Filled-In Pre-Analysis Plan for *The Allocation of Authority in Organizations: A Field Experiment with Bureaucrats*[*]

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This document contains the pre-analysis plan for the experiment preregistered in the Social Science Registry at https://www.socialscienceregistry.org/trials/610. The text of the original PAP is in italics, and the corresponding results follow each point.

1 Analysis

Our main analysis of the experimental results will consist of running regressions of the form

$$y_{it} = \alpha + \beta_1 T_{1,it} + \beta_2 T_{2,it} + \beta_3 T_{1,it} \times T_{2,it} + \mathbf{S}_{igt}\delta + \varepsilon_{igt} \quad (1)$$

where y_{it} is one of the outcomes of interest described below in section 2; $T_{1,it}$ is a dummy indicating that the Drawing and Disbursing Officer (DDO) making purchase i is in the performance incentives treatment at time t; $T_{2,it}$ is a dummy indicating that the DDO is in the rules treatment at time t; $\mathbf{S}_{igt}$ is a vector of stratification variables used in the randomization (department and

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district) and ε_{igt} is a residual. We will estimate both average and quantile treatment effects, and perform randomization inference whenever our sample sizes make it appropriate.

2 Outcome variables

2.1 Main outcomes

Our main outcome is value for money, as measured by the quality-adjusted unit price paid for each item. To create it we follow Bandiera et al. (2009) and run regressions of the following form for each good

$$p_{igt} = \mathbf{X}_{igt}\beta + \gamma_t + department_i + district_i + \mu_{igt} \tag{2}$$

where p_{igt} is the log of the unit price paid in transaction i for good g in month t; $\mathbf{X}_{igt}$ is a vector of observables including the log quantity purchased and all the good's attributes; γ_t are month fixed effects, $department_i$ are department fixed effects; $district_i$ are district fixed effects; and μ_{igt} is a residual which will become our main outcome of interest: the quality-adjusted log unit-price.

During our analysis, we realized that we can be less restrictive than this specification implies in two ways. First, rather than using the residuals from (2) as the dependent variable, we use the fitted values as a control variable, thus avoiding imposing a coefficient of 1 on it. Second, we use good-specific time trends instead of month fixed effects in (2) and drop the department and district fixed effects. Instead we control for department $\times$ district in the main regressions.

Neither of these changes makes much difference, but since they are less restrictive we preferred them. Table 1 contains the results from all possible permutations of these two changes for transparency. In column 1 we use the specification (2) with its fitted values as controls in the main equation (1). Column 2 is our preferred specification. It uses our preferred way of controling for the variety of the item being purchased and puts the fitted values in the main equation as controls. Column 3 uses the residuals from specification (2) as the outcome variable, and column 4 uses the residuals from our preferred way of controling for item variety as the outcome.

In all cases we see a strong treatment effect of the autonomy treatment (around 8–9%) and also of the combined treatment (around 7–8%), but not of the incentives treatment. These treatment effects are fairly constant throughout the distribution of prices: figure 1 shows quantile treatment effects for the three treatments.

We will also estimate treatment effects on two main other outcomes. First, measures of how much time the bureaucrat spends on procurement relative to other duties. Second, whether the bureaucrat successfully spends his/her entire budget before the end of the fiscal year, and if not,

how much of it they are able to spend.

Figure 2 shows our analysis of the effects of the experiment on the time bureaucrats allocate to procurement. We use questions 207 and 208 in the attached end-line survey. Question 207 asks bureaucrats to allocate months of the year to "very busy", "somewhat busy" and "not busy" months for procurement. Question 208 then asks bureaucrats to specify the fraction of their time in each type of month they spend on procurement. We first combine these into a measure of the total amount of time in the year spent on procurement by averaging the answers to question 208, weighting by the answers to question 207. We find a 12% increase in the total amount of time spent on procurement in the autonomy treatment. The other groups show increases but they are not significant, and we cannot reject the hypothesis that the increase is the same in all three groups. Breaking this up, panel B shows that there are fewer months that are not busy for procurement (question 207), and panel C shows that months that are very busy are busier (question 208).

Table 2 shows our analysis of the effects of the experiment on budget utilization. We use administrative data from the finance department on budgets and expenditures in each accounting category. We aggregate the data up to the three main categories that include procurement spending: Operating Expenses, Physical Assets, and Repairs & Maintenance. We also look at the office's full budget, budget on items covered by POPS and budget for items that appear in our analysis dataset. For each of these we measure budget utilization as the ratio of the amount spent to the amount in the office's budget for that category. We regress this on treatment dummies, randomization stratum fixed effects and a control for the number of accounting entities in each office. We do not see any evidence of an effect of the experiment on any of these outcomes, consistent with the demand for spending in these categories being relatively inelastic.

2.2 Other outcomes

In addition to our main outcomes, we will study impacts on two secondary outcomes that may be affected by the treatments.

1. *Legal/procedural compliance. We will measure this using the number and type of irregularities reported in supervisor and auditor reports on the DDO.*
   This has not been possible with the data we have. We learned after writing the PAP that formal reports of irregularities and audit investigations only happen long after the fact. For irregularities during our experiment (July 2014-June 2016) we would have to look at audit reports filed and investigations presented to the Public Accounts Committee of the provincial assembly one or two years later. The investiga-

tions often take another year after the initial findings to reach a resolution and so the data we would need to do this didn't exist at the time of writing.

2. *Favoring corrupt DDOs. We will measure this in two ways. First, by looking for heterogeneous treatment effects by corruptibility as measured by the DDO's score on a dice game we implemented based on Fischbacher & Föllmi-Heusi (2013) and Hanna & Wang (2017). The protocol is attached in section 5 of the attached survey instrument. Second, by looking for heterogeneous effects by the survey measure of political capital in question 111 of the attached survey instrument.*

Unfortunately, the dice game measure seems not to be capturing POs' types successfuly. Figure 3 shows that there is significant variation across POs in their dice scores (relative to what would be expected from honest reporting of scores) in panel A. However, as panel B shows, the POs' scores are not predictive of prices at baseline, suggesting these scores are not successfully capturing POs' types. Unsurprisingly, as table 3 shows, the dice scores also do not predict heterogeneity in the treatment effects.

At the time of surveying, we decided to cut questions 106–114 from the survey due to time constraints, so we are unable to look for heterogeneity by question 111.

3 Imperfect compliance

There are two reasons that being assigned to a treatment group will not necessarily mean that a DDO is treated. First, some of the treatments required the DDOs and their staff to actively decide to participate. Second, some DDOs may have found the treatments hard to understand and/or may have believed that the promises the treatments contained were not credible.

Below we describe how we can measure imperfect compliance. With these measures we can then instrument for actual takeup with treatment assignment to estimate treatment effects on the treated.

3.1 Takeup

We can measure takeup of the Punjab Online Procurement System (POPS) by the number of trainings and information sessions that a DDO was invited to that he/she actually attended. Furthermore we can compare the POPS data to administrative data on expenditures from the finance department to calculate how much of the expenditure that should be recorded in POPS actually appears in POPS.

Offices were told by their departments that they were part of a a study to evaluate the impact of policy reforms under consideration for rollout across the province and that their participation was mandatory, including entering data into the POPS system and cooperating with occasional survey team visits. With this backing, 587 offices, or 85% of the sample, participated in trainings on the POPS system and on the implications of their treatment status for how they conduct procurement. The departments repeatedly reminded offices that entering data into POPS was mandatory and so the offices in our study also entered the vast majority of their purchases into POPS.

Table 4 shows that the attrition of items out of the POPS data is balanced across the treatments and across observable characteristics of the expense. To do this, for each bill reported in the administrative data, we calculate the share of its value that matches to items in the POPS data. We then regress this on treatment indicators and a wide range of observables. We do this separately for all generic goods (columns 1–4) and only the ones appearing in our analysis sample (columns 5–8). The odd-numbered columns use data from year 1 and the even-numbered columns use year 2. In columns 1, 2, 5, and 6 the dependent variable is the share of the bill that is matched to POPS while in the remaining columns the dependent variable is the share of the bill that is in our final analysis dataset. The table reveals some patterns in the types of bills that are less likely to be complete in the data, but no evidence that attrition is differential across the treatment groups.

Moreover, figure 4 shows that the full distribution of attrition rates is balanced across treatments. We measure each office's average attrition rate using procurement office fixed effects δ_o from regressions of the form $s_{bco} = \mathbf{X}_{bco} + \gamma_c + \delta_o + \varepsilon_{bco}$ where s_{bco} is the share of a (bill) b in accounting code c by office o that is reported in POPS (panels A and B) or that is represented in our anlysis sample (panels C and D); $\mathbf{X}_{bco}$ are quadratic time and bill amount controls, γ_c are accounting code fixed effects, δ_o are procurement office fixed effects, and ε_{bco} is an error term. The panels show kernel density estimates of the distributions of the procurement office fixed effects in the 3 treatment groups and the control group. Panels A and C use bills from year 1 of the experiment, while panels B and D analyze year 2. The panels also show exact P-values form Kolmogorov-Smirnov tests of the equality of each treatment group's distribution and the control group's. The figure shows that the full distributions of office fixed effects–their average attrition rates–are balanced across the treatments.

The rules treatment requires the DDOs and their staff to actively take up the treatment. To measure this we will use the following measures

- *Time taken to release budget relative to previous years*

- *refusal of imprest money*
- *receipt of imprest money*
- *Survey responses on whether the DDO has read the pre-audit checklist. Specifically, responses to survey questions 319–323 in the attached survey.*

The budget release portion of the rules treatment was not implemented. Largely this was due to the government's greater interest in the other parts of the treatment, and partly also to logistical challenges in implementing the treatment. Figure 5 confirms that it was not implemented. Panel A shows how the average share of offices' annual budget evolves over each year in each treatment group. The treatment year (July 2015–June 2016) does not look visibly different from the other years, and any slight differences from other years appear to have affected all four groups in the same way. Panel B shows estimates of the η_{km} coefficients from a differences in differences estimation of

$$s_{ot} = \sum_{k=1}^{3} \sum_{m=Jul}^{Jun} \eta_{km} \text{Treatment}_o^k \times \mathbf{1}\{\text{Month of year} = m\} \times \mathbf{1}\{\text{Fiscal Year 2015–16}\} + \delta_t + \gamma_o + \varepsilon_{ot}$$

where s_{ot} is the share of office o's annual budget that has been released to it by month t, δ_t are month fixed effects, γ_o are office fixed effects and ε_{ot} are residuals. Overlaid on the figure are estimates of difference in difference coefficients of the average effect in the 2015–16 fiscal year in each treatment group.

Table 5 shows our analysis of the takeup of the petty cash. We use our records on which offices accepted to start the process to receive the petty cash (columns 2 and 3) and which offices completed the process and received the petty cash (columns 4 and 5) to measure takeup. 82% of offices in the autonomy treatment, and 76% of offices in the combined treatment accepted petty cash. In the end 72% of offices in the autonomy treatment and 69% of offices in the combined treatment received petty cash. Column 1 shows our baseline specification, the intent to treat regression of unit prices on assignment to treatment and controls. Columns 2 and 4 show OLS regressions of unit prices on treatment takeup and controls. Columns 3 and 5 show IV regressions of unit prices on takeup instrumented by treatment assignment. The table shows clustered standard errors in parentheses and p-Values from randomization inference under the null of no treatment effects in square brackets. As expected, both measures of takeup show much stronger treatment effects in the IV specification focusing on the effect on those who received petty cash.

Table 6 shows our analysis of the takeup of the AG checklist. We use responses in the endline survey on weather respondents had received (columns 2 and 3), read (columns

4 and 5) and used (columns 6 and 7) the checklist to measure takeup. 42% of offices in the autonomy treatment recalled receiving the AG checklist, of whom 95% had read it, and 93% had used it. 34% of offices in the combined treatment recalled receiving the AG checklist, of whom 98% had read it, and 96% had used it. Column 1 shows our baseline specification, the intent to treat regression of unit prices on assignment to treatment and controls. Columns 2, 4 and 6 show OLS regressions of unit prices on treatment takeup and controls. Columns 3, 5 and 7 show IV regressions of unit prices on takeup instrumented by treatment assignment. The table shows clustered standard errors in parentheses and p-Values from randomization inference under the null of no treatment effects in square brackets. As expected, all measures of takeup show much stronger treatment effects in the IV specification focusing on the effect on those who recalled receiving the checklist.

