Outside Options and the Supply of Sex Work

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Abstract

Despite near-global bans on sex work, a $180 billion market for sex employs workers who often report preferring sex work's amenities over those of other available work. This suggests that bans may not align with workers' valuations of sex work. In this paper, we estimate how workers trade off sex vs. non-sex work using a cluster RCT in Cambodia that increases the relative returns to non-sex work. We find that incentivizing workers' outside option increases overall earnings by 13%, and in turn, decreases sex work by 13%, relative to both pure control and unconditional cash transfer arms. This suggests that the incentives' price effects are not driven by income effects. We use these reduced form effects to estimate sex (-0.58) and non-sex work (0.46) labor supply elasticities. We also structurally estimate the relative disutility of sex work in a model of worker labor supply choice. Both exercises validate that sex work's relative disutility is larger than non-sex work, but well-compensated relative to other risky jobs. This suggests that sex workers may be internalizing the costs of market participation, even without the need for bans. Thus, our results demonstrate that, first, incentivizing outside options decreases sex work supply, and second, near-universal bans may be mismatched with worker valuations.

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1 Introduction

There are an estimated 52 million sex workers globally (Scelles Foundation, 2011). The total market for sex is estimated at $180 billion (International Union of Sex Workers, 2024). Sex work is an active area of policy intervention owing to numerous concerns, such as serious health risks and a lack of agency amongst workers. It is one of few professions that is near-globally banned, ostensibly due to these significant disamenities. In contrast, workers choosing it over other available work options report that they value not only its attractive pay, but also its *amenities*, like flexible pay and scheduling (Benoit et al. (2021); Maher et al. (2012)). A large labor literature shows that the combination of pay and amenities can dictate workers' labor supply choices and that this is especially true for women (Mas and Pallais (2017); Caldwell and Oehlsen (2018)).[1] Therefore, measuring sex work vs. "traditional", non-sex work labor supply decisions can inform our understanding of how vulnerable workers value alternative work arrangements in labor markets and respond to prevailing policy. However, given the highly sensitive nature of the topic and illicit nature of the market for sex, causal evidence is limited.

In this paper, we use a cluster RCT to study how altering the relative returns to sex work vs. an outside option affects labor supply decisions between sex and non-sex work as well as subsequent health and financial decision-making and conditions. We recruited a sample of sex workers who engage in both traditional work (in the form of service work in venues like bars, beer gardens, restaurants, and karaokes) and sex work. We randomized workers to receive either service work incentives, an unconditional cash transfer, or neither. We find that the incentives increase service work, measured via incentive task effort as well as an increase in earnings from and time devoted to service work. We also find that this increase is associated with a meaningful decrease in sex work. Participants in the incentives arm also substituted to more remunerative sex work clients, increased overall income, and decreased debt levels. An unconditional cash transfer, of similar value to average earnings for those offered the service work incentives, had null effects across these outcomes.

In order to translate these reduced form estimates into a measure of the relative disutility of sex work, we then combine these findings with a labor supply model. In the model, a worker chooses levels of effort to devote to sex vs. non-sex work. Relative to the non-sex work, sex work earns more income but also comes with higher disutility. This is the key trade-off faced by the worker. The model shows us that the incentives could affect sex work decisions via three possible mechanisms: price effects, income effects, or targeting based on incentives take-up. Our data show that the incentives' decreases to sex work are consistent with a price effect, rather than an income effect or improved transfer

[1] The value of sex work's amenities alongside its pay may explain why unconditional cash transfers have led to limited effects on the supply of sex in population data (Richterman and Thirumurthy (2021)

targeting. We thus interpret our empirical estimates to reflect the disutility of sex work relative to traditional work. Accordingly, we estimate, first, an own price labor supply elasticity for non-sex work and, second, a cross price labor supply elasticity between sex and non-sex work. Comparing the estimates' signs and magnitudes suggests that workers view the two types of work as substitute. It also implies the relative disutility of sex work outweighs that of non-sex work. We then use the model to structurally estimate the relative disutility of sex work, which is approximately 8. This implies that the marginal disutility is positive and increase. Finally, we estimate the compensating wage differential for sex work fatality risk and its suggestive figure for the value of a statistical life. A comparison to existing findings suggests that the market's price for sex may sufficiently compensate workers for this large relative disutility of sex work. These results combine to suggests that the prevailing policy response, bans to sex work, may be misaligned with worker valuations.

To design this experiment, we recruited a sample of 600 female workers across 300 service venues in Phnom Penh, Cambodia who do part-time service work and part-time indoor (non-street-based) sex work. Their service work is representative of more traditional work, involving daily service shifts in venues like bars, beer gardens, restaurants, and karaokes. Sex work clients are typically met in service venues during work hours while sex work takes place offsite, normally at hotels and guesthouses. Several aspects of this population make it attractive for our study. First, since an estimated 80% of sex workers are indoor (non-street-based), 80% are part-time, and 80% are female, our study sample is highly representative of sex workers across geographies (International Union of Sex Workers (2024); Scelles Foundation (2011); Cunningham and Kendall (2011)). Second, with about half of these workers' income earned from service work and the other half earned from sex work, their daily labor supply decisions embed our parameter of interest - the labor supply elasticity for sex vs. non-sex work. Third, validated by this study's qualitative data on workers' dynamics with service venue managers, law enforcement, and sex work clients, the intensive margin between sex vs. non-sex work is most relevant to these workers' daily labor supply decisions.[2] These facts combine to make this set of recruitees an ideal population to select for this experiment.

For a month-long experimental period, venues employing study participants were randomly assigned to one of two treatment arms or a control arm. Participants working in venues assigned to the first treatment arm were invited to participate in a service work incentives program which offered an opportunity to earn bonus weekly service work pay for completion of a task designed to mimic both their daily service work duties and

[2]For example, as is common in commercial sex markets, condom use rates with sex work clients are relatively high and repetition of sex work clients is low (Phalkun, 2022). This implies that partnerships between sex workers and their clients are short, with little trust built such that condom use rates are likely to change in response to this intervention.

earnings structure. Our baseline data and industry reports (Oxfam (2019)) show that these are servicing customers and customer-based tips, respectively. We designed was an incentives task to mimic the time, risk, and payment structure in the average service work job: a venue customer survey. This is a common customer-based service worker duty, like customer surveys sponsored by alcoholic beverage companies which hire female service workers to conduct market research in such venues (McKinsey and Company (2020)). Participants working in venues assigned to the second treatment arm received a fixed, unconditional weekly cash transfer equivalent to expected average earnings, as per pilot data, of those in the incentives treatment arm. The control group was a pure control, with no incentives or cash payments offered. We collected biweekly worker panel data before, during, and after the experimental period and merged it with the study's metadata data on incentives participation for analysis. We also conducted venue surveys to assess any adverse events, like worker employment status and conditions.

We find meaningful decreases in sex work in the incentives arm relative to *both* the pure control and unconditional cash transfer arms. We find, first, an increase in service work. This is demonstrated by 65% take-up among those offered the incentives. Relative to the pure control arm, this take-up translates to a 20% average increase in earnings from and 9% increase in time devoted to daily service work. We thus estimate an own-price labor supply elasticity for service work of 0.46. We find, second, that this increase in service work is associated with a 13% decrease in sex work relative to the pure control arm. We thus estimate a cross price labor supply elasticity for sex work of -0.58. Put together, these elasticity estimates imply that sex work has higher disutility relative to non-sex work. These reduced form labor supply effects are surprising given both the complementarity of service and sex work in this setting and the limited effects found in similar studies which test supply-side interventions to decrease sex work (Jones and Gong (2021); Gong et al. (2019)).

The incentives intervention's labor supply effects have downstream impacts to workers' financial and health decision-making and conditions. Relative to the pure control arm, it also led to an increase in overall estimated income by around 13%, decreases in average debt levels across workers, and substitution to more remunerative sex work clients. In a brief back-of-the-envelope calculation to estimate potential health impacts of a scaled intervention, we combine data on population-level incidence with our experimental data to project a modest 0.3% decreased incidence of sexually-transmitted infections per worker. Heterogeneity analysis shows that targeting the incentives to sex workers using contraceptives or with more sex work exposure may increase the intervention's impact. Analysis of labor supply, financial, and health outcomes in the unconditional cash transfer arm finds no effects.

We write a model of labor supply choice to structurally estimate the relative disutility of sex work. The model features a worker who chooses levels of effort to devote to sex

vs. non-sex work. Sex work is more highly compensated but also may be associated with higher disutility. The worker determines their effort allocation according to this trade-off between the higher paying sex work but potentially lower disutility non-sex work. The model indicates that a negative transfer effect on sex work can be decomposed into three broad mechanisms. First, a transfer can have price effects via increasing the marginal utility of non-sex work relative to sex work. Second, a transfer can have income effects via decreasing the marginal utility of consumption by increasing income. Third, a transfer can have targeting effects which allocate resources toward those who are more likely to decrease sex work, i.e. targeting based on take-up. Two reduced form effects facilitate identifying the mechanisms that explain our findings. First, we do not find a decrease in sex work in the study's unconditional cash transfer arm. Second, we do not find that there are greater decreases in sex work as incentives task participation increases in the incentives arm. Hence, our empirical results are consistent with the incentives' negative effect on sex work being driven by price effects, rather than income effects or targeting based on take-up.

Structurally estimating the model's parameter for the relative disutility of sex work per day verifies that it is, on average, around 8. This estimate is positive and greater than 1. We interpret it to mean that the disutility of sex work exceeds that non-sex work and increases with levels of sex work. However, estimation of compensating wage differentials suggests that sex work's higher disutility is already compensated for by prices in the market. We estimate a present discounted value of a statistical life estimate of roughly $8 million for a 1 in 17,000 probability of death. These estimates are not statistically significant at conventional levels so we interpret them with caution. However, the average in the literature is around $10-12 million for US workers (Kniesner and Viscusi, 2019). After a PPP adjustment, we might expect a estimate in this study's setting on the order of $1-2 million. Thus, our estimate may suggest that the market's price of sex already compensates for the large relative disutility of sex work. This may potentially imply that bans aimed at encouraging sex workers to internalize the costs of sex work may not be necessary. Overall, our results demonstrate that, first, increasing the returns to outside options can decrease sex work, and second, the near-global bans to sex work may be misaligned with workers' valuations of sex work.

We provide the first labor supply estimates on the decision to earn from sex work vs. non-sex work. Our estimates for non-sex work (0.46) and sex work (-0.58) labor supply elasticity fall within the 0.23-0.59 range, magnitude-wise, of elasticities estimated using labor force data for women in more traditional work settings in western countries (Bargain and Peichl (2016); McClelland and Mok (2012)). These comparisons imply that female labor in a developing country setting is as elastic as labor for their demographic counterparts in developed countries. Meanwhile, our estimates are on the smaller end of those found in a broad literature which exploits quasi-random and experimental vari-

ation in settings with daily variation in earnings and labor supply (0.5-1.5).[3] This is unsurprising for both our estimates given that we estimate Marshallian rather than the Hicksian or Frischian elasticities of focus in this literature. Further, this literature also focuses on jobs for which labor supply is less employer-dictated than service work, significantly less risky than sex work, and relatively male-dominated. Indeed, our sex work estimate is closer in magnitude to those for other higher-risk labor, 0.06-0.21 (Stafford (2015); Nguyen and Leung (2013)).

