

ABSTRACT

Title of Document: SOCIAL PREFERENCES AMONG
CLINICIANS IN TANZANIA: EVIDENCE
FROM THE LAB AND THE FIELD

Judith Michelle Brock, PhD 2011

Directed By: Professor Kenneth L. Leonard, Department of
Agricultural and Resource Economics

Health worker effort can have a dramatic influence on patient outcomes. This is especially true in developing countries, where poor overall quality of healthcare systems is the norm. There is evidence, however, that despite low levels of education and experience, clinicians in Tanzania underperform relative to their ability (Leonard and Masatu, 2005). Understanding clinicians' intrinsic motivations may help us identify nonmonetary incentives for improving quality of care. To this end, this dissertation considers how risk, pride and social information impact altruism among Tanzanian clinicians.

In Chapter 4, we study how risky environments impact social preferences. With experimental evidence from games with risky outcomes, we establish that social preferences of players who give in standard dictator games are best described by consideration of equating ex ante chances to win rather than of ex post payoffs. The more

money decision-makers transfer in the dictator game, the more likely they are to equalize payoff chances under risk. Risk to the recipient does, however, generally decrease the transferred amount.

Also, while some people behave generously regardless of the attributes of others, pride and knowledge about the recipient characteristics may also motivate altruistic behavior. In Chapter 5, we explore the role of social information and pride in determining pro-social behavior among clinicians in Tanzania. We find that making someone feel proud increases the number of “fair” allocations (50/50 giving) and that those who do not respond to decreased partner anonymity are less responsive to induced pride.

Chapter 6 combines laboratory data on social preferences and field data on clinicians’ workplace effort. This study is unique in that we observe the same subjects from the laboratory in a field setting, where pro-social behavior has large welfare impacts. We use modified dictator games to define subjects as fair types, social information responsive types and pride responsive types and test how those characteristics are correlated with effort in the workplace. We find that clinicians responsive to both pride and social information provide higher than average effort in the workplace. These results are suggestive of Ellingsen and Johannesson’s (2008) theory of social preferences wherein social identity and esteem interact to motivate altruism.

SOCIAL PREFERENCES AMONG CLINICIANS IN TANZANIA:
EVIDENCE FROM THE LAB AND THE FIELD

By

J. Michelle Brock

Dissertation submitted to the Faculty of the Graduate School of the
University of Maryland, College Park, in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
2011

Advisory Committee:
Professor Kenneth L. Leonard, Chair
Professor Mark Duggan
Professor Vivian Hoffmann
Professor Marc Nerlove
Professor Erkut Ozbay

© Copyright by
J. Michelle Brock
2001

Dedication

To Colston, for being the best “dissertation baby” he could be. Believe in yourself and surround yourself with people who believe in you and you will follow your dreams to the end. And may you always have a dream!

Acknowledgements

This research was funded with seed grants from the Maryland Population Research Center and the Maryland Agricultural Experiment Station Competitive Grants program. We also received generous funding from The World Bank.

I am grateful to my advisor, Kenneth Leonard, for his guidance these last three years, for keeping me funded and for all that he has done to make the department a thriving home of development studies. Working with Professor Leonard has been integral to my success at AREC and to me rediscovering how much I enjoy in what I do. I am also grateful to Kenneth Leonard and Andreas Lange for hiring me to be the R.A. for their project. It was one of the best phone calls I have gotten in my six years here. Special thanks are also due to Andreas Lange for helping me design, fund and implement my experiments. I also benefitted from Professor Lange's guidance in and enthusiasm for the field of Experimental and Behavioral Economics. I also acknowledge Professor Erkut Ozbay and Professor Lange for working with me on our paper, which comprises a large part of Chapter 4 in this dissertation. Finally, I want to acknowledge my dissertation committee: Professor Vivian Hoffmann, Professor Marc Nerlove, Professor Erkut Ozbay and Professor Mark Duggan.

Other professors who have shaped my time in graduate school and to whom I am grateful are Professor Lars Olson, Professor Ted McConnell, Professor Robert Chambers and Professor Ramon Lopez.

I could not have done the field work for this dissertation without the help and wisdom of Dr. Melkiory Masatu and Dr. Beatus Leon. Thank you also to the Tanzanian Ministry of

Health and to our partner institution in Arusha, the Center for Educational Development of Health Arusha (CEDHA) for hosting me and our research project. A special acknowledgement is due Amina Rajab; I did not think it possible someone could enter data with so little human error. Thank you also to our enumerators and the clinicians and nurses who worked with us to collect our data. Thank you to William Francis for managing the project while I was back in the States. I also want to thank the Jacobsons and ALMC for providing me a safe and comfortable home while I was in Arusha. On a personal note, I am grateful to JMe, Bob, Linda, Dolly and Paive for their support and friendship while I was in Tanzania.

My family and friends have provided endless support to me during this process. Thank you to my dad who never hesitates to feed my intellectual curiosity; I am convinced that conversations with him planted the seed of my dissertation topic in my head when I was a wee one. I am grateful to my mom for encouraging me to seek balance in my life while I have been absorbed in graduate school. Thank you to my siblings: Amie, Dave, Jenni, Dan, Katie and Michael, and especially to my husband Emory who celebrated the high times with me and rode out the storms of me being in a PhD program with grace. Last but not least I cannot thank enough my peers in at UMD who have shared this journey with me: Asif, Katya, Dimitrios, Kahwa, Kohei, Qing, Elisabeth, Nitish, Andy, Steve and Geret you have all made my experience at AREC so much more than I expected. Special mention goes to Kahwa Douoguih for her amazing help with my experiment at Maryland Day. I have had some excellent peer mentors in my career at AREC: Nate, Sarah and Jia you have convinced me more than once, or twice, that I would find my path and excel. You are all examples I look to frequently.

Table of Contents

Dedication.....	ii
Acknowledgements.....	iii
Table of Contents.....	vi
List of Tables.....	viii
List of Figures.....	x
Chapter 1 : Introduction.....	1
Chapter 2 : Altruism, Workplace Motivation, and Risky Giving.....	10
2.1. Theory.....	11
2.1.1. Social preferences.....	11
2.1.2. Social preferences and risk.....	15
2.1.3. Pro-social behavior in the work environment.....	17
2.2. Evidence.....	22
2.2.1. Social preferences and risk.....	23
2.2.2. Pro-social behavior in the work environment.....	26
2.2.3. Summary.....	29
2.3. Conclusion.....	29
Chapter 3 : Healthcare and Health Workers in Tanzania.....	31
3.1. Introduction.....	31
3.2. Pride, social identity and risk in health worker decision making.....	32
3.3. Health workers’ social preferences, theory.....	34
3.4. Measuring healthcare quality in developing countries.....	36
3.5. Healthcare in Tanzania.....	40
Chapter 4 : Risk and Altruism.....	45
4.1. Introduction.....	45
4.2. A Model of behavior for risk allocation and risk sharing.....	50
4.3. The Experiment.....	52
4.4. Pilot implementation: UMD students.....	57
4.4.1. Pilot results and discussion.....	60
4.5. Full implementation: Tanzanian clinicians.....	67
4.5.1. Preliminary results, Tanzanian clinicians.....	68
4.6. Discussion and conclusions.....	72
4.7. Tables and figures.....	75
Chapter 5 : Pride and Social Identity Based Social Preferences.....	89
5.1. Introduction.....	89
5.3. The Experiment.....	93
5.4. Maryland Day pilot implementation: General public.....	95
5.5. Full implementation: Tanzanian clinicians.....	102
5.5.1. Main results.....	106
5.6. Conclusion.....	109
5.7. Tables and figures.....	111
5.8. Appendix.....	119
5.7.1. A Model of behavior for social preferences in clinician decision making.....	120
5.7.2. Suggestive results on the social distance hypothesis.....	123

5.7.3. Appendix Tables	128
Chapter 6 : Clinicians’ Social Preferences in the Workplace	131
6.1. Introduction	131
6.2.1. The sample -- clinicians in Arusha	132
6.2.2. Procedures	133
6.2.3. Instruments	134
6.3.1. Main estimation	136
6.3.2. Alternative specifications	139
6.4. Results and discussion	141
6.4.1. Results from the main estimation and alternate specifications	141
6.4.2. Reverse analysis – evaluating the role of negative social attitudes	148
6.5. Conclusion	152
6.6. Tables and figures	154
Chapter 7 : Sensitivity Analysis	163
7.1. Introduction	163
7.2. Investigating random assignment	163
7.2.1. Evidence of non-random assignment	165
7.2.2. Controlling for facilities with random assignment	168
7.2.3. Controlling for number of clinicians per facility	169
7.3. Including clinician ability and item difficulty	171
7.3.1. Item Response Theory	171
7.4. Difficulty-weighted effort	175
7.4.1. Constructing the weighted average	176
7.5. Conclusion	180
7.6. Tables and figures	182
Chapter 8 : Conclusion	187
Appendices	193
Appendix A: Laboratory Experiment Instructions	193
A.1. Risk and altruism experiment, UMD students	193
A.2. Social information and pride experiments, “Maryland Day” sample	207
A.3. All treatments, Tanzanian clinicians	217
Appendix B: Survey Instruments	251
B.1. Retrospective Consultation Review	251
B.2. Clinician Consent Visit Survey	254
Appendix C: Item Response Theory Parameter Estimates	256
Bibliography	260

List of Tables

Table 1.1. Data sources, corresponding subjects and where it appears in the dissertation.	6
Table 4.1. Summary of tasks.....	75
Table 4.2. Summary statistics of the dictators' choices	75
Table 4.3. Summary statistics of the recipients' expectations.....	76
Table 4.4. Summary statistics of giving by task, conditional on $TG > 0$	76
Table 4.5. Differences in average tokens given, conditional on $TG > 0$	76
Table 4.6. Differences in average tokens given, unconditional (N=76).....	77
Table 4.7. Tobit regression of choices in tasks on dictator game decisions, with cluster robust standard errors, full data set.....	77
Table 4.8. Tobit regression of choice sin tasks on dictator game decisions, with cluster robust standard errors, conditional on giving in at least one task.....	78
Table 4.9. Tobit regression of choices in tasks on dictator game decisions, with cluster robust standard errors, conditional on $Task1 > 0$	79
Table 4.10. Linear regression of choices in tasks on dictator game decisions, with cluster robust errors, conditional on $Task1 > 0$ and $Taski > 0$	79
Table 4.11. (selfish binary) Tobit regression of choices in tasks on dictator game decisions, with cluster robust standard errors	80
Table 4.12. Maximum likelihood estimates with random effects (column 1); Probit models (columns 2-4) on dictators; choices of the difference tasks (baseline is dictator game T1).....	81
Table 4.13. Summary statistics of dictator allocation choices, Tanzanian sample.....	84
Table 4.14. Summary statistics of recipient expectations, Tanzanian sample.....	85
Table 4.15. Difference in tokens given, Tanzanian sample.....	85
Table 4.16. Regressions of choices in tasks on dictator game decisions.....	86
Table 4.17. Maximum likelihood estimates of treatment effects, probits of dictator type on dictators' choices for the different tasks, Tanzanian sample.....	86
Table 4.18. Linear maximum likelihood estimates of treatment effects; dependent variable is dictators' choices for the different tasks	87
Table 5.1. Respondent characteristics, T2 and T3 (demographic data was not collected for T1).....	111
Table 5.2. Maryland Day participant characteristics by player role.....	112
Table 5.3. Maryland Day dictator characteristics by treatment.....	112
Table 5.4. Summary of dictator game giving, UMD Maryland Day.....	113
Table 5.5. Differences in tokens given, UMD Maryland Day.....	113
Table 5.6. Participant characteristics, Tanzanian sample	114
Table 5.7. Summary statistics of dictator allocation choices, Tanzanian sample.....	115
Table 5.8. Summary statistics of recipient expectations, Tanzanian sample.....	115
Table 5.9. Differences in tokens given, Tanzanian sample	116
Table 5.10. Distribution of selfish giving, by treatment.....	116
Table 5.11. Maximum likelihood estimates of treatment effects with individual random effects (column 1) and probit models (columns 2-4) of dictators type on dictators' choices for the different tasks	118
Table 5.12. Differences in giving by paired characteristic	128

Table 5.13. Differences in giving by dictator characteristic	129
Table 5.14. Differences in giving by recipient characteristic	130
Table 6.1. Patient characteristics by facility type, full sample	154
Table 6.2. Patient Characteristics by facility type, reduced sample	154
Table 6.3. Clinician characteristics by facility type.....	155
Table 6.4. Clinician characteristics by facility type, reduced sample.....	155
Table 6.5. Distribution of each credential level by facility type, full sample.....	155
Table 6.6. Distribution of each credential level by facility type, reduced sample.....	156
Table 6.7. Probit of participation in laboratory experiment on practice quality, clinician level with robust standard errors.....	156
Table 6.8. OLS regression of average effort by consultation on social attitudes, with facility level random effects and various error structures.....	157
Table 6.9. OLS regression of average effort with facility level random effects, controlling for experience, errors clustered at the facility level.....	158
Table 6.10. OLS regression of average effort on social attitudes, excluding rural facilities	159
Table 6.11. OLS regression of average effort on social attitudes, additional specifications as indicated by the column headers	160
Table 6.12. OLS regression of average effort on negative social attitudes; column headers refer to the definition of social attitude variables in each regression, labeled "Social Information" and "Pride" in the row headers	162
Table 7.1. Checking random assignment, results from a MANOVA by patient characteristics age, sex and education (p-values on F tests for facilities with non-random assignment)	182
Table 7.2. OLS estimation controlling for non-random assignment and facility size (clinicians per facility)	183
Table 7.3. Summary statistics for the difficulty weighted average and related variables	184
Table 7.4. OLS regression of difficulty-weighted average effort on social attitudes, controlling for practice ability	185

List of Figures

Figure 4.1. Average tokens given by task.....	82
Figure 4.2. Percent of subjects that choose to give non-zero amounts.....	82
Figure 4.3. Average tokens given, conditional on giving greater than zero.....	83
Figure 4.4. Choices and expectations in the respective tasks.....	83
Figure 4.5. Tokens given by treatment, Tanzanian clinicians (bin labels are the lower bound of bin contents).	84
Figure 4.6. Distribution of tokens given and tokens expected.....	88
Figure 5.1. Empirical distributions by treatment.....	116
Figure 5.2. Frequency distribution of tokens given across treatments T1, T2 and T3 ...	117
Figure 5.3. Distributions of tokens given and tokens expected.....	117
Figure 6.1. Distribution of social information responsive, pride responsive and fair types.....	156
Figure 7.1. Scree Plots from a factor analysis of task completion, by symptom.....	184

Chapter 1 : Introduction

Healthcare workers, like workers in all industries, face the daily choice of how much effort they should exert in their jobs. The common problems of optimal wage contracts, shirking and free-riding on overall firm productivity apply to construction workers and health workers equally. But unlike in most other industries, the effort choices that doctors and nurses make can have dramatic impacts on their patients' lives. The impact can be positive or negative. Shirking in this context can have deleterious effects for patients. This is especially true in developing countries, where poor overall quality of health care systems is the norm. In these resource-poor settings, low quality is attributed, at least in part, to under qualification of health workers. Efforts to improve healthcare in developing countries, therefore, typically focus on increasing human capital: training doctors, nurses and rural health workers in the skills required to do their jobs effectively. There is evidence, however, that despite low levels of education and experience, health workers underperform relative to their ability (Leonard and Masatu, 2005; Leonard et al., 2007). Incentivizing healthcare professionals to exert maximal effort for their patients is the subject of a vast body of literature, describing a wide variety of experiences in both more- and less- developed countries. But how to best use incentives to improve performance among health professionals is still not well understood. Monetary incentives, such as increased pay or bonuses, are an obvious option for motivating doctors and nurses. But non-monetary incentives present another – and less well-understood – set of options for motivating better performance from health workers. The established literature on

nonmonetary incentives is small, and typically focuses on the ways in which monitoring and feedback can motivate doctors to provide more effort. In order to better understand how to construct effective nonmonetary incentives, researchers have also begun to consider the underlying, intrinsic motivations of health workers. What factors drive health workers to exert more than the minimal effort for their patients?

This dissertation contributes to this literature by providing some insight on the intrinsic motivations of health workers. Our focus is the Tanzanian healthcare system. We aim to answer the question of whether clinicians have pro-social preferences and how it may impact their effort choices in outpatient consultations. Laboratory-generated evidence of pro-social behavior has been documented for countless samples of university students (e.g. Andreoni, 1990; Andreoni, 1995; Charness and Rabin, 2002; Eckel and Grossman, 1996), but looking at social preferences in the workplace or among professionals is a newer phenomenon (Bandiera et al., 2005; Levitt et al., 2010; List and Mason, 2011). To our knowledge, clinicians in particular have not been brought into the laboratory. This dissertation focuses on two key components of clinicians' workplace that may impact their expression of social preferences: an "interpersonal aspect" and a "risky aspect." We consider the extent to which a certain type of interpersonal impure altruism may play a role in effort choice and the extent to which risky environments may impact generosity. Our research combines laboratory experiments and data from the field. We use the laboratory to address basic questions about social preferences that cannot be identified in the field. We also look at the same sample of clinicians in their ordinary workplace setting to determine the correspondence of social attitudes measured in the laboratory with behavior in the field. The aim of this approach is to provide evidence on the role of

social incentives in effort choices for workers in the Tanzanian healthcare industry, and how these incentives might reflect on the broader choices made by the wider population of healthcare professionals in other settings.

Healthcare workers provide an interesting sample because they are often perceived to be socially-oriented. There has been little work done, however, on how social incentives may (or may not) motivate their effort choices at work. The ethics of acting in the patient's interest are central to the Hippocratic oath sworn by all doctors, but the extent to which health workers should sacrifice their own wellbeing is debated (Pellegrino, 1987; Reid, 2005; Straus et al., 2004; Tomlinson, 2008). A health worker's job requires balancing of the worker's own self-interest against the interest of patients and the interests of the institution that employs them (Morreim, 1995). The question of social incentives in the healthcare field is further clouded by the status benefits often associated with being a health worker. The title "doctor" carries respect in most societies and, in developing countries where perhaps the pay is not particularly impressive, it is at least a profession that offers more or less consistent employment. Still, there is some evidence that health workers bring altruistic attitudes to the job. Health workers in Ethiopia, for example, report a desire to help others as a reason for entering the profession (Lindelow and Serneels, 2006; Serneels et al., 2010). Perhaps most likely is that doctors are heterogeneous in their altruism (Delfgaauw, 2007), with some doctors more motivated by monetary incentives and other more motivated by social (non-monetary) incentives. The question then turns to what forms of social preferences motivate doctors to work hard. A clinician, after all, has not only the patient to think about, but also may internalize organizational goals or behave strategically so as to earn the good opinion of their peers.

Leonard and Masatu (2006) find that in Tanzania clinicians try to “buy” approval from their peers with increased effort levels when under scrutiny. The opinion of peers can also be wrapped into the clinician’s desire to qualify as “professional”. Professionalism in healthcare is important for establishing oneself as a reliable provider, to both peers and patients (Freidson, 2001). Furthermore, it is possible that this kind of “esteem-seeking” behavior may also occur to some degree with patients, and that the attributes of the patient, relative to the clinician’s own attributes, help to determine the resultant effort exerted on the patient’s behalf. This suggests that there is more behind clinician behavior toward patients than has been controlled for in past studies.

For this research, we collected data with laboratory experiments and with a field survey. We conducted the laboratory experiments with three different groups of people. The primary group, and the focus of our research, is the Tanzanian clinicians. The clinicians participated in five experimental treatments, all of which were variations on a standard dictator game. The baseline treatment is a standard dictator game. Two of the other treatments explore the role of social information and pride in altruism. Finally, the other two treatments look at how risk impacts altruism. We piloted these five treatments (as well as four other treatments relating to risk and altruism) at the University of Maryland, before implementing them in Tanzania. The two groups that participated in the pilots were University of Maryland (UMD) students and a segment of the general public affiliated with UMD. The goal of the pilots was to learn which treatments would perform best in the experiments with the clinicians. We evaluated performance of the treatments in terms of the hypotheses tested, the saliency of payoffs to the different players, the clarity of instructions and the logistics of implementation. Since time and money were

limited for running the experiments in Tanzania, we chose only five of the nine treatments that were piloted.

The treatments that deal with risk and altruism were piloted with UMD students. This pilot took place in a computer lab on the UMD campus in September 2009. We ran 7 sessions. We designed the experiments in order to investigate how risk influences altruism in the laboratory. While serving as a pilot for the experiments we ran in Tanzania with the clinicians, these treatments also reveal results that are interesting in their own right. Results appear in Chapter 4. A complete write up appears in Brock, Lange and Ozbay (2011).

We conducted the pilot of the social information and pride treatments on the UMD campus with a sample from the general public at an event known as “Maryland Day”. This took place in Spring 2010. We designed this second set of experiments to explore the role of pride and social information on altruism. The experiment took place outside; subjects used paper and pencil to record their choices, which mimicked the set-up we anticipated in Tanzania. Results and discussion of these treatments appear in Chapter 5.

The final set of experiments with the Tanzanian clinicians occurred July, 2010 in Arusha, Tanzania. This implementation included 2 of the 6 treatments on risk and altruism that we piloted with the UMD students. It also included the 2 treatments on pride and social information that we piloted at Maryland Day. Lastly, it included a context free, standard dictator game as a baseline treatment. We conducted these experiments on the campus of the Center for Educational Development in health Arusha (CEDHA) in Arusha. We designed the treatments so that we could run the experiment without using computers.

The results from these experiments are split into two different chapters. Our report on the risk and altruism treatments appear in Chapter 4, with the results from the corresponding pilot. Our report on the Tanzanian implementation of the pride and social information treatments is included in Chapter 5, with *its* corresponding pilot. Table 1.1. summarizes the different datasets and how we use them in this dissertation. The total number of subjects reported includes decision makers in the experiments (“dictators”) as well as recipients. The primary analyses throughout this dissertation use only the dictator choices, so we report number of dictators in each sample in parentheses below the overall totals for each data source.

Table 1.1. Data sources, corresponding subjects and where it appears in the dissertation

Data collection activity	Subjects of study	Number of subjects	What we measure with the data	Chapter in which the results appear
Laboratory experiments in a computer lab	University of Maryland students	152 (67 dictators)	how risk affects altruism	Chapter 4
“Maryland Day” – a university-wide fair	General public, affiliates of the University of Maryland	52 (26 dictators)	how pride and social distance affect altruism	Chapter 5
Laboratory experiments in a classroom	Tanzanian clinicians	146 (68 dictators)	how risk affects altruism; how pride and social distance affect altruism	Chapter 4 and 5 (results also used to define variables that are used in Chapter 6 and Chapter 7)
Field survey in Tanzanian healthcare facilities, exit interviews with patients	Tanzanian clinicians	4,512 consultations	workplace effort	Chapter 6 and Chapter 7

The field survey we conducted in the outpatient setting in Tanzania. The data collection ran from November 2008 until July 2010. We employed a team of enumerators that consisted of three medical clinicians, six nurses and nine non-nurse enumerators. All enumerators received three days of training on the data collection goals and procedures. Enumerators collected data in teams of two, visiting one facility per day to conduct exit interviews with patients there. Clinician enumerators worked as observers for the data collection visits where we observed clinician subjects directly. We also employed a project manager to coordinate enumerators and obtain consent from clinicians at participating hospitals. We spent a total of 3 months in the field getting permissions from the national and regional Ministry of Health authorities, obtaining consent from hospitals and clinicians, and training enumerators and the project manager. Enumerators conducted 4,512 exit interviews and observed 562 consultations. In this paper we utilize data only from the exit interviews.

Most notably, the laboratory experiments with the Tanzanian clinicians and the field survey include the same subjects. Because these two datasets share a subject pool, we can use results from the lab experiment in our analysis of the field data. Also, the clinician subject pool is unique. Typically laboratory experiments are conducted using samples of university students. Conducting the laboratory experiment with the clinicians allows us to determine the nature and magnitude of clinicians' pro-social behavior. One of the treatments tests how they behave in a basic dictator game; additional treatments show how their pro-social behavior changes from this baseline when we introduce information about other players or an induced feeling of pride. Then, combined with the survey data, we use the results on how pride and social information impact social preferences in the

laboratory to analyze variations in workplace effort. Combining data from the laboratory with data from the clinicians at work provides unique insights into the role of social preferences in clinician effort choices. It also helps us approach the deeper philosophical question about what “laboratory experiments measuring social preferences reveal about the real world (Levitt and List, 2007),” which is increasingly a concern among experimental and non-experimental economists. Together, these two data sources present an exciting opportunity to relate lab results directly to a real world setting.

The remaining chapters of this dissertation explore these issues in depth. Chapter 2 provides a background for this research. We discuss dominant theories of pro-social behavior, theories of social preferences in the work place, the apparent clash between intrinsic and extrinsic incentives and the interplay between social preferences and risk. A review of empirical evidence of pro-social behavior follows, with a section discussing the broader literature on risk and altruism. Chapter 3 provides a more in depth summary of social preferences among clinicians, with a section outlining the healthcare system in Tanzania. Chapters 4 and 5 present results from laboratory experiments. The relationship between altruism and risk occupies Chapter 4. Chapter 5 considers pride and social information as factors determining the expression of social preferences. Chapter 6 presents analysis of the survey data, combined with results from the laboratory. It is in this chapter that we ask whether social preferences help explain variation in clinician effort. A final chapter presents a series of sensitivity checks to the model analyzed in Chapter 6, and addresses the potential for non-random assignment of patients to clinicians in the field (identification strategy). It also describes our estimation of an ability variable use in the main estimations, and attends to plausible inaccuracy of the

effort measure (dependent variable). Overall we report that the results in the main estimation are robust to specification error.

In conclusion, this research combines behavioral economics concepts with development economics to try to better understand effort choices of health workers in a highly resource-constrained setting. In the semi-urban health clinics of Tanzania, it is possible to examine the trade-offs between costly exertion of effort and potential social benefits among clinicians. Two unique data sets make it possible to test the hypotheses that a) clinicians behave pro-socially in simple dictator games, b) social information and induced pride influence giving in the laboratory and c) social preferences as measured in the lab help explain the unaccounted for variance in clinician effort.

Chapter 2 : Altruism, Workplace Motivation, and Risky Giving

Social preferences play an important role in the research on motivation and performance at work. Workers are assumed to make trade-offs between costly effort exertion and the resultant benefits that accrue to themselves or their organization. Major themes in the literature include altruism, gift exchange and reciprocity, crowding out of intrinsic incentives, nonmonetary motivation, and optimal institutional arrangement (given a work force with social preferences). The expression of altruism may also be impacted by the presence of risk (Brock et al., 2010; Tomlinson, 2008). Individuals may have preferences over equity in terms of chance, as opposed to the more commonly assumed preferences over equity in outcomes. Further, how an employee allocates effort to reduce risk involves consideration of both her own risk exposure as well as the risk exposure of others, such as co-workers, employers or consumers. This chapter outlines the currents of this literature on social preferences and workplace behavior, including a focus on the role of risk in determining pro-social behavior.

In the first section I discuss dominant theories of pro-social behavior. The second section focuses on theories of social preferences in the work place, concentrating on the apparent clash between intrinsic and extrinsic incentives and the interplay between social preferences and risk. A review of empirical evidence of pro-social behavior follows, with a section discussing the literature on risk and altruism. A final section concludes.

2.1. Theory

2.1.1. Social preferences

Social preferences in economics can be defined as making economic decisions that benefit another. Charness and Rabin (2002) suggest that a person with social preferences would be characterized as “not maximizing own monetary payoffs when those actions affect others’ payoffs”. They assume people are self-interested and are additionally concerned about the payoff of others. Another term that describes such preferences is “other-regarding”, which refers to both positive and negative dispositions toward the other. This general definition includes positively leaning social preferences, as well as neutral (inequality aversion) and negatively leaning social preferences (negative reciprocity).

A *pro-social* individual can be considered to have positively leaning social preferences, as opposed to neutral or negatively leaning. A pro-social person is in favor of others or society. In general, it is not considered important whether such pro-sociality is independent of social pressure and norms. In some cases, it is assumed to be a natural function of social pressures and norms, where pro-social attitudes come from internalization of norms that occurs overtime. Benabou and Tirole (2006) consider a pro-social person as one who voluntarily engages in an activity that is costly to oneself but benefits others. Altruism is one form of pro-social behavior.

Altruism is the subject of a growing body of theoretical and empirical work. A denotative definition of altruism is “unselfish concern for the welfare of others” (The American

Heritage Dictionary of the English Language, Fourth Edition, 2009). According to Andreoni (1989), “The term was introduced by Barro (1974) and Becker (1974)...”

Becker’s model has been the mainstay of work on altruism; it employs a public goods approach, where the total amount of a social good, Z , enters into an individual’s utility function, $U_i = U_i(x_i, Z)$. Becker’s model assumes that the individual does not get any personal consumption benefit from the good, but rather get utility from the total amount provided (not necessarily by them). Sugden (1982) does not think this model is descriptive of reality, however, pointing out that in reality people may maximize something other than their own utility and still be motivated to act altruistically. For example, he states that “An act utilitarian - or a rule utilitarian, or a Kantian - does not have to experience an altruistic externality in order to conclude that he has a moral obligation to help the poor, the sick and the handicapped.”

In his seminal papers on altruism, Andreoni builds on Sugden’s insight, suggesting that individuals have an impure form of altruism – they like to contribute to the material gain of others because it makes them feel good (Andreoni, 1989; 1990). Andreoni (1989) refers to this as *warm glow*. In his model, individuals may contribute to a public good for two reasons. First, people demand more of the public good. This conforms with the Becker model (1974). Second, people get some private goods benefit from their gift, which Andreoni refers to as a *warm glow*. Because of this second, egoistic motive, his is considered a model of “impure altruism.” He presents preferences that include a combination of both altruism and egoism (the warm glow motivated component, g_i): $U_i = U_i(x_i, Y, g_i)$. The distinction between pure and impure altruism is discussed at length in Rose-Ackerman (1996). The author criticizes the use of these terms, since the pure altruist is in fact someone who

would prefer to free-ride rather than give of himself to help another. Rose-Ackerman discusses briefly some of the different forms of pro-social attitudes, other than pure and impure altruism, remarking that all motives for giving are linked.

Another type of social preferences, linked to the altruism concept, is Rabin's "fairness equilibrium" model (1993). Rabin's theory suggests that individuals have conditional interest for other's welfare. Rabin focuses on reciprocal kindness as the driving factor in determining pro-social behavior. The reciprocity in Rabin's model is unlike Andreoni's impure altruism, where the pro-social behavior is solely a function of the individual. In the fairness equilibrium, players help those who are nice to them and hurt those who are not nice toward them. Each player considers the pay-off of the other in his objective function. Rabin's model additionally accommodates difference aversion or inequality aversion. In fact, Rabin's model pre-dates the popular Fehr-Schmidt model of inequality aversion (Fehr and Schmidt, 1999), though the importance of the interaction to trigger reciprocity is absent in the Fehr-Schmidt work. Thus the Rabin model offers a framework for thinking about interpersonal utility functions, with a focus on reciprocity.

As an implicit answer to such narrow applicability in previous models, Benabou and Tirole (2006) develop an extensive model that accounts for a wider range of pro-social behavior than any other author. As is stated in their abstract, Benabou and Tirole's model envelopes heterogeneity in individual altruism along with concerns for social reputation and self-respect. Like Rabin's model, Benabou and Tirole focus on external motivations for pro-social behavior. They concentrate primarily on how others perceive an individual. Benabou and Tirole do include self-image in their model as part of the reputational pay-

off function. The model thus accounts for an individual wanting to appear pro-social and non-greedy, but does not allow for this desire to change depending on who the other may be. Their theory also includes the idea that individuals employ visible actions to garner the good opinion of others. Despite the wide range of behaviors their model can capture, however, the pride component of Ellingsen and Johannesson's model (discussed below) is not one of them. Regardless, because of its individual heterogeneity and its general form, Benabou and Tirole's work significantly informs a great deal of papers on social preferences.

Building off of Benabou and Tirole's work, Ellingsen and Johannesson (2008) construct a model of interdependent preferences that includes a term to capture the "feeling of being esteemed" by others. The model is one of many that parse pro-social behavior into potential component parts. Examples from empirical papers discussed above include Andreoni's impure altruism (Andreoni, 1989) and Levine's altruism and spite paradigm (Levine, 1998). Ellingsen and Johannesson narrow in on how the individual sees himself. While narrow in its purview, the model's strength lies in its detailed assessment of internal factors potentially behind individual altruism. Namely, Ellingsen and Johannesson focus on an individual's own perception of how others may see him. The authors suggest that this "feeling of being esteemed" by another is equal to the other's *actual* esteem, weighted by how much the individual values the other's esteem. Ellingsen and Johannesson posit that this influence can motivate altruistic acts, in the sense of impure altruism, as well as what looks like altruistic behavior among non-altruistic parties. Their model also allows for pure altruism or pure materialism. Ellingsen and Johannesson's model does not exclude the external motivation, but the external

motivation is relegated to being an indirect influence on behavior, filtered through the individual's own self perception. Whereas it is the own self-perception that gives the person increased utility. Ellingsen and Johannesson' model of interdependent preferences thus illuminates the importance of one's perception of himself and how he thinks others view him in determining other regarding behavior.

Joel Sobel (2005) presents a compendium of work on pro-social behavior in his review of interdependent preferences and reciprocity. Engelmann and Strobel (2004) provide a rigorous comparison of the strength of various models in explaining patterns in the data. Their key result is that while these models of fairness and social preferences are important, they do not supersede more basic models of selfishness and efficiency concerns. This result provides a check on how important fairness concerns are in determining allocations. And while this work focuses on the role of social preferences in clinician decision making, it is rooted in a body of work that evaluates other, more classical factors that may explain the bulk of variation in decision making. Evaluating how social preferences may operate (in the healthcare setting in particular) is in fact most valuable as a complement to existing work on other kinds of incentives.

2.1.2. Social preferences and risk

All of the conceptualizations of social preferences discussed thus far, and indeed the dominant theories on pro-social behavior in the literature, exclude risk. That some subjects display non-selfish behavior, e.g. choose a 50-50 split in dictator games, is the basis for theories on inequality aversion, which are based on utilities over final payoffs (see Bolton and Ockenfels, 2000; Fehr and Schmidt, 1999). But Falk, Fehr and

Fischbacher (2008) show that besides distributional preferences on the fairness of outcomes, the interpretation of fairness intentions plays an important role in subjects' decisions. Thus the process by which a fair outcome is reached is potentially a non-trivial part of final utility. A related strand of the literature considers procedural (or ex ante) fairness: Machina (1989) provides a classical example. Consider a mother with two children. She has a treat that she wants to give to them, but the treat cannot be divided. How will she determine who gets the treat? Although she may be indifferent between allocating the indivisible treat to one child or the other, she may strictly prefer allocating the treat based on the result of a coin toss. The coin toss is a fair procedure, as it gives both children the same chance to win. Nonetheless, it will not result in a fair outcome as only one child can get the treat. Just as in this example of not discriminating between the two children, the ethical debate on outcome versus procedural fairness is usually rooted in normative considerations (e.g. Grant, 1995). One model that attempts to include explicitly what people think they should get appears in Krawczyk (2008), which incorporates preferences over expected payoffs and is an extension to Bolton and Ockenfels (2000). The resulting motivation function includes not only the share of the total endowment, but also the *expected* share of the total *expected* endowment. This nascent body of work thus attempts to extend social preference theories to risky situations.

Brock, Lange and Ozbay (2010) yields further insights into this debate by considering the choices of individuals who are themselves directly affected by the outcome, rather than the beliefs or expectations of the recipients. Norms that might determine a receiver's acceptance probability are eliminated from the story. Rather than deciding the allocation

between two other persons as in Machina's example, the decision maker decides the allocation between herself and one other person. Their model is similar to Trautmann (2009), but with a more general expression of preferences. And unlike the Bolton and Ockenfels models where the expectations of the recipient directly impact actual payoffs (as in the ultimatum game), the Brock et al. and Trautmann models consider situations where recipients do not have direct influence over payoffs (as in the dictator game). Such a model describes situations such as a grandparent choosing to save money for a newborn grandchild or a doctor choosing how much care to give to a patient. These examples are discussed further in chapter three.

2.1.3. Pro-social behavior in the work environment

Pro-social behavior can also occur in the work environment. Employees make decisions about how much costly effort to exert. To the extent that they choose to exert more effort than is minimally required to retain their job, they may be acting out of interest for the welfare of co-workers or of the organization. Much of the literature approaches this issue from the angle of worker motivation. Some workers may be motivated to exert excess effort to earn positive recommendations or build up work history, but these motives will not be discussed here. Instead we review the empirical work on crowding out. Crowding out in this setting refers to the clash between intrinsic and extrinsic motivation; there is evidence that people with intrinsic motivation to provide high levels of effort react negatively to extrinsic (monetary) incentives. We also discuss the importance of institutional structure and institutional culture. Suffice it to say that adherence to job

requirements and organizational goals requires motivation. To that end, pro-social attitudes may explain some workers' motivation in the work environment.

Crowding out occupies a central role in the literature on pro-social behavior in the workplace. Franco, Bennett, and Kanfer (2002) classify motivation into 3 categories: internal, organizational, and cultural. Organizational and cultural motivation comes from outside the individual (extrinsic factors) while internal motivation is about how the individual chooses to act absent external impetus. Kreps (1997) discusses how norms (preexisting intrinsic incentives) interact with economic/extrinsic incentives to determine behavior. He observes the "stylized fact" (though it has been well studied in the psychology literature) that extrinsic motivation (rewards and punishments) may dissuade individuals from working as hard as they otherwise would, absent the external influence (Deci, 1971; 1972; Deci et al., 1975). This is referred to as crowding out of intrinsic motivation. Kreps posits two possible rationalizations for the crowding out: 1) task ambiguity or 2) change of preferences (due to external queues). Sliwka's 2007 work offers an alternative interpretation of behavior that may otherwise look like pro-social attitudes in the work place. His theory suggests that offering a flat wage with little or no control over employee effort levels (i.e. no targets or minimums) will reduce the crowding out more than if one offers an incentive-based wage and exercises more control over effort levels. Complementary to Sliwka's model, Fershtman and Weiss (1993) suggest that employers can compensate workers with status, rather than wage, to activate intrinsic motivation. Other work in this vein includes Canton (2005), Seabright (2004), Francois (2007), and Acemoglu, Kremer and Mian (2008). Conclusions from all of the

theories presented fall along similar lines, thus generating a well vetted economic argument for the motivation crowding out hypothesis.

Institutional structure matters for how workers will respond to employers' attempts to address this dilemma between internal and external motivation. Notably, Francois (2003) asks whether a public or private firm has an advantage in eliciting workers' altruistically motivated contribution to the organization's output. He concludes that a nonprofit organization is better equipped than a for-profit alternative to obtain "care motivated effort", in the form of labor donations, as well as pecuniarily motivated effort. The theory relies on a residual claimant story, where the individual worker in a for-profit enterprise prefers not to donate extra effort in the event that their boss is the one to gain from the work. If, instead, a worthy cause reaps the rewards from extra work, the pro-social worker is more likely to donate additional labor. The result holds under a regime of extrinsic incentives. The crowding out argument thus does not apply when comparing the relative effectiveness of non-profits and for-profits in motivating effort in caring professions.

Another form of pro-social behavior in the workplace that is dependent on institutional characteristics is professionalism; a person who practices professionalism does not reap 100% of the benefits from his behavior. Positive externalities extend to the firm, other co-workers and the customers. Leonard and Masatu (2008) explore the role of professionalism among health workers. They define professionalism as "a set of characteristics describing a member of a profession and the institutions—such as peer influence or organizational identity (Akerlof and Kranton, 2000; 2005)—that encourage

all members to hold to accepted standards.” Along these same lines is the concept of a work ethic. Like professionalism, a strong work ethic generates positive externalities and thus workers have an incentive to ascribe to a weaker work ethic. In Rauh and Ramalingham’s model (2009), the principal, or employer, determines the work ethic for the firm. The employee chooses whether or not to internalize/buy into that work ethic. Basu’s theory of teacher truancy in India (2006) also supports this idea that norms are important for determining behavior in the workplace. In Basu’s work, however, the norms are not set by the employer but are preexisting in society. Akerlof and Kranton’s recent work (2008) explores these ideas more thoroughly with a model of monitoring in effort, where “what matters is...how employees think of themselves in relation to the firm”. Their paradigm is based on the idea that a worker’s identity determines his effort choices, and that supervision can cause worker not to identify with the employer and thus provide less effort. Any of these models, where good behavior in the workplace generates a positive externality, would suggest that an employer or policy maker should address the worker’s group identity, either by determining it or using it as it currently exists. Ultimately, it is agreed that institutional structure matters in activating employee altruism, but ideas as to why that is or how to address it are varied.

Francois and Vlassopoulos (2008) provide an overview of these major themes in the literature. They review papers that look at incentives in the workplace, worker motivation or pro-sociality, and optimal worker-employer contracts. They also focus a great deal on the crowding out of intrinsic motivation by extrinsic motivation. All of this they categorize as action-oriented altruism (a.k.a. impure altruism, the action itself generates utility). Secondly, they consider theoretical papers on worker effort choices relative to

institutional goals and potential free riding problems that can arise. The worker is considered to have preferences over the final output of the institution, rather than his own contribution to it. Francois and Vlassopoulos consider this “output oriented altruism” (a.k.a. pure altruism). In the last section of their review, Francois and Vlassopoulos discuss papers that look at optimal institutional arrangement for provision of public goods. These papers focus on what type of institution best handles the contractibility of quality, effort, and other non-observables. In other words, they ask “When is a non-profit better for provision and when is a for profit better for provision, when individual worker motivation is instrumental in firm output?” The papers reviewed in this last section discuss the pro-sociality of institutions, rather than individuals. Thus, in their review, Francois and Vlassopoulos take stock of the literature on altruism in the workplace, both in terms of individual decision making and in terms of optimal institutional arrangement.

The theories presented here extend pro-social behavior to the work environment. Putting in more than minimal required effort is one way to conceptualize work-related social preferences. But sources of worker motivation are likely varied and workers’ social preferences may cause them to put in *less* effort at work than is required (or optimal). Institutional structure emerges as central to theorizing about worker social preferences. Thus far none of the theory considers the role of risk in social preferences in the workplace. Nonetheless, pro-sociality remains an integral factor in the assorted theories on motivation for determining workplace effort.

2.2. Evidence

Just as theories of pro-social behavior are varied, so follows the evidence. Much of the empirical work on social preferences has occurred in an experimental economics lab. While how to interpret the source of pro-social behavior remains a challenge, the presence of pro-social behavior has been documented in countless lab experiments. Andreoni (1995) conducted lab experiments to explore the idea of impure altruism. He found that people indeed contribute to public goods out of *some form* of kindness. As a complement to Andreoni's work, Goeree, Holt and Laury (2002) found evidence of impure altruism, where giving to a public good increased as the group-level benefit of contributing increased. Evidence of pro-social behavior from simple games such as the dictator game, the ultimatum game, the trust game, and public goods games are overwhelming; they show that the expected outcomes rooted in self-interest do not dominate (Andreoni and Miller, 2002; Forsythe et al., 1994; Palfrey and Prisbrey, 1996; 1997). And while many authors have studied gift-exchange in the work place as a way to explain how workers decide on effort levels (Akerlof, 1982; Gneezy and Rustichini, 2000; Gneezy and List, 2006; Rigdon, 2002), few have considered the impact of pro-social behavior on these effort decisions outside of the gift exchange context (see Charness, 2004). Similarly, there is a limited literature about the impact of risk on altruism. Bolton, Brandts and Ockenfels (2005) provided a seminal paper on the issue, considering whether individuals focus on ex post or ex ante fairness. Work by Bohnet and Zeckhauser (2004) and Bohnet, Hausmann and Zeckhauser(2008) further consider

the preferences of the a recipient in ultimatum game decisions under risk. In general, procedural fairness is an important factor in individuals' perception of overall fairness and affects willingness to trust. Brock et al. (2010) looks into how risk impacts preferences for fairness among givers. Their findings echo others' results, and also bring to light a new result, that giving in a standard dictator game context is highly predictive of giving under risk. Net, this evidence from relatively simple games forms the foundation for a more detailed look into pro-social attitudes among clinicians.

2.2.1. Social preferences and risk

There has been limited work on the interplay between altruistic behavior and risk. Studies focus on both recipient response to dictator allocations in the presence of risk, as well as the dictator behavior itself. Overall, recipients tend to be more forgiving of unfair outcomes if they originate from an ex ante fair allocation of chances. Dictators do appear to adjust giving patterns when risk is involved, but this response depends crucially on the choice options available to them. In the health setting, Arana and Leon (2002) find evidence of willingness to pay for health risk reduction to others. In the medical profession specifically there is a sense that providers are duty-bound to assume additional risk to themselves in order to reduce the risk of a poor health outcome for their patients (Tomlinson, 2008). In what follows we summarize the empirical work on risk and social preferences. In chapter three we present results from a set of laboratory experiments in this direction done with clinician subjects, the results of which also appear in Brock, Lange and Ozbay (2010).

The empirical work studying risk and social preferences is dominated by laboratory-generated evidence. One of the foundational papers on the subject, Bolton, Brandts and Ockenfels (2005), considers whether individuals focus on ex post or ex ante fairness when making their allocation choices. These authors use ultimatum and battle-of-the-sexes games to look at the trade-off between how an outcome is determined and the fairness of the outcome from recipients' perspective. Their results reveal a complex story for recipients' preferences over ex ante and ex post fairness: an unfair outcome may be acceptable given a fair procedure but a fair outcome is preferred regardless of procedure. Bolton et al. does not, however, identify if the recipient preferences for a fair procedure stem from the human interaction or from a blanket preference for ex ante fairness. Investigating such a dynamic directly, Bohnet and Zeckhauser (2004) and Bohnet et al. (Bohnet et al., 2008) analyze how recipients in risky dictator and trust games adjust acceptance rates depending on whether an actual person or a random process determines the outcome of the game. Results point to biases against human partners, which the authors interpret as the result of minimizing "betrayal costs". And while none of these authors consider how giving decisions are directly affected by risk, their results do suggest the presence of norms that might influence giver behavior.

Echoing and building on these results, Kircher, Ludwig and Sandroni (2009) compare whether the type of good being allocated (a private good versus a good with some social value) influences individuals' allocation choices. This extension is directly applicable to the healthcare setting, where health may be considered a good with social value. The authors allow subjects to choose the allocation mechanism: a subject can choose between the more selfish and less selfish allocations themselves or they can let a coin flip

determine the final allocation. They find that while some subjects do prefer to choose the allocation themselves, approximately 30% of subjects choose the coin flip when the good in question has some social value. Those with ambivalence between social preferences and selfish preferences prefer to switch responsibility to nature. In this way they retain the opportunity to achieve the selfish allocation, but they will not be considered selfish if that is the outcome (also referred to as “moral wiggle room” in Dana et al. (2007)). Hence the authors provide evidence of preference for procedural fairness. In using discrete allocation choices, however, the authors cannot speak to the magnitude of this preference or determine the point at which individuals who prefer to choose the allocation themselves might switch to preferences for procedural fairness. Nonetheless, the paper firmly establishes procedural fairness as a meaningful empirical concept among decision makers for socially valued goods. Generalizing this concept, Bolton and Ockenfels (2010) explore how dictator choices between a safe and a risky option for themselves depend on the corresponding payoffs to the recipient. In their experiments, dictators have a binary choice between a safe payout option and a risky payout option. They do not vary the degree of risk in the risky options. They find that dictators tend to be more risk averse when the risk applies to themselves as well as to others. They also find that dictators prefer the risky situation over a situation where outcomes are unfair with certainty. While this study reveals that decision makers are sensitive to risk borne by recipients, it falls short of addressing the degree to which dictators are willing to surrender their own sure gains in order to reduce the risk of a partner. Thus we know that procedural fairness is important to decision makers for both socially valued goods and more generally as well.

Finally, Brock, Lange and Ozbay (2010), included as Chapter 4 of this paper, fill a gap in this literature by considering how dictators' preferences for procedural fairness stack up against preferences for ex post fairness. They give decision makers a continuous choice set and vary the distribution of risky versus certain outcomes for the dictator and the recipient, respectively. The between design allows the authors to determine how much decision makers are willing to pay in order to shield either themselves or their partner from risk or an unfair outcome. In particular, their design compares generosity in terms of monetary outcomes with generosity in terms of chances at increased monetary outcomes. They find that altruism in the absence of risk is a reliable predictor of altruism over chances; dictators tend to give the same amount away when the units are dollars as when the units are lottery tickets. That dictators' generosity is not substantively affected when allocating chances suggests a preference for ex ante fairness – dictators are not giving less when giving chances, even though outcomes maybe very unequal. The results are in line with Kircher, Ludwig and Sandroni (2009) and Bolton and Ockenfels (2010). The novelty of the Brock et al. results lies in the continuity of the dictators' choice and the ability to measure the degree of tradeoff decision makers choose between their own risk reduction and that of their partner. Thus Brock et al. contribute to the literature of risk and altruism and further confirms that considerations for ex ante fairness is important in evaluating decision making under risk.

