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Quoc Lap Nguyen

lapnq344@gmail.com

University of Economics and Law

Research Article

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Own money, open competition? Local revenue autonomy, EU funds and procurement integrity in Polish municipalities

Quoc Lap Nguyen[1,2*]

[1*]University of Economics and Law, Ho Chi Minh City, Vietnam.
[2]Vietnam National University, Ho Chi Minh City, Vietnam.

Corresponding author(s). E-mail(s): lapnq344@gmail.com, lapnqc26602-1-nc@sdh.uel.edu.vn;

Abstract

Fiscal decentralisation is often expected to improve local government because officials who spend money raised from their own residents have stronger reasons to buy at low cost. Whether this logic extends to the way public contracts are awarded has rarely been tested with municipal data. This study documents conditional associations in a panel of about 2,200 Polish municipalities between 2008 and 2019, relating the composition of local revenue and the share of EU-financed capital spending to three procurement integrity indicators: single bidding, non-open procedures and a composite risk index. Two-way fixed-effects estimates do not support the view that revenue autonomy promotes open competition. Associations are small and mixed, close to zero for local taxes, and modestly positive for non-tax own revenue and non-open procedures. EU capital spending is associated with more single bidding, particularly where own revenue is high, although this interaction is exploratory and absent in the later sample. Timing tests, placebo leads, measurement diagnostics and robustness exercises suggest that the results are not artefacts of contract matching, although identification remains observational. Fiscal autonomy alone does not secure competitive procurement, and the source of local money matters.

Keywords: Fiscal decentralisation, Public procurement, Local revenue autonomy, EU funds, Single bidding, Poland

JEL Classification: H72 , H77 , D73 , R50

1 Introduction

In the average Polish municipality-year observed between 2008 and 2019, nearly three in ten public contracts were awarded after a tender that attracted a single bidder, and roughly one in twelve was awarded through a procedure other than open competition. Both patterns are treated in the procurement literature as warning signs, because a market that is not tested leaves little discipline on price and quality (García Lafuente and Mochón Sáez 2022) and gives awarding officials room to steer contracts (Fazekas et al. 2016; Fazekas and Kocsis 2020). Procurement is also where local budgets become roads, schools and water systems. Why some municipalities run more competitive tenders than others is therefore a question about how public money is spent, and about who is in a position to notice when it is spent badly.

Public finance offers a clear expectation. A government that raises its own revenue answers more directly to the residents who pay, and the cost of waste is not shifted to someone else (Tiebout 1956; Oates 1999; Weingast 2009). Evidence from Brazilian municipalities points in the same direction. Governments treat revenue raised through taxation differently from funds received as transfers (Gadenne 2017), citizens condition their willingness to pay taxes on how revenue is used (Timmons and Garfias 2015), and windfall transfers weaken the selection of politicians and raise corruption (Brollo et al. 2013). If these mechanisms operate in procurement, municipalities that finance a larger share of spending from their own resources should award contracts more competitively. The cross-country evidence on decentralisation and corruption is less tidy (Fisman and Gatti 2002; Treisman 2000; Fan et al. 2009; Martinez-Vazquez et al. 2017), and almost no work follows the argument down to the level of individual tenders within a single institutional setting.

Poland is a favourable setting for this question. Its roughly 2,500 municipalities (*gminy*) share one legal framework and one national procurement regime, yet differ widely in how they finance themselves, and they received large EU co-financed investment flows during the period studied (Charasz and Vogler 2021; Banaszewska and Bischoff 2021; Bachtrögler et al. 2019). Earlier work has examined how tax autonomy relates to fiscal discipline and to the composition of local spending in Polish municipalities (Bukowska and Siwińska-Gorzelak 2019; Siwińska-Gorzelak et al. 2020). Whether the same autonomy relates to the integrity of the procurement process has not been established.

This paper builds a municipality-year panel from public procurement tender records, municipal budget data and administrative statistics. The main regression sample contains 19,383 municipality-years for 2,202 municipalities, drawn from 454,319 contracts. Procurement integrity is measured by the share of contracts awarded with a single bidder, the share awarded through non-open procedures, and a composite risk index. The explanatory variables are the share of own-source revenue that excludes shared income-tax receipts, a decomposition of that share into local taxes and non-tax own revenue, and the share of expenditure that is EU-funded capital spending. All regressors enter with a one-year lag in models with municipality and year fixed effects, and standard errors are clustered by municipality. Because several results are small, inference is reported through confidence intervals, minimum detectable effects and equivalence tests, so that the data can say how large an association could be and

not only whether it differs from zero. The design is descriptive. It documents conditional associations within municipalities and bounds their size, but it does not identify causal effects, and Section 5 states which confounders remain.

The evidence does not support the simple version of the fiscal-autonomy hypothesis. A ten-percentage-point higher own-source revenue share is associated with 0.59 percentage points more non-open procurement and no detectable change in single bidding. The association is concentrated in non-tax own revenue, while local taxes and fees show no association with any outcome and are bounded within one tenth of a standard deviation. The positive link operates through the intensive margin, since municipalities that already use non-open procedures rely on them more. Timing matters as well: the association with single bidding is negative within the year and turns positive when the regressor is lagged by two years, while the lead placebo is null. EU capital spending is associated with more single bidding on average, and this association is stronger where own-source revenue is high. The interaction was found in exploratory threshold analysis and is reported as such: it survives leave-one-voivodeship-out and split-sample checks and is not driven by weakly matched contracts, but it is absent in 2014 to 2019.

The paper makes four contributions. First, it provides, to the author's knowledge, the most extensive municipal evidence on whether local revenue autonomy relates to procurement integrity, using nearly the full universe of Polish municipalities and more than 450,000 contracts. It extends the decentralisation and corruption literature from country comparisons (Fisman and Gatti 2002; Treisman 2000) to tender-level outcomes measured within one legal system. Second, it indicates that the composition of own revenue matters: the average autonomy association appears to mask a contrast between local taxes and non-tax receipts, in line with the idea that the source of finance shapes accountability (Gadenne 2017; Brollo et al. 2013). Third, it adds to the literature on EU funds and local governance (Becker et al. 2010; Rodríguez-Pose and Garcilazo 2015; Charasz and Vogler 2021) by relating EU-financed capital spending to procurement outcomes and showing how that relation depends on local fiscal structure. Fourth, it offers a template for credible reporting under uncertainty: the size of each association is bounded with equivalence tests, an exploratory finding is separated from pre-specified tests, and measurement validity of administrative procurement data is examined directly, a concern that applies to most studies built on matched tender records (Fazekas et al. 2016; Fazekas and Kocsis 2020).

The remainder of the paper proceeds as follows. Section 2 develops the conceptual background and hypotheses. Section 3 describes the institutional setting. Section 4 presents the data and measurement. Section 5 sets out the empirical strategy. Section 6 reports the results. Section 7 discusses their interpretation, Section 8 states limitations, and Section 9 concludes.

2 Conceptual background and hypotheses

2.1 Revenue autonomy and accountability

The fiscal federalism tradition places the link between who raises revenue and who spends it at the centre of local government performance. Decentralised provision

can match services to local preferences and discipline governments through mobility (Tiebout 1956; Oates 1999). Second-generation theories add that these gains materialise only when local officials face hard budget constraints and bear a political cost for raising revenue (Weingast 2009). Where municipalities live off transfers, the cost of waste falls on taxpayers elsewhere, and local voters have less reason to monitor.

Empirical work supports a link between the source of finance and the quality of spending. In Brazilian municipalities, governments respond differently to revenue raised through taxes than to funds received as transfers (Gadenne 2017), and property tax collection falls where audits reveal corruption, which suggests that citizens condition their contributions on how public money is used (Timmons and Garfias 2015). Windfall transfers, by contrast, lower the quality of politicians and increase corruption (Brollo et al. 2013). Audits and electoral sanctions also change behaviour, and voters punish revealed misconduct (Ferraz and Finan 2008, 2011), but monitoring by itself is not always effective (Olken 2007). Cross-country studies relate expenditure decentralisation to lower perceived corruption (Fisman and Gatti 2002), whereas federal structures and a larger number of government tiers are associated with more corruption in other samples (Treisman 2000; Fan et al. 2009). The effect of decentralisation on corruption appears to unfold over time (Fiorino et al. 2015), and relationships between governance spending and corruption can be nonlinear (Guerrero and Castañeda 2021). Surveys conclude that the overall evidence on decentralisation is far from uniform (Martinez-Vazquez et al. 2017). A structural argument also cuts the other way, since decentralised officials may extract bribes in a less coordinated and more distortionary way (Shleifer and Vishny 1993).

These considerations lead to a first, accountability-based expectation.

H1 (fiscal link). A higher share of own-source revenue is associated with lower procurement integrity risk, so that single bidding and non-open procedures are less frequent.

2.2 The composition of own revenue

Own-source revenue is not homogeneous. Local taxes and fees are plausibly visible to many residents, are paid regularly, and are collected under rules that officials cannot easily redirect. Non-tax receipts, such as income from property, rents and asset sales, are plausibly lumpier, less visible to voters, and tied to assets that local officials control. The accountability logic is strongest for revenue that voters recognise as their own contribution (Timmons and Garfias 2015; Gadenne 2017) and weakest for resource-like income, which behaves more like a windfall (Brollo et al. 2013). In Poland, tax autonomy has been linked to local fiscal discipline (Bukowska and Siwińska-Gorzelak 2019), and revenue autonomy shapes the composition of local spending (Siwińska-Gorzelak et al. 2020). A natural extension is that the two components of own revenue relate differently to procurement integrity.

H2 (composition). The association with procurement integrity risk is more negative for local taxes and fees than for non-tax own revenue.

2.3 Procurement integrity indicators

Procurement data permit objective, large-scale measures of integrity risk. Single bidding, the award of a contract in a tender that drew only one offer, is the most widely used indicator, because it signals that competition was absent or foreclosed (Fazekas et al. 2016; Fazekas and Kocsis 2020). Administrative data have been used to relate such indicators to the meritocracy of bureaucracies (Charron et al. 2017) and to the use of EU funds (Fazekas and King 2019), and related work links corruption in procurement to the composition of public budgets (Hessami 2014). A second family of indicators concerns the procedure used. Rules on award mechanisms, thresholds and procedure types constrain the discretion of buyers, and changes in them are associated with differences in who wins and at what price (Decarolis 2014; Palguta and Pertold 2017; Coviello et al. 2018; Baltrunaite et al. 2021; Szucs 2024). Publicity requirements widen participation (Coviello and Mariniello 2014), and electronic systems can raise the quality of the infrastructure that is procured (Lewis-Faupel et al. 2016). Formal procurement law and actual practice do not always coincide (Bosio et al. 2022), and much of the waste in public purchasing arises from limited capacity as well as from rent-seeking (Bandiera et al. 2009).

Local politics shapes procurement as well. Longer tenure of local executives is associated with fewer bidders and higher prices (Coviello and Gagliarducci 2017), corruption in procurement displays a political cycle (Mironov and Zhuravskaya 2016), and political contributions relate to the allocation of contracts (Baltrunaite 2020). Project complexity is related to both bidder participation and corruption risk (Baldi et al. 2016), and theoretical work shows that corruption and incompetence in the screening of bids can interact in non-obvious ways (Dastidar and Jain 2023). Taken together, this literature supports the use of single bidding and non-open procedure shares as complementary indicators, one capturing the outcome of competition and the other the discretion exercised before bids arrive.

2.4 EU funds

EU-financed capital spending arrives with its own rules, monitoring and reporting, and may therefore discipline local buyers. It also relaxes the local budget constraint with money that residents did not raise, and it usually finances large and specialised projects. Research on cohesion policy shows that effects on regional performance vary (Becker et al. 2010) and that returns depend on absorptive capacity (Becker et al. 2013) and on the quality of government (Rodríguez-Pose and Garcilazo 2015), that quality of government differs sharply across European regions (Charron et al. 2014), and that administrative capacity and political governance jointly shape the use of funds in new member states (Surubaru 2017). For Central and Eastern Europe, the integrity of EU-funded contracts depends on the interaction between external rules and domestic institutions (Fazekas and King 2019). In Polish municipalities, EU funding has been linked to local state capacity and to electoral outcomes (Charasz and Vogler 2021; Banaszewska and Bischoff 2021). Because project complexity is related to participation and corruption risk (Baldi et al. 2016), the net association between EU-financed spending and competition is ambiguous.

H3 (EU funds). The association between EU capital spending and procurement integrity risk is not signed a priori, because external rules may discipline buyers while project scale and complexity may reduce competition.

Money from different sources is not equally accountable (Gadenne 2017; Brollo et al. 2013), which raises the possibility that EU funds and own revenue interact in shaping local procurement. No specific direction was pre-specified. The interaction between own-source revenue and EU capital spending emerged during exploratory threshold analysis and is treated as an exploratory hypothesis (H4) whose estimates are interpreted with the caution that flexible analysis requires (Simmons et al. 2011).

H4 (exploratory interaction). The association between EU capital spending and procurement integrity risk varies with the own-source revenue share.

3 Institutional setting

The municipality (*gmina*) is the basic tier of local government in Poland, and each municipality finances its activities from several sources: receipts that it sets and collects itself, shares of nationally collected personal and corporate income taxes, transfers from the central budget and, for investment, EU co-financing (Bukowska and Siwińska-Gorzelak 2019; Siwińska-Gorzelak et al. 2020; Charasz and Vogler 2021). Shared income-tax receipts are determined by national rules and do not reflect local policy choices (Bukowska and Siwińska-Gorzelak 2019), which is why this study measures autonomy by own-source revenue that excludes them. Within own-source revenue, local taxes and fees are distinguished from non-tax receipts such as income from property, and Section 4 shows that the two components differ in size and in within-municipality variation.

