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Social Evaluation and Compensation of Exclusion

An Experimental Study

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List of Acronyms

AGG Allgemeines Gleichbehandlungsgesetz

CET Certainty Equivalence Test

EET Equality Equivalence Test

EOP Equality of Opportunity

ERC equity, reciprocity, and competition

ESS European Social Survey

OLS ordinary least squares

RE random-effects

U.S. United States of America

1 Introduction

Probably the most universally supported conception of justice in advanced societies is that of equal opportunity. (Roemer 2002, p. 455)

In recent years, Equality of Opportunity (EOP), or rather inequality of opportunity, especially due to discrimination, has increasingly come into the public eye. Although most advanced societies are based on the equality of their citizens, different people have different opportunities depending on factors such as their place of birth or social background. On top of that, discrimination, e.g. on the basis of gender, ethnicity, age and other factors that cannot be influenced, further disadvantages some parts of society. An example of discrimination that has been repeatedly discussed in public debates is the fact that some parts of society earn less than others through no fault of their own and have fewer opportunities for advancement. Some people are even excluded entirely from certain jobs because of personal characteristics, despite having sufficient qualifications. This discriminatory exclusion massively violates the assumed EOP for all members of a society, and is the focus of this thesis.

Due to its high relevance, it is hardly surprising that discrimination in a society is addressed by several initiatives to prevent or compensate for violations of EOP. In Germany, for example, the General Equal Treatment Act, or Allgemeines Gleichbehandlungsgesetz (AGG), was passed in 2006 as a means of combating injustice due to discrimination. This Act is designed "to prevent or to stop discrimination on the grounds of race or ethnic origin, gender, religion or belief, disability, age, or sexual orientation" (§ 1 AGG),[1] and until November 2022, already 2200 judgments have been handed down in court on the basis of the AGG.[2]

The issue of discrimination leading to a lack of control and thus responsibility for one's own income has also found its way into behavioral economics, especially the steadily growing experimental economics literature on fairness norms. In this experimental research, ordinary people's endorsements of certain distributive principles provide social scientists with information on the variety, complexity and context-specificity of everyday justice judgments. While these popular judgments do not suffice to justify any principles of distributive justice, they are important factors for these principles' feasibility or legitimacy (Swift 1999, p. 361). Accordingly, results from these experimental studies are important also to legal theorists. As e.g. Hoffman and Spitzer (1985, p. 261) point out, it is a widely accepted argument that the law should (or does) reflect commonly held moral norms.

[1]The english translation of the AGG can be found e.g. under `https://www.antidiskriminierungsstelle.de/EN/homepage/_documents/download_general_act_on_equal_treatment_2018.pdf?__blob=publicationFile&v=1`

[2]All judgments are available in the dejure.org (2021) database.

An extensive literature confirms that the relatively easily observed forms of discrimination, such as unequal pay for equal work, are perceived as "unfair" discrimination and need to be compensated. Examples for such experimental studies finding that laypersons compensate randomly determined unequal opportunities and discriminatory wages are the studies by, e.g., Konow (2000), Cappelen et al. (2010), Rodriguez-Lara and Moreno-Garrido (2012), Mittone and Ploner (2012), and Lefgren et al. (2016). Other studies examine more closely how inequality of opportunity is compensated when individuals have some responsibility (e.g. Cappelen et al. 2013a, Mollerstrom et al. 2015, and Akbaş et al. 2019). The results of all these studies show that responsible choice matters, and that a lack of EOP an individual is not responsible for is indeed often perceived as unfair and hence compensated.[3]

Nevertheless, in the case of unequal pay for equal work, individuals continue to perform work and acquire entitlements to their wages. In the case of discriminatory exclusion from income opportunities, in contrast, they do not work at all, just like shirking individuals who deliberately choose not to work and remain idle. So far, the scenario of complete exclusion from an income opportunity, although common in "real life", has been largely neglected in the behavioral and experimental economics literature. From a legal point of view, this issue has already been clarified: §15 AGG stipulates that employers are obliged to compensate a discriminated party for the damage caused by discrimination. This also applies if plaintiffs are not hired due to the discrimination. In this case, they are entitled to compensation of up to three months' gross salary of the job offered. Such a scenario is of particular interest, since plaintiffs do not work at all due to the experienced discrimination, at least not in the desired position.

Whether the compensation for exclusion guaranteed in §15 AGG is consistent with the norms of laypersons and supported by their everyday judgments has not been experimentally investigated so far. This dissertation therefore aims to shed light on this question and contribute to the literature by experimentally investigating the social evaluation of discriminatory exclusion from income-generating activities.

While Akbaş et al. have already included some form of exclusion in their study and model inequality of opportunity by depriving one subject of free production choices, this thesis is the first to more specifically address discriminatory exclusion *against* an individual's choice to participate in income generating activities, henceforth also called production. Furthermore, in contrast to Akbaş et al., who consider the distribution decisions of impartial observers, this thesis investigates the distribution decisions of involved dictators in order to analyze the trade-off between maximizing one's own income and giving up own income in order to compensate someone for being discriminated.

[3] Note that in this dissertation, the terms justice and fairness are used as interchangeable synonyms.

To examine the social evaluation of discriminatory exclusion, this dissertation analyzes differences in dictator giving either to a) shirking recipients who are fully responsible for their suspension of competitive production, or b) excluded recipients who choose to participate but, against their will, have to suspend production due to discrimination in terms of an arbitrary criterion and hence lack responsibility for not working. It also explores the question of whether compensation for exclusion is in line with the responsibility principle.

Therefore, Section 2.1 begins with an overview of the normative discourse on principles of distributive justice in political philosophy. It presents the literature on responsibility and Equality of Opportunity as well as other principles of distributive justice, such as utilitarianism, egalitarianism, need, desert and libertarianism. There is no established consensus in the political philosophy on how "just" (re)distribution looks like, and political philosophers reasonably justify fundamentally opposed justice principles and various graduations in between. The related empirical literature is, however, less polarized and finds evidence for the application of all principles, depending on context. The empirical and mostly experimental literature which covers (some of) these principles from a predominantly economic perspective is presented in Section 2.2.

Chapter 3 follows Arneson (1989, 1990) and Cohen (1989) in defining the responsibility principle as:

Individuals should bear the consequences of their voluntary choices when these concern welfare or resource gains and losses, but be compensated for disadvantages they suffer directly from arbitrarily restricted access to earning opportunities without the possibility to have avoided or overcome these restrictions (see Section 3.1.2).

Furthermore, Chapter 3 defines exclusion and a model of financial compensation in case thereof. Besides a general rule, the model incorporates the availability of an alternative income in case excluded individuals manage to find other work than their initially intended production, which reduces the disadvantage they suffer from discrimination. It also considers that compensation for exclusion might be influenced by the type of production an individual chooses to participate in but is excluded from and its associated level of control, as previous results by Mollerstrom et al. (2015) suggest that choices can impact compensation even for unrelated situations.

The predictions of this model are tested using a two-treatment strategic dictator game, the exact design of which is presented in Chapter 4. In the experiment, recipients can "shirk" or "work", i.e. suspend or participate in income generating competitive production. This can be done according to their choice in the *No Exclusion* treatment. However, in the *Exclusion* treatment, some recipients who choose to work are excluded on the basis of an arbitrary criterion, so that they are not responsible for their suspension of production. Dictators in the *No Exclusion* treatment make donation decisions for shirking recipients who are fully responsible for their suspension of competitive production. Dictators in the *Exclusion* treatment make donation decisions for excluded recipients who choose to participate but must suspend production against their will. The treatment effect in dictator giving in this between-subjects design can be interpreted as compensation for exclusion.

The experiment is complemented by a within-subject variation of the safe amount given to recipients as alternative income in case they suspend competitive production, as well as by a variation of three different production tasks (a lottery, a real-effort task and a strategic game). These variations are used to examine whether dictator giving is influenced by the availability and level of alternative income, and/or by the level of control of the production type that a recipient chooses to participate in but is excluded from.

The experimental results are presented and analyzed in Chapter 5 and support the model defined in Chapter 3. They show that subjects are indeed compensated for being excluded, albeit only at the extensive and not the intensive margin: in case of exclusion, dictators are more likely to give anything, but the respective donation amount given to excluded recipients is similar to the donation amount given to shirking recipients. The higher likelihood for dictator giving in case of exclusion also prevails when excluded recipients do not suffer from financial disadvantages thanks to a high alternative income, which suggests that exclusion has a negative intrinsic value and is compensated regardless of its financial consequences. Donation amounts do, however, decrease as soon as alternative incomes are available, which is in line with Boulding's (1988) theory of a need threshold. Furthermore, the experimental data support the findings of Mollerstrom et al. (2015) that responsible choices matter even in unrelated situations: dictators reduce compensation for exclusion if recipients choose to participate in production games that are associated with lower level of controls (and therefore higher levels of perceived risk).

Finally, Chapter 6 concludes this thesis with a short summary and contextualization of the main arguments and results. The latter show that if an individual wants to work, but is prevented to do so due to discrimination, compensation as outlined by §15 AGG is reflected in the everyday judgments and giving decisions of this experiment's participants. Thus, this thesis' results support the appropriateness and social acceptance of instruments such as the AGG and confirm the initial statement by Roemer, that EOP is the most universally supported conception of justice.

2 Distributive Justice: Normative Principles and Empirical Findings

No single principle [...] can possibly capture the richness of human thinking about justice. (Miller 2013, p. 3)

Despite or due to the impossibility to ultimately define justice, a vast amount of literature from different academic fields has been addressing that very problem. This thesis ignores several angles and aspects of justice and keeps a very narrow focus on distributive justice, and especially the distributive justice principles that have been addressed and examined by preceding experimental economic studies. Therefore, Section 2.1 first presents in a nutshell how the normative political-philosophical literature developed utilitarianism, egalitarianism, need, desert, libertarianism, as well as the responsibility principle as different theories of distributive justice. The normative perspective is then complemented with a selected overview on empirical findings on distributive justice from the experimental economic literature that are relevant for this thesis in Section 2.2. In both perspectives, the responsibility principle is emphasized (Sections 2.1.6 and 2.2.3). As can be expected, no universally valid principle of justice is emerging neither from the normative nor from the empirical perspective. Still, the various experiments confirm that all theories of distributive justice are indeed applied by individuals, even if only partially or in combination with each other. The different extent of application depends on the specifics of the different contexts.

2.1 Normative Principles of Distributive Justice

Obviously, the very extensive discourse on justice can be traced back to antiquity, and all philosophical concepts are not only rooted in the preceding philosophical tradition but also in constant exchange of ideas with other current philosophers. To nevertheless keep this section at a reasonable length, it includes only the most important ideas on distributive justice, and only for the purpose of getting a rough idea of the theoretical foundations of redistribution in general, and of the topic of this thesis — compensation for exclusion — in particular. Accordingly, this incomplete overview on distributive justice principles omits the older philosophical concepts and starts with the principle of utility, or *utilitarianism*, in Section 2.1.1, mainly because it has influenced the development of economics as an independent field (Capaldi and Lloyd 2016, p. 144). Furthermore, it is the shared political-philosophical foundation of all other justice principles presented here, the first of which is the equality-postulating *egalitarianism* in Section 2.1.2. This is followed in Section 2.1.3 by the principle of *need*, which postulates the ensuring of basic needs, in Section 2.1.4 by the principle of *desert*, according to which income distributions should reflect the desert, e.g. effort, of individuals, and in Section 2.1.5 by the theory of *libertarianism*, which is fundamentally opposed to redistribution. Finally, Section 2.1.6 presents the *responsibility* principle, which (roughly) calls for compensation for disadvantages an individual cannot be held responsible for and provides the basis for this thesis' theoretical and empirical examination of the compensation for discriminatory exclusion.

2.1.1 Utilitarianism

The first systematic account of *utilitarianism* is formalized by Bentham (1748–1832) from the late 18th to the early 19th century. His book "An introduction to the Principles of Morals and Legislation" is printed first in 1780, completed in 1789 and printed in an updated version in 1823 (Bentham 1823, pp. xxxvii–xliii). His formalization implies that social policy can be evaluated by calculating the consequences of alternative courses of action (Capaldi and Lloyd 2016, p. 144). According to Bentham, any action is in line with the principle of utility, "when the tendency it has to augment the happiness of the community is greater than any it has to diminish it" (Bentham 1823, pp. 12–13). Bentham's principle is built on a rich tradition of utilitarian thinkers, such as the theological utilitarians and many other important philosophers, in particular Rousseau, Hobbes, Locke, Hume and Smith (Rosen 2003, p. 3), neither of whom will be further addressed in this thesis.[4]

Bentham's line of thought is further developed by his student Mill (1806–1873). In his work "Utilitarianism" from 1861, Mill defends Bentham's premise that any policy should offer the greatest good for the greatest number and explores some of central criticisms directed at utilitarianism, such as hedonism and the strong deviation of the general understanding of fairness (Mill 1861). Mill is a strong defender of individual liberty, which he extensively justifies in his very influential essay "On Liberty" from 1859. He considers various aspects of liberty and not only preferred economic activity to be unrestrained by state interventions, but also embraces civil liberties such as women's rights (Mill 1890). In this context, he prioritizes individual autonomy over economic efficiency, which he views only as a means to an end (Capaldi and Lloyd 2016, p. 96).

Besides Bentham and Mill, Sidgwick (1838–1900) also prominently defend utilitarianism. Even beyond the time of these classical utilitarians, the concept of utilitarianism dominates the political philosophical discourse, which (mostly) consents that all actions and social policies should aim to the maximization of a society's efficiency, i.e. the overall welfare or income in a society. When Rawls (1972) dissents to

[4] Especially Hume and Smith are important to Bentham: his understanding of utility as a good, pleasure generating virtue explicitly refers to Hume's writings, and Smith's incorporating of utility in the economic sphere strongly influences Bentham's views on individual liberty and competitive markets (Rosen 2003, pp. 48–49, p. 59, pp. 80–81).

utilitarianism, arguing that its aggregative character makes the distribution of welfare irrelevant in justice considerations unless to break ties (ibid., p. 26), especially Harsanyi still defends utilitarianism and argues that when rational, self-interested individuals had to pick one of different decision rules without knowing which result they would get, they would choose the expected-utility maximization instead of the maximin principle (see Section 2.1.2) proposed by Rawls as an alternative (Harsanyi 1975, pp. 44–45). Nevertheless, Rawls's position is extremely influential and has replaced the dominance and general acceptance of utilitarianism by establishing the theoretical foundations of *egalitarianism*. Utilitarianism is also mostly neglected in the context of this dissertation. Egalitarianism, however, is often considered in the argumentation and now outlined in the following Section 2.1.2.

2.1.2 Egalitarianism and Equality of Resources

In 1971, Rawls publishes his influential work "A Theory of Justice" and hereby revives the normative debate on justice in political philosophy, which before has rather been focusing on metaethics or considering the at the time prominent utilitarianism (Vallentyne 2014, p. 86). Rawls however radically rejects utilitarianism based on his criticism that the focus on only the sum of satisfactions, but not their distribution, neglects the distinction between persons and might be used to justify a violation of the liberty of a few for the gains of the many (Rawls 1972, pp. 22–27). Moreover, he rejects the idea of impartial observers being ideal to establish a just society: since impartial observers take up a general perspective and individual pleasures cancel out others pains, their ultimate approval expresses the total welfare result and is equivalent to the utilitarian position (ibid., pp. 186–188). Instead, Rawls argues for an *egalitarian liberalism*, or *egalitarianism*, that would emerge if individuals in their (hypothetical) "original position" would choose their preferred justice principles behind a "veil of ignorance".[5] This veil of ignorance implies that individuals are uncertain about their share of income, and thus would choose the two following principles of justices[6] which are the base of egalitarianism (ibid., p. 302):

1. *Each person is to have an equal right to the most extensive total system of equal basic liberties compatible with a similar system of liberty for all.*

2. *Social and economic inequalities are to be arranged so that they are both: (a) to the greatest benefit of the least advantaged [...] and (b) attached to offices and positions open to all under conditions of fair equality of opportunity.*

[5]The veil of ignorance is an often used concept in behavioral economics. While the term is coined by Rawls, the concept has already been used by earlier philosophers, such as Friedman (1953), and it's formalization in economics has most notably been helped by Harsanyi (1953), (1955).

[6]These principles are arranged in a serial order, so that all social and economic arrangements following the second principle must be consistent with the first principle of equal basic liberties for all (Rawls 1972, p. 61).

The *maximin criterion*, the operational version of the second principle which posits that inequalities should be to the greatest benefit of the least advantaged, is another very influential aspect of Rawls's book. It has been adopted by the economic sphere and introduced e.g. to calculate optimal taxation and redistribution schemes, and is often used as point of reference in the current fairness literature (Mongin and Pivato 2021, pp. 1500–1501). Rawls argues that of several efficient distributions,[7] a society will choose the particular distribution that fulfills the *difference Principle*, which maximizes the expectations of the most disadvantaged individuals (Rawls 1972, pp. 75–80). This is because under the veil of ignorance, people would follow the *maximin* rule and rank the alternative income distributions by their worst possible outcomes and select the distribution of which the worst outcome is superior to the other alternatives' worst outcomes (ibid., pp. 152–153).[8]

With respect to balancing the maximin principle and other fairness principles, Rawls uses the lexicial difference principle established by Sen (1970): first, the welfare of the worst-off individual[9] is maximized, before repeating this welfare-maximization for each next-upward position until the best-off individual respectively, under the condition that this does not affect the least advantaged (Rawls 1972, pp. 82–83). This recursive application of the maximin principle is called the *leximin criterion* and breaks any indifferences left by maximin (Mongin and Pivato 2021, p. 1500).

A major impact of Rawls's "Theory of Justice" stems from his radical argument that "justice as fairness requires that all primary goods be distributed equally unless an unequal distribution would be to everyone's advantage" (Rawls 1972, p. 150),[10] which is taken up, refused or refined, in probably all works on distributive justice.

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[7]A distribution is considered efficient when it is Pareto optimal, i.e. when there is no way of improving the situation for some individuals without doing worse for others (Rawls 1972, p. 67). Pareto optimality is a standard concept in economics and has been introduced by Vilfredo Pareto in 1909 (a recent translation has been published in 2014). Besides Rawls, several other authors have addressed fair Pareto-efficient allocations, such as e.g. Varian (1974), or Pazner and Schmeidler (1978).

[8]One point of criticism is e.g. that Rawls does not differentiate between relative and absolute disadvantages in his discussion of the maximin criterion (Capaldi and Lloyd 2016, p. 175).

[9]Temkin (1993) stresses the distinction between the worst-off individual and the worst-off group is important, as "egalitarians are affected by inequality in a highly personal way" (ibid., p. 305), so that improving the *average member* of the worst-off group is not sufficient (ibid., pp. 91–117).

[10]It is important to note that while also embracing equality as ideal, Rawls's "Theory of Justice" is fundamentally different from Marx's socialist vision of equality. Marx is, like the utilitarianists, influenced by Rousseau, Kant, Locke, Smith, and others, but whereas the utilitarianists, especially Mill, emphasize the importance of liberty, Marx "s[eeks] to revive, recapture and update the ancient conception of community" (Capaldi and Lloyd 2016, p. 111). Thus, in this respect Marx's class focused interpretation of equality is opposed to the utilitarian idea of individual liberty (ibid., p. 122), and also to Rawls's, who bases his two fairness principles on these very liberties (Rawls 2003, pp. 425–437). Thus, Rawls's *egalitarianism* is strongly rooted in the utilitarian tradition and centered on individuals and their liberties, just like *libertarianism* and all other justice principles that have been taken up by experimental behavioral economics and are presented in this thesis. Despite their shared roots, their view on distributional fairness diverges quite strongly, as demonstrated in this section.

Some philosophers suggest different approaches to equality, such as Sen (1980), who centers the equality of individuals' basic capabilities, and suggests an unequal resource allocation to account for the different need of individuals, depending on e.g. health or body size. Others, such as Nagel (1991), support Rawls's idea that equality of resources in a final distribution has an intrinsic value of its own. Dworkin (1981a) on the other hand defends initial equality of resources, and emphasizes the importance of an egalitarian economic market for resource allocation via auction. Since all individuals enter on equal terms and have the same purchasing power, they all end up with the same value of resources, but an individual bundle of resources. This way, each individual can choose how they want to live their life with respect to the cost that these choices impose on others (Dworkin 1981b, pp. 284–290).

Furthermore, Dworkin stresses that his theory aims to make people's impersonal resources sensitive to their choices but insensitive to their circumstances (Dworkin 2001, p. 323), and formulates two main ethical principles for a government: the first being "that it is important that the lives of people go well, and it is equally important that each person's life go well" (ibid., p. 324), the second that "nevertheless each person has a special responsibility for his own life that includes deciding what kind of life is appropriate to him, and how to best use his resources to secure it" (ibid., p. 324). This idea of responsibility, already present in his groundbreaking essay "What is equality?" from 1981, leads several more philosophers to refine his attempt to reconcile justice and choice and thereby establishes the strand of literature on the responsibility principle, presented in Section 2.1.6. Thus, Dworkin is considered the "chief architect of the luck egalitarian position" by Arneson (2004, p. 1), although Dworkin himself rejects that label for himself (Sher 2014, p. 2).

As popular as these different egalitarian theories have been, they also face a lot of criticism. Two major arguments against equality in resources are the *slavery of the talented* and the *leveling down objection*. The *slavery of the talented* problem is identified and coined by Dworkin (1981b, p. 312) himself, who notes that a just distribution should only reflect individuals' leisure and effort choices, but not different endowments of talent. *Slavery of the talented* would arise if different talents were treated as a resource and auctioned at the market: in this case, the time of talented people would be very valuable, so that they would either work a lot or pay a large part of their initial endowment for their leisure and hence suffer from deprivation. Since people should neither benefit from nor be penalized for their superior talent, Dworkin refuses the market solution and instead suggests a form of income tax to redistribute and neutralize the effects of different talents (ibid., pp. 310–313).

The term *leveling down objection* is coined by Parfit (1995, p. 17). He fleshes out an initial point of criticism by Nozick (1974) who has questioned whether "something [would] be done to remove [...] exceptional assets and talents" if these "couldn't be harnessed to serve others" (ibid., p. 229). Parfit (1995) graphically translates this argument into the example of a society in which half of the people are blind, and argues that according to egalitarianism which intrinsically values equality, “it would be in one way better if we removed the eyes of the sighted” (Parfit 1995, p. 17), even if these cannot be given to the blind and hence do not improve their situation.

While this leveling down would be a consequence of the concept of radical equality, there are barely any sophisticated versions of egalitarianism that have such a narrow view. Instead several authors argue that a general claim for equality exists, but that this claim “is overridden in certain circumstances by reasons which are relevant to unequal treatment” (Raphael 1980, p. 5). Indeed, many egalitarians have a pluralist view and additionally adopt e.g. the Pareto principle, which leads to a social ranking in which the better-off individuals also have a positive weight, although less than the worse-off (Fleurbaey 2015, pp. 212–213).

Temkin (2003, p. 63) also supports the idea that a “reasonable egalitarianist” is a pluralist and considers other independent, normatively significant fairness ideals besides equality. Nevertheless, he states that self-identified egalitarians who argue that lowering the better-off to the level of worse-off can never morally improve a situation, are “simply nonegalitarians” (Temkin 1993, p. 69) and defend another, overlapping justice principle such as the priority principle instead. In a way, his view is replicated in the experimental literature, which also tries to separate the different justice principles as precisely as possible (see Section 2.2.1): it understands egalitarianism as pure inequality aversion and a preference for equal sharing regardless of other characteristics (see e.g Hoffman and Spitzer 1985, p. 264). Still, it rarely explicitly explores the leveling down objection. Both of these aspects apply to this thesis as well.

2.1.3 The Principle of Priority and Need

Alongside with the leveling down objection, Parfit (1995) establishes the *priority principle*, which he defines as “[b]enefiting people matters more the worse off these people are” (ibid., p. 19). He argues that the chief difference between prioritarianism and egalitarianism is the fact that prioritarians do “not believe in equality” (ibid., p. 22) and therefore are “concerned only with people’s absolute levels” (ibid., p. 23), but not, like egalitarians, about their relative levels. Thus, although prioritarianism also often postulates an equal distribution, this would not be justified with a reduction of inequality, but instead with benefiting the worst-off for moral reasons.[11]

Furthermore, Parfit states that prioritarians do not care whether some people are *better* off than others (ibid., p. 23). This shows that prioritarianism is closely related to the principle of *need*, which postulates that *all* individuals’ needs in a society, not only the deserving poor, must be covered to achieve social justice (Boulding

[11] This distinction based on intentions only is criticized as unconvincing e.g. by Fleurbaey, who states that all distributions preferred by a prioritarian would also be preferred by egalitarians, since “one can simply never find an example where, in a non-controversial way, the situation of the worst off improves while inequality rises” (Fleurbaey 2015, p. 209).

1988, p. 54), but provides no further restrictions to distribution once these needs are covered. As Frankfurt (1987) puts it, it is not important that "everyone should have *the same*, but that each should have *enough*" (ibid., p. 21, emphasis in the original).

In fact, most scholars endorsing the principle of need, such as Raphael (1980), Frankfurt (1987), Braybrooke (1987), Boulding (1988), Nussbaum (2000, 2011), Crisp (2003), and Sher (2014) assume that above the threshold of ensured basic needs, people are free in their decisions and can receive benefits according to efficiency or merit. While it is rarely disputed that basic needs should be covered, how these needs are actually defined is less clear. Due to the given diversity of people, needs within a society can be expected to be heterogeneous (Doyal and Gough 1984, 1991), especially since needs encompass not only a biological minimum, but instead goods and activities that make up a normal human life and as the term is easily "morally loaded" (Miller 1999, pp. 203, 210).[12] Thus, the definition of needs and the extent of their realization should depend on a consensus within the society and can be expected to vary "in different societies and at different periods of history, depending both on economic circumstances and on the level of social morality" (Raphael 1980, pp. 54–55).

Several egalitarian authors, such as Dworkin and Nagel, are however not convinced by the need principle. They argue that justice is not a question of fulfilled basic needs, "but of equal status" (Dworkin 2001, p. 198), and that the "method of pairwise comparison with priority going to the lower member of the pair simply does not cease to apply above the level of basic needs" (Nagel 1991, p. 70). Furthermore, the principle of need has no universal solution for a situation in which resources are scarce and different needs need to be weighed against each other (Miller 1999, pp. 204–225).

Nevertheless, the need principle is an often used and important concept, and although not explicitly explored by this thesis, it is relevant for the interpretation of results. The need principle is mostly, e.g. by Benbaji (2006), Siebel and Schramme (2020), or Nullmeier (2020), supplemented with the principle of desert, which is presented in the following section.[13] Boulding argues that the need principle is the frequent compromise of the otherwise irreconcilable justice concepts of *equality* and *desert* (Boulding 1988, p. 53). Raphael even states that the combination of equality in basic needs and desert-based rewards above these needs, which reminds of the set-up of existing welfare states, ensures the protection of all individuals' interests and freedom (Raphael 1980, p. 37).

[12]In the economic context, Deaton and Muellbauer (1980) have established the concept that necessities or basic needs are those goods for which people have a income elasticity less than one. This idea is empirically supported by Baxter and Moosa (1996), who define food, energy, clothing, and housing as basic goods and use macro level data to show that basic needs expenditure is indeed stable whereas other consumption is volatile and varies with time and income.

[13]Adler (2018) also includes the principle of desert into prioritarianism.

2.1.4 Principle of Desert, Equity and Accountability

The principle of desert is, like equality, an ancient idea, and postulates broadly that people should get what they deserve. How exactly this idea is interpreted is discussed by several contemporary philosophers. Some philosophers argue that the deserved reward for useful labor should accurately reflect the value of an individual's labor, so that in case of a joint effort, the share each individual deserves of a product's total value reflects their comparative contribution to that product (Miller 1989, pp. 56–59). Others, e.g. Lamont (1994, pp. 50–52), argue that such an understanding conflates desert with entitlements.[14] A major interpretation of desert is that people should be rewarded according to the effort they spend (see e.g. Sadurski 1985), which is strongly rooted in Locke's argument that individuals deserve the resources they have accumulated or developed through their expenditure of effort (c.f. Hoffman and Spitzer 1985, p. 264).

The principle of desert is strongly rejected by Rawls, who criticizes "a person's moral worth does not vary according to how many offer similar skills, or happen to want what he can produce" (Rawls 1972, p. 311), although he agrees that distribution by effort is the closest to rewarding moral desert. However, since "the effort a person is willing to make is influenced by his natural abilities and skills and the alternatives open to him" (ibid., p. 312), Rawls rejects the idea of distribution by effort because it would benefit the better endowed. Other authors, such as Miller (1999) and Scanlon (2013) do not reject the desert principle in general, but emphasize that while a just society ensures that people receive the benefits to which they are entitled, the desert principle alone is not sufficient to fully justify a society's distributive institutions, which need some additional justifying features. Sher (1979, 1989), on the other hand, defends the desert principle, stating that "no satisfactory theory of justice can afford to ignore personal desert" (Sher 1979, p. 376).

The principle of desert has been translated into the principle of equity primarily by sociologists and social psychologists, most notably Adams (1965) and Homans (1974) (see Nicklisch and Paetzel 2020, p. 171). Walster et al. (1978) refine the equity formula to strict proportionality of intrinsic inputs, which are only vaguely defined and may include effort expended or resulting output as well as personal characteristics such as intelligence or social status (Konow 1996, p. 14; 2003, pp. 1211–1213). Therefore, no testable hypotheses can be derived without further restricting the relevant inputs (Hoffman and Spitzer 1985, p. 265), so most experimental studies investigating the equity principle assume that effort and/or output are the only factors that define deserts.

[14] The main difference between desert and entitlement is that entitlements are the value attached to the results of preceding actions or characteristics of an individual, whereas desert refers only to current contributions or efforts (Hülle et al. 2018, p. 668) and depends on voluntariness, thereby reflecting peoples' goals and values (Lamont 1994, pp. 50–52).

Konow further specifies the equity principle as the *accountability principle*, which "requires that a person's entitlement or fair allocation (e.g., of income) vary in proportion to the relevant variables which he can influence (e.g., work effort), but not according to those which he cannot reasonably influence (e.g., a physical handicap)" (Konow 1996, p. 14). For him, the accountability is fundamentally different from the principles previously considered in its impact on distributive justice, as it can be counted as procedural justice, whereas equality, need and efficiency are merely distributional goals. Therefore, Konow claims that efficiency and need as distributional principles are considered "just", because their distributions are "good", while distributions in accordance with the accountability principle are considered "good" because they are "just" (Konow 2001, p. 156).

In certain contexts, the accountability principle as defined by Konow overlaps with the responsibility principle which is central to this thesis (see Sections 2.1.6 and 3.1.2), because both principles reject inequalities that result from external, uninfluenceable factors. This is particularly relevant for the experimental studies presented in Sections 2.2.2 and 2.2.3 which examine redistribution in an effort context with arbitrary influences, and which are hence closely related to this dissertation.

So far, all of the principles of distributive justice presented have postulated some redistribution, the extent of which depends on the particular principle and context. This changes in the next section, where the principle of libertarianism is presented, which can generally be understood as a fundamental rejection of any institutionalized form of redistribution.

2.1.5 Libertarianism

Libertarianism refers to an understanding of justice that emphasizes people's individual liberties and entitlements, and is therefore opposed to any form of institutionalized redistribution. It is established primarily by Nozick in 1974, when he explicitly refutes Rawls' "Theory of Justice" in his book "Anarchy, State and Utopia". Instead, Nozick draws on the ideas of earlier liberal thinkers, such as Locke (1689) and Hayek (1960), to develop his own theory of justice, the *entitlement theory*. This theory borrows closely from several of Locke's arguments, such as that all individuals have equal rights and are perfectly free in their accumulation of private property, which leaves nobody worse off and should be protected by the state (Nozick 1974, pp. 174–175; Locke 1689, pp. xxiv–xli). It accepts all inequalities, e.g., on the basis of age, birth, and property, and makes the fulfillment of distributive justice conditional only on all individuals being entitled to their holdings by the principles of justice in acquisition and transfer, or by the principle of rectification of injustice (Nozick 1974, p. 151):

1. *A person who acquires a holding in accordance with the principle of justice in acquisition is entitled to that holding.*

2. *A person who acquires a holding in accordance with the principle of justice in transfer, from someone else entitled to the holding, is entitled to the holding.*

3. *No one is entitled to a holding except by (repeated) applications of 1 and 2.*

Nozick defends a minimal state without redistributive purposes, limited to basic functions such as enforcing contracts or protecting its citizens from crimes such as violence, theft, and fraud, which would come into being through an invisible-hand process (Nozick 1974, pp. 26, 88–119). He strictly opposes taxation, which he considers a violation of freedom and an illegitimate system of forced labor (see, e.g., ibid., pp. 169–170).[15] According to him, redistribution to benefit the poor can only occur through voluntary charitable transfers that are consistent with entitlement theory because they serve a purpose and help the one transferring gain something or achieve a goal (ibid., p. 159). As he puts it, such legitimate transfers can be summarized as "[f]rom each as they choose, to each as they are chosen" (ibid., p. 160).

Similar to egalitarianism, libertarianism is defended and developed by contemporary writers such as Mack (1995), Shapiro (2007), and Brennan (2012). An important development, for example, is so-called left-libertarianism, which assumes that "natural resources (land, minerals, air, and the like) belong to everyone in some egalitarian sense. [...] Merely being the first person to claim, discover, or mix labor with an unappropriated natural resource does not [...] generate a full private property right in that natural resource" (Vallentyne et al. 2005, p. 201). This contrasts Nozick's libertarianism, which invokes certain property restrictions only when ownership of a resource worsens the situation for others, such as appropriating "the only water hole in a desert and charge what he will" (Nozick 1974, p. 180).

However, the central position of self-ownership and rejection of taxes is defended. Therefore, libertarianism is typically interpreted as being fundamentally opposed to any form of redistribution.[16] Accordingly, strict opposition to redistribution found e.g. in economic experiments is interpreted as libertarianism by the empirical economic literature on distributive justice, and throughout this thesis (see Section 2.2). Whereas e.g. Tomasi (2012) tries to reconcile economic liberty with a fair distribution of goods and opportunities, Nozick rejects redistribution also in situations when unearned natural assets or accumulated effects of individuals' actions and transfers lead to unequal opportunities. According to Nozick, inequality of opportunity cannot be justly equalized, because this would require resources or actions to which other individuals may already be entitled (Nozick 1974, pp. 235–238). He acknowledges that his view conflicts with the principle of equality of opportunity,

[15] Dworkin emphasizes, however, that there is no conflict between liberty and equality, since he considers liberty as a resource. Thus "the rights to liberty [...] are a part or aspect of distributional equality, and so are automatically protected whenever equality is achieved" (Dworkin 2001, p. 133).

[16] Although Nozick includes voluntary donations in his libertarian theory (Nozick 1974, p. 159).

which is presented in the following section, and is "to many writers [...] the minimal egalitarian goal, questionable (if at all) only for being too weak" (ibid., p. 235).

2.1.6 The Responsibility Principle and Equality of Opportunity

As mentioned in Section 2.1.2, the egalitarian philosopher Dworkin states that "each person has a special responsibility for his own life that includes deciding what kind of life is appropriate to him, and how to best use his resources to secure it" (Dworkin 2001, p. 324). This idea of making people's impersonal resources sensitive to their choices but insensitive to their circumstances (ibid., p. 323) inspires several more philosophers to refine this attempt to reconcile justice and choice and thereby establishes the strand of literature on the *responsibility* principle, the application of which is the main focus of this thesis.

Especially Arneson (1989, 1990) and Cohen (1989) shift the discussion on distributive justice to the influence people have on their income situations and hereby develop the responsibility principle, with the central assumption of *equality of opportunity*. Equality of opportunity can be considered as the "probably [...] most universally supported conception of justice in advanced societies" (Roemer 2002, p. 455),[17] and Arneson initially even uses the term *equal opportunities for welfare* as fairness ideal, before he starts to use the term *responsibility* in later papers, e.g. Arneson (1999). He argues that "the idea of equal opportunity for welfare [is the] best interpretation of the ideal of distributive equality" (Arneson 1989, p. 77), and that this interpretation also avoids slavery of talented. He describes three different fair distributions, in which persons have

- *either* equal opportunities and negotiation abilities,
- *or* unequal opportunities counterbalancing unequal negotiation abilities,
- *or* equal opportunities and unequal negotiation abilities due to own (responsible) choices.

He shows that if the assumption of hard determinism came true, then this fairness ideal would be the same with equality of welfare, but that soft determinism and indeterminism lead to different distribution results (ibid., p. 86). A central argument of his theory is that it is "morally fitting to hold individuals responsible for the

[17] Of course, there are other, earlier defenders of some form of equal opportunities, such as Friedman (1953, p. 290), who argues that whereas inequalities between individuals based on their deliberate decisions whether or not to participate in income generating lotteries reflect tastes and preferences of society members and can be considered fair, inequalities imposed on individuals from the outside pose a very different normative issue, and Raphael (1980, p. 53), who defines equal opportunities as "the maximum opportunity for all to develop the potentialities they have, and [...] discrimination in the provision of a particular type of opportunity for some and not for others should depend on the potentialities that the prospective recipients have, instead of depending on 'irrelevant' considerations such as birth or wealth".

foreseeable consequences of their voluntary choices" (Arneson 1989, p. 88), especially when these are concerning welfare or resource gains and losses.

Cohen (1989) also establishes the responsibility principle, influenced by Arneson's (1989) ideas, but primarily in response to considerations by Sen (1976) of what aspects of a person's condition should count in a fundamental way for egalitarians, and as a critique to Dworkin (1981a). Coming from this different perspective, Cohen calls his principle *equality of access to advantage* (Cohen 1989, pp. 916–921). He emphasizes that while differences in advantages are unjust unless they result from differences in genuine choice, this principle does not propose to equalize genuine choices (Cohen 1990, pp. 24–25). As for the effect of responsibility on compensation claims, he argues that if a person is disadvantaged, and could have avoided that, she has no claim for compensation. If she however could not have avoided the disadvantage and it is not possible to overcome the disadvantage, then her disadvantage should be compensated. If the disadvantage was unavoidable but possible to overcome, then the person can ask for subsidies for the effort to overcome the disadvantage, but not for compensation for the disadvantage, unless overcoming is more costly than the compensation for the disadvantage without overcoming it. Cohen's definition of disadvantages as deficiencies of both resources and welfare remains however quite vague, as he considers this a difficult normative question (Cohen 1989, pp. 920–921). Roemer (1993, 2021) also endorses the responsibility principle and substantially develops the theory and model of equal opportunities, according to which a society should equalize opportunities for all members, but hold them responsible for their own choices.

There are some other authors who can be understood as advocates of the responsibility principle, although they define themselves as egalitarians, such as Nagel and Temkin. Nagel (1991) states that "merely having less than someone else is not in itself counted as an evil" (ibid., p. 72) and that inequalities resulting from individuals' own responsible choices "should not be objectionable to an egalitarian" (ibid., p. 71).[18] Similarly, Temkin (1993) argues that "deserved inequalities are not bad *at all.* Rather, what is objectionable is some being worse off than others *through no fault of their own*" (ibid., p. 17, emphasis in the original). Nagel explicitly lists four sources of inequalities, namely discrimination, class, talent, and effort, and argued that only effort can count as a legitimate cause of inequalities. While inequality resulting from the other three sources is always unacceptable in an egalitarian framework, he argues that discrimination is the most objectionable source of inequality, since "it involves deliberate imposition of disadvantages on some by others [...]" (Nagel 1991, p. 108), whereas class and talent are more systemic sources for which no one is specifically responsible (ibid., pp. 102–109). Temkin also stresses the influence of class, as he

[18]Accordingly, Nagel (1991, pp. 71–72) suggests that only advantages and disadvantages that people are not responsible for, such as their birth or their life's framework, should fall under the egalitarian principle.

cautions that equality of opportunity is not sufficient to achieve distributive justice. At the example of doctors and ditchdiggers, he states that the massive inequality between these two groups is unfair, but cannot be altered by equal opportunities, since the latter would only change the composition of these two groups, but not the inequality between them (Temkin 1993, p. 300).

All in all, it becomes obvious that these different interpretations of the responsibility principle identify several similar situations in which individuals should be compensated, but are mostly less clear in determining how much compensation is just[19] and how this compensation should take place. Due to the obvious complexity, Miller (2013) argues that the responsibility principle is too demanding to be implemented on a national scale, as some agency would be needed to monitor and evaluate how much of the state's subjects' resource (or whatever benefit is chosen as relevant) level arises from luck and how much from choice and neutralize the effect of luck after estimating the resource level without luck's influence (ibid., p. 2).

Sometimes, the responsibility principle based on Arneson and Cohen is understood in a way that individuals can never be held responsible for outcomes influenced by luck. This would imply that they should not bear the consequences of risky decisions, and is often referred to as *luck egalitarianism*, a term coined by Anderson (1999, p. 289; also see Fleurbaey 2015, pp. 153–154, and Barry 2006, p. 89).[20] This interpretation is defended by Arneson (2004), who argues that gamblers are equally deserving, or that the some consequences from risky choices can be out of proportion (ibid., pp. 4–6). Other authors, e.g. Le Grand (1991), suggest that risk takers should be fully insured to receive their expectation value even in case of bad option luck, but be able to benefit from the risk taking if the outcome is above this expectation value. Most authors however follow Dworkin's distinction between *option luck*, such as the rise or fall of a bought stock, and *brute luck*, such as being hit by a meteor. Whereas option luck refers to "a matter of how deliberate and calculated gambles turn out[,] [...] [b]rute luck is a matter of how risks fall out that are not in that sense deliberate gambles" (Dworkin 1981b, p. 293). Similarly, Miller (1999) subsumes such issues under circumstantial luck, and argues that only integral luck, which clearly and directly reduces individuals' chances of reward should be factored out to achieve fairness (ibid., pp. 143–146). Although Dworkin concedes that it is quite difficult in practice to distinguish between the consequences of circumstance and the consequences of past choices such as health care, risk, or education,[21] he stresses

[19]One of the notable exception is Fleurbaey's (2008, pp. 73–100) model of optimal, incentive-compatible compensation.

[20]One prominent example for this is Dworkin (2003, p. 191), who argues that his egalitarian theory is not in line with luck egalitarianism since it postulates equal opportunity to insure against bad luck, and only in absence of that opportunity suggests compensation, whereas luck egalitarianism postulates the compensation of any bad luck.

[21]Nagel (1991, p. 71) goes even further and points out that "there is a wide disagreement when an individual is responsible for what happens to him, ranging from disputes over freedom of the will in general to disputes over the conditions of knowledge and opportunity needed to confer responsibility for an outcome, to disputes over when the use of a natural ability or

that society should only mitigate the influence of circumstance on an individual's resources, but not the consequences of own choices (Dworkin 2001, pp. 324–325), which encompasses the consequences of chosen risk taking. Fleurbaey (2008) also argues that option luck should not be compensated, simply because it could have been avoided.[22]

Such statements are criticized as too harsh by several authors, most prominently by Anderson (1999). She exemplifies (among other points) that an uninsured driver who is involved in an accident due to a wrong turn would be left to die by luck egalitarians, irrespective of whether the driver dropped the insurance due to financial restrictions (ibid., pp. 295, 298). This criticism is addressed by several philosophers defending luck egalitarianism, such as Barry (2006), Voigt (2007), and Segall (2007), who stress that her criticism ignores that the responsibility principle accounts for reasonable expectations as well as for external influences such as financial needs limiting one's choices (Voigt 2007, p. 396–397). Nevertheless, Barry acknowledges that the harsh treatment poses a challenge in some, albeit only a few real life situations, and suggests that in these cases, the responsibility principle should be combined with the principle of need to ensure the basic necessities of life for everyone (Barry 2006, p. 102).

Still, Sher (2014, p. 17) remains unconvinced and criticizes such a pragmatic approach (as well as many luck egalitarians unofficially accepting some unchosen economic inequalities in return for higher productive efficiency) as opposed to the underlying principle of luck egalitarianism. Furthermore, he points out that the assumption that all unchosen inequalities are unjust does *not* automatically imply that all inequalities based on individual choices *are* just (ibid., p. 4).

Nevertheless, according to Miller (2013), such specific problems do not devalidate a theory: he argues that a principle of justice should not directly guide practice, but instead only be seen as an ideal to strive for when adapting it to real life problems. Therefore, limitations of realizations should not be already implemented in the theory, as otherwise, "our theory of justice will be contaminated by irrelevant contingencies. It will become more conservative and less demanding than it should be" (ibid., p. 2). Instead, he suggests that context decides which justice principle should be applied in which situation, which is indeed supported by the empirical findings now presented in the next section.

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fortunate circumstance for which one is not responsible nevertheless makes one responsible for the results".

[22]Note however that the financial consequences of avoiding risk should serve as reference point to evaluate the risk choice, and that Fleurbaey's (2008, pp. 175, 255–258) responsibility-based theory of equal autonomy posits that a certain level must always be ensured so that individuals keep a minimum level of autonomy and decision-making competence.

2.2 Empirical and Experimental Distributive Justice Research

Having presented the most important normative principles of distributive justice in Section 2.1, this thesis now turns to the empirical evidence on just behavior as gathered by distribution experiments. Since the experimental design used in this study is a dictator game, the focus of this section mainly lies on experimental studies which are also based on the dictator game to allow for comparisons, although some results of other games are included, especially in the general overview of fairness preferences. All the presented distribution experiments are concerned with distributive judgments laypersons are making, which often are, as Swift (1999) points out, contradictory and inconsistent and thus less sophisticated than the rational justification of justice principles by philosophers: "because what people believe and what they ought to believe are different in kind" (ibid., p. 341). Accordingly, the research summarized in this section finds sometimes contradictory evidence for all the different distributive fairness preferences presented above.

At first, Section 2.2.1 roughly outlines basic approaches of distributive justice research, before some general experimental results on the different distribution principles are presented in Section 2.2.2, with the exception of the responsibility principle. The experimental studies on this are presented separately and in more detail in Section 2.2.3. Lastly, Section 2.2.4 provides an overview on different factors that have been shown to influence participants' distribution decisions in experiments besides their fairness preferences.

2.2.1 Methods and Operationalization of Empirical Distributive Justice Research

The distributive justice norms presented in Section 2.1 have been operationalized by economists into rather simple, empirically testable distribution principles. While the exact definitions differ across studies, the basic distributional statements of the respective distributional principles can be formulated as follows:

- Selfishness/homo economicus: subjects choose the level of redistribution (all/ nothing) that benefits themselves most
- Strict libertarianism: subjects do not redistribute at all
- Strict egalitarianism: subjects unconditionally equalize incomes
- Inequality aversion: subjects unconditionally reduce income inequality
- Efficiency/utilitarianism: subjects choose the income distribution which maximizes the overall payoff
- Maximin: subjects redistribute to benefit the worst-off

- Need: subjects ensure that an income threshold is met
- Desert/equity/accountability: subjects distribute income proportionally to contribution from individuals' internal factors, free of exogenous variables
- Responsibility/equal opportunities: subjects compensate income inequality that arises from situations beyond an individual's control,[23] but not income inequalities that individuals can be held responsible for

Various experimental studies have shown that people's (re)distribution[24] behavior deviates from the selfish behavior suggested by the standard economic model of the *homo economicus* and instead follow one of the other presented principles. Accordingly, several authors developed economic models of preferences or utility to explain why chosen income distributions diverge from homo economicus predictions, such as Andreoni (1990), Rabin (1993), Fehr and Schmidt (1999), Bolton and Ockenfels (2000), Charness and Rabin (2002), Dufwenberg and Kirchsteiger (2004), and Falk and Fischbacher (2006).

Some of these models are outcome-based and consider final payoff distributions. Examples for this are the models by Andreoni (1990) and Andreoni and Miller (2002), who assume that individuals are altruistic and have a standard Cobb-Douglas utility function which albeit increases in other's payoffs ("warm glow"). Similarly, in the models by Loewenstein et al. (1989), Fehr and Schmidt (1999), and Bolton and Ockenfels (2000), the utility of inequality-averse individuals is reduced by differences between their payoff and those of the other players in an experiment, so that they are willing to give up some payoff to reduce the inequality.[25] Other theories focus on the income allocating procedure and incorporate the importance of reciprocity, such as the studies by Rabin (1993), Dufwenberg and Kirchsteiger (2004), and Falk and Fischbacher (2006).[26] Finally, some models combine outcome-based fairness and reciprocity, such as those by Levine (1998), Charness and Rabin (2002), or

[23]As already mentioned in Section 2.1.6, this does not only apply to e.g. production choices or effort, but also to risky decisions. According to the responsibility principle, only uninfluenceable negative brute luck is compensated, but not bad results of deliberate and calculated gambles (option luck, see Dworkin 1981b, p. 293).

[24]While some experiments focus on distribution decisions, i.e. the determination of an income distribution, others focus on redistribution decision, i.e. the changing of an already existing income distribution.

[25]In both Loewenstein et al. (1989) and Fehr and Schmidt (1999), disadvantageous inequality reduces utility more strongly than advantageous inequality. In Bolton and Ockenfels's (2000, p. 167–172) ERC-model (*equity, reciprocity, and competition*), a similar idea is modeled by two thresholds at which behavior diverges from the usual self-interested preferences, since the model posits a tension between an equal split (assumed to be the social reference point) and self-interest for more money.

[26]In Rabin's model, individuals react to the intention of others' actions: punish others if these hurt them, and be nice to those who are being kind. While Rabin's approach is developed for normal form games, and measures kindness by the equitability of the resulting payoff distribution, Dufwenberg and Kirchsteiger (2004) extended the model to extensive games with an explicitly sequential structure, and Falk and Fischbacher (2006) separated the consequences of an actor's action and their underlying intention and incorporated both aspects into their model.

Bicchieri and Zhang (2012).[27] A social utility model which unifies both outcome and procedural fairness is however still required (Bolton et al. 2005, p. 1055).

To test the different models or to examine the more general (re)distribution principles, a vast and continuously expanding body of experimental studies using a variety of different games has been conducted. Depending on the experimental design, different justice principles can be examined. The studies comprise several different allocation mechanisms with (re)distribution in experiments being achieved either by a social planner, or by taxes which can be exogenously imposed or endogenously decided and target either everyone or only the wealthy, or by donations from a richer person to a worse-off person. In case of redistribution, transfers reduce the income gap between the better-off and the worse-off. Thus, they satisfy the Pigou-Dalton condition which states that transfers from the rich to the poor reduce inequality but preserve the individuals' ranks.[28]

The simplest game to gain information on subjects' fairness preferences is the dictator game (Forsythe et al. 1994) which is also used in this thesis. It is well suited to examine especially equality and selfishness preferences, whereas the information on other justice principles depends on the exact design. Accordingly, the experiment presented in Chapter 4 is designed specifically to gain information on the application of the responsibility principle.

In the dictator game, one player receives money and can either keep it to herself or share any amount with another player, the recipient. It is based on the ultimatum game,[29] in which a recipient can accept or refuse a proposed allocation so that both players receive either the accepted distribution or nothing, with the crucial difference that recipients in the dictator game do not have a veto. Therefore, dictators' offers have no tactical components but merely reflect their own fairness preferences. This makes dictator games ideal for studying individuals' preferences for equality, selfishness, and also other distributive justice norms, provided that these are manipulated by the treatment design.[30]

[27] Levine (1998) uses Andreoni's (1990) idea of altruistic utility functions and models decisions based on individuals varying own altruism or spitefulness and their expectation of their opponents level of altruism. Similarly, in the model by Charness and Rabin (2002, pp. 851–858), individuals maximize their expected utility depending on their social-welfare preferences, given the other players strategy, and in the model by Bicchieri and Zhang (2012, p. 579), individuals prefer to obey a certain outcome-related fairness norm, conditional on the expectation that others follow the same norm.

[28] As Temkin (1993, p. 82) points out, this is not necessarily the case, as it is possible that transfers from the rich to the poor simply lead to a switch in positions, which would not reduce inequality at all. Such a revolution does however not fulfill the rank-preserving Pigou-Dalton condition.

[29] The dictator game was first used by Kahneman et al. in a classroom experiment, following the design of Güth et al.'s (1982) ultimatum game, but without the possibility of rejection by the recipient and instead a possible punishment by an observer (Kahneman et al. 1986, pp. S290–S291). Forsythe et al. further simplified the game, so that dictators offer an amount to a recipient without the possibility of rejection or punishment, and also coined the term dictator game (Forsythe et al. 1994, pp. 348–350). Their version of the dictator game has since been used in an enormous variety of experiments.

[30] Of course, other games also provide important information on social norms, e.g. the ultimatum game sheds light on the expectation of proposers on recipients' rejection behavior based on their

As already stated by Rawls (1972, pp. 316–317), it is not always possible to clearly identify the motives of individuals from only observing their distribution judgments but not their reasons. As Temkin (1993, pp. 7–8) points out, only because someone ensures an equal distribution does not mean that they are actually egalitarians. Instead, the equalization of income could simply be the best way to fulfill another justice motive, as in the example of unequal wages for equal work. The resulting inequality violates several justice norms, so that people who equalize the resulting inequalities could follow the egalitarian, equity or responsibility principle. Thus it is not surprising that the experimental literature presented in the following section finds no clear ranking of the different justice principles, but instead supports all distributive principles to some extent and demonstrates the importance of context and options.

2.2.2 Empirical Evidence for the Application of Distributive Justice Principles

As mentioned above, the dictator game is ideal for studying some distributive justice norms, depending on its design, and additionally attracts by its simplicity. Therefore, it has been used by so many studies that it is almost impossible to summarize them comprehensively. Nonetheless, Engel (2011) has published a meta-study 10 years ago on the state of the experimental literature on the dictator game at the time, which includes 131 studies.[31] Although classical economic theory with the assumption of self-interested rational individuals predicts that an amount of zero is offered in a dictator game, Engel finds that in real-life experiments, dictators give about 28.3% of their endowments, by both a random-effect meta-analysis as well as by simply calculating the grand mean from all individually reported/constructed treatment means (ibid., pp. 586–588). Although this amount might be upward biased,[32] this shows that subjects do not solely care about payoffs, but also about fairness. The average value of 28.3% however omits both the variation in giving between the different studies included in the meta-analysis, as well as the strong variation of giving behavior on the individual level found in all experimental studies. Engel's meta-analysis confirms that giving is not evenly distributed. Instead a large share of individuals (36.11%) does not give anything (in dictator games, this can be explained either by selfishness or by strict libertarianism), while another

fairness norms (Bicchieri and Zhang 2012, p. 578). Cooperative games, such as public good games and trust games, or competitive games, such as market games, also show that humans deviate from the predictions for homo economicus, but since they are not closely related to the distributive justice norms presented in Section 2.1, they will mostly be neglected in this thesis.

[31] For related ultimatum games, a similar endeavor of summarizing the current experimental results has been undertaken by Camerer (2003, pp. 48–83) and Güth and Kocher (2014).

[32] Not only could giving in a laboratory experiment be increased due to an experimenter-demand-effect (see e.g. Rosenthal 1976 or Orne 1981), but furthermore, Lazear et al. (2012, pp. 143–147) argue that giving in a laboratory does not accurately reflect generosity in the real world, where people can avoid sharing environments. Their experimental results from a dictator game suggest that such an avoidance option leads to strong and significant reduction of the average amount shared, due to the opting out of relatively generous but reluctant sharers.

group of subjects (16.74%) equalizes incomes (strict egalitarians). The giving behavior of the other half of subjects is mostly rather evenly distributed between these two options, although a small share gives more than half of the endowment, with 5.44% even giving everything (ibid., p. 589). The meta-study could not provide any insight into the prevalence, relative importance or combinability of the other distributive justice principles, since these can only be observed if an experiment's treatments manipulate need, efficiency, desert, and/or responsibility. Still, this has been done by a vast amount of studies using various experimental designs of many different games, the main findings of which are now shortly summarized below.

Libertarianism and Selfishness vs. Inequality Aversion and Egalitarianism

As shown in Engel's meta-study, despite redistribution decisions deviating from the homo economicus predictions, self-interest is still the dominant motive: dictators on average giving 28% of their income (ibid., p. 588) means that they on average keep 72% for themselves. That this pattern is best explained by selfishness and not Nozick's libertarianism is shown especially in several voting experiments in which, in line with theoretical prediction, the majority of rich subjects opposes redistribution (see e.g. Cabrales et al. 2012, Höchtl et al. 2012, or Agranov and Palfrey 2015), and most poor individuals vote *for* redistribution.[33] There are only a few poor individuals (e.g. about 4% in Tyran and Sausgruber (2006, p. 480), or about 9–14% in Cabrales et al. (2012, pp. 289–291)) who deviate from self-interest and vote against redistribution and could hence classified as strict libertarians.[34]

Indeed, self-interest seems to be the biggest single explanation for (re)distribution decisions in almost all studies, see e.g. Andreoni and Miller (2002, p. 745), Bolton and Ockenfels (2006, p. 1907), Fisman et al. (2007, p. 1864), Durante et al. (2014, p. 1061), and Tepe et al. (2021, p. 1059). This dominance of self-interest also has implications on results supporting egalitarian preferences based on equal-split decisions made by subjects behind a veil of ignorance, since these could be explained not only by egalitarian preferences but also by their self-interest and risk aversion. The relevance of this is demonstrated by many studies, such as Beck (1994), Traub et al. (2009), Schildberg-Hörisch (2010), and Iriberri and Rey-Biel (2011). Their results all show that dictators display higher levels of generosity under a veil of ignorance, i.e. when they are not certain of their position. Role-certainty however

[33] Besides selfishness and inequality aversion, redistribution votes seem to be additionally influenced by the class structure of the electorate, such as the respective sizes of rich and poor groups or the existence of a middle class (see e.g. Höchtl et al. (2012) and Kittel et al. (2015)). Furthermore, results by Tyran (2004) suggest that a significant share of subjects conditions their vote on their expectations of how many others approve of the proposal.

