14)	0.329***	(9.61)	298,257
Entropy-balanced sample (Table 12, Panel B)	0.008	(1.56)	−0.028***	(−4.63)	0.294***	(8.70)	511,208
Industry Exclusions							
Excl. financials (SNI 64–66)	−0.008	(−1.43)	−0.049***	(−8.14)	0.627***	(18.28)	499,100
Excl. real estate (SNI 68)	−0.008	(−1.46)	−0.046***	(−7.64)	0.628***	(17.10)	458,325
Excl. healthcare (SNI 86–88)	−0.009*	(−1.66)	−0.040***	(−6.12)	0.610***	(16.91)	493,464
Excl. financials + real estate + healthcare	−0.012*	(−1.95)	−0.040***	(−6.07)	0.601***	(15.04)	428,473
Measurement Checks							
Alternative PSR: excl. government-owned companies	−0.003	(−0.73)	−0.035***	(−6.73)	0.489***	(16.22)	511,208
Outlier treatment: trimming at p5/p95	0.022***	(5.99)	−0.056***	(−16.23)	0.582***	(21.95)	459,969
Standard Error Structures							
Two-way clustering (firm × year)	−0.007	(−1.42)	−0.047***	(−7.61)	0.630***	(8.62)	511,208
SE clustered by 2-digit SNI industry (not by firm)	−0.007	(−0.51)	−0.047***	(−3.53)	0.630***	(4.89)	511,208

***, **, * Represent statistical significance based on two-tailed tests at the 0.01, 0.05, and 0.10 levels, respectively. Each row reports the PSR coefficient from the main specification (industry + year FE, firm-clustered SE) on the indicated sample or with the indicated modification. The baseline row reproduces the main result from Table 4 (column 4). Controls include log total assets, firm age, log sales growth, and financial leverage. The extended sample row adds calendar year 2024 (N = 747,830). The firm-size rows split at the median of beginning total assets (2,535 SEK thousands). The matched-sample rows reproduce the PSR coefficients from Table 12 (Panel A: PSM; Panel B: entropy balancing) for direct comparison with the baseline. The industry-exclusion rows sequentially drop financials (SNI 64–66), real estate (SNI 68), healthcare (SNI 86–88), and all three combined. The alternative PSR row replaces PSR with PSR_excl_kb, computed after excluding invoices from government-owned companies. The trimming row drops observations beyond the 5th and 95th percentiles of each dependent variable instead of winsorizing. The standard-error rows use two-way *(continued on next page)*

(firm × year) and industry-level clustering; coefficients are identical to the baseline by construction. Continuous variables are winsorized at their 1st and 99th percentiles unless otherwise noted. All variables are defined in the Appendix.

Table 11. Leverage Decomposition

	Panel A: Full Sample				*Panel B: Intensity Margin*			
Variable	PSR coef.	t-stat	N	Adj. R^2	PSR coef.	t-stat	N	Adj. R^2
COL	0.008***	(7.46)	509,485	0.039	−0.011***	(−8.85)	117,657	0.027
LEV_SPREAD	−0.026***	(−4.48)	509,485	0.150	0.047***	(7.20)	117,657	0.127
L/E	−0.370*	(−1.73)	511,208	0.120	0.553**	(2.25)	117,794	0.074
PRETAX_ROE	0.033	(1.41)	511,208	0.075	0.078***	(2.88)	117,794	0.076
EFF_TAX	0.002	(0.43)	391,253	0.019	−0.003	(−0.58)	91,773	0.020
ROE_LEVERAGE	0.107***	(5.49)	389,969	0.292	−0.001	(−0.06)	91,668	0.347

***, **, * Represent statistical significance based on two-tailed tests at the 0.01, 0.05, and 0.10 levels, respectively. This table reports the association between PSR and leverage-decomposition variables following the extended DuPont identity of Johansson and Runsten (2014): ROE = [ROA + (ROA − COL) × (L/E)] × (1 − t). COL = (interest expense − interest income) / opening total liabilities. LEV_SPREAD = ROA − COL. L/E = opening total liabilities / opening equity. PRETAX_ROE = earnings before tax / opening equity. EFF_TAX = tax expense / earnings before tax. ROE_LEVERAGE = implied ROE from the leverage identity. For COL and L/E, financial leverage (FLEV) is excluded from the control vector to avoid near-tautological regressions, since FLEV = beginning total assets / beginning equity is mechanically related to L/E = beginning total liabilities / beginning equity (as total assets = equity + liabilities). The 'Full Sample' columns use the baseline sample; the 'Intensity Margin' columns restrict to procurement data-covered firms (PSR > 0). EFF_TAX and ROE_LEVERAGE are defined only for profitable firm-years with positive earnings before tax; N = 391,253 and 389,969 respectively for the full sample, and N = 91,773 and 91,668 respectively for the Intensity Margin sample. All specifications include (2-digit SNI) industry and year fixed effects with firm-clustered standard errors. t-statistics are presented in parentheses. Continuous variables are winsorized at their 1st and 99th percentiles. All variables are defined in the Appendix.

5.5.4. Leverage Decomposition

Table 11 traces the path from operating performance through financing to the return on equity using the extended DuPont decomposition of Johansson and Runsten (2014). The decomposition is presented as additional analysis rather than as part of the main DuPont test, since the leverage identity is approximate when all items between EBIT and EBT are not fully captured.

The cost of liabilities (COL) is positively associated with PSR in the full sample (0.0085, t = 7.46) but flips sign on the intensity margin (−0.0107, t = −8.85), indicating that within-supplier increases in PSR are associated with lower borrowing costs. The leverage spread (LEV_SPREAD, calculated as the difference between ROA and COL) follows the same economic pattern, moving from −0.026 (t = −4.48) in the full sample to 0.047 (t = 7.20) on the intensity margin. Since the spread mechanically increases with ROA and decreases with COL, the within-supplier combination of higher ROA and lower COL widens the spread. Leverage thus amplifies the PSR–ROE association. PRETAX_ROE confirms that the operating-return improvement transmits to equity holders before the tax wedge (0.078, t = 2.88 on the intensity margin).

The COL pattern on the intensity margin aligns with the demand-uncertainty channel (Cohen and Li, 2020), in which more stable public-sector revenues lower the firm's default risk and therefore its credit-risk premium. While government oversight also acts as a substitute for lender monitoring (Samuels, 2021), Cohen et al. (2022) note that such monitoring substitution primarily reduces covenant intensity rather than the cost of debt itself. Therefore, our reduced-form coefficient for COL is most directly explained by the risk-reduction channel, potentially reinforced by the transparency benefits of public-sector buyers.

