The Role of Career and Wage Incentives in Labor Productivity:

Evidence from a Two-stage Field Experiment in Malawi*

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3 May 2019

Abstract

We study how career and wage incentives affect labor productivity through self-selection and incentive effect channels using a two-stage field experiment in Malawi. First, recent secondary school graduates were hired with either career or wage incentives. After employment, a half of workers with career incentives randomly received wage incentives, and a half of workers with wage incentives randomly received career incentives. Career incentives attract higher-performing workers than wage incentives, but do not increase productivity conditional on selection. Wage incentives increase productivity for those recruited through career incentives. Observable characteristics are limited in explaining selection effects of entry-level workers.

Keywords: Career Incentive, Wage Incentive, Internship, Self-selection, Labor Productivity

JEL Classification: J30, O15, M52

* We are grateful to the following staff members of Africa Future Foundation for their field assistance: Narshil Choi, Jungeun Kim, Seungchul Lee, Hanyoun So, and Gi Sun Yang. We also thank Derek Lougee for excellent research assistance. In addition, we thank Syngjoo Choi, Andrew Foster, Dan Hamermesh, Guojun He, Kohei Kawaguchi, Asim Khwaja, Etienne Lalé, Kevin Lang, Suejin Lee, Pauline Leung, Zhuan Pei, Cristian Pop-Eleches, Victoria Prowse, Imran Rasul, Nick Sanders, Slesh Shrestha, and Armand Sim as well as seminar participants at Cornell University, Hitotsubashi University, Hanyang University, Korea Development Institute, National University of Singapore, Singapore Management University, Seoul National University, NEUDC 2016, SJE International Conference on Human Capital and Economic Development, First IZA Junior/Senior Labor Symposium, IZA/OECD/World Bank/UCW Workshop on Job Quality in Post-transition, Emerging and Developing Countries, and UNU-WIDER Conference on Human Capital and Growth for their valuable comments. This research was supported by the Singapore Ministry of Education (MOE) Academic Research Fund (AcRF) Tier 1 grant. All errors are our own.
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1. Introduction

Work incentives are essential tools to improve labor productivity. Firms try to recruit productive workers and motivate existing employees to exert more effort through work incentives. Career incentives (tenure and promotion) and financial incentives (higher wage, cash bonus, and employee stock option) are common examples of work incentives. There are two channels through which work incentives can affect labor productivity: *selection* and *incentive effects*.[1] A better understanding of how different incentives affect labor productivity would enable firms to design optimal hiring and compensation strategies that maximize labor productivity and reduce the need for costly screening processes.

We provide experimental evidence on how career and wage incentives affect labor productivity through self-selection and incentive effect channels. We conduct a two-stage randomized controlled trial to separately isolate the selection and incentive effects of these incentives in collaboration with Africa Future Foundation (AFF), an international non-governmental organization (NGO), in the context of a recruitment drive for entry-level enumerators for a population census survey in rural Malawi.[2]

The career incentives we study consist of a future job prospect and a recommendation letter,

[1] The *incentive* effect refers to the difference in labor productivity when incentives affect performance holding employee composition constant. The *selection* effect refers to the difference in labor productivity driven by workers' self-selection into the job.

[2] Our two-stage experimental design is similar to recent experimental studies in development economics (Karlan and Zinman, 2009; Ashraf et al., 2010; Cohen and Dupas, 2010; Beaman et al., 2015). In terms of the research context, Beaman et al. (2018) also use a recruitment drive for survey enumerators when they examine whether and how much women have (dis)advantages via the use of job referrals to understand gender disparities.

which are typical benefits of an internship position.[3] The wage incentives in our study are composed of a lump-sum salary and performance-related bonus payment. Firms might expect that career incentives attract workers more forward-looking and/or risk-loving than others because an internship position implies taking the risk of not being employed at the end of the internship. On the other hand, firms might expect that wage incentives attract workers more extrinsically motivated by monetary compensation.

Our research setting, the recruitment of entry-level enumerators in Malawi, is suitable to study the role of work incentives in productivity because we are able to measure high frequency individual-level labor productivity. The nature of an enumerator job is multidimensional because enumerators are expected to conduct interviews both quickly and accurately. Thus, we measure job performance by the number of surveys conducted per day (survey quantity) and the proportion of errors/mistakes made in a survey (survey quality). In addition, our setting has advantages to study the role of work incentives especially in worker self-selection. Worker screening in developing countries is difficult because observable information on worker skills such as certification, accreditation, and the past work history are limited. It is even more challenging to observe the productivity of entry-level workers due to no or short work history.

To hire enumerators, AFF approached 440 randomly selected recent high school graduates in its project areas. As shown in Figure 1, in the first stage, study subjects were randomly assigned to one of the two groups: (i) those who received a job offer with career incentives (hereafter the *Internship* group) and (ii) those who received a job offer with wage incentives (hereafter the *Wage*

[3] An internship is a temporary position that can be paid or unpaid, and is distinguished from a short-term job in that it emphasizes on-the-job training for students or entry-level workers. According to a 2011 survey of the US-based National Association of Colleges and Employers, more than 50% of graduating college students had internship experiences (Nunley et al., 2016). Internship programs are also widely available in Malawi in the public, private, and NGO sectors. For example, about 20% of regular workers in AFF are hired through the internship program.

group). Those assigned to the *Internship* group received an internship opportunity that comes with (a) a potential long-term employment opportunity at AFF as a regular employee and (b) a recommendation letter specifying their job performance.[4] A one-time temporary work opportunity with a lump-sum wage and a bonus payment based on job performance was offered to those assigned to the *Wage* group.

Individuals who accepted the job opportunity in the first stage proceeded to enumerator training and the second-stage randomization. After completing the training, a randomly selected half of the job takers in the *Internship* group additionally received the same wage incentives of the *Wage* group without prior notice. In the same manner, a randomly selected half of the job takers in the *Wage* group additionally received the same career incentives of the *Internship* group without prior notice. As a result, this research design creates four sub-groups: *Group 1 (G1)* and *Group 2 (G2)* became enumerators through career incentives, but only *G2* received additional wage incentives. Similarly, *Group 3 (G3)* and *Group 4 (G4)* became enumerators through wage incentives, but only *G3* received additional career incentives.

We isolate the selection effect on labor productivity by comparing *G2* and *G3*, both of which have identical incentives (both career and wage incentives) during the work period. However, the channels through which they were attracted to the job are different.[5] Our identifying assumption of the selection effect is that sequences in which first-stage and second-stage incentives are presented to G2 and G3 participants are independent of the combined value of the career and

[4] An entry-level regular position (enumerator or data entry clerk) at AFF has career advancement prospects that lead to more advanced positions, such as head enumerator, junior project assistant, senior project assistant, and project manager. AFF did not explicitly state the actual probability of being hired to the *Internship* group. We acknowledge that changing probabilities of being hired after the internship might affect effort levels, but we do not compare different levels of the same incentive, but rather two different types of incentives.

[5] The comparison of *G2* and *G3* can be also interpreted as the selection effect of the wage incentives evaluated against the career incentives, but for the sake of convenience, we focus on the career incentives.

wage incentives. This assumption is required both in the conceptual framework (Section 3.2) and the empirical analysis (Section 5.3). We discuss the reliability of this assumption with further details in Section 5.3.

In addition, we estimate the incentive effects of wage incentives (henceforth, wage incentive effects) on job performance among the job takers in the *Internship* group by comparing *G1* and *G2*. Both groups became enumerators through the career incentives, but only *G2* received additional wage incentives. Hence, any difference in performance between *G1* and *G2* can be interpreted as wage incentive effects among the job takers in the *Internship* group. Similarly, we estimate the incentive effects of career incentives (henceforth, career incentive effects) on job performance among the job takers in the *Wage* group by comparing *G3* and *G4*. Any difference in performance between *G3* and *G4* can be interpreted as career incentive effects among the job takers in the *Wage* group.[6]

Of 440 randomly selected recent male high school graduates whom AFF approached for the baseline survey of this study without prior notice of job opportunity, 362 (82.3%) participated in the baseline survey.[7] Of 176 study participants assigned to the *Wage* group, 74 (42.0%) accepted a job offer by joining the training session. Of 186 study participants assigned to the *Internship* group, 74 (39.8%) took up the job offer. Of 148 trainees, 11 dropped out from the training. As a result, 137 enumerators worked in the field for an average of 18 days interviewing 21,561 households.[8]

[6] It is noteworthy that the estimated career and wage incentive effects are local treatment effects upon the samples of job takers recruited through wage and career incentives in the first stage, respectively.

[7] There were 536 eligible study subjects who were male and recent high school graduates in AFF's project areas. Of the 536, AFF provided job offers to a randomly selected group of 440. The other 96 subjects were also invited to participate in the baseline survey, although they did not receive a job offer. Individual characteristics and the balance between the two groups (440 vs. 96) are shown in Table A.1.

[8] Throughout this paper, *target study participants* refer to the 440 individuals who were invited to participate in the baseline survey, *study participants* refer to the 362 individuals who participated in the baseline survey; *trainees (job*

We reach four main conclusions using data on labor productivity measured by survey quality and survey quantity. First, we find that career incentives, compared to wage incentives, attract workers with higher labor productivity through the self-selection mechanism. Second, we find that the incentive effects of career incentives among those recruited by wage incentives are limited in improving labor productivity. Third, we find that wage incentives causally increase labor productivity among those recruited through career incentives. As a result, overall job performance is highest among *G2* enumerators who were hired through the career incentive channel and additionally received wage incentives. Lastly, we find that observable individual characteristics are limited in explaining the selection effect of entry-level workers, suggesting a limitation of screening based on observable characteristics and a need for a self-selection mechanism that can attract productive workers with desirable (unobserved) characteristics.

Our primary contribution to the literature is that we study career and wage incentives, the most common types of work incentives, jointly in the same setting, and provide real-world evidence on how these incentives affect labor productivity by identifying the selection and incentive effect channels through two-stage randomization.

Previous studies estimating the selection and incentive effects separately focus only on financial incentives (Lazear, 2000; Gagliarducci and Nannicini, 2013; Guiteras and Jack, 2018). Moreover, their findings on relative importance of selection and incentive effects are mixed. For example, Lazear (2000) isolates worker selection and incentive effects of pay-for-performance using non-experimental panel data on job performance from a large manufacturing factory in the US. He shows evidence that the change to piece rate pay increases labor productivity by 44% with

takers) refer to the 148 individuals who joined the training; and *enumerators* refer to the 137 individuals who worked in the field.

a half of it coming from the selection effect and the other half comes from the incentive effect. Gagliarducci and Nannicini (2013) also identify the selection and incentive effects of wage incentives on the performance of politicians by exploiting policies that discontinuously change their salaries and limit political terms. They find that a higher wage attracts more educated candidates and leads to improved efficiency of public finance through the selection channel. By contrast, Guiteras and Jack (2018) find evidence from bean-sorting workers in rural Malawi that a higher piece rate increases productivity only through the incentive effect channel, not through the worker selection channel. Our results are consistent with Lazear's (2000) findings that both selection and incentive effects are important.

There are several studies focusing on the selection effects of work incentives. Dohmen and Falk (2011) show that sorting of workers largely explains higher labor productivity under a variable-payment scheme compared to a fixed-payment scheme in a laboratory experiment setting. Dal Bó et al. (2013) show that a higher wage attracts more qualified applicants without the cost of losing workers with strong public service motivation in a recruitment drive for Mexico's public sector workers. Ashraf et al. (2016) similarly show that salient career incentives attract more productive workers without discouraging those with pro-social preferences from applying for a job in a recruitment drive for community health workers in Zambia. On the other hand, Deserranno (2018) finds that the expectation of a higher salary for a newly created health-promoter position discourages job applications from socially motivated candidates in Uganda. While the previous literature estimated selection effects of either financial incentives or career incentives, we estimate selection effects of career incentives evaluated against wage incentives.

In addition, our study is related to another strand of the literature on incentive effects on

job performance.[9] The previous literature mainly focuses on financial incentives, to the best of our knowledge (Gneezy and List, 2006; Shearer, 2004; Glewwe et al., 2010; Duflo et al., 2012; Fryer, 2013; Ashraf et al., 2014). For example, Gneezy and List (2006) empirically test the gift exchange theory developed by Akerlof (1984) and show that workers exert more efforts when they receive a financial incentive ("gift") from their employers. Shearer (2004) presents experimental evidence from Canadian tree planters that piece rates induce more effort than do fixed wages. By contrast, ours is the first of its kind to estimate career incentive effects.

Lastly, our study is related to the literature on internships. Most existing studies on internships are descriptive (Brooks et al., 1995; D'Abate et al., 2009; Liu et al., 2014). A rare exception is Nunley et al. (2016), which sends out fake résumés with randomly changed characteristics of applicants. They find that a résumé with internship experience receives 14% more callbacks from potential employers. However, a major limitation of the résumé audit study is lack of job performance data. Since career incentives in this study closely follow the structure of an (unpaid) internship program in the real world, this study offers experimental evidence on the effects of an internship on worker selection and job performance.

The remainder of the paper is structured as follows: Section 2 outlines the research context and design. Section 3 presents the conceptual framework. Section 4 describes the data and reports sample statistics. Section 5 presents the main results on labor productivity and discusses the findings. Section 6 concludes.

[9] Bandiera et al. (2011) and Oyer and Schaefer (2011) provide excellent surveys of the literature.

2. Research Context and Design

2.1. Research Context

Malawi is one of the least developed countries in the world with GDP per capita in 2015 of US$382 (World Bank, 2016). Among 20–29 years old males, 19.6% completed secondary school education according to the 2010 Malawi Demographic and Health Survey. Employment in the official sector is 11% and the median monthly income is US$28.8 (13,420 MWK) (National Statistical Office of Malawi, 2014).[10]

AFF conducted a district-wide population census of Chimutu, a rural district located outside of the capital city of Malawi, in January 2015. Chimutu district consists of 52 catchment areas with about 94,000 people (around 24,000 households). AFF planned to complete a census within a month by hiring more than 130 enumerators.

The enumerator position could be an attractive starting job for entry-level young workers because it offers a competitive salary and confers career-advancing incentives. For example, AFF's many regular staff members were initially recruited as enumerators. The role of the census enumerators was to interview household heads to collect basic demographic, socioeconomic, and health information. During the census period, enumerators stayed at a house in the assigned catchment area rented by AFF. Since enumerators interviewed many residents in remote villages to collect a variety of personal and complex information, the job required both cognitive and interpersonal skills as well as physical endurance.

Study participants to whom AFF offered the enumerator job were drawn from the sample of individuals who participated in the 2011 secondary school student survey in four districts in

[10] MWK denotes Malawi Kwacha. As of January 1, 2015, US$1 was equivalent to 466 MWK. Throughout the paper, we use this as the currency exchange rate.

Malawi, including Chimutu. This 2011 survey was a baseline survey for AFF's previous research program that randomly provided HIV/AIDS education, male circumcision, and financial support for female education in their catchment areas.[11] Of the 536 males who participated in the 2011 secondary school survey and graduated from secondary school in July 2014, AFF randomly selected 440 as target study participants. 362 study participants participated in the survey (i.e., the baseline survey of this study) without prior notice of a potential job offer. This sample recruitment approach allowed AFF to hire workers familiar with the census area. AFF considered only males due to security concerns in the field. In addition, AFF required secondary school graduation as proof of minimum cognitive skill requirements.

Outside options for the enumerator job are other formal sector jobs, household farming, and repeating secondary school. For instance, at the time of the baseline survey, 4.7% of our study participants were working for pay in formal sectors, 4.3% were working for their family business (mainly farming), and 15.8% were attending vocational schools or colleges.[12] About 60% were actively searching for jobs.

Our sample recruitment strategy has two advantages. First, we observe the population of a young cohort whose members are potentially interested in a job opportunity in the local labor market, contrary to existing studies that observe only job applicants. This feature of our sampling allows our findings to have greater external validity by addressing the concern that individual characteristics of job applicants may be systematically different from those of non-applicants. For

[11] AFF's catchment areas include the following four districts: Chimutu, Chitukula, Tsbango, and Kalumba. For details of AFF programs, see Data Appendix A.4.

[12] Repeating the final year of the secondary school to improve their secondary school graduation exam scores is common in Malawi because the graduation exam score plays an important role in signaling the job candidate's ability. Thus, it could be an important alternative to working for AFF as a short-term enumerator. In the baseline survey, 1.3% of study participants were repeating the final year of the secondary school, even though they had already graduated. This number increases to 27% in the follow-up survey implemented one year after the experiment.

example, applicants could be more likely to possess the necessary skills, have better access to the information (at least for a job vacancy), and/or be less likely to be happy with their existing positions if they are currently working for another employer. Hence, the estimation of selection effects of any work incentives is inherently local to job applicants. Second, approaching those who just graduated from secondary school is relevant to an internship, which mainly targets young and entry-level workers.

2.2. Experimental Design

2.2.1. Baseline survey and first-stage randomization

We describe the research stages in chronological order as shown in Table 1. As stated in the introduction, AFF invited 440 males who met the eligibility criteria (target study participants) for the baseline survey (Row A) and 362 (82.3%) participated in the baseline survey (Row B).[13] In addition, AFF invited study participants soon after the census was completed between April and June 2015 to measure time and risk preferences and rational decision-making ability.[14]

To minimize unexpected interaction among workers with different incentives, first-stage randomization was performed in advance, and the baseline survey and training were also conducted separately for the *Internship* group and the *Wage* group. Study participants were given a job offer with detailed information on an enumerator position at the end of the baseline survey.[15]

[13] Those who did not participate in the survey were unreachable (45%), refused to participate (13%), or could not participate in the survey because they were at school (32%) or working (10%).

[14] This survey was conducted to measure time and risk preferences and rational decision-making ability after the census was completed under the assumption that these measures are not affected by our interventions. Out of 440 target study participants, 334 (76%) participated in the survey. We further discuss the data collected from these surveys in Section 4.

[15] A job offer was valid conditional on successful completion of the training. We refer to a conditional job offer simply as a job offer henceforth.

Study participants were not aware of the other type of incentives when they received an offer.[16]

Of 220 target study participants assigned to the *Wage* group, 176 (80.0%) showed up for the baseline survey (Row B) and were given a short-term (verbal) job offer, each with a fixed salary of 10,000 MWK (US$21.5) for up to 30 days and performance pay of 500 MWK (US$1.1) for every extra 8 households after the first 160 households.[17] Of 220 target study participants assigned to the *Internship* group, 186 (84.5%) showed up for the baseline survey (Row B) and were given a (verbal) job offer with career incentives which consist of a recommendation letter and the prospect of working at AFF as a regular staff member.

The base wage of 10,000 MWK (US$21.5) was competitive for young workers who had just graduated from secondary schools because the median monthly salary of secondary school graduates in 2013 was 12,000 MWK (US$25.8), according to the Malawi Labor Force Survey (NSO, 2014).[18] AFF notified the *Internship* group that there would be a chance of a long-term contract, without specifying the precise probability, depending on job performance during the contract period and AFF's job vacancies. Working as an intern without knowing the exact probability of hiring is close to the general internship setting. Lastly, one-time transportation support, on average about 1,500 MWK (US$3.2), was given to both *Wage* and *Internship* groups

[16] At this stage, the study participants did not know the details of the contract and incentive structure of the other study group, as AFF did not tell the study participants about those with the other incentives. Even though we cannot fully exclude possible interaction between study groups, it is unlikely that study participants in different study groups actively interact with each other, because only about 14 people per school (440 from 31 schools) were invited for the study.

[17] This rule gives an impression to enumerators that surveying 160 households is the *de facto* expectation of good performance. We acknowledge that this reference could increase or decrease average survey completion, but having a specific rule or a cut-off point about performance is unavoidable if an organization has to offer rule-based performance pay.

