

Identifying Delivery Strategies for Cost-Effective Health Interventions

Ex-Ante Analysis Plan submitted to JPAL Hypothesis Registry

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I. Project Overview

A. Design and Main Hypotheses

The goals of the project are to measure the amount of leakage and extortion in the distribution of subsidized health products through health facilities, to estimate the effectiveness of various anti-corruption measures that distribution programs could implement, and to analyze the correlates of corrupt behavior to try to learn about the determinants of corruption.

Our sample consists of 72 health facilities selected for inclusion based on a census conducted of all of the health facilities in one region (Brong Ahafo) in Ghana. We will conduct a distribution program of free Long Lasting Insecticide-Treated Nets (LLINs) through the Antenatal Clinics (ANCs) at these facilities, for 3 to 4 months, and randomly vary several dimensions of the program that we believe will affect leakage and extortion. Pregnant women visiting these facilities for a routine check-up with ANC staff will be eligible for one free LLIN.

We randomly assigned 5 dimensions of the program:

1. Distribution mechanism: whether the subsidized product is distributed directly through health centers, or indirectly through a voucher scheme wherein the clinic distributes vouchers that can be redeemed at local stores (Direct vs. Voucher)
2. Staff monitoring: whether the health worker is monitored through audits (Audit vs. No Audit). Clinics in the Audit group will be informed that if leakage is detected in the audit, the program will be shut down.
3. Incentives: whether, for Direct clinics only, the health worker receives compensation for implementing the distribution (Compensation vs. No Compensation)
4. Stock levels: whether, for Direct clinics only, the clinic receives a high or low level of net stock (High stock vs. Low stock)
5. Competition: whether, for Voucher clinics only, vouchers can be redeemed in 1 or 2 stores (1-store vs. 2-store)

All treatments were randomly assigned in cross-cutting fashion, to produce a total of 12 treatment arms. That is, clinics were randomly assigned to Direct vs. Voucher. Stratified based on that, they were assigned to Audit vs. No Audit. Then, stratified based on audit assignment, Direct clinics were randomly assigned to Compensation vs. No Compensation and to High stock vs. Low stock. Stratified based on audit assignment, Voucher clinics were assigned to 1-store vs. 2-store (all Voucher clinics are No Payment, and receive a large enough stock of vouchers to carry them through the full program).

NOTE: In clinics sampled for the audit treatment, the audits are supposed to be announced and introduced around week 5 or 6 of the distribution program. If, however, the levels of leakage observed in the first 5-6 weeks are extremely low, making any audit intervention obviously cost-ineffective, we will cancel the audit treatments in all clinics. This will not affect our ability to estimate the effects of all the other treatments and their interactions.

B. Data Sources

The primary data sources for the study will be:

1. Census Data: Baseline data on basic facility characteristics collected before the fieldwork, e.g., size, number of workers, etc.
2. Health Facility Administrative Data: Antenatal Clinic (ANC) Registers (records the names and visit dates of all ANC patients)
3. Net Program Administrative Data
 - a. Net Program Product Distribution Logs: Logs in which facilities and stores record how many products were distributed and information about product recipients
 - b. Net Program Facility Call Logs and Delivery Logs: Logs tracking when facilities call for nets to be restocked, how many nets are dropped off at the facility
4. Mystery Client visits
 - a. Direct: Mystery clients (undercover research staff members who are clearly ineligible for the subsidized product under program protocol) will try to obtain nets/vouchers at the program clinics (and when applicable, program stores). Afterwards, they will record whether or not they are asked to pay a bribe in order to obtain the product, the bribe they were asked to pay, and other information about the visit.
 - b. Indirect: Mystery clients (undercover research staff members) will also ask ineligible residents of the community surrounding the clinic to try to obtain the net or voucher on their behalf. They will ask the community member whether or not he was asked to pay a bribe in order to obtain the product at the clinic/store, and the bribe they were asked to pay.
5. Eligible Client Surveys:
 - a. Endline Client Survey: We will randomly sample clients from the Net Distribution log and ANC register and visit them at their homes at the end of the study for a short survey about their experiences when visiting the clinic
 - b. Outside-Clinic Client Survey: During the program period, undercover research staff members will randomly approach women leaving the ANC clinics and ask them whether they received a net, for what price, etc.

