(Un)Principled Principals, (Un)Principled Agents: The Differential Effects of Managerial Civil Service Reforms on Corruption in Developing and OECD Countries[*]

Christian Schuster[1], Jan Meyer-Sahling[2], and Kim Sass Mikkelsen[3]

[1]University College London
[2]University of Nottingham
[3]Roskilde University

Abstract

Do management practices have similar anti-corruption effects in OECD and developing countries? Despite prominent cautions against 'New Zealand' reforms which enhance managerial discretion in developing countries, scholars have not assessed this question statistically. Our paper addresses this gap through a conjoint experiment with 6,500 public servants in three developing and one OECD country. Our experiment assesses Weberian relative to managerial approaches to recruitment, job stability and pay. We argue that, in developing countries with institutionalized corruption and weak rule-of-law – yet not OECD countries without such features – 'unprincipled' principals use managerial discretion over hiring, firing and pay to favor 'unprincipled' bureaucratic agents who engage in corruption. Our results support this argument: managerial practices are associated with greater bureaucratic corruption in our surveyed developing countries, yet have little effect in our OECD country. Alleged best practices in public management in OECD countries may thus be worst practices in developing countries.

Keywords: Weber; Corruption; Conjoint Experiments; Replication; Developing Countries

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Introduction

Public sector organizations in developing countries frequently adopt management practices which have been originally developed in OECD contexts – be these performance pay schemes, medium-term public expenditure frameworks or open government initiatives, among many examples (cf., e.g., Andrews 2013; Brinkerhoff and Brinkerhoff 2015). Public sector institutional reforms featured in more than US$50bn of World Bank project lending between 2006 and 2011, for instance; the majority of these projects advances part of a common set of reforms (Andrews 2013). Stereotypically, thus, in these projects, 'international organizations, local policy makers, and private consultants combine to enforce the presumption that the most advanced countries have already discovered the one best institutional blueprint for development and that its applicability transcends national cultures and circumstances.' (Evans 2004, p. 33)

Given the centrality of this approach to public sector reform in developing countries, an important task for public administration scholarship is arguably to assess whether the same management practices and institutional reforms have similar effects in developing and OECD countries. A prominent qualitative body of works has developed around this question. In light of the poor track record of public sector reforms in developing countries (see, e.g., World Bank 2008; Andrews 2013), this body of works has taken a largely skeptical view. Public sector reforms imported from OECD countries are argued to be insufficiently focused on the problems facing policymakers in developing countries, leading to mimicry in form but not function (Andrews 2013); unnecessary to spur economic and political development (Grindle 2004); and, most of all, ill suited for the public sector contexts of developing countries – for instance as they lack the necessary administrative preconditions to successfully implement OECD public sector innovations (see, e.g., Levy 2014; Brinkerhoff and Brinkerhoff 2015; Evans 2004; Schick 1998). This has led to shifts in discourse – and, at times, practice – from 'best practice' to 'best fit'

in public sector reform projects in developing countries (Yanguas and Bukenya 2016).

Considering the prominence of these arguments in scholarship and practice, one would expect a vibrant industry of scholarship that has rigorously tested them, by employing the same research design in OECD and developing countries to test whether similar public management practices have similar or dissimilar effects.[1] Contrary to this expectation, however, this literature – and, in particular, quantitative studies which systematically compare the effects of similar management practices in OECD and developing countries – is scant (see, e.g., Sundell 2014, for an exception). We thus lack rigorous evidence about the relative effects of the same management practices in developing and OECD countries.

Our paper helps address this gap. It does so by assessing one of the most prominent claims in the aforementioned literature: that developing countries should not introduce 'New Zealand'-inspired New Public Management (NPM) reforms which enhance managerial discretion. In weak-rule-of-law developing countries, such reforms are argued to invite abuse and corruption (Schick 1998). Notwithstanding this claim, NPM reforms continue to form part of the 'standard responses' to public sector reform challenges in developing countries (Brinkerhoff and Brinkerhoff 2015, p. 222). Assessing this two-decade-old claim about administrative preconditions for NPM reforms thus matters. Given prominent concerns about abuse of managerial discretion in weak-rule-of-law contexts (e.g. Schick 1998; World Bank 2004; Sundell 2014), our paper assesses, in particular, the effects of NPM reforms in developing and OECD country contexts on bureaucratic corruption.

We do so for one management function which features centrally in public sector reform projects in developing countries: civil service management (World Bank 2008). Popular managerial civil service reforms – which are frequently undertaken in both OECD and developing countries –

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[1]Showing that public sector reforms fail in developing countries by itself is, of course, insufficient evidence for claims about dissimilar effects, as reforms frequently fail in OECD countries as well (see, e.g., Dunleavy et al. 2005)

include performance pay schemes, the introduction of temporary contracts, and the delegation of recruitment powers to managers, among others (Brewer and Kellough 2016; Manning 2001; Meyer-Sahling, Schuster, and Mikkelsen 2018). Our paper thus focuses on these reforms, assessing them relative to their Weberian alternatives – i.e. pay based on performance (versus homogenous pay); recruitment based on managerial discretion (versus public competitions); and temporary contracts (versus permanent career contracts).

Expanding on Schick (1998), we argue that managerial civil service practices significantly enhance corruption – understood as the 'abuse of public office for private gain' (World Bank 1997) – in weak-rule-of-law developing country governments, yet not OECD countries. In developing countries with institutionalized corruption, 'unprincipled' principals are likely to use managerial discretion over hiring, firing and performance pay to favour 'unprincipled' agents – political or personal cronies – who prioritize private interests over public interests. In the weak rule-of-law contexts characteristic of such countries – where abuses of public office for private gain often go unsanctioned – 'unprincipled' agents are more likely to engage in corruption. By contrast, in OECD countries with stronger norms of integrity and meritocracy, more 'principled' principals are less likely to abuse managerial discretion to favour 'unprincipled' agents[2]. Moreover, stronger rule-of-law and thus sanctioning of corrupt behavior discourage unprincipled agents from engaging in corruption. Consequently, we only expect marginal effects of managerial civil service practices on corruption in OECD contexts.

We test – and find empirical support – for this assertion through a conjoint experiment with 6,500 public servants in three developing countries with medium to high levels of corruption and weak rule of law (Ghana, Kosovo, and Uganda); and one OECD country with low levels of corruption and strong rule of law (Chile). Our conjoint experiment allows us to plausibly isolate

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[2]Related principal-agent logics have been advanced in studies of private sector firms, which show that CEOs abuse discretionary influence over their own pay in the absence of accountability mechanisms (Guthrie, Kwon, and Sokolowsky 2017; Bertrand and Mullainathan 2000); and, in a broader sense, studies of democracy and corruption, which show that 'unprincipled' (political) principals abuse discretion over state resources (Drury, Krieckhaus, and Lusztig 2006)

the effects of similar management practices in these different country contexts, as perceived by public servants. We find that Weberian practices are associated with lower – and managerial practices greater – bureaucratic corruption in our three developing countries, yet have only minor effects in the OECD country.

Our perception-based experiment thus suggests that prominent concerns about the abuse of managerial discretion over personnel management decisions in developing countries are well-warranted: reforms to expand managerial discretion should be usefully preceded by Weberian reforms which strengthen rule-based governance and norms of meritocracy and integrity.

More generally, our findings point to the importance of a research agenda which systematically – that is with the same research designs – assesses whether the same management practices have similar or dissimilar effects in developing and OECD countries. Most quantitative public administration studies have focused on OECD countries (Milward et al. 2016). For their applicability to developing countries, our study – finding differential effects between OECD and developing countries – is bad news. Public administration scholars would thus do well to put greater emphasis on assessing theoretically and empirically – through replication studies or cross-country research designs – which of their findings travel between developed and developing countries, and which lack generalizability.

The Differential Effects of Civil Service Management Practices on Bureaucratic Corruption in OECD and Developing Countries

Numerous studies have assessed the effects of civil service management practices on bureaucratic corruption (see Meyer-Sahling, Mikkelsen, and Schuster 2018, for a review of 36 studies). To develop our hypotheses, we combine insights from this literature with studies cautioning against managerial reforms in developing countries (e.g. Schick 1998; Manning 2001; World Bank 2004;

Sundell 2014; Yanguas and Bukenya 2016).

These latter studies have argued that developing countries lack the necessary preconditions for 'letting the managers manage' through greater managerial discretion over inputs and, concurrently, accountability for results. Classically, Schick (1998) argued that public organizations need to strengthen rule-based governance first, by establishing a 'formal rule-based, honest public sector' which 'encourages managers to internalize a public ethic of proper behavior' (Schick 1998, p. 130). Without rule compliance and internalized *Rechtsstaat* values, managers may be expected to abuse discretion over inputs – for instance by hiring political party affiliates or family members (World Bank 2004; Schick 1998; Manning 2001; Yanguas and Bukenya 2016). Sundell (2014), in particular, shows in cross-country data that managerial discretion over recruitment is associated with hiring less skilled public employees in countries where the risk of patronage is high.

We extend this line of reasoning to hypothesize that granting discretion to managerial principals in corrupt and weak-rule-of-law developing countries enhances corruption by their bureaucratic agents. Two related mechanisms are at play. First, in developing countries with institutionalized corruption, principals (managers) are more likely to be 'unprincipled' – that is prioritizing private gain over public interest. Granted discretion over personnel management, they are more likely to favour 'unprincipled' bureaucratic agents who pursue both their own and their principal's private interests – and are thus more likely to engage in corruption. By contrast, in OECD countries with stronger social norms of integrity and meritocracy (see, e.g. Gächter and Schulz 2016), more 'principled' principals are less likely to abuse managerial discretion to favour 'unprincipled' agents over principled ones in personnel management decisions. Second, in weak-rule-of-law contexts characteristic of developing countries – where abuses of public office for private gain often go unsanctioned (Rose-Ackerman and Palifka 2016) – unprincipled agents face greater incentives to engage in greater corruption than in OECD countries with,

typically, stronger monitoring and sanctioning systems.

As a result of these two mechanisms, managerial civil service practices are – relative to traditional, Weberian alternatives – associated with greater bureaucratic corruption in weak-rule-of-law developing countries with institutionalized corruption. By contrast, in OECD countries with strong rule of law and social norms of integrity, we would expect only marginal effects on bureaucratic corruption.

This overarching argument can be applied to each of the three managerial civil service reforms this paper assesses: recruitment based on discretion (versus Weberian public competitions); temporary contracts (versus Weberian permanent career contracts); and pay differentiation based on performance (versus Weberian pay equality at similar grades).