Mayors are directly elected in Polish municipalities, and incumbents hold a substantial electoral advantage (Bartnicki et al. 2022). EU funds have been shown to matter for mayoral elections (Banaszewska and Bischoff 2021) and for the capacity of municipal administrations (Charasz and Vogler 2021). These features justify examining electoral competition as a moderator of the main relationships, which is done in the appendix.

Public procurement by municipalities follows national legislation within the EU framework. In such regimes, open tendering is the standard competitive route, other procedures are permitted under defined conditions, and thresholds determine which rules apply (Bosio et al. 2022; Palguta and Pertold 2017; Coviello and Mariniello 2014). Buyers retain some discretion over procedure choice, and the use of that discretion is the target of the non-open procedure indicator. For the purposes of this study, what matters is that all municipalities operate under the same legal regime during the sample period, so that differences in outcomes are not driven by national rule changes that differ across units, and that year effects absorb national changes over time.

4 Data and measurement

4.1 Procurement outcomes

Procurement outcomes are constructed from national public-procurement tender records for Poland, taken from the Global Public Procurement Dataset, which compiles contract-level information on bids and procedures. The estimation window runs from 2008 to 2019. Contracts are assigned to municipalities through the buyer's REGON identification number whenever it is available and through the buyer's name otherwise. In total, 454,319 contracts are used, of which 53.0 percent are matched through REGON (Table 2). The REGON share rises from 47.0 percent in 2008 to 60.2 percent in 2015 and falls to roughly 32 percent in 2018 and 2019, a pattern that motivates the measurement checks reported below. Procurement-based indicators of this kind depend on the completeness and consistency of administrative records (Fazekas and Kocsis 2020).

Three outcomes are analysed. The single-bidding share is the percentage of a municipality's contracts in a year that were awarded with exactly one bid, among contracts with bid-count information. The non-open procedure share is the percentage of contracts awarded through a procedure other than the open procedure, among contracts with procedure information. A municipality-year enters an outcome only if at least five contracts carry the relevant information, which yields 19,435 observations for single bidding, 16,783 for non-open procedures and 16,760 for the risk index (Table A1). The risk index is the mean of the standardised single-bidding and non-open shares and requires both components. A third candidate component, contracts awarded without a call for tender, was excluded because it hardly varies (mean 0.56 percent); a three-component index is reported as a robustness check. Following the logic of earlier work that treats single bidding and procedure restrictions as warning signs of reduced competition (Fazekas et al. 2016; Coviello et al. 2018), higher values of all three outcomes indicate greater integrity risk.

Table 1 summarises the variables. Single bidding averages 28.8 percent of contracts per municipality-year (standard deviation 19.3) and the non-open share averages 8.2 percent (13.4). Coverage expands from 886 municipality-years in 2008 to between 1,900 and 2,050 in each year from 2011 to 2014 and contracts afterwards to 1,250 in 2016 (Table 2). Figure 1 shows that the mean single-bidding share starts at 39.8 percent in 2008, when coverage is thin, falls to 27.0 percent in 2009, reaches 32.8 percent in 2011, declines to 20.6 percent in 2016 and rebounds to 37.2 percent in 2018. The non-open share declines from 11.3 percent in 2008 to 5.4 percent in 2017 and then recovers to 12.9 percent in 2019. Year fixed effects absorb these national movements in all specifications.

4.2 Fiscal variables

Revenue autonomy is measured as own-source revenue excluding shared personal and corporate income-tax receipts, divided by total revenue. The measure is decomposed into local taxes and fees, and a residual of non-tax own revenue. EU involvement is

Table 1 Summary statistics, Polish municipalities, 2008-2019

	N	Mean	SD	P5	Median	P95	Within SD
A. Procurement outcomes (municipality-years with at least 5 contracts carrying the relevant information)							
Single-bidding share (%)	19,435	28.80	19.34	0.00	26.21	65.31	15.85
Non-open procedure share (%)	16,783	8.19	13.38	0.00	0.00	35.00	9.99
Risk index (mean z-score of the two shares)	16,760	−0.020	0.819	−1.051	−0.189	1.484	0.625
Contracts per municipality-year (all municipality-years)	29,739	15.3	29.5	0.0	8.0	55.0	18.1
B. Fiscal and socio-economic variables (main regression sample, single-bidding outcome)							
Own-source revenue excl. PIT/CIT, % of revenue	19,383	26.70	10.79	11.80	25.26	46.74	3.82
of which local taxes and fees, % of revenue	19,383	14.49	6.89	5.55	13.30	27.72	2.00
of which non-tax own revenue, % of revenue	19,383	12.16	6.44	4.40	10.76	24.73	3.64
Own-source revenue incl. PIT/CIT, % of revenue	19,383	41.04	15.19	19.14	39.17	67.88	3.96
EU-funded capital expenditure, % of expenditure	19,383	6.63	7.71	0.00	3.88	23.50	6.03
Population	19,383	16,837	55,157	3,623	8,606	43,919	982
Population density (persons per km^2)	19,383	250.9	498.0	26.0	72.0	1,447.0	20.3
Unemployed, % of working-age population	19,383	7.98	3.83	2.62	7.53	14.92	0.97
Real revenue per capita (2015 PLN)	19,383	3,622	1,493	2,475	3,341	5,410	447
Debt, % of revenue	19,383	26.87	16.00	1.85	25.86	54.83	7.86
Budget balance, % of revenue	19,383	−2.48	8.55	−18.89	−1.20	9.37	7.13
C. Electoral competition (next local election)							
Incumbent unopposed (0/1)	18,171	0.112	0.315	0.000	0.000	1.000	0.224
Number of candidates (capped at 6)	18,171	3.19	1.53	1.00	3.00	6.00	0.98

Notes: Unit of observation: municipality-year. Within SD: standard deviation after removing municipality and year fixed effects. Fiscal variables are winsorised at the 1st and 99th percentiles. Panel B and C statistics refer to the main regression sample (non-missing single-bidding share and regressors); regressors enter the models lagged by one year.

Table 2 Procurement data coverage and contract-to-municipality matching, by year

	Contracts	Matched via REGON (%)	Municipality-years in sample (≥5 contracts)	Share of municipalities (%)	Contracts per municipality	Single bidding (%)	Non-open (%)
2008	13,615	47.0	886	35.8	5.5	39.8	11.3
2009	41,978	47.1	1,749	70.6	16.9	27.0	9.3
2010	49,582	50.9	1,891	76.3	20.0	29.6	10.0
2011	58,522	53.7	1,907	76.9	23.6	32.8	8.6
2012	60,417	56.2	1,959	79.0	24.4	27.3	7.4
2013	55,856	57.2	2,036	82.1	22.5	26.9	7.8
2014	51,543	59.7	1,987	80.2	20.8	25.6	7.3
2015	34,480	60.2	1,704	68.8	13.9	24.6	8.2
2016	23,081	59.2	1,250	50.4	9.3	20.6	6.9
2017	29,223	52.3	1,590	64.2	11.8	30.2	5.4
2018	18,666	31.5	1,261	50.9	7.5	37.2	10.7
2019	17,356	31.8	1,215	49.1	7.0	30.6	12.9
All years	454,319	53.0	19,435	65.4	15.3	28.8	8.2

Notes: Contracts are Polish public-procurement tender records (Global Public Procurement Dataset) assigned to municipalities by the buyer's REGON number where available and by buyer name otherwise. Share of municipalities = municipality-years with at least 5 contracts with single-bidding information divided by all municipalities in the panel that year.

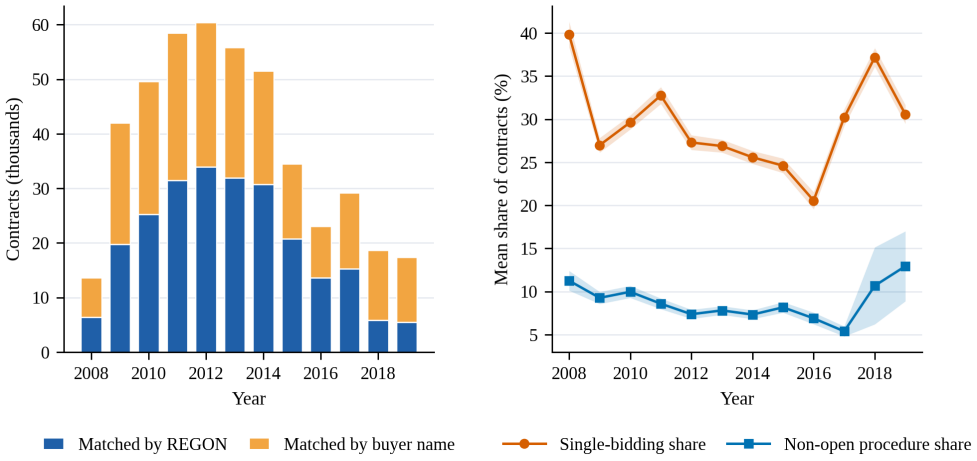


Fig. 1 Procurement data coverage and mean integrity indicators by year. Left panel: number of contracts per year, split by matching method (REGON identifier or buyer name). Right panel: annual mean single-bidding and non-open procedure shares across municipality-years with at least five contracts carrying the relevant information, 2008 to 2019. Year effects absorb these national movements in all regressions.

measured by capital expenditure financed with EU funds as a share of total expenditure. Fiscal ratios are winsorised at the 1st and 99th percentiles and expressed in units of ten percentage points so that coefficients refer to a change of ten points in a share. All regressors are lagged one year in the baseline.

The descriptive statistics indicate where identifying variation comes from. Own-source revenue averages 26.7 percent of revenue, but its within-municipality standard deviation is only 3.8 points against a total of 10.8 (Table 1). A ten-point change therefore represents about 2.6 within-municipality standard deviations, and the estimates below should be scaled accordingly when judged against realistic changes. Local taxes and fees account for 14.5 percent of revenue with a within standard deviation of 2.0, and non-tax own revenue for 12.2 percent with 3.6, which shows that most of the time variation in the autonomy share is generated by the non-tax component. EU-funded capital spending averages 6.6 percent of expenditure and varies mostly over time within municipalities (within standard deviation 6.0 of 7.7).

Controls are the logarithms of population, population density and real revenue per capita (2015 prices), the registered unemployment rate, and debt and budget balance relative to revenue. Local electoral competition is measured by an indicator for an unopposed incumbent in the next local election (11.2 percent of observations) and the number of candidates (mean 3.19). Variable definitions and sources are collected in Table A15.

4.3 Measurement validity and sample selection

Three features of the data call for explicit diagnostics. First, contracts matched only by name may be assigned to the wrong municipality. Where REGON-matched and

name-matched contracts of the same municipality-year can be compared, the cross-sample correlation of the single-bidding share is 0.10 and that of the non-open share 0.13 (Table A7). These correlations are low, but the two measures are computed from disjoint sets of contracts, each containing at least five observations, so sampling noise alone attenuates them, and they speak to the plausibility of name matching more than to measure equivalence. Second, the EU capital-spending share from budget data should correspond to EU-funded procurement. A ten-point higher EU capital share in the same year is associated with 5.2 points more contracts flagged as EU-funded in the procurement file, while the lagged share shows no association when entered alone and a small negative one when both enter jointly (Table A8), which supports the validity of the proxy as a contemporaneous indicator. Third, municipalities that enter the sample differ from those that do not: they are larger (15,877 against 11,713 residents) and receive slightly more EU capital spending (6.05 against 5.46 percent) (Table A2). The analysis therefore reports results by match quality and with inverse-probability weights for sample selection.

5 Empirical strategy

5.1 Baseline specification

The baseline model relates procurement outcomes of municipality i in year t to lagged fiscal structure:

$$Y_{it} = \beta\, A_{i,t-1} + \gamma\, E_{i,t-1} + \mathbf{X}'_{i,t-1}\boldsymbol{\delta} + \mu_i + \tau_t + \varepsilon_{it}, \tag{1}$$

where Y_{it} is one of the three outcomes, $A_{i,t-1}$ is the own-source revenue share, $E_{i,t-1}$ the EU capital-spending share (both in units of ten percentage points), $\mathbf{X}_{i,t-1}$ the vector of controls, μ_i municipality fixed effects and τ_t year fixed effects. Municipality fixed effects remove all time-invariant differences, including institutional tradition, geography and long-run administrative capacity. Year effects absorb national changes in rules and economic conditions. The one-year lag reflects the timing of budgets and awards and reduces simultaneity, although it does not by itself establish causality. Standard errors are clustered by municipality, which accounts for serial correlation within units (Bertrand et al. 2004) and matches the level at which fiscal structure varies (Abadie et al. 2023). Clustering by county (*powiat*) is reported as a robustness check. Because the two main outcomes are tested jointly, Holm adjustments are applied across outcomes (Holm 1979).

