# Outside Options and the Supply of Sex Work

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**Abstract**

Evidence shows that criminalizing participation in the market for sex often reduces welfare for sex workers. We use a field experiment in Cambodia to demonstrate that this trade-off between decreasing market size and worker welfare can be avoided. We randomize two interventions aimed at improving labor market conditions for sex workers. We find that incentivizing the outside work option increased outside option earnings by 20% and decreased sex work by 13%, resulting in a 13% increase in overall earnings. These price effects do not appear to be driven by income effects, as a separate unconditional cash transfer intervention did not decrease sex work. We use our results to estimate a cross-price labor supply elasticity of $-0.57$ between sex and non-sex work and an own-price labor supply elasticity of 0.45 for non-sex work. To generalize these findings to other outside options, we develop a model of labor choice; our calibration implies that, on the margin, the additional disutility from sex work is 16 times that of non-sex work. Overall, our paper shows that because labor supply for sex work is elastic, policies that leverage workers' responsiveness to prices can decrease the size of the market for sex without compromising worker welfare.

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# 1 Introduction

There are 52 million sex workers in a $180 billion global market for sex (International Union of Sex Workers, 2024; Scelles Foundation, 2011). Despite this large market size, sex work is near-globally banned. Such "protective legislation" (Tertilt et al., 2022) is in part motivated by concerns about worker agency and exploitation. These policies thus implicitly assume that sex work labor supply is inelastic. However, many workers do appear to choose sex work over other available jobs, not only for the pay premium but also for relative amenities, such as flexible pay and scheduling (Benoit et al., 2021; Maher et al., 2012). Likewise, sex-worker-led organizations advocate policy alternatives to bans that instead improve workers' labor market conditions.[1] This disconnect between policies and the market may explain why population data show that unconditional cash transfers have had little effect on the supply of sex (Richterman and Thirumurthy, 2021) and why studies show that bans *decrease* worker welfare (Cameron et al., 2021; Cunningham and Shah, 2018). For better policy to decrease the market size, it is crucial to understand the decision-making behind worker participation in the market. However, because the topic is sensitive and the market is illicit, data—and causal evidence—are scarce.

In this paper, we use a field experiment to study how improving labor market conditions for sex workers affects labor supply choices between sex and non-sex work, as well as subsequent health and financial status and decisions. We recruited a representative sample of sex workers who engage in both sex work and traditional service work, such as waitressing in bars or hostessing in karaoke venues, and randomized them to receive either service work incentives, an unconditional cash transfer, or neither. We find that the incentives increased participation in service work, measured via effort exerted in the incentives task as well as service work earnings and work hours. We find a corresponding decrease in sex work, implying sex work labor supply *is* elastic. In contrast, an unconditional cash transfer of similar value to the incentives had null effects on sex work. To assess how the supply of sex work may respond to a service work subsidy, we use our results to estimate labor supply elasticities for non-sex work and between sex and non-sex work. The contrasting signs on these estimates suggest that these jobs are substitutes.

To translate our findings into a measure of worker preferences beyond our setting, we interpret our findings through the lens of a labor supply model. In the model, a worker chooses levels of effort to devote to sex work versus non-sex work or leisure. We allow the two types of work to have different prices and disutilities; this is the worker's key trade-off. The model identifies mechanisms through which our interventions may affect sex work, and our data help us pin down magnitudes for each mechanism. Using estimates from our experiment, we calibrate the model to quantify the structural parameter for the

[1] See Bogg et al. (2020), Carrabine et al. (2020), Tremblay (2020), Flowers (2011), and Sanders et al. (2009).

relative disutility of sex work. Our estimate implies that the disutility of sex work not only outweighs that of non-sex work but is also marginally increasing in sex work. We then calculate a disutility-equivalence between sex and non-sex work which implies that, on the margin, sex work's relative disutility far exceeds that of non-sex work. Overall, our paper shows that since sex work labor supply *is* elastic and workers *do* substitute to alternatives, policies beyond bans can shrink the market *without* forgoing worker welfare.

To implement this cluster RCT, we partnered with an NGO and a survey firm to recruit 600 female workers across 300 service venues in Phnom Penh, Cambodia. The workers engage in both service work and sex work. While the service work is a traditional waitressing or hostessing job, it also provides opportunities to meet potential sex work clients in venues during work hours. Importantly, since sex work is illegal in the setting, transactions occur offsite, usually at hotels. Several aspects of this population make it ideal for our study. First, since most sex workers are non-street based, part time, and female, our sample is highly representative of sex workers in many geographies (International Union of Sex Workers, 2024; Scelles Foundation, 2011; Cunningham and Kendall, 2011). Second, these workers have mixed income sources from service work and sex work, and thus their labor choices embed our parameter of interest—the sex work versus non-sex work choice. Third, the intensive margin between sex work versus non-sex work is most relevant to these workers' daily labor decisions. Following extensive focus groups and pilots, we designed and followed a rigorous recruitment strategy to enroll a sample that was representative of the service sector and market for sex in this setting.

For our research design, we randomly assigned venues (clusters) employing participants to one of two intervention arms or to a control arm for one month. Participants working in venues assigned to the first intervention arm were offered a service work incentives program, which offered bonus weekly pay for conducting surveys with venue customers in venues. This incentives task mimics the duties and earnings structure of the average service work job (McKinsey and Company, 2020; Oxfam, 2019). Participants working in venues assigned to the second intervention arm were offered a fixed, unconditional weekly cash transfer. The transfer amount equaled expected average earnings, as per pilot data, in the incentives arm. The control arm had no experimental intervention.

We find that the incentives intervention decreased sex work relative to both the control and unconditional cash transfers. We find that the incentives first increased service work, exhibited by a 65% take-up[2]. Relative to the control, this take-up increased daily service work earnings by 20% and hours by 9%. We thus estimate a Marshallian own-price labor supply elasticity for service work of 0.45. We find that the incentives second decreased sex work relative to the control by 13%. We therefore estimate a Marshallian cross-price labor supply elasticity for sex work of –0.57. Our results are

[2]Take-up is defined as at least one incentives task submission.

surprising given the common assumption that sex work labor supply is inelastic, the complementarity of service and sex work in our setting, and the limited effects of other programs on sex work (Gong et al., 2019). Together, our elasticity estimates suggest that alternative work can substitute sex work.

We develop a model of labor choice to formalize intuition on mechanisms and estimate the relative disutility of sex work. The model features a worker who chooses levels of effort for sex work versus non-sex work or leisure. We allow for varied payoffs and disutilities for each work type, and the worker chooses her effort allocation according to this trade-off. We first use the model to identify the mechanism driving our experimental findings. According to the model, a negative sex work effect decomposes into three possible mechanisms: price effects via increasing the marginal utility of non-sex work relative to sex work, income effects via decreasing the marginal utility of consumption, and targeting effects via allocating resources to those more likely to decrease sex work, i.e., targeting based on take-up. Our data support the price mechanism as we do not find that unconditional cash transfers decreased sex work (income mechanism) nor were there greater decreases in sex work as incentives submissions increased (targeting mechanism). We second use the model to estimate the disutility of sex vs. non-sex work using daily labor data. The estimate is, on average, greater than 1, implying that workers prefer non-sex work and disutility increases in sex work. Accordingly, we find that, on the margin, the additional disutility from sex work is 16 times that of non-sex work.

Overall, our findings help to inform protective legislation (Tertilt et al., 2022; Doepke et al., 2012; Goldin, 1988) on the market for sex. Prevailing bans often trade off decreasing market size with worker and general welfare (Cameron et al., 2021; Cunningham and Shah, 2018; Bisschop et al., 2017). Our study shows that market size can be reduced without trading off worker welfare. Specifically, our experimental results show that decreasing the price premium for sex work encourages substitution to outside options. This is a promising result as our structural estimate shows that workers prefer to earn from their outside option. As policymakers test strategies to improve welfare for sex workers (Khmer Times, 2023), our findings provide timely evidence that scaling our incentives intervention[3] may decrease the market for sex without compromising worker welfare.

We contribute to the literature on sex work and transactional sex by testing and directly comparing two interventions and being among the first to meaningfully decrease sex work. We test two novel approaches—outside option incentives and unconditional cash transfers—to address the well-documented trade-off between money and risk faced by sex workers. Given that research has shown that (risky) sex work receives a sig-

[3]One practical scaling approach is implementation of a daily attendance bonuses for service workers.

nificant price premium,[4] it is perhaps not surprising that studies on health-risk-related conditional cash transfers have generally found minimal decreases in the overall supply of sex work (Gong et al., 2019; Kohler and Thornton, 2012; de Walque et al., 2012). In contrast, Jones and Gong (2021) tested a saving promotion program with women similar to those in our study. Their program decreased sex work as a response to economic shocks, suggesting that targeting the financial side of the money-risk trade-off may be promising. Motivated by this finding, our experiment directly tests which of multiple financial interventions has the greatest effect on sex work labor supply. We find that an incentive program decreases the supply of sex, while unconditional cash transfers do not.

We also provide the first labor supply estimates on sex work versus non-sex work, thus making two contributions to the labor literature. First, our non-sex work elasticity estimate is among the first experimental estimates for women and, further, women in developing countries. Second, we are among the first to estimate elasticities for workers choosing between high-risk versus traditional work. Existing literature exploits observational data to estimate elasticities for women in traditional work in Western countries[5] and quasi-random and experimental variation in contexts with mainly male workforces.[6] In addition, while studies also use observational data to estimate own-price elasticities for other high-risk work (Stafford, 2015; Nguyen and Leung, 2013), low-skilled workers are disproportionately likely to both combine multiple jobs and engage in risky work (U.S. Census Bureau, 2024); our cross-price elasticity estimates for sex versus traditional work thus fill a gap in the literature on the labor choices of this large worker population.

Finally, we advance the development literature by being the first to study how female labor force participation interacts with the large and risky market for sex. We extend and complement existing research by providing experimental evidence on the labor choices of the large group of women who engage in *both* formal and informal labor. For formal labor, female labor force entry in developing countries is typically studied in manufacturing and business ownership (see Heath and Jayachandran (2016) for a review). In contrast, the service sector remains understudied despite its predominantly female labor force and increasingly common role in development strategies (World Bank, 2021). For informal labor, the closest papers study women who engage in sex work full time in a street- or brothel-based setting (Ghosal et al., 2022; Gertler et al., 2005). However, most sex workers are female, part time, and non-street based (International Union of Sex Workers,

[4]The literature has documented that suppliers of sex face a striking and unique set of risks including criminal penalties, health impacts, and death (UNAIDS, 2023; Manian, 2021; Deering et al., 2014). Studies have also shown that there is a substantial price premium for risk (Jakubowski et al., 2016; Elmes et al., 2014; Gertler et al., 2005) and women's engagement in sex work and transactional sex is partly driven by economic needs (Gichane et al., 2022; Robinson and Yeh, 2011).

[5]Bargain and Peichl (2016) and McClelland and Mok (2012) provide reviews.

[6]This includes rideshare and taxi drivers (Thakral and Tô, 2021; Angrist et al., 2021; Chou, 2002; Camerer et al., 1997), bike messengers (Fehr and Goette, 2007), and baseball stadium vendors (Oettinger, 1999).

2024; Scelles Foundation, 2011; Cunningham and Kendall, 2011). Thus, we study the large portion of women on the margins of the labor market and market for sex.

The rest of the paper is as organized follows. Section 2 describes our setting, Section 3 describes the research design, and Section 4 details our data and empirical strategies. Section 5 presents our experiment results. Section 6 develops our model of labor choice. Section 7 presents our results on mechanisms, labor supply elasticities, and structural estimation. Section 8 concludes.

# 2 Setting

In this section, we characterize the supply and demand sides of the market for sex in this setting using data from our sample,[7] followed by a description of the nature of the sex work transactions. Together, these three aspects make it an ideal setting for this study.

## 2.1 Supply of Sex

The supply side of the market for sex in this setting is composed of service-venue-based female sex workers in Phnom Penh, Cambodia. Sex work is extremely common in this setting; we estimate that 50% of service venue workers engage in sex work.[8] The top panel of Table 2 summarizes worker characteristics. Workers have a mean age of 28, 21% are married, most are primary school graduates, 32% are from Phnom Penh, the average household size is 4, and 65% are in debt at baseline. In terms of sexual health, 2% are on HIV prevention medicine, 16% use hormonal contraceptives, and 97% used a condom with their most recent sex work clients. These statistics are all broadly consistent with country-wide population statistics on service-venue-based female sex workers, except our study's average age is higher. Country-wide statistics report a lower average age than that of our sample due to the national survey's minimum recruitment age of 15 (Phalkun, 2022).[9]

Workers' labor supply characteristics are of particular interest in this study. First, in terms of time devoted to each labor option, Table 2 shows that participants report working an average of 9–10 hours per day in their service venue in the prior work day and 3 hours in sex work in the prior two weeks. In service venues, workers typically have nighttime shifts from around 5:00pm to around 2:00am to 3:00am, and 95% of this

[7] A discussion of our data's representativeness of the market for sex in this setting is available in Section 3, which details how our field activities, like recruitment, were geared toward achieving this goal.

[8] This estimate is based on our field team's reported success rate in identifying service venue workers who meet the study's sex work participation eligibility criteria, which is detailed in Section 3.2.

[9] Our study was approved by both Brown University's IRB and the local IRB to recruit workers who were aged 18 or older.

sample reported working shifts every day. For sex work, workers reported during focus group discussions and baseline surveys that the market benchmark is to spend 1 hour with each sex work client.

Second, in terms of earnings from each labor option, participants earn 800 USD per month between daily nighttime service shifts and occasional transactions with sex work clients. Figure 1 is instructive in understanding workers' earnings structure. The left panel shows that just less than 50% of estimated monthly earnings comes from service work. Workers are compensated via a combination of variable and fixed earnings in their service venue. 36% of service earnings comes from fixed monthly wages (base salary), and 64% of service earnings comes from variable earnings, e.g., communal venue and individual venue customer tips. The other 50% of estimated monthly earnings comes from sex work.[10] At baseline, workers received a proposal for sex work on 36% of nights, negotiated with potential sex work clients for an average of 12 minutes on transaction terms (e.g., price and condom use), and eventually accepted only 60% of proposals for sex work.

Third, in terms of worker decision-making between labor options, since workers may face a service versus sex work choice on some work nights, the work night is the closest approximation to the "unit" of these workers' margin of labor supply decision-making.[11] Thus, the right panel of Figure 1 compares the difference in relative nightly earnings by source, and it suggests that sex work pays around four times more than service work per night of work for workers in our sample. We note that beyond the service venue base salary—which is only 17% of total estimated monthly earnings—these workers do not technically earn any hourly, daily, or monthly wages. Thus, we caution that this panel is purely suggestive to provide a means to compare the difference in relative earnings by source.

Fourth, in terms of prior work experience, 47% of our sample previously worked in a similar sex-work-adjacent service venue, 16% previously worked in manufacturing, and 9% previously worked in a non-sex-work-adjacent service venue (e.g., a cafe).[12] For 27% of our sample, the service venue in which they were recruited for this study was their first employer.

[10]This estimate is calculated by multiplying the baseline reported average price of sex work, 67 USD, by the baseline reported average number of sex work clients per month, 6.

[11]For example, during focus groups, we learned that the number of service venue customers at 10:00pm may impact a workers' service versus sex work effort decisions at 1:00am. A busier (emptier) venue at 10:00pm would make accepting a sex work proposal later during the night less (more) likely.

[12]Manufacturing is commonly suggested as an outside option for these workers. While shifting from manufacturing to the service sector is common, the reverse is rare. The Cambodian minimum wage in manufacturing is 200 USD (2022), which is 25% of average wages at baseline for study participants.

## 2.2 Demand for Sex

The demand side of the market for sex is composed of predominantly male sex work clients. The bottom panel of Table 2 displays summary statistics describing the study participants' sex work clients at baseline. As we were unable to survey male sex work clients, all of these statistics come from our participant surveys with female workers. However, they are broadly consistent with existing descriptive data on typical sex work clients across geographies (International Union of Sex Workers, 2024). As shown in the bottom panel, participants perceived that accepted sex work clients were on average 39 years old, 88% were thought to be of average income level (the choices offered were as follows: low, average, high), and 21% were foreign. It is worth noting that only 1% of accepted sex work clients were perceived as violent, rude, or aggressive, while 33% of rejected clients were perceived as such. This difference suggests that sex workers in this setting have sufficient agency to reject sex work clients who they perceive as presenting a risk of violence.