The relationship between competitive, Weberian recruitment and bureaucratic corruption has seen most studies. Five studies associate competitive or merit-based recruitment with lower corruption (Meyer-Sahling and Mikkelsen 2016; Rauch and Evans 2000; Oliveros and Schuster 2018; Charron et al. 2017; Dahlström, Lapuente, and Teorell 2012). They argue that Weberian competitions – in which positions are publicly advertised, candidates systematically assessed and the best candidate selected by a commission – curb corruption by fostering an *esprit de corps* of public service and integrity (Rauch and Evans 2000); and by separating the interests of public servants and political principals, as public servants no longer owe their positions to (political) principals. Principals and bureaucratic agents thus face greater incentives to hold each other accountable for corrupt behavior (Dahlström, Lapuente, and Teorell 2012). Vice versa, to use our principal-agent terminology, 'unprincipled' principals are argued to abuse discretionary appointment powers to select 'unprincipled' agents, with weaker professionalism and norms of integrity and stronger identification with the principals' private – personal or political – interests.

Note, though, that managerial discretion need not translate into the recruitment of 'unprincipled' agents. In line with our overarching argument, 'principled' principals in OECD countries with stronger norms of integrity may be expected to use their hiring powers to select competent professionals (Sundell 2014; Kopecký et al. 2016) – that is 'principled' agents who identify with professional and public service norms. Moreover, in OECD governments with strong rule of law, stronger sanctioning of corrupt behavior reduces their incentives to engage in corruption in the first place. Hypotheses H1a and H1b reflect these diverging empirical expectations:[3]

Hypothesis 1a (H1a): In developing countries, recruitment through discretionary appointments is, relative to Weberian competitions, associated with greater bureaucratic corruption.

Hypothesis 1b (H1b): In OECD countries, recruitment through discretionary appointments has, relative to Weberian competitions, *no* significant association with bureaucratic corruption.

A similar line of reasoning can be extended to the relationship between permanent (career) contracts and bureaucratic corruption. Advocates of Weberian bureaucracy favor lifelong contracts to protect bureaucratic agents from threats of dismissals which might pressure them into politically-motivated corruption; and to foster long-term socialization processes in civil services into an *esprit de corps* of integrity (Dahlström, Lapuente, and Teorell 2012; Rauch and Evans 2000). Advocates of managerial reforms and temporary contracts, however, argue that career contracts protect corrupt bureaucratic agents from dismissals (Oliveros and Schuster 2018).

Which, if any, of these arguments hold may, however, again be expected to vary between developing and OECD countries. 'Unprincipled' principals in developing countries can use discretion over firing to pressure bureaucratic agents into corruption for the principals' gain and preclude an 'esprit de corps' of integrity (see Brierley 2019, for an analogous argument about discretionary power over transfers). Agents thus become more 'unprincipled' and, with weak-

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[3]To enhance conciseness in our hypotheses, we refer to countries with weaker rule of law and institutionalized corruption as developing countries; and countries with stronger rule of law and social norms of integrity as OECD countries. We acknowledge, of course, that this linguistic shorthand has its limits: some OECD and some developing countries are the exception.

rule-of-law limiting sanctioning of corruption, likelier to engage in corruption. By contrast, we would not expect more 'principled' principals in OECD governments to abuse discretion over dismissals to pressure bureaucratic agents into corruption. Moreover, stronger rule-of-law in OECD countries dis-incentivizes corruption in the first place, by sanctioning bureaucratic agents for acts of corruption (e.g. through effective disciplinary procedures or the courts). We thus do not expect managerial discretion over dismissals to significantly affect corruption in OECD contexts.

Hypothesis 2a (H2a): In developing countries, temporary job contracts are, relative to permanent contracts, associated with greater bureaucratic corruption.

Hypothesis 2b (H2b): In OECD countries, temporary job contracts have, relative to permanent contracts, *no* significant association with bureaucratic corruption.

Lastly, we also hypothesize differential effects of performance pay. While the literature on performance pay and corruption in public administration is scant to non-existent (Kwon (2014) is a partial exception), two implications of performance pay for public sector pay setting may be expected to shape its effect on corruption: pay differentiation and discretion of principals over pay.

The first implication of performance pay is horizontal pay differentiation between public servants. Performance pay leaves some bureaucratic agents – who do not receive performance pay – on relatively inferior pay. While the relationship between pay and corruption is contested (for a review, see Meyer-Sahling, Mikkelsen, and Schuster 2018), several studies associate inferior pay with greater corruption, as it lowers the opportunity costs of corruption when caught; enhances pressures for needs-based corruption, with public servants stealing to sustain their families (see, e.g. Van Rijckeghem and Weder 2001); *and* lowers moral standards, with dishonest behavior deemed more acceptable (see, e.g. Sharma et al. 2014).

The second implication of performance pay is greater discretion of principals over bureaucratic agents' pay, due to the subjectivity inherent in public employee performance evaluations. 'Unprincipled' principals can use this discretion to reward corrupt behavior by agents. Performance pay schemes may thus enhance corruption by public servants granted superior pay by unprincipled principals. In developing countries with 'unprincipled' principals and weak sanctioning of corrupt behavior, discretion by managers over performance pay may thus enhance corruption of both bureaucrats on superior pay and of their colleagues on, consequently, inferior pay.

By contrast, in OECD countries with 'principled' principals who are not prone to abuse discretion over pay to reward corrupt behavior, in addition to stronger sanctioning of corrupt behavior through rule-of-law institutions and less pressure for needs-based corruption to sustain families, we would not expect performance pay to increase corruption.[4]

Hypothesis 3a (H3a): In developing countries, performance-based pay differentiation is, related to homogenous pay, associated with greater bureaucratic corruption.

Hypothesis 3b (H3b): In OECD countries, performance-based pay differentiation has, relative to homogenous pay, *no* significant association with bureaucratic corruption.

Research Design and Data

We assess our hypotheses with a cross-country conjoint survey experiment with public servants. In conjoint experiments, respondents (public servants in our case) choose between profiles with attributes whose values are randomly assigned (Hainmueller, Hopkins, and Yamamoto 2014).

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[4]At the same time, we would not expect it to curb corruption either. While classic works on managerial discretion in developing countries (e.g. Schick 1998) – and our own hypotheses – do not foresee this, such an effect is theoretically not inconceivable. Performance-based pay differentiation and temporary contracts offer 'principled' principals levers to sanction 'unprincipled' agents. They, thus, could be expected to curb corruption. As anti-corruption legislation is enforced in OECD countries with strong rule of law, however, managerial discretion is not, in fact, needed to sanction corrupt agents. For instance, while 'principled' principals with discretionary dismissal powers could sanction corrupt agents by dismissing them, corrupt agents would be equally dismissed (or otherwise sanctioned) through the court system in strong-rule-of-law contexts, irrespective of managerial discretion over dismissals. In line with classic qualitative works (e.g. Schick 1998), we thus hypothesize no significant effects in OECD contexts.

We follow Oliveros and Schuster (2018) and ask public servants who, in a comparison of two hypothetical colleagues in their organization, is more likely to engage in corruption. We randomize the attribute values of these colleagues – for instance whether they are on a permanent or temporary contract.

Conjoint experiments offer several validity advantages over observational inferences. They allow us to randomly assign management practices in the profile comparison. In observational studies, concerns with reverse causality and omitted variable bias loom large. Whether an organization introduces managerial discretion over personnel, for instance, is likely to be affected by both prior levels of corruption and managerial concerns about corruption.

Conjoints also lower social desirability bias – a frequent concern in corruption surveys. Respondents need not reveal why they preferred one public servant over another (cf. Hainmueller, Hopkins, and Yamamoto 2014). Conjoints also offer greater realism than a unidimensional elicitation of preferences: they involve trade-offs between preferences for different characteristics of public servants (Hainmueller, Hopkins, and Yamamoto 2014). In light of these advantages, studies find that conjoints haver greater external validity than other survey experiments (Hainmueller, Hangartner, and Yamamoto 2015).

Our conjoint design nonetheless has limitations, particularly by basing inferences on hypothetical profiles of public servants and a perception-based outcome variable. Perceptions of corruption by public servants with certain characteristics, of course, need not correspond directly to corrupt behavior. Our particular set-up, however, arguably lends itself to plausible inferences: public servants work with colleagues with the attributes they assess on a daily basis. They can thus respond based on first-hand knowledge – that is actual experiences with colleagues with similar characteristics.

Case Selection

To assess our argument about differential effects, we sought country cases which vary in rule of law and institutionalized corruption and which, at the same time, enable meaningful conjoint inferences. Meaningful conjoint inferences are feasible where: first, recruitment, contract and pay practices vary within institutions. Hypothetical public servants with, for instance, varying job contracts thus have realistic real-world referents on which respondents can base their assessment. Second, management practices are not perfectly co-linear (e.g. the type of job contract does not directly follow from the recruitment procedure), thus enabling estimations of *independent* effects of each management practice. Third, variation in designs of recruitment, job contract, and performance pay practices are sufficiently comparable to enable meaningful cross-country comparisons of effects.

With these criteria, we selected one OECD country (Chile) and three developing countries (Ghana, Kosovo and Uganda). Chile is an OECD country with strong rule of law and social anti-corruption norms, ranking globally in the top quintile in both the World Bank's Rule of Law Governance indicator and – as a proxy for institutionalized corruption – Transparency International's Corruption Perceptions Index (CPI) (Appendix A.1). By contrast, Ghana, Kosovo and Uganda rank between the third and fifth quintiles in the rule-of-law and corruption indicators. The four countries thus enable us to assess the differential effects of management practices in countries with weak (strong) rule-of-law and (lack of) institutionalized corruption.

The four countries also offer within-country variation in civil service management practices, without perfect co-linearity between them. The share of public servants hired through examinations, for instance, ranges from 17% (Ghana) to 63% (Uganda), while the share indicating that their pay is tied to performance stands between 17% (Uganda) and 41% (Chile) (Appendix A.2). Our conjoint attributes thus realistically reflect variation in attributes of colleagues with

whom our respondents interact in their jobs.

Moreover, civil service management designs are relatively comparable across the four countries, allowing us to assess whether similar civil service management practices have the theorized differential effects. Across countries, public employees may be selected through Weberian competitions or discretionary appointments; and Weberian competitions combine requirements for job advertisements – variably in government gazettes, newspapers or online portals – selection commissions, and written and/or oral examinations. Moreover, public servants can either be recruited on permanent positions with tenure protections (often denominated 'civil service' positions); or on temporary (term) contracts. Lastly, all four countries feature horizontal pay inequity: managers *can* be granted discretion over pay, by assigning salary complements, increments or higher-graded positions to employees. Public servants can thus earn different incomes despite having similar responsibilities (Meyer-Sahling, Schuster, and Mikkelsen 2018). Our conjoint experiment thus assesses performance-based pay differentiation generally, rather than formalized pay-for-performance schemes.