[18] The prospect of a regular entry-level staff position at AFF whose entry-level monthly salary is 26,000 MWK (US$55.8) could be attractive. In addition, those promoted to a project manager position at AFF were paid between US$100 and US$160 (46,600 MWK and 74,560 MWK) per month during the study time.

depending on the distance from the worker's home and the dispatched village.

2.2.2. Training

Those who took the job offer were required to participate in a 1-week training program in January 2015. It was designed to equip trainees with the necessary skills and knowledge for the census work. The training outcomes were measured by a quiz score and the proportion of erroneous entries in a practice survey. To prevent interaction between participants with different incentives, the *Internship* group (the first week) and *Wage* group (the second week) joined the training sessions separately, but the instructors and the training materials were identical.

Out of the 186 study participants in the *Internship* group, 74 (39.8%) participated in the training session, as did 74 out of 176 (42%) study participants in the *Wage* group (Row C). The job take-up rates (training participation rates) between the *Internship* group and the *Wage* group were not statistically different. However, 11 trainees from the *Internship* group were not hired because of low training performance, while no one failed from the *Wage* group (Row D). In total, 137 enumerators were finally hired, 63 of which were from the *Internship* group and 74 from the *Wage* group (Row E). As a result, we do not observe job performance of 11 trainees from the *Internship* group who failed the training requirement.[19]

2.2.3. Second-stage randomization

Second-stage randomization was conducted during the training, and the randomization results were announced after the training completion but before the dispatch to the catchment area. The wage incentives were given to a randomly selected half of the *Internship* group. Similarly, the career incentives were given to a randomly selected half of the *Wage* group. The second-stage

[19] We discuss this further in footnote 46.

randomization was announced publicly. Therefore, both G1 and G2 enumerators learned about the additional wage incentives, and both G3 and G4 enumerators learned about the additional career incentives. AFF staff explained to enumerators that they would distribute additional incentives in a random manner due to budget constraints.[20] No enumerators refused to accept the additional incentives, which implies that the composition of worker characteristics between *G1* and *G2* and between *G3* and *G4* remains the same.

Right after the second-stage randomization, AFF supervisors and enumerators had a one-on-one session to explain the details of the contract, and the enumerators signed the employment contract as shown in Figures A.1, A.2, and A.3.[21] To illustrate, the employment contract of *G1* explicitly states that enumerators will not be given any financial compensation and will be provided with a recommendation letter and a potential job opportunity based on their performance.

2.2.4 Census and post-enumeration survey

Enumerators were dispatched to 52 catchment areas in January 2015. They were randomly assigned to catchment areas stratified by population and land size, and worked independently. Enumerators in the same catchment area have the same incentives to prevent unexpected peer effects. In addition, enumerators were not assigned to areas from which they originally came, as locality could affect their performance. The census survey took about 25 minutes on average to interview a household head. Enumerators were expected to survey at least eight households per day. In total, enumerators surveyed 21,561 households during the contract period.

AFF supervisor teams, which consisted of two supervisors per team, visited enumerators

[20] It is worth noting that random assignment of additional work incentives is not common in the real-world setting. Hence, the results should be interpreted with this caveat.

[21] Through the one-on-one meeting, AFF explained to G4 enumerators that their position would be a one-time employment opportunity even though it was not explicitly mentioned in the contract.

to monitor and guide enumeration work on randomly selected dates without prior notice. Supervisors are AFF's regular staff members, each with at least 3 years of experience conducting field surveys. AFF randomly assigned five supervisor teams to 52 catchment areas for their visits. Most enumerators met a supervisor team at least once during the census period; 37% of the enumerators met supervisors twice and the remaining 60% met supervisors once.[22] Enumerators were aware of supervisor visits but did not know the exact date. Supervisors joined each enumerator for interviews of about three households, addressed common errors, and provided overall comments at the end of the visit.

Shortly after the completion of the census, AFF conducted a post enumeration survey (PES) to correct errors found in the original census interview, find omitted households, and measure subjective performance evaluation (SPE) by revisiting all households in Chimutu. AFF announced a PES plan to evaluate the performance before the field dispatch to prevent enumerators from outright cheating or fabricating census interview sheets. Some enumerators from *G1*, *G2*, and *G3* (those with career incentives) were temporarily hired to conduct the PES.[23]

As stated in the employment contract, AFF provided recommendation letters to the enumerators with career incentives (*G1*, *G2*, and *G3*) in May 2015. The recommendation letter was signed jointly by the director of AFF and the head of the Chimutu district. The letter specified the job description of an enumerator and his relative job performance.[24]

[22] A supervisor team failed to visit one catchment area where four enumerators were assigned because the enumeration work was completed before the randomly selected first visit took place.
[23] Hiring enumerators as regular staff members required the careful calculation of job performance after the completion of the census, which can take at least two months. Meanwhile, AFF hired 43 PES enumerators among 98 census enumerators with career incentives (*G1*, *G2*, and *G3*) on a temporary basis (2–3 months) through a simple performance evaluation based on SPE by supervisors and error rates measured from five randomly selected surveys.
[24] If an enumerator has higher job performance than the average, the letter specifies a very strong recommendation. If an enumerator has performance below the average, the letter specifies a somewhat lukewarm recommendation.

3. Conceptual Framework

In this section, we present a conceptual framework that motivates our experimental design and provides guidelines for the empirical analysis. In essence, this framework aims to explain how different incentive schemes attract workers with different abilities and affect their effort levels.

3.1. Setting

Consider agents with an effort allocation decision. Agents make an effort decision facing a total cost of effort (e), ce, and a benefit of effort depending on the incentives they receive. Agents are endowed with a cost of effort, $c \in \{c_L, c_H\}$, with $c_H < c_L = 1$. For simplicity, we assume that there are a known, equal number of agents with a high cost ($c = c_H$) and a low cost ($c = c_L$), respectively. Since agents are randomly assigned to either *Internship* or *Wage* group in the first stage, high-cost and low-cost agents are evenly distributed in each study group.

For those who are offered the wage incentives, we assume that the benefit of exerting effort is $wln(e)$, where w is the offered wage. For those who are offered the career incentives, we assume that the benefit of effort depends on how productive an agent is relative to the productivity of other agents with the career incentives. That is, agent j's payoff is $\kappa \frac{e_j}{\sum_{k \in X} e_k}$ for $\kappa > 0$ where X is the (expected) group of agents competing for career incentives.[25] Type-dependent outside options are given by $\overline{u_H}$and $\overline{u_L}$, respectively, with $\overline{u_H} > \overline{u_L} = 0$.

Agents maximize the following utility function:

Utility of those with the wage incentives: $U_w(e) = wln(e) - ce$ (1)

[25] We applied Tullock contest-style payoff (Corchón, 2007) for the career incentive. This makes it explicit that agents are competing against each other for payoffs. The generality further allows $\kappa \frac{e}{\sum_{k \in X} e_k} - ce$ to denote the expected value of participating at level e. When $e_k = 0$ for all $k \in X$, the benefit will be set equal to $\frac{\kappa}{|X|}$.

Utility of those with the career incentives: $U_c(e) = \kappa \frac{e}{\sum_{k \in X} e_k} - ce$ (2)

First, we show the first order condition and an equilibrium effort level for those with the wage incentives, derived from (1):

$$\frac{w}{e} - c = 0 \leftrightarrow e_w^*(c) = \frac{w}{c} \quad (3)$$

An individual who received a job offer with the wage incentives will accept the offer if and only if his utility of working (i.e., making efforts) is higher than his reservation wage: $U_w\left(e_w^*(c_j)\right) > \overline{u_j}$ for $j \in \{L, H\}$. That is,

$$wln(\frac{w}{c_j}) - c_j \frac{w}{c_j} \geq \underline{u}_j \leftrightarrow wln(w) - w \geq \underline{u}_j + wln(c_j)$$

Second, we show the first order condition and an equilibrium effort level for those with the career incentives, derived from (2):

$$\kappa \frac{e_{-i}}{(e_{-i}+e_i)^2} - c_i = 0 \quad (4)$$

where $e_{-i} = \sum_{k \in X} e_k - e_i$.

Applying a type-symmetric Tullock contest model (Corchón, 2007), we can derive a unique equilibrium effort level, $e_i = e^*$ for all i. for those who accepted the career incentives at:

$$\kappa \frac{(X-1)e^*}{(Xe^*)^2} - c_i = 0 \leftrightarrow e_c^*(c) = \frac{X-1}{X^2}\frac{\kappa}{c} \quad (5)$$

where X is the number of agents competing together for the career incentives. Then, the career incentive job offer will be accepted if and only if $U_c\left(e_c^*(c_j)\right) > \overline{u_j}\ for\ j \in \{L, H\}$ such that:

$$\kappa \frac{e_c^*(c_j)}{\sum_{k \in X} e_c^*(c_j)} - c_j e_c^*(c_j) \geq \overline{u_j} \leftrightarrow \frac{\kappa}{X^2} \geq \overline{u_j} \quad (6)$$

3.2. Prediction

3.2.1 Selection Effects

Proposition 1. Under the settings specified in Section 3.1, there exists the following two equilibria depending on the parameter values of the model: First, high-ability agents accept the career incentive offer and low-ability agents accept the wage incentive offer. Second, high-ability agents accept the wage incentive offer and low-ability agents accept the career incentive offer.

(Proof) See the Appendix for the proof.

It is a priori ambiguous which equilibrium explains the selection effects of work incentives in the labor market for entry-level workers in Malawi because the equilibrium outcome depends on the choice of the parameter values in the model. Hence, we will test which separating equilibrium can better fit our experimental data and discuss the estimated selection effects according to the corresponding separating equilibrium.

Currently, our conceptual framework does not consider the two-stage sequence of incentive provision (as in the actual field experiment) to keep the model parsimonious. Instead, we assume that the sequence by which additional incentives were presented does not matter when enumerators make effort decisions, and thus it would not be consequential in deriving the prediction about the selection effects.

3.2.2 Incentive Effects

Our conceptual framework also predicts that the provision of additional incentives increases efforts due to greater rewards.

Proposition 2 (wage incentive effect). Consider two groups who were originally offered a career incentive only (G1 and G2). Between these two groups, those who were additionally offered wage incentives (G2) make greater efforts than those who were not offered additional wage incentive (G1). That is, $e_2 > e_1$.

(Proof) See the Appendix for the proof.

Proposition 3 (career incentive effect). Consider two groups who were originally offered a wage incentive only (G3 and G4). Between these two groups, those who were additionally offered career incentives (G3) make greater efforts than those who were not offered additional career incentive (G4). That is, $e_3 > e_4$.

(Proof) See the Appendix for the proof.

4. Data

We use data from various sources, including baseline and follow-up surveys, administrative data on training and job performance, and the Chimutu population census. First, we use data from the 2011 secondary school student survey. It contains rich information on a variety of areas covering demographics, socioeconomic status, health, and cognitive ability. Second, we use data from the 2014 baseline survey, which collects information on demographics, education, employment history, cognitive abilities, non-cognitive traits, and HIV/AIDS related outcomes.

We measure cognitive ability in two distinct ways. The first measure is Math and English scores of the 2014 Malawi School Certificate of Education (MSCE) test, which are easily observable in the local labor market.[26] The second measure is the scores of Raven's matrices test and the verbal and clerical ability tests of the O*NET, which are difficult to observe for potential employers.[27] Data Appendix A.1 provides the definitions of these cognitive ability measures.

[26] MSCE is an official test that all Malawian students must take to graduate from secondary school. AFF had access to the administrative MSCE score data via the cooperation of the Ministry of Education of the Republic of Malawi. We use Math and English test scores only because they are mandatory subjects of the MSCE test.

[27] Raven's progressive matrices test is a non-verbal test of thinking and observation skills. The O*NET® test is a tool for career exploration developed by the U.S. Department of Labor. We use verbal and clerical perception ability test scores of O*NET®, which are directly related to enumerator job characteristics.

Non-cognitive traits include self-esteem, intrinsic motivation, extrinsic motivation, and the Big Five personality test (extraversion, openness, conscientiousness, agreeableness, and neuroticism). The additional baseline survey conducted in April–June 2015 collected data on risk and time preferences and rational decision-making ability using the tests recently developed by Choi et al (2014).[28]

Training outcomes are measured by a quiz score and the proportion of erroneous entries in a practice survey.[29] The quiz tested specific knowledge on the census details. It consists of 12 questions, a mixture of open-ended and true/false type questions. The full text of the quiz is presented in Figure A.4.

Main job performance measures during the census are survey quantity and quality. Survey quantity is measured by the number of households surveyed by each enumerator per day. Survey quality is measured by the proportion of systematically inconsistent or incorrect entries in the census questionnaire specific to each household surveyed. For example, if a respondent has a child, the information about her child should be filled in. If not, it is counted as an error. Data Appendix A.2 provides the details about how we calculate the survey error rate. We also use subjective performance evaluations (SPE) measured by census respondents because we expect enumerators to give good impressions to community members as an NGO worker that serves local communities.

[28] As explained in Subsection 2.2.1, risk and time preferences, and rational decision-making ability were measured after the census was completed. We included these measures in the randomization balance test under the assumption that these traits were not affected by our experiment. Data Appendix A.1 provides the details of how we measure them.

[29] The purpose of the practice survey was to practice interview skills before enumerators were dispatched to the field. The practice survey performance was evaluated as follows: First, we randomly matched two trainees. Each trainee in a randomly assigned pair received a pre-filled census questionnaire sheet and a blank survey questionnaire sheet. Then, one trainee interviewed the other matched trainee in the same pair and the latter trainee responded based on the assigned survey sheet. There were two different types of pre-filled questionnaire sheets with different hypothetical household information. Thus, trainees in the same pair acted as if they were two different households. Each trainee in every pair conducted this practice survey by changing roles. After conducting practice survey sessions, supervisors collected the survey sheets and calculated the error rate.

During the PES, census respondents were asked to evaluate how carefully the enumerator had explained the questions.[30] In addition, after the completion of the census, 12 supervisors jointly evaluated the work attitude of each enumerator (SPEs measured by AFF supervisors).[31]

Lastly, census data were used to calculate the average characteristics of the catchment area so that we can use them as the control vector in the main regression analysis. [32]

Columns (2) and (3) of Table 2 present the baseline characteristics of the *Internship* and *Wage* groups, respectively. The results of the first- and second-stage randomization balance are presented in Columns (4), (5) and (6). Panel A represents individual baseline characteristics of study participants. Study participants are about 20 years old and only 9% work in the official sector reflecting weak labor demand in Malawi.[33] Data Appendix A.1 provides the specific definition of the variables presented in Panel A. Panel B represents the catchment area characteristics where enumerators were dispatched. The results confirm that the study groups are well balanced: the proportion of statistically significant mean difference at the 10% significance level is 2 out of 28 (7.1%) in Column (4), 3 out 28 (10.7%) in Column (5), and 4 out of 28 (14.3%) in Column (6).[34]

[30] The question asked was "Whenever you were confused or could not understand the meaning of any question, did the enumerator carefully explain the meaning of the questions to you?" Enumerators who conducted the PES sought to interview the original census respondent particularly for this question. However, the original respondent was not always available, and thus, we could analyze SPE by census respondents only when the census respondent and the PES respondent were identical. In addition, we conducted the test of equality between households in which the census respondent and the PES respondent were identical and households in which they were different. The probabilities that an original census respondent was a PES respondent are 77%, 77%, 83%, and 82% for *G1*, *G2*, *G3*, and *G4*, respectively. These rates are significantly different. Hence, the interpretation of the SPE analysis by respondents should be taken with caution.

[31] We asked a group of supervisors to evaluate general work attitude of enumerators. Enumerators were scored on a scale of 1 to 3.

[32] Regarding catchment area size, we could not acquire information on the exact land size of each catchment area. However, we had an unofficial, categorical measure of land size ranging from 1 (smallest) to 10 (largest). The land size category for each catchment area was jointly determined by AFF supervisors who have worked in the Chimutu district for five years or longer, and in consultation with local health surveillance assistants working in each catchment area to ensure its accuracy.

[33] The employment rate of baseline survey non-participants is similar. We reached non-participants via phone calls and 9.7% of them told us that they did not attend because they were working.

[34] The number of siblings, the only unbalanced individual variable in Column (4) of Table 2, eligibility for AFF's past interventions and catchment area characteristics controls are included in all specifications of the main analysis.

We also examine whether the baseline survey participants and non-participants are systematically different. Table A.2 shows that they are not statistically different from each other in most dimensions except for the household asset score. In addition, Table A.3 shows no systematic differences across enumerators assigned to each supervisor team, which confirms that the supervisor team randomization went well.

5. Main Results

5.1. Job Offer Take-up

Column (1) of Table 3 shows that the job offer take-up rates between the *Internship* and *Wage* groups are not statistically different. We test multidimensional sorting discussed in Dohmen and Falk (2011) by exploring whether career and wage incentives attract those with different observable characteristics. Columns (2) to (18) of Table 3 show the regression results of the following equation:

$$Accept_i = \alpha + \delta \cdot Internship_i + \lambda \cdot Trait_i + \varphi \cdot Internship_i \cdot Trait_i + \epsilon_i \qquad (7)$$

where $Accept_i$ is a binary indicator that equals 1 if individual i accepted a job offer, and 0 otherwise. $Internship_i$ is a binary indicator if individual i belongs to the *Internship* group and the omitted category is the *Wage* group. $Trait_i$ is an individual characteristic variable that we evaluate one by one. ϵ_i is an error term. We test whether career incentives attract workers differently over a variety of individual characteristics including demographic and socioeconomic characteristics, cognitive ability index, and non-cognitive traits.

Our coefficient of interest is φ, which captures differential take-up of a job offer between the *Internship* group and the *Wage* group by individual traits. We find that none of the estimates

of φ across individual traits is statistically significant at the 5% level.[35] These findings imply that observable characteristics are not likely to predict self-selection.

Table A.4 provides additional evidence on self-selection by comparing the observable characteristics of job offer takers between the *Internship* group and the *Wage* group. The results in Table A.4 confirm the results in Table 3 that the two groups are not systematically different in terms of both statistical and economic significance.[36]

The absence of systematic differences in observable characteristics does not necessarily mean that unobservable characteristics, training outcomes, and job performance would be the same if some of the unobservable characteristics were to affect training outcomes and job performance.

5.2. Training Outcomes

Even though we do not find any differences in observable characteristics between job takers of the two groups, we might find a difference in training outcomes if career and wage incentives attract people with different unobservable characteristics. Panel A of Figure 2 displays the kernel density estimates of the training outcomes measured by the quiz score and the practice survey error rate. Table 4 shows the corresponding results from the following specification:

$$Training_i = \alpha + \beta \cdot Internship_i + \omega_i \quad (8)$$

where $Training_i$ is the training outcomes such as practice survey error rate and quiz score for

[35] There might be concern about statistical power due to relatively small sample size (N=362). However, for most variables we are able to detect 15% differences between the two groups. For example, Column 2 of Table 3 shows we are able to detect age difference between the two groups that is bigger than 0.07 (=0.037*1.96) years, which is a 0.36% change (=(0.07/20.4)*100). Nonetheless, we cannot fully rule out the possibility that we are unable to detect small differences between the two groups. Therefore, the results should be interpreted with this caveat.

[36] We acknowledge that study participants could have responded to the self-reported non-cognitive tests in a way that they believed to be desirable from the perspective of a potential employer, even though they were not aware of the possibility of a job offer at the time of the baseline survey. This is consistent with the real world in which job seekers are not able to manipulate test scores (cognitive ability) in a pre-employment test but might try to respond to a personality test in a way in which they have a desirable non-cognitive skill.

individual *i*. For the practice survey error rate regression, we control for a practice survey type and pair–fixed effect in the regression.[37]

Panel A of Figure 2 shows that the *Wage* group performs better than the *Internship* group in terms of both quiz score and practice survey error rate. Panel A of Table 4 provides corresponding results from the regression. It confirms that the quiz score of the *Internship* group trainees is 2.0 points (23.8%) lower than that of the *Wage* group trainees as shown in Column (1). Similarly, the survey error rate is 10.4 percentage points (38.2%) higher among the *Internship* group trainees than that among the *Wage* group trainees as shown in Column (3).