6. Community Interviews
 - a. Market Interview: During the distribution period, undercover research staff members will sample people in the market and survey them about their knowledge of the program, including eligibility criteria and price for a net, and whether they think an ineligible person would be able to obtain a net
 - b. Compound Interviews: During the distribution period, undercover research staff members will conduct home interviews at residential compounds randomly selected among those near the facility about respondent knowledge of the program and whether they think an ineligible person would be able to obtain a net
7. Store Surveys: Visits to local stores to check for availability and price of bednets.
8. Health Worker Survey: A survey will be administered at the end of the distribution program, to measure health worker characteristics and preferences. The survey will aim to measure time-invariant characteristics that we do not expect to be affected by the various treatments (ideally this survey would be done at baseline, but it is not possible given that we do not want health workers to be aware that there is a study going on; thus this survey has to take place at endline, at the time we debrief health workers (as per IRB requirements) about the study.

C. Comments on Empirical Specifications

All empirical specifications where we are regressing outcomes on treatment dummies will control for randomization-stratum fixed effects (FE). There are 6 strata of 12 clinics each. Clinics were matched into strata based mainly on clinic volume (specifically, baseline ANC visits per month) and location (whether or not they were close to the national border). For robustness checks, we will run regressions without stratum FE as well.

All empirical specifications where we are regressing outcomes on treatment dummies will be clustered by clinic (or will aggregate the outcome to the clinic level).

II. Balance

We will verify that baseline clinic variables were balanced across the different treatment assignments; specifically, comparing the baseline characteristics across each dimension of randomization (e.g., Audit vs. No Audit), as well as comparing each of the 12 treatment arms with the others. Outcome variables that we will use for the balance analysis will include the following variables (all data from the census):

- Number of monthly ANC registrants

- Number of monthly ANC visits
- Anticipated number of nets given per month (calculated based on ANC registrants and ANC visits data)
- Number of ANC midwives
- Number of ANC staff
- Years of facility operation
- Whether the facility is a "CHPS", a category denoting a small health facility, normally found in more remote areas
- Whether facility has a maternity ward
- Number of other ANC facilities within 10 km
- Whether the facility has distributed nets in the past
- Whether the facility is accessible in the rainy season

III. Treatment Effects Analysis and Hypotheses

A. Comparisons of Means and Time Trends

We will first calculate the average measures of all of the different leakage and corruption measures (outlined below in the treatment effects analysis section), both across the full sample and within each treatment arm (e.g., average for all direct clinics with no payment and no audit threat) as well as for each broader treatment group (e.g., average for all voucher clinics, average for all direct distribution clinics, etc.). We will also look at how these measures change over time (e.g., by two-week groupings from program launch date in a given clinic, across the full sample and within treatment groups) to trace out the time path of corruption; if it fades out quickly, it is less of a policy concern than if it is more persistent.

B. Regression Specifications-- All Treatments Except the Audit Treatment

The baseline specification will test whether there are differences in outcome variables (outlined below) across our different treatments:

$$y_{ic} = \alpha + \beta Treat_c + \varepsilon_{ic} \quad (1)$$

Where i indexes an individual observation (e.g., a client for outcomes that come from client surveys), c indexes a clinic, and $Treat_c$ is a vector of the several cross-cutting treatments. For outcomes at a lower level than the clinic level, all standard errors will be clustered at the clinic level.

Treatment variables we will use:

- Voucher (full sample)
- 2-store (voucher clinics only)
- Payment (direct distribution clinics only)

- Low stock (direct distribution clinics only)

We will also test for the interactions between the different treatments:

$$y_{ic} = \alpha + \beta_1 Treat^1_c + \beta_2 Treat^2_c + \beta_3 Treat^1_c * Treat^2_c + \varepsilon_{ic}$$

where we will test for interactions between:

- Payment * Low stock (direct distribution clinics only): If baseline leakage is lower when stocks are kept low, then the payment treatment may have a smaller effect

As mentioned before, all regressions will control for stratum FE. To increase precision, we will also show a specification with control variables, which will include:

- Rollout Group FE (indicates which area the program was rolled out in when)
- Indicator for being in the first rollout group (which was used as a “mini-pilot” to fine-tune distribution protocols)
- Baseline control variables: ANC attendance at baseline, # of other ANC facilities within 10 km radius, # of midwives working at facility, etc.

