Mongolia Technical, Vocational, and Education Project
Impact Evaluation Strategy

Innovations for Poverty Action

Original: May 2010
First Revision: June 2012
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List of Acronyms

ERR	Economic Rate of Return
GoM	Government of Mongolia
GPA	Grade Point Average
IPA	Innovations for Poverty Action
IRB	Institutional Review Board
JPAL	Abdul Latif Jameel Poverty Action Lab
MCA-M	Millennium Challenge Account-Mongolia
MCC	Millennium Challenge Corporation
MDE	Minimum Detectable Effect
M&E	Monitoring and Evaluation unit
MNT	Mongolian Tugrik
PI	Principal Investigator
TVET	Technical Vocational and Education Training
USD	United States Dollar
VET	Vocational Education Training

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I. INTRODUCTION

A. Context for TVET Education in Mongolia and Prior Donor Activity

During the socialist era, Mongolia had a robust Technical Vocational and Education Training (TVET) program fashioned after the Soviet model and supported by the Soviet Union. The TVET sector in Mongolia had the capacity to produce sufficient skilled labor not only for industries in Mongolia but also for the Soviet Union[1]. However, as Mongolia transitioned to a market economy in the 1990's, without the financial and technical support from the Soviet Union, the TVET sector deteriorated with training equipment becoming outdated and teachers no longer qualified to teach the trades. Simultaneously, during the country's transition to a market economy, the demand for skilled labor in Mongolia grew as Mongolia experienced substantial growth in its gross domestic product from the growth of its mining sector. As Mongolia adapted to its economic growth, there was an increasing need for skilled workers in the sectors new to Mongolia: processing industry, construction, mining, and infrastructure.

Government of Mongolia (GoM), recognizing the lack of skilled workers and inadequate capacity of TVET institutions to produce such workers, set goals to increase TVET school enrollment and to improve the quality of the TVET education. Those goals were part of GoM's Second Education Master Plan 2006- 2015. With GoM's focus on TVET education and with the need for skilled labor becoming more apparent with the growth of the mining sector, a number of international donors began addressing the need to remake Mongolia's TVET sector relevant in a rapidly changing labor market. Asian Development Bank was the first donor to work in the TVET sector, advising the GoM on its education reform since 1991 and assisting in the formulation of the Second Master Plan. German International Cooperation and Nordic Development Fund were also active early on, investing in select vocational schools and assisting in the development of vocational education activities in the Second Education Master Plan.

In 2008, Millennium Challenge Corporation (MCC) became a major donor in the TVET sector. From 2008 to 2013, MCC, acting through the Millennium Challenge Account-Mongolia (MCA-M)'s Vocational Education Training (VET) Project, implemented wide-ranging measures to bring Mongolia's TVET sector up to international standards and produce competent market-ready TVET graduates. The VET Project initiated institutional reforms, created competency-based curricula for key priority trades, retrained teachers, introduced labor market information systems and career counseling, and upgraded training equipment and physical infrastructures at select TVET institutions. MCA-M's 52 million USD VET Project has been the largest donor investment in Mongolia's TVET sector so far.

Subsequent to MCC's entry into the TVET sector, the Swiss Agency for Development Cooperation, the German International Cooperation, and the European Union have been active in areas of curriculum development and teacher training with various vocational schools in

[1] Personal communication from Stephen Duguun, MCC Consultant on VET Project

Mongolia. Additionally, Oyu Tolgoi, a joint venture between the multinational mining corporation Rio Tinto and the GoM, which operates the largest mine in the country, has been a major contributor to the TVET sector, making tangible investment in mining schools, training teachers, and operating its own apprentice programs to adequately train workers for its mines.

B. Objectives of this Report

This document outlines the details of the proposed plan to evaluate the impact of one of the sub activities of MCA-M's VET Project. Section II provides an overview of the VET Project and the project logic and the expected outcomes. Section III reviews the existing literature on the effects of vocational education interventions. Section IV discusses the research design in depth by outlining the key research questions and evaluation methodology; Section V summarizes the survey instruments and the data collection schedule. Section VI includes discussions of risks to the current research methodology and the generalizability of the findings. Section VII covers administrative details related to the evaluation.

II. OVERVIEW OF THE COMPACT AND THE INTERVENTIONS EVALUATED

A. Overview of the Project and Implementation Plan

The MCA-M's VET Project aimed to reduce poverty by implementing improvements in the TVET sector in Mongolia that were meant to increase the employability and productivity of vocational education graduates. To this end, the MCA-M's VET Project implemented activities in the following five project component areas:[2]

1. *Reforms to TVET policy and operational framework activities*: Aimed to strengthen the policy and operational framework, to create an efficient governance and standard setting mechanism, to promote private-public partnership through National Competitive Grants Program, and to establish Center of Excellences which can serve as model institutions in Health, Mining and Construction sectors.
2. *Creation of skills standards and competencies system activities:* Focused on the establishment 28 competency-based curricula and national vocational qualification framework. The Project also established the National Learning Resource Center which can serve as a hub of teacher training, curricula and instructional packages.
3. *Competency-based training system activities:* Implemented the new competency-based training system in TVET schools through training of 1,500 teachers and technicians and administrators. Under this project component, the Project also trained 200 instructors in technical English so that instructors could understand instructional materials and be able to use the equipment properly.

[2] MCC Mongolia Compact Amendment page #14, "Annex I – Summary of Program – C. Vocational Education Project, 2. Activities"

4. *Career guidance system activities:* Provided career guidance and employment information services through establishing an online portal where TVET school graduates and applicants can access information on the available jobs and labor markets
5. *Improvement of learning environment in selected TVET schools activities:* Updated equipment and renovated facilities at 24 select schools and provided instructional technology and media

Activities under these project areas were also meant to encourage private-public partnerships, expose Mongolian TVET administrators to international standards and practices, encourage knowledge sharing between schools and among teachers, and train teachers and students on modern curricula and equipment relevant to priority sectors. The VET Project invested 50.2 million United States Dollars (USD) in the Mongolian TVET sector with more than half, nearly 30 million,[3] being spent on upgrading equipment and constructing and rehabilitating workshops.

1. Project Participants

MCA-M's VET Project was national in scope and the activities of much of the project components aimed to reach all vocational school students, teachers, and administrators. Reforms to TVET policy, for example, would be relevant and impactful for any student studying in a TVET institution. However, the curriculum update and equipment upgrade components of the project focused on implementing their activities in priority sectors, with the result that the various trades taught at vocational schools did not experience the same intensity of intervention. Although 28 competency-based curricula were developed for use by teachers in any TVET institution in Mongolia, these 28 curricula pertained to trades in seven priority sectors: construction, agriculture, mining, food and light industry, energy, information technology, and transportation. Similarly, upgraded equipment and renovated facilities focused on nine priority trades: heavy machinery operator, welding, plumbing, electricity and electronics, lathe-milling, heating and air conditioning technician, mining, health, and construction.

