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Can "Good" HR Practices be Exported?

Evidence from a Field Experiment in Ghana

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ABSTRACT

Incentive pay is a key component of management strategy, and yet field evidence on the impacts of both individual and team incentives is limited to studies carried out in high-income countries. The mechanisms that lie behind individual responses to incentives go far beyond rational considerations of wage maximization, and encompass concerns for social visibility, preferences for collective work and other behavioral norms. These norms tend to vary by culture, potentially creating considerable heterogeneity in responses to incentives across countries. We present evidence from a field experiment designed to evaluate the impact of individual and group monetary incentives and individual and group rank incentives in Accra, Ghana. We precisely estimate that, contrary to earlier findings in other settings, these incentives have no impact on productivity, work quality and firm profitability. The findings indicate that more research is needed to shed light on the cultural characteristics of the setting that determines whether performance pay is effective.

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INTRODUCTION

Increasing the productivity of people lies at the core of the development process. Yet, the drivers of worker behavior that determine productivity in low income countries remain largely unknown. Recent survey evidence shows that the most profitable and productive firms tend to adopt personnel policies that link pay to performance and that firms in low-income countries are less likely to use these "good" human resources management practices (Bloom and Van Reenen 2010; Bloom, Sadun and Van Reenen 2012). However, observational studies cannot establish causality, and rigorous field evidence on the effectiveness of pay for performance contracts is limited largely to the US and the UK. Whether pay for performance contracts can be effective at increasing productivity in developing countries remains an open question, the answer to which likely depends on how incentives interact with local cultural norms.

This observation is consistent with two alternative explanations with radically different policy implications. First, performance rewards are equally effective in low-income countries and firms want to offer them, but informational, institutional, or legal constraints prevent them from doing so. Second, performance rewards are not effective in low-income countries, possibly because different cultural norms govern agents' responses to incentives, and hence firms optimally choose not to offer them. In particular the level of "individualism" of a given society will determine the extent to which "standing out" is desirable and hence whether achieving individual success is laudable or frowned upon. This naturally maps to the effectiveness of incentive schemes that reward good performance, especially if this is made public.

We have almost no evidence that performance pay schemes that have been shown to be effective in Anglo-Saxon countries would work in low-income countries (LICs). The exceptions, carried out in India (Muralidharan and Sundararaman 2011) and Zambia (Ashraf et al 2012) suggest that there may be significant heterogeneity in the response to these incentive schemes across countries. This study seeks to build on this growing body of evidence. We implement a field experiment designed to measure the effect of individual and team performance pay in Ghana. According to widely used measures of individualism, Ghana scores at the bottom of the scale, and is thus a good candidate for a setting in which, if the cultural interpretation is empirically relevant, performance incentives would not lead to high productivity. [1]

We set up a data-entry firm and hire workers for short-term data entry positions entering real data subject to contractual terms that vary experimentally. In addition to providing an immediate data entry service to researchers in Ghana, this setting incorporated features from field and lab experiments including (i) a natural setting with real stakes; (ii) precise control and

[1] For the most commonly used measure of individualism see Hofstede, Geert . "National Culture." http://geert-hofstede.com. Accessed November 11th, 2012.

measurement and (iii) comparable task variation within country. The experiment recruited subjects who would normally work in data entry and their participation in the experiment followed conditions typical for data entry jobs. We structured the experimental treatments to explore existing “real world” incentive schemes and to test variation that interacts with documented differences in preferences for individualism vs. collectivism (Hofstede, 2001; Gelfand et al, 2007). The treatments include: flat wage, individual piece rate (IPR) and group piece rate (GPR) contracts. To disentangle financial incentives from cultural factors related to the visibility of individual achievements, we will also vary the visibility of performance in these treatments by providing ranking of relative worker performance at set intervals throughout the two-day contract. In a culture where individualism is not held in high regard, the public vs. private comparison within the individual piece rate treatment will allow us to test whether individuals do not respond to piece rates because they are concerned about others seeing them deviating from the cultural norm or because the norm is hard-wired.

We find that performance rewards, both individual and team-based, do not increase productivity, work quality or firm profitability: workers who are offered performance rewards are not significantly more productive than their colleagues who receive hourly wages. These estimates are in line with evidence from Zambia (Ashraf et al 2012). The findings are not driven by the visibility of the rewards: incentives are equally ineffective regardless of whether performance is made public. Overall the findings indicate that more research is needed to explore the effectiveness of performance pay in different cultural settings, and to provide evidence whether responses to incentives are indeed driven by differences in individualism.

THE CONTEXT AND FIELD EXPERIMENT

Why data entry?

To study the impact of different incentive contracts on firm productivity, we established a model firm in Accra, Ghana. The model firm needed to be highly realistic and with work productivity and quality that could be easily and accurately quantified and measured. To this end, we founded a data entry firm in Ghana that would digitize a variety of different surveys on behalf of Innovations for Poverty Action (IPA) over the course of the ten-month experiment. We called this data entry firm IPA Data Services and advertised it to applicants and workers as an entity dedicated to data entry.

Data entry is a task representative of the type of employment available to individuals with a secondary education in LICs. Under the umbrella of Business Process Outsourcing (BPO), data entry and similar tasks are being shifted to LICs to take advantage of lower labor costs there. Data entry is therefore a common economic activity in LICs, including Ghana, and one that will likely continue to grow as companies continue to outsource back office tasks. All workers involved in this experiment were people who would normally work in data entry. We hired them through normal channels and paid them a typical wage.

Data entry has a second advantage of being highly measureable. By definition, workers, also known as data entry operators or DEOs, carry out all work on computers in custom-made data entry interfaces. We used these interfaces, along with statistical analysis programs, to measure worker productivity over time in key strokes per hour. Data entry therefore gives us an extremely accurate and precise overview of worker behavior over time.

Structure of the firm

We established our firm in the Osu neighborhood of Accra, Ghana in September 2010. The office space consisted of four rooms: one waiting room, one managerial office, and two separate rooms for employees, with total space for five employees in each room (Appendix 1). The office contained ten identical ten-inch netbooks with entry via both the keyboard and a separate num pad. We disabled internet connectivity on the netbooks and removed extraneous programs to ensure that workers had equivalent workstations with minimal distractions. A Wireless Local Area Network (WLAN) connected these computers to a central server computer, which permitted the manager to monitor worker progress via a specialized desktop monitoring program, further reducing the propensity to engage in non-work activities. This also enabled the manager to electronically collect worker output and generate wages at the end of contracts based on productivity. Workers sat at common tables rather than desks, with up to five people per table per room. The manager had recently worked at a local data entry firm, and so had deep experience in the process of managing workers and ensuring compliance with office policies. The office space, IT systems, and managerial capacity allowed the experiment to assess up to ten workers at a time, and many more during the firm's mass-interview process.

Recruitment

The recruitment process occurred over the course of two months, October to November 2010. Managerial staff posted an advertisement on JobsinGhana.com, one of Ghana's leading recruitment websites (Appendix 2). The advertisement required prospective employees to have a minimum of a secondary education, knowledge of computer applications, advanced English skills and to be at least eighteen years old. In the course of the two-month recruitment period, IPA Data Services received 855 applications for employment. The advertisement directed applicants to an online form through which applicants provided their contact information (name, phone number, email address) and the following baseline data: gender, age (over or under 18), education level, previous experience in data entry and previous general employment experience (Appendix 2). The benefits of using an online advertisement included low-cost, wide-dispersion, the ability to collect electronically a portion of the baseline data. The online form also served as a preliminary confirmation of prospective employees' level of computer savvy.

Interviewing

The recruitment phase provided the experiment with a large pool from which to build the experimental sample. Managerial staff called these applicants in the order they applied and invited them to in-person interviews. The in-person interviews took place in groups of up to ten applicants, or one for each available workstation. The interviews took approximately one hour and consisted of two baseline typing tests and a demographic and work preferences survey. Prior to the start of the interview, the on-site manager would explain IPA Data Services' mission to applicants: to provide high-quality paper-to-computer data entry services to IPA. The on-site manager also detailed the logistics associated with the two-day contracts and explained that we would store all applicant information in a secure database. We also made an effort to temper expectations regarding the length of employment.

Applicants took one baseline typing test in Excel, and the other in Epidata, a commonly used data entry interface software (Appendix 3). The Excel test featured a recurring list of numbers that applicants entered as quickly and accurately as possible for five minutes. During the fifteen-minute Epidata test, applicants entered the Pure Home Water Survey (PHWS), one of the paper surveys we ultimately used during the experimental contracts. Prior to both tests, we gave applicants brief oral instructions on how to enter data and, in the case of the Epidata test, how to enter codes for blank variables and skip patterns. The instructions were brief, no more than five minutes in length, and we told applicants that they could ask the manager questions at any time during the test.