Our reduced form effects on sex work labor supply also allow us to be among the first to estimate compensating wage differentials for women and thus provide policy implications on sex work criminality. Our estimates of compensating wage differentials are larger than expected given those in the literature which study US and developing individuals (Lavetti (2023); Ito and Zhang (2020); Kniesner and Viscusi (2019); León and Miguel (2017); Kremer et al. (2011)). This literature focuses on high-risk jobs where fatality risk is salient. In our setting, fatality risk is relevant but also accompanied by other meaningful disamenities, like criminalization and social stigma. This may explains our differing estimates. It also allows us to use our estimates to inform the broadly debated and studied topic of sex work bans (Ciacci and Sviatschi (2022); Cameron et al. (2021); Cunningham and Shah (2018); Bisschop et al. (2017)). While the existing research focuses on primarily health and crime outcomes, we extend and complement it by being among the first to assess the relationship bans have with worker labor supply and valuations. Thus, our paper advances research on female labor supply choice by being among the first to study how it interacts with the large and risky market for sex.

This paper also contributes to the literature on sex work and transactional sex by testing and directly comparing two interventions and being among the first to find meaningful decreases to sex work. We test two novel approaches – "outside option" incentives and unconditional cash transfers – to address the well-documented income-risk trade-off faced by individuals involved in sex work. Given that existing research has shown that (risky) sex work receives a significant price premium[4], it is perhaps not surprising that health risk-related conditional cash transfers have generally found minimal decreases to the overall supply of sex work (Gong et al. (2019); Kohler and Thornton (2012); de Walque et al. (2012)). Meanwhile, Jones and Gong (2021) recently tested a savings incentive in similar populations which resulted in a decrease to sex work as a shock-coping response. This suggests that targeting the income side of the income-risk trade-off may

[3]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).

[4]On the one hand, studies show a substantial price premium for risk (Jakubowski et al. (2016); Elmes et al. (2014); Gertler et al. (2005)) and also that women's engagement in sex work and transactional sex is partly driven by economic needs (Gichane et al. (2022); Robinson and Yeh (2011)). On the other hand, 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)).

be promising. Our findings are therefore notable because they demonstrate how an unconditional, financial program can decrease the supply of sex. Our experimental design further facilitates us directly testing which of these strategies might better impact sex work labor supply.

The rest of the paper is organized as follows. Section 2 provides an overview of the setting and sample population as well as our motivation for the experiment. Section 3 describes the experimental design. Section 4 details the data sources, data collection methods, and the empirical strategies employed. Section 5 presents the main results. Section 6 develops a model of labor supply choice. Section 7 investigates mechanisms highlighted in the model. Section 8 estimates labor supply elasticities. Section 9 conducts a structural estimation exercise. Section 10 presents other results. Section 11 concludes.

2 Setting

We start by characterizing the supply and demand sides of the market for sex in this setting. We then describe the nature of the sex work transactions. These three aspects of this market for sex combine to make it an ideal setting for this study.

2.1 Supply of Sex

We recruit a representative sample of service venue-based female sex workers in Phnom Penh, Cambodia. Recruited workers had a mean age of 28, 20% were married, most were primary school graduates, 33% were from urban areas, average household size was 4, and 65% were in debt at baseline. In terms of prior work experience, 47% 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. cafe).[5] For 27% of the sample, the service venue in which they were recruited was their first employer. In terms of sexual health, 1% were on HIV prevention medicine and 12% used hormonal contraceptives. These statistics are all broadly consistent with country-wide population statistics on service venue-based female sex workers, in exception to the sample's average age (Phalkun, 2022). Country-wide statistics have a lower average age, owing to the national survey's minimum recruitment age of 15.[6]

[5]Manufacturing may be considered the logical outside option for this population of women in this setting. While shifting from manufacturing to the service sector is common, shifting from the service sector to manufacturing is rare. Cambodian minimum wage in the manufacturing sector as of 2022 was around 200 USD, which is around 50% of average wages in the sex work-adjacent service sector, as detailed in the next paragraph. This simple earning potential comparison may explain why shifts from the service sector to manufacturing are rare.

[6]Our study was approved by Brown University's and local IRB to recruit participants who were 18 years and above.

Workers typically have daily, night-time shifts compensated via a combination of variable and fixed earnings. At baseline, worker reported typically working shifts in their service venues from 5:00pm to 2:00am. 95% of the sample reported working daily shifts. We breakdown average fixed and variable income for our sample to provide a picture of their estimated income. At baseline, estimated biweekly earnings was around 391 USD. Just less than half of this biweekly earnings estimate, 190 USD, came from service work. Of this service work earning share, 67 USD (17% of total biweekly earnings) came from fixed monthly wages and 123 USD (31% of total biweekly earnings) came from variable earnings, e.g. communal venue and individual venue customer tips. The other half, 201 USD (51% of total biweekly earnings), came from sex work. This estimate comes from multiplying the baseline reported average price of accepted sex work, 67 USD, by the baseline reported average number of accepted sex work clients, 3. Thus, as is common for female service workers, workers in our sample reported earning more from variable earnings than fixed wages.

2.2 Demand for Sex

Sex work clients in this setting typically initiate interaction with sex workers in service venues. The sex work clients thus are a subset of service venue customers. However, not all service venue customers become sex work clients. At baseline, workers received a proposal for sex work on 36% of nights, negotiate with potential sex work clients for an average of 12 minutes on aspects of the transactions (like price and condom use), and eventually accepted only 60% of proposals. Participants perceived that accepted sex work clients on average were around 39 years old, 88% were thought to be of average income level (choices offered were as follows: low, average, high), and 20% were foreign. As we were unable to survey male sex work clients, these statistics are all from our participant surveys with female workers. However, these statistics are broadly consistent with existing descriptive data on typical sex work clients across geographies (International Union of Sex Workers, 2024).

2.3 Sex Work Transactions

The potential for workers to choose sex work over service work makes this setting especially well-suited for our study. Workers report an average of 3 biweekly transactions at baseline. On a given workday, if the worker receives, negotiates on, and accepts a proposal before the end of their service venue shift, there is potential for direct trade off between service and sex work if a worker chooses to leave their shift early. In focus group discussions, we were informed that this is most likely to occur toward the end of workers' shifts, between 12:00am - 2:00am. At the same time, 9:00pm - 2:00am is normally the

busy window, in which customer flow in the service venue peaks. Therefore, 12:00am - 2:00am is also critical income-generating time for a service worker to be in the venue, e.g. to earn from venue customer tips.

The setting is also attractive to conduct this study because institutional factors ensure that service and sex work cannot take place simultaneously. Sex work does not take place at venues given that sex work is criminalized in Cambodia. Thus transactions need to take place in environments that are discreet from law enforcement. In focus group discussions, we were informed that a worker and her accepted sex work client will typically travel to a nearby hotel for the transaction to take place, and thereafter, she is unlikely to return to the service venue to complete her shift. This factor is a critical motivation for our intervention task.

The sex work transactions in this setting should be understood as more consistent with short-term, commercial sex transactions than otherwise more steady financially-dependent relationships. In general, sex work transactions can be portrayed as lying on a spectrum, with transactions with non- or minimally-repeating commercial partnerships on one end and long-term sexual and economic partnerships on the other end. The latter have been the subject of various related studies (Thirumurthy et al. (2021); Dupas (2011)). We categorize the partnerships of focus in this study as closer to the former end of the spectrum (short-term commercial partnerships). During focus groups discussions, participants reported that if sex work clients repeated, this generally happened only 2-3 times. In the same vein, 95% of participants reported condom use 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 suggest there exists scope for workers to negotiate condom use with commercial sex work clients.

3 Experiment

To implement this experiment, we developed a partnership with a local survey firm and a local NGO which provides sexual and reproductive health services for female sex workers. Our team approached service venue workers at venues during work hours for recruitment for this 1-month cluster RCT.

We recruited 600 female workers employed across 300 service venues and participating in the sex trade. Eligibility criteria were being at least 18 years old, having been compensated for sex at least once in the month prior to enrollment, and receiving a fixed wage from service venue employers.[7] The sex work client criteria served as a proxy for

[7]The age requirement was set to meet the requirements of our IRB approval.

participating in sex work, consistent proxies used in the literature (Thirumurthy et al., 2021). Finally, the fixed wage criteria served as a proxy for working at a service venue rather than an effective brothel: venues that behave more like brothels typically do not pay any fixed wages and thus offer limited intensive margin for workers to trade-off between service and sex work.

The unit of randomization was the service venue. Service venue include bars, beer gardens, restaurants, and karaokes. The venue, rather than individual, was chosen as the unit of randomization in order to avoid potential spillover effects between participants recruited within a single venue. Venues employing study participants were randomly assigned to one of two treatment arms or a control arm. Based on power calculations, we recruited a maximum of 2 workers per venue.

Venues employing study participants were randomly assigned to one of two treatment arms or a control arm. Participants working in venues assigned to the first treatment arm were invited to participate in a service work incentives program which offered an opportunity to earn bonus weekly service work pay for completion of a task designed to mimic their daily service work duties. Participants working in venues assigned to the second treatment arm received a fixed weekly cash transfer equivalent to expected average earnings, as per pilot data, of those in the incentives treatment arm. The control group was a pure control, with no incentives or cash payments offered.

3.1 Study Arms

3.1.1 Control Arm

The control group was a pure control, with no task requirements and no cash transfers, with the exception of survey completion incentives. Control arm participants were requested to take part in the study's three biweekly surveys (baseline, midline, and endline).

3.1.2 Incentives Arm

Participants working in venues assigned to this treatment arm were invited to participate in a service work incentives program which offered an opportunity for participants to earn bonus weekly service work pay for completion of a task. The task was designed to mimic their daily service work duties and earnings structure. In terms of the economic interpretation, the goal was to simulate increasing the relative returns to service vs. sex work, and more broadly, to emulate an increase in service sector wages.

The incentives task was a customer survey. Participants earned an incentive for each valid submission. In specific, participants were invited to administer a short survey with customers which 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 created using Brown University's Qualtrics software, which offered various metadata to ensure accurate records for determining submission eligibility. The incentives earned per customer survey submission were paid weekly. Piloting offered evidence that this intervention was feasible, acceptable, and safe in this population.

We designed this task to have a few key features. First, this task was analogous to female service workers' real work tasks. "Bar promotion" activities like the customer survey task are precisely the type of techniques alcoholic beverage companies hire female service workers to do to conduct market research (McKinsey and Company (2020)); they are also the typical work duty described by female service workers in the Cambodian setting (Oxfam (2019)). Second, given that a major means of income-generation for workers are customer-based tips, this task was able to mimic the workers existing earnings structure.[8] Third, because data collection was carefully designed to collect rich metadata on incentives task participation (like submission dates, timestamps, and geographic coordinates), submission validity was determined directly by the researchers, thereby minimizing opportunity for payments for false submissions.

