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

James Andreoni
Michael Callen
Muhammad Yasir Khan
Karrar Jaffar
Charles Sprenger

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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 structural estimates of time preferences to customize incentives for polio vaccinators in Lahore, Pakistan. We measure time preferences using intertemporal allocations of effort, and derive the mapping between these structural estimates and individually optimized incentives. We evaluate the effect of matching contract terms to discounting parameters in a subsequent experiment with the same vaccinators. This exercise provides a test of the specific point predictions given by structural estimates of discounting parameters. We demonstrate that tailoring contract terms to individual discounting moves allocation behavior significantly towards the intended objective.

James Andreoni
Department of Economics
University of California, San Diego
9500 Gilman Drive
La Jolla, CA 92093-0508
and NBER
andreoni@ucsd.edu

Michael Callen
Rady School of Management
University of California, San Diego
Wells Fargo Hall, Room 4W104
9500 Gilman Drive #0553
La Jolla, CA 92093-0553
and NBER
mjcallen@ucsd.edu

Muhammad Yasir Khan
UC Berkeley Haas School of Business
yasir.khan@berkeley.edu

Karrar Jaffar
USC Department of Economics
karrar.jaffar@gmail.com

Charles Sprenger
UC San Diego
Rady School of Business
9500 Gilman Drive
La Jolla , CA 93093
c.sprenger@gmail.com

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1 Introduction

Nearly every economic decision people make entails some tradeoff through time, whether it is consumption versus savings, doing a task now or later, building human capital, or investing in one's career. Characterizing such choices with structural models of discounting has been a core challenge for economists for much of the last century, with important contributions by Samuelson (1937); Koopmans (1960); Laibson (1997) and O'Donoghue and Rabin (2001). The preference parameters governing such models are of unique value for understanding a broad range of behaviors, and so have received a great deal of attention in the empirical literature on intertemporal choice.[1]

This paper seeks to understand whether the out-of-sample predictions given by structural estimates of discounting are empirically valid. We conduct a field experiment on workers' allocation of effort through time, a decision where evidence suggests that present-biased models of decision-making may be particularly relevant.[2] We first estimate the individual discounting parameters of our workers, and then use these estimates to customize each worker's contract to their identified preferences with the intent of reaching a specific intertemporal pattern of work. That is, we tailor incentives within-subject with the objective of reaching a predicted out-of-sample target. Our core test of predictive validity compares tailored workers to a control group which receives untailored, random contract terms.

Relatively little research makes use of the predictive value gained from the articulation and estimation of structural models of discounting.[3] When structural estimates or related measures

[1] Examples include Hausman (1979); Lawrance (1991); Warner and Pleeter (2001); Cagetti (2003); Laibson, Repetto and Tobacman (2005); Mahajan and Tarozzi (2011); Fang and Wang (2015); Harrison, Lau and Williams (2002); Andersen, Harrison, Lau and Rutstrom (2008); Andreoni and Sprenger (2012a).

[2] For recent experimental examples, see Kaur, Kremer and Mullainathan (2010, 2015); Augenblick, Niederle and Sprenger (2015); Carvalho, Meier and Wang (2014) and Augenblick and Rabin (2015).

[3] What structural models have been used for is for comparison to market interest rates (Hausman, 1979), for comparison across samples, time, or elicitation and estimation strategies (Coller and Williams, 1999; Frederick, Loewenstein and O'Donoghue, 2002; Meier and Sprenger, 2015; Andersen et al., 2008), to assess differences in patience across subpopulations (Kirby, Petry and Bickel, 1999; Tanaka, Camerer and Nguyen, 2010; Harrison et al., 2002; Dohmen, Falk, Huffman and Sunde, 2010; Lawrance, 1991; Warner and Pleeter, 2001), to conduct welfare analyses (Laibson, 1997), and to conduct standard counterfactual exercises without out-of-sample testing (e.g., for how price changes should alter demand (Mahajan and Tarozzi, 2011)).

are used in out-of-sample prediction exercises, the analysis has often been indirect, linking differences in measured patience to differences in other behaviors without an articulated model for the precise relationship between the two (Chabris, Laibson, Morris, Schuldt and Taubinsky, 2008b; Meier and Sprenger, 2008, 2012, 2010; Ashraf, Karlan and Yin, 2006; Dohmen, Falk, Huffman and Sunde, 2006; Castillo, Ferraro, Jordan and Petrie, 2011).[4] Though such correlational exercises yield valuable insights, they could potentially be made more powerful by directly employing the theoretical parameters in developing the out-of-sample prediction.[5]

Our project engages government health workers—termed Lady Health Workers (LHWs)—associated with polio eradication efforts for the Department of Health in Lahore, Pakistan. 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.[6] The disease largely affects children under five. The function of LHW vaccinators is to provide oral polio vaccine to children during government organized vaccination drives, which usually last two or more days and are conducted approximately every month. Vaccinators are given a supply of oral vaccine and a neighborhood map, and are asked to travel door-to-door vaccinating children with a suggested target for vaccinations. Prior to our project there was no technology for monitoring vaccinators, and achievement was self reported. As one might imagine, vaccinators often fell short of their suggested targets, but rarely reported doing so. This behavior is consistent with the large literature on public sector absenteeism (Banerjee and Duflo, 2006; Banerjee, Duflo and Glennerster, 2008; Chaudhury, Hammer, Kremer, Muralidharan and Rogers, 2006; Callen, Gulzar, Hasanain and Khan, 2015).

[4]One exception is Mahajan and Tarozzi (2011) who use monetary measures for time inconsistency and purchase and treatment decisions for insecticide treated bednets together to estimate the extent of present bias and 'sophistication' thereof. This exercise can be thought of as articulating the relationship between the experimental measures of time inconsistency and contract choice to deliver estimates of present bias. One point noted by Mahajan and Tarozzi (2011) is that the experimental measures wind up having limited predictive power for estimates of present bias that result from their structural exercise.

[5]Indeed, such exercises could by-and-large be conducted without appeal to structural estimation. Linking either non-parametric measures of discounting or structural parameters thereof to other behavior yields largely the same correlational insights if one does not articulate precisely how how the structural parameters should predict behavior.

[6]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.

Since our study requires implementing performance-based incentives, it hinges fundamentally on an accurate measure of productivity.[7] To this end, each vaccinator in our sample is provided a smartphone, equipped with a precise real-time reporting application developed expressly for this project.

Our tool for both measuring intertemporal preferences and tailoring intertemporal incentives is a special bonus contract. In this contract, vaccinators set daily work targets, and, conditional on reaching these targets, receive a sizable bonus. In particular, vaccinators set daily targets of v_1 and v_2 vaccination attempts on day 1 and day 2 of the drive, respectively. Vaccinators face an interest rate, R, such that a single vaccination that is allocated to day 2 reduces by R the number of vaccinations required on day 1. That is, v_1 and v_2 satisfy the constraint

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

where $V = 300$. The bonus contract offers a fixed bonus of 1000 rupees (10 times the daily vaccinator wage) for meeting *both* of their v_1 and v_2 vaccination target attempts over a two-day drive.[8] If either daily target, v_1 or v_2, is not met, the bonus is not received. Following the laboratory study of working over time conducted by Augenblick et al. (2015), our contracts are the first field implementation of the Convex Time Budget (Andreoni and Sprenger, 2012a) for eliciting intertemporal preferences using allocations of effort.

Chosen allocations, (v_1, v_2), can be used to structurally estimate discounting parameters for vaccinators. Experimental variation permits identification of an important behavioral aspect of intertemporal choice: the existence of present-biased preferences (Laibson, 1997; O'Donoghue

[7]This links our work to a substantial body of recent research in development economics examining the role of incentives and monitoring in improving public sector performance (Bertrand, Burgess, Chawla and Xu, 2016; Basinga, Gertler, Binagwaho, Soucat, Sturdy and Vermeesch, 2011; Miller, Luo, Zhang, Sylvia, Shi, Foo, Zhao, Martorell, Medina and Rozelle, 2012; Olken, Onishi and Wong, 2014; Khan, Khwaja and Olken, 2015; Muralidharan and Sundararaman, 2011).

[8]Our bonus program paid vaccinators for attempted rather than successful vaccinations to avoid concerns that vaccinators, motivated by the incentives, would coerce individuals to receive vaccination. Details on the incentive program are provided in Section 2.2. Slightly more than half of vaccination attempts are successful, with slightly less than half of vaccination attempts reporting no child present. Appendix Figure A.7 reports vaccination behavior for each half-hour of the study, demonstrating limited variation in the proportion of successful and failed vaccination attempts throughout the work day.

and Rabin, 1999). Vaccinators are randomly assigned to make their allocation decision either in advance of the first day of the drive or immediately on day 1 itself. Additionally, vaccinators are randomly assigned an interest rate, R. Under specific structural assumptions, the experiment identifies a set of aggregate discounting parameters (for similar estimation strategies see Andreoni and Sprenger, 2012a; Augenblick et al., 2015). And, under additional assumptions, each vaccinator's allocation identifies her individual discount factor.

Unlike laboratory settings where sizable completion bonuses have been used to ensure near one-hundred percent completion rates, in our field setting even a bonus of 10 times daily wages does not ensure uniform completion.[9] Roughly half of our subjects do not successfully complete their chosen allocations. Such failure to complete has the potential to confound identification of time preferences from experimental choice. Uncertainty alters the worker's optimization problem, requiring them to balance their true preferences against the failure probabilities induced by choice. Recognizing this issue and the likelihood that other field implementations of such elicitation methods will likely face a similar challenge, we develop methodology to simultaneously estimate parameters related to preferences and failure probabilities. This methodology can be used to generate not only out-of-sample predictions for allocation behavior, but also for subsequent completion rates.[10]

We use the individual discounting parameters from an initial drive to predict behavior in a follow-up drive. We couch our out-of-sample exercise in a policy experiment which tailors

[9]For college subjects Augenblick et al. (2015) employ bonuses \$100 in their six-week study and achieve 88% completion. In their follow-up work conditions they employ bonuses of \$60 for a three week study and achieve 95% completion.

[10]The lack of uniform completion was not a feature of the data we initially expected, but, in retrospect, is something we should have anticipated. Data from drives prior to our intervention showed that vaccinators 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. Though we suspected prior data to reflect some over-reporting, they did guide our choice, in collaboration with the Department of Health, of $V = 300$. Indeed, the Department of Health was insistent that the target number of vaccinations not stray too far from prior drive targets. In our initial drive, seventy-two vaccinators were provided with a phone alone and no additional incentives for work, mimicking their standard work environment. Sixty vaccinators used the application and completed an average of 203 vaccination attempts. Only twenty-four vaccinators recorded 300 or more vaccination attempts in the drive. Given our lack of foresight, neither the functional forms estimated for failure probabilities nor the implemented out-of-sample exercise predicting completion rates were in our study registration. As such, they should be viewed with appropriate caveats.

intertemporal incentives to worker time preferences. The tailoring policy we adopt is one which uses the interest rate, R, to induce smooth allocation of vaccinations through time, $v_1 = v_2$, for every vaccinator. The optimal choice under perfect completion is simple: to ensure smooth allocation of service, the policy must give each vaccinator an interest rate equal to their (appropriately defined) discount factor. Distance to the policy objective is compared between a group of tailored vaccinators and a control group which receives random interest rates.[11] Our tailored policy makes vaccinators 'pay' for their impatience by facing a more severe interest rate the more impatient they are, requiring more work in total. As such, it also generates out-of-sample predictions for completion probabilities, with less patient vaccinators being predicted to be more likely to fail under the tailored policy regime.

In a sample of 337 vaccinators, we document three principal findings. First, on aggregate, a present bias exists in vaccination behavior. Vaccinators allocating in advance of day 1 of the drive allocate significantly fewer vaccinations to v_1 than those allocating on the morning the drive actually commences. Corresponding estimates of present bias accord with those of prior laboratory exercises with or without accounting for potential failure. Second, substantial heterogeneity in discounting is observed. This heterogeneity is important as it points to possible gains from individually-tailored contracts. Third, tailored contracts work. Relative to random contracts, vaccinators with tailored contracts provide significantly smoother service. More generally, the point predictions for behavior and completion rates out-of-sample are largely valid. We are able to predict with accuracy not only individual choices under new contract terms, but the probability with which they will complete the contract and the empirical relationships between completion rates, contract terms, and preferences.

This paper makes four contributions. First, our exercise uses field behavior about effort to examine non-standard time preferences, providing the first field operationalization of the Augenblick et al. (2015) technique for measuring preferences.[12] This joins a growing literature

[11] The policy objective we adopt is admittedly arbitrary. As the importance of the exercise is specifically in examining point predictions for out-of-sample behavior, however, even our random interest rate treatment arm possess the variation that can be used to assess the predictive validity of structural estimates.

[12] Documenting dynamic inconsistency outside of the laboratory and outside of the standard experimental

which identifies present bias from 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 et al., 2010, 2015; Carvalho et al., 2014).[13] As in prior research, our results show that when investigating non-monetary choices, dynamic inconsistency may well have empirical support.

Second, we develop a methodology to measure time preferences over work in settings where task completion is not guaranteed, which is often the case in the field where managers set ambitious targets for workers. This is certainly true in our case, where the government faced massive international pressure to report successfully administering large numbers of polio vaccinations. Measuring preferences in these settings, therefore, requires allowing for the possibility that workers do not meet their targets. More subtly, it may be that workers set their targets with these considerations in mind. This could confound preference measures. The methodology developed here to simultaneously estimate beliefs about completion and time preferences, therefore, may be valuable to researchers conducting field elicitations of preference.

Third, we find that the predictions given by time preference estimates are accurate. There is considerable debate regarding the value of measuring preferences. A substantial literature argues for their usefulness by showing that preference measures correlate with economic behavior. Our approach to evaluating the informativeness of measured preferences is to test whether the specific point predictions they give for behavior are accurate. To our knowledge, this is the first paper to do so.

The fourth contribution of our paper is to provide proof-of-concept that preference measures can be used by principals to improve agents' incentives.[14] Much of the contract theory literature

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, 2012a; Augenblick et al., 2015; Carvalho et al., 2014).

[13]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).

[14]Research in personnel economics documents the potential benefits of implementing piece rates relative to lump sum payments (Lazear, 2000; Paarsch and Shearer, 1999; Shearer, 2004) and the elasticity of effort with respect to the piece rate (Paarsch and Shearer, 2009). Our focus is not on the incentive effects of piece rates,

points to the central role of preferences in determining the optimal design of incentives.[15] The unobservability of preferences poses a key obstacle to testing the optimality of implemented contracts.[16] We measure a normally unobservable preference parameter and then use such measures in subsequent contract design. Rather than examining whether existing contracts are optimal, we derive the optimal compensation scheme given a policy instrument (the interest rate), a policy objective (smooth provision of service), and the measures of preferences, and then experimentally test whether the optimized scheme improves performance. Our tailored contracts demonstrate that structural preference estimates based on experimental procedures can assist the design of incentives. In this sense, our study is connected to efforts in precision medicine, where medical treatments are customized based on individuals' genetic information. We use the information contained in measurements of the primitives that govern economic behavior (decision parameters) to customize incentives, in the same way that precision medicine uses the information contained in DNA to customize healthcare.

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

2 Experimental Design and Structural Estimation

Our experiment has three components: eliciting the time preferences of vaccinators; identifying individual discounting parameters, and, after assigning individually tailored contracts to workers, testing whether these tailored contracts deliver on their specific objective.

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but rather the benefits of using preference estimates to customize incentives.

[15] The relevant empirical literature points to a role for risk preference in contractual settings (Jensen and Murphy, 1990; Haubrich, 1994; Ackerberg and Botticini, 2002; Dubois, 2002; Bellemare and Shearer, 2013) Chiappori and Salanié (2003) provide a review of empirical tests of predictions from contract theory about the design of incentives.

[16] In a review of the literature, Prendergast (1999) summarizes this point: "While the conclusions taken from this literature could be correct, this seems a poor method of testing agency theory...because many of the factors relevant for choosing the level of compensation are unobserved; the optimal piece rate depends on risk aversion and the returns to effort, both of which are unknown to the econometrician...it is a little like claiming that prices are too high without knowing costs." (p. 19)

In the first subsection below, we describe the smart-phone monitoring application we developed to track the productivity of our workers. We then describe how we identify discounting parameters, and how we use these to design our tailored contracts. A fourth subsection provides details of our experimental design.

2.1 Vaccinations and Smartphone Monitoring

The Department of Health in Lahore, Pakistan, employs Lady Health Worker vaccinators throughout the city to conduct polio vaccination drives. Every month there is a vaccination drive that is at least two days long. Vaccinators are organized into teams of one senior worker and one junior assistant. These teams work together throughout the drive. Our experiment focuses on the incentives of the senior vaccinator.

Prior to our study, the standard protocol for vaccination drives was to provide each vaccinator a fixed target for total vaccinations over the drive and a map of potential households (called a "micro-plan"). No explicit incentives for completing vaccinations were provided and vaccinators received a fixed daily wage of 100 rupees (around $1). Vaccinators were asked to walk their map, knocking on each compound door, and vaccinating each child for whom parental permission was granted.[17] At the end of each day, vaccinators in each neighborhood convened with their supervisor and self-reported their vaccination activity for the day.[18] In principle, a monitor could verify the claims.[19] In practice, however, there was virtually no monitoring, and strong reasons to suspect over-reporting.[20]

[17]Vaccinating a child consists of administering a few drops of oral vaccine. As there is no medical risk of over-vaccination, vaccinators are encouraged to vaccinate every child for whom permission is granted. For each attempted vaccination, vaccinators 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.

[18]Appendix Figure A.4 provides a picture of the form capturing the self-reports. The second column records the number of vaccinations for the day. The seventh column reports the number of vials of vaccine used in the process.

[19]This could potentially be done by walking the micro-plan and examining the chalk markings on each compound wall.

[20]We attempted to independently audit vaccinators 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

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."

In collaboration with the Department of Health, we designed a smartphone-based monitoring system. Each vaccinator in our study was given a smartphone equipped with a vaccination monitoring application. The vaccinator was asked to record information related to each vaccination. Then, she 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.[21]

the targets associated with each micro-plan prior to our monitoring intervention and that vaccinators almost always reported meeting their targets exactly. Even with a bonus incentive and smartphone monitoring in place, we find that vaccinators on average achieve only 62 percent (s.d. = 58 percent) of the target given by their micro-plans. Vaccinators likely would achieve a smaller share of their target in the absence of both monitoring and financial incentives.

[21]This dashboard system is based on the technology described in Callen et al. (2015) and is depicted in Appendix Figure A.5.

The smartphone system allows us to register vaccination attempts and provides a basis for creating intertemporal bonus contracts designed to elicit vaccinator time preferences. We next provide an outline of the bonus contracts.

2.2 Intertemporal Bonus Contracts

We worked with the Department of Health to implement intertemporal bonus contracts in two-day drives in September, November and December of 2014.

The intertemporal bonus contracts required workers to complete a present value total of $V = 300$ vaccination attempts in exchange for a fixed bonus of 1000 rupees. Vaccinators 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 vaccinator would receive only her standard wage.

Each vaccinator was randomly assigned an interest rate 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 intertemporal bonus contract is identical to an experimental device termed a Convex Time Budget used to investigate time preferences (Andreoni and Sprenger, 2012a,b).[22] The intertemporal allocation (v_1, v_2) potentially carries information on the time preferences of each vaccinator. We next describe the relevant experimental variation and structural assumptions that permit us to identify discounting parameters at the aggregate and individual level.

[22]For applications to field studies and effort allocations, see Augenblick et al. (2015); Carvalho et al. (2014); Gine, Goldberg, Silverman and Yang (2010). We also borrow an additional design element from such studies—minimum allocation requirements—from such studies. In order to avoid vaccinators 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 vaccinators to work on both days of the drive, we avoid confounding extreme patience or extreme impatience with vaccinators simply not wishing to come to work on one of the two days.