We will also try robustness checks controlling for surveyor fixed effects or date fixed effects (e.g., if there is a political disturbance in the area, we may want to control for whether clinics were operating at that time).

We will also look at the time horizon of effects, e.g., performing the regression separately by two-week groupings, or including interactions between the time since the program started and the treatments, to trace out whether the effects of the treatments differ by time (e.g., leakage could be higher in direct clinics than voucher clinics but only in the first few weeks following program launch).

B. Regression Specifications-- Audit Treatment

Base Audit Specifications

If leakage is sufficiently high, we will roll out the audit threat approximately 5 weeks after the program has launched in a given facility in order to enable a difference-in-differences approach to analyze the effect of audits. Thus, we will run regressions of the form:

$$y_{ict} = \alpha + \beta Audit_c * Post_{ict} + \delta Audit_c + \lambda Post_{ict} + \varepsilon_{ict} \quad (2)$$

Where i indexes individuals (for outcomes at the individual level), c indexes health facilities, and t indexes the time period; y_{ict} is one of the outcome variables listed below; and, $Post_{ict}$ is an indicator for being in a time period after the audit threat was conducted in a clinic’s rollout group (for logistical purposes, clinics will be phased into the net distribution program (one group of 12 clinics every 2 weeks); thus,

since audit threats will occur a fixed amount of time after rollout for each clinic, they will also be phased). We will use the standard approaches to correct standard errors in a difference-in-differences framework (e.g., clustering standard errors at the clinic level).

To increase power and check robustness, we will try this regression with stratum FE and with clinic FE, and with and without the clinic baseline control variables outlined above.

Note that this specification assumes that the relevant time dimension that we need to control for is weeks-from-launch in a given clinic, not calendar time. Although this is reasonable given the very short time horizon of the program and the fact that net demand is likely to vary significantly with the time from launch, we will also control for the calendar time (e.g., week or month) as a robustness check.

Audit Interaction Specifications

We will also test whether there are any interactions between the audits and the other treatments, i.e., run regressions of the form:

$$y_{ict} = \alpha + \beta_1 Audit_c * Post_{ict} + \beta_2 Audit_c * Post_{ict} * Treat_c + \delta_1 Audit_c + \delta_2 Audit_c * Treat_c + \lambda_1 Post_{ict} + \lambda_2 Post_{ict} * Treat_c + \gamma Treat_c + \varepsilon_{ict} \quad (3)$$

and test whether $\beta_1 = 0$ and whether $\beta_2 = 0$ where $Treat_c$ represents one of the following treatment variables:

1. Payment (Direct clinics only): The audit threat might be more effective if combined with the payment treatment, since the health workers will then have a greater direct financial incentive (and one that does not depend on profiting from leakage) to keep the program running.
2. Voucher: If leakage or bribery is lower under the voucher treatment, the audit should have a smaller effect
3. Low stock (direct clinics only): If low stock clinics have lower leakage, the audit could have a smaller effect
4. 2 stores: If voucher clinics partnered with 2 stores have lower leakage, the audit could have a smaller effect

D. Families of Outcome Variables and Corresponding Hypotheses

We will use the following categories of outcome variables (the specific way we will measure each outcome is detailed in the next section):

1. Leakage to ineligible population

Primary Hypotheses to test:

1. Vouchers: Ineligibles may not fully trust vouchers or that the shopkeepers would allow them to redeem the vouchers, so this may decrease the health worker's ability to extract bribes and should decrease health-worker leakage. If health workers fear that shopkeepers will report that ineligibles are redeeming vouchers, this could also reduce leakage.
2. Compensation: Higher compensation may decrease leakage if honesty is a normal good or if compensation increases the costs (e.g., guilt) associated with disobeying program rules.
3. Audits (without compensation): Audits can decrease leakage if health workers are altruistic and so do not want to risk the program ending, or if it is profit-maximizing for health workers to decrease their leakage to keep the program running for longer. Although health workers have tenure, they could also think that there are non-monetary ways that they could be punished for non-compliance.
4. Audits (with compensation): Interacting compensation with audits should differentially decrease leakage through the efficiency wage effect of deterring corruption by increasing the private benefit of keeping the program running
5. Low-stock: The appearance of scarcity makes health workers more aware of the "opportunity cost" of giving/selling products to ineligibles
6. 2-stores: Increasing the number of stores could increase the leakage of vouchers by health workers if a greater number of shopkeepers means that there is a greater chance one would be willing to accept vouchers from ineligibles/collude with the healthworker, or if the competition induced by the extra store decreased the bribe amounts requested at the shop level and the health workers could credibly signal this to ineligibles thereby increasing the value of the voucher and their ability to extract bribes.

2. Extortion of eligible clients

Primary Hypotheses to test:

1. Vouchers: Eligibles may not fully trust vouchers (e.g., if they think that the shopkeepers can refuse a voucher, or if they think that there might be more vouchers than products in the shops), so this may decrease the health worker's ability to extract bribes.
2. Compensation: Higher compensation may decrease extortion if honesty is a normal good or if compensation increases the costs (e.g., guilt) associated with disobeying program rules
3. Audits (without compensation): Audits can decrease extortion if it is profit-maximizing for health-workers to decrease extortion to keep the program running for longer, or if health workers are altruistic and do not want to risk the program ending (and they think that the welfare of the eligible clients still increases when they receive a net even if they have to pay a bribe).
4. Audits (with compensation): Interacting compensation with audits should differentially decrease extortion through the efficiency wage effect of deterring corruption by increasing the private benefit of keeping the program running

5. Low-stock: Low stock could change the percent of eligibles charged bribes if health workers have a target total amount of bribes collected per net delivery
6. 2-store: Greater competition at the store level decreases the shopkeeper's market power and so should decrease their ability to extract bribes

3. Share of eligible population not offered net/voucher

Primary Hypotheses to test:

1. Vouchers/Direct:
 - a. Share of eligible population offered net should be less than 100% for both treatments since offering the product and preventing stockouts take effort
 - b. Vouchers should increase coverage relative to direct, since it could be less effort for health workers to distribute a piece of paper and since stockouts will be less frequent because stocks are larger.
 - c. Coverage should increase with corruption opportunities in direct distribution (e.g., higher coverage among richer clients if they pay larger bribes)
2. Compensation:
 - a. Higher compensation may increase coverage if honesty or effort increase with income, or if health workers feel more guilty disobeying program rules when they are receiving compensation
3. Audits (without compensation):
 - a. Audits can improve compliance with program rules if health workers are altruistic and so do not want to risk the program ending (and they internalize the welfare of the eligibles to whom they offer the product), or if health workers assume they can avoid detection if they reduce corruption under a certain level.
4. Audits (with compensation):
 - a. Interacting compensation with audits should differentially increase the share offered nets through the efficiency wage effect of increasing program compliance by increasing the private benefit of keeping the program running
5. Low-stock:
 - a. Low stock could decrease the share offered if there is a higher perceived opportunity cost of nets and health workers want to save nets to sell them for a higher price to ineligibles, or if they are altruistic and receive heterogeneous value from giving nets to different types of eligible clients (e.g., they do not want to "waste" nets on rich eligible clients who could afford them themselves)
6. 2-store:
 - a. Greater competition at the store level decreases the shopkeeper's market power and so decreases their ability to extract bribes, which could increase coverage if health workers are more likely to make the effort to give vouchers if they think the client would be able to redeem it without having to pay a bribe

4. Share of eligible population covered by product

1. Vouchers:
 - a. Usage rates conditional on obtaining net should be higher for vouchers than direct since those with lower value for the nets may not pay the time/effort cost to go obtain a net
 - b. Combined effect on average percent eligibles using a net will depend on the difference in product offering rates, the voucher redemption rate, and differential take-up conditional on receiving the net
2. All other treatments:
 - a. Hypothesized effect is the net effect across the effect on extortion among eligibles (yielding lower coverage if eligibles don't pay the bribes) and the effect on product offering rates