2. Geographic Coverage

While institutional reform, curriculum development, and teacher training activities were implemented nationwide, the VET Project's tangible investment was also wide-spread, including 20 out of 21 provinces in Mongolia. Equipment upgrades and renovation to facilities happened in 15 provinces, schools in 15 provinces participated in the National Competitive Grants Program, Regional Methodological Centers were established in four provinces, and three Centers of Excellence were established in two provinces. The VET Project made additional investments in Regional Methodological Centers and Centers of Excellence to make them model schools for other TVET institutions to emulate. Regional Methodological Centers were established for

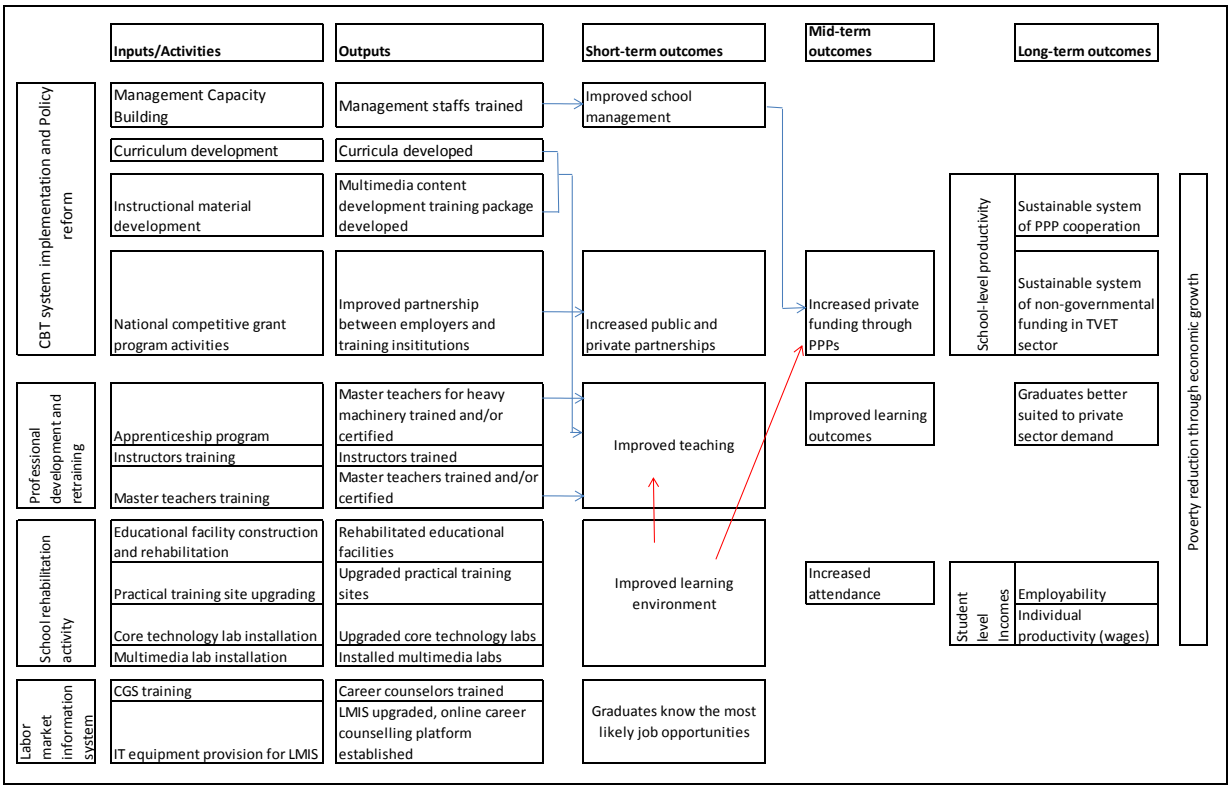
[3] Total VET Project investment amount and amount invested in tangible improvements to learning environment taken from *Introduction to Vocational Education and Training Project Activities*[Pamphlet] (2013), Ulaanbaatar, Mongolia: VET Project Implementation Unit

northern, southern, eastern and western regions of Mongolia; each Center was meant to serve as a hub for all TVET institutions in provinces in the region.

B. Key Project Indicators and How They Lead to Expected Outcomes

VET Project's project logic, shown in Figure 1, focused on improving student-level outcomes of employability and wages as a means of reducing poverty in Mongolia. Some of the project components focused directly on increasing student productivity while other components were meant to increase school-level productivity as measured through sustained private-public partnerships, and sustained non-governmental funding for the TVET sector. Increase in school-level productivity was meant to increase employability and wages of TVET school graduates as schools produce graduates more suited to the demands of the workplace. Activities in the first project component area, *Reforms to TVET policy and operational framework activities*, were meant to increase TVET school's management capacity, which in turn should improve school management. This improvement, coupled with an increase in partnership between training institutions and employers as promoted by the National Competitive Grant Program, was intended to increase private funding and eventually lead to sustained private-public partnerships. Development of competency-based curricula competency standards and training of teachers and administrators were meant to improve the quality of instruction in TVET institutions. Improved instruction and improved learning environment, which are outcomes of equipment upgrades and the rehabilitation of facilities, were intended to enhance learning outcomes for graduates and make them more suited to the demands of employers. The improved match between graduates' skills and employers' needs should increase the graduates' employability and wages. Labor Market Information System and career counseling should also reduce the possible mismatch between the trades and skills that the students are learning, and employment opportunities that the graduates are seeking. The expected key student-level outcomes for the VET project are increases in employment and productivity as measured by their wages.

Figure 1. Project Logic Diagram



C. Link to ERR and Beneficiary Analysis

Before choosing to implement the VET Project, MCC conducted an analysis of the likely Economic Rate of Return (ERR).[4] That analysis projects a total rate of return from all activities of 19.8 percent. This is based on the assumption that the projects will result in a six percent increase in the number of graduates from TVET institutions between 2007 and 2012, an 8.3 percent increase in wages and five percent increase in probability of employment. Because our analysis focuses only on the equipment upgrades, we will not be able to assess the accuracy of these assumptions for the full package of projects. However, we will be able to provide relevant statistics for the equipment upgrades alone – estimates of the difference in the probability of employment, the increase in wages, as well as the probability of working in the professions in which a student trains.

[4] Mongolia's Vocational Education Project ERR can be accessed here: http://www.mcc.gov/pages/countries/err/mongolia-compact

III. LITERATURE REVIEW

A. Summary of the Existing Evidence

While vocational training programs have been extensively reviewed and evaluated in developed countries, a review by Kluve (2010) of over one hundred active labor market programs found that traditional vocational training programs tend to have only a moderate positive impact on participants' labor market outcomes. Despite this finding, researchers suspected that vocational training could have a much greater impact on participants in low or middle income countries due to lower program costs and lower levels of education across populations.

In the past three years, researchers have performed five randomized control trials pertaining to vocational training in low or middle income countries. Hirschleifer et. al. (2014), Card et. al. (2011), and Cho et. al. (2013) found little to no effect of vocational training on employment or earnings in their evaluations of programs in Turkey, the Dominican Republic, and Malawi, respectively. In contrast, Maitra and Mani (2013) found that a vocational training program in India targeted towards women led to significant increases in income. Attanasio et. al. (2011) also found large and cost effective benefits to vocational training among both men and women in Colombia.

B. Gaps in Literature

The overall effect of vocational training on labor market outcomes in low and middle income countries remains uncertain. Due to the small number of studies conducted and the varied results, it is important to continue to evaluate the effects of vocational training in these contexts. Additionally, few studies have examined the importance of access to proper equipment and infrastructure for vocational training programs. Finally, this study will contribute to the existing literature by being the first of its kind conducted in East Asia.

IV. EVALUATION DESIGN

A. Overview of the Design

While all of the components of the VET Project described in Section II.A were considered, only the equipment upgrades component was eventually identified as a candidate for rigorous evaluation. As mentioned in Section II.A.1, some of the project components either directly affected all of the TVET schools or were available for use by all schools. Others, such as providing computers for administrative purposes, were deemed unlikely to have a direct effect on students' academic or job market performance. The trade-based equipment upgrades were only provided to specific schools and implementation was rolled out over a three-year period. Variation in timing and selection of school that received equipment upgrades as well as the supposed direct benefit that a student would receive from training on modern equipment made the equipment upgrade component a good candidate for evaluation.

B. Evaluation Question and Key Outcomes

The main question that the evaluation is designed to answer is: What is the effect of learning and training with upgraded equipment on graduates' employability and wages?