Survey sample

Across countries, we sought a comparable survey population: central government employees in administrative roles in the broadest sense (excluding, for instance, police, military or teachers) across levels of hierarchy.[5]

While we surveyed comparable populations, local circumstances obliged us to vary survey modes. In Ghana and Uganda, governments did not have – or were unwilling to share – complete lists of public employees in central government institutions, nor their email addresses. We thus relied on in-person surveys and informal quota sampling. Our local collaborators contacted individual government institutions and asked for access, with an effort to quota sample

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[5]Our Kosovo sample additionally comprised local government employees.

respondents across central government institutions, education levels, job functions, contract types, ages and ranks in hierarchy. Enumerators then conducted face-to-face interviews with 1,422 (Uganda) and 1,601 (Ghana) public servants (from January to September 2017). A random subset of one third (Uganda) and two thirds (Ghana) of respondents were presented with the conjoint experiment, leading to 459 (Uganda) and 1,059 (Ghana) conjoint respondents (Appendix A.2).

In Chile and Kosovo, by contrast, governments provided us with email lists of all public servants in 11 central government institutions (Chile) or all central and local government civil servants (Kosovo).[6] 5,742 (Chile; 37% response rate) and 2,470 (Kosovo; 14% response rate) public servants responded to our survey online; and 4,192 and 817 respondents completed the conjoint embedded in the survey (see Appendix A.2). Our sampling strategy yielded a diverse set of public servants (Appendix A.2). In Ghana and Chile – where we count on data to assess representativeness – our samples roughly approximate survey populations in gender and education (Appendix A.3). At least on some demographics, our samples thus appear to meaningfully reflect public servant populations.

Choice experiment

In our experiment, we asked public servants to choose which of two hypothetical colleagues in their institution would be more inclined to engage in corruption. We randomly assigned values of six attributes of these colleagues: position, year of appointment into the public sector, education, recruitment into public sector, contract and salary relative to colleagues with similar responsibilities.[7] The values randomly assigned to each attribute are listed in Table

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[6]In Chile, the sample includes the Civil Service Agency, Treasury, Economic Development Agency, Social Security Administration, National Health Fund, Solidarity and Social Investment Fund, Attorney General, Directorate for Libraries, Archives and Museums, Planning Directorate in the Ministry of Public Works, National Fishery Service and Legal Medical Service.

[7]To avoid primacy effects, we randomized the attribute order across respondents. In Kosovo and Africa – where we presented respondents with two successive profile comparisons – we fixed this randomized attribute order across iterations for respondents to limit cognitive complexity.

1. They were phrased to ensure realism in local contexts *and* comparability across countries. To exclude profiles with combinations without real-world referents, we imposed restrictions on randomization where necessary.[8]

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[8]In Kosovo and Chile, we excluded the combination technical-professional level and high school education (as university education is required for technical-professional ranks). In Chile, we also excluded the combination appointment and permanent contract (as all permanent recruits are hired through Weberian competitions).

Table 1: Attributes and their Values in the Conjoint Experiment, by Country

Attributes	Chile	Ghana	Kosovo	Uganda
Education	- Secondary School - University Degree	- Secondary School - University Degree (Bachelor, Master or PhD)	- Secondary School - University Degree	- Secondary School - University Degree (Bachelor, Master or PhD)
Position	- Administrative - Technical-professional	- Administrative Support - Technical-Professional	- Technical-administrative level - Professional level	- Administrative Support - Technical-Professional
Year of appointment into the public sector	- 2005 - 2010 - 2015	- 2005 - 2010 - 2015	- 2005 - 2010 - 2015	- 2005 - 2015
Recruitment into the public sector	- Direct appointment - Public competition	- Recruited through Competitive Exam Process - Recruited through Competitive Interview Process - Appointed without Competition	- Appointment based on previous connections of the candidate - Personal interview - Written examination and personal interview	- Recruited through an examination - Recruited through direct appointment
Contract	- Permanent - Contract (temporary) - Consultancy (temporary)	- Permanent - Temporary	- Career civil servants (permanent contract) - Non-career civil servants (temporary contract)	- Permanent - Temporary
Salary relative to colleagues with similar responsibilities	- Inferior - Comparable - Superior	- Inferior - Comparable - Superior	- Inferior - Comparable - Superior	- Inferior - Comparable - Superior

To visualize the experiment, figure 1 contains a typical profile comparison, using the example of Uganda. The experiment asks respondents to 'look carefully at the two profiles.' This was followed by a corruption question which asked respondents to choose between the two profiles.[9]

We measure corruption with the question: 'Which public servant would be more likely to accept money or a personal present at work, if given the opportunity?' We developed and pre-tested this measure in each country through local-language cognitive interviews with ten public servants. Our cognitive interviews suggest that respondents across countries understood this as a measure of abuse of public office for private gain. This 'private gain', of course, accrues in the first place to bureaucratic agents – not their principals. Where agents owe their employment and pay to principals, however, they are often expected – and do – pass part of these private gains on to their principals (see, e.g. Gingerich 2014).

Three conjoint attributes assess civil service management practices: 'Recruitment into the public sector', 'Contract' and 'Salary relative to colleagues'. To assess **H1a** and **H1b**, the 'Recruitment' attribute randomly takes on public competitions or appointments as values.[10] To assess **H2a** and **H2b**, our 'Contract' attribute randomly takes on 'permanent' or 'temporary' contracts.[11]

Finally, we assess **H3a** and **H3b** with a combination of the 'Salary relative to colleagues' attribute – which randomizes whether public servants have superior, inferior or comparable salaries to colleagues with similar responsibilities – and a survey question which asked respondents how frequently, in their institution, the following statement applies: 'If a public servant earns more than colleagues with similar responsibilities, it is because he/she has performed

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[9]Our experiment contained additional post-conjoint questions, whose order was randomized to minimize priming effects.

[10]The wording for these values was adapted to country contexts to safeguard measurement validity. In Kosovo and Ghana, we further differentiate competitive interviews and competitive exams to disentangle different competitive selection mechanisms.

[11]To reflect local terminology, we adapted wording in Chile and Kosovo, and differentiated two different temporary contracts in Chile.

Figure 1: **Example Profile Comparison (Uganda)**

Next, I would like to ask you to assess profiles of hypothetical colleagues. I will now present you with a comparison of two profiles of hypothetical public servants in your institution. Please carefully look at the two profiles and then answer the questions below based on your experience in your institution.

	Public servant 1	Public servant 2
Position	Administrative Support	Technical-Professional
Recruitment into the public sector	Recruited through direct appointment	Recruited through direct appointment
Education	University Degree (Bachelor, Master or PhD)	Secondary School
Year of appointment into the public sector	2005	2015
Salary (incl. allowances) relative to colleagues with similar responsibilities	Comparable	Comparable
Contract	Temporary	Permanent

Which public servant would be more likely to accept money or a personal present at work, if given the opportunity?

Public servant 1   Public servant 2
  ◉   ○

better' (5-point scale: 'Never or almost never' to 'Always or Almost always'). This allows us to compare the effects of pay differentiation between subgroups of respondents who associate pay differentiation with performance differences and respondents who do not. We can thus assess the two implications of performance pay for public sector pay setting which, as argued, may shape its effect on corruption: pay differentiation, including inferior pay for some public servants; and discretion by principals over pay. As noted, 'principled' principals can use this discretion to reward performance (in our measure: superior pay is perceived to be due to performance), while 'unprincipled' principals can use it for other purposes (in our measure: superior pay is perceived to be detached from performance).

Following Oliveros and Schuster (2018), our conjoint also includes three common correlates of corruption: education (university versus high school), rank (administrative support vs. technical-professional) and years of experience in the public sector (year of entry in 2005, 2010 or 2015). The additional attributes strengthen the realism of the presented profiles and

Figure 2: **Example Conjoint Results for all Attributes (Uganda)**
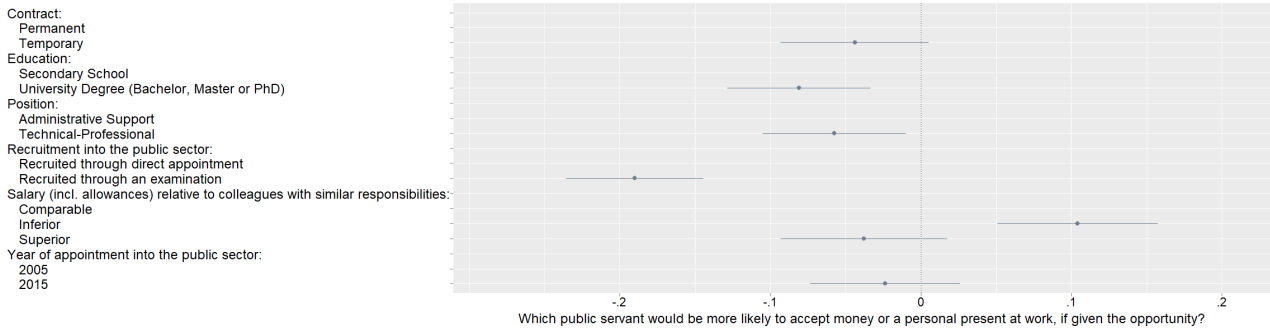
reduce confounding risks (see Dafoe, Zhang, and Caughey 2018).

Results

We estimated our conjoint models with the cjoint R package (Barari et al., 2017).[12] Appendix B contains full sample results for each country. To facilitate their interpretation, figure 2 plots point estimates – average marginal component effects (AMCEs) – with 95% confidence intervals for one illustrative country (Uganda). AMCEs represent the average effect of an attribute on the probability that a public servant would find the hypothetical colleague with this attribute more or less likely to engage in corruption, relative to a hypothetical colleague with a baseline value. In Uganda, for instance, public servants are more likely to associate greater corruption with hypothetical colleagues on inferior pay (relative to a baseline value of colleagues with comparable pay); and lower corruption with hypothetical colleagues hired through examinations (relative to appointments), who are university educated (relative to high school) and at technical-professional (relative to administrative support) ranks. Other attributes have no significant effects. With this interpretation in mind, figures 3 to 6 compare the effects of recruitment, contracts and pay across countries.

H1a and **H1b** presuppose that Weberian competitions for recruitment are, relative to appointments, associated with lower corruption in Ghana, Uganda and Kosovo – yet not Chile. Figure

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[12]We ran separate regressions for each country rather than pooling data across countries. Where public servants responded to two conjoint iterations, we clustered standard errors by respondent (cf. Hainmueller, Hopkins, and Yamamoto 2014).