During enrollment, participants in the incentives arm were briefed on the incentives task and submission process. In addition, participants were instructed 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[9]. In order to allow the incentives task to behave like a self-selection mechanism, participants were given no submission limit. Finally, we priced the incentives to reflect the sex vs. service trade off window as described to us during focus groups and piloting activities. Participants were instructed that submissions before 12:00am would be paid 1.50 USD per submission and submissions after 12:00am would be paid 3.00 USD per submission.

3.1.3 Unconditional Cash Transfer Arm

Participants from venues assigned to the unconditional cash transfer arm received a fixed, unconditional cash transfer equivalent to expected average earnings, as per pilot data, of those in the incentives arm. Like the earnings in the incentives arm, the unconditional cash transfer was paid out weekly.

The motivation for including this treatment arm was twofold. First, since any relative

[8] At baseline, such variable earnings were 31% of total estimated earnings, which was 63% of total service work earnings.

[9] Shift start and end times were collected as a part of the baseline survey. Baseline surveys were conducted following enrollment and prior to revealing study arm assignment in order to avoid misreporting of shift duration. Reported average shift start and end times by participants in this study arm are similar to those reported in other arms, offering little evidence of misreporting.

increase in returns to a labor option decompose into an income and/or substitution effect, we hypothesized that these two treatment arms may have different effects if substitution effects are nonzero. By only providing a cash transfer, one goal of this treatment arm was to isolate only the income effect from the incentives program in the incentives arm. Combined with the results of the incentives arm, the cash transfer arm therefore enabled us to assess if an increase in the relative returns to service work impacts outcomes via the income vs. substitution effect. The closest related existing research tests the effects of offering conditional cash transfer to incentivize safer sex in more generalized populations (Gong et al. (2019); Björkman Nyqvist et al. (2018); de Walque et al. (2012); Kohler and Thornton (2012)). Therefore, including an unconditional cash transfer arm also allows us to compare our findings to the closest studies in the literature.

Second, participation in the incentives task in the incentives arm involved voluntary effort investment to earn income while receipt of the unconditional cash transfer in the unconditional cash transfer arm was homogeneously offered to all of its participants. This means take-up was 100% by default. Thus, the incentives arm could act as a self-targeting mechanism by identifying the characteristics of participants who are potentially more likely to substitute to service work from sex work, i.e. the "marginal substituters". Identifying this type of heterogeneity may be important for targeting a scaled intervention.

4 Data and Empirical Strategy

4.1 Data

The key data used in our analysis came from two sources: metadata on incentives task submissions and biweekly panel data on workers. For incentives task data, we designed a system to collect rich and objective metadata on incentives task participation via Brown Qualtrics. This metadata included fields like dates, timestamps, and geographic coordinates for each submission. For the biweekly panel data on workers, the field team conducted biweekly surveys with all participants enrolled in this study. This included a baseline, midline, and endline survey. Field staff also used Brown Qualtrics for these surveys.

Surveys 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[10], client characteristics, and condom use. For financial activity and consumption, we asked about debt status, amount, and party, as well as consumption in the following categories:

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

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, make up and hair services, debt repayment, and remittances to parents/village. For health, we asked about self-perceived health status, health facility utilization, and experience of symptoms of common STIs.

We also conducted venue supervisor surveys for a random sample of venues assigned to the incentives arm to address concerns about potential adverse impacts to participants' employment status and conditions. These supervisors were surveyed at endline and asked about any workforce changes like hiring, firing, and quitting as well as general thoughts on the incentives task. Venue supervisors did not report any cases of adverse impacts to 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. Participant survey responses were consistent with venue supervisor responses.

4.2 Balance

Tables 1 and 2 shows the balance of demographics and outcomes variables, respectively, at baseline for the incentives vs. control arms. Tables A18 and A19 compare the cash transfer and control arms. Tables A20 and A21 compare the incentives and cash transfer arms. Demographics were generally balanced across study arms at baseline, with the exception of some quantitative imbalances in age which were small. They qualitatively did not impact the composition of ages in the study arms. Outcomes were also generally balanced across study arms at baseline, with the exception of perceived violence risk when comparing cash transfer 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 facts that debt status is not also 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

Our empirical strategy relies on the cluster randomized design of this study. We use an OLS regression to evaluate the effect of service work incentives on participants' labor supply, financial, and health-related decision-making:

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

y_{ict} captures outcomes for participant i in cluster c at survey round t. X_i is a vector of baseline participant characteristics like age, marital status, education, and service work wage. π_t are follow-up survey round fixed effects. We clustered standard errors at the venue level. β estimates of the ITT effect of a given treatment on outcomes due to the intervention.

We use data from the incentives and control arm participants to translate effects on labor supply decisions into labor supply elasticities. To do so, we use the following two-stage least squares (2SLS) regression. The first stage is as follows:

$$Income_{ict} = \kappa + \delta Treatment_c + \nu X_i + \gamma_t + \upsilon_{ict} \tag{2}$$

The second stage is as follows:

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

y_{ict} is ln Service Hours or ln Sex Work Hours for participant i in cluster c at survey round t. $Income_{ict}$ is ln Income from service work for participant i in cluster c at survey round t. X_i is a vector of baseline participant characteristics. γ_t and ρ_t are follow-up survey round fixed effects and standard errors are clustered as explained in the discussion following equation 1.

Two sufficient conditions to interpret 2SLS estimates as causal effects of changes to service work income are that: $E[Income_{ict}\upsilon_{ict}] = 0$ and $corr(Treat_c, y_{ict})|E[Income_{ict}] = 0$. The first condition holds due to the randomized experimental design. The second requires that access to the incentives intervention influences outcomes only through service work income changes. This assumption is standard in labor supply elasticity calculations in the literature (Angrist et al., 2021). We thus interpret the 2SLS estimates of ϕ as the own or cross price elasticity for service work and sex work, respectively.

5 Main Results

5.1 Incentives Arm

5.1.1 Take-up

Overall, we find that the incentives intervention was able to increase *some* workers' effort toward service work. Figure 1 displays the distribution of valid incentives task submissions per worker. Our key takeaway from Figure 1 is that 65% of participants in the incentives arm took up the offered service work incentives. We also note that there were 71 mean valid submissions per worker across all incentives arm participants and an average reported time spent of 3 minutes per submission. Therefore, we estimate a

given worker submitted roughly 2.5 submissions per work day, which would translate to 1 hour spent on the incentives task per work week.

The 65% take-up rate is validated by several robustness checks. First, it is robust to dropping participants who were unable to take up the incentives for exogenous reasons. Appendix Table A22 tabulates reported reasons for non-participation in the incentives task for participants in the incentives arm 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 N=3 participants consistently reported these exogenous reasons for not participating in the incentives task. Second, we also show in Appendix Table A22 that non-participation is not concentrated in specific clusters, meaning that the take-up rate approximates what we may expect in a scaled intervention.

There are additionally two interesting observations regarding the shape of the distribution which demonstrate worker heterogeneity. First, the leftmost bar shows us that 35% of workers had 0 valid submissions, corresponding to the 65% take-up rate noted above. This rate provides evidence against a model in which workers try to maximize income across all income-generating opportunities. This model may ex ante have been considered appropriate given 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 low-effort incentives task. This 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 350-600 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 that there is meaningful heterogeneity across workers.

Figure 2 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 1, we see a sharp departure from the y-axis at 35% and long tail for values in the right half of the figure. We can also observe that around 70% of the values of mean weekly earnings per worker were below the mean across all workers, \$19.66. This mean earnings value (blue line) was similar to the fixed weekly amount paid to participants in the cash transfer arm of \$15 (green line). This similarity facilitates our later analysis which decomposes the intervention's price effects into income and substitution effects.

Figure 3 displays the distribution of submissions over the time. In line with most of the incentives arm participants having night time shifts, we observe that most submissions took place from 4:00pm to 4:00am. The day time submissions reflect the small portion of incentives arm participants who had day time shifts. We also observe a small

bump in submissions at 12:00am, potentially demonstrating a response to our varied pricing scheme that doubled compensation for valid submissions post-12:00am.

Since the intervention was designed with the intention of increasing service work effort, we may hypothesize that increased effort may spillover to increases in workers' service work hours, variable earnings (e.g. tips), and venue customers served. While such spillovers need not be necessary following participation in the incentives task, Table 3 provides accompanying evidence that the incentives-induced effort increase translated to increases in daily service work hours by 0.67 hours (40 minutes), earnings from communal venue tips by 0.50 USD, and biweekly venue customers served by almost 7 customers. These regressions provide corroborating evidence that service work effort indeed increased for incentives arm participants.

5.1.2 Sex Work

Our key finding is that the incentives arm was associated with a meaningful decrease in sex work. Table 5 shows regression results following the specification in equation 1 comparing incentives vs. control arm participants' reported biweekly sex work hours during follow-up surveys. Column 1 shows that incentives arm participants reported 0.38 fewer biweekly sex work hours. Given a baseline reported mean of 3 biweekly sex work hours, this implies an approximately 13% decrease in sex work hours. Columns 2 and 3 of Table 5 show that these results look similar with the inclusion of baseline biweekly sex work hours and demographic controls, respectively.

There are several reasons why this result is surprising. First, these results are consistent with what economic theory would predict for two jobs that are substitutes. However, while workers in this setting *can* substitute their effort between each type of work, it is ex ante non-obvious that they *do* substitute. In fact, given that most workers' sex work clients start as service venue customers, there is evidence that the two types of work are complementary. In addition, given that individuals involved in sex work typically face extremely high marginal utility of consumption, supply-slide interventions that aim to disincentivize sex work have found mixed success: 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) finds limited decreases in response to a conditional cash transfer. Thus, it is surprising that offering incentives for non-sex work, *unconditional* on the resulting level of sex work, would lead 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 nature of the incentives task involves intervening in the interactions between workers and their potential sex work clients. So, there may be concern that the task itself may impact the reported number of sex work hours through altering the

number of sex work client proposals. The direction of the 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. Appendix Table A24 resolves this concern. it shows that, in our follow-up data, we find that incentives arm participants report a 0.55 higher number of 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. While estimating an increase in proposals provides evidence against decreased proposals mechanically decreasing the reported number of sex work hours, it also warrants caution against drawing a strong conclusion that the intervention is unambiguously beneficial to workers since it does appear to increase their pool of potential sex work clients.

Second, we validate that our results are robust to concerns about social desirability. In particular, we may be concerned about social desirability in this setting, especially given the sensitivity of surveying on topics like sexual choices and health. While our pure control group mitigates such concerns to some extent, we may still be concerned if we detect differential social desirability between the incentives and control arms participants, specifically if responses from incentives arm participants are disproportionately more likely to suffer from such bias. We ensure 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 A23 tabulates reported perceptions of the study's goal across participants in the incentives and control arms. Results of this robustness check can be found in Appendix Table A25. They show that the decrease of sex work observed in the incentives arms remains at a similar magnitude and significance level. This robustness check offers confidence that social desirability is not driving our results.

Third, we validate that our results are robust to concerns about using the number of sex work clients as a proxy for sex work hours. While participants in focus group and baseline data reported that 1 hour per sex work client was the benchmark in the market, 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) ITT results if the true hours per client were larger than 1 (smaller than 1). Elasticity estimates would remain unchanged. A more probable bias may occur if those reporting lower prices were working fewer hours and higher price were working more hours. Appendix Table A26 provides evidence against this concern. It shows that our results are robust to winsorizing the 1st and 99th percentile of outcome data. This robustness check offers confidence that measurement error for sex work hours is not driving our results.