Sex work clients in this setting typically initiate interactions with workers in service venues during the workers' shifts, and thus sex work clients are a subset of service venue customers. However, not all service venue customers become sex work clients. Only 13% of customers served by participants at baseline are estimated to initiate sex work proposals. Thus, these service venues do not appear to depend on this subset of service venue customers for their profits.[13]

## 2.3 Sex Work Transactions

The potential for workers to choose to earn from sex work versus service work makes this setting especially well-suited for our study. Workers trade off earnings both directly and indirectly with respect to time, i.e., direct substitution vs. indirect substitution. At baseline, 65% of sex work transactions started after workers completed their service work shifts, i.e., indirect substitution. The other 35% of transactions involved workers leaving their shift early to depart for sex work, i.e., direct substitution. In focus group discussions, participants reported that direct substitution is most likely to occur toward the end of workers' shifts, between 12:00am and 3:00am if and when venue customer flow is low.

This setting is also attractive for conducting our study because institutional factors ensure that service and sex work cannot occur simultaneously. Sex work does not take place at venues because it is criminalized in Cambodia, and thus transactions must occur in locations that are discreet from law enforcement. In focus group discussions, participants reported that a worker and her accepted sex work client will typically travel

[13]The venue surveys conducted during focus grouping and at endline indicate that service venue profits are primarily earned through selling drinks and food.

to a nearby hotel for the transaction to take place. This separation of sex and service work was a key motivation for our incentives intervention design, detailed in Section 3.

The partnerships between sex workers and their clients in this setting are minimal on a scale of relationship intensity and length. During focus group discussions, participants reported that sex work clients generally did not repeat, and if so, only two to three times. In general, sex work transactions can be portrayed as lying on a spectrum. Transactions with non-repeating or minimally repeating commercial clients are on one end, while transactions with clients where long-term sexual and economic partnership are established are on the other. We categorize the partnerships of focus in this study as closer to the former end of the spectrum. In contrast, the latter have been the subject of various related studies (Thirumurthy et al., 2021; Dupas, 2011).

Finally, reported condom use rates are high, with 97% of participants reporting using condoms with their most recent sex work client at baseline. This statistic is consistent with country-wide population statistics on service-venue-based female sex workers (Phalkun, 2022) and statistics in part-time sex work more broadly (International Union of Sex Workers, 2024). These data thus suggest there is scope for workers in this setting to negotiate condom use with commercial sex work clients.

# 3 Experiment

## 3.1 Overview

We ran a one-month cluster RCT in Phnom Penh, Cambodia. To implement our experiment, we collaborated with Cambodia's key policy partner in this topic area to recruit a sample of participants who would be representative of the global population of sex workers. The policy partner is an NGO that provides programming for vulnerable female workers in the country.[14] One of its two main program teams provides sexual and reproductive health services for female sex workers, with whom we collaborated for this study. To augment the field team's capacity, we also added field and management staff from a local survey firm. The full team then participated in intensive training and sensitization sessions before starting fieldwork.

Experiment implementation followed two intensive preparatory phases. In the first phase, we conducted extensive focus groups with female service workers employed in service venues in November 2022 and June 2023. In the second phase, we conducted intervention pilots in service venues in September and October 2023. We then implemented the experiment from October 2023 to January 2024.

[14]Prior to this study, we had established a working relationship with the policy partner. Our pre-existing relationship was established with one of this NGO's two main program teams, which offers programming, like financial literacy training, to female garment factory workers. That team thus helped us recruit female garment factory workers for our other studies.

## 3.2 Recruitment

We ensured that we recruited from a set of service venues that were representative of service venues across the city. Before the start of fieldwork, we carefully mapped out a sparse set of candidate service venues to recruit from,[15] and then shared this map with the field team as valid service venues to serve as a starting point for recruitment. We set the following eligibility criteria for service venues: the service venue must pay a base wage to its workers, and the service venue supervisor must have been briefed on and thereafter permitted their workers to participate in any of the three study arms, without yet knowing their venue's assignment. The base wage criteria served as our proxy that the establishment is a service venue rather than a brothel. This proxy was based on our partner NGO's recruitment criteria for their own service provision as their programming differs if a given participant works at a service venue versus a brothel.[16] The supervisor permission criteria helped mitigate concerns that recruited service venues were selected.[17] As a result, we recruited from around 300 service venues.

We also ensured that we recruited a set of workers who were representative of indoor, part-time female sex workers (see Table 1 for statistics). Workers were recruited at their service venues in the few hours before the start of their daily shift hours.[18] We set the following eligibility criteria for these workers: the worker must be at least 18 years old and must have been compensated for sex at least once in the month before enrollment. The age criteria was set to meet the requirements of our IRB approval. The sex work client criteria served as a proxy for participating in sex work and is consistent with standard proxies used in the literature (Thirumurthy et al., 2021). As a result, we recruited around 600 service workers, with 2 workers recruited per venue.

## 3.3 Intervention Randomization and Data Collection

We randomized the interventions at the service venue level. We chose the venue, rather than the individual, as the unit of randomization to avoid potential intervention spillover effects between participants recruited within a single venue. We randomly assigned venues employing study participants to one of two intervention arms or a control arm. Based on power calculations, we recruited a maximum of 2 workers per venue.

We randomly assigned venues employing study participants to one of two intervention

[15] We do not include a map with locations to protect the privacy of participants.

[16] Venues that behave more like brothels typically do not pay any base wages. Thus, they would offer limited intensive margin for workers to trade off between service and sex work, making them a poor fit for recruitment for our study.

[17] For example, if some venue supervisors only agreed to recruiting control arm participants from their venue and we then assigned the venue to the control arm, this may have created interpretation problems during the analysis stage.

[18] Workers normally spend one to two hours at the service venue preparing for their shift before it starts. This time includes activities like doing their hair and makeup and/or attending all-staff meetings.

arms or a control arm. We invited participants working in venues assigned to the first intervention arm to participate in a service work incentives program, which offered them an opportunity to earn bonus weekly service work pay for submissions of a task designed to mimic their daily service work duties and earnings structure. We invited participants working in venues assigned to the second intervention arm to receive a weekly unconditional cash transfer, equivalent to the expected average earnings, as per pilot data, of those in the incentives arm. Finally, we did not offer control group participants a service work incentives task nor pay them an unconditional cash transfer. Therefore, the control group was a pure control.

We invited all participants across the three study arms to complete three participant surveys—baseline, midline, and endline—administered by the field team. Upon completion of each survey, participants were compensated with a small survey incentive.

## 3.4 Intervention Arms

### 3.4.1 Incentives Arm

We invited participants working in venues that we randomly assigned to the intervention arm to participate in a service work incentives program. This program offered an opportunity to earn bonus weekly service work pay for completion of a piece-rate incentives task: conducting customer surveys. Specifically, we invited participants to administer a short survey with service venue customers that asked questions eliciting the customer's rating of the venue's service and food/drink selection. Participants filled out the customer surveys on their own devices via a survey link we generated using Brown University's Qualtrics software, which we leveraged to collect various metadata to ensure accurate records for determining submission eligibility. We paid participants an incentive for each valid submission, distributing the payments weekly based on all valid customer survey submissions from the preceding week. Our piloting provided evidence that this intervention was feasible, acceptable, and safe in this worker population.

Our primary design goal was to simulate increasing the relative returns to service versus sex work and, more broadly, to emulate an increase in service sector effort. To this end, we designed this task to have several key features. First, the task is analogous to female service workers' real work tasks. "Bar promotion" activities like the customer survey task are precisely the types of techniques that alcoholic beverage companies hire female service workers to perform for market research (McKinsey and Company, 2020). They are also a typical work duty described by female service workers in the Cambodian setting (Oxfam, 2019). Second, each customer survey required a customer interaction. Hence, our task design was able to mimic the workers' existing earnings structure. Our baseline data show that the primary margin of daily income generation for workers are customer-based tips: such variable earnings were 31% of total estimated earnings,

which was 63% of total service work earnings. Third, we designed data collection to carefully collect rich metadata on incentives task participation, such as submission dates, timestamps, and geographic coordinates. We then determined submission validity using these metadata. Thus, our design minimized opportunity for workers to be compensated for false submissions.

We familiarized participants in the incentives arm on the incentives task and submission process during enrollment. In addition, we instructed them that incentives task submissions would be considered valid if the following criteria were met: customers responded to all the questions, customers provided their signature, the submission's latitude and longitude data matched that of the venue, and the submission's timestamp was during the worker's reported shift time.[19] Moreover, we did not impose a submission limit on participants, with the goal of analyzing whether heterogeneity in incentives task take-up was correlated with outcome responsiveness, i.e., magnitude of decreases in sex work. Finally, we priced the incentives to reflect the sex work versus service work direct substitution window as described to us during focus groups and piloting activities. Therefore, we instructed participants that submissions before 12:00am would be paid 1.50 USD per submission and those after 12:00am would be paid 3 USD per submission.

### 3.4.2 Unconditional Cash Transfers Arm

We invited participants working in service venues that we randomly assigned to the unconditional cash transfers arm a fixed, unconditional cash transfer that was equivalent to expected average earnings, based on pilot data, of those in the incentives arm. As with the earnings in the incentives arm, the transfer was distributed weekly.

We included this particular intervention arm in our experimental design for two reasons. First, we are motivated to quantify if there are income effects on sex work for two reasons. The first reason is that the estimate would help us answer our research broader question by identifying if improving labor market opportunities for sex worker decreases sex work simply due to income effects. This would help us identify or rule out classes of models, like a Stone–Geary utility function, for workers in this setting and thus speak to the design of a scaled policy. The second reason is that standard labor supply models imply that our two intervention arms may have different effects if substitution effects are non-zero, as any relative increase in returns to a labor option decompose into an income and/or substitution effect. As long as earnings in the incentive arm were similar to the amount paid out in the unconditional cash transfers arm, we could use our data to isolate the income effect attributable to the incentives program in the incentives

[19]Shift start and end times were collected as part of the baseline survey. Baseline surveys were conducted after enrollment and before revealing study arm assignment to avoid misreporting of shift duration. Average reported shift start and end times by participants in this study arm are similar to those reported in other arms, offering little evidence of misreporting.

arm. Combined with the results of the incentives arm, the unconditional cash transfers arm therefore allows us to assess if an increase in the relative returns to service work impacts outcomes via the income versus substitution effect.

Second, the unconditional cash transfer in the unconditional cash transfers arm was homogeneously offered to all of its participants, meaning take-up was expected to be 100% by default. In contrast, we designed the incentives intervention such that participation in the incentives task was based on voluntary effort investment to earn income. Thus, the incentives arm has the potential to act as a self-targeting mechanism. Participation data could help identify the ex ante characteristics of workers who are more likely to potentially substitute to service work from sex work, i.e., "marginal substituters." Identifying this type of heterogeneity may be important from a policy perspective when targeting a scaled intervention.

# 4 Data and Empirical Strategy

## 4.1 Data

We use two key datasets in our analysis: metadata on incentives task submissions and worker surveys. For the incentives task data, we design a system to collect rich and objective metadata on incentives task participation via Brown Qualtrics. These metadata include fields such as dates, timestamps, and geographic coordinates for each submission. For the worker surveys, the field team conducted three surveys—baseline, midline, and endline—each two weeks apart, with all participants enrolled in this study. The field team also used Brown Qualtrics for these surveys.

In worker surveys, we asked about topics including labor supply, financial activity and consumption, and health. For labor supply, we asked about earnings from and time supplied to each type of work, and for sex work specifically, the number of clients,[20] client characteristics, and condom use. For financial activity and consumption, we asked about debt status, amount, and the involved party as well as consumption in the following categories: food, rent, medicine and doctor visits, medicine and doctor visits specific to sexual and reproductive health, children's medicine and doctor visits, children's school fees, clothes/handbags/etc., alcohol, makeup and hair services, debt repayment, and remittances to parents/village. For health, we asked about self-perceived health status, health facility usage, and experience of symptoms of common STIs.

We also conducted venue supervisor surveys at endline for a random sample of venues assigned to the incentives arm to address concerns about potential adverse impacts on participants' employment status and conditions. We asked about any workforce changes

[20]In the analyses that follow, we use the market's benchmark of one sex work client per one sex work hour for conversion, as reported by participants during focus grouping and baseline surveying.

like hiring, firing, and quitting as well as general thoughts on the incentives task. Venue supervisors did not report any cases of adverse impacts on participants' employment status or conditions. As an additional check, we also asked the same set of questions to participants in the incentives arm in the midline and endline surveys, and the participant survey responses were consistent with the venue supervisor responses.

## 4.2 Balance

Table 3 and Appendix Table A1 show that the demographics and outcomes variables, respectively, were balanced at baseline. Demographics were generally balanced across study arms at baseline except for some quantitative imbalances in age that were small. These imbalances did not impact the composition of ages in the study arms qualitatively. Outcomes were also generally balanced across study arms at baseline, except for perceived violence risk when comparing the unconditional cash transfers and control arms and debt status when comparing the incentives and cash transfer arms. Given that perceived violence risk was such a rare outcome, and other variables capturing perceptions of risk were not similarly imbalanced, we do not interpret this imbalance to be a threat to randomization. In addition, cash transfer arm participants appear to be more likely to be in debt than participants in the other two arms. We are reassured by the finding that debt status is also not imbalanced when comparing the cash transfer and control arm participants and that there are no additional imbalances in total income and total consumption.

## 4.3 Empirical Strategy

We first assess the impacts of exposure to each of our interventions. To estimate reduced-form effects, our empirical strategy relies on the cluster randomized design of this study. We use an OLS regression to evaluate the effect of our two interventions on participants' labor supply, financial, and health-related decision-making:

$$y_{ict} = \alpha + \beta Intervention_c + \gamma X_i + \pi_t + \epsilon_{ict}, \tag{1}$$

where $y_{ict}$ captures outcomes for participant $i$ in cluster $c$ at survey round $t$. $X_i$ is a vector of baseline participant characteristics such as age, marital status, education, and service work wage. $\pi_t$ are follow-up survey round fixed effects. We clustered standard errors at the venue level. Thus, $\beta$ is the intent-to-treat (ITT) effect of a given intervention on outcomes.

We next assess the magnitude of labor supply impacts for a given change in wage. To do so, we use data from the incentives and control arm participants to translate reduced-form effects on labor supply decisions into labor supply elasticities. Specifically,

we estimate the following two-stage least squares (2SLS) regression. The first stage is

$$WageServ_{ict} = \kappa + \delta Intervention_c + \nu X_i + \gamma_t + \upsilon_{ict}, \tag{2}$$

and the second stage is

$$y_{ict} = \theta + \phi \widehat{WageServ}_{ict} + \sigma X_i + \rho_t + \mu_{ict}, \tag{3}$$

where $y_{ict}$ is *ln Service Hours* or *ln Sex Work Hours* for participant $i$ in cluster $c$ at survey round $t$. $WageServ_{ict}$ is ln of daily wage from service work for participant $i$ in cluster $c$ at survey round $t$, and $X_i$ is a vector of baseline participant characteristics. $\gamma_t$ and $\rho_t$ are follow-up survey round fixed effects, and standard errors are clustered at the venue level.

We require four assumptions to use $Intervention_c$ as an instrument. First, we require ignorability of $Intervention_c$, i.e., that it is (conditionally) randomized with respect to the outcomes and $WageServ_{ict}$. This condition holds due to the randomized experimental design and is evidenced by Table 3 and Appendix Table A1. Second, we require that $Cov(WageServ_{ict}, Intervention_c) \neq 0$. We provide evidence in favor of this condition in Section 7. Third, we require monotonicity, i.e., that there were no defiers. We do not find evidence that $WageServ_{ict}$ increased (decreased) for control (incentives) arm participants beyond expected temporary fluctuations given the varible nature of service work wage in this setting; more generally, we do not find evidence of any impact to employment conditions or status across arms that may have more meaningful impacts on $WageServ_{ict}$.

