Charles University in Prague

Faculty of Social Sciences

Institute of Economic Studies

Bachelor's Thesis

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Experimental Testing of Game-Theoretical Predictions: The Ultimatum Game

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Abstract

This thesis focuses on testing of game theoretical predictions in the ultimatum game by means of controlled experiments. This game has become one of the most scrutinized games from the area of bargaining game theory. The theoretical division of the reward, which the players bargain over, is such that one player gets virtually all the reward while the second player is left with nothing. Because of such an extreme division of the reward, the game represents a severe test for the theory. In fact, experimental results do not confirm to the theory. This thesis provides a survey of the experimental studies investigating different aspects that may affect the subjects' behavior in the game. Furthermore, some possible explanations for why the theoretical solution is not observed to be played by the subjects in the laboratory are presented. I show several new models, which try to capture the real nature of the subjects' behavior in the game. I also focus on the proposers' behavior from the income-maximizing point of view if the distribution of the responder's minimum acceptance thresholds is known to them. Outline of a new experiment examining such behavior is then presented.

JEL Classification: C71, C90

Key words: bargaining, experimental economics, game theory, ultimatum game

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Abstrakt

Práce se zaměřuje na ověřování teoretických předpovědí ohledně ultimatum game pomocí kontrolovaných experimentů. Tato hra se stala jednou z nejvíce zkoumaných her z oblasti vyjednávání, jakožto jedné části teorie her. Teoreticke rozdělení odměny, o kterou se hráči dohadují, je takové, že jeden z hráčů obdrží prakticky celou odměnu, zatímco podíl druhého hráče na odměně je nulový. Protože teoretické rozdělení odměny je extrémní, hra představuje těžkou zkoušku pro teorii. Ve skutečnosti, experimentální výsledky ji nepotvrzují. Tato práce poskytuje přehled experimentálních prací zkoumajících různé aspekty, které mohou ovlivnit chování subjektů ve hře. Dále jsou uvedeny některá z možných vysvětlení, proč subjekty v laboratořích nehrají teoretické řešní. Představuji několik nových modelů, které se snaží zachytit skutečnou podstatu chování subjektů ve hře. Také zkoumám chování prvních hráčů z pohledu maximalizace jejich příjmů v případě, že znají rozdělení minimálních přijatelných hranic druhých hráčů. Poté je předtaven nástin nového experimentu, který takové chování zkoumá.

Klasifikace JEL: C71, C90

Klíčová slova: experimentální ekonomie, teorie her, ultimatum game, vyjednávání

Rozsah práce: 115 930 znaků

Declaration of authorship

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Prague, July 31, 2011

signature

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Garant studijního programu Vám dle zákona č. 111/1998 Sb. o vysokých školách a Studijního a zkušebního řádu UK v Praze určuje následující bakalářskou práci

Předpokládaný název BP:

Experimentální ověřování základů teorie her

Charakteristika tématu, současný stav poznání, případné zvláštní metody zpracování tématu:

Teorie her analyzuje konfliktní situace nejen v ekonomii, ale i v řadě dalších oborů. Aktuální otevřenou otázkou však je, zda a jak lze experimentálně ověřovat její teoretické základy. Práce bude vycházet z výzkumu vedeného Kenem Binmorem, který je shrnut v knize *Does Game Theory Work?*. Součástí práce by mělo být i zopakování některého z experimentů, avšak s pozměněným odměňováním hráčů.

Struktura BP:

Abstrakt

Nejprve popíšu základy teorie her, kde se soustředím zejména na předpoklady, na nichž je založena. Dále uvedu několik modelových situací, jejichž řešení teorie her poskytuje. Poté se budu věnovat analýze dosahování výsledků v některých experimentech. Na závěr se pokusím zopakovat některý z popisovaných experimentů pro hráče s odlišnou motivací k výhře.

Osnova

1. Metodologické základy teorie her.
2. Příklady her.
3. Možnosti experimentálního ověřování teorie her.
4. Experimenty Kena Binmora.
5. Popis vlastního experimentu.
6. Shrnutí a závěr.

Seznam základních pramenů a odborné literatury:

Ken Binmore, *Does Game Theory Work? The Bargaining Challenge* , MIT Press, Cambridge and London, 2007.
Eric Rasmusen, *Games and Information, An Introduction to Game Theory*, Blackwell Publishing, Malden, Oxford and Victoria, 2007.

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Contents

List of Figures

List of Tables

Introduction

For most of its existence, economics was viewed as a nonexperimental science. However, this attitude was not meant to last. With the emergence of new theories in the 1940s that were focused on behavior of individuals as opposed to macroeconomic analyses, the possibility of experimental testing of such theories suddenly did not seem unrealistic. Nonetheless, it still took additional 40 years before having been recognized as a legitimate economic method by a larger audience of economists (although many of them find it controversial to this day). Since that time, the number of papers from the area of experimental economics reported in academic journals has grown exponentially.

Even though some people might think of experiments just as playing games under circumstances which do not correspond to the real world, there are several methods how to quiet such voices, at least to some degree. Actually, whether experiments are better than studies of naturally-occurring data, or whether it is the other way round, is not the real question. Every method has its advantages and disadvantages. They both can help in penetrating the mystery of human behavior. For this purpose, they can be seen as complementarities, not substitutes. Some thoughts on the justification of experiments in economics are presented at the beginning of this work. As the topic of experimental economics is vast, in this thesis I cover just one segment, and only partly. That is the behavior of individuals during strategic interactions, which is the core of the game theory.

Analysis of strategic thinking is not only connected with the times of warriors, when the correct anticipation of moves of their opponents could lead to great victories, or with strategic games such as chess or whist. The scope of reciprocal actions which game theory can be applied to is much larger, and economics certainly belongs to them. This thesis provides a closer look at one aspect of our life, which is bargaining.

Orthodox game theory, as Paul von Neumann and Oskar Morgenstern (1944) see it, works with an assumption of perfect rationality[1] that is commonly known[2]. No room for mistakes or any other variables, which can influence the decision-making process, is left in it. However, many experimental results do not support theoretical predictions and thus explaining these deviations has become a challenge for game theorists.

This work goes into details of one such game, that gives different experimental results than one predicted by the theory, namely through the ultimatum game. It belongs to an archetypal bargaining problem, which is splitting a pie. The ultimatum game can be seen as a representation of the last stage of the bargaining process if the number of stages is finite. Another application of the game can be bargaining under deadline, if the delay is not costly. Then the player who moves first can wait until the very end of the time and make take-it-or-leave-it proposal.

Under the assumption that players are income-maximizers, the theory predicts a very extreme division, giving virtually the whole pie to one of the players, while the other player receives virtually zero. Nonetheless, the experimental results do not confirm to these predictions, and therefore the ultimatum game has become one of the most investigated game in the field of bargaining behavior. The results are often quoted as an evidence for fairness being an important determinant of subjects' behavior during the bargaining, or during economic interactions in general. New models, that employ concerns for fairness into the utility function, have been developed in a response to the deviations from theoretic predictions, and even more studies examining these new models have been conducted. Aim of this thesis is to provide a survey of experimental studies focused on this game, and to give an introduction to some of the new models.

It has been observed that with gaining experience the players who move first seem to propose the division in order to maximize their outcome, whereas

[1] For definition of a *rational* player see page 21.
[2] For definition of *common knowledge* see page 21.

the second players do not adjust their behavior in such direction. Therefore, I examine in more detail what the income-maximizing behavior of the first players is when they know that some positive offers may be declined. I present a design of an experiment investigating whether the subjects act as income-maximizers when being given an information allowing them to compute the optimal offer.

The thesis is divided into seven sections. The first section covers the subject of experimental economics as a whole, focusing primarily on the methodology of experiments. The second section contains the theoretical side of game theory. I set definitions of the terms I work later with, and I go through some types of the games. Further, I introduce the concept of backward induction, which is one tool for refinement of multiple equilibria in sequential games.

Sections three and four cover the main focus of this thesis, the ultimatum game, which represents the experimental side of game theory. Section three acquaints the reader with the most common findings of experimental studies together with summary statistics of some works discussed later in this thesis. Section four provides a more thorough analysis of these experimental works that are divided in accordance with the aspects the studies have focused on. At the end of the section, the reader can find the main findings of these studies, and the discussion of the background of the results.

Section five introduces the dictator game that has been developed in order to distinguish between two different theories explaining behavior of players who move first in the ultimatum game. I review what these theories say and I show the results of two experimental studies on the dictator game with the implications they bring to the theories. Section six is devoted to the theoretical models that have been developed in the light of experimental results. I present two groups — the static models, that try to capture the motives behind the subjects' behavior, without implementing the element of time, and the dynamic models, that focus on changes in the behavior with time (in a sense when subjects repeat the game, while playing against different opponent).

Section seven focuses on income-maximization behavior when players are provided with information allowing them to compute the optimal offer maximizing the average outcome. First, I analyze what such behavior means, and I provide the reader with a few examples. Further, I outline the idea of my experiment examining the income-maximization behavior. At the end of the thesis, I summarize the main findings of this work.

1 A few words on experimental economics

1.1 Methodology

1.1.1 Microeconomic system

When examining behavior of individuals, one should first understand what the elements of a microeconomic system are. According to Smith (1982), the microeconomic system is defined by an environment and an institution. The environment consists of a list of commodities and economic agents who are characterized by their preference maps and initial endowments of commodities and technology/knowledge. The institution states the framework under which a trade can take place — the message space (a bid, an offer, an acceptance) that is necessary for market communication, the rules that regulate an exchange of messages, and the rules how the messages become binding commitments. Finally, the concern of economists is to know enough about the environment and to observe behavior of the agents as a function of alternative institutions and the environment.

The task of laboratory experiments is to create such a system while maintaining certain control over it. There are variables whose influence on subjects the researcher wants to understand — *focus variables*. However, other variables might also have an effect on behavior of agents and then the researcher must not forget taking them into account (otherwise the interpretation of the results may be false). Those are known as *nuisance variables*. A style how the instructions are written can serve as an example. Fundamental instruments how to tackle the problem of nuisance variables are control and randomization.

Control over variables is the key feature that distinguishes experiments from analysis of naturally-occurring data. It can be achieved in two ways. One can either keep them constant throughout the whole experiment, or she can set them to different levels at certain stages of the experiment. The latter ones are called *treatment* variables. It is often useful to employ the focus variables in the role of treatment variables — by changing the levels, their effects can be captured more easily. If control is the greatest advantage of experiments, one should obviously seek to prevent its loosing. Varying more than one factor in the same time can bring a problem, because the researcher might not be able to determine the source of eventual deviation then.

Another problem is that some nuisance variable cannot be controlled for. This gives a hard time for sustaining independence[3] which is important for generating valid results. Randomization is the trick that provides independence — the higher the number of subjects, whom it is being randomized over, the greater the guarantee of independence. (Friedman and Cassar, 2004)

1.1.2 Induced value theory

One of the challenges experimenters face is to have control over the subjects' preferences. It appears as an impossible task, because the structure of utility function is a private information, moreover, each utility function is unique (even though they have similarities). Nevertheless, Smith (1982) frames the concept of induced valuation, which deals with this problem. It is the idea that adequate use of a reward structure induces known characteristics in the subjects[4]. He states four sufficient conditions for the concept of induced valuation, which, if they hold, will ensure the control:

1. *Nonsatiation (monotonicity).* Utility is a monotone increasing function of the reward medium.

2. *Saliency.* Rewards are linked to the performance of subjects in the experiment. Subjects are aware of it and understand what combina-

[3] "Treatment variables are *independent* if knowing the value of one variable does not give any information about the level of other variables." (Friedman and Crosson, 2005)

[4] For more about the theory of induced valuation see Smith (1976).

tion of actions gives what outcome, along with the mechanism of how obtained outcome is translated to the reward medium afterward.

3. *Dominance.* The reward structure dominates all other components of subjects' utility function.

4. *Privacy.* Each subject knows nothing but her own payoff alternatives.

In his paper, Smith (1982) does not further specify what should be used as such reward medium. However, it has become common practice to employ pecuniary rewards. Use of money is quite straightforward. Economics assumes utility functions to be monotone increasing in amount of money one owns, which meets the first condition.

Whether the second condition holds or not is detected by letting subjects to answer a brief questionnaire before launching the treatment[5]. The questions are on a basis "if you take an action A and the second player takes an action B, your outcome will be ..... which represents ..... $". If subject answers incorrectly, or if she reports confusion in this direction, experimenters do not put her data into the subsequent analysis.

The third condition is the most problematic one. If it does not hold, subjects may actually be playing a different game than experimenters want to investigate. The dominance is not present in situations when subjects perceive high costs of thinking relatively to the reward, when they are bored, when they try to play a game against/with the experimenter whom they believe to have certain expectations of their play, etc. This is interconnected with an often cited critique that the payoffs[6] in experiments are too small. The obvious response to such critique is to run experiments with higher values of the reward. In many cases, this step has been undertaken, indeed. Stake effects are mixed. There are experiments which detect some significant differences for different size of stake (i.e. size of reward), but there are

[5] For explanation of the term *treatment* see page 11.

[6] For definition of *payoff* go to page 21.

others which do not observe any such changes.[7] Even if one admits that in some cases stake effects are relevant, than how she should determine "what the "right" level of stakes should be? How often do people make decisions involving monthly incomes (Falk and Heckmann, 2009)?"

The last condition deals with the possibility that utility of a subject might depend not only on one's own payoff, but also on what her opponents' payoffs are. Then, the induced characteristics are not the same as experimenters think of. Nonetheless, implementing the need for incomplete information[8] into experiments restrict enormously a set of situations available to closer examination. Therefore, this condition is often violated, and then new models of payoff-interdependent preferences might come into play.

Smith (1982) does not say that all the conditions must be met for having control over the preferences, i.e. these conditions are sufficient, not necessary. He only says that if they all hold then the control is achieved.

1.2 Design of experiments

1.2.1 Building blocks of an experiment

Term: Session

The term *session* represents everything what happens between the arrival of subjects and the payments of their earnings by the experimenter.

Term: Treatment and control

Treatment is a procedure covering one level of a treatment variable. A common practice is to create more different treatments. One of them acts as a *control* — the benchmark phenomenon —

[7] For an illustration, experiments on ultimatum game that focus on reward size are to be found in subsection 4.1 at page 39.

[8] For definition of *incomplete information* go to page 22.

and the others are compared with the results of the control to see effects caused by procedural variation.

1.2.2 Laboratory vs field

When speaking of experiments, most people envision experiments in the laboratory. Nonetheless, the range is far broader. The spectrum of how the data of interest are collected and further analyzed can be divided into three basic groups. On one side there are laboratory experiments creating artificial institutions. On the other side there are measurement models of naturally-occurring data. Field experiments, the last group, serve as a "bridge" between these two opposite banks. (List, 2007)

Harrison and List (2004) propose a classification scheme of experiments based on variability in six criteria: the nature of the subject pool, the nature of the information that the subjects bring to the task, the nature of the commodity, the nature of the task or trading rules applied, the nature of the stakes, and the nature of the environment that the subjects operate in.

The first class are *conventional laboratory experiments*, which use undergraduate students as a subject pool and create artificial institutions. The criticism of (un)representativeness of students has resulted in the class called *artefactual field experiment*s. These experiments are identical with the conventional lab experiments except for the selection of the subject pool; agents are selected from population outside the school walls. Further in the text, I use the term "laboratory/lab experiments" when referring either to the conventional laboratory, or to the artefactual field experiments, and the term "field experiments" when referring to the framed field, or to the natural field experiments, that are discussed immediately.

Framed field experiment, besides of using a nonstandard subject pool, further extends field context into either the commodity, the task, the stakes, the information set of the subjects, or any combination of them. Lab experiments use context-free scripts without emotionally charged words, because any insinuation might influence the process of decision-making of subjects. The supporters of field experiments, on the other hand, claim that field referents can help subjects with confusion about the task, which can lead to the loss of control.

