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# The impact of government size on corruption: A meta-regression analysis

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**Abstract**

Government size—the degree of participation by governments in the economy—has been considered a potential breeding ground for corruption. However, heterogeneity in reported findings— reflecting different viewpoints on the role of large governments—makes it difficult to assess the size of the effect and, consequently, the design of anti-corruption measures. To address this issue, we perform a meta-regression analysis (MRA) of the literature on government size and corruption, examining 450 empirical estimates retrieved from 44 primary papers published from 1998 to 2022. We find that the considerable heterogeneity in results depends mainly on whether or not the paper is published, if it accounts for endogeneity, and whether it uses panel or cross-sectional data. There is evidence of bias in favor of publishing studies reporting a positive effect-size estimate. However, after controlling for publication selection bias, a negative or zero mean effect remains, which overturns the conventional wisdom. Moreover, the type of measures of corruption has a significant impact on the sign of the relationship with government size. Finally, several robustness checks confirm our main results.

**KEYWORDS**
corruption, government size, meta-analysis, public expenditure, survey

**JEL CLASSIFICATION**
H11, H50, D73, C83

## 1 INTRODUCTION

In 2018, the UN Secretary-General António Guterres, citing estimates by the World Economic Forum, said the global cost of corruption amounted to at least $2.6 trillion, or 5% of global GDP. In addition, businesses and individuals paid more than $1 trillion in bribes every year.[1] Although the credibility of corruption statistics has been questioned (Wathne & Stephenson, 2021), there is general agreement that the cost of corruption is greater than the sum of money lost: according to Mauro (2021), distortions in spending priorities caused by corruption undermine the ability of the state to promote sustainable and inclusive growth, diverting public resources away from education, healthcare and infrastructure, the types of investments that typically improve economic performance and living standards.

Since the 1990s, when data became more widely available, a large body of literature has investigated the causes and consequences of corruption, defined as the exploitation of a public office for personal benefit (Mauro, 1995; Rose-Ackerman, 1999, 2007). As a result, several surveys have reviewed the achievements and the missing points in the literature (e.g., Aidt, 2003; Jain, 2001; Lambsdorff, 2006; Rose-Ackerman, 1999; Tanzi, 1998; and Treisman, 2007). In particular, the causes of corruption have received a great deal of attention. They fall into several groups, including economic variables (economic development, government size, openness to international trade, state intervention in the economy, the endowment of natural resources), sociocultural variables (legal system, colonial heritage, religion, ethnolinguistic fragmentation, education) and political variables (basic political rights, uninterrupted democracy, freedom of information, the spread of mass media, federalism, the electoral system, political instability).

Among these, government size—the degree of participation by governments in the economy—has been considered a potential breeding ground for corruption. A relevant point in the literature that analyzes the link between corruption and government size concerns the definition and, as strictly linked to the latter, the measurement of government size. The term refers to public intervention in the form of spending decisions and employment (bureaucracy). The first hinges on public budgets and, therefore, the size of public expenditure. The second concerns the number of bureaucrats and/or the related expenses, such as wages and salaries (see Niskanen, 1971).[2] Empirical models on the relationship between government size and corruption have produced mixed results, reflecting different viewpoints on the role of large governments. Most of the literature considers that while a certain degree of government intervention is instrumental in remedying market failures, excessive intervention (an increase in government size) provides more opportunities for political rent-seeking (more resources can be stolen from the public budget), leading politicians and monopolist bureaucrats into corruption, inhibiting market competition and generating government failures (e.g., Rose-Ackerman, 1999). Hence, larger governments may increase the risk of predatory behavior by government agents. This view is directly related to the "crime and punishment" model (Becker, 1968), which suggests that big governments increase the expected benefits (payoffs) of illegal activities and therefore incentivize them, including corruption. This is especially the case in spending areas characterized by low competition, high technological content (such as defense spending), and/or discretionary and therefore less transparent spending (Gupta et al., 2002; Adsera et al., 2003; Glaeser & Shleifer, 2003). Nevertheless, differing explanations and controversial results for the linkage under investigation are rather common (e.g., Alesina & Angeletos, 2005; Billger & Goel, 2009; Kotera et al., 2012; Méon & Sekkat, 2005). For example, some prominent studies suggest that increasing government size should reduce corruption since a larger government can establish a system of checks and balances (i.e., improved oversight) and strengthen voice and accountability. This view is based on the evidence that long-established economically-developed liberal democracies generally have larger governments and are less corrupt than developing countries, since large governments are better able to provide public goods, such as education and services, which in turn boost human capital and the quality of life (Berry & Lowery, 1987), encouraging entrepreneurship and efficient capital markets (e.g., Audretsch et al., 2015), as well as providing citizens with more tools to monitor corrupt activities (e.g., Billger & Goel, 2009; La Porta et al., 1999; Lipset, 1959). Given these empirical results, it is unclear whether large governments enhance corruption. This makes it difficult for researchers and policymakers to draw unambiguous conclusions about the effect of the former on the latter, which in turn has significant consequences in terms of the policy design of anti-corruption measures.

This paper aims to provide the first meta-regression analysis (henceforth MRA) of the government size/corruption nexus, filling the evidence gap in the literature. Specifically, we (i) provide a statistical synthesis of the existing research on government size as a driver of corruption; (ii) assess the competing claims about the impact of government size on corruption; (iii) explore the sensitivity of the reported empirical results; and (iv) investigate and correct the evidence base for publication and misspecification biases. To this end, we select 44 articles (for 450 observations/estimations) using quantitative methods to evaluate the impact of government size on corruption. Our sample mirrors the diversity in the literature. As indicated above, most of the existing literature points to a positive effect of government size on corruption; however, we found articles that suggest the opposite and others that rule out any link between the two. A closer look at these divergent findings shows that they depend on the sample of countries analyzed, the measures adopted for corruption and government size, estimation methods, data structure, and the model specifications used by scholars.

Within this framework, meta-analysis provides an objective and verifiable means to synthesize the evidence and explain why the results systematically differ in and between the various studies (Cooper et al., 2019; Hedges & Olkin, 1985). Meta-analysis embodies a rigorous approach combining heterogeneous outcomes in a single estimation. It also ensures objectivity and the replicability of the results, following a peer-reviewed and prepublished systematic protocol specifying the search, inclusion/exclusion, and data extraction criteria.

By applying MRA to a wide set of observations, we address several relevant issues. The first concerns whether the sample size, measures, estimation methods, data structure, and specification of the models used in the primary papers influence the estimated impact of government size on corruption. Since all of these factors refine the focus of the problem, they can create heterogeneity in the reported estimates, making it difficult for traditional narrative reviews to draw robust and valid inferences. Moreover, taking into account the country to which the primary paper refers, MRA includes corruption defined at the country level. This country variable is intended to capture how the context in which the countries operate affects heterogeneity in the government size/corruption nexus.

Overall, our results indicate that a positive relationship exists between government size and corruption and that there is evidence of bias in favor of publishing studies that tend to report positive effect-size estimates. However, after controlling for publication selection bias, precision-effect and after carrying out funnel asymmetry tests to quantify the publication selection bias and to verify if a genuine effect exists beyond bias, the findings show a negative or zero effect of government size on corruption, indicating a possible "research revision effect" (see Gechert et al., 2024).

By contributing to the debate on methodological issues, we indirectly add to the broad discussion concerning the various proxies for corruption, particularly the divergence between perceived—Transparency International or World Bank—and experience-based measures of bribery—the International Crime Victims Survey—(see, e.g., Gutmann et al., 2020; Kurtz & Schrank, 2007; Svensson, 2005; Treisman, 2007). Our paper also indirectly sheds light on the policy implications raised by the analyses of the government size/corruption nexus. Indeed, the positive or negative sign of government size in relation to corruption helps to answer the question of whether larger government intervention can remedy market failures and promote economic development without increasing corruption.

The paper is organized as follows: Section 2 briefly introduces meta-analysis techniques. Section 3 describes the primary literature. Section 4 illustrates the empirical strategy, and Section 5 presents the empirical results. Section 6 concludes.

## 2 | META-REGRESSION ANALYSIS IN BRIEF

Since the contributions of Glass (1978) and Stanley and Jarrell (1989), meta-analyses have become increasingly popular. Several new techniques have emerged in recent years, and a sort of gold standard procedure has been codified (Havránek et al., 2020; Irsova et al., 2024). More than 1100 MRA papers in Economics from 1980 to 2020 were published, with exponential growth in the 2000s.

In this paper, we propose MRA to analyze the literature on government size as a determinant of corruption. MRA offers several advantages over a qualitative survey.[3] A meta-regression analysis is a statistical method that uncovers more about a phenomenon studied in a large set of empirical works. By investigating the relationship between the dependent variable (i.e., the efficiency scores of primary studies) and some features of each paper, MRA provides a systematic synthesis of a substantial number of studies and quantifies the role that specific aspects of original papers play in explaining the heterogeneity of the results (Glass, 1976; Glass et al., 1981; Stanley, 2001; Stanley & Jarrell, 1989).

