Self-Control and Demand for Preventive Health: Evidence from Hypertension in India*

Liang Bai † Benjamin Handel ‡ Edward Miguel § Gautam Rao ¶

February 16, 2020

Abstract

Self-control problems constitute a potential explanation for the under-investment in preventive health in low-income countries. Behavioral economics offers a tool to solve such problems: commitment devices. We conduct a field experiment to evaluate the effectiveness of different types of theoretically-motivated commitment contracts in increasing preventive doctor visits by hypertensive patients in rural India. Despite achieving high take-up of such contracts in some treatment arms, we find no effects on actual doctor visits or individual health outcomes. A substantial number of individuals pay for commitment but fail to follow through on the doctor visit, losing money without experiencing health benefits. We develop and structurally estimate a pre-specified model of consumer behavior under present bias with varying levels of naivete. The results are consistent with a large share of individuals being partially naive about their own self-control problems: sophisticated enough to demand some commitment, but overly optimistic about whether a given level of commitment is sufficiently strong to be effective. The results suggest that commitment devices may in practice be welfare diminishing, at least in some contexts, and serve as a cautionary tale about their role in health care.

*We thank Child Relief International, the Center for Equitable Growth, and the Berkeley Population Center for funding. the We thank Allyson Barnett, Fenella Carpena, Carson Christiano, Jen Kwok, and Deepak Saraswat for excellent research assistance. We are also grateful to seminar audiences at Berkeley, Harvard, Stanford, CEU, Wharton, UPF and numerous others for useful suggestions. All errors remain our own.

†School of Economics, University of Edinburgh

‡Department of Economics, University of California, Berkeley and NBER

§Department of Economics, University of California, Berkeley and NBER

¶Department of Economics, Harvard University and NBER

1 Introduction

We study the role of self-control problems in an important preventive health context: the management of hypertension in India.[1] Like much preventive care, managing hypertension requires behaviors – such as visiting a doctor or changing one's diet – that involve *utility costs* in the present, while the returns lie in the distant future.[2] An individual with limited self-control, say due to present bias, might procrastinate on engaging in such preventive behaviors if she is naive or overconfident about her future self-control (O'Donoghue and Rabin 1999). While the theoretical insight is well known, the importance of present bias in explaining under-utilization of preventive care remains controversial (Dupas and Miguel, 2017; Kremer et al. 2019).

The most common way researchers have attempted to tackle self-control problems is through the provision of commitment devices or contracts (CCs). CCs allow individuals to voluntarily restrict their future choice set, or increase the costs of certain potential future actions. An individual without self-control problems should not have demand for CCs. Yet researchers have documented demand for commitment in a number of contexts, such as savings commitments (Ashraf et al. 2006), smoking cessation (Gine et al. 2010), alcohol consumption (Schilbach 2017), fertilizer use (Duflo et al. 2011), work effort (Kaur et al. 2015), and exercise behavior (Royer et al. 2015), among others.[3] These studies have provided "smoking gun" evidence on the existence of self-control problems, and commitment devices have increasingly come to be viewed as a policy tool.[4]

However, theory also provides an important warning about the effectiveness and welfare implications of commitment contracts: they are predicted to work well only when agents are sufficiently sophisticated (Heidhues and Kőszegi, 2009). Sophisticated agents, who have accurate beliefs about their future levels of self-control, will correctly predict how the incentives embedded in a particular CC will lead them to act in the future. They will choose commitment wisely to reach the first best from a long-run perspective. In contrast, agents who are (partially) *naive*—in that they are overconfident about their future self-control—will either have no demand for CCs or might even be harmed by them, due to unwisely purchasing CCs which provide "too little" commitment.[5] Yet the literature provides little systematic evidence on the distribution of sophistication and naivete in the population.[6] While CCs have the potential to improve individual health outcomes and well-being,

[1] Hypertension is an increasingly important public health problem in low-income countries. In India, it is estimated that nearly half of adults aged 45 years and above are at risk, and rural India has seen an eight-fold increase in prevalence over the past six decades (Association of Physicians of India [API], 2013; Mohan et al. 2013).

[2] Compared to other more acute conditions, an important feature of hypertension is the lack of everyday symptoms, which theoretically makes procrastination more likely.

[3] Bryan et al. (2010) provide a review of the earlier literature.

[4] For instance, the World Development Report in 2015 mentions CCs 45 times, and concludes that they are "likely to have strong and positive impacts on behavior" (Pg 120, World Bank, 2015).

[5] This point has been made theoretically by Heidhues and Kőszegi (2009) and DellaVigna and Malmendier (2004), and empirically by DellaVigna and Malmendier (2006) in the context of gym attendance in the United States, and John (2019) in the context of savings plans in the Philippines.

[6] An exception is Augenblick and Rabin (2018), who estimate a high degree of naivete using a real-effort task in a lab experiment.

it remains an open empirical question whether the risks for partially naive agents outweigh the benefits for sophisticated agents.

The current study was designed to explore these issues in the case of preventive care for hypertension in rural Punjab, India. The experimental interventions consisted of different types of commitment contracts for attendance at village "Hypertension Day" health camps conducted by a private-sector health care provider (henceforth the Provider). The health camps represent extremely high-quality health care in the context of rural India, featuring a consultation with trained and licensed medical personnel, including doctors and nurses, and state-of-the-art diagnostics and access to standard-of-care medications. The goal of the interventions was to boost preventive care for hypertension among adults in our sample villages who had been identified (through screening) to either have hypertension or to be at high risk of developing it. All participants received information about the health condition and recommended preventive health behaviors.

The CCs we studied asked individuals to make an up-front payment, reducing the marginal cost of health camp visits in the future, as in the model of DellaVigna and Malmendier (2004). Some contracts required up-front payments larger than the visit fee, such that participants received some money back each time they made a recommended bi-monthly preventive visit to a health camp. We also subsidized the visits for a cross-cutting random subset of participants. These price discounts served two roles: to assess the role of affordability or liquidity constraints, and to encourage even naive agents to sign up for commitment contracts, in anticipation of benefiting from the price discount. The control group instead pays for the preventive visits in a typical fee-for-service manner.

An important aspect of the research design is the use of both *fixed commitment contracts*, where individuals are asked to accept or reject a particular contract (with a given up-front payment and future money-back amounts) as well as *personalized commitment contracts*, where the individual is able to choose the amount of up-front payment, and thus the strength of the commitment. This latter contract provides rich information on the amount of commitment that individuals desire, shedding light on underlying preferences. If individuals are sophisticated, such personalized contracts should boost take-up and health care usage. In contrast, if individuals are partially naive, allowing them to choose their level of commitment could lead to systematic mistakes, such as choosing commitment amounts too small to ensure follow-through.

These different contract options were offered to random subsets of n=1728 participants, allowing us to assess impacts on preventive health care utilization as well as health outcomes, including blood pressure, using a combination of administrative data and an endline survey six months after treatment. We pre-specified the reduced-form econometric tests in a pre-analysis plan on the AEA RCT registry (#AEARCTR-0000062), as well as the theoretical model that forms the basis of the structural estimation.[7]

The first empirical result is a finding of modest take-up of unsubsidized CCs, but much higher

[7] This latter document can be accessed using the "request information" option under "Supporting Documents and Materials".

take-up when CCs are bundled with a price discount. In the non-subsidized treatment arms, take-up of fixed and personalized commitment contracts was 13.7 and 14.1%, respectively, compared to 25.9% and 38.6% for the fixed and personalized subsidized commitment contracts. The low level of demand in the unsubsidized CCs is within the range typically found in the literature (Laibson 2015; Schilbach 2019).[8] The level of commitment chosen in the personalized CC arms – whether subsidized or not – was generally low (lower than in the fixed contracts). In contrast, the high take-up of the subsidized contracts – which provide commitment ex post (even if that commitment is not valued ex ante) – suggests an avenue for successfully increasing take-up of commitment.

The second empirical result, however, is a finding of low attendance at the health camps, even in the arms with high take-up of CCs. Less than 10% of individuals make at least one visit (out of the recommended three over six months) in the full-price groups, with unconditional attendance rates nearly identical and not statistically distinguishable in the control (8.9%), fixed CC (9.5%), and personalized CC (9.9%) groups. In addition to providing evidence on the ineffectiveness of a particular CC in our context, this also provides novel evidence on the limited gains—or losses—from allowing consumers to design their own commitment contracts. Despite the high take-up of the CCs when bundled with a 50% price subsidy, only 13.7% of respondents made at least one visit to a health camp in those groups, a rate slightly *lower* than those who were offered a straight half-off price discount but no commitment (14.5%), and only 6.2% attended all three visits that they had paid for. Given the low levels of attendance at the preventive health visit, it is perhaps not surprising that we do not find statistically significant treatment effects on respondents' endline health outcomes in terms of blood pressure and body weight.

Across the different CCs, between 62-77% of those who paid for a commitment contract failed to make even one visit to a health camp. Overall, between 8% (in the fixed contract with no discount) and 30% (personalized contracts with discounts) of all individuals chose to purchase a commitment contract but then failed to attend any health camps. Under reasonable assumptions, this suggests that a substantial fraction of the participant population experienced reduced welfare due to the commitment contract offers. Through the lens of our theory, these are likely to be individuals who were partially naive (i.e., moderately over-confident) about the extent of their own time-inconsistent preferences. They appear to understand their own present-bias problem enough to demand some commitment, but ultimately purchase too little to actually overcome procrastination. Similarly, inducing potentially naive individuals to enter commitment contracts by bundling them with discounts did not produce improvements in the "committed" behaviors.

A simple calculation suggests that the utility benefits for people who visited the doctor when offered a fixed CC would have to be substantial—Rs. 115 (approximately $2) per visit—to outweigh

[8] Table 1 in Schilbach (2019) reports the variation in take-up levels of commitment contracts in the literature. While our model emphasizes the role of naivete about self-control problems in explaining the low take-up, other context-specific factors surely also matter. We speculate that the requirement to pay money up front, and low valuation for preventive health overall, play a role.

the losses incurred by those who accepted the CC but failed to visit the doctor. In contrast, in the absence of a CC, benefits would only need to exceed the per-visit fee of Rs. 30 to enhance consumer welfare.

To formally quantify welfare changes across treatments, including the (perceived) benefits of doctor visits, and the impacts of counterfactual contracts, we develop and estimate a model of consumer choice with present bias and variation in naivete, providing estimated distributions of present bias, (β, as in Laibson (1997)), and naivete/sophistication ($\hat{\beta}$, as in O'Donoghue and Rabin (1999)) in our population. The structural results imply that a large share of individuals are partially naive about their own time inconsistency problems. In particular, the estimated mean of β is 0.365, while the estimated mean of $\hat{\beta}$ is 0.795. Under the assumptions in the structural model, and adopting a utilitarian perspective, *consumer welfare* is considerably lower in the undiscounted CC treatment arms than in the control group. In theory, bundling subsidies with commitment induces respondents who might not otherwise use CCs to do so, boosting or hurting welfare depending on whether CCs are effective for such individuals. Consistent with the reduced form findings of low follow-through, we find that consumer welfare (net of the discount) is also reduced in these treatments. In contrast, *social welfare* increases due to modest increases in utilization; this finding is driven by losses in consumer welfare due to failed commitment contracts, which create profits for the firm. Finally, we conduct counter-factual simulations to assess whether welfare would be improved by offering CCs with different features. Given the degree of naivete regarding present bias that we estimate in this population, the simulations imply that providing (marginally) greater upfront commitment would lead to even greater consumer welfare losses.[9]

To summarize, we find that even a forceful intervention targeting an almost ideal sample of at-risk individuals, deploying commitment, subsidies, information provision, a high-quality provider and home-visit reminders, achieved very low overall utilization of preventive care. At the least, our results suggest that commitment contracts are not a panacea for low usage of preventive health care in low-income settings, such as rural India. In fact, there are good reasons to believe that offering these contracts may have even reduced consumer welfare in our setting. Of course, it remains possible that CCs would have more beneficial impacts in other settings, for instance, where individuals are more sophisticated about their present bias, or where individuals have more experience with the health services provided.

2 Study Setting and Research Design

Hypertension, otherwise known as high blood pressure, is one of the most prevalent chronic illnesses. In 2008, approximately 40% of adults aged 25 and over had been diagnosed with hypertension worldwide, and the condition accounted for at least 9.4 million deaths globally each year (World

[9] In Section 4, we provide evidence against alternative explanations for our results, including consumer confusion or social pressure to commit, forgetfulness, and shocks to the costs or benefits of making doctor visits.

Health Organization 2013). In low-income countries such as India, where the public health system is characterized by low service quality (Banerjee et al. 2004), the disease burden from hypertension is especially high: in India, the prevalence of hypertension over the past six decades has grown almost 13-fold nationally in urban areas and 8-fold in rural areas, and among individuals aged 45 year and older, 45% are considered to be at risk of the disease (API, 2013).

Yet most hypertensive patients in India go undiagnosed, and few are actively managing their condition, perhaps due the lack of overt warning signs or symptoms. For instance, one study in urban India found that only one-third of the study population were aware of their high blood pressure, and among those who knew, less than half acted to keep their blood pressure under control (Mohan et al. 2013). Amidst this backdrop, we leverage recent insights from behavioral economics to examine the determinants of preventive health among hypertensive individuals.

This study was carried out in four rural villages in the state of Punjab. The study was implemented in partnership with an organization that delivers primary medical care services and clean drinking water to rural markets using community health clinics. In particular, the Provider conducts "Hypertension Day" health camps wherein an experienced doctor from a nearby city visits each village every week to treat hypertension patients.[10] The consultation fee to see the doctor during these weekly clinics is Rs. 30 (excluding the cost of medicines and lab tests).[11] During the visit, the doctor takes health measurements (blood pressure, height, weight, and waist circumference), provides the patient with information about hypertension, and prescribes an appropriate treatment plan. The doctor also encourages the patient to make dietary and lifestyle changes such as decreasing salt intake and maintaining a healthy weight.

The standard medical advice, for individuals who either have been diagnosed as hypertensive or are at high risk, is regular monitoring of their condition through bi-monthly consultations with a doctor (API 2013b). Despite the large potential benefits, very few patients adhere to this recommendation. Combined with a commonly expressed desire to manage their condition, a candidate explanation for this lack of follow-through is time inconsistency or present bias.

2.1 Sample Selection

Since the health camps are targeted towards patients with high blood pressure, our study sample consists of individuals above the age of 30 who either have hypertension or are at high risk of developing it. We follow widely accepted medical guidelines and define hypertensive patients as those with systolic blood pressure above 140 or diastolic blood pressure above 90.[12]

To identify such individuals, in 2012 we carried out a census in the four villages where the

[10] Note that all individuals, even those without hypertension, are able to see the doctor during the camps. However, hypertension patients receive priority given that the program was launched specifically to address high blood pressure.

[11] As a point of reference, Punjab's current legal minimum wage for agriculture is Rs. 250 per day, or roughly 5 USD, thus the consultation fee is worth roughly one hour of labor at this rate.

[12] Both the Association of Physicians of India and the NIH define hypertension in this manner.

weekly camps were to be held, during which enumerators first screened all members of a particular household by taking their blood pressure readings using an automatic blood pressure measurement device (Figure 1).[13] If a blood pressure reading is above the thresholds previously described, the enumerator immediately invited the individual to participate in the study and complete the baseline survey. In the event that more than one household member had hypertension, the member with the more severe stage of the condition was invited to take part. Furthermore, in the event that more than one household member was at the same stage (i.e., Stage 1 or Stage 2), the member with the highest systolic blood pressure reading was invited. Finally, non-hypertensive but high-risk individuals were also identified using a score algorithm based on age, gender, family history of hypertension and diabetes, tobacco use, physical activity, and waist circumference.[14]

Across the four sample villages, a total of 20,824 individuals from 4028 households were screened in the census activity. From this initial pool, 2004 households with at least one hypertensive member and an additional 276 households with at least one high-risk member were selected for the study, yielding 2280 households. Of these, 1725, or 75.7%, accepted the invitation to participate in the study. The main sample consists of these 1725 individual respondents who completed the baseline survey (Figure 1).

2.2 Research Design

Immediately after administration of the baseline survey, the respondent was offered a commitment contract or discount coupons (or both) to visit the weekly health camp in their village 3 times in 6 months, a frequency in line with the medical guidelines described above. We randomized the type of contract offered to each household, stratified by hamlet (a geographic cluster within the village) and household head's education, by using a computer random number generator prior to the enumerator's visit. Specifically, households were either offered a fixed contract, a personalized contract, or no contract, all with equal probability:

Group 1: No Commitment Contract This group was not offered a commitment contract. Respondent only received information about managing hypertension and a flyer with the times and location of the health camps, which were provided to study participants in all treatment arms.

Group 2: Fixed Commitment Contract This group was offered a commitment contract for 3 visits to the health camps during a 6-month period. The respondent was required to pay in advance for all 3 doctor visits (Rs. 90, or Rs. 30 per visit). The respondent was also asked to pay an additional commitment amount of Rs. 45, which she receives back in equal installments of Rs. 15 at each visit. In other words, the respondent pays Rs. 135 up front, and receives Rs. 15 on each of the 3 visits; see Figure 2 for a graphical illustration.

[13] Enumerators were trained in operating the device, the Citizen CH-452 model, which has been validated by the ESH protocol and was selected for the project in consultation with a local medical doctor.

[14] This 100-point hypertension risk score algorithm is based on the current literature and was developed in consultation with doctors at the Provider.

Group 3: Personalized Commitment Contract Respondents in this group can choose their own commitment amount beginning as low as Rs. 0.[15] As above, the respondent receives this amount back in 3 equal installments every time she visits the doctor. The respondent is also required to pay in advance for 3 visits, so the total upfront payment is Rs. 90 for consultation fees plus the selected personalized commitment amount.

Each of these three groups was cross-cut with a price discount treatment in order to compare the effectiveness of commitment contracts with that of simple price incentives. Specifically, treatment individuals received 3 coupons that entitled them to half-price consultations (Rs. 15 instead of Rs. 30). To sum up, our baseline randomization yielded a total of 6 arms (5 treatment and 1 control), as shown in Figure 1.

The bundling of commitment contracts with discount coupons also has the useful feature of potentially making the former more attractive to naive individuals. In particular, time-inconsistent respondents who do not regard themselves as suffering from self-control issues may be persuaded to sign up for a commitment contract to take advantage of the price incentives. While they may not view the commitment contracts as useful upfront, they may benefit ex-post from the changed incentives to attend.

Respondents in the commitment contract groups could sign up for their respective contracts in several ways. First, they could accept the contract on the spot with the enumerator during the baseline survey, who subsequently collected payment. Second, respondents could sign up with the Provider's village health workers (VHW) and health coordinators (HC), both of whom were well-known in the village since they often go door-to-door to assess health needs. Specifically, around 3 to 4 days after the enumerator offered the commitment contract to a particular household, the VHW and HC visited households who had not yet signed up for the contract. The VHW and HC then asked these respondents whether they would like to take up the contract on offer, as well as reminding them about the health camp schedule. Note that the VHW and HC visited all households in the study to remind them about the camps, including those in the control group, to hold constant any effect the VHW and HC's visit may have. Lastly, respondents in the commitment contract groups were also able to sign up for the contracts directly at the clinic at any time during the course of the study.[16] In all cases, respondents had to go through a detailed intervention questionnaire with the enumerators to check that they had understood the contract/coupon they

[15] In practice, the respondent's chosen commitment amount is rounded up or down so that it is divisible by 3.

[16] Although the hypertension camps are only held once a week, the clinic is open Mondays through Fridays to sell medicine and conduct lab tests. Each respondent could sign up only for the commitment contract she was originally offered. While both the commitment contracts and price discount coupons covered 3 health camp visits, respondents were given the opportunity to renew these contracts and coupons at the clinic for the remainder of the 6-month program. These renewals were described to respondents when the contracts and discount coupons were initially introduced by enumerators. In the case of commitment contracts, for example, respondents who completed 3 visits in the first 2 months of the program could take up another commitment contract for 3 visits in the remaining 4 months. Similarly, for discount coupons, respondents who used up all 3 coupons in the first 2 months of the program could ask for another set of 3 coupons, which were valid for the remaining 4 months.

were being offered prior to signing up.