The last type, whose design is the closest to naturally-occurring situations, are *natural field experiment*s. Such experiments take place in the environment where the subjects ordinarily solve the tasks, moreover, they are not aware of themselves being participants in the experiment. (Harrison and List, 2004)

One method carrying the word experiment still remains, although it is not an experiment in the true sense. *Natural experiments*, also called *quasi-experiments*, belong between the studies of naturally-occurring events but treating them as if they were experiments. As in the classical experiments, the researcher compares a treatment group with a control group, but the treatment variations are not in the hands of the researcher, they have arisen unplanned (e.g. by changes in government policies, by nature, by an accident). (Greenstone and Gayer, 2009)

1.3 Validity

Experiments in the laboratory examine human behavior in very specific settings, that rarely (if ever) can be found in the real world. Scientists are not really eager to know "what happens when a group of undergraduate students play lottery games" *per se*, rather they are interested in whether the observed pattern of their behavior can be generalized to how people behave in some particular market outside the lab (Guala, 2005). Two dimensions, internal validity and external validity, are cores of this issue.

1.3.1 Internal validity

> ***Def.: Internal validity***
> *Internal validity* is "the ability of the researcher to argue that observed correlations are causal" Roe and Just (2009).

Internal validity asks what happens when examining a particular design of an experiment and whether the drawn inference is valid within this design. For example, we might doubt internal validity of the experiment where control is lost. Even though high replicability of an experiment does not imply internal validity, low replicability tend to signalize that internal validity might be in question. (Bardsley et al., 2010)

Roe and Just (2009) suggest several points that may threaten internal validity:

> PROBLEM 1:
> *Lack of temporal clarity.*

If one does not capture properly the timing of stimulus and the timing of potential response, then she cannot establish causality among them. This concern does not apply to the lab experiments, whose structure allows the experimenter to recognize exact timing of both the stimulus and the response. On the other hand, from field data, the researcher may not be able to identify the direction causality runs either due to roughness in the time frame, in which the data are obtained, or due to recall bias when subjects are filling out questionnaires that are further being analyzed.

> PROBLEM 2:
> *Systematic differences in treatment group.*

The researcher needs to be able to state that the treatment and the control groups of participants are (statistically) identical, that they do not differ in their characteristics. In the laboratory it is achieved by random assignment of participants to either the treatment or the control group. Natural settings do not dispose of such mechanism and they may not eliminate systematic differences.

PROBLEM 3:
Concurrent third elements that confound the outcome.

Uncontrolled variation of different variables than the focus one causes problem with establishing causality. Any found correlation might be false, because the third element might influence both the stimulus and the response. Laboratory, with its tight control, can limit the possibility of systematic variation of the third elements between the treatments. As one moves from laboratory experiments toward field experiments, and toward analysis of naturally-occurring data, the third elements become more likely to be present.

1.3.2 External validity

Def.: External validity
External validity is “the ability to generalize the relationship found in a study to other persons, times, and settings” Roe and Just (2009).

External validity is a big issue fiercely discussed among experimental and nonexperimental scientists. In lab experiments, internal validity can be seen as an antecedent to external validity. If the stage one, the results are internally valid, is achieved, then it is the turn of stage two — whether external validity holds as well. It does not make much sense in situation when the stage 1 fails think of external validity at all. (Guala, 2005)

Smith (1982) thinks of external validity in his fifth condition for a microeconomic experiment called *parallelism* (the first four conditions are nonsatiation, saliency, dominance, and privacy, that are sufficient for induced valuation; see page 9) that is oriented on sufficiency of transferability of obtained results. It states that propositions drawn from manifestation of subject’s behavior under the particular experimental institution should apply also to nonlab microeconomic world "where similar *ceteris paribus* conditions hold" (Smith, 1982).

Roe and Just (2009) raise several threats to external validity:

> PROBLEM 1:
> *Potential interaction with elements and context not found in study.*

Trying to apply relationship identified under controlled settings to an environment which involves uncontrolled variables might diminish the predictive power about the outcome. This seems to be the biggest disadvantage of laboratory experiments. The microeconomic world they create is often simplified and lacks many contextual elements that are present in a microeconomic world that has evolved outside the lab. Hence, these additional factors can limit the applicability of the evidence found in laboratory to other settings. Contrary to laboratory experiments, if causality under one natural setting has been found internally valid, then the chance of the researcher to make good predictions about the outcome under another, similar, setting might increase.

> PROBLEM 2:
> *Limited variation within stimulus or response.*

Laboratory experiments are scanty in terms of confronting subjects with unlimited levels of the stimulus and responses, such as long term horizons, scope of possible subjects' losses, etc. The evidence observed in the lab might not detect what the results would be without such restrictions.

There exists a trade-off between external and internal validity. Laboratory experiments are internally valid the most likely, on the other hand, they suffer from threats about their external validity greatly. Moving toward natural settings, the likelihood of high internal validity as opposed to low external validity becomes to change in favor of external validity. Field and natural experiments reach certain balance between these two dimensions, on the other hand, they are limited in the scope of topics available for the analysis. For example, field experiments may be limited due to needs for some qualification or personal contacts in the markets/institutions of interest. Even though the researchers finally gain access to desirable market, they may face restrictions on what interventions are allowed to be implemented. Another limitation of field experiments is the difficulty with their replicability. (Roe and Just, 2009)

1.3.3 Battlefield: In favor, or against laboratory?

The most common critiques of experiments are very often interconnected with external validity. In this section, I present the most common objectives that have been put forward against laboratory experiments, sometimes in order to favour field experiments, sometimes in order to give some thoughts on the problems the whole sector of experimentation faces (following discussion on all the objections is taken from Falk and Heckmann (2009)):

> **O**BJECTION 1:
> *Experiments in laboratory produce "unrealistic data". If one wants to run an experiment that is more likely to give a true picture of the reality, it is better to conduct a field experiment.*

Notion about unrealism is very often raised against lab while favoring field experimentation. Actually, laboratory versus field is not the question. Consider a so called "all causes model"[9]

$$Y = f(X_1, X_2, \dots, X_n) \tag{1}$$

where Y is an outcome of interest, and $X_1, \dots, X_n$ are all possible causes affecting Y. The real issue is the best method how to separate and identify

[9] All causes model is explained in Heckman (2008).

the causal effect of interest, i.e. an effect originated by varying only one X_i while the rest $\tilde{X} = X_{-i}$ stay fixed at the same level.

The effect of interest is not determined by variation of X_i only, but also by the level set for $\tilde{X}$. In the lab settings the researcher examines Y for certain values of $\tilde{X}$, e.g. mostly with undergraduates as a subject pool and under artificial institutions. In the "natural" settings (reference to the field experiments) she analyzes Y for another set of values $\tilde{X}'$, with field context in the subject pool, in the environment, etc. This evidence for Y may be different from one in the lab. However, if we are interested in how the outcome is determined under even different, third conditions $\tilde{X}''$ (the real world without experimenters' interferences), it is not really obvious which one of the settings, whether the lab or the field, is more informative.

Lab experiments are useful whenever tight control of $\tilde{X}$ is fundamental. They are able to implement various levels of $\tilde{X}$, such as different number of subjects, or different framework of the trade market, and by this method explore the robustness of the causal effect of X_i.

> OBJECTION 2:
> *Students are unrepresentative sample of the overall population; moreover, they lack experience.*

Comparing to adults, undergraduates as a subject pool have several advantages. First, they have lower opportunity costs, which implies smaller demands on budget size. Moreover, they are more flexible in their time, thus they are easier to be arranged a session.

As to the calls for representative evidence, when experiments are run in order to test theories, employing students does not bring a problem. Mostly, economic models are independent of assumption about subject pool.

For the purpose of exploring some particular aspects of behavior, students might be unrepresentative sample of a target population of interest, indeed. When subjects are confronted with settings of an experiment, they might lack experience with the particular market situations that experimenters intend to investigate. Hence, their behavior might be significantly different from what would be behavior of experienced individuals when put to the same situation. In such case, it is the turn to accede to artefactual field experiments, and to field experiments in general.

A different situation is when one wants to investigate how gaining experience and learning affects behavior, and how the dynamic of such process looks like. For this purpose, it is as appropriate to use students as it is to use any other subject pool.

> OBJECTION 3:
> *Self-selection into experiments may bias results.*

Participation in the experiments is voluntary and it can happened that only a certain type of people is selected instead of a representative sample of the overall population or of a target community. This is called self-selection. It is not necessarily a catastrophe. Self-selection can become a source of information on some part of subjects' utility. It is also possible to collect information on the socio-demographic characteristics of the participants to control for the selection, or to "explicitly study selection in a controlled way" (Falk and Heckmann, 2009).

2 A theory of games

When we say the word game, everybody imagines some activity played for fun, such as card games, board games, or matches in team sports like baseball, soccer, and others. What these activities have in common is that the fate of one player does not entirely depends on her own actions, but also it depends on what others', either opponents' or teammates', moves are. These games are just one part of strategic interactions which game theory analyzes. More importantly, game theory can be applied to situations we might encounter in real life. Bargaining over the wages, auctions, lobbying, election, or even evolution path of animals, these are few examples of other side of game theory.

> *"The essence of strategic thinking is the ability to put yourself in other's shoes and trying to figure out what they will do."*
>
> Polak (2007a)

2.1 A bit of terminology

If we want to play any game, first we have to set its rules. The same applies to the models game theory is dealing with. It is necessary to state terms and clarify the rules, therefore this chapter is devoted to the definitions (all the definition are taken from Rasmusen (2007), and Carmichael (2005)):

Def.: Components of a game:

1. *players*: i, j = individuals who make decision
2. *action* or *move* by a player i: a_i = choice which the player i can undertake (e.g. in a game rock-paper-scissors one action is playing paper)

3. *action set* of a player i: $A_i = \{a_i\}$ = all actions available to the player i {e.g. playing scissors; playing paper; playing rock}

4. particular *strategy* of a player i: s_i = rule (or a function) of the player i which action to choose at each instant of the game (e.g. to play paper with probability 1)

5. particular strategies of all players except of a player i: s_{-i}

6. *set of strategies* of a player i: S_i = set of all possible strategies of the player i

7. *strategy profile/list*: $s = (s_1, \ldots, s_n) = (s_i, s_{-i})$ = particular combination of strategies of each player, those which were actually played out by each player in the game

8. *payoff*: $\pi_i(s_1, \ldots, s_i, \ldots, s_n) = \pi_i(s) = \pi_i(s_i, s_{-i})$. This function expresses either an utility function or an expected utility function. Payoffs are very important as can be expressed by the name of Joe Jackson's song *"You can't get what you want till you know what you want"*.

9. *outcome* of a game: set of elements the modelers pick from the values of actions, payoffs, and other variables after the game is played out

Def.: Rationality

Rational player is someone who is maximizing her utility/payoff in the game. In other words, given two alternatives to choose between, a rational player picks the most preferred.

Def.: Common knowledge

We say that information is *common knowledge* if all players know it, if every player knows that all players know it, if every player knows that every player knows that all players know it, and so on to infinity.

Def.: (In)complete information

In the game of *incomplete information*, the 'nature' (or 'chance'), a pseudo-player who moves in a random way with specified probabilities, takes an action that is not clearly observed by at least one of the players. Otherwise we say the game is of *complete information*.

Def.: Pure strategy

Pure strategy is a strategy that predetermines specific actions that a player will take without means of random devices.

Def.: Strictly dominated strategy

Player i's strategy s_i' is *strictly dominated by* player i's strategy s_i if

$$\pi_i(s_i, s_{-i}) > \pi_i(s_i', s_{-i}) \ \forall s_{-i}. \tag{2}$$

In other words it means the strategy s_i always yields greater payoff of the player i than her payoff yielded by the strategy s_i' regardless of what others do. One conclusion can be driven from it: a payoff maximizing player will never play a strategy that is strictly dominated by another one. (Polak, 2007b)

Def.: Weakly dominated strategy

Player i's strategy s_i' is *weakly dominated by* player i's strategy s_i if

$$\pi_i(s_i, s_{-i}) \geqslant \pi_i(s_i', s_{-i}) \ \forall s_{-i} \,\&\, \exists \tilde{s}_{-i} \colon \pi_i(s_i, \tilde{s}_{-i}) > \pi_i(s_i', \tilde{s}_{-i}). \tag{3}$$

This happens if there is a strategy s_i that never yields worse payoff than a payoff yielded by s_i' for any combination of strategies played by other players. Moreover, there is at least one $\tilde{s}_{-i}$ such that for a player i the strategy s_i is strictly better than the strategy s_i'.

> ***Def.: Best response***
> Player i's *best response* to a particular combination s_{-i} chosen by her opponents is a strategy s_i^* yielding him the greatest payoff possible, i.e.,
>
> $$\pi_i(s_i^*, s_{-i}) \geqslant \pi_i(s_i', s_{-i}) \ \forall s_i' \in S_i. \tag{4}$$

The best response is clearly connected with rationalizing, it is the best move you can follow responding to your beliefs about what others are playing. The strategy s_i^* solves a maximization problem

$$\max_{s_i} \pi_i(s_i, s_{-i}). \tag{5}$$

The definition of strictly dominated strategy implies that such a strategy can never be the best response to any s_{-i}.

> ***Def.: Nash equilibrium (NE)***
> A strategy profile $s^* = (s_1^*, \ldots, s_n^*)$ is *Nash equilibrium* if a strategy s_i^* of each player is the best response to the other players' combination of strategies s_{-i}^*. Mathematically,
>
> $$\forall i \in \{1, \ldots, n\}\colon \pi_i(s_i^*, s_{-i}^*) \geqslant \pi_i(s_i', s_{-i}^*) \ \forall s_i' \in S_i. \tag{6}$$

NE is one of the most applied concepts used in game theory. If the players are in NE neither of them has any incentive to deviate from her position (given that she knows that others do not deviate). In a graph where the axes represent strategies of each player, NE is an interception of their best responses. Nevertheless, one should not come to the conclusion that people necessarily play NE. There can be more than just one NE and then a coordination problem can emerge, because players do not know which equilibrium to choose. In that case communication can help. If games are repeated it can lead to convergence to one of the equilibria.

2.2 Classification of games[10]

Cooperative and noncooperative games is the basic classification provided by game theory. The essence that distinguishes the two cases is the possibility of making binding commitments. In a *cooperative* game, the players can communicate among themselves and, moreover, they can reach an agreement which they are able to bind themselves to. One such agreement may be offering and accepting side-payments as a compensation for having reached an outcome which otherwise would not have been interesting for one (or more) of the players. Cooperative games can ergo be further divided into two groups, one, where side-payments are allowed, the other, where they are not.

In *noncooperative* games, commitments are never binding. Binmore (1990) adopts the word *contest* in a relation with noncooperative games from which all informal communication is excluded.

Other distinction of games is that between static games and dynamic games. In a *static* game, the players move simultaneously. A simple definition follows: *dynamic* games are games that are not static. Here players move in some sort of order. An important feature of the sequence of the moves is not the time that one player moves before the other, but the information — the second player can observe what was the first player's move **before** she makes her own move, and the first player knows that this would be the case.

Games can be written either in a normal-form or in an extensive-form. The *normal-form,* also called a *strategic-form*, is represented by a payoff

[10] Taken from Binmore (1990).

matrix, that combines action sets of all players.The *extensive-form* is represented by a game tree. It starts at a single point — a start node — from which there run several branches (how many exactly depends on the number of possible moves for the first player at that moment). These branches are closed with other nodes — decision nodes — at which it is the other player's turn, then it continues similarly. Finally, when no player is to move any further, the end nodes show the players' payoffs achieved if they have chosen the particular path ending there.

Two more definitions go hand in hand with the extensive-form game (taken from Rasmusen (2007), and Carmichael (2005)):

> ***Def.: Information set***
> Player i's *information set* at any moment of the game is a set of nodes (belonging to one player) on different branches of a game tree that the player knows may be the actual nodes, but which one of them it is exactly she cannot directly observe.

> ***Def.: Subgame***
> A *subgame* is a part of a game, that begins at a node which is a singleton in every player's information set (there is no uncertainty), and ends at terminal nodes that are the node's successors (together with the correspondent payoffs).

2.3 Refinements of equilibria

In a case that there are multiple Nash equilibria, one concern to theorists is to exclude those that are less likely to be played, for example because they may rely on believing in noncredible threats.

A subgame-perfect Nash equilibrium is an example of such a refinement of a Nash equilibrium in dynamic games.

