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Effects of Public Sector Wages on Corruption

Wage Inequality Matters

Asli Demirgüç-Kunt
Michael Lokshin
Vladimir Kolchin

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Abstract

The paper uses a new country-level, panel data set to study the effect of public sector wages on corruption. The results show that wage inequality in the public sector is an important determinant of the effectiveness of anti-corruption policies. Increasing the wages of public officials could help reduce corruption in countries with low public sector wage inequality. In countries where public sector wages are highly unequal, however, raising the wages of government employees could increase corruption. These results are robust to a wide range of empirical model specifications, estimation methods, and distributional assumptions. The relation persists when controlling for latent omitted variables, using the share of contracts in the private sector as an instrument for the public-private wage differential. Combining increases in public sector wages with policies affecting the wage distribution could help policy makers design cost-effective programs to reduce corruption in their countries.

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Effects of Public Sector Wages on Corruption:

Wage Inequality Matters.

Asli Demirgüç-Kunt, Michael Lokshin, and Vladimir Kolchin*

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1. Introduction

Anti-corruption policies in many countries rely on the notion that corruption is caused by low wages in the public sector. In attempts to curtail corruption, Argentina, Georgia, Ghana, Peru, Singapore, and other countries have implemented public sector reforms to increase the wages of government officials.

The evidence on the effectiveness of such interventions is mixed. Some studies find that higher wages in the public sector were associated with lower corruption (Van Rijckeghem and Weder 2001, An and Kweon 2017). Others find no significant effect (Panizza 2001, Ades and DiTella 1997, and Treisman 2000 2007) or reverse causation, with high levels of corruption leading to low wages in the public sector (Rose-Ackerman and Søreide, 2012).

Differences in the availability, quality, and comparability of data, as well as methodological issues related to the potential effects of unobservable factors, account for the mixed results (Treisman 2007). Newly available cross-country data from the Worldwide Bureaucracy Indicators database allow us to address many of these problems and present new results on the effectiveness of increasing wages as an anti-corruption measure.

Our findings suggest that the distribution of wages in the public sector could be an important determinant of the effectiveness of anti-corruption policies.[1] The inconclusive results of previous studies on the impact of higher wages on corruption could be explained by heterogeneity of these effects with respect to wage inequality in the public sector. We find that wages in the public sector have no significant impact on the level of corruption in a country, on average, but that higher wages may reduce corruption in countries with relatively compressed wages in the public sector. In contrast, increases in the wages of public servants can encourage corruption if public sector wages are highly unequal. Combining the increases in public sector wages with public sector wage decompression might allow policy makers to design cost-effective programs to reduce corruption in their countries.

[1] Meyer-Sahling et al. (2018) show that the effect of pay levels on corruption may be context specific, depending on a range of characteristics of pay systems, including the public wage compression ratio. To our knowledge, our paper is the first empirical work to address that issue.

The longitudinal structure of our data allows us to tackle a range of econometric issues that previous studies could not address. We also use indicators obtained from micro-level surveys. Most cross-country studies of corruption and wages rely on macro-level data to derive the public-private wage premium (e.g., Van Rijckeghem and Weder 2001, An and Kweon 2017, Treisman 2000). Such an approach is associated with persistent measurement errors and fails to control for age, gender, education, location, and other individual characteristics in deriving the public wage premium (Schiavo-Campo et al. 1997, Le et al. 2018). This "unadjusted" wage differential captures the differences between the characteristics of workers in the public and private sectors and not the differences in returns to these characteristics. Our analysis relies on an "adjusted pay premium" that reflects the differences in wages between comparable workers employed in the two sectors (Borjas 2012). Our results are robust to a wide range of empirical model specifications, estimation methods, and distributional assumptions. We address the reverse causation and potential omitted variable bias by using the share of contracts in the private sector as an instrument for public-private wage differential.

The next section reviews the literature on the effect of public sector wages on corruption. Section 3 describes the data and main variables. Section 4 presents the empirical model. Section 5 presents the main results. Section 6 addresses the endogeneity of the public-private wage differential. Section 7 presents the robustness checks, and Section 8 concludes with policy implications.

2. Literature review

A large body of literature investigates the effects of the compensation of public sector employees on corruption. The Becker and Stigler (1974) "shirking model" predicts that public officials will engage in corruption if the expected returns from such activities are higher than their expected wage incomes. The "fair wage" hypothesis of Akerlof and Yellen (1990) postulates that public officials engage in corruption until their wage rises to what they perceive to be the fair wage. Fair compensation of public employees may lead societies to condemn corruption rather than perceive it as an instilled cultural norm (Van Rijckeghem and Weder, 2001). The empirical evidence on the effect of public sector wages on corruption remains inconclusive.

Little micro-level empirical literature is available on corruption and wages, given the difficulties of collecting good-quality data (Gans-Morse et al. 2017, Olken and Pande 2012). Individuals who use public office for private gain have no incentives to reveal information that may compromise them (e.g., Ackerman 2016).

Foltz and Opoku-Ageyemang (2015) investigate the effect of doubling the salaries of police officers on bribe extortions from truck drivers in Ghana. They find that the hike in police salary increased the amount and frequency of bribes truck drivers paid to the police. The authors conjecture that the wage reform raised the social status of the officers and changed their reference of the "fair" income level, leading to upward revisions of the expected amounts of bribes. Mishra et al. (2008) look at the effects of a 1997 pay reform in India that increased the wages of customs officials. They find that the reform had no impact on tariff evasion: Officials kept taking bribes at the same rate after receiving pay increases. Light (2013) argues that a drastic increase in the wages of police officers as part of the police system's reform led to a significant reduction in corruption in Georgia. Di Tella and Schargrodsky (2003) study the nexus between wage premiums and corruption in Argentina. They find that frequent audits reduce corruption. However, the higher wages paid to procurement officers fail to reduce corruption when the probability of detection is either low or very high; larger wage premiums combined with intermediate auditing levels reduce corruption.

The evidence on the impact of wage decreases on corruption is more conclusive. Public sector wage cuts are likely to spur corruption levels. Borcan et al. (2015) investigate the effect of an unanticipated 25 percent wage reduction in public schools on the passing rates on standardized exams in Romania. They find that the share of students who passed the exams in public schools relative to private schools increased. They attribute that difference to increased corruption among public school teachers. Gorodnichenko and Peter (2007) find that public sector employees in Ukraine who are underpaid relative to their counterparts in the private sector may compensate for that difference by taking bribes. They note that the wage gap between the public and private sectors widens at the top of the wage distribution, suggesting that decompressing public sector wages might curb corruption.

Although they provide useful country-level evidence, the findings of single-country studies may suffer from the problem of external validity. The issues of corruption are so multidimensional, and the effectiveness of different measures to fight corruption depends on so many factors (including legal, institutional, and cultural factors, which are often unobservable to researchers) that it may be difficult to generalize the experience of one country to others.

Several studies analyze the relationship between corruption and public-private wage differentials in a cross-country setting. Goel and Rich (1989) document that the incidence of bribery convictions of civil servants is inversely related to the public-private wage premium across states in the United States. Van Rijckeghem and Weder (2000) find a negative correlation between the wage premium for public sector employees and corruption in developing and lower-income countries. They estimate that paying public employees twice the average private sector wage is associated with a decrease of 0.5 point on a corruption measure that ranges from 0 to 6.

Studying countries in Latin America and the Caribbean, Panizza (2001) finds no significant correlation between corruption and the public-private wage differential. However, he reports a significant positive correlation between corruption and the public-private wage differential for formal sector workers with low education. Le et al. (2013) find a negative relationship between government wages and corruption in a large set of countries. They find that the impact of wages on corruption is stronger in low-income countries. An and Kweon (2017) analyze a panel of 43 countries between 1999 and 2008. They find that the public sector wage premium has a modest effect on reducing corruption. The average non–OECD country would need to increase public sector compensation by a factor of 10 to reduce its corruption to the levels of the OECD countries, according to their study.

Several macro-level studies argue that wage premiums have either very limited or no impact on curbing corruption (Alt and Lassen 2014, Dahlström et al. 2012, Rauch and Evans 2000, Treisman 2000). Dahlström et al. (2012) suggest that corruption is affected by the meritocratic recruitment of public workers rather than by their remuneration levels. Treisman (2000) reports an insignificant correlation between wages and corruption in different specifications based on a small sample of countries.

An and Kweon (2017) is the only study known to us that addresses omitted variable bias by applying country fixed effect (FE) estimation. All the other studies cited above rely on estimators that exploit cross-country variation and therefore have a greater risk of introducing omitted variable bias. In addition, all the cross-country studies cited above except Le et al. (2013) and Panizza (2011) use macro-level data to impute the average wages of public and private sector workers. Wage differentials estimated from micro-level survey data hold several advantages over differentials constructed from macro statistics.

3. Empirical specification

Our empirical strategy relies on theoretical frameworks developed to explain the incentives of public officials to engage in corruption. Traditional static frameworks (such as the models of Becker and Stigler 1974, Besley and McLaren 1993, and Akerlof and Yellen 1994) and the more recent dynamic principal-agent models (e.g., Sosa 2004, An and Kweon 2017) posit that officials are less likely to commit an act of corruption the higher their wages, the higher the expected penalty on detected corruption, and the lower the potential corruption rent. We form our empirical specification based on the predictions of the theoretical models and the variables used in the previous studies.