The fully reconstructed identity ROE_LEVERAGE should be interpreted with some caution. Its correlation with the directly measured ROE is 0.72, so the decomposition approximates rather than reproduces the actual equity return. The gap mainly reflects a deliberate choice to preserve the economic interpretability of the component coefficients but introduces incongruities into the identity.[15] Given these limitations, it is included for completeness and directional interpretation rather than as an exact reconstruction of equity returns. There is a trade-off between strict component definitions, which keep each PSR coefficient interpretable and comparable to other studies, and closing the identity, which would require absorbing additional items into COL, reclassifying untaxed reserves between liabilities and equity and writing back the corresponding appropriations[16] and winsorizing the reconstructed series rather than its inputs, at the cost of distorting the component-level coefficients.

[15] We construct COL from interest paid and received to external actors only (*externa räntekostnader* and *externa ränteintäkter*), in order to isolate the cost of external leverage. All other items between EBIT and profit after financial items (*resultat efter finansiella poster*) are omitted.

[16] *Bokslutsdispositioner* are tax-driven appropriations Swedish legal entities use to smooth taxable income and shift profits between affiliated companies, which sit between profit after financial items and EBT (*resultat före skatt*). For the identity to hold exactly, the untaxed reserves on the balance sheet would need to be allocated between liabilities and equity at the statutory tax rate and the appropriations written back in the income statement.

5.5.5. Listed Firm Indicator

Approximately 0.3 percent of the baseline (1,355 firm-years) are exchange-listed. These firms differ from the predominantly unlisted population on size, age, and reporting regime. We therefore interact PSR with a listed-firm indicator (Tables A8–A9) as a heterogeneity test rather than a causal subgroup analysis. The interaction is statistically insignificant for ROA, OM, ATO, and PM, indicating no detectable differential association by listing status, and decomposing further into regulated-market and MTF firms (Table A9) yields the same insignificant interactions.

5.6. Robustness Tests

These findings are robust across alternative samples, variable definitions, and inference procedures, summarized in Table 10. Table 12 separately addresses selection on observables.

5.6.1. Selection on Observables

Public-sector suppliers differ systematically from non-suppliers on size, age, sales growth, and demand uncertainty (Table 2). Industry-by-year fixed effects absorb the average industry-level component of these differences but not the within-industry, within-year residual imbalance. To assess how much of the full-sample association reflects this observable selection, we re-estimate Equation (22) on (i) a 1:1 nearest-neighbor propensity-score-matched sample and (ii) an entropy-balanced reweighted sample, in both cases targeting balance on log total assets, firm age, log sales growth, financial leverage, and demand uncertainty. The PSM estimator yields a PSR coefficient on ROA of 0.014 (t = 2.67), while entropy balancing yields 0.008 (t = 1.56), positive but insignificant. Both estimators produce OM and ATO magnitudes that are attenuated relative to the full-sample baseline in Table 4. Observable differences in size, age, growth, and demand uncertainty therefore explain part, but not all, of the full-sample association. Because both methods address selection on observables only, unobserved heterogeneity remains a concern, and we present these results as informative rather than as a decisive causal test.

5.6.2. Sample Variation

Extending the sample to include calendar year 2024 leaves the baseline coefficients qualitatively unchanged. Given the data-coverage concerns documented in Section 4.6, the 2024 results are reported as a sensitivity check rather than as a preferred specification.

5.6.3. Industry Exclusions

Sequentially dropping financials (SNI 64–66), real estate (SNI 68), and healthcare (SNI 86–88), both individually and jointly, leaves the main coefficients qualitatively unchanged.

5.6.4. Alternative PSR Construction

Replacing PSR with PSR_excl_kb (computed after excluding invoices from publicly owned companies) leaves the baseline coefficients qualitatively unchanged, indicating that the results are not driven by the inclusion of transactions with state- or municipally-owned enterprises.

5.6.5. Outlier Treatment

Trimming observations beyond the 5th and 95th percentiles of each dependent variable preserves the OM and ATO coefficients close to baseline. The ROA coefficient turns positive and significant (0.022, t = 5.99), reflecting sensitivity to outlier observations. The trimmed ROA coefficient estimate sits between the full-sample estimate and the intensive-margin estimate.

5.6.6. Inference

Two-way (firm × year) and industry-level clustering produce point estimates identical to the baseline by construction.

Table 12. Selection on Observables — Matching

	Panel A: PSM (1:1 Nearest-Neighbor)			Panel B: Entropy Balancing		
Variable	ROA	OM	ATO	ROA	OM	ATO
PSR	0.014***	−0.025***	0.329***	0.008	−0.028***	0.294***
	(2.67)	(−4.14)	(9.61)	(1.56)	(−4.63)	(8.70)
ln(TA)	−0.008***	−0.004***	−0.174***	−0.008***	−0.005***	−0.168***
	(−21.97)	(−8.18)	(−75.58)	(−26.96)	(−9.54)	(−79.02)
AGE	−0.001***	−0.001***	−0.007***	−0.001***	−0.001***	−0.006***
	(−33.43)	(−10.82)	(−31.93)	(−31.77)	(−9.81)	(−28.51)
SALE_GR	0.221***	0.116***	1.224***	0.225***	0.117***	1.244***
	(108.62)	(51.10)	(114.62)	(134.29)	(63.52)	(140.9)
FLEV	−0.002***	−0.001***	0.002***	−0.002***	−0.001***	0.003***
	(−47.26)	(−14.87)	(6.70)	(−51.62)	(−16.01)	(8.12)
Industry + Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	298,257	298,257	298,257	511,208	511,208	511,208
Adj. R^2	0.147	0.07	0.275	0.148	0.07	0.275

, **, * Represent statistical significance based on two-tailed tests at the 0.01, 0.05, and 0.10 levels, respectively. This table addresses selection on observables by re-estimating the baseline H1 specification on matched and reweighted samples. Treatment is defined as having procurement data coverage (i.e., the firm appears in the procurement dataset). Panel A, i.e. propensity score matching (nearest-neighbor, 1:1 without replacement) matches on log total assets, firm age, log sales growth, financial leverage, and firm-level demand uncertainty. Panel B, i.e. entropy balancing, reweighs control observations to exactly match the first moment of the treated group's covariate distribution. Attenuation in coefficient magnitude relative to the unmatched results (Table 4) indicates the share of the raw association attributable to selection on observables. The PSM result for ROA is significant (0.014, t = 2.67) while the entropy-balanced result is not (0.008, t = 1.56), suggesting some sensitivity to the matching method. Both methods attenuate the OM and ATO coefficients relative to Table 4, indicating that a portion of the full-sample association is attributable to selection on observables. All regression models include both (2-digit SNI) industry and year fixed effects, and standard errors are clustered at the firm level. t-statistics are presented in parentheses. Continuous variables are winsorized at their 1st and 99th percentiles. All variables are defined in the Appendix.