Def.: Subgame-perfect Nash equilibrium[11]
The strategy combination is called a *subgame-perfect Nash equilibrium* if it satisfies:

1. It is a Nash equilibrium for the entire game; and
2. It yields a Nash equilibrium in every subgame, no matter whether these subgames are reached in equilibrium or not.

In the case of finite games with every player's information set as a singleton, a common practice how to find such an equilibrium is by means of *backward induction* which is built on anticipation — the first players anticipates how others will later act. The motto says: "Look forward, work backward" (Polak, 2007c). We look at the last decision node of the game and work out what action should the last player take in order to maximize her payoff. Given that, we move on to the first to last decision node and determine which action the rational player, who is to move at that node, should take, and so on until we reach the beginning of a game tree. The strategy combinations that are not eliminated by this process are then all subgame-perfect equilibria.

[11] Taken from Rasmusen (2007), and Carmichael (2005).

3 The ultimatum game: Basic information

Cooperative games provide us with a very interesting field, namely with bargaining games. Bargaining can be seen as a dispute over how to divide a surplus generated from a transaction among involved parties. The interesting thing is that, to some degree, human behavior is governed by social norms, among them by fairness considerations. What happens when these norms crash with pure self-interest maximization? Will strategic behavior come out as a winner, or will it be the fair play what beats the selfish creature in us?

One such game, in which social norms and self-interest maximization go in opposite direction, is the ultimatum game.

> THE ULTIMATUM GAME:
> *The ultimatum game is a two-person sequential game. The game consists in how to divide an amount of money* π[12] *among two players. The first player, the proposer, makes an offer* $\varepsilon \in \langle 0; \pi \rangle$ *to the second player, the responder. If the responder accepts her offer, the money will be divided as suggested, meaning that the responder gets amount* ε *and the proposer gets* $(\pi - \varepsilon)$. *On the contrary, if the responder refuses the offer, neither the proposer nor the responder gets anything.*

According to the theory, every accepted offer is a Nash equilibrium, thus there is an extensive number of them. Nonetheless, the game contains only two candidates for a subgame-perfect Nash equilibrium. Assuming that each player is rational, together with an additional assumption that the responder with certainty (!) accepts a zero offer (despite being indifferent between rejecting an accepting it), then the only subgame-perfect equilibrium is to offer zero, which is accepted. However, this equilibrium counts with a weakly

[12] It is called a “pie”.

dominated strategy. Therefore, if the probability of acceptance of zero offer becomes less than one, the previous equilibrium is replaced by a new subgame-perfect Nash equilibrium, which is to offer the smallest possible positive amount of money ε (e.g. the smallest denomination within a currency system, say, one penny), that is being accepted; by accepting, the responder is always better-off than if she rejects and gets nothing.

The ultimatum game was first performed by Güth et al. (1982). The results deviated greatly from predictions of the theory. The average share the first player demanded for herself was less than 70%, both for the "naive" players who played the game for the first time, and for the "experienced" who repeated the game a week later. Only two naive players (out of 21) made their offer in accordance with the subgame-perfection when they offered zero, but one such offer was rejected. As the players gained experience, only one player offering virtually zero remained, but his offer was rejected as well. Among naive players the modal offer was an equal split, which was proposed by one third of the players, and the modal offer was one quarter of the pie for the experienced players, proposed by 19% of all the players, which is still far from theoretical predictions. Moreover, all experienced players offered positive amounts, but 29% of the proposals was declined, which contradicts the assumption that the second player prefer receiving whatever positive amount to receiving zero.

Since Güth et al. (1982) presented the game, which has earned a label of anomaly[13], numerous subsequent experiments questioning the robustness of the findings have been conducted.

3.1 Basic results

In the standard ultimatum experiment, each subject is randomly assigned a particular role and then randomly matched against an anonymous opponent,

[13] "An empirical result qualifies as anomaly if it is difficult to "rationalize," or if implausible assumptions are necessary to explain it within the paradigm." (Thaler, 1988)

unless it is of target interest to investigate the effect of earning the right to move first. There are two screenplays how the game can be played. The first is a so-called *direct method*: the proposer states an offer, the responder sees this proposal and then she rejects it or accepts it. The alternative method, the *strategy method*, explores a minimum acceptance threshold of the responders. When the proposer states her offer, the responder simultaneously states a minimal offer she is willing to accept, and only after that she sees the actual offer. It is rejected if found below the stated threshold, otherwise it is accepted. This method is called the strategy method, because it explores the whole strategy set of the responder, not only her reaction to one particular offer. Especially, it is useful for exploring her reaction to low offers that are seldom observed.

The typical ultimatum game is a single-shot game, i.e. played only once. An extension to this version is when the game is repeated for several rounds, and each round, subjects are randomly rematched with a new anonymous opponent whom they have not faced before yet. Behavior in particular rounds is then compared to each other to see whether learning takes place and the deviations from the theory is an artifact of the inexperience of the subjects with the task. Indeed, as subjects gain experience there are observed some slight movements toward lowering the offers. The role of experience is discussed in more detail in the section 4.2.

The framing of the game can vary as well. The standard way of presenting the problem to subjects is the "dividing the cake" problem. In other words, the players are told that they have some certain amount of the reward, and that they have to agree on how to divide it, given the rules of the ultimatum game. Alternatively, it is presented as an "exchange" problem. In such a case, the first player is called the seller, the second player is called the buyer, and they have to agree on the price, which the buyer can buy or not. If it is bought, the value that remains when the price is deducted from the given amount (the size of stake for the treatment) is the buyer's outcome, whereas the seller's outcome is the value of the price; otherwise both players receive zero. There is an evidence that extending the task with the buyer-seller context affects subjects' behavior, the first players offer significantly less while the rejection does not increase (see Hoffman et al. (1994), discussed in section 5.1 at page 59).

Tables 1 and 2 show the basic statistics of some of the experiments on the ultimatum game that are discussed more thoroughly in following parts of this work. Table 1 provides summary statistics for each of the papers. Each study is divided into its particular treatments, and for each treatment the number of pairs of players, the size of the reward to be divided (Stake), the mean offer, the median offer, the mode offer together with the fraction of subjects who proposed such offer[14], the minimal and the maximal offer, the overall rejection rate, and the fraction of people from the sample who offered less than 20% of the pie (Offers < 0.2)[15] together with the conditional rejection rate with respect to these offers (Rej. r. of off. < 0.2) are listed. Table 2 shows the relative frequency of offers (O)[16] made in a particular interval together with the conditional rejection rate (RR) with respect to these offers, and it shows the average offer and the overall rejection rate. The results are taken from studies implementing the direct method and using inexperienced subjects (i.e. single-shot ultimatum), unless noted otherwise.

Despite the variations in the settings, the behavioral pattern is very similar in most of the treatments. The modal offer is mostly 50%, less often 40% or 45%. Among the provided studies, there are only 3 treatments whose modal offer dropped below 40%. In particular, it happened in study of Hoff-

[14] If there are listed two numbers, both are the modal offers. The percentage then shows a fraction of subjects (playing the modal offer) for one of the modal offers only.

[15] The symbol $\sim$ before zero means that there was (at least) one player offering less than 20% of the pie, but she did not constitute either a percentage of the total of the playing pairs.

[16] Again, the symbol $\sim$ before zero means that there was (at least) one player proposing an offer in the particular interval, but she did not constitute either a percentage of the total of the playing pairs.

man et al. (1994), in treatments employing the seller-buyer framing to the game (30% and 20%), and in the study of Güth et al. (1982) for experienced players (25%). The median offer lies mostly in the closed interval between 40 and 50% as well, and the mean offer lies in the closed interval between 35 and 45%. Offers of more than 50%, or less or equal to 10% are rarely observed. Offers less than 20% are made by less than 20% of the pairs, but being rejected with probability higher than 1/2. The rejection rate decreases as the offers increase, and finally, the offers higher than 40% are seldom rejected.

Tab. 1: *Summary statistics*

Treatment	**No. of pairs**	**Stake**	**Mean offer**	**Median offer**	**Mode offer (% of sample)**	**Min. offer**	**Max. offer**	**Reject. rate**	**Offers < 0.2**	**Rej. r. of off. < 0.2**	**Comments**
Cameron (1999)											
low stake (LS)	91	5,000 Rp	0.43	0.40	0.50; 0.40 (31%)	0.00	0.85	0.19	15%	0.64	Rp = Indonesian Rupiah;
medium stake (MS)	35	40,000 Rp	0.44	0.50	0.50 (57%)	0.25	0.75	0.09	0%		problem subjects
high stake (HS)	34	200,000 Rp	0.41	0.45	0.50 (47%)	0.00	0.50	0.12	9%	1.00	excluded; Round 1: LS
Croson (1996)											Round 2: MS or HS
%I	28	$10	0.42	0.40	0.50 (39%)	0.10	1.00	0.21	15%		offers made in
%U	29	$10	0.39	0.50	0.50 (59%)	0.05	0.50	0.03	20%		percentage, or in
$I	28	$10	0.45	0.50	0.50 (57%)	0.10	0.60	0.07	8%		amount of $ (%/$);
$U	26	$10	0.36	0.39	0.50 (31%)	0.10	0.70	0.04	27%		responder un/informed about pie size (U/I)
Fajfar (2006)											
without information	47	2 credits	0.45	0.50	0.50 (62%)	0.10	0.55	0.06	2%	1.00	stake: extra
with information	41	2 credits	0.46	0.50	0.50 (76%)	0.25	0.50	0.00	0%		mid-term credits
Forsythe et al. (1994)											
April UG	20	$5	0.44	0.50	0.50 (55%)	0.20	0.60	0.15	15%	0.67	UG: Ultimatum G.;
April D	21	$5	0.19	0.20	0.00 (43%)	0.00	0.60		71%		D: Dictator G.
September UG	23	$5	0.47	0.50	0.50 (52 %)	0.20	0.60	0.00	4%	0.00	
September D	24	$5	0.25	0.20	0.00; 0.20 (29%)	0.00	0.60		58%		
September UG	24	$10	0.47	0.50	0.50 (71%)	0.20	0.60	0.04	4%	0.00	
September D	24	$10	0.23	0.25	0.30 (29%)	0.00	0.50		50%		

Source: Author

Tab. 1 *(continued)*

Treatment	**No. of pairs**	**Stake**	**Mean offer**	**Median offer**	**Mode offer (% of sample)**	**Min. offer**	**Max. offer**	**Reject. rate**	**Offers < 0.2**	**Rej. r. of off. < 0.2**	**Comments**
Güth et al. (1982)											
naive	21	4–10 DM	0.35	0.40	0.50 (33%)	0.00	0.50	0.10	24%	0.40	DM = Deutsche Mark;
experienced	21	4–10 DM	0.31	0.30	0.25 (19%)	ε	0.50	0.29	24%	0.60	experienced: 9–12 rounds, $\varepsilon = \frac{0.01}{5}$
Hoffman et al. (1994)											
UG, random, divide	24	$10	0.43	0.40	0.50 (71%)	0.30	0.50	0.08	0%		UG: Ultimatum G.;
UG, contest, divide	24	$10	0.36	0.40	0.40 (50%)	0.20	0.50	0.00	4%	0.00	D: Dictator G.;
UG, random, exchange	24	$10	0.37	0.35	0.30 (38%)	0.20	0.70	0.08	13%	0.00	exchange:
UG, contest, exchange	24	$10	0.30	0.30	0.20 (33%)	0.10	0.50	0.13	42%	0.30	buyer/seller
D, random, exchange	24	$10	0.27	0.30	0.30 (42%)	0.00	0.50		29%		instructions;
D, contest, exchange	24	$10	0.13	0.10	0.00 (42%)	0.00	0.40		79%		divide:
D, random, divide, DB 1	36	$10	0.09	0.00	0.00 (64%)	0.00	0.90		89%		splitting-a-pie
D, random, divide, DB 2	41	$10	0.10	0.00	0.00 (59%)	0.00	0.50		80%		instructions;
											DB: double blind
Lusk and Hudson (2004)											
without cheap talk	56	$10	0.49	0.50	0.50 (77%)	0.35	0.60	0.00	0%		
with cheap talk	27	$10	0.45	0.50	0.50 (63%)	0.20	0.50	0.00	0%		
Oosterbeek et al. (2004)											
Africa			0.40			0.14					
Asia			0.43			0.19					
Europe East			0.41			0.19					
Europe West			0.37			0.23					
Israel			0.40			0.22					
South America			0.35			0.05					
US East			0.40			0.17					
US West			0.43			0.09					

Source: Author

Tab. 1 *(continued)*

Treatment	**No. of pairs**	**Stake**	**Mean offer**	**Median offer**	**Mode offer (% of sample)**	**Min. offer**	**Max. offer**	**Reject. rate**	**Offers < 0.2**	**Rej. r. of off. < 0.2**	**Comments**
Roth et al. (1991)											
US Round 1	27	$10	0.45	0.50	0.50 (52%)	0.20	0.63	0.22	4%	0.00	YUD = Yugoslav
US Round 10	27	$10	0.44	0.48	0.50 (48%)	0.20	0.50	0.19	4%	1.00	Dinar;
US Round 1	10	$30	0.50	0.50	0.50 (50%)	0.45	0.55	0.20	0%		IS = Israeli Shekel
US Round 10	10	$30	0.46	0.49	0.50 (40%)	0.30	0.53	0.10	0%		stake: in terms of
Yugoslavia Round 1	30	400,000 YUD	0.44	0.45	0.50 (40%)	0.15	0.55	0.27	3%	1.00	1,000 tokens
Yugoslavia Round 10	30	400,000 YUD	0.44	0.45	0.50 (30%)	0.30	0.50	0.23	0%		
Japan Round 1	29	2,000 Yen	0.45	0.50	0.50 (34%)	0.05	1.00	0.24	14%	0.50	
Japan Round 10	29	2,000 Yen	0.40	0.40	0.40; 0.45 (24%)	0.25	0.50	0.14	0%		
Israel Round 1	30	20 IS	0.36	0.40	0.50 (27%)	0.05	0.55	0.27	23%	0.71	
Israel Round 10	30	20 IS	0.33	0.35	0.40 (33%)	0.10	0.50	0.13	17%	0.20	
Slonim and Roth (1998)											
low stakes (LS)	240	60 SK	0.45	0.47	0.50 (28%)	0.20	0.65	0.17	~0%	1.00	SK = Slovak Crown
medium stakes (MS)	330	300 SK	0.42	0.45	0.50 (22%)	0.05	0.70	0.12	3%	0.82	aggregated across
high stakes (HS)	250	1,500 SK	0.43	0.41	0.50 (30%)	0.10	0.80	0.08	2%	0.75	all rounds
LS Round 1	24	60 SK	0.45	0.47	0.50 (38%)	0.35	0.52	0.13	0%		
LS Round 10	24	60 SK	0.44	0.44	0.40 (21%)	0.34	0.55	0.08	0%		
MS Round 1	33	300 SK	0.43	0.48	0.50 (33%)	0.05	0.70	0.03	3%	1.00	
MS Round 10	33	300 SK	0.41	0.43	0.50 (18%)	0.05	0.55	0.18	3%	1.00	
HS Round 1	25	1,500 SK	0.43	0.44	0.50 (28%)	0.10	0.80	0.16	4%	1.00	
HS Round 10	25	1,500 SK	0.40	0.40	0.50 (32%)	0.17	0.50	0.08	4%	1.00	