3.2 Understanding & credibility

To measure the DDOs' perceptions of what the treatments entailed we use their responses to survey questions 301–362 in the attached survey. In addition, for the incentives group, we use whether a DDO had sufficient entries in POPS to be ranked in one of the interim performance evaluations as a proxy for the credibility of the treatment.

Figure 6 summarizes responses to our endline survey questions on which treatment respondents had been assigned, and what their group's treatment implied. Panel A shows that most offices knew which treatment they were in. Panels B through D show that they also knew what the treatments entailed. The contrast between the responses of the incentives group in panel B and and their responses in panels C and D suggest that respondents may have misunderstood the questions in panels C and D to mean whether they actually received a bonus rather than whether they were eligible to receive one. Otherwise, all groups seem to have good knowledge of what they were supposed to receive and that they actually did receive it.

Figure 7 summarizes responses to our endline survey questions on the incentives treatment. Panel A shows that participants understood the treatment and panel C that they understood why it was awarded. Panel B shows that most respondents who were awarded an honorarium also remembered receiving it. Finally, panel D shows that respondents believed it motivated them to improve performance.

Figure 8 summarizes responses to our endline questions on the autonomy treatment. Panel A shows that those that wanted the petty cash did tend to receive it. Panel B shows that most people thought that the petty cash would be useful and why. Panel C shows the reasons the few people who thought it wouldn't be useful had for this view. Panel D

shows that many respondents remembered the AG checklist and those that did thought it was useful.

4 Mechanisms

4.1 Ways of achieving better value for money

To measure how *the relationships between DDOs and vendors change in response to the treatments we measure*

- *Whether DDOs change vendors*
- *Whether DDOs achieve better prices from the same vendors*
- *Whether DDOs get more quotes or a wider range of prices quoted to them*
- *Whether DDOs get quotes from a larger set of vendors*

Our POPS data contains some data about the vendors, however this data has been difficult to work with. Many participants were reluctant to enter the vendors' tax IDs, meaning the only way to identify vendors in the data is with their names. However, different offices often use slightly different spellings of the vendors' names, and so merging would be imperfect and extremely time-consuming. We have therefore chosen not to use this data to date. We do, however, have data from the endline survey in which we ask DDOs how they interact with vendors. We analyze this data in section 4.2 below.

4.2 Time use

To measure how *DDOs change their behavior to improve value for money, we gather data on the way they allocate their effort in procurement. In particular, we use their responses to questions 201–209 in the attached survey.*

Figure 9 analyzes questions 201 and 202. Panel A shows the reasons DDOs state for poor procurement performance. The main differences are that all treatment groups are less likely to attribute poor performance to lack of training, and that the incentives group is more likely to state that lack of petty cash with which to make purchases quickly is a barrier to good performance. Panel B shows that the autonomy and incentives groups spend less of their time working on paperwork, and more time working on finding good vendors. Time allocated to ensuring the quality of goods is appropriate and negotiating prices with vendors (rather than switching vendors) is unchanged.

Figure 10 analyzes the responses to question 203. We do not find evidence that the treatments significantly affected the answers to these questions. Figure 11 analyzes the responses to question 204–206. Panel A shows that when allocating time among tasks to reduce the amount paid for goods, the autonomy treatment reduced the amount of time spent on instructing staff, negotiating approvals at the AG and negotiating with vendors. Both treatments seem to have increased the time spent on searching for low price opportunities. Panel B suggests DDOs in all treatments spend less time instructing their staff when working to secure quality goods. Panel C suggests that DDOs in the autonomy treatment focus less on whether vendors provide credit and on whether vendors help get approvals at the AG office when choosing vendors.

Questions 207 and 208 are analyzed in figure 2 discussed in section 2.1 above. Figure 12 analyzes the responses to question 209. Two main findings emerge. First, DDOs in the incentives treatment are more likely to agree that saving public money gives them a sense of accomplishment. This may be a direct effect of the financial rewards they receive for saving money. Second, DDOs in the autonomy treatment seem to think that procurement matters more for their careers, but not because they are punished for poor performance or because their performance is recorded in their annual reviews. This suggests that they see less downside in performing poorly but more upside from performing well.

4.3 Perceptions of reasons for (lack of) improvement

To measure the DDOs' perceptions of how the treatments affected their procurement behavior we use their responses to survey questions 201–209 and 301–362 in the attached survey.

Questions 201–209 are analyzed in figures 9–12, discussed in section 4.2 above. Questions 301–362 are analyzed in figures 6–8 and table 6.

4.4 Traits that might determine response

There are a number of DDO traits that might influence how they respond to the treatments. We will use data on

1. *Tenure as DDO, experience in the Civil service, and position on the pay scale (questions 101–108 of the attached survey)*
2. *Proximity to Accountant General's Office/District Accounts Office*
3. *Proximity to line department's secretariat*
4. *Size of budget allocated*

5. *Proximity to supervisors*

6. *IQ (RAVEN's matrices in section 4 of the attached survey)*

7. *Reason to join civil service (question 109 of the attached survey)*

Table 7 analyzes DDO traits. In column 1 we analyze the DDO's tenure (measured as years since induction to the civil service). Column 2 analyzes the DDO's position on the pay scale. We create a dummy for being high (grade 19 or 20) on the pay scale and interact it with the treatment indicators. Column 3 analyzes the DDO's education. We create a dummy for having a high (Masters of PhD) level of education and interact it with the treatment indicators. Column 4 combines all three DDO traits. The results suggest that the effects of the autonomy treatment are concentrated among DDOs with less tenure, possibly because they are younger and less entrenched in the existing systems of monitoring. Neither pay nor education seem to affect the impact of the treatments.

Table 8 analyzes office traits. In column 1 we analyze the office's distance from its Accountant General office (we use the straight-line distance, measured in 100s of kilometers). In column 2 we analyze the office's distance from its department's secretariat in Lahore (we use the straight-line distance, measured in 100s of kilometers). In column 3 we analyze the share of the office's budget allocated to procurement of gerneric goods. In column 4 we combine all three office traits. The resuls suggest that proximity to the Accountant General or department secretariat aren't associated with different treatment effects. However, as we might expect, offices for whom procurement of generic goods is a larger share of their budgets see larger treatment effects. At the time of surveying, we decided to cut questions 106–114 and the RAVEN's matrices from the survey due to time constraints, so we are unable to look for heterogeneity by question 109 or IQ.

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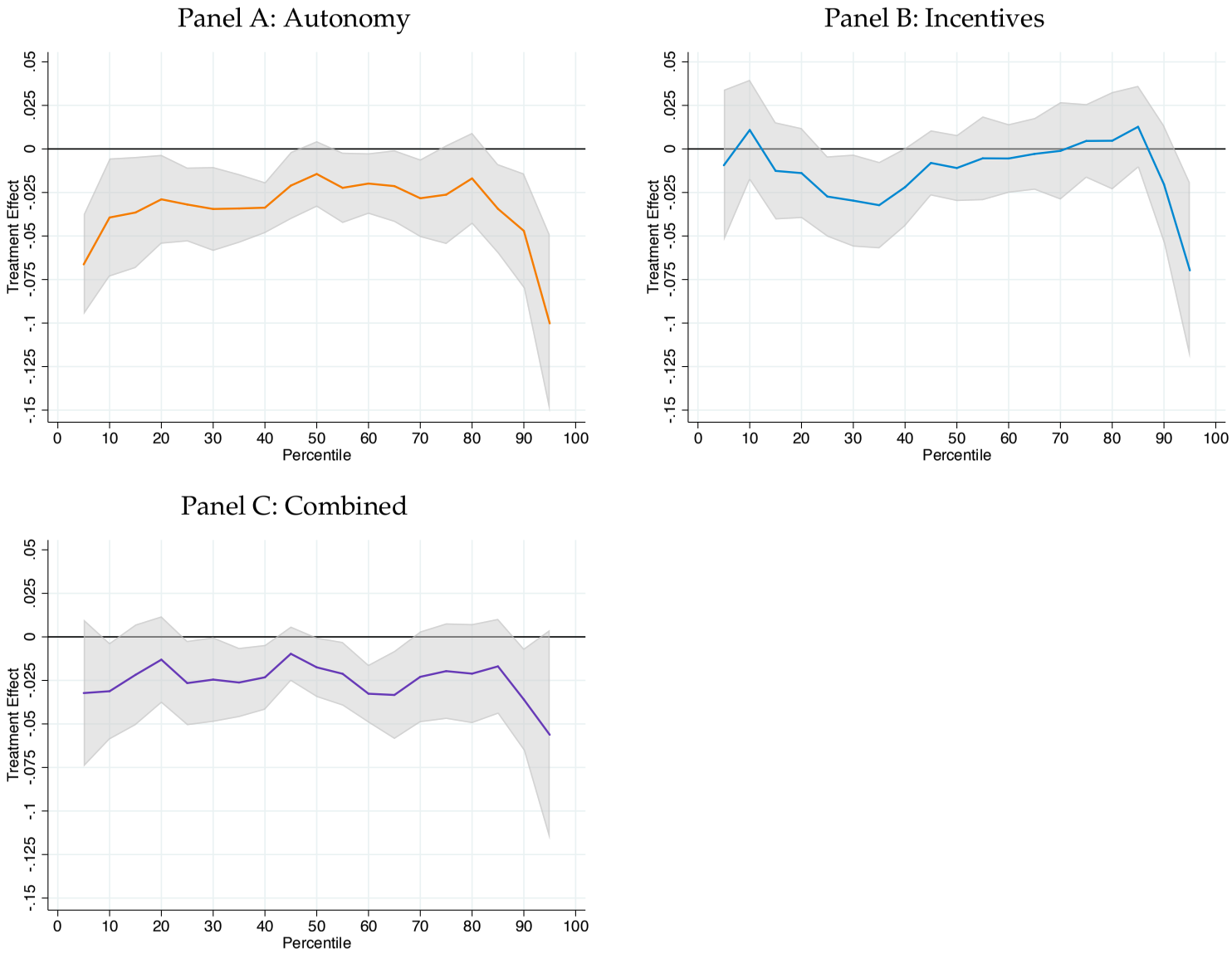
Figures & Tables