Our country-level model relates an indicator of corruption with the public-private wage differential, variables reflecting the probability of detecting illicit acts, and a set of controls to account for country differences. The baseline specification has the following form:

$$CI_{i,t} = \beta WP_{i,t} + \pi X_{i,t} + \gamma Y_t + v_i + \varepsilon_{i,t}\ , \quad (1)$$

where $CI_{i,t}$ is the corruption indicator for country i on date t; $WP_{i,t}$ is a measure of the public-private wage premium; $X_{i,t}$ is a set of the country- and time-specific controls; Y_t is a vector of year dummies; v_i is the country-specific fixed effect; $\varepsilon_{i,t}$ is an *i.i.d.* innovation term; and $\beta, \pi,$ and γ are the estimated parameters.

We extend specification (1) by adding a measure of wage inequality within the public sector and its interaction with the wage differential. The research shows that compressed distributions of wages in the public sector induce the sorting of employees with different characteristics into and out of the public sector if tight public sector wage compression prevents them from getting the wages they desire (see, e.g., Borjas 2012, Hausman et al. 2020). Highly skilled people in the upper tail of the wage distribution may be less inclined to consider jobs in the public sector. Such non-random sorting may affect the levels of corruption. The wage compression ratio is also used as one of the main indicators for evaluating government performance and compensation (see, e.g., Clements et al. 2010). The new specification becomes:

$$CI_{i,t} = \beta_1 WP_{i,t} + \beta_2 WC_{i,t} + \beta_3 (WP_{i,t} \times WC_{i,t}) + \pi X_{i,t} + \gamma Y_t + v_i + \epsilon_{i,t} \, , \qquad (2)$$

where β_2 is a coefficient on the inequality of wages in the public sector ($WC_{i,t}$), and β_3 is the coefficient on the interaction term.

The country-specific unobserved fixed effects (v_i) may be correlated with our variables of interest, potentially resulting in biased estimates. Assuming that these unobserved effects are time-invariant, such endogeneity could be addressed by estimating (1) and (2) by the FE regressions (e.g., Wooldridge 2001). We introduce time-effect dummies to remove the aggregate variation caused by global or regional shocks. We also assume that some important omitted variables, such as cultural and social norms toward corruption, are stable over time. For example, Fisman and Miguel (2007) present evidence on the "stickiness" of corruption cultures.[2] We perform a number of robustness checks to ensure the internal validity of our results.

Besley and McLaren (1993) raise the possibility that corrupt countries may deliberately pay low wages to government officials to maintain the corrupt bureaucracy. Bribes could be perceived as compensation for the low pay of government employees in some countries (Van Rijckeghem and Weder 2001). Alternatively, when corruption is a drain on public resources, the government cannot afford high wages (Di Tella and Van Rijckeghem 2001). These arguments indicate a potential reverse causation problem: Corruption could affect wages in the public sector. We address the reverse causality problem by using an instrument for the wage differential in equation (2).

We employ a range of approaches to test the robustness of our results. First, we estimate equations (1) and (2) using pooled ordinary least squares (OLS) regression and OLS using one observation per country by averaging all variables for each country in the sample (see, e.g., Van Rijckeghem and Weder 2001). Second, in the panel setup, we estimate equations (1) and (2) by random effect regressions and test the validity of random effect versus FE assumptions. Third, we examine the robustness of our results by varying the length of the panel. Fourth, we use extreme bound analysis, as in, for example, Levine and Renelt (1992).

[2] Fisman and Miguel (2007) argue that cultural norms related to corruption are deeply engrained. For example, public officials from Scandinavian countries exhibit rule-compliant behavior even when the threat of legal enforcement is absent.

4. Data and variable definitions

We use three measures of corruption. The Control of Corruption Indicator comes from the Worldwide Governance Indicators (WGI) database produced by the World Bank annually since 1996 for over 200 countries (Kaufmann et al. 2010). The indicator reflects perceptions of both petty and grand forms of corruption and "capture" of the state by elites and private interests. We refer to this measure as *Corruption_WGI*.

Since 2007, the World Economic Forum (WEF) has produced the Ethics and Corruption Indicator for over 140 countries (World Economic Forum 2018). It reflects respondents' perceptions of whether governments prevent the illegal diversion of public funds and how frequently investors and companies make unofficial payments. We refer to this measure as *Corruption_WEF*.

The Corruption Perception Index (CPI) comes from Transparency International (2020). It is a composite index of corruption in the public sector as perceived by experts and businesspeople. TI has been producing the CPI annually since 1996 for over 180 countries. A change in the methodology in 2012 constrains the time series to the period from 2012 to 2018. We refer to this measure as *Corruption_TI.*[3] We normalize all corruption indicators to be between 0 and 1, where 0 indicates the lowest and 1 the highest level of perceived corruption.[4]

Table 1 summarizes the statistics for the main variables used in our analysis. The WGI corruption index ranks Paraguay and the Russian Federation as the most corrupt countries in our sample and

[3] Table A1 in the appendix lists the countries we use to estimate the minimum and maximum values of each of the three corruption indicators. We considered using three other indicators of corruption: the Absence of Corruption component of the World Justice Project's (WJP) Rule of Law Index (World Justice Project 2020), the Anti-Corruption Policy component of the Bertelsmann Transformation Index (BTI) (Bertelsmann Stiftung 2020), and the Corruption component of the International Country Risk Guide (ICRG) (PRS Group 2020). The first two indicators significantly reduce the number of countries in the sample. The WJP data are not comparable before 2015, which results in fewer than 80 observations for 17 countries. The BTI includes only developing countries and transition economies, limiting the number of countries in our analysis to 19. We decided not to use the ICRG indicator following Knack (2006) and Treisman (2007), who advise against using this cross-country corruption indicator for longitudinal analysis.

[4] Several studies (e.g., Knack 2006) question the validity of using the WGI and TI corruption indicators for longitudinal analysis. Changes in data sources and in the methodology of constructing indicators are among the main issues that can affect the results of longitudinal estimates. We are aware of these critiques and address them in Section 6 by replicating our results on panels of different lengths and using lagged independent variables.

Finland as the least corrupt. Finland also has the lowest score on the TI corruption index, on which Honduras and Greece rank as most corrupt.[5]

Most of our control variables come from the Worldwide Bureaucracy Indicators (WWBI) data set (World Bank 2020b). The main feature of this data set is that it derives country-level indicators from micro-level labor force surveys. Differences in wages between the public and private sectors derived from macro data are persistently biased compared with differences obtained from micro-level surveys (Le et al. 2018). Any distributional statistics, such as wage compression ratios, cannot be derived from macro-level data at all.

The WWBI data set is a panel of 132 countries covering the period 2000–19. Forty-four countries in the data set have at least 4 panel observations, 41 countries have at least 6, 36 countries have at least 8, and 33 countries have at least 10. The sample has 454 or 507 country-year observations depending on the measure of the public-private wage differential we use.

The public sector wage premium is estimated as a coefficient on the public sector employment dummy in the standard log-earnings regressions for each country (Mincer 1974).[6] The sample sizes for these estimations range from 6,799 observations for Russia to 1.8 million for Brazil. The WWBI data set contains two standard measures of the public sector wage premium ($WP_{i,t}$). The first is estimated on a sample of public sector workers and their counterparts in the formal private sector. The second is estimated on a sample that includes employees of both the formal and informal parts of the private sector. Our baseline specification uses the wage differential between the public and formal private sector workers, as public employees are more likely to compare their wages with wages in the formal private sector (Goel and Rich 1989).

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[5] Russia and Paraguay are not included in the TI sample because both countries have fewer than five longitudinal observations between 2012 and 2018.

[6] The public sector includes the central government, nongovernmental organizations, the armed forces, and state-owned companies. The private sector is the part of the economy that is both run for private profit and not controlled by the state (World Bank 2020b). The public–private wage differential is estimated on the sample of employees in each country. Formally, the empirical specification for this estimation is $\ln(wage_i) = \alpha + \beta Public_i + \pi X_i + \varepsilon_i$, where $Public_i$ equals 1 if a person is employed in the public sector and X_i is the set of controls, which include age, age squared, gender, education, and location of a worker. The estimated coefficient β is then delogged and reduced by 1 ($\exp(\hat{\beta}) - 1$). The resulting wage differentials are negative if public sector wages are lower than the private sector wages and positive otherwise (Gindling et al. 2020).

The WWBI data set also includes public sector wage premium indicators derived for different occupations. The first compares the wages of senior public officials in the public sector with employees in related occupations in the formal private sector. The second compares the wages of all professionals with employees in corresponding occupations in the formal private sector. Both indicators capture specific dimensions of the wage differential. This occupational metric is available only for comparing public sector workers and their counterparts in the formal private sector. We use these indicators to validate our main results.