Source: Author

Tab. 2: *Frequencies of offers with conditional reject. rate*

Treatment	***No. of pairs***		***Frequency of offers (O) — percent offered/Conditional rejection rate (RR)***											***Mean off./ Rej. rate***
			0	⟨0, 10⟩	⟨10, 20⟩	⟨20, 30⟩	⟨30, 40⟩	⟨40, 50⟩	⟨50, 60⟩	⟨60, 70⟩	⟨70, 80⟩	⟨80, 90⟩	⟨90, 100⟩	
Cameron (1999)														
low stake	91	O	0.01	0.02	0.12	0.05	0.32	0.34	0.04	0.03	0.04	0.01		0.43
		RR	1.00	0.50	0.64	0.20	0.21	0.03	0.00	0.00	0.00	0.00		0.19
medium stake	35	O				0.14	0.17	0.66			0.03			0.44
		RR				0.40	0.17	0.00			0.0			0.09
high stake	32	O	0.03	0.03	0.03	0.06	0.24	0.62						0.41
		RR	1.00	1.00	1.00	0.00	0.00	0.05						0.12
Croson (1996)														
%I	28	O		0.04	0.11	0.14	0.25	0.39	0.04				0.04	0.42
%U	29	O		0.10	0.10	0.10	0.10	0.59						0.39
$I	28	O		0.04	0.04	0.04	0.25	0.57	0.07					0.45
$U	26	O		0.12	0.15	0.15	0.23	0.31		0.04				0.36
Fajfar (2006)														
without information	47	O		0.02		0.06	0.15	0.75	0.02					0.45
		RR		1.00		0.67	0.00	0.00	0.00					0.06
with information	41	O				0.09	0.15	0.76						0.46
		RR				0.00	0.00	0.00						0.00
Forsythe et al. (1994)														
April UG	20	O			0.15		0.20	0.55	0.10					0.44
		RR			0.67		0.25	0.00	0.00					0.15
April D	21	O	0.43		0.29	0.05	0.05	0.14	0.05					0.19
September UG ($10)	23	O			0.04	0.04	0.26	0.52	0.13					0.47
		RR			0.00	0.00	0.00	0.00	0.00					0.00
September D ($10)	24	O	0.29		0.29	0.04	0.13	0.15	0.04					0.25

Source: Author

Tab. 2: (continued)

Treatment	***No. of pairs***		***Frequency of offers (O) — percent offered/Conditional rejection rate (RR)***											***Mean off./ Rej. rate***
			0	⟨0, 10⟩	⟨10, 20⟩	⟨20, 30⟩	⟨30, 40⟩	⟨40, 50⟩	⟨50, 60⟩	⟨60, 70⟩	⟨70, 80⟩	⟨80, 90⟩	⟨90, 100⟩	
Forsythe et al. (1994)	(continued)													
September UG ($30)	24	O			0.04	0.04	0.17	0.71	0.04					0.47
		RR			0.00	0.00	0.25	0.00	0.00					0.04
September D ($30)	24	O	0.21	0.17	0.13	0.29		0.21						0.23
Güth et al. (1982)														
naive	21	O	0.10		0.14	0.10	0.24	0.43						0.35
		RR	0.50		0.33	0.00	0.00	0.00						0.10
experienced	21	O	0.00	0.05	0.19	0.29	0.24	0.24						0.31
		RR		1.00	0.50	0.50	0.00	0.20						0.29
Hoffman et al. (1994)														
UG, random, divide	24	O				0.17	0.38	0.46						0.43
		RR				0.25	0.11	0.00						0.08
UG, contest, divide	24	O			0.04	0.38	0.50	0.08						0.36
		RR			0.00	0.00	0.00	0.00						0.00
UG, random, exchange	24	O			0.13	0.38	0.29	0.17		0.04				0.37
		RR			0.00	0.22	0.00	0.00		0.00				0.08
UG, contest, exchange	24	O		0.08	0.33	0.21	0.29	0.08						0.30
		RR		0.00	0.38	0.00	0.00	0.00						0.13
D, random, exchange	24	O	0.21	0.04	0.04	0.42	0.17	0.13						0.27
D, contest, exchange	24	O	0.42	0.17	0.21	0.17	0.04							0.13
D, random, divide, DB 1	36	O	0.64	0.19	0.06	0.03		0.06				0.03		0.09
D, random, divide, DB2	41	O	0.59	0.20	0.02	0.07	0.02	0.10						0.10

Source: Author

Tab. 2: *(continued)*

Treatment	***No. of pairs***		***Frequency of offers (O) — percent offered/Conditional rejection rate (RR)***											***Mean off./ Rrej. rate***
			⟨0, 10)	⟨10, 20)	⟨20, 30)	⟨30, 40)	⟨40, 50)	⟨50, 60)	⟨60, 70)	⟨70, 80)	⟨80, 90)	⟨90, 100)	100	
Lusk and Hudson (2004)	(note: semi-closed intervals changed)													
without cheap talk	56	O				0.02	0.16	0.80	0.02					0.49
		RR				0.00	0.00	0.00	0.00					0.00
with cheap talk	27	O			0.04	0.15	0.18	0.63						0.45
		RR			0.00	0.00	0.00	0.00						0.00
			0	⟨0, 10⟩	⟨10, 20⟩	⟨20, 30⟩	⟨30, 40⟩	⟨40, 50⟩	⟨50, 60⟩	⟨60, 70⟩	⟨70, 80⟩	⟨80, 90⟩	⟨90, 100⟩	
Roth et al. (1991)														
US Round 1 ($10)	27	O			0.04	0.11	0.22	0.56	0.04	0.04				0.45
		RR			0.00	0.67	0.17	0.13	0.00	1.00				0.22
US Round 10 ($10)	27	O			0.04		0.33	0.63						0.44
		RR			1.00		0.22	0.12						0.19
US Round 1 ($30)	10	O						0.80	0.20					0.50
		RR						0.25	0.00					0.20
US Round 10 ($30)	10	O				0.10		0.80	0.10					0.46
		RR				0.00		0.13	0.00					0.10
Yugosl. Round 1	30	O			0.03	0.07	0.13	0.73	0.03					0.44
		RR			1.00	0.50	0.25	0.23	0.00					0.27
Yugosl. Round 10	30	O				0.03	0.27	0.70						0.44
		RR				1.00	0.63	0.05						0.23
Japan Round 1	29	O		0.07	0.07	0.10	0.14	0.41	0.07	0.07			0.07	0.45
		RR		0.50	0.50	0.67	0.25	0.08	0.50	0.00			0.00	0.24
Japan Round 10	29	O				0.17	0.34	0.48						0.40
		RR				0.20	0.10	0.14						0.14
Israel Round 1	30	O		0.17	0.07	0.10	0.20	0.43	0.03					0.36
		RR		0.80	0.50	0.33	0.00	0.15	0.00					0.27
Israel Round 10	30	O		0.03	0.13	0.20	0.57	0.07						0.33
		RR		0.00	0.25	0.17	0.12	0.00						0.13

Source: Author

Tab. 2: *(continued)*

Treatment	***No. of pairs***		***Frequency of offers (O) — percent offered/Conditional rejection rate (RR)***											***Mean off./ Rej. rate***
			0	⟨0, 10⟩	⟨10, 20⟩	⟨20, 30⟩	⟨30, 40⟩	⟨40, 50⟩	⟨50, 60⟩	⟨60, 70⟩	⟨70, 80⟩	⟨80, 90⟩	⟨90, 100⟩	
Slonim and Roth (1991)														
low stakes (LS)	240	O			∼0.00	0.04	0.28	0.61	0.06	∼0.00				0.45
		RR			1.00	0.89	0.34	0.06	0.07	0.00				0.17
medium stakes (MS)	330	O		0.03	∼0.00	0.07	0.26	0.57	0.05	0.01				0.42
		RR		0.82	0.00	0.26	0.15	0.05	0.00	0.25				0.12
high stakes (HS)	250	O		0.01	0.06	0.06	0.33	0.44	0.03	0.01	0.03			0.43
		RR		0.50	0.60	0.31	0.05	0.01	0.00	0.00	0.00			0.08
LS Round 1	24	O					0.29	0.67	0.04					0.45
		RR					0.29	0.06	0.00					0.13
LS Round 10	24	O					0.38	0.50	0.13					0.44
		RR					0.22	0.00	0.00					0.08
MS Round 1	33	O		0.03		0.03	0.15	0.73	0.03	0.03				0.43
		RR		1.00		0.00	0.00	0.00	0.00	0.00				0.03
MS Round 10	33	O		0.03		0.06	0.33	0.55	0.03					0.41
		RR		1.00		1.00	0.27	0.00	0.00					0.18
HS Round 1	25	O		0.04	0.04	0.12	0.16	0.56			0.08			0.43
		RR		1.00	1.00	0.33	0.25	0.00			0.00			0.16
HS Round 10	25	O			0.08		0.52	0.40						0.40
		RR			0.50		0.00	0.10						0.08

Source: Author

4 The ultimatum game: Robustness of the results

4.1 Stake size

Experiments that focus on changes in players' behavior as a result of changes in the pie size come against an ubiquitous skepticism that stakes (i.e. the size of reward) in experiments are too small. The straightforward way how to discover whether a pattern changes as the stakes increase is to run an experiment containing a treatment with "low" stakes and a treatment, in which those stakes are multiplied by a factor greater than one, and then compare the findings. One way is then by means of the direct method. The disadvantage of this method is that one only knows the reaction r_2 of the player 2 to a specific demand ε', $r_2(\varepsilon')$, but not the whole function $r_2(\varepsilon)$, $\varepsilon \in \langle 0, \pi \rangle$. Therefore, if proposer's behavior does not change, by this method one might not be able to detect changes in responder's behavior such as willingness to accept smaller offers, in relative terms. Therefore, some researchers implemented the strategy method to investigate the stake effect instead.

Hoffman et al. (1996), Slonim and Roth (1998), or Cameron (1999) represent the first mentioned approach. Neither of the paper provides any evidence that the deviations of experimental results from game-theoretic predictions are an artifact of small stakes. Except from Hoffman et al. (1996) the experiments were held in countries where the wage levels were relatively low, so it was possible to raise financial incentives and still remained within a given budget — Cameron (1999) came to Indonesia, Slonim and Roth (1998) to the Slovak Republic. The largest stake in Indonesia corresponded to about three times the average monthly expenditure of the participants, the counterpart in Slovakia was approximately 62,5 hours of the average wages.

Common conclusion from all these experiments is that, as the stakes increase, there is no statistically significant difference in offer distribution (for inexperienced subjects who play the game for the first time), but the responders reject offers less often. For example, in the three treatments — low stake (LS), medium stake (MS), high stake (HS) — Cameron (1999) has conducted, the mean offer remained within 3%: the lowest mean offer was 41% in the HS, and the highest was 44% in the MS. Furthermore, the modal offer - an equal split - was the same for all the three treatments. If one looks at the rejection rate, it goes from 0.19 in the LS to 0.9 in the MS, and then to 0.12 in the HS.

Munier and Zaharia (2002), who employ the strategy method, agree on this findings about responders' behavior when they notice in their experiment a substantive decrease in the responder's acceptance thresholds with an increase of the pie size; the number of the responders playing the equilibrium strategy (i.e. setting the threshold at zero, or at the smallest unit, in which the offers can be made, respectively) doubled when facing the high stake treatment. However, game-theoretic predictions still do not perform well when they try to describe the proposer's behavior.[17]

4.2 Learning and the role of experience

When subjects come into the laboratory and the game is presented to them, the situation they are faced with is completely new to them. Therefore, another aspect that is examined is whether their play changes with getting experienced.

4.2.1 Learning from one's own history

The natural way how one gains experience is through her own actions, if the game is repeated. It has become a common practice to let the subjects play the game more than one round, each time with new randomly matched

[17] Further studies using the strategy method are, for example, Straub and Murnighan (1995) who variates the amount of information provided to the subjects, or Tompkinson and Bethwaite (1995) who use a questionnaire with hypothetical stakes rather than monetary incentives.

opponent. Roth et al. (1991) employed such method when they examined the ultimatum game in four different countries, the United States, Yugoslavia, Japan, and Israel. Each session lasted ten rounds. In all countries, subjects lowered their offers over time while the rejection rate decreased, but the changes were very slight, esp. in the US and Yugoslavia. Subjects in each country began with the modal offer of 50% in round 1. By round 10, only the modal offer of the US and Yugoslavia remained at the same value, in Israel and Japan it changed to 40% (in Japan there were two modal offers, 40% and 45%). Subjects' behavior of Israelis changed the most. For example, the proportion of subjects offering more than 40% of the pie decreased from 46% in round 1 to 7% in round 10. The different learning path is likely to be affected by the different acceptance rates in each subject pool. The authors compared the acceptance rates for the proposals that were observed at least ten times, and the rates for Israel were the highest, i.e. for a given proposal the probability of acceptance is higher in Israel than in any other country. The modal offers detected in each country in round ten corresponded to the proposed share that maximized the proposer's average earnings in these countries (given the particular acceptance rates). Therefore, as they gain experience, the proposers seem to adjust their behavior in a way consistent with simple income-maximization, whereas it does not seem to be true for the responders who constantly reject some offers.

Slonim and Roth (1998) explored what the effect of learning is under different size of stake. Within each different treatment (low stake, medium stake, high stake) they ran 10 rounds. They found out that the behavior of inexperienced players is alike across stakes, but becomes to differ with repetitions of the game. In particular, the proposers under the medium stake treatment lowered their offers the most, those under the highest stake treatment lowered them the second most, whereas the offers under the smallest stake treatment decreased the least. However, the changes were very slight.

Slonim and Roth (1998) also ran computerized simulations of adaptive learning model of Roth and Erev (1995), which is discussed later in section 6.2 at page 69. This model assumes each player to have some initial propensity to play each of a finite number of pure strategies. This propensity is updated whenever the particular strategy is played by adding the value of payoff just received to the corresponding propensity. This propensity then determines the probability that a particular strategy will be played — the higher the propensity of the strategy relatively to the others, the greater the likelihood that it will be played. The authors ran 5,000 simulations using the initial propensities computed from the experimental data of first two rounds and the path successfully tracked the actual behavior observed in all three treatments, even though the behavior in simulations changed more slowly. Furthermore, they ran simulations using different initial propensities of the proposer, but using initial propensities of the responders computed from either the low stake, or the higher stake treatments. The results show that "no matter what the initial propensities of the proposers, the change in offers over time depend critically on the responders they played against" (Slonim and Roth, 1998), which is very similar conclusion implied in the findings of the study of Roth et al. (1991).

4.2.2 Learning from a history of previous experiment

Another manner of gaining experience with the task is to know how other people, who have already been put to the same situations, have reacted. Lusk and Hudson (2004) and Fajfar (2006) explored this issue in the single-shot ultimatum game when they apprised the subjects of results of the same experiment which they had already realized before. Lusk and Hudson (2004) compared two treatments, one without the cheap talk, the other with the cheap talk. In the cheap talk treatment, prior to the game, the subjects were given an information about what the theoretical solution of the game is together with some basic statistics compiled from a game already had been realized (but with subjects not being given any such additional information)

— these were the results of the treatment without the cheap talk. The treatment without the cheap talk was the standard ultimatum game. Comparing the results from both the treatments, subjects who were given the information offered significantly less than those who were not given it. For example, the average offer was 45%, and 49%, resp., even though the modal offer, 50%, was in both treatments the same. Further, the proportion of subjects offering less than 50% of the pie doubled to 37% in the cheap talk treatment. Despite different offer distribution, all offers in both treatments were accepted by all players, likely due to the provided cheap talk information.

On the other hand, Pablo Fajfar (2006) found no statistically significant difference between the ultimatum game where subjects were given an additional information and the ultimatum game where they were not given any such information. Looking at the proportion of players offering an equal split, it increased even more in the treatment with the information (from 62% to 76%). However, the experimental settings of Fajfar (2006) and Lusk and Hudson (2004) differ in many aspects. The main difference was that, in study of Fajfar (2006), the piece of information provided to subjects did not consist of the theoretical solution. Furthermore, subjects did not bargain over the monetary pie, they bargained over extra credits instead, that could help them to pass an exam. The roles were not assigned at random, but on the basis of how well the subjects scored in the mid-term test, those with better score became the proposers.

4.3 Culture

Roth et al.'s (1991) experimental study Roth et al. (1991) are the pioneers in investigating of cultural differences in ultimatum bargaining behavior. The goal of their work was not only to compare behavior in different subject pools (from Israel, Yugoslavia, Japan, and the US), but there was also a methodological goal to show an empirical way how to "deal with formidable problems of experimental design in multinational experiments" (Roth et al., 1991).

Language effects The goal is to make sure that systematic differences (if observed) between countries have not arisen because of the language in which the instructions were written — even approximate synonyms might have different connotations, so it is necessary to use the same words that have the same connotations when translating the instructions. The experiment of Roth et al. (1991) contained of two-person bargaining and multi-person market environments. The instructions used the same words for both of the environments, so if the effects exist, it should show up in both of the environments (which was not the case).[18]

Currency effects Different currencies have different numerical scales. To control for these differences, a pie was made in terms of 1,000 tokens and an exchange rate tokens/domestic currency was established and presented to subjects. Moreover, this exchange rate was chosen to give the same purchasing power on 1,000 tokens in each country.