2.2.1 Experimental Variation and Structural Identification

Our design generates two sources of experimental variation. First, each vaccinator is randomly assigned an interest rate, 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 vaccinator with a slider bar on the introduction screen of the smartphone application. Figure 1, Panel B depicts the slider bar with an assigned interest rate, R, equal to 1.25. The vaccinator 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 vaccinator 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 'Advance' decisions and the second as 'Immediate' decisions. The assignment to either the Advance or Immediate group was independent of the interest rate assignment. Section 2.4 describes the efforts taken to make everything else besides allocation timing equal between these conditions.

Random assignment to Advance or Immediate choice and random assignment of R are both critical design elements for identifying the discounting parameters of interest. We assume that individuals minimize the discounted costs of effort subject to the intertemporal budget constraint provided by their bonus contract. We make two further structural assumptions. 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 the convex costs of effort. Second, we assume that individuals discount the future quasi-hyperbolically (Laibson, 1997; O'Donoghue and Rabin, 1999). Hence, the worker's disutility of effort can be written as

$$v_1^\gamma + \beta^{\mathbf{1}_{d=1}} \delta \cdot v_2^\gamma.$$

The indicator $\mathbf{1}_{d=1}$ captures whether the decision is made in advance or immediately on day 1. The parameters β and δ summarize individual discounting with β capturing the degree of present bias, active for vaccinators who make Immediate decisions, that is, $\mathbf{1}_{d=1} = 1$. If $\beta = 1$,

the model nests exponential discounting with discount factor δ, while if $\beta < 1$ the decisionmaker exhibits a present bias, being less patient in Immediate relative to Advance decisions.

Minimizing discounted costs subject to the intertemporal budget constraint of the experiment yields marginal condition:

$$\gamma v_1^{\gamma-1} - \frac{\beta^{\mathbf{1}_{d=1}}\delta}{R}\gamma v_2^{\gamma-1} = 0. \tag{1}$$

Interpreting this marginal condition as a moment requirement, time preferences can potentially be estimated with standard minimum distance estimation techniques (Hansen, 1982; Hansen and Singleton, 1982). Experimental manipulation of R and $\mathbf{1}_{d=1}$ provides identifying variation.[23]

One critical assumption to the development above revolves around the force of the implemented incentives. The contracts we implement feature a completion bonus of 1000 rupees paid the day after the drive if both targets, v_1 and v_2 are met. The choice of large bonuses (around 10 times daily wages) followed the design logic discussed in Augenblick et al. (2015). Not completing allocated vaccinations creates a sizable penalty at any given point in time. Vaccinators

[23]A previous version of this paper expressed the Euler equation of (1) as

$$\left(\frac{v_1}{v_2}\right)^{\gamma-1}\frac{1}{\beta^{\mathbf{1}_{d=1}}\delta} = \frac{1}{R}.$$

Taking logs and rearranging yields

$$log\left(\frac{v_1}{v_2}\right) = \frac{log\delta}{\gamma-1} + \frac{log\beta}{\gamma-1}\mathbf{1}_{d=1} - \frac{1}{\gamma-1}logR.$$

If we assume that allocations satisfy the above equation subject to an additive error term, ϵ, we arrive at the linear regression equation

$$log\left(\frac{v_1}{v_2}\right) = \frac{log\delta}{\gamma-1} + \frac{log\beta}{\gamma-1}\mathbf{1}_{d=1} - \frac{1}{\gamma-1}logR + \epsilon,$$

which can also be estimated with standard techniques. Incorporating potential failure into such a linear estimator was not feasible, but it does provide intuition for the identification of structural parameters from vaccinator allocations, and make clear the purpose of our experimental variation in R and $\mathbf{1}_{d=1}$. Variation in the interest rate, R, identifies the shape of the cost function, γ, while variation in $\mathbf{1}_{d=1}$ identifies β. Note that δ would be identified from the average level of v_1 relative to v_2 when decisions are made in advance (i.e., identified from the constant). An identical strategy for structurally estimating time preferences was introduced in controlled experiments by Andreoni and Sprenger (2012a), and has precedents in a body of macroeconomic research identifying aggregate preferences from consumption data. See, for example, Shapiro (1984); Zeldes (1989); Lawrance (1991). Very similar results are obtained for our baseline estimates using this method and the minimum distance method now implemented.

should forecast that they will indeed complete the required vaccinations and so allocate them according to their true preferences. If vaccinators forecast not completing required vaccinations with some chance, the probability of completion has the potential to confound this approach to measuring preferences.

Consider a vaccinator with probability $p(v_1, v_2)$ of successfully completing her allocated targets. Hence, the expected disutility of effort is

$$p(v_1, v_2)[v_1^\gamma + \beta^{\mathbf{1}_{d=1}}\delta \cdot v_2^\gamma] + (1 - p(v_1, v_2))[v_{1,n}^\gamma + \beta^{\mathbf{1}_{d=1}}\delta \cdot v_{2,n}^\gamma],$$

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

$$p(v_1, v_2)\delta^2 u(1000) + (1 - p(v_1, v_2))\delta^2 u(0).$$

For simplicity, we normalize the net utility under non-completion $\delta^2 u(0) - v_{1,n}^\gamma - \beta^{\mathbf{1}_{d=1}}\delta \cdot v_{2,n}^\gamma$ to be zero. Under this assumption, allocations are delivered by the constrained optimization problem

$$max_{v_1,v_2} p(v_1, v_2)[\delta^2 u(1000) - v_1^\gamma - \beta^{\mathbf{1}_{d=1}}\delta \cdot v_2^\gamma]$$
$$s.t.\ v_1 + Rv_2 = V.$$

The corresponding marginal condition,

$$\gamma v_1^{\gamma-1} - \frac{\beta^{\mathbf{1}_{d=1}}\delta}{R}\gamma v_2^{\gamma-1} = \left(\frac{\frac{\partial p(v_1,v_2)}{\partial v_1} - \frac{1}{R}\frac{\partial p(v_1,v_2)}{\partial v_2}}{p(v_1, v_2)}\right)[\delta^2 u(1000) - v_1^\gamma - \beta^{\mathbf{1}_{d=1}}\delta \cdot v_2^\gamma],$$

highlights a central tradeoff between discounted marginal costs and marginal completion probabilities. Of course, if the probability of success is independent of choice, $\frac{\partial p(v_1,v_2)}{\partial v_1}, \frac{\partial p(v_1,v_2)}{\partial v_2} = 0$, the formulation provided in equation (1) is maintained. Otherwise, probabilistic completion can create a wedge, influencing choice and biasing resulting inference on time preference if

equation (1) is assumed.

The challenge created by probabilistic completion in settings like ours can be overcome with additional assumptions of functional form and internal consistency. Provided a functional form for $p(v_1, v_2)$, we assume vaccinators know the correct mapping,

$$p(v_1, v_2) = p^*(v_1, v_2),$$

where $p^*(v_1, v_2)$ is the true completion probability induced by a given allocation (v_1, v_2). The researcher observes either success or failure as draws from the distribution $p^*(v_1, v_2)$.[24] To provide a functional form for $p(v_1, v_2)$, we assume that the probability of completing a target of v on day 1 or 2 is

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

Provided $\alpha > 0$, this completion function assumes that success is assured at $v = 0$ and diminishes as v increases. As such $p(v_1, v_2) = \frac{1}{1+\alpha v_1}\frac{1}{1+\alpha v_2}$.

Under such probabilistic completion and internal consistency, two moment conditions obtain:

$$\gamma v_1^{\gamma-1} - \frac{\beta^{\mathbf{1}_{d=1}}\delta}{R}\gamma v_2^{\gamma-1} - \left(\frac{-\alpha}{(1+\alpha v_1)} - \frac{1}{R}\frac{-\alpha}{(1+\alpha v_2)}\right)[\delta^2 u(1000) - v_1^\gamma - \beta^{\mathbf{1}_{d=1}}\delta \cdot v_2^\gamma] = 0, \quad (2)$$

$$\frac{1}{1+\alpha v_1}\frac{1}{1+\alpha v_2} - p^*(v_1, v_2) = 0. \quad (3)$$

Again, standard minimum distance methods can be applied to simultaneously estimate the parameters of $p(v_1, v_2)$ and the discounting parameters of interest.[25] In effect, imposing internal consistency on completion rates allows the researcher to quantify the wedge induced by consid-

[24] Hence, the function $p(v_1, v_2)$, known to the vaccinator, can be recovered from choice and observed success. It is as if $p(v_1, v_2)$ represents the physical possibility of achieving a given allocation. Given that we assume all vaccinators 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.

[25] Considering completion alone, equation (3) could be estimated with non-linear least squares in a similar way to linear probability models with ordinary least squares. Though, in principle, one might predict completion probabilities outside of the bounds [0,1], in practice this does not occur.

ering 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.[26]

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, $[\delta^2 u(1000) - v_1^\gamma - \beta^{\mathbf{1}_{d=1}}\delta \cdot v_2^\gamma]$, can be set to any number with suitable definition of $u(1000)$. Of course, for allocations to carry any information, an obvious participation constraint, $[\delta^2 u(1000) - v_1^\gamma - \beta^{\mathbf{1}_{d=1}}\delta \cdot v_2^\gamma] \geq \delta^2 u(0) - v_{1,n}^\gamma - \beta^{\mathbf{1}_{d=1}}\delta \cdot v_{2,n}^\gamma = 0$, needs to be satisfied.[27] To understand how slack this constraint was, we asked our vaccinators 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 $[\delta^2 u(1000) - v_1^\gamma - \beta^{\mathbf{1}_{d=1}}\delta \cdot v_2^\gamma]$ may be only slightly higher than the normalized non-participation value of zero. When assessing probabilistic completion, we set $[\delta^2 u(1000) - v_1^\gamma - \beta^{\mathbf{1}_{d=1}}\delta \cdot v_2^\gamma] = 100$.[28]

The above development delivers aggregate estimates of discounting parameters with each vaccinator's allocation contributing a single observation to the aggregate. Exercises exploring heterogeneity in time preferences document substantial differences across people, even from relatively homogeneous populations (see e.g., Harrison et al., 2002; Ashraf et al., 2006; Meier and Sprenger, 2015). Given only a single observation per vaccinator, estimation of all parameters at the individual level is infeasible. However, we can calculate each vaccinator's discount factor, which is either δ_i for those who make Advance decisions or $(\beta\delta)_i$ for those who make Immediate decisions. To make such a calculation, two further structural assumptions are required. First, we assume every vaccinator shares a common cost function, $\gamma = 2$, corresponding to quadratic

[26] Naturally, the predictions may be sensitive to the imposed functional form of $p(v_1, v_2)$. As such, in subsection 4.2 we discuss several alternative forms for $p(v_1, v_2)$.

[27] Otherwise the individual would want to set v_1, v_2 to increase the probability of non-completion.

[28] In subsection 4.2 we provide sensitivity analysis for changes to this assumed value.

cost. Second, when assessing probabilistic completion we assume a common completion function, $p(v_1, v_2)$, evaluated using the aggregate estimated completion parameter, α. Third, we assume relevant marginal conditions (equation (1) or equation (2) in the case of probabilistic completion) hold exactly. Let R_i be the value of R assigned to individual i, let $\mathbf{1}_{d=1,i}$ be their assignment to Advance or Immediate choice, and let $(v_{1,i}, v_{2,i})$ be their allocation of vaccinations. Without considerations related to probabilistic completion,

$$\frac{R_i \cdot v_{1,i}}{v_{2,i}} = (\beta^{\mathbf{1}_{d=1,i}}\delta)_i, \tag{4}$$

the interest rate-adjusted ratio of allocated vaccinations identifies a discount factor for each individual, i. Under our formulation of probabilistic completion, this becomes

$$\frac{R_i \cdot \left(v_{1,i} - \left(\frac{-\alpha}{(1+\alpha v_{1,i})} - \frac{1}{R}\frac{-\alpha}{(1+\alpha v_{2,i})}\right)[100]\right)}{v_{2,i}} = (\beta^{\mathbf{1}_{d=1,i}}\delta)_i. \tag{5}$$

The structural assumptions required for identification of aggregate and individual discount factors are potentially quite restrictive. Our research design, which involves tailoring contracts to individual discount factors, required commitment to the specific functional forms of equations (1) and (4). As noted above, imperfect completion was not an issue that we had forecast and so our tailored contracts do not focus on achieving target completion rates. However, our tailoring exercise does yield clear auxiliary predictions for patterns of completion across groups. As such, we assess these predictions alongside those for allocation behavior. Not being pre-specified ex-ante, these analyses should be viewed as exploratory. In sub-section 4.2, we also assess the validity of a set of required assumptions and present further exploratory analysis related to alternative functional forms.

2.3 Tailored Contracts

Each vaccinator's allocation in an intertemporal bonus contract identifies her discount factor for vaccinations, either δ_i for those who make Advance decisions or $(\beta\delta)_i$ for those who make

Immediate decisions. We consider a policymaker who knows such preferences and wishes to achieve a specific policy objective. The policymaker has only one policy lever: manipulation of the interest rate, R_i, at the individual level. We formalize the problem as maximizing policy preferences, $Q(v_{1,i}(R_i), v_{2,i}(R_i))$, subject to the vaccinator's offer curve. The problem is stated as

$$max_{R,i}\ Q(v^*_{1,i}(R_i), v^*_{2,i}(R_i)),$$

where $(v^*_{1,i}(R_i), v^*_{2,i}(R_i))$ are defined as the solution to the vaccinator's minimization problem noted above. The solution maps the policy preferences into an interest rate for each vaccinator. One can consider many potential forms of policy preference, with policymakers desiring a variety of intertemporal patterns of effort. As proof-of-concept, we consider first a policy maker with one extreme form of preference, $Q(v_{1,i}(R_i), v_{2,i}(R_i)) = min[v_{1,i}(R_i), v_{2,i}(R_i)]$.[29] Such Leontief preferences correspond to a policymaker who desires perfectly smooth provision of service. This problem has an intuitive solution under perfect completion. The worker's intertemporal Euler equation (4) yields smooth allocations and provision, $v_{1,i} = v_{2,i}$, when $R_i = (\beta^{\mathbf{1}_{d=1,i}}\delta)_i$. Hence, the tailored contracts give each vaccinator a value of R equal to their discount factor defined in equation (4). Note that the structural discounting parameters are critical in this development. With information on discount factors, contracts can be tailored for each worker to achieve specific policy objectives.

In a second two-day drive, we investigate the promise of tailored contracts. All vaccinators from the first drive were invited to participate in a second intertemporal bonus contract. Vaccinators were unaware that their previously measured behavior would be used to potentially inform their subsequent contracts. This sidesteps an important possibility that vaccinators might alter their first drive behavior in order to receive a more desirable interest rate in the second drive.

[29]The ability of our data to speak to alternative policy preferences is discussed in section 4.3.6. Leontief preferences in this environment are extreme, but there is general interest in understanding mechanisms to drive smooth behavior, particularly for saving and for avoiding procrastination.

Half of vaccinators were given an individually tailored intertemporal bonus contract,

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

where R_i^* is defined as in equation (4), either $(\beta\delta)_i$ or δ_i depending on whether they made Immediate or Advance decisions.[30] Some vaccinators' allocation behavior in the first drive implied extreme discount factors and hence extreme values of R_i^*. Our tailoring exercise focused only on a Tailoring Sample of vaccinators with discount factors between 0.75 and 1.5.[31] Vaccinators outside of these bounds were given either the upper or lower bound accordingly.

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

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

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 groups for tailored interest rates by drawing subsets of vaccinators assigned to the $\tilde{R}_i$ condition (see section 4.2.3 for details). We find our results are robust to a range of comparison groups.

Random assignment to tailoring in Drive 2 is stratified on the measure of absolute distance to equal provision $|\frac{v_1}{v_2} - 1|$, based on allocations from Drive 1.[32] This measure of distance to equal provision also serves as our eventual outcome measure when analyzing the effect of assignment to 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.

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

[31] Of our sample of 338 vaccinators, 57 exhibit discount factors outside of this range. The Tailoring Sample consists of the remaining 281 vaccinators.

[32] 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.

Recognizing imperfect completion generates nuanced predictions in the tailored drive. First, providing the tailored interest rate based on equation (4) is no longer predicted to exactly equalize $v_{1,i}$ and $v_{2,i}$. Rather it leads to a prediction adjusted for marginal completion probabilities as in equation (5). In practice, this difference is quite small under our estimated completion function. As such, the policy target of equal provision is effectively unchanged by examining imperfect completion. Second, and more importantly, the tailored policy assigns more impatient subjects lower values of R_i^*. Given the present-value budget constraint $v_{1,i} + R_i^* \cdot v_{2,i} = V$, more impatient individuals are required to do weakly more work than less impatient ones. In effect, impatient vaccinators 'pay' for their impatience with a lower R and a corresponding increase in total work. Given that $p(v_1, v_2)$ is decreasing in its arguments, more impatient vaccinators should be less likely to succeed under the tailored policy regime. Indeed, we can assess the predictive accuracy of our estimated model for patterns of failure and the relationship between completion, contract terms, and preferences across tailored and untailored groups.

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.

2.4.1 Training and Allocation Decisions

On November 7, all vaccinators participating in the November 10-11 drive received two hours of training at one of three locations in central Lahore on using the monitoring features of the smartphone application. Both Advance and Immediate vaccinators were given identical training on the intertemporal bonus contracts and the process by which allocations were made and submitted.

At the end of the training, vaccinators 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. Vaccinators assigned to Immediate decision 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 vaccinators had previously been trained on the smartphone application, this portion of the training was conducted as a refresher.

2.4.2 Experimental Timeline

Figure 2 summarizes our experimental timeline and the sample for each vaccination drive of our study.

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 vaccinators 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 apparently a disruption in the mobile network that prevented 82 of 168 Immediate decision vaccinators 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. Figure 2 provides sample details.[33] For completeness, we present data from Drive 0 in Appendix Table A.2, but do not use Drive 0 for the purposes of tailoring contracts.

Drive 1, November 7-11, 2014: Of the original 336 vaccinators in our failed drive, 57 did not

[33]Appendix Table A.1 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.

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 vaccinators to participate in the intertemporal bonus program. The entire sample was re-randomized into interest rate and allocation timing conditions. 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 vaccinators 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 vaccinators in the Advance Choice condition provided an allocation decision, only 164 of 175 in the Immediate Choice condition provided an allocation. Because 11 vaccinators attrited from the Immediate Choice condition, we also provide bounds on the estimated effect of decision timing using the method of Lee (2009). In addition, for 232 vaccinators, we have allocation decisions in both the failed drive, Drive 0, and Drive 1, forming a potentially valuable panel of response. Figure 2 provides sample details.