[34] At least in case of Tyran and Sausgruber (2006). In Cabrales et al. (2012), endowments are earned, therefore opposing redistribution can be explained by both libertarianism and the principle of desert.

leads to the dominance of selfish preferences,[35] which can therefore also be expected in this thesis.

Despite the general dominance of selfish preferences, in all studies a certain proportion of subjects, varying in size in each case, deviates from self interest and chooses costly redistribution even if positions are already known. Combined with the fact that equality seems to be a dominant principle when individuals make distribution decisions as disinterested observers (see e.g. Johansson-Stenman et al. 2002, Carlsson et al. 2005, or Croson and Konow 2009), this highlights the importance of inequality aversion. Nevertheless, there are only few studies, especially incentivized ones, in which inequality aversion dominates selfishness such as in Tyran and Sausgruber (2006).[36]

The equal split postulated by egalitarianism also continues to be shown as a particular focal point, chosen e.g. by about 30% of participants in both Andreoni and Miller (2002, p. 745) and Tyran and Sausgruber (2006, p. 480), where endowments are endogenously allocated, and still by 16%–27% of participants in the real-effort experiments by Cappelen et al. (2007) as well as Cappelen et al. (2013b). Yet, equality has been found by several experiments (see below) to be dominated by other fairness norms such as need, efficiency and especially equity in direct comparisons.[37] These empirical observations lead Konow (2003, p. 1233) to postulate that equality is merely a default chosen in experiments in the absence of distribution-relevant information for distributive justice judgments, such as effort, choices, needs, etc., which are then assumed to be the same for all participants in the experiment.

The maximin principle postulated by Rawls's egalitarianism is even less supported by experimental data than strict equality. While some redistribution behavior in line with the maximin principle, albeit to different extent, is found, e.g. in the studies of Engelmann and Strobel (2004),[38] Michelbach et al. (2003),[39] Schildberg-Hörisch

[35] Yet risk aversion itself seems to be stable and not to be influenced by one's own relative position in a society, as shown by Linde and Sonnemans (2015).

[36] Only 25% of redistribution decisions in Tyran and Sausgruber's strategic voting experiment can be explained by selfishness, whereas weak or strong inequality aversion explains more than twice than that (Tyran and Sausgruber 2006, p. 480). Similarly high levels of preferences for equality are otherwise limited to unicentivized surveys, such as Faravelli (2007), where the support for egalitarianism is however also shown to differ between sociologists and the less egalitarian economists.

[37] Interestingly, while this general finding results from experiments in various countries, an analysis of European Social Survey (ESS) data shows that the lower support of equality relative to need and equity (which are similarly supported) is even more pronounced in case of Germany than for the rest of Europe (Adriaans et al. 2019, p. 402).

[38] Engelmann and Strobel (2004, p. 866) argue that a combination of maximin preferences, efficiency concerns and selfishness explain their data better than inequality aversion.

[39] Michelbach et al. (2003, p. 534) find that the behavior of about 18% of participants in their experiment is in line with the maximin principle, and can also show that these subjects are more sensitive to needs than egalitarian or efficiency-maximizing subjects.

(2010),[40] and Fisman et al. (2021),[41] it seems that participants prefer to support the worst-off members in a society by implementing a minimum income. Above this minimum income, experimental results suggest that participants favor Boulding's idea of maximizing a society's efficiency while ensuring a floor constraint, as shown e.g. by Frohlich and Oppenheimer (1990, pp. 58–59), who emphasize the importance of the equity principle on top of a minimum income to preserve incentives. Similarly, in the experimental study by Traub et al. (2005, p. 297), maximizing average income while observing a minimum income is one of the most supported principles, whereas the maximin principle continuously performs worst.

The importance of an insurance-like floor constraint is not only suggested by the above mentioned studies and the design of existing welfare states (see e.g. Gough et al. 1997), but also by the growing strand of experimental literature which finds that the principle of need is an important redistribution motive and is now presented in more detail below.

Need

That need matters in distributive judgments is suggested by an increasing strand of literature.[42] One example are the several experimental studies showing that subjects give more to charities or needy individuals than, e.g., in an anonymous baseline setting with university students as recipients (see e.g. Eckel and Grossman 1996; Small and Loewenstein 2003; Carpenter et al. 2008; and Fong and Luttmer 2009, 2011). Similarly, in a real-effort dictator game by Cappelen et al. (2013b) dictators especially from high-income countries condition their distribution choice on individual contribution to the total surplus, but give higher-than-earned shares to participants from low-income countries, and lower-than-earned shares to participants from high-income countries, which underlines the importance of a combination of equity and need (ibid., pp. 582–583). While some of these findings might be partially explained with the reduced anonymity of the recipients (see Section 2.2.4), the importance of need considerations is supported by the results from different surveys, e.g. by Yaari and Bar-Hillel (1984), Gaertner et al. (2001), whose cross-cultural survey however suggests that needs can be weighted very differently depending on both the cultural background of respondents and the decision context, as well as by Konow (2001) and Faravelli (2007). Furthermore, incentivized laboratory experiments also confirm the relevance of a minimum income as distributive norm: results by Lorenz et al. (2017) show that a redistribution decision framed as a decision on a minimum income leads to higher redistribution than a decision on a distributionally equivalent

[40] Schildberg-Hörisch (2010, p. 1065) find that about 14% of subjects' redistribution decisions are in line with the maximin principle. Their results also suggest that this behavior can be explained better by egalitarian preferences than by risk aversion.

[41] Fisman et al. (2021) also find that the maximin principle dominates efficiency concerns in hypothetical income distributions that do not vary the deciding subject's income.

[42] For an overview on costly need-based redistribution, its application and welfare consequences, see Nicklisch and Paetzel (2020).

tax. This finding is replicated in the follow-up study by Paetzel et al. (2018), which also shows that this framing effect vanishes if there is more transparent information on the equivalent distributional consequences. The study by Kittel et al. (2020) does not use a general minimum income, but instead models as individual threshold incomes needed for "survival" in their laboratory experiment. The results confirm the strong influence of needs on distributive justice decisions, since payoffs indeed shift systematically toward the need thresholds. With increasing need thresholds, the satisfaction of needs declines, and even collapses as soon as the needed threshold income is higher than an equal split of endowments and thus conflicts with both self-interest as well as the principle of equality (ibid., pp. 6, 9).

Another strand of literature examines how the principle of need interacts with other principles of distributive justice. Here, especially the studies examining the interaction of needs with the responsibility principle are of interest, as this thesis' design also allows some insights into this topic.

One study that does *not* find a moderating effect of responsibility for being needy on the compensation of need is the modified dictator experiment by Buitrago et al. (2009), in which dictators do not condition their strategic giving decision on their recipient's effort or luck to overcome their poverty but instead seem only to react to the inherent inequality of incomes (ibid., p. 83). In contrast to this, most other studies such as Konow (2001), Faravelli (2007), Schwettmann (2012), and Bauer et al. (2022) show that while needs are considered by a majority of respondents, their fulfillment is reduced when individuals are responsible for their neediness. Similar questions in Gaertner and Schwettmann (2007, pp. 640–644) however fail to produce a comparably clear answer, instead responsibility for needs seems to have either no impact on their fulfillment, or a gender-specific one. Nevertheless, all in all the empirical evidence seems to suggest that the fulfillment of need is moderated by responsibility considerations, but that the principle of need is still dominant compared to the responsibility principle.

This picture changes when need and efficiency are the competing distribution motives: While Faravelli (2007, p. 1413) finds that the introduction of a need threshold drastically reduces utilitarian preferences, results by Konow (2001, pp. 149–151) suggest that the fulfillment of basic needs dominate efficiency considerations for low, but not for high efficiency costs. Gaertner and Schwettmann's (2007, p. 640) survey also shows that respondents decide against the fulfillment of basic needs if another, more efficient alternative exists. That efficiency is indeed a crucial motive especially for redistribution decisions is shown by several studies now summarized below.

Efficiency/Utilitarianism

The principle of efficiency, based on utilitarianism, aims to maximize expected utility by maximizing overall income. While it is not the focus of this dissertation and hence not considered in the experimental design, there are many other studies which examine efficiency and find that it affects redistribution in two dimensions.

First, several studies find that transfers are reduced when redistribution is associated with a reduction of efficiency (see e.g Beckman et al. 2004; Gaertner and Schwettmann 2007; Krawczyk 2010; and Durante et al. 2014).

Second, several studies find that some individuals decide for redistribution if this increases total payoffs, even as their own payoff are hereby reduced (see e.g. Andreoni and Miller 2002; Kritikos and Bolle 2001; Charness and Rabin 2002; Engelmann and Strobel 2004; Bolton and Ockenfels 2006; and Iriberri and Rey-Biel 2011). While all these studies suggest that efficiency preferences play an important role in (re)distribution decisions, the evidence on whether efficiency is the, or at least one of the dominant motives (self-interest aside)[43] is mixed. While e.g. Fisman et al. (2007, p. 1864) or Durante et al. (2014, p. 1061) find only a minor influence of utilitarian efficiency preferences on allocation decisions, other studies, such as Charness and Rabin (2002), Engelmann and Strobel (2004), Iriberri and Rey-Biel (2011), and Paetzel et al. (2014), find that efficiency explains a significant share of distributive choices and even dominates inequality aversion as well as the principle of equality. This is contrasted by the findings of e.g. Andreoni and Miller (2002, p. 745) and Bolton and Ockenfels (2006, p. 1908), which suggest that egalitarian preferences dominate utilitarian ones.

As already reported in the above section, results for the respective dominance of efficiency and need concerns is similarly mixed, and seems to depend both on the level of need and the level of efficiency loss. As found by Engelmann and Strobel (2004, p. 866), a combination of these two motives serves as a powerful explanation of distribution decisions, since a combination of efficiency, maximin preferences and selfishness rationalizes most of their data.

Unsurprisingly, the direct comparison of efficiency and equity as dominant motives also leads to contradictory results. Whereas e.g. in Kritikos and Bolle (2001) and Traub et al. (2009), efficiency preferences are more important than the principle of equity, the findings by e.g. Bolton and Ockenfels (2006, p. 1908) and Pelligra and Stanca (2013) suggest that equity dominates efficiency. Furthermore, Pelligra and Stanca suggest that efficiency preferences are more prevalent among higher educated individuals, which might indicate that experiments with student subjects, as most of the studies mentioned in this section, tend to overestimate the importance of efficiency (ibid., pp. 7–8). This would even further increase the importance of equity as distributive justice norm, the empirical evidence for which is presented next.

[43]Basically all studies confirm that self-interest is the dominant motive to explain distribution choices. This only changes when one's position in society (and thus the selfish action) is uncertain, as e.g. in Iriberri and Rey-Biel (2011), where efficiency clearly dominates all other motives behind the veil of ignorance (ibid., p. 162).

Hoffman and Spitzer (1985, p. 283) already find that as soon as endowments in an experiment are earned, the norm to equalize incomes gets rivaled by the principle of desert. Later studies, such as List and Cherry (2008), Balafoutas et al. (2013), Durante et al. (2014), Kittel et al. (2015), and Tepe et al. (2021), confirm that performance-based incomes reduce redistribution in form of dictator giving or chosen tax rates, whereas an arbitrary allocation of endowments leads to a higher preference for equality. Albeit less clearly, this pattern is found even for an ultimatum game using the strategy method (Barber and English 2019, pp. 35–38).

Furthermore, as posited by the formalization of the equity or accountability principle, many subjects indeed choose an income distribution which proportionally reflects the contributions of each subject to the total amount. Although shown to be a generally relevant fairness norm, equity can not be further examined based on this thesis. Its experimental design focuses on exclusion, in which case no effort is conducted by recipients (see Chapter 4), hence dictator giving can not be proportional to effort.

Equity is found to be the dominant principle of distributive justice in several studies which examine hypothetical contexts (such as Gächter and Riedl 2006; Bosmans and Schokkaert 2009; and Herrero et al. 2010). However, Gächter and Riedl (2006) also show that while proportionality to effort is the preferred allocation under impartial normative judgments, as soon as subjects bargain over the allocation, equality is another important focal point.

This difference in normative judgments and actual stakeholder behavior emphasizes the important role of self-interest, since e.g. Rutström and Williams (2000) and Rodriguez-Lara and Moreno-Garrido (2012) argue that all choices in their respective experiments can be explained by selfishness rather than by equity, and that subjects choose the distribution principle which maximizes their own payoff. Such a masking of self-interest as equity could also explain the results of Cherry et al. (2002, p. 1219), in whose experiment the number of giving dictators falls from about 75–81% that give anything greater than zero to 30–31% once dictators earn their endowment. Accordingly, in Karagözoğlu and Riedl (2015) subjects tend to equally share total surplus if individual contributions are partially or completely omitted or randomly affected,[44] whereas information or beliefs about performance skew entitlements towards the better performer (ibid., pp. 2617–2168, 2622). Results of another real-effort experiment by Gantner and Kerschbamer (2016, p. 27) also show that high performers tend to prefer proportional shares of the totals surplus, whereas low per-

[44] This is one exemplary finding that seems to confirm Konow's (2003, p. 1233) statement that equality is the chosen default in the absence of distribution-relevant information.

formers prefer allocations closer to an equal split. The self-serving bias influences not only the preferred fairness norms, but also the efficiency of bargaining.[45]

A possibly resulting association of equity with selfishness is however contradicted by the findings of Konow et al. (2020). In his real-effort experiment, impersonal third party spectators favor equity in their decisions, but stakeholders allocate amounts closer to equality. While they are still influenced by equity, this behavior clearly deviates from their self-interest, and cannot be explained by possibly strategic components as the similar results of Gächter and Riedl's (2006) and Gantner and Kerschbamer's (2016) bargaining experiments. The dominance of equity over selfishness is even more pronounced in the study by Oxoby and Spraggon (2008). Here, dictators have no entitlements on their own but receive their endowment completely from their recipient's effort, in which case some dictators even give all the endowment to their recipient. That equity seems to be accepted over selfish interests is also supported by the findings of Paetzel and Sausgruber (2018), that show that entitlements from quiz performance are accepted by lower performing subjects against selfish interests. Furthermore, Gerber et al. (2019, p. 246) find that even in strategic distribution decisions under partial or no information on individual productivity, a majority of subjects prefers either no redistribution according to the libertarian principle (under partial information), or proportional redistribution according to the equity principle (under no information).

The accountability principle, Konow's (1996) specification of the equity principle, is also extensively researched. This is often done by adding arbitrary components, such as different wages or productivity rates, to real-effort experiments. As already mentioned in Section 2.1.4, there is no final normative consensus over whether it is appropriate to base an individual's desert on the number of units produced, or on the arbitrarily chosen financial value of what is produced. While the equity principle mostly ignores this differentiation, the accountability principle clearly posits that only the effort, i.e. the number of units produced, is relevant to determine the desired proportional distribution of total surplus.

One study which allows to examine the difference between these two conflicting interpretations of desert has been conducted by Cappelen et al. (2013b). In their experiment with randomly assigned prices for effort, proportional sharing is, as expected, a major focal point besides the equal split and no sharing. Nevertheless, the dispute in the normative literature cannot be solved by their experimental results, but is instead reproduced: the number of dictators who base their proportional income distribution on the value of each contributors production is similar to the

[45] Therefore, Gantner and Kerschbamer (2016, p. 33) suggest an impartial decision rule to allocate income shares, as this mechanism performs best in terms of efficiency as well as perceived procedural and distributional fairness.

number of dictators who outbalance the randomly assigned prices and distribute according to production effort (Cappelen et al. 2013b, pp. 582–584).[46]

Another study by Becker (2013) shows that especially high performing subjects extend their entitlements to their randomly assigned higher wages and allocate comparatively higher shares to themselves. However, distributions in case of equal wages for effort either combined with endowment luck or especially without any random influence strongly follow the equity principle.

In contrast to this, other studies examining redistribution in case of unequal wages for equal work are remarkably consistent in showing that subjects determine just income based on performance and compensate for randomly assigned, different wages through redistribution. This is especially true for impartial observers (see Konow 2000), whereas stakeholders display a (rather strong) selfish bias, but still redistribute a significant share towards those subjects that received lower or no wages (as found to different extents in the real-effort experiments by e.g. Konow 2000; Cappelen et al. 2010; Rodriguez-Lara and Moreno-Garrido 2012; Mittone and Ploner 2012; and Lefgren et al. 2016). The above findings[47] can be explained not only by the accountability principle, but also by the responsibility principle which is central to this thesis. It compensates individuals for disadvantages arising from situations not under their control, such as the arbitrary wage differences for the same effort tasks, and is now addressed more detailed in the section below.

2.2.3 Empirical Evidence for Equality of Opportunity and the Responsibility Principle

As mentioned above, the empirical literature on the equity and the accountability principle demonstrates that subjects are, indeed, compensated for arbitrariness of wages when exerting the same effort, which is in line with the responsibility principle. Moreover, according to the responsibility principle, disadvantageous inequality due to all factors beyond one's own control should be compensated — not only arbitrary wages, but also bad brute luck, inequality of income opportunities, or the discriminatory total exclusion of individuals from production, the compensation for the latter being the focus of this thesis (see Chapter 3 for a comprehensive illustration of this problem).

[46] However, in a preceding study by Cappelen et al. (2010), a majority of subjects compensates unequal wages, and holds people responsible only for their chosen working time and productivity.

[47] Note that Mittone and Ploner (2012, p. 140) point out possible friction between their results and the accountability principle. In their study, only half of the recipients conduct the same real-effort task as dictators without being rewarded. These recipients receive significantly higher shares than those who don't conduct the real effort task, although the latter are exogenously restricted from taking the quiz. Nevertheless, since the allocation of recipients into the treatments with or without effort is random and not based on own choices, their results in my opinion do not contradict the responsibility principle.

Although the literature on this broader aspect of responsibility is less comprehensive than on other distribution principles, e.g. equity, it shows that many different aspects of responsibility do indeed influence distributive justice judgments. For instance, the already mentioned studies by e.g. Konow (2001), Faravelli (2007), Schwettmann (2012), and Bauer et al. (2022) show that responsibility matters in justice judgments and that being responsible for a needy status reduces the fulfillment of these needs, although the principle of need still seems to be the dominant fairness norm.

Furthermore, it has been shown that societal beliefs on individual responsibility for one's economic position are powerful explanations for political support or preferences for redistribution (see Fong 2001 and Alesina et al. 2018) as well as cross-country variation in redistribution when there is no further information on how income is determined (see Alesina and Angeletos 2005 and Rey-Biel et al. 2018). Nevertheless, survey results by Weinzierl (2017) suggest that, at least in the U.S., income inequalities due to brute luck seem to be accepted by a majority of respondents. While this contradicts the responsibility principle, it is in line with earlier results by Schokkaert and Devooght (2003), who find for an international student sample that even inequalities arising from rather clear cases of bad brute luck are not fully compensated by a surprisingly big share of respondents. Note that the results of these hypothetical surveys clearly suggest heterogeneous preferences, as respondent's payoffs are independent from their answer and therefore provide no selfish motive against redistribution.

This is different from most incentivized laboratory experiments examining varying aspects of the responsibility principle. Some of these therefore find only low levels of redistribution, such as e.g. Buitrago et al. (2009), Esarey et al. (2012), and Cabrales et al. (2012), where distribution choices surprisingly are not significantly related to the varying responsibility of some subjects' poverty. While Esarey et al. (2012) still find that at least liberal subjects support higher redistribution when effort-based income can be destroyed by random brute luck events, whereas conservatives oppose redistribution even in this situation,[48] Buitrago et al. (2009) find no influence at all of responsibility considerations on redistribution. In their modified dictator experiment, giving can only be explained by inequality aversion, since dictators don't condition their strategic giving decision on their recipients effort or luck to overcome their poverty (ibid., p. 83). Similarly, in Cabrales et al. (2012), voting for redistribution increases only slightly (from 13% to 22%) if the rich vote on redistribution not for the "lazy ones", but only for recipients who participate in a costly lottery but are unlucky. As the authors put it, "[t]here seems to be little empathy towards the unfortunate" (ibid., p. 299). Nevertheless, since the participation in the costly lottery is a voluntary choice by recipients, all its outcomes can be classified as option

[48] Only in case of almost certain losses, high redistribution is realized due to self-interest (Esarey et al. 2012, pp. 693–695).

luck. Therefore, in Cabrales et al.'s design, all income inequalities between recipients are the consequences of their choices and hence justified by the responsibility principle.

This apparent tolerance of bad option luck is contrasted by the study of Cappelen et al. (2013a), who find that a majority of subjects equalizes inequalities resulting from different luck among risk-takers, which is not compatible with the responsibility principle (albeit with luck egalitarianism). They however also find that income inequalities due to different risk choices are mostly not reduced, as posited by the responsibility principle. The fact that risk choices matter in redistribution decisions, but that subjects do not clearly distinguish between option luck and brute luck, is also shown in the experiment by Mollerstrom et al. (2015). Here, impartial third-party spectators decide on the redistribution of resources between two subjects whose endowment can be reduced by two random events. An insurance can be taken out against only one of these two situations, which allows the distinction between uninsurable bad brute luck and insurable bad option luck. On average, Mollerstrom et al. find indeed a higher compensation for brute luck than for option luck, which is in line with the responsibility principle. Nevertheless, their results also show that the compensation of brute luck is conditional to the subjects' choices on option luck, which they call "choice compensation" (ibid., p. 34).

Cappelen et al. (2020) further investigate choice compensation. They specifically examine whether choices have to be actual choices to influence redistribution decisions, or whether people are also held responsible for nominal and forced choices that do not meet the conditions of an actual choice between two acceptable alternatives. Nominal choice refers to the option to choose between two identical lotteries, and forced choice refers to the possibility to choose between a lottery and a safe amount that is almost as low as the worst lottery outcome. They find that only completely arbitrary income inequalities resulting from a random lottery are compensated by a majority of impartial observers through redistribution, whereas similar income inequalities are tolerated in the presence of forced and nominal choices. This seems to show that subjects accept the illusion of responsibility for income inequality, and only perceive the clear lack of responsibility in the arbitrary allocation of incomes as unfair and worthy of compensation.

Overall, the experimental studies presented above show that the principle of responsibility and individual choices play an important role in assessments of distributive justice.[49] At the same time, they confirm Nagel's statement that "there is wide disagreement when an individual is responsible for what happens to him" (Nagel 1991, p. 71).

[49] A lack of responsibility can also influence rejection rates in ultimatum games, as shown by Bolton et al. (2005) who vary the proposal options of proposers. They find that proposed unequal distributions, unacceptable in the presence of fair procedures or proposal options, become as acceptable as equal distributions if they result from a random draw, but not from arbitrary procedures (ibid., pp. 1061–1065).

Another strand of literature is less concerned with whether people are held accountable for their choices, but instead focuses on the importance of equal opportunities.[50] For this purpose, Karni et al. (2008) let subjects allocate the winning probabilities for a lottery among their group. Their results show that about 41% of subjects choose almost equal winning probabilities (ibid., p. 183), which emphasizes the importance of establishing equal opportunities even if this conflicts with self-interest. In the study by Krawczyk (2010), on the other hand, unequal opportunities do not have the expected effect: although different probabilities of winning are indeed perceived as unfair, redistribution is barely influenced by an increased dispersion of winning probabilities in a group, irrespective of whether these differences are effort-based or randomly assigned (ibid., pp. 134–138).[51]

Finally, the study by Akbaş et al. (2019) includes an interpretation inequality of opportunities in a similar way as in this thesis, namely as arbitrary denial of production choice. In their baseline design, two stakeholders can earn their income by choosing between a safe amount or a risky, but more efficient lottery. In line with the responsibility principle, income inequalities arising from the respective choice and different option luck are mostly tolerated by impartial observers and result in only low redistribution. Inequality of opportunity is modeled by giving one subject the voluntary choice between the safe amount and the lottery and forcing the other subject to take the safe amount, which can be understood as arbitrary discrimination, since the second subject is excluded from the lottery irrespective of their production preferences.[52] This violation of EOP significantly increases redistribution compared to the baseline treatment. Yet, it is still significantly lower and more heterogeneous than in a third treatment in which income is allocated randomly for both stakeholders. Here, observers decide on high redistribution and equalization of incomes, which joins the results of all previous studies with arbitrary income allocations. Thus, Akbaş et al.'s findings again highlight the importance of voluntary choices[53] and also confirm that inequality of production opportunities is at least partially compensated.

[50] That unequal opportunities have an actual impact in the real world which varies between countries is shown by several studies, e.g. by Keane and Roemer (2009) for the U.S., and by Björklund et al. (2012) for Sweden.

[51] Krawczyk attribute this surprising finding e.g. to each group member having the same probability to realize their preferred distribution due to the random-dictator design (whereas in Karni et al. (2008), subjects know whether their probability choice is realized or not), or to the fact that subjects behave predominantly self-centered (Krawczyk 2010, p. 138).

[52] Note that this is one crucial difference between Akbaş et al.'s design and the design of this thesis, in which the production preferences of subjects are known. Furthermore, this thesis examines different production contexts and uses interested dictators to decide on redistribution instead of impartial observers to see whether responsibility considerations can trump selfishness.

[53] Note that voluntary choices are not identical to voluntariness of choices: Voluntariness of choice means that individuals are free to make any decision, which includes not making a decision. Voluntary choices on the other hand also exist in situations where individuals are forced to choose between several options. In such a situation, they have no voluntariness of choice, but still make a voluntary choice for one of the given options. In this thesis and most related studies, only voluntary choices are considered.

All in all, the empirical literature confirms that responsibility plays an important role in the assessment of distributive justice judgments. As mentioned above, the studies examining arbitrary wages for effort, in which case the responsibility principle overlaps with the accountability principle, show very consistently that the resulting income inequalities are balanced out or at least compensated. This section however shows that the same can not be said for the few experimental studies (see Table 2.1) focusing on the other, broader aspects of responsibility such as brute luck, option luck or inequality of opportunity. In these studies, results vary strongly and are only partially consistent with the responsibility principle.

Table 2.1: Application of the Responsibility Principle in Experimental Studies

Responsibility Aspect	Study	Design	Results
Equality of Opportunity	Karni et al. (2008)	D	+
	Krawczyk (2010)	RD	–
	Akbaş et al. (2019)	IO	+
Brute Luck	Buitrago et al. (2009)	RD	–
	Esarey et al. (2012)	V	–
Brute Luck & Option Luck	Mollerstrom et al. (2015)	IO	BL: – OL: +
Option Luck & Choices	Cappelen et al. (2013a)	RD & IO	OL: – C: +
Choices	Cappelen et al. (2020)	IO	(–)

Table Notes: Result: in line (+) or not line (–) with the responsibility principle. Design: dictator game (D), random dictator game (RD), voting game (V), impartial observer (IO). Responsibility Aspect: brute luck (BL), option luck (OL).

Furthermore, there are still many research gaps in the literature on inequality of opportunity and compensation for it. This is surprising, since this topic seems especially relevant in the context of discrimination. While there are several studies confirming the existence of discrimination in different contexts, these studies examine only discrimination based on identity. This is done both for real life ethnic groups, by e.g. Fershtman and Gneezy (2001) and Hoff and Pandey (2006), and for artificial groups in laboratory experiments, see e.g. the influential paper of Chen and Li (2009), or the studies by e.g. Ben-Ner et al. (2009), and Ockenfels and Werner (2014). Nevertheless, while identity is one major reason for discrimination, it is not the only one, as discrimination can be based also on other factors. This is not captured by the above studies.

In fact, discrimination in form of pure, arbitrary exclusion which violates EOP is, as shown above, still barely researched. Also, the existing designs allow no differentiation between individuals who are poor due to their own voluntary choice to shirk, and those who are poor due to exclusion and unequal opportunities, despite their willingness to participate in production. Therefore, this thesis aims to further fill these research gaps and to experimentally investigate the social evaluation and compensation for pure discriminatory exclusion.

2.2.4 Other Important Determinants of Distribution Behavior in Dictator Games

In all the studies above, it is shown that not only justice principles, but also additional explanatory variables can explain distribution decisions by individuals. The importance and consistency of these variables are prominently examined in the meta-studies on the dictator game by Engel (2011), and on the related ultimatum game by Camerer (2003, pp. 48–83) and Güth and Kocher (2014).

Therefore, this section presents a selection of determinants that are most likely to influence distribution behavior in the dictator game experiment presented in this thesis. The order of presentation roughly follows the closeness of the respective categories to standard economic theory as suggested by Engel (2011, p. 590), so that this section first summarizes empirical evidence on the influence of *incentives*, i.e. stake size, and continues with the influence of *anonymity and information* and of *game structure and entitlements*, before it ends with the influence of *socio-demographic variables*.

Influence of Incentives: Stake Size

The influence of stake size in the dictator game is addressed by several studies, e.g. by Carpenter et al. (2005), Cherry et al. (2002), and List and Cherry (2008), which find mixed effects of stake-size.[54] This is also reflected in the meta-analysis by Engel (2011), who finds no effect of stake-size in the total sample of dictator games. In the meta-regression limited to those treatments that actually manipulated stake size, results suggest however that higher stakes reduce dictator's generosity, since they donate a lower share of their endowment, both in absolute and relative terms (ibid., pp. 591–592).

The evidence for the ultimatum game is similarly mixed. Some studies, e.g. by Slonim and Roth (1998) and by Andersen et al. (2011), find that increased stake sizes lead to relatively lower offers,[55] whereas results by Cameron (1999) and Carpenter et al. (2005) suggest no strong influence of stake size on offers. In his book, Camerer

[54] Whereas Carpenter et al. (2005) and Cherry et al. (2002) find no effect of stake size on donations, List and Cherry (2008, p. 4) find that an increase in stakes leads to a less than proportional increase in donations, although the variation in the data is not sufficient to allow any statistical analyses.

[55] Slonim and Roth (1998) find that stake size does not matter in initial rounds of a repeated ultimatum game without re-matching, but that learning over time leads to a stronger decrease in offers for high stake proposers than for low stake proposers. Andersen et al. (2011, pp. 3431–3435) find a strong decreasing effect of increasing stake size on the shared proportion in a one-shot ultimatum game, which they attribute e.g. to their experiment's unusually high stake

(2003, p. 61) summarizes that very large changes in stake size in ultimatum games only lead to moderate changes in offers and rejections.

All in all, stake size seems to be rather negligible factor in studies concerned with distributive justice, which is also confirmed by a recent meta-analysis of 31 studies by Larney et al. (2019). Their analysis shows that stake size has no effect on offers in ultimatum games, but confirms Engel's results that in dictator games, dictators reduce their donations with increasing stake size, albeit only a little. As Larney et al. point out, this small effect of differing stake size is however not problematic for most experiments, since it does not affect the relative amounts between the different treatments of an experiment which have the same constant stakes as the control condition (ibid., p. 68). Thus, one can conclude that while very strong variations of stake size might slightly influence donations in a dictator game and the amount of equal splits found by an experimental study, it is not relevant for the analysis of contextual influence on people's distributive justice preferences. It can therefore be expected that the results of this thesis are comparable to the results of other dictator studies, regardless of the respective stake sizes.

Influence of Anonymity and Information

That dictator giving in an experiment might not be explained by generosity, but instead by strategic considerations and social influences of the experimenter, is suggested by Hoffmann et al. (1994). In their experimental study, they analyze the importance of anonymity for distribution decisions and introduce the influential double-blind procedure for dictator games. Their results show that donations in the double-blind treatment are actually significantly lower than those in a treatment with standard between-subject anonymity[56] which is also used in this thesis. Many subsequent studies vary the anonymity between subjects as well as between subjects and experimenter, and several of those show that increased anonymity in dictator games is associated with more selfish behavior of the dictators, see e.g. Hoffman et al. (1996), Charness and Gneezy (2008), Franzen and Pointner (2012), and Krysowski and Tremewan (2021). Yet other studies point in another direction, e.g. Bolton et al. (1998) can not find a subject-experimenter anonymity effect and

size. Despite this proportional decrease, the actual amount offered still increases with stake size, so that rejections also decrease up to an almost perfectly full acceptance of offers.

[56] In a double-blind procedure, neither subjects nor experimenter know the identity or individual decisions of (other) subjects. This is ensured by providing subjects with two envelopes, one with e.g. ten $1-bills and the other with the same number of blank slips of paper. After opening the envelopes in privacy, subjects decide how many of the $1-bills they want to keep and exchange these with the blank slips of paper so that in both envelopes, the number of bills plus slips of paper adds up to same number. One envelope is kept by each dictator. The other envelopes are collected and taken to the recipients in another room, who are called out individually, select one of the envelopes to open, and keep its content after the amount of money was anonymously recorded. Under between-subject anonymity, which the standard for most private bargaining studies and attributed to Siegel and Fouraker (1960), subjects in bargaining pairs do not know the other subject's identity, and no subject knows the identity or decisions of other bargaining pairs. The experimenter however knows everything (Hoffmann et al. 1994, p. 355).

attribute the reduced donation amounts to the different ways that the treatments frame the decision tasks for the dictator (ibid., pp. 289–290). Bohnet and Frey (1999) find that identifying the recipients for dictators (but not vice versa) lead to less zero donations, but do not change the mean of donations, and in the experiment of Dufwenberg and Muren (2006b), dictators even give less when they are paid on stage rather than in private. The meta-study by Engel suggests that there is in general no significant difference between double-blind and normal between-subject anonymity, but that in more complicated dictator designs with repeated interactions increased anonymity leads to a small and weekly significant reduction of generosity (Engel 2011, p. 593). As this thesis uses standard between-subject anonymity with several strategic decisions, its results might also be subject to the possible upward bias on generosity.

Furthermore, the prominent experimental results by Rigdon et al. (2009) suggest that even the illusion of less anonymity has an impact. In their study, placing three dots in a “watching-eyes” configuration in a laboratory cubicle to simulate being observed significantly increases giving. A later study by Raihani and Bshary (2012) finds however that the effect of such cues of being observed are not very stable and e.g. only show effects in relatively public settings, but are completely ignored in truly anonymous situations. Nevertheless, the meta-study by Engel shows that overall, social control increases both the probability to give anything and the amount given in a dictator game (Engel 2011, p. 592). From this it can be concluded that in dictator games the perception of anonymity, between-subject anonymity and, although only sometimes and to a lesser extent, dictator-experimenter anonymity seem to have an impact on generosity in distribution decisions. However, this does not apply to strategic decisions, as no or only very little influence of any type of anonymity on offers was observed for the ultimatum game (see e.g. Camerer 2003, pp. 62–63, Charness and Gneezy 2008).

The influence of information and transparency is in some parts closely related to anonymity, such as in the study of Eckel and Grossman (1996), who find that dictators strongly increase their donations when a recipient is identified as a deserving charity compared to another anonymous student. That the identification of recipients as charities or needy individuals increases donations is confirmed by e.g. Small and Loewenstein (2003), Carpenter et al. (2008), and Fong and Luttmer (2009, 2011). While these results might be attributed to the information on the neediness or the worthiness of the good cause, this effect can also stem from the reduced anonymity of the recipient, similar to the study of Charness and Gneezy (2008). Other information maintains the anonymity of recipients, but still influences generosity: Cason and Mui (1998) find that while irrelevant information (recipient’s birthday) leads to more selfish behavior of dictators, relevant information (recipient’s distribution decision) does not alter dictator allocations.

Considering that openly available information on recipients potentially increases dictators' donations, some studies consider whether dictators would not rather avoid information. Thus, the prominent experimental study by Dana et al. (2007) manipulates the transparency of a standard dictator game[57] so that a recipient's payoff is not certainly determined by the respective dictator, although dictators have the cost-free possibility to ensure an equal allocation of payoffs. Indeed, they find that while the majority of dictators implement the equal allocation in the standard baseline treatment, only about half as many dictators do so in the intransparent treatments (ibid., p. 78).[58]

Related studies by Feiler (2014) and Grossman (2014) find similar results of information avoidance by dictators to justify more selfish behavior, although Grossman can show that information avoidance is dramatically reduced (from 45% to 3%) when subjects have to actively decide to avoid information (ibid., p. 2662). Furthermore, Dana et al.'s findings are partially contradicted by the studies of Thunström et al. (2016) and Brocas and Carrillo (2020). Both studies include optional information on the varying deservingness of recipients or in payoffs, respectively, into a dictator game, and find that similar shares of around 75%–79% of dictators choose information about recipient deservingness (Thunström et al. 2016, p. 119) or payoffs (Brocas and Carrillo 2020, p. 4).

All in all, while the above findings suggest that the availability of information on recipients influences donations by dictators, and that some dictators avoid such information, there is no consensus yet on the extent of this phenomenon. However, this is not relevant in the context of this dissertation, as the experimental design ensures that dictators receive information about the recipient's responsibility for her financial situation.

Influence of Game Structure and Entitlements

As shown in Section 2.2.2 and also found in Engel's meta-study on dictator games, actual changes to game structure, such as the reduction of a treatment's efficiency due to redistribution or the allocation of endowments not as manna from heaven (as in this thesis), but earned by dictators, significantly reduce the generosity of dictators (Engel 2011, pp. 593–595). Furthermore, selfish interests strongly decrease redistribution, so that it matters whether income distributions are implemented by impartial observers, stakeholders who know their position, or stakeholders behind a veil of ignorance. It seems however not to matter whether dictator games are played using the strategy method as in this thesis, or with repeated rounds and changed

[57] By hiding information on recipients' payoff, by assigning multiple dictators, or by introducing a cutoff time for donation decisions.

[58] As Dana et al. point out, these results show that situational factors play a very strong role in giving decisions, but do not negate the importance of fundamental fairness norms: while the lack of transparency strongly reduces the share of equal splits, there is still roughly one third of subjects that implements equal outcomes (Dana et al. 2007, p. 78).

recipients, or whether single subjects or a group of subjects act as the dictator (ibid., p. 592).

Interestingly, not only has actual variation in game structure a strong effect on generosity, but also *perceived* game structure resulting from framing. In his meta-study, Engel can not empirically estimate the general effect of frames from his sample studies since they are too fundamentally different from each other, with the exception of comparing virtual and real money, for which he finds that the handling of actual coins or notes leads to significantly more donations (ibid., pp. 595–596). This is however in contrast to the findings of Reinstein and Riener (2012), who find significantly lower donations when subjects are paid in cash than when their endowment is only shown on the computer screen (ibid., p. 235).

Probably one of the most researched frames in the dictator game is the giving or taking frame. Several studies find that the inclusion of a taking option significantly lowers dictators' generosity, such as e.g. List (2007), Bardsley (2008), and Grossman and Eckel (2015).[59] Krupka and Weber (2013) argue that this effect can be explained by the fact that adding a taking option alters the menu of socially acceptable behaviors, and that subjects have a stable preference for complying with social norms. Yet other studies, such as Hauge et al. (2016), Chowdhury et al. (2017), and Goerg et al. (2020), observe no significant differences between giving and taking frames in dictator games.[60]

Another strand of the framing literature considers the influence of morally loaded language in the instructions, such as Brañas-Garza (2007), who find a significant difference between treatments that only differ by the inclusion of one "non-neutral" sentence in the instructions, or Lorenz et al. (2017), in whose experimental study the level of preferred redistribution is strongly reduced when the word "tax" is used compared to a minimum income frame. Dreber et al. (2013) however find no significant effects of using different words in the description of otherwise identical games. Thus, overall, it seems that the prosocial behavior of dictators can be influenced by framing, but the exact mechanism and extent of this framing effect has not yet been conclusively clarified. This dissertation therefore uses a standard giving frame and avoids framing the language of the instructions as much as possible.

Influence of Socio-Demographic Variables

Finally, socio-demographic factors are a very important explanation for giving behavior. As found in Engel's meta-study, the most robust socio-demographic explanation for giving behavior is age, but being an economics student, gender, and culture also show significant effects on donation behavior (Engel 2011, p. 606).

[59] Unlike List and Bardsley, Grossman and Eckel (2015) find the framing effect of taking only in anonymous settings, and not for an identified, deserving charity as recipient.

[60] Note however that Chowdhury et al. (2017) find that giving and taking frames influence the generosity of men and women in opposite directions, so that these effects cancel each other out and no differences between the frames are visible in the overall result.

That adult **age** effects economic behavior is found by many studies. While this is mostly a byproduct from its use as a control variable in experiments with a different research focus than age, a steadily growing strand of experimental literature specifically examines generosity (and other economic behaviors) in children and adolescents,[61] e.g. Murnighan and Saxon (1998), Harbaugh and Krause (2000), Benenson et al. (2007), Fehr et al. (2013), Bauer et al. (2014), Angerer et al. (2015), Ben-Ner et al. (2017), Sutter et al. (2018), and Brocas and Carrillo (2020).

These studies show that age influences the preferred justice principle and that generous behavior is learned over time: studies by Fehr et al. (2008), Almås et al. (2010), and Fehr et al. (2013) find that in young children, egalitarianism becomes more important with increasing age,[62] before it is replaced with desert (Almås et al. 2010) or efficiency (Fehr et al. 2013) as main motive in adolescents. From then on, higher age is associated with higher generosity. Kids and adolescents behave more selfish than adults, and elderly people give significantly more than middle aged ones, since increasing age not only reduces the probability of giving nothing, but also increases the given amount: the mode for middle aged adults is the equal split, and for elderly people even to give everything (Engel 2011, p. 599).

The influence of **gender** on economic behavior is also examined by a large number of studies, although unfortunately not all dictator studies report gender. The observed effects of gender on economic behavior are less consistent than those of age, even when limited to giving in dictator games. For example, in some dictator games, such as Bolton and Katok (1995), Dufwenberg and Muren (2006b), and Tammi (2011), no differences in average donations are found between men and women; in others, such as Eckel and Grossman (1998), Dufwenberg and Muren (2006a), Schildberg-Hörisch (2010), and Brañas-Garza et al. (2018), women appear to be more generous than men, and rarely, e.g. in Ben-Ner et al. (2004), men display slightly (but not significantly) more generous behavior.

Furthermore, the findings of Cappelen et al. (2015) suggest that gender effects from student samples differ from those of representative samples. Overall, Engel's meta-analysis finds a positive and significant effect of being female on giving behavior in dictator games (Engel 2011, pp. 597–598), although some studies suggest a more nuanced relationship between gender and giving behavior.[63] Andreoni and Vesterlund (2001) for example find that men and women differ in their sensitivity to the cost of giving: while men are more generous when the price of giving is low, women are more generous when the price of giving is high. Their other finding, that men are more likely to be perfectly selfish and women are significantly

[61]For a detailed survey on various types of economic behavior of children and adolescents, see Sutter et al. (2019).

[62]Similar results of Fehr et al. (2013) and Bauer et al. (2014) suggest that egalitarianism peaks as main justice principle at the age of 8 years, or 10–12 years, respectively.

[63]The same is true for the inconclusive results on the effect of gender on altruistic behavior in public good and bargaining games, see Eckel and Grossman (2008a), Croson and Gneezy (2009), and Kagel and Roth (2020, pp. 512–525) for an overview.

more inequality-averse has since been confirmed by several other studies (not only for adults but also already for children, see e.g. Almås et al. 2010 or Sutter et al. 2018). Therefore, the variation in women's generosity between different designs suggests that their donation decisions are more context-specific than men's (Croson and Gneezy 2009, p. 458). This could also explain later results of gender differences in dictator games, e.g. by Chowdhury et al. (2017), who find that men and women react differently giving and taking frames, or by Klinowski (2018), who find that women donate more, but are also more likely to retract this donation.

Being an **economics student** has also been shown to influence giving behavior, and the evidence is less contradictory than the evidence on gender, at least for laboratory experiments[64] (see Kirchgässner (2005) for an overview). That economics students behave more selfishly or closer to the predictions based on the homo economicus model is supported by several experiments, such as public good games (Marwell and Ames 1981), ultimatum games (Carter and Irons 1991),[65] prisoner's dilemma games (Frank et al. 1993), and also dictator games (e.g. Hole 2013, and Cappelen et al. 2015). Results by e.g. Carter and Irons (1991) and Frey and Meier (2003) suggest that this effect mostly results from self-selection of rather selfish people into economic studies, as expressed by the famous quote "Economists Are Born, Not Made" (Carter and Irons 1991, p. 174), although some studies suggest that economics students are additionally trained to be selfish (see e.g. Frank et al. 1993, and Faravelli 2007). In addition to economics students being more selfish than students in other disciplines, Engel's meta-analysis of dictator games shows that students in general are more selfish than non-students (Engel 2011, p. 597). This is supported by recent findings of Cappelen et al. (2015, p. 1318), who therefore conclude that laboratory experiments using student participants actually underestimate the deviation of human behavior from economic models, and that students samples consisting of non-economics students are preferable for laboratory experiments since they reduce this bias. Since the experiment of this dissertation is conducted with a student sample, it is therefore to be expected that the results also show a selfish bias.

Finally, **cultural differences** are another important socio-demographic explanation of giving behavior in dictator games. Several studies can convincingly demonstrate that giving norms vary between different cultures, most prominently Henrich (2000), Henrich et al. (2001), and Henrich et al. (2010), but also Roth et al. (1991), and Buchan et al. (2004). These economic studies are supported by anthropologists in determining which societies actually differ in important cultural features, and in distinguishing the effects of culture from confounds such as different relative stake

[64] Field experiments produce contradictory findings: in Yezer et al. (1996) lost-letters field experiment, economics students are more cooperative than other students, and in another study using real-life data, Laband and Beil (1999) find that economists are as cooperative/honest as political scientists, and even more cooperative/honest than sociologists.

[65] Their findings can however not be replicated in some other studies, e.g. by Kagel et al. (1996). In their survey on the ultimatum game, Güth and Kocher (2014, p. 403) also summarize that education effects are inconclusive.

sizes, different languages, experimenter effects and other differences besides culture between the selected sample populations (Camerer 2003, pp. 68–74). Overall, in indigenous societies, the willingness to share endowment in a dictator game is a lot higher than in developed western subject pools, while giving behavior in developing countries lies in between (Engel 2011, p. 598).[66]

With the exception of cultural differences, socio-demographic variables might indeed influence giving behavior in this thesis' laboratory experiment. Therefore, they are later on used as control variables in the experimental design, presented in Chapter 4. Before that, the next chapter first defines exclusion and responsibility, and presents a model of fair compensation for exclusion.

[66] For ultimatum games, a meta-analysis of 37 studies on cultural differences is conducted by Oosterbeek et al. (2004).

3 Compensation for Exclusion: A Model

The empirical studies presented in the previous chapter show that different people have different conceptions of distributive justice, and these are context-dependent. This chapter now attempts to justify and model redistribution based on the responsibility principle in a society with competitive production. The model refers exclusively to competitive production with fixed profits that are not affected by the number of participants in a competition (think, for example, of a competition in which the winner benefits from a construction contract, the volume of which is independent of the number of competing architectural firms), and in which some participants experience discriminatory exclusion, meaning that some participants do not have equal access to financially beneficial income opportunities for reasons beyond their control (such as an arbitrary refusal to participate in the competition for the exemplary construction contract).

Section 3.1 first provides detailed definitions of the *responsibility principle*, and also an explanation of when compensation for exclusion is justified by the responsibility principle and when it is not. Then, in Section 3.2, it is briefly discussed how compensation can best be carried out: what kind of institutions are needed to ensure just compensation, and which individuals in a society can be asked to put up money to compensate the excluded individual without creating new injustice. Finally, Section 3.3 explains how a just compensation amount for excluded subjects can be determined both in general and in specific contexts, such as the presence of alternative income opportunities or perceived differences between the excluded individual's initial production choices.

3.1 Exclusion and the Responsibility Principle

This section provides working definitions of the terms *exclusion* and *responsibility principle.*

3.1.1 Defining Exclusion

In practical terms, the meaning of "exclusion" can refer to slipping through social insurance systems, being disaffected and isolated from society, social closure, or the active exclusion of one group by another (see Burchardt et al. 2002, p. 1–2). A more formal definition of exclusion states that

[a]n individual is socially excluded if (a) he or she is geographically resident in a society but (b) for reasons beyond his or her control he or she cannot participate in the normal activities of citizens in that society and (c) he or she would like to so participate (Burchardt et al. 1999, p. 229).

While the analysis of exclusion is oftentimes intertwined with the analysis of economic deprivation, "the notion of poverty is primarily distributional, [whereas] the concept of social exclusion focuses primarily on relational issues (detachment from labour markets, low participation, social isolation and especially the exercise of power)" (Madanipour et al. 2015, p. 725). The exercise of power is also highlighted by Byrne (2005): "although the term [exclusion] is clearly systemic, [...] at the same time it has implications for agency. 'Exclusion' is something that is done by some people to other people" (ibid., p. 2).

For the purpose of this thesis, it is assumed that income in a society is based on participation and performance in competitive production. In a world without exclusion, individuals are free to decide whether to participate in production or rather remain idle and poor. Therefore, *exclusion* is defined as follows:

> **Definition 3.1:** *Individuals are excluded if for reasons beyond their control they cannot participate in income-generating production although they would like to do so.*

Exclusion is denoted by the dummy variable $\mathcal{E} = \{0 \text{ if no exclusion}, 1 \text{ if exclusion}\}$. In a hypothetical society, idleness and exclusion are not the only reasons for poverty. The outcomes of competitive production are based on either pure luck, effort, or strategy, and participation in it generates both rich winners and poor losers. Yet, as long as everyone has equal opportunities to participate in production, a society based only on the responsibility principle will not redistribute at all. However, this thesis examines situations in which individuals who wish to participate in competitive production are excluded due to an arbitrary criterion such as name, gender, race, age, or other. The criterion used to prevent the participation of excluded individuals would not affect their chances of winning or losing the competitive production. Imagine a racetrack where the winner receives money, but runners whose family name begins with the letter M are not allowed to participate. This is obviously unfair, especially if the competition is the only way to increase one's income. In a society based on the responsibility principle, this exclusion, that directly and arbitrarily affects individuals' welfare opportunities, would therefore be compensated. This is justified in the following Section 3.1.2.

The economic disadvantage suffered by excluded individuals $i_{\mathcal{E}}$ as a result of their foregone income may vary. On the one hand, they might simply remain poor. On the other hand, they might find another job opportunity that provides them with an alternative income, which might be similar to, lower or perhaps even higher than their foregone income. As it reduces the economic disadvantage resulting from exclusion accordingly, it seems likely that the level of an alternative income influences compensation considerations. This is explained in more detail in Section 3.3.

3.1.2 Defining Responsibility

In this thesis, it is investigated whether and how the responsibility principle is applied in the compensation for economic disadvantages resulting from unequal access to income opportunities due to exclusion ($\mathcal{E} = 1$), leaving aside other responsibility related issues such as differential productivity, or differential pay for equal work. The responsibility principle is defined following and combining the principles and arguments developed by Arneson (1989) and Arneson (1990), Cohen (1989) and Miller (1999) (for details and differences, see Section 2.1.6) as:

Definition 3.2: *Individuals should bear the consequences of their voluntary choices when these concern welfare or resource gains and losses, but be compensated for disadvantages they suffer directly from arbitrarily restricted access to earning opportunities without the possibility to have avoided or overcome these restrictions.*

Thus, to qualify for compensation, exclusion must be based on *uninfluenceable and arbitrary criteria* for which the individual is not responsible and which he or she cannot avoid or overcome. This also means that understandable and reasonable restrictions on participation, such as educational credentials and test scores, do not qualify as exclusions because their existence and quality can be influenced and are the individual's own responsibility. Furthermore, for exclusion to be compensable, it must have a clear and *direct* impact on the individual's chances for reward, as indirect and long term effects of exclusion cannot be captured in the experiment.

Once initial equality of access is assured, the above definition of responsibility states that individuals are rewarded according to their desert (see Section 2.1.4) and that any final inequalities arising from different deserts are fully justified, since all individuals who voluntarily choose to enter or stay out of production are fully responsible for that choice and the resulting reward. To illustrate, see Examples 3.1–3.4, where the final incomes are the same in all situations, but the reasons for an individual's income of zero change.

Example 3.1: Suppose individuals A, B and C are unemployed and have an income of 0. All three are invited to an assessment center and participate. A performs best and gets the job with an income of 10. The final incomes are thus: A=10, B=0, C=0.
Responsibility Principle: Fair, $\mathcal{E} = 0$

Example 3.2: Suppose individuals A, B and C are unemployed and have an income of 0. All three are invited to an assessment center. However, B is not very interested in the job and stays at home. A does better than C and gets the job with an income of 10. The final incomes are thus: A=10, B=0, C=0.
Responsibility Principle: Fair, $\mathcal{E} = 0$

Example 3.3: Suppose individuals A, B and C are unemployed and have an income of 0. A and C are invited to an assessment center. However, B is not invited because her academic performance is worse than that of the others. A performs better than C and gets the job with the income of 10. The final incomes are thus: A=10, B=0, C=0.
Responsibility Principle: Fair, $\mathcal{E} = 0$

Example 3.4: Suppose individuals A, B and C are unemployed and have an income of 0. A and C are invited to an assessment center. B, however, is not invited because the company discriminates against employees named B. A performs better than C and gets the job with an income of 10. The final incomes are thus: A=10, B=0, C=0.
Responsibility Principle: Unfair, $\mathcal{E} = 1$

Note that this outcome is conditional on responsibility: while the lack of responsibility of the excluded individuals $i_{\mathcal{E}}$ justifies their compensation claims, it does not (for the most part) reduce the desert claim of the productive individuals, since the latter remains justified by their choices and efforts made.

3.2 Fair Compensation Institutions

The following model of compensation for exclusion assumes that an independently funded institution will determine the eligibility of excluded persons for compensation and its fair amount. This premise was chosen because it is well known that only an impartial party can be a fair judge (see e.g. Frohlich and Oppenheimer 1992, pp. 11–12), and competitors competing for the same chance cannot per se be impartial in assessing compensation, even less so if they are obliged to give away (part of) their own winnings. For one thing, each excluded person increases the participants' chances of winning, so their assessment of compensation might be inflated compared to that of an impartial observer because of guilt or as a thank-you for the higher probability of winning. On the other hand, their compensation assessment is most likely biased downward compared to that of an impartial observer, as they would now suffer a reduction in their honestly won winnings even though they were not responsible for the exclusion of others.[67]

Theoretically, this problem could be addressed by having members of society strategically decide on possible compensation behind a veil of ignorance, i.e., before they know their future income position, so that they are involved but still impartial (see Section 2.1.2). Nevertheless, even this solution would not be optimal: Frohlich and Oppenheimer (ibid., pp. 5–6) point out that there is a tension between the distribution one perceives as fair before one knows one's position in society and the distribution one prefers after one has acquired entitlements to one's income. This has been confirmed, for example, by the experimental results of Kittel et al. (2017), which show that knowledge of one's social position triggers self-interested behavior that overrides community-oriented motives. Thus, individuals who originally perceived a particular distribution pattern as fair may now assert more claims and eventually challenge the arrangement, leading to instability. Moreover, if the winners of the production have to pay compensation, this reduces the expected value of the production, especially if it involves costs, and thus the incentives to participate in it.

All in all, it is clear that a financially independent third-party institution is best suited to organize and disburse compensation fairly: It can decide whether or not a person is entitled to compensation without being influenced by guilt or entitlement, and without distorting incentives to engage in production. Still, no solution is perfect: as the large strand of literature on economic institutions and organizations shows, institutions also suffer from problems such as incomplete information, inefficiencies in collecting taxes for funding, and self-interest in resource allocation.

This dissertation simplifies these problems and models such an independent, but self-interested institution by using dictators that are neither involved in nor influenced by production. They are not financed by taxes, but instead receive their fixed budget from an external source, like manna from heaven, and can give a share of this budget to poor individuals or selfishly keep the budget for themselves. Because dictators receive information about the behavior and income of a poor individual, they are able to judge how much compensation this individual deserves, and can allocate the appropriate share of their fixed budget.

[67]Naturally, one might ask whether or not it is their moral duty to give away their gains from positive luck that happened to result from another individual's bad luck. However, to the best of my knowledge, this moral argument is not extensively explored in the fairness literature, which, as outlined in Section 2.1.6, focuses mainly on identifying and mitigating the effects of bad luck. Accordingly, this thesis focuses exclusively on compensating for bad luck that leads to inequality of opportunity, ignoring the associated positive luck for the other individuals in the form of a slightly higher probability of winning.

3.3 Fair Compensation Levels

Having established in Section 3.1 *that* existing inequality due to exclusion $\mathcal{E} = 1$ is unjust and should be compensated, it still remains rather complicated to establish normatively justifiable rules for *how much* compensation is just. Nevertheless, Section 3.3.1 attempts to develop a possible formalization of a socially acceptable level of compensation. To adapt this general rule to specific problems, two aspects are added to it: First, Section 3.3.2 modifies the general rule for compensation in the situation where excluded individuals receive income from an alternative source than their preferred but inaccessible labor. Then, Section 3.3.3 suggests how compensation might be affected by individuals' responsible production choices when available production options differ with associated levels of control over outcomes.