5. Effort: Absenteeism, Number of Patients Seen

Primary Hypotheses to test:

1. Vouchers/Direct: Vouchers could increase absenteeism and decrease the number of patients seen relative to direct if health workers can extract more bribes from eligibles in direct clinics and so they increase their attendance and patients seen to extract more bribes (assumes the effort of seeing more patients is less than the effort of finding extra ineligibles to leak to, net of the differential guilt from leaking to ineligibles). Vouchers could also increase the number of patients seen relative to direct if health workers in direct clinics decrease their patient load to avoid the extra work of giving patients a net, but vouchers are easier for them to distribute.
2. Compensation: Higher compensation may increase effort if the cost of effort decreases with income (e.g., because workers are happier)
3. Audits (without compensation): Audits could improve effort if health workers perceive that their effort or attendance is also being monitored (and they have altruistic or profit-maximizing incentives to keep the program running), or there are Hawthorne effects (i.e., they act differently because they are being monitored)
4. Audits (with compensation): Interacting compensation with audits could differentially decrease leakage through the efficiency wage effect if health workers perceive that their effort or attendance is also being monitored

7. Effort: Frequency of stockouts of subsidized product

Primary Hypotheses to test:

1. Vouchers/Direct: Vouchers decrease stockouts because stocks delivered are much larger (vouchers take up almost no room and thus even small health facilities can store large amounts of them). There is no inventory management problem for health workers to think about.
2. Compensation: Higher compensation may decrease stockouts if the cost of effort decreases with income (e.g., because workers are happier). It could also decrease stockouts if it decreases leakage and stockouts are more frequent with higher leakage.

3. Audits (without compensation): Audits could decrease stockouts if health workers think that stockouts would cause the program to be shut down and they have altruistic or profit-maximizing motives to keep the program running, or if audits decrease leakage and leakage increases stockouts
4. Audits (with compensation): Interacting compensation with audits could differentially decrease leakage and extortion through the efficiency wage effect; this could decrease stockouts if healthworkers think program will end as result of stockouts, or increase stockouts if it decreases the incentive to keeps nets in stock and health workers don't think the program will be shut down for failing to prevent stockouts
5. Low-stock: Low stock could increase the frequency of stockouts, e.g., if health workers monitor stocks at fixed intervals because monitoring is costly.

7. Effort: Quality of ANC and Net Distribution records kept by clinic

Primary Hypotheses to test:

1. Vouchers/Direct: Vouchers increase quality of record keeping if they lead to lower leakage rates and staff hide leakage through poor record keeping
2. Compensation: Higher compensation may increase record keeping quality if the cost of effort decreases with income (e.g., because workers are happier). It could also increase record quality if it decreases leakage and bad records are more frequent with higher leakage.
3. Audits (without compensation): Audits could increase record-keeping quality if health workers think that poor records would cause the program to be shut down and they have altruistic or profit-maximizing motives to keep the program running, or if audits decrease leakage and leakage increases poor record-keeping.
4. Audits (with compensation): Interacting compensation with audits could differentially decrease leakage and extortion through the efficiency wage effect; this could increase record-keeping quality if healthworkers think program will end as result of poor records.
5. Low-stock: Low stock could increase record quality if it decreases leakage and bad records are more frequent with higher leakage.

E. Outcome variables to be used for the above analyses:

We will use the following outcome variables in the above specifications. Note that the control variables used may vary by specification, particularly w.r.t. elements of the measurement details (for example, when looking at health worker attendance, we will want to condition on the time of day the measure was taken).

1. Leakage to ineligible population

a. Measures Derived from Mystery Client Visits and Indirect Mystery Client Visits

- Whether mystery client had the option to acquire a net, and if yes, at what price

- Whether mystery client had the option to acquire a program net, and if yes, at what price
- Whether mystery client was told he couldn't acquire a net because of ineligibility
- Whether mystery client was told he couldn't acquire a net because the clinic ran out of nets
- Whether mystery client was offered a net in exchange for a bribe under a certain threshold (e.g., 2 GHC, 5 GHC, 10 GHC)
- Difference in each of the above measures between direct and indirect mystery clients

b. Measures Derived from Administrative Data

- Quality of records kept by clinic (both ANC registers and Net Product Distribution Log).
- Share of subsidized products diverted to ineligible population, calculated either as:
 - Nets delivered *minus* Eligible clients seen (calculated using the ANC registers, adjusted downwards by the undercoverage rate calculated through client surveys and stockout estimates)
 - Nets delivered *minus* Clients listed in Net Product Distribution Log (adjusted downwards by rate of ineligible or fake patients, as calculated based on client surveys and finding rates)