5.2 Cash Transfer Arm

The cash transfer arm did not have a statistically significant impact on either service nor sex work labor supply. First, the cash transfer arm was not designed to impact service work effort and Appendix Table A28 validates that this was not the case. Second, in terms of sex work effort, Table 7 shows regression results following the specification in equation 1 comparing cash transfer vs. control arm participants' reported biweekly sex work hours during follow-up surveys. Column 1 of Table 7 shows that the cash transfer arm participants reported a statistically insignificant 0.03 fewer biweekly sex work hours. Columns 2 and 3 of Table 7 likewise show that these results look similar with the inclusion of baseline biweekly sex work hours and baseline characteristic controls, respectively. Table 8 displays regression results comparing reported biweekly sex work hours during follow-up surveys for the incentives vs. cash transfer arm participants. These point estimates are statistically significant and similar in magnitude to those in Table 5.

6 Model

We introduce a model of labor supply choice for two reasons. First, we use this model to formalize our intuition about mechanisms that can explain our results. Second, we use our model to estimate the relative disutility of sex work vs. 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 comes from her total income, y. y is the sum of her initial nonlabor income, y_0, and her labor income. For labor income, she can choose her level of sex work effort, s, which earns her income p_s, and service work effort, t, which earns her p_t. For simplicity, we think of t as effort toward the incentives task given that service work effort tracks with incentives task effort. Her consumption can be written as follows

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

She also experiences utility from leisure time which is captured by

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

The central focus of this study is to determine whether there is additional relative disutility from sex work. We will capture this as follows

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

6.2 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, but will also be impacted by the disutility from sex work. Thus, it can be written as follows

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

For $v(T - s - t) = T - s - t$, implicit differentiation shows that

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

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

For a closed form solution, we can specify worker's utility as follows

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

In $U(y_0, t, s)$, the disutility from sex work effort is captured by $\alpha w(s) = \alpha s^2$.

6.3 Worker's Optimal Choices

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

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

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

6.4 Worker's Response to the Experiment

Our experiment increased the relative returns to service work, i.e. increased p_t. This model allows us to consider the mechanisms contributing to workers' effort allocation responses to an increase in the returns to service work. Service work incentives increased service work effort, $\frac{dt}{dp_t} > 0$. The accompanying decrease in sex work effort can be a result of three mechanisms. First, the decrease in sex work effort can result from an income effect, i.e. $\frac{dy}{dp_t} > 0 \implies \frac{ds}{dy} < 0$. This means 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 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. $\frac{ds}{dp_t}$ or $\frac{ds}{dy}$ decrease with α. This means a transfer can have targeting effects which 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 this experiment, may operate through increasing the marginal disutility of sex work. 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. We now turn to our experimental data to assess the relative importance of each of these potential mechanisms.

7 Mechanisms

In this section, we empirically assess the relative importance of the three mechanisms captured in our model: price effects, income effect, or targeting effects.

7.1 Price Effects

If there are price effects ($\frac{ds}{dp_t} < 0$), then we should observe workers decreasing sex work effort and increasing service work effort. Our main results in Table 5 provide the primary evidence in favor of the price effects mechanism. This implies that sex-to-service substitution was taking place in the incentive arm. An additional test of this mechanism could be conducted in the service-sex work tradeoff window describe in Section 2. To investigate evidence in favor of *direct* substitution during this tradeoff window, we couple the main results in Table 5 with an assessment of whether service work increased and sex work decreased during the tradeoff window in Figure 4.

As discussed, Table 3 provides evidence that the incentives intervention increases service work effort. Figure 4 shows that at least a portion of the critical mass of submissions falls within the tradeoff window. This implies that the service work effort increase may have taken place at least in part during the trade-off window. The pink and blue vertical lines in Figure 4 also show that, if accepting a sex work client during their service work shift (i.e. when the constraint binds), incentives arm participants report departing for sex work on average around 50 minutes later than control arm participants. In a regression on departure time following equation 1, we find a point estimate of 0.82 hours (p-value < 0.01). We note that these two lines are measuring average departure times in slightly different pools of data since the main results in Table 5 show that the average

number of accepted sex work hours decreases amongst incentives arm participants. Put together with the decrease in sex work estimated in Table 5, these results provide evidence of *direct* time substitution from sex to service work. These results provide further evidence in favor of the price effects described in our conceptual framework.

However, we note that direct time substitution does not capture the full story. The inframarginal time (e.g. pre-12:00am), when service venue time is unlikely to be substituted with sex work, also appears to be important. A regression of 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 submit fewer submissions during the inframarginal time (point estimate = -60.1, p-value < 0.01). This implies that incentives task earnings during the inframarginal time also play an important role in workers' decisions to substitution away from sex toward service work.

7.2 Income Effects

If there are income effects ($\frac{ds}{dy} < 0$), then we should observe workers decreasing sex work in response to an increase in income. The experiment's unconditional cash transfer arm facilitates assessing the importance of the marginal utility of consumption mechanism, i.e. income effects. The findings in Table 7 imply that income effects do not purely explain this study's main finding that service work incentives decrease sex work.

An important caveat to this analysis is that it observes income effects in isolation to the intervention, making it a temporary effect which ignores permanent income effects. If participants in the cash transfer 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, i.e. 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 A29. In column 1, a regression of submissions on a baseline variable capturing low initial endowment status (if the participant reported experiencing a shock in the 2 weeks prior to the baseline survey) shows that those with lower initial endowment submit more incentives tasks. Similarly, in column 2, as discussed earlier, a regression of submissions on a baseline variable which proxies for permanent income (number of individuals in the participant's household with a steady income) showed that those with higher permanent income submit fewer submissions. This result exploiting the study's observational data offers some caution in drawing the conclusion that income effects are completely unimportant.

7.3 Targeting

If there are targeting effects ($\frac{ds}{dp_t}$ or $\frac{ds}{dy}$ decrease with α), then we should observe workers doing increasingly less sex work as incentives task earnings increase. If we think of the incentives task as an administrative ordeal, we can interpret it as acting like a self-selection mechanism that facilitates identifying 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 facilitates assessing the importance of the targeting mechanism, i.e. if heterogeneity in income and price effects drives our results.

First, we test for if the incentives were able to facilitate targeting 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 incentives arm participants. Further, we may desire that this outcome responsiveness track with program cost of the participant; in this study, the program cost per participant 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 biweekly earnings.

Second, we test for if the incentives were able to facilitate targeting 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 cash transfer arm participants. Further, we may desire that this outcome responsiveness track with program cost of the participant; as a reminder, in this study, the program cost per participant is captured by individual worker earnings from the incentives task.

We build on the price effects analysis 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 the incentives arm participants only. With this data, we predict potential incentives task earnings for cash transfer arm participants. We define a high type earner as one who is predicted to earn above median earnings of incentives arm participants. This is our measure of worker type. Second, for cash transfer arm participants, we then estimate changes in sex work hours per dollar. For this prediction, we use (1) probability weights on the likelihood of being a high vs. low type earner and (2) actual follow-up data on change in sex work hours per cash transfer dollar. This is our measure of outcome

responsiveness. Third, we fit a polynomial to this data to assess outcome responsiveness by worker type.

Figure 5 shows outcome responsiveness (change in hours per dollar) by biweekly earnings. 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 higher earners are not necessarily the "marginal substititers". Similarly, Figure 6 shows outcome responsiveness (predicted change in hours per dollar) by worker type (probability of being a high type earner). The weakly negative slope of the polynomial as we move 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 the "marginal substititers".

These results provides 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 the individuals who are most likely to substitute away from sex work, i.e. the 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

$$\frac{S}{X} = \sum \frac{s_i}{x_i} \frac{x_i}{X} \tag{13}$$

This can be decomposed as follows

$$\frac{S}{X} = \sum \frac{s_i}{x_i} \frac{x_i}{X} = \sum \left[\frac{s(i)}{x(i)} - avg\left(\frac{s(i)}{x(i)} \right) \right] \frac{x(i)}{X} + avg\left(\frac{s(i)}{x(i)} \right) \tag{14}$$

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 the 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 results on the effects of the incentives intervention show that our data are not consistent with this idea, this remains an important area of future research.

However, alternatively, analysis of heterogeneous effects by ex ante characteristics provides some promising suggestive evidence of targeting by ex ante characteristics were the intervention scaled. Figures 7, 8, and 9 provide aggregate outcome responsiveness and estimated heterogeneous effects by ex ante characteristics, respectively. For example, Figure 9 would suggest that for a policymaker interested in decreasing aggregate levels

of sex work, targeting those with more ex ante sex work exposure – measured via if they participated in sex work prior to their current job and/or reported current sex work levels – may best achieve such a policy goal.

8 Labor Supply Elasticities

We use our reduced form effects to estimate an own price labor supply elasticity for service work and cross-price labor supply elasticity for sex work.

8.0.1 Own Price Labor Supply Elasticity

We use our reduced form effects to estimate a Marshallian own price labor supply elasticity for service work of 0.46. To arrive at this estimate, we use the randomized assignment to the incentives arm as an instrument in a 2SLS setup that captures the impact of incentives task participation on measures of labor supply. In particular, workers' responses to the incentives task can be utilized to measure an own price labor supply elasticity for non-sex work. We do this by replacing the dependent variable in equation 3 with ln daily hours of service work. We also replace the endogenous variable in equation 2 with ln earnings from service work. This value includes incentives task earnings for participants in the incentives arm. Column 1 of Table 4 shows that the first stage effect of incentives arm treatment status on earnings is 20%. Column 2 of Table 4 shows that given this effective earnings increase, we estimate an own price labor supply elasticity of 0.46. Column 3 of Table 4 shows the OLS of *ln Service Venue Hours* on incentives arm treatment status, which when divided by the estimate in Column 1 of Table 4 is approximately equal to the estimate in Column 2 of Table 4.

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 childless women, and single mothers in western countries, 0.23-0.59 (Bargain and Peichl (2016); McClelland and Mok (2012)).

8.0.2 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 -0.58. Similar to our previously estimated own price elasticity analysis 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 natural logarithm (ln) of earnings from service work. Again, this value includes incentives task earnings for participants in our incentives arm. However, this time, we replace the dependent variable

in equation 3 with ln of biweekly sex work hours. Column 1 of Table 6 shows that the first stage effect of incentives arm treatment status on earnings is 21%. Column 2 of Table 6 shows that given this effective earnings increase, we estimate a cross price labor supply elasticity of -0.58. Column 3 of Table 6 shows the OLS of *ln Sex Work Hours* on incentives arm treatment status, which when divided by the estimate in Column 1 of Table 6 is approximately equal to the estimate in Column 2 of Table 6. Appendix Table A27 shows that our results are robust to the inverse hyperbolic sine transformation of *Sex Work Hours*.

