Valuing the Time of the Self-Employed*

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Abstract

People's value of their own time is a key input in public policy evaluations—these evaluations should account for time taken away from work or leisure as a result of policy. Measuring this value for the self-employed is challenging, as, by definition, it is not priced by the market. Using rich choice data collected from farming households in western Kenya, we show that households exhibit non-transitive preferences. As a result, neither market wages nor standard valuation techniques correctly measure participants' value of time. Using a structural model, we identify the behavioral wedges in participants' choices, and find that distortions appear when households exchange cash either for time or for goods. Our model estimates suggest that valuing the time of the self-employed at 60% of the market wage is a reasonable rule of thumb.

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1 Introduction

Many development interventions aim to increase the profitability of small owner-operated businesses and farms, the primary source of income for the vast majority of poor households (Merotto et al., 2018). Accurately measuring the value that the self-employed assign to their own time is essential for evaluating the profitability and welfare impacts of most such interventions. The majority of such evaluations ascribe a value of zero to the time of the self-employed.[1] A minority use the prevailing market wage, which likely overstates the value of time in the presence of the labor-market frictions endemic to developing economies (Kaur, 2019; Breza et al., 2021; Jones et al., 2022).[2] Directly assessing participants' value of time—by, for example, eliciting the minimum wage they would accept for comparable labor—may be unreliable, as the frictions that distort labor markets may originate in individual choices.

We create a method that pairs multiple choices with structural estimation to recover individuals' value of their own time in the presence of labor and credit market imperfections, as well as a broad array of behavioral phenomena. We elicit the preferences of self-employed farmers in western Kenya over trade-offs involving three things: money, time, and lottery tickets for an irrigation pump. The choices over these alternatives show that many farmers in our study have intransitive preferences, confirming that direct trade-offs between money and time may produce unreliable results. Still, these choices alone bound the average value of time between 40–100% of the average *market wage*—the wage for casual labor in our sample. We then use a structural model that adopts a reduced-form approach to behavioral phenomena (Mullainathan et al., 2012; Gabaix, 2019) by modeling them as *wedges* that may separately affect each choice. This produces a more precise estimate of the average value of time: 60% of the average market wage. The results of the structural estimation indicate that wedges only appear in choices that involve money, rather than choices between time and a good. This finding is consistent with a class of behavioral models in which behavioral phenomena manifest only in transactions involving cash.

Our findings imply that common methods for valuing the time of the self-employed are likely to be inaccurate, and we offer several methods for researchers to obtain better measures. The common undervaluing of the time of the self-employed overstates the value of

[1]See Section 6.2 for a survey of studies in economics. It is worth noting that, in addition to the majority that value time at zero, an additional 24% do not attempt to value time at all. Of these 24%, several note that they would like to use *some* value of time, but believe it is too difficult, in their setting, to measure one.

[2]Putting this another way, de Janvry et al. (2017, p. 458) note, "It is well known that a large number of family farms do not seem economically viable when family labor is valued at the observed market wage rate in the casual labor market, implying that this is not the correct way to value family labor."

technologies or interventions that increase time commitments, and understates the value of those that save time.[3] This may explain why some technologies that appear profitable in evaluations are not adopted, and why labor-saving interventions attract relatively less attention (Suri, 2011; de Janvry et al., 2017). This is unfortunate, as more free time is associated with large improvements in mental and physical health, female labor-force participation, and education.[4] Our findings can be easily applied in different ways depending on the setting, allowing researchers to more accurately value interventions. Finally, our results suggest an additional explanation for the persistence of self-employment in places with relatively informal labor markets: the wedges driving choices in our data may hinder casual labor market transactions. Behavioral phenomena may cause workers to undervalue wages obtained through one-on-one negotiation, and employers to ration jobs. We find shading when wages are paid in cash, but not in goods. The former finding is consistent with the theory of efficiency wages (Hart and Moore, 2008; Fehr et al., 2011).

Our study augments an elicitation that directly measures participants' value of time—their reservation wage for temporary jobs—with two others that allow for an indirect assessment of the value of time, as described in Section 2. Those additional elicitations allow participants to express the value of a good—lottery tickets with a 1/10 chance of winning an irrigation pump—in both money and hours of casual labor. By dividing these two quantities, we obtain an indirect assessment of participants' value of time. Each elicitation is based on a standard Becker-DeGroot-Marschak (BDM) mechanism, which has been widely used to obtain valuations, including in low-income contexts (Becker et al., 1964; Crockett and Oprea, 2012; Holt and Smith, 2016; Azrieli et al., 2018; Berry et al., 2020; Burchardi et al., 2021).

Under a benchmark expected-utility model that allows for labor market rigidities and credit constraints, the direct and indirect values of time should be the same, but, in our choice data, they are not, as described in Section 3. The average value of time measured directly is similar to the average market wage, while the value of time measured indirectly is 40% of the average market wage. This difference is caused by a large proportion of our participants making intransitive choices.[5] Despite these intransitivities, the direct and

[3] Valuing time using the market wage would tend to have the opposite effect.

[4] See for example Xiao et al. (2013); Albanesi and Olivetti (2016); Schilbach (2019); Bessone et al. (2021); Whillans and West (2021).

[5] As described in the AEA RCT Registry (AEARCTR-0004110), our prior was that wedges between the direct and indirect values of time and the market wage might arise from characteristics of the labor market, or from characteristics of laborers—for example, norms against accepting lower wages (Agness et al., 2019). As described in Appendix E.4, we did not find that norms surrounding low-wage work are influential in this setting.

indirect measures bound the average value of time between 40–100% of the average market wage. These bounds may be sufficient for some studies; however, others may require a point estimate.

We show how a model with wedges can be structurally estimated on our experimental data to recover an un-wedged value of time in Section 4, and find it is, on average, 60% of the average market wage. The structural model uses data from all three elicitations to identify—under assumptions supported by our data—the relative magnitude of the wedge present in each trade-off. Once identified, the effect of the wedges can be removed to produce estimates of individuals' value of time. As this model nests the benchmark model, this estimate is robust to credit constraints or labor rigidities, in addition to a broad class of behavioral features. The model estimation shows that wedges affect choices in which money is either spent on goods or received for labor, but not when labor is exchanged for goods.

The un-wedged value of time is identified regardless of the source of the wedges in farmers' choices. That is, the economic interpretation of wedges is only important when a researcher seeks to apply a structural parameter in a different setting. As many interventions evaluate naturalistic trade-offs made by farmers between time and a good—for example, working longer for additional crop yield—wedges generated by either behavioral phenomena or features of the elicitation design are unlikely to be present, and therefore an un-wedged value of time is likely to be appropriate across a broad range of settings.

Our results are consistent with a behavioral model, described in Section 5, in which decision makers deflate the value of cash they receive as wages, and inflate the value of cash they pay for goods. That is, our results can be explained by a self-serving bias, or loss aversion, that applies only to cash transactions. We show that wedges are smaller for two groups in which these phenomena are likely to be muted—experienced casual laborers, and those experienced with paying for goods in cash. We then consider several potential alternative sources of our findings—including a tightening of credit constraints, stigma for accepting low wages, and present bias. These alternatives are ruled out by either our study design, or by examining additional data from within our study.

We conclude with a discussion of the broader implications of our results, including how our data improve the understanding of labor markets in developing countries in Section 6. We methodically review the economic literature from 2016–2021, and show that it uses relatively extreme values of time, with the majority of studies using a value of zero. We then describe how researchers can best make use of our results, and offer guidance for bounding the value of time based on differences in labor market conditions—which can be measured with a brief survey. Finally, we apply our results to some prior studies to illustrate when more reliable estimates for the value of time are likely to affect program evaluations.

Our results inform a broad literature evaluating the welfare impacts of interventions. For example, providing agricultural inputs—such as fertilizer or seeds—increases hours worked on the farm (Duflo et al., 2011; Emerick et al., 2016), while supporting mechanization decreases hours worked (Caunedo and Kala, 2021). Similarly, improving tenancy contracts (Burchardi et al., 2018) or property rights (Goldstein et al., 2018) affects work hours. Measuring the welfare effects of these interventions requires an estimate of workers' value of time, but market wages are often a poor proxy for this value, as incomplete factor markets drive a wedge between shadow and market prices (Benjamin, 1992; LaFave and Thomas, 2016).

Difficulty assigning a value to workers' time has consequently led to widely varying methodologies. For example, Goldstein et al. (2018) assume the household does not face an opportunity cost of supplying labor when studying the effect of a change in property rights. In contrast, Emerick et al. (2016) value all labor at the average market wage when estimating the profitability of a flood-resistant type of rice in India.[6]

Mas and Pallais (2019) offer the first experimental estimates of the value of time among job-seekers in the U.S., but do not consider behavioral phenomena.[7] Instead, they use estimates obtained by simply offering a choice between time and money, a choice that we show produces unreliable estimates. In their study of the gains from mechanization in agriculture, Caunedo and Kala (2021) estimate the shadow cost of family labor in India to be approximately 90% of the average market wage in rural India. In contrast to their approach, our method can be directly applied without relying on noisy measurements of farm inputs or structural assumptions required to identify smallholder production functions.

A related, but methodologically distinct, literature uses travel-cost-based estimates of household time valuation as inputs for welfare analysis, benefit-cost analysis, and value of statistical life calculations (Jeuland et al., 2010; Kremer et al., 2011; Jeuland and Pattanayak, 2012). Studies in this literature measure the value of travel time using either stated willing-

[6]A similar issue arises among researchers testing for labor misallocation: evaluating welfare gaps requires an estimate of the value of time gained or lost when workers transition across sectors. There is a substantial wage premium in the non-agricultural sector of most low-income countries—possibly owing to migration barriers, such as inadequate information (Baseler, 2023) and financial constraints (Bryan et al., 2014)—but non-agricultural workers also work longer hours on average (Caselli, 2005; Restuccia et al., 2008; Gollin et al., 2014). When measuring this agricultural productivity gap, Gollin et al. (2014) control for hours worked, while Pulido and Święcki (2018) do not.

[7]As behavioral phenomena, such as self-serving bias, are common in high-income contexts (see, for example, Babcock et al., 1995; Babcock and Loewenstein, 1997), the market wage and other standard valuation techniques may also produce unreliable estimates of the value of time in high-income economies.

ness to pay—which we show is inaccurate—or a revealed preference approach using variation in observed, non-work travel times—which biases estimates in the presence of credit constraints, as faster modes of transport are usually more expensive. Our approach improves on these methods by identifying the marginal value of work time—the relevant input for most economic cost-benefit analyses—at the individual level, while accounting for a broad class of market imperfections and behavioral phenomena.

2 Study Design and Choice Data

In this section we describe our study setting, before turning to a more detailed description of the choices offered to farmers.

2.1 Setting

The study took place in rural Kenya in April and May, 2019, with a sample of farming households that had first been enumerated in 2014 for a separate randomized controlled trial (Chassang et al., 2023). In that trial, KickStart irrigation pumps were distributed to some farmers in "treatment" villages. For the present study, we focus on the "control" villages from Chassang et al. (2023), in which no pumps were distributed. Villages in Chassang et al. (2023) were selected to ensure there were a sufficient number of farmers with land suitable for manual pump irrigation—that is, close to a water source. In each village, an "anchor farmer" was identified who lived close to a water source, and a snowball sampling technique was used to generate a list of 10 to 25 neighboring farmers with land suitable for pump irrigation. Focusing on control villages from the earlier study gave a list of 411 potential households for our study, out of which we were able to find and complete activities with 332, or 81%. Appendix B.1 provides further details about sampling.

To mimic a setting in which households endogenously choose how to allocate labor supply across individuals, we allowed each household to choose a single adult member to participate after the household learned about the study. We required that this individual participate in all activities. Ninety-five percent of households chose either the female or male head of household. As shown in Table C.1, average values of time were consistent across various demographic groups, suggesting that households did, indeed, allocate time similarly regardless of the identity of the person chosen.

Table 1 displays sample summary statistics. The average participant was 48 years old and had 6.8 years of education. Women comprised 69% of our sample. Men and women in our sample had very similar values of time; see Table C.1. The average household in our study earned about 50,000 KSh ($461) per year. Households in our study all did at least some agricultural work, and had land suitable for manual irrigation. On average, about 40% of households' income came from selling crops they had grown. Most households also engaged in micro-entrepreneurship, or provided casual labor on neighbors' farms.