FIGURE 1: QUANTILE TREATMENT EFFECTS

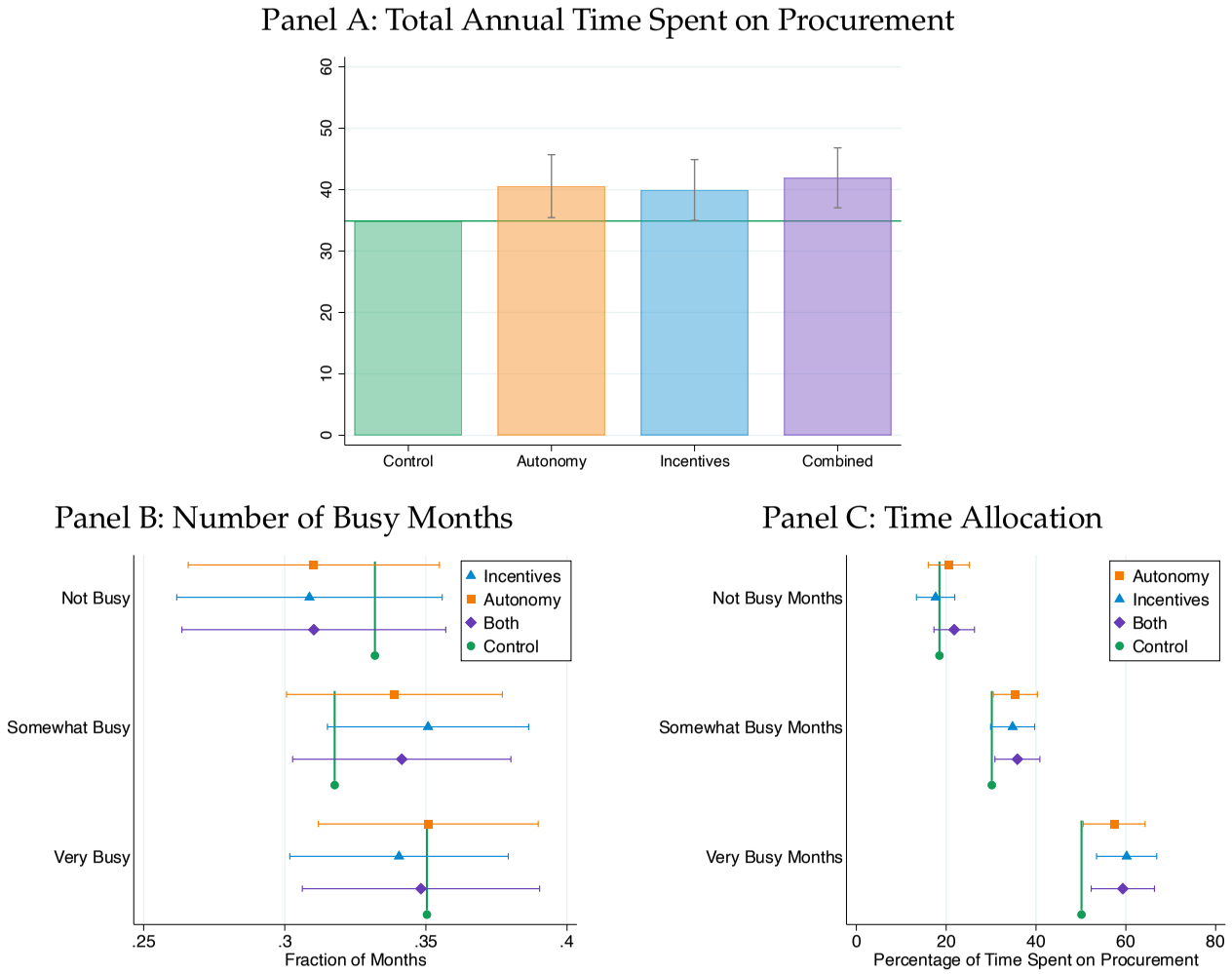


Notes: The figure shows quantile treatment effects of the three treatments on prices paid. We run quantile regressions of the form

$$y_{it} = \alpha + \beta_1 T_{1,it} + \beta_2 T_{2,it} + \beta_3 T_{1,it} \times T_{2,it} + \mathbf{S}_{igt}\delta + \varepsilon_{igt}$$

where y_{it} is a measure of log unit prices paid; $T_{1,it}$ is a dummy indicating that the Drawing and Disbursing Officer (DDO) making purchase i is in the performance incentives treatment at time t; $T_{2,it}$ is a dummy indicating that the DDO is in the rules treatment at time t; $\mathbf{S}_{igt}$ is a vector of stratification variables used in the randomization (department and district) and ε_{igt} is a residual. We estimate treatment effects from the 5th to the 95th percentile, in increments of 5.

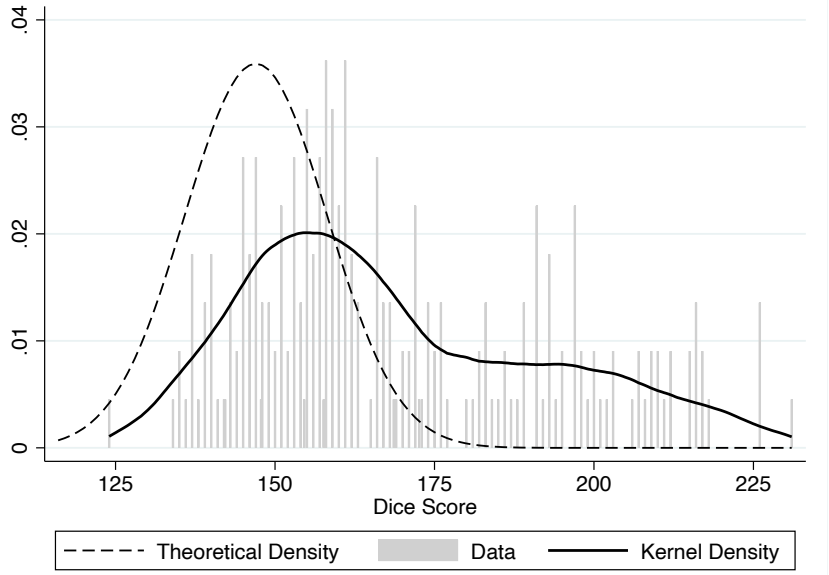
FIGURE 2: TREATMENT EFFECTS ON TIME ALLOCATED TO PROCUREMENT



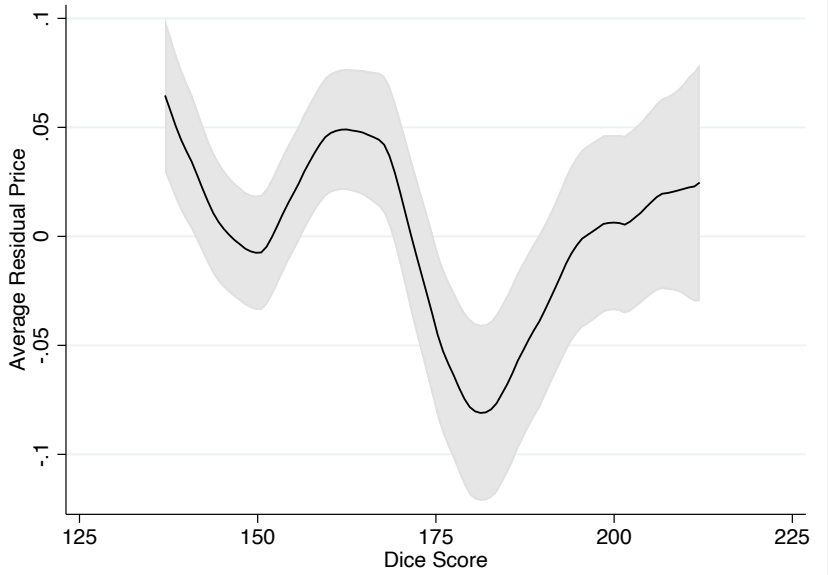
Notes: The figure shows our analysis of the effects of the experiment on the time bureaucrats allocate to procurement. We use questions 207 and 208 in the attached end-line survey. Question 207 asks bureaucrats to allocate months of the year to "very busy", "somewhat busy" and "not busy" months for procurement. Question 208 then asks bureaucrats to specify the fraction of their time in each type of month they spend on procurement. We first combine these into a measure of the total amount of time in the year spent on procurement by averaging the answers to question 208, weighting by the answers to question 207. We find a 14% increase in the total amount of time spent on procurement in the incentives treatment, a 16% increase in the autonomy group, and a 20% increase in the combined group. We cannot reject the hypothesis that the increase is the same in all three groups ($p = 0.70$). Breaking this up, panel B shows that there are no discernible effects on the number of months that are busy,(question 207), and panel C shows that months that are somewhat or very busy are busier (question 208).

FIGURE 3: DICE SCORES AS A PROXY FOR PO TYPE DO NOT PREDICT YEAR 1 PRICES

Panel A: Distribution of Dice Scores

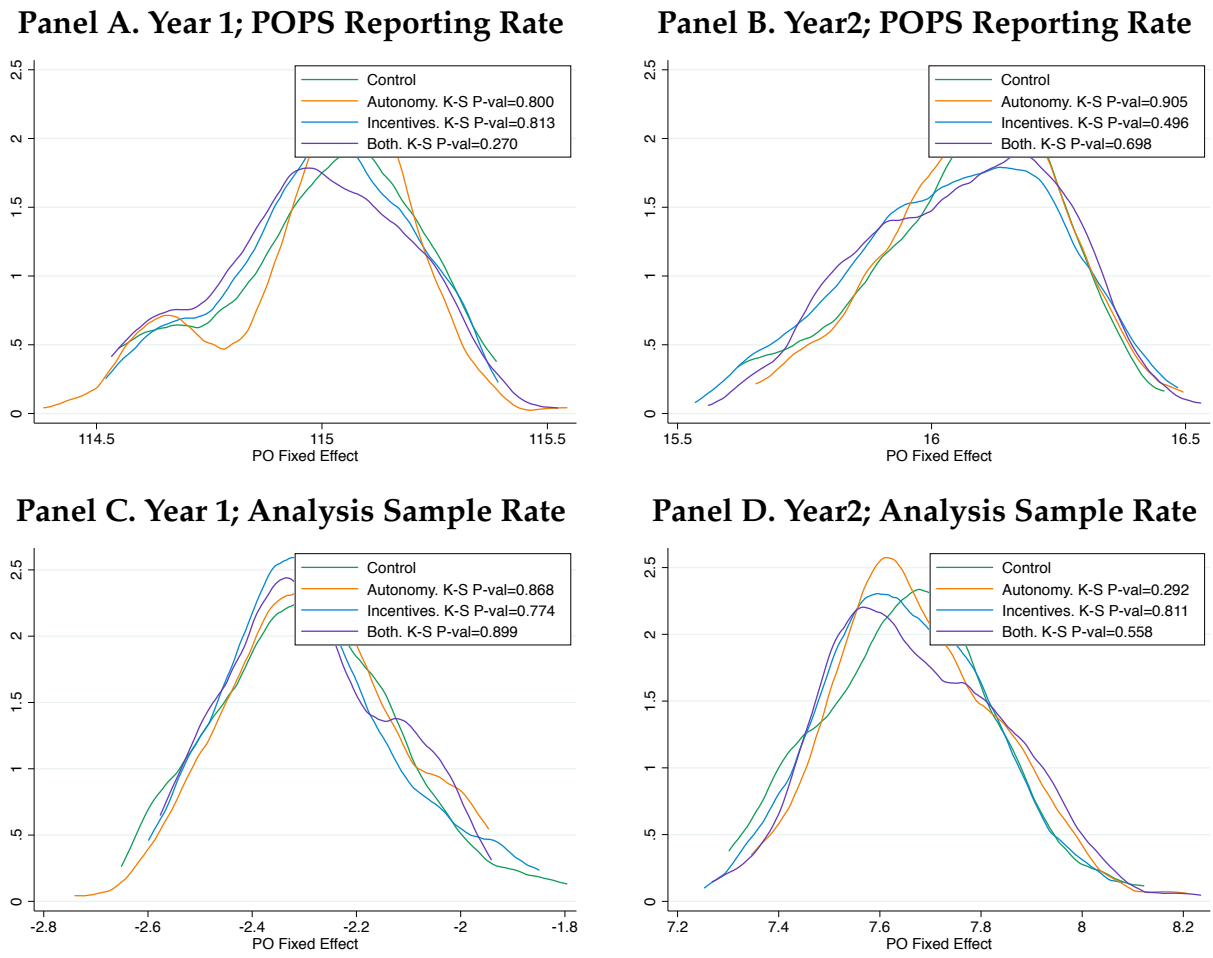


Panel B: Dice Scores do not Predict Year 1 Prices



Notes: The figure shows that the dice scores in the lab in the field measure of dishonesty studied in Fischbacher & Föllmi-Heusi (2013) and Hanna & Wang (2017) are a poor proxy for PO type in our setting. The dice scores come from a game in which subjects privately roll a die 42 times and report each roll. In each roll they are free to report the number either on the top or the bottom of the die. Subjects play against each other and those achieving the highest scores win prizes. The dashed line in panel A shows the theoretical distribution of the total scores if a fair die is rolled 42 times. The histogram and the solid line (kernel density) show the totals achieved by our subjects. Panel B shows a semi-parametric regression of log unit prices in year 1 in the control group and the autonomy group on controls and the dice scores, showing that the dice scores do not predict prices in year 1. Together, the findings in panel A and B suggest that while there is significant variation in the dice scores in our sample, it is not predictive of procurement performance and hence is a poor proxy for PO type in our setting.