Our elasticity estimate is within the range of prior elasticity estimates for other worker populations studied. Our estimate implies that the sex work cross price labor supply elasticity is negative and inelastic, but still more elastic than the service work own price labor supply elasticity calculated above (0.46). This estimate falls at the higher end of the range of analogous elasticity estimates using labor force data for married women, single childless women, and single mothers in western countries, 0.23-0.59 (Bargain and Peichl (2016); McClelland and Mok (2012)), implying that labor supply is more responsive for sex work than for most other, "traditional" labor options.

Further, our estimates are within but on the smaller end of the broad range of those in papers which exploit quasi-random and experimental variation in other settings with daily variation in earnings and labor supply (0.5-1.5), such as 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). This is perhaps unsurprising given we estimate the Marshallian rather than the Hicksian or Frischian labor supply elasticities of focus in the prior literature. It is also perhaps unsurprising given that this other work focuses on professions for which labor supply is significantly less risky than sex work. However, as expected, labor supply elasticities estimated in settings with daily laborers in other higher-risk work are closer in magnitude, 0.06-0.21 (Stafford (2015); Nguyen and Leung (2013)).

9 Structural Estimation

We write an augmented conceptual framework 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 as follows

$$U(y_0, t, s) = ln(y_0 + p_s s + p_t t) + (T - s - t) - \alpha s^2 - 2\alpha\beta_i s \tag{15}$$

Then, the optimal level of sex work effort, s_i, would be

$$s_i = \frac{p_{s,i} - p_{t,i}}{2\alpha p_{t,i}} - \beta_i \tag{16}$$

Above, $\frac{p_{s,i}-p_{t,i}}{p_{t,i}}$ is the sex vs. service work price premium. The relative disutility of sex work, α, weighs the price premium. s_i therefore says that workers with larger positive α values should be more outcome responsive to the price premium.

Random assignment of the incentives intervention offers variation in $PricePremium_{ict}$. Therefore, the empirical analogues to equation 16 can be expressed as follows

$$SexHours_{ict} = \nu + \frac{1}{2\alpha} PricePremium_{ict} - \beta_i \tag{17}$$

where we measure $PricePremium_{ict}$ at the daily level. We can then estimate α leveraging a 2SLS specification. The first stage is as follows

$$PricePremium_{ict} = \pi + \mu Incentives_c + \kappa X_i + \chi Time_t + \delta_{ict} \tag{18}$$

Then, the second stage is as follows

$$SexHours_{ict} = \tau + \xi \widehat{PricePremium}_i + \psi X_i + \omega Time_t - (-\beta_{ict}) \tag{19}$$

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

We find that, on average, $\hat{\alpha} = 8.06$. First, we observe that this estimate is non-zero. This implies that, on average, the relative disutility of sex work is larger than that of service work. In our reduced form results, our own price elasticity estimate for service work was 0.46 and price elasticity estimates for sex vs. service work was -0.58. 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 reduced form finding. Second, we observe that this estimate is positive. This implies that the marginal disutility of sex work is increasing.

10 Other Results

10.1 Sex Work

Tables 9 and 10 show which characteristics of sex work clients may be more or less important to workers. Our results imply that the incentives intervention minimally impacted how perceived risk mediates sex work client choices. In addition to changes in labor supply, we may expect the incentives intervention to decrease the amount of risk workers are willing to accept with their sex work clients. This may be particularly important when it comes HIV, other STIs, or violence risk. Table 9 show ITT regression results comparing incentives vs. control arm participants' reported perceptions of their most recent accepted sex work client (Columns 1-3) and rejected sex work client

(Columns 4-6) risk profiles during follow-up surveys, respectively. In general, we find that clients accepted by incentives arm participants were perceived to have 0.1 points more risk with respect to HIV and STI on a Likert scale of 1-5; this translates to an increase of 7-8%. Meanwhile, we also find that incentives arm participants' accepted sex work clients were 1% less likely to be perceived to be violent. We interpret these results to be suggestive evidence on the potential impacts of the incentives intervention on accepted client risk profile. However, none these effects are significant at conventional levels. Meanwhile, rejected sex work clients are no less likely to be perceived as risky across all these dimensions of risk.

While these results on perceived client risk of sex work clients may reflect limited effects of the incentives intervention to sex work client profiles, they may also be explained by workers' difficulties in assessing sex work client risk. The latter explanation is consistent with our qualitative findings accompanying this intervention in which workers expressed difficulty determining sex work clients' risk profiles. Instead, we might expect to estimate effects on more salient characteristics among sex work clients.

Indeed, our results provide evidence that the incentives intervention led to meaningful impacts to the perceiving income of sex work clients; in particular workers in the incentives arm were more likely to reject less remunerative sex work clients. Data on participants' perceptions of sex work clients' attractiveness, age, and income level facilitate conducting this analysis. Columns 3 and 4 of Table 10 show that incentives arm participants were more likely to report that their most recent rejected sex work client was perceived as poor and less likely to report that their most recent rejected sex work clients was perceived to be at an average income level. These point estimates are similar in magnitude. Similarly, we also observe in Column 6 of Table 10 that incentives arm participants were 10% (p-value = 0.08) more likely to report that they rejected their most recent rejected sex work client because the price he offered was too low.

10.2 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 20 and 21 below. Note that since we found minimal impacts of the incentives intervention to the perceived risk profiles of accepted sex work clients in Table 9, we assume similar risk across clients.

$$\frac{Incidence}{Client} = \frac{Incidence}{Worker} \times \frac{Clients}{Partners} \div \frac{Clients}{Year} \tag{20}$$

$$\frac{\Delta Incidence}{Worker} = \frac{Incidence}{Client} \times \frac{\Delta Clients}{Year} \times \frac{NonCondom}{Clients} \tag{21}$$

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).

Meanwhile, Table 11 shows ITT regression results comparing incentives vs. control arm participants' reported percent condom use with sex work clients, self-perceived likelihood of having an STI, and health site visits during follow-up surveys. In general, we find minimal health impacts. This is perhaps unsurprising given the 1-month duration of our intervention and the time needed to detect differences in incidences of common STIs. For example, HIV antibody tests normally report a 3-month "window" period post-exposure in which an infected person may not yet test HIV-positive.

10.3 Financial Conditions and Status

The incentives intervention appears to improve overall financial conditions by increasing overall income and relieving debt, without compromising consumption.

10.3.1 Overall Income

Column 1 of Table 12 shows that participants in the incentives arm reported 45.74 USD more in overall estimated income across service work (including incentives earnings), sex work, and any other sources of income relative to participants in the control arm. This translates to a 13% increase in overall income across all sources. Combined with the finding that the incentives lead to a decrease in sex work, these results imply that a scaled intervention can facilitate workers decreasing sex work effort without sacrificing, and potentially even increasing, overall income. We note, however, that our research design intentionally did not limit the number of incentives tasks that participants in the incentives arm could submit. Subsidies to outside labor options beyond this experiment may face resource constraints from program administrators. This presents a caveat to the external validity of this finding of an increase in overall income.

For a given 2-week period, we estimate income across all sources of work by summing income reported from biweekly fixed service wages (as reported at baseline), any variable service earnings in the past 2 weeks, the product of the number of reported sex work clients and the accepted price of the most recently accepted sex work client, and any other sources in the past 2 weeks. Since this measure estimates total income, we conduct a back-of-the-envelope exercise to validated if the estimated effect reported above triangulates with the remaining experimental data. At baseline, participants report serving

38 venue customers in the past 2 weeks and working an average of 9 service work hours per day; this translate to 0.3 customers served per service work hour. Similarly, Column 1 of Table 3 shows that participants in the incentives arm reported devoting 40 more minutes to service work per workday; this translates to 9.1 more hours in 2 weeks. Put together, this would mean around 30 venue customers on average with whom participants could complete the incentives task. Multiplying this by our incentive task prices would imply a 45-90 USD increase in income. This rough calculation implies that our estimated regression findings appear reasonable.

10.3.2 Overall Consumption

Column 2 of Table 12 shows that participants in the incentives arm reported 20.98 USD more in overall consumption than participants in the control arm, but this effect was not significant at conventional levels. Participants were also asked about 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, make up and hair services, debt repayment, and remittances to parents/village. Across categories, we similarly did not find effects that were significant at conventional levels. These results are perhaps unsurprising given the month-long duration of the intervention.

10.3.3 Debt

Column 3 of Table 12 shows that there was no statistically significant difference in reported debt status between participants in the incentives vs. control arms. However, investigating debt categories within participants who report being in debt across groups shows suggestive evidence that the intervention led to the alleviation of debt on the intensive margin. Column 4 of Table 12 shows that participants in the incentives arm report a higher likelihood of being in the medium category of debt (1001-1500 USD) and Column 5 of Table 12 shows that participants in the incentives arm report report a lower likelihood of being in the highest category of debt (2000+ USD) relative to participants in the control arm. This may imply that the intervention can alleviate the intensity of debt experienced by workers if the intervention were scaled. Running analogous regressions as in Table 12 for the unconditional cash transfer arm shows no statistically significant changes across income, consumption, or debt status and intensity.

10.4 Compliers Analysis

Overall, intervention compliers are more likely to be unmarried with larger households and have less service sector tenure and use of contraceptives. Because the intention of

the incentives task was to redirect worker effort to service work, we define compliance as follows: compliers are incentives arm participants who worked a full shift in workday prior to a given follow-up survey. Likewise, we define always-takers as control arm participants who worked a full shift in workday prior to a given follow-up survey and never-takers as incentives arm participants 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. We then follow the steps outlined in the compliers analysis in Marbach and Hangartner (2020) to profile complier characteristics.

Tables 13 displays our compliers analysis for baseline participant demographics. Moving left to right, the columns display means and standard deviations, respectively. The last two columns profile compliers. They displays the mean and standard deviation for a given demographic variable amongst the incentives group compliers. This is estimated by subtracted the sum of weighted demographic characteristic means in each of the groups of always-takers and never=takers from the weighted demographic characteristic means of all incentives and control group participants. Each group's mean is weighted by the estimated sample shares of compliers. We bootstrap to obtain standard deviations that reflect the estimation uncertainty in both the covariate means and the sample proportions. This procedure is as explained in Marbach and Hangartner (2020). For example, the "Household Size" row says that compliers may be disproportionately likely to be from larger households. While participants across the incentives and control arm had an average household size of 3.9, the average household size of compliers was 4.08.

Tables 14 displays our compliers analysis for baseline participant characteristics. The characteristics in this table are all analogous to data already collected by the largest NGO providing sexual and reproductive health services to female sex workers in this setting. Hence, this table is policy relevant in that it facilitates identifying characteristics of workers that could predict compliance beyond this study.

10.5 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. These results are displayed in Table 15. 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 16, 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 17 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 hourly 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 17 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 on this topic 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 market prices may be *more than* compensating for fatality risk to workers in this setting.

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.

11 Conclusion

In a representative population of venue-based female sex workers, this study tests the effects of incentivizing non-sex work labor on the supply of sex work labor. For women engaged in indoor, part-time sex work – the majority of sex workers worldwide – we show that incentivizing the outside work option decreases the supply of sex work labor relative to both a pure control and similarly-valued fixed cash transfer. This headline finding was accompanied by 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. Incentivizing outside work options may thus enable women to decrease sex work while maintaining, or even increasing, overall income and potentially decreasing debt and health risks.