Note: Both of these measures will require records kept by the clinics. If those records are missing or badly kept, we will not be able to compute these measures for some clinics.

c. Leakage Measures Derived from Community (Market and Home) Interviews

- Percent of Community (Market and Home) Survey respondents that think the Mystery Client could obtain a net
- Average Price at which the Community (Market and Home) Survey respondents think the Mystery Client could obtain a net, conditional on thinking the Mystery Client could obtain a net
- Percent of Community (Market and Home) Survey respondents that think the health center program is only for pregnant women, conditional on knowing something about the program
- Percent of Mystery Client Indirect respondents approached who agree to try to get a net (MCI respondents will be recruited through Community Interviews)

d. Other Leakage Measures

- Share of clients listed in net program distribution log who turn out to be ineligible (as determined by client survey) or that we cannot find
- Percent of stores monitored that are selling nets of the same type we sold in our nets program

- Price of bednets at local markets (should go down as leakage increases)

e. Leakage Motivation Measures

- Breakdown of reasons that the mystery client thought the healthworker offered them a net (i.e., sympathy, profit, misunderstanding program requirements, etc.), conditional on Mystery Client being offered a net
- Percent of mystery clients and indirect Mystery Clients told that nets were only for pregnant women

2. Extortion of Eligible Clients

a. Measures Derived from Endline ANC Client Surveys, Endline Net Log Client Surveys, and Outside-Clinic Client Surveys (clients who did not receive product previously only). For all three samples, we will compute:

- Percent from whom a monetary bribe was requested at the health facility
 - Overall average
 - Conditional on client visiting when products in stock
- Average bribe amount elicited at the health facility: Unconditional (no bribe requested treated as a bribe of 0 GHC) and conditional on bribe being requested
 - Overall average
 - Conditional on client visiting when products in stock
- Percent asked explicitly or implicitly for some form of payment at the health facility, not just monetary (Endline Client Surveys only)
 - Overall average
 - Conditional on client visiting when products in stock
- Percent asked to pay bribe by a store clerk (Voucher clinics only; Endline Client Surveys only)
 - Overall average
 - Conditional on client going to store to redeem
- Average bribe amount requested at the store (Endline Client Surveys only).
 - Unconditional (no bribe requested treated as a bribe of 0 GHC)
 - Conditional on bribe being requested
- Average total bribe amount requested, i.e., sum across the store and health facility (Voucher clinics) or health facility only (Direct clinics) from eligible clients

We will compare these measures across all three sources of data. This will enable us to test the extent to which health workers “forged” the records (ANC registers and Net Distribution Log) in order to hide leakage, and how that varied with the treatments (e.g., an audit threat could cause health workers not to request fewer bribes but to start writing fake names of clients in the Net Distribution Log instead of real names when they had requested a bribe from the client).

3. Share of eligible population not offered net/voucher

a. Measures, Derived from Endline Client Surveys and Outside-Clinic Client Surveys

- Percent not offered product at health facility, as calculated from endline client surveys (clients sampled from ANC register only) and outside-clinic client survey
- Percent not offered product, analyzed separately by the reasons (i.e., that the product was out of stock, that the health-worker seemed to only offer to clients who could pay bribes, that the client does not know the reason, etc.)

