Revisiting the Link between Politicians' Salaries and Corruption

November 26, 2025

Abstract

It has long been argued that paying politicians higher salaries should help decrease corruption. However, the empirical evidence is mixed, partly due to the large variation in contexts, research designs, conceptual definitions and measures of corruption, and the predominance of case studies with potentially limited generalizability. To alleviate those challenges, we evaluate uniformly defined and validated corruption risk indicators from an original dataset of more than 2.4 million government contracts in 11 EU countries, covering more than half of its population and GDP. To aid causal identification, we exploit sizable changes in salaries of local politicians tied to population size across close to 100 discrete salary thresholds. Applying fixed effects estimators, regression discontinuity, and difference-in-discontinuities designs, we consistently find that better-paid local politicians (by about 15% on average) oversee less risky procurement contracts, by a third to one standard deviation on our measure of corruption risk.

Word count (excluding references): 4,490

Well-known arguments underpin the claim that paying politicians higher salaries should help reduce corruption—the misuse of public resources for personal or political gain. Higher pay increases the value of holding office, imposing greater opportunity costs of corruption (Shapiro and Stiglitz, 1984). By lowering the consumption-related need for bribes, higher salaries also decrease their marginal utility (Becker and Stigler, 1974). In addition to improving incentives, better pay may have positive selection effects by attracting higher-quality candidates who, presumably, are also less rapacious (Besley, 2004; Bond, 2008).

Empirical evidence, however, is mixed. While some studies report positive effects of higher salaries on incentives or selection, whether with respect to corruption specifically (Di Tella and Schargrodsky, 2003; Klašnja, 2015) or related political outcomes more broadly (Ferraz and Finan, 2009; Gagliarducci and Nannicini, 2013; Kotakorpi and Poutvaara, 2011; Van Rijckeghem and Weder, 2001), others find no such links (Alt and Lassen, 2012; Dahlström, Lapuente and Teorell, 2012; Treisman, 2007), ambiguous effects (Braendle, 2015; Fisman et al., 2015) or even the opposite effect (Pique, 2019). This mixed evidence may in part reflect the plausibility of countervailing arguments. Wages may need to be set at prohibitively high levels to produce the intended corruption-reducing effects (Besley and McLaren, 1993). Potential bribers may attenuate the beneficial salary effects by increasing bribes (Mookherjee and Png, 1995) or even threats (Pulejo and Querubín, Forthcoming). And higher wages may even lead to negative selection if they attract less publicly-spirited candidates (Besley, 2004), who may be more prone to rent-seeking (Barfort et al., 2019; Hanna and Wang, 2017).

This said, the mixed evidence may also in part be due to the large variation in the contexts, definitions, measures, and methodological approaches in the existing literature. Most previous studies focus on single-country cases, making it hard to assess their generalizability. Studies with broader scope conditions are typically correlational, limiting our ability to ascribe causality. Moreover, existing works vary widely in the types of outcomes examined, some of which are only indirectly related to corruption. Our contribution in this letter is to provide new evidence drawing on comprehensive data covering eleven European countries

over more than a decade, featuring comparable and well-tailored measures of corruption. In addition, we leverage a large number of plausible natural experiments in the determination of politicians' salaries, which combined with the use of several research designs allows us to capture plausibly general and causal estimates. In what follows, we describe our novel data, methods and results, with many additional details described in the Supplementary Appendix (henceforth SA).

Study Design: Sample, Data, Measures, and Empirical Strategies

Estimating the effects of politicians' salaries is difficult because of endogeneity problems arising from a variety of strategic and contextual factors that determine salary levels. To address these challenges, we searched for all instances in countries in the EU of changes in salaries of local government executives (led by mayors and co-governed by municipal councils)[1] tied to the population of their municipalities. This setup is advantageous because the formula-based determination of remuneration provides a plausibly exogenous source of variation in salaries. We identified 97 such instances where salaries change discontinuously at discrete population thresholds in 11 countries: Austria, Belgium, France, Hungary, Italy, Lithuania, Portugal, Romania, Slovakia, Slovenia, and Spain.[2] All thresholds are listed in Supplementary Appendix (SA) Section A.