FIGURE 4: BALANCE OF THE DISTRIBUTION OF ATTRITION RATES ACROSS OFFICES



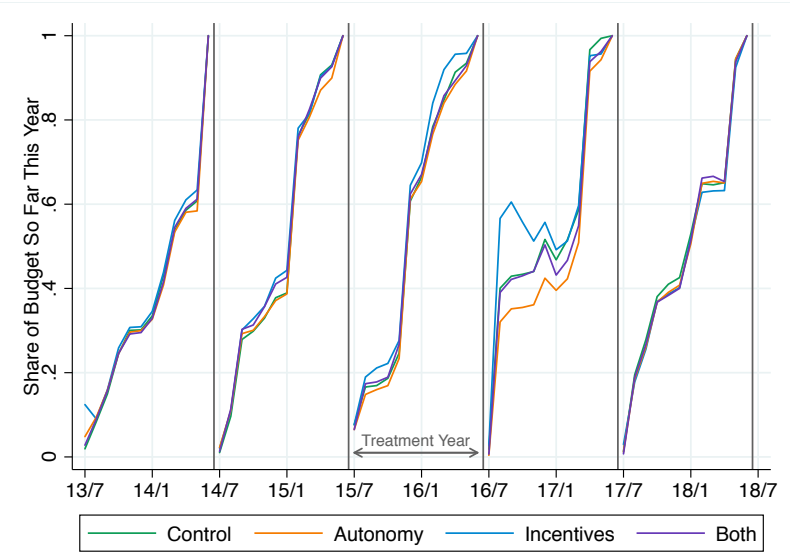
Notes: The figure shows the distribution of procurement office fixed effects δ_o in regressions of the form

$$s_{bco} = \mathbf{X}_{bco}\beta + \gamma_c + \delta_o + \varepsilon_{bco}$$

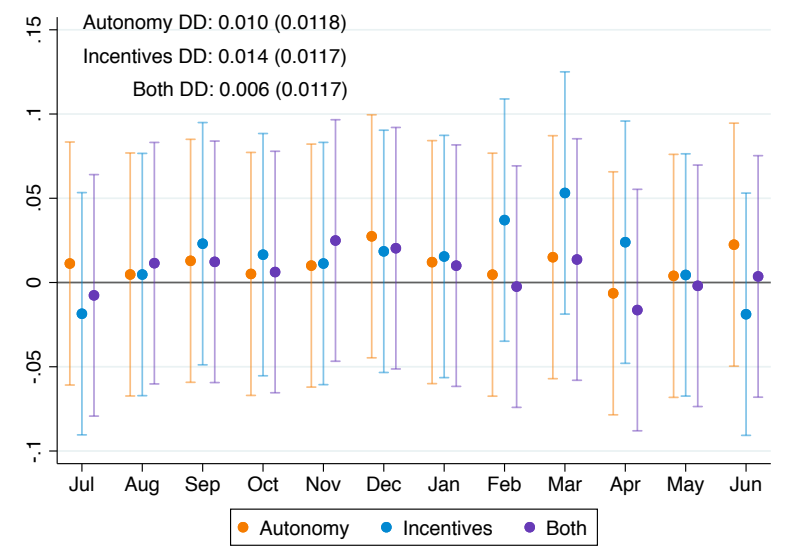
where s_{bco} is the share of a transaction (bill) b by office c in an accounting code o that is reported in POPS (panels A and B) or that is represented in our anlysis sample (panels C and D); $\mathbf{X}_{bco}$ are quadratic time and bill amount controls, γ_c are accounting code fixed effect, δ_o are procurement office fixed effects, and ε_{bco} is an error term. Panels A and C use bills from year 1 of the experiment, while panels B and D analyze year 2. The panels show kernel density estimates of the distributions of the procurement office fixed effects in the 3 treatment groups and the control group. The panels also show exact P-values form Kolmogorov-Smirnov tests of the equality of each treatment group's distribution and the control group's.

FIGURE 5: BUDGET RELEASE TIMING UNAFFECTED

Panel A: Share of Budget Released Over Time



Panel B: Difference in Differences Estimates

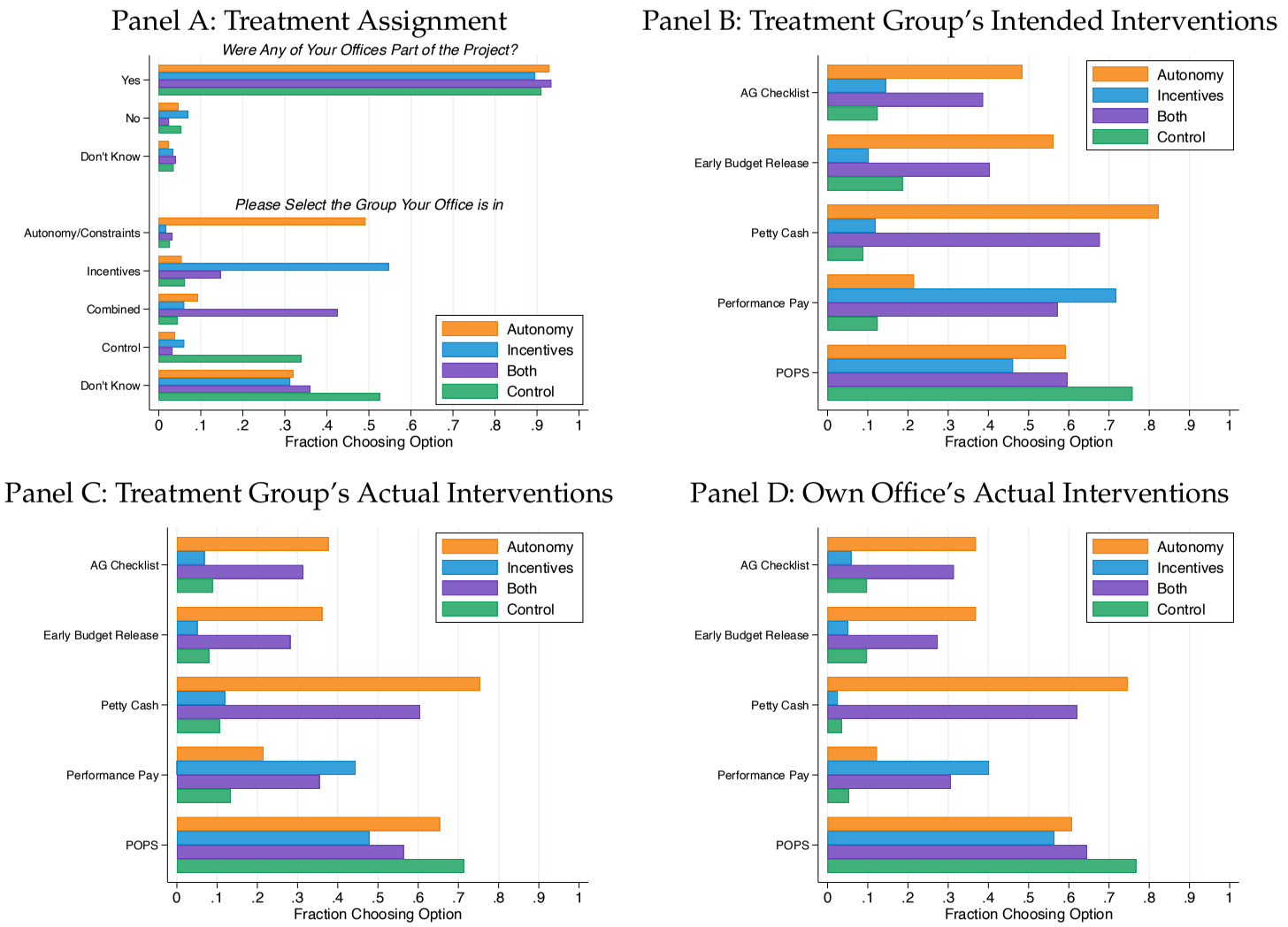


Notes: The figure shows that the timing of budget releases to the offices in the study was unaffected. A third component of the autonomy treatment attmpted to improve the frequency and regularity of budget releases, but it was not possible to implement this. Panel A shows how the average share of offices' annual budget evolves over each year in each treatment group. The treatment year (July 2015–June 2016) does not look visibly different from the other years, and any slight differences from other years appear to have affected all four groups in the same way. Panel B shows estimates of the η_{km} coefficients from a differences in differences estimation of

$$s_{ot} = \sum_{k=1}^{3} \sum_{m=Jul}^{Jun} \eta_{km} \text{Treatment}_o^k \times \mathbf{1}\{\text{Month of year} = m\} \times \mathbf{1}\{\text{Fiscal Year 2015–16}\} + \delta_t + \gamma_o + \varepsilon_{ot}$$

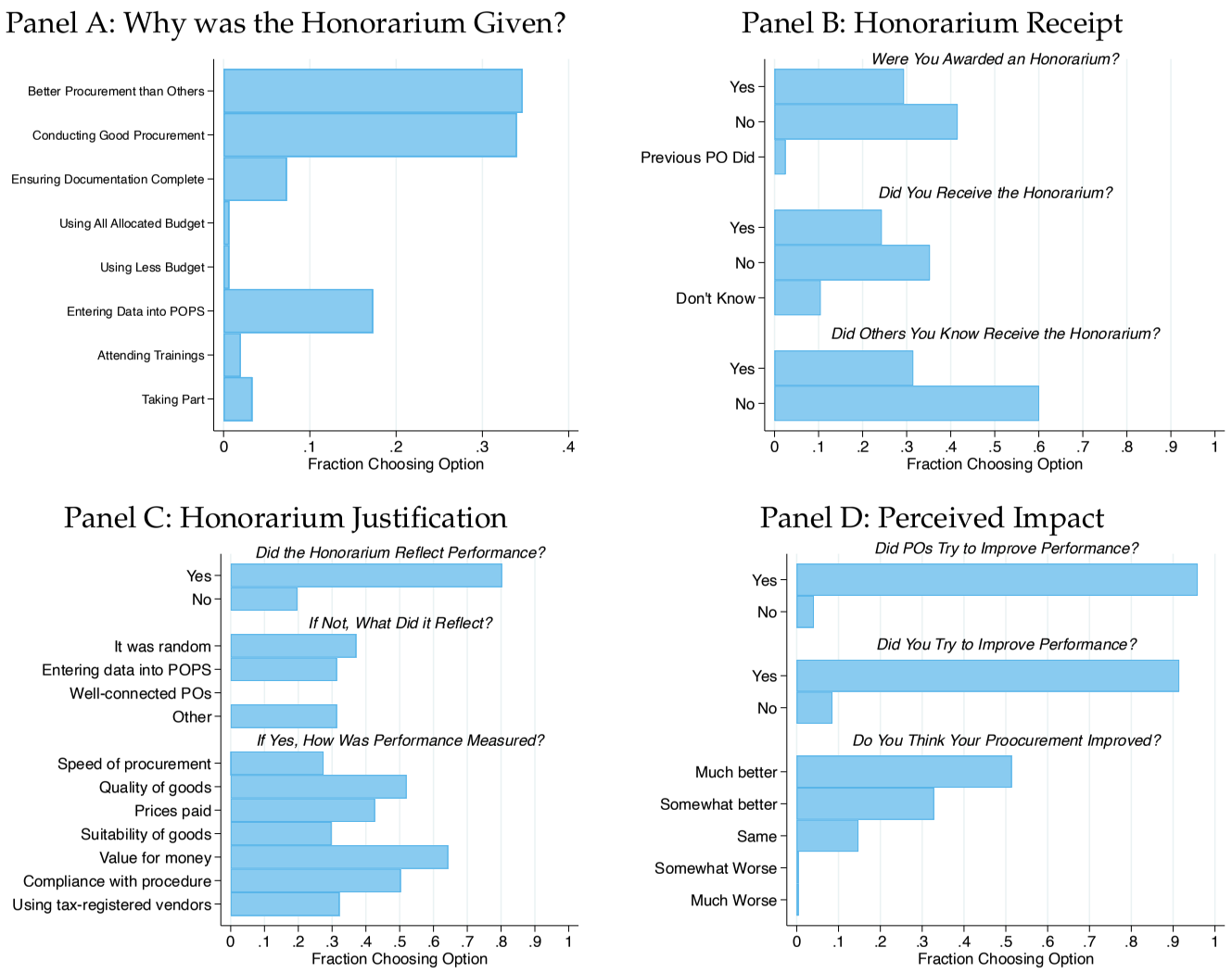
where s_{ot} is the share of office o's annual budget that has been released to it by month t, δ_t are month fixed effects, γ_o are office fixed effects and ε_{ot} are residuals. Overlaid on the figure are estimates of difference in difference coefficients of the average effect in the 2015–16 fiscal year in each treatment group.