5.2 Precision and equivalence

Several results are small, so inference is complemented with measures of precision. For each estimate, the minimum detectable effect at 5 percent size and 80 percent power is computed as $\mathrm{MDE}_{80} = (z_{0.975} + z_{0.80})\,\widehat{\mathrm{SE}} \approx 2.8\,\widehat{\mathrm{SE}}$ and expressed in standard deviations of the outcome. Equivalence is assessed through two one-sided tests, which reverse the burden of proof by taking a non-negligible effect as the null and testing whether the data rule it out (Lakens 2017). This is the appropriate logic for

arguing that an effect is negligible (Rainey 2014). Margins of ± 0.05 and ± 0.10 standard deviations are considered, and the smallest margin δ^* for which equivalence can be established at the 5 percent level is reported.

5.3 Composition and interaction

To examine H2, own-source revenue is split into local taxes and fees (T) and non-tax own revenue (N):

$$Y_{it} = \beta_T \, T_{i,t-1} + \beta_N \, N_{i,t-1} + \gamma \, E_{i,t-1} + \mathbf{X}'_{i,t-1}\boldsymbol{\delta} + \mu_i + \tau_t + \varepsilon_{it}. \tag{2}$$

The exploratory interaction is estimated with variables centred at their sample means so that main effects are evaluated at the average municipality:

$$Y_{it} = \beta \, \tilde{A}_{i,t-1} + \gamma \, \tilde{E}_{i,t-1} + \theta \, \tilde{A}_{i,t-1}\tilde{E}_{i,t-1} + \mathbf{X}'_{i,t-1}\boldsymbol{\delta} + \mu_i + \tau_t + \varepsilon_{it}. \tag{3}$$

The marginal effect of EU capital spending is $\gamma + \theta\tilde{A}$, evaluated at percentiles of the own-source distribution. Since the interaction was identified in threshold analyses and was not pre-specified, results are accompanied by a validation battery: sub-periods, lead and alternative lag placebos, additional interactions between EU spending and controls, alternative autonomy definitions, match-quality subsamples, leave-one-voivodeship-out estimates and repeated random split-sample estimates. These checks assess stability and do not provide confirmatory tests, a distinction that matters given the sensitivity of significance to analytic flexibility (Simmons et al. 2011).

5.4 Timing and mechanism

Timing is examined with contemporaneous, lagged and lead regressors. A lead of the regressor serves as a placebo, since future fiscal structure should not predict current outcomes once current and lagged values are controlled. For non-open procedures, the effect is decomposed into an extensive margin (whether a municipality uses any non-open procedure in a year) and an intensive margin (the share among those that do).

5.5 Supplementary designs

Four further designs are reported in the appendix and used as exploratory evidence only. A fixed-effects threshold model with the own-source share as threshold variable follows Hansen (1999), with bootstrap p-values from wild-cluster resampling (Cameron et al. 2008). A stacked event study around the first EU investment drive of a municipality uses not-yet-treated units as controls, which avoids the biases that arise when staggered treatment timing is handled with two-way fixed effects (Cengiz et al. 2019; Baker et al. 2022). A shift-share instrument uses baseline municipal EU exposure interacted with national spending shifts (Goldsmith-Pinkham et al. 2020; Borusyak et al. 2022), with Anderson-Rubin confidence sets that remain valid under weak instruments (Anderson and Rubin 1949; Andrews et al. 2019). Finally, inverse-probability weights address selection into the outcome sample (Wooldridge 2002).

5.6 Interpretation

The design identifies conditional associations within municipalities. Fixed effects do not remove time-varying confounders, and non-tax own revenue, which includes income from asset disposals, may respond to the same local political conditions that influence procurement. Estimates are therefore described as associations. The supplementary designs are informative about specific threats and are discussed with that limited purpose.

6 Results

6.1 Main associations

Table 3 reports the baseline two-way fixed-effects estimates. A ten-point higher own-source revenue share is associated with 0.59 percentage points more non-open procedures (standard error 0.24, $p = 0.015$) and a 0.031 higher risk index (0.015), while the coefficient for single bidding is 0.36 (0.36) and statistically indistinguishable from zero. The signs are positive throughout. Greater revenue autonomy is therefore not accompanied by more competitive procurement, and the two significant associations, for non-open procedures and the risk index, run against the expectation in H1. In relative terms, the non-open coefficient equals about 7 percent of the sample mean of 8.2 percent. Because within-municipality variation in the own-source share is modest, a one within-standard-deviation change of 3.8 points corresponds to roughly 0.2 percentage points of non-open procurement.

EU capital spending is associated with 0.68 percentage points more single bidding per ten points of expenditure (standard error 0.27, $p < 0.05$), with weaker and imprecise coefficients for non-open procedures (0.21) and the risk index (0.021, $p < 0.10$). Among the controls, higher debt relative to revenue is consistently associated with more risk (7.9 points of single bidding and 0.31 on the risk index), which is compatible with fiscally stressed municipalities turning to less competitive purchasing. The within R^2 values of 0.003 to 0.004 show that fiscal structure explains a small fraction of year-to-year variation, as expected for outcomes that depend on many contract-level decisions.

6.2 How large could the effects be?

Table 4 and Figure 2 translate the estimates into standardised units and test for equivalence. Total own-source revenue is associated with 0.018 standard deviations of single bidding (95 percent confidence interval -0.018 to 0.054), 0.044 of non-open procedures (0.008 to 0.080) and 0.038 of the risk index (0.002 to 0.074). Minimum detectable effects are about 0.05 standard deviations for all three outcomes, so the design can detect effects of that size with 80 percent power. The equivalence tests reject effects larger than ± 0.10 standard deviations for every outcome ($p < 0.001$ for single bidding and the risk index and $p = 0.001$ for non-open procedures). At the stricter margin of ± 0.05, equivalence is established for single bidding ($p = 0.043$) but not for the other two outcomes, which is consistent with their small but non-zero point estimates. The substantive reading is that revenue autonomy neither buys open

Table 3 Own-source revenue autonomy, EU capital spending and procurement integrity: two-way fixed-effects estimates

	(1) Single bidding	(2) Non-open procedures	(3) Risk index
Own-source revenue share (+10 pp), $t-1$	0.356	0.589**	0.031**
	(0.356)	(0.243)	(0.015)
EU capital-spending share (+10 pp), $t-1$	0.678**	0.209	0.021*
	(0.270)	(0.195)	(0.012)
log population, $t-1$	15.638**	−8.149	0.159
	(6.530)	(5.251)	(0.312)
log population density, $t-1$	−11.190**	4.737*	−0.040
	(4.582)	(2.482)	(0.146)
Unemployment rate (x10), $t-1$	1.379	−1.878*	−0.026
	(1.428)	(1.106)	(0.067)
log real revenue per capita, $t-1$	−1.042	2.881**	0.111
	(1.680)	(1.182)	(0.073)
Debt / revenue, $t-1$	7.941***	2.644*	0.307***
	(1.881)	(1.361)	(0.084)
Balance / revenue, $t-1$	−1.245	−3.260**	−0.168*
	(2.084)	(1.460)	(0.090)
Municipality fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	19,383	16,723	16,699
Municipalities	2,202	2,159	2,158
Within R^2	0.003	0.004	0.004
Mean of dep. var.	28.80	8.20	−0.02
SD of dep. var.	19.34	13.39	0.82

Notes: Dependent variables: percentage of contracts awarded with a single bidder (1) or through a non-open procedure (2); risk index = mean of the two standardised shares (3). Regressors are lagged one year; own-source revenue excludes shared PIT/CIT and is expressed in units of 10 percentage points of revenue; EU capital spending is the share of total expenditure, also in 10-pp units. Standard errors clustered by municipality in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

competition nor is associated with large deterioration: any association is contained within roughly one tenth of a standard deviation.

6.3 Local taxes versus non-tax own revenue

Table 5 separates the two components of own revenue. Local taxes and fees show no association with any outcome: the coefficients are −0.31 (0.75) for single bidding, −0.24 (0.52) for non-open procedures and essentially zero for the risk index. Non-tax own revenue is positively related to non-open procedures (0.71, standard error 0.26, $p = 0.007$) and to the risk index (0.034, $p = 0.039$), with a positive but imprecise coefficient for single bidding (0.41). Panel B of Table 4 shows that the equivalence tests also bound the tax component within ±0.10 standard deviations (all $p < 0.02$), although its minimum detectable effect, about 0.11 standard deviations, is twice as large as for the pooled measure. Formal tests do not strongly separate the components: the null that the two coefficients are equal is rejected only at the 10 percent level for non-open procedures ($p = 0.083$) and not at all for the other outcomes ($p = 0.36$

Table 4 Precision of the estimated effect of revenue autonomy: confidence intervals, minimum detectable effects and equivalence tests

	Estimate	SE	95% CI	p	Holm p	MDE	MDE/SD	δ^*/SD	TOST p (0.05 SD)	TOST p (0.10 SD)
A. Total own-source revenue share (excl. PIT/CIT), +10 pp										
Single bidding	0.356	0.356	[−0.342, 1.054]	0.318	0.318	0.997	0.052	0.049	0.043	<0.001
Non-open procedures	0.589	0.243	[0.113, 1.065]	0.015	0.046	0.681	0.051	0.074	0.371	0.001
Risk index	0.031	0.015	[0.002, 0.061]	0.039	0.078	0.042	0.052	0.068	0.259	<0.001
B. Local taxes and fees share, +10 pp (joint model with non-tax own revenue)										
Single bidding	−0.314	0.745	[−1.775, 1.147]	0.674	1.000	2.088	0.108	0.080	0.190	0.015
Non-open procedures	−0.235	0.515	[−1.246, 0.775]	0.648	1.000	1.444	0.108	0.081	0.200	0.016
Risk index	0.000	0.033	[−0.064, 0.064]	0.995	1.000	0.091	0.111	0.066	0.106	0.006
C. Non-tax own revenue share, +10 pp (joint model with local taxes)										
Single bidding	0.412	0.383	[−0.339, 1.163]	0.282	0.282	1.073	0.055	0.054	0.074	<0.001
Non-open procedures	0.711	0.263	[0.196, 1.227]	0.007	0.021	0.737	0.055	0.085	0.563	0.009
Risk index	0.034	0.016	[0.002, 0.066]	0.039	0.078	0.046	0.056	0.075	0.336	0.002

Notes: Estimates are in outcome units (percentage points for the two shares, standard deviations of the pooled shares for the index) per +10 pp of the revenue share, lagged one year, with controls as in Table 3. MDE: minimum detectable effect at 5% size and 80% power (MDE/SD: in standard deviations of the dependent variable). δ^*/SD: smallest symmetric equivalence margin, in SD of the dependent variable, for which the null of a larger effect is rejected at 5% (two one-sided tests). Observations as in Table 3. TOST p: p-value of the two one-sided tests for equivalence within ±0.05 or ±0.10 standard deviations. Holm adjustment applies within each panel across the three outcomes.

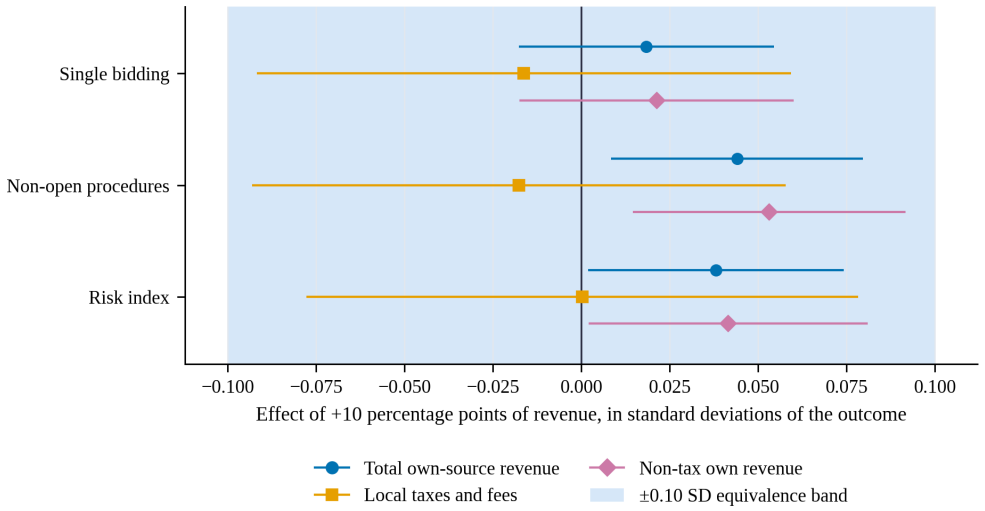


Fig. 2 Precision of the autonomy association in standardised units. Points are coefficients per +10 percentage points of the revenue share, divided by the standard deviation of the outcome, with 95 percent confidence intervals; the shaded band marks the ±0.10 SD equivalence margin. Estimates correspond to Table 4.

and 0.34). The pattern is therefore consistent with H2 in sign, but the evidence for a difference between the components is suggestive.

Panel B of Table 5 adds the interaction of each component with EU capital spending. The interaction with non-tax own revenue is positive for single bidding (0.50, $p < 0.05$) and the risk index (0.021, $p < 0.10$), while the interaction with local taxes is small and imprecise. The exploratory interaction discussed below is thus concentrated in the non-tax component.