A final set of treatments were implemented two weeks before the conclusion of the 6-month program. In each village, half of the respondents were randomly selected to receive a short reminder about the hypertension camps. These respondents were personally visited by our team of enumerators, and were informed that there were 2 weeks left until the contracts or coupons would expire, if applicable. The other half of the respondents served as control, and did not receive the reminder. This intervention was designed to test whether inattention is an important factor in dampening attendance and health care utilization.

3 Reduced-Form Results

We use three main data sets in the analysis. First, a baseline survey (mentioned above) was conducted prior to presenting the treatments to respondents. This survey collected information on respondent and household characteristics, as well as the respondent's health status, health-seeking behavior and knowledge, as well as time and risk preferences.

Second, we collected data on attendance at the hypertension camps for all study participants. For the 6-month period in which commitment contracts and discount coupons were valid, a member of our field staff was present during the weekly camps in each village to record the household ID number and names of all study participants who came to see the doctor. Furthermore, we collected such attendance data for one month after contracts and coupons expired, which allows us to examine treatment effects in a setting where commitment contracts were no longer available.

Finally, an endline survey was conducted one week after the contracts and coupons expired, and asked questions similar to the baseline. In addition, it gathered information on the doctor visits and utilization of other health care providers. This survey also included the respondent's blood pressure, weight and waist circumference measurements, as well as self-reported dietary and exercise changes.

3.1 Summary Statistics

Baseline characteristics for our sample are shown in Table 1 (Panels A and B). Our respondents come from households with 5.5 members on average, with a mean annual household income of Rs. 102,000. 59% of our sample is female. Among household heads, the most common occupation is self-employment in agriculture (at 37%), and 45% can both read and write.

As intended, a large portion of our respondents also have characteristics that place them at risk for hypertension. For instance, average age is 53.7 years, and the risk of high blood pressure increases with age. 51% of our sample is overweight, defined as having a Body Mass Index (BMI) over 25. While 71% of our sample reported knowing they had hypertension, only 50% of these individuals are currently taking medication to manage their condition.

A randomization check of our commitment contract and discount treatments do not show any systematic, statistically significant differences across key variables (Table A.1).

Before discussing our structural estimates of the model (from Section 4), we first present experimental findings for (i) the take-up rates of commitment contracts, (ii) utilization of health care services, and (iii) health outcomes and behavior.

3.2 Contract Take-Up

Table 2 (Panel A) reports commitment contract take-up by treatment group. A number of patterns emerge. First, take-up without a discount is 13.6% (39 out of 286) for the fixed contract, and 14.1% (40 out of 283) for the personalized contract. So at the undiscounted price point, providing consumers with greater flexibility to design their own contracts does not increase demand. However, those who take up the personalized contract choose lower commitment amounts on average than those offered in the fixed contract (see Figure A.2), which implies that many consumers consider the fixed contract to be "too strong". Indeed, over 80% of personalized contracts had participants choosing only to pay the consultation fee upfront, without any additional commitment ($m = 0$).[17] The modest overall demand we find for unsubsidized CCs is similar to levels found in previous studies of CCs in the health domain, for smoking cessation (Gine et al. 2010; Halpern et al. 2015) and gym attendance (Royer et al. 2015); however, low demand for commitment is by no means universal (see Schilbach (2019) Table 1).

Second, discounts have a substantial impact on take-up: for the fixed contract with discount, 25.7% (72 out of 280) take up while for the personalized contract it is 38.7% (113 out of 292). This suggests that both (i) discounts have a marked impact on take-up and (ii) that the personalized contract and the discount are complementary. People are more likely to take up the contract with discount when they have the option to specify the amount that they commit. The pattern of results is consistent with the idea that a bundled discount induces consumers with low demand for commitment to sign up, particularly when they can choose small commitment amounts.

Table 3 (Panel A) presents results on contract take-up for a particularly interesting, albeit not pre-specified, sub-sample. This "ideal" sample is comprised of individuals who both believe "it is possible to be healthy with hypertension if blood pressure is frequently monitored" *and* who state that they trust the service provider. Conceptually, our contracts should be more attractive to these individuals, compared to those who think otherwise. There is indeed evidence consistent with this hypothesis. In particular, contract take-up is uniformly higher for the 439 respondents in this sub-sample. For instance, 19% of such individuals took up the undiscounted personalized contract, compared to 14% in the overall sample. Similarly, 49% of "ideal" sample individuals took up the discounted personalized contract, compared to 39% in the overall sample.

[17] We suspect that, had we allowed individuals in the personalized contracts to choose even smaller up-front payments, many would have done so.

In terms of gender heterogeneity, Figure A.3 presents the take-up rates differentially for female and male respondents. While there are no discernible differences in their take-up of the undiscounted commitment contracts, men did have significantly higher take-up rates of the discounted contracts than women in our sample: for instance, while 35% of male respondents signed up for a fixed contract with discount, only 20% of female respondents did so.

3.3 Doctor Visits

The next step is to assess the effect of being offered a commitment contract or discount coupon on health camp attendance. In our next finding, we find a uniformly low rate of attendance across all treatment groups, varying between 5 and 10% of the recommended number of visits during our study period (Figure 3). In particular, being offered a commitment contract by itself does not significantly increase health camp attendance on average. Compared to the 4.6% proportion of attendance in the control group, those offered the full-price fixed commitment contract made 5.4% of the recommended visits, and attendance for those offered a full-price personalized contract is a similar 6.6%. Allowing consumers to design their own commitment contracts thus has no effect on attendance. Furthermore, bundling discounts with commitment contracts does not increase attendance relative to simple price discounts. The patterns are identical if we measure utilization using an indicator for visiting at least once instead of the proportion of visits.

Analyzing health camp visits in the "ideal" sample, as defined above, indicates that our interventions may be slightly more effective for those in this latter group. Specifically, both the simple discount and the discounted personalized commitment contract treatment led to discernible increases in utilization when compared to the control group. As shown in Table 3, these effects can be sizable: among those in the "ideal" sample, 19.5% of individuals offered a discounted personalized contract went to the health camp at least once, compared to 6.5% of control individuals.

Another result is that, conditional on signing up for a commitment contract, consumers in contracts bundled with discounts do worse than those in contracts without discounts. Figure 3 shows that consumers who took up discounted contracts (Panels E and F) are more likely to fail to visit the doctor. This may be because some partially naive consumers may be attracted by the discounted consultations, but the commitment is not strong enough to increase their attendance. As such, more individuals appear to lose out when they are offered a discounted contract.

Unlike the differential take-up rates of discounted contracts among men compared to women noted above, there are no differences between female and male respondents in their attendance at the health camps. This is consistent with the male respondents being more over-confident than their female counterparts regarding the usefulness of commitment contracts, but not being any more likely to follow through.

Across the different CCs, between 62-77% of those who paid for a commitment contract failed to make even one visit to a health camp. This high rate of failure of commitment is consistent with partial naivete: such individuals are sophisticated enough to demand commitment, but naive enough to choose too-small commitment amounts. Our finding is consistent with recent work by John (2019), who finds that over half of individuals who accept commitment savings accounts default on their commitment. While not emphasized in the literature, it is also reflected in many other papers on commitment contracts. For instance, about half of those who took up CCs failed to reach their savings goal in Ashraf et al. (2006), two-thirds failed to cease smoking in Gine et al. (2010), and over a third failed to reach their gym-attendance target in Royer et al. (2015).

Finally, the low rate of health camp attendance cannot be explained by simple inattention: randomized reminders had no effect on doctor visits across all arms (Figure A.4).

3.4 Health Outcomes

Table 2 (Panel B) and Figures A.6 and A.8 present the average treatment effects on key health outcomes collected in the endline survey. Given the earlier results on doctor visits, it is perhaps not surprising that our treatments did not significantly improve health outcomes. For instance, the proportion of respondents with hypertension at endline ranged from 49% in the control and discount groups to 54% in the personalized CC group, a difference that is not statistically significant. The same pattern holds for other measures (e.g., pre-hypertension, overweight and obesity status). In terms of treatment heterogeneity (Table 3, Panel B), the estimated coefficients for the "ideal" sample are similar to those for the main sample. Therefore the experimental treatments did not lead to significant improvements in health outcomes even among those individuals who seemed most likely to benefit from them in our sample.

3.5 Reduced-Form Welfare Effects

We next adopt a utilitarian perspective to derive simple welfare results. Specifically, we combine the results on contract take-up and doctor visits, in order to compute the average personal benefit that would need to be generated by each doctor visit to offset the monetary losses incurred when contracts are purchased but no doctor visits completed.

For instance, in the fixed CC group, a total of 47 doctor visits were made, while 136 were purchased (at a price of Rs. 30 each). Recalling the standard commitment amount of Rs. 15 per visit, this implies that each realized visit needs to be worth at least Rs. 115 (or roughly 2 USD at the time of the study) for consumer utility to be non-negative. In contrast the benefits would only need to exceed the visit fee of Rs. 30 to be welfare-enhancing in the case of the control group.

Similarly, in the personalized CC group, a total of 57 visits were made, while 157 were purchased (at a price of Rs. 30 each). The vast majority of people who signed up for a personalized contract opted for zero additional commitment (Figure A.2), which implies that each realized visit needs to be worth at least Rs. 83 for consumer utility to be non-negative among this group.

We can compute similar statistics for the combined CC and discount treatments. In the fixed CC plus discount group, for instance, a total of 52 visits were made, while 234 were purchased (at a price of Rs. 15 each). Given the fixed commitment amount of Rs. 15 per visit, each realized visit needs to be worth at least Rs. 120 for consumer utility to be non-negative. Finally, in the personalized CC plus discount group, a total of 78 visits were made, while 376 were purchased (at a price of Rs. 15 each). Once again, given the large proportion of participants opting for zero additional commitment, this implies that each realized visit needs to be worth at least Rs. 72 for consumer utility to be non-negative for this group.

This raises the question of how large the benefits of doctor visits are. The structural model that follows estimates the *perceived* discounted benefit that enters the consumer's decision. Of course, these perceived benefits may differ from the true long-run benefits of the doctor visit, on which we do not take a stand. For context, however, our baseline survey reveals that the median expenditure on visits to a healthcare provider for treatment (rather than prevention) was Rs. 150 per visit.

While useful as a benchmark, these simple welfare calculations do not allow us to study the distribution of welfare changes as we move from one treatment to another, nor do they allow us to study the impacts of counterfactual contracts. To further investigate welfare effects, we structurally estimate a simple, pre-specified, commitment model, which we now describe.

4 Model

In this section, we set up and estimate a model of consumer behavior, with an emphasis on the potential roles that self-control and limited sophisticated may play. Our primary goals are to (i) estimate key time preference, sophistication, and demand parameters and (ii) study the implications of those estimates for the effects of commitment contracts in our setting, and their welfare properties. In particular, we use our estimates to study the welfare impacts of (marginally) more aggressive commitment contracts, which, in theory, have an ambiguous impact on consumer contract take-up, a positive impact on follow-through by consumers conditional on contract take-up, but potentially a negative impact on consumer *welfare* if many consumers still do not follow through conditional on contract take-up.

Model. We model consumer demand for commitment and preventive health care with three time periods, reflecting the key decisions that consumers make in our environment:

- $\mathbf{t = 0}$**:** Consumers choose whether to enter a commitment contract if offered one, depending on the randomized intervention. If they enter a personalized commitment contract, they also choose their commitment level.
- $\mathbf{t = 1}$**:** Consumers choose whether or not to go to the doctor for recommended treatment. In our empirical setting, consumers have many opportunities to attend in the relevant time window. In the model, for simplicity and tractability, we collapse these into a single time period and decision.

- $\mathbf{t = 2:}$ The health benefits of attending the doctor (or not attending) at $t = 1$ are realized.

We describe the model working backwards from $t = 2$. At $t = 2$ we denote the benefit from treatment at $t = 1$ (relative to no treatment) as b_i.[18]

At $t = 1$ consumers decide whether or not to go to the doctor for recommended treatment, which we model as a binary decision. In practice, most consumers who attend do so once in the six month time period over which we evaluate them. In our structural model, we therefore consider a consumer to have attended if they visited the doctor at least once during the study period, and assume that they realize the full benefit of treatment b_i in period two. Their utility from going to the doctor during $t = 1$ is:

$$\begin{aligned} U_{i,Attend} &= \beta(X_i^{''})\delta b_i - C(X_i') - p_i \\ p_i &= f_i - d_i \text{ if no commitment contract} \\ p_i &= -m_i \text{ if commitment contract} \end{aligned}$$

Here, p_i is the marginal payment consumers make to attend at $t = 1$. In our environment, because consumers may have previously entered a commitment contract at $t = 0$, this price is person-specific and may be negative, i.e., consumers may be paid to go to the physician at $t = 1$. We break down p_i into three relevant components: (i) f_i, the standard per visit fee for a consumer with no commitment contract, (ii) d_i, a per-visit discount given to consumer i (known in advance of the visit), and (iii) m_i the incremental payment a consumer receives above and beyond the typical per-visit fees if they entered into a commitment contract. We assume that monetary costs and rewards in period $t = 1$ translate into losses and gains in consumption utility in the same period – a plausible assumption given the small sums of money, and the fact that $t = 2$ represents the distant future where health benefits are realized. The $t = 1$ decision also depends on $C(X_i')$, consumer i's non-financial costs of going to the doctor. Empirically, we allow this cost to depend on observable variables X_i' such that $C(X_i') = \alpha_C + \kappa_C X_i'$.

In our environment, when a consumer enters into a commitment contract, they never pay when going to the doctor at $t = 1$ and typically receive money in return. Given the specifics of our environment, the possible p_i for consumers at $t = 1$, denoted in rupees per visit, are:

$$p_i = \begin{cases} 30 & \text{if Control} \\ 15 & \text{if Discount Only} \\ \text{-}15 & \text{if Fixed Contract} \\ \text{-}m_i & \text{if Personalized Contract} \end{cases}$$

We allow consumers to potentially be present-biased through the parameter β, which reflects the extent to which consumers more heavily discount all future periods relative to the present period.

[18] We assume b_i is distributed normally conditional on observed variables X_i as $F(b_i|X_i) = \mathcal{N}(\alpha_b + \kappa_b X_i, \sigma_\epsilon)$. We allow for observable heterogeneity X_i to impact the mean benefit, with unobserved heterogeneity independent of X_i with variance σ_ϵ^2.

We allow β to vary across individuals, depending upon a set of observable variables $X_i^{''}$ and an unobservable component ς_i such that $F(\beta_i^*|X_i^{''}) = \mathcal{N}(\alpha_\beta + \kappa_\beta X_i^{''}, \sigma_\varsigma)$ with $\beta_i = \max[\min[\beta_i^*, 1], 0]$.

A consumer chooses to visit the doctor if and only if $U_{i,Attend} \geq 0$ at $t = 1$. At $t = 1$, when consumers decide whether to go to the doctor or not, they are potentially subject to present bias since they have to incur the cost of visiting the doctor now, but do not receive the benefits from treatment until $t = 2$.

We now turn to the consumer decision of whether to enter into a commitment contract at $t = 0$. Consumers who are offered a commitment contract decide whether or not to take up that contract at $t = 0$, based on their *perceptions* of their present bias and whether or not they believe the commitment contract will allow them to overcome present bias that prevents them from going to the doctor at $t = 1$. Consumers choose to take up the fixed commitment contract, where they commit the fees for three visits plus an additional 45 rupees up front (15 rupees per visit), if they perceive that the commitment has positive value. In the treatment group with no per visit discount, this occurs when a consumer believes at $t = 0$ that (i) they want themselves to go to the doctor at $t = 1$, (ii) they will not go at $t = 1$ without additional commitment, and (iii) the commitment amount in the contract is strong enough to induce them to go at $t = 1$.[19]

Formally, a consumer with a fixed contract offer with no lump sum discount will take-up that offer if and only if the following conditions hold:

$$\begin{aligned} \delta b_i &\geq C_i(X_i^{'}) + 30 && (1) \\ C_i(X_i^{'}) + 30 &\geq \hat{\beta}(X_i^{'''})\delta b_i \\ 15 &\geq C_i(X_i^{'}) - \hat{\beta}(X_i^{'''})\delta b_i && (2) \end{aligned}$$

Note an important assumption: given the short length of the $t = 0$ time period and consumers' anticipation of receiving this commitment amount back upon attending at $t = 1$, we assume that they do not cut back on other consumption at $t = 0$ due to the perceived temporary allocation of the commitment amount m_i to the provider. That is, liquidity constraints are not so tight that paying up front for commitment translates into a full and immediate drop in consumption. This is both empirically plausible in our setting, and similar to the assumptions in models of up-front payment for commitment, such as DellaVigna and Malmendier (2004) and Duflo et al. (2011).

The conditions for taking up a fixed commitment contract with a discount are similar to those for a fixed contract with no discount. However, because the discount is bundled with the commitment contract in our discount treatments, the condition simply becomes that the individual believes they will go to the doctor at $t = 1$ with the contract (without the discount; they will not take up the

[19] It is important to note that the simplifying assumption in the model of a binary follow-through choice plausibly causes us to underestimate consumer losses from signing up for a commitment contract in our setting. The model does not allow for partial follow-through to commitment. As a result, consumers who attend once are counted as having followed-through, though they may experience higher losses in practice by paying for second and third visits that they do not follow through on. Yet since many consumers choose commitment but do not follow through at all, the model arguably approximates our observed environment well despite this simplification.

commitment device if they think they will go to the doctor at $t = 1$ without the contract). Thus, they will take up the fixed discount contract if the following two conditions hold:

$$\delta b_i \quad \geq C(X_i^{'}) + 15 \tag{3}$$

$$15 \quad \geq C(X_i^{'}) - \hat{\beta}(X_i^{'''})\delta b_i \tag{4}$$

If a consumer is in a treatment where they are offered a personalized commitment contract without a bundled up front discount, they take-up the contract if equation 1 is satisfied. This is true because any demand for commitment is sufficient to take up a personalized contract, given that they can choose a commitment amount greater than or equal to 0. If they take up the contract, they commit a per visit amount m_i up front (on top of normal fees) such that:

$$m_i = \max[C_i(X_i^{'}) - \hat{\beta}(X_i^{'''})\delta b_i, 0] \tag{5}$$

Note that we assume that consumers choose the smallest permitted commitment amount they perceive as being necessary to ensure follow-through. This could result from, say, a small (unmodeled) liquidity cost. For the personalized contract with a discount, a consumer accepts if the equation 3 above holds, and, if so, they commit the same per-visit amount that is equal to what they would choose without a discount, as described above.

$\hat{\beta}$ represents consumers' *beliefs* about the degree of present-bias they will have when making the decision of whether to go to the doctor at $t = 1$. As is typical in the literature, we assume that $\beta \leq \hat{\beta} \leq 1$. When $\beta = \hat{\beta}$ a consumer is *sophisticated* about their present-bias, i.e., they exactly perceive the extent to which they will be present-biased at $t = 1$. When $\beta < \hat{\beta} = 1$ a consumer is *fully unsophisticated* or *fully naive*: they think they will have no present-bias in the future, though they actually will. When $\beta < \hat{\beta} < 1$ consumers perceive some but not all of the present-bias they will have at $t = 1$ and they are said to be *partially sophisticated* or *partially naive.* A key goal of our empirical analysis is to estimate the joint distribution of β and $\hat{\beta}$.

Our primary specification allows for unobservable heterogeneity, in addition to observable heterogeneity. We parametrize unobservable heterogeneity in $\hat{\beta}$ with a two parameter Beta distribution. Specifically, we assume that, across consumers in the population, the distance that $\hat{\beta}_i$ is between β_i and 1 is distributed Beta with parameters τ_1 and τ_2. Formally:

$$\hat{\beta} \rightarrow \mathcal{B}(\tau_1 + \kappa_{\hat{\beta}} X'''_i, \tau_2)$$

$$\hat{\beta}_i = \beta_i + (1 - \beta_i)\hat{\beta}$$

This statistical specification for heterogeneity in $\hat{\beta}$ flexibly allows for $\hat{\beta}_i$ to vary between β and 1 while remaining within those boundaries for each consumer. In an alternative specification that we investigate for robustness, we also allow for the distance that $\hat{\beta}$ is between β and 1 to be correlated with β to add flexibility in modeling the behavior of low β consumers relative to high β consumers.