This approach has been utilized in prior research studying the effect of salaries on governance outcomes (Klašnja, 2015; Pulejo and Querubín, Forthcoming; Pique, 2019).[3] Our contribution is to draw on a significantly larger number of cases, as our sample comprises close to 18,000 municipalities and covers countries accounting for more than half of the EU's population and half of its GDP. The breadth of our sample gives us rich and meaningful variation in salaries. Panel (a) of Figure 1 shows that the average increase in mayoral

---

[1] In SA Section C.1.7, we show that our results remain robust to considering mayors separately from councils.
[2] We focus on the EU because the relative similarity in regulatory environments, economic development, and data quality and availability allow us to plausibly rule out a variety of hard-to-condition-on contextual factors.
[3] Other studies examine the effects of salaries on different policy outcomes; see Eggers et al. (2018) for a recent review.

monthly salary across all salary thresholds is approximately 15 percent (from about PPP
€2,650 to €3,050). Salary jumps vary between less than 1 percent and 95 percent, with the
inter-quartile range of 5-30 percent (SA Figure C2).

Our focus on local executives is warranted on more than just research design grounds.
Local governments in the EU have substantial policy responsibilities in the provision of public
goods and services, such as the maintenance of local infrastructure and utilities and the
administration of local safety, health, education, and social protection (Kuhlmann, Dumas
and Heuberger, 2022). In the countries in our sample, local governments account for a
fifth of total public expenditures and revenues, two-fifths of public investments, and a third
of the total value of government contracting.[4] Local executives are crucial in formulating
and implementing these policies, including public procurement, which is highly discretionary
and thus subject to considerable corruption risks. SA Section B gives an overview of the
organization, resources, and responsibilities of local executives in each country, highlighting
their central policy roles and powers.

Given the local politicians' role in the allocation of government resources, we focus on
measuring corruption in local government spending. We assembled a large original dataset
of more than 2.4 million public procurement contracts in the countries in our sample over
the period 2006-2020. About a third of government spending in the EU is implemented
through public procurement—around €2 trillion annually.[5] We collected the data directly
from official government sources and open data repositories using web scraping algorithms
(Fazekas et al., 2024).[6] Using the information on contracting authorities, we focus on ten-
ders administered by local governments (accounting for around a third of all procurement),

---

[4]Local governments account for similar shares in the whole EU; see: OECD, 2018, "Key Data on Local and
Regional Governments in the European Union," https://www.oecd.org/regional/EU-Local-government-
key-data.pdf.

[5]See: European Court of Auditors, "Special report 28/2023: Public procurement in the EU," https://www.
eca.europa.eu/en/publications?ref=SR-2023-28.

[6]The data can be accessed at the https://opentender.eu website. It is processed through a comprehensive
and robust cleaning pipeline to ensure comparability across different national regulatory frameworks and
data infrastructures.

geocoding each contract and linking it to the data on local politicians' salaries and salary thresholds. SA Table C1 gives details for each country sample.

We define six corruption risk indicators that approximate separate, but often correlated aspects of corrupt deals in public procurement. In terms of procurement procedure, we track: (a) the use of non-open (i.e. non-competitive) tender procedures, (b) the absence of call-for-tender announcements, (c) excessively short advertisement periods, and (d) excessively short award decision periods. With respect to tender outcomes, we track two indicators: presence of only a single bidder, and the spending concentration of contracting authorities (i.e. local market concentration). In isolation, these indicators may reflect idiosyncratic, benign tender characteristics rather than corruption. To reduce the potential for such misinterpretation, we average the indicators into a composite procurement Corruption Risk Index (CRI) that varies between 0 and 1, with higher values indicating higher risk. Therefore, we assume that a contract characterized by multiple red flags is more likely to represent a corrupt tender. The index varies considerably between and within countries in our sample. SA Section C.1.2 provides more details for each indicator and the composite index.

The index is comparable across countries and has been empirically validated. For example, it correlates well with survey-based measures of corruption and is strongly predictive of overpricing in procurement tenders (Fazekas, Tóth and King, 2016; Fazekas and Kocsis, 2020). The CRI therefore hues closer to measuring resource misallocation than the measures in much of the prior literature on salaries in which some of the outcomes are only indirectly related to or themselves influenced by corruption, such as government size, local investment, or political competition (Ferraz and Finan, 2009; Gagliarducci and Nannicini, 2013; Pique, 2019). Others also use procurement-based performance or manipulation indicators (Klašnja, 2015; Pulejo and Querubín, Forthcoming); however, unlike the CRI, some are context-specific and cannot be constructed more broadly (e.g. bunching around regulation-specific contract value thresholds).