The jobs we offered—weeding and preparing land—were designed to mimic paid casual labor that most households engage in. Casual labor is, by far, the second most common source of income (after farming) for participants. In our sample, 42% of participants had performed casual labor—and 46% of households had hired casual laborers—in the prior 3 months. These participants had worked an average of 13 days in the prior 3 months, with an average workday of 4.2 hours. Average wages were 82 KSh (about $0.77) per hour.[8]

Farmers in our sample reported struggling to find paid work. While most farmers (53%) reported that they could definitely find one day of work with a week's notice, only 27% believed they could find a full week (six days) of work. Only 34% believed they could find a day of work with one day's notice. Moreover, farmers believed that working hours would be limited: of those who believed they could find work with a day's notice, the maximum amount of work they said they could find was 4.3 hours, on average. This suggests that farmers in this setting cannot flexibly choose how much labor to supply to the market—a widespread feature of rural labor markets (Breza et al., 2021)—and that market wages may not accurately measure the value that individuals assign to their time.

The irrigation pump used in this study approximates a common impact-evaluation environment: adoption of a technology with low baseline usage rates. Our prior work had identified a sample of farmers with suitable land for pump irrigation, and who were familiar with, but had not widely adopted, the pump.

Our analysis in Section 3.1 relies on the good in the experimental choices having a small value compared to the farmers' overall budgets. The pump is expensive compared to farmers' budgets, so we used lottery tickets offering a 1-in-10 chance of winning a pump. As expected, these tickets had a relatively small average subjective value of 111 KSh, about what the average participant could earn from 1.4 hours of casual labor.[9]

[8] These wages are high relative to average daily household earnings of 135 KSh. This is because average working hours are low—about 4 hours per week among those who worked—consistent with labor rationing. In line with the literature, we use the term *labor rationing* to describe situations in which qualified workers would like to work additional hours at the market wage, but cannot find employment.

[9] The average subjective value for a lottery ticket is well below 950 KSh—one-tenth of the pump's retail price—likely due to risk aversion and low willingness to pay for productive technologies in general (see

Table 1: Summary Statistics

	Mean	Std. Dev.	N
Demographics			
Age	47.7	14.3	328
Years of education	6.77	3.60	307
Female	0.69	0.46	332
No male head in household	0.14	0.35	326
Number of adults (age 18 or over) in household	2.68	1.29	324
Number of children (under 18 years) in household	3.97	2.37	324
Household Income and Wealth			
Land area under cultivation	2.28	1.85	324
Household income (KSh, past year)	49,121	68,358	330
Income share from sale of crops	0.41	0.38	330
Does not have 5,000 KSh saved	0.76	0.43	326
Micro-entrepreneur	0.44	0.50	330
Casual Labor			
Performed or hired casual labor within past 3 months	0.72	0.45	332
Supplies casual labor	0.42	0.50	332
of which, days worked in last 3 months	13.1	16.5	141
during which, hours worked per day	4.21	1.39	141
among which, hourly earnings	81.6	66.2	129
Hires casual labor	0.46	0.50	332
of which, days hired in last 3 months	6.53	8.48	154
during which, number of workers hired	3.18	3.49	154
among which, hours hired per day	3.95	1.27	154
among which, hourly wage paid	59.8	33.4	137
Could find 6 days of work next week	0.27	0.45	332
Could find 1 day of work next week	0.53	0.50	332
Could find 2 hours of work next week	0.43	0.50	332
Could find work tomorrow	0.34	0.47	332
if so, maximum hours available	4.28	1.95	113
Exposure to Irrigation Pump			
Owns a MoneyMaker irrigation pump	0.01	0.10	332
Has used a MoneyMaker irrigation pump	0.11	0.32	332
Familiar with the MoneyMaker irrigation pump	0.99	0.10	332
Considered buying pump	0.59	0.49	319
Self-reported valuation of pump (KSh)	4,432	3,318	303

Each observation is a single farmer. Data are taken from multiple rounds of household surveys between 2014–2019. Values are coded as missing if: the farmer was not surveyed when the relevant information was collected; they answered "Don't Know"; or if the question is not applicable. All monetary units are expressed in 2019 Kenyan shillings (KSh).

Footnote 20). Importantly, risk aversion does not affect the predictions of Section 3.1, as discussed in Section 5.3.

The manually powered irrigation pumps we used (branded as "MoneyMaker" by KickStart) are specifically designed for smallholder farmers. An experiment that allocated these pumps to women in Kenya found that they increase net farm revenue by 13%, offsetting their purchase cost after 3 years (Dyer and Shapiro, 2023), although the study did not account for farmers' value of time. However, at baseline, only 11% of farmers in our study had tried a KickStart pump themselves. The main reasons given for this low uptake are the pumps' expensiveness (they retail for 9,500 KSh, or about $89), and the fear that the pumps may be uncomfortable to operate.

2.2 Choices

Each farmer in our sample was given three choices that used the BDM design (Becker et al., 1964), as implemented in Berry et al. (2020).[10] This implementation made the choices relatively simple and naturalistic. Participants were asked to state their preferences for some object—for example a lottery ticket for a pump—in some unit of payment—for example, hours of labor. After stating their preferences, a random price was drawn, and if their stated value was higher than the price, that is what they paid for the object. If their value was lower than the price, no transaction occurred.[11] Burchardi et al. (2021) implement similar BDMs in rural Uganda, and find high comprehension across several design variations.

Choice RW: Reservation Wage. In the *reservation wage* (RW) choice, farmers were offered the option to receive a cash payment for casual labor.

We explained to each farmer that we were offering one-time, 2-hour jobs performing casual agricultural labor in a different village. We asked each farmer whether they would be willing to accept the job at 120 KSh per hour. If they answered "no," we asked about their reservation wage directly. If they answered "yes," we asked whether they would accept the job at incrementally lower wages until they changed their answer to "no." The minimum amount of money the farmer was willing to accept for the job is denoted by m^{RW}.

Choice CB: Cash Bid. In the *cash bid* (CB) choice, farmers were offered the option to obtain a lottery ticket for the MoneyMaker pump in exchange for money.

[10]Specifically, the surveyor read a description of the procedure, emphasizing that no negotiation would be allowed, and played practice rounds to ensure comprehension.

[11]Thus, the BDM design is like a second-price auction with a single participant and a random reserve price. Like a second-price auction, the BDM design is incentive compatible, and revelation of true values is a dominant strategy. Complete implementation details are provided in Appendix B. Full scripts are available here.

We explained to each farmer that we were selling lottery tickets offering 1-in-10 odds of winning a MoneyMaker pump. We collected willingness to pay in cash by asking the farmer whether they would be willing to pay a low price of 20 KSh, and then asking the same question for increasingly higher prices, until the farmer declined the offer.[12] The maximum amount of money the farmer was willing to pay for the lottery ticket is denoted by m^{CB}.

Choice TB: Time Bid. In the *time bid* (TB) choice, farmers were offered the option to obtain a lottery ticket for the MoneyMaker pump in exchange for casual labor.

As in Choice CB, we explained to each farmer that we were offering lottery tickets with 1-in-10 odds of winning a MoneyMaker pump. We collected willingness to pay in time by asking the farmer whether they would be willing to work 30 minutes for the ticket, and then asking the same question for increasingly higher amounts of time, until the farmer declined the offer. The maximum amount of time the farmer was willing to work for the lottery ticket is denoted by h^{TB}.

Offer Revelation and Payment. Choices CB and TB occurred at the beginning of the survey, in random order, and Choice RW came next. Farmers were told they would receive a random price for either Choice CB or TB, but not both, to minimize interactions across these choices. Prices were drawn at the end of the three elicitations. Scripts read to each farmer explained that there could be absolutely no bargaining once the prices were drawn. Work days as a result of choices in RW were scheduled about 1 week after either work days for choices in TB or payments for choices in CB, in order to further reduce interactions across choices.

We implemented the random draws such that farmers could be sure their choices did not influence the drawn prices. Before the survey, we assigned each farmer a random ticket price in either cash or time (but not both), and a random cash wage. Cash wages were assigned independently of ticket price. This information was written on a card and inserted into a sealed envelope, which was shown to the farmer at the beginning of the survey. After the farmer had made their three choices, the envelope was opened, and the ticket price, payment denomination (cash or time), and wage were revealed.

Cash winners—farmers who drew a cash price weakly lower than m^{CB}—were asked to make a down payment of 20 KSh (\$0.19), and were given about one week to collect the remainder. This ensured that farmers were not limited by their cash-on-hand the day of the

[12] We chose descending wages in RW, and ascending prices in CB and TB, so that in all choices participants would start by answering "Yes" until switching to "No."

survey. Time winners—farmers who drew a time price weakly lower than h^{TB}—were scheduled for casual work approximately one week from the date of the survey. Casual jobs for eligible wage workers—farmers who drew an hourly cash wage weakly greater than $m^{RW}/2$—were scheduled approximately two weeks from the date of the survey.[13] We provided transportation to and from job sites, and transport time counted towards work commitments.[14]

Direct and Indirect Value of Time. Our design lets us compute two measures of each farmer's value of time: an hourly *direct value of time* (DVT)—$m^{RW}/2$—reflecting preferences over direct trade-offs between time and money; and an hourly *indirect value of time* (IVT)—m^{CB}/h^{TB}—reflecting trade-offs between money and the lottery, and time and the lottery.

In the next section, we show that these two different values of time should be approximately equal under our benchmark model.

3 The Benchmark Model and Evidence Against It

We model farmers' choices in a framework that allows for credit constraints and *labor rationing*. Labor rationing implies that a farmer's reservation wage may be strictly less than the market wage. The literature discusses a number of mechanisms that may result in workers being off their labor supply curve, for example, downward wage rigidity resulting from social norms or effort retaliation (Kaur, 2019), or workers acting as a cartel to withhold work from the market and increase wages (Breza et al., 2019). While our model is agnostic as to the source of labor rationing, in Section 6.1 we discuss possible mechanisms that are consistent with our data.

A farmer makes decisions over bundles $b \equiv (\tau, h, m)$ corresponding to:

- obtaining or not the lottery ticket $\tau \in \{0, 1\}$,
- time spent on work $h \in \mathbb{R}^+$,
- a monetary transfer m that can be sent ($m > 0$ for symmetry with h) or received ($m < 0$).

[13] Compliance rates were 88% for cash payments and 74–75% for casual labor tasks. We discuss implications of non-compliance in Section E.5.

[14] We told every respondent a specific time on a specific day when we would meet them to begin the work. The relevant part of the script was, "We will provide transport to and from the job site. This will happen on [DATE] starting at [TIME]. Someone from IPA would come and get you (and possibly other workers from your village) at that time." We set the work day 1–2 weeks out from the initial survey, giving farmers substantial time to reschedule tasks. Section E.3 shows it is unlikely that unobserved fixed costs associated with the casual jobs are influencing our results.

Preferences are represented by the indirect utility function

$$V(\tau, h, m) = \max_{c,l} u(c, l+h) + \mathbb{E}_\theta[v(I + wl + \tau\theta - c - m)] \tag{1}$$
$$l, c \text{ s.t. } l \leq \bar{l}$$
$$I + wl - c - m \geq \underline{k}$$

Choice variables c and l denote current consumption and labor supply, respectively. Utility function u captures preferences over consumption and labor. The continuation value of next period wealth is captured by v. Non-labor income is denoted by I, w is the wage per unit of labor, and $\theta \in [0, \overline{\theta}]$ is a random variable capturing the returns to the lottery. Labor rationing is imposed through $\bar{l}$, while credit constraints are modeled with $\underline{k}$—the lower bound on remaining capital after decisions are made. The Lagrange multipliers associated with the labor and capital constraints are denoted by λ and κ, respectively.