At the end of the training, AFF disqualified 11 trainees who did not meet the minimum qualification requirement. As the abovementioned regression results indicate, the *Internship* group performed worse than the *Wage* group did. Thus, all dropouts (11 trainees) came from the *Internship* group only. Panel B of Table 4 presents the training outcomes of enumerators dispatched to the field by excluding the 11 training failures. The regression results between the two panels are qualitatively similar, but the magnitude of the coefficient estimates is larger in Panel A than in Panel B, because those who failed training are all from the *Internship* group.

The specification used in Columns (2) and (5) is to test whether individual observable characteristics can explain the differences in the training outcomes between the two groups. The individual observable characteristics include age, household asset score, cognitive ability index, and non-cognitive traits, such as self-esteem, intrinsic and extrinsic motivation, and Big 5 personality scales. We find similar coefficient estimates between Columns (1) and (2). For example, observable characteristics explain only 2.5% (=(2.01-1.96)/2.01) of the difference in quiz

[37] All regressions include number of siblings, which is not balanced in the baseline, and eligibility for AFF's past interventions as a control vector. When analyzing the practice survey error rate, we additionally include survey pair fixed effect.

score. In the case of the practice survey error rate, controlling for individual characteristics in Column (5) makes coefficient estimates statistically insignificant and larger. These findings imply that observable characteristics are somewhat limited in explaining the difference in the training outcomes.

In summary, we find that those attracted by a job offer with wage incentives outperformed those attracted by a job offer with career incentives in the training. This difference could be caused by workers with different characteristics selecting into different work incentives, thereby creating the difference in the training outcomes (selection effect).

However, there are several reasons why the observed difference in training performance could be different from the true selection effect. For instance, those in the Internship group have an incentive to exert more effort than the Wage group due to the future job prospect of the career incentives. That is, in the absence of such an effect, the difference in training performance due to selection could be larger than the observed difference in training performance. On the other hand, the difference in training performance due to selection could be smaller if there was a learning-by-doing effect for training instructors. Instructors could deliver lectures more efficiently in the second session (for the Wage group) than in the first session (for the Internship group). Therefore, the analysis of the training results should be interpreted with caution due to these possibilities that can potentially bias the selection effect

5.3. Selection effect of career incentives on labor productivity

In this subsection, we examine the selection effect of career incentives evaluated against wage incentives on job performance. As previously discussed, *G2* and *G3* have the same incentives at work, but the channels by which they were recruited are different. Therefore, we interpret that

differences in performance are driven by the selection effect.

Our identifying assumption is that G2 and G3 enumerators perceive their work incentives identical at work even though the sequences by which career and wage incentives were presented are different. The different sequence could form different perceived valuation of the incentives that affect enumerators' feelings leading to different levels of work efforts. As a result, our estimates of the selection effect would be biased as Abeler et al. (2011) discussed. However, we argue this is unlikely. If there were such a difference in feelings, we expect that differences in job performance would become smaller over time because the difference in feelings might diminish with time. Figure A.5 shows that the difference in job performance is fairly constant over time.[38]

Panel B of Figure 2 suggests that *G2* has higher labor productivity than *G3* in terms of survey quality and quantity. This finding is surprising because the *Wage* group had better training outcomes than the *Internship* group did. We test this graphical evidence formally by estimating the following equation:

$$Y_{ijklt} = \alpha + \beta \cdot G2_j + \gamma \cdot H_{ik} + \varphi \cdot Z_k + V_{lt} + \sigma_t + \psi_{ijklt} \quad (9)$$

where Y_{ijklt} is job performance measured in the survey collected from household *i* by enumerator *j* whose supervisor is *l*, in catchment area *k*, surveyed on the *t*-th work day. $G2_j$ is 1 if enumerator *j* belongs to *G2* and 0 if he belongs to *G3*. H_{ik} is a vector of respondents' household characteristics and Z_k is a vector of catchment area characteristics.[39] V_{lt} is the supervisor team-specific post-visit

[38] The different sequence could still generate bias if those recruited with career incentives might misunderstand the addition of wage incentives as a reward for good performance during training, while those recruited with wage incentives might misunderstand the addition of career incentives as a windfall gain, not a reward. However, this is also unlikely because we clearly indicated that the additional provision of incentives in the second stage was randomly determined.

[39] Respondent's household characteristics include the fixed effect for family size. Catchment area characteristics include the total number of households, size of the catchment area, asset score, birth rate, malaria incidence, rate of birth with the assistance of a health professional, and death rate.

effect and σ_t is the survey date fixed effect.[40] Standard errors are clustered at the catchment area level. For dependent variables, we use survey quality measured by the survey error rate ($Error_{ijktl}$) and survey quantity measured by the number of surveys per day ($Survey_{jktl}$).

Panel A of Table 5 presents the regression results from equation (9). It shows that *G2* outperforms *G3* in two main measures of job performance, even though *G3* outperforms *G2* during the training.[41] The error rate is 2.2 percentage points (28.6%) lower in *G2* than *G3*, as shown in Column (1). The survey quantity of *G2* is higher than that of *G3* by 1.39 households per day (13.0%), as shown in Column (4).

To assess how much observable individual characteristics and training performance can explain the selection effect estimated in Columns (1) and (4), we additionally control for enumerator characteristics such as demographic and socioeconomic status, cognitive ability (MSCE scores and Raven's matrices/O*NET scores), and non-cognitive traits in Columns (2) and (5) as well as training performance in Columns (3) and (6). As shown in Columns (2) and (5), observable individual characteristics of enumerators are limited in explaining the estimated selection effect. On survey quality, the inclusion of observed individual characteristics does not explain the estimated selection effect of career incentives at all. It explains survey quantity only by 7.2% (=(1.39-1.29)/1.39). Additionally controlling for training performance also remains limited in explaining the selection effects.

We present the selection effect on SPEs in Table A.5.[42] *G2* has a 67.9% higher SPE score by survey respondents than *G3*, as shown in Column (1). Adding enumerator characteristics

[40] $V_{lt} = \eta_0 + \eta_{1l} I(t > First) + \eta_{2l} I(t > Second)$ where First and Second are the dates of supervisor team *l*'s first and second visits, respectively, to enumerator *j*.

[41] See Figure A.6 for the training outcomes of each study group.

[42] We do not control for σ_t and V_{lt} when we analyze *SPE (supervisor)* because it does not vary over time and catchment area.

explains only 7.0% of the selection effect on SPE by respondents. This result is consistent with the fact that the observable characteristics of job takers between the *Internship* group and the *Wage* group are not different. Lastly, we find that the SPE score by supervisors is higher in *G3* than in *G2* (Column (4)), but it is not statistically significant at the 5% level.

In Table A.6, we report the results that decompose the main outcomes. To understand where survey errors come from, we decompose errors into incorrectly entered entries (e.g., filling in 179 for a person's age) and incorrectly missing entries (e.g., a child is present in the household but his/her age is missing). To better understand how survey quantity changes, we conduct regression analyses on three time-use variables such as total work hours per day, average survey time per household, and intermission time between surveys.[43] Column (3) in Panel A indicates that the selection effect of career incentives on survey quality reported in Table 5 is mostly driven by the decrease in incorrectly missing entries. In addition, we find that the selection effect of career incentives on survey quantity comes from longer work hours, shorter survey time per household, and shorter intermission time as shown in Columns (5)-(10) of Table A.6. However, these coefficient estimates are not precisely estimated. We find that observable enumerator characteristics and training performance do not explain differences between *G2* and *G3* much.

Then, why do *G2* enumerators outperform *G3* enumerators in actual job performance, while the *Wage* group outperforms the *Internship* group during training? One possible explanation is that different skill sets are required in each setting. The test taken during the training was in a

[43] Work hours per day are the difference between the beginning time of the first survey and the end time of the last survey of the day. Survey time per household is the length of each survey. Intermission time is defined as the difference between the beginning time of a survey and the end time of the previous survey. The survey beginning and end times were recorded as a part of the census questionnaire. However, there was a sizable number of missing values in the interview beginning and end times, so we imputed the missing values. See Data Appendix A.3 for the imputation process. However, the results remain similar even if we do not use the observations with imputed time values.

classroom setting, while job performance resulted from actual interactions with respondents in the field. It is plausible that enumerators selected through career incentives have comparative advantages in on-the-job performance but not in tests in a classroom setting. A critical characteristic of an enumerator is the skill to ask strangers sensitive questions about their households. This kind of skill might not be captured easily in a test taken in a laboratory setting.[44,45]

5.4. Incentive effects of work incentives on labor productivity

To measure causal impacts of career incentives on labor productivity, we compare job performance of enumerators who receive both wage and career incentives (*G3*) and that of enumerators with wage incentives only (*G4*). Similarly, we measure causal impacts of wage incentives by comparing job performance between enumerators with only career incentives (*G1*) and enumerators with both career and wage incentives (*G2*). We estimate incentive effects of wage and career incentives among job takers of the *Internship* and *Wage* groups, respectively; therefore, these incentive effects are not directly comparable. Panels B and C of Table 5 report the incentive effects of career and wage incentives on job performance estimated among the *Wage* and *Internship* groups, respectively. Panels C and D in Figure 2 present the corresponding graphical evidence.

[44] Alternatively, it is possible that the *Internship* group initially had lower performance in the training but caught up with the *Wage* group later in the field owing to a steeper learning curve. However, this is less likely, as we find no evidence of performance catch-up. Job performance between the *Internship* and *Wage* groups remained constant over the study period (see Figure A.5 for the daily performance trend). In addition, it is possible that screening out 11 trainees in the *Internship* group served as a reminder or a credible threat to those with career incentives that only some of them would be hired as regular workers in AFF, causing *G2* to work harder than *G3*.

[45] Another point to discuss is unbalanced dropouts of the trainees. All 11 trainees who were dropped were from the *Internship* group. Therefore, if the labor productivity of the dropouts were lower than that of the hired enumerators, the performance-improving selection effects would be overestimated. However, we do not consider that any particular adjustment is necessary in the main analysis because screening out trainees who did not meet the minimum requirement is a regular business practice. Nevertheless, we re-estimate equation (9) after dropping 11 trainees with the lowest training scores from the *Wage* group (six from *G3* and five from *G4*). Panel A of Table A.7 shows that the results for the selection effects remain mostly robust; the size of the coefficients for the selection effect on survey quality becomes smaller, while that for survey quantity becomes larger. We find similar results on incentive effects (Panels B and C) and combined effects (Panel D).

Our conceptual framework predicted that the additional provision of career incentives would motivate enumerators to exert more effort and improve job performance. However, in Panel B of Table 5, we find no such evidence in main labor productivity outcomes. However, column (4) of Table A.5 shows that SPE measured by supervisors significantly increases by 51.5%. In summary, career incentives given to existing workers hired through the wage incentive channel do not improve labor productivity, but they induce enumerators to have better evaluation from supervisors. We speculate that the effort level of the *Wage* group enumerators was already high, and thus it is difficult for them to improve work performance at least in the short run. They rather exerted effort in building their relationships with supervisors.[46]

There might be a concern that, despite high frequency data, the relatively small number of enumerators allows for the detection of only relatively large effects and makes it difficult to interpret null results. Indeed, we are somewhat underpowered in the regression analysis of Panel B of Table 5 in the sense that the size of the standard errors is not small enough to capture the small effect (if any) of the work incentives. To illustrate, we are able to capture the impacts of career incentive on survey quality and quantity only if the change is greater than 16.7% (=0.007×1.96/0.082) and 13.0% (=0.763×1.96/11.5), respectively.

Panel C of Table 5 shows that wage incentives, additionally given to the *Internship* group enumerators, improve job performance. We find that survey errors decrease by 3.8 percentage points (a 50.1% decrease) in Column (1) without statistically significant changes in survey quantity

[46] Another possibility is that career incentives might not be very appealing to enumerators recruited through wage incentives conditional on self-selection. For example, enumerators might not have needed a job for a longer period. Alternatively, the marginal effects of career incentives in the second stage could be small, because enumerators had already received wage incentives in the first stage. However, this possibility does not explain an increase in SPE by supervisors. Lastly, there might be concern that the differences in performance could be driven by the decrease in control group productivity due to disappointment at not receiving the second-stage incentives. However, this possibility is less likely because this psychological mechanism, if present, would decline over time as such feeling might diminish with time, which does not correspond to the results shown in Figure A.5.

(Column (5)) and SPEs (Panel C of Table A.5). Panel C of Table A.6 shows that the decrease in the survey error rate is explained mostly by a decrease in illogical missing entries, as shown in Column (3).[47] This finding is consistent with the gift exchange model of the efficiency wage theory formulated by Akerlof (1984). In the model, a worker exerts more efforts upon receiving a gift from an employer that exceeds the minimum level of compensation for the minimum level of effort. We also acknowledge that a part of the productivity improvements in *G2* (evaluated against *G1*) might not be completely due to the gift exchange motive because the wage incentives include a performance bonus component.

Panel D of Table 5, which compares *G1* versus *G4*, resembles the combined effects of selection and incentive effects on productivity in that participants were attracted to accept a job offer via different incentives and the incentives at work also remained different. It is noteworthy that the combined effects of career incentives (Panel D) are not necessarily a simple sum of the selection effect (Panel A) and incentive effect (Panel B), because of potential interaction between selection and incentive effects.[48] We find no significant difference in the combined effects between *G1* and *G4* in the main productivity outcomes, implying the importance of separating selection and incentive effects. However, we find that *G1* enumerators have significantly better SPE by supervisors than *G4* enumerators do (Panel D of Table A.5), which is consistent with the fact that career incentives causally improve SPE by supervisors in Panel B.

[47] One might wonder that the *G1* enumerators who have career incentives only performed poorly due to lack of money for meals in the field. To minimize this possibility, we ensured that the *G1* enumerators understood that there would be no financial support. In addition, AFF informed all enumerators in advance that it would be difficult to find a shop or restaurant to purchase food in the field, and encouraged them to bring enough of their own food during the work period. On the day when we started the census, we were able to observe that most enumerators brought their own food. In addition, when AFF arranged housing in each catchment area for enumerators to stay during the census, AFF ensured that the enumerators were able to use the kitchen for cooking.

[48] In addition, the study sample used in Panel D of Table 5 is different from that in Panels A and B. In Panel A, we use *G2* and *G3* to estimate the selection effect of career incentives. In Panel B, we use *G3* and *G4* to estimate the incentive effect of career incentives.

6. Conclusion

This study analyzes how career and wage incentives affect labor productivity through a two-stage randomized controlled trial in the context of a recruitment drive for census enumerators in Malawi. Even though career and wage incentives are the most common types of work incentives, no study has considered these incentives in the same setting, to the best of our knowledge.

We find that career incentives of an internship significantly improve labor productivity through the self-selection of workers: The *Internship* group (those attracted by career incentives) outperformed the *Wage* group (those attracted by wage incentives) at work, even though the *Wage* group was better than the *Internship* group during the training. Observable individual characteristics, including training outcomes, are limited in explaining the difference in labor productivity. The fact that neither observable characteristics nor training outcomes predict actual job performance implies that screening via observable characteristics is imperfect, particularly when hiring entry-level workers who have no track record of past job history or credentials to verify their unobserved productivity. Furthermore, these findings highlight the importance of a recruitment strategy in attracting workers with strong unobservable skills via self-selection (e.g., an internship).

Regarding the career incentive effect, we find no positive evidence for the career incentive effects on labor productivity conditional on selection except for the SPE by supervisors. Our findings suggest that career incentives are effective in improving labor productivity mainly through the selection effect channel. Lastly, we find that additional financial incentives can be an effective means to improve labor productivity (e.g., survey quality) for those recruited by career incentives. As a result, labor productivity is highest in *G2*, who were recruited by career incentives and received additional wage incentive.

We show how work incentives affect labor productivity among entry-level workers in Malawi. Therefore, our setting is closest to situations in which firms hire entry-level workers in developing countries whose productivity is not easily observable and worker characteristics are similar due to the similarity in contexts. Our analysis has implications for settings in which employers have difficulties screening productive workers with no or short employment history and are looking for effective means to motivate existing workers.

There are limitations to our study. First, we acknowledge that the approach by which we estimate the incentive effects might not perfectly characterize the real world. In the real world, workers might not always receive additional incentives without prior notice. Second, the length of the job we study is relatively short-term. As such, we cannot study whether the estimated selection and incentive effects of career and wage incentives remain constant over longer periods. The short-term nature of our study also limits the analysis of the effects of work incentives on retention. Third, we do not directly observe the individual's perception of the value of work incentives. In addition, we do not measure how career and wage incentives change workers' belief about the probability of retention by AFF. Hence, we do not know whether the selection effect of career incentives operates through the expectation of a job prospect at AFF or a potentially favorable recommendation letter. Fourth, the non-cognitive traits used in this study are self-reported psychometric scales measured based on a paper test. It would be interesting to know whether such paper-based and self-reported non-cognitive traits are highly correlated with non-cognitive traits measured in other settings. Fifth, the relatively small number of enumerators may prevent us from interpreting relatively small and insignificant effects, especially in estimating the career incentive effects. However, most major outcomes (selection effects and wage incentives effects) are large enough to detect their effects.

The difficulty in effective screening of job applicants and lack of motivation among existing workers are key drivers of low labor productivity, particularly in developing countries. A better understanding of selection and incentive effects of work incentives would allow employers to design optimal employment strategies. Based on our findings, we argue that active adoption of career incentives in the workplace as a hiring strategy could be an effective means to increase labor productivity of an organization hiring entry-level workers.