Using this data, our first empirical strategy is to estimate the following fixed effects model:

$$Y_{imtc} = \alpha_{imtc} + \beta S_{mtc} + \delta X_{mtc} + \tau_{mtc} + \xi_m + \eta_t + \epsilon_{imtc}, \qquad (1)$$

where Y is the CRI for contract i in municipality m in year t and country c; S is the mayoral salary in the same municipality and year (expressed in base-2020 PPP euros and logged); X is the municipal population (logged), τ indicate the closest population-based salary threshold, ξ and η are the municipality and year fixed effects, respectively. We estimate the model with OLS and cluster the standard errors at the municipality level.

The key coefficient, $\hat{\beta}$, utilizes both the within- and across-municipality (and across-threshold) variation in mayoral salaries,[7] allowing us to use our large dataset to comprehensively describe the association between the population-driven variation in salary and the CRI. However, there may be various unobserved, time-varying characteristics of municipalities (or their politicians) that the model may not account for.[8] To overcome that challenge, we exploit the discontinuous nature of the increases in salary to apply a regression discontinuity (RD) design. This approach aims to estimate the causal effect of salary by analyzing corruption outcomes in plausibly comparable municipalities very close to the salary thresholds. Our RD model is as follows:

$$Y_{imtc} = \alpha_{imtc} + \theta \mathbf{1}[\tilde{X} \geq 0]_{mtc} + \gamma f(\tilde{X})_{mtc} + \lambda \left(\mathbf{1}[\tilde{X} \geq 0]]_{mtc} \times f(\tilde{X})_{mtc} \right) + \psi_c + \eta_t + \epsilon_{imtc}, \quad (2)$$

where variables with the same notation as in equation 1 have the same interpretation; $\tilde{X} = \frac{X - x_0}{x_0}$ is a municipality's percent distance in population X to the closest threshold x_0;[9] ψ are the country dummies. $\hat{\theta}$ captures the effect of a salary increase at the appropriate population-

---

[7] The threshold indicators τ are collinear with the municipality fixed effects η except when identifying the instances of municipalities changing thresholds over time.

[8] We have comparable data for a limited set of time-varying municipality characteristics, but with relatively large shares of missing values (see SA Section C.1.4). We therefore omit those covariates from the model in equation 1.

[9] The cutoff for the closest salary threshold is defined as the mid-point between the two adjacent thresholds.

based threshold. Since we have thresholds at various population sizes, we normalize the distance of each municipality to a threshold so that the cutoff is zero for all municipalities. Therefore, $\hat{\theta}$ represents a weighted average of the local average treatment effects across all the included salary thresholds (Cattaneo et al., 2016).
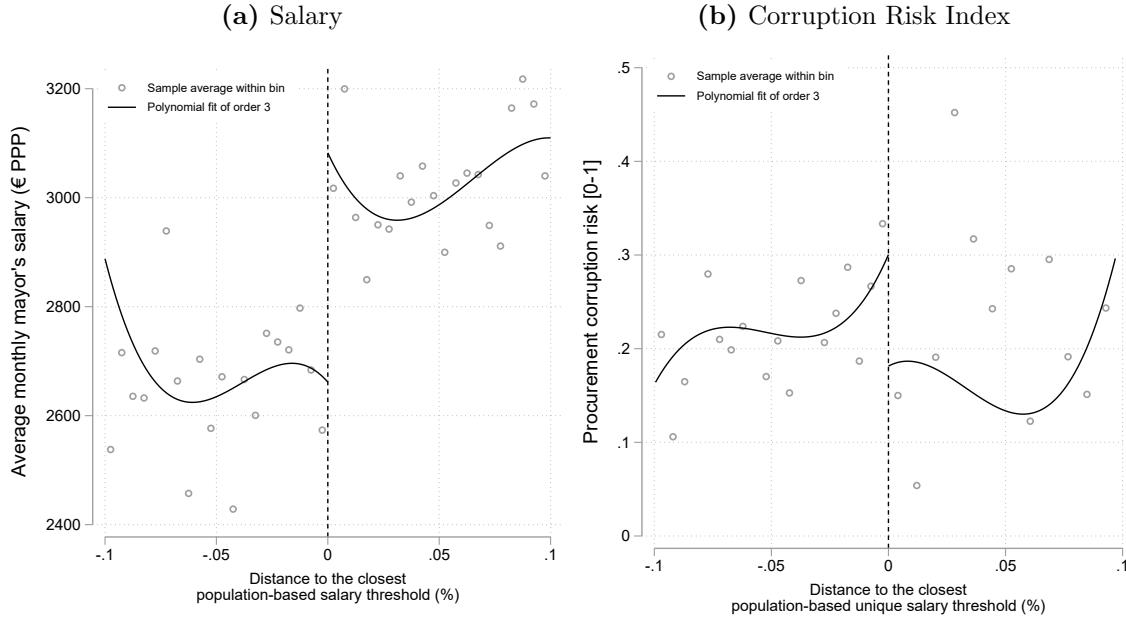
We employ local linear regressions (i.e. $f(\tilde{X})$ is of polynomial order 1) with a triangular kernel fit within the MSE-optimal bandwidth selected by the robust, data-driven procedure by Calonico, Cattaneo and Titiunik (2014) (our key result is insensitive to bandwidth size, as shown in SA Figure C4). We report the bias-corrected point estimates and standard errors, clustered at the locality level. The key identifying assumption is that the potential outcomes are continuous and smooth across each threshold, implying that: (a) except mayoral salary, no other (predetermined) municipality characteristics change discontinuously at the cutoff; (b) politicians do not sort their municipalities precisely on one or the other side of a salary threshold. We evaluate these implications in detail in SA Section C.1.4. Observable municipal characteristics, including lagged corruption risk indicators, are broadly balanced, and there is no evidence of potential strategic sorting.[10]