We conducted a 1-month cluster RCT in which venue-based female sex workers were randomized to receive either unconditional service work incentives, an unconditional cash transfer, or neither. We write a simple model of labor supply to help assess mechanisms behind a negative transfer effect on sex work. Minimal impacts to sex work labor supply in the fixed cash transfer arm imply that income effects do not purely explain the negative transfer effects. Our results also do not appear to be consistent with a targeting story in which the opt-in design of the incentives is able to better identify the "marginal substituters". Instead, the negative transfer effect appears to be primarily explained by price effects, or an increase in the marginal disutility of sex work. 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 only and between sex and non-sex work. Comparing the estimates' signs and magnitudes suggests that workers substitute between two types of work. It also implies 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 validate that it is higher than that of non-sex work. Meanwhile, estimate of compensating wage differential for sex work fatality risk and its suggestive figure for the value of a statistical life combine to imply that the relative disutility we estimate is compensated for by prices in the market. These results potentially suggests that the prevailing policy response, bans to sex work, may be misaligned with worker valuations.

There are important limitations of the study's broader interpretation. First, the increase in sex work proposals in response to the incentives may be an unintended consequence from a policy maker'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 responsive to the incentives may also be an un-

intended consequence from a policy maker's perspective, albeit these results were not significant at conventional levels. Third, the incentives in this study were designed to have no limit for the purposes identifying worker heterogeneity. However, in reality, subsidies to outside labor option 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 meaningfully impacts their labor supply choices, revealing heterogeneity across female sex workers that existing programs may overlook.

Understanding the extent to which individuals *choose* sex work vs. outside options is vital to assessing the impacts of policies which regulate the market for sex. This paper demonstrates how altering the relative returns to sex work can lead to meaningful changes in labor supply decisions for workers in resource-limited settings. Meanwhile, it also shows that providing unconditional cash transfers does not have similar effects. A growing literature has documented various negative effects of regulations to 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 increasing the costs to sex work may eliminate sex work, but individuals' responses depend on their underlying preferences for sex vs. non-sex work. This paper provides experimental evidence to quantify this unknown parameter.

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12 Figures

Figure 1: PDF of Incentive Task Submissions in the Incentives Arm

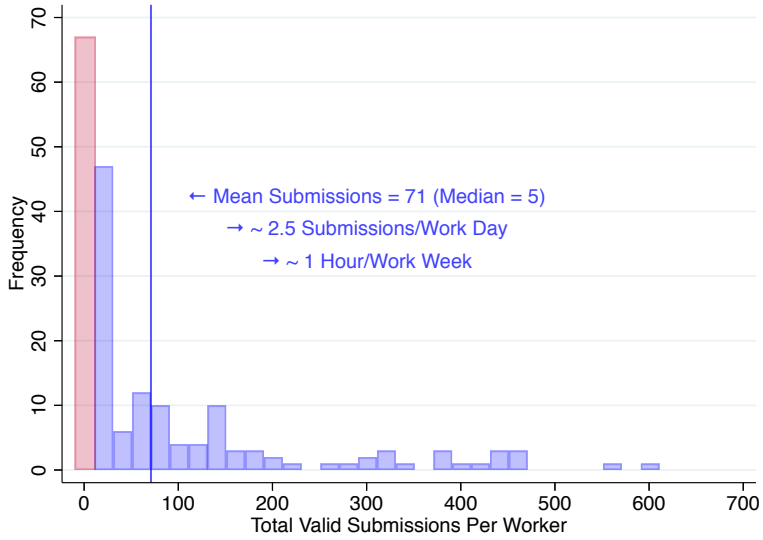


Figure 2: CDF of Mean Weekly Incentives Task Earnings in the Incentives Arm

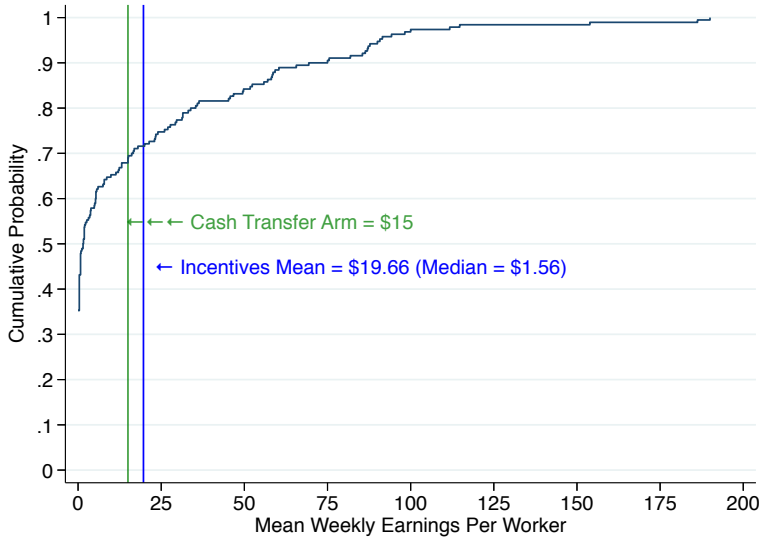


Figure 3: Distribution of Incentives Task Submission Times in the Incentives Arm

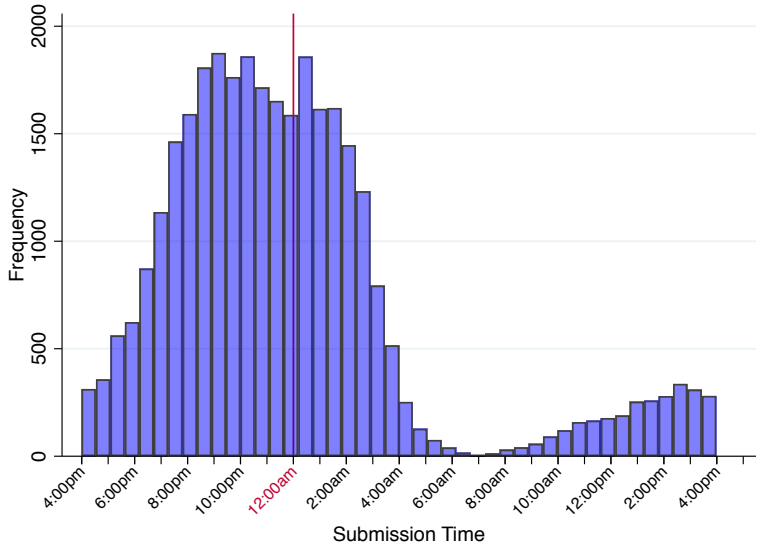


Figure 4: Sex Work Departure Time in Incentives vs. Control Arms

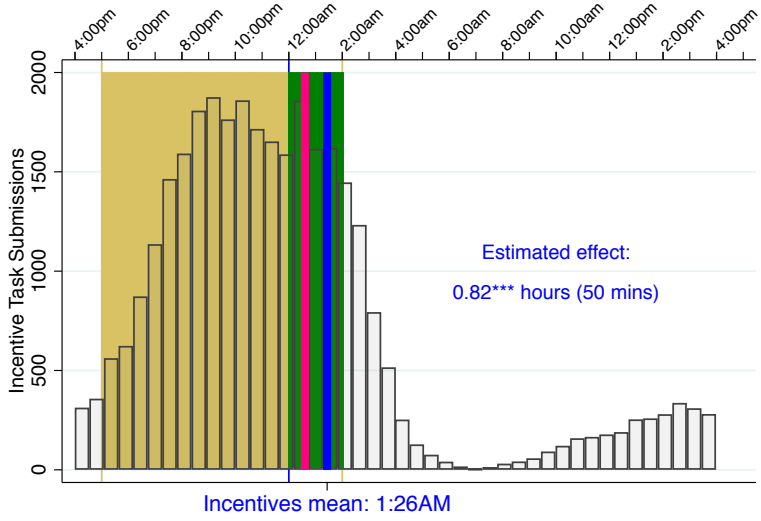


Figure 5: Outcome Responsiveness by Biweekly Earnings in the Incentives Arm

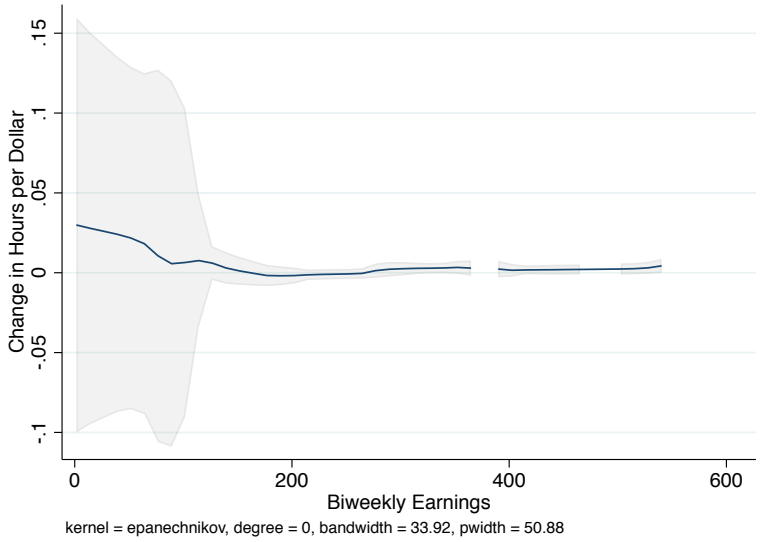


Figure 6: Outcome Responsiveness by Probability of Being High Type Worker in the Cash Transfer Arm

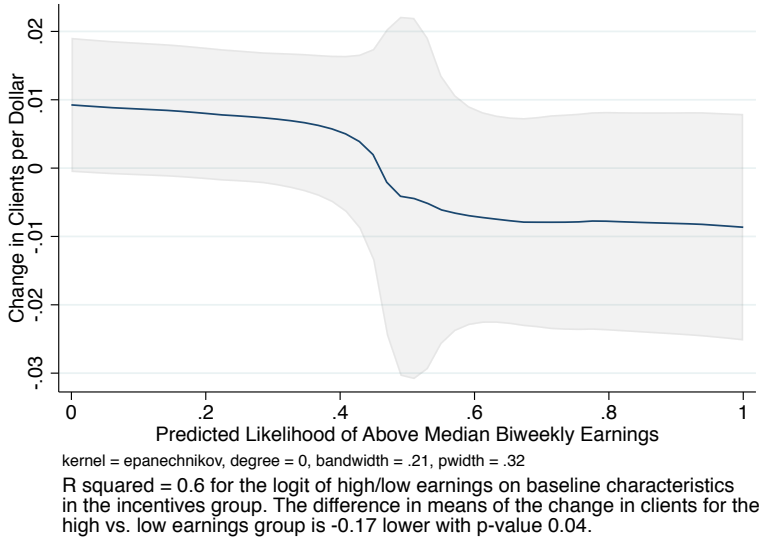


Figure 7: Heterogeneous Effects on Sex Work Levels by Characteristic in the Incentives Arm

