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A dictator game study on human expectations of generosity using time as a reward medium

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Abstract

Measuring expectations of generosity when agents make decisions about allocating their time is essential to prevent frictions arising from the under-fulfillment of beliefs. For this reason, we developed a dictator game based on imaginary sharing situations. Subjects in hypothetical recipient roles made point estimates of how long fictitious allocators would be willing to spend alone in a low-stimulus room while varying the social distance and the stake size. The results obtained are in line with those observed in laboratory experiments applying distributions of monetary resources. Most participants predicted dictators would choose an equal split, and only a minority projected selfish or hyper-altruistic allocation. On average, those who perceived high social distance anticipated the same degree of generosity as those who marked their beliefs in response to a description of a low social distance environment. Expectations typically showed the same similarity when different stake sizes were in question. Age and gender did not have significant effects either.

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1. Introduction

Human prosociality is frequently measured by incentivized laboratory experiments using monetary rewards. However, money is not the only distributable resource in the field of behavior analysis. The participants' own time can also be allocated to benefit others. As economically active individuals, we often find ourselves in situations where someone counts on our help. For instance, imagine a trainee who has recently joined a company asking an experienced co-worker for assistance (e.g., understanding a business management software better) outside of regular working hours. From the perspective of the person in need of help, lending a hand can reduce the time associated with discomfort caused by the task's difficulty. From the helpers' point-of-view, it corresponds to a donation of free time. People might be prone to sacrifice a part of their leisure even though the provided help is sometimes boring. Furthermore, the parties requesting assistance may expect a certain degree of generosity. One can assume that differences between actual helping behaviors and relevant anticipations could generate workplace conflicts. Hence, quantifying first-order beliefs about others' prosociality seems essential in preventing potential frictions and misunderstandings. To this end, we attempted to elicit these kinds of predictions.[1]

The dictator game is a suitable research vehicle for assessing expectations of generosity.[2] In the typical one-shot anonymous interaction with randomly paired participants, the 1st player ("*dictator*") determines how to divide a specific amount of endowment (e) between herself (d) and a 2nd player ("*recipient*") ($r = e\text{-}d$).[3] The recipient has no decision-making power; thus, strategic considerations do not distort the allocation process. The pie is split according to the specified ratio, expressing dictators' selfishness or generosity. Anticipations about the distribution can be measured (with the exclusion of hedging) beforehand by eliciting guesses from external observers or subjects in the roles of imaginary recipients who do not receive the dictators' donations.[4] We used the latter to explore beliefs in hypothetical dictator games with time as a reward medium.

There are many different types of time applications in bargaining games.[6] For instance, Bekkers' (2010) subjects indicated their willingness to donate and volunteer for non-profit organizations in hypothetical cases. It was concluded that the smaller the social distance to someone asking for a contribution, the more likely that a person will give time and money. Charness *et al*. (2016) created an environment where subjects had to work for 60 minutes to benefit their own interests or a charity and then had a chance to volunteer for a 30-minute extension. By using high piece rates, workers' performance was better (they entered more real data from an unrelated experiment) when the payment was direct, compared to the organizational treatment. However, they observed an opposite relationship with low piece rates. In addition, a larger percentage participated in the voluntary second stage when the money was donated to the charity. In Lilley and Slonim's (2014) theoretical model, agents' utility over pure and impure motives varied across monetary donations and volunteering time.

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[1] The following terms are used interchangeably: anticipation, belief, expectation, and prediction.

[2] See Engel's (2011) meta-analysis to get a comprehensive picture of dictator game experiments.

[3] See Forsythe *et al*. (1994).

[4] For similar methods, see, e.g., Dufwenberg and Gneezy (2000), Aguiar *et al*. (2009), Iriberri and Rey-Biel (2013), Yamagishi *et al*. (2013), Capraro and Kuilder (2016), and Brañas-Garza *et al*. (2018).

[6] It is essential to highlight the fundamental differences between money and time. Although both are scarce and context-dependent resources, time is not storable and fungible; it cannot be saved or gained. Moreover, the variation in utility between losing or winning time of equal value to money is generally smaller (Leclerc *et al.* 1995, Okada and Hoch 2004, Danilov and Vogelsang 2015, and Leder *et al.* 2020).

