Institutions, Norms and Worker Decision Making

This pre-analysis plan (PAP) is intended as an addendum to a previously submitted PAP looking at worker conditions and productivity and adherence to rules in Tanzania. Data collection / entering for an additional field-based replication in Burundi of some aspects of the Tanzania lab-in-the-field study is ongoing. The key motivation, and questions being examined in this supplemental empirical exercise remains similar to the main pre-registration submitted pre-covid.

Pre-analysis Plan

1. Introduction

    In late Summer 2019 and early Fall, the research team conducted several pilots, surveys and implemented lab-in-the-field experiments in Arusha, Tanzania. We were able to collect most of the lab-based data prior to the COVID-19 pandemic. However, we were unable to conduct a field-portion of the project due to delays resulting from the pandemic.

    In summer-fall 2023, we signed a contract with a survey and data entry firm located in Bujumbura, Burundi with the aim of finishing all data collection and any follow up by early summer 2024. The firm has allowed us some limited control of the working environment that has allowed us to randomize some operations, as well as permission to collect individual level worker data on their data entry operations. This will enable examination of some of the key economic hypotheses in more of a field-context. In addition, this will enable us to examine the extent to which the results from our lab-based work completed in Tanzania in 2019, translate over to a different country context. Accordingly, our primary objective is to re-examine the impact of incentives on productivity and corruption in the context of a real firm.

    As with the project in Tanzania, in Burundi the aim is to randomly assign workers to either a high trust or a low trust group. In the high trust group, managers will deliver a message that the management group trusts workers to follow the rules, while in the low-trust group managers deliver a more typical message that monitoring is ongoing, and that rule-breaking will be punished.

    Given that we are using the offices of a firm, our partner organization has imposed some constraints in terms of what we can and cannot do. These restrictions have resulted in some deviations from our lab-in-the-field exercise that we conducted in Tanzania. First, we cannot, for instance implement a pre/post survey with workers to illicit private honesty. Second, aspects of worker recruitment by our partner organization were largely out of our control. While this has implications for the empirical execution, we aim to, as close as we can, implement a field-version of the lab-in-the-field results already collected in Tanzania.

2. Experimental Design

Recruiting: The firm with whom we are partnering, recruits temporary employees from a list that they have compiled, of office workers in Bujumbura, Burundi. The recruitment list is compiled from two main sources. First, is the Commune administrators (*mustanberi*). These

administrators (of administrative sub-units in Bujumbura) maintain lists of employees looking for work, along with some basic information like their education levels and contact details. Prospective employs can register for these lists in the neighborhoods that they live in. Second, the firm also uses WhatsApp groups to recruit potential workers in Bujumbura. When the firm is looking to recruit temporary workers, they will often post on the group what they are looking for, and ask workers to contact a manager, who will choose from the administrative list and the WhatsApp group list. The implication of this recruitment strategy is that for any given recruitment campaign, the workers are often geographically clustered, coming from few neighborhoods, especially if recruitment relied more heavily on the administrative neighborhood lists. We plan to flexibly control for group composition to account for this, which is different from what we had pre-registered for the Tanzania analysis.

Task: When workers arrive, they complete paperwork for a project that the firm was hired to complete. The paperwork involves assessing health risk scores based on medical indicators. They must look up the indicator against a table that relates ranges on various values to risk scores, and then tally-up the scores to record an overall risk assessment.

Experimental Context: We have been given permission by our partner organization to alter *three* aspects of the work conditions.

First, is that we are allowed to vary the pay and the allocation of forms across workers. In particular, this allocation of high or low rewards based on a dice roll by a supervisor.

Second, managers read a different script at the start of each half-day. Typically, a manager reinforces the process and rules, reviews any questions, or concerns both at the start of the day, and at the mid-way point. While this protocol is to be followed in most treatments, there is one exception. In particular, in the high-trust treatment the message of worker trust is to be reinforced. So the message of trust is to be reiterated instead of once again describing the rules and the punitive consequences for violating them.

Third, we can, for some subset of workers, vary supervisor rotations. The idea behind this is that when supervisors rotate frequently, taking worker-supervisors out of a repeated game may reduce cheating. This is because we expect any 'kickbacks' given to supervisors to be difficult to enforce when supervisors are not consistent, day-to-day. For instance, when a worker is paid after day 1, if they do not share the agreed upon amount to the supervisor, the supervisor can discipline the worker on day 2 given their power over form allocation. However, when supervisors rotate, that's not the case. Furthermore, supervisor rotations remove the opportunity for supervisors and workers to organize side-agreements. If neither treatment nor control groups are cheating in any case, then we expect the high trust treatment to have less scope to work as anticipated.