Fourth, we require that $Cov(Intervention_c, \mu_{ict}) = 0$. There are several ways the exclusion restriction may be violated in this setting, i.e., that intervention status impacts outcomes, such as sex work hours, directly. For example, we may be concerned that our interventions may impact participants' amount and composition of sex work proposals. We provide evidence against this concern in Section 7. As another example, we may also be concerned that our interventions impact participants' decision-making in determining which sex work proposal to accept. While we did not collect data to provide evidence against this concern, we argue that this is unlikely given the lack of overlap between the type of information conveyed in the interventions[21] versus types of client characteristics which results in Section 7 show appear to impact participants' sex work decision-making. We thus interpret the 2SLS estimates of $\phi$ as the own- or cross-price elasticity for service

[21] In the incentives arm,the incentives task asked about venue customers' opinions on innocuous topics like the service venue's food and drink selection. We also show in Section 7 that participation in the incentives task led to a short average interaction time between workers and venue customers, implying there was limited to for the incentive task itself to convey a great deal of information on the venue customer. Meanwhile, we do not anticipate the unconditional cash transfers directly impacted participants' interactions with sex work clients.

work and sex work, respectively.

# 5 Experiment Results

## 5.1 Take-Up

### 5.1.1 Incentives Arm

Overall, we find that the incentives intervention was able to incentivize service work for 65% of workers in the incentives intervention arm. Figure 2 displays the distribution of valid incentives task submissions per worker, showing that 65% of participants in the incentives arm took up the offered service work incentives.[22] The figure also shows that there were 71 mean valid submissions per worker across all participants in the incentives arm and an average reported time spent of three minutes per submission. Therefore, we estimate that a given participant in the incentives arm submitted roughly 2.5 submissions per work day, which would translate to one hour spent on the incentives task per work week. Appendix Table A2 provides an analysis of how baseline characteristics differ by compliers, never takers, and always takers. Compliers are more likely to be unmarried with larger households and have less service sector tenure and use of contraceptives.

The 65% participation rate is validated by several robustness checks. First, it is robust to dropping participants who could not take up the incentives for exogenous reasons. Appendix Table A3 tabulates reported reasons for non-participation in the incentives task for participants across 89 participant-survey rounds. We categorize three of these reasons as exogenous to the participant: customer denials, supervisor denials, and low customer flow as they mechanically prevent participation. Only three participants consistently reported these exogenous reasons for not participating in the incentives task. Second, we also find in Appendix Table A3 that non-participation is not concentrated in specific clusters, implying that our take-up rate reflects participants' versus venues' choices. Together, these checks suggest that the observed take-up rate approximates what we may expect in a scaled intervention.

In Figure 2, two observations about the distribution's shape demonstrate worker heterogeneity. First, the leftmost bar shows that 35% of workers had 0 valid submissions, corresponding to the 65% participation rate noted above. This non-participation rate provides evidence against a model in which workers try to maximize income across all income-generating opportunities, such as a Stone-Geary preferences. Ex ante, this model may have been considered appropriate given that such populations typically face extremely high marginal utility of consumption. Further, the incomplete take-up provides evidence that at least some workers face non-zero disutility from our seemingly

[22] We define take-up as submitting at least one incentives task.

low-effort incentives task, which provides a basis for us to anticipate variation in trading off between work types across workers. Second, in contrast, the bars on the right half of the figure display a long tail from 350 to 600 valid submissions, indicating that there were some workers for whom this intervention was highly valuable. While very few in number, such outlier submission counts are consistent with baseline venue-level customer flow data. In sum, these observations highlight meaningful heterogeneity across workers.

Figure 3 shows the cumulative distribution of mean weekly incentives earnings per worker. Since incentives were compensated according to fixed prices, we should expect the distribution of earnings to have a similar shape as submissions. Indeed, as in Figure 2, we observe a sharp departure from the y-axis at 35% and a long tail for values in the right half of the figure. Additionally, around 70% of the values of mean weekly earnings per worker are below the mean across all workers, at $19.66. This means that the earnings value (blue line) is similar to the fixed weekly amount paid to participants in the unconditional cash transfers arm of $15 (green line). This similarity facilitates our analysis in Section 7.1 that decomposes the intervention's price effects into income and substitution effects.

Figure 4 displays the distribution of submissions throughout the day. In line with most of the participants having nighttime shifts, we observe that most submissions occurred from 4:00pm to 4:00am. The daytime submissions reflect the small portion of participants who had daytime shifts. We also observe a small bump in submissions at 12:00am, which may demonstrate a response to our varied pricing scheme that doubled compensation for valid submissions after 12:00am.

Since we designed the intervention with the intention of increasing service work effort, we hypothesize that increased effort may spill over to increases in workers' service work hours, variable earnings (e.g., tips, drink incentives), and venue customers served. While such spillovers are not necessary to demonstrate participation in the incentives task, Table 4 provides accompanying evidence that the incentives-induced effort increase led to a 0.67-hour (40-minute) increase in service work hours in the prior work day, a 0.50 USD increase in earnings from communal venue tips, and an increase of almost seven venue customers served in the last two weeks. These regressions provide corroborating evidence that service work effort indeed increased for participants in the incentives arm.

### 5.1.2 Unconditional Cash Transfers Arm

In the unconditional cash transfers arm, there was 100% transfer take-up, which led to decreases in service work effort but no impact on sex work. The bottom panel of Table 4 displays the transfer's impact on service work effort. In contrast to the effects discussed for the incentives arm, comparing the unconditional cash transfer participants to control participants shows that the transfer did not impact service work effort.

## 5.2 Sex Work

### 5.2.1 Incentives Arm

We find that the incentives arm led to a decrease in sex work. Table 5 presents regression results following the specification in equation 1, comparing the reported sex work hours of incentives versus control arm participants during follow-up surveys. Column 1 shows that participants in the incentives arm reported 0.38 fewer sex work hours over the prior two weeks. Given a baseline reported mean of 3 sex work hours over the prior two weeks across all participants, this implies an approximate 13% decrease. Columns 2 and 3 show that these results look similar with the inclusion of baseline sex work hours and demographic controls, respectively.

This result is surprising for several reasons. First, this study tests the common assumption that sex work labor supply is inelastic, but our finding provides evidence to the contrary. Second, our result is consistent with what economic theory would predict for two jobs that are behavioral substitutes. However, while workers in this setting *can* substitute their effort between each type of work, it is ex ante not obvious that they *do* substitute. In fact, given that most workers' sex work clients initiate interactions with workers in service venues as customers, there is evidence that the two types of work are complementary, i.e., in production. Third, given that individuals involved in sex work typically face extremely high marginal utility of consumption, supply-side interventions that aim to disincentivize sex work have found mixed results: Jones and Gong (2021) estimate decreases in sex work as a shock-coping behavior in response to a savings incentive, while Gong et al. (2019) find limited decreases in response to a conditional cash transfer. Thus, it is surprising that offering incentives for non-sex work, *unconditional* to levels of sex work, would lead to a decrease in it.

We run several robustness checks to validate this finding of a decrease in sex work. First, we validate that this decrease cannot be explained by a mechanical decrease in sex work proposals. The incentives task intervened in the interactions between workers and their potential sex work clients, which may raise the concern that the task itself may impact the reported number of sex work hours by altering the number of sex work client proposals. The direction of this impact is ex ante ambiguous. On the one hand, if venue customers find the task to be a disturbance to their venue experience, proposals may decrease. On the other hand, if workers interact with more venue customers (who later can become sex work clients) as a result of the task, proposals may increase.

The first panel of Appendix Table A5 alleviates this concern. It shows that, in our follow-up data, participants in the incentives arm reported 0.55 more sex work client proposals relative to the control arm. This means that despite an increase in sex work client proposals, participants in the incentives arm still decreased sex work effort via decreases in *accepted* sex work clients. This increase in proposals provides evidence against

a concern that a decrease in proposals mechanically decreases the reported number of sex work hours. However, the increase in proposals may also warrant caution against drawing a strong conclusion that the incentives intervention is unambiguously beneficial to workers, as it does appear to increase their pool of potential sex work clients.

Second, we validate that our result is robust to concerns about social desirability, especially given the sensitivity of surveying on topics like sexual choices and health. While our pure control group mitigates these concerns to some extent, differential social desirability between the incentives and control arms participants may still be a concern. This would arise if responses from participants in the incentives arm are disproportionately more likely to suffer from such bias. We address these concerns by ensuring that our analysis is robust to dropping all observations from the 3% of participants who report at endline that their perception of the study's goal was to decrease sex work. Appendix Table A4 tabulates reported perceptions of the study's goal across participants in the incentives and control arms. The results, presented in the second panel of Appendix Table A5, show that the decrease in sex work observed in the incentives arms remains at a similar magnitude and significance level. This robustness check offers assurance that social desirability is not driving our results.

Third, we validate that our result is robust to using the number of sex work clients as a proxy for sex work hours. Participants in the focus group reported that one hour per sex work client was the benchmark in the market. However, there may be concerns that workers spent more or less time with clients, thereby altering realized hourly sex work wages. To the extent that the bias is similar across participants, our proxy would simply underestimate (overestimate) the ITT results if the true hours per client were larger than one (smaller than one), and elasticity estimates would remain unchanged. A more concerning bias may occur if those reporting lower (higher) prices for sex were working fewer (more) hours. This would imply that assuming 1:1 client to hours is a poor proxy for assessing impacts on worker sex work effort, as we may be underestimating (overestimating) effort. The third panel of Appendix Table A5 addresses this concern, showing that our results are robust to winsorizing the 1st and 99th percentile of outcome data. This provides evidence against that measurement error for sex work hours is not driving our results.

Finally, we validate that participants' primary response to sex work was to the level of sex work rather than the *type* of sex work. In addition to changes in labor supply, we may expect the incentives intervention to change sex work on the intensive margin; for example, it may decrease the amount of risk that workers are willing to accept with their sex work clients, particularly regarding HIV, other STIs, or the risk of violence. Appendix Table A12 presents ITT regression results comparing incentives and control arm participants' reported perceptions of the risk profiles of their most recent accepted sex work client (Columns 1–3) and rejected sex work client (Columns 4–6) during follow-

up surveys. In general, we find no impacts on sex work client risk. However, Columns 3 and 4 of Appendix Table A13 show that participants in the incentives arm were 3% more likely to report that they perceived their most recent rejected sex work client to be poor and 4% less likely to report that they perceived them as having an average income level.[23] These point estimates are similar in magnitude, suggesting that participants leveraged the incentives intervention to substitute away from poorer-seeming clients. This result may have implications for what we may expect in general equilibrium in response to a potentially scaled intervention.

### 5.2.2 Unconditional Cash Transfers Arm

We do not find that the unconditional cash transfers had a statistically significant impact on sex work labor supply, as shown in Table 6. These regressions follow the specification in equation 1 and compares the reported sex work hours of unconditional cash transfer versus control arm participants during follow-up surveys. Column 1 shows that participants in the unconditional cash transfers arm reported a statistically insignificant 0.03 fewer sex work hours over the prior two weeks. Columns 2 and 3 show that results are similar with the inclusion of baseline sex work hours and baseline characteristic controls, respectively.

## 5.3 Financial and Health Outcomes

For the incentives arm, we conduct a brief back-of-the-envelope analysis to assess the projected health impact were the incentives intervention to be scaled. We project modest decreases in risk to sexual health (see Appendix Section 11.3.1) and find minimal health impacts during the intervention. Appendix Table A10 shows ITT regression results comparing incentives versus control arm participants' reported percentage condom use with sex work clients, self-perceived likelihood of having an STI, and health site visits during follow-up surveys. The minimal change to health status is expected given the one-month intervention duration and the time needed to detect differences in incidences of common STIs.[24] Meanwhile, the incentives intervention appears to improve overall financial conditions, increasing overall income by 13% and relieving debt intensity by 12% for those at the highest intensity (see Column 1 and 5 of Appendix Table A11). These improvements occurred without participants compromising their consumption behavior (see Column 3 of Appendix Table A11). In contrast, the unconditional cash transfers arm did not result in similar improvements in financial conditions.

[23] Similarly, Column 6 of Table A13 shows that participants in the incentives arm were 10% (p-value = 0.08) more likely to report rejecting their most recent rejected sex work client because the price he offered was too low.

[24] For example, HIV antibody tests normally report a three-month "window" period post-exposure in which an infected person may not yet test HIV-positive.

# 6 Model

We write a model of labor supply choice with two goals: to formalize our intuition about mechanisms that can explain our experimental results, and to estimate workers' relative disutility of sex work versus non-sex work.

## 6.1 Model Setup

To set up the model, we consider a worker whose utility is a function of her consumption, $c$, which is constrained by her total income, $y$. $y$ is the sum of her initial non-labor income, $y_0$, and her labor income. For labor income, she can choose her level of sex work effort, $s$, which earns her $p_s$, and service work effort, $t$, which earns her $p_t$. Her consumption can be written as

$$c = y = y_0 + p_s s + p_t t. \tag{4}$$

This can be considered analogous to a budget constraint in a standard setup. She also experiences utility from leisure time, which is captured by

$$v(T - s - t). \tag{5}$$

This effort constraint can be considered analogous to a time constraint in a standard setup. The central focus of this study is to determine whether there is additional relative disutility from sex work. We capture this as

$$\alpha w(s). \tag{6}$$

## 6.2 The Worker's Problem

The worker determines her effort toward each type of work to maximize her utility. Utility is a function of consumption and leisure (assumed to be additively separable), but it will also be impacted by the potential additional disutility from sex work. Thus, it can be written as

$$U(y_0, t, s) = u(y_0 + p_s s + p_t t) + v(T - s - t) - \alpha w(s). \tag{7}$$

We now consider transparent functional forms that are consistent with our data. We start by focusing on the worker's effort constraint. For $v(T - s - t) = T - s - t$, we can show that

$$\frac{ds}{dp_t} = -\frac{\frac{du}{ds}(y + p_s s + p_t t)p_s}{\frac{d^2w}{ds^2}\alpha p_t} < 0, \tag{8}$$

$$\frac{ds}{dy} = 0. \tag{9}$$

Thus, we think about the worker's problem in the case that $v(T-s-t) = T-s-t$. A model that is sufficiently general and consistent with our data can be specified as

$$U(y_0, t, s) = ln(y_0 + p_s s + p_t t) + (T - s - t) - \alpha s^2. \tag{10}$$

Our goal with this specification is threefold. First, we want to consider the possibility of linear income effects, and thus a quasi-linear structure is helpful. Second, we want to consider the possibility of non-linear effort. Hence, the disutility from sex work effort is captured by $\alpha w(s) = \alpha s^2$. Third, we want to consider the simplest possible model to estimate the relative disutility of sex work, so only include a parameter, $\alpha$, for $w(s)$.

## 6.3 The Worker's Optimal Choices

If incentives task effort, $t$, is endogenous, then the optimal $s$ and $t$ can be solved for and characterized as

$$s = \frac{p_s - p_t}{2\alpha p_t}, \tag{11}$$

$$t = \frac{2\alpha p_t^2 - 2y\alpha p_t - p_s^2 + p_s p_t}{2\alpha p_t^2}. \tag{12}$$

Intuitively, the optimal $s$ is the price premium for sex, $\frac{p_s - p_t}{p_t}$, weighted by the relative disutility of sex work, $\alpha$. As a result, we can calculate comparative statics to show that

$$\frac{ds}{dy} = 0, \tag{13}$$

$$\frac{ds}{dp_t} = -\frac{1}{2\alpha p_t} - \frac{p_s - p_t}{2\alpha p_t^2} < 0, \tag{14}$$

$$\frac{dt}{dy} = -\frac{1}{p_t} < 0, \tag{15}$$

$$\frac{dt}{dp_t} = \frac{4\alpha p_t - 2\alpha y + p_s}{2\alpha p_t^2} - \frac{2\alpha p_t^2 - 2y\alpha p_t - p_s^2 + p_s p_t}{\alpha p_t^3} > 0. \tag{16}$$

## 6.4 The Worker's Response to the Experiment

The model allows us to consider the mechanisms contributing to workers' effort allocation responses to the improved labor market opportunities tested in our study. Specifically, the incentives arm increased the relative returns to service work (i.e., increased

$p_t$), while the unconditional cash transfers arm increased income (i.e., increased $y$ by $dy$). The model says that service work incentives increase service work effort, $\frac{dt}{dp_t} > 0$.