Under laboratory testing, subjects gave more in the impure than in the pure frame when money and work time were simultaneously allocated to themselves and a charitable organization. Brown *et al.* (2019) explored the allocations in identical effort tasks. Their actors had a chance to switch between earning money for themselves or a charity at differential wage rates. Participants could donate their earnings to exploit potentially favorable conditions in a treatment operating with time and money. Here, a strong desire was identified to give via time. Other treatments were conducted, restricting the allocation to time or earnings only. Even though the activities resulted the same outcome, subjects gave more when they worked directly for the organization than when they worked for themselves and later donated the earnings. More recently, Leder *et al.*'s (2020) study on money and time in charitable giving suggests that people are more generous in donating the latter when its opportunity costs are controlled. The papers discussed above show that methodological approaches in this domain are diversified. Without losing sight of our research objectives, it is necessary to emphasize the findings drawn exclusively from interpersonal bargaining experiments. Ellingsen and Johannesson (2009) measured the proposals under time and monetary investments in an ultimatum game.[7] A third of the players demanded no compensation for their time investments as opposed to the monetary ones, for which almost all of the actors did. Danilov and Vogelsang (2015) analyzed whether participants engage in prosocial behavior if it requires a sacrifice of their time. Most of the dictators were willing to work on a tedious task when it benefited the exogenously disadvantaged recipients who otherwise did not receive any earnings. Noussair and Stoop (2015) conducted an ultimatum[8], a dictator, and a trust game[9] using time as a reward medium on a within- and between-subject basis. Before the participants received their earnings in the dictator game, the allocators distributed waiting time between themselves and the recipients. Subjects had to stay in their cubicles without using any object or doing any activity until the end of the designated period. The time-based decisions were similar to the monetary-based ones. The dictators, on average, donated 31 percent of the endowment, indicating that social preferences remain stable when the reward medium is different. Although these papers are similar to our research in many respects, they focus exclusively on the divisions and do not examine the actors' prior beliefs on the other side.

[7] First, proposers indicated their decisions regarding the implementation of an investment. Subsequently, the randomly paired anonymous individuals in different rooms bargained over how to divide a SEK 100 potential gain from trade. The first treatment was a standard ultimatum game with zero investment cost. The investment of the second one meant for the proposer a 20-minute stay after the end of the experiment, while in the third one, it was equal to the monetary cost of this act. The authors separately elicited a distribution for the time cost in the latter case. The monetary cost was randomly drawn for each proposer based on this distribution (Ellingsen and Johannesson, 2009).

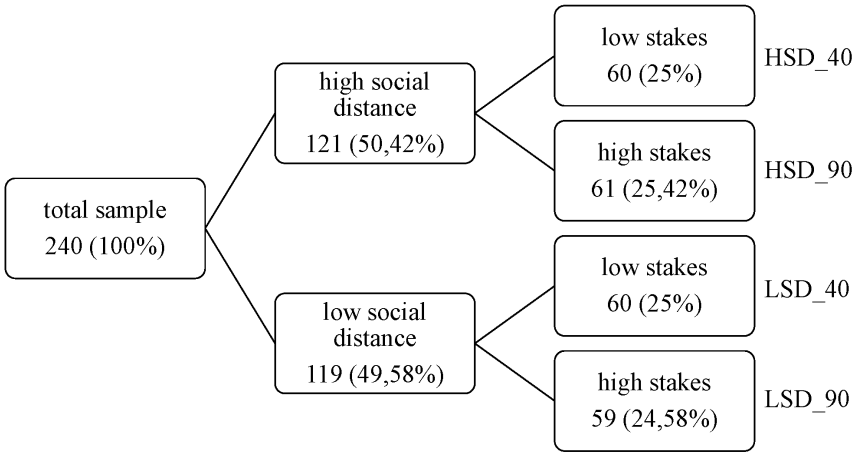
[8] Proposers split EUR 10 in the ultimatum games rewarded monetarily. In cases played for time, they indicated how long subjects had to wait in their cubicles out of 30 or 60 minutes. The division was made if the proposal was accepted. A rejection meant neither the proposer nor the respondent received any money, while in the time-based treatments, it required participants to wait for the maximum durations mentioned above (Noussair and Stoop, 2015).

[9] In the trust games conducted with monetary payments, respondents received three euros for every euro the proposers sent from their EUR 10 endowments. The former could choose to send money back, and the latter got one euro for each euro that had been returned. When time was used as a reward medium, subjects faced the prospect of waiting for 30 or 60 minutes. Proposers could stay longer, for 10 or 20 minutes at maximum. Respondents had the option to leave the experimental venue three minutes earlier for every minute the proposers agreed to stay longer. As a result, the respondents' current requirement was equal to the baseline duration minus three times the minutes received. They also could choose to stay longer. Proposers were free to leave one minute earlier for every minute their matched respondents marked in respect of the additional waitings (Noussair and Stoop, 2015).