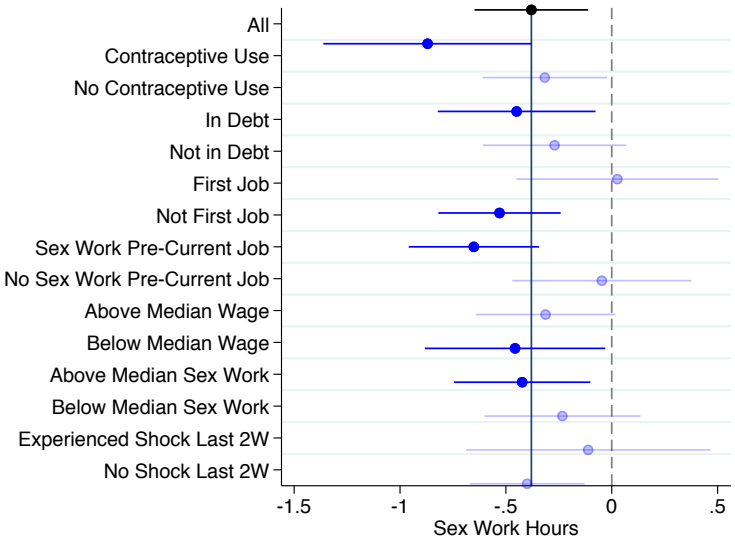


Figure 8: Heterogeneous Effects on Percent Change in Sex Work vs. Baseline by Characteristic in the Incentives Arm

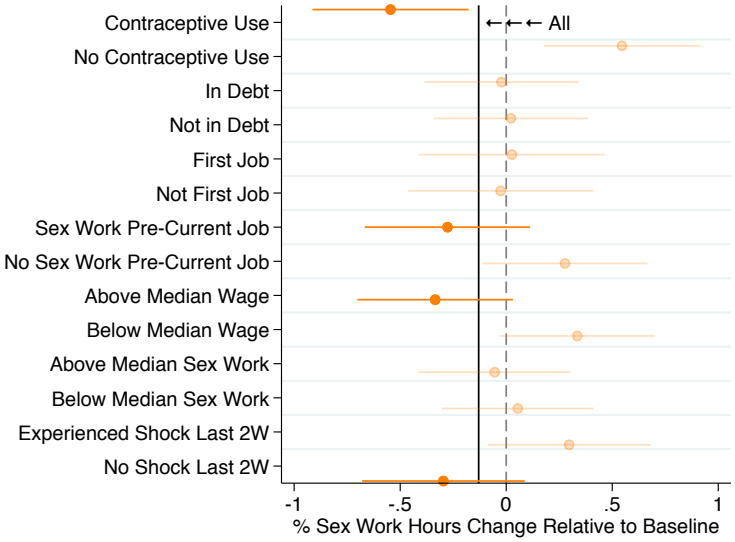
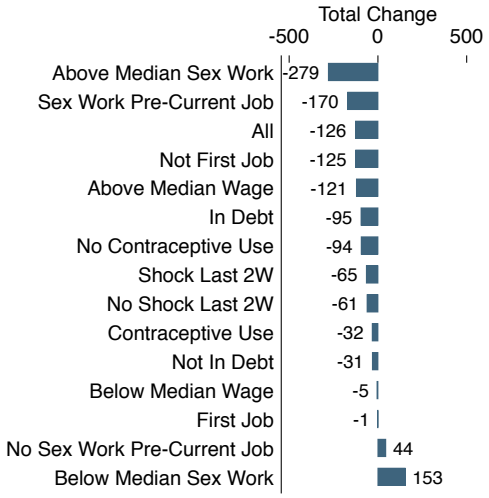


Figure 9: Heterogeneity in Total Sex Work Change by Characteristic in the Incentives Arm



13 Tables

Table 1: Balance: Demographics

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

Note: All variables are at baseline. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Balance: Outcomes

Variable	All		Control		Incentives		
	Mean	SD	Mean	SD	Mean	SD	Difference
2W Fixed Service Income	67	23.01	64.57	34	65.97	23.01	1.423
2W Variable Service Income	123	112.97	128.80	119	120.41	112.97	-8.131
1D Service Venue Hours	9.9	2.97	9.59	3.3	10.15	2.97	0.569
2W Customers Served	38	43.62	37.70	33	42.29	43.62	4.291
Biweekly Sex Work Hours	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
Accepted: HIV Risk	1.3	0.62	1.31	.6	1.31	0.62	0.005
Accepted: STI Risk	1.4	0.64	1.37	.69	1.37	0.64	0.002
Accepted: Violence Risk	.0099	0.07	0.02	.15	0.01	0.07	-0.018
Rejected: Client Poor	.075	0.21	0.10	.31	0.05	0.21	-0.055
Rejected: Client Average	.88	0.31	0.86	.35	0.90	0.31	0.038
Rejected: Client Rich	.042	0.23	0.04	.2	0.06	0.23	0.017
Rejected: 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
Total Income	376	242.78	377.67	199	385.44	242.78	8.440
Total Consumption	213	115.44	208.86	103	215.45	115.44	6.343
Debt Status	.65	0.49	0.63	.48	0.62	0.49	-0.021

Note: *2W Fixed Service Income* is the biweekly portion of her venue's fixed monthly wages the participant reported at baseline. *2W Variable Service Income* is sum of variable earnings the participant reported earning in the 2 weeks prior to the baseline survey. *1D Service Venue Hours* is the number of hours between the participant's reported start and end time the day before the baseline survey. *2W Customers Served* is the number of customers the participant reported serving in the 2 weeks prior to the baseline survey. *2W Sex Work Clients* is the reported number of accepted sex work clients the participant had in the 2 weeks prior to the baseline survey. *2W Sex Work Clients* is the reported number of accepted sex work clients the participant had in the 2 weeks prior to the baseline survey. *% Client Condom* is the percent of sex work clients with whom the participant reporting using a condom with in the 2 weeks prior to the baseline survey. *Acc/Rej: HIV Risk* is the participant's perception of the likelihood that the most recent accepted/rejected sex work client at baseline had HIV at the time of sex on a Likert scale of 1-5. *Acc/Rej: STI Risk* is the participant's perception of the likelihood that the most recent accepted/rejected sex work client at baseline had any STI other than HIV at the time of sex on a Likert scale of 1-5. *Acc/Rej: Violence Risk* is the participant's perception of if the most recent accepted/rejected sex work client at baseline seemed violent, aggressive, or rude (yes/no). *Rejected: Poor, Average, Rich* is the participant's perception of her most recent rejected sex work client at baseline's wealth status, given poor, average, and rich as choices. *Rejected: Price Too Low* is if the participant reported rejected her most recent rejected sex work client at baseline because the price he offered was too low. *Thinks has STI* is if the participant reported thinking she had any STI on a Likert scale of 1-5 at baseline. *Total Income* is the participant's total estimated earning across all sources in the 2 weeks prior to the baseline survey, summing up non-service or -sex work earnings in the last 2 weeks, biweekly fixed service work wages, 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. *Total Consumption* is the participant's total spending across spending categories in the 2 weeks prior to the baseline survey. *Debt Status* is the participant's reported debt status at the time of the baseline survey (yes/no). Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effects on Service Work in the Incentives Arm

	(1) 1D Service Venue Hours	(2) 2W Venue Tip Split	(3) 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

Note: Regressions are at the participant-survey round level. *1D Service 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 the participant reported start receiving since the last biweekly participant survey. *2W Customers Served* is the number of customers the participant reported serving since the last biweekly participant 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 4: Service Work Own Price Labor Supply Elasticity in the Incentives Arm

	(1) ln Income	(2) ln Service Venue Hours	(3) ln Service Venue Hours
Incentives	0.20***		0.09**
	(0.05)		(0.04)
ln Income		0.46**	
		(0.22)	
Round FEs	Yes	Yes	Yes
Clustered SEs	Yes	Yes	Yes
Controls	Yes	Yes	Yes
N	690	690	690

Note: Regressions are at the participant-survey round level. Excludes observations with missing service venue hours or income data. *ln Income* is the ln of total reported service work income since the last biweekly participant survey; for the incentives arm, this includes incentives task earnings. *ln Service Venue Hours* is the ln of the number of hours between the participant's reported start and end time the day before the given participant survey. Column 1 shows the first stage of the 2SLS estimates. Column 2 shows the 2SLS estimates. Column 3 shows the OLS of *ln Service Venue Hours* on incentives arm treatment 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 5: Effects on Sex Work in the Incentives Arm

	(1) Biweekly Sex Work Hours	(2) Biweekly Sex Work Hours	(3) Biweekly 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)
Pimary 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: Regressions are at the participant-survey round level. *2W Sex Work Clients* is the reported number of accepted sex work clients the participant had since the last biweekly participant 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 6: Sex Work Cross Price Labor Supply Elasticity in the Incentives Arm

	(1) ln Income	(2) ln Sex Work Hours	(3) ln Sex Work Hours
Incentives	0.21***		-0.12***
	(0.05)		(0.05)
ln Income		-0.58**	
		(0.26)	
Round FEs	Yes	Yes	Yes
Clustered SEs	Yes	Yes	Yes
Controls	Yes	Yes	Yes
N	679	679	679

Note: Regressions are at the participant-survey round level. Excludes observations with missing sex work client or income data. *ln Income* is the ln of total reported service work income since the last biweekly participant survey; for the incentives arm, this includes incentives task earnings. *ln Sex Work Clients* is the ln of the number of sex work clients the participant reported in the last 2 weeks. Column 1 shows the first stage of the 2SLS estimates. Column 2 shows the 2SLS estimates. Column 3 shows the OLS of *ln Sex Work Clients* on incentives arm treatment 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 7: Effects on Sex Work in the Cash Transfer Arm

	(1) Biweekly Sex Work Hours	(2) Biweekly Sex Work Hours	(3) Biweekly Sex Work Hours
Cash Transfers	-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)
Pimary 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: Regressions are at the participant-survey round level. *2W Sex Work Clients* is the reported number of accepted sex work clients the participant had since the last biweekly participant 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 8: Effects on Sex Work in the Incentives vs. Cash Transfer Arms

	(1) Biweekly Sex Work Hours	(2) Biweekly Sex Work Hours	(3) Biweekly 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)
Pimary 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: Regressions are at the participant-survey round level. *2W Sex Work Clients* is the reported number of accepted sex work clients the participant had since the last biweekly participant 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 9: Effects on Accepted and Rejected Sex Work Clients Risk in the Incentives Arm

	(1) Accept: HIV	(2) Accept: STI	(3) Accept: Violent	(4) Reject: HIV	(5) Reject: STI	(6) Reject: 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: Regressions are at the participant-survey round level. Columns 1-3 focus on the most recent 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 10: Effects on Rejected Sex Work Clients Characteristics in the Incentives Arm

	(1) Attractive	(2) Age	(3) Poor	(4) Average Income	(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: 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 11: 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: 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 biweekly participant survey; this variable excludes participant-survey rounds in which 0 sex work clients were reported since the last biweekly participant 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 biweekly participant 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 12: 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: *Income* is the participant's total estimated earning across all sources since the last biweekly participant survey, summing up non-service or -sex work earnings in the last 2 weeks, biweekly fixed service work wages, 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 biweekly participant 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 13: Complier Demographics