5.7. Summary of Main Findings

The full-sample association between PSR and profitability is small and statistically insignificant for ROA (−0.007), negative for margins (OM: −0.047, PM: −0.128), and positive for asset turnover (0.630). These patterns reflect level differences between public-sector suppliers and non-suppliers in firm size, age, industry mix, and asset structure rather than variation in PSR intensity.

Within the intensity margin, higher PSR is associated with significantly higher ROA (0.030) and OM (0.058) and with significantly lower ATO (−0.721). H1 is supported on the intensive margin, and the direction of the PSR–profitability association reverses once the participation margin effects are removed. For an active public-sector supplier, a ten-percentage-point increase in PSR is associated with a 0.31 percentage-point increase in ROA and a 0.58 percentage-point increase in OM, corresponding to 2.1 percent and 7.4 percent of the PSR-

sample mean values of 14.2 percent and 7.8 percent, respectively. The corresponding ATO effect is a decline of roughly 0.072 turnover units, or 3.2 percent of the PSR-sample mean.

H2 finds support in the sense that the PSR–profitability association varies significantly with industry-level demand volatility, but the direction of the moderation is the opposite of what the demand-uncertainty channel would predict. The interaction between PSR and DEM_UNC_IND is negative on ROA (−0.494, t = −3.08) and ATO (−2.273, t = −2.13). The result should be interpreted with caution given the limited within-industry variation in demand volatility over the two-year panel.

H3 is supported. The PSR–profitability association differs significantly across buyer types, with regional PSR (0.040, t = 4.21) and central-government PSR (0.049, t = 3.32) exhibiting larger positive ROA associations per unit than municipal PSR (0.017, t = 2.48). A firm earning its public revenue primarily from regional authorities or national agencies realizes approximately three times the per-unit ROA association of a firm relying primarily on municipal contracts. The pattern is consistent with the institutional differences across levels of government described in Section 3, with the qualification that part of the ranking may reflect firm-size composition across buyer types rather than buyer-type fiscal characteristics in isolation.

H4 finds mixed support. Public-buyer concentration moderates the PSR–profitability relationship, but in opposite directions for margins and turnover. The OM interaction is positive (0.062, t = 3.86), consistent with efficiency gains from concentrated public-buyer relationships, while the ATO interaction is negative (−1.264, t = −12.54), consistent with hold-up risk and dedicated-capacity costs. The two effects largely offset on the net ROA measure.

The mechanism tests in Section 5.5 show that PSR is associated with lower historical demand volatility (Panel A in Table 9), though backward-looking measurement does not separate a stabilization mechanism from selection on demand-volatility history. The investment-efficiency channel (Panel B in Table 9) receives weaker support, as the forward investment-responsiveness interaction is positive but insignificant, while the contemporaneous interaction is significant but more susceptible to reverse causality. The leverage decomposition in Section 5.5.4 confirms that the intensity-margin operating benefit transmits to pre-tax equity returns and is amplified, not eroded, by leverage.

6. Discussion and Implications

This thesis examines the association between public-sector revenue share and supplier profitability in Sweden. The central finding is that this association depends critically on whether one compares across or within the population of public-sector suppliers. In the full sample, the association with ROA is indistinguishable from zero, masking a negative margin effect and a positive asset-turnover effect that offset each other. Restricting to firms that already supply the public sector reverses this picture, with higher PSR associated with significantly higher ROA and operating margin, though at the cost of lower asset turnover. The participation-versus-intensity margin decomposition, showing that who enters the supplier population matters as much as how much they supply, has no direct precedent in the government-customer literature and constitutes the primary empirical contribution of this study. The buyer-type decomposition further documents that the intensity-margin association is not uniform across the public sector, a dimension that the U.S. listed-firm evidence cannot observe.

The positive intensity-margin association is consistent with the demand-uncertainty channel documented by Cohen and Li (2020) in the U.S. setting, and our mechanism test confirms that PSR is negatively associated with firm-level demand volatility. The implication is that, conditional on being a public-sector supplier, deeper engagement with the public sector is associated with more predictable demand, which in turn supports higher profitability. Standard risk-return reasoning predicts that more predictable demand should be priced into bids, compressing margins. The positive intensity-margin association may therefore reflect insufficient competition in certain procurement niches, or operating-efficiency gains that outweigh any bid-price discount, a question future research with contract-level price data could address. Furthermore, in Cohen and Li (2020), higher government customer concentration is associated with higher asset turnover, with the authors interpreting this as enhanced asset utilisation in the presence of major government customers. In our intensity margin specification we find the opposite sign on ATO, where higher PSR is associated with lower asset turnover among firms that already supply the public sector. The margin-side comparison is less informative because Cohen and Li (2020) decompose operating margin into gross margin and SGA components, neither of which is consistently reported by Swedish private firms under the K2 regime, leaving us unable to identify which component drives the positive operating margin association we document.

The buyer-type decomposition reveals that the positive intensity-margin association is not uniform across the public sector. Central government and regional exposure exhibit ROA associations roughly two to three times larger than municipal exposure, a gradient consistent with the institutional differences described in Section 3.3. This finding carries practical implications. A firm deriving more of its public-sector revenue from central government agencies may be in a meaningfully different profitability position than one deriving the same share from municipalities, even if their aggregate PSR is identical. This suggests that the composition of a firm's public-sector revenue, apart from just its level, is associated with profitability outcomes. Neither aggregated PSR measures nor the government customer concentration measure of Falcone et al. (2024) can capture this dimension. The U.S. revenue-

dependence work of Josephson et al. (2019) also cannot observe it, since their reliance on federal procurement databases structurally excludes regional and municipal contracting.

Within the public segment, buyer concentration amplifies the margin benefit of public-sector exposure but also amplifies the asset-turnover penalty, with the two effects largely offsetting on net ROA. This pattern implies that firms concentrating their public-sector sales among fewer buyers may achieve higher margins, possibly through relationship-specific efficiencies or lower tendering costs, but at the expense of tying up more assets per unit of revenue. For practitioners, the H4 result suggests that the strategic choice between a broad and a narrow public-sector customer portfolio is not a straightforward profitability trade-off. Rather, it reshapes how profitability is generated, favoring margins over asset efficiency.