Without loss of generality, we normalize $V(0, 0, 0) = 0$ and assume:

Assumption 1 (smooth preferences)**.** *u and v are strictly concave, and continuously differentiable.*

An immediate implication is that consumption and labor choices c and l, as well as Lagrange multipliers κ and λ, are continuous functions of experimental bundle b.[15]

Lemma 1. *Given $b = (\tau, h, m)$, optimal choices $c|_b$ and $l|_b$ in* (1) *are unique and continuous in b. Lagrange multipliers $\kappa|_b$ and $\lambda|_b$ are also unique and continuous in b.*

The fact that the Lagrange multipliers are continuous plays a central role in our interpretation. Small changes in choice variables τ, h, and m parameterizing optimization problem (1) have a small impact on the shadow value of capital and labor.[16] This appears to be a reasonable assumption; in Section 5.3, we explore the possibility that purchasing the lottery ticket has second-order effects on credit constraints, and can rule this out with our data.

Lemma 1 and the Envelope Theorem for Lagrange multipliers (Milgrom and Segal, 2002) imply that the following first order approximation (using the familiar Big-O notation) holds.

[15] We extend V to values of τ in $(0, 1)$ using the right-hand side of (1), capturing scaled-down returns $\tau\theta$ to owning a pump.

[16] Work days were scheduled 1 to 2 weeks in advance so that farmers could reshuffle tasks across days, implying that within-day changes in working hours should be marginal. Lottery tickets had a relatively small average subjective value of 111 KSh, representing roughly what the average participant could earn from 1.4 hours of casual labor, implying that purchasing a ticket is unlikely to significantly change returns to capital.

Theorem 1 (first-order approximation)**.** *Under Assumption 1,*

$$V(\tau, h, m) = \tau V_\tau + hV_h + mV_m + O\left(\overline{\theta}^2 + h^2 + m^2\right) \tag{2}$$

with

$$V_\tau = \mathbb{E}_\theta[\theta v'(I + wl|_0 - c|_0)], \quad V_h = u_l(c|_0, l|_0), \quad V_m = -v'(I + wl|_0 - c|_0) - \kappa|_0.$$

Where $l|_0$, $c|_0$, and $\kappa|_0$ denote the values of $l|_b$, $c|_b$, and $\kappa|_b$ at $b = (\tau, h, m) = (0, 0, 0)$.

Theorem 1 shows that the indirect utility function V is a locally linear function of experimental choices (τ, h, m), weighted by preference parameters reflecting the marginal indirect utility value of those choices (V_τ, V_h, V_m). The fact that credit constraints enter (2) only though the value of money, V_m, is useful in examining the potential second-order effects of credit constraints in Section 5.3.

We refer to parameter V_h/V_m, the value of time expressed in the numeraire KSh, as the *structural value of time* (SVT).

3.1 Testable Implication of the Benchmark Model

Importantly, we believe that the choices in our study satisfy the requirements of Theorem 1: farmers are making decisions over bundles with values that are small compared to the total value of their overall optimization problem. Choice RW (reservation wage) involved 2 hours of work. The average cash bid m^{CB} for lottery tickets in choice CB was 111 KSh (equivalent to about 1.4 times the hourly average market wage). The average time bid h^{TB} for lottery tickets in choice TB was 4 hours—roughly equivalent to an average day of casual labor. As a result, the remainder of this section attempts to interpret choice data using linearized preferences (2). We show that this leads to a contradiction.

Direct Value of Time. A farmer's optimal choice m^{RW} corresponds to the amount of money for which the farmer is indifferent between performing two hours of work for an amount m^{RW}, and the status quo:

$$V(\tau = 0, h = 2, m = -m^{RW}) = V(\tau = 0, h = 0, m = 0).$$

Using the first-order approximation (2), this implies that $2V_h - m^{RW}V_m = 0$. Thus, the direct value of time (DVT), defined as $\text{DVT} \equiv \frac{m^{RW}}{2}$, correctly estimates SVT:

$$\text{DVT} \equiv \frac{m^{RW}}{2} = \frac{V_h}{V_m} = \text{SVT}.$$

Indirect Value of Time. The indirect value of time (IVT), defined as $\text{IVT} \equiv \frac{m^{CB}}{h^{TB}}$, can also be interpreted using (2). A farmer's optimal choices m^{CB} and h^{TB} satisfy

$$V(\tau = 1, h = 0, m = m^{CB}) = V(0,0,0) \quad \text{and} \quad V(\tau = 1, h = h^{TB}, m = 0) = V(0,0,0),$$

respectively. Theorem 1 implies that

$$m^{CB} = -\frac{V_\tau}{V_m} \quad \text{and} \quad h^{TB} = -\frac{V_\tau}{V_h}.$$

Hence,

$$\text{IVT} \equiv \frac{m^{CB}}{h^{TB}} = \frac{V_h}{V_m} = \text{DVT} = \text{SVT}. \tag{3}$$

Thus, under our benchmark model, the direct and indirect values of time should be equal. The next subsection shows that, in our choice data, they are not. This implies that at least one of IVT and DVT, and possibly both, incorrectly estimate SVT.

3.2 Evidence of Preference Intransitivity

The data clearly reject the benchmark model, as shown in Table 2. The average direct value of time, DVT, elicited through choice RW, is 83 KSh/hour. This is close to the average reported wage for casual labor (82 KSh/hour). In contrast, the average indirect value of time, IVT, inferred from choices CB and TB, is 30 KSh/hour, substantially below the mean DVT (difference = 53 KSh/hour; p-val < 0.01). Moreover, the distribution of DVT first-order stochastically dominates the distribution of IVT, as shown in Figure 1. Indeed, 81% of farmers expressed a DVT strictly above their IVT.

At the individual level, these data suggest that a majority of farmers have cyclical, non-transitive preferences. For instance, one of the farmers in our study, from the village of Turumba A, expressed $m^{RW}/2 = 80$ KSh, $m^{CB} = 100$ KSh, and $h^{TB} = 4$ hours (which matches the average values of these choices). This farmer would then exhibit the following choice behavior:

Table 2: Choice Data ($N = 332$ farmers)

	Mean	Std. Dev.	p25	p50	p75
Direct value of time ($DVT_i = m_i^{RW}/2$)	82.8	54.0	50	80	100
Indirect value of time (IVT_i)	29.8	35.2	2.79	20	40
Cash bid (m_i^{CB})	110.8	125.5	20	100	155
Time bid (h_i^{TB})	4.01	2.17	3	4	5
DVT–IVT wedge ($\widehat{\omega}_i$)	0.30	1.22	0.28	0.71	0.98

Each observation is a farmer. Currency units are Kenyan shillings (1 USD = 107 KSh). Cash bids, time bids, and DVT elicited through BDM. IVT = cash bid / time bid. DVT–IVT wedge = $1 - IVT/DVT$. p25, p50, and p75 are the 25th, 50th, and 75th percentiles.

- 150 KSh $\prec$ 3 hours (as $m^{RW}/2 = 80$),
- $\tau = 1 \prec$ 150 KSh (as $m^{CB} = 100 < 150$), and
- 3 hours labor $\prec \tau = 1$ (as $h^{TB} = 4$).

Examining these choices starting from the bottom reveals a cycle: 3 hours $\prec \tau = 1 \prec$ 150 KSh $\prec$ 3 hours.

For each farmer, we define

$$\widehat{\omega}_i = 1 - \frac{\mathrm{IVT_i}}{\mathrm{DVT_i}} \tag{4}$$

as a measure of preference intransitivity, which we term the *DVT–IVT wedge*.[17] The average value of $\widehat{\omega}_i$ is 0.3, substantially higher than the benchmark prediction $\widehat{\omega}_i = 0$ (p-val<0.01).[18]

Credit and Labor Constraints. Although our model explicitly builds in credit and labor constraints, describing why they are unlikely to be driving the wedge between IVT and DVT provides a deeper understanding of Theorem 1. The important condition underlying this result is that the choices we offer have only second-order effects on the shadow value of money or time.

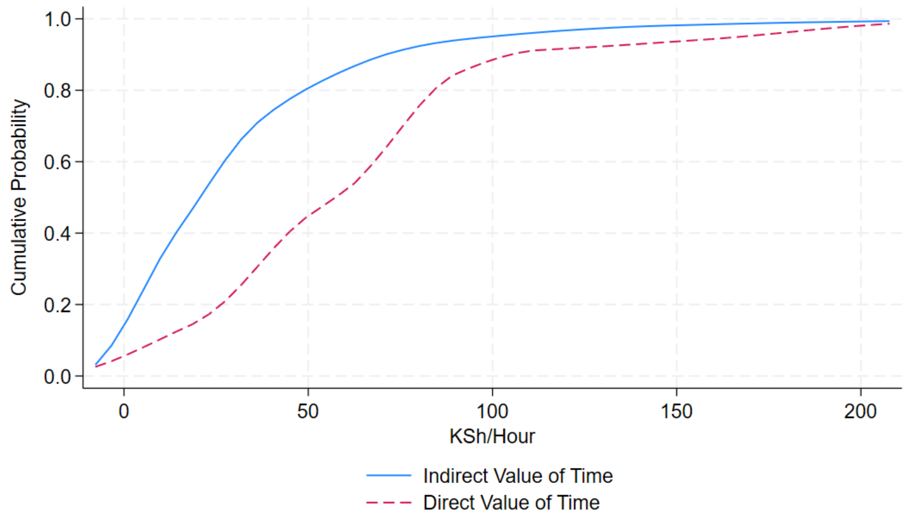
If a farmer is credit constrained, then they will have a high shadow value of money, but this will be reflected in both their IVT and DVT. In particular, a higher shadow value of money will lower both a farmer's willingness to pay for a lottery ticket, m^{CB}, as well as their reservation wage, m^{RW}.[19] This will lower both IVT and DVT equally, resulting in no

[17] The hat emphasizes that $\widehat{\omega}_i$ is empirically observable from choice data.

[18] Note that the median value of $\widehat{\omega}_i$, 0.71, is much larger than the mean of 0.3. This is due to a long left tail in the distribution, with 17% of farmers exhibiting a $\widehat{\omega}_i < 0$. Estimating SVT does not require that $\widehat{\omega}_i$ be positive. Moreover, our results are robust to truncating these negative values—see Appendix Table E.4. We can also reject that the median of $\widehat{\omega}_i$ is equal to 0 (p-val < 0.01).

[19] We gave farmers one week to pay, so that they were not constrained by their cash on hand the day they made their bid.

Figure 1: The value of time is smaller when estimated indirectly through bids in money and time for the same good than when estimated directly through reservation wages.



Kernel-smoothed cumulative distribution functions (van Kerm, 2012) estimated on all farmers.

wedge between the two. The only way that credit constraints could create such a wedge would be if the decision to buy a lottery ticket significantly tightened credit constraints, or if working for two hours significantly loosened them. In Section 5.3, we consider a model in which purchasing the lottery ticket ($\tau = 1$) significantly tightens credit constraints, and show that it is inconsistent with our data. This is not surprising, as many farmers were probably already credit constrained before facing the choices we offered. Moreover, the impact of investing in a lottery ticket is very minor compared to other investment opportunities.[20]

4 Structural Estimation of a Model With Wedges

In this section, we add wedges to the benchmark model of Section 3 that can explain the observed difference between DVT and IVT. We then estimate this extended model on our choice data to recover SVT, which—as we argue in Section 5.1—is the appropriate parameter for researchers to use in most settings. We then interpret the results of this estimation in terms of behavioral and other factors.

[20]Examples of high-return investment opportunities with low take-up rates include grain storage facilities (Burke et al., 2018), irrigation (Jones et al., 2022), or, outside the realm of agriculture, antimalarial bed nets (Cohen and Dupas, 2010). Similar logic applies to labor constraints.

4.1 A Model With Wedges

To account for choice intransitivities, we allow farmers' choice problems to exhibit three separate wedges: under reservation wage choice RW, the size of monetary benefit is reduced by a factor $1-\omega^{RW}$; under cash bid CB, the returns θ to owning the pump are scaled down by a factor $1-\omega^{CB}$; under time bid TB, the returns θ to owning the pump are scaled down by a factor $1-\omega^{TB}$. Thus, if $\omega^j = 0$, this implies that the associated choice j is not affected by a wedge. Choices RW, CB, and TB are characterized by the indifference conditions

$$\begin{aligned} V(0,2,-(1-\omega^{RW})m^{RW}) &= 0 \\ V(1-\omega^{CB}, m^{CB}, 0) &= 0 \\ V(1-\omega^{TB}, 0, h^{TB}) &= 0 \end{aligned} \qquad \Rightarrow \qquad \begin{aligned} 2V_h - (1-\omega^{RW})V_m m^{RW} &= 0, \\ (1-\omega^{CB})V_\tau + V_m m^{CB} &= 0, \\ (1-\omega^{TB})V_\tau + V_h h^{TB} &= 0, \end{aligned} \tag{5}$$

where the equations on the right-hand side follow from linearizing using (2).