3.3.1 A General Rule

A crucial problem with compensatory exclusion in a competitive production context is that the outcomes of participation are entirely hypothetical. Thus, it is not a foregone income that can be compensated, but only a foregone *opportunity* for income. Recall Example 3.4: Given three identical (in the sense of having no objective differences in qualifications) individuals competing for a job, it cannot be said that the excluded individual B, had they been allowed to participate, would have won income $y_{\text{win}} = 10$. Instead, there is a good chance that, just like C, they would have lost to A and left the assessment center with no income. In this case, their exclusion would have had no effect on her income. But of course, they could also have won, and in that case would have lost an income of $y_{\text{win}} = 10$ due to exclusion.[68]

According to the responsibility principle, individuals must be compensated for disadvantages they suffer *directly* from restricted access to earning opportunities. It follows that the compensation must be positive, but not greater than the foregone income, so that $y_{\text{C}} \in [0, y_{\text{win}}]$. However, the responsibility principle does not imply that the disadvantages from exclusion must be compensated *fully*. While this would be possible in a model that assumes a sufficient external budget for the compensating institution, it would be impossible in other situations, such as when the compensating budget is limited or tax-funded.

One could argue with the need principle (see Section 2.1.3) that it is sufficient if the compensation ensures a certain need threshold. In this case, subjects would still be worse off than in their desire production, but the satisfaction of the need threshold would objectively prevent any suffering. However, this would negate the fact that exclusion has another dimension than just financial disadvantage. If compensation means only ensuring the needs threshold, this essentially disadvantages people who

[68] Since it is assumed that all persons are equally qualified for the job, this situation only has a negative impact on B. However, if B were better qualified, the exclusion would not only harm B, but also lead to an inefficient distribution of labor.

qualify for and apply for high-quality production, since they would be just as badly off as "lazy" people who are entirely responsible for their financial situation.

For this reason, and since the outcome of participation in competitive production is uncertain, it seems reasonable to link the amount of compensation y_C to the *expected value* of participation in the competition, μ, which is determined by the income in the case of winning and losing and the respective probability of doing so. This probability is the same for all individuals, since it is assumed that all individual differences (such as talent, commitment, etc.) are unobservable and therefore need not be taken into account in the ex ante calculation of the probability of winning. If there were indeed observable differences in competitiveness, the non-participation of less qualified individuals would not be an exclusion, and any resulting income differences would be justified under the responsibility principle. If excluded individuals were sufficiently qualified to participate but were unlikely to win because of a relative lack of talent, their compensation should be reduced accordingly. Similarly, fair compensation should reflect a clearly higher probability of winning (imagine, for example, Usain Bolt being arbitrarily excluded from sprinting competitions).

Since such situations are excluded in this model, the probability of becoming the winner of competitive production depends only on the number of competing participants, denoted n. Thus, the compensation y_C for an excluded individual can be calculated based on the expected value of the production, using only possible outcomes and the number of participants in the production. Since the compensating institution is modeled as selfish, this compensation is reduced by a selfishness parameter $\beta \geq 0$, where $\beta = 0$ in case of completely selfish compensating dictators, and $\beta > 1$ for altruistic dictators. Therefore, compensation for exclusion can be calculated by:

$$y_C = \beta\mu = \beta\left(\frac{1}{n} \cdot y_{\text{win}} + \frac{n-1}{n} \cdot y_{\text{lose}}\right) \tag{3.1}$$

In case of Example 3.4, there are three identical individuals competing for a prize of 10, losers get nothing, so that $n = 3$, $y_{\text{win}} = 10$, $y_{\text{lose}} = 0$. As the probability that B wins is $\frac{1}{3}$, the amount of compensation should be $y_C = \beta \cdot 10 \cdot \frac{1}{3} = 3.\overline{3} \cdot \beta$, and depends on the selfishness parameter β of the compensating institution, i.e. the dictator. Since this independently funded compensation leaves the winner's income untouched, it is likely a socially acceptable concept of restorative justice.

However, limiting the maximum compensation to μ would mean that the unfair act of exclusion itself is simply ignored, even though it may be worthy of compensation in its own right. Therefore, the model also includes an additional discrimination premium $\alpha \in [0, \infty)$ that allows compensation to be increased above the expected value of the lost income opportunity.

$$y_{C'} = \alpha + \beta\mu \tag{3.2}$$

Therefore, compensation $y_{C'}$ depends on the expected income μ of the forgone opportunity, as well as the selfishness parameter β of the compensating institution and the discrimination premium α, both of which are independent of the type and expected value of production.

3.3.2 A Conditional Rule: Alternative Incomes

Having established $y_{C'} = \alpha + \beta\mu$ as the level of just compensation in the case where an individual $i_{\mathcal{E}}$ is excluded from competitive production, one must consider the situation of partial exclusion, i.e., when $i_{\mathcal{E}}$ is excluded from only one particular income opportunity but not from all opportunities. This seems particularly important since in any society there are several different ways to earn an income. Thus, if individual B from Example 3.4 is excluded from one job, she might find another job. This job may not be her preferred job with the expected income μ, but it provides her with an alternative income $y_A \in [0, \infty)$, reducing the opportunity cost of exclusion.

Income alternatives are incorporated into our model in the following way: If excluded individuals overcome their financial disadvantage by obtaining a better job with $y_A \geq \mu$, they are no longer eligible for compensation under the responsibility principle. Thus, once excluded individuals receive income from an alternative income opportunity, they are entitled to compensation under the responsibility principle only if that income is worse than the expected value of the income opportunity from which they are excluded. Moreover, compensation must be reduced by the alternative income to prevent excluded individuals from being better off than they would have been without exclusion. Nevertheless, excluded individuals could be entitled to the discrimination premium α in any case, even if their alternative income is equal to or even higher than the expected value of the income opportunity from which they were excluded, because they still experienced discrimination. Therefore, compensation for exclusion is calculated as follows:

$$y_{C''} = \begin{cases} \alpha + \beta(\mu - y_A), & \text{if } y_A < \mu \\ \alpha & \text{otherwise.} \end{cases} \tag{3.3}$$

According to this Equation 3.3, all disadvantaged excluded individuals $i_{\mathcal{E}}$ receive a compensation that is reduced by the opportunity cost of exclusion, i.e. net of the alternative income. The compensation for exclusion is limited to the discrimination premium α once the alternative income is equal to or greater than the expected value μ of the lost income opportunity. For illustration, consider the following examples:

Example 3.5: Suppose individual B does not find another job, so that $y_A = 0$. As this is less than the income of $\mu = 10$ from the job in Examples 3.1–3.4, B suffers an economic disadvantage and is eligible for compensation.
$y_A < \mu \Rightarrow y_{C''} = \alpha + 10\beta$

Example 3.6: Suppose individual B finds another job with an income of $y_A = 5$. As this is less than the income of $\mu = 10$ from the job in Examples 3.1–3.4, B suffers an economic disadvantage and is eligible for compensation, albeit reduced by the alternative income.
$y_A < \mu \Rightarrow y_{C''} = \alpha + 5\beta$

Example 3.7: Suppose individual B finds another job with an income of $y_A = 10$. As this is equal to the income of $\mu = 10$ from the job in Examples 3.1–3.4, B does not suffer an economic disadvantage, but is still eligible for the discrimination premium α.
$y_A = \mu \Rightarrow y_{C''} = \alpha$

Example 3.8: Suppose individual B finds another job with an income of $y_A = 15$. As this is greater than the income of $\mu = 10$ from the job in Examples 3.1–3.4, B does not suffer an economic disadvantage, but is still eligible for the discrimination premium α.
$y_A > \mu \Rightarrow y_{C''} = \alpha$

3.3.3 A Conditional Rule: Responsible Choices

In a next step, it is investigated whether and how different production contexts affect compensation for exclusion when excluded individuals were previously free to decide whether to participate in different types of production. As mentioned in Section 2.2.3, experimental results by Mollerstrom et al. (2015) show that decisions for controllable situations also play a role in compensating for unrelated uncontrollable situations, even if the responsibility principle is violated.

Therefore, it seems possible that compensation for exclusion is affected by the perceived level of control an individual has over the outcome of her chosen production type, hereafter referred to as γ, as this might be interpreted as the individual's willingness to take risks. Previous research has shown that higher perceptions of control reduce perceived risk and increase risk taking (for a brief but comprehensive review, see the introduction by Damen 2019). People prefer seemingly controllable risks, even though perceived control over outcomes is by no means identical to actual control over outcomes, but is instead subject to bias or illusion of control (Langer 1975). Some examples of the many studies on the (mis)perception of control and risk are the studies of Klein and Kunda (1994), finding that people prefer controllable risks to less dangerous but uncontrollable risks, conditioned on subjects' biased beliefs about their own abilities to control outcomes, Horswill and McKenna (1999), showing that subjects in the driver's perspective (in control) make riskier driving decisions than as passengers without control, and Martinez et al. (2011), showing that illusion of control in a lottery increases risk taking. This is also supported by Damen's (2019) recent experimental study showing that a stronger sense of *agency*, which he defines as "the feeling that we cause and control our actions and through those actions change our environment" (ibid., p. 10), gives subjects more confidence in their ability to control risk and increases their likelihood of risk-taking behavior.

Overall, it can be observed that the higher the perceived control $\gamma \in [0, 1]$ over the outcome of a situation (0 = no control, 1 = full control), the lower the perceived risk of the situation and thus the higher the perceived chances of success. This means that even if the objective expectation value of production μ is the same in all competitive production contexts with homogeneous and equally skilled individuals, the perceived expected value of production μ increases as the level of perceived control γ increases.

Accordingly, it can be assumed that the compensating institution in this model adjusts compensation for exclusion not only to the objective expected value of the denied income opportunity, but also its relatively higher or lower level of control.[69] This is expressed by the following equation:

$$y_{C'''} = \begin{cases} \alpha + \beta(\gamma\mu - y_A), & \text{if } y_A < \gamma\mu \\ \alpha & \text{otherwise} \end{cases} \tag{3.4}$$

Having established that the expectation value, and hence the appropriate amount of compensation, of a production depends on the corresponding level of control, three specific cases of competitive production are now shortly considered: luck-based L, effort-based E, and strategy-based S.

- **Luck-based Competitive Production:**
  In luck-based competitive production L, such as a lottery, no effort is exerted, only uninfluenceable luck determines the outcome of all participants. Thus the expected value of production is simply determined by the possible outcomes and the number of participants.There is no possibility that individuals can control the situation, so that $\gamma_L = 0$.

- **Effort-based Competitive Production:**
  In an effort-based production E, such as a contest, success is determined by

[69]Note that, since compensation is given for exclusion from the initial production choice, it is only the level of control of this voluntarily chosen production that matters, but not the level of control of the remaining alternative production.

tedious and costly effort over which individuals have full control. Since individuals are assumed to be homogeneous and equally skilled, the results of effort-based production are independent of talent, and the only way to win the contest is to try harder than the others. Thus the most eager of the equally skilled individuals has the highest probability of winning. This cannot be determined in advance, however, because individual effort willingness is not observable.[70] Thus, to an outside observer, all individuals participating in the contest have the same ex ante probability of success, which is determined by the outcomes and the number of participants, as in the lottery. However, since the effort exerted is entirely in the hands of the participants, they have a very high degree of control over the outcomes. In fact, there is no higher degree of control in this context, so that $\gamma_E = 1$.

- **Strategy-based Competitive Production:**
  In a strategy-based competitive production S, a strategic decision determines who wins and who loses. Since individuals have no information to adjust their strategy as the optimal response to that of their opponent, they again have exactly the same ex ante probability of winning as in a lottery or competition. However, based on the above studies, the active strategic decision in S can be expected to result in a perceived level of control that is above that of the lottery but below that of the contest, so that $0 < \gamma_S < 1$.

Applying these types of production to Equation 3.4, it can be seen that voluntarily choosing to participate in the uncontrollable lottery would lead to lower compensation for arbitrary exclusion, while choosing a contest or a strategic game with a higher perceived level of control would justify higher compensation for exclusion. This is also elaborated in Section 4.5.

[70]Individual observations of laziness or eagerness would be complicated, costly, and subject to incentive problems once individuals know they are being observed. However, if one could measure individual differences in willingness to perform, the probability of winning would no longer be defined by the number of participants alone, and more eager excluded individuals would deserve more compensation for exclusion than lazy excluded individuals. The remainder of the thesis, however, sticks to the assumption that individual behavior is not sufficiently predictable.

4 The Experiment

The aim of this dissertation is to experimentally examine whether exclusion is actually differentiated from shirking and, if so, is financially compensated to restore distributive justice. For this purpose, a strategic dictator game is conducted as a laboratory experiment in which the income of recipients is generated by competitive production. Access is either fully voluntary or restricted by arbitrary exclusion. In addition, the experiment allows to analyze whether compensation is affected by alternative incomes and by responsible choices, thereby testing the predictions of the model presented in Chapter 3.

The *No Exclusion* treatment is characterized by full voluntariness of choices: recipients, hereafter referred to as productive players, have the option to participate in three different competitive production games (lottery, contest, and strategic game; explained in Section 4.1) or to suspend production and pause. In the case of *Exclusion*, productive players can still decide voluntarily whether to participate in production, but some players are selected according to arbitrary criteria and automatically excluded from production. Participation in competitive production carries the possibility of winning and receiving a high income, or losing and receiving a low income, while suspension results in a guaranteed safe amount. Section 4.2 provides more details on the rules and participation options for productive players, while Section 4.3 presents the treatment for the dictators who can increase the safe amount of their recipients with their donation. Dictators make the donation decision either for players who do not participate in production due to their own decisions or for excluded players. This allows to examine whether the resulting distribution is consistent with the responsibility principle. To control for heterogeneity in preferences or personal characteristics, the experiment includes social preference elicitation tasks and a questionnaire. These are described in Section 4.4. Finally, Section 4.5 presents the main hypotheses about the expected effects of variations in the production games and the level of the safe amount on redistribution.

4.1 Competitive Production

Production in real life is often competitive: when acquiring orders, selling similar products, buying lottery tickets, or applying for jobs or project funding, all competitors must make decisions and invest effort or money, but only one or a few succeed at the most financially lucrative opportunities. The unsuccessful competitors may still succeed on the second-best opportunities and benefit from their work, but to a lesser extent than the successful ones (e.g. acquiring smaller orders or selling products at a lower price). Therefore, production is modeled below by putting productive players in pairs of two and having them compete against each other. One player wins a high income, $y_{\text{win}} = 100$, the other loses and receives a low income $y_{\text{lose}} = 40$.[71] Throughout the experiment, 100 points is equivalent to 5 €.

In order to test whether Equation 3.4 holds, this is carried out for three different competitive production games: a lottery, a contest, and a strategic game. The different games are chosen to model the income generating processes with different levels of control presented in Section 3.3.3. The exact rules of the three production games are explained in Section 4.1.1 for the lottery, in Section 4.1.2 for the contest and in Section 4.1.3 for the strategic game.

4.1.1 Lottery

To model risky luck-based production, a lottery is used. This follows the literature that considers lottery winnings to be truly exogenous non-labor income that does not depend on individual labor decisions, such as in Imbens et al. (2001) or Lindahl (2005). Lotteries are also the standard game to measure risk aversion, and have been used, for example, in several studies that find that women are more risk averse than men when exposed to gambling[72] (see Eckel and Grossman (2008a) for a comprehensive overview). Moreover, as mentioned in Section 2.2.3, the choice between lotteries and safe amounts followed by redistribution from impartial observers, has already been used by Akbaş et al. (2019) to study the application of the responsibility principle.

In this experiment, players participating in the lottery are randomly matched into groups of two. One player is randomly selected as player 1 and can choose a lottery ticket A or B; the other player is designated as player 2 and is given the remaining ticket. There is a 50% chance that A (or B) will be the winning ticket. The player with the winning ticket receives $y_{\text{win}} = 100$, the other player receives $y_{\text{lose}} = 40$.

[71] If the number of participating subjects is uneven (or only a single subjects enters production), one subject is randomly selected to play alone. In this case, she automatically wins $y_{\text{win}} = 100$ in the contest and in the strategic game. However, in the lottery case, she might lose depending on the ticket she chooses. Although this procedure slightly changes the expected values between the lottery compared to the other two games, it maintains the character of the production games, is easy to understand, and creates transparency for the productive players. It also seems unlikely to affect players' entry into production, since the probability of being an individual player is low and cannot be estimated individually. Indeed, in 36 matching rounds in 2 sessions, there were a total of only 22 situations in which 1 of the 36 players remained unmatched, meaning that the individual probability of becoming a single player was $\frac{22}{36 \cdot 36} \approx 1.7\%$ per round. In any case, the matching problem is irrelevant to the interpretation of redistribution in this experiment: To best inform their donation decisions, dictators were given extensive information about their recipients' production rules, but some technical details, such as the matching problem, were omitted to keep the instructions manageable.

[72] Gender differences in the likelihood to gamble should however not influence dictator giving, as they do not know their recipient's gender.

4.1.2 Contest

In the literature, contest-based competitive production, such as labor markets, is often simulated using rank-order tournaments. In these contests, effort is invested, and the best contestant wins the prize with certainty, i.e., gets the job (Dechenaux et al. 2015, pp. 610–611). This study follows this approach and uses an encryption task first established by Erkal et al. (2011), in the adapted version of Benndorf et al. (2019). Participating players are matched in groups of two and have 60 seconds to encrypt as many "words" as possible. These words consist of random 3-letter-combinations and are different for each player and in each round. On each player's screen, one word is displayed next to an encryption table that randomly assigns a three-digit number to each letter and which is rearranged for each new word. A new word is displayed once a player correctly substitutes the 3-letter-combination of the old word with the numbers from the encryption table and confirms their input. The player who encrypts the most words wins the contest and receives $y_{\text{win}} = 100$ points, while the losing player receives $y_{\text{lose}} = 40$ points. In case of a tie, the winner is the player who confirms her last encrypted word first.

The encryption task has already been successfully used by Lefgren et al. (2016) to examine the application of the accountability principle (see Section 2.2.2). The task is found to be gender neutral by Kuhn and Villeval (2015) (Benndorf et al. (2019) however find that women have a small advantage) and is not subject to learning-by-doing effects (Charness et al. 2014), which is supported by the results of Benndorf et al. (2019).

Although Lefgren et al. argue that encryption effort is independent of talent, in this experiment it is possible that more competitive subjects choose production more often than other subjects, since effort is not assigned but can be freely chosen. However, since the dictators do not participate in the production themselves, this possible selection bias does not affect the analysis of compensation for exclusion.

4.1.3 Strategic Game

To model production as a strategic situation in which one person benefits in the case of equal decisions and the other person benefits in the case of different decisions, a version of the matching pennies (constant sum) game is used in this experiment. A standard example of such a strategic situation is the conflicting interest of goalkeepers and scorers, where the goalkeeper wants to leap in the direction of the shot, but the scorer wants to shoot in the opposite direction than the goalkeeper leaps.

In this symmetric matching pennies game, shown in Figure 4.1, one productive player is randomly chosen as player 1, and the other is player 2. The probability of becoming player 1 is $p = 0.5$ and is identical for both players who simultaneously and independently choose strategy A or B. If both players choose a different strategy, player 1 wins and receives $y_{\text{win}} = 100$, while player 2 receives $y_{\text{lose}} = 40$. If both players choose the same strategy, player 2 wins and receives $y_{\text{win}} = 100$, while player 1 loses and gets $y_{\text{lose}} = 40$.

Figure 4.1: Strategic Game

		1	
		A	B
2	A	100^{40}	40^{100}
	B	40^{100}	100^{40}

This matrix shows the payoffs of players 1 (right superscript number) and 2 (left number) depending on their choice of strategies A or B.

Previous research, e.g. by Ochs (1995), Mookherjee and Sopher (1994), Mookherjee and Sopher (1997) and Erev and Roth (1998), finds, for both symmetric and asymmetric versions of the matching pennies game, that players' prior experience or average scores during the experiment explain their strategic decisions better than equilibrium predictions. However, all of these studies involve games consisting of 40 or more periods, and in some of these games players even remain matched for all rounds. In contrast, this experiment repeats the strategic game only six times and has random matching with strangers each time, so learning effects seem quite unlikely. Therefore, each player's chance of winning a round is given as 50%, which is identical to the lottery. Another similarity with the lottery is the degree of risk aversion: Goeree et al. (2003) have found that a subject's degree of risk aversion is quite stable across different strategic games and is only slightly higher than the individual estimate based on that subject's lottery choice.

Nevertheless, there might be a relevant difference between the lottery and the strategic game, since only in the latter do *both* players take an active choice and thus seem to have some level of control over their outcome. This not only means that the risk associated with the strategic game might be perceived differently by dictators (for the relationship between control and risk perception, see Section 4.5.1), but may also affect outcomes: studying a symmetric matching pennies game similar to this one, Eliaz and Rubinstein (2011) show that it is advantageous to be player 2, as both moving second (even though the opponent's action is unobserved) and having to match the opponent's action slightly increase the probability to win. Whether this difference influences redistribution remains an open question: to my best knowledge, matching pennies games have not been used as a basis for redistribution decisions or in the context of responsibility.

4.2 Production and Exclusion of Productive Players

The three games explained above in Section 4.1 are played in two different treatments, *No Exclusion* and *Exclusion*. For recipients, the treatments are varied within subjects. This is done as their participation behavior is not relevant to the analysis of compensation of exclusion, but instead might offer insights into their expectation on treatment-dependent dictator giving.

The total of six game and treatment variations are presented in random order. Each game is played for six rounds, and productive players decide separately for each round whether they want to participate in that round or prefer to suspend production. Suspending means waiting while participating players complete the round and receiving a safe amount y_{out} which varies over the six rounds of a game, so that $y_{\text{out}} \in \{0, 20, 40, 60, 80, 100\}$, and is known to the players. In the *Exclusion* treatment, some players who decide to participate in the games are excluded from production and automatically assigned to suspension. In this case, they also receive a safe amount, $y_{\text{ex}} \in \{0, 20, 40, 60, 80, 100\}$, which is identical to the safe amount for voluntary suspension y_{out}.

Furthermore, the safe amount can be increased by donations. For each treatment, one dictator is assigned to a player. The dictator in the *No Exclusion* treatment can make a donation to increase the safe amount for voluntary suspension y_{out}, whereas the other dictator, in the *Exclusion* treatment, can only increase the safe amount for exclusion, y_{ex}, but not y_{out} (for details on dictator donations, see Section 4.3). This fact is known by players, but they have no information on the amount donations.

The two different treatments, *No Exclusion* and *Exclusion*, are used in this experiment to model discrimination-free and discriminatory settings, respectively. *No Exclusion* is discrimination free, because all players can participate in the games according to their own decisions. In *Exclusion*, some players are excluded from production despite their decision to participate in the games. Here, exclusion is discriminatory, as it is based on a player's cubicle number in the laboratory. The cubicle numbers are a standard feature of the lab to identify otherwise identical cubicles, all of which are equipped with computers and separated by walls and curtains. They are regularly used for random placement of subjects in the laboratory: upon entering the lab, subjects choose from a stack of shuffled flipped down cards and take a seat in the cubicle identified by the number on the chosen card. Cabin numbers are used as an exclusion criterion because they cannot be influenced by the subjects and represent an obviously arbitrary criterion, since they are neither related to performance in the games nor randomly selected, but are determined in advance by the experimenter. Furthermore, this procedure does not collide with ethical concerns and can be communicated transparently.

It should be noted that this mode of exclusion is still an unbiased random process, since the initial placement in the laboratory is randomly determined. Moreover, each cubicle is selected for exclusion in exactly one game, so each player experiences the automatic assignment to suspension once.[73] Therefore, this process does not

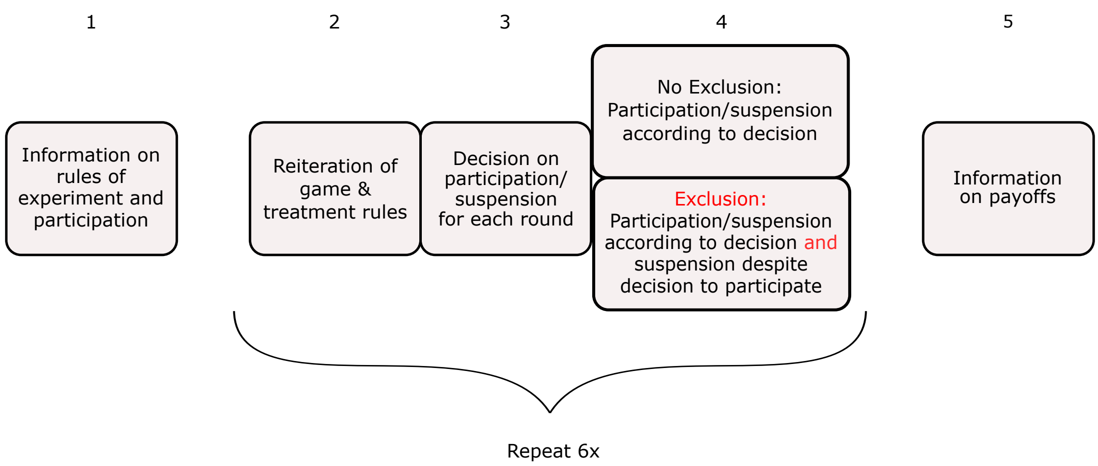
[73] That is, only if players decide to participate in that game for at least one round. If they decide to suspend production in each round, they would not learn that they are designated for exclusion.

constitute true discrimination, which is tied to more personal characteristics and is experienced only by some, but not all, members of a society. However, this argument only strengthens the analysis: if this design, although modeling discrimination in a rather mild form, provides evidence that exclusion is compensated, then it can be concluded that discrimination costs and their compensation are even more relevant in real contexts with actual discrimination.

The fact that they are excluded is revealed to the affected players only after they have completed their participation decisions for all rounds of a game, and only if they have decided to participate. However, the possibility of exclusion is public knowledge: at the beginning of the experiment, in stage 1, players are informed about the two different treatments *No Exclusion* and *Exclusion*, about the possible donations by dictators, and about the exact rules of all three production games as described in Section 4.1, which includes playing a non-incentivized trial round of these. In addition, the options of participating in a game or suspending production and the corresponding payoffs are described in detail.

After this initial information stage, productive players enter the production phase. This is the main part of their experiment, the main structure of which is depicted in Figure 4.2.

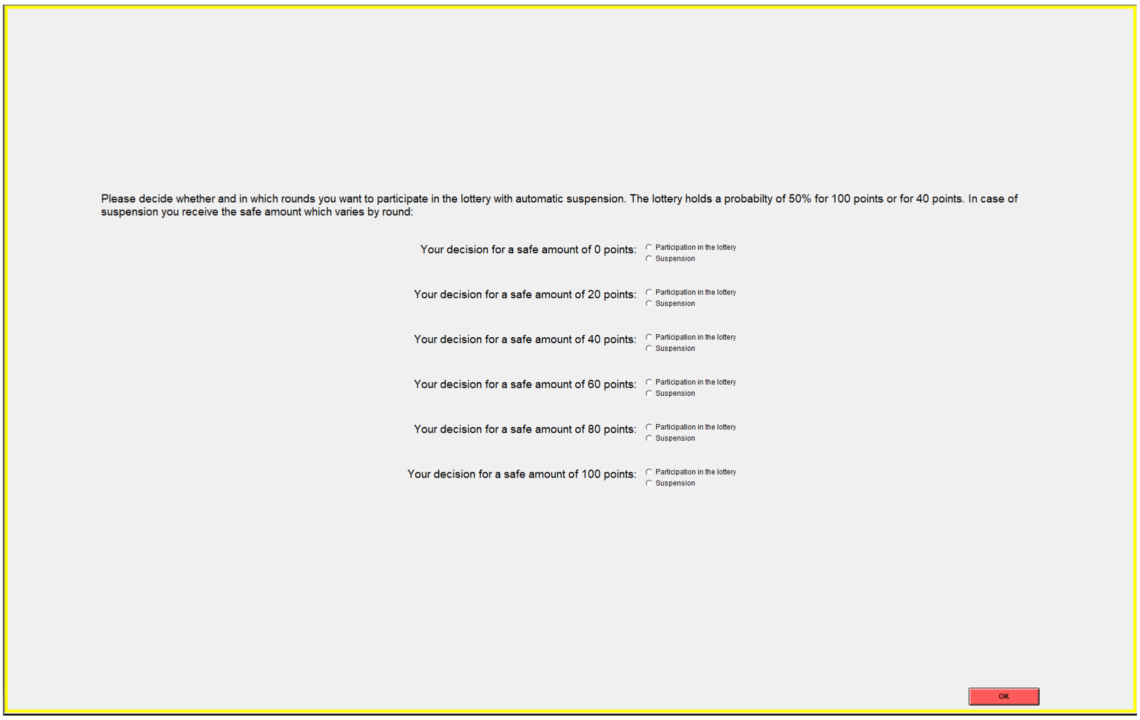
Figure 4.2: Stages of Productive Players' Main Part of the Experiment



Stages 2–4 are played six times in a random order, as each of the three production games are played one time with exclusion and one time with full choice.

The production phase consists of stages 2 to 4, which are repeated six times, so that each of the three games is played once in each treatment variation. In stage 2, productive players are told which production game in which treatment variant they can play in the next six rounds, and they are given an information screen that again briefly recounts the respective main game and (using Figures 4.4 and 4.5) donation rules. Then they enter stage 3, where they face a participation decision screen, which is depicted in Figure 4.3.

Figure 4.3: Participation Decision



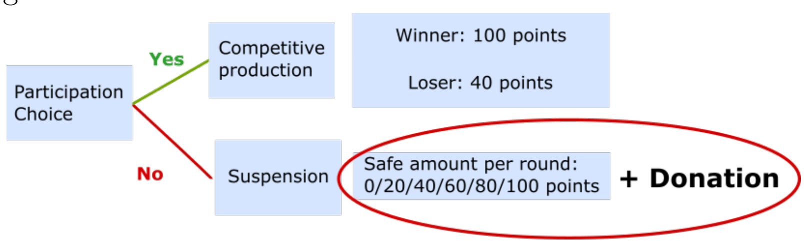
This screen shows the example of the lottery game in the *Exclusion* treatment. Production game and treatment variation are always specified when players make their participation decisions.

The screen displays each round with the corresponding amount of y_{out} and the options "Participation in the [game]" and "Suspension". The amounts for winning and losing the game are also once again displayed, so that players are fully informed on both the opportunity costs and the expected value of production (which is always given by 70 points, as $y_{\text{win}} = 100$ and $y_{\text{lose}} = 40$ are held constant across all games). The safe amount y_{out} is displayed in a continuous order to avoid inconsistencies. Furthermore, to avoid ordering effects, y_{out} is displayed in increasing order ($y_{\text{out}} \in \{0, 20, 40, 60, 80, 100\}$) for 50% of productive players per session, and in decreasing order ($y_{\text{out}} \in \{100, 80, 60, 40, 20, 0\}$) for the other half. In total, each productive player makes 36 entry decisions, with one decision for each of the six different safe amounts in each of the six variations of games and entry mode. This allows comparing participation decisions on a within-subject level with respect to the different treatments, safe amounts, and production games.

In stage 4, the participating non-excluded players are randomly matched to an opponent for each of their selected rounds. Each round played by these groups of two results in a winner with income $y_{\text{win}} = 100$ and a loser with income $y_{\text{lose}} = 40$. For the rounds in which players decide to suspend production, they receive the safe amount of $y_{\text{out}} = y_{\text{ex}} \in \{0, 20, 40, 60, 80, 100\}$. In the 6 rounds of each game, all players participate or suspend production according to their participation decision, unless their cubicles are designated for exclusion. In this case, players who have previously opted to participate in production are automatically excluded from production and instead receive the safe amount y_{ex}. Only at this moment they are informed about their exclusion.

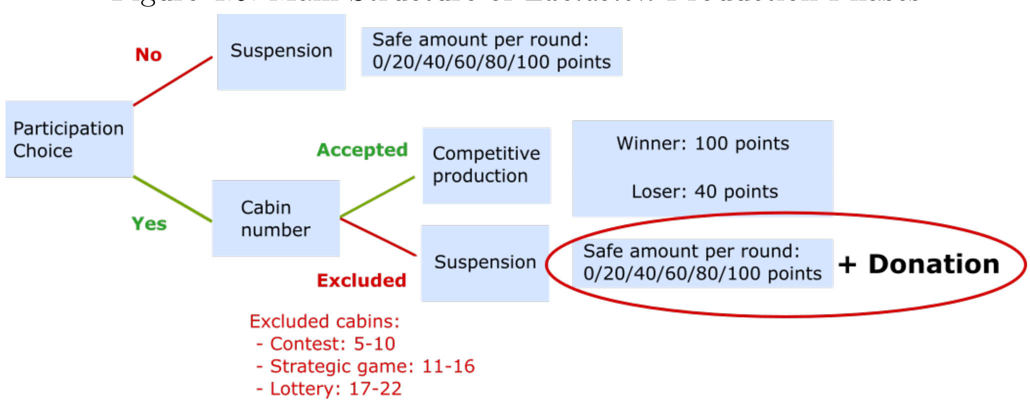
This procedure ensures that players' participation decisions are not influenced by their knowledge of their exclusion from a particular game, but only by the expected values of the games, the opportunity cost of production, and the potential donation. As noted above, donations in the *No Exclusion* treatment can only increase the safe amount when production is voluntarily suspended, whereas in *Exclusion* donations can only increase the safe amount of players who want to participate in production but are excluded. This within-subject variation in treatment provides information about productive players' expectations regarding dictators' fairness norms and donation behavior, and is highlighted by the red-circled elements in Figures 4.4 and 4.5. These figures depict the main structure of a productive player's production phase for the two treatments *No Exclusion* and *Exclusion*, respectively.[74]

Figure 4.4: Main Structure of *No Exclusion* Production Phases



In the *No Exclusion* production phase, players participate or suspend production according to their decision. Only in the case of voluntary suspension they receive a donation in addition to the safe amount.

Figure 4.5: Main Structure of *Exclusion* Production Phases



In the *Exclusion* production phase, players suspend production according to their decision. If they choose to participate, they either take part in the production or are excluded and have to suspend against their decision, depending on their cubicle number. Only in the case of this involuntary exclusion they receive a donation in addition to the safe amount.

[74] These figures are used in the respective dictator instructions for *No Exclusion* and *Exclusion*, which can be found in Appendix A.1.2 and A.1.3. They are also shown to productive players in stage 2 to inform them of the relevant treatment variation for the following production phase, but without specifying which cubicle numbers are targeted for exclusion in which games.

At the end of the experiment, in phase 5, all players are informed about their payoffs. These are based on their participation in and the results of two rounds previously randomly determined to be payoff-relevant (one round from the treatment *No Exclusion*, the other round from *Exclusion*). They are also given their dictators' donations and asked to rate them with a short text of no more than 600 characters. Finally, subjects are asked to complete a questionnaire that includes social background variables as well as expectations about the dictators' donation behavior. They then receive their individual payoffs and leave the lab. Instructions for the productive players can be found in Appendix A.

4.3 Dictator Decision on Redistribution

The dictator game is conducted prior to the productive players' sessions. The dictators are endowed with 200 points and assigned one of the players from the productive players' sessions as recipient. For simplicity, this is done by matching subjects with the same cubicle number. Then, dictators are asked to decide how many points D they want to donate to their recipients, where $0 \leq D \leq 100$ points. The level of endowment is chosen from the literature, with \$10 being the standard endowment of dictators (see e.g. the meta-study of Engel 2011). The amount of donation is capped to ensure that dictators receive the minimum payoff recommended by the laboratory even if they choose to donate the maximum.

For dictators' donations, the strategy method[75] is used, that is, dictators decide for each safe amount in each game, how much they give to their recipients if these to not participate in the production games. Of these 18 donation decisions, only one decision is randomly chosen as payoff relevant.

The differences in dictators' donations between the two treatments *No Exclusion* and *Exclusion* are the main focus of this study, as the resulting treatment effect reflects compensation for exclusion. For dictators' decisions, a between-subjects design is used. The between-subjects design also minimizes the experimenter-demand effect. This well-known effect refers to subjects' tendency to alter their behavior to help the experimenter to get the "right" results. It has long been known, see, e.g., Rosenthal (1976) or Orne (1981). The actual extent and critical relevance of this effect is still subject to research (see, e.g., Quidt et al. 2018), but it has been shown that varying treatments only between, rather than within, sessions ensures that the experimenter demand effect is negligible (Zizzo 2010, pp. 90–91).

Therefore, for the dictators, the treatments *No Exclusion* and *Exclusion* are varied between subjects: one half of the dictators decides only on the donation for recipients in *No Exclusion* (when suspension results only from a productive player's

[75] As shown in the study by Brandts and Charness (2011), treatment effects found with the strategy method are also observed with direct-response methods. For this reason and since it also ensures an equal number of observations of dictator decisions on donations for all possible outcomes of recipients, the strategy method is chosen for this design.

responsible choice); the other half of the dictators decides only on the donation for the same recipients in *Exclusion* (when a productive player chooses to participate in a game but is excluded). Consequently, there are twice as many dictators (72) as recipients (36). Because the two dictator treatments are each conducted in two sessions with 18 participants, dictators from the first and second sessions are assigned to recipients with the same cubicle number from the first and second sessions of productive players, respectively.

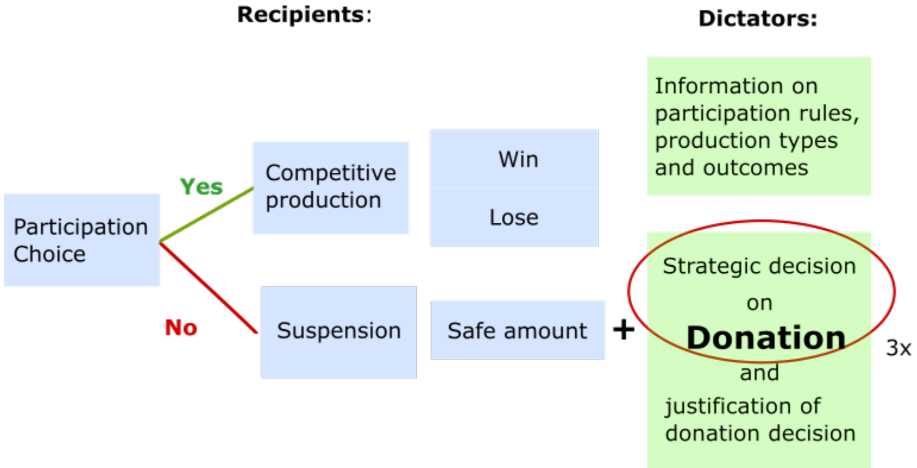
Dictators receive information only on their own treatments. After strategically deciding on the donations for all six rounds of all three games in the respective treatment variation, for each dictator, one of these 18 donations is randomly selected as payoff-relevant for herself and her recipient. The payoff-relevant round is not revealed and paid out to the dictators until after the productive players complete their sessions. This is due to the timing of dictators' and recipients' decisions: in *No Exclusion*, donations can only be transferred if recipients voluntarily suspend production in the payoff-relevant round, whereas in *Exclusion*, donations can only be transferred if recipients choose to participate in the payoff-relevant round but are excluded. Only if these conditions are met, is the strategically chosen donation transferred to the recipient and the dictator's endowment reduced accordingly. If the donation conditions are not met, dictators keep their initial endowments.

The treatment design is depicted in Figure 4.6 for the *No Exclusion* treatment, and in Figure 4.7 for the *Exclusion* treatment. One can see that the only difference between the treatments is the absence or presence of discriminatory exclusion. Any resulting treatment effect would be captured by differences in dictators' donations between these treatments. Therefore, these donations are the variable of interest when examining compensation for exclusion, as highlighted by the red circled elements.

In stage 1, at the beginning of their sessions of the experiment, dictators receive comprehensive information. They are informed that 100 points are equivalent to 5 €; that they have an endowment of 200 points; have to make strategic donations for recipients, one of which becomes binding; and that donations can be freely chosen from 0 to 100 points. Dictators are also informed on the rules of the three games, described in Section 4.1. To further familiarize them with the effort necessary to solve the tasks and the risks associated with the different games, they play an unincentivized trial round for each production game.

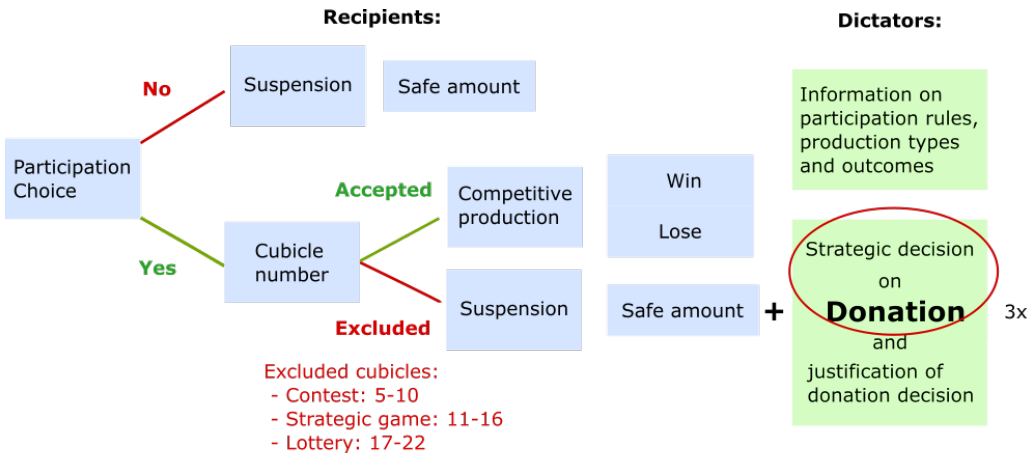
The instructions also detail the recipients' options to participate in the games or to suspend production, as well as the corresponding payoffs. In this respect, the dictators' instructions for the *No Exclusion* treatment differ slightly from the *Exclusion* treatment, since only in the latter are dictators informed about the existence of exclusion and the rules of exclusion, i.e. which cubicle numbers are excluded in which games, and that exclusion only affects players that decide to participate in production. For better understanding, the instructions for *No Exclusion* and *Ex-*

Figure 4.6: Dictators: *No Exclusion* Treatment



In the *No Exclusion* treatment, dictators make strategic donation decisions for all 3 games without automatic suspension. The decisions screens for the 3 different games are presented in random order.

Figure 4.7: Dictators: *Exclusion* Treatment

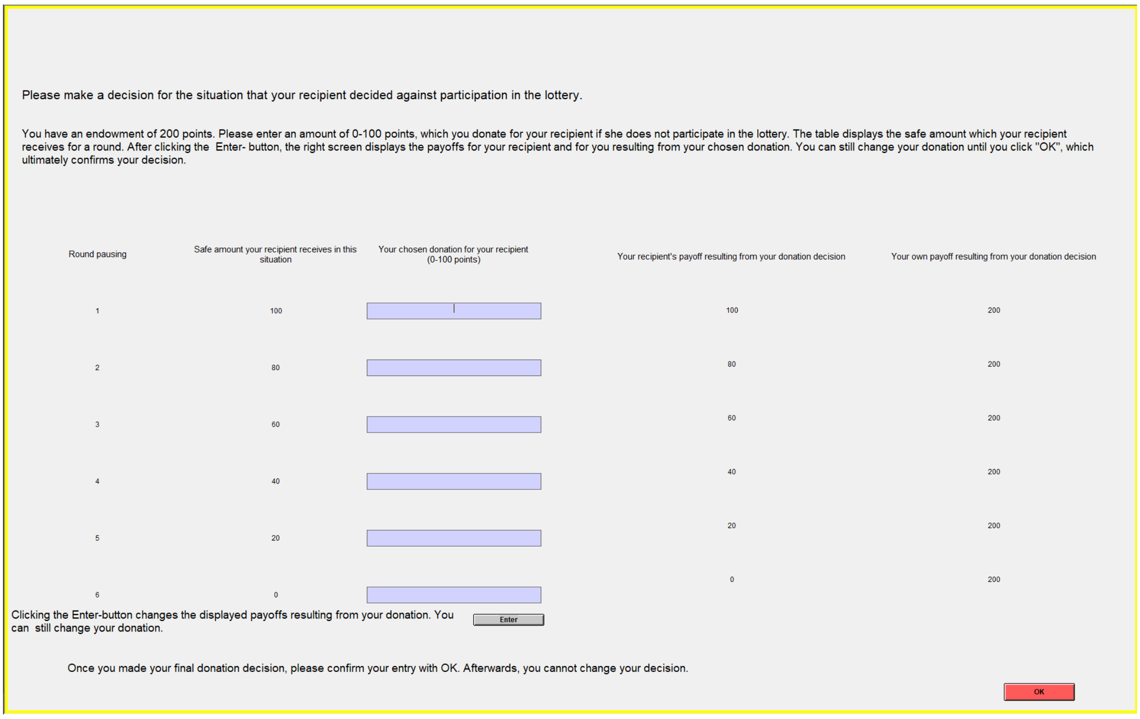


In the *Exclusion* treatment, dictators make strategic donation decisions for all 3 games with automatic suspension. The decisions screens for the 3 different games are presented in random order.

clusion include Figures 4.4 and 4.5, respectively. In both treatments, dictators are aware of their recipients' situation: dictators in the *No Exclusion* treatment know that they can only donate for recipients that voluntarily suspend production, while dictators in the *Exclusion* treatment know that they can donate only to recipients who are excluded against their will.

After the comprehensive information stage, dictators enter stage 2, where they are asked to strategically decide on the 18 donation amounts for all six rounds of the three games. The decision screen is presented in Figure 4.8. The screen displays the varying safe amounts $y_{\text{out}} \in \{0, 20, 40, 60, 80, 100\}$ associated with voluntary suspension in case of the *No Exclusion* treatment, or $y_{\text{ex}} \in \{0, 20, 40, 60, 80, 100\}$ associated with exclusion in case of the *Exclusion* treatment. To avoid inconsistencies and ordering effects, these safe amounts are displayed in continuously increasing or decreasing order in two randomizations for each session.

Figure 4.8: Donation Decision



This decision screen shows the example of a lottery game in the *No Exclusion* treatment. Game and treatment are always specified when dictators are asked to make their donation decisions. The safe amount is continuously displayed in increasing order for 50% of subjects and in decreasing order for the other 50%.

In the input fields next to the safe amounts, dictators enter the preferred donation amount D. When subjects confirm their donations, the computer calculates the resulting payoffs for the dictator and the recipient and displays them on the screen. Dictators have the option to change the donation until they finally confirm it by clicking the OK button. After confirmation, dictators are presented with an income matrix. It displays all possible payoffs of their recipient, conditioned by the different safe amounts, the donation, whether they win or lose the production task, and (in *Exclusion* only) the safe amount for voluntarily suspension. The income matrix is presented to the dictators so that they are able to consider distributive fairness between themselves and all their recipient's potential egos. It is supplemented by a text gadget in which dictators are asked to give reasons for their donation choice in an open questionnaire limited to 600 characters. Dictators' statements are later categorized according to the underlying fairness ideals to provide some additional information on relevant fairness norms (see Section 5.2.1). The procedure is conducted once for each of the three production games, the order of which is randomly determined.

After dictators complete the three rounds of stage 2, they are asked to fill out a questionnaire that asks for information about their social background and other aspects (see Section 4.4.3). Dictators are then allowed to leave the laboratory one at a time. Remember that they receive no payoff upon leaving the lab, but have to confirm their cubicle number, which is later used to match dictators with recipients.

About two weeks later, after the recipient session is completed and the resulting payoffs are calculated, the dictators are notified by email that they can collect their payoffs at the lab's office. When they pick up their money, they also receive a printout showing a brief summary of the payoff-relevant factors. All printouts are individualized by cubicle numbers and provide information on the payoff-relevant round, the dictator's decision for that round, their recipients decision on whether to participate and, if so, whether they have been excluded (only for dictators in the *Exclusion* treatment), and the resulting payoffs for the dictators and recipient. These printouts, the template of which can be found in Appendix B.3.2, are shredded after reading. The instructions for the dictators' main part of the experiment can be found in Appendix A.1.2 and A.1.3.

4.4 Controls

As mentioned above, subjects are asked to complete a short questionnaire on socio-demographic aspects to control whether the composition of participants in the different treatments is sufficiently similar to each other. Only in this case, the difference in behavior between treatments can actually be interpreted as a treatment effect and not as a bias due to differences between participants. In addition, some control questions are asked about the experimental design and the responsibility principle, see Section 4.4.3 for details.

The answers in the questionnaire are not incentivized. Therefore, some responses may not reflect the true characteristics of a participant, but rather, for example, social conformity (Bertrand and Mullainathan 2001). Such a difference between stated and revealed preferences has been extensively demonstrated in environmental economics (see e.g. Kroes and Sheldon (1988), Adamowicz et al. (1994), or Murphy et al. (2005) for an overview). Hence in experimental economics, risk or distributional preferences are essentially always calculated from incentivized options. Still, Gaertner and Schokkaert (2012) argue that while behavior cannot be predicted by questionnaire studies due to cheap talk, responses to values and social norms by questionnaire are informative and not arbitrary. They also find that people generally self-report the same fairness ideal that is revealed in their distributional behavior, which is (mildly) supported by Beranek et al. (2015) who find weak correlations between stated and revealed preferences.

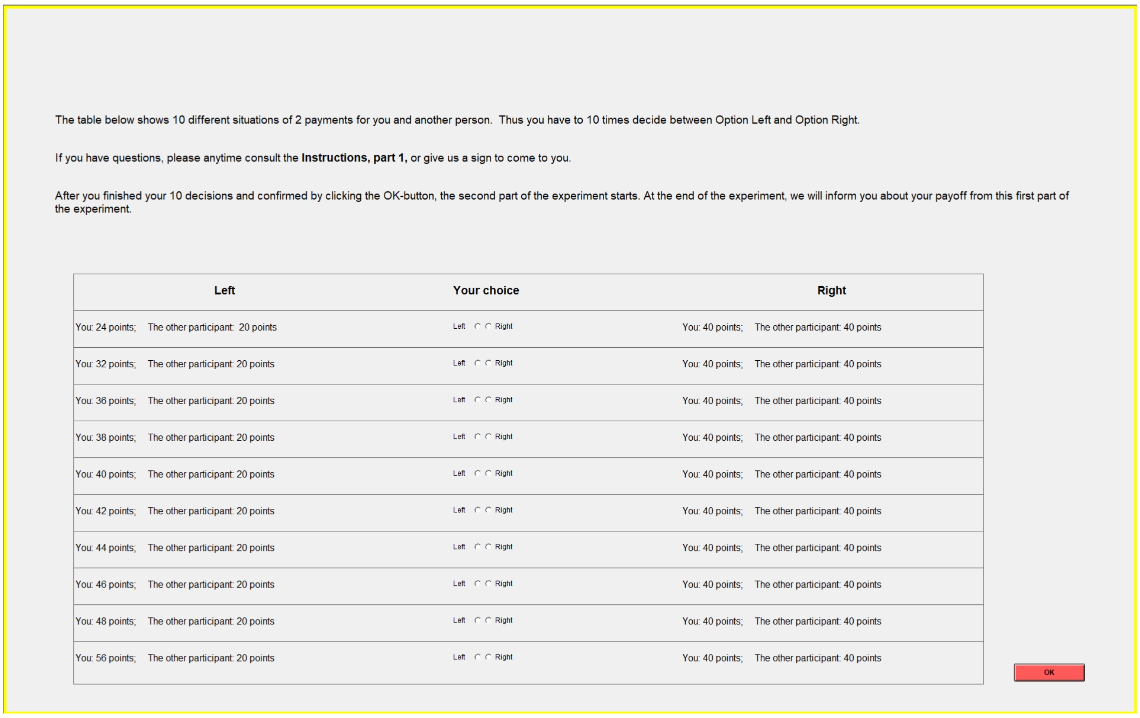
Since the fairness preferences of the dictators are the central aspect of this experiment, they are both asked in the questionnaire and elicited with an incentivized task on inequality aversion, explained in Section 4.4.1. For the recipients, the main treatment variation consists of the different levels of control and hence the different perceived risk levels of the production games, as explained in Section 4.4.2. Thus, instead of inequality aversion, their risk preferences are elicited with an incentivized task. Both dictators and recipients play the incentivized elicitation tasks at the beginning of the experiment, but they are not informed about their payoffs until the end of the experiment to avoid behavioral changes due to wealth or house money effects (see Thaler and Johnson 1990).

4.4.1 Elicitation Task on Social Preferences of Dictators

As reported in Section 2.2.2, empirical distributive preferences are heterogeneous. To ensure that differences in dictators' donations between treatments can correctly be interpreted as the treatment effect of exclusion, the frequency of subjects with egalitarian, selfish, and other distributional preferences must be equally distributed among treatments. To test for this, dictators' distributional preferences are elicited with the Equality Equivalence Test (EET) by Kerschbamer (2015), in an extended version by Kerschbamer and Müller (2020). In twenty decision problems, dictators are asked to choose one of two payoff pairs that define the income of the deciding subject, the "active person", and another randomly assigned "passive person". One of the choices is randomly selected for payoff. In addition, each subject receives a second payoff resulting from another subject's active decision in which she acts as the "passive person".

The decision problems are presented using the double price list technique: Income pairs are listed as rows in a table, and active subjects choose which side of the table (left or right) they prefer. The task is divided into two randomly arranged blocks: In one block, subjects are shown ten payoff pairs in which efficiency (the number of total points) is traded-off with advantageous inequality. This is shown in Figure 4.9. In the other block of 10 payoff pairs, subjects have to trade efficiency with disadvantageous inequality. The corresponding screen is included in the instructions and can be found in Appendix A.2.1. The two blocks allow for the measurement of inequality aversion and efficiency preferences, respectively, because they are designed

Figure 4.9: Advantageous Inequality Block


The table below shows 10 different situations of 2 payments for you and another person. Thus you have to 10 times decide between Option Left and Option Right.

If you have questions, please anytime consult the **Instructions, part 1,** or give us a sign to come to you.

After you finished your 10 decisions and confirmed by clicking the OK-button, the second part of the experiment starts. At the end of the experiment, we will inform you about your payoff from this first part of the experiment.

Left	Your choice	Right
You: 24 points; The other participant: 20 points	Left ◌ ◌ Right	You: 40 points; The other participant: 40 points
You: 32 points; The other participant: 20 points	Left ◌ ◌ Right	You: 40 points; The other participant: 40 points
You: 36 points; The other participant: 20 points	Left ◌ ◌ Right	You: 40 points; The other participant: 40 points
You: 38 points; The other participant: 20 points	Left ◌ ◌ Right	You: 40 points; The other participant: 40 points
You: 40 points; The other participant: 20 points	Left ◌ ◌ Right	You: 40 points; The other participant: 40 points
You: 42 points; The other participant: 20 points	Left ◌ ◌ Right	You: 40 points; The other participant: 40 points
You: 44 points; The other participant: 20 points	Left ◌ ◌ Right	You: 40 points; The other participant: 40 points
You: 46 points; The other participant: 20 points	Left ◌ ◌ Right	You: 40 points; The other participant: 40 points
You: 48 points; The other participant: 20 points	Left ◌ ◌ Right	You: 40 points; The other participant: 40 points
You: 56 points; The other participant: 20 points	Left ◌ ◌ Right	You: 40 points; The other participant: 40 points

OK

This figure shows the decision screen for the EET block in which subjects decide between advantageous inequality and efficient equality. The order of this screen and the one displaying the disadvantageous inequality block is randomized.

so that subjects with monotonic equity-equality-preferences switch from right to left at most once and never in the other direction (see Kerschbamer (2015, pp. 94–98) for a more detailed description of the EET).

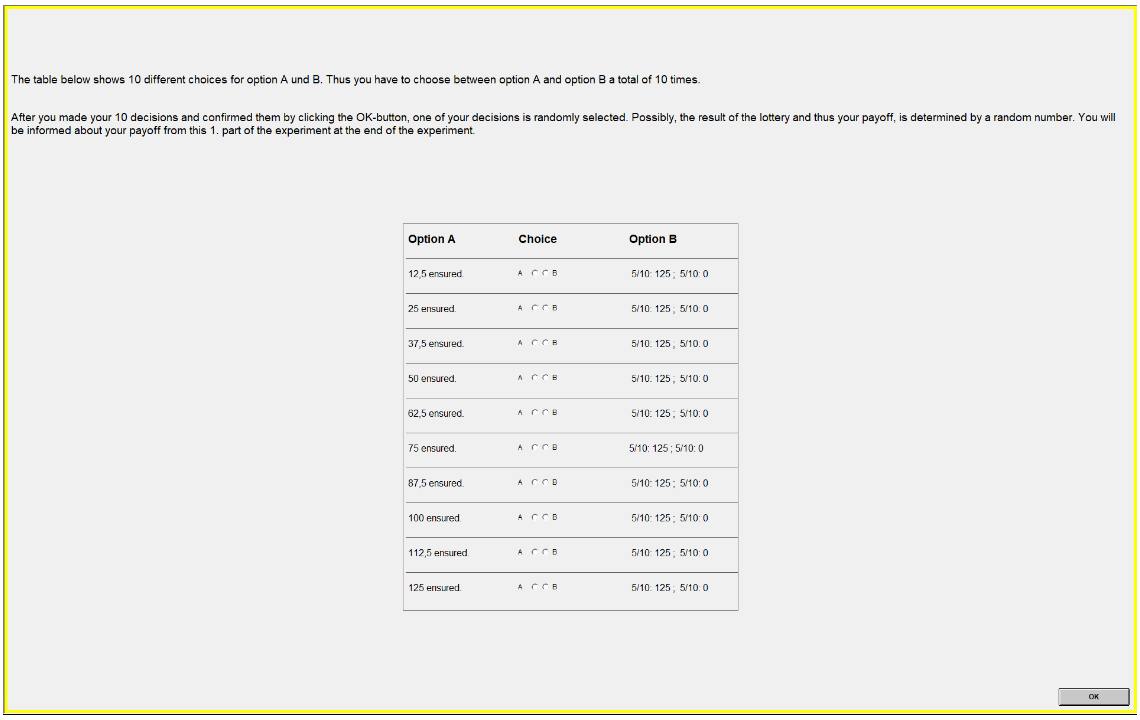
In the advantageous inequality block, the switching point from the more efficient equal payoff pair to the self-advantageous allocation elicits a subject's willingness-to-pay for advantageous inequality $WTP^a \in [-0.8, 1]$. A positive WTP^a indicates that a subject sacrifices efficiency for equality; a negative value indicates that the subject prefers the more efficient allocation. Analogously, in the disadvantageous inequality block, the switching point from the equal payoff pair to the more efficient but self-disadvantageous allocation elicits a subject's willingness-to-pay for disadvantageous inequality, $WTP^d \in [-1, 0.6]$. Here, a positive value of WTP^d indicates that a subject prefers efficiency despite receiving a lower income than the "passive person". A negative value shows that a subject prefers equal payoffs over the more efficient, but disadvantageous allocation. Appendix A.1.1 provides the instructions for this first part of the dictators' experiment.