2.2.2. Pro-social behavior in the work environment

Finally, we discuss the empirical evidence on pro-social behavior in the workplace. When considering how to incentivize employees to work harder, the natural assumption is that

increasing the monetary compensation from working will induce higher levels of effort and thus better quality (Akerlof, 1982). But the strictly positive correlation between monetary compensation and worker effort is up to debate (Deci, 1971; Mas, 2006). In addition, it is not clear what kind of scheme might be appropriate to motivate increases in effort given the potential conflicts between intrinsic and extrinsic motivations (Deci, 1972; Kreps, 1997). Gneezy and Rustichini (2000) conducted experiments with treatments of either getting paid to do a specific task or not paid to do the task. Their results confirm that higher compensation can induce higher effort, if compensation was offered at the outset. But they also found that when moving from a no compensation regime to a compensation regime, performance is lower. Moreover, in an investigation of taxi drivers in New York, Farber (2008) found that many set target income levels, taking leisure after reaching their goal for the day, and forgoing income if the goal was reached before normal quitting hours. Other work looks at the issue in terms of the workers response to employer prompting. Gächter and Falk (2000) looks at overcoming inefficiencies from incomplete contracting using reciprocity, repeated game effects (reputation), social embeddedness, and incentives. Social embeddedness is simulated with face-to-face repeated partner interactions. They find that social embeddedness does not change the behavior relative to an anonymous partner treatment (playing with the same person over and over in long term contracts experiments). This should not be a surprise. The value of social embeddedness as a contract enforcement device is in non-repeated games, where removing anonymity creates a superficial but influential connection between the parties (Bohnet and Frey, 1999; Eckel and Grossman, 1996). This superficial connection is unnecessary for repeated interactions where strategic behavior can create

the desired contract enforcement, as is the case in long term contracting. As is predicted in Sliwka (2007), Gächter and Falk also find that trust performs better as a contract enforcement device than monetary incentives. Thus, when workers have social preferences, it is clear that strictly increasing monetary incentives may not be the most effective in motivating workers to provide more effort.

The counterintuitive results that more money is not always effective in motivating workers suggests that social preferences play a potentially important role in workers effort and job choices. There are a few studies that look at intrinsic motivation directly, rather than considering the interaction between intrinsic and extrinsic incentives. Rotolo and Wilson (2006) finds evidence of “higher civic mindedness” of nonprofit employees. They use people’s likelihood to volunteer outside of the workplace as the measure of civic mindedness and find a positive correlation between that and job type. Similarly, Gregg et al. (Gregg et al., 2011) compares donated labor, measured as unpaid overtime, in “caring industries”, between public (nonprofit) and private (for profit) organizations. Those with more unpaid overtime are considered to have “high public service motivation”. They find that in the caring industries those with high public service motivation are more likely to choose a job in the public sector. However, they also generate evidence that people who switch between the private and public sectors do not change their propensity to put in the extra hours after switching. Such evidence points to the possibility that social preferences are more individually driven than institutionally driven, and does not detract from the fact that social preferences are active in workplace decision making in certain industries.

2.2.3. Summary

This concludes the discussion of the empirical work that has been done on social preferences in the workplace and generosity in the presence of risk. Ultimately there is extensive evidence of social preferences, from impure altruism to fairness concerns to expectations of reciprocity. Currently, laboratory experimentation on the role of social preferences in workplace-like effort decisions is limited to a gift exchange context. Outside of the laboratory, there is evidence of pro-sociality in workplace effort choices. In these cases institutional context appears to play a role. No one to our knowledge has looked at how risk may factor into effort choices. In fact the body of empirical literature on risk and social preferences is still somewhat new and many rudimentary questions remain up to debate. For example, the literature is dominated by experimental work. Further, while all authors find that risk is a meaningful dimension to investigate, there is no consensus on which results best summarize the role of risk in the expression of social preferences. Taken together, the evidence synthesized here is the basis for the empirical aspects of the succeeding chapters.

2.3. *Conclusion*

In the first section of this chapter we discussed the dominant theories of pro-social behavior, particularly as it applies to the workplace setting. It was followed by a review of empirical evidence of pro-social behavior. The theories are as diverse as the evidence, but together tell a story about how pro-social behavior may impact economic decision making. What is clear is that people faced with economic decisions do have the inclination to act as if they care about the welfare of others. The other may be a partner in

a laboratory, a charity, an employer or even an institution (broadly speaking). What is less clear is *why* individuals may make less than completely selfish decisions. Theories point to fairness norms, warm glow, reciprocity and intrinsic motivation, to name a few. The empirical evidence on pro-social behavior in the workplace is limited but varied. Most of the data has been analyzed by looking at the interaction between intrinsic and extrinsic incentives. Pro-social behavior is observed in effort choices of “employees” in laboratory and in field experiments as a kind of gift-exchange scenario with an employer (as opposed to the more simple structure of doing work and getting paid for what one does). Pro-social behavior also stands in the presence of risk, but the story becomes more complex and the structure of the risk appears to matter a great deal. As a case in point, how an employee allocates effort to reduce risk implicates not only her own payoffs but also that of her employer, the company and any consumer of the product or service the company provides. Though considering pro-sociality in the presence of risk workplace may be especially important because of the economic implications, it is a venue that has yet to be explored. As it stands, we know that social preferences play an important role in workers’ effort choices. In the empirical studies that follow we delve into the topics reviewed in this chapter, specifically with respect to clinician effort choices in the Tanzanian healthcare system. The chapter that follows describes this system and develops motivation for studying these issues with respect to healthcare workers.

Chapter 3 : Healthcare and Health Workers in Tanzania

3.1. Introduction

In this chapter, we explain the application of research on social preferences in the workplace to Tanzanian healthcare. Healthcare workers provide an interesting application for studying social preferences in the workplace because they are often perceived to be socially minded. Healthcare is by name about providing care for another person. As it goes with teachers or daycare providers, clinicians are expected to make effort choices that are in the best interest of others. Such “caring industries” provide settings wherein workers’ social preferences may easily be expressed as some form of altruism. For example, a clinician may choose to provide effort above and beyond what is required in response to a particularly sick patient. The healthcare industry also may compel workers with social preferences a la Akerlof and Kranton (2005; 2008) to act in the interests of their institution. Health workers are couched in a larger institutional context that itself embodies a specific set of priorities. For example, public health workers must make choices for their patient in addition to paying mind to facility, state, and national goals for the health of the population. Private facility clinicians are not exempt from the larger institutional context – they too must learn specific protocol in school, obtain the required degree and practice according to nationally set guidelines. Thus the health worker faces various demands on her time and energy that come from her patients’ needs, facility level priorities, state agendas and national goals. Finally and perhaps above all, the health worker is likely inclined to pursue her own self-interest. Balancing self-interest, patient

interests and institutional interests thus provide ample opportunities for health workers social preferences to be translated into pro-social behavior. In turn, the setting is ripe with opportunities to study the role of social preferences in the work setting.

The next section in this chapter discusses the specific type of social preferences we investigate in this report. It is followed by a review of the theoretical literature, albeit sparse, on altruism among health workers. After that we include a discussion of empirical evidence on health worker behavior in developing countries, including an extensive section on how effort and quality are measured in the healthcare setting. A description of the Tanzanian healthcare system concludes.

3.2. Pride, social identity and risk in health worker decision making

In this report we look specifically at the expression of health worker altruism with respect to patients. We develop our research in light of the results that identity matters in economic transactions (Becker, 1971; Bohnet and Zeckhauser, 2004; Bohnet and Frey, 1999). Our research also draws heavily on Ellingsen and Johannesson's theory of esteem-based altruism (2008). In their theory, identity and pride work together to generate a perception of being esteemed. The decision maker accrues benefits in the warm glow sense, but rather than simply being pleased with her own action, she also gets utility from her perception of the recipient's good opinion of her. Namely, she feels pride at her altruistic actions. The strength of that pride depends on how much she values esteem from the other. A more extensive discussion of this theory appears in Chapter 5.

It is perhaps easy to see how this dynamic would exist between a clinician and her peer (or superior). Results from Leonard and Masatu (2008) suggest that this in fact may be

the case in Tanzania. But to what extent does patient identity matter for clinician effort decisions? How might patient characteristics and his or her esteem interact with clinician effort levels? These are empirical questions that we begin to address in this report. The answer depends on how the clinician and the patient evaluate each other's "value" based on observable characteristics. For example, a patient's speech may betray a low level of education, his dress a low level of income. The clinician may respond to that by putting in less effort, assuming that it takes less to impress this person. Alternatively, the clinician may exert little effort because she is less interested in impressing this person. In both circumstances, the clinician's action reveals that that patient's regard has less social value to the clinician. The opposite may be true if the poorly educated low-income patient represents the population in the area and the clinician wants to expand his patient base. The clinicians also may respond to this kind of patient if the clinician has a personal agenda that may be considered "pro-poor". Even in the latter case, however, the clinician may adjust her effort level according to how much she values an individual's esteem and how easy or difficult it is to gain that esteem. Note that this behavior is distinct from discrimination because the clinician here is not biased or prejudiced against the patient in and of himself, but rather tailors her effort according to the social returns she gets from impressing that patient. These are the dynamics we consider in our exploration of health worker altruism with respect to patients.

Finally, we include an extensive empirical study on social preferences and risk. Risk is a salient feature of any health care system and almost certainly plays into health workers' effort allocation choices. Health workers are not only called to mitigate the risk of illness on their patients' behalf, but also must consider the risk to themselves. In some cases,

being a health worker carries considerable risks. The risk may be to her own health -- a patient might come in with a highly contagious life threatening disease, such as tuberculosis. It may also be with respect to error; making an error can expose the health workers to blame, loss of reputation or a lawsuit. In any case, health workers face risks of daily. We suggest that the structure of the risk impacts the expression of any social preference they may have. If a clinician is particularly altruistic, but also quite risk averse, her behavior will reflect that. Faced with the risk of contracting a severe illness, she might behave less generously than in a situation that carries less risk to herself. Alternately, a generous clinician may exert more effort than usual in order to address a very ill patient's chance of getting well, thus reducing the chance of a bad outcome. Since the impact of risk on pro-social behavior is a relatively new avenue of research, we do not attempt to address many of these issues in this dissertation. Rather we tackle some more basic issues that we may then build upon moving forward. The description here serves to motivate those investigations in the context of health care and place them into the overall theme of the work.

3.3. Health workers' social preferences, theory

The theory presented in the preceding chapter raises a number of questions about role of pro-social motivation in the provision of health care. To what extent are health workers motivated by social preferences? What is the role of pride? How does the riskiness of the healthcare setting interact with clinician generosity? These questions are not well explored for health workers, though authors have studied the general relationship between intrinsic and extrinsic motivation, as well as issues surrounding preferences for general public wellness.

A small body of literature exists on how social incentives may (or may not) motivate healthcare providers' effort choices at work. In his theory, Prendergast (2007) finds that when pro-social attitudes, or "public-spirited" attitudes, are not observable people do not necessarily self-select into the appropriate jobs (i.e. non-prosocial people do not necessarily sort themselves out of public service jobs). Delfgaauw (2007) applies a derivative of this question to a model of health worker job choice, with two types of doctors (purely altruistic and purely selfish) and 2 sectors (private and public). Doctors can chose between sectors. He finds physicians with higher intrinsic motivation to improve patient wellbeing are more likely to choose a job working in the public sector. These pro-social physicians are also the ones providing superior quality in that sector. The theory suggests that these physicians get utility out of the fact that they can contribute to the patient's welfare more than can other doctors of lower quality in the same sector. Finally, Dolea and Adams (2005) provide a review of some literature on the motivation of health care workers from the perspective of "needs theories" and "process theories". The authors state that neither theory holds true empirically, but that managers continue to use approaches informed by these theories. Since the theories appear to be empirically defunct, however, they are not covered in this review. Thus we see that social preferences do appear to matter, but the theory specific to this area is fairly sparse. Accordingly, in this work we will rely most heavily on the more general theories of social preferences.

3.4. Measuring healthcare quality in developing countries

There is a large body of empirical work on evaluating clinician performance in both the US and abroad. This section discusses evaluating clinician performance, particularly in developing countries. First and foremost we touch on how to accurately measure health provider quality. In more developed countries, researchers typically use randomized controlled trials to study clinician behavior with respect to specific procedures. Institutional details have allowed health economists studying less developed countries to obtain more detailed measures of clinicians' actual behavior, as opposed to only observing the outcomes of the behavior. The results from the latter set of studies paint a picture about the sources of variation between physicians, which we briefly summarize. We discuss one particular paper in detail that focuses on strategic effort choices, as it also considers the question of whether individual identity, beyond case mix, helps to explain variation in quality of care. Overall, we use the work in this area to inform our own measures of process quality and effort, which are covered in depth in the chapters that follow. We also point out that while clinician behavior and quality of care are well studied, there remains ignorance as to what explains variation in care and how to get under-performing physicians to work harder. We believe that capitalizing on health worker social preferences is a meaningful path to this end and that studying the role of social preferences in determining individual effort a key tool.

Authors studying quality in more developed countries typically study doctor effort by looking at clinician performance over a defined set of tasks and have not, to our knowledge, explored the behavioral constructs behind their results. Patient chart review is probably the most common tool used to measure quality of care in the United States. It

offers the most potential for getting an overall measure of quality. Other data sources include national databases on health outcomes. These sources have serious limits, however, in that they do not allow for analysis of actual clinician behavior. Randomized controlled intervention studies are a useful alternative for looking at clinician actions. They are usually employed with an aim to determine which means (or interventions) may be effective for improving adherence to specific sets of protocol (Boekeloo et al., 1990; Fairbrother et al., 1999; Tierney et al., 1986). But few of these studies use economic theory to inform the interventions (see Fairbrother et al., 1999 for an example). And while some may hint at the behavioral underpinnings of their interventions (Soumerai and Avorn, 1990) these issues have not been studied empirically. Thus, while randomized controlled trials are powerful tools for evaluating clinician behavior, and widely used in more developed countries, they do not allow for study of more general quality of care and none to date have been designed to study behavioral underpinnings of health worker motivation.

Authors working on evaluating and/or improving provider quality in developing countries have made inroads into measuring individual clinician quality overall, otherwise known as process quality. Studies of process quality have been carried out in Indonesia (Santoso et al., 1996), Paraguay (Das and Sohnesen, 2007), India (Das and Hammer, 2005; 2007), and Tanzania (Leonard and Masatu, 2005; Leonard et al., 2007; Leonard, 2008; 2006). The consensus from these studies is that quality is poor in developing countries not only because of structural shortcomings, but also because clinician competence is low. Using educational interventions combined with chart review of physician prescribing practices, Santoso (1996) shows that healthcare providers in Indonesia are under-educated on the

appropriate use of drugs for acute diarrhea, a leading cause of child morbidity and mortality. Das and Hammer (2005) determine that there are overall low levels of competence in their study region in India and that clinicians who are highly competent relative to the sample merely have “the ability to identify life-threatening conditions and act accordingly”. In this study and others Das and Hammer utilize medical vignettes and direct observation. Vignettes are test-like consultations, designed by researchers, where the patient is not a sick person, but rather someone who has been trained on how to respond to clinicians’ questions according to the case mix he is supposed to represent. An observer scores physicians according to how well they treat this imaginary patient. Direct observation entails the presence of a non-patient observer in the consultation room. Also using vignettes and direct observation, Leonard and Masatu (2005) find that despite low levels of competence, practice quality is still lower than it could be; clinicians in Tanzania have the capacity to improve their quality even without additional training. In later work Leonard and Masatu (Leonard, 2008; Leonard and Masatu, 2006) also use patient exit interviews to collect data on physician behavior. Exit interviews complement the vignettes and/or direct observation with data on clinician effort while no one is observing. These methods allow the authors to look at within clinician variation, an approach that is well suited to our goals for studying the role of social preferences in clinician effort choices. With these various tools, researchers studying health provider quality in developing countries have thus been able to garner more detail on quality of health care than is typically available in developed countries.

Previous work has identified important components of variation in quality of care in developing countries, but social preferences have received little attention as a factor in

clinician effort choice. Using vignettes and direct observation, researchers have studied the variation in quality by cadre, type of organization, and tenure. Das and Hammer (2005) find that the differences in competence between doctors in India are largely explained by training. In contrast, they also find that work experience in a neighborhood has little impact on competence. Training also plays an important role in determining differences in the gap between knowledge and actual practice among Tanzanian clinicians (Leonard et al., 2007). But, while non-trivial, training difference between clinicians does not tell the entire story. Besides training, the study highlights the importance of the type of organization a clinician works for (public, NGO, private). In their sample, organization type accounts for 50% of the variation in the dependent variable. However, the authors are limited by their sample size and are forced to run a number of reduced form regressions, making the meaning of these results unclear. Getting closer to looking at the role of social influences, Das and Sohnesen (2007) consider the possibility that clinicians in Paraguay make strategic effort choices. Strategic effort choices would imply that their motivation is intrinsic and tied to the characteristics of the patients they serve. They analyze clinicians' behavior toward patients relative to clinician and patient characteristics. For patients, they focused particularly on wealth levels, hypothesizing that clinicians would discriminate against patients based on income. Primary results suggest little difference in doctor effort across different patient backgrounds and large difference across physicians and facilities along the lines of doctor gender, contract type, facility type, and doctor salary. Importantly, in their sample, clinicians did not vary effort according to the income level of their patients. The authors conclude that clinicians do not discriminate against poor patients, but it is not clear

whether the authors are capturing social preferences with this result. In sum, while understanding the factors influencing quality of care occupies a prominent place in health care research in developing countries, social preferences remain under-explored.

3.5. Healthcare in Tanzania

Our work takes place in the region of Arusha, a semi-urban area of northeastern Tanzania. Healthcare in Arusha can be thought of in terms of the types of facilities, the services offered (e.g. clinician credentials as well as laboratory equipment) at the facilities and the facility ownership. Another salient feature is how the patients access the care. We first discuss the role the patient fills and then talk in more detail about clinician and facility characteristics.

A defining feature in the Tanzanian healthcare system, from a patient's perspective, is the system of queuing. Unlike many systems in the developed world, a Tanzanian patient does not make an appointment to see a clinician. Rather once they arrive at a facility they register with a nurse and the nurse directs them to queue up with any of the general practitioners working that day; assignment is essentially random. The exception to the more or less random assignment is when a clinician offers specialty services, such as consultation and testing for AIDS, TB or diabetes. The system is identical across private, public and NGO facilities and is parallel to the urgent care or walk-in clinic arrangements present in many more developed countries. We present tests of this more or less random assignment of patients to clinicians in Chapter 7.

Assignment of patients to doctors within each facility is as follows. For outpatient services, patients queue up at the facility reception. Reception nurses take the patient

name and any insurance information and assign them to one of the doctors in the facility taking outpatient clients that day. For example, one facility operates on a “take a number” system. When a patient’s number is next in the queue the patient gets his or her appointment. Other facilities depend on patients to keep track of their own place in the line. Many smaller facilities keep medical records for their patients, which a nurse will pull out and either hand to the patient to bring in to the consultation or give directly to the clinician, in which case the doctor collects patient medical records as the nurses bring them. In some cases the nurses keep an eye out for who is next, but this is not standard (perhaps not even formalized) or reliable. Specialty clinics are offered at some facilities and nurses can direct patients accordingly, but these are operated separately from the ordinary outpatient system and are not included in our dataset. Thus, when a patient visits a healthcare facility seeking outpatient care, they do not know which doctor they will see. This is especially true for public facilities where clinicians have irregular schedules and are moved (by the Ministry of Health) between facilities frequently.

Waits can be substantial. A patient may wait in line from 10 minutes to 2 hours. The number of people waiting when one queues up is not necessarily an indication of how long the wait is. Two 2 patients in line may still mean a 40 minute wait. Also, if the facility offers more than only outpatient services, the clinician may be called away to deal with an emergency. In that case his office remains empty and patients continue to wait for him to return and resume with the outpatient visits. Presumably due to the long waits patients almost invariably arrive to queue up before the clinicians themselves arrive. For their part, most clinicians arrive as is convenient, though they do have set schedules. Some arrive on time reliably and others interpret a 9:30am start time as somewhere

between 9:30am and 11am. While there is not data on clinician tardiness, we estimate that on average clinicians arrive no more than 30-40 minutes late.

A consultation with a clinician can last from 5 to 20 minutes. Typically they are on the shorter side. The clinician performs some diagnostic tasks, some history taking tasks and may give a prescription, order a lab, or ask the patient to return a second time. If a laboratory test is ordered, the patient is responsible for returning to the clinician with the laboratory results, which may happen the same day or on a return visit. Sometimes the patient can get an appointment if asked to return. In that case, the patient would arrive for the appointment and be first in line, ahead of whoever was already there queuing. If the clinician is away from his office for emergency, a patient with an appointment waits for him to return with everyone else. These visits to health facilities often take the better part of the day, preventing people from working those days. A return visit often means another day of worked missed.

Referrals are uncommon in the outpatient setting. A referral eliminates the random assignment in terms of which doctor a patient sees, but the patient still must queue up on the day they visit the facility or doctor to which they have been referred. In rare cases a clinician may self-refer, asking the patient to return to him for a follow-up consultation. In the event that the two parties do schedule an appointment for the follow-up, it typically means that the patient gets to wait near the head of the queue when they arrive, similar to the system in place in the United States. An appointment does not mean a patient gets seen immediately and the returning patient may wait for hours before being seen.

Clinicians schedules are determined 1 to 2 weeks in advance, and are frequently posted even later, though informal understandings underlie most scheduling and clinicians typically have a good idea of when they will be expected to work. In the more organized facilities there are predictable patterns of work, though such patterns rarely follow the days of the week. One such pattern is to work 3 days in the AM shift, 3 days in the PM shift, and then 2 days off. Clinicians are notorious for arriving late to their shift and taking long unscheduled lunch breaks. For example, when conducting consent visits we took down schedule information for the next week and planned to visit any given clinician within their working hours. Upon arrival at a facility to collect data, we often found that the clinician of interest had not arrived as scheduled and no one was able to tell us where he was or when he would return. With the exception of a few clinicians who seem to always be at their post, this lack of reliability holds for private, public and NGO institutions alike.

Healthcare staff in the outpatient setting consists of clinicians and nurses. Clinicians provide the primary diagnostic care; they fill the role of “doctor”, though the majority of them do not have full medical degrees. The four cadres of clinicians include: assistant clinical officer (ACO), clinical officer (CO), assistant medical officer (AMO), and medical officer (MO). Each of these titles requires a specific degree. The medical training required for each depends on the degrees an individual already has. Typically, with no other degrees and 4 years of secondary school, it requires 3 years of training to become a CO. ACOs have less training. AMOs have on average 3.5 years of schooling, though again this depends on whether they already have their CO. MOs have the equivalent of a United States MD degree.

Facilities vary in size and ownership. The smallest facilities have a single clinician and perhaps one part-time nurse. Larger facilities have staffing structures similar to hospitals in the United States, with multiple clinicians and nurses providing both inpatient and outpatient care.¹ Larger facilities also have a greater variety of services, which may include maternity and prenatal care, specialty clinics and ophthalmology. It is not uncommon to find on-site laboratories to test samples taken from patients in large and small facilities alike. The other defining characteristic of facilities, ownership, is perhaps the most germane to this work. Facilities may be publically owned, owned by a non-governmental organization or privately owned. Quality of care is typically highest at private facilities, closely followed by NGOs. Posts at private facilities are coveted positions. Many clinicians work their first few years at public facilities before securing a place in one of the privately owned or NGO institutions. Insurance plays a very small role in healthcare in Arusha, as the vast majority of patients do not carry insurance. As such, patients can choose to seek care at whichever facility meets their needs, in terms of services offered, quality desired, and prices for consultations. For a more an extended description of healthcare system in Tanzania, as well as a model describing health care provision, see Leonard et al. (forthcoming).

¹ In US hospitals outpatient services are restricted to emergency rooms. In Tanzania the outpatient care at hospitals ranges from mild illnesses to urgent care to emergency care. Smaller facilities do not handle emergencies.

Chapter 4 : Risk and Altruism²

4.1. Introduction

In many real life settings, actions taken by some persons alter the risks of others. Examples are widespread: climate policy involves (sure) abatement costs for the current generation while future benefits are uncertain, depending on the sensitivity of the climate to the atmospheric stock of greenhouse gases; parents have safe and risky options to invest or save for their children; donors to charities might not perfectly know the success of their investments. More germane to the applications in this dissertation is the example that physicians must choose how to allocate (costly) effort in order to increase the patients' *chances* to be healed. Putting forth full effort may increase chances for a positive health outcome substantively, but a clinician cannot fully expend herself for every patient every day. Common to all these examples is that a decision maker foregoes some benefits in order to increase payoff chances of others, rather than transferring income or benefits for sure. By studying giving decisions in risky environments, we address the question of whether individual perceptions of fairness relate to comparisons of outcomes or rather to comparisons of opportunities, i.e. to the procedure that determines the outcomes.

In this chapter we present results from laboratory experiments that study how the riskiness of such transfers affects decisions. The experiments are modifications on a

² This chapter is an augmented version of Brock, Lange and Ozbay (2010). Much of the motivation and literature review is the same, but Brock et al. only include results from the laboratory experiments done with the University of Maryland students, referred to here as “the pilot” implementation.

standard dictator game that capture different variants of risky transfers. The treatments were piloted with a sample of University of Maryland students (on the UMD campus) and then implemented with a sample of Tanzanian clinicians (in Arusha, Tanzania). The goal of the pilot was twofold: 1) to take a first step in the empirical research on how risk effects giving decisions in dictator games and 2) to isolate which treatments best parallel the clinician-patient relationship while also eliciting meaningful responses from subjects in the laboratory. We learned that while all treatments help to elucidate the impact of risk on altruism, two in particular were most appropriate for implanting with the Tanzanian clinicians. Thus the pilot allowed us to select the two treatments most appropriate to the Tanzanian setting and most germane to our research questions with respect to the Tanzanian clinicians. Results from both the pilot and the Tanzanian implementation are reported and discussed in this chapter.

With this, we contribute to a large experimental and behavioral literature that investigates potential social behavior of subjects. Dictator, gift exchange, public good and other games show that some subjects are willing to transfer money to other players without having any material benefits as a result of doing so (see Camerer, 2003). Such giving decisions are often interpreted as a preference for equitable or efficient outcomes (Charness and Rabin, 2002; Engelmann and Strobel, 2004; Fehr and Schmidt, 1999), as a preference for giving (Andreoni, 1990), or as a desire for being seen as behaving fairly (Andreoni and Bernheim, 2009; Benabou and Tirole, 2006; Dana et al., 2007). Little thought has been given so far to the role of risk in giving decisions or to if and how such social preferences extend to environments of risky decision making.

Whether or not social preferences extend to risky environments can be described with a theory on ex ante and ex post fairness considerations. We outline the theory below. The experiments are then based on this theory. The theory proposes that individuals consider ex ante (or procedural) fairness in risky decision making in addition to or instead of exclusively considering ex post fairness. Recall the example of Machina (1989) mentioned in the first chapter as a reference for how to think about ex ante fairness. A mother with two children may be indifferent between allocating the indivisible treat to either of her children, but she may strictly prefer giving the treat based on a result of a coin toss. Although being a fair procedure, as it gives both kids the same chance to win, it will not result in a fair outcome as only one child can get the treat. In this paper, we yield new insights into this debate by considering the choices of individuals who are themselves directly affected by the outcome. That is, rather than deciding the allocation between two other persons as in Machina's example, the decision maker decides the allocation between herself and one other person. Investigating how risk impacts the self-interest and altruism in this way allows us to discuss how social preference theories may extend to risky situations.

This work is also related to a body of recent work that examines the role of social preferences for risk-taking. Similar to Bolton, Brandts and Ockenfels (2005), we consider whether dictators focus on ex post or ex ante fairness. These authors use ultimatum and battle-of-the-sexes games to look at the trade-off between how an outcome is determined and the fairness of the outcome from recipients' perspective. Unlike their work, however, we study the dictator's allocation choice rather than recipient preferences. Our work is also informed by Bohnet and Zeckhauser (2004) and Bohnet et al. (2008). Their studies

analyze how recipients in a risky dictator game adjust acceptance rates depending on whether an actual person or a random process determines the outcome of the game. But they also do not consider how giving decisions are directly affected by risk. We use variations on ordinary dictator games and instead of looking at recipient preferences, we consider dictator behavior. Thus in our setting the recipient is a completely passive player. In that sense our work builds on Bolton and Ockenfels (2010) who explore how dictator choices between a safe and a risky option for themselves depend on the corresponding payoffs to the recipient. In their experiments, dictators have a binary choice between a safe payout option and a risky payout option. They do not vary the degree of risk in the risky options. They find that dictators tend to be more risk averse when the risk applies to themselves as well as to others. They also find that dictators prefer the risky situation over a situation where outcomes are unfair with certainty. While this study reveals that decision makers are sensitive to risk borne by recipients, it falls short of addressing the degree to which dictators are willing to surrender their own sure gains in order to reduce the risk of a partner. We address this by giving decision makers a continuous choice set and varying the distribution of risky versus certain outcomes for the dictator and the recipient, respectively. In addition, they use a between subjects study design, while we offer results from a within design.

In summary, our work complements the literature on social preferences for risk by looking at how dictators' giving varies when the outcome for the recipient is uncertain. We fill in the gap in knowledge about the degree to which the dictator is willing to surrender his or her own wealth, or chances, to increase the chances of the recipient. Specifically, we ask if giving in non-risky situations is predictive of how dictators behave

when risk is involved. We also ask: do dictators give as if they are considering ex post outcome inequality or ex ante equity of chances. We thus combine the two subjects: how risk impacts choices and whether decision makers consider procedural fairness or outcomes fairness differently when allocating resources. Our experimental treatments are designed to differentiate between these ex post and ex ante formulations and to lend insights into their structure.

One abstraction from reality limits the generalizability of our results. In the treatments with risk to both parties, we define the lotteries such that both dictators and receivers have the same expected value for one additional chance to win the lottery (i.e. one token kept versus token given). Situations where risk allocation or risk sharing occur in real life, however, do not necessarily share this trait. For example, when a doctor is deciding on how much effort she is going to exert for a given patient, she considers the effort required for the other patients she will see that day, as well as the energy she wants to have left over to herself at the end of the day. Her expected value for one unit of saved effort, if you will, is potentially different from the expected benefit for the patient, were the doctor to exert that unit on the patient's behalf. If the patient is very sick and the doctor has the tools to help him, the patient's expected value of one additional unit of effort is higher. In the reverse case, where the patient is not severely ill or the doctor does not have the ability to treat him, the doctor's expected value of one additional unit of effort may be higher. This chapter offers a first treatment of the baseline case, where the expected value for one additional chance to win a lottery is identical for both dictator and recipient. To our knowledge there are no studies that cover the baseline case. Extensions to unequal marginal expected values will be an important follow-up to this paper. Thus,

we believe that our series of dictator choices, where payoffs are equal those in the standard dictator game in terms of expected value, contribute substantial new insights into social preferences under risk.

The next section outlines a model of behavior that informs our experiments and analysis. It is followed by a description of 6 treatments (tasks) – while all of these treatments are included in the pilot, only the two most salient treatments are part of the Tanzanian implementation. The succeeding two sections include more detailed descriptions of the pilot and Tanzanian implementations, respectively. A final section concludes.

4.2. A Model of behavior for risk allocation and risk sharing

Existing models of social preferences consider individual preferences over certain payoffs, represented by a utility function $u(c^1, c^2)$ where c^1 and c^2 are (final) consumption of person 1 and 2, respectively. Charness and Rabin (2002) define $u(c^1, c^2)$ with a combination of own payoff, minimum payoff and efficiency concerns. Fehr and Schmidt (1999) and Bolton and Ockenfels (2000) study inequality aversion, where $u(c^1, c^2)$ captures aversion toward payoff *differences* between players. None of these authors looks at how these kind of social preferences extend to situations under risk. To address these issues, we consider individual preferences over joint payoff distributions $F(c^1, c^2)$. This framework allows us to differentiate between situations in which individuals compare their payoffs ex post or their payoff *chances* ex ante.

Under the assumption of expected utility maximization, preferences of an individual who focuses on ex post comparisons are described by:

$$W^{ex-post}(F) = \int u^1(c^1, c^2) dF(c^1, c^2) \quad (1)$$

In contrast, to formalize preferences on ex ante comparisons of payoff chances, we assume that each agent's utility is a function of expected payoffs, $E(c)$, for both themselves and their partner, where the expectations for person one and person two are evaluated over the lotteries F^1 and F^2 , respectively. The utility based on ex ante comparison is then given by

$$W^{ex-ante}(F^1, F^2) = u^1(E(c^1), E(c^2)) \quad (2)$$

Thus, in this formulation we assume that agents compare their respective ex ante expected values.

To highlight the different utility constructs under risk, consider an adaptation of Machina's example to an allocation of an indivisible object between the decision-maker and the recipient. Any outcome leads to ex post inequality. If the decision-maker at least marginally prefers ex post inequality in her favor rather than the other person's favor, she would choose an allocation procedure that secures the object to herself. Differently, suppose the decision-maker is ex post inequality averse, but is willing to accept the inequitable outcome as long as it is decided upon fairly, as in Bolton, Brants and Ockenfels (2005). Then, given the option, she would avoid *ex ante* inequality using an allocation procedure that gives equal chances to the decision-maker and the other person to obtain the object. For example, 50/50 gamble, such as a coin toss, would equalize the chances to win the item and therefore avoid inequality from an ex ante perspective.

The model brings to light the primary research question of this chapter: whether individual perceptions of fairness differ when considering outcomes versus considering opportunities for outcomes. We discuss the implication of the model for the different treatments in turn, considering first the ex-ante formulation followed by that of the ex post formulation. We use the experiments to determine the dominant decision making rubric among subjects, ex post or ex ante.

4.3. The Experiment

To explore the determinants of giving under risk, we ran a series of modified dictator games. Treatment 1 replicates the standard dictator game.³ This standard dictator game highlights the decision makers' fairness in outcomes between the recipient and himself and serves as a baseline for the other treatments. In this set of experiments, we are interested in whether fairness in outcomes translates into ex ante fairness in risky situations. Ex ante fairness is equality of chances or initial conditions rather than outcomes. The modified treatments coincide with the standard dictator game in terms of expected payoffs. The payoff to the decision-maker or to the recipient or to both is, however, subject to risk. For example, in the second and third treatments, the dictator receives a certain amount of money but the recipient does not. By sacrificing some of his monetary payoff, the dictator can increase the recipient's chance to win a prize. If the dictator does not give any money, then the recipient will definitely not get the prize. If he

³ A vast literature has been devoted to studying giving behavior in such games in which one player (dictator) is asked to allocate a certain amount between himself and another player (recipient). While any dictator who is solely maximizing his or her own payoff should keep the entire endowment, Kahneman, Knetsch, and Thaler (1986) were first to show that most subjects choose an even split giving \$10 to each player over an uneven split (\$18, \$2) that favored themselves. Following the first dictator experiment with a continuous choice (Forsythe et al., 1994), most studies show that a significant proportion of dictators give positive amounts (for summary see Camerer (2003)). List (2007) shows that if taking is allowed, less but still a significant portion of players does not choose the selfish outcome.

gives the maximal amount, the recipient wins the prize for sure. Another treatment involves a transfer of lottery tickets. This situation is similar to the example of a mother allocating a treat to her two children, only that the decision maker needs to choose the *probability* with which *she herself* or the other person wins the prize (i.e. the treat). That is, the decision maker dictates the allocation of *chances* to win a given prize: giving zero secures the prize to the dictator. Increasing giving increases chances of winning for the recipient and decreases the dictator's chances. These treatments allow us to evaluate whether – when valuing equality – individuals compare their outcomes *after* resolution of uncertainty (*ex post* comparison) or if they compare their *ex ante* chances to gain certain incomes (*ex ante* comparison): no player who solely considers *ex post* distribution of payoffs would give a positive amount if the lottery draws are exclusive, i.e. if only one of the players wins the prize. We complement these treatments with one in which the dictator *cannot* change the expected value allocated to himself and the recipient, but only their exposure to risk.

The experiment was first piloted at the University of Maryland before we implemented them in Tanzania with the clinicians. We first describe the pilot experiment and results. We follow with the report on the experiment done in Tanzania.

Description of Tasks

The pilot experiment consisted of six treatments, or tasks. In each task, the decision-maker was asked to allocate 100 tokens between himself and the recipient, giving away $x \in [0, 100]$ and keeping $100 - x$ tokens. The payoff consequences differed between tasks

and were denoted in Experimental Currency Units (ECU) during the experiment ($100\text{ECU} = 10\text{USD}$). Table 4.1 summarizes the payoff consequences for each task.

Task 1 (*T1*) replicates the ordinary dictator game, as a baseline for comparison with risky decisions. The players' payoffs are given by $(c^1, c^2) = (100 - x, x)$. The purpose of this task is to position our results within the existing work on the dictator game, as well as to serve as a benchmark for other tasks.

In Tasks 2 and 3, the dictator allocates tokens as in Task 1, but unlike Task 1 the tokens given to the recipient represent lottery tickets. Tokens kept by the dictator are interpreted the same as in Task 1. More formally, in Tasks 2 and 3, the dictator receives a certain payoff in ECU equal to his allocation of tokens kept, $c^1 = 100 - x$, while giving the recipient the chance to win a prize. The recipient earns the prize of $P=100$ tokens with probability $\pi(x) = x/100$, $x \in [0, 100]$, in *T2*. In *T3* the recipient can win the prize $P=50$ tokens with probability $\pi(x) = x/50$, $x \in [0, 50]$. In these two treatments the dictator does not face any risk himself. For the recipient a lottery is drawn to determine if he receives the payment. *T2* and *T3* resemble situations as described in the introduction, for example a physician's costly effort to increase the healing chances of patients or bearing greenhouse gas abatement costs to reduce climate change faced by future generations.

We can attribute any difference between the dictator's decisions in *T2* and *T3* and that in *T1* to his assessment of the risk to the recipient, as both the dictator's payoff and the recipient's expected value are identical across the three treatments, as a function of x . A risk-averse dictator with preferences based on ex ante comparisons would evaluate the certainty equivalent to the recipient below the expected value and would give less in *T2*

and $T3$ than in $T1$. If he is interested in equalizing ex ante chances by equalizing the expected values, he might allocate more tokens to the recipient in the treatments with risk, $T2$ and $T3$. The reverse holds for risk-loving agents. If, on the other hand, the agent compares ex post payoffs and is highly averse to unfavorable inequality, he would reduce giving in $T2$ compared with $T1$. Task $T3$ avoids this unfavorable inequality as the recipient can only win a maximum of $c^2 = 50$. If agents are therefore largely driven by ex post inequality concerns, we should expect more giving in $T3$ than in $T2$.

Task 4 ($T4$) aims to test whether preferences based on ex ante or ex post comparisons are more appropriate to model dictators' allocation decisions under risk. In this treatment, both the dictator and recipient face risk. Here, the dictator distributes the *chances* to win a prize. The probability for winning the prize of $P=100$ is given by $\pi^1(x) = 1 - x/100$ and $\pi^2(x) = x/100$. Thus the token allocations represent the chances of winning a lottery. In task $T4$, the draws are dependent: either the dictator or recipient wins. Again, $T4$ was designed to differentiate between preferences based on ex ante and ex post comparisons. Note that ex post formulations of preferences (1) imply

$$W^{T4, \text{ex post}}(x) = (1 - x/100)u(100, 0) + (x/100)u(0, 100)$$

such that for any preference with $u(100, 0) > u(0, 100)$, we expect subjects to choose $x^{T4} = 0$. As long as agents put slightly more weight on their own than on others' payoffs, we have a clear theoretical prediction. Note that this assumption is satisfied by all models in the literature (e.g. Charness and Rabin, 2002; Fehr and Schmidt, 1999). Conversely, if agents have preferences based on ex ante comparisons as in (2), they will generally give

positive amounts in $T4$. If, for example, subjects try to avoid inequality of expected values or try to maximize the minimal ex ante utility, we expect them to choose

$$x^{T4} = 50.^4$$

Task 5 ($T5$) is identical to $T4$ except that instead of one lottery, two independent lotteries are drawn, one for each player. Here, one of the players, both players, or neither of them wins the prize. In terms of ex post comparisons, $T4$ and $T5$ therefore differ. Ex ante (i.e. when comparing expected values, these tasks are the same. Comparing $T4$ and $T5$ therefore also allows us to further differentiate between ex post or ex ante comparisons.

We complement these five treatments with one additional task, $T6$, in which the dictator *cannot* change the expected value allocated to himself and recipient, but can change the risk exposure involved. The potential allocations are a 50/50-gamble between $x/2$ and $100 - x/2$ for person 1 and a 50/50-gamble between $50 - x/2$ and $50 + x/2$ for person 2. Independent lotteries are drawn for each player to determine if they win the high or low ECU amount. The purpose of this final treatment is to gain insights into whether social preferences affect the allocation of risks consistently with the allocation of expected payoffs. As such, predictions for $T6$ complement those in $T4$. Ex ante equality in chances would be generated by a choice of $x^{T6} = 50$, for which both players face a gamble between 25 and 75. We would therefore expect players with preferences based on ex ante comparisons who choose to give larger amounts in the standard dictator game to choose an allocation close to $x^{T6} = 50$. If, however, dictators are fully selfish (they give

⁴ Note that the same prediction of zero giving would result if just give in the dictator game because of identifiable actions. In $T4$ and $T5$, a zero payoff to the recipient could result even if the dictator gave all but one token to the recipient. Consistent with Dana et al. (2007), we would then also expect less giving than in $T1$.

nothing in the dictator game) we would expect $x^{T6} = 100$ if they are risk-averse and $x^{T6} = 0$ if they are risk-loving. We thus predict that decisions in $T1$ should be informative for the absolute distance between decisions in $T6$ and 50.

In all treatments, recipients were not informed about the actual choice, x . They only learned about their own final payoff at the end of the experiment. Dictators did not receive direct information about the final payoff to the recipient. The effect of such information on giving decisions is left to further research.

4.4. Pilot implementation: UMD students

The pilot results reported here are from Brock, Lange and Ozbay (2010). Results from the pilot and lessons from implementing the experiment with the UMD students inform the choice of treatments to bring to the laboratory with the Tanzanian clinicians. Beyond that primary goal, however, this pilot provides unique and novel evidence on the impact of risk on altruism. With this pilot data, we first establish that social preferences of most players who give non-zero amounts in a standard dictator game are best described as being defined over ex ante distribution of risk. These players do not appear to compare ex post payoffs, but rather look at equalizing the ex ante chances to win. Decisions are, however, affected by the riskiness of final payoffs: decision-makers generally give up less income than in the standard dictator game if the transfer is risky, that is, if it does not increase the recipient's income for sure but only her chances to gain income. We also show that the propensity to give in a standard dictator-game is a good predictor for giving in risky situations: those who transfer more money in the dictator game are more likely to

equalize the *ex ante* situation, i.e. payoff chances, in other games. Our results thus bring to light how existing theories of social preferences can extend to risky contexts.

Recall that our experiment consisted of a series of dictator games in which the dictator must allocate 100 tokens between himself/herself and a second player (recipient). We report the results of 6 choice tasks. Tasks differ according to the payoff consequences for each of the players. One of the tasks replicates the standard dictator game. In the other 5 tasks, the dictators allocate risk for their recipient counterparts or between themselves and their counterparts.

We conducted our experiment in September of 2009 in the Experimental Economics Laboratory at the University of Maryland. A total of 152 subjects were recruited from among University of Maryland undergraduates representing a variety of undergraduate majors, including but not limited to economics, finance, chemistry, government, and biology. Subjects first gathered in one room where they reviewed consent forms. After signing a consent form, all subjects were given a copy of the general instructions, which were also read aloud by an experimenter. Subjects were randomly assigned to be either person 1 (dictator) or person 2 (recipient).⁵ The dictator subjects were then led into a separate room. The recipient subjects remained in the first room. Each dictator was randomly matched with one recipient without revealing the identity to either of the subjects. No subjects were permitted to communicate before or during the session. An experimenter was present in each of the two rooms for the duration of the experiment. A copy of the instructions is included in the Appendix.

⁵ In the experiment, the words “dictator” and “recipient” were not used.

All subjects participated in all 6 choice tasks, resultantly our results are within rather than between comparisons. Dictators submitted all of their allocation decisions via computer and did not learn of the outcomes of their choices between rounds. Computer stations were randomly assigned. Using computers allowed us to also randomize the order of tasks for each dictator to minimize order effects⁶.

The receivers filled out decision forms using paper and pen and also did not learn dictator choices between rounds. Their task was to determine how much they *expected* their dictator partner to allocate to them for each task. The recipients' decisions had no bearing on the final allocations and this was made clear before each session began. Dictators did not learn recipients' expectations, either between tasks or at the end of the experiment. It should be noted that the recipient task was not incentivized; there were no consequences for reporting beliefs inaccurately, but there were also no reasons for recipients not to disclose their true beliefs. Because this task was somewhat informal, we do not provide a rigorous exposition of these results. Rather, outcomes from the recipient task are largely exploratory.

After all subjects completed all tasks, payment was determined from one randomly selected task round. Using the computer, we selected payment rounds independently for each dictator-recipient pair. We did not reveal which round was the randomly selected

⁶ The randomization of treatment order was successful for eliminating any potential order effects, with the exception of when Task 1 was ordered before Task 3. When Task 1 was before Task 3 subjects gave significantly more in Task 3. This may be because of the significant correlation between giving in Task 1 and the pattern of when Task 1 comes before Task 3 – when Task 1 comes before Task 3, Task 3 often falls at the end of the series of choice tasks. The correlation coefficient between giving in Task 1 and Task 3's order of appearance is 0.35 and it is significant at a .2% level ($p < 0.01$). Since the maximum allowable giving in Task 3 is half of what it is in all other tasks, and since those that already gave more in Task 1 tended to see Task 3 later in their task ordering, Task 3's appearance at the end of the task set may have caused an unintentional upward bias in T3 giving for those subjects.

payment round or what the dictator choice was in that round. Thus, subjects did not learn the outcomes of their choices at any time during or after the experiment. They only learned of their final earnings. Likewise, the recipients did not know if their final earnings were the result of a kind (or unkind) dictator or due to a lottery. Subjects received \$1.00 in cash at the end of the session for each 10 experimental currency units (ECU's) they earned in the randomly selected task round. A \$5 show-up fee was included in the subject payments, which were paid at the end of each session. Dictators and receivers were paid separately and in private.

4.4.1. Pilot results and discussion

The results on the dictators' choices and the recipients' expectations are summarized in Table 4.2 and Table 4.3, respectively. These tables provide the summary statistics of average choices as well as the proportion of players choosing $x = 0$ or $x = 50$ in each task. For example, average number of tokens given out of 100 tokens in the dictator game is $x = 21.07$ and thereby consistent with numbers reported in the literature (Camerer, 2003). It can immediately be seen that significant positive giving occurs for all tasks. Figure 4.1 again shows the average contribution by task, while Figure 4.2 displays the percentage of subjects giving non-zero amounts (participation rate) and Figure 4.3 shows the average contributions for those that chose to give non-zero amounts. The summary statistics of these conditional contributions is given in Table 4.4. Notably, the figures already show important differences between treatments. We explore those in detail below.

In a first step, we can study giving decisions in $T4$. Recall that if ex ante considerations dominate we expect subjects to give positive amounts, whereas if subjects have ex post considerations we would expect very little positive giving and giving in $T4$ to be less than giving in $T1$. In our sample, giving in $T4$ is significantly different from zero: 33 subjects (43%) chose to give positive amounts. Additionally, the conditional contributions in $T1$ and $T4$ coincide (see Figure 4.3 and Wilcoxon test in Table 4.5). We also include a table reporting the unconditional difference in means and significance using Wilcoxon signed-rank tests (Table 4.6). Here, too, we find no significant difference between giving in $T1$ and giving in $T4$. The unconditional sample includes those who did not give positive amounts in either treatment being compared and thus averages are skewed by the concentration of giving at zero. Nonetheless, the directions of differences between treatments are the same as in the conditional giving comparisons. Thus, by excluding zeros from the analysis we are simply concentrating on a pattern that exists more generally in the data. We therefore can clearly reject the hypotheses that ex post comparisons are able to explain subjects' behavior.