Specifically, it evaluates the relationship between the dependent variable (i.e., the main result of the analyzed studies) and numerous features in each paper. The dependent variable is the "effect size" of the original papers. In other words, by modelling all the relevant differences between studies of a given subject, MRA fosters an understanding of the role of each varying factor in determining the heterogeneity of outcomes. Briefly, it resolves the difficulty of comparing the results of empirical works. As in any other survey, selecting the studies to be meta-reviewed is an important research phase. The selection is driven by a set of criteria to be satisfied and tends to cover all the literature without restrictions based on the reviewer's judgement. This ensures that meta-studies suffer less than qualitative reviews from potential bias when reviewing the literature on a specific topic.

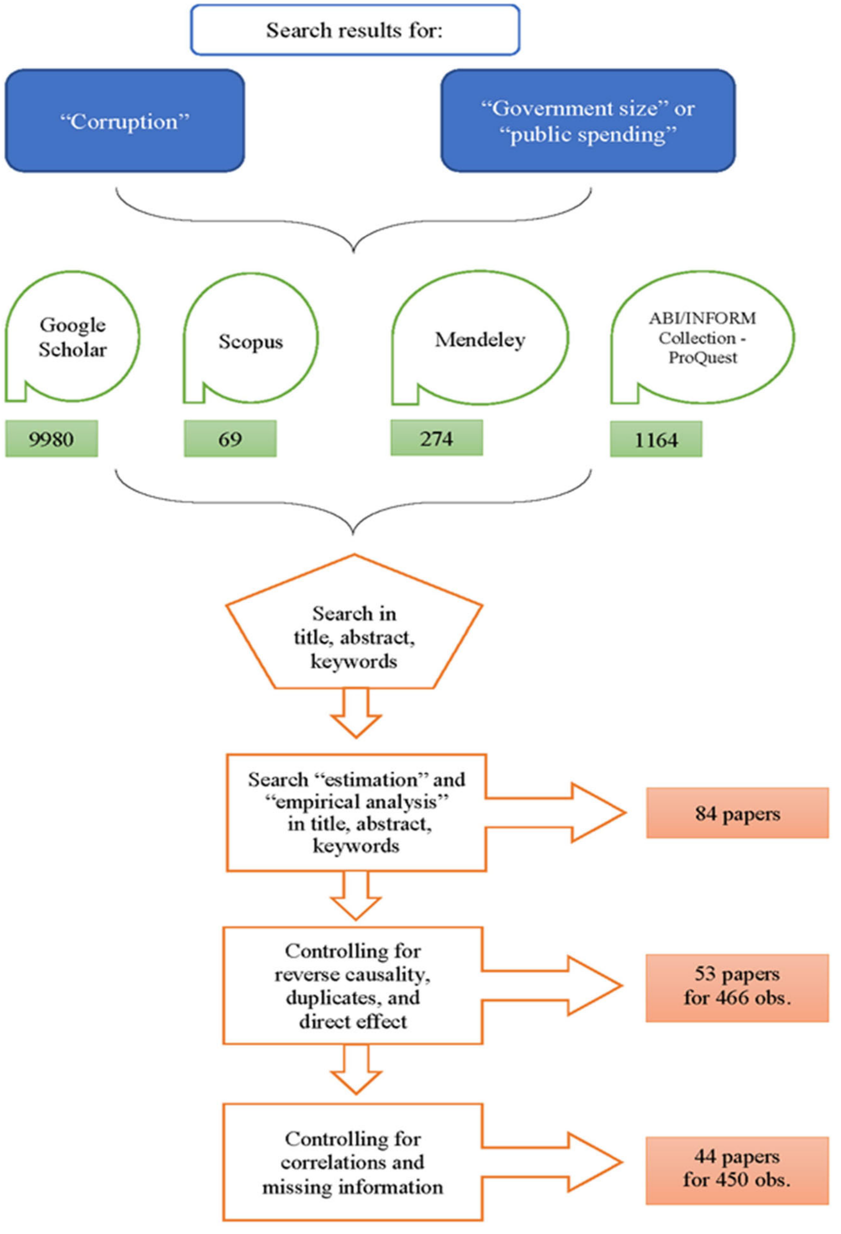


**FIGURE 1** The PRISMA of corruption-government size literature. PRISMA, preferred reporting items for systematic reviews and meta-analyses. [Colour figure can be viewed at wileyonlinelibrary.com]

## 3 | DATASET COLLECTION

To conduct a reliable MRA, we collect the primary papers from numerous archives: Google Scholar, Scopus, Mendeley, ABI Inform, and references from qualitative reviews (Gusenbauer & Haddaway, 2020). In addition, cross-paper searches were carried out. Some journal archives are available from the University of Calabria library system (including via *Proxy* service). The dataset assembling process is shown in Figure 1, which sets out the PRISMA[4] chart

(Havránek et al., 2020) and illustrates details of the different steps followed to collect the primary papers.

We carried out the paper search using various criteria. First, the words “corruption” and “government size” or “public spending” were used to search for the title of the paper, abstract, and keywords. Google Scholar turned up 9980 results, while in scientific databases the number was significantly lower: 69 in Scopus, 274 in Mendeley, and 1164 in ABI Inform (the latter including theses).

Second, the search was refined by looking for “estimation” and “empirical analysis” in the titles, abstracts, and keywords. The main journals in the field were consulted manually, and papers were further selected with a focus on the impact of government size on corruption. References from the qualitative survey of Dimant and Tosato (2018) were subsequently scanned. Before filtering this sample of papers, we verified that they (a) included empirical analyses and (b) were published in English in a journal or as a discussion paper. Up to this step, the process resulted in the selection of 84 papers and was concluded on February 28, 2023.

We then checked the papers to see if they (a) focused on the direct impact of government size on corruption, (b) did not investigate the reverse relationship, and (c) did not appear twice in the sample. As a result, 31 contributions were withdrawn, resulting in a sample of 53 papers and 466 observations.

Finally, 9 papers (and 16 observations) were removed because they did not provide the essential data required for the meta-analysis (the estimated outcome and its standard error). Hence, the search yielded a sample of 44 papers (including 5 working papers) published from 1998 to 2022 with 450 observations (Figure 1).[5] In the Appendix, Table A1 sets out the primary papers collected.

## 4 | EMPIRICAL STRATEGY

### 4.1 | The partial correlation index and estimation technique

Since different studies use different units of measurement, their estimates are not directly comparable. To summarize and compare the results from various studies, it is necessary to compute standardized effect sizes. We, therefore, calculate the partial correlation coefficients (PCCs), which measure the association between corruption and government size, while other explanatory variables are held constant. PCCs are comparable because they are unrelated to the metrics used to measure the independent and dependent variables.

The partial correlation index (PCC) is defined as follows (see, among others, Ugur, 2014; Valickova et al., 2015; and Doucouliagos et al., 2022):

$$PCC_{ij} = \frac{t_{ij}}{\sqrt{t_{ij}^2 + df_{ij}}} \tag{1}$$

where $i$ indicates the single estimation in the $j$th primary paper, $t$ is the test statistic for the significance of $\beta$, and $df$ is the degrees of freedom for estimating $\beta$.

The standard error for the PCC is:

$$SE_{ij} = \frac{PCC_{ij}}{t_{ij}} \tag{2}$$

To run our MRA on the impact of government size on corruption, we use the following model:

$$PCC_{ij} = \beta_0 + \beta_1 SEr_{ij} + \sum_k \beta_k X_{kij} + \varepsilon_{ij} + u_i \tag{3}$$

where $\varepsilon_{ij} \sim N(0, \sigma^2_{ij})$ is the within-study disturbance and $u_i \sim N(0, \tau^2)$ is the deviation due to residual nonobservable heterogeneity (between-study variance). The parameter $\tau^2$ is a measure of between-study variability and is estimated as in Harbord and Higgins (2008). The group of variables $X_{kij}$ comprises the explanatory variables summarizing various model characteristics in each study.

We adopt a two-step procedure as proposed by Gallet and Doucouliagos (2014) and applied in Aiello and Bonanno (2018, 2019). A random effect maximum likelihood (REML) regression is run in the first step, and in the second step, a WLS regression in which the weights include $SEr_{ij}$ to correct the default heteroskedasticity and the value of $\tau^2$ retrieved from the first step. This ensures that the estimates are robust to clustering at the study level.

## 4.2 | Assessing heterogeneity in the literature on the corruption/government size nexus

An MRA can be run where there is heterogeneity in the literature. The sources of the heterogeneity must be included in the regressions as explanatory variables. Figure 2 and Table 1 show the sources of heterogeneity in the study of government size and corruption, complementing the regression analysis shown in Section 5.

After reporting the kernel density for the whole sample, it is useful to investigate the different sources of heterogeneity.[6] The first is the type of contribution (published/not published). Therefore, we estimate the kernel density of the two types of paper. Figure 2 shows a clear difference in the density estimates for the two types. The result is confirmed when testing for differences in means, as shown in Table 1 (5% significance level).

A second source of heterogeneity is the data type employed in the studies, either panel or cross-section data. The kernel density estimates for the two types of data tend to differ substantially. Testing for differences in means confirms the results (5% significance level; see Table 1).