Discussion and Alternative Mechanisms. The model makes a number of assumptions and simplifications in order to focus on the key issues of self-control, commitment, and affordability. It therefore excludes a number of potential alternative channels, which we discuss below.

Uncertainty and learning about benefits. First, we assume that consumers are fully informed about the potential health benefits of preventive health care both when choosing commitment at $t = 0$ and when choosing attendance at $t = 1$. In our experiment, we implemented an extensive information campaign to all consumers at baseline, including those in the control group, to mitigate the role of limited information. That said, consumers in our environment are often unfamiliar with, and potentially suspicious of, western-style medical care, and the health provider we work with was relatively new to the study area. This could explain low overall demand for doctor visits, as well as low demand for commitment (if consumers would not like to attend even in the long run). However, it would only explain failure to follow through on commitment contracts if consumers update negatively about the benefits over time, e.g., by learning from the early attendees that the quality of the provider is lower than expected (or the costs of attending are higher than expected). To measure this, we included baseline and endline survey questions asking the respondents to report their trust in and satisfaction with the provider. We find no change in the average level or distribution of reported trust in the provider over time, and individual changes over time are uncorrelated with attendance. Nor do those who fail to follow through report reductions in trust over time. However, about 6.5% of those who failed to follow through did report dissatisfaction with the Provider as a reason for not attending in the endline survey.

Social pressure. Another assumption is that consumers do not purchase a commitment contract simply due to social pressure exerted by surveyors – with no intention of actually attending. Scripts were carefully designed and implemented to explain what a commitment contract is, while maintaining neutrality about whether a consumer should purchase a contract or not. The approximately \$3 required is a meaningful share of the daily minimum agricultural wage (\$4) and of average daily household income (\$5.6) – a much higher fraction than typically estimated social pressure costs (DellaVigna et al. 2012). Having said that, 12% of consumers who took up a commitment contract mentioned persuasion or social pressure (broadly construed) as a reason for signing up when debriefed during the endline survey.

Memory and Salience. Consumers might have simply forgotten that they signed up for a commitment contract. To address this concern, we cross-randomized the entire sample to receive reminders of the health camps (and the CCs, if they had signed up) through a home visit two weeks before the health camps ended. As noted above, these reminders had no effect on health-camp attendance, suggesting that our results were not driven by limited memory (Figure A.4). Another possibility is that our initial information intervention and the offer of the commitment contract itself temporarily raised the salience of hypertension, causing an increased valuation of the doctor visits which faded over time. This would not have necessarily been captured in our baseline survey, which preceded the offer of the contract. While we do not have any direct evidence in favor of this mechanism, we cannot rule it out. We caution that this may more generally be an issue in the take-up of commitment devices.

Uncertainty in costs of attending. Another assumption is a lack of uncertainty in the environment, especially in the costs of going to the doctor. Travel costs are unlikely to be a major obstacle, as around two thirds of our respondents live less than 1km away from their local clinic, while the remaining third live between 1 km and 5 km away. We assume for simplicity that the costs (including non-pecuniary costs) are fixed and known in advance. Suppose, instead, that consumers understand their average cost of going to a doctor, but idiosyncratic shocks may increase or decrease their costs on a given day. We first note that, in practice, individuals have six months of weekly opportunities to attend, and are largely retired or self-employed adults, for whom the opportunity cost of time is likely to be low at least sometimes. Moreover, a substantial majority of those who sign up for commitment never attend. Thus, mean-zero idiosyncratic weekly shocks are very unlikely to explain our results. Instead, could aggregate shocks at the village level, or permanent shocks affecting a large share of individuals be important? For example, unexpected crop failure might have induced a large share of individuals to temporarily migrate for work. Yet our endline survey finds no evidence of such aggregate shocks.

Utility costs at t=0. We assume that pre-paying for the CC at $t = 0$ does not involve immediate utility costs (which a present-biased agent might particularly dislike). This is consistent with models of present bias, which are understood to apply to utility, rather than to money. Since monetary costs today should not translate into substantial immediate drops in consumption (excepting extreme liquidity constraints), pre-paying a commitment amount – which is anticipated to be returned in a few weeks – should not result in meaningful consumption reductions at $t = 0$. Indeed, present-biased agents may be extremely patient in the long run, and pre-payment as a form of commitment is an established phenomenon which has similarly been modeled as present bias (see, e.g., DellaVigna and Malmendier (2004) and Duflo et al. 2011).

Poor Comprehension. A final possibility is that individuals simply did not understand the contracts they had signed up for. In fact, participants went through detailed comprehension checks before being permitted to sign up for the contracts, including being asked to work through the financial consequences in the case of non-attendance. Answering the comprehension questions correctly on the first attempt does not predict failure-rates of the commitment contracts.

In summary, we do not find positive evidence in favor of the specific confounds we consider. However, it is entirely plausible that they play *some* role in generating the behaviors we observe. In recent work, Carrera et al. (2019) have argued for an important role for experimenter demand effects and imperfect perception of contract value in generating take-up of commitment, even in cases where such demand is theoretically predicted to be low (e.g. offering commitment contracts to exercise less). While the contracts we consider are simpler than those in Carrera et al., some combination of social pressure and a temporary salience of hypertension due to the offer may explain some of the take-up of commitment without subsequent doctor visits we observe.

Identification. We now discuss econometric identification of the model, given these assumptions. The key parameters to separately identify are (i) α_β, κ_β, and σ_ς for β, (ii) τ_1, τ_2, $\kappa_{\hat{\beta}}$ for $\hat{\beta}$, (iii) α_b, κ_b, σ_ϵ for b, (iv) α_C, κ_C for C, and (v) δ.

The basis for our identification is the randomization of consumers into the six different experimental arms, including the control group and five treatments. Our identification arguments depend crucially on the multiple different treatments along with the experimental randomization, which provide cross-sectional variation in commitment contracts and pricing that impacts the distribution of decision paths.

Here, we discuss identification of β from $\hat{\beta}$. We defer the remainder of the identification discussion to the appendix. As a result of our experimental randomization, the joint distribution of β and $\hat{\beta}$ for consumers in each treatment will be the same as sample size goes to infinity. For fixed values of the other parameters, different combinations of β and $\hat{\beta}$ imply different sequential decisions for consumers randomized into commitment contract treatments.

We illustrate this in Figure 4, which shows how the choice to take up a fixed commitment contract or not (when offered) at $t = 0$ identifies regions of $\hat{\beta}$ for a given consumer. Consumers with high $\hat{\beta}$ do not believe they have genuine commitment issues so are unlikely to demand commitment without a corresponding bundled discount. Consumers with medium $\hat{\beta}$ will demand commitment when they intrinsically value recommended medical care, because it will help them overcome their procrastination at $t = 1$. Consumers with low $\hat{\beta}$ will not take up a fixed contract, even if they want commitment to consume medical care, because they perceive the fixed commitment amount will not be enough to get them to visit the doctor at $t = 1$.

Figure 4 also examines what different sequences of decisions imply for $\hat{\beta}$ and β jointly. For example, Region 2 with medium $\hat{\beta}$ and low β is a region where consumers think commitment is valuable for them, but once they commit money up front they do not follow through on their commitment at $t = 1$ because their β is low. Similarly, Region 5 has medium β and high $\hat{\beta}$, showing consumers who think they will go regardless of whether they have commitment, so do not purchase a contract without a discount, but end up only going if they have a commitment contract. This group of consumers will benefit from being offered a commitment contract paired with a bundled discount, relative to our other treatments.

Crucially, our experimental design generates multiple kinds of variation to separately identify β and $\hat{\beta}$. In addition to partitioning the space of $(\beta, \hat{\beta})$ based on the sequence of choices made when offered a commitment contract (as shown in Figure 4), having cross-cutting treatments that pair discounts with commitment contract offers helps to identify the joint distribution of $(\beta, \hat{\beta})$ by shifting *anyone* who thinks they should go to the doctor into a commitment device (rather than just people who feel they need commitment). Additionally, having the control group and treatment with per visit discounts only helps identify the distribution of $\beta\delta b$ for consumers separately from $\hat{\beta}$.

Finally, the personalized commitment contract treatments help identify $\hat{\beta}$ more precisely relative to β because each consumer in this arm makes an individual-specific choice signaling their $\hat{\beta}$.

Estimation. We estimate the model with a smoothed Accept-Reject simulated maximum likelihood methodology that, given the candidate parameters, matches the predicted decision paths for consumers in the population to their actual decision paths (see, e.g., Handel (2013) for another applied example with a similar approach). Define the set of parameters to be estimated as Θ.[20] For a consumer with set of observables X in treatment T, we match their sequence of decisions (which depends on T) to the predicted sequences of decisions for candidate parameters, and choose the parameters with the best match given choices across the control and all five treatments.

There are three types of decisions that could enter the decision path, and hence the likelihood function, for a given individual. The first is choice of commitment contract (if offered). The second is what amount they commit specifically if the contract offered is a personalized contract. The third is whether the visit the doctor or not at $t = 1$. Appendix C provides substantial detail on the likelihood function and estimation process, which we omit here for parsimony.

5 Structural Results and Counterfactuals

Table 4 presents the primary model estimates. The key parameters are the joint distribution of β and $\hat{\beta}$. We present three specifications: (i) a baseline specification with limited observable heterogeneity, (ii) this same baseline specification incorporating correlation between the unobserved components of β and $\hat{\beta}$, and (iii) the primary specification that incorporates observable heterogeneity on a range of potentially important dimensions.

All three specifications show relatively similar results for the joint distribution of β and $\hat{\beta}$. Consumers have relatively low β on average, indicating a meaningful degree of present-bias when making the decision of whether or not to visit the doctor at $t = 1$. In our primary specification the average β is 0.365. There is meaningful dispersion in the estimated β as well, with an estimated population standard deviation of 0.395 (incorporating truncation at 0 and 1). 39.8% of consumers have $\beta = 0$, indicating that when it gets to $t = 1$ a meaningful portion of consumers feel that the doctor visit at $t = 1$ is not valuable at all in that moment. On the flip side, 15.1% of consumers have $\beta = 1$, corresponding to no present-bias. Our primary specification also estimates a lower average β for males relative to females, though we find limited gender effects for $\hat{\beta}$. The mean and standard deviation of β are both slightly lower in the baseline specifications.

The estimated mean of $\hat{\beta}$ in our primary specification is 0.795, with a population standard deviation of 0.13, most of which comes from unobservable heterogeneity. Thus, when making decisions at $t = 0$, consumers perceive that they will have some present-bias at $t = 1$ but much less than they will actually have. Figure 5 presents the empirical joint distribution of β and $\hat{\beta}$,

[20] In our primary specification, these parameters include α_β, κ_β, σ_ς, τ_1, τ_2, $\kappa_{\hat{\beta}}$, α_b, κ_b, σ_ϵ, α_C, κ_C, and δ.

which can be compared to Figure 4 and the corresponding discussion of how sequences of individual decisions identify β and $\hat{\beta}$.

It is worth contrasting our estimates with the existing literature. Our estimate of average present bias $\beta = 0.365$ is lower than recent estimates from lab experiments but within the range of estimates from field experiments. For instance, Augenblick et al. (2015) estimate a mean $\beta = 0.9$ and Augenblick and Rabin (2018) estimate a mean $\beta = 0.83$, both from real-effort lab experiments. From field settings, Mahajan et al. (2019) estimate a mean $\beta = 0.31$ from commitment contracts for insecticide-treated bednets in India, similar to our estimates, while Carrera et al. (2019) estimate a mean $\beta = 0.66$ from incentives for gym attendance in the United States. An extended model which allows some role for social pressure in sign-ups or temporarily raised valuation of the health service at the time of sign-up would potentially result in estimates of β more in line with the carefully-controlled lab experiments. Without these features, our model struggles to fit the low rates of follow through even in the subsidized contracts (where relatively naive individuals may have taken up the contracts), pushing the estimates of β to levels that are arguably too low.

Our estimates of partial naivete fall broadly within the range in the recent literature. Augenblick and Rabin (2018) estimate a mean $\hat{\beta} = 1$ (full naivete). Our model naturally estimates less naivete ($\hat{\beta} = 0.795$), given the meaningful demand for commitment we observe. This estimate is in line with other recent estimates from the field, where Carrera et al. (2019) estimate $\hat{\beta} = 0.88$ and Mahajan et al. (2019) estimate $\hat{\beta} = 0.73$.

Table 4 also presents a baseline specification that allows for correlation in the unobserved heterogeneity terms for β and $\hat{\beta}$. This is implemented by allowing for different distributions of $\hat{\beta}$ conditional on whether β is low ($\beta < 0.5$) or high ($\beta \geq 0.5$). There is only limited correlation in these unobserved heterogeneity components: the mean distance from β to 1 is 47% of the way towards 1 from β for ($\beta < 0.5$), and 55% of the way there for ($\beta \geq 0.5$).[21] Both distributions of $\hat{\beta}$ have little dispersion, given β. Since these correlations have limited impact on predicted behavior, we omit them for parsimony in our primary specification, where $\hat{\beta}$ is on average 66% of the distance from β to 1, for all β.

Across our specifications, we estimate the perceived benefits and costs of going to the doctor based on consumers' revealed preferences. In our primary specification, which estimates both observed and unobserved heterogeneity in costs and benefits, the mean perceived benefit is 61.3 rupees (roughly 1 USD at the time) while the mean perceived cost is 26.5 rupees. Consumers who have high blood pressure and who are thus at high risk for hypertension-related health problems have higher preferences for attending the doctor, equal to 40.34 rupees on average. Consumers high on the sickness index (which is not specifically related to hypertension) value going to the physician for hypertension treatment by 13 rupees less than other consumers. Consumers with prior hypertension medication value going to the doctor by 13 rupees less as well. Males value

[21] Note that β and $\hat{\beta}$ are mechanically correlated by the fact that $\hat{\beta}$ must be between β and 1. The correlation being estimated here is whether the proportional distance from β to 1 for $\hat{\beta}$ varies as a function of β.

going to the doctor by 45.37 rupees more than females, while literate consumers value going to the doctor by 36.56 rupees more than illiterate consumers. For consumer costs, being employed increases costs very slightly, by 4 rupees on average, while our baseline specifications show that distance to the camp has a limited impact on preferences.[22] The unobserved component of perceived benefits, σ_ϵ has a standard deviation of 197.7 rupees, around the mean of 61, implying this is an important component of consumer demand in our setting.

It is important to note that, though the discount factor δ is technically identified from perceived benefit b, in practice with our sample these factors are not robustly estimated separately from one another across our specifications, due to the limited sample size. δ is estimated to have a value of 0.234 in our primary specification, but has estimated values of 0.780 and 0.687 in our two baseline specifications. Since choices at $t = 0$ are based on the perceived net benefit of attending the doctor $\delta * b - c$, we present the distribution of this net benefit in Table 4 since this is a quantity that is robustly identified. For our primary specification, the mean of this net benefit is -19.3, with 25th quantile equal to -55.50 and 75th quantile equal to 17.93. At the high end of the distribution, this net benefit is greater than 100. The estimated mean net benefit is similar to this in both baseline specifications, while the spread is similar but slightly larger. This suggests that, though it is difficult to separately identify δ and b in practice, we are robustly identifying the perceived net benefit of visiting the doctor $\delta * b - c$, which is the important sufficient statistic for identifying the joint distribution of β and $\hat{\beta}$; the appendix contains further discussion of the identification of δ, b, and c.

Table 5 (Panel A) presents some statistics related to model fit. The model predicts take-up of 7.2% for the commitment contracts without bundled discounts, compared to 13.8% in the data. It predicts take-up of 26.7% for commitment contracts with bundled discounts, compared to 32.1% in our data. It predicts 4%, 8%, and 8% doctor attendance for individuals in the control, fixed contract with no discount, and personalized contract with no discount treatments respectively, compared to actual values of 8%, 9%, and 9% in the data for these treatment groups. These equivalent predicted values for the treatments with bundled discounts are 8.4%, 18%, and 14.3% while these moments in the data are 14.4%, 12.5%, and 13.3%. For the personalized commitment treatments, the average additional commitment amount predicted by the model is 1 rupee (close to the minimum) while in the data this average is 4.55 rupees. The model fit overall is thus quite strong in replicating the levels of these three key sets of moments.

Welfare and Distributional Implications. We now turn to the welfare implications of the different commitment contract and discount offerings. We discuss both the mean welfare implications and the distributional implications, since commitment contracts generate both winners,

[22] 15% of our observations have missing distance values, which we measure as either being less than 1 km from the doctor, between 1 and 5 km for the doctor, or greater than 5 km. As a result, we include this in our baseline specification, and once we verify that it has a very limited impact there (1.36 rupees per kilometer additional cost per visit) we do not include this in our primary specification so we can include these 15% of observations.

who use the commitment to help them go to the doctor, and losers who make a commitment but do not follow through on it. All welfare criteria take a long-run perspective in the sense that they do not consider present bias β to be welfare-relevant, and only consider welfare from the perspective of the $t = 0$ decision-maker. This means that consumer welfare in each treatment equals their discounted benefit of going to the doctor, minus the costs of going (both pecuniary and non-pecuniary), and minus any pledged commitments amount lost due to not following through:

$$CW_{i,t} = 3 * (\delta b_i - c_i - f_i + d_i \mathbf{1}[D_{i,t} = 1] - m_{i,t}\mathbf{1}[C_{i,t} = 1, A_{i,t} = 0]) \tag{6}$$

Here, $\mathbf{1}[D_{i,t} = 1]$ is an indicator taking on value one if a consumer receives a per-visit discount in treatment t and $\mathbf{1}[C_{i,t} = 1, A_{i,t} = 0]$ is an indicator taking on value one if a consumer accepts a commitment contract but loses their pledged amount due to not following through on that commitment. We multiply per-visit welfare by 3 since all decisions (including commitment) are made with respect to a bundle of three visits in the model, a useful rescaling to match the empirical setting. The difference in welfare between two treatments T and T' is:

$$\Delta CW(T, T') = \Sigma_{i=1}^{N} CW_{i,T} - \Sigma_{i=1}^{N} CW_{i,T'}$$

It is also useful to consider consumer welfare net of the discount. If a planner providing discounts cares about the health behavior, but considers the discount to be a net transfer, e.g., coming from some other transfer program, then this is the relevant measure of consumer welfare. We define this consumer welfare net of discounts as:

$$\begin{aligned} CW_{i,T}^{ND} &= 3 * (\delta b_i - c_i - f_i - m_{i,T}\mathbf{1}[C_{i,T} = 1, A_{i,T} = 0]) \\ \Delta CW^{ND}(T, T') &= \Sigma_{i=1}^{N} CW_{i,T}^{ND} - \Sigma_{i=1}^{N} CW_{i,T'}^{ND} \end{aligned}$$

Finally, we also consider a social welfare criterion that incorporates firm costs and profits. If the planner counts firm welfare the same as consumer welfare, then a consumer paying money to the firm from a commitment contract, but not following through, is just an even transfer, rather than a cost. Thus, social welfare gives a sense of how much a treatment improves consumer medical benefits, net of firm costs and consumer attendence costs. Our primary approach assumes that costs are fully marginal, denoted c^* and equal to the cost of a visit in the baseline environment of 30 rupees. Given this, we define social welfare as follows:

$$\begin{aligned} SW_{i,T} &= 3 * (\delta b_i - c_i - c^*) \\ \Delta SW(T, T') &= \Sigma_{i=1}^{N} SW_{i,T} - \Sigma_{i=1}^{N} SW_{i,T'} \end{aligned}$$

This social welfare criterion will yield strictly higher welfare than consumer welfare netting out discounts, CW^{ND}, since some of the lost consumer surplus is transferred to the firm but the firm does not incur the cost of the consumer's care.