4. Share of eligible population covered by product

a. Measures, Derived from Endline Client Surveys and Outside-Clinic Client Surveys

- Percent that have net hanging in home
- Percent that obtained net
- Percent that pick up a net conditional on receiving a voucher from the health clinic (Vouchers only; compare with 100% for direct distribution)
- Percent that did not obtain net because a bribe was elicited, either at the health facility or at the store (endline client surveys only)

b. "Overcoverage" (from Endline Client Survey)

- Percent that obtained a second product at the health facility
- Percent offered a second product at the health facility, for a bribe
- Average bribe amount for second product offered at the health facility, conditional on bribe request

5. Effort: Absenteeism, Number of Patients Seen

a. Absenteeism Measures

- Absenteeism measure derived from Net Product Distribution Log and/or ANC registers (specifically, count number of days when the number of visits or nets logged by a given health worker are much lower than their average for the surrounding period)
- Whether clinic was open, and whether all health workers in charge of the program were absent, as measured by Mystery Client surveys

b. Number of Patients seen

- Total Number of Patients Seen per day
 - Estimated using the ANC registers
 - Estimated using the Net Product Distribution Log (adjusted downwards by rate of ineligible or fake patients, as calculated based on client surveys and finding rates)
- Time Spent per Patient Visit (measured through endline client surveys) (Will test the hypotheses that (a) profit-driven increases in effort to see more patients and extract more bribes could crowd out time spent per patient, or that (b) audit- or compensation-induced increases in effort may have positive spillovers on all effort aspects, not just net distribution)

6. Frequency of stockouts of subsidized product at the health facility

- Calculated from Net Program Delivery Logs: How many nets were in stock when the net program distribution officer came to restock the clinic (normalized for clinic volume)
- Calculated from Net Program Facility Call Logs: How many nets the health worker reported were still in stock when they called for re-stock (normalized for clinic volume)
- Calculated from client surveys and mystery client visits: Percent of calendar days on which a mystery client or endline survey respondent visited the clinic but on which subsidized products were not available

7. Other Outcomes

a. Market landscape

- Market price for nets: price surveys across shops in the areas surrounding the clinics in the study sample

b. Awareness

- Percent of market and home interviewees that are aware of the nets program

8. Notes on outcome variables

For all of the above outcome variables that do not have a specific date but rather cover several days/weeks and thus might combine some of the pre-audit with the post-audit period (e.g., the administrative data estimate of leakage might not correspond perfectly), we will need to estimate how to split those variables between the pre- and post-audit period.

IV. Correlates of Corruption and Treatment Effect Heterogeneity

A. Correlates of Corruption

We are also interested in understanding many other determinants of corruption. To examine how other characteristics correlate with observed levels of corruption, we will run regressions of the form:

$$y_c = \alpha + \beta X_c + \varepsilon_c \quad (4)$$

Where y_j will be the clinic-average measure of one of the corruption measures outlined above, and X_c will contain a selected vector of the characteristics described below.

Competitive Landscape Characteristics

- Number of shops carrying nets and the market net price (voucher clinics):
One key reason why shops would be able to charge bribes is if they have

market power over distribution of nets. We will have exogenously varied the level of competition with the 2-stores treatment, but there will be additional non-experimental variation across areas in the number of stores that carry nets. Thus, we will test whether bribery solicitation at the stores is lower in areas where there are a greater number of shops carrying nets. We will also test whether the prices of the competing nets has an effect on bribery; for example, the more low-priced net options there are that are substitutes for the free nets, the lower the bribe the shop-keeper will be able to charge. Finally, we can test for whether the competition induced by additional stores that are directly participating in the free nets program (i.e., the competition induced in the 2-stores treatment does) has a greater effect than the competition induced by naturally-occurring stores selling nets, as would be expected since the non-program stores would provide much less direct competition since their nets would be sold at much higher prices and/or lower quality. Although we would expect the primary effects to be at the shop level, the changes induced by greater competition at the shop level could also affect clinic-level behavior (e.g., perhaps the fact that the clients would be charged lower bribes would encourage altruistic health workers to exert more effort in offering vouchers to all clients), and so we will also check whether there is heterogeneity in clinic-level outcomes (e.g., undercoverage).

- Number of shops carrying nets and the market net price (direct distribution clinics): In addition to affecting the stores, competition can also directly affect the clinic's ability to charge bribes. Thus, we will test whether the number of shops carrying nets and the price charged for those nets in the market affects bribes charged by health workers, or whether the other nets sold in the area are of too dissimilar price/quality to pose real competition. The market landscape for nets will also affect the demand for nets, and so we will also test how these net market variables correlate with leakage.
- Density of nearby clinics with our nets program (or other free net distribution programs): If there are more sources for free nets in a given area, then there will be greater competition, potentially leading to lower ability to extract bribes.
- Density of nearby clinics: The density of nearby clinics can be used as a proxy for population density. In denser areas, the demand for program nets might be much higher (if there is crowd-out from non-program clinics into program clinics) and that might drive the price of program nets up.