A third possible source of heterogeneity lies in the techniques used in the studies and whether or not they check for endogeneity among the variables of interest. The kernel density estimates for these two types and the results point toward the existence of differences between them. The difference in means test rejects the null hypothesis at the 10% significance level (Table 1).

The primary papers we analyzed show significant heterogeneity (variability) in both the proxies used to measure corruption and government size.[7] In this case, the tests for the presence of differences in means also provide strong and significant results, as shown in Table 1.

## 4.3 | Variables and descriptive statistics

After showing heterogeneity in our MRA sample, which is preparatory to the regression analysis, we set out in detail all the variables used in the models. Table 2 shows descriptive statistics and definitions of all variables included in the regressions.[8] Tables A2 and A3 in the Appendix

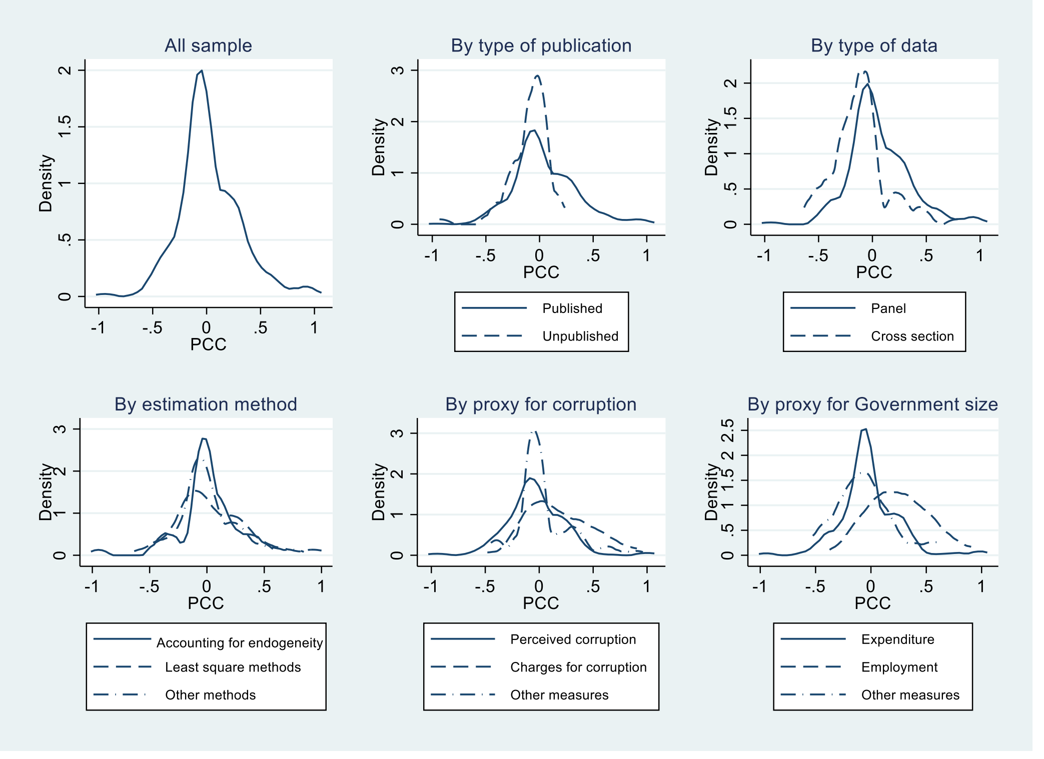


**FIGURE 2** Kernel density estimates for investigation of the presence of heterogeneity. [Colour figure can be viewed at wileyonlinelibrary.com]

show respectively the correlation matrix and a detailed analysis of sample heterogeneity. The Supplemental Material also provides evidence of heterogeneity.

In detail, we employ a dummy variable equal to 1 for estimations retrieved from peer-reviewed papers, and 0 for estimations retrieved from working papers (*Published*). *Panel* is equal to 1 if the primary papers refer to estimations on panel data and 0 for cross-sectional data. Regarding the estimation method, we include two binary variables that capture the estimations obtained through the Ordinary Least Square technique and methods accounting for the endogeneity (*Least square* and *Endogeneity*, respectively). The control group comprises all estimations obtained from the other methods used in the primary papers.

After checking all the factors deemed to be study design variables, we use a set of regressors capturing specific characteristics of the literature. In particular, we include *Perceived corruption* (i.e., data not measuring corruption itself, only opinions about its prevalence) and *Charges for corruption* to investigate the role of the type of proxy for corruption used in the primary papers; other proxies for corruption (*Other measures*) are the control group.

Our focus is solely on the direct effects of government size on corruption. Primary studies draw upon 44 main sources of corruption data, which consist of scores based on perceived levels of corruption as well as on one objective measure of corruption. The scores have different scales, ranging from 0 to 6 for ICRG data, from −2.5 to +2.5 for World Governance Indicators data, from 0 to 12 for TI data, and different ranges in *Other corruption data sources*.[9] Except for Transparency International data, the higher the score, the less the corruption. To ensure consistency, most original

**TABLE 1** Sources of heterogeneity in the corruption-government size literature.

| | Mean | SD | Obs. |
|---|---|---|---|
| Full sample | 0.031 | 0.286 | 450 |
| *Publication status* | | | |
| Unpublished | −0.092 | 0.188 | 55 |
| Published | 0.048 | 0.293 | 395 |
| Test on the difference in means (*p*-value) | **.000** | | |
| *Data type* | | | |
| Cross-section | −0.116 | 0.256 | 79 |
| Panel | 0.062 | 0.283 | 371 |
| Test on the difference in means (*p*-value) | **.000** | | |
| *Estimation approach* | | | |
| Endogeneity | 0.002 | 0.251 | 99 |
| Least square | 0.051 | 0.315 | 230 |
| Other | 0.017 | 0.252 | 121 |
| Test on the difference in means (*p*-value) | **.067** | | |
| *Proxy for corruption* | | | |
| Perceived corruption | −0.035 | 0.262 | 260 |
| Charges for corruption | 0.188 | 0.302 | 102 |
| Other | 0.043 | 0.265 | 88 |
| Test on the difference in means (*p*-value)* | **.000** | | |
| *Proxy for Government size* | | | |
| Government size—Expenditure | −0.018 | 0.259 | 325 |
| Government size—Employment | 0.253 | 0.283 | 87 |
| Other | −0.059 | 0.270 | 38 |
| Test on the difference in means (*p*-value)* | **.000** | | |

*Note*: Authors' elaboration on collected data. Means are unweighted. *t*-test for difference in means: the bold *p*-values mean that the difference is statistically significant.

*All *t*-tests are carried out by comparing the category versus all others.

studies transform corruption indices, so a higher score indicates increased corruption. Regarding the objective measure of corruption, *Charges for Corruption* is the most commonly used indicator in our sample.

Similarly, *Govsize employment* and *Govsize expenditure* are used to check for the different measurements of government size. In this case, we use the two variables alternatively since they are strongly correlated. Another control includes the time trend based on the year of publication.

Finally, we add two further variables as robustness checks. The first is the degree of corruption at the country level, namely *Countries corruption*, to take into account the specific effects of each country included in the samples of primary papers (Aiello & Bonanno, 2018; 2019; Doucouliagos et al., 2022). The rationale is that MRA can provide evidence on whether studies on the corruption-government size nexus in countries characterized by high corruption yield results that differ from those obtained when focusing on less corrupt countries. We use *Control of Corruption* as a control variable defined at the country level. It is retrieved from the World Bank DataBank website, capturing perceptions of the extent to which public power is exercised for private gain. The index uses a scale of −2.5 to 2.5, where low values denote high corruption.[10]