Note that there is a symmetry between shrinking the value of one object of choice and inflating the value of the other object: for example, shrinking the value of the monetary payment in Choice RW (reservation wage) by an amount $1-\omega^{RW}$ is equivalent to inflating the value of the number of hours worked in that choice by $1/(1-\omega^{RW})$. Using this structure, we can solve for m^{RW}, m^{CB}, and h^{TB} in the three choices and obtain:

$$\text{DVT} \equiv \frac{m^{RW}}{2} = \frac{V_h}{(1-\omega^{RW})V_m} \quad \text{and} \quad \text{IVT} \equiv \frac{m^{CB}}{h^{TB}} = \frac{(1-\omega^{CB})V_h}{(1-\omega^{TB})V_m},$$

leading to an empirically observable DVT–IVT wedge $\widehat{\omega}$ defined as

$$\widehat{\omega} \equiv 1 - \frac{\text{IVT}}{\text{DVT}} = 1 - \frac{(1-\omega^{RW})(1-\omega^{CB})}{(1-\omega^{TB})}. \tag{6}$$

Bounding SVT. The preference parameter V_h/V_m—the structural value of time (SVT)—is not identified by choice data alone, as any triplet $(\omega^{RW}, \omega^{CB}, \omega^{TB})$ that satisfies (6) rationalizes the wedge between DVT and IVT. For example, note that a wedge in only Choice RW ($\omega^{RW} = \widehat{\omega}$ and $\omega^{CB} = \omega^{TB} = 0$) would lead to IVT=SVT, and DVT>SVT. A wedge in only Choice CB ($\omega^{CB} = \widehat{\omega}$ and $\omega^{RW} = \omega^{TB} = 0$) would lead to DVT=SVT, and IVT<SVT. For interior values of the wedges ω^{RW}, ω^{CB}, and ω^{TB} that satisfy (6), SVT will be a weighted average of DVT and IVT, with weights determined by the (unknown) values of the wedges. Assuming that $\omega^{CB} \geq \omega^{TB}$—which holds in our estimation results—we can bound SVT in [IVT, DVT] without additional assumptions—see Appendix A for a proof of this statement.

In our data, those bounds correspond to about 40% and 100% of the average market wage. As we show in Section 6 by re-examining the conclusions of prior evaluations, knowing that the value of time is somewhere in this broad range may be sufficient to draw conclusions about whether or not a particular intervention is beneficial.

Point Identification of SVT. There are also interventions where more precise estimates are necessary. In the next subsection, we use the fact that different combinations of wedges do not predict the same patterns of correlation across choices m^{RW}, m^{CB}, and h^{TB} to identify, under some assumptions, the distribution of preference parameters ω^{RW}, ω^{CB}, and ω^{TB} in the population. This yields a precise estimate of SVT.

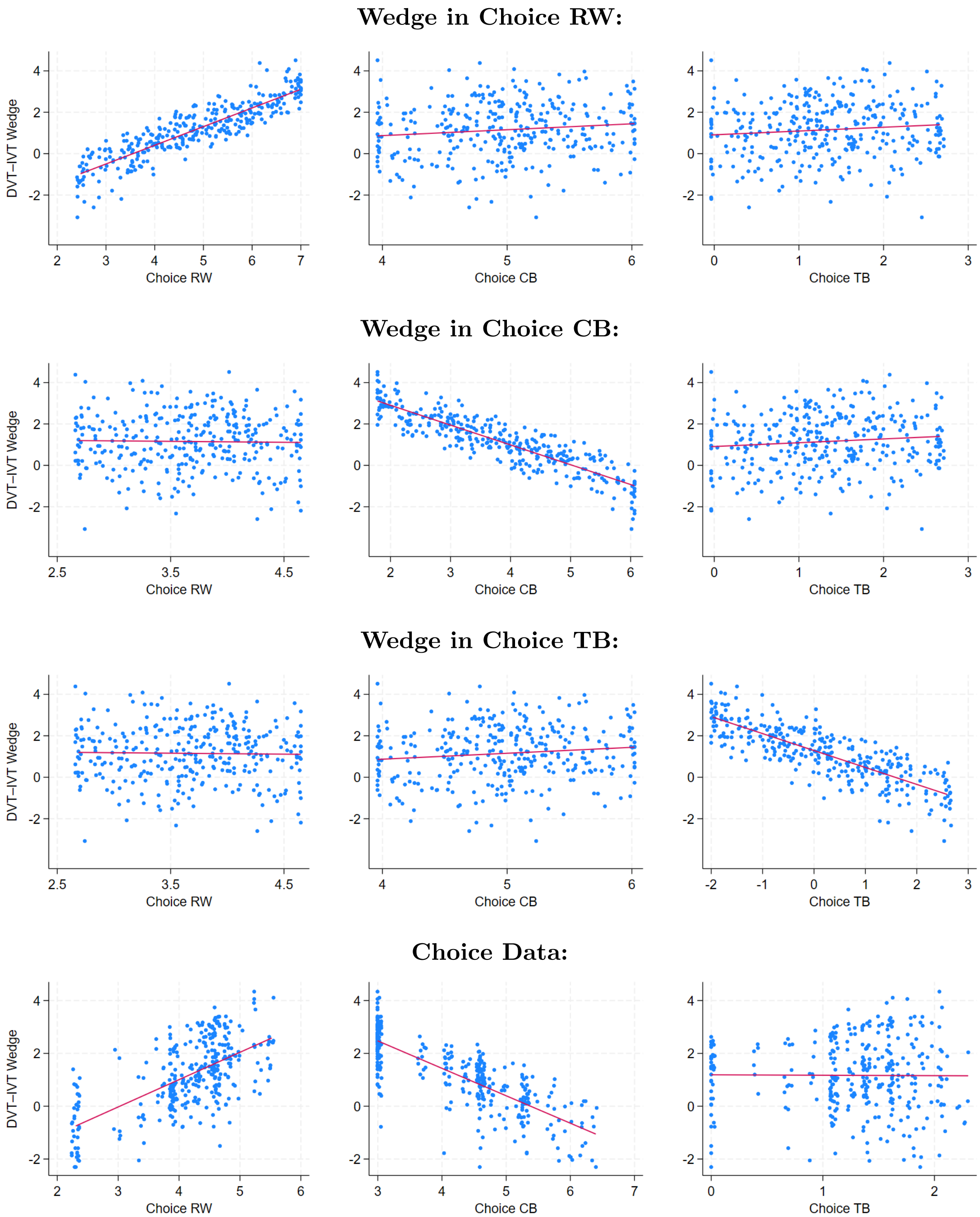
Before we estimate the model, it is useful to provide an intuitive argument for why identification of specific wedges may be possible. In our model, individuals with a large aggregate wedge will exhibit more distorted choices, on average. Thus, the correlations between the aggregate wedge and individual decisions tells us which of those decisions is more or less distorted. We show this graphically in the first three rows of Figure 2, which simulates the relationship between choice data m^{RW}, m^{CB}, h^{TB} and the log-linearized DVT–IVT wedge $-\log(1-\widehat{\omega})$, with only one wedge present per panel. The fourth panel presents choice data from our study.

In our data, farmers' time bids h^{TB} are uncorrelated with the DVT–IVT wedge $\widehat{\omega}$, whereas $\widehat{\omega}$ is positively correlated with m^{RW}, and negatively correlated with choice m^{CB}. Taken together, these correlations can be explained by positive wedges in the RW and CB choices, and no wedge in the TB choice—that is, $\omega^{RW} > 0, \omega^{CB} > 0$, and $\omega^{TB} = 0$. In the next subsection, we formalize this intuitive argument.

4.2 Framework and Data-Generating Process

We return to the general model in (5), which contains parameters ω^{RW}, ω^{CB}, and ω^{TB} that can affect each choice in a distinct way. We use this model to specify variation in preferences across farmers. We index farmers by $i \in \{1, \cdots, N\}$, and allow for farmer-level heterogeneity

Figure 2: Aggregate choice data allow us to identify wedges and the structural value of time.



Rows 1–3 show the relationships between choices Choices RW ($m^{RW}/2$), CB (m^{CB}), and TB (h^{TB}) and the DVT–IVT wedge $\widehat{\omega}$ that would arise if a wedge is present in only Choice RW, CB, or TB, respectively. The fourth row shows the same relationships observed between choices in our data. Each observation is a farmer with a 3% jitter. OLS line in red. All variables are log transformed.

so that (5) takes the form

$$2V_{h,i} - (1-\omega_i^{RW})V_{m,i}m_i^{RW} = 0, \quad (1-\omega_i^{CB})V_{\tau,i} + V_{m,i}m_i^{CB} = 0, \quad (1-\omega_i^{TB})V_{\tau,i} + V_{h,i}h_i^{TB} = 0. \tag{7}$$

It is convenient to re-express farmer i's wedges ω_i^{RW}, ω_i^{CB}, and ω_i^{TB} as

$$1-\omega_i^{RW} = \exp(-\rho_i\gamma_i^{RW}), \qquad 1-\omega_i^{CB} = \exp(-\rho_i\gamma_i^{CB}), \qquad 1-\omega_i^{TB} = \exp(-\rho_i\gamma_i^{TB})$$

with γ parameters normalized so that $\gamma_i^{RW} + \gamma_i^{CB} + \gamma_i^{TB} = 1$.

Thus, parameter ρ_i is an index of farmer i's aggregate wedge, while parameters γ_i^{RW}, γ_i^{CB}, and γ_i^{TB} capture the relative intensity with which that wedge manifests across choice problems.

We make the following assumption:

Assumption 2. *Farmers vary in their aggregate wedge (ρ_i), but not in the relative intensity of each wedge (γ_i^X fixed across all i for $X \in \{RW, CB, TB\}$).*

Using Assumption 2, we can rewrite (7) as

$$\begin{aligned} \log(m_i^{RW}/2) &= \log(V_{h,i}/V_{m,i}) + \rho_i\gamma^{RW} \\ \log m_i^{CB} &= \log(-V_{\tau,i}/V_{m,i}) - \rho_i\gamma^{CB} \\ \log h_i^{TB} &= \log(-V_{\tau,i}/V_{h,i}) - \rho_i\gamma^{TB}. \end{aligned} \tag{8}$$

Recall that a farmer's empirical DVT–IVT wedge $\widehat{\omega}_i$ is

$$1-\widehat{\omega}_i = \frac{\mathrm{IVT_i}}{\mathrm{DVT_i}} = \frac{2m_i^{CB}}{m_i^{RW}h_i^{TB}}.$$

Hence, it follows from (8) that

$$\log\frac{1}{1-\widehat{\omega}_i} = \log(m_i^{RW}/2) - \log(m_i^{CB}) + \log(h_i^{TB}) = \rho_i(\gamma^{RW} + \gamma^{CB} - \gamma^{TB}). \tag{9}$$

Note that ρ_i can only be estimated if $\gamma^{RW} + \gamma^{CB} - \gamma^{TB} \neq 0$. As $\widehat{\omega}_i \neq 0$ for many farmers, (9) implies this condition holds.