FIGURE 6: UNDERSTANDING & CREDIBILITY: TREATMENT ASSIGNMENT



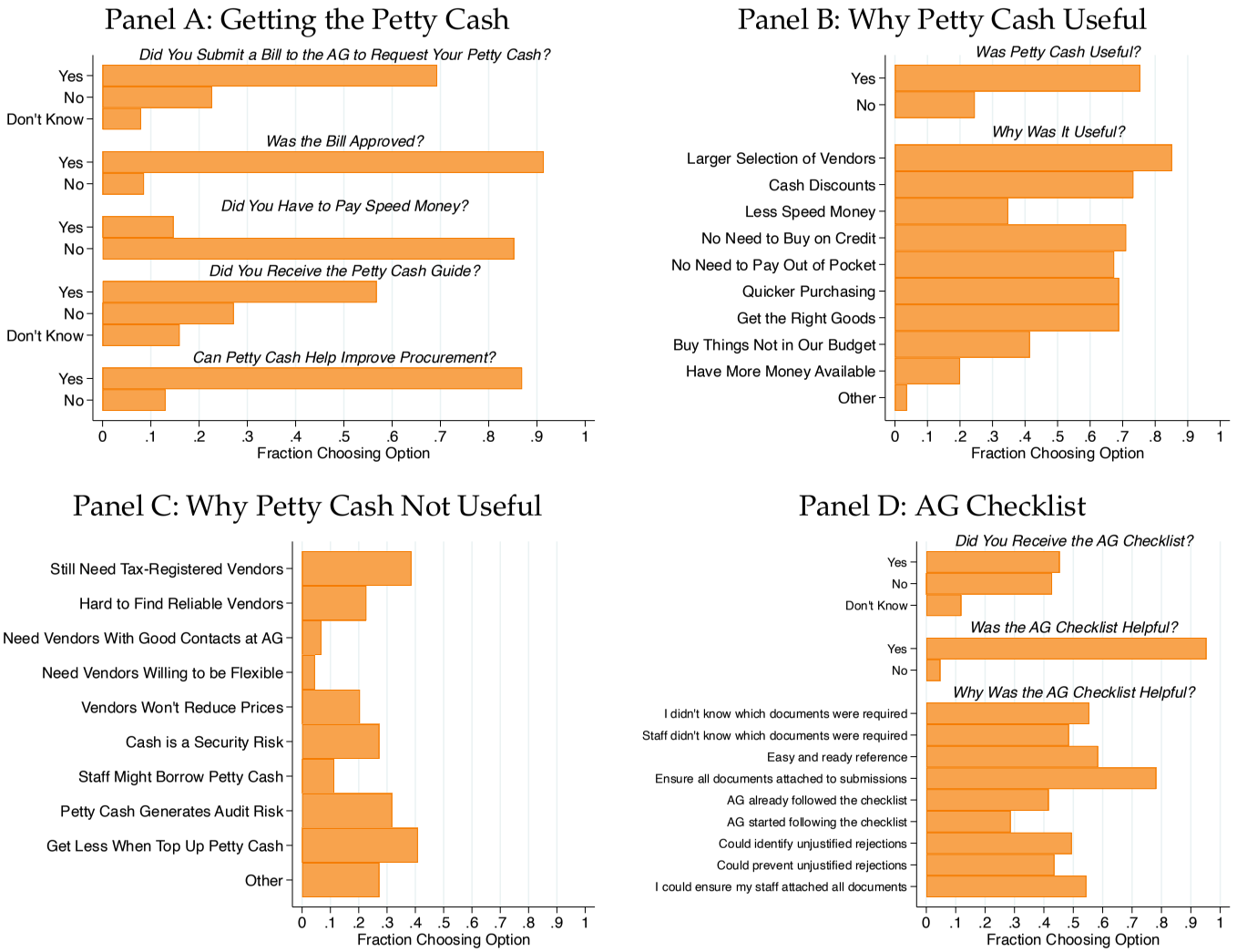
Notes: The figure summarizes responses to our endline survey questions on which treatment respondents had been assigned, and what their group's treatment implied. Panel A shows that most offices knew which treatment they were in. Panels B through D show that they also knew what the treatments entailed. Panel A shows responses to the questions *"Were any of the cost centers you are DDO/Staff of part of the Evidence Based Procurement Reforms project?"* and *"Please select the group your cost center(s) is/are in (select one)."* Panel B shows answers to *"Please select the intervention/s that the cost centers in your group were supposed to receive (select all that you think apply)."* Panel C shows answers to *"Please select the intervention you think the cost centers in your group (not necessarily your cost center) did receive (select all that you think apply)."* Panel D shows answers to *"Please select the interventions your cost center actually did receive (select all that you think apply)."* The possible responses are shortened to fit in the figures. The full text of the responses is in the attached survey questionnaire.

FIGURE 7: UNDERSTANDING & CREDIBILITY: INCENTIVES TREATMENT



Notes: The figure summarizes responses to our endline survey questions on the incentives treatment. The panels show the fraction of respondents picking each option. They show that participants understood the treatment and believed it motivated them to improve performance. Panel A shows responses to *"If your group was in the group for performance based honorarium, why was the honorarium being given?"*. Panel B shows responses to *"Were you awarded a performance honorarium?"; Was the honorarium released by your department?"; and "Did you know that other DDOs received a performance honorarium?"*. Panel C shows responses to *"Do you think that the performance honorarium reflected DDOs' performance on procurement?"; "If you selected no, what do you think it reflected? (select one)"*; and *"If you think that the performance honorarium reflected DDOs' performance on procurement, how do you think the performance was measured? (select all that apply)"*. Panel D shows responses to *"Do you think the honorarium encouraged DDOs to try and improve their performance?"; "Did the prospect of competing for honorarium inspire you to try and conduct better procurement?"* and *"Do you think your procurement improved?"*. The possible responses are shortened to fit in the figures. The full text of the responses is in the attached survey questionnaire.

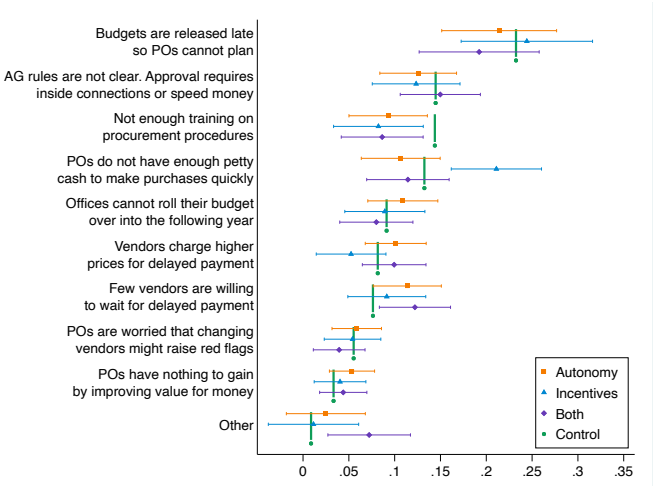
FIGURE 8: UNDERSTANDING & CREDIBILITY: AUTONOMY TREATMENT



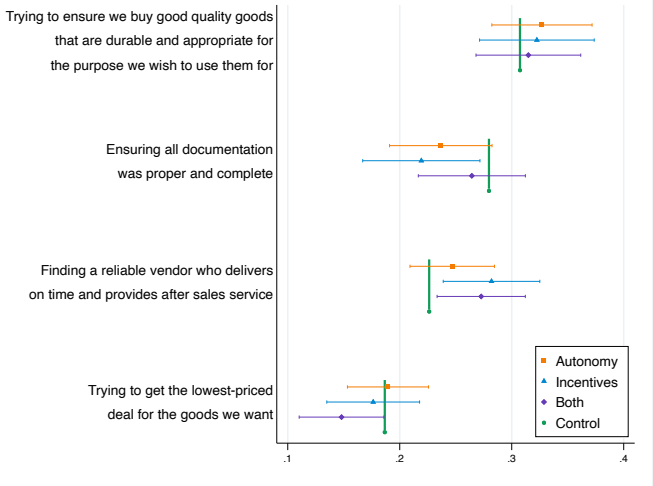
Notes: The figure summarizes responses to our endline survey questions on the incentives treatment. The panels show the fraction of respondents picking each option. They show that participants received the treatments and believed they would improve procurement. Panel A shows responses to the questions *"If your group was slated to receive Imprest Money, did your cost center submit a bill to the A.G. Office or DAO asking for the money to be released?"*; *"If your cost center submitted a bill to the A.G. Office or DAO asking for imprest, was the bill passed?"*; *"Did you, your staff, or staff or DDOs from another cost center get asked to pay speed money at AG/DAO to get your imprest bill passed?"*; *"Did you receive the imprest guide instructing you how to use imprest?"*; and *"Do you think Imprest Cash could be useful in helping conduct better procurement for procurement valued at less than Rs. 100,000?"*. Panel B shows responses to *"If your imprest checque was issued and encashed, did you find it useful in conducting better procurement?"* and *"If you think Imprest Cash was useful for procurement valued at less than R. 100,000, why do you think so? (select all that apply)"*. Panel C shows responses to *"If you think Imprest Cash was not useful for procurement valued at less than Rs. 100,000, why do you think so? (select all that apply)"*. Panel D shows responses to *"Did your cost center receive the AG/DAO checklist?"*; *"Did you or your cost center staff find the checklist helpful or useful?"* and *"If yes, please why: (check all that apply)*. The possible responses are shortened to fit in the figures. The full text of the responses is in the attached survey questionnaire.