Following both tests, applicants took a 20-minute survey. This survey collected demographic information, including age, gender, ethnicity and languages spoken. In addition to demographic information, the survey collected information regarding work experience and preferences, including prior experience in data entry and prior wages received (flat wage or piece rate).

Following the group interview, the on-site manager told applicants that we would contact qualified applicants in the coming months as short-term contracts became available. In order to replicate actual working conditions, we selected only to strongest applicants in our pool. During piloting, we found that performance on the Epidata test did a better job than the Excel test of predicting endline contract performance. Using these results, we therefore attempted to replicate actual data entry firm hiring practices. We eliminated from consideration individuals whose accuracy during the Epidata test was below 65%. We then stratified individuals using three variables: gender, ethnicity (dominant vs. all others) and baseline Epidata performance (above vs. below median productivity). Over the course of the experiment, we carried out fifteen rounds of interviews and executed a stratified randomization after each interview to assign individuals to either control or one of four treatment groups.

Invitation to contracts

We invited all applicants who scored above 65% accuracy on the baseline Epidata test to at least one contract and a maximum of two contracts. We ran two two-day contracts per week over the course of ten months from December 2010 to late October 2011. We randomly assigned workers to one of the following contracts: flat wage, individual piece rate (IPR), individual piece rate with rank (IPR rank), group piece rate (GPR), or group piece rate with rank (GPR rank).

Over the course of two rounds, 50 workers were assigned to the flat wage contract and 100 workers to each of the piece rate contracts, for 450 total two-day observations. Occasionally, workers who confirmed attendance would not show up for contracts. Despite inviting ten workers to each contract, we found that approximately seven workers attended each contract on average. To limit this phenomenon, we invited workers to work on an alternative date in the event that they were unable to attend on their originally assigned date. If a worker did not answer his or her phone after at least two attempts to contact him or her, we eliminated them from consideration for future contracts. Individuals were not informed of their contract assignment before they arrived and there is no relationship between treatment type and attrition.

Contracts

In order to capture order effects, we offered each worker up to two contracts under either the same or different payment terms. During the first contract, all workers entered the PHWS survey. During the second contract, they entered the Formal Savings survey (FS).

To minimize selection bias, the on-site manager invited workers to contracts one to three days in advance of the contract by telephone and text message. During all invitation calls, the on-site manager did not indicate which contract type the worker would receive. The on-site manager told workers that the actual salary would depend on the contract type they received, but that the average worker should expect to receive 25 Ghana Cedis (GHC) per day, and that the contract could be either flat wage or piece rate. During the call, the on-site manager also reiterated that the contract was for two-days and that working hours would be from 8:30am – 4:30pm with one hour for lunch. Despite these instructions and due in large part to unpredictable traffic in Accra, 26.8% of workers arrived after 8:30am to their contracts. The average wage of 50 GHC per two-day contract was approximately 20% above the market average and compensated workers for the short-term nature of the assignment.

The flat wage contract paid a salary of 50 GHC for two days, regardless of the quantity or quality of the data entered. The piece rate contracts paid a fixed amount per keystroke, whether correct or incorrect, defined as "a keyboard action that results in data capture in the used program." We calculated the piece rate separately for each of the two surveys. For the PHWS survey, we calculated the piece rate as the flat wage divided by median number of total key strokes entered during the first flat wage contract (held December 20th-21st, 2010) or 50/46,729 = .00107 GHC per keystroke. Likewise, for the FS survey, we calculated the piece rate as the flat wage divided by median number of total key strokes entered during the first flat wage contract (held April 28th-29th, 2011) or 50/80,425 = .0006217 GHC per keystroke.

We based payment in all four piece rate contracts on this piece rate calculation. In the IPR contract, we paid the workers the survey-specific piece rate multiplied by the total number of key strokes entered over two days. In the GPR contracts, we divided workers into two groups and paid a salary equal to the total number of key strokes entered by all group members multiplied by the survey-specific piece rate and divided by the number of workers in the group. The IPR rank and GPR rank contracts had identical terms to their non-rank counterparts, but we ranked workers based on their productivity three times a day at regular intervals to provide public visibility of relative and absolute performance. The on-site manager used Stata to calculate payment amounts and ranking.

Table 1

	Productivity Information	
Contract type	Private	Public
Flat wage	x	
Individual piece rate (IPR)	x	x
Group piece rate (GPR)	x	x

To avoid spillovers, all workers in a given two-day contract received the same contract (i.e., all workers in the office at a given time received flat wage or IPR). We provide these workers with a written contract to sign that laid out the contract terms. The on-site manager also passed around a simplified description of the given contract, and spent five minutes explaining how we would pay the workers and answering questions regarding remuneration to ensure that all workers understood the contract terms before work began (Appendix 4).

The manager also explained work policies to all workers. Workers would work until 4:30pm, would have one hour for lunch from 12:00pm to 1:00pm, and would be free to use the bathroom or take brief breaks during the day as needed. For treatments with a rank component, we told workers that the on-site manager would post full names and unique identifiers on white boards at the front of each room three times a day at approximately 10:00am, 1:00pm and 3:00pm. Finally, the on-site manager was available all day to answer questions regarding the data entry task or administrative issues. For the 26.8% of workers who arrived after 8:30am, the on-site manager provided expedited individual instructions upon arrival.

Production task

Following the brief orientation, we assigned workers to one of ten workstations and the on-site manager led workers through a fifteen-minute, computer-based training. By using a standardized presentation to train workers, we eliminated variations in training quality that could have occurred over the course of the experiment. The on-site manager walked workers through this training to ensure that they understood how to code certain responses and how to move through the surveys. This training also introduced workers to the basic functionality of the computers and number pads for use in data entry. Given that workers submitted applications online and that the interview featured a typing test similar to the production task, workers were well prepared for work when contracts began.

The on-site manager provided each worker with a stack of approximately fifty paper surveys to enter. We also instructed workers to speak with the on-site manager when he or she had finished entering those surveys. Throughout the day, the on-site manager provided new surveys to workers as needed to ensure any exogenous interruptions to the workflow. The production task itself required workers to enter coded data from paper surveys into the Epidata interface. In general, the surveys contained short numeric and string fields, although four fields in both PHWS and FS contained longer strings.

The data entry interfaces captured the total number of key strokes entered by workers over the course of the two-day contract. In addition, the data entry interface captured key timing variables, including the daily start time, end time and the number of seconds to complete one survey. The on-site manager also recorded the lunchtime on a daily basis, which occasionally varied from the 12:00pm-1:00pm schedule. The on-site manager recorded additional anomalies, such as a workers leaving after a few hours due to an emergency or leaving for long breaks midday, separately.

At the end of the two-day contract, the on-site manager paid workers in cash. For workers under the flat wage contract, we did not carry out a salary calculation, and paid each worker 50 GHC. For the four incentive contracts, the on-site manager collected all data entered by the workers and calculated their pay using based on the conditions of the workers' assigned contracts. Workers received cash along with a receipt stating the number of key strokes entered and the calculation used to define their two-day wage. Before the workers left the office, we told them that they would potentially be called for a second contract in the near future. If employees missed the second day of work, we left one day of pay with a security guard at the main IPA office down the block from the IPA Data Services office.

Why create a model data entry firm?

Creating a dedicated data entry firm for the purposes of this experiment gave us considerable flexibility to explore a variety of incentives and contract structures. The firm was not subject to market forces, which gave us flexibility on two key dimensions. Along the first dimension, we were able to eliminate variation in firm structure and managerial practices that might have been correlated with firm performance. In the second dimension, we were able to implement exactly the same incentive schemes throughout the experiment, even if some schemes led to an economic loss or low-quality output. This approach contrasts with that of firms that operate within real product markets. These firms are constrained in implementing incentives among those that can increase profits (Bandiera et al 2011). Our Ghana firm allowed us to implement the same exact schedule, training modules, production task, and contracts throughout the ten-month experiment.

In addition to enabling consistency throughout the experiment, data entry is measurable and strongly or perfectly correlated with effort, productivity and work quality (Kaur, Kremer, Mullainathan 2011). Productivity is perfectly correlated with the number of key strokes per contract, work quality perfectly correlates with accuracy, and effort is a combination of productivity and accuracy. This allows us to precisely measure our key output variables.

Creating a dedicated data entry firm also came with some disadvantages. In order to gain statistical power, we chose to offer short-term contracts to workers. While these types of contracts are common in data entry operations, where firms carry out projects on a contract basis and oftentimes have to scale their workforce very quickly, they may have an impact on responses to incentives. In particular, the short-term contract structure likely muted career concerns, social connections (and particularly in- vs. out-group concerns) and selection.