These findings add to the recent supply-chain literature on the shape of the government-exposure–performance relationship. Josephson et al. (2019), using U.S. listed firms, find a positive nonlinear (accelerating) effect of government customer emphasis on firm value. Falcone et al. (2024), using a government customer count-based ratio, find an inverse-U relationship with ROA. Our results sit between these. The intensity-margin slope is positive, consistent with the direction in Josephson et al., but the participation-margin compositional effect is negative, a dynamic that Compustat-based public-firm U.S. samples cannot observe because they lack information on the vast population of small, private non-suppliers. The size heterogeneity we document echoes Falcone et al.'s (2024) firm-size gradient and extends it to a private-firm population in which the smallest firms experience negative ROA effects from public-sector exposure.

7. Limitations and Avenues for Further Research

Several limitations of the current analysis should be noted. The most binding constraint is the two-year panel. With only two calendar years, we cannot include firm fixed effects, cannot track how firms' profitability evolves as their public-sector exposure changes over time, and cannot distinguish persistent effects from transient phenomena.

A second limitation is that we cannot directly correct for self-selection into public procurement. Firms that pursue public contracts may differ on unobservable dimensions from firms that do not. With longer panels, future research could apply two-step selection corrections, two-stage least squares with appropriate instruments, or generalized method of moments (GMM) estimation, as is standard in the recent literature.[17] We do not have a credible instrument for PSR in the Swedish setting, but the procurement-invoice data could in principle be combined with within-firm variation in PSR over a longer panel and an industry-level shock to procurement intensity to construct one.

A longer panel would also allow the PSR–profitability association to be studied across different macroeconomic environments. The baseline period in 2022–2023 was characterized by rising and then declining inflation in Sweden. During this period, the Riksbank policy rate hiked from zero to 4.0 percent between early 2022 and September 2023 (Statistics Sweden, 2026; Sveriges Riksbank, 2026). Goldman (2020) documents that the stabilizing benefits of government customers are particularly pronounced during periods of macroeconomic stress, estimating his key magnitudes off the global financial crisis. Whether the positive intensity-margin association documented here persists, strengthens, or attenuates through the cycle is therefore an open question that cannot be addressed with the current data. We leave it to future research to provide a more direct replication of Goldman's (2020) setting in the Swedish context as the procurement dataset expands.

The size heterogeneity documented in Table 10 raises a question that our data can identify but not fully resolve. The positive intensity-margin ROA association is concentrated among larger firms, while smaller firms experience negative ROA effects from public-sector exposure, driven by their inability to offset margin compression with asset-turnover gains. Whether this reflects genuine scale-dependent benefits of public procurement relationships, such as the ability to absorb tendering costs or to negotiate payment terms that reduce working-capital requirements, or whether larger firms simply self-select into more favorable contract types, is a question that contract-level data could help resolve. Linking invoice records to the publicly available contract award data in TED (European Union, 2026)[18] or other national contract award data would allow researchers to condition on contract characteristics directly, which could help to disentangle the scale-dependence and self-selection channels behind the size heterogeneity.

[17] See, for example, previous studies employing two-stage least squares and instrumental variable approaches (Cohen and Li, 2016; Dhaliwal et al, 2016; Campello and Gao, 2017; Falcone et al, 2024; Chen et al, 2025), generalized method of moments (Bloom et al, 2007; Josephson et al, 2019; Falcone et al, 2023), and two-step selection corrections (Josephson et al, 2019) to address endogeneity and selection bias in the literature.

[18] Tenders Electronic Daily, TED, contains all active tender notices published as a supplement to the official journal of the EU.

The negative interaction documented under H2, indicating that the PSR–profitability association is attenuated rather than amplified in volatile industries, remains an open question. Our industry-level demand uncertainty measure aggregates public and private demand shocks, so it may not isolate the private-sector volatility against which public-sector revenue is theoretically most valuable. An alternative test would construct industry demand volatility from private-sector sales only, excluding public-procurement-driven revenue.

Beyond accounting profitability, public-sector exposure plausibly matters for other corporate outcomes. One question is how the market values public-sector relationships when they change hands in M&A transactions. Dong et al. (2021) find that U.S. acquirers experience better announcement returns when public targets have major government rather than corporate customers. Whether this lower-risk revenue base is similarly valued in private-market M&A transactions, where information asymmetry is higher, remains an open question.

Public-sector exposure may also shape supplier innovation, though the direction is not obvious. Predictable cash flows from high PSR reduce financing frictions and lower the opportunity cost of R&D spending, but public procurement often rewards compliance with detailed specifications rather than differentiation, which may dampen the incentive to innovate. Chen et al. (2025) document a related trade-off in a general customer-concentration setting, showing that customer concentration drives firms to increase innovation volume at the expense of innovation radicalness. Whether the composition of the customer base, specifically, the share and type of public-sector buyers, exerts an independent effect is a question our data could address, at least for the subset of firms that report R&D or intangible investment.

A final direction concerns the broader information environment of supplier firms. Our finding that public-sector suppliers differ systematically from non-suppliers on observable dimensions such as size, age, and industry composition raises the question of whether public procurement participation also affects the quality of information available to outside stakeholders. Badertscher et al. (2013) show that the presence of publicly listed firms in an industry enriches the information environment and facilitates more efficient private-firm investment. An analogous question in our setting is whether the transparency inherent in public procurement, where contract awards, invoice volumes, and buyer identities are observable, spills over into how private suppliers report, disclose, or are monitored. The procurement dataset, which makes the public-sector side of supplier relationships visible at a granularity unusual for private firms, offers a natural setting for that kind of analysis.

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9. Appendix

Table A1. PSR Distribution

Conditional on Supplying the Public Sector

	2022			2023		
PSR	N	Mean ROA	Mean Sales	N	Mean ROA	Mean Sales
0.00 - 0.01	28,679	0.145	48,872	27,595	0.126	50,794
0.01 - 0.05	14,288	0.153	30,067	12,993	0.127	34,033
0.05 - 0.10	5,232	0.154	34,555	4,583	0.132	37,053
0.10 - 0.25	5,805	0.154	35,969	5,334	0.137	35,769
0.25 - 0.50	3,738	0.174	33,552	3,536	0.155	35,420
0.50 - 1.00	3,778	0.194	26,848	2,871	0.175	27,427

This table reports the distribution of public-sector revenue share (PSR) among firm-years with positive public-sector invoices (N = 118,432). Note the regression sample in Table 5 Panel B is 117,794 after removing fixed-effect singletons. The largest share of PSR > 0 firm-years (47%) fall in the 0.00–0.01 range, indicating that most public-sector suppliers derive only a small fraction of revenue from the public sector. Mean ROA increases monotonically with PSR intensity, from 0.145 (lowest bucket) to 0.194 (highest bucket) in 2022, previewing the positive intensity-margin association documented in Table 5. Mean Sales (SEK thousands) are reported for reference. An additional 31,362 firm-years are covered in the procurement dataset at the firm level but report zero invoices in the observation year; these are excluded from this table but included in the PSR Sample in Table 2. All continuous variables are winsorized at their 1st and 99th percentiles.