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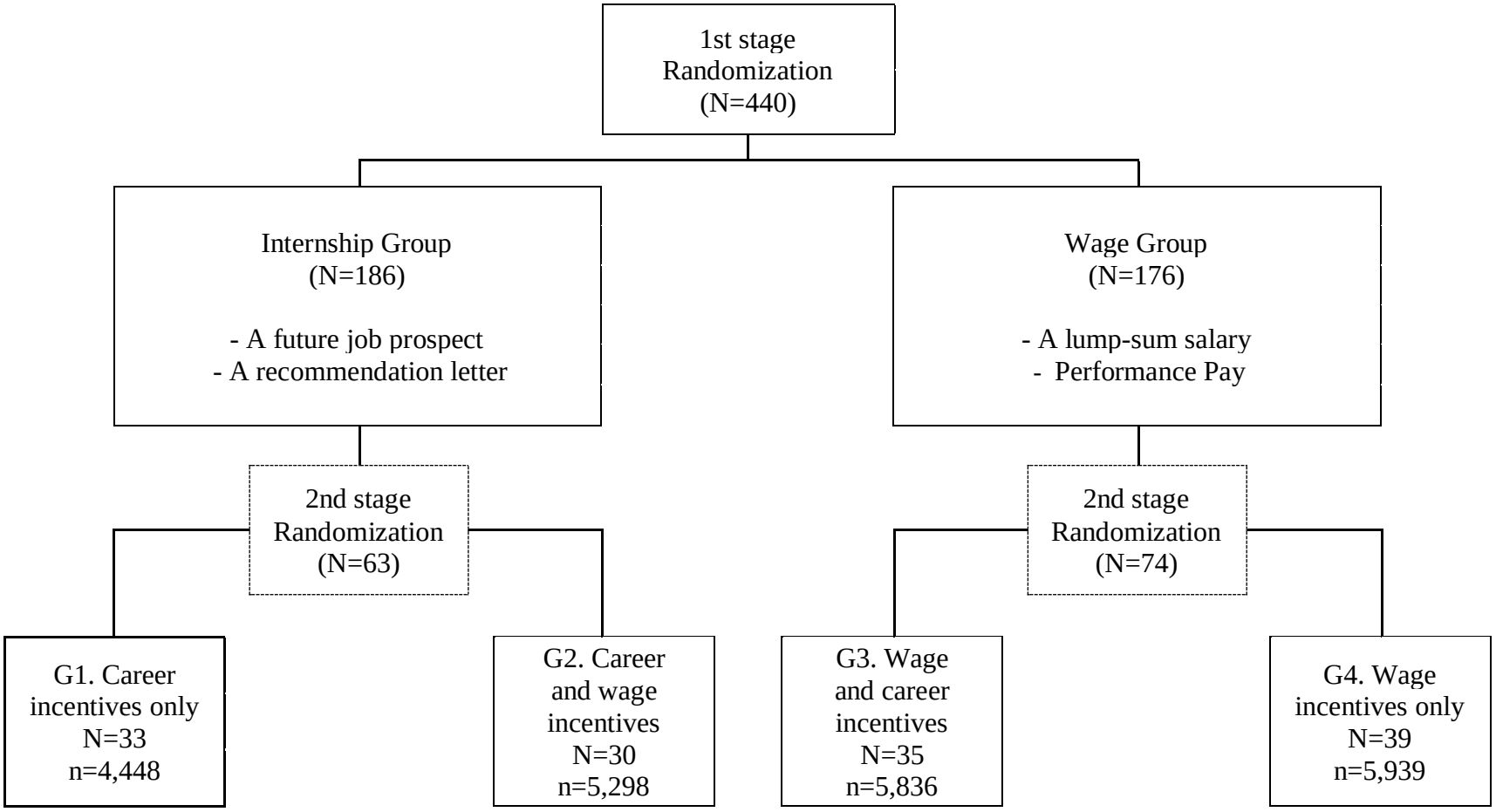
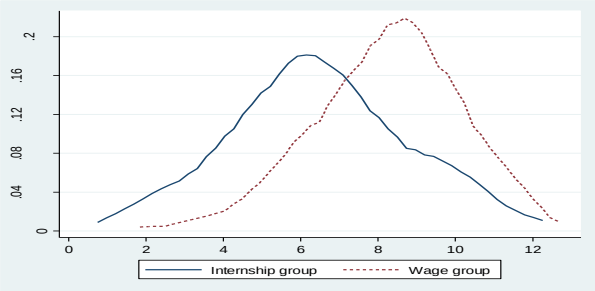
Figure 1: Experimental Design



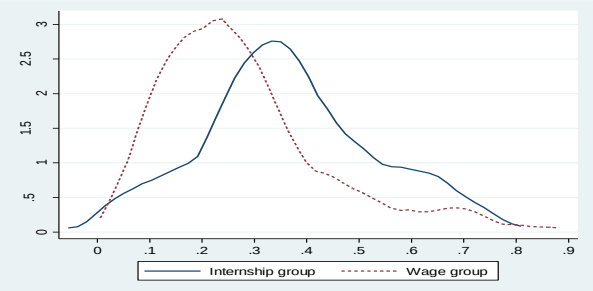
Notes: Upper case N indicates the number of participants in each stage. Lower case n indicates the number of surveys conducted by census enumerators.

Figure 2: Training Performance, Selection, Incentive, and Combined Effects

Panel A: Training performance (*Internship* group vs. *Wage* group)

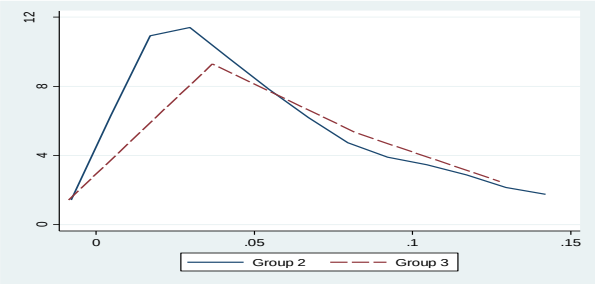


Quiz score

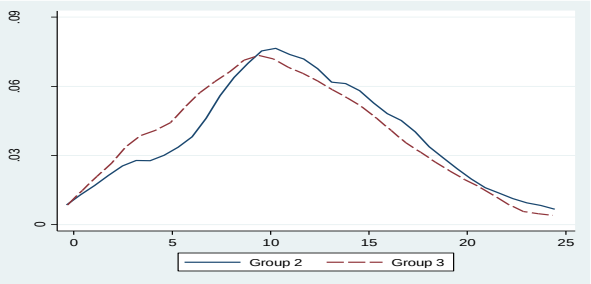


Practice survey error rate

Panel B: Selection Effect (G2 vs. G3)

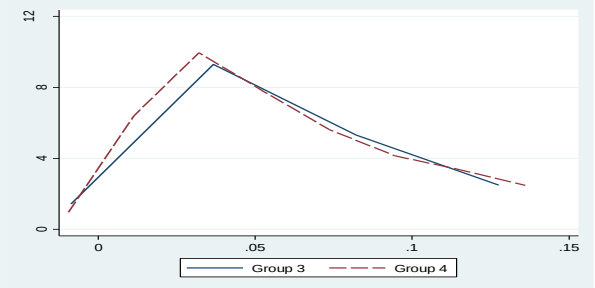


Survey quality (error rate)

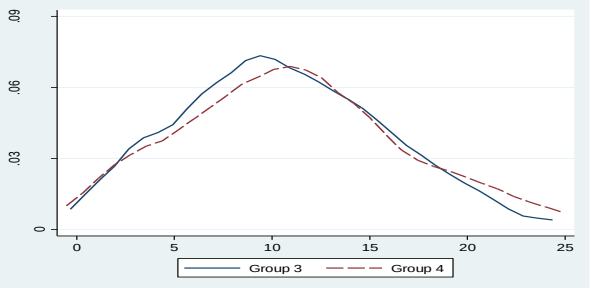


Survey quantity (number of surveys per day)

Panel C: Incentive Effect of Career Incentives (G3 vs. G4)

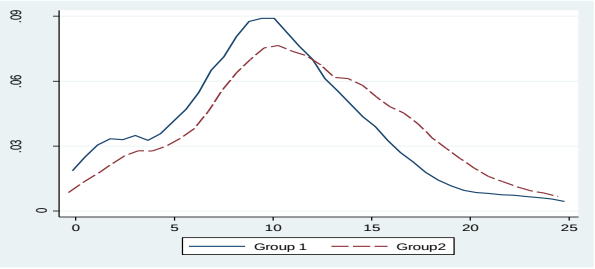


Survey quality (error rate)

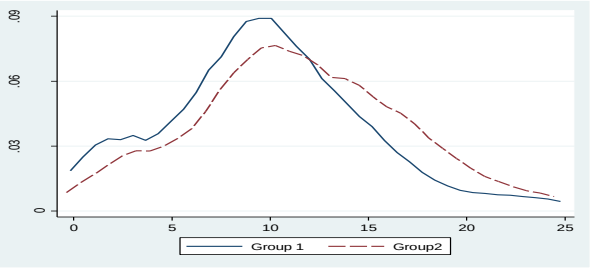


Survey quantity (number of surveys per day)

Panel D: Incentive Effect of Wage Incentives (G1 vs. G2)



Survey quality (error rate)



Survey quantity (number of surveys per day)

Notes: Panel A presents kernel density estimates of quiz score and practice survey error rate during the training. The *Internship* group received an unpaid job offer with career incentives in the first stage, while the *Wage* group received a non-renewable paid job offer in the first stage. Panels B, C, and D present kernel density estimates of survey quality and survey quantity. Groups 1 and 2 received career incentives in the first stage, but only Group 2 received additional wage incentives in the second stage. Groups 3 and 4 received wage incentives in the first stage, but only Group 3 received additional career incentives in the second stage.

Table 1 Experiment Stages

Stage of experiment			Number of individuals					
			G1 (career incentives only)	G2 (career incentives and additional wage incentives)	G3 (wage incentives and additional career incentives)	G4 (wage incentives only)	p-value	Total
A	Target study subjects	2011 Dec	220		220		-	440
B	Study participants (baseline survey participants)	2014 Dec	186 (84.1%)		176 (80.0%)		.265	362
C	Trainees	2015 Jan	74 (39.8%)		74 (42.0%)		.663	148
D	Trainees who failed training		11		0		-	11
E	Enumerators	2015 Jan-Feb	63 (33.9%)		74 (42.0%)		-	137
			33	30	35	39		
F	Number of surveys		4,448	5,298	5,836	5,939	-	21,521

Notes: The proportions of individuals remaining over experiment stages are in parentheses. The number of participants in the stage B is divided by the number of participants in the stage A, and the number of participants in the stages C and E are divided by the number of participants in the stage B.

Table 2 Randomization Balance Check

Variable	Number of observations	Internship group	Wage group	Mean difference (p-value) Internship vs Wage	Mean difference (p-value) G2 vs G1	Mean difference (p-value) G3 vs G4
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: 2014 baseline survey						
Age	362	20.5	20.4	.065	-.200	-.207
		(.120)	(.126)	(.707)	(.629)	(.520)
Number of siblings	362	4.60	4.17	.432**	-5.00	-.158
		(.132)	(.134)	(.022)	(.315)	(.650)
Asset score	362	1.09	1.19	-.102	.133	.048
		(.066)	(.067)	(.282)	(.489)	(.799)
Currently working	362	.097	.074	.023	-.036	-.006
		(.022)	(.020)	(.436)	(.514)	(.913)
Self-esteem	362	19.4	19.3	.158	-.441	-.768
		(.283)	(.265)	(.684)	(.662)	(.341)
Intrinsic motivation	362	3.10	3.09	.016	-.033	-.075
		(.024)	(.026)	(.645)	(.642)	(.372)
Extrinsic motivation	361	2.84	2.84	.004	-.031	.004
		(.021)	(.021)	(.896)	(.646)	(.956)
Extroversion	358	3.61	3.47	.145	-.055	-.246
		(.082)	(.091)	(.237)	(.851)	(.393)
Agreeableness	362	5.10	5.10	.008	.035	-.268
		(.106)	(.103)	(.955)	(.927)	(.408)
Conscientiousness	361	5.69	5.68	.016	-.094	-.054
		(.098)	(.103)	(.908)	(.779)	(.850)
Emotional stability	360	5.08	5.06	.018	-.064	-.190
		(.109)	(.107)	(.905)	(.868)	(.591)
Openness to experiences	362	5.14	5.10	.043	.035	-.268
		(.114)	(.103)	(.778)	(.927)	(.408)
Time preference	334	.394	.398	-.004	-.072*	.013
		(.011)	(.011)	(.783)	(.050)	(.697)
Risk preference	335	.629	.642	-.012	-.008	-.033*
		(.007)	(.006)	(.181)	(.714)	(.077)
Rational decision-making ability	334	.817	.836	-.019	-.037	-.007
		(.012)	(.011)	(.234)	(.353)	(.820)
MSCE score	362	-.035	.010	-.045	-.061	-.101
		(.061)	(.067)	(.620)	(.751)	(.596)
Raven and O*NET score	362	-.009	.076	-.084	-.112	.069
		(.049)	(.049)	(.224)	(.519)	(.635)

Table 2 Randomization Balance Check (continued)

Variable	Number of observation	Internship group	Wage group	Mean difference (p-value) Internship vs Wage	Mean difference (p-value) G2 vs G1	Mean difference (p-value) G3 vs G4
	(1)	(2)	(3)	(4)	(5)	(6)
Male circumcision treatment	362	.425	.460	-.035	-.006	-.226**
		(.036)	(.038)	(.498)	(.962)	(.042)
HIV/AIDS education treatment	362	.511	.443	.068	-.009	.030
		(.037)	(.038)	(.199)	(.943)	(.800)
Scholarship treatment	362	.414	.500	-.086	.021	-.024
		(.036)	(.038)	(.101)	(.868)	(.838)
Transportation reimburse	362	1525	1547.7	-22.7	-103.9	-57.2
		(43.8)	(41.8)	(.708)	(.516)	(.707)
Panel B: Characteristics of dispatched catchment areas						
Number of households per enumerator	137	155.3	159.1	-3.79	40.6***	14.5
		(5.09)	(7.48)	(.676)	(.000)	(.335)
Catchment area size	137	3.11	3.45	-.335	.361	.238
		(.133)	(.255)	(.248)	(.178)	(.657)
Family size	137	3.94	3.79	.148	.022	.114
		(.068)	(.081)	(.165)	(.870)	(.486)
Household asset score	137	.241	.253	-.012	.017	.028*
		(.006)	(.007)	(.201)	(.170)	(.058)
Birth rate	137	.071	.065	.006**	-.005	.010**
		(.002)	(.002)	(.019)	(.119)	(.026)
Death rate	137	.006	.006	.000	-.001	-.001
		(.001)	(.001)	(.981)	(.590)	(.717)
Malaria incidence (under age 3)	137	.525	.513	.012	.063**	-.018
		(.014)	(.019)	(.615)	(.025)	(.651)
Number of Observations		186	176		63	74

Notes: Standard errors are reported in parentheses in column (2) and (3). ***, **, and * denote the significance level at 1%, 5%, and 10%, respectively. Asset score is the number of items owned by a household out of the following: an improved toilet, a refrigerator, and a bicycle. See Data Appendix A.1 for detailed definitions of MSCE score, Raven and O*NET score, and non-cognitive trait variables. Male circumcision treatment, HIV/AIDS education treatment, and scholarship treatment are binary indicators for the treatment status of AFF's previous projects. Number of households is the average number of households that each enumerator was supposed to survey. Catchment area size is the land size subjectively determined by AFF supervisors in consultation with local health workers on a scale from 1 to 10. Family size is the average number of family members per household. Birth rate is the average number of births in the last 3 years per household. Death rate is the number of deaths in the last 12 months per household.

Table 3 Job Offer Acceptance by Individual Trait

Dependent Variable (Job offer acceptance)	(1)	(2) Age	(3) Number of siblings	(4) Asset score	(5) Currently working	(6) Self-esteem	(7) Intrinsic motivation	(8) Extrinsic motivation	(9) Extroversion
Trait		.042	.038*	-.068*	-.107	-.024**	-.012	-.019	-.058*
		(.030)	(.019)	(.040)	(.136)	(.010)	(.108)	(.136)	(.032)
Internship group	-.024	-.323	-.029	-.023	-.025	-.321	.521	.733	-.297*
	(.052)	(.747)	(.131)	(.085)	(.055)	(.278)	(.491)	(.520)	(.173)
Trait * Internship group		.015	-.002	-.009	.028	.015	-.176	-.266	.077*
		(.037)	(.028)	(.054)	(.180)	(.014)	(.157)	(.182)	(.046)
Constant	.481***	-.372	.326***	.558***	.491***	.931***	.517	.537	.683***
	(.055)	(.613)	(.094)	(.073)	(.057)	(.205)	(.336)	(.387)	(.126)
Observations	362	362	362	362	362	362	362	361	358
R-squared	.018	.046	.036	.036	.021	.034	.027	.031	.027
Mean (SD)		20.4(1.65)	4.39(1.80)	1.14(.896)	.086(.280)	19.3(3.69)	3.09(.340)	2.84(.282)	3.54(1.16)

Dependent Variable (Job offer acceptance)	(10) Agreeableness	(11) Conscientiousness	(12) Emotional stability	(13) Openness to experiences	(14) Time preference	(15) Risk preference	(16) Rational decision-making ability	(17) MSCE score	(18) Raven and O*NET score
Trait	-.001	.046*	.011	-.001	.196	.288	-.019	-.051	-.140***
	(.027)	(.026)	(.027)	(.027)	(.284)	(.498)	(.274)	(.040)	(.053)
Internship group	.025	.251	.145	.041	-.096	.388	-.228	-.028	-.035
	(.196)	(.216)	(.195)	(.187)	(.158)	(.413)	(.305)	(.052)	(.052)
Trait * Internship group	-.010	-.049	-.033	-.013	.199	-.644	.257	-.033	-.050
	(.037)	(.037)	(.037)	(.035)	(.384)	(.640)	(.363)	(.056)	(.071)
Constant	.486***	.223	.426***	.485***	.407***	.299	.502**	.483***	.496***
	(.148)	(.152)	(.148)	(.148)	(.130)	(.324)	(.234)	(.055)	(.053)
Observations	362	361	360	362	334	335	334	362	362
R-squared	.019	.026	.020	0.019	.024	.019	.019	0.033	.069
Mean (SD)	5.11(1.39)	5.68(1.35)	5.07(1.45)	5.36(1.35)	.396(.144)	.635(.083)	.826(.149)	-.013(.857)	.037(.658)

Notes: Robust standard errors are reported in parentheses. ***, **, and * denote the significance level at 1%, 5%, and 10%, respectively. Asset score is the sum of items owned out of improved toilet, refrigerator, and bicycle. See Data Appendix A.1 for the definitions of MSCE score, Raven and O*NET score, and non-cognitive trait variables.

Table 4: Training Performance

Dependent variable	Quiz score		Practice survey error rate		
	(1)	(2)	(3)	(4)	(5)
Panel A: 148 Trainee Sample					
Internship group	-2.01***	-1.96***	.104***	.089***	.323
	(.344)	(.303)	(.026)	(.029)	(.206)
Observations	148	148	148	148	148
R-squared	.228	.534	.114	.239	.811
Wage Group Mean (SD)	8.43 (1.82)		.272 (.142)		
Panel B: 137 Enumerator Sample					
Internship group	-1.44***	-1.47***	.094***	.080***	.302
	(.329)	(.286)	(.028)	(.030)	(.210)
Observations	137	137	137	137	137
R-squared	.163	.511	.099	.243	.862
Wage Group Mean (SD)	8.43 (1.82)		.272 (.142)		
Individual characteristics	No	YES	No	No	YES
Practice survey pair FE	No	No	No	YES	YES

Notes: Robust standard errors are reported in parentheses. ***, **, and * denote the significance level at 1%, 5%, and 10%, respectively. All specifications (columns 1-5) include the number of siblings and binary indicators for previous AFF programs. The practice survey error rate regression includes a binary indicator for the survey questionnaire type. Columns 2, 4, and 5 include age, asset score, MSCE score, Raven and O*NET score, and a set of non-cognitive traits (self-esteem, intrinsic and extrinsic motivation, and Big 5 personality items). Column 5 includes dummies for each trainee pair who conducted the practice survey with each other.

Table 5 Selection and Incentive Effects of Work Incentives on Job Performance

VARIABLES	Survey quality (error rate)			Survey quantity (number of surveys per day)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Selection effect (G2 vs G3)						
G2	-.022**	-.023**	-.023**	1.39**	1.29**	1.09*
	(.009)	(.009)	(.009)	(.610)	(.542)	(.611)
Observations	11,130	11,130	11,130	1,003	1,003	1,003
R-squared	.162	.307	.308	.145	.170	.180
Mean (SD) of G3	.077 (.078)			10.7 (5.45)		
Panel B: Incentive effect of career incentives (G3 vs. G4)						
G3	.007	.006	.006	-.763	-1.14*	-1.14*
	(.009)	(.010)	(.010)	(.681)	(.628)	(.613)
Observations	11,775	11,775	11,775	1,063	1,063	1,063
R-squared	.189	.269	.276	.152	.195	.199
Mean (SD) of G4	.082 (074)			11.5 (6.36)		
Panel C: Incentive effect of wage (G1 vs. G2)						
G2	-.038**	-.022**	-.019*	1.05	.644	.247
	(.016)	(.010)	(.010)	(.879)	(.941)	(.999)
Observations	9,779	9,779	9,779	914	914	914
R-squared	.178	.357	.358	.203	.232	.242
Mean (SD) of G1	.075 (.068)			9.84 (5.19)		
Panel D: Combined effect (G1 vs. G4)						
G1	-.001	-.003	-.005	-1.41	-.732	-.259
	(.015)	(.013)	(.013)	(1.31)	(1.18)	(1.06)
Observations	10,424	10,424	10,424	974	974	974
R-squared	.194	.276	.277	.157	.232	.235
Mean (SD) of G4	.082 (074)			11.5 (6.36)		
Individual characteristics	NO	YES	YES	NO	YES	YES
Training performance	NO	NO	YES	NO	NO	YES

Notes: Robust standard errors clustered at the catchment area level are reported in parentheses. ***, **, and * denote the significance level at 1%, 5%, and 10%, respectively. All specifications (columns 1−6) include the number of siblings, catchment area characteristics, supervisor team-specific post-visit variables, survey date-fixed effect, and binary indicator variables for previous AFF programs. Catchment area characteristics include the total number of households, catchment area size, family size, asset score, number of births in the last 3 years, incidence of malaria among children under 3, and deaths in the last 12 months. Columns 2, 3, 5, and 6 include age, asset score, MSCE score, Raven and O*NET score, and a set of non-cognitive traits (self-esteem, intrinsic and extrinsic motivation, and Big 5 personality items). Columns 3 and 6 additionally include the two measures of training performances: the quiz score and practice survey error rate.