Issues

Selection

Following the posting of the initial job advertisement, IPA Data Services had a total recruitment pool of 855 workers. We invited these individuals to interviews in the order they applied. In total, we called 518 workers to invite them to an interview. Of the 518 workers called, we scheduled 483 for interviews, 379 of which actually attended an interview. We assigned all 379 interviewed workers to treatment contracts independently for the PHWS survey and FS survey rounds. For the PHWS survey round, we called workers in *descending* order within each treatment or control group of the random number used to assign workers to the PHWS round treatment and control groups. For the FS round, we called workers in *ascending* order within each treatment or control group of the random number used to assign workers to the PHWS round treatment and control groups. By sorting workers using the same random number in both rounds, but sorting by the inverse of that number in the second round, we increased the probability that we would invite all interviewed workers to at least one contract. Of the 379 workers we interviewed and assigned to treatment and control groups, we invited 343 to contracts, 319 of which were interested in attending a contract. Of the 319 who expressed interest in attending a contract, 294 ultimately attended at least one contract, with 165 or 56.1% attending two contracts.

Table 2: Attendance Selection

	Workers	Percent
Total applicants	855	100.0%
Invited to interview	518	60.6%
Interested in interview	483	56.5%
Attended interview	379	44.3%
Invited to contract	343	40.1%
Interested in contract	319	37.3%
Attended contract	294	34.4%
Attended two contracts	165	19.3%
Observations	855	-

Attrition and tardiness

Workers occasionally left during the day for long periods, left early, attended only the first day of a two-day contract, or otherwise did not comply with the stated work schedule. With 294 unique employees across 61 separate two-day contracts, attrition and tardiness were inevitable. We expected certain forms of attrition and irregularity, particularly where heavy traffic, family emergencies, university exams or other contingencies required a worker to leave the office for all or a portion of a workday. We would expect these situations to arise and to be uncorrelated with the treatment or control assignment.

The standard contract was for two days for a total of twelve working hours and two hours for lunch. We consider individuals who worked fewer than ten hours to have worked an abnormal schedule. We confirm through regression analysis that the relationship between abnormal hours and treatment assignment is not significant (Table A1). We also discuss the possibility that workers who we interviewed and invited to a contract did not come to the contract due to endogenous factors. This would be unlikely because we invited all workers to contracts using the same script, thereby avoiding selection based on the contract assignment.

Spillovers

We addressed spillover issues through two different mechanisms. We designed the experiment such that all workers within a given two-day work period received the same contract. Workers would naturally assume that this contract was the only one offered for entering this specific survey, given that his or her colleagues received the same contract. Because we controlled all elements of the hiring and contracting process, we eliminated the possibility that a worker would work a contract that we did not assign to them.

We also invited workers to only one contract with the PHWS survey or FS survey. By reinforcing the idea that each work task has its own payment structure, we attempted to reduce the impact that knowledge of other contract types would have on behavior. Nevertheless, it is possible that through exogenous social connections, workers learned about other contract types, which might have set expectations about contract type *ex ante*.

Hawthorne effects

One of the benefits of starting a dedicated data entry firm was that we maintained control over key administrative and managerial decisions within a real-stakes work environment. IPA Data Services employed workers on short-term contracts as real data entry employees. Consequently, we avoided disclosing the experimental ends of worker employment. All workers gave consent to IPA Data Services to collect survey and work data and an ethics committee approved the survey and experimental design.

Working conditions as well as the production task mimicked those of a real firm. By giving all workers within a given two-day work period the same contract, we avoided unnatural divisions between and among workers. The daily salary as well as all contract documentation conformed to local business practices and the on-site manager had previously worked as a data entry manager for a local firm. By holding contracts using two different surveys, we avoided situations in which one worker received two different contracts or payment types for entering the same survey. Varying the work task permitted us to explore order effects without introducing experimental bias or Hawthorne effects. Finally, workers entered real survey data collected in Ghana. These efforts eliminated any sense of abnormality within the working environment and minimized the probability that the experimental context altered worker behavior.

BASELINE DATA AND BALANCE ACROSS TREATMENT

The objective of the baseline was to collect information to verify the comparability of the experimental groups and to be able to carry out heterogeneity analyses later in the experiment. Baseline data collection took place on fifteen days between August 22nd, 2010 and July 14th, 2011. The baseline consisted of: (i) a demographic and work preferences survey; (ii) a five-minute typing test in Excel; (iii) a fifteen-minute typing test. The survey and both typing tests all served as our interview; however, all interested applicants were offered employment regardless of their performance on the tests.

Each treatment cell contains approximately 100 observations, with the number of observations between and among contracts varied slightly due to unpredictable worker attendance.[2] Despite having space for ten workers at a time, our average attendance over the course of the experiment was 7.53 workers, with a range of 7.29 (flat wage) to 7.85 (IPR) workers per contract. Average working hours is consistent across contracts, with the range being approximately 10 minutes and the standard deviation 6 minutes among them.

We collected baseline data during the baseline interview and the application process. The demographic and experience data comes from both data streams. We stratified assignment to the contract treatments based on gender, baseline ability and ethnicity as strata during the randomization process. As shown in Table 3, these characteristics are roughly balance across treatment cells; however, because of the relatively small samples required per cell, allocation from eight possible strata combinations, and the need for rolling assignment to treatments throughout the application process, this is not exact. The majority of workers were males, and the sample is balanced with between 67% and 73% of each group being male. Likewise, baseline ability as measured by key strokes per minute during the Epidata baseline test is balanced. Individuals in all incentive treatments are 12%-16% more likely to be Akan than those in the flat wage treatment. We control for all stratification variables in the analysis.

In addition to the stratification variables, throughout the analysis, we utilize key control variables from the baseline including whether the worker: (i) has data entry experience; (ii) has piece rate experience; (iii) likes recognition and (iv) has already participated in a contract (i.e., this observation is the worker's second contract). These characteristics are relatively well balanced across the contracts. Additional demographic variables of interest, including education level, paid work experience and age are also mostly balanced based on a summary of means across treatment and control groups. Nearly all subjects report having a university education and previously paid employment. The average age of subjects is 27.6 years.

Baseline performance variables

We measure baseline productivity and accuracy using the interview Epidata test. We gave workers fifteen minutes to enter as many surveys as quickly and accurately as possible. From this test, we derive two measures of ability: one based on the number of key strokes entered per minute and the other based on the number of accurate key strokes entered per minute. Figure 1 presents the kernel density of the former by contract type. Baseline ability follows roughly the same distribution across all contracts, with productivity skewed rightwards. Table A2 presents the means and standard deviations of baseline ability, and shows that the mean baseline ability is within less than one-tenth of one standard deviation of the overall mean for

[2] Because of the relatively low variability in performance under the flat wage contract, we targeted only 50 observations for this cell.

all contracts. Baseline accuracy, another useful measure of ability, is similarly well balanced around the mean across all treatments.

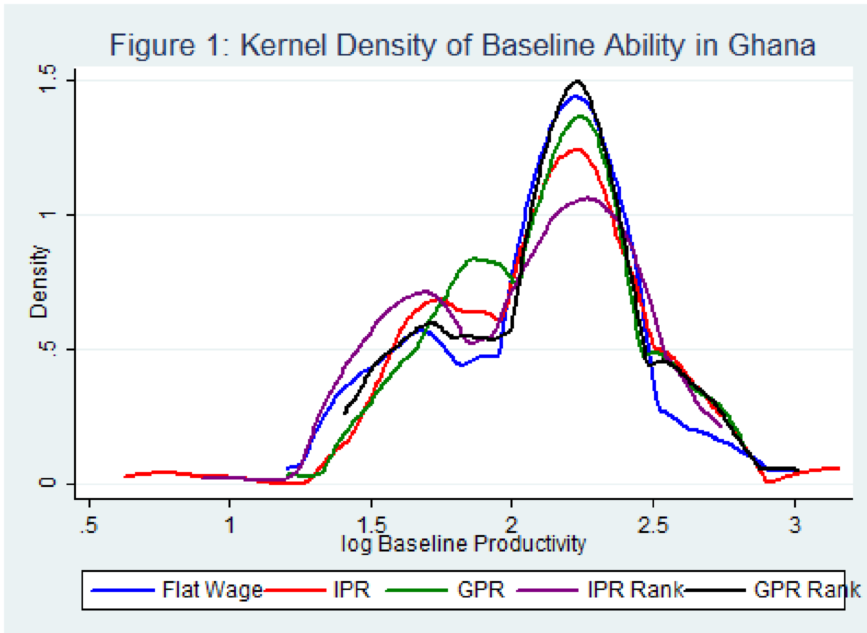

Figure 1: Kernel Density of Baseline Ability in Ghana

Endline performance variables

The primary outcome variables are productivity, accuracy and firm profitability. We make use of the log of key strokes entered per hour over the course of two days. We also break this figure down by day. Likewise, we use the log of the number of accurate key strokes per hour. Our profitability calculation is based on a cost model built out in the Profits section of this paper. Table A3 displays the means and standard deviations of these five key variables across each contract. Figure 2 presents the kernel density of endline performance by contract type.