Europe-wide Results

Column 1 of Table 1 shows the results of our FE model from equation 1. Using the full sample of contracts and municipalities, a one percent increase in mayoral salary is associated with a precisely estimated decrease in the CRI of 0.019 points, or about 1/13 of the sample standard deviation in CRI. Based on this estimate, an average increse in salary of 15 percent (panel (a) of Figure 1) is associated with slightly more than a one standard deviation decrease in the CRI. To verify that this association is not idiosyncratically driven by municipalities with very low numbers of tenders, the remaining columns limit the sample to municipalities with a progressively larger minimum number of contracts per year. The estimated association is similar, and even somewhat larger.

---

[10]That said, our key result seems robust to potential sorting very close to the cutoff (SA Section C.1.5).

Figure 1: RD Plots of Change in Average Mayoral Salary and Corruption Risk Index at the Cutoff

(a) Salary   **(b)** Corruption Risk Index

Notes: Panel (a) includes all population-based salary thresholds; panel (b) includes only the unique thresholds described in the text. Dots are averages of the y variable within small ranges (or "bins") of the x (or "running") variable, which represents the percent distance of a town to the closest salary threshold (indicated by the vertical dashed line); lines on each side of the discontinuity are cubic best-fit curves fit to the data (separately on the left and right of the discontinuity). Bins of the running variable are determined with the data-driven procedure by Calonico, Cattaneo and Titiunik (2015).

Table 1: Higher Salaries Associated with Lower Procurement Corruption Risks

	All localities	At least 5 tenders/year	At least 10 tenders/year	At least 20 tenders/year
FE estimate	-0.019	-0.027	-0.024	-0.025
St. error	(0.007)	(0.007)	(0.007)	(0.006)
p-value	[0.009]	[0.000]	[0.000]	[0.000]
N	2,429,589	1,839,024	1,696,290	1,486,526

Notes: Outcome is the corruption risk index (CRI), described in the text. Model specification is given in equation 1. Standard errors are clustered by municipality.

We next turn to the RD analyses. To unambiguously ascribe the effect to politicians' salary, no other policy may change at the same thresholds (Eggers et al., 2018). This is the case for 25 of the 97 thresholds in our data, across six of the eleven countries. Henceforth, we

call these thresholds 'unique.'[11] The remaining thresholds entail compound treatments that include the effect of one or more other policies varying along with salaries, such as the size of the municipal assembly or fiscal transfers from the central government. We therefore face a trade-off: unique thresholds provide greater clarity and internal validity, but limit the sample size and generalizability of an already local RD estimand. Our preferred sample is of the unique thresholds; however, we also report the results for a broader sample of ('compound') thresholds with no more than two other policies co-varying (53 thresholds covering ten of the eleven countries), as well as all the thresholds (as in the FE analysis). SA Section A indicates which threshold belongs to each sample.