Table A2. Sector Distribution

By SNI Section, Year, and Public-Sector Supplier Status

	2022						2023					
	Full Sample		PSR Sample		Non-PSR Sample		Full Sample		PSR Sample		Non-PSR Sample	
SNI Section	N	% Share	N	% Share	N	% Share	N	% Share	N	% Share	N	% Share
A - Agriculture, forestry, fishing	6,598	2.6%	1,607	2.2%	4,991	2.8%	6,796	2.6%	1,679	2.2%	5,117	2.8%
B - Mining and quarrying	216	0.1%	85	0.1%	131	0.1%	214	0.1%	87	0.1%	127	0.1%
C - Manufacturing	15,367	6.1%	6,420	8.6%	8,947	5.0%	15,346	5.9%	6,378	8.5%	8,968	4.9%
D - Electricity, gas, steam	1,019	0.4%	294	0.4%	725	0.4%	1,009	0.4%	288	0.4%	721	0.4%
E - Water supply, waste mgmt	577	0.2%	305	0.4%	272	0.2%	585	0.2%	309	0.4%	276	0.2%
F - Construction	32,257	12.7%	8,068	10.9%	24,189	13.5%	32,944	12.7%	8,096	10.7%	24,848	13.5%
G - Wholesale and retail trade	33,766	13.3%	14,462	19.5%	19,304	10.8%	33,951	13.1%	14,411	19.1%	19,540	10.6%
H - Transportation and storage	9,555	3.8%	2,246	3.0%	7,309	4.1%	9,590	3.7%	2,203	2.9%	7,387	4.0%
I - Accommodation and food	9,912	3.9%	5,335	7.2%	4,577	2.6%	10,213	3.9%	5,552	7.4%	4,661	2.5%
J - Publishing, broadcasting	4,673	1.9%	1,392	1.9%	3,281	1.8%	4,900	1.9%	1,401	1.9%	3,499	1.9%
K - Telecom, IT services	12,165	4.8%	2,227	3.0%	9,938	5.6%	12,714	4.9%	2,212	2.9%	10,502	5.7%
L - Financial and insurance	6,102	2.4%	1,109	1.5%	4,993	2.8%	6,006	2.3%	1,138	1.5%	4,868	2.6%
M - Real estate	26,024	10.3%	4,353	5.9%	21,671	12.1%	26,859	10.3%	4,523	6.0%	22,336	12.1%
N - Professional, scientific	49,034	19.4%	10,377	14.0%	38,657	21.6%	50,501	19.5%	10,639	14.1%	39,862	21.6%
O - Admin and support services	12,943	5.1%	3,813	5.1%	9,130	5.1%	13,417	5.2%	3,823	5.1%	9,594	5.2%
P - Public administration	26	0.0%	13	0.0%	13	0.0%	30	0.0%	12	0.0%	18	0.0%
Q - Education	5,557	2.2%	2,485	3.3%	3,072	1.7%	5,858	2.3%	2,620	3.5%	3,238	1.8%
R - Health and social work	8,701	3.4%	3,205	4.3%	5,496	3.1%	9,043	3.5%	3,308	4.4%	5,735	3.1%
S - Arts, sports, recreation	7,250	2.9%	2,699	3.6%	4,551	2.5%	7,746	3.0%	2,914	3.9%	4,832	2.6%
T - Other service activities	10,516	4.2%	3,434	4.6%	7,082	4.0%	11,050	4.3%	3,498	4.6%	7,552	4.1%
U - Households as employers	1	-	n.a.	n.a.	1	-	2	-	1	-	1	-
Missing	933	0.4%	376	0.5%	557	0.3%	911	0.4%	397	0.5%	514	0.3%

This table reports the industry composition of the baseline sample by SNI section (the highest level of the Swedish Standard Industrial Classification), separately for each calendar year and sample split. The PSR Sample comprises procurement data-covered firm-years; the Non-PSR Sample comprises non-covered firm-years. The three largest sectors by observation count are Professional/Scientific (N, 19%), Wholesale/Retail trade (G, 13%), and Construction (F, 13%). PSR firms are over-represented in Wholesale/Retail (19% vs 11%), Accommodation/Food (7% vs 3%), and Manufacturing (9% vs 5%), and under-represented in Professional/Scientific (14% vs 22%) and Real Estate (6% vs 12%). These compositional differences motivate the use of industry fixed effects in the regression specifications.

Table A3. Sales Decile Distribution

By Sales Decile, Year, and Public-Sector Supplier Status

		2022						2023					
		Full Sample		PSR Sample		Non-PSR Sample		Full Sample		PSR Sample		Non-PSR Sample	
Decile	Sales	N	% Share	N	% Share	N	% Share	N	% Share	N	% Share	N	% Share
1	272 - 511	24,954	9.9%	2,880	3.9%	22,074	12.3%	26,334	10.1%	3,239	4.3%	23,095	12.5%
2	511 - 837	25,185	10.0%	3,270	4.4%	21,915	12.3%	26,103	10.1%	3,582	4.8%	22,521	12.2%
3	837 - 1,218	25,173	9.9%	3,703	5.0%	21,470	12.0%	26,115	10.1%	3,936	5.2%	22,179	12.0%
4	1,218 - 1,682	25,067	9.9%	4,022	5.4%	21,045	11.8%	26,221	10.1%	4,218	5.6%	22,003	12.0%
5	1,682 - 2,388	25,088	9.9%	4,922	6.6%	20,166	11.3%	26,200	10.1%	5,040	6.7%	21,160	11.5%
6	2,388 - 3,658	25,206	10.0%	6,549	8.8%	18,657	10.4%	26,082	10.0%	6,641	8.8%	19,441	10.6%
7	3,658 - 6,102	25,474	10.1%	8,709	11.7%	16,765	9.4%	25,814	9.9%	8,668	11.5%	17,146	9.3%
8	6,102 - 11,314	25,671	10.1%	11,001	14.8%	14,670	8.2%	25,616	9.9%	11,090	14.7%	14,526	7.9%
9	11,314 - 28,391	25,661	10.1%	13,283	17.9%	12,378	6.9%	25,626	9.9%	13,216	17.5%	12,410	6.7%
10	28,391 - 489,705	25,713	10.2%	15,966	21.5%	9,747	5.5%	25,574	9.9%	15,859	21.0%	9,715	5.3%

This table reports the distribution of firm-years across sales deciles (defined on the pooled full sample), separately by year and sample split. Sales boundaries are in SEK thousands. PSR firms are concentrated in the upper deciles: 21% of PSR firms fall in decile 10 (sales above 28,391 SEK thousands) compared to 5% of Non-PSR firms. Conversely, only 4% of PSR firms are in the lowest decile versus 12% of Non-PSR firms. This size gradient motivates the firm-size subsample analysis in Table 10.