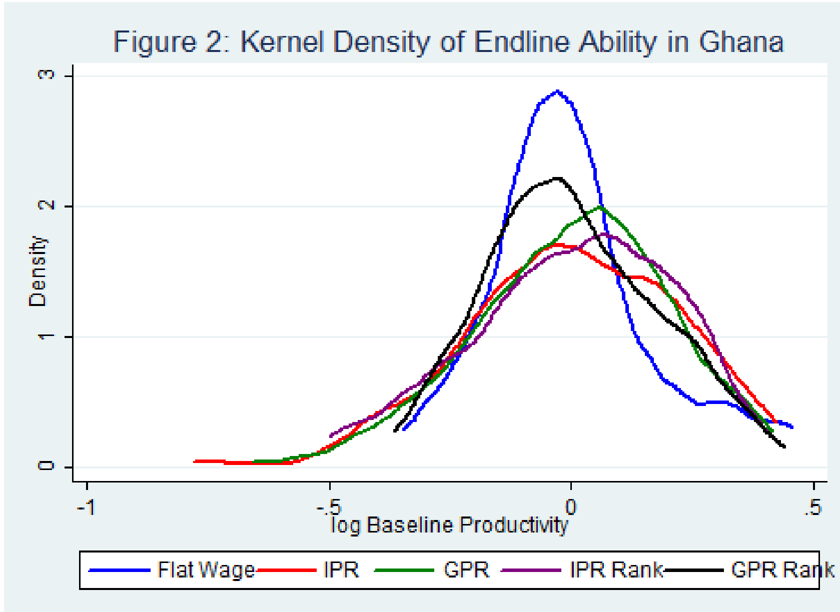

Figure 2: Kernel Density of Endline Ability in Ghana

Selection and attrition

There was attrition over the course of the experiment from the first contact with workers (the application) to the experiment itself. Table 2 shows the eight distinct phases of the experiment. These phases included: (i) job application via the online form; (ii) invitation to the interview; (iii) expression of interest in interviewing; (iv) interview attendance; (v) invitation to a contract; (vi) expression of interest in working a contract; (vii) contract attendance; (viii) two contract attendance. We also display attrition as a percent of the total number of applicants in this table.

Table A1 reviews the relationship between abnormal hours and treatment assignment. We define abnormal hours at those workers who worked fewer than ten hours over the course of two days. We do not find a significant relationship between workers who worked abnormal hours and their treatment assignment at the 10% significance level. While workers were not informed of their contract assignment until they arrived for work on the first day of the contract, it is possible that their decision to return for the second day of the contract could be affected by their treatment assignment. We find no significant effects of contract assignment on attrition, tardiness, early departure, or any other measure of abnormal attendance. Workers' stated causes for tardiness, early departure, long breaks and truancy include traffic, unexpected emergencies, university exams and other jobs.

MODEL: INCENTIVES AND PRODUCTIVITY

Where y_{it} is the average productivity (key strokes per hour) of worker i at time (month) t over the two-day contract. x_i is a vector of the worker's characteristics, including ability. α and β

measure the causal effect of incentives on productivity under the assumption that incentive treatments are orthogonal to n_{it}. Identifying assumptions can fail because of either endogenous drop-outs or spillovers. As described above, neither appears to be relevant in this setting.

$$y_{it} = \alpha IP_i + \beta TP_i + x_i\gamma + n_t + n_{it}$$

IMPACT: PRODUCTIVITY, ACCURACY AND PROFITABILITY

In the previous two sections, we discussed a host of descriptive statistics, with the end goal being to demonstrate balance in the sample. To show that the relationships between contract type and end line productivity, accuracy and profitability figures are statistically significant, we carry out regression analyses following on the model described in the previous section. We build upon this model by analyzing heterogeneous effects, the mechanisms by which incentives change worker behavior, profitability and inter-day relationships that cause changes in productivity, work quality and firm profitability. In the following regressions, we use a dummy variable for Akan, the aforementioned baseline ability variable and a dummy variable for male as controls. In addition to these stratification variables, we control on experience with data entry, whether this is the first or second contract for the worker, whether this is the first time working for a piece rate wage.

Survey and monthly fixed effects

Our model includes fixed effects for both month and survey type entered (PHWS or FS). We never used the two surveys in the same month, but we did use each survey for several months. As such, we control for unobserved heterogeneity over time. This is particularly relevant for the fixed effects related to the survey used. The FS survey had higher productivity per hour than the PHWS survey, and so the fixed effects model controls for variation in survey type as well.

Average treatment effects

There is no significant effect for any of the treatment contracts. The coefficients on all four contracts range from .006 to .039, none of which are significant at the 10% confidence level. The treatment effects are stable, and adding controls and fixed effects improves the precision of this estimate. We cannot reject performance under the IPR, GPR, IPR rank or GPR rank are identical. This suggests that in this context neither monetary nor social incentives are effective at improving productivity. This would also suggest that information does not help cooperation in the GPR and GPR rank treatments.

Unsurprisingly, baseline ability has a significant impact on productivity, with a coefficient of .225 significant at the 1% level (Column 5, Table 4). In other words, we expect a .22% increase in endline productivity when baseline productivity increases by 1%. This suggests that individuals with a greater preliminary endowment of ability in this task are more likely to succeed at it in the short term. We show later that this effect diminishes after one day.

We also find that workers exhibit significant learning across contracts. The coefficient on the workers first contract variable is -.169 significant at the 1% level (Column 5, Table 4). We expect workers in their second contract to be 15.5% more productive than workers in their first contract. We therefore find that workers' skills improve significantly over a short period of time when the task and environment are very similar, with major gains in productivity being seen after only two days of work.

Heterogeneous treatment effects

Table 5 explores heterogeneous effects on productivity. We allow the response to incentives to differ along several dimensions which include gender, ability, previous experience with data entry and with piece rates. The table makes clear that the treatment effects are consistently negligible for all worker characteristics . All the partitions we consider reveal that workers in this setting do not respond to monetary incentives regardless of their gender, ability or experience.

Exploring other determinants of productivity, we find that Akan males are 5.1% less productive than other males (β = -.052 p=.088 n=174) (Column 2, Table 5). Among workers with low baseline ability, we find that workers with piece rate experience are 14.2% more productive than other workers with low baseline ability (β = .133 p=.072 n=123) (Column 4, Table 5). Among workers who have data entry experience, we find that Akan workers are 5.5% less productive than other workers with data entry experience (β = -.056 p=.08 n=193) (Column 5, Table 5). Among workers who don't have data entry experience, we find that people with piece rate experience are 18.8% more productive than workers with data entry experience (β = .172 p=.02 n=58) (Column 6, Table 5). Among workers with no piece rate experience, we find that those who find recognition to be important are 4.9% more productive than workers with piece rate experience (β = .048 p=.081 n=220) (Column 8, Table 5). We should be cautious in interpreting these results because some of these cells have very few observations.

Mechanisms

Does information help group cooperation

Cooperation does not appear to drive the response to group incentives. By adding the ranking element to the GPR treatment, we hoped to assess whether workers' performance improves or declines in the presence of information regarding team members' relative performance. If information helped group cooperation, we would expect to find higher productivity among GPR rank workers than among GPR workers. We do not, however, find a significant difference in the response to incentives among workers assigned to these two contracts. The coefficients on GPR and GPR rank are β = .018 (p=.621) and β = .033 (p=.381), respectively, neither of which is significant at the 10% significance level (Column 2, Table 6).

Tall poppy vs. recognition

The literature contains a number of studies which examine the idea that in some African cultures, people discourage individual success (Platteau 2000, Baland et al 2010, Comola and Fafchamps 2010). If this were the case, we would expect to see significantly higher productivity in the IPR contract than in the IPR rank contract. We find, however, that there is no significant difference between productivity in the IPR and IPR rank contracts. The coefficients on these contracts are β = .036 (p=.366) and β = .060 (p=.144), respectively, neither of which is significant at the 10% significance level (Column 3, Table 6).

Quality vs. quantity

There does not appear to be a tradeoff between quality and quantity. We look at both quality and quality per hour. We derive both quality variables by determining the percent of entries the worker entered correct over the course of the contract. We find that in both the quality and log of quality models, the coefficients on each of the four contract types are not significant at the 10% significance level. The coefficients on all of the treatments in the quality model are approximately zero (Table 7).

