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USING PREFERENCE ESTIMATES TO CUSTOMIZE INCENTIVES: AN APPLICATION TO POLIO VACCINATION DRIVES IN PAKISTAN

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Using Preference Estimates to Customize Incentives: An Application to Polio Vaccination Drives in Pakistan

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ABSTRACT

We use estimates of time preferences to customize incentives for polio vaccinators in Lahore, Pakistan. We measure time preferences using intertemporal allocations of effort, and use these estimates to construct individually-tailored incentives. We evaluate the effect of matching contract terms to discounting parameters in a subsequent experiment with the same vaccinators. Our tailored policy is compared to alternatives that either rely on atheoretic reduced-form relationships for policy guidance or apply the same policy to all individuals. We find that contracts tailored to individual discounting outperform this range of policy alternatives.

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1 Introduction

The preference parameters governing intertemporal decisions affect a broad range of outcomes, justifying the considerable theoretical and empirical investments made to describe the level and shape of discounting.[1] An understanding of intertemporal preference parameters also provides valuable policy guidance. Indeed, a number of recent policies are motivated by empirical research on time preferences: commitment savings products, default retirement allocations, and the Save More Tomorrow retirement savings program are all partly motivated by the insight that time preferences may be 'present-biased' (for discussion and examples see, e.g., Laibson, 1997; Benartzi and Thaler, 2004; Beshears, Choi, Laibson and Madrian, 2009; Ashraf, Karlan and Yin, 2006; Blumenstock, Callen and Ghani, 2018).

Policy interventions may be further enhanced by using individualized, rather than broad, information on time preferences. Differences in experimental measures of time preferences correlate with differences in a number of policy-relevant behaviors such as take-up of commitment devices and credit card borrowing (examples include Chabris, Laibson, Morris, Schuldt and Taubinsky, 2008b; Meier and Sprenger, 2008, 2012, 2010; Ashraf et al., 2006; Dohmen, Falk, Huffman and Sunde, 2006; Castillo, Ferraro, Jordan and Petrie, 2011).[2] These correlations suggest that interventions could leverage individual information on time preference to tailor unique policies for each person. The purpose of tailoring policies is to shape real-world behavior. Therefore, we study the potential to deploy lab protocols for measuring preferences and structurally tailoring contracts in the field.

This paper studies the promise of theoretically-informed, individually-tailored policy inter-

[1] Central examples of theoretical work include Samuelson (1937); Koopmans (1960); Laibson (1997) and O'Donoghue and Rabin (2001). Empirical exercises in field and laboratory settings focusing on parameter estimation include Hausman (1979); Lawrance (1991); Warner and Pleeter (2001); Cagetti (2003); Laibson, Repetto and Tobacman (2005); Mahajan, Michel and Tarozzi (2020); Harrison, Lau and Williams (2002); Andreoni and Sprenger (2012).

[2] It should be noted that none of these examples linking structural estimates of time preference to other behaviors provide an articulated model for what the precise correlation between the two values should be. Unlike our own efforts, such exercises could be conducted without appeal to structural estimation.

ventions in a real-world policy setting. Our project engages government health workers—termed Lady Health Workers (LHWs)—associated with polio eradication efforts for the Department of Health in Lahore, Pakistan.[3] The function of LHWs is to provide oral polio vaccine to children during monthly vaccination drives, which usually last two days. We introduce a monitoring and incentive system to measure intertemporal preferences via effort choices at work. Closely following the Convex Time Budget (CTB) design of Andreoni and Sprenger (2012); Augenblick, Niederle and Sprenger (2015), LHWs are asked to trade off work between the two days of a vaccination drive. Unlike standard laboratory measurements of time preferences, for empirical realism, each LHW only makes a single CTB choice. Completion of the allocated work is tied to a bonus of 10 times the standard LHW daily wage. Under a set of structural assumptions, the distribution of work allocations identifies the distribution of time preferences accounting for shocks to the marginal costs of effort. Each LHW's choice can, in turn, be linked to an expected level of discounting at the individual level under the estimated distributions of time preferences and shocks.

We then tailor policy based upon these measured preferences in a subsequent work decision. The structural, tailored policy we examine attempts to equalize vaccinations over time by changing relative prices for each LHW.[4] We compare this tailored policy to alternatives that span the policy space on two dimensions: broad vs. tailored and structural vs. atheoretic. A broad policy is one that is applied uniformly to all individuals, while a tailored policy is one individualized to each based on some characteristic. In our setting, a broad policy is one where all vaccinators face the same price for trading vaccinations off across the two days, while an individualized policy is one where each vaccinator faces their own price. A structural policy

[3]Polio is endemic in Pakistan. Of 350 new worldwide cases in 2014, 297 occurred in Pakistan, constituting a 'global public health emergency' according to the World Health Organization.Between 95 percent and 99 percent of individuals carrying polio are asymptomatic. One infection is therefore enough to indicate a substantial degree of ambient wild polio virus. The disease largely affects children under five.

[4]Our tailored policy was pre-specified prior to conducting the study, and was chosen to precisely equalize vaccinations in the absence of shocks. Accounting for shocks to marginal costs in our distributional estimates alters the policy target slightly from equal vaccinations over time.

is one that draws policy guidance from a theoretical model of preferences, while an atheoretic policy draws policy guidance from some reduced-form relationship.[5] In our setting, we use a model to translate LHWs' allocations of vaccinations to an estimate of their preferences. Our structural policy uses information on measured preferences, while atheoretic policies do not. Our structural, tailored policy is compared to broad policies which set relative prices to achieve the same objective based on aggregate values, or the reduced-form price-sensitivity of effort; and a tailored, but atheoretic policy, which sets relative prices based on a simple rule of giving higher relative prices to plausibly more patient individuals. These comparisons are facilitated by an overarching control group that receives a uniform random price, from which we draw subsets for comparison purposes.

In a sample of 338 LHWs, we document three principal findings. First, on aggregate, we estimate effectively no present bias in between-subjects comparisons. LHWs choosing their work in advance of the first day of the vaccination drive allocate slightly more of their vaccinations to the first day of the drive than those allocating on the morning the drive actually commences, but corresponding estimates of the distribution of time preferences indicate only a small and insignificant degree of present bias. Interestingly, when examining within-subjects data, comparing individual LHWs across drives, our estimates of present bias grow more substantial, and accord with estimates from prior laboratory studies of present bias in effort. Second, there is a large degree of heterogeneity in time preferences across subjects and this heterogeneity is markedly more pronounced when LHWs make immediate choices. This sizable cross-sectional variation also resonates with prior experimental exercises. Third, and most importantly, our structural, tailored policy works. Relative to a range of policy alternatives, our intervention generates behavior around 30% closer to the policy target of equal allocation. Interestingly, when focusing on conditions where subjects are asked to make allocations which take effect immediately – that is, when present bias may be relevant – the tailored policy generates a

[5]Though far from an exhaustive labeling of potential policies, this 2-by-2 labeling helps to organize the comparisons we investigate.

roughly 50% improvement.

This paper makes two contributions. First, our exercise uses field behavior about effort to examine time preferences, reflecting, to our knowledge, the first attempt to use the Augenblick et al. (2015) methods for measurement based on an actual real-world work (vaccinations).[6] This effort yields some valuable lessons for future field implementations. While lab experiments have elicited preferences based on a few hours of effort, participants in our study make decisions over days' worth of work. Shocks to the marginal cost of effort have plausibly greater consequences when allocating several hundred vaccinations attempts in the field rather than fifty greek transcriptions online as in Augenblick et al. (2015). We, correspondingly, find an important role for shocks in our estimations. The volume of work required in our field setting generates a further difference from lab implementations. While roughly 90% of Augenblick et al.'s subjects completed all allocated tasks and requirements of the study and received a $100 completion bonus, fewer of our subjects completed all their allocated vaccinations. While most LHWs appear to have made substantial efforts to meet their targets, completing about 75% of the required vaccinations, only about 50% of our sample successfully completed all vaccinations, even with a bonus of 10 times the daily LHW wage.[7] This difference between the lab and the field requires us to account for probabilistic completion and the potential distortionary effects of noncompletion in our analysis. We develop empirical tools for estimating the distribution of time preferences from a limited number of CTB choices, accounting for shocks and probabilistic completion, which may be valuable for researchers conducting future field efforts in this vein.

[6] Documenting dynamic inconsistency outside of the laboratory and outside of the standard experimental domain of time-dated monetary payments is particularly valuable given recent discussions on the elicitation of present-biased preferences using potentially fungible monetary payments (Cubitt and Read, 2007; Chabris, Laibson and Schuldt, 2008a; Andreoni and Sprenger, 2012; Augenblick et al., 2015; Carvalho, Meier and Wang, 2014).

[7] The lack of uniform completion was not a feature of the data we initially expected, but, in retrospect, is something we could have anticipated. Data from drives prior to our intervention, which were subject to almost no monitoring or scrutiny, showed that LHWs almost without exception hit their prescribed targets exactly. We believe these reports are at least partially driven by the fact that polio is a politicized issue in Pakistan, with a number of stakeholders and international donors being eager to demonstrate high numbers of vaccinations. Given our lack of foresight, the functional forms accounting for probabilistic non-completion were in our study registration. As such, they should be viewed with appropriate caveats.

The consistencies between our findings and prior lab experimental work helps to support the growing literature which identifies present bias from CTBs and non-monetary choices in the field (Read and van Leeuwen, 1998; Sadoff, Samek and Sprenger, 2015; Read, Loewenstein and Kalyanaraman, 1999; Sayman and Onculer, 2009; Kaur, Kremer and Mullainathan, 2010, 2015; Carvalho et al., 2014).[8]

Second, we provide the first empirical evidence to date of the value in structural, tailored policies for intertemporal choice in a field setting (or otherwise). Given these results, there is clear opportunity to expand the scope of interventions beyond uniform policy strategies. The policy objective we consider attempts to implement a smooth allocation over time. In our setting equalizing vaccine provision is important for the logistics of the vaccine supply chain. Polio vaccine requires cold storage. Managers therefore prefer that the amount of vaccine delivered every day meets a consistent and manageable target. Smoothing is also a natural objective to consider for other intertemporal decisions but may, of course, not be appropriate in all settings.[9] There are many other policy alternatives that could be considered, including the alternative of maximizing total vaccine provision[10] Hence, our results provide a simple

[8]These studies include examination of present bias or dynamic inconsistency for food choices (Read and van Leeuwen, 1998; Sadoff et al., 2015); for highbrow and lowbrow movie choices (Read et al., 1999); for cafe reward choices (Sayman and Onculer, 2009); for completing survey items (Carvalho et al., 2014); and for fertilizer purchase decisions (Duflo, Kremer and Robinson, 2011). For discussion of this literature, see Sprenger (2015).

[9]Evidence suggests that consumption of Supplemental Nutritional Assistance Program (SNAP) benefits may be subject to present bias, leading to declining consumption during the benefit period (see, e.g., Shapiro, 2005; Hastings and Washington, 2010). Our results indicate that a tailored policy along the lines implemented here could help smooth benefit consumption. Such a policy could complement alternatives that have been discussed, including increasing the frequency of benefit payments (Shapiro, 2005).

[10] One can consider a range of tailoring exercises using interest rate, R, to maximize a policymaker's objective function subject to each LHW's offer curve. If $P(w_{1,i}(R), w_{2,i}(R))$ is the objective function at LHW allocation $(w_{1,i}, w_{2,i})$, then under the structural assumptions and experimental design of this paper the general problem is

$$\begin{aligned} max_R\ P(w^*_{1,i}(R), w^*_{2,i}(R))\ s.t \\ (w^*_{1,i}(R), w^*_{2,i}(R)) = argmin\ (w_{1,i})^\gamma + \beta_i^{\mathbf{1}_{d=1}}\delta_i \cdot (w_{2,i})^\gamma\ s.t. \\ w_{1,i} + R \cdot w_{2,i} = V. \end{aligned}$$

Our exercise assumes a Leontief policymaker, $P(w_{1,i}, w_{2,i}) = min[w_{1,i}, w_{2,i}]$. If, instead, one assumed a maximizing policymaker, $P(w_{1,i}, w_{2,i}) = w_{1,i} + w_{2,i}$, then when $\gamma = 2$, the optimal $R^* = \sqrt{\beta_i^{\mathbf{1}_{t=1}}\delta_i(1 + \beta_i^{\mathbf{1}_{d=1}}\delta_i)} - \beta_i^{\mathbf{1}_{d=1}}\delta_i$. We estimate average values of $\beta_i\delta_i$ around 0.95, and so R^* for maximizing vaccinations is roughly 0.4. This was an unrealistic value for implementation in our setting, but such a policy should be investigated in future work.

proof-of-concept for tailoring to a range of targets.

The paper proceeds as follows: Section 2 presents our experimental design and corresponding theoretical considerations for estimating time preferences and tailoring contracts, Section 3 presents results, Section 4 provides robustness tests, and Section 5 concludes.

2 Experimental Design

Our experiment has three components: implementing a high resolution smartphone monitoring system similar to that described in Callen, Gulzar, Hasanain, Khan and Rezaee (2019), eliciting individual discounting parameters using the Convex Time Budget (CTB) technique (Andreoni and Sprenger, 2012; Augenblick et al., 2015), and, after assigning tailored contracts to LHWs, testing whether these tailored contracts outperform comparison policies.

2.1 Vaccinations, Smartphone Monitoring, and Evaluation of Monitoring Technology

The Department of Health in Lahore, Pakistan, employs LHWs throughout the city to conduct polio vaccination drives. Every month there is a vaccination drive that is at least two days long. Prior to our study, the standard protocol for vaccination drives was to provide each LHW a fixed target for total vaccinations over the drive and a map of potential households (called a "micro-plan"). LHWs received no explicit benefits for reaching targets; they received a fixed daily wage of 100 rupees (around $1 at contemporary exchange rates). LHWs mapped their walk with pen and ink, knocking on each compound door, and vaccinating each child if their parents granted permission. Vaccinating a child consists of administering a few drops of oral vaccine. As there is no medical risk of over-vaccination, LHWs are encouraged to vaccinate every child for whom permission is granted. For each attempted vaccination, LHWs were asked to mark information related to the attempt (number of children vaccinated, whether or not

all children were available for vaccination, etc.) in chalk on the compound wall. Appendix Figure A.1 provides an example of neighborhood micro-plan, Appendix Figure A.2 provides an example of a vaccination attempt, and Appendix Figure A.3 provides a picture of a chalk marking on a compound wall. At the end of each day, LHWs in each neighborhood convened with their supervisor and self-reported their vaccination activity for the day (see Appendix Figure A.4 for an example of the form). In principle, a monitor could verify the claims. In practice, however, there was virtually no monitoring, and reasons to suspect over-reporting.[11]

In collaboration with the Department of Health, we designed a smartphone-based monitoring system. The Department of Health provided a smartphone equipped with a vaccination monitoring application to all LHWs in our sample in order to record information related to each vaccination. For each vaccination attempt, the LHW was asked to take a picture of the home visited and her current vial of vaccine. An image of the main page of the application is provided as Figure 1, Panel A. Data from the smartphone system were aggregated in real-time on a dashboard available to senior health administrators (see Appendix Figure A.5 for an example of the dashboard). In order to separate the effects of this smartphone monitoring system from those of our incentive program discussed next, a sample of vaccinators (157 total between two vaccination drives) were given a smartphone equipped with the application and instructed on its use, but were not given any additional incentives beyond their daily wage.

[11]We attempted to independently audit LHWs by following the trail of chalk markings, but our enumerators found the process too difficult to produce a reliable audit of houses visited. We do, however, know the targets associated with each micro-plan prior to our monitoring intervention and that LHWs almost always reported meeting their targets exactly. Even with a bonus incentive and smartphone monitoring in place, we find that LHWs on average achieve only 62 percent (s.d. = 58 percent) of the target given by their micro-plans. LHWs likely would achieve a smaller share of their target in the absence of both monitoring and financial incentives.

Panel A: Splash Page

Panel B: Slider Bar

Figure 1: Vaccination Monitoring Smartphone App

Notes: The picture is of two screenshots from the smartphone app used by Vaccinators. Panel A is depicted after partially scrolling down. The top bar in Panel A (white letters) translates to "polio survey." The next panel down (blue letters) translates to "Dashboard" (literally transliterated). The black letters under the top button translate to "new activity", the letters under the second button translate to "send activity" and the letters under the lowest button translate to "set target". The blue letters in panel B translate to "set target". The next line translates to "First day: 133; Second day: 133". The text next to the box translates to "finalize target" and the black letters on the bar translate to "set target."

2.2 Drive 1: Intertemporal Bonus Contracts and the Measurement of Preferences

The smartphone monitoring application was equipped with additional functionality for implementing intertemporal bonus contracts. These contracts had LHWs set daily work targets in order to receive a substantial bonus. The chosen targets provide critical intertemporal preference information that we use to estimate the distribution of preferences, and, subsequently, to tailor future contracts.

We worked with the Department of Health to implement these contracts in two-day drives in September, November and December of 2014. The contracts required workers to complete a present value total of $V = 300$ vaccination attempts in exchange for a fixed bonus of 1000 rupees. LHWs set daily targets, v_1 and v_2, corresponding to vaccinations on day 1 and day 2 of the drive, respectively. If either of the vaccination targets, v_1 or v_2, were not met, the 1000 rupees would not be received, and the LHW would receive only her standard wage.

Each LHW was randomly assigned a relative price, R, translating vaccinations on day 1 to vaccinations on day 2. For each vaccination allocated to day 2, the number of vaccinations allocated to day 1 would be reduced by R. Hence, the targets v_1 and v_2 satisfy the intertemporal budget constraint

$$v_1 + R \cdot v_2 = V.$$

This bonus contract is identical to an experimental device termed a Convex Time Budget used to investigate time preferences (Andreoni and Sprenger, 2012; Augenblick et al., 2015).[12] Intertemporal allocations, (v_1, v_2), carry information on the time preferences of each LHW that can be used to estimate the distribution of time preferences.

[12]We also borrow an additional design element from such studies: minimum allocation requirements. In order to avoid LHWs allocating all their vaccinations to a single day of the drive, we placed minimum work requirements of $v_1 \geq 12$ and $v_2 \geq 12$. The objective of minimum allocation requirements is to avoid confounds related to fixed costs. That is, by requiring LHWs to work on both days of the drive, we avoid confounding extreme patience or extreme impatience with LHWs simply not wishing to come to work on one of the two days.

2.2.1 Experimental Variation

Our design leverages two sources of experimental variation. First, each LHW is randomly assigned a single relative price, R, from the set $R \in \{0.9, 1, 1.1, 1.25\}$. These values were chosen following Augenblick et al. (2015). Operationally, experimental variation in R was implemented by providing each LHW with a slider bar on the introduction screen of the smartphone application. Figure 1, Panel B depicts the slider bar with R equal to 1.25. The LHW was asked to pull the slider bar to their desired allocation (v_1, v_2) and then submit. The allocation was required to be submitted before commencing vaccination.

Second, each LHW was randomly assigned to either submit their allocation in advance of day 1 of the drive or on the morning of day 1. We refer to the first of these as the 'Advance' treatment arm and the second as 'Immediate' treatment arm. The assignment to either the Advance or the Immediate group was cross-randomized with the assignment of R, creating a 2 x 4 design within our incentive trreatments. Section 2.4 describes the efforts taken to make everything else besides allocation timing equal between the Advance and Immediate conditions.

Variation in the timing and interest rate of allocations provides relevant experimental variation for estimating the distribution of time preferences and evaluating whether the distribution is dynamically consistent. The distribution of LHW allocations in these conditions is mapped to a distribution of discounting parameters accounting for the influence of shocks. Distributional estimates in hand, we calculate an expected discount factor for each LHW based on their allocation under the estimated distributions of time preferences and shocks.

2.2.2 Structural Assumptions for Estimating Time Preferences

We make a number of structural assumptions in order to map from LHW allocations to discounting parameter distributions. First, we assume a stationary power cost of effort function $c(v) = v^\gamma$, where v represents vaccinations performed on a given day, and $\gamma > 1$ captures convex costs of effort. In our pre-specified analysis plan, we posited γ to be constant across individuals

and our tailoring exercise was conducted under the assumption of $\gamma = 2$, quadratic costs. While the field implementation of intertemporal discounting experiments is likely to differ substantially from laboratory examples, quadratic costs are close to the prior laboratory findings for effort (see, e.g., Augenblick et al., 2015). While the cost function is constant across individuals and over time, we assume below that individual allocations are subject to random shocks in relative marginal costs across period, and we estimate the distribution of those shocks. Hence, in calculating an individual's expected discount factor we take into account this potential source of variation in costs.