Consistent with the project logic model illustrated in Figure 1, we hypothesize that the equipment upgrades will improve students' employment prospects and increase their earnings. These changes will then ultimately improve the overall economic wellbeing of the household. The theory of change is straight forward. Receiving the equipment upgrades will cause students in treated trades to be more likely to train with improved equipment. This experience with modern, relevant equipment will improve their factual understanding of the trade and familiarize the students with the tools used by employers. Employers should then find students more productive than they otherwise would, making it more likely that students will be able to find employment and increasing the wages that employers are willing to pay them.

In addition to the primary intended outcomes of the projects, the intervention might also have ancillary effects on graduates' household assets and consumption levels. The higher income will allow graduates to purchase various household assets, increase consumption, and provide general support to the household. However, we do not anticipate strong short-term effects on household assets and consumption. Over time, with demonstrated wage increases and employability of graduates, it may be possible to detect effects on household assets and consumption in longer-term follow-up surveys. For now, we focus on wage increase and employability as main outcomes of interest.

C. Policy Relevance of the Evaluation

More than half of MCA-M's investment in the TVET sector paid for equipment upgrades and improvement of training facilities. This costly investment was made under the assumption that hands-on training on modern equipment in classroom settings was a critical aspect of turning out graduates who would succeed in the labor market. At the time of writing, Mongolian policy makers and donors are actively trying to fill in the various gaps in TVET education, by funding development of up-to-date curricula, training the teachers, and investing in training equipment. Understanding the real effect of learning on modernized equipment will help Mongolian policy makers and international donors to assess the relative value of investing in equipment upgrades, which tend to be the most costly investment in the TVET sector, in relation to other potential areas of investment such as teacher training, curriculum development, and private-public partnerships.

Globally, this research complements the existing literature on vocational education programs in developing countries. For example, while existing studies on vocational education focus on understanding the effect of the entire vocational education program, there is no evidence regarding the effectiveness of specific components of vocational education such as training on relevant and modern equipment.

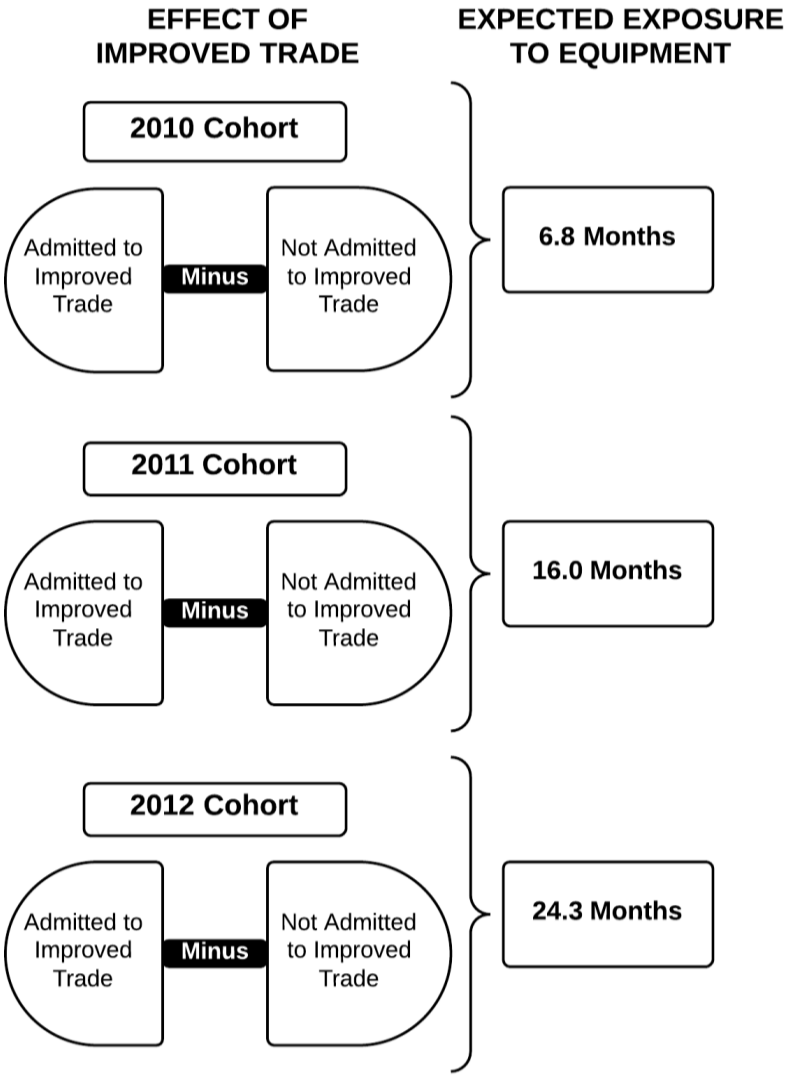
D. Methodology

The simplest strategy for evaluating the effects of the equipment upgrades would be to compare the outcomes of students who study in one of the trades that receives the upgrades to students who study in other trades. However, this strategy faces two key limitations. First, students may self-select into specific trades. This raises the possibility that students in trades that received the upgraded equipment may be different than those in trades that do not receive the upgrades. Second, in comparing across trades, trades differ in more than whether or not they receive the equipment upgrades. The job markets for students who study nursing, for example, may be very different than the job market for students who study automotive repair, even absent the fact that one might have received upgraded equipment.

The proposed research design seeks to identify the effects of the equipment upgrades by exploiting both random assignment of students to trades and the fact that the upgrades were rolled out over time to a subset of schools. We recruited students in three annual cohorts starting in 2010. First, we identified all trades that received the upgraded equipment. We refer to them collective as "improved trades". We then use the random assignment of students to improved trades to estimate the effect of being offered admission to an improved trade for each cohort. This is done by comparing the average outcomes of students assigned to receive an offer of admission to an improved trade to those students who did not receive an offer. We then compare this effect across cohorts, exploiting the fact that the students in later cohorts were much more exposed to the equipment upgrades than students in the 2010 cohort. Since we expect students to benefit from the upgraded equipment, we expect the effect of attending an improved trade to increase each year as each cohort is more exposed to the equipment. The research design thus ultimately involves estimating two levels of differences: (1) comparing students assigned to study an improved trade to those not offered a position in an improved trade and (2) comparing the difference in student performance by assignment to improved trades across cohorts.

This strategy is described graphically in Figure 2. For each cohort, we use the random assignment of applicants to trades to estimate the difference in wages and employment rates by subtracting the averages of those not admitted to the project from those who are admitted. This is depicted for each of the three cohorts by the semi circles representing the averages of each group that are differenced. The change in the probability of the equipment exposure is depicted in the second column with the increased anticipated exposure increasing by cohort. The 2010 cohort would only be exposed to the upgrades for 6.8 months while the 2012 cohort would be exposed for more than 24 months.

Figure 2. Evaluation Strategy



One of the key components of the design is the random assignment of students to trades that do and do not receive the equipment upgrades. To do this, we partnered with 10 TVET schools[5] who allowed us to replace their admissions systems with an admissions lottery run by MCA-M and IPA. For each school, we allocated offers of admission to seats in individual trades amongst eligible students via a public lottery process. Specifically, students provided a list of their preferences amongst the available trades. Students were then randomly selected and placed into their most preferred trade for which seats were still available. If none of the trades with open spaces were among the trades that a student indicated that they would be willing to study, then

[5] These schools were chosen because they were oversubscribed, receiving more applicants than they could accommodate each year.

the student was not admitted to any trade. Similarly, once all of the positions at a school were filled, the remaining students were not admitted to any trade.