Profits

Firm profitability is a relevant factor in considering which incentives to use. Businesses are not only interested in high productivity, but also in high quality output. In our model firm, we consider the various costs the firm incurs in order to produce a finished product. In this case, data entry firms are generally held to high accuracy standards, with most contracts specifying accuracy of 99.5%. Reaching this high level of accuracy requires a number of steps, and the workers' productivity and accuracy influences the costs associated with these steps. In our model, we consider each worker to be an independent contract. We imagine that we hired 1 worker to enter a certain number of surveys and that all fixed costs apply to that single worker, rather than to the actual number of DEOs present during a given 2-day experimental session. We first determine the worker's salary cost of entering the surveys, which is equal to the actual amount we paid the worker under the terms of his or her contract. Then we determine the managerial salary cost, which is equal to the manager's salary for two days, or 78.5 GHC per day. We also determine the fixed cost, which is equal to rent and services for two days, or 270.4 GHC per day. The cost per field entered DE_c is therefore a function of his or her salary W_c and the managerial M_c and fixed costs F_c multiplied by the number of days T_d worked by the worker divided by the total number of fields entered P. We multiply by 2 to account for double data entry.

$$DE_C = 2\left(\frac{W_c + T_d(M_c + F_c)}{P}\right)$$

To ensure that accuracy rates do not drop below 99.5%, data entry firms typically double enter and then reconcile their data. As noted above, we assume that the same worker carries out the process of double entry. During reconciliation, data entry firms compare two datasets, flag discrepancies, and return to the original data to confirm the correct answer. Errors are probably more costly than the original double entry, so we create a second assumption that defines the reconciliation cost based on different inputs than the original double entry cost. It is also possible that double entry with reconciliation fails to ensure 99.5% accuracy because there is some non-convexity in that high error rates make it more likely that errors are not caught by double entry, which can lead to contract penalties. We create three separate assumptions to treat these three cases.

The first case is very straightforward. We assume that the unit cost of reconciliation R_{c1} is based on the original unit cost of double entry DE_c , because reconciliation is usually double entered, multiplied by the number of error fields N_e and divided by the total number of fields entered. We added the original unit cost of double entry to determine the full unit cost for double entry and reconciliation of errors at the cost of double entry.

$$R_{c1} = DE_c + \frac{(DE_c \times N_e)}{P}$$

The second case establishes a separate set of inputs based on the assumption that reconciliation is much slower than double entry. The first step is to determine the amount of worker time W_{tR} in hours T_h required to double reconcile all errors where a worker can reconcile N_{wR} errors per day, which we assume to be equal to 500.

$$W_{tR} = T_h\left(\frac{2 \times N_e}{N_{wR}}\right)$$

We also create an estimated worker salary cost per reconciliation W_{CR}. In this equation, we determine the total number of errors the worker must reconcile and divide that by the number of errors a worker can reconcile per day, multiplied by the daily salary W_s, in this case based on the flat wage of 25 GHC per day.

$$W_{cR} = W_s \times \frac{2 \times N_e}{N_{wR}}$$

We estimate managerial salary cost per reconciliation M_{cR} required to manage the reconciliation process. We assume that managerial time M_{tR} scales by a linear factor N_{mR} for every 500 surveys to be reconciled, with the total number of surveys in the dataset and multiply the total time by the manager's daily salary M_s.

$$M_{cR} = N_{mR} \times M_{tR} \times M_s$$

Finally we calculate the fixed cost of reconciliation F_{cR} based on the amount of managerial and worker time required to complete the reconciliation.

$$F_{cR} = F_c(W_{tR} + M_{tR})$$

With these costs in hand, we find the cost of reconciliation R_{c2} in this second case to equal the worker, managerial, and fixed costs divided by the total number of fields entered by the worker. As in case 1, we add the original unit cost of double entry to this total to determine the full unit cost for double entry and reconciliation of errors at the reconciliation cost defined by $\frac{W_{cR}+M_{cR}+F_{cR}}{P_t}$.

$$R_{c2} = DE_c + \frac{W_{cR} + M_{cR} + F_{cR}}{P_t}$$

The third case assumes that the cost of reconciliation also includes the cost of false positives, including the probability of violating contract terms that require 99.5% accuracy. We first determine the expected value of double errors $\varepsilon(dbl\ err)$ for each worker. We do this by analyzing the error rate E_n on each question Q_n for both the PHWS survey and FS survey. We square the expected value for each question to obtain the overall expected value of entering the same questions wrong two times during double entry.

$$\varepsilon(dbl\ err) = (Q_1E_1)^2 + (Q_2E_2)^2 + (Q_3E_3)^2 + \cdots + (Q_nE_n)^2$$

We also calculate the expected percent of entries $\varepsilon(pct\ dbl\ err)$ each worker will get wrong twice. This is calculated as the expected number of double error entries divided by the total number of fields P_t entered by each worker.

$$\varepsilon(pct\ dbl\ err) = \frac{\varepsilon(dbl\ err)}{P_t}$$

With both expected values in hand, we can calculate the potential cost of violating the contract V_c as a function of the reconciliation cost we calculate in case 2 and the contract violation cost. We calculate the total cost of double errors as the product of the expected number of double errors times the reconciliation cost noted in case 2 $\varepsilon(dbl\ err) \times R_{c2}$. We divide this by the total fields entered to get per unit cost of correcting all double error across all entries. We add to this the difference between the contract violation terms V, which we assume to be 99.5%, and the expected error rate multiplied by the reconciliation cost noted in case 2. We do not divide this by the total fields entered by the worker because the per unit cost is implicit to R_{c2}.

$$V_c = \frac{(\varepsilon(dbl\ err) \times\ R_{c2})}{P_t} + \left((\varepsilon(pct\ dbl\ err) - V) \times R_{c2}\right)$$

The unit cost of reconciliation with violation R_{c3} takes two forms. In the first form, the unit cost of reconciliation with violation is equal to the unit cost of the original unit cost of double entry plus the unit cost of reconciliation at the cost of reconciliation plus the unit cost of double errors at the cost of reconciliation $\frac{(\varepsilon(dbl\ err) \times R_{c2})}{P_t}$ plus the expected unit cost of contract violation where the expected error rate is greater than .05%.

$$R_{c3} = DE_c + R_{c2} + \frac{(\varepsilon(dbl\ err) \times R_{c2})}{P_t} + V_c \text{ if } 0 \geq \varepsilon(pct\ dbl\ err) - V$$

The second form includes only the first three components outlined in the first form, namely the original unit cost for double entry, the unit cost for reconciliation, and the unit cost for double errors. Because the expected error rate is less than .05% in these cases, no additional expected violation cost is added.

$$R_{c3} = DE_c + R_{c2} + \frac{(\varepsilon(dbl\ err) \times R_{c2})}{P_t} \text{ if } 0 < \varepsilon(pct\ dbl\ err) - V$$

This approach has one shortfall. In the third case, we likely over count the probability of a double error, because we assume that there error would be the same, and therefore undetectable during the reconciliation process, during both the first and second entries.

Table 9 reports the estimated treatment effects on each profit estimate. We do not find a significant effect for any of the treatments for any of the three measures. The coefficients on all the regressors are between -.051and approximately 0, with none significant at the 10% significance level. We cannot reject the hypothesis that the four contracts have different effects on each measure of firm profit.

Interday

We carry out an analysis of responses to productivity on a daily basis to determine if responses vary with time Table 9 shows that responses to incentives are muted on both days, with the sole exception of the IPR treatment that leads to a modest 7.4% increase in productivity (β = .072 p=.068). We cannot, however, reject the hypothesis that the effect of IPR is the same on both days.

The productivity of male workers decreases by 4.5% on day 2 (p = .029) at the 5% confidence level. This suggests that, holding treatments constant, male workers complete less work (but not necessarily lower quality work) during day 2 than their female counterparts (Column 2).

The shift from day one to day two causes the productivity penalty associated with first timers to decrease from 19.8% (β = -.22 p=0) to 12.7% (β = -.136 p=0). This 36% improvement in performance for workers attending their first contract suggests that workers improve very rapidly (Columns 1 and 2).

From day one to day two, the effect baseline productivity has on endline productivity also decreases. On day one, a 1% increase in baseline productivity results in a .25% increase in endline productivity (β = .255 p=0). On day two, that figure drops to a .18% increase in endline productivity (β = .186 p=0). The 28% change in the impact of baseline productivity on endline productivity suggests that workers improve rapidly over time. It may also suggest that the baseline productivity measure is less a proxy for innate work ability than an indicator of familiarity with data entry in this context.

CONCLUSIONS

In the context of our data entry firm in Accra, Ghana, we find that performance pay does not increase worker productivity, work quality, or overall firm profitability. This is consistent with the hypothesis that firms that do not implement such incentives are making profit maximizing decisions. Ghana falls very low on the Hofstede individualism scale, and this study suggests that alternative incentive structures may be optimal in such countries. When exploring some of the mechanisms that might drive responses to these incentives, we find that information does not help groups to cooperate (i.e., productivity during the group piece rate with ranking contract was not significantly higher than productivity during the standard group piece rate contract). However, we do note that productivity under the individual piece rate contract with publically observed ranking is significantly lower than under the individual piece rate contract without.

Future research will explore whether this pattern holds in other countries with similarly low levels of individualism and whether, in contrast, incentives are effective in countries that have individualism scores closer to those measured in the United States and United Kingdom, where performance pay is common.