Online Appendix (not for publication)

Appendix Tables

Table A.1: Randomization balance check between treatment and non-selected groups

Variable	Number of observations	Internship + Wage group	Non-selected group	Mean difference (p-value) Internship + Wage vs Non-selected
	(1)	(2)	(3)	(4)
Panel A: 2011 secondary school census survey				
Height	534	164.6	164.0	.500
		(.374)	(.664)	(.512)
Weight	535	53.7	53.2	.437
		(.365)	(.780)	(.613)
Age in 2011	536	16.1	15.9	.206
		(.075)	(.149)	(.218)
Living with a father	536	.630	.688	-.058
		(.023)	(.048)	(.275)
Living with a mother	536	.730	.750	-.020
		(.021)	(.044)	(.678)
Asset score in 2011	530	2.58	2.46	.114
		(.080)	(.176)	(.555)
Subjective health is good or very good	536	.450	.458	-.008
		(.024)	(.051)	(.883)
Raven's matrices test score	452	19.7	20.1	-.445
		(.262)	(.500)	(.433)
Number of observations	536	440	96	
Panel B: 2014 baseline survey				
Age in 2014	443	20.4	20.0	.395**
		(.087)	(.159)	(.031)
Number of siblings	443	4.39	4.32	.071
		(.094)	(.226)	(.771)
Asset score in 2014	443	1.14	1.22	-.084
		(.047)	(.102)	(.457)
Currently working	442	.086	.100	-.014
		(.015)	(.033)	(.697)
Self-esteem	443	19.3	20.0	-.706
		(.194)	(.426)	(.134)
Intrinsic motivation	443	3.09	3.10	-.005
		(.018)	(.041)	(.912)
Extrinsic motivation	442	2.84	2.81	.026
		(.015)	(.033)	(.480)

Table A.1: Randomization balance check between treatment and non-selected groups (continued)

Variable	Number of observations	Internship + Wage group	Non-selected group	Mean difference (p-value) Internship + Wage vs Non-selected
	(1)	(2)	(3)	(4)
Extroversion	433	3.54	3.44	.103
		(.061)	(.149)	(.523)
Agreeableness	443	5.10	5.46	-.356**
		(.074)	(.149)	(.034)
Conscientiousness	442	5.69	6.17	-.487***
		(.071)	(.137)	(.002)
Emotional stability	439	5.07	5.31	-.237
		(.076)	(.171)	(.207)
Openness to experiences	443	5.12	5.45	-.332
		(.077)	(.194)	(.115)
Time preference	402	.396	.366	.030
		(.008)	(.016)	(.101)
Risk preference	403	.635	.656	-.020*
		(.005)	(.011)	(.089)
Rational decision-making ability	402	.826	.786	.040*
		(.008)	(.020)	(.068)
MSCE score	443	-.013	.047	-.060
		(.045)	(.097)	(.576)
Raven and O*NET score	443	.032	-.145	.177**
		(.035)	(.076)	(.036)
Male circumcision treatment	443	.442	.506	-.064
		(.026)	(.056)	(.300)
HIV/AIDS education treatment	443	.478	.506	-.028
		(.026)	(.056)	(.648)
Scholarship treatment	443	.456	.469	-.013
		(.026)	(.056)	(.829)
Transportation reimburse	443	1536	1511.1	24.9
		(30.3)	(69.2)	(.742)
Number of observations	443	362	81	

Notes: Standard errors are reported in parentheses in column (2) and (3). ***, **, and * denote the significance level at 1%, 5%, and 10%, respectively. Both asset scores in 2011 and 2014 are the number of items owned by a household out of the following: an improved toilet, a refrigerator, and a bicycle. See Data Appendix A.1 for detailed definitions of MSCE score, Raven and O*NET score, and non-cognitive trait variables. Male circumcision treatment, HIV/AIDS education treatment, and scholarship treatment are binary indicators for the treatment status of AFF's previous projects.

Table A.2: Individual characteristics between baseline survey participants and non-participants

Variable	Participants	Non-participants	Mean difference between participants and non-participants (p-value)
	(1)	(2)	(3)
Height	164.6	164.5	.071
	(.420)	(.818)	(.939)
Weight	53.6	54.1	-.486
	(.377)	(1.09)	(.674)
Age	16.1	16.0	.134
	(.078)	(.222)	(.571)
Living with a father	.667	.622	-.045
	(.054)	(.026)	(.450)
Living with a mother	.740	.679	.061
	(.023)	(.053)	(.296)
Asset score	2.46	3.12	-.656***
	(.086)	(.197)	(.003)
Subjective health (good or very good)	.428	.551	-.123*
	(.026)	(.057)	(.051)
Raven's matrices test score	19.9	18.8	1.04
	(.274)	(.785)	(.216)
Number of observations	362	78	

Notes: Standard errors are reported in parentheses in column (1) and (2). ***, **, and * denote the significance level at 1%, 5%, and 10%, respectively. The statistics are calculated based on data from the 2011 secondary school survey. Columns (1) and (2) show group-specific means and standard deviations. 440 male secondary school graduates were randomly selected to receive a job offer without prior notice, but only 362 showed up on the survey date. Asset score is the number of items owned by a household out of the following: an improved toilet, a refrigerator, and a bicycle. See Data Appendix A.1 for detailed definitions of Raven's matrices test.

Table A.3: Characteristics difference between enumerators by the supervision teams

Variable	Number of observations	Supervision team 1	Mean difference (p-value) vs. team 2	Mean difference (p-value) vs. team 3	Mean difference (p-value) vs. team 4	Mean difference (p-value) vs. team 5
	(1)	(2)	(4)	(4)	(5)	(6)
Age	137	20.9	.170	.537	.228	.218
		(1.48)	(.667)	(.199)	(.570)	(.611)
Number of siblings	137	4.46	-.146	-.060	-.195	-.580
		(2.10)	(.771)	(.903)	(.718)	(.297)
Asset score	137	1.04	.181	-.179	.070	.038
		(.720)	(.371)	(.494)	(.732)	(.837)
Currently working	137	.154	.118	.110	.154**	.154**
		(.368)	(.151)	(.198)	(.043)	(.043)
Self-esteem	137	18.3	-.446	-.090	-.930	-1.19
		(2.99)	(.624)	(.930)	(.331)	(.207)
Intrinsic motivation	137	3.05	-.054	.019	-.097	.013
		(.342)	(.539)	(.874)	(.232)	(.882)
Extrinsic motivation	137	2.81	-.026	-.039	-.001	.048
		(.210)	(.702)	(.592)	(.992)	(.493)
Extroversion	137	3.67	.298	.564	-.061	.194
		(1.20)	(.396)	(.125)	(.840)	(.543)
Agreeableness	137	4.81	-.389	-.931**	-.239	.058
		(1.41)	(.310)	(.023)	(.523)	(.891)
Conscientiousness	137	5.81	.361	-.410	.026	.120
		(1.23)	(.282)	(.212)	(.940)	(.731)
Emotional stability	137	5.17	.405	-.001	.111	-.014
		(1.32)	(.329)	(.998)	(.773)	(.969)
Openness to experiences	137	4.81	-.389	-.931**	-.239	.058
		(1.41)	(.310)	(.023)	(.523)	(.891)
Time preference	122	.409	-.032	-.038	.013	.021
		(.135)	(.481)	(.263)	(.705)	(.576)
Risk preference	122	.649	.022	.026	.013	.012
		(.074)	(.308)	(.321)	(.515)	(.589)
Rational decision-making ability	122	.818	-.057	.000	-.001	-.033
		(.124)	(.107)	(.992)	(.986)	(.312)
MSCE score	137	-.271	-.263	-.121	-.204	-.243
		(.852)	(.264)	(.603)	(.326)	(.288)
Raven and O*NET score	137	-.208	-.152	-.149	-.078	-.217
		(.619)	(.354)	(.452)	(.633)	(.219)
Male circumcision treatment	137	.346	-.082	.085	-.060	.013
		(.485)	(.543)	(.526)	(.645)	(.926)
HIV/AIDS education treatment	137	.538	.181	-.027	.163	-.003
		(.508)	(.188)	(.855)	(.222)	(.982)
Scholarship treatment	137	.346	-.190	-.176	-.123	-.112
		(.485)	(.167)	(.225)	(.352)	(.430)
Transportation reimburse	137	1546.2	-78.8	-214.7	-132.0	-20.5
		(471.8)	(.631)	(.191)	(.402)	(.896)
Number of Observations		26	54	49	58	50

Notes: Standard deviations are reported in parentheses in column (2). ***, **, and * denote the significance level at 1%, 5%, and 10%, respectively. Asset score is the number of items owned by a household out of the following: an improved toilet, a refrigerator, and a bicycle. See Data Appendix A.1 for detailed definitions of MSCE score, Raven and O*NET score, and non-cognitive trait variables. Male circumcision treatment, HIV/AIDS education treatment, and scholarship treatment are binary indicators for the treatment status of AFF's previous projects.

Table A.4: Individual characteristics after job offer acceptance

Variable	Number of observations	Internship group offer takers	Wage group offer takers	Mean Difference	Standard Deviation
	(1)	(2)	(3)	(4)=(2)-(3)	(5)
Age	148	20.8	20.7	.162	1.46
BMI	148	19.9	19.5	.413	2.08
Number of siblings	148	4.86	4.46	.405	1.70
Asset score	148	.932	1.05	-.122	.804
Currently working	148	.081	.054	.027	.252
Self-esteem	148	19.1	18.6	.521	3.71
Intrinsic motivation	148	3.05	3.08	-.029	.326
Extrinsic motivation	148	2.79	2.83	-.046	.274
Extroversion	148	3.68	3.27	.405**	1.19
Agreeableness	148	5.03	5.10	-.074	1.44
Conscientiousness	148	5.68	5.87	-.196	1.26
Emotional stability	148	4.95	5.13	-.182	1.50
Openness to experiences	148	5.03	5.10	-.074	1.44
Time preference	137	.414	.411	.003	.136
Risk preference	137	.621	.645	-.024*	.079
Rational decision-making ability	137	.831	.834	-.004	.139
MSCE score	148	-.178	-.096	-.081	.766
Raven and O*NET score	148	-.214	-.064	-.150	.640
Male circumcision treatment	148	.392	.338	.054	.483
HIV/AIDS education treatment	148	.473	.473	.000	.501
Scholarship treatment	148	.459	.473	-.014	.501
Transportation reimburse	148	1602.7	1652.7	-50.0	628.2
F-statistics (p-value)				.910 (.576)	
Number of Individuals		74	74	148	148

Notes: ***, **, and * denote the significance level at 1%, 5%, and 10%, respectively. Asset score is the number of items owned by a household out of the following: an improved toilet, a refrigerator, and a bicycle. See Data Appendix A.1 for detailed definitions of MSCE score, Raven and O*NET score, and non-cognitive trait variables. Male circumcision treatment, HIV/AIDS education treatment, and scholarship treatment are binary indicators for the treatment status of AFF's previous projects.

Table A.5: Selection and incentive effects of work incentives on job performance: subjective performance evaluation

VARIABLES	Subjective performance evaluation (by survey respondents)			Subjective performance evaluation (by supervisors)		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Selection effect (G2 vs G3)						
G2	1.42***	1.32**	1.37**	-.181*	-.145	-.141
	(.499)	(.523)	(.583)	(.097)	(.104)	(.098)
Observations	6,464	6,464	6,464	65	65	65
R-squared	.528	.662	.668	.410	.641	.652
Mean (SD) of G3	2.09 (1.65)			.850 (.163)		
Panel B: Incentive effect of career incentives (G3 vs. G4)						
G3	.199	.387	.341	.300***	.324***	.340***
	(.500)	(.446)	(.456)	(.038)	(.043)	(.045)
Observations	7,223	7,223	7,223	74	74	74
R-squared	.411	.522	.527	.620	.732	.752
Mean (SD) of G4	2.08 (1.59)			.583 (.119)		
Panel C: Incentive effect of wage incentives (G1 vs. G2)						
G2	.925	.113	.053	-.206	-.218	-.277
	(.893)	(.741)	(.789)	(.223)	(.268)	(.228)
Observations	4,512	4,512	4,512	63	63	63
R-squared	.560	.711	.746	.392	.516	.571
Mean (SD) of G1	2.67 (1.66)			.803 (.162)		
Panel D: Combined effect (G1 vs. G4)						
G1	-1.07	-.975*	-.889	.305***	.340***	.331***
	(.752)	(.543)	(.559)	(.081)	(.107)	(.111)
Observations	5,271	5,271	5,271	72	72	72
R-squared	.614	.712	.714	.614	.679	.686
Mean (SD) of G4	2.09 (1.59)			.583 (.119)		
Individual characteristics	NO	YES	YES	NO	YES	YES
Training performance	NO	NO	YES	NO	NO	YES

Notes: Robust standard errors clustered at the catchment area level are reported in parentheses. ***, **, and * denote the significance level at 1%, 5%, and 10%, respectively. All specifications (columns 1−6) include the number of siblings, catchment area characteristics, supervisor team-specific post-visit variables, survey date-fixed effect, and binary indicator variables for previous AFF programs. Catchment area characteristics include the total number of households, catchment area size, family size, asset score, number of births in the last 3 years, incidence of malaria among children under 3, and deaths in the last 12 months. Columns 2, 3, 5, and 6 include the following individual characteristics: age, asset score, MSCE score, Raven's matrices test score and O*NET test score, and a set of non-cognitive traits (self-esteem, intrinsic and extrinsic motivation, and Big 5 personality items). Columns 3 and 6 include the following training performances: the quiz score and practice survey error rate.

Table A.6: Selection and incentive effects of work incentives on job performance: additional outcomes

VARIABLES	Survey quality				Survey quantity					
	Proportion of entries incorrectly entered		Proportion of entries incorrectly blank		Work hours (in mins)		Survey time per household (in mins)		Intermission time between surveys (in mins)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Selection effect (G2 vs G3)										
G2	0.00	-.001	-.023***	-.021**	2.16	-1.16	-1.20	-.751	-2.90	-1.94
	(.003)	(.002)	(.008)	(.008)	(25.0)	(23.7)	(1.10)	(.887)	(3.10)	(2.39)
Observations	11,130	11,130	11,130	11,130	988	988	11,130	11,130	8,222	8,222
R-squared	.112	.267	.152	.265	.147	.179	.285	.326	.022	.032
Mean (SD) of G3	.016 (.018)		.062 (.070)		422.7 (198.7)		25.5 (11.9)		23.1 (50.2)	
Panel B: Incentive effect of career incentives (G3 vs. G4)										
G3	.001	-0.00	.007	.006	44.4**	31.6	1.25	1.17	7.54***	6.60***
	(.003)	(.003)	(.007)	(.008)	(21.5)	(21.3)	(1.16)	(1.15)	(1.96)	(1.75)
Observations	11,775	11,775	11,775	11,775	1,053	1,053	11,775	11,775	9,040	9,040
R-squared	.168	.301	.167	.225	.145	.173	.251	.270	.020	.027
Mean (SD) of G4	.019 (.021)		.063 (.066)		387.7 (194.1)		23.9 (11.2)		17.3 (44.0)	
Panel C: Incentive effect of wage incentives (G1 vs. G2)										
G2	-.007	-.006	-.031**	-.013	19.0	30.8	-2.74	.189	-.115	1.42
	(.004)	(.005)	(.015)	(.010)	(39.7)	(42.8)	(1.97)	(2.20)	(4.51)	(4.46)
Observations	9,779	9,779	9,779	9,779	888	888	9,780	9,780	7,205	7,205
R-squared	.103	.237	.161	.302	.189	.228	.305	.345	.022	.032
Mean (SD) of G1	.019 (.019)		.056 (.061)		383.1 (187.1)		27.4 (12.1)		19.3 (41.8)	
Panel D: Combined effect (G1 vs. G4)										
G1	.007	.014**	-.008	-.019*	-21.7	-29.8	1.97	-.779	1.52	-.618
	(.004)	(.005)	(.012)	(.010)	(33.5)	(26.3)	(1.63)	(1.86)	(2.31)	(1.87)
Observations	10,424	10,424	10,424	10,424	953	953	10,425	10,425	8,023	8,023
R-squared	.158	.272	.167	.240	.156	.194	.283	.335	.015	.024
Mean (SD) of G4	.019 (.021)		.063 (.066)		387.7 (194.1)		23.9 (11.2)		17.3 (44.0)	
Individual characteristics	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Training performance	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES

Notes: Robust standard errors clustered at the catchment area level are reported in parentheses. ***, **, and * denote the significance level at 1%, 5%, and 10%, respectively. All specifications (columns 1–10) include the number of siblings, catchment area characteristics, supervisor team-specific post-visit variables, survey date-fixed effect, and binary indicator variables for previous AFF programs. Catchment area characteristics include the total number of households, catchment area size, family size, asset score, number of births in the last 3 years, incidence of malaria among children under 3, and deaths in the last 12 months. Columns 2, 4, 6, 8, and 10 include the following individual characteristics: age, asset score, MSCE score, Raven's matrices test score and O*NET test score, and a set of non-cognitive traits (self-esteem, intrinsic and extrinsic motivation, and Big 5 personality items).