Table 1. Data Collection Schedule

Cohort	Program Duration	Year of Data Collection 2010-11	2011-12	2012-13	2013-14	2014-15
2010	1 year	Admissions Admin Tracking	Grad FU	Grad FU[6]	Tracking	Tracking
	2-2.5 year	Admissions Admin Tracking	Tracking	Grad FU	Tracking	Tracking
2011	1 year		Admissions Admin Tracking	Grad FU	Tracking	Tracking
	2-2.5 year		Admissions Admin Tracking	Tracking	Grad FU	Tracking
2012	1 year			Admissions Admin Tracking	Grad FU	Tracking
	2-2.5 year			Admissions Admin Tracking	Tracking	Grad FU

To support this analysis, data is being collected according to the schedule described in Table 1. Students were recruited over three annual cohorts starting in 2010 and applied for 1 year or 2-2.5 year programs. For each cohort, students completed an Admissions Survey in which they provided demographic information and took a simple aptitude test that IPA developed in coordination with the TVET schools. Students were then either offered admissions to a particular trade in a school to which they applied, or they were rejected.[7] Afterwards, students completed a phone-based Tracking Survey each year to update contact information and to collect basic information on their educational and job-related activities. In the year after they graduate, each student completes an in-person Graduate Follow-Up Survey which includes all of the information in the Tracking Survey as well as a written trade-based skills test and questions about asset ownership, consumption, and other household activities. Students are then followed annually using the Tracking Survey. We also have a contingency plan to collect additional in-person data: once we observe sufficiently large effects on employment or wages, we will then conduct a second in-person Follow-Up Survey.

[6] The Graduate Follow-Up Survey was administered to the 2010 1 year cohort twice. This was done because the Graduate Follow-Up survey instrument was slightly revised for this survey round. So, we decided to re-administer it to the 2010 1 year cohort so that students will be assessed with comparable survey instruments.

[7] Lotteries were held at each participating school. The process started with excluding applicants who did not meet the minimum criteria for the TVET school, and then screening the applicants for their trade-specific eligibility. Admission to specific trades was then assigned from a pool of eligible applicants who applied for each trade; an applicant was admitted to the applicant's highest ranked trades in which admissions slots were still available.

To measure the effects laid out in Section IV.B, three types of survey instruments will be administered to students. The baseline information needed to assess differential impacts within the sample was collected in the Admissions Survey. The Tracking Surveys allow us to follow the students over time, to assess students' compliance with the lottery outcome, and to assess their employment experiences over the last year. The Graduate Follow-Up Survey, which is administered after the end of the programs to which the students applied, provides a test of students' knowledge about the trades in which they studied (and applied to) and provides information on students' educational history, employment history, asset ownership, consumption, and the activities of others in the household. Given these data, we will estimate the effect of the upgrades by estimating the effect of the number of months to which a student is exposed to the equipment on the students' probability of being employed, wages, and the other outcomes described above.[8] In addition to the three surveys administered to the students, an Administrative Survey will be administered to teachers and administrators at TVET institutions to capture school characteristics such as the school size, school funding, and availability and utilization of equipment. The Administrative Survey data will be used to control for school-level variation in the student outcomes. Each survey that will be used is described in more detail in Section V.

At this point, we have completed all of the baseline and administrative data collection, and we *are in the middle of* collecting the follow-up data. The Graduate Follow-Up Survey was administered to all students in the 2010 cohort and the one-year program students in the 2011 cohort during spring and summer of 2013. Data collection was completed in August 2014 for Graduate Follow-Up Survey for the 2 and 2.5 year program students in the 2011 cohort and the 1 year program students in the 2012 cohort. Students in the 2 and 2.5 year program in the 2012 cohort will complete the Graduate Follow-Up Survey in spring and summer of 2015. During

[8] Specifically, we plan to estimate the effects of the upgrades using the following model to be estimated via ordinary least squares:

$$y_{ijklm} = \tau Exposed_i * Admitted + \partial Exposed_i + \phi Admitted_i + \beta' X_{ijklm} + f(p_{ijklm}) + \varepsilon_i$$

The variable y_{ijk} is the outcome of interest for student i in school k participating in round l of the lottery for cohort m. The number of months students are exposed to the equipment upgrades given their enrollment decisions is $Exposed_i$, and $Admitted_i$ is an indicator variable for whether or not a student was admitted to an improved trade by the lottery. The coefficient of interest is the coefficient on the interaction of the two — the difference in treatment effects for admitted students based on the number of months they were exposed to the equipment upgrades. Finally, X_{jklm} is a vector of baseline controls including demographic information and the baseline test scores from the admissions survey as well as fixed effects for the students' school and program type, while $f(p_{jklm})$ is a polynomial of the probability that each student was assigned to an improved trade given their and their peers' trade preferences. For the analysis in this memo, we use a tenth order polynomial. The probabilities were estimated empirically based on 10,000 iterations of the actual lottery algorithms used to assign students in the admissions process. The function also includes indicator variables for students who were guaranteed admissions (either because their most preferred trade was not oversubscribed or because they met the school's criteria for preferential admission) or who had no chance of being admitted (because they did not meet the minimum requirements for any of the trades to which they applied).

each data collection, all students who do not complete the Graduate Follow-Up Surveys are part of the Tracking Survey.

E. Study Sample

The study sample includes 12,806 prospective students to the 10 schools participating in the study. Of these, 12,250 applicants were qualified for at least one of the trades to which they applied. These criteria included conditions such as a minimum grade point average (GPA) or subject test scores. The minimum qualification requirements varied from school to school and were determined by the school itself. Of the 12,250 applicants, another 526 met the conditions for guaranteed admissions, also designated by the schools usually due to superior academic performance or special talents.

As a result, only the 11,724 qualified students who were not guaranteed admissions participated in the randomized admissions process described in Section IV.D.[9] Some applicants were randomly assigned to improved trades. This group had potential to use upgraded equipment for varying lengths of time, depending on the admission year and the date that the trade received upgraded equipment. In comparison, the complement to this group includes applicants who were admitted to a non-improved trades and those who were not admitted to any trade at any school. Randomized admission to trades depended on applicants' ranked preference for various trades available at the school.

The 10 schools that were part of the study were not randomly chosen and are not a representative sample of TVET schools in Mongolia. In order to have randomized admission, schools that were recruited as part of the study had to have an applicant pool that was larger than the number of available admission slots. Thus, these schools were likely more competitive than the typical TVET institution in Mongolia. There were a total of 12 oversubscribed schools that were identified as potential partners for this study. All 12 schools were approached and ultimately 10 of the oversubscribed schools decided to participate in the study.

Because the schools are not representative of either the 72 TVET institutions in Mongolia or the 52 TVET institutions that the VET Project counted among its beneficiary schools, an Administrative Survey was collected from all 52 VET Project beneficiary schools, so that the 10 evaluation schools can be compared to the larger set of upgraded TVET institutions in Mongolia, and the generalizability of the findings from these 10 schools can be explored.

Finally, one of the main challenges with our sample is that they are highly mobile. To minimize the number of respondents that cannot be located for follow-up surveys, we have constructed a detailed tracking questionnaire that respondents complete every year. This questionnaire collects

[9] For the analysis described in the next section, only the 11,724 qualified students who did not have guaranteed admissions will be used to estimate the treatment effects. The other students will be included in the statistical analysis. However, they will be included such that their outcomes are only used to estimate correlations between outcomes and baseline characteristics, rather than estimate treatment effects.

phone numbers and addresses of the respondent, the respondent's guardians, close family members and friends. Initial attempt to reach each respondent is made by calling the phone number that the respondent has provided. The respondent is called at least three times at various times during the day. If the respondent cannot be reached, phone numbers of his contacts are called in attempts to reach the respondent. If enumerators cannot contact any of the numbers listed as contacts for the respondent, an enumerator visits the respondent's given address in order to administer the survey. Enumerators also consult administrative addresses kept by local district or city offices in order to find new administrative addresses in the event that the respondent has moved. The target response rate for the data collection companies is currently 90 percent. In addition, we will work with the data collection companies both to monitor the response rate as the data is collected and to compare the rates between students assigned to improved trades and those who are not in order to limit the odds of differential attrition between our two key sample subsets.