Panel (b) of Figure 1 shows descriptively the change in the CRI at the cutoff across the unique thresholds. Similar to the correlations from the FE model, there is a clearly identifiable drop, from around .3 (on a 0-1 scale) below the salary discontinuity to .18 immediately above it. That said, there is a fair bit of variability in the CRI, especially above the cutoff (as indicated by the dots which summarize in small 'bins' of the running variables the unplotted raw values of CRI for each contract). In Table 2, we therefore report the RD effects using the model in equation 2. The first column of Table 2 shows the effect across the unique thresholds.[12] At the cutoff, the CRI decreases by 0.078 points ($p < 0.01$), from about .12 to .04 (on a 0-1 scale)—a 67 percent decrease.[13] Since this estimate draws on a very limited sample of contracts (from 165 municipalities, or about one percent of municipalities in our data), column 2 uses the sample of compound thresholds that includes slightly more than half of all thresholds.[14] The estimated effect drops roughly by 35 percent but remains precisely estimated (-0.051, or about a 20 percent decrease, $p < 0.01$). Column 3 reports the effect using the entire sample, with the caveat that the confounding may be substantial given that (many) other policies vary alongside mayoral salaries. The

---

[11]We count the thresholds where salaries of other municipal politicians co-vary as unique. See SA Section A and SA Table C5 for more details.

[12]We exclude localities far away from a threshold (with the percent distance larger than 1), mainly big cities with many contracts but less plausible counterfactuals.

[13]Results for each component red flag are shown in SA Section C.1.11.

[14]In analyses with compound thresholds, we add as controls the threshold dummies.

point estimate is smaller and less precisely estimated, but still suggests a drop in the CRI at the cutoff of 0.02 points, or about 5 percent. Altogether, these results point to a plausible negative effect of higher salaries on procurement corruption risk, ranging in magnitude from relatively small to relatively large.

Table 2: Higher Salaries Reduce Procurement Corruption Risks

	Unique thresholds	Compound thresholds	All thresholds
RD estimate	-0.078	-0.051	-0.021
St. error	(0.017)	(0.019)	(0.012)
p-value	[0.000]	[0.007]	[0.065]
Bandwidth	0.111	0.133	0.083
N	2,430	31,007	127,232

Notes: Outcome is the corruption risk index (CRI), described in the text. Model specification is given in equation 2. Standard errors are clustered by municipality.

These negative effects may be driven not by the decrease in corruption risks but a change in the composition of contracts that may impact our red flag indicators. For example, an average tender in higher-salary municipalities may be of higher value, which in turn may attract fewer bidders (for example, because they involve more complex products or services), and fewer bids may justify shorter submission and decision periods, or result in greater spending concentration. The RD design, however, should rule out these compositional differences. SA Table C6 confirms this, as we observe no salary effect on average contract value, contract volume or average procurement expenditures.

To further assess the plausibility of our results, we evaluate three additional implications. First, if the effects are indeed driven by the sensitivity of corruption to wages and not some other aspects of municipalities correlated with changes in salaries, we should observe stronger effects at thresholds with larger salary increases. SA Table C7 confirms this, as the negative effect is larger at the larger-increase thresholds. Second, if CRI captures the risk of corruption rather than some other characteristic of contracts, markets or municipalities, we should observe a stronger effect where corruption is a greater concern. SA Table C8 confirms

this, as the negative salary effect is concentrated at thresholds in municipalities located in more corrupt EU regions. Third, since corruption increases waste in public spending, we should observe that higher salaries increase the efficiency in procurement. CA Table C10 (column 1) confirms this: at the cutoff, the average ratio of the final bid price to the ex-ante estimated price falls by about 5 percentage points.

Evidence from Difference-in-Discontinuities in Romania

To further alleviate concerns about the influence of unobserved differences across municipalities (even in the vicinity of salary thresholds), we employ an alternative design that exploits *changes* in the location of a number of population-based salary thresholds in Romania. In 2018, six new salary thresholds were introduced and four existing thresholds abolished, with three thresholds remaining unchanged (SA Section C.2.1 gives more details). This setup allows for a difference-in-discontinuities design, where we can compare trends in the CRI close to the salary thresholds among two groups: localities affected (treatment group) and unaffected (control group) by the 2018 reform.[15] SA Section C.2.2 shows that the key (additional) identifying assumption of parallel trends is plausible, and that predetermined locality characteristics in both treatment and control groups are well-balanced.