Experimenter effects When sessions in each country are run by different experimenters, uncontrolled procedural or personal differences might be present. Each of the experimenter conducted at least one session in the US. If such effects existed they would be observed not only in comparisons between sessions in different countries, but also in comparisons between the US sessions.

Subjects' behavior did not converge to the perfect-equilibrium predictions in any country, and the authors observed only minor, but significant differences in proposers' behavior. In each country they ran 10 rounds of the game. The aggregated modal proposal across 10 rounds in US and Yugoslav subject

[18] Another common approach is *back translation*, in which

$$\text{original language} \xrightarrow{T_1} \text{second language} \xrightarrow{T_2} \text{original language}$$

where T_i is a translator, $T_1 \neq T_2$, so that the two versions in one language can be compared for differences.

pool was an equal split — 500 tokens, whereas this modal proposal in Japan and Israel was 400 tokens. The significant differences between the countries remained even when comparing results round by round, the differences even increased with the rounds (except for the US and Yugoslavia, between which the significant differences did not exist at any round). Despite the different offer distribution, the responders from subject pool facing the lower offers did not disagree more often than those from high-offer subject pool. In fact, the lowest rejection rate (0.27 in round 1 which changes to 0.13 in round 10) was observed in Israel where the offers were the lowest.[19]

Henrich et al. (2001): 15 small-scale societies Henrich et al. (2001) undertook an extensive multinational field experiment in which his team went to 12 countries on five continents (see Figure 1) where they recruited subjects from 15 small-scale societies[20]. As incentives, they used either pecuniary rewards, or barter goods. Subjects' behavior differ between the groups considerably, with the smallest mean offer of 26% of the pie among Peruvian Machiguenga tribe[21] to the largest mean offer of 57% among Indonesian Lamelara tribe. The rejection rates varied greatly as well between all the tribes, from 0.00 to 0.40. Unlike experiments conducted in industrial societies, where rejection of offers below 20% occurs with probability $p \in \langle 0.4, 0.6 \rangle$, the rejection was seldom observed among some groups. For example, there was only 1 rejection (out of 21 pairs of players) among subjects of Machiguenga. On the contrary, subjects recruited from tribes of Papua New Guinea (PNG)[22] rejected even some "hyper-fair" offers ($> 50\%$). The summary statistics can be found in Figure 1.

[19] For another experiment (from Russia and the US) in which the authors re-examine the data and extend an experimental design of Roth et al. (1991) by collecting information on socio-demographics of the participants see Botelho et al. (2001).

[20] The paper is revised in Henrich et al. (2005), where also some subsequent comments by other economists are to be found.

[21] For a detailed description of the part of the experiment taken in Peru among Machiguenga society see Henrich (2000).

[22] These are the Au and Gnau tribes In Table 3.

The authors discuss that behavior exhibited during the ultimatum game experiment is in accordance with the patterns of daily lives of the participants. For example, they link the PNG rejections to their culture of gift-giving, where accepting gifts comes with strings attached by committing one to return the favor when a donor asks for it. Similarly, Hadza tribe exhibited low-offer behavior (with mean offer of 33%) and high rejection rate, which might have reflected their unwilling process of sharing. While they preserve a tradition of vast meat sharing, many hunters seize the opportunity to avoid it whenever they can. When they share their meat, it is often thanks to the fear of potential consequences like informal social sanctions, ostracism, and others.

Tab. 3: *Ultimatum game in 15 small-scale societies - summary statistics*

Group	Mean	No. of Pairs	Percentage female	Stake	Mode (% of sample)[1]	Rejections	Low Rejections[2]
Lamalera[3]	0.57	19	55	10	0.50 (63%)	4/20 (sham)[4]	3/8 (sham)[4]
Aché	0.48	51	54	1	0.40 (22%)	0/51	0/2
Shona (resettled)	0.45	86	45	1	0.50 (69%)	6/86	4/7
Shona (all)	0.44	117	46	1	0.50 (65%)	9/118	6/13
Orma	0.44	56	38	1	0.50 (54%)	2/56	0/0
Au	0.43	30	48	1.4	0.3 (33%)	8/30	1/1
Achuar	0.43	14	50	1	0.50 (36%)	2/15[5]	1/3
Sangu (herders)	0.42	20	50	1	0.50 (40%)	1/20	1/1
Sangu (farmers)	0.41	20	50	1	0.50 (35%)	5/20	1/1
Sangu	0.41	40	50	1	0.50 (38%)	6/40	2/2
Shona (unresettled)	0.41	31	48	1	0.50 (55%)	3/31	2/6
Hadza (big camp)	0.40	26	50	3	0.50 (35%)	5/26	4/5
Gnau	0.38	25	46	1.4	0.4 (32%)	10/25	3/6
Tsimane	0.37	70	51	1.2	0.5/0.3 (44%)	0/70	0/5
Kazakh	0.36	10	45	8	0.38 (50%)	0/10	0/1
Torguud	0.35	10	50	8	0.25 (30%)	1/10	0/0
Mapuche	0.34	31	13	1	0.50/0.33 (42%)	2/31	2/12
Hadza (all camps)	0.33	55	50	3	0.20/0.50 (47%)	13/55	9/21
Hadza (small camp)	0.27	29	51	3	0.20 (38%)	8/29	5/16
Quichua	0.25	15	48	1	0.25 (47%)	0/14[5]	0/3
Machiguenga	0.26	21	19	2.3	0.15/0.25 (72%)	1	1/10

[1]If more than one mode is listed, the first number is the most popular offer, the second number is the second most popular, and so forth. The percentage in parentheses is the fraction of the sample at the mode(s). For example, for the Machiguenga 72% of the sample offered either 0.15 or 0.25.

[2]This is the frequency of rejections for offers equal to or less than 20% of the pie.

[3]In Lamalera, Alvard used packs of cigarettes instead of money to avoid the appearance of gambling. Cigarettes can be exchanged for goods/favors.

[4]Instead of giving responders the actual offers, Alvard gave 20 "sham" offers that ranged from 10% to 50% (mean sham offer = 30%). These are response frequencies to the sham offers.

[5]Because Patton randomly paired Quichua and Achuar players, there were 14 Achuar proposers and 15 Achuar responders, and 15 Quichua proposers and 14 Quichua responders.

Source: Henrich et al. (2005)

Fig. 1: *Locations of 15 small-scale societies*

Source: Henrich et al. (2005)

Meta-analysis The ultimatum game has been thoroughly investigated all over the world, of which Oosterbeek et al. (2004) have taken advantage, and thus they decided to realize a meta-analysis examining the data of 32 papers from 20 countries. They examined the culture differences according to three classifications, one geographical and two based on cultural classifications: one based on work of Hofstede (1991), the other based on combination of the works of Huntington (1996) and Inglehart (2000). In the end, they conclude that "the differences tend to follow geographical lines rather than some common cultural classifications" (Oosterbeek et al., 2004). Average offers with their rejection rate of countries grouped on geographical basis can be found in Table 1 at page 32. On the basis of geographical grouping, the lowest mean offer of 35% together with the lowest rejection rate of 0.05 was found for South America, whereas the highest mean offer of 43% was detected in Asia and western US states. In particular, the players offered significantly less in South America than the players in Asia, Europe East and South America.[23]

[23] Other, rather unusual, experiments are e.g. Chen and Tang (2009) who examined the ultimatum game among Tibetans and ethnic Han Chinese with respect to their dif-

4.4 Information

Any trade can be seen as "a bargaining game over the surplus generated from the exchange" (Croson, 1996), e.g. bargaining between a seller and a buyer. When buying goods the buyer may not know the total amount of surplus that is to be divided in the transaction — she may not know the costs to the seller caused by the production of goods. The game of incomplete information where the responder is not acquainted with the size of pie can be thought of as a model situation to such conditions. (Croson, 1996)

Croson's (1996) experimental study Croson (1996) conducted an experiment investigating an effect of information being varied on two dimensions. The first dimension was the way how the offers were made, either as a percentage or as a sum of money. The second dimension was represented by amount of information about the pie size provided to the responder. The four treatments were:

Tab. 4: *4 treatments of Croson's (1996) exp. study*

	Responder's information about pie size	
	Informed	Uninformed
Offer in Dollars	$I	$U
Offer as a percentage	%I	%U

Source: Author

The particular settings of these treatments were chosen in order to examine predictions of four different models. According to the *subgame-perfection*, equilibrium behavior, i.e. offering the smallest possible ε and accepting it by

ferent cultural traits and religious beliefs. Brañas-Garza and Cobo-Reyes (2006) realized a strategy-method experiment among gypsy minority living in the slums outside Madrid. The results were quite surprising — zero was the modal acceptance threshold, whereas 97% of the proposed division was an equal split. The most common argument for accepting zero was "si él lo necesita - if he really needs it".

the responder, does not change in any of the settings. *Bolton's (1991) comparative model* incorporates concerns for fairness in utility functions of players and thus an occurrence of rejection of positive offers is possible. Nonetheless, the manner how the offers are presented should not matter when the responders being completely informed, therefore it predicts behavior of \$I = %I. Ochs and Roth (1989) state that players may have a minimum acceptance threshold t_i. By accepting an offer $\leqslant t_i$ the increase in player's utility (caused by receiving that monetary amount) is smaller than disutility from agreeing on such a small offer, which leads to rejecting it. The threshold may be either in *absolute* terms (amount of money) or in relative terms (minimal *percentage*). Then it predicts behavior of \$I = %I = \$U and \$I = %I = %U, respectively.

The results did not support any of the models. First of all, behavior under any of the settings did not converge to the subgame-perfection. The remaining hypotheses were all rejected, because offers in \$I were significantly greater than offers in \$U, and in %I, resp. For example, the mean offer was 36% in \$U, 39% in %U, 42% in %I, and 45% in \$I. Furthermore, there was evidence that rejection occurred the most frequently in treatment %I than in any other treatment. In particular, the rejection rate in %I was 0.21, whereas in the remaining treatments it was less than 0.10.

4.5 Summary and discussion

The papers discussed in the previous section point to the fact that the results in the ultimatum game are robust with respect to many variations. Behavior of players do not converge to the subgame-perfect Nash equilibrium in any of the settings. The first players do not offer virtually zero, and rejection even of some positive offers, especially those less or equal to 20 percent, is quite common.

Nonetheless, players' behavior is sensitive to changes of some variables. From the methodological point of view, these are the variables examined due to the doubts that are always raised whenever there are discrepancies between the data and the theory — the size of the reward, which the players are bargaining over, and the experience of the subjects with the task. The papers investigating the stake effect conclude that inexperienced players do not change their offers with increasing stake size, despite the greater willingness of the second players to accept relatively lower offers as stake rises.

Why do not the proposers lower their offers? One explanation might be that the players who reject some offers do not disappear completely, even as the stake increases. In a case that the proposer would lower her offer but it would be rejected, the amount of money he would loose is much higher, in absolute terms, in the high stake treatment than in the low stake treatment. Hence, when the stake increases, the effect of decreasing the risk of rejection is compensated by the effect of higher absolute "probable loss". These effects influence proposers' behavior in opposite directions, and the offers remain unchanged then.

When inexperienced are compared to experienced players, some movements, like lowering the offers, are detected, but they are mostly negligible, or very small. Roth et al. (1991) shows that most of the proposers state their offers in a way that maximize the average earnings as they gain experience, whereas the responders persist to reject some positive offers, i.e. their behavior continues not to be in consistence with income-maximization. Furthermore, how large and quick the changes are depends primarily on the characteristics of the responders and their willingness to accept the offers, which is shown by the simulations Slonim and Roth (1998) have run. If they reject particular offers less often, then the proposers lower their offers more. Lusk and Hudson (2004) add an evidence that if subjects know what the theoretical solution is together with how other subjects have already played the game before, they significantly lower their offers without the rejection rate being increasing.

Study of Fajfar (2006) is a bit confusing. It shows that although players are provided with the results of the same experiment already undertaken (where subjects were not being given this information), they do not significantly change their behavior in comparison with the previous experiment, which is not the same pattern as one found in Lusk and Hudson (2004). Even though one omits that in Fajfar's (2006) experiment subjects were not given the theoretical solution, that certainly could affect the results, I think there is another additional explanation of the different behavior observed in these studies. In Fajfar's (2006) experiment, subjects were taken from two courses of the first year of a degree academic program, thus they were schoolmates and actually know each other. No matter who they were paired with, the other player happened very likely to be a person they knew. Hence, they could perceive the game as "sharing a cake with a friend", which could elicit a sense of solidarity. The fact that they bargained over credits that could help them in passing the exam of the course together with the rule how the roles in the game were assigned (students who had scored in the mid-term test above the median score became the proposers, the rest became the responders) could have strengthen the solidarity even more. Since the responders were students with worse scores and thus in greater need for additional credits, the proposers would not take advantage of their position (e.g. in the treatment with the information provided, 76% of students offered an equal share). Even though one gets to know how others acted in the same situation then, it does not have to affect her the same way as it does if she perceives the game as "dividing a cake among myself and a stranger".

Further, it has been tested whether the people from different cultural backgrounds conceive the game differently. Roth et al. (1991) observed some, rather small, variation in offer distributions among subject pools taken from different countries while the rejection rates did not differ significantly. This findings led to a hypothesis that "what varies between subject pools is a perception of what constitutes a reasonable offer under the circumstances" (Roth et al., 1991). On the contrary, there has been observed a great variation in offer distributions and their corresponding rejection rates in the study of Henrich et al. (2001), which does not support such hypothesis. Henrich et al. (2001) found out that the degree of market integration (MI) of the tribe (and also the degree of dependence on cooperation with family non-members in their everyday life) is significant variable in explaining behavior in the game. It can be the case that subjects who are used to trade on the market look at the decontextual ultimatum game from a similar perspective, whereas those who do not experience abstract sharing principles concerning behavior toward strangers may look at the game from a much greater variety of perspectives. Therefore, as MI decreases, there can be observed greater differences in subjects behavior. The least variation would be detected among industrialized countries, where people rely on market exchange in the high degree, which is exactly the case of Roth et al.'s (1991) study. Then, a possible extension of the hypothesis of Roth et al. (1991) may sound: "what varies between subject pools *with high market integration experience* is ....".

The study of Henrich et al. (1991) brought a lot of attention among scientists, and many subsequent comments were written. For example, Ortmann (2005) points out that in the experiment of Henrich et al. (2001), substantial procedural differences were present (changes in instructions, dismissals of several subjects who did not understand the game, which might lead to creating beliefs that there is some normative solution that is expected to be found and implemented, and many others) and that we do not know whether the differences in the results can be attributed to culture or rather to the procedural variability. Gigerenzer and Gigerenzer (2005) suggest that the presence of nonanymous experimenter can actually be perceived as the third person in the game, which may influence subjects in the field experiments even more than in the lab experiments. For more comments followed by authors' reactions see Henrich et al. (2005).

I have also dealt with implementation of incomplete information into the ultimatum game. When the responders do not know the actual size of the pie and the offers are expressed as an absolute amount of money, they cannot evaluate whether the proposed amount is a "fair" share of a small stake or an "unfair" share of a larger stake. By rejecting seemingly small offer they might actually punish players who have treated them fairly and their particular proposal only reflected the fact that the stake was small. Therefore, they might decide to accept quite small offers, in absolute terms. The proposers then exploit such situation and they make offers which likely would have been rejected if the proposers were completely informed. When the offers are made as a percentage, the situation is different, because the responders know exactly how "fair" the division is. However, they cannot be sure what the absolute size is, which would have been lost if rejected. As already said, people are more willing to accept relatively lower offers with increasing stake. The responders might decide to accept some offers on a basis of "what if it happened to be a share of a large stake?" doubt, which can be perceived and exploited by the proposers again. Study of Croson (1996) is consistent with this view. However, the difference observed between the treatments when the responders knew the size of the pie, and the settings varied in manner how the offers were made only, is quite surprising. Croson (1996) argues that if the comparative model of Bolton (1991) is extended with contingent weighting, it can still sustain as a descriptive theory applicable to her results. Contingent weighting deals with a recognition that under different settings people tend to put different weight on the same attributes[24]. In the case of her experiment it implies that the responders vary an importance they place on relative and absolute payoff with changing treatments.