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

2.4.3 Sample Details

Table 1 summarizes our sample of vaccinators from Drive 1 and provides tests of experimental balance on observables. Column (1) presents the mean and standard deviation for each variable; columns (2) to (9) present the mean and standard error for each of our eight treatment arms, and column 10 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

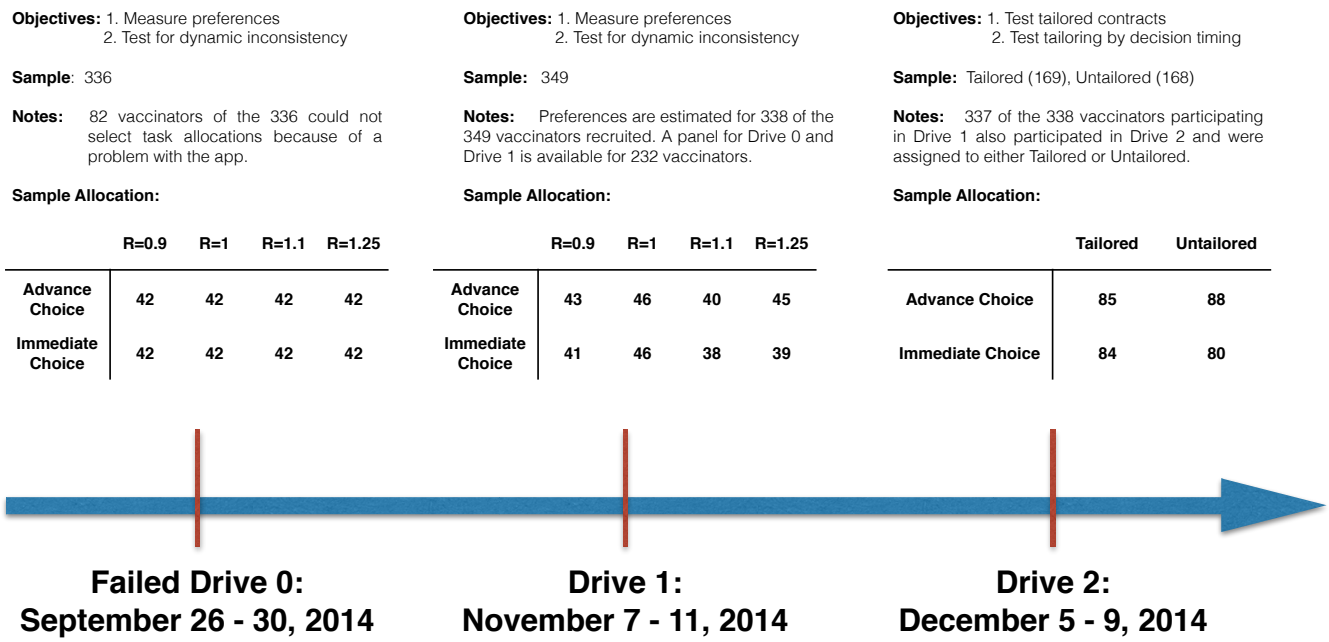

Figure 2: Experiment Overview

Notes: This figure provides an overview of the timing and sample breakdown of the experiment. Assignment to the advance choice and immediate choice condition in Drive 2 is inherited from vaccination Drive 1. Note that: (i) 57 vaccinators participated only in Failed Drive 0; (ii) 6 vaccinators participated in Drive 1 only; (iii) 1 vaccinator participated in Failed Drive 0 and Drive 1, but not in Drive 2 (iii) 67 vaccinators participated in drives 2 and 3 only; (iv) 271 vaccinators participated in all three rounds.

access to formal savings accounts. Vaccinators 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, only the test performed on an indicator variable equal to one for Punjabi subjects suggests baseline imbalance.

Table 1: Summary Statistics and Covariates Balance

	Full	Advance Decision				Immediate Decision				p-value
	Sample	R=0.9	R=1	R=1.1	R=1.25	R=0.9	R=1	R=1.1	R=1.25	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Demographics										
Gender (Female = 1)	0.985	1.000	1.000	1.000	0.978	0.975	0.978	0.947	1.000	0.284
	[0.121]	(0.000)	(0.000)	(0.000)	(0.022)	(0.025)	(0.022)	(0.037)	(0.000)	
Years of Education	10.415	10.767	10.652	10.650	10.279	9.850	10.565	10.184	10.282	0.500
	[2.291]	(0.416)	(0.273)	(0.462)	(0.330)	(0.298)	(0.368)	(0.238)	(0.395)	
Number of Children	3.424	3.419	3.422	3.538	3.286	3.605	3.391	3.421	3.333	0.997
	[1.826]	(0.279)	(0.301)	(0.309)	(0.296)	(0.286)	(0.274)	(0.243)	(0.294)	
Punjabi (=1)	0.952	0.930	0.932	1.000	0.955	0.950	0.978	0.917	0.947	0.022
	[0.215]	(0.039)	(0.038)	(0.000)	(0.032)	(0.035)	(0.022)	(0.047)	(0.037)	
Financial Background										
Has a Savings Account (=1)	0.269	0.310	0.250	0.275	0.302	0.350	0.283	0.189	0.179	0.630
	[0.444]	(0.072)	(0.066)	(0.071)	(0.071)	(0.076)	(0.067)	(0.065)	(0.062)	
Participated in a ROSCA (=1)	0.389	0.349	0.378	0.425	0.350	0.500	0.289	0.351	0.487	0.482
	[0.488]	(0.074)	(0.073)	(0.079)	(0.076)	(0.080)	(0.068)	(0.079)	(0.081)	
Health Work Experience										
Years in Health Department	10.520	10.605	10.578	10.211	11.549	9.050	10.678	10.395	11.026	0.456
	[4.961]	(0.777)	(0.695)	(0.685)	(0.792)	(0.695)	(0.846)	(0.867)	(0.808)	
Years as Polio Vaccinator	10.428	10.209	10.728	11.050	11.143	9.238	9.935	10.447	10.692	0.581
	[4.727]	(0.758)	(0.689)	(0.668)	(0.743)	(0.689)	(0.713)	(0.858)	(0.751)	
# Vaccinators	338	43	46	40	45	41	46	38	39	

Notes: This table checks balance across the eight treatment groups. Column 1 presents the mean for each variable based on our sample of 338 vaccinators. These 338 vaccinators comprise the estimation sample in Table 2, which reports tests of dynamic inconsistency. Standard deviations are in brackets. Columns 2 to 9 report the mean level of each variable, with standard errors in parentheses, for each treatment cell. For each variable, Column 10 reports the p-value of a joint test that the mean levels are the same for all treatment cells (Columns 2–9). The last row presents the number of observations in each treatment condition. A ROSCA is an informal Rotating Savings and Credit Association. Some calculations used a smaller sample size due to missing information. The proportion of subjects with missing information for each variable is never greater than 3.5 percent (8 vaccinators did not report whether they had participated in a ROSCA).

3 Results

We first report results related to the elicitation of intertemporal preference parameters, then evaluate the possibility of tailoring incentives based on individual preferences.[34]

3.1 Elicitation of Time Preferences

3.1.1 Aggregate Behavior

Figure 3 presents median behavior in the elicitation phase of our experiment, graphing the allocation to the sooner work date, v_1, for each interest rate.[35] Separate series are provided for Advance and Immediate choice. In Panel A we provide data for our Full Sample of 338 vaccinators who provided allocations in Drive 1. In Panel B we focus only on our Tailoring Sample of 281 vaccinators, trimming 57 vaccinators with extreme allocation behavior that would imply individual discount factors from equation (4) outside of the range of [0.75, 1.5]. Two features of Figure 3 are notable. First, subjects appear to respond to the between-subject variation in interest rate. As the value R increases, vaccinations allocated to v_1 count relatively less towards reaching the two-day target of $V = 300$. Vaccinators respond to this changing incentive by reducing their allocation of v_1. Second, there is a tendency of present bias. Vaccinators appear to allocate fewer vaccinations to v_1 when making Immediate choice.

Also graphed in Figure 3 are patterns of completion across experimental conditions in Drive 1. We determine completion by examining the records obtained from each vaccinator's cell phone application. Of 338 vaccinators in Drive 1, 288 registered activity in their cell phone application during the drive, while 50 generated no data. The cellular network in Lahore

[34] In addition, to test just the effect of providing the \$10 bonus, we randomly assigned 85 vaccinators in Drive 0 to carry a phone but not receive an incentive. 72 of these vaccinators also participated in Drive 1, retaining the same 'phone only' treatment status. In Drive 1, vaccinators in the 'phone only' group attempted 169.47 vaccinations (s.e. = 15.98) and vaccinators in the phone plus incentives group attempted 205.82 vaccinations (se = 7.79) yielding an estimated increase of 36.35 attempts (s.e. = 18.42, p = 0.05). 49.3% of vaccination attempts were successful for the 'phone only' group while 49.1% of vaccinations were successful for the 'phone plus incentives' group. The difference in success rates between the two groups is small (0.2 percentage points) and statistically insignificant (p=0.69).

[35] We opt to provide medians as the average data are influenced by several extreme outliers in allocation behavior. Qualitatively similar patterns are, however, observed.

is known to have some coverage gaps. As such, we consider a subject to have successfully completed their work if they completed an average of 90% or more of their required tasks.[36] One-hundred seventy-four (51.5%) subjects successfully completed by this measure. Appendix Figure A.6 presents the histogram of average completion percentages across subjects, showing a bimodal distribution of success and failure. Successful completion seems largely unrelated to assigned interest rate in Drive 1. Interestingly, however, subjects assigned to Immediate choice conditions seem to complete at lower rates than their Advance choice counterparts. This evidence is additionally supportive of a present-biased interpretation. Subjects in the Immediate choice condition postpone more work, which they are subsequently unable to satisfactorily complete.

Table 2 presents corresponding median regression analysis for aggregate behavior in Drive 1.[37] In Panel A, We regress v_1 on R and whether the allocation decision is immediate. Column (1) echoes the findings from Figure 3, Panel A: in our Full Sample, vaccinators assigned to Immediate choice allocate a median of 2.00 (s.e. = 1.13) fewer vaccinations to v_1 than those assigned to Advance choice. Similar patterns are observed in column (2), focusing only on our Tailoring Sample. Vaccinators in the Tailoring Sample allocate a median of 3 fewer vaccinations to v_1 when making immediate choice.[38] In Panel B, we repeat these analyses with linear probability models and an indicator for completion, $Complete(=1)$, as dependent variable. As in Figure 3, we find no discernible relationship between assigned interest rate and completion. However, individuals assigned to Immediate choice are between 9 and 13 percentage points less likely to satisfactorily completed their allocated vaccinations.

[36] Average completion rates are calculated as $1/2(min(Completed_1/v_1, 1) + min(Completed_2/v_2, 1))$

[37] Appendix Table A.2 presents identical analysis incorporating data from failed Drive 0, and identifies qualitatively similar effects.

[38] As discussed in Section 2.4.2 above, 11 vaccinators attrited from the sample in the immediate choice condition in Drive 1. Bounding the effect of being assigned to the immediate choice condition on v_1 allocations using the method of Lee (2009) provides a lower bound of -3.78 tasks (s.e. = 2.06) and an upper bound of 0.205 (2.06) tasks.

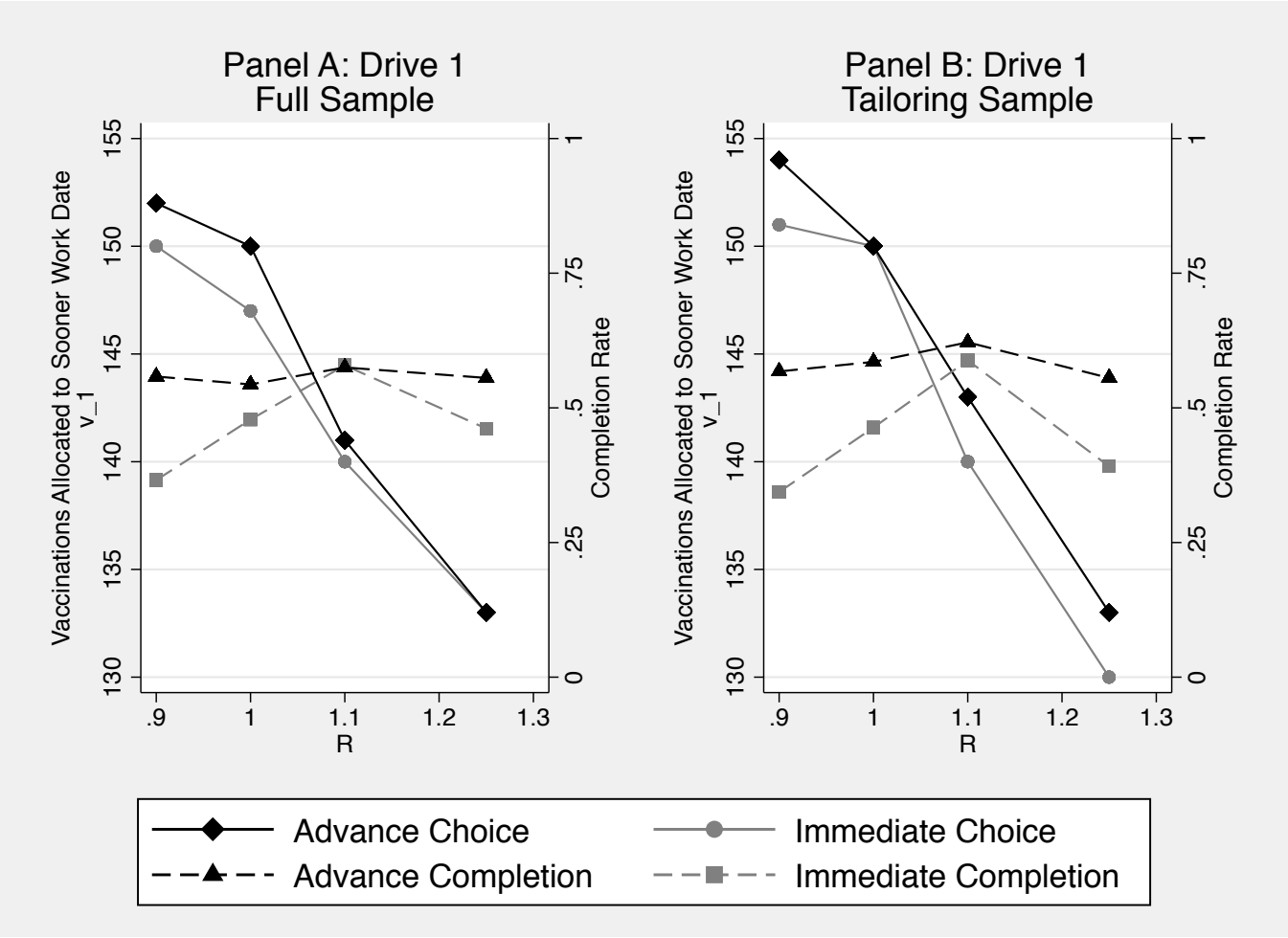

Figure 3: Aggregate Experimental Response

Notes: This figure examines whether tasks assigned to the sooner work date and completion respond to the experimental variation in the interest rate R and in decision timing. Allocation data represent medians for each of the eight treatment groups and completion data represent group averages. Panel A depicts the Full Sample and Panel B depicts the tailoring sample (vaccinators with $R^* < 0.75$ or $R^* > 1.5$). Black series are advance choice groups and gray series are immediate choice groups.

Table 2: Aggregate Drive 1 Behavior

	(1) Full Sample	(2) Tailoring Sample
Panel A: Dependent Variable: v_1		
Immediate Decision (=1)	-2.00*	-3.00***
	(1.13)	(0.91)
Interest Rate (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
Panel B: Dependent Variable: Completed (=1)		
Immediate Decision (=1)	-0.087	-0.128**
	(0.054)	(0.060)
Interest Rate (R)	0.143	0.095
	(0.210)	(0.241)
Constant	0.405*	0.483*
	(0.227)	(0.258)
Average Completion Rate	0.515	0.523
# Observations	338	281

Notes: This table reports on the effects of decision timing and interest rate variation on vaccinations allocated to the first day of the drive Panel B: Linear probability model for completion. Standard errors in parentheses. *Levels of Significance:* $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

3.1.2 Aggregate Preference Parameters

The raw data of Figure 3 and analysis of Table 2 indicate responsiveness of vaccinators to our experimental parameters, R and whether allocations are Immediate or Advance. The development of section 2 links allocation behavior and completion to these experimental parameters via structural models of choice. In Table 3, we present parameter estimates from minimum distance estimates from equation (1) or equations (2) and (3) when considering probabilistic completion.

In Panel A of Table 3, we present estimates for the Full Sample under varying assumptions for γ. In columns (1) and (2), we restrict to $\gamma = 2$, providing an aggregate benchmark for our individual analysis which calculates individual discount factors under the assumption of quadratic costs. Without controlling for probabilistic completion, we find $\beta = 0.935$ (s.e. $= 0.030$), and reject the null hypothesis of no present bias ($\chi^2(1) = 4.841$, ($p = 0.028$). Simultaneously estimating probabilistic completion increases the point estimate for both β and δ, such that the extent of present bias falls outside of the range of standard statistical significance ($p = 0.104$). The key completion parameter α is estimated precisely to be 0.003, such that an individual assigned $R = 1$ who allocates 150 vaccinations to each date would be expected to complete with probability around 0.50. A similar pattern is observed in Panel B for the Tailoring Sample. The parameter β is estimated to be less than one, at the cusp of statistical significance when controlling for probabilistic completion.

As the assumed degree of curvature is increased in Table 3, columns (3) through (8), both β and δ decrease, but the general conclusions are maintained. A measure of model fit, the criterion value, does tend to improve as γ is increased. However, increasing γ further to 3.5 generates a sharp change in the quality of fit and the completion parameter α is estimated to be negative, inconsistent with our assumption that $p(v_1, v_2)$ is declining in its arguments.[39] In principle, variation in the interest rate R, should provide an opportunity to identify γ without restriction. Unfortunately, our minimum distance estimators did not reliably converge without

[39]Results available upon request.

restrictions. This highlights a potentially important issue with respect to the estimates of Table 3: the estimated parameters predict more sensitivity to R than truly exists in the data.[40] This mis-specification presents a clear challenge for using individual preference parameters for tailored contracts. Having committed to a possibly mis-specified functional form ex-ante, any success in tailoring contracts should likely be viewed as a lower bound on the potential benefits of such initiatives.

Table 3: Aggregate Parameter Estimates, Drive 1

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\gamma = 2$		$\gamma = 2.5$		$\gamma = 3$		$\gamma = 3.25$	
Panel A: Full Sample								
β	0.935	0.952	0.922	0.946	0.906	0.938	0.896	0.934
	(0.030)	(0.029)	(0.040)	(0.039)	(0.050)	(0.050)	(0.055)	(0.055)
δ	0.985	0.992	0.958	0.967	0.932	0.942	0.919	0.931
	(0.017)	(0.017)	(0.022)	(0.022)	(0.027)	(0.027)	(0.029)	(0.030)
α		0.003		0.003		0.003		0.003
		(0.000)		(0.000)		(0.000)		(0.000)
$H_0 : \beta = 1.$ $(\chi^2(1))$	4.841	2.637	3.889	1.856	3.600	1.531	3.609	1.449
[p-value]	[0.028]	[0.104]	[0.049]	[0.173]	[0.058]	[0.216]	[0.057]	[0.229]
Criterion Value	0.278	0.310	0.217	0.249	0.180	0.212	0.166	0.199
# Vaccinators	338	338	338	338	338	338	338	338
Panel B: Tailoring Sample								
β	0.969	0.970	0.962	0.963	0.954	0.955	0.949	0.951
	(0.018)	(0.018)	(0.023)	(0.023)	(0.028)	(0.028)	(0.031)	(0.031)
δ	1.017	1.018	1.003	1.004	0.990	0.991	0.984	0.985
	(0.013)	(0.013)	(0.016)	(0.016)	(0.019)	(0.019)	(0.020)	(0.020)
α		0.002		0.003		0.003		0.003
		(0.000)		(0.000)		(0.000)		(0.000)
$H_0 : \beta = 1.$ $(\chi^2(1))$	2.880	2.713	2.716	2.575	2.729	2.592	2.754	2.618
[p-value]	[0.090]	[0.100]	[0.099]	[0.109]	[0.099]	[0.107]	[0.097]	[0.106]
Criterion Value	0.370	0.384	0.268	0.284	0.196	0.213	0.170	0.186
# Vaccinators	281	281	281	281	281	281	281	281

Notes: This reports structural estimates of β, δ, and α obtained using minimum distance estimation of equations (1) in even columns or (2) and (3) in odd columns. Standard errors are reported in parentheses. Test statistic for $\beta = 1$ with p-value in brackets. Panel A provides estimates for the Full Sample, Panel B provides estimates for the Tailoring Sample.

[40] Appendix Figure A.8 reproduces Figure 3, with in-sample predictions from Table 3, column (2). Though the estimates do match the responsiveness of behavior from $R = 1$ to $R = 1.1$, they do not generate the lack of sensitivity for other changes in R.

3.1.3 Individual Preference Parameters

The aggregate estimates of Table 3 mask substantial heterogeneity across subjects. Following equations (4) and (5), we calculate individual discount factors for each vaccinator assuming quadratic costs. For those vaccinators assigned to Advance choice, this discount factor corresponds to δ_i, while for those assigned to Immediate choice it corresponds to $(\beta\delta)_i$. Without accounting for completion In Drive 1, the median [25th, 75th percentile] discount factor in Advance choice is 1.015 [0.88, 1.18], while the median discount factor in Immediate choice is 1 [0.84, 1.21]. Accounting for completion, the median [25th,75th percentile] discount factor in Advance choice is again 1.015 [0.88, 1.18], while the median discount factor in Immediate choice is again 1 [0.84, 1.21]. The correlation in discount factors with and without accounting for completion is effectively 1, indicating probabilistic completion does not dramatically confound any individual inferences. Indeed, the difference between the implied discount factor with and without accounting for completion has a median [25th-75th %-ile] value of 0.00004 [-0.0002, 0.0001]. Both discount factors are used in our analysis with the relevant calculation noted.