F. Analysis Plan

The analysis of the data will proceed in three main steps. First, we will assess the internal validity of the research design. The purpose of this analysis will be to assess the likelihood that other differences between applicants might confound our ability to determine the effects of the equipment upgrades. To do so, we will first assess whether individuals who were assigned to improved trades differ significantly from those who were not. If the random assignment functioned properly, the odds of such significant differences will be very low. In addition, we will assess whether the differences in student characteristics are correlated with variation in exposure levels over time. Finally, we will assess for the possibility of differential attrition rates by checking for differences in the probability that applicants completed follow-up surveys in each round using the same types of comparisons.

After we validate the research design, the next step is to determine whether or not students assigned to improved trades did actually use upgraded equipment at higher rates than those not assigned to these trades. This involves two assessments. First, we will check that schools assigned to receive equipment do report receiving it and that schools and trades not assigned to be upgraded are not upgraded from sources other than MCC. Second, we will assess applicants' matriculation and graduation rates to verify that they were more likely to enroll in a course of study in an improved trade if assigned to study an improved trade in the admissions lottery.

Finally, we will evaluate the effects of exposure to the improved trades using the methodology described in Figure 2. The main focus will be to utilize the Graduate Follow-Up Survey to assess the effects on the two primary outcomes: differences in the probability that a student is employed and differences in average earnings. We will also seek to better understand the reasons for any differences observed by also assessing differences in students' factual knowledge about their assigned trades using the knowledge tests and by more carefully assessing their employment history by, for example, assessing differences in the type of jobs applicants hold. Finally, while we do not anticipate finding such long-term effects this soon, we will also look for differences in

household welfare by checking for differences in asset ownership, consumption, and the activities of other household members.

G. Timeframe of Exposure

With the current schedule of data collection activities, outlined in Table 1, Graduate Follow-Up Surveys for all three cohorts are planned for three to ten months after the student is scheduled to graduate. Since the key outcomes being measured are the graduates' employability and wages, capturing labor market outcomes shortly after graduation is the most logical timing for the follow-up data collection. Tracking Surveys, which are being administered each year after the initial Graduate Follow-Up Survey for two of the cohorts, will also capture employability and wage differences for years after graduation. Since the cohorts had different levels of exposure to equipment as well as subsets of respondents who studied improved trades and respondents who did not, the Tracking Survey will also allow an analysis of wage and employability more than a year after graduation.

V. DATA COLLECTION PLANS

A total of four surveys are planned to identify the impact of the equipment upgrades: Admissions Survey, Administrative Survey, Graduate Follow-Up Survey, and Tracking Survey. All three waves of the Admissions and Administrative Surveys have been completed. Graduate Follow-Up and Tracking Surveys are being administered to different cohorts each year. This section discusses the various surveys and the data collection timeline as depicted in Table 1 in Section IV.D.

A. Survey Instruments

1. Admissions Survey

This questionnaire was developed to record basic social, economic, and demographic characteristics, and contact information of students. The survey also functioned as the application form for the 10 schools that participated in the study. The survey included a general knowledge test to measure skill levels and academic performance. IPA and MCA-M M&E worked together with the Education Evaluation Center, a Mongolia government agency, to develop a single standardized uniform test that incorporates elements from all the tests designed by the schools in the sample. Test scores for skills tests and GPA were used by some schools as part of their admissions criteria.

2. Administrative Survey

The Administrative Survey was administered in fall of 2011, 2012, and 2013. The first round of the Administrative Survey was done in all 10 evaluation schools and consisted of three parts: Management, Teacher, and Student Performance. The Management and Teacher questionnaires were administered to school directors and teachers in order to record important school level

characteristics including price of tuition, application, enrollment, and graduation rates, teacher to student ratio, teacher competence, availability of equipment, donor activity in each of the schools, and provision of career guidance and labor market information.

The Student Performance questionnaire was administered to teachers of the students enrolled in the TVET schools. Up to two teachers of each student were asked to independently assess the student's performance in both the theory and practical aspects of the program. Each individual trade has a separate questionnaire since the courses and requirements are different for each trade, but they cover three common areas to allow for comparability between trades. In other words, students are assessed in three common areas (technical ability, attitude and approach to work, and teamwork and cooperation), but students are also assessed on their performance of tasks or understanding of concepts that are specific to the trade. The information gathered from this questionnaire will be used as another way to measure the skill level of students and will be useful to make comparisons within the group of students who received benefits of upgraded equipment.

Starting in 2012, the Administrative Survey included the Classroom Observation questionnaires in which enumerators assessed teachers' application and utilization of MCA-M funded resources along with the teachers' teaching methods. Secondary Information Questionnaire was also added to the Administrative Survey to capture information regarding school curriculum, school funding source, private-public partnerships and grants, and donor activities. The latter two rounds of Administrative Survey included five survey instruments: Teacher, Management, Student Performance, Classroom Observation, and Secondary Information questionnaires and these questionnaires were administered in 50 TVET institutions.[10] Data from the 50 TVET schools will be used extrapolated findings from the 10 schools participating in the study to a larger set of TVET students. This concern is described in more detail in Section VI.D.

3. Graduate Follow-Up Survey

The Graduate Follow-Up Survey is the primary instrument used to capture post-graduation labor market outcomes. The survey will be administered after students graduate in order learn about the short-term impact of studying improved trades. The survey covers the following measures: household composition, educational experience and future plans, employment history including both monetary and in-kind wages, type and duration of employment, employment prospects, asset ownership, consumption and expenditures, and detailed contact information. This survey also includes the administration of trade-specific skill tests. Respondents, from both the group of respondents who were assigned to improved trades and the group who were not, will be assigned to take skills tests in up to three trades: the trade that respondent was assigned to, their first ranked trade, and their second ranked trade. Most respondents take two tests since most got into their first or second ranked trade. The purpose of the skill tests is to identify changes in skill level due to studying a trade at a TVET school.

[10] Not all 52 VET Project beneficiary schools were part of the Administrative Survey. Only 50 VET Project beneficiary schools were included in the survey due to contract issues

4. Tracking Survey

In order for this evaluation strategy to work, students, rejected applicants and graduates from both improved and non-improved trades must be tracked and re-interviewed several times over multiple years. This will be a challenging task as many respondents will undoubtedly relocate during the period of data collection. If some respondents cannot be located at the time of a follow-up interview, then this will introduce attrition bias, which will skew and distort our estimates of the upgraded equipment's impact.

Thus, IPA and MCA-M developed the Tracking Survey, which collects the latest contact information on the respondents and the respondents' parents, relatives, and friends so that this data can be used to track respondents for the more extensive Graduate Follow-Up Survey (and any future in-person follow-up surveys that may be planned). The Tracking Survey is administered on the phone. For respondents who cannot be reached via phone, the Tracking Survey is administered in person. In addition to updating contact information, the Tracking Survey also collects abbreviated employment and educational attainment questions. For respondents who have already completed the Graduate Follow-Up survey, the employment and educational attainment questions can be used to assess labor market outcomes a year or more after the respondent has graduated from their TVET institution.

B. Data Collection Schedule

The data collection for this evaluation is currently in progress according to the schedule outline in Table 1. Admissions Surveys were administered in early summer and early fall of 2010, 2011, and 2012. Administrative Surveys were administered in fall of 2011, 2012 and 2013. Tracking and Graduate Follow-Up Surveys are collected in spring and summer of each year. The last round of Graduate Follow-Up Survey is scheduled for spring and summer of 2015.