The model then suggests that an accompanying decrease in sex work effort can be a result of three possible mechanisms. First, the decrease in sex work effort can result from an income effect, i.e., $\frac{ds}{dy} < 0$. This means that a transfer can decrease the marginal utility of consumption by increasing income. This may decrease sex work by decreasing the perceived need for income. Second, the decrease in sex work effort can result from a price effect, i.e., $\frac{ds}{dp_t} < 0$. This means that a transfer can incentivize decisions that increase the marginal disutility of sex work, which can be driven by either an increase in the marginal utility of leisure or the marginal disutility of effort more generally. Third, the decrease in sex work effort can result from the incentives targeting the same average transfer to workers for whom either of these effects are more important, i.e., $\frac{ds^2}{dp_t d\alpha} < 0$ or $\frac{ds^2}{dy d\alpha} < 0$. This means that a transfer can have targeting effects that allocate resources toward those who are more likely to decrease sex work.

The importance of each mechanism will depend on the transfer's design. A conditional transfer, including one that increases the returns to non-sex work as in our incentives arm, may operate through increasing the marginal disutility of sex work. In contrast, an unconditional transfer may operate through decreasing the marginal utility of consumption. Transfers that are mediated by an administrative ordeal may operate through targeting effects if there is heterogeneity in the cost of completing the ordeal (in this case, the incentives task) and it aligns with the disutility of sex work. We now turn to our experimental data to quantify the magnitude and thus assess the relative importance of each of these potential mechanisms.

# 7 Main Results

## 7.1 Mechanisms

In this section, we empirically assess the relative importance of the three mechanisms that our model says will decrease sex work: price effects, income effects, and targeting effects.

### 7.1.1 Price Effects

Our model says that $\frac{ds}{dp_t} < 0$; that is, the price effect will decrease sex work. We collect data that can help quantify the magnitude of this parameter: since the incentives intervention increases $p_t$, the results presented in Table 5 provide evidence that $\frac{ds}{dp_t} < 0$, implying large price effects. In the rest of this section, we further decompose these price effects.

Price effects can be decomposed into substitution and income effects. We focus on substitution effects in this section and income effects in Section 7.1.2. Substitution from sex to service work can occur directly and indirectly during work hours. For evidence of direct substitution, we can assess if it occurred in the service-sex work trade-off window described in Section 2 (after 12:00am) by coupling the main results in Table 5 with an assessment of whether service work increased and sex work decreased during the trade-off window in Figure 5. For evidence of indirect substitution, we can assess worker incentives task participation during infra-marginal time, i.e., the time when service work is unlikely to be substituted for sex work (before 12:00am).

We find that participants appear to be participating in direct substitution from sex to service work. As previously shown in Table 4, the incentives intervention increased service work effort. Figure 5 shows that at least a portion of the critical mass of submissions falls within the trade-off window. This implies that the increase in service work effort occurred at least in part during the trade-off window. The pink and blue vertical lines in the figure also show that when the constraint was binding and participants could choose between accepting a sex work client or completing their service work shift, those in the incentives arm departed for sex work on average around 50 minutes later than those in the control arm. In a regression on departure time during this trade-off window for the incentives versus control arm, we find a point estimate of 0.82 hours (p-value $<$ 0.01). Coupled with the decrease in sex work estimated in Table 5, these results provide suggestive evidence of *direct* time substitution from sex to service work. These results provide further evidence in favor of the price effects described in our model.

We find that participants also appear to be participating in indirect substitution from sex to service work. Figure 4 shows that the majority of incentives task submissions occurred during the infra-marginal time. Further, we investigate if participants in the incentives arm with high (low) initial endowment status are less (more) likely to participate in the incentives task during the infra-marginal time. Indeed, this appears to be the case. A regression of incentives task submissions on a baseline variable capturing high initial endowment status (number of individuals in the participant's household with a steady income) shows that those with high initial endowment submitted fewer submissions during the infra-marginal time (point estimate = –60.1, p-value $<$ 0.01). This implies that incentives task earnings during the infra-marginal time also play an important role in workers' decisions to substitute away from sex toward service work. These results also provide further evidence in favor of the price effects described in our model.

### 7.1.2 Income Effects

Our model says that $\frac{ds}{dy} < 0$; that is, the income effect will decrease sex work. We collect data that to help quantify the magnitude of this parameter as well: since our unconditional cash transfers arm provides participants in this arm with $dy$, the results in Table 6 imply that income effects do not explain the reduction in sex work associated with service work incentives. This interpretation relies on the fact that the average earnings in the incentives arm was comparable to the payment amount in the unconditional cash transfers arm, as shown in Figure 3. These results are robust to several robustness checks, including winsorizing the outcome variable (Appendix Table A8) and comparing sex work for incentives versus unconditional cash transfers arm participants (Appendix Table A7). The point estimates in Appendix Table A7 are statistically significant and similar in magnitude to those in Table 5, which compares incentives versus control arm participants.

We consider an important caveat to our conclusion that there are no income effects to sex work in this setting. While we measure if there are income effects in response to our unconditional cash transfer intervention, our estimates may reflect a temporary effect that ignores permanent income effects given the month-long duration of our experiment. If participants in the unconditional cash transfers arm are amortizing payments, then income effects may appear small or close to zero. In contrast, the price effect in the incentives arm can impact labor supply choices during the course of the intervention itself; that is, the supply of service work effort impacts sex work effort.

We exploit variation in baseline income levels to test for permanent income effects in Appendix Table A9. Column 1 displays results from a regression of submissions on a baseline variable capturing low initial endowment status (if the participant reported experiencing a shock in the two weeks before the baseline survey). The result shows that those with lower initial endowment submit more incentives tasks. Similarly, Column 2 displays results from a regression of submissions on a baseline variable that proxies for permanent income (the number of individuals in the participant's household with a steady income). The result shows that those with higher permanent income submit fewer submissions. These results exploiting the study's observational data offer some caution in drawing the conclusion that income effects are completely unimportant.

We calculate a back-of-the-envelope estimate of expected effects to sex work in response to our unconditional cash transfer, which suggests a potential 0.02 decrease in sex work. This is close to our experimentally estimated effects for the unconditional cash transfers arm and falls within that estimate's confidence interval (see Table 6). We arrive at this back-of-the-envelope estimate as follows. First, we use data on mean income in this population to estimate the mean difference in income at baseline for households with above or below our sample's median number of household members with steady income.

Second, we use this estimate to calculate the change in sex work per dollar based on baseline differences in sex work. Third, we multiply this estimated change in sex work per dollar by the total dollar value of our unconditional cash transfer. This back-of-the-envelope estimate is 0.02. This suggestive exercise exploiting the study's observational data suggests that income effects are small in this setting.

### 7.1.3 Targeting Effects

If there are targeting effects ($\frac{ds^2}{dp_t d\alpha} < 0$ or $\frac{ds^2}{dyd\alpha} < 0$), then we should observe workers doing increasingly less sex work as incentives task earnings increase. If we view the incentives task as an administrative ordeal, it can be interpreted as acting like a self-selection mechanism that helps identify the highest-benefit recipients of the transfer. The idea is that we are pairing benefits (the transfer) with an action (the incentives task), where the required action is relatively less costly for the highest-benefit recipients than for the rest (Nichols and Zeckhauser, 1982). Thus, the incentives intervention's opt-in design helps assess the importance of the targeting mechanism, i.e., if heterogeneity in income or price effects is driving our results.

First, we examine if the incentives arm's opt-in design helped target money to workers for whom potential price effects would be important. To do so, we test for the presence of heterogeneous price effects across workers. If our data were consistent with a model of heterogeneous price effects across workers, then we would expect differential outcome responsiveness across participants in the incentives arm. Further, we may desire that this outcome responsiveness correspond with the program cost per participant, which in this study is captured by individual worker earnings from the incentives task. To test this hypothesis, we fit a polynomial to our data to assess outcome responsiveness by earnings in the last two weeks.

Appendix Figure A1 presents the results. It displays outcome responsiveness (change in sex work hours per dollar) by incentives task earnings in the last two weeks. The weakly negative slope of the polynomial as we move from low to high earnings implies that workers lack meaningful heterogeneity in price effects. Further, the lack of concavity in the polynomial implies that the higher earners are not necessarily the marginal substituters. These results would imply that $\frac{ds}{dp_t}$ does not necessarily decrease with $\alpha$.

Second, we examine if the incentives arm's opt-in design helped target money to workers for whom potential *income effects* would be important. To do so, we test for the presence of heterogeneous income effects across workers. If our data were consistent with a model of heterogeneous income effects across workers, then we would expect differential outcome responsiveness across the unconditional cash transfers arm participants. Further, we may desire that this outcome responsiveness correspond with the program cost per participant; as a reminder, the program cost per participant is captured by

individual worker earnings from the incentives task.

We build on the price effects analysis in Appendix Figure A1 to proceed with this analysis in three steps. First, we predict potential incentives task earnings for cash transfer arm participants. For this prediction exercise, we use (1) baseline data on incentives and cash transfer arm participants as well as (2) follow-up data on actual incentives task earnings for only participants in the incentives arm only. We define a high type earner as one who is predicted to earn above-median earnings of participants in the incentives arm. This is our measure of worker type. Second, we estimate changes in sex work hours per dollar for unconditional cash transfers arm participants. For this prediction, we use (1) probability weights on the likelihood of being a high- versus low-type earner and (2) actual follow-up data on the change in sex work hours per unconditional cash transfer dollar. This is our measure of outcome responsiveness. Third, we fit a polynomial to these data to assess outcome responsiveness by worker type.

Appendix Figure A2 shows outcome responsiveness (the predicted change in sex work ours per dollar) by worker type (probability of being a high-type earner). The weakly negative slope of the polynomial when moving from low to high predicted likelihood of being a high earner implies that workers also lack meaningful heterogeneity in income effects. Further, the lack of concavity in the polynomial implies that higher-cost workers are not necessarily marginal substituters. These results would imply that $\frac{ds}{dy}$ does not necessarily decrease with $\alpha$.

These results provide some nuance to the merits of the incentives intervention. A program administrator may be constrained to a fixed budget with the potential goal of decreasing the supply of sex work. Thus, they may desire that the budget is disproportionately allocated to individuals who are most likely to substitute away from sex work, i.e., marginal substituters. If $S$ is the change in total sex work across workers associated with total program expenditures $X$, this idea can be written as

$$\frac{S}{X} = \sum \frac{s_i}{x_i} \frac{x_i}{X}. \tag{17}$$

This can be decomposed as

$$\frac{S}{X} = \sum \frac{s_i}{x_i} \frac{x_i}{X} = \sum \left[ \frac{s(i)}{x(i)} - avg\left( \frac{s(i)}{x(i)} \right) \right] \frac{x(i)}{X} + avg\left( \frac{s(i)}{x(i)} \right). \tag{18}$$

The right-hand side says that the total change in sex work per total expenditure can be decomposed into two parts: the covariance of expenditure with the change in sex work and the average effect of the expenditure on sex work. If we are interested in targeting marginal substituters, then the covariance should be important and positive. In particular, $s(i)$ needs to grow more than proportionately relative to $x(i)$ for there to be a positive covariance. An intervention that identifies and can leverage this type

of covariance relationship may be attractive to a policymaker. Since our results on the effects of the incentives intervention show that the data are not consistent with this idea, this remains an important area of future research.

However, alternatively, analysis of heterogeneous effects based on ex ante characteristics provides promising suggestive evidence of the expected impact of targeting by these characteristics if the intervention were scaled. Appendix Figures A3, A4, and A5 provide aggregate outcome responsiveness and estimated heterogeneous effects by ex ante characteristics. For example, Figure A5 suggests that for a policymaker aiming to decrease aggregate levels of sex work, targeting individuals with higher ex ante exposure to sex work may be the most effective strategy to achieve this goal.[25]

## 7.2 Labor Supply Elasticities

Our empirical analysis of mechanisms above implies that price effects explain our experimental results. Comparing how both sex and non-sex labor supply respond to price changes is of interest in this setting, as it aids in understanding workers' decision-making and could inform alternative labor market policies for sex workers. We can use our data to quantify and compare the price effect on sex work versus non-sex work. Specifically, we estimate two labor supply elasticities in this setting using our experimental results. First, we estimate the own-price labor supply elasticity for service work by incorporating experimental results on daily service venue wages and daily service venue hours. Second, we estimate the cross-price labor supply elasticity for sex work by incorporating experimental results on daily service venue wages and sex work hours over the prior two weeks.

### 7.2.1 Own-Price Labor Supply Elasticity

We use our reduced-form effects to estimate a Marshallian own-price labor supply elasticity for service work of 0.45. To arrive at this estimate, we employ a 2SLS setup in which we exploit randomized participant assignment to the incentives arm as an instrument increasing service venue wages. Specifically, we estimate the own-price labor supply elasticity for service work by replacing the dependent variable in equation 3 with *ln Service Hours*. We also replace the endogenous variable in equation 2 with *ln Wage* from service work. This wage value includes incentives task earnings for participants in the incentives arm.

In the first panel of Table 7, Column 1 shows that the first-stage effect of the incentives arm intervention status on earnings is 20%. Column 2 shows that given this effective earnings increase, we estimate an own-price labor supply elasticity of 0.45. Column 3

[25]Exposure is measured in two ways: based on if the worker participated in sex work before their current job and by reported sex work levels.

shows the OLS of *ln Service Venue Hours* on incentives arm intervention status, which, when divided by the estimate in Column 1, is approximately equal to the estimate in Column 2.

Our elasticity estimate is within the range of prior, comparable elasticity estimates. This estimate implies that the outside option's own-price labor supply elasticity is positive and inelastic. The estimate falls within the range of analogous elasticity estimates using labor force data for married women, single and childless women, and single mothers in western countries, which ranges from 0.23 to 0.59 (Bargain and Peichl, 2016; McClelland and Mok, 2012). This implies that female workers in this setting are as elastic to "traditional work" (relative to sex work) as their demographic counterparts in developed country settings are to similarly traditional work. Additionally, our estimate is smaller than those found in the literature that leverages quasi-random and experimental variation in contexts with daily variation in earnings and labor supply (0.5–1.5).[26] This follows from the fact that this literature focuses on Hicksian or Frischian elasticities, while we calculate a Marshallian elasticity. Further, service work effort is dictated by regimented daily shifts in this setting. Thus, it also follows that workers' responses to a change in wages is less elastic in our setting relative to the settings studied in this literature.

### 7.2.2 Cross-Price Labor Supply Elasticity

We also use our reduced-form effects to estimate a Marshallian cross-price labor supply elasticity between sex and non-sex work of –0.57. Similar to our own-price elasticity estimates for service work, workers' sex work responses to the incentives task can be utilized to measure a cross-price labor supply elasticity for sex work. As before, we replace the endogenous variable in equation 2 with the *ln Wage* from service work. Again, this wage value includes incentives task earnings for participants in the incentives arm. However, this time, we replace the dependent variable in equation 3 with *ln Sex Work Hours* over the prior two weeks.

In the second panel of Table 7, Column 1 shows that the first-stage effect of the incentives arm intervention status on earnings is 22%. Column 2 shows that given this effective earnings increase, we estimate a cross-price labor supply elasticity of –0.57. Column 3 shows the OLS of *ln Sex Work Hours* on incentives arm intervention status, which, when divided by the estimate in Column 1, is approximately equal to the estimate in Column 2. Appendix Table A6 further shows that our results are robust to the inverse hyperbolic sine transformation of *Sex Work Hours*.