Table 5 presents these welfare results. Relative to the control treatment, the discount only treatment increases CW by 5.47 rupees, averaged across all consumers, including those with no welfare change. The first panel in Figure A.10 plots the distribution of consumer welfare impacts (excluding zeros) for the discount-only treatment relative to control. Even once discounts are netted out, the discount-only treatment increases consumer welfare (CW^{ND}) by 1.51 rupees per consumer, since attendance and thus health benefits increase.

In contrast, the undiscounted fixed commitment contract treatment causes consumer welfare *losses* (under both criteria) of 5.08 rupees per consumer. 3% more consumers attend the doctor under the fixed contract, which leads to a benefit, but over half (59%) of the consumers taking up this contract do not follow through on their pledge and lose money as a result. On net, the availability of this fixed commitment contract is worse for consumers than no intervention at all. The second panel of Figure A.10 plots the entire non-zero distribution of these welfare impacts. The undiscounted personalized commitment contract treatment similarly reduces consumer welfare by an average of 4.12 rupees.

When discounts are bundled with commitment, the flexible commitment contract becomes worse than the fixed contract. For example, using CW^{ND} as a criterion, the personalized contract with discount is 2.89 rupees worse on average than control, compared to 2.42 for fixed without discount. This occurs because, with personalized contracts, bundled discounts bring more naive and partially naive consumers into the commitment contract, but they choose too low a commitment amount to ensure follow-through. 12.51% of consumers lose money under personalized contracts with discount, the highest percentage of any treatment, while fewer consumers go to the doctor (14.25%) than under the fixed contract with discount (18.00%). Conversely, the fixed commitment contract with discount makes consumers slightly better off than the control (or the discount-only treatment) under baseline consumer welfare CW, due to the stronger commitment; see also the third panel of Figure A.10. 8.7 % of consumers lose money relative to the discount only treatment, but 9.6 % more consumers are predicted to go to the doctor.

These results have a number of implications. First, the option to enter into a commitment contract can reduce consumer welfare on average, and generate more losers than winners. This occurs in our environment where consumers exhibit (i) a high degree of present-bias and (ii) are relatively unsophisticated about that present-bias. Second, whether fixed or personalized commitment contracts are a better option depends on the tradeoff between the money partially naive consumers might lose if they do not follow through versus the increased probability they will follow through with a higher commitment amount. With bundled discounts, fixed contracts are better than personalized ones in our setting since the impact of the fixed contract on follow through outweighs the incremental losses from naive consumers who do not follow through. Without bundled discounts, the reverse is true, and the losses from those not following through outweigh the benefit from increasing the probability of follow-through.

Third, the discounts to consumers in our environment have a meaningful and positive impact under both consumer welfare metrics, relative to corresponding treatments without discounts. This need not necessarily be the case if the discount selects in consumers with high naivete about present-bias relative to those who leverage these discounts to attend the doctor.[23] Fourth, though the treatments with discounts are positive for consumer welfare on average, they do generate more losers than the treatments without discounts. Finally, though the best treatment for average baseline consumer welfare CW in our setting is the fixed contract with bundled discounts treatment, once discounts are netted out in CW^{ND} the discount-only treatment is the best. Once discounts are netted out of consumer welfare, all commitment contract treatments are welfare negative, due to limited consumer follow-through on these commitments.

Table 5 also presents the social welfare results. Social welfare is positive for all treatments relative to the baseline control, and especially for the commitment contracts with bundled discounts. Overall, more consumers go to the doctor under those treatments, and the benefits to the consumer of going outweigh the social costs (consumer costs of attendence and firm costs). In these treatments, while social welfare increases, on average all of the surplus accrues to the firm while consumers are on average, net losers (though some gain and some lose). The table shows similar, but smaller impacts for the CC treatments not bundled with discounts. These results illustrate both that (i) firms can benefit from offering commitment contracts, at the expense of consumers and (ii) that it is important for such policies whether a planner wants to consider consumer or social welfare as their benchmark.

One important assumption behind the welfare analysis is that the perceived benefits from doctor visits that we estimate correspond to the actual welfare-relevant benefits. It could instead be that consumers do not correctly perceive the benefits of preventive medical treatments, especially in our setting where consumers have limited experience with formal scientific bio-medicine. Table 5 presents consumer welfare results, with CW^{ND}, for each treatment, assuming the actual benefit consumers get from going to the doctor is larger than their perceived benefits. We present three cases where actual consumer benefits equal 500, 5,000 or 50,000 rupees (approximately 10, 100, and 1,000 USD per visit, respectively). As the benefits from treatment get larger, the number of losers (i.e., those who purchase contracts but do not go to the doctor) and their welfare losses remain the same, but the actual benefits to winners are now considerably larger. Thus, even if commitment contracts only encourage a small percentage of consumers to follow through, they might be worth implementing, since those consumers might benefit by a much larger amount than the losers lose. Moreover, in a model with uncertainty, even individuals who realize they only have a small chance of following through may optimally sign up for commitment if the benefits of attending are sufficiently large.

Counterfactual Contracts. We are also able to use our structural estimates to study the welfare impacts of unobserved contract configurations. We study a grid of fixed commitment

[23] In addition, discounts could bring in consumers who value the treatment at less than f, which would lead to a consumer welfare loss in our setting when netting out discounts.

contracts for the discount d and fixed per-visit commitment amount m. We focus on fixed contracts since personalized contracts will always have consumers choosing the same amount m^* if they take up the contract (which in our environment is typically close to 0); in practice, the fixed contracts with $m = 0$ mimic the personalized contracts in our setting closely. We study five values of d (0, 7.5, 15, 22.5 and 30) and four values of m (0, 15, 30 and 45) such that we have 20 commitment contract in our simulations. We also present the discount only results (no bundled commitment contract) for comparison.

Table 6 presents four statistics for the case where the entire population we study is offered each potential contract. It presents (i) the % of consumers attending the doctor under each offered contract, (ii) the mean consumer welfare impact of each contract netting out discounts (CW^{ND}), (iii) the mean baseline consumer welfare impact (CW) of each contract relative to our control treatment, and (iv) the % losers (from a CW perspective) relative to the control case. The simulations assume that only one contract is offered to consumers in each case, and thus does not consider things like menu design with multiple commitment contract options.

A number of results emerge. First, from a positive standpoint, doctor attendence is always increasing with the discount d conditional on a given commitment amount m. For example, for $m = 15$, the percentage of consumers going to the doctor increases from 7.36 to 21.61 as the discount increases from 0 to 30 (which is basically giving the service away for free, except for the commitment amount). Second, for fixed d, doctor attendence is always increasing in m in our simulations. In our model, weakly more consumers will take up contracts with higher commitment amounts, while those larger commitment amounts will increase participation at $t = 1$. If consumers are apprehensive about higher commitment amounts because of uncertainty about follow-through ability, something that we do not consider, or due to liquidity constraints, this result might not hold. Overall, the proportion of losers decreases as m increases, though the amount lost per loser rises.

Figure 6 displays the consumer welfare results, netting out discounts (CW^{ND}), from Table 6. A key insight is that welfare is not monotonic in the commitment level m, given a discount level d. For example, when $d = 0$, availability of a commitment contract where $m = 15$ decreases welfare by an average of 5.08 rupees per person, but when m is raised to 45 the availability of this contract now increases welfare by an average of 8.38 rupees per person. The top panel of the figure reveals that this is a consistent pattern across the different d levels we consider: relative to the corresponding discount only treatments, commitment contracts with level $m = 0$ and $m = 15$ are welfare decreasing, but those with $m = 30$ and $m = 45$ are welfare increasing. This reveals that if partial naivete about present-bias is the main driver for lack of follow-through on a commitment, either (i) removing the option to commit or (ii) making the commitment stronger would be welfare improving in our setting, where $m = 15$ is the fixed commitment contract commitment level. When the ability to commit is removed, consumers cannot lose money through commitments they do not follow through on. Conversely, when the commitment amount is sufficiently strong, even most partially naive consumers will follow through.

The results also have interesting implications for discounts, and how they are bundled with different commitment levels. The bottom panel of Figure 6 plots the welfare impacts of different discount amounts as a function of the possible commitment amounts m. Whether discounts are complementary to commitment amounts m depends on the level of m. For high levels of m, discounts are welfare decreasing. Given that almost all consumers signing up for contracts follow through at high m, welfare is decreasing in d because such discounts induce consumers with benefits lower than the social costs to utilize preventive care. This can also be seen in the fact that, for the discount-only treatments, welfare is increasing in d for $d \leq 15$ but decreasing for $d > 15$. For low commitment levels m, the impacts of discounts are non-monotonic: at low d, welfare is increasing in d, but at higher levels of d welfare is decreasing in d.

Taken together, our results suggest that commitment contracts and bundled discounts induce subtle trade-offs whose welfare implications depend on the specifics of the empirical context. Changes to up-front fixed commitment amounts induce a trade-off between the amount that losers lose, the number of losers, and the number of consumers going to the doctor. In our setting, the amount lost by losers generally outweighs the gains from increased attendence by winners at low m, but the reverse is true at high m. Introducing bundled discounts changes the nature of this trade-off, with higher discounts being better at low m, but not so at higher m. Importantly, bundled discounts may also lead to a traditional source of inefficiency whereby consumers who value a product at less than its social cost are induced to buy it because of the discount.

6 Conclusion

We conducted a field experiment in rural India to evaluate whether commitment contracts and price discounts increase preventive health visits by hypertensive patients. The results are – at best – mixed from the point of view of harnessing commitment contracts in the health sector: while we succeeded at designing contracts with high take-up in some arms, few of those who purchased the contracts ended up utilizing health services, and objective health outcomes (blood pressure, weight) do not change in the treatment groups. Under plausible model assumptions, offering individuals commitment contracts reduces social welfare in the context we study.

A methodological contribution of the project is to design a natural field experiment with a tight link to a theoretical model, generating both robust reduced-form facts, and structural estimates of the key utility and belief parameters. This approach remains relatively rare in the development economics literature, although it builds on a recent stream of work in structural behavioral economics (e.g., DellaVigna et al. 2012; Augenblick et al. 2015). An advantage of this approach is that the identification of structural parameters here relies on exogenous variation due to randomization. Recovering the structural parameters allows us to conduct welfare and counterfactual analysis, which ultimately may be tested through additional experimentation.

A substantive contribution of this article is to provide one of the first estimates of the distributions of sophistication about present bias. The theoretical literature has highlighted the importance of this parameter (Laibson 1997; O'Donoghue and Rabin 1999). Only sufficiently-naive agents are predicted to engage in procrastination; conversely, only sufficiently-sophisticated agents will demand commitment and use it to achieve the first-best. Understanding the distribution of sophistication is thus crucial to understanding the nature of self-control challenges, and to identify the appropriate policy responses. Yet, to date, there are few empirical estimates, with Augenblick and Rabin (2018) being a notable exception. We provide evidence of partial naivete on average, with substantial variation. We complement the existing literature by providing evidence from a field setting, and by relying not on incentivized predictions of future behavior, but instead by examining the amount of costly commitment that agents choose when designing their own commitment contracts.

Our findings and the underlying theory both suggest the need for caution in the design of commitment contracts. If partial naivete is common, as our results indicate, then agents are likely to demand costly commitment, but then systematically accept commitments which are not strong enough to succeed. One policy response would be to restrict the set of available commitments to *strong* commitments. But this poses a tradeoff with consumers' demand for flexibility in an uncertain world (Laibson 2015; Amador et al. 2006). Another implication of widespread partial naivete is that the many agents who do *not* demand a particular commitment device might simply be over-optimistic about their self-control problems, as opposed to not having a self-control problem to begin with. That is, even offering strong commitments may not end up helping many consumers with self-control problems.

A potential solution to these problems that emerges from our approach would involve the design of optimal contracts, utilizing the estimated distributions of β and $\hat{\beta}$ to design a menu of commitment options. Indeed, with enough individual choice data, one could in principle design optimal contracts for each individual, as in Andreoni et al. (2016). While this remains an intellectually valuable exercise, it faces obvious shortcomings as a scalable public health tool to target consumers. A practical solution could instead be to allow consumers to experience different levels of incentives before they are offered commitment, providing them an opportunity to learn how strong an incentive they require to overcome their self-control problem in a given context.

Given these open questions, it seems clear that commitment contracts are not a panacea for improving preventive health behaviors. What, then, is the best approach for boosting preventive care in low and middle income countries? This is a critical public policy issue in many countries in Asia, Africa and Latin America, as lifestyle diseases, metabolic disorders and other chronic conditions related to diet and obesity plague increasingly prosperous — and aging — populations.

One, perhaps unsurprising, lesson that does emerge from our data is that simple subsidies are effective tools for driving higher demand for these services. Yet, even with a forceful intervention targeting an at-risk population, deploying subsidies, commitment, information provision, a high-quality provider and home-visit reminders, overall utilization remains very low in our setting. Increasing take-up of commitment contracts by bundling them with attractive subsidies similarly did not increase utilization. Reducing hassle costs and achieving better understanding of the dimensions of provider quality consumers care about is likely to be a more promising approach, as is providing direct incentives to utilize preventive care.

References

Acland, Dan and Matthew R. Levy, "Naivete, Projection Bias, and Habit Formation in Gym Attendance," *Management Science*, 2015, *61* (1), 146–160.

Amador, Manuel, Iván Werning, and George-Marios Angeletos, "Commitment vs. flexibility," *Econometrica*, 2006, *74* (2), 365–396.

Andreoni, James, Karrar Callen, Michael, Jaffar, Muhammad Yasir Khan, and Charles Sprenger, "Using Preference Estimates to Customize Incentives: An Application to Polio Vaccination Drives in Pakistan," 2016.

Ashraf, Nava, Dean Karlan, and Wesley Yin, "Tying Odysseus to the Mast: evidence from a Commitment Savings Product in the Philippines," *Quarterly Journal of Economics*, 2006, *121* (2), 635–672.

Association of Physicians of India, "Epidemiology of Hypertension," *Journal of the Association of Physicians of India*, 2013, *61* (6).

_ , "Management of Hypertension," *Journal of the Association of Physicians of India*, 2013, *61* (6).

Augenblick, Ned and Matthew Rabin, "An experiment on time preference and misprediction in unpleasant tasks," *Review of Economic Studies*, 2018, *86* (3), 941–975.

_ , Muriel Niederle, and Charles Sprenger, "Working Over Time: Inconsistency in Real Effort Tasks," *Quarterly Journal of Economics*, 2015, *130* (3), 1067–1115.

Banerjee, Abhijit, Angus Deaton, and Esther Duflo, "Health, health care, and economic development: Wealth, health, and health services in rural Rajasthan," *The American economic review*, 2004, *94* (2), 326.

Bryan, Gharad, Dean Karlan, and Scott Nelson, "Commitment Devices," *Annual Review of Economics*, 2010, *2*, 671–698.

Carrera, Mariana, Heather Royer, Mark Stehr, Justin Sydnor, and Dmitry Taubinsky, "How are preferences for commitment revealed?," Technical Report, National Bureau of Economic Research 2019.

DellaVigna, Stefano and Ulrike Malmendier, "Contract Design and Self-Control: Theory and Evidence," *Quarterly Journal of Economics*, 2004, *119* (2), 353–402.

— **and —**, "Paying not to go to the gym," *The American Economic Review*, 2006, *96* (3), 694–719.

— **, John A List, and Ulrike Malmendier**, "Testing for altruism and social pressure in charitable giving," *The Quarterly Journal of Economics*, 2012, p. qjr050.

Duflo, Esther, Michael Kremer, and Jonathan Robinson, "Nudging Farmers to Use Fertilizer: Theory and Experimental Evidence from Kenya," *American Economic Review*, 2011, *101* (6).

Dupas, Pascaline and Edward Miguel, "Impacts and determinants of health levels in low-income countries," *Handbook of Economic Field Experiments*, 2017, *2*, 3–93.

Gine, Xavier, Dean Karlan, and Jonathan Zinman, "Put Your Money Where Your Butt Is: A Commitment Contract for Smoking Cessation," *American Economic Journal: Applied Economics*, 2010, *2*, 213–235.

Halpern, Scott D, Benjamin French, Dylan S Small, Kathryn Saulsgiver, Michael O Harhay, Janet Audrain-McGovern, George Loewenstein, Troyen A Brennan, David A Asch, and Kevin G Volpp, "Randomized trial of four financial-incentive programs for smoking cessation," *New England Journal of Medicine*, 2015, *372* (22), 2108–2117.

Handel, Benjamin R, "Adverse selection and inertia in health insurance markets: When nudging hurts," *The American Economic Review*, 2013, *103* (7), 2643–2682.

Heidhues, Paul and Botond Kőszegi, "Futile attempts at self-control," *Journal of the European Economic Association*, 2009, *7* (2-3), 423–434.

— **and —**, "Exploiting naivete about self-control in the credit market," *The American Economic Review*, 2010, *100* (5), 2279–2303.

John, Anett, "When Commitment Fails – Evidence from a Regular Saver Product in the Philippines," *mimeo*, 2015.

Karlan, Dean and Leigh L Linden, "Loose knots: strong versus weak commitments to save for education in Uganda," 2017.

Kaur, Supreet, Michael Kremer, and Sendhil Mullainathan, "Self-Control at Work," *Journal of Political Economy*, 2015, *Forthcomin.*

Kremer, Michael, Gautam Rao, and Frank Schilbach, "Behavioral development economics," in B. Douglas Bernheim, Stefano DellaVigna, and David Laibson, eds., *Handbook of Behavioral Economics - Foundations and Applications 2*, Vol. 2 of *Handbook of Behavioral Economics: Applications and Foundations 1*, North-Holland, 2019, pp. 345 – 458.

Laibson, David, "Golden eggs and hyperbolic discounting," *The Quarterly Journal of Economics*, 1997, *112* (2), 443–478.

—, "Why don't present-biased agents make commitments?," *The American Economic Review*, 2015, *105* (5), 267–272.

Mahajan, Aprajit, Christian Michel, and Alessandro Tarozzi, "Identification of Time-Inconsistent Models: The Case of Insecticide Treated Nets," 2019.

Mohan, Viswanathan, Yackoob Seedat, and Rajendra Pradeepa, "The rising burden of diabetes and hypertension in southeast asian and african regions: need for effective strategies for prevention and control in primary health care settings," *International journal of hypertension*, 2013, *2013.*

O'Donoghue, Ted and Matthew Rabin, "Doing It Now or Later," *American Economic Review*, 1999, *89* (1), 103–124.