VI. LIMITATIONS AND CHALLENGES

Like all designs, there are potential risks that need to be considered. With the current design, we are concerned with four primary risks: First, we are concerned that given the highly mobile sample, applicants may not complete the follow-up surveys. Second, applicants may not comply with the random assignment lotteries. Additionally, we need to be concerned that the sample is large enough to provide meaningful information. And finally, we must be concerned about whether or not the facts that we learn from the study can be related to other vocational programs in Mongolia and abroad.

A. Attrition

As described in the previous section, the research design is built around a longitudinal dataset in which students who initially apply for admissions to TVET schools are tracked over several years before completing the final follow-up survey. Since these students are young adults in

search of first jobs, they are highly mobile. And as a result, they may be very difficult to track over time. If the attrition rate is too high because too many applicants are difficult to find, then we may run into issues of our results not being representative of applicants in general, similar to the issues described in Section VI.D below. Alternatively, if we have problems with different fractions of students admitted to the improved or non-improved trades not taking the survey we could end up with confounding issues like those described in Section IV.F.

Our strategy for mitigating this risk is to follow the procedures described in Section IV.E. This includes: (1.) collecting very detailed contact information on the applicants, their friends, and family, (2.) following a careful procedure for determining when to quit looking for subjects, and (3.) using data generated during the survey process itself to monitor progress towards the overall target response rate and limiting differential attrition between subjects who were assigned to improved trades and those who were not.[11]

B. Compliance

The research design relies on two key components: that a sufficient number of students participate in the programs to which they were assigned by the lottery and that given this participation pattern, students admitted to improved trades in later cohorts are exposed to the upgraded equipment for longer periods of time than unadmitted students. The first concern arises because we cannot force students to attend TVET schools or to study the assigned trades. We can only randomly choose to whom admission is offered. Students can decline the offer and rejected students may study a given trade at another TVET school outside of our sample. Some rejected students even ended up studying at the TVET school to which they applied, either because the school bent to the demands of the students or because the school needed to fill vacancies due to declined offers.

Non-compliance with treatment assignment is problematic because it dilutes the treatment effect. The experiment is designed so that students assigned to an improved trade experience that type of trade while those who are not assigned do not. When treatment students fail to study an improved trade, they fail to experience the treatment. Since they must still be classified as treatment students, the net effect of this is that the treatment group will include students who do not experience a treatment effect, reducing the outcomes in the treatment group relative to what they would have been with perfect compliance.[12]

We tried to minimize this risk during the lottery process in two ways. First, we provided the schools a means of guaranteeing admissions to the students with special status, or particularly

[11] Initial concerns about being able to track such a mobile sample have been allayed. The detailed tracking system that we developed early on has, for example, allowed us to limit attrition in the last Graduate Follow-Up Survey to 5.8 percent of the baseline sample. In addition, the data collection firms in Mongolia have proven capable. Thus far, they have been able to meet all of our data quality targets.

[12] Technically, the Intent to Treat estimate underestimates the true treatment effect when there is non-compliance.

talents.[13] Second, we took students' trade preference into account when conducting the lotteries by asking students to rank those trades that they would be willing to study if offered admissions and admitting students to their highest ranked trade for which open spaces were available.

The second concern regarding the increased exposure levels over time is beyond our control because they depend on the implementing agencies. The expected months of exposure to upgraded equipment shown in Figure 2 is based on year of admission for the three cohorts. We will monitor the implementation of the upgrades using the Administrative Survey to determine if the equipment is, in fact, delivered on schedule.

C. Power

As with any study, a major issue is whether or not the study is capable of detecting meaningful treatment effects. If the study can only detect very large treatment effects, then unless the intervention is extremely effective, the experiment is not likely to yield meaningful results. If the experiment detects effects, then this suggests that the intervention is extremely effective. However, if the experiment does not detect effects, then we can only conclude that the intervention was not extremely effective. It may be that the intervention was entirely ineffective, but it might also be possible that the intervention was effective but just not extremely effective.

At this point in the project, the best way to check the capacity of the study to deliver meaningfully precise treatment effect estimates is to use the data available from the first round of the Graduate Follow-Up Survey. This only provides information from 4,958 of the 12,250 students in the sample, but it is still the best data we have for these purposes. To do this, we use the data we do have to create simulations of the data that we will have once we complete the survey two years from now.[14] However, the first wave of the survey is the largest. So, we create two simulated data sets. One that over-estimates the sample size by simulating all three waves of the survey (14,874 observations in total – triple the 4,958 sample) and one that under-estimates the sample by only simulating two rounds (9,916 observations or twice the 4,958 sample). The smallest treatment effects that would be statistically significant (known as the Minimum Detectable Effect-Size or MDE) from the three sample simulation should under-estimate the real

[13] These students are not considered to have been exposed to the lottery, and are not used to estimate the various treatment effects.

[14] Specifically, the simulated sample includes three rounds of data. The first round is the round of data that we already have from the Graduate Follow-Up Survey. To simulate the second cohort, we start with a duplicate of the first cohort, but assume students started a year later than they actually did, exposing them to the equipment for a longer period of time. So, for example, if a student began studying an improved trade in September 2010 and finished in December 2012 and the trade received upgraded equipment in September of 2012, then the level of exposure in the first cohort is calculated as four months. That same student is then included again in the second round, but assuming she started in September 2011 and finished in December 2013, exposing her to the equipment for a longer period of time. The third round is then constructed using a similar process, but assuming students started two years later than they actually did. This strategy allows us to preserve the relationship between application to an improved trade, the probability of admissions, and the observed outcome while simulating increased exposure across subsequent cohorts.

value, while those from the two sample simulation should over-estimate them.[15] As a result, these estimates should provide an upper and lower bound, and the true MDEs should fall between the two estimates.

Table 2. Projected Minimum Detectable Effect Sizes

	Knowledge Test Score (Std. Deviations)	Employed (Percent)	Monthly Income (MNT)
Three Simulated Rounds			
Monthly Exposure to Improved Equipment	0.005	0.23	1,470
Implied Difference between Admitted and Unadmitted Students	0.056	2.4	15,601
Two Simulated Rounds			
Monthly Exposure to Improved Equipment	0.008	0.35	2,270
Implied Difference between Admitted and Unadmitted Students	0.065	2.8	18,324

We estimate MDEs on three primary outcomes – the trade-based knowledge test, employment and monthly earnings at the time of the survey – the results are presented in Table 2. For each outcome we present the estimated effect per month of exposure. Because this is measured in a per-month effect, it is a bit difficult to interpret. As a result, we also report the overall difference in outcomes that corresponds to the per-month effect. To estimate the MDE for the monthly effect, we calculate the smallest statistically significant effect at the five percent significance level using the standard errors from the research design discussed in Section IV.[16] To estimate the implied overall effect on students admitted to an improved trade relative to those who were not admitted, we multiply the monthly MDE by the average number of months admitted students are exposed to the equipment in the simulated sample. This is 10.6 months in the three round sample and 8.1 months in the two round sample.