Our elasticity estimate falls within the range of previous elasticity estimates for other

[26]This literature includes work on rideshare and taxicab drivers (Thakral and Tô, 2021; Angrist et al., 2021; Chou, 2002; Camerer et al., 1997), bike messengers (Fehr and Goette, 2007), and baseball stadium vendors (Oettinger, 1999).

worker populations studied. It implies that while the cross-price labor supply elasticity for sex work is negative and inelastic, it is still more elastic than the own-price labor supply elasticity for service work calculated above (0.45). This estimate falls at the higher end (in magnitude) of the range of analogous elasticity estimates (0.23–0.59) using labor force data for married women, single and childless women, and single mothers in Western countries (Bargain and Peichl, 2016; McClelland and Mok, 2012). This implies that labor supply is more responsive for sex work than for other "traditional" labor options.

Further, our estimates are within but on the smaller end, in magnitude, of the broad range of those in papers that exploit quasi-random and experimental variation in other settings with daily variation in earnings and labor supply (0.5–1.5).[27] As with our own-price labor supply elasticity estimates, this contrast is perhaps unsurprising given that we estimate the Marshallian rather than the Hicksian or Frischian labor supply elasticities of focus in this literature. It is also perhaps unsurprising given that the cited research focuses on professions for which labor supply is significantly less risky than sex work. In contrast, as expected, estimates of labor supply elasticities in settings with daily laborers in other higher-risk work are closer in magnitude, ranging from 0.06 to 0.21 (Stafford, 2015; Nguyen and Leung, 2013).

## 7.3 Structural Estimation

We write an augmented model to estimate the disutility of sex work relative to service work. If the worker chooses levels of effort to devote to service work and sex work versus leisure as

$$U(y_0, t, s) = ln(y_0 + p_s s + p_t t) + (T - s - t) - \alpha s^2 - 2\alpha\beta_i s, \tag{19}$$

then the optimal level of sex work effort, $s_i$, would be

$$s_i = \frac{p_{s,i} - p_{t,i}}{2\alpha p_{t,i}} - \beta_i. \tag{20}$$

$\frac{p_{s,i}-p_{t,i}}{p_{t,i}}$ is the sex work versus service work price premium. The relative disutility of sex work, $\alpha$, weighs the price premium, and $\beta_i$ captures the worker-level difference in relative disutility of sex work from the sample mean captured by $\alpha$. We interpret $s_i$ to say that a sample mean $\alpha$ value greater than 1 implies that workers are increasingly outcome responsive to the price premium.

Random assignment of the incentives intervention offers variation in $PricePremium_{ict}$. Specifically, the incentives intervention decreases the price premium on average. There-

[27] As previously mentioned, this literature includes work on rideshare and taxicab drivers (Thakral and Tô, 2021; Angrist et al., 2021; Chou, 2002; Camerer et al., 1997), bike messengers (Fehr and Goette, 2007), and baseball stadium vendors (Oettinger, 1999).

fore, the empirical analogues to equation 20 can be expressed as

$$SexHours_{ict} = \nu + \frac{1}{2\alpha} PricePremium_{ict} - \beta_i, \tag{21}$$

where $PricePremium_{ict}$ is measured at the daily level. We can then estimate $\alpha$ leveraging a 2SLS specification. The first stage is

$$PricePremium_{ict} = \pi + \mu Incentives_c + \kappa X_i + \chi Time_t + \delta_{ict}, \tag{22}$$

and the second stage is

$$SexHours_{ict} = \tau + \xi \widehat{PricePremium}_i + \psi X_i + \omega Time_t - (-\beta_{ict}), \tag{23}$$

where $\xi = \frac{1}{2\alpha}$ estimates the 2SLS effect on sex work hours during the intervention of the average difference in price premiums between the incentives and control arms.

We find that, on average, $\hat{\alpha} = 8.06$, implying that the relative disutility of sex work is larger than that of service work, increases with higher levels of sex work, and far exceed that of non-sex work. First, we find that this estimate is non-zero. In Section 7.2, we found that the own-price elasticity estimate for service work was 0.45 and the cross-price elasticity estimates for sex work versus service work was –0.57. A comparison of the magnitude of these elasticities implies that the relative disutility of sex is higher than that of service work. This structural estimation validates this notion. Second, we find that this estimate is positive. This implies that the relative marginal disutility of sex work is increasing as well. Third, we calculate that, on the margin, one unit of sex work is associated with the disutility-equivalent of an additional 16 times the disutility of one unit of non-sex work. Our estimation result suggests that the significant policy attention focused on decreasing the size of the market for sex appears justified. However, it also underscores the need for evidence on the effects of interventions like those tested in this paper which can decrease the size of the market without the welfare losses associated with status quo bans (Cameron et al., 2021; Cunningham and Shah, 2018).

To consider if this high disutility value is sufficiently compensated for in the market, we conduct a brief suggestive exercise in Appendix Section 11.3.2. We estimate compensating wage differentials for the key disamenity associated with sex work reported workers in our sample, sexual health risk. We find suggestive evidence that prices for sex may compensate for this primary driver of this relative disamenity value.

# 8 Conclusion

In a representative population of sex workers, we test the effects of improving non-sex work labor market opportunities on the supply of sex work labor. For women engaged

in indoor, part-time sex work—the majority of sex workers worldwide—we find that incentivizing the outside work option decreased the supply of sex work labor relative to both a pure control and a similarly valued fixed unconditional cash transfer. We also find increases in overall income, decreases in average debt levels across workers, and substitution to more remunerative clients. For a scaled intervention, we anticipate modest projected decreases in HIV and/or STI incidence per worker. Contrary to assumptions that commonly motivate protective legislation, our reduced-form effects show that sex work labor supply is elastic. Thus, we proceed to write a model of labor supply choice, empirically investigate mechanisms explaining our reduced-form results, calculate labor supply elasticities, and structurally estimate worker valuations to their available labor supply options.

We conducted a one-month cluster RCT in which venue-based female sex workers were randomized to receive either service work incentives, an unconditional cash transfer, or neither. We write a model of labor supply to both provide structure to our empirical investigation of mechanisms and estimate the relative disutility of sex work. Minimal impacts on sex work labor supply in the unconditional cash transfers arm imply that income effects do not explain the incentives' negative effects to sex work. Our results also do not appear to be consistent with a targeting story in which the opt-in design of the incentives can better identify the marginal substituters. Instead, the incentives' negative effects on sex work appears to be primarily explained by price effects. We thus interpret our empirical estimates to reflect the disutility of sex work relative to traditional work.

Accordingly, we estimate labor supply elasticities for both non-sex work and between sex and non-sex work. Comparing the estimates' signs and magnitudes suggests that workers substitute between the two types of work and that the relative disutility of sex work outweighs that of non-sex work. Structural estimation of the model's parameter for the relative disutility of sex work further validates that it is higher than that of non-sex work and, further, implies that is increasing in sex work. Overall, our findings imply that there is scope to improve policy responses to the market for sex. In contrast to prevailing protective legislation, which often takes the form of bans, we find that the size of the market for sex can be reduced without trading off worker welfare.

There are important limitations of our study's broader interpretation. First, the increase in sex work proposals in response to the incentives may be an unintended consequence from a policymaker's perspective. Reassuringly, however, the incentives still led to a decrease in *accepted* sex work proposals, revealing that workers' preferences against sex work dominated. Second, the potential increase in (perceived) risk-taking (with respect to partner profiles) in response to the incentives may also be an unintended consequence from a policymaker's perspective, though this result is not significant at conventional levels. Third, the incentives task in this study was designed with no limit for the purposes of identifying worker heterogeneity in participation. However, in re-

ality, subsidies to outside labor options would face resource constraints from program administrators. This offers some caution in interpreting our finding of increases in overall worker income. Finally, our compensating wage differentials and value of a statistical life estimates are only suggestive in nature given their imprecision. Nevertheless our study shows that for workers on the margin, the opportunity to earn more from their outside option can meaningfully impact their labor supply choices, providing evidence against the common notion that sex work labor supply is inelastic.

Understanding the extent to which individuals *choose* sex work versus outside options is vital for assessing the impacts of policies regulating the market for sex. We demonstrate how improving labor market conditions can lead to meaningful changes in labor supply decisions for workers in resource-limited settings and that providing unconditional cash transfers does not have similar effects. A growing literature has documented various negative effects of criminalizing sex work, especially for the sex workers themselves (Ciacci and Sviatschi, 2022; Cameron et al., 2021; Cunningham and Shah, 2018; Bisschop et al., 2017). Many of these regulations assume that increasing the costs to sex work may eliminate sex work, but individuals' responses depend on their underlying preferences for sex work versus non-sex work. We provide experimental evidence to quantify this unknown parameter.

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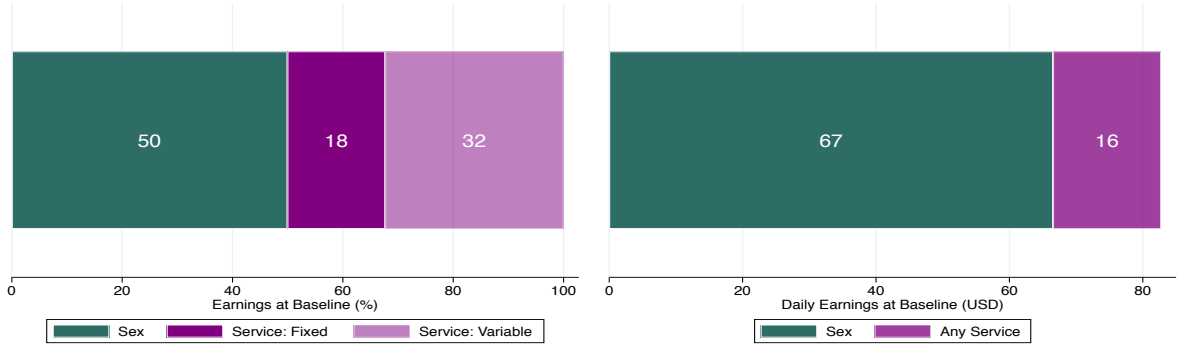
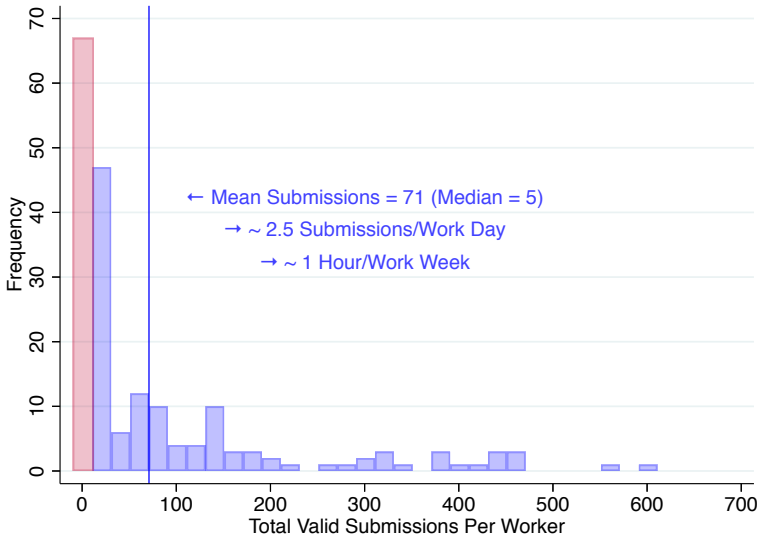
# 9 Figures

Figure 1: Labor Supply Characteristics at Baseline



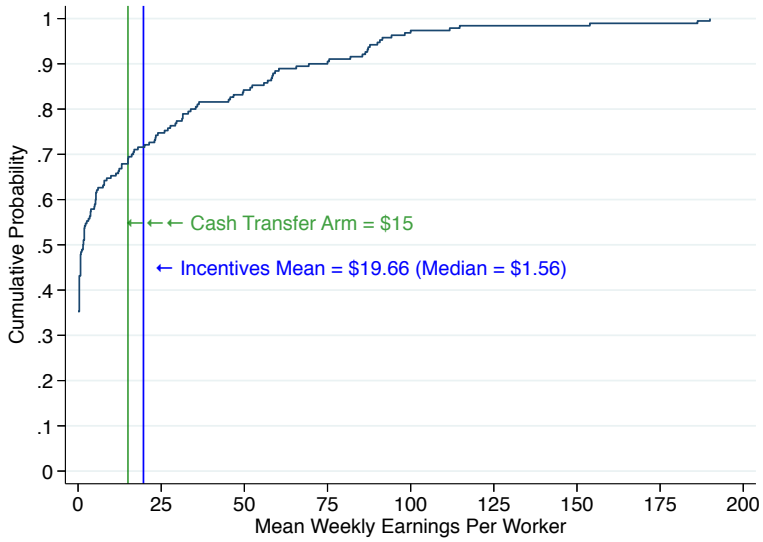

Note: This figure provides statistics on workers' earnings at baseline. The left-hand size displays the percentage of total monthly earnings by source at baseline. Sex work earnings are estimated by multiplying the reported price of sex per hour by the number of reported sex work hours at baseline. Service work earnings are asked about directly by category at baseline. Service work earnings are displayed by fixed service work earnings (base salary from the service venue) and variable service work earnings (venue-level communal tip split, drink incentives earned from venue, and customer tips). The right-hand side displays the amount of daily earnings (in USD) by sex work versus service work at baseline.

Figure 2: Distribution of Incentives Task Submissions in the Incentives Arm



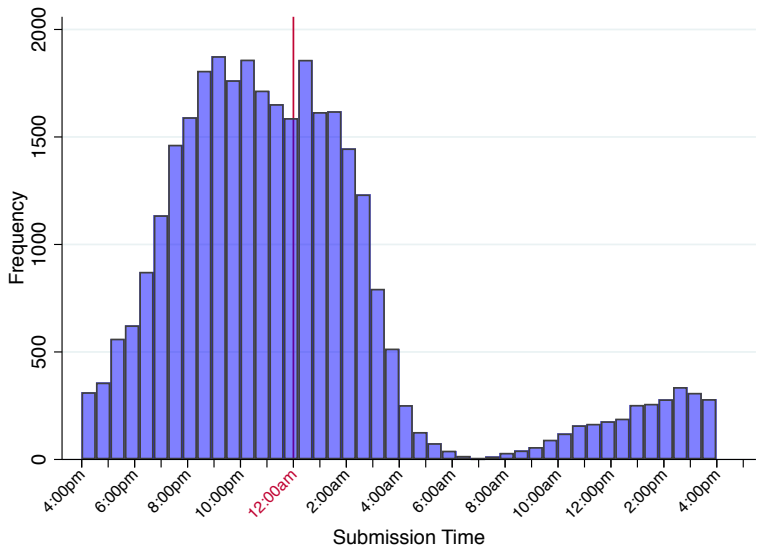

Note: This figure displays the distribution of total valid incentives task submissions per worker participating in the incentives arm during the one-month intervention period. Workers submitting zero valid submissions are captured by the pink bar to the left. The vertical purple line denotes the mean number of submissions across workers. The number of submissions per work day is estimated by dividing the mean number of submissions across workers by the mean number of work days across workers. The mean number of hours devoted to the incentives task per work week is estimated by multiplying the mean number of minutes devoted to the incentives task (three minutes) by the mean number of incentives task submissions per work week.

Figure 3: Cumulative Distribution Function of Mean Weekly Incentives Task Earnings in the Incentives Arm



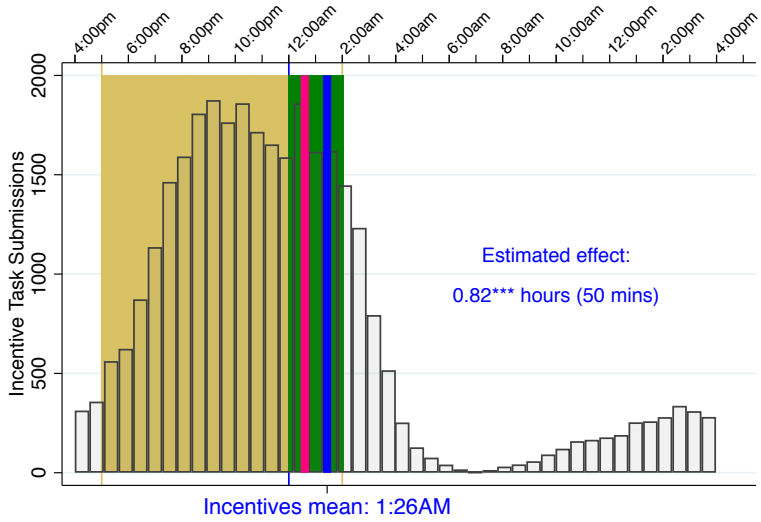

Note: This cumulative distribution function displays the mean weekly incentives task earnings across workweeks per worker for workers participating in the incentives arm during the one-month intervention period. The vertical green line denotes the fixed weekly unconditional cash transfer payment ($15) for workers participating in the unconditional cash transfers arm. The vertical purple line denotes the mean weekly earnings amount ($19.66) for workers participating in the incentives arm.