Let $\widehat{\delta}^{RW}$, $\widehat{\delta}^{CB}$, and $\widehat{\delta}^{TB}$ denote the OLS estimates obtained from the linear model:

$$\begin{aligned}
\log(m_i^{RW}/2) &= c_A + \widehat{\delta}^{RW} \log \frac{1}{1-\widehat{\omega}_i} + \epsilon_i^{RW} \\
\log m_i^{CB} &= c_B - \widehat{\delta}^{CB} \log \frac{1}{1-\widehat{\omega}_i} + \epsilon_i^{CB} \\
\log h_i^{TB} &= c_C - \widehat{\delta}^{TB} \log \frac{1}{1-\widehat{\omega}_i} + \epsilon_i^{TB}.
\end{aligned} \tag{10}$$

With the following assumption, we can identify the main parameters of the structural model:

Assumption 3. *Conditional on observable characteristics, behavioral parameter ρ_i is uncorrelated with the logarithms of preference parameters $-V_{\tau,i}/V_{m,i}$, and $V_{h,i}/V_{m,i}$.*

Theorem 2 (identification)**.** *With probability one as the sample size N gets large:*

- *For all $X \in \{RW, CB, TB\}$,*

$$\widehat{\gamma}^X \equiv \frac{\widehat{\delta}^X}{\widehat{\delta}^{RW} + \widehat{\delta}^{CB} + \widehat{\delta}^{TB}} \to \gamma^X;$$

- *For all $i \in \{1, \cdots, N\}$,*

$$\widehat{\rho}_i \equiv (\widehat{\delta}^{RW} + \widehat{\delta}^{CB} + \widehat{\delta}^{TB}) \log \frac{1}{1-\widehat{\omega}_i} \to \rho_i.$$

Moreover, the OLS estimates of δ^X are as efficient as those estimated from a seemingly unrelated regressions model.

Simulations show that these estimators perform well for sample sizes similar to that of our data.[21] Standard errors are obtained using the bootstrap with 10,000 draws.

To understand the role of Assumption 3 in identifying the model, it is useful to write down the structural analogues of the estimation equations (10)—which come from combining

[21]Over 10,000 simulations, estimating (10) on data generated from each of three models with a single wedge produces average estimates of the γs that are at most 0.014 away from the true values. Simulating data with the estimated parameters—$\gamma^{RW} = 0.39$, $\gamma^{CB} = 0.61$, $\gamma^{TB} = 0.00$—also produces estimates that are at most 0.014 away from the true values.

(8) and (9):

$$\begin{aligned}
\log(m_i^{RW}/2) &= \log(V_{h,i}/V_{m,i}) + \frac{\gamma^{RW}}{\gamma^{RW}+\gamma^{CB}-\gamma^{TB}} \log \frac{1}{1-\widehat{\omega}_i} \\
\log m_i^{CB} &= \log(-V_{\tau,i}/V_{m,i}) - \frac{\gamma^{CB}}{\gamma^{RW}+\gamma^{CB}-\gamma^{TB}} \log \frac{1}{1-\widehat{\omega}_i} \\
\log h_i^{TB} &= \log(-V_{\tau,i}/V_{h,i}) - \frac{\gamma^{TB}}{\gamma^{RW}+\gamma^{CB}-\gamma^{TB}} \log \frac{1}{1-\widehat{\omega}_i}.
\end{aligned} \tag{11}$$

Consistent estimation of the first and second equation in (10) requires the omitted variables $\log(V_{h,i}/V_{m,i})$ and $\log(-V_{\tau,i}/V_{m,i})$ to be uncorrelated with $\log \frac{1}{1-\widehat{\omega}_i}$, which is a linear function of ρ_i. This is exactly Assumption 3. To see that Assumption 3 also gives consistent estimation of the third equation, it is helpful to note that $\log(-V_{\tau,i}/V_{h,i}) = \log(-V_{\tau,i}/V_{m,i}) - \log(V_{h,i}/V_{m,i})$, which are both uncorrelated with ρ_i by assumption.

While $\log(V_{h,i}/V_{m,i})$ and $\log(-V_{\tau,i}/V_{m,i})$ are not directly observable, we show that survey-based proxies can be constructed. These proxies can be used to test for the influence of omitted variable bias arising from a violation of Assumption 3, a point we return to in Section 4.4.

Consistent estimates of the structural value of time of farmer i, $\widehat{\text{SVT}}_i$, can be recovered using (8) and Theorem 2:

$$\widehat{\text{SVT}}_i = \widehat{V_{h,i}/V_{m,i}} \equiv \frac{m_i^{RW}}{2} \exp\left(-\widehat{\delta}_i^{RW} \log\left(\frac{m_i^{RW} h_i^{TB}}{2m_i^{CB}}\right)\right). \tag{12}$$

This formula represents the process described intuitively in the introduction: data from all three choices are used to estimate the extent to which choice RW is impacted by a wedge, and then to remove that effect. Note that as consistently estimating $\widehat{\text{SVT}}_i$ requires only a consistent estimate of $\widehat{\delta}^{RW}$—see the first equation of (11)—it requires only that $\log(V_{h,i}/V_{m,i})$ is uncorrelated with ρ_i, a subset of Assumption 3.

4.3 Estimation Results

Across the specifications and sub-populations in Table 3, all estimated using Theorem 2, choice TB shows no evidence of distortions ($\widehat{\gamma}^{TB} = 0$), while those choices that involve cash are the source of distortions ($\widehat{\gamma}^{RW}, \widehat{\gamma}^{CB} > 0$).[22] This pattern is the same as that shown in

[22] As we bottom code cash and time bids that are outside the range of allowed prices—bids below 20 KSh or 1 hour, respectively—and top code DVT above 250 KSh/hour, we test for sensitivity to recoding in

Figure 2: distortions are consistent with non-zero wedges only in choices involving cash.

Fitting data from the full sample, in Column 1, results in a mean structural value of time equal to 49 KSh/hour, or 60% of the average wage for casual labor. As expected, this lies inside the range of estimates produced by IVT and DVT (40% to 100% of the average market wage).

4.4 Threats to Identification

Our strategy produces valid estimates of all our model parameters as long as identifying Assumptions 2 and 3 hold in our data. We thus examine a number of different specifications and subgroups that provide support for these assumptions.

4.4.1 Stability of Estimates Across Subgroups

To investigate whether both Assumptions 2 and 3 are reasonable, we estimate our model separately within groups of economically similar farmers.[23] There is likely to be less confounding variation in preferences within these groups, so that independence between the DVT–IVT wedge $\widehat{\omega}_i$ and the parameters $\log(-V_{\tau,i}/V_{m,i})$ and $\log(V_{h,i}/V_{m,i})$ is more likely to hold. Estimating our model separately also provides a check of whether γ^{RW}, γ^{CB}, and γ^{TB} are stable across heterogeneous subgroups. We form four groups using Partitioning Around Medoids (PAM) cluster analysis, which is described in Appendix D. We characterize these four groups—sorted from lowest to highest average DVT–IVT wedge $\mathbb{E}_i[\widehat{\omega}_i]$—as consisting of the low-skill self-employed, low-skill employees, hirers of casual labor, and older, low-education households. These characterizations are based on the strongest predictors of membership in each group, as shown in Table D.1.

Estimated parameters γ^{RW}, γ^{CB}, and γ^{TB} are stable across groups, as shown in Columns 2–5 of Table 3. This supports Assumption 2: that the relative intensities γ are fixed across the sample. The estimated structural value of time is also stable, varying from 54–67% of the average market wage. This is true despite substantial variation in the average DVT–IVT wedge $\mathbb{E}_i[\widehat{\omega}_i]$ across clusters—from 0.12 to 0.74. This provides some evidence that 60% of the

Columns 1–4 of Appendix Table E.4. The estimated relative intensities $\widehat{\gamma}^{RW}, \widehat{\gamma}^{CB}, \widehat{\gamma}^{TB}$ change little across specifications, and the estimated mean structural value of time is very stable at 58–60% of the average market wage.

[23] Table C.1 shows how our estimates of the SVT vary based on respondent gender, age, education, income, the presence of a child under 3, and whether someone in the household operates a micro-enterprise. Estimates of SVT are highly stable across subgroups, varying from 54–64% of the average market wage.

Table 3: Structural Estimates of the Value of Time

	Full sample	Cluster breakdown: Low-skill self-employed	Low-skill employees	Hires casual workers	Older, low-edu households	Full sample +controls	Casual laborers	Considered buying pump
Structural estimation								
Reservation wage	0.39	0.39	0.40	0.36	0.42	0.39	0.39	0.40
relative intensity, $\widehat{\gamma}^{RW}$	(0.02)	(0.03)	(0.05)	(0.05)	(0.06)	(0.02)	(0.04)	(0.03)
Cash bid	0.61	0.59	0.60	0.63	0.58	0.61	0.61	0.60
relative intensity, $\widehat{\gamma}^{CB}$	(0.03)	(0.04)	(0.05)	(0.06)	(0.06)	(0.02)	(0.04)	(0.03)
Time bid	0.00	0.03	0.00	0.01	0.00	0.00	0.00	0.00
relative intensity, $\widehat{\gamma}^{TB}$	(0.01)	(0.03)	(0.01)	(0.03)	(0.02)	(0.01)	(0.01)	(0.02)
Structural value of	49	45	46	58	46	49	46	44
time, $\mathbb{E}_i[\widehat{SVT}_i]$	(2.5)	(3.8)	(4.2)	(5.7)	(6.4)	(2.4)	(3.5)	(2.7)
Market wage, $\mathbb{E}_i[w_i]$	82	84	77	86	81	82	73	82
	(1.8)	(3.8)	(3.4)	(3.4)	(3.5)	(1.8)	(3.1)	(2.4)
Relative value of	0.60	0.54	0.59	0.67	0.57	0.60	0.63	0.54
time, $\mathbb{E}_i[\widehat{SVT}_i]/\mathbb{E}_i[w_i]$	(0.03)	(0.05)	(0.06)	(0.08)	(0.08)	(0.03)	(0.06)	(0.04)
Choices								
Direct value	83	79	70	92	98	83	72	73
of time, $\mathbb{E}_i[DVT_i]$	(3.0)	(5.8)	(4.1)	(6.2)	(8.4)	(3.0)	(3.9)	(3.2)
Indirect value	30	31	31	35	17	30	31	29
of time, $\mathbb{E}_i[IVT_i]$	(1.9)	(4.0)	(3.4)	(4.1)	(3.2)	(1.9)	(3.1)	(2.4)
Cash bid, $\mathbb{E}_i[m_i^{CB}]$	111	127	125	117	50	111	129	123
	(6.9)	(16.2)	(12.6)	(13.2)	(7.6)	(6.9)	(11.4)	(9.6)
Time bid, $\mathbb{E}_i[h_i^{TB}]$	4.0	4.2	4.5	3.8	3.3	4.0	4.6	4.4
	(0.1)	(0.2)	(0.2)	(0.3)	(0.2)	(0.1)	(0.2)	(0.2)
DVT–IVT wedge, $\mathbb{E}_i[\widehat{\omega}_i]$	0.30	0.12	0.18	0.33	0.74	0.30	0.19	0.17
	(0.07)	(0.17)	(0.13)	(0.11)	(0.05)	(0.07)	(0.11)	(0.10)
Observations	332	75	108	93	56	332	141	189

Each observation is a farmer. Currency units are Kenyan shillings (1 USD = 107 KSh). Columns (2)–(5) show results estimated separately within clusters of similar farmers. Column (6) controls for unincentivized proxies of the value of time and the valuation of the lottery ticket. Column (7) shows results estimated on farmers who performed casual labor within the past 3 months. Column (8) shows results estimated on farmers who report having considered buying a MoneyMaker irrigation pump. Cash and time bids bottom-coded at 20 KSh and 1 hour respectively. Bootstrap standard errors in parentheses.

average market wage is a reasonable rule of thumb for the SVT, even across heterogeneous subgroups.

4.4.2 Robustness to Controlling for Proxies of V_τ and V_h

A further test of the plausibility of Assumption 3—that farmers' aggregate wedges ρ_i are uncorrelated with $\log(-V_{\tau,i}/V_{m,i})$ and $\log(V_{h,i}/V_{m,i})$—comes from examining the estimates of $\widehat{\rho}_i$, $\widehat{\gamma}^{RW}$, $\widehat{\gamma}^{CB}$, and $\widehat{\gamma}^{TB}$ after controlling for the logs of $-V_{\tau,i}/V_{m,i}$ and $V_{h,i}/V_{m,i}$ in (10). As shown in (8), choices in our model are determined solely by the logs of $-V_{\tau,i}/V_{m,i}$ and $V_{h,i}/V_{m,i}$, ρ_i, and parameters γ^{RW}, γ^{CB}, and γ^{TB}. While $-V_{\tau,i}/V_{m,i}$ and $V_{h,i}/V_{m,i}$ cannot be observed directly, our survey data offer proxies. If our model estimates are unaffected by controlling for the log of such proxies, this implies that ρ_i is uncorrelated with $\log(-V_{\tau,i}/V_{m,i})$ and $\log(V_{h,i}/V_{m,i})$.