Similar to Noussair and Stoop (2015), we used the waiting time spent in boredom as the medium of reward to elicit expectations related to the life situation outlined in the first paragraph. Time allocations were anticipated within a between-subject 2×2 factorial design with four experimental conditions depicted in Figure 1.[10] In the case of treatments with low (high) stakes, participants expressed their beliefs about the distribution of a 40 (90) minute long waiting period. In addition to the stake sizes, two different levels of social distances were applied. The game instructions described an imaginary dictator who was allocating her time alone (HSD: high social distance) or in the presence of the subject as a hypothetical recipient (LSD: low social distance). Our primary objective was to measure how recipients perceive the dictators' willingness to take negative impacts upon themselves to cause less discomfort for the passive parties. In this context, the relevant research question was formulated as follows:

RQ: Do expectations of generosity significantly differ by social distance and stake size?

Figure 1: Sample classification



Brañas-Garza *et al.* (2017) found that most people anticipate hyper-fair behavior (i.e., an equal split) when money is the reward medium. Moreover, human expectations of generosity are not appreciably affected by the social distance or the stake size, meaning that the average magnitudes of predictions remain the same across various treatment conditions. Since Noussair and Stoop (2015) did not identify significant differences in dictators' donating behavior concerning monetary payment and time allocations, we hypothesize the following:

H1. The anticipated time fraction is independent of the social distance: by using high and low social distances, subjects expect, on average, the same level of generosity from hypothetical dictators.

H2. The anticipated time fraction is independent of the stake size: by using low and high stakes, subjects expect, on average, the same level of generosity from hypothetical dictators.

The remainder of the paper is structured as follows. Section 2 presents the experimental procedure and treatments. Section 3 contains the results of the statistical and econometric analyses. Finally, section 4 concludes and highlights the limitations and future research directions.

[10] The Experimental procedure and treatments section provides a detailed explanation of each condition.

2. Experimental procedure and treatments

A double-blind experiment based on hypothetical decision problems was conducted over seven sessions with the participation of undergraduate volunteers in an auditorium of the University of Pecs, Faculty of Business and Economics, between 14-17 November 2022. Subjects were recruited through Financial Literacy, Human Resource Management and Management Organization courses and received educational incentives as "show-up fees".[14] Altogether 240 individuals (97 males and 143 females; mean age = 19.97 ± 1.49 years) participated in the analysis without any prior bargaining game experiences. Deceptive techniques were not applied, and fully informed consent was obtained before the students entered the experiment. The data management complied with the relevant data protection laws applicable in Hungary (in particular, the Act CXII of 2011 on the Right of Informational Self-Determination and on Freedom of Information and the General Data Protection Regulation (EU) 2016/679). Subjects' decisions were registered in the database using a randomly generated 8-digit numerical code to ensure complete anonymity.[15] The Disciplinary and Ethics Committee approved the procedure. All methods were performed following the ethical principles and requirements in the current University Regulations.

At the start of each session, individuals got identity numbers in the order of arrival, which were then assigned one of the four treatments by a random group generator. Subjects were seated in a large lecture hall (non-lab environment) at a suitable distance from each other. The numbers allocated on arrival were read aloud. Participants of the first group (procedure) indicated their membership by a show of hands and then received specific decision sheets and general consent forms in sealed envelopes.[21] The process has been repeated according to the remaining treatments. After everyone received the necessary documents, the research leader described the task and answered any questions.[22]

In the HSD_40 treatment with high social distance and low stakes, subjects made their predictions after reading a hypothetical allocation described as follows:

"*Imagine that an unknown, randomly selected person[24], with whom you will never meet, can distribute a 40-minute waiting time between herself and you in any proportion. In an extreme case, this person could devolve the entire waiting time to you or, on the contrary, take it over completely. Until the end of the waiting times set by the person's allocation, you should stay seated alone in separate rooms without any communication or use of equipment. The rooms would have plain white walls and a skylight to provide a low-stimulus environment with only one chair. Please indicate how many minutes out of 40 you think the person above would take over. You should stay seated for the rest of the time.*"

After estimating the duration, subjects also indicated their age and gender on the decision sheets, provided the requested personal data on the consent forms and then placed them in two separate temporarily sealed containers when they left the lecture hall.

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[14] Enrolled students were entitled to five extra points each for the courses in which they indicated their intention to participate.