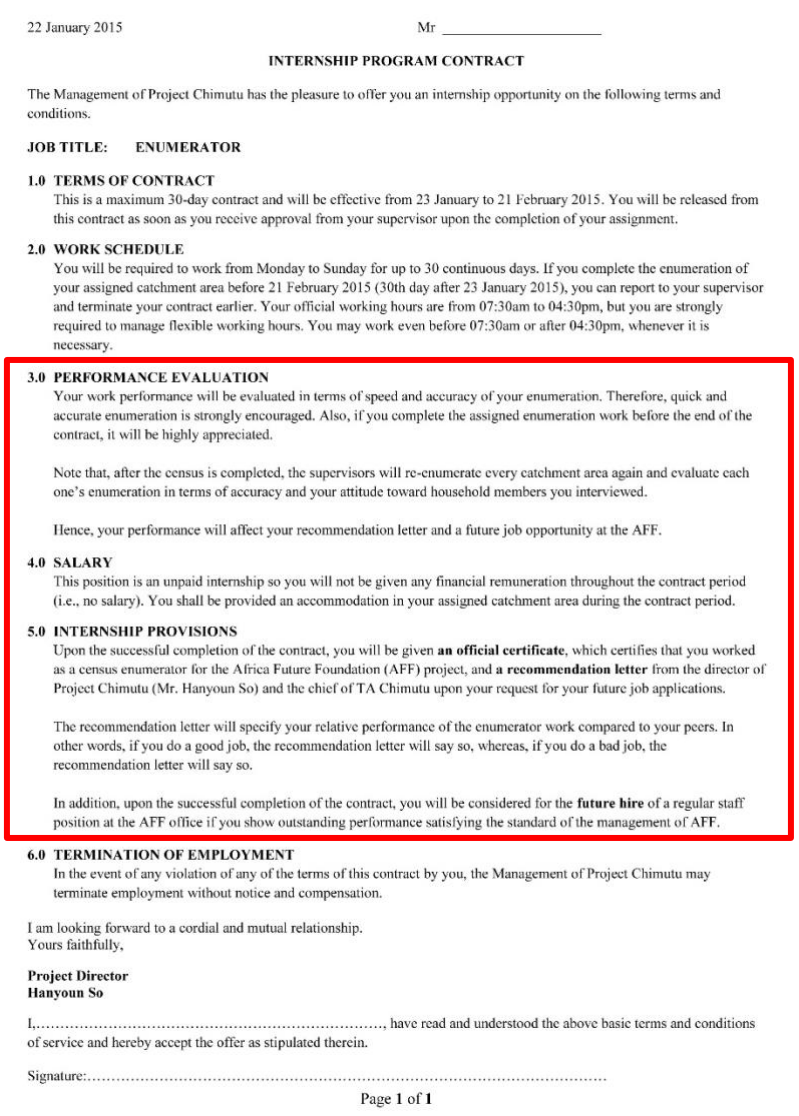
Table A.7: Selection and incentive effects of work incentives on job performance after excluding 11 enumerators from the *Wage* group

VARIABLES	Survey quality (error rate)			Survey quantity (number of surveys)			Subjective performance evaluation (by survey respondents)			Subjective performance evaluation (by supervisors)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Selection effect (G2 vs G3)												
G2	-.009	-.018**	-.015**	1.74***	1.68***	1.39**	1.39**	1.51***	1.49***	-.193**	-.183	-.179
	(.007)	(.007)	(.007)	(.599)	(.519)	(.632)	(.515)	(.459)	(.517)	(.089)	(.153)	(.156)
Observations	10,150	10,150	10,150	917	917	917	5,899	5,899	5,899	59	59	59
R-squared	.176	.306	.309	.152	.178	.185	.533	.679	.686	.402	.681	.691
Mean (SD) of G3	.067 (.064)			10.6 (5.60)			2.11 (1.66)			.845 (.169)		
Panel B: Incentive effect of career incentives (G3 vs. G4)												
G3	.003	.009	.009	-1.75**	-2.04***	-2.02***	.457	.561	.480	.329***	.340***	.335***
	(.010)	(.009)	(.009)	(.771)	(.726)	(.713)	(.530)	(.441)	(.432)	(.056)	(.068)	(.069)
Observations	9,666	9,666	9,666	876	876	876	5,977	5,977	5,977	63	63	63
R-squared	.213	.269	.270	.187	.214	.218	.384	.574	.581	.610	.767	.786
Mean (SD) of G4	.085 (.076)			11.5 (6.47)			1.94 (1.52)			.596 (.123)		
Panel C: Incentive effect of wage incentives (G1 vs. G2)												
G2	-.038**	-.022**	-.019*	1.05	.644	.247	.925	.113	.053	-.206	-.217	-.272
	(.016)	(.010)	(.010)	(.879)	(.941)	(.999)	(.893)	(.741)	(.789)	(.222)	(.265)	(.224)
Observations	9,779	9,779	9,779	914	914	914	4,512	4,512	4,512	63	63	63
R-squared	.178	.357	.358	.203	.232	.242	.560	.711	.746	.392	.516	.570
Mean (SD) of G1	.075 (.068)			9.84 (5.19)			2.67 (1.66)			.803 (.162)		
Panel D: Combined effect (G1 vs. G4)												
G1	-.003	-.003	-.001	-.916	-.346	.752	-.673	-.053	.040	.233***	.179	.147
	(.014)	(.013)	(.015)	(1.41)	(1.27)	(.942)	(.693)	(.599)	(.534)	(.062)	(.111)	(.122)
Observations	9,295	9,295	9,295	873	873	873	4,590	4,590	4,590	67	67	67
R-squared	.197	.283	.292	.179	.240	.248	.653	.764	.773	.626	.737	.749
Mean (SD) of G4	.085 (.076)			11.5 (6.47)			1.94 (1.52)			.596 (.123)		
Individual characteristics	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES
Training performance	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES

Notes: 11 enumerators in the Wage group whose training performance is the lowest are excluded. Robust standard errors clustered at the catchment area level are reported in parentheses. ***, **, and * denote the significance level at 1%, 5%, and 10%, respectively. All specifications (columns 1–12) include the number of siblings, catchment area characteristics, supervisor team-specific post-visit variables, survey date-fixed effect, and binary indicator variables for previous AFF programs. Catchment area characteristics include the total number of households, catchment area size, family size, asset score, number of births in the last 3 years, incidence of malaria among children under 3, and deaths in the last 12 months. Columns 2, 3, 5, 6, 8, 9, 11, and 12 include the following individual characteristics: age, asset score, MSCE score, Raven's matrices test score and O*NET test score, and a set of non-cognitive traits (self-esteem, intrinsic and extrinsic motivation, and Big 5 personality items). Columns 3, 6, 9, and 12 include the following training performances: the quiz score and practice survey error rate.

Appendix Figures

Figure A.1: Contract letter for Group 1 (G1)



22 January 2015 Mr _____

INTERNSHIP PROGRAM CONTRACT

The Management of Project Chimutu has the pleasure to offer you an internship opportunity on the following terms and conditions.

JOB TITLE: ENUMERATOR

1.0 TERMS OF CONTRACT
This is a maximum 30-day contract and will be effective from 23 January to 21 February 2015. You will be released from this contract as soon as you receive approval from your supervisor upon the completion of your assignment.

2.0 WORK SCHEDULE
You will be required to work from Monday to Sunday for up to 30 continuous days. If you complete the enumeration of your assigned catchment area before 21 February 2015 (30th day after 23 January 2015), you can report to your supervisor and terminate your contract earlier. Your official working hours are from 07:30am to 04:30pm, but you are strongly required to manage flexible working hours. You may work even before 07:30am or after 04:30pm, whenever it is necessary.

3.0 PERFORMANCE EVALUATION
Your work performance will be evaluated in terms of speed and accuracy of your enumeration. Therefore, quick and accurate enumeration is strongly encouraged. Also, if you complete the assigned enumeration work before the end of the contract, it will be highly appreciated.

Note that, after the census is completed, the supervisors will re-enumerate every catchment area again and evaluate each one's enumeration in terms of accuracy and your attitude toward household members you interviewed.

Hence, your performance will affect your recommendation letter and a future job opportunity at the AFF.

4.0 SALARY
This position is an unpaid internship so you will not be given any financial remuneration throughout the contract period (i.e., no salary). You shall be provided an accommodation in your assigned catchment area during the contract period.

5.0 INTERNSHIP PROVISIONS
Upon the successful completion of the contract, you will be given **an official certificate**, which certifies that you worked as a census enumerator for the Africa Future Foundation (AFF) project, and **a recommendation letter** from the director of Project Chimutu (Mr. Hanyoun So) and the chief of TA Chimutu upon your request for your future job applications.

The recommendation letter will specify your relative performance of the enumerator work compared to your peers. In other words, if you do a good job, the recommendation letter will say so, whereas, if you do a bad job, the recommendation letter will say so.

In addition, upon the successful completion of the contract, you will be considered for the **future hire** of a regular staff position at the AFF office if you show outstanding performance satisfying the standard of the management of AFF.

6.0 TERMINATION OF EMPLOYMENT
In the event of any violation of any of the terms of this contract by you, the Management of Project Chimutu may terminate employment without notice and compensation.

I am looking forward to a cordial and mutual relationship.
Yours faithfully,

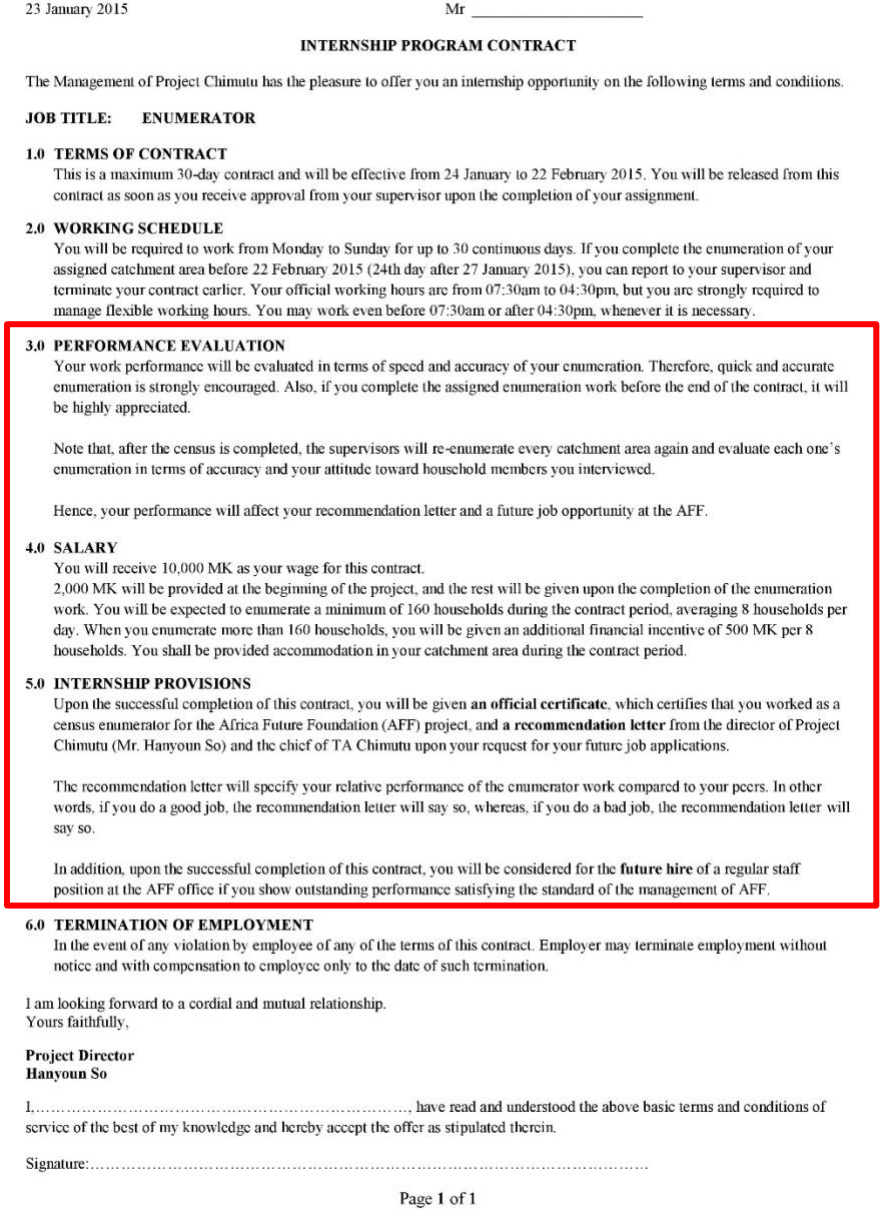
Project Director
Hanyoun So

I,……, have read and understood the above basic terms and conditions of service and hereby accept the offer as stipulated therein.

Signature:……

Page **1** of **1**

Figure A.2: Contract letter for Group 2 (G2) and Group (G3) (the same contract letter for both groups)



23 January 2015 Mr _____

INTERNSHIP PROGRAM CONTRACT

The Management of Project Chimutu has the pleasure to offer you an internship opportunity on the following terms and conditions.

JOB TITLE: ENUMERATOR

1.0 TERMS OF CONTRACT
This is a maximum 30-day contract and will be effective from 24 January to 22 February 2015. You will be released from this contract as soon as you receive approval from your supervisor upon the completion of your assignment.

2.0 WORKING SCHEDULE
You will be required to work from Monday to Sunday for up to 30 continuous days. If you complete the enumeration of your assigned catchment area before 22 February 2015 (24th day after 27 January 2015), you can report to your supervisor and terminate your contract earlier. Your official working hours are from 07:30am to 04:30pm, but you are strongly required to manage flexible working hours. You may work even before 07:30am or after 04:30pm, whenever it is necessary.

3.0 PERFORMANCE EVALUATION
Your work performance will be evaluated in terms of speed and accuracy of your enumeration. Therefore, quick and accurate enumeration is strongly encouraged. Also, if you complete the assigned enumeration work before the end of the contract, it will be highly appreciated.

Note that, after the census is completed, the supervisors will re-enumerate every catchment area again and evaluate each one's enumeration in terms of accuracy and your attitude toward household members you interviewed.

Hence, your performance will affect your recommendation letter and a future job opportunity at the AFF.

4.0 SALARY
You will receive 10,000 MK as your wage for this contract.
2,000 MK will be provided at the beginning of the project, and the rest will be given upon the completion of the enumeration work. You will be expected to enumerate a minimum of 160 households during the contract period, averaging 8 households per day. When you enumerate more than 160 households, you will be given an additional financial incentive of 500 MK per 8 households. You shall be provided accommodation in your catchment area during the contract period.

5.0 INTERNSHIP PROVISIONS
Upon the successful completion of this contract, you will be given **an official certificate**, which certifies that you worked as a census enumerator for the Africa Future Foundation (AFF) project, and **a recommendation letter** from the director of Project Chimutu (Mr. Hanyoun So) and the chief of TA Chimutu upon your request for your future job applications.

The recommendation letter will specify your relative performance of the enumerator work compared to your peers. In other words, if you do a good job, the recommendation letter will say so, whereas, if you do a bad job, the recommendation letter will say so.

In addition, upon the successful completion of this contract, you will be considered for the **future hire** of a regular staff position at the AFF office if you show outstanding performance satisfying the standard of the management of AFF.

6.0 TERMINATION OF EMPLOYMENT
In the event of any violation by employee of any of the terms of this contract. Employer may terminate employment without notice and with compensation to employee only to the date of such termination.

I am looking forward to a cordial and mutual relationship.
Yours faithfully,

Project Director
Hanyoun So

I,……, have read and understood the above basic terms and conditions of service of the best of my knowledge and hereby accept the offer as stipulated therein.

Signature:……

Page **1** of 1

Figure A.3: Contract letter for Group 4 (G4)

AFRICA FUTURE FOUNDATION

26 January 2015 Mr _____

TEMPORARY EMPLOYMENT CONTRACT

The Management of Project Chimutu has the pleasure to offer you a temporary employment opportunity on the following terms and conditions.

JOB TITLE: ENUMERATOR

1.0 TERMS OF CONTRACT

This is a maximum 30-day contract and will be effective from 27 January to 25 February 2015. You will be released from this contract as soon as you receive approval from your supervisor upon the completion of your assignment.

2.0 WORK SCHEDULE

You will be required to work from Monday to Sunday for up to 30 continuous days. If you complete the enumeration of your assigned catchment area before 25 February 2015 (30th day after 27 January 2015), you can report to your supervisor and terminate your contract earlier. Your official working hours are from 07:30am to 04:30pm, but you are strongly required to manage flexible working hours. You may work even before 07:30am or after 04:30pm, whenever it is necessary.

3.0 PERFORMANCE EVALUATION

Your work performance will be evaluated in terms of speed and accuracy of your enumeration. Therefore, quick and accurate enumeration is strongly encouraged. Also, if you complete the assigned enumeration work before the end of the contract, it will be highly appreciated.

Note that, after the census is completed, the supervisors will re-enumerate every catchment area again and evaluate each one's enumeration in terms of accuracy and your attitude toward household members you interviewed.

4.0 SALARY

You will receive 10,000 MK as your wage for this contract.
2,000 MK will be provided at the beginning of the project, and the rest will be given upon the completion of the enumeration work. You will be expected to enumerate a minimum of 160 households during the contract period, averaging 8 households per day. When you enumerate more than 160 households, you will be given an additional financial incentive of 500 MK per 8 households. You shall be provided accommodation in your catchment area during the contract period.

5.0 TERMINATION OF EMPLOYMENT

In the event of any violation of any of the terms of this contract by you, the Management of Project Chimutu may terminate employment without notice and compensation.

I am looking forward to a cordial and mutual relationship.
Yours faithfully,

Project Director
Hanyoun So

I,……, have read and understood the above basic terms and conditions of service and hereby accept the offer as stipulated therein.

Signature:……

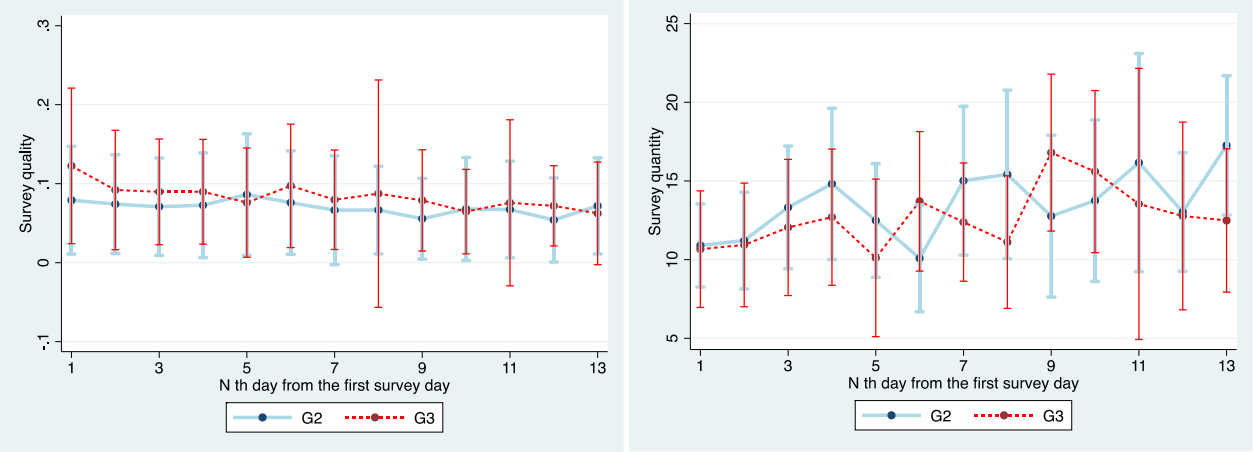
Figure A.4: Training quiz questionnaire

No.	Question	Answer (Point)
1	An important reason for conducting the census is to achieve an improvement of overall quality of health in TA Chimutu. Describe the other two reasons why we conduct the census.	a. To make it possible to reach out to every pregnant woman who wanted to participate in the AFF MCH program. (0.5) b. To enrich the stock of socio-demographic data in T/A Chimutu that is necessary for elaboration of the AFF MCH program. (0.5)
2	Regarding the roles of the enumerator, there are two functions you should NOT perform. Please fill them in the blank spaces below. A) Not to _____ B) Not to _____	a. Not to make any influence on answers (0.5) b. Not to change orders or words of questions (0.5)
3	What is the main standard required for households to be enumerated in the "2015 census of TA Chimutu," a modified version of the "population and housing census"?	Enumeration of all people, all housing units, and all other structures in TA Chimutu, who have stayed in TA Chimutu for more than 3 months during the past 12 months (1)
4	What is the name of the document that proves your eligibility to conduct the census?	Endorsement letter (1)
5	As what kind of structure would you categorize the following? *"A structure with sun-dried brick walls and asbestos roof"*	Semi-permanent (1)
6	Choose one that is <u>not</u> counted as a collective household. A) Hospitals, including three staff houses sharing food B) Lodge, including staff dwelling and sharing food C) Prison with many inmates' dwelling D) Store with owner's dwelling E) Military barracks with soldiers' dwelling	D (1)
7	What is the name of the document you have to sign before you start enumeration?	Consent form (1)
8	What are the three things you have to check before you leave the household?	Questionnaire, outbuildings, and Household ID number. (1, 0.5 point for partially correct)
9	What number do you put when you cannot meet any respondent from the household?	a. Do not put any number and just note down the household. (0.5) b. Put the latest number on it if you arrange to meet later. (0.5)
10	Your distributed alphabet is "C" and this household is the third household you enumerated in the catchment area. How did you place an ID number on the wall of the household?	0003C (1)
11	<u>True or false questions</u> A) It is okay if the questionnaire gets wet when there is heavy rain. B) You should not come to the completion meeting if you did not finish enumeration of your area. C) If you complete enumeration in your area, you should report to your supervisors immediately. D) You should bring all your housing necessities to the kickoff meeting.	A) False (0.5) B) False (0.5) C) True (0.5) D) True (0.5)

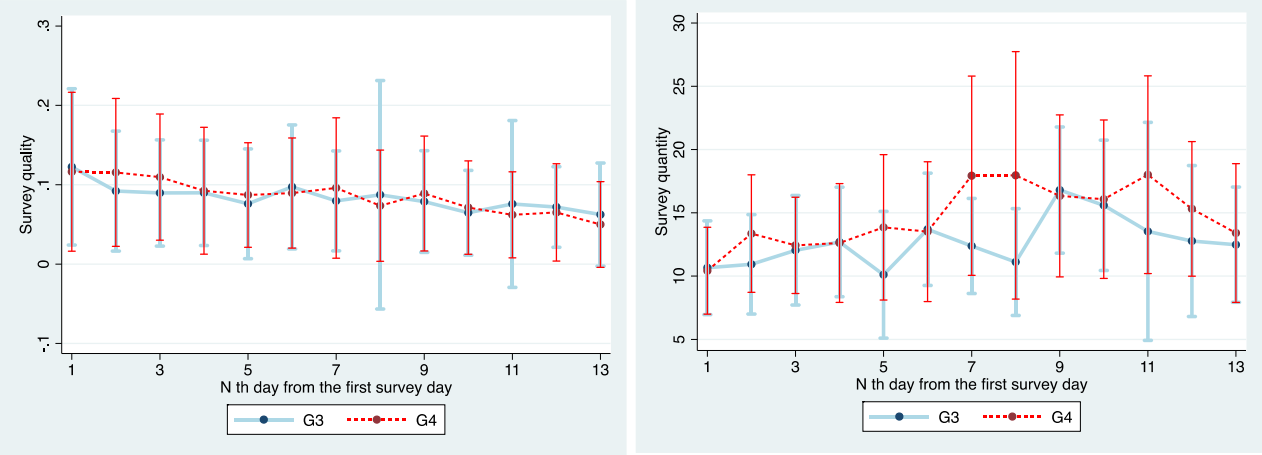
Note: The answers were not indicated in the actual training quiz questionnaire.