In our sample, public sector workers earn 5.6 percent more than their comparators in the formal private sector on average. The public sector premium increases to 15.1 percent when employees of the informal sector are also accounted for, a finding similar to that of Gindling et al. (2020). Using the formal private sector for comparison, Peru (–34.2 percent) and the Dominican Republic (–30.1 percent) have the lowest differentials, and Ecuador (50.9 percent) and Cyprus (48.2 percent) have the largest. When all workers in the private sector (formal and informal) are used for comparison, Russia (–29.0 percent) and the Dominican Republic (–16.4 percent) have the lowest wage differentials, and Costa Rica (74.0 percent) and Pakistan (69.2 percent) have the largest.

Our measure of wage dispersion—the wage compression ratio ($WC_{i,t}$)—is defined as a ratio of the weekly wages of the 90th to the 10th percentiles of public sector employees (e.g., Heyman 2008, Almeida-Santos and Mumford 2005, Brunello 2001). In our sample, the Slovak Republic (2.4) and Croatia (2.6) have the lowest wage compression ratios. The public sector wage distribution is most unequal in the Russian Federation (10.3) and Brazil (9.5).[7]

We use several control variables in our estimations. The share of public workers with tertiary education comes from the WWBI data set. The smallest share is in Uruguay (28.3 percent), and the largest shares are in Lithuania (83.9 percent) and Ireland (78.0 percent).

The International Country Risk Guide (ICRG) data set provides the index of quality of the bureaucracy (PRS Group 2020). It gauges how well bureaucratic institutions can deliver public services under political pressure, especially when governments change. The conjecture is that a strong professional bureaucratic body can counter attempts by newly elected politicians to seek

[7] The wage compression ratios in the public and private sectors are positively correlated in our sample, with a Pearson correlation coefficient of 0.633 ($p < 0.000$).

economic rents. Dahlström et al. (2012) show the vital role professional bureaucrats may play in reducing corruption. The quality of the bureaucracy index ranges from 1 to 4. The Dominican Republic, Paraguay, Romania, and the Russian Federation have the lowest scores. Austria, Belgium, Cyprus, Finland, Ireland, Luxemburg, Switzerland, and the United Kingdom all achieved the highest score (4).

We also include a measure of the rule of law from the WGI database to account for the effectiveness of law enforcement institutions in penalizing corrupt behavior. Elbahnasawy and Revier (2012) show that this measure explains variation in corruption indicators at the country level. This indicator ranges from –2.5 to 2.5. In our sample, Honduras (–1.16) and Ecuador (–1.25) rank lowest, and Finland (2.1) and Switzerland (1.9) rank highest.

The country fragility index is produced by the Center for Systemic Peace (Marshall and Cole 2018). It ranges from 0 to 24, with higher values indicating greater state fragility. Fragility can provide fertile ground for corruption because it is associated with economic hardship and weakened control over the public sector. Fifteen countries in our sample received rankings of 0: Austria, Belgium, the Czech Republic, Estonia, Finland, France, Hungary, Ireland, Italy, Latvia, Luxembourg, Poland, Portugal, Spain, and the United Kingdom. Ecuador has the highest fragility rating in our sample (14), followed by Peru and Bolivia, both with ratings of 13.

We also control for GDP per capita (in constant 2011 purchasing power parity dollars) and government final consumption expenditure as a share of GDP (World Bank 2020a). GDP per capita is a standard control variable used in cross-country studies of corruption (Van Rijckeghem and Weder 2001, Panizza 2001, Le et al. 2013, An and Kweon 2017). Government spending could be correlated with corruption levels (Le et al. 2013). The share of government spending is smallest in the Dominican Republic (7.6 percent) and Paraguay (7.3 percent) and largest in Finland (24.6 percent) and Belgium (24.4 percent).

5. Results

Table 2 shows the FE estimations of equations (1) and (2) for *Corruption_WGI*. The first column shows the significant and negative effect of the public sector wage differential on corruption. Countries with higher values of the rule of law indicator have lower levels of corruption. In column (2), we add a variable on the public wage compression ratio. It has no significant effect on corruption. All other coefficients are similar to those in column (1). Column (3) adds the interaction between the public-private wage differential and the wage compression ratio. The wage differential coefficient decreases from –0.077 in specification (2) to –0.479 in specification (3). The coefficient on the interaction term is positive and significant. The effect of the wage differential on corruption, estimated at the sample means, is statistically insignificant (Prob (χ^2) = 0.544). Both higher-quality bureaucracy and the better rule of law reduce corruption.[8] These results are consistent with the findings of other studies (Alt and Lassen 2014, Dahlström et al. 2012). We use specification (3) as our baseline econometric specification for the analysis in this paper.[9] Specification (4) is a specification for the extreme bounds analysis in the sensitivity analysis section.

Table 3 presents estimations based on different definitions of the public-private wage differential and the WGI, WEF, and TI corruption indicators. Specification (1) in Table 3 regresses the *Corruption_WGI* on the wage differential between public sector workers and similar workers in the formal private sector.[10] Specification (2) uses the wage differential estimated on the sample of all workers. The coefficient on the wage differential remains negative but becomes barely significant, and the coefficient on the interaction term loses significance. These changes in significance could be explained by more noisy data in the sample of employees from both the formal and informal private sectors. The estimations of *Corruption_WEF* regression (columns 3 and 4) are similar to the *Corruption_WGI* regression, in both magnitude and sign.

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[8] One could argue that the quality of bureaucracy indicator might be endogenous to corruption. As we noted in the data section, this indicator focuses on a very particular aspect of the quality of bureaucracy and gauges how well bureaucratic institutions can deliver public services under political pressure. We argue that this characteristic might not be causally related to corruption. We also re-estimated the specifications in Table 2 excluding this variable, which resulted in no qualitative change in the coefficients of interest. The quality of bureaucracy indicator varies little over time and is constant for the WEF and TI samples. For that reason, it is not included in these estimations.

[9] We tried adding several other control variables to the main specification to ensure that our results remain robust. Among them are the government efficiency index (WDI), the voice and accountability index (WDI), the law-and-order index (ICRG), and the democracy index (Polity5: Regime Authority Characteristics and Transitions Datasets). Of these variables, only the voice and accountability index is significant in the estimation. Despite its significance, we opted to keep only one control variable from the WGI data set, because WGI variables are highly correlated, and our preference was to keep the model parsimonious. These estimations are available from the authors on request. We use them in the extreme bound analysis in Section 6.

[10] This estimation replicates the results shown in column (3) of Table 2.

Columns (5) and (6) in Table 3 show a similar pair of specifications for *Corruption_TI*. Qualitatively, the results of these estimations confirm the results of our main specification. The wage differential has a strong negative and significant effect on corruption. The wage compression is not significant on its own, but the interaction of the wage differential and public wage compression is significant and positive for both definitions of the wage differential.

We repeat the estimations in Table 3 using two alternative measures of the public-private wage differential. The first compares wages of professionals in the public sector with the wages of corresponding professionals in the formal private sector. The second contrasts the wages of senior officials to the wages of their counterparts from the formal private sector. Table A2 in the appendix shows that estimates based on both measures produce results that are qualitatively similar to our main specification: The coefficients on wage differential variables are negative and significant, and the coefficients on the interaction terms are positive and significant. The effects of the wage differential and the interaction terms have the expected signs but are insignificant in the *Corruption_WEF* regressions. The coefficients in the *Corruption_TI* regressions are consistent with our main results for the estimation, in which we use the wage differential based on the comparison of professional wages. We find no significant results in the regression in which the wage differential is estimated based on the differences in the wages of senior officials.

Figure 1 shows the simulations of the effect of the public-private wage differential on corruption as a function of wage compression in the public sector.[11] This effect is negative and significant for the wage compression ratios, ranging from 1.2 (the minimum in our sample) to about 5. In that range, the negative effect of higher wages on corruption dominates the positive effect of the interaction term. When the wage distribution in the public sector is flat (i.e., the compression ratio is close to 1), a unit increase in the public-private wage differential leads to a 0.39 unit reduction in the corruption index. Changes in public sector wages have no significant impact on corruption when the compression ratio is about 5.5. For compression ratios above 6.0, the positive effect of the wage differential/compression interaction on corruption dominates the negative effect of the higher wages. In countries with such high levels of public sector wage inequality, increases in public servants' wages increase the incidence of corruption.

[11] The simulations are based on specification (3) in Table 2.

Figure 2 depicts a contour plot of the predicted levels of corruption as a function of the public-private wage differential and the wage compression ratio. The contours indicate the areas in which predicted corruption lies within a particular range, with darker colors indicating higher corruption. The plot confirms the results reported in Figure 1. An increase in the wage differential would not reduce corruption for countries like Bulgaria, where the wage compression ratio was 5.21 in 2018, because raising public sector wages would keep Bulgaria in the region of constant corruption. Countries with lower public sector wage inequality, such as Albania and Croatia, might reduce corruption by increasing public sector wages. Higher public sector wages would move these countries from the darker areas of high corruption to lighter areas of lower corruption.

Countries with high wage inequality in the public sector might not benefit from increasing the wages of public employees. In Russia, for example, public sector wages are low, and corruption is high. The natural inclination would be to try raising public sector wages to reduce corruption. However, the high wage inequality in the public sector would counterweight the effect of higher wages on corruption. As a result of a wage increase, Russia is expected to move up from the areas of lower corruption to the areas of higher corruption.