[15] The dataset is available upon request from the authors.

[21] The decision sheets did not contain any information to enable participants to be identified. All the personal data for crediting educational incentives were collected separately via the consent forms.

[22] The research leader stayed in the lecture hall to ensure that subjects made their predictions independently. However, he kept his distance to minimize the experimenter demand effect.

[24] We did not deliberately use "*dictator*" to avoid framing.

For low social distances (LSD_40 and LSD_90), the following wording was applied:

"*Imagine that an unknown, randomly selected person in the same location as you, with whom you see each other but are not allowed to communicate, can distribute a 40-minute waiting time between herself and you in any proportion. (...)*"

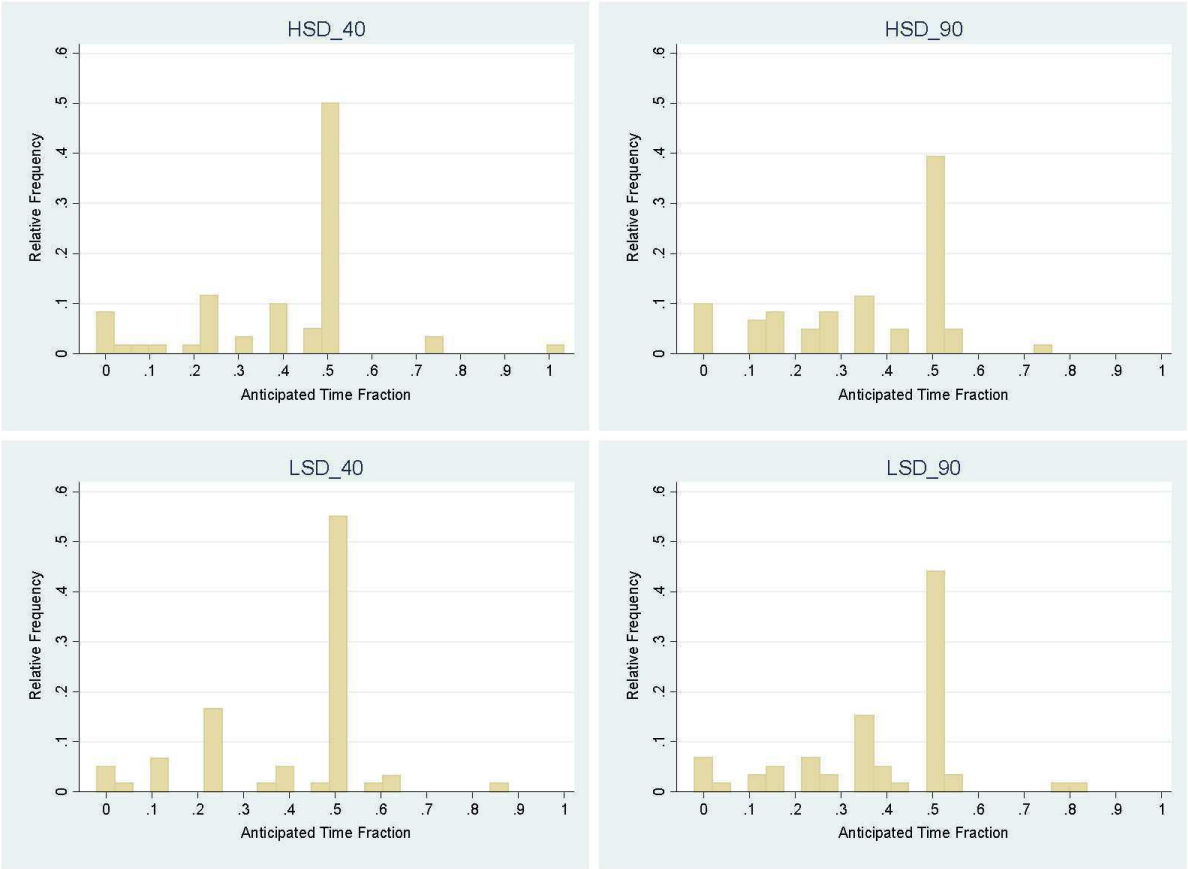
In treatments with high stakes (HSD_90 and LSD_90), the modification was:

"(…) can distribute a 90-minute waiting time between herself and you in any proportion. (…) Please indicate how many minutes out of 90 you think the person above would take over. (…)"

3. Results

Subjects estimated how much time the hypothetical dictators would take over and spend alone in a low-stimulus environment. Higher anticipated values indicate longer waiting periods for the allocators, which means that recipients expected more generous behavior. We divided these predictions by 40 or 90 depending on the treatments' stake sizes to make them comparable. 38% of the subjects anticipated hyper-fair behavior (i.e., the equal split) in the total sample. Extremes such as selfishness (7.5%) and hyper-altruism (0.4%) were rarely expected. Descriptive statistics can be found in Table A.I (Appendices), while Figure 2 provides information regarding the shapes and variabilities.