Result 1: *Preferences based on ex post payoff comparisons cannot explain giving decisions under risk.*

Comparing the distributions of giving in $T4$ and the standard dictator game may suggest that dictator giving with risk may be distinct from non-risky giving -- for $T4$ there is slightly more mass on $x=0$ and slightly less mass on $x=50$ than for $T1$. But the difference is indeed small and a Wilcoxon sign-rank test cannot reject the equality of the underlying distributions. This finding is consistent with an ex ante comparison of payoff consequences, and cannot be explained by any preference structure that solely relies on

ex post comparisons. In line with this result is the apparent similarity between $T4$ and $T5$; behavior in $T4$ and $T5$ should be the same if evaluating payoff prospects ex ante, but they would differ in terms of ex post comparisons.

The comparison between $T2$ and $T3$ also informs whether or not dictators evaluate ex post payoff differences. As is discussed in the description of the tasks, if agents are largely driven by ex post inequality concerns, we would expect more giving in $T3$ than in $T2$. We find the opposite to be true, however. Conditional on giving, task 2 has a significantly higher mean than in task 3, which is not in line with the ex post predictions.

As another indication for preferences that consider ex ante chances rather than ex post payoff realizations, we can compare individual decisions in the standard dictator game with those in $T6$. In $T6$, the dictator faces a 50/50-gamble between $x/2$ and $100 - x/2$ while the recipient faces potential outcomes of $50 - x/2$ and $50 + x/2$. As such, the decision x does not affect the expected value for either player, but it does impact the risk allocation. For $x = 50$, both players face the same potential payoffs. An ex ante oriented player who allocates more to the recipient in the dictator game can therefore be expected to choose closer to $x=50$ in $T6$. Indeed, we can establish this result:

Result 2: *The more subjects give in a standard dictator game, the more they equalize the ex ante risk exposure for risky decisions.*

Table 4.7 provides evidence for this result based on a series of tobit regressions that explain the choice in the respective tasks as a function of the choice in the standard

dictator game (*TI*).⁷ For example, the absolute value of the difference $|x^{T6} - 50|$ is smaller the larger the contribution in the dictator game (1% significance). That is, even if the decision does not involve a trade off of own expected value, agents' choices in the dictator game are informative for the allocation of risks between themselves and some recipient. This is also supported by the analysis of the relationship between giving in *TI* and the absolute value of the difference $|x^{T6} - 100|$. When *TI* giving is higher, so is the deviation from the safe option (i.e. giving all the risk to one's partner). This serves as further evidence that the generosity in the standard dictator game predicts a tendency toward equating ex ante chances. Similarly, but perhaps less surprisingly, agents are more likely to give in all tasks (1% significance) the more they gave in the dictator game (Table 4.7).

In order to confirm that this result is not driven by those who give zero in all tasks (i.e. that the regressions are not simply telling us that selfish dictators in *TI* are selfish in all the other treatments), we also report results from these regressions with an adjusted sample to exclude the selfish players. "Selfish" is defined as people who give zero in all tasks. When we exclude these subjects, we find that the relationships between giving in the dictator game and giving in the risky decisions remains. These results are reported in Table 4.8.⁸ The result holds for alternative definitions of "selfish"; analyses reported in Table 4.9 and Table 4.10 use samples with giving conditional on $TI > 0$ or $TI > 0$ and $Ti > 0$, respectively. We further test the importance of the selfish players by regressing the

⁷ We use tobits because of the concentration of giving at zero in all tasks.

⁸ Tobit regressions still make sense when excluding selfish types because there is still 30-42% zeros in the various tasks. That is, selfish is defined as giving zero in all tasks. We do not consider those that give zero in at least one task to be selfish, so many zero values remain after removing the "selfish" players from the sample.

decision in each task on a binary variable equal to 0 if the person was selfish in *T1* (Table 4.11). We do not find that being selfish in *T1* predicts selfishness in other tasks; regression results show that non-selfish people are driving the results seen in regressions on the whole sample. Indeed there are a few participants that chose to give zero in all tasks, but they do not drive our results. These results confirm that selfish people are not driving Result 2.

We do find, however, evidence that risk faced by the recipient motivates increased selfishness among dictators. A series of Wilcoxon sign rank tests reveals that agents give more in the standard dictator game than in *T2* (5% significance) and *T3* (10% significance), that is when the recipient's payoff is subject to risk while the dictator's is not. As such, we establish the following result:

Result 3: *Players' decisions are affected by the recipient's exposure to risk.*

Further insights into this result can be obtained from explicitly comparing the distributions for the decisions (see Table 4.4). Table 4.12 provides a series of probit models where we explain the choice to participate (Column 2), choices being between 1 and 49 (Column 3), and choices being equal to 50 (Column 5) (always coded as a binary variable taking value 1 if the choice fits the criteria) by the decision tasks. For this we defined explanatory dummy variables that take the value of 1 if the task is *T2*, *T3*, *T4*, *T5*, respectively.

Columns 1 to 3 of Table 4.12 show that contributions tend to be lower in the tasks involving risk than in the standard dictator game. The result is robust to multiple specifications. In the first specification (columns 1 and 2) we use a hurdle model,

regressing the participation indicator on the treatment dummies in the first stage. In the second stage we perform a truncated regression (truncated from below at zero), to adjust the distributional assumption of normality. The truncated regression differs from the GLS model in magnitude of the coefficients and in one case in significance of coefficients ($T5$ is not significant in the truncated model). Otherwise the truncated regression gives the same pattern of significance and the coefficients have the same signs as the single regression model. While this result is also illustrated in Figure 4.1, Figure 4.2 and Figure 4.3 reveal that this effect is primarily driven by a reduction in the conditional contributions, rather than by a change in the participation rate. In fact, a Wilcoxon test (see Table 4.5) shows a difference in conditional contributions between $T1$ and $T2$ (1% level of significance) and $T1$ and $T3$ (1% level). We also show significance in the comparison of $T2$ versus $T3$, which gives us transitivity with respect to $T1$, $T2$ and $T3$ (i.e. $T1 > T2$, $T2 > T3$, $T1 > T3$).

This result is consistent with the results in columns 4-6 of Table 4.12 where we decompose the choice options to distinguish between positive giving, giving between 1 and 49 and giving equal to 50. We find that fewer subjects choose to give 50 in $T2$ and $T3$, than in the standard dictator game, while more agents give smaller amounts (between 1 and 49). This observation is in line with findings by Dana et al. (2007): since the potential payoffs to the recipient do not depend on the dictator's choice, the dictator can exploit the "moral-wiggle room". The recipient will not be able to perfectly infer the dictator's action from observing the outcome.

It is interesting and puzzling to see, however, that the proportion of players giving zero is also smaller in $T3$ than in $T1$ (the difference between $T2$ and $T1$ is insignificant). This

indicates that some players who displayed selfish behavior in the standard dictator game give a positive amount in $T3$, thereby giving the recipient a chance to win some large amount.

Our experimental design further allows us to compare the decisions made by dictators with the expectations of the recipient. While recipients' answers were not incentivized, we believe that the comparison of their expectations with the actual choices of the dictators provides interesting insights. Table 4.3 displays the respective averages, standard deviations, and proportion of subjects expecting $x = 0$ or $x = 50$ for all tasks. Comparing expectations with actual choices, we see that they almost coincide for the standard dictator game. In presence of risk, however, expectations generally differ from choices. For $T2$ and $T3$, subjects expect more generosity than dictators actually provide (t-test at 1% significance, Mann-Whitney at 5% for $T3$). Recipients therefore do *not* expect the dictator's choices to change when only recipients are exposed to risk. It is interesting to see, however, that the expectations for $T4$ are significantly lower than those in the standard dictator game (1%, Wilcoxon). The expectations of recipients are therefore much more in line with potential ex post comparisons: 58% of them expect to get a zero allocation if the dictator allocates lottery tickets which only allow either person to win. They expect a more generous allocation in $T5$ when both agents could potentially win (1%, Wilcoxon between expectations in $T4$ and $T5$). This expectation, however, is not justified by the actual decisions (10% significance difference in $T5$, Mann-Whitney). Finally, in task $T6$ recipients expect a larger exposure to risk, i.e. they anticipate the dictator to choose safer options than these actually do (Mann-Whitney, 1% significance). This is in particular driven by recipients not expecting a risk-loving choice ($x = 0$): this

extreme choice is taken by 16% of dictators while it was only expected by 3% of recipients. We can summarize this discussion as follows:

Result 4: *While correctly anticipating decisions in the dictator game, subjects are less able to predict choices when payoffs are risky.*

Result 4 has implications for extensions of the current experimental setup to strategic environments: it can be problematic to find equilibrium strategies when beliefs do not coincide with actual behavior. Similarly, when extending the current dictator game to an ultimatum game context, for example, wrong expectations may affect acceptance decisions if players' preferences depend on expectations (e.g., reference-based models).

4.5. Full implementation: Tanzanian clinicians

The pilot study results are interesting and novel in their own right. They also inform our choice of treatments to bring to the laboratory with the Tanzanian clinicians. We brought *T1*, *T2* and *T4* from the pilot to the Tanzanian implementation. We retained *T1* because it serves as the baseline for comparing the impact of the different treatments on altruism. The second treatment measures what we call risk allocation – how much risk the dictator is willing to give their partner when they themselves do not face risk. We feel this approximates the clinician-patient interaction in that the clinician must expend effort (give of his tokens) in order to improve the patient's chances of getting well. To the extent that he does not expend maximally for the patient, or reserves effort, the clinician benefits, and his benefit may not be exposed to risk. Meanwhile *T4* approximates the same kind of situation, but incorporates the fact that in many cases the clinician actually exposes himself to risk (or perceived risk) when he exerts on the patient's behalf. We

choose *T4*, with dependent lotteries, instead of *T5* in order to capture the idea that the more effort the clinician exerts for any given patient, the more he risks his own benefit. This may be the case when treating a highly contagious patient where optimal treatment requires close physical contact between doctor and patient. Our choice of these three treatments does not suggest that the other treatments do not somehow mirror the doctor-patient relationship and future research may include further investigation into these alternate treatments.

To clarify the discussion in this chapter, we refer to the Tanzanian implementation of *T1*, *T2* and *T4* from the pilot as *T1T*, *T2T* and *T4T*, respectively.⁹ *T1T* also corresponds to *T1* in chapter 4. *T2T* and *T4T* do not correspond to any other treatments reported in other chapters; their results are reported here only.

4.5.1. Preliminary results, Tanzanian clinicians

The results on the dictators' choices and the recipients' expectations from the Tanzanian implementation are summarized in Table 4.13, Table 4.14 and in Figure 4.5. Giving results in the standard dictator game, labeled as *T1T* in the table, are the same as those reported in Chapter 5.¹⁰ In *T1T*, the clinicians' mean tokens given was 34, with 60% of participants giving fewer than 50, 29% giving half of their allocation and 7% giving more than 50 tokens. Since the Tanzanian sample does not have the conventional concentration

⁹ Note that the naming convention of the treatments in this chapter conflict with those in Chapter 2; *T2* in chapter 2 is a different treatment than *T2T* in this chapter. Both chapters report results on experiments with clinicians, and originally the treatments in this chapter were labeled *T4* and *T5*. We adjusted the names of these treatments to correspond with the matching treatment from the pilot study. This facilitates the discussion comparing the behavior of the Maryland students with the Tanzanian clinicians in this chapter.

¹⁰ All the Tanzanian experiments were run during the same sessions and we include the *T1T* results here for easy comparison with *T2T* and *T4T*.

at zero, the comparable table from the pilot results is Table 4.4, which shows giving averages among those UMD students who choose to give a non-zero amount in any task. It can immediately be seen that more subjects choose to give over fifty percent of their tokens in *T2T* compared to the baseline, but that the portion of participants giving 50 or more drops substantially in *T4T*, from 40% in *T2T* to 23% in *T4T*, shifting the mass of givers into the sub-fifty range. This is in contrast to the pilot results, where percentage of selfish givers ($0 < x < 50$) is greater in *T2* than in the *T4*. Also, for the clinicians, the action is on either side of the 50/50 allocation, while the UMD students did not tend to give more than 50 tokens to their partners. In fact, while the percentage of those giving 50% among the clinicians does not differ between *T2T* and *T4T* (and is half as large as *T1T*), more dictators choose the 50/50 allocation in the pilot's *T4* than in *T2*.

As in the pilot, giving in *T4T* is significantly different from zero, but somewhat unsurprisingly for this sample the percentage of subjects giving more than zero is extremely high: 66 subjects (98%) chose to give positive amounts. We test the difference in means between treatments with Wilcoxon signed-rank tests (Table 4.15). Unlike the pilot where we found no significant difference between giving in *T1* and giving in *T4*, among the clinicians giving in *T4T* is significantly lower than in any of the other treatments; all differences are significant at the 1% level. On the other hand, giving in the risk allocation treatment, *T2T*, does not differ significantly from *T1T*. Recall that we expect people with a preference for ex ante fairness to equate expected values and thus give closer to the 50/50 allocation in the treatments with risk, whereas those who aim for ex post fairness will behave selfishly. Hence, while the pilot suggests that preferences based on ex post payoff considerations cannot explain giving decisions under risk (Result

1), we cannot reject the hypothesis that ex post comparisons are able to explain their behavior in the clinician sample. Furthermore, unlike the pilot sample's Result 3, we do not find evidence that risk faced by the recipient affects the clinicians' choices. A series of Wilcoxon sign rank tests reveals that agents do not give more in the standard dictator game than in *T2T*. Again, it is *T4T* that motivates changes in generosity, with significant reductions in giving when the agents themselves are exposed to risk.

Since giving in *T4T* is very different than in other treatments, it is not surprising that giving in the standard dictator game is not predictive of *T4T* giving. Table 4.16 juxtaposes these results with the comparable results from the pilot. This result also brings Result 2 into question, though it is tempered by the fact that agents are more likely to give in *T2T* (risk allocation) the more they gave in the dictator game (1% significance, also in Table 4.16). This suggests that clinicians perceive allocating risk similarly to allocating tokens when they themselves do not face risk. Generosity is scaled back considerably, however, when the clinicians face risk in their own outcomes. Whether or not this is a direct result of their work environment (i.e. an artifact of practicing defensive medicine) or a reflection of the defensive posture toward risk in the developing economy in general is an important question for further investigation.

Table 4.17 provides a series of probit models where we explain giving patterns by way of the decision tasks. We look at selfish choices (giving less than or equal to zero, Column 2), choices being equal to 50 (Column 3) and generous choices (giving more than 50%, Column 4). This is similar to Table 4.12 from the pilot results, where the dependent variable is coded as a binary variable taking value 1 if the choice fits the criteria. For this we defined explanatory dummy variables that take value 1 if the task is *T2T* or *T4T*,

respectively. The baseline treatment is *T1T*. Here, too, we see that in *T4T* clinicians are significantly more likely to be selfish (at the 1% level) and also less likely to be fair (significant at the 5% level) than in other treatments. In *T2T*, on the other hand, clinicians are significantly more generous, which is evidence in favor of Result 3 from the pilot study – that clinician subjects are responsive to recipients’ exposure to risk. A regression of tokens given across all treatments on treatment dummies (Table 4.18) echoes these results: when the clinician him or herself is exposed to risk, he or she gives significantly fewer tokens than in the standard dictator game. On average dictators decrease their giving by about a fifth. The outcome is not sensitive to controlling for clinician characteristics, none of which are significant across all specifications in explaining behavior.

Also in Table 4.17, we see that fewer subjects choose to give a fair allocation, splitting the endowment roughly in half, in *T4T* than in the standard dictator game. To some extent this may be reflective of subjects exploiting “moral wiggle room” as we observe in the pilot, but the response to own exposure to risk and the similarity in mean giving between *T2T* and *T4T* suggest a more straightforward behavior: subjects are generous when they know how much it will impact them directly. In a sense, it is like giving a donation after you have met all your other monthly expenses, when you can carefully weigh how nice you can afford to be, rather than before you receive your paycheck.

As in the pilot, our experimental design further allows us to compare the decisions made by dictators with the expectations of the recipient. Recall that recipients’ answers were not incentivized. Table 4.14 displays the respective averages, standard deviations, and proportion of subjects expecting $x = 0$ or $x = 50$. Figure 4.6 shows the averages of

choices and expectations for all tasks. Comparing expectations with actual choices, we see that despite variations in giving across treatments, expectations hover right around 50 tokens. The exception is treatment two; recipients on average expected more than half of the pie in that treatment. In all treatments expectations exceeded actual giving, especially in the case where both players are exposed to risk and giving decreases so much. These results appear to suggest that either recipients did not identify with the givers or that in the absence of incentive compatibility recipients put little effort into understanding the difference between the tasks. Lastly, the fact that recipients would settle on the 50/50 allocation as a default is evidence of the strength of the 50/50 norm in the non-student population.

4.6. Discussion and conclusions

Many recent theories attempt to explain behavior in laboratory and field experiments by modeling some sort of social preferences. Giving in dictator, ultimatum, gift exchange, public good, and many other games has been rationalized using preference structures that allow for motivations other than selfishness, such as inequality aversion, concerns for efficiency, or consideration of lowest payoffs. It remained an open question, however, how such “social” behavior extended to situations that involve risk and how the theories can be extended. This chapter provides compelling evidence on various dimensions of this issue.

In particular, we address the issue of whether social preferences are based on comparisons of *final (ex post) payoffs* or on comparisons of *ex ante chances*. By observing decisions in situations that expose the decision-maker, another person, or both,

to risk, we differentiate between these two preference structures. We find that the behavior in a standard dictator game serves as a good predictor for social preferences under risk, but that this predictive power is diminished among clinicians when risk is shared. Thus, while the behavior of a substantial fraction of student subjects is consistent with dictators comparing ex ante chances, rather than ex post payoff, results suggest the opposite for clinicians. Our experimental treatments for the risk allocation and risk sharing experiments allow us to differentiate between the ex post and ex ante formulations and to lend insights into their structure, but they do not allow us to determine why these two populations might behave differently in the laboratory in the presence of risk. As we develop these results further, we may consider if there is a model that incorporates both types of behavior.

Our study clearly can only provide a first step towards a better understanding of giving decisions under risk that affect other subjects than the decision-maker. For example, while we fixed the attainable payoff levels in the lottery situations, it appears worthwhile to explore how downside versus upside risk affects behavior or how the availability of insurance options changes transfer decisions. Also, it remains an open question whether differences in expected value between players impacts generosity. We leave those questions to future research.

Nonetheless, our findings have widespread policy implications, with applications in fields ranging from charitable giving to healthcare to environmental conservation. Donations to charitable organizations must be made based on *beliefs* about how the money is used and if the financed projects are successful. Physicians make efforts to increase the chances of healing the patient but may never know the health outcome or benefit themselves from

these efforts. Environmental policies, such as those aimed at climate change, regularly require costly actions whose benefits are uncertain and might accrue to someone other than the decision maker. In the case of climate policy, current generations decide on costly abatement of greenhouse gas emissions, while the potential benefits from reduced climate change are uncertain and will be experienced by future generations. Our results indicate how such uncertainties may affect the willingness of people to give up consumption in order to benefit others.

In summary, our work complements the literature on social preferences for risk by looking at how dictators' giving varies when the outcome for the recipient is uncertain. We fill in the gap in knowledge about the degree to which the dictator is willing to surrender his or her own wealth or chances to increase the chances of the recipient. Specifically, we ask if giving in non-risky situations is predictive of how dictators behave when risk is involved. We also ask: do dictators give as if they are considering ex post outcome inequality or ex ante equity of chances. We thus combine the two subjects: how risk impacts choices and whether decision makers consider procedural fairness or outcomes fairness differently when allocating resources. We believe that our series of dictator choices, where payoffs equal those in the standard dictator game in terms of expected value, contribute substantial new insights into social preferences under risk.

4.7. Tables and figures

Table 4.1. Summary of tasks

Task	Payoff for The dictator (ECU)	Payoff for Recipient (ECU)
T1	$100 - x$	x
T2	$100 - x$	0 or 100 determined by a lottery with chances of winning $x/100$
T3	$100 - x$	0 or 50 determined by a lottery with chances of winning $x/50$
T4	0 or 100 determined by a shared lottery, chance of winning $1 - x/100$	0 or 100 determined by a shared lottery, chance of winning $x/100$
T5	0 or 100 determined by an independent lottery, chance of winning $1 - x/100$	0 or 100 determined by an independent lottery, chance of winning $x/100$
T6	50/50 gamble between $x/2$ and $100 - x/2$ determined by an independent lottery	50/50 gamble between $50 - x/2$ and $50 + x/2$ determined by an independent lottery

Table 4.2. Summary statistics of the dictators' choices

	Number of subjects	Mean of choices	SD of choices	Number of subjects with $x=0$	Number of subjects with $x=50$	% of subjects with $x=0$	% of subjects with $x=50$
T1	76	21.08	27.45	38	17	50%	22%
T2	76	15.57	20.13	37	9	49%	12%
T3	76	15.44	17.67	30	9	39%	12%
T4	76	18.24	27.12	43	12	57%	16%
T5	76	16.30	21.74	41	12	54%	16%
T6	76	48.16	33.59	12	17	16%	22%

Table 4.3. Summary statistics of the recipients' expectations

	Number of subjects	Mean of choices	SD of choices	Number of subjects with x=0	Number of subjects with x=50	% of subjects with x=0	% of subjects with x=50
T1	76	21.43	23.80	32	18	42%	24%
T2	76	21.25	26.77	32	11	42%	14%
T3	76	23.51	20.74	20	17	26%	22%
T4	76	15.74	23.01	44	10	58%	13%
T5	76	22.72	23.06	29	17	38%	22%
T6	76	65.91	28.91	2	26	3%	34%

Table 4.4. Summary statistics of giving by task, conditional on TG>0

	Number of subjects	Mean of choices	SD of choices	% of subjects with x=50	% of subjects with 0<x<50
T1	38	42.16	24.79	45%	45%
T2	39	30.33	18.44	23%	72%
T3	46	25.52	16.06	20%	80%
T4	33	42.00	26.36	36%	45%
T5	35	35.40	18.62	34%	57%
T6	64	57.19	28.62	27%	34%

* All subjects who give positive amounts in tasks 1-5 also give positive amounts in task6.

Table 4.5. Differences in average tokens given, conditional on TG>0

Task	2	3	4	5
1	12.55*** (31)	14.94*** (35)	0.39 (26)	8.04 (30)
2		6.34** (32)	-7.27 (26)	-5.22** (27)
3			-16.76*** (29)	-10.10*** (32)
4				3.63 (27)

Sample size for each comparison in brackets. Differences tested with Wilcoxon signed-rank tests. *** (**, *) indicates significance at 1% (5%, 10%) level.

Table 4.6. Differences in average tokens given, unconditional (N=76)

Task	2	3	4	5
1	5.51**	5.63*	2.84	4.78
2		0.12	-2.67	-0.74
3			-2.79	-0.86
4				1.93

Differences tested with Wilcoxon signed-rank tests.

*** (**, *) indicates significance at 1% (5%, 10%) level.

Table 4.7. Tobit regression of choices in tasks on dictator game decisions, with cluster robust standard errors, full data set

	Dependent Variable					
	Tokens Given in T2	Tokens Given in T3	Tokens Given in T4	Tokens Given in T5	T6-50	T6-100
Tokens Given in T1	0.71*** (0.14)	0.47*** (0.12)	0.85*** (0.23)	0.87*** (0.15)	-0.30*** (0.12)	0.26* (0.14)
Constant	-11.04** (4.86)	-1.68 (3.98)	-23.73*** (8.96)	-17.87*** (6.23)	30.16*** (3.78)	42.74*** (7.21)
Pseudo R-squared	0.07	0.04	0.04	0.08	0.01	0.004

Standard errors in brackets. *** (**, *) indicates significance at 1% (5%) level.

Table 4.8. Tobit regression of choice sin tasks on dictator game decisions, with cluster robust standard errors, conditional on giving in at least one task

	Dependent Variable					
	Tokens Given in T2	Tokens Given in T3	Tokens Given in T4	Tokens Given in T5	T6-50	T6-100
Tokens Given in T1	0.46*** (0.11)	0.21** (0.09)	0.43** (0.21)	0.60*** (0.14)	-0.25** (0.12)	0.53*** (0.17)
Constant	3.38 (5.05)	13.57*** (3.86)	0.72 (9.20)	-2.40 (6.27)	26.99*** (4.92)	27.56*** (7.02)
Pseudo R-squared	0.04	0.01	0.01	0.04	0.01	0.02

Standard errors in brackets. *** (**) indicates significance at 1% (5%) level.

Table 4.9. Tobit regression of choices in tasks on dictator game decisions, with cluster robust standard errors, conditional on Task1>0

	Dependent Variable					
	Tokens Given in T2	Tokens Given in T3	Tokens Given in T4	Tokens Given in T5	T6-50	T6-100
Tokens Given in T1	0.45*** (0.14)	0.14 (0.11)	0.38 (0.25)	0.38** (0.17)	-0.24 (0.15)	0.36* (0.19)
Constant	4.62 (6.94)	17.25*** (5.51)	5.04 (12.31)	10.16*** (8.53)	27.32*** (7.40)	36.97*** (9.50)
Pseudo R-squared	0.03	0.01	0.01	0.02	0.01	0.01
Percentage of Censored Observations	49%	39%	57%	54%	22%	18%

Standard errors in brackets. *** (**, *) indicates significance at 1% (5%) level.

Table 4.10. Linear regression of choices in tasks on dictator game decisions, with cluster robust errors, conditional on Task1>0 and Taski>0

	Dependent Variable			
	Tokens Given in T2	Tokens Given in T3	Tokens Given in T4	Tokens Given in T5
Tokens Given in T1	0.45*** (0.16)	0.25* (0.13)	0.67*** (0.17)	0.20 (0.18)
Constant	11.39* (5.51)	15.93*** (5.01)	13.41*** (6.70)	27.89*** (7.80)
Pseudo R-squared	0.35	0.02	0.39	0.08

Standard errors in brackets. *** (**, *) indicates significance at 1% (5%, 10%) level.

Table 4.11. (selfish binary) Tobit regression of choices in tasks on dictator game decisions, with cluster robust standard errors

	Dependent Variable					
	Tokens Given in T2	Tokens Given in T3	Tokens Given in T4	Tokens Given in T5	T6-50	T6-100
Non Selfish	41.33*** (7.58)	30.96*** (5.68)	53.47*** (12.47)	55.88** (8.37)	-13.98** (5.56)	6.97 (9.48)
Constant	-18.67 (6.80)	17.25*** (5.51)	-35.19*** (11.56)	-29.99*** (8.25)	30.84*** (4.17)	44.74*** (8.16)
Pseudo R-squared	0.07	0.06	0.04	0.10	0.01	0.001

Standard errors in brackets. *** (**) indicates significance at 1% (5%) level.

Table 4.12. Maximum likelihood estimates with random effects (column 1); Probit models (columns 2-4) on dictators; choices of the difference tasks (baseline is dictator game T1)

	Hurdle model		Other models			
	Probit Participate (Choice>0)	Truncated linear regression (Choice)	Linear Random Effects model, GLS robust se's; (Choice)	Probit Participate (Choice>0)	Probit Choice in [1,49]	Probit Choice=50
T2	0.07 (0.28)	-17.51** (7.34)	-5.51** (2.55)	0.07 (0.28)	0.66** (0.27)	-0.65** (0.32)
T3	0.57** (0.29)	-26.67*** (7.60)	-5.63** (2.84)	0.57** (0.29)	1.13*** (0.28)	-0.65* (0.32)
T4	-0.34 (0.29)	-0.20 (6.98)	-2.84 (3.36)	-0.34 (0.29)	-0.12 (0.28)	-0.39 (0.31)
T5	-0.21 (0.29)	-9.37 (7.17)	-4.78* (2.52)	-0.21 (0.29)	0.21 (0.27)	-0.37 (0.30)
Constant	-0.02 (0.30)	38.57*** (4.95)	21.07*** (3.17)	-0.02 (0.30)	-1.19*** (0.26)	-1.24*** (0.29)

Standard errors in brackets. *** (**, *) indicates significance at 1% (5%, 10%) level.

Figure 4.1. Average tokens given by task

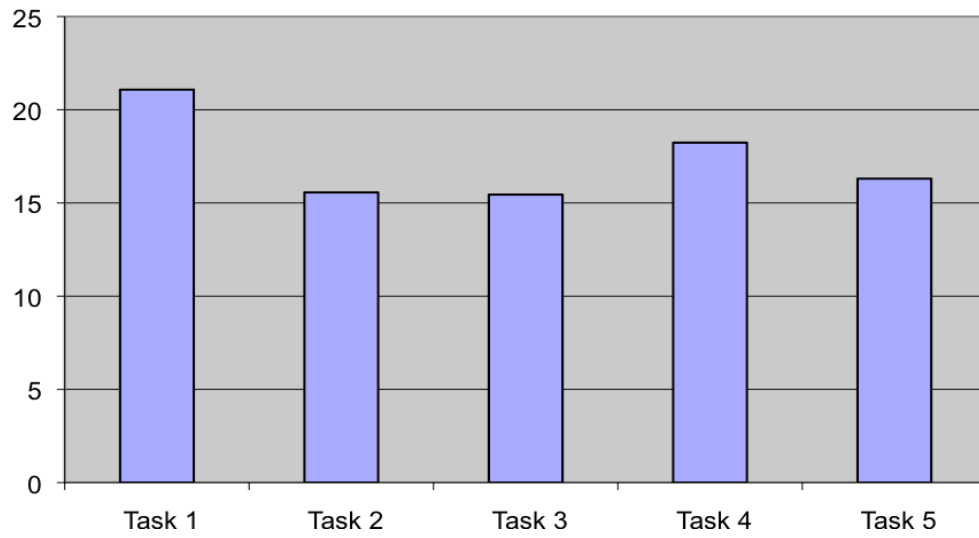


Figure 4.2. Percent of subjects that choose to give non-zero amounts

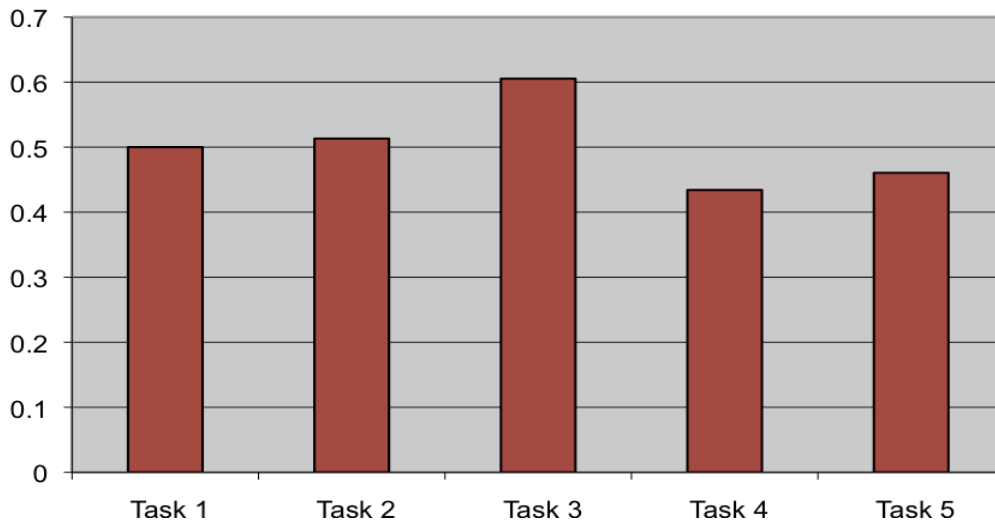


Figure 4.3. Average tokens given, conditional on giving greater than zero

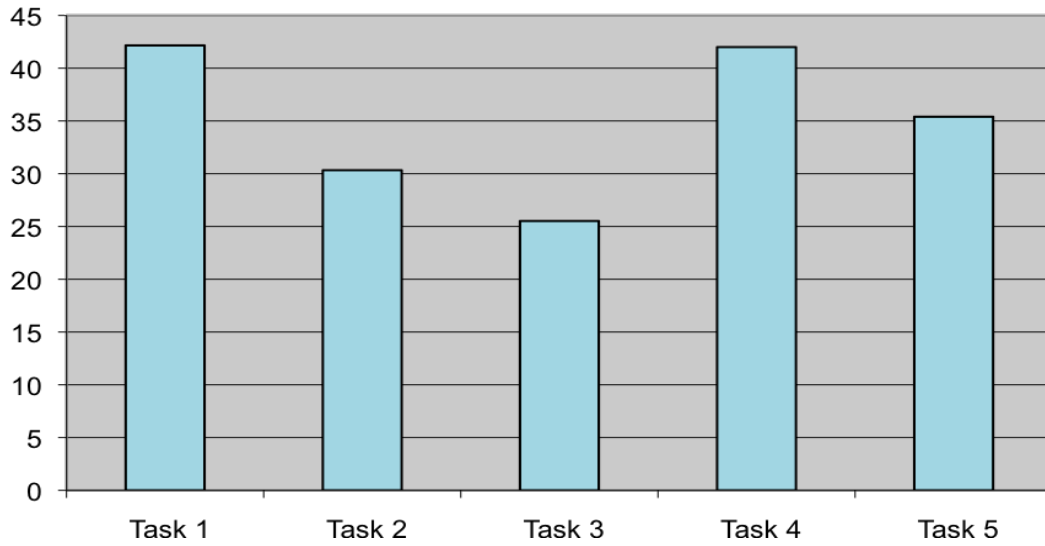


Figure 4.4. Choices and expectations in the respective tasks

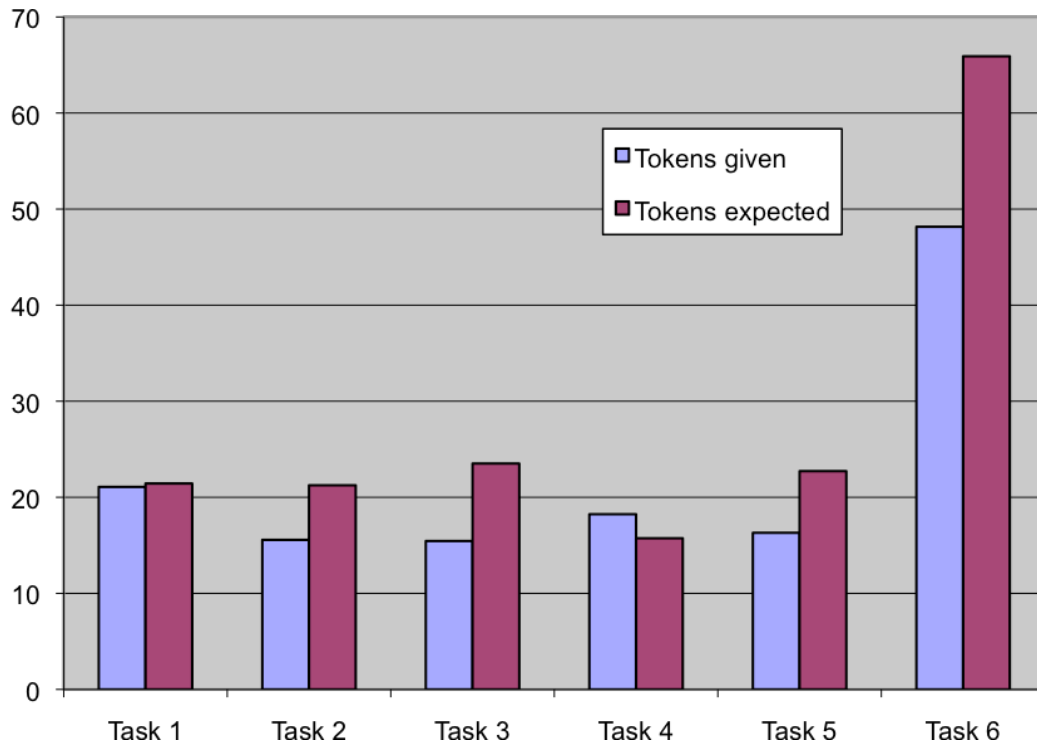


Figure 4.5. Tokens given by treatment, Tanzanian clinicians (bin labels are the lower bound of bin contents).

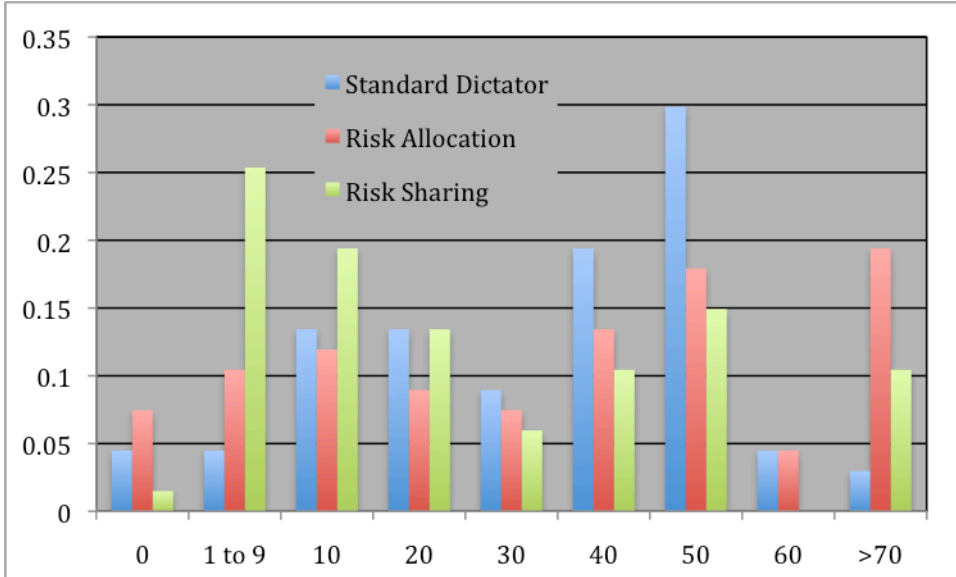


Table 4.13. Summary statistics of dictator allocation choices, Tanzanian sample

	Mean of choices	SD of choices	Median	Mode	% of subjects with x=0	% of subjects with 0<x<50	% of subjects with x=50	% of subjects with x>50
Standard Dictator Game (<i>T1T</i>)	34.66	19.60	40	50	4%	60%	29%	7%
Dictator Game, allocating risk (<i>T2T</i>)	39.74	28.73	40	50	7%	53%	13%	27%
Dictator Game, sharing risk (<i>T4T</i>)	28.53	26.63	50	20,50	2%	75%	13%	10%

The number of dictator (clinician) subjects is 68.

Table 4.14. Summary statistics of recipient expectations, Tanzanian sample

	Mean of choices	SD of choices	Median = Mode	% of subjects with x=0	% of subjects with 0<x<50	% of subjects with x=50	% of subjects with x>50
Standard Dictator Game (T1T)	49.61	22.91	50	0%	36%	41%	23%
Dictator Game, allocating risk (T2T)	50.62	25.88	50	0%	46%	22%	32%
Dictator Game, sharing risk (T4T)	50.39	26.57	50	0%	45%	25%	30%

The number of recipient subjects is 69.

Table 4.15. Difference in tokens given, Tanzanian sample

	Difference in Means	
	Dictator Allocating Risk (T2T)	Dictator Sharing Risk (T4T)
Dictator Game (T1T)	5.08 (0.160)	-6.13** (0.017)
Dictator Allocating Risk (T2T)		-11.21*** (0.001)

Differences in means tested with Wilcoxon signed rank tests. p-values shown in parentheses. *** (**, *) indicates significance at 1% (5%, 10%) level.

Table 4.16. Regressions of choices in tasks on dictator game decisions

Dependent Variable -- Tokens Given to Partner				
<i>Treatment</i> <i>Subjects</i>	Risk allocation (T2/T2T)		Risk sharing (T4/T4T)	
	Clinicians (OLS)	Students (Tobit)	Clinicians (OLS)	Students (Tobit)
Tokens Given in T1	0.79*** (0.15)	0.71*** (0.14)	0.23 (0.16)	0.85*** (0.23)
Constant	12.34** (6.04)	-11.04** (4.86)	20.58 (6.55)	-23.73*** (8.96)
R-Squared	0.28	0.07	0.01	0.04

Standard errors in parentheses. *** (**, *) indicates significance at 1% (5%, 10%) level.

Table 4.17. Maximum likelihood estimates of treatment effects, probits of dictator type on dictators' choices for the different tasks, Tanzanian sample

	Probit Model, Selfish (TG<10 vs TG>=10)	Probit Model, Fair (50<=TG<=55 vs TG<50)	Probit Model, Generous (TG>50 vs TG<50)
Risk Allocation Treatment (T2T)	0.75* (0.063)	-0.48 (0.114)	1.06*** (0.006)
Risk Sharing Treatment (T4T)	1.30*** (0.002)	-0.69** (0.020)	0.15 (0.705)
Constant	-2.46*** (0.000)	-0.59*** (0.006)	-1.67*** (0.000)

Baseline is Treatment 1, the standard dictator game
p-values in parentheses. *** (**, *) indicates significance at 1% (5%, 10%) level.

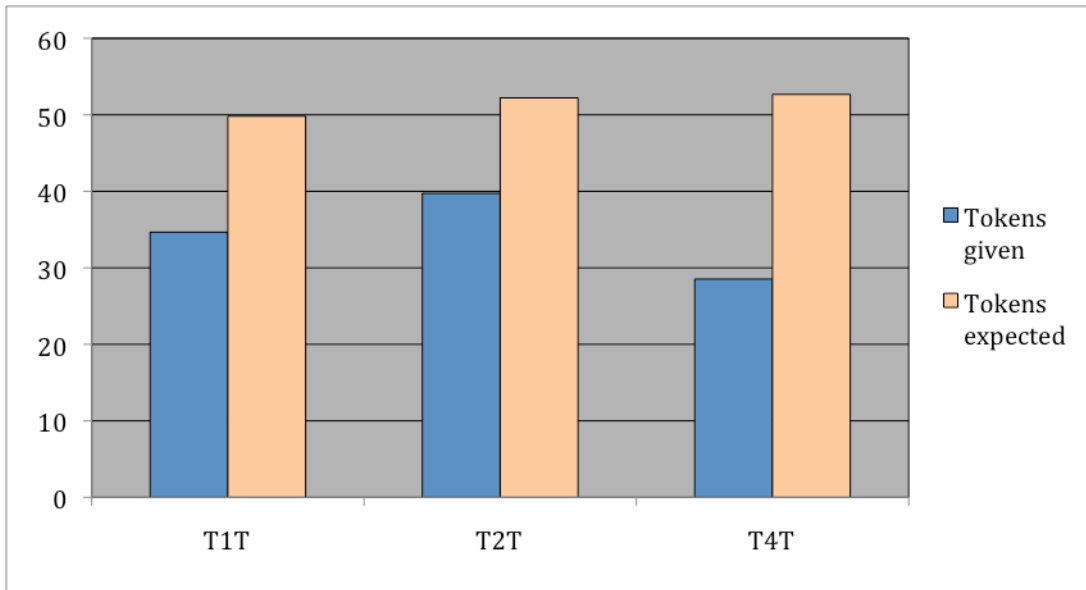
Table 4.18. Linear maximum likelihood estimates of treatment effects; dependent variable is dictators' choices for the different tasks

	Tokens Given (TG), re	Chars1	Chars2	Chars3
Risk Allocation Treatment	5.07 (3.27)	5.45* (3.30)	5.68* (3.33)	5.95* (3.51)
Risk Sharing Treatment	-6.13* (3.22)	-6.00* (3.28)	-5.94* (3.36)	-5.35 (3.45)
Private Facility		0.52 (5.46)	-4.24 (6.30)	-5.55 (6.82)
NGO facility		-5.23 (6.65)	-7.05 (6.53)	-5.86 (7.23)
Clinician Income			-2.46 (2.43)	-5.27* (3.09)
Clinician Age				0.51 (0.35)
Clinician Gender				-4.86 (5.88)
Order of Treatments	0.50 (4.67)	1.25 (4.85)	3.84 (5.10)	4.35 (5.24)
Constant	34.43*** (3.47)	35.43*** (4.00)	44.83*** (9.29)	34.56** (14.53)
R ²	0.03	0.04	0.06	0.09

Baseline is Treatment 1, the standard dictator game

Standard errors in parentheses. *** (**, *) indicates significance at 1% (5%, 10%) level.

Figure 4.6. Distribution of tokens given and tokens expected



Chapter 5 : Pride and Social Identity Based Social Preferences

5.1. Introduction

While some people behave generously regardless of the attributes of others, pride and knowledge about the recipient characteristics may also motivate altruistic behavior (Ellingsen and Johannesson, 2008, hereafter referred to as EJ). This is particularly relevant in the healthcare setting, where doctors' own self-perception and the opinion of patients and peers may influence the quality of care doctors provide. In this chapter we explore the role of social information and pride in determining pro-social behavior. We ask: Does pride change patterns of generosity? How does the response to being chosen differ from simply knowing something about one's partner? We find that pride impacts decision making and that providing information about one's partner reduces selfishness. While previous research has considered the role of partner identity for student samples, it has not been widely investigated among non-student samples. The pride results are the first that we know of addressing that research question. Together these results shed light onto a new aspect of altruism in the workplace in general, and among clinicians in particular.

In this chapter we present results from laboratory experiments that study the role of social information and pride in altruism. We discuss results from a pilot implementation and an implementation with Tanzanian clinicians. The pilot implementation was with a sample from the general public at a university-wide event (on the UMD campus). The implementation in Tanzania occurred in the same sessions as the treatments on risk and

altruism, with the same clinicians. This took place in Arusha, Tanzania. The goal of the pilot was to test the payoff structure, the effectiveness of the experimental design and the logistics of implementation. From the pilot, we learned that the standard dictator game, with a continuous payoff structure, would be more advantageous than the discrete choice payoff structure of the trust game, which was used in the pilot. We also worked out the details of implementing the pride treatment without computers, the details of which are covered in this chapter. The sample size of the pilot bars more than a basic analysis, and we focus most of our attention in this chapter on results from the Tanzanian implementation.

We motivate these treatments with a discussion of the literature on social context and altruism. It has been shown that people respond to reduced partner anonymity when it comes to social preferences. Bohnet and Frey (1999) and Eckel and Grossman (1996), among others, find that knowing more about a partner in the laboratory increases giving in the dictator game. Further, EJ theorize that pride may motivate us to behave more pro-socially, with pride being dependent on co-partner similarity. There is also evidence to this effect showing that doctors, specifically, alter their effort at work in order to gain peer esteem (Leonard and Masatu, 2006). That a clinician offers more effort for a patient when he is being observed by a peer suggests that the peer's esteem motivates increased utility from increased effort, as in EJ's theory. In that theory, decision makers gain utility from altruistic acts toward or performed in front of others who are like them, thus increasing the pride they feel from the action. An extension of this idea is that individuals seek to gain the esteem of those who are similar to them in some way. This research

considers the possibility that patient esteem is important for clinician effort choices and that knowledge of patient identities impact the salience of that esteem.

Group and individual identity are known to be important contributors to economic behavior. Identity affects how people behave in various settings, including professionally (Akerlof and Kranton, 2000; Akerlof and Kranton, 2008). Abundant evidence of type-based discrimination in economic transactions reveals the importance of observable characteristics in how different groups are treated (e.g. Ayres and Siegelman, 1995; Becker, 1971; Das and Sohnesen, 2007). Altruism is also found to be contingent on recipient characteristics (Eckel, 2007; List, 2004). Notably, Bohnet and Frey (1999) investigate how being able to see your partner changes giving patterns. They find that visual identification of partners increases giving in dictator games, but only if it is two-way or accompanied by additional personal information, such as hobbies. Their results are evidence that knowing who one's partner is, beyond what is observable, matters for altruism. They do not test the importance of any specific set of characteristics, but rather focus on the visual identification impact. Eckel and Grossman (1996) evaluate how a specific attribute, deservedness of recipient, changes giving patterns. Their dictators give more frequently and are more likely to give large amounts (as much as their entire endowment) when a charitable organization is the recipient than when the recipient is an anonymous student subject. Notably they use well-known organizations that most decision makers would be familiar with, rather than unknown organizations. Thus individual attributes do impact economic decision making, even when it is not in an individual's own self interest.

Results in this vein are from samples drawn from rather homogeneous populations (university students). No study matches service providers with potential clients in the laboratory. Also, little research has addressed how recipient preferences and opinions impact dictator giving. Holm and Engfeld do look at the partner preferences of recipients in an ultimatum game, but their design does not allow them to determine how proposers' giving changed according to the recipients' partner preferences. Moreover, the importance of pride has been overlooked in empirical work. It is clear that decision makers do respond to co-partner identity, but the relevant set of characteristics is not well established, the samples used thus far are limited and the passive role of the recipient is largely ignored. This chapter fills gaps in the literature by considering the role of pride in dictator decisions and extends the evidence on the importance of social identity to a sample of healthcare providers (i.e. non-student sample), mimicking the client-physician relationship in the laboratory.