Starting with the three round simulation, the estimates suggest that the MDEs will be sufficiently small to detect meaningful effects. For test scores, we that we will be able to detect an effect of at least 0.005 standard deviations per month of exposure and an implied difference between admitted and unadmitted students of 0.056 standard deviations. For employment, we will be able to estimate an effect of 0.23 percentage points per month and an overall effect of 2.4 percentage points. This is in addition to the 19.6 percent employment rate for students not admitted to the improved trades. Finally, for earnings, we will be able to detect an effect of 1,470 Mongolian Tugrik (MNT) (0.86 USD) per month and an overall effect of 15,601 MNT (9.18 USD), both over the average earnings for unadmitted students of 109,226 MNT (64.26 USD).[17] As expected, the estimates using two rounds are less precise, but they are only slightly larger than the three

[15] These estimates are likely a lower bound for two reasons. First, the sample size is smaller than the actual sample, and second, without the last round of data, there is significantly less variation in equipment exposure than there will be in the full sample.
[16] This is the upper bound on corresponding confidence interval centered at zero.
[17] Exchange rate of 1700 MNT/USD was used.

sample estimates. Even these lower-bound estimates are sufficiently precise to provide a meaningful test of the equipment upgrades.

D. Generalizability

Most research designs can only estimate treatment effects based on a limited subsample of the data. Regression discontinuity designs, for example, are only directly applicable to subjects at the point of discontinuity in treatment assignment. In this design, we are primarily limited by the fact that only a portion of the TVET schools in Mongolia had more applications than seats in 2010. As a result, the sample of schools in our study includes 10 of the 72 TVET schools in Mongolia, and the question become to what degree can the results from these 10 schools suggest the effects of the equipment upgrades in the other schools that received them.

To address the issue, we collected information from the 50 TVET schools included in the VET Project. This data will allow us to compare the schools in our sample to the other schools and to assess the degree to which estimated treatment effects vary along dimensions in which the schools differ. For example, if we find in the survey that schools in our sample are, on average, smaller than the rest of the schools in the country, we can use the variation in school size within our sample to estimate where the effect of studying in an improved trade is different for larger versus smaller schools. The goal of this analysis is to attempt to provide an estimate of the effects of the upgrades for most of the beneficiary schools[18]. It will also provide information on the degree to which students in other trades or schools using outdated equipment might benefit from future upgrades.

VII. ADMINSTRATIVE

A. Summary of IRB Requirements and Clearances

Studies conducted by IPA must obtain approval through IPA's Institutional Review Board (IRB). The prospective study must demonstrate that there is no or negligible risk of harm to the respondent through participation in the study. The prospective study must also outline a data security protocol and a survey protocol that will ensure personally identifying information and any sensitive information on the respondent is properly stored and protected. Researchers affiliated with this study submitted an application to IPA's IRB regarding this study and received approval on August 2010. Researchers must submit annual renewal requests with any changes or updates to the research design, consent form, or surveying protocol. Approvals for annual renewal have been submitted and obtained every year for this study.

B. Preparing Data Files for Access, Privacy and Documentation

[18] Administrative Survey was collected among 50 out of 52 beneficiary schools so the estimate of the effect of the upgrades cannot be provided for all beneficiary schools

IPA is acutely aware that the data that is collected as part of this evaluation contains sensitive and potentially identifying information. All information identifying individuals is treated as strictly confidential. IPA has and will release identifying information to MCA-M and MCC employees and select contractors that participate directly in the development, execution, and evaluation of the VET Project and for whom such data is absolutely necessary. As part of the IRB approval process, the evaluation must lay out a plan of securing digital and paper-based data. All identifying data such as names, national ID, and addresses of respondents or respondents' contacts are removed from the dataset at the earliest possible time and stored in a digitally secure manner. Paper-based records are secured in locked compartments in a room with a security system.

For data publication, IPA plans to follow MCC's Data Publication Guideline to properly anonymize the data and to identify any potential risk to respondents before a dataset is ready to be shared publicly. In fact, IPA has followed the MCC Data Publication Guidelines and discussed the anonymization process and risks for identification with MMC's Data Review Board to publish the Admissions Survey on MCC's website. IPA intends to follow the same process for follow-up and tracking data once the full data collection rounds are completed.

C. Dissemination Plan

After all rounds of follow-up data have been collected on all three cohorts of students and researchers are able to produce the final impact evaluation report, IPA plans on holding a dissemination event in Mongolia with relevant Mongolia stakeholders in the Ministry of Labor, Institute of Labor Studies, Ministry of Education, Cabinet Secretariat, interested TVET institution directors, and representatives of other donor agencies and private companies that have invested in vocational education in Mongolia. IPA plans on holding this event in spring 2016. IPA researchers are also planning to present the findings from the study in DC with relevant MCC stakeholders, and to publish the results in an academic journal. IPA also plans on writing policy briefs and to publicize the results through the IPA and JPAL (Abdul Latif Jameel Poverty Action Lab) network. IPA has a dedicated team that seeks to publicize interesting results through various media outlets.

D. Evaluation Team Roles and Responsibilities

The evaluation team comprises the lead Principal Investigator (PI), Dr. Leigh Linden, other PI's on the evaluation, Dr. Erica Field, Dr. Shing-Yi Wang, and Dr. Daniel Rubenson, as well as the field staff. Dr. Linden will make critical decisions regarding study design, sampling, analysis, and interpretation of results. Dr. Linden will also take the lead in analysis and interpretation of results and in drafting of all reports to MCC, including design documents, survey questionnaires, interim data collection reports, and the final impact assessment report. The other PI's on the project also will contribute to decisions regarding the study and support Dr. Linden as necessary.

The field staff typically consists of the Country Representative, Project Coordinator, Mongolian-speaking Project Officer, and Data Quality Monitors. The Project Coordinator acts as the primary in-country contact for the evaluation, organizing all activities related to the randomization and data collection and coordinating closely with data collection firms. The Project Coordinator is responsible for executing the randomization according to plan, procuring the data collection firm, piloting and testing of survey instruments, ensuring the data collection adheres to survey protocol. The Project Coordinator is also responsible for cleaning, merging, and preparing the data sets for analysis and for executing the preliminary analysis according to the lead PI's specifications and analysis plan. Additionally, the Project Coordinator is responsible for preparing data sets for publication and with assistance from the lead PI, interpreting results and drafting reports to MCC and other stakeholders. The Project Coordinator is also responsible for communicating with local Mongolian stakeholders to provide information regarding the evaluations and understanding Mongolian stakeholders concerns and questions regarding the evaluation.

The Mongolian-speaking Project Officer is responsible for daily interaction with surveyors, survey management staff, and quality assurance staff. The Project Officer trains and supervises data quality staff who are tasked with reviewing random sample of surveys to ensure surveys are being collected according to established protocol. The Project Officer will arrange trips to verify project and survey activities and is responsible for ensuring surveys are translated correctly into Mongolian and is suitable to the Mongolian context. The Project Officer also translates all reports and relevant document for Mongolian audiences and researchers relevant issues and news that may have bearing on the evaluation. Baasansuren Enkhtungalag has been working as Project Officer for this evaluation since July 2013.

The Country Representative is responsible for reviewing the quality of the Project Coordinator's work and facilitating any relationship with government entities that the Project Coordinator may need. The Country Representative is also responsible for operational activities that are necessary for IPA to operate as a Non-Governmental Organization in Mongolia and is responsible for executing any activities associated with contractual obligation that IPA has in the country and with MCC. Jihae Hong has been fulfilling duties of Country Representative and Project Coordinator of this study since May 2013.

E. Evaluation Timeline and Reporting Schedule

As described in Section IV.D and summarized in Table 1, admissions baseline data collection occurred between June and September of 2010, 2011, and 2012 admission years. Graduate Follow-Up and Tracking Surveys are scheduled for spring and summer of 2013, 2014, and 2015 to capture post-graduation student-level outcomes of applicants. We have finished administering Graduate Follow-Up and Tracking Surveys to two out of three cohorts. Administrative Survey was collected during fall of 2011, 2012, and 2013 and will be integrated into the full set of follow-up data to inform analysis and interpretation of the student-level outcomes.