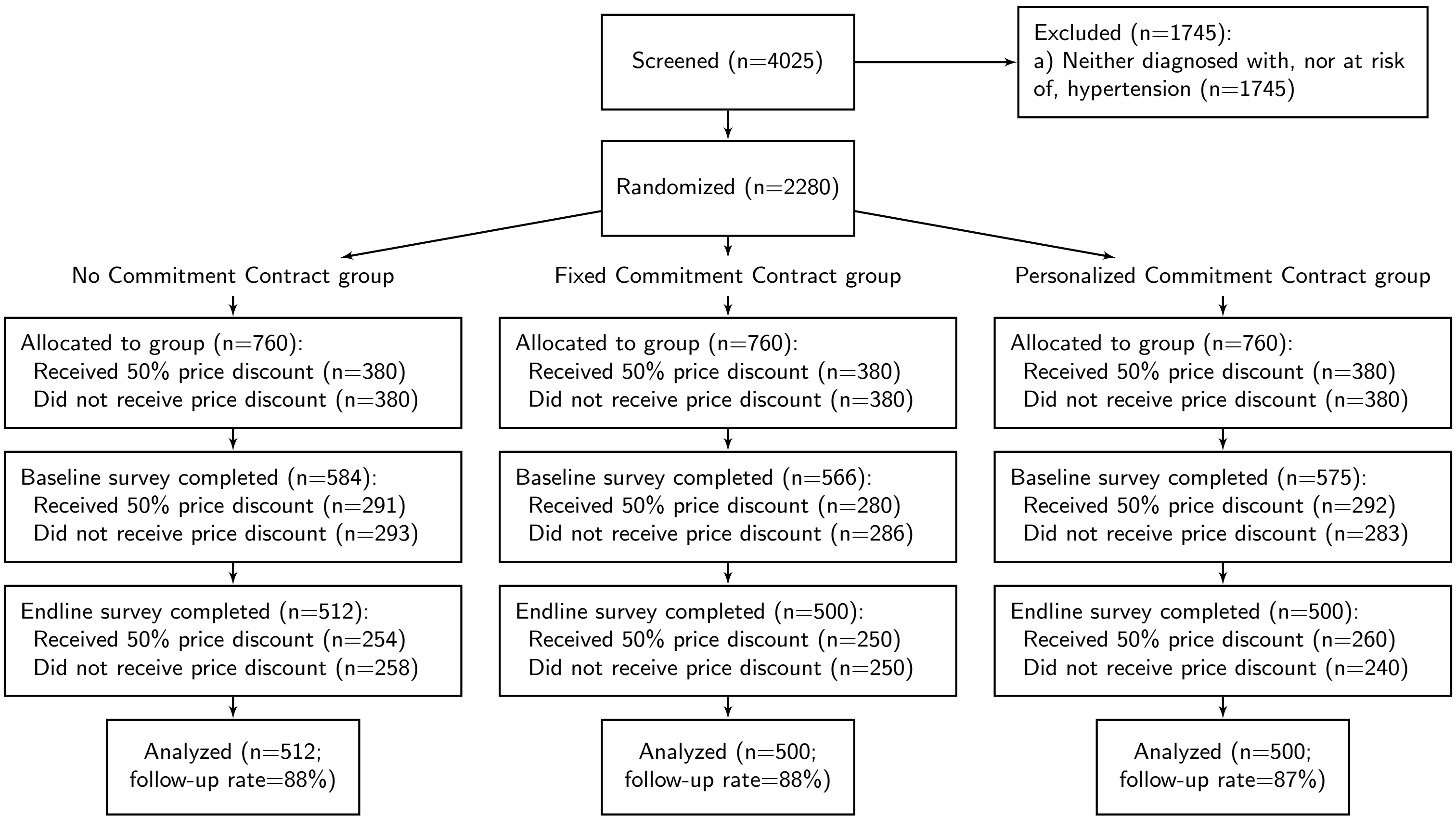
Royer, Heather, Mark Stehr, and Justin Sydnor, "Incentives, commitments, and habit formation in exercise: evidence from a field experiment with workers at a fortune-500 company," *American Economic Journal: Applied Economics*, 2015, *7* (3), 51–84.

Schilbach, Frank, "Alcohol and self-control: A field experiment in India," *American economic review*, 2019, *109* (4), 1290–1322.

World Bank, *World Development Report 2015: Mind, Society, and Behavior*, World Bank Group, 2015.

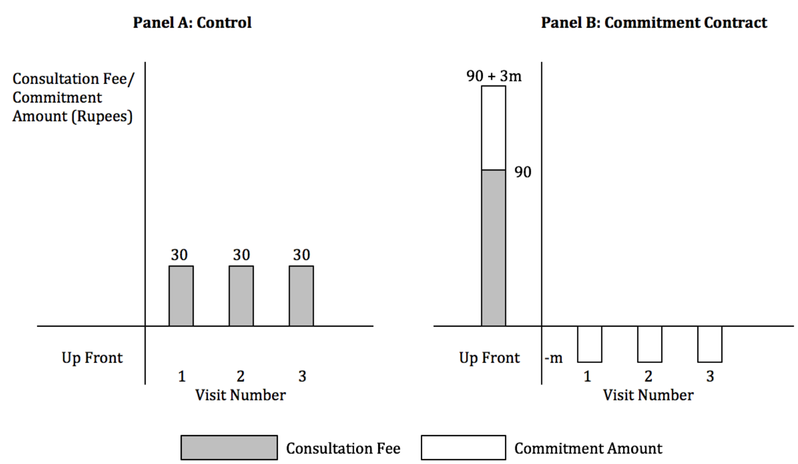
World Health Organization, "A global brief on hypertension: silent killer, global public health crisis: World Health Day 2013," 2013.

Figure 1: Flowchart of participants' progress through phases of the trial



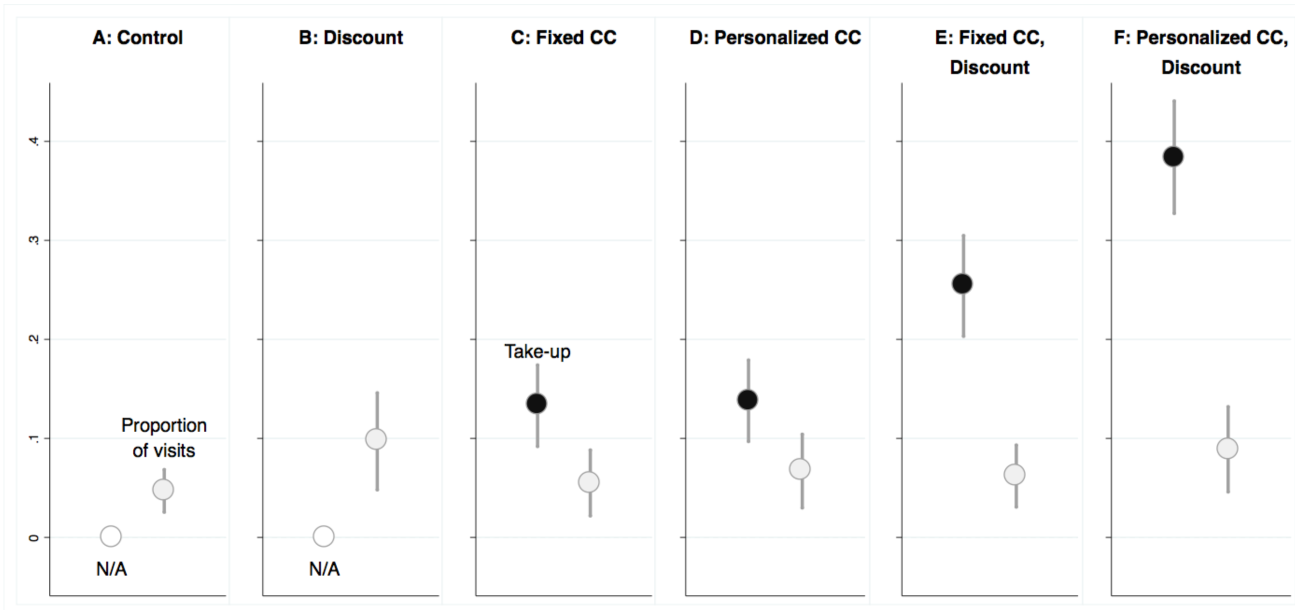
Notes: The initial round of screening was carried out during May-June 2012. The baseline survey and commitment contract offers were done during October 2012 - January 2013. The weekly hypertension health camps were held during August 2012 - June 2013. The endline surveys were completed during May-July 2013.

Figure 2: Experimental Interventions



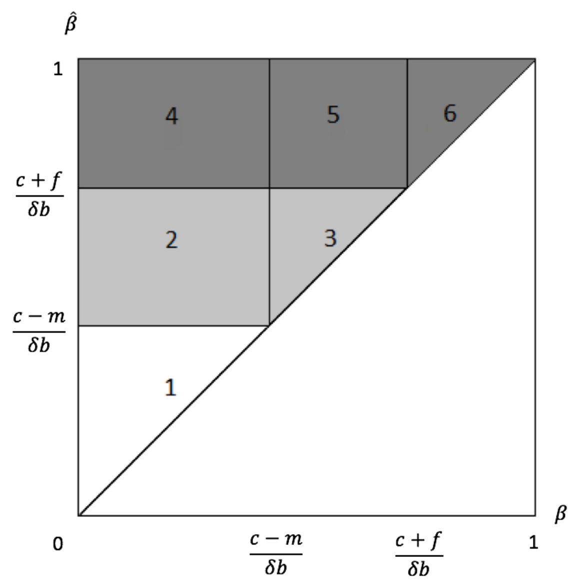
Notes: Control group participants can visit the health camps on a pay-as-you-go basis (30 rupees per consultation), while commitment contract group participants were offered the opportunity to pay for multiple visits upfront. This is combined with either a fixed (15 rupees) or personalized (self-chosen) commitment amount, which they then receive back during future visits. Finally, the above three treatments are cross-cut with a simple discount treatment, where participants were charged 15 rupees per consultation.

Figure 3: Commitment Contract Take-up and Fraction of Clinic Visits by Treatment Group



Notes: Take-up refers to the fraction of study participants in each treatment group that signed up for the commitment contract on offer. It is therefore not applicable to the control and discount groups (panels A and B respectively), as participants in these groups were not offered commitment contracts. Proportion of visits is the average for participants in each treatment group during the six-months intervention period, out of the recommended three visits. Black circles denote average take-up rates of commitment contracts in each treatment group, while gray circles denote average proportions of health camp visits in each treatment group. The vertical gray lines correspond to 95% confidence intervals around coefficient estimates of treatment group indicators in a regression with village fixed effects.

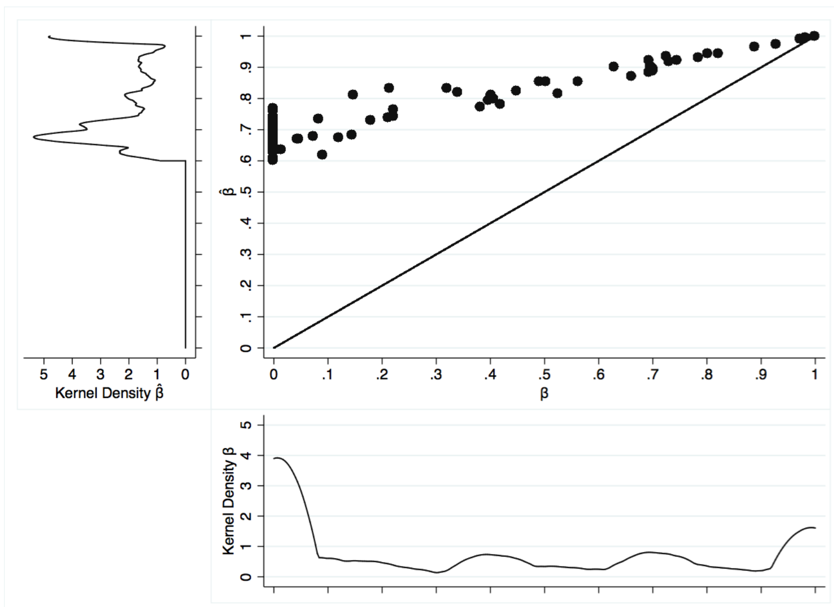
Figure 4: Identification of $\hat{\beta}$ and β in Theoretical Model



1. Predicts will not visit doctor even with CC, and does not visit even with CC.
2. Predicts will visit doctor only with CC, but does not visit even with CC.
3. Predicts will visit doctor only with CC, and visits only with CC.
4. Predicts will visit with and without CC, but does not visit even with CC.
5. Predicts will visit with and without CC, but visits only with CC.
6. Predicts will visit with and without CC, and visits with and without CC.

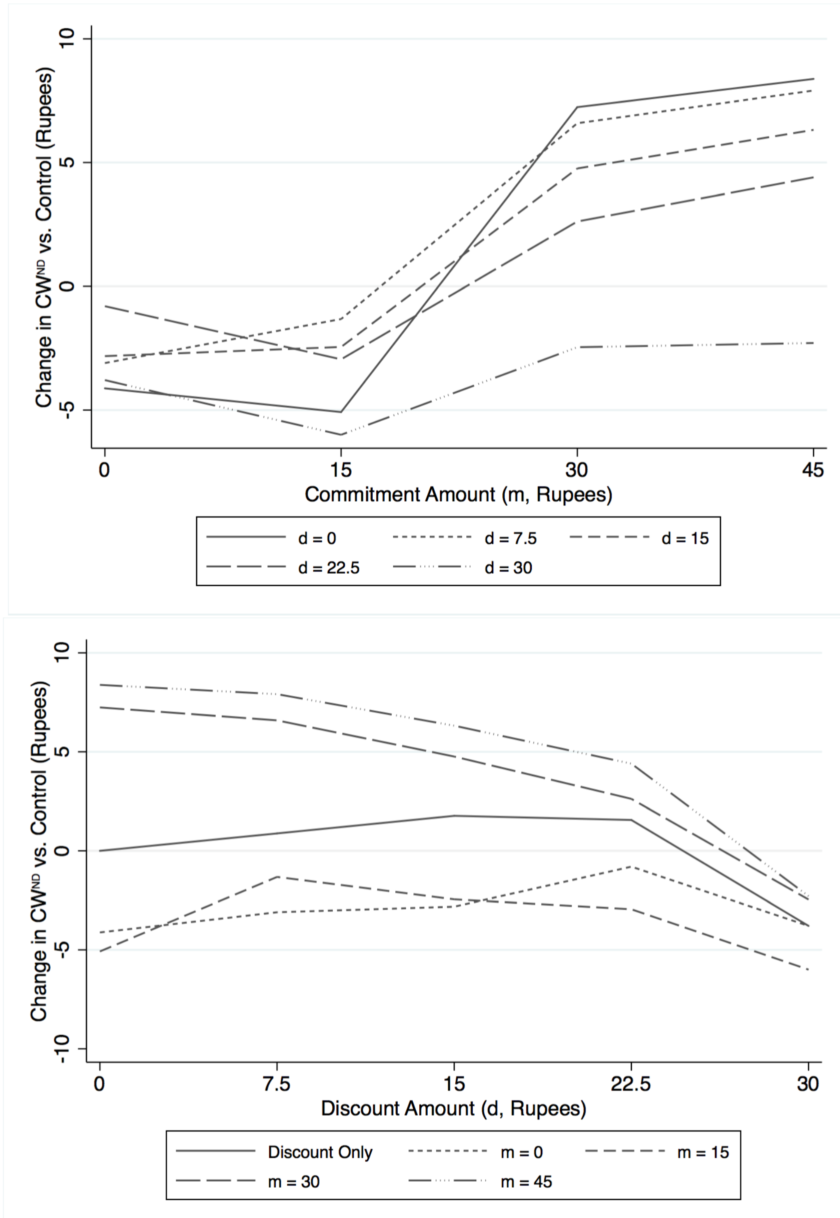
Notes: This figure illustrates how we can identify the parameters $\hat{\beta}$ and β from commitment contract choices and subsequent doctor visits. The white region correspond to individuals who predict they will not visit the doctor even with a commitment contract. The light gray region correspond to individuals who predict they will visit the doctor only with a commitment contract. The dark gray region correspond to individuals who predict they will visit the doctor both with and without a commitment contract. In the theoretical model of Section 4, b is the benefit from visiting the doctor, f is the standard per visit fee, c is the non-financial cost of going to the doctor, m is the commitment amount and δ is the exponential discount factor.

Figure 5: Joint distribution of β and $\hat{\beta}$ estimates from primary specification



Notes: This figure plots the joint distribution of β and $\hat{\beta}$ estimates from our primary specification. Such a specification incorporates observable heterogeneity across a number of important dimensions. These include (i) our hypertension severity index (ii) our general health index (iii) gender (iv) whether the consumer was already taking hypertension medication prior to the study and (v) whether they are literate. The estimated mean of β is 0.365, with a population standard deviation of 0.395. The estimated mean of $\hat{\beta}$ is 0.795 with a population standard deviation of 0.13.

Figure 6: Consumer welfare (CW^{ND}) impacts of counterfactual commitment contracts as a function of the commitment level m and discount level d



Notes: This figure illustrates results from our counterfactual simulations that study alternative bundled discount-fixed commitment contract designs. Specifically, we study five different values of d (0, 7.5, 15, 22.5 and 30) and four values of m (0, 15, 30 and 45). The top panel plots the consumer welfare impacts, netting out discounts as transfers, of different commitment amounts m for each possible discount d, while the bottom panel plots these welfare impacts for different discount amounts corresponding to each possible commitment amount m.

Table 1: Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Panel A: Demographic Characteristics					
Household Size	1728	5.51	2.26	1	21
Gender (Female = 0, Male = 1)	1728	0.41	0.49	0	1
Age	1728	53.7	14.3	30	105
HH Head Literate (Yes = 1, No = 0)	933	0.45	0.50	0	1
Household Income (1000 Rs)	1539	101.9	161.5	0	2000
HH Head Self-Employed Agriculture (Yes = 1, No = 0)	908	0.37	0.48	0	1
Panel B: Baseline Health Indicators					
Blood Pressure (Systolic)	1718	142.4	24.5	87	264
Blood Pressure (Diastolic)	1718	85.8	14.1	37	182
Weight (kg)	1677	64.9	14.7	28.3	164.9
Pre-hypertension (Yes = 1, No = 0)	1718	0.86	0.34	0	1
Hypertension (Yes = 1, No = 0)	1718	0.58	0.49	0	1
Overweight (Yes = 1, No = 0)	1677	0.51	0.50	0	1
Obesity (Yes = 1, No = 0)	1677	0.20	0.40	0	1
Panel C: Take-up and Service Utilization					
Contract Take-up (Yes = 1, No = 0)	1725	0.15	0.36	0	1
Proportion of Doctor Visits	1725	0.07	0.27	0	4
Any Doctor Visit (Yes = 1, No = 0)	1725	0.11	0.32	0	1
Panel D: Endline Health Indicators					
Blood Pressure (Systolic)	1512	138.8	24.2	64	245
Blood Pressure (Diastolic)	1512	85.0	15.4	12	161
Weight (kg)	1512	65.9	14.6	29.7	119.4
Pre-hypertension (Yes = 1, No = 0)	1512	0.82	0.38	0	1
Hypertension (Yes = 1, No = 0)	1512	0.50	0.50	0	1
Overweight (Yes = 1, No = 0)	1512	0.53	0.50	0	1
Obesity (Yes = 1, No = 0)	1512	0.20	0.40	0	1

Table 2: Main Study Outcomes by Treatment Group: Full Sample

Panel A: Take-up and Service Utilization	(1) Take-up	(2) Any visit	(3) Proportion of visits
Discount	-0.000	0.056	0.050
	(0.005)	(0.027)**	(0.025)**
Fixed CC	0.133	0.006	0.008
	(0.021)***	(0.024)	(0.017)
Personalized CC	0.138	0.010	0.020
	(0.021)***	(0.024)	(0.019)
Fixed CC + Discount	0.254	0.036	0.015
	(0.026)***	(0.026)	(0.016)
Personalized CC + Discount	0.384	0.048	0.042
	(0.029)***	(0.026)*	(0.022)*
Observations	1,725	1,725	1,725
R-squared	0.14	0.00	0.00
Control Group Mean (SD)	0.003 (0.058)	0.089 (0.285)	0.046 (0.192)
Panel B: Endline Health Outcomes	**BP (systolic)**	**BP (diastolic)**	**Weight (kg)**
Discount	0.614	0.081	-0.162
	(1.591)	(1.154)	(0.487)
Fixed CC	-0.286	0.031	-0.442
	(1.558)	(1.153)	(0.312)
Personalized CC	0.600	1.933	-0.128
	(1.506)	(1.180)	(0.322)
Fixed CC + Discount	-0.461	-0.082	0.173
	(1.556)	(1.076)	(0.324)
Personalized CC + Discount	1.105	1.747	-0.182
	(1.610)	(1.246)	(0.292)
Observations	1,481	1,481	1,481
R-squared	0.48	0.29	0.91
Control Group Mean (SD)	139.7 (25.0)	85.0 (15.6)	67.4 (14.1)

Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05,* p<0.10. Village indicators are included in all specifications. Baseline blood pressure and weight measures are included as controls in Panel B regressions. This is done to improve the precision of our estimates. Regressions without baseline controls, as specified in our pre-analysis plan, are reported in Table A2.