FIGURE 9: MECHANISMS ENDLINE SURVEY (1/4)

Panel A: Reasons for Low Value for Money

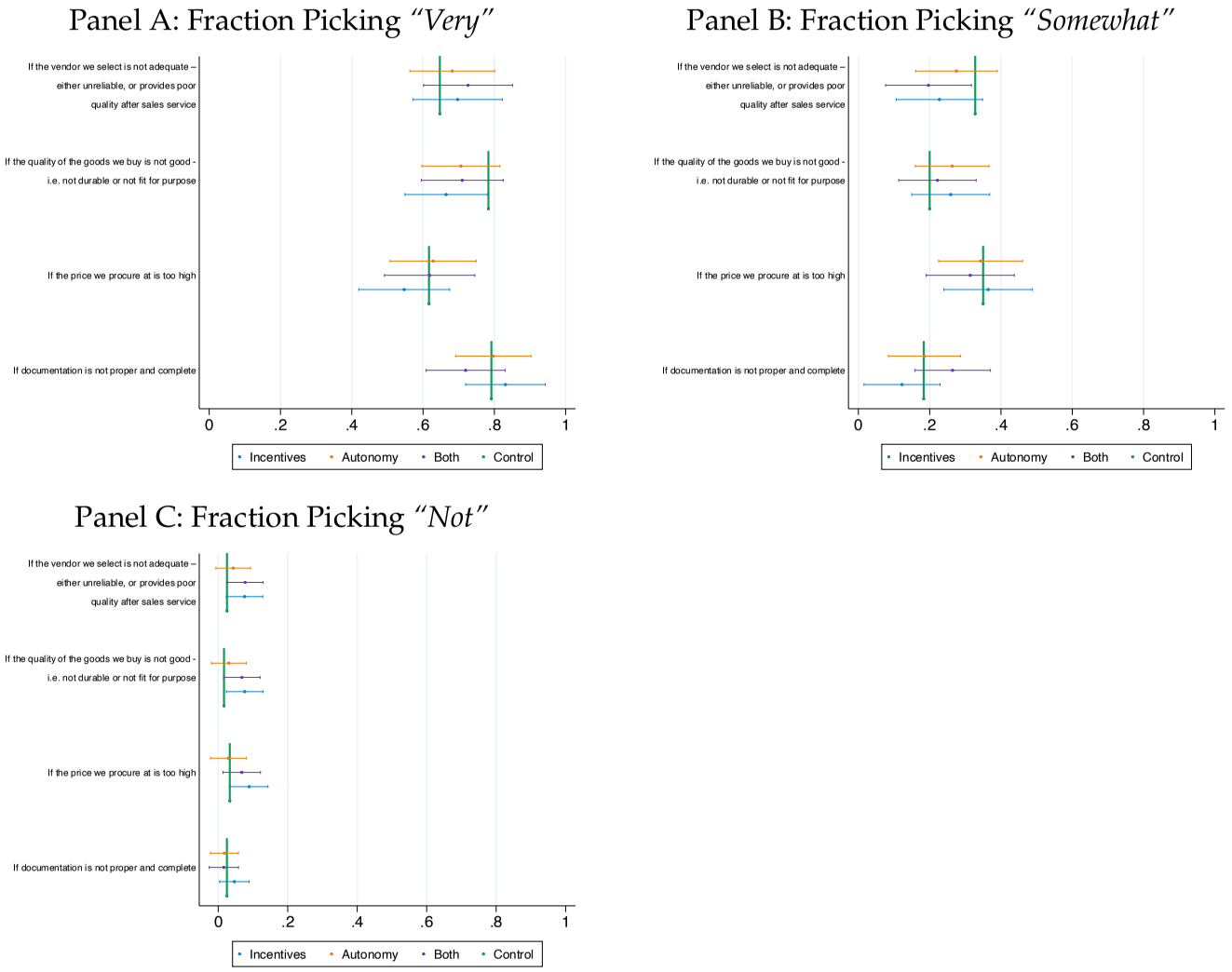


Panel B: Time Allocation Across Procurement Objectives



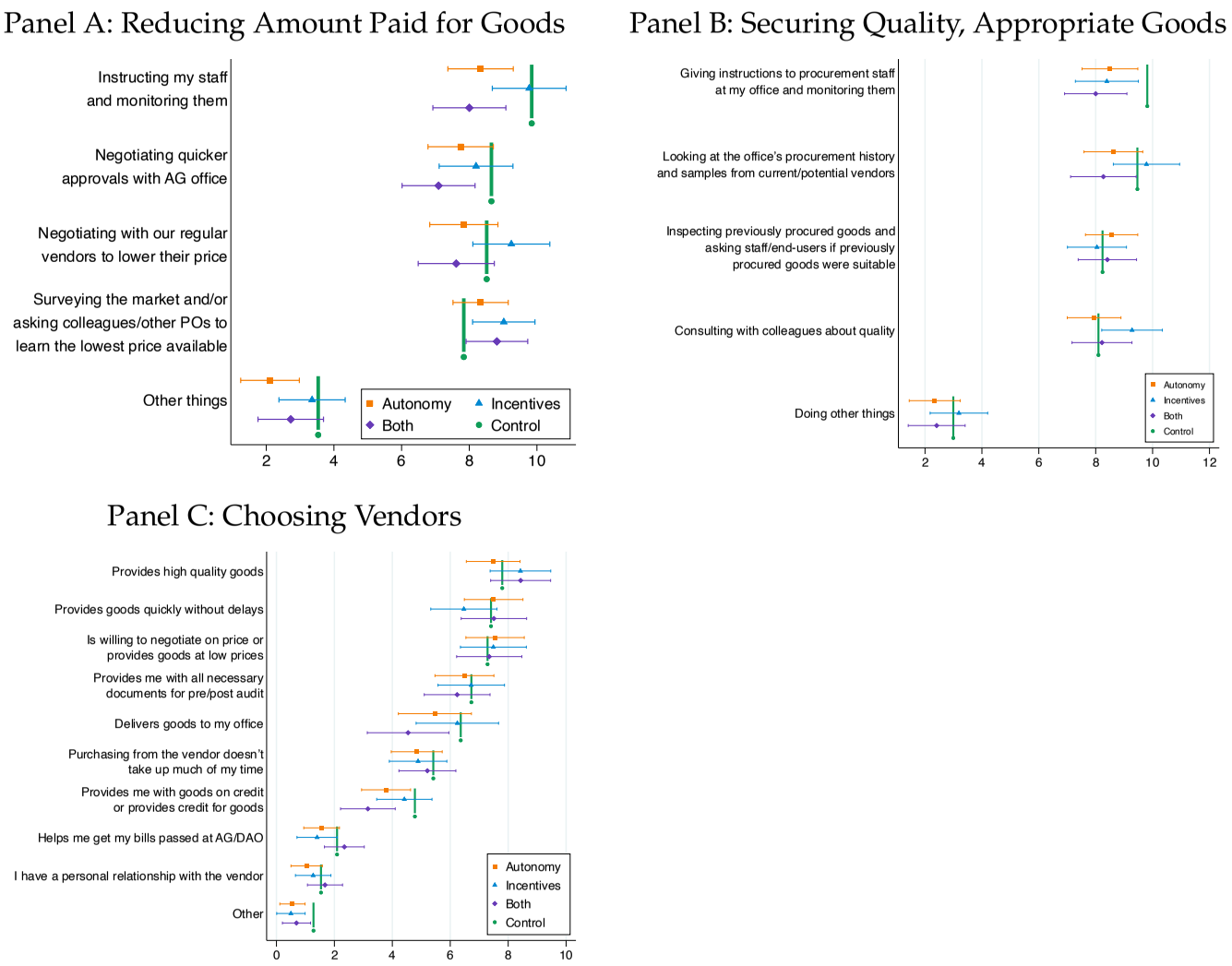
Notes: The figure shows analysis of the responses to our endline survey questions on mechanisms. The panels show differences (and their 95% confidence intervals) in mean responses across the 4 treatment arms, weighting offices by the number of purchases they make. The control group mean is in green, autonomy in orange, incentives in blue, and combined in purple. Panel A shows responses to the question *"These are potential reasons for why DDOs don't achieve good value for money. In your experience how important is each of these?"*. Panel B shows responses to *"Of all the time you and your staff spend trying to do better procurement, what percentage of your time do you spend on each of the objectives below?"* The possible responses are shortened to fit in the figures. The full text of the responses is in the attached survey questionnaire.

FIGURE 10: MECHANISMS ENDLINE SURVEY (2/4)



Notes: The figure shows analysis of the responses to our endline survey question on mechanisms *"Please rate how damaging each of the following could be for your career prospects. (Please tick one box in each option)"*. The panels show differences (and their 95% confidence intervals) in the fractions picking an option across the 4 treatment arms, weighting offices by the number of purchases they make. The control group mean is in green, autonomy in orange, incentives in blue, and combined in purple. Panel A shows the fraction picking *"very damaging"*, panel B shows the fraction picking *"somewhat damaging"*, and panel C the fraction picking *"not damaging"*. The possible responses are shortened to fit in the figures. The full text of the responses is in the attached survey questionnaire.

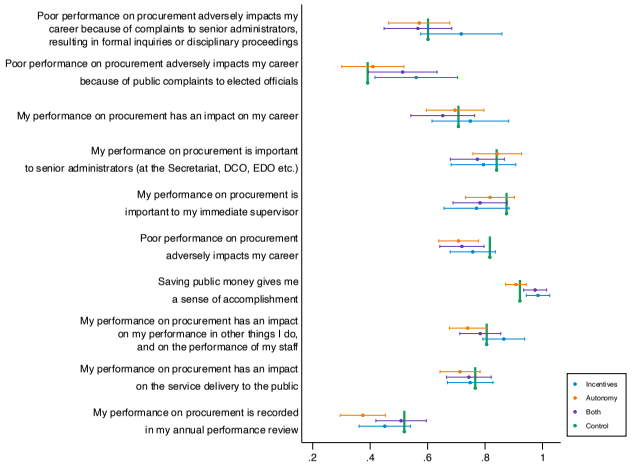
FIGURE 11: MECHANISMS ENDLINE SURVEY (3/4)



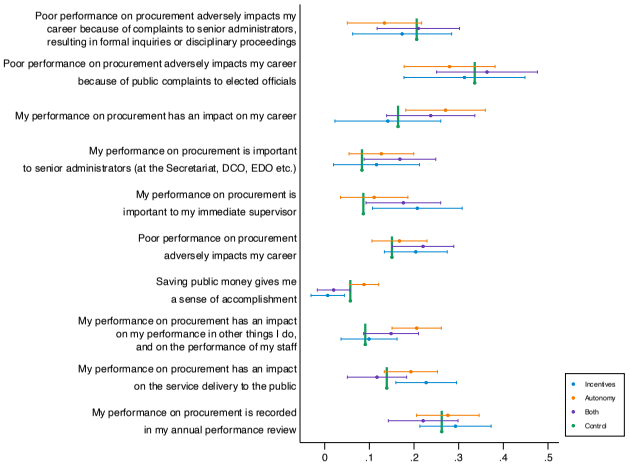
Notes: The figure shows analysis of the responses to our endline survey questions on mechanisms. The panels show differences (and their 95% confidence intervals) in mean responses across the 4 treatment arms, weighting offices by the number of purchases they make. The control group mean is in green, autonomy in orange, incentives in blue, and combined in purple. Panel A shows responses to the question *"Of all the time you spend trying to reduce the total amount your cost center pays (including hidden costs) for the goods you want, what percentage of your time do you and your staff spend on each of the tasks below?"* Panel B shows responses to *"Of all the time you and your staff spend trying to buy good quality, appropriate, and durable goods, what percentage of your time do you and your staff spend on each of the tasks below?"* Panel C shows responses to *"Please think about the vendors you currently make contingent purchases from, and the vendors you could potentially make contingent purchases from. Which of the following characteristics of vendors are important to you in deciding which vendor(s) to buy from?"* The possible responses are shortened to fit in the figures. The full text of the responses is in the attached survey questionnaire. Answers may not sum to 100 since respondents seem in many cases to have interpreted the questions to mean percentage of total time rather than percentage of time spent *on procurement*.

FIGURE 12: MECHANISMS ENDLINE SURVEY (4/4)

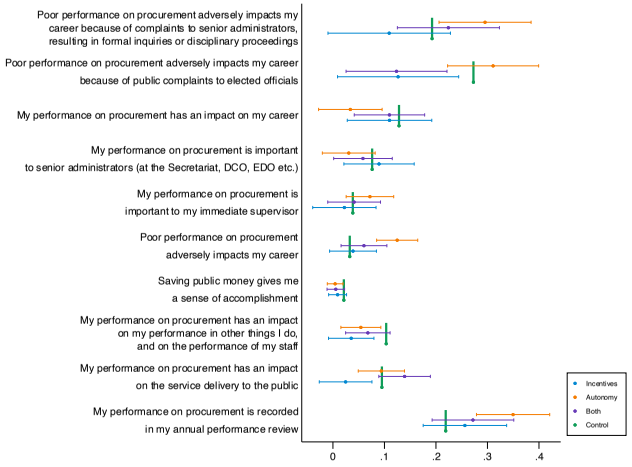
Panel A: Fraction Picking *"Agree"*



Panel B: Fraction Picking *"Neither"*



Panel C: Fraction Picking *"Disagree"*



Notes: The figure shows analysis of the responses to our endline survey question on mechanisms *"Do you agree or disagree with the following statements?"*. The panels show differences (and their 95% confidence intervals) in the fractions picking an option across the 4 treatment arms, weighting offices by the number of purchases they make. The control group mean is in green, autonomy in orange, incentives in blue, and combined in purple. Panel A shows the fraction picking *"agree"*, panel B shows the fraction picking *"neither agree nor disagree"*, and panel C the fraction picking *"disagree"*. The possible responses are shortened to fit in the figures. The full text of the responses is in the attached survey questionnaire.