Decompression of public sector wages might reduce corruption for countries with low wages in the public sector, like Albania or Ukraine (the argument developed by Gorodnichenko and Peter 2007). In Figure 2, an increase in public sector wage inequality would move these countries to the right, from the regions of high corruption (darker) to the areas of low corruption (lighter). Wage decompression is expected to have the opposite effect, increasing corruption for countries with relatively high wages in the public sector, like Brazil.

We can estimate the elasticities of corruption with respect to changes in public sector wages. These elasticities differ in magnitude and even change sign depending on the combination of wage rates and the wage distribution. It makes sense to simulate these elasticities for a particular country. For example, doubling public servants' wages while keeping the wage compression constant would decrease the WGI–measured corruption in Albania from 0.74 to 0.52, a 42 percent reduction in the WGI corruption index.[12] This intervention would bring corruption in Albania to the level of Italy.

[12] These elasticities are comparable to the elasticity of –0.26 reported by An and Kweon (2017) for non–OECD countries. Barr, Lindelow, and Serneels (2009) estimate a lower elasticity of corruption with respect to changes in the public sector wages of about –0.15.

The same policy would have an entirely different effect in Brazil, a country with a relatively high wage compression ratio. In Brazil, doubling the wages of government employees would increase corruption from the original value of 0.65 to 0.88, a 26 percent increase. That new level of corruption would correspond to levels in Honduras or Russia.

Neglecting the wage compression ratio would compromise the design of anti-corruption policies. The external validity of the results of previous studies, especially single-country studies, might be questioned if the wage distribution in the public sector is not considered. Several researchers point out the high costs of anti-corruption policies based on raising wages in the public sector (e.g., An and Kweon 2017; Barr, Lindelow, and Serneels 2009, Sosa 2002). Our results demonstrate that policies that combine modest increases in public sector wages with a relaxation of wage constraints for top public officials might be more efficient in reducing corruption in some situations.

A well-established body of literature indicates that, across the world, top public sector officials tend to have zero or even negative pay premiums (e.g., Borjas 2002, Hospido and Moral-Benito 2016). The private sector is almost as generous as the public sector in terms of pensions and fringe benefits in top-level jobs. At the same time, in most countries, the public sector pays higher average wages than the private sector for employees with similar characteristics (Mizala et al. 2011, Gindling et al. 2019). Such a situation puts negative selection pressure on the top talents in the public sector and creates incentives for a positive selection into the public sector of less capable employees. By making wages less compressed, the public sector could align the wages of its top performers with market wages to attract and retain talented and experienced employees.

Wage structures of the public sector are often highly compressed in developed economies. Barth et al. (2013) argue that the complementarity of wage compression and generous welfare states fuel investment and enhance productivity in Scandinavian countries. Compressed wages are also seen as a mechanism for selecting motivated individuals into the public sector (e.g., Barford et al. 2019, Navot et al. 2016).

Explaining our results

We propose several explanations of the mechanisms driving our results. Suppose some kinds of corruption require investment. To engage in corrupt behavior, a public sector employee needs to be connected to the right people in the private sector. Maintaining such connections may be costly.

High inequality in pay in the public sector would allow top managers to access circles—both formal, such as clubs, and informal, such as parties—in which they can interact and build rapport with potential "clients." A proportional rise in the wages of public officials translates into larger absolute increases for the highest-paid officials, which could facilitate their integration into the business community.

Tirole (1992) and Laffont and Martimort (1997) developed a theoretical framework to investigate the possibility of collusion between supervisors and workers. The model illustrates the "incentive effect" of increasing wage spreads within a firm. When a firm raises wages to incentivize hard work, shirkers who are caught are penalized by losing a higher wage bonus. As a result, shirking is reduced. At the same time, higher wage bonuses create more room for arbitrage between employees and their supervisors. A shirker could try bribing his supervisor to avoid that loss. If the performance wage spread is wide enough, the "corruption effect" dominates the "incentive effect." When sectoral wage inequality is high, a proportional wage raise would increase potential losses for shirkers and increase corruption. If wages are flat, "the incentive effect" of higher wages dominates "the corruption effect," and corruption declines.[13]

An alternative explanation comes from studies of corruption in experimental settings. Barr et al. (2009) find that auditors put more effort into exposing highly paid officials. Their results corroborate Abbink's (2004) conclusions, also based on experiments, that distributive fairness notions may affect behavioral responses toward changes in relative pay. Following this logic, raising the wages of public officials when wage inequality is high would result in higher rates of detection of corruption and, consequently, higher perceived corruption.

6. Addressing the endogeneity of the public-private wage premium

As we explained in Section 3, one can argue that our results are driven by reverse causality between corruption and wages in the public sector. We use changes in the share of employees with a contract in the *private sector* as an instrument for the public-private wage differential.[14] We assume that wages in the public sector are anchored to the compensation offered for similar

[13] Khemani (2019) provides empirical support for this theory, using examples from the education sectors of Finland and the Republic of Korea.

[14] We use the broadest definition of a contract, to accommodate variations in definitions across countries, differences in the wording of the corresponding questions, and local understanding of the term.

positions in the private sector, the "prevailing wage" principle (Fogel and Levin 1974). The larger share of contracts makes jobs in the private sector more attractive. Public sector compensation packages adjust to keep the public sector competitive in attracting qualified employees.

Several studies demonstrate the importance of sectoral differences in the type of contracts in determining the public-private wage differential (e.g., Ghinetti and Lucifora 2013, Ramos et al. 2014). But the share of jobs with contracts in the private sector could also be interpreted as a measure of the gray economy in the private sector, which could be correlated with corruption levels. We control for that channel by including a share of the shadow economy in our main specification.[15] With that control, our exclusion restriction is based on the assumption that changes in the share of jobs with contracts in the private sector could affect corruption only through its effect on the public sector wages.

Table 4 shows the results of FE instrumental variables (IV) estimations for the WGI and WEF measures of corruption and two types of wage differentials.[16] We implement the FE IV estimator as a two-stage estimation. In the first stage, we instrument the wage differential with the share of contractual jobs in the private sector. We use the predicted value from this FE regression to construct the interaction term between the predicted wage differential and the wage compression ratio. In the second stage, we estimate the FE regression of the corruption indicator on the predicted wage differential, the constructed interaction term, and the set of exogenous variables.

The first stage regression confirmed that our instrument is a significant predictor of the public-private wage differential.[17] Assuming that all other covariates are exogeneous, if the wage differential is uncorrelated with the omitted variables (and hence the error terms) in our regressions, then the error terms in these regressions will be uncorrelated with the residuals of the

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[15] We use data from the global data set on the shadow economy assembled by Medina and Schneider (2019). As it contains information only on a limited number of countries, we lose a significant number of observations compared with our main specification. For this reason, we do not use the information about the share of the shadow economy in our main analysis. The empirical evidence on the relationship between corruption and the share of the shadow economy is mixed and can vary in the short and long run (Mughal and Schneider 2019).

[16] We failed to achieve identification (the coefficient on the instrument is insignificant in the first stage regression) for the *Corruption_TI* sample.

[17] According to table 4, the share of employees with a contract in the private sector in the first stage had coefficients of 0.292 and 0.758 with standard error of 0.012 and 0.228 for the specifications based on wage differential for formal and all employees, respectively. Their corresponding F-statistics are 5.87 and 11.09.

first-stage regression. Based on robust standard errors, the residuals from the first-stage regression are significant (prob.=0.01) when added to the second stage regression in column (1), implying that we reject exogeneity and need to use the instrument. These residuals are insignificant for specifications (2), (3), and (4), implying that we cannot reject exogeneity. The Hausman tests also passed comfortably for these regressions with controls (as shown in the bottom part of Table 4).

The results of the FE IV estimations are broadly consistent with the results shown in Table 3. The wage differential has a negative and significant effect on corruption for both definitions of the public-private wage gap and for two corruption indicators. The interaction of the wage differential and the wage compression is positive and significant in specifications with formal wage differential, but these coefficients lose significance for specifications based on all private sector wages.[18]

7. Sensitivity analysis

The validity of our results could be questioned because of the challenges of aggregating predictions of micro-level theoretical models to explain inter-country variations in corruption. Theoretical models provide no guidance about the distributional assumptions for the empirical specifications, leaving the choice of estimation methodology to the researcher. Another problem is that the indicators we use in our analysis come from different sources, and the methodology of generating such indicators might change over time. We try to address these issues by applying a range of methods and approaches used in previous research and testing the robustness of our results to different specifications and assumptions.[19]

[18] We tried using changes in the minimum wage as an instrument for the public–private wage differential (similar to Dreher and Schneider 2010). The exclusion restriction here would be that exogenous changes in the minimum wage affect the wage differential but have no direct impact on corruption. The argument against that assumption is that a higher minimum wage might make jobs scarce, inducing people controlling access to them in the public sector to try to extract corruption rents. The results of this FE IV estimation confirmed the main results regarding our variables of interest. Table A3 in the appendix shows these estimations.

[19] We tested and rejected the hypothesis that the wage differential and wage compression ratios, which are from the micro-data, are interdependent, for two reasons First, the two variables are not correlated in any significant way. Second, we estimated a battery of regressions in which we used the wage differential as a dependent variable and the wage compression ratio and all our explanatory variables as controls. The coefficients on the wage compression ratio and the interaction term were insignificant. The results of these estimations are available from the authors on request.