As noted above, an important minority of vaccinators have extreme discount factor calculations. Without accounting for completion, fifty-seven of 338 subjects in Drive 1 have implied discount factors either above 1.5 or below 0.75.[41] We term such vaccinators the 'Boundary Sample.' As our tailoring exercise focuses on individuals with discount factors between 0.75 and 1.5, we restrict our individual analysis to the 281 vaccinators in the Tailoring Sample, and discuss the Boundary Sample in robustness tests (see section 4.2). Figure 4 presents histograms of implied discount factors for the 280 Tailoring Sample vaccinators in Advance and Immediate decisions. Two features are notable. First, in both contexts substantial heterogeneity in discount factors is observed. The 25th to 75th percentile ranges from 0.92 to 1.15 in Advance choice and from 0.88 to 1.15 in Immediate choice. Second, a present bias is observed in the shape of the distributions. The one period discount factors are skewed below 1 in Immediate

[41] Such extreme behavior is slightly more pronounced in Immediate choice (34 vaccinators) relative to Advance choice (23 vaccinators), ($t = 1.84,\ p = 0.07$).

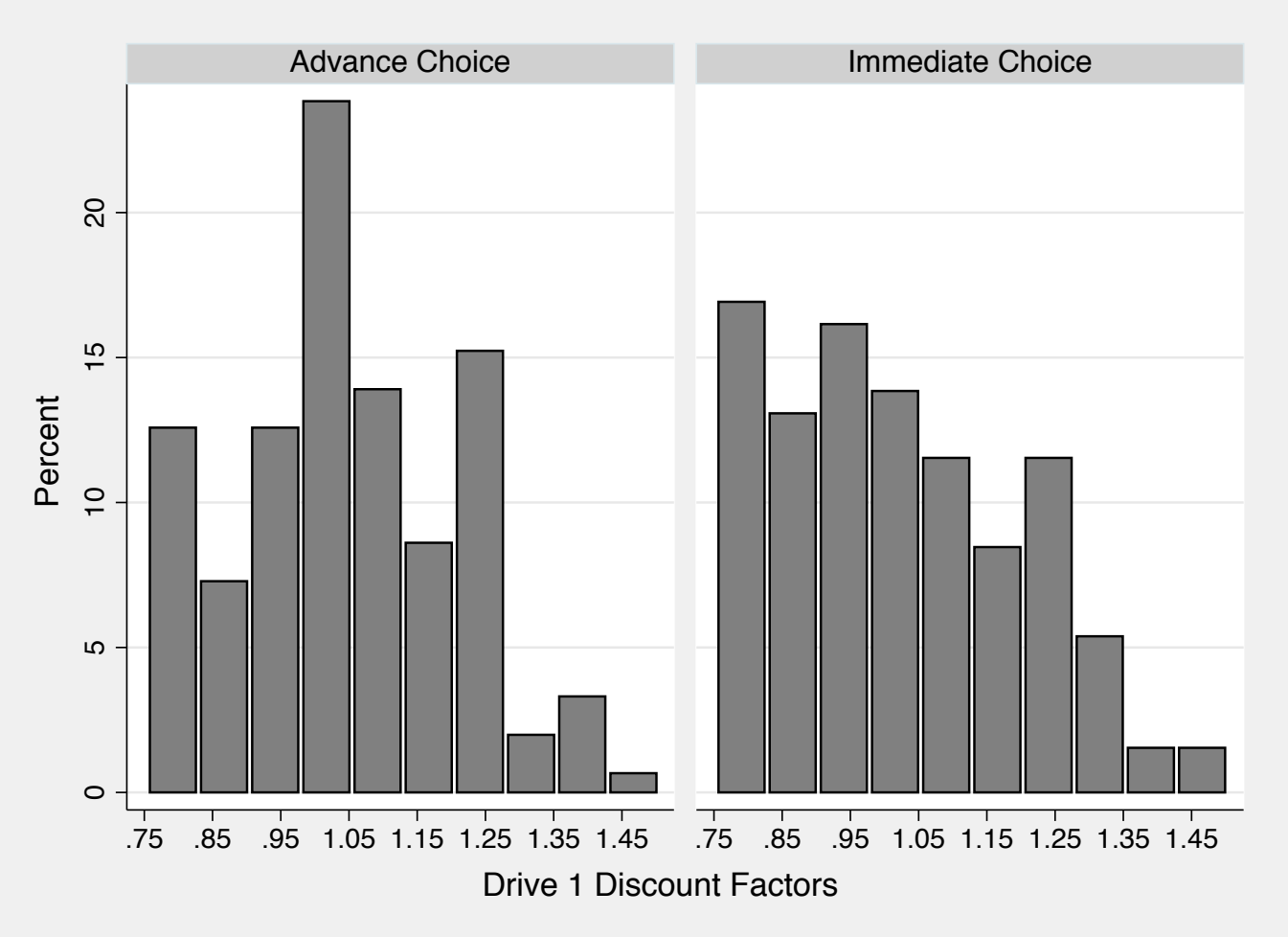

Figure 4: Individual Discount Factors in the Tailoring Sample

Notes: This figure provides histograms of one period discount factors calculated from equation (4) separately for subjects in the Advance Choice condition (left panel) and the Immediate Choice condition (right panel). The sample is restricted to vaccinators in the Tailoring Sample (vaccinators with $R_i^* \geq 0.75$ or $R_i^* \leq 1.5$). Calculating discount factors from equation (5) accounting for probabilistic completion yields an identical figure.

relative to Advance choice. A Kolmogorov-Smirnov test for equality of distributions sits at the cusp of statistical significance ($D_{KS} = 0.15,\ p = 0.10$).

The observed heterogeneity in discount factors across vaccinators resonates with prior exercises demonstrating heterogeneity of preferences even with relatively homogeneous samples (see e.g., Harrison et al., 2002; Ashraf et al., 2006; Meier and Sprenger, 2015). Further, this heterogeneity is precisely the reason there is promise in tailoring contracts individually.

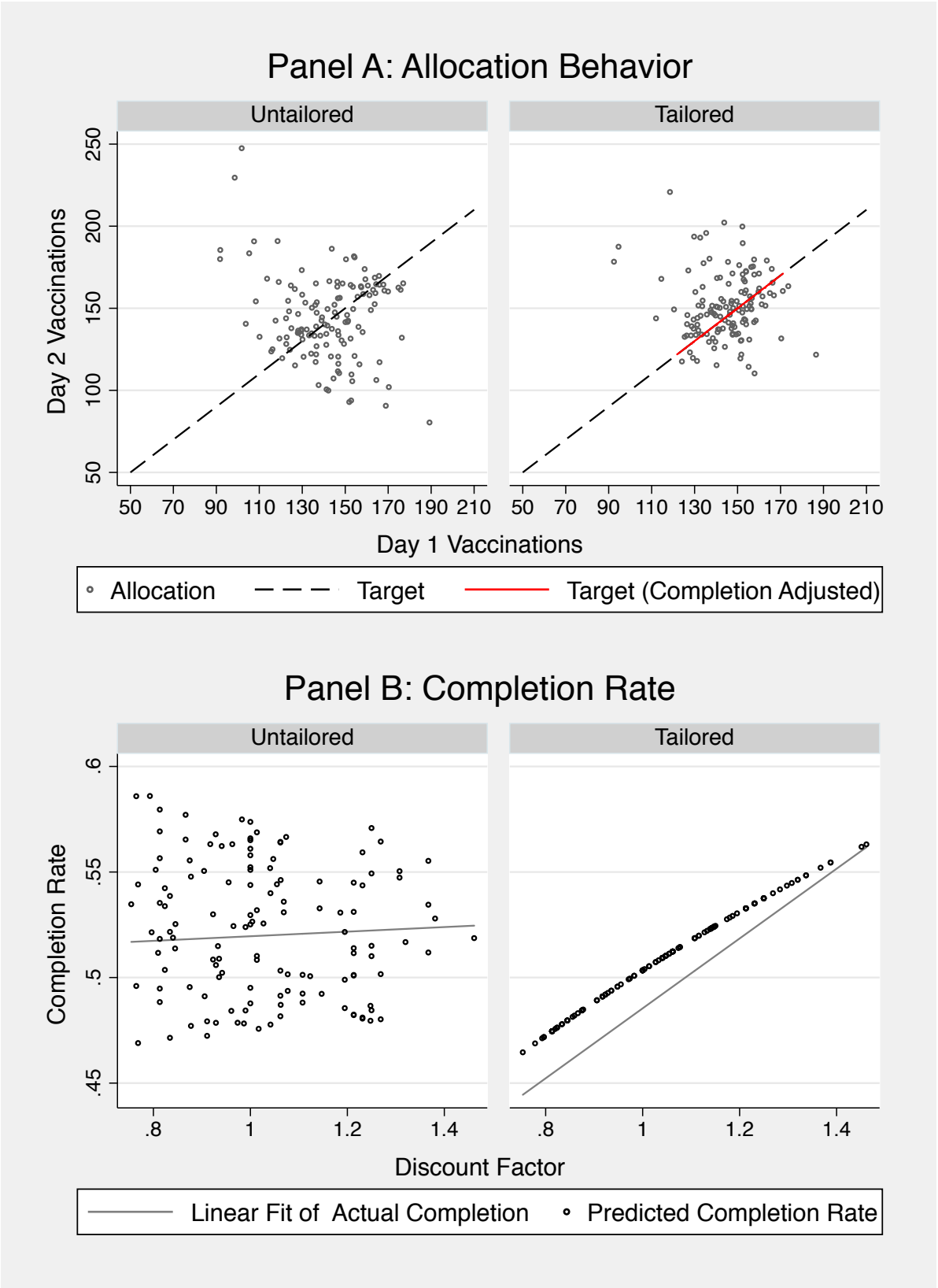

Figure 5: Discounting and Tailoring

3.2 Tailored Contracts

Individual discount factors from Drive 1 in hand, we turn to the possibility of tailoring intertemporal contracts to individual preferences. Of the 281 vaccinators in the Tailoring Sample, 280 participated in Drive 2.[42] Of these, 142 vaccinators were assigned a value of R equal to their discount factor. That is, tailored vaccinators were assigned $R_i^* = (\beta^{\mathbf{1}_{d=1,i}}\delta)_i$, from equation (4), which should induce equal provision of effort through time, $v_{1,i} = v_{2,i}$. Given the minute differences between implied discount factors with and without controlling for completion, this policy target is effectively identical regardless of whether the discount factor from equation (4) or equation (5) is used. The remaining 138 vaccinators serve as control and were assigned a uniform random interest rate $\tilde{R}_i \in U[0.75, 1.5]$.[43]

Figure 5, Panel A plots vaccinations allocated to the first day of the drive against vaccinations allocated to the second day of Drive 2 separately for the tailored and the untailored group. Notable from Figure 5 is the relative dispersion of the untailored controls around the 45-degree line of equal provision relative to the tailored treatments. Also graphed in Panel A for the tailored group is the policy target accounting for probabilistic completion. This line is effectively indistinguishable from the 45 degree line such that whether one accounts for completion or not, the policy should induce smooth allocations.

We examine differences in the distance from the 45-degree line using the metric $|\frac{v_{1,i}}{v_{2,i}} - 1|$, the absolute percentage difference between v_1 and v_2. The mean distance for the untailored group is 0.61 (s.d. = 3.64) while the mean distance for the tailored group is 0.14 (s.d. = 0.23), $t_{278} = 1.54,\ (p = 0.13)$. The lack of statistical significance is due primarily to several substantial distance outliers. Trimming the top and bottom 1% of the sample of Drive 2 allocations, the mean distance for the untailored group is 0.15 (s.d. = 0.19), while the mean distance for the tailored group is 0.10 (s.d. = 0.11), $t_{265} = 3.07,\ (p < 0.01)$.

[42] Vaccinators from the boundary sample 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 section 4.2 for analysis of the boundary sample.

[43] 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.

In Table 4, we provide corresponding least squares regression analysis. Following best practice for such analysis (Bruhn and McKenzie, 2009), we control for fixed effects for each stratum in the stratified randomization. In column (1), we analyze all 280 subjects and note a sizable reduction in distance under tailoring that falls just outside the range of significance. Echoing our raw results, when excluding outliers in column (2), we find that tailoring serves to reduce distance from equal provision significantly by around six percentage points. Relative to the untailored controls, tailoring reduces distance from equal provision by around one-third, indicating substantial benefits to our tailored policy initiative. In column (3), we additionally control for the value of R_i^* or $\tilde{R}_i$ assigned in Drive 2. This regression identifies whether tailoring generates more equal provision for a given value of R, and hence controls for any differences in interest rates across tailored and untailored groups. Again, tailoring serves to reduce distance significantly.

Vaccinators 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 columns (3)-(6) of Table 4, we examine differential effects of tailoring across these two groups. Given that the individual discount factors skew lower in Immediate choice, one might expect larger distance measures in Immediate controls (and hence greater benefits to tailoring). This is precisely what is observed. Untailored Immediate choice is associated with significantly larger distance measures and tailoring for Immediate choice significantly reduces these distances. In columns (5) and (6), excluding outliers, we find that tailoring in Immediate choice reduces distance from equal provision by around one-half. Note that this effect size (8.4 percentage points) is similar to the effect of moving a vaccinator from advance to immediate choice in the untailored group (8 percentage points). Tailoring in Advance choice appears to directionally reduce distance as well, but the effect is not significant, potentially due to the relatively small average distance measure identified in untailored Advance choice.

In addition to allocation behavior, we can use individual discount factor measures from equation (5) in Drive 1 to predict completion probabilities out-of-sample for Drive 2. Our

Table 4: The Effect of Tailoring Intertemporal Incentives

Dependent variable:	$\left\lvert\frac{v_{1,i}}{v_{2,i}} - 1\right\rvert$					
	(1)	(2)	(3)	(4)	(5)	(6)
Tailored (=1)	-0.489	-0.059***	-0.049***	-0.070	-0.019	-0.014
	(0.321)	(0.019)	(0.018)	(0.069)	(0.019)	(0.019)
Immediate Choice (=1)				0.982*	0.125***	0.117***
				(0.573)	(0.035)	(0.035)
Tailored x Immediate				-0.888	-0.090**	-0.084**
				(0.604)	(0.040)	(0.040)
Constant	1.407	0.159***	0.022	0.873	0.094***	-0.004
	(0.860)	(0.023)	(0.058)	(0.552)	(0.026)	(0.057)
Stratum FEs	Yes	Yes	Yes	Yes	Yes	Yes
Exclude 99th and 1st Percentiles	No	Yes	Yes	No	Yes	Yes
Drive 2 R_i^* or $\tilde{R}_i$	No	No	Yes	No	No	Yes
R-Squared	0.035	0.060	0.082	0.053	0.142	0.154
Mean in Untailored Contract	0.612	0.153	0.153	0.612	0.153	0.153
Mean in Untailored Advance				0.089	0.089	0.089
Mean in Untailored Immediate				0.701	0.169	0.169
# Vaccinators	280	267	267	280	267	267

Notes: This table reports the effects of tailoring on the equality of effort provision over time. The measure $|\frac{v_t}{v_{t+1}} - 1|$ (the percentage difference between tasks allocated to day 1 and day 2 of the drive) reflects the distance of the task allocation (v_1,v_2) from equality ($v_1 = v_2$). Column (1) reports a regression of this measure on an indicator equal to one for subjects in the tailored group. Column (2) reports estimates from the same specification excluding outliers. Column (3) controls for the interest rate assignment in round 2. Column (4) provides estimates on the same sample as column (1) interacting treatment with being in the immediate choice condition. Columns (5) and (6) apply the same restrictions to the sample as columns (2) and (3) respectively. Ordinary least squares regressions. Heteroskedasticity robust White standard errors reported in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

tailored policy links individual discount factors to assigned interest rates. Impatient subjects 'pay' for their impatience with lower values of R_i^*. Given the intertemporal constraint $v_1 + R_i^* \cdot v_2 = 300$, this means less patient individuals will ultimately have to do weakly more work. Under our assumed functional form for $p(v_1, v_2)$ this implies a relationship between completion probabilities and patience. In the tailored group, less patient individuals should be substantially less likely to successfully complete their Drive 2 targets. In Figure 5, Panel B, we plot the predicted completion rate in Drive 2 against the individual discount factor. A tight relationship between patience and predicted completion exists in the tailored group, and no discernible relationship exists for the untailored group. In Panel B, we also plot the linear fit from the regression of realized Drive 2 completion against discount factors.[44] Closely matching the prediction, true completion probabilities are positively correlated with discount factors under the tailored contract terms, but are effectively unrelated to discount factors for the untailored group.

Table 5 provides corresponding logit regressions showing a significant positive relationship between discounting and completion only for the tailored group of subjects. Table 2 also provides actual and predicted completion rates separately for tailored and untailored subjects. The two groups are predicted to differ in their completion rates due to different contract terms[45], and they indeed do differ in the predicted direction. Overall, predicted and actual completion rates are significantly correlated, as those individuals who actually do fail were predicted to do so with higher probability, 0.521 (s.e. = 0.002) vs 0.509 (0.002), $t_{335} = 3.80,\ (p < 0.01)$.[46]

4 Robustness Tests and Additional Exercises

The analysis to this point indicates three key findings. First, there appears to be a present bias in vaccinator allocation behavior. Those individuals making Immediate choice allocate fewer

[44] Though based on a linear probability model, this fitted value is restricted to be the expectation conditional on being in the interval (0,1).

[45] Due to the random uniform assignment the untailored group has relatively more interest rates above 1.

[46] For the Tailoring sample alone, these values are 0.520 (s.e. = 0.003) vs 0.512 (0.002), $t_{278} = 2.30,\ (p < 0.05)$

Table 5: Tailoring, Discount Factors, and Completion

Dependent variable:	Drive 2 Completed (=1)			
	Untailored		Tailored	
	(1)	(2)	(3)	(4)
Drive 1 Discount Factor	0.693	0.152	2.506**	2.312**
	(1.049)	(1.086)	(1.043)	(1.069)
Constant	-0.533	0.118	-2.780**	-2.516**
	(1.082)	(1.131)	(1.120)	(1.150)
# Vaccinators	138	132	142	135
Log-Likeliihood	-94.908	-90.254	-95.022	-91.078
Exclude 99th and 1st Percentiles	No	Yes	No	Yes
Actual Completion Rate	0.543	0.568	0.458	0.474
Predicted Completion Rate	0.524	0.523	0.508	0.508

Notes: This table reports logit regressions for successful completion in Drive 2 on Drive 1 discount factor for tailored and untailored subjects. Individual discount factor calculated from equation (5) based on Drive 1 allocation. Predicted completion rate calculated as $p(\hat{v}_{1,i}, \hat{v}_{2,i})$ at predicted Drive 2 allocation $(\hat{v}_{1,i}, \hat{v}_{2,i})$. Heteroskedasticity robust White standard errors reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

vaccinations to v_1 than those making Advance choice. Second, despite the general tendency towards less patience in Immediate choice, substantial heterogeneity in discounting is observed. Both of these effects resonate with prior experimental findings and highlight the potential for policy interventions tailored to individual preferences. Third, tailored contracts work. Those individuals given a tailored interest rate equal to their previously measured discount factor provide smoother service than untailored controls. Furthermore, our tailored contracts generate additional predictions with regards to completion probabilities which are also borne out in the data. In the following sub-sections, we explore robustness to a set of plausible alternative interpretations and provide a set of natural additional examinations.