**TABLE 2** Descriptive statistics of the variables included in the regressions.

| Variables | Description | Obs | Mean | St. dev. | Min | Max |
|---|---|---|---|---|---|---|
| *Dependent variable* | | | | | | |
| PCC | Partial correlation coefficient as in Equation (1). | 450 | 0.0309 | 0.2861 | −0.9603 | 1 |
| *Study design* | | | | | | |
| Ser | Standard error of PCC as in Equation (2). | 450 | 0.0904 | 0.0795 | 0.0013 | 0.78 |
| Precision effect (1/SEr) | The inverse of SEr. | 450 | 38.29 | 76.28 | 1.28 | 750.44 |
| Published | Dummy equal to 1 for estimations retrieved from peer-reviewed papers, and 0 otherwise. | 450 | 0.8778 | 0.3279 | 0 | 1 |
| Panel | Dummy equal to 1 for estimations on panel data, and 0 otherwise. | 450 | 0.8244 | 0.3809 | 0 | 1 |
| Year of publication_trend | Time trend based on the year of publication. | 450 | 17.74 | 6.6032 | 1 | 26 |
| *Estimation approach* | | | | | | |
| Endogeneity | Dummy equal to 1 for estimations obtained using methods accounting for endogeneity, and 0 otherwise. | 450 | 0.2200 | 0.4147 | 0 | 1 |
| Least Square | Dummy equal to 1 for estimations obtained using least square method, and 0 otherwise. | 450 | 0.5111 | 0.5004 | 0 | 1 |
| *Specific variables* | | | | | | |
| Perceived corruption | Dummy equal to 1 if the measure for corruption in the primary paper refers to data that do not measure corruption itself but only opinions about its prevalence, and 0 otherwise. | 450 | 0.5778 | 0.4945 | 0 | 1 |
| Charges for corruption | Dummy equal to 1 if the measure for corruption in the primary paper refers to the number of charged people for corruption issues, and 0 otherwise. | 450 | 0.2267 | 0.4191 | 0 | 1 |
| Govsize expenditure | Dummy equal to 1 if the measure of government size in the primary paper is related to expenditure, and 0 otherwise. | 450 | 0.7222 | 0.4484 | 0 | 1 |
| *Robustness checks* | | | | | | |
| Govsize employment | Dummy equal to 1 if the measure of government size in the primary paper is related to employment, and 0 otherwise. | 450 | 0.1933 | 0.3954 | 0 | 1 |
| Countries corruption | Degree of corruption of the country to which the primary paper estimate refers. | 450 | 0.4829 | 0.7615 | −0.6105 | 2.2340 |
| Dummy_ABS (Quality of journals) | Dummy variable equal to 1 if the primary paper is published in a 3- or 4-star ABS journal, and 0 otherwise. | 450 | 0.1422 | 0.3497 | 0 | 1 |

*Note*: Authors' elaboration on collected data.

The second variable, namely *Dummy_ABS*, measures the quality of journals where primary papers are published. *Dummy_ABS* is a dummy variable equal to 1 if the primary paper is published in a 3- or 4-star ABS journal, and 0 otherwise.

## 5 | EMPIRICAL RESULTS

This section delves into the empirical outcomes of our MRA analysis. Initially, we set out findings pertinent to the investigation of publication bias within primary papers. Subsequently, we analyze results from the estimated models and carry out some robustness checks. Finally, in the last subsection we show the outcomes derived from the regression of various conditional models.

### 5.1 | Investigation of publication bias

Studies with statistically significant findings are more likely to be published and are published more quickly than studies with null results. This is the issue of publication selection bias, whereby some researchers report only statistically significant results or results consistent with their priors (Christensen & Miguel, 2018). The publication bias distorts meta-averages, inflating them by a factor of 2 or more (Ioannidis et al., 2017).

To address this issue, we carry out the funnel-asymmetry test and precision-effect test (FAT-PET) (Stanley, 2008; Stanley & Doucouliagos, 2007) by estimating the following equation:

$$r_{ij} = \beta_0 + \beta_1 SEr_{ij} + e_{ij} \tag{4}$$

Corrected for heteroskedasticity this becomes (dividing by $SEr_{ij}$):

$$t_{ij} = \beta_1 + \beta_0 1/SEr_{ij} + \varepsilon_{ij} \tag{5}$$

The FAT involves the test for $\beta_1 = 0$ and the PET tests for $\beta_0 = 0$ (funnel asymmetry and precision effect tests, respectively).[11]

Although the FAT is known to have low power—which means a low probability of rejecting the null hypothesis when the latter is false—it has the advantage of testing for the presence of a genuine effect when publication selection bias is controlled for (Ugur, 2014). Additionally, the direction of the bias is positive, given that the estimation of $\beta_1$ is positive and significant.

We also employed the FAT-PET-PEESE equation, namely the precision-effect estimation with standard errors test, which includes the square of SEr, to test for the presence of a nonlinear effect between the PCC and its SEr (Doucouliagos et al., 2022). The PEESE enables us to obtain a corrected estimate of $\beta_{\mathrm{SEr}}$ (replacing $\beta_1$ in the FAT-PET). This test demonstrates a considerable positive bias given that the estimated coefficient is greater than 1, confirming the presence of publication bias. The PEESE equation is as follows:

$$t_{ij} = \beta_{SEr} SEr + \beta_0 1/SEr_{ij} + v_{ij} \tag{6}$$

Table 3 presents the results of the analyses testing for the presence of publication bias using FAT-PET and FAT-PET-PEESE regressions. The table displays coefficients and standard errors for each model. In the FAT-PET regression, the coefficient $\beta_1$ associated with bias is estimated at

**TABLE 3** Testing the presence of publication bias: FAT-PET and FAT-PET-PEESE regressions.

| | (1) FAT-PET with robust SE | (2) FAT-PET-PEESE with robust SE |
|---|---|---|
| $\beta_1$ (Bias) | 0.7751*** | |
| | (0.2135) | |
| $\beta_0$ (Precision term) | −0.0369** | 0.0059 |
| | (0.0179) | (0.0130) |
| $\beta_{SEr}$ (SEr) | | 1.7308*** |
| | | (0.6359) |
| Observations | 450 | 450 |
| Prob > $F$ | 0.000 | 0.000 |
| Adjusted $R$ square | 4.08% | 1.31% |

*Note*: Authors' elaboration on data collected. The dependent variable of the models is the partial correlation coefficient (PCC). Significance levels and standard errors result from the WLS procedure proposed by Gallet and Doucouliagos (2014).
Significance levels: ***$p < .01$ **$p < .05$ *$p < 0.1$.

0.7751 and statistically significant at 1%, indicating evidence of publication bias. Additionally, the precision term ($\beta_0$) is estimated at −0.0369 with a significance level of 5%.

In the FAT-PET-PEESE regression, the coefficient for the standard error term ($\beta_{SEr}$) is estimated at 1.7308 and is statistically significant at 1%, while the precision term ($\beta_0$) is not significant. The publication bias may indeed be present in the dataset, as shown by the significant coefficients associated with bias in both models. However, further investigation may be needed to understand the specific mechanisms driving this bias and to assess the robustness of the results.

Overall, we find a positive and often highly statistically significant $\beta_1$ in many of our specifications. This means that the literature seems to overstate the positive effect of government size on corruption. Additionally, we find a negative and statistically significant mean beyond bias ($\beta_0$) in many of our specifications (in particular for FAT-PET, but also partly for FAT-PEESE full models reported in Table A4). This is a highly significant finding in relation to our research question. Obviously, the uncorrected simple mean of the PCC is slightly positive. However, after controlling publication bias, there remains a negative or zero effect of government size on corruption, overturning conventional wisdom. This is in line with examples of "research revision" detected by several meta-analyses (Gechert et al., 2024; Paldam, 2022) showing that after correcting for observable biases, the empirical economic effects are typically much closer to zero and sometimes switch signs.

Furthermore, Figure 3, alongside Table 3, reveals a consistent asymmetry in the distribution of the partial correlation coefficients, as shown in the Funnel plot. This asymmetry strongly suggests the presence of publication selection bias, given that literature devoid of such bias would typically exhibit a symmetrical funnel shape.

## 5.2 | Estimated models and robustness checks

In this section, we present the multivariate MRA carried out to establish the extent to which differences in some moderating variables account for variations in the effect-size estimates.

The results shown in Table 4 start from the FAT-PET regression and describe the evolution of information on the funnel-asymmetry test.[12] Column 2 shows a regression including only the

**TABLE 4** Main meta-regressions on corruption and government size.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | **Baseline models** | | | | **Robustness checks** | | |
| | **FAT-PET** | **Study design** | **Estimation method** | **Specific variables** | **Govsize employment** | **Countries observables** | **Quality of journal** |
| $\beta_1$(Bias) | 0.7751*** | 0.7430*** | 0.7065*** | 0.4541** | 0.3612 | 0.3291 | 0.4825** |
| | (0.2135) | (0.2229) | (0.2284) | (0.2305) | (0.2380) | (0.2470) | (0.2375) |
| $\beta_0$(Precision term) | −0.0369** | −0.1904*** | −0.1774*** | −0.1398*** | −0.2584*** | −0.1721*** | −0.1095** |
| | (0.0179) | (0.0477) | (0.0498) | (0.0506) | (0.0489) | (0.0514) | (0.0520) |
| Published | | 0.0638** | 0.0659** | 0.0523* | 0.0238 | 0.0556* | 0.0619* |
| | | (0.0305) | (0.0307) | (0.0312) | (0.0327) | (0.0307) | (0.0347) |
| Panel | | 0.1383*** | 0.1396*** | 0.0982*** | 0.0899** | 0.1007*** | 0.0921** |
| | | (0.0347) | (0.0350) | (0.0363) | (0.0352) | (0.0369) | (0.0377) |
| Year of publication_trend | | −0.0008 | −0.0011 | 0.0057** | 0.0061** | 0.0062** | 0.0047* |
| | | (0.0023) | (0.0023) | (0.0025) | (0.0024) | (0.0025) | (0.0026) |
| Endogeneity | | | −0.0306 | −0.0323 | −0.0249 | −0.0430 | −0.0363 |
| | | | (0.0332) | (0.0333) | (0.0329) | (0.0344) | (0.0340) |
| Least square | | | −0.0023 | 0.0044 | 0.0210 | −0.0018 | 0.0002 |
| | | | (0.0294) | (0.0277) | (0.0279) | (0.0282) | (0.0279) |
| Perceived corruption | | | | −0.0661* | −0.0322 | −0.0410 | −0.0769** |
| | | | | (0.0341) | (0.0352) | (0.0388) | (0.0366) |
| Charges for corruption | | | | 0.1063** | 0.1169** | 0.1107** | 0.0898* |
| | | | | (0.0472) | (0.0458) | (0.0478) | (0.0508) |
| Govsize expenditure | | | | −0.1067*** | | −0.1051*** | −0.1064*** |
| | | | | (0.0316) | | (0.0313) | (0.0320) |
| Govsize employment | | | | | 0.2157*** | | |
| | | | | | (0.0388) | | |