3. Outcomes

3.1 Primary Outcomes

We have high wage and low wage forms that workers can complete. When a worker rolls doubles, they receive a high wage form, so this should – in the absence of cheating - happen 1/6th of the time.

Our two main outcomes, consistent with the Tanzania lab-in-the-field experiment are the reported share of doubles rolled by any worker, and the implied probability of cheating based on the total rolls and the doubles rolled.[1]

3.2 Secondary Outcomes

We may also examine some secondary outcomes that are a function of different economic/institutional environments. The two main measures we will be able to examine are quality and productivity. Quality can be measured by the degree of inaccuracy in the data-entry task completed the workers. There are a lot of forms generated by the task, so we will collect, by hand, all information on accuracy and productivity for a subset of workers. If feasible, we may also try to scan and OCR the forms. This will depend on the success of the OCR routine, which tends to depend on quality and legibility of handwriting. We need to wait to see how the forms are completed before we assess whether this is feasible. The firm has several layers of quality control in place, so we do not expect to find much variation in accuracy. This differs from our expectations in Tanzania, where there was one layer of quality control, by an experiment subject, who was more plausibly subject to peer-influence.

Similarly, we can examine productivity, which is recorded by the firm in any case, so this can be assessed with some certainty for the whole sample. We intend to simply examine the count of forms completed, or the log of the count, depending on what the distribution looks like. In any case, the plan is to run regressions using both measures, but which one ends up being the "preferred" measure will depend on which seems more appropriate based on the distribution.

3.3 Other Outcomes

We are also interested in examining whether ethnic composition of teams (including ethnicities of supervisor-worker) affects corruption and productivity. Despite not being a central focus of our analysis, we investigate this given that we are conducting our intervention in Burundi – a country with a long history of conflict across the two main ethnicities - the Hutus and the Tutsis. However, the strength of these ethnic effects (if they exist at all) may be quite muted given that our firm is located in urban Bujumbura.

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[1] That probability is calculated as follows:

$$D_i = 1 - \sum \binom{R_i}{d_i} p^d (1-p)^{R_i - d_i}$$

Where D_i is the dishonesty of worker i. R_i is the number of rolls a worker completes and d_i is the number of doubles. p is the probability of rolling doubles (in this case 1/6).

Moreover, we also intend to investigate the balance of our treatments, and plan to run regressions that include the treatments as outcomes to determine whether our various treatments are correlated with observables.

4. Estimation Strategy

Our planed estimation strategy is straightforward. We plan to control for various observables in order to reduce residual variance, but otherwise the estimation is simple due to the randomized nature of the various treatments. In general, we plan the following as our main estimating equation:

$$Outcome_{it} = \beta_0 + \beta_1 T_i + \Gamma X_{it} + \epsilon_{it}$$

The outcomes and treatment (*T*) are as described above. We plan to include several controls in X, including age, gender and ethnicity of both the worker and the supervisor. Age will be included quadratically, as is standard in the literature. We also intend to control for supervisor-worker matches on each of these factors, to account for the possibility of discrimination based on observable characteristics.

In addition, we intend to control for the education of respondents – this is a main variable on which the firm recruits. We also plan to wait and see if we have enough variation within neighbourhoods to include a neighbourhood fixed effect. Given that recruitment-campaigns are typically heavily within neighbourhood, but we don't yet know how heavily, so it is not yet clear if this is a good idea. We will also control for how many days a worker has been working on the project to account for some learning as time passes.

Because recruitment is heavily neighborhood specific, we also intend to interact each of these controls with one of the four recruitment campaigns we plan to study (similar in nature to Cornelissen et al, 2017). These are expected to potentially have a significant effect on the composition of the workforce on a given day, and we want to account for the fact that, for instance, being Hutu on a day where recruitment was predominantly from a Tutsi neighborhood (and where a large share of other workers are Tutsi) may have be much different from being Hutu on a day where recruitment was predominantly from a Hutu neighborhood. This wasn't a concern in Tanzania where we had more control over the recruiting process, but our discussions with the firm suggest that (a) neighborhoods tend to contain ethnic clusters; (b) accordingly, they often have highly varying and non-representative groups based on how they recruit; (c) these compositional factors may have a role in affecting productivity / socialization.

References:

Cornelissen, Thomas, Christian Dustmann and Uta Schonberg (2017), "Peer Effects in the Workplace." American Economic Review, 107(2), 425-456.