TABLE 1: MAIN OUTCOME: UNIT PRICES

	Variety on rhs		Residuals on lhs	
	(1)	(2)	(3)	(4)
Autonomy	-0.080	-0.080	-0.075	-0.078
	(0.032)	(0.031)	(0.029)	(0.029)
	[0.030]	[0.023]	[0.021]	[0.015]
Incentives	-0.018	-0.022	-0.019	-0.024
	(0.034)	(0.033)	(0.034)	(0.032)
	[0.651]	[0.571]	[0.618]	[0.528]
Both	-0.073	-0.072	-0.077	-0.072
	(0.035)	(0.033)	(0.032)	(0.032)
	[0.058]	[0.053]	[0.039]	[0.045]
Item Variety Control	PAP	Preferred	PAP	Preferred
p(All = 0)	0.099	0.093	0.079	0.075
p(Autonomy = Incentives)	0.133	0.119	0.170	0.128
p(Autonomy = Both)	0.860	0.807	0.970	0.840
p(Incentives = Both)	0.205	0.227	0.199	0.231
Observations	11,771	11,771	11,771	11,771

Notes: The table shows our analysis of the effects of the experiment on prices paid. The table shows the results from regressions of the form

$$y_{it} = \alpha + \beta_1 T_{1,it} + \beta_2 T_{2,it} + \beta_3 T_{1,it} \times T_{2,it} + \mathbf{S}_{igt}\delta + \varepsilon_{igt}$$

where y_{it} is a measure of log unit prices paid; $T_{1,it}$ is a dummy indicating that the Drawing and Disbursing Officer (DDO) making purchase i is in the performance incentives treatment at time t; $T_{2,it}$ is a dummy indicating that the DDO is in the rules treatment at time t; $\mathbf{S}_{igt}$ is a vector of stratification variables used in the randomization (department and district) and ε_{igt} is a residual. We us four ways of constructing our otcome measure. We run regressions of log unit prices on item variety controls of the form

$$p_{igt} = \mathbf{X}_{igt}\beta + \gamma_t + \text{department}_i + \text{district}_i + \mu_{igt}$$

where p_{igt} is the log of the unit price paid in transaction i for good g in month t; $\mathbf{X}_{igt}$ is a vector of observables including the log quantity purchased and all the good's attributes; γ_t are month fixed effects, department$_i$ are department fixed effects; district$_i$ are district fixed effects. We either use this specification (columns 1 and 3) or one where we drop the department and district fixed effects and instead control for department $\times$ district in the main regressions (columns 2 and 4). We either use the residuals from this regression (columns 3 and 4) as the dependent variable, or the fitted values as controls (columns 1 and 2). The table shows clustered standard errors in parentheses and p-Values from randomization inference under the null of no treatment effects in square brackets. Our preferred specification is in column 2. All specifications show the same results.

TABLE 2: MAIN OUTCOME: BUDGET UTILIZATION

	(1) Operating Expenses	(2) Physical Assets	(3) Repairs & Maintenance	(4) Full Budget	(5) POPS Universe	(6) Analysis Sample
Autonomy	-0.004	0.001	-0.033	0.016	-0.004	-0.015
	(0.013)	(0.013)	(0.026)	(0.013)	(0.016)	(0.017)
Incentives	0.009	0.001	-0.015	0.018	0.012	0.004
	(0.013)	(0.014)	(0.026)	(0.013)	(0.016)	(0.018)
Combined	-0.007	0.006	-0.066	0.000	-0.004	-0.024
	(0.013)	(0.013)	(0.026)	(0.013)	(0.016)	(0.017)
Control Mean	0.862	0.912	0.907	0.858	0.880	0.909
Observations	578	70	571	580	580	580

Notes: The table shows our analysis of the effects of the experiment on budget utilization. We use administrative data from the finance department on budgets and expenditures in each accounting category. We aggregate the data up to the three main categories that include procurement spending: Operating Expenses, Physical Assets, and Repairs & Maintenance. We also look at the office's full budget, budget on items covered by POPS and budget for items that appear in our analysis dataset. For each of these we measure budget utilization as the ratio of the amount spent to the amount in the office's budget for that category. We regress this on treatment dummies, randomization stratum fixed effects and a control for the number of accounting entities in each office. We do not see any evidence of an effect of the experiment on any of these outcomes, consistent with the demand for spending in these categories being relatively inelastic.

TABLE 3: HETEROGENEITY OF TREATMENT EFFECTS BY PROCUREMENT OFFICER DICE SCORE

	(1)	(2)	(3)	(4)
Autonomy	0.2791	0.4386	0.3442	0.4123
	(0.2820)	(0.2396)	(0.2317)	(0.2589)
	[0.396]	[0.134]	[0.213]	[0.180]
Incentives	-0.0413	0.2079	0.0963	0.1967
	(0.3089)	(0.2457)	(0.2574)	(0.2774)
	[0.915]	[0.505]	[0.770]	[0.579]
Both	-0.0431	0.2665	0.1409	0.1225
	(0.4106)	(0.3199)	(0.3319)	(0.3965)
	[0.915]	[0.504]	[0.717]	[0.797]
Autonomy × Dice Score	-0.0023	-0.0033	-0.0026	-0.0030
	(0.0017)	(0.0015)	(0.0014)	(0.0016)
	[0.249]	[0.071]	[0.122]	[0.112]
Incentives × Dice Score	0.0001	-0.0015	-0.0007	-0.0013
	(0.0019)	(0.0015)	(0.0016)	(0.0017)
	[0.954]	[0.426]	[0.698]	[0.541]
Both × Dice Score	-0.0003	-0.0022	-0.0013	-0.0013
	(0.0025)	(0.0019)	(0.0020)	(0.0024)
	[0.918]	[0.336]	[0.579]	[0.648]
Item Variety Control	None	Attribs	Scalar	Coarse
p(All Interactions = 0)	0.167	0.056	0.156	0.132
Observations	10,283	10,283	10,283	10,283

Notes: The table shows heterogeneity of treatment effects by the degree of misalignment of the procurement officer, as measured by their score in the dice game measure of dishonesty studied in Fischbacher & Föllmi-Heusi (2013) and Hanna & Wang (2017) and summarized in appendix figure 3. We estimate treatment effect heterogeneity by interacting our proxy for PO type $\hat{\mu}_o$ with treatment dummies $p_{igto} = \alpha + \eta \text{Autonomy}_o + \zeta \text{Autonomy}_o \times \hat{\mu}_o + \mathbf{X}_{igto}\beta + \rho_g q_{igto} + \delta_s + \gamma_g + \varepsilon_{igto}$.

TABLE 4: BALANCE OF ATTRITION OF ITEMS

	All Generics				Analysis Objects			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Incentives	0.006	0.003	-0.003	0.005	0.009	0.005	-0.002	0.006
	(0.015)	(0.017)	(0.013)	(0.012)	(0.018)	(0.020)	(0.015)	(0.015)
Autonomy	-0.011	-0.009	-0.009	-0.003	-0.010	0.000	-0.008	-0.001
	(0.016)	(0.016)	(0.013)	(0.012)	(0.018)	(0.019)	(0.015)	(0.015)
Both	-0.038*	-0.013	-0.017	-0.001	-0.041*	-0.013	-0.020	-0.002
	(0.018)	(0.018)	(0.014)	(0.013)	(0.020)	(0.020)	(0.016)	(0.017)
Assets: Fertilizer	0.000	0.000	0.000	0.000				
	(.)	(.)	(.)	(.)				
Assets: General Utility Chemicals	-0.061	-0.108*	0.019	-0.014				
	(0.053)	(0.053)	(0.022)	(0.019)				
Assets: Insecticides	0.111	-0.174***	-0.019**	-0.011				
	(0.067)	(0.049)	(0.007)	(0.006)				
Assets: Lab Equipment	-0.263***	-0.422***	0.069**	0.066*				
	(0.055)	(0.046)	(0.026)	(0.029)				
Assets: Other Commodity	0.073	-0.053	-0.019	-0.020*	0.000	0.000	0.000	0.000
	(0.093)	(0.068)	(0.012)	(0.009)	(.)	(.)	(.)	(.)
Assets: Other Stocks and Stores	-0.068	-0.188	0.044	0.009				
	(0.138)	(0.150)	(0.036)	(0.015)				
Assets: Purchase of Furniture & Fixture	-0.108	-0.248***	0.047*	0.104***	-0.167	-0.132	0.081***	0.168***
	(0.067)	(0.066)	(0.019)	(0.021)	(0.114)	(0.097)	(0.020)	(0.031)
Assets: Purchase of Plant & Machinery	-0.273***	-0.420***	0.079***	0.039	-0.301**	-0.341***	0.122***	0.078**

	(0.071)	(0.079)	(0.021)	(0.025)	(0.111)	(0.094)	(0.022)	(0.027)
Assets: Purchase of Transport	-0.288***	-0.442***	0.032	0.087***				
	(0.061)	(0.051)	(0.029)	(0.020)				
Assets: Specific Utility Chemicals	-0.055	-0.282***	0.008	0.037**	-0.120	-0.199*	0.031	0.077**
	(0.084)	(0.073)	(0.010)	(0.012)	(0.123)	(0.092)	(0.017)	(0.024)
OpEx: Advertising	-0.124*	-0.314***	0.217***	0.238***	-0.203	-0.266***	0.232***	0.254***
	(0.058)	(0.046)	(0.023)	(0.023)	(0.105)	(0.073)	(0.026)	(0.025)
OpEx: Courier	-0.455***	-0.735***	-0.055	-0.139**				
	(0.090)	(0.062)	(0.049)	(0.042)				
OpEx: Electricity	0.138*	-0.135**	0.495***	0.437***	0.055	-0.090	0.506***	0.450***
	(0.061)	(0.046)	(0.027)	(0.025)	(0.105)	(0.073)	(0.027)	(0.025)
OpEx: Elextronic Communication	-0.382***	-0.678***	-0.000	-0.088*				
	(0.092)	(0.101)	(0.037)	(0.039)				
OpEx: Medicines	-0.196***	-0.422***	0.134***	0.119***				
	(0.055)	(0.045)	(0.014)	(0.015)				
OpEx: Newspapers	0.147*	-0.156***	0.289***	0.309***	0.070	-0.107	0.301***	0.324***
	(0.064)	(0.046)	(0.022)	(0.024)	(0.107)	(0.073)	(0.022)	(0.024)
OpEx: Other	0.009	-0.256***	0.197***	0.177***	-0.065	-0.209**	0.214***	0.194***
	(0.055)	(0.043)	(0.015)	(0.016)	(0.105)	(0.072)	(0.018)	(0.018)
OpEx: Other Stores	-0.148**	-0.366***	0.070***	0.058***	-0.212*	-0.310***	0.093***	0.080***
	(0.055)	(0.043)	(0.015)	(0.013)	(0.104)	(0.072)	(0.016)	(0.015)
OpEx: Other Stores: Computer/Stationery	0.090	-0.167**	0.367***	0.371***	0.014	-0.118	0.385***	0.388***
	(0.070)	(0.061)	(0.050)	(0.048)	(0.112)	(0.084)	(0.049)	(0.047)
OpEx: Other Utilities	-0.245***	-0.420***	0.071*	0.137	-0.339**	0.123	0.066**	0.590***
	(0.058)	(0.103)	(0.033)	(0.082)	(0.104)	(0.110)	(0.025)	(0.133)