Table A4. Firm Age Distribution

By Age Bucket, Year, and Public-Sector Supplier Status

	2022						2023					
	Full Sample		PSR Sample		Non-PSR Sample		Full Sample		PSR Sample		Non-PSR Sample	
Age	N	% Share	N	% Share	N	% Share	N	% Share	N	% Share	N	% Share
3-5	38,688	15.3%	7,679	10.3%	31,009	17.3%	38,690	14.9%	7,451	9.9%	31,239	17.0%
6-10	72,354	28.6%	17,366	23.4%	54,988	30.7%	73,233	28.2%	17,007	22.5%	56,226	30.5%
11-20	73,113	28.9%	22,346	30.1%	50,767	28.4%	78,321	30.2%	23,674	31.4%	54,647	29.7%
21-30	30,814	12.2%	11,551	15.6%	19,263	10.8%	30,702	11.8%	11,617	15.4%	19,085	10.4%
31-50	29,832	11.8%	11,417	15.4%	18,415	10.3%	29,719	11.4%	11,483	15.2%	18,236	9.9%
50+	8,391	3.3%	3,946	5.3%	4,445	2.5%	9,020	3.5%	4,257	5.6%	4,763	2.6%

This table reports the distribution of firm-years across age buckets, separately by year and sample split. Age is measured in years since incorporation. PSR firms skew older: 36% are aged 21 or above, compared to 24% of Non-PSR firms. Only 10% of PSR firms are in the youngest bracket (3–5 years) versus 17% of Non-PSR firms. Firm age is included as a control variable in all regression specifications.

Table A5. Employee Distribution

By Employee Count Bracket, Year, and Public-Sector Supplier Status

	2022						2023					
	Full Sample		PSR Sample		Non-PSR Sample		Full Sample		PSR Sample		Non-PSR Sample	
Employees	N	% Share	N	% Share	N	% Share	N	% Share	N	% Share	N	% Share
0	49,981	19.7%	7,075	9.5%	42,906	24.0%	53,782	20.7%	7,883	10.4%	45,899	24.9%
1	81,272	32.1%	13,288	17.9%	67,984	38.0%	84,943	32.7%	14,181	18.8%	70,762	38.4%
2-5	71,223	28.1%	24,015	32.3%	47,208	26.4%	70,883	27.3%	23,812	31.5%	47,071	25.6%
6-10	22,155	8.8%	11,522	15.5%	10,633	5.9%	22,012	8.5%	11,467	15.2%	10,545	5.7%
11-20	13,702	5.4%	8,214	11.1%	5,488	3.1%	13,349	5.1%	8,029	10.6%	5,320	2.9%
21-50	8,685	3.4%	5,780	7.8%	2,905	1.6%	8,556	3.3%	5,665	7.5%	2,891	1.6%
51-100	2,912	1.2%	2,036	2.7%	876	0.5%	2,856	1.1%	2,024	2.7%	832	0.5%
101-500	3,262	1.3%	2,375	3.2%	887	0.5%	3,304	1.3%	2,428	3.2%	876	0.5%

This table reports the distribution of firm-years by number of employees, separately by year and sample split. PSR firms are larger in employment: 32% have 2–5 employees (the modal bracket for PSR firms) compared to 26% of Non-PSR firms, and 3.2% have 101–500 employees compared to 0.5%. Conversely, 38% of Non-PSR firms report exactly one employee versus 18% of PSR firms. The 20% of full-sample observations reporting zero employees reflects owner-operated firms without salaried staff.

Table A6. County Distribution

By Swedish County (Län), Year, and Public-Sector Supplier Status

	2022						2023					
	Full Sample		PSR Sample		Non-PSR Sample		Full Sample		PSR Sample		Non-PSR Sample	
County	N	% Share	N	% Share	N	% Share	N	% Share	N	% Share	N	% Share
Blekinge County	2,580	1%	1,083	1%	1,497	1%	2,665	1%	1,107	1%	1,558	1%
Dalarnas County	6,394	3%	2,295	3%	4,099	2%	6,626	3%	2,360	3%	4,266	2%
Gotlands County	1,683	1%	584	1%	1,099	1%	1,715	1%	594	1%	1,121	1%
Gävleborgs County	5,587	2%	1,998	3%	3,589	2%	5,711	2%	2,031	3%	3,680	2%
Hallands County	8,434	3%	2,327	3%	6,107	3%	8,629	3%	2,365	3%	6,264	3%
Jämtlands County	3,596	1%	1,396	2%	2,200	1%	3,741	1%	1,430	2%	2,311	1%
Jönköpings County	7,986	3%	3,121	4%	4,865	3%	8,254	3%	3,172	4%	5,082	3%
Kalmar County	4,847	2%	1,931	3%	2,916	2%	4,968	2%	1,963	3%	3,005	2%
Kronobergs County	3,968	2%	1,496	2%	2,472	1%	4,113	2%	1,517	2%	2,596	1%
Norrbottens County	5,301	2%	2,108	3%	3,193	2%	5,498	2%	2,164	3%	3,334	2%
Skåne County	31,975	13%	9,191	12%	22,784	13%	32,620	13%	9,318	12%	23,302	13%
Stockholms County	79,340	31%	16,637	22%	62,703	35%	81,167	31%	16,769	22%	64,398	35%
Södermanlands County	5,738	2%	1,821	2%	3,917	2%	5,906	2%	1,851	2%	4,055	2%
Uppsala County	7,810	3%	2,290	3%	5,520	3%	8,048	3%	2,307	3%	5,741	3%
Värmlands County	5,645	2%	2,192	3%	3,453	2%	5,788	2%	2,219	3%	3,569	2%
Västerbottens County	5,612	2%	2,163	3%	3,449	2%	5,706	2%	2,178	3%	3,528	2%
Västernorrlands County	4,846	2%	1,884	3%	2,962	2%	4,967	2%	1,938	3%	3,029	2%
Västmanlands County	5,479	2%	1,721	2%	3,758	2%	5,684	2%	1,774	2%	3,910	2%
Västra Götalands County	41,371	16%	12,896	17%	28,475	16%	42,443	16%	13,222	18%	29,221	16%
Örebro County	5,772	2%	2,075	3%	3,697	2%	5,932	2%	2,086	3%	3,846	2%
Östergötlands County	9,227	4%	3,095	4%	6,132	3%	9,503	4%	3,123	4%	6,380	3%
Missing	1	-	1	-	n.a.	n.a.	1	-	1	-	n.a.	n.a.