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	7.01
Married	.22	0.41	0.25	.44	0.20	0.41	0.19	0.39
On Govt Aid	.0053	0.07	0.00	0	0.00	0.00	0.01	0.11
From City	.32	0.47	0.26	.44	0.28	0.46	0.37	0.49
Household Size	3.9	2.32	3.75	1.9	3.88	2.73	4.08	2.53

Table 14: Complier Characteristics

Variable	All		Always-Takers		Never-Takers		Compliers	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
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 Hours	.7	0.46	0.70	.46	0.60	0.50	0.71	0.46

Table 15: Compensating Wage Differentials in the Incentives Arm

	(1) HIV Fatality Risk	(2) Sex Work Wage	(3) Sex Work Wage
Incentives	0.00000002		5.28e+00
	(0.00000004)		(3.78e+00)
HIV Fatality Risk		2.27e+08	
		(3.96e+08)	
Round FEs	Yes	Yes	Yes
Clustered SEs	Yes	Yes	Yes
Controls	Yes	Yes	Yes
N	683	664	664

Note: Regressions are at the participant-survey round level. *HIV Fatality Risk* is estimated by adjusting the ITT *HIV Exposure Risk* estimate by the following: HIV transmission risk as per follow-up data, non-condom use rates as per follow-up data, HIV transmission rates for unprotected receptive penile-vaginal intercourse (CDC, 2019), HIV transmission rates for intercourse with proper male condom use (USAID, 2015), and death rate of persons living with HIV in Cambodia (KHANA, 2022). *Sex Work Wage* is the reported price of sex for the most recent accepted sex work client. Sample size drops from N=717 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 16: Workers' Reported Sex Work Disamenities

	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
Aging out	1	0
Fear of her family finding out, violence risk	1	0
Gain leisure time	1	0
Sexual health risk, violence risk	1	0
Stigma, violence risk, Sexual health risk	1	0
Stress	1	0
Total	358	100

Note: 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) given are tabulated above.

Table 17: Effects on HIV Risk and Sex Work Wages in the Incentives Arm

	(1)	(2)	(3)
	HIV Exposure Risk	Sex Work Wage	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: *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=717 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: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

14 Appendix

14.1 Appendix Tables

Table A18: Balance: Demographics for Cash Transfer vs. Control

Variable	All		Control		Cash Transfers		
	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

Note: All variables are at baseline. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A19: Balance: Outcomes for Cash Transfer vs. Control

Variable	All		Control		Cash Transfers		
	Mean	SD	Mean	SD	Mean	SD	Difference
2W Fixed Service Income	67	50.96	64.57	34	71.58	50.96	6.896
2W Variable Service Income	123	119.80	128.80	119	118.09	119.80	-10.230
1D Service Venue Hours	9.9	3.16	9.59	3.3	10.07	3.16	0.490
2W Customers Served	38	37.86	37.70	33	33.77	37.86	-3.794
Biweekly Sex Work Hours	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
Accepted: HIV Risk	1.3	0.72	1.31	.6	1.40	0.72	0.094
Accepted: STI Risk	1.4	0.73	1.37	.69	1.45	0.73	0.081
Accepted: Violence Risk	.0099	0.00	0.02	.15	0.00	0.00	-0.024**
Rejected: Client Poor	.075	0.26	0.10	.31	0.07	0.26	-0.028
Rejected: Client Average	.88	0.30	0.86	.35	0.90	0.30	0.040
Rejected: Client Rich	.042	0.17	0.04	.2	0.03	0.17	-0.012
Rejected: 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
Total Income	376	215.31	377.67	199	363.52	215.31	-13.188
Total Consumption	213	122.99	208.86	103	215.89	122.99	6.372
Debt Status	.65	0.46	0.63	.48	0.71	0.46	0.078

Note: *2W Fixed Service Income* is the biweekly portion of her venue's fixed monthly wages the participant reported at baseline. *2W Variable Service Income* is sum of variable earnings the participant reported earning in the 2 weeks prior to the baseline survey. *1D Service Venue Hours* is the number of hours between the participant's reported start and end time the day before the baseline survey. *2W Customers Served* is the number of customers the participant reported serving in the 2 weeks prior to the baseline survey. *2W Sex Work Clients* is the reported number of accepted sex work clients the participant had in the 2 weeks prior to the baseline survey. *2W Sex Work Clients* is the reported number of accepted sex work clients the participant had in the 2 weeks prior to the baseline survey. *% Client Condom* is the percent of sex work clients with whom the participant reporting using a condom with in the 2 weeks prior to the baseline survey. *Acc/Rej: HIV* is the participant's perception of the likelihood that the most recent accepted/rejected sex work client at baseline 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 at baseline 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 at baseline seemed violent, aggressive, or rude (yes/no). *Rej: Poor, Average Income, Rich* is the participant's perception of her most recent rejected sex work client at baseline's wealth status, given poor, average, and rich as choices. *Reject: Price Too Low* is if the participant reported rejected her most recent rejected sex work client at baseline because the price he offered was too low. *Thinks has STI* is if the participant reported thinking she had any STI on a Likert scale of 1-5 at baseline. *Total Income* is the participant's total estimated earning across all sources in the 2 weeks prior to the baseline survey, summing up non-service or -sex work earnings in the last 2 weeks, biweekly fixed service work wages, 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 in the 2 weeks prior to the baseline survey. *Debt Status* is the participant's reported debt status at the time of the baseline survey (yes/no). Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A20: Balance: Demographics for Incentives vs. Cash Transfer

Variable	All		Cash Transfers		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: All variables are at baseline. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A21: Balance: Outcomes for Incentives vs. Cash Transfer

Variable	All		Cash Transfers		Incentives		
	Mean	SD	Mean	SD	Mean	SD	Difference
2W Fixed Service Income	67	23.01	71.58	51	65.97	23.01	-5.473
2W Variable Service Income	123	112.97	118.09	120	120.41	112.97	2.099
1D Service Venue Hours	9.9	2.97	10.07	3.2	10.15	2.97	0.079
2W Customers Served	38	43.62	33.77	38	42.29	43.62	8.085
Biweekly Sex Work Hours	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
Accepted: HIV Risk	1.3	0.62	1.40	.72	1.31	0.62	-0.089
Accepted: STI Risk	1.4	0.64	1.45	.73	1.37	0.64	-0.079
Accepted: Violence Risk	.0099	0.07	0.00	0	0.01	0.07	0.006
Rejected: Client Poor	.075	0.21	0.07	.26	0.05	0.21	-0.026
Rejected: Client Average	.88	0.31	0.90	.3	0.90	0.31	-0.002
Rejected: Client Rich	.042	0.23	0.03	.17	0.06	0.23	0.028
Rejected: 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
Total Income	376	242.78	363.52	215	385.44	242.78	21.628
Total Consumption	213	115.44	215.89	123	215.45	115.44	-0.029
Debt Status	.65	0.49	0.71	.46	0.62	0.49	-0.099*

Note: *2W Fixed Service Income* is the biweekly portion of her venue's fixed monthly wages the participant reported at baseline. *2W Variable Service Income* is sum of variable earnings the participant reported earning in the 2 weeks prior to the baseline survey. *1D Service Venue Hours* is the number of hours between the participant's reported start and end time the day before the baseline survey. *2W Customers Served* is the number of customers the participant reported serving in the 2 weeks prior to the baseline survey. *2W Sex Work Clients* is the reported number of accepted sex work clients the participant had in the 2 weeks prior to the baseline survey. *2W Sex Work Clients* is the reported number of accepted sex work clients the participant had in the 2 weeks prior to the baseline survey. *% Client Condom* is the percent of sex work clients with whom the participant reporting using a condom with in the 2 weeks prior to the baseline survey. *Acc/Rej: HIV* is the participant's perception of the likelihood that the most recent accepted/rejected sex work client at baseline 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 at baseline 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 at baseline seemed violent, aggressive, or rude (yes/no). *Rej: Poor, Average Income, Rich* is the participant's perception of her most recent rejected sex work client at baseline's wealth status, given poor, average, and rich as choices. *Reject: Price Too Low* is if the participant reported rejected her most recent rejected sex work client at baseline because the price he offered was too low. *Thinks has STI* is if the participant reported thinking she had any STI on a Likert scale of 1-5 at baseline. *Total Income* is the participant's total estimated earning across all sources in the 2 weeks prior to the baseline survey, summing up non-service or -sex work earnings in the last 2 weeks, biweekly fixed service work wages, 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 in the 2 weeks prior to the baseline survey. *Debt Status* is the participant's reported debt status at the time of the baseline survey (yes/no). Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A22: Reported Reasons for Lack of Incentives Task Take-up

	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: Tabulations are by participant-survey round.

Table A23: Reported Guess 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: Tabulations are by participant.

Table A24: Regression of Sex Work Proposals on Treatment Status for Incentives vs. Control in the Incentives Arm

	(1)	(2)	(3)
	Sex Work Client Proposals	Sex Work Client Proposals	Sex Work 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

Note: Regressions are at the participant-survey round level. *2W Sex Work Clients* is the reported number of accepted sex work clients the participant had in the 2 weeks prior to the baseline 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 A25: Regression of Sex Work on Treatment Status for Incentives vs. Control in the Incentives Arm: Adjusted for Social Desirability

	(1) Biweekly Sex Work Hours	(2) Biweekly Sex Work Hours	(3) Biweekly 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

Note: Regressions are at the participant-survey round level. *2W Sex Work Clients* is the reported number of accepted sex work clients the participant had in the 2 weeks prior to the baseline 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 A26: Regression of Sex Work on Treatment Status for Incentives vs. Control in the Incentives Arm: Winsorized

	(1) Biweekly Sex Work Hours	(2) Biweekly Sex Work Hours	(3) Biweekly 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: Regressions are at the participant-survey round level. Bottom 1st and top 99th percentile of outcome data are winsorized. *2W Sex Work Clients* is the reported number of accepted sex work clients the participant had in the 2 weeks prior to the baseline 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 A27: Sex Work Cross Price Labor Supply Elasticity in the Incentives Arm: Inverse Hyperbolic Sine Outcome Transformation

	(1) ln Income	(2) IHS Sex Work Hours	(3) IHS Sex Work Hours
Incentives	0.21***		-0.13**
	(0.05)		(0.05)
ln Income		-0.54**	
		(0.24)	
Round FEs	Yes	Yes	Yes
Clustered SEs	Yes	Yes	Yes
Controls	Yes	Yes	Yes
N	679	679	711

Note: Regressions are at the participant-survey round level. Bottom 1st and top 99th percentile of outcome data are winsorized. *2W Sex Work Clients* is the reported number of accepted sex work clients the participant had in the 2 weeks prior to the baseline 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 A28: Effects on Service Work in the Cash Transfer Arm

	(1) 1D Service Venue Hours	(2) 2W Venue Tip Split	(3) 2W Customers Served
Cash Transfers	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: Regressions are at the participant-survey round level. *1D Service 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 the participant reported start receiving since the last biweekly participant survey. *2W Customers Served* is the number of customers the participant reported serving since the last biweekly participant 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 A29: 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: 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. *HH Members Earn Income*: The number of household members in the participant's household at baseline who she reports earns a steady income. Standard errors clustered are the venue level are indicated in parentheses. Stars highlight significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.