Figure 4: Distribution of Incentives Task Submission Times in the Incentives Arm




Note: This figure displays the distribution of incentives task submission times throughout the day for workers participating in the incentives arm during the one-month intervention period. The vertical red line denotes 12:00am, the point after which we doubled incentives task compensation from $1.50 to $3.00. This compensation structure was motivated by our focus group finding that the 12:00am onward time period was most pivotal to a worker to decide whether she would engage in sex work that work night.

Figure 5: Sex Work Departure Time in Incentives vs. Control Arms




Note: This figure displays the distribution of incentives task submission times throughout the day for workers participating in the incentives arm during the one-month intervention period. Based on our focus group findings, the yellow-shaded region denotes workers' average service work shift time in which they are unlikely to be deciding whether to accept a sex work proposal if received from a service venue customer. Likewise, also based on focus group findings, the green-shaded region denotes workers' average service work shift time in which they *are* likely to be deciding whether to accept a sex work proposal if received from a service venue customer. The vertical pink line denotes the mean reported departure time for sex work in the control arm, while the vertical blue line denotes the mean reported departure time for sex work in the incentives arm. The blue text on the right displays the regression estimate capturing the delay in sex work departure time for incentives versus control arm participants. This regression is run among the pool of most recent sex work clients for whom participants left their shift early during the 12:00am onward service versus sex trade-off window.

# 10 Tables

Table 1: Global Population of Sex Workers

| **Statistic** | **Estimate** |
|---|---|
| Size (USD) | 100-180 Billion |
| Size (Workers) | 42-52 Million |
| Portion Female | 4/5 |
| Portion Part-time | 4/5 |
| Portion Non-street-based/Indoor | 4/5 |
| Age | 75% age 13-25 |
| Portion Workers Trafficked | 1/10 |

Note: This table summarizes statistics on the global market for sex. We sought to recruit a sample of workers representative of this market. Sources: International Union of Sex Workers (2024), Scelles Foundation (2011), Cunningham and Kendall (2011), ILO (2024).

Table 2: Sex Worker and Sex Work Client Characteristics

| | Mean | SD |
|---|---|---|
| **Sex Workers** | | |
| Age | 28.22 | 6.80 |
| Married | 0.21 | 0.41 |
| Primary School Complete | 0.99 | 0.10 |
| From City | 0.32 | 0.47 |
| Household Size | 3.88 | 2.21 |
| In Debt | 0.65 | 0.48 |
| Service Hours Yesterday | 9.93 | 3.14 |
| Sex Hours Last 2 Weeks | 2.93 | 2.02 |
| Uses HIV Prevention | 0.02 | 0.13 |
| Uses Hormonal Contraceptive | 0.16 | 0.37 |
| Condom Used Last Client | 0.97 | 0.18 |
| **Sex Work Clients** | | |
| Age | 39.44 | 8.67 |
| Income: Poor | 0.01 | 0.11 |
| Income: Average | 0.88 | 0.33 |
| Income: Rich | 0.11 | 0.31 |
| Violent, Rude, or Aggressive | 0.01 | 0.10 |
| Local | 0.79 | 0.41 |

Note: This table uses our baseline data summarizes key characteristics on the supply and demand of sides of the market for sex in our setting. The top panel summarizes characteristics of the workers in our sample, while the bottom panel summarizes characteristics of the workers' most recent sex work clients.

Table 3: Balance: Demographics

**Incentives vs. Control**

| Variable | All | | Control | | Incentives | | |
|---|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD | Difference |
| Age | 28 | 6.89 | 27.71 | 7 | 27.77 | 6.89 | 0.100 |
| Married | .21 | 0.39 | 0.25 | .43 | 0.18 | 0.39 | -0.063 |
| Pimary School Complete | .99 | 0.10 | 0.99 | .1 | 0.99 | 0.10 | 0.005 |
| On Govt Aid | .0055 | 0.10 | 0.00 | 0 | 0.01 | 0.10 | 0.011 |
| From City | .32 | 0.47 | 0.30 | .46 | 0.33 | 0.47 | 0.032 |
| Household Size | 3.9 | 2.64 | 3.96 | 2 | 3.89 | 2.64 | -0.080 |

**UCTs vs. Control**

| Variable | All | | Control | | UCTs | | |
|---|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD | Difference |
| Age | 28 | 6.41 | 27.71 | 7 | 29.29 | 6.41 | 1.590** |
| Married | .21 | 0.39 | 0.25 | .43 | 0.19 | 0.39 | -0.057 |
| Pimary School Complete | .99 | 0.08 | 0.99 | .1 | 0.99 | 0.08 | 0.005 |
| On Govt Aid | .0055 | 0.08 | 0.00 | 0 | 0.01 | 0.08 | 0.006 |
| From City | .32 | 0.47 | 0.30 | .46 | 0.32 | 0.47 | 0.016 |
| Household Size | 3.9 | 1.93 | 3.96 | 2 | 3.77 | 1.93 | -0.186 |

**UCTs vs. Incentives**

| Variable | All | | UCTs | | Incentives | | |
|---|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD | Difference |
| Age | 28 | 6.89 | 29.29 | 6.4 | 27.77 | 6.89 | -1.491* |
| Married | .21 | 0.39 | 0.19 | .39 | 0.18 | 0.39 | -0.006 |
| Pimary School Complete | .99 | 0.10 | 0.99 | .077 | 0.99 | 0.10 | 0.001 |
| On Govt Aid | .0055 | 0.10 | 0.01 | .077 | 0.01 | 0.10 | 0.005 |
| From City | .32 | 0.47 | 0.32 | .47 | 0.33 | 0.47 | 0.016 |
| Household Size | 3.9 | 2.64 | 3.77 | 1.9 | 3.89 | 2.64 | 0.106 |

Note: This table displays balance of demographic characteristics for participants in our sample at baseline. The first panel compares characteristics of participants in the incentives versus the control arm. The second panel compares characteristics of participants in the unconditional cash transfers versus the control arm. The third panel compares characteristics of participants in the unconditional cash transfers versus the incentives arm. The first column displays the baseline characteristics being summarized. From left to right, means and SDs are displayed per sample, as described by the column headers. For example, in the second panel, the third and fourth columns show the mean and SD for the control group for the baseline characteristic denoted in the leftmost column of each row. The rightmost column displays the mean difference in baseline characteristics between the study arms of interest in a given panel. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Effects on Service Work Across Intervention Arms

| **Incentives Arm** | | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | 1D Venue Hours | 2W Venue Tip Split | 2W Customers Served |
| Incentives | 0.67** | 0.47** | 6.53* |
| | (0.33) | (0.23) | (3.58) |
| Constant | 7.79*** | -0.12 | 44.40*** |
| | (0.69) | (0.80) | (14.40) |
| Round FEs | Yes | Yes | Yes |
| Clustered SEs | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes |
| $N$ | 696 | 713 | 717 |
| **Unconditional Cash Transfers Arm** | | | |
| | (1) | (2) | (3) |
| | 1D Venue Hours | 2W Venue Tip Split | 2W Customers Served |
| UCTs | 0.00 | 0.07 | 0.62 |
| | (0.35) | (0.05) | (3.58) |
| Constant | 7.53*** | 0.02 | 57.58** |
| | (0.90) | (0.09) | (27.25) |
| Round FEs | Yes | Yes | Yes |
| Clustered SEs | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes |
| $N$ | 663 | 681 | 683 |

Note: This table displays the estimated effect on service work effort over various measures of effort during the one-month intervention period. The top panel focuses on the incentives arm, while the bottom panel focuses on the unconditional cash transfers arm. Regressions are at the participant-survey round level. *1D Venue Hours* is the number of hours between the participant's reported start and end time the day before the given participant survey. *2W Venue Tip Split* is the portion of her venue's communal venue tip split that she reported receiving since the last participant follow-up survey. *2W Customers Served* is the number of customers the participant reported serving since the last participant follow-up survey. Standard errors clustered at the venue level are indicated in parentheses. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Effects on Sex Work in the Incentives Arm

| | (1) | (2) | (3) |
|---|---|---|---|
| | 2W Sex Work Hours | 2W Sex Work Hours | 2W Sex Work Hours |
| Incentives | -0.38*** | -0.37*** | -0.36*** |
| | (0.14) | (0.13) | (0.13) |
| Round 3 | -0.07 | -0.07 | -0.08 |
| | (0.08) | (0.08) | (0.08) |
| Outcome at Baseline | | 0.13** | 0.14*** |
| | | (0.05) | (0.05) |
| Age | | | -0.02** |
| | | | (0.01) |
| Primary School Complete | | | 0.96 |
| | | | (0.59) |
| Married | | | 0.12 |
| | | | (0.15) |
| Baseline Earnings | | | -0.00 |
| | | | (0.00) |
| Constant | 2.97*** | 2.58*** | 2.23*** |
| | (0.11) | (0.18) | (0.68) |
| Round FEs | Yes | Yes | Yes |
| Clustered SEs | Yes | Yes | Yes |
| Controls | No | No | Yes |
| $N$ | 731 | 731 | 717 |

Note: This table displays the estimated effect on sex work effort (hours) for incentives versus control arm participants during the one-month intervention period. Regressions are at the participant-survey round level. *2W Sex Work Hours* is the reported number of sex work hours since the last follow-up survey. Standard errors clustered at the venue level are indicated in parentheses. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effects on Sex Work in the Unconditional Cash Transfers Arm

| | (1) | (2) | (3) |
|---|---|---|---|
| | 2W Sex Work Hours | 2W Sex Work Hours | 2W Sex Work Hours |
| UCTs | -0.03 | 0.00 | 0.00 |
| | (0.14) | (0.13) | (0.13) |
| Round 3 | -0.01 | -0.00 | -0.02 |
| | (0.10) | (0.10) | (0.10) |
| Outcome at Baseline | | 0.29*** | 0.30*** |
| | | (0.05) | (0.05) |
| Age | | | -0.00 |
| | | | (0.01) |
| Primary School Complete | | | 0.94** |
| | | | (0.43) |
| Married | | | 0.12 |
| | | | (0.18) |
| Baseline Earnings | | | -0.00** |
| | | | (0.00) |
| Constant | 2.94*** | 2.08*** | 1.28** |
| | (0.12) | (0.17) | (0.57) |
| Round FEs | Yes | Yes | Yes |
| Clustered SEs | Yes | Yes | Yes |
| Controls | No | No | Yes |
| $N$ | 703 | 703 | 685 |

Note: This table displays the estimated effect on sex work effort (hours) for unconditional cash transfer versus control arm participants during the one-month intervention period. Regressions are at the participant-survey round level. *2W Sex Work Hours* is the reported number of sex work hours reported since the last follow-up survey. Standard errors clustered at the venue level are indicated in parentheses. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Labor Supply Elasticities in the Incentives Arm

| **Service Work Own-Price Labor Supply Elasticity** | | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | FS: ln Wage | 2SLS: ln Service Hours | OLS: ln Service Hours |
| Incentives | 0.20*** | | 0.09** |
| | (0.05) | | (0.04) |
| ln Wage | | 0.45** | |
| | | (0.22) | |
| Round FEs | Yes | Yes | Yes |
| Clustered SEs | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes |
| *N* | 690 | 690 | 690 |

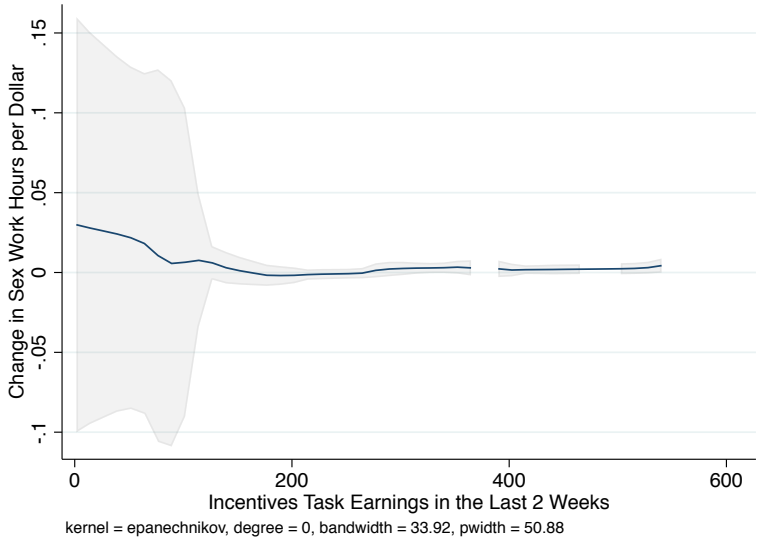
| **Sex Work Cross-Price Labor Supply Elasticity** | | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | FS: ln Wage | 2SLS: ln Sex Hours | OLS: ln Sex Hours |
| Incentives | 0.22*** | | -0.12*** |
| | (0.05) | | (0.05) |
| ln Wage | | -0.57** | |
| | | (0.25) | |
| Round FEs | Yes | Yes | Yes |
| Clustered SEs | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes |
| *N* | 679 | 679 | 679 |

Note: This table displays the estimated labor supply elasticities using data on incentives versus control arm participants during the one-month intervention period. The top panel focuses on the own-price labor supply elasticity of service work, investigating the effect of service work wage increases on service work effort (hours). The bottom panel focuses on the cross-price labor supply elasticity for sex work, investigating the effect of service work wage increases on sex work effort (hours). Regressions are at the participant-survey round level, and observations with missing service venue hours or income data are excluded. *ln Wage* is the ln of total reported service work income since the last follow-up survey divided by the number of days worked since the last follow-up survey; for the incentives arm, this includes incentives task earnings. *ln Service Hours* is the ln of number of hours between the participant's reported start and end time the day before the given participant survey. *ln Sex Hours* is the ln of number of sex work hours reported since the last follow-up survey. Column 1 shows the first-stage estimates and Column 2 shows the 2SLS estimates. Column 3 shows the OLS of *ln Service Hours* or *ln Sex Hours* on incentives arm intervention status. Standard errors clustered at the venue level are indicated in parentheses. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

# 11 Appendix

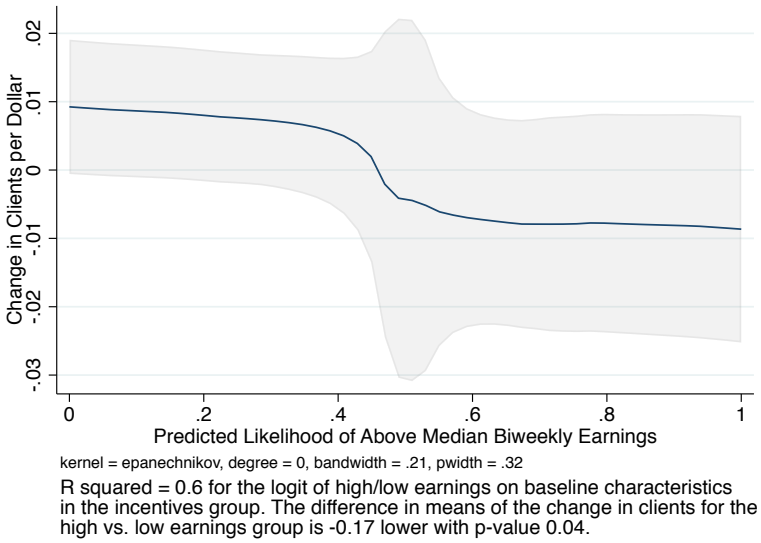
## 11.1 Appendix Figures

Figure A1: Outcome Responsiveness by Earnings in Last 2 Weeks in Incentives Arm



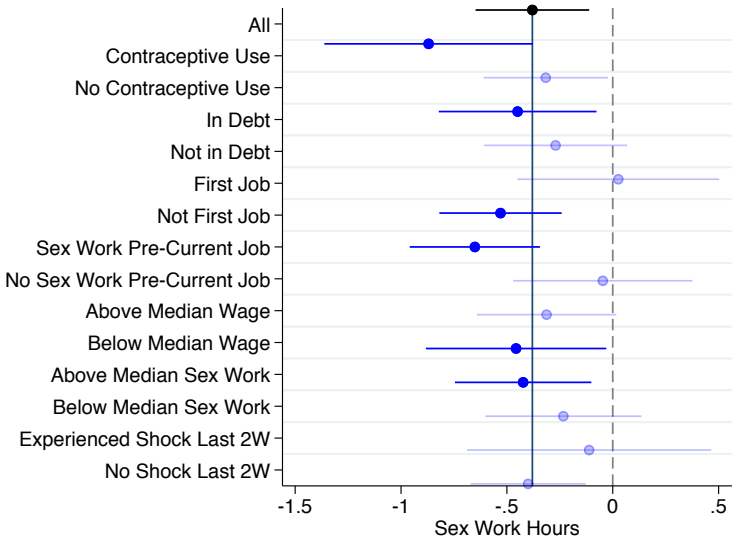

Note: This figure plots the change in sex work hours per dollar earned (outcome responsiveness) by incentive task earnings in the 2 weeks prior to a given follow-up survey for participants in the incentives arm.