Second, we assume that individuals discount the future quasi-hyperbolically (Laibson, 1997; O'Donoghue and Rabin, 1999). For a given LHW, i, making allocation $(v_{1,i}, v_{2,i})$ the discounted disutility of effort can be written as

$$v_{1,i}^{\gamma} + \beta_i^{\mathbf{1}_{d=1}} \delta_i \cdot v_{2,i}^{\gamma}.$$

The indicator $\mathbf{1}_{d=1}$ captures whether the decision is made in advance or immediately on day 1. The parameters β_i and δ_i summarize individual discounting. The parameter β_i captures the degree of present bias active for LHWs who make Immediate decisions when $\mathbf{1}_{d=1} = 1$. If $\beta_i = 1$, the vaccinator adheres to exponential discounting with discount factor δ_i, while if $\beta_i < 1$ the vaccinator exhibits a present bias, being less patient in Immediate relative to Advance decisions.

Third, we assume that the distribution of one period discount factors is normal in each condition

$$\begin{aligned} \delta_i &\sim N(\mu_\delta, \sigma_\delta^2) \quad \text{if} \quad d = 0 \\ \beta_i \delta_i &\sim N(\mu_{\beta\delta}, \sigma_{\beta\delta}^2) \quad \text{if} \quad d = 1. \end{aligned}$$

Our objective is to estimate the relevant distributional parameters of the population: $\mu_\delta, \sigma_\delta^2$ in the Advance condition, and $\mu_{\beta\delta}, \sigma_{\beta\delta}^2$ in the Immediate condition.

Fourth, we assume that LHWs minimize the discounted costs of effort subject to the intertemporal budget constraint provided by their bonus contract. This yields a marginal condition

$$R\gamma v_{1,i}^{\gamma-1} - \beta_i^{\mathbf{1}_{d=1}}\delta_i\gamma v_{2,i}^{\gamma-1} = 0. \tag{1}$$

Note that in the absence of shocks, equation 1 provides a deterministic mapping from allocation behavior to individual discount factors,

$$R * (\frac{v_{1,i}}{v_{2,i}})^{\gamma-1} = \beta_i^{\mathbf{1}_{d=1}}\delta_i, \tag{2}$$

which can be used to provide initial guidance on the extent of heterogeneity in choice.

Fifth, we assume vaccinators' behavior is not the deterministic function of their preferences provided in equations 1 and 2. Rather, their optimization is subject to an additive shock to relative marginal costs in the two periods, ϵ_i. We assume these relative marginal cost shocks to be normal and mean zero,

$$\epsilon_i \sim N(0, \sigma_\epsilon^2).$$

Minimizing discounted costs subject to the intertemporal budget constraint of the experiment and adding the relative marginal cost shock yields an adjusted marginal condition:

$$\gamma v_{1,i}^{\gamma-1} - \frac{\beta_i^{\mathbf{1}_{d=1}}\delta_i}{R}\gamma v_{2,i}^{\gamma-1} = \epsilon_i.^{13} \tag{3}$$

[13]This approach for structurally estimating time preferences by assuming a marginal condition is satisfied up to a random shock was introduced in controlled experiments by Andreoni and Sprenger (2012), and has precedents in a body of macroeconomic research identifying aggregate preferences from consumption data. See, for example, Shapiro (1984); Zeldes (1989); Lawrance (1991).

Under the above assumptions, the conditional likelihood of an allocation $(v_{1,i}, v_{2,i})$ given $\beta_i^{\mathbf{1}_{d=1}}\delta_i$ is

$$L(v_{1,i}, v_{2,i}|\delta_i) = \phi\left(\frac{\gamma v_{1,i}^{\gamma-1} - \frac{\delta_i}{R}\gamma v_{2,i}^{\gamma-1}}{\sigma_\epsilon}\right) \quad \text{if} \quad d = 0$$
$$L(v_{1,i}, v_{2,i}|\beta_i\delta_i) = \phi\left(\frac{\gamma v_{1,i}^{\gamma-1} - \frac{\beta_i\delta_i}{R}\gamma v_{2,i}^{\gamma-1}}{\sigma_\epsilon}\right) \quad \text{if} \quad d = 1$$

where $\phi(\cdot)$ is the density of the standard normal distribution. Integrating over the relevant distribution of preference parameters in Advance and Immediate conditions gives the likelihoods

$$L(v_{1,i}, v_{2,i}) = \int \phi\left(\frac{\gamma v_{1,i}^{\gamma-1} - \frac{\delta_i}{R}\gamma v_{2,i}^{\gamma-1}}{\sigma_\epsilon}\right) f(\delta_i)d\delta_i \quad \text{if} \quad d = 0 \tag{4}$$
$$L(v_{1,i}, v_{2,i}) = \int \phi\left(\frac{\gamma v_{1,i}^{\gamma-1} - \frac{\beta_i\delta_i}{R}\gamma v_{2,i}^{\gamma-1}}{\sigma_\epsilon}\right) g(\beta_i\delta_i)d\beta_i\delta_i \quad \text{if} \quad d = 1, \tag{5}$$

where $f(\cdot)$ and $g(\cdot)$ are the normal densities from which δ_i and $\beta_i\delta_i$ are drawn. We deploy the Method of Simulated Likelihood (MSL) with 1000 random Halton draws from the simulated distributions, $f(\cdot)$ and $g(\cdot)$, at each observation to estimate simulation analogs of equations (4) and (5). Appendix A.1 provides additional detail on the estimation methodology and robustness of corresponding results.

In our setting, vaccinators receive a bonus of 1000 rupees paid the day after the drive if they meet both targets v_1 and v_2.[14] Not completing allocated vaccinations creates a sizable penalty at any given point in time. This design choice was made to encourage LHWs to forecast that they will complete the required vaccinations and so allocate them according to their true preferences. Nonetheless, the total present value of vaccinations, $V = 300$, is ambitious. Even with substantial effort, LHWs may not meet their targets, and so may need to account for

[14]The choice of large bonuses (around 10 times daily wages) followed the design logic discussed in Augenblick et al. (2015), who implemented a $100 completion bonus.

the potential for failure when making their allocations. To incorporate potential failure, we introduce a sixth assumption: LHWs forecast completing v allocations with probability

$$p(v) = \frac{1}{1 + \alpha v}.$$

Forecasted non-completion, introduces a wedge in the LHW's marginal condition, as they must recognize the marginal impact of an allocation on failure and loss of the completion bonus. This, in turn, changes the estimation procedure in two ways: the completion probabilities, $p(v_{1,i})$ and $p(v_{2,i})$, are a component of the total likelihood of an observation, and the component of the likelihood associated with the marginal condition is adjusted for the non-completion loss of the bonus. In Appendix A.1 we present the extended methodology that incorporates probabilistic non-completion in estimation and provide additional empirical results. For completeness we present our distributional results with and without the incorporation of the completion parameter, α, and find little difference in estimated time preferences.

Our structural exercise links the distributions of behavior in each of our 8 experimental conditions of R and Advance vs Immediate Choice in Drive 1 to a heterogeneous distribution of discount factors and a distribution shocks. Absent heterogeneity in discount factors or shocks, behavior would be deterministic, and all LHWs in a given condition should provide identical allocations. Permitting shocks alone, the distributions of behavior would reflect a constant shock variance, but be displaced in a proportional way across conditions (e.g., from $\phi\left(\frac{\gamma v_{1,i}^{\gamma-1} - \frac{\delta}{R}\gamma v_{2,i}^{\gamma-1}}{\sigma_\epsilon}\right)$ to $\phi\left(\frac{\gamma v_{1,i}^{\gamma-1} - \frac{\delta}{R'}\gamma v_{2,i}^{\gamma-1}}{\sigma_\epsilon}\right)$ between R and R'). A substantial role for heterogeneous preferences would be recovered if this restriction provided a relatively poor fit to the data. Identifying heterogeneity in preferences in this way may be challenging if assumptions like a constant distribution for ϵ are inaccurate. Importantly, our project is predicated on using the resulting measures to tailor contracts and predict subsequent Drive 2 allocations. As such, if we mischaracterize behavior that should be attributable to shocks or other forces as evidence

of heterogeneous discounting, our exercise should do a notably poor job of tailoring incentives.

2.2.3 Deriving Individual Expected Discount Factors From Allocation Behavior

The methodology discussed to here yields estimated distributions of preference parameters and additive marginal cost shocks $N(\hat{\mu_\delta}, \hat{\sigma_\delta^2})$, $N(\hat{\mu_{\beta\delta}}, \hat{\sigma_{\beta\delta}^2})$, and $N(0, \hat{\sigma_\epsilon^2})$. These distributions in hand, we map back to expected values of discounting for each individual. We do this by simulating behavior (allocations of $v_{1,i}$ and $v_{2,i}$) using the estimated distributions – $N(\hat{\mu_\delta}, \hat{\sigma_\delta^2})$, $N(\hat{\mu_{\beta\delta}}, \hat{\sigma_{\beta\delta}^2})$, and $N(0, \hat{\sigma_\epsilon^2})$ – and then calculating the expected value of δ_i or $\beta_i\delta_i$ associated with a given allocation, $E[\delta_i|v_{1,i}, v_{2,i}, R]$ if $d = 0$ and $E[\beta_i\delta_i|v_{1,i}, v_{2,i}, R]$ if $d = 1$. Specifically, we take 1 million draws from the distribution of preference parameters and shocks separately for the Advance and Immediate Conditions. We then simulate the distribution of allocations at each value of $R \in \{0.9, 1, 1.1, 1.25\}$. We construct $E[\delta_i|v_{1,i}, v_{2,i}, R]$ if $d = 0$ and $E[\beta_i\delta_i|v_{1,i}, v_{2,i}, R]$ if $d = 1$ as the expected discount factor associated with simulated allocations within 2.5 tasks of the actual allocation's value of $v_{1,i}$. Such simulation-based methodology for calculating individual level parameters is analogous to the approach taken for discrete choice models after MSL estimation (See Train (2009) for examples).

The assumptions required for the calculation of expected discount factors are potentially quite restrictive. Our research design, which involves tailoring contracts to discounting, required commitment to specific functional forms for costs and discounting. Our empirical exercise estimates the distribution of discounting parameters accounting for shocks to relative marginal costs and the probability of non-completion. In sub-section 4.1, we assess the validity of our assumptions and present further exploratory analysis related to alternative functional forms.

2.3 Drive 2: Test of Structural, Tailored Contracts

In a second two-day drive, we investigate tailored contracts. All LHWs from the first drive were invited to participate in a second intertemporal bonus contract. LHWs were unaware that their

previously measured behavior would be used to potentially inform their subsequent contracts. This sidesteps an important possibility that LHWs might alter their first drive behavior in order to receive a more desirable contract in the second drive.

Half of LHWs received an individually-tailored intertemporal bonus contract,

$$w_{1,i} + R_i^* \cdot w_{2,i} = V,$$

where $w_{1,i}$ and $w_{2,i}$ are Drive 2 allocations. The value

$$R_i^* = \frac{R \cdot v_{1,i}}{v_{2,i}} \tag{6}$$

is their individually-tailored interest rate based upon their Drive 1 allocations. Absent shocks and with perfect completion, when $\gamma = 2$ in (1), $\frac{R \cdot v_{1,i}}{v_{2,i}} = \beta_i \delta_i$ or δ_i, depending on whether the LHW made an Immediate or Advance decision in Drive 1.[15] Hence, the tailored contract was pre-specified to be equal to the LHW's discount factor under deterministic choice and perfect completion. Absent shocks and probabilistic completion, setting the relative price, R_i^*, in this way should lead LHWs to allocate an equal number of vaccinations to each day of the drive, $w_{1,i} = w_{2,i}$. Accounting for shocks and probabilistic completion alters this prediction slightly, but the prediction of roughly equal allocations to each period is maintained. Though LHWs in this group receive different relative prices, the contract is designed for each of them to achieve the same objective of smoothing vaccinations through time. Some LHWs' allocation behavior in the first drive implied extreme values of R_i^*. Our tailoring exercise focused only on a Tailoring Sample of LHWs with values of R_i^* between 0.75 and 1.5.[16] LHWs outside of these bounds were given either the upper or lower bound accordingly.

[15]Note that this tailoring exercise requires that LHWs remain in either the Immediate or Advance assignment across drives.

[16]Of our sample of 338 LHWs, 57 exhibit $\frac{Rv_{1,i}}{v_{2,i}}$ outside of this range. The Tailoring Sample consists of the remaining 281 LHWs. One LHW, out of 281 does, not report vaccination activity via mobile phone in Drive 2.

The other half of LHWs were given a random intertemporal bonus contract,

$$w_{1,i} + \tilde{R}_i \cdot w_{2,i} = V,$$

where $\tilde{R}_i$ was drawn from a random uniform distribution $U[0.75, 1.5]$. The bounds on the distribution of $\tilde{R}_i$ were determined to match the bounds on R_i^*, while the choice of a random uniform control—rather than a single value of $\tilde{R}_i$ or some alternative distribution—was chosen to provide flexible scope for constructing a range of comparison policies by drawing subsets of LHWs assigned to the $\tilde{R}_i$ condition. Relevant subgroups that we draw from this group of LHWs are: 1) structural, broad: those with values of $\tilde{R}_i$ close to the average value of R_i^*; 2) atheoretic, broad: those with values of $\tilde{R}_i$ that are close to the optimal value for achieving $w_{1,i} = w_{2,i}$ implied by a reduced form exercise; 3) atheoretic, tailored: those with values of $\tilde{R}_i$ that are generally increasing in patience but not required to be linear as in the structurally tailored policy, $R_i^* = \frac{R \cdot v_{1,i}}{v_{2,i}}$. These comparisons span the policy space of being either atheoretic vs. structural and tailored vs. broad. Comparison is also provided for the full group of LHWs who received random bonus contracts.

Random assignment to structural tailoring in Drive 2 is stratified on the measure of absolute distance to equal allocations $|\frac{v_{1,i}}{v_{2,i}} - 1|$, based on allocations from Drive 1.[17] This measure of distance to equal provision also serves as our eventual outcome measure when analyzing the effect of assignment to structural tailoring in Drive 2. Stratifying assignment on key outcomes of interest is standard practice in the field experimental literature (Bruhn and McKenzie, 2009), as it generally increases precision in estimating treatment effects.

[17]Specifically, subjects are divided into terciles by this measure, with a roughly even number in each bin being assigned to the tailoring and to the control condition.

2.4 Design Details

Our experiment is divided into two drives. The first drive took place November 10-11, 2014 with training on November 7. The second drive took place December 8-9, 2014 with training on December 5. These drives are denoted Drive 1 and Drive 2 and are used for measuring preference, and tailoring contracts, respectively. We had actually anticipated beginning our field study in September, 2014. However, as noted below, a disruption to the mobile network prevented us from measuring the preferences for all LHWs in this drive. We call this failed drive Drive 0, and leverage the data that was collected for within-subjects panel analysis.

2.4.1 Training and Allocation Decisions

On November 7, all LHWs participating in Drive 1 received two hours of training at one of three locations in central Lahore on using the monitoring features of the smartphone application and the process by which allocations were made. Both Advance and Immediate LHWs were given identical training. At the end of the training, LHWs assigned to Advance decision were asked to select their allocations by using the page on their smartphone application. Assistance was available from training staff for those who required it. LHWs assigned to the Immediate condition were told they would select their allocations using their smartphone application on Monday morning before beginning work. A hotline number was provided if assistance was required for those in the Immediate condition. The training activities on December 5, for the December 8-9 drive were identical. However, because LHWs had previously been trained on the smartphone application, this portion of the training was conducted as a refresher.

2.4.2 Experimental Timeline

Drive 0, Failed Drive, September 26-30, 2014: We had hoped to begin our study on Friday, September 26th, 2014 with a training session. 336 LHWs had been recruited, were randomized into treatments, and trained. Advance allocation decisions were collected from half of the

subjects on Friday, September 26th. On Monday, September 29th, when we attempted to collect immediate allocation decisions, there was a disruption in the mobile network that prevented 82 of 168 Immediate decision LHWs from submitting their allocations. This caused us to abandon this drive for the purposes of measuring preferences for subsequent tailoring of contracts. The drive, however, was completed and intertemporal bonuses were paid. For the 82 individuals who did not make their allocations, we contacted them, allowed them to continue working, and paid bonuses for all.[18] We present data from Drive 0 in our robustness exercises, but do not use Drive 0 for the purposes of tailoring contracts. In addition to the 336 LHWs who experienced intertemporal bonus contracts, Drive 0 also had a separate a sample of 85 LHWs who received a phone but no bonus contract to study the differential effect of our incentives.

Drive 1, November 7-11, 2014: Of the original 336 bonus contract LHWs in our failed drive, 57 did not participate in the next drive organized for November 7 - 11. We recruited replacements with the help of the Department of Health, identifying a total of 349 LHWs to participate in the intertemporal bonus program. The entire sample was re-randomized into R and allocation timing conditions. We again included in our study a sample of 72 vaccinators in Drive 1 who received a phone but no financial bonus. Training was conducted on November 7, and Advance allocation decisions were collected. The drive began on November 10, and Immediate allocation decisions were collected. 174 LHWs were assigned to the Advance Choice condition and 175 were assigned to the Immediate Choice condition. Bonuses were paid on November 12. While all 174 LHWs in the Advance Choice condition provided an allocation decision, only 164 of 175 in the Immediate Choice condition provided an allocation, generating a usable sample of 338 allocations in Drive 1. For 232 LHWs, we have allocation decisions in both the failed drive, Drive 0, and Drive 1, forming a potentially valuable panel of response. In addition

[18] Appendix Table A.5 checks for balance by failure of the smartphone application in Drive 0. Only one of the eight comparison of means hypothesis tests reject equality at the 10 percent level.

to the 338 LHWs who experienced intertemporal bonus contracts and provided allocations, Drive 1 also had a separate a sample of 73 LHWs who received a phone but no bonus contract to study the differential effect of our incentives.

Drive 2, December 5-9, 2014: Of the 338 LHWs who participated in Drive 1 and provided an allocation, 337 again participated in Drive 2. These LHWs were randomly assigned to be structurally tailored or receive a random price in their Drive 2 bonus contracts. Importantly, LHWs retained their Advance or Immediate assignment, such that Drive 2 delivers a 2x2 design for structural tailoring and allocation timing. This allows us to investigate the effect of structural tailoring in general, and if the effects depend on whether present bias may be active.

2.4.3 Sample Details

Table 1 summarizes our sample of LHWs from Drive 1 and provides tests of experimental balance on observables. Columns (1) through (4) present the mean and standard deviation of a number of demographic characteristics for LHWs assigned to intertemporal bonus contracts in Drive 1. These values are separated by Advance and Immediate Choice and whether the LHW was subsequently assigned to be Tailored or Untailored in Drive 2. Column (5) presents the means and standard deviations for LHWs that participated in our study (including Drive 0), but did not receive incentives.[19] Column (6) presents a p-value corresponding to joint tests of equality. Our sample is almost exclusively female, more than 90 percent Punjabi in all treatment arms, and broadly without access to formal savings accounts. LHWs are generally highly experienced with an average of 10.5 years of health work experience and 10.4 years of polio work experience. Consistent with randomization, of the 8 tests performed on demographic balance, only the test performed on an indicator variable equal to one for female subjects suggests baseline imbalance.

[19] 13 LHWs were in the Phone Only group in Drive 0 and re-randomized into the incentive group in Drive 1. They are included in the Table 1, column (5).

Table 1: Drive 1 Summary Statistics and Covariate Balance

	Intertemporal Bonus Contracts				Phone Only	p-value
	Advance Choice		Immediate Choice			
	Tailored	Untailored	Tailored	Untailored		
	(1)	(2)	(3)	(4)	(5)	(6)
Demographics						
Gender (Female = 1)	0.989	1.000	0.976	0.978	0.958	0.089
	(0.011)	(0.000)	(0.017)	(0.015)	(0.024)	
Years of Education	10.713	10.459	10.232	10.207	10.528	0.613
	(0.300)	(0.214)	(0.200)	(0.245)	(0.290)	
Number of Children	3.593	3.229	3.438	3.451	3.470	0.803
	(0.212)	(0.202)	(0.191)	(0.195)	(0.198)	
Punjabi (=1)	0.954	0.952	0.938	0.944	0.957	0.984
	(0.023)	(0.023)	(0.027)	(0.025)	(0.024)	
Financial Background						
Has a Savings Account (=1)	0.247	0.321	0.268	0.242	0.243	0.773
	(0.047)	(0.051)	(0.049)	(0.045)	(0.052)	
Participated in a ROSCA (=1)	0.388	0.361	0.400	0.391	0.429	0.947
	(0.053)	(0.053)	(0.055)	(0.051)	(0.060)	
Health Work Experience						
Years in Health Department	10.826	10.648	10.256	10.319	11.197	0.742
	(0.535)	(0.509)	(0.556)	(0.555)	(0.546)	
Years as Polio Vaccinator	11.041	10.506	10.323	9.647	10.831	0.340
	(0.504)	(0.504)	(0.529)	(0.500)	(0.530)	
Vaccination Data						
Drive 1 Behavior: $\frac{R \cdot v_{1,i}}{v_{2,i}}$	1.039	1.043	1.045	1.024		0.975
	(0.024)	(0.029)	(0.036)	(0.036)		
Proportion: $0.75 \leq \frac{R \cdot v_{1,i}}{v_{2,i}} \leq 1.5$	0.876	0.859	0.783	0.706		0.020
	(0.035)	(0.038)	(0.045)	(0.047)		
Registered Vaccination Activity	0.809	0.824	0.916	0.815	0.833	0.148
	(0.042)	(0.042)	(0.031)	(0.041)	(0.044)	
Proportion of Targets Completed	0.797	0.811	0.682	0.731		0.119
	(0.040)	(0.038)	(0.047)	(0.042)		
Total Vaccinations Attempted	254.333	261.143	212.513	233.587	203.367	0.021
	(13.506)	(14.032)	(15.337)	(14.810)	(15.847)	
# Vaccinators	89	85	83	92	72	

Notes: Mean and standard error in parentheses from Drive 1 observations in columns (1) through (5). Statistical significance level of F-test for constant means in column (6). Some calculations use smaller sample than that reported in final row due to missing information. The proportion of subjects with missing demographic information for is never greater than 3.5 percent (8 Vaccinators did not report whether they had participated in a ROSCA). A ROSCA is an informal Rotating Savings and Credit Association. Some calculations used a smaller sample size due to missing information.