Future research will also explore alternative methods to motivate workers and increase their productivity in settings where monetary incentives fail.

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TABLES

Table 3: Summary Statistics by Treatment Assignment

	Flat Wage	IPR	GPR	IPR Rank	GPR Rank	All
Total man hours	682.7	1296.4	1364.0	1329.6	1363.6	6036.3
Average hours per contract	13.39	13.23	13.37	13.3	13.5	13.35
Male	0.73	0.67	0.70	0.67	0.68	0.69
	(0.45)	(0.47)	(0.46)	(0.47)	(0.47)	(0.46)
Akan	0.35	0.51	0.47	0.51	0.51	0.48
	(0.48)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Baseline ability (log)	2.08	2.10	2.11	2.04	2.12	2.09
	(0.36)	(0.40)	(0.33)	(0.38)	(0.34)	(0.36)
Data entry experience	0.86	0.70	0.78	0.76	0.71	0.75
	(0.35)	(0.46)	(0.41)	(0.43)	(0.45)	(0.43)
Piece rate experience	0.08	0.13	0.14	0.14	0.19	0.14
	(0.27)	(0.34)	(0.35)	(0.35)	(0.39)	(0.35)
DEO's first contract	0.51	0.66	0.66	0.67	0.65	0.64
	(0.50)	(0.48)	(0.48)	(0.47)	(0.48)	(0.48)
Recognition most imp.	0.55	0.45	0.41	0.56	0.42	0.47
	(0.50)	(0.50)	(0.49)	(0.50)	(0.50)	(0.50)
University education or more	0.98	0.98	0.99	0.97	0.95	0.97
	(0.14)	(0.14)	(0.10)	(0.17)	(0.22)	(0.16)
Age	27.9	27.6	27.7	27.4	27.8	27.6
	(3.9)	(4.1)	(3.7)	(3.1)	(3.7)	(3.7)
Paid work experience	1.00	0.96	0.99	0.96	0.94	0.97
	(0.00)	(0.20)	(0.10)	(0.20)	(0.24)	(0.18)
Observations	51	98	102	100	101	452

Notes: *Baseline ability* calculated from log keystrokes per minute in Epidata data entry test during interviews. *Recognition most important* equals 1 if respondent indicates that receiving "recognition for good peformance" on the job is of "greatest importance" (5 on 5-point scale) during pre-employment interview.

Table 4: Average treatment effects

Dependent variable: log keystrokes per hour	(1) Unconditional	(2) Controls	(3) Month FE	(4) Survey FE	(5) All Treatments
Treat: Indiv. Piece Rate	0.023	0.088**	0.034	0.034	0.039
	(0.055)	(0.041)	(0.040)	(0.040)	(0.037)
Treat: Group Piece Rate	0.006	0.067	0.020	0.020	0.022
	(0.054)	(0.041)	(0.041)	(0.041)	(0.037)
Male		-0.027	-0.037	-0.037	-0.036*
		(0.032)	(0.029)	(0.029)	(0.021)
Baseline ability		0.208***	0.209***	0.209***	0.225***
		(0.041)	(0.036)	(0.036)	(0.026)
Akan		-0.028	-0.033	-0.033	-0.034*
		(0.030)	(0.027)	(0.027)	(0.019)
Data entry experience		0.005	-0.034	-0.034	-0.010
		(0.035)	(0.032)	(0.032)	(0.022)
Piece rate experience		0.009	0.047	0.047	0.032
		(0.046)	(0.040)	(0.040)	(0.027)
DEO's first contract		-0.419***	-0.150***	-0.150***	-0.169***
		(0.031)	(0.052)	(0.052)	(0.034)
Recognition most imp.		0.025	0.036	0.036	0.025
		(0.030)	(0.027)	(0.027)	(0.019)
Treat: Indiv. Piece Rate with Rank					0.053
					(0.038)
Treat: Group Piece Rate with Rank					0.041
					(0.038)
Constant	8.379***	8.170***	7.824***	7.824***	7.790***
	(0.044)	(0.101)	(0.121)	(0.121)	(0.097)
Observations	251	251	251	251	452
Adjusted R-squared	-0.007	0.466	0.596	0.596	0.594

Notes: *Baseline ability* calculated from log keystrokes per minute in Epidata data entry test during interviews. *Recognition most important* equals 1 if respondent indicates that receiving "recognition for good peformance" on the job is of "greatest importance" (5 on 5-point scale) during pre-employment interview. Standard errors in parentheses. * p<0.1 ** p<0.05 *** p<0.01

Table 5: Heterogenous treatment effects

Dependent variable: log keystrokes per hour	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Women	Men	High Ability	Low Ability	Experienced	Not Experienced	Piece Rate Experience	No Piece Rate Experience
Treat: Indiv. Piece Rate	0.043	0.030	0.030	0.058	0.040	0.056	-0.002	0.045
	(0.083)	(0.044)	(0.050)	(0.063)	(0.046)	(0.092)	(0.160)	(0.040)
Treat: Group Piece Rate	0.006	0.033	0.058	-0.008	0.025	0.049	-0.064	0.046
	(0.088)	(0.045)	(0.050)	(0.065)	(0.046)	(0.108)	(0.161)	(0.042)
Baseline ability per minute (log scale)	0.111	0.273***	0.140	0.139*	0.197***	0.298***	-0.019	0.274***
	(0.075)	(0.042)	(0.094)	(0.078)	(0.041)	(0.089)	(0.103)	(0.040)
Akan	-0.002	-0.052*	-0.038	-0.012	-0.056*	0.023	-0.032	-0.034
	(0.058)	(0.030)	(0.038)	(0.041)	(0.032)	(0.057)	(0.091)	(0.028)
Data entry experience	-0.104	-0.004	0.001	-0.048			-0.162	0.003
	(0.072)	(0.035)	(0.041)	(0.049)			(0.099)	(0.033)
Piece rate experience	0.109	-0.042	0.007	0.133*	-0.027	0.172**		
	(0.074)	(0.050)	(0.051)	(0.073)	(0.051)	(0.071)		
DEO's first contract	-0.059	-0.150***	-0.103	-0.189**	-0.145**	-0.172	-0.423	-0.144***
	(0.154)	(0.057)	(0.078)	(0.074)	(0.057)	(0.161)	(0.251)	(0.052)
Recognition most imp.	0.083	0.022	0.043	0.034	0.036	0.077	-0.067	0.048*
	(0.060)	(0.030)	(0.036)	(0.040)	(0.031)	(0.062)	(0.090)	(0.027)
Observations	77	174	128	123	193	58	31	220
Adjusted R-squared	0.482	0.655	0.612	0.561	0.583	0.653	0.631	0.617

Notes: *Baseline ability* calculated from log keystrokes per minute in Epidata data entry test during interviews. *Recognition most important* equals 1 if respondent indicates that receiving "recognition for good peformance" on the job is of "greatest importance" (5 on 5-point scale) during pre-employment interview. Standard errors in parentheses. * p<0.1 ** p<0.05 *** p<0.01

Table 6: Mechanisms

Dependent variable: log keystrokes per hour	(1) All Treatments	(2) Team Pay Information	(3) Individual Pay Information
IPR	0.039		0.036
	(0.037)		(0.039)
IPR rank	0.053		0.060
	(0.038)		(0.041)
GPR	0.022	0.018	
	(0.037)	(0.037)	
GPR rank	0.041	0.033	
	(0.038)	(0.038)	
Male	-0.036*	-0.021	-0.051*
	(0.021)	(0.026)	(0.029)
Baseline ability	0.225***	0.230***	0.221***
	(0.026)	(0.035)	(0.036)
Akan	-0.034*	-0.034	-0.038
	(0.019)	(0.024)	(0.026)
Data entry experience	-0.010	-0.039	0.013
	(0.022)	(0.028)	(0.031)
Piece rate experience	0.032	0.033	0.031
	(0.027)	(0.034)	(0.040)
DEO's first contract	-0.169***	-0.162***	-0.160***
	(0.034)	(0.046)	(0.052)
Recognition most imp.	0.025	0.039	0.005
	(0.019)	(0.024)	(0.026)
Observations	452	254	249
Adjusted R-squared	0.594	0.644	0.579

Notes: *Baseline ability* calculated from log keystrokes per minute in Epidata data entry test during interviews. *Recognition most important* equals 1 if respondent indicates that receiving "recognition for good peformance" on the job is of "greatest importance" (5 on 5-point scale) during pre-employment interview. Standard errors in parentheses. * p<0.1 ** p<0.05 *** p<0.01