Figure 3: **Weberian Competition for Recruitment and Bureaucratic Corruption**
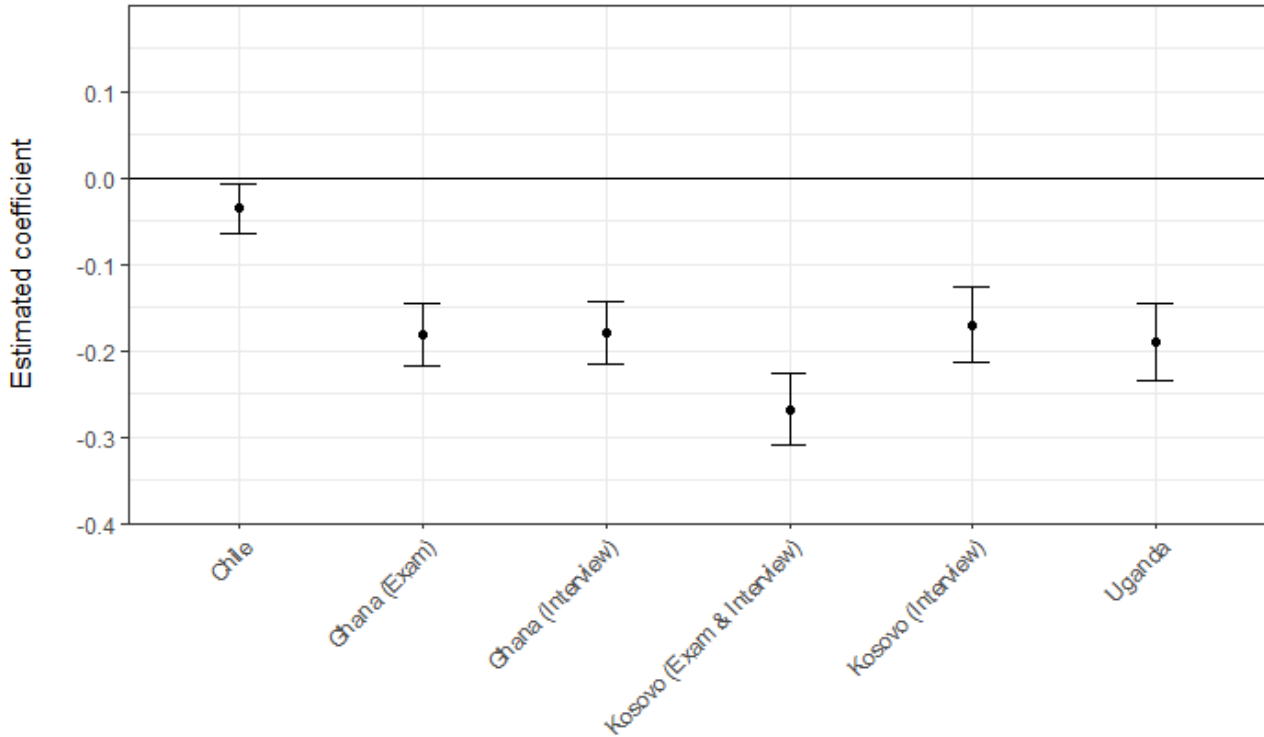
3 provides qualified support. Confirming **H1a**, Weberian competitions are associated with significantly lower corruption in all three surveyed developing countries. Contrary to **H1b**, however, Weberian competitions also have a significant effect in the surveyed OECD country (Chile). Substantively, however, this effect is marginal. Respondents are only 3.5% more likely to find colleagues hired through Weberian competitions – relative to appointees – to be less corrupt. The effect size in the three developing countries is larger by a factor of five to eight. In line with our theoretical reasoning, managerial discretion over recruitment thus only marginally affects corruption in the OECD country, but has large effects in developing countries.

Our conjoint results also provide some – albeit more limited – qualified support for **H2a** and **H2b** (figure 4). Consistent with **H2b**, permanent contracts have no significant effect in our surveyed OECD country (Chile) – irrespective of the type of temporary contract (annual or shorter term) used as a baseline. In Ghana and Kosovo, by contrast, permanent contracts are associated with lower corruption, suggesting that **H2a** holds in those two cases. As theorized,

Figure 4: **Permanent Job Contracts and Bureaucratic Corruption**
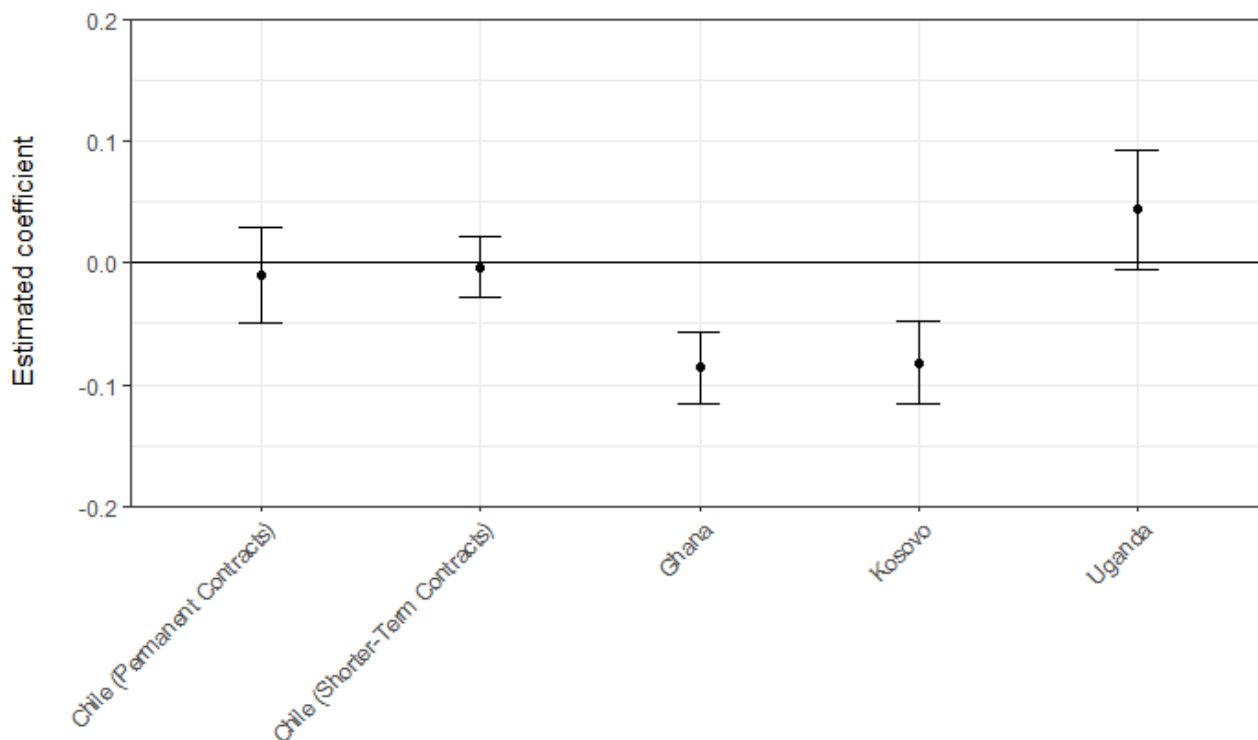
managerial discretion over dismissals is thus associated with greater corruption in two surveyed developing countries, yet not the surveyed OECD country. In our third surveyed developing country (Uganda), however, job contracts have no significant effect. Our theoretical argument thus does not fully explain the observed cross-country variation in job contract effects; other factors are also at play. We return to this qualification of our findings in the conclusion.

Lastly, our findings provide some – albeit again more limited – support for **H3a** and **H3b**. We present these findings in two steps, reflecting the two implications of performance-based pay differentiation which, as argued, may shape its effects on corruption: pay differentiation and discretion by principals over bureaucratic pay. Figure 5 visualizes the effects of pay differentiation, without probing whether principals differentiate pay based on performance or for other reasons, such as corruption. In Chile, pay differentiation has no significant effects, confirming **H3b**. By contrast, as theorized, pay differentiation does matter in the surveyed developing countries. Inferior pay is, as hypothesized, associated with greater corruption in all three coun-

Figure 5: **Pay Differentiation and Bureaucratic Corruption**
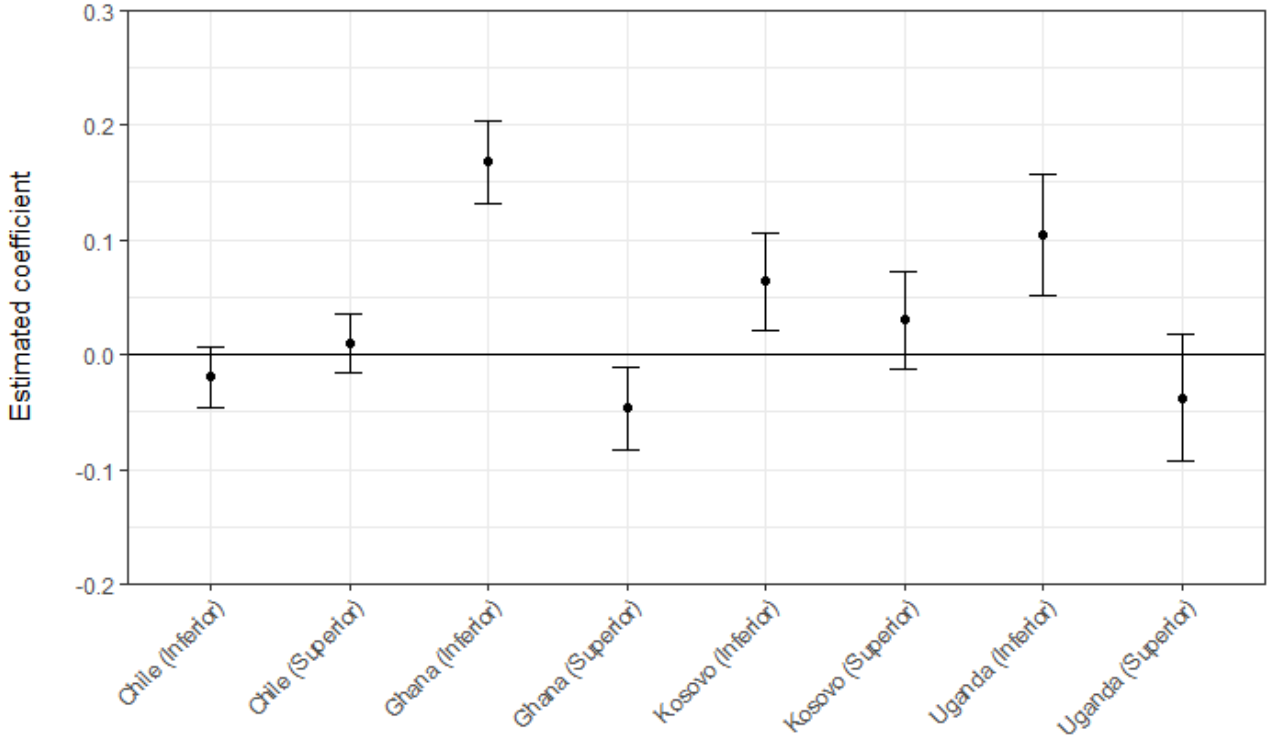
tries. At the same time, superior pay is associated with significantly *lower* corruption in Ghana – albeit not Uganda and Kosovo.