4.1 Repeated Measurement and Within-Subject Variation

In Drive 1, when relying on between subjects tests, statistical tests of present bias fall at the cusp of significance. Given the wide heterogeneity in observed patience regardless of decision timing,

one may fail to statistically identify present bias even if it exists on average. 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 vaccinators who changed from Advance to Immediate choice (or vice versa) across drives. Appendix Table A.3, reconducts the analysis of Table 3, columns (1) and (2) using this augmented data set. First, we analyze all potential observations, drawing from 622 choices made by 390 vaccinators in either Drive 0 or Drive 1.[47] There, we estimate $\beta = 0.903$ (clustered s.e. $= 0.030$) and reject the null hypothesis of no present bias at all conventional levels, $\chi^2(1) = 10.571,\ (p = 0.001)$ when controlling for probabilistic completion. The estimated degree of present bias corresponds closely with other recent estimates of working over time from laboratory studies Augenblick et al. (2015); Augenblick and Rabin (2015). Examining, only our panel of 232 individuals who participated in both Drive 0 and Drive 1, a similar estimate is obtained $\beta = 0.931$ (0.032), $\chi^2(1) = 4.497,\ (p = 0.034)$. This significant degree of present bias is driven by within-subject variation. For those individuals who transition from Advance to Immediate Choice or vice-versa across drives we find $\beta = 0.912$ (0.042), and reject the null hypothesis of no present bias $\chi^2(1) = 4.344,\ (p = 0.037)$. As with our aggregate estimates, when identifying only from between-subject variation, though present bias is of similar magnitude, precision remains an issue.

Repeated measurement from our panel of subjects provide for two additional analyses. First, the 126 subjects who change from Advance to Immediate choice provide an opportunity to investigate present bias at the individual level. Following equation (4), we calculate a discount factor for each condition the vaccinator faces. The parameter δ is identified as the discount factor from Advance choice while β is identified as the discount factor from Immediate choice divided by that of Advance choice. Interestingly, as in our analysis of discount factors, we find

[47] Only Drive 1 completion data is used to estimate the completion function given the noted cell network issues that generated the challenges for Drive 1.

δ is centered around 1 with a median value of 1.04, and that β is skewed below 1 with a median value of 0.95. Sixty-nine (54.8%) of 126 vaccinators have $\beta < 1$, 6 (4.8%) have $\beta = 1$, and 51 (40.5%) have $\beta > 1$. A sign test for the null hypothesis that the median β is equal to 1 yields a p-value of 0.12 (two-sided test).[48] Together, these results show that general patterns of present bias are observed at the aggregate and individual level when investigating only within-subject variation in a sub-sample of 126 vaccinators who change between Advance to Immediate choice across conditions. See Figure A.9 for graphical detail.

Second, for the 106 vaccinators who maintain their Advance or Immediate choice assignment, we have two discount factor measures from Drive 0 and Drive 1. One-hundred five of these subjects also participated in Drive 2. An important question for such subjects is what is the additional value of having a second measure of preferences. Are improved predictions of behavior made? To answer this question, we derive the predicted value of $v_{1,i}$ for the preferences identified in Drive 0 and Drive 1.[49] In Appendix Table A.4, we regress the actual allocation on predicted allocations separately for tailored and untailored subjects. For tailored subjects, there appears to be little additional value of incorporating the Drive 0 measures, while for untailored subjects the Drive 0 measures are even more predictive than those in Drive 1. In either case, the value added of additional measures is limited as the R-squared obtained with the single more predictive measure is over 95% of that obtained with both. This suggests that the marginal value of additional observations (particularly for those we attempt to tailor) declines rather sharply in this context.

[48] For the one-sided test with an alternative of $\beta < 1$, the p-value is 0.06. Excluding a single subject with β in excess of 19 reduces the two-sided (one-sided) p-value to 0.10 (0.05).

[49] Following equation (4), the predicted allocation to v_1 is calculated as

$$\hat{v}_{1,i} = \frac{\frac{(\beta^{\mathbf{1}_{d=1,i}}\delta)_i}{R^2}}{1 + \frac{(\beta^{\mathbf{1}_{d=1,i}}\delta)_i}{R^2}}.$$

4.2 Structural Assumptions

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

Assumption 1: Stationarity of the Cost Function: We assume the cost function is the same for day 1 and day 2. If sooner costs are forecasted to be more severe than later costs, vaccinators 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 measured by β is conflated with non-stationarity.

Importantly, our monitoring technology provides time-stamps and geo-stamps for vaccination activity. Time stamps are recorded 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 vaccinator, we identify the median time lapse between vaccination attempts and the median distance covered per 30 minute window each day.[50] Of our 338 vaccinators, measures for median time lapse between vaccination attempts are available for 277 on either Day 1 or Day 2 and for 228 vaccinators on both days of Drive 1.[51] Of our 338 vaccinators, measures for median distance traveled every 15 minutes

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

[51] 265 vaccinators have Day 1 lapse data while 240 have Day 2 lapse data. Of the 73 vaccinators 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 vaccinators 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 vaccinators who completed vaccinations but did not have interaction with the server had their vaccination records pulled manually from their phones after the drive.

are available for 274 on either Day 1 or Day 2 and for 226 vaccinators on both days of Drive 1.[52]

Vaccinators 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 discount factors within condition. Time and distance are also uncorrelated with Advance choice and discount factors 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 discount factors 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 patience or our finding of present bias.[53] These results are all presented in Appendix Table A.5.

Assumption 2: Unobserved Idiosyncratic Costs: We assume that vaccinations are the only argument of costs when identifying time preferences. However, there may be idiosyncratic costs across time or individuals that could influence measured patience. For example, a vaccinator with an appointment lasting 2 hours on Day 1 and no appointments on Day 2 may find it extremely costly to allocate vaccinations to Day 1. This may appear to the researcher as impatience, but only reflects the vaccinator's idiosyncratic costs across days. Further, if such idiosyncratic events are easier to re-organize when making Advance choice, present bias may be conflated with ease of scheduling.

[52]257 vaccinators have Day 1 distance data while 240 have Day 2 distance data. Of the 81 vaccinators 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 vaccinators 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.

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

Here, again, the additional data on vaccinator time use available from the monitoring application is potentially valuable. We can investigate whether extended periods of non-vaccination exist and if they are correlated with measured preferences and allocation timing. As in the example above, a vaccinator with an extended period of non-vaccination may well be experiencing forecasted idiosyncratic costs unrelated to vaccinations. Appendix Table A.6 repeats the analysis from Appendix Table A.5, with dependent variables of the maximum daily time lapse between vaccination attempts and whether the longest daily break is in excess of two hours. Longest daily breaks are, on average, around 59 minutes on Day 1 with around 13% of vaccinators taking longest breaks in excess of 2 hours. Focusing on individuals with measures on both days of the drive, we find that the length of longest breaks and the probability of 2 hour breaks are uncorrelated with Advance choice and uncorrelated with discount factors within condition. Almost identical patterns are observed on Day 2 of the drive. Differences in break behavior across days are statistically indistinguishable from zero, uncorrelated with allocation timing, and uncorrelated with discount factors within condition. These data suggest that idiosyncratic costs identified from taking extended breaks do not explain the extent of impatience in the sample, and that potential difficulties in rescheduling do not explain observed present bias.

Assumption 3: Probabilistic Completion: Our exercise assumes that indivduals know the mapping from vaccinations to completion probabilities and trade off discounted marginal costs and marginal failure probabilities. Two important functional form assumptions inform our development. First, we assume the failure probability (known to the vaccinator) is given by $p(v_1, v_2) = \frac{1}{\alpha v_1}\frac{1}{\alpha v_2}$. In Appendix Table A.7, we reconduct the analysis of Table 3, Panel A with two alternate functional forms for $p(v_1, v_2)$. First, we assume $p(v_1, v_2) = \frac{1}{1+\alpha'(v_1^2+v_2^2)}$. Second, we assume $p(v_1, v_2) = \frac{1}{1+\alpha''(v_1^3+v_2^3)}$. Both functional forms carry the property that failure probabilities are declining with the volume of work as long as $\alpha', \alpha'' > 0$. They differ only in the marginal tradeoffs they entail. Very limited differences are observed in aggregate estimates

across these functional forms and the one used in the main text. Additionally, when assuming $\gamma = 2$, the pairwise correlations between individual discount factor measures using these three functional forms all exceed 0.99.

Our exercise additionally restricts the net utility of completion, $[\delta^2 u(1000) - v_1^\gamma - \beta^{\mathbf{1}_{d=1}} \delta \cdot v_2^\gamma]$, to be equal to 100. In Appendix Table A.8, we reconduct the analysis of Table 3, Panel A assuming this net utility equal to 1000 or to 10000. Only small changes in the aggregate estimates are observed. Furthermore, at the individual level when assuming $\gamma = 2$, the pairwise correlations between individual discount factor measures using these three net completion utility values all exceed 0.99.

Though our exercise assumes individuals know the relationship between allocations and failure probabilities under a given cost function, a plausible alternative is that costs are uncertain. The natural evolution of uncertainty through time may lead to differences in measured preference parameters across groups. Though the resolution of uncertainty may lead to apparent dynamic inconsistency, the direction is not clear. Some vaccinators may grow more patient as uncertainty is resolved, some less so. Naturally, if shocks to costs do underly our observed differences in patience across individuals, one might not expect to be able to tailor contracts at the individual level over time with the success that we have.

Assumption 4: Identical Cost Functions: Our aggregate exercise assumes identical costs across subjects, and our individual elicitation assumes identical quadratic costs. 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 allocation timing with differences in costs. One natural view would be to assume that individuals do not discount at all, $\delta = 1$ and $\beta = 1$, such that allocations identify only the shape of the cost function. In this case, when $R = 1$, all vaccinators, regardless of allocation timing, should exhibit $v_1 = v_2 = 150$ for all values of γ.[54] Examining the Drive 0 and Drive

[54]This is because the Euler equation reduces to $(\frac{v_1}{v_2})^\gamma = R = 1$, which implies $\frac{v_1}{v_2} = 1$.

1 data, we find that for 163 vaccinators who were assigned $R = 1$, the mean allocation is $v_1 = 140.84$ ($s.d. = 24.76$).[55] Though the median allocation is indeed 150, responses range widely with 5th-95th percentiles of response being 103 to 160. If heterogeneity in costs were driving response and discounting was not a key feature 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 does not easily rationalize the observed present bias in the data.

Assumption 5: Only Failure, No Shirking: Our structural exercise assumes individuals know their likelihood to succeed and work only some minimal amount (e.g., that associated with the outside option) in the case where 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 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 vaccinators' proportion of successful or failed vaccination attempts remains largely steady throughout the workday. This suggests that allocated vaccination attempts are conducted with due diligence.

Assumption 6: No Biases in Choice: Our study assumes that the allocation environment itself induces no biases in choice such that vaccinator allocations are directly informative of preferences. A substantial literature in experimental economics suggests that aspects of the

[55] 42 of 163 vaccinators allocated exactly $v_1 = v_2 = 150$.

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 $v_1 = v_2$, or choosing an allocation in the middle of their budget constraint, $v_1 = Rv_2$. Only 31 of 338 vaccinators (9%) exhibit $v_1 = v_2$. Taking a less conservative measure of $v_2 - 2.5 \leq v_1 \leq v_2 + 2.5$, we find that still only 58 of 338 vaccinators (17%) are within 5 vaccinations of $v_1 = v_2$.[56] Only 35 of 338 vaccinators (10.3%) exhibit $v_1 = Rv_2$. Taking a less conservative measure of $Rv_2 - 2.5 \leq v_1 \leq Rv_2 + 2.5$, we find that 83 of 338 vaccinators (25%) are within 5 vaccinations of $v_1 = Rv_2$.[57] 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.

4.3 Tailoring Robustness Tests

Our Drive 2 data show that vaccinators who are given bonus contracts with a value of R equal to their estimated discount factors provide significantly smoother service. Here we examine robustness of this result to alternative comparison groups, alternative measures for smoothness in service provision, and alternative measures for treatment. We conclude the section by providing results from a set of additional exercises assessing the value of atheoretic approaches to tailoring, and the possibility for alternative interventions based on different policy preferences.

4.3.1 Alternative Comparison Groups

Our results demonstrate that, relative to a comparison group with uniform random values of R, tailoring serves to reduce distance to the policy target by around one-third. A natural question is whether these tailoring benefits are observed relative to alternative controls. In Appendix Table A.9, we present three additional analyses. A first natural control is the use of a single value of R applied to all individuals. Unfortunately, it is not possible to compare only a single

[56] As an even less conservative measure, 145 of 338 (43%) satisfy $v_2 - 10 \leq v_1 \leq v_2 + 10$.

[57] As an even less conservative measure, 137 of 338 (40.5%) satsify $Rv_2 - 10 \leq v_1 \leq Rv_2 + 10$.

value of R given our uniform random assignment protocol. However, we can examine a section of the untailored group around a given value. In column (1) of Table A.9, we repeat the analysis of Table 4, column (3), but use as the comparison group only those untailored vaccinators who received a value of R within one standard deviation of their group's mean R_i^* of 1.036. Tailoring continues to decrease the distance from smooth provision relative to this more limited control group. In column (2), we repeat this analysis excluding those individuals from the untailored group who randomly received a value of $\tilde{R}_i$ within 0.10 of their true value of R_i^*. The benefits of tailoring are observed with increased precision.[58] A second potential control group would be a subset of the untailored group who receive the same distribution of R as those in the tailored group. Matching on the 1st, 5th, 10th, 25th, 50th, 75th, 95th and 99th percentiles, column (3) demonstrates that relative to a control group receiving a matched distribution of R, tailoring continues to significantly reduce the distance from smooth provision.

4.3.2 Tailoring and Completion

Our analysis to here treats probabilistic completion through the lens of a structural 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 failure exists. If the *outcome* of failure is perfectly forecasted by the vaccinator, 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 vaccinators are allocating according to equation (1), while unsuccessful vaccinators are providing only noisy response. Under this assumption, we should be dramatically less able to predict allocation behavior for vaccinators who fail in Drive 1.

Table A.10 repeats the analysis of Table 4, columns (1) through (3) separately for subject

[58] Comparing tailored individuals to those who received close to the untailored group's mean value of R_i^* is important because, in principle, the mean value of R^*i should yield smooth provision for the average subject. Not only is the average distance for these comparison groups substantial, 0.132 to 0.150, but the tailoring yields additional benefits at the individual level by leveraging the heterogeneity in discount factors across vaccinators.

who completed and failed to complete their Drive 1 targets. Similar magnitude effects are observed for both sets of subjects, with 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. Furthermore, the fact that predictive accuracy remains for subjects who fail to complete demonstrates that there is content to the allocations subjects make regardless of ex-post completion.

4.3.3 Alternative Measures for Smooth Provision

Our analysis measures the distance to equal provision using the metric $|\frac{v_{1,i}}{v_{2,i}} - 1|$. In Table A.11, we reconduct the analysis of Table 4, using five alternate measures for smoothness. Panel A presents the Euclidean distance to the 45 degree line, $\frac{|v_{1,i}-v_{2,i}|}{\sqrt{2}}$. Panel B presents the Euclidean distance normalized by the total number of vaccinations allocated, $\frac{|v_{1,i}-v_{2,i}|}{\sqrt{2}(v_{1,i}+v_{2,i})}$. Panel C presents the number of sooner vaccinations that would need to be reallocated to reach the 45 degree line, $|v_{1,i} - \frac{300}{1+R}|$. Panel D presents probit regressions for needing to reallocate more than 25 vaccinations, $|v_{1,i} - \frac{300}{1+R}| > 25$. And finally, Panel E presents the value of the policymaker's objective function, $min[v_{1,i}, v_{2,i}]$. Across all specifications, the main conclusions are reproduced. However, the results with respect to additional tailoring benefits in Immediate choice fall, at times, outside the range of statistical significance. These alternative measures of smooth provision indicate that our results on the potential benefits of tailoring are not an artifact of how one measures the outcome of interest.

4.3.4 Alternative Sample Restrictions and Treatment Measures

Our tailoring exercise focused on vaccinators with discount factors between 0.75 and 1.5. Of 337 vaccinators in Drive 2, 280 satisfied this requirement. Those vaccinators whose discount factors fell outside of this range were given either $R = 0.75$ or 1.5 depending on which bound they were closest to. For such individuals, tailoring is not a binary treatment, but rather a continuous difference between their discount factor and the exogenously given one. Indeed,

for all vaccinators in the untailored group, treatment is also a continuous measure. In Table A.12, Panel A, we reconduct the analysis of Table 4 using as the measure of treatment the absolute difference between each vaccinator's discount factor and their assigned interest rate, which we label *Tailor Intensity*. The main results are reproduced; the closer discount factors are to interest rates, the smoother is provision.[59] In Panel B, we include those individuals in the Boundary Sample with discount factors that lie outside of the bounds of interest rate assignment. 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 vaccinators will receive larger treatment intensity on average.[60]

4.3.5 Atheoretic Approaches

Our exercise demonstrates that structural estimates of time preferences can be useful in predicting subsequent behavior. An alternative to developing a structural model of choice would rely on the researcher recovering the relationship between key parameters of interest, R and $\mathbf{1}_{d=1}$, and behavior, and developing a subsequent prediction without filtering the relationship through the structural model.

We examine one example of such an exercise. For Drive 1 behavior, we recover the relationship between v_1 and R and $\mathbf{1}_{d=1}$ by conducting a Least Absoluate Shrinkage and Selection Operator (LASSO) regression with penalty parameter chosen via 10-fold cross validation (Tibshirani, 1996) of v_1 on a cubic polynomial in R interacted with $\mathbf{1}_{d=1}$.[61] The corresponding selected lasso coefficients deliver an atheoretic representation of the most predictive (in terms of cross-validated mean squared error) relationship between v_1 and key parameters of interest in Drive 1. Given the values of R and $\mathbf{1}_{d=1}$ provided in Drive 2 and the Drive 1 lasso coefficients,

[59] Restricting attention only to the untailored group reveals directionally similar, though insignificant, results across all specifications.

[60] Using the indicator for tailoring would not be an appropriate solution to this problem as tailored vaccinators with extreme patience or impatience may actually receive interest rates that are further from their policy-optimal interest rates than those in the untailored condition.

[61] The provided regressors are a constant and normalized values of $R, R^2, R^3, \mathbf{1}_{d=1}, R \times \mathbf{1}_{d=1}, R^2 \times \mathbf{1}_{d=1}$, and $R^3 \times \mathbf{1}_{d=1}$. The lasso regression and cross-fold validation procedure were implemented using the glmnet package in R.

we predict the Drive 2 value of v_1 for each individual.[62] Similarly, we predict Drive 2 value of v_1 from the assigned values of R and $\mathbf{1}_{d=1}$ and the individual discount factor identified in Drive 1 from equation (4). In Appendix Figure A.10 we examine the predictive validity of the structural versus the non-structural approaches by plotting predicted and actual values of v_1 in Drive 2 for the 337 subjects who participated in both Drive 1 and Drive 2. We also provide non-parametric lowess curves for the relationship between predicted and actual values.[63] Notable from Appendix Figure A.10 is the generally close adherence between structurally predicted and actual values. The lowess line follows the 45 degree line of perfect prediction through the majority of the space. Less adherence is observed between non-structural predictions and actual behavior. The structural predictions deliver higher correlations with real behavior ($\rho = 0.18$) than do the non-structural predictions ($\rho = 0.16$) and lower bias in prediction (27.9 vs. 32). However, the root mean squared error is lower for the non-structural predictions (32.8 vs 39.1). Hence, on the basis of bias and correlation, our structural exercise outperforms the machine learning lasso algorithm trained on Drive 1.

There is an additional important difference between the LASSO and structural predictions presented here. The structural prediction for a given vaccinator's behavior in Drive 2 derives from their allocation in Drive 1 and the interest rate assigned in Drive 2. It therefore relies on a single data point. By contrast, the LASSO prediction for a given vaccinator is derived by training a model on the entire cross-section of data from Drive 1, and then obtaining a fitted value based on the interest rate assigned in Drive 2. This means the LASSO prediction will be identical for all subjects assigned the same interest rate while the structural prediction may vary depending on individually measured preferences. Hence, the two methods are not only different in the sense of being structural or atheoretic, they also differ both in the amount of data informing the prediction and in the extent of heterogeneity they can predict.

[62]Note that this prediction will be identical for all vaccinators given the same value of R and $\mathbf{1}_{d=1}$, and hence will mispredict any heterogeneity across vaccinators.

[63]The lasso procedure on Drive 1 selects an intercept and R as delivering coefficients of sufficient size given the cross-validated constraint choice.