Table 1 also provides some additional information on vaccination activity in Drive 1 for each group. Average allocations are roughly equal in Advance Choice and Immediate Choice, but the Immediate condition exhibits greater variation in behavior. Correspondingly slightly fewer LHWs in the Immediate condition exhibit behavior within the bounds $0.75 \leq \frac{R \cdot v_{1,i}}{v_{2,i}} \leq 1.5$. Of 338 LHWs in our Drive 1 incentive conditions, 288 registered activity in their cell phone application during the drive, while 50 generated no data.[20] Table 1 demonstrates slightly more

[20]Appendix Table A.9 suggests that this data is unrelated to experimental variation in R, but that LHWs in

registered activity in the Immediate Choice condition. This may be due to the fact that LHWs in Immediate Choice made their allocations in the application on the morning the drive began and so could be more likely to remember to register their activity. Conditional on registering vaccination activity, LHWs in our incentive conditions completed around 75% of their target vaccination attempts, with somewhat greater completion rates in the Advance condition.[21] As noted in section 2.2.2, we account for forecasted non-completion in our empirical approach to estimating the distributions of discount factors and in subsection 4.1 we explore the determinants of target completion. Conditional on registering vaccination activity, LHWs in our incentive conditions attempted an average of around 235 vaccinations in Drive 1. LHWs assigned to the Phone Only condition without incentives completed only around 203 vaccinations. In subsection 4.4 we further explore the separate question of differential effects of incentives relative to monitoring alone for increasing vaccinations.

3 Results

Our project has two phases. The first phase measures intertemporal preferences. The second phase evaluates the effects of structural, tailored contracts. These results of these two phases are presented in the next two subsections. A third subsection provides additional analyses and robustness tests.

the Advance condition are slightly more likely to have not registered activity.

[21] Completion rates are calculated as $1/2(min(Completed_{1,i}/v_{1,i}, 1) + min(Completed_{2,i}/v_{2,i}, 1))$. Appendix Figure A.6 presents the histogram of average completion percentages across subjects, showing a bimodal distribution of success and failure. In Appendix Table A.9, we examine the determinants of completion with linear probability models and an indicator for completion, $Complete(= 1)$, as dependent variable. Within the Advance and Immediate groups we find no discernible relationship between R and completion. However, individuals assigned to Immediate choice are significantly less likely to satisfactorily complete their allocated vaccinations. This evidence may have a present-biased interpretation. If subjects in the Immediate condition postpone more work due to present bias, they may be subsequently unable to satisfactorily complete said work.

3.1 Drive 1: Individual Behavior and Distributions of Intertemporal Preferences

Each LHW in Drive 1 provides a single allocation, $(v_{1,i}, v_{2,i})$, at their randomly assigned value of R and Advance versus Immediate Choice assignment. To provide a comparable value of these allocations across conditions, Figure 2, Panel A plots the distribution of $\frac{Rv_{1,i}}{v_{2,i}}$ separately for the 174 LHWs in the Advance condition and 164 LHWs in the Immediate condition. Figure 2, Panel A also provides indicators for how our Drive 2 Tailoring Sample is constructed. In total 57 LHWs with values of $\frac{Rv_{1,i}}{v_{2,i}}$ outside of the region $[0.75, 1.5]$ are excluded from the Tailoring Sample, while the remaining 281 are the focus of our Drive 2 analysis.

Absent shocks to marginal costs and probabilistic non-completion, the quantity $\frac{Rv_{1,i}}{v_{2,i}}$ would be equivalent to each LHW's one period discount factor. Two features of Figure 2, Panel A warrant attention. First, the median value of $\frac{Rv_{1,i}}{v_{2,i}}$ in the Advance condition is 1.02, while the median in the immediate condition is 1. LHWs allocate slightly fewer vaccination attempts to day 1 of the drive when making immediate allocations, but this difference in medians is not statistically significant, Pearson's $\chi^2 = 1.18,\ (p = 0.28)$. Second, LHWs in both conditions exhibit substantial variation in allocation behavior. The 25th-75th percentiles of $\frac{Rv_{1,i}}{v_{2,i}}$ range from $[0.88 - 1.18]$ in the Advance condition, and from $[0.84 - 1.21]$ in the Immediate condition. While this wide variation is suggestive of heterogeneous time preferences, preferences are unlikely to be the sole source of heterogeneity. Given that in the absence of shocks $\frac{Rv_{1,i}}{v_{2,i}}$ identifies an individual discount factor, this would imply daily discount factors well beyond empirical rates of interest. A plausible additional driver of these individual differences is shocks to the marginal costs of vaccination.

The structural exercise developed in section 2.2.2 links the distributions of behavior presented in Figure 2, Panel A to distributions of time preferences accounting for both shocks and probabilistic completion. Table 2 provides the implementations of this methodology for subjects in Drive 1. Columns (1) through (3) of Table 2 presents the distributional discount-

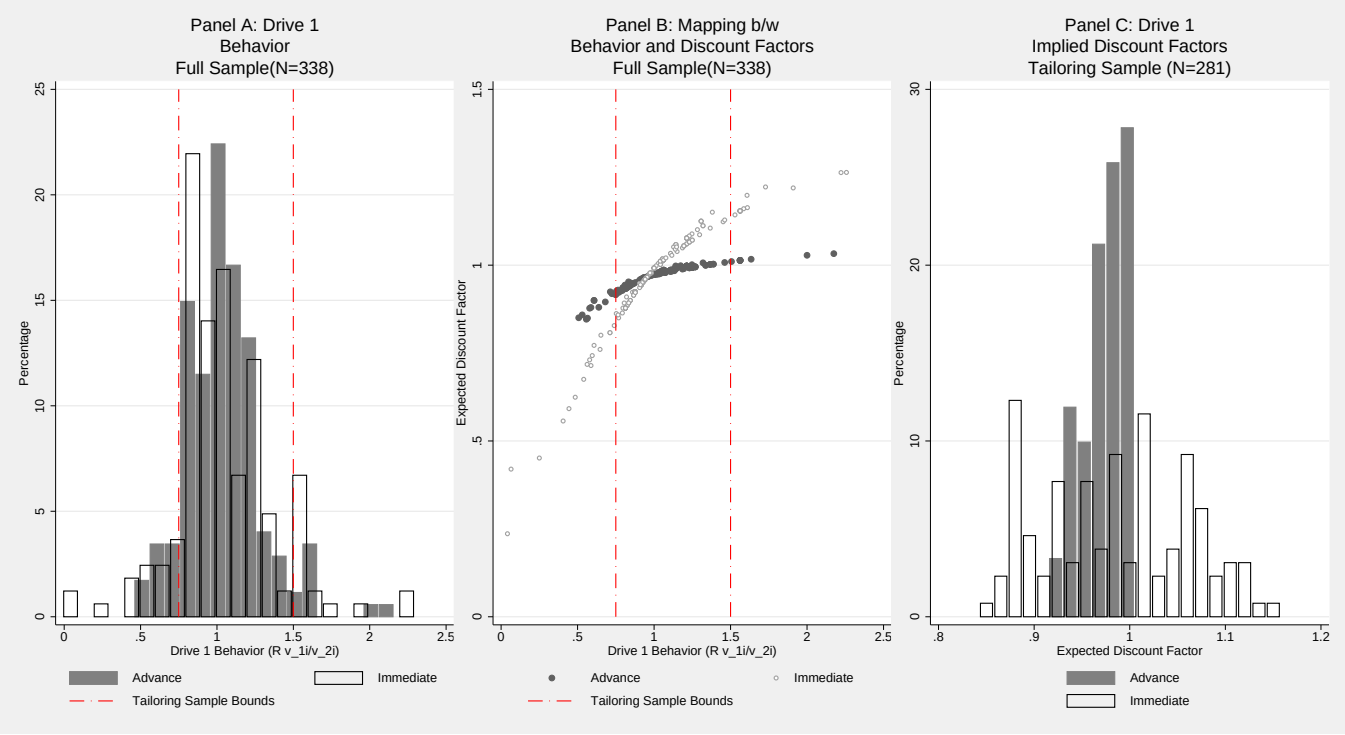

Figure 2: Individual Behavior and Expected Discount Factors

Notes: Panel A reports individual allocation behavior, $\frac{Rv_{1,i}}{v_{2,i}}$, vaccinators in Advance and Immediate conditions for Full Sample of 338 vaccinators. Dashed red lines correspond to the boundaries $\frac{Rv_{1,i}}{v_{2,i}} = 0.75$ and $\frac{Rv_{1,i}}{v_{2,i}} = 1.5$ which define the Tailoring Sample. Panel B presents the mapping between individual behavior and expected discount factors in Advance and Immediate conditions based on the structural models estimated in columns (3) and (6) of Table 2. Dashed red lines correspond to the boundaries $\frac{Rv_{1,i}}{v_{2,i}} = 0.75$ and $\frac{Rv_{1,i}}{v_{2,i}} = 1.5$ which define the Tailoring Sample. Panel C presents the expected discount factors in Advance and Immediate condition specifically for the Tailoring Sample of 281 vaccinators in Drive 1.

ing estimates for LHWs in the Advance Choice condition, $N(\mu_\delta, \sigma_\delta^2)$. In column (1), we set $ln(\sigma_\delta) = -10$ such that there is effectively zero heterogeneity in preferences, and estimate a homogeneous value $\mu_\delta = 0.954$ (0.016) and a substantial role for shocks to relative marginal costs, $ln(\sigma_\epsilon) = 4.155$ (0.054). In column (2), we permit heterogeneity in discount factors and estimate $\mu_\delta = 0.965$ (0.022) and $ln(\sigma_\delta) = -2.449$ (0.587), implying a standard deviation of δ of 0.09. This specification identifies a slightly smaller role for shocks to relative marginal costs. However, heterogeneous preferences provides only a minor improvement in the estimated likelihood relative to the homogeneous model, suggesting that many LHWs have values of δ_i close to the aggregate value. Columns (4) and (5) of Table 2 repeat this analysis for all subjects in the Immediate Choice condition. Absent heterogeneity we obtain $\mu_{\beta\delta} = 0.895$ (0.024) and $\sigma_\epsilon = 4.529$ (0.055). Incorporating heterogeneity, we estimate $\mu_{\beta\delta} = 0.959$ (0.029) and $\sigma_{\beta\delta} = -1.526$ (0.164), implying a standard deviation of $\beta_i\delta_i$ of 0.22, more than twice as large as that of δ_i alone. In the Immediate Choice condition, heterogeneous preferences provides a substantial improvement in the estimated likelihood relative to the homogeneous model.

The differential discounting estimates between the Advance and Immediate conditions are suggestive of limited present bias in discounting: $\mu_\delta/\mu_{\beta\delta} = 0.938$ and 0.992 without and with accounting for heterogeneous preferences, respectively. Moreover, the estimated distribution of preferences in the Immediate condition suggests a greater degree of heterogeneity, perhaps reflecting that it incorporates two dimensions of individual difference, both δ and β. Table 2 identifies the distributions of discount factors using only the between-subject variation in R and Advance vs. Immediate choice of Drive 1. In subsection 4.3, we reconduct the analysis making using of our failed Drive 0 data, and the corresponding within-subject variation of LHWs transitioning between conditions across drives. These estimates show a quantitatively larger degree of present bias, $\mu_\delta/\mu_{\beta\delta} \approx 0.90$ and continue to demonstrate substantial heterogeneity in discounting.

Of the 338 LHWs in Drive 1, 288 registered activity in their cell phone application during the

Table 2: Distributional Estimates

	Advance Choice			Immediate Choice		
	(1)	(2)	(3)	(4)	(5)	(6)
Discounting Parameters:						
μ_δ	0.954	0.966	0.966			
	(0.016)	(0.022)	(0.022)			
$ln(\sigma_\delta)$	-10	-2.449	-2.450			
	(-)	(0.587)	(0.582)			
$\mu_{\beta\delta}$				0.895	0.959	0.959
				(0.024)	(0.029)	(0.029)
$ln(\sigma_{\beta\delta})$				-10	-1.526	-1.527
				(-)	(0.164)	(0.163)
Completion Parameter:						
α			0.001			0.003
			(0.000)			(0.000)
Shock Parameter:						
$ln(\sigma_\epsilon)$	4.155	4.067	4.067	4.529	4.146	4.146
	(0.054)	(0.120)	(0.118)	(0.055)	(0.148)	(0.147)
# Vaccinators Allocation Obs.	174	174	174	164	164	164
# Cell Phone Completion Obs.	-	-	142	-	-	146
Log-Likelihood	-969.78	-969.40	-1057.98	-975.41	-967.05	-1068.03

Notes: Parameters from maximum simulated likelihood estimation for Drive 1 allocations and completion data where noted. Allocation data provided by Full Sample of 338 vaccinators. Estimates in column (1) assume $ln(\sigma_\delta) = -10$ such that there is zero heterogeneity in preferences. Completion data provided by 288 vaccinators. Standard errors in parentheses.

drive. The cellular network in Lahore is known to have some coverage gaps, and so we consider a subject to have successfully completed their work if they registered activity and completed an average of 90% or more of their required tasks on each day of the drive. By this measure, one-hundred seventy-four LHWs successfully completed their Drive 1 allocations. Columns (3) and (6) of Table 2 conduct estimates accounting for probabilistic completion using the completion data from the 288 subjects with registered cell-phone activity. Missing completion data is given a likelihood contribution of zero. Accounting for probabilistic completion has little influence on the estimated discounting distributions in both the Advanced and Immediate conditions, and we estimate a completion parameter of $\alpha = 0.001 - 0.003$. In Appendix A.1, we provide additional analyses related to probabilistic completion, demonstrating the robustness of these results to altered assumptions on the utility impact of forecasted non-completion and different sample pools.

The distributional estimates of Table 2, columns (3) and (6) facilitate the simulation exercise described in section 2.2.3 for calculating each LHW's expected discount factor: δ_i for LHWs in the Advance condition, and $\beta_i\delta_i$ for LHWs in the Immediate condition. For each value of R we simulate 1 million allocations under the assumed distributions for preferences and shocks accounting for probabilistic completion estimated in Table 2, columns (3) and (6). For each LHW, we assign a value of δ_i or $\beta_i\delta_i$ as the expected value of corresponding simulated discount factors that yield allocations within 2.5 vaccinations of the LHWs allocation, $(v_{1,i}, v_{2,i})$. That is, we calculate the posteriors $E[\delta_i|v_{1,i}, v_{2,i}, R]$ and $E[\beta_i\delta_i|v_{1,i}, v_{2,i}, R]$ using the simulated distributions of behavior. Figure 2, Panel B provides the mapping in Advance and Immediate conditions from behavior, $\frac{Rv_{1,i}}{v_{2,i}}$, to individual expected discount factor calculations. In both conditions, the approach deployed substantially shrinks the degree of individual differences. Extreme allocations are estimated to be due to shocks, not preferences, and so are mapped back to more reasonable expected discount factors. Given the greater heterogeneity in estimated preferences in Immediate Choice, the mapping for $E[\beta_i\delta_i|v_{1,i}, v_{2,i}, R]$ permits wider variation in

calculated expected discount factors. Panel B also provides the indicators for how our Drive 2 Tailoring Sample is constructed, eliminating those 57 LHWs with values of $\frac{Rv_{1,i}}{v_{2,i}}$ outside of the region $[0.75, 1.5]$ and discounting estimates outside of the corresponding bounds for $E[\delta_i|v_{1,i}, v_{2,i}, R]$ and $E[\beta_i\delta_i|v_{1,i}, v_{2,i}, R]$.

Figure 2, Panel C focuses on the 281 LHWs in the Tailoring Sample and provides the the obtained distributions of $E[\delta_i|v_{1,i}, v_{2,i}, R]$ and $E[\beta_i\delta_i|v_{1,i}, v_{2,i}, R]$. The median value of $E[\delta_i|v_{1,i}, v_{2,i}, R]$ in the Advance condition is 0.976, while the median value of $E[\beta_i\delta_i|v_{1,i}, v_{2,i}, R]$ in the Immediate condition is 0.991. The 25th-75th percentiles of expected discount factors range from $[0.960 - 0.991]$ in the Advance condition, and from $[0.925 - 1.055]$ in the Immediate condition, echoing the substantial heterogeneity in reduced form behavior. The LHWs in the Tailoring Sample are focus of our evaluation of structural tailored contracts in the next section.

3.2 Drive 2: Evaluating Structural, Tailored Contracts

Individual expected discount factors from Drive 1 in hand, we evaluate contracts tailored to individual discounting parameters. Of the 338 LHWs who participated in Drive 1, 281 provided allocations that were within our Tailoring Sample, $0.75 \leq \frac{Rv_{1,i}}{v_{2,i}} \leq 1.5$, and 280 participated in Drive 2.[22] Within this Tailoring Sample, 142 LHWs were assigned a value of $R_i^* = \frac{Rv_{1,i}}{v_{2,i}}$, which should induce approximately equal allocation of vaccinations through time, $w_{1,i} = w_{2,i}$. The remaining 138 LHWs provide the basis for the different comparison policies that we consider, and were assigned a uniform random price $\tilde{R}_i \in U[0.75, 1.5]$.[23]

Structurally tailored LHWs who received $R_i^* = \frac{Rv_{1,i}}{v_{2,i}}$ have an average values of $|\frac{w_{1,i}}{w_{2,i}} - 1| = 0.14$ (0.02), while those who received $\tilde{R}_i \in U[0.75, 1.5]$ have an average value of 0.61 (0.31),

[22]LHWs with allocations outside of these bounds were allowed to participate in Drive 2 and were either assigned $\tilde{R}_i \in U[0.75, 1.5]$ if they were in the untailored control group (31 subjects) or assigned $R_i = 0.75$ or $R_i = 1.5$ if they were in the tailored group and had $R_i^* < 0.75$ (15 subjects) or $R_i^* > 1.5$ (11 subjects). See subsection 4.2.2 for analysis of these observations.

[23]As noted in section 2.3, assignment to the tailored or the untailored group was conducted via stratified randomization with strata based upon the tercile of differences from equal provision of effort in Drive 1.

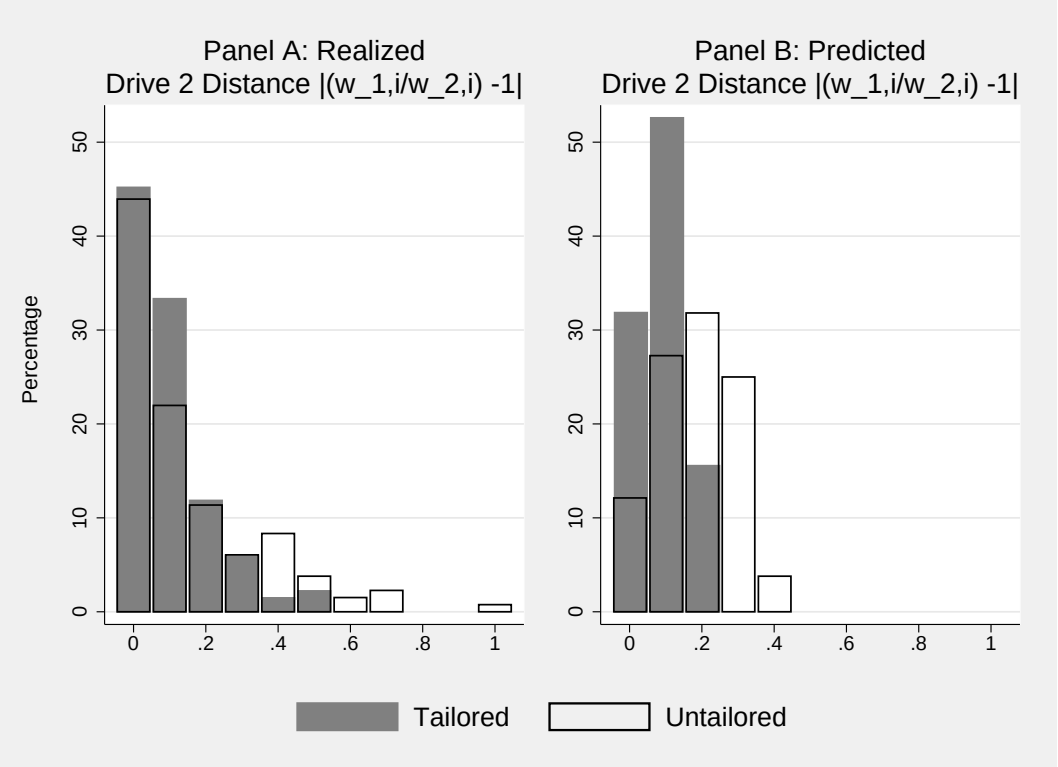

Figure 3: Realized and Predicted Distances

Notes: Panel A presents the realized distance to the policy target, $|\frac{w_{1,i}}{w_{2,i}} - 1|$ in Drive 2 for 267 Tailored and Untailored vaccinators in the Tailoring Sample. Thirteen of 280 Tailoring Sample vaccinators with extreme Drive 2 allocations are excluded. Panel B presents the predicted distances for these same vaccinators in the Tailored and Untailored conditions. Predictions generated by simulating allocations for each LHW associated with their Drive 2 assigned interest rate and new shock realizations.