[24] For more about an idea of contingent weighting see Tversky et al. (1988).

4.5.1 Where the problem lies

When the experimental data is not consistent with the theoretical predictions, it is natural to ask why. Binmore (2005), in his commentary on Henrich et al. (2005), writes his point of view he has on this topic. In my opinion, he describes the nature of the problem precisely. Therefore, in this section, I present several thoughts primarily taken from Binmore (2005).

First of all, a lot of about "selfishness axiom" being violated has been said in the literature, suggesting it to be a failure of one of the assumptions the economic theory is based on. In fact, the orthodox theory does not assume selfishness at all.

> *"The orthodox theory only requires that people behave consistently. It is then shown that they will then necessarily behave* ***as thought*** *they are maximizing something. Economists call this something* ***utility****, but they emphatically do not argue that people have little utility generators in their heads. Still less do they argue that people come equipped with mental cash registers that respond only to dollars."*
>
> *Binmore (2005)*

Thus, under some circumstances, people may put others' not just their own wealth into account. This fact, however, does not violate theoretic assumptions, as far as they behave consistently.

The ultimatum game is generally considered as a one-shot game. However, when subjects come to the laboratory and the game is presented to them, they does not have to see it that way. They are playing one huge indefinitely repeated game (Binmore (2005) calls it the *game of life*) for their whole life. One important feature of this game is the large number of Nash equilibria[25] it has. Social norms, that has evolved over time, deal with the equilibrium selection problem. Among these norms, we can find a notion about reciprocal actions, fairness considerations, punishment of exploiting behavior, and others. When subjects are exposed to the ultimatum game, they likely implement an equilibrium which they would carry out if they were put to a real situation with similar framing, social norms taking into account. However, such situation would not be a one-shot game but rather one of the repetitions in the game of life. Therefore, subjects actually play the ultimatum game as the repeated game they know, not as the one-shot game theorists have in their mind. Study of Henrich et al. (2001) is an illustration of such a behavior. For instance, subjects from Papua New Guinea seemed to be playing "gift-giving game", which has certain consequences for high offers/gifts being accepted in the real life, instead of the ultimatum game, which does not have any of this.

The question is how to get subjects to play the game as a one-shot game, without solving it by means of repeated-game equilibrium. The mainstream theory predicts that if one lets subjects to play the game repeatedly, each round matching them with new opponents, they eventually stop playing the equilibrium of the repeated game. After some time, they converge to one of the equilibria which is the solution to the actual game they are exposed to in the laboratory. The difficulty with the ultimatum game is the extensive amount of Nash equilibria it offers. The initial equilibrium can be situated far from the subgame-perfect equilibrium. It may take a very long time to move from that particular equilibrium toward the subgame-perfect equilib-

[25] This statement about the number of Nash equilibria in the game is derived from a so-called *Folk theorem*. It applies to an infinitely repeated n-person game of complete information with finite action sets at each repetition without high discounting of future payments (i.e. players do care a lot about their own future outcome). The theorem states that these games have large number of Nash equilibria and any outcome satisfying minimax condition can become a solution. The minimax condition suggests that a player chooses such a strategy that her maximum loss, she could ever encounter in the game, is minimized. (Binmore, 2005, and Rasmusen, 2007)

rium. This is demonstrated by the computerized simulations of Roth and Erev (1995), when they observed the convergence to the subgame-perfect equilibrium by round 1,000,000. We cannot expect any big changes in behavior during the ten rounds conducted in laboratory then.

5 Fairness explanation scrutinized: The dictator game

The reason why the responders reject some proposals is often attributed to punishment on the proposers for having taken advantage of their position and offering "unfair" division, whatever the perception of unfair is. On the other hand, behavior of the proposers can be explained by two aspects. Concerns for being fair is the first one. However, if the proposers assume that some low offers are likely to be rejected, offering less extreme division is consistent with payoff maximization. Hence, it would be the strategic thinking what governs behavior of the proposers. Such players are described as gamesmen, not altruists.

To test whether willingness to make nontrivial offers can be attributed to fairness consideration only, the dictator game has been developed.

> THE DICTATOR GAME:
> *The dictator game presents a problem how to divide a fixed sum of money between two players. The first player, the dictator, suggests a division of the sum between herself and the other player, the recipient. Unlike in the ultimatum game, the second player does not have a veto power, she has to always accept the division. The sum is split accordingly to the division suggested by the dictator.*

The dictator game is not a game in the sense of game theory, because the outcome of the dictator is not affected by anybody else's but her own moves. The situation is thus one of decision theory. Nonetheless, an optimal decision (i.e. a decision that yields an outcome that cannot be outperformed by an outcome yielded by any other available decision) is the division giving all the money to the dictator and zero to the recipient, the same division as predicted in the ultimatum game by subgame-perfection. If the reason for occurrence of nontrivial offers in the ultimatum game is the fear from rejection, players should give zero to their opponent when put in the role of dictators.

5.1 Experimental results

Forsythe et al.'s (1994) experimental study The first thorough study examining the ultimatum game against the dictator game was Forsythe et al. (1994). Each subject was exposed to one of the two games and each subject played it just once. The fairness motive hypothesis implies the offer distribution in the ultimatum game and in the dictator game to be (statistically) identical. Nonetheless, this hypothesis was rejected at the 0.01 level by all statistical tests the authors had run. Players in the dictator game offered significantly less. Little more than one third of the players proved to be gamesmen when they offered zero in the dictator game (no zero offer was observed in the ultimatum game). However, there were also players who offered an equal split or better in the dictator game, even though the proportion of such players was lower in comparison with the ultimatum game (22% as opposed to 65% in the ultimatum game).[26]

Hoffman et al.'s (1994) experimental study Hoffman et al. (1994) focused on investigating whether the behavior in the dictator game is not altered by nonanonymity of subjects to the experimenters. For example, subjects might not want to appear greedy. Preference for fairness does not result from "personal" preference then, rather they are derived from expected judgment by others. To guarantee anonymity of subjects not only with respect to other participants but also with respect to the experimenter Hoffman et al. (1994) used a treatment they called Double Blind. It consisted in use of envelopes containing 10 one-dollar bills, cardboard boxes ensuring privacy to the dictators while opening envelopes, and other procedures ensuring privacy[27].

[26] Forsythe et al. (1994) also examined the two games under the no payment conditions, but the results described above are taken from games where subjects received the payments only.

[27] They ran two Double Blinds treatments, the Double Blind 1 consists of the monitor voluntary selected among the participants, the Double Blind 2 employed the experimenter

Tab. 5: *Eight treatments of Hoffman et al.'s (1994) exp. study*

game + context framing	entitlement rule	
	contest	random
Ultimatum Game, divide $10	x	x
Ultimatum Game, exchange $10	x	x
Dictator Game, divide $10, Double Blind 1		x
Dictator Game, divide $10, Double Blind 2		x
Dictator Game, exchange $10	x	x

Source: Author

Furthermore, the authors investigated the effect of property rights, i.e. when the right to move first is earned, not randomly assigned (in their experiments it was based on how well subject had scored on a general knowledge test). The last treatment variable Hoffman et al. (1994) implemented was the form of instructions. They submitted the game to subjects either as dividing an amount of money provisionally allocated to each pair, or as a buyer-seller exchange. Table 5 shows the eight treatments the authors has run.

The authors has drawn several conclusion. In the ultimatum games, they note sensitivity of the first player behavior to both the context framing (divide/exchange) and the entitlement rule (random/contest). When the first players perceive the game as an exchange problem instead of a division problem, or when they earn the right to move first instead of by means of random device, they significantly lower their offers. The combination of the exchange context together with the contest entitlement affects the players in the direction of lowering their offers the most, when compared to other ultimatum treatments. For example, the modal offer was 20% of the pie and the median offer was 30% in the ultimatum exchange contest treatment, whereas 50% and 40% were the equivalent numbers in the ultimatum divide random treatment. Moreover, the rejection rates of all the treatments were

in the role of the monitor.

not significantly different. This results is in consistence with suggestions that in real life, the position one has in bargaining situations might reflect her abilities, experience, previous risks taken, etc., which the others can see as giving her the right to take advantage of her position, and thus not feel offended or angry when treated “unfairly”. Perceiving this, in the ultimatum contest entitlement treatment the proposers should lower their offers (to some extend) while the rejection rate should not increase, in comparison with random entitlement treatment, which was exactly the case.

As to the dictator games, their replication of the Forsythe et al.’s (1994) experiment reinforces their results. Furthermore, they also note sensitivity to the entitlement rule, it affects the first player behavior in the same direction as it does in the ultimatum game. Looking at the percentage of players offering zero, it doubled to 42% when the setting was changed from random to contest assignment. However, over half of the players still gives some positive amounts to the second players. Hence, there is other-regarding behavior that cannot be ignored in any of the two games under the usual (non)anonymity framework. The Double Blind treatments can help to better understand the motives of this other-regarding behavior. In the Double Blind 1 treatment, only 16% of players offered 20% of a pie or more, and 64% offered zero; the data from the Double Blind 2 treatment are not significantly different.

5.2 Summary

The results show that the fairness considerations by itself cannot explain the behavior of the proposers in the ultimatum game. Forsythe et al. (1994) suggest that the ultimatum game should be seen as a game of incomplete information in which different types of players are to be found. The proposers can be divided between pure gamesmen (keeping all the money to themselves) and players with taste for fairness, to varying degree; the responders can be divided between pure gamesmen (accepting all offers) and players with taste for punishment when treated unfairly.

Hoffman et al.'s (1994) results from the Double Blind treatments amplify the view that the first mover behavior in the ultimatum game or the dictator game should not be attributed primarily to preferences for "fairness", but rather it should be modeled in terms of expectations. They suggest that in ultimatum games the players have explicit expectations about how the second player will react to particular offers, in dictator game it would be implicit expectations of what the experimenters might think of the players who are not anonymous to them. The results of the Double Blind treatments do not support the explanation that the first mover behavior is governed by autonomous, private, other-regarding preferences. The results show that if the other-regarding preferences arise after all, a great part of them might be attributed rather to a derivation from expectational considerations than to an intrinsic own preference for fairness "the players are born with". For example, Henrich et al. (2001) support this view when they discuss the meat sharing among Hadza tribe. The tradition of meat sharing is primarily derived from fear of social consequences, which may arise when not-sharing is found out, than from actual preferences for sharing.

6 The ultimatum game: Theoretical models

In the light of the experimental results, game theorists have developed new models, some of which are presented below.

6.1 Static models

The models described in this section belong to the class of static models, which do not account for the element of time (or learning). Models in here represent “a theory of 'local behavior' in the sense that it explains stationary patterns for relatively simple games, played over a short time span in a constant frame” (Bolton and Ockenfels, 2000).

6.1.1 Payoff-interdependent preferences

One explanation of the discrepancies between the data and the theory is that players have interdependent preferences in a sense that their utility function depends not only on one's own monetary payoff, but also on the payoff of their opponent.

Model of inequity aversion Fehr and Schmidt (1999) present a model of inequity aversion[28]. They assume that, along with purely selfish subjects, there is a fraction of subjects who dislike inequitable outcomes. The utility function can be written as

$$u_i(\pi_i, \pi_j) = \pi_i - \alpha_i \max\{\pi_j - \pi_i, 0\} - \beta_i \max\{\pi_i - \pi_j, 0\}, \qquad (7)$$

[28] “Inequity aversion means that people resist inequitable outcomes, i.e., they are willing to give up some material payoff to move in the direction of more equitable outcomes.” (Fehr and Schmidt, 1999)

where

$$\beta_i \leqslant \alpha_i, \quad (8)$$
$$0 \leqslant \beta_i < 1. \quad (9)$$

The parameters α_i, β_i represent the disutility from inequity, with higher emphasis on disadvantageous inequality. The model does not include subjects who seek to have more than others, i.e those with $\beta_i < 0$. It can be shown that, when working with the utility function described above, equilibrium behavior of the games the authors consider is not virtually affected by such individuals.

Bolton and Ockenfels's (2000) ERC model ERC is an abbreviation for words of equity, reciprocity, and competition. The three words refer to experimental games which seem to give disparate results — equity is considered in determining behavior during bargaining games, reciprocity is cited to influence behavior in games such as the prisoner's dilemma, and competition refers to games where the theory of "competitive" self-interest predicts quite well, such as Bertand markets. The ERC model connects all three types then. Applying ERC to the two-person ultimatum game, an utility function (the authors call it motivation function) is given by

$$u_i(\pi_i, \pi_j) = v_i(\pi_i, \sigma_i), \quad (10)$$

where

$$\sigma_i(\pi_i, \pi_j) = \begin{cases} \frac{1}{2} & \text{if } c = 0 \\ \frac{\pi_i}{c} & \text{if } c > 0 \end{cases} \quad (11)$$

$$c = \pi_i + \pi_j. \quad (12)$$

The function v_i is an indirect utility function and $c > 0$ is the stake the players bargain over. It is assumed that v_i is increasing and concave in the first argument, which is consistent with the standard assumption about preferences for money. Furthermore, it is assumed v_i to be strictly concave in the second argument, attaining a maximum for $\sigma_i = \frac{1}{2}$. In other words, the first argument of v_i is player's own monetary payoff, whereas the second argument, σ_i, is the player's payoff relative to the others. She prefers to have the same share as her opponent the most (i.e. when both receive either half of the stake, or zero), and dislikes to deviate from this equal share no matter the direction (whether in the direction of favoring herself, or her opponent's).

Moreover, each player has 2 thresholds r_i and s_i such that

$$r_i(c) = \arg\max_{\sigma_i} v_i(c\sigma_i, \sigma_i) \quad c > 0, \tag{13}$$

$$v_i(cs_i, s_i) = v_i\left(0, \frac{1}{2}\right) \quad c > 0,\ s_i \leqslant \frac{1}{2}. \tag{14}$$

The threshold r_i shows the quantity the player i is willing to give to the second player in the dictator game. The second threshold, s_i, is the rejection threshold of the player i as the responder in the ultimatum game. It is such a division that gives the same value of v_i as if both players receive zero.

Comparative model Other model whose core lies in the idea of payoff-interdependent preferences is Bolton's (1991) comparative model, that is very similar to the ERC model. He considers a utility function in a form of

$$u_i(\pi_i, \pi_j) = v_i(\pi_i, m_i), \tag{15}$$

where

$$m_i = m_i(\pi_i, \pi_j) = \begin{cases} 1 & \text{if } \pi_i = \pi_j = 0 \\ \frac{\pi_i}{\pi_j} & \text{otherwise} \end{cases}. \tag{16}$$

Again, the function v_i is an indirect utility function, where the argument m_i represents the relative share of the stake. The function v_i is increasing in its first argument, i.e. in the absolute amount of received money, and it is also increasing in its second argument, which is the ratio of one's own monetary payoff to one's opponent's monetary payoff. It is further assumed, that if the point where the player receives the same share as her opponent is reached or is exceeded, i.e. $m_i \geqslant 1$, the only thing she cares about is amount of money she earns regardless of the payoff of the other player (i.e. for $m_i \geqslant 1$, v_i is constant in its second argument, set at its maximum).

6.1.2 Comparison of the models

All the models are based on the idea that player takes in consideration her own absolute monetary payoff, and furthermore, she compares it to the payoff of the second player, when deciding what action to take. The comparative model and the ERC model are very closely related. Both the models assume that the player cares about an absolute size of her own payoff together with the share which it represents *relatively* to the other player. These two aspects are embedded in the utility function more or less as substitutes. For example, if both players receive zero, it is the saddle point of the utility function (i.e. it attains the minimum with respect to the argument representing the absolute payoff, but it attains the maximum with respect to the argument representing the relative payoff).

The key difference between the model of inequity aversion and the other two models consists in how they incorporate the concerns about the opponent's payoff in the player's utility function. The ERC model and the comparative model use an argument representing relative share, whereas the model of inequity aversion incorporates it as the *absolute* difference between the two payoffs.