Figure 6 probes whether this pattern is consistent with our theoretical reasoning – that the effects of pay differentiation depend on whether managers use discretion to reward performance or less 'principled' purposes – by comparing the effects of pay differentiation based on performance with pay differentiation detached from performance.[13] Where respondents link pay differentiation to work performance, we may expect that principals are not using pay discretion to favor politically or personally-connected 'unprincipled' agents – and pay differentiation should not enhance corruption. We find qualified support for this argument. Inferior pay is associated with significantly greater corruption in all three countries (figure 5). At the same time, superior pay is only associated with lower corruption for the minority of respondents

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[13]We do so by splitting the sample into respondents who indicate that the statement 'If a public servant earns more than colleagues with similar responsibilities, it is because he/she has performed better' applies 'Always or almost always', 'Often' or 'About half the time' in their organization, and respondents indicating 'Never or almost never' or 'Rarely'.

22

in Uganda and Ghana who perceive pay to be tied to work performance (Appendix A.2; in Kosovo, there is no significant effect). It has no significant anti-corruption effect where it is detached from performance. Where 'unprincipled' principals in developing countries do not base pay differentiation on performance – and most of our respondents suggest they do not – relatively superior pay does not, in the perception of our respondents, curb corruption and thus offset the greater corruption of those on relatively inferior pay. Managerial discretion over pay thus appears to be associated with greater corruption under 'unprincipled' principals relative to Weberian pay equality, by enhancing corruption of those on relatively lower pay who do not receive performance pay.

While we thus find qualified evidence for our theoretical argument about differential effects of pay differentiation between developing and OECD countries, one important caveat to our evidence for **H3a** is due: our findings also underscore differential effects of pay differentiation *within* developing countries. According to our data, a minority of principals in developing countries are 'principled': they use discretion over pay to tie pay to performance. In their weak rule of law contexts – where disciplinary procedures and courts do not sanction corruption – superior pay linked to performance can curb corruption, albeit at the cost of greater corruption of those who are, by implication, on inferior pay. Lower corruption by those receiving higher, performance-based pay thereby fully (Uganda) or partially (Ghana) offsets greater corruption by those on relatively lower pay. In other words, for the minority of 'principled' principals in developing country governments, we do not find consistent support for **H3a** (nor would we expect to, as H3a assumes principals are 'unprincipled' in developing country governments).

In sum and with this caveat in mind, our conjoint results support our overarching theoretical argument that managerial discretion over personnel decisions is associated with greater corruption in developing countries, yet not OECD countries. In our three developing countries, we observe the theorized effects for recruitment in all three countries; for dismissals in two coun-

Figure 6: **Performance-Based Pay Differentiation and Bureaucratic Corruption**
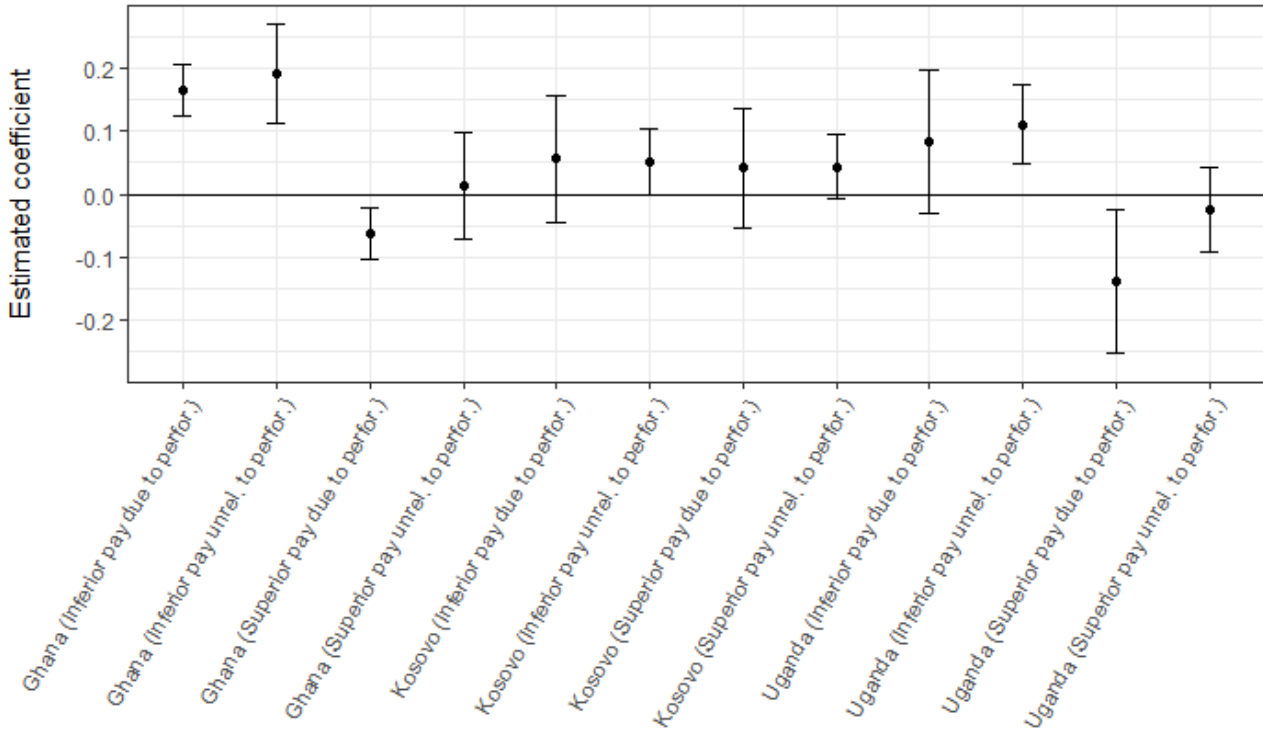
tries (with no significant effect in the third country); and for performance pay in two countries (with pay differentiation enhancing corruption of public servants on inferior pay, without a commensurate decrease in corruption by those on higher pay). By contrast, effects are, across personnel decisions, marginal in our surveyed OECD country.

Importantly, these findings are not merely an artefact of respondents preferring hypothetical colleagues with characteristics like their own. In all four countries, respondents with high school and university education do not assess the effects of university education differently; and respondents at different ranks do not assess the effects of administrative support and professional ranks differently. Moreover, in three of four countries, respondents on permanent and temporary contracts do not provide significantly different effects for job contracts (Appendix C). Our conjoint estimates thus plausibly reflect valid associations.

Conclusions and Implications

Developing country governments frequently adopt management practices developed in OECD contexts. Do management practices have the same effects in developing and OECD countries? A large qualitative literature is skeptical, cautioning against 'best practice' transfers (see, e.g., Andrews 2013; Grindle 2004; Levy 2014; Brinkerhoff and Brinkerhoff 2015; Evans 2004; Schick 1998). Given the prominence of these concerns in scholarship and their weight in development practice, one would expect numerous studies which have rigorously – with the same research design – estimated quantitatively which management practices have similar and which have diverging effects in developing and OECD countries. Yet, this literature is largely non-existent. We thus lack rigorous comparative evidence about the effects of the same management practices in developing and OECD countries.

The results from our four-country conjoint experiment with 6,500 public servants help address this gap for a 'best practice' transfer of particular concern in qualitative works: 'New Zealand' reforms which enhance managerial discretion. Developing countries are argued to lack the necessary rule-of-law and integrity norm preconditions to 'let the managers manage' (Schick 1998). Expanding on this precondition argument for civil service management, we argued that, in developing countries with institutionalized corruption, 'unprincipled' principals abuse managerial discretion over hiring, firing and pay to favour 'unprincipled' agents in the bureaucracy who help principals advance private corruption interests. Weak rule-of-law – and thus limited sanctioning of corrupt behavior – in such contexts enables 'unprincipled' agents to commit 'unprincipled' corruption acts. By contrast, in OECD countries with stronger norms of integrity, more 'principled' principals are more likely to favour 'principled' agents – and strong rule-of-law deters acts of corruption by agents. As a consequence, managerial civil service practices enhance corruption in developing countries, yet have only marginal effects in OECD contexts.

Our conjoint results provide confirmatory evidence: managerial discretion over recruitment, firing and pay are, in the perception of our respondents, associated with greater corruption in most (firing and pay) or all (recruitment) surveyed developing countries – while having no (firing and pay) or marginal (recruitment) effects in the surveyed OECD country.

Our results have important implications for scholarship and practice. Most immediately, our findings provide quantitative evidence for prominent concerns in qualitative works about reforms which expand managerial discretion in developing countries. Our conjoint results suggest they are associated with greater corruption in such contexts. This OECD (alleged) 'best practice' may thus well be 'worst practice' in developing countries. Weberian reforms which instil rule compliance and norms of integrity should, instead, come first. In OECD contries where such norms and rule of law are in place, by contrast, our results suggest that concerns about abuse of managerial discretion are rightfully less prominent; perceived effects in Chile were marginal. Unchecked managerial discretion can, of course, still spur corruption – but our evidence suggests that this risk is significantly reduced.

More generally, our findings underscore the importance of a public administration research agenda which systematically – using the same research designs – assesses whether similar management practices have similar or dissimilar effects in developing and OECD countries. Public administration as a discipline has principally focused on OECD countries (Milward et al., 2016). Our study is bad news for the applicability of the findings from this discipline to developing countries. At least for the management practices we had assessed, the effects in the OECD country studied were not generalizable to developing countries. Public administration scholars would thus do well to put greater emphasis on assessing theoretically and empirically – through replication studies or cross-country research designs – which of their findings travel between developed and developing countries, and which lack generalizability.

Our study thus also adds weight to the replication agenda in public administration (see, e.g.

Walker, James, and Brewer 2017). With journals valuing novel theories or methods, publication incentives are tilted against replication or, as in our case, funding- and time-intensive multi-country experiments (Witteloostuijn 2016). This risks disproportionate reporting of false positives or findings with limited external validity (Grimmelikhuijsen et al. 2017). Our study – showing differential effects between OECD and developing countries – underscores the severity of this risk to knowledge accumulation in public administration.

In providing evidence for this pitfall, we hope our study encourages other scholars to undertake cross-country experiments with public servants. Conjoint experiments are one useful method (cf. Oliveros and Schuster 2018). Our design lends itself other management practices – from ethics trainings to leadership practices to teleworking, to name a few – and other outcomes beyond corruption.

These contributions notwithstanding, our experiment comes with limitations, which point to future research avenues. Most obviously, we drew our inferences from a survey rather than field experiment. Our inferences rest on perceptions of public servants about hypothetical colleagues with certain characteristics. These characteristics have real-world referents for our respondents: they work with colleagues with such characteristics on a daily basis and can thus make plausible inferences (cf. Oliveros and Schuster 2018). Yet, these inferences remain perception-based and potentially subject to confounding and measurement error. Whether our results hold in a field experiment thus remains for future works to assess.