$t_{278} = 1.53,\ (p = 0.13)$. The presence of a few extreme outliers skews the central measures of the data somewhat. Trimming the top and bottom 1% of the sample of Drive 2 allocations, mean distance for the tailored group is 0.10 (0.01), while the mean distance for the untailored control group is 0.15 (0.02), $t_{265} = 3.07,\ (p = 0.002)$.[24] We focus our analysis on this trimmed sample, but provide results corresponding to the full sample in Appendix Table A.6 and also report results using the same specifications but winsorizing the sample at the top and bottom percentile in Appendix Table A.7. Figure 3, Panel A presents the distributions of distances from equal allocation, $|\frac{w_{1,i}}{w_{2,i}} - 1|$ for the trimmed sample. Tailored LHWs provide systematically lower distance measures than untailored LHWs.

[24]This trimming eliminates 13 total observations, 7 from tailored group, and 6 from the untailored group.

Table 3 provides corresponding regression. We find that tailoring reduces distance to equal provision by around 5%-age points. Recall that LHWs assigned to Advance Choice in Drive 1 remain in Advance Choice in Drive 2, while those assigned to Immediate Choice remain in Immediate Choice. In the even columns of Table 3, we examine differential effects across these two groups. One might expect larger distance measures in Immediate conditions (and hence greater benefits to structural tailoring) if LHWs were present-biased. This is what is observed. Immediate Choice is associated with significantly larger distance measures and structural tailoring in Immediate Choice significantly reduces these distances. Structural tailoring in Immediate Choice reduces distance from equal provision by around one-half. Note that the effect size is similar to the effect of moving a comparison LHW from Advance to Immediate choice. That is, structural tailoring effectively eliminates present bias in allocations.

Figure 3, Panel B presents the corresponding distributions of expected allocations in the tailored treatment group and in the untailored controls. To calculate these distributions we simulate new allocations of $w_{1,i}$ and $w_{2,i}$ for each LHW at their Drive 2 assigned value of $\tilde{R}_i$ or R_i^* and new potential shock realizations. Specifically, we take the Drive 1 window of simulated allocations used to calculate each LHW's expected discount factor, and for every simulant in the window we re-simulate behavior with the new interest rate and a new shock realization. We then construct the average simulated allocation and the corresponding simulated average distance to equal allocations, $|\widehat{\frac{w_{1,i}}{w_{2,i}} - 1}|$. The distance to equal allocations should, indeed, be substantially higher for those individuals receiving a random value of $\tilde{R}_i \in U[0.75, 1.5]$ compared to those with structural tailored values, $R_i^* = \frac{Rv_{1,i}}{v_{2,i}}$

3.2.1 Alternative Counterfactuals for Structural Tailored Contracts

In Drive 2 of our study, the group receiving a uniform random interest rate, $\tilde{R}_i$, provides the counterfactual for evaluating the effect of our intervention. While this is one reasonable counterfactual to consider, a stricter evaluation of structural tailored interventions is possible.

We can draw subsets from our random control group to approximate counterfactuals that are atheoretic, broad, or both. Comparison of our treatment group to these various controls allows us to evaluate the source of any potential benefits of the structural, tailored policy relative to alternative policy approaches. Table 3 provides additional analysis considering the following three policy alternatives:

1. *Structural, Broad Policy:* This corresponds to a case where the policymaker knows workers' discount factors, but can offer only one interest rate to all workers. Absent shocks and probabilistic noncompletion, the quantity $\frac{Rv_{1,i}}{v_{2,i}}$ identifies individual discount factors. In the Tailoring Sample, the average value of $\frac{Rv_{1,i}}{v_{2,i}}$ for Drive 1 is 1.041 for the Advanced condition and 1.019 for the Immediate condition. These correspond to the broad values of $\tilde{R}$ required to approximately equalize allocations in a similar manner as our structural tailored policy, but without each individual getting a unique interest rate. To approximate this broad, structural policy we select the 54 individuals from the condition who received $\tilde{R}_i \in U[0.75, 1.5]$ within one standard deviation of these prices. Appendix Figure A.8, Panel A indicates the exact assignments for this subgroup. The relative prices implied by the aggregate model are structural, informed by preferences in Drive 1 in the same manner as our treatment group, but not tailored to each individual. Table 3, Columns (3) and (4) repeat the analysis of columns (1) and (2), but using this alternative control group. We find that our structural, tailored policy induces allocations that are directionally closer to equality than this structural, broad alternative, but the effect is not statistically significant.

2. *Atheoretic, Broad Policy:*

   To develop an atheoretic broad policy, we first estimate the reduced-form relationship between Drive 1 allocations and the experimentally varied parameters R and $d = 0$ or $d = 1$. Appendix Table A.8 provides regression coefficients indicating the sensitivity of the

allocation v_1 to R and whether the decision is immediate. The reduced-form relationship estimated for the Tailoring Sample in Drive 1 is $v_1 = 216.33 - 3.00 \times \mathbf{1}_{d=1} - 66.67 \times R$. In order to equate $w_1 = w_2$ under the constraint $w_1 + Rw_2 = 300$, one requires $(1 + R)w_1 - 300 = 0$. Substituting in for the reduced-form relationship, one obtains $f(R) = (1+R)(216.33 - 3.00 \times \mathbf{1}_{d=1} - 66.67 \times R) - 300 = 0$. Note that $f(R)$ is quadratic in R. In Advance Choice, it obtains the value of zero at $R = 1.05$ and $R = 1.19$. In Immediate Choice, $f(R)$ does not achieve the value zero, but has a maximum value of $f(R) = -6.01$ at $R = 1.10$. To approximate this broad atheoretic policy, we select the 54 individuals from the subsample who received $\tilde{R}_i \in U[0.75, 1.5]$ within one standard deviation of $R = 1.1$. Appendix Figure A.8, Panel B indicates the exact assignments for this subgroup. The relative prices implied in this case are atheoretic, informed only by the estimated reduced-form sensitivities, and not tailored to each individual. Table 3, Columns (5) and (6) repeat the analysis of columns (1) and (2), but using this alternative control group. We find that our structural, tailored policy induces allocations closer to equality than this atheoretic broad alternative.

3. *Atheoretic, Tailored Policy:* From the subsample who received $\tilde{R}_i \in U[0.75, 1.5]$, random assignment generates a match between the random price received, $\tilde{R}_i$, and Drive 1 allocation behavior. Even without structural guidance on the correct value of R to achieve equal allocations, random assignment will at times assign higher values of $\tilde{R}_i$ to individuals with higher values of $\frac{Rv_{1,i}}{v_{2,i}}$. These assignments give higher prices to more patient LHWs, but do not require that the relationship between prices and patience be linear as in the structural, tailored policy, which gives $R_i^* = \frac{Rv_{1,i}}{v_{2,i}}$. For each LHW who received a random price in Drive 2, we count the percent of LHWs who were more patient in Drive 1 but received a lower value of $\tilde{R}_i$. From the subsample who received $\tilde{R}_i \in U[0.75, 1.5]$, we select the 49 LHWs for whom this number is less than or equal to 10%, being effectively in order with at least 90% of the sample. Appendix Figure A.8, Panel C indicates the exact

assignments for this subgroup. The relative prices implied in this case are atheoretic — loosely related to patience, but not designed to achieve a specific objective beyond giving more patient LHWs higher prices — and tailored to each individual. Table 3, Columns (7) and (8) repeat the analysis of columns (1) and (2), but using this alternative control group. We find that our structural, tailored policy induces allocations closer to equality than this atheoretic, tailored alternative.

Table 3: The Effect of Tailoring Intertemporal Incentives

Dependent variable:	$\lvert\frac{w_{1,i}}{w_{2,i}} - 1\rvert$							
Policy Comparison Group	Random Price		Structural, Broad		Atheoretic, Broad		Atheoretic, Tailored	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Structural Tailored (=1)	-0.049***	-0.014	-0.034	-0.021	-0.051**	-0.010	-0.054*	-0.014
	(0.018)	(0.019)	(0.021)	(0.024)	(0.025)	(0.022)	(0.029)	(0.023)
Immediate Choice		0.117***		0.082*		0.137***		0.169***
		(0.035)		(0.044)		(0.051)		(0.062)
Structural Tailored x Immediate		-0.084**		-0.045		-0.102*		-0.132**
		(0.040)		(0.047)		(0.055)		(0.064)
Constant	0.022	-0.004	0.202***	0.185**	0.168**	0.143**	0.083	0.032
	(0.058)	(0.057)	(0.071)	(0.073)	(0.065)	(0.063)	(0.066)	(0.066)
Stratum FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exclude 99th and 1st Percentiles	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Drive 2 R_i^* or $\tilde{R}_i$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.082	0.154	0.076	0.122	0.114	0.194	0.085	0.193
Mean in Untailored Contract	0.153	0.153	0.129	0.129	0.145	0.145	0.148	0.148
Mean in Untailored Advance		0.098		0.103		0.088		0.091
Mean in Untailored Immediated		0.222		0.177		0.223		0.265
# Vaccinators	267	267	191	191	194	194	184	184
# Comparison Vaccinators	132	132	56	56	59	59	49	49

Notes: Table reports the effect of structural, tailored policy relative to alternatives on realized distance to the policy target, $|\frac{w_{1,i}}{w_{2,i}} - 1|$ in Drive 2 for 267 Tailored and Untailored vaccinators in the Tailoring Sample. Thirteen of 280 Tailoring Sample vaccinators with extreme Drive 2 allocations are excluded. Ordinary least squares regressions. Heteroskedasticity robust White standard errors reported in parentheses. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The analysis to this point indicates two key findings. First, heterogeneity in discounting is observed in both Advance and Immediate Choice, with substantially greater heterogeneity when present bias is implicated. This highlights the potential for policy interventions tailored

to individual preferences. Second, structural, tailored contracts work. Those LHWs given a tailored price equal to their previously measured expected discount factor provide smoother service than a set of alternatives that span the policy space of structural vs. atheoretic and broad vs. tailored. In the following section, we explore robustness and provide a set of additional examinations.

4 Robustness Tests and Additional Exercises

4.1 Evaluating Model Assumptions

As in any structural exercise, a number of assumptions are required to infer discounting parameters from LHW allocation behavior. Four assumptions are relevant for the present discussion, which we discuss below.

Stationarity of Deterministic Costs: We assume the deterministic portion of marginal costs is the same for day 1 and day 2 of each drive. If sooner costs are forecasted to be more severe than later costs, LHWs may appear disproportionately impatient, while if later costs are forecasted to be more severe, they may appear disproportionately patient. Further, if perceived costliness of vaccinations changes from Advance to Immediate choice, present bias is conflated with non-stationarity.

Importantly, our monitoring technology provides time-stamps and geo-stamps for vaccination activity. Time stamps are recorded for every vaccination attempt, while geo-stamps are collected approximately every 10 vaccination attempts. This may provide independent means for assessing the costliness of tasks from time use. For each LHW, we identify the median time lapse between vaccination attempts and the median distance covered per 30 minute window each day.[25] Of our 338 LHWs, measures for median time lapse between vaccination attempts

[25]We focus only on the distance traveled and time taken for vaccinations between 8 am and 6pm each day.

are available for 277 on either Day 1 or Day 2 and for 228 LHWs on both days of Drive 1.[26] Of our 338 LHWs, measures for median distance traveled every 15 minutes are available for 274 on either Day 1 or Day 2 and for 226 LHWs on both days of Drive 1.[27]

LHWs take around 3.4 minutes between vaccination attempts and walk around 0.06 miles per 15 minutes on Day 1. Focusing on individuals with measures on both days of the drive, we find that time taken and distance traveled are uncorrelated both with Advance choice and with allocation behavior within condition. Time and distance are also uncorrelated with Advance choice and allocation behavior on Day 2 of the drive. Further, differences in time taken or distance walked are statistically indistinguishable from zero, uncorrelated with allocation timing, and uncorrelated with allocation behavior within condition. These data indicate stability in required average effort per vaccination which is unrelated to assignment to Advance or Immediate choice, and that changes in efficacy are unrelated to measured preferences. This suggests that perceived changes in costs likely do not drive our measures of discounting.[28] These results are all presented in Appendix Table A.10.

The distribution of time taken and distance traveled carried some extreme outliers for some subjects. As such, we felt the median was an appropriate summary statistic. Though we had expected to receive geo-stamp data approximately every 10 vaccination attempts, when the monitoring data arrived we noted substantial variance in the number of vaccinations with common geo-stamps and sequences of geo-stamps which 'bounced' back and forth between geographic coordinates. In order to not overstate subject movements, we opted to take average coordinates within a 15 minute window and calculate direct-line distance between window-average coordinates as our measures of distance.

[26]265 LHWs have Day 1 lapse data while 240 have Day 2 lapse data. Of the 73 LHWs with missing Day 1 data, 68 completed either zero or one vaccination on Day 1 such that time lapse between vaccination attempts is not calculable. The remaining 5 conducted vaccinations but did not have phones that interacted with the server to report time use. Of the 98 LHWs with missing Day 2 data, 92 of them completed either zero or one vaccination on Day 2 and the remaining 6 did not have phones that interacted with the server to report time use. Those LHWs who completed vaccinations but did not have interaction with the server had their vaccination records pulled manually from their phones after the drive.

[27]257 LHWs have Day 1 distance data while 240 have Day 2 distance data. Of the 81 LHWs with missing Day 1 data, 75 completed four or fewer vaccination attempts on Day 1 such that distance traveled between 15 minute windows is not calculable. The remaining 6 conducted vaccinations but either did not have phones that interacted with the server to report location or had faulty Global Position Systems (GPS) in their phones. Of the 98 LHWs with missing Day 2 data, 96 of them completed four or fewer vaccination attempts on Day 2 and the remaining 2 did not have phones that interacted with the server to report location or had faulty GPS.

[28]Ultimately, such stationarity is likely to be expected given that LHWs are already well-versed in vaccination procedures, have an average of 10.5 years of experience as LHWs, and received a half day's training on the vaccination monitoring application.

Identical Cost Functions: Our calculation of expected discount factors assumes identical quadratic cost functions with additive cost shocks. Though these assumptions allow for straightforward estimation and calculation of time preferences, any violation would lead us to confound differences in patience across individuals or across Advance/Immediate timing with differences in costs. One natural view would be to assume that individuals do not discount at all, $\delta = 1$ and $\beta = 1$ and allocations are deterministic, such that allocations identify only the shape of the cost function. In this case, when $R = 1$, all LHWs, regardless of allocation timing, should exhibit $v_{1,i} = v_{2,i} = 150$ for all values of γ.[29] Examining the Drive 0 and Drive 1 data, we find that for 163 LHWs who were assigned $R = 1$, the mean allocation is $v_1 = 140.84$ ($s.d. = 24.76$).[30] Though the median allocation is indeed 150, responses range widely with 5th-95th percentiles of response being 103 to 160. If heterogeneity in costs alone were driving response, and discounting and shocks were not key features of the data, one would not expect to see this extent of variation in response when $R = 1$. Further, given random assignment to allocation timing, heterogeneity in costs alone does not easily rationalize the difference in discounting distributions between Advance and Immediate conditions.

Only Failure, No Shirking: Our structural exercise assumes individuals know their likelihood to succeed and should work only some minimal amount if their target is not attainable. Appendix Figure A.6 demonstrates the plausibility of this assumption with a bimodal pattern of almost complete success and almost complete failure. Another possibility is that subjects find an alternate way to renege on their contracts by shirking and still receiving pay. Not all vaccination attempts are equally challenging. In Appendix Figure A.7 we plot for each half-hour of Drive 1 the total number of attempted vaccinations along with the probability of successful vaccination and the probability that no child was reported as present. Reporting

[29] This is because the Euler equation reduces to $(\frac{v_{1,i}}{v_{2,i}})^{\gamma} = R = 1$, which implies $\frac{v_{1,i}}{v_{2,i}} = 1$.

[30] 42 of 163 LHWs allocated exactly $v_{1,i} = v_{2,i} = 150$.

that no child was present is likely to be less time consuming than a successful vaccination and easier to falsify. The vast majority of vaccination activity occurs before 3:00pm, there exists no sharp uptick in activity as days end, and we find evidence that LHWs' proportion of successful or failed vaccination attempts remains largely steady throughout the workday. This suggests that allocated vaccination attempts are conducted with due diligence.

No Biases in Choice: Our study assumes that the allocation environment itself induces no biases in choice such that LHW allocations are directly informative of preferences. A substantial literature in experimental economics suggests that aspects of the decision environment may deeply influence measures of preferences (for recent examples, see Harrison, Lau, Rutstrom and Sullivan, 2005; Beauchamp, Benjamin, Chabris and Laibson, 2015). One common view is that subjects are biased towards the middle of a choice set. In our environment, this could involve subjects opting for either equal allocations of $v_{1,i} = v_{2,i}$, or choosing an allocation in the middle of their budget constraint, $v_{1,i} = Rv_{2,i}$. Only 31 of 338 LHWs (9%) exhibit $v_{1,i} = v_{2,i}$ in Drive 1. Taking a less conservative measure of $v_{2,i} - 2.5 \leq v_{1,i} \leq v_{2,i} + 2.5$, we find that still only 58 of 338 LHWs (17%) are within 5 vaccinations of $v_{1,i} = v_{2,i}$.[31] Only 35 of 338 LHWs (10.3%) exhibit $v_{1,i} = Rv_{2,i}$. Taking a less conservative measure of $Rv_{2,i} - 2.5 \leq v_{1,i} \leq Rv_{2,i} + 2.5$, we find that 83 of 338 LHWs (25%) are within 5 vaccinations of $v_{1,i} = Rv_{2,i}$.[32] Taken together, this suggests that biases towards the middle of the budget constraint or towards equal allocation are unlikely to be driving substantial portions of allocation behavior.

4.2 Tailoring Robustness Tests

Our Drive 2 data show that LHWs who are given bonus contracts with an interest rate linked to their expected discount factors provide significantly smoother service than a number of

[31] As an even less conservative measure, 145 of 338 (43%) satisfy $v_{2,i} - 10 \leq v_{1,i} \leq v_{2,i} + 10$.
[32] As an even less conservative measure, 137 of 338 (40.5%) satsify $Rv_{2,i} - 10 \leq v_{1,i} \leq Rv_{2,i} + 10$.

policy comparison groups. Here we examine robustness of this result to alternative measures for smoothness in service provision and alternative measures for tailoring. We also provide an analysis of tailoring by completion.

4.2.1 Alternative Measures for Smooth Provision

Our analysis measures the distance to equal provision using the metric $|\frac{w_{1,i}}{w_{2,i}} - 1|$. In Table A.11, we reconduct the analysis of Table 3, using five alternate measures for smoothness. Panel A presents the Euclidean distance to the 45 degree line, $\frac{|w_{1,i}-w_{2,i}|}{\sqrt{2}}$. Panel B presents the Euclidean distance normalized by the total number of vaccinations allocated, $\frac{|w_{1,i}-w_{2,i}|}{\sqrt{2}(w_{1,i}+w_{2,i})}$. Panel C presents the number of sooner vaccinations that would need to be reallocated to reach the 45 degree line, $|w_{1,i} - \frac{300}{1+R}|$. Panel D presents probit regressions for needing to reallocate more than 10 vaccinations, $|w_{1,i} - \frac{300}{1+R}| > 10$. And finally, Panel E presents the value, $min[w_{1,i}, w_{2,i}]$. Across all specifications, the main conclusions are reproduced. However, the results with respect to additional structural tailoring benefits in Immediate Choice fall, at times, outside the range of statistical significance. These alternative measures of smooth provision indicate that our results are not an artifact of how one measures the outcome of interest.