We first test the sensitivity of our results to the number of observations per country in our panel. Our main specification is based on a sample that includes at least eight observations for *Corruption_WGI*, at least eight observations for *Corruption_WEF*, and at least five observations per country for the *Corruption_TI* indicator. Table 5 shows the results of estimations of equation (2) on panels of three different lengths (coefficients and standard errors for the public-private wage differential, the compression ratio, and their interaction). These estimations confirm the findings shown in Tables 2 and 3: The coefficients on the wage differential are negative and significant for the formal sector in the *Corruption_WGI* and *Corruption_WEF* estimations and for both specifications of the *Corruption_TI* estimation. All three coefficients are insignificant in the *Corruption_WGI* and *Corruption_WEF* estimations when the wage differential is calculated for all private sector workers.

Kaufman and Kraay (2002) caution about the potential misinterpretation of the inter-year changes in the WGI; the TI team expressed similar caution about their corruption indicator. Kaufman et al. (2006) argue that changes over longer periods could reflect trends in corruption perceptions more reliably.

To address these concerns, we repeat our estimations on a panel constructed by averaging our variables over four consequent observations. These estimations demonstrate that the wage differential and compression ratio coefficients have the expected signs and are significant for the WGI and WEF/formal private sector specification and marginally significant for the TI/formal private sector specification. The coefficients on these variables are insignificant in specifications in which the wage differential is based on comparison with all private sector workers.

The next part of Table 5 tests whether the results are robust with respect to intertemporal changes in the methods of data collection and data aggregation. In addition, political and economic changes may affect perceived corruption with lags. We estimate our model with independent variables lagged by one or two periods.[20] The estimations with the lagged independent variables confirm the results of our main specification for *Corruption_WGI* (columns 1 and 2) and the specification with

[20] Panel observations in our sample might be spaced by one, two, or more years. When generating lagged independent variables, we use the previous observation for lag 1 and the second-period observation for lag 2.

Corruption_WEF (columns 3 and 4). The coefficients of the variables of interest lose significance in the *Corruption_TI* specifications (columns 5 and 6).

We also estimate our model with a range of econometric techniques used in other studies (see Table 6). Previous studies resorted to OLS based on averaged country values (Van Rijckeghem and Weder 2001), pooled OLS with time fixed effects (Le et al. 2013), and random effect estimation (An and Kweon 2017, Panizza 2001).[21] Both the OLS and random effect estimators assume that errors are uncorrelated with country characteristics included in the model. As we pointed out above, country-specific fixed effects are likely to be correlated with our variables of interest. Because of these methodological deficiencies, we present these results for comparisons with other studies.

Pooled OLS regressions produce estimates that are qualitatively similar to our main results in Table 2. The coefficient on the wage differential is negative and significant, and the coefficient on the interaction term is positive and significant in specification (1), where the dependent variable is *Corruption_WDI* or *Corruption_WEF*. The pooled OLS fails to produce significant estimates for the *Corruption_TI*. OLS estimations based on the averaged data produce no significant coefficients for any of the specifications. The random effect estimation results are very close to our main estimation results.[22]

We perform extreme bounds analysis to test the robustness of our coefficients to changes in the model specification (e.g., Levin and Renelt 1992). We estimate the FE regressions for all possible combinations (permutations) of our control variables.[23] We then identify the highest and lowest values for the coefficients on our variables of interest, β_1 and β_3 in equation (2). The degree of confidence that is warranted can be inferred from the extreme bounds on the coefficients β_1 and β_3. If the coefficients remain significant and their signs are the same at the extreme bounds

[21] The primary reasons for using these approaches are the relatively small sample sizes and the low intra-country variation in the explanatory variables. Le et al. (2013) control for regional effects when using pooled OLS regression.

[22] The Wald test—an analog of the Hausman fixed-versus-random effects test that permits differentiating the models with clustered errors—rejects random effects results as inconsistent.

[23] If n is the number of variables in X (equation 2), the total number of combinations is o $2^n - 1$. In our extreme bounds analysis, we use 10 variables that produce 1,023 unique regression specifications.

as in our baseline estimation (Table 2, specification 3), one can be fairly confident about the validity of our main estimates.

Table 7 shows the results of the extreme bounds analysis for equation (2). At the lower bound, the wage differential coefficient is negative (–0.488) and significant, with a *t*-statistic of –5.38. The upper bound for the coefficient is also negative (–0.297) and significant. Thus, the robust negative relationship between the public-private wage differential and corruption is consistent with a wide range of model specifications. The extreme bounds analysis also indicates the stable positive correlation between corruption levels and the interaction of the wage compression ratio and the public-private wage differential. The estimates of the interaction coefficient range from 0.064 to 0.090. Both lower- and upper-bound estimates are significant, with *t*-statistics of 2.78 and 4.37, respectively. Overall, the results of the extreme bounds analysis confirm the robustness of our estimates under different assumptions and for different empirical specifications.

Although our findings give a strong indication of the importance of the wage distribution in the public sector as a determinant of levels of corruption, they are not conclusive for several reasons. First, we rely on perception-based measures of corruption. Second, our empirical model controls for many factors affecting corruption but omits other important variables because of data limitations or the desire to keep the model simple. Third, we did not address the non-random selection of workers into the public sector, which might affect our results. Future research could use alternative measures of corruption and richer econometric specifications.

8. Conclusions

This paper uses a new country-level, panel data set to study the effect of changes in public sector wages on corruption. This data set contains several previously unavailable indicators derived from micro-level surveys that allow us to analyze the heterogeneity of the effect of the public sector wage on corruption. Our results show that wage inequality in the public sector is an important determinant of the effectiveness of anti-corruption policies. Increasing the wages of public officials could reduce corruption in settings with low wage inequality in the public sector. Raising public employees' wages might lead to a higher incidence of corruption when public sector wages are highly unequal. These results are robust to a wide range of empirical model specifications, estimation methods, and distributional assumptions. The relation persists when controlling for latent omitted variables, using the share of contracts in the private sector as an instrument for the public-private wage differential. Combining the increases in public sector wages with public sector wage decompression might allow policy makers to design cost-efficient programs to reduce corruption in their countries.