Second, our study lacks evidence to disentangle the relative weight our two complementary mechanisms to explain differential effects in developing countries: less 'principled' principals abusing managerial discretion to reward or hire 'unprincipled' agents who are more prone to engage in corruption; and weak rule-of-law precluding sanctions - and thus discouragement - of corruption by 'unprincipled' agents. Other studies suggest that both are plausible. Politicization and nepotism has been found to be prevalent in personnel decisions in developing,

yet not OECD governments (cf. Kopecký et al. 2016; Sundell 2014; Meyer-Sahling, Schuster, and Mikkelsen 2018). Moreover, sanctioning of corruption varies sharply between OECD and developing countries (Global Integrity 2010).

Third, we generalized about effects in developing and OECD countries based on data from three developing countries and a single OECD country. Our three developing countries are in different regions (Eastern Europe and Africa) and feature different political systems, varying between a free democracy (Ghana) and a partially autocratic state (Uganda). Mostly similar effects despite this variation enhance confidence that findings are generalizable to other developing countries. Our claim to generalizability across OECD countries is empirically weaker, with data from a single OECD country. Theoretically, however, our findings may well be expected to travel. Most other OECD countries score slightly above Chile in rule of law and corruption control. As such, managerial practices could be expected to have an even more marginal effect on corruption.

These claims about generalizability, however, come with a caveat: our theoretical argument cannot fully explain observed cross-country variation in the effects of civil service management practices *within* the developing world. In Uganda, temporary contracts were not associated with greater corruption; and, in Ghana, relatively superior pay was associated with significantly lower corruption. Country-level factors beyond those we had theorized about thus shape the effects of management practices. Future quantitative cross-country work on management practices would do well to cover further countries to understand more systematically the range of country factors conditioning the effects of management practices. In light of this, our study represents, most of all, a call for more public administration research which systematically tests findings across and within developing and developed regions.

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Appendix

Appendix A. Survey Sample

Appendix A.1 Country Characteristics

	Rule of Law Rank (World Bank Governance Indicators)	Corruption Perception Rank (Transparency International)
Chile	1st Quintile Rank (82nd Pct.)	1st Quintile Rank (Score: 67/100)
Ghana	3rd Quintile Rank (59th Pct.)	3rd Quintile Rank (Score: 40/100)
Uganda	3rd Quintile Rank (42nd Pct.)	5th Quintile Rank (Score: 26/100)
Kosovo	4th Quintile Rank (36th Pct.)	3rd Quintile Rank (Score: 39/100)

Appendix A.2 Survey Sample Demographics by Country

	Chile	Ghana	Uganda	Kosovo	All
Survey mode	Online	In-person	In-person	Online	-
Number of respondents	5,742	1,601	1,422	2,470	11,235
Number of conjoint respondents	4,192	1,059	459	814	6,524
Number of conjoint iterations	One	Two	Two	Two	-
Percentage female	56.1%	46.8%	43.5%	42.7%	47.3%
Percentage university graduated	49.6%	80.4%	87.9%	95.4%	78.3%
Percentage managers	23.1%	11.6%	12.2%	29.5%	19.1%
Percentage technical-professional	53.6%	39.6%	43.5%	61.0%	49.4%
Percentage administrative support	23.3%	47.4%	43.8%	7.9%	30.6%
Mean age (in years)	40[a]	35	36	40	38
Mean years of service (in years)	16	14	11	13	14
Percentage recruited through exams	39.9%	16.9%	61.9%	49.6%	42.1%
Percentage on permanent contracts	40.3%	93.1%	79.1%	96.2%	77.2%
Percentage indicating that their pay is tied to work performance	40.9%	31.5%	17.3%	38.9%	32.2%

Notes: Values for full sample and complete cases. (a) banded age variable recoded using midpoints.

Appendix A.3 Survey Representativeness in Ghana and Chile

	Chile		Ghana	
	Survey sample (conjoint respondents)	Survey population (all central government)	Survey sample (conjoint respondents, excl. administrative assistants)	Survey population (excl. administrative assistants)
Percentage female	56.1%	58%	46.8%	45%
Percentage university educated	49.6%	50%	80.4%	74%
Mean age	39[a]	n/a	35	42

Notes: (a) banded age variable recoded using midpoints.

Sources: Direccion de Presupuestos del Ministerio de Hacienda (2017); Rasul, Rogger and Williams (2015)

Appendix B. Conjoint Results (Full Sample), by Country

Appendix B.1 Kosovo

Attribute	Level	Estimate	*SE*
Contract	Permanent contract	-0.082***	0.018
Education	University Degree	-0.100***	0.025
Position	Tech.-Admin. level	0.039*	0.017
Recruitment into the public sector	Personal interview	-0.170***	0.022
Recruitment into the public sector	Written exam & personal interview	-0.268***	0.021
Salary relative to colleagues with similar responsibilities	Inferior	0.064**	0.022
Salary relative to colleagues with similar responsibilities	Superior	0.030	0.022
Year appointment into the public sector	2008	0.009	0.022
Year appointment into the public sector	2010	0.008	0.022

Notes: Number of Obs. = 3,112. Number of Resp. = 814. Signif. codes: '***' 0.001 '**' 0.01 '*' 0.05.

Appendix B.2 Chile

Attribute	Level	Estimate	*SE*
Contract	Temporary contract (Shorter-term)	0.004	0.013
Contract	Permanent contract	0.01	0.021
Education	University Degree	-0.021	0.013
Position	Tech.-Professional level	0.009	0.014
Recruitment into the public sector	Competitive recruitment	-0.035*	0.014
Salary relative to colleagues with similar responsibilities	Inferior	-0.019	0.014
Salary relative to colleagues with similar responsibilities	Superior	0.010	0.013
Year appointment into the public sector	2010	-0.026	0.014
Year appointment into the public sector	2015	0.010	0.013

Notes: Number of Obs. = 8,384. Number of Resp. = 4,192 . Signif. codes: '***' 0.001 '**' 0.01 '*' 0.05.

Appendix B.3 Uganda

Attribute	Level	Estimate	SE
Contract	Permanent contract	0.044	0.025
Education	University Degree	-0.081***	0.024
Position	Tech.-Professional level	-0.058*	0.024
Recruitment into the public sector	Written exam	-0.190***	0.023
Salary relative to colleagues with similar responsibilities	Inferior	0.104***	0.027
Salary relative to colleagues with similar responsibilities	Superior	-0.038	0.028
Year appointment into the public sector	2015	-0.024	0.025

Notes: Number of Obs. = 1,814. Number of Resp. = 459. Signif. codes: '***' 0.001 '**' 0.01 '*' 0.05.
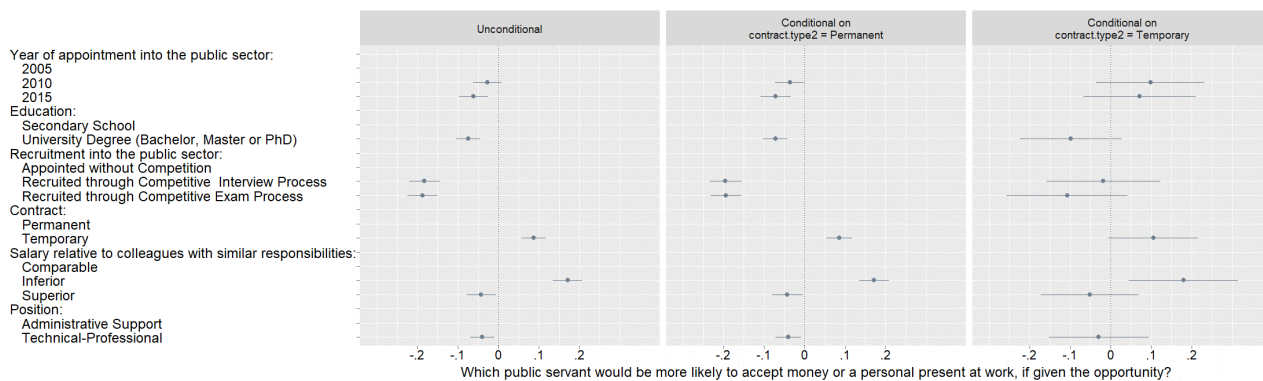
Appendix B.4 Ghana

Attribute	Level	Estimate	*SE*
Contract	Permanent contract	-0.086***	0.015
Education	University Degree	-0.074***	0.015
Position	Tech.-Professional level	-0.041**	0.015
Recruitment into the public sector	Personal interview	-0.179***	0.019
Recruitment into the public sector	Written exam	-0.182***	0.019
Salary relative to colleagues with similar responsibilities	Inferior	0.168***	0.018
Salary relative to colleagues with similar responsibilities	Superior	-0.047*	0.018
Year appointment into the public sector	2010	-0.026	0.018
Year appointment into the public sector	2015	-0.065***	0.018

Notes: Number of Obs. = 4,200. Number of Resp. =1,059. Signif. codes: '***' 0.001 '**' 0.01 '*' 0.05.
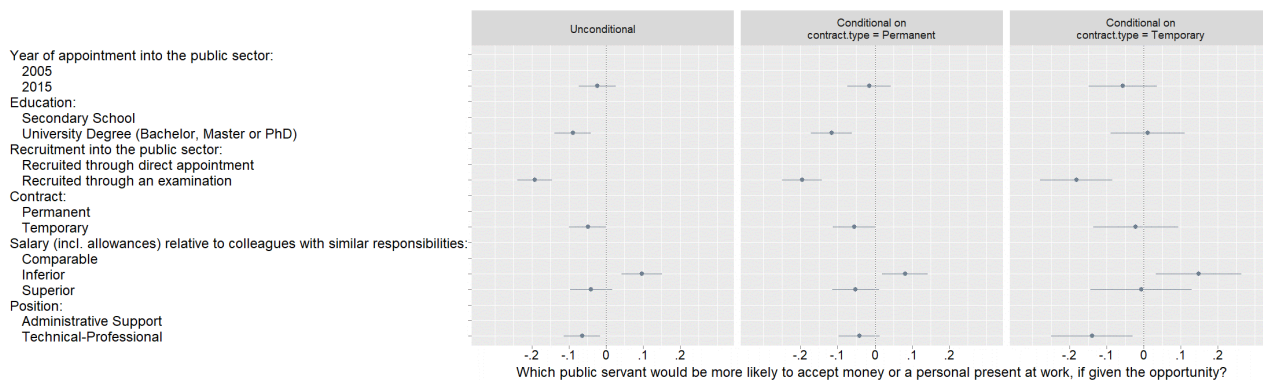
Appendix C. Robustness Check: Bias to Favor Public Servants Who Share Characteristics with Respondents