6.4 Timing and the non-open mechanism

Table 6 and Figure 3 show that the sign of the association depends on timing. With the regressor measured in the same year, the single-bidding coefficient is negative (−0.71, $p < 0.05$) and the risk index coefficient weakly negative (−0.028, $p < 0.10$). With one lag the single-bidding coefficient is 0.36, and with two lags it is 1.34 (standard error 0.32), equivalent to 0.069 standard deviations. For non-open procedures the coefficients are 0.59 and 0.62 at one and two lags. Averaging the first two lags gives 1.30 for single bidding, 0.93 for non-open procedures and 0.071 for the risk index, all significant at the 1 percent level. The lead of the regressor, which acts as a placebo, is small and insignificant for all outcomes, so there is no sign that future fiscal structure predicts current outcomes. The sum of coefficients over the contemporaneous term and two lags is 0.72 for single bidding, 0.64 for non-open procedures and 0.045 for the risk index, with only the last marginally significant. The sign reversal for single bidding between contemporaneous and lagged regressors indicates that this outcome is sensitive to timing, and conclusions about it should be held loosely.

Table 5 Decomposing revenue autonomy: local taxes and fees versus non-tax own revenue

	(1) Single bidding	(2) Non-open procedures	(3) Risk index
A. Decomposition of own-source revenue autonomy			
Local taxes and fees share (+10 pp), $t-1$	−0.314	−0.235	0.000
	(0.745)	(0.515)	(0.033)
Non-tax own-revenue share (+10 pp), $t-1$	0.412	0.711***	0.034**
	(0.383)	(0.263)	(0.016)
EU capital-spending share (+10 pp), $t-1$	0.644**	0.166	0.019
	(0.273)	(0.195)	(0.012)
p-value: local taxes = non-tax own revenue	0.360	0.083	0.337
Observations	19,383	16,723	16,699
Municipalities	2,202	2,159	2,158
Within R^2	0.003	0.004	0.004
Mean of dep. var.	28.80	8.20	−0.02
SD of dep. var.	19.34	13.39	0.82
B. Interaction of each component with EU capital spending			
Local taxes and fees share (centred)	−0.317	−0.215	0.001
	(0.749)	(0.513)	(0.032)
Non-tax own-revenue share (centred)	0.155	0.596**	0.023
	(0.402)	(0.267)	(0.017)
EU capital-spending share (centred)	0.568**	0.141	0.016
	(0.279)	(0.195)	(0.012)
Local taxes × EU capital spending	0.211	0.232	0.014
	(0.307)	(0.210)	(0.014)
Non-tax own revenue × EU capital spending	0.504**	0.219	0.021*
	(0.252)	(0.187)	(0.011)
Observations	19,383	16,723	16,699
Municipalities	2,202	2,159	2,158

Notes: Both components enter jointly with municipality and year fixed effects and the controls of Table 3 (lagged one year); +10 pp of revenue units. Panel B centres each variable at its sample mean so that main effects are evaluated at the average municipality. Standard errors clustered by municipality. * p<0.10, ** p<0.05, *** p<0.01.

Table 7 explores the mechanism behind the non-open result. The association does not operate on the extensive margin: own-source revenue is unrelated to the probability that a municipality uses any non-open procedure in a year (-0.18, $p = 0.83$). It operates on the intensive margin: among municipality-years with at least one non-open procedure, a ten-point higher own-source share is associated with 1.11 points more non-open procurement ($p = 0.008$), against a mean of 17.5. Controlling for contract value or the number of contracts leaves the estimate at 0.56 and 0.54, restricting the sample to municipalities that ever use non-open procedures gives 0.63, and requiring at least ten known procedures raises it to 0.95. Municipalities with more own revenue are therefore not more likely to use restricted procedures at all, but those that use them rely on them to a greater degree. The intensive-margin estimate is conditional

Table 6 Timing of the effect of revenue autonomy

	Single bidding	Non-open procedures	Risk index
Contemporaneous (t)	−0.712**	−0.238	−0.028*
	(0.363)	(0.252)	(0.015)
Lag 1 (baseline specification)	0.356	0.589**	0.031**
	(0.356)	(0.243)	(0.015)
Lag 2	1.340***	0.616***	0.060***
	(0.324)	(0.233)	(0.014)
Average of lags 1 and 2	1.300***	0.927***	0.071***
	(0.437)	(0.323)	(0.019)
Sum of coefficients, t to $t-2$ (distributed lag)	0.723	0.635	0.045*
	(0.531)	(0.394)	(0.024)
Lead $t+1$ (placebo, controlling for lag 1)	−0.148	−0.042	−0.009
	(0.360)	(0.243)	(0.015)

Notes: Each cell is the coefficient (or sum of coefficients) on own-source revenue share in 10-pp units; municipality and year fixed effects; SE clustered by municipality; EU capital spending and the controls of Table 3 are timed consistently with the autonomy regressor. Observations range from 16,607 to 19,383 across cells. * p<0.10, ** p<0.05, *** p<0.01.

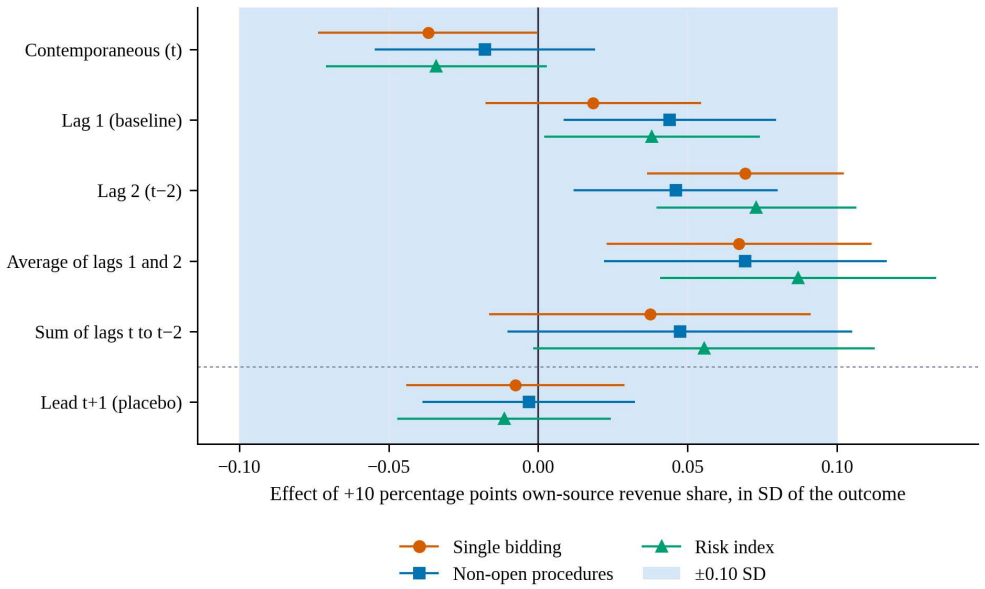


Fig. 3 Timing of the association between own-source revenue and procurement integrity. Coefficients on the own-source revenue share (+10 pp) in standard-deviation units with 95 percent confidence intervals, for contemporaneous, lagged, averaged, cumulative and lead (placebo) regressors; the shaded band marks ±0.10 SD. Estimates correspond to Table 6.

on a positive share and is therefore subject to selection, so it is read together with the checks in the remaining rows.

6.5 EU funds and the conditionality of their association

Table 8 reports the interaction model. The own-source by EU capital-spending interaction is positive for all three outcomes: 0.40 for single bidding ($p < 0.05$), 0.24 for non-open procedures ($p < 0.05$) and 0.019 for the risk index ($p < 0.05$). Holm-adjusted p-values for the two primary outcomes are 0.023 for single bidding and for the risk index. At the average own-source share, EU capital spending is associated with 0.63 points more single bidding. Panel B shows how the marginal effect varies with fiscal structure. At the 10th percentile of the own-source share (13.8 percent of revenue), the association of EU capital spending with single bidding is 0.14 and insignificant; at the median (24.6 percent) it is 0.57 ($p < 0.05$); at the 90th percentile (40.2 percent) it is 1.19 ($p < 0.01$), which equals 0.061 standard deviations and 4.1 percent of the mean. For the risk index, the corresponding values rise from −0.005 to 0.044 ($p < 0.01$). The relationship between EU funding and weaker competition is therefore concentrated in municipalities that rely heavily on their own revenue (Figure 4).

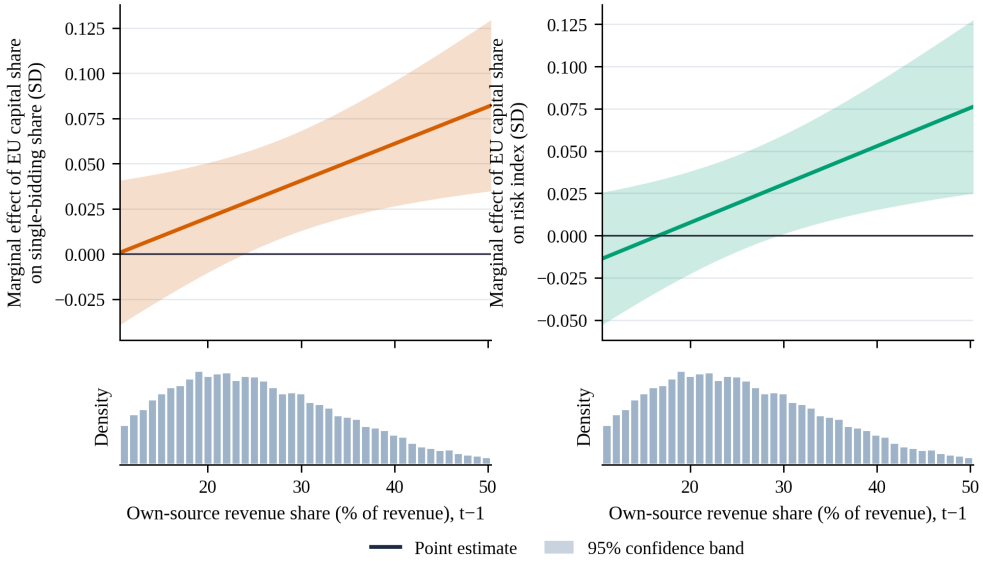


Fig. 4 Marginal association of EU capital spending with procurement integrity across the own-source revenue distribution. Upper panels: marginal effect of +10 pp of EU capital spending (in SD of the outcome) at different levels of the lagged own-source revenue share, with 95 percent confidence bands, for single bidding and the risk index. Lower panels: density of the own-source share. Estimates are based on the interaction model of Table 8; the interaction was identified in exploratory analysis.

Restricting the sample to municipalities with positive EU capital spending in the previous year (columns 4 to 6 of Table 8) reduces the interaction for single bidding to

Table 7 Own-source revenue autonomy and non-open procedures: margins and composition checks

	Own-source share (+10 pp)	Std. error	p-value	Observations	Municipalities	Mean of dep. var.
Baseline: non-open share (pp)	0.589**	0.243	0.015	16,723	2,159	8.20
Extensive margin: P(any non-open procedure) × 100	−0.183	0.866	0.833	16,723	2,159	47.13
Intensive margin: non-open share if positive (pp)	1.106***	0.417	0.008	7,556	1,431	17.50
Baseline + log average contract value	0.559**	0.243	0.022	16,723	2,159	8.20
Baseline + log(1 + number of contracts)	0.537**	0.242	0.026	16,723	2,159	8.20
Only municipalities that ever use non-open procedures	0.628**	0.276	0.023	14,588	1,808	9.40
Municipality-years with at least 10 known procedures	0.954***	0.295	0.001	11,191	1,822	9.50

Notes: Each row is a separate two-way fixed-effects regression with the controls and EU capital-spending share of Table 3 (all lagged one year); the coefficient reported is that on the own-source revenue share (+10 pp). Standard errors clustered by municipality. * p<0.10, ** p<0.05, *** p<0.01.

Table 8 Conditionality: EU capital spending and own-source revenue autonomy

	(1) Single bidding	(2) Non-open procedures	(3) Risk index	(4) Single bidding, EU>0	(5) Non-open procedures, EU>0	(6) Risk index, EU>0
A. Interaction model						
Own-source revenue share (+10 pp, centred)	0.188	0.481*	0.023	0.339	0.526**	0.028*
	(0.363)	(0.246)	(0.015)	(0.388)	(0.264)	(0.017)
EU capital-spending share (+10 pp, centred)	0.631**	0.171	0.018	0.457	0.029	0.007
	(0.271)	(0.191)	(0.012)	(0.292)	(0.206)	(0.013)
Own-source share × EU capital share	0.396**	0.235**	0.019**	0.274	0.239*	0.017**
	(0.168)	(0.119)	(0.007)	(0.183)	(0.129)	(0.008)
Holm-adjusted p, interaction	0.023		0.023			
Municipality fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	19,383	16,723	16,699	15,612	13,321	13,301
Municipalities	2,202	2,159	2,158	2,157	2,112	2,110
Within R^2	0.004	0.004	0.005	0.003	0.004	0.004
Mean of dep. var.	28.80	8.20	−0.02	28.70	8.22	−0.02
B. Marginal effect of EU capital-spending share (+10 pp) at percentiles of own-source revenue share						
P10 of own-source share (13.8% of revenue)	0.142	−0.119	−0.005	0.115	−0.269	−0.014
	(0.357)	(0.228)	(0.015)	(0.383)	(0.249)	(0.016)
P25 of own-source share (18.3% of revenue)	0.320	−0.014	0.003	0.238	−0.162	−0.006
	(0.313)	(0.203)	(0.013)	(0.335)	(0.221)	(0.014)
P50 of own-source share (24.6% of revenue)	0.570**	0.135	0.015	0.412	−0.011	0.004
	(0.275)	(0.190)	(0.012)	(0.296)	(0.205)	(0.013)
P75 of own-source share (32.3% of revenue)	0.875***	0.316	0.029**	0.623**	0.173	0.017
	(0.281)	(0.213)	(0.013)	(0.305)	(0.227)	(0.014)
P90 of own-source share (40.2% of revenue)	1.188***	0.501*	0.044***	0.839**	0.363	0.030*
	(0.342)	(0.268)	(0.016)	(0.374)	(0.287)	(0.017)

Notes: Columns (1)-(3): all municipality-years; columns (4)-(6): municipality-years with positive lagged EU capital spending, regressors re-centred at the sub-sample means. Regressors lagged one year; controls as in Table 3. The own-source x EU interaction was identified in exploratory threshold analyses and is not a pre-specified hypothesis; Holm adjustment covers only the two primary outcomes and does not account for that specification search. Panel B: marginal effect of +10 pp EU capital-spending share evaluated at percentiles of the pooled distribution of the own-source revenue share. Standard errors clustered by municipality. * p<0.10, ** p<0.05, *** p<0.01.