4.3.6 Alternative Policy Preferences

While our results suggest that tailored contracts can improve success in achieving a Leontief policy objective, a natural question is whether this approach could be put to use to achieve other policy objectives. Ultimately, any attempt to tailor contracts will rely on whether initially elicited preferences are stable. If vaccinator time preferences are stable, then changes in incentives will have predictable effects on behavior.

To provide a general assessment of the promise of alternative policy objectives, we examine the stability of identified discount factors across Drives 1 and 2 by calculating the corresponding discount factor from equation (4) for each vaccinator in each Drive. Figure A.11 presents the calculated discount factors for Drive 1 and Drive 2 along with the 45 degree line for 317 of 337 vaccinators.[64] The correlation in discount factors across rounds is $\rho = 0.41,\ (p < 0.01)$, indicating stability in preferences.[65] Our findings correspond with those of Meier and Sprenger (2015), who investigate subjects participating in an identical monetary discounting experiment approximately one year apart and identify a one-year correlation of around 0.5 for monetary choices. The level of correlation in discount factors across drives indicates stability in preferences such that alternative policy objectives may also be achievable with tailored contracts.[66]

5 Conclusion

Structural parameters for intertemporal preferences have been at the center of theoretical and empirical research modeling intertemporal choice for much of the last century. This paper

[64] Eliminated from the figure and from our calculations of stability are 20 vaccinators with discount factors in excess of two in one or both drives.

[65] Including the remaining 20 extreme vaccinators, the correlation changes substantially to $\rho = 0.01,\ (p = 0.87)$. It should be noted that the correlation in identified discount factors is substantially higher in the tailored condition, $\rho = 0.67,\ (p < 0.01)$, relative to the untailored condition, $\rho = 0.17,\ (p < 0.05)$. We believe this is due to some sensitivity of behavior to extreme values of R in Drive 2. For untailored subjects who coincidentally receive a value of $\tilde{R}_i$ within 0.25 of their value of R_i^*, the correlation in discount factors is $\rho = 0.53,\ (p < 0.01)$.

[66] One natural alternative is to maximize performance, regardless of timing. In such a case, we consider a policymaker with linear preferences, $P(v_1, v_2) = v_1 + v_2$, who wishes to maximize the total number of completed vaccinations regardless of timing. Maximizing this objective function subject to the vaccinator's offer curve, yields an optimal $R^*_{max} = \sqrt{\beta^{\mathbf{1}_{t=1}}\delta(1+\beta^{\mathbf{1}_{t=1}}\delta)} - \beta^{\mathbf{1}_{t=1}}\delta$. Unfortunately, our assigned values of R are generally quite far from R^*_{max} making it difficult to test for the possibility of a maximizing contract.

seeks to understand a heretofore unexplored question: are the out-of-sample predictions given by structural estimates of discounting empirically valid? 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 our workers' efforts using a smartphone application developed especially for our project. Workers in the Advanced condition state their targets for both days of the vaccination drive on the Friday before the drive, which is conducted on the following Monday and Tuesday. Those in the Immediate condition wait until Monday morning to state their targets. Those who reach their targets get a 1000 rupee bonus (around $10 US). As anticipated, subjects stating targets on Monday morning are skewed to delaying vaccinations until day 2 of the drive. That is, vaccinators exhibit a present bias in effort allocations. With assumptions on costs of delivering vaccines we are able to identify (somewhat rough) estimates of discounting parameters for each of our workers. In the second stage of our study, conducted a month later, half of workers were offered a contract tailored to their own discounting parameters, designed to induce equal provision of vaccinations on both days of the drive. The initial preference measures are critical to the design of these contracts as, without a measure of preferences, there would be no prescription for contract terms. The policy objective of equal provision of vaccinations on both days is admittedly arbitrary. Hence, we view our exercise as a proof-of-concept for the possibility of tailored incentives.

Our findings are encouraging. Those workers who receive effort contracts that were tailored to their individual discounting parameters were significantly more likely to meet the policy objective relative to untailored workers. That is, using structurally identified estimates of discounting parameters to form a new incentive contract can indeed have a predictable effect on allocation behavior. Additional predictions for subsequent task completion from our structural analysis are also borne out in the data. To date, little research makes use of such predictive value of structural 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 structural models in this domain has been well-invested.

This paper also speaks to a recent discussion on the external validity of experimentally administered 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).[67] 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.[68] 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 et al., 2016; Finan, Olken and Pande, Forthcoming; Dal Bó, Finan and Rossi, 2013; Deseranno, 2016; Callen, Gulzar, Hasanain, Khan and Rezaee, 2016). This literature emphasizes the idea that states play a vital role in delivering services and facilitating economic growth, and so their internal dynamics should be studied with the same degree of attention as has been applied to firms. 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., 2016). 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 Drive 2 when Drive 1 preferences are elicited. The mechanism design problem of eliciting preferences and tailoring on said preferences with complete information will be critical

[67] 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.

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

if one wishes to implement tailored 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 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 contracts, multi-period settings, and alternative policy targets that are worthy of study.

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A Appendix

A.1 Appendix Figures

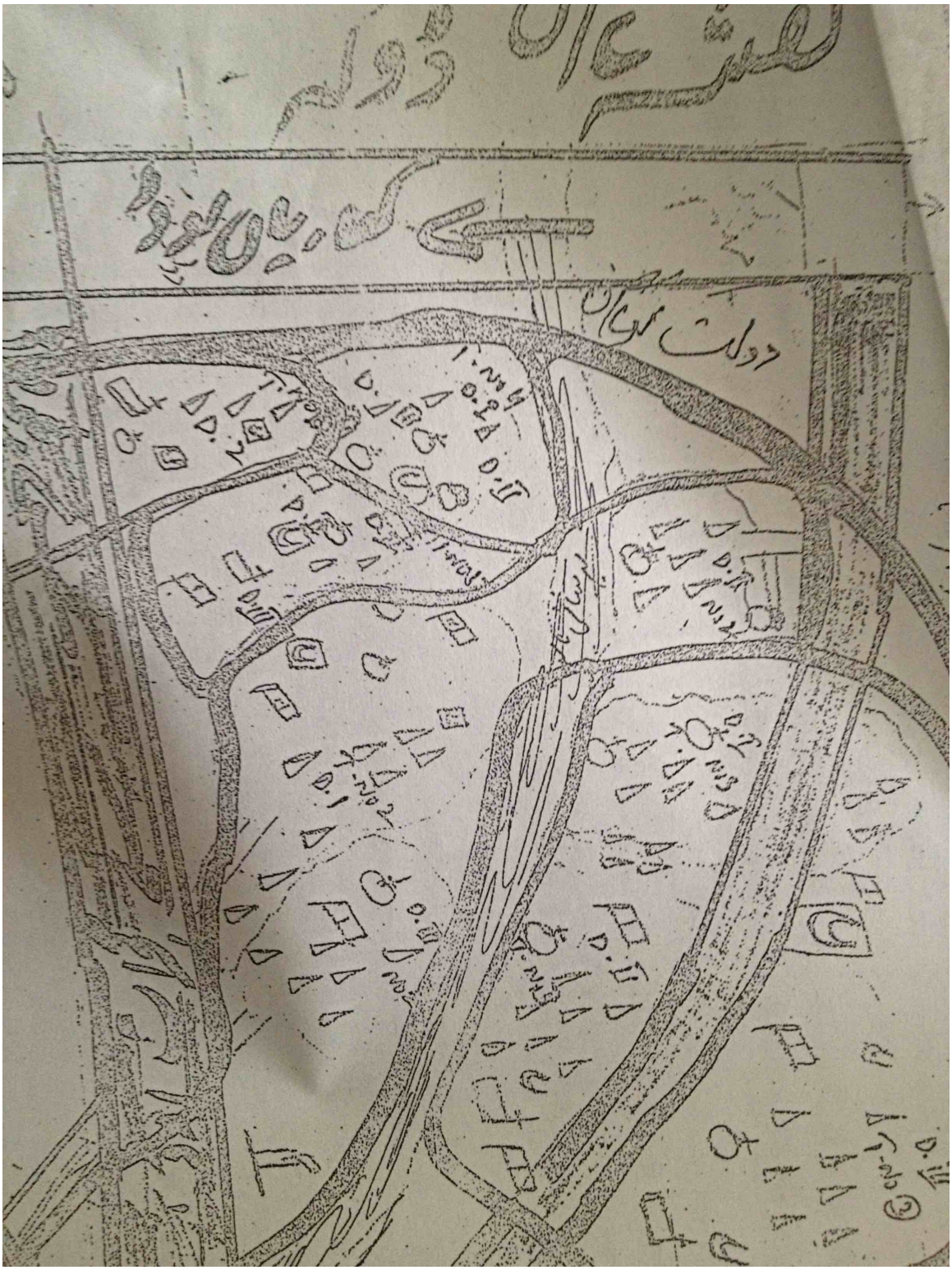
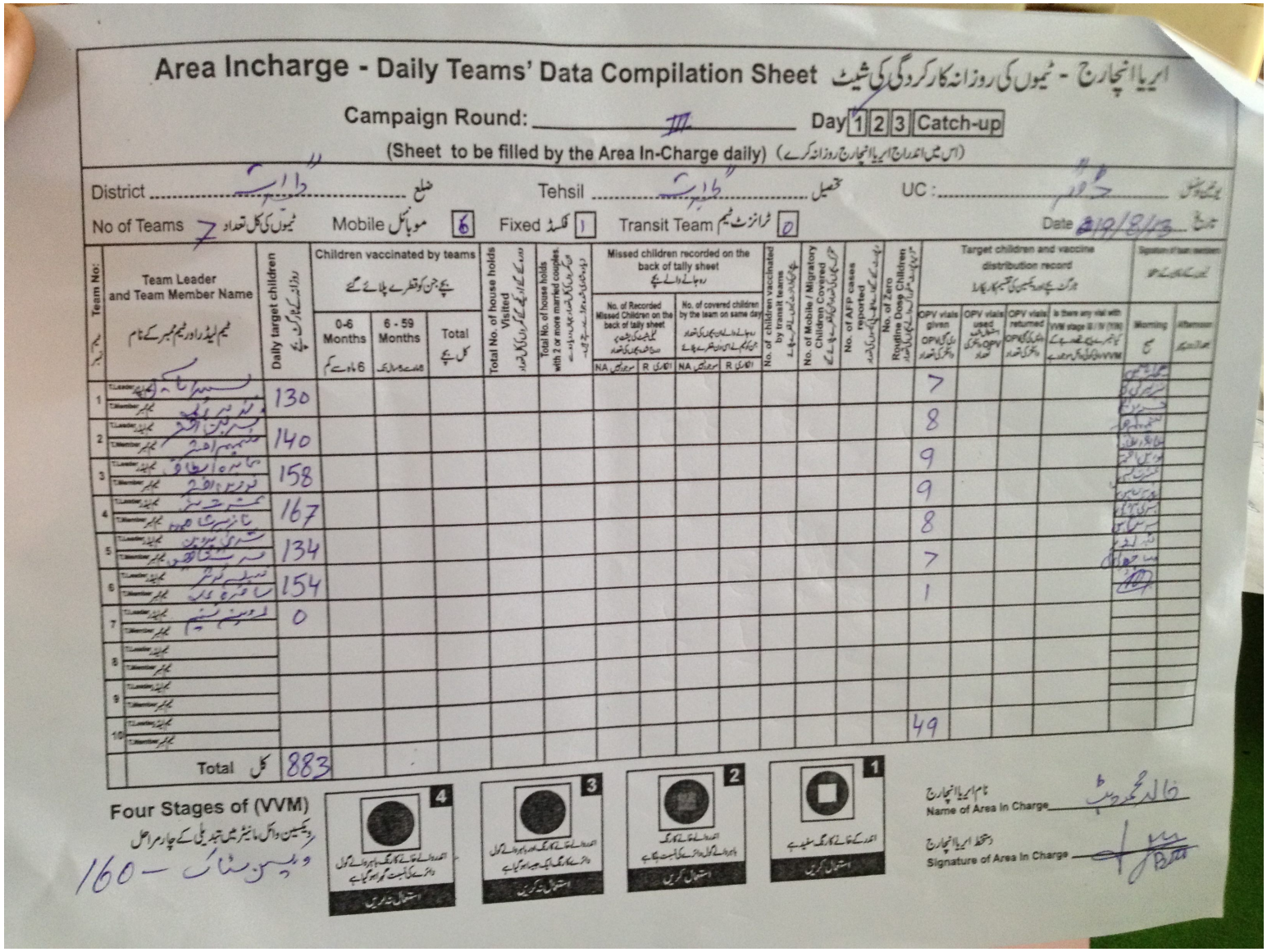


Figure A.1: Map Given to Vaccinators to Plan Route

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 ڈاڈر ضلع Tehsil ڈاڈر تحصیل UC: ڈاڈر یونین کونسل

No of Teams 7 ٹیموں کی کل تعداد Mobile موبائل 6 Fixed فکسڈ 1 Transit Team ٹرانزٹ ٹیم 0 Date 19/8/13 تاریخ

Team No: ٹیم نمبر	Team Leader and Team Member Name ٹیم لیڈر اور ٹیم ممبر کے نام	Daily target children روزانہ بچوں کے ہدف	Children vaccinated by teams بچے جن کو قطرے پلائے گئے: 0-6 Months 6 ماہ سے کم	6 - 59 Months 6 ماہ سے 5 سال تک	Total کل بچے	Total No. of house holds Visited دورہ کیے گئے گھروں کی کل تعداد	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 teams	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 کی دی گئی شیشیوں کی تعداد	OPV vials used OPV کی استعمال شدہ شیشیوں کی تعداد	OPV vials returned OPV کی واپس کی گئی شیشیوں کی تعداد	Is there any vial with VVM stage III / IV (Y/N)	Signature of team members: Morning صبح	Afternoon
1	Leader / Member	130														7					
2	Leader / Member	140														8					
3	Leader / Member	158														9					
4	Leader / Member	167														9					
5	Leader / Member	134														8					
6	Leader / Member	154														7					
7	Leader / Member	0														1					
8	Leader / Member																				
9	Leader / Member																				
10	Leader / Member																				
	Total کل	883														49					