We have two such proxies. First, we use stated willingness to work—in hours—for a lottery ticket for an irrigation pump (collected as part of a baseline survey conducted five years earlier, in 2014) as a proxy for $-V_{\tau,i}/V_{m,i}$. Second, we use the stated minimum amount of money for which the respondent would be willing to travel one hour (collected during our main 2019 survey) as a proxy for $V_{h,i}/V_{m,i}$. We find that these unincentivized proxies are strongly correlated with farmers' choices, but uncorrelated with wedges, suggesting that they are good proxies for $-V_{\tau,i}/V_{m,i}$ and $V_{h,i}/V_{m,i}$.[24]

Controlling for the log of the unincentivized proxies of $-V_{\tau,i}/V_{m,i}$ and $V_{h,i}/V_{m,i}$, in Column 6 of Table 3, has very little effect on our estimates. In particular, $\widehat{\rho}_i$ changes very little between Columns 1 and 6—from an average of 1.18 to 1.17—and $\widehat{\gamma}^{RW}$, $\widehat{\gamma}^{CB}$, and $\widehat{\gamma}^{TB}$ are also highly stable. This suggests that, indeed, $\log(-V_{\tau,i}/V_{m,i})$ and $\log(V_{h,i}/V_{m,i})$ are uncorrelated with ρ_i, which is exactly Assumption 3.[25]

[24] The p-value from the bivariate regression of $-\log(1-\widehat{\omega}_i)$ on the logarithm of the unincentivized willingness to work for the ticket is 0.50; on the logarithm of the unincentivized reservation payment for traveling one hour, it is 0.29. The p-values from bivariate regressions of $\log(m_i^{CB})$ and $\log(h_i^{TB})$ on the logarithm of the unincentivized willingness to work for the ticket are 0.03 and 0.00, respectively, and the p-value from the bivariate regression of $\log(m_i^{RW}/2)$ on the logarithm of the unincentivized reservation payment for traveling one hour is 0.01.

[25] Additionally, if $-V_{\tau,i}/V_{m,i}$ and $V_{h,i}/V_{m,i}$ are uncorrelated with ρ_i, then the DVT among farmers exhibiting no wedges should approximate the average value of time in the sample. Consistent with this prediction, we find that farmers with $|\widehat{\omega}_i| < 0.15$ have an average DVT of 54 KSh/hour, close to the average SVT of 49 KSh/hour in the remaining sample ($p = 0.49$).

5 Interpretation and Robustness

In this section, we consider potential economic interpretations of the decision wedges. We first explain why the SVT is relevant for evaluating welfare in many settings, regardless of the specific mechanisms driving the wedges. We then outline behavioral models that can rationalize our results in Section 5.2. Finally, we describe other possible interpretations of our results that we can reject by our design or data in Section 5.3.

5.1 When Is the SVT Welfare Relevant?

Under the assumptions discussed in Section 4.3, and checked in Section 4.4, SVT is identified. In this section, we provide guidance to researchers interested in using the SVT to assess the welfare impacts of interventions.

Our results show that wedges do not affect choices that trade off time for a good: across several heterogeneous subgroups, and regardless of whether we estimate our model with or without control variables, our estimate of the wedge ω^{TB} is a precisely estimated zero. Applying this finding can help researchers decide when the SVT is the appropriate parameter for welfare evaluation. An intervention that changes time spent working on one's own farm or small business is best modeled as a trade-off between time and goods, and thus one where the "unwedged" value of time—the SVT—correctly reflects the opportunity cost of time. As many interventions evaluate similarly naturalistic trade-offs, the SVT is appropriate across a broad range of settings. In contrast, an intervention that leads farmers to trade-off time for money—for example, one that increases hiring by reducing labor market frictions—would require the researcher to take a stand on whether to incorporate wedges into welfare evaluations. In cases where the intervention is likely to evoke a behavioral response, using the DVT to evaluate welfare may be appropriate. If a researcher is unsure, they can consider using SVT and DVT as bounds.

5.2 Interpreting Wedges: Potential Models

Explaining the wedge between DVT and IVT requires a steep change, or "kink," in the indirect utility function (1). The estimation results in Section 4.3 indicate that this kink arises in our study whenever transactions involve cash. Cash-specific wedges could arise in an environment where farmers regularly make opportunity cost calculations in terms of goods and time—for example, deciding how much time to work on their field in order to obtain a greater yield—but rarely use cash. However, researchers interested in understanding the surplus generated by a new technology often wish to translate changes in yield or time use into a single numeraire by assigning them a cash value. Making this translation—either by offering cash for work or by selling a good for both cash and time—could cause farmers to make trade-offs that do not represent their underlying value of time or of the good. In this subsection, we discuss potential behavioral models that could drive this cash-specific kink. Distinguishing between these models is not necessary for identifying SVT, but may be relevant for researchers applying our estimates in different environments.

Cash-Specific Self-Serving Bias. The results of our estimation can be explained by a *self-serving bias* that arises only in transactions that involve cash. In models with self-serving bias, people discount the value of goods obtained from other parties (Loewenstein et al., 1993; Babcock et al., 1995; Babcock and Loewenstein, 1997). Using this frame to interpret our results suggests that farmers over-value their labor when compensated in money (but not goods), and under-value goods when paying in money (but not time). To give this a more succinct, but less precise, interpretation: farmers fear being taken advantage of—or think negotiation is more important—when transactions involve cash.

Cash-Specific Loss Aversion. Our results can also be explained by a model of loss aversion (Kahneman et al., 1991; Kahneman and Tversky, 1979). As with a self-serving bias, loss aversion would need to occur only in transactions involving cash. This could arise in settings where goods are frequently traded but monetary transactions are relatively rare.

Cash-Specific Risk Aversion. As explained in Section 5.3, standard models of risk aversion will not generate a wedge between DVT and IVT. For risk aversion to explain our results, farmers would need to be differentially averse to risk when paying in cash compared to time.[26]

Evidence for Behavioral Models. We find some support for a cash-specific behavioral bias in our data. Under the assumption that behavioral phenomena will be less pronounced when individuals are experienced with specific choices (List, 2003; Feng and Seasholes, 2005; Kőszegi and Rabin, 2006; Carney et al., 2019), we can analyze the choices of those who have performed or hired casual labor within the past three months, and those who have experience exchanging their cash for goods. These are all proxies for experience transacting in cash, and were measured in a baseline survey.[27] In these three groups, the average DVT–IVT wedge

[26] In theory, a similar result could arise if farmers are averse to spending cash, but not time, on an unfamiliar good. However, farmers in our study seem familiar with the pump; see Section 5.3.

[27] Specifically, we compute the first principal component of eight indicators for whether the farmer purchased (or rented) agricultural equipment or inputs, home durables, land, buildings, cattle, chickens, or other

$\mathbb{E}_i[\widehat{\omega}_i]$ is smaller than in the full sample, as shown in Appendix Tables C.2 and C.3, which present formal regression analysis showing the predictive power of these three, and other, covariates.[28]

5.3 Interpreting Wedges: Models Rejected by Our Data or Design

In this section, we discuss and summarize evidence against several potential alternative interpretations of the wedges. While identification does not depend on the specific model generating wedges, the source of wedges may be relevant when applying our estimates in different environments. Appendix E expands on each model listed in this section.

First-Order Effects of Credit or Labor Constraints. First-order effects of credit or labor constraints are incorporated into our benchmark model, and thus, cannot explain a wedge between DVT and IVT. If a farmer is credit constrained, they will have a high shadow value of money, but this will be reflected in both their IVT and DVT equally through the value of money $V_{m,i}$.

Second-Order Effects of Credit or Labor Constraints. Second-order effects of credit and labor constraints can explain the DVT–IVT wedge; however, this explanation gives rise to an additional testable prediction that is inconsistent with our data, as we show in Appendix E.2. Specifically, this explanation predicts that reservation wages should be negatively correlated with $\widehat{\omega}_i$, because the value of money will be higher for farmers facing tightened credit constraints, thereby decreasing reservation wages and increasing the DVT–IVT wedge. However, as shown in Panel 4 of Figure 2, the DVT–IVT wedge is strongly positively correlated with the reservation wage.

Uncompensated Costs of the Work Activity. We provided transportation to and from job sites, and the time this took was credited towards farmers' work commitments. However, farmers needed to make room in their schedule to attend the work session, and spend time traveling between their home and the pickup location in the village center. This could appear as a wedge in Choice RW or TB. Work days were scheduled 1 to 2 weeks in advance so that farmers could reshuffle tasks across days, implying that within-day changes in working hours should be small. Additionally, if some component of transport costs is not observed—for example, some people live farther than others—the benchmark model implies

livestock; or made business investments. We split the sample based on the median value of this component.

[28]The relative intensities γ are similar in these groups to those in the full sample, implying that differences in the choice-specific wedges ω_i^{RW} and ω_i^{CB} are driven by differences in ρ_i rather than by differences in γ.

restrictions on farmers' choices that are rejected in our data. Appendix E.3 formalizes this argument.

Stigma of Accepting Low Wages. If accepting low-wage work is stigmatized, as in (Breza et al., 2019), this could inflate DVT above SVT. To test for these norms, we elicited survey reactions to a story about a farmer accepting a wage 50% below the market rate, and found that positive reactions were much more common than negative ones to both the worker and the hirer. This points to a limited scope for low-wage stigma in our setting. Additionally, these survey reactions are not significantly correlated with DVT, suggesting that their influence on our results is minimal (see Appendix E.4). More general versions of an aversion to low-cash wages are possible—for example, if self-image is tied to hourly wages but not to a low implied wage in Choice TB, possibly because the implied wage is more opaque than a cash wage.

Non-Compliance. If farmers inflate their cash or time bids above their willingness to pay—or deflate their reservation wages below their willingness to accept—while intending to later renege by not making the payment or completing work, this could appear as a wedge. Reneging was possible, as our design gave farmers 1–2 weeks before their full cash payment was due, or before they completed casual work for a lottery ticket or a payment. The rate of follow-through for cash payments was high. Among farmers who drew a random cash price below their willingness to pay (so were eligible to buy a ticket), 88% paid the correct price on or before collection day. Follow-through in choices TB and RW was lower: among farmers who drew a time price below their willingness to pay, 75% completed their work on the scheduled work day. Among farmers selected for wage work who had a reservation wage weakly below their wage draw, 74% completed their work on the scheduled work day.[29] As we discuss in Appendix E.5, the correlations between compliance and choices suggest that most farmers were not planning on reneging when making their choices. Finally, restricting estimation to farmers with high predicted compliance does not significantly affect our results.

BDM Comprehension. The BDM elicitation method we use is common in studies of the self-employed (Berry et al., 2020; Burchardi et al., 2021). Four pieces of evidence, described in Appendix E.6, suggest that features which may be present in the BDM design are not

[29] Our compliance rate for cash payments is in line with other studies using BDM: see Maffioli et al. (2023) for a discussion of reneging after BDMs. The lower compliance rate when paying in time is likely due to the down payment used in choice CB, which is difficult to mimic for choices that involve a time commitment. A multivariate test of means rejects equality of compliance rates with $p = 0.03$. As discussed in Appendix E.1, compliance does not depend on the amount of time a farmer had to obtain cash.

driving the intransitivities we observe. First, we find no significant order effects when we randomize the sequence of Choices CB and TB. Second, we find no evidence that farmers are anchoring their choices either to the prevailing wage or to the starting points of the BDM procedure. Third, very few farmers took the opportunities we offered them to revise their bids. Fourth, and finally, very few farmers expressed regret about their choice after the random price was drawn. While these facts are reassuring, it is worth noting that any technique for eliciting the value of time may introduce wedges, which would need to be estimated in order to recover SVT.