4.2.2 Alternative Sample Restrictions and Treatment Measures

Our exercise focuses on LHWs with $\frac{Rv_{1,i}}{v_{2,i}}$ between 0.75 and 1.5. Of 338 LHWs in Drive 1, 280 satisfied this requirement and participated in Drive 2. Those LHWs who were assigned to be tailored, but exhibited behavior that fell outside of these bounds behavior were given either $R_i^* = 0.75$ or 1.5 depending on which boundary they crossed. For such individuals, structural tailoring is not a binary treatment, but rather a continuous difference between their expected discount factor and the exogenously given value of $R_i^* = 0.75$ or 1.5. Indeed, for all LHWs in the untailored group, treatment is also a continuous measure. In columns (1) and (2) of Table A.12, we reconduct the analysis of Table 3, columns (1) and (2) using as the measure of treatment

the absolute difference between each LHW's value of $\frac{Rv_{1,i}}{v_{2,i}}$ in Drive 1 and their assigned value of R in Drive 2, which we label *Structural Tailoring Intensity*. Structural Tailoring Intensity takes value zero for all tailored LHWs in the Tailoring Sample and is a positive number for all others. Hence, larger values of intensity should be associated with greater distances to equal provision. The main results of Table 3 are reproduced with in the Tailoring Sample; the closer the connection between patience and the assigned value of R, the smoother is provision. In columns (3) and (4) of Table A.12, we include those LHWs in the Boundary Sample. Including these observations does not alter the conclusions; however, it should be noted that treatment is no longer orthogonal to individual preferences as extremely patient and impatient LHWs will receive larger intensity measures on average. Using the indicator for 'Structurally Tailored' would not be an appropriate solution to this problem as tailored LHWs with extreme patience or impatience may actually receive relative prices that are further from their policy-optimal values than those in the untailored condition. In columns (5) and (6) of Table A.12, we include both the Boundary Sample and those LHWs with extreme Drive 2 allocations. As in our other analyses including outliers, we find similar directional effects but very wide confidence intervals.

4.2.3 Tailoring and Completion

Our analysis of probabilistic completion evaluates completion through the lens of a model and attempts to assess the trade-off between marginal completion probabilities and discounted marginal costs. Though this analysis seems both tractable and yields valuable predictive insights, an alternative interpretation for non-completion exists. If the *outcome* of failure rather than its probability is perfectly forecasted by the LHW, there is no incentive to respond truthfully. As such, the targets set in Drive 1, and our corresponding inference on time preferences, would be systematically inaccurate for individuals expecting to fail. In effect, successful LHWs are allocating according to equation (2), while unsuccessful LHWs are providing only noisy response. Under this assumption, we should be dramatically less able to predict allocation

behavior for LHWs who fail in Drive 1.

Table A.13 repeats the analysis of Table 3, columns (1) and (2) separately for LHWs who completed and failed to complete their Drive 1 targets. Similar magnitude effects are observed for both sets of LHWs, with structural tailoring serving to reduce distance from the equal provision by around one third. Focusing only on the completing subjects, we would reach effectively the same conclusion as our initial analysis.

4.3 Repeated Measurement and Estimation of Present Bias

In Drive 1, when relying on between-subject differences in behavior, aggregate presents bias appears limited. Given the wide heterogeneity in behavior in both Advance and Immediate Choice, one may fail to identify present bias due to sampling variation even if it exists. Indeed, most studies of present bias and dynamic inconsistency are conducted as within-subject exercises with more choices, potentially because of such wide heterogeneity.

Fortunately, our failed Drive 0 and the corresponding re-randomization in Drive 1 allows us to identify present bias using both more data and within-subject variation for LHWs who changed from Advance to Immediate choice (or vice versa) across drives. Appendix Table A.3, re-estimates the specifications of Table 2, columns (2) and (5) using both Drive 0 and Drive 1 behavior, drawing from 622 choices made by 390 LHWs in either Drive 0 or Drive 1. We find mean estimates $\mu_\delta = 0.970$ (0.019) and $\mu_{\beta\delta} = 0.889$ (0.027), indicating a present bias of $\beta = 0.916$. When focusing only within subject variation by estimating on the 126 LHWs who transition from Advance to Immediate choice across drives, we estimate $\mu_\delta = 0.982$ (0.029) and $\mu_{\beta\delta} = 0.886$ (0.039), indicating an average present bias of $\beta = 0.902$. These values are closely consistent with recent laboratory work eliciting time preferences over effort (Augenblick et al., 2015; Augenblick and Rabin, 2015). Figure 4, plots expected present bias for the panel of 125 of 126 LHWs who transition from Advance to Immediate choice across

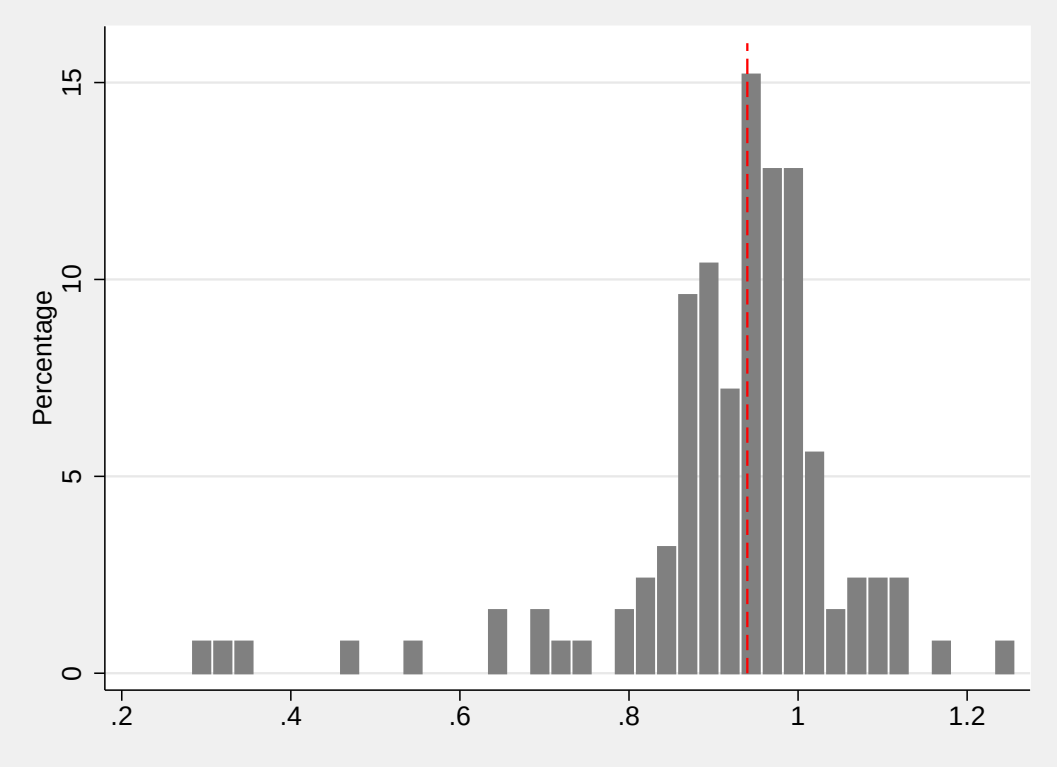

Figure 4: Within-Vaccinator Present Bias

Notes: Figure presents within-vaccinator measures of present bias for 125 vaccinators who transitioned from Advance to Immediate conditions across Drive 0 and 1. Measure of present bias calculated as the ratio of conditional expected discount factors given behavior in the two drives, $E[\beta_i\delta_i|\cdot]/E[\delta_i|\cdot]$. Median value presented as dashed red line.

drives vice versa.[33] We find a median estimate of present bias of $E[\beta_i\delta_i|\cdot]/E[\delta_i|\cdot] = 0.943$, and a mean estimate of 0.919 ($s.d. = 0.143$). As in laboratory work identifying present bias within-subject from longitudinal designs (Augenblick et al., 2015; Augenblick and Rabin, 2015), these individual estimates of present bias can be provided only for the selection of LHWs who provided allocations in both drives. Of the 254 LHWs who successfully provided an allocation in Drive 0, 232 provide an allocation in Drive 1. This level of attrition over time compares favorably to that of (Augenblick et al., 2015; Augenblick and Rabin, 2015).

[33]One LHW provided an extreme observation in one of the drives for which our simulation exercise yielded zero simulants in the relevant window of allocations.

4.4 Monitoring Without Incentives

We include in our study a sample of 85 LHWs in Drive 0 and 72 LHWs in Drive 1 who received a phone but no financial bonus. Importantly, we can measure the number of vaccination attempts for LHWs who were and were not assigned to received an additional financial incentive. Of the 157 LHWs who received a smartphone monitoring device, but no financial bonus in Drives 0 or 1, 134 register any vaccination information. We combine these 134 observations with 573 observations from Drives 0 and 1 from our incentive treatments who register vaccination information. Table A.4 reports the impact of providing financial incentives and monitoring relative to monitoring alone. Without incentives, LHWs register 182 vaccination attempts over each two day drive. With incentives, this quantity grows to 218 in Drives 0 and 1. This large increase in total vaccinations reflects the impact of the substantial completion bonus provided in our incentive treatments. Additionally Table A.4 examines whether within Drive 2, being assigned to the tailored or untailored group influences total recorded vaccinations. Of the 267 LHWs noted in Table 3, 225 registered vaccination information in Drive 2. For these LHWs tailoring is associated with an insignificant reduction of about 5% in the total number of vaccinations recorded. As our tailoring policy was not designed to increase total vaccinations, but rather to equalize vaccinations over time, such limited effects should be expected. Hence, incentives increase vaccination attempts above monitoring alone, while tailoring for equalization of vaccinations provides no further increase.

5 Conclusion

This paper examines the potential for policy interventions to be tailored to individual time preferences. We couch this question in an effort to customize contracts for 337 vaccination workers who spend two days each month attempting to deliver polio vaccines in the neighborhoods of Lahore, Pakistan.

We monitor workers' efforts using a smartphone application developed especially for our project, and elicit preferences using a Convex Time Budget design (Andreoni and Sprenger, 2012; Augenblick et al., 2015). Workers in an Advance condition allocate vaccinations over a two day drive prior to the beginning of the drive, while workers in an Immediate condition state their allocations at the beginning of the first day. Each worker also faces a randomized relative price for converting vaccinations across days. Worker behavior in this drive is used to identify individual time preferences. In a subsequent drive, we tailor contract terms to individual time preferences for half of the workers. This is done by choosing a relative price designed to encourage equal provision of effort over the two days of the drive. The other half of workers is given a random uniform price. We contrast our structural, tailored policy with three alternatives drawn from the random uniform condition that span the policy space in two dimensions: atheoretic vs. structural and broad vs. tailored.

Our findings are encouraging. Those workers who receive structural, tailored contract terms are substantially closer to the policy objective than the alternate policies considered. Using individual discounting parameter estimates to form a new incentive contract does indeed have the predicted effect on allocation behavior. To date, little research makes use of such predictive value of discounting estimates. Our results show not only that estimates are predictive, but also that useful parameter estimates are identifiable from a very limited number of experimental choices. This suggests that the substantial effort of articulating and estimating models in this domain has been well-invested. Policymakers should be encouraged by these findings to consider such tailored interventions. In the domain of intertemporal choice, the specific intervention we consider may be of interest for policymakers wishing to achieve smoothness in allocation behavior or consumption over time.

This paper also speaks to a recent discussion on the external validity of randomized control trials. Developing structural models through which to interpret experimental treatment effects potentially provides a means for generalizing results to other settings (Acemoglu, 2010;

Banerjee, Chassang and Snowberg, 2016).[34] In our setting, translating from our reduced form experimental treatment effects to a structural model of choice requires a set of potentially strong (and implausible) assumptions.[35] Nonetheless, the findings of predictive validity in this case suggests there is indeed potential for using structure as a means of increasing the external validity of results obtained from a single sample.

Separately, our results link to the growing literature on the personnel economics of the state (Ashraf, Bandiera and Lee, 2015; Bertrand, Burgess, Chawla and Xu, 2016; Finan, Olken and Pande, Forthcoming; Dal Bó, Finan and Rossi, 2013; Deseranno, 2016; Callen et al., 2019). Within this literature, there is interest in understanding whether heterogeneity in competencies and in motivation of state actors is linked to meaningful differences in state performance or service provsion (Ashraf et al., 2015; Dal Bó et al., 2013; Deseranno, 2016; Callen et al., 2019). We take the additional step of asking not only whether this heterogeneity matters for outcomes, but also whether it can be acknowledged and reflected in the design of individual incentives.

There are a number of clear limitations to our study which should be addressed by future research. First, our study sidesteps the critical issue of incentive compatibility by not informing subjects of the possibility that their initial behavior would potentially be subsequently used to inform their own contract terms. The mechanism design problem of eliciting preferences and tailoring on said preferences with complete information will be critical if one wishes to implement such contracts repeatedly in the field. Second, future research should seek to gain more precise estimates of preferences. Our exercise requires restrictive assumptions that could be relaxed in the presence of more data. If our results point to a lower bound in the promise of structural, tailored contracts, it is important to know how much more can be achieved. Third, alternative policy objectives and contract types should be investigated to ensure robustness of the identified predictive validity. Our findings have natural extensions to piece rate con-

[34]Attanasio and Meghir (2012), Duflo, Hanna and Ryan (2012), and Duflo, Greenstone, Pande and Ryan (2016) provide examples in development of using experiments to estimate key policy parameters.

[35]Banerjee et al. (2016) discuss how the plausibility of such identifying assumptions might limit external validity.

tracts, multi-period settings, and alternative policy targets that are worthy of study. Notable contributions in this vein include the recent work of Bai, Handel, Miguel and Rao (2019) and Aggarwal, Dizon-Ross and Zucker (2019).

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A Appendix

A.1 Empirical Methodology

We make a number of structural assumptions in order to map from LHW allocations to discounting parameter distributions:

- First, we assume a stationary cost of effort function $c(v) = v^\gamma$, where v represents vaccinations performed on a given day, and $\gamma > 1$ captures convex costs of effort. In our pre-specified analysis plan, we posited γ to be constant across individuals and our tailoring exercise was conducted under the assumption of $\gamma = 2$, quadratic costs.

- Second, we assume that individuals discount the future quasi-hyperbolically such that for a given LHW, i, making allocation $(v_{1,i}, v_{2,i})$ the discounted disutility of effort can be written as

$$v_{1,i}^\gamma + \beta_i^{\mathbf{1}_{d=1}} \delta_i \cdot v_{2,i}^\gamma.$$

  The indicator $\mathbf{1}_{d=1}$ captures whether the decision is made in advance or immediately on day 1.

- Third, we assume that the distribution of one period discount factors is normal in each condition

$$\delta_i \sim N(\mu_\delta, \sigma_\delta^2) \quad \text{if} \quad d = 0$$
$$\beta_i \delta_i \sim N(\mu_{\beta\delta}, \sigma_{\beta\delta}^2) \quad \text{if} \quad d = 1.$$

- Fourth, we assume that LHWs minimize the discounted costs of effort subject to the intertemporal budget constraint provided by their bonus contract.

- Fifth, we assume vaccinators' behavior is not the deterministic function of their preferences provided in equations 1 and 2. Rather, their optimization is subject to an additive individual random shock to marginal costs, ϵ_i. We assume these random cost shocks to be normal and mean zero,

$$\epsilon_i \sim N(0, \sigma_\epsilon^2).$$

Minimizing discounted costs subject to the intertemporal budget constraint of the experiment and marginal cost shock yields a shock-adjusted marginal condition:

$$\gamma v_{1,i}^{\gamma-1} - \frac{\beta_i^{\mathbf{1}_{d=1}}\delta_i}{R}\gamma v_{2,i}^{\gamma-1} = \epsilon_i.$$

- Sixth, we assume that despite our large bonus payments LHWs may not forecast completing all of their target vaccinations. Consider a LHW with probability $p(v_{1,i}, v_{2,i})$ of successfully completing her allocated targets. Hence, the expected disutility of effort is

$$p(v_{1,i}, v_{2,i})[v_{1,i}^{\gamma} + \beta_i^{\mathbf{1}_{d=1}}\delta_i \cdot v_{2,i}^{\gamma}] + (1 - p(v_{1,i}, v_{2,i}))[{v_{1,i}^n}^{\gamma} + \beta_i^{\mathbf{1}_{d=1}}\delta_i \cdot {v_{2,i}^n}^{\gamma}],$$

where $(v_{1,i}^n, v_{2,i}^n)$ are expected work to be completed on days one and two when not able to complete the contract. Similarly, the expected bonus utility is

$$p(v_{1,i}, v_{2,i})\delta^2 u(1000) + (1 - p(v_{1,i}, v_{2,i}))\delta^2 u(0),$$

for a 1000 rupee bonus paid following the second day of the drive. For simplicity, we normalize the net utility under non-completion, $\delta_i^2 u(0) - {v_{1,i}^n}^{\gamma} - \beta_i^{\mathbf{1}_{d=1}}\delta_i \cdot {v_{2,i}^n}^{\gamma}$, to be zero (e.g., no work and no additional earnings). Under this assumption, allocations are delivered by

the constrained optimization problem

$$max_{v_{1,i},v_{2,i}} p(v_{1,i},v_{2,i})[\delta_i^2 u(1000) - v_{1,i}^{\gamma} - \beta_i^{\mathbf{1}_{d=1}}\delta_i \cdot v_{2,i}^{\gamma}]$$
$$s.t.\ v_{1,i} + Rv_{2,i} = V.$$

The corresponding shock-adjusted marginal condition,

$$\gamma v_{1,i}^{\gamma-1} - \frac{\beta_i^{\mathbf{1}_{d=1}}\delta_i}{R}\gamma v_{2,i}^{\gamma-1} - \left(\frac{\frac{\partial p(v_{1,i},v_{2,i})}{\partial v_{1,i}} - \frac{1}{R}\frac{\partial p(v_{1,i},v_{2,i})}{\partial v_{2,i}}}{p(v_{1,i},v_{2,i})}\right)[\delta_i^2 u(1000) - v_{1,i}^{\gamma} - \beta_i^{\mathbf{1}_{d=1}}\delta_i \cdot v_{2,i}^{\gamma}] = \epsilon_i$$

highlights a central tradeoff between discounted marginal costs and marginal completion probabilities. If the probability of success is independent of choice, $\frac{\partial p(v_{1,i},v_{2,i})}{\partial v_{1,i}}, \frac{\partial p(v_{1,i},v_{2,i})}{\partial v_{2,i}} = 0$, the likelihood formulation without adjusting for completion provided in the main text is maintained. Otherwise, probabilistic completion can create a wedge, influencing choice and estimates.

The challenge created by probabilistic completion can be overcome with additional assumptions of functional form and internal consistency. Provided a functional form for $p(v_{1,i},v_{2,i})$, we assume LHWs know the correct mapping,

$$p(v_{1,i},v_{2,i}) = p^*(v_{1,i},v_{2,i}),$$

where $p^*(v_{1,i},v_{2,i})$ is the true completion probability induced by a given allocation $(v_{1,i},v_{2,i})$. The researcher observes either success or failure as draws from the distribution $p^*(v_{1,i},v_{2,i})$.[36] To provide a functional form for $p(v_{1,i},v_{2,i})$, we assume that the

[36]Hence, the function $p(v_{1,i},v_{2,i})$, known to the LHW, can be recovered from choice and observed success. It is as if $p(v_{1,i},v_{2,i})$ represents the physical possibility of achieving a given allocation. Given that we assume all LHWs know this mapping, we assume away failures of rational expectations such as believing one can achieve with higher probability than the truth. Intuitively, as in DellaVigna and Malmendier (2006) such misguided beliefs about efficacy would carry quite similar predictions to those of present-biased preferences.

probability of completing a target of v on day 1 or 2 is

$$p_1(v) = p_2(v) = \frac{1}{1+\alpha v},$$

with α homogeneous in the population. Provided $\alpha > 0$, this completion function assumes that success is assured at $v = 0$ and diminishes as v increases. As such $p(v_{1,i}, v_{2,i}) = \frac{1}{1+\alpha v_{1,i}} \frac{1}{1+\alpha v_{2,i}}$.

Under this form of probabilistic completion two conditions obtain:

$$\gamma v_{1,i}^{\gamma-1} - \frac{\beta_i^{\mathbf{1}_{d=1}}\delta_i}{R}\gamma v_{2,i}^{\gamma-1} - \left(\frac{-\alpha}{(1+\alpha v_{1,i})} - \frac{1}{R}\frac{-\alpha}{(1+\alpha v_{2,i})}\right)[\delta_i^2 u(1000) - v_{1,i}^{\gamma} - \beta_i^{\mathbf{1}_{d=1}}\delta_i \cdot v_2^{\gamma}] = \epsilon_i,$$

$$\frac{1}{1+\alpha v_{1,i}}\frac{1}{1+\alpha v_{2,i}} = \mathbf{1}_{p^*(v_{1,i},v_{2,i})},$$

where $\mathbf{1}_{p^*(v_{1,i},v_{2,i})}$ is an indicator for whether the LHW completed their targets, a draw from the correct mapping.