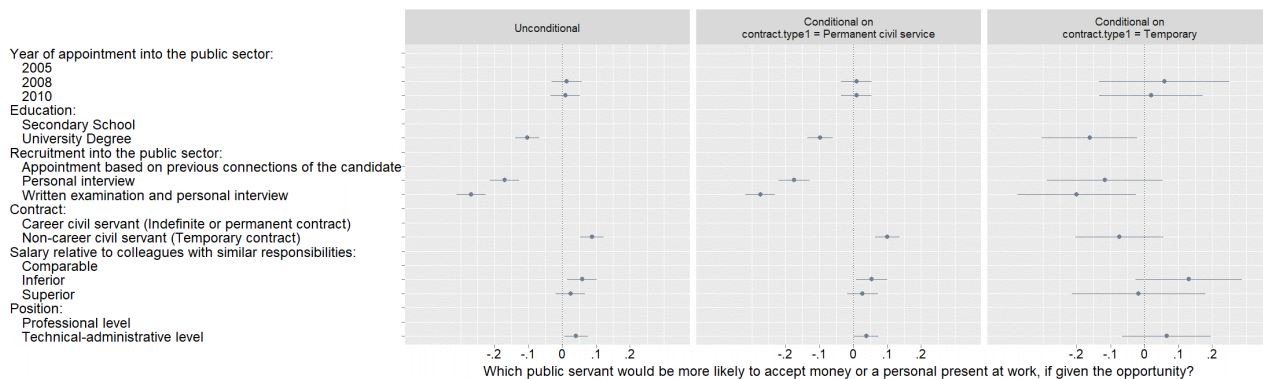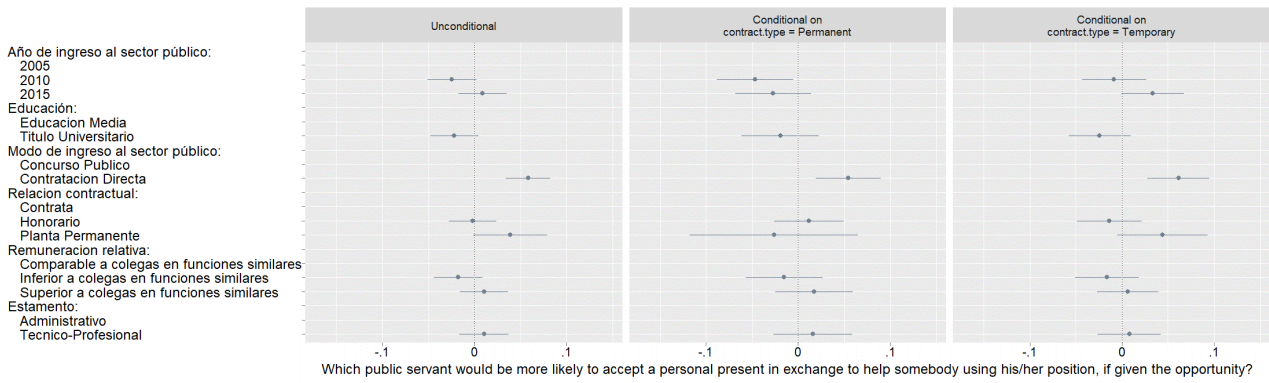## Appendix C.1 AMCEs by Contract Type of Respondent

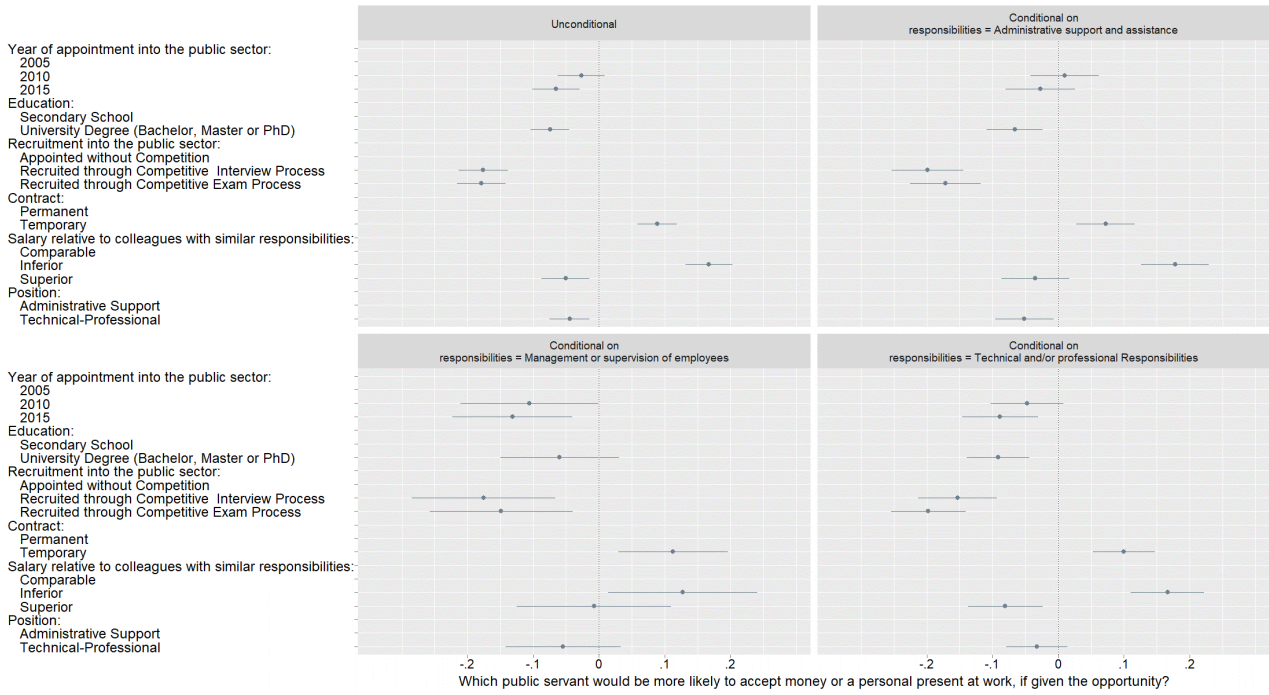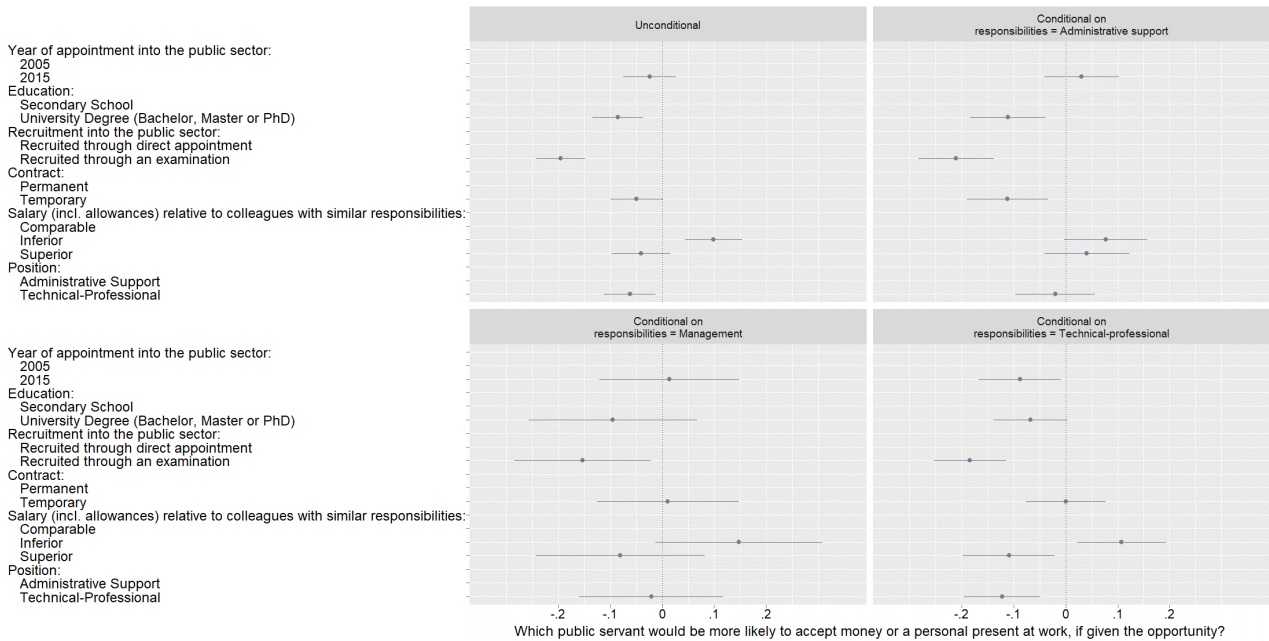### Ghana

Uganda

Kosovo

Chile

	Unconditional	Conditional on contract.type = Permanent	Conditional on contract.type = Temporary
Año de ingreso al sector público:			
2005			
2010			
2015			
Educación:			
Educacion Media			
Titulo Universitario			
Modo de ingreso al sector público:			
Concurso Publico			
Contratacion Directa			
Relacion contractual:			
Contrata			
Honorario			
Planta Permanente			
Remuneracion relativa:			
Comparable a colegas en funciones similares			
Inferior a colegas en funciones similares			
Superior a colegas en funciones similares			
Estamento:			
Administrativo			
Tecnico-Profesional			

Which public servant would be more likely to accept a personal present in exchange to help somebody using his/her position, if given the opportunity?