Table 3: Main Study Outcomes by Treatment Group: "Ideal" Sample

Panel A: Take-up and Service Utilization	(1) Take-up	(2) Any visit	(3) Proportion of visits
Discount	0.000	0.106	0.119
	(0.000)	(0.052)**	(0.076)
Fixed CC	0.172	0.107	0.077
	(0.047)***	(0.055)*	(0.052)
Personalized CC	0.192	0.031	0.025
	(0.046)***	(0.045)	(0.045)
Fixed CC + Discount	0.319	0.074	0.040
	(0.055)***	(0.050)	(0.041)
Personalized CC + Discount	0.494	0.130	0.121
	(0.057)***	(0.054)**	(0.067)*
Observations	439	439	439
R-squared	0.20	0.02	0.01
Control Group Mean (SD)	0.000 (0.000)	0.065 (0.248)	0.047 (0.246)

Panel B: Endline Health Outcomes	BP (systolic)	BP (diastolic)	Weight (kg)
Discount	0.276	-0.988	-0.318
	(3.207)	(1.905)	(0.443)
Fixed CC	-0.802	-0.638	-0.910
	(2.906)	(2.008)	(0.521)*
Personalized CC	3.557	3.526	-0.228
	(2.955)	(1.996)*	(0.468)
Fixed CC + Discount	0.978	1.380	-0.972
	(2.979)	(1.830)	(0.624)
Personalized CC + Discount	3.967	4.724	-0.553
	(3.034)	(2.128)**	(0.646)
Observations	395	395	395
R-squared	0.49	0.36	0.95
Control Group Mean (SD)	143.4 (28.7)	85.6 (15.2)	69.4 (16.0)

Notes: Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$,* $p<0.10$. Village indicators are included in all specifications. Baseline blood pressure and weight measures are included as controls in Panel B regressions. The "ideal" sample is defined as respondents who both believe "it is possible to be healthy with hypertension if blood pressure is frequently monitored" *and* who trust the service provider.

Table 4: Structural Estimates of Theoretical Model

	(1) Baseline	(2) Baseline w/ Correlations	(3) Primary
β, mean	0.190	0.103	0.365
β, std. dev.	0.268	0.132	0.395
β, % = 1	0%	0%	15.1%
β, % = 0	47.8%	34.0%	39.8%
$\hat{\beta}$, mean	0.804	0.543	0.795
$\hat{\beta}$, std. dev.	0.04	0.12	0.13
mean $\frac{\hat{\beta}-\beta}{1-\beta}$, $\beta < 0.5$	0.04	0.47	0.66
mean $\frac{\hat{\beta}-\beta}{1-\beta}$, $\beta \geq 0.5$	0.04	0.55	0.66
κ_b, HBP Index	–	–	40.43
κ_b, Sickness Index	–	–	-13.70
κ_b, Male	–	–	45.37
κ_b, Prior Meds	–	–	-13.40
κ_b, Literate	–	–	36.56
κ_c, Employment	–	–	4.00
κ_c, Distance	1.36	0.31	–
κ_β, Male	–	–	-0.48
$\kappa_{\hat{\beta}}$, Male	–	–	0.047
ϵ, std. dev. (Rs)	69.2	190.0	197.7
b, mean (Rs)	30	30	61.3
c, mean (Rs)	32.1	30.5	26.5
δ, mean	0.780	0.687	0.234
$\delta * b - c$, 25th percentile	-49.96	-110.76	-55.50
$\delta * b - c$, mean	-3.6	-7.2	-19.3
$\delta * b - c$, 75th percentile	42.76	73.43	17.93
Log-Likelihood	-1399	-1416	-1327
N	1496	1496	1729

Notes: This table presents our structural estimates. Column 1 presents the estimates form a baseline model with limited observable heterogeneity. Column 2 is the same baseline model with additional correlations between unobserved heterogeneity in β and $\hat{\beta}$. Column 3 is our primary model, which incorporates observable heterogeneity on a range of potentially important dimensions (e.g. health and demographics). b and c are perceived benefits and costs of visiting the clinic. ϵ is the unobserved component of perceived benefits. δ is the exponential discount factor. $\delta * b - c$ is the perceived net benefit of visiting the clinic.

Table 5: Consumer Welfare and Social Welfare Impacts

	(1) Control	(2) Discount	(3) Fixed CC	(4) Personalized CC	(5) Fixed CC, Discount	(6) Personalized CC, Discount
Panel A: Model Fit						
% Purchasing CC	n/a	n/a	7.2	7.2	26.7	26.7
% Commitment Amount > 0	n/a	n/a	100	1	100	1
% Visiting Doctor	4.37	8.37	7.36	7.63	18.00	14.25
Panel B: Baseline, Control						
Mean ΔCW^{ND}	0	1.51	-5.08	-4.12	-2.42	-2.89
Mean ΔCW	0	5.44	-5.08	-4.12	5.68	3.53
Mean ΔSW	0	1.51	8.27	9.53	27.13	39.31
Panel C: Baseline, No Doctor						
% Zero Impact	95.63	91.60	88.42	88.43	73.21	73.29
% Losers	0	0	4.24	4.95	8.71	12.51
Mean ΔCW	6.38	11.82	1.30	2.26	12.06	9.91
Mean if $\neq 0$	145.85	141.08	11.20	19.51	45.06	37.11
Median	0	0	0	0	0	0
Max	421.12	466.50	421.32	421.32	466.5	466.5
Min	0	0	-135	-90	-90	-51.2
10th Percentile	0	0	0	0	0	-45
90th Percentile	0	0	0	0	77.38	53.21
Panel D: Mean ΔCW^{ND}						
b = 500	8.73	19.09	8.73	8.79	33.16	26.69
b = 5000	146.03	285.64	241.09	218.66	602.49	476.67
b = 50000	1534.5	2936.60	2564.02	2317.56	6295.42	4966.20

Notes: This table reports the welfare impacts of each treatment under study. These are computed assuming that our entire population is enrolled in that treatment, and applying our parameter estimates (see Table 5) to study decision-making and subsequent outcomes. We compute welfare from a long run, or $t = 0$ perspective. This means that baseline consumer welfare, CW, in each treatment equals their discounted benefit of visiting the doctor, minus the costs (both pecuniary and non-pecuniary), and minus any pledged commitment amounts lost due to not following through. CW^{ND} nets out discounts given to consumers, such that discount d_i is a transfer and not a consumer benefit. Social welfare SW gives medical benefits net of consumer attendence costs and assumed firm costs.

Table 6: Counterfactual Simulations (Fixed Contracts)

	(1) d = 0	(2) d=7.5	(3) d=15	(4) d=22.5	(5) d=30
Panel A: % Visiting the Clinic					
Discount Only	4.38	5.96	8.37	10.4	18.7
m = 0	7.10	12.4	14.2	15.0	18.7
m = 15	7.36	15.21	18.00	20.24	21.61
m = 30	17.46	21.29	25.66	29.31	33.01
m = 45	17.91	22.01	26.70	30.17	34.89
Panel B: Mean ΔCW^{ND}, Baseline Control					
Discount Only	0	0.88	1.77	1.56	-3.79
m = 0	-4.12	-3.10	-2.82	-0.80	-3.79
m = 15	-5.08	-1.32	-2.45	-2.95	-6.00
m = 30	7.24	6.59	4.76	2.62	-2.46
m = 45	8.38	7.91	6.32	4.40	-2.29
Panel C: Mean ΔCW, Baseline Control					
Discount Only	0	2.22	5.54	8.57	12.92
m = 0	-4.12	-0.31	3.57	9.32	12.92
m = 15	-5.08	2.11	5.65	10.71	13.44
m = 30	7.24	11.38	16.30	22.39	26.24
m = 45	8.38	12.86	18.33	24.76	29.11
Panel D: % Losers, ΔCW, Baseline Control					
Discount Only	0	0	0	0	0
m = 0	4.95	9.6	12.4	13.8	0
m = 15	4.24	6.8	8.72	10.47	15.82
m = 30	0.51	0.73	1.12	1.40	4.42
m = 45	0	0	0	0	0

Notes: This table describes the results from our counterfactual simulations that study alternative bundled discount-fixed commitment contract designs. Specifically, we study five different values of d (0, 7.5, 15, 22.5 and 30) and four values of m (0, 15, 30 and 45). For each potential contract (i.e. d and m combination), we present four statistics where the entire population is offered the contract. These are: (i) the % of consumers visiting the clinic, (ii) the mean consumer welfare impact, netting out discounts, relative to our control treatment, (iii) the mean baseline consumer welfare impact relative to our control treatment, and (iv) the % of losers relative to the control treatment. The simulations assume that only one contract is offered in each case, and thus does not consider menu design with multiple commitment contract options.

A Additional Tables and Figures

Table A.1: Balance Check Across Treatment Groups

Variable	Control	Discount	Fixed CC	Personalized CC	Fixed CC, Discount	Personalized CC, Discount	F-test p-value
Panel A: Demographic Characteristics							
Household Size	5.50	5.29	5.59	5.77	5.33	5.59	0.098
Gender (Female = 0; Male = 1)	0.43	0.37	0.46	0.41	0.38	0.40	0.286
Age	53.4	53.6	54.0	54.0	53.8	53.2	0.978
Literate (Yes = 1, No = 0)	0.35	0.37	0.33	0.37	0.36	0.33	0.811
Household Income (1000 Rs)	120	94.2	92.3	106	99.4	99.2	0.582
Self-Employed Agriculture (Yes = 1, No = 0)	0.18	0.11	0.13	0.15	0.11	0.16	0.137
Panel B: Baseline Health Indicators							
Blood Pressure (Systolic)	143	141	145	143	142	141	0.401
Blood Pressure (Diastolic)	86	86	87	86	85	85	0.311
Weight (kg)	65.6	65.2	64.0	64.9	65.4	64.3	0.749
Panel C: Target Sample Characteristics							
Values Service (Yes = 1, No = 0)	0.66	0.64	0.62	0.71	0.67	0.66	0.317
Trusts Provider (Completely Trust =1, Completely Distrust = 5)	3.12	2.95	3.30	3.16	3.07	3.12	0.418

Notes: This table reports mean values of each variable for every treatment group. The final column reports the joint significance level of treatment indicators in a regression with village indicators. The respondent is classified as valuing the service if he/she believes "it is possible to be healthy with hypertension if blood pressure is frequently monitored."

Table A.2: Endline Health Outcomes: Incidence of Hypertension and Obesity

Panel A: Full Sample	(1) Pre-hypertension	(2) Hypertension	(3) Overweight	(4) Obesity
Discount	-0.027	-0.001	0.020	0.032
	(0.031)	(0.036)	(0.034)	(0.030)
Fixed CC	0.022	0.018	-0.012	0.010
	(0.030)	(0.038)	(0.035)	(0.030)
Personalized CC	0.008	0.051	0.030	0.055
	(0.030)	(0.038)	(0.035)	(0.031)*
Fixed CC + Discount	0.012	0.006	0.004	0.041
	(0.030)	(0.037)	(0.034)	(0.030)
Personalized CC + Discount	-0.008	0.045	0.026	0.038
	(0.030)	(0.037)	(0.033)	(0.029)
Observations	1,481	1,481	1,481	1,481
R-squared	0.18	0.30	0.41	0.34
Control Group Mean (SD)	0.83(0.37)	0.49(0.50)	0.56(0.50)	0.20(0.40)

Panel B: "Ideal" Sample	Pre-hypertension	Hypertension	Overweight	Obesity
Discount	0.027	-0.046	-0.012	0.032
	(0.055)	(0.068)	(0.062)	(0.061)
Fixed CC	0.042	0.047	-0.023	-0.039
	(0.061)	(0.073)	(0.067)	(0.061)
Personalized CC	0.092	0.182	0.015	0.156
	(0.055)*	(0.075)**	(0.069)	(0.064)**
Fixed CC + Discount	0.056	0.068	0.013	-0.073
	(0.060)	(0.071)	(0.072)	(0.055)
Personalized CC + Discount	0.067	0.158	0.059	0.004
	(0.059)	(0.070)**	(0.065)	(0.056)
Observations	395	395	395	395
R-squared	0.19	0.33	0.41	0.33
Control Group Mean (SD)	0.84(0.37)	0.51(0.50)	0.59(0.49)	0.20(0.41)

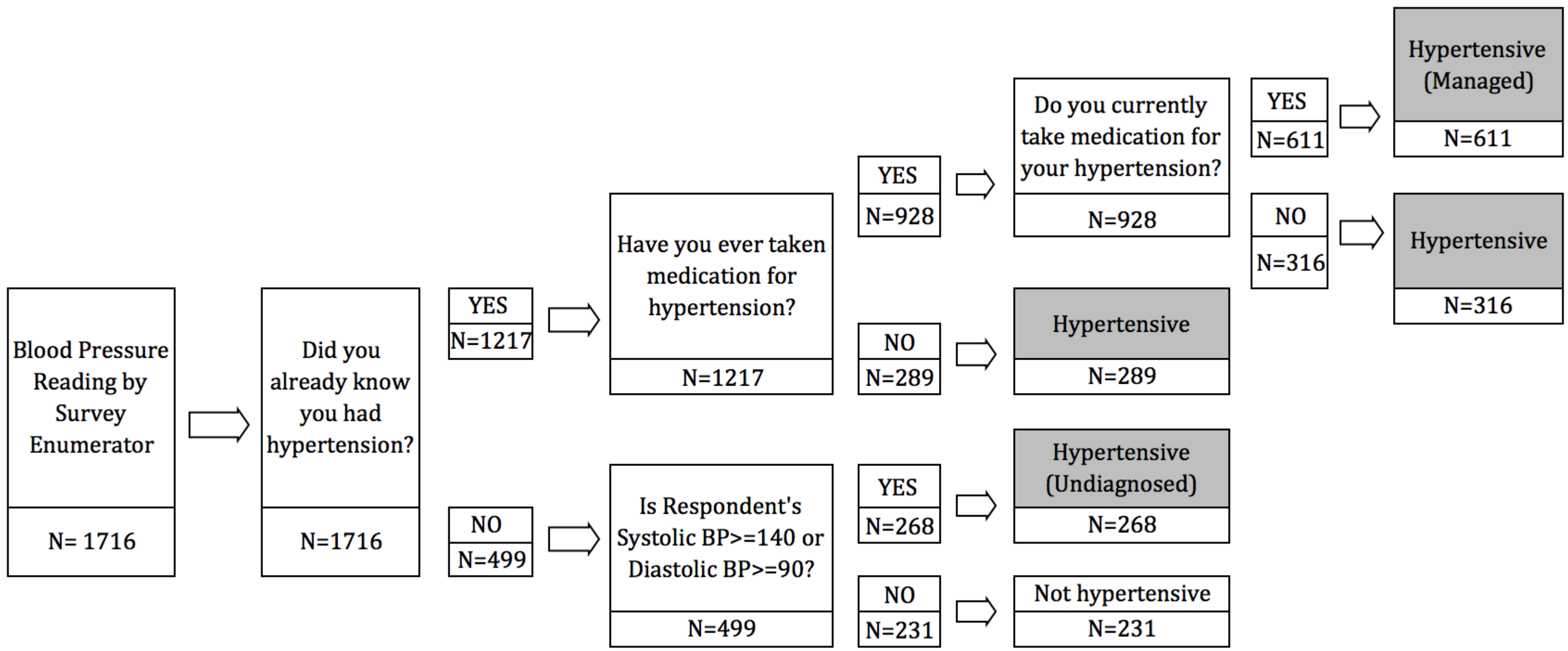
Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05,* p<0.10. Pre-hypertension is equal to one if BP(systolic)>120 or BP(diastolic)>80, and to zero otherwise. Hypertension is equal to one if BP(systolic)>140 or BP(diastolic)>90, and to zero otherwise. Overweight is equal to one if BMI>25, and to zero otherwise. Obesity is equal to one if BMI>30, and to zero otherwise. Village indicators are included in all specifications. Baseline blood pressure and weight measures are included as controls in all regressions. The "ideal" sample is defined as respondents who both believe "it is possible to be healthy with hypertension if blood pressure is frequently monitored" *and* who trust the service provider.

Table A.3: Endline Health Outcomes by Treatment Group: Without Baseline Controls

	(1)	(2)	(3)
Panel A: Full Sample	BP (systolic)	BP (diastolic)	Weight (kg)
Discount	-1.583	-0.845	-1.380
	(2.197)	(1.371)	(1.281)
Fixed CC	-0.308	0.070	-2.753
	(2.175)	(1.392)	(1.270)**
Personalized CC	-0.567	1.363	-2.090
	(2.141)	(1.407)	(1.272)
Fixed CC + Discount	-2.515	-1.230	-0.705
	(2.142)	(1.256)	(1.291)
Personalized CC + Discount	-1.041	0.454	-2.349
	(2.199)	(1.428)	(1.265)*
Observations	1,512	1,512	1,512
R-squared	0.00	0.00	0.00
Control Group Mean (SD)	139.7 (25.0)	85.0 (15.6)	67.4 (14.1)
Panel B: "Ideal" Sample	BP (systolic)	BP (diastolic)	Weight (kg)
Discount	-4.336	-1.100	0.381
	(4.563)	(2.499)	(2.706)
Fixed CC	-6.536	-2.969	-4.158
	(4.479)	(2.806)	(2.907)
Personalized CC	-3.082	1.185	-2.525
	(4.341)	(2.527)	(2.612)
Fixed CC + Discount	-7.405	-2.253	-4.266
	(4.394)*	(2.274)	(2.478)*
Personalized CC + Discount	-6.547	0.167	-2.585
	(4.410)	(2.628)	(2.669)
Observations	395	395	395
R-squared	0.01	0.01	0.01
Control Group Mean (SD)	143.4 (28.7)	85.6 (15.2)	69.4 (16.0)

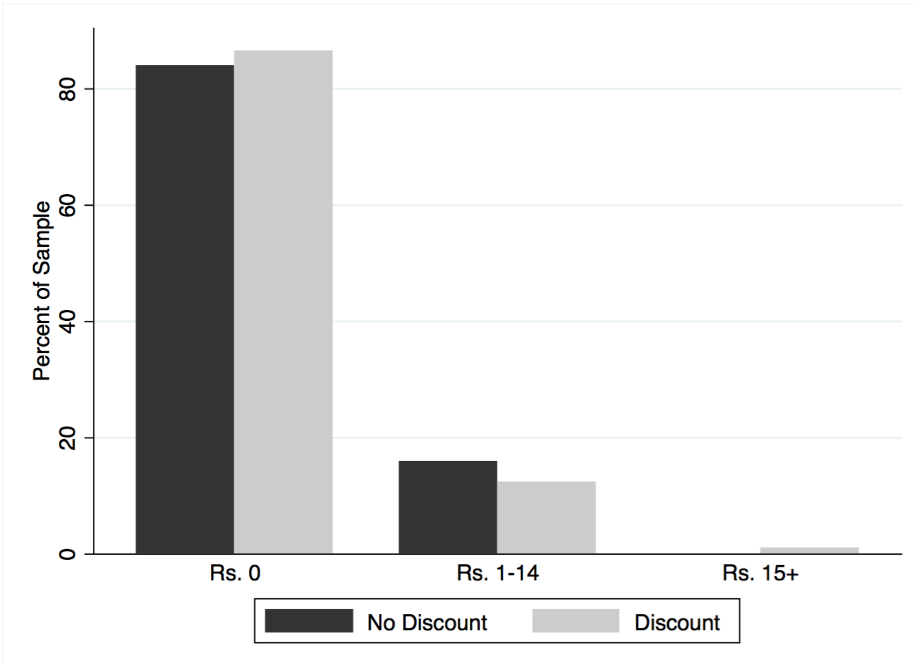
Notes: Robust standard errors in parentheses. *** p<0.01, ** p<0.05,* p<0.10. Village indicators are included in all specifications. The "ideal" sample is defined as respondents who both believe "it is possible to be healthy with hypertension if blood pressure is frequently monitored" *and* who trust the service provider.