4.4.2 Elicitation Task on Risk Aversion of Recipients

Since the three production games involve different levels of (strategic) risk, an elicitation task on recipients' risk preferences is conducted. The risk level of a game might affect subjects' participation in production, as the literature argues that income uncertainty affects the labor allocation of risk-averse economic agents. The direction of the predicted influence of risk aversion on labor allocation is however controversial and depends, among other things, on assumptions about utility functions and their response to risk (for theoretical foundations, see, e.g., Knight 1921, Block and Heineke 1973, Tressler and Menezes 1980, and Chiu and Eeckhoudt 2010). Such an effect of risk has indeed been demonstrated experimentally, though mainly for behavior in the asset market (see e.g. Eckel and Grossman (2008b) for a short overview), but also for strategic choices by Klijn et al. (2013). The elicitation task is conducted only for recipients, since dictators do not participate in the different games and it is shown, e.g., by Rohde and Rohde (2011), that the risk of others, in contrast to own risk, does not influence subjects' decisions.

In experimental economics, the Certainty Equivalence Test (CET), a lottery selection task developed by Holt and Laury (2002), is the standard procedure to elicit subjects' risk preferences. The CET is also used in this experiment, following the modification of Balafoutas et al. (2012, p. 128), which allows for more precision due to the measurement of preference intensity. Subjects are asked to make 10 binary choices between a coin-flip lottery and a safe payoff (randomly displayed in either increasing or decreasing order), with only one of this choices being randomly selected for payoff. The corresponding decision screen is shown in Figure 4.10.

Figure 4.10: Elicitation of Risk Preferences


The table below shows 10 different choices for option A und B. Thus you have to choose between option A and option B a total of 10 times.

After you made your 10 decisions and confirmed them by clicking the OK-button, one of your decisions is randomly selected. Possibly, the result of the lottery and thus your payoff, is determined by a random number. You will be informed about your payoff from this 1. part of the experiment at the end of the experiment.

Option A	Choice	Option B
12,5 ensured.	A ○ ○ B	5/10: 125 ; 5/10: 0
25 ensured.	A ○ ○ B	5/10: 125 ; 5/10: 0
37,5 ensured.	A ○ ○ B	5/10: 125 ; 5/10: 0
50 ensured.	A ○ ○ B	5/10: 125 ; 5/10: 0
62,5 ensured.	A ○ ○ B	5/10: 125 ; 5/10: 0
75 ensured.	A ○ ○ B	5/10: 125 ; 5/10: 0
87,5 ensured.	A ○ ○ B	5/10: 125 ; 5/10: 0
100 ensured.	A ○ ○ B	5/10: 125 ; 5/10: 0
112,5 ensured.	A ○ ○ B	5/10: 125 ; 5/10: 0
125 ensured.	A ○ ○ B	5/10: 125 ; 5/10: 0

OK

This is the decision screen for the CET in which subjects choose between option A, a safe payoff displayed randomly in increasing or decreasing order, and option B, a coin-flip lottery.

The binary lottery with a 50% probability each of winning 125 (6.25 €) points or nothing is held constant for the 10 rounds. In contrast, the safe amount is continuously increased in 12.5-point increments from 12.5 points to 125 points. When a subject behaves rationally, she switches from the lottery option to the safe payoff exactly once.

Using this switching point, an index of risk attitude is constructed by dividing the number of the switching point by ten and assigning subjects a score $R \in [0, 1]$. Very risk averse subjects who choose the safe payoff from the beginning are assigned $R = 0.1$. $R = 1$ is assigned to those subjects who choose the safe payment only in the last decision, when the lottery is first order stochastically dominated by the safe payoff. Thus, lower values of R indicate risk aversion, higher values indicate risk seeking, and $R = 0.5$ indicates risk neutrality. The instructions for this first part of the recipients' experiment are provided in Appendix A.2.1.

4.4.3 Questionnaire

The questionnaire that all participants complete at the end of the experiment can be found in Appendices B.1.2 and B.2 for dictators and recipients, respectively. It contains standard questions on gender, age, as well as field and duration of studies, as these social background variables have been shown to influence generosity or risk aversion in some studies (see Section 2.2.4 and e.g. Andreoni and Vesterlund (2001), Eckel and Grossman (2008b), and Charness and Gneezy (2012) on gender differences, and e.g. Marwell and Ames (1981) and Cappelen et al. (2015) on the comparatively selfish behavior of economists). In addition, subjects are asked to indicate their risk preferences on a 5-point scale ranging from "very risk averse", over "rather risk averse", "risk neutral", and "rather risk loving", to "very risk loving" to control for potential bias in behavior due to risk preferences.

Questions related to Expected Behavior

Beyond these questions, dictators and recipients are asked about their beliefs on the behavior of their respective counterpart. These questions are phrased differently for dictators and recipients. Since the analysis of compensation for exclusion focuses on the dictator experiment, only the exact questions asked of dictators are elaborated below, while the questions asked of recipients will be described only briefly here but can be found in Appendix B.2.

First, dictators are asked at what threshold of the safe amount they would suspend production, and whether this depends on the type of game. The purpose of this question is to compare dictators' hypothetical suspension with recipients' actual suspension to determine whether dictators are realistic about their recipients' situation, but also to control for whether, if any, extreme views about participation (always/never) influence generosity.

Imagine you were in place of your recipient: When would you decide to suspend production?	Always (I would never participate) Starting from the safe amount of 20 Starting from the safe amount of 40 Starting from the safe amount of 60 Starting from the safe amount of 80 Only starting from the safe amount of 100 Never (I would always participate) Depending on the type of game

In addition, dictators are asked to indicate in which game they expect the most or least players to voluntarily suspend production at a safe amount of 40. These questions are used first, to obtain information on which production game is perceived as the most difficult/easiest, and second, to control for whether these expectations influence donations.

Please make a guess: In which game do the <u>most</u> participants of the subsequent experiment choose to suspend production for a safe amount of 40?	Lottery Contest Strategic game

Please make a guess: In which game do the <u>least</u> participants of the subsequent experiment choose to suspend production for a safe amount of 40?	Lottery Contest Strategic game

The questions asked of recipients mirror those asked of dictators. To see if expected donations influence participation behavior, recipients are asked up to what safe amount they would donate something if they were dictators. They are also asked for which game they expect most/least donations to control participation for these expectations, and to compare these expectations to actual donations.

Questions related to Fairness Norms

The remaining part of the questionnaire deals with fairness norms and responsibility preferences. Dictators are asked to rate the fairness of the game to their recipients on a 6-point scale from "very fair" to "very unfair" and to explain their response to determine whether exclusion reduces the perceived fairness of the experiment and whether this is explicitly stated. This question is mirrored in the recipients' questionnaire, where subjects are asked to rate the fairness of their experiment on the same 6-point scale, but without explanation.

How fair do you perceive the rules for your recipients?	Very fair Fair Rather fair Rather unfair Unfair Very unfair

Please shortly explain your answer

Dictators are also asked to self-report their fairness preference on a 4-point scale, ranging from "not at all important" to "very important". The question is supplemented by asking them to indicate, on a 6-point scale ranging from "not at all" to "very strongly", how strongly they ensured that their recipient received a fair payoff. The two questions are asked with the intent to compare them with the elicited inequality aversion and strategic donations of dictators, to allow a more precise categorization of dictators' fairness preferences and to examine their influence on giving behavior.

How important is fairness to you?	Not at all important Not important Important Very important

When making your donation decision, to what extent did you consider the fairness of your recipient's payoff?	Not at all Little Rather little Rather strongly Strongly Very strongly

The final two questions of the questionnaire cover principles of distributive justice to allow the comparison whether subjects' stated distributional preferences match their revealed ones. The second last question asks respondents to what extent their willingness to help someone depends on the person's responsibility for the difficult situation, while the last question asks respondents to select their preferred method of social security from four options that differ in their preconditions.

Under which circumstances do you help someone in a difficult situation?	Under all circumstances. Only if the situation is self-imposed. Only if the situation is not self-imposed. Rather if the situation is self-imposed. Rather if the situation is not self-imposed. Under no circumstances.

Which option of social security do you prefer?	Unconditional basic income Conditional basic security such as Hartz IV [German workfare system] Social security depending on working entitlements such as an unemployment insurance No social security, instead individual responsibility and donations in cases of emergency

4.5 Hypotheses

It follows from the experimental design that donation differences between the *No Exclusion* and *Exclusion* treatments capture financial compensation for discriminatory exclusion. Since the aim of this thesis is to analyze whether recipients are compensated for exclusion and whether the responsibility principle is applied, dictator giving is the main focus of the analysis. The following Section 4.5.1 presents the main hypothesis on compensation for exclusion, as well as additional hypotheses on how compensation is influenced by varying the production game and the outside option in terms of the safe amount. Subsequently, Section 4.5.2 briefly outlines the expected participation behavior of productive players and how it might be influenced by expected donations.

4.5.1 Expected Behavior of Dictators

Donations from dictators in the *Exclusion* treatment, D_1, can be expected to be higher than donations from dictators in the *No Exclusion* treatment, D_0, as dictators compensate for the arbitrary discriminatory exclusion of their recipients. This leads to the following hypothesis:

> **Hypothesis 1 (Compensation for exclusion)** *Donations are higher for excluded recipients than for recipients who voluntarily suspended production;* $D_1 > D_0$.

In addition to the between-subjects treatment effect posited in Hypothesis 1, the effect of within-subject variation in terms of the production games and the safe amounts on donations will also be examined. With respect to the variation in production games presented in Section 4.1, it can be expected that, given identical outcomes, the actual or perceived control over these outcomes (see Section 3.3.3) in the respective games is crucial for dictators' giving decisions. Control over outcomes in risky situations is perceived differently from control over risk exposure (see Nordgren et al. 2007), which recipients in all three games in both treatments can fully control through their participation or suspension decisions. Because the potential outcomes are also identical, differences in donations across games should reflect the dictators' assessment of participants' risk-taking. This assessment depends on dictators' risk perception of the different games, which in turn depends on the level of agency or perceived control over outcomes in a game. Here, the lottery can be expected to give participants little perceived control over outcomes: It is completely random, uninfluenceable, and provides little agency for player 1 and none at all for player 2. Winning the strategic game, on the other hand, depends directly on the decisions of individual players and should therefore be associated with a higher level of agency and perceived control. Since in the contest the probability of winning can indeed be increased by increasing encryption efforts, it can be assumed that the sense of agency and perceived control over outcomes should be highest here.

Therefore, choosing to participate in the lottery could be interpreted as willingness to take a higher risk. Assuming that the dictators condition their giving on their agents' decision to minimize their risk exposure (as in Mollerstrom et al. 2015), participation in a riskier game with low control should have a *negative* effect on donations. This implies that participation in the lottery leads to the lowest donations, surpassed by donations for the strategic game, which in turn are exceeded by the donations for the encryption contest. Note that this effect can only be observed *within* a treatment: dictators in the *No Exclusion* treatment can only reward voluntary suspension from a game with low levels of control, whereas dictators in the *Exclusion* treatment can only withhold or reduce compensation for the intended, but prevented by exclusion participation in a game with low levels of control. Thus, participation in a game is expected to have an opposite effect in the two treatments:

Hypothesis 2 (Level of control) *Donations increase in the level of perceived control.*

Next, the expected impact of opportunity costs is considered. As already discussed in Section 3.3.2, the compensation for the forgone income opportunity should be influenced by the level of alternative income. Specifically, it is assumed that giving is highest in rounds where recipients do not receive alternative income, as this is where the opportunity cost of suspension/exclusion is highest: players participating in production would receive either 40 or 100 points, i.e. a positive income in either case. As safe amounts increase, these opportunity costs, and hence the financial disadvantages of suspension and exclusion, decrease. Therefore, donations should also decrease accordingly, both in the *No Exclusion* and the *Exclusion* treatment. This could also be a consequence of inequality aversion in dictators: the more alternative income a recipient receives, the smaller the gap with the dictator's income and thus the inequality, which reduces the incentive to donate.

Hypothesis 3 (Moderating effect of opportunity costs) *With decreasing opportunity costs of suspension/exclusion due to increasing amounts of the alternative income y_A, donations decline as well.*

These mechanisms were formalized as a general rule of compensation in Section 3.3.2, leading to Equation 3.3, which is repeated here for convenience:

$$y_{\mathrm{C}''} = \begin{cases} \alpha + \beta(\mu - y_\mathrm{A}), & \text{if } y_\mathrm{A} < \mu \\ \alpha & \text{otherwise} \end{cases}$$

This equation also holds that in the *Exclusion* treatment, once the safe amount exceeds the production's expected value of 70 points, i.e. once $y_{\text{ex}} \geq 80$, there should be no longer any compensation for exclusion, since in this case excluded players do not suffer any financial disadvantage from exclusion. Admittedly, very optimistic subjects might assume higher winning probabilities, and thus agree only for the highest safe amount $y_{\text{ex}} = 100 = y_{\text{win}}$ that exclusion does not lead to a disadvantage compared to participation in production. Following this argument, there should probably be no compensation for exclusion if it leads to an income of $y_{\text{ex}} = 80$, and a fortiori no compensation for exclusion once $y_{\text{ex}} = 100$.

Nevertheless, as argued in Section 3.3.2, dictators might compensate the perceived "unfairness" of exclusion regardless of financial disadvantages by paying a discrimination premium α. This would lead to consistently higher donations in the *Exclusion* treatment compared to the *No Exclusion* treatment, even for the rounds with a safe amount of 80 or 100 points:

Hypothesis 4 (Discrimination premium) *Compensation for exclusion includes a component independent of financial disadvantages; $D_1 > D_0$ even if $y_{\text{ex}} \geq 80$.*

As noted in Section 4.2, the actual extent of discrimination in this experiment is quite small. However, this actually strengthens the significance: if the existence of a discrimination premium is shown even despite the rather unbiased random exclusion procedure in this experiment, it is even more likely to be found in real world situations.

4.5.2 Expected Behavior of Productive Players

As productive players play all games in both the *No Exclusion* and the *Exclusion* treatment, they receive two dictators' donations, i.e. one per treatment and under its respective rules. Under the assumption that productive players believe that dictators are benevolent, the possibility of receiving donations may influence their participation decision: since only the safe amount in case of voluntary suspension, y_{out}, can be increased by dictators' donations in the *No Exclusion* treatment, the opportunity cost of production is higher here than in *Exclusion* where this is not the case. Thus, players in *No Exclusion* probably prefer to voluntarily suspend production. In contrast, in *Exclusion*, dictators can not add donations to y_{out}, but only to the safe amount in case of exclusion, y_{ex}. Since y_{ex} is conditional on a player's willingness to participate in production, *Exclusion* can be expected to have a higher participation rate than *No Exclusion*:

Hypothesis 5 (Donations as allocation signal) *In* Exclusion, *recipients choose participation in production more often than in* No Exclusion.

Obviously, this difference in participation decisions can only appear when recipients expect positive donations from dictators. When they think all dictators to be selfish, participation should be the same for both treatments.

The differences in participation decisions in the different games are also interesting for the analysis, as they can be used to determine which production is actually perceived as the most/least attractive. As elaborated in Section 4.1 and in Hypothesis 2, the three games differ in the level of perceived control a subject can exert over outcomes and should therefore be associated with different risk perceptions. Assuming that players are risk-averse, they should, on average, suspend production in the lottery already for smaller safe amounts than in the strategic game or the contest. Accordingly, as outcomes of the encryption contest are more clearly influenced by own effort decisions than the outcomes of the strategic game, it is expected that risk-averse subjects suspend production in the strategic game for smaller safe amounts than in the contest.

Hypothesis 6 (Participation according to risk exposure)
Participation is highest in the contest, followed by the strategic game, and lowest in the lottery.

However, since the encryption contest is certainly the most tiring production, it could be that recipients face a trade-off between risk and effort. This is briefly explored on basis of the experimental data in Section 5.1.4 of the results chapter below.

5 Analysis and Results

Now, this chapter turns to the results of the experiment conducted in June 2019 in the laboratory of the University of Hamburg using the software z-Tree (Fischbacher 2007). Subjects were recruited using hroot (Bock et al. 2014). Of the 72 dictators, 54% were female (19 in the *No Exclusion* treatment and 20 in the *Exclusion* treatment) and 46% male (17 and 16, respectively). Of the 36 recipients, 19 were male and 17 female. The average total dictator payoff was 280 points (14 €), and that of recipients 267.2 points (13.36 €).

This chapter analyzes dictators' donation decisions as elicited by the strategy method and is divided into two sections. It begins with a largely descriptive analysis of the main treatment effects in Section 5.1, which distinguishes between the extensive and intensive donation margins, that is, between the probability of donating anything at all and the level of positive donations. This initial analysis is complemented and mostly confirmed by an econometric analysis in Section 5.2. Finally, Section 5.3 summarizes and assesses the results of the main treatment effects from the descriptive and econometric analyses and discusses their implications for the model of compensation for discriminatory exclusion presented in Chapter 3.

The results strongly support the main Hypothesis 1 that exclusion is compensated, albeit only at the extensive margin. The other hypotheses are also partly supported: risk-taking by recipients reduces donations (Hypothesis 2) at the intensive margin, increasing opportunity costs of production reduce donations (Hypothesis 3) at both extensive and intensive margins, and there is weak support for the existence of a discrimination premium (Hypothesis 4) at the extensive margin.

5.1 Main Treatment Effects

The following section provides an initial descriptive analysis of the main treatment effects. It begins in Section 5.1.1 with a focus on the main Hypothesis 1, compensation for exclusion, before examining how donations are influenced by the different production games in Section 5.1.2 and by opportunity costs in Section 5.1.3.

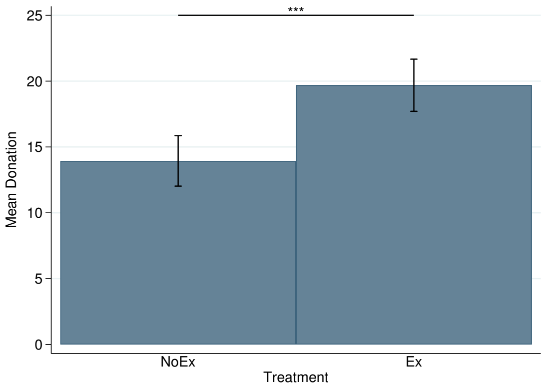
5.1.1 Compensation for Exclusion

This analysis starts by comparing the means of all donation decisions in case of *No Exclusion* and *Exclusion*. As can be seen from Figure 5.1, *Exclusion* leads to donations which are highly significantly (t-test: $p = 0.000$, $t = 4.099$; Mann-Whitney-U-test:[76] $p = 0.000$, $z = 6.431$) higher than donations in *No Exclusion*,

[76]Since the experimental data are not normally distributed, t-tests were supplemented in the analysis with Mann-Whitney-U-tests for non-parametric data. For the most part, both tests

with 19.69 points compared to 13.94 points, about 40% more. This can be considered to be the main treatment effect supporting Hypothesis 1, $D_1 > D_0$.

Figure 5.1: Mean Total Donations by Treatment



N per Treatment (assuming independence of observations) = 648; Confidence-intervals: 95%; Significance levels according to t-test results indicated by $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

Moreover, there exists a strikingly different pattern of donations between treatments. Defining dictators who do not give a positive donation in any round as strictly libertarian dictators, there are 8 of these libertarian dictators in case of *Exclusion*. In case of *No Exclusion*, there are 15 strictly libertarian dictators, almost twice as many. This difference is significant at the 10%-level for both χ^2 and t-test ($p = 0.077$, $\chi^2 = 3.13$; and $p = 0.079$, $t = 1.784$, respectively). No gender effects can be observed in this libertarian behavior: 13 libertarians are male (8 in *No Exclusion*, 5 in *Exclusion*) and 10 female (7 in *No Exclusion*, 3 in *Exclusion*).

Apart from this, the questionnaire reveals only one other significant difference between the two treatments, namely in the hypothetical own suspension of production, which was reported to be more depending on production games in the *No Exclusion* treatment. Furthermore, there is the (albeit not significant) tendency for dictators in the *Exclusion* treatment to more often report having paid attention to fairness in their donation decisions than dictators in the *No Exclusion* treatment. The implications of these differences are discussed in more detail in Section 5.2.

These observations suggest two different possible effects of *Exclusion*, which are explored in more detail in this first part of the analysis: on the one hand, the different number of libertarians could mean that *Exclusion* induces more dictators to donate anything at all and thus affects the probability of donation. On the other hand, the possibly higher consideration of fairness in donation decisions could indicate a higher level of donations due to *Exclusion*.

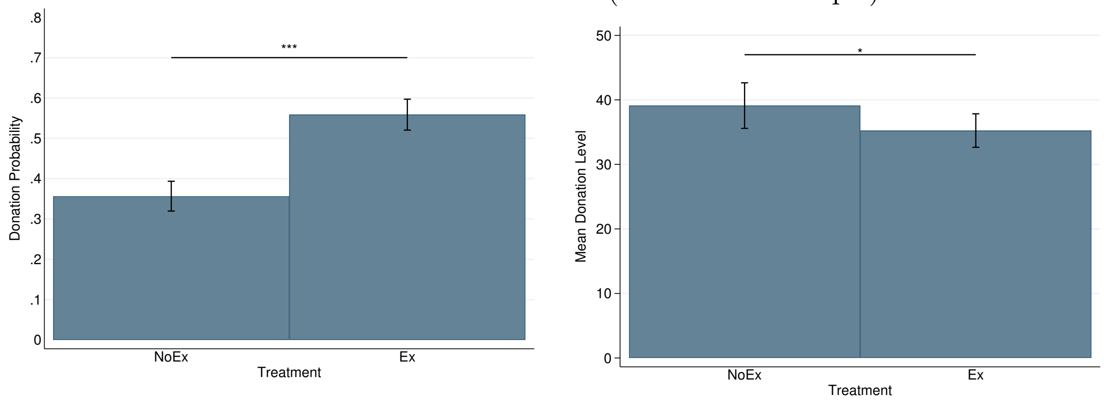
Considering donation probability first, each donation of $D > 0$ is coded as 1 and each donation of zero is coded as 0. Calculating the mean thereof, one obtains the

yield very similar results, with the Mann-Whitney-U-test reporting a slightly higher number of significant differences. Therefore, the results of the stricter parametric t-test are given in the following to indicate significant differences in both the text and the figures.

Figure 5.2: Donation Probability and Level by Treatment

(a) Donation Probability by Treatment

(b) Mean Donation Level by Treatment (Conditional Sample)



N per Treatment (assuming independence of observations) = 648 (a), 231/362 (b); Confidence-intervals: 95%; Significance levels according to t-test results indicated by $*p < 0.10$, $**p < 0.05$, $***p < 0.01$.

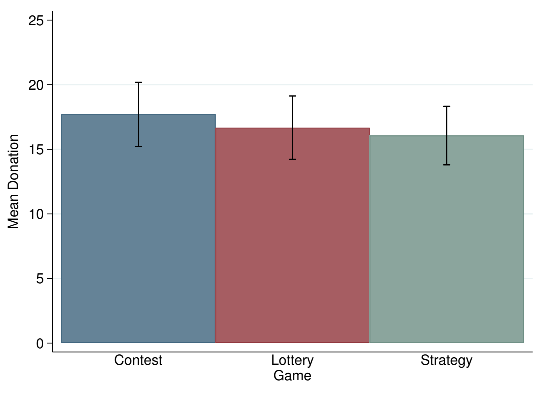
donation probability by treatment, shown in Figure 5.2a. As expected by Hypothesis 1 and in agreement with Figure 5.1, donation probability in *Exclusion* (0.56%) is about 55% higher than in *No Exclusion* (0.36%). This difference turns out to be highly significant (t-test: $p = 0.000$, $t = 7.454$).

To compare donation levels, a conditional sample is created: all donations of zero are ignored and only donations where the dictators decide to transfer a positive amount to the recipient are considered. Figure 5.2b shows the mean values of the conditional sample with all donations greater than zero by treatment. Surprisingly, the previously found pattern of compensation for exclusion is not reflected. Instead, donation levels are actually slightly *lower* in *Exclusion* than in *No Exclusion*. With an average of 35.25 points in *Exclusion* and 39.11 points in *No Exclusion*, the relative difference is rather small, but significant at the 10% level (t-test: $p = 0.078$, $t = 1.769$). These findings can be interpreted to indicate that dictators compensate for exclusion, as posited by Hypothesis 1, but only at the extensive margin by donating more frequently, while the selected donation amount is similar to or even slightly lower than in the case of less frequent donation in *No Exclusion*.

5.1.2 Influence of Production Choice

After Section 5.1.1 above finds support for compensation for exclusion, the next step is to examine whether the different (perceived) risk levels of the chosen production games affect donation behavior. For this purpose, it is first checked whether there are unexpected fundamental differences in donation across games, regardless of treatment. That this is not the case is shown by Figures 5.3, 5.4a and 5.4b, which illustrate that mean total donations, donation probability and donation level are virtually identical between all three games.

Figure 5.3: Mean Total Donations by Game

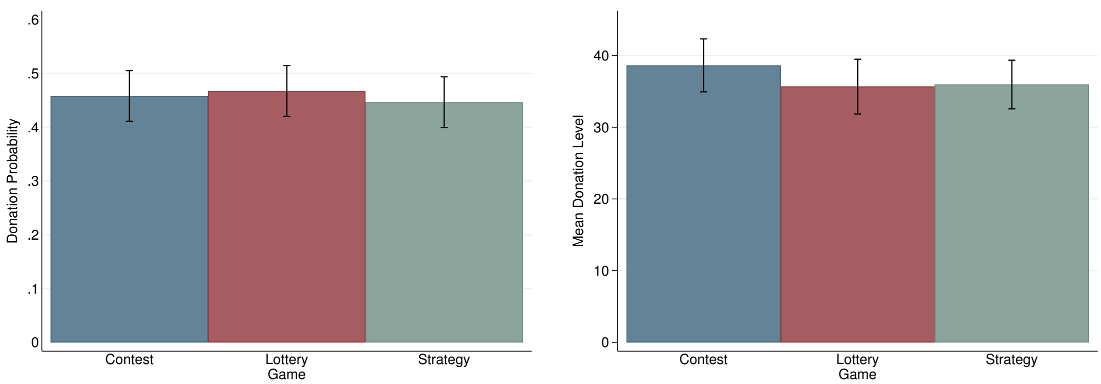


N per Game (assuming independence of observations) = 432; Confidence-intervals: 95%; Significance levels according to t-test results indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 5.4: Donation Probability and Level by Game

(a) Donation Probability

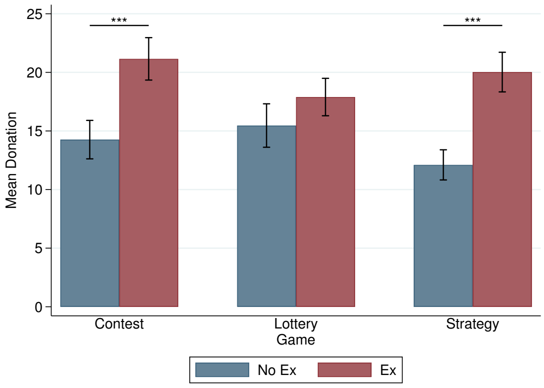
(b) Donation Level (Conditional Sample)



N per Game (assuming independence of observations) = 432 (a), 193/198/202 (b); Confidence-intervals: 95%; Significance levels according to t-test results indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Thus, the analysis proceeds to examine Hypothesis 2, which states that donations increase in the level of perceived control. Indeed, the interaction effect of games and treatment displayed by Figure 5.5 shows that, as expected, there are substantial differences between the lottery and the other two games: while the treatment effect of the lottery is very small and not significant, both the contest and the strategic game show a strong and highly significant increase (t-test: contest $p = 0.006$, $t = 2.749$; strategy $p = 0.001$, $t = 3.474$) in mean total donations for the case of *Exclusion*. At first sight, although the donation differences between the three games are not significant within treatments, this lack of compensation for exclusion in the lottery seems to provide some support for Hypothesis 2.

Figure 5.5: Mean Total Donations by Game and Treatment

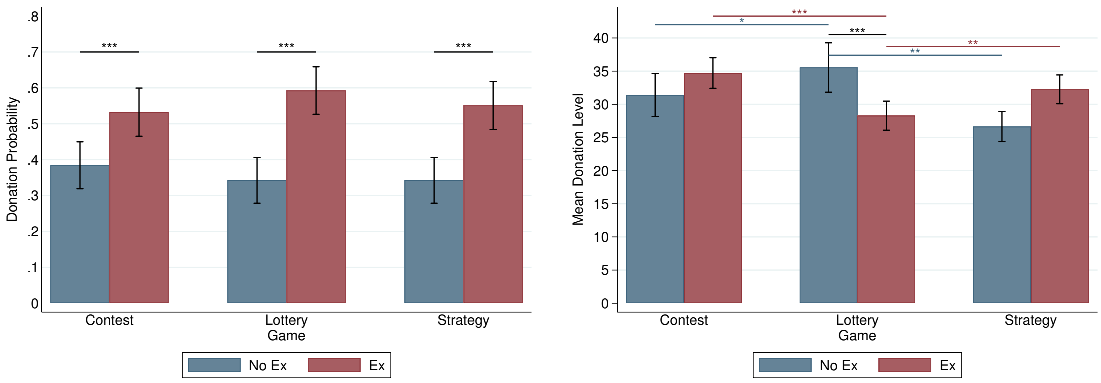


N per Game and Treatment (assuming independence of observations) = 216; Confidence-intervals: 95%; Significance levels according to t-test results indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 5.6: Donation Probability and Level by Game and Treatment

(a) Donation Probability

(b) Donation Level (Conditional Sample)



N per Game and Treatment (assuming independence of observations) = 216 (a); 83/115, 74/128, 74/119 (b); Confidence-intervals: 95%; Significance levels according to t-test results indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

However, this is not true for the interaction effect of donation probability and treatment shown in Figure 5.6a: here, the treatment effect of *Exclusion* is strikingly similar and highly significant ($p < 0.01$)[77] for all games, and even slightly more pronounced for the lottery. This shows that compensation for exclusion is not conditioned on production choice, in contrast to the choice compensation found by Mollerstrom et al. (2015, see Section 2.2.3). Nevertheless, the level of compensation is affected by the level of control of the production chosen by the recipient, as shown by the interaction effect of donation level and treatment in Figure 5.6b. While donation levels for the contest and the strategic game are quite similar to each other, donation levels for the lottery differ significantly: in *No Exclusion*, donation levels for the lottery are significantly *higher* than for the contest (t-test: $p = 0.08$, $t = 1.762$) and the strategic game (t-test: $p = 0.026$, $t = 2.246$), while in the *Exclusion* treatment, donation levels for the lottery are significantly *lower* than for the contest (t-test: $p = 0.004$, $t = 2.946$) and the strategic game (t-test: $p = 0.051$, $t = 1.958$).

This reversal of direction in differences in donation levels could be expected, as already explained in Section 4.5.1: in *No Exclusion*, dictators can only reward the voluntary suspension of production perceived as riskier due to low levels of control (see Section 3.3.3), i.e. donate higher amounts in the case their recipient suspends the risky lottery than in the case she suspends the contest or the strategic game with higher perceived levels of control. In *Exclusion*, on the other hand, dictators can only withhold or reduce compensation if recipients voluntarily choose the riskier production with lower levels of control. As a result, only the lottery exhibits a significant treatment effect — but one that is completely at odds with compensation for exclusion, as donation levels are significantly higher (t-test: $p = 0.000$, $t = 3.839$) in the *No Exclusion* treatment than in the *Exclusion* treatment.

Surprisingly, there is no difference between the strategic game and the contest, although it could expected that the contest, which allows more control over the outcome, is perceived as less risky than the strategic game. There are two possible explanations for this. One possibility is that the risk perception of dictators who do not participate in the games themselves is not very sensitive to the different levels of control in the contest and the strategic game, but only to its absence in the lottery. In this case, dictators would only distinguish between the uncontrollable, risky lottery and the more controllable and hence less risky other games, which would explain the results. The other explanation could be the additional dimension of effort in the encryption task, which counterbalances its higher level of control and makes participation in the contest more unattractive than in the strategic game. This argument is supported by the fact that in the questionnaire (see Section 4.4.3), lottery and contest are chosen most often as hardest production with strikingly similar frequencies, while the strategic game is chosen as hardest production only half as often. The easiest production question is inconclusive, however, because all games are chosen with similar frequency.

[77]T-test: contest $p = 0.002$, $t = 3.117$, lottery $p = 0.000$, $t = 5.366$, strategy $p = 0.000$, $t = 4.443$.

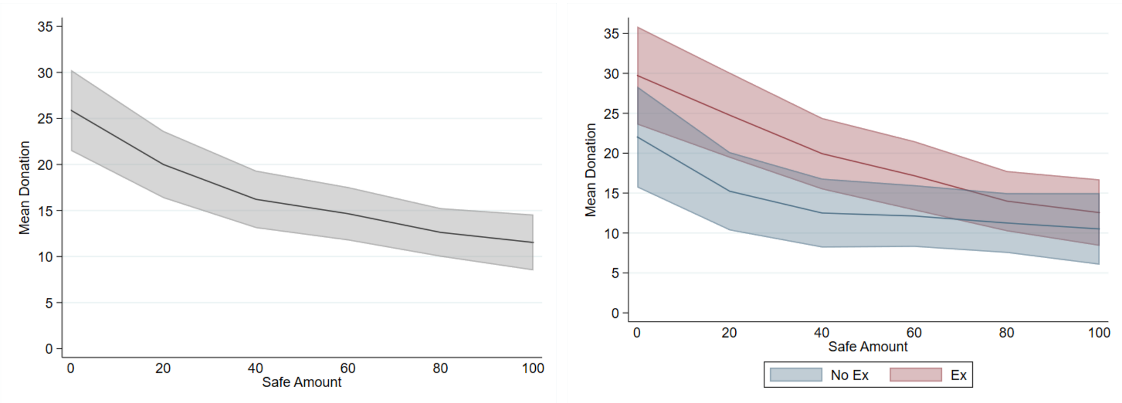
This experiment does not allow differentiation between these two effects and hence lacks a definitive explanation for the lack of donation differences between the strategic game and the contest. Still, the outstanding reverse treatment-effect for the lottery appears to support Hypothesis 2 to some degree. This is further examined in the econometric analysis in Section 5.2.3.

5.1.3 Influence of Opportunity Costs

This section now turns to the expected relationship between donations and opportunity costs. Figure 5.7a, which plots mean total donations with 95% intervals by safe amount, clearly shows that dictators continuously reduce their donations with increasing safe amounts received by recipients upon suspension/exclusion, as posited in Hypothesis 3. It can be seen from the line graph's gradual flattening that the moderating effect of opportunity costs is most pronounced at the beginning, with a strong drop in mean donations between the situation with no safe amount, where the opportunity cost of suspension/exclusion is highest, and the safe amount of 20 points. As safe amounts increase, the downward trend decreases but persists.

Figure 5.7: Mean Total Donations by Safe Amount

(a) by Safe Amount (b) by Safe Amount and Treatment



N per Safe Amount (assuming independence of observations) = 216 (a); *N* per Safe Amount and Treatment (assuming independence of observations) = 108 (b); Confidence-interval: 95%.

This pattern is reflected in Figure 5.7b, which displays the effect of opportunity costs on mean total donations for each treatment separately. The graph for *Exclusion* starts significantly higher than the one for *No Exclusion*, and although they converge to the level of *No Exclusion* for the safe amounts of 80 and 100 points, mean donations in *Exclusion* remain consistently at a higher level. This is shown in more detail in Table C.2, which displays the values of donation means by safe amount and by treatment, as well as the respective test statistics for both Mann-Whitney-U- and t-test.

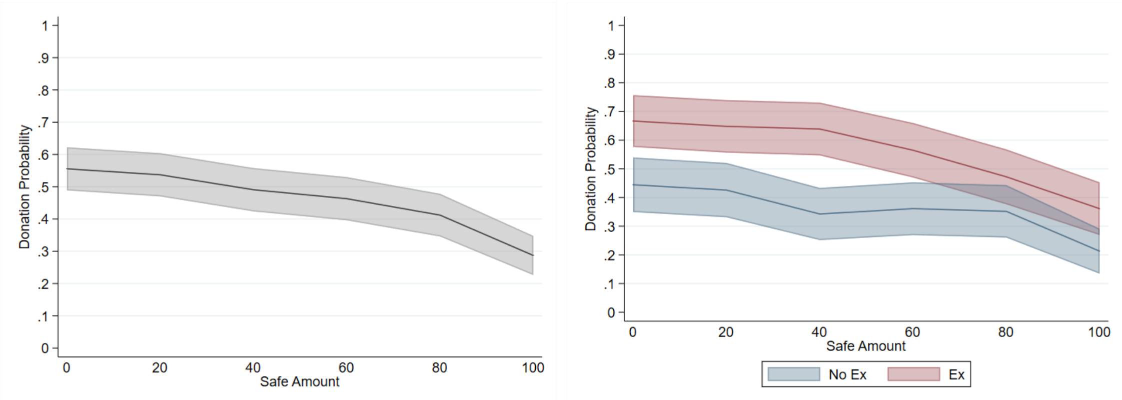
The emerging pattern again confirms the main treatment effect of *Exclusion* and at first sight also seems to support the existence of a discrimination premium which is independent of exclusion-induced losses, as posited in Hypothesis 4. However, the round-based significance tests in Table C.2 show that the difference in mean donations between treatments is significant (of varying degrees) only for the safe amounts of 0–60 points, but not for the safe amounts of 80 and 100 points. Therefore, the existence of a discrimination premium cannot be substantiated by mean total donations.

Moreover, it is evident from Figure 5.7b that the line graph for *Exclusion* shows a steady decline in mean total donations with increasing safe amounts. The line graph for *No Exclusion*, however, shows only a flat, slight decline after the initial drop in mean total donations between the safe amounts of zero and 20 points.

Next, consider the impact of *Exclusion* on donation probabilities. Figure 5.8a shows that the overall donation probability continuously declines with increasing safe amounts, with a slightly larger decrease in the last round. This discernible trend supports Hypothesis 3.

Figure 5.8: Donation Probability by Safe Amount

(a) by Safe Amount (b) by Safe Amount and Treatment



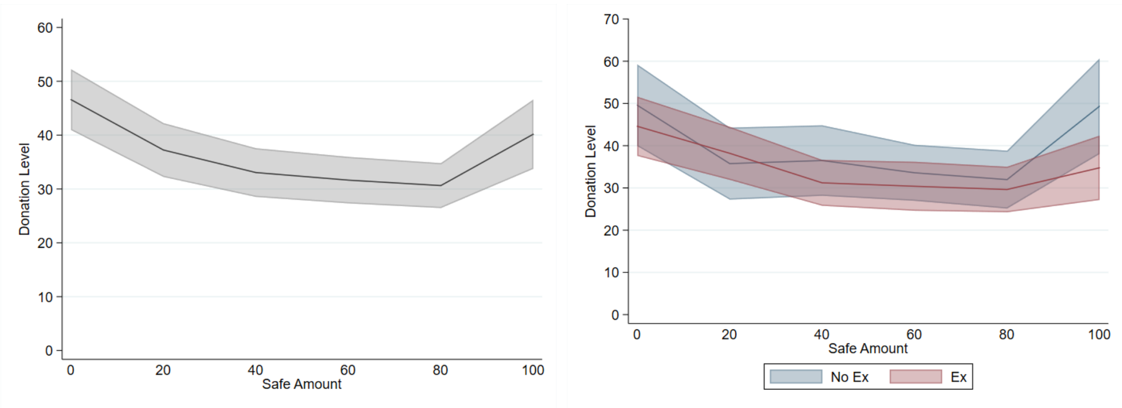
N per safe amount (assuming independence of observations) = 216 (a); N per safe amount and treatment (assuming independence of observations) = 108 (b); Confidence-interval: 95%.

Figure 5.8b illustrates the differences in donation probability by safe amount between treatments. It can be seen that donation probability is constantly higher in *Exclusion* than in *No Exclusion*, with confidence intervals barely overlapping even in the final rounds. Again, this strongly supports the main Hypothesis 1 that exclusion is compensated. As indicated in the graphs, round-based significance tests confirm that this difference is statistically significant at the 10%-level for each round, even for the safe amounts of 80 and 100 points (see Table C.3). In these two final rounds, recipients are not financially disadvantaged by exclusion and hence should not be compensated under the responsibility principle. The fact that donations for these safe amounts are nonetheless more likely in *Exclusion* than in *No Exclusion* provides initial support for the existence of a discrimination premium α that compensates for the fundamental unfairness of arbitrary exclusion, as postulated in Hypothesis 4.

Both the line graphs for donation probability and the line graphs for mean donations decrease constantly with increasing safe amounts. This pattern now changes in Figures 5.9a and 5.9b, which display the donation level by safe amount, i.e. the mean values of all donations greater than zero: surprisingly, the resulting line graphs are U-shaped: dictators donate the most when their recipient receives nothing — or 100 points. This is in stark contrast to expectations.

Figure 5.9: Donation Level by Safe Amount (Conditional Sample)

(a) by Safe Amount (b) by Safe Amount and Treatment



N per Safe Amount (assuming independence of observations) = 62/89/100/106/116/120 (a); *N* per Safe Amount and Treatment (assuming independence of observations) = 48/72, 46/70, 37/69, 39/61, 38/51, 23/39 (b); Confidence-interval: 95%.

However, this effect, which is surprising only at first glance, is mainly caused by the fact that dictators with a donation of zero are excluded from this analysis and the number of excluded dictators increases as the safe amount increases. Consequently, only dictators with a relatively high willingness to donate remain.

Overall, the impression from the figures is that donation levels respond strongly to the absence of a positive safe amount, but are barely influenced by increasing safe amounts once they are greater than zero. This suggests that dictators mainly decide whether or not to donate and adjust this binary choice to reflect changing opportunity costs, but vary the actual donation amount only slightly. Figure 5.9b shows that this is also true between treatments, as donation levels in *Exclusion* and *No Exclusion* are nearly identical. In fact, donations in *No Exclusion* are even a bit higher than in *Exclusion*, although this tendency is mostly not significant, see Table C.4 in the appendix. This is consistent with Figure 5.2b, and might be partially driven by a slightly higher number of egalitarians in the *No Exclusion* treatment, see Section 5.2.5.

In summary, this initial analysis suggests that exclusion is indeed compensated as assumed in Hypothesis 1, albeit only at the extensive, but not at the intensive margin: In the case of the *Exclusion* treatment, the probability that a dictator will donate anything is higher than in the *No Exclusion* treatment, the expected amount of a donation is however not increased. Thus, the data arguably not only support Hypothesis 3, which posits that compensation for exclusion declines with decreasing opportunity costs, but also Hypothesis 4: since compensation for exclusion only translates into increased donation probabilities, and since *Exclusion* increases donation probabilities even in situations where recipients are not financially disadvantaged, it appears that a discrimination premium does indeed exist.

5.1.4 Participation Behavior of Recipients

This section briefly outlines the recipients' behavior to see whether they anticipate the dictators' donation decisions and condition their decision to participate in or suspend production on the treatment-dependent possibility to receive donations in case of either voluntary suspension or exclusion. Furthermore, it investigates whether they are influenced by the variations in games, i.e. whether their participation behavior reflects the games' different levels of control.

As shown in Figure 5.10a, production participation decisions are not significantly increased in *Exclusion*, although donations are conditional on this decision. However, as can be seen in Figure 5.10b, this is due to the fact that at low safe amounts in case of suspension, almost all recipients in both treatments choose to participate in production. Participation only begins to decrease slightly at the safe amount of 40, for which suspension ensures the same payoff as losing a game. From now on, it drops sharply with each subsequent increase in safe amounts. In these rounds with reduced participation, one can see that participation is higher in *Exclusion* than in *No Exclusion*, as expected. The difference is significant only for the safe amounts of 60 (t-test: $p = 0.073$, $t = 1.802$) and 80 (t-test: $p = 0.028$, $t = 1.151$) points, see Table C.5 in the appendix which displays recipients' participation decisions by safe amount and the results of significance tests on the differences between treatments. However, it is exactly these rounds where the possibility of donations is a reasonable motive for participation.[78] Thus, this behavior provides strong support for Hypothesis 5 that the possibility of donations drives participation.

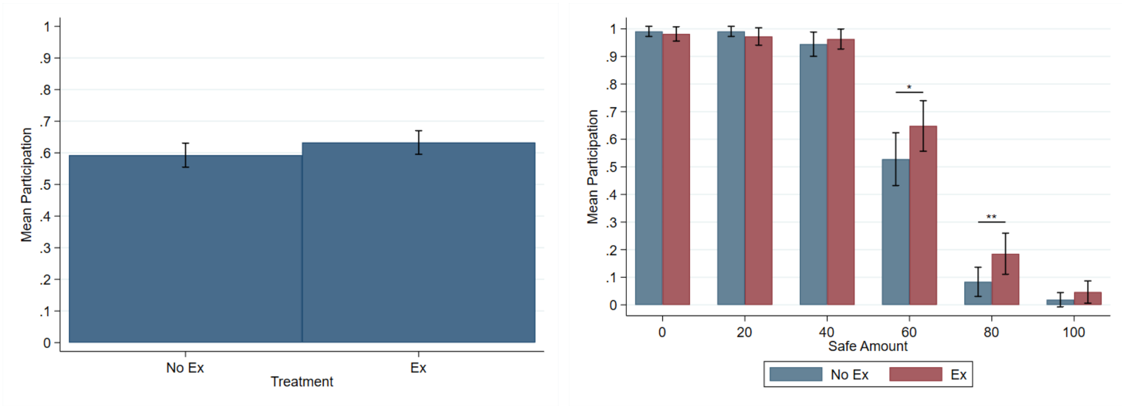
Figure 5.11a displays recipients' participation behavior by game and treatment. The bar graphs shows that mean participation rates are identical both with respect to game and treatment. Again, only when comparing by safe amounts, some small but significant differences (similar for both treatments) emerge between participation in the lottery and participation in either the contest or the strategic game.

This finding is not fully consistent with Hypothesis 6, which posits that participation is highest in the contest, followed by the strategic game and lowest in the lottery. Nevertheless, the few significant differences and the displayed tendencies seem to support the earlier conjecture, detailed in Section 5.1.2, that the luck-based

[78] Below 60 points, the value of participation in the games, even in case of losing, is higher or equal than the safe amount of suspension. For 100 points, suspension leads to the same amount as winning a game. Thus, for these rounds it seems unlikely that the possibility to receive a donation on top of the respective safe amount influences participation decisions.

Figure 5.10: Recipients' Participation Behavior

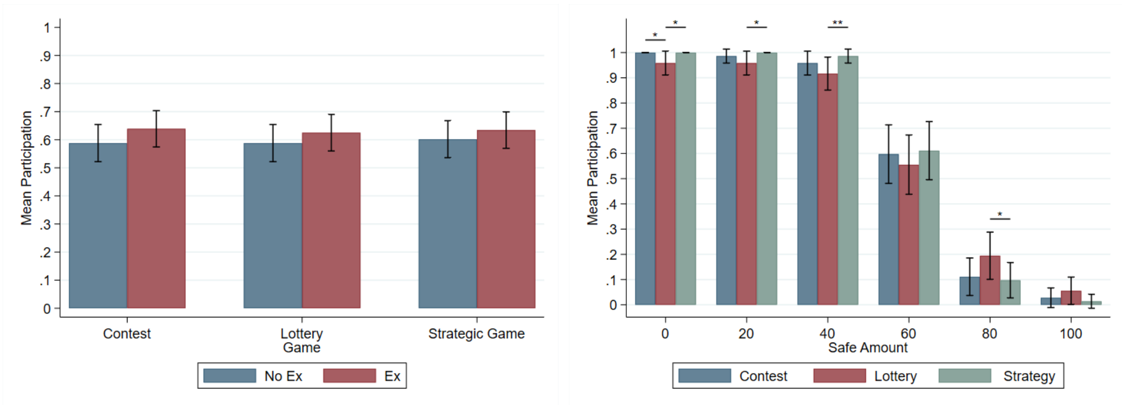
(a) by Treatment (b) by Safe Amount



N per Treatment (assuming independence of observations) = 648 (a); N per Safe Amount and Treatment (assuming independence of observations) = 108 (b); Confidence-interval: 95%; Significance levels according to t-test results indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 5.11: Recipients' Participation Behavior by Game

(a) by Game and Treatment (b) by Game and Safe Amount



N per Game and Treatment (assuming independence of observations) = 216 (a); N per Safe Amount and Game (assuming independence of observations) = 72 (b); Confidence-interval: 95%; Significance levels according to t-test results indicated by $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

lottery is perceived differently from production games with more own control over outcomes, such as the contest and the strategic game.[79]

Beyond the observations presented, the behavior of recipients does not provide additional insights into dictators' compensation for exclusion. Therefore, the empirical analysis in Section 5.2 focuses only on dictators' donation decisions and associated controls to further examine the effect of discriminatory exclusion on compensation.

[79] Due to lack of data, it is not possible to further investigate why participation in the contest or the strategic game is apparently considered equally reasonable. As previously mentioned, one possible reason might be the higher level of effort associated with the contest, which serves as a counterbalance to its higher level of control compared to the strategic game. However, there is also the possibility that subjects are insensitive to differences in the level of control and show only a rather binary response to the presence or absence of control over outcomes. Because the various possible reasons for the similar perceptions of the contest and the strategic game cannot be definitively unraveled in this dissertation, future research specifically addressing this question is needed.

5.2 Econometric Analysis

Now, the econometric analysis of the experimental data assesses whether the conclusions from the initial analysis of main treatment effects in Section 5.1 are stable. The econometric analysis consists of five parts. First, Section 5.2.1 details variable selection and data preparation. Then, Section 5.2.2 econometrically analyzes the effect of *Exclusion* on donations. Next, Sections 5.2.3 and 5.2.4 continue to analyze the influence of game choice and opportunity cost, respectively, on donations. The results, which are summarized and discussed in Section 5.3, are by and large consistent with the descriptive results of Section 5.1.

5.2.1 Data Selection and Preparation

So far, the analysis has focused only on the main treatment variations *Exclusion*, *Production Games*, and *Opportunity Costs*. Now, control variables are introduced into the econometric analysis. These are divided into three categories, which are then included separately in the regressions to determine the individual impact of each category. The three categories are:

1. Elicited controls, specifically the variables WTP^a and WTP^d that capture advantageous and disadvantageous inequality aversion of dictators (see Section 4.4.1).

2. Socio-demographic controls, abbreviated as socio-dem. controls in the regression results tables. These include the variables *gender*, year of birth (*age*), student in the field of economics (*Econstud*), and *semester*.

3. Attitudes as self-reported by the dictators in the questionnaire described in Section 4.4.3. These were questions on the fairness of the experiment (*Expfairness*), own hypothetical behavior (*hypownpause*), and difficulty of production games (*easiestprod*, *hardestprod*), as well as own risk preferences (*riskpref*), fairness norms (*fairnesspref*), importance of responsibility (*responsibility*), and preferred social security system (*socialsecpref*). These variables are mostly factor variables, the individual categories of which can be found in the general variable overview in Table C.1 in the appendix.

As already pointed out in Section 5.1.1, the questionnaire revealed some differences between the treatments. To see whether these differences or other variables are correlated with donations and hence drive or bias the treatment effect, Table 5.1 displays the correlations between donations and all control variables.

It reveals that most variables are not or only barely correlated with donation,[80] with the exception of *donationfair*, which is strongly and highly significantly ($p <$

[80] The variable *hypownpause*, which indicates the safe amounts dictators would suspend production for if they participated in the production games, is the only variable that is significantly different ($p < 0.1$) between the two treatments. Yet there is no significant effect of *hypownpause*

Table 5.1: Correlation of Donation and Control Variables

	Donation	WTP^a	WTP^d	gender	age	Econstud
WTP^a	0.0274	1				
WTP^d	-0.162***	-0.127***	1			
gender	0.0514	0.218***	-0.0155	1		
age	0.0521	-0.0776**	-0.0907**	0.110***	1	
Econstud	-0.0513	-0.149***	-0.146***	-0.0872**	-0.0485	1
semester	-0.0697*	-0.130***	-0.0945***	-0.197***	-0.561***	-0.0889**
fairnesspref	0.120***	0.0815**	-0.0499	-0.151***	-0.296***	-0.00811
riskpref	0.0834**	0.0466	0.0339	-0.184***	-0.274***	-0.117***
hypownpause	-0.0412	-0.219***	0.0785**	-0.237***	-0.163***	0.102***
socialsecpref	0.0148	0.0354	0.000374	-0.261***	0.166***	-0.0174
donationfair	0.510***	0.142***	-0.227***	0.198***	0.0104	-0.0450
Expfairness	0.143***	0.0254	-0.0812**	0.0971***	-0.0249	0.0160
responsibility	0.0775**	0.00678	0.0329	0.0933***	-0.100***	-0.139***
hardestprod	0.0157	0.134***	-0.0586*	0.0157	-0.111***	-0.0594*
easiestprod	-0.226***	-0.179***	0.119***	-0.229***	0.109***	0.0385

	semester	fairnesspref	riskpref	hypownpause	socialsecpref
fairnesspref	0.112***	1			
riskpref	0.392***	0.134***	1		
hypownpause	0.165***	0.0916***	0.153***	1	
socialsecprefs	-0.178***	0.228***	-0.0781**	-0.00226	1
donationfair	-0.0130	0.259***	0.107***	0.0205	-0.0286
Expfairness	-0.220***	0.0624*	0.0622*	-0.0692*	0.0223
responsibility	0.0840**	0.320***	0.0544	-0.0775**	0.0214
hardestprod	0.124***	0.150***	0.0452	-0.0360	0.215***
easiestprod	0.0246	-0.109***	-0.0877**	0.0887**	0.0938***

	donationfair	Expfairness	respimp	hardestprod	easiestprod
Expfairness	0.311***	1			
responsibility	0.0869**	-0.0242	1		
hardestprod	0.113***	0.0628*	-0.00987	1	
easiestprod	-0.263***	-0.238***	-0.105***	-0.410***	1

$^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$

0.01) correlated with donation amounts. Hence, donation fair is dropped from the econometric regressions and instead used as robustness check, see Section 5.2.5. The same is done for the categorized dictators' justifications for their donation decisions, as the inclusion of this self-reported, qualitative data into behavioral-only data does not improve but rather diminish the performance of regression specifications, which is in line with the findings by Cappelen et al. (2011, pp. 118–119).

Table 5.1 reveals only one other very strong and significant correlation, between *easiestprod* and *hardestprod*. This correlation could be expected, and confirms a reasonable consistency of answers. A simple graphical analysis shows that *easiestprod* is associated with slightly more variation in donations than *hardestprod*, although the respective games are chosen almost equally often in this category. In *hardestprod*, on the other hand, the strategic game is chosen only half as often as the other two production games. Therefore, *hardestprod* is excluded from the analysis, and only the effects of *easiestprod* are examined. Table 5.1 does show a few more significantly correlated variables, yet their correlation factors are small (see, e.g., *fairnesspref* and the variables capturing advantageous and disadvantageous inequality aversion, WTP^a and WTP^d) and thus do not warrant further reduction of the data set.

5.2.2 Compensation for Exclusion

The econometric analysis of the effect of the *Exclusion* treatment on donations proceeds as the descriptive analysis, and separately examines the effect of *Exclusion* and the other main treatment variables, i.e. production games (*Lottery*, *Contest*, *Strategy*) in Section 5.2.3, and opportunity costs (*safeamount*) in Section 5.2.4. In each section, the analysis starts with a pooled ordinary least squares (OLS)-[81] and a random-effects (RE) Tobit-regression (Tobin 1958) examining how total donations are influenced. It then proceeds to the extensive margin, with a pooled OLS- and a Probit-regression identifying the relevant variables influencing donation probability. Finally, in each section a pooled OLS- and a RE Tobit-regression consider the intensive margin, examining the influence of variables on donation level for the conditional sample. The RE Tobit-regressions are left-censored by the minimum possible donation amount of 0 points and right-censored by the maximum possible donation amount of 100 points.

This combination of regressions for total donations, extensive and intensive margin is used to reflect that the overall treatment effects are determined by the influence of treatment variations on both the donation probability and the donation level,

on donations, nor does it have any exceptionally strong correlations with other explanatory variables (see Table 5.1). Thus it can be concluded that the results are not biased due to the difference of *hypownpause* between treatments.