Appendix C.2 AMCEs by Rank of Respondent

Ghana

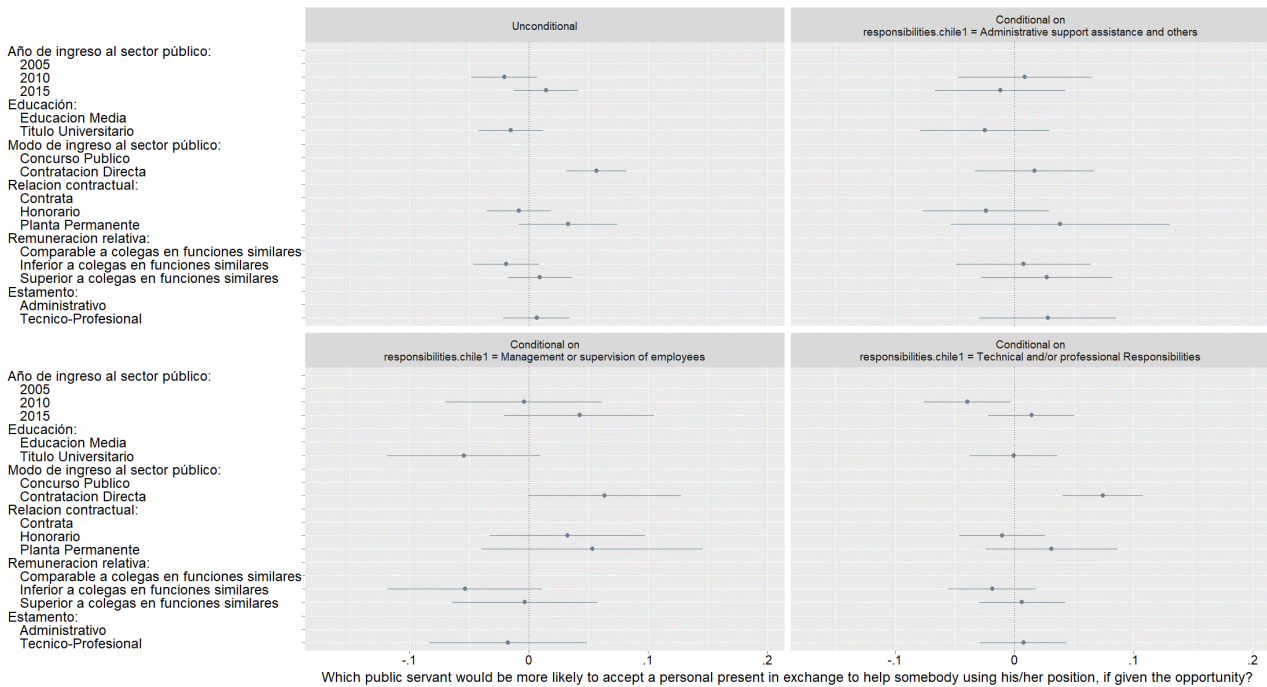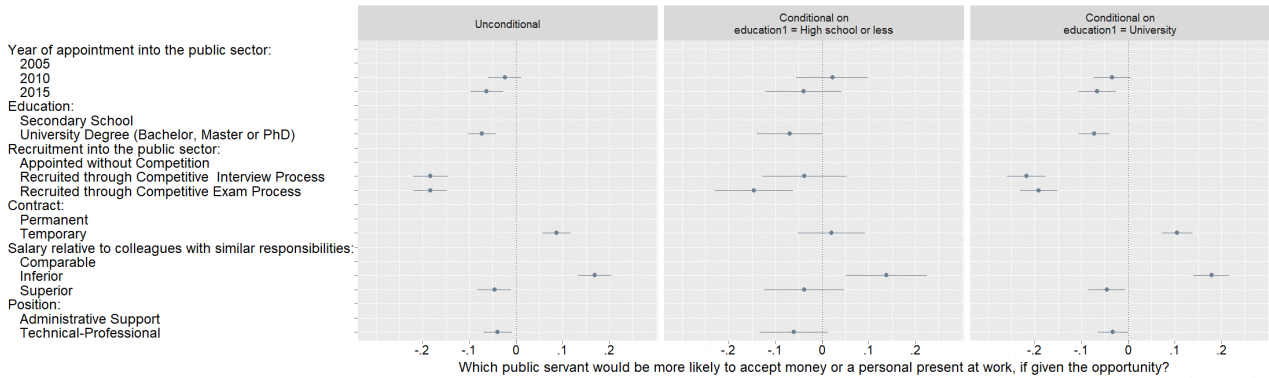## Uganda

Kosovo

Which public servant would be more likely to accept money or a personal present at work, if given the opportunity?

Chile

Which public servant would be more likely to accept a personal present in exchange to help somebody using his/her position, if given the opportunity?
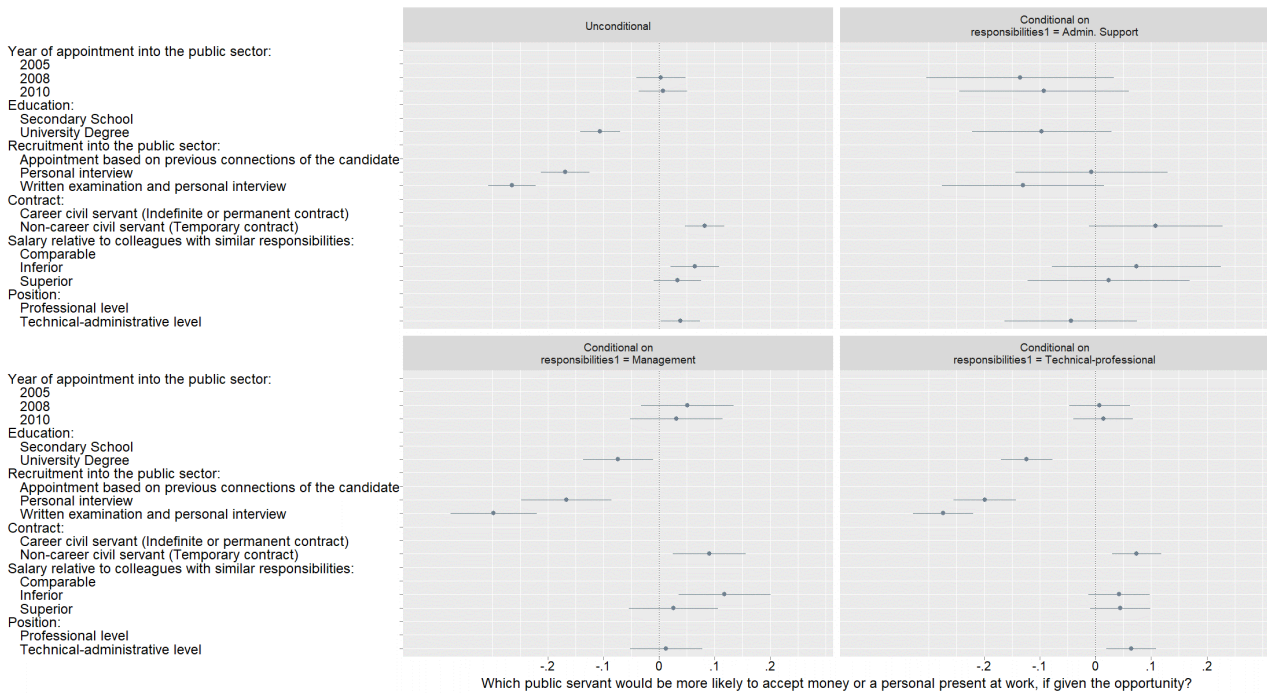
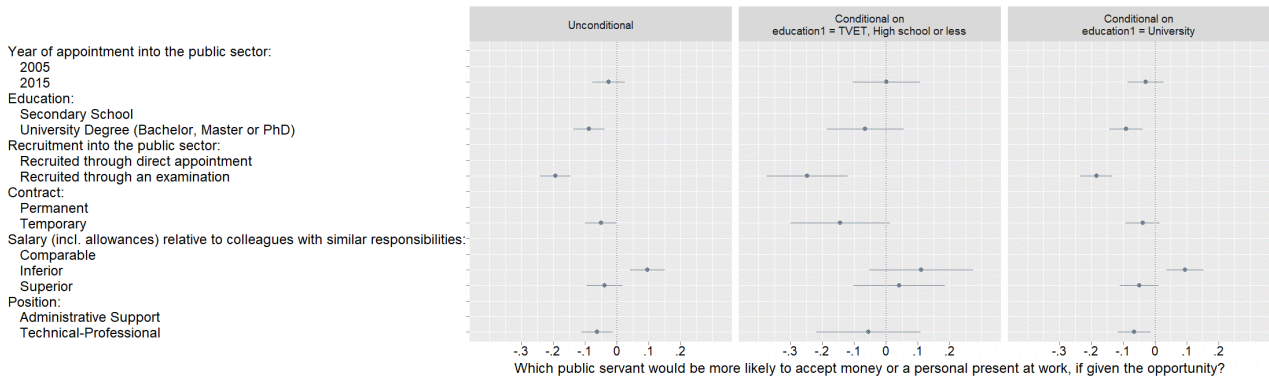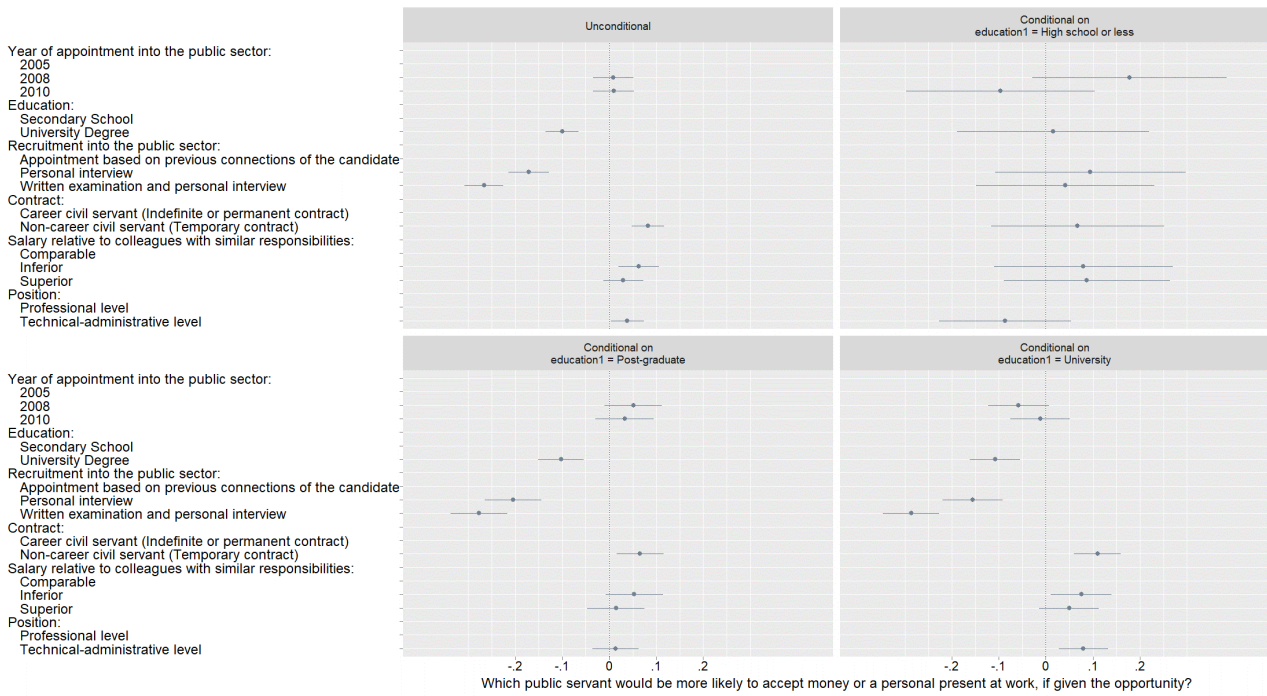# Appendix C.3 AMCEs by Education of Respondent

Ghana

Uganda

Kosovo

Which public servant would be more likely to accept money or a personal present at work, if given the opportunity?

Chile

Which public servant would be more likely to accept a personal present in exchange to help somebody using his/her position, if given the opportunity?