The chapter proceeds as follows. First, we describe the pride and social information treatments, mentioned briefly in the introduction, in more detail. Then we report results from both the Maryland Day pilot and the Tanzanian implementation. A final section concludes. We include as an appendix to this chapter a model that aims to formalize the patterns of behavior we observed in the laboratory with respect to social information and pride. The model is based on the EJ theory. It is cast in the framework of clinician decision making, where social similarity between clinician and patient plays a key role. This is followed by a discussion of some suggestive results on the importance of social similarity in clinician workplace behavior.

5.3. The Experiment

We use basic allocation choice experiments (i.e. trust game and dictator game) to explore the importance of pride and social information in individual generosity. While there are slight differences between the pilot of the experiments and the Tanzanian implementation, the basic set-up is that in one treatment (T2) dictators (P1) know the demographic characteristics of their randomly matched partner (P2) and in another treatment (T3) the P2 players chose their P1 partners based on P1's demographic information. Information here includes age, sex, income, years of education, region of birth and area of work for the pilot sample. For the Tanzanian sample we collected information on tribe instead of region of birth. Both implementations include a context-free standard dictator game to measure baseline levels of altruism (T1). For T2, the dictators review the information and make their allocation choice.

The final treatment, T3, tests the impact of pride on giving. In this treatment we allow non-clinician subjects to choose their partner based on the others' characteristics. The characteristics the receiver sees are partner age, sex, income, education and tribe. In T3, we give information about at least two P1 players to each of the recipients. Recipients review the information on possible P1 partners and rank the potential partners according to preference. We then match each P2 with either their first or second choice P1 player. Knowing that they are chosen thus, P1's make the allocation choice. We assume that the partner that has been chosen experiences a positive feeling associated with being chosen. Following the convention of EJ we call this feeling "pride". Note that in this pride

treatment the identity of the P2's remains anonymous. P1's simply know that the partner has chosen them and that the recipient knew their characteristics when making that choice. In this way we induce in the decision makers a feeling of being esteemed for who they are. Results from this treatment reveal if invoking pride changes the nature of the social preferences.

Comparing how the allocation choice changes from one treatment to the next reveals how pride and social identity may impact individual generosity. More importantly, since this is a within design we are able to compare the relative strength of pride and social information at induce changes in giving within individuals. We compare each treatment to a context-free baseline to determine the impact of social information and pride on allocation decisions. Given past research we expect people to give more in the social information treatment. There are no similar experiments that test hypotheses about pride, but based on the EJ theory discussed above we predict giving to also increase under the pride treatment. We do expect P1's to respond to being chosen and that there will be more dictators choosing the fair allocation than in the baseline treatment.

To summarize, the aim of the experiment is threefold: to determine if a clinician exhibits pro-social behavior toward (potential) patients in a lab; to establish the result that knowledge of partner identity increases giving in this sample; and to reveal whether induced pride changes the clinicians' degrees of pro-social behavior. In Chapters 6 and 7, we combine data on clinicians' types, as measured in the laboratory, with field data on subjects' actual workplace effort to determine the extent to which social preferences impact workplace decision making. In what follows we first describe the specifics and

results of the Maryland Day pilot. We then present the details and results from the Tanzanian implementation. The final section concludes the chapter.

5.4. Maryland Day pilot implementation: General public

The experiments in the Maryland Day pilot study were modified involuntary trust games (ITG) McCabe, Rigdon and Smith (2003), where subjects were asked to choose between two different token allocation options for splitting up a token endowment between themselves and a partner. In the involuntary trust game a decision maker (P1) must decide between two allocation options to distribute an endowment between himself and an anonymous partner (P2). It is different from the voluntary trust game (VTG) in that the recipient partner is completely passive; in the VTG P2 makes the first move of whether or not to trust his partner, P1. If P2 decides to trust, P1 makes the final allocation decision. If P2 decides not to trust, a default allocation obtains. Since P2 is passive in the ITG the only decision in the game is made by P1, and P1 makes that choice absent any norms of reciprocity that might be factors in VTG decision-making. Aside from the importance of the choice to trust or not trust, the game's other distinguishing feature is the pay-off structure. As in McCabe, Rigdon and Smith (2003) decision makers in our experiment chose between an option where both players get 25 tokens and an alternative option where the decision maker gets 30 tokens and their partner gets 15. Note that the fair option, where both players get 25 tokens, is also the socially efficient optimum. This means that preferences for fairness, efficiency or both may motivate a decision maker to choose that option. In other words, the design tests for other-regarding preferences but does not distinguish the specific type of other-regarding

preferences. This ITG treatment acts as a baseline in the experiment. Subsequent treatments are variations on this standard game, as described above.

Pilot treatments were run from 10am to 2pm on April 24, 2010 at Maryland Day on the University of Maryland campus. Maryland Day is an annual festival-like event hosted by the University of Maryland. The event attracts people of all ages and family situations from among the student body and the surrounding communities. The event is set up like a fair, with booths featuring different groups and departments on campus. All attractions are outdoors; this included the pilot implementation. Given this format, the over-riding expectation of attendees is to browse the booths without spending too much time at any one place. The upshot of this is that each session run with attendees consisted of only one of the three treatments described above. All comparisons between treatments are thus between comparisons. While between comparisons are not as ideal as within comparisons, the results from the pilot nonetheless point to important patterns that we explore further in the within design of the full implementation (described below). Sessions/treatments were run sequentially and each session lasted roughly 15 minutes. Signs and flyers advertising the event helps draw participants, as did members of our team who went up to individuals passing by to invite them to participate. Participation was voluntary, so we expect a selection bias. Despite the limitations of running the pilot at the Maryland Day event, it gives us insight into how to run the experiments in Tanzania with a non-student sample.

Based on the literature, we expect that decision makers in T2 will be more likely to choose the fair allocation than in T1, even though P1 is not subject to reciprocity norms as the result of a trusting move by P2. In some sense, the social information primes the

decision maker to recall social norms that dictate greater generosity when there is a “human face” for the recipient. The second variation, called the pride treatment, allows P2 to choose their P1 partners. If being chosen is salient for the decision makers, we expect higher giving in T3 than in T1. In line with McCabe, Rigdon and Smith (2003) we estimate that at least one third of decision makers will choose the fair allocation. Thus we predict that priming of social norms (T2) and inducing pride (T3) will decrease the number of participants choosing the selfish (but not purely selfish) allocation option.

Each player’s own characteristics represented their identity to the other players for the pride and social information treatments. We used a form to collect data on the subjects’ actual age, sex, income, education level, region of birth and area of work. Participants filled out the form at the start of each session for treatments 2 and 3. In order to avoid introducing unnecessary bias in the baseline, the form was filled out at the conclusion of the session for treatment one.¹¹ A copy of the form used to collect these data is included in the Appendix of the dissertation. The information was not sufficient for anyone to identify who their partner was. The subjects in the Maryland sample were diverse, with an age range of 18 to 76 years and incomes from \$0 (students) to \$200,000 per year. Subjects included students, families, seniors and younger single people. No one under 18 was permitted to participate. Table 5.1, Table 5.2, and Table 5.3 summarize the participants’ characteristics.

Subjects completed consent forms and we read instructions out loud at the start of all treatments. They also received a printed copy of the instructions so that they could read

¹¹ Due to participants being in a hurry, combined with experimenter error, many treatment one subjects ended up leaving the booth without completing the characteristics form. Since the full implementation was a within study, we did not have to adjust for this problem beyond the pilot.

as we read aloud. Dictators and receivers were already separated at this point. For treatment 1 (T1) we followed directly with the decision task. Dictators received a decision task form with a summary of the decision task and a place for them to record their allocation choice. Recipients received a similar form that also described the decision task, but their job was to report what they expected their partners to decide. The recipient task was included to avoid boredom and the task was not incentive compatible. Participants recorded all decisions (expectations) using paper and pencil. This was the most effective way to accommodate the outdoor setting and also to mimic the situation we would face in Tanzanian/full implementation, absent the typical computer resources available for laboratory experiments.

In order to preserve anonymity between subjects, we did not allow them to mingle before the experiment. If a family participated we tried to assign all family members to be either P1 or P2 players, rather than distributing them across player types. While this could lead to bias in that some families might be more or less selfish (i.e. a shift in generosity compared to average), it was more important to ensure that the giving patterns could not be confounded by relationships within a family. Aside from not allowing family members to be matched with each other, assignment to be P1 or P2 was done on a first come first serve basis, alternating assignment into each group. The number of participants in each session was under 10 and so distributing people in this way did not require any sophisticated procedure. Using multivariate ANOVA we reject the hypothesis that subjects are randomly distributed according to age, sex, income and years of education between player types at a 3% level of significance. Inspection of the summary statistics shows that average age of the P2 subjects is 14 years more than that of the P1 players.

This difference is driven by four subjects over the age of 60 in the P2 group; there are no players above 60 in the P1 group and below age 60 the age distributions are similar. Removing age from the MANOVA procedure we cannot reject the hypothesis that subjects are randomly distributed according to characteristics between player types.

At the end of each session we collected the decision sheets and distributed cards with IDs printed on them according to the ID number on each person's decision sheet. Winnings were determined in private and we called participants up by ID number to distribute winnings to each participant according to the partner pairing and allocation choices. Partner pairing in T1 and T2 was random. Since the experiment was done without the use of computers, the random matching was done in advance according to a set of experiment ID codes. Codes were preprinted on all decision sheets so that upon collecting the sheets the proctor would be able to easily determine who was matched with whom. In T3 the recipients chose their partners. A similar technique of pre-coding was used but it did not involve setting up the matching before hand.

Payoff options in the game were in experimental currency units (ECUs). Subjects received the show-up fee and their experimental earnings in lottery tickets rather than cash.¹² For every 10 ECU earned they received 1 lottery ticket. The show-up fee was 2 lottery tickets. Including the show-up fee, participants each earned 5 lottery tickets on average. Subjects learned the translation of ECUs to lottery tickets in the instructions that we read at the beginning of each session. The lottery tickets represented chances to win a 3rd generation iPod shuffle, an item with a retail value of approximately \$55. A lottery was drawn at the end of the session to select a session winner. We collected winner

¹² This detail was a result of restrictions put in place by "Maryland Day".

contact information from each session winner and at the end of the day held a grand prize drawing. The winner from each session was represented in this grand prize drawing with one ticket. Three winners were selected from this final lottery. Winners received an iPod shuffle, which we mailed to them.

The tickets were double raffle tickets. Each participant received one side of each of their raffle tickets and the other side they put into a large paper bag. After all tickets were distributed for that session, and the corresponding pairs collected in the paper bag, the bag was shuffled and one of the participants was recruited to draw the winning ticket out of the bag, which we announced out loud to the group. Participants were then dismissed. The person with the winning ticket met with the proctor before leaving the experiment area and the proctor recorded their name and phone number. We discarded all tickets from that session except the winning ticket, which went into a separate bag reserved for the final lottery. At the end of the day we drew the final three winners from the bag of session-winning tickets.

The pilot results show some interesting patterns, though the sample size in each treatment was small and significance is low. Nonetheless, the patterns are in line with the literature and informative of potential trends that we discuss in more detail with the data from the full implementation. Half of the decision makers choose the fair allocation in the baseline treatments (ITG with no embellishments) – our participants prove to be less selfish than the sample of university students in McCabe, Rigdon and Smith (2003), 1/3 of whom chose the fair options in the ITG. Meanwhile 58% and 63% chose the fair allocation in T2 and T3 respectively. Thus it appears that simply providing the social information does motivate switching to the fair allocation in this mixed population, as has been reported

for the dictator game among student samples in Bohnet and Frey (1999) and Eckel and Grossman (1996). Inducing pride also increases generosity by 13% among decision makers. But an unpaired t-test reveals that the difference in giving between the pride treatment and the baseline treatment is not significant at conventional levels. Recipient expectations of generosity also were higher in T2 and T3, though notably more people expected to receive the fair allocation in T2 than in T3. In fact, recipients in the social information treatment were much more likely to expect the fair, welfare maximizing allocation than recipients in other treatments. There is less than 1% significance for $T1 < T2$ and 2% significance for $T3 < T2$. T1 is not found to be different from T3. We tested each of these relationships using unpaired t-tests. Results are reported in Table 5.5. This is the opposite of the pattern between treatments among decision makers, implying that recipients underestimated the influence of being chosen on selfishness. Looking at giving by demographic characteristics we find two strong results: (1) across all treatments, more educated decision makers are 35% more likely to choose the selfish allocation (11% significance) and (2) older decision makers are 2% more likely to choose the fair allocation (10% significance). A multivariate analysis of variance confirms no pattern to assignment among treatments based on observables. We conclude that the patterns in giving by demographic characteristic are not being driven by unequal assignment of certain types of people to the different treatments. But since subjects are not randomly distributed by age to player types, it is possible that the older people who are more generous may be driving the (insignificant) pattern of higher generosity in certain treatments. Indeed, the 2 of the 4 people over 60 years old are P2 subjects in T2 and 2 of them are P2 subjects in T3. Further, from Table 5.4 we see that the number of

people choosing the fair allocation in both T2 and T3 is exactly 2 greater than the number of people choosing the selfish option. Hence, it may be that the skewed age distribution is responsible for what appear to be treatment effects. This issue will can be addressed in the future implementations by carrying out a within design rather than a between design.

All in all the pilot was successful in shedding light on effects of pride and social identity on altruism and in revealing weaknesses in the experimental design. Net the lack of significance for some results, we interpret our outcomes as supporting the general hypotheses of the value of social information and pride in motivating pro-social behavior. Drawbacks to this pilot that were addressed in the Tanzanian implementation include the small sample size, the between nature of the design and the discrete payoff structure. We follow-up on these results and address the weaknesses in the design in the full implementation discussion that follows.

5.5. Full implementation: Tanzanian clinicians

Clinicians in Tanzania constitute an ideal subject pool for this investigation because of the existing body of literature on this group and because of the importance of understanding motivations of this group. Poor overall quality of the health care systems in many developing countries is often attributed, at least in part, to under qualification of the health workers. There is evidence, however, that even given low levels of education and experience, health workers underperform relative to their already low ability in outpatient consultations (Leonard and Masatu, 2005; Leonard and Masatu, 2007). We also know that clinicians in Tanzania try to “buy” approval from their peers with increased effort levels when under scrutiny (Leonard and Masatu, 2006). It is possible

that the esteem-seeking behavior seen in response to the presence of other clinicians may occur to some degree with patients. And while fellow clinicians are of similar social status as the clinician being observed, the attributes of patients are highly varied. Hence, in determining how much effort to exert for any given patient, the clinician is responding to various factors: how responsive he is to the patient's esteem of him, whether the patient's identity matters to him (differentially from the illness) and his own innate preference for fairness or identifying with the profession's value of service to others. Thus the clinician sample is well suited to studying the role of pride and social information in altruism. The succeeding discussion first details the differences between the pilot and the Tanzanian implementation and then covers the results of the Tanzanian implementation.

The structure of the experiments that we ran in Tanzania was similar to the Maryland Day pilot, with a few exceptions. First, the baseline game was the standard dictator game, rather than the involuntary trust game (ITG). The difference between these two games is that the ITG presents participants with two alternative discrete payoffs to choose from, whereas the choice is continuous in the dictator game. We made this change in order to better capture nuances in selfish and generous behavior, beyond the switching between selfish and fair observed in the pilot. Second, in the Tanzania experiments, payoffs were in terms of local currency, Tanzanian shillings, Tshs. Participants received 150 Tshs for every token earned in a randomly selected payment round. The maximum possible earnings from any of the rounds, 15,000 Tshs, was equal to about three day's of work for non-clinicians reporting the median income and three day's work or less for clinician subjects. For most clinicians in the sample, 15,000 Tshs is roughly what they earn in a

day. Third, all the participants played all the different treatments in the Tanzania implementation, allowing for within comparisons and more extensive analysis on the interaction of pride and social information responsiveness. The other advantage of the between design is the additional statistical power we get from a similar sample size (67 clinicians participated in the experiment, compared with 52 subjects on Maryland Day). We also made sure to have a larger sample size. Fourth, since we were able to bring the clinician subjects into a classroom we were better able to preserve anonymity, as clinicians and their non-clinician partners never came into contact and were only able to see each other from a distance.¹³ Lastly, the clinicians did two additional treatments beyond what was piloted at Maryland Day. These additional treatments were piloted separately and we discuss that pilot and the results from the clinicians in Chapter 4.

To clarify the difference in the payoff structure, the experiments were modified dictator games, where subjects were asked to allocate 100 tokens between themselves and a partner. As in the pilot, in one treatment (T2) dictators (P1) knew the demographic characteristics of their randomly matched partner (P2) and in another treatment the P2 players chose their P1 partners based on P1's demographic information. Treatment 1 (T1), the experimental control, was a standard dictator game. Partners P1 and P2 were assigned randomly. P1 decided on an amount to give P2. We expect to see conventional results for this type of game, with substantial portion of the population giving selfish amounts, and another pooling of giving at the 50/50 split. In treatment 2 (T2), P1 and P2 are again randomly assigned. Characteristics of P2 were revealed to their P1 partner. This

¹³ Allowing clinicians and non-clinicians to see each other from a distance was an important step in establishing with the subjects that their partners were real and that they were from a certain sector of the population (which one could discern at the distance from the clothing each group habitually wore).

treatment mimics the daily situation of receiving a patient. Information available to dictators in T2 included the sex, age, education, income, area of work and tribe of their partner. P1 decided on an amount to give P2 based on the characteristics of the other. We expect that both the dictator's own attributes and those of their partner may impact giving decisions. Informed by the pilot as well as previous studies showing the importance of social identity in motivating pro-social behavior, we hypothesize that a clinician will behave likewise and increase giving in the presence of social information. T3 was as in the pilot, with recipients choosing partners based on their characteristics. P1 then makes the (continuous) allocation choice knowing they have been chosen based on their characteristics. Given the results from our pilot we expect that inducing pride will increase giving compared to the baseline and that clinicians will be more likely to choose the fair allocation in this treatment than in the baseline and in the social information treatment.

The experiment allows us to answer: if you choose me from among a group with diverse characteristics do I give more money? Do I give you more money if I know something about you, than if I do not know? If knowledge of another's characteristics promotes greater amounts of sharing, we can conclude that these clinicians resemble other populations that responded to decreased anonymity in the laboratory. We can also explore the implications that this social attitude has on workplace effort. If they increase their giving when another player chooses them, we can conclude that the feeling induced by being chosen impacts the expression of a clinician's social preferences, as would be predicted by the diverse theories discussed above, and most explicitly predicted by the EJ theory.

5.5.1. Main results

Recall that the contribution of the experiments is three-fold: 1) to test a new treatment with induced pride and compare the relative importance of pride with social information, 2) to compare this population (professionals with non-professional partners) to others (university students) using the dictator game and the dictator game with social information, and 3) to generate measures of social attitudes in the presence of social information and pride for use in the analysis of the field data. Table 5.7 presents a summary of giving across treatments. Overall, we see that increased personal information about recipients induces more giving than in the baseline. This is consistent with previous findings discussed above. Also, when compared with baseline, instilling pride in givers increases giving. Differences between T1 and T2 averages and T1 and T3 averages are significant at the 5% level using Wilcoxon signed-rank test¹⁴. Average tokens given in T2 is also greater than in T3, but the difference is not significant. Results from these tests are presented in Table 5.9. While both pride and social information increase giving relative to the baseline, exposure to partners' personal information elicits more generosity than instilling pride in the giver. Furthermore, those who do not respond to decreased partner anonymity are less responsive to induced pride; we cannot reject equality of the underlying distributions of giving in T1 and T3 in the sample of those who do not respond to T2 with increased giving. Conversely, those who do respond to decreased anonymity are also much more likely to respond to pride (we reject equality of T1 and T3 giving at $p < 0.001$ using a Wilcoxon signed rank test).

¹⁴ Figure 2 suggests that the results are not distributed normally and rather are right skewed with a heavy left hand tail. Thus I opt for the non-parametric Wilcoxon signed-rank test, rather than the more powerful t-test.

Even with the standard dictator game resulting in nearly 30% of subjects choosing the fair allocation, subjects did not remain fixed at that level of giving across treatments. Unexpected is that the percentage of dictators choosing the fair allocation goes down from T1 to T2 in part because subjects act with more generosity in T2; subjects who start off at the fair allocation who want to increase giving in response to the partner information actually do allocate more to their partner than themselves rather than remain at the 50/50 allocation. Giving greater than 50% of the endowment is unusual in dictator games and it is suggestive of people being unfamiliar with the game setting such that their baseline giving is high. Also, in the pride treatment dictators continue to allocate more than 50 tokens, with many moving from below 50 to the fair allocation. Thus although the fair allocation is prominently represented, giving varies quite a bit from treatment to treatment.

Adjusting for unconventionally low levels of selfish behavior, we still find evidence that distributions of giving vary from treatment to treatment. The paucity of zeros (Table 5.10) in all treatments is not a surprise in the context of this sample. Tanzanian culture is focused on being polite, which invariably biases experiment and survey responses upward (Henrich et al., 2001; K. L. Leonard, 2008). It also potentially speaks to the limits of running lab-style experiments in this setting. While we did ensure that decisions were anonymous, limits of space meant that decision makers were seated somewhat close together and decisions had to be written down on a piece of paper rather than typed into a computer. Thus dictators may have felt that their decisions could be seen by peers sitting near-by. As such the within results are much more meaningful than the between results (comparison across treatments rather than within a treatment). Furthermore, since we did

not use a double blind experimental design, we did not expect many purely selfish decisions.¹⁵ If we consider amounts given of 10 tokens or less as the selfish choice, percent of selfish decision by treatment goes up to 21%, 18% and 21%, respectively, which is close to commonly reported patterns of giving in dictator games. Notable, however, is that the number of purely selfish decisions decreases from treatment to treatment, while percent giving 10 or fewer tokens remains relatively constant. Distributions of giving in this “selfish range” are reported in Table 5.10. Together with the overall difference in means, this suggests that the distribution of giving is shifting between treatments.

Frequency distributions of giving in the three treatments are presented in Figure 5.2. Tokens given results in T1 and T3 have bimodal distributions. Secondary peaks are at 10 tokens given, further suggesting that there are two primary types in this population: those that give 10 and those that give 50. T2 does not appear to have a secondary peak, suggesting a continuum of types in terms of response to social information. While the distribution of giving for T2 is somewhat similar to that of T1 and T3, T2 has fewer subjects choosing the equal allocation and at least 10% more players giving over 50% of the endowment, which contributes substantially to pushing the T2 mean higher than the other two treatments.

Ordinary least squares with treatment dummies reveals that giving in both T2 and T3 is substantially higher than in T1 (Table 5.11). Additional regression results show that subjects are no less likely to make a selfish choice in the social distance/social

¹⁵ While ideal for cross study comparisons, the double blind design was not practical in this setting. The game procedures needed to be as simple as possible since the subjects are quite unfamiliar with the concept of experimental games.

information treatment. Selfish here is defined as fewer than 10 tokens to one's partner, to correct for the upward bias in the sample. Generosity, defined as giving more than 50% of the endowment to one's partner, is prevalent and more likely in T2 and T3 than in T1. Finally, and most compellingly, pride may influence subjects' likelihood of giving the fair amount ($p = 0.106$); the same is not true for social information. This result is, however, very sensitive to how we define fair giving. For example, if we restrict the definition of fair to those who give 50% the coefficient on the pride treatment is no longer significant. Nonetheless, this is an interesting result because while pride does not increase average giving over social information, it may increase fair giving. This may be because being chosen by one's partner takes the charity aspect away from the allocation task and brings it more into the realm of sharing with an active partner – decision makers are free of the social influence that may encourage generosity to strangers.

5.6. Conclusion

In this chapter we looked at two factors that may play a role in the expression of altruism in the workplace: social identity and pride. We studied two samples: general public participants at a fair-like event at the University of Maryland and medical clinicians in Arusha, Tanzania. Past research suggests that social identity influences generosity among university students, but is silent about the impact of pride. We had no reason to suspect that social identity results would differ in our samples and indeed we found that dictators tended to give more when we provided them with socio-demographic information about their partners. Among the clinicians, social information significantly increases the likelihood of a generous allocation compared to the standard dictator game. But it does not significantly reduce the likelihood of a selfish allocation. With the Maryland Day

sample social information also motivated increased generosity, but results are not significant, which we attribute to the small sample size. The most novel result we present is that inducing pride does increase average giving, even when the dictator knows nothing about their partner. Notably, induced pride appears to encourage more participants (in either sample) to revert to giving 50-55 tokens than the presence of social information.

Results from the pride treatments are suggestive of EJ's theory of social preferences wherein the interaction of social identity and esteem motivate pro-social behavior. Interestingly, induced pride appears to be important even when the partners are of a lower socio-economic class, as in the experiments with the clinicians and "patients". We juxtapose these laboratory results with the work of Leonard and Masatu (2006) showing clinicians' response to peer esteem. Apparently clinicians are responsive to pride stimuli from both patients and peers, though it remains to be seen whether these sources of pride are complements or substitutes. Thus this research finds that patient esteem is important for clinician generosity in the laboratory.

This chapter contributes to the literature with a novel experimental treatment testing the impact of pride, unique samples and approximations of real world relationships in the experimental design. We study both a general population sample (pilot) and a sample of clinicians (full implementation). In the full implementation the treatments mirrored the forces potentially influencing clinicians in their everyday workplace decisions. Our results both backup and build on existing findings. Further, our results speak to the accuracy of theoretical models that include individual heterogeneity of social preferences and the idea that interpersonal aspects of decision making influence pro-social behavior.

5.7. Tables and figures

Table 5.1. Respondent characteristics, T2 and T3 (demographic data was not collected for T1)

Characteristics		Respondents	Dictators
N		23	23
Sex			
	Male	8	9
	Female	12	11
Age			
	Mean	46.55	32.58
	St. Dev.	(17.58)	(13.21)
	Minimum	18	18
	Maximum	76	59
Education (years)			
	Mode	16	16
	Minimum	12	11
	Maximum	21+	21+
Income ^a			
	Proportion of students (income < \$20,000)	20%	39%
	Median	\$50,000	\$75,000
	Minimum	\$6,000	\$8,000
	Maximum	\$200,000	\$150,000

^a Income is in US dollars reported by year. The mean, standard deviation, max and min do not include students because they heavily skew the distribution and make it more difficult to summarize the tendencies of the data.

Table 5.2. Maryland Day participant characteristics by player role

	Number of subjects	Mean age	Mode years of Education	Percent Female	Median income (USD)	Proportion of students
Dictator	12	50 (15.5)	16 (3.1)	42%	\$48,000	42%
Recipient	8	41 (18.9)	18 (2.5)	88%	\$32,000	38%

Table 5.3. Maryland Day dictator characteristics by treatment

	Number of subjects	Mean age	Mode years of Education	Percent Female	Median income (USD)	Proportion of students
Standard Dictator Game (T1)	6	NA	NA	NA	NA	NA
Dictator Game with Information (T2)	12	50 (15.5)	16 (3.1)	42%	\$48,000	42%
Dictator Game with Pride (T3)	8	41 (18.9)	18 (2.5)	88%	\$32,000	38%

Standard deviations are reported in parentheses.

Table 5.4. Summary of dictator game giving, UMD Maryland Day

	Number of subjects	Number choosing (25,25)	Number Choosing (30,15)	% choosing (25,25)	% choosing (30,15)
Decision Maker Giving					
Standard ITG (T1)	6	3	3	50%	50%
ITG with Information (T2)	12	7	5	58%	42%
ITG with Pride (T3)	8	5	3	63%	38%
Recipient Expectations					
Standard ITG (T1)	6	1	5	17%	83%
ITG with Information (T2)	12	10	2	83%	17%
ITG with Pride (T3)	8	3	5	38%	63%

Table 5.5. Differences in tokens given, UMD Maryland Day

	Difference in Percentage Choosing Fair Allocation		Difference in Percentage Expecting Fair Allocation	
	Dictator with Information (T2)	Dictator with Pride (T3)	Dictator with Information (T2)	Dictator with Pride (T3)
Dictator Game (T1)	8%	13%	67%***	21%
Dictator with Information (T2)		5%		- 46%**

Differences in means tested with unpaired t-tests.

*** (**, *) indicates significance at 1% (5%, 10%) level.

Table 5.6. Participant characteristics, Tanzanian sample

Characteristics	Respondents	Dictators
N	69	68
Sex		
Male	31	47
Female	38	20
Age		
Mean	32.71	42.16
St. Dev.	(10.74)	(9.53)
Minimum	18	24
Maximum	66	65
Education (years)		
Mean	9.00	16.14
St. Dev.	(2.98)	(1.69)
Minimum	4	11
Maximum	19	22
Income ^a		
<100	47	0
100-200	13	4
201-300	1	12
301-400	2	23
401-500	0	11
>500	0	19

^a Income is thousands of Tanzanian shillings (Tshs) reported by month. \$1USD is approximately equal to 1300 Tshs.

Table 5.7. Summary statistics of dictator allocation choices, Tanzanian sample

	Number of subjects	Mean of choices	SD of choices	Median	% of subjects with x=0	% of subjects with 0<x<50	% of subjects with x=50	% of subjects with x>50
Standard Dictator Game (T1)	68	34.66	19.60	40	4%	59%	29%	8%
Dictator Game with Information (T2)	68	41.84	25.53	40	2%	52%	24%	22%
Dictator Game with Pride (T3)	68	39.34	23.48	50	2%	47%	34%	17%

Mode is equal to 50 tokens given for all treatments.

Table 5.8. Summary statistics of recipient expectations, Tanzanian sample

	Number of subjects	Mean of choices	SD of choices	Median	% of subjects with x=0	% of subjects with 0<x<50	% of subjects with x=50	% of subjects with x>50
Standard Dictator Game (T1)	69	49.61	22.91	50	0%	36%	41%	23%
Dictator Game with Information (T2)	69	55.46	24.62	50	0%	30%	26%	44%
Dictator Game with Pride (T3)	69	48.91	22.23	50	0%	41%	29%	30%

Mode is equal to 50 tokens given for all treatments.

Table 5.9. Differences in tokens given, Tanzanian sample

	Difference in Means	
	Dictator with Information (T2)	Dictator with Pride (T3)
Dictator Game (T1)	6.82** (0.015)	4.68** (0.028)
Dictator with Information (T2)		-2.50 (2.65)

Differences in means tested with Wilcoxon signed rank tests. p-value in parentheses. *** (**,*) indicates significance at 1% (5%, 10%) level.

Table 5.10. Distribution of selfish giving, by treatment

Giving Range	Dictator game	Dictator with information	Dictator with pride
0	3	1	0
1 to 9	3	4	7
10	8	7	7
0-10	14	12	14

Figure 5.1. Empirical distributions by treatment

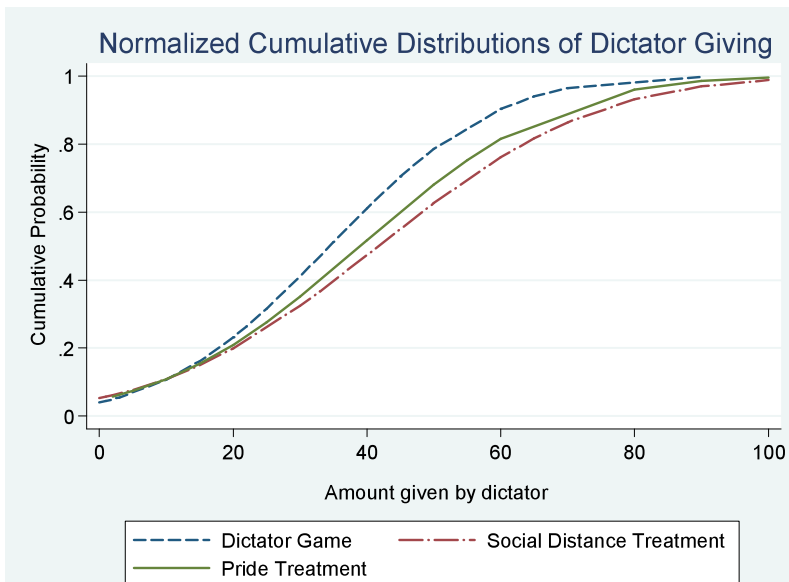


Figure 5.2. Frequency distribution of tokens given across treatments T1, T2 and T3

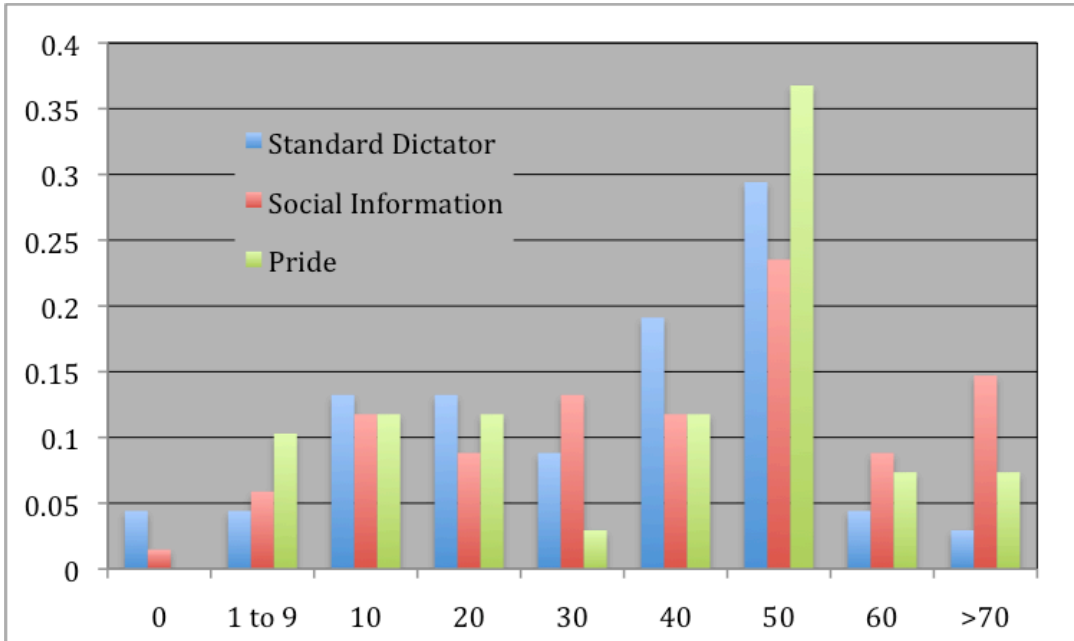


Figure 5.3. Distributions of tokens given and tokens expected

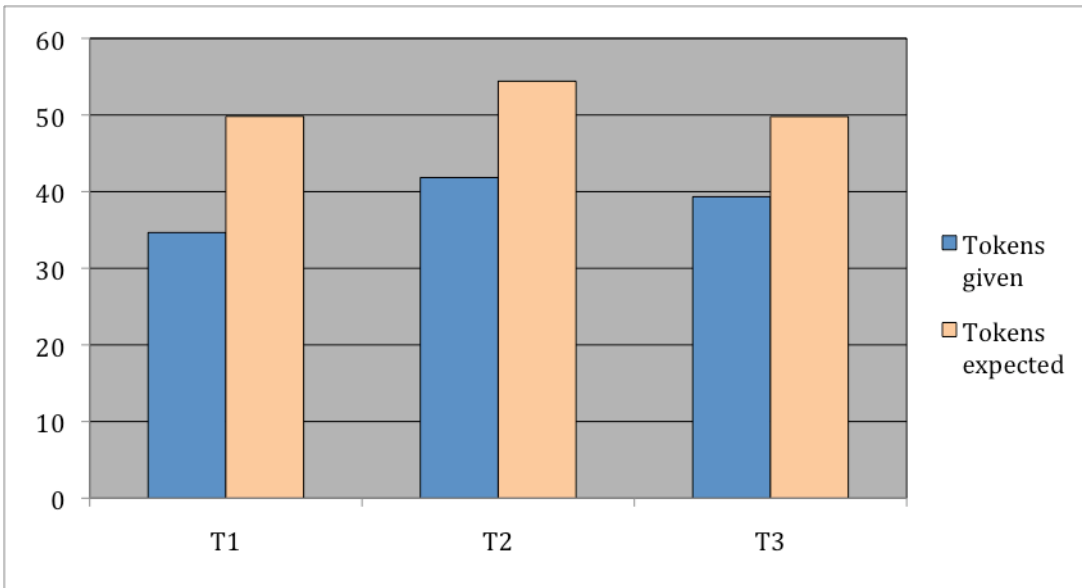


Table 5.11. Maximum likelihood estimates of treatment effects with individual random effects (column 1) and probit models (columns 2-4) of dictators type on dictators' choices for the different tasks

	Linear Random Effects Model, Tokens Given (TG)	Probit Model, Selfish (TG<10 vs TG>=10)	Probit Model, Fair (TG>=50 vs TG<=55)	Probit Model, Generous (TG>50 vs TG<50)
Social information Treatment	7.18*** (2.48)	-0.21 (0.47)	0.12 (0.30)	1.11* (0.49)
Pride Treatment	4.68** (2.31)	-0.18 (0.44)	0.47 (0.29)	1.30* (0.58)
Constant	34.66*** (2.51)	-2.74*** (0.68)	-0.69*** (0.26)	-2.32*** (0.66)

Baseline is Treatment 1, the standard dictator game

Standard errors in parentheses. *** (**, *) indicates significance at 1% (5%, 10%) level.
p-value for the pride treatment in the “fair” probit is 0.106.

5.8. *Appendix*

Results from this chapter showed that social information and pride are meaningful social attitudes in evaluating variation in clinician effort. The idea for the experiment was heavily influenced by the EJ theory of esteem-based altruism. The main results of the chapter test hypotheses about presence of social information and pride. A related hypothesis is that the attributes of the patient relative to the clinician's own attributes help to determine the clinician's generosity in the laboratory. We refer to this as the "social distance" between the clinician and the patient. We also use the term "social similarity" interchangeably with "social distance". In this appendix we develop a model to formalize some of the trends we see in the main results in the context of social distance. The model is an adaptation of the EJ model. The primary difference is that in this model social distance plays a central role in motivating responsiveness to pride and social information. The model suggests that the salience of another's esteem is a function of the social distance between the giver and the receiver. In contrast, the EJ theory considers the similarity in altruistic posture between giver and receiver. In what follows we first motivate the case for social distance as a meaningful economic concept. We then present the model. Lastly, we present some suggestive results from the laboratory experiments. Note that the experiments were not designed to test the social distance hypothesis and any correlations between giving and social similarity are not statistically identified. Nonetheless, the trends are suggestive of the adapted EJ model.

From the literature presented in the main body of this chapter and from our results we see that decision makers clearly care about the identity of others in economic interactions. Social distance may be a driving force behind such preferences. But evidence about the

impact of similarity between actors is mixed. When given the chance to choose partners, subjects show clear preferences and may even prefer to choose partners who are like them (Holm and Engfeld, 2005). In the same study, however, giving levels or frequency did not change with partner similarity. (2009) considers the idea that like-prefers-like in terms of the professional peer group. He finds in-group behavior to be more generous than out-group behavior. Bergstressor's results thus support the hypothesis that social distance in terms of group membership matters. Bohnet and Frey (1999) show that two-way visual identification increases giving, but it is not clear whether that is because of shared characteristics between the co-partners or purely because partners were no longer anonymous. In their trust game experiments Eckel and Wilson (2003) find that decision makers are more likely to choose a trusting move when they are more similar to their partners. Similarity is measured according to sex, favorite color and hobbies. Yet they do not find sex of decision makers or partner's sex to be significant in determining the probability of a trusting move. Hence while social distance does appear to impact generosity in the laboratory, the evidence is mixed and the dimensions along which it is most influential have not been identified.

5.7.1. A Model of behavior for social preferences in clinician decision making

The following model describes a 2-person game that potentially underlies the behavior in our experiments exploring pride and social information. We base the model on the real world interaction of a clinician (player 1) with their patient (player 2) so as to maintain links to the field environment and the data that have inspired it.

Players are heterogeneous and we allow heterogeneity to be multidimensional along subject attitudes and characteristics. Attitudes and characteristics together make up a player's attribute set, ω_i . A player's attribute set has observable elements, λ_i , and private elements, θ_i , where $(\lambda_i, \theta_i) = \omega_i$ and $\omega_i \in \Omega = (\Lambda, \Theta)$. Λ is the set of all possible observable elements in the population and Θ is the set of all possible unobservable elements. Player i can learn a player j 's observable (or self-reported private) attributes. Each player i estimates their partner's true attribute set based on the other's observable and self-reported characteristics, λ_i , such that $E[\omega_j] = E[\theta_j | \lambda_j]$.¹⁶ Players do not observe others' attitudes ex ante; attitudes are revealed as a result of the interaction between partners. Players can easily modify behavior based on perceived partner attribute set, but attitudes are ex ante unobservable.

In this study characteristics include age, sex, income and education. Attitudes encompass values and social preferences, including altruism, θ_i^A . The model looks at how giving in a one-shot dictator game changes with the interaction of characteristic sets. This is a departure from EJ and BT; they define heterogeneity in terms of relative altruism or relative selfishness of the players. Players hold beliefs about others' attribute sets and they assign probabilities to the chance that the other's attribute set satisfies their expectations, as a function of observable characteristics. Players have no opportunity to update since each treatment is a one-shot game with a new partner. We do not include concern for the other in the players' attribute sets. Characterizing players in this way

¹⁶ Player i 's expectation of their partner's true attribute set can also be based on player i 's own experience, \bar{h} , such that $E[\omega_j] = E[\theta_j | \lambda_j, \bar{h}]$. For example, suppose patient j is a mother. A clinician who has experience treating mothers may make assumptions about patient j 's unobservable characteristics based on what he has observed from other mothers. In this study we do not investigate the role of experience and thus omit it from our descriptive model for simplicity.

assumes that “I care about the material gain of the other person because they are like me in some dimension”. This modification is in the spirit of EJ’s model, as in their model the individual is considered esteemed by other players who are of the same type.

Players have preferences over their own material gains, m_i , and their partner’s material pay-off, m_j . Dictator preferences may also include a feeling of being esteemed by the partner (the patient, in this case), which we refer to as pride. Pride is $\hat{\theta}_{ji}$. Thus the utility function of clinician i concerning interaction with patient j is

$$u_i = m_i + \theta_i^A m_j + \hat{\theta}_{ji}.$$

as in EJ, where $0 \leq \theta_i^A < 1$. In EJ, pride is esteem from the partner weighted by the salience of a given patient’s esteem. Salience is a function of social distance. Smaller social distance makes that esteem more salient. We modify their pride concept slightly to better fit the doctor-patient context: we allow pride to have an intrinsic and an extrinsic component. Intrinsic pride, \bar{p}_i , can be thought of as (fixed) base pride. It is the pride a clinician feels in his or her identity as a clinician, independent of anything he or she actually does and the patients he or she treats. Extrinsic pride, p_i , is variable and depends on the situation. It is the pride that a clinician gets from the interaction with the patient and the actions he or she performs as part of his or her job. Extrinsic pride is weighted by salience, σ_{ji} . Salience is a function of doctor and patient social identities, ω_i and $E[\omega_j]$, respectively. Social identities affect salience as a result of norms of behavior within groups (a clinician and his/her professional peers) and norms of behavior toward certain groups. Pride can thus be expressed as

$$\hat{\theta}_{ji} = \bar{p}_i + \sigma_{ji}P_i$$

where $\sigma_{ji} \geq 0$ and $\sigma_{ji} = \sigma(\omega_i, E[\omega_j])$. We do not specify a specific functional form for salience to keep the model generalizable. In this study, however, we do consider the specification that salience is a function of social distance, $\omega_i - E[\omega_j]$. Smaller social distance makes extrinsic pride more salient. The exception to this is when one party wants to impress another who is better than them in some way, such as a professional peer or a high status patient. In that case, a greater social distance may lead to much greater salience.

Based primarily on the work of BT and EJ, this model helps to formalize some of the trends revealed in this study. It also sets the groundwork for further research. One area of future research that we explore in this appendix is the role of social distance in motivating increased altruism. While the experiment is not designed to identify this effect, we find some suggestive results on this hypothesis.

Following from this model, we explore whether clinician behavior in the laboratory changes when a clinician shares a common characteristic with a patient. According to the theory, P1 should give higher amounts if they are more similar to their partner in terms of the known characteristics.

5.7.2. Suggestive results on the social distance hypothesis

One of the hypotheses that come out of the theory presented in this paper is that decreased social distance increases giving. While the experimental design does not aim to test this hypothesis directly, the detail of the laboratory data do allow for a discussion of

the issue. We first present results of average giving by social distance category: differences in age, sex, education and income between clinician and non-clinician partners. But social distance is not experimentally controlled and patterns of giving by social distance may be driven by the social distance itself, by the clinician's own social reference group or by clinicians' response to partner identity (independent of his own identity). Thus, we then break down the social distance results to consider differences in giving by clinician and non-clinician characteristics, independently. The results reported here are only from the clinician sample. The Maryland Day sample was not large enough to look at giving by characteristic in the social information treatment.

We now turn to the outcomes from looking at similarity between partners. Comparing average giving between clinician dictators in T1 and T2 implies that social distance is important in determining giving behavior, but not necessarily uniformly in the direction that the theory suggests. Table 5.12 reports results on social distance and giving across treatments. Decreased social distance increases giving in the age category; dictators whose partners are no more than 5 years younger or older than them give significantly more than dictators whose partners are outside of that range. In terms of sex and income, on the other hand, greater social distance motivates more giving. Dictators partnered with receivers of the opposite sex or with receivers of lower income than them give more when they have the sex and income information of their partner. For income, however, this effect disappears when income differences reach 300,000 Tshs per month. These results show that the significant response to the presence of social information in the laboratory may in part be motivated by the interaction of dictator and recipient characteristics.

While the significance of these results is strong, no clear pattern emerges with respect to social distance and giving. And because the effects are absolute (rather than marginal) and unidentified, it is necessary to discuss two alternative hypotheses. The first is that the dictators own social reference groups drive the social distance results. This hypothesis, that individuals behave according to the norms established within social reference groups, conforms to the literature on in-group behavior. The alternate hypothesis is that clinicians respond to individual patient characteristics with varying levels of generosity. In particular, clinicians may behave more generously to the more deserving recipients, as in Eckel and Grossman (1996).

Comparing the results from the social distance analysis with results on giving by clinician characteristics reveals that in at least two categories social reference group may be more important than social distance. We find that women give more in both T2 and T3 than in T1, independent of with whom they are paired (Table 5.13). This result is mirrored in the results on social distance, where female clinicians matched with male partners give more in T2 than they give in T1, but these same clinicians also give more in T3, where they do not know the characteristics of their partners. A Wilcoxon signed rank test confirms that female dictators matched with female partners also give significantly more in the social information treatment than they give in the baseline treatment, at 5% significance. This group also is more generous in the second treatment than their peers matched with male partners. Hence, the social distance result is not supported; while women matched with male partners give significantly more in T2 than in T1, so do their female peers matched with female partners. The other category where clinician social reference category appears to drive results is income. Giving is higher in T2 and T3 when income between

partners is 200,000 Tshs. A 200,000 Tshs difference in income appears to make little sense as motivating additional giving. But when one considers the distribution of clinician and non-clinician incomes combined with the results in Table 5.13 on giving by clinician characteristic, it is apparent that this result is not driven by the 200,000 Tshs income difference. Rather, clinicians with average income (between 200,000 and 400,000 Tshs) do give significantly more in T2 and T3 than in T1. Most recipients fall in the lowest income ranges, which is roughly 200,000 Tshs less than their average partner. Hence, clinician income group appears to be more behind this result than the difference between their own and their partner's income.

This reasoning, however, does not explain the significantly higher giving by male dictators when learning that their partner is a female, since average giving among male dictators does not change across treatments. Likewise, the result that a dictator who is younger than their partner gives more in T2 than in T3 is not reflected in patterns of giving among young dictators. These dictators do not give much more in T2 than in T1, but they do decrease their giving in T3 relative to T2, when the social information is not present. For this group, being matched with a partner who is older than them motivates more giving than being matched with a partner who has chosen them.

There is also support for the second alternative hypothesis that giving increases with the deservedness of the recipient. Differences in giving between the baseline and the social information treatment are significant for recipients that are female, that have lower education, that have lower income and that are younger. For those groups, giving in T2 is significantly higher than in the ordinary dictator game (Table 5.14). That giving would be higher in T2 for these groups is evidence of the “hero clinician” – a health worker who

exerts most of his or her effort toward helping the most needy patients -- in this sample. An overriding “hero clinician” mentality may confound responsiveness to social distance results – dictators may be sensitive to social distance, but if they are more responsive to the characteristics of the other than to the difference between themselves and the other then the social distance results will not come through. No significant differences by patient characteristics emerge when comparing T2 and T3; this is as expected since dictators did not have partner information in T3. Thus, we do find support for the hypothesis that giving varies by social distance, but the pattern is more complex than the EJ theory suggests. Responses to social distance are not identified in this laboratory data and patterns supporting alternative hypotheses emerge.