This table reports the geographic distribution of firm-years across Sweden's 21 counties (*län*), separately by year and sample split. Stockholm County accounts for 31% of the full sample but only 22% of the PSR Sample, reflecting the capital's concentration of service firms that are less likely to supply the public sector through formal procurement. Västra Götaland (16–18%) and Skåne (12–13%) are the second and third largest counties in both samples. Geographic variation is not part of the identification strategy and this table is provided for sample documentation.

Table A7. Alternative Variable Definitions

Specification	PSR coef.	t-stat	N	Adj. R^2
Dependent variable: ROE	−0.041***	(−2.96)	511,208	0.071
Control: LIAB/TA for FLEV	0.012**	(2.32)	511,208	0.193
Control: Percentage sales growth	−0.011**	(−1.98)	511,208	0.050
DEM_UNC: 3-year rolling window	−0.137***	(−16.24)	504,196	0.079
DEM_UNC: 7-year rolling window	−0.158***	(−18.27)	511,124	0.087
$INVEST_{t+1}$: TA-scaled (Cohen & Li)	0.013	(1.59)	311,471	0.030
$INVEST_{t+1}$: PPE-only	0.536**	(2.22)	305,604	0.022
Control: ln(Sales) for ln(TA)	−0.021***	(−3.96)	511,208	0.165

***, **, * Represent statistical significance based on two-tailed tests at the 0.01, 0.05, and 0.10 levels, respectively. This table presents the baseline H1 specification under alternative variable definitions to test measurement robustness. Each row reports the PSR coefficient from a separate regression using one substituted variable, with all other variables held at their baseline definitions. All regression models include both (2-digit SNI) industry and year fixed effects, and standard errors are clustered at the firm level. t-statistics are presented in parentheses. Continuous variables are winsorized at their 1st and 99th percentiles. All variables are defined in the Appendix.

Table A8. Listed-Firm Heterogeneity (Pooled)

	ROA		OM		ATO		PM	
Variable	ROA	t-stat	OM	t-stat	ATO	t-stat	PM	t-stat
PSR	−0.008	(−1.56)	−0.050***	(−8.37)	0.630***	(18.48)	−0.131***	(−23.70)
Listed	−0.217***	(−19.81)	−0.354***	(−15.74)	0.136***	(2.98)	−0.451***	(−24.29)
PSR × *Listed*	0.064	(0.52)	0.382	(1.33)	0.484	(0.80)	0.270	(1.290)

***, **, * Represent statistical significance based on two-tailed tests at the 0.01, 0.05, and 0.10 levels, respectively. This table tests whether the association between PSR and firm profitability differs for exchange-listed firms. The coefficient on PSR × Listed captures the differential effect relative to unlisted firms (reference group). Listed firms are identified from the Retriever *Noterade företag* field and include firms on regulated markets (Nasdaq Stockholm Large, Mid, and Small Cap, and NGM Equity) and multilateral trading facilities (First North, Spotlight Stock Market, and NGM Nordic MTF/AIF), for a total of 1,355 listed firm-years (0.3 percent of the baseline). This is a descriptive heterogeneity test, not a causal identification: listed firms differ systematically from unlisted firms on size, age, reporting regime (K3 or IFRS for listed firms versus predominantly K2 for unlisted firms), and growth trajectory, and these differences cannot be separated from listing status within the baseline specification. All regression models include both (2-digit SNI) industry and year fixed effects, and standard errors are clustered at the firm level. The vector of control variables includes log total assets, firm age, log sales growth, and financial leverage (FLEV). t-statistics are presented in parentheses. Continuous variables are winsorized at their 1st and 99th percentiles. All variables are defined in the Appendix.

Table A9. Listed-Firm Heterogeneity (Decomposed)

	ROA		OM		ATO		PM	
Variable	ROA	t-stat	OM	t-stat	ATO	t-stat	PM	t-stat
PSR	−0.008	(−1.53)	−0.049***	(−8.31)	0.630***	(18.50)	−0.131***	(−23.66)
PSR × *Regulated*	−0.180	(−1.18)	0.192	(0.620)	−0.364	(−0.48)	0.022	(0.110)
PSR × *MTF*	0.220	(1.270)	0.413	(0.960)	1.076	(1.16)	0.402	(1.190)
Regulated	−0.083***	(−7.49)	−0.113***	(−5.01)	0.517***	(8.87)	−0.278***	(−14.59)
MTF	−0.340***	(−21.97)	−0.578***	(−17.25)	−0.216***	(−3.44)	−0.612***	(−21.76)

***, **, * Represent statistical significance based on two-tailed tests at the 0.01, 0.05, and 0.10 levels, respectively. This table decomposes the Table A8 listed-firm interaction by exchange tier. Regulated-market firms (659 firm-years) include Nasdaq Stockholm Large, Mid, and Small Cap, and NGM Equity, and are predominantly mature profitable companies. MTF firms (696 firm-years) include First North, Spotlight Stock Market, and NGM Nordic, and are predominantly growth-stage loss-making companies. PSR × Regulated and PSR × MTF capture the incremental PSR effect for each tier relative to unlisted firms (reference group). As in Table A8, this is a descriptive heterogeneity test rather than a causal identification, and the listed-firm subsample is small relative to the unlisted baseline, so interaction estimates are imprecise. All regression models include both (2-digit SNI) industry and year fixed effects, and standard errors are clustered at the firm level. The vector of control variables includes log total assets, firm age, log sales growth, and financial leverage (FLEV). t-statistics are presented in parentheses. Continuous variables are winsorized at their 1st and 99th percentiles. All variables are defined in the Appendix.