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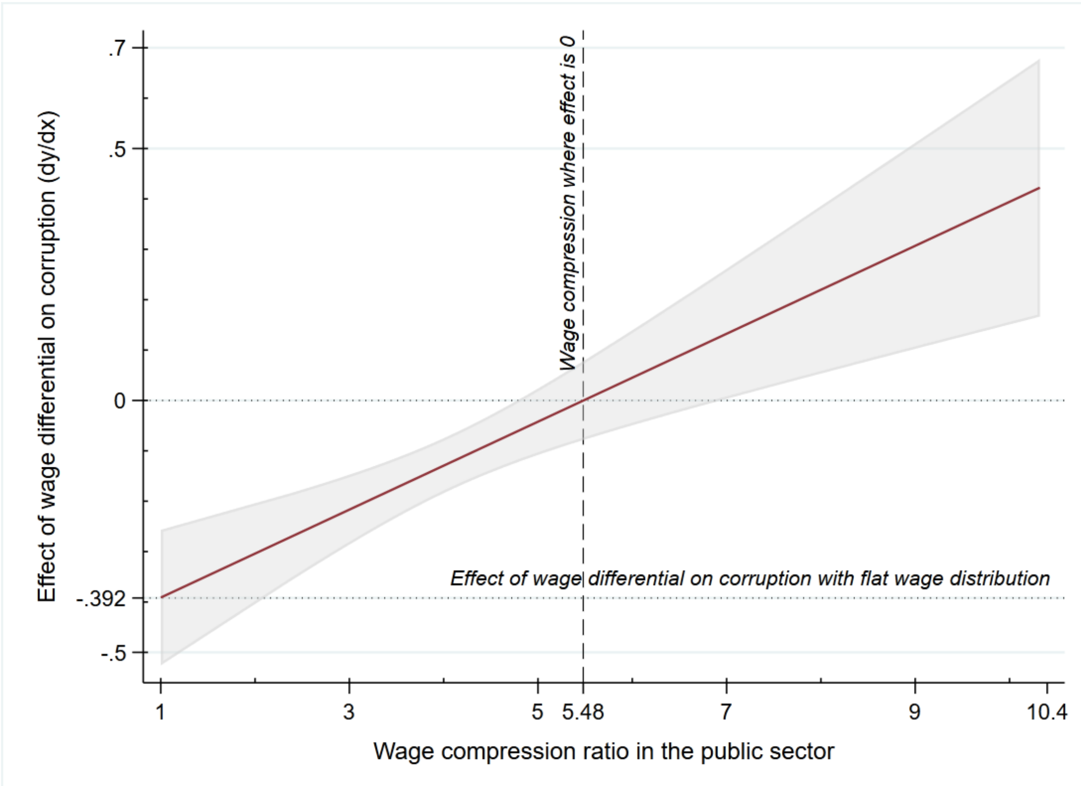
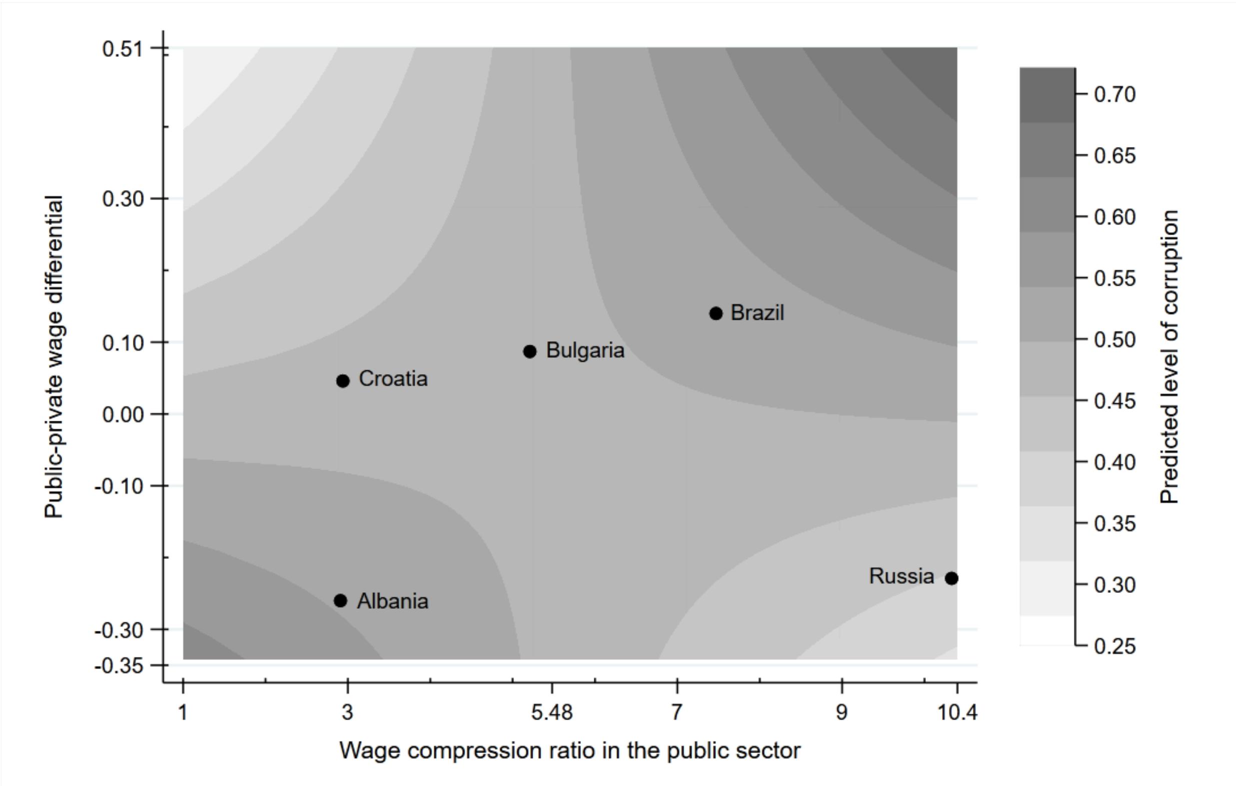
Figure 1: Effect of changes in public-private wage differential on corruption for different levels of wage compression in the public sector.



Note: Simulations are based on the FE estimation of equation (2) of the main specification (column 3 in Table 2). The shaded area shows 95% confidence intervals. The solid line represents a linear relationship between the effect of the public-private wage differential on corruption and the compression of wages in the public sector. The public sector wage compression ratio is defined as the ratio of the 90th to the 10th percentile of the weekly wage distribution in the public sector (a wage compression ratio of 1 indicates a uniform distribution of wages in the public sector).

Figure 2: Contour plot of predicted levels of corruption as a function of public-private wage differential and wage compression ratio in the public sector



Note: The contour plot is based on the FE estimation of equation (2) of the main specification (column 3 in Table 2). The contour levels show areas with similar levels of predicted corruption. The public sector wage compression ratio is defined as the ratio of the 90th to the 10th percentile of the weekly wage distribution in the public sector (a wage compression ratio of 1 indicates a uniform distribution of wages in the public sector). The public-private wage differential compares the wages of employees in the public sector to similar workers in the formal private sector.

Table 1: **Descriptive statistics for dependent and main independent variables**

Variable	*Mean*	*Standard deviation*	*Minimum*	*Maximum*	*Data source*
Dependent variables					
Corruption WGI	0.479	0.220	0.032	0.922	WGI
Corruption WEF	0.466	0.159	0.131	0.742	WEF
Corruption TI	0.378	0.182	0.024	0.786	TI
Controls					
Wage differential, formal sector	0.056	0.146	–0.342	0.509	WWBI
Wage differential, all	0.151	0.182	–-0.290	0.740	WWBI
Wage compression	4.772	1.377	2.372	10.333	WWBI
GDP per capita (/10,000)	2.652	1.792	0.381	9.786	WDI
Quality of bureaucracy	2.706	0.914	1.000	4.000	ICRG
Rule of law	0.518	0.927	–1.251	2.100	WGI
Fragility index	2.921	3.539	0.000	14.000	SFIM
Government spending as percent of GDP	17.059	3.829	7.196	24.536	WDI
Tertiary education (share)	0.564	0.117	0.283	0.839	WWBI

Note: WGI = Worldwide Governance Indicator database; WEF = World Economic Forum; TI = Transparency International; WWBI = Worldwide Bureaucracy Indicators dataset; WDI = World Development Indicators database; ICRG = International Country Risk Guide dataset; SFIM - State Fragility Index and Matrix dataset.

Table 2: Fixed effect estimations of WGI Corruption Index

	(1)		(2)		(3)		(4)	
Variable	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*
Wage differential, formal sector	–0.083**	0.035	–0.077**	0.035	–0.479***	0.088	–0.416***	0.102
Wage compression			0.004	0.005	0.003	0.004	0.006	0.005
Wage differential (formal) × wage compression					0.087***	0.020	0.081***	0.023
Log GDP per capita	–0.024	0.040	–0.022	0.039	–0.037	0.036		
Quality of bureaucracy	–0.054**	0.026	–0.045*	0.026	–0.063**	0.027		
Rule of law	–0.110***	0.024	–0.109***	0.023	–0.108***	0.023		
Fragility index	–0.003	0.003	–0.002	0.003	-0.005	0.003		
Government spending (percent of GDP)	0.002	0.002	0.002	0.002	0.001	0.002		
Tertiary education (share)	–0.045	0.113	–0.029	0.110	–0.023	0.105		
Constant	0.929**	0.400	0.859**	0.377	1.083***	0.375	0.462***	0.025
Wald (Hausman) test[a]	1,847.93	0.000	1,035.01	0.000	706.80	0.000	51,389.60	0.000
R^2	0.210		0.214		0.298		0.130	
Country fixed effects	Yes		Yes		Yes		Yes	
Year fixed effects	Yes		Yes		Yes		Yes	
Number of countries	36		36		36		36	
Number of observations	454		454		454		454	

Note: Results are based on a panel of countries for which there were at least eight longitudinal observations. Standard errors are corrected for clustering at a country level. Specification (4) includes only the three main variables of interest, which are used as a baseline for the extreme bounds analysis in the sensitivity analysis.

a. The Wald test was used instead of a standard Hausman fixed-versus-random effects test in order to differentiate the models with clustered errors (Hausman 1976). The two tests are asymptotically equivalent under an assumption of conditional homoskedasticity (Arellano 1993, Wooldridge 2001). For the Wald test, $\chi^2(24)$, $\chi^2(25)$, $\chi^2(26)$, and $\chi^2(20)$ are shown for specification (1), (2), (3), and (4), respectively; *p*-values are reported instead of standard errors.

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 3: Fixed effect estimations of three measures of corruption and two measures of public-private wage differential

	Corruption WGI				*Corruption WEF*				*Corruption TI*			
	(1)		*(2)*		*(3)*		*(4)*		*(5)*		*(6)*	
Variable	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*
Wage differential, formal sector	–0.479***	0.088			–0.406**	0.158			–0.705***	0.223		
Wage differential, all			–0.159	0.100			–0.040	0.133			–0.510**	0.235
Wage compression	0.003	0.004	0.002	0.005	–0.003	0.005	0.001	0.006	–0.001	0.007	–0.001	0.008
Wage differential × wage compression	0.087***	0.020	0.030	0.021	0.067*	0.035	–0.004	0.031	0.140***	0.044	0.099**	0.047
Log GDP per capita	–0.037	0.036	–0.046	0.037	–0.276***	0.057	–0.282***	0.052	0.041	0.121	0.023	0.122
Quality of bureaucracy	–0.063**	0.027	–0.028	0.024								
Rule of law	–0.108***	0.023	–0.092***	0.025	–0.064**	0.028	–0.063**	0.030	–0.022	0.041	–0.027	0.044
Fragility index	–0.005	0.003	–0.003	0.003	0.004	0.009	0.003	0.009	–0.019	0.014	–0.021	0.014
Government spending (percent of GDP)	0.001	0.002	–0.002	0.002	0.001	0.004	0.001	0.003	–0.004	0.005	–0.004	0.005
Tertiary education (share)	–0.023	0.105	0.004	0.100	–0.155	0.170	–0.112	0.149	–0.190	0.158	–0.236	0.174
Constant	1.083***	0.375	1.112***	0.358	3.404***	0.663	3.403***	0.572	0.214	1.243	0.439	1.240
Wald (Hausman) test[a]	706.80	0.000	354.74	0.000	611.61	0.000	74.56	0.000	26.47	0.006	27.54	0.004
R^2	0.298		0.196		0.477		0.459		0.355		0.324	
Country fixed effects	Yes		Yes		Yes		Yes		Yes		Yes	
Year fixed effects	Yes		Yes		Yes		Yes		Yes		Yes	
Number of countries	36		40		24		26		25		25	
Number of observations	454		507		235		252		165		165	