All models assume the player to dislike unequal payoffs. Nonetheless, in the inequity aversion model, the player puts more negative weight when it is herself who receives less than when it is the second player who receives less. The ERC model captures the disutiliy from all unequal division, but it is not further specified whether the player puts different weight depending on who gets the smaller share. On the other hand, in the comparative model, the player puts no negative weight when it is herself who receives more. However, such model then cannot explain positive offers observed in the dictator game.

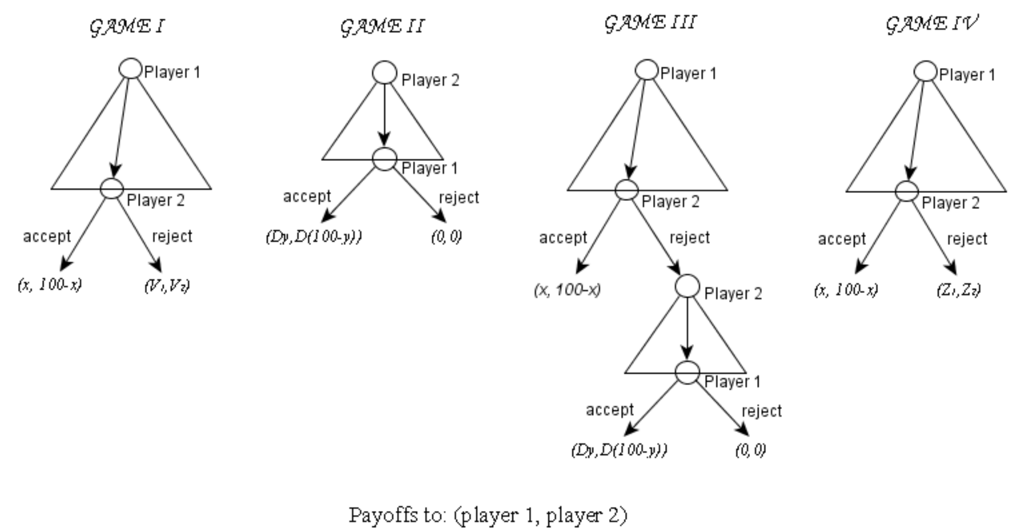
Models of payoff-interdependent preferences are a rather special case. More general approach is represented in models which perceives utility to be interdependent also on other attributes of one's opponent than only on the opponent's pecuniary reward, like intentions of the players, or alternatives of the outcome not have been reached in the game. One such model is Levine (1998) who further introduces coefficients of altruism/spitefulness. Whether, and how much, an utility of a player i is increasing, or decreasing, resp., in a payoff of a player j depends on her coefficient as well as her beliefs she has about the coefficient of her opponent. When an opponent is believed to be altruistic, the other player puts more positive weight on money received from such a person. On the contrary, when an opponent is believed to be spiteful, the other player puts more negative weight on it.

6.1.3 Experimental evidence

Binmore et al.'s (2002) experimental study Binmore et al. (2002) conducted an experiment on backward induction, but the backward induction failed. They discuss that the failure of subgame-perfection is often attributed to utilities described in the previous section. However, they detected systematic violations that cannot be explained by such means only.

They identified three components of backward induction, which they investigated separately: *i*) rationality, *ii*) subgame consistency — the position of a subgame in a game does not change subjects' behavior in that subgame, and *iii*) truncation consistency — if a subgame is represented by its equilibrium payoff instead, behavior in unchanged stages of a game remains the same. Offers were made in terms of percents of a pie. For this purpose they examined 4 games described in Figure 2.

Fig. 2: *Four games in an experiment of Binmore et al. (2002)*



Source: Author

Game I is a modified ultimatum game with positive disagreement payoffs (V_1, V_2), that were varied between three levels: $\{(10, 60), (10, 10), (70, 10)\}$. Rationality, together with inequity aversion, predicts equilibrium behavior of the treatment of $(V_1, V_2) = (10, 60)$ giving 60% of the pie to player 2 and 40% to player 1. Initially, subjects proposed such division, but it met with a very high rate of rejection. This evidence suggests that payoff-interdependent preferences are not the whole story.

Games II and III work with a discount factor D that represents costs originated by postponing an agreement. The factor ranged from 0.2 to 0.9. The subgame consistency says that if, no matter why, the stage 2 of Game III is reached, subjects should behave (statistically) identically as they behave in stand-alone Game II. Nonetheless, in the second stage of Game III the proposers (players 2 at that moment; they switched the roles of the proposer and the responder when they reached the second stage) offered a less favorable division for themselves than they did in Game II.[29]

Game IV introduces new payoffs (Z_1, Z_2) specific for each subject i, that were calculated on the basis of subject i's plays and her realized payoffs in Game III. These payoffs represented the expected value from playing a particular subgame. The way how to calculate them is quite a difficult, but the authors came up with several methods. The least complicated method was to take the average payoffs: $Z_1(i)$ was the average of the payoffs of subject i (occupying the role of player 1 at that moment) from the second-stage games in which subject i participated (in a role of player 1), and $Z_2(j)$ was calculated analogically from payoffs of subject j as player 2 in those games. By comparing subjects' behavior when the discount factor D was varied in Game III with subjects' behavior when payoffs (Z_1, Z_2) were changed equivalently, the authors examined the truncation consistency. The results did not support it, subjects exhibited more sensitivity to changes in D than to equivalent changes in (Z_1, Z_2).

The experiment shows that "preferences in seemingly identical games depend upon the larger context in which the games are played" (Binmore et al., 2002).

6.2 Dynamic models

The stochastic dynamic models do not discuss the motives behind behavior so far, they rather investigate a process of convergence of behavior as time passes, or, in other words, as subjects gain experience. In this section, I discuss one of the simplest models coming from this family.

[29] The orthodox theory suggests that the second stage should never be reached. A subgame-perfect Nash equilibrium for this game is a division giving payoffs of $(100 - 100D, 100D)$, that is accepted in the first stage.

6.2.1 Adaptive learning model

Roth and Erev (1995) presents an adaptive learning model which focuses on a process of evolution of "the probability that a given player will choose a given pure strategy at time t".

The basic model The model is built on an assumption that, at the beginning, each player has an initial propensity to play her k^{th} pure strategy. This propensity to play the k^{th} pure strategy of a player i yielding him a payoff x in the time t is updated in the time $t+1$ by adding up x. In other words, an outcome earned in previous experience based on a played strategy is converted to current player's choices of a strategy to be played out.

ADAPTIVE LEARNING MODEL:
Let $k \in \{1, \ldots, s\}$ be a pure strategy available to a player i, $q_{ik}(1)$ be an initial propensity of player i to play her k^{th} pure strategy, $q_{ik}(t)$ be a propensity of player i to play her k^{th} pure strategy at time t, and $p_{ik}(t)$ be a probability that player i plays her k^{th} pure strategy at time t.
Let player i plays her k^{th} pure strategy at time t and receives a payoff of x.
Then

$$\begin{aligned} q_{ik}(t+1) &= q_{ik}(t) + x, && (17) \\ q_{ij}(t+1) &= q_{ij}(t) \quad j \neq k,\, j \in \{1, \ldots, s\}. && (18) \end{aligned}$$

For the probability $p_{ik}(t)$ it holds

$$p_{ik}(t) = \frac{q_{ik}(t)}{\sum_{j=1}^{s} q_{ij}(t)}. \tag{19}$$

Such model obey the "law of effect", which says that the strategies that have met with a success are more likely to reoccur in the future. Furthermore, the learning curve gets flatter over time (i.e. payoff of x received from playing the k^{th} pure strategy at time t has decreasing effect on the probability $p_{ik}(t)$ with increasing t). The authors then loosely distinguish between the short term and the intermediate term with a cutoff being situated at the part of the learning curve where it verges from the steep into the flat part.

Modification In their work, Roth and Erev (1995) further modify the basic model in three ways. In the first modification, they introduce a parameter μ, which is a (small) *cutoff probability*. Whenever the probability $p_{ik}(t)$ drops below this value, we set $p_{ik}(t) = q_{ik}(t) = 0$. It ensures that cases with unobservably small probabilities do not affect the outcome.

The new parameter of the second modification is a *persistent local experimentation or error parameter* $\varepsilon \geqslant 0$. Instead of adding x to the propensity of the particular strategy we add $(1-\varepsilon)x$, and the remaining εx is divided between the propensities of strategies adjacent to that strategy (in the ultimatum game, these are the strategies that yield payoff of $x \pm$ one unit)[30]. By this method, a successful strategy increases also a probability of strategy that is close to it.

The last modification considers gradual forgetting, which prevents $\sum q_{ij}(t)$ from growing without bound as t goes to infinity. Every propensity $q_{ik}(t)$ is multiplied by $(1-\varphi)$, where φ represents a *forgetting parameter* (which is very small, e.g., $\varphi = 0.001$). At some point, $\sum q_{ij}(t)$ will become approximately constant, because the average received payoff per round will equal

[30] In the case that the played strategy k yields a payoff of x that is the extreme from a given range, i.e. there is only one strategy l adjacent to the strategy k (yielding a payoff of x minus one unit), then

$$\begin{aligned} q_{ik}(t+1) &= \left(1-\frac{\varepsilon}{2}\right)x \\ q_{il}(t+1) &= \frac{\varepsilon}{2}x. \end{aligned}$$

the average amount forgotten per round.

6.2.2 Simulations and experimental data

It would require great volumes of data to estimate the initial propensities of all the distinct strategies available in an unsimplified ultimatum game. Therefore, Roth and Erev (1995) reduced the strategy set available to each subject into a range of 9 strategies: for the proposer it was to choose a demand for herself from integers of 1 to 9; for the responder it was to state a maximal acceptable demand from the same range of numbers.

First, the authors ran computerized simulations, several for each modification of the basic model, using initial propensities randomly drawn from a uniform distribution[31]. The simulations of the cutoff model never converged to a subgame-perfect Nash equilibrium (demanding and accepting 9 with certainty); by the round 100,000 demands not greater than 7 only had positive probabilities of playing. These simulations converged both to imperfect equilibria, as well as to non-equilibria, i.e. state when a particular demand is chosen with certainty, but the responder would not reject even a higher demand. The other modified models (with the error, or/and the forgetting parameter) began to move toward the perfect Nash equilibrium, but only after a very long time (by the round 1,000,000).

Second, the authors examined the data of experiments of Roth et al. (1991) and Prasnikar and Roth (1992) to estimate the initial propensities for each pure strategy of the simplified ultimatum game.[32] After having

[31] "For each player, a *pre-propensity* for each of his pure strategies is drawn from a uniform distribution of $\langle 0, 1 \rangle$, and then these are normalized so that $S(I)$, the strength of the initial propensities, equals 10." (Roth and Erev, 1995)

[32] They first transformed the first two played demands of the proposer into the closest values of the integers of 1 to 9, and then initial propensity for each pure strategy was computed as the relative frequency of these demands (when some demand fell precisely in the middle of two values, the demand was transformed into the lower of the two numbers). To estimate initial propensities of the responder to reject the demand, they computed the rejection rate of each of the transformed demands. In the simulations, the authors employed the strategy method, which implies that the initial propensities to reject should

computed the (corrected) initial propensities, the authors ran several computerized simulations for each of the modification of the basic model, which were then compared to the actual plays.

To conclude, simulations showed different patterns of the convergence when the samples taken for estimation of initial propensities were drawn from different countries, which was in consistence with experimental data. Moreover, in an intermediate term, the results of simulations followed the differences of subjects' behavior observed in the experiments.

This model is a very simplified model, which does not count for many other aspects, e.g information players may have about the play, or beliefs about the future actions of their opponents players may held. However, when initiated with behavior observed in a real game, it tracks an evolutionary path of behavior quite well. Roth and Erev (1995) discuss that these results suggests that "a substantial part of how players' knowledge and beliefs influence the game may be reflected already in the first round data".

be nondecreasing in demand. Therefore, these proportions of rejection should exhibit the same monotonicity. A few noticed violations were attributed to random error, and the authors were able to restore the monotonicity by a mechanism they called "pooling".

7 Income maximization: The proposer

Roth et al. (1991) found that as players gain experience, the proposers state their offers in accordance with income-maximization. In this section, I examine more deeply what income-maximization behavior means and whether the proposers play as income-maximizers if they are provided with information that allows them to accurately compute the optimal offer (i.e. offer that maximizes the expected payoff). For this purpose, I consider the strategy method, i.e. the responders state their minimum acceptance thresholds α_i, which is fixed during one round. If the proposed offer is found below the threshold, it is rejected, otherwise it is accepted. Moreover, I consider the offers made in terms of the share of the pie π, i.e. the proposers state $x \in \langle 0, 1 \rangle$, where $\pi = 1$. If it is accepted, the proposer's outcome is $(1 - x)$ and the responder's outcome is x; if rejected, both players receive zero. I analyze the situation when proposers play against the responder randomly drawn from a set of all responders $\{1, \ldots, n\}$, she can possibly be matched with, whose thresholds are known.

LEMMA 1: (Income-maximizing behavior)
Let $n \in \mathcal{N}$, $\{1, \ldots, n\}$ *be the set of all possible responders the proposer can be matched with,* $i \in \{1, 2, \ldots n\}$, $x \in \langle 0, 1 \rangle$, *and* $\alpha_i \in \langle 0, 1 \rangle$. *Let* $\rho(x)$ *be the probability density function of the cumulative distribution of variable* α.
Then an income-maximizing proposer states an offer $x^* \in \langle 0, 1 \rangle$ *such that it maximizes the expected profit function* $\Pi(x)$, *where*

$$\Pi(x) = (1 - x) \int_0^x \rho(\xi) \; d\xi, \tag{20}$$

which can be rewritten to

$$\Pi(x) = (1-x)\int\limits_0^x \frac{1}{n}\sum_{i=1}^{n}\delta\left(\xi-\alpha_i\right)\,d\xi, \tag{21}$$

where $\delta(\cdot)$ is the symbol for the Dirac δ-function.

Def.: The Dirac δ-function

Let $a, x \in \mathcal{R}$. Then the *Dirac δ-funcion* $\delta(x-a)$ is a generalized function defined by the properties:

$$\delta(x-a) = \begin{cases} 0 & \text{if } x \neq a \\ +\infty & \text{if } x = a \end{cases}, \tag{22}$$

and

$$\int\limits_{-\infty}^{+\infty} \delta\left(\xi - a\right)\,\mathrm{d}\,\xi = 1. \tag{23}$$

The function $\Pi(x)$ attains its only local maxima at each of the stated thresholds, i.e. for $x = \alpha_i$. Therefore, the optimal offer is always one of the thresholds. If $\{\alpha_{(i)}\}_{i=1}^{n}$ is the sequence of the order statistics of the thresholds α_i, then the global maximum of the function $\Pi(x)$ is

$$\max\left\{\frac{1}{n}\left(1-\alpha_{(1)}\right),,\ldots,\frac{i}{n}\left(1-\alpha_{(i)}\right),\ldots\frac{n}{n}\left(1-\alpha_{(n)}\right)\right\}, \tag{24}$$

and the optimal offer is the corresponding threshold.

7.1 Examples

In this subsection, several examples of possible forms of the function $\Pi(x)$, with corresponding optimal offers, are presented. The first two examples consider the cumulative distribution function of variable α to be discrete, whereas the last example shows the solution in a case when the distribution function is continuous.

7.1.1 Set of the responders: One player only

For $n = 1$, the function $\Pi(x)$ is in form

$$\Pi(x) = (1 - x) \int_0^x \delta\left(\xi - \alpha_1\right) \mathrm{d}\,\xi. \tag{25}$$

The function $\Pi(x)$ is shown in Figure 3a. When there is only one candidate whom the proposer may be matched with, the solution is really obvious. As we can see, the function attains the maximum at

$$x^* = \alpha_1, \tag{26}$$

which says that in order to maximize her payoff, the proposer should set the offer at the responder's minimum acceptance threshold.