If higher salaries indeed lead to lower procurement corruption, we should observe a persistent negative RD effect in the control group, but a change in the effect in the treatment group in line with the change in the salary thresholds. Patterns in Table 3 are consistent with these expectations. We run RD models similar to that in equation 2.[16] Standard errors are again clustered by locality. Column 1 reports the placebo salary effect: it compares the CRI in localities around the cutoff in the treatment group either before the salary thresholds have been established or after they have been abolished. As we should expect, the RD effect is essentially zero, both substantively and statistically. By contrast, in column 2 the effect

---

[15]There were similar changes in Belgium, Lithuania, and Spain, but they happened either before or after the period for which we have procurement data.

[16]Obviously, we exclude country dummies but control for the indicator variables for each salary threshold.

in the presence of actual salary thresholds (after their introduction or before their abolition) is much larger and statistically more precise, with CRI decreasing by 0.2 points, or about two-thirds ($p < 0.05$).[17] Columns 3-6 separate these effects by whether the thresholds were removed or introduced. In both cases, the patterns are consistent with expectations. In localities where the new thresholds were introduced in 2018, we see no difference in the CRI before the reform but a clear negative effect afterwards (-0.180, $p < 0.01$). In localities where the thresholds were removed in 2018, we see a large negative effect before the removal (-0.238, $p = 0.07$) but a significantly smaller and noisier difference thereafter (-0.109, $p = 0.49$).[18]

Finally, in columns 7-9 we report the results for the localities in the control group, where the salary thresholds remained unchanged throughout the period. As expected, we observe a negative salary effect both before and after 2018, and the point estimates vary only slightly in magnitude (though somewhat more in precision). In sum, the patterns in Table 3 are consistent with our cross-national results.

Table 3: Similar Salary Effects from Difference in Discontinuities in Romania

	Treatment group						Control group		
	All thresholds		*Introduced in 2018*		*Eliminated in 2018*		*No change*		
	No threshold	Threshold	Pre-2018	2018-2020	Pre-2018	2018-2020	Pre-2018	2018-2020	All years
RD estimate	-0.008	-0.196	0.012	-0.180	-0.238	-0.109	-0.143	-0.122	-0.140
St. error	(0.079)	(0.098)	(0.061)	(0.059)	(0.129)	(0.159)	(0.083)	(0.059)	(0.069)
p-value	[0.917]	[0.045]	[0.844]	[0.002]	[0.065]	[0.493]	[0.084]	[0.039]	[0.043]
Bandwidth	0.091	0.124	0.116	0.069	0.109	0.096	0.134	0.220	0.139
N	27,885	16,161	28,235	7,032	5,695	1,874	34,872	9,089	42,517

Potential mechanisms

What mechanism may be driving the observed negative effect of higher wages on corruption? As mentioned earlier, the literature outlines two common channels: (electoral) incentives and selection (Besley, 2004; Gagliarducci and Nannicini, 2013). The incentive mechanism

---

[17]The effect sizes are different from the cross-national analyses in part because the CRI in Romania includes fewer red flag indicators. See SA Section C.2.2.

[18]That the estimate post-reform in localities where the thresholds were removed is still negative and relatively large suggests the possibility of lingering effects. This is a fruitful avenue for future research.

works through higher salaries augmenting reelection concerns because of the higher continuation value of occupying public office. The selection mechanism works through higher wages attracting higher-quality candidates, who may be less prone to engaging in corruption. Following Gagliarducci and Nannicini (2013), we explore the relative weight of these two mechanisms by exploiting the existence of mayoral term limits in two of the eleven countries (Italy and Portugal; SA Section C.3 provides institutional details). If reelection concerns play an important role, we should observe a weaker negative effect of higher salary on corruption among term-limited than among reelection-eligible incumbents, since reelection incentives are absent for the former but not the latter. However, if the selection mechanism predominates, we should observe similar salary effects irrespective of a mayor's term (for mayors serving multiple terms), as the wage effects would be driven by the (presumably time-invariant) quality differences between better-paid and worse-paid mayors. While our analyses are constrained by the absence of term limits in other countries, limited sample size, and data comparability challenges, the results (SA Table C13) suggest that selection is the dominant channel. These patterns are consistent with prior findings (Gagliarducci and Nannicini, 2013), albeit for fiscal rather than corruption outcomes and in a narrower sample.