Figure A.1: Managing Hypertension



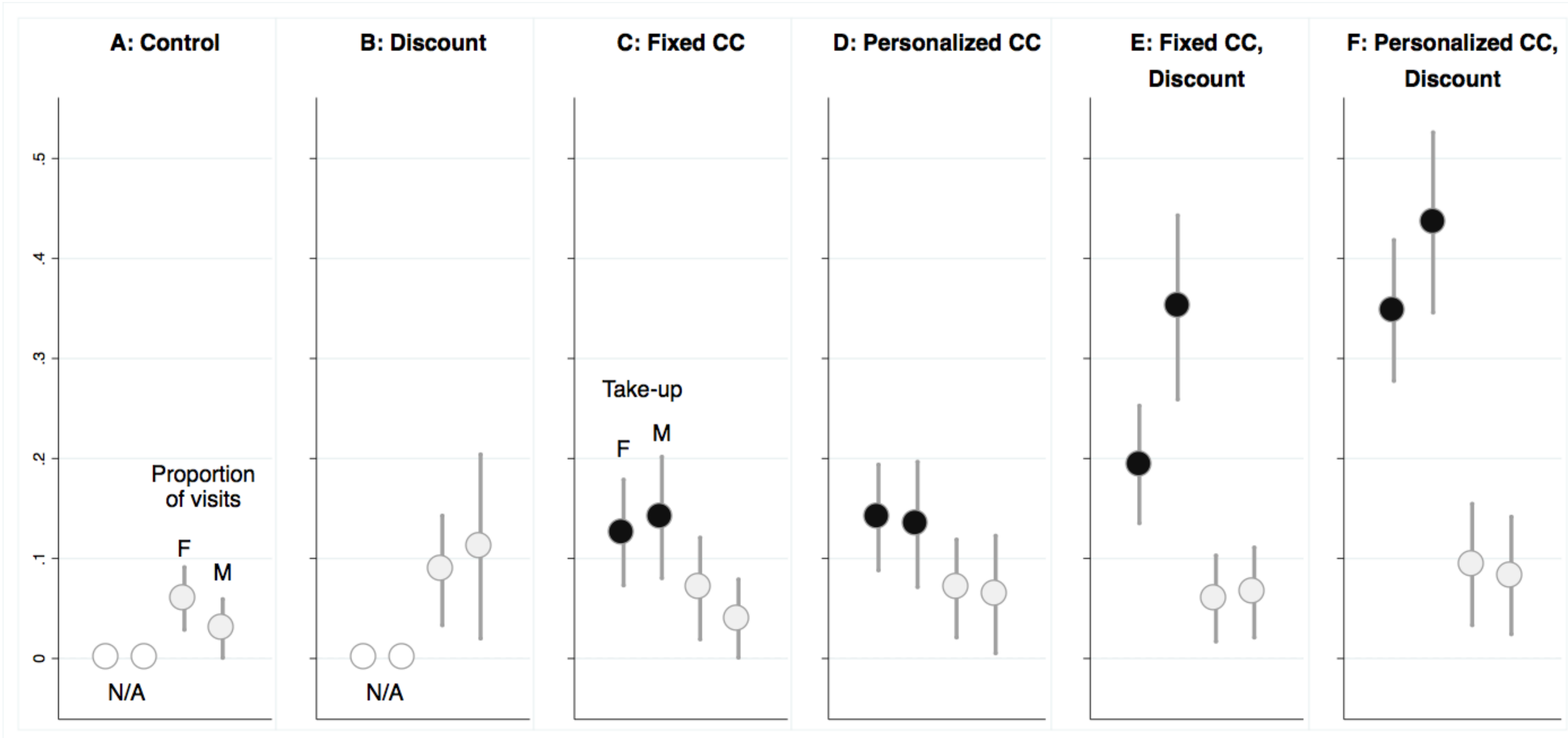
Notes: Questions and sample sizes are taken from the baseline survey.

Figure A.2: Personalized Commitment Contract Contributions



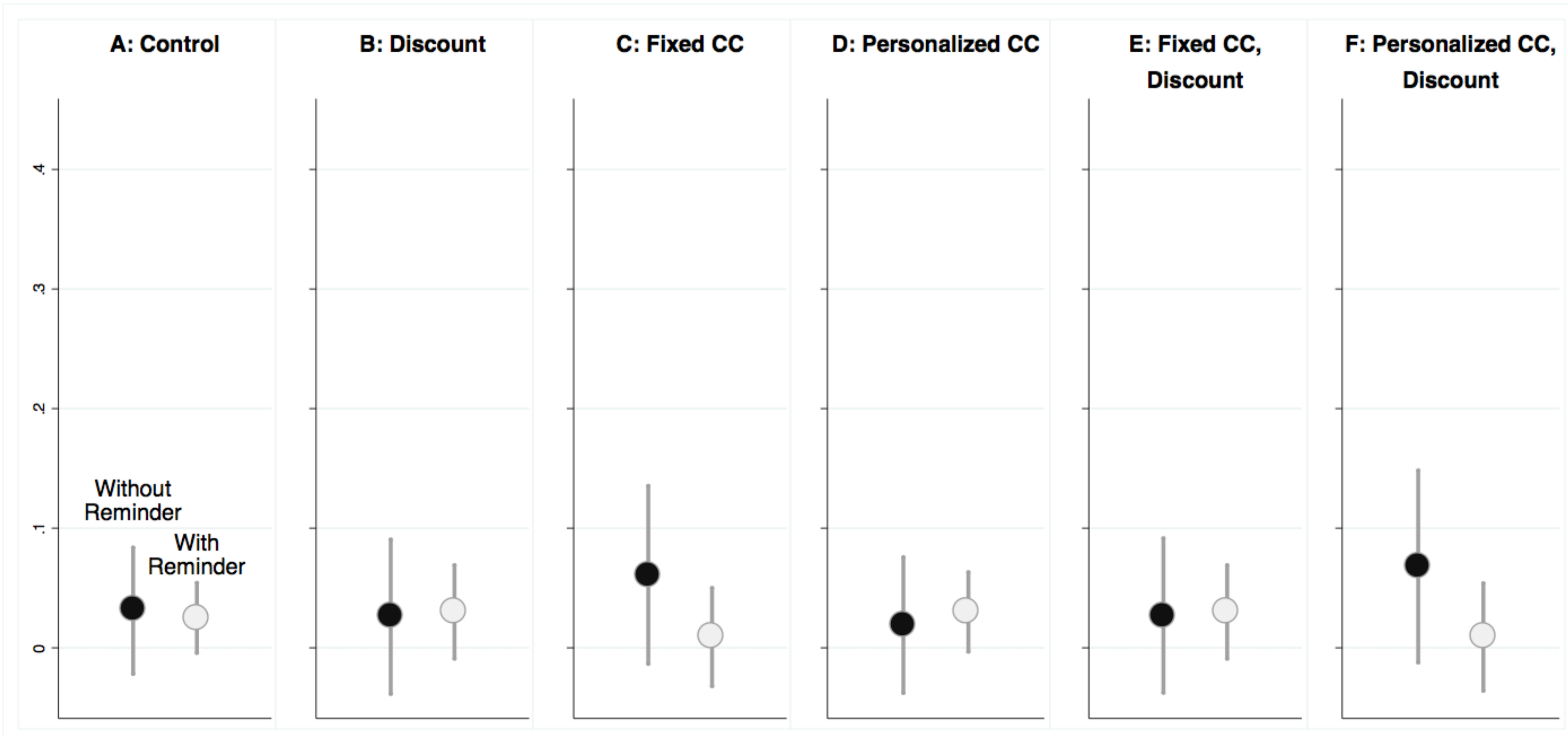
Notes: For participants who signed up for a personalized commitment contract, they could choose the amount m to be received at each future health camp visit. In the fixed commitment contract group, it was Rs. 15. In addition, the discount group also received 50% off the price of consultations (normally Rs. 30). The sample used here is comprised of the 114 study participants that signed up for a personalized commitment contract. Of these, 25 were in the personalized CC group, while 89 were in the personalized CC + discount group. There was only one case where a participant chose a commitment amount (Rs. 40 per visit) larger than that in the fixed CC.

Figure A.3: Commitment Contract Take-up and Fraction of Clinic Visits by Treatment Group and Gender



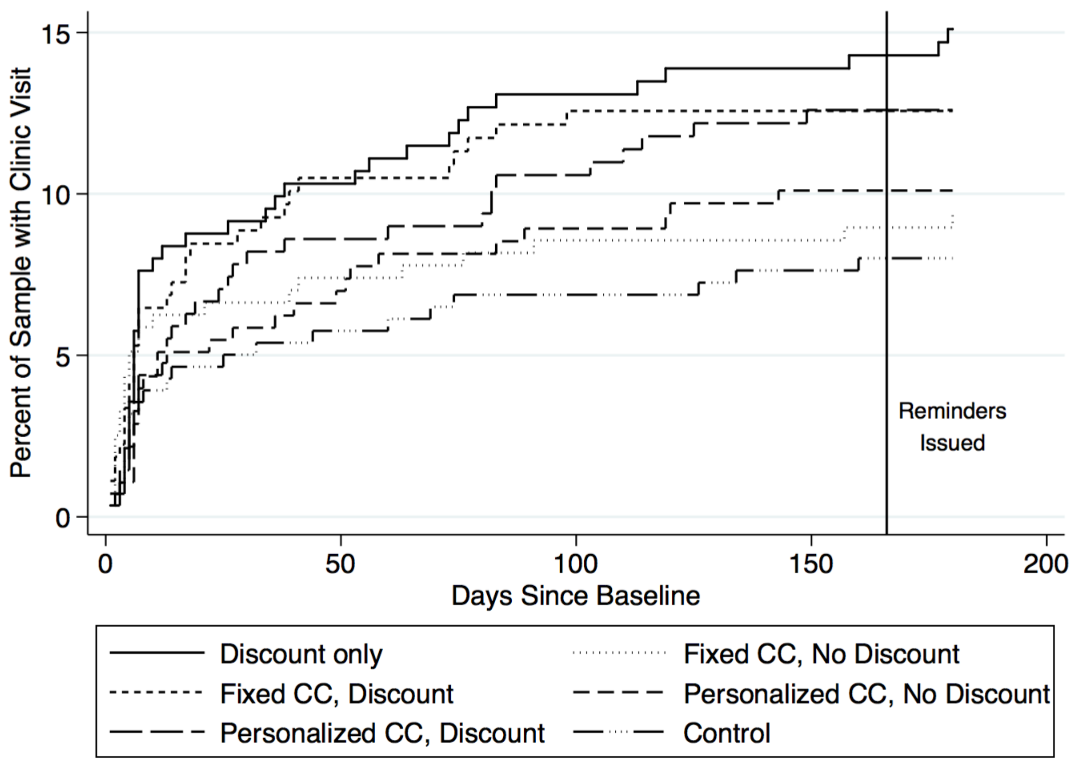
Notes: Take-up refers to the fraction of study participants in each treatment group that signed up for the commitment contract on offer. It is therefore not applicable to the control and discount groups (panels A and B respectively), as participants in these groups were not offered commitment contracts. Proportion of visits is the average for participants in each treatment group during the six-months intervention period, out of the recommended three visits. Black circles denote average take-up rates of commitment contracts in each treatment group, while gray circles denote average proportions of health camp visits in each treatment group. The vertical gray lines correspond to 95% confidence intervals around coefficient estimates of treatment group indicators in a regression with village fixed effects.

Figure A.4: Post-Reminder Clinic Visits by Treatment Group



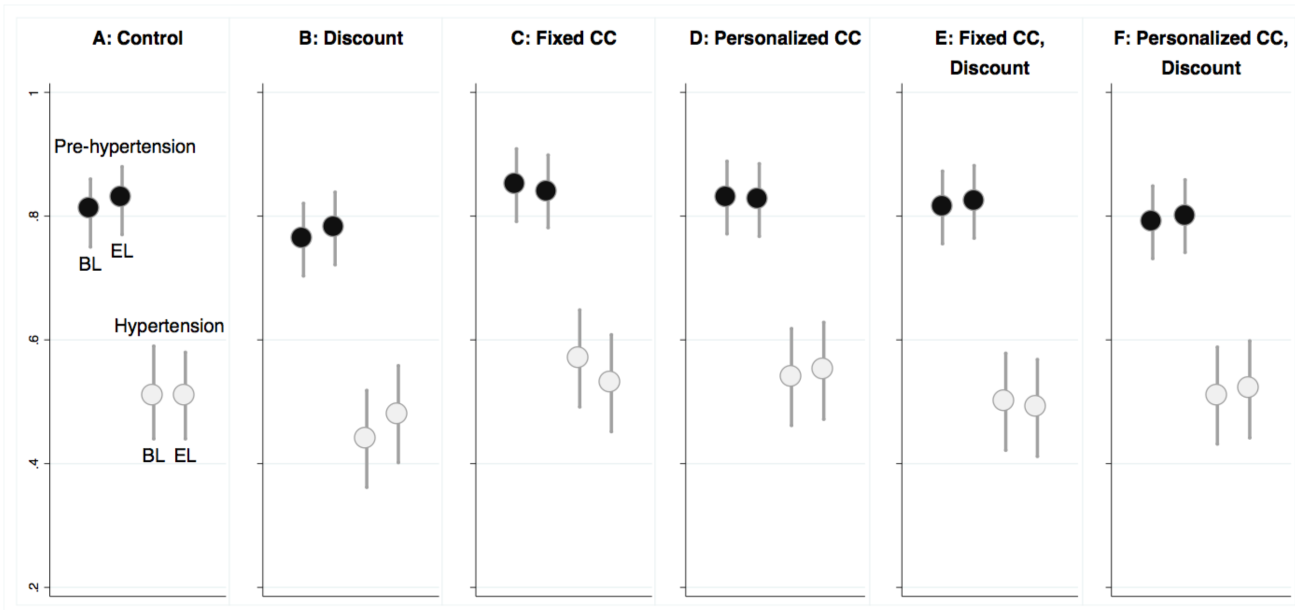
Notes: This figure shows the proportion of participants who visited the doctor after receiving randomized reminders. These were delivered in person, by enumerators at least two weeks before the discount coupons and commitment contracts expired. Hence, participants had at least two more opportunities to visit the doctor. Black circles denote average proportions of health camp visits in each treatment group for those that did not receive a reminder, while gray circles denote average proportions of post-reminder visits in each treatment group for those that did receive a reminder. The vertical gray lines correspond to 95% confidence intervals around coefficient estimates of treatment group indicators in a regression with village fixed effects.

Figure A.5: Preventive Visits Over Time



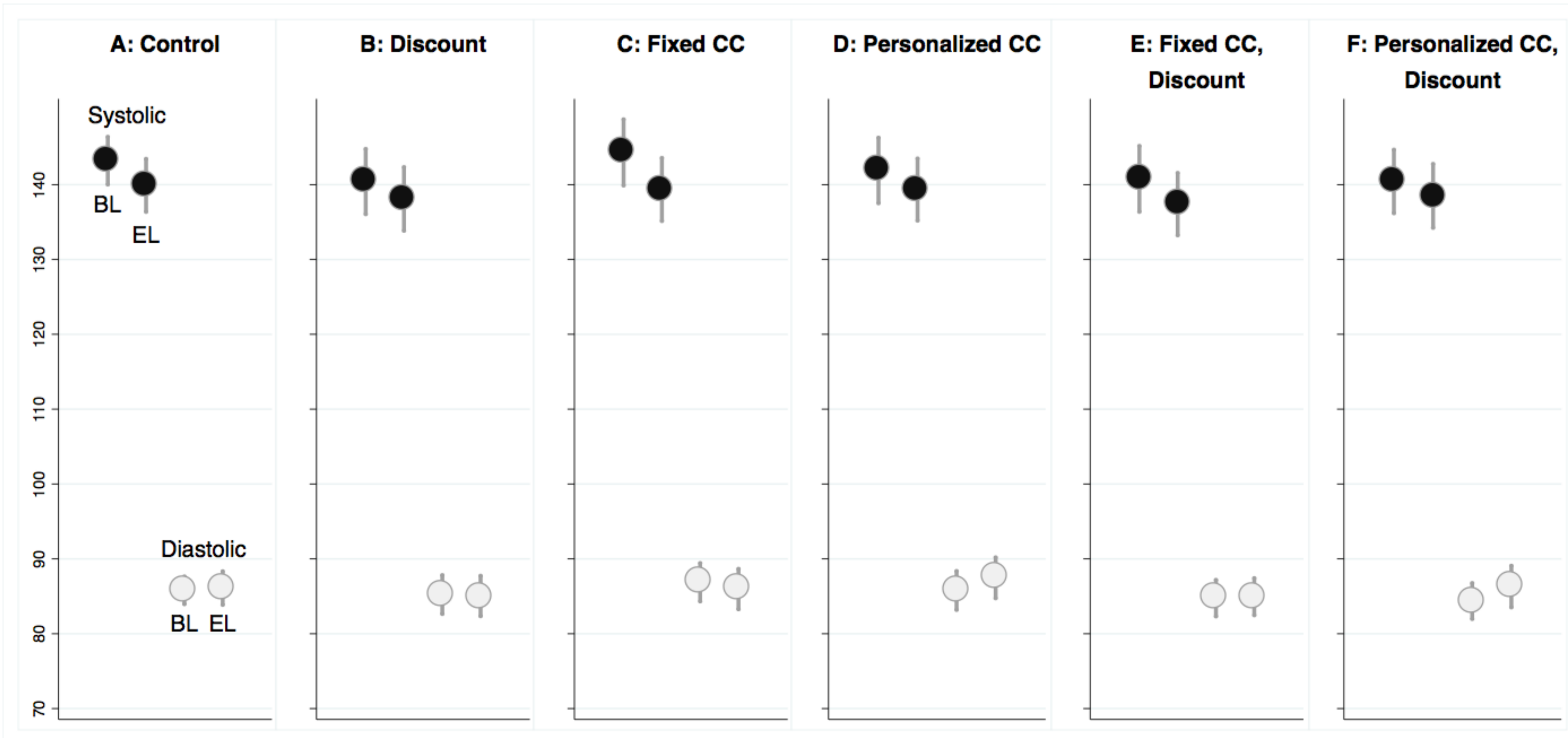
Notes: This figure shows the percentage of respondents who visited the clinic over time, by treatment group. The vertical line corresponds to the time when reminders were delivered to a randomly selected subset of study participants.