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

160 — ویکسین سٹاک

4 — استعمال نہ کریں

3 — استعمال نہ کریں

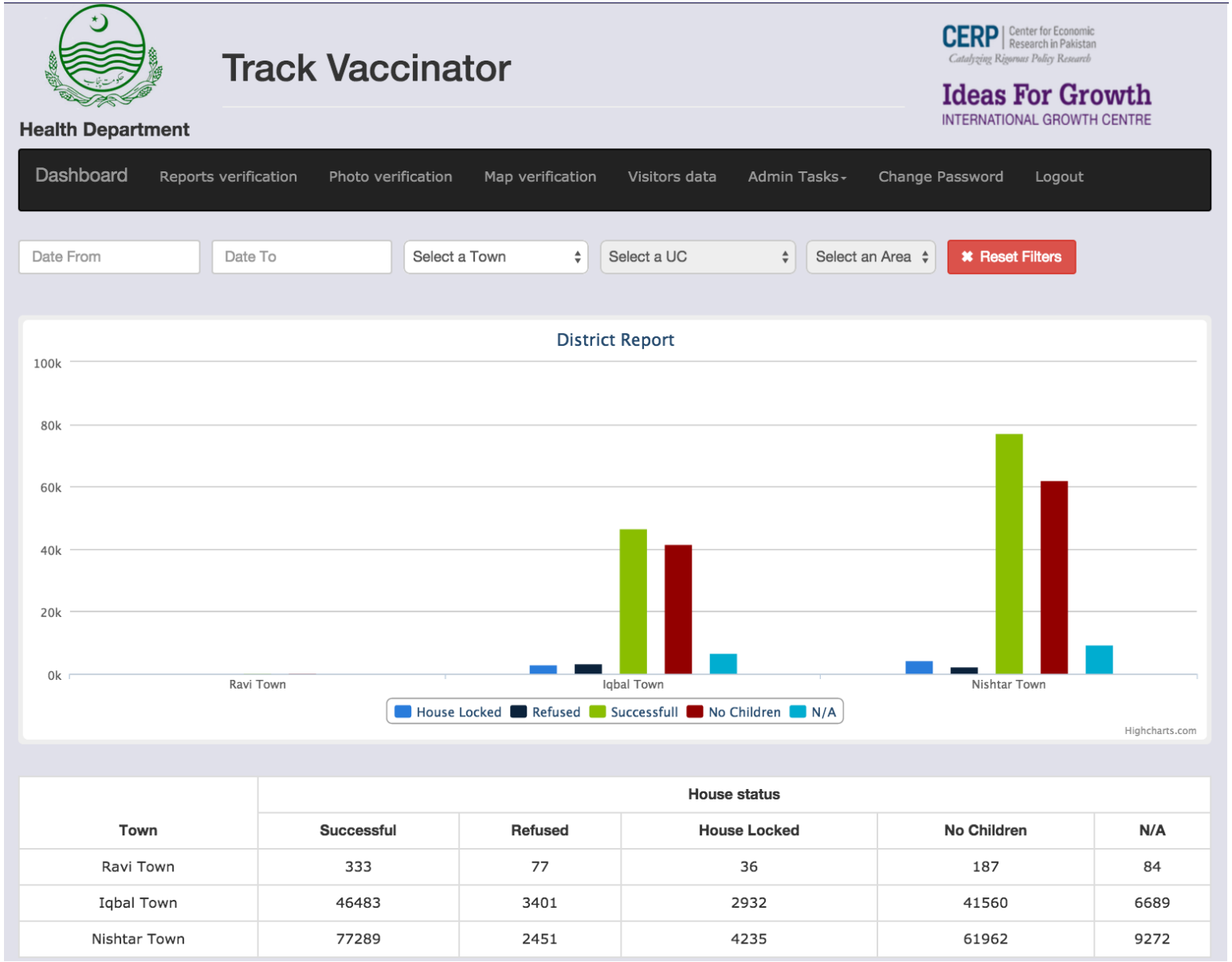
2 — استعمال کریں

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

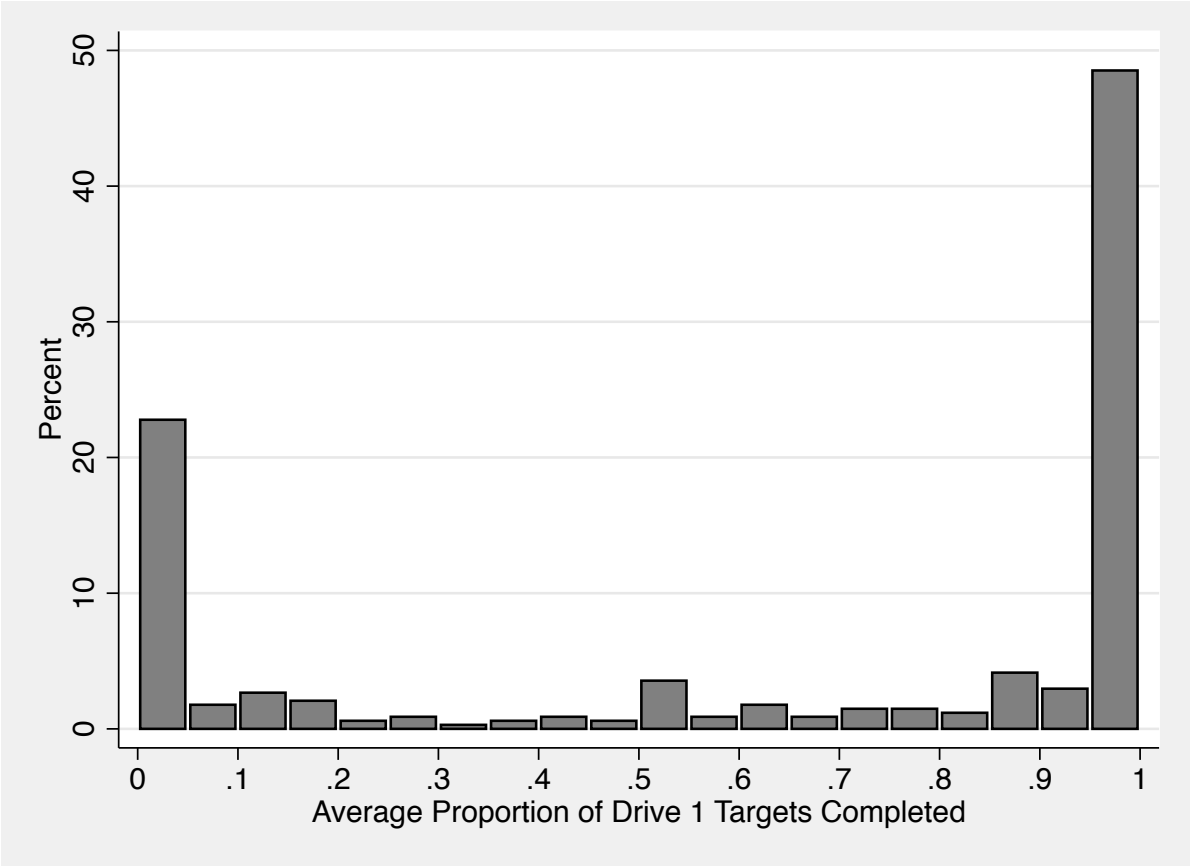


Figure A.6: Individual Completion Rates

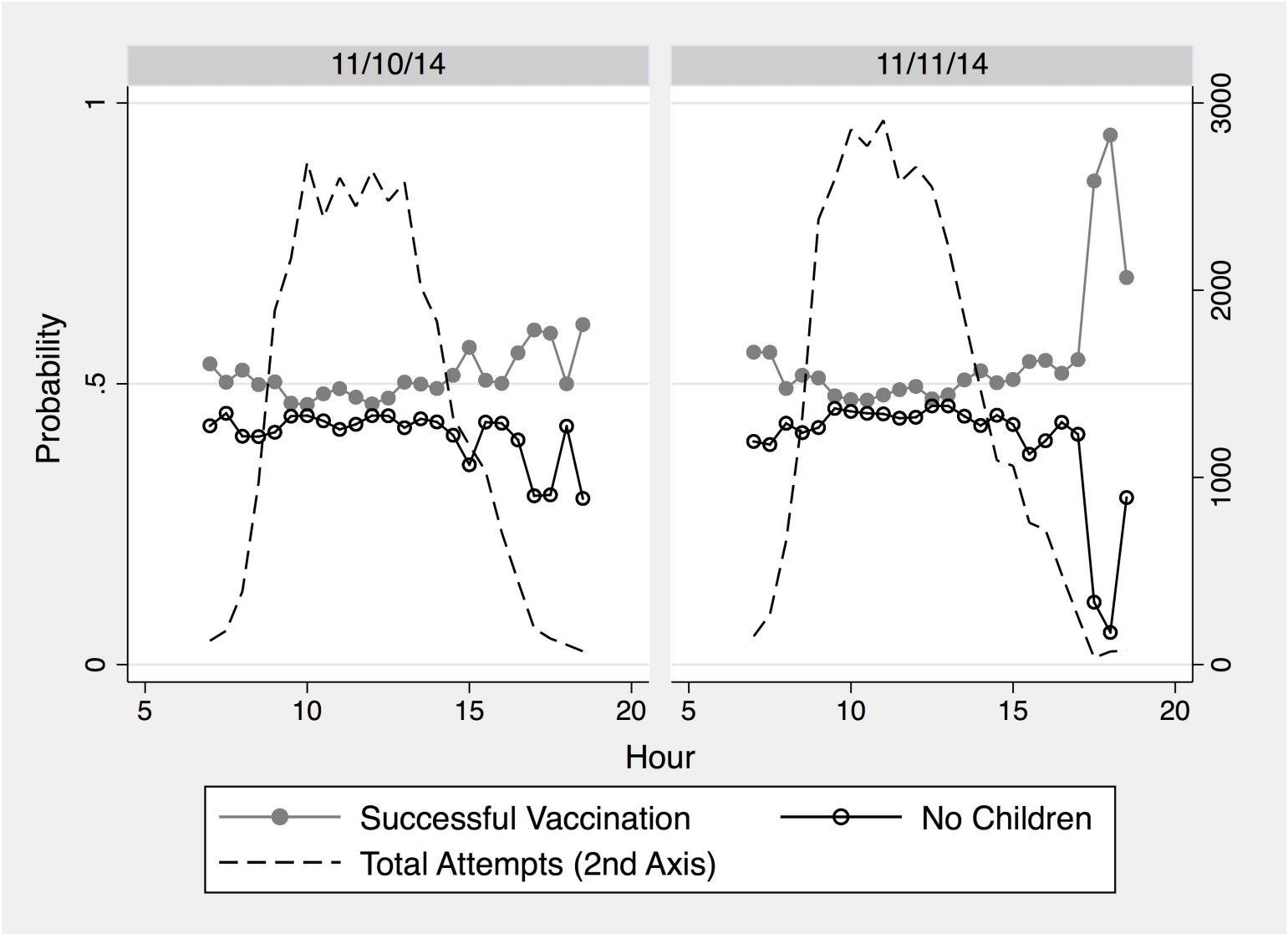


Figure A.7: Drive 1 Vaccination Activity

Notes: 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.

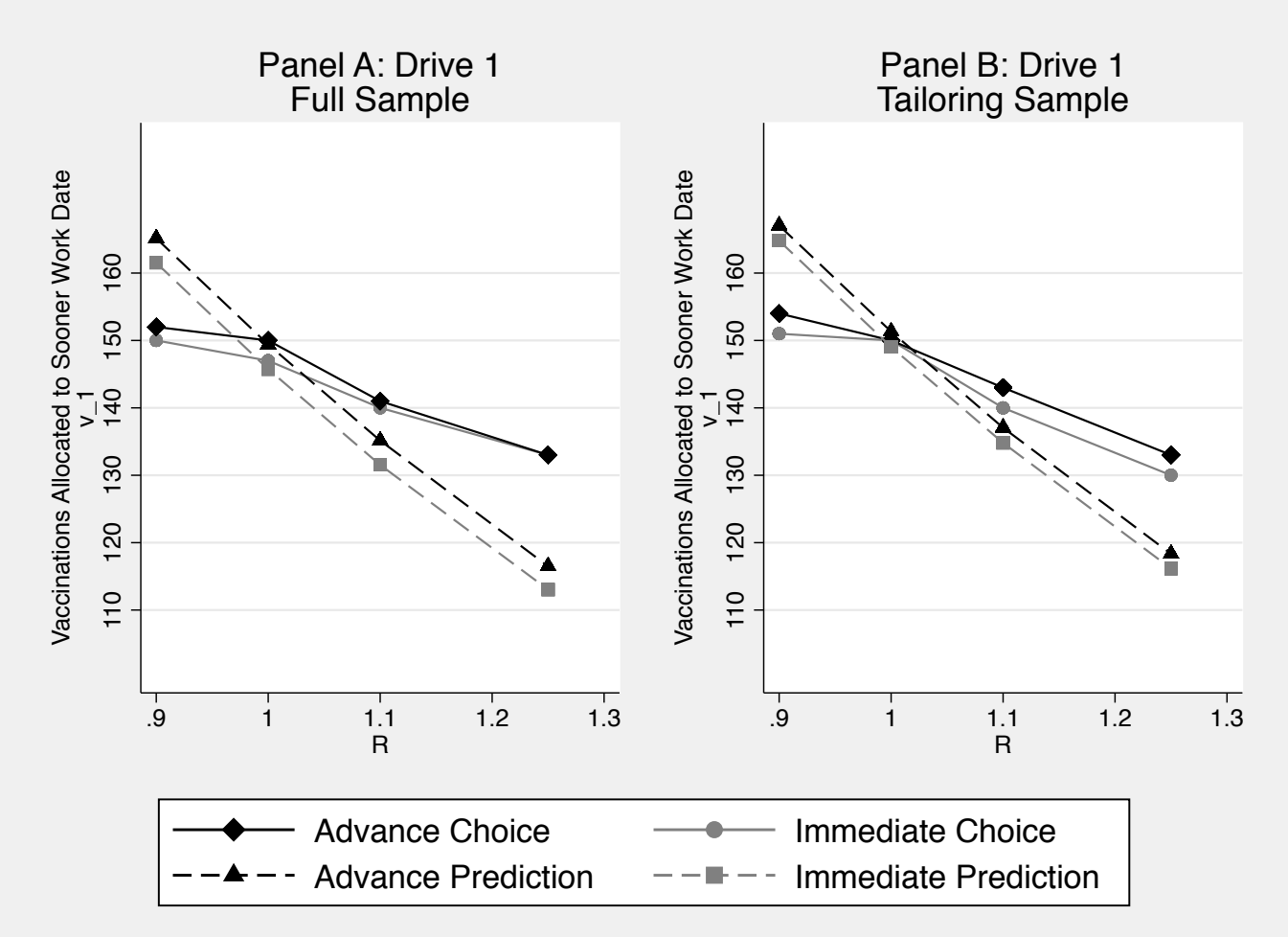


Figure A.8: Predicted and Actual Experimental Response

Notes: Points in the plots are medians for each of the eight treatment groups respectively. Panel A depicts the Full Sample and Panel B depicts the tailoring sample (vaccinators with $R^* < 0.75$ or $R^* > 1.5$). Black are advance choice groups and gray are immediate choice groups. The series for predictions correspond to Table 3, column (2).

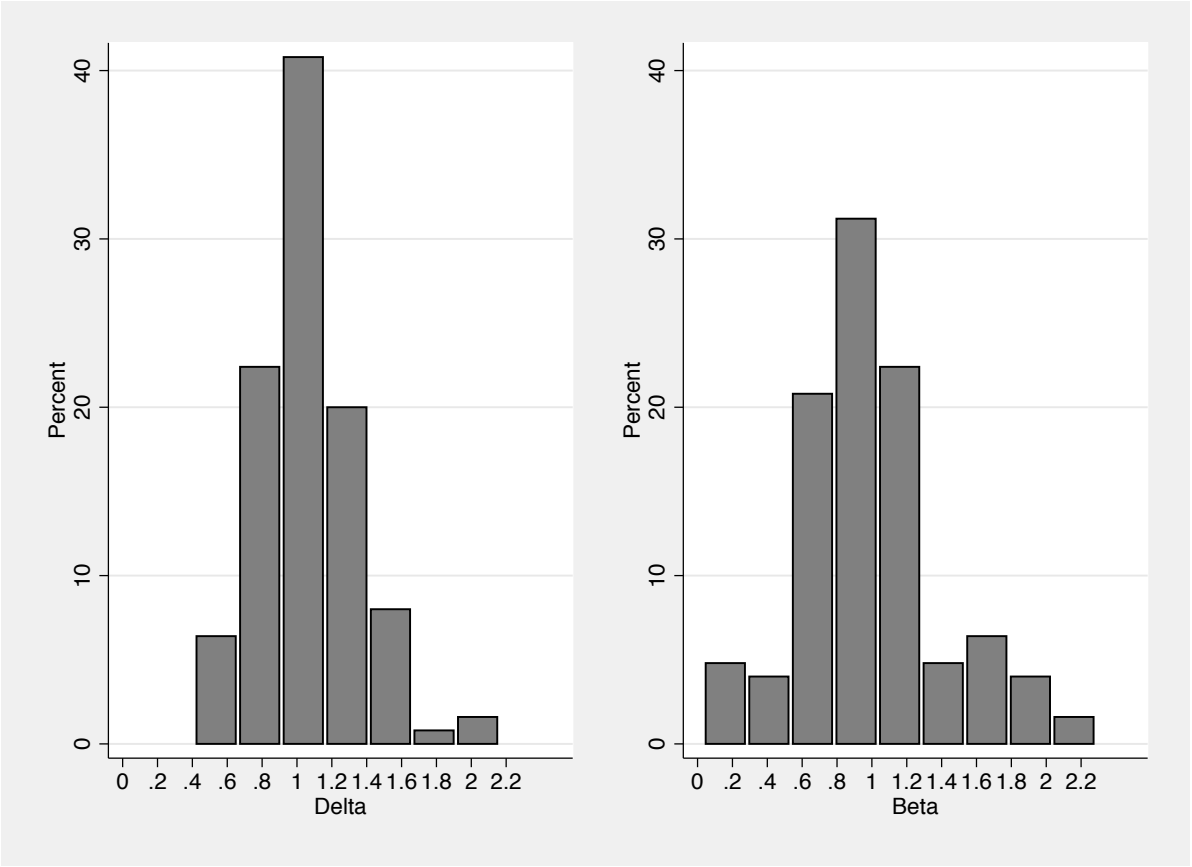


Figure A.9: Within-Subject Parameter Calculations

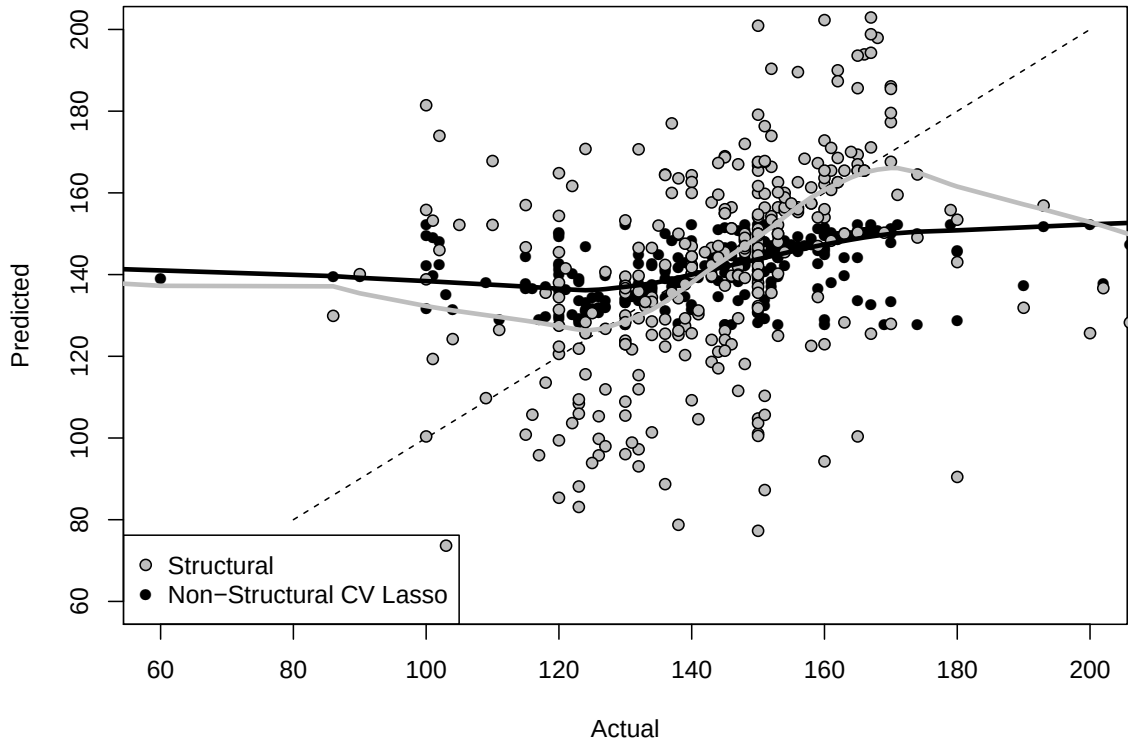


Figure A.10: Structural and Non-Structural Prediction

Notes: Predicted and actual Drive 2 value of v_1 for structural and non-structural models. Structural prediction based on individual discount factor calculated from Drive 1. Non-structural prediction based on lasso regression of v_1 on cubic polynomial in R interacted with $\mathbf{1}_{d=1}$ from Drive 1. Lowess curves for non-parametric adherence to 45-degree line of perfect prediction.

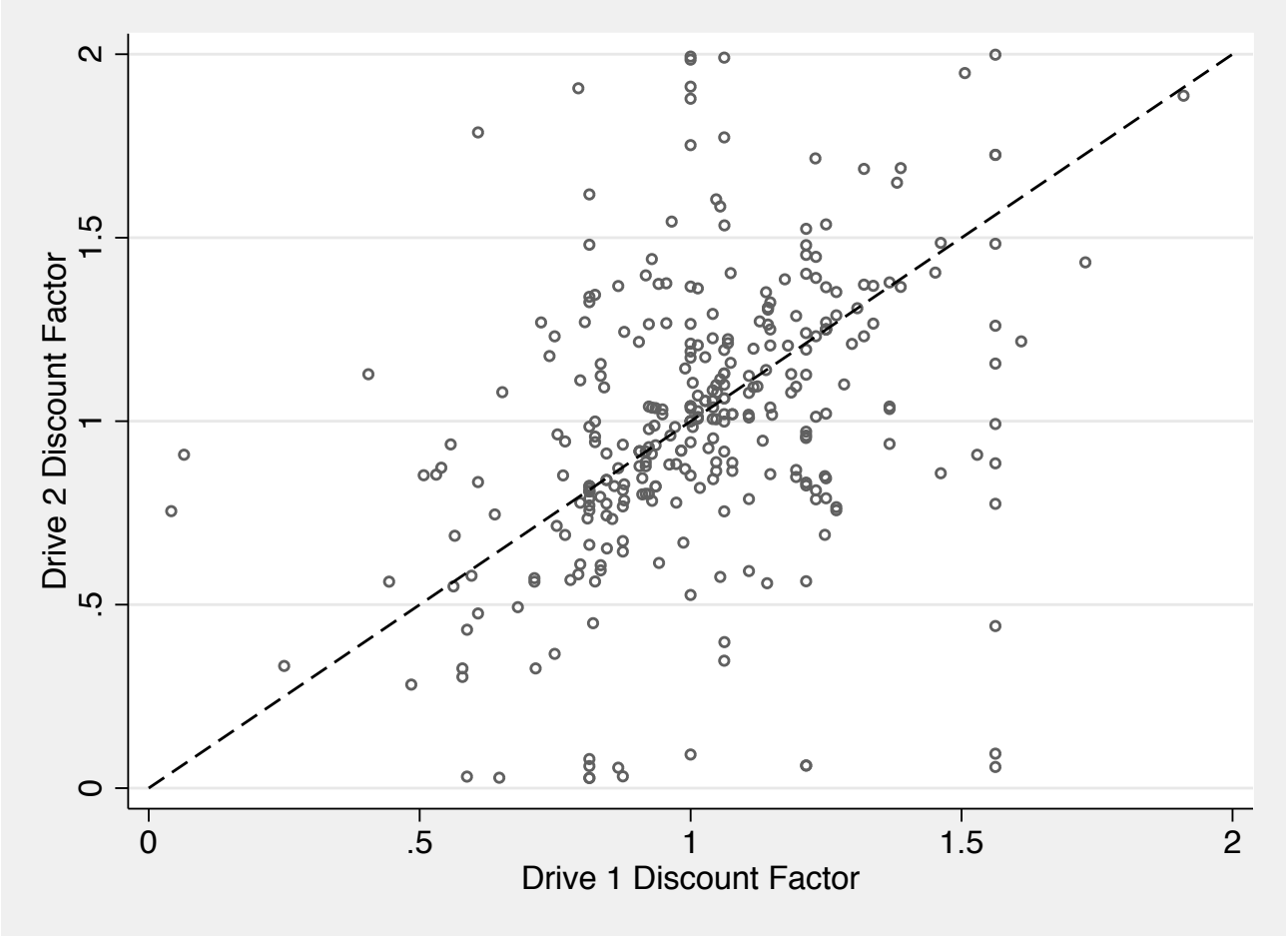


Figure A.11: Stability of Preferences

Notes: Drive 1 and Drive 2 discount factors calculated from equation (4) for each allocation. Figure includes 317 of 337 vaccinators present in both drives. Excluded are 20 vaccinators with calculated discount factors in excess of 2 in one or both drives. Correlation: $\rho = 0.41,\ (p < 0.01)$.