0.27 (0.18), while those for non-open procedures (0.24, $p < 0.10$) and the risk index (0.017, $p < 0.05$) remain similar. The note to the table repeats the caveat that the interaction was found in exploratory work and that the Holm adjustment does not account for the specification search.

6.6 Validating the interaction

Table 9 and Figure 5 subject the interaction to a battery of checks. Several results favour stability. Adding interactions between EU spending and all controls lowers the single-bidding coefficient to 0.30 ($p < 0.10$) but leaves its sign and magnitude broadly intact. Using own-source revenue that includes shared income-tax receipts yields significant interactions for all outcomes (0.36, 0.26 and 0.019). Controlling for the share of REGON-matched contracts or dropping 2018 and 2019, where matching is weakest, changes nothing material. In cells where at least half of the contracts are REGON-matched, the single-bidding interaction rises to 0.47 ($p < 0.05$) and the risk index interaction to 0.021, while in cells matched only by name it falls to an insignificant 0.29, which argues against the pattern being created by noisy matching. Dropping each of the 16 voivodeships in turn gives single-bidding interactions between 0.35 and 0.46, significant at the 5 percent level in all runs, with risk index interactions between 0.016 and 0.021 significant in all runs and non-open interactions significant at 5 percent in 7 of 16 runs. In 100 random 50-50 splits of municipalities, the interaction is positive in 98 percent of half-samples for single bidding and non-open procedures and in all half-samples for the risk index, and no half-sample shows a significantly negative coefficient, although only 36, 22 and 41 percent are significantly positive, which reflects the loss of power from halving the sample.

Other results qualify this picture. The interaction is significant in 2008 to 2013 (0.67 for single bidding, $p < 0.01$) but absent in 2014 to 2019 (-0.16, with a small negative coefficient for the risk index), so the finding is period-specific. The placebo lead of EU capital spending has a negative, marginally significant coefficient for single bidding (-0.30, $p < 0.10$), which is the opposite of the baseline sign and therefore not what a spurious positive pre-pattern would produce, although it shows that the result is sensitive to timing. Table 10 shows that the interaction is smaller and insignificant when EU capital spending is measured in the same year (0.26), larger at one lag (0.40) and larger still at two lags (0.62, $p < 0.01$), and that averaging the same-year and two lagged values gives 0.94 ($p < 0.01$). This lag structure is consistent with award decisions preceding expenditure, since the share of EU capital spending in a given year reflects projects whose procurement was decided earlier.

6.7 Robustness of the autonomy association

Table 11 and Figure 6 vary the measurement of regressors, the estimator, the sample and the outcome definition. The non-open association is the most stable result. It remains positive and significant when own-source revenue includes shared taxes (0.61), when fiscal ratios are not winsorised (0.50), when observations are weighted by the number of contracts (1.30), when standard errors are clustered by county (0.59, standard error 0.23), when voivodeship-by-year fixed effects are added (0.57), when

Table 9 Validation of the own-source × EU capital-spending interaction

	N (single bidding)	Single bidding	Non-open procedures	Risk index
A. Alternative specifications and samples (interaction coefficient, standard error in parentheses)				
Baseline (all municipality-years)	19,383	0.396**	0.235**	0.019**
		(0.168)	(0.119)	(0.007)
2008-2013 only	10,353	0.674***	0.181	0.023***
		(0.212)	(0.141)	(0.009)
2014-2019 only	8,822	−0.158	0.164	−0.004
		(0.293)	(0.252)	(0.015)
Placebo: EU capital share at $t+1$ (lead)	18,164	−0.301*	0.096	−0.004
		(0.174)	(0.117)	(0.007)
EU capital share at $t-2$	19,382	0.616***	−0.083	0.015*
		(0.170)	(0.123)	(0.008)
Adding EU × (size, density, unemployment, income, debt, balance)	19,383	0.301*	0.154	0.011
		(0.180)	(0.126)	(0.008)
Own-source revenue including PIT/CIT	19,383	0.357***	0.263***	0.019***
		(0.128)	(0.100)	(0.006)
Local taxes and fees share only	19,383	0.334	0.277	0.019
		(0.298)	(0.203)	(0.013)
Dropping 2018-2019	16,883	0.386**	0.221*	0.018**
		(0.177)	(0.119)	(0.007)
Controlling for share of REGON-matched contracts	19,383	0.394**	0.236**	0.019**
		(0.168)	(0.119)	(0.007)
Dropping 2018-2019, controlling for REGON share	16,883	0.387**	0.222*	0.018**
		(0.177)	(0.119)	(0.007)
Cells with REGON-matched share ≥ 0.5	9,816	0.472**	0.269*	0.021**
		(0.216)	(0.151)	(0.009)
Cells with REGON-matched share ≥ 0.8	9,334	0.474**	0.254	0.020**
		(0.221)	(0.155)	(0.010)
Cells matched by name only (REGON share = 0)	8,401	0.288	0.129	0.011
		(0.292)	(0.189)	(0.012)

extended controls are included (0.46, $p < 0.10$), when extreme municipalities are dropped (0.55), and when outcomes are built from REGON-matched contracts only (0.69) or from an alternative procurement file (0.54). It strengthens as the minimum number of contracts per cell increases: 0.42 with at least 3 contracts, 0.59 with 5, 0.95 with 10 and 1.38 with 20 contracts, a pattern consistent with measurement noise attenuating estimates in thin cells.

Table 9 Validation of the own-source × EU capital-spending interaction (continued)

	N (single bidding)	Single bidding	Non-open procedures	Risk index
B. Leave-one-voivodeship-out (16 runs)				
Range of interaction coefficient		0.348 to 0.460	0.166 to 0.271	0.016 to 0.021
Number of runs with p<0.05		16 of 16	7 of 16	16 of 16
Number of runs with p<0.10		16 of 16	15 of 16	16 of 16
C. Random 50/50 splits of municipalities (100 splits, 200 half-samples)				
Mean interaction coefficient across half-samples		0.396	0.235	0.019
Share of half-samples with positive coefficient		0.98	0.98	1.00
Share positive and p<0.05		0.36	0.22	0.41
Share negative and p<0.05		0.00	0.00	0.00

Notes: Cells in Panel A report the coefficient on (own-source revenue share × EU capital-spending share), both centred and in 10-pp units, from regressions with municipality and year fixed effects and the controls of Table 3. Rows 'Own-source including PIT/CIT' and 'Local taxes and fees share' replace the own-source regressor by the stated measure. Standard errors clustered by municipality. * p<0.10, ** p<0.05, *** p<0.01. The interaction is an exploratory finding; the validation checks assess its stability rather than provide independent confirmatory tests.

The results also reveal limits. The non-open coefficient is no longer significant with inverse-probability weights (0.39, standard error 0.30), and it is insignificant in 2014 to 2019 (0.18), whereas it is 0.79 in 2008 to 2013. Single bidding rarely differs from zero across specifications, with exceptions at the contemporaneous timing and, at the 10 percent level only, when the first and last years are dropped (0.65) or when the sample is restricted to 2008 to 2013 (0.78). The risk index shrinks to 0.015 (0.012) when the third component is added. The period contrast for single bidding is not stable across designs: split-sample estimates are positive in 2008 to 2013 and negative in 2014 to 2019 (Table 11), whereas the pooled model with group-specific slopes in Table A4 yields the opposite ordering (0.15 and 1.11). No reliance is therefore placed on a period pattern for this outcome. Overall, the autonomy association is best described as small, concentrated in non-open procedures, and dependent on period and sample weighting.

6.8 Further evidence

The appendix examines additional questions. Heterogeneity analysis in Table A4 finds that equality of slopes is rejected for single bidding across periods ($p < 0.001$, Holm-adjusted $p = 0.005$), with a coefficient of 1.11 in 2014 to 2019 against 0.15 in 2008 to 2013, whereas differences across municipality types and population terciles do not survive adjustment. Electoral competition does not moderate the autonomy association, and the interactions with an unopposed incumbent or the number of candidates are small and insignificant (Table A5). Results by match quality (Table A6) confirm that the interaction is not a product of name-matched contracts.

Exploratory nonlinear models provide limited support for thresholds. The fixed-effects threshold model places the own-source threshold at about 21 to 28 percent of revenue, but bootstrap p-values of 0.07, 0.12 and 0.05 become 0.16 after Holm adjustment (Table A9). The smooth-transition model rejects linearity at the 5 percent

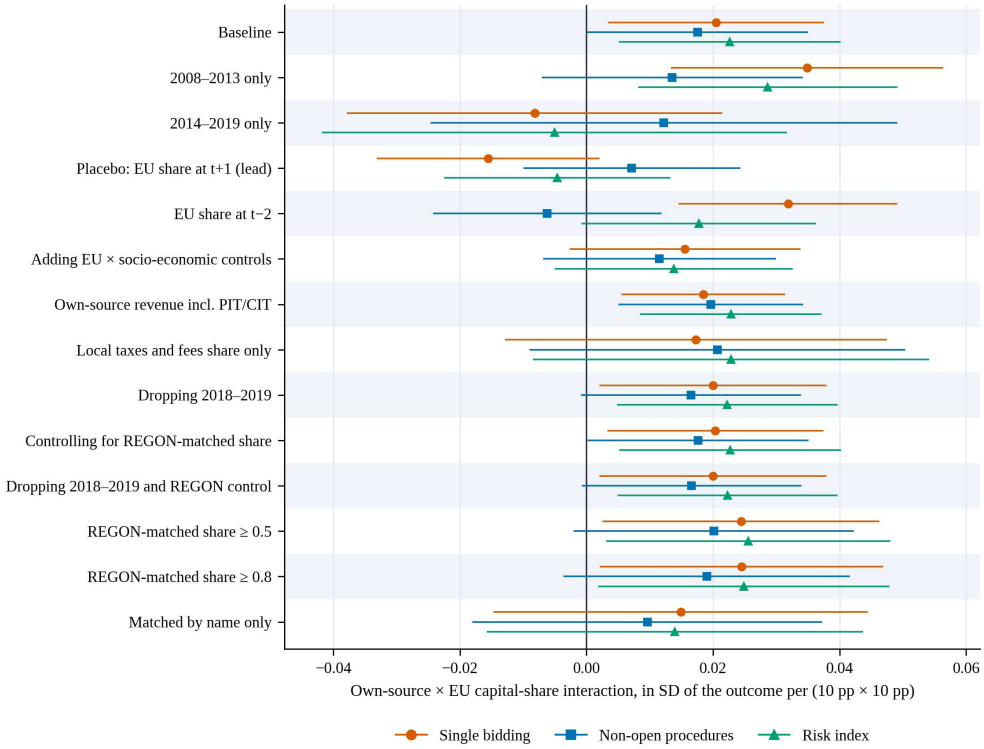


Fig. 5 Validation of the own-source by EU capital-spending interaction. Interaction coefficients (SD of the outcome per 10 pp by 10 pp) with 95 percent confidence intervals for alternative specifications, sub-periods, placebo timing and match-quality subsamples. Estimates correspond to Panel A of Table 9.

level for single bidding and the risk index in the LM-F test, but adjusted p-values of 0.12 to 0.14 do not (Table A10), and the risk index estimate sits at the boundary of the parameter space. The stacked event study around the first EU investment drive gives no effect for one definition of the drive and large positive effects for the other (5.3 points of single bidding with 85 treated municipalities), but the non-open specification for that definition fails the pre-trend test ($p = 0.019$) (Table A11). The shift-share instrument is weak, with first-stage F statistics between 3.2 and 8.3, and Anderson-Rubin sets are unbounded or too wide to be informative (Table A12). The instrumental-variable results are therefore reported for transparency and do not alter the interpretation. Leave-one-voivodeship-out estimates are listed in Table A13, and inverse-probability weighting diagnostics are in Table A14, where weights average about 1.5 and attenuate the autonomy coefficient for non-open procedures to 0.39.