Table A10. Variable Definitions

Variables	Definition	Eq.	Usage
Explanatory Variables			
Public-Sector Exposure Level			
PSR	Share of a firm's revenue coming from public-sector buyers in year t.	(1)	Baseline
Buyer-Structure and Concentration			
PSR_HHI	Sum of squared invoice shares across public-sector buyers using total public-sector sales as denominator.	(28)	Additional
CC_PUB	Public-sector customer concentration = $PSR^2 \times PSR_HHI$; within-public concentration scaled by PSR.		Additional
PSR_LG	Largest public buyer's invoice revenue / total public-sector sales in year t.	(29)	Additional
PSR_LG_TOT	Largest public buyer's invoice revenue / total net sales, computed as $PSR_LG \times PSR$.		Additional
N_BUYERS	Number of distinct public-sector buyers with positive public-sector invoices for the firm-year.		Additional
Buyer-Type Composition			
PSR_MUNICIPALITY	Invoice revenue from municipal buyers / total net sales (PSR-covered firms only).		Additional
PSR_REGION	Invoice revenue from regional authorities / total net sales (PSR-covered firms only).		Additional
PSR_GOVERNMENT	Invoice revenue from central government agencies / total net sales (PSR-covered firms only).		Additional
PSR_GOVT_CO	Invoice revenue from publicly owned companies / total net sales (PSR-covered firms only).		Additional
PSR_UNIVERSITY	Invoice revenue from universities / total net sales (PSR-covered firms only).		Additional
PSR_COLLEGE	Invoice revenue from university colleges / total net sales (PSR-covered firms only).		Additional
Dependent Variables			
Profitability and Performance			
ROA	EBIT / beginning-of-period total assets; primary profitability outcome.	(2)	Baseline
OM	EBIT / net sales; operating-margin component in the DuPont decomposition.	(3)	Baseline
ATO	Net sales / beginning-of-period total assets; asset-turnover component in the DuPont decomposition.	(4)	Baseline
PM	Net income / net sales; net profit margin.	(5)	Baseline
ROE	Net income / beginning-of-period book equity; accounting return on equity.	(6)	Additional
Mechanism Outcomes			
DEM_UNC [(a)]	Standard deviation of log sales growth over a backward 5-year window (≥3 valid years).	(9)	Baseline
DEM_UNC_IND [(a)]	Industry-level counterpart of DEM_UNC based on mean log sales growth within 2-digit SNI industry-years.	(10)	Additional

(continued on next page)

INVEST	Capital investment rate based on balance-sheet changes in fixed assets plus depreciation.	(11) (12)	Additional
INVEST $_{t+1}$	Forward investment rate: INVEST in year t+1, attached back to firm-year t where next-year data exist.		Additional
INVEST_TA	Same capex proxy as INVEST / beginning-of-period total assets; total-assets-scaled investment rate.		Robustness
INVEST_PPE	(Δ tangible fixed assets + D&A) / beginning tangible fixed assets; PPE-only investment rate.		Robustness
ROA_VOL [(a)]	Standard deviation of ROA over a backward 5-year window (≥3 valid ROA observations).	(13)	Additional
Leverage and Return Decomposition			
COL	(Interest expenses − interest income) / beginning total liabilities; cost of liabilities.	(16)	Additional
L/E	Beginning total liabilities / beginning book equity; leverage ratio.	(17)	Additional
EFF_TAX	Tax expense / earnings before tax; defined only for firm-years with positive earnings before tax.	(18)	Additional
LEV_SPREAD	ROA − COL; difference between return on assets and cost of liabilities.	(19)	Additional
PRETAX_ROE	Earnings before tax / beginning book equity; pre-tax analogue of ROE.	(20)	Additional
ROE_LEVERAGE	Implied ROE from the Johansson–Runsten leverage identity.	(21)	Additional
Control Variables			
Size, Age, and Growth			
ln(TA)	Log of total assets; firm size control.		Baseline
ln(Sales)	Log of net sales; alternative size control.		Robustness
AGE	Calendar year − formation year, with registration year used if the formation year is missing.		Baseline
SALE_GR	Log sales growth between year t−1 and t.	(8)	Baseline
Capital Structure and Liquidity			
FLEV	Financial leverage (equity multiplier) = beginning total assets / beginning book equity.	(7)	Baseline
CASH/TA	Cash-to-assets ratio.	(14)	Additional
LIAB/TA	Liabilities-to-assets ratio, i.e. total liabilities / total assets.	(15)	Additional

Baseline: denotes variables used in the core H1 and participation–versus–intensity margin regressions and included in all main specifications. Additional: denotes variables central to follow-on analyses (mechanisms, buyer-structure and buyer-type decompositions, leverage and listed-firm heterogeneity) but not part of the baseline specification. Robustness: denotes variables or alternative definitions used only in sensitivity checks, trimming, alternative control sets, or window-length tests. [(a)] DEM_UNC, DEM_UNC_IND and ROA_VOL are based on a 5-year rolling window in the main specifications. Alternative 3-year and 7-year windows are used in robustness analyses and reported where relevant.

AI Disclosure Statement

Generative artificial intelligence tools were used in three narrow and clearly delineated roles in connection with this thesis. First, the AI-assisted literature discovery tool Research Rabbit was employed as a search and organization aid during the ideation phase and in the preparation of the literature review, solely to help identify and structure potentially relevant academic sources. Second, the conversational AI coding assistant Claude was used, at the authors' direction, as a support tool in R programming to propose possible code formulations, highlight potential issues, and suggest refinements to code written by the authors. Third, the AI-powered writing assistant Perplexity was used on an editorial basis to provide suggestions regarding spelling, grammar, sentence structure, and textual clarity in drafts authored by the authors. At no point did any AI system independently determine the research questions, design the methodology, perform the data analysis, or originate the substantive reasoning, interpretations, or conclusions. The thesis's intellectual content, argumentation, and final written text and code remain entirely the work and responsibility of the authors.

Research Rabbit

Research Rabbit is an AI-assisted literature discovery and visualization platform designed to help researchers identify, organize, and explore academic publications related to specified topics or seed articles. It uses algorithmic techniques to expand from an initial set of works to a broader set of potentially relevant publications, highlighting citation and co-citation relationships and depicting connections among articles, authors, and themes in graphical form. By structuring and visualizing these relationships, the tool supplements conventional database searches and facilitates the systematic mapping and navigation of a field's scholarly landscape, while all evaluative and interpretive judgments remain with the users.

Claude

Claude is a conversational AI system developed by Anthropic that can assist with a range of natural-language and programming-related tasks, including reviewing source code, suggesting alternative implementations or modifications, clarifying programming concepts, and indicating potential issues or inconsistencies. The system operates interactively, responding to user prompts, questions, or code excerpts with text-based suggestions and explanations rather than executing or deploying code. It is intended to function as an assistive tool that offers optional lines of inquiry and possible approaches, while all decisions regarding system design, implementation choices, validation, and acceptance or rejection of any suggested content remain with the users.

Perplexity

Perplexity is an AI-based writing and question-answering assistant that uses large language models to provide text-based responses and editorial suggestions in reaction to user-supplied prompts or excerpts. When presented with passages of draft text, it can propose alternative phrasings, identify apparent issues in spelling or grammar, and suggest adjustments to sentence structure and overall clarity, without substituting its own substantive views for those of the users. The tool is designed to support users in refining and polishing their own writing by offering possible revisions and formulations, while all decisions about the substance of the text and which suggestions to adopt, modify, or disregard are made by the users.