Figure A.6: Hypertension Status by Treatment Group



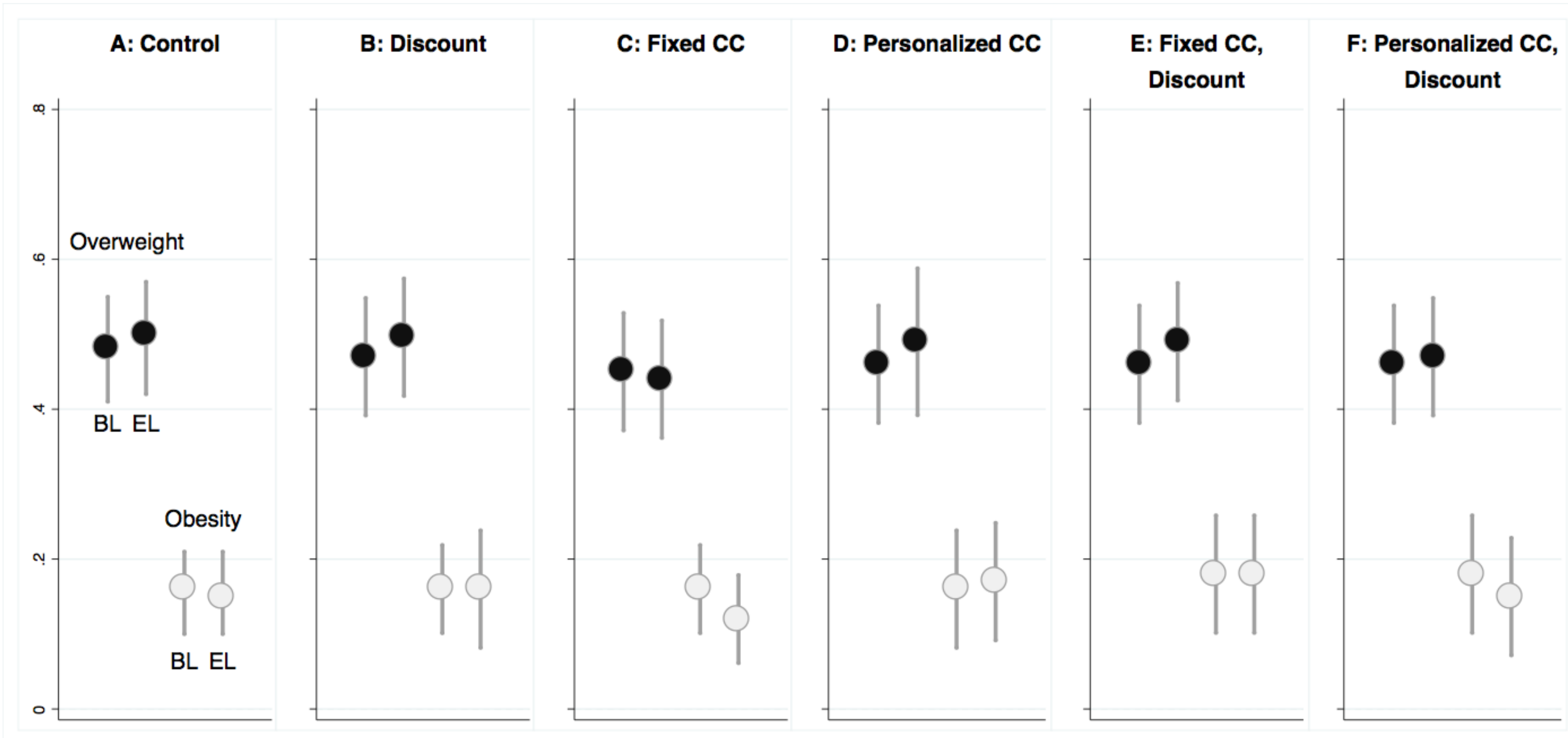
Notes: A normal systolic (diastolic) blood pressure is below 120(80) mmHg. A systolic (diastolic) blood pressure number between 120(80) and 139(89) mmHg is considered to be pre-hypertension. A systolic (diastolic) blood pressure number of 140(90) mmHg or higher is considered to be hypertension. Black circles denote the average baseline (BL) and endline (EL) incidence of pre-hypertension and hypertension in each treatment group, while gray circles denote the average baseline (BL) and endline (EL) incidence of hypertension in each treatment group. The vertical gray lines correspond to 95% confidence intervals around coefficient estimates of treatment group indicators in a regression with village fixed effects.

Figure A.7: Baseline and Endline Blood Pressure by Treatment Group



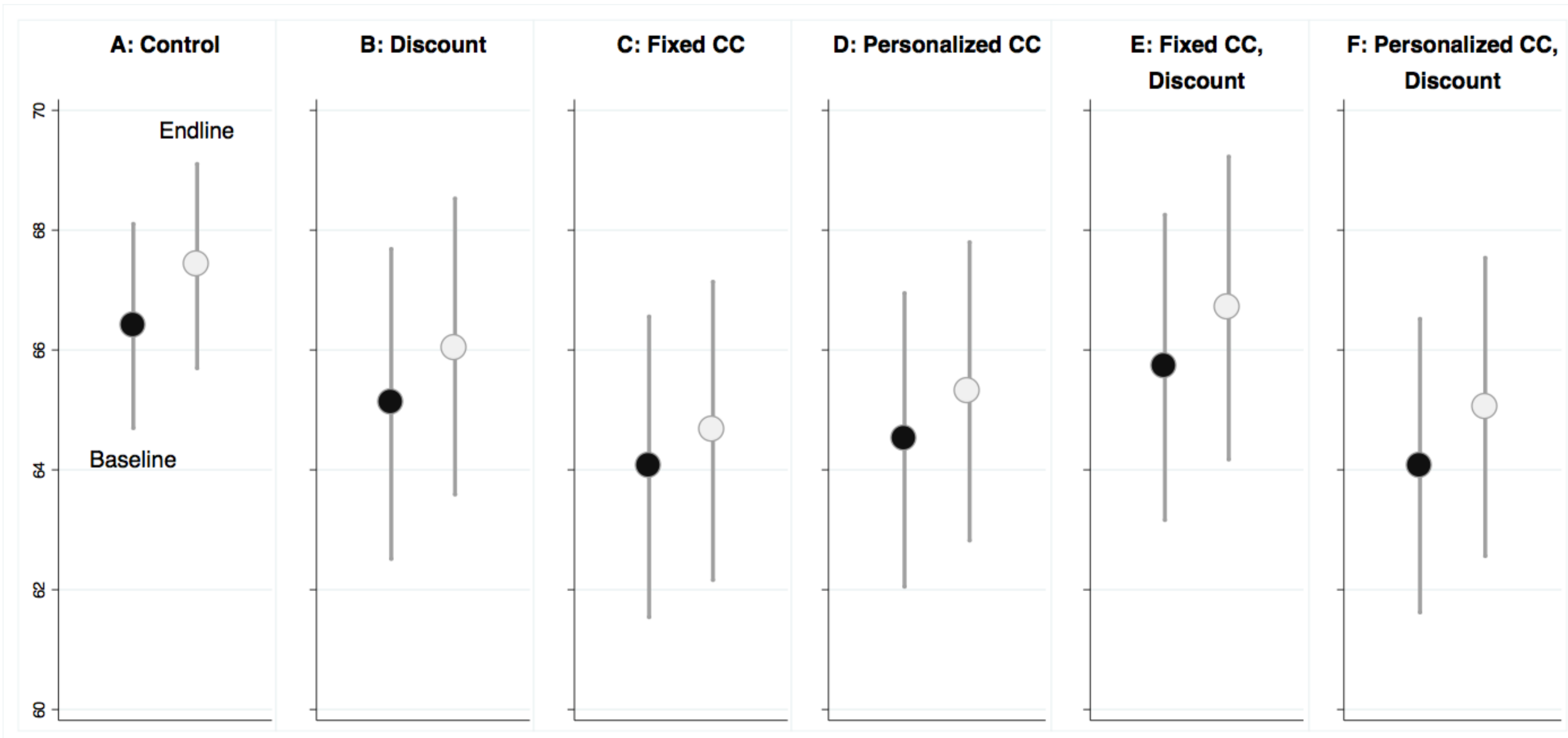
Notes: A normal systolic (diastolic) blood pressure is below 120(80) mmHg. A systolic (diastolic) blood pressure number between 120(80) and 139(89) mmHg is considered to be pre-hypertension. A systolic (diastolic) blood pressure number of 140(90) mmHg or higher is considered to be hypertension. Black circles denote average baseline (BL) and endline (EL) systolic blood pressure measures in each treatment group, while gray circles denote average baseline (BL) and endline (EL) diastolic blood pressure measures in each treatment group. The verticle gray lines correspond to 95% confidence intervals around coefficient estimates of treatment group indicators in a regression with village fixed effects.

Figure A.8: Weight Status by Treatment Group



Notes: Overweight is defined as a body mass index (BMI) of 25 or above. Obesity is defined as a BMI of 30 or above. Weight measurements were carried out by enumerators during baseline and endline surveys. Black circles denote the average baseline (BL) and endline (EL) incidence of overweight in each treatment group, while gray circles denote the average baseline (BL) and endline (EL) incidence of obesity in each treatment group. The vertical gray lines correspond to 95% confidence intervals around coefficient estimates of treatment group indicators in a regression with village fixed effects.

Figure A.9: Weight by Treatment Group



Notes: Weight measurements (in kilograms) were carried out by enumerators during baseline and endline surveys. Black circles denote average baseline body weights in each treatment group, while gray circles denote average endline body weights in each treatment group. The vertical gray lines correspond to 95% confidence intervals around coefficient estimates of treatment group indicators in a regression with village fixed effects.

B Identification: Additional Discussion

In Section 4 we discussed identification of the structural model parameters, with an emphasis on the identification of β and $\hat{\beta}$. It is also important to discuss how the remaining parameters are identified. δ is separately identified from β because δ describes relative $t = 1$ and $t = 2$ utility from a $t = 0$ decision perspective, but $\beta\delta$ describes that relative utility from a $t = 1$ perspective.

b is identified separately from β because at $t = 0$, when choosing commitment contracts, consumers choose based on $\hat{\beta}$ and δ, and b but not β. Then, given randomization across different treatments, choices made at $t = 0$ reflect b but not β, as long as consumers are not perfectly sophisticated with respect to their self-control. Cost is identified separately from other factors because cost occurs at $t = 1$, and thus is not indexed by δ, and is only multiplied by $\hat{\beta}$ at $t = 0$ but not by β at any point.

Finally, it is important to note that δ is not non-parametrically identified separately from b since these factors always multiply each other in consumer utility at all time periods they enter. They are parametrically identified given the assumptions that benefits are linear in observables and have normally distributed unobserved heterogeneity, while we do not allow for heterogeneity in δ. In practice, this means that $\delta * b$ should be thought of as one identified quantity, rather than ascribing a specific proportion of this quantity to δ or b. We treat $\delta * b$ this way in our results discussion in the main text.

C Estimation Details

This appendix proides additional detail on the estimation methodology for the our primary structural approach, described in Section 4 in the text. We estimate the model with a smoothed Accept-Reject simulated maximum likelihood methodology that, given the candidate parameters, matches the predicted decision paths for consumers in the population to their actual decision paths [see e.g. Train (2009) for an econometric discussion and Handel (2013) for another applied example]. Define the set of parameters to be estimated as Θ.[24] For a consumer with set of observables X in treatment T we match their sequence of decisions (which depends on T) to the predicted sequences of decisions for candidate parameters, and choose the parameters with the best match given choices across the control and all five treatments.

There are three types of decisions that could enter the decision path for a given individual. The first is choice of commitment contract (if offered). The second is what amount they commit specifically if the contract offered is a personalized contract. The third is whether the visit the doctor or not at $t = 1$. For the random coefficient parameters representing unobserved heterogeneity on each dimension, we take 50 simulated draws, above which the estimation results are stable.

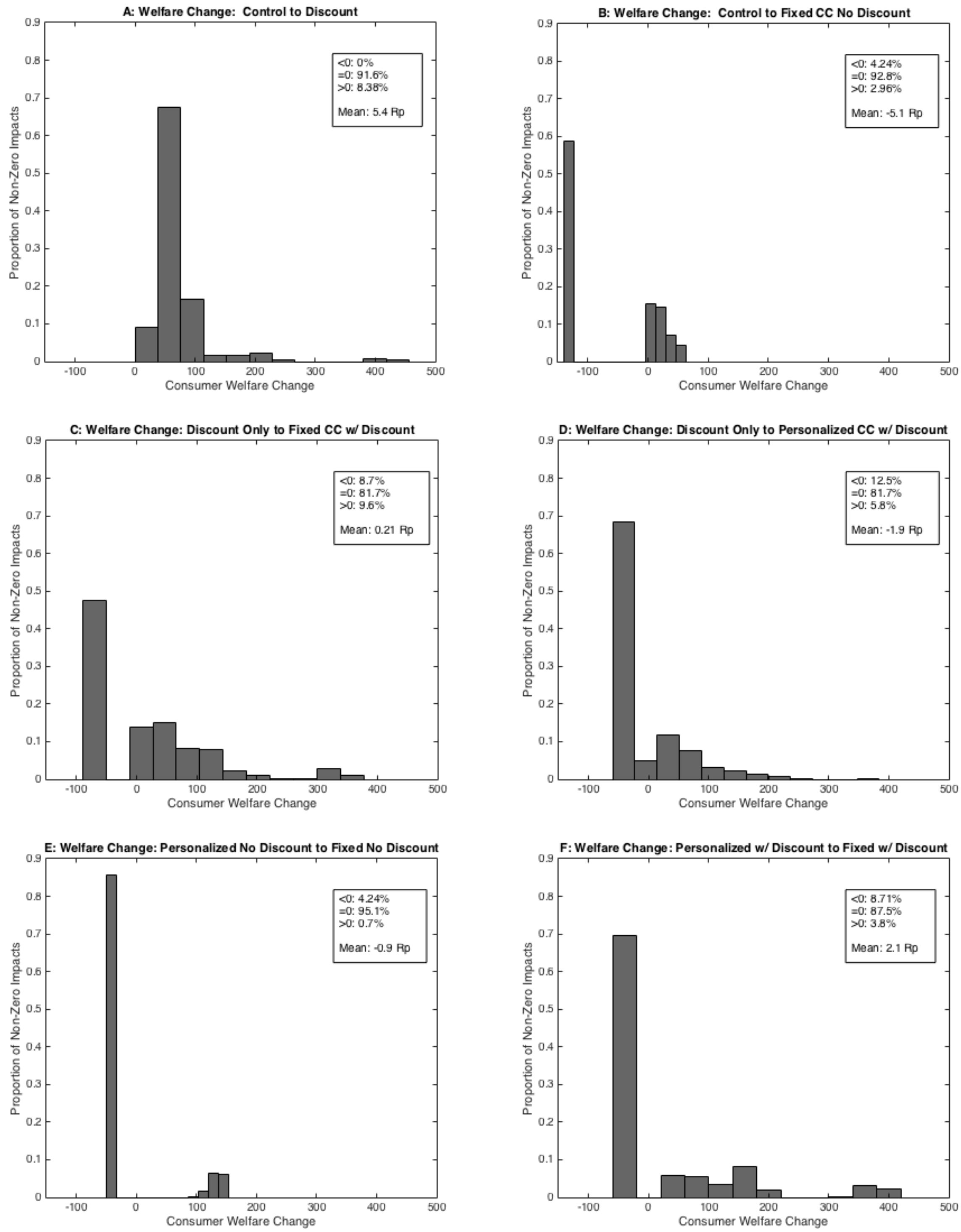
We now describe the likelihood function construction. For doctor attendance, for a given simulated draw of the parameters, the probability someone visits the doctor if in the control group is:

$$P_s(d_{i,v} = 1|\Theta) = \mathbf{1}[C(X_i) + f \leq \beta\delta b_i - \min(\max(\frac{0.01}{\beta\delta b_i - C(X_i) - f}, -0.01), 0.01)]$$

Here, $d_{i,v}$ is an indicator variable that equals 1 if someone goes to the doctor and 0 otherwise. For a given simulated draw, without smoothing, this always equals 1 or 0. Accept-Reject smoothing

[24] In our primary specification, these parameters include α_β, κ_β, σ_ς, τ_1, τ_2, $\kappa_{\hat{\beta}}$, α_b, κ_b, σ_ϵ, α_C, κ_C, and δ.

Figure A.10: Distribution of Consumer Welfare Impacts from Different Treatments



Notes: This figure plots the distribution of non-zero consumer welfare impacts (CW) from moving across different treatment arms. These are computed assuming that our entire population is enrolled in a given treatment, and applying our parameter estimates (see Table 5) to study decision-making and subsequent outcomes. Furthermore, they are computed from an ex ante, or $t = 0$ perspective. This means that consumer welfare in each treatment equals their discounted benefit of visiting the doctor, minus the costs (both pecuniary and non-pecuniary), and minus any pledged commitment amounts lost due to not following through.

helps the simulator function continuously by making this outcome probabilistic, but be equal to 1 or 0 in the limit as preferences become stronger for or against visiting the doctor. The smoothing term is the second fractional term, and helps optimization function but does not impact the ultimate results since it approximates the true binary values in the limit.

Given this value of P_s for each draw s, the overall probability of a doctor visit for candidate parameters Θ is:

$$P(d_{i,v} = 1|\Theta) = \Sigma_{s=1}^{S} \frac{P_s(d_{i,v} = 1|\Theta)}{S}$$

The construction of $P(d_{i,v} = 1|\Theta)$ for consumers in each of the five treatments is similar, but the per visit fees change accordingly.

For consumers who get to choose a commitment contract, we define $d_{i,c}$ equal to 1 if they accept the contract and 0 if they decline it. We introduce accept-reject smoothing on this decision as well, since it is a binary decision with simulated draws. For example, for a given simulated draw s, consumers who (i) are offered a fixed commitment contract with no discount and (ii) have preferences that satisfy equations 1 and 2:

$$P_s(d_{i,c} = 1|\Theta) = 1 - \max(\min(\frac{0.1}{C(X_i) + f - \hat{\beta}\delta b_i}, 0.01), 0)$$

Again, here the smoothing implemented brings this probably to 1 in the limit as the preference for commitment becomes larger and larger. We implement a similar condition for when a consumer declines the contract offer, in which case $P_s(d_{i,c} = 1|\Theta)$ limits to 0. For a given consumer, the likelihood of accepting a contract offer given Θ is:

$$P(d_{i,c} = 1|\Theta) = \Sigma_{s=1}^{S} \frac{P_s(d_{i,c} = 1|\Theta)}{S}$$

For consumers offered either a personalized commitment contract, or a commitment contract with a discount, the process for constructing the likelihood is similar, and follows the equations for accepting the contract laid out earlier in this section.

Finally, for consumers who accept a personalized commitment contract, they have a third decision: how much to commit. This is a continuous decision, except for people choosing the boundary commitment amount of $M_i = 0$, in which case they just pay the lump sum for all recommended doctor visits up front (with no additional commitment). For a given draw of parameters s, consumers who accept a commitment contract have a predicted commitment amount m_i that satisfies equation 5. We use a uniform kernel likelihood for this decision such that:

$$P_s(m_{s,i,f} = m_{i,f}) = 1 \text{ iff}[m_{s,i,f}[\Theta] - 5 \leq m_{i,f} \leq m_{s,i,f}[\Theta] + 5]$$

Here, if the actual amount committed in the personalized contract is within 5 rupees of the predicted amount $m_{s,i,f}[\Theta]$ for a given draw, it counts as the same outcome, and otherwise counts as a 0, or different outcome.[25] The probability that someone commits their actual amount $m_{i,f}$ given Θ is:

$$P(m_{i,f}|\Theta) = \Sigma_{s=1}^{S} \frac{P_s(m_{s,i,f} = m_{i,f})|\Theta)}{S}$$

Of these three possible decisions, all individuals always decide whether or not to attend the doctor, regardless of the treatment. The other two decisions depend on whether the individual is offered a commitment contract and whether they accept a personalized contract. Given this, we define the

[25] In practice, varying this kernel threshold around 5 does not impact the results.

log-likelihood contribution for each individual based on their decision paths we need to consider. The log-likelihood contribution for individuals in the control and discount only treatments is:

$$SLL_i(\Theta) = [P(d_{i,v} = 1|\Theta)\mathbf{1}[d_{i,v} = 1] + [1 - P(d_{i,v} = 1|\Theta)]\mathbf{1}[(1 - d_{i,v}) = 1]]$$

For individuals who are offered a commitment contract, but don't accept a personalized contract, their log-likelihood contribution is:

$$\begin{aligned} SLL_i(\Theta) \quad &= [P(d_{i,v} = 1|\Theta)\mathbf{1}[d_{i,v} = 1] + [1 - P(d_{i,v} = 1|\Theta)]\mathbf{1}[(1 - d_{i,v}) = 1]] * \\ &[P(d_{i,c} = 1|\Theta)\mathbf{1}[d_{i,c} = 1] + [1 - P(d_{i,c} = 1|\Theta)]\mathbf{1}[(1 - d_{i,c}) = 1]] \end{aligned}$$

For individual who are offered a personalized contract, and accept that contract,the log-likelihood contribution is:

$$\begin{aligned} SLL_i(\Theta) \quad &= [P(d_{i,v} = 1|\Theta)\mathbf{1}[d_{i,v} = 1] + [1 - P(d_{i,v} = 1|\Theta)]\mathbf{1}[(1 - d_{i,v}) = 1]] * \\ &[P(d_{i,c} = 1|\Theta)\mathbf{1}[d_{i,c} = 1] + [1 - P(d_{i,c} = 1|\Theta)]\mathbf{1}[(1 - d_{i,c}) = 1]] * \\ &P(m_{i,f}|\Theta) \end{aligned}$$

Given this, the simulated log-likelihood function that we maximize over Θ for the population is:

$$SLL(\Theta) = \Sigma_{i=1}^{I} SLL_i(\Theta)$$

In order to simplify estimation, we use an outer loop with a coarse grid for the intercepts for the costs and benefits of going to the doctor (α_b and α_c respectively). The final estimates are for the combination of these two parameters that yield estimates with the best likelihood function value.