No strong patterns emerge in either sample, however, when considering the hypothesis that this increased generosity varies with social distance. Our results on social distance unfortunately do not clear up the nature of the interaction between social identity and altruistic action. Our at best suggestive evidence, however, does fall on the side of indiscriminate altruism. We point out that this comes from a sample of clinicians. One may expect medical professionals not to discriminate purposefully between patients. To the extent that reactions to social distance are involuntary, our data point to that clinicians also may not discriminate unintentionally. The preliminary results reported here echo those of Das and Sohnesen (2007), which also betray no evidence of discrimination. Thus, while social theory and some evidence tells us that humans care about the similarity of others in economic interactions, we do not find that social distance impacts giving among the pilot sample or plays a role in clinician behavior in the workplace.

5.7.3. Appendix Tables

Table 5.12. Differences in giving by paired characteristic

	Comparing giving in T1 (no partner information) to T2 (with partner information)	How the same dictators behave in other treatments (not the same partners as in T2)		
	T2-T1	T3-T1	T2-T3	N
Male dictator, Female partner	9.96**	3.92*	6.04	25
Same Sex	6.34	5.51	0.83	29
Female dictator, Male partner	7.00*	-1.50	8.50**	10
Dictator Younger	1.73	-7.54	9.27*	11
Same Age ^a	11.40**	8.87**	2.53	15
Dictator Older	7.27	6.17	1.10	41
100,000 Tshs apart ^b	-3.50	-3.25	-0.25	4
200,000 Tshs apart	7.82**	5.64**	2.18	33
300,000 Tshs apart	3.52	1.39	2.13	23
Same Edu Level ^c	9.29*	6.58	2.71	24
Dictator with More Edu	7.12*	3.12	4.00	40

Differences in means tested with Wilcoxon signed rank tests.

*** (**, *) indicates significance at 1% (5%, 10%) level.

^a 'Same age' is defined as being within 5 years of each others' age.

^b \$1USD is equal to approximately 1300 Tshs. Income is monthly.

^c Education levels are primary school, secondary school, post-secondary school/college, and graduate school. 'Same Edu Level' means that the partners completed some or all of the same level of school, or are one level apart. Dictators with more education are those that have schooling 2 levels above their partner.

Table 5.13. Differences in giving by dictator characteristic

Characteristics		T2-T1	T3-T1	T2-T3	N
Gender	Male	4.77	2.04	2.73	44
	Female	14.65**	7.65	7.00**	20
Education	Post-Secondary	6.74**	4.40*	2.34	62
	Beyond	14.00	6.00	8.00	5
Income ^a (thousands of Tshs)	<100,000	NA	NA	NA	0
	100-200	-3.50	-9.50	6.00	4
	200-300	4.58	10.08**	-5.5	12
	300-400	15.28***	8.09	7.19*	22
	400-500	1.55	-1.82	3.37	11
	500-600	6.12	3.65	2.47	17
Age (years)	up to 30	10.75**	-0.38	11.12	8
	31 to 40	4.28	1.47	2.81	21
	41 to 50	10.25	10.55***	-0.30	27
	51 to 65	3.18	-0.91	4.09	11
Facility Type	Public	8.32*	7.43	0.89	28
	Private	1.79	0.12	1.67	24
	NGO	16.57**	5.86	10.71	14

Differences in means tested with Wilcoxon signed rank tests.

*** (**,*) indicates significance at 1% (5%, 10%) level.

^a Income is reported by month. Tshs are Tanzanian shillings. \$1USD is approximately equal to 1300 Tshs.

Table 5.14. Differences in giving by recipient characteristic

Characteristics		T2-T1	T2-T3	N
Gender	Male	1.04	1.14	30
	Female	12.35***	4.08	37
Education	Primary School	6.98*	3.44	38
	Secondary School	8.71	1.67	21
	Post-Secondary	12.00*	12.00*	5
Income ^a (thousands of Tshs)	<100,000	7.44**	3.11	45
	100-200	-2.31	-2.31	13
	200-300	0	0	1
	300-400	5	0	2
	Age	up to 30	12.26***	4.83
(years)	31 to 40	-2.65	-5.88	17
	41 to 50	5.40	10.60	10
	51 to 65	10.00	2.00	5

Differences in means tested with Wilcoxon signed rank tests.

*** (**, *) indicates significance at 1% (5%, 10%) level.

^a Income is reported by month. Tshs are Tanzanian shillings. \$1USD is approximately equal to 1300 Tshs.

Chapter 6 : Clinicians' Social Preferences in the Workplace

6.1. Introduction

Beyond the basic laboratory results, I obtain additional insights into the pro-social behavior of the clinician subjects by exploiting the fact that the subjects from the laboratory experiments are the same as the ones for whom I have field data. We use the giving patterns in the laboratory in order to classify clinicians as being responsive to social information, responsive to pride and/or as being fair. Using clinician effort in the field as the dependent variable, I determine how important these attributes are in clinicians' effort choices in the workplace.

In this chapter we present results from a field survey on clinician behavior at work. Data were collected in healthcare facilities in Arusha, Tanzania. We also use results from the pride and social information treatments of the laboratory-style experiments reported in Chapter 5. Specifically, we use the results from the implementation in Arusha, Tanzania, which we carried out with the same Tanzanian clinicians for whom we have the field data.

Combining data from the laboratory with data from the clinicians at work provides unique insights into the role of social preferences in clinician effort choices. The results reported in this chapter are particularly compelling because the subjects for whom we collected field data are the same clinicians that participated in the laboratory experiments. This adds substantial strength to the subsequent analysis. Secondly, the subjects are

professionals rather than being from the more common university student subject pool. Moreover, it is uncommon to combine laboratory with field data in this way and in doing so we contribute to the ongoing debate of how to utilize and interpret data collected in the controlled laboratory setting with respect to the field setting.

6.2. Data collection

6.2.1. The sample -- clinicians in Arusha

We collected the field data from 104 clinicians in the semi-urban area of Arusha, Tanzania from November 2008 until August 2010. This period covered the 18 months prior to the laboratory experiments. The data collection involved observing clinicians at work and conducting exit interviews with their patients. In total, we spoke with 4,512 patients from these 104 clinicians. Field data collection lasted from November 2008 to June 2010. Not all of the clinicians in the field data chose to participate in the laboratory experiment. Since we match behavior in the laboratory with behavior in the field for this chapter, we restrict our attention to the subsample of clinicians who also agreed to participate in the laboratory experiments.

We sampled 100 percent of the healthcare facilities in the area with outpatient departments, though some facilities were excluded based on convenience; they were either too difficult to reach or had too small a patient volume. The sample includes public, private, and non-profit/charitable facilities. Clinicians were randomly sampled within each health care facility enrolled in the study. We restrict our attention to clinicians because they are the primary health workers who provide the outpatient care in

the area, as is discussed in Chapter 3. Clinicians are those health workers with one of the following degrees: ACO, CO, AMO, MO.

None of the facilities approached declined participation, but attrition was a minor problem for the individual doctors involved in the study. Only 2 clinicians that had originally consented opted out later. There was additional attrition as a result of clinicians taking their annual leave or attending compulsory continuing education seminars. Whenever possible, we maintained contact with these clinicians and continued to collect data when they returned to work. None of these types of attrition are correlated with observable clinician characteristics or quality of care, except for very high quality doctors whose advanced degrees did not necessitate continuing education and who took little vacation. We use data from one day to three days of data collection for each clinician, depending on the above-mentioned attrition and the clinicians' presence at work on the days we went to collect data.

6.2.2. Procedures

We collected data for each clinician on at least 7 (and not exceeding 9) separate occasions over a period of 3 weeks. Start dates were staggered and the days on which we collected data for any given clinician were not announced in advance. Length of time between the different visits varied according to clinician schedule. We obtained consent and collected data on clinician characteristics in an initial visit that preceded the 7-9 patient data collection visits. On each of the 7-9 data collection visits we interviewed all the patients the clinician saw over a 4-hour window. During one of these data visits we also observed the clinician working. Finally, there were two times during the course of

the research when a clinician from our team met with each clinician subject, but did not collect data. In order to avoid anomalies in the data resulting from these intermittent meetings, we limit our analysis in this paper to the first three data collection visits. The sample in this study is further reduced because not all clinicians who participated in the field survey also participated in the laboratory experiment. Further, six of the clinicians participating in the experiments did not take part in the field study. Taking these limitations into account, the final sample size that we use in this paper includes 61 clinicians and 805 patients. A regression of experiment participation on practice quality shows that although these are negatively correlated, the relationship is not significant (Table 6.7). A t-test confirms that those who participated in the experiment are not significantly different from those who did not in terms of quality of care. Table 6.1-Table 6.6 summarize patient and clinician characteristics in the field data. Standard deviations are reported in parentheses.

6.2.3. Instruments

This study uses data from 2 different data collection instruments. It also uses data from the laboratory experiments on social information and pride discussed in Chapter 5. The first instrument is a small survey that we used to collect clinician characteristics, such as age, education, and income. We administered this instrument during the consent process, before the research began. The second instrument, the Retrospective Consultation Review (RCR), was administered during each data collection visit for each clinician. The RCR is an exit interview survey designed to measure clinician effort. It is administered to patients at the health facility after their visit to the doctor has ended. The RCR we use is a

slightly modified version of the instrument used in Leonard & Masatu (2006).¹⁷ The RCR asks the patient what their symptoms were (including the dominant symptom) and what tasks the doctor performed during their consultation (by symptom where appropriate). It also records information about patient satisfaction, reasons for visiting the facility, and general questions on patient wellness. The data from the RCR are at the patient level and observations are uniquely identified by the two variables doctor and patient. Each patient was interviewed only once during the course of the study. This instrument was administered in Swahili.

The RCR also asks patients' socio-economic questions such as their job (if employed), the materials used to build their home, their education level, ownership of various assets and patient sex and age. In the case of accompanied minors we collected the socio-economic information of their guardian. In the analysis, patient age and sex refer to the minor (patient) themselves and education refers to the guardian. This allows us to include patient characteristics that would be correlated with illness (type and severity), where guardian education is a proxy for family income. Combined with the laboratory data, the information from these two instruments permits us to evaluate the impact of social preferences on effort in the field.

¹⁷ Effort measured using RCR data is an accurate approximation of what the doctor is actually doing for the patient (Leonard & Masatu, 2006). We therefore use RCR data for our analysis rather than data from direct observation, which gives me a significantly larger dataset to work with.

6.3 Methods

6.3.1. Main estimation

The main variables of interest in our estimation in this chapter are social information responsiveness, pride responsiveness, and the interaction between these two attributes. These three attributes are measured in the laboratory (Chapter 5). They are not mutually exclusive. Those that give more in T2 than in the ordinary dictator game are considered responsive to social information. Those that increase giving when the partner has chosen them are pride responsive. Finally, those that give half of the endowment to their partner in the standard dictator game, T1, we classify as “fair”. All three are binary variables, though the definition of fair is not exhaustive. A clinician-dictator is a fair type if he/she gave 50 tokens to the partner and not a fair type if he/she gave fewer than 50 tokens. Those that gave over 50 are not included in this definition.¹⁸ I also include an interaction term for those responsive to both social information and pride. This reflects the idea from Ellingsen and Johannesson (2008) that pride and social information go hand in hand in motivating pro-social behavior. Nearly half of the sample responded with higher giving to the social information and pride treatments, but only one-third responded to both treatments. Less than one-third of the participants can be considered fair types. Figure 6.1 is a histogram displaying proportion of the population categorized as each type.

We use an ordinary least squares model with facility level random effects, c_j , to evaluate the importance of these attributes in clinicians’ effort choices. The model is

$$Effort_{ijk} = \beta S_i + \delta X_{ik} + c_j + e_{ijk}$$

¹⁸ Results do not change for alternate definitions of the fair-type that do include giving over 50 tokens.

$Effort_{ijk}$ is the percentage of symptom-specific protocol items completed by clinician i at facility j for patient k . S_i is a vector of the four social preference measures and X_{ik} is a vector of patient and clinician characteristics including patient age, gender, education level and wealth. X_{ik} also includes controls for data collection visit number. In some specifications X_{ik} include facility type (private, public or NGO) and average daily patient volume. The error term is assumed to be independently and identically normally distributed. Random effects is appropriate in this setting since individual facilities were not chosen explicitly. Hence, following Nerlove (2002), we assume the presence of an effect unique to each facility which produces a constant error in the measures from that facility. The effect is considered random in that it is not something we have purposely varied – no facility was chosen for any merits particular to that facility. We simply recruited as many facilities, and clinicians therein, as we could. A Hausman test confirms that individual facility effects are uncorrelated with other covariates; we fail to reject the null that coefficients from the random effects estimation are different from those of the fixed effects estimation ($p=0.99$).

Unobserved patient attributes should also not be correlated with doctor characteristics because in the outpatient system in Tanzania patients are essentially randomly assigned to doctors, once they have chosen the facility. Patients do not have appointments and cannot choose their doctor. When they check in at a facility, the reception directs them to a clinician's queue randomly.¹⁹ Also, clinician schedules are not regular so the patient cannot come on a specific day of the week, anticipating a certain clinician will be working that day. Consequently, we rely on these details that patients are essentially

¹⁹ Presumably if a patient arrives in an emergency situation, the receptionist can direct them accordingly. We do not include emergency rooms in our field sample.

randomized across doctors and that there is no unobserved patient characteristic that would be correlated with doctor characteristics. We explore this assumption in detail in Chapter 7.

We further control for data collection visit number to account for any visit-specific effects. This assumes that the structure of how effort may (or may not) change between visits is the same for all clinicians and uncorrelated with effort. This is an important point. Recall that during the second visit a medical professional from our research team is present to observe the clinician at work. From past research we know that this will induce most clinicians to work harder and that the pattern of effort changing follows a predictable path consistent with a Hawthorne effect (Leonard et al., 2007; Leonard and Masatu, 2006). By controlling for visit we take into account this predictable change in effort, similar to the effect of including a time trend variable accounts for external shocks to a sample and imbalance in number of observations over time (Greene, 2003).²⁰ Finally, we use the panel-robust variance-covariance matrix and facility level clustering to correct for autocorrelation (correlation between consultations within each facility) and any potential heteroskedasticity. An extensive discussion of this treatment of the standard errors is included in the next section, on clustering. Thus, with this random effects model we estimate the extent to which clinicians' social attitudes, as measured in the laboratory, help to explain variation in effort.

²⁰ One circumstance that would invalidate the use of visit number dummies to adjust for panel imbalance over time is if observations are not missing at random – that not having observations for visits 2 and or 3 is correlated with behavior in the lab. We do not adjust for this in this paper.

6.3.2. Alternative specifications

In alternative specifications of this model we also control for clinician experience (years as a health worker) instead of and in addition to monthly income. Including patient attributes controls for case mix, which is invariably important in clinician effort – a sicker patient requires more effort and it may be more difficult to provide a given proportion of the required care for very sick patients than for relatively healthy patients. The key variables of interest in these regressions are again the social preference variables, S_i , which were measured in the laboratory with the same sample.

6.3.3. Unbalanced panel and clustering

Clustering adjusts the variance-covariance estimates for the fact that the panel is unbalanced. It is sufficient as an adjustment (i.e. there is no need to also worry about consistency) as long as the reason for the imbalance is not correlated with the dependent variable. In this dataset, imbalance in the number of patients per clinician and clinicians per facility reflects the population. For the data we use in this study, we planned to visit each doctor 1 to 3 times to collect data. During each data collection visit we surveyed all of the consenting patients a clinician saw during the time we were at the facility. Hence, the number of observations from each data collection visit correspond directly to the number of patients a clinician saw that day. Also, for each facility, we engaged as many clinicians as possible.²¹ As these distributions are reflective of the actual population from

²¹ Since the number of small facilities in Arusha is large and our sample reflects this, larger facilities are not represented more in the data than smaller facilities. Indeed, 49.7% of the observations are from large facilities. 27.9% of the facilities are large and nearly half of all clinicians, 48.2%, are from large facilities.

which we source our sample, unequal numbers of patients per clinician and clinician per facility, while not unimportant, should not contribute to estimate bias.

As stated above, imbalance affects the efficiency of estimates if not appropriately adjusted for in the estimates of variance. While the sample is clustered first at the clinician level and then at the facility level, with clinicians nested within facilities, it is only necessary in our analysis to cluster at the highest level of aggregation (Cameron et al., 2006). There are 28 facility clusters in this sample, with cluster size ranging from 1 to 8 clinicians. Even with differently sized clusters we can use a sandwich, robust variance estimator to achieve estimates that are robust to intracluster correlation and arbitrary heteroskedasticity (Wooldridge, 2002). We also present results clustered at the clinician level (55 clusters, patients per clinician ranging from 4 to 30) for comparison (Table 6.8). Clustering at the clinician level does not appreciably differ from clustering at the facility level. Some significance in key variable is lost, but it does not disappear. In conclusion, with the cluster dummy variable model, the OLS estimate with random effects is consistent; correcting for inter-cluster heteroskedasticity we achieve more efficient estimates.

One additional note on panel imbalance regarding patients per clinician bears mentioning. In our sample, the average number of patients per day per clinician is significantly negatively correlated with average quality by consultation ($\rho=-0.14$, $p<0.001$). We maintain that this correlation is not a threat to the efficiency of our estimates. Ordinarily, if the reason for an unbalanced panel is correlated with the error term (i.e. the dependent variable) it is indicative of attrition based on the outcome variable. This kind of attrition leads to estimates that are based on an unrepresentative sample relative to the population

and thus biased. Recall, our sampling was such that we interviewed all patients that the clinician saw on that day and the clinician has no control over the number of patients seen any given day. Thus, while this correlation may point to either lower quality clinicians have more patients per day or people with more patients per day providing lower quality, it does not indicate a problem in our estimation as is discussed above. As an aside, it may be true that clinicians who are of lower quality spend less time with each patient (completing fewer of the required tasks) and thus can fit more patients into their schedule. Alternatively, these clinicians with high volume may be forced to spend less time with each patient *in order to* see all the patients that are assigned to them each day. Either way, if minutes per consultation is correlated with the error term as well as consultation quality, the dependent variable, (or any of the independent variable of interest), there is essentially an omitted variable problem²². The omitted variable, time spent per consultation, is not likely to be correlated with the variables of interest and thus does not pose a problem for our estimations in terms of consistency. Threats to minimum variance estimates are addressed with the variance-covariance estimation techniques discussed above.

6.4. Results and discussion

6.4.1. Results from the main estimation and alternate specifications

Results from the main estimations appear in Table 6.8 through 6.11. In Tables 6.9-6.11 the column labeled “Baseline” has output from the same central estimation, which

²² Note that this is not a facility type effect (private versus public); of the seven clinicians in the 90th percentile of average daily patient volume, three practice at public facilities and four practice at one of three private facilities. Moreover, the significance of the correlation holds in a regression of patient volume on effort controlling for facility type.

appears in column 2 of Table 6.8. The most robust result in this regression is that those subjects who are both pride responsive and social information responsive provide better average effort than their peers who are responsive only to pride. The coefficient on the interaction term is positive and highly significant in all model specifications, as is the marginal effect of social information responsiveness in the presence of pride (equal to 0.11 at 1% significance in the baseline regression). On the other hand, responsiveness to social information alone or pride alone does not appear to be correlated with workplace effort (not significant at conventional levels in the baseline regression). Further, in the presence of social information responsiveness the marginal effect of pride responsiveness is insignificant. This is due to the fact that although pride responsive people tend to provide worse effort than average, having the attribute of also being responsive to social information mitigates that negative response. For the clinicians responsive to both social information and pride, patient identity is important and knowing more about the patient is likely a source of motivation for them. The pride they feel is apparently linked to the social interaction inherent in their profession. These are potentially the clinicians who feel pride from the service aspect of their job – the pride in the field is triggered by that personal interaction. Conversely, clinicians who are only pride responsive (and not social information responsive) provide lower effort at significance levels of less than 1%, but controlling for clinician income renders the result insignificant at the 10% level. Recall that in the Tanzanian outpatient context patients are assigned randomly to clinicians. Clinicians know this and pride responsive clinicians who are not also motivated by patient identity thus have no social incentive to provide higher effort.

Note that the negative pure pride effect pulls in the opposite direction of the pride-augmented social information effect. In fact, a Wald test on the sum of coefficients reveals that the total impact of the social attitude variables for those that are responsive to both pride and social information is not distinguishable from zero. Nonetheless the regression helps to explain the variation in effort by decomposing the average. While this group of clinicians is not different from average, the marginal effects show that social information responsiveness mitigates what would otherwise be the negative impact of pride. Also, in the presence of pride, the marginal impact of social information responsiveness is greater. This is due to the large magnitude and positive sign of the interaction term: combined together, pride and social information responsiveness lead to higher effort than pride responsiveness alone. Hence, even though the net effect of both pride and social information responsiveness together is not distinguishable from zero, the impact of the combined social attitudes is important in that it keeps pride responsive clinicians from providing even worse effort.

We also reject the joint linear hypothesis that all coefficients of the social attitudes variables are equal to zero at $p < 0.05$. Fairness tends to mean higher effort on average and has similar magnitude and robustness to the attribute of being both social information and pride responsive. This suggests that in this clinician-patient context preferences for fairness are at least as important as other social attitudes in determining workplace effort. Patient and illness characteristics do impact average effort, as expected. Because these observables proxy patient case-mix, they cannot be used to validate the evidence from the laboratory of a 'hero clinician' norm in this sample. Clinician age and experience are also highly significant (Table 6.9), but adding experience reduces model fit considerably.

Including both income and experience remedies the loss of model fit, but with equally dire consequences in terms of the variance components estimates; estimates of the random effects' variance appear to be negative and so the estimation reduces to OLS, a decidedly incorrect specification. Consequently the (pseudo) R^2 reported is misleading. Also, in this (incorrect) OLS specification the explanatory power of the social attitudes variables disappears. Experience appears to absorb all of the explanatory power from these variables, though of the four, it is only correlated with the attribute of fairness ($p=0.03$). Clinicians with more than 20 years of experience are much more likely to be fair in the standard dictator game. This correlation may merit further investigation, but since there do not appear to be any overriding correlations between tenure and the other social attitudes, we leave it out of this study. Thus, we perform the sensitivity checks on a specification that includes age, not experience; the two share a correlation coefficient of 0.64 with $p<0.001$. We ultimately reject the hypothesis that preferences for fairness and social information and pride responsiveness do not play an important role in explaining clinician workplace effort.

As a specification check, we perform the regression analysis with different error structures. Our baseline model includes robust standard errors, which is appropriate if one considers each patient-doctor interaction as having its own unique variance. If instead the variance is the same within a facility or within a clinician, clustering is the more appropriate technique. Results reported in Table 6.8 show that clustering at the facility or clinician levels does not change the primary results. It must be noted however, that the significance of the positive coefficient on the interaction term goes up to 1% with facility clusters and down to 10% with clinician level clusters. Thus the estimates from the

specifications with clustered standard errors serve as bounds on the significance of being both social information and pride responsive for explaining variation in effort.

Primary results do not change under various alternate specifications. Excluding rural facilities we still find the same pattern of results (Table 6.10). Results are also robust to using item level dependent variables rather than consultation level averages (Brock, Lange and Leonard, 2011). Note that performing the regression at the consultation level may over-simplify the relationship between social attitudes and clinician effort. Different tasks a clinician does are more or less representative of her different skill sets, such as medical knowledge or bedside manner. It may be that social attitudes help explain variation in certain tasks and not others. If this is the case then collapsing the dependent variable into a consultation level average may be simplifying too much and the correlation between social attitudes and effort may be spurious or misleading. We do not find this to be the case, as results are unchanged when looking at the data at the item level. See Brock, Lange and Leonard (2011) for details.

Further, we estimate a model that includes practice ability in the right hand side. Skill, or ability, is almost always a cause of concern in regressions that try to explain performance (Angrist and Krueger, 1999; Card, 1999). Although we do not have reason to believe that ability is correlated with pride or social information responsiveness, it certainly helps to explain variation in tasks completed. Also, ability as an omitted variable may affect the variance-covariance estimates. We estimate clinician ability using a latent variable model²³. When ability is included, fairness drops by almost half and become significant, but our central result does not change. Tendency to be fair in the laboratory is

²³ See the succeeding chapter for an in depth discussion of how we estimate clinician ability.

significantly correlated with practice ability, ($\rho=0.218$, $p<1\%$) so that including both fairness and practice ability as covariates in the regression leads to potentially misleading results with respect to the importance of either characteristic. Indeed, we reject the joint hypothesis that both coefficients are equal to zero (Wald test with $p<1\%$). Results are reported in Table 6.11. Ultimately, ability does not alter the primary results of the regression with respect to pride and social information responsiveness. Though it does affect our estimates of the importance of fairness, the strength of the correlation between the two variables makes the impact difficult to interpret.

Finally, we estimate the baseline also controlling facility type and patient volume. We do this in order to test the extent to which our results are industry wide or whether they are being driven by facility culture. Past research suggests that facility type (public, private, NGO) and patient volume define elements of facility culture that in turn impacts quality of care (Gachter and Falk, 2000; Leonard and Masatu, 2008; Serneels et al., 2009; Serra et al.). Facility culture can also have heavy influence on intrinsic motivation and pro-social behavior in the workplace (Akerlof and Kranton, 2005). Modeling these relationships with respect to the social attitudes measured in the laboratory -- pride responsiveness, social information responsiveness and fairness – is beyond the scope of this study. Nevertheless, we report the extent to which our results are robust to controlling for these facility characteristics so as to inform future research on this topic. The results from these estimations appear in Table 6.11, in which we also reproduce the baseline regression results for easy comparison. Adding in dummies for facility type and patient volume does not change the main result that those responsive to both social information and pride provide better average effort. The coefficient on the interaction

term stays roughly the same until we add ability and volume, ability and facility type, or all three to the baseline specification. In the fullest specification it is 40% the size of its counterpart in the baseline specification and is not significant. The fullest specification includes ability, facility type and patient volume controls.

In their own right, facility type and patient volume do not have significant impacts on the average quality of care. Consultations in private facilities tend to be met with more effort than in public facilities, a result consistent with the literature (Leonard et al., 2007). Patient volume tends to have a negative impact on average clinician effort, but the effect is only significant in two specifications, the one with ability and the fullest specification (the final column). This is presumably due to the small but significant correlation between ability and patient volume ($\rho=-0.032$, $p<5\%$). Note that practice ability is positively correlated with clinician effort whereas patient volume is negatively correlated, so that when ability is not present in the regression, volume is picking up some of that positive correlation with effort. This, in turn, reduces the patient volume coefficient and it becomes insignificant (the variance estimate for patient volume does not change much across specifications).

Adding facility type and patient volume together reduces the significance on the social attitudes interaction term. We believe this is not necessarily because the result is weaker, but rather due to the strong correlations between the kind of facility one works at and the tendency to be both social information and pride responsive. We find that those who work at large public or large private facilities are much less likely to be both social information and pride responsive than their small facility counterparts. Conversely, those that work at large NGO facilities are more likely to be responsive to both stimuli in the

laboratory than *their* small facility counterparts. The relationships are all significant at less than 1%. Thus, including both facility size, facility type and social attitudes means these variables are all tugging various directions, and potentially eliminating significance even for meaningful relationships. Hence even though our results on the impact of being both social information and pride responsive are rather robust to controlling for facility characteristics, there is clearly a link between these attitudes and facility culture (as it is summarized by patient volume and ownership). Practice ability also figures into the story. Exploring what that relationship may be between these components is left to future research.

6.4.2. Reverse analysis – evaluating the role of negative social attitudes

In the preceding analysis we defined social attitudes measured in the laboratory in accordance with the Ellingsen and Johannesson (2008) theory of pride and salience of another's esteem. An omission to the analysis follows from such a focused view. Social information and pride responsiveness are not the only kinds of behavior possible in the experiments. Here we consider the subjects that may respond negatively to either social information or pride. Does a negative response to knowing about one's partner or to induced pride describe a social attitude that might help explain variation in clinician workplace effort?

Considering this empirical question, we adjust the definitions of social attitudes to encompass negative responses to the experimental treatments. We refer to those who give fewer tokens to their partner in the pride treatment as negatively responsive to pride. Likewise, giving less in the social information treatment earns the subject the

classification of being negatively responsive to social information. 25% of the clinicians responded negatively to induced pride in the laboratory. A clinician who responds negatively to induced pride in the laboratory may have two different effort outcomes at work. She may provide less effort at work, signifying a disutility from attracting attention or esteem from others. We will call this kind of clinician an introvert. Alternatively the clinician who responds negatively to pride in the laboratory may not behave differently from her peers in the field because, despite a potential disutility attached to attracting esteem in general, she does not get pride out of what she does. A similar story can shape our predictions for how those who are negatively responsive to social information may behave in the workplace. Negative response to receiving partner information in the laboratory points to having disutility from social interactions. We label this type of clinician a misanthrope. In the laboratory, 23% of the sample can be classified as misanthropes. The misanthropes will always offer less effort than their peers because it is impossible to escape the social part of the interaction with the patient. Results appear in column 2 of Table 6.12. The column headers in the table refer to how the social information and pride variables are defined.

This analysis gives evidence that those responding negatively to the stimuli in the lab may constitute a different “type” of pro-social preferences that we should account for in our models. We see evidence of misanthropes – the coefficient on social information is significant at the 10% level and implies that those who are negatively responsive to social information (and not negatively responsive to pride) give 9% less effort than their peers. Put another, more salient way, where the average patient can expect their clinician to perform 7 out of 10 required items correctly, the patients with misanthropic clinicians

will only receive 6 of the 10 required items. The economic significance of this result ultimately depends on what item is not being performed. The coefficient for negatively responsive to pride is insignificant, so we do not see evidence of introverts. Column 1 of the Table 6.12 juxtaposes these results with a more inclusive definition of social information and pride responsive (in the positive sense). In this column, social information responsive includes not only those that gave more tokens in T2 than in the standard dictator game, but also those who did not change their giving patterns (the “no-changers”). Adjusting the definition allows us to directly see how the negatively responsive people are behaving in comparison with everyone else. The comparison underscores the uniqueness of the result pointing to the presence of misanthropes in the clinician sample.

The regression with the adjusted definitions (column 1 of Table 6.12) also reveals that the original definitions of social information and pride responsiveness are indeed meaningful measures. The definitions of altruism in response to social information and pride that include the no-changers do not have the same explanatory power as the original definitions. The converse is true for an identical variation on the definition of the negatively responsive types. Including the no-changers in the definition of negatively responsive actually strengthens the results from that regression (Column 3, Table 6.12). Further we see that the interaction term representing those that are negatively responsive to both social information and pride is highly significant ($p < 1\%$). The net impact of all three negative responsiveness terms, $\beta_{NEG-SI} + \beta_{NEG-PRIDE} + \beta_{NEG-BOTH}$, is however not significant. So, while the interaction of the attitudes may be important in explaining variation in effort, those that are negatively responsive to both do not actually perform

differently from their peers once we consider the individual contribution of the constituent attitudes. It does mean that those negatively responsive to both pride and social information provide more effort than if they were only negatively responsive to social information. In some sense, pride appears to be keeping the misanthropes from performing too low below average. In conclusion, there is some evidence that a negative response to knowing about one's partner helps explain variation in clinician workplace effort, but the impact does not appear to be economically meaningful.

While the results of this "reverse analysis" do not provide much additional insight into the role of pride and social identity in clinician effort choices, they do suggest an alternate hypothesis to the ones put forth in the main estimation. The alternate hypothesis hinges on how we think about social information responsiveness and the interpretation of its coefficient. To be social information responsive in the laboratory is telling of the power of social identity in our lives. But when we think about how this characteristic might play out in a clinician's workplace, it is not altogether clear that the social information responsive people should behave much differently from their peers or how one might use the characteristic to craft a policy, since everyone receives social information of patients all the time. We see from the reverse analysis, however, that the provocative characteristic may not be social information responsiveness, but rather negative response to social information. Replacing social information responsive with the negative counterpart in the main estimation does not however, reproduce the significance of the social information characteristic seen in Table 6.12 (Columns 2-4). Other results do not change. This alternate hypothesis may be the focus of future research.

6.5. Conclusion

In this chapter we explored the role of social preferences in explaining variation in average clinician effort in the workplace. Our analysis concerns the real workplace performance of medical clinicians, something that is rare in behavioral and experimental economics research. Also, the results reported here directly complement our findings in the laboratory since the subjects in both analyses were the same. Using measures from the laboratory thusly in our field data analysis we contribute to the literature on social preferences in the workplace and the literature on making legitimate links between the laboratory and the field.

Clinicians' attitudes as measured in the laboratory do appear to explain significant variation in how they behave at work. Those who are responsive to both social information and pride in the laboratory provide roughly 10% more effort than their peers. Meanwhile responsiveness to social information alone or pride alone does not have any explanatory power. The findings are robust to various specifications, including controlling for practice ability, facility type or facility size. When all three additional controls are added, however, the results disappear. This belies the complex relationship between clinician attitudes and facility culture. But theory is not clear on what these relationships might look like. We thus leave modeling of this and the interplay with social preferences to future work. We also find that those who give half of their endowment in the standard dictator game (fair types) also provide more effort on average, but these results are not robust to controlling for practice ability. Surprisingly, practice ability does not gain significance unless we control for patient volume and/or facility type

concurrently, at which point it is positive and significant in explaining variation in clinician effort.

Considering the reverse question of whether a negative response to the laboratory stimuli is correlated with average effort in the workplace, we find only that those negatively responsive to social information, or misanthropes, appear to be distinct from their peers, offering approximately 10% worse effort. This result lies outside of the social preferences model on which this work is based, but stands as evidence with other results on negative social preferences, such as spite (Bradler, 2009; Levine, 1998).

Taken together, these results suggest that social preferences do impact effort in the workplace and speak to the accuracy of the Ellingsen and Johannesson (2008) and Benabou and Tirole (2006) theories as complements to models of equity-seeking such as the Fehr and Schmidt model (1999). Identifying whether this result is in fact derivative of the complex relationships between facility type, facility size and practice ability or if it is contributing to those relationships remains to be seen. More generally, this analysis shows that attributes measured in the lab can illuminate patterns in field data that would be otherwise difficult to identify.

6.6. Tables and figures

Table 6.1. Patient characteristics by facility type, full sample

	Mean age	Mean years of Education	Percent Female
Public	18.67 (16.65)	8.26 (2.40)	56.97%
Private	22.44 (15.73)	9.34 (2.90)	54.53%
NGO	26.65 (18.45)	8.98 (2.94)	54.66%
Overall	21.83 (16.90)	8.87 (2.78)	54.88%

Table 6.2. Patient Characteristics by facility type, reduced sample

	Mean age	Mean years of Education	Percent Female
Public	17.60 (16.63)	8.18 (2.33)	55.56%
Private	23.35 (15.22)	9.32 (3.02)	53.47%
NGO	25.41 (17.84)	8.88 (3.12)	55.49%
Overall	21.03 (16.81)	8.68 (2.77)	54.91%

Table 6.3. Clinician characteristics by facility type

	Mean age	Mean years of Education	Percent Female	Average years experience as a health worker
Public	43.06 (8.06)	15.94 (1.78)	50%	19.33 (8.35)
Private	43 (11.14)	16.06 (1.87)	15.38%	16.77 (10.84)
NGO	39.79 (8.93)	16.32 (1.95)	26.32%	14.63 (10.17)
Overall	42.35 (9.65)	16.07 (1.84)	25.71%	17.25 (9.87)

Table 6.4. Clinician characteristics by facility type, reduced sample

	Mean age	Mean years of Education	Percent Female	Average years experience as a health worker
Public	43.85 (7.86)	15.82 (1.74)	48.15%	20.90 (7.40)
Private	40.68 (10.67)	15.89 (0.99)	18.18%	15.33 (9.45)
NGO	39.64 (9.90)	16.57 (2.17)	28.57%	15.00 (11.24)
Overall	41.92 (9.33)	16.01 (1.63)	32.81%	17.91 (9.28)

Table 6.5. Distribution of each credential level by facility type, full sample

	Public	Private	NGO	Total
Overall	35	48	21	104
ACO	2	3	0	5
CO	20	28	16	64
AMO	9	6	2	17
MO and above	0	2	1	3
Missing	4	9	2	15

Table 6.6. Distribution of each credential level by facility type, reduced sample

	Public	Private	NGO	Total
Overall	26	21	14	61
ACO	1	0	0	1
CO	17	19	12	49
AMO	7	2	2	11
MO and above	0	0	0	0
Missing	1	0	0	1

Table 6.7. Probit of participation in laboratory experiment on practice quality, clinician level with robust standard errors

	Participation in the Experiment
Practice Quality	-0.33 (0.78)
Constant	0.50 (0.51)

p-values are shown in parentheses.

Figure 6.1. Distribution of social information responsive, pride responsive and fair types

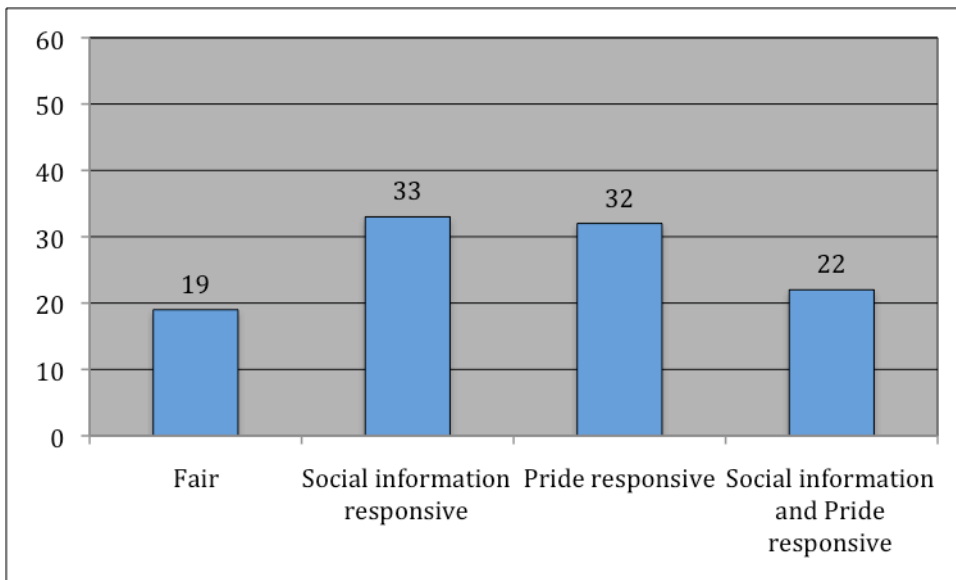


Table 6.8. OLS regression of average effort by consultation on social attitudes, with facility level random effects and various error structures

Independent Variables	With Huber-White sandwich standard error estimates	With errors clustered at the facility level	With errors clustered at the clinician level
Responsive to social information	-0.008 (0.035)	-0.008 (0.041)	-0.008 (0.037)
Responsive to pride	-0.06 (0.039)	-0.06 (0.053)	-0.06 (0.05)
Attribute of fairness	0.074*** (0.028)	0.074*** (0.028)	0.074** (0.03)
Responsive to both social information and pride	0.109** (0.049)	0.109*** (0.039)	0.109* (0.062)
<i>Clinician characteristics</i>			
Sex	-0.06** (0.026)	-0.06 (0.04)	-0.06* (0.033)
Age	-0.003* (0.002)	-0.003** (0.001)	-0.003 (0.002)
Education (years)	-0.002 (0.007)	-0.002 (0.01)	-0.002 (0.009)
Income	-0.021 (0.016)	-0.021 (0.016)	-0.021 (0.018)
Visit 2 dummy (observer present)	0.054*** (0.019)	0.054** (0.022)	0.054** (0.026)
Visit 3 dummy	0.022 (0.022)	0.022 (0.024)	0.022 (0.026)
Constant	0.905*** (0.126)	0.905*** (0.175)	0.905*** (0.153)
Number of facilities	28	28	28
Number of clusters	NA	28	54
R ²	0.173	0.173	0.173
F-test, social attitudes (p-value)	0.004	0.004	0.066

Standard errors in parentheses. *** (**, *) indicates significance at 1% (5%, 10%) level. Sample includes 798. Case mix controls included are patient age, sex, education, illness severity (proxied with an AM/PM dummy) and presence of fever, cough or diarrhea as a primary symptom.

Table 6.9. OLS regression of average effort with facility level random effects, controlling for experience, errors clustered at the facility level

Independent Variables	Baseline (with income)	With experience	With income and experience (OLS)
Responsive to social information	-0.008 (0.041)	0.019 (0.026)	0.029 (0.048)
Responsive to pride	-0.06 (0.053)	-0.109 (0.091)	-0.03 (0.067)
Attribute of fairness	0.074*** (0.028)	0.022 (0.048)	0.05 (0.052)
Responsive to both social information and pride	0.109*** (0.04)	0.077 (0.069)	0.051 (0.076)
<i>Clinician characteristics</i>			
Sex	-0.06 (0.04)	-0.015 (0.055)	-0.127** (0.054)
Age	-0.003** (0.001)	-0.004*** (0.001)	-0.007** (0.003)
Education (years)	-0.002 (0.01)	0.003 (0.015)	0.013 (0.013)
Experience as health worker (years)		0.003*** (0.001)	0.006*** (0.002)
Income	-0.021 (0.016)		-0.045*** (0.017)
Visit 2 dummy (observer present)	0.054** (0.022)	0.053** (0.021)	0.06** (0.024)
Visit 3 dummy	0.022 (0.024)	0.027 (0.024)	0.032 (0.028)
Constant	0.905*** (0.175)	0.711*** (0.203)	0.691*** (0.254)
Number of facilities	28	19	19
N	798	629	622
R ²	0.173	0.134	0.212
F-test, social attitudes (p-value)	0.004	0.002	0.546

Standard errors in parentheses. *** (**, *) indicates significance at 1% (5%, 10%) level. Case mix controls included are patient age, sex, education, illness severity (proxied with an AM/PM dummy) and presence of fever, cough or diarrhea as a primary symptom.

Table 6.10. OLS regression of average effort on social attitudes, excluding rural facilities

	Non-rural	Non-rural, controlling for facility type
S.I. Type	-0.006 (0.037)	0.004 (0.035)
Pride Type	-0.082 (0.059)	-0.083 (0.061)
Fair Type	0.082*** (0.027)	0.08*** (0.027)
S.I. & Pride	0.147*** (0.038)	0.136*** (0.04)
<i>Clinician Characteristics</i>		
Sex	-0.065* (0.039)	-0.056 (0.039)
Age	-0.004*** (0.001)	-0.004*** (0.001)
Education (yrs)	-0.008 (0.008)	-0.008 (0.009)
Income	-0.012 (0.017)	-0.008 (0.016)
Visit 2 dummy	0.056** (0.023)	0.058** (0.023)
Visit 3 dummy	0.021 (0.025)	0.024 (0.026)
Private Facility		0.096* (0.049)
NGO Facility		0.101* (0.058)
Constant	0.988*** (0.147)	0.916*** (0.164)
R-squared	0.183	0.256
F-test, social attitudes (p-value)	0.000	0.000
F-test, facility type (p-value)	NA	0.125

Standard errors in parentheses. *** (**, *) indicates significance at 1% (5%, 10%) level. Sample includes 798 consultations and 28 facilities. Case mix controls included are patient age, sex, education, illness severity (proxied with an AM/PM dummy) and presence of fever, cough or diarrhea as a primary symptom.

Table 6.11. OLS regression of average effort on social attitudes, additional specifications as indicated by the column headers

Independent Variables	Baseline	Ability	Patient Volume	Facility Type
Social information	-0.008 (0.041)	-0.022 (0.045)	0.002 (0.041)	-0.005 (0.039)
Pride	-0.06 (0.053)	-0.067 (0.05)	-0.062 (0.053)	-0.063 (0.055)
Attribute of fairness	0.074*** (0.028)	0.043 (0.031)	0.077*** (0.028)	0.071** (0.028)
Social information x Pride	0.109*** (0.04)	0.09** (0.036)	0.099** (0.039)	0.104*** (0.04)
<i>Clinician characteristics</i>				
Sex	-0.06 (0.04)	-0.035 (0.031)	-0.05 (0.038)	-0.055 (0.04)
Age	-0.003** (0.001)	-0.003* (0.002)	-0.004** (0.001)	-0.003** (0.001)
Education (yrs)	-0.002 (0.01)	0.009 (0.01)	0 (0.01)	-0.002 (0.01)
Income	-0.021 (0.016)	-0.015 (0.019)	-0.012 (0.017)	-0.019 (0.015)
Visit 2 dummy	0.054** (0.022)	0.050** (0.024)	0.055** (0.023)	0.055** (0.022)
Visit 3 dummy	0.022 (0.024)	0.018 (0.024)	0.023 (0.024)	0.024 (0.024)
Practice Ability		0.337 (0.215)		
Private Facility				0.056 (0.049)
NGO Facility				0.068 (0.056)
Large Facility			-0.085 (0.055)	
Constant	0.905*** (0.175)	0.500 (0.318)	0.865*** (0.183)	0.85*** (0.187)
R-squared	0.173	0.191	0.218	0.211
F-test, social attitudes (p-value)	0.004	0.017	0.012	0.011
F-test, facility type (p-value)	NA	NA	NA	0.421

Standard errors in parentheses. *** (**, *) indicates significance at 1% (5%, 10%) level. Sample includes 798 consultations and 28 facilities. Case mix controls included are patient age, sex, education, illness severity (proxied with an AM/PM dummy) and presence of fever, cough or diarrhea as a primary symptom.

Table 6.11.(continued) OLS regression of average effort on social attitudes, additional specifications as indicated by the column headers

Independent Variables	Facility Type and Patient Volume	Ability and Patient Volume	Ability and Facility Type	Ability, Facility Type and Patient Volume
Social information	0.004 (0.041)	-0.012 (0.042)	-0.018 (0.041)	-0.004 (0.036)
Pride	-0.065 (0.055)	-0.069 (0.05)	-0.069 (0.053)	-0.07 (0.053)
Attribute of fairness	0.075*** (0.028)	0.044 (0.03)	0.033 (0.03)	0.032 (0.029)
Social information x Pride	0.096** (0.039)	0.076** (0.036)	0.078** (0.039)	0.06 (0.041)
Practice Ability		0.372* (0.195)	0.386** (0.195)	0.434** (0.17)
<i>Clinician Characteristics</i>				
Sex	-0.046 (0.039)	-0.021 (0.028)	-0.023 (0.031)	-0.005 (0.028)
Age	-0.003** (0.001)	-0.003* (0.002)	-0.003* (0.002)	-0.003* (0.001)
Education (yrs)	0.001 (0.01)	0.013 (0.011)	0.011 (0.011)	0.015 (0.012)
Income	-0.011 (0.016)	-0.004 (0.019)	-0.011 (0.017)	0.001 (0.016)
Visit 2 dummy	0.056** (0.022)	0.051** (0.024)	0.051** (0.024)	0.053** (0.023)
Visit 3 dummy	0.025 (0.024)	0.019 (0.024)	0.021 (0.024)	0.022 (0.024)
Private Facility	0.041 (0.047)		0.092** (0.036)	0.083** (0.036)
NGO Facility	0.062 (0.045)		0.076 (0.054)	0.07* (0.041)
Large Facility	-0.08 (0.053)	-0.103** (0.05)		-0.098** (0.044)
Constant	0.818*** (0.187)	0.419 (0.308)	0.374 (0.300)	0.267 (0.279)
R-squared	0.240	0.252	0.245	0.281
F-test, social attitudes (p-value)	0.020	0.042	0.118	0.240
F-test, facility type (p-value)	0.386	NA	0.039	0.039

Standard errors in parentheses. *** (**, *) indicates significance at 1% (5%, 10%) level. Sample includes 798 consultations and 28 facilities. Case mix controls included are patient age, sex, education, illness severity (proxied with an AM/PM dummy) and presence of fever, cough or diarrhea as a primary symptom.

Table 6.12. OLS regression of average effort on negative social attitudes; column headers refer to the definition of social attitude variables in each regression, labeled "Social Information" and "Pride" in the row headers

Independent Variables	Positive changers		Negative changers + No-changers
	+ No-changers	Negative changers	
Social information	-0.091* (0.056)	0.056 (0.061)	-0.100* (0.056)
Pride	-0.026 (0.031)	-0.009 (0.078)	-0.048 (0.048)
Attribute of fairness	0.067** (0.031)	0.067** (0.031)	0.073*** (0.028)
Social information x Pride	0.036 (0.086)	0.036 (0.086)	0.108*** (0.039)
<i>Clinician characteristics</i>			
Sex	-0.04 (0.043)	-0.04 (0.043)	-0.058 (0.040)
Age	-0.002 (0.002)	-0.002 (0.002)	-0.003*** (0.001)
Education (years)	-0.003 (0.008)	-0.003 (0.008)	0.002 (0.010)
Income	-0.019* (0.012)	-0.019* (0.012)	-0.020 (0.016)
Visit 2 (observer present)	0.057*** (0.021)	0.057*** (0.021)	0.053** (0.022)
Visit 3	0.029 (0.022)	0.029 (0.022)	0.022 (0.024)
Constant	0.865*** (0.149)	0.783*** (0.152)	0.943*** (0.192)
R ²	0.161	0.161	0.168
F-test, social attitudes (p-value)	0.066	0.066	0.004

Standard errors in parentheses. *** (**, *) indicates significance at 1% (5%, 10%) level. Negative changers are those who less in T2 than T1 and/or less in T3 than T1. No-changers are those that do not change from T1 to T2 or T1 to T3. Sample includes 798 consultations and 28 facilities. Case mix controls included are patient age, sex, education, illness severity (proxied with an AM/PM dummy) and presence of fever, cough or diarrhea as a primary symptom.