Figure 2: Histograms of expectations



Econometric analysis was performed to estimate the factor effects on expectations. The significance level was set at $\alpha = 0.05$. Table I contains the results of the applied Tobit regression.[26] The values of the outcome variable are defined in proportion to the stake sizes.[29] While models (1) and (2) have the factor dummies separately, we included them simultaneously in model (3). Participants' *age* and *gender* were also added as controls. In all cases, null effects were obtained. However, the negative Pseudo R^2 values indicate that we used an inappropriate model specification. A heteroscedasticity-robust hurdle approach was therefore implemented, allowing for the impact of excess zeros.

Table I: Summary of Tobit regression results

	Coefficient[32]			Average marginal effect[33]		
	(1)	(2)	(3)	(1)	(2)	(3)
Low social distance	0.023		0.023	0.022		0.023
	(0.026)		(0.026)	(0.025)		(0.025)
High stakes		-0.040	-0.040		-0.039	-0.039
		(0.026)	(0.026)		(0.025)	(0.025)
Age	-0.009	-0.010	-0.011	-0.009	-0.010	-0.010
	(0.009)	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)
Females	0.006	0.003	0.003	0.005	0.003	0.003
	(0.026)	(0.026)	(0.026)	(0.026)	(0.025)	(0.025)
Constant	0.538***	0.596***	0.598***			
	(0.184)	(0.193)	(0.192)			
F-value	0.50	0.94	0.91			
Pseudo R^2	-0.0495	-0.0958	-0.1196			
Observations	240	240	240			

*p < 0.10, **p < 0.05, ***p < 0.01

The Hurdle is a mixture model that estimates a binary component (i.e., the threshold probabilities between zero and positive magnitudes) independently and a count component (in our case, the natural logarithms of the anticipated time fractions). More precisely, it separates the willingness to predict positive waiting times (Hurdle0) from the forces that determine how subjects' expectations on hypothetical dictator decisions develop (Hurdle+). Table II contains the results of the applied Hurdle regression.[34] The values of the outcome variable are defined in proportion to the stake sizes.[36] Our models fit significantly better than ones with no predictors. Similar null effects can be identified here, as in the Tobit procedure, supporting the hypotheses outlined in the introduction. In addition, no significant age or gender effects were found.

[26] Treatment effects can be found in Table A.II (Appendices).
[29] 18 observations are left-censored at expectation ≤ 0, 221 are uncensored, and 1 is right-censored at expectation ≥ 1.
[32] Robust standard errors in parentheses.
[33] Delta-method standard errors in parentheses.
[34] Treatment effects can be found in Table A.III (Appendices).
[36] 18 observations are censored, and 222 are uncensored.

Table II: Summary of Hurdle regression results

	Hurdle0 Coefficient[39]			Hurdle+ Coefficient[40]			Hurdle+ Average marginal effect[41]		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Low social distance	0.034		0.021	0.022		0.022	0.022		0.022
	(0.072)		(0.042)	(0.071)		(0.071)	(0.032)		(0.031)
High stakes		-0.311	-0.002		-0.076	-0.076		-0.040	-0.044
		(0.435)	(0.004)		(0.074)	(0.074)		(0.032)	(0.032)
Age	-0.004	-0.033	-0.003	-0.018	-0.021	-0.022	-0.004	-0.013	-0.005
	(0.007)	(0.043)	(0.005)	(0.024)	(0.025)	(0.025)	(0.011)	(0.011)	(0.012)
Females	0.006	0.133	0.004	-0.063	-0.067	-0.067	-0.006	-0.015	-0.009
	(0.013)	(0.239)	(0.009)	(0.070)	(0.068)	(0.068)	(0.032)	(0.031)	(0.031)
Constant	0.099	1.147	0.072	-0.598	-0.482	-0.476			
	(0.166)	(1.481)	(0.122)	(0.491)	(0.525)	(0.523)			
Wald Chi-Square	1744.35***	118.46***	2496.40***						
Pseudo R^2	0.0190	0.0143	0.0295						
Observations	240	240	240						