Table 10 Timing of the EU capital-spending effect and of the interaction

	Single bidding	Non-open procedures	Risk index
A. Interaction coefficient (own-source share at $t-1$ × EU capital share, timing as indicated)			
EU capital share at t	0.256	0.167	0.010
	(0.158)	(0.133)	(0.008)
EU capital share at $t-1$ (baseline)	0.396**	0.235**	0.019**
	(0.168)	(0.119)	(0.007)
EU capital share at $t-2$	0.616***	−0.083	0.015*
	(0.170)	(0.123)	(0.008)
Average of EU capital share, t and $t-1$	0.509**	0.306*	0.022**
	(0.210)	(0.170)	(0.010)
Average of EU capital share, t to $t-2$	0.944***	0.228	0.032**
	(0.272)	(0.212)	(0.013)
B. Main effect of EU capital-spending share (+10 pp) at the mean own-source share			
EU capital share at t	0.067	0.213	0.015*
	(0.209)	(0.153)	(0.009)
EU capital share at $t-1$ (baseline)	0.631**	0.171	0.018
	(0.271)	(0.191)	(0.012)
EU capital share at $t-2$	−0.169	−0.077	−0.009
	(0.230)	(0.150)	(0.009)
Average of EU capital share, t and $t-1$	0.475	0.339	0.028*
	(0.323)	(0.239)	(0.015)
Average of EU capital share, t to $t-2$	0.316	0.254	0.019
	(0.418)	(0.310)	(0.019)
C. Interaction of EU capital share at t with alternative lags of own-source share			
Own-source share at $t-1$	0.256	0.167	0.010
	(0.158)	(0.133)	(0.008)
Own-source share at $t-2$	0.245	0.271**	0.014*
	(0.159)	(0.132)	(0.007)
Average own-source share, $t-1$ and $t-2$	0.272*	0.243*	0.013*
	(0.164)	(0.141)	(0.008)

Notes: All regressions include municipality and year fixed effects and the controls of Table 3; regressors centred at sample means; +10 pp units. Standard errors clustered by municipality. * p<0.10, ** p<0.05, *** p<0.01. EU capital-spending share is measured as a share of total expenditure; the lag structure of the interaction is therefore informative about whether the result reflects contemporaneous spending or lagged award decisions.

7 Discussion

7.1 Why revenue autonomy does not buy open competition

The fiscal-link argument predicts that officials who spend locally raised money face stronger scrutiny and therefore buy more competitively (Oates 1999; Weingast 2009). The estimates do not bear this out. Several features of procurement help to explain

Table 11 Robustness of the effect of own-source revenue autonomy on procurement integrity

	Single bidding: coef. (SE)	Single bidding: N	Non-open procedures: coef. (SE)	Non-open procedures: N	Risk index: coef. (SE)	Risk index: N
A. Measurement of the regressors						
Baseline (Table 3)	0.356 (0.356)	19,383	0.589** (0.243)	16,723	0.031** (0.015)	16,699
Own-source revenue including PIT/CIT	0.071 (0.338)	19,383	0.608** (0.242)	16,723	0.024* (0.015)	16,699
Local taxes and fees share only (replaces own-source share)	−0.428 (0.737)	19,383	−0.451 (0.510)	16,723	−0.010 (0.032)	16,699
Total EU-funded expenditure share instead of EU capital share	0.363 (0.356)	19,383	0.591** (0.243)	16,723	0.031** (0.015)	16,699
No winsorisation of fiscal ratios	0.296 (0.344)	19,383	0.502** (0.236)	16,723	0.026* (0.015)	16,699
Contemporaneous regressors (t)	−0.712** (0.363)	19,383	−0.238 (0.252)	16,723	−0.028* (0.015)	16,699
B. Estimation						
Weighted by number of contracts (capped at 100)	0.403 (0.449)	19,383	1.297*** (0.408)	16,723	0.062*** (0.023)	16,699
Standard errors clustered by county (powiat)	0.356 (0.334)	19,383	0.589** (0.230)	16,723	0.031** (0.015)	16,699
Voivodeship-by-year fixed effects	0.187 (0.365)	19,383	0.570** (0.244)	16,723	0.025* (0.015)	16,699
Extended controls (grants, capital-spending share)	0.389 (0.409)	19,383	0.462* (0.276)	16,723	0.024 (0.017)	16,699
Inverse-probability weights for sample selection	0.290 (0.395)	19,383	0.392 (0.295)	16,723	0.016 (0.017)	16,699
C. Sample						
Excluding city-counties and the top 1% by population	0.391 (0.360)	18,964	0.552** (0.241)	16,346	0.031** (0.015)	16,324
Excluding 2008 and 2018-2019	0.645* (0.375)	15,997	0.683*** (0.236)	15,667	0.041*** (0.015)	15,643
2008-2013	0.783* (0.463)	10,353	0.794*** (0.298)	10,362	0.050*** (0.019)	10,353
2014-2019	−0.484 (0.646)	8,822	0.181 (0.480)	6,047	−0.034 (0.032)	6,030
Municipalities with at least 8 valid years	0.126 (0.396)	15,883	0.726** (0.311)	11,878	0.026 (0.019)	11,842
Minimum of 3 contracts per municipality-year	0.234 (0.352)	22,323	0.417* (0.236)	19,222	0.021 (0.014)	19,199
Minimum of 10 contracts per municipality-year	0.200 (0.429)	12,487	0.954*** (0.295)	11,191	0.043** (0.018)	11,144
Minimum of 20 contracts per municipality-year	−0.121 (0.627)	5,767	1.378*** (0.511)	5,366	0.054* (0.029)	5,333
D. Outcome definition						
Risk index with three components (adds no-call-for-tender)					0.015 (0.012)	19,407
Contracts matched by REGON only	−0.232 (0.483)	9,820	0.692** (0.337)	9,235		
Alternative procurement file (DIB), 2008-2017	0.280 (0.360)	17,155	0.535** (0.208)	17,802		

Notes: Each cell reports the coefficient on the own-source revenue share (+10 pp, lagged one year unless stated) from a separate two-way fixed-effects regression; standard errors clustered by municipality in parentheses unless stated. In 'Local taxes and fees share only' and 'Including PIT/CIT' the coefficient refers to the substituted measure; in outcome rows of panel D the dependent variable is redefined as stated. * p<0.10, ** p<0.05, *** p<0.01.

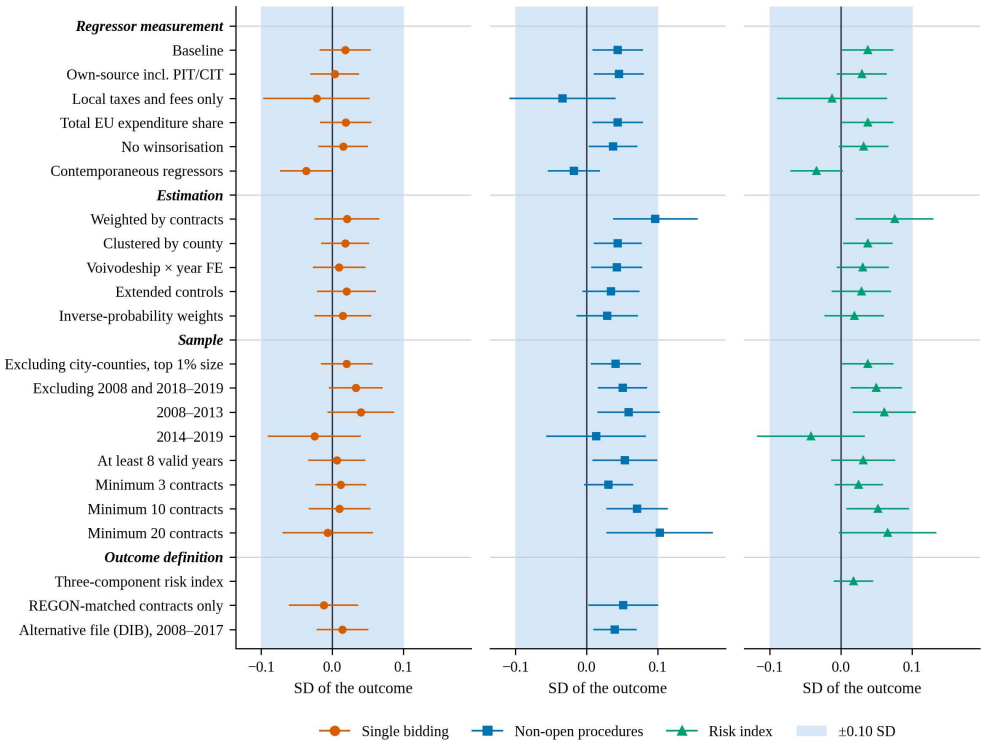


Fig. 6 Robustness of the association between own-source revenue and procurement integrity. Coefficients on the own-source revenue share (+10 pp) in SD units with 95 percent confidence intervals, by outcome, for alternative measurement, estimation, sample and outcome definitions; the shaded band marks ±0.10 SD. Estimates correspond to Table 11.

why. The accountability mechanism documented for taxation operates where citizens can observe both what they pay and what is delivered (Gadenne 2017; Timmons and Garfias 2015). Tender procedures are far less visible than services or tax bills, and a field experiment in Indonesia found that monitoring by community members did little to reduce missing expenditure, in contrast to external audits (Olken 2007). Even when information about misconduct reaches voters, its electoral consequences depend on the context in which it is released (Ferraz and Finan 2008, 2011). A resident may therefore withhold support for a mayor who raises taxes without ever learning how many bidders took part in a municipal tender.

The contrast between the two components of own revenue is consistent with this reading, though it should be interpreted cautiously because formal tests of equality are weak. Local taxes and fees show no association with any outcome, and equivalence tests bound them within one tenth of a standard deviation. The association with non-open procedures comes from non-tax revenue, which plausibly includes income from property and asset use that officials can influence and that residents rarely perceive as their own contribution. Revenue of this kind resembles a windfall in its lack of visibility,

and windfalls have been shown to weaken the selection and discipline of politicians (Brollo et al. 2013). Whether the positive coefficient reflects discretion that is exploited or discretion that is exercised for legitimate reasons cannot be separated with these data. Non-open procedures are not intrinsically improper, and the literature reports heterogeneous consequences of buyer discretion (Coviello et al. 2018; Baltrunaite et al. 2021), while in other settings it opens room for favouritism and manipulation (Palguta and Pertold 2017; Szucs 2024; Baltrunaite 2020). The finding that the association appears on the intensive margin, among municipalities that already use restricted procedures, is compatible with either interpretation. It argues against the notion that autonomy changes the basic procedural regime, and in favour of the idea that it changes how much existing flexibility is used.

Timing adds a second lesson. The association with single bidding is negative within the year, insignificant after one year and positive after two years, while the lead of the regressor is null. A fiscal structure that is measured at the same time as procurement outcomes may reflect common shocks, such as a large investment programme that raises revenue and procurement activity together, whereas lagged measures are closer to the budgetary conditions under which tenders were prepared. Studies of procurement point to the importance of the pre-award stage, in which buyers choose procedures, thresholds and specifications (Decarolis 2014; Coviello and Mariniello 2014; Bosio et al. 2022). If fiscal conditions shape these choices, effects would be expected to appear with a delay, which is the pattern observed. The sensitivity of the single-bidding coefficient to timing nevertheless implies that it is a fragile outcome with which to test H1.

7.2 EU capital spending and local fiscal structure

EU-funded capital spending is associated with more single bidding on average and, more clearly, in municipalities that finance a larger share of their budget from their own revenue. Two non-exclusive interpretations are consistent with the data. The first concerns project characteristics. Large and technically demanding projects attract fewer bidders (Baldi et al. 2016), and co-financed investments are typically of this kind. Municipalities with a strong own-revenue base are plausibly better placed to supply the co-financing that such projects require, so the same fiscal structure that facilitates EU absorption may bring larger and more complex tenders. The second concerns the interaction between sources of money. External rules operate on top of domestic institutions, and the integrity of EU-funded contracts has been shown to depend on that domestic context (Fazekas and King 2019). Their disciplining effect may be weaker where local officials control a large pool of discretionary resources. The evidence in this study cannot discriminate between these accounts.

Three results qualify any strong reading. The interaction was identified in exploratory analysis (Simmons et al. 2011), it is concentrated in 2008 to 2013, and it is absent in 2014 to 2019. A change in the programming period, in project mix or in national procurement rules over the sample may account for the difference, and cohesion-policy research shows that the effects of EU transfers depend on absorptive capacity and the quality of local governance, both of which change over time (Becker et al. 2013; Surubaru 2017; Rodríguez-Pose and Garcilazo 2015). The pattern across

lags, with the interaction growing from the contemporaneous to the two-year lagged measure, fits a sequence in which procurement decisions precede recorded expenditure. Finally, the validation checks show that the finding is not driven by a single region, by weakly matched contracts or by a particular half of the municipalities, but these are tests of stability and not of causation.

7.3 Implications

For the design of fiscal decentralisation, the results indicate that granting municipalities more own revenue should not be expected, by itself, to improve competition in procurement. Complementary institutions appear more relevant. Evidence from other settings suggests that publicity requirements, electronic procurement and rules that limit buyer discretion can change participation and performance (Coviello and Mariniello 2014; Lewis-Faupel et al. 2016; Decarolis 2014), and that formal rules and actual practice often diverge (Bosio et al. 2022). The composition of revenue is a second dimension that policy debate often ignores: in this sample, tax-based autonomy shows no association with integrity risk, while the non-tax component is associated with greater use of restricted procedures. For managers of EU funds, the results raise the possibility that monitoring could be directed at municipalities that combine high EU spending with high own-source shares, where single bidding is more frequent. This is a hypothesis for further testing and not a recommendation, because the evidence is observational and the pattern is absent in the later part of the sample.

For research that uses administrative procurement data, the study illustrates the value of bounding effects and of reporting measurement diagnostics. Indicators built from tender records depend on the completeness of the records and on the way contracts are linked to buyers (Fazekas and Kocsis 2020; Fazekas et al. 2016). The comparison between REGON-matched and name-matched contracts, and the fact that the main interaction is at least as large in better-matched cells, shows how such checks can be integrated into the analysis. The approach to equivalence testing may be of use beyond this case, for contexts where the policy-relevant question is whether an effect is negligible (Lakens 2017; Rainey 2014).