[81] While it seems unlikely that this experimental dataset, with its dependent variable being censored, matches the assumptions underlying an OLS-estimation, OLS-regressions are very simple, easy to interpret, and also a widely used conventional regression method in other experimental studies (see, e.g., Young 2019, p. 565). Therefore, OLS is used here to contrast the RE Tobit- and RE Probit-regressions used by this conventional and comparable standard method.

as shown in the descriptive analysis. Because this influence varies with the step-wise increase of the safe amount across rounds, this approach seems preferable to a double-hurdle model (Cragg 1971).[82]

Five different specifications are estimated for each type of regression: first, only the main treatment variables are included (I) before being supplemented by each category of control variables (see Section 5.2.1) individually in regressions II–IV. Finally, the fully specified regression V consists of the main variables and all control variables. In all regressions, the step-wise increasing safe mount is included as cardinal variable. This is done as it is measured in monetary units and only its average linear tendency is of interest for the other treatment variations. Whether this assumption holds is checked in Section 5.2.4.

Beginning the econometric analysis of the treatment effect, the results of the pooled OLS- and a RE Tobit-regression on total donations are presented in Tables 5.2 and 5.3. A first look at the two tables immediately shows that the simple OLS-regression yields considerably more significant results than the RE Tobit-regression, which is more appropriate for the data at hand. As this continues throughout the empirical analysis, the OLS results may be considered overly optimistic, so for the most part only those variables that show significant results in both regressions will be discussed. In terms of coefficient size, however, the RE Tobit-regression is more optimistic: throughout the analysis, the coefficients resulting from the RE Tobit-regression (which reports marginal effects and is hence comparable to the OLS-results) are significantly larger than those in the OLS-results, which is also the case in the meta-study on dictator-games by Engel (2011).

In the OLS-regression, the step-wise inclusion of controls does not affect the highly significant impact of *Exclusion* on donations, but only slightly changes the coefficient size, which is constantly at only about 25% of the respective effect size in the RE Tobit-regression. In the latter, however, the effect of *Exclusion* is not consistently significant but varies with the inclusion of controls: the inclusion of elicited inequality aversion as well as the inclusion of socio-demographic variables removes the significant influence of *Exclusion*, while the inclusion of attitudes increases its significance, so that in the complete regression *Exclusion* significantly increases do-

[82] Using a double-hurdle model (Cragg 1971) that accounts for strictly libertarian dictators while allowing for zero censoring, may be appropriate due to the large number of zero donations in the experiment. In this way, the number of individuals who never clear the first hurdle for donation can be estimated based on the treatment and observable characteristics of subjects (Moffat 2016, p. 257). Indeed, a significant proportion of null observations in this experiment appear to be treatment-dependent (since the number of strictly libertarian dictators differs between treatments with $p < 0.1$). Based on the graphical analysis in Section 5.1, it is nevertheless assumed that *Exclusion* and some other factors affect the probability of donation not only at the first hurdle (donating anything at all), but across all rounds. However, this is not accounted for in a hurdle model whose second hurdle outcome reflects only the amounts actually donated (ibid., p. 258), which seems to restrictive for this analysis. Therefore, as mentioned above, the regressions are split into total effects, extensive and intensive margin, similar to the graphical analysis, and censored RE Tobit-regressions are used for the regressions on total donations (0,100) and donation levels (1,100).

Table 5.2: OLS-Regression on Determinants of Total Donations

	I	II	III	IV	V
Exclusion	5.75***	4.88***	5.69***	6.25***	6.26***
(0=No Ex, 1= Ex)	(1.38)	(1.38)	(1.45)	(1.53)	(1.70)
Contest	1.03	1.03	1.03	1.03	1.03
(0=Lottery, 1=C)	(1.69)	(1.67)	(1.68)	(1.54)	(1.52)
Strategy	-0.61	-0.61	-0.61	-0.61	-0.61
(0=Lottery, 1=S)	(1.69)	(1.67)	(1.68)	(1.54)	(1.52)
safeamount	-0.14***	-0.14***	-0.14***	-0.14***	-0.14***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
constant	20.62***	28.28***	318.20	-1.31	591.40
	(1.71)	(2.27)	(362.16)	(6.21)	(423.86)
elicited controls	no	yes	no	no	yes
socio-dem. c.'s	no	no	yes	no	yes
attitudes	no	no	no	yes	yes
R2	0.047***	0.070***	0.055***	0.219***	0.244***

$^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. $N = 1296$. Dependent variable is *Donation*, standard errors in parentheses. Assuming independence of observations. This table has been shortened. The complete table includes the results of all control variables and can be found in Appendix C.6. The omitted categories are *No Exclusion* for the treatment variable, and Lottery for the production games.

nations at the 10%-level. The individual effects of control variables can be found in Section 5.2.5.

Taken together, the results of both regressions seem to confirm the main treatment effect of *Exclusion* found in Section 5.1.1, albeit weakly: *Exclusion* is compensated by higher mean donations. Also consistent with the initial analysis in Sections 5.1.2 and 5.1.3, the regressions show no significant influence of the different games (an additional χ^2-test confirms that there are also no significant differences between the contest and the strategic game), but a highly significant negative impact of opportunity cost on mean donations: increasing the safe amount by 1 point leads to a decrease in donations of about 0.14 to 0.34 points.

Regressions on Donation Probability

Now, this section moves on to examine the effect of *Exclusion* on donation probability, to confirm that the overall effect of *Exclusion* on donations is mostly driven by the extensive margin as suggested by the previous analysis. For this, both a pooled

Table 5.3: RE Tobit-Regression on Determinants of Total Donations

	I	II	III	IV	V
Exclusion	22.49*	18.21	20.37	25.42**	24.84*
(0=No Ex, 1=Ex)	(12.81)	(12.31)	(13.03)	(12.81)	(13.70)
Contest	1.14	1.14	1.15	1.15	1.15
(0=Lottery, 1=C)	(2.10)	(2.11)	(2.11)	(2.11)	(2.11)
Strategy	-2.29	-2.29	-2.28	-2.28	-2.28
(0=Lottery, 1=S)	(2.11)	(2.11)	(2.11)	(2.11)	(2.12)
safeamount	-0.33***	-0.33***	-0.33***	-0.33***	-0.34***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
constant	-8.56	17.17	-98.79	-49.74	2213.08
	(9.50)	(16.10)	(3305.43)	(50.52)	(3435.20)
elicited controls	no	yes	no	no	yes
socio-dem. controls	no	no	yes	no	yes
attitudes	no	no	no	yes	yes
σ_u	51.46***	49.41***	50.68***	41.94***	40.14***
	(5.89)	(5.63)	(5.77)	(4.80)	(4.58)
σ_e	23.55***	23.56***	23.56***	23.56***	23.57***
	(0.76)	(0.76)	(0.76)	(0.76)	(0.76)
Wald-chi2	165.463***	170.291***	168.545***	182.096***	187.114***

$^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. $N = 1296$. Dependent variable is *Donation*, standard errors in parentheses. Assuming independence of observations. Lower limit of the RE Tobit-regression 0, upper limit 100. This table has been shortened. The complete table can be found in Appendix C.7.

OLS-regression and a RE Probit-regression are conducted to estimate the relevant factors for the binary donation probability (no/yes). The results are presented in Tables 5.4 and 5.5, respectively. Both regressions show a strong and significant positive effect of *Exclusion* on donation probability, confirming the results from Section 5.1.1.

Table 5.4: OLS-Regression on Determinants of Donation Probability

	I	II	III	IV	V
Exclusion	0.202***	0.174***	0.209***	0.253***	0.272***
(0=No Ex, 1=Ex)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Contest	-0.009	-0.009	-0.009	-0.009	-0.009
(0=Lottery, 1=C)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Strategy	-0.021	-0.021	-0.021	-0.021	-0.021
(0=Lottery, 1=S)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
safeamount	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
constant	0.491***	0.675***	14.854**	0.472***	33.590***
	(0.03)	(0.04)	(6.96)	(0.11)	(7.99)
elicited controls	no	yes	no	no	yes
socio-dem. controls	no	no	yes	no	yes
attitudes	no	no	no	yes	yes
R2	0.071***	0.119***	0.095***	0.257***	0.299***
N	1296	1296	1296	1242	1242

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Dependent variable is *Donation-binary*, standard errors in parentheses. Assuming independence of observations. This table has been shortened. The complete table can be found in Appendix C.8.

Even though in the RE Probit-regression the step-wise inclusion of controls slightly reduces the impact of *Exclusion*, it still remains significant at the 5%-level. With all controls included, its coefficient is again significant at the 1%-level, as in the OLS-regression. The effect size is surprisingly similar for both regression types: according to the results of OLS-regression V (with all controls included), *Exclusion* increases the donation probability by 27%, which is almost identical to the marginal effect (0.28%) of RE Probit-regression V.[83]

Both regressions confirm that the different production games all have a similar and negligible insignificant influence on donation probability, and that opportunity cost has a highly significant negative effect on donations, as suggested by the descriptive

[83]Note that the RE Probit-results in Table 5.5 report the latent variable, so the coefficients cannot be directly compared with the OLS-results.

Table 5.5: RE Probit: Determinants of Donation Probability

	I	II	III	IV	V
Exclusion	1.70**	1.43**	1.65**	1.80***	1.93***
(0=No Ex, 1=Ex)	(0.71)	(0.64)	(0.70)	(0.62)	(0.66)
Contest	-0.07	-0.07	-0.07	-0.07	-0.07
(0=Lottery, 1=C)	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)
Strategy	-0.17	-0.17	-0.17	-0.17	-0.17
(0=Lottery, 1=S)	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)
safeamount	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
constant	-0.55	0.73	104.47	-1.05	224.80
	(0.50)	(0.83)	(178.95)	(2.44)	(163.99)
elicited controls	no	yes	no	no	yes
socio-dem. controls	no	no	yes	no	yes
attitudes	no	no	no	yes	yes
$\ln\sigma_u^2$	1.90***	1.80***	1.85***	1.40***	1.26***
	(0.28)	(0.27)	(0.27)	(0.27)	(0.27)
Wald-chi2	90.631***	96.380***	94.492***	109.592***	113.358***
N	1296	1296	1296	1242	1242

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Dependent variable is *Donation*, standard errors in parentheses. Assuming independence of observations. This table has been shortened. The complete table can be found in Appendix C.9.

analysis in Sections 5.1.2 and 5.1.3. The effect size of opportunity cost is again similar in the two regressions: the marginal effects of the fully specified RE Probit-regression show that a 1-point increase in the safe amount leads to a 0.23% decrease in the donation probability, which is only slightly lower than the 0.25% indicated by the OLS-regression. For the individual effects of controls, see Section 5.2.5.

Regressions on Donation Level (Conditional Sample)

For the empirical analysis of the intensive margin, the donation level, only donations greater than zero are included in the regressions, i.e., all observations of $D = 0$ are dropped. Again, both a pooled OLS- and a RE Tobit-regression are used. The RE Tobit-regression is, unlike for total mean donations, left-censored by 1 point, and also right-censored by 100 points. The results are presented in Tables 5.6 and 5.7, in which it can be seen that both regressions produce very similar results, both in terms of the magnitude and significance of the coefficients.

While the regressions show the small negative impact of *Exclusion* on donations found in Section 5.1.1, this impact is not significant, with a few exceptions ($0.05 < p < 0.1$) in OLS-regressions I–III. Moreover, as noted in Sections 5.1.3 and 5.2.1, this insignificant negative effect might be partially explained by the slightly higher number of egalitarians in the *No Exclusion* treatment. This would suggest that *Exclusion* does not affect donation levels at the intensive margin at all.

For the productions games, the regression results are consistent with the findings from Section 5.1.2 and confirm that donation levels are similar for the lottery and the other two production games, although in the RE Tobit-regression an additional χ^2-test suggests that donation levels are significantly lower for the strategic game than for the contest ($p < 0.1$). The effect of opportunity cost is even more pronounced than the initial analysis in Section 5.1.3 suggests, as both OLS- and RE Tobit-regression show highly significant negative coefficients across all regression specifications (I–V). The effects of control variables are detailed in Section 5.2.5.

Although *Exclusion* has no positive influence on donation levels, Hypothesis 1 can be confirmed on average. This effect is however driven only by the extensive and not by the intensive margin. In the case of *Exclusion*, a dictator is more likely to donate anything than in the case of *No Exclusion*, but the given donation amount is determined independently of *Exclusion* and based mainly on opportunity cost, as well as socio-demographic factors and some personal characteristics loosely related to fairness and risk preferences (see Section 5.2.5).

The strong influence of opportunity cost also already confirms Hypothesis 3 that compensation decreases with increasing alternative income. This average effect is driven by both the extensive and intensive margins. Opportunity costs are further examined in Section 5.2.4, in light of Hypothesis 4. Before that, the next Section 5.2.3 empirically examines Hypothesis 2, the interaction effect of production choice and treatment already found in Section 5.1.2.

Table 5.6: OLS-Regression on Determinants of Donation Level

	I	II	III	IV	V
Exclusion	-3.75*	-3.64*	-4.63**	-4.07	-3.40
(0=No Ex, 1=Ex)	(2.17)	(2.20)	(2.23)	(2.97)	(2.92)
Contest	2.79	2.71	2.86	1.72	2.20
(0=Lottery, 1=C)	(2.58)	(2.58)	(2.57)	(2.43)	(2.28)
Strategy	0.19	0.02	0.19	-0.24	-0.48
(0=Lottery, 1=S)	(2.59)	(2.59)	(2.59)	(2.43)	(2.28)
safeamount	-0.11***	-0.10***	-0.11***	-0.13***	-0.15***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
constant	42.70***	44.79***	-316.80	9.91	-811.22
	(2.68)	(3.47)	(651.06)	(10.00)	(770.64)
elicited controls	no	yes	no	no	yes
socio-dem. controls	no	no	yes	no	yes
attitudes	no	no	no	yes	yes
R2	0.026***	0.030***	0.039***	0.168***	0.279***

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 593$ (only observations with donations > 0 included). Dependent variable is *Donation* ≥ 1, standard errors in parentheses. Assuming independence of observations. This table has been shortened. The complete table can be found in Appendix C.10.

5.2.3 Influence of Production Choice

Now, this section further examines Hypothesis 2, positing that recipients' production choices and the associated level of control serve as basis for compensation. Tables 5.2 to 5.7, discussed above, already confirm econometrically that there is no fundamental difference between the donations for the different production games. Therefore, the analysis proceeds to further investigate the interaction effect of production games and *Exclusion* found in Section 5.1.2. Since the previous tables in Section 5.2.2 do not show any notable systematic effects of the step-wise inclusion of control variables, the tables that follow from now on include only specification I without control variables, and specification V, from now on II, with all control variables.

Again, the analysis of the effects of the different production games focuses first on mean donations before empirically examining the effects at the extensive and intensive margins. The first results are presented in Tables 5.8 and 5.9, which display the effect of *Exclusion* on mean donations by the different production games separately for both a pooled OLS- and a RE Tobit-regression.

Table 5.7: RE Tobit-Regression: Determinants of Donation Level

	I	II	III	IV	V
Exclusion	-4.65	-5.13	-4.70	-8.81	-7.12
(0=No Ex, 1=Ex)	(6.14)	(6.14)	(6.19)	(7.12)	(6.97)
Contest	3.07	3.08	3.07	2.95	3.05
(0= Lottery, 1=C)	(1.90)	(1.90)	(1.90)	(1.90)	(1.90)
Strategy	-0.27	-0.29	-0.27	-0.31	-0.29
(0= Lottery, 1=S)	(1.89)	(1.89)	(1.89)	(1.89)	(1.89)
safeamount	-0.21***	-0.21***	-0.21***	-0.21***	-0.21***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
constant	43.05***	48.72***	318.58	7.89	-129.43
	(4.88)	(7.74)	(1769.45)	(26.35)	(1841.60)
elicited controls	no	yes	no	no	yes
socio-dem. controls	no	no	yes	no	yes
attitutes	no	no	no	yes	yes
σ_u	20.31***	20.16***	20.08***	16.92***	14.91***
	(2.25)	(2.23)	(2.23)	(1.90)	(1.75)
σ_e	18.37***	18.36***	18.37***	18.34***	18.36***
	(0.59)	(0.59)	(0.59)	(0.59)	(0.59)
Wald-chi2	73.940***	74.806***	74.933***	91.820***	106.043***

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 593$ (only observations with donations >0 included). Dependent variable is $Donation \geq 1$, standard errors in parentheses. Assuming independence of observations. Lower limit of the RE Tobit-regression 1, upper limit 100. This table has been shortened. The complete table can be found in Appendix C.11.

Table 5.8: OLS-Regression on Total Donations by Production Choice

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
Exclusion	2.43	-0.27	6.89***	9.15***	7.92***	9.90***
(0=No Ex, 1=Ex)	(2.45)	(3.09)	(2.48)	(2.97)	(2.23)	(2.74)
safeamount	-0.14***	-0.14***	-0.13***	-0.13***	-0.14***	-0.14***
	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)
constant	22.45***	-1013.98	20.58***	1124.46	19.26***	1664.14**
	(2.50)	(772.13)	(2.52)	(742.75)	(2.27)	(685.72)
controls (V)	no	yes	no	yes	no	yes
R2	0.036***	0.227***	0.044***	0.303***	0.069***	0.288***

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 432$. Dependent variable is *Donation*, standard errors in parentheses. Assuming independence of observations. This table has been shortened. The complete table with all control variables can be found in Appendix C.12.

Table 5.9: RE Tobit-Regression on Total Donations by Production Choice

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
Exclusion	16.60	14.92	15.53	20.01	22.91**	25.92**
(0=No Ex, 1=Ex)	(11.95)	(13.55)	(13.43)	(14.33)	(11.20)	(12.11)
safeamount	-0.33***	-0.33***	-0.30***	-0.30***	-0.33***	-0.34***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
constant	-0.18	-623.08	-3.81	2374.37	-4.45	2829.05
	(8.99)	(3438.36)	(10.02)	(3575.62)	(8.53)	(3014.77)
controls (V)	no	yes	no	yes	no	yes
σ_u	46.94***	38.05***	52.89***	40.37***	43.47***	33.27***
	(5.62)	(4.57)	(6.72)	(5.22)	(5.36)	(4.20)
σ_e	20.73***	20.76***	21.34***	21.35***	21.35***	21.39***
	(1.22)	(1.23)	(1.24)	(1.25)	(1.25)	(1.26)
Wald-chi2	67.559***	86.345***	53.915***	75.517***	68.255***	89.625***

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 432$. Dependent variable is *Donation*, standard errors in parentheses. Assuming independence of observations. Lower limit of the RE Tobit-regression 0, upper limit 100. This table has been shortened. The complete table with all control variables can be found in Appendix C.13.

They show that the most stable and highly significant effect ($p < 0.01$) is again caused by opportunity cost, as increasing the safe amount by 1 point reduces donations by 0.13 (I) to 0.14 (II) points in each production game. For the lottery and the strategic game, both the OLS- and the RE Tobit-regression confirm the earlier descriptive results of Section 5.1.2 that *Exclusion* has no effect on donations in the lottery, but a positive, strong and highly significant effect in the strategic game. This difference is confirmed to be significant by cross-equation tests, for the specifications with controls at the 5%-level, and for those without at the 10%-level.

Only the regression results for the contest are not consistent with the previous findings. While the first analysis (see Figure 5.5 in Section 5.1.2) finds a significant effect of *Exclusion* in the contest rounds, this effect is only econometrically confirmed in the OLS-regressions in Table 5.8, but not in the RE Tobit-regressions in Table 5.9. Similarly, cross-equation tests for the OLS-regressions also yield conflicting results on whether the coefficient of *Exclusion* differs significantly between the contest and the lottery. The fact that willingness to participate is clearly rewarded by higher donations only in the strategic game, not in the contest, connects back to the argument from Section 5.1.2: it is possible that the higher level of control in the contest is offset by its higher effort, as the contest and the lottery are chosen equally often as hardest production, whereas the strategic game is selected only half as often. The influence of control variables is detailed in Section 5.2.5. Now, the next section moves on to the analysis of Hypothesis 2 at the extensive margin.

Regressions on Donation Probability

The analysis of the interaction effect of *Exclusion* and the different production games at the extensive margin and is again conducted with both a pooled OLS- and a RE Probit-regression on donation probability by the different production games separately, once without (I) and with (II) controls. Results are displayed in Tables 5.10 and 5.11.

The econometric results of both regressions are fully consistent with the initial analysis (see Section 5.1.2, Figure 5.6a) and show that *Exclusion* significantly increases donation probability for all three games. This is confirmed by cross-equation tests. There are also no major surprises for opportunity cost: increasing the safe amount reduces the donation probability equally for all three production games. This effect is again highly significant ($p < 0.01$) and has the same size as in Tables 5.4 and 5.5 from Section 5.2.2 (0.23% to 0.25%). The individual results of the control variables are shown in Section 5.2.5. Having confirmed that there is no interaction effect of *Exclusion* and production choice at the extensive margin, the analysis now turns to the intensive margin.

Table 5.10: OLS-Regression on Donation Probability by Production Choice

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
Exclusion	0.25***	0.29***	0.15***	0.24***	0.21***	0.29***
(0=No Ex, 1=Ex)	(0.05)	(0.06)	(0.05)	(0.06)	(0.05)	(0.06)
safeamount	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
constant	0.47***	22.50	0.50***	42.12***	0.47***	35.85**
	(0.05)	(13.88)	(0.05)	(14.05)	(0.05)	(13.91)
controls (V)	no	yes	no	yes	no	yes
R2	0.093	0.328	0.049	0.310	0.075	0.320

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 432$. Dependent variable is *Donation-binary*, standard errors in parentheses. Assuming independence of observations. This table has been shortened. The complete table with all control variables can be found in Appendix C.14.

Table 5.11: RE Probit-Regression on Donation Probability by Production Choice

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
Exclusion	1.75**	1.89**	1.73	2.22**	1.34**	1.65***
(0=No Ex, 1=Ex)	(0.73)	(0.75)	(1.24)	(0.96)	(0.60)	(0.63)
safeamount	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
constant	-0.28	140.56	-0.42	364.73	-0.35	213.20
	(0.52)	(183.66)	(0.75)	(230.41)	(0.45)	(155.06)
controls (V)	no	yes	no	yes	no	yes
$ln\sigma_u^2$	1.95***	1.30***	2.51***	1.85***	1.61***	0.92***
	(0.36)	(0.36)	(0.40)	(0.36)	(0.33)	(0.34)
Wald-chi2	32.837***	44.153***	27.842***	45.063***	31.100***	45.863***
N	432	414	432	414	432	414

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Dependent variable is *Donation*, standard errors in parentheses. Assuming independence of observations. This table has been shortened. The complete table with all control variables can be found in Appendix C.15.

Regressions on Donation Level

To econometrically estimate the interaction effect of production choice and *Exclusion* at the intensive margin, the analysis proceeds as before: an OLS- and a RE Tobit-regression are conducted only for the observations where $D > 0$. The results can be found in Tables 5.12 and 5.13.

Table 5.12: OLS-Regression on Donation Level by Production Choice

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
Exclusion	-15.06***	-14.16***	2.47	3.30	1.41	0.66
(0=No Ex, 1=Ex)	(3.86)	(5.33)	(3.80)	(4.79)	(3.51)	(5.03)
safeamount	-0.12**	-0.16***	-0.08	-0.13**	-0.13**	-0.17***
	(0.06)	(0.05)	(0.06)	(0.05)	(0.05)	(0.05)
constant	50.32***	-1638.86	40.74***	-1011.93	40.54***	1214.71
	(3.97)	(1454.59)	(3.87)	(1244.28)	(3.50)	(1302.94)
controls (V)	no	yes	no	yes	no	yes
R2	0.088***	0.365***	0.012***	0.407***	0.030***	0.325***
N	202	202	198	198	193	193

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Dependent variable is *Donation* ≥ 1, standard errors in parentheses. Assuming independence of observations. This table has been shortened. The complete table with all control variables can be found in Appendix C.16.

They show that choosing the lottery in the *Exclusion* treatment has a significant and strong negative effect on donation levels, whereas the results for the contest and strategic game are not significant, small and mostly positive. Cross-equation tests confirm that in the OLS-regressions both with and without controls, the coefficients of *Exclusion* for the lottery are significantly smaller than those for the contest (at the 1%-level) and the strategic game (at the 5%-level). This pattern almost exactly matches the findings from Figure 5.6b in Section 5.1.2 and suggests that in case of a recipient's voluntary decision to participate in a production game with a low level of (perceived) control, compensation is reduced as posited by Hypothesis 2, but at the intensive margin only. Since no significant difference in donation levels is observed between the contest and the strategic game, it seems possible that dictators do not distinguish between different levels of control over outcomes, but only between the absence of control, as in the lottery, and the presence of control, as in the two other games. This could be further explored in future studies.

As in all previous regressions, the highly significant negative effect of opportunity costs on donation levels is confirmed for all production games, with the coefficients

Table 5.13: RE Tobit-Regression on Donation Level by Production Choice

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
Exclusion	-12.79*	-14.38*	0.11	1.19	0.13	-2.86
(0=No Ex, 1=Ex)	(7.21)	(8.64)	(7.24)	(7.33)	(6.24)	(6.98)
safeamount	-0.22***	-0.23***	-0.18***	-0.18***	-0.20***	-0.22***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
constant	49.53***	-607.17	44.00***	-778.69	41.16***	2029.02
	(5.77)	(2413.47)	(5.68)	(1897.37)	(5.01)	(1835.29)
controls (V)	no	yes	no	yes	no	yes
σ_u	22.54***	17.20***	22.07***	13.54***	18.85***	12.68***
	(2.70)	(2.27)	(2.84)	(2.15)	(2.41)	(1.88)
σ_e	16.68***	16.68***	17.32***	17.30***	15.96***	15.92***
	(1.00)	(1.01)	(1.02)	(1.02)	(0.96)	(0.96)
Wald-chi2	34.027***	59.412***	19.350***	64.268***	27.049***	63.192***
N	202	202	198	198	193	193

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Dependent variable is *Donation* ≥ 1, standard errors in parentheses. Assuming independence of observations. Lower limit of the RE Tobit-regression 1, upper limit 100. This table has been shortened. The complete table with all control variables can be found in Appendix C.17.

showing no significant differences among the three games. The effects of the control variables are shown in Section 5.2.5.

All in all, this section provides some evidence for Hypothesis 2: while there are no differences between production games at the extensive margin, voluntarily decisions to participate in the lottery are associated with lower donation levels. On average, only the participation decision in the strategic game leads to higher donations. Therefore, Hypothesis 2 is partially supported only at the intensive margin, and the expected order of the contest and the strategic game according to their different levels of control cannot be confirmed.

5.2.4 Influence of Opportunity Costs

This section now continues the analysis by econometrically examining the influence of opportunity costs on compensation for exclusion. At this point, however, Hypothesis 3, positing that donations decline with decreasing opportunity costs of suspension/exclusion, is no longer relevant, as it has already been confirmed by all the results presented above that the donations decrease as the level of alternative income increases. Instead, this section specifically aims to provide more precise insights into the existence of a discrimination premium as posited in Hypothesis

4. Therefore, unlike in the previous sections, it is no longer assumed that the safe amount has a constant linear tendency, and it is hence no longer included as a cardinal variable. Instead, the effects of *Exclusion* and all other variables are estimated separately for each safe amount. Otherwise, the analysis proceeds exactly the same as before, and first conducts the econometric regressions for total donations, and then for extensive and intensive margins. This is again done both without (I) and with (II) all control variables.

Tables 5.14 and 5.15 show the regression results for the OLS- and RE Tobit-regression, respectively, on total donations by safe amount. They indicate that *Exclusion* does not increase donations for safe amounts equal to or greater than 60 points as an alternative income, so there is no evidence of a discrimination premium in total donations. It is furthermore notable that *Exclusion* also has no significant effect at a safe amount of zero. This could possibly indicate that the alternative income of zero is perceived as *Need* (see Section 2.1.3) by some or most dictators, which is then also compensated in the *No Exclusion* treatment and could cancel out any further injustice by exclusion in the *Exclusion* treatment. In this case, the safe amount of 20 points could be interpreted as the threshold income accepted as meeting need, since at this point donations drop sharply (see Figure 5.9b) and the regressions show that exclusion is compensated. The effects of control variables are detailed in Section 5.2.5.

All in all, the above results show that exclusion is compensated for medium-high safe amounts, and that mean total donations do not support the existence of a discrimination premium as posited in Hypothesis 4. However, this clear picture changes when the focus shifts to the extensive margin.

Regressions on Donation Probability

Tables 5.16 and 5.17 display the results of the pooled OLS- and RE Probit-regression on donation probability by safe amount. In the OLS-regression, the positive impact of *Exclusion* is shown to be highly significant for all safe amounts except 80 points, where the inclusion of controls renders the effect insignificant. In the RE Probit-regression, *Exclusion* also significantly increases the donation probability for all safe amounts except 80 points, although here *Exclusion* is only significant at the 10% level for the safe amount of 100 points, and only as long as no control variables are included. These results provide only weak evidence for a discrimination premium at the extensive margin, but show that the effect of the increasing safe amount does not correspond to the initially assumed linear trend.

Therefore, the analysis is supplemented by predictive margins for the interaction effect of *safeamount* and *Exclusion* on donation probability, based on both an OLS-regression and a RE Probit-regression including all control variables. The resulting predictive margins can be found in Appendices C.25 and C.26, and are plotted in Figures 5.12a and 5.12b. These show that the predicted donation probability in the

Table 5.14: OLS-Regression on Total Donations by Safe Amount

	S.A.=0		S.A.=20		S.A.=40	
	I	II	I	II	I	II
Exclusion	7.69*	6.64	9.53**	9.14*	7.44**	10.04**
(0=No Ex, 1=Ex)	(4.47)	(5.67)	(3.68)	(4.64)	(3.15)	(3.87)
Contest	-0.25	-0.25	-0.19	-0.19	3.01	3.01
(0=L, 1=C)	(5.48)	(5.08)	(4.50)	(4.16)	(3.86)	(3.47)
Strategy	-1.51	-1.51	0.14	0.14	0.64	0.64
(0=L, 1=S)	(5.48)	(5.08)	(4.50)	(4.16)	(3.86)	(3.47)
constant	22.63***	-534.57	15.25***	554.56	11.28***	1183.67
	(4.47)	(1414.79)	(3.68)	(1157.74)	(3.15)	(965.45)
controls (V)	no	yes	no	yes	no	yes
R2	0.014	0.245***	0.031*	0.265***	0.029*	0.302***

	S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II
Exclusion	5.04*	5.44	2.74	3.79	2.05	2.52
(0=No Ex, 1=Ex)	(2.94)	(3.61)	(2.69)	(3.29)	(3.10)	(3.82)
Contest	0.78	0.78	2.21	2.21	0.61	0.61
(0=L, 1=C)	(3.60)	(3.24)	(3.30)	(2.95)	(3.80)	(3.42)
Strategy	-1.88	-1.87	0.82	0.82	-1.89	-1.89
(0=L, 1=S)	(3.60)	(3.24)	(3.30)	(2.95)	(3.80)	(3.42)
constant	12.50***	850.79	10.24***	858.34	10.94***	660.25
	(2.94)	(901.66)	(2.69)	(820.69)	(3.10)	(952.69)
controls (V)	no	yes	no	yes	no	yes
R2	0.016	0.293***	0.007	0.295***	0.004	0.280***

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 216$. Dependent variable is *Donation*, standard errors in parentheses. Assuming independence of observations. This table has been shortened. The complete table with all control variables can be found in Appendix C.18.

Table 5.15: RE Tobit-Regression on Total Donations by Safe Amount

	S.A.=0		S.A.=20		S.A.=40	
	I	II	I	II	I	II
Exclusion	22.74	25.59	21.75*	25.76*	22.60**	29.11**
(0=No Ex, 1=Ex)	(16.35)	(18.72)	(11.96)	(13.41)	(10.78)	(11.55)
Contest	-1.30	-1.25	-1.23	-1.27	5.54*	5.55*
(0=L, 1=C)	(4.77)	(4.77)	(3.41)	(3.42)	(2.91)	(2.91)
Strategy	-3.44	-3.45	-0.66	-0.68	0.27	0.26
(0=L, 1=S)	(4.80)	(4.80)	(3.40)	(3.40)	(2.95)	(2.96)
constant	-5.25	273.69	-9.96	2327.46	-17.31**	4329.57
	(12.61)	(4617.99)	(9.28)	(3304.73)	(8.35)	(2880.37)
controls (V)	no	yes	no	yes	no	yes
σ_u	61.60***	51.02***	44.92***	36.35***	40.48***	30.77***
	(8.07)	(6.77)	(5.64)	(4.59)	(5.15)	(3.91)
σ_e	22.40***	22.42***	16.13***	16.12***	13.33***	13.33***
	(1.99)	(2.00)	(1.29)	(1.30)	(1.13)	(1.14)
Wald-chi2	2.459	18.590	3.438	24.693	8.945**	36.520*

	S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II
Exclusion	15.62	18.12	10.46	13.19	26.37	23.18
(0=No Ex, 1=Ex)	(11.04)	(11.86)	(11.32)	(12.15)	(19.60)	(21.30)
Contest	0.68	0.74	3.95	3.97	2.56	2.27
(0=L, 1=C)	(3.49)	(3.49)	(3.65)	(3.64)	(7.87)	(7.93)
Strategy	-4.27	-4.25	0.20	0.20	-5.58	-5.66
(0=L, 1=S)	(3.52)	(3.52)	(3.69)	(3.68)	(7.95)	(8.00)
constant	-14.60*	3645.66	-20.98**	3888.54	-63.76***	6082.26
	(8.72)	(3020.00)	(9.01)	(3099.59)	(17.38)	(5546.64)
controls (V)	no	yes	no	yes	no	yes
σ_u	41.32***	30.84***	42.52***	31.37***	69.70***	43.62***
	(5.65)	(4.26)	(6.16)	(4.65)	(11.86)	(8.39)
σ_e	15.75***	15.71***	15.65***	15.61***	29.35***	29.51***
	(1.36)	(1.36)	(1.39)	(1.39)	(3.32)	(3.37)
Wald-chi2	4.284	31.255	2.316	26.751	2.865	24.324

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 216$. Dependent variable is *Donation*, standard errors in parentheses. Assuming independence of observations. Lower limit of the RE Tobit-regression 0, upper limit 100. This table has been shortened. The complete table with all control variables can be found in Appendix C.19.

Table 5.16: OLS-Regression on Donation Probability by Safe Amount

	S.A.=0		S.A.=20		S.A.=40	
	I	II	I	II	I	II
Exclusion	0.22***	0.34***	0.22***	0.31***	0.30***	0.41***
(0=No Ex,1=Ex)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)
Contest	-0.01	-0.01	-0.04	-0.04	0.03	0.03
(0=L, 1=C)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)
Strategy	-0.03	-0.03	-0.01	-0.01	-0.01	-0.01
(0=L, 1=S)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)
constant	0.46***	15.46	0.44***	24.76	0.34***	46.05**
	(0.07)	(20.29)	(0.07)	(20.37)	(0.07)	(19.24)
controls (V)	no	yes	no	yes	no	yes
R2	0.051***	0.324***	0.051***	0.323***	0.089***	0.399***

	S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II
Exclusion	0.20***	0.25***	0.12*	0.12	0.15**	0.19***
(0=No Ex,1=Ex)	(0.07)	(0.08)	(0.07)	(0.08)	(0.06)	(0.07)
Contest	-0.01	-0.01	-0.01	-0.01	-0.00	0.00
(0=L, 1=C)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.06)
Strategy	-0.01	-0.01	-0.04	-0.04	-0.01	-0.01
(0=L, 1=S)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.06)
constant	0.37***	35.26*	0.37***	33.53	0.22***	45.74***
	(0.07)	(20.17)	(0.07)	(20.70)	(0.06)	(17.27)
controls (V)	no	yes	no	yes	no	yes
R2	0.042**	0.336***	0.016	0.283***	0.027	0.408***

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 216$. Dependent variable is *Donation-binary*, standard errors in parentheses. Assuming independence of observations. This table has been shortened. The complete table with all control variables can be found in Appendix C.20.

Table 5.17: RE Probit-Regression on Donation Probability by Safe Amount

	S.A.=0		S.A.=20		S.A.=40	
	I	II	I	II	I	II
Exclusion	1.93*	2.23**	3.70***	4.50***	6.03***	4.77***
(0=No Ex, 1=Ex)	(0.99)	(0.96)	(0.85)	(1.32)	(0.88)	(1.63)
Contest	-0.11	-0.12	-0.57	-0.57	0.38	0.36
(0=L, 1=C)	(0.36)	(0.36)	(0.48)	(0.48)	(0.47)	(0.45)
Strategy	-0.23	-0.23	-0.19	-0.18	-0.21	-0.18
(0=L, 1=S)	(0.36)	(0.36)	(0.47)	(0.47)	(0.48)	(0.46)
constant	-0.46	44.77	-1.11*	287.08	-2.89***	612.92
	(0.71)	(217.60)	(0.66)	(334.57)	(0.66)	(373.40)
controls (V)	no	yes	no	yes	no	yes
$ln\sigma_u^2$	2.22***	1.47***	3.33***	2.79***	3.53***	2.30***
	(0.50)	(0.51)	(0.48)	(0.47)	(0.42)	(0.51)
Wald-chi2	4.131	14.896	19.638***	31.048	47.340***	21.434
N	216	207	216	207	216	207

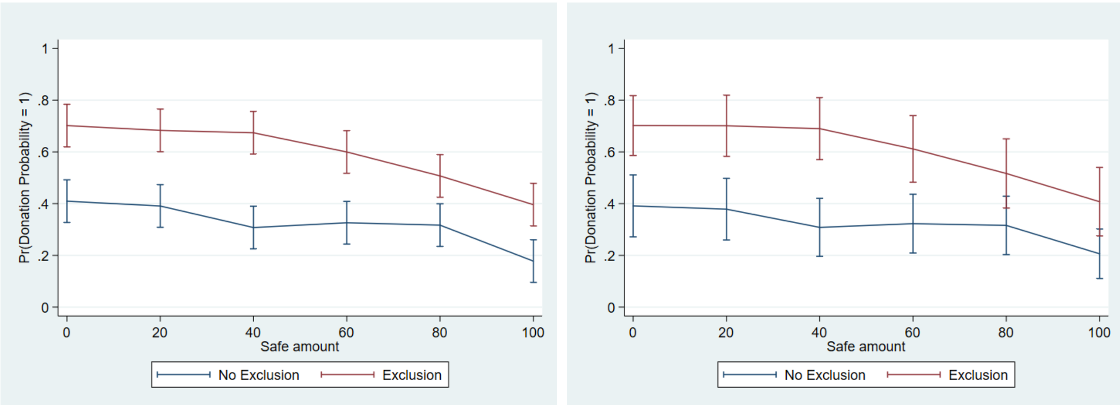
	S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II
Exclusion	3.40***	2.99**	0.67	2.10	1.50*	2.17
(0=No Ex, 1=Ex)	(0.91)	(1.52)	(1.14)	(1.53)	(0.82)	(1.51)
Contest	-0.17	-0.16	-0.20	-0.18	-0.00	-0.01
(0=L, 1=C)	(0.44)	(0.44)	(0.47)	(0.46)	(0.47)	(0.47)
Strategy	-0.17	-0.17	-0.61	-0.57	-0.18	-0.19
(0=L, 1=S)	(0.44)	(0.44)	(0.49)	(0.48)	(0.47)	(0.47)
constant	-1.88***	435.48	-1.81***	414.01	-3.77***	478.08
	(0.64)	(419.54)	(0.63)	(328.41)	(0.61)	(353.64)
controls (V)	no	yes	no	yes	no	yes
$ln\sigma_u^2$	3.17***	2.50***	3.43***	2.78***	3.12***	1.91***
	(0.49)	(0.45)	(0.50)	(0.46)	(0.41)	(0.73)
Wald-chi2	14.202***	27.597	1.983	34.657*	3.564	9.125
N	216	207	216	207	216	207

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Dependent variable is *Donation*, standard errors in parentheses. Assuming independence of observations. This table has been shortened. The complete table with all control variables can be found in Appendix C.21.

Exclusion treatment is significantly higher than in the *No Exclusion* treatment for *all* safe amounts. Thus, there is weak evidence for the discrimination premium at the extensive margin.

Figure 5.12: Predictive Margins of Donation Probability

(a) OLS-based (b) RE Probit-based



Regressions on Donation Level (Conditional Sample)

Next, the analysis turns to the effect of opportunity cost on compensation for *Exclusion* at the intensive margin. Tables 5.18 and 5.19 show the results of the pooled OLS- and RE Tobit-regression, both for the observations of $D > 1$ only.

These econometric results confirm the findings from Section 5.1.3 that *Exclusion* does not lead to a significant increase in donations levels. Instead, it seems that *Exclusion* actually significantly decreases donation levels in the last round. As explained in Section 5.1.3, this artifact can be explained by the continuous reduction of observations, so that donations are biased upward by the few remaining donors who are insensitive to opportunity costs.

It can therefore be concluded that although these results strongly support Hypothesis 3 for both the extensive and intensive margins, the discrimination premium posited by Hypothesis 4 exists, if at all, only at the extensive margin.

5.2.5 Robustness Checks

This section presents the findings on robustness checks elicited via the questionnaire as well as on control variables in the regressions. The complete Regression Tables C.6 to C.23 in the appendix show that most control variables are insignificant. The few that are significant in one or both of the respective regression types per analysis are examined in more detail after the robustness checks have been presented. These consist of dictator statements regarding the fairness and reason for their donation decision.

Table 5.18: OLS-Regression on Donation Level by Safe Amount

	S.A.=0		S.A.=20		S.A.=40	
	I	II	I	II	I	II
Exclusion	-4.99	-9.80	2.49	6.87	-4.96	5.68
(0=No Ex,1=Ex)	(5.89)	(7.36)	(5.23)	(7.36)	(4.85)	(7.42)
Contest	0.66	1.37	2.62	1.54	3.60	4.03
(0=L, 1=C)	(7.02)	(5.97)	(6.29)	(5.51)	(5.61)	(4.86)
Strategy	-0.51	0.05	1.27	1.40	1.93	1.06
(0=L, 1=S)	(7.07)	(5.94)	(6.20)	(5.38)	(5.71)	(4.91)
constant	49.53***	-2398.33	34.47***	-1110.33	34.40***	-640.09
	(6.10)	(1884.12)	(5.45)	(1748.63)	(5.29)	(1764.63)
controls (V)	no	yes	no	yes	no	yes
R2	0.006	0.440***	0.003	0.400***	0.016	0.434***
N	120	120	116	116	106	106

	S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II
Exclusion	-3.10	-4.38	-1.96	-5.04	-14.25**	-46.26*
(0=No Ex,1=Ex)	(4.51)	(6.40)	(4.30)	(6.97)	(6.57)	(23.50)
Contest	2.44	0.89	5.98	4.00	2.77	4.01
(0=L, 1=C)	(5.37)	(4.72)	(5.14)	(4.62)	(7.67)	(6.97)
Strategy	-3.25	-5.26	4.99	1.81	-2.92	0.74
(0=L, 1=S)	(5.37)	(4.74)	(5.21)	(4.66)	(7.80)	(7.25)
constant	33.80***	454.83	28.17***	219.29	49.14***	-7688.81**
	(4.76)	(1734.60)	(4.45)	(1906.83)	(6.59)	(3403.83)
controls (V)	no	yes	no	yes	no	yes
R2	0.017	0.418***	0.021	0.429**	0.087	0.551**
N	100	100	89	89	62	62

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Dependent variable is *Donation* ≥ 1, standard errors in parentheses. Assuming independence of observations. This table has been shortened. The complete table with all control variables can be found in Appendix C.22.

Table 5.19: RE Tobit-Regression on Donation Level by Safe Amount

	S.A.=0		S.A.=20		S.A.=40	
	I	II	I	II	I	II
Exclusion	-7.29	-15.89	2.37	6.08	-2.70	6.15
(0=No Ex,1=Ex)	(10.25)	(11.23)	(7.72)	(9.26)	(6.96)	(8.34)
Contest	1.46	1.56	2.52	2.27	4.52	4.53
(0=L, 1=C)	(3.97)	(3.99)	(3.39)	(3.37)	(2.97)	(2.94)
Strategy	-0.45	-0.32	1.07	1.11	1.05	0.85
(0=L, 1=S)	(3.99)	(4.00)	(3.30)	(3.27)	(2.95)	(2.92)
constant	51.50***	-3582.41	33.68***	-947.89	30.96***	-625.62
	(8.24)	(2827.00)	(6.22)	(2213.47)	(5.82)	(2093.76)
controls (V)	no	yes	no	yes	no	yes
σ_u	32.18***	21.57***	23.21***	15.87***	20.07***	13.18***
	(4.03)	(3.01)	(2.88)	(2.24)	(2.56)	(1.93)
σ_e	16.45***	16.47***	14.32***	14.23***	11.95***	11.83***
	(1.50)	(1.51)	(1.20)	(1.18)	(1.06)	(1.03)
Wald-chi2	0.757	41.906***	0.643	38.581***	2.818	44.428***
N	120	120	116	116	106	106

	S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II
Exclusion	-2.93	-3.42	-2.00	-5.11	-18.27**	-53.29***
(0=No Ex,1=Ex)	(6.21)	(6.77)	(5.54)	(6.17)	(8.63)	(20.57)
Contest	1.45	0.87	5.49	4.17	3.75	4.98
(0=L, 1=C)	(3.66)	(3.65)	(3.95)	(3.87)	(6.99)	(5.82)
Strategy	-4.54	-5.41	3.81	1.89	-2.46	0.55
(0=L, 1=S)	(3.66)	(3.66)	(3.99)	(3.91)	(7.16)	(6.02)
constant	33.35***	426.08	27.70***	183.39	51.82***	-8946.27***
controls (V)	no	yes	no	yes	no	yes
	(5.26)	(1881.49)	(4.79)	(1689.40)	(7.64)	(2970.37)
σ_u	16.35***	8.79***	12.72***	3.26	13.99***	0.00
	(2.49)	(2.25)	(2.49)	(3.90)	(5.02)	(2.91)
σ_e	14.56***	14.60***	15.04***	14.71***	22.12***	18.02***
	(1.32)	(1.33)	(1.44)	(1.37)	(2.83)	(1.72)
Wald-chi2	3.144	46.959***	2.251	61.080***	5.523	63.534***
N	100	100	89	89	62	62

$*p < 0.10$, $**p < 0.05$, $***p < 0.01$. Dependent variable is *Donation* ≥ 1, standard errors in parentheses. Assuming independence of observations. Lower limit of the RE Tobit-regression 1, upper limit 100. This table has been shortened. The complete table with all control variables can be found in Appendix C.23.

Dictators' Statements on their Donation Decision

A first robustness check on the compensation for exclusion is given by the variable *donationfair*. It captures the answer to the last question of the dictators' questionnaire "How strongly did you consider in your donation decision that this leads to a fair payoff for your recipient?". Although just not significantly different between treatments (t-test: $p = 0.135$, $t = 1.511$; χ^2-test: $p = 0.155$, $\chi^2 = 2.025$), this ex-post statement explains almost half of the donation decisions, see the correlation results in Section 5.2.1. A simple OLS-regression confirms that the higher consideration of fairness in the *Exclusion*-treatment is itself a treatment effect, see Table 5.20. Thus, *donationfair* is dropped from the econometric analysis to not bias the treatment effect of *Exclusion*, and only serves as a robustness check that exclusion is indeed compensated because it violates fairness norms of dictators.

Table 5.20: OLS-regression on Determinants of *Donationfair*

Exclusion	0.65***
(0=NoEx, 1=Ex)	(0.09)
Contest	-0.00
(0=Lottery, 1=Contest)	(0.08)
Strategy	-0.00
(0=Lottery, 1=Strategy)	(0.08)
safeamount	-0.00
	(0.00)
controls (V)	yes
constant	-13.20
	(23.25)
R2	0.398

$^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. $N = 1296$. Dependent variable is *Donationfair*, standard errors in parentheses. Assuming independence of observations. This table has been shortened. The complete table includes the results of all control variables and can be found in Appendix C.24.

Second, dictators' justifications for their donation decisions are checked for consistency to with their donation behavior in order to verify that dictators take the experiment seriously. For this purpose, the given justifications are categorized according to the main fairness ideal they express. Based on these qualitative criteria, the dictators are then classified into different types of fairness preferences. The categorization is not entirely clear-cut, as some answers contain references to more than one fairness ideal, while other answers are incomprehensible or obviously not meant seriously. Therefore, the resulting dictator categories cannot be compared with the categories found, for example, by Cappelen et al. (2007) based on the actual donation behavior of dictators. Nevertheless, this thesis shows that most of the comprehensible justifications can be roughly divided into three groups, as they are either selfish (18 subjects), refer to non-specific fairness norms (14 subjects) or to specific thresholds (20 subjects). A fourth group clearly relates to egalitarian ideals, but includes significantly fewer (8) dictators. Although these justifications are not very precise, they show that the donation justifications of most dictators are indeed consistent with their donation behavior, see Table 5.21.

Table 5.21: Mean Donations by Dictator Type

Dictator Type	Percent	Mean Donation	Std. Err.	Std. Dev.
Incomprehensible	8.33	25.056	2.762	28.7
Selfish	25	2.593	.603	10.85
Desert / Effort	8.33	1.667	.525	5.457
Fairness (unspecific)	19.44	18.948	1.389	22.046
Egalitarian	11.11	40.958	2.847	34.17
Threshold	27.78	20.542	1.331	25.253

The table displays the share of dictator types in percent, as well as the mean donation, standard error and standard deviation per dictator type.

Those subjects who give selfish justifications donate almost nothing, which is significantly less than all other subjects. Accordingly, the few dictators with clearly egalitarian reasons donate significantly more than all other subjects, whose average donations are, as expected, between those of the selfish and the egalitarian dictators. Indeed, the fact that five of the eight egalitarian dictators participate in the *No Exclusion* treatment may explain the unexpected finding that donation levels are slightly higher in the *No Exclusion* treatment than in the *Exclusion* treatment, as described in Section 5.1.3. Finally, the importance of certain thresholds in the justifications also serves as a robustness check for the significant effect of opportunity cost on donations found in both the descriptive analysis in Section 5.1.3 and the econometric regressions in Section 5.2.

That dictators' justifications and donation behavior are consistent is in line with the study by Cappelen et al. (2011), which also shows that self-reported data on fairness preferences have substantial informational value due to a low number of misreports. However, they find that including self-reports in pure behavioral data does not improve the performance of regression specifications (ibid., pp. 118–119), which is also true for this analysis: the inclusion of justifications in the empirical regressions either has little impact on the results or leads to convergence problems in the more detailed regressions due to the unbalanced and small number of observations in the different categories. Therefore, the donation justifications are not used as a control variable in the econometric analysis, but only as a robustness check. The control variables used are now examined in more detail below.

Socio-Demographic Control Variables

The few socio-demographic control variables that have some statistically significant effects are $Female$ and being an economics student, *Econstud*. Both variables significantly lower donation levels. Being female lowers donation levels even more than being an economics student. Furthermore, the negative effect of $Female$ on donation levels persists for all safe amounts, whereas the impact of being an economics student is only significant for lower safe amounts. *Econstud* also shows a few significant negative effects on total donations, whereas $Female$ has no significant effect on it. In general, neither variable affects donation probability, although there are a few regressions (see Tables C.8, C.14, C.20, and C.21) in which $Female$ has a small but significant positive effect on donation probability.

While the negative effect of *Econstud* on generosity fits the existing literature, e.g. Marwell and Ames (1981), Carter and Irons (1991), and Cappelen et al. (2015), the negative effect of $Female$ on donation levels contrasts with Engel's (2011) findings that women are on average more generous than men in dictator games. However, it is consistent with the fact that the evidence for gender effects in donation behavior is rather mixed (see Section 2.2.4). It is also similar to the findings of Andreoni and Vesterlund (2001) and Maaser et al. (2022) according to which women are more likely to donate than men, but are less generous with the given donation amount.

Elicited Inequality Aversion

Inequality aversion is elicited by the variable WTP^d. Looking at the complete regression Tables C.6 to C.23 in the appendix, one can see both total donations and donation probability are highly significantly and negatively influenced by WTP^d. Especially for safe amounts equal or greater than 40 points, WTP^d can be considered a major determinant of total donations, given that there are only a few other variables that sometimes and unsystematically show a barely significant effect. Donation levels are, however, not influenced by elicited inequality aversion.

That WTP^d has a significant negative effect on total donations is not surprising. As explained in Section 4.4.1, the value of WTP^d is lower when subjects sacrifice efficiency to prevent their paired participant from receiving even more points than they do. Accordingly, it is higher when subjects accept the disadvantageous inequality for the more efficient allocation with a higher own income. Thus, the negative sign of WTP^d means nothing other than that subjects who are not (disadvantageously) inequality-averse but have selfish, payoff-maximizing preferences donate less. This is consistent with previous findings in the empirical literature on distributive preferences (see Section 2.2).

Stated Preferences

Similarly to inequality aversion as elicited by WTP^d, self-stated attitudes on the importance of fairness also mostly significantly influence donation behavior. As can be expected, subjects who consider fairness to be very important, which is captured by the variable *fairvimp*, donate more often and more points than other subjects.

A more surprising significant effect is found for the control variables *Expfairness* and *easiestprod*. These variables capture whether dictators judge the experimental rules to be fair, and the game that they consider the easiest production game, respectively.

The effect of *Expfair* is surprising in that it works in the wrong direction: dictators who perceive the experimental rules as fair to their recipient donate more often and more points than those who perceive the rules as unfair to their recipients. Nevertheless, this can be explained by that the fact that subjects seem to understand the question more in terms about the rules of, for example, the different production games, but not in terms of the rules of participation: Although the variable has been included with the expectation that it would be negatively affected by the *Exclusion* treatment, this is not the case: *Expfair* shows no differences between treatments, as in both *No Exclusion* and *Exclusion*, an almost identical number of subjects, about 60%, rates the experimental rules as *unfair*.

With regard to *easiestprod*, especially *StratEasy*, i.e. choosing the strategic game as easiest production, has a stronger and more significant impact on donations than *Exclusion*. This effect is prevalent in most of the regressions on total donations, donation probability and donation levels. *ContEasy*, i.e. choosing the contest as easiest production, is also associated with higher total donations and donation frequency, albeit to a lesser extent. In fact, *StratEasy*, which positively and strongly influences donations, is the only variable that is significant for all safe amounts, and even the *only* significant variable for the safe amount of zero.

While there might be some correlation between a dictator's game preferences and their donation behavior, this is likely an artifact of this experiment: Looking more closely at the *easiestprod* variable, one finds that the selection of a certain game as the easiest production game by a subset of dictators does not lead to differences in their donations between games. Instead, the total donations and donation frequency of dictators who choose the strategic game as the easiest production game is similar for all production games and significantly higher than the mean donations of dictators who choose one of the other production games as the easiest game. These dictators donate less on average, but also similar amounts across all production games. Dictators who select the lottery as easiest game donate the fewest points with the lowest frequency. This is however not significantly different from the donation behavior of those dictators who perceive the contest as easiest game.

Thus, no obvious connection can be established between a dictator's evaluation of a game's difficulty and her donation for that specific game. Instead, the perceived difficulty of production games seems to somewhat correlate with a dictator's generosity: at least in the specific subject sample of this experiment, dictators who choose the strategic game as easiest production game are also the most generous dictators, both in terms of donation probability and level. Whether this is merely an artifact of this experiment or whether these two characteristics are actually related cannot be clarified on the basis of the available data.

Finally, self-stated risk-preferences also have some impact on donations: in some of the regressions, the self-assessment of being very risk-loving, $vrisk^L$, has a small, negative significant impact on willingness to donate. Its stronger effect on donation levels is however inverse to this, as it is positive. The significant positive effect of being risk-loving on donation levels is puzzling, as it does not seem to fit the previous observation that voluntarily choosing a risky production game leads to (somewhat) reduced compensation at the intensive margin.

Nevertheless, $vrisk^L$, similar to *StratEasy*, seems to confirm that the different games and their different levels of control matter for dictators' donation decisions: risk-loving subjects are less likely to donate anything for suspending (voluntarily or involuntarily) the lottery or the strategic game than for suspending the contest (see Tables C.14 and C.15). However, when they donate, they seem to give higher amounts than risk-averse subjects in all production games. For the contest, there is also an isolated significance of self-stated fairness importance and responsibility preferences. Taken together, this shows that donation decisions for the effort-based contest are driven by somewhat different factors than donation decisions for the luck-based lottery and the strategic game.

While the control variables support that the three games are perceived differently, the data of this experiment are too limited to establish any precise systematic relationships. Future, more targeted studies could follow up on this, and, for example, examine the interactions of a production game's level of control and its production costs. Following the arguments in Section 5.1.2, the fact that choosing the strategic game leads to even higher mean donations than choosing the contest with its higher level of control could be due to the objectively higher effort costs of the latter. This is somewhat supported by the fact that only seven dictators consider the strategic game to be the hardest game, while about twice as many each consider the lottery or the contest to be so. Thus, accounting for both effort costs and level of control would explain the results in case of the contest, but this reasoning is admittedly rather speculative and would need to be further investigated by future research.

5.3 Discussion of Results

In conclusion, this section now first briefly summarizes the main results in Section 5.3.1, applies them to the hypotheses and model presented, and also highlights some further interesting results in Section 5.3.2. In doing so, it also identifies some shortcomings of this dissertation and logical extensions of it for further related research.