*p < 0.10, **p < 0.05, ***p < 0.01

4. Conclusion

Our experiment used the waiting time spent in a low-stimulus environment as a reward medium to measure human expectations of generosity via hypothetical dictator games. It aimed to quantify recipients' beliefs about imagined dictators' willingness to help experience less discomfort. Anticipations were elicited in four treatments varying in two factors (social distance and stake size). In line with the literature on conventional dictator games conducted with monetary payment allocation, the findings here support that most subjects predict hyper-fair behavior (i.e., an equal split). We did not identify significant discrepancies in the factor comparisons, indicating that our earlier assumptions have been confirmed. Expectations were, on average, identical when recipients read that the hypothetical distribution is anonymous and when they knew dictators could see them during the decision-making process. The same conclusion stands for small and high stakes as well. In other words, humans expect a relatively high level of generosity when the opposite parties need to sacrifice their own time. The extent of these anticipations does not seem to fluctuate substantially through different life situations. Age and gender are not determinants either. Accordingly, it is essential to consider this specificity to avoid conflicts arising from the potential under-fulfillment of beliefs.

This approach dealt with a non-standard hypothetical framework by not using monetary incentives. Hence, our work is only preliminary, and we interpret the findings as "suggestive" evidence with several limitations for the following reasons. On the one hand, the null effects might have occurred as a result of negligence if participants did not pay close attention to the wording of different decision sheets. On the other hand, a student sample may not be considered an appropriate subject pool to study social behavior due to the lack of age, gender, and cultural context representativeness. Moreover, we focused solely on the recipients' beliefs and did not elicit real dictators' allocations and expectations. Therefore, further analyses are needed to

[39] Robust standard errors in parentheses.
[40] Robust standard errors in parentheses.
[41] Delta-method standard errors in parentheses.

quantify decisions in a laboratory environment with a larger sample size and different amounts of stakes where subjects can receive cash payments for their correct predictions.[42]

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[42] See, e.g., Capraro and Kuilder (2016), Brañas-Garza *et al.* (2017), and Brañas-Garza *et al.* (2018).

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Appendices

Table A.I: Descriptive statistics of expectations

Variable	Obs	Min	Max	Mean	Median	Std. Dev.	Variance	Skewness	Kurtosis
HSD_40	60	0.00	1.00	0.398	0.500	0.196	0.039	-0.271	3.870
HSD_90	61	0.00	0.72	0.351	0.444	0.184	0.034	-0.519	2.158
LSD_40	60	0.00	0.88	0.403	0.500	0.177	0.031	-0.642	3.303
LSD_90	59	0.00	0.83	0.383	0.500	0.181	0.033	-0.425	3.088

Table A.II: Summary of Tobit regression results

	Coefficient[44]	Average marginal effect[45]
HSD_90	-0.053 (0.038)	-0.052 (0.036)
LSD_40	0.010 (0.037)	0.010 (0.036)
LSD_90	-0.017 (0.037)	-0.016 (0.036)
Age	-0.011 (0.010)	-0.010 (0.009)
Females	0.002 (0.026)	0.002 (0.025)
Constant	0.601*** (0.193)	
F value	0.79	
Pseudo R^2	-0.1275	
Observations	240[46]	

*p < 0.10, **p < 0.05, ***p < 0.01

[44] Robust standard errors in parentheses.
[45] Delta-method standard errors in parentheses.
[46] 18 observations are left-censored at expectation ≤ 0, 221 are uncensored, and 1 is right-censored at expectation ≥ 1.

Table A.III: Summary of Hurdle regression results

	Hurdle0 coefficient[48]	Hurdle+ coefficient[49]	Average marginal effect[50]
HSD_90	-0.002 (0.005)	-0.117 (0.102)	-0.054 (0.042)
LSD_40	117.116*** (22.532)	-0.019 (0.106)	0.007 (0.046)
LSD_90	0.002 (0.006)	-0.054 (0.102)	-0.025 (0.044)
Age	-0.003 (0.004)	-0.021 (0.025)	-0.006 (0.012)
Females	0.004 (0.008)	-0.068 (0.069)	-0.007 (0.031)
Constant	0.060 (0.104)	-0.476 (0.525)	
Wald Chi-Square	2280.55***		
Pseudo R^2	0.0373		
Observations	240[51]		

*p < 0.10, **p < 0.05, ***p < 0.01

[48] Robust standard errors in parentheses.
[49] Robust standard errors in parentheses.
[50] Delta-method standard errors in parentheses.
[51] 18 observations are censored, and 222 are uncensored.