In effect, imposing internal consistency on completion rates allows the researcher to quantify the wedge induced by considering marginal completion probabilities. It is important to note that without quality data on actual completion, the exercise would be effectively impossible; highlighting the value of our implemented monitoring technology. Naturally, the estimates may be sensitive to the imposed functional form of $p(v_{1,i}, v_{2,i})$. In Appendix Table A.1, we reconduct the analysis of Table 2, columns (3) and (6) with an alternate functional form, $p(v_1, v_2) = \frac{1}{1+\alpha'(v_1^2+v_2^2)}$. This functional form carries the property that success probabilities are declining with the volume of work as long as $\alpha' > 0$. Very limited differences are observed in the estimates of discounting across this functional forms and the one used in the main text.

An additional issue generated by probabilistic completion is the presence of monetary utility, $u(1000)$. This value partially pins down the magnitude of the wedge created by

marginal completion probabilities. Indeed the net utility of completion, $[\delta_i^2 u(1000) - v_{1,i}^{\gamma} - \beta_i^{\mathbf{1}_{d=1}} \delta_i \cdot v_{2,i}^{\gamma}]$, can be set to any number with suitable definition of $u(1000)$. Of course, for allocations to carry any information, an obvious participation constraint needs to be satisfied,

$$[\delta_i^2 u(1000) - v_{1,i}^{\gamma} - \beta_i^{\mathbf{1}_{d=1}} \delta_i \cdot v_{2,i}^{\gamma}] \geq \delta_i^2 u(0) - {v_{1,i}^n}^{\gamma} - \beta_i^{\mathbf{1}_{d=1}} \delta_i \cdot {v_{2,i}^n}^{\gamma} = 0.$$[37]

To understand how slack this constraint was, we asked our LHWs survey questions attempting to identify the minimum bonus they would require to participate in the program again. Of 330 respondents, 329 said they would participate again for the same 1000 rupees bonus while only 42 said they would participate again if the bonus were 900 rupees. Of course, such responses can be difficult to interpret given a lack of incentives, but one view is that the value $[\delta_i^2 u(1000) - v_{1,i}^{\gamma} - \beta_i^{\mathbf{1}_{d=1}} \delta_i \cdot v_{2,i}^{\gamma}]$ may be only slightly higher than the normalized non-participation value of zero. When assessing probabilistic completion in the main text we set $[\delta_i^2 u(1000) - v_{1,i}^{\gamma} - \beta_i^{\mathbf{1}_{d=1}} \delta_i \cdot v_{2,i}^{\gamma}] = 100$. In Appendix Table A.2, we reconduct the analysis of Table 2, columns (3) and (6) assuming this net utility equal to 1000 or to 10000. Only small changes in the estimates are observed.

Under the above assumptions, the conditional likelihood of an allocation $(v_{1,i}, v_{2,i})$ and

[37] Otherwise the LHW would want to set $v_{1,i}, v_{2,i}$ to increase the probability of non-completion.

completion, $c_i \in \{0,1\}$ given $\beta_i^{\mathbf{1}_{d=1}}\delta_i$ and α is

$$L(v_{1,i}, v_{2,i}, c_i|\delta_i, \alpha) = \quad (\tfrac{1}{1+\alpha v_{1,i}} \tfrac{1}{1+\alpha v_{2,i}})\phi\left(\frac{\gamma v_{1,i}^{\gamma-1} - \frac{\delta_i}{R}\gamma v_{2,i}^{\gamma-1} - \left(\frac{-\alpha}{(1+\alpha v_{1,i})} - \frac{1}{R}\frac{-\alpha}{(1+\alpha v_{2,i})}\right)[100]}{\sigma_\epsilon}\right) \quad \text{if } d=0,\ c_i=1,$$

$$L(v_{1,i}, v_{2,i}, c_i|\delta_i, \alpha) = \quad (1 - \tfrac{1}{1+\alpha v_{1,i}} \tfrac{1}{1+\alpha v_{2,i}})\phi\left(\frac{\gamma v_{1,i}^{\gamma-1} - \frac{\delta_i}{R}\gamma v_{2,i}^{\gamma-1} - \left(\frac{-\alpha}{(1+\alpha v_{1,i})} - \frac{1}{R}\frac{-\alpha}{(1+\alpha v_{2,i})}\right)[100]}{\sigma_\epsilon}\right) \quad \text{if } d=0,\ c_i=0,$$

$$L(v_{1,i}, v_{2,i}, c_i|\delta_i, \alpha) = \quad \phi\left(\frac{\gamma v_{1,i}^{\gamma-1} - \frac{\delta_i}{R}\gamma v_{2,i}^{\gamma-1} - \left(\frac{-\alpha}{(1+\alpha v_{1,i})} - \frac{1}{R}\frac{-\alpha}{(1+\alpha v_{2,i})}\right)[100]}{\sigma_\epsilon}\right) \quad \text{if } d=0,\ c_i=.,$$

$$L(v_{1,i}, v_{2,i}, c_i|\beta\delta_i, \alpha) = \quad (\tfrac{1}{1+\alpha v_{1,i}} \tfrac{1}{1+\alpha v_{2,i}})\phi\left(\frac{\gamma v_{1,i}^{\gamma-1} - \frac{\beta\delta_i}{R}\gamma v_{2,i}^{\gamma-1} - \left(\frac{-\alpha}{(1+\alpha v_{1,i})} - \frac{1}{R}\frac{-\alpha}{(1+\alpha v_{2,i})}\right)[100]}{\sigma_\epsilon}\right) \quad \text{if } d=1,\ c_i=1,$$

$$L(v_{1,i}, v_{2,i}, c_i|\beta\delta_i, \alpha) = \quad (1 - \tfrac{1}{1+\alpha v_{1,i}} \tfrac{1}{1+\alpha v_{2,i}})\phi\left(\frac{\gamma v_{1,i}^{\gamma-1} - \frac{\beta\delta_i}{R}\gamma v_{2,i}^{\gamma-1} - \left(\frac{-\alpha}{(1+\alpha v_{1,i})} - \frac{1}{R}\frac{-\alpha}{(1+\alpha v_{2,i})}\right)[100]}{\sigma_\epsilon}\right) \quad \text{if } d=1,\ c_i=0,$$

$$L(v_{1,i}, v_{2,i}, c_i|\beta\delta_i, \alpha) = \quad \phi\left(\frac{\gamma v_{1,i}^{\gamma-1} - \frac{\beta\delta_i}{R}\gamma v_{2,i}^{\gamma-1} - \left(\frac{-\alpha}{(1+\alpha v_{1,i})} - \frac{1}{R}\frac{-\alpha}{(1+\alpha v_{2,i})}\right)[100]}{\sigma_\epsilon}\right) \quad \text{if } d=1,\ c_i=.,$$

where $\phi(\cdot)$ is the density of the standard normal distribution. Integrating over the relevant distribution of preference parameters in Advance and Immediate conditions gives the likelihoods

$$L(v_{1,i}, v_{2,i}, c_i) = \int L(v_{1,i}, v_{2,i}, c_i|\delta_i, \alpha) f(\delta_i) d\delta_i \quad \text{if} \quad d=0 \tag{7}$$

$$L(v_{1,i}, v_{2,i}, c_i) = \int L(v_{1,i}, v_{2,i}, c_i|\beta\delta_i, \alpha) g(\beta_i\delta_i) d\beta_i\delta_i \quad \text{if} \quad d=1, \tag{8}$$

where $f(\cdot)$ and $g(\cdot)$ are the normal densities from which δ_i and $\beta_i\delta_i$ are drawn. We deploy the Method of Simulated Likelihood (MSL) with 1000 random Halton draws from the simulated distributions, $f(\cdot)$ and $g(\cdot)$, at each observation to estimate simulation analogs of equations (7) and (8). The average simulated likelihood over the 1000 draws is logged to arrived at a log simulated average likelihood, which is then maximized using the BFGS algorithm in Stata. The code for this estimator is provided below:

```
capture program drop moment_ml4
program define moment_ml4
* specifiy the arguments for the program
    args lnf dbar dsd lnsd alpha
    * declare temporary variables
    tempvar td1 td2 R  s mu1 mu2 sim_f sim_avef d  g
    quietly {
        * initialize the data
        generate double `td1' = $ML_y1
        generate double `td2' = $ML_y2
        generate double `R' = $ML_y3
        generate double `s' = $ML_y4

        * initiate simulation average likelihood
        generate double `sim_avef' = 0

        * set seed equivalent to prior seed
        set seed 10101

        * simulate likelihood at each draw of beta
        forvalues drawnum = 1/1000 {

            *draw delta
            generate double `d' = `dbar'  + exp(`dsd')*invnormal(draws1_`drawnum')

            *establish gamma
            generate double `g' = 2

            * moment 1
             generate double `mu1' =  ( ///
                    (`g'*`td1'^(`g'-1)- (1/`R')*(`d')*`g'*`td2'^(`g'-1)) ///
                    - ///
                    (  ///
                        (((-`alpha'*((1+(`alpha'*`td1'))^(-1))))*(Vval)) ///
                        -  ///
                        (((-`alpha'*((1+(`alpha'*`td2'))^(-1)))/(`R'))*(Vval)) ///
                    ) ///
                    )

            * moment 2
            generate double `mu2' = (1/(1 + `alpha'*`td1'))*(1/(1 + `alpha'*`td2'))

            *simulated likelihood
            gen `sim_f' = .
            replace `sim_f' = normalden(`mu1', 0,  exp(`lnsd'))*`mu2' if `s'==1
            replace `sim_f' = normalden(`mu1', 0,  exp(`lnsd'))*(1-`mu2') if `s'==0
            replace `sim_f' = normalden(`mu1', 0,  exp(`lnsd'))*1 if `s'==-1

            *update average simulated likelihood
            replace `sim_avef' = `sim_avef' + (`sim_f'/1000)

            * drop out values
            drop `d' `g' `mu1' `mu2' `sim_f'

        }

    * Establish log simulated likelihood
    replace `lnf' = ln(`sim_avef')

     }

end
```

A.2 Appendix Figures

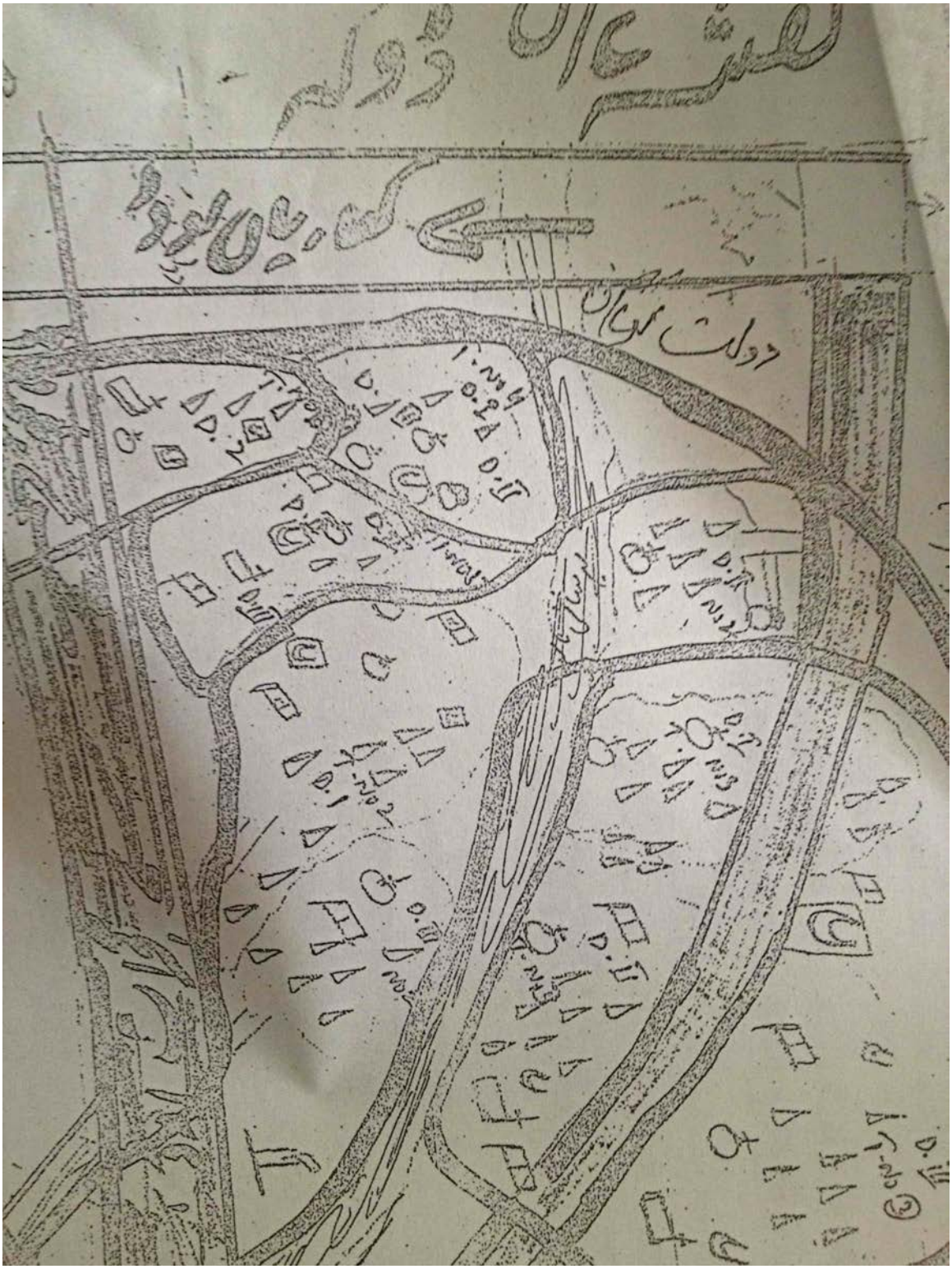


Figure A.1: Map Given to Vaccinators to Plan Route

Figure A.2: Picture of a Door-to-Door Vaccination During a Drive

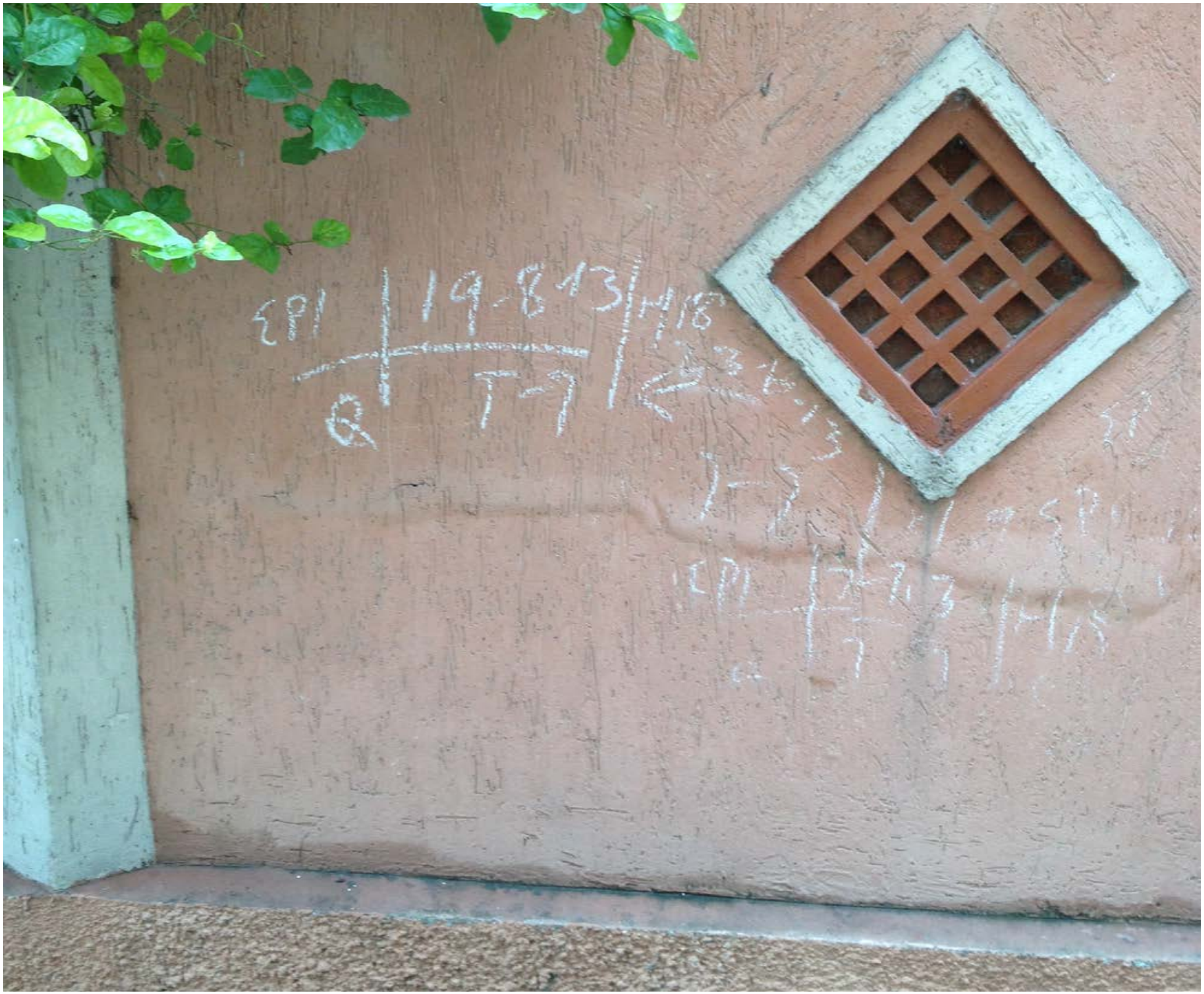
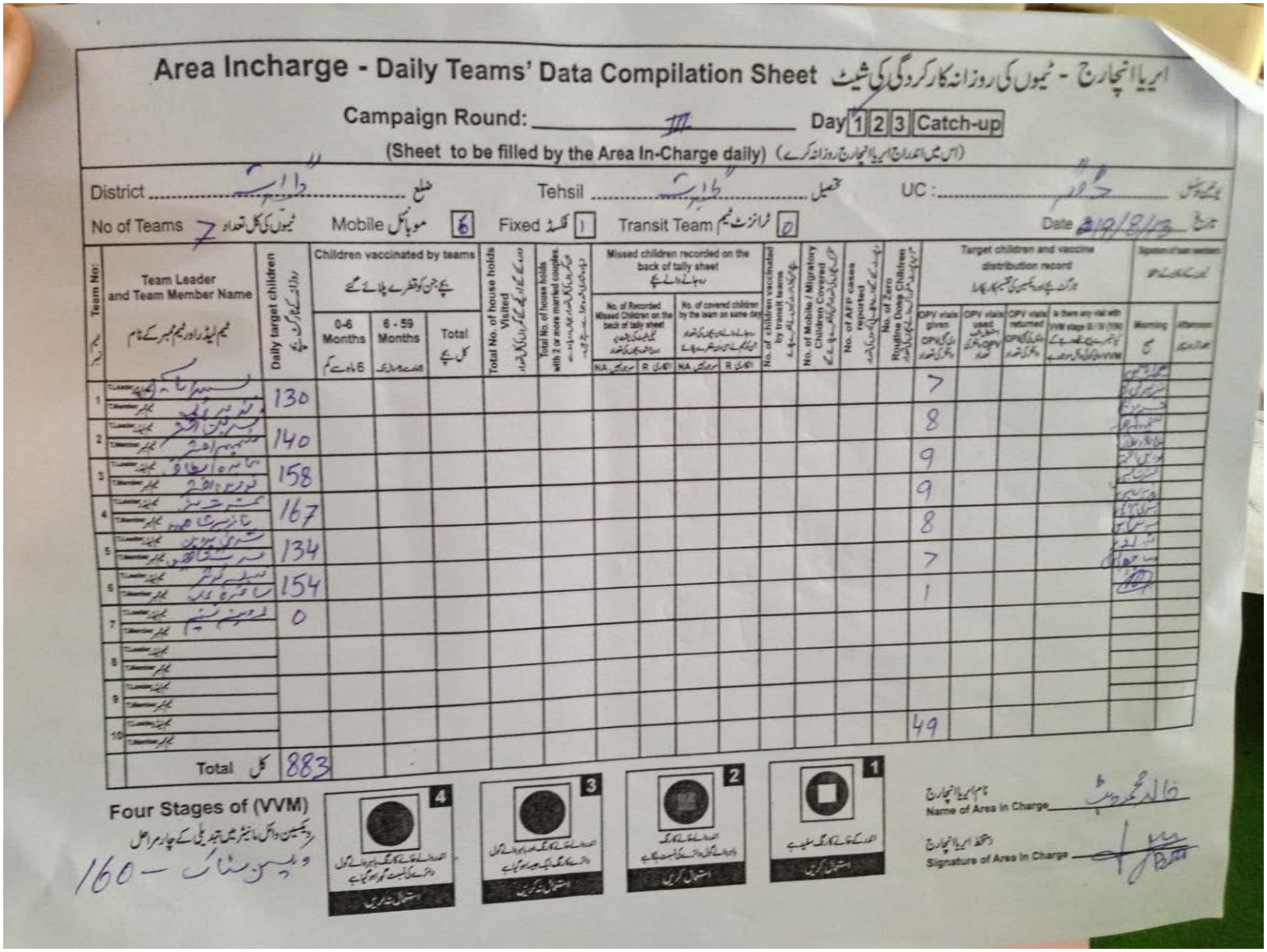