Figure A.5: Daily job performance trend

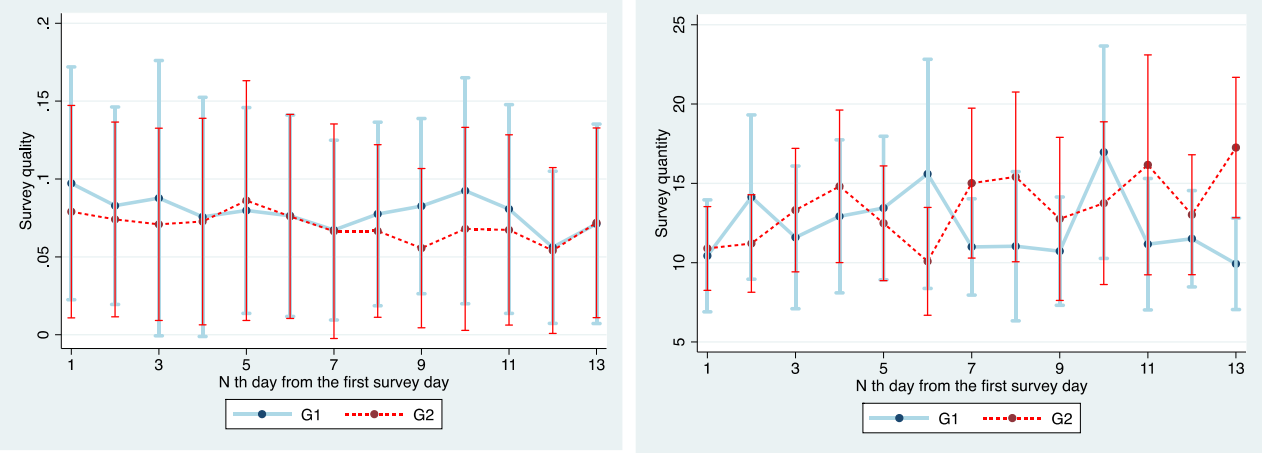
Panel A: Selection Effect (G2 vs. G3)



Panel B: Incentive Effect of Career Incentives (G3 vs. G4)



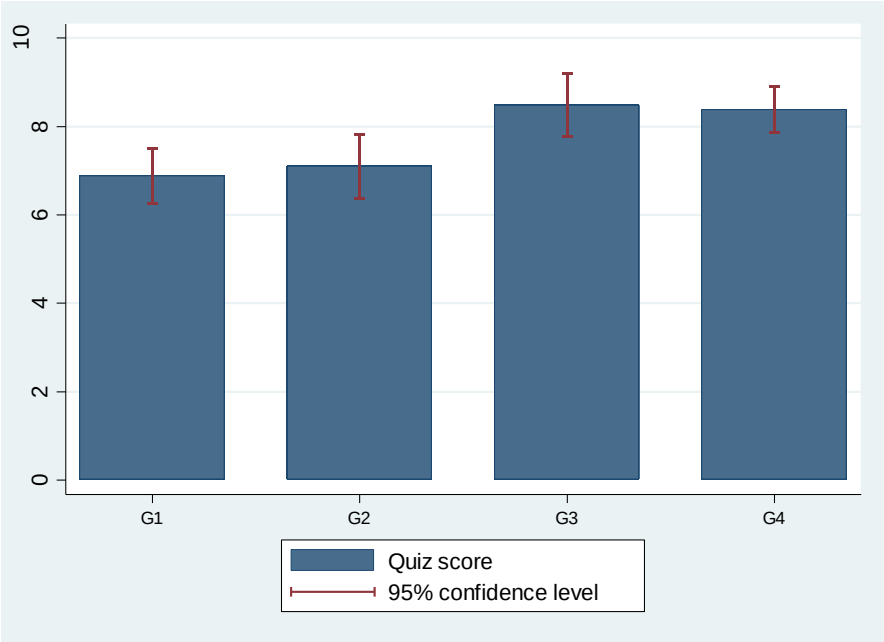
Panel C: Incentive Effect of Wage Incentives (G1 vs. G2)



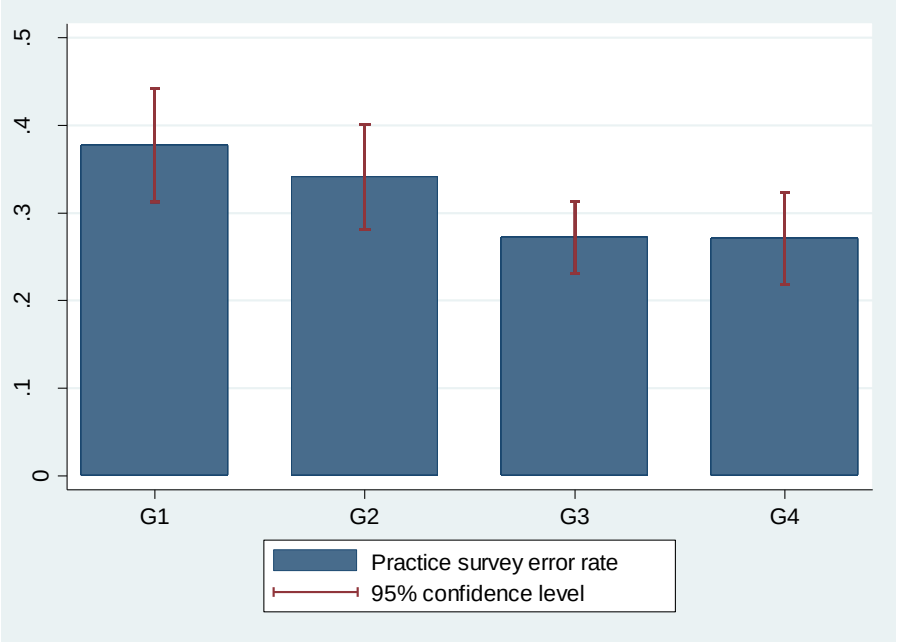
Notes: The vertical lines with caps indicate the 95% confidence intervals. Survey quality is measured by the survey error rate. Survey quantity is measured by the number of surveys conducted per day. The blue and red dots represent the conditional mean values of survey quality and survey quantity, respectively.

Figure A.6: Training performance by study group

Panel A: Quiz score



Panel B: Practice survey error rate



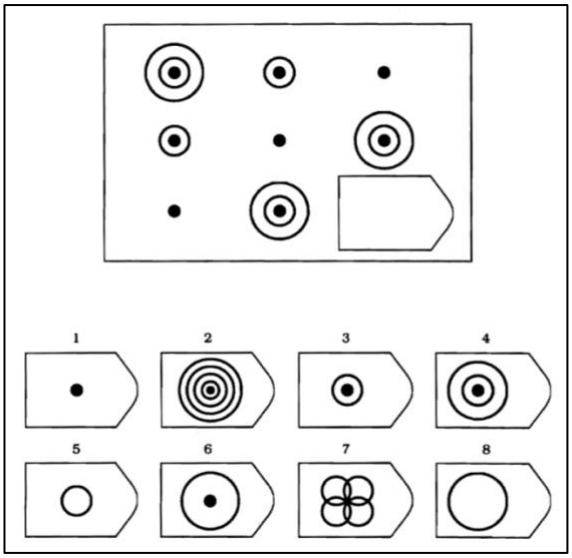
Notes: The maximum quiz score is 12. The vertical lines indicate 95% confidence intervals.

Data Appendix

Data A.1: Measurement of Raven's matrices, O*NET score, MSCE score, and non-cognitive traits

A.1.1. Raven's Progressive Matrices test

This is a widely used non-verbal test that evaluates "observation skills and clear-thinking ability" (Raven et al., 1998). Since it is independent of language skills, it is very easy to conduct in any setting including developing countries where the mother tongue is not English. The following figure is one example of the test questionnaire. In the test, a subject is required to choose one of eight options that match a missing pattern in the box. All questions follow similar visual patterns.



A.1.2. O*NET Ability Profiler (O*NET score)

The O*NET Ability Profiler was originally developed by the United States Department of Labor as "a career exploration tool to help understand job seekers on their work skills (O*NET Resource Center, 2010, p.1)". We use the verbal and clerical ability tests of the Ability Profiler, as these skills are the most relevant for the enumerator job.

a. The verbal ability test measures how well a test subject understands the definition of English words and properly uses them in conversation. The following is an example of the test questionnaire:

"Choose the two words that are either most closely the same or most closely opposite in meaning."

1. A. push
   B. dine
   C. nap
   D. eat

b. The clerical perception test measures an individual's "ability to see details in written materials quickly and correctly. It involves noticing if there are mistakes in the text and numbers, or if there are careless errors in working math problems (O*NET Resource Center, 2010, p. 2)." The following is an example of the test questionnaire:

On the line in the middle, write S if the two names are exactly the same and write D if they are different.

1.	Paramore & Co.	—	Paramore & Co.
2.	Bimler	—	Binler
3.	E-Z Neon	—	E-Z Neon
4.	Blackstone	—	Blackstone
5.	Chris Brasch	—	Chris Grasch

A.1.3. Math and English scores of Malawi School Leaving Certificate Exam in 2014 (MSCE score)

All secondary school students in Malawi are required to take the Malawi School Leaving Certificate Exam during the third semester in Form 4 of secondary school (Grade 12 in the U.S.) to achieve an official secondary school graduation status. The Malawi National Examination Board (MANEB) administers the whole process of the exam. Each student chooses 6–8 subjects out of approximately 20 subjects prepared by MANEB (MANEB, 2014). Math and English are mandatory subjects. The results of each subject are reported in terms of a scale from 1 to 9. We use English and math test scores because they are mandatory subjects and thus, there are no missing values in the exam transcripts. We obtained the administrative record of the MSCE exam transcripts for all study participants through the Malawi Ministry of Education.

A.1.4. Non-cognitive traits

Rosenberg self-esteem scale

This is a 10-item scale developed by Rosenberg (1965) and is widely used to measure self-esteem by measuring positive and negative feelings about the self. All items are answered using a 4-point Likert scale format ranging from *strongly agree* to *strongly disagree*.

Intrinsic motivation

Intrinsic motivation is an individual's trait that captures whether the individual is motivated to do things by intrinsic rewards such as his/her own desire to pursue goals or challenges. It is the opposite of extrinsic motivation described below. We measure intrinsic motivation using a 15-item scale (Amabile et al., 1994). All items are answered using a 4-point Likert scale format ranging from *strongly agree* to *strongly disagree*.

Extrinsic motivation

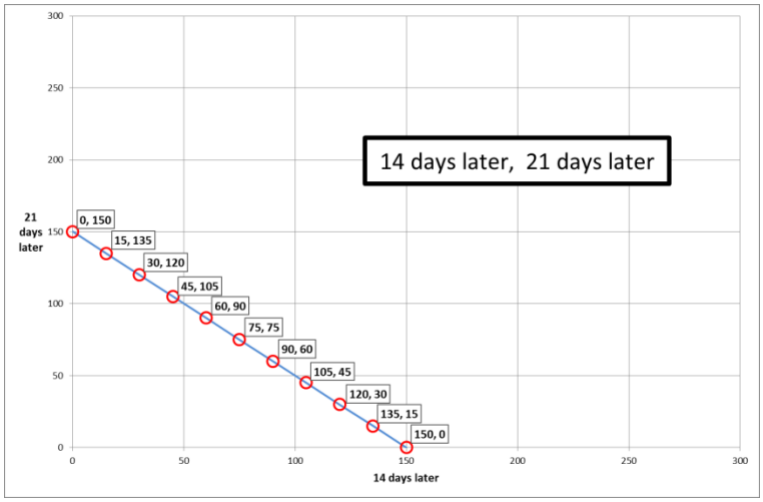
Extrinsic motivation is an individual's trait that captures whether the individual is motivated by external rewards, such as reputation, to do things. We use a 15-item scale to measure the level of motivation triggered by extrinsic values (Amabile et al., 1994). All items are answered using a 4-point Likert scale format ranging from *strongly agree* to *strongly disagree*.

Ten-item Big Five personality inventory (TIPI)

We measure an individual's personality types using a 10-item scale that assesses the respondent's characteristics based on traits commonly known as the Big 5 personality traits (openness to experience, conscientiousness, extroversion, agreeableness, and emotional stability) (Gosling et al., 2003). All items are answered using a 7-point Likert scale format (*Disagree strongly, Disagree moderately, Disagree a little, Neither agree nor disagree, Agree a little, Agree moderately, and Agree strongly*).

Time preference

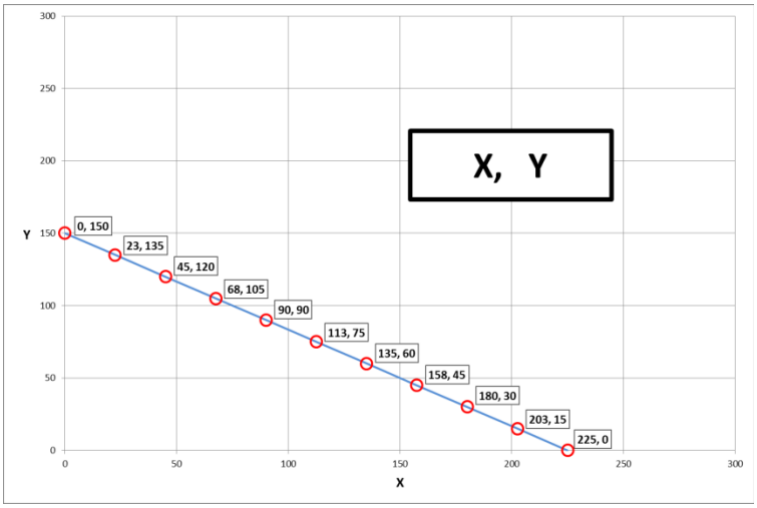
Participants were given 20 decision problems. In each, they were asked to choose 1 out of 11 options on the line. Each option [X, Y] is a payoff set indicating the amount of money (X) they would receive 14 days later and the amount of money (Y) they would receive 21 days later (see the figure below). Participants were informed that AFF would randomly choose 1 out of 20 problems and would provide the amount of payoff the participants selected in the chosen decision problem according to the payoff rule.



The choices that individuals made through this experiment were used to infer their time preference, measured between 0 and 1 following the methodology proposed by Choi et al. (2007). The closer the value is to 1, the more impatient a participant is, and the closer the value is to 0, the more patient the participant is.

Risk preference

Participants were given 20 decision problems. In each, they were asked to choose 1 option out of 11 options on the line. An option [X, Y] indicates the amount of money a participant would earn if the X-axis (the horizontal axis) was chosen and the amount of money a participant would earn if the Y-axis (the vertical axis) was chosen (see the figure below). Participants were informed that AFF would randomly choose one out of 20 problems, and again randomly choose either X or Y with equal probability, and that the chosen payoff would be provided to the participants.



The choices made by individuals through this experiment were used to infer their individual-level risk preference, measured between 0 and 1 following the methodology proposed by Choi

et al. (2007). The closer the value is to 1, the more risk-taking a participant is, and the closer the value is to 0.5, the more risk-averse the participant is. Values lower than 0.5 reflect a violation of stochastic dominance and are excluded from the analysis (Choi et al, 2007).

Rational decision-making ability

Using the Critical Cost Efficiency Index (CCEI; Afriat, 1972), we measured a level of consistency with the Generalized Axiom of Revealed Preference (GARP) based on the results from the time preference experiment. Considering all 20 decision problems in the time preference experiment, CCEI counts by how much the slope of the budget line in each problem should be adjusted to remove all violations of GARP. We took CCEI into account for the level of rational decision-making ability (Choi et al, 2014). CCEI is measured between 0 and 1. The closer CCEI is to 1, the more a participant satisfies GARP overall, and the more rational (from an economic prospective) are the decisions made.

Data A.2: Measurement of survey quality

AFF checked each questionnaire one by one and counted systematically inconsistent errors. First, census supervisors listed all possible systematic errors that could result from enumerators, not respondents. Second, data-entry clerks went through repeated training to catch those errors. Then, they started counting the number of systematic errors caused by enumerators for each sheet of the census survey.

Error collecting work was carried out in the following steps.

1. Two error-collecting data entry clerks checked one questionnaire separately.
2. They counted the total number of questions that must be answered.
3. Three types of errors from each page of the questionnaire were counted, as follows.
    1) The total number of questions that must be answered but are blank.
    2) The total number of questions that must be answered but are incorrectly answered.
    3) The total number of questions that must not be answered but are answered.
4. All the numbers on each page are added up and the total number of errors is recorded
5. The total number of errors independently counted by the two clerks is compared.
6. If the difference between the total errors counted by the two data entry clerks is larger than 5, a recount is undertaken.
7. The mean of the number of errors counted by the two data entry clerks is recorded.

The following table provides the basic statistics of each number counted.

Index	Measurement	Mean (SD)
A	The total number of all questions that must be answered	221.7 (61.8)
B	The total number of questions that must be answered but are blank	7.59 (10.3)
C	The total number of questions that must be answered but are incorrectly answered	3.90 (4.26)
D	The total number of questions that must not be answered but are answered	5.53 (9.28)

Note: *A* could be different across households due to differences in household-specific characteristics, such as family structure.