Note: Panel of countries with at least 8 longitudinal observations in columns (1), (2), (3), and (4). Panels of countries with at least 5 longitudinal observations in columns (5) and (6). Standard errors are corrected for clustering at the country level.

a. The Wald test was used instead of a standard Hausman fixed-versus-random effects test in order to differentiate the models with clustered errors (Hausman 1976). The two tests are asymptotically equivalent under an assumption of conditional homoskedasticity (Arellano 1993, Wooldridge 2001). For the Wald test, $\chi^2(24)$, $\chi^2(25)$, $\chi^2(26)$, and $\chi^2(20)$ are shown for specification (1), (2), (3), and (4), respectively; *p*-values are reported instead of standard errors.

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 4: Two-stage fixed effect instrumental variable estimations of WGI and WEF corruption indicators

	Corruption WGI				Corruption WEF			
	(1)		(2)		(3)		(4)	
Variable	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*
Wage differential, formal sector	–0.953***	*0.227*			–0.915***	*0.264*		
Wage differential, all			–0.312	*0.221*			–0.352	*0.235*
Wage compression	–0.006	0.005	0.002	0.005	–0.010*	0.005	–0.004	0.006
Wage differential × wage compression	0.095***	0.020	0.009	0.018	0.111***	0.033	0.028	0.029
Log GDP per capita	–0.013	*0.040*	–0.036	*0.049*	–0.280***	*0.075*	–0.291***	*0.083*
Log shadow economy	0.004	*0.089*	0.077	*0.088*	0.129	*0.106*	0.082	*0.110*
Quality of bureaucracy	–0.004	0.060	0.053	0.069				
Rule of law	–0.144***	0.024	–0.110***	0.019	–0.022	0.032	–0.033	*0.035*
Fragility index	–0.008**	0.004	–0.004	0.003	–0.005	0.008	0.000	*0.006*
Government spending (percent of GDP)	0.008	0.005	–0.001	0.004	–0.001	0.004	–0.002	*0.003*
Tertiary education (share)	0.008	0.059	–0.037	0.089	–0.052	0.116	–0.109	*0.110*
Constant	0.626	0.628	0.594	0.712	3.097***	0.975	3.345***	*1.104*
	First stage regression: wage differential							
	Formal sector		*All*		*Formal sector*		*All*	
Share of workers with contracts, private sector	0.292**	0.120	0.758***	0.228	0.280**	0.129	0.685***	0.235
F-test	5.87		11.09		4.69		8.53	
Hausman test (2nd stage)/ p-value	6.58**	0.011	1.243	0.266	2.717	0.101	1.119	0.292
R^2	0.389		0.315		0.477		0.444	
Country fixed effects	Yes		Yes		Yes		Yes	
Year fixed effects	Yes		Yes		Yes		Yes	
Number of countries	30		33		22		24	
Number of observations	324		347		212		229	

Note: Results are based on a panel of countries for which there were at least eight longitudinal observations. Standard errors are calculated by bootstrapping the system of two equations. Standard errors are corrected for clustering at the country level.

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 5: Estimations of alternative model specifications

	Corruption WGI				Corruption WEF				Corruption TI			
Wage differential	*Private formal*		*Private all*		*Private formal*		*Private all*		*Private formal*		*Private all*	
	(1)		(2)		(3)		(4)		(5)		(6)	
Variable	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*
Panel duration 1	*At least 4 panel observations*				*At least 4 panel observations*				*At least 3 panel observations*			
Wage differential	–0.418***	0.086	–0.139	0.084	–0.256	0.160	–0.020	0.136	–0.613***	0.211	–0.319*	0.188
Wage compression	0.003	0.004	0.002	0.004	–0.000	0.005	0.002	0.006	–0.001	0.007	–0.001	0.008
Interaction	0.080***	0.019	0.027	0.017	0.045	0.034	–0.002	0.031	0.124***	0.040	0.064*	0.037
Panel duration 2	*At least 6 panel observations*				*At least 6 panel observations*				*At least 4 panel observations*			
Wage compression	–0.426***	0.089	–0.146	0.088	–0.406**	0.150	–0.103	0.145	–0.693***	0.219	–0.488**	0.232
Wage compression	0.002	0.004	0.001	0.004	–0.003	0.005	0.000	0.006	–0.001	0.007	–0.001	0.008
Interaction	0.080***	0.020	0.027	0.018	0.068**	0.033	0.008	0.032	0.138***	0.043	0.096**	0.046
Panel duration 3	*At least 10 panel observations*				*At least 10 panel observations*				*At least 6 panel observations*			
Wage differential	–0.491***	0.090	–0.168	0.106	–0.360*	0.193	–0.173	0.190	–0.709***	0.226	–0.709***	0.227
Wage compression	0.003	0.004	0.001	0.005	–0.001	0.007	0.001	0.007	–0.001	0.008	–0.001	0.008
Interaction	0.089***	0.021	0.029	0.021	0.057	0.042	0.026	0.040	0.139***	0.043	0.138***	0.044
Panel of averaged indicators					*Panel of observations averaged over 4 years*							
Wage differential	–0.560***	0.125	–0.165	0.144	–0.729**	0.274	–0.220	0.289	–1.437*	0.716	–0.703	0.742
Wage compression	0.003	0.006	0.002	0.007	–0.010	0.009	–0.002	0.011	–0.014	0.026	0.001	0.034
Interaction	0.106***	0.033	0.041	0.031	0.124**	0.055	0.018	0.061	0.305	0.181	0.132	0.191
Lagged variables												
One-period lag												
Wage differential	–0.406***	0.098	–0.169	0.101	–0.388*	0.207	–0.178	0.182	–0.338*	0.192	–0.232	0.188
Wage compression	0.003	0.004	0.002	0.005	–0.001	0.007	0.001	0.007	0.006	0.006	0.007	0.006
Interaction	0.075***	0.022	0.030	0.020	0.070	0.042	0.030	0.040	0.071*	0.040	0.046	0.039
Two-period lag												
Wage differential	–0.276**	0.121	–0.107	0.108	–0.107	0.239	0.002	0.186	0.196	0.206	0.180	0.180
Wage compression	0.001	0.004	0.002	0.005	0.007	0.007	0.009	0.008	0.013	0.008	0.013	0.009
Interaction	0.056**	0.026	0.023	0.021	0.033	0.043	0.009	0.035	–0.025	0.040	–0.021	0.036

Note: Standard errors are clustered at the country level. All estimations except OLS using averaged data include year dummies. Panel durations 1, 2, and 3 use different minimum numbers of observations per country to include in the sample and are based on the baseline fixed effects estimator (see Table 3), which requires at least eight observations per country for the WGI, eight for the WEF, and five for the TI corruption models. The panel of averaged indicators uses averaged data over three and four years for the WEF sample, which spans 10 years (2007–16).

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 6: Estimations using alternative econometric techniques

	Corruption WGI				*Corruption WEF*				*Corruption TI*			
Wage differential	*Private formal*		*Private all*		*Private formal*		*Private all*		*Private formal*		*Private all*	
	(1)		*(2)*		*(3)*		*(4)*		*(5)*		*(6)*	
Variable	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*
Pooled OLS												
Wage differential	–0.516***	0.157	–0.242*	0.130	–0.074	0.309	0.056	0.233	–0.235	0.271	–0.177	0.234
Wage compression	–0.005	0.005	–0.007	0.005	–0.013	0.008	–0.014*	0.003	–0.003	0.008	–0.002	0.008
Interaction	0.102***	0.031	0.053**	0.025	0.026	0.059	0.035	0.045	0.064	0.052	0.054	0.045
OLS using averaged data												
Wage differential	–0.537	0.393	–0.261	0.268	0.073	0.518	–1.126**	0.505	–0.210	0.464	–0.280	0.499
Wage compression	–0.007	0.009	–0.013*	0.007	–0.036	0.021	–0.023	0.019	0.002	0.012	0.001	0.013
Interaction	0.004	0.003	0.002	0.002	0.001	0.005	0.011**	0.005	0.002	0.003	0.002	0.003
Random effects												
Wage differential	–0.477***	0.084	–0.167*	0.094	–0.300	0.190	–0.046	0.161	–0.556***	0.188	–0.416**	0.175
Wage compression	0.001	0.004	–0.000	0.004	–0.009	0.007	–0.003	0.007	–0.007	0.006	–0.007	0.006
Interaction	0.085***	0.019	0.031*	0.018	0.049	0.040	0.002	0.033	0.120***	0.036	0.093***	0.034

Note: Standard errors are clustered at the country level. All estimations except OLS using averaged data include year dummies. Pooled OLS include regional dummies (e.g., Le et al. 2013). OLS using averaged data weights the data by the estimated country-specific error variances, which depend on the number of observations available for each country. Weighted are used to obtain cross-section data and run between estimator with robust standard errors (e.g., Van Rijckeghem and Weder 2001). The random effects estimator assumes that the variation across countries is random and errors are uncorrelated with independent variables. The Wald test rejects the random effect specification in favor of the fixed effect specification for all estimations.
*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table 7: Extreme bounds analysis of coefficients on public-private wage differential and interaction term of wage compression ratio and public–private wage differential

Variable	*Coefficient*	*Standard error*	t-*statistic*	*Probability*
Wage differential				
High	–0.297***	*0.105*	–2.82	0.007
Baseline	–0.416***	*0.102*	–4.06	0.000
Low	–0.488***	*0.090*	–5.38	0.000
Interaction term				
High	0.090***	*0.021*	4.37	0.000
Baseline	0.081***	*0.023*	3.51	0.001
Low	0.064***	*0.023*	2.78	0.009

Note: Standard errors are clustered at the country level. Baseline specification corresponds to column (4) in Table 2.
*** significant at the 1% level.