Figure A.3: Chalk Marking to Record Visit by Vaccination Team



Area Incharge - Daily Teams' Data Compilation Sheet ایریا انچارج - ٹیموں کی روزانہ کارکردگی کی شیٹ

Campaign Round: III Day 1 2 3 Catch-up

(Sheet to be filled by the Area In-Charge daily) (اس میں اندراج ایریا انچارج روزانہ کرے)

District ..... ضلع Tehsil ..... تحصیل UC: ..... یونین کونسل

No of Teams 7 ٹیموں کی کل تعداد Mobile موبائل 6 Fixed فکسڈ 1 Transit Team ٹرانزٹ ٹیم 0 Date 19/8/19 تاریخ

Team No: ٹیم نمبر	Team Leader and Team Member Name ٹیم لیڈر اور ٹیم ممبر کے نام	Daily target children روزانہ ہدف کے بچے	Children vaccinated by teams بچے جن کو قطرے پلائے گئے: 0-6 Months 6 ماہ سے کم	6 - 59 Months 6 ماہ سے 59 ماہ تک	Total کل	Total No. of house holds Visited گھروں کی تعداد جہاں ٹیم گئی	Total No. of house holds with 2 or more married couples	Missed children recorded on the back of tally sheet: No. of Recorded Missed Children on the back of tally sheet NA	R	No. of covered children by the team on same day NA	R	No. of children vaccinated by Transit team	No. of Mobile / Migratory Children Covered	No. of AFP cases reported	No. of Zero Routine Dosg Children	Target children and vaccine distribution record: OPV vials given	OPV vials used	OPV vials returned	Is there any vial with VVM stage 3 / 4	Signature of team members: Morning	Afternoon
1		130														7					
2		140														8					
3		158														9					
4		167														9					
5		134														8					
6		154														7					
7		0														1					
8																					
9																					
10																					
	Total کل	883														49					

Four Stages of (VVM) وکسین وائل مانیٹر میں تبدیلی کے چار مراحل

160 — ویکسین سٹاک

4 — استعمال نہ کریں

3 — استعمال نہ کریں

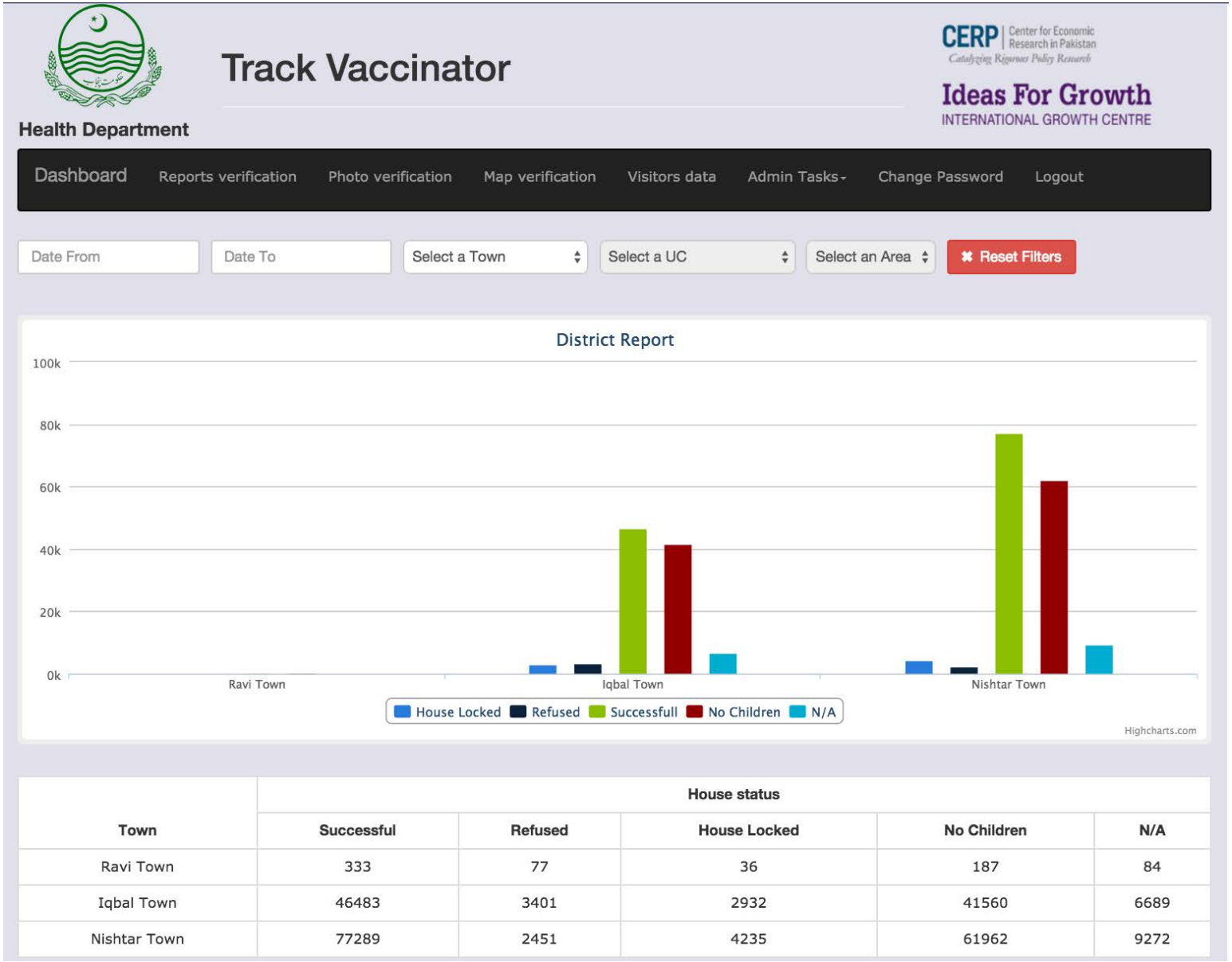
2 — استعمال کریں

1 — استعمال کریں

نام ایریا انچارج Name of Area in Charge

دستخط ایریا انچارج Signature of Area In Charge

Figure A.4: End-of-Day Compilation of Self-Reports by Vaccination Teams



Town	House status				
	Successful	Refused	House Locked	No Children	N/A
Ravi Town	333	77	36	187	84
Iqbal Town	46483	3401	2932	41560	6689
Nishtar Town	77289	2451	4235	61962	9272

Figure A.5: Screenshot of the Track Vaccinator Dashboard

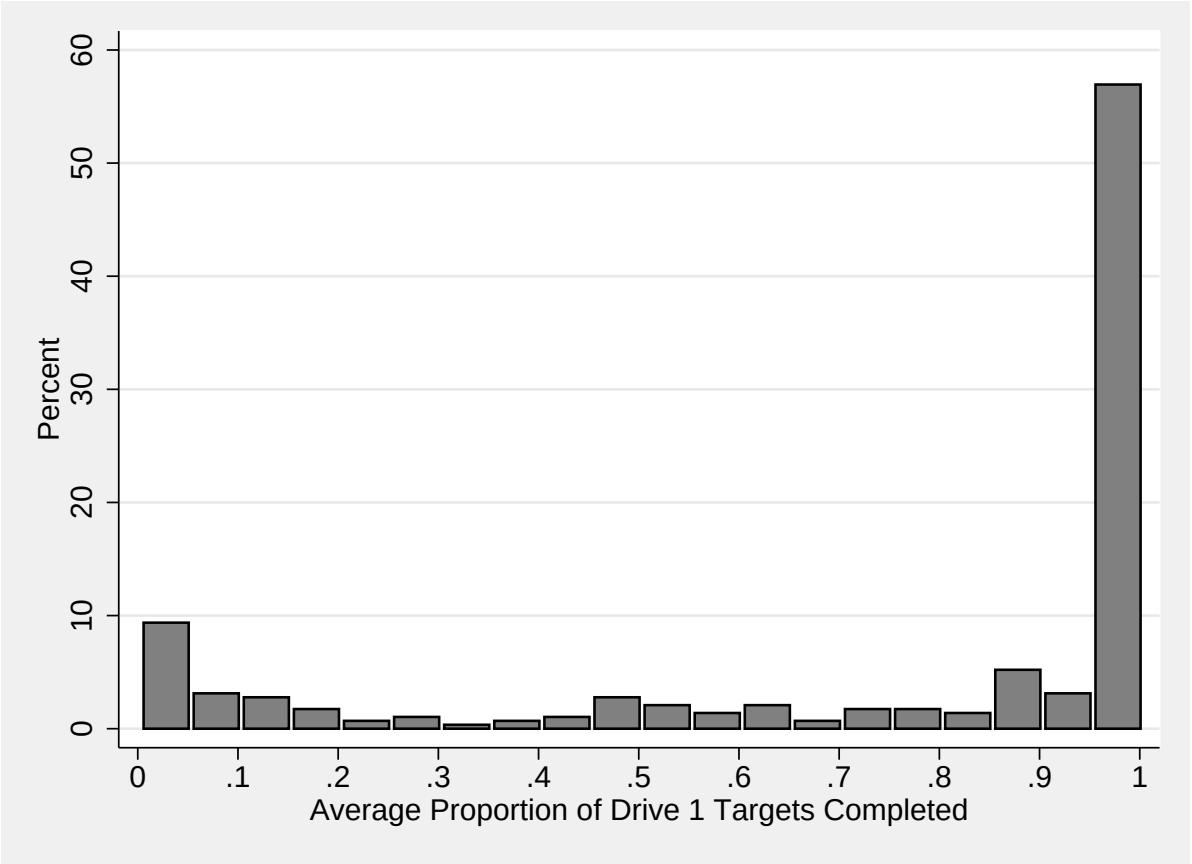


Figure A.6: Individual Completion Rates

Notes: Figure reports individual average completion rates in Drive 1 for 288 vaccinators who registered any vaccination activity . The individual average completion rate is calculated as $1/2(min(Completed_{1,i}/v_{1,i}, 1) + min(Completed_{2,i}/v_{2,i}, 1))$.

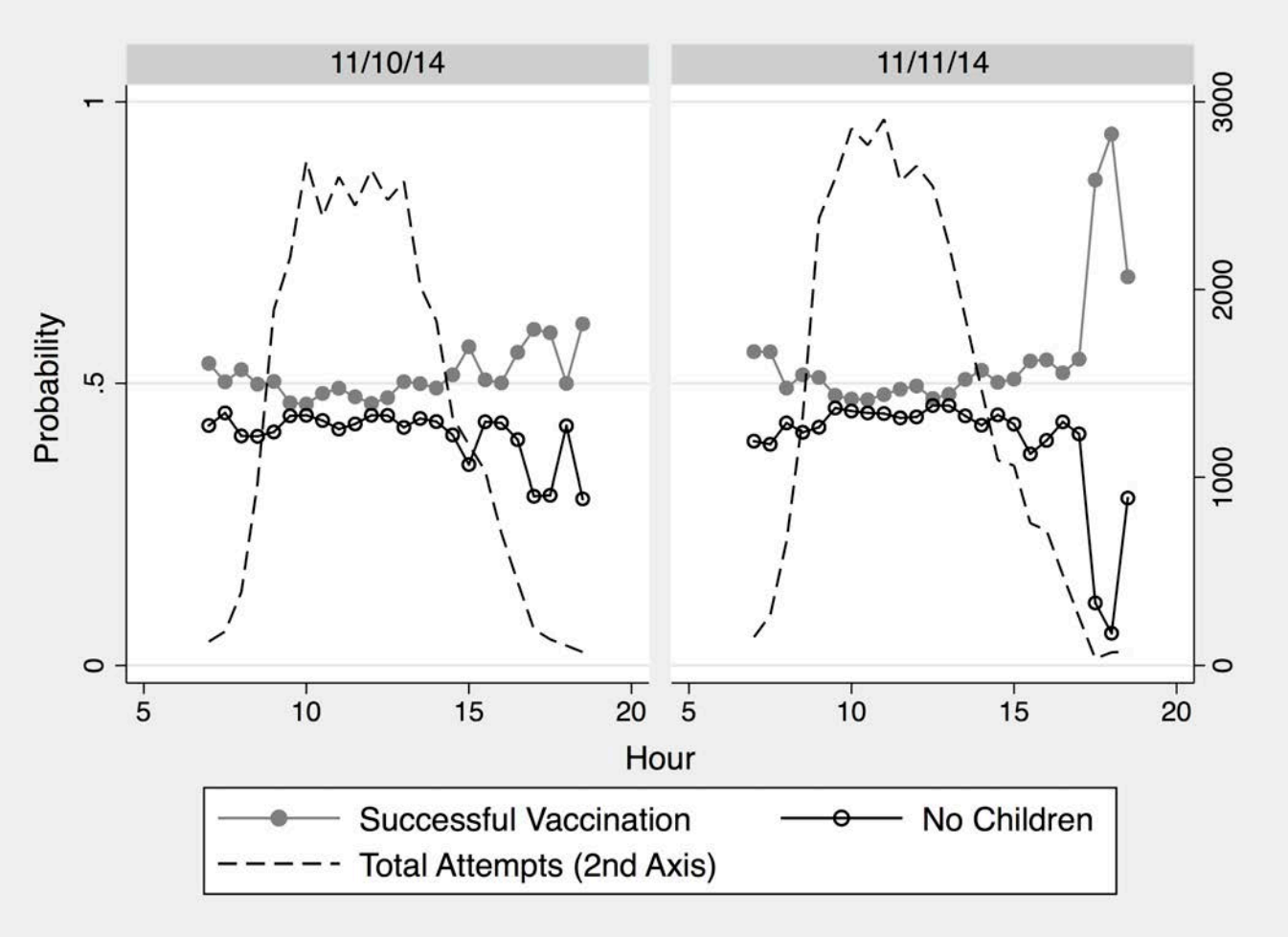


Figure A.7: Drive 1 Vaccination Activity

Notes: Figure reports Drive 1 vaccination attempts for 288 vaccinators who registered vaccination activity. The solid light grey circles are the share of all vaccination attempts that reflect a successful vaccination during the indicated hour. The hollow dark black circles are the share of all vaccination attempts that report no children being available during the attempt. These quantities are compared against the left axis. The dotted line indicates the total number of vaccination attempts for all Vaccinators in the sample. This quantity is compared against the right axis.

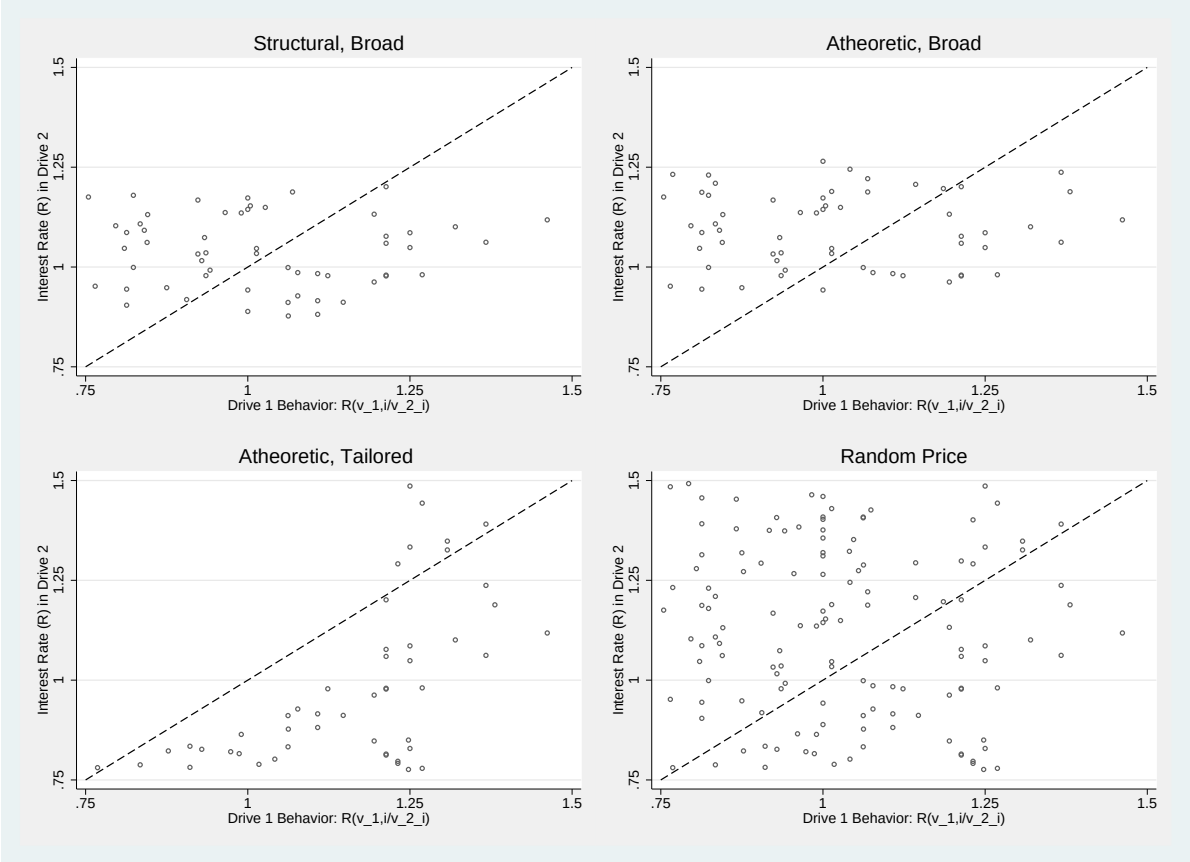


Figure A.8: Exact Assignment of Comparison Policies

Notes: Figure presents the exact assignments of 280 vaccinators in the Tailoring Sample to four policy comparison groups: Structural, Broad; Atheoretic, Broad; Atheoretic, Tailored; and Random Price.

A.3 Appendix Tables

Table A.1: Robustness to Changing Completion Function

	$p(v_{1,i}, v_{2,i}) = \frac{1}{1+\alpha'(v_{1,i}^2+v_{2,i}^2)}$	
	Advance Choice	Immediate Choice
	(1)	(2)
Discounting Parameters:		
μ_δ	0.966	
	(0.022)	
$ln(\sigma_\delta)$	-2.449	
	(0.584)	
$\mu_{\beta\delta}$		0.959
		(0.029)
$ln(\sigma_{\beta\delta})$		-1.526
		(0.164)
Completion Parameter:		
α'	0.00001	0.00002
	(0.000)	(0.000)
Shock Parameter:		
$ln(\sigma_\epsilon)$	4.068	4.147
	(0.119)	(0.148)
# Vaccinators	174	164
# Cell Phone Completion Obs.	142	146
Log-Likelihood	-1057.87	-1067.45

Notes: Parameters from maximum simulated likelihood estimation for Drive 1 allocations and completion data where noted. Allocation data provided by Full Sample of 338 vaccinators. Completion data provided by 288 vaccinators. Standard errors in parentheses.

Table A.2: Robustness to Changing Completion Utility

$[\delta_i^2 u(1000) - v_{1,i}^{\gamma} - \beta_i^{\mathbf{1}_{d=1}} \delta_i \cdot v_{2,i}^{\gamma}]$:	Advance		Immediate	
	1000	10000	1000	10000
	(1)	(2)	(3)	(4)
Discounting Parameters:				
μ_δ	0.966	0.967		
	(0.022)	(0.022)		
$ln(\sigma_\delta)$	-2.454	-2.496		
	(0.589)	(0.660)		
$\mu_{\beta\delta}$			0.959	0.962
			(0.029)	(0.029)
$ln(\sigma_{\beta\delta})$			-1.528	-1.545
			(0.164)	(0.167)
Completion Parameter:				
α	0.001	0.001	0.003	0.003
	(0.000)	(0.000)	(0.000)	(0.001)
Shock Parameter:				
$ln(\sigma_\epsilon)$	4.069	4.088	4.148	4.164
	(0.119)	(0.118)	(0.147)	(0.144)
# Vaccinators	174	174	164	164
# Cell Phone Completion Obs.	142	142	146	146
Log-Likelihood	-1058.16	-1059.94	-1068.07	-1068.54

Notes: Parameters from maximum simulated likelihood estimation for Drive 1 allocations and completion data where noted. Allocation data provided by Full Sample of 338 vaccinators. Completion data provided by 288 vaccinators. Standard errors in parentheses.