Chapter 7 : Sensitivity Analysis

7.1. Introduction

This chapter presents a series of additions and alterations to the model specification in Chapter 6 as checks to the sensitivity of the results. We address the potential for non-random assignment of patients to clinicians in the field (identification strategy), omission of ability as an independent variable and plausible inaccuracy of the effort measure (dependent variable). Overall we report that the results in the main estimation are robust to specification error and alternate independent variable. The effect of random or non-random assignment is more nuanced, but there is evidence that non-random assignment over patient unobservables may influence results. We begin with a discussion of random assignment, followed by a detailed treatment of estimating clinician practice ability and adjusting the dependent variable to account for item difficulty.

7.2. Investigating random assignment

Identification of the impact of social attitudes relies on the institutional characteristics of the Tanzanian health care system that suggest a more or less random assignment of patients to clinicians, within facility. In some medical systems, patients choose their physician and thus thwart any chance that a clinician's case-mix (or patient mix) is random. In Tanzania, however, patients do not choose their clinicians. Rather they choose the facility of where to seek care. Receptionists distribute patients to clinicians rather than patients choosing clinicians. If patients are randomly assigned to doctors there should be

no omitted variable bias as a result of unobserved patient characteristics. On the other hand, if random assignment does not hold and patient characteristics are correlated with clinicians' social attitudes, the coefficients of social information and pride responsiveness will be biased. Suppose, for example, that patients can influence to whom they are assigned or that receptionists practice some form of non-random assignment (e.g. favoritism or trying to match patients to doctors according to severity of illness and clinician skill). This kind of sorting could invalidate our central identifying assumption.

It is meaningful to adjust for this bias as it may be substantial if randomness does not hold. Small amounts of bias do not affect our main conclusions or any policy recommendations because we do not treat the analysis as causal or structural. We aim only to determine the magnitude, sign and significance of the relationship between social attitudes and effort. But the bias could be substantial (e.g. change the magnitude) if the endogeneity results from a clinician having the same type of patient on average over time, such that the exposure to their patients changes the way they perceive themselves or their job. Self-perception in turn shapes their response to social information and pride cues in the laboratory. The omitted variable influence then enters twice into the analysis; once to determine the laboratory measures and once to influence effort. It is thus important to explore the potential for non-random assignment and understand how such non-randomness may impact the coefficient estimates.

Our analysis of the randomization of patients to clinicians is three-pronged. We use multivariate ANOVA (MANOVA) to look specifically at the distribution of patient characteristics by clinician. We also present results from splitting the sample according to those facilities where assignment appears random and those where it does not. Lastly, we

rerun our regressions controlling for very small facilities, where patients essentially choose the clinician once they commit to a facility.

7.2.1. Evidence of non-random assignment

First, we report results from the MANOVA procedure. The aim of the MANOVA is to determine which facilities, if any, appear to have non-random assignment of patients to clinicians over observable characteristics. We perform the MANOVA with observables and take the results as suggestive of some facilities having non-random assignment over unobservables. MANOVA treats each clinician as a plausible treatment and evaluates if knowing the clinician can “predict” the characteristics of the patient. By design MANOVA tests the hypothesis that the clinician effects are jointly non-random with respect to outcomes. We essentially regress each characteristic on clinician dummies and test the joint hypothesis that the coefficients on the doctors are equal to zero in all equations – there is one equation for each patient char. If the coefficient on Dr. Smith is equal to zero for all patient characteristics, we conclude that Dr. Smith does not have a predictable case mix. If this holds for all the clinicians in the facility, we treat this as evidence of more or less random assignment of patients across clinicians in that facility. Any such test of our random assignment by facility assumption has the limitation that we cannot evaluate the randomness of patient mix for facilities that have only one clinician. For one-clinician facilities the equivalent random assignment assumption would be that patients choose the facility more or less randomly. This is probably an invalid assumption and we adjust our analysis for one- and two-clinician facilities later in this section. Omitting the one-clinician facilities reduces the sample on which we run MANOVA to 23 facilities. Across these 23 facilities there is an average of 48.65 patients per clinician.

The distribution is left skewed, with a median equal to 51 patients per clinician. We run the MANOVA separately for each facility and evaluate the random assignment of the (observable) patient characteristics age, sex, years of education and presence of fever, cough or diarrhea.

Multivariate analysis of variance reveals that while assignment of patient characteristics is more or less random within the facility, there is evidence of sorting by age or gender in some facilities. We reject the null hypothesis that the distribution of characteristics among clinicians is random for nine of the 23 facilities. These nine facilities constitute 21% of all facilities, 38% of all clinicians and 39% of patients in the data. The nine facilities account for 39% of all facilities with more than one clinician, 46% of corresponding clinicians, and 47% of the corresponding patients. Together with the one-clinician facilities, the nine with potential non-random assignment make up over half of the patient observations (56%) in the sample and 56% of the clinicians.

While a violation of our identifying assumption, the non-random assignment appears to follow a predictable pattern that may not be correlated with unobservables. For those facilities with potentially non-random assignment we recover the marginal effects of each characteristic to determine if one or more of them has significant explanatory power on clinician-characteristic match. Table 7.1 lists significance for each facility's characteristic equations, for the nine facilities with evidence of non-random assignment. The significance values are for the joint hypothesis that the clinicians explain variation in a given patient attribute. Values reporting conventional levels of significance are bolded. Looking at the bolded values we can see strong evidence of sorting on age and gender in many of these facilities. Also, what looks like sorting on symptom (fever and diarrhea)

may in fact be the result of sorting on age, where children represent the majority of the cases of these symptoms. For example, children in developing countries suffer disproportionately from diarrheal diseases (Lopez et al., 2006). This is also true in our data – children account for 32% of all patients but 48% of the diarrhea cases. Malaria, the most common cause of fever in the area, is a leading cause of death among children in Africa (Rosenberg, 2007) In Northeast Tanzanian, the burden of the disease falls primarily on children (Lusingu et al., 2004; Winskill et al., 2011). Thus the nonrandom matching appears to be due to the overriding pattern that some clinicians are *de facto* pediatricians or women’s health care providers.

Note that non-random assignment due only to this apparent pattern on observables does not pose a problem for our analysis, unless it is indicative of non-random assignment over unobservables. Recall that we control for all of these observable characteristics in our estimations. They are the variables that we refer to as controlling for case mix. Since we control for case-mix, non-random assignment relative to these variables will not impact our estimates. Nonetheless the evidence of non-random assignment on observables may suggest a more insidious problem. That in some facilities the patients are not perfectly randomly distributed on observable means that a) receptionists sort on observables, b) certain types of patients seek certain doctors, by facility, c) patients ignore the orders of the receptionist and queue up with others who are like them (i.e. herding behavior) and/or d) unobservable patient characteristics are correlated with clinician characteristics. The trick would be if there is an unobservable patient characteristic that is important for effort and correlated with attitudes. Then our estimates are biased. Estimating MANOVA gives us an idea of this. If we do not have

randomization on observables it is less likely that we have randomization on unobservables. That being said, the overriding pattern we describe above may suggest that sorting on observables is independent of sorting on unobservables; receptionists may indeed practice some form of triage, but may pay little attention to sorting patients beyond accounting for the patients' medically relevant, observable attributes. In the next two subsections we aim to determine whether the main results from our estimations obtain when we account for potential non-random assignment.

7.2.2. Controlling for facilities with random assignment

Is the pattern really all there is, or are there unobservables correlated with social attitudes that are also non-randomly assigned? If the presence of *de facto* pediatricians and women's health practitioners is the extent of the sorting, we would expect clinicians from non-random assignment facilities to behave no differently than those from random assignment facilities, controlling for patient observables. Recall that random assignment is important for two reasons: 1) we depend on it for defending the unbiasedness of our estimates and 2) even if the non-random assignment is not correlated with social attitudes it may impact the efficiency of our estimates. We estimate our baseline model including a dummy variable to control for non-random assignment and find that the average quality of care does not differ among those that are randomly assigned. The variable labeled "Facility with Non-Random Assignment" (Table 7.2) is a dummy variable equal to one for facilities with non-random assignment. This model assumes that clinicians from facilities with non-random assignment will differ in the average effort, if at all. The coefficient on non-random assignment is positive, but insignificant at conventional levels; average effort for any given consultation is not different between facilities with

random and non-random assignment. More importantly, the economic interpretation of our results does not change when controlling for non-random assignment, as the pattern of sign and significance in our social attitude variables remains.

7.2.3. Controlling for number of clinicians per facility

A special case where random assignment may not hold is very small facilities, which are often owned and operated by a single clinician. In this case patients choose the facility and the doctor simultaneously. We again estimate our main model, this time controlling for facility size. Small facilities are defined as having one or two clinicians in our sample²⁴. This is a rough estimate of facility size since we did not sample all clinicians from each facility. Nonetheless, there were more frequently more clinicians in the study for the larger facilities. 26 facilities, 35 clinicians and 1,464 patients in our data are associated with small facilities. This constitutes 60%, 34% and 32% of all facilities, clinicians and patients in our sample, respectively. We use number of clinicians for this analysis instead of patient volume since number of clinicians is the variable that may thwart random assignment. Like controlling for non-random assignment, controlling for number of clinicians in the facility does not change the sign, significance, or magnitude of the original results. The coefficient on the size indicator itself is also not significant, though it is significant and negative when we use a continuous version. Results appear in Table 7.2.

What we have shown in this random assignment analysis is that although there is evidence of non-random assignment in some facilities, being in one of the facilities with

²⁴ We only have data on number of clinicians at each facility for a subset of facilities, so we cannot use that variable to define size.

non-random assignment or in one of the small facilities is not correlated with social attitudes to the extent that it changes our primary result. The regression analyses controlling for facility size or non-random assignment come with the caveat that the two subsamples would not behave differently for any other reason than being in one of the groups of random or non-random assignment. That is, we assume there is no confounding factor perfectly correlated with the division of the data in this way that would impact behavior. This assumption is more difficult to substantiate with respect to facility size (i.e. number of clinicians per facility) because of the previously mentioned correlations between facility size (in terms of patient volume), facility type, practice ability and social attitudes. Importantly, it may be true that certain subpopulations display meaningful patterns in terms of social information and pride responsiveness and effort. We reiterate that modeling this relationship is outside of the scope of this paper. Finally, the element of random assignment that we aimed to address with this analysis is non-random assignment with respect to unobservables. We took an indirect route, treating non-random assignment on observables as potential evidence of non-random assignment over unobservables. Closer inspection revealed a predictable pattern that would not be overtly correlated with unobservables. Further, controlling for membership in a group with potential non-random assignment does not change the sign or significance found in our original estimations. Thus, we conclude that while non-random assignment is evidence against one of our primary assumptions, it does not severely impact the underlying results.

7.3. Including clinician ability and item difficulty

In this section we elaborate on the measurement of clinician practice ability. Skill, or ability, is almost always a cause of concern in regressions that try to explain performance (Angrist and Krueger, 1999; Card, 1999). Although we do not have reason to believe that ability is correlated with pride or social information responsiveness, it certainly helps to explain variation in tasks completed. Also, ability as an omitted variable may affect the variance-covariance estimates. In order to adjust for this omitted variable we estimate it using a latent variable estimation model called Item Response Theory (IRT). The theory comes out of the field of educational statistics. Practitioners use it to evaluate classroom-type test construction and examinee competence or skill. We use a 2 parameter IRT model to jointly estimate item/task difficulty and clinician ability. The basics of IRT are covered in some detail below, followed by summary of the ability estimates and a more extensive section on incorporating item/task difficulty into our analysis.

7.3.1. Item Response Theory

Item response theory (IRT) offers a technique for estimating both clinician ability and item or task difficulty, where difficulty and ability are considered latent variables. IRT relies on the idea that we can explain performance on a test item based on a set of examinee and item-specific latent variables. It also specifies that we can model the relationship between performance and the latent traits as a monotonically increasing logistic function. Skill or ability is the most common latent examinee trait measured and its interpretation is a function of the test itself. IRT techniques also allow estimation of

item difficulty, which is constant for each item across all examinees. In our case, the examinees are clinicians. The item difficulty scores can be thought of as capturing the common component of how challenging an item is to complete, independent of the facility or available equipment. Constant item difficulty ignores that facility level or patient level characteristics that may complicate how hard any item is to complete. We control for this using facility and case-mix controls. The IRT model is

$$p(x_{it} = 1) = \frac{e^{a_t(\theta_i + b_t)}}{1 + e^{a_t(\theta_i + b_t)}}$$

where $p(x_{it}=1)$ is the probability that clinician i completes item t successfully, θ_i is examinee ability and b_t is the difficulty of completing item t correctly. The other parameter in this model, a_t , is the slope parameter. It represents how well performance on an item signals true high ability or true low ability. The estimate of a_t is called the item's discrimination score. For an item with a higher discrimination score, the clinician's ability will play more into the probability of completing the item. A two-parameter model also allows for estimation of item discrimination, or the extent to which. Because model fit cannot be reliably tested directly (Hambleton et al., 1991), choosing between a one or two parameter model requires understanding of what the true relationship between observable performance and latent variables may be. In the context of our sample we follow Leonard, Masatu and Vialou (2007) and use a 2-parameter specification to estimate clinician ability as well as the two parameters item difficulty and item discrimination.

Using the tools of IRT requires three assumptions: unidimensionality, local independence and that the logistic function specified captures the “true” relationship between the latent variables, or unobservables, and the observed item responses. First, we briefly discuss the extent to which our context satisfies these assumptions and the impact violations have on the properties of our estimates. We then provide details on how we use the IRT techniques to construct the difficulty-weighted measure of effort. Finally, we present results from running the baseline and augmented regression analyses with this new dependent variable.

Unidimensionality

The unidimensionality assumption is that there is one dominant latent trait that a test measures. This is most commonly labeled as the skill or ability relevant to the type of test. For example, data from a math test will lead to measurements of mathematical ability. Interpretation can be even more precise: data from a calculus test can be used to estimate examinee calculus ability. We assume a single latent trait, practice ability, or ability to practice the required protocol items learned during a clinician’s medical education. If in fact the estimated θ is a combination of ability and some other trait, ξ , such that $\theta = ABILITY + \xi$, the main estimating equation estimates on the corresponding coefficient will suffer from attenuation bias. Violations of strict unidimensionality such as this are common, however, and IRT models are robust to most violations in the presence of a *single dominant* trait (Drasgow and Parsons, 1983; Harrison, 1986). Figure 7.1, a scree plot from a factor analysis of task completion by symptom, confirms that

there is one dominant latent trait in the RCR data.²⁵ This result is consistent with results reported in Leonard, Masatu and Vialou (2007) and in Das and Hammer (2005), which uses the same instrument to measure ability. The upshot is that the assumption of unidimensionality appears to hold in our data to the extent that it matters for estimating latent ability and difficulty parameters.

Local Independence

Local independence is the characteristic that the probability of a correct response to each item is independent of the probability of a correct response for any other item, conditional on ability. Formally, let $p(x_{it}|\theta_i)$ be the probability of a correct response to task t , where x_{it} is the response given to task t and θ_i is the test taker i 's ability. Local independence means

$$p(x_{1j}, x_{2i}, \dots, x_{ni} | \theta_i) = p(x_{1i} | \theta_i) p(x_{2i} | \theta_i) \dots p(x_{ni} | \theta_i)$$

This is the same as conditional independence. Thus it is assumed that the correlation in performance across items for an examinee is only due to ability (i.e. the dominant factor that influences performance). We rely on the result that given unidimensionality, local independence always holds (Hambleton et al. 1991). In the absence of local independence the standard errors of the ability estimates will be understated (Ip, 2001). This, in turn, would lead to inflated coefficient estimates and deflated t-values for ability in the main estimating equation. Since ability is being added as a control, rather than as a variable of

²⁵ The analysis includes data from all clinicians in the sample, not just those in the restricted sample of our main estimating equation. Using all of the data to confirm unidimensionality is more precise since the restricted sample almost certainly suffers from a selection bias, as it is composed of those that chose to attend the dissemination conference and classroom experiments.

central interest, we are less concerned about imprecision of ability's parameter estimates due to inaccurate standard error estimates. This brings us back to the reliance on unidimensionality, the factor analysis above suggesting unidimensionality and the results in the literature showing that IRT is robust to violations of that assumption.

Estimates of difficulty and discrimination for each item appear in Appendix C. Practice ability has a uni-modal distribution that ranges from 0.41 to 0.95, with a median of 0.62, a mean of 0.63 and a standard deviation of 0.10. Regressions with practice ability appear in Chapter 6. Including ability as an independent variable in our equations of interest does not change the results for those that are both pride and social information responsive, but the quality of being fair in the laboratory loses its significance, as does the quality of being pride responsive. Specific results and discussion can be found in Chapter 6.

7.4. Difficulty-weighted effort

As we discuss above, the IRT procedure also produces difficulty estimates for each item. We use these estimates in order to construct a difficulty-weighted average effort for each consultation, thus taking advantage of the item level detail in the data. While running our analysis at the consultation level simplifies estimation of the variance-covariance matrix, thus making the estimates in some sense more reliable, doing so ignores the information available in the task level nature of the data. Some tasks are more difficult than others. This detail is not captured in the analysis. We thus provide sensitivity check estimations that use difficulty-weighted average effort as the dependent variable. In brief, we find that primary results are robust to the alternate specification. The first subsection describes

the construction of the variable and the second subsection presents the results from the estimation.

7.4.1. Constructing the weighted average

In this section we elaborate on an alternative way to measure clinician effort that incorporates more information from the tasks performed in each consultation. The alternative measure is a weighted version of the original measure from our estimating equation in Chapter 6. Recall that $Effort_{ijk}$ from Chapter 6 is the average performance of clinician i at facility j for patient k . This can be expressed as

$$Effort_{ijk} = \frac{1}{\bar{p}_k} \sum_{t=1}^{\bar{p}_k} p_{t,ijk}$$

where p_{tk} is equal to one if the clinician completed symptom-specific task t for patient k and \bar{p}_k is the total number of tasks required by protocol for the symptoms patient k presents. Note that the total number of protocol items, by symptom, is constant across clinicians and facilities. It is also constant across patients with the same presenting symptoms. The k subscript, while a slight abuse of notation, captures that the number of tasks required depends on the information the patient brings in to the consultation, his symptoms, and that this varies across patients. This measure of effort accurately reflects a clinician's ability to apply the skills learned in school in the work setting, but it falls short in that it does not account for varying degrees of item difficulty. We can account for item difficulty by building a difficulty-weighted measure of effort, which we will call $WEffort_{ijk}$.

We use item response theory estimates of item difficulty as weights in the calculation of $WEffort_{ijk}$. Difficulty estimates are obtained simultaneously with the ability and discrimination estimates as is described above. Task difficulty, b_t , is defined as the point on the ability scale where the probability of a correct answer for task t is equal to 0.50. Thus this parameter indicates how skilled one has to be to have an even chance of performing the task when it is required. Because the ability scale is centered on zero, b_t can take positive or negative values. The scale of the difficulty parameter is arbitrary and difficulty measures only have meaning in reference to one another. Scale of the depending variable, however, is not arbitrary; bounding predicted values between zero and one makes the marginal effects easily interpretable. In order to retain the scaling of the dependent variable we adjust b_t to range between zero and 1, with difficult items given higher weight values. Costless items are not included in the weighted effort calculation. The difficulty weights we denote as \tilde{b}_t , where $\tilde{b}_t \in [0,1]$ captures the relative nature of the difficulty estimates, but allows for some items to be costless (effortless) to perform. To construct the new dependent variable we multiply each item by a difficulty weight, \tilde{b}_t . The difficulty adjusted effort measure is

$$WEffort_{ijk} = \frac{1}{\bar{p}_k} \sum_{t=1}^{\bar{p}_k} \tilde{b}_t p_{t,ijk}$$

Table 7.3 has the descriptive statistics for the two different dependent variables and the difficulty weights. A full table with all the weights by survey item can be found in the appendix. While at first glance the weighted and un-weighted dependent variables look very different, recall that the weights not only apply different values to completing each

item but that those values are between zero and one. Multiplying the weight by the binary zero/one task variable condenses the dependent variable's range. Where the original dependent variable can be equal to 1 any time the clinician does all the items required of him by protocol for that consultation, the weighted value will be less than one (unless all required items have a weight of 1 themselves). $WEffort_{ijk}$ thus serves as an alternate way to measure effort, the dependent variable, that incorporates information on item difficulty.

Primary results on the role of social information and pride obtain in the difficulty-weighted effort regressions (Table 7.4), with one main exception. In all difficulty-weighted specifications the size of the coefficient on the interaction term, social distance and pride responsive, is reduced by approximately half. A smaller coefficient may mean that the size of the original coefficient was driven by performance of relatively easy items and that the clinicians deemed as putting forth more effort simply performed more of the easy items. Clinicians deemed as putting forth less effort may have been in fact working just as hard but performing fewer, more difficult items. More likely, the reduction in the size of the coefficient is a result of the fact that the difficulty weighting changes the range of the dependent variable. The reduced range, with the maximum approximately half of that for the unweighted version, also limits the value of coefficients for variables that explain deviations from the mean, such as the social preferences dummies. As in the original results, the coefficient on the interaction term cannot be distinguished from zero in the most extensive specification. The most extensive specification controls for practice ability, facility type and average daily patient volume for each facility. Also as in the main estimations of Chapter 6, we see in these results that the significance of preferences

for fairness is not robust to alternate specifications. Finally, pride is not significant in any of the difficulty-weighted specifications. Likewise the marginal effect of pride is insignificant across the board. As is the case with the non-weighted dependent variable, practice ability does not gain significance unless we control for patient volume and/or facility type concurrently, at which point it is positive and significant in explaining variation in clinician effort. Case mix remains important in these results, as does patient volume and being in a private facility. Thus the difficulty-weighted dependent variable yields the same pattern of results as we see with the unweighted effort variable, albeit with coefficients of different values.

Adjusting the dependent variable for item difficulty is an effective way to incorporate the item level detail from the data into the analysis. Where as the dependent variable in the main estimations of Chapter 6 is a simple average of protocol items completed for a consultation, the weighted dependent variable presented here allows the score to vary with item difficulty. This means that clinicians doing fewer but more difficult items may obtain the same or even higher effort score than clinicians completing many relatively easy items. We used item difficulty estimates from an item response theory procedure, adjusted to a [0,1] scale, as the difficulty weights and constructed a weighted average of symptom-specific protocol items completed for each consultation. While this construction incorporates more detail into the dependent variable, using it does not change the pattern of results. This alternative specification does, however, yield different estimate values than in the original analysis, though no difference is so drastic as to produce a change in estimate magnitude. Since the estimation is reduced form, the exact value of the estimate has little meaning. Rather, the magnitude and significance are what

matter. Thus, we conclude that the difficulty-weighted specification, while perhaps more precisely representing effort, supports the interpretation and conclusions corresponding to the primary estimations of Chapter 6.

7.5. Conclusion

This Chapter presents a sensitivity analysis to augment the results in Chapter 6. We address the potential for non-random assignment of patients to clinicians in the field, omission of ability as an independent variable and plausible inaccuracy of the effort measure (dependent variable). With these various alterations to the model, the sign, significance and magnitude of the primary results remain, though the impact of potential non-random assignment is not fully understood.

For the analysis of our assumption of random assignment we looked at the distribution of observable characteristics across clinicians. We found that some clinicians did appear to have concentrations of certain characteristics, a result inconsistent with random assignment on observables. Certain clinicians were significantly more likely to have female patients or children (paired with the diseases for which children bare the larger burden in the population). This predictable pattern of sorting on observables suggests that some clinicians are *de facto* specialists. If patients are being funneled to certain clinicians due to observables only, this does not compromise our results since we already control for the patient characteristics. As long as there are not any unobservables that sort along these same lines (sex or age) then this result is not indicative of non-random assignment on unobservables. Controlling for non-random assignment and number of clinicians in a facility, which may thwart random assignment, does not impact our main results. That

being said, there are very likely interesting and important subgroups of the sample for which the relationship between social preference and workplace effort is distinct. Studying these relationships and the role of facility culture in determining social preferences we leave to future research.

The second and third elements of this chapter were to explain the estimation of practice ability, which we used in Chapter 6, and to estimate the model with a difficulty-weighted effort variable. Estimating ability and difficulty weights both involved a latent variable estimation technique called Item Response Theory (IRT). The IRT procedure is essentially an iterated maximum likelihood procedure that estimates difficulty parameters and the latent variable practice ability simultaneously. We report summary statistics for practice ability. We estimate the main model with the difficulty-weighted dependent variable. Results from this difficulty-weighted estimation do not differ from our main results. Thus, while the difficulty-weighted specification incorporates more information into the estimation, the added information does not appear to have a bearing on the relationship between social attitudes and workplace effort.

With this we conclude that neither potential non-random assignment nor using difficulty-weighted average effort as the dependant variable appreciably compromise the strength and economic interpretation of our central results.

7.6. Tables and figures

Table 7.1. Checking random assignment, results from a MANOVA by patient characteristics age, sex and education (p-values on F tests for facilities with non-random assignment)

Facility ID	N	# of clinicians	Patient age	Patient sex	Patient education
8	99	2	0.929	0.769	0.982
9	165	4	0.426	0.499	0.031
14	335	9	0.013	0.540	0.320
15	93	2	0.574	0.235	0.004
21	155	3	0.000	0.519	0.141
28	173	3	0.000	0.001	0.350
33	51	2	0.004	0.870	0.545
35	362	9	0.000	0.013	0.000
37	98	3	0.008	0.000	0.637

Table 7.1.(continued) Checking random assignment, results from a MANOVA by primary symptom (p-values on F tests for facilities with non-random assignment)

Facility ID	N	# of clinicians	Fever	Cough	Diarrhea
8	99	2	0.073	0.012	NA
9	165	4	0.062	0.548	0.460
14	335	9	0.017	0.123	0.178
15	93	2	0.525	0.273	0.346
21	155	3	0.536	0.908	0.309
28	173	3	0.024	0.489	0.582
33	51	2	0.021	0.075	NA
35	362	9	0.002	0.206	0.232
37	98	3	0.076	0.158	0.141

Table 7.2. OLS estimation controlling for non-random assignment and facility size (clinicians per facility)

Independent Variables	Controlling for non-random assignment	Baseline, controlling for # of Clinicians (binary)	Controlling for # of clinicians (continuous)
Social Information (S.I.) Responsive	-0.016 (0.042)	-0.006 (0.042)	-0.001 (0.039)
Pride Responsive	-0.062 (0.052)	-0.06 (0.055)	-0.06 (0.053)
Fair	0.071** (0.029)	0.074*** (0.028)	0.069** (0.028)
S.I. and Pride Responsive	0.118*** (0.037)	0.107*** (0.039)	0.098** (0.043)
Non-Random Assignment (facility)	0.055 (0.052)		
3 or more Clinicians at the Facility		-0.008 (0.053)	
Number of clinicians per facility			-0.018** (0.007)
<i>Clinician Characteristics</i>			
Sex	-0.065* (0.037)	-0.058 (0.038)	-0.046 (0.037)
Age	-0.004*** (0.001)	-0.003** (0.001)	-0.003** (0.001)
Years of Education	-0.003 (0.009)	-0.002 (0.010)	0.000 (0.010)
Income	-0.025 (0.017)	-0.019 (0.016)	-0.014 (0.014)
Visit 2 (Observer present)	0.053** (0.022)	0.054** (0.023)	0.056** (0.022)
Visit 3	0.021 (0.024)	0.023 (0.024)	0.024 (0.024)
Constant	0.912*** (0.171)	0.899*** (0.175)	0.896*** (0.168)
Case mix controls	yes	yes	yes
N	798	798	798
R-squared	0.169	0.176	0.247

Standard errors in parentheses. *** (**, *) indicates significance at 1% (5%, 10%) level. Case mix controls include patient age, sex, education, illness severity (proxied with an AM/PM dummy) and presence of fever, cough or diarrhea as a primary symptom.

Figure 7.1. Scree Plots from a factor analysis of task completion, by symptom

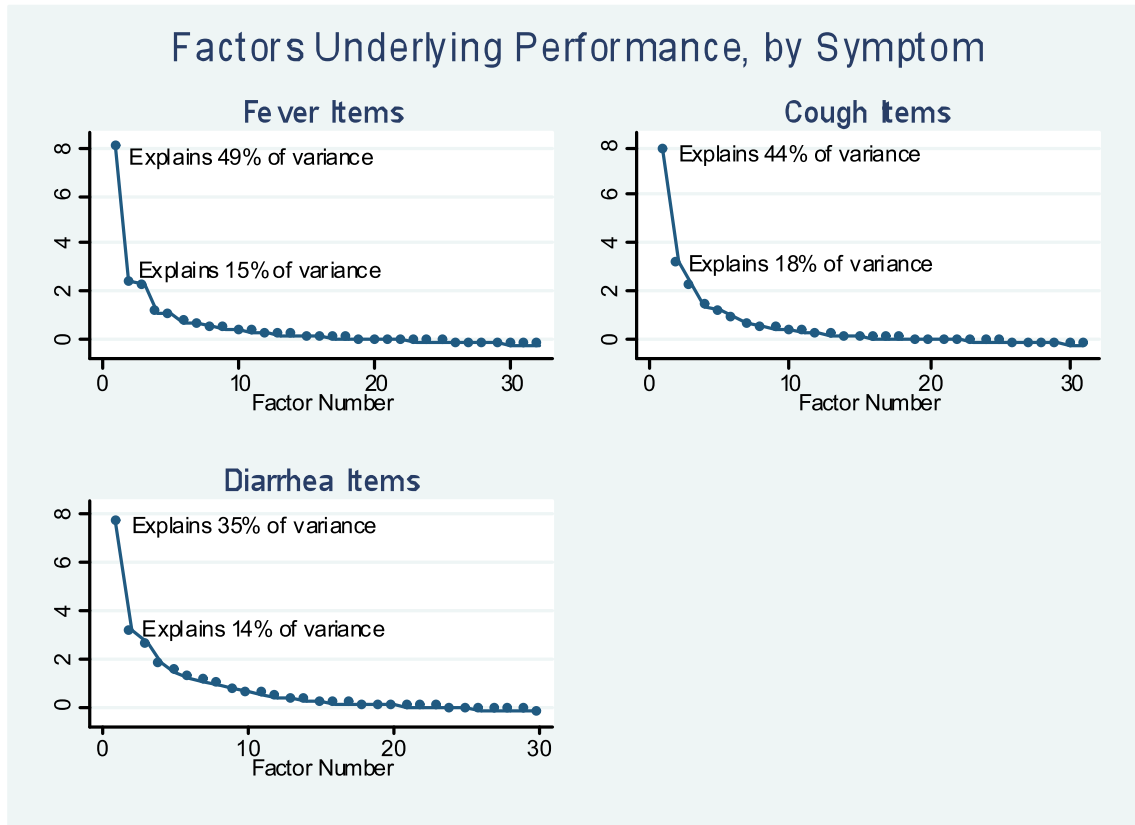


Table 7.3. Summary statistics for the difficulty weighted average and related variables

Variable	Mean	Std. Dev.	Minimum	Maximum
Average effort	0.77	0.25	0	1.00
Average Effort with Difficulty Weights	0.32	0.11	0	0.44
Difficulty Weights	0.46	0.11	0	1.00

Table 7.4. OLS regression of difficulty-weighted average effort on social attitudes, controlling for practice ability

Independent Variables	Baseline	Baseline with Ability
S.I. Responsive	-0.006 (0.024)	-0.014 (0.025)
Pride Responsive	-0.034 (0.029)	-0.038 (0.027)
Fair	0.047*** (0.017)	0.029 (0.018)
S.I. and Pride Responsive	0.064*** (0.023)	0.053** (0.021)
Sex	-0.036 (0.023)	-0.021 (0.018)
Age	-0.002** (0.001)	-0.002 (0.001)
Education	-0.001 (0.005)	0.005 (0.006)
Income	-0.012 (0.009)	-0.009 (0.011)
Practice Ability		0.191 (0.124)
Visit 2	0.032** (0.013)	0.03** (0.013)
Visit 3	0.013 (0.013)	0.011 (0.013)
Private Facility		
NGO Facility		
Patient Volume		
Constant	0.496*** (0.096)	0.266 (0.182)
Case mix controls	yes	yes
N	798	798
Overall R squared	0.189	0.209

Standard errors in parentheses. *** (**, *) indicates significance at 1% (5%, 10%) level. Case mix controls include patient age, sex, education, illness severity (proxied with an AM/PM dummy) and presence of fever, cough or diarrhea as a primary symptom.

Table 7.5.(continued) OLS regression of difficulty-weighted average effort on social attitudes, various specifications

Independent Variables	Baseline	Baseline with Ability and Patient Volume	Baseline with Ability and Facility Type	Baseline with Ability, Facility Type and Patient Volume
S.I. Responsive	-0.006 (0.024)	-0.008 (0.023)	-0.012 (0.023)	-0.004 (0.02)
Pride Responsive	-0.034 (0.029)	-0.039 (0.027)	-0.039 (0.029)	-0.04 (0.029)
Fair	0.047*** (0.017)	0.029* (0.018)	0.024 (0.018)	0.023 (0.017)
S.I. and Pride Responsive	0.064*** (0.023)	0.045** (0.021)	0.047** (0.023)	0.037 (0.023)
Sex	-0.036 (0.023)	-0.013 (0.017)	-0.016 (0.018)	-0.005 (0.017)
Age	-0.002** (0.001)	-0.002* (0.001)	-0.002* (0.001)	-0.002* (0.001)
Education	-0.001 (0.005)	0.007 (0.006)	0.006 (0.006)	0.009 (0.006)
Income	-0.012 (0.009)	-0.003 (0.011)	-0.007 (0.01)	0 (0.009)
Practice Ability		0.214* (0.111)	0.216* (0.115)	0.247** (0.098)
Visit 2	0.032** (0.013)	0.031** (0.013)	0.031** (0.013)	0.032** (0.013)
Visit 3	0.013 (0.013)	0.012 (0.013)	0.012 (0.013)	0.013 (0.013)
Private Facility			0.046** (0.022)	0.041* (0.022)
NGO Facility			0.039 (0.03)	0.035 (0.023)
Patient Volume		-0.061** (0.027)		-0.058** (0.025)
Constant	0.496*** (0.096)	0.216 (0.174)	0.202 (0.175)	0.137 (0.158)
Case mix controls	yes	yes	yes	yes
N	798	798	798	798
Overall R squared	0.189	0.271	0.257	0.296

Standard errors in parentheses. *** (**, *) indicates significance at 1% (5%, 10%) level. Case mix controls include patient age, sex, education, illness severity (proxied with an AM/PM dummy) and presence of fever, cough or diarrhea as a primary symptom.

Chapter 8 : Conclusion

Empirical research on worker effort choice is the subject of a vast body of work, but thus far the role of social preferences in effort choice has received limited attention. Workers are subject to varying sources of non-monetary incentives, including their own posture toward altruism and the social norms that may be present in their field or organization. One theory of social preferences that may be particularly important in effort choices among health workers is that of Ellingsen and Johannesson (2008) that specifies altruism as a function of the esteem one gains from others due to performing an altruistic act. Health workers are a compelling subject pool for this research because we expect them to be socially minded. Indeed, many enter the profession with the stated purpose of helping people. And while social preferences may motivate clinicians to work hard, exposure for themselves and their patient to risk may complicate the translation of social preferences into increased effort. In this work we used laboratory and field data from a sample of clinicians in Tanzania to explore the nature of clinicians' social preferences and the extent to which such preferences impact workplace effort choices.

We specifically examine the trade-offs between costly exertion of effort and potential social benefits among clinicians in a semi-urban area of Tanzania. Thus, this research combines behavioral economics concepts with development economics to try to better understand effort choices of health workers in a highly resource-constrained setting. We use two unique data sets to test the hypotheses that a) clinicians behave pro-socially in simple dictator games, b) social information and induced pride influence giving in the

laboratory and c) social preferences as measured in the lab help explain the unaccounted for variance in clinician effort.

Our research questions in chapters 5 and 6 centered on the expression of social preferences in the workplace, with a focus on a specific type of impure altruism first outlined by Ellingsen and Johannesson (2008, hereafter referred to as EJ). Do clinicians respond to pride and social information with greater generosity, as the EJ theory would suggest? If so, do those tendencies help explain variation in effort among clinicians at work? We first explore these questions in the laboratory, with pilot using subjects from the general public in College Park, Maryland and then an implementation with the Tanzanian clinicians. Finally, using the laboratory results we construct the measures of social attitudes “social information responsive”, “pride responsive” and “fair”. We use these measures to determine the extent to which social preferences matter in explaining clinicians’ workplace effort. Past research suggests that social identity influences generosity among university students, but is silent about the impact of pride. We had no reason to suspect that social identity results would differ in our samples and indeed we found that dictators from both the pilot and the Tanzanian implementation tended to give more when we provided them with socio-demographic information about their partners. Among the clinicians, social information significantly increases the likelihood of a generous allocation. But social information does not reduce the likelihood of a selfish allocation compared to other treatments. In both samples induced pride increased giving and among clinicians it increases the likelihood of a generous allocation. Notably, induced pride also appears to motivate dictators to settle on a 50/50 allocation of their endowment than in treatments without induced pride. Results from the pride treatments

are suggestive of EJ's theory of social preferences wherein the interaction of social identity and esteem motivate pro-social behavior. We juxtapose these laboratory results with the work of Leonard and Masatu (2006) showing clinicians response to peer esteem. Apparently clinicians are responsive to pride from both patients and peers, though it remains to be seen whether these sources of pride are complements or substitutes. As in Das and Sohnesen (2007) we do not find significant evidence of discrimination in altruism based on social identity or social distance. Linking these behavioral results to data on the subjects' actual workplace effort, we find that while clinicians responsive to only pride or only social information do not behave differently from their peers, those responsive to both stimuli provide on average 10% more effort. A sensitivity analysis shows that these results are robust to various specifications. This work contributes to the literature with a novel experimental treatment testing the impact of pride, a unique sample of health care workers and approximations of real world relationships in the experimental design. Thus, Clinicians' attitudes as measured in the laboratory do appear to explain significant variation in how they behave at work. These also results stand as evidence in support of the EJ theory relating altruism, pride and social identity. More generally, this analysis shows that attributes measured in the lab can illuminate patterns in field data that would be otherwise difficult to identify.

Thus, we assert that social preferences are important in explaining variation in effort and can thus be exploited in building incentive structures for improving effort in this population. The interaction of pride and patient and/or clinician social identity in particular plays a role. Policy ideas stemming from our results should center on activating the social identity-based pride that can lead to higher effort from some clinicians. One

way to do this would be to allow patients to choose their clinician. Lessons to be drawn from this work are limited, however, by the intimate relationship between facility culture and clinician attitudes. Due to limitations of our data we cannot address these questions in this report. Suffice to say, any specific policy recommendations would have to take into account the fact that the role of social preferences may vary by facility size and type. With this we contribute to the research on the role of intrinsic motivation among health workers and, more broadly, that of social preferences in the workplace. We also provide an example of how laboratory data can be legitimately linked to behavior in the field.

While pro-social behavior captured in the laboratory is correlated with clinician effort in the field, it does not tell the whole story. A host of additional factors besides social preferences determines how a clinician will respond to her patient. In particular, a clinician's job is wrought with risk. The clinician treats her patient's not knowing whether or not there will be a favorable outcome – does she ration her effort according to that risk? The clinician also faces risk to her own wellbeing because of constant contact with contagious diseases and liability from making an error. Does she hold back on exerting maximally for her patients in order to protect herself from illness or fatigue related mistakes? In our fifth chapter we address the question of whether risk impacts altruism and if that impact varies with the structure of that risk. We use laboratory experiments in our study of these issues with two implementations: a pilot with university students and an implementation with Tanzanian clinicians. The experiments consisted of variations on a dictator game, which allow us to evaluate changes in generosity when the recipient is exposed to risk or both the recipient and the dictator are exposed to risk, holding expected values constant between treatments and between players. In particular,

we address the issue of whether social preferences are based on comparisons of *final (ex post) payoffs* or on comparisons of *ex ante chances*. By observing decisions in situations that expose the decision-maker, another person, or both to risk, we differentiate between these two preference structures. Surprisingly we find that, among university students, giving under mutual risk is not different from giving when neither partner is exposed to risk; the later is highly predictive of the former. Additionally, dictators are sensitive to the risk borne by their partners. They give less on average but they are more likely to give non-zero amounts. The opposite is true for clinicians, who increase average giving when their partner is exposed to risk. Meanwhile, mutual risk causes clinicians to give significantly less than in the standard dictator game. Thus, while the behavior of a substantial fraction of student subjects is consistent with dictators comparing *ex ante chances*, rather than *ex post payoff*, preliminary results suggest the opposite for clinicians. Future work may focus on determining the underlying structure of preferences or institutions that might lead to such a discrepancy. Future work may also focus on upside and downside risk or variations in expected value. Hence, our research in this direction is a first step in understanding the dynamics of social preferences in the presence of risk. Identifying the whether people dominantly display preferences for procedural fairness or outcomes fairness can inform policy where multiple stakeholders are involved and outcomes are uncertain. In summary, our work complements the literature on social preferences for risk by looking at how dictators' giving varies when the outcome for the recipient is uncertain. We fill in the gap in knowledge about the degree to which the dictator is willing to surrender his or her own wealth or chances to increase the chances of the recipient.

Overall, this research addresses question surrounding the role of social preferences in clinicians' workplace effort decisions. We studied a population of medical clinicians in Tanzania, collecting both laboratory and field data from them. In the laboratory, treatments mirrored the forces potentially influencing clinicians in their everyday workplace decisions. We find strong evidence of social preferences in this population, as well as interesting behavior relative to pro-social behavior and risk exposure. Our results both back-up and build on existing findings. Further, our results speak to the accuracy of theoretical models that include individual heterogeneity of social preferences and the idea that interpersonal aspects of decision making influence pro-social behavior. We also contribute novel results in the field of risk and social preferences.

Appendices

Appendix A: Laboratory Experiment Instructions

A.1. Risk and altruism experiment, UMD students

A.1.1. General Rules

This is an experiment in economic decision making. If you follow the instructions carefully and make good decisions you can earn a considerable amount of money. You will be paid in private and in cash at the end of the session.

It is important that you do not talk, or in any way try to communicate, with other people during the session. If you have a question, raise your hand and a monitor will come over to where you are sitting and answer your question in private.

The experiment will consist of several independent rounds. In each, you will face a specific decision task. Tasks will be explained in detail before you have to make your decision.

In each round, you will be randomly matched with one other participant. This matching will change each round. You will not know which of the other people in the room you are matched with. Likewise, the other people in the session will not know with whom they are grouped.

In each round, you will have the opportunity to earn points. At the end of this session, one of the rounds will be randomly selected as the payment round. You will be

paid in cash an amount that will be determined by the number of ECUs (Experimental Currency Units) you earn during the randomly selected payment round.

At the beginning of the experiment, you will be assigned the role of either Person 1 or Person 2. Those selected for the role of Person 2 will leave the room with one of the experimenters. They will be explained the decision tasks, but then wait until person 1 has made all decisions. They will later be paid in private. That is, the identity of the decision maker (person 1) will not be revealed.

Those selected as Person 1 will remain in the room and will take a seat at one of the computers. Once all of the Person 2 players have left the room, we will explain the decision rules for each of the decision tasks to the Person 1 players. In all rounds, each Person 1 player will decide how to allocate 100 tokens between him- or herself and Person 2.

The total number of tokens must sum up to 100.

That is, **Tokens Kept (TK)** by person 1 and **Tokens Given (TG)** to person 2 add up to 100.

$$\mathbf{TK + TG = 100.}$$

The payoff consequences of the token allocation may differ between the Person 1 and Person 2 and from round to round. Payoff consequences will be explained to all Person 1 and Person 2 players at the beginning of each round.

In each period you should record the number of tokens allocated to you and to the other person on the record sheet.

How earnings are determined

At the end of today's session, one round will be randomly selected as the payment round and payments will be determined based on the ECU earnings that round. Each round has the same probability of being chosen as the payment round. Your payments will be displayed on the computer.

Record the selected round and your profit in *ECU* for that round in the space provided at the bottom of the record sheet.

You will receive **\$1.00** in cash at the end of the session for every **10 ECU** you have earned in the payment round. This amount is recorded in the space titled earnings. In addition, you will earn a **\$5** as show-up fee.

If you have any questions during the experiment, please quietly raise your hand and one of the experimenters will come to you to answer your question. It is important that you do not talk with any of the other participants.

A.1.2. Instructions for the specific rounds – Person 1

Treatment 1

You have been randomly assigned to be Person 1. In this round, you will decide on the number of tokens for each of you that sum to 100.

That is, Tokens kept (TK) and Tokens given (TG) add up to 100.

$$TK + TG = 100.$$

If this round is selected for payments,

You will receive TK ECU

Person 2 will receive TG ECU as payoff

Please enter how many tokens you would like to allocate to Person 2 (TG). Recall, you can choose any number between 0 and 100.

Treatment 2

You have been randomly assigned to be Person 1. In this round, you will decide on the number of tokens for each of you that sum to 100.

That is, Tokens kept (TK) and Tokens given (TG) add up to 100.

$$TK + TG = 100.$$

If this round is selected for payments,

You will receive TK ECU

Person 2 will receive TG out of 100 lottery tickets which gives him or her the chance to win 100 ECU. That is, Person 2 has a TG out of 100 chance of winning 100 ECU.

The more tokens you allocate to the Person 2, the higher are Person 2's chances to win 100ECU, but the smaller will be your own payoff.

For example, if you allocate all 100 tokens to Person 2, the Person 2 has a 100 out of 100 chance to win 100ECU, that is Person 2 wins the prize for sure, while you do not get any

payoff. Alternatively, if you allocate 0 tokens to Person 2, Person 2 has no chance to win the 100ECU prize, while you get a payoff of 100ECU.

Recall, you can choose any allocation to Person 2 between 0 and 100.

Please enter how many tokens you would like to allocate to Person 2(TG):

Treatment 3

You have been randomly assigned to be Person 1. In this round, you will decide on the number of tokens for each of you that sum to 100. You can allocate at most 50 tokens to the other person.

That is, Tokens kept (TK) and Tokens given (TG) add up to 100.

$$TK + TG = 100.$$

If this round is selected for payments,

You will receive TK ECU

Person 2 will receive 2xTG out of 100 lottery tickets which gives Person 2 the chance to win 50 ECU.

That is, Person 2 has a 2xTG out of 100 chance of winning 50 ECU.

The more tokens you allocate to the Person 2, the higher are Person 2's chances to win 50ECU, but the smaller will be your own payoff. For example, if you allocate 50 tokens to Person 2, Person 2 receives 100 lottery tickets and therefore has a 100 out of 100 chance to win 50ECU. That is, Person 2 wins the prize for sure, while you do receive

50ECU for sure. Alternatively, if you allocate 0 tokens to Person 2, Person 2 has no chance to win the 50ECU prize, while you get a payoff of 100ECU.

Recall, you can choose any allocation for the Person 2 between 0 and 50.

Please enter how many tokens you would like to allocate to Person 2 (TG):

Treatment 4

You have been randomly assigned to be Person 1. In this round, you will decide on the number of tokens for each of you that sum to 100.

That is, Tokens kept (TK) and Tokens given (TG) add up to 100.

$$TK + TG = 100.$$

If this round is selected for payments,

You will receive TK unique lottery tickets

Person 2 will receive TG unique lottery tickets

At the end a lottery with a prize of 100 ECU will take place where one of the unique lottery tickets wins. **Exactly one, and only one, of you will win the prize.**

Your odds of winning equal TK over 100. Correspondingly, the odds for Person 2 will equal TG over 100. That is, the more tokens you allocate to the Person 2, the higher are Person 2's chances to win 100ECU, but the smaller are your own chances to win. For example, if you allocate all 100 tokens to Person 2, person 2 has a 100 out of 100 chance to win 100ECU, that is person 2 wins the prize for sure, while you do not get any payoff.

Alternatively, if you allocate 0 tokens to Person 2, Person 2 has no chance to win the 100ECU prize, while you win for sure.