5.3.1 Main Findings

The data of this experiment strongly support the compensation of exclusion predicted by Hypothesis 1, since total donations are significantly higher in the *Exclusion* treatment than in the *No Exclusion* treatment, as shown by a simple comparison of means as well as both an OLS- and a RE Tobit-regression. Closer inspection reveals that this strong and highly significant mean effect is driven only by the extensive margin, not the intensive margin: all parts of the analysis have shown that the donation probability is strongly influenced by *Exclusion*. Additionally, opportunity cost, preferences to maximize payoffs, and surprisingly, production game preferences influence the donation probability, whereas socio-demographic factors do not. However, as the overall econometric analysis shows, donation levels are not significantly affected by *Exclusion*. Instead, these depend on socio-demographic factors (gender and being an economics student), some personal fairness-related characteristics and recipients' opportunity cost. It can be concluded that exclusion seems to be compensated, as suggested by the responsibility principle. Thus, the validity of Hypothesis 1 can be assumed.

That the chosen level of control serves as basis for compensation, as posited in Hypothesis 2, is also supported by the data, albeit only at the intensive margin: the decision to participate in the lottery leads to significantly lower donation levels in the *Exclusion* treatment compared to the donations for exclusion from the contest or the strategic game, which are similar to each other. The extensive margin of donations is however not affected by the choice of different production games.

Since the lottery arguably is the most risky production game with little or no any agency, these lower donation levels indicate that dictators reduce compensation for exclusion from a voluntary chosen risky production when a risk-free alternative income opportunity is available. However, there are no further differences between the strategic game with more agency than the lottery and the effort-based contest with most agency. This could possibly be due to a binary assessment of control over outcomes, differentiating only between the choice of no control as opposed to the choice of any control. Another possible explanation are the differences in production-related effort costs, which are objectively higher in the effort-based contest and might be factored into the assessment. The latter possibility is supported by the fact that the variables explaining donations in the contest differ slightly from the explanatory variables of the two other games, and that the contest is chosen as the hardest production game as often as the lottery, whereas the strategic game is chosen only half as often. This experiment is however not designed to measure the perceived level control or the effort costs of a production game. Also, the recipients' participation behavior is not informative enough to derive a preferred order of production games. Attempting to clarify how exactly effort and risk choice are related to perceptions and assessments of the different production games is therefore left to future research.

Hypothesis 3, the moderating effect of increasing opportunity costs of production on compensation for exclusion, is strongly supported the data. All figures and regressions confirm that increasing a recipient's alternative income significantly reduces donations, most strongly at the extensive margin but also significantly at the intensive margin. However, Hypothesis 4, the existence of a discrimination premium, can be clearly rejected for the mean of total donations and at the intensive margin. For the extensive margin, the evidence is less clear. While initial results seem to be consistent with Hypothesis 4, the inconclusive regression results of the econometric analysis allow neither its rejection nor its acceptance. This may be due to the sample size being somewhat too small: a power analysis based on the means and standard deviations of the two treatments indicated that 115 or more subjects per group would be required to achieve a power of 0.8, which is narrowly missed in this experiment with 108 observations per safe amount and treatment (and a resulting power of 0.78). Nonetheless, because the predictive margins based on the econometric regressions show that the donation probability for *Exclusion* is significantly and consistently higher than the donation probability for *No Exclusion*, the data appear to provide weak support for Hypothesis 4 at the extensive margin.

While the above influencing factors of compensation for exclusion at the extensive and intensive margins partially deviate from expectations, the mean compensation for *Exclusion* is surprisingly consistent with the model of compensation for exclusion as given by Equation 3.4, which is repeated here for convenience:

$$y_{\mathrm{C}'''} = \begin{cases} \alpha + \beta(\gamma\mu - y_{\mathrm{A}}), & \text{if } y_{\mathrm{A}} < \gamma\mu \\ \alpha & \text{otherwise.} \end{cases}$$

Exclusion is compensated by the discrimination premium α, which affects the extensive margin and thus only indirectly increases the average compensation: *Exclusion* increases the likelihood of donating anything at all. This is presumably also true even for those cases where recipients are no longer eligible for compensation under the responsibility principle, since they are no longer financially disadvantaged by the exclusion thanks to their sufficiently high alternative income. Donation levels are also slightly influenced by a chosen production games' perceived level of control γ. Most notably, opportunity costs y_{A} provide the strongest explanation for donations, both at the extensive and intensive margin.

5.3.2 Further Findings

In addition to supporting the model and the main hypotheses, there are several other interesting effects in the data, some of which are consistent with previous literature, and some of which lend themselves to future research.

First, with respect to opportunity costs, the results illustrate the strong influence of alternative incomes on giving, thus providing a link to the literature on need (see Section 2.1.3). Both the descriptive and econometric analysis have shown that a safe amount of zero leads to significantly more donations than higher safe amounts. This effect even overpowers the effect of exclusion, as dictators' donations are similar in both treatments when zero is used as alternative income. This is reminiscent of the concept of a need threshold, which would arguably not be met with zero income. Once the alternative income reaches 20 points, donations drop sharply, and from this point on, *Exclusion* is associated with significantly higher donations than *No Exclusion*. This could imply that 20 points is already the acceptable need threshold, above which other distributive justice principles determine donations. Obviously, additional treatments to this experimental design would be required to assess whether and how exactly need and responsibility interact in the context of discriminatory exclusion. As described in Section 2.2.2, the results of previous studies on the interaction of need and responsibility are inconsistent, but on the whole they find that responsibility for neediness significantly reduces compensation for it, see, e.g., Konow (2001), Faravelli (2007), and Bauer et al. (2022). However, this is not the case for the results of this experiment, so that they are most consistent with the results of Gaertner and Schwettmann (2007) and Buitrago et al. (2009), which also show no influence of responsibility for neediness on redistribution.

In addition, this study contributes to the mixed results on the importance of socio-demographic factors (see Section 2.2.4): in this specific student sample, donations are surprisingly little explained by socio-demographic factors. Only gender and being an economics student show some significant effects. In the case of being an economics student, the effect is negative in all regression types, which could be expected and is consistent with most of the existing literature (see Section 2.2.4). However, as far as gender is concerned, the direction of the effect is somewhat surprising: being a woman has a highly significant and negative effect on donation levels. This is in contrast to much of the existing literature, which finds that women donate more than men. While there are many studies that find no gender effect, they rarely find a negative one (see also 2.2.4). Nonetheless, this finding should not be overstated, because although women clearly donate less than men in this experiment, they tend to donate somewhat more often: being female usually has no significant effect on the probability of donation, but in the few regressions where it does, it is slightly positive. This mirrors to some extent the findings of Andreoni and Vesterlund (2001) and Maaser et al. (2022): Andreoni and Vesterlund (2001, p.

301) show for a modified dictator game that men are more likely to be selfish and donate nothing, but that those men who do donate are more likely to be completely altruistic and donate higher amounts than women. Similarly, Maaser et al. (2022) show that women are more inclusive, but donate less overall. The inverse direction of these two effects may also explain the absence of any gender effects in the regressions for total donations in this experiment.

Furthermore, there is a significant increase in dictators' generosity associated with their preference for the strategic game, i.e. considering it the easiest game, which may also be interpreted as a type of risk preference. Indeed, it appears that self-reported risk preferences influence the willingness to donate, with inverse directions for donation probability and amount. Unfortunately, the present data do not provide further insight into whether preference for a particular production game can be explained by an individual's risk preferences or how exactly these risk preferences interact with distributional decisions. These questions could therefore be the subject of future research, possibly in an impartial observer design, to eliminate the influence of self-interest and thus potentially increase the donation differences between production games.

Some other results of this experiment cannot be interpreted conclusively, as they are probably artifacts of this experiment or result from too imprecise formulations in the questionnaire, such as the increase in donations when the experimental rules are rated as fair. Also, the participation behavior of the recipients is too uninformative to allow conclusions about the preference for different production games. Since participation behavior by production mode suggests some influence of the possibility of receiving donations, a possible extension of this study would be to examine the unbiased participation behavior of production players when dictators are completely removed from the design. However, the recipients' questionnaire revealed that they correctly expected very low dictator donations. Therefore, it seems questionable whether their participation behavior would differ much from that found in this thesis in the absence of dictators. Instead, it seems more useful to investigate what fundamental differences exist between production games and influence both participation and donation decisions: Is it really risk perception that is influenced by agency and control over outcomes, or do other factors such as cost of effort or the like play a more important role?

Finally, a methodological note: the presented results support the finding of Engel (2011, p. 603) that Tobit-regressions tend to inflate the coefficient sizes compared to OLS-regressions. In addition, they show that some variables have effects only on either donation probability or donation level. Since Engel (ibid., p. 603) also finds that the hurdle model outperforms OLS-regressions in analyzing dictator games, it seems reasonable to generally include both extensive and intensive margin in the analysis of these. This can be done either through separate regressions as in this study, or through a hurdle model, depending on the research focus.

6 Concluding Remarks

The aim of this dissertation has been to empirically examine the social evaluation of discriminatory exclusion by means of a laboratory experiment. An extensive literature has already confirmed that the more observable forms of discrimination, such as unequal pay for equal work, are perceived as “unfair” discrimination and are compensated. Yet the common scenario of complete exclusion from income generating activities has been largely neglected in the behavioral and experimental economics literature. This thesis therefore has aimed to establish whether compensation for exclusion, as guaranteed by §15 AGG, is consistent with the norms of laypersons and supported by their everyday judgments, and thus experimentally examines the social evaluation of discriminatory exclusion from income generating activities.

Discriminatory exclusion refers to a situation in which individuals for reasons beyond their control cannot participate in income-generating production although they would like to participate. Such a situation means that individuals, through no fault of their own, are deprived of the opportunity to earn an income. This violates the principle of EOP, one of the important normative principles of distributive justice presented in Chapter 2.

Distributive justice principles differ widely in their implications for income (re-) distribution: While classical utilitarianism is only interested in maximizing total welfare, regardless of its distribution, egalitarianism based on Rawls sees equal distribution of resources as the ideal and justifies redistribution according to need. The desert principle, on the other hand, postulates that a just distribution should be solely proportional to the desert of individuals, which is usually interpreted as effort. The classical libertarian principle of Nozick even rejects any institutionalized redistribution, even in cases where individuals do not have equal initial opportunities to earn entitlements. This is in stark contrast to the responsibility principle which argues that individuals should bear the consequences of their voluntary choices, but be compensated for disadvantages they suffer directly from arbitrarily restricted access to earning opportunities.

Section 2.2 shows that the existing experimental economics literature has extensively studied and documented the distributive justice principles of utilitarianism, egalitarianism, need, desert, and libertarianism. In contrast, there are fewer empirical studies on the responsibility principle, if one categorizes the existing studies that focus on unequal wage rates for equal effort as studies of the accountability principle and thus as part of the literature on the desert principle. In particular, the social evaluation of an individual’s complete exclusion from production due to arbitrary discrimination is scarcely explored empirically. Only Akbaş et al. (2019) have a similar approach to this thesis, but their design does not allow to observe whether subjects do not participate because of exclusion or because of their own choices. This distinction between voluntary suspension and discriminatory exclusion, and compensation only for the latter, thus represents this dissertation's main contribution to the current state of research.

Chapter 3 first defines exclusion and then develops a simple formalization of financial compensation for exclusion based on the principle of responsibility. The model takes into account both that excluded individuals may have alternative income opportunities and that, while they are not responsible for their exclusion, they are responsible for their initial decision to participate in different types of production with different levels of control and hence risk properties.

The validity of the model is tested with a laboratory experiment, the design of which is described in Chapter 4. Compensation for exclusion is captured by the treatment effect of dictator giving in a strategic dictator game with a between-subjects variation of one treatment with exclusion, and another without. Exclusion is modeled by first letting the recipients decide whether they participate in or suspend production in three different, income-generating production games with varying levels of control over outcomes, i.e. a lottery, a real-effort contest and a strategic game. After the participation decision, some of the recipients are suspended from the games, irrespective of their decision to participate therein. The involuntary suspension is based on an arbitrary criterion and cannot be influenced by the recipients. In case of suspension of production, irrespective whether due to own choice or due to exclusion, recipients receive a safe amount which varies over the rounds of a game. Depending on the treatment, dictators strategically decide on donations to those recipients who are excluded or to those who voluntarily suspend production. Accordingly, the treatment difference in dictator giving can be interpreted as compensation for exclusion.

The design allows to test four hypotheses on dictator giving: The main Hypothesis 1 posits that exclusion is compensated, and hence that donations are higher for excluded recipients than for recipients who voluntarily suspend production. Hypothesis 2 posits that donations increase in the level of perceived control over a game's outcomes, while Hypothesis 3 states that with decreasing opportunity costs of suspension/exclusion due to increasing amounts of the alternative safe amount, donations decline as well. Finally, Hypothesis 4 posits the existence of a discrimination premium, i.e. that compensation for exclusion includes a component independent of financial disadvantages.

Chapter 5 presents the results of the laboratory experiment and clearly finds support for the first hypothesis: discriminatory exclusion is compensated, albeit only at the extensive, not the intensive margin. Exclusion from production leads to significantly more frequent donations than the voluntary suspension of production. Donation levels, i.e. the respective amounts of given donations, in case of exclusion are however not higher than in case of voluntary suspension of production.

These results empirically confirm that exclusion is negatively evaluated by society and that a lack of equal income opportunities is compensated for. This is in line with the responsibility principle and links this dissertation to the few other experimental studies which examine the application of the responsibility principle and the results of which also support, at least to some extent, that equal opportunities and responsibility matter for individuals' decisions, especially Karni et al. (2008), Mollerstrom et al. (2015), Akbaş et al. (2019), and Cappelen et al. (2020).

Besides showing that the responsibility principle is applied in the everyday decisions of laypersons, the data also confirms that selfishness is another major distributional motive. This is in line with the results of several previous experimental studies by, e.g., Andreoni and Miller (2002), Bolton and Ockenfels (2006), Fisman et al. (2007), Durante et al. (2014), and Tepe et al. (2021).

Furthermore, the results suggest that the principle of need also plays a role and may even take precedence over the responsibility principle in the absence of alternative income opportunities. While this is in line with the results of Gaertner and Schwettmann (2007) as well as Buitrago et al. (2009), who find no influence of responsibility for neediness on redistribution, it is opposed to the results by Konow (2001), Faravelli (2007), and Bauer et al. (2022), whose findings show that responsibility for neediness reduces compensation for it. Although still inconclusive in the experimental literature, the precedence of needs over exclusion as suggested by the data of this experiment is indeed an existing feature of welfare states. These aim to ensure the existential minimum and, often, a minimum standard of living. Above these thresholds, benefits are conditioned on their citizens behavior, i.e., previous work effort or the ongoing search and application for a new job. This similarity supports the external validity of the experimental results. That compensation only takes place on the extensive, but not intensive margin is however less observable in "real life" situations. Future experimental research should thus further investigate the relationship of exclusion, responsibility and need to see how the distributive justice principles of responsibility and need interact or dominate each other.

The findings on how a game's level of control influences giving are less clear and not fully in line with Hypothesis 2, but show that recipients' production choices matter for dictators' giving decisions. While the production choices do not affect compensation for exclusion at the extensive margin, they do affect donation levels, as dictators donate lower (higher) amounts if recipients choose to participate in (suspend) a production game with comparatively low control over outcomes. This links this thesis to earlier studies on the responsibility principle, especially Cappelen et al. (2013a) and Mollerstrom et al. (2015), who find that compensation is conditional to unrelated, voluntary choices. It remains unclear, however, whether this effect is only due to differences in the games' levels of control, so that the lower expected values of production with lower agency are factored into the donation amount given, or whether it is also based on other aspects that affect the attractiveness of a production game, such as the effort involved. To determine the relationship between production game characteristics and compensation for exclusion, more data from further experimental studies are needed to establish how levels of control and effort costs can be measured and how they interact.

Besides the main finding that exclusion is compensated, Hypotheses 3 and 4 are also supported by the results: As expected, compensation for exclusion decreases as a recipient's alternative income increases, and thus her financial disadvantage from exclusion decreases. Furthermore, there is some weak evidence for a discrimination premium at the extensive margin, i.e. that discriminatory exclusion is compensated by more frequent donations even in situations where recipients are no longer financially disadvantaged by exclusion because of their high alternative income. This highlights that EOP is indeed strongly evaluated by society, as its violation is compensated even in absence of negative financial consequences. Further research, ideally with more statistical power, is needed to examine such a discrimination premium which compensates discriminatory exclusion irrespective of financial disadvantage in more detail.

Earlier experimental studies have shown that dictator giving is often influenced by individual characteristics such as socio-demographic variables, as shown in Section 2.2.4. This is however only partially reproduced in the results of this thesis. Here, the only socio-demographic variables which have at least some influence on donations, though only at the intensive margin, are being an economics student and gender. The negative effect of being an economics student on donations is in line with the findings of the experimental studies by, e.g., Marwell and Ames (1981), Carter and Irons (1991), and Cappelen et al. (2015). For gender, this thesis' results show that women have a higher likelihood to donate anything than men, but are less generous with respect to their chosen donation amount. This is similar to the findings by Andreoni and Vesterlund (2001) and Maaser et al. (2022) and thus supports the validity of these findings on gender effects which are in the experimental literature still mostly inconclusive.

The main contribution of this dissertation is the empirical confirmation that discriminatory exclusion which violates EOP is negatively evaluated by society, and that a lack of equal income opportunities is compensated in line with the responsibility principle: if an individual wants to work, but is excluded from production due to discrimination, compensation as outlined by §15 AGG is supported by the everyday judgments and giving decisions of participants.

In fact, the results show that discriminatory exclusion is compensated with a discrimination premium even if no financial disadvantage arises therefrom. This finding suggests that the violation of EOP due to exclusion has a negative value on its own, and not only due to its financial consequences. It is the first time that this notion is supported by experimental evidence, which confirms the initial statement by Roemer, that EOP is "[p]robably the most universally supported conception of justice in advanced societies" (Roemer 2002, p. 455).

That the compensation for exclusion by participants is compatible with compensation as outlined by §15 AGG is not the only similarity of the experimental results with features of existing welfare states, as the data also show that absolute need, as in this case an income of zero, leads to financial support irrespective of whether an individual is responsible for this situation or not. This suggests that laypersons do not interpret the responsibility principle in a narrow and possibly cruel way, but instead also consider the principle of need in their decisions: First, a certain threshold should be ensured for all members of a society, and only above that threshold are benefits determined by individuals' choices and their consequences. The combination of fairness principles is, as mentioned above, reflected in existing welfare states and highlights the importance of a combination of different fairness principles which are applied context-dependent. Responsibility and facing the consequences of one's own choices are important to provide members of society with incentives to appropriately consider risks and benefits in their decisions. Still, once an individual's very existence is threatened, even if due to own choices, the principle of need takes precedence.

Unfortunately, in "real life", it is more difficult to distinguish whether someone is responsible for a situation or not than in this experiment. From the outside it is all but impossible to observe whether someone is willing to work in a certain position but is not hired due to discrimination, which violates the principle of EOP, or whether she is performing worse in respective interviews due to being less qualified or due to in fact not being willing to work, in which case the principle of EOP would not be violated. Therefore, while the compensation of exclusion and its "just" amount can be codified and reliably paid by an impartial institution, the main challenge consists in identifying the eligible recipients.

Furthermore, discriminatory exclusion can have persistent negative effects on future chances of income, especially if individuals are excluded from education or positions which enable certain career paths. The correct estimation and compensation of all the negative consequences of exclusion is already theoretically challenging, hence the responsibility principle considers only the direct financial consequences of an act of exclusion. Nevertheless, especially in terms of access to education or entry positions to lucrative career paths, such a compensation of direct effects only might not be sufficient. Think, e.g., of internship positions: exclusion from such a no- or low-income position has only negligible direct financial consequences, but can have substantial consequences on an individual's career and income over time. Still, compensation covering all consequences of exclusion from a hypothetical career path is obviously impossible.

Thus, while instruments such as compensation as outlined by §15 AGG are important and valuable, the major challenge remains the elimination of discrimination from society. This is obviously easier said than done, as some factors, such as the mindset of discriminating people or established hierarchies, seem difficult to be changed in a short or medium time. Still, other approaches seem more promising, such as improving certain processes so that they are less vulnerable to individual biases and arbitrary decisions. Examples are the already established practice to have multiple people present during job interviews, and the provisioning of guidelines against discriminatory decision patterns in these interviews. While none of these solutions perfectly prevent discrimination and discriminatory exclusion, they are a step in the right direction to enable EOP for all individuals, so that compensation for exclusion is not needed in the first place.

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Appendices

A Instructions

A.1 Instructions for Dictators

A.1.1 Instructions for the EET

Original Instructions for the EET in German

Instruktionen, Teil 1:

Allgemeine Erklärungen Willkommen zum Experiment. Wenn Sie die Instruktionen aufmerksam lesen und alle Regeln beachten, können Sie in diesem Experiment Geld verdienen. Während des Experimentes sprechen wir nicht von Euro, sondern von Punkten. Diese werden gemäß folgendem Wechselkurs umgerechnet:

100 Punkte = 5 Euro.

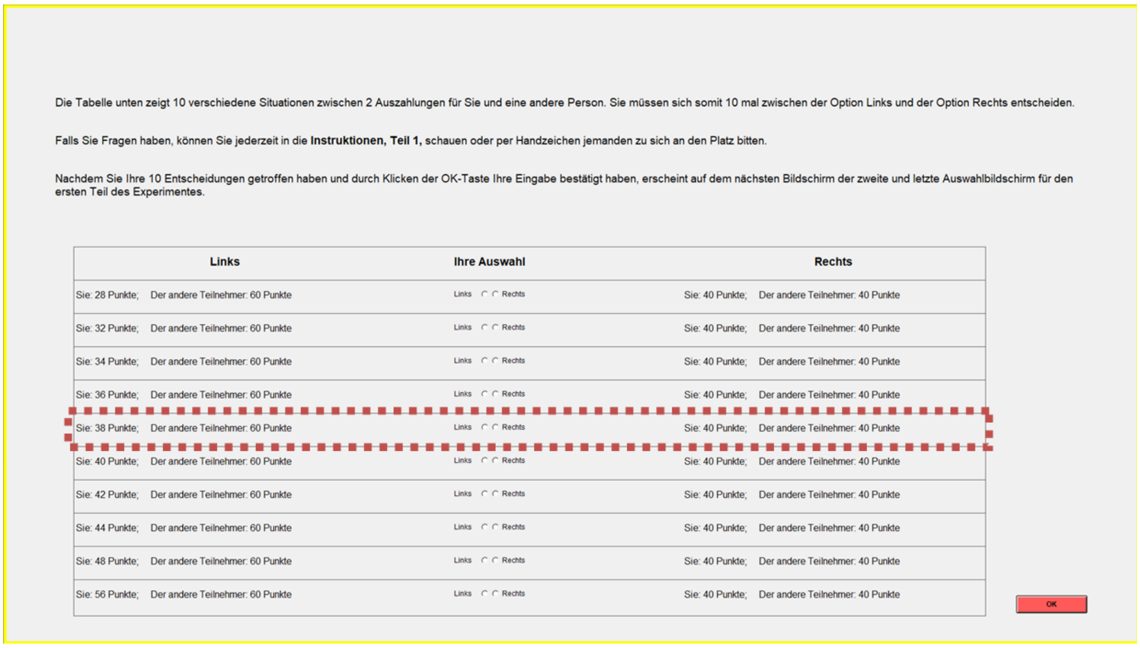
Während des gesamten Experimentes ist das Sprechen mit anderen Teilnehmern nicht erlaubt. Wenn Sie Fragen haben, richten Sie diese bitte ausschließlich an uns. Wir beantworten Ihre Fragen gerne individuell. Die Einhaltung dieser Regel ist sehr wichtig. Andernfalls sind die Ergebnisse dieses Experimentes wissenschaftlich wertlos.

Dieses Experiment besteht aus **2 Teilen**, wobei jeder Teil nacheinander einzeln erläutert wird. In jedem der 2 Teile des Experimentes können Sie Geld verdienen. Die Summe Ihrer Auszahlungen erhalten Sie erst zeitversetzt innerhalb der nächsten 30 Tage. Nähere Informationen hierzu folgen im weiteren Verlauf des Experimentes. Das komplette Experiment wird voraussichtlich 60 Minuten dauern. Für bessere Verständlichkeit wird im gesamten Experiment das generische Maskulinum verwendet. Im Folgenden wird Ihnen nun der erste Teil erläutert.

Detaillierte Informationen zu Teil 1 des Experimentes
Im ersten Teil des Experimentes möchten wir Sie bitten, 20 Entscheidungen zu treffen, die in zwei 10er-Blöcken präsentiert werden. In jeder dieser 20 Entscheidungen bilden Sie mit einem „anderen Teilnehmer“ eine 2er-Gruppe. Der „andere Teilnehmer“ bleibt wie Sie auch anonym und wird in jeder der 20 Entscheidungen zufällig neu ausgewählt. In den Entscheidungen müssen Sie sich immer zwischen Links und Rechts entscheiden, wobei die Optionen Links und Rechts immer mit einer Auszahlung für Sie selbst und einer Auszahlung für den „anderen Teilnehmer“ verbunden sind. Eine dieser zwanzig Entscheidungen wird zufällig als auszahlungsrel-

evant ausgewählt. Dies ist beispielhaft in Abbildung 1 dargestellt.

Abbildung 1: Entscheidungsbildschirm für die Wahl unterschiedlicher Verteilungen



Die Tabelle unten zeigt 10 verschiedene Situationen zwischen 2 Auszahlungen für Sie und eine andere Person. Sie müssen sich somit 10 mal zwischen der Option Links und der Option Rechts entscheiden.

Falls Sie Fragen haben, können Sie jederzeit in die **Instruktionen, Teil 1,** schauen oder per Handzeichen jemanden zu sich an den Platz bitten.

Nachdem Sie Ihre 10 Entscheidungen getroffen haben und durch Klicken der OK-Taste Ihre Eingabe bestätigt haben, erscheint auf dem nächsten Bildschirm der zweite und letzte Auswahlbildschirm für den ersten Teil des Experimentes.

Links	Ihre Auswahl	Rechts
Sie: 28 Punkte; Der andere Teilnehmer: 60 Punkte	Links ◦ ◦ Rechts	Sie: 40 Punkte; Der andere Teilnehmer: 40 Punkte
Sie: 32 Punkte; Der andere Teilnehmer: 60 Punkte	Links ◦ ◦ Rechts	Sie: 40 Punkte; Der andere Teilnehmer: 40 Punkte
Sie: 34 Punkte; Der andere Teilnehmer: 60 Punkte	Links ◦ ◦ Rechts	Sie: 40 Punkte; Der andere Teilnehmer: 40 Punkte
Sie: 36 Punkte; Der andere Teilnehmer: 60 Punkte	Links ◦ ◦ Rechts	Sie: 40 Punkte; Der andere Teilnehmer: 40 Punkte
Sie: 38 Punkte; Der andere Teilnehmer: 60 Punkte	Links ◦ ◦ Rechts	Sie: 40 Punkte; Der andere Teilnehmer: 40 Punkte
Sie: 40 Punkte; Der andere Teilnehmer: 60 Punkte	Links ◦ ◦ Rechts	Sie: 40 Punkte; Der andere Teilnehmer: 40 Punkte
Sie: 42 Punkte; Der andere Teilnehmer: 60 Punkte	Links ◦ ◦ Rechts	Sie: 40 Punkte; Der andere Teilnehmer: 40 Punkte
Sie: 44 Punkte; Der andere Teilnehmer: 60 Punkte	Links ◦ ◦ Rechts	Sie: 40 Punkte; Der andere Teilnehmer: 40 Punkte
Sie: 48 Punkte; Der andere Teilnehmer: 60 Punkte	Links ◦ ◦ Rechts	Sie: 40 Punkte; Der andere Teilnehmer: 40 Punkte
Sie: 56 Punkte; Der andere Teilnehmer: 60 Punkte	Links ◦ ◦ Rechts	Sie: 40 Punkte; Der andere Teilnehmer: 40 Punkte

OK

Beispiel: Die Option Links in der 5. Zeile lautet: Sie 38 Punkte, „Der andere Teilnehmer“ 60 Punkte. Die Option Rechts in der 5. Zeile lautet: Sie 40 Punkte, „Der andere Teilnehmer“ 40 Punkte. Das bedeutet, dass wenn Sie in der fünften Zeile z.B. Links wählen und diese Situation dann zufällig als auszahlungsrelevant ausgewählt wird, Sie eine Auszahlung von 38 Punkten bekommen und der „andere Teilnehmer“ eine Auszahlung von 60 Punkten erhält.

Wir bitten Sie im Folgenden, für jede der 20 Situationen zeilenweise die Optionen Links und Rechts zu vergleichen und sich durch Klicken von Links oder Rechts für eine der Optionen zu entscheiden.

Berechnung Ihrer Auszahlung in Teil 1

Ihre Auszahlung aus diesem Teil des Experimentes ergibt sich aus zwei Teilauszahlungen.

Auszahlung als „aktiver Entscheider“:

Am Ende des ersten Teils des Experimentes wird eine Ihrer 20 Entscheidungen zufällig als auszahlungsrelevant ausgewählt. Ihre darin gewählte Links-Rechts-Auswahl bestimmt somit die Auszahlung für Sie selbst und für den „anderen Teilnehmer“, der Ihnen für diese Entscheidung zugeordnet wurde. Im Beispiel unter der Abbildung 1 würden Sie demnach nach Ihrer Wahl von Links 38 Punkte erhalten und der andere Teilnehmer 60.

Auszahlung als „anderer Teilnehmer“:
Nach der gleichen Prozedur legt einer Ihrer Mitspieler im Experiment durch seine zufällig als auszahlungsrelevant bestimmte Links-Rechts-Entscheidung fest, wie hoch Ihre Auszahlung als zufällig zugeordneter „anderer Teilnehmer“ ist, ohne dass Sie selbst darauf Einfluss nehmen können. Es ist allerdings sichergestellt, dass keine Entscheidungssituation entsteht, in der Sie und ein anderer Mitspieler im Experiment wechselseitig „aktiver Entscheider“ und „anderer Teilnehmer“ sind. Ihre Gesamtauszahlung aus dem ersten Teil des Experimentes ergibt sich durch Addition der Auszahlung als „aktiver Entscheider“ und „anderer Teilnehmer“.

Falls Sie nun noch Fragen haben, heben Sie bitte die Hand aus der Kabine. Es wird dann jemand zu Ihnen kommen, um Ihre Frage zu beantworten. Wenn Sie keine weiteren Fragen haben, können Sie nun die Auswahl der verschiedenen Optionen Links und Rechts am Bildschirm treffen.

Translated Instructions for the EET

Instructions, Part 1:

Instructions, Part 1:
General Explanation
Welcome to the experiment. If you read the instructions carefully and follow all the rules, you can earn money in this experiment. During the experiment we do not speak of Euros, but of points. These are converted according to the following exchange rate:

100 points = 5 euros.

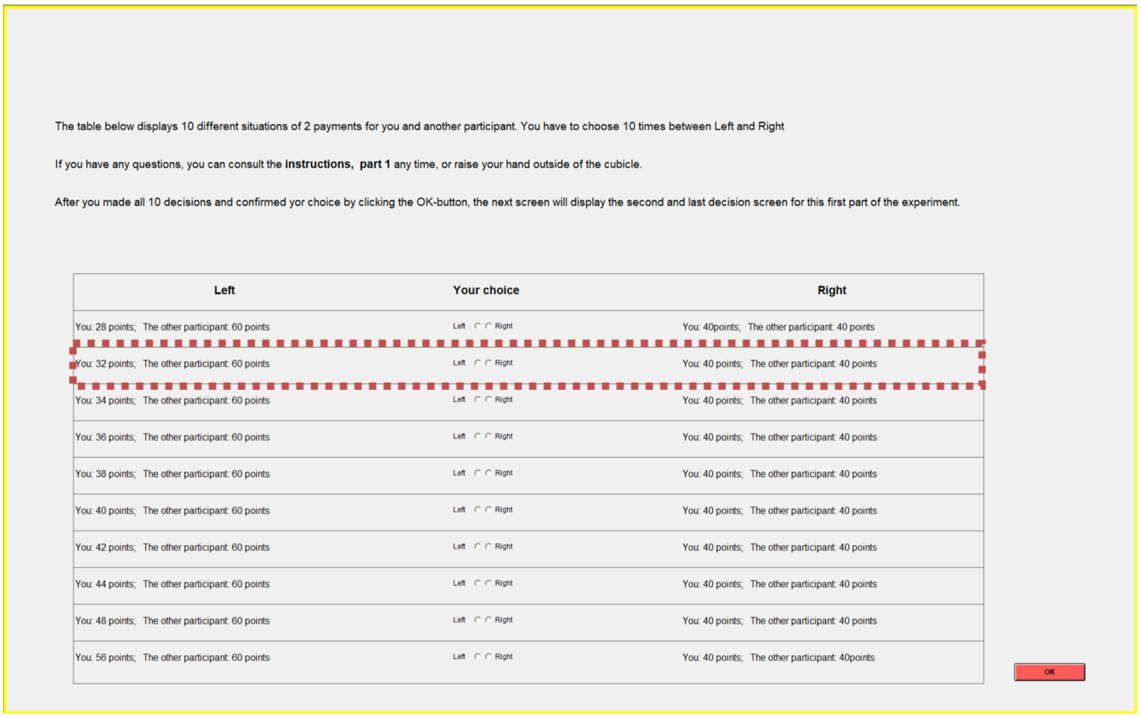
Talking to other participants is not allowed during the entire experiment. If you have any questions, please address them to us only. We are happy to answer your questions individually. Compliance with this rule is very important. Otherwise, the results of this experiment are scientifically worthless.

This experiment consists of 2 parts, each part being explained one after the other. You can make money in each of the 2 parts of the experiment. You will receive your total payment only with a time delay within the next 30 days. Further information on this will be provided throughout the experiment. The complete experiment is expected to take 60 minutes. The generic masculine is used throughout the experiment for better understanding. The first part is explained below.

Detailed Information on Part 1 of the Experiment
In the first part of the experiment we ask you to make 20 decisions, which are presented in two blocks of 10. In each of these 20 decisions you will form a two-person group with an “other participant”. Like you, “other participant” remains anonymous

and is randomly selected for each of the 20 decisions. For each of the decisions you will have to choose between left and right; each option left and right is linked to a payment for yourself and for the "other participant". One of these twenty decisions will be randomly selected as relevant to your payoff. This is shown for example in Figure 1.

Figure 1: Decision-Making Screen for the Choice of Varying Distributions



Example: The Left option in the 5th line reads: You 38 points, "the other participant" 60 points. The Right option in the 5th line reads: You 40 points, "the other participant" 40 points. That means that if you choose e.g. Left in the 5th line, then you will receive a payment of 38 points and the "other participant" will receive a payment of 60 points.

In the following we ask you to compare the options Left and Right for each line and to make your decision by clicking one of the options Left or Right.

Calculation of the Payment in Part 1

Your payoff for this part of the experiment is the result of two partial payments.

Payment as "Active Decision Maker":

At the end of the first part of the experiment one of your 20 decisions will be randomly selected as relevant for your payment. Hereby your choice Left or Right will determine the payment for you and for the "other participant" who was assigned to you for this decision. For the example in Figure 1 that means that, for your choice of Left, you receive 38 points, and the "other participant" receives 60.

Payment as "Other Participant":
Through the same procedure, by choosing Left or Right one of your teammates will determine how high your own payoff as the randomly assigned "other participant" will be, whereby you yourself will have no influence on this determination. However, it is ensured that no decision situation arises in which you and another player in the experiment reciprocate each other as "active decision maker" and "other participant". Your total payoff for the first part of this experiment will be the sum of your payments as "active decision maker" and "other participant".

If you now have any questions, please raise your hand outside of the cubicle. Someone will then come to you in order to answer your question. If you don't have any further questions, then you can now start making choices on your screen.

A.1.2 Instructions for Dictators' Baseline Treatment

Original Instructions for Dictators' Baseline Treatment in German

Instruktionen, Teil 2:
Der erste Teil des Experimentes ist nun zu Ende. Der folgende zweite Teil ist davon unabhängig und hat keine Auswirkung auf Ihre Auszahlung aus Teil 1 des Experimentes. In Teil 2 bestimmen Sie die endgültige Auszahlung eines Ihnen zugeordneten Teilnehmers (im Folgenden „Empfänger" genannt). Ihr Empfänger kann in einem nachfolgenden Experiment an mehreren Runden drei verschiedener Aufgaben teilnehmen. Sie selbst werden an diesem nachfolgenden Experiment nicht teilnehmen. Der Wechselkurs für diesen Teil des Experimentes (sowohl für Sie als auch für Ihren Empfänger) ist identisch zu Teil 1 und liegt somit weiterhin bei 100 Punkte = 5 Euro.

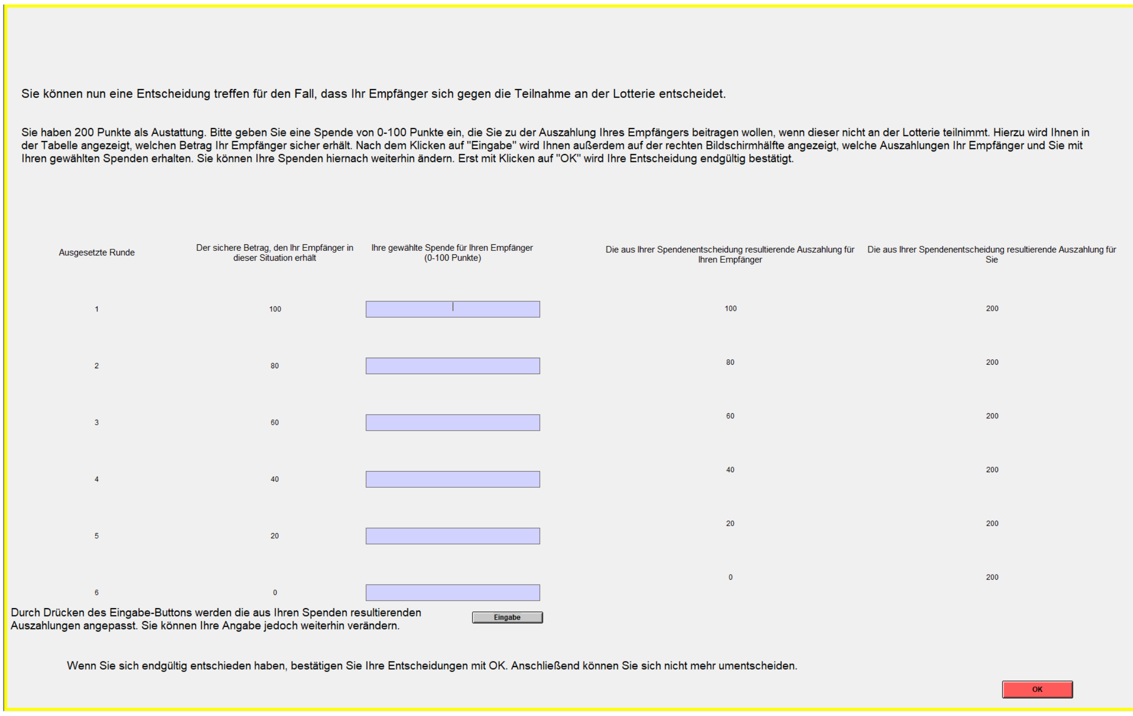
Ihre Aufgabe in diesem Teil des Experimentes
Sie erhalten, wie jeder andere Teilnehmer an diesem Experiment, 200 Punkte als Ausstattung und Ihnen wird genau ein Empfänger aus dem Nachfolgeexperiment zufällig zugeordnet. Ihrem Empfänger können Sie von Ihren 200 Punkten 0 bis 100 Punkte spenden. Sie treffen Ihre Entscheidung über Ihre Spende an Ihren Empfänger für den Fall, dass Ihr Empfänger sich gegen die Teilnahme an einer bestimmten Aufgabe im Nachfolgeexperiment entschlossen hat. Für den Fall dieses Aussetzens bekommt Ihr Empfänger – je nachdem in welcher Runde der Aufgabe er ist – eine bestimmte Anzahl von Punkten als sicheren Betrag zugewiesen. Dies wird Ihnen im Folgenden detailliert erläutert. Sie entscheiden strategisch, d.h. im Voraus für alle möglichen Aufgaben und Runden bzw. sicheren Beträge Ihres Empfängers, weil Sie

die Ergebnisse des Nachfolgeexperimentes ja noch nicht kennen.

Festlegen der Auszahlung aus Teil 2

Am Ende Ihres Experimentes wird eine einzige Ihrer Auszahlungsentscheidungen zufällig für Sie und Ihren Empfänger als auszahlungsrelevant festgelegt. Falls Ihr Empfänger in der auszahlungsrelevanten Runde aussetzt und dafür einen sicheren Betrag erhält, wird ihm zusätzlich die von Ihnen gewählte Spende ausgezahlt. In diesem Fall reduziert sich Ihre Ausstattung von 200 Punkten um die von Ihnen gewählte Spende. Falls Ihr Empfänger an der auszahlungsrelevanten Runde teilnimmt, bestimmt sich seine Auszahlung nur über seine Entscheidungen und Ergebnisse im Spiel und Sie behalten Ihre volle Ausstattung von 200 Punkten. Ihre Spendenentscheidung und die Berechnung der Auszahlung werden in Abbildung 2 am Beispiel von 20 Punkten als sicherer Betrag erklärt.

Abbildung 2: Spendenentscheidung für einen sicheren Betrag von 20 Punkten



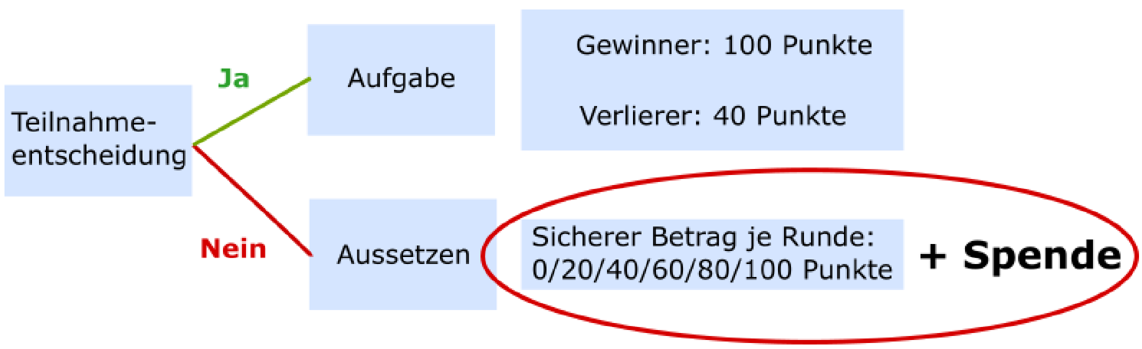
Beispiel 2: Auf der linken Seite des Bildschirms stehen die sicheren Beträge, die Ihr Empfänger pro Runde erhält, wenn er freiwillig aussetzt. Daneben können Sie eine Spende von 0-100 Punkten eingeben, die Sie in der jeweiligen Runde zur Auszahlung Ihres Empfängers beitragen wollen. Angenommen, Ihr Empfänger erhält 20 Punkte als sicheren Betrag und Sie geben einen Betrag von 30 Punkten ein, so erhalten Sie entweder:

- 200-30=170 Punkte als Auszahlung, wenn Ihr Empfänger in dieser Runde aussetzt und den sicheren Betrag erhält. Ihr Empfänger erhält in diesem Fall eine Auszahlung von 20+30=50.

- 200 Punkte als Auszahlung, wenn Ihr Empfänger in dieser Runde an der Aufgabe teilnimmt. In diesem Fall hängt die Auszahlung für Ihren Empfänger alleine von seiner Auswahl und seinen Ergebnissen ab.

Weitere Informationen zur Auszahlung aus Teil 2
Da Ihre Auszahlung vom Teilnahmeverhalten Ihres Empfängers abhängt, erfolgt diese erst nach Ende des nachfolgenden Experimentes. Das bedeutet, dass Sie sofort am Ende dieses Experimentes lediglich Ihre Auszahlung für Teil 1 erhalten. Des Weiteren erhalten Sie in den nächsten 30 Tagen, nach Beendigung des nachfolgenden Experimentes, eine E-Mail, in der Sie informiert werden, wann Sie Ihre Auszahlung abholen können. Bei Abholung der Auszahlung werden Sie informiert, welche Runde zufällig als auszahlungsrelevant festgelegt wurde und ob Ihr Empfänger in dieser Runde ausgesetzt hat und somit Ihre Spende als Teil seiner Auszahlung erhält. In diesem Fall erhalten Sie Ihre Ausstattung minus Ihre Spende. Hat Ihr Empfänger an dieser Runde teilgenommen, erhalten Sie Ihre gesamte Ausstattung als Auszahlung. Die genauen Informationen zur Abholung des Geldes finden Sie dann in der E-Mail.

Aufgabe Ihres Empfängers
Im Nachfolgeexperiment spielt Ihr Empfänger drei verschiedene Aufgaben (Wettbewerb, Lotterie und Strategiespiel) in jeweils sechs Runden gegen einen wechselnden anderen Spieler. Der Gewinner jeder Aufgabenrunde erhält 100 Punkte, der Verlierer erhält 40 Punkte. Ihr Empfänger hat jedoch für jede einzelne Runde einer Aufgabe die Wahlmöglichkeit, nicht zu spielen, sondern stattdessen auszusetzen und einen sicheren Betrag zu erhalten. Die Höhe des sicheren Betrags beträgt in den 6 Runden jeder Aufgabe 0, 20, 40, 60, 80 und 100 Punkte.



Spielregeln für Ihren Empfänger
Damit Sie Ihre Entscheidung optimal treffen können, erhalten Sie hier die Regeln der drei Aufgaben, die Ihr Empfänger gegen wechselnde Gegenspieler spielen kann. Diese werden Ihnen vor jeder Beitragsentscheidung auf den Bildschirmen nochmals erläutert und mit einer kurzen Proberunde für die jeweilige Aufgabe verdeutlicht.

Lotterie

Die Spieler bekommen vom Computer zufällig die Positionen Spieler 1 und Spieler 2 zugewiesen und haben hierbei die gleiche Wahrscheinlichkeit (50%) für die Position Spieler 1. Spieler 1 kann zuerst ein Los A oder B wählen, Spieler 2 erhält das verbliebene Los. In 50% der Fälle führt Los **A** zu 100 Punkten und Los **B** zu 40 Punkten. In den anderen 50% der Fälle führt Los **A** zu 40 Punkten und Los **B** zu 100 Punkten.

Wettbewerb

Für die Dauer von 60 Sekunden nutzen die beiden konkurrierenden Spieler eine vorgegebene Zahlen-Buchstaben-Tabelle, um möglichst viele 3-Buchstaben-Kombinationen in die entsprechenden Zahlenkombinationen umzuwandeln. Derjenige, der innerhalb dieser Zeit mehr „Worte" korrekt in Zahlen umwandelt, gewinnt den Wettbewerb und erhält 100 Punkte. Der Andere verliert und erhält 40 Punkte. Bei Gleichstand erhalten beide 100 Punkte.

Strategiespiel

Die Spieler bekommen vom Computer zufällig die Positionen Spieler 1 und Spieler 2 zugewiesen und haben hierbei die gleiche Wahrscheinlichkeit (50%) für die Position Spieler 1. Anschließend besteht die Wahl zwischen zwei Optionen, A und B. Falls beide **verschiedene** Optionen wählen (A, B oder B, A) erhält Spieler **1** 100 Punkte und Spieler **2** erhält 40 Punkte. Falls beide die **gleiche** Option wählen (A, A oder B, B), erhält Spieler **1** 40 Punkte und Spieler **2** erhält 100 Punkte.

Nachdem alle Teilnehmer den zweiten Teil des Experimentes abgeschlossen haben, folgt ein kurzer Fragebogen. Anschließend können Sie das Labor einzeln leise verlassen. Die Informationen zu Ihren Ergebnissen und Ihrer Auszahlung erhalten Sie zeitversetzt. Sobald das nachfolgende Experiment im Laufe der nächsten 30 Tage abgeschlossen ist, werden Sie per E-Mail über die Abholungsformalitäten informiert.

Falls Sie nun noch Fragen haben, heben Sie bitte die Hand aus der Kabine. Es wird dann jemand zu Ihnen kommen, um Ihre Frage zu beantworten. Wenn Sie keine weiteren Fragen haben, können Sie auf OK klicken und mit dem zweiten Teil beginnen.

Vielen Dank für Ihre Teilnahme

Translated Instructions for Dictators' Baseline Treatment

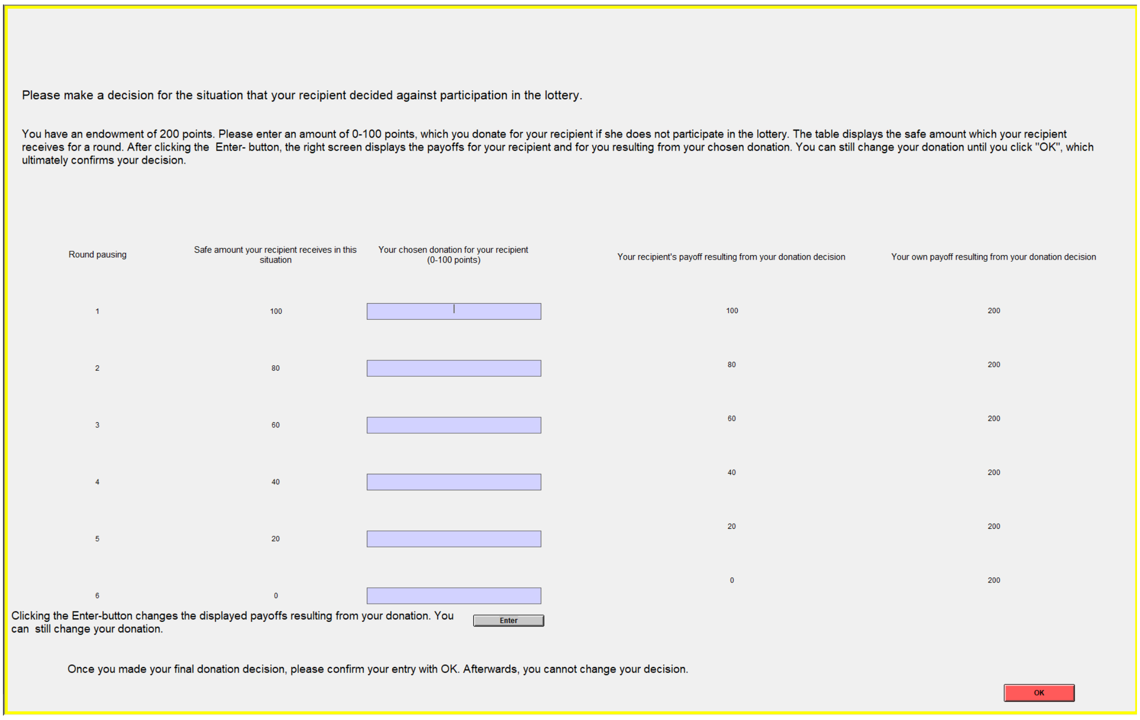
Instructions, Part 2:
The first part of the experiment is now over. The following second part is independent of this and has no effect on your payment from Part 1 of the experiment. In Part 2 you determine the final payment of a participant assigned to you (hereinafter referred to as the "recipient"). Your recipient can participate in several rounds of three different tasks in a subsequent experiment. You yourself will not participate in this subsequent experiment. The exchange rate for this part of the experiment (both for you and for your recipient) is identical to Part 1 and is therefore still 100 points = 5 euros.

Your Task in this Part of the Experiment
Like every other participant in this experiment, you receive 200 points as endowment and exactly one recipient from the follow-up experiment is randomly assigned to you. You can donate 0 to 100 points to your recipient from your 200 points. You make your decision about your donation to your recipient in the event that your recipient has decided against participating in a specific task in the follow-up experiment. In the event of suspension, your recipient – depending on which round of the task he is in – will be assigned a certain number of points as a safe amount. This is explained in detail below. You decide strategically, i.e. in advance of all possible tasks and rounds or ensured amounts of your recipient, because you do not yet know the results of the follow-up experiment.

Determination of the Payoff from Part 2
At the end of your experiment, one of your payment decisions is randomly determined to be relevant for you and your recipient. If your recipient suspends production in the payoff-related round and receives an ensured amount for it, the donation you have selected will also be paid out. In this case, your endowment of 200 points will be reduced by the donation amount you have chosen. If your recipient participates in the payoff-relevant round, their payoff is determined only by their decisions and results in the game, and you keep your full endowment of 200 points. Your donation decision and the calculation of the payment are explained in Figure 2 using the example of 20 points as an ensured amount.

Example 2: On the left side of the screen are the ensured amounts that your recipient receives per round if he voluntarily suspends production. You can also enter a donation of 0-100 points that you want to contribute to the payoff of your recipient in each round. Assuming that your recipient receives 20 points as an ensured amount and you enter 30 points, you will either receive:

Figure 2: Donation Decision for an Ensured Amount of 20 Points



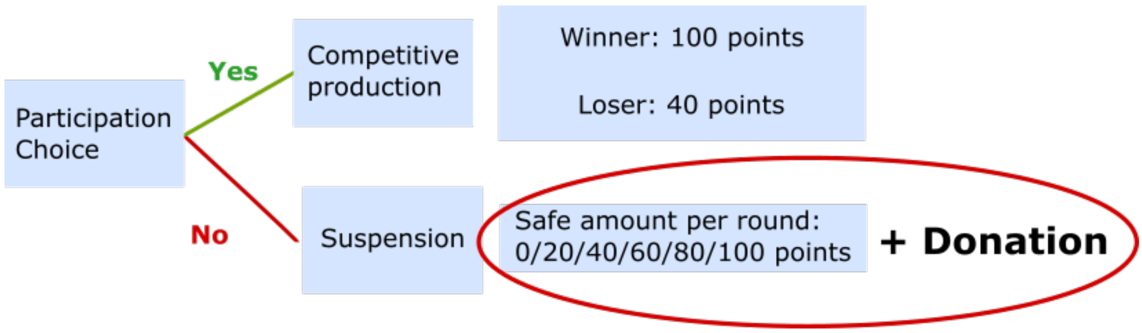
- 200 - 30 = 170 points as a payoff if your receiver suspends this round and receives the safe amount. In this case, your recipient will receive a payment of 20 + 30 = 50.
- 200 points as a payoff if your recipient participates in the task in this round. In this case, the payoff for your recipient depends solely on their selection and results.

More Information about the Payment from Part 2

Since your payment depends on your recipient's participation behavior, this will only take place after the end of the following experiment. This means that within the next 30 days after the end of the experiment that follows, you will receive an email informing you when you can collect your payoff. When you pick up the payoff, you will be informed which round was selected as relevant for the payoff and whether your recipient has suspended this round and thus receives your donation as part of his payoff. In this case, you will receive your endowment minus your donation, plus your payment from Part 1. If your recipient has participated in this round, you will receive all of your endowment plus your result from Part 1 as a payment. You will find the exact information on how to collect the money in the email.

Your Recipient's Task

In the follow-up experiment, your recipient plays three different tasks (competition, lottery and strategic game) in six rounds against a different player. The winner of each task round receives 100 points, the loser 40 points. However, for each round of a task, your recipient can choose not to play, but instead to suspend himself and receive an ensured amount. The amount of the ensured payment is 0, 20, 40, 60, 80 and 100 points in the 6 rounds of each task.



Rules of the Game for Your Recipient

To help you make the best possible decision, here you will find the rules for the three tasks that your recipient can play against changing opponents. These are explained to you on the screens before each decision on contributions and illustrated with a short trial round for the respective task.

Lottery

Players are randomly assigned player positions 1 and 2 by the computer and have the same probability (50%) of position Player 1. Player 1 can choose lot A or B first, Player 2 receives the remaining lot. In 50% of the cases, Lot **A** leads to 100 points and Lot **B** to 40 points. In the other 50% of cases, Lot **A** leads to 40 points and Lot **B** leads to 100 points.

Competition

For a period of 60 seconds, the two competing players use a given number-letter table to convert as many 3-letter combinations as possible into the corresponding number combinations. The one who correctly converts more “words” into numbers within this time wins the competition and receives 100 points. The other loses and receives 40 points. If there is a tie, both score 100 points.

Strategic Game

The players are randomly assigned the positions Player 1 and Player 2 by the computer and have the same probability (50%) for position Player 1. Then there is a choice between two options, A and B. If both choose **different** options (A, B or B, A) Player **1** receives 100 points and player **2** receives 40 points. If both choose the same option (A, A or B, B), Player **1** will get 40 points and Player **2** will get 100 points.

After all participants have completed the second part of the experiment, a short questionnaire will follow. You can then quietly leave the laboratory individually.

You will receive the information on your results and your payment with a time delay. As soon as the subsequent experiment is completed within the next 30 days, you will be informed of the collection formalities by email.