Table A.3: Panel Distributional Estimates

	Advance Choice			Immediate Choice		
	(1)	(2)	(3)	(4)	(5)	(6)
Discounting Parameters:						
μ_δ	0.970	0.958	0.982			
	(0.019)	(0.020)	(0.029)			
$ln(\sigma_\delta)$	-1.649	-2.538	-2.141			
	(0.108)	(0.655)	(0.420)			
$\mu_{\beta\delta}$				0.889	0.910	0.886
				(0.027)	(0.030)	(0.039)
$ln(\sigma_{\beta\delta})$				-1.354	-1.443	-1.475
				(0.090)	(0.110)	(0.139)
Shock Parameter:						
$ln(\sigma_\epsilon)$	4.052	4.210	4.149	4.488	4.502	4.581
	(0.101)	(0.089)	(0.131)	(0.071)	(0.076)	(0.088)
Drive 0 Included	Yes	Yes	Yes	Yes	Yes	Yes
Drive 1 Included	Yes	Yes	Yes	Yes	Yes	Yes
Only Panel Vaccinators	No	Yes	Yes	No	Yes	Yes
Only Changed Timing	No	No	Yes	No	No	Yes
# Vaccinators	340	254	126	282	210	126
Log-Likelihood	-1968.35	-1444.11	-717.30	-1747.04	-1293.67	-782.22

Notes: Parameters from maximum simulated likelihood estimation for Drive 0 and Drive 1 allocations. Allocation data from 622 vaccinator-observations over the two drives. 232 vaccinators participated in both Drive 0 and Drive 1, and 126 participated in both drives and transitioned from Advance to Immediate conditions across drives. Standard errors in parentheses. Standard errors in parentheses.

Table A.4: Impacts of Financial Incentives and Tailoring on Total Vaccination Attempts

Dependent Variable:	Total Vaccinations Attempted During Two-Day Drive					
	(1)	(2)	(3)	(4)	(5)	(6)
Incentive Group (=1)	36.195***	33.224***	30.709**	33.452**	-	-
	(11.384)	(10.559)	(13.662)	(16.690)	-	-
Structural Tailored (=1)					-12.455	-6.664
					(14.144)	(18.254)
Immediate Choice						10.433
						(20.529)
Structural Tailored x Immediate						-13.664
						(28.589)
Constant	181.851***	184.259***	164.535***	205.843***	215.550***	212.958***
	(10.200)	(9.422)	(11.884)	(15.130)	(43.341)	(43.237)
R-squared	0.01	0.18	0.20	0.21	0.15	0.15
Union Council Fixed Effects	No	Yes	Yes	Yes	Yes	Yes
Stratum Fixed Effects	No	No	No	No	Yes	Yes
Exclude 99th and 1st Percentiles	No	No	No	No	Yes	Yes
Vaccination Drives	Zero and One	Zero and One	Zero	One	Two	Two
Mean in Phone Only Group	181.85	181.85	164.41	203.37	N/A	N/A
# Vaccinators	434	434	354	353	225	225
# Observations	707	707	354	353	225	225

Notes: Table reports the effect of offering incentives on the total number of vaccinations attempted relative to the group which carried a smartphone but was not offered incentives. Columns 1 and 2 combine data from Drives 0 and 1. Column 3 reports the estimates for Drive 0 alone and column 4 reports the estimates for Drive 1 alone. Columns 4 and 5 report estimates from Drive 2 alone. Column 5 and 6 focus on the same subgroup as Table 3, excluding the 1st and 99th percentile of Drive 2 allocations, and similarly control for the stratum fixed effects and the Drive 2 value of R_i^* or $\tilde{R}_i$. Standard errors clustered at the participant level reported in parentheses in columns 1 and 2 and robust standard errors reported in parentheses in columns 3 through 6. *Level of significance*: $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.5: No Allocation Provided in Drive 0

	Allocation Provided (1)	No Allocation Provided (2)	p-value (3)
Gender (Female = 1)	0.965	1.000	0.082
	(0.020)	(0.000)	
Years of Education	10.294	10.146	0.608
	(0.220)	(0.185)	
Number of Children	3.268	3.388	0.695
	(0.239)	(0.188)	
Punjabi (=1)	0.952	0.975	0.440
	(0.023)	(0.018)	
Has a Savings Account (=1)	0.317	0.305	0.867
	(0.052)	(0.051)	
Participated in a Rosca (=1)	0.446	0.378	0.380
	(0.055)	(0.054)	
Years in Health Department	10.135	10.886	0.337
	(0.554)	(0.547)	
Years as Polio Vaccinator	9.994	10.531	0.467
	(0.538)	(0.502)	
# Vaccinators	86	82	

Notes: Table tests whether the failure of the smartphone app during Drive 0 for LHWs in the Immediate condition was systematic. Standard errors reported in parentheses. Column 3 reports a p-value corresponding to the null that the mean in the Did Not Fail group is equal to the Failed group.

Table A.6: Tailoring Intertemporal Incentives, Untrimmed Tailoring Sample

Dependent variable:	$\lvert\frac{w_{1,i}}{w_{2,i}} - 1\rvert$							
Policy Comparison Group	Random Price		Structural, Broad		Atheoretic, Broad		Atheoretic, Tailored	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Structural Tailored (=1)	-0.346	-0.002	-0.428	-0.055	-0.402	-0.031	-0.008	-0.023
	(0.234)	(0.086)	(0.423)	(0.053)	(0.378)	(0.042)	(0.030)	(0.022)
Immediate Choice		0.866*		1.127		0.989		0.144**
		(0.496)		(1.007)		(0.802)		(0.061)
Structural Tailored x Immediate		-0.782		-1.011		-0.873		-0.010
		(0.532)		(1.024)		(0.819)		(0.074)
Constant	-0.244	-0.416	0.434	0.163	0.740	0.558	0.131	0.090
	(0.992)	(1.009)	(0.295)	(0.224)	(0.608)	(0.473)	(0.099)	(0.099)
Stratum FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exclude 99th and 1st Percentiles	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Drive 2 R_i^* or $\tilde{R}_i$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.047	0.061	0.033	0.059	0.031	0.053	0.028	0.125
Mean in Untailored Contract	0.612	0.612	0.575	0.575	0.575	0.575	0.143	0.143
Mean in Untailored Advance		0.098		0.103		0.088		0.095
Mean in Untailored Immediated		1.190		1.349		1.167		0.250
# Vaccinators	280	280	200	200	204	204	197	197
# Comparison Vaccinators	138	138	58	58	62	62	55	55

Notes: Table reports the effect of structural, tailored policy relative to alternatives on realized distance to the policy target, $\lvert\frac{w_{1,i}}{w_{2,i}} - 1\rvert$ in Drive 2 for 280 Tailored and Untailored vaccinators in the Tailoring Sample. Ordinary least squares regressions. Heteroskedasticity robust White standard errors reported in parentheses. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.7: Tailoring Intertemporal Incentives, Winsorized Tailoring Sample (1%)

Dependent variable:	$\lvert\frac{w_{1,i}}{w_{2,i}} - 1\rvert$							
Policy Comparison Group	Random Price		Structural, Broad		Atheoretic, Broad		Atheoretic, Tailored	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Structural Tailored (=1)	-0.037	-0.013	-0.025	-0.023	-0.044	-0.009	-0.009	-0.023
	(0.028)	(0.019)	(0.035)	(0.024)	(0.038)	(0.023)	(0.029)	(0.022)
Immediate Choice		0.185***		0.159**		0.220***		0.144**
		(0.043)		(0.069)		(0.068)		(0.062)
Structural Tailored x Immediate		-0.060		-0.031		-0.092		-0.013
		(0.058)		(0.080)		(0.078)		(0.073)
Constant	0.004	-0.033	0.236*	0.204	0.198	0.162	0.140	0.099
	(0.091)	(0.089)	(0.127)	(0.126)	(0.130)	(0.126)	(0.095)	(0.095)
Stratum FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Drive 2 R_i^* or $\tilde{R}_i$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.063	0.162	0.033	0.123	0.056	0.166	0.032	0.132
Mean in Untailored Contract	0.194	0.194	0.162	0.162	0.188	0.188	0.143	0.143
Mean in Untailored Advance		0.098		0.103		0.088		0.095
Mean in Untailored Immediated		0.302		0.260		0.311		0.250
# Vaccinators	280	280	200	200	204	204	197	197
# Comparison Vaccinators	138	138	58	58	62	62	55	55

Notes: Table reports the effect of structural, tailored policy relative to alternatives on realized distance to the policy target, $\lvert\frac{w_{1,i}}{w_{2,i}} - 1\rvert$ in Drive 2 for 280 Tailored and Untailored vaccinators in the Tailoring Sample. Dependent variable winsorized at top and bottom 1%. Ordinary least squares regressions. Heteroskedasticity robust White standard errors reported in parentheses. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.8: Aggregate Drive 1 Behavior

	(1) Full Sample	(2) Tailoring Sample
Dependent Variable: v_1		
Immediate Decision (=1)	-2.00*	-3.00***
	(1.13)	(0.91)
Relative Price (R)	-54.29***	-66.67***
	(4.38)	(3.66)
Constant	201.86***	216.33***
	(4.72)	(3.93)
Median Advance Choice	146.5	148
# Observations	338	281

Notes: Table reports on the effects of decision timing and relative price variation on vaccinations allocated to the first day of the drive. Median regression. *Levels of Significance:* $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.9: Drive 1 Completion

Dependent Variable:	(1) *Registered Vaccination Activity*	(2) *Successfully Completed Targets*
Immediate Decision (=1)	0.074*	-0.097
	(0.038)	(0.059)
Relative Price (R)	-0.071	0.160
	(0.157)	(0.230)
Constant	0.891***	0.373
	(0.167)	(0.247)
Advance Completion Probability	0.816	0.542
# Observations	338	288

Notes: Table reports on the effects of decision timing and relative price variation on registering vaccination activity and completion in Drive 1. Column (1) based on Full Sample of 338 vaccinators. Column (2) based on sample of 288 vaccinators who registered vaccination data. Linear probability models with robust standard errors. *Levels of Significance:* $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.10: Testing Stationarity of Costs Across Days

Panel A: Time Lapse Between Vaccinations (in minutes)

Dependent variable:	Day 1 Med. Time Lapse			Day 2 Med. Time Lapse			Day 1 - Day 2 Med. Time Lapse	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Advance Choice (=1)	0.519	1.134	1.011	-0.910	-1.161	-0.829	2.295	1.840
	(2.492)	(1.163)	(1.045)	(3.164)	(3.324)	(3.182)	(3.527)	(3.343)
$\frac{Rv_{1,i}}{v_{2,i}}$			-3.697			10.004		-13.701
			(3.504)			(8.247)		(9.000)
Constant	3.370*	1.422***	5.337	4.447*	4.540*	-6.053	-3.118	11.390
	(1.851)	(0.084)	(3.708)	(2.372)	(2.501)	(6.558)	(2.501)	(7.581)
R-Squared	0.000	0.004	0.016	0.000	0.001	0.013	0.002	0.022
# Observations	265	228	228	240	228	228	228	228

Panel B: Distance Walked Between Vaccinations (in Kilometers)

Dependent variable:	Day 1 Med. Distance			Day 2 Med. Distance			Day 1 - Day 2 Med. Distance	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Advance Choice (=1)	0.112	0.146	0.132	-0.148	-0.171	-0.154	0.317	0.286
	(0.144)	(0.154)	(0.139)	(0.152)	(0.161)	(0.144)	(0.223)	(0.199)
$\frac{Rv_{1,i}}{v_{2,i}}$			-0.444			0.509		-0.953
			(0.466)			(0.516)		(0.697)
Constant	0.059**	0.038***	0.507	0.201	0.201	-0.337	-0.164	0.844
	(0.026)	(0.010)	(0.492)	(0.151)	(0.161)	(0.388)	(0.162)	(0.629)
R-Squared	0.002	0.004	0.014	0.004	0.005	0.020	0.009	0.033
# Observations	257	226	226	240	226	226	226	226

Notes: Table reports on the relationship between decision timing and the one period expected discount factor with two proxies of the cost of performing a vaccination (the amount of time that lapses between vaccinations and the distance traveled between vaccinations). Samples drawn from 288 vaccinators who registered vaccination data in Drive 1. Location and time lapse data not available for all. Heteroskedasticity robust White standard errors reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.11: Robustness Tests for Tailoring Intertemporal Incentives

Policy Comparison Group	Random Price		Structural, Broad		Atheoretic, Broad		Atheoretic, Tailored	
Panel A: Dependent variable $\frac{\lvert w_{1,i}-w_{2,i}\rvert}{\sqrt{2}}$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Structural Tailored (=1)	-4.481**	-1.868	-4.450	-3.224	-4.852*	-1.734	-5.313	-1.511
	(2.068)	(2.229)	(2.779)	(3.149)	(2.469)	(2.571)	(3.223)	(3.058)
Immediate Choice		10.597***		8.996		12.325**		17.503**
		(3.449)		(5.648)		(4.868)		(7.720)
Structural Tailored x Immediate		-6.220		-4.325		-7.933		-12.911
		(4.136)		(6.117)		(5.375)		(8.140)
Constant	16.412**	14.128**	32.672***	30.805***	26.219***	24.051***	21.422***	16.129**
Panel B: Dependent variable $\frac{\lvert w_{1,i}-w_{2,i}\rvert}{\sqrt{2}(w_{1,i}+w_{2,i})}$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Structural Tailored (=1)	-0.016**	-0.007	-0.016*	-0.011	-0.018**	-0.006	-0.019*	-0.005
	(0.007)	(0.008)	(0.009)	(0.011)	(0.009)	(0.009)	(0.011)	(0.010)
Immediate Choice		0.037***		0.031		0.044**		0.059**
		(0.012)		(0.019)		(0.017)		(0.024)
Structural Tailored x Immediate		-0.023*		-0.017		-0.030		-0.045*
		(0.014)		(0.021)		(0.019)		(0.026)
Constant	0.033	0.025	0.089***	0.083**	0.070***	0.062**	0.052**	0.034
Panel C: Dependent variable $\lvert w_{1,i}-\frac{300}{1+R}\rvert$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Structural Tailored (=1)	-3.445**	-1.405	-3.353*	-2.401	-3.856**	-1.452	-3.942*	-1.121
	(1.459)	(1.591)	(1.976)	(2.238)	(1.825)	(1.891)	(2.294)	(2.135)
Immediate Choice		7.844***		6.473		9.095**		12.664**
		(2.509)		(4.095)		(3.636)		(5.287)
Structural Tailored x Immediate		-4.850		-3.277		-6.092		-9.512*
		(2.974)		(4.402)		(3.970)		(5.582)
Constant	7.571	5.871	19.468***	18.107**	15.291***	13.666***	11.378**	7.539
Panel D: Dependent variable $\lvert w_{1,i}-\frac{300}{1+R}\rvert > 10$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Structural Tailored (=1)	0.206	0.146	0.119	0.198	0.245	0.175	0.137	0.021
	(0.168)	(0.234)	(0.219)	(0.292)	(0.212)	(0.302)	(0.242)	(0.313)
Immediate Choice		-0.573**		-0.346		-0.640*		-0.866**
		(0.238)		(0.390)		(0.362)		(0.415)
Structural Tailored x Immediate		0.144		-0.107		0.188		0.409
		(0.340)		(0.456)		(0.438)		(0.487)
Constant	1.530***	1.705***	0.134	0.201	0.617	0.720	0.950	1.253**
Panel E: Dependent variable $min[w_{1,i}, w_{2,i}]$								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Structural Tailored (=1)	2.540*	0.843	4.057**	3.079	4.404**	2.179	2.711	0.263
	(1.416)	(1.567)	(1.966)	(2.228)	(1.766)	(1.885)	(2.187)	(2.178)
Immediate Choice		-6.815***		-6.221		-8.383**		-11.173**
		(2.332)		(4.106)		(3.513)		(5.149)
Structural Tailored x Immediate		4.037		3.299		5.639		8.292
		(2.806)		(4.406)		(3.844)		(5.451)
Constant	208.758***	210.228***	200.751***	202.074***	204.405***	205.905***	208.831***	212.213***
	(4.541)	(4.433)	(6.759)	(6.971)	(5.357)	(5.228)	(5.476)	(5.306)
Stratum FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exclude 99th and 1st Percentiles	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Drive 2 R_i^s or $\tilde{R}_i$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
# Vaccinators	267	267	191	191	194	194	184	184
# Comparison Vaccinators	132	132	56	56	59	59	49	49

Notes: Table reports the effect of structural, tailored policy relative to alternatives on realized distance to a range of policy targets in Drive 2 for 267 Tailored and Untailored vaccinators in the Tailoring Sample. Thirteen of 280 Tailoring Sample vaccinators with extreme Drive 2 allocations are excluded. Ordinary least squares regressions. Heteroskedasticity robust White standard errors reported in parentheses. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.12: Structural Tailoring Intensity

Dependent variable:	$\lvert\frac{w_{1,i}}{w_{2,i}} - 1\rvert$					
	(1)	(2)	(3)	(4)	(5)	(6)
Structural Tailoring Intensity	0.110*	0.089	0.124*	0.025	0.874	-0.353
	(0.063)	(0.076)	(0.065)	(0.054)	(0.745)	(0.361)
Immediate Choice		0.068***		0.064**		0.069
		(0.022)		(0.025)		(0.215)
Structural Tailoring Intensity x Immediate		0.057		0.154		2.087
		(0.131)		(0.114)		(1.841)
Constant	-0.009	-0.018	0.044	0.016	-0.448	-0.451
	(0.058)	(0.058)	(0.065)	(0.063)	(0.727)	(0.632)
# Vaccinators	267	267	320	320	337	337
Include Boundary Sample	No	No	Yes	Yes	Yes	Yes
Stratum FEs	Yes	Yes	Yes	Yes	Yes	Yes
Exclude 99th and 1st Percentiles	Yes	Yes	Yes	Yes	No	No
Drive 2 R	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table reports the effect of structural tailoring intensity on realized distance to the policy target, $\lvert\frac{w_{1,i}}{w_{2,i}} - 1\rvert$ in Drive 2. Individuals in the Tailoring Sample who are tailored have intensity measures equal to zero and all others are calculated as the absolute distance between their assigned Drive 2 R and their value of $\frac{Rv_{1,i}}{v_{2,i}}$ in Drive 1. Column 1 provides results for 267 of 280 LHWs in the Tailoring Sample, excluding 13 LHWs with extreme Drive 2 allocations. Column 2 includes the Boundary Sample and provides results for 320 of 337 LHWs, excluding 17 LHWs with extreme Drive 2 allocations. Column 4 includes the Boundary Sample and provides results for all 337 LHWs. Ordinary least squares regressions. Heteroskedasticity robust White standard errors reported in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.13: Structural Tailoring and Completion

Dependent variable:	$\lvert\frac{w_{1,i}}{w_{2,i}} - 1\rvert$							
Policy Comparison Group	Random Price		Structural, Broad		Atheoretic, Broad		Atheoretic, Tailored	
Panel A: Completed Drive 1								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Structural Tailored (=1)	-0.042*	-0.004	-0.030	-0.009	-0.012	0.011	-0.107**	-0.051
	(0.024)	(0.025)	(0.029)	(0.031)	(0.024)	(0.023)	(0.049)	(0.037)
Immediate Choice		0.119***		0.116*		0.111**		0.160*
		(0.045)		(0.062)		(0.050)		(0.089)
Structural Tailored x Immediate		-0.096*		-0.079		-0.076		-0.135
		(0.051)		(0.067)		(0.054)		(0.094)
Constant	-0.051	-0.077	0.217*	0.190	0.110	0.091	-0.043	-0.088
	(0.088)	(0.090)	(0.119)	(0.122)	(0.084)	(0.076)	(0.097)	(0.111)
# Vaccinators	142	142	98	98	101	101	93	93
Panel B: Failed Drive 1								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Structural Tailored (=1)	-0.053	-0.009	0.004	-0.009	-0.085	-0.027	-0.007	0.049
	(0.039)	(0.042)	(0.035)	(0.056)	(0.068)	(0.071)	(0.049)	(0.044)
Immediate Choice		0.098		-0.013		0.127		0.209*
		(0.070)		(0.072)		(0.128)		(0.110)
Structural Tailored x Immediate		-0.086		0.023		-0.106		-0.201*
		(0.075)		(0.078)		(0.125)		(0.116)
Constant	0.028	0.016	0.116	0.119	0.199	0.186	0.114	0.093
	(0.098)	(0.092)	(0.101)	(0.103)	(0.149)	(0.138)	(0.096)	(0.086)
# Vaccinators	87	87	65	65	66	66	63	63
Stratum FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Exclude 99th and 1st Percentiles	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Drive 2 R_i^* or $\tilde{R}_i$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table reports the effect of structural, tailored policy relative to alternatives on realized distance to the policy target, $|\frac{w_{1,i}}{w_{2,i}} - 1|$ in Drive 2 for 229 vaccinators in the Tailoring Sample who also registered cell-phone vaccination activity. Separate estimates provided for those who succeeded or failied to hit their Drive 1 targets. Thirteen of 280 Tailoring Sample vaccinators with extreme Drive 2 allocations are excluded and an additional 38 vaccinators without registered vaccination activity are excluded. Ordinary least squares regressions. Heteroskedasticity robust White standard errors reported in parentheses. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.