Appendix

Table A1: **List of countries with three measures of corruption used in main estimations**

	Corruption WGI		*Corruption WEF*		*Corruption TI*	
Country	*Minimum*	*Maximum*	*Minimum*	*Maximum*	*Minimum*	*Maximum*
Argentina	0.635	0.718	0.681	0.742	0.631	0.690
Austria	0.100	0.258	0.242	0.385	0.190	0.274
Belgium	0.190	0.277	0.246	0.401	0.179	0.202
Bolivia	0.685	0.804				
Brazil	0.550	0.716			0.583	0.655
Bulgaria	0.625	0.653	0.560	0.694	0.583	0.607
Costa Rica	0.381	0.488			0.393	0.452
Croatia	0.531	0.587			0.488	0.548
Cyprus	0.291	0.402	0.301	0.457	0.310	0.440
Czech Republic	0.454	0.535	0.543	0.656	0.393	0.524
Denmark					0.024	0.048
Dominican Republic	0.703	0.798				
Ecuador	0.727	0.802			0.702	0.714
El Salvador	0.649	0.751				
Estonia	0.230	0.354	0.304	0.460	0.226	0.333
Finland	0.032	0.073			0.024	0.083
France	0.224	0.289	0.301	0.393	0.238	0.274
Greece	0.525	0.634	0.490	0.646	0.524	0.667
Honduras	0.724	0.817	0.546	0.680	0.726	0.786
Hungary	0.432	0.576	0.545	0.648	0.440	0.560
Ireland	0.169	0.282	0.192	0.370	0.202	0.274
Italy	0.509	0.596	0.565	0.636	0.476	0.595
Latvia	0.461	0.535	0.489	0.612	0.405	0.512
Lithuania	0.419	0.558	0.463	0.623	0.393	0.452
Luxembourg	0.074	0.194	0.131	0.201	0.083	0.143
Pakistan					0.750	0.774
Panama	0.610	0.678				
Paraguay	0.760	0.922				
Peru	0.634	0.723	0.569	0.683	0.643	0.643
Poland	0.413	0.526	0.448	0.621	0.345	0.405
Portugal	0.324	0.386	0.384	0.441	0.333	0.357
Romania	0.593	0.652	0.552	0.665	0.524	0.583
Russian Federation	0.771	0.859				
Serbia					0.595	0.631
Slovak Republic	0.472	0.569	0.586	0.660	0.488	0.548
Spain	0.305	0.472	0.409	0.551	0.321	0.417
Switzerland	0.076	0.115	0.151	0.180	0.071	0.083
United Kingdom	0.136	0.206	0.233	0.351	0.131	0.214
Uruguay	0.260	0.323			0.226	0.262

Table A2: Fixed effect estimation of main specification of three measures of corruption and public-private wage differential for different occupations (compared within formal sector only)

	Corruption WGI				*Corruption WEF*				*Corruption TI*			
	(1)		*(2)*		*(3)*		*(4)*		*(5)*		*(6)*	
Variable	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*
Wage differential, professional	–0.395***	0.097			–0.053	0.143			–0.369*	0.204		
Wage differential, senior official			–0.146***	0.053			–0.096	0.076			0.026	0.051
Wage compression	0.010*	0.005	0.009	0.006	0.002	0.005	0.004	0.005	0.012	0.008	0.011	0.010
Wage differential × wage compression	0.071***	0.021	0.022*	0.011	0.002	0.030	0.016	0.016	0.081*	0.043	–0.011	0.011
Log GDP per capita	–0.044	0.039	–0.054	0.042	–0.254***	0.062	–0.268***	0.057	0.036	0.130	–0.052	0.115
Quality of bureaucracy	–0.034	0.029	–0.019	0.028								
Rule of law	–0.105***	0.025	–0.100***	0.026	–0.071**	0.030	–0.058*	0.028	–0.033	0.045	–0.032	0.048
Fragility index	–0.006*	0.003	–0.005	0.004	0.003	0.009	0.003	0.009	–0.020	0.012	–0.025*	0.014
Government spending (percent of GDP)	0.000	0.002	–0.000	0.003	0.002	0.004	0.000	0.004	–0.007	0.005	–0.007	0.006
Tertiary education (share)	0.001	0.112	0.003	0.116	–0.167	0.166	–0.182	0.185	–0.220	0.182	–0.179	0.188
Constant	1.020**	0.397	1.087**	0.414	3.149***	0.709	3.314***	0.671	0.277	1.324	1.174	1.164
Wald (Hausman) test[a]	3,213.81	0.000	1,831.61	0.000	451.92	0.000	339.21	0.000	27.74	0.004	26.75	0.005
R^2	0.296		0.253		0.461		0.462		0.301		0.262	
Country fixed effects	Yes		Yes		Yes		Yes		Yes		Yes	
Year fixed effects	Yes		Yes		Yes		Yes		Yes		Yes	
Number of countries	34		33		24		24		25		25	
Number of observations	415		406		235		235		165		165	

Note: Panel of countries with at least eight years of observations in columns (1), (2), (3) and (4). Panel of countries with at least 5 longitudinal observations in columns (5) and (6). Standard errors are corrected for clustering at the country level.

a. The Wald test was used instead of a standard Hausman fixed-versus-random effects test in order to differentiate the models with clustered errors (Hausman 1976). The two tests are asymptotically equivalent under an assumption of conditional homoskedasticity (Arellano 1993, Wooldridge 2001). For the Wald test, $\chi^2(24)$, $\chi^2(25)$, $\chi^2(26)$, and $\chi^2(20)$ are shown for specification (1), (2), (3), and (4), respectively; *p*-values are reported instead of standard errors.

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.

Table A3: Two-stage fixed effect instrumental variable estimations of WGI and WEF corruption indicators

	Corruption WGI				*Corruption WEF*			
	(1)		*(2)*		*(3)*		*(4)*	
Variable	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*	*Coef.*	*SE*
Wage differential, formal sector	–0.590***	0.200			–0.920**	0.369		
Wage differential, all			–0.086	0.212			–0.306	0.359
Wage compression	0.002	0.005	0.004	0.005	–0.012**	0.005	–0.006	0.006
Wage differential × wage compression	0.125***	0.027	0.022	0.024	0.139***	0.037	0.042	0.033
Log GDP per capita	–0.066	0.047	–0.058	0.045	–0.146	0.098	–0.184*	0.099
Log shadow economy	0.140	0.112	0.156	0.095	0.490***	0.156	0.341**	0.150
Quality of bureaucracy	0.020	0.064	0.029	0.069				
Rule of law	–0.111***	0.029	–0.102***	0.024	–0.008	0.037	–0.027	0.043
Fragility index	–0.007*	0.004	–0.011***	0.003	–0.020**	0.010	–0.010	0.007
Government spending (percent of GDP)	–0.005	0.006	–0.006	0.005	–0.014**	0.006	–0.009**	0.005
Tertiary education (share)	0.250***	0.076	0.221**	0.086	–0.114	0.114	-0.127	0.135
Constant	0.666	0.736	0.540	0.670	0.980	1.310	1.670	1.360
First stage	*Wage differential*				*Wage differential*			
	Formal sector		*All*		*Formal sector*		*All*	
Minimum wage	–0.118**	0.040	–0.100*	0.056	–0.096**	0.040	–0.091**	0.048
F-test	8.93		3.71		5.75		3.54	
R^2	0.435		0.355		0.635		0.576	
Country fixed effects	Yes		Yes		Yes		Yes	
Year fixed effects	Yes		Yes		Yes		Yes	
Number of countries	22		25		17		19	
Number of observations	249		274		167		182	

Note: Results are based on a panel of countries for which there were at least eight longitudinal observations. Standard errors are calculated by bootstrapping the system of two equations. Standard errors are corrected for clustering at a country level.

*** significant at the 1% level, ** significant at the 5% level, * significant at the 10% level.