Familiarity With the Work Activity and Good. The specific work activity or good used in our choices—casual labor and a lottery for an irrigation pump—are unlikely to drive the wedges we observe. Casual labor is very common in this setting, and nearly all farmers were familiar with the pumps, with most having considered purchasing one in the past. To test whether familiarity with the BDM activities matters for our results, we re-estimate our model separately within the set of farmers who have recently performed casual labor, and within those who have considered purchasing the irrigation pump in the past. The SVT for these subgroups as a fraction of the market wage, shown in Columns 7 and 8 of Table 3, is 63% and 54%, respectively, close to the 60% estimate in the overall sample.

Present Bias. Standard models of time discounting, in which decision makers value a good less the longer they have to wait for it, cannot explain our findings. Workers received payment in Choice RW immediately after work was completed. Lotteries were held as soon as payment and work were complete. None of the choices in our study involved trade-offs between the present and the future (with the exception of the 20-KSh down payment for the lottery ticket when paid in cash). As such, present bias cannot contribute to our results.

Intra-Household Decision-Making. Our study design mimicked real-world decisions by allowing the household to choose which member participated in the study. If farmers who participated in our study are expected to consult their family members about cash purchases, but not time spent on work, this could potentially generate a wedge between DVT and IVT. All surveys were held at participants' homes, and spouses were permitted to sit in, so consulting with them was possible.[30] We find that single-headed and smaller

[30] We did not observe significantly different wedges when spouses sat in on the activities. We observe larger wedges for women than for men, but not different values of time, as shown in Appendix Table C.1. The gender difference in wedges disappears when controlling for other characteristics, as shown in Appendix Table C.2.

households exhibit a greater DVT–IVT wedge on average, which is difficult to reconcile with intra-household decision-making dynamics driving our results.

Risk Aversion. Farmers whose preferences exhibit risk aversion will be willing to pay—in cash or in time—less than the expected value of the lottery ticket in Choices CB and TB. However, this will affect choices only through the farmer's value of the ticket $V_{\tau,i}$, which does not enter IVT or DVT.

6 Discussion

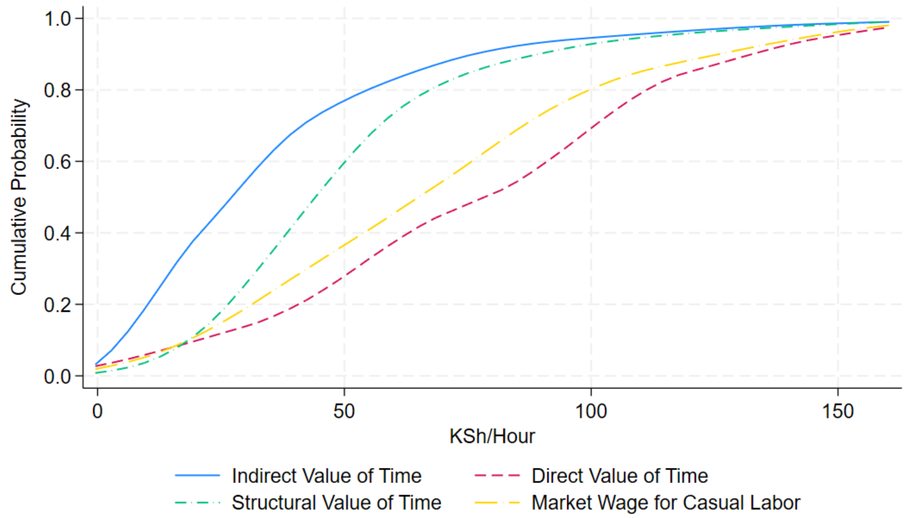
This paper seeks to better understand how to measure people's value of time in policy evaluations. We show that a direct, incentivized elicitation in which participants perform casual labor for money may not produce a valid estimate of the value of time due to behavioral wedges. In particular, participants seem to overvalue their time when exchanging it for cash. Using a design involving choices between time, money, and a good, we are able to identify the effects of wedges, and recover a welfare-relevant structural value of time. This value of time is roughly 60% of both the value elicited through a direct BDM mechanism and the average market wage for casual labor. Figure 3 displays these facts visually. Market wages and reservation wages elicited through a direct BDM mechanism are fairly similar. However, the structural value of time is much lower than either the market wage or the BDM elicitation.

6.1 Implications for Labor Markets

Self-employment in the informal sector accounts for the majority of work in Africa (O'Higgins et al., 2020). Self-employment may be disguised excess labor supply (Breza et al., 2021) generated by frictions such as wage rigidity (Kaur, 2019) or other labor market constraints (Benjamin, 1992; Jones et al., 2022). Our results suggest an additional factor contributing to high self-employment levels: behavioral responses to negotiations involving cash, such as a cash-specific self-serving bias. As this phenomenon can cause an impasse in negotiations even when information is complete (Babcock and Loewenstein, 1997), it may lead workers to opt for self-employment over higher-paying casual jobs.[31] Further, this phenomenon may

[31] It could also cause those who hire casual labor to undervalue it relative to cash during negotiations. Unfortunately, we do not observe willingness to pay for labor in any of our activities. Note that our analysis does not imply that behavioral phenomena are welfare reducing in equilibrium, even for a given individual. In strategic contexts, like wage bargaining, behavioral phenomena can influence the behavior of other parties, helping individuals to obtain better terms.

Figure 3: The structural value of time is lower than wages and the direct value of time.



Kernel-smoothed cumulative distribution functions (van Kerm, 2012) estimated on all farmers. All variables top coded at 150 KSh/hour.

make maintaining norms of not accepting low-wage jobs easier, which Breza et al. (2019) identify as a source of labor-market distortions. Finally, survey questions requiring farmers to estimate the cash value of in-kind payments or of agricultural production may be inaccurate in settings where goods and time are typically transacted without cash.

Alternatively, if this phenomenon does not extend to most negotiations, then the finding that market wages for casual labor first-order stochastically dominate the structural value of time suggests that wages are higher than the market-clearing rate, and that casual jobs are rationed. Labor rationing may be a response to shading of job performance due to wage deviations below a laborer's reference point (Hart and Moore, 2008; Fehr et al., 2011). We are able to test for this in our setting using the random variation in hourly wages paid for casual work in choices RW and TB. Specifically, we test whether the quality of work performed—as evaluated by field staff after work was completed—depends on the random wage paid. For example, in the RW choice, the wage paid for day work is random, and—because only those who drew a wage higher than their DVT were eligible to work—eligibility is random conditional on DVT. We find significant evidence of shading at lower wages, but only for wages below reference wages—the amount farmers told us they thought they could earn for casual labor—as shown in Appendix Table F.1. Moreover, shading only occurred when the farmer was working for a cash wage, as opposed to a set reward. This suggests that, when paying cash, employers may find it worthwhile to pay a higher wage to increase the average quality of work, leading to fewer jobs.

6.2 Value of Time Assumptions in the Literature

In this section, we survey the extant literature to understand how it accounts for the value of time of the self-employed. We searched top economics journals for any study from 2016–2021 of the self-employed in a low-income country, in which revenue or profits were measured.[32] This search resulted in a total of 106 studies, of which only 42 had collected enough information, in theory, for us to reinterpret their results in light of our findings.[33]

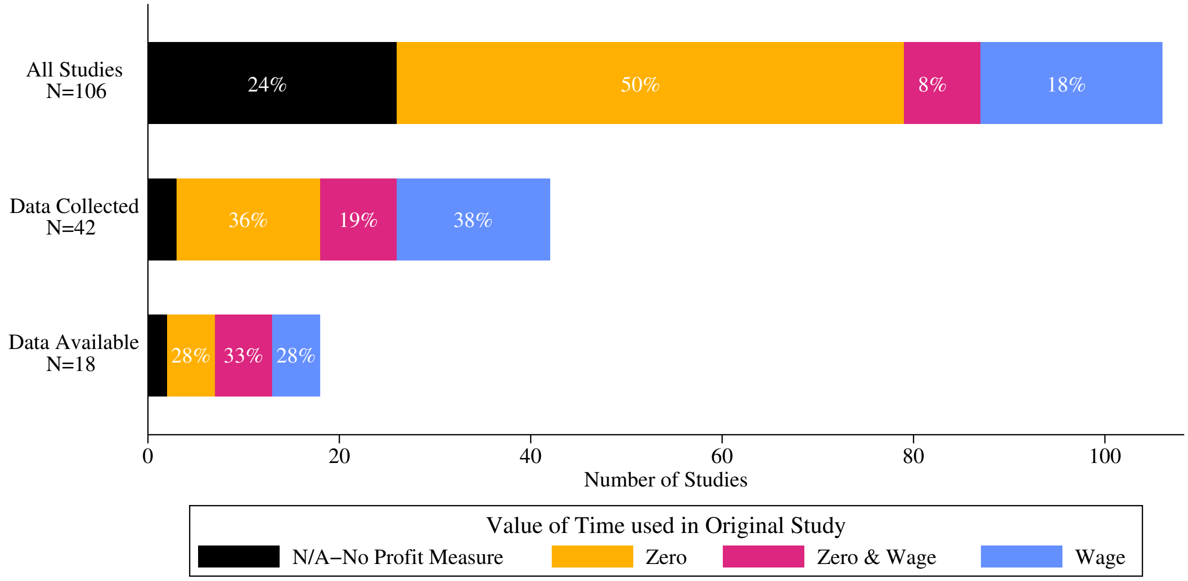
As shown in the top-left bar of Figure 4, 24% of the 106 studies do not attempt to use profit as an outcome, instead only reporting output-oriented measures, such as yields or revenue, that do not account for changing costs. Many of these papers justify their focus on output with the fact that it is difficult to measure the value of time for the self-employed (see, for example, Suri, 2011; Ahmed et al., 2021; Beaman et al., 2021). An additional 50% of the studies compute profit estimates using zero as the value of time. That is, together, 74% of the studies either avoid evaluating welfare impacts, or omit participants' value of time when doing so. The remaining studies (23%) use the average market wage to value the time of the self-employed. A subset of these (8% of all studies) use both zero and the average market wage to bound profit estimates under a range of values of time, similar to our first simple strategy above—although we recommend a lower bound of 40% of the average market wage.

Studies that collected sufficient information to, in principle, calculate profits under different values of time ($N = 42$) were more likely to value the time of the self-employed, with 57% assigning a positive value in at least some specifications, as shown in the center bar of Figure 4. Among those studies where we could obtain the necessary data for these

[32] In particular, we searched Top-5 journals, plus top applied journals (*Journal of Development Economics* and *American Economic Journal: Applied Economics*), and top ag-econ journals (*American Journal of Agricultural Economics* and *European Review of Agricultural Economics*) for papers with 45 *JEL* codes during the years 2016–2021. The reviewed *JEL* codes can be found in Appendix G. The papers that resulted from this search were then read to find those about the self-employed that measured revenue or profits.

[33] Analyzing the sensitivity of results to assumptions about the value of time requires three pieces of information: household labor hours, the locally prevailing market wage, and revenue net of other input costs. From what we could gather, 64 of the 106 studies did not collect all necessary data. In particular, only 8 (12.5%) of these 64 studies appear to have collected data on household labor supply, and 14 (22%) on market wages.

Figure 4: Value of Time Used in Prior Literature on the Self-Employed



calculations ($N = 18$), 61% assigned a positive value in at least some specifications.[34]

The fact that many recent studies do not measure input costs, even though they consider profits as a primary outcome, may be surprising. This may stem, in part, from the findings of De Mel et al. (2009), which suggest that asking the self-employed to self-report accounting profits is more accurate than eliciting revenues and costs, and computing profits from these quantities. However, that study does not consider the hours worked by the self-employed as a cost in their profit measure.[35] Yet, two programs that impact accounting profits equally, but affect work hours for the business owner differently, will clearly have different welfare impacts. Even if one were to only ask the self-employed about accounting profits, as De Mel et al. (2009) suggest, our results indicate that one should additionally ask about the hours worked by the self-employed, and use this information in calculating profits.

[34] Of the 42 studies that collected the data needed to re-calculate profits, 6 contained sufficient information in the paper itself for us to re-evaluate their results, 12 had replication datasets with sufficient information available online, and an additional 15 studies required us to gather the source data for the paper. We received a complete replication dataset for 2 of those 15. We thank the authors who provided these data.