Table 7: Quality vs. quantity

	(1)	(2)
Dependent variable:	*correct keystrokes per hour*	*log correct keystrokes per hour*
Treat: Indiv. Piece Rate	0.001	0.042
	(0.004)	(0.038)
Treat: Indiv. Piece Rate with Rank	-0.003	0.048
	(0.004)	(0.039)
Treat: Group Piece Rate	0.003	0.017
	(0.004)	(0.039)
Treat: Group Piece Rate with Rank	-0.001	0.039
	(0.004)	(0.039)
Epi Test: Correct Rate	0.043**	0.087
	(0.018)	(0.162)
Male	0.000	-0.035*
	(0.002)	(0.021)
Baseline ability	0.007**	
	(0.003)	
Akan	0.001	-0.035*
	(0.002)	(0.019)
Data entry experience	0.006**	-0.001
	(0.002)	(0.023)
Piece rate experience	-0.009***	0.008
	(0.003)	(0.028)
DEO's first contract	-0.004	-0.166***
	(0.004)	(0.035)
Recognition most imp.	-0.002	0.022
	(0.002)	(0.020)
Baseline accuracy		0.229***
		(0.027)
Observations	452	452
Adjusted R-squared	0.201	0.243

Notes: *Baseline ability* equals from log keystrokes per minute in Epidata data entry test during interviews. *Baseline accuracy* equals log *correct* keystrokes per minute in Epidata data entry test during interviews. *Recognition most important* equals 1 if respondent indicates that receiving "recognition for good peformance" on the job is of "greatest importance" (5 on 5-point scale) during pre-employment interview. Standard errors in parentheses. * p<0.1 ** p<0.05 *** p<0.01

Table 8: Profitability

Dependent variable: estimated profits			
	(1)	(2)	(3)
Profit calculation:	Double Entry	Double Entry @ Recon	Double Entry @ Recon + False Positives
Treat: Indiv. Piece Rate	-0.025	-0.016	-0.015
	(0.041)	(0.034)	(0.035)
Treat: Indiv. Piece Rate with Rank	-0.041	-0.006	-0.007
	(0.042)	(0.035)	(0.035)
Treat: Group Piece Rate	-0.026	-0.031	-0.030
	(0.041)	(0.035)	(0.035)
Treat: Group Piece Rate with Rank	-0.050	-0.028	-0.022
	(0.042)	(0.035)	(0.036)
Male	0.026	0.012	0.009
	(0.023)	(0.019)	(0.019)
Baseline ability	-0.198***	-0.141***	-0.147***
	(0.029)	(0.024)	(0.025)
Akan	0.038*	0.017	0.020
	(0.021)	(0.017)	(0.018)
Data entry experience	-0.004	-0.033	-0.028
	(0.024)	(0.020)	(0.020)
Piece rate experience	-0.029	0.014	0.013
	(0.030)	(0.025)	(0.025)
DEO's first contract	0.132***	0.094***	0.105***
	(0.037)	(0.031)	(0.032)
Recognition most imp.	-0.011	-0.002	-0.002
	(0.021)	(0.018)	(0.018)
Observations	452	452	452
Adjusted R-squared	0.155	0.183	0.195

Notes: See section "Impact: Productivity, Accuracy and Profitability, Profits" for detailed description of profit estimates. *Baseline ability* equals from log keystrokes per minute in Epidata data entry test during interviews. *Baseline accuracy* equals log *correct* keystrokes per minute in Epidata data entry test during interviews. *Recognition most important* equals 1 if respondent indicates that receiving "recognition for good peformance" on the job is of "greatest importance" (5 on 5-point scale) during pre-employment interview. Standard errors in parentheses. * p<0.1 ** p<0.05 *** p<0.01.

Table 9: Interday treatment effects

	(1)	(2)
	Ghana Day 1	Ghana Day 2
Treat: Indiv. Piece Rate	0.013	0.050
	(0.037)	(0.038)
Treat: Indiv. Piece Rate with Rank	0.017	0.072*
	(0.038)	(0.039)
Treat: Group Piece Rate	-0.005	0.046
	(0.038)	(0.039)
Treat: Group Piece Rate with Rank	0.018	0.048
	(0.038)	(0.039)
Male	-0.031	-0.047**
	(0.021)	(0.021)
Baseline ability	0.255***	0.186***
	(0.026)	(0.027)
Akan	-0.028	-0.040**
	(0.019)	(0.020)
Data entry experience	-0.013	-0.023
	(0.022)	(0.023)
Piece rate experience	0.033	0.025
	(0.027)	(0.028)
DEO's first contract	-0.220***	-0.136***
	(0.034)	(0.035)
Recognition most imp.	0.015	0.044**
	(0.019)	(0.020)
Observations	452	441
Adjusted R-squared	0.661	0.521

Notes: *Baseline ability* calculated from log keystrokes per minute in Epidata data entry test during interviews. *Recognition most important* equals 1 if respondent indicates that receiving "recognition for good peformance" on the job is of "greatest importance" (5 on 5-point scale) during pre-employment interview. Standard errors in parentheses. * p<0.1 ** p<0.05 *** p<0.01

APPENDICES

APPENDIX 1: OFFICE SETUP

APPENDIX 2: RECRUITMENT

JOB ADVERTISEMENT: Data Entry Agent

TITLE: DATA ENTRY AGENT

COMPANY: IPA DATA SERVICES

INDUSTRY: BUSINESS SERVICES

CATEGORY: RESEARCH/ANALYSIS

LOCATION: ACCRA

JOB STATUS: TEMPORARY/SHORT TERM CONTRACTS

SALARY: NOT SPECIFIED

EDUCATION: SSS/HND/DEGREE

JOB SPECIFICATION

- ENCODES DATA FROM DOCUMENT TO COMPUTER

- VERIFIES THAT KEYED INFORMATION IS ENTERED ACCURATELY
- PERFORMS OTHER FUNCTIONS AS AND WHEN NECESSARY

REQUIRED SKILLS/ EXPERIENCE

- MINIMUM OF SENIOR HIGH CERTIFICATE/HND OR AN EQUIVALENT PROFESSIONAL QUALIFICATION FROM A RECOGNIZED UNIVERSITY
- KNOWLEDGE IN COMPUTER APPLICATIONS, PARTICULARLY DATA PROGRAMS
- GOOD TYPING SKILLS
- FLUENCY IN ENGLISH LANGUAGE
- DATA ENTRY EXPERIENCE ADVANTAGEOUS BUT NOT NECESSARY

IPA Data Services

Welcome!

This questionnaire is the first step to applying for a job with IPA Data Entry Services. If you would like to be considered further, please make sure to also submit your CV or resume to ipa.ghana.ds@gmail.com

Thank you.

* Required

First Name *

Last Name *

Phone Number *
Please use the format ###-###-#### (for example: 024-000-1234)

Phone Number (confirmation) *
Please repeat the number you entered above. DO NOT enter a second number.

Email Address *

Email Address (confirmation) *

Gender *
- Male
- Female

Are you 18 years of age or older? *
- Yes
- No

Education Level *
Please select the highest level of education that you have ALREADY COMPLETED
- Primary
- JSS
- SSS
- Tertiary: Vocational/Technical/Computer
- Tertiary: University
- Other tertiary
- Koranic Stage
- Other:

Have you ever worked in a data entry job before? *
- Yes
- No

Have you ever had any kind of paid employment before? *
- Yes
- No

Submit

APPENDIX 3: INTERVIEW

Baseline Excel Test

Welcome to the IPA DATA ENTRY TEST! Please enter your ID number and name below	
ID number	
First name and last name	
Please Enter the values in Column C	
Q - 1	
Q - 2	
Q - 3	
Q - 4	
Q - 5	
Q - 6	
Q - 7	
Q - 8	
Q - 9	
Q - 10	
Q - 11	
Q - 12	
Q - 13	
Q - 14	
Q - 15	

Baseline Epidata Test

IPA

HOUSEHOLD CLEAN WATER TECHNOLOGY IN NORTHERN GHANA

WATER SAMPLING AND HEALTH EDUCATION TREATMENT

IPA DATA ENTRY TEST

20100814

****

starttime

startdate

****

id Your ID Number

checkid Please input again your ID Number

apname Your FIRST and LAST name

survnum Survey Number(Number written at the top by hand)

aa1 Survey ID Number

aa2 Respondent Adult ID Number

aa3 Respondent Name(Census Spelling):

aa4 Compound ID Number

aa5 Compound Name

aa6 Village ID Number

aa7 Village

aa8 District

aa9 Assignment

Date of Visit

aa10day aa10month aa10year


IPA Data Entry Services

Interview Questionnaire

Welcome | Section A | Section B | Section C - Part 1 | Section C - Part 2 | Section C - Part 3

Demographic Information

Postal Address

Home Address

Date of Birth

For example: 03-Mar-77

Marital Status

Single

Married

Divorced

Widowed

Place of Birth (city, region, country)

Ethnicity/Tribe

Religion

First Language

Other Languages Spoken

Previous Employment Information

If you have never had a job before, please leave this section blank. If you have only had one job before this one, answer the section for "Job 1" and leave the rest blank. If you have had 2 jobs before this, also fill out the section for "Job 2", etc.