OpEx: Payments for Services	-0.298***	-0.574***	0.058***	-0.009				
	(0.054)	(0.043)	(0.015)	(0.015)				
OpEx: Printing	-0.044	-0.270***	0.173***	0.125***	-0.120	-0.219**	0.190***	0.143***
	(0.054)	(0.045)	(0.016)	(0.019)	(0.104)	(0.073)	(0.019)	(0.020)
OpEx: Rent not on Building	-0.437***	-0.604***	0.003	0.020				
	(0.064)	(0.069)	(0.021)	(0.024)				
OpEx: Rent of Machine	-0.443***	-0.625***	-0.007	0.023				
	(0.065)	(0.069)	(0.021)	(0.023)				
OpEx: Stationery	0.076	-0.138**	0.352***	0.372***	0.002	-0.091	0.369***	0.389***
	(0.056)	(0.042)	(0.018)	(0.015)	(0.104)	(0.072)	(0.019)	(0.020)
Repairs: Computer Hardware	-0.155*	-0.304***	0.107**	0.116**	-0.237	-0.249*	0.124**	0.136**
	(0.079)	(0.086)	(0.041)	(0.045)	(0.121)	(0.100)	(0.041)	(0.045)
Repairs: Computer Software	-0.328***	-0.538***	0.042	-0.019				
	(0.058)	(0.088)	(0.021)	(0.017)				
Repairs: Furniture & Fixtures	-0.380***	-0.651***	-0.006	-0.077***	-0.459***	-0.606***	0.009	-0.063***
	(0.055)	(0.043)	(0.015)	(0.015)	(0.103)	(0.072)	(0.015)	(0.016)
Repairs: IT Equipment	-0.220	-0.053	0.085	0.199***	-0.290	0.018	0.103	0.230***
	(0.123)	(0.167)	(0.066)	(0.040)	(0.153)	(0.170)	(0.068)	(0.040)
Repairs: Machinery & Equipment	-0.321***	-0.569***	0.020	-0.026	-0.399***	-0.521***	0.035*	-0.009
	(0.055)	(0.044)	(0.016)	(0.015)	(0.104)	(0.072)	(0.016)	(0.016)
Repairs: Other Building	-0.142**	-0.485***	0.150***	0.058*				
	(0.053)	(0.052)	(0.012)	(0.026)				
Date	-0.007	-0.001***	0.004	-0.000***	-0.005	-0.001***	0.006	-0.000***
	(0.006)	(0.000)	(0.006)	(0.000)	(0.007)	(0.000)	(0.007)	(0.000)
$Date^2$	0.000	0.000***	-0.000	0.000***	0.000	0.000***	-0.000	0.000***

	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
log Amount	-0.121***	-0.132***	-0.108***	-0.144***	-0.082**	-0.095**	-0.101***	-0.128***
	(0.028)	(0.020)	(0.023)	(0.024)	(0.027)	(0.032)	(0.025)	(0.031)
log(Amount)^2	0.004***	0.005***	0.004***	0.005***	0.002	0.002	0.004**	0.004**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
Assets: Generic Consumables		-0.400***		0.131***				
		(0.051)		(0.019)				
Constant	69.447	13.868***	-41.798	6.610***	47.408	15.965***	-60.546	7.598***
	(61.980)	(1.333)	(63.492)	(0.944)	(69.733)	(1.531)	(66.118)	(1.075)
Observations	23,423	22,498	23,423	22,498	17,361	16,553	17,361	16,553
R^2	0.33	0.33	0.28	0.32	0.25	0.24	0.24	0.27
Year	Year 1	Year 2	Year 1	Year 2	Year 1	Year 2	Year 1	Year 2
Reporting Share	POPS	POPS	Analysis	Analysis	POPS	POPS	Analysis	Analysis

TABLE 5: TAKEUP: PETTY CASH

		Accepted Petty Cash		Received Petty Cash	
	(1) ITT	(2) OLS	(3) IV	(4) OLS	(5) IV
Autonomy	-0.080	-0.080	-0.111	-0.087	-0.120
	(0.031)	(0.033)	(0.042)	(0.033)	(0.045)
	[0.023]	[0.025]	[0.022]	[0.019]	[0.020]
Incentives	-0.022	-0.008	-0.021	-0.005	-0.020
	(0.033)	(0.032)	(0.032)	(0.031)	(0.032)
	[0.571]	[0.831]	[0.579]	[0.879]	[0.603]
Both	-0.072	-0.076	-0.095	-0.074	-0.100
	(0.033)	(0.037)	(0.044)	(0.038)	(0.047)
	[0.053]	[0.061]	[0.051]	[0.086]	[0.053]
p(All = 0)	0.092	0.085	0.093	0.066	0.090
Observations	11,771	11,771	11,771	11,771	11,771

Notes: The table shows our analysis of the effects of the takeup of the petty cash component of the autonomy treatment. We use our records on which offices accepted to start the process to receive the petty cash (columns 2 and 3) and which offices completed the process and received the petty cash (columns 4 and 5) to measure takeup. Column 1 shows our baseline specification, the intent to treat regression of unit prices on assignment to treatment and controls. Columns 2 and 4 show OLS regressions of unit prices on treatment takeup and controls. Columns 3 and 5 show IV regressions of unit prices on takeup instrumented by treatment assignment. The table shows clustered standard errors in parentheses and p-Values from randomization inference under the null of no treatment effects in square brackets.

TABLE 6: TAKEUP: AG CHECKLIST

		Received Checklist		Read Checklist		Used Checklist	
	(1) ITT	(2) OLS	(3) IV	(4) OLS	(5) IV	(6) OLS	(7) IV
Autonomy	-0.083	-0.035	-0.210	-0.045	-0.228	-0.037	-0.223
	(0.032)	(0.037)	(0.083)	(0.039)	(0.090)	(0.038)	(0.088)
	[0.021]	[0.411]	[0.021]	[0.325]	[0.010]	[0.392]	[0.014]
Incentives	-0.035	0.010	-0.044	0.009	-0.046	0.012	-0.045
	(0.034)	(0.030)	(0.035)	(0.030)	(0.036)	(0.030)	(0.035)
	[0.356]	[0.763]	[0.266]	[0.805]	[0.271]	[0.724]	[0.316]
Both	-0.080	-0.048	-0.203	-0.049	-0.209	-0.038	-0.214
	(0.035)	(0.052)	(0.086)	(0.052)	(0.087)	(0.053)	(0.090)
	[0.037]	[0.399]	[0.022]	[0.389]	[0.022]	[0.487]	[0.035]
p(All = 0)	0.086	0.692	0.088	0.622	0.077	0.719	0.088
Observations	10,887	10,887	10,887	10,887	10,887	10,887	10,887

Notes: The table shows our analysis of the effects of the takeup of the petty cash component of the autonomy treatment. We use our records on which offices accepted to start the process to receive the petty cash (columns 2 and 3) and which offices completed the process and received the petty cash (columns 4 and 5) to measure takeup. Column 1 shows our baseline specification, the intent to treat regression of unit prices on assignment to treatment and controls. Columns 2 and 4 show OLS regressions of unit prices on treatment takeup and controls. Columns 3 and 5 show IV regressions of unit prices on takeup instrumented by treatment assignment. The table shows clustered standard errors in parentheses and p-Values from randomization inference under the null of no treatment effects in square brackets.

TABLE 7: MECHANISMS: HETEROGENEITY BY DDO TRAITS

	(1)	(2)	(3)	(4)
Autonomy	-0.157	-0.094	0.007	-0.103
	(0.052)	(0.053)	(0.121)	(0.131)
	[0.006]	[0.119]	[0.971]	[0.605]
Incentives	0.028	-0.087	-0.098	-0.049
	(0.064)	(0.051)	(0.100)	(0.116)
	[0.715]	[0.121]	[0.485]	[0.787]
Combined	-0.092	-0.068	0.132	0.129
	(0.064)	(0.058)	(0.095)	(0.111)
	[0.204]	[0.297]	[0.348]	[0.440]
Autonomy × Tenure	0.006			0.007
	(0.003)			(0.003)
	[0.018]			[0.008]
Incentives × Tenure	-0.003			-0.002
	(0.003)			(0.003)
	[0.389]			[0.489]
Combined × Tenure	0.001			0.001
	(0.003)			(0.003)
	[0.793]			[0.673]
Autonomy × Pay Scale		0.042		0.041
		(0.068)		(0.067)
		[0.590]		[0.587]
Incentives × Pay Scale		0.086		0.065
		(0.073)		(0.072)
		[0.279]		[0.423]
Combined × Pay Scale		-0.026		0.000
		(0.085)		(0.087)
		[0.770]		[0.998]
Autonomy × Education			-0.096	-0.082
			(0.125)	(0.126)
			[0.600]	[0.664]
Incentives × Education			0.064	0.028
			(0.108)	(0.115)
			[0.675]	[0.856]
Combined × Education			-0.243	-0.250
			(0.099)	(0.108)
			[0.106]	[0.113]
Observations	11,771	11,771	11,771	11,771

Notes: The table shows our analysis of how DDO traits interact with the treatment effects. In column 1 we analyze the DDO's tenure (measured as years since induction to the civil service). Column 2 analyzes the DDO's position on the pay scale. We create a dummy for being high (grade 19 or 20) on the pay scale and interact it with the treatment indicators. Column 3 analyzes the DDO's education. We create a dummy for having a high (Masters of PhD) level of education and interact it with the treatment indicators. Column 4 combines all three DDO traits.

TABLE 8: MECHANISMS: HETEROGENEITY BY OFFICE TRAITS

	(1)	(2)	(3)	(4)
Autonomy	-0.084	-0.147	-0.153	-0.074
	(0.043)	(0.067)	(0.075)	(0.086)
	[0.093]	[0.042]	[0.059]	[0.427]
Incentives	-0.002	-0.056	-0.040	0.037
	(0.044)	(0.065)	(0.073)	(0.085)
	[0.977]	[0.438]	[0.618]	[0.685]
Combined	-0.053	-0.088	-0.061	0.044
	(0.044)	(0.067)	(0.072)	(0.084)
	[0.310]	[0.255]	[0.461]	[0.650]
Autonomy × AG Distance	-0.002		-0.007	0.090
	(0.161)		(0.159)	(0.181)
	[0.991]		[0.972]	[0.651]
Incentives × AG Distance	-0.128		-0.135	-0.041
	(0.154)		(0.151)	(0.168)
	[0.421]		[0.402]	[0.820]
Combined × AG Distance	-0.135		-0.175	-0.049
	(0.164)		(0.165)	(0.176)
	[0.478]		[0.358]	[0.806]
Autonomy × HQ Distance		0.029	0.035	0.036
		(0.032)	(0.032)	(0.032)
		[0.416]	[0.341]	[0.335]
Incentives × HQ Distance		0.014	0.020	0.021
		(0.031)	(0.030)	(0.030)
		[0.696]	[0.568]	[0.567]
Combined × HQ Distance		0.001	0.005	0.001
		(0.029)	(0.030)	(0.030)
		[0.980]	[0.887]	[0.971]
Autonomy × Generic Budget Share				-0.176
				(0.113)
				[0.178]
Incentives × Generic Budget Share				-0.170
				(0.120)
				[0.201]
Combined × Generic Budget Share				-0.217
				(0.113)
				[0.080]
Observations	11,771	11,771	11,771	11,771

Notes: The table shows our analysis of how office traits interact with the treatment effects. In column 1 we analyze the office's distance from its Accountant General office (we use the straight-line distance, measured in 100s of kilometers). In column 2 we analyze the office's distance from its department's secretariat in Lahore (we use the straight-line distance, measured in 100s of kilometers). In column 3 we analyze the share of the office's budget allocated to procurement of gerneric goods. In column 4 we combine all three office traits.