The reporting schedule is fairly simple. The work plan contains three reports, including this Impact Evaluation Strategy. The other reports are the admissions baseline report which has already been submitted to and accepted by MCC, and the Final Impact Evaluation Report which will include all of the impact estimates and other analyses described above. The Final Impact Evaluation Report is expected to be completed by the end of 2015.

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X. ANNEXES

A. Graduate Follow-Up Survey Estimation

Assumptions: - total respondents - 3856

3 interviews a day per enumerator

1 field visits:
- 9 aimags where participating schools are located
- 21 aimags possibly

Required number of total field staff - 43

Required number of field staff in UB - 30

Required number of field staff traveling outside UB- 20 (in the past MMCG used same enumerator to travel to multiple aimags, so this number includes duplicates)

Required number of supervisors - 5

Required working days in the field per enumerator in UB: 16

Required working days in the field for field staff traveling outside UB (higher density aimags): 48

Required working days in the field for field staff traveling outside UB (lower density aimag): 17

Required number of surveys in higher density aimag ~ 1900

Required number of surveys in lower density aimag ~ 500

Required number of surveys in UB~ 1400

Transportation for field visit 1	**per item**	**Times**	**Enumerators**	**total**	**total in USD**
Land travel to the aimag center	300,000.00₮	1	15	4,500,000.00₮	$ 2,812.50
Air travel to the aimag center	500,000.00₮	1	15	7,500,000.00₮	$ 4,687.50
Land travel between soums	60,000.00₮	12	20	14,400,000.00₮	$ 9,000.00
Travel within UB	15,000.00₮	16	30	7,200,000.00₮	$ 4,500.00
Sub-total (Graduate Follow Up Survey and Tracking)				33,600,000.00₮	$ 21,000.00
Sub-total Graduate Follow Up Survey				**23,520,000.00₮**	$ 14,700.00

Travel Expenses	per item	Days			
Per diem for travel (UB)	- ₮	16	30	- ₮	$ -
Per diem (Field) Lower Density	45,000.00₮	17	10	7,650,000.00₮	$ 4,781.25
Per diem (Field) Higher Density	45,000.00₮	48	20	43,200,000.00₮	$ 27,000.00
Phone units for enumerators, supervisors	5,000.00₮	29	48	6,960,000.00₮	$ 4,350.00
Sub-total (Graduate Follow Up Survey and Tracking)				57,810,000.00₮	$ 36,131.25
Sub-total Graduate Follow Up Survey				**40,467,000.00₮**	$ 25,291.88
Prices	per item	Respondents/days/times			
Incentives*	10,000.00₮	3856	1	38,560,000.00₮	$ 24,100.00
Supervisor salary	60,000.00₮	100	5	30,000,000.00₮	$ 18,750.00
Enumerator salary (UB)	40,000.00₮	21	30	25,200,000.00₮	$ 15,750.00
Enumerator Salary (Field) Lower Density	40,000.00₮	20	10	8,000,000.00₮	$ 5,000.00
Enumerator Salary (Field) Higher Density	40,000.00₮	50	20	40,000,000.00₮	$ 25,000.00
Sub-total				**141,760,000.00₮**	$ 88,600.00
TOTAL AMOUNT - MNT				**205,747,000.00₮**	$ 128,591.88

Cost for per questionnaire				**80,271.52₮**	**$ 50.17**

Price	per item	USD	months/times	personnel/respondents	total MNT	TOTAL USD
Team leader salary	3,500,000.00₮	$ 2,187.50	7	1	24,500,000.00₮	$ 15,312.50
Database manager salary	2,940,000.00₮	$ 1,837.50	7	1	20,580,000.00₮	$ 12,862.50
Field Manager salary	5,000,000.00₮	$ 3,125.00	5	1	25,000,000.00₮	$ 15,625.00
Sub-total		$ -			**70,080,000.00₮**	$ 43,800.00
Instrument testing and piloting	500,000.00₮	$ 312.50	1	1	500,000.00₮	$ 312.50
Training Cost (UB)	3,200,000.00₮	$ 2,000.00	1	1	3,200,000.00₮	$ 2,000.00
Sub-total		$ -			**3,700,000.00₮**	$ 2,312.50
Follow-up survey data collection cost					**205,747,000.00₮**	$ 128,591.88
Total					**279,527,000.00₮**	$ 174,704.38
Contingency/Overhead cost*					**30,000,000.00₮**	$ 18,750.00
GRAND TOTAL					**309,527,000.00₮**	**$ 193,454.38**

B. Tracking Survey Budget Estimation

Assumptions: - total respondents - 8000

6 interviews a day per enumerator

Assumption: 30 enumerator

50 enumerator days required including 6 extra days

Transportation for field visit 1	**per item**	**Times**	**Enumerators**	**total**	**total in USD**
Land travel to the aimag center	300,000.00₮	1	15	4,500,000.00₮	$ 2,812.50
Air travel to the aimag center	500,000.00₮	1	15	7,500,000.00₮	$ 4,687.50
Land travel between soums	60,000.00₮	12	20	14,400,000.00₮	$ 9,000.00
Travel within UB	15,000.00₮	16	30	7,200,000.00₮	$ 4,500.00
Sub-total (Graduate Follow Up Survey and Tracking)				**33,600,000.00₮**	$ 21,000.00
Sub-total Tracking				**10,080,000.00₮**	$ 6,300.00
Travel Expenses	per item	Days			
Per diem for travel (UB)	- ₮	16	30	- ₮	$ -
Per diem (Field) Lower Density	45,000.00₮	17	10	7,650,000.00₮	$ 4,781.25
Per diem (Field) Higher Density	45,000.00₮	48	20	43,200,000.00₮	$ 27,000.00
Phone units for enumerators, supervisors	5,000.00₮	29	65	9,425,000.00₮	$ 5,890.63
Sub-total (Graduate Follow Up Survey					$

and Tracking)				**60,275,000.00₮**	37,671.88
Sub-total Tracking				**18,082,500.00₮**	$ 11,301.56
Prices	per item	Respondents/days/times			
Incentives*	5,000.00₮	8000	1	40,000,000.00₮	$ 25,000.00
Supervisor salary	60,000.00₮	50	2	6,000,000.00₮	$ 3,750.00
Enumerator salary	40,000.00₮	50	30	60,000,000.00₮	$ 37,500.00
Sub-total				**106,000,000.00₮**	$ 66,250.00
TOTAL AMOUNT - MNT				**134,162,500.00₮**	$ 83,851.56

Cost for per questionnaire				**28,297.81₮**	**$ 17.69**

Price	**per item**	**USD**	**months/times**	**personnel/respondents**	**total MNT**	**TOTAL USD**
Team leader salary	1,500,000.00₮	$ 937.50	7	1	10,500,000.00₮	$ 6,562.50
Database manager salary	1,260,000.00₮	$ 787.50	7	1	8,820,000.00₮	$ 5,512.50
Field Manager salary	5,000,000.00₮	$ 3,125.00	4	1	20,000,000.00₮	$ 12,500.00
Sub-total		$ -			**39,320,000.00₮**	$ 24,575.00
Cost of calling center	10,000,000.00₮	$ 6,250.00	3	1	30,000,000.00₮	$ 18,750.00
Training Cost (UB)	2,900,000.00₮	$ 1,812.50	1	1	2,900,000.00₮	$ 1,812.50
Sub-total		$ -			**32,900,000.00₮**	$ 20,562.50

Follow-up survey data collection cost					**134,162,500.00₮**	$ 83,851.56
Total					**206,382,500.00₮**	$ 128,989.06
Contingency/Overhead cost*					**20,000,000.00₮**	$ 12,500.00
GRAND TOTAL					**226,382,500.00₮**	**$ 141,489.06**