[35] When eliciting profits directly, they ask: "What was the total income the business earned during the month of [March] after paying all expenses including the wages of employees, *but not including any income you paid yourself?* That is, what were the profits of your business during [March]?" (emphasis ours).

6.3 Practical Implications for Researchers

Overall, our findings suggest the need for more understanding of how the self-employed value their own time. However, they also suggest approaches that can be immediately applied. In this subsection, we describe some rules of thumb and their limitations, and, in the next, apply these simple techniques to prior studies in order to illustrate their potential usefulness.

We begin with two simple strategies for valuing the time of the self-employed:

Use a range of 40–100% of the average market wage. This does not require committing to a particular model or choice(s) as "correct," consistent with the approach in Bernheim and Rangel (2009). As we illustrate below, in Figure 5, this approach is sometimes sufficient for evaluating whether or not a particular intervention is beneficial. However, for some applications, a point estimate may be necessary, in which case we suggest:

Use 60% of the average market wage. Researchers evaluating interventions in similar contexts as ours could opt to rely on our estimate that the value of time is close to 60% of the average market wage for casual labor; see Acampora et al. (2022) for a recent example of an application of this rule of thumb. This follows the "parametric tradition" of welfare evaluation: see Sadoff et al. (2020) for a brief summary and other examples.

A more complex strategy, but one that might be useful for large-scale studies that need a precise value of time, would be to replicate our activities and associated analysis.[36] Interventions that are likely to substantially increase or decrease family labor supply are the most likely to meet this criterion. If the study is large enough, adding a replication of our method may have a relatively low marginal cost. This does present some challenges—it requires scheduling workdays and transporting workers to and from work sites—so conducting this exercise within a subset of participants may be optimal.

External Validity. The main limitation of our two simple approaches is external validity: factors that keep wages above the value of time are likely to be context specific. For example, because our estimates are local to the season in which our activities took place—in this case,

[36]Unincentivized choices are likely to be seen as an attractive alternative, but should be used with extreme caution. In particular, unincentivized survey-based measures modeled on our choices are likely to produce unreliable results. In our sample, farmers' reservation wages elicited through an unincentivized survey question are significantly higher than the incentivized reservation wage m^{RW} ($p < 0.01$)—although the incentivized and unincentivized quantities are highly correlated, as described in Section 4.3.

the end of sowing season—we cannot rule out that labor is increasingly rationed during lean seasons, as in Breza et al. (2021). Nevertheless, we observe a striking robustness in the relative value of time across subgroups in our data, lending some credibility to a rule of thumb approach, especially in similar environments.[37] We recommend that researchers applying the 60% rule of thumb also present bounds on estimated impacts if working in a dissimilar environment.

Researchers who are concerned that the degree of labor rationing may be different in their setting can consider adjusting or bounding our rule of thumb. Doing so would require a measure of labor rationing in the new setting. In Appendix H.1, we show that a proxy can be computed from two survey questions: each worker's recent market wage, and their potential wage if they were to seek work tomorrow. This proxy is strongly correlated with the individual-level measure of labor rationing λ_i (the Lagrangian on the labor constraint) identified by our model. In Appendix Figure H.1, we offer rules of thumb specific to bands of this proxy, which researchers can use depending on which band appears to best represent their setting. Additionally, for researchers anticipating non-uniform labor supply responses to an intervention—which may be correlated with SVT—we recommend using an unincentivized measure to capture the relevant heterogeneity, and adjusting the rule of thumb as described in Appendix H.2.

Identification in Other Settings. Researchers setting up similar choices to those used in this paper, in order to produce their own estimate of SVT, will need to impose Assumption 2. However, SVT can be estimated without Assumption 3, if the researcher has a proxy for the logarithm of V_h/V_m. A hypothetical question about willingness to travel for cash is easy to measure, and appears, in our data, to serve as a good proxy. In cases where estimates of relative intensities γ are not stable across subgroups, the researcher could opt to estimate our model separately within groups of economically similar farmers.

Variation in the Cost of Time Across Settings. The opportunity cost of time for a given worker is likely to vary across tasks and periods of time. When benchmarking the value of time against a market wage—or when designing a task to serve as a benchmark—researchers should choose benchmarks that are comparable to the labor changes induced by their intervention. For example, workers are likely to require higher wages to work on a fixed schedule than on a flexible one: the market wage for flexible casual work would thus

[37]Beyond the subgroup analysis presented in Table 3, we find relatively little variation in the relative value of time across villages: the 25th, 50th, and 75th percentiles of the village-level averages are 0.52, 0.59, and 0.72, respectively. The minimum and maximum are 0.39 and 0.77.

be too low of a benchmark for a technology that requires labor input at a specific hour every day. Because the task used in this study was typical of the casual jobs commonly performed in settings like ours, our measure of the SVT is likely appropriate for a broad set of activities in similar environments. Relatedly, technologies leading to large changes in daily time use would need to be handled with care. A researcher may need to elicit marginal values for different lengths of the work day. In these cases, our rule of thumb cost may be useful as a lower bound when an intervention increases workload, or an upper bound when an intervention leads to decreases in workload.

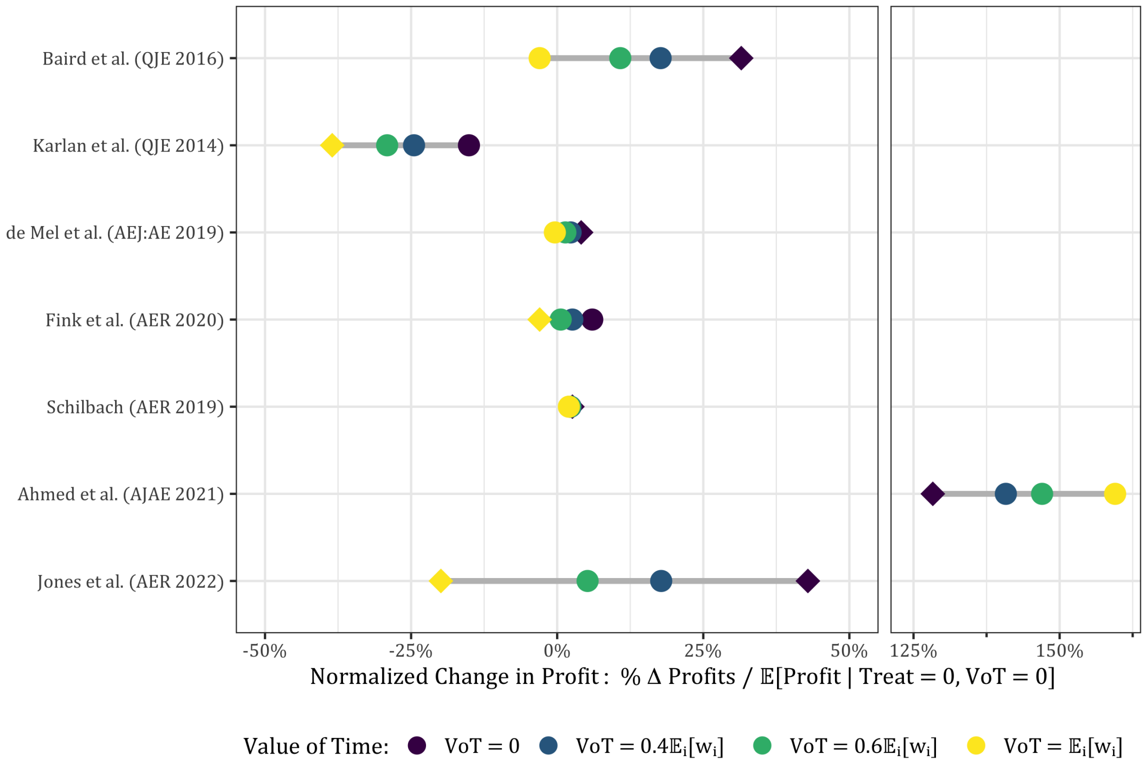
6.4 Applying Our Results to the Literature

Finally, we apply our bounding and rule of thumb strategies to prior studies. We calculate treatment impacts under four values of time of the self-employed: 0%, 40%, 60%, and 100% of the average market wage. Figure 5 shows results for six studies selected for their illustrative value. Results for the full set of studies that we could reevaluate are shown in Table G.1. To standardize outcome measures across studies, we report treatment effects on profits, normalized by mean profits in the control group. Note that most of these papers treat the value of time conservatively: valuing it at zero for time-saving interventions, and w for those that increase time use.

Impact assessments are most sensitive to assumptions about the value of time when the intervention significantly changes participants' labor. A few examples are Jones et al. (2022), which estimates the impact of irrigation by small-scale farmers; Baird et al. (2016), which finds long-run labor supply effects of de-worming; and Karlan et al. (2014), which studies the introduction of rainfall index insurance. In each case, treatment effect estimates vary dramatically depending on the assumed value of time. In particular, for Jones et al. (2022), as the authors themselves point out, impacts are negative when valuing time at the average market wage, but very large when the labor is valued at zero. A similar pattern can be seen in Baird et al. (2016).

For interventions producing modest changes in labor supply, the assumed value of time remains important, though its effects are less dramatic. Two examples are de Mel et al. (2019), which subsidizes paid employees of micro-enterprises, and Fink et al. (2020), which subsidizes loans to farmers during the lean season. In each study, estimated treatment effects are positive when valuing time using our rule of thumb of 60% of the average market wage, but negative when valuing time at the average market wage. For de Mel et al. (2019),

Figure 5: Sensitivity of Estimated Profit Impacts to the Assumed Value of Time



Diamonds represent the value of time assumed by the authors. Note the jump in the x-axis.

estimated treatment effects are statistically significant using the authors' assumed value of time of 0, but statistically insignificant when time is valued at 60% of the average market wage.

For interventions that do not meaningfully change labor supply, the assumed value of time of the self-employed is less important when calculating treatment impacts, even when labor represents a large share of costs. For example, in Schilbach (2019), the increase in household labor associated with the sobriety incentives is small (0.4%). Consequently, the normalized change in profits varies from 2.6% when household labor is valued at zero, to 2.0% when household labor is valued at the average market wage. Note, however, that valuing time appropriately is still likely to be be important for researchers measuring profit levels.

Finally, for labor saving technologies, using a more reliable value of time can increase their apparent efficacy. For example, Ahmed et al. (2021) studies the introduction of genetically modified eggplant in Bangladesh, which reduces the amount of time farmers spend weeding and applying pesticides. Note that profit estimates for this study, in Figure 5, are in reverse order—highest when time is most highly valued. In particular, valuing time at zero leads to an estimate that is too low, as it fails to account for the saved farmer labor. This highlights a general point: relative to more appropriate assumptions about the value of time, valuing participants' time at zero overestimates the efficacy of interventions that increase participants' time use, and underestimates the efficacy of those that save time.

6.5 Conclusion

Consistent with researchers often focusing on yield or revenue maximization rather than costs, reviews of technology adoption in low-income countries indicate there has been little study of labor-saving technologies (de Janvry et al., 2017; Magruder, 2018; Macours, 2019). The failure to properly account for labor—often a primary cost—may explain adoption failures for some technologies that appear welfare-improving. Further, technologies that could improve welfare by saving users' time may appear less useful in evaluations, and thus may not be deployed by development agencies.

Under the principle that we only value what we measure, accounting for the labor of self-employed workers may help redirect efforts to improve the lives of the poor in novel and useful ways. There are many channels by which labor-saving technologies can improve welfare: increased leisure (Devoto et al., 2012); increased female labor participation (Albanesi and Olivetti, 2016); increased school participation;[38] improved mental health (Whillans and West, 2021); improved cognitive capability (Bessone et al., 2021); reduced pain (Xiao et al., 2013), and reduced pain management through alcohol (Schilbach, 2019).

7 Data Availability Statement

The data and code underlying this research is available on Zenodo at https://dx.doi.org/10.5281/zenodo.13621574 (Agness et al., 2024).

[38] Pinker (2018, p. 231) cites this tractor advertisement from 1921: "By investing in a Case Tractor and Ground Detour Plow and Harrow outfit now, your boy can get his schooling without interruption, and the Spring work will not suffer by his absence. Keep the boy in school—and let a Case Kerosene Tractor take his place in the field. You'll never regret either investment."

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