Recall, you can choose any allocation to the Person 2 between 0 and 100.

Please enter how many tokens you would like to allocate to Person 2 (TG):

Treatment 5

You have been randomly assigned to be Person 1. In this round, you will decide on the number of tokens for each of you that sum to 100.

That is, Tokens kept (TK) and Tokens given (TG) add up to 100.

$$TK + TG = 100.$$

If this round is selected for payments,

You will receive TK lottery tickets

Person 2 will receive TG lottery tickets

At the end, for you and Person 2, lotteries will be drawn with prizes of 100 ECU. Your odds of winning equal TK over 100. The odds for Person 2 will equal TG over 100. **The draws for you and Person 2 are independent. That is, both of you could win 100 points, only one of you could win, or both of you could end up without a prize.**

That is, the more tokens you allocate to the Person 2, the higher are Person 2 chances to win 100ECU, but the smaller are your own chances to win. For example, if you allocate all 100 tokens to Person 2, Person 2 has a 100 out of 100 chance to win 100ECU, that is

person 2 wins the prize for sure, while you do not get any payoff. Alternatively, if you allocate 0 tokens to Person 2, person 2 has no chance to win the 100ECU prize, while you win for sure.

Recall, you can choose any allocation to Person 2 between 0 and 100.

Please enter how many tokens you would like to allocate to Person 2 (TG):

Treatment 6

You have been randomly assigned to be Person 1. In this round, you will decide on the number of tokens for each of you that sum to 100.

That is, Tokens kept (TK) and Tokens given (TG) add up to 100.

$$TK + TG = 100.$$

If this round is selected for payments,

You will have a 50/50-chance to either receive

$$50+TK/2 \text{ ECU}$$

$$50-TK/2 \text{ ECU}$$

Person 2 will face a 50/50-chance to either receive

$$50+TG/2 \text{ ECU}$$

$$50-TG/2 \text{ ECU}$$

In the extreme, if you do not allocate any tokens to Person 2, Person 2's payoff is 50ECU while you face a 50/50 chance to win 100ECU or win nothing. If you allocate all 100tokens to Person 2, you will have 50ECU for sure while Person 2 faces the 50/50

gamble of winning 100ECU or nothing. Alternatively, if you allocate 0 tokens to Person 2, Person 2 has no chance to win the 100ECU prize, while you win for sure.

Recall, you can choose any allocation to Person 2 between 0 and 100.

Please enter how many tokens you would like to allocate to Person 2 (TG):

A.1.3. Instructions for the specific rounds – Person 2

Treatment 1

You will receive a number of tokens allocated to you by Person 1. The number of tokens allocated between you and Person 1 sums to 100. That is, Tokens kept (TK) and Tokens given (TG) add up to 100. $TK + TG = 100$.

If this round is selected for payments,

Person 1 will receive TK ECU

You will receive TG ECU as payoff

Please enter how many tokens you expect to receive from Person 1 (TG). Recall, Person 1 can allocate any number of tokens between 0 and 100.

Treatment 2

You will receive a number of tokens allocated to you by Person 1. The number of tokens allocated between you and Person 1 sums to 100. That is, Tokens kept (TK) and Tokens given (TG) add up to 100. $TK + TG = 100$.

If this round is selected for payments,

Person 1 will receive TK ECU

You will receive TG out of 100 lottery tickets, which gives you the chance to win 100 ECU. That is, you have a TG out of 100 chance of winning 100 ECU.

The more tokens Person 1 allocates to you, the higher are your chances to win 100ECU, but the smaller will be Person 1's own payoff.

For example, if Person 1 allocates 100 tokens to you, then you have a 100 out of 100 chance to win 100ECU. That is you win the prize for sure, while Person 1 does not get any payoff. Alternatively, if Person 1 allocates 0 tokens to you, then you have no chance to win the 100ECU prize, while Person 1 gets a payoff of 100ECU.

Recall, Person 1 can choose any allocation to you between 0 and 100.

Please enter in the record sheet how many tokens you expect Person 1 (TG) to allocate to you.

Treatment 3

You will receive a number of tokens allocated to you by Person 1. The number of tokens allocated between you and Person 1 sums to 100, but Person 1 can allocate to you at most 50 tokens. That is, Tokens kept (TK) and Tokens given (TG) add up to 100 ($TK + TG = 100$).

If this round is selected for payments,

Person 1 will receive TK ECU

You will receive $2 \times TG$ out of 100 lottery tickets, each of which gives you an equal chance to win 50 ECU.

That is, you will have a $2 \times TG$ out of 100 chance of winning 50 ECU.

The more tokens Person 1 allocates to you, the higher are your chances to win 50 ECU, but the smaller will be their own payoff. For example, if Person 1 allocates 50 tokens to you, you receive 100 lottery tickets and therefore would have a 100 out of 100 chance to win 50 ECU. That is, you win the prize for sure, while Person 1 receives 50 ECU for sure. Alternatively, if Person 1 allocates 0 tokens to you, you have no chance to win the 50 ECU prize, while Person 1 gets a payoff of 100 ECU.

Recall, Person 1 can choose any allocation for you between 0 and 50.

Please enter in the record sheet how many tokens you expect Person 1 to allocate to you (TG).

Treatment 4

You will receive a number of tokens allocated to you by Person 1. The number of tokens allocated between you and Person 1 sums to 100. That is, Tokens kept (TK) and Tokens given (TG) add up to 100. $TK + TG = 100$.

If this round is selected for payments,

Person 1 will receive TK unique lottery tickets

You will receive TG unique lottery tickets

At the end of the session a lottery with a prize of 100 ECU will take place where exactly one of the unique lottery tickets wins. **One, and only one, of you will win the prize.**

Your odds of winning equal TG over 100. Correspondingly, the odds for Person 1 will equal TK over 100. That is, the more tokens Person 1 allocates to you, the higher are your chances to win 100ECU, but the smaller are Person 1's own chances to win. For example, if you receive all 100 tokens from Person 1, you have has a 100 out of 100 chance to win 100ECU, that is you win the prize for sure, while Person 1 does not get any payoff. Alternatively, if you receive 0 tokens from Person 1, you have no chance to win the 100ECU prize, while Person 1 wins for sure.

Recall, Person 1 can choose any allocation between 0 and 100 for you.

Please enter in the record sheet how many tokens you expect Person 1 to allocate to you (TG).

Treatment 5

You will receive a number of tokens allocated to you by Person 1. The number of tokens allocated between you and Person 1 sums to 100. That is, Tokens kept (TK) and Tokens given (TG) add up to 100. $TK + TG = 100$.

If this round is selected for payments,

Person 1 will receive TK lottery tickets

You will receive TG lottery tickets

At the end of the session, lotteries will be drawn for you and Person 1 with prizes of 100ECU each. Your odds of winning equal TG over 100. The odds for Person 1 will equal TK over 100. **The draws for you and Person 1 are independent. That is, both of you could win 100 points, only one of you could win, or both of you could end up without a prize.**

That is, the more tokens Person 1 allocates to you, the higher are your chances to win 100ECU, but the smaller are their own chances to win. For example, if Person 1 allocates all 100 tokens to you, you have a 100 out of 100 chance to win 100ECU. That is you win the prize for sure, while Person 1 does not get any payoff. Alternatively, if you Person 1 allocates 0 tokens to you, you have no chance to win the 100ECU prize, while Person 1 wins for sure.

Recall, Person 1 can choose any allocation between 0 and 100 for you. Please enter in the record sheet how many tokens you expect Person 1 to allocate to you (TG).

Treatment 6

You will receive a number of tokens allocated to you by Person 1. The number of tokens allocated between you and Person 1 sums to 100. That is, Tokens kept (TK) and Tokens given (TG) add up to 100. $TK + TG = 100$.

If this round is selected for payments,

Person 1 will have a 50/50-chance to either receive

$$50+TK/2 \text{ ECU}$$

$$50-TK/2 \text{ ECU}$$

You will face an independent 50/50-chance to either receive

$$50+TG/2 \text{ ECU}$$

$$50-TG/2 \text{ ECU}$$

Note that the lotteries faced by you and Person 1 are independent. If you receive from the other person a non-zero number of tokens, you will face a lottery of winning either something more than 50ECU or something less than 50ECU. Concurrently, Person 1 would face a separate lottery of winning something more than 50ECU or something less than 50ECU. The outcome of your lottery does not impact the outcome of Person 1's lottery, and vice versa.

While the outcomes of the two lotteries are independent, Person 1's choice of token allocations determines the potential winnings of both players. If Person 1 allocates a non-zero number of Tokens to you, then each of you faces lotteries with the two potential outcomes of something greater than 50ECU and something less than 50ECU (potential outcomes are not necessarily the same between you).

In the extreme, if Person 1 allocates zero tokens to you ($TG=0$), then $TG/2=0$ and your payoff is 50ECU for sure. Person 1 would then face a 50/50 chance to win 100ECU or win nothing ($50+100/2=100$ and $50-100/2=0$). Alternatively, if you receive all 100 tokens, Person 1 will have exactly 50ECU for sure while you face the gamble of winning 100ECU or nothing. So while the outcome of the lotteries are not connected, the potential gains from the lotteries are determined by the allocations chosen by Person 1.

Recall, Person 1 can choose any allocation between 0 and 100 for you.

Please enter in the record sheet how many tokens you expect Person 1 to allocate to you (TG).

A.2. Social information and pride experiments, "Maryland Day" sample

A.2.1. General rules

This is an experiment in economic decision making. If you follow the instructions carefully and make good decisions you can earn a considerable amount of money. You will be paid in private and in cash at the end of the session.

It is important that you do not talk, or in any way try to communicate, with other people during the session. If you have a question, raise your hand and a monitor will come over to where you are sitting and answer your question in private.

The experiment will consist of several independent rounds. In each, you will face a specific decision task. Tasks will be explained in detail before you have to make your decision.

In each round, you will be randomly matched with one other participant. This matching will change each round. You will not know which of the other people in the room you are matched with. Likewise, the other people in the session will not know with whom they are grouped.

In each round, you will have the opportunity to earn points. At the end of this session, one of the rounds will be randomly selected as the payment round. You will be paid in cash an amount that will be determined by the number of ECUs (Experimental Currency Units) you earn during the randomly selected payment round.

At the beginning of the experiment, you will be assigned the role of either Person 1 or Person 2. Those selected for the role of Person 2 will leave the room with one of the experimenters. They will be explained the decision tasks, but then wait until person 1 has made all decisions. They will later be paid in private. That is, the identity of the decision maker (person 1) will not be revealed.

Those selected as Person 1 will remain in the room and will take a seat at one of the computers. Once all of the Person 2 players have left the room, we will explain the decision rules for each of the decision tasks to the Person 1 players..

In all rounds, each Person 1 player will decide how to allocate 100 tokens between him- or herself and Person 2.

The total number of tokens must sum up to 100.

That is, Tokens **Kept (TK)** by person 1 and Tokens **Given (TG)** to person 2 add up to 100.

$$\mathbf{TK + TG = 100.}$$

The payoff consequences of the token allocation may differ between the Person 1 and Person 2 and from round to round. Payoff consequences will be explained to all Person 1 and Person 2 players at the beginning of each round.

In each period you should record the number of tokens allocated to you and to the other person on the record sheet.

How earnings are determined

At the end of today's session, one round will be randomly selected as the payment round and payments will be determined based on the ECU earnings that round. Each round has the same probability of being chosen as the payment round. Your payments will be displayed on the computer.

Record the selected round and your profit in *ECU* for that round in the space provided at the bottom of the record sheet.

You will receive **\$1.00** in cash at the end of the session for every **10 ECU** you have earned in the payment round. This amount is recorded in the space titled earnings. In addition, you will earn a **\$5** as show-up fee.

Round 2

You have been randomly assigned to be Person 2. In this round, you will decide on the number of tokens to allocate to yourself and to Person 1.

Payments for this round are:

You will receive TK ECU

Person 1 will receive TG ECU as payoff

You can choose between two allocations.

Option 1) TK=25 & TG=25

Option 2) TK=30 & TG=15

Note that in the first option, $TK+TG=50$. In the second option, $TK+TG=45$.

One Person 1 partner has been randomly assigned to you. Their payment for this experiment depends on how many tokens you allocate to them. The experimenter will provide an information sheet to you with information about your partner's characteristics. Your partner will also receive an information sheet about your characteristics.

Please review the information sheet and enter how many tokens you would like to allocate to Person 1 (TG). Recall, you can choose any number between 0 and 100.

I choose payment option (*circle one*): **1** **2**

Round 3

You have been randomly assigned to be Person 2. In this round, you will decide on the number of tokens to allocate to yourself and to Person 1.

Payments for this round are:

You will receive TK ECU

Person 1 will receive TG ECU as payoff

You can choose between two allocations.

Option 1) TK=25 & TG=25

Option 2) TK=30 & TG=15

Note that in the first option, $TK+TG=50$. In the second option, $TK+TG=45$.

One Person 1 partner has been randomly assigned to you *based on their preferences for your characteristics*. Their payment for this experiment depends on how many tokens you allocate to them.

Please review the information sheet and enter how many tokens you would like to allocate to Person 1 (TG). Recall, you can choose any number between 0 and 100.

I choose payment option (*circle one*): **1** **2**

A.2.3. Instructions for the specific rounds – Person 1

Round 1

You have been randomly assigned to be Person 1. In this round, you will receive a number of tokens allocated to you by Person 2.

Payments for this round are:

You will receive TK ECU

Person 1 will receive TG ECU as payoff

Person 2 can choose between two allocations.

Option 1) TK=25 & TG=25

Option 2) TK=30 & TG=15

Note that in the first option, $TK+TG=50$. In the second option, $TK+TG=45$.

One Person 2 partner has been randomly assigned to you. Your payment for this experiment depends on how many tokens they allocate to you.

Please enter how many tokens you expect to receive from Person 2 (TG).

I think my partner will choose payment option (*circle one*): **1** **2**

Round 2

You have been randomly assigned to be Person 1. In this round, you will receive a number of tokens allocated to you by Person 2.

Payments for this round are:

You will receive	TK ECU
Person 1 will receive	TG ECU as payoff

Person 2 can choose between two allocations.

Option 1) TK=25 & TG=25

Option 2) TK=30 & TG=15

Note that in the first option, $TK+TG=50$. In the second option, $TK+TG=45$.

One Person 2 partner has been randomly assigned to you. Your payment for this experiment depends on how many tokens they allocate to you. The experimenter will provide an information sheet to you with information about your partner's characteristics. Your partner will also receive an information sheet about your characteristics.

Please review the information sheet and enter how many tokens you expect to receive from Person 2 (TG).

I think my partner will choose payment option (*circle one*): **1** **2**

Round 3

You have been randomly assigned to be Person 1. In this round, you will receive a number of tokens allocated to you by Person 2.

Payments for this round are:

You will receive TK ECU

Person 1 will receive TG ECU as payoff

Person 2 can choose between two allocations.

Option 1) TK=25 & TG=25

Option 2) TK=30 & TG=15

Note that in the first option, $TK+TG=50$. In the second option, $TK+TG=45$.

One Person 2 partner will be randomly assigned to you *based on your preferences for their characteristics*. Your payment for this experiment depends on how many tokens they allocate to you.

The experimenter will provide you with information on two potential partners. Please rank these potential partners in order of your preference based on their characteristics by writing “1” at the top of the sheet for your first choice and “2” at the top of the sheet of your second choice.

Please review the information sheet and enter how many tokens you expect to receive from Person 2 (TG).

Note: You have two tasks to perform this round.

I think my partner will choose payment option (*circle one*): **1** **2**

A.3. All treatments, Tanzanian clinicians

A.3.1. General rules, Person 1

A.3.1.a. English version (Experiment instructions)

This is an experiment in decision making. You will be asked to make decisions and will be given the opportunity to earn money from your choices. You will be paid in private and in cash at the end of the session. The money you earn during the experiment is in addition to the per diem you are receiving for attending the meeting today.

It is important that you do not talk with other people once the experiment has begun. If you have a question, raise your hand and someone will come over to where you are sitting and answer your question in private.

The experiment will consist of 6 rounds. In each round, you will make a choice between options that will be explained in detail before you have to make your decision.

In each round, **you will be randomly matched with one other participant.** These other participants are gathered in a separate room in this building. These participants are ordinary people from around Arusha town. The person you are matched with will change each round. You will never know who you are matched with and they will never know that they are matched with you.

In each round, your decision gives you the opportunity to earn money. **At the end of the**

session, one of the rounds will be randomly selected by drawing one of six cards from a bag **and you will be paid in cash based on the money you earned during this specific round.** In addition, all participants receive 5000TSH for participating in the experiment. Each round has the same chance of being chosen. We will record the selected round and then your earnings for that round on a receipt for you to review.

In all rounds, you are assigned the role of Person 1 and you will decide how to divide 100 tokens between yourself and Person 2.

The total number of tokens must sum up to 100.

That is, Tokens you **Keep (TK)** and Tokens you **Give (TG)** to Person 2 add up to 100.

TK + TG = 100.

For example if you give 10 to the Person 2, how many do you keep? _____

If you choose to give 90 to Person 2, how many do you keep? _____

Can you give 60 to Person 2 and keep 50 for yourself? Yes No

Can you give 40 to Person 2 and keep 40 for yourself? Yes No

The way Person 1 and Person 2 may exchange tokens into money is not always the same for Person 1 and Person 2, but will be explained to both people before each round. In each period you should **record the number of tokens you give to the other person (TG)** in the space provided on the forms we will give you for each round.

A.3.1.b. Swahili version (Maelekezo ya Jaribio)

Hili ni jaribio linalohusiana na kufanya uamuzi. Utatakiwa kufanya maamuzi na utapata fursa ya kulipwa fedha kutokana na uamuzi wako. Utalipwa fedha taslimu mwishoni mwa zoezi hili. Fedha utakazolipwa katika jaribio hili ni nyongeza kwenye ile posho utakayolipwa kwa kuhudhuria mkutano huu leo.

Zingatia kutokuongea na wengine pindi jaribio hili likishaanza. Kama una swali, nyanyua mkono wako na mara atakuja mtu hadi hapo ulipoketi na kujibu maswali yako faraghani.

Jaribio litakuwa na awamu 6. Katika kila awamu, utafanya uamuzi kati ya chaguo kadhaa utakazoelezwa kwa kina kabla ya kufanya uamuzi huo.

Katika kila awamu, **utapangwa na mshiriki mwingine mmoja kwa kubahatisha.** Hawa washiriki wengine wamekusanyika kwenye chumba tofauti katika jengo hili. Washiriki hawa ni watu wa kawaida tu kutoka hapahapa Arusha mjini. mtu utakayepangiwa atabadilishwa katika kila awamu. Hutamjua mtu auliyepangiwa na yeye pia hatajua kama amepangwa pamoja na wewe.

Katika kila awamu, uamuzi wako utakupa fursa ya kupata fedha. **Mwishoni mwa zoezi hili, awamu moja itachaguliwa kwa kubahatisha** kwa kuokota toka mfukoni kadi moja kati ya sita **na utalipwa fedha taslimu kulingana na fedha utakazokuwa umepata katika awamu hii itakayokuwa imechaguliwa.** Zaidi ya hizo, kila mshiriki atalipwa sh 5000 kwa kushiriki jaribio hili. Kila awamu ina fursa sawa ya kuchaguliwa .

Tutaweka kumbukumbu za awamu iliyochaguliwa, kisha malipo yako kwa awamu hiyo yataandikwa kwenye risiti ili uyapitie.

Katika awamu zote, umepewa uhusika kama Mtu 1 na utaamua jinsi ya kugawana kete 100 baina yako na Mtu 2.

Jumla ya kete lazima itimie 100.

Yaani, kete utakazochukua ama kubaki nazo (zako) na zile utakazomgawia mwingine (zake) lazima jumla yake itimie 100.

zako + zake = 100.

Kwa mfano, ukiamua kumpa Mtu 2 kete 10, wewe utabaki na ngapi? _____

Ukiamua kumpa Mtu 2 kete 90, wewe utabaki na ngapi? _____

Je naweza kumpa mwingine 60 nami nikabaki na 50? Yes No

Je naweza kumpa mwingine 40 nami kubaki na 40? Yes No

Utaratibu utakaotumika baina ya Mtu 1 na Mtu 2 kubadilisha kete kuwa fedha utakuwa ukibadilika kwa kila awamu, na maelezo yatatolewa kwa wote wawili kabla ya kila awamu. Mara zote utapaswa kuandika idadi ya kete In each period you should **kuandika idadi ya kete unazompa mwenzio (zake)** kwenye nafasi iliyoachwa wazi katika fomu utakazopewa kwa kila awamu.

Recall, that while you do not know the characteristics of Person 2, you know that they chose you to make the decision rather than another person.

Please write down how many tokens you would like to give to Person 2 (TG). Recall, you can choose any number between 0 and 100.

TG: _____

Round 4, Person 1

In this round, you again have been randomly matched with a person 2. You do not know anything about them. In particular, they may not be the same person who you were matched with in the previous periods. In this round, you will again decide on the number of tokens for each of you that sum to 100.

That is, Tokens you Keep (TK) and Tokens you Give to Person 2 (TG) add up to 100.

$$TK + TG = 100.$$

The payments are, however, different from the previous periods. If this round is the round selected by drawing a card from the bag:

You will receive **150 TSH for each token you keep (150xTK)**

Person 2 will receive

**1 Ticket for each token you give which may win
Person 2 a prize of 15000 TSH.**

If this round is the round selected by drawing a card from the bag, there will be a drawing with a prize of 15000 TSH to determine Person 2's earnings. We draw a ticket randomly from a bag that contains 100 numbered tickets just like the tickets you have been shown. Each ticket in the bag has a matched ticket and Person 2 will receive one matched ticket for each token you give him, **giving Person 2 tickets numbered 1 to TG**. Person 2 wins 15000 TSH if he or she owns the winning ticket, otherwise they will receive nothing. That is, if the ticket drawn from the bag shows a number less or equal to TG, they will win 15000 TSH. If the ticket drawn from the bag shows a larger number than TG, they receive nothing. The more tokens you give them, the more chance they have of picking a winning ticket.

You will receive 150 TSH for each token you keep (TK) no matter what the outcome for Person 2 is. You will not be informed if person 2 won. Person 2 will know the number of tokens allocated to her or him and whether or not they won.

Please write down how many tokens you would like to give to Person 2 (TG). Recall, you can choose any number between 0 and 100.

TG: _____

Round 5, Person 1

In this round, you will again decide on the number of tickets for each of you that sum to 100. Remember, Tokens you keep (TK) and Tokens you give to Person 2 (TG) again add up to 100.

$$TK + TG = 100.$$

If this round is the round selected by drawing a card from the bag:

You will receive 1 Ticket for each token you keep which may win you 15000 TSH

Person 2 will receive 1 Ticket for each token you give which may win Person 2 a prize of 15000 TSH.

If this round is the round selected by drawing a card from the bag, there will be a drawing with a prize of 15000 TSH to determine if you or Person 2's are the winner. Only one of you wins the prize.

If this round is the round selected for payment, we draw a ticket randomly from a bag that contains 100 numbered tickets just like the tickets you have been shown. Each ticket in the bag has a matched ticket and Person 2 will receive one matched ticket for each token you give him, giving him tickets numbered 1 to TG. You will keep the remaining tickets. The winner is the person who was assigned the ticket matching the one drawn from the bag:

Tafadhali andika hapa chini ni kete ngapi utakazopenda kumpa mwenzio. Kumbuka unaweza kuchagua namba yoyote kuanzia 0 hadi 100.

zake: _____

Awamu 2, Mtu 1

Katika awamu hii pia mtu mwingine mmoja amepangwa pamoja nawe kwa kubahatisha.

Katika awamu hii utaweza kujua machache kuhusu mwenzio huyu uliyepangiwa.

Mtafiti atakupatia karatasi yenye taarifa zinazomhusu mtu uliyepangiwa.

Utaamua jinsi utakavyogawana kete 100 na mtu uliyepangiwa. Kumbuka, jumla ya kete utakazobakia nazo pamoja na zile utakazomgawia mwenzio lazima iwe 100.

$$\text{zako} + \text{zake} = 100.$$

Ikiwa awamu hii ndiyo itakayochaguliwa kwa kuokota kadi kwenye mfuko:

**Utapata TSh 150 kwa kila kete utakayobakia nayo
(100x zako)**

Mwenzio atapata TSh 150 kwa kila kete utakayompa (150x zake)

Tafadhali pitia taarifa ulizopewa kuhusu mwenzio. Kisha andika ni kete ngapi ungependa kumpa. Kumbuka, waweza kuchagua namba yoyote kuanzia 0 hadi 100.

zake: _____

Awamu 3, Mtu 1

Katika awamu hii, mtu mwingine atapangwa kwako kwa mujibu wa jinsi alivyoyapenda maelezo yanayokuhusu wewe. Ni kwamba, Mtu 2 ameyasoma maelezo yanayokuhusu wewe na yanayomhusu mtu mwingine, na akapendelea kupangwa awe na wewe badala ya Yule mtu mwingine. Hakujui wewe ni nani, anajua tu mambo machache kutokana na ile fomu uliyojaza mwanzoni mwa zoezi hili. Kumbuka kuwa huyu si lazima akawa yuleyule uliyepangiwa katika awamu zilizotangulia.

Kama mwanzo, utaamua jinsi utakavyogawa kete 100 baina yenu. Kumbuka, kete unazobaki nazo (zako) zikijumlishwa na zile utakazomgawia (zake) lazima zifike 100.

$$\text{zako} + \text{zake} = 100.$$

Ikiwa awamu hii ndiyo itakayochaguliwa kwa kuokota kadi kwenye mfuko:

Utapata **TSh 150 kwa kila kete utakayobakia nayo**
(100x zako)

Mwenzio atapata **TSh 150 kwa kila kete utakayompa (150x zake)**

Kumbuka, ingawa hujui chochote kuhusu Mtu 2, unajua ndiye aliyekuchagua wewe ufanye uamuzi huu badala ya yule mtu mwingine.

Tafadhali andika hapa chini ni kete ngapi utakazopenda kumpa mwenzio. Kumbuka unaweza kuchagua namba yoyote kuanzia 0 hadi 100.

zake: _____

Awamu 4, Mtu 1

Katika awamu hii umepangwa tena na mtu 2. Hujui chochote kumhusu ispokuwa tu kwamba amekuchagua. Inawezekana pia siyo mtu yuleyule uliyepangiwa katika awamu zilizotangulia. Katika awamu hii utaamua tena idadi ya kete mtakazogawana baina yenu ambazo jumla yake ni 100.

Yaani, kete utakazobaki nazo pamoja na zile utakazompa mwingine jumla yake iwe 100.

$$\text{zako} + \text{zake} = 100.$$

Hata hivyo, malipo katika awamu hii ni tofauti na awamu zilizotangulia. Iwapo awamu hii ni ile itakayochaguliwa kwa kuokota kadi kwenye mfuko:

Utapata **TSH 150 kwa kila kete utakayobaki nayo (150 x zako)**

Mtu 2 atapata **Tiketi 1 kwa kila kete utakayompa, ambayo atatumia katika bahati nasibu ya kumwezesha kushinda zawadi ya TSh 15000.**

Kama hii ni awamu itakayochaguliwa kwa kuokota kadi kwenye mfuko, kutakuwa na bahati nasibu yenye zawadi ya Tsh 15,000 kwa ajili ya mtu 2. Kama awamu hii itachaguliwa kwa ajili ya malipo, tutaokota tiketi kwa kubahatisha kutoka kwenye mfuko wenye tiketi 100 zenye namba kama hizo zilizoko kwenye tiketi ulizooneshwa.

Kila tiketi kwenye mfuko huo ina tiketi inayoshabihiana nayo na Mtu 2 atapata tiketi moja kwa kila kete utakayompa, ambazo zitaanzia namba 1 hadi namba itakayoendana na idadi ya kete ulizompa. Mtu 2 atashinda sh 15,000 iwapo atabahatika kuwa na tiketi itakayoshinda, vinginevyo hatapata chochote. Yaani, iwapo tiketi itakayookotwa kwenye mfuko itakuwa na namba sawa au ndogo kuliko idadi ya kete utakazompatia, atashinda Tsh 15,000. Kama namba ya tiketi itakayookotwa kwenye mfuko ni kubwa kuliko idadi ya kete ulizompatia, hatapata chochote. Kadiri unavyompa kete nyingi ndivyo unavyomwongezea uwezekano wa kuokota tiketi itakayoshinda.

Utapata Tsh 150 kwa kila kete utakayobaki nayo bila kujali kitakachotokea kwa Mtu 2. Hutajulishwa iwapo Mtu 2 ameshinda au la. Mtu 2 atajua idadi ya kete utakazompatia na atajua pia iwapo ameshinda au la, lakini hatakujua wewe ni nani.

Tafadhali andika hapa idadi ya kete ambazo ungependa kumpa Mtu 2 (zake). Kumbuka unaweza kuchagua namba yoyote kuanzia 0 hadi 100.

zake: _____

Awamu 5, Mtu 1

Katika awamu hii, utaamua kuhusu idadi ya tiketi za kugawana baina yenu ambazo jumla yake ni 100. Kumbuka, jumla ya kete utakazobaki nazo pamoja na zile utakazompa mtu 2 lazima itimie 100.

$$\text{zako} + \text{zake} = 100.$$

Kama hii ni awamu itakayochaguliwa kwa kuokota kadi kwenye mfuko:

Utapata **Tiketi moja kwa kila kete utakayobaki nayo, ambayo inaweza kukushindia sh 15,000.**

Mtu 2 atapata **Tiketi moja kwa kila kete utakayompatia, ambayo inaweza kumshindia sh 15,000.**

Kama awamu hii itachaguliwa kwa kuokota kadi kwenye mfuko, kutakuwa na bahati nasibu yenye zawadi ya sh 15,000 ambayo ni mmoja wenu tu awezaye kushinda na siyo wote wawili.

Kama awamu hii itachaguliwa kwa ajili ya malipo, tutaokota tiketi kwa kubahatisha kutoka kwenye mfuko wenye tiketi 100 zenye namba kama hizo tiketi ulizooneshwa. Kila tiketi kwenye mfuko ina namba zinazoshabihiana na hizo ulizoon, na Mtu 2 atapata tiketi moja kwa kila kete utakayompatia, na hivyo kumwezesha kupata tiketi zenye namba kuanzia 1 hadi namba inayoendana na idadi ya kete ulizompatia. Tiketi zitakazobaki zitakuwa za kwako. Mshindi ni yule atakayekuwa na tiketi itakayoshabihiana na ile itakayookotwa kwenye mfuko.

Mtu 2 atashinda sh 15,000 iwapo tiketi itakayookotwa kwenye mfuko itaonesha namba ndogo au sawa na idadi ya kete utakazompatia. Utashinda sh 15,000 iwapo tiketi itakayookotwa kwenye mfuko itaonesha namba kubwa kuliko idadi ya kete ulizompatia

mwenzio. Yaani, kadri unavyompa Mtu 2 kete nyingi ndivyo unavyomwongezea fursa ya kushinda huku fursa yako ya kushinda ikipungua.

Tafadhali andika hapa idadi ya kete ambazo ungependa kumpa Mtu 2 (zake). Kumbuka unaweza kuchagua namba yoyote kuanzia 0 hadi 100.

zake: _____

A.3.3. General rules, Person 2

A.3.2.b. English version (Experiment instructions)

This is an experiment in decision making. You will be asked to make decisions and will be given the opportunity to earn money from your choices. You will be paid in private and in cash at the end of the session. The money you earn during the experiment is in addition to the 5000 TSH you are receiving for attending the meeting today.

It is important that you do not talk with other people once the experiment has begun. If you have a question, raise your hand and someone will come over to where you are sitting and answer your question in private.

The experiment will consist of 6 rounds. In each round, **you will be randomly matched with one other participant.** In each round, this person (Person 1) will make a choice between options that will be explained in detail to you. You are asked to state your expectation about Person 1's choice. These other participants are gathered in a separate room in this building and are attending a research conference at CEDHA. The person you are matched with will change each round. You will never know who you are matched with and they will never know that they are matched with you.

Each round gives you the opportunity to earn money. **At the end of the session, one of the rounds will be randomly selected** by drawing one of six cards from a bag **and you**

will be paid in cash based on the money you earned during this specific round. In addition, all participants receive 5000TSH for participating in the experiment. Each round has the same chance of being chosen. We will record the selected round and then your earnings for that round on a receipt for you to review.

In all rounds, you are assigned the role of Person 2. The other person (Person 1) will decide how to divide 100 tokens between you and Person 1.

The total number of tokens must sum up to 100.

That is, Tokens you **Keep (TK)** and Tokens you **Give (TG)** to Person 2 add up to 100.

TK + TG = 100.

For example if Person 1 gives 10 to you, how many does Person 1 keep? _____

If Person 1 gives 90 to you, how many does Person 1 keep? _____

Can Person 1 give 60 to you and keep 50 for him- or herself? Yes No

Can Person 1 give 40 to you and keep 40 for him- or herself? Yes No

The way Person 1 and Person 2 may exchange tokens into money is not always the same for Person 1 and Person 2, but will be explained to both before each round. In each period you should **record the number of tokens you expect the other person to give to you (TG)** in the space provided on the forms we will give you for each round.

A.3.2.b. Swahili version (Maelekezo ya Jaribio)

Hili ni jaribio linalohusiana na kufanya uamuzi. Mtu 1 aliyeko kule ukumbini atafanya maamuzi yanayoweza kukupa fursa ya kulipwa fedha kutokana na uamuzi wake. Utalipwa fedha taslimu mwishoni mwa zoezi hili. Fedha utakazolipwa mwishoni mwa zoezi hili ni nyongeza kwenye zile sh 5000 utakazopata kwa kuhudhuria mkutano huu leo.

Zingatia kutokuongea na wengine pindi jaribio hili likishaanza. Kama una swali, nyanyua mkono wako na mara atakuja mtu hadi hapo ulipoketi na kujibu maswali yako faragani.

Jaribio litakuwa na awamu 6. Katika kila awamu, utafanya uamuzi kati ya chaguo kadhaa utakazoelezwa kwa kina kabla ya kufanya uamuzi huo.

Katika kila awamu, **utapangwa na mshiriki mwingine mmoja kwa kubahatisha.** Katika kila awamu, mtu huyo atafanya maamuzi baina ya mambo mbalimbali ambayo utaelezwa kwa kina baadae. Hawa washiriki wengine wamekusanyika kwenye ukumbi hapa CEDHA ambapo wanahudhuria kongamano la utafiti. Mtu utakayepangiwa atabadilishwa katika kila awamu. Hutamjua mtu uliyepangiwa na yeye pia hatajua kama amepangwa pamoja na wewe.

Kila awamu, itakupa fursa ya kupata fedha. **Mwishoni mwa zoezi hili, awamu moja itachaguliwa kwa kubahatisha** kwa kuokota toka mfukoni kadi moja kati ya sita **na utalipwa fedha taslimu kulingana na fedha utakazokuwa umepata katika awamu hii**

itakayokuwa imechaguliwa. Zaidi ya hayo, kila mshiriki atalipwa sh 5000 kwa kushiriki jaribio hili. Kila awamu itakuwa na fursa sawa ya kuchaguliwa. Tutaweka kumbukumbu za awamu iliyochaguliwa, kisha malipo yako kwa awamu hiyo yataandikwa kwenye risiti ili uyapitie.

Katika awamu zote, umepewa uhusika kama Mtu 2. Yule mtu mwingine uliyepangwa naye aliyeko ukumbini (Mtu 1) ataamua jinsi ya kugawana kete 100 baina yenu wawili.

Jumla ya kete lazima itimie 100.

Yaani, kete Mtu 1 atakazokugawia (zako) na zile atakazobaki nazo (zake) lazima jumla yake itimie 100.

zako + zake = 100.

Kwa mfano, Mtu 1 akikupa 2 kete 10, yeye atabaki na ngapi? _____

Mtu 1 akiamua kukupa kete 90, yeye atabaki na ngapi? _____

Je Mtu 1 anaweza kukupa 60 naye akabaki na 50? Ndiyo Hapana

Je Mtu 1 anaweza kukupa 40 naye kubaki na 40? Ndiyo Hapana

Utaratibu utakaotumika baina ya Mtu 1 na Mtu 2 kubadilisha kete kuwa fedha utakuwa ukibadilika kwa kila awamu, na maelezo yatatolewa kwa wote wawili kabla ya kila awamu. Mara zote utapaswa **kuandika idadi ya kete unazotarajia kupewa (zako)** kwenye nafasi iliyoachwa wazi katika fomuu utakazopewa kwa kila awamu.

A.3.4. Instructions for specific rounds, Person 2

A.3.4.a. English version

Round 1, Person 2

In this round, one person has been randomly assigned to you. You will receive a number of tokens allocated to you by Person 1. The number of tokens allocated between you and Person 1 sums to 100.

Remember, Tokens Person 1 keeps (TK) and Tokens given to you (TG) add up to 100.

$$TK + TG = 100.$$

If this round is the round selected by drawing a card from the bag:

Person 1 will receive **150 TSH for each token Person 1 keeps**
(150xTK)

You will receive **150 TSH for each token Person 1 gives to you**
(150xTG)

Please write down how many tokens you expect to receive from Person 1 (TG). Recall, Person 1 can allocate any number of tokens to you between 0 and 100.

TG: _____

Round 2, Person 2

In this round, again, one other person has been randomly assigned to you. In this round, Person 1 knows a few things about you. **The experimenter will provide an information sheet to Person 1 with the information about your characteristics that you provided before.**

You will receive a number of tokens allocated to you by Person 1. The number of tokens allocated between you and Person 1 sums to 100.

Remember, Tokens Person 1 keeps (TK) and Tokens given to you (TG) add up to 100.

$$TK + TG = 100.$$

If this round is the round selected by drawing a card from the bag:

**Person 1 will receive 150 TSH for each token Person 1 keeps
(150xTK)**

**You will receive 150 TSH for each token Person 1 gives to you
(150xTG)**

Please write down how many tokens you expect to receive from Person 1 (TG). Recall, Person 1 can allocate any number of tokens to you between 0 and 100 and that Person 1 has received some information about your characteristics.

TG: _____

Round 3, Person 2

In this round, the experimenter will provide you information sheets about characteristics of two different person 1.

Please carefully review these information sheets. Please choose who you prefer to be matched with by writing a “1” (for first choice) or a “2” (for second choice) on the information sheets.

Person 1 will know that you preferred to be matched with him or her, but will not know your characteristics. Note that this Person 1 is not necessarily the same person with whom you were matched with in the previous period. You will receive a number of tokens allocated to you by Person 1. The number of tokens allocated between you and Person 1 sums to 100.

Remember, Tokens Person 1 keeps (TK) and Tokens given to you (TG) add up to 100.

$$TK + TG = 100.$$

If this round is the round selected by drawing a card from the bag:

Person 1 will receive **150 TSH for each token Person 1 keeps**
(150xTK)

You will receive **150 TSH for each token Person 1 gives to you**
(150xTG)

Recall, that while Person 1 does not know your characteristics, Person 1 will know that you preferred him or her to make the decision rather than another person.

Please write down how many tokens you expect to receive from Person 1 (TG). Recall, Person 1 can allocate any number of tokens to you between 0 and 100

TG: _____

Round 4, Person 2

In this round, again, one other person has been randomly assigned to you. They do not know anything about you. In particular, they may not be the same person who you were matched with in the previous periods.

You will receive a number of tokens allocated to you by Person 1. The number of tokens allocated between you and Person 1 sums to 100.

Remember, Tokens Person 1 keeps (TK) and Tokens given to you (TG) add up to 100.

$$TK + TG = 100.$$

The payments are, however, different from the previous periods. If this round is the round selected by drawing a card from the bag:

Person 1 will receive	150 TSH for each token Person 1 keeps (150xTK)
You will receive	1 Ticket for each token Person 1 gives to you which may win you a prize of 15000 TSH.

If this round is the round selected by drawing a card from the bag, there will be a drawing with a prize of 15000 TSH to determine your earnings. We draw a ticket randomly from a bag that contains 100 numbered tickets just like the tickets you have been shown. Each ticket in the bag has a matched ticket and you will receive one matched ticket for each token you give him, **giving you tickets numbered 1 to TG**. Person 2 wins 15000 TSH if he or she owns the winning ticket, otherwise they will receive nothing. That is, if the ticket drawn from the bag shows a number less or equal to TG, you will win 15000 TSH. If the ticket drawn from the bag shows a larger number than TG, you receive nothing. The more tokens Person 1 gives to you, the more chance you have of picking a winning ticket.

Person 1 will receive 150 TSH for each token Person 1 keeps (TK) no matter what the outcome for Person 2 is. They will not be informed if person 2 (you) won. Person 2 will know the number of tokens allocated to her or him and whether or not they won.

Please write down how many tokens you expect to receive from Person 1 (TG). Recall, Person 1 can allocate any number of tokens to you between 0 and 100

TG: _____

Round 5, Person 2

In this round, again, one other person has been randomly assigned to you. You will receive a number of tokens allocated to you by Person 1. The number of tokens allocated between you and Person 1 sums to 100.

Remember, Tokens Person 1 keeps (TK) and Tokens given to you (TG) add up to 100.

$$TK + TG = 100.$$

If this round is the round selected by drawing a card from the bag:

Person 1 will receive **1 Ticket for each token Person 1 keeps which may win Person 1 a prize of 15000 TSH**

You will receive **1 Ticket for each token Person 1 gives to you which may win you a prize of 15000 TSH.**

If this round is the round selected by drawing a card from the bag, there will be a drawing with a prize of 15000 TSH to determine if you or Person 1's are the winner. Only one of you wins the prize.

If this round is the round selected for payment, we draw a ticket randomly from a bag that contains 100 numbered tickets just like the tickets you have been shown. Each ticket in the bag has a matched ticket and you will receive one matched ticket for each token Person 1 gives to you, giving you tickets numbered 1 to TG. Person 1 will keep the remaining tickets. The winner is the person who was assigned the ticket matching the one drawn from the bag:

You win 15000 TSH if the ticket drawn from the bag shows a number less or equal to TG. Person 1 wins 15000 TSH if the ticket drawn from the bag shows a number larger than TG. That is, the more tokens Person 1 gives to you, the more chance you have of picking a winning ticket, while Person 1 chances decrease.

Please write down how many tickets you expect to receive from Person 1 (TG). Recall, Person 1 can allocate any number of tokens to you between 0 and 100.

TG: _____

A.3.4.b. Swahili version

Awamu 1, Mtu 2

Katika awamu hii umepangiwa mtu mmoja kwa kubahatisha. Utapata idadi ya kete ambazo Mtu 1 atakugawia. Jumla ya kete zote mnazogawana ni 100. Kumbuka, kete Mtu 1 atakazobaki nazo (zake) zikijumlishwa na zile atakazokugawia (zako) ni lazima jumla yake iwe 100.

$$\text{zako} + \text{zake} = 100.$$

Ikiwa hii ndiyo awamu itakayochaguliwa kwa kuokota kadi kwenye mfuko:

Mtu 1 atapata TSh 150 kwa kila kete atakayobakia nayo (100x zake)

Utapata TSh 150 kwa kila kete atakayokupa (150x zako)

Tafadhali andika hapa chini ni kete ngapi unazotarajia Mtu 1 atakupa. Kumbuka unaweza kuchagua namba yoyote kuanzia 0 hadi 100.

zako: _____

Awamu 2, Mtu 2

Katika awamu hii pia mtu mwingine mmoja amepangwa pamoja nawe kwa kubahatisha.

Katika awamu hii Mtu 1 anajua mambo machache kuhusu wewe. **Mtafiti atampatia Mtu 1 karatasi yenye taarifa zinazokuhusu ambazo umetupatia.**

Utapata idadi ya kete ambazo Mtu 1 atakugawia. Jumla ya kete zote mnazogawana ni 100. Kumbuka, kete Mtu 1 atakazobaki nazo (zake) zikijumlishwa na zile atakazokugawia (zako) ni lazima jumla yake iwe 100.

$$\text{zako} + \text{zake} = 100.$$

Ikiwa hii ndiyo awamu itakayochaguliwa kwa kuokota kadi kwenye mfuko:

Mtu 1 atapata TSh 150 kwa kila kete atakayobakia nayo (100x zake)

Utapata TSh 150 kwa kila kete atakayokupa (150x zako)

Tafadhali andika hapa chini ni kete ngapi unazotarajia Mtu 1 atakupa. Kumbuka Mtu 1 anaweza kukupa idadi yoyote ya kete kuanzia 0 hadi 100, na kwamba Mtu 1 amepokea taarifa ulizotoa zinazokuhusu.

zako: _____

Awamu 3, Mtu 2

Katika awamu hii, mtafiti atakupatia karatasi zenye taarifa kuhusu watu wawili tofauti. Tafadhali zipitie taarifa hizi kwa makini. Tafadhali chagua kati ya hawa wawili ni yupi ungependa kupangwa naye kwa kuandika “1” (kwa chaguo la kwanza) au “2” (kwa chaguo la pili) kwenye karatasi hizo zenye taarifa.

Mtu 1 atajua kwamba umemchagua lakini hatajua chochote kuhusu wewe. Kumbuka kuwa huyu si lazima akawa yuleyule uliyepangiwa katika awamu zilizotangulia.

Utapata idadi ya kete ambazo Mtu 1 atakugawia. Jumla ya kete zote mnazogawana ni 100. Kumbuka, kete Mtu 1 atakazobaki nazo (zake) zikijumlishwa na zile atakazokugawia (zako) ni lazima jumla yake iwe 100.

$$\text{zako} + \text{zake} = 100.$$

Ikiwa hii ndiyo awamu itakayochaguliwa kwa kuokota kadi kwenye mfuko:

Mtu 1 atapata TSh 150 kwa kila kete atakayobakia nayo (100x zake)

Utapata TSh 150 kwa kila kete atakayokupa (150x zako)

Kumbuka kwamba Mtu 1 hana taarifa zako lakini anafahamu kuwa wewe umemchagua afanye uamuzi huu. Tafadhali andika hapa chini ni kete ngapi unazotarajia Mtu 1 atakupata. Kumbuka unaweza kuchagua namba yoyote kuanzia 0 hadi 100.

zako: _____

Awamu 4, Mtu 2

Katika awamu hii umepangwa tena na Mtu 1. Hajui chochote kinachokuhusu. Inawezekana pia siyo mtu yuleyule uliyepangiwa katika awamu zilizotangulia. Utapata idadi ya kete ambazo Mtu 1 atakugawia. Jumla ya kete mtakazogawana baina yenu ni 100.

Yaani, kete atakazobaki nazo pamoja na zile atakazokugawia jumla yake ni 100.

$$\text{zako} + \text{zake} = 100.$$

Hata hivyo, malipo katika awamu hii ni tofauti na awamu zilizotangulia. Iwapo awamu hii ndiyo itakayochaguliwa kwa kuokota kadi kwenye mfuko:

Mtu 1 atapata	TSH 150 kwa kila kete utakayobaki nayo (150 x zako)
Utapata	Tiketi 1 kwa kila kete utakayompa, ambayo atatumia katika bahati nasibu ya kumwezesha kushinda zawadi ya TSh 15000.

Kama hii ndiyo awamu itakayochaguliwa kwa kuokota kadi kwenye mfuko, kutakuwa na bahati nasibu yenye zawadi ya Tsh 15,000 kwa ajili ya kubainisha mapato yako. Kama awamu hii itachaguliwa kwa ajili ya malipo, tutaokota tiketi kwa kubahatisha kutoka kwenye mfuko wenye tiketi 100 zenye namba kama hizo zilizoko kwenye tiketi ulizooneshwa. Kila tiketi kwenye mfuko huo ina tiketi inayoshabihiana nayo na utapata tiketi moja kwa kila kete Mtu 1 atakayokupa, ambazo zitaanzia namba 1 hadi namba

itakayoendana na idadi ya kete ulipewa. Utashinda sh 15,000 iwapo utabahatika kuwa na tiketi itakayoshinda, vinginevyo hutapata chochote. Yaani, iwapo tiketi itakayookotwa kwenye mfuko itakuwa na namba sawa au ndogo kuliko idadi ya kete utakazopewa, utashinda Tsh 15,000. Kama namba ya tiketi itakayookotwa kwenye mfuko ni kubwa kuliko idadi ya kete ulizopewa, hutapata chochote. Kadiri utakavyopewa kete nyingi ndivyo unavyoongezewa uwezekano wa kuokota tiketi itakayoshinda.

Mtu 1 atapata Tsh 150 kwa kila kete atakayobaki nayo bila kujali kitakachotokea kwako. Mtu 1 hatajulishwa iwapo umeshinda au la.