Finally, the final variable we use for survey quality (error rate) in the analysis is constructed as follows:

$$\text{error}_i = (\text{B}_i + \text{C}_i + \text{D}_i)/\text{A}_i$$

where $error_i$ is the error rate of a specific census questionnaire *i* surveyed by an enumerator. A_i, B_i, C_i, and D_i are the corresponding numbers counted from the *i*-th census survey questionnaire by data clerks.

Data A.3: Imputation of missing survey beginning and end times

We find that there are significant missing values in the entries for the survey beginning time and end time of census interviews due to the enumerators' mistakes. To preserve the sample size, we impute either the survey beginning time or the end time when only one of them is missing. Specifically, we run the regression of the questionnaire-specific length of survey.

$$Surveytime_{ijklt} = \alpha + \gamma \cdot H_i + \phi \cdot Z_k + V_{lt} + \sigma_t + \psi_{ijklt} \quad (A1)$$

$Surveytime_{ijktl}$ is survey time of household *i* by enumerator *j* whose supervisor is *l*, in catchment area *k,* surveyed on the *t*-th work day. H_i is a vector of respondents' household characteristics and Z_k is a vector of catchment area characteristics. σ_t is the survey-date fixed effect. V_{lt} is the supervisor team-specific post-visit effect.

For the surveyed census questionnaire sheets with either missing start time or end time, we impute the missing time using the predicted length of a survey from the above regression. Note that we cannot use this method for an observation when both starting and ending times are missing. In this case, we do not make any changes and thus the intermission time and survey length remain missing.

Data A.4: 2011 HIV/AIDS prevention programs of African Future Foundation

The HIV/AIDS prevention program of AFF covered 33 public schools in four districts in 2011: Traditional Authority (TA) Chimutu, TA Chitukula, TA Tsabango, and TA Kalumba. In Table A.6, the experimental design of the 2011 HIV/AIDS prevention program is summarized. The randomization process was implemented in two stages. Three types of interventions were randomly assigned to treatment groups independently. For the HIV/AIDS education and male circumcision programs, classrooms were randomly assigned to one of the three groups: 100% Treatment, 50% Treatment, and No Treatment classrooms. Treated students in the 50% Treatment classrooms were randomly selected at the individual level. The treatments were given to everybody in 100% Treatment classrooms. No one received the treatment in the No Treatment classrooms. For the girls' education support program, classrooms were randomly assigned either to the 100% Treatment or No Treatment group. AFF expected minimal spill-over between classes because there were limited cross-classroom activities and the majority (29 out of 33) of the schools had only one class per grade.

The HIV/AIDS education intervention was designed to provide the most comprehensive HIV/AIDS education. In addition to the existing HIV/AIDS education curriculum, AFF provided information on the medical benefits of male circumcision and the relative risk of cross-generational sexual relationships. The education was provided to both male and female students by trained staff members with a government certificate. The HIV/AIDS education was comprised of a 45-minute lecture and a 15-minute follow-up discussion. Study participants were assigned to one of four research groups: 100% Treatment (E1), Treated in 50% Treatment (E2), Untreated in 50% Treatment (E3), and No Treatment (E4).

The male circumcision offer consisted of free surgery at the assigned hospital, two complication check-ups (3-days and 1-week after surgery) at students' schools, and

transportation support. Free surgery and complication check-ups were available for all study participants, but transportation support was randomly given. Selected students could either choose a direct pick-up service or use a transportation voucher that is reimbursed after the circumcision surgery at the assigned hospital. The value of the transportation voucher varied according to the distance between the hospital and a student's school. Study participants were also assigned to one of four research groups: 100% Treatment (C1), Treated in 50% Treatment (C2), Untreated in 50% Treatment (C3), and No Treatment (C4). Transportation support was given to groups C1 and C2 during the study period, and the remaining temporarily untreated group (groups C3 and C4) received the same treatment one year later.

Table A.6: Experimental Design

1) HIV/AIDS Education				
	Group	Assignment	Classrooms	Students
100% Treatment	E1	Treatment	41	2480
50% Treatment	E2	Treatment	41	1303
	E3	No Treatment		1263
No Treatment	E4	No Treatment (Control)	42	2925
Total			124	7971
2) Male Circumcision				
100% Treatment	C1	Treatment	41	1293
50% Treatment	C2	Treatment	41	679
	C3	No Treatment		679
No Treatment	C4	No Treatment (Control)	42	1323
Total			124	3974
3) Girls' Education Support				
100% Treatment	S1	Treatment	62	2102
No Treatment	S2	No Treatment (Control)	62	1895
Total			124	3997

Notes: For the HIV/AIDS education and Male circumcision interventions, the randomization was done in two stages. First, classrooms for each grade across 33 schools were randomly assigned to one of three groups: 100% treatment, 50% treatment, and no treatment. Then, within the 50% treatment group, only half of the students were randomly assigned to receive the treatment.

The girls' education support program provided a one-year school tuition and monthly cash stipends to female students in randomly selected classrooms (S1). School tuition and fees per semester (on average US$7.5, 3,500 MWK) were directly deposited to each school's account and monthly cash stipends of 0.6 USD (300 MWK) were distributed directly to treated students. The total amount of scholarship was approximately US$24 per student during the study period.

Conceptual Framework Appendix

The following provide the proof of the propositions stated in Section 3.2.

A. Proof of Proposition 1

Proposition 1: Under the conditions specified in Section 3.1, there exists the following three equilibria depending on the parameter values of the model:
First, high-ability agents accept the career incentive offer and low-ability agents accept the wage incentive offer.
Second, high-ability agents accept the wage incentive offer and low-ability agents accept the career incentive offer.

For simplicity, we assume that the number of high-ability and low-ability agents are identical, denoted by 4H and 4L. Note that a high type agent has a lower effort cost ($c = c_H$) lower than that of a low type agent ($c = c_L > c_H$ and c_L is assumed to be 1). Since agents are randomly assigned to either *Internship* or *Wage* groups, there are 2H and 2L agents in each treatment group.

In the first separating equilibrium, the low-ability agents accept a job offer with only the wage incentives and the high-type agents accept a job offer with only the career incentives under the following 4 conditions:

(1) Individual Rationality for Low-type Agents (IR_L): 2L in the *Wage* group accept the wage incentive offer. That is, $U_w(e_L^*) > \overline{u_L} \Leftrightarrow w\ln(\mathrm{w}) - \mathrm{w} > 0$.
(2) Incentive Compatibility for High-type Agents (IC_H): 2H in the *Wage* group reject the wage incentive offer. That is, $U_w(e_H^*) < \overline{u_H} \Leftrightarrow w\ln(\mathrm{w}) - \mathrm{w} < \overline{u_H} + lnc_H$.
(3) Individual Rationality for High-type Agents (IR_H): 2H in the *Internship* group accept the career incentive offer. That is, $U_C(e_H^*) > \overline{u_H} \Leftrightarrow \frac{\kappa}{(2H)^2} > \overline{u_H}$. If only high types accept the career incentive offer, then this becomes a symmetric Tullock context. Thus, the unique equilibrium occurs at $e_H^* = \frac{2H-1}{(2H)^2}\frac{\kappa}{c_H}$.[53] Then, the expected utility of accepting the career incentive offer by high-type agents becomes $\frac{\kappa}{(2H)^2}$.

[53] See Corchón (2007).

(4) Incentive Compatibility for Low-type Agents (IC_L): 2L in the *Internship* group reject the career incentive offer. That is, $c_H < \frac{2H-1}{2H}$. Suppose low-type agents accept the career incentive offer when all high-type agents accept the same offer and choose the effort level e_H. In this case, the low type agent's expected utility is $\kappa \frac{e_L}{e_{-L}+e_L} - e_L$ where $e_{-L} = 2H \cdot e_H = \frac{2H-1}{2H}\frac{\kappa}{c_H}$ is the sum of all high type agents' efforts. Since the first-order condition is $\kappa \frac{e_{-L}}{(e_{-L}+e_L)^2} - 1 = 0$, the optimal effort level $e_L^* = \sqrt{\kappa e_{-L}} - e_{-L}$. Thus, a low-type agent will necessarily reject the offer so long as his expected utility is lower than the reservation utility, i.e., $\frac{\kappa e_L^*}{e_{-L}+e_L^*} - e_L < \overline{u_L}$. By substituting e_L^* and e_{-L} into the equation, $\frac{\kappa e_L^*}{e_{-L}+e_L^*} - e_L < \overline{u_L} \Leftrightarrow$.

$\frac{\kappa(\sqrt{\kappa e_{-L}}-e_{-L})}{e_{-L}+(\sqrt{\kappa e_{-L}}-e_{-L})} - (\sqrt{\kappa e_{-L}} - e_{-L}) = \kappa - \frac{\kappa e_{-L}}{\sqrt{\kappa e_{-L}}} - \sqrt{\kappa e_{-L}} + e_{-L} = \kappa - 2\sqrt{\kappa e_{-L}} + e_{-L} =$

$\left(\sqrt{\kappa} - \sqrt{e_{-L}}\right)^2 < \overline{u_L}\ (=0) \Leftrightarrow \kappa < e_{-L}$

Thus, using $e_{-L} = 2H \cdot e_H = \frac{2H-1}{2H}\frac{\kappa}{c_H}$, we get $\kappa < \frac{(2H-1)}{2H}\frac{\kappa}{c_H} \Leftrightarrow c_H < \frac{2H-1}{2H}$.

However, this condition implies $e_L^* < 0$. Therefore, 2L in the *Internship* group would reject the career incentive offer if $c_H < \frac{2H-1}{2H}$.

In sum, we rewrite these conditions in terms of the parameters in the model would be as follows:

(1') IR_L: $w\ln(\mathrm{w}) - \mathrm{w} > 0$

(2') IC_H : $w\ln(\mathrm{w}) - \mathrm{w} < \overline{u_H} + lnc_H$.

(3') IR_H: $\frac{\kappa}{(2H)^2} > \overline{u_H}$

(4') IC_L : $c_H < \frac{2H-1}{2H}$

Hence, as long as the above 4 conditions are satisfied, we have a separating equilibrium in which low types accept the wage incentive offer and high types accept the career incentive offer. For example, $w = e$, $H = 25$, $c_H = 0.97$, $\overline{u_H} = 0.05$, and $\kappa = 130$ satisfy the four requirements. Stated succinctly is the following proposition:

Proposition A.1. (first separating equilibrium) Under conditions (1') – (4'), there exists a separating equilibrium in which low types accept the wage incentive offer only and high types accept the career incentive offer only.

We now turn to the other separating equilibrium in which high types accept the wage incentive offer only and low types accept the career incentive offer only. This second separating equilibrium exists with characteristics opposite to the first separating equilibrium, which requires the following four conditions:

(5) Individual Rationality for Low-type Agents (IR_L): 2L in the *Internship* group accept the career incentive offer. That is, $U_C(e_L^*) > \overline{u_L} \Leftrightarrow \kappa > 0$

Low-type worker will accept career incentive if $U_C(e_L) = \kappa \frac{e_L}{e_{-L}+e_L} - e_L > \overline{u_L}$. Since the corresponding FOC is $\kappa \frac{e_{-L}}{(e_{-L}+e_L)^2} - 1 = 0$, applying a symmetric Tullock contest model (Corchón, 2007), we can derive an unique equilibrium effort level, $e_L = e_L^*$ for all low-type agents.

$$\kappa \frac{(2L-1)e_L^*}{(2Le_L^*)^2} - 1 = 0 \Rightarrow e_L^* = \frac{2L-1}{(2L)^2}\kappa$$

$$\therefore U_c(e_L^*) = \kappa \frac{e_L^*}{2Le_L^*} - e_L^* = \frac{\kappa\left(\frac{(2L-1)}{(2L)^2}\kappa\right)}{2L\frac{(2L-1)}{(2L)^2}\kappa} - \frac{(2L-1)}{(2L)^2}\kappa = \frac{\kappa}{2L} - \frac{(2L-1)\kappa}{(2L)^2} = \frac{\kappa}{2L^2}$$

Therefore, $U_c(e_L^*) = \frac{\kappa}{2L^2} > \overline{u_L} = 0,\ \kappa > 0.$

(6) Incentive Compatibility for High-type Agents (IC_H): 2H in the *Internship* group reject the career incentive offer. That is, $\sqrt{u_H} > \sqrt{\kappa} - \sqrt{\frac{c_H\kappa(2L-1)}{2L}}$

If a high type accepts the career incentive offer, its expected utility is $\kappa \frac{e_H}{e_{-H}+e_H} - c_H e_H$, where $e_{-H} = 2Le_L = \frac{2L-1}{2L}\kappa$. The first order condition is then, $\kappa \frac{e_{-H}}{(e_{-H}+e_H)^2} - c_H = 0 \leftrightarrow e_H^* = \sqrt{\kappa e_{-H}/c_H} - e_{-H}$.

Thus, a high-type agent will reject the offer when $\overline{u_H} > \frac{\kappa e_H}{e_{-H}+e_H} - c_H e_H = \frac{\kappa\left(\sqrt{\frac{\kappa e_{-H}}{c_H}} - e_{-H}\right)}{e_{-H}+\left(\sqrt{\frac{\kappa e_{-H}}{c_H}} - e_{-H}\right)} - c_H\left(\sqrt{\frac{\kappa e_{-H}}{c_H}} - e_{-H}\right)$

$$= \kappa - c_H \frac{\frac{\kappa e_{-H}}{c_H}}{\sqrt{\frac{\kappa e_{-H}}{c_H}}} - c_H\sqrt{\frac{\kappa e_{-H}}{c_H}} + c_H e_{-H} = \kappa - 2c_H\sqrt{\frac{\kappa e_{-H}}{c_H}} + c_H^2\frac{e_{-H}}{c_H} = \left(\sqrt{\kappa} - c_H\sqrt{\frac{e_{-H}}{c_H}}\right)^2$$

$$\therefore \sqrt{u_H} > \sqrt{\kappa} - \sqrt{\frac{c_H\kappa(2L-1)}{2L}}$$

(7) Individual Rationality for High-type Agents (IR_H): 2H in the *Wage* group accept the wage incentive offer. That is, $U_W(e_H^*) > \overline{u_H} \Leftrightarrow\ w\ln(\mathrm{w}) - \mathrm{w} > \bar{u}_{\mathrm{H}} + w\ln(c_H)$

(8) Incentive Compatibility for Low-type Agents (IC_L): 2L in the *Wage* group reject the wage incentive offer. That is, $U_W(e_L^*) < \overline{u_L} \Leftrightarrow w\ln(\mathrm{w}) - \mathrm{w} < 0$

In sum, we rewrite these conditions in terms of the parameters in the model would be as follows:

(5') IR_L: $\kappa > 0$

(6') IC_H: $\sqrt{u_H} > \sqrt{\kappa} - \sqrt{\frac{c_H\kappa(2L-1)}{2L}}$

(7') IR_H: $w\ln(\mathrm{w}) - \mathrm{w} > \bar{u}_{\mathrm{H}} + w\ln(c_H)$

(8') IC_L: $w\ln(\mathrm{w}) - \mathrm{w} < 0$

Hence, as long as the above 4 conditions are satisfied, we have a separating equilibrium in which low types accept the career incentive offer and high types accept the wage incentive offer. For example, $\bar{u}_{\mathrm{H}} = 1$, $w = 2$, $c_H = 0.1$, $L = 25$, and $\kappa = 1$ satisfy the four requirements. Stated succinctly is the following proposition:

Proposition A.2. Under conditions (5') – (8'), there exists a separating equilibrium in which low types accept the career incentive offer and high types accept the wage incentive offer.

The propositions A.1 and A.2 imply that there are (disjoint) ranges that elicit one separating equilibrium or another. If we assume that the first separating equilibrium, we can derive a prediction that the effort levels of G2 would be greater than those of G3. Suppose that those who accept the job offer are split evenly into two subgroups. Thus, there are four subgroups as follows:

1) Group 1 (G1): those who accepted the career incentive offer and are only compensated thusly
2) Group 2 (G2): those who accepted the career incentive offer and are additionally paid the wage incentives
3) Group 3 (G3): those who accepted the wage incentive offer and are additionally received the career incentives
4) Group 4 (G4): those who accepted the wage incentive offer and are only paid wages.

G1 and G2 have high types, and G3 and G4 have low types in the first separating equilibrium. G4's effort decision will be the same as before, but Groups 1, 2, and 3 will alter their

decisions. Let $U_G \equiv Utility\ of\ group\ G$, and $Y \equiv \{i|\ i\ is\ individual\ in\ G1, G2, G3\}$. Then, the utility function of each group will be as follow:

$$U_2(e_i) = U_3(e_i) = wln(e_i) + \kappa \frac{e_i}{\sum_{k\in Y} e_k} - ce_i$$

$$U_1(e_i) = \kappa \frac{e_i}{\sum_{k\in Y} e_k} - ce_i$$

$$U_4(e_i) = wln(e_i) - ce_i$$

Consider a symmetric equilibrium, let $e_i^G \equiv e_G\ for\ all,\ E \equiv \sum_{k\in Y} e_k$. Then, the FOCs become;

G1: $\kappa \frac{E-e_1}{E^2} = c_H$

G2: $\frac{w}{e_2} + \kappa \frac{E-e_2}{E^2} = c_H$

G3: $\frac{w}{e_3} + \kappa \frac{E-e_3}{E^2} = c_L$

G4: $\frac{w}{e_4} = c_L$

By the FOC of G2 and G3, $\frac{w}{e_2} + \kappa \frac{E-e_2}{E^2} = c_H < c_L = \frac{w}{e_3} + \kappa \frac{E-e_3}{E^2}$

Since $\frac{\partial}{\partial e}\left(\frac{w}{e} + \kappa \frac{(E-e)}{E^2}\right) = -\frac{w}{e^2} - \frac{\kappa}{E^2} < 0 \quad and \quad c_H < c_L$,

$\therefore\ e_2 > e_3$

That is, agents who were recruited through a career incentive offer (Group 2) outperform those who were recruited through a wage incentive offer (Group 3).

B. Proof of Proposition 2

Proposition 2 (wage incentive effect). Consider the two groups who were originally offered a career incentive only (G1 and G2). Then those who were additionally offered wage incentives (G2) outperform those who were not offered additional wage incentive (G1). That is, $e_1 < e_2$ *by incentive effect of wage incentives.*

(Proof) By the FOCs of G1 and G2,

$\kappa \frac{E-e_1}{E^2} = c_H$, and

$\left(\frac{w}{e_1}\right) + \kappa \frac{E-e_1}{E^2} = \left(\frac{w}{e_1}\right) + c_H > c_H = \frac{w}{e_2} + \kappa \frac{E-e_2}{E^2}$.

Since $\frac{\partial}{\partial e}\left(\frac{w}{e} + \kappa \frac{(E-e)}{E^2}\right) < 0 \quad and \quad c_H + \frac{w}{e_1} > c_H$,

$\therefore\ e_1 < e_2$

C. Proof of Proposition 3

Proposition 3 (career incentive effect). Consider the two groups who were originally offered a wage incentive only (G3 and G4). Then those who were additionally offered career incentives (G3) outperform those who were not offered additional career incentive (G4). That is, $e_4 < e_3$ *by incentive effect of career incentives.*

(Proof) By the FOCs of G3 and G4,

$$\frac{w}{e_4} = c_L$$

$$\frac{w}{e_4} + (\kappa \frac{E - e_4}{E^2}) = c_L + (\kappa \frac{E - e_4}{E^2}) > c_L = \frac{w}{e_3} + \kappa \frac{E - e_3}{E^2}$$

Since $\frac{\partial}{\partial e}\left(\frac{w}{e} + \kappa \frac{(E-e)}{E^2}\right) < 0 \quad and \quad c_L + (\kappa \frac{E-e_4}{E^2}) > c_L$,

$\therefore\ e_4 < e_3$

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