That reelection incentives seem to play a minor role does not necessarily mean they cannot moderate the link between wages and corruption. Specifically, electoral incentives and wages may act as substitutes, so that wages matter more when mayors face weaker accountability constraints but less when such constraints are strong. We tentatively evaluate this possibility by exploiting another source of both cross-country and within-country institutional variation (described in SA Section C.3): whether mayors are elected directly by voters or indirectly through local assemblies. Direct elections may provide strong enough electoral incentives that the additional impact of salaries might be small, and vice versa for indirect elections.[19] The results (shown in SA Table C14) are mixed, though potentially consistent with the possibility of this substitution mechanism.

---

[19]SA Section C.3 discusses potential countervailing factors that may result in similar salary effects across election rules.

Discussion

In this letter, we used comprehensive public contracting data involving local governments in eleven EU countries over more than a decade to analyze objective and validated measures of procurement corruption risk (CRI). We matched more than 2.4 million tenders with almost 100 instances of plausibly exogenously determined variations in local politicians' salaries across more than 18,000 municipalities. Applying three alternative designs, we consistently find that higher salaries reduce corruption. They also increase efficiency in procurement (SA Table C10). Simple back-of-the-envelope calculations (presented in SA Section C.1.12) suggest that an 18% across-the-board increase in local politicians' salaries could result in net savings of up to PPP €81 billion over the same period of time as covered by our data (roughly 15 years). That is almost 4% of the EU's annual procurement budget.[20]

We note that our study primarily aimed to provide new *inferential* evidence rather than to comprehensive arbitrate between existing or generate new *explanations* (Spirling and Stewart, Forthcoming). For example, while our additional analyses suggest that higher salaries primarily reduce procurement corruption through selection rather than incentives, we leave it to future work to evaluate such questions more thoroughly. That said, our study may be helpful in influencing the relative weights accorded to some of the existing explanations. For example, our findings indicate plausible corruption-reducing effects even for salary jumps that are not very large—on the order of 15-20%—indicating that concerns that salary increases need to be unsustainably large to bear fruit (Besley and McLaren, 1993; Van Rijckeghem and Weder, 2001) may be overestimated. We, therefore, hope our results will spur new theoretical and empirical studies on the impacts of salaries on corruption and other aspects of government performance.

---

[20]See: the European Court of Auditors Special Report 28/2023, https://www.eca.europa.eu/en/publications/sr-2023-28.

participants of the conference "20 Years of the Quality of Government Research: Taking stock and moving forward," in particular Nicholas Charron and Victor Lapuente.

Competing Interests   None.

Data Availability Statement   Replication data and code can be found in Harvard Dataverse: [to be added once the review process is complete].

Ethical Standards   The research meets all ethical guidelines, including adherence to the legal requirements of the study country.

Author Contributions   Conceptualization: M.K.; M.F. Methodology: M.K; M.F. Data curation: A.A.; M.F. Data visualisation: M.K. Writing original draft: M.K; M.F. All authors approved the final submitted draft.

References

Alt, James E. and David Dreyer Lassen. 2012. "Enforcement and Public Corruption: Evidence from the American States." *The Journal of Law, Economics, and Organization* 30(2):306–338.

Barfort, Sebastian, Nikolaj A. Harmon, Frederik Hjorth and Asmus Leth Olsen. 2019. "Sustaining Honesty in Public Service: The Role of Selection." *American Economic Journal: Economic Policy* 11(4):96–123.

Becker, Gary S. and George J. Stigler. 1974. "Law Enforcement, Malfeasance, and Compensation of Enforcers." *The Journal of Legal Studies* 3(1):1–18.

Besley, Timothy. 2004. "Paying Politicians: Theory and Evidence." *Journal of the European Economic Association* 2(2-3):193–215.

Besley, Timothy and John McLaren. 1993. "Taxes and Bribery: the Role of Wage Incentives." *The Economic Journal* 103(416):119–141.

Bond, Philip. 2008. "Persistent Court Corruption." *The Economic Journal* 118(531):1333–1353.

Braendle, Thomas. 2015. "Does Remuneration Affect the Discipline and the Selection of Politicians? Evidence from Pay Harmonization in the European Parliament." *Public Choice* 162(1):1–24.

Calonico, Sebastian, Matias D. Cattaneo and Rocío Titiunik. 2014. "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs." *Econometrica* 82(6):2295–2326.

Calonico, Sebastian, Matias D. Cattaneo and Rocío Titiunik. 2015. "Optimal Data-Driven Regression Discontinuity Plots." *Journal of the American Statistical Association* 110(512):1753–1769.