Clinic Characteristics

- Clinic volume (one of main variables behind stratification): Larger clinics may have more people around so harder to leak nets and/or extract bribes, but also have larger stocks and potentially lower accountability so easier to

engage in corrupt behaviors; nurses could also be overworked in larger clinics so could have more under-coverage.

- Clinic geography (one of main variables behind stratification) and Clinic accessibility (e.g., Whether clinic accessible in rainy season): Staff in less accessible clinics could feel less monitored generally, leading to higher leakage and bribery. This lack of accessibility could either cause them to respond more to audits, since those treatments are associated with a larger (perceived) change in monitoring, or less, if they doubt the monitors will make it all the way to their clinics.

What's more, staff in clinics near the national border or near main roads may have greater opportunities to leak the bednets out in large numbers than staff further away.

- Clinic type (CHPS, health center, other): Different organizational structures in the different clinic types could produce different levels of accountability
- Number of midwives and number of clinic staff: In smaller clinics, midwives may feel that it is less likely that they will be reported for misbehavior, potentially leading to higher leakage; it could also be less easy to engage in clandestine behavior in smaller clinics, potentially leading to lower leakage.
- Characteristics of In-Charge, and relationship between In-Charge and the staff: Health workers might respect the protocol better in clinics where the In-Charge is perceived to have authority or where the In-Charge is less likely to collude with the health workers (for example if the In-Charge is from another area).
- Whether clinic is private: Private clinics have much lower levels of accountability in the system so could have higher leakage; may also respond more to accountability-enhancing treatments (e.g., audits) since those treatments are associated with a larger perceived change in monitoring.
- Whether clinic offer bednets for sale: If clinic or health workers get revenue from selling bednets at baseline, they might be less well disposed towards the free distribution program, which will cut their revenue. We are likely to see higher rates of stockouts or higher rates of non-compliance (in particular, refusal to implement the program) in those clinics.

Healthworker Characteristics

There are a variety of healthworker characteristics which we might expect to be correlated with corrupt behavior. We will measure the following characteristics through a survey of health workers, aggregate them each into clinic-level averages, and then create a vector of the characteristics to use for estimation in equation (4).

- Wealth, Income, Assets, and Land Ownership: Richer healthworkers might be less corrupt if they have a lower need for assets/honesty is a normal good. If concave relationship, they might also respond less to additional compensation.
- Other-regarding preferences and Risk Aversion: Health workers with stronger other-regarding preferences (i.e. health workers that care more about other individuals or the community) may be less likely to divert

resources or charge bribes. We will measure attitudes towards private and community health, attitudes towards work, and a psychological module (adapted from previous work by Robinson) to measure attitudes towards others, locus of control, and other factors. In addition, we will ask health workers to play several experimental games, to measure other-regarding preferences and risk aversion. In particular, to measure altruism, we will ask health workers to play a dictator game in which they will be asked to split a sum of money between themselves and another individual in the community. To measure risk aversion, we will ask individuals to choose between various gambles in which the size of the payoffs and the probability of winning are varied.

- Health workers from the area: Health workers of the same ethnicity as the target population, and health workers who are originally from the area, may be more altruistic to the target population.

Characteristics of the Ineligibles Asking for Nets/Vouchers

- We will collect data on the characteristics of the Mystery Clients we employ for the study, including their physical appearance as well as the “story” that they use when asking health workers for a net. We expect that health workers might be more willing to procure a net to those who claim to have a pregnant spouse, appear poorer, or are able to speak the local language.

B. Heterogeneity in Treatment Effects

We will also check for heterogeneity in the treatment effects based on all of the non-experimental determinants of corruption outlined in Section IV.A.

To evaluate heterogeneous effects, we will incorporate main effects for the baseline characteristics described above as well as interactions with treatment indicators into the regressions specified in section III.