Figure A2: Outcome Responsiveness by Probability of Being High Type Worker in Unconditional Cash Transfers Arm




Note: This figure plots the change in sex work clients (hours) per dollar earned (outcome responsiveness) by predicted likelihood of being a high type worker for unconditional cash transfers arm participants. We define a high type worker as one who is predicted to earn above the incentives arm participants' median incentives task earnings throughout the intervention period. Earnings predictions for unconditional cash transfers arm participants are based on (1) baseline characteristics for incentives and unconditional cash transfers arm participants and (2) incentives' arm participants' incentives task earnings.

Figure A3: Heterogeneous Effects on Sex Work Levels by Baseline Characteristic in the Incentives Arm

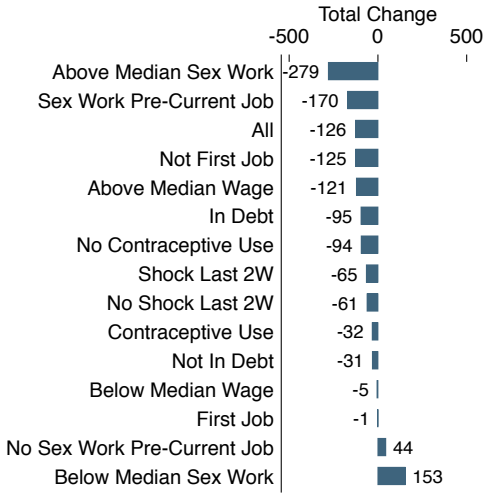



Note: This coefficient plot displays regression coefficients for the interaction of an indicator for incentives arm status and a given baseline characteristic as displayed on the left of the plot. The outcome studied is *2W Sex Work Hours*. *2W Sex Work Hours* is the reported number of sex work hours reported since the last follow-up survey. The mean of *2W Sex Work Hours* in the incentives arm during the follow-up period was 2.55 and captured by the black coefficient at the top for "All".

Figure A4: Heterogeneous Effects on Percent Change in Sex Work vs. Baseline by Characteristic in the Incentives Arm


Note: This coefficient plot displays regression coefficients for the interaction of an indicator for incentives arm status and a given baseline characteristic as displayed on the left of the plot. The outcome studied is percentage change in *2W Sex Work Hours* relative to baseline. *2W Sex Work Hours* is the reported number of sex work hours reported since the last follow-up survey. The mean percentage decrease in *2W Sex Work Hours* for the incentives arm overall was 13% which is denoted by the black vertical line which is referred to as "All".

Figure A5: Heterogeneity in Total Sex Work Change by Characteristic in the Incentives Arm




Note: This figure displays bars aggregating absolute change in sex work hours for participants in the incentives arm described by a given baseline characteristic as labeled on the left of the plot. The outcome studied is total change in *2W Sex Work Hours* relative to baseline. *2W Sex Work Hours* is the reported number of sex work hours reported since the last follow-up survey. The total decrease in *2W Sex Work Hours* relative to baseline for the incentives arm overall was -126 hours.

## 11.2 Appendix Tables

Table A1: Balance: Outcomes

**Incentives vs. Control**

| Variable | All | | Control | | Incentives | | |
|---|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD | Difference |
| Fixed Income | 67 | 23.01 | 64.57 | 34 | 65.97 | 23.01 | 1.423 |
| Variable Income | 123 | 112.97 | 128.80 | 119 | 120.41 | 112.97 | -8.131 |
| Hours | 9.9 | 2.97 | 9.59 | 3.3 | 10.15 | 2.97 | 0.569 |
| Sex Work Clients | 2.9 | 2.46 | 2.98 | 1.7 | 2.94 | 2.46 | -0.042 |
| % Client Condom | .95 | 0.20 | 0.95 | .19 | 0.95 | 0.20 | -0.003 |
| Violence Risk | .0099 | 0.07 | 0.02 | .15 | 0.01 | 0.07 | -0.018 |
| Reject: Price Too Low | .45 | 0.50 | 0.47 | .5 | 0.46 | 0.50 | -0.017 |
| Thinks has STI | 1.2 | 0.70 | 1.20 | .53 | 1.26 | 0.70 | 0.058 |
| Debt Status | .65 | 0.49 | 0.63 | .48 | 0.62 | 0.49 | -0.021 |

**UCTs vs. Control**

| Variable | All | | Control | | UCTs | | |
|---|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD | Difference |
| Fixed Income | 67 | 50.96 | 64.57 | 34 | 71.58 | 50.96 | 6.896 |
| Variable Income | 123 | 119.80 | 128.80 | 119 | 118.09 | 119.80 | -10.230 |
| Hours | 9.9 | 3.16 | 9.59 | 3.3 | 10.07 | 3.16 | 0.490 |
| Sex Work Clients | 2.9 | 1.77 | 2.98 | 1.7 | 2.86 | 1.77 | -0.111 |
| % Client Condom | .95 | 0.21 | 0.95 | .19 | 0.94 | 0.21 | -0.011 |
| Violence Risk | .0099 | 0.00 | 0.02 | .15 | 0.00 | 0.00 | -0.024** |
| Reject: Price Too Low | .45 | 0.49 | 0.47 | .5 | 0.41 | 0.49 | -0.066 |
| Thinks has STI | 1.2 | 0.64 | 1.20 | .53 | 1.29 | 0.64 | 0.098 |
| Debt Status | .65 | 0.46 | 0.63 | .48 | 0.71 | 0.46 | 0.078 |

**UCTs vs. Incentives**

| Variable | All | | UCTs | | Incentives | | |
|---|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD | Difference |
| Fixed Income | 67 | 23.01 | 71.58 | 51 | 65.97 | 23.01 | -5.473 |
| Variable Income | 123 | 112.97 | 118.09 | 120 | 120.41 | 112.97 | 2.099 |
| Hours | 9.9 | 2.97 | 10.07 | 3.2 | 10.15 | 2.97 | 0.079 |
| Sex Work Clients | 2.9 | 2.46 | 2.86 | 1.8 | 2.94 | 2.46 | 0.068 |
| % Client Condom | .95 | 0.20 | 0.94 | .21 | 0.95 | 0.20 | 0.008 |
| Violence Risk | .0099 | 0.07 | 0.00 | 0 | 0.01 | 0.07 | 0.006 |
| Reject: Price Too Low | .45 | 0.50 | 0.41 | .49 | 0.46 | 0.50 | 0.050 |
| Thinks has STI | 1.2 | 0.70 | 1.29 | .64 | 1.26 | 0.70 | -0.039 |
| Debt Status | .65 | 0.49 | 0.71 | .46 | 0.62 | 0.49 | -0.099* |

Note: This table balance of study outcomes for participants in our sample at baseline. The first panel compares characteristics of participants in the incentives vs. control arm. The second panel compares characteristics of participants in the unconditional cash transfers vs. control arm. The third panel compares characteristics of participants in the incentives vs. unconditional cash transfers arm. The first column displays the baseline characteristic being summarized. From left to right, means and SDs are displayed per sample as described by the column headers. For example, the third and fourth columns in the second panel display the mean and SD for the control group of the baseline characteristic in each row denoted in the leftmost column. The right most column displays the difference in baseline characteristic between the given study arms of interest in a given panel and the stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Complier Characteristics

| Variable | All | | Always-Takers | | Never-Takers | | Compliers | |
|---|---|---|---|---|---|---|---|---|
| | Mean | SD | Mean | SD | Mean | SD | Mean | SD |
| Age | 28 | 6.93 | 27.83 | 6.7 | 27.76 | 6.37 | 27.67 | 8.16 |
| Married | .22 | 0.41 | 0.25 | .44 | 0.20 | 0.41 | 0.19 | 0.41 |
| On Govt Aid | .0053 | 0.07 | 0.00 | 0 | 0.00 | 0.00 | 0.01 | 0.10 |
| From City | .32 | 0.47 | 0.26 | .44 | 0.28 | 0.46 | 0.37 | 0.58 |
| Household Size | 3.9 | 2.32 | 3.75 | 1.9 | 3.88 | 2.73 | 4.08 | 2.96 |
| New to Sex-Service | .087 | 0.28 | 0.04 | .19 | 0.24 | 0.44 | 0.11 | 0.32 |
| 2Y in Sex-Service | .65 | 0.48 | 0.68 | .47 | 0.68 | 0.48 | 0.62 | 0.52 |
| Prior Job Pure Service | .0079 | 0.09 | 0.00 | 0 | 0.00 | 0.00 | 0.02 | 0.11 |
| Prior Job Sex-Service | .47 | 0.50 | 0.55 | .5 | 0.28 | 0.46 | 0.43 | 0.50 |
| Prior Job Manufacturing | .14 | 0.35 | 0.12 | .32 | 0.16 | 0.37 | 0.16 | 0.37 |
| On Contraceptives | .12 | 0.33 | 0.15 | .36 | 0.24 | 0.44 | 0.09 | 0.29 |
| In Debt | .63 | 0.48 | 0.60 | .49 | 0.64 | 0.49 | 0.64 | 0.47 |
| Below Median Sex Work | .7 | 0.46 | 0.70 | .46 | 0.60 | 0.50 | 0.71 | 0.46 |

Note: This table profile characteristics for compliers, always-takers, and never-takers of the incentives intervention in the incentives arm. It follow the steps outlined in the compliers analysis in Marbach and Hangartner (2020) to profile characteristics for compliers, always-takers, and never-takers. Because the intention of the incentives task was to redirect worker effort to service work, we define compliance as follows: compliers are participants in the incentives arm who worked a full shift in workday prior to a given follow-up survey. We define always-takers as control arm participants who worked a full shift in workday prior to a given follow-up survey. We define never-takers as participants in the incentives arm who did not work a full shift in workday prior to a given follow-up survey. Note that since we did not conduct daily surveys with participants, measuring completion rates of shifts per day between follow-up surveys is not possible.

Table A3: Reported Reasons for Lack of Incentives Task Participation

| | Frequency | Percent | Unique Clusters |
|---|---|---|---|
| Busy | 20 | 22.47 | 19 |
| Customers denied | 17 | 19.10 | 15 |
| Did not want to ask customers | 17 | 19.10 | 11 |
| Supervisor denied | 15 | 16.85 | 11 |
| Not interested | 8 | 8.989 | 8 |
| Forgot | 6 | 6.742 | 5 |
| Low customer flow | 3 | 3.371 | 3 |
| Drunk | 2 | 2.247 | 1 |
| Out of town | 1 | 1.124 | 1 |
| Total | 89 | 100 | 53 |

Note: This table tabulates statistics for reported reasons for not submitting the incentives task for participants in the incentives arm throughout the intervention period. Tabulations are by participant-survey round. The survey question allowed for an open-ended response and translators categorized them into the reason categories above. The first column displays reported reasons. The second column counts frequencies for each reason. The third column calculates percentages for each reason. The fourth column counts study clusters in which each reason was reported.

Table A4: Reported Conjecture of Study's Goal in the Incentives Arm

| | Percent |
|---|---|
| No idea | 50 |
| Learn about labor and/or health conditions | 19 |
| Educate us about our health | 12 |
| Support us | 5 |
| Reduce health risks | 5 |
| Reduce sex work | 3 |
| Learn about venue customer traffic and satisfaction | 2 |
| Assess sexual health knowledge | 1 |
| Reduce violence | 1 |
| Assess sexual health conditions | 0 |
| Educate workers | 0 |
| Support us | 0 |
| Total | 100 |

Note: This table tabulates reported conjectures of the overall study's goal for participants in the incentives arm. Tabulations are by participant. The survey question allowed for an open-ended response and translators categorized them into the conjecture categories above. The first column displays reported conjectures. The second column calculates percentages for each conjecture.

Table A5: Robustness for Regression of Sex Work on Intervention Status for Incentives vs. Control in the Incentives Arm

| **Proposals** | | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| | 2W Client Proposals | 2W Client Proposals | 2W Client Proposals |
| Incentives | 0.55** | 0.56** | 0.55** |
| | (0.24) | (0.24) | (0.23) |
| Round FEs | Yes | Yes | Yes |
| Clustered SEs | Yes | Yes | Yes |
| Controls | No | No | Yes |
| *N* | 731 | 731 | 717 |
| **Social Desirability** | | | |
| | (1) | (2) | (3) |
| | 2W Sex Work Hours | 2W Sex Work Hours | 2W Sex Work Hours |
| Incentives | -0.37*** | -0.36*** | -0.35** |
| | (0.14) | (0.13) | (0.13) |
| Round FEs | Yes | Yes | Yes |
| Clustered SEs | Yes | Yes | Yes |
| Controls | No | No | Yes |
| *N* | 719 | 719 | 705 |
| **Winsorized** | | | |
| | (1) | (2) | (3) |
| | 2W Sex Work Hours | 2W Sex Work Hours | 2W Sex Work Hours |
| Incentives | -0.37*** | -0.36*** | -0.35*** |
| | (0.13) | (0.13) | (0.13) |
| Round FEs | Yes | Yes | Yes |
| Clustered SEs | Yes | Yes | Yes |
| Controls | No | No | Yes |
| *N* | 731 | 731 | 717 |

Note: This table displays robustness checks for our main estimated effect on sex work effort (hours) for incentives vs. control arm participants throughout the course of the 1-month intervention period. Regressions are at the participant-survey round level. In first panel, *2W Sex Work Proposals* is the reported number of sex work proposals since the last follow-up survey. In second panel, we drop the 3% of participants who report at endline that their perception of the study's goal was to decrease sex work. In third panel, the bottom 1st and top 99th percentile of outcome data are winsorized. Standard errors clustered are the venue level are indicated in parentheses. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Sex Work Cross Price Labor Supply Elasticity in the Incentives Arm: Inverse Hyperbolic Sine Outcome Transformation

| | (1) FS: ln Wage | (2) 2SLS: IHS Sex Hours | (3) OLS: IHS Sex Hours |
|---|---|---|---|
| Incentives | 0.22*** | | -0.13** |
| | (0.05) | | (0.05) |
| ln Wage | | -0.52** | |
| | | (0.23) | |
| Round FEs | Yes | Yes | Yes |
| Clustered SEs | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes |
| $N$ | 679 | 679 | 711 |