8 Limitations and future research

Several limitations bear on the interpretation. First, the identification strategy rests on within-municipality variation and cannot exclude time-varying confounders, such as local political conditions that affect both revenue structure and procurement behaviour. Standard errors address serial correlation but not omitted variables (Bertrand et al. 2004; Abadie et al. 2023). The supplementary designs do not resolve this: the shift-share instrument is weak, the event study is sensitive to the definition of treatment, and the threshold and smooth-transition models are exploratory (Andrews et al. 2019; Baker et al. 2022; Hansen 1999). Second, the outcomes are proxies. Single bidding and non-open procedures indicate reduced competition and not misconduct, and non-open procedures are sometimes appropriate (Fazekas et al. 2016; Coviello et al. 2018). Third, the contract-to-municipality link relies on REGON identifiers for 53 percent of contracts and on names for the rest, and the REGON share declines

in the final years. The diagnostics reduce but do not remove concern about misclassification. Fourth, the sample depends on a minimum number of contracts per cell. Selection into the sample is related to size and EU spending, and inverse-probability weighting changes the non-open estimate from significant to insignificant. Finally, the interaction was found in exploratory analysis and is period-specific, so the associated p-values should not be read as confirmatory.

These limitations point to a research agenda. Direct tests of the interaction in later programming periods would show whether it persists. Contract-level data on project size and complexity would help to distinguish between the project-characteristics and discipline explanations. Sources of exogenous variation in own revenue, such as changes in local tax rules or reclassifications of revenue, could provide sharper identification. Evidence on whether restricted procedures in high-autonomy municipalities lead to higher prices or lower quality would clarify the welfare relevance of the non-open association. Replication in other decentralised systems with comparable procurement data, including European countries where municipal borrowing limits and oversight differ (Bröthaler et al. 2015), would indicate how general the results are (Fisman and Gatti 2002; Fan et al. 2009; Martinez-Vazquez et al. 2017).

9 Conclusion

This study asked whether municipalities that finance themselves more from their own revenue award public contracts more competitively. Using a panel of Polish municipalities from 2008 to 2019, the analysis finds no support for that proposition. Higher own-source revenue shares are not associated with less single bidding and are weakly associated with more non-open procurement, an association that comes from non-tax own revenue and from municipalities already using restricted procedures. Local taxes and fees, the component most closely tied to the accountability argument, show no association with any outcome, and the data rule out effects larger than one tenth of a standard deviation. EU capital spending is associated with more single bidding, particularly where own-source revenue is high, although this pattern emerged from exploratory work and does not hold in the later part of the sample.

The results change how the fiscal-autonomy hypothesis should be framed. Revenue autonomy appears to be neither a safeguard against nor a cause of weak procurement competition. Its relevance depends on which revenue is concerned, when it is measured, and which other sources of money are present. Future work with sharper identification and richer contract-level information is needed to establish whether the associations reported here reflect behaviour that is causally induced by fiscal structure or arise from the types of municipalities and projects that coincide with it.

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Declarations

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- **Conflict of interest/Competing interests**: The author declares no competing interests.
- **Ethics approval and consent to participate**: Not applicable. The study uses aggregated administrative and public-procurement records and involves no human participants.
- **Consent for publication**: Not applicable.
- **Data availability**: The study combines public-procurement tender records from the Global Public Procurement Dataset, municipal budget data and Statistics Poland Local Data Bank indicators. The assembled municipality-year panel is available from the corresponding author on reasonable request.
- **Materials availability**: Not applicable.
- **Code availability**: The Python code used to produce all tables and figures is available from the corresponding author on reasonable request.
- **Author contribution**: The sole author conceived the study, assembled the data, carried out the analysis, and wrote and revised the manuscript.

Appendix A Additional tables

This appendix reports sample construction and representativeness (Tables A1 and A2), the step-by-step baseline specifications (Table A3), heterogeneity and electoral moderators (Tables A4 and A5), measurement validity (Tables A6 to A8), exploratory nonlinear and design-based analyses (Tables A9 to A12), leave-one-out and weighting diagnostics (Tables A13 and A14) and variable definitions (Table A15).

Table A1 Sample construction

	Municipality-years	Municipalities	Contracts
Single bidding: all municipality-years with at least 5 contracts	19,435	2,254	439,566
Non-open procedures: all municipality-years with at least 5 contracts	16,783	2,219	410,042
Risk index: all municipality-years with at least 5 contracts	16,760	2,219	409,759
Single bidding: main regression sample (Table 3)	19,383	2,202	439,139
Non-open procedures: main regression sample (Table 3)	16,723	2,159	409,428
Risk index: main regression sample (Table 3)	16,699	2,158	409,140

Notes: The estimation window is 2008-2019. A municipality-year enters an outcome only if it has at least 5 contracts with the relevant information (bid count or procedure type); the risk index requires both. Regression samples additionally require non-missing lagged regressors and exclude singleton municipalities.

Table A2 Representativeness of the main regression sample

	In sample	Not in sample	Difference	p-value (Welch)
Population	15,877	11,713	4,165	0.028
Own-source revenue share (%)	25.9	25.1	0.7	0.204
EU capital-spending share (%)	6.05	5.46	0.59	0.004
Total revenue (PLN)	65,094,873	45,327,154	19,767,719	0.057
Share of city-county or urban municipalities (%)	12.5	9.3	3.2	0.125
Contracts per municipality-year	16.8	0.2	16.5	<0.001
Number of municipalities	2,254	225		

Notes: Municipality means over 2008-2019. 'In sample' denotes municipalities contributing at least one observation to the main single-bidding regression of Table 3. p-values from Welch two-sample t-tests.

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Table A3 Baseline specifications: building up from the bivariate model

	(1)	(2)	(3)	(4)
Single bidding				
Own-source revenue share (+10 pp), $t-1$	0.224	0.198	0.356	0.320
	(0.353)	(0.353)	(0.356)	(0.371)
EU capital-spending share (+10 pp), $t-1$		0.850***	0.678**	0.755***
		(0.218)	(0.270)	(0.280)
Incumbent unopposed (0/1)				−0.300
				(1.463)
Own-source share × unopposed incumbent				0.199
				(0.491)
Number of candidates				0.229
				(0.163)
log population, $t-1$			15.638**	10.916
			(6.530)	(7.170)
log population density, $t-1$			−11.190**	−7.229
			(4.582)	(5.095)
Unemployment rate (x10), $t-1$			1.379	0.825
			(1.428)	(1.497)
log real revenue per capita, $t-1$			−1.042	−1.091
			(1.680)	(1.717)
Debt / revenue, $t-1$			7.941***	8.072***
			(1.881)	(1.949)
Balance / revenue, $t-1$			−1.245	−0.880
			(2.084)	(2.138)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	19,383	19,383	19,383	18,159
Municipalities	2,202	2,202	2,202	2,190
Within R^2	0.000	0.001	0.003	0.003
Mean of dep. var.	28.80	28.80	28.80	28.68
SD of dep. var.	19.34	19.34	19.34	19.33

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Table A3 Baseline specifications: building up from the bivariate model (continued)

	(1)	(2)	(3)	(4)
Non-open procedures				
Own-source revenue share (+10 pp), $t-1$	0.688***	0.656***	0.589**	0.560**
	(0.238)	(0.235)	(0.243)	(0.249)
EU capital-spending share (+10 pp), $t-1$		0.597***	0.209	0.208
		(0.160)	(0.195)	(0.194)
Incumbent unopposed (0/1)				−1.032
				(0.942)
Own-source share × unopposed incumbent				0.344
				(0.293)
Number of candidates				−0.046
				(0.123)
log population, $t-1$			−8.149	−9.771*
			(5.251)	(5.552)
log population density, $t-1$			4.737*	8.579**
			(2.482)	(3.336)
Unemployment rate (x10), $t-1$			−1.878*	−1.548
			(1.106)	(1.095)
log real revenue per capita, $t-1$			2.881**	2.940**
			(1.182)	(1.171)
Debt / revenue, $t-1$			2.644*	2.832**
			(1.361)	(1.360)
Balance / revenue, $t-1$			−3.260**	−3.363**
			(1.460)	(1.449)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	16,723	16,723	16,723	16,625
Municipalities	2,159	2,159	2,159	2,157
Within R^2	0.001	0.002	0.004	0.004
Mean of dep. var.	8.20	8.20	8.20	8.18
SD of dep. var.	13.39	13.39	13.39	13.34

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Table A3 Baseline specifications: building up from the bivariate model (continued)

	(1)	(2)	(3)	(4)
Risk index				
Own-source revenue share (+10 pp), $t-1$	0.032**	0.029**	0.031**	0.029*
	(0.015)	(0.015)	(0.015)	(0.016)
EU capital-spending share (+10 pp), $t-1$		0.042***	0.021*	0.020*
		(0.010)	(0.012)	(0.012)
Incumbent unopposed (0/1)				−0.049
				(0.062)
Own-source share × unopposed incumbent				0.020
				(0.020)
Number of candidates				0.003
				(0.007)
log population, $t-1$			0.159	0.043
			(0.312)	(0.326)
log population density, $t-1$			−0.040	0.219
			(0.146)	(0.189)
Unemployment rate (x10), $t-1$			−0.026	−0.010
			(0.067)	(0.067)
log real revenue per capita, $t-1$			0.111	0.114
			(0.073)	(0.073)
Debt / revenue, $t-1$			0.307***	0.313***
			(0.084)	(0.085)
Balance / revenue, $t-1$			−0.168*	−0.175*
			(0.090)	(0.089)
Municipality fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Observations	16,699	16,699	16,699	16,601
Municipalities	2,158	2,158	2,158	2,156
Within R^2	0.000	0.002	0.004	0.004
Mean of dep. var.	−0.02	−0.02	−0.02	−0.02
SD of dep. var.	0.82	0.82	0.82	0.82

Notes: Column (3) is the specification reported in Table 3. Column (4) adds electoral competition in the next local election. Regressors lagged one year; municipality and year fixed effects; standard errors clustered by municipality. * p<0.10, ** p<0.05, *** p<0.01.

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Table A4 Heterogeneity in the effect of own-source revenue autonomy

	Single bidding	Non-open procedures	Risk index
Municipality type			
Rural	0.878** (0.434)	0.493* (0.267)	0.037** (0.017)
Urban-rural	−1.004 (0.684)	0.163 (0.537)	−0.014 (0.034)
Urban	0.501 (0.884)	1.793** (0.777)	0.081* (0.047)
City-county	−0.351 (2.425)	1.415 (3.486)	0.080 (0.178)
p-value: equal slopes	0.112	0.367	0.365
Holm-adjusted p (nine tests)	0.671	1.000	1.000
Population tercile			
Small	0.575 (0.618)	0.321 (0.339)	0.027 (0.023)
Medium	0.435 (0.556)	0.101 (0.357)	0.017 (0.022)
Large	0.070 (0.569)	1.399*** (0.485)	0.051* (0.029)
p-value: equal slopes	0.807	0.076	0.632
Holm-adjusted p (nine tests)	1.000	0.533	1.000
Period			
2008-2013	0.153 (0.361)	0.663*** (0.247)	0.030* (0.015)
2014-2019	1.113*** (0.419)	0.285 (0.279)	0.037** (0.018)
p-value: equal slopes	<0.001	0.044	0.533
Holm-adjusted p (nine tests)	0.005	0.354	1.000

Notes: Cells report group-specific coefficients on the own-source revenue share (+10 pp, lagged) from a single regression per outcome and dimension that interacts the regressor with group indicators; EU share and controls as in Table 3. p-value: joint Wald test of equal slopes (cluster-robust). Holm adjustment across the nine tests (three dimensions, three outcomes). Groups with fewer than 300 observations are omitted. * p<0.10, ** p<0.05, *** p<0.01.

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Table A5 Electoral competition as moderator of revenue autonomy

	Single bidding	Non-open procedures	Risk index
A. Unopposed incumbent in next election			
Own-source revenue share (+10 pp), $t-1$	0.332	0.557**	0.029*
	(0.371)	(0.249)	(0.016)
EU capital-spending share (+10 pp), $t-1$	0.757***	0.208	0.020*
	(0.280)	(0.194)	(0.012)
Incumbent unopposed (0/1)	−0.690	−0.955	−0.053
	(1.435)	(0.912)	(0.061)
Own-source share × unopposed incumbent	0.178	0.349	0.020
	(0.492)	(0.293)	(0.020)
Observations	18,159	16,625	16,601
Municipalities	2,190	2,157	2,156
B. Number of candidates in next election			
Own-source revenue share (+10 pp), $t-1$	0.266	0.480	0.025
	(0.528)	(0.319)	(0.021)
EU capital-spending share (+10 pp), $t-1$	0.749***	0.203	0.020*
	(0.279)	(0.194)	(0.012)
Number of candidates	0.129	−0.156	−0.004
	(0.395)	(0.259)	(0.017)
Own-source share × number of candidates	0.028	0.043	0.002
	(0.130)	(0.085)	(0.005)
Observations	18,159	16,625	16,601
Municipalities	2,190	2,157	2,156

Notes: Municipality and year fixed effects, controls as in Table 3, standard errors clustered by municipality. * p<0.10, ** p<0.05, *** p<0.01.