(Continues)

**TABLE 4** (Continued)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | **Baseline models** | | | | **Robustness checks** | | |
| | **FAT-PET** | **Study design** | **Estimation method** | **Specific variables** | **Govsize employment** | **Countries observables** | **Quality of journal** |
| Countries corruption | | | | | | 0.0387** | |
| | | | | | | (0.0194) | |
| Dummy_ABS | | | | | | | −0.0412 |
| | | | | | | | (0.0443) |
| Observations | 450 | 450 | 450 | 450 | 450 | 450 | 450 |
| F | 7.083 | 10.24 | 7.510 | 9.242 | 12.93 | 8.54 | 8.57 |
| Prob > $F$ | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |

*Note*: Authors' elaboration on collected data. The dependent variable in the models is the partial correlation coefficient (PCC). Significance levels and standard errors are derived from WLS estimations conducted as the second step, following the procedure proposed by Gallet and Doucouliagos (2014).

Significance levels: ***$p < .01$ **$p < .05$ *$p < .1$.

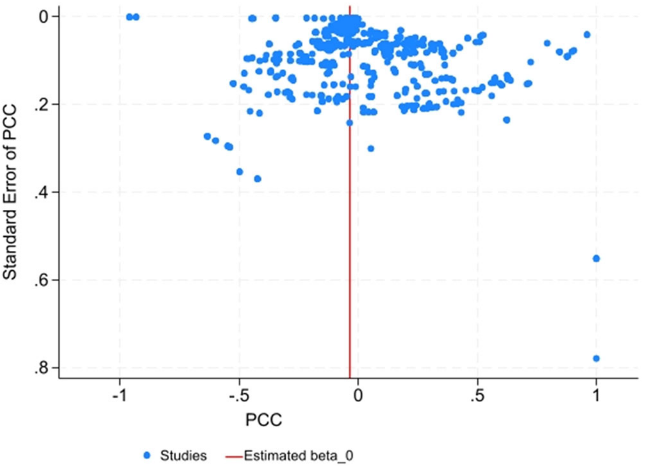


**FIGURE 3** Funnel plot. [Colour figure can be viewed at wileyonlinelibrary.com]

dummies related to the study design used in the primary paper. Column 3 adds the variables related to the estimation method, and column 4 includes literature-specific variables. Columns from 5 to 7 present robustness checks.

Before discussing the results, it is worth noting that at the bottom of the table, the diagnostics of the models (*F*-statistics) are set out, guaranteeing the suitability of our estimation setting.

As far as the study design is concerned, *Published* shows a slightly significant positive effect in columns 2–4 and 6, although it turns out not to be significant in the robustness models 5 and 7.

Interestingly, when the estimated results of the primary papers are set out on panel data, the effect of government size on corruption increases, since the estimated coefficients of *Panel* are always positive and equal to about 0.13 (except for column 4, where the coefficient is equal to about 0.1). This result is in line with an issue raised in the literature. Transparency International, provider of probably the most commonly used perception index (CPI), warned against the use of the index over time (TI, 2011). Treisman (2007) and Andersson and Heywood (2009) have critically discussed this use. Moreover, from an econometric point of view, these indices exhibit limited variation over time, not only on a year-by-year basis but also over more extended periods (Heywood & Rose, 2014). These authors call for the use of 10-year averages to analyze the evolution of corruption over time. Therefore, the positive effect of *Panel* may be an artifice of the data rather than a genuine outcome. The time trend based on the year of publication is significant only in column 7. Also, in this case the estimated sign is positive.[13]

Examining the estimation methods, neither of the variables used (*Least Square* and *Endogeneity*) enable us to disentangle any change in the effect size compared to other estimation techniques. The conclusion confirms that method types do not matter for the understanding of the relationship between corruption and government size.

Results are inconsistent when referring to the proxy for corruption used in the primary regressions. *Perceived corruption* has a significantly negative impact on the effect size, while *Charges for corruption* has a positive impact, although at a lower level. The latter positive effect may be related to the higher number of public officials in countries with large governments, increasing the potential for crime (Glaeser & Saks, 2006).[14] Nevertheless, we do not find these results surprising. The literature is characterized by a long-standing debate on the measurement of corruption, a challenging issue due to the secretive nature of the phenomenon. Perception indices are the most commonly used but suffer from several limitations. Treisman (2007, p. 212) warns that subjective indices may capture "not observations of the frequency of corruption but inferences made by experts and survey respondents based on conventional understandings of corruption". In addition, perceptions have been shown not to predict the actual experience of corruption. Experience-based indicators, which deal with individual incidents of corruption and are included in the "Other" group in our analysis, have been proposed. However, these indicators are based on what respondents remember and how they assess whether an official expects a bribe, which hardly makes them an objective measure.

Gutmann et al. (2020) have shown that perceived- and experience-based indicators are not correlated and that variations in individual corruption perception cannot be explained by experience alone but are also affected by respondent and country characteristics. Finally, judicial statistics—such as *Charges for corruption*—strongly depend on a country's cultural characteristics and criminal policy, and therefore are unsuitable for cross-country comparison. Overall, our results point toward the risk of over/under-estimating the effect of government size on corruption, using different corruption measures.

Our main meta-regressions include the dummy *Govsize expenditure* since more than 70% of our sample uses this proxy for government size. The variable has a significantly negative impact.

Finally, the last three columns of Table 4 set out robustness checks. In detail, when we use *Govsize employment* as a robustness check, replacing *Govsize expenditure*, the measure of government size is significantly positive, and all other results are quite robust. In addition, with regard to the positive coefficient associated with *Countries corruption*, high index values (i.e., low corruption in the country) lead to a higher effect size. As already pointed out, our dependent variable is a partial correlation coefficient. Although this makes it unsuitable for an economic interpretation, it does provide some hints. In this light, the latter finding is interesting, though unsurprising. A stream of literature maintains that higher-income countries tend to overestimate corruption, while the opposite happens in poorer countries (Gutmann et al., 2020), possibly because in rich countries the expectation for greater accountability is somehow frustrated.

The last column in Table 4 includes the variable *Dummy_ABS*. This variable, added to control for the quality of journals where primary papers are published, is not significant.

## 5.3 | Conditional models

Despite some differences with the overall analysis shown in Table 4, a number of interesting results are obtained from the conditional MRA (Table 5). Some evidence appears quite robust, particularly with reference to significant coefficients estimated in Table 4. For example, *Panel* has a positive and significant impact in almost all models; *Govsize expenditure* shows a negative impact in many models. However, it is worth noting that when we use only observations for which the measure of corruption used in the primary papers is the number of charged people for crimes of corruption, *Panel* has a negative sign while in the sub-sample related to all the other measures of corruption the coefficient is not significant. Furthermore, examining the methodology block in Table 5, estimation techniques evidently matter, unlike the results of Table 4, and there are some noticeable differences compared to the overall analysis. When methods accounting for endogeneity are used to obtain the primary estimates, a higher PCC is found when the paper is published in a peer-reviewed journal compared to working papers. In addition, the time trend has a negative sign.

Focusing on the two groups related to data types, a few results drive the overall findings. While the estimated coefficients associated with the time trend and *Govsize expenditure* for the group of