Note: This table displays a robustness check for our main estimated sex work cross price elasticity on sex work effort (hours) using data on incentives vs. control arm participants throughout the course of the 1-month intervention period. Regressions are at the participant-survey round level. *ln Wage*: ln of total reported service work income since last follow-up survey divided by the number of days worked since the last follow-up survey; for incentives arm, this includes incentives task earnings. *IHS Sex Hours* is the inverse hyperbolic sine transformation of the reported number of sex work hours since the last follow-up survey. Standard errors clustered are the venue level are indicated in parentheses. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Effects on Sex Work in the Incentives vs. Cash Transfer Arms

| | (1) 2W Sex Work Hours | (2) 2W Sex Work Hours | (3) 2W Sex Work Hours |
|---|---|---|---|
| Incentives | -0.35** | -0.35** | -0.33** |
| | (0.14) | (0.14) | (0.14) |
| Round 3 | -0.05 | -0.05 | -0.05 |
| | (0.09) | (0.09) | (0.09) |
| Outcome at Baseline | | 0.12** | 0.12** |
| | | (0.05) | (0.06) |
| Age | | | 0.00 |
| | | | (0.01) |
| Primary School Complete | | | -0.99*** |
| | | | (0.32) |
| Married | | | 0.12 |
| | | | (0.19) |
| Baseline Earnings | | | -0.00 |
| | | | (0.00) |
| Constant | 2.92*** | 2.58*** | 3.46*** |
| | (0.12) | (0.19) | (0.57) |
| Round FEs | Yes | Yes | Yes |
| Clustered SEs | Yes | Yes | Yes |
| Controls | No | No | Yes |
| $N$ | 694 | 694 | 686 |

Note: This table displays the estimated effect on sex work effort (hours) for incentives vs. unconditional cash transfers arm participants throughout the course of the 1-month intervention period. Regressions are at the participant-survey round level. *2W Sex Work Hours* is the reported number of sex work hours reported since the last follow-up survey. Standard errors clustered are the venue level are indicated in parentheses. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Regression of Sex Work on Intervention Status for Unconditional Cash Transfers vs. Control in the Incentives Arm: Winsorized

| | (1) 2W Sex Work Hours | (2) 2W Sex Work Hours | (3) 2W Sex Work Hours |
|---|---|---|---|
| UCTs | -0.07 | -0.04 | -0.04 |
| | (0.13) | (0.12) | (0.12) |
| Round FEs | Yes | Yes | Yes |
| Clustered SEs | Yes | Yes | Yes |
| Controls | No | No | Yes |
| $N$ | 703 | 703 | 685 |

Note: This table displays the estimated effect on sex work effort (hours) for incentives vs. control arm participants throughout the course of the 1-month intervention period. Regressions are at the participant-survey round level. Bottom 1st and top 99th percentile of outcome data are winsorized. *2W Sex Work Hours* is the reported number of sex work hours reported since the last follow-up survey. Standard errors clustered are the venue level are indicated in parentheses. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Effect of Permanent Income on Incentives Task Participation in the Incentives Arm

| | (1) Submissions | (2) Submissions |
|---|---|---|
| 2W Experienced Shock | 161.71* | |
| | (84.54) | |
| HH Members Earn Income | | -53.93** |
| | | (21.25) |
| Constant | -0.81 | 275.71* |
| | (55.45) | (144.39) |
| Clustered SEs | Yes | Yes |
| Controls | Yes | Yes |
| $N$ | 188 | 187 |

Note: This table displays the heterogeneous estimated incentives task participation effect among participants in the incentives arm for participants who are described by the given measures of permanent income at baseline. Regressions are at the participant level. *2W Experienced Shock*: If the participant reported experiencing a shock in the 2 weeks prior to the baseline survey. We use this to proxy for low permanent income status. *HH Members Earn Income*: The number of household members in the participant's household at baseline who she reports earns a steady income. We use this to proxy for high permanent income status. Standard errors clustered are the venue level are indicated in parentheses. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Effects on Health in the Incentives Arm

| | (1) % Client Condom | (2) Thinks has STI | (3) Health Site Visits |
|---|---|---|---|
| Incentives | -0.01 | 0.03 | 0.00 |
| | (0.01) | (0.05) | (0.02) |
| Constant | 1.03*** | 1.16*** | 0.03 |
| | (0.03) | (0.28) | (0.03) |
| Round FEs | Yes | Yes | Yes |
| Clustered SEs | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes |
| $N$ | 685 | 717 | 717 |

Note: This table displays the estimated effects to various health measures for incentives vs. control arm participants throughout the course of the 1-month intervention period. Regressions are at the participant-survey round level. *% Client Condom* is the percent of sex work clients with whom the participant reporting using a condom since the last participant follow-up survey; this variable excludes participant-survey rounds in which 0 sex work clients were reported since the last participant follow-up survey. *Thinks has STI* is if the participant reported thinking she had any STI on a Likert scale of 1-5 at the time of a given participant survey. *Health Site Visits* is if the participant reported visiting a health facility since the last participant follow-up survey. Standard errors clustered are the venue level are indicated in parentheses. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Effects on Financial Conditions and Decisions in the Incentives Arm

| | (1) Income | (2) Consumption | (3) Debt Status | (4) Debt: Medium | (5) Debt: Highest |
|---|---|---|---|---|---|
| Incentives | 45.74*** | 20.98 | -0.03 | 0.05* | -0.12** |
| | (12.32) | (13.42) | (0.04) | (0.03) | (0.06) |
| Constant | 217.34*** | 149.75*** | 1.12*** | 0.07 | -0.11 |
| | (28.17) | (37.33) | (0.10) | (0.06) | (0.13) |
| Round FEs | Yes | Yes | Yes | Yes | Yes |
| Clustered SEs | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes |
| $N$ | 658 | 717 | 717 | 414 | 414 |

Note: This table displays the estimated effects to various measures of financial conditions and decisions for incentives vs. control arm participants throughout the course of the 1-month intervention period. Regressions are at the participant-survey round level. *Income* is the participant's total estimated earning across all sources since the last participant follow-up survey, summing up non-service or -sex work earnings in the last 2 weeks, fixed service work earnings in the last 2 weeks, reported variable service work earnings in the last 2 weeks, and the product of the number of sex work clients and the accepted price of sex for the most recent accepted sex work client. *Consumption* is the participant's total spending across spending categories since the last participant follow-up survey. *Debt Status* is the participant's reported debt status at the time of the given participant survey (yes/no). *Debt: Medium/Highest* is the participant's reported range of debt in USD conditional in reporting being in debt, where categories are as follows: lowest (0-500 USD), low (501-1000 USD), medium (1001-1500 USD), high (1501-2000 USD), highest (2000+ USD). Standard errors clustered are the venue level are indicated in parentheses. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A12: Effects on Accepted and Rejected Sex Work Clients Risk in the Incentives Arm

| | Accepted | | | Rejected | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | HIV | STI | Violent | HIV | STI | Violent |
| Incentives | 0.10* | 0.10* | -0.01* | 0.15 | 0.18 | -0.08 |
| | (0.06) | (0.06) | (0.00) | (0.11) | (0.11) | (0.05) |
| Constant | 0.94*** | 1.00*** | -0.01 | 2.15*** | 1.76*** | 0.18 |
| | (0.14) | (0.16) | (0.01) | (0.65) | (0.61) | (0.11) |
| Round FEs | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered SEs | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| $N$ | 683 | 683 | 683 | 461 | 461 | 461 |

Note: This table displays the estimated effects to participants' perceptions of various risk measures for their most recent accepted and rejected sex work clients for incentives vs. control arm participants throughout the course of the 1-month intervention period. Regressions are at the participant-survey round level. Columns 1-3 focus on the most recent accepted sex work client and thus excludes participant-survey rounds with no accepted sex work clients. Columns 4-6 focus on the most recent rejected sex work client and thus excludes participant-survey rounds with no rejected sex work clients. *Acc/Rej: HIV* is the participant's perception of the likelihood that the most recent accepted/rejected sex work client had HIV at the time of sex on a Likert scale of 1-5. *Acc/Rej: STI* is the participant's perception of the likelihood that the most recent accepted/rejected sex work client had any STI other than HIV at the time of sex on a Likert scale of 1-5. *Acc/Rej: Violent* is the participant's perception of if the most recent accepted/rejected sex work client seemed violent, aggressive, or rude (yes/no). Standard errors clustered are the venue level are indicated in parentheses. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Effects on Rejected Sex Work Clients Characteristics in the Incentives Arm

| | (1) Looks | (2) Age | (3) Poor | (4) Average | (5) Rich | (6) Price Too Low |
|---|---|---|---|---|---|---|
| Incentives | -0.03 | -0.37 | 0.03** | -0.04** | 0.01 | 0.10* |
| | (0.17) | (1.19) | (0.01) | (0.01) | (0.01) | (0.05) |
| Constant | 4.72*** | 46.17*** | -0.00 | 1.02*** | -0.02 | -0.34*** |
| | (0.34) | (3.56) | (0.03) | (0.04) | (0.03) | (0.12) |
| Round FEs | Yes | Yes | Yes | Yes | Yes | Yes |
| Clustered SEs | Yes | Yes | Yes | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| $N$ | 461 | 459 | 461 | 461 | 461 | 461 |

Note: This table displays the estimated effects to participants' perceptions of various characteristics for their most recent rejected sex work clients for incentives vs. control arm participants throughout the course of the 1-month intervention period. Regressions are at the participant-survey round level. Excludes participant-survey rounds with no rejected sex work clients. *Age* is the participant's perception of her most recent rejected sex work client's age. *Poor, Average Income, Rich* is the participant's perception of her most recent rejected sex work client's wealth status, given poor, average, and rich as choices. *Price Too Low* is if the participant reported rejected her most recent rejected sex work client because the price he offered was too low. Standard errors clustered are the venue level are indicated in parentheses. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A14: Workers' Reported Sex Work Disamenities

| Disamenity | N | Percent |
|---|---|---|
| Sexual health risk | 193 | 54 |
| Violence risk | 30 | 8 |
| Violence risk, Sexual health risk | 26 | 7 |
| Stigma, Sexual health risk | 16 | 4 |
| Fear of her family finding out | 14 | 4 |
| Underpaid at service venue | 14 | 4 |
| Stigma | 13 | 4 |
| Fear of her partner finding out | 12 | 3 |
| Fear of her partner and family finding out | 10 | 3 |
| General risk | 5 | 1 |
| Prefers service work | 5 | 1 |
| Sexual health risk, stigma | 5 | 1 |
| Fear of her partner finding out, Sexual health risk | 3 | 1 |
| Sexual health risk, fear of her partner finding out | 3 | 1 |
| Stigma, violence risk | 3 | 1 |
| Total | 358 | 100 |

Note: This table tabulates workers' reported disamenities from sex work. In endline surveys, participants were asked a two-part question on if they were paid more for their service work, then (1) would decrease sex work and (2) if so, why. The reasons for (2) are tabulated above. For the latter, the survey question allowed for an open-ended response and translators categorized them into the disamenity categories above. A total of N=358 participants reported "Yes" to the first part of the question. The first column displays the reported disamenity or set of disamenities. The second column displays the count of participants with reports for each disamenity or set of disamenities. The third column displays the percentage of participants with reports of each disamenity or set of disamenities.

Table A15: Effects on HIV Risk and Sex Work Wages in the Incentives Arm

| | (1) HIV Exposure Risk | (2) Sex Work Wage | (3) Sex Work Wage |
|---|---|---|---|
| Incentives | 0.02* | | 5.28 |
| | (0.01) | | (3.78) |
| HIV Exposure Risk | | 267.97 | |
| | | (268.76) | |
| Round FEs | Yes | Yes | Yes |
| Clustered SEs | Yes | Yes | Yes |
| Controls | Yes | Yes | Yes |
| $N$ | 683 | 664 | 664 |

Note: This table displays the estimated effects to participants' *HIV Exposure Risk* and *Sex Work Wage* for incentives vs. control arm participants throughout the course of the 1-month intervention period. Regressions are at the participant-survey round level. *HIV Exposure Risk* reflects the percentage of HIV transmission risk using data on the participant's perception of the likelihood that her most recent accepted sex client had HIV at time of sex on a Likert scale of 1-5. *Sex Work Wage* is the reported hourly price of sex for the most recent accepted sex work client. Sample size drops from N=683 to N=664 between Columns 1 to 2 and 3 as the latter includes only participant-survey rounds with at least 1 reported sex client. Standard errors clustered are the venue level are indicated in parentheses. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## 11.3 Analysis

### 11.3.1 Projected Health Impacts

We conduct a brief back-of-the-envelope analysis to assess projected health impact were this intervention to be scaled. We leverage HIV and STI incidence rates in the literature for this same population in (Couture et al., 2011) and follow equations 24 and 25 below. Note that since we found minimal impacts of the incentives intervention to the perceived risk profiles of accepted sex work clients in Table A12, we assume similar risk across clients.

$$\frac{Incidence}{Client} = \frac{Incidence}{Worker} \times \frac{Clients}{Partners} \div \frac{Clients}{Year} \tag{24}$$

$$\frac{\Delta Incidence}{Worker} = \frac{Incidence}{Client} \times \frac{\Delta Clients}{Year} \times \frac{NonCondom}{Clients} \tag{25}$$

Using incidence data from (Couture et al., 2011) and our study's data, we estimate a projected 0.3% decrease annual change in HIV or STI incidence per worker. This estimated decrease is modest but validated by the similarly modest sexual health impacts found in similar studies providing financial interventions (Kohler and Thornton, 2012).

### 11.3.2 Compensating Wage Differentials

We further exploit the incentives intervention to calculate a compensating wage differential for fatality risk of around $227 million for a 1 in 500,000 probability of death. To arrive at this estimate, we exploit the randomized assignment of the incentives intervention as an instrument for fatality risk in a 2SLS setup that estimates the impact of fatality risk on sex work wages. We focus on fatality risk as it is the standard measure studied to estimate the value of a statistical life (VSL) (Lavetti, 2023). As demonstrated in Table A14, participants in this study attribute almost 75% of sex work disamenity value to sexual health risks. For this exercise, we thus load sex work disamenity value onto HIV risk in order to translate it to a fatality risk. Table A15 displays suggestive results of the ITT impact of the incentives intervention on HIV exposure risk (column 1). It also displays the 2SLS effects of that exposure on sex work wages (column 2).

We then translate this effect of HIV exposure risk to a fatality risk using the ITT estimates in column 1 of Table A15 and several population statistics. First, we use the outcome measure of HIV exposure risk as our worker-level measure of potential HIV exposure risk. Second, we adjust this worker-level measure of potential HIV exposure risk by non-condom use rates as per follow-up data, HIV transmission rates for unprotected receptive penile-vaginal intercourse (CDC, 2019), and HIV transmission rates for intercourse with protected receptive penile-vaginal intercourse (USAID, 2015). Finally, we multiply these adjusted HIV transmission risk values by the death rate for people living with HIV in Cambodia (Khmer HIV/AIDS NGO Alliance, 2022). Thus, in line with the literature on compensating wage differentials (Lavetti, 2023), we exploit random variation in fatality risk in this setting to estimate the effect of fatality risk on wages.

Our estimates would suggest a potential present discounted VSL on the order of $8 million for 1 in 17,000 probability of death. Estimates in the literature generally range around $10-12 million in the US (Kniesner and Viscusi, 2019). Thus, our calculations would imply a VSL that is smaller than that of the existing estimates in the literature. Theory predicts, however, that estimates for other countries would generally be lower than estimates for workers in the US given the positive income elasticity of the VSL (Kniesner and Viscusi, 2019). Cambodia's GDP's per capita PPP is less than one tenth that of the US, where existing studies have been conducted. In light of this fact, the gap between our suggestive VSL estimate and those in the literature is surprisingly small. This may imply that prices may be *more than* compensating for fatality risk to workers.

The magnitude of our VSL calculation may be informative about the role of current prevailing policy responses sex work. For example, one often-cited motivation to justify sex work bans is that they should be welfare-improving for sex workers as sex workers may not internalize the full set of costs associated with engaging in sex work. Thus, a ban can facilitate internalization by raising the costs. Comparing our suggested VSL

calculation to those in the literature implies that, prices for sex—and thus sex workers—may internalize the costs of sex work, even independent of bans. To the extent that policies can be leveraged to facilitate sex workers better internalizing the costs of market participation, our suggestive VSL calculations imply that alternatives to bans may be more appropriate tools to align worker valuations and market risk.