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Table A6 Measurement validity: results by quality of the contract-to-municipality match

	Observations	Own-source share	EU capital share	Interaction
REGON-matched share ≥ 0.5				
Single bidding	9,816	−0.036 (0.477)	0.395 (0.375)	0.472** (0.216)
Non-open procedures	9,163	0.683** (0.329)	0.250 (0.253)	0.269* (0.151)
Risk index	9,145	0.027 (0.020)	0.020 (0.016)	0.021** (0.009)
REGON-matched share < 0.5				
Single bidding	9,307	0.395 (0.552)	0.833** (0.409)	0.419 (0.278)
Non-open procedures	7,417	0.652* (0.372)	0.233 (0.305)	0.183 (0.200)
Risk index	7,411	0.038 (0.024)	0.027 (0.019)	0.017 (0.012)
REGON-matched share ≥ 0.8				
Single bidding	9,334	0.018 (0.490)	0.415 (0.384)	0.474** (0.221)
Non-open procedures	8,769	0.717** (0.336)	0.326 (0.256)	0.254 (0.155)
Risk index	8,752	0.031 (0.020)	0.022 (0.016)	0.020** (0.010)
Matched by name only (REGON share = 0)				
Single bidding	8,401	0.519 (0.578)	0.640 (0.434)	0.288 (0.292)
Non-open procedures	6,830	0.533 (0.376)	0.084 (0.309)	0.129 (0.189)
Risk index	6,825	0.032 (0.025)	0.017 (0.020)	0.011 (0.012)

Notes: REGON-matched share: fraction of a municipality-year's contracts assigned to the municipality through the buyer's REGON identifier (as opposed to buyer-name matching). Interaction: own-source x EU capital share, both centred within the sub-sample. Municipality and year fixed effects; controls as in Table 3; standard errors clustered by municipality. * p<0.10, ** p<0.05, *** p<0.01.

Table A7 Agreement between REGON-matched and name-matched contracts within the same municipality-year

	Overlapping municipality-years	Municipalities	Pearson correlation	Spearman correlation	Correlation, year-demeaned	Mean: REGON / name (%)
Single-bidding share	436	262	0.101	0.130	0.091	30.1 / 34.2
Non-open procedure share	274	143	0.128	0.101	0.115	11.1 / 8.7

Notes: The two measures are computed from disjoint sets of contracts of the same municipality-year (each with at least 5 contracts); correlations are therefore attenuated by sampling noise and are informative about whether name matching assigns contracts to the correct municipality, not about identity of measures.

Table A8 Validity of the EU capital-spending proxy: share of contracts that are EU-funded

	Coefficient (+10 pp)	Std. error	p-value	Observations	Mean of dep. var. (%)
EU capital share at t	5.172***	0.280	<0.001	15,400	23.30
EU capital share at $t-1$	−0.005	0.354	0.988	15,400	23.30
EU capital share at t (joint model)	5.231***	0.283	<0.001	15,400	23.30
EU capital share at $t-1$ (joint model)	−0.697**	0.349	0.046	15,400	23.30

Notes: Dependent variable: percentage of contracts flagged as EU-funded in the procurement file (municipality-years with at least 5 flagged-or-known contracts, 2008-2016). Municipality and year fixed effects, own-source share and controls included; standard errors clustered by municipality. * p<0.10, ** p<0.05, *** p<0.01.

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Table A9 Exploratory fixed-effects threshold models with own-source revenue share as threshold variable

	Threshold (% of revenue)	95% LR set	Share in low regime (%)	Sup-F	Bootstrap p	Holm p	Own-source effect, low regime	Own-source effect, high regime	p: regimes equal
Single bidding	21.3	[20.0, 39.0]	35.2	14.48	0.066	0.162	−0.546 (0.540)	−0.108 (0.379)	0.157
Non-open procedures	27.9	[12.9, 42.7]	56.3	13.18	0.121	0.162	1.128^{***} (0.335)	0.641^{**} (0.262)	0.004
Risk index	21.8	[18.9, 39.2]	35.2	15.55	0.054	0.162	0.022 (0.023)	0.015 (0.016)	0.610

Notes: Hansen (1999) fixed-effects threshold regression; regime-dependent coefficients on own-source and EU capital-spending shares, common controls and year effects. Threshold searched over the 5th-95th percentile of the lagged own-source share. Bootstrap p-values from 999 wild-cluster (Rademacher, by municipality) replications under the linear null; Holm adjustment across three outcomes. LR set: 95% confidence set from the likelihood-ratio statistic. Exploratory analysis; not used for inference in the main text. A threshold model with EU capital spending as threshold variable was estimated but is not reported because the high-regime sample is too small to identify the regime coefficients.

Table A10 Exploratory panel smooth-transition regression with own-source revenue share as transition variable

	Slope γ	Location c (% of revenue)	Parameter at bound	LM-F p (linearity)	Wald p (linearity)	Holm p	p: remaining nonlinearity
Single bidding	13.08	22.6	No	0.031	0.054	0.115	0.787
Non-open procedures	12.11	28.2	No	0.075	0.140	0.140	0.794
Risk index	400.00	21.8	Yes	0.016	0.038	0.115	0.912

Table A10 Exploratory panel smooth-transition regression with own-source revenue share as transition variable (continued)

	EU effect at P10	EU effect at P50	EU effect at P90
Single bidding	0.015 (0.374)	1.115*** (0.311)	1.150*** (0.318)
Non-open procedures	−0.053 (0.215)	0.000 (0.203)	0.527* (0.288)
Risk index	−0.015 (0.015)	0.042*** (0.014)	0.042*** (0.014)

Notes: Logistic PSTR (González, Teräsvirta and van Dijk 2005) with municipality fixed effects, year dummies and controls; slope γ is scaled by the SD of the transition variable. Marginal effects of a +10 pp EU capital-spending share at percentiles of the own-source share. 'Parameter at bound' flags estimates at the edge of the admissible parameter region, for which the transition-function parameters are not reliably identified. Exploratory. * p<0.10, ** p<0.05, *** p<0.01.

Table A11 Stacked event study around the first EU investment drive

	ATT (treated × post)	p: pre-trend ($t-3$, $t-2$)	Observations	Treated municipalities
S3_EU: Single bidding	0.834 (1.162)	0.737	39,181	189
S3_EU: Non-open procedures	1.064 (0.846)	0.533	39,218	189
S3_EU: Risk index	0.064 (0.050)	0.614	39,181	189
S3_EU: log(1 + contracts)	0.037 (0.045)	0.529	52,157	220
I40_EU: Single bidding	5.293*** (1.834)	0.385	41,899	85
I40_EU: Non-open procedures	4.065*** (1.290)	0.019	41,935	85
I40_EU: Risk index	0.289*** (0.074)	0.566	41,899	85
I40_EU: log(1 + contracts)	0.032 (0.075)	0.217	55,716	91

Notes: Stacked cohort design (cohorts 2011-2014; not-yet-treated municipalities as controls; observations up to 2015 because the investment-drive information is unavailable for 2016-2017). Fixed effects for municipality-by-cohort and cohort-by-year; standard errors clustered by municipality. S3_EU and I40_EU are alternative definitions of a municipality's first EU investment drive. Pre-trend: joint Wald test of leads $t-3$ and $t-2$ relative to $t-1$. * p<0.10, ** p<0.05, *** p<0.01.

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Table A12 Instrumental-variable inference on the effect of EU capital spending: Anderson-Rubin confidence sets

	OLS coefficient	First-stage F (cluster-robust)	95% Anderson-Rubin set	Observations
Single bidding: Shift-share	0.678	4.81	unbounded	19,378
Single bidding: Shift-share + 2010-15 exposure	0.678	3.15	unbounded	19,378
Non-open procedures: Shift-share	0.209	3.25	unbounded	16,718
Non-open procedures: Shift-share + 2010-15 exposure	0.209	8.33	[−9.71, 16.73]	16,718
Risk index: Shift-share	0.021	3.19	unbounded	16,694
Risk index: Shift-share + 2010-15 exposure	0.021	8.26	[−0.51, 1.04]	16,694

Notes: Instruments: baseline (2004-2007) municipal EU exposure interacted with the national (excluding own voivodeship) year-specific EU spending shift, and optionally with a 2010-2015 indicator. First-stage F below 10 indicates weak identification; Anderson-Rubin sets remain valid under weak instruments. 'Unbounded' denotes that the confidence set covers the entire search grid (±4 SD of the outcome) and the instruments are uninformative; 'empty set' denotes rejection of the over-identifying restriction at every grid value. Reported for transparency; not used for inference in the main text.

Table A13 Leave-one-voivodeship-out estimates of the own-source × EU capital-spending interaction

	Single bidding	Non-open procedures	Risk index
Excluding Lower Silesian	0.460*** (0.176)	0.246** (0.125)	0.021*** (0.008)
Excluding Kuyavian-Pomeranian	0.355** (0.171)	0.213* (0.120)	0.016** (0.007)
Excluding Lublin	0.407** (0.176)	0.271** (0.127)	0.020*** (0.008)
Excluding Lubusz	0.381** (0.172)	0.240* (0.123)	0.018** (0.008)
Excluding Lodz	0.400** (0.176)	0.230* (0.123)	0.019** (0.008)
Excluding Lesser Poland	0.386** (0.173)	0.166 (0.117)	0.016** (0.007)
Excluding Masovian	0.444** (0.178)	0.228* (0.126)	0.020*** (0.008)
Excluding Opole	0.388** (0.170)	0.246** (0.120)	0.019** (0.007)
Excluding Subcarpathian	0.386** (0.174)	0.267** (0.123)	0.019** (0.008)
Excluding Podlaskie	0.425** (0.173)	0.232* (0.125)	0.019** (0.008)
Excluding Pomeranian	0.375** (0.175)	0.248** (0.126)	0.019** (0.008)
Excluding Silesian	0.408** (0.170)	0.207* (0.118)	0.017** (0.007)
Excluding Swietokrzyskie	0.367** (0.174)	0.223* (0.123)	0.017** (0.008)
Excluding Warmian-Masurian	0.348** (0.172)	0.240** (0.122)	0.017** (0.008)
Excluding Greater Poland	0.416** (0.173)	0.240* (0.124)	0.019** (0.008)
Excluding West Pomeranian	0.399** (0.178)	0.265** (0.128)	0.020** (0.008)

Notes: Each row drops all municipalities of one voivodeship and re-estimates the interaction model of Table 8, column (1)-(3). Standard errors clustered by municipality. * p<0.10, ** p<0.05, *** p<0.01.

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Table A14 Sample selection: inverse-probability weighting diagnostics

	Candidate observations	Selected into outcome sample	Mean selection probability	Mean weight	Maximum weight	Unweighted coefficient	IPW coefficient
Single bidding	29,738	19,435	0.654	1.51	10.56	0.356 (0.356)	0.290 (0.395)
Non-open procedures	29,738	16,783	0.564	1.58	20.00	0.589** (0.243)	0.392 (0.295)
Risk index	29,738	16,760	0.564	1.58	20.00	0.031** (0.015)	0.016 (0.017)

Notes: Selection (outcome observed, i.e. at least 5 contracts with the relevant information) is modelled by a logit on lagged fiscal and demographic controls, own-source and EU shares, and year, municipality-type and voivodeship dummies; weights are the inverse of the predicted probability (floored at 0.05). Coefficients refer to the own-source revenue share (+10 pp). * p<0.10, ** p<0.05, *** p<0.01.

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Table A15 Variable definitions and sources

Variable	Definition	Source
Single-bidding share (%)	Percentage of contracts in a municipality-year awarded with exactly one bid, among contracts with bid-count information; defined if at least 5 such contracts.	Public-procurement tender records
Non-open procedure share (%)	Percentage of contracts awarded through a procedure type other than the open procedure, among contracts with procedure information; at least 5 contracts.	Public-procurement tender records
Risk index	Mean of the standardised single-bidding and non-open shares (z-scores over the estimation window); requires both components.	Constructed
Own-source revenue share	Own-source revenue excluding shared personal and corporate income-tax receipts, divided by total revenue; winsorised at 1%/99%; expressed in units of 10 pp.	Municipal budget data
Local taxes and fees share	Revenue from local taxes and fees divided by total revenue (units of 10 pp).	Municipal budget data
Non-tax own revenue share	Own-source revenue excluding shared taxes, less local taxes and fees, divided by total revenue (units of 10 pp).	Municipal budget data
EU capital-spending share	Capital expenditure financed with EU funds divided by total expenditure; winsorised at 1%/99%; units of 10 pp.	Municipal budget data
Population, density	Resident population; persons per square kilometre (enter in logs).	Statistics Poland, Local Data Bank
Unemployment rate	Registered unemployed relative to working-age population (units of 10 pp).	Statistics Poland, Local Data Bank
Real revenue per capita	Total revenue deflated to 2015 prices, per resident (enters in logs).	Municipal budget data; deflator
Debt and balance	Municipal debt and budget balance, each divided by total revenue; winsorised at 1%/99%.	Municipal budget data
Unopposed incumbent	Indicator that the incumbent is the only candidate in the next local election.	Local election records
Number of candidates	Number of candidates in the next local election, capped at 6.	Local election records
REGON-matched share	Share of a municipality-year's contracts assigned through the buyer's REGON identifier rather than buyer-name matching.	Constructed
Municipality type	City-county, urban, rural or urban-rural municipality.	Statistics Poland classification

Notes: All regressors except those labelled contemporaneous are lagged one year.