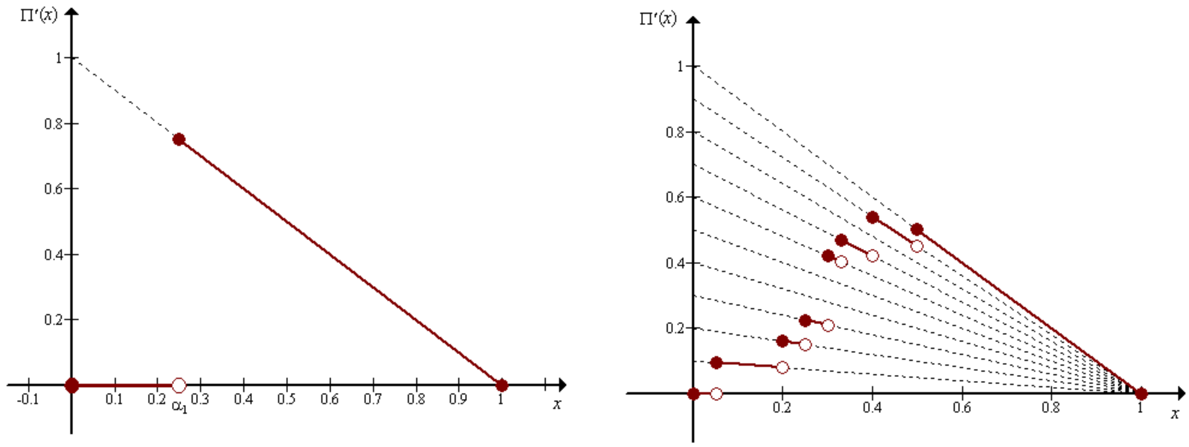
7.1.2 Concrete example

For illustration, I show an example solution for specific numbers.
Consider the set of the responders consisting of ten subjects (i.e. $n = 10$) with the following thresholds: $\alpha_1 = 0.05$, $\alpha_2 = 0.20$, $\alpha_3 = 0.25$, $\alpha_4 = \alpha_5 = \alpha_6 = 0.30$, $\alpha_7 = 0.33$, $\alpha_8 = \alpha_9 = 0.40$, $\alpha_{10} = 0.50$.
The function

$$\Pi(x) = (1 - x) \int_0^x \frac{1}{10} \sum_{i=1}^{10} \delta\left(\xi - \alpha_i\right) \mathrm{d}\,\xi \tag{27}$$

Fig. 3: $\Pi(x)$ *function*

a) $n = 1$ b) $n = 10$



Source: Author

is shown in Figure 3b.
Then, the optimal offer x^* that maximizes the function $\Pi(x)$ is

$$x^* = 0.40 \tag{28}$$

7.1.3 Continuous function

If one does not know the respective thresholds of the responders, but she knows that the responders are drawn from a population with a continuous smooth probability distribution of α with known density $\rho(x)$, then the solution x^* maximizing the function $\Pi(x)$ must meet two maximization conditions:

1. First condition:

$$\frac{\partial \Pi(x)}{\partial x} = \frac{\partial \left((1-x) \int_0^x \rho\left(\xi\right)\,\mathrm{d}\,\xi \right)}{\partial x} = 0, \tag{29}$$

   thus

$$-1 \left(\int\limits_0^{x^*} \rho\left(\xi\right)\,\mathrm{d}\,\xi \right) + \left(1-x^*\right)\rho\left(x^*\right) = 0, \tag{30}$$

   that is

$$\left(1-x^*\right)\rho\left(x^*\right) = \int\limits_0^{x^*} \rho\left(\xi\right)\,\mathrm{d}\,\xi. \tag{31}$$

2. Second condition:

$$\frac{\partial^2 \Pi(x)}{\partial x^2} < 0, \tag{32}$$

   that is

$$-2\rho\left(x^*\right) + \left(1-x^*\right)\rho'\left(x^*\right) < 0. \tag{33}$$

One must first choose all $x' \in \langle 0, 1\rangle$ locally maximizing the function $\Pi(x)$, i.e. x' satisfying the two conditions above. The optimal offer x^* is one of these x' such that $\forall x'$: $\Pi\left(x^*\right) \geqslant \Pi\left(x'\right)$. The first condition shows that the optimal offer heavily depends on the form of the probability function. For simple probability functions it can be expected that $\Pi(x)$ has one local maximum only (which is also the global maximum then).

For instance, let $x \in \langle 0, 1\rangle$, $a \in (0, 1\rangle$. Furthermore, consider that the thresholds are uniformly distributed over the interval $\langle 0, a\rangle$, i.e.

$$\rho(x) = \begin{cases} \frac{1}{a} & \text{for } x \leqslant a \\ 0 & \text{for } x > a \end{cases}. \tag{34}$$

Two possible situations can arise:

- $a \in \left\langle \frac{1}{2}, 1 \right\rangle$

If $x > a$, the second maximization condition is not satisfied.
Then for $x < a$ one may find

$$\frac{\partial \Pi(x)}{\partial x} = -\int_0^{x^*} \frac{1}{a}\,\mathrm{d}\,\xi + (1 - x^*)\,\frac{1}{a} = 0, \tag{35}$$

that is

$$(1 - x^*)\,\frac{1}{a} = \int_0^{x^*} \frac{1}{a}\,\mathrm{d}\,\xi. \tag{36}$$

Therefore

$$(1 - x^*)\,\frac{1}{a} = \frac{x^*}{a}, \tag{37}$$

thus

$$x^* = 0.50, \tag{38}$$

which is the optimal offer (for $x = a$: $\Pi(a) \leqslant \Pi\,(x^*)$).

- $a \in \left(0, \frac{1}{2}\right)$

If $x > a$, the second maximization condition is not satisfied.
Furthermore, $\forall x$, $x < a$:

$$0 \leqslant x < 1 - x, \tag{39}$$

therefore

$$\int_0^{x} \frac{1}{a}\,\mathrm{d}\,\xi = \frac{x}{a} < (1 - x)\,\frac{1}{a}, \tag{40}$$

and thus

$$\frac{\partial \Pi(x)}{\partial x} = -\int_0^{x} \frac{1}{a}\,\mathrm{d}\,\xi + (1 - x)\,\frac{1}{a} > 0. \tag{41}$$

The function $\Pi(x)$ thus does not attain a maximum at any inner point of the interval $(0, a\rangle$. However, the function $\Pi(x)$ is increasing at this interval, and therefore attains its maximum at the extreme point, i.e. the optimal offer is

$$x^* = a. \tag{42}$$

7.2 Experiment

The payoff-interdependent models, or interdependent models in general, capture the disutility from unequal division. Therefore, they suggest that some players may decline certain offers, if the disutility is high enough with respect to the utility elicited by receiving the particular amount of money (no matter the circumstances). A very strong majority of the experimental studies show that there is a fraction of subjects who reject offers, indeed, especially if they are faced with offers less or equal to 20% of the pie.

As to the proposers, it is observed that they frequently make nontrivial offers (in many experiments, the mode offer is an equal split). Such behavior may result from two alternative motives. One is the "fairness motive" saying that subjects dislike unequal division even though it is in their favor, as it is captured in the Fehr and Schmidt's (1999) model of inequity aversion, for example. Therefore, they offer less extreme division than the subgame-perfection predicts. The other motive is the fear of rejection. The proposers may have some beliefs about how likely each offer will be rejected, and then they state their offer in order to maximize the profit function, based on their beliefs.

Experiments on the dictator game show that when the threat of the rejection is removed, subjects significantly lower their offers. For instance, in the Double Blind Treatment, Hoffman et al. (1994) noted 64% of the dictators to offer zero to their recipients. This suggests the "fear motive" to be the main motor that triggers the proposers' actions. I want to contribute with a study that employs methods different than the dictator game, in order to examine whether the proposers act as income-maximizers. In my experiments, the proposers will be given additional information allowing them to compute the probability of rejection of each offer, and thus to find the optimal offer. Another aspect that I would like to examine is the case when there exist proposers with some taste for fairness, however when the optimal offer is consistent with this "taste" then they will act as income-maximizers.

7.2.1 Experimental design

My experiment will consist of three treatments, and I will use the within-subject design (i.e. each subject will be exposed to all three treatments). In two of the treatments, the played game will be the ultimatum game using the strategy method, i.e. proposers will state their minimum acceptable threshold before they see the actual offer. If the proposed offer will be found below the threshold, it will be rejected, otherwise it will be accepted. The last treatment will be the dictator game. In all treatments, the players will be dividing 1,000 tokens, with an exchange rate 200CZK for 1,000 tokens. The offers will be made in multiples of five[33]. I want to use 1,000 tokens such that one can easily compare the two shares (her own share to her opponent's share), as if the offers were made as a percentage of the pie, but without actually using the term of percentage.[34]

The first treatment (U), that will act as the control, will be the standard ultimatum game. In the second game — the treatment with information (I) — first, the responders state their thresholds. Then the proposers will be given the information about what the distribution of these thresholds is. Only after that they will state their offers. Each proposer will be randomly

[33] 5 tokens = 1CZK, which is the smallest denomination of the currency of the Czech republic.

[34] Croson (1996) found that if offers are made as a percentage, the responders reject some offers more frequently than when offers are made as amount of money. I do not want to elicit such behavior, because then it might well happen that the equal split would actually be the optimal offer and it would not be possible to distinguish between income-maximizers and "fairnessmen".

matched with one responder, and the offer will be then accepted or rejected accordingly. The fact that the proposers are provided with such information will be the common knowledge. In the third treatment (D), the subjects will play the standard dictator game. The sequence of the treatments will be randomly chosen, but under the condition that the U-treatment will precede the I-treatment. The reason is that once the proposers are given the information, their beliefs about the distribution of thresholds in the uninformed treatment would likely be very affected by the preceding information.

If all the proposers act as income maximizers, then the the following hypothesis will not be rejected:

H_1: In the treatment with information, the proposers state their offers in order to maximize their expected payoff.

However, the evidence from the dictator games show that there is a fraction of the proposers who are not simple income-maximizers; they offer some positive amounts even though their proposal cannot be declined. Nonetheless, if their "thresholds of fairness" — the amount of money they offer in the dictator game — is below the value of the optimal offer computed for the I-treatment, they should state their offers in accordance with the income-maximization when playing the game under the I-treatment setting. Then another hypothesis — income-maximizing with taste for fairness — can be stated:

H_2: The players whose dictator-game-offers are lower than the optimal offer computed for the I-treatment will state their offers at the optimal offer (maximizing the expected payoff) when being exposed to the I-treatment.

To examine the two hypotheses, it would be sufficient to run only the I- and D- treatments. However, I want to investigate whether providing this kind of information affects the subjects in any way and whether some of the proposers state their offer at 50% because they think it is optimal and not because they think it is fair. For this purpose, I will also run the U-treatment and I will compare its results to the results of the I-treatment.

With my experiment, I will examine whether the theoretical assumption of rationality of the players is not violated. It may happen that both hypotheses will be rejected. Actually, computing the optimal offer may be a bit difficult, and players then could learn how to do it when given more time and more experience. The extension of the experiment would then be to play all the games repeatedly, while rematching the pairs in each new round, and then compare the three games for each of the rounds. Another problem that could arise is that the optimal offer computed for the I-treatment could actually be an equal split, and then I would not be able to distinguish between those offering an equal split because they perceive it fair, and those who offer this division because they are maximizing their expected payoff. A variation that prevents this from happening is to run a treatment where the responders' action sets will be restricted, such that they will have to state their thresholds below 50% of the pie.

On the other hand, if there is an evidence that a considerable number of proposers act as pure income-maximizers (offering zero in the dictator game and optimal offer in the informed game), one might try to present a new model. I think of a modification of the Roth and Erev's (1995) adaptive learning model. I would say that there is a fraction of proposers θ who are pure income-maximizers. These proposers have some initial beliefs about the distribution of the thresholds of the responders they face in the game. They state their offers in consistence with the maximization of their expected payoff, given their beliefs. With each rejection or acceptance of the offer, the beliefs change, and then they change their subsequent offers according to their new believed optimal offer (thus, for each round they would play only one strategy with certainty, which is their believed optimal offer). For the rest of the players, i.e. all responders and $1 - \theta$ of proposers (e.g in this fraction the altruistic players, or the players who misunderstood the game are included), I would use the setting of the adaptive learning model, i.e. the players have some initial propensity to play each of the strategies (no matter the reason) that change over time depending on how successful the strategy is, where an earned outcome play an important role. Then one may discuss how the behavior observed in the game is affected depending on θ. A thorough analysis of such a model could also be a separate topic for a thesis, therefore I will not go any further in my discussion now. However, I would like to focus on a detailed description of such a model in some of my works in the future.

Conclusion

In this thesis, I have focused on experimental testing of theoretical predictions in the ultimatum game. Under the assumption that players are income-maximizers, the theory predicts that the proposers offer zero, or the minimal possible amount, to the second player, and the responder will accept such an offer. Nonetheless, the experimental results do not confirm to these predictions, and therefore the ultimatum game has become a theme for a vivid discussion among the theorists. I have provided a survey of experimental studies examining different aspects of the ultimatum game. I have divided them according to several variables, following the particular interest of the studies. These variables were stake size, role of the experience, culture, and information. I have compared the findings of the studies to each another and I have suggested an explanation of seemingly inconsistency between some of the results.

First, I have provided the basic results of these studies, and then I have discussed each of them in more detail. Even though the settings vary across the studies, the subjects exhibit similar behavioral patterns. The first players propose a nontrivial share of the pie; the mode offer is frequently 50% of the pie. There is usually a fraction of the responders who reject some positive offers, and with decreasing offers, the probability of rejection increases. From the experimental findings, I can conclude that increasing amount of money the players are bargaining over does not affect the proposers' behavior, but the responders become more willing to accept relatively smaller shares of the pie. When the ultimatum game is played for more rounds, while rematching the pairs in each round, the proposers seem to adjust their behavior in accordance with the income-maximizing behavior, given the rejection rate of the responders they face in the game. However, the responders persist not to accept all positive offers. The studies investigating the effect of culture show that subjects from industrialized societies behave in a very similar manner, especially when they are inexperienced. Nonetheless, subjects' behavior varies greatly when the participants are members of small-scale societies, and the patterns of their daily lives seem to influence their decision in the game. When the responders lack the information about the pie size, they become more willing to accept lower offers, of which the proposers take advantage and they lower their offers accordingly. There is an evidence that the manner how the offers are made affects the responders' behavior even though they are completely informed; rejection of identical offers is higher when it is presented as a percentage of the pie than when it is presented as an amount of money.

Furthermore, I have presented an explanation of why certain discrepancies between the theory and the data are observed. Primarily, I have taken the thoughts from a reputable experimenter Ken Binmore. He suggests that even though one presents the game as the ultimatum game, the subjects are actually playing a different game, which is a part of their indefinitely repeated "game of life". Therefore, in the laboratory, they employ a solution in accordance with their game of life, instead with the one-shot ultimatum game. He argues that if the subjects were given enough time, they would eventually converge to the theoretical solution. However, the amount of time that can be provided in the laboratory is not sufficient enough. I have also mentioned the dictator game, that investigate the often quoted fairness explanation, and some experimental studies on this game.

Moreover, I have focused on several new models. The first group is taken from the family of static models. In order to be in consistence with the data, these models add new variables into the utility function. Besides the utility from receiving certain amount of the reward medium, they also capture the concerns for fairness, and/or other characteristics of the game or the subjects. However, there is an experimental evidence that cannot be explained by the new models either. The second group is taken from the family of dynamic models, which capture the changes in subjects' behavior over time. In particular, I have presented the adaptive learning model of Roth and Erev (1995).

The computerized simulations of this model track the actual behavior of real players well, within the limitation of the simplicity of the model. The simulations also seem to confirm to the opinion of K. Binmore, that subjects would eventually converge to the theoretic equilibrium with time; in some simulations, the behavior converged to the predicted equilibrium. However, it took about 1,000,000 repetitions to get to the predicted equilibrium.

Finally, I have discussed the behavior of proposers as income-maximizers if they know that some offers are rejected with particular probability. First, I have provided the theoretical optimal offer when the proposer knows the distribution of the thresholds of the responders, one of them she will be randomly matched with. I have shown what the optimal offer is in several examples. Furthermore, I have outlined the experiment I would like to run in order to examine whether the proposers make nontrivial offers because they maximize their expected payoff, or because they have a "taste for fairness". If the results of the experiment show that there is a considerable large number of proposers who are pure income-maximizers, I have suggested that new model should be developed. I have proposed one modification to the adaptive learning model of Roth and Erev (1995), which is dividing the proposers between pure income-maximizer and others, where each of them changes its behavior on slightly different basis.

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