Tafadhali andika hapa idadi ya kete ambazo unatarajia Mtu 1 atakupatia. Kumbuka Mtu 1 anaweza kukugawia idadi yoyote ya kete kuanzia 0 hadi 100.

zako: _____

Awamu 5, Mtu 2

Katika awamu hii, umepangiwa tena Mtu 1 kwa kubahatisha. Utapata idadi ya kete ambazo Mtu 1 atakugawia. Jumla ya idadi ya kete mnazogawana ni 100.

Kumbuka, jumla ya kete atakazobaki nazo pamoja na zile atakazokugawia lazima itimie 100.

$$\text{zako} + \text{zake} = 100.$$

Kama hii ni awamu itakayochaguliwa kwa kuokota kadi kwenye mfuko:

Mtu 1 atapata **Tiketi moja kwa kila kete atakayobaki nayo, ambayo inaweza kumshindia sh 15,000.**

Utapata **Tiketi moja kwa kila kete Mtu 1 atakayokupatia, ambayo inaweza kukushindia sh 15,000.**

Kama awamu hii itachaguliwa kwa kuokota kadi kwenye mfuko, kutakuwa na bahati nasibu yenye zawadi ya sh 15,000 ambayo ni mmoja wenu tu awezaye kushinda na siyo wote wawili.

Kama awamu hii itachaguliwa kwa ajili ya malipo, tutaokota tiketi kwa kubahatisha kutoka kwenye mfuko wenye tiketi 100 zenye namba kama hizo tiketi ulizooneshwa. Kila tiketi kwenye mfuko ina namba zinazoshabihiana na hizo ulizoon, na utapata tiketi moja kwa kila kete utakayopewa, na hivyo kukuwezesha kupata tiketi zenye namba kuanzia 1 hadi namba inayoendana na idadi ya kete ulizopewa. Tiketi zitakazobaki zitakuwa za Mtu 1. Mshindi ni yule atakayekuwa na tiketi itakayoshabihiana na ile itakayookotwa kwenye mfuko.

Utashinda sh 15,000 iwapo tiketi itakayookotwa kwenye mfuko itaonesha namba ndogo au sawa na idadi ya kete utakazopewa. Mtu 1 atashinda sh 15,000 iwapo tiketi itakayookotwa kwenye mfuko itaonesha namba kubwa kuliko idadi ya kete

alizokupatia. Yaani, kadri unavyopewa kete nyingi ndivyo zivyokuongezea fursa ya kushinda huku fursa Mtu 1 ya kushinda ikipungua.

Tafadhali andika hapa idadi ya kete ambazo unatarajia Mtu 1 atakupatia. Kumbuka Mtu 1 anaweza kukugawia idadi yoyote ya kete kuanzia 0 hadi 100.

zako: _____

Appendix B: Survey Instruments

B.1. Retrospective Consultation Review

OPD Technical Quality Evaluation **PATIENT'S HOUSEHOLD RESOURCES** Retrospective Consultation Review Page 5

"Now I am going to ask you some questions that are not related to your doctor visit. Please answer the following questions the best you can."

Housing and Work

10 Are you currently employed? YES NO

11 If yes, what job do you do?

12 How do you get to and from work each day?
 [Circle One:] walk, drive car, bike, dala-dala, taxi, other (specify)

13 How long does it take you to get to work (in minutes)?

14 What type of house do you live in?
 [Circle One:] free-standing house, apartment, other (specify)

15 If it is a free-standing house, what is the wall material in the building where you live?
 [Circle One:] mud brick, burned brick, cement blocks, other (specify)

16 How many people live in your house, apartment, other?
 [Circle One:] own, rent, job provided, relative owns

17 Do you own or rent your house/apartment/room?
 [Circle One:] own, rent, job provided, relative owns

18 Where do you get your water from?
 Inside the house/compound/apartment YES NO
 Outside the house/compound/apartment YES NO

19 Is it running water?
 If it is outside the house/compound/apartment:
 How far is your water source (in minutes)?

20 Do you have electricity? YES NO

Human Capital

21 Are you attending or have you attended school?
 (If the answer is no, skip to item 57) YES NO

22 What is your highest level of education attained?
 [Circle One:] none, primary, secondary, tertiary, post-tertiary

23 What was the first year you attended school?
 [Circle One:] none, primary, secondary, tertiary, post-tertiary

24 What was the last year you attended school?
 [Circle One:] none, primary, secondary, tertiary, post-tertiary

25 Have you completed any vocational training or professional training?
 Explain

26 Can you write a letter? YES NO

27 Can you do written calculations? YES NO

Durable Goods

28 Do you own any of the following items?

Item	#	year purchased
1 radio		
2 mobile phone		
3 book		
4 camera		
6 refrigerator		
7 bicycle		
8 motorcycle		
9 car		
10 generator		

Animals

29 Do you own any animals?
 30 Do you buy or sell this kind of animal regularly?
 31 If you do not own animals, why not?

Animal	#	buy or sell regularly?
1 cow		
2 goat/sheep		
3 donkey		
4 pigs		
5 chickens		
6 ducks		
99) other (specify)		

Tukitika kuongea na wewe siku nyingine je, inawezakana? YES NO

OPD Technical Quality Evaluation **PATIENT'S HOUSEHOLD RESOURCES** Retrospective Consultation Review Page 1

What was the doctor's name?

Facility Name Date

Facility Number Patient Number

Patient Age Respondent Age

Patient Gender Respondent Gender

How much time since your consultation finished? Just now Time lapsed

Is this the first place you have visited for this illness? YES NO

Why did you choose this facility, today not another? (circle all reasons)
 What is the most important reason (put a star next to all reasons)

00 no choice	12	server is excellent	22	far to all patients
01 good personal experience	13	little waiting time	23	employer arrangement
02 good reputation	04	comfortable facilities	24	level of facility
03 recommended to me	15	qualifications of staff	25	owner of facility
04 friend works there	16	severity of illness	26	location of market
05 relative works there	17	non-severity of illness	27	location of relative
06 inexpensive	18	same religion	28	random choice
07 close	19	drugs are available	29	given referral
08 cleanliness	20	inexpensive drugs	30	accustomed to go there
09 friendly staff	10	elder told me to	31	had to travel there
10 someone who is not an elder and not an employer made me come here				

If you received a referral to this facility:

Is that why you are here today? YES NO

Did you know what you are to do for this visit? YES NO

Did you get a referral to a specific doctor? YES NO

Will you see that doctor today? YES NO

If not, why not?

Place of residence

Method of travel

Approximate cost of travel

Fees Paid today (including drugs, etc)

Fees Paid before today, for this illness

How long have you been suffering before you came?

Were you bedridden at all during that time? YES NO (specify days, weeks, etc)

(If the patient was bedridden) for how many days?

Have you visited here before today (for any illness)? YES NO

Is this the first time you have been here for this illness? YES NO

first time here I have been here before

(If they are returning, ask) Why are you returning?
 Still sick return with results more medicine
 Fill whole survey Fill opinion, Correcting and from 5.1 Fill opinion and Correcting only

Greeting and Receiving

- 1.1 Did the doctor welcome and greet you?
- 1.3 Did the doctor listen to your description of the illness?
- 1.4 Did you have a chair to sit in?

HISTORY TAKING

- 2.03 Did the doctor ask you how long you had been suffering?
- 2.05 Did the doctor ask you if there were other symptoms different from the main complaint?
- 2.07 Did the doctor ask if you already received treatment elsewhere or took medicine?

Fever

Check if fever is a primary or significant symptom

- 3.01 Did the doctor ask you how long you had had a fever?
- 3.02 Did the doctor ask you if you had chills or sweats?
- 3.03 Did the doctor ask you if you had a cough or difficulty breathing?
- 3.04 Did the doctor ask you if you had diarrhea or vomiting?
- 3.05 Did the doctor ask if you had a runny nose? NA

If child is under 5:

Check if the child is under 5 years old

- 3.11 Did the doctor ask the child had convulsions? NA
- 3.12 Did the doctor ask about difficulty drinking or breastfeeding?
- 3.13 Listen to the child's breathing, or use a stethoscope?
- 3.14 Did the doctor check the child's ear?
- 3.15 Did the doctor ask questions about the child's vaccinations?

Cough

Check if cough is a primary or significant symptom

- 3.21 Did the doctor ask the duration of the cough?
- 3.22 Did the doctor ask if there was sputum?
- 3.23 Did the doctor ask if you had blood in your cough?
- 3.24 Did the doctor ask if you had difficulty breathing?
- 3.25 Did the doctor ask if you also have a fever? NA

If child is under 5:

Check if the child is under 5 years old

- 3.31 Did the doctor ask about the history of vaccinations?
- 3.32 Did the doctor ask about difficulty drinking or breastfeeding?
- 3.33 Did the doctor ask if the child had convulsions?
- 3.34 Did the doctor check the child's ear?
- 3.35 Did the doctor ask if the child had diarrhea or vomiting?
- 3.36 Did the doctor ask questions about the child's vaccinations?

Diarrhea

Check if diarrhea is a primary or significant symptom

- 3.41 Did the doctor ask how long you have had diarrhea?
- 3.42 Did the doctor ask how often you have a movement?
- 3.43 Did the doctor ask about the way the stool looks?
- 3.44 Did the doctor ask if there was blood in the stool?
- 3.45 Did the doctor ask if you are vomiting?
- 3.46 Did the doctor ask if you also have a fever? NA

If child is under 5:

Check if the child is under 5 years old

- 3.51 Did the doctor ask about difficulty drinking or breastfeeding?
- 3.52 Did the doctor ask if the child had convulsions?
- 3.53 Did the doctor check the child's ear?
- 3.54 Did the doctor ask if the child had diarrhea or vomiting?
- 3.55 Did the doctor ask questions about the child's vaccinations?

General

- 3.61 Did the doctor ask you many questions or just a few? MANY FEW

→ GO TO PHYSICAL EXAMINATION, PAGE 2

What are your problems? (If you are writing a complaint for "Other" (34 or 35), record the SYMPTOM, not the diagnosis)

HEAD	LIMB	GENERAL
01 headache	15 general injury	25 hump
02 fever	16 broken bone/ fracture	26 chest pain
03 running nose	17 deep cut	27 coughing up blood
04 cough	18 pain in extremities	28 short of breath
05 sore throat	19 swelling in extremities	29 back pain
06 eye problems	20 accident/ trauma	30 general weakness/ malaise
07 ear problems	21 head (eye) injury	31 weight loss
STOMACH	CHILDREN	32 high blood pressure
08 loss of appetite	21 slow growth	33 skin rash
09 vomiting	22 convulsions	34 Other _____
10 diarrhoea	23 epilepsy	35 Other _____
11 constipation	24 worms (seen)	
12 abdominal pain		
13 distended abdomen		
14 stomach ache		

PHYSICAL EXAMINATION

→ GO TO HISTORY TAKING, PAGE 3 ←

Fever

4.02 Did the doctor take your temperature?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.03 Did the doctor check for neck stiffness?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.04 Did he ask if you felt weakness from lack of blood?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.05 Did he look in your ears or throat?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.06 Did he check your stomach?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.07 Did he ask for a blood slide?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

If child is under 5:

4.11 Did the doctor check if the child was sleepy, try to wake up the child?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.12 Did the doctor pinch the skin fold of the child?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.13 Did the doctor check both of the child's feet?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.14 Did the doctor check the child's weight against a chart?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

Cough

4.21 Did he look at your throat?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.22 Did he listen to your chest?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.23 Did he take your temperature?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

If child is under 5:

4.31 Did the doctor check if the child was sleepy, try to wake up the child?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.32 Did the doctor pinch the skin fold of the child?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.33 Did the doctor check the child's eyes, tongue, and palms?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.34 Did the doctor check both of the child's feet?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.35 Did the doctor check the child's weight against a chart?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

Diarrhoea

4.41 Did he pinch the skin on the stomach?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.42 Did he take your temperature?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.43 If the child is under two years: Did he look at the child's head?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

If child is under 5:

4.51 Did the doctor offer the child a drink of water or observe breastfeeding?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.52 Did the doctor check the child's eyes, tongue, and palms?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.53 Did the doctor check both of the child's feet?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4.54 Did the doctor check the child's weight against a chart?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

General

4.61 Did the doctor examine you?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
----------------------------------	--------------------------	--------------------------	--------------------------	--------------------------

Health education

5.1 Did he give you a name for your illness?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
5.2 Did he explain your illness?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
5.3 Did he explain the treatment?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
5.4 Did he give you advice to improve your health?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
5.5a Did he explain if you need to return?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
5.5b Were drugs prescribed?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
5.6 Did the doctor explain what the drugs are for?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
5.7 Did the doctor clearly explain instructions for the drugs?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
5.7b Did the doctor order a lab test?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
5.8 If so, did the doctor explain why you would have this test?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

Referral

6.0 Did he refer you to another facility?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
6.1 Did he explain why you were referred?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
6.2 Did he tell you what to do?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Were you satisfied with your visit to this provider?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Was the experience I better, 2 worse or 3 the same as you expected?	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

Do you plan to seek more treatment for this illness?

YES NO DON'T KNOW

Please assess the facility along the following dimensions:

	Very good	Good	Mediocre	Poor	Very poor
Skill of the clinician at diagnosing illnesses	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Skill of nursing care	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
The manner of your treatment, was it polite	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Availability of medicine	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Value of services rendered	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Mention the type of care that was given by the nurse (services)	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

Are there any other facilities that you frequently visit? List all mentioned and level (disp, HC, hosp)

<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
--------------------------	--------------------------	--------------------------	--------------------------	--------------------------

I am going to ask you some questions about your ability to perform simple tasks.

I want to know if you can do these things now, and if you could do these things before you became ill.

[Circle all that apply. B stands for "before illness" and N stands for "now"]

<i>For all patients under 5 ask:</i>				
Eat with good appetite	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<i>For all patients 11 and under:</i>				
Phys appropriately to age	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<i>For all female patients ask:</i>				
Cook food for family	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
<i>For all adult patients ask:</i>				
Dress themselves	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Barber themselves	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Walk for 5 minutes	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Work on the farm	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Walk for 20 minutes	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Eat with good appetite	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Lift or carry a bucket of water	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Walk for 30 minutes	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
Perform all tasks required of you for work	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

B.2. Clinician Consent Visit Survey

OPD Technical Quality Evaluation			
Consultation observation, Pre-survey			
1 Facility Ownership <input style="width: 100%;" type="text"/>	3 Enumerator <input style="width: 100%;" type="text"/>	DATE <input style="width: 100%;" type="text"/>	
2 Name of Facility <input style="width: 100%;" type="text"/>	4 Clinician's Name <input style="width: 100%;" type="text"/>	Clinician number <input style="width: 50%;" type="text"/> (leave blank)	
5 Cadre of Clinician			
MO <input style="width: 30%;" type="text"/>	AMO <input style="width: 30%;" type="text"/>	CO <input style="width: 30%;" type="text"/>	ACO <input style="width: 30%;" type="text"/>
			OTHER <input style="width: 30%;" type="text"/> Specify <input style="width: 100%;" type="text"/>
6 Years of Experience as a health worker <input style="width: 100%;" type="text"/>			
7 Date on which doctor started working at this facility <input style="width: 100%;" type="text"/> (DD-MM-YY)			
8 How long the doctor has been working as an <input style="width: 100%;" type="text"/> (months or years)			
9 How many days a week do you work in this facility? <input style="width: 100%;" type="text"/> (record hours if more appropriate)			
9a Which days of the week do you work? <input style="width: 100%;" type="text"/>			
9b When do you arrive each day and when do you leave each day?			
M: <input style="width: 100%;" type="text"/>			
T: <input style="width: 100%;" type="text"/>			
W: <input style="width: 100%;" type="text"/>			
Th: <input style="width: 100%;" type="text"/>			
F: <input style="width: 100%;" type="text"/>			
Sa: <input style="width: 100%;" type="text"/>			
Sun: <input style="width: 100%;" type="text"/>			
9c Which days and times are you in theatre? <input style="width: 100%;" type="text"/>			
9d Do you offer any specialty clinics? Which days and times are these clinics? <input style="width: 100%;" type="text"/>			
9e When shall you be on likizo? Starting day: <input style="width: 100%;" type="text"/> Ending day: <input style="width: 100%;" type="text"/>			
10 Do you work at other facilities? YES NO			
11 If so, which are they?			
	<u>Facility Name</u>	<u>Hours there per week</u>	
a	<input style="width: 100%;" type="text"/>	<input style="width: 100%;" type="text"/>	
b	<input style="width: 100%;" type="text"/>	<input style="width: 100%;" type="text"/>	
c	<input style="width: 100%;" type="text"/>	<input style="width: 100%;" type="text"/>	
13 What is your place of residence (neighborhood)? <input style="width: 100%;" type="text"/>			
14 How would you describe this facility to someone who knows nothing about it? <i>(If the clinician needs prompting for this question, ask for adjectives that come to mind when they think of the facility)</i>			
15 In general, do patients get to choose the physician they go to? <input style="width: 50%;" type="text"/> YES <input style="width: 50%;" type="text"/> NO			
16 Why would a patient choose this facility and not another? [circle]			
17 Which do you think is the most important reason? [star]			
00	no choice	08	severity of illness
01	good personal experience	09	non-severity of illness
02	good reputation	10	drugs are available
03	friend works there	11	inexpensive drugs
04	relative works there	12	friendly staff
05	inexpensive	13	cleanliness
06	proximity to their house	14	little waiting time
07	qualifications of staff	15	comfortable facilities
16	the service they want is available	17	same religion
18	fair to all patients	19	elders' recommendation
20	location of market	21	location of relative
22	good quality	23	other (specify)

Appendix C: Item Response Theory Parameter Estimates

Item type	Item	Difficulty score	Difficulty, standard error	Discrimination score	Discrimination, standard error
Greeting and Receiving	Did the doctor welcome and greet you?	0.079	(0.176)	6.166	(1.668)
	Did the doctor listen to your description of the illness?	-1.576	(0.218)	1.667	(1.979)
	Did you have a chair to sit in?	-0.876	(0.190)	2.418	(1.739)
Education	Did the doctor ask you how long you had been suffering	0.393	(0.159)	9.313	(1.549)
	Did the doctor ask you if there were other symptoms different from the main complaint?	0.516	(0.147)	10.098	(1.444)
	Did the doctor ask if you already received treatment elsewhere or took medicine?	0.567	(0.159)	11.453	(1.562)
	Did he give you a name for your illness?	0.550	(0.111)	7.968	(1.069)
	Did he explain your illness?	0.544	(0.135)	10.567	(1.319)
	Did he explain the treatment?	0.435	(0.174)	11.913	(1.717)
	Did he give you advice to improve your health?	0.582	(0.169)	12.741	(1.668)
	Did he explain if you need to return?	0.565	(0.141)	10.213	(1.389)
	Did the doctor explain what the drugs are for?	0.437	(0.237)	15.507	(2.353)
	Did the doctor <i>clearly</i> explain instructions for the drugs?	0.476	(0.226)	15.550	(2.244)
Fever, history taking	If so, did the doctor explain why you would have this test?	0.407	(0.284)	12.947	(2.771)
	Did the doctor order a lab test?	0.637	(0.063)	2.420	(0.563)
	Did he explain why you were referred?	0.491	(0.950)	17.663	(9.448)
	Did he tell you what to do?	0.492	(0.559)	9.218	(5.333)
	Did the doctor ask you how long you had had a fever?	0.560	(0.196)	9.304	(1.885)
	Did the doctor ask you if you had chills or sweats?	0.687	(0.160)	8.624	(1.507)
	Did the doctor ask you if you had a cough or difficulty breathing?	0.649	(0.139)	6.725	(1.310)
	Did the doctor ask you if you had diarrhea or vomiting?	0.623	(0.174)	8.943	(1.667)
	Did the doctor ask if you had a runny nose?	0.618	(0.180)	8.971	(1.716)

Item type	Item	Difficulty score	Difficulty, standard error	Discrimination score	Discrimination, standard error
Fever, history taking, under 5	Did the doctor ask the child had convulsions?	1.238	(0.177)	3.449	(1.595)
	Did the doctor ask about difficulty drinking or breastfeeding?	0.837	(0.181)	5.044	(1.666)
	Listen to the child's breathing, or use a stethoscope?	0.637	(0.225)	8.606	(2.107)
	Did the doctor check the child's ear?	0.767	(0.197)	8.611	(1.793)
	Did the doctor ask questions about the child's vaccinations?	0.750	(0.237)	10.449	(2.214)
Cough, history taking	Did the doctor ask the duration of the cough?	0.459	(0.231)	7.224	(2.201)
	Did the doctor ask if there was sputum?	0.659	(0.174)	8.740	(1.652)
	Did the doctor ask if you had blood in your cough?	0.754	(0.175)	8.925	(1.628)
	Did the doctor ask if you had difficulty breathing?	0.610	(0.175)	8.411	(1.657)
	Did the doctor ask if you also have a fever?	0.516	(0.195)	6.876	(1.846)
Cough, history taking, under 5	Did the doctor ask about the history of vaccinations?	0.757	(0.353)	13.150	(3.214)
	Did the doctor ask about difficulty drinking or breastfeeding?	0.779	(0.243)	8.221	(2.239)
	Did the doctor ask if the child had convulsions?	1.058	(0.235)	5.007	(2.101)
	Did the doctor check the child's ear?	0.754	(0.284)	10.851	(2.618)
	Did the doctor ask if the child had diarrhea or vomiting?	0.734	(0.214)	6.902	(1.992)

Item type	Item	Difficulty, score	Difficulty, standard error	Discrimination score	Discrimination, standard error
Diarrhea, history taking	Did the doctor ask how long you have had diarrhea?	0.529	(0.329)	5.325	(3.084)
	Did the doctor ask how often you have a movement?	0.631	(0.383)	8.730	(3.619)
	Did the doctor ask about the way the stool looks?	0.602	(0.320)	7.795	(3.012)
	Did the doctor ask if there was blood in the stool?	0.679	(0.335)	9.792	(3.141)
	Did the doctor ask if you are vomiting?	0.574	(0.277)	7.304	(2.583)
	Did the doctor ask if you also have a fever?	0.574	(0.322)	7.887	(3.024)
	Did the doctor ask about difficulty drinking or breastfeeding?	0.756	(0.530)	6.306	(4.915)
	Did the doctor ask if the child had convulsions?	2.753	(0.571)	0.991	(5.268)
	Did the doctor check the child's ear?	0.713	(0.934)	16.307	(8.798)
	Did the doctor ask if the child had diarrhea or vomiting?	0.578	(0.886)	11.767	(8.394)
Fever, diagnostic	Did the doctor ask questions about the child's vaccinations?	0.894	(0.509)	5.209	(4.674)
	Did the doctor take your temperature?	0.609	(0.216)	12.69	(2.079)
	Did the doctor check for neck stiffness?	0.908	(0.133)	5.730	(1.213)
	Did he ask if you felt weakness from lack of blood?	0.882	(0.140)	5.858	(1.285)
	Did he look in your ears or throat?	0.852	(0.170)	7.934	(1.561)
	Did he check your stomach?	0.915	(0.131)	5.839	(1.183)
	Did he ask for a blood slide?	0.588	(0.142)	6.023	(1.336)
	Did the doctor check if the child was sleepy, try to wake up the child?	0.950	(0.172)	5.438	(1.563)
	Did the doctor pinch the skin fold of the child?	0.765	(0.265)	10.426	(2.461)
	Did the doctor check both of the child's feet?	0.853	(0.240)	9.917	(2.207)
Fever, diagnostic, under 5	Did the doctor check the child's weight against a chart?	0.828	(0.191)	5.955	(1.744)

Item type	Item	Difficulty,		Discrimination score	Discrimination, standard error
		score	standard error		
Cough, diagnostic	Did he look at your throat?	0.780	(0.161)	7.032	(1.481)
	Did he listen to your chest?	0.622	(0.176)	7.504	(1.646)
	Did he take your temperature?	0.668	(0.186)	9.439	(1.746)
Cough, diagnostic, under 5	Did the doctor check if the child was sleepy, try to wake up the child?	0.953	(0.207)	4.593	(1.877)
	Did the doctor pinch the skin fold of the child?	0.778	(0.267)	9.640	(2.452)
	Did the doctor check the child's eyes, tongue, and palms?	0.782	(0.277)	10.260	(2.528)
	Did the doctor check both of the child's feet?	0.864	(0.322)	10.841	(2.855)
	Did the doctor check the child's weight against a chart?	0.933	(0.209)	5.502	(1.886)
	Did he pinch the skin on the stomach?	0.77	(0.332)	10.123	(3.054)
	Did he take your temperature?	0.705	(0.421)	10.779	(3.928)
Diarrhea, diagnostic	<i>If the child is under two years, Did he look at the child's head?</i>	1.023	(0.716)	10.298	(6.151)
	Did the doctor offer the child a drink of water or observe breastfeeding?	1.079	(0.910)	6.838	(8.079)
Diarrhea, diagnostic, under 5	Did the doctor check the child's eyes, tongue, and palms?	0.907	(0.551)	6.526	(5.034)
	Did the doctor check both of the child's feet?	3.364	(5.249)	1.508	(48.419)
	Did the doctor check the child's weight against a chart?	0.086	(0.500)	-2.884	(4.672)
General, diagnostic	Did the doctor examine you?	0.562	(0.140)	9.623	(1.368)

Bibliography

- Acemoglu, D., Kremer, M. & Mian, A. 2008, "Incentives in Markets, Firms, and Governments", *Journal of Law, Economics, and Organization*, vol. 24, no. 2, pp. 273-306.
- Akerlof, G.A. 1982, "Labor Contracts as Partial Gift Exchange", *The Quarterly Journal of Economics*, vol. 97, no. 4, pp. 543-569.
- Akerlof, G.A. & Kranton, R.E. 2008, "Identity, Supervision, and Work Groups", *American Economic Review*, vol. 98, no. 2, pp. 212-217.
- Akerlof, G.A. & Kranton, R.E. 2005, "Identity and the Economics of Organizations", *Journal of Economic Perspectives*, vol. 19, no. 1, pp. 9-32.
- Akerlof, G.A. & Kranton, R.E. 2000, "Economics and Identity", *Quarterly Journal of Economics*, vol. 115, no. 3, pp. 715-753.
- Akerlof, G.,A. 1982, "Labor Contracts as Partial Gift Exchange", *The Quarterly Journal of Economics*, vol. 97, no. 4, pp. 543-69.
- Andreoni, J. 1995, "Cooperation in Public-Goods Experiments: Kindness or Confusion?", *American Economic Review*, vol. 85, no. 4, pp. 891-904.
- Andreoni, J. 1990, "Impure Altruism and Donations to Public Goods: A Theory of Warm-Glow Giving?", *Economic Journal*, vol. 100, no. 401, pp. 464-77.
- Andreoni, J. 1989, "Giving with Impure Altruism: Applications to Charity and Ricardian Equivalence", *Journal of Political Economy*, vol. 97, no. 6, pp. 1447-58.
- Andreoni, J. & Bernheim, B.D. 2009, "Social Image and the 50?50 Norm: A Theoretical and Experimental Analysis of Audience Effects", *Econometrica*, vol. 77, no. 5, pp. 1607-1636.
- Andreoni, J. & Miller, J. 2002, "Giving According to GARP: An Experimental Test of the Consistency of Preferences for Altruism", *Econometrica*, vol. 70, no. 2, pp. 737-753.
- Angrist, J.D. & Krueger, A.B. 1999, "Chapter 23 Empirical strategies in labor economics" in *Handbook of Labor Economics*, ed. Orley C. Ashenfelter and David Card, Elsevier, , pp. 1277-1366.
- Ayres, I. & Siegelman, P. 1995, "Race and Gender Discrimination in Bargaining for a New Car", *American Economic Review*, vol. 85, no. 3, pp. 304-321.

- Bandiera, O., Barankay, I. & Rasul, I. 2005, "Social Preferences and the Response to Incentives: Evidence from Personnel Data", *The Quarterly Journal of Economics*, vol. 120, no. 3, pp. 917-962.
- Barro, R.J. 1974, "Are Government Bonds Net Wealth?", *Journal of Political Economy*, vol. 82, no. 6, pp. 1095-1117.
- Basu, K. 2006, "Teacher Truancy in India: The Role of Culture, Norms and Economic Incentives", *SSRN eLibrary*, .
- Becker, G.S. 1974, "A Theory of Social Interactions", *The Journal of Political Economy*, vol. 82, no. 6, pp. 1063-1093.
- Becker, G.S. 1971, *The Economics of Discrimination*, 2d edn, University of Chicago Press, Chicago.
- Benabou, R. & Tirole, J. 2006, "Incentives and Prosocial Behavior", *The American Economic Review*, vol. 96, no. 5, pp. 1652-1678.
- Bergstresser, K. 2009, *Effects of in-group bias in a gift-exchange transaction: A theory of employee ownership and evidence from a laboratory experiment*, University of Maryland.
- Boekeloo, B.O., Becker, D.M., Levine, D.M., Belitsos, P.C. & Pearson, T.A. 1990, "Strategies for Increasing House Staff Management of Cholesterol with Inpatients", *American Journal of Preventative Medicine*, vol. 6, no. 2, pp. 51-59.
- Bohnet, I., Greig, F., Hausmann, B. & Zeckhauser, R. 2008, "Betrayal aversion: Evidence from Brazil, China, Oman, Switzerland, Turkey, and the United States", *American Economic Review*, vol. 98, pp. 294-310.
- Bohnet, I. & Zeckhauser, R. 2004, "Social Comparisons in Ultimatum Bargaining", *Scandinavian Journal of Economics*, vol. 106, no. 3, pp. 495-510.
- Bohnet, I. & Frey, B.S. 1999, "Social Distance and Other-Regarding Behavior in Dictator Games: Comment", *American Economic Review*, vol. 89, no. 1, pp. 335-339.
- Bolton, G.E. & Ockenfels, A. 2010, "Betrayal Aversion: Evidence from Brazil, China, Oman, Switzerland, Turkey, and the United States: Comment", *American Economic Review*, vol. 100, no. 1, pp. 628-633.
- Bolton, G.E., Brandts, J. & Ockenfels, A. 2005, "Fair Procedures: Evidence from Games Involving Lotteries", *The Economic Journal*, vol. 115, no. 506, pp. 1054-1076.
- Bolton, G.E. & Ockenfels, A. 2000, "ERC: A Theory of Equity, Reciprocity, and Competition", *The American Economic Review*, vol. 90, no. 1, pp. 166-193.

- Bradler, C. 2009, "Social Preferences Under Risk - An Experimental Analysis", *SSRN eLibrary*, .
- Brock, J.M., Lange, A. & Ozbay, E. 2010, *Dictating the risks -- Experimental evidence on norms of giving in risky environments*, mimeo edn.
- Camerer, C. 2003, *Behavioral Game Theory: Experiments on Strategic Interaction*, Princeton University Press., Princeton, NJ.
- Cameron, A.C., Gelbach, J.B. & Miller, D.L. 2006, *Robust Inference with Multi-way Clustering*, National Bureau of Economic Research, Cambridge, Mass.
- Canton, E. 2005, "Power of Incentives in Public Organizations When Employees Are Intrinsically Motivated", *Journal of Institutional and Theoretical Economics*, vol. 161, no. 4, pp. 664-680.
- Card, D. 1999, "Chapter 30 The causal effect of education on earnings" in *Handbook of Labor Economics*, ed. Orley C. Ashenfelter and David Card, Elsevier, , pp. 1801-1863.
- Charness, G. 2004, "Attribution and Reciprocity in an Experimental Labor Market", *Journal of Labor Economics*, vol. 22, no. 3, pp. 665-688.
- Charness, G. & Rabin, M. 2002, "Understanding Social Preferences with Simple Tests", *Quarterly Journal of Economics*, vol. 117, no. 3, pp. 817-869.
- Dana, J., Weber, R.A. & Xi Kuang, J. 2007, "Exploiting moral wiggle room: experiments demonstrating an illusory preference for fairness", *Economics Theory*, vol. 33, no. 1, pp. 67-80.
- Das, J. & Hammer, J. 2007, "Location, Location, Location: Residence, Wealth, and the Quality of Medical Care in Delhi, India", *Health Affairs*, vol. 26, no. 3, pp. w338-351-w338-351.
- Das, J. & Hammer, J. 2005, "Which Doctor? Combining Vignettes and Item Response to Measure Clinical Competence", *Journal of Development Economics*, vol. 78, no. 2, pp. 348-383.
- Das, J. & Sohnesen, T.P. 2007, "Variations in Doctor Effort: Evidence from Paraguay", *Health Affairs*, vol. 26, no. 3, pp. w324-337.
- Deci, E.L. 1972, "Intrinsic motivation, extrinsic reinforcement, and inequity", *Journal of personality and social psychology*, vol. 22, no. 1, pp. 113-120.
- Deci, E.L. 1971, "Effects of externally mediated rewards on intrinsic motivation", *Journal of personality and social psychology*, vol. 18, no. 1, pp. 105-115.

- Deci, E.L., Cascio, W.F. & Krusell, J. 1975, "Cognitive evaluation theory and some comments on the Calder and Staw critique", *Journal of personality and social psychology*, vol. 31, no. 1, pp. 81-85.
- Delfgaauw, J. 2007, "Dedicated Doctors: Public and Private Provision of Health Care with Altruistic Physicians", *Tinbergen Institute Discussion Papers 07-010/1*, Tinbergen Institute, revised 17 Sep 2007, .
- Dolea, C. & Adams, O. 2005, "Motivation of Health Care Workers-Review of Theories and Empirical Evidence", *Cah Sociol Demogr Med*, vol. 45, no. 1, pp. 135-161.
- Dragow, F. & Parsons, C.K. 1983, "Application of Unidimensional Item Response Theory Models to Multidimensional Data", *Applied Psychological Measurement*, vol. 7, pp. 189-199.
- Eckel, C.C. 2007, "People Playing Games: The Human Face of Experimental Economics", *Southern Economic Journal*, vol. 73, no. 4, pp. 840-857.
- Eckel, C.C. & Grossman, P.J. 1996, "Altruism in Anonymous Dictator Games", *Games and Economic Behavior*, vol. 16, no. 2, pp. 181-191.
- Eckel, C.C. & Wilson, R.K. 2003, "Conditional trust: sex, race, and facial expressions in a trust game", *Conference on Trust and Institutions*, April 24–26, 2003.
- Ellingsen, T. & Johannesson, M. 2008, "Pride and Prejudice: The Human Side of Incentive Theory", *American Economic Review*, vol. 98, no. 3, pp. 990-1008.
- Engelmann, D. & Strobel, M. 2004, "Inequality Aversion, Efficiency, and Maximin Preferences in Simple Distribution Experiments", *American Economic Review*, vol. 94, no. 4, pp. 857-869.
- Fairbrother, G., Hanson, K.L., Friedman, S. & Butts, G.C. 1999, "The Impact of Physician Bonuses, Enhanced Fees, and Feedback on Childhood Immunization Coverage Rates", *American Journal of Public Health*, vol. 89, no. 2, pp. 171-175.
- Falk, A., Fehr, E. & Fischbacher, U. 2008, "Testing theories of fairness—Intentions matter", *Games and Economic Behavior*, vol. 62, no. 1, pp. 287-303.
- Farber, H.S. 2008, "Reference-Dependent Preferences and Labor Supply: The Case of New York City Taxi Drivers", *American Economic Review*, vol. 98, no. 3, pp. 1069-1082.
- Fehr, E. & Schmidt, K.M. 1999, "A Theory Of Fairness, Competition, and Cooperation*", *Quarterly Journal of Economics*, vol. 114, no. 3, pp. 817-868.
- Fershtman, C. & Weiss, Y. 1993, "Social Status, Culture and Economic Performance", *Economic Journal*, vol. 103, no. 419, pp. 946-959.

- Forsythe, R., Horowitz, J.L., Savin, N.E. & Sefton, M. 1994, "Fairness in Simple Bargaining Experiments", *Games and Economic Behavior*, vol. 6, no. 3, pp. 347-369.
- Franco, L.M., Bennett, S. & Kanfer, R. 2002, "Health sector reform and public sector health worker motivation: a conceptual framework", *Social science & medicine* (1982), vol. 54, no. 8, pp. 1255-1266.
- Francois, P. 2007, "Making a Difference", *RAND Journal of Economics*, vol. 38, no. 3, pp. 714-732.
- Francois, P. 2003, "Not-for-Profit Provision of Public Services", *Economic Journal*, vol. 113, no. 486, pp. C53-61; C53-61.
- Francois, P. & Vlassopoulos, M. 2008, "Pro-social Motivation and the Delivery of Social Services", *CESifo Economic Studies*, , pp. ifn002-ifn002.
- Freidson, E. 2001, *Professionalism, the Third Logic: On the Practice of Knowledge*, University Of Chicago Press.
- Gachter, S. & Falk, A. 2000, "Work Motivation, Institutions, and Performance", *SSRN eLibrary*, .
- Gneezy, U. & List, J.A. 2006, "Putting Behavioral Economics to Work: Testing for Gift Exchange in Labor Markets Using Field Experiments", *Econometrica*, vol. 74, no. 5, pp. 1365-1384.
- Gneezy, U. & Rustichini, A. 2000, "Pay Enough or Don't Pay at All", *Quarterly Journal of Economics*, vol. 115, no. 3, pp. 791-810.
- Goeree, J.K., Holt, C.A. & Laury, S.K. 2002, "Private Costs and Public Benefits: Unraveling the Effects of Altruism and Noisy Behavior", *Journal of Public Economics*, vol. 83, no. 2, pp. 255-276.
- Grant, S. 1995, "Subjective Probability Without Monotonicity: or How Machina's Mom May Also be Probabilistically Sophisticated", *Econometrica*, vol. 63, no. 1, pp. pp. 159-189.
- Greene, W.H. 2003, *Econometric analysis*, Prentice Hall, Upper Saddle River, N.J.
- Gregg, P., Grout, P.A., Ratcliffe, A., Smith, S. & Windmeijer, F. 2011, "How important is pro-social behaviour in the delivery of public services?", *Journal of Public Economics*, vol. 95, no. 7-8, pp. 758-766.
- Hambleton, R.K., Swaminathan, H. & Rogers, H.J. 1991, *Fundamentals of Item Response Theory*, Sage Publications, Newbury Park, CA.

- Harrison, D.A. 1986, "Robustness of Irt Parameter Estimation to Violations of The Unidimensionality Assumption", *Journal of Educational and Behavioral Statistics*, vol. 11, no. 2, pp. 91-115.
- Holm, H. & Engfeld, P. 2005, "Choosing Bargaining Partners – an Experimental Study on the Impact of Information about Income, Status and Gender", *Experimental Economics*, vol. 8, no. 3, pp. 183-216.
- Ip, E. 2001, "Testing for local dependency in dichotomous and polytomous item response models", *Psychometrika*, vol. 66, pp. 109-132.
- Kahneman, D., Knetsch, J.L. & Thaler, R. 1986, "Fairness as a Constraint on Profit Seeking: Entitlements in the Market", *The American Economic Review*, vol. 76, no. 4, pp. pp. 728-741.
- Kircher, P., Ludwig, S. & Sandroni, A. 2009, *Fairness: a critique to the utilitarian approach*, discussion paper edn, SFB/TR 15 GESY, Bonn, Germany.
- Krawczyk, M. & Le Lec, F. 2008, *Social decisions under risk. Evidence from the probabilistic dictator game*, working paper edn, University of Amsterdam.
- Kreps, D.M. 1997, "Intrinsic Motivation and Extrinsic Incentives", *American Economic Review*, vol. 87, no. 2, pp. 359-364.
- Leonard, K.L., Serneels, P., Brock, J.M. & Masatu, M.C. forthcoming, "Health Worker Performance" in *Human Resources for Health in Africa: A New Look at the Crisis*, eds. A. Soucat & R.M. Scheffler, University of California, Berkeley and The World Bank, .
- Leonard, K.L. 2008, "Is Patient Satisfaction Sensitive to Changes in the Quality of Care? An Exploitation of the Hawthorne Effect", *Journal of health economics*, vol. 27, no. 2, pp. 444-459.
- Leonard, K.L. & Masatu, M.C. 2008, "Professionalism, Latent Professionalism and Organizational Demands for Health Care Quality in a Developing Country", *Working Papers 42883, University of Maryland, Department of Agricultural and Resource Economics*, .
- Leonard, K.L. & Masatu, M.C. 2005, "The Use of Direct Clinician Observation and Vignettes for Health Services Quality Evaluation in Developing Countries", *Social Science & Medicine*, vol. 61, no. 9, pp. 1944-1951.
- Leonard, K.L., Masatu, M.C. & Vialou, A. 2007, "Getting Doctors to Do Their Best: The Roles of Ability and Motivation in Health Care Quality", *Journal of Human Resources*, vol. 42, no. 3, pp. 682-700.

- Leonard, K.,L. & Masatu, M.,C. 2007, "Variations in the quality of care accessible to rural communities in Tanzania", *Health affairs (Project Hope)*, vol. 26, no. 3, pp. w380-392; w380-392.
- Leonard, K. & Masatu, M.C. 2006, "Outpatient Process Quality Evaluation and the Hawthorne Effect", *Social Science & Medicine*, vol. 63, no. 9, pp. 2330-2340.
- Levine, D.K. 1998, "Modeling Altruism and Spitefulness in Experiments", *Review of Economic Dynamics*, vol. 1, no. 3, pp. 593-622.
- Levitt, S.D. & List, J.A. 2007, "What Do Laboratory Experiments Measuring Social Preferences Reveal about the Real World?", *Journal of Economic Perspectives*, vol. 21, no. 2, pp. 153-174.
- Levitt, S.D., List, J.A. & Reiley, D.H. 2010, "What Happens in the Field Stays in the Field: Exploring Whether Professionals Play Minimax in Laboratory Experiments", *Econometrica*, vol. 78, no. 4, pp. 1413-1434.
- Lindelow, M. & Serneels, P. 2006, "The performance of health workers in Ethiopia: Results from qualitative research", *Social science & medicine*, vol. 62, no. 9, pp. 2225-2235.
- List, J.A. 2004, "Young, Selfish and Male: Field Evidence of Social Preferences", *The Economic Journal*, vol. 114, no. 1, pp. 121-149.
- List, J.A. & Mason, C.F. 2011, "Are CEOs expected utility maximizers?", *Journal of Econometrics*, vol. 162, no. 1, pp. 114-123.
- List, J. 2007, "On the Interpretation of Giving in Dictator Games", *The Journal of Political Economy*, vol. 115, no. 3, pp. pp. 482-493.
- Lopez, A.D., Mathers, C.D., Ezzati, M., Jamison, D.T. & Murray, C.J.L. (eds) 2006, *Global Burden of Disease and Risk Factors*, Oxford University Press, New York.
- Lusingu, J., Vestergaard, L., Mmbando, B., Drakeley, C., Jones, C., Akida, J., Savaeli, Z., Kitua, A., Lemnge, M. & Theander, T. 2004, "Malaria morbidity and immunity among residents of villages with different Plasmodium falciparum transmission intensity in North-Eastern Tanzania", *Malaria Journal*, vol. 3, no. 1, pp. 26.
- Machina, M.J. 1989, "Dynamic Consistency and Non-Expected Utility Models of Choice Under Uncertainty", *Journal of Economic Literature*, vol. 27, no. 4, pp. pp. 1622-1668.
- Mas, A. 2006, "Pay, Reference Points, and Police Performance", *Quarterly Journal of Economics*, vol. 121, no. 3, pp. 783-821.

- McCabe, K.A., Rigdon, M.L. & Smith, V.L. 2003, "Positive Reciprocity and Intentions in Trust Games", *Journal of Economic Behavior & Organization*, vol. 52, no. 2, pp. 267-275.
- Morreim, E.H. 1995, *Balancing act : the new medical ethics of medicine's new economics*, Georgetown University Press, Washington, D.C.
- Nerlove, M. 2002, *Essays in panel data econometrics*, Cambridge University Press, Cambridge ; New York.
- Palfrey, T.R. & Prisbrey, J.E. 1997, "Anomalous Behavior in Public Goods Experiments: How Much and Why?", *American Economic Review*, vol. 87, no. 5, pp. 829-846.
- Palfrey, T.R. & Prisbrey, J.E. 1996, "Altruism, Reputation and Noise in Linear Public Goods Experiments", *Journal of Public Economics*, vol. 61, no. 3, pp. 409-427.
- Pellegrino, E.D. 1987, "Altruism, Self-interest, and Medical Ethics", *JAMA: The Journal of the American Medical Association*, vol. 258, no. 14, pp. 1939-1940.
- Prendergast, C. 2007, "The Motivation and Bias of Bureaucrats", *American Economic Review*, vol. 97, no. 1, pp. 180-196.
- Rabin, M. 1993, "Incorporating Fairness into Game Theory and Economics", *American Economic Review*, vol. 83, no. 5, pp. 1281-1302.
- Ramalingam, A. & Rauh, M.T. 2009, "The Firm as an Intrinsic Motivation Device", *SSRN eLibrary*, .
- Reid, L. 2005, "Diminishing Returns? Risk and the Duty to Care in the SARS Epidemic", *Bioethics*, vol. 19, no. 4, pp. 348-361.
- Rigdon, M.L. 2002, "Efficiency wages in an experimental labor market", *Proceedings of the National Academy of Sciences of the United States of America*, vol. 99, no. 20, pp. 13348-13351.
- Roberts, R.D. 1987, "Financing Public Goods", *Journal of Political Economy*, vol. 95, no. 2, pp. 420-437.
- Rose-Ackerman, S. 1996, "Altruism, Nonprofits, and Economic Theory", *Journal of Economic Literature*, vol. 34, no. 2, pp. 701-728.
- Rosenberg, M. 2007, "Global Child Health: Burden of Disease, Achievements, and Future Challenges", *Current Problems in Pediatric and Adolescent Health Care*, vol. 37, no. 9, pp. 338-362.
- Rotolo, T. & Wilson, J. 2006, "Employment Sector and Volunteering: The Contribution of Nonprofit and Public Sector Workers to the Volunteer Labor Force", *The Sociological Quarterly*, vol. 47, no. 1, pp. 21-40.

- Santoso, B., Suryawati, S. & Prawaitasari, J.E. 1996, "Small Group Intervention vs Formal Seminar for Improving Appropriate Drug Use", *Social Science & Medicine*, vol. 42, no. 8, pp. 1163-1168.
- Seabright, P. 2004, "Continuous Preferences Can Cause Discontinuous Choices: An Application to the Impact of Incentives on Altruism", *SSRN eLibrary*, .
- Serneels, P., Montalvo, J. & Lindelow, M. 2009, *Public Service and Selection. Unraveling Health Worker Job Choice*.
- Serneels, P., Montalvo, J., Pettersson, G., Lievens, T., Butera, J.D. & Kidanu, A. 2010, *Who Wants to Work in a Rural Health Post? The Role of Intrinsic Motivation, Rural Background and Faith Based Institutions in Rwanda and Ethiopia*.
- Serra, D., Serneels, P. & Barr, A. "Intrinsic motivations and the non-profit health sector: Evidence from Ethiopia", *Personality and Individual Differences*, vol. In Press, Corrected Proof.
- Sliwka, D. 2007, "Trust as a Signal of a Social Norm and the Hidden Costs of Incentive Schemes", *American Economic Review*, vol. 97, no. 3, pp. 999-1012.
- Sobel, J. 2005, "Interdependent Preferences and Reciprocity", *Journal of Economic Literature*, vol. 43, no. 2, pp. 392-436.
- Soumerai, S.B. & Avorn, J. 1990, "Principles of Educational Outreach ('academic detailing') to Improve Clinical Decision Making", *Journal of the American Medical Association*, vol. 263, no. 4, pp. 549-556.
- Straus, S.E., Wilson, K., Rambaldini, G., Rath, D., Lin, Y., Gold, W.L. & Kapral, M.K. 2004, "Severe acute respiratory syndrome and its impact on professionalism: qualitative study of physicians' behaviour during an emerging healthcare crisis", *BMJ*, vol. 329, no. 7457, pp. 83.
- Sugden, R. 1982, "On the Economics of Philanthropy", *Economic Journal*, vol. 92, no. 366, pp. 341-350.
- Tierney, W.M., Hui, S.L. & McDonald, C.J. 1986, "Delayed Feedback of Physician Performance Versus Immediate Reminders to Perform Preventive Care. Effects on Physician Compliance", *Medical Care*, vol. 24, no. 8, pp. 659-666.
- Tomlinson, T. 2008, "Caring for risky patients: duty or virtue?", *Journal of medical ethics*, vol. 34, no. 6, pp. 458-462.
- Trautmann, S.T. 2009, "A tractable model of process fairness under risk", *Journal of Economic Psychology*, vol. 30, no. 5, pp. 803-813.
- Winskill, P., Rowland, M., Mtove, G., Malima, R. & Kirby, M. 2011, "Malaria risk factors in north-east Tanzania", *Malaria Journal*, vol. 10, no. 1, pp. 98.

Wooldridge, J.M. 2002, *Econometric analysis of cross section and panel data*, MIT Press, Cambridge, Mass.