A.2 Appendix Tables

Table A.1: 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: This table tests whether the failure of the smartphone app during Drive 0 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.2: Aggregate Drive 0 and 1 Behavior

Dependent variable:	Tasks Allocated to the First Day of the Drive (v_1)	
	Full Sample	Tailoring Sample
	(1) Median	(2) Median
Immediate Decision (=1)	-2.00**	-3.00***
	(0.95)	(0.88)
Interest Rate (R)	-40.00***	-60.00***
	(6.04)	(4.12)
Constant	188.00***	210.00***
	(6.06)	(4.34)
Median Advance Choice	150	150
# Observations	622	475

Notes: This table reports on the effects of making Immediate allocation decisions and interest rate on Drive 0 and Drive 1 vaccinations allocated to the first day of the drive. Median regression coefficients with standard errors reported in parentheses. Standard errors are clustered at the vaccinator level. Clustered standard errors for quantile regressions are calculated using the approach in Parente and Santos Silva (2016). Immediate Decision is an indicator equal to one for vaccinators selecting their allocations on the morning of the vaccination drive. The interest rate R takes the values $R \in \{0.9, 1, 1.1, 1.25\}$. *Levels of Significance:* $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.3: Within Subject Parameter Estimates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All Observations		Panel Only		Panel No Change		Panel Change	
β	0.895	0.903	0.930	0.931	0.946	0.944	0.906	0.912
	(0.030)	(0.030)	(0.032)	(0.032)	(0.048)	(0.048)	(0.043)	(0.042)
δ	0.979	0.979	0.988	0.988	0.967	0.967	1.012	1.010
	(0.017)	(0.017)	(0.016)	(0.016)	(0.023)	(0.023)	(0.023)	(0.023)
α		0.003		0.002		0.003		0.002
		(0.000)		(0.000)		(0.000)		(0.000)
$H_0 : \beta = 1.$ ($\chi^2(1)$)	12.375	10.571	4.624	4.497	1.283	1.399	4.895	4.344
[p-value]	[0.000]	[0.001]	[0.032]	[0.034]	[0.257]	[0.237]	[0.027]	[0.037]
Criterion Value	0.182	0.191	0.198	0.200	0.175	0.177	0.217	0.221
# Observations	622	622	464	464	212	212	252	252
# Vaccinators	390	390	232	232	106	106	126	126

Notes: This reports structural estimates of β, δ, and α obtained using minimum distance estimation of equations (1) in even columns or (2) and (3) in odd columns. Estimates provided for either all possible observations or panel of individuals participating in both Failed Drive 0 and Drive 1. Only Drive 1 data used for estimation of completion parameter, α. Standard errors clustered on individual level are reported in parentheses. Test statistic for $\beta = 1$ with p-value in brackets.

Table A.4: Additional Discounting Measures

Dependent variable:	Drive 2 Allocation to Sooner Date $v_{1,i}$			
	Untailored		Tailored	
	(1)	(2)	(3)	(4)
Drive 0 Prediction	0.339*	0.203**	0.009	-0.017
	(0.174)	(0.081)	(0.066)	(0.068)
Drive 1 Prediction	-0.108	-0.051	0.406*	0.701***
	(0.187)	(0.137)	(0.212)	(0.182)
Constant	109.010***	122.367***	83.530***	43.805**
	(13.592)	(14.128)	(24.742)	(20.610)
# Vaccinators	52	42	53	46
Tailoring Sample	No	Yes	No	Yes
R-Squared	0.275	0.165	0.166	0.362
R-Squared (Drive 0 Prediction Alone)	0.262	0.162	0.103	0.175
R-Squared (Drive 1 Prediction Alone)	0.037	0.070	0.166	0.361

Notes: This table reports regressions for Drive 2 allocation on Drive 0 or Drive 1 predicted allocation for panel of 105 individuals that remain in either Advance or Immediate choice across Drives 0 and 1. Predicted allocation from equation (4) individual discount factor. Heteroskedasticity robust White standard errors reported in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table A.5: 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)
Discount Factor			-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)
Discount Factor			-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: This table reports on the relationship between decision timing and the one period 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). Heteroskedasticity robust White standard errors reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.6: Testing for Idiosyncratic Shocks

Panel A: Maximum Daily Time Lapse Between Vaccinations (in Minutes)

Dependent variable:	Max Day 1 Time Lapse			Max Day 2 Time Lapse			Day 1 - Day 2 Max Time Lapse	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Advance Choice (=1)	-0.702	0.578	0.739	8.067	5.274	5.574	-4.695	-4.836
	(9.783)	(9.104)	(9.026)	(8.885)	(9.114)	(9.021)	(12.319)	(12.211)
Discount Factor			4.831			9.054		-4.223
			(14.139)			(15.570)		(19.824)
Constant	59.258***	54.880***	49.764***	53.437***	54.362***	44.774***	0.518	4.990
	(7.920)	(7.254)	(15.266)	(5.178)	(5.404)	(15.351)	(8.724)	(20.662)
R-Squared	0.000	0.000	0.000	0.003	0.001	0.003	0.001	0.001
# Observations	265	228	228	240	228	228	228	228

Panel B: Maximum Time Lapse > 2 hours

Dependent variable:	Max Day 1 Lapse > 2hr.			Max Day 2 Time Lapse > 2hr.			Day 1 > 2hr. - Day 2 > 2hr.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Advance Choice (=1)	0.051	0.042	0.044	0.026	0.001	0.002	0.041	0.042
	(0.045)	(0.047)	(0.047)	(0.041)	(0.042)	(0.041)	(0.060)	(0.060)
Discount Factor			0.053			0.032		0.021
			(0.077)			(0.071)		(0.100)
Constant	0.133***	0.127***	0.071	0.103***	0.109***	0.075	0.018	-0.004
	(0.029)	(0.032)	(0.085)	(0.028)	(0.030)	(0.075)	(0.043)	(0.111)
R-Squared	0.005	0.004	0.005	0.002	0.000	0.001	0.002	0.002
# Observations	265	228	228	240	228	228	228	228

Notes: This table reports on the relationship between decision timing and the one period discount factor with two proxies for experiencing a shock during the drive (the maximum time lapse between vaccinations and whether a lapse of more than 2 hours occurred). Heteroskedasticity robust White standard errors reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.7: Aggregate Parameter Estimates, Alternate Probabilistic Completion

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\gamma = 2$		$\gamma = 2.5$		$\gamma = 3$		$\gamma = 3.25$	
$p(v_1, v_2) =$	$\frac{1}{1+\alpha'(v_1^2+v_2^2)}$	$\frac{1}{1+\alpha''(v_1^3+v_2^3)}$	$\frac{1}{1+\alpha'(v_1^2+v_2^2)}$	$\frac{1}{1+\alpha''(v_1^3+v_2^3)}$	$\frac{1}{1+\alpha'(v_1^2+v_2^2)}$	$\frac{1}{1+\alpha''(v_1^3+v_2^3)}$	$\frac{1}{1+\alpha'(v_1^2+v_2^2)}$	$\frac{1}{1+\alpha''(v_1^3+v_2^3)}$
β	0.944	0.936	0.935	0.924	0.924	0.910	0.917	0.883
	(0.030)	(0.030)	(0.040)	(0.039)	(0.050)	(0.050)	(0.055)	(0.053)
δ	0.992	0.992	0.967	0.966	0.943	0.942	0.931	0.926
	(0.017)	(0.017)	(0.022)	(0.022)	(0.027)	(0.027)	(0.030)	(0.028)
α'	0.000022		0.000022		0.000022		0.000022	
	(0.000002)		(0.000002)		(0.000002)		(0.000002)	
α''		$1.48e^{-7}$		$1.50e^{-7}$		$1.50e^{-7}$		6.58
		$(1.63e^{-8})$		$(1.64e^{-8})$		$(1.65e^{-8})$		(.)
$H_0 : \beta = 1.$ $(\chi^2(1))$	3.583	4.650	2.700	3.681	2.341	3.299	2.261	4.904
[p-value]	[0.058]	[0.031]	[0.100]	[0.055]	[0.126]	[0.069]	[0.133]	[0.027]
Criterion Value	0.308	0.306	0.248	0.247	0.211	0.210	0.198	0.551
# Vaccinators	338	338	338	338	338	338	338	338

Notes: This reports structural estimates of β, δ, and α obtained using minimum distance estimation of equations (1) in even columns or (2) and (3) in odd columns. Standard errors are reported in parentheses. Test statistic for $\beta = 1$ with p-value in brackets.

Table A.8: Aggregate Parameter Estimates, Alternate Net Completion Utility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\gamma = 2$		$\gamma = 2.5$		$\gamma = 3$		$\gamma = 3.25$	
$[\delta^2 u(1000) - v_1^\gamma - \beta^{\mathbf{1}_{d=1}}\delta \cdot v_2^\gamma] =$	1000	10000	1000	10000	1000	10000	1000	10000
β	0.952	0.952	0.946	0.946	0.938	0.940	0.934	0.906
	(0.029)	(0.029)	(0.039)	(0.039)	(0.050)	(0.050)	(0.055)	(0.053)
δ	0.992	0.996	0.967	0.967	0.942	0.943	0.931	0.915
	(0.017)	(0.017)	(0.022)	(0.022)	(0.027)	(0.027)	(0.030)	(0.029)
α	0.003	0.003	0.003	0.003	0.003	0.003	0.003	-0.016
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
$H_0 : \beta = 1.$ $(\chi^2(1))$	2.644	2.710	1.856	1.855	1.525	1.466	1.425	3.070
[p-value]	[0.104]	[0.100]	[0.173]	[0.173]	[0.217]	[0.226]	[0.233]	[0.080]
Criterion Value	0.311	0.326	0.249	0.250	0.212	0.212	0.199	0.185
# Vaccinators	338	338	338	338	338	338	338	338

Notes: This reports structural estimates of β, δ, and α obtained using minimum distance estimation of equations (1) in even columns or (2) and (3) in odd columns. Standard errors are reported in parentheses. Test statistic for $\beta = 1$ with p-value in brackets.

Table A.9: Tailoring with Alternative Controls

Dependent variable:	$\left\lvert \frac{v_{1,i}}{v_{2,i}} - 1 \right\rvert$		
Comparison group:	Untailored with $\tilde{R}_i \in +/-$ 1 s.d of Mean R_i^*	Untailored with $\tilde{R}_i \in +/-$ 1 s.d of Mean R_i^* $\lvert R_i^* - \tilde{R}_i \rvert > 0.10$	Untailored with Matched Distribution of $\tilde{R}_i$
	(1)	(2)	(3)
Tailored (=1)	-0.039*	-0.052**	-0.047**
	(0.021)	(0.025)	(0.022)
Constant	0.200***	0.231***	0.085
	(0.071)	(0.074)	(0.072)
Stratum FEs	Yes	Yes	Yes
Exclude 99th and 1st Percentiles	Yes	Yes	Yes
Drive 2 R_i^* or $\tilde{R}_i$	Yes	Yes	Yes
R-Squared	0.081	0.086	0.071
Mean in Untailored Contract	0.132	0.150	0.143
# Vaccinators	194	181	207
# Untailored Vaccinators	59	46	72

Notes: This table reports the effects of tailoring on the equality of effort provision over time. The measure $\lvert \frac{v_t}{v_{t+1}} - 1 \rvert$ (the percentage difference between tasks allocated to day 1 and day 2 of the drive) reflects the distance of the task allocation (v_1,v_2) from equality ($v_1 = v_2$). Mean (standard deviation) R_i^* in untailored group = 1.036 (0.165). Heteroskedasticity robust White standard errors reported in parentheses. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.10: Tailoring and Completion

Dependent variable:	$\lvert\frac{v_{1,i}}{v_{2,i}} - 1\rvert$					
	(1)	(2)	(3)	(4)	(5)	(6)
	Completed Drive 1			Failed Drive 1		
Tailored (=1)	-0.031	-0.055**	-0.042*	-0.977	-0.075**	-0.065**
	(0.039)	(0.025)	(0.024)	(0.652)	(0.030)	(0.029)
Constant	0.100***	0.114***	-0.051	1.967	0.181***	0.032
	(0.037)	(0.032)	(0.088)	(1.222)	(0.030)	(0.079)
Stratum FEs	Yes	Yes	Yes	Yes	Yes	Yes
Exclude 99th and 1st Percentiles	No	Yes	Yes	No	Yes	Yes
Drive 2 R_i^* or $\tilde{R}_i$	No	No	Yes	No	No	Yes
R-Squared	0.051	0.095	0.130	0.045	0.053	0.078
Mean in Untailored Contract	0.151	0.129	0.129	1.161	0.183	0.183
# Vaccinators	147	142	142	133	125	125

Notes: This table reports the effects of tailoring on the equality of effort provision over time. The measure $|\frac{v_t}{v_{t+1}} - 1|$ (the percentage difference between tasks allocated to day 1 and day 2 of the drive) reflects the distance of the task allocation (v_1,v_2) from equality ($v_1 = v_2$). Column (1) reports a regression of this measure on an indicator equal to one for subjects in the tailored group. Column (2) reports estimates from the same specification excluding outliers. Column (3) controls for the interest rate assignment in round 2. Column (4) provides estimates on the same sample as column (1) interacting treatment with being in the immediate choice condition. Columns (5) and (6) apply the same restrictions to the sample as columns (2) and (3) respectively. Ordinary least squares regressions. 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

Panel A: Dependent variable $\frac{\lvert v_{1,i}-v_{2,i}\rvert}{\sqrt{2}}$						
	(1)	(2)	(3)	(4)	(5)	(6)
Tailored (=1)	-1.758	-4.480**	-4.481**	-2.401	-1.703	-1.868
	(4.588)	(1.954)	(2.068)	(2.302)	(2.176)	(2.229)
Immediate Choice				20.994***	10.365***	10.597***
				(5.856)	(3.335)	(3.449)
Tailored x Immediate				2.127	-6.026	-6.220
				(9.793)	(4.084)	(4.136)
Constant	31.386***	16.399***	16.412**	19.464***	10.992***	14.128**
	(7.334)	(2.296)	(6.857)	(6.433)	(2.777)	(6.671)
Panel B: Dependent variable $\frac{\lvert v_{1,i}-v_{2,i}\rvert}{\sqrt{2}(v_{1,i}+v_{2,i})}$						
	(1)	(2)	(3)	(4)	(5)	(6)
Tailored (=1)	-0.014	-0.018***	-0.016**	-0.009	-0.007	-0.007
	(0.015)	(0.007)	(0.007)	(0.008)	(0.007)	(0.008)
Immediate Choice				0.080***	0.038***	0.037***
				(0.022)	(0.012)	(0.012)
Tailored x Immediate				-0.008	-0.024*	-0.023*
				(0.032)	(0.014)	(0.014)
Constant	0.103***	0.057***	0.033	0.058***	0.037***	0.025
	(0.023)	(0.008)	(0.022)	(0.020)	(0.010)	(0.022)
Panel C: Dependent variable $\lvert v_{1,i} - \frac{300}{1+R}\rvert$						
	(1)	(2)	(3)	(4)	(5)	(6)
Tailored (=1)	-2.612	-3.789***	-3.445**	-1.959	-1.509	-1.405
	(3.282)	(1.410)	(1.459)	(1.643)	(1.575)	(1.591)
Immediate Choice				16.650***	7.990***	7.844***
				(4.496)	(2.451)	(2.509)
Tailored x Immediate				-0.867	-4.972*	-4.850
				(6.891)	(2.938)	(2.974)
Constant	22.736***	12.014***	7.571	13.340***	7.849***	5.871
	(5.301)	(1.682)	(4.735)	(4.501)	(2.048)	(4.622)
Panel D: Dependent variable $\lvert v_{1,i} - \frac{300}{1+R}\rvert > 25$						
	(1)	(2)	(3)	(4)	(5)	(6)
Tailored (=1)	-0.454**	-0.654***	-0.578***	-0.282	-0.282	-0.235
	(0.191)	(0.221)	(0.216)	(0.317)	(0.314)	(0.309)
Immediate Choice				0.920***	0.720**	0.697**
				(0.272)	(0.283)	(0.286)
Tailored x Immediate				-0.275	-0.676	-0.653
				(0.408)	(0.462)	(0.460)
Constant	-0.639***	-0.887***	-1.568**	-1.231***	-1.312***	-1.812***
	(0.206)	(0.239)	(0.637)	(0.304)	(0.335)	(0.665)
Panel E: Dependent variable $min[v_{1,i}, v_{2,i}]$						
	(1)	(2)	(3)	(4)	(5)	(6)
Tailored (=1)	7.210**	8.112***	2.540*	4.764*	4.381*	0.843
	(3.512)	(2.051)	(1.416)	(2.650)	(2.643)	(1.567)
Immediate Choice				-20.562***	-11.804***	-6.815***
				(5.042)	(3.492)	(2.332)
Tailored x Immediate				4.672	8.194**	4.037
				(7.206)	(4.052)	(2.806)
Constant	126.824***	136.728***	208.758***	138.343***	142.870***	210.228***
	(5.181)	(2.645)	(4.541)	(4.905)	(3.359)	(4.433)
Stratum FEs	Yes	Yes	Yes	Yes	Yes	Yes
Exclude 99th and 1st Percentiles	No	Yes	Yes	No	Yes	Yes
Drive 2 R	No	No	Yes	No	No	Yes
# Vaccinators	280	267	267	280	267	267

Notes: This table reports the effects of tailoring on the equality of effort provision over time using several different measures of the distance of the task allocation (v_1,v_2) from equality ($v_1 = v_2$). Column (1) reports a regression of this measure on an indicator equal to one for subjects in the tailored group. Column (1) reports a regression of this measure on an indicator equal to one for subjects in the tailored group. Column (2) reports estimates from the same specification excluding outliers. Column (3) controls for the interest rate assignment in round 2. Column (4) provides estimates on the same sample as column (1) interacting treatment with being in the immediate choice condition. Columns (5) and (6) apply the same restrictions to the sample as columns (2) and (3) respectively. Heteroskedasticity robust White standard errors reported in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.12: Alternate Treatment Measures and Sample Restrictions

Panel A: Tailoring Intensity						
Dependent variable:	$\lvert\frac{v_{1,i}}{v_{2,i}} - 1\rvert$					
	(1)	(2)	(3)	(4)	(5)	(6)
Tailor Intensity	2.521	0.164**	0.110*	0.078	0.124	0.089
	(2.057)	(0.065)	(0.063)	(0.188)	(0.081)	(0.076)
Immediate Choice				-0.065	0.071***	0.068***
				(0.312)	(0.023)	(0.022)
Tailor Intensity x Immediate				4.537	0.071	0.057
				(3.809)	(0.133)	(0.131)
Constant	0.780*	0.104***	-0.009	0.765	0.067***	-0.018
	(0.457)	(0.018)	(0.058)	(0.482)	(0.020)	(0.058)
# Vaccinators	280	267	267	280	267	267
Panel B: Tailoring Intensity and Boundary Sample						
Dependent variable:	$\lvert\frac{v_{1,i}}{v_{2,i}} - 1\rvert$					
	(1)	(2)	(3)	(4)	(5)	(6)
Tailor Intensity	1.200	0.151**	0.124*	-0.038	0.054	0.025
	(1.006)	(0.069)	(0.065)	(0.127)	(0.060)	(0.054)
Immediate Choice				0.094	0.066***	0.064**
				(0.202)	(0.025)	(0.025)
Tailor Intensity x Immediate				2.075	0.148	0.154
				(1.848)	(0.119)	(0.114)
Constant	0.712*	0.152***	0.044	0.652*	0.119***	0.016
	(0.368)	(0.026)	(0.065)	(0.369)	(0.026)	(0.063)
# Vaccinators	337	320	320	337	320	320
Stratum FEs	Yes	Yes	Yes	Yes	Yes	Yes
Exclude 99th and 1st Percentiles	No	Yes	Yes	No	Yes	Yes
Drive 2 R	No	No	Yes	No	No	Yes

Notes: This table reports the effects of tailoring on the equality of effort provision over time. The measure $|\frac{v_t}{v_{t+1}} - 1|$ reflects the distance of the task allocation (v_1,v_2) from equality ($v_1 = v_2$). Column (1) reports a regression of this measure on an indicator equal to one for subjects in the tailored group. Column (2) reports estimates from the same specification excluding outliers. Column (3) controls for the interest rate assignment in round 2. Column (4) provides estimates on the same sample as column (1) interacting treatment with being in the immediate choice condition. Columns (5) and (6) apply the same restrictions to the sample as columns (2) and (3) respectively. Heteroskedasticity robust White standard errors reported in parentheses. $^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

report$^*p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.