Job 1

How were you paid at your last job?
- Flat wage
- Piece rate

Was this a data entry position?
- Yes
- No

Job 2

How were you paid at your second to last job?
- Flat wage
- Piece rate

Was this a data entry position?
- Yes
- No

Job3

How were you paid at your third to last job?
- Flat wage
- Piece rate

Was this a data entry position?
- Yes
- No

Directions: For the following set of questions, please use the scales below to indicate your responses.

How important is it to you to...	No or very little importance	Little importance	Moderate importance	Very important	Greatest importance to me
C.1 Have challenging work to do--work from which you can get a personal sense of accomplishment	○	○	○	○	○
C.2 Live in a desirable area	○	○	○	○	○
C.3 Have an opportunity for high earnings?	○	○	○	○	○
C.4 Work with people who cooperate well with one another?	○	○	○	○	○
C.5 Have training opportunities (to improve your skills or to learn new skills)?	○	○	○	○	○
C.6 Have good fringe benefits?	○	○	○	○	○
C.7 Get recognition for good performance	○	○	○	○	○
C.8 Have good physical working conditions (good ventilation and lighting, adequate work space, etc.)?	○	○	○	○	○
C.9 Have considerable freedom to adopt your own approach to the job?	○	○	○	○	○

Directions: For the following set of questions, please use the scales below to indicate your responses.

How important is it to you to...	No or very little importance	Little importance	Moderate importance	Very important	Greatest importance to me
C.10 Have security of employment	○	○	○	○	○
C.11 Have chances for promotion or advancement	○	○	○	○	○
C.12 Have a good working relationship with your manager?	○	○	○	○	○
C.13 Fully use your skills and abilities on the job?	○	○	○	○	○
C.14 Have sufficient time for your personal or home life	○	○	○	○	○
C.15 Have a boss (direct superior) you can respect?	○	○	○	○	○
C.16 Have pleasant people to work with?	○	○	○	○	○
C.17 Do work that is interesting?	○	○	○	○	○
C.18 Be consulted by your boss in decisions involving your work?	○	○	○	○	○
C.19 Have a job respected by your family and friends?	○	○	○	○	○

Directions: For the following set of questions, please use the scales below to indicate your responses.

	Never	Seldom	Sometimes	Often	Always
C20. How often, in your experience, are subordinates afraid to contradict their boss (or students their teacher)?	○	○	○	○	○

	Strongly Disagree	Disagree	Undecided	Agree	Strongly Agree
C21. An organization structure in which certain subordinates have two bosses should be avoided	○	○	○	○	○

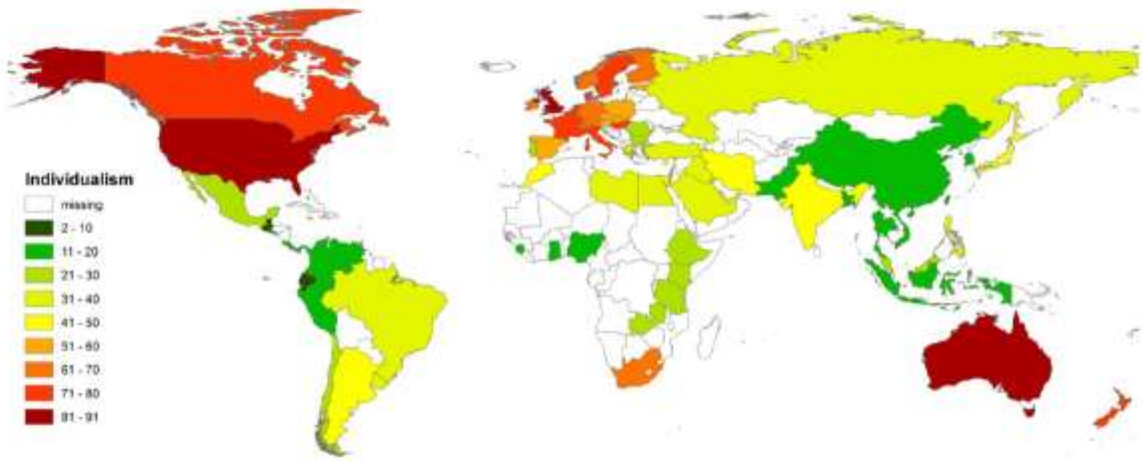
APPENDIX 4: CONTRACT DESCRIPTION

Individual Piece Rate

You will be paid based on the amount of data you enter. The contract states that your wage will equal .00107 Cedis per keystroke. The more data you enter over the course of the 2 day contract, the higher your final wage will be. For example, if you enter 60,000 keystrokes over the course of the 2 day contract, you will be paid 60,000 * .00107 Cedis, which equals 64.2 Cedis. Your wage will vary based on the amount of data you enter, so you may receive more or less than 50 Cedis for the 2 day contract.

APPENDIX 5: HOFSTEDE INDIVIDUALISM MAP

Appendix 5: Hofstede's (2001) measure of individualism



APPENDIX TABLES

Table A1: Abnormal hours

	(1) Unconditional	(2) Controls	(3) Month FE	(4) Survey FE	(5) All Treatments
Treat: Indiv. Piece Rate	0.053	0.046	0.012	0.012	0.015
	(0.049)	(0.051)	(0.056)	(0.056)	(0.052)
Treat: Group Piece Rate	0.020	0.018	-0.006	-0.006	-0.020
	(0.049)	(0.050)	(0.058)	(0.058)	(0.052)
Gender (=1 if male)		-0.020	-0.019	-0.019	-0.007
		(0.040)	(0.040)	(0.040)	(0.029)
Baseline ability per minute (log scale)		0.024	0.020	0.020	-0.019
		(0.050)	(0.052)	(0.052)	(0.037)
Akan (=1 if akan)		0.048	0.048	0.048	0.023
		(0.037)	(0.038)	(0.038)	(0.026)
Data entry experience (=1 if yes)		-0.018	-0.028	-0.028	-0.005
		(0.044)	(0.045)	(0.045)	(0.030)
Piece rate experience (=1 if yes)		-0.037	-0.020	-0.020	0.023
		(0.056)	(0.057)	(0.057)	(0.038)
DEO's first contract		0.011	0.070	0.070	0.038
		(0.038)	(0.075)	(0.075)	(0.047)
Recognition most imp.		0.044	0.047	0.047	0.055**
		(0.037)	(0.038)	(0.038)	(0.026)
Treat: Indiv. Piece Rate with Rank					-0.029
					(0.053)
Treat: Group Piece Rate with Rank					-0.053
					(0.053)
Constant	0.059	-0.005	-0.104	-0.104	-0.024
	(0.040)	(0.124)	(0.172)	(0.172)	(0.135)
Observations	251	251	251	251	452
Adjusted R-squared	-0.003	-0.011	-0.018	-0.018	0.015

Standard errors in parentheses. * p<0.1 ** p<0.05 *** p<0.01

Table A2: Baseline ability

	Flat Wage mean/sd	IPR mean/sd	GPR mean/sd	IPR Rank mean/sd	GPR Rank mean/sd	Total mean/sd
Baseline ability	2.08	2.10	2.11	2.04	2.12	2.09
	(0.36)	(0.40)	(0.33)	(0.38)	(0.34)	(0.36)
Baseline accuracy	2.00	2.03	2.05	1.96	2.06	2.02
	(0.36)	(0.41)	(0.34)	(0.38)	(0.35)	(0.37)
Observations	51	98	102	100	101	452

Notes: *Baseline ability* calculated from log keystrokes per minute in Epidata data entry test during interviews. *Baseline accuracy* adjusts abilty for errors.

Table A3: Endline performance detail by contract

Prouctivity measure (logs)	Flat Wage mean/sd	IPR mean/sd	GPR mean/sd	IPR Rank mean/sd	GPR Rank mean/sd	Total mean/sd
Endline key strokes/hr	8.38	8.40	8.38	8.42	8.42	8.40
	(0.32)	(0.31)	(0.32)	(0.31)	(0.28)	(0.31)
Endline accurate key strokes/hr	6.79	6.82	6.81	6.81	6.83	6.82
	(0.19)	(0.25)	(0.24)	(0.25)	(0.20)	(0.23)
Cost per entry	-3.71	-3.72	-3.74	-3.70	-3.73	-3.72
	(0.20)	(0.24)	(0.19)	(0.20)	(0.18)	(0.20)
Endline key strokes/hr day 1	8.29	8.28	8.26	8.28	8.30	8.28
	(0.35)	(0.35)	(0.35)	(0.34)	(0.31)	(0.34)
Endline key strokes/hr day 2	8.47	8.51	8.51	8.54	8.52	8.51
	(0.31)	(0.29)	(0.30)	(0.29)	(0.27)	(0.29)
Observations	51	98	102	100	101	452

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