**TABLE 5** Conditional MRA on corruption and government size.

| | Measures for corruption | | | Methodology | | | Data type | | Geographical context | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Perceived corruption | Charges for corruption | Other measures | Endogeneity | Least square | Other methods | Panel | Cross section | One Country | More Countries |
| $\beta_1$ (Bias) | 0.4097 | −2.3832*** | 2.4862*** | 0.1663 | 0.3519 | 1.1880* | 0.8848*** | −2.6218*** | −0.2174 | 0.7863** |
| | (0.3981) | (0.5498) | (0.8818) | (0.7294) | (0.3167) | (0.6108) | (0.2674) | (0.7791) | (0.5175) | (0.3673) |
| $\beta_0$ (Precision term) | −0.2362*** | 0.5075*** | −0.0706 | 0.2493* | −0.1437* | −0.0359 | −0.0424 | −0.1224 | −0.0225 | −0.2707*** |
| | (0.0795) | (0.1316) | (0.1896) | (0.1339) | (0.0789) | (0.1193) | (0.0641) | (0.1346) | (0.1000) | (0.0940) |
| Published | 0.0218 | – | 0.0713 | 0.2012* | −0.0352 | −0.0013 | 0.0710 | 0.0736 | −0.0526 | 0.0320 |
| | (0.0507) | | (0.0476) | (0.1099) | (0.0574) | (0.0327) | (0.0471) | (0.0579) | (0.0577) | (0.0438) |
| Panel | 0.1827*** | −0.2202** | −0.0309 | 0.0024 | 0.1068 | 0.2420*** | | | 0.0536 | 0.2098*** |
| | (0.0564) | (0.0973) | (0.0628) | (0.0887) | (0.0679) | (0.0908) | | | (0.0741) | (0.0563) |
| Year of publication_trend | 0.0000 | 0.0366*** | −0.0042 | −0.0168*** | 0.0101*** | 0.0007 | 0.0052** | 0.0033 | 0.0113*** | 0.0015 |
| | (0.0032) | (0.0058) | (0.0114) | (0.0047) | (0.0034) | (0.0064) | (0.0025) | (0.0092) | (0.0041) | (0.0032) |
| Endogeneity | −0.0234 | −0.2200** | −0.2076*** | | | | −0.0492 | 0.0628 | 0.0133 | −0.0518 |
| | (0.0479) | (0.0891) | (0.0749) | | | | (0.0407) | (0.0855) | (0.0619) | (0.0457) |
| Least square | −0.0025 | −0.2625*** | −0.1493 | | | | −0.0354 | 0.0742 | 0.0541 | −0.0212 |
| | (0.0311) | (0.0948) | (0.1005) | | | | (0.0340) | (0.0813) | (0.0796) | (0.0337) |
| Perceived corruption | | | | 0.0290 | −0.1181** | −0.0682 | −0.0295 | 0.1086* | – | −0.0259 |
| | | | | (0.0836) | (0.0567) | (0.0558) | (0.0411) | (0.0628) | | (0.0608) |
| Charges for corruption | | | | −0.0221 | 0.1303* | −0.0837 | 0.0811* | 0.5664*** | 0.1418** | 0.2708*** |
| | | | | (0.0854) | (0.0728) | (0.1408) | (0.0488) | (0.1295) | (0.0658) | (0.1020) |
| Govsize expenditure | 0.0026 | −0.3727*** | 0.1482 | −0.1819* | −0.0508 | −0.2183*** | −0.1470*** | 0.0073 | −0.2274*** | −0.0106 |
| | (0.0487) | (0.0518) | (0.0909) | (0.0918) | (0.0587) | (0.0631) | (0.0398) | (0.0559) | (0.0693) | (0.0493) |
| Observations | 260 | 102 | 88 | 99 | 230 | 121 | 371 | 79 | 145 | 305 |
| *F* | 3.421 | 17.11 | 13.52 | 6.069 | 6.588 | 4.317 | 8.631 | 9.393 | 7.521 | 5.358 |
| Prob > *F* | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |

*Note*: Authors' elaboration on collected data. The dependent variable of the models is the partial correlation coefficient (PCC). The dependent variable in the models is the partial correlation coefficient (PCC). Significance levels and standard errors are derived from WLS estimations conducted as the second step, following the procedure proposed by Gallet and Doucouliagos (2014). Significance levels: ***$p < .01$ **$p < .05$ *$p < .1$. "-" Dropped for collinearity.

primary estimates obtained for panel data are the same as those in column 4, Table 4, these coefficients are not significant for the complementary group of estimates obtained for cross-sectional data.

Finally, mixed results emerge for the sample broken down into the number of countries analyzed in the primary papers (the last two columns in Table 5). *Published* shows a negative coefficient when a single country is considered, but the impact is not significant when more countries are analyzed. The parameter of *Panel* is significant only for the second group of primary estimates. By the same reasoning, the coefficients associated with the year of publication (trend) and the parameter of *Govsize expenditure* are significant only for 1-country-estimations (positive and negative, respectively).

After conducting these analyses, it may be useful to estimate a "best practice model" to calculate the average effect of PCC. Considering the full specification reported in column 4 of Table 4, a "best practice" approach could involve assuming the absence of publication bias (setting $\beta_1 = 0$ and $\beta_0 = 1$), along with values for key variables: *Published* = 1, *Panel* = 1, *Endogeneity* = 1, *Least Square* = 0, *Govsize Expenditure* = 1, and the mean value of the *Year of publication_trend* (17.74).

As discussed above, there is no clear reason to prefer either *Perceived Corruption* (more frequently used in empirical analyses) or *Charges for Corruption* as indicators. Therefore, we start from the "best practice" estimate, and derive two scenarios:

a. For *Perceived corruption* = 1 and *Charges for corruption* = 0, we obtain $PCC_{Perceived}$ = -0.0933.
b. For *Perceived corruption* = 0 and *Charges for corruption* = 1, we obtain $PCC_{Charges}$ = 0.0791.

The opposite results are unsurprising since in Tables 4 (and to a lesser extent 5) the two variables showed opposite signs. This outcome further strengthens the crucial nature of the choice of the proxy for corruption.

## 6 | CONCLUDING REMARKS

Does the size of the government affect corruption? This paper seeks to answer the question by collecting 450 observations from 44 primary studies published over the period 1998–2022, using a meta-analysis to evaluate the possible impact of government size on corruption in primary studies. Overall, our results show substantial heterogeneity in the estimates and identify several sources of variability. First, the coefficients of the impact of government size on corruption in published papers are significantly higher than in unpublished papers. Furthermore, the bias is to publish papers finding a positive relationship between government size and corruption. This may partially reflect publication bias, as explored in recent studies (e.g., Brodeur et al., 2023). However, after controlling for publication selection bias precision-effect and funnel asymmetry tests to quantify the bias and to verify if a genuine effect exists beyond bias, the negative or zero effects of government size on corruption reverse the conventional wisdom for effect size, indicating a possible "research revision effect" (see Gechert et al., 2024).

Second, when estimates are made by methods that correct for endogeneity, the impact of government size on corruption is significantly lower. Third, the use of panel data yields significantly higher government size impacts on corruption, but, as we argued above, this is probably the result of the low variability in the corruption data over time.

Finally, our MRA concludes that the results are sensitive to the measures used in the primary papers, an outcome that reinforces the issue of the reliability of data on corruption as raised after the initial publication of papers on corruption, including in leading journals. This remains an open question, and our results suggest that the challenge of future research will be to refine the measures of both government size and corruption in order then to examine the patterns they reveal. This is not merely a scholarly issue. As UNDP (2008, 8) stated: “To put it plainly, there is little value in a measurement if it does not tell us what needs to be fixed,” therefore actionable indicators need to be developed which, in turn, lead to policy decisions that can reduce the levels of corruption.

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## CONFLICT OF INTEREST STATEMENT

The authors declare no conflicts of interest.

## DATA AVAILABILITY STATEMENT

Data are available upon request from the authors.

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## ENDNOTES

[1] https://press.un.org/en/2018/sc13493.doc.htm

[2] Niskanen (1971) defines government bureaucrats as agents seeking to maximize the size of their budgets and points out that they have no incentive to be efficient.

[3] Traditional systematic reviews provide qualitative analyses of a stream of literature, and usefully summarize a topic, tending to focus on statistical significance testing to determine whether there is an effect, but involve two shortcomings. First, significance testing is not well suited to the task because it depends significantly on sample size. Second, the comparability of studies can be problematic because it may not be easy to establish which studies are indeed the same.

[4] PRISMA stands for preferred reporting items for systematic reviews and meta-analyses.

[5] Additional details on the dataset construction process are available upon request.

[6] This type of graph enables us to show the heterogeneity in the effect size for different groups, which means drawing separate kernels for the different values of categorial variables able to capture variety. This analysis is very popular in the MRA literature and justifies the use of some regressors (Aiello & Bonanno, 2018, 2019).

[7] Several measures of corruption are used. For instance, the Corruption Perception Index (CPI) is compiled from various sources, including TI (Transparency International), GCR-WEF (The Global Competitiveness Report—World Economic Forum), EUBusSurvey (Eurobarometer Businesses’ Attitudes toward Corruption), WGI (Worldwide Governance Indicators), Enterprise Surveys from the World Bank, Corruption Index from

ICRG (International Country Risk Guide), and World Bank data. Other proxies involve tracking individuals charged with corruption and documented cases of corruption. Additionally, corruption may be measured by indicators such as Corruption Experience, corruption risks, and the legal processes initiated, investigated, and adjudicated. When examining government size, the most commonly used measure is Government's final consumption expenditures as a percentage of GDP. However, other measures, such as Government's investment expenditures as a share of GDP and the number of public employees, are also utilized.

[8] We use Stata commands "meta forestplot" and "meta summarize" to graphically and analytically show the presence of significant heterogeneity in our sample, both within and between studies.

[9] Other corruption data sources include: Business Environment Risk Intelligence at http://www.beri.com/; Dreher et al. (2007) index at http://129.3.20.41/eps/pe/papers/0406/ 0406004.pdf; the Economist Intelligence Unit Country Risk Service and Democracy Index at http://www.eiu.com/public/#; and the Sachs and Warner (1997) index at http://jae.oxfordjournals.org/ content/6/3/335.full.pdf}html.