Cattaneo, Matias D., Rocío Titiunik, Gonzalo Vazquez-Bare and Luke Keele. 2016. "Interpreting Regression Discontinuity Designs with Multiple Cutoffs." *The Journal of Politics* 78(4):1229–1248.

Dahlström, Carl, Victor Lapuente and Jan Teorell. 2012. "The Merit of Meritocratization: Politics, Bureaucracy, and the Institutional Deterrents of Corruption." *Political Research Quarterly* 65(3):656–668.

Di Tella, Rafael and Ernesto Schargrodsky. 2003. "The Role of Wages and Auditing during a Crackdown on Corruption in the City of Buenos Aires." *The Journal of Law and Economics* 46(1):269–292.

Eggers, Andrew C., Ronny Freier, Veronica Grembi and Tommaso Nannicini. 2018. "Regression Discontinuity Designs Based on Population Thresholds: Pitfalls and Solutions." *American Journal of Political Science* 62(1):210–229.

Fazekas, Mihály, Bence Tóth, Aly Abdou and Ahmed Al-Shaibani. 2024. "Global Contract-level Public Procurement Dataset." *Data in Brief* 54:110412.

Fazekas, Mihály and Gábor Kocsis. 2020. "Uncovering High-Level Corruption: Cross-National Objective Corruption Risk Indicators using Public Procurement Data." *British Journal of Political Science* 50(1):155–164.

Fazekas, Mihály, István János Tóth and Lawrence Peter King. 2016. "An Objective Corruption Risk Index Using Public Procurement Data." *European Journal on Criminal Policy and Research* 22(3):369–397.

Ferraz, Claudio and Frederico Finan. 2009. Motivating Politicians: The Impacts of Monetary Incentives on Quality and Performance. Technical report National Bureau of Economic Research Working Paper No. w14906, https://www.nber.org/papers/w14906.

Fisman, Raymond, Nikolaj A. Harmon, Emir Kamenica and Inger Munk. 2015. "Labor Supply of Politicians." *Journal of the European Economic Association* 13(5):871–905.

Gagliarducci, Stefano and Tommaso Nannicini. 2013. "Do Better Paid Politicians Perform Better? Disentangling Incentives from Selection." *Journal of the European Economic Association* 11(2):369–398.

Hanna, Rema and Shing-Yi Wang. 2017. "Dishonesty and Selection into Public Service: Evidence from India." *American Economic Journal: Economic Policy* 9(3):262–290.

Klašnja, Marko. 2015. "Corruption and the Incumbency Disadvantage: Theory and Evidence." *The Journal of Politics* 77(4):928–942.

Kotakorpi, Kaisa and Panu Poutvaara. 2011. "Pay for Politicians and Candidate Selection: An Empirical Analysis." *Journal of Public Economics* 95(7-8):877–885.

Kuhlmann, Sabine, Benoît Paul Dumas and Moritz Heuberger. 2022. *The Capacity of Local Governments in Europe: Autonomy, Responsibilities and Reforms.* Springer Nature.

Mookherjee, Dilip and Ivan Paak-Liang Png. 1995. "Corruptible Law Enforcers: How Should They be Compensated?" *The Economic Journal* 105(428):145–159.

Pique, Ricardo. 2019. "Higher Pay, Worse Outcomes? The Impact of Mayoral Wages on Local Government Quality in Peru." *Journal of Public Economics* 173:1–20.

Pulejo, Massimo and Pablo Querubín. Forthcoming. "Plata y Plomo: How Higher Wages Expose Politicians." *American Economic Journal: Economic Policy* .

Shapiro, Carl and Joseph E. Stiglitz. 1984. "Equilibrium Unemployment as a Worker Discipline Device." *The American Economic Review* 74(3):433–444.

Spirling, Arthur and Brandon M. Stewart. Forthcoming. "What Good is a Regression?" *The Journal of Politics* .

Treisman, Daniel. 2007. "What Have We Learned about the Causes of Corruption from Ten Years of Cross-National Empirical Research?" *Annual Review of Political Science* 10:211–244.

Van Rijckeghem, Caroline and Beatrice Weder. 2001. "Bureaucratic Corruption and the Rate of Temptation: Do Wages in the Civil Service Affect Corruption, and By How Much?" *Journal of Development Economics* 65(2):307–331.