If you still have questions, please raise your hand out of the cabin. Someone will come to you to answer your question. If you have no further questions, you can click OK and start with the second part.
Thank you for your participation

A.1.3 Instructions for Dictators' Exclusion Treatment

Original Instructions for Dictators' Exclusion Treatment in German

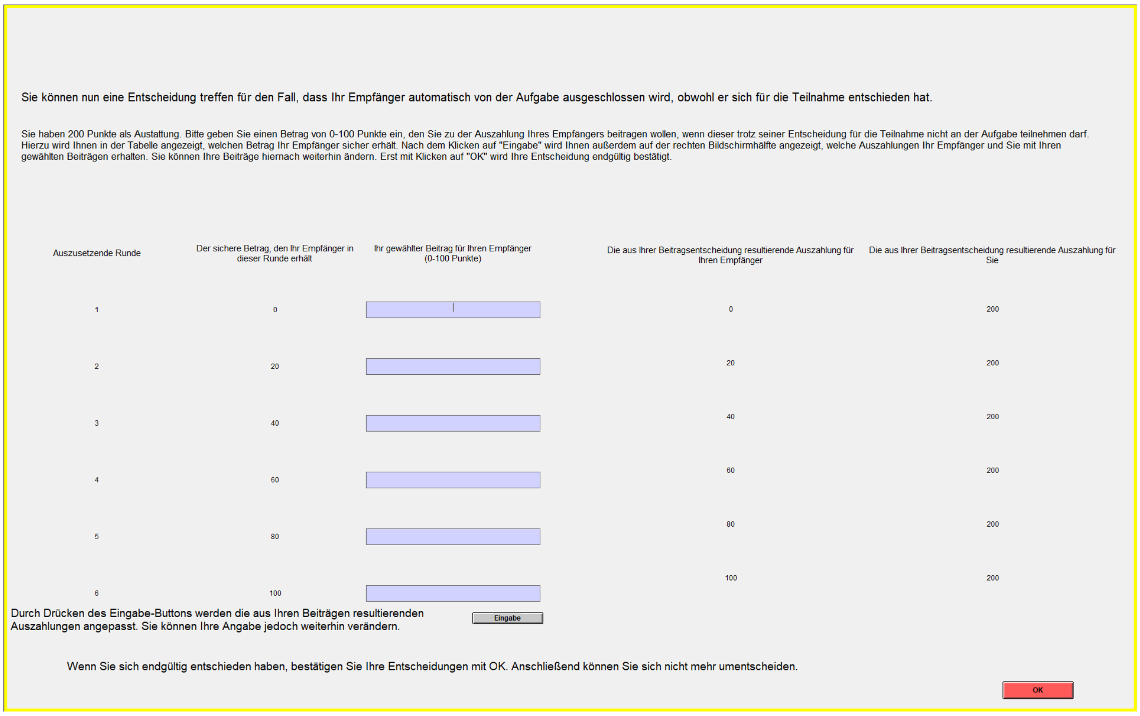
Instruktionen, Teil 2:
Der erste Teil des Experimentes ist nun zu Ende. Der folgende zweite Teil ist davon unabhängig und hat keine Auswirkung auf Ihre Auszahlung aus Teil 1 des Experimentes. In Teil 2 bestimmen Sie die endgültige Auszahlung eines Ihnen zugeordneten Teilnehmers (im Folgenden „Empfänger" genannt). Ihr Empfänger kann in einem nachfolgenden Experiment an mehreren Runden drei verschiedener Aufgaben teilnehmen. Sie selbst werden an diesem nachfolgenden Experiment nicht teilnehmen. Der Wechselkurs für diesen Teil des Experimentes (sowohl für Sie als auch für Ihren Empfänger) ist identisch zu Teil 1 und liegt somit weiterhin bei 100 Punkte = 5 Euro.

Ihre Aufgabe in diesem Teil des Experimentes Sie erhalten, wie jeder andere Teilnehmer an diesem Experiment, 200 Punkte als Ausstattung und Ihnen wird genau ein Empfänger aus dem Nachfolgeexperiment zufällig zugeordnet. Ihrem Empfänger können Sie von Ihren 200 Punkten 0 bis 100 Punkte spenden. Sie treffen Ihre Entscheidung über Ihre Spende an Ihren Empfänger für den Fall, dass zwei Voraussetzungen zutreffen: 1. Ihr Empfänger muss sich zur Teilnahme an einer bestimmten Aufgabe im Nachfolgeexperiment entschlossen haben. 2. Er musste trotz seines Teilnahmewunsches aussetzen. Für den Fall des Aussetzens bekommt Ihr Empfänger – je nachdem in welcher Runde der Aufgabe er ist – eine bestimmte Anzahl von Punkten als sicheren Betrag zugewiesen. Dies wird Ihnen im Folgenden detailliert erläutert. Sie entscheiden strategisch, d.h. im Voraus für alle möglichen Aufgaben und Runden bzw. sicheren Beträge Ihres Empfängers, weil Sie die Ergebnisse des Nachfolgeexperimentes ja noch nicht kennen.

Festlegen der Auszahlung aus Teil 2
Am Ende Ihres Experimentes wird eine einzige Ihrer Auszahlungsentscheidungen zufällig für Sie und Ihren Empfänger als auszahlungsrelevant festgelegt. Falls Ihr Empfänger in der auszahlungsrelevanten Runde teilnehmen wollte, aber aussetzen

musste und dafür einen sicheren Betrag erhält, wird ihm zusätzlich die von Ihnen gewählte Spende ausgezahlt. In diesem Fall reduziert sich Ihre Ausstattung von 200 Punkten um die von Ihnen gewählte Spende. Falls Ihr Empfänger an der Aufgabe teilnehmen durfte, bestimmt sich seine Auszahlung nur über seine Entscheidungen und Ergebnisse im Spiel und Sie behalten Ihre volle Ausstattung von 200 Punkten. Ihre Spendenentscheidung und die Berechnung der Auszahlung werden in Abbildung 2 am Beispiel von 20 Punkten als sicherer Betrag erklärt.

Abbildung 2: Spendenentscheidung für einen sicheren Betrag von 20 Punkten



Beispiel 2: Auf der linken Seite des Bildschirms stehen die sicheren Beträge, die Ihr Empfänger pro Runde erhält, wenn er in dieser Aufgabe automatisch aussetzen muss. Daneben können Sie eine Spende von 0-100 Punkten eingeben, die Sie in der jeweiligen Runde zur Auszahlung Ihres Empfängers beitragen wollen. Angenommen, Ihr Empfänger erhält 20 Punkte als sicheren Betrag und Sie geben einen Betrag von 30 Punkten ein, so erhalten Sie entweder:

- 200-30=170 Punkte als Auszahlung, wenn Ihr Empfänger in dieser Runde aussetzen muss und den sicheren Betrag erhält (Wahrscheinlichkeit von 1/3). Ihr Empfänger erhält in diesem Fall eine Auszahlung von 20+30=50.
- 200 Punkte als Auszahlung, wenn Ihr Empfänger in dieser Runde an der Aufgabe teilnimmt. In diesem Fall hängt die Auszahlung für Ihren Empfänger alleine von seiner Auswahl und seinen Ergebnissen ab.

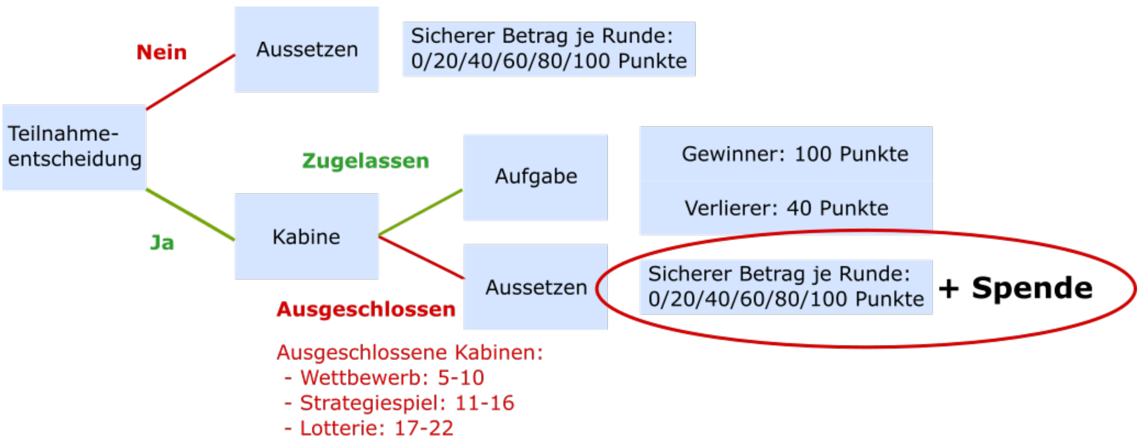
Weitere Informationen zur Auszahlung aus Teil 2

Da Ihre Auszahlung vom Teilnahmeverhalten Ihres Empfängers abhängt, erfolgt diese erst nach Ende des nachfolgenden Experimentes. Das bedeutet, dass Sie sofort am Ende dieses Experimentes lediglich Ihre Auszahlung für Teil 1 erhalten. Des Weiteren erhalten Sie in den nächsten 30 Tagen, nach Beendigung des nachfolgenden Experimentes, eine E-Mail, in der Sie informiert werden, wann Sie Ihre Auszahlung abholen können. Bei Abholung der Auszahlung werden Sie informiert, welche Runde zufällig als auszahlungsrelevant festgelegt wurde und ob Ihr Empfänger in dieser Runde aussetzen musste und somit Ihre Spende als Teil seiner Auszahlung erhält. In diesem Fall erhalten Sie Ihre Ausstattung minus Ihre Spende. Musste Ihr Empfänger nicht aussetzen, erhalten Sie Ihre gesamte Ausstattung als Auszahlung. Die genauen Informationen zur Abholung des Geldes finden Sie dann in der E-Mail.

Aufgabe Ihres Empfängers

Im Nachfolgeexperiment spielt Ihr Empfänger drei verschiedene Aufgaben (Wettbewerb, Lotterie und Strategiespiel) in jeweils sechs Runden gegen einen wechselnden anderen Spieler. Der Gewinner jeder Aufgabenrunde erhält 100 Punkte, der Verlierer erhält 40 Punkte. Ihr Empfänger hat jedoch für jede einzelne Runde einer Aufgabe die Wahlmöglichkeit, nicht zu spielen, sondern stattdessen einen sicheren Betrag zu erhalten. Die Höhe des sicheren Betrags beträgt in den 6 Runden jeder Aufgabe 0, 20, 40, 60, 80 und 100 Punkte. Außerdem besteht die Möglichkeit, dass er unabhängig von seiner Entscheidung **für** die Teilnahme an der Aufgabe automatisch dem sicheren Betrag zugeordnet wird und alle 6 Runden des jeweiligen Spiels aussetzen muss. Dies ist je nach Aufgabe der Fall, wenn

- Ihr Empfänger in den Kabinen 5-10 sitzt (Wettbewerb),
- Ihr Empfänger in den Kabinen 11-16 sitzt (Strategiespiel)
- oder Ihr Empfänger in den Kabinen 17-22 sitzt (Lotterie).



Ob er teilnehmen darf oder aussetzen muss, erfährt Ihr Empfänger jedoch erst **nachdem** er sich für oder gegen die Teilnahme an den einzelnen Runden der jeweiligen Aufgabe entschieden hat. Beachten Sie bitte, dass für jede Aufgabe 6 Kabinen von der Teilnahme ausgeschlossen werden. An dem Nachfolgeexperiment nehmen

18 Teilnehmer teil, sodass Ihr Empfänger in genau einer Aufgabe automatisch aussetzen muss.

Spielregeln für Ihren Empfänger

Damit Sie Ihre Entscheidung optimal treffen können, erhalten Sie hier die Regeln der drei Aufgaben, die Ihr Empfänger gegen wechselnde Gegenspieler spielen kann. Diese werden Ihnen vor jeder Beitragsentscheidung auf den Bildschirmen nochmals erläutert und mit einer kurzen Proberunde für die jeweilige Aufgabe verdeutlicht.

Lotterie

Die Spieler bekommen vom Computer zufällig die Positionen Spieler 1 und Spieler 2 zugewiesen und haben hierbei die gleiche Wahrscheinlichkeit (50%) für die Position Spieler 1. Spieler 1 kann zuerst ein Los A oder B wählen, Spieler 2 erhält das verbliebene Los. In 50% der Fälle führt Los **A** zu 100 Punkten und Los **B** zu 40 Punkten. In den anderen 50% der Fälle führt Los **A** zu 40 Punkten und Los **B** zu 100 Punkten.

Wettbewerb

Für die Dauer von 60 Sekunden nutzen die beiden konkurrierenden Spieler eine vorgegebene Zahlen-Buchstaben-Tabelle, um möglichst viele 3-Buchstaben-Kombinationen in die entsprechenden Zahlenkombinationen umzuwandeln. Derjenige, der innerhalb dieser Zeit mehr "Worte" korrekt in Zahlen umwandelt, gewinnt den Wettbewerb und erhält 100 Punkte. Der Andere verliert und erhält 40 Punkte. Bei Gleichstand erhalten beide 100 Punkte.

Strategiespiel

Die Spieler bekommen vom Computer zufällig die Positionen Spieler 1 und Spieler 2 zugewiesen und haben hierbei die gleiche Wahrscheinlichkeit (50%) für die Position Spieler 1. Anschließend besteht die Wahl zwischen zwei Optionen, A und B. Falls beide **verschiedene** Optionen wählen (A, B oder B, A) erhält Spieler **1** 100 Punkte und Spieler **2** erhält 40 Punkte. Falls beide die **gleiche** Option wählen (A, A oder B, B), erhält Spieler **1** 40 Punkte und Spieler **2** erhält 100 Punkte.

Nachdem alle Teilnehmer den zweiten Teil des Experimentes abgeschlossen haben, folgt ein kurzer Fragebogen. Anschließend können Sie das Labor einzeln leise verlassen. Die Informationen zu Ihren Ergebnissen und Ihrer Auszahlung erhalten Sie zeitversetzt. Sobald das nachfolgende Experiment im Laufe der nächsten 30 Tage abgeschlossen ist, werden Sie per E-Mail über die Abholungsformalitäten informiert. Falls Sie nun noch Fragen haben, heben Sie bitte die Hand aus der Kabine. Es wird dann jemand zu Ihnen kommen, um Ihre Frage zu beantworten. Wenn Sie keine weiteren Fragen haben, können Sie auf OK klicken und mit dem zweiten Teil beginnen.

Vielen Dank für Ihre Teilnahme

Translated Instructions for Dictators' Exclusion Treatment

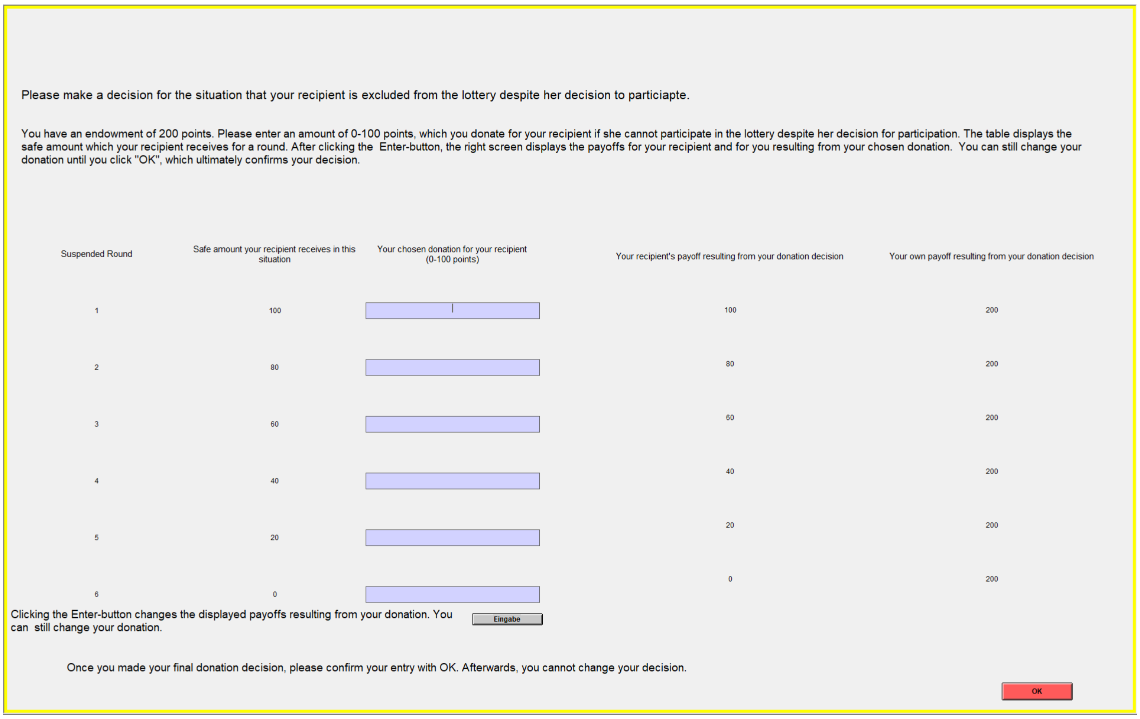
Instructions, Part 2:
The first part of the experiment is now over. The following second part is independent of this and has no effect on your payment from Part 1 of the experiment. In Part 2 you determine the final payment of a participant assigned to you (hereinafter referred to as the "recipient"). Your recipient can participate in several rounds of three different tasks in a subsequent experiment. You yourself will not participate in this subsequent experiment. The exchange rate for this part of the experiment (both for you and for your recipient) is identical to Part 1 and is therefore still 100 points = 5 euros.

Your Task in this Part of the Experiment
Like every other participant in this experiment, you receive 200 points as endowment and exactly one recipient from the follow-up experiment is randomly assigned to you. You can donate 0 to 100 points to your recipient from your 200 points. You make your decision about your donation to your recipient in the event that two conditions are met: 1. Your recipient must have decided to participate in a specific task in the follow-up experiment. 2. Despite his desire to participate, he has to suspend production. In the event of suspension, your recipient – depending on which round of the task they are in – will be assigned a certain number of points as a safe amount. This is explained in detail below. You decide strategically, i.e. in advance of all possible tasks and rounds or ensured amounts of your recipient, because you do not yet know the results of the follow-up experiment.

Determination of the Payoff from Part 2
At the end of your experiment, one of your payment decisions is randomly determined to be relevant for you and your recipient. If your recipient wanted to participate in the payoff-related round, but had suspend production and receive a safe amount, the donation you selected will also be paid to him. In this case, your endowment of 200 points will be reduced by the donation amount you have chosen. If your recipient is allowed to participate in the payoff-relevant round, their payoff is determined only by their decisions and results in the game, and you keep your full endowment of 200 points. Your donation decision and the calculation of the payment are explained in Figure 2 using the example of 20 points as an ensured amount.

Example 2: On the left side of the screen are the safe amounts that your recipient receives per round if they have to be suspended in this task. You can also enter a donation of 0-100 points that you want to contribute to the payoff of your recipient

Figure 2: Donation Decision for an Ensured Amount of 20 Points



in each round. Assuming that your recipient receives 20 points as a safe amount and you enter 30 points, you will either receive:

- 200 - 30 = 170 points as a payoff if your receiver has to be suspended in this round and receives the safe amount. In this case, your recipient will receive a payment of 20 + 30 = 50.
- 200 points as a payoff if your recipient participates in the task in this round. In this case, the payoff for your recipient depends solely on their selection and results.

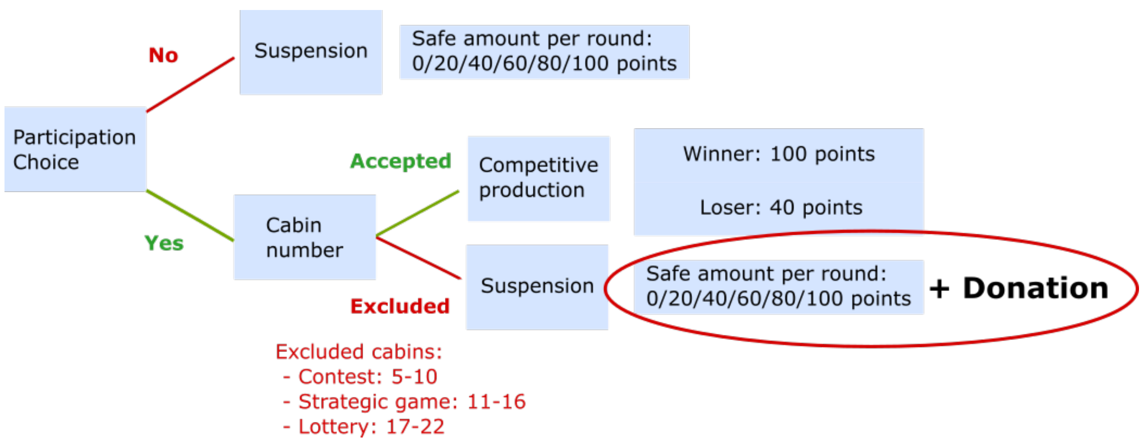
More Information about the Payment from Part 2

Since your payment depends on your recipient's participation behavior, this will only take place after the end of the following experiment. This means that within the next 30 days after the end of the experiment that follows, you will receive an email informing you when you can collect your payoff. When you pick up the payoff, you will be informed which round was selected as relevant for the payoff and whether your recipient had to be suspended in this round and thus receives your donation as part of his payoff. In this case, you will receive your endowment minus your donation, plus your payment from Part 1. If your recipient did not have to be suspended, you will receive all of your endowment plus your result from Part 1 as a payment. You will find the exact information on how to collect the money in the email.

Your Recipient's Task

In the follow-up experiment, your recipient plays three different tasks (competition, lottery and strategic game) in six rounds against a different player. The winner of each task round receives 100 points, the loser 40 points. However, for each round of a task, your recipient can choose not to play, but instead to voluntarily suspend production and receive a safe amount. The amount of the ensured payment is 0, 20, 40, 60, 80 and 100 points in the 6 rounds of each task. Moreover, it is possible that your recipient is suspended from all 6 rounds of a task and receives the respective safe amount,irrespective of his decision **to participate** in a task. Depending on the type of task, this is the case if

- Your Recipient is placed in cubicles 5-10 (Competition),
- Your Recipient is placed in cubicles 11-16 (Strategic Game)
- or Your Recipient is placed in cubicles 17-22 (Lottery).



Your recipient is informed whether he may participate or is suspended, only **after** he decided for or against participation for each single round of the respective task. Note that for each task, 6 cubicles are excluded from participation. There are 18 participants in the subsequent experiment, thus your participant will automatically be suspended from exactly one task.

Rules of the Game for Your Recipient

To help you make the best possible decision, here you will find the rules for the three tasks that your recipient can play against changing opponents. These are explained to you on the screens before each decision on contributions and illustrated with a short trial round for the respective task.

Lottery

Players are randomly assigned player positions 1 and 2 by the computer and have the same probability (50%) of position Player 1. Player 1 can choose lot A or B first, Player 2 receives the remaining lot. In 50% of the cases, Lot **A** leads to 100 points and Lot **B** to 40 points. In the other 50% of cases, Lot **A** leads to 40 points and Lot **B** leads to 100 points.

Competition

For a period of 60 seconds, the two competing players use a given number-letter table to convert as many 3-letter combinations as possible into the corresponding number combinations. The one who correctly converts more "words" into numbers within this time wins the competition and receives 100 points. The other loses and receives 40 points. If there is a tie, both score 100 points.

Strategic Game

The players are randomly assigned the positions Player 1 and Player 2 by the computer and have the same probability (50%) for position Player 1. Then there is a choice between two options, A and B. If both choose **different** options (A, B or B, A) Player **1** receives 100 points and player **2** receives 40 points. If both choose the same option (A, A or B, B), Player **1** will get 40 points and Player **2** will get 100 points.

After all participants have completed the second part of the experiment, a short questionnaire will follow. You can then quietly leave the laboratory individually. You will receive the information on your results and your payment with a time delay. As soon as the subsequent experiment is completed within the next 30 days, you will be informed of the collection formalities by email.
If you still have questions, please raise your hand out of the cabin. Someone will come to you to answer your question. If you have no further questions, you can click OK and start with the second part.
Thank you for your participation

A.2 Instructions for Recipients

A.2.1 Instructions for Certainty Equivalence Test

Original Instructions for Certainty Equivalence Test in German

Instruktionen, Teil 1:

Allgemeine Erklärungen
Willkommen zum Experiment. Wenn Sie die Instruktionen aufmerksam lesen und alle Regeln beachten, können Sie in diesem Experiment Geld verdienen. Während des Experimentes sprechen wir nicht von Euro, sondern von Punkten. Diese werden gemäß folgendem Wechselkurs umgerechnet:

100 Punkte = 5 Euro.

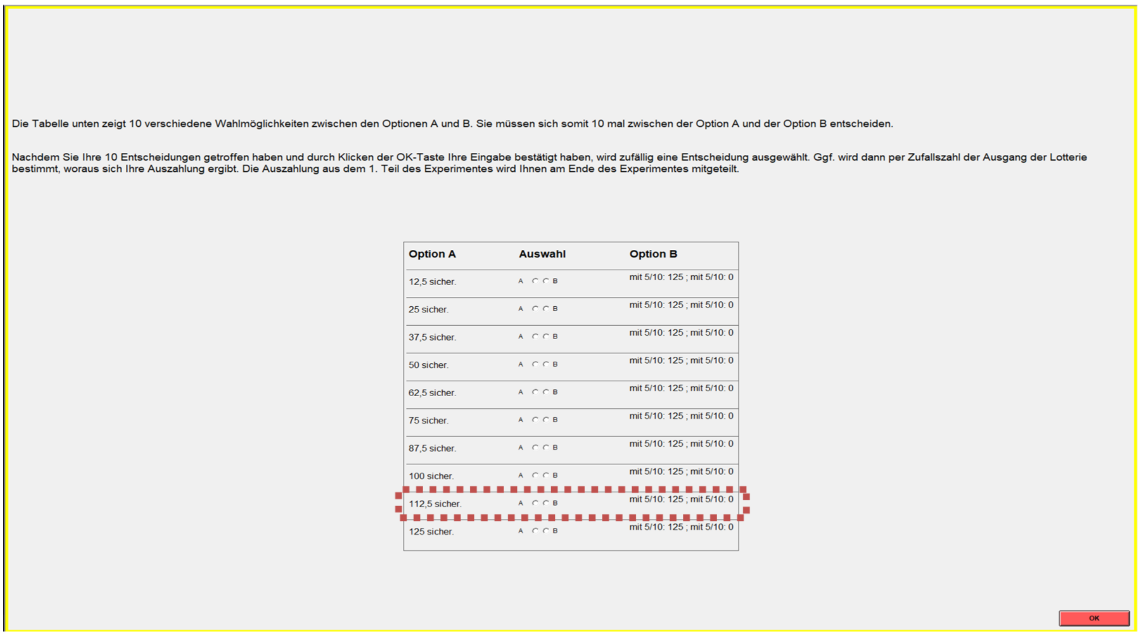
Während des gesamten Experimentes ist das Sprechen mit anderen Teilnehmern nicht erlaubt. Wenn Sie Fragen haben, richten Sie diese bitte ausschließlich an uns. Wir beantworten Ihre Fragen gerne individuell. Die Einhaltung dieser Regel ist sehr wichtig. Andernfalls sind die Ergebnisse dieses Experimentes wissenschaftlich wertlos.

Dieses Experiment besteht aus **2 Teilen**, wobei jeder Teil nacheinander einzeln erläutert wird. In jedem der 2 Teile des Experimentes können Sie Geld verdienen. Ihre Auszahlung erhalten Sie in bar am Ende des Experimentes. Das komplette Experiment wird voraussichtlich 60 Minuten dauern. Für bessere Verständlichkeit wird im gesamten Experiment das generische Maskulinum verwendet. Im Folgenden wird Ihnen nun der erste Teil erläutert.

Detaillierte Informationen zu Teil 1 des Experimentes In dem ersten Teil wählen Sie für 10 verschiedene Situationen jeweils zwischen zwei Optionen A und B, das heißt Sie müssen sich 10-mal zwischen der Option A und der Option B entscheiden. Die Option A ist immer eine sichere Auszahlung in bestimmter Höhe. Die Option B ist eine gleichbleibende Lotterie. Die Tabelle unten zeigt die 10 Situationen und die in jeder Situation zur Auswahl stehenden Optionen. Hierbei legt der Computer zufällig fest, ob Ihnen die Tabelle wie in Abbildung 3 oder in umgekehrter Reihenfolge angezeigt wird.

Beispiel: Die Option A in der 9. Zeile lautet: 112,5 sicher. Die Option B in der 9. Zeile lautet: mit 5/10: 125 und mit 5/10: 0. Wenn Sie in der 9. Zeile die Option A wählen, haben Sie eine Auszahlung von 112,5 Punkten sicher. Wenn Sie in der 9. Zeile die Option B wählen, haben Sie in 5 aus 10 Fällen (50%) eine Auszahlung von 125 Punkten und in 5 aus 10 Fällen (50%) eine Auszahlung von 0 Punkten.

Abbildung 3: Entscheidungsbildschirm für die Wahl zwischen einer sicheren Auszahlung und einer Lotterien



Wir bitten Sie im Folgenden, sich für jede dieser 10 Situationen zwischen den Optionen A und B zu entscheiden. Vergleichen Sie bitte zeilenweise die Optionen A und B und entscheiden Sie für jede Zeile durch Klicken von A oder B.

Berechnung der Auszahlung aus Teil 1:
Ihre Auszahlung aus diesem Teil bestimmt sich wie folgt: der Computer wählt aus den 10 Situationen zufällig eine aus. Ihre Entscheidung in dieser Situation ist für Ihre Auszahlung relevant. Haben Sie sich zum Beispiel in Zeile 2 für Option B entschieden, wird die Lotterie ausgespielt wonach Sie mit einer Wahrscheinlichkeit von 5 aus 10 Fällen (50%) 125 Punkte als Auszahlung und mit 5 aus 10 Fällen (50%) 0 Punkte als Auszahlung erhalten. Das Ausspielen dieser Lotterie können Sie sich anhand einer Urne gefüllt mit 5 weißen und 5 roten Kugeln vorstellen. Greift jemand mit verbundenen Augen in die Urne und zieht eine weiße Kugel heraus, erhalten Sie eine Auszahlung von 125. Falls die gezogene Kugel rot ist, erhalten Sie 0 Punkte. Im Experiment wird das „Ziehen der Kugeln“ automatisiert und vom Computer durchgeführt.

Falls Sie nun noch Fragen haben, heben Sie bitte die Hand aus der Kabine. Es wird dann jemand zu Ihnen kommen, um Ihre Frage zu beantworten. Wenn Sie keine weiteren Fragen haben, können Sie nun die Auswahl der Optionen A und B am Bildschirm treffen.

Translated Instructions for Certainty Equivalence Test

Instructions Part 1

General Explanation for the Participants

Welcome to the experiment and thank you for your participation. You can earn money in this experiment if you read the instructions carefully and follow all of the rules. Throughout the entire experiment we will speak of points rather than Euros. These points will be converted according to the following exchange rate:

100 Points = 5 Euro.

Talking with other participants is not allowed throughout the entire experiment. If you have questions, please direct them at us. We will gladly answer your questions individually. Following this rule is very important. Otherwise the results of this experiment will have no scientific value.

This experiment consists of **2 parts**, and each part will be explained one after the other. In each of the 2 parts of the experiment you can earn money. You will be paid in cash after the experiment. The whole experiment is expected to take 60 minutes. For improved understandability, we will use generic masculine throughout the whole experiment. The following will now explain the first part of the experiment.

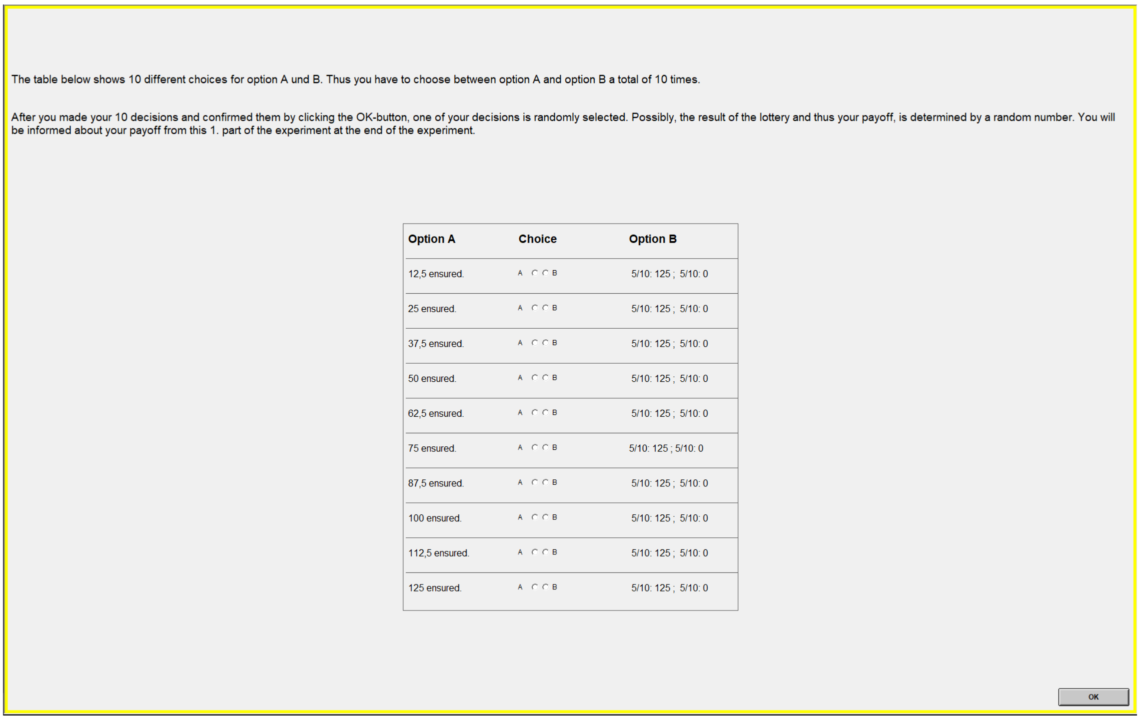
Detailed Information for Part 1 of the Experiment

In the first part of the experiment we ask you to choose between two options, A or B, for each of 10 different situations. That means you have to choose between option A and option B a total of 10 times. Option A is always an ensured payment at a definite amount. Option B is always the same lottery. The table below shows the 10 situations and the choices for options A and B in each situation. The table will be shown to you either as it is in Image 3 below or in reverse order. How the table is presented to you is determined randomly.

Example: Option A in the 9th row reads: 112.5 points ensured. Option B in the 9th row reads: 5/10: 125 points and 5/10: 0 points. If you choose option A in the 9th row, then you are ensured to receive a payment of 112.5 points. If you choose option B in the 9th row, then you have a 5 out of 10 (50%) chance of receiving a payment of 125 points and a 5 out of 10 (50%) chance of receiving a payment of 0 points .

We ask you to choose between options A and B in each of the following 10 situations. Please compare between options A and B in each row and make your decision

Image 3: Decision screen for the choice between a ensured payment and a lottery



for each row by clicking A or B.

Payment Calculation from Part 1:

Your payment for this part of the experiment will be determined as follows: The computer randomly chooses 1 situation from the 10. Your decision in this situation is relevant for your payment. If for example you chose option B in row 2, then the lottery will be played, whereby you have a 5 out of 10 (50%) chance of receiving a payment of 125 points and a 5 out of 10 (50%) chance of receiving a payment of 0 points. You can imagine the playing of this lottery using an urn filled with 5 white and 5 red marbles. If a blindfolded person reaches into the urn and pulls out a white marble, then you receive a payment of 125 points. If the marble is red, then you receive 0 points. In the experiment 'pulling out a marble' is automated and executed by a computer.

If you now have any questions, please raise your hand. Someone will come to you in order to answer your questions. If you don't have any questions, then you can now start making choices on your screen.

A.2.2 Instructions for Recipients' Main Experiment

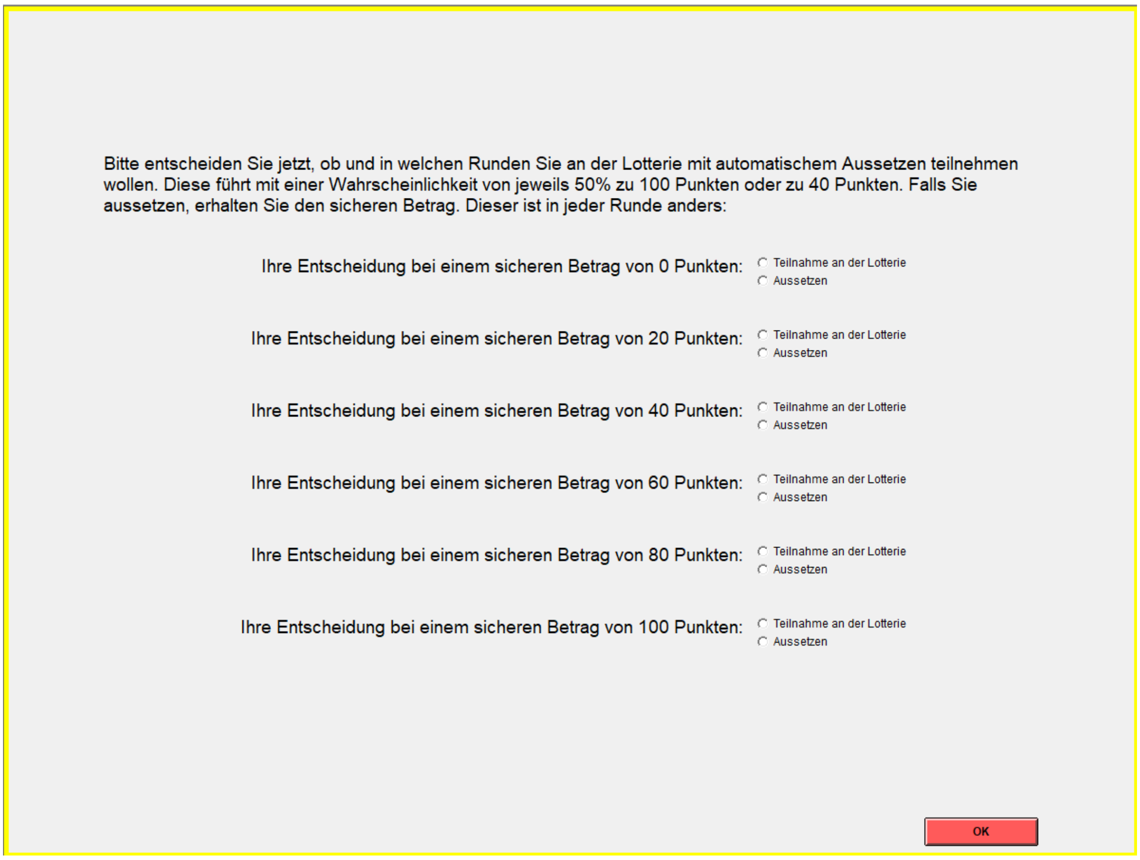
Original Instructions for Recipients' Main Experiment in German

Instruktionen, Teil 2:
Der erste Teil des Experimentes ist nun zu Ende. Der folgende zweite Teil hat keine Auswirkung auf Ihre Auszahlung aus Teil 1 des Experimentes. In Teil 2 haben Sie die Möglichkeit, an drei verschiedenen Aufgaben mit jeweils sechs Runden jeweils zweimal teilzunehmen, insgesamt also sechs Spiele zu spielen. Sie können für jede Runde der Aufgaben einzeln entscheiden, ob Sie an der Aufgabe teilnehmen wollen, oder lieber aussetzen und einen sicheren Betrag erhalten. Dieser unterscheidet sich in den einzelnen Runden und beträgt entweder 0, 20, 40, 60, 80 oder 100 Punkte. Falls Sie sich zur Teilnahme an einer Runde entscheiden, spielen Sie gegen einen anderen Teilnehmer, der Ihnen jede Runde neu zufällig zugeordnet wird. Derjenige von Ihnen, der eine Runde gewinnt, erhält 100 Punkte, der Verlierer 40 Punkte. Die sechs Spiele bestehen aus den drei verschiedenen Aufgabentypen Lotterie, Strategiespiel und Wettbewerb. Diese werden jeweils einmal mit bindender Entscheidung und einmal mit automatischem Aussetzen gespielt. Bindende Entscheidung heißt, dass Sie definitiv an allen Runden teilnehmen, für die Sie sich entscheiden. Automatisches Aussetzen hingegen heißt, dass bereits vorab immer 6 Kabinen, also 1/3 der Teilnehmer, von der Teilnahme an einer Aufgabe ausgeschlossen wurden. Falls Sie in einer der Aufgaben davon betroffen sind, erfahren Sie dies erst nachdem Sie sich für oder gegen die Teilnahme entschieden haben. Wie Sie Ihre Entscheidungen treffen, wird Ihnen in Abbildung 2 verdeutlicht. Hinweis: Nach Bestätigen Ihrer Teilnahmeentscheidungen mit „OK“ kann es zu längeren Wartezeiten kommen.

Auf dem abgebildeten Bildschirm sehen Sie die sicheren Beträge für jede Runde der Lotterie mit automatischem Aussetzen und können für jede Runde entweder die Option „Teilnahme an der Lotterie“ oder „Aussetzen“ auswählen. Für die Runden, für die Sie „Aussetzen“ ausgewählt haben, setzen Sie aus und erhalten den für die jeweilige Runde angegebenen sicheren Betrag, während die anderen Teilnehmer spielen können. In den Runden, in denen Sie „Teilnahme an der Lotterie“ ausgewählt haben, warten Sie, bis Ihr Gegenspieler ebenfalls in der gleichen Runde angelangt ist und spielen dann gegen ihn. Wenn Sie jedoch in einer der Kabinen sitzen, die von der Lotterie ausgeschlossen sind, müssen Sie auch dann aussetzen und den sicheren Betrag erhalten, wenn Sie sich zur Teilnahme entschieden haben. Falls Sie an einem Spiel teilnehmen, gelten die folgenden **Spielregeln:**

Lotterie
Sie bekommen vom Computer zufällig die Position Spieler 1 oder Spieler 2 zugewiesen, mit der jeweils gleichen Wahrscheinlichkeit (50%). Spieler 1 kann zuerst ein Los A oder B wählen, Spieler 2 erhält das verbliebene Los. In 50% der Fälle führt Los

Abbildung 2: Teilnahmeentscheidung



A zu 100 Punkten und Los **B** zu 40 Punkten. In den anderen 50% der Fälle führt Los **A** zu 40 Punkten und Los **B** zu 100 Punkten. Falls Sie sich als einziges zur Teilnahme entscheiden, sind Sie automatisch Spieler 1 und können zwischen Los A und B wählen. Falls eine ungerade Anzahl an Spielern in einer Runde teilnimmt, spielt ein zufällig bestimmter Spieler alleine als Spieler 1.

Wettbewerb

Innerhalb von 60 Sekunden sollen Sie mit einer vorgegebenen, zufällig generierten Zahlen-Buchstaben-Tabelle möglichst viele 3-Buchstaben-Kombinationen in Zahlenkombinationen umwandeln. Wenn Sie in dieser Zeit mehr „Worte“ korrekt verschlüsseln als Ihr Gegenspieler, gewinnen Sie und erhalten 100 Punkte. Anderenfalls verlieren Sie und erhalten 40 Punkte. Bei gleicher Wortanzahl gewinnt der Schnellere, also derjenige, der das letzte Wort zuerst korrekt eingegeben hat. Falls Sie als einziges am Wettbewerb teilnehmen, gewinnen Sie automatisch. Falls eine ungerade Anzahl an Spielern in einer Runde teilnimmt, spielt ein zufällig bestimmter Spieler alleine und gewinnt automatisch.

Strategiespiel

Sie bekommen vom Computer zufällig die Position Spieler 1 oder Spieler 2 zugewiesen, mit der jeweils gleichen Wahrscheinlichkeit (50%). Anschließend können Sie und der andere Teilnehmer jeweils zwischen den Optionen A und B wählen. Falls Sie beide **verschiedene** Optionen wählen (A, B oder B, A) erhält Spieler **1** 100 Punkte und Spieler **2** erhält 40 Punkte. Falls Sie beide die **gleiche** Option wählen (A, A oder B, B), erhält Spieler **1** 40 Punkte und Spieler **2** erhält 100 Punkte. Falls Sie als einziges an dem Strategiespiel teilnehmen, gewinnen Sie automatisch. Falls eine ungerade Anzahl an Spielern in einer Runde teilnimmt, spielt ein zufällig bestimmter Spieler alleine und gewinnt automatisch.

Berechnung Ihrer Auszahlung in Teil 2

Nach Ende des Spiels wird aus allen gespielten Runden mit bindender Entscheidung und allen mit automatischem Aussetzen jeweils eine Runde, also insgesamt zwei Runden, zufällig als für Sie auszahlungsrelevant bestimmt. Ihre Auszahlung ist die Summe der Punkte, die Sie in diesen beiden Runden durch Ihre Teilnahme am Spiel oder beim Aussetzen erhalten haben. Außerdem ist es möglich, dass Ihre Auszahlung durch vorher festgelegte Spendenbeträge erhöht wird.

Spendenbedingungen:

In einem vorherigen Experiment wurden Ihnen zufällig zwei Sender zugeordnet, die für jede einzelne Runde des Experimentes einen Geldbetrag zwischen 0 und 100 Punkten festgelegt haben, den Sie Ihnen **nur im Falle Ihres Aussetzens** spenden. Diese Spende wird dann zu dem sicheren Betrag addiert, den Sie in der entsprechenden Runde beim Aussetzen erhalten.

Für die Spenden gelten jedoch unterschiedliche Regeln: Ihr Sender, der für die Aufgabentypen mit bindender Entscheidung entscheidet, kann Ihnen nur eine Spende für die Runden geben, in denen Sie **freiwillig** aussetzen. Der andere Sender ist für die Aufgabentypen mit automatischem Aussetzen zuständig und kann Ihnen nur etwas spenden, wenn Sie sich für die Teilnahme an einer Runde entscheiden, aber aufgrund des automatischen Aussetzens **nicht** an dieser **teilnehmen dürfen**.

Wenn Sie nicht aussetzen, erhalten Sie keine Spende, sondern das Ergebnis des Spiels in der auszahlungsrelevanten Runde, also je nach Ihrem Abschneiden 40 oder 100 Punkte.

Ihre finale Auszahlung aus Teil 2 hängt somit ab von:

- Ihrer Entscheidung für oder gegen die Teilnahme an den Spielen,
- Ihrem Erfolg in den Spielen,
- Ihrer zufälligen Zuordnung zum automatischen Aussetzen,
- der Spendenentscheidungen Ihrer Sender und
- der zufällig als auszahlungsrelevant gezogenen Runde.

Nachdem alle Teilnehmer den zweiten Teil des Experimentes abgeschlossen haben, folgt ein kurzer Fragebogen. Anschließend erhalten Sie Ihre Auszahlung einzeln in bar. Falls Sie nun noch Fragen haben, heben Sie bitte die Hand aus der Kabine. Es wird dann jemand zu Ihnen kommen, um Ihre Frage zu beantworten. Wenn Sie keine weiteren Fragen haben, können Sie auf OK klicken und mit dem zweiten Teil beginnen.
Vielen Dank für Ihre Teilnahme

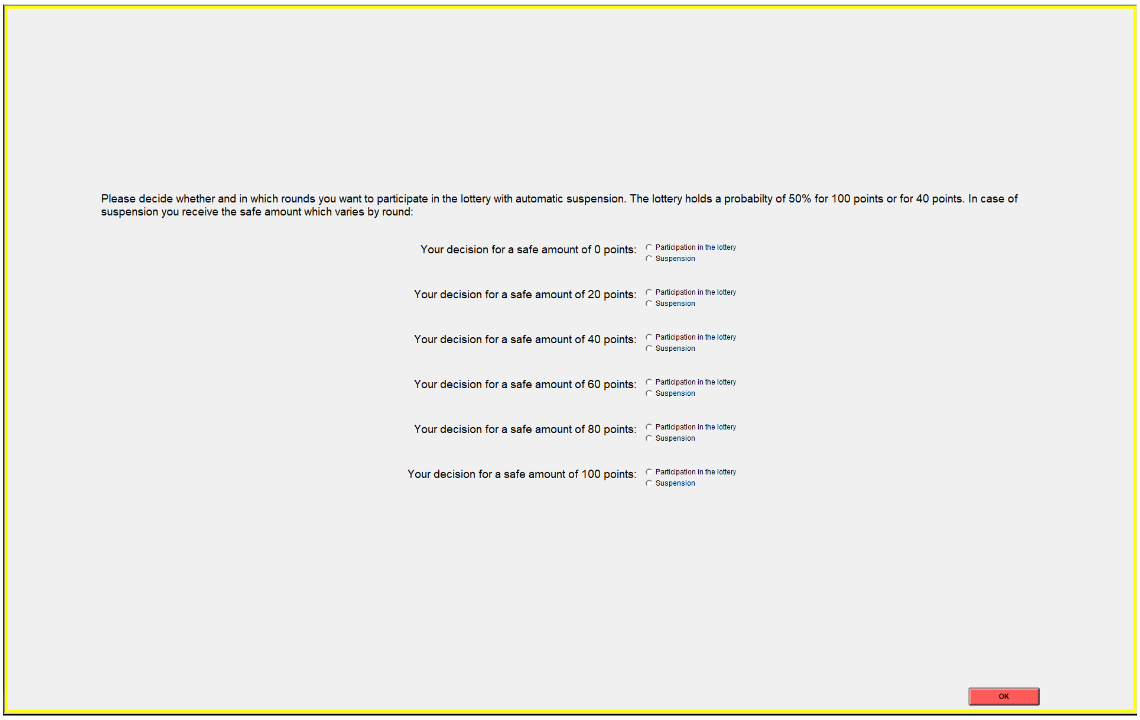
Translated Instructions for Recipients' Main Experiment

Instructions, Part 2:
The first part of the experiment is now over. The following second part has no effect on your payment from part 1 of the experiment. In Part 2, you have the option of participating twice in three different tasks, each with six rounds, for a total of six games. For each round of tasks, you can decide individually whether you want to take part in the task or rather suspend and receive a safe amount. This differs in the individual rounds and is either 0, 20, 40, 60, 80 or 100 points. If you decide to take part in a round, you are playing against another participant who will be randomly assigned to each round. The one of you who wins a round receives 100 points, the loser 40 points. The six games consist of three different types of tasks: lottery, strategic game and competition. These are played once with a binding decision and once with automatic suspension. A binding decision means that you definitely participate in all the rounds you choose. Automatic suspension, on the other hand, means that 6 cubicles, i.e. 1/3 of the participants, have always been excluded from participating in a task. If you are affected by one of the tasks, you will only find out after you have decided for or against participating. How you make your decisions is illustrated in Figure 2. Note: After confirming your participation decisions with "OK" there may be longer waiting times.

On the screen you can see the safe amounts for each round of the lottery with automatic suspension and can choose either the option "Participation in the lottery" or "Suspension" for each round. For the rounds for which you have selected "Suspend", you sit down and receive the ensured amount specified for the respective round while the other participants can play. In the rounds in which you selected "Participation in the lottery", wait until your opponent has also reached the same round and then play against them. However, if you sit in one of the booths that are excluded from the lottery, you will still have to stay and receive the safe amount even if you have decided to participate. If you participate in a game, the following **rules** apply:

Lottery
The computer randomly assigns you to Player 1 or Player 2 with the same proba-

Figure 2: Decision to Participate



bility (50%). Player 1 can choose lot A or B first; Player 2 gets the remaining lot. In 50% of the cases, Lot **A** leads to 100 points and Lot **B** to 40 points. In the other 50% of cases, Lot **A** leads to 40 points and Lot **B** leads to 100 points. If you are the only one who decides to participate, you are automatically Player 1 and can choose between lots A and B. If an odd number of players take part in a round, a randomly chosen player plays alone as Player 1.

Competition

Within 60 seconds you should use a given, randomly generated number-letter table to convert as many 3-letter combinations as possible into number combinations. If you correctly encrypt more "words" than your opponent during this time, you win and get 100 points. Otherwise you lose and get 40 points. With the same number of words, the faster one wins, i.e. the one who entered the last word correctly first. If you are the only one participating in the competition, you will automatically win. If an odd number of players take part in a round, a randomly chosen player plays alone and wins automatically.

Strategic Game

The computer randomly assigns you to Player 1 or Player 2 with the same probability (50%). You and the other participant can then choose between options A and B. If you choose both **different** options (A, B or B, A) Player **1** will receive 100 points and Player **2** will receive 40 points. If you both choose the **same** option (A, A or B, B), Player **1** will receive 40 points and Player **2** will receive 100 points. If you are the only one participating in the strategic game, you will automatically win. If an odd number of players take part in a round, a randomly chosen player plays alone and wins automatically.

Calculation of your Payoff in Part 2

At the end of the game, one round out of all rounds with a binding decision and all with automatic suspension, i.e. a total of two rounds, will be randomly determined to be relevant for you. Your payoff is the sum of the points you received in either of these rounds by participating in the game or by being suspended. It is also possible that your payment will be increased by predetermined donation amounts.

Donations Conditions:

In a previous experiment, you were randomly assigned to two transmitters that set a monetary amount between 0 and 100 points for each round of the experiment, which you only donate if you are suspended. This donation is then added to the safe amount that you will receive in the corresponding round when you are suspended.

However, different rules apply to donations: Your sender, who decides on the task types with a binding decision, can only give you a donation for the rounds in which you **voluntarily** suspend. The other sender is responsible for the types of tasks with automatic suspension and can only donate something if you decide to take part in a round, but are **not allowed to take part** because of the automatic suspension.

If you are not suspended, you will not receive a donation, but instead the result of the game in the payoff round, i.e. 40 or 100 points depending on your performance.

Your final payment from Part 2 thus depends on:

- Your decision for or against participation in the games,
- Your success in the games,
- Your random assignment for automatic suspension,
- The donation decisions of your sender and
- The round drawn at random as relevant for payment.

After all participants have completed the second part of the experiment, a short questionnaire will follow. You will then receive your payment individually in cash. If you still have questions, please raise your hand out of the cubicle. Someone will come to you to answer your question. If you have no further questions, you can click OK and start with the second part.

Thank you for your participation.

B Questionnaire

B.1 Questionnaire for Dictators

B.1.1 Original Questionnaire for Dictators in German

Table B.1: Questionnaire for Dictators (German)

Geschlecht	männlich weiblich divers
Studienrichtung/ Fakultät:	Rechtswissenschaft Wirtschafts- und Sozialwissenschaften Medizin Erziehungswissenschaften Geisteswissenschaften Mathe, Informatik, Naturwissenschaften Psychologie Sonstiges
Jahrgang (JJJJ):	
Semester:	
Würden Sie sich eher als risikofreudig oder risikoscheu einordnen?	sehr risikofreudig eher risikofreudig risikoneutral eher risikoscheu sehr risikoscheu
Angenommen, Sie würden in der Rolle Ihres Empfängers stecken: Wann würden Sie sich entscheiden auszusetzen?	Immer (ich würde nie teilnehmen) Ab 20 als sicherer Betrag Ab 40 als sicherer Betrag Ab 60 als sicherer Betrag Ab 80 als sicherer Betrag Erst ab 100 als sicherer Betrag Nie (ich würde immer teilnehmen) Je nach Aufgabentyp verschieden

Wie fair finden Sie die Spielregeln für Ihren Empfänger?	Sehr fair Fair Eher fair Eher unfair Unfair Sehr unfair
Bitte begründen Sie Ihre Angabe kurz	
Schätzen Sie: Für welche Aufgabe entscheiden sich die <u>meisten</u> Teilnehmer des nachfolgenden Experimentes bei einem sicheren Betrag von 40 freiwillig für das Aussetzen?	Lotterie Wettbewerb Strategiespiel
Schätzen Sie: Für welche Aufgabe entscheiden sich die <u>wenigsten</u> Teilnehmer des nachfolgenden Experimentes bei einem sicheren Betrag von 40 freiwillig für das Aussetzen?	Lotterie Wettbewerb Strategiespiel
Wie wichtig ist Ihnen Gerechtigkeit?	Gar nicht wichtig Nicht wichtig Wichtig Sehr wichtig
Wie sehr haben Sie bei Ihrer Abgabeentscheidung darauf geachtet, dass die Auszahlung Ihres Empfängers gerecht ist?	Gar nicht Wenig Eher wenig Eher stark Stark Sehr stark
Wann helfen Sie jemandem in einer schwierigen Situation?	In jedem Fall. Nur, wenn die Situation selbst verschuldet ist. Nur, wenn die Situation nicht selbst verschuldet ist. Eher, wenn die Situation selbst verschuldet ist. Eher, wenn die Situation nicht selbst verschuldet ist. In keinem Fall.

Welche der folgenden Varianten von sozialer Sicherheit bevorzugen Sie?	Bedingungsloses Grundeinkommen An Bedingungen geknüpfte Sicherung wie Hartz IV Von Erwerbsleistungen abhängige Sicherung wie Arbeitslosenversicherung Keinerlei soziale Sicherung, stattdessen indiviuelle Verantwortung und Spenden bei Notfällen
Die Auszahlung, die Sie für den ersten Teil bekommen beträgt in Euro: Diesen Betrag bekommen Sie zusammen mit Ihrer Auszahlung von Teil 2 zeitversetzt ausgezahlt. Ihre Auszahlung von Teil 2 wird zwischen 5 und 10 Euro betragen. Über Ihren genauen Auszahlungsbetrag und die Abholungsmodalitäten informieren wir Sie innerhalb der nächsten 30 Tage per E-Mail. Vielen Dank für Ihre Teilnahme. Sie können das Labor jetzt leise verlassen.	