[10] We employ this country-level observable only as an additional robustness check although we are aware that the use of this variable has some limitations. For example, when the primary estimation refers to a group of countries or to panel data, its value is the average corruption level for the entire group. Nonetheless, including *Countries corruption* emphasizes that all other estimated coefficients appear quite robust.

[11] Since many primary papers provide very few estimations (see Table A1 in the Appendix), we cannot compute clustered standard errors.

[12] Empirical results of FAT-PET-PEESE are shown in the Appendix where full models are also shown. Findings are quite robust (see Table A4 in the Appendix).

[13] We re-estimated the same models replacing the time trend based on the year of publication with the year of estimation. The results are quite robust and available on request from the authors.

[14] Our dependent variable is PCC; hence, direct economic interpretations are excluded. However, some general indications can be drawn.

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## APPENDIX

**TABLE A1** An overview of the papers included in our meta-dataset.

| Studies | Number of estimates | Corruption data | Government size data | Sign of the estimated coefficients | Significant |
|---|---|---|---|---|---|
| Adsera et al. (2003) | 4 | ICRG, Other | World Bank, Other | Positive[a] | Yes, no |
| Ali and Isse (2002) | 1 | TI | Other | Positive | Yes |
| Amegavi (2022) | 11 | ICRG | World bank | Negative | Yes |
| Amegavi et al. (2022) | 6 | Other | World bank | Positive[a] | Yes |
| Amirzadi and Khosrozadeh (2015) | 3 | TI | World bank | Negative[a] | Yes |
| Angelopoulos and Philippopoulos (2005)[b] | 2 | ICRG | Other | Negative[a] | Yes |
| Ariva (2020) | 1 | Other | Other | Positive | Yes |
| Arvate et al. (2010), and Miessi Sanches (2010) | 4 | TI | World bank | Negative[a] | Yes |
| Aswar et al. (2011), and Nopiyanti (2022) | 1 | Other | Other | Positive | Yes |
| Baklouti and Boujelbene (2018) | 8 | TI | World bank | Positive[a] | Yes |
| Bel (2022) | 30 | TI, GCR-WEF, EUBusSurvey, Other | Eurostat | Positive, negative | No, yes |
| Bergh et al. (2012), and Öhrvall (2012)[b] | 9 | Other | Other | Negative | Yes, no |
| Bergh et al. (2017), and Öhrvall (2017) | 8 | Other | Other | Negative | Yes |
| Billger and Goel (2009) | 12 | TI | World bank | Negative | Yes |

(Continues)

**TABLE A1** (Continued)

| Studies | Number of estimates | Corruption data | Government size data | Sign of the estimated coefficients | Significant |
|---|---|---|---|---|---|
| Corrado and Rossetti (2018) | 4 | Other | ISTAT | Positive, negative | Yes, no |
| Del Monte and Papagni (2007) | 18 | Other | Other | Positive, negative | Yes, no |
| Fiorino et al. (2015), and Padovano (2015) | 19 | TI, WGI | IMF | Positive, negative[a] | Yes, no |
| Glaeser and Saks (2006) | 26 | Other | Other | Positive, negative | Yes, no |
| Goel (2014) | 34 | Other | Other | Positive | No, yes |
| Goel and Budak (2006) | 4 | TI | EBRD | Positive[a] | Yes |
| Goel and Korhonen (2009)[b] | 13 | TI | World bank | Negative | Yes |
| Goel and Nelson (1998) | 12 | Other | Other | Positive, negative | Yes, no |
| Goel and Nelson (2007) | 18 | Other | Other | Negative[a] | Yes |
| Goel and Nelson (2010) | 5 | TI | World bank | Negative | Yes |
| Goel and Nelson (2021) | 23 | TI, ICRG | World bank | Positive, negative | Yes |
| Goel et al. (2012), and Rajh (2012) | 8 | Other | Other | Negative | Yes, no |
| Goel et al. (2021), and Ram (2021)[b] | 22 | Enterprise Surveys | World bank | Negative | Yes |
| Goel et al. (2022), and Ram (2022) | 20 | Enterprise Surveys | World bank | Negative | Yes |
| Khan and Majeed (2018) | 14 | ICRG | World bank | Positive[a] | Yes |
| Khodapanah et al. (2022), and Shojaeian (2022) | 6 | TI | World bank | Negative, positive[a] | Yes |
| Kiswanto and Fitriani (2019) | 2 | Other | Other | Positive, negative | Yes |
| Kotera et al. (2010), and Samreth (2010)[b] | 9 | TI | Other | Positive, negative | Yes, no |
| Kotera et al. (2012) | 22 | TI, WGI | World bank | Positive, negative | Yes |
| Lash and Batavia (2013) | 5 | TI | World bank | Negative[a] | Yes |
| Montinola and Jackman (2002) | 7 | BI | Penn World Table | Positive[a] | Yes |
| O'Connor and Fischer (2012) | 3 | Other | World bank | Positive[a] | Yes, no |

(Continues)

**TABLE A1** (Continued)

| Studies | Number of estimates | Corruption data | Government size data | Sign of the estimated coefficients | Significant |
|---|---|---|---|---|---|
| Paiva et al. (2021), and Gomes (2021) | 4 | Other | Other | Negative | Yes |
| Saha and Ali (2017) | 15 | ICRG | Penn World Table | Positive | Yes |
| Shabbir and Butt (2014) | 6 | TI | World bank | Positive, negative | Yes |
| Themudo (2014) | 8 | TI | Frasier Institute | Negative | Yes |
| Treisman (2000) | 6 | TI | Other | Negative | Yes, no |
| Visković et al. (2021), and Herman (2021) | 2 | WGI | Penn World Table | Negative[a] | Yes |
| Zhou and Tao (2009) | 10 | Other | Other | Positive | Yes |
| Zhao and Xu (2015) | 5 | TI | World bank | Positive[a] | Yes |
| *TOTAL 44* | 450 | | | | |

*Note*: Authors' processsingelaboration.

Abbreviations: GCR-WEF, The Global Competitiveness Report—World Economic Forum; EUBusSurvey, Eurobarometer Businesses' Attitudes toward Corruption; ICRG, International Country Risk Guide; TI, Transparency International; WGI, Worldwide Governance Indicators; Enterprise Surveys from the World Bank.

[a]Indicates the primary papers that consider higher values of the corruption variable as the lack of corruption.

[b]Stands for working paper.

**TABLE A2** Correlation matrix (obs = 450).

| | Precision effect (1/Ser) | Published | Panel | Year of pub_trend | Endogeneity | Least Square | Perceived corruption | Charges for corruption | Govsize expenditure | Govsize employment | Countries corruption | Dummy_ABS |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Precision effect (1/Ser) | 1 | | | | | | | | | | | |
| Published | −0.3184 | 1 | | | | | | | | | | |
| Panel | 0.1249 | 0.2201 | 1 | | | | | | | | | |
| Year of publication_trend | 0.3866 | −0.0319 | 0.2965 | 1 | | | | | | | | |
| Endogeneity | 0.0655 | 0.0180 | 0.0336 | −0.0054 | 1 | | | | | | | |
| Least square | −0.2974 | 0.0287 | 0.0044 | −0.1451 | −0.5430 | 1 | | | | | | |
| Perceived corruption | 0.0147 | −0.0443 | 0.0194 | 0.2568 | −0.0673 | 0.0550 | 1 | | | | | |
| Charges for corruption | −0.1906 | 0.2020 | 0.1103 | −0.3717 | 0.0712 | 0.0198 | −0.4756 | 1 | | | | |
| Govsize expenditure | 0.2087 | −0.0951 | −0.0254 | 0.4198 | 0.0898 | −0.1698 | 0.3347 | −0.5175 | 1 | | | |
| Govsize employment | −0.1640 | 0.1827 | 0.0632 | −0.3120 | −0.0291 | 0.0285 | −0.3620 | 0.4339 | **−0.7894** | 1 | | |
| Countries corruption | −0.2276 | 0.0522 | −0.0588 | −0.3462 | 0.0707 | 0.0903 | −0.3700 | 0.3965 | −0.4228 | 0.3718 | 1 | |
| Dummy_ABS | −0.1478 | 0.1519 | −0.3473 | −0.5032 | −0.0627 | −0.0091 | −0.2445 | −0.0381 | −0.202 | 0.284 | 0.3490 | 1 |

*Note*: Authors' elaboration.

**TABLE A3** Heterogeneity summary.

| Study | Authors | Year of publication | *df* | *Q* | $p > Q$ | $\tau^2$ | % I2 | H2 |
|---|---|---|---|---|---|---|---|---|
| 1 | Adsera et al. | 2003 | 3 | 1.64 | .65 | 0 | 0 | 1 |
| 2 | Ali and Isse | 2002 | 0 | 0 | . | 0 | . | . |
| 3 | Corrado and Rossetti | 2018 | 3 | 5.36 | .147 | 0.005 | 43.81 | 1.78 |
| 4 | Del Monte and Papagni | 2007 | 17 | 42.72 | .001 | 0.003 | 59.62 | 2.48 |
| 5 | Fiorino et al. | 2015 | 18 | 69.42 | 0 | 0.027 | 79.46 | 4.87 |
| 6 | Glaeser and Saks | 2006 | 25 | 74.15 | 0 | 0.051 | 65.49 | 2.9 |
| 7 | Goel et al. | 2021 | 21 | 2.60E+05 | 0 | 0.053 | 99.97 | 3115.36 |
| 8 | Goel and Nelson | 1998 | 11 | 150.12 | 0 | 0.077 | 90.96 | 11.06 |
| 9 | Kotera et al. | 2012 | 21 | 190.66 | 0 | 0.032 | 90.38 | 10.4 |
| 10 | Saha, S., Ali | 2017 | 14 | 8.96 | .833 | 0 | 0 | 1 |
| 11 | Montinola and Jackman | 2002 | 6 | 6.99 | .322 | 0.002 | 10.88 | 1.12 |
| 12 | Treisman | 2000 | 5 | 0.26 | .998 | 0 | 0 | 1 |
| 13 | Billger and Goel | 2009 | 11 | 22.06 | .024 | 0.01 | 50.1 | 2 |
| 14 | Goel and Korhonen | 2009 | 12 | 23.4 | .024 | 0.009 | 48.75 | 1.95 |
| 15 | Goel et al. | 2022 | 19 | 2.40E+05 | 0 | 0.055 | 99.97 | 3773.02 |
| 16 | Kotera et al. | 2010 | 8 | 39.85 | 0 | 0.007 | 81.92 | 5.53 |
| 17 | Zhou and Tao | 2009 | 9 | 139.62 | 0 | 0.03 | 93.26 | 14.84 |
| 18 | Amegavi et al. | 2022 | 5 | 49.86 | 0 | 0.08 | 90.42 | 10.44 |
| 19 | Zhou and Xu | 2015 | 4 | 2.32 | .678 | 0 | 0 | 1 |
| 20 | Amegavi | 2022 | 10 | 12.07 | .281 | 0 | 17.02 | 1.21 |
| 21 | Amirzadi and Khosrozadeh | 2015 | 2 | 0 | 1 | 0 | 0 | 1 |
| 22 | Aswar et al. | 2022 | 0 | 0 | . | 0 | . | . |
| 23 | Themudo | 2014 | 7 | 15.5 | .03 | 0.047 | 55.58 | 2.25 |
| 24 | Baklouti and Boujelbene | 2018 | 7 | 12.89 | .075 | 0.005 | 45.18 | 1.82 |
| 25 | Bel | 2022 | 29 | 35.32 | .194 | 0 | 0 | 1 |
| 26 | Bergh et al. | 2017 | 7 | 0.39 | 1 | 0 | 0.04 | 1 |
| 27 | Shabbir and Butt | 2014 | 5 | 63.92 | 0 | 0.081 | 91.98 | 12.47 |
| 28 | Arvate et al. | 2010 | 3 | 17.89 | 0 | 0.032 | 78.91 | 4.74 |
| 29 | Paiva et al. | 2021 | 3 | 6.42 | .093 | 0 | 51.79 | 2.07 |
| 30 | Bergh et al. | 2012 | 8 | 5.58 | .694 | 0 | 0.04 | 1 |
| 31 | Lash and Batavia | 2013 | 4 | 38.24 | 0 | 0.05 | 93.38 | 15.1 |
| 32 | Khodapanah et al. | 2022 | 5 | 28.86 | 0 | 0.028 | 82.62 | 5.75 |
| 33 | Ariva | 2020 | 0 | 0 | . | 0 | . | . |
| 34 | Goel and Budak | 2006 | 3 | 0.21 | .977 | 0 | 0 | 1 |
| 35 | Goel et al. | 2012 | 7 | 7.53 | .376 | 0 | 6.63 | 1.07 |
| 36 | Goel and Nelson | 2021 | 22 | 41.27 | .008 | 0 | 0 | 1 |
| 37 | Khan and Majeed | 2018 | 13 | 76.63 | 0 | 0.021 | 96.64 | 29.77 |
| 38 | Angelopoulos and Philippopoulos | 2005 | 1 | 0.03 | .874 | 0 | 0.01 | 1 |

(Continues)

**TABLE A3** (Continued)

| Study | Authors | Year of publication | *df* | *Q* | *p* > *Q* | $\tau^2$ | % I2 | H2 |
|---|---|---|---|---|---|---|---|---|
| 39 | Goel and Nelson | 2007 | 17 | 79.44 | 0 | 0.078 | 81.62 | 5.44 |
| 40 | O'Connor and Fischer | 2012 | 2 | 3.4 | .183 | 0.012 | 41.46 | 1.71 |
| 41 | Visković et al | 2021 | 1 | 0 | .966 | 0 | 0 | 1 |
| 42 | Kiswanto et al. | 2019 | 1 | 2.98 | .084 | 0.036 | 66.43 | 2.98 |
| 43 | Goel | 2014 | 33 | 130.73 | 0 | 0.06 | 70.85 | 3.43 |
| 44 | Goel and Nelson | 2010 | 4 | 0.17 | .996 | 0 | 0.01 | 1 |
| Overall | | | 449 | 5.50E+05 | 0 | 0.06 | 99.75 | 407.39 |

*Note*: Authors' elaboration. Test of group differences: Q_b = chi$^2$(43) = 1246.51; Prob > Q_b = 0.000. *df* stands for degrees of freedom; columns 5–9 report statistics and tests to account for heterogeneity within and between studies (Higgins & Thompson, 2002).

**TABLE A4** PET-FAT-PEESE and full models.

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | **Baseline models** | | | | **Robustness checks** | | |
| | **FAT-PET-PEESE** | **Study design** | **Estimation method** | **Specific variables** | **Govsize employment** | **Countries observables** | **Quality of journal** |
| $SEr^2$ | 1.7307*** | 1.4630** | 1.3307** | 0.6653 | 0.4201 | 0.4282 | 0.7186 |
| | (0.6359) | (0.6035) | (0.6220) | (0.7298) | (0.7855) | (0.7969) | (0.7253) |
| $\beta_0$ (Precision term) | 0.0059 | −0.1280*** | −0.1236*** | −0.1017** | −0.2320*** | −0.1494*** | −0.0758 |
| | (0.0130) | (0.0411) | (0.0462) | (0.0485) | (0.0486) | (0.0501) | (0.0532) |
| Published | | 0.0838*** | 0.0852*** | 0.0650** | 0.0335 | 0.0650** | 0.0732** |
| | | (0.0303) | (0.0301) | (0.0311) | (0.0329) | (0.0307) | (0.0353) |
| Panel | | 0.1370*** | 0.1379*** | 0.0945*** | 0.0868** | 0.0984*** | 0.0895** |
| | | (0.0341) | (0.0342) | (0.0359) | (0.0347) | (0.0367) | (0.0374) |
| Year of publication_trend | | −0.0027 | −0.0029 | 0.0047* | 0.0052** | 0.0055** | 0.0039 |
| | | (0.0021) | (0.0022) | (0.0025) | (0.0024) | (0.0024) | (0.0026) |
| Endogeneity | | | −0.0314 | −0.0326 | −0.0254 | −0.0446 | −0.0358 |
| | | | (0.0336) | (0.0336) | (0.0333) | (0.0347) | (0.0344) |
| Least square | | | 0.0088 | 0.0114 | 0.0278 | 0.0024 | 0.0083 |
| | | | (0.0288) | (0.0272) | (0.0270) | (0.0278) | (0.0273) |
| Perceived corruption | | | | −0.0617* | −0.0284 | −0.0348 | −0.0702* |
| | | | | (0.0341) | (0.0349) | (0.0379) | (0.0365) |
| Charges for corruption | | | | 0.1104** | 0.1212*** | 0.1140** | 0.0972* |
| | | | | (0.0470) | (0.0453) | (0.0474) | (0.0503) |
| Govsize expenditure | | | | −0.1154*** | | −0.1111*** | −0.1155*** |
| | | | | (0.0311) | | (0.0305) | (0.0314) |
| Govsize employment | | | | | 0.2229*** | | |
| | | | | | (0.0381) | | |

(Continues)

**TABLE A4** (Continued)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---|---|---|---|---|---|---|---|
| | **Baseline models** | | | | **Robustness checks** | | |
| | **FAT-PET-PEESE** | **Study design** | **Estimation method** | **Specific variables** | **Govsize employment** | **Countries observables** | **Quality of journal** |
| Countries corruption | | | | | | 0.0435** | |
| | | | | | | (0.0188) | |
| Dummy_ABS | | | | | | | −0.0332 |
| | | | | | | | (0.0436) |
| Observations | 450 | 450 | 450 | 450 | 450 | 450 | 450 |
| F | 4.762 | 9.887 | 7.038 | 8.949 | 12.62 | 8.209 | 8.332 |
| Prob > $F$ | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |

*Note*: Authors' elaboration. The dependent variable of the models is the partial correlation coefficient (PCC). Significance levels and standard errors are derived from WLS estimations conducted as the second step, following the procedure of Gallet and Doucouliagos (2014).

Significance levels: ***$p < 0.01$, **$p < 0.5$, *$p < 0.1$.