B.1.2 Translated Questionnaire for Dictators

Table B.2: Questionnaire for Dictators

Sex	Male Female Third
Field of studies / faculty:	Law Economics and social sciences Medical studies Comparative education Humanities Mathematics, informatics, natural sciences Psychology Else
Year of Birth (YYYY):	
Semester:	

Would you rather rate yourself as risk loving or risk averse?	Very risk loving Rather risk loving Risk neutral Rather risk averse Very risk averse	
Imagine you were in place of your recipient: When would you decide to suspend production?	Always (I would never participate) Starting from the safe amount of 20 Starting from the safe amount of 40 Starting from the safe amount of 60 Starting from the safe amount of 80 Only starting from the safe amount of 100 Never (I would always participate) Depending on the type of game	
How fair do you perceive the rules for your recipients?	Very fair Fair Rather fair Rather unfair Unfair Very unfair	
Please shortly explain your answer		
Please make a guess: In which game do the <u>most</u> participants of the subsequent experiment choose to suspend production for a safe amount of 40?		Lottery Contest Strategy
Please make a guess: In which game do the <u>least</u> participants of the subsequent experiment choose to suspend production for a safe amount of 40?		Lottery Contest Strategy
How important is fairness to you?	Not at all important Not important Important Very important	
When making your donation decision, to what extent did you consider the fairness of your recipient's payoff?	Not at all Little Rather little Rather strongly Strongly Very strongly	

Under which circumstances do you help someone in a difficult situation?	Under all circumstances. Only if the situation is self-imposed. Only if the situation is not self-imposed. Rather if the situation is self-imposed. Rather if the situation is not self-imposed. Under no circumstances.
Which option of social security do you prefer?	Unconditional basic income Conditional basic security such as Hartz IV Social security depending on working entitlements such as an unemployment insurance No social security, instead individual responsibility and donations in cases of emergency

Your payoff resulting from the first part of the experiment amounts in euros:
You will receive this payoff time-displaced, along with your payoff resulting from part 2.

Your payoff for part 2 will amount to between 5 and 10 euro. We will email you within the next 30 days to inform you about your payoff and the collection procedure.

Thank you for your participation. You may now silently leave the laboratory.

B.2 Questionnaire for Recipients

B.2.1 Original Questionnaire for Recipients in German

Table B.3: Questionnaire for Recipients (German)

Geschlecht	männlich weiblich divers
Studienrichtung/ Fakultät:	Rechtswissenschaft Wirtschafts- und Sozialwissenschaften Medizin Erziehungswissenschaften Geisteswissenschaften Mathe, Informatik, Naturwissenschaften Psychologie Sonstiges
Jahrgang (JJJJ):	
Semester:	
Würden Sie sich eher als risikofreudig oder risikoscheu einordnen?	sehr risikofreudig eher risikofreudig risikoneutral eher risikoscheu sehr risikoscheu
Angenommen, Sie würden in der Rolle Ihres Senders stecken: Wann würden Sie sich entscheiden, etwas zu spenden?	Nie (ich würde nie etwas spenden) Nur bei 0 als sicherer Betrag Bis 20 als sicherer Betrag Bis 40 als sicherer Betrag Bis 60 als sicherer Betrag Bis 80 als sicherer Betrag Immer (ich würde immer etwas spenden) Je nach Aufgabentyp verschieden

Wie fair fanden Sie die Spielregeln des Experimentes?	Sehr fair Fair Eher fair Eher unfair Unfair Sehr unfair
Schätzen Sie: Für welche Aufgabe ist der durchschnittliche Spendenbetrag aller Sender am **höchsten**?	Lotterie mit bindender Entscheidung Lotterie mit automatischem Aussetzen Wettbewerb mit bindender Entscheidung Wettbewerb mit automatischem Aussetzen Strategiespiel mit bindender Entscheidung Strategiespiel mit automatischem Aussetzen
Schätzen Sie: Für welche Aufgabe ist der durchschnittliche Spendenbetrag aller Sender am **niedrigsten**?	Lotterie mit bindender Entscheidung Lotterie mit automatischem Aussetzen Wettbewerb mit bindender Entscheidung Wettbewerb mit automatischem Aussetzen Strategiespiel mit bindender Entscheidung Strategiespiel mit automatischem Aussetzen
Wann helfen Sie jemandem in einer schwierigen Situation?	In jedem Fall. Nur, wenn die Situation selbst verschuldet ist. Nur, wenn die Situation nicht selbst verschuldet ist. Eher, wenn die Situation selbst verschuldet ist. Eher, wenn die Situation nicht selbst verschuldet ist. In keinem Fall.
Welche der folgenden Varianten von sozialer Sicherheit bevorzugen Sie?	Bedingungsloses Grundeinkommen An Bedingungen geknüpfte Sicherung wie Hartz IV Von Erwerbsleistungen abhängige Sicherung wie Arbeitslosenversicherung Keinerlei soziale Sicherung, stattdessen indiviuelle Verantwortung und Spenden bei Notfällen

Die Auszahlung, die Sie durch Ihre Teilnahme am Experiment verdient haben, beträgt in Euro:

Vielen Dank für Ihre Teilnahme. Bitte warten Sie, bis wir Sie einzeln zur Auszahlung aufrufen.

B.2.2 Translated Questionnaire for Recipients

Table B.4: Questionnaire for Recipients

Sex	Male Female Third
Field of studies / faculty:	Law Economics and social sciences Medical studies Comparative education Humanities Mathematics, informatics, natural sciences Psychology Else
Year of Birth (YYYY):	
Semester:	
Would you rather rate yourself as risk loving or risk averse?	Very risk loving Rather risk loving Risk neutral Rather risk averse Very risk averse
Imagine you were in place of your donor: When would you decide to donate something?	Never (I would never donate anything) Only with 0 as safe amount Up to 20 as a safe amount Up to 40 as a safe amount Up to 60 as a safe amount Up to 80 as a safe amount Always (I would always donate something) Depending on the type of game
How fair do you perceive the rules of the experiment?	Very fair Fair Rather fair Rather unfair Unfair Very unfair

Continuation of Table B.4

Estimate: For which task is the average donation amount of all donors the **highest**?	Lottery with binding decision Lottery with automatic suspension Contest with binding decision Contest with automatic suspension Strategic game with binding decision Strategic game with automatic suspension
Estimate: For which task is the average donation amount of all donors the **lowest**?	Lottery with binding decision Lottery with automatic suspension Contest with binding decision Contest with automatic suspension Strategic game with binding decision Strategic game with automatic suspension
Under which circumstances do you help someone in a difficult situation?	Under all circumstances. Only if the situation is self-imposed. Only if the situation is not self-imposed. Rather if the situation is self-imposed. Rather if the situation is not self-imposed. Under no circumstances.
Which option of social security do you prefer?	Unconditional basic income Conditional basic security such as Hartz IV Social security depending on working entitlements such as an unemployment insurance No social security, instead individual responsibility and donations in cases of emergency

The payoff you earned by participating in the experiment is in euros:

Thank you for your participation. Please wait until we call you individually for payment.

B.3 Template for Dictators' Result Printout

B.3.1 Original Template for Dictators' Result Printout in German

Session: «Session» Kabinennummer: «Kabine_Nr»

Für Sie und Ihren Empfänger wurde Runde «Relevante_Runde_in_Text» zufällig als auszahlungsrelevant ausgewählt. In dieser Runde haben Sie «Höhe_der_-Spende» Punkte als Spende ausgewählt. Ihr Empfänger ist in dieser Runde «Spendenberechtigt_Text» spendenberechtigt, da er «Spendenberechtigt_Text» «Spielregel_-wann_Spende» und erhält Ihre Spende daher «Spendenberechtigt_Text». Heute erhalten Sie außerdem die «Höhe_KerschbamerAuszahlung_KB» Punkte, die Sie in Teil 1 des Experimentes verdient haben. Ihre Auszahlung beträgt somit «Auszahlung_-Punkte_200SpendeSB_KB» Punkte, also «Auszahlung_Euro_Punkte_005» Euro.

Vielen Dank für Ihre Teilnahme!

B.3.2 Translated Template for Dictators' Result Printout

Session: «Session» Cubicle number: «Cubicle_No»

For you and your recipient, round «Relevant_round_in_text» was randomly selected as payoff relevant. In this round, you have selected «Amount_of_donation» points as donation. Your recipient is «eligible_text» eligible for donations in this round because he did «eligible_text» «Treatmentrule_when_donation» production and therefore does «eligible_text» receive your donation. Today you will also receive the «Amount_KerschbamerPayoff_KB» points you earned in part 1 of the experiment. Your payoff is therefore «Payout_points_200donationSB_KB» points, i.e. «Payout_Euro_points_005» Euro.

Thank you for your participation!

C Additional Regression Tables

Table C.1: List of Variables and their Abbreviations

Abbreviation	Variable Name	Short Description
Main treatment variables		
No Ex	No Exclusion	categorical treatment variable
Ex	Exclusion	categorical treatment variable
S	Strategy	production game variable (categorical) for the strategic game
L	Lottery	production game variable (categorical) for the lottery
C	Contest	production game variable (categorical) for the contest
S.A.	safeamount	continuous variable for alternative income
Elicited controls		
WTP^a	WTP^a	continuous variable for advantageous inequality aversion
WTP^d	WTP^d	continuous variable for disadvantageous inequality aversion
Socio-demographic controls		
Age	Age	continuous variable year of birth

Table continues.

Continuation of Table C.1

Abbreviation	Variable Name	Short Description
Semester	Semester	Continuous variable semester
gender		
Male	Male	categorical variable for gender
Female	Female	categorical variable for gender
field of studies		
Econstud	Economicstudent=1	categorical variable: field of studies = Economics
NoEcon	Economicstudent=0	categorical variable: field of studies ≠ Economics
Attitudes		
hypownpause	hypownpause	ordinal variable capturing hypothetical suspension of production
donationfair (dropped)		ordinal variable on fairness consideration in donation decision. Dropped due to collinearity
fairnesspref: ordinal variable for self-stated fairness-preference		
fairzeroimp	fairnesspref=1	fairness not at all important
fairnotimp	fairnesspref=2	fairness unimportant
fairimp	fairnesspref=3	fairness important
fairvimp	fairnesspref=4	fairness very important
Expfairness: categorical variable on fairness of experimental rules		
Expunfair	Expfairness=0	experimental rules unfair
Expfair	Expfairness=1	experimental rules fair

Table continues.

Continuation of Table C.1

Abbreviation	Variable Name	Short Description
socialsecpref: categorical variable for social security preference		
NoSoSec	socialsecpref=1	preference for no social security
EarnSoSec	socialsecpref=2	preference for social security depending on desert
ConSoSec	socialsecpref=3	preference for conditional social security
UncSoSec	socialsecpref=4	preference for unconditional basic income
responsibility: categorical variable on preference when to help someone		
rIresp	responsibility=2	preference to help rather when situation not due to own responsibility
Iresp	responsibility=3	preference to help only when situation not due to own responsibility
rResp	responsibility=4	preference to help rather when situation due to own responsibility
Resp	responsibility=5	preference to help only when situation due to own responsibility
uncResp	responsibility=6	preference to help always, irrespective of responsibility
riskpref: ordinal variable for self-stated risk-preference		
$vrisk^A$	riskpref=1	very risk averse
$risk^A$	riskpref=2	risk averse

Table continues.

Continuation of Table C.1

Abbreviation	Variable Name	Short Description
$risk^N$	riskpref=3	risk neutral
$risk^L$	riskpref=4	risk loving
$vrisk^L$	riskpref=5	very risk loving
easiestprod		
LotEasy	easiestprod=2	categorical variable: guess that the lottery is played most often for a safe amount of 40 points
StratEasy	easiestprod=1	categorical variable: guess that the strategic game is played most often for a safe amount of 40 points
ContEasy	easiestprod=3	categorical variable: guess that the contest played most often for a safe amount of 40 points
hardestprod (dropped)		
LotHard	hardestprod=2	categorical variable: guess that the lottery is played least often for a safe amount of 40 points
StratHard	hardestprod=1	categorical variable: guess that the strategic game is played least often for a safe amount of 40 points
ContHard	hardestprod=3	categorical variable: guess that the contest is played least often for a safe amount of 40 points

Table C.2: Mean of Total Donations by Safe Amount

Safe Amount	0	20	40	60	80	100
No Ex	22.03704	15.23148	12.5	12.12963	11.25	10.50926
	(3.192804)	(2.477298)	(2.181116)	(1.952627)	(1.890619)	(2.262364)
Ex	29.73148	24.75926	19.93519	17.16667	13.99074	12.55556
	(3.102317)	(2.695293)	(2.261591)	(2.189116)	(1.906679)	(2.103884)
Difference	-7.694444	-9.527778	-7.435185	-5.037037	-2.740741	-2.046296
	(4.451783)	(3.66082)	(3.141984)	(2.933425)	(2.68512)	(3.089437)
Mann-Whitney-U-test	0.0071	0.0008	0.0002	0.0129	0.1019	0.0536
	(2.693)	(3.361)	(3.666)	(2.487)	(1.636)	(1.930)
T-test	0.0854	0.0099	0.0189	0.0874	0.3085	0.5085
	(1.7284)	(2.6026)	(2.3664)	(1.7171)	(1.0207)	(0.7458)

The table shows the mean values of donations per treatment with standard errors in parentheses, and the difference of means between treatments. The last columns display the p-values of the difference between treatments as reported by a Mann-Whitney-U-test and T-test, respectively, with the z- and t-value in parentheses.

Table C.3: Donation Probability by Safe Amount

Safe Amount	0	20	40	60	80	100
No Ex	0.4444444	0.4259259	0.3425926	0.3611111	0.3518519	0.212963
	(0.0480375)	(0.0478034)	(0.045879)	(0.0464345)	(0.0461663)	(0.0395784)
Ex	0.6666667	0.6481481	0.6388889	0.5648148	0.4722222	0.3611111
	(0.0455724)	(0.0461663)	(0.0464345)	(0.047929)	(0.0482622)	(0.0464345)
Difference	-0.2222222	-0.2222222	-0.2962963	-0.2037037	-0.1203704	-0.1481481
	(0.0662152)	(0.0664567)	(0.0652767)	(0.0667335)	(0.0667875)	(0.0610132)
Mann-Whitney-U-test	0.0010	0.0011	0.0000	0.0027	0.0730	0.0164
	(3.279)	(3.267)	(4.345)	(2.995)	(1.793)	(2.401)
T-test	0.0009	0.0010	0.0000	0.0026	0.0729	0.0160
	(3.3561)	(3.3439)	(4.5391)	(3.0525)	(1.8023)	(2.4281)

The table shows the donation probability per treatment with standard errors in parentheses, and the difference of donation probability between treatments. The last columns display the p-values of the difference between treatments as reported by a Mann-Whitney-U-test and T-test, respectively, with the z- and t-value in parentheses.

Table C.4: Mean of Donation Level by Safe Amount (conditional sample)

Safe Amount	0	20	40	60	80	100
No Ex	49.58333	35.76087	36.48649	33.58974	31.97368	49.34783
	(4.812148)	(4.23628)	(4.118482)	(3.280359)	(3.386755)	(5.456478)
Ex	44.59722	38.2	31.2029	30.39344	29.62745	34.76923
	(3.524043)	(3.152744)	(2.72524)	(2.903576)	(2.685029)	(3.766903)
Difference	4.986111	-2.43913	5.283588	3.196301	2.346233	14.5786
	(5.835527)	(5.187526)	(4.788722)	(4.480549)	(4.26812)	(6.448257)
Mann-Whitney-U-test	0.5294	0.3422	0.2802	0.3188	0.7835	0.0210
	(0.629)	(0.950)	(1.080)	(0.997)	(0.275)	(2.307)
T-test	0.3946	0.6391	0.2724	0.4773	0.5839	0.0274
	(0.8544)	(0.4702)	(1.1033)	(0.7134)	(0.5497)	(2.2609)

The table shows the mean values of donation levels per treatment (conditional sample with $Donation \geq 1$) with standard errors in parentheses, and the difference of means between treatments. The last columns display the p-values of the difference between treatments as reported by a Mann-Whitney-U-test and T-test, respectively, with the z- and t-value in parentheses.

Table C.5: Recipients' Participation by Safe Amount

Safe Amount	0	20	40	60	80	100
No Ex	0.9907407	0.9907407	0.9444444	0.5277778	0.0833333	0.0185185
	(0.0092593)	(0.0092593)	(0.0221442)	(0.0482622)	(0.0267192)	(0.0130332)
Ex	0.9814815	0.9722222	0.962963	0.6481481	0.1851852	0.0462963
	(0.0130332)	(0.0158869)	(0.0182571)	(0.0461663)	(0.0375527)	(0.0203137)
Difference	0.0092593	0.0185185	-0.0185185	-0.1203704	-0.1018519	-0.0277778
	(0.0159875)	(0.0183883)	(0.0286999)	(0.0667875)	(0.0460881)	(0.0241352)
Mann-Whitney-U-test	0.5619	0.3139	0.5182	0.0730	0.0285	0.2501
	(0.580)	(1.007)	(0.646)	(1.793)	(2.190)	(1.150)
T-test	0.5631	0.3150	0.5195	0.0729	0.0282	0.2510
	(0.5792)	(1.0071)	(0.6452)	(1.8023)	(2.2099)	(1.1509)

The table shows the mean values of recipients' participation decisions per treatment with standard errors in parentheses, and the difference of means between treatments. The last columns display the p-values of the difference between treatments as reported by a Mann-Whitney-U-test and T-test, respectively, with the z- and t-value in parentheses.

C.1 Complete Regression Tables from Section 5.2.2

Table C.6: OLS-Regression on Determinants of Total Donations

	I	II	III	IV	V
Exclusion	5.75***	4.88***	5.69***	6.25***	6.26***
(0=No Ex, 1=Ex)	(1.38)	(1.38)	(1.45)	(1.53)	(1.70)
Contest	1.03	1.03	1.03	1.03	1.03
(0=Lottery, 1=Contest)	(1.69)	(1.67)	(1.68)	(1.54)	(1.52)
Strategy	-0.61	-0.61	-0.61	-0.61	-0.61
(0=Lottery, 1=Strategy)	(1.69)	(1.67)	(1.68)	(1.54)	(1.52)
safeamount	-0.14***	-0.14***	-0.14***	-0.14***	-0.14***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
WTP^a		0.00			-1.39***

Continuation of Table C.6

I	II	III	IV	V
	(0.38)			(0.44)
WTP^d	-10.03***			-10.23***
	(1.81)			(1.89)
Female		1.63		-2.52
(0=Male,1=Female)		(1.42)		(1.65)
Econstud		-2.55*		-3.91**
(0=NoEcon,1=Econstud)		(1.50)		(1.82)
Age		-0.15		-0.29
		(0.18)		(0.21)
Semester		-0.38**		-0.47**
		(0.17)		(0.21)
fairnotimp			-8.35***	-5.12
(0=fairimp,1=fairnotimp)			(2.95)	(3.22)
fairvimp			5.53***	6.44***
(0=fairimp,1=fairvimp)			(1.69)	(1.80)
Expfair			5.42***	4.36***
(0= Expun-fair,1=Expfair)			(1.46)	(1.56)
EarnSoSec			-7.65	-8.55
(0=NoSoSec,1=EarnSoSec)			(4.83)	(5.40)
ConSoSec			0.65	0.89
(0=NoSoSec,1=ConSoSec)			(4.39)	(4.79)
UncSoSec			-2.67	-4.16
(0=NoSoSec,1=UncSoSec)			(4.41)	(4.91)
Iresp			11.82***	13.01***
(0=rIresp,1=Iresp)			(2.91)	(3.35)
rResp			-4.29	-3.39

Continuation of Table C.6

	I	II	III	IV	V
(0=rIresp,1=rResp)				(4.57)	(5.05)
Resp				6.63*	10.18***
(0=rIresp,1=Resp)				(3.49)	(3.70)
uncResp				3.82**	4.38**
(0=rIresp,1=uncResp)				(1.73)	(1.77)
$vrisk^A$				-7.32	-3.43
(0=$risk^N$,1=$vrisk^A$)				(6.20)	(6.80)
$risk^A$				-6.06***	-5.12**
(0=$risk^N$,1=$risk^A$)				(1.82)	(2.18)
$risk^L$				-7.61***	-5.80***
(0=$risk^N$,1=$risk^L$)				(1.89)	(2.08)
$vrisk^L$				-3.37	-2.55
(0=$risk^N$,1=$vrisk^L$)				(3.06)	(3.27)
hypownpause				-0.74*	-0.81*
				(0.43)	(0.45)
StratEasy				19.10***	19.62***
(0=LotEasy,1=StratEasy)				(1.68)	(1.71)
ContEasy				7.37***	8.31***
(0=LotEasy,1=ContEasy)				(1.67)	(1.76)
constant	20.62***	28.28***	318.20	14.65***	602.32
	(1.71)	(2.27)	(362.16)	(5.60)	(423.08)
R2	0.047***	0.070***	0.055***	0.219***	0.244***

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$, $N = 1296$. Dependent variable is *Donation*, standard errors in parentheses. Assuming independence of observations.

Table C.7: RE Tobit-Regression on Determinants of Total Donations

	I	II	III	IV	V
Exclusion	22.49*	18.21	20.37	25.42**	24.84*
(0=No Ex, 1=Ex)	(12.81)	(12.31)	(13.03)	(12.81)	(13.70)
Contest	1.14	1.14	1.15	1.15	1.15
(0=Lottery, 1=Contest)	(2.10)	(2.11)	(2.11)	(2.11)	(2.11)
Strategy	-2.29	-2.29	-2.28	-2.28	-2.28
(0=Lottery, 1=Strategy)	(2.11)	(2.11)	(2.11)	(2.11)	(2.12)
safeamount	-0.33***	-0.33***	-0.33***	-0.33***	-0.34***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
WTP^a		1.96			-1.80
		(3.28)			(3.43)
WTP^d		-36.35**			-36.22**
		(16.11)			(15.11)
Female			11.00		4.59
(0=Male,1=Female)			(12.83)		(13.26)
Econstud			-5.47		-6.79
(0=NoEcon,1=Econstud)			(13.69)		(15.04)
Age			0.05		-1.11
			(1.66)		(1.72)
Semester			-1.75		-1.36
			(1.48)		(1.73)
fairnotimp				4.85	13.10
(0=fairimp,1=fairnotimp)				(26.53)	(27.56)
fairvimp				16.14	15.42
(0=fairimp,1=fairvimp)				(13.82)	(14.36)
Expfair				20.94*	16.01

Continuation of Table C.7

	I	II	III	IV	V
(0=Expunfair,1=Expfair)				(12.17)	(12.48)
EarnSoSec				-25.85	-26.05
(0=NoSoSec,1=EarnSoSec)				(40.06)	(43.78)
ConSoSec				-1.56	-3.64
(0=NoSoSec,1=ConSoSec)				(36.36)	(38.50)
UncSoSec				-1.76	-2.70
(0=NoSoSec,1=UncSoSec)				(36.48)	(39.74)
Iresp				27.19	22.04
(0=rIresp,1=Iresp)				(24.76)	(27.46)
rResp				-276.38	-263.34
(0=rIresp,1=rResp)				(48505.73)	(47999.92)
Resp				6.99	24.75
(0=rIresp,1=Resp)				(29.04)	(30.09)
uncResp				10.25	9.97
(0=rIresp,1=uncResp)				(14.34)	(14.38)
$vrisk^A$				-246.99	-235.64
(0=$risk^N$,1=$vrisk^A$)				(65722.52)	(67634.28)
$risk^A$				-6.73	-3.84
(0=$risk^N$,1=$risk^A$)				(15.33)	(18.16)
$risk^L$				-16.13	-7.85
(0=$risk^N$,1=$risk^L$)				(15.48)	(16.73)
$vrisk^L$				-34.08	-25.46
(0=$risk^N$,1=$vrisk^L$)				(25.65)	(26.67)
hypownpause				0.57	1.55
				(3.53)	(3.70)
StratEasy				38.71***	37.20***
(0=LotEasy,1=StratEasy)				(13.93)	(13.79)

Continuation of Table C.7

	I	II	III	IV	V
ContEasy				20.09	24.91*
(0=LotEasy,1=ContEasy)				(14.25)	(14.78)
constant	-8.56	17.17	-98.79	-38.45	2207.82
	(9.50)	(16.10)	(3305.43)	(46.23)	(3427.89)
σ_u	51.46***	49.41***	50.68***	41.94***	40.14***
	(5.89)	(5.63)	(5.77)	(4.80)	(4.58)
σ_e	23.55***	23.56***	23.56***	23.56***	23.57***
	(0.76)	(0.76)	(0.76)	(0.76)	(0.76)
Wald-chi2	165.46***	170.29***	168.55***	182.10***	187.11***

$^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$, $N = 1296$. Dependent variable is *Donation*, standard errors in parentheses. Assuming independence of observations. Lower limit of the RE Tobit-regression 0, upper limit 100.

Table C.8: OLS-Regression on Determinants of Donation Probability

	I	II	III	IV	V
Exclusion	0.202***	0.174***	0.209***	0.253***	0.272***
(0=NoEx, 1=Ex)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Contest	-0.009	-0.009	-0.009	-0.009	-0.009
(0=Lottery, 1=Contest)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Strategy	-0.021	-0.021	-0.021	-0.021	-0.021
(0=Lottery, 1=Strategy)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
safeamount	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
WTP^a		0.016**			-0.018**
		(0.01)			(0.01)
WTP^d		-0.267***			-0.296***
		(0.03)			(0.04)
Female			0.098***		0.080**
(0=Male,1=Female)			(0.03)		(0.03)
Econstud			-0.045		-0.047
(0=NoEcon,1=Econstud)			(0.03)		(0.03)
Age			-0.007**		-0.017***
			(0.00)		(0.00)
Semester			-0.012***		-0.011***
			(0.00)		(0.00)
fairnotimp				-0.011	0.088
(0=fairimp,1=fairnotimp)				(0.06)	(0.06)
fairvimp				0.140***	0.123***
(0=fairimp,1=fairvimp)				(0.03)	(0.03)
Expfair				0.157***	0.114***

Continuation of Table C.8

	I	II	III	IV	V
(0=Expunfair,1=Expfair)				(0.03)	(0.03)
EarnSoSec				-0.335***	-0.287***
(0=NoSoSec,1=EarnSoSec)				(0.09)	(0.10)
ConSoSec				-0.187**	-0.156*
(0=NoSoSec,1=ConSoSec)				(0.08)	(0.09)
UncSoSec				-0.180**	-0.115
(0=NoSoSec,1=UncSoSec)				(0.08)	(0.09)
Iresp				0.224***	0.160**
(0=rIresp,1=Iresp)				(0.06)	(0.06)
rResp				-0.337***	-0.346***
(0=rIresp,1=rResp)				(0.09)	(0.10)
Resp				0.041	0.215***
(0=rIresp,1=Resp)				(0.07)	(0.07)
uncResp				0.076**	0.064*
(0=rIresp,1=uncResp)				(0.03)	(0.03)
$vrisk^A$				-0.247**	-0.268**
(0=$risk^N$,1=$vrisk^A$)				(0.12)	(0.13)
$risk^A$				-0.114***	-0.082**
(0=$risk^N$,1=$risk^A$)				(0.03)	(0.04)
$risk^L$				-0.235***	-0.178***
(0=$risk^N$,1=$risk^L$)				(0.04)	(0.04)
$vrisk^L$				-0.331***	-0.279***
(0=$risk^N$,1=$vrisk^L$)				(0.06)	(0.06)
hypownpause				0.004	0.010
				(0.01)	(0.01)
StratEasy				0.279***	0.273***
(0=LotEasy,1=StratEasy)				(0.03)	(0.03)

Continuation of Table C.8

	I	II	III	IV	V
ContEasy				0.129***	0.189***
(0=LotEasy,1=ContEasy)				(0.03)	(0.03)
constant	0.491***	0.675***	14.854**	0.472***	33.590***
	(0.03)	(0.04)	(6.96)	(0.11)	(7.99)
R2	0.071	0.119	0.095	0.257	0.299
N	1296	1296	1296	1242	1242

$^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Dependent variable is *Donation-binary*, standard errors in parentheses. Assuming independence of observations.

Table C.9: RE Probit Regression on Determinants of Donation Probability

	I	II	III	IV	V
Exclusion	1.70**	1.43**	1.65**	1.80***	1.93***
(0=NoEx, 1=Ex)	(0.71)	(0.64)	(0.70)	(0.62)	(0.66)
Contest	-0.07	-0.07	-0.07	-0.07	-0.07
(0=Lottery, 1=Contest)	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)
Strategy	-0.17	-0.17	-0.17	-0.17	-0.17
(0=Lottery, 1=Strategy)	(0.14)	(0.14)	(0.14)	(0.14)	(0.14)
safeamount	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
WTP^a		0.16			-0.08
		(0.16)			(0.17)
WTP^d		-1.87**			-1.93***
		(0.84)			(0.73)
Female			0.58		0.50
(0=Male,1=Female)			(0.69)		(0.63)
Econstud			-0.46		-0.37
(0=NoEcon,1=Econstud)			(0.72)		(0.71)
Age			-0.05		-0.11
			(0.09)		(0.08)
Semester			-0.12		-0.07
			(0.08)		(0.08)
fairnotimp				0.42	0.84
(0=fairimp,1=fairnotimp)				(1.27)	(1.30)
fairvimp				1.01	0.73
(0=fairimp,1=fairvimp)				(0.68)	(0.69)
Expfair				1.34**	1.06*

Continuation of Table C.9

	I	II	III	IV	V
(0=Expunfair,1=Expfair)				(0.59)	(0.59)
EarnSoSec				-2.68	-2.24
(0=NoSoSec,1=EarnSoSec)				(1.96)	(2.16)
ConSoSec				-1.88	-1.59
(0=NoSoSec,1=ConSoSec)				(1.77)	(1.92)
UncSoSec				-1.70	-1.15
(0=NoSoSec,1=UncSoSec)				(1.78)	(1.98)
Iresp				1.01	0.65
(0=rIresp,1=Iresp)				(1.19)	(1.29)
rResp				0.00	0.00
(0=rIresp,1=rResp)				(.)	(.)
Resp				0.74	1.85
(0=rIresp,1=Resp)				(1.40)	(1.44)
uncResp				0.36	0.27
(0=rIresp,1=uncResp)				(0.69)	(0.68)
$vrisk^A$				0.00	0.00
(0=$risk^N$,1=$vrisk^A$)				(.)	(.)
$risk^A$				-0.59	-0.33
(0=$risk^N$,1=$risk^A$)				(0.74)	(0.88)
$risk^L$				-1.35*	-0.91
(0=$risk^N$,1=$risk^L$)				(0.76)	(0.80)
$vrisk^L$				-2.77**	-2.26*
(0=$risk^N$,1=$vrisk^L$)				(1.25)	(1.27)
hypownpause				0.03	0.08
				(0.17)	(0.17)
StratEasy				1.82***	1.77***
(0=LotEasy,1=StratEasy)				(0.68)	(0.66)

Continuation of Table C.9

	I	II	III	IV	V
ContEasy				0.96	1.38*
(0=LotEasy,1=ContEasy)				(0.68)	(0.70)
constant	-0.55	0.73	104.47	-0.11	224.87
	(0.50)	(0.83)	(178.95)	(2.20)	(163.65)
lnsig2u	1.90***	1.80***	1.85***	1.40***	1.26***
	(0.28)	(0.27)	(0.27)	(0.27)	(0.27)
Wald-chi2	90.63***	96.38***	94.49***	109.59***	113.36***
N	1296	1296	1296	1242	1242

$^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Dependent variable is *Donation*, standard errors in parentheses. Assuming independence of observations.

Table C.10: OLS-Regression on Determinants of Donation Level

	I	II	III	IV	V
Exclusion	-3.75*	-3.64*	-4.63**	-4.07	-3.40
(0=NoEx, 1=Ex)	(2.17)	(2.20)	(2.23)	(2.97)	(2.92)
Contest	2.79	2.71	2.86	1.72	2.20
(0=Lottery, 1=Contest)	(2.58)	(2.58)	(2.57)	(2.43)	(2.28)
Strategy	0.19	0.02	0.19	-0.24	-0.48
(0=Lottery, 1=Strategy)	(2.59)	(2.59)	(2.59)	(2.43)	(2.28)
safeamount	-0.11***	-0.10***	-0.11***	-0.13***	-0.15***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
WTP^a		-0.78			-1.44**
		(0.49)			(0.64)
WTP^d		-1.45			-6.05*
		(2.69)			(3.22)
Female			-5.74***		-24.47***
(0=Male,1=Female)			(2.21)		(2.88)
Econstud			-3.01		-17.49***
(0=NoEcon,1=Econstud)			(2.44)		(3.71)
Age			0.18		0.43
			(0.33)		(0.38)
Semester			0.27		-0.09
			(0.33)		(0.36)
fairnotimp				-20.22***	-5.09
(0=fairimp,1=fairnotimp)				(6.18)	(6.77)
fairvimp				3.29	14.14***
(0=fairimp,1=fairvimp)				(2.57)	(2.90)
Expfair				-3.20	5.05*

Continuation of Table C.10

	I	II	III	IV	V
(0=Expunfair,1=Expfair)				(2.62)	(2.67)
EarnSoSec				8.02	-12.79
(0=NoSoSec,1=EarnSoSec)				(7.44)	(8.84)
ConSoSec				11.39*	0.38
(0=NoSoSec,1=ConSoSec)				(6.70)	(7.35)
UncSoSec				5.44	-16.17**
(0=NoSoSec,1=UncSoSec)				(6.66)	(7.44)
Iresp				16.28***	25.64***
(0=rIresp,1=Iresp)				(6.02)	(6.52)
Resp				13.45**	-6.90
(0=rIresp,1=Resp)				(6.52)	(7.13)
uncResp				8.86***	8.27***
(0=rIresp,1=uncResp)				(3.10)	(3.16)
$risk^A$				-3.29	4.91
(0=$risk^N$,1=$risk^A$)				(3.37)	(4.15)
$risk^L$				1.31	0.92
(0=$risk^N$,1=$risk^L$)				(3.01)	(3.32)
$vrisk^L$				17.52***	32.59***
(0=$risk^N$,1=$vrisk^L$)				(5.37)	(6.69)
hypownpause				-2.00***	-1.57*
				(0.74)	(0.84)
StratEasy				18.22***	21.61***
(0=LotEasy,1=StratEasy)				(3.20)	(3.17)
ContEasy				7.76**	4.15
(0=LotEasy,1=ContEasy)				(3.33)	(3.49)
constant	42.70***	44.79***	-316.80	28.82***	-807.05
	(2.68)	(3.47)	(651.06)	(10.30)	(768.51)

Continuation of Table C.10

	I	II	III	IV	V
R2	0.026***	0.030***	0.039***	0.168***	0.279***

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 593$ (only observations with donations>0 included). Dependent variable is *Donation*≥ 1, standard errors in parentheses. Assuming independence of observations.

Table C.11: RE Tobit-Regression on Determinants of Donation Level

	I	II	III	IV	V
Exclusion	-4.65	-5.13	-4.70	-8.81	-7.12
(0=NoEx, 1=Ex)	(6.14)	(6.14)	(6.19)	(7.12)	(6.97)
Contest	3.07	3.08	3.07	2.95	3.05
(0=Lottery, 1=Contest)	(1.90)	(1.90)	(1.90)	(1.90)	(1.90)
Strategy	-0.27	-0.29	-0.27	-0.31	-0.29
(0=Lottery, 1=Strategy)	(1.89)	(1.89)	(1.89)	(1.89)	(1.89)
safeamount	-0.21***	-0.21***	-0.21***	-0.21***	-0.21***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
WTP^a		-0.57			-0.95
		(1.39)			(1.60)
WTP^d		-6.88			-7.60
		(7.68)			(7.41)
Female			-4.97		-19.11***
(0=Male,1=Female)			(6.22)		(6.63)
Econstud			-3.81		-16.22**
(0=NoEcon,1=Econstud)			(6.83)		(7.99)
Age			-0.14		0.09
			(0.89)		(0.92)
Semester			0.27		-0.24
			(0.89)		(0.84)
fairnotimp				-23.98	-15.72
(0=fairimp,1=fairnotimp)				(15.08)	(15.78)
fairvimp				6.61	12.22*
(0=fairimp,1=fairvimp)				(6.65)	(6.72)
Expfair				-6.34	-0.85

Continuation of Table C.11

	I	II	III	IV	V
(0=Expunfair,1=Expfair)				(6.36)	(6.06)
EarnSoSec				11.25	-10.71
(0=NoSoSec,1=EarnSoSec)				(21.43)	(22.68)
ConSoSec				10.90	-1.76
(0=NoSoSec,1=ConSoSec)				(19.76)	(19.49)
UncSoSec				6.27	-14.63
(0=NoSoSec,1=UncSoSec)				(19.43)	(19.63)
Iresp				13.17	21.86
(0=rIresp,1=Iresp)				(14.94)	(15.84)
Resp				2.40	-5.62
(0=rIresp,1=Resp)				(16.00)	(16.47)
uncResp				9.65	10.32
(0=rIresp,1=uncResp)				(7.85)	(7.52)
$risk^A$				-6.31	2.17
(0=$risk^N$,1=$risk^A$)				(8.57)	(9.82)
$risk^L$				-1.79	1.30
(0=$risk^N$,1=$risk^L$)				(7.92)	(8.19)
$vrisk^L$				23.94	40.08**
(0=$risk^N$,1=$vrisk^L$)				(14.97)	(16.32)
hypownpause				-1.94	-1.63
				(1.89)	(1.92)
StratEasy				20.08***	20.81***
(0=LotEasy,1=StratEasy)				(7.46)	(6.95)
ContEasy				7.70	4.98
(0=LotEasy,1=ContEasy)				(8.32)	(8.39)
constant	43.05***	48.72***	318.58	33.66	-115.00
	(4.88)	(7.74)	(1769.45)	(27.70)	(1836.35)

Continuation of Table C.11

	I	II	III	IV	V
σ_u	20.31***	20.16***	20.08***	16.92***	14.91***
	(2.25)	(2.23)	(2.23)	(1.90)	(1.75)
σ_e	18.37***	18.36***	18.37***	18.34***	18.36***
	(0.59)	(0.59)	(0.59)	(0.59)	(0.59)
Wald-chi2	73.94***	74.81***	74.93***	91.82***	106.04***

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 593$ (only observations with donations>0 included). Dependent variable is *Donation*≥ 1, standard errors in parentheses. Assuming independence of observations. Lower limit of the RE Tobit-regression 1, upper limit 100.

C.2 Complete Tables from Section 5.2.3

Table C.12: OLS-Regression on Total Donations by Production Choice

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
Exclusion	2.43	-0.27	6.89***	9.15***	7.92***	9.90***
(0=NoEx, 1=Ex)	(2.45)	(3.09)	(2.48)	(2.97)	(2.23)	(2.74)
safeamount	-0.14***	-0.14***	-0.13***	-0.13***	-0.14***	-0.14***
	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)
WTP^a		-1.61**		-0.58		-1.99***
		(0.79)		(0.76)		(0.71)
WTP^d		-10.93***		-6.07*		-13.69***
		(3.44)		(3.31)		(3.05)
Female		-2.87		-4.46		-0.23
(0=Male,1=Female)		(3.01)		(2.89)		(2.67)
Econstud		-2.18		-7.20**		-2.35
(0=NoEcon,1=Econstud)		(3.31)		(3.18)		(2.94)
Age		0.52		-0.56		-0.82**
		(0.39)		(0.37)		(0.34)
Semester		-0.13		-0.61*		-0.67*
		(0.38)		(0.37)		(0.34)
fairnotimp		1.00		-13.20**		-3.17
(0=fairimp,1=fairnotimp)		(5.86)		(5.64)		(5.20)
fairvimp		10.38***		1.51		7.44**
(0=fairimp,1=fairvimp)		(3.27)		(3.15)		(2.91)
Expfair		3.98		7.58***		1.53
(0=Expunfair,1=Expfair)		(2.84)		(2.74)		(2.53)
EarnSoSec		-3.41		-16.88*		-5.34
(0=NoSoSec,1=EarnSoSec)		(9.84)		(9.46)		(8.73)

Continuation of Table C.12

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
ConSoSec		-3.47		-1.15		7.28
(0=NoSoSec,1=ConSoSec)		(8.73)		(8.40)		(7.76)
UncSoSec		-7.46		-5.82		0.80
(0=NoSoSec,1=UncSoSec)		(8.95)		(8.61)		(7.95)
Iresp		11.12*		21.10***		6.82
(0=rIresp,1=Iresp)		(6.09)		(5.86)		(5.41)
rResp		-4.05		0.02		-6.13
(0=rIresp,1=rResp)		(9.19)		(8.84)		(8.16)
Resp		15.72**		7.72		7.10
(0=rIresp,1=Resp)		(6.74)		(6.48)		(5.99)
uncResp		6.39**		7.12**		-0.37
(0=rIresp,1=uncResp)		(3.22)		(3.10)		(2.86)
$vrisk^A$		-7.56		0.68		-3.41
(0=$risk^N$,1=$vrisk^A$)		(12.38)		(11.91)		(11.00)
$risk^A$		-4.73		-4.49		-6.13*
(0=$risk^N$,1=$risk^A$)		(3.98)		(3.83)		(3.53)
$risk^L$		-3.57		-7.14*		-6.69**
(0=$risk^N$,1=$risk^L$)		(3.79)		(3.64)		(3.36)
$vrisk^L$		-6.92		0.17		-0.89
(0=$risk^N$,1=$vrisk^L$)		(5.96)		(5.73)		(5.29)
hypownpause		-0.35		-0.88		-1.21
		(0.83)		(0.80)		(0.74)
StratEasy		17.37***		23.59***		17.89***
(0=LotEasy,1=StratEasy)		(3.11)		(2.99)		(2.76)
ContEasy		5.25		8.99***		10.69***
(0=LotEasy,1=ContEasy)		(3.21)		(3.09)		(2.85)

Continuation of Table C.12

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
constant	22.45***	-1011.4	20.58***	1144.8	19.26***	1674.0**
	(2.50)	(770.71)	(2.52)	(741.38)	(2.27)	(684.46)
R2	0.036***	0.227***	0.044***	0.303***	0.069***	0.288***

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 432$. Dependent variable is *Donation*, standard errors in parentheses. Assuming independence of observations.

Table C.13: RE Tobit-Regression on Total Donations by Production Choice

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
Exclusion	16.60	14.92	15.53	20.01	22.91**	25.92**
(0=NoEx, 1=Ex)	(11.95)	(13.55)	(13.43)	(14.33)	(11.20)	(12.11)
safeamount	-0.33***	-0.33***	-0.30***	-0.30***	-0.33***	-0.34***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
WTP^a		-1.99		-1.93		-4.90
		(3.34)		(3.62)		(3.22)
WTP^d		-31.96**		-22.62		-37.65***
		(14.90)		(15.79)		(13.23)
Female		0.30		0.61		3.51
(0=Male,1=Female)		(13.19)		(13.89)		(11.68)
Econstud		-8.01		-8.18		1.17
(0=NoEcon,1=Econstud)		(14.88)		(15.87)		(13.38)
Age		0.32		-1.22		-1.42
		(1.72)		(1.79)		(1.51)
Semester		-1.04		-0.82		-0.64
		(1.72)		(1.81)		(1.53)
fairnotimp		22.40		-21.51		15.63
(0=fairimp,1=fairnotimp)		(27.28)		(30.19)		(24.54)
fairvimp		22.73		10.95		20.45
(0=fairimp,1=fairvimp)		(14.19)		(15.16)		(12.68)
Expfair		13.41		22.16*		10.68
(0=Expunfair,1=Expfair)		(12.47)		(13.16)		(10.98)
EarnSoSec		-25.92		-18.61		-8.45
(0=NoSoSec,1=EarnSoSec)		(43.56)		(45.70)		(38.29)
ConSoSec		-11.73		9.34		14.58

Continuation of Table C.13

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
(0=NoSoSec,1=ConSoSec)		(37.77)		(40.10)		(33.57)
UncSoSec		-12.20		5.48		3.44
(0=NoSoSec,1=UncSoSec)		(39.12)		(41.31)		(34.52)
Iresp		19.19		42.13		12.09
(0=rIresp,1=Iresp)		(27.54)		(29.24)		(24.39)
rResp		-213.79		-286.46		-201.54
(0=rIresp,1=rResp)		(5560.28)		(798528.75)		(5889.76)
Resp		11.75		22.85		18.75
(0=rIresp,1=Resp)		(32.24)		(31.75)		(26.57)
uncResp		13.05		16.04		3.99
(0=rIresp,1=uncResp)		(14.36)		(15.24)		(12.86)
$vrisk^A$		-175.68		-269.01		-179.39
(0=$risk^N$,1=$vrisk^A$)		(8281.51)		(882283.91)		(8247.36)
$risk^A$		-2.55		-2.30		-12.50
(0=$risk^N$,1=$risk^A$)		(18.34)		(19.43)		(16.29)
$risk^L$		-6.51		-9.87		-19.02
(0=$risk^N$,1=$risk^L$)		(16.54)		(17.70)		(15.12)
$vrisk^L$		-25.34		-8.79		-26.68
(0=$risk^N$,1=$vrisk^L$)		(26.34)		(28.07)		(24.06)
hypownpause		2.25		-0.31		-0.92
		(3.67)		(3.90)		(3.31)
StratEasy		36.07***		44.73***		30.58**
(0=LotEasy,1=StratEasy)		(13.77)		(14.73)		(12.28)
ContEasy		21.70		24.45		21.41
(0=LotEasy,1=ContEasy)		(15.02)		(15.75)		(13.25)
constant	-0.18	-638.97	-3.81	2405.75	-4.45	2832.43

Continuation of Table C.13

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
	(8.99)	(3430.81)	(10.02)	(3567.68)	(8.53)	(3008.15)
σ_u	46.94***	38.05***	52.89***	40.37***	43.47***	33.27***
	(5.62)	(4.57)	(6.72)	(5.22)	(5.36)	(4.20)
σ_e	20.73***	20.76***	21.34***	21.35***	21.35***	21.39***
	(1.22)	(1.23)	(1.24)	(1.25)	(1.25)	(1.26)
Wald-chi2	67.56***	86.35***	53.92***	75.51***	68.26***	89.63***

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 432$. Dependent variable is *Donation*, standard errors in parentheses. Assuming independence of observations. Lower limit of the RE Tobit-regression 0, upper limit 100.

Table C.14: OLS-Regression on Donation Probability by Production Choice

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
Exclusion	0.25***	0.29***	0.15***	0.24***	0.21***	0.29***
(0=NoEx, 1=Ex)	(0.05)	(0.06)	(0.05)	(0.06)	(0.05)	(0.06)
safeamount	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***	-0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
WTP^a		-0.01		-0.01		-0.03**
		(0.01)		(0.01)		(0.01)
WTP^d		-0.29***		-0.24***		-0.36***
		(0.06)		(0.06)		(0.06)
Female		0.11**		0.07		0.06
(0=Male,1=Female)		(0.05)		(0.05)		(0.05)
Econstud		-0.05		-0.10		0.00
(0=NoEcon,1=Econstud)		(0.06)		(0.06)		(0.06)
Age		-0.01		-0.02***		-0.02**
		(0.01)		(0.01)		(0.01)
Semester		-0.01		-0.01**		-0.01
		(0.01)		(0.01)		(0.01)
fairnotimp		0.26**		-0.16		0.16
(0=fairimp,1=fairnotimp)		(0.11)		(0.11)		(0.11)
fairvimp		0.15**		0.06		0.16***
(0=fairimp,1=fairvimp)		(0.06)		(0.06)		(0.06)
Expfair		0.06		0.18***		0.10*
(0=Expunfair,1=Expfair)		(0.05)		(0.05)		(0.05)
EarnSoSec		-0.34*		-0.28		-0.24
(0=NoSoSec,1=EarnSoSec)		(0.18)		(0.18)		(0.18)
ConSoSec		-0.24		-0.13		-0.09

Continuation of Table C.14

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
(0=NoSoSec,1=ConSoSec)		(0.16)		(0.16)		(0.16)
UncSoSec		-0.16		-0.06		-0.12
(0=NoSoSec,1=UncSoSec)		(0.16)		(0.16)		(0.16)
Iresp		0.04		0.35***		0.09
(0=rIresp,1=Iresp)		(0.11)		(0.11)		(0.11)
rResp		-0.41**		-0.25		-0.38**
(0=rIresp,1=rResp)		(0.17)		(0.17)		(0.17)
Resp		0.11		0.31**		0.22*
(0=rIresp,1=Resp)		(0.12)		(0.12)		(0.12)
uncResp		0.06		0.09		0.04
(0=rIresp,1=uncResp)		(0.06)		(0.06)		(0.06)
$vrisk^A$		-0.26		-0.24		-0.31
(0=$risk^N$,1=$vrisk^A$)		(0.22)		(0.23)		(0.22)
$risk^A$		-0.11		-0.03		-0.11
(0=$risk^N$,1=$risk^A$)		(0.07)		(0.07)		(0.07)
$risk^L$		-0.18***		-0.10		-0.26***
(0=$risk^N$,1=$risk^L$)		(0.07)		(0.07)		(0.07)
$vrisk^L$		-0.37***		-0.12		-0.34***
(0=$risk^N$,1=$vrisk^L$)		(0.11)		(0.11)		(0.11)
hypownpause		0.01		0.01		0.01
		(0.01)		(0.02)		(0.01)
StratEasy		0.24***		0.32***		0.26***
(0=LotEasy,1=StratEasy)		(0.06)		(0.06)		(0.06)
ContEasy		0.16***		0.21***		0.19***
(0=LotEasy,1=ContEasy)		(0.06)		(0.06)		(0.06)
constant	0.47***	22.42	0.50***	42.38***	0.47***	35.94***

Continuation of Table C.14

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
	(0.05)	(13.85)	(0.05)	(14.02)	(0.05)	(13.88)
R2	0.093***	0.328***	0.049***	0.310***	0.075***	0.320***

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 432$. Dependent variable is *Donation-binary*, standard errors in parentheses. Assuming independence of observations.

Table C.15: RE Probit-Regression on Donation Probability by Production Choice

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
Exclusion	1.75**	1.89**	1.73	2.22**	1.34**	1.65***
(0=NoEx, 1=Ex)	(0.73)	(0.75)	(1.24)	(0.96)	(0.60)	(0.63)
safeamount	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
WTP^a		0.01		-0.01		-0.21
		(0.21)		(0.26)		(0.17)
WTP^d		-2.20**		-2.36**		-2.19***
		(0.87)		(1.06)		(0.71)
Female		0.44		0.68		0.34
(0=Male, 1=Female)		(0.71)		(0.90)		(0.60)
Econstud		-0.47		-0.56		0.18
(0=NoEcon, 1=Econstud)		(0.79)		(0.99)		(0.66)
Age		-0.07		-0.18		-0.11
		(0.09)		(0.12)		(0.08)
Semester		-0.07		-0.10		-0.03
		(0.09)		(0.12)		(0.08)
fairnotimp		1.95		-2.05		0.85
(0=fairimp, 1=fairnotimp)		(1.53)		(1.91)		(1.24)
fairvimp		0.84		0.50		0.95
(0=fairimp, 1=fairvimp)		(0.79)		(0.98)		(0.66)
Expfair		0.80		1.85**		0.76
(0=Expunfair, 1=Expfair)		(0.67)		(0.86)		(0.56)

Continuation of Table C.15

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
EarnSoSec		-2.54		-1.65		-1.07
(0=NoSoSec, 1=EarnSoSec)		(2.45)		(3.02)		(2.02)
ConSoSec		-1.81		-0.85		-0.40
(0=NoSoSec, 1=ConSoSec)		(2.15)		(2.68)		(1.79)
UncSoSec		-1.39		-0.05		-0.46
(0=NoSoSec, 1=UncSoSec)		(2.24)		(2.78)		(1.85)
Iresp		0.14		2.78		0.51
(0=rIresp, 1=Iresp)		(1.46)		(1.90)		(1.26)
Resp		0.39		2.76		1.45
(0=rIresp, 1=Resp)		(1.78)		(2.02)		(1.39)
uncResp		0.25		0.46		0.13
(0=rIresp, 1=uncResp)		(0.79)		(0.98)		(0.66)
$risk^A$		-0.51		-0.40		-0.80
(0=$risk^N$, 1=$risk^A$)		(1.00)		(1.31)		(0.85)
$risk^L$		-1.13		-0.85		-1.52*
(0=$risk^N$, 1=$risk^L$)		(0.92)		(1.19)		(0.78)
$vrisk^L$		-2.69*		-1.06		-2.09*
(0=$risk^N$, 1=$vrisk^L$)		(1.45)		(1.81)		(1.25)
hypownpause		0.09		0.06		0.02
		(0.19)		(0.25)		(0.16)

Continuation of Table C.15

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
StratEasy		1.70**		2.71***		1.48**
(0=LotEasy, 1=StratEasy)		(0.77)		(0.96)		(0.63)
ContEasy		1.32		2.04**		1.15*
(0=LotEasy, 1=ContEasy)		(0.82)		(1.03)		(0.68)
constant	-0.28	139.74	-0.42	367.63	-0.35	213.88
	(0.52)	(183.21)	(0.75)	(229.94)	(0.45)	(154.75)
lnsig2u	1.95***	1.30***	2.51***	1.85***	1.61***	0.92***
	(0.36)	(0.36)	(0.40)	(0.36)	(0.33)	(0.34)
Wald-chi2	32.84***	44.15***	27.84***	45.06***	31.10***	45.86***
N	432	414	432	414	432	414

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 432$. Dependent variable is *Donation*, standard errors in parentheses. Assuming independence of observations.

Table C.16: OLS-Regression on Donation Level by Production Choice

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
Exclusion	-15.06***	-14.16***	2.47	3.30	1.41	0.66
(0=NoEx, 1=Ex)	(3.86)	(5.33)	(3.80)	(4.79)	(3.51)	(5.03)
safeamount	-0.12**	-0.16***	-0.08	-0.13**	-0.13**	-0.17***
	(0.06)	(0.05)	(0.06)	(0.05)	(0.05)	(0.05)
WTP^a		-1.16		-1.70*		-1.00
		(1.14)		(1.02)		(1.37)
WTP^d		-10.83*		0.92		-7.33
		(5.95)		(5.60)		(5.14)
Female		-26.61***		-31.59***		-16.76***
(0=Male, 1=Female)		(5.13)		(4.86)		(4.77)
Econstud		-10.63		-27.08***		-15.75***
(0=NoEcon, 1=Econstud)		(6.61)		(6.33)		(5.96)
Age		0.85		0.53		-0.58
		(0.73)		(0.62)		(0.65)
Semester		0.51		-0.20		-0.76
		(0.65)		(0.60)		(0.62)
fairnotimp		-1.21		-12.36		-16.24
(0=fairimp, 1=fairnotimp)		(11.54)		(14.43)		(11.23)
fairvimp		9.85*		17.84***		11.52**
(0=fairimp, 1=fairvimp)		(5.16)		(4.98)		(5.02)
Expfair		11.54**		6.64		-2.47

Continuation of Table C.16

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
(0=Expunfair, 1=Expfair)		(4.84)		(4.52)		(4.37)
EarnSoSec		-2.50		-30.40**		-8.15
(0=NoSoSec, 1=EarnSoSec)		(16.24)		(14.67)		(14.17)
ConSoSec		-0.19		-6.99		8.41
(0=NoSoSec, 1=ConSoSec)		(13.10)		(12.29)		(11.87)
UncSoSec		-14.43		-27.83**		-5.55
(0=NoSoSec, 1=UncSoSec)		(13.37)		(12.38)		(12.25)
Iresp		30.01**		43.16***		19.61*
(0=rIresp, 1=Iresp)		(12.39)		(12.09)		(10.33)
Resp		22.74		-20.56*		-6.87
(0=rIresp, 1=Resp)		(14.75)		(11.13)		(11.41)
uncResp		13.94**		11.51**		2.53
(0=rIresp, 1=uncResp)		(5.63)		(5.23)		(5.53)
$risk^A$		4.83		11.32		0.84
(0=$risk^N$, 1=$vrisk^A$)		(7.60)		(6.93)		(6.85)
$risk^L$		4.49		-3.89		6.08
(0=$risk^N$, 1=$risk^L$)		(5.89)		(5.48)		(5.99)
$vrisk^L$		26.75**		38.97***		31.98***
(0=$risk^N$, 1=$vrisk^L$)		(12.24)		(11.20)		(11.35)

Continuation of Table C.16

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
hypownpause		-1.32		-1.73		-2.69*
		(1.52)		(1.41)		(1.38)
StratEasy		14.79***		33.37***		17.41***
(0=LotEasy, 1=StratEasy)		(5.51)		(5.70)		(5.51)
ContEasy		-0.29		8.73		6.27
(0=LotEasy, 1=ContEasy)		(6.31)		(5.95)		(5.96)
constant	50.32***	-1642.14	40.74***	-995.68	40.54***	1224.87
	(3.97)	(1451.24)	(3.87)	(1240.38)	(3.50)	(1299.85)
R2	0.088***	0.365***	0.012***	0.407***	0.030***	0.325***
N	202	202	198	198	193	193

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Dependent variable is *Donation*≥ 1, standard errors in parentheses. Assuming independence of observations.

Table C.17: RE Tobit-Regression on Donation Level by Production Choice

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
Exclusion	-12.79*	-14.38*	0.11	1.19	0.13	-2.86
(0=NoEx, 1=Ex)	(7.21)	(8.64)	(7.24)	(7.33)	(6.24)	(6.98)
safeamount	-0.22***	-0.23***	-0.18***	-0.18***	-0.20***	-0.22***
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
WTP^a		-0.44		-1.74		-0.06
		(1.95)		(1.70)		(2.08)
WTP^d		-10.21		2.30		-9.14
		(9.43)		(8.20)		(7.28)
Female		-21.45**		-28.65***		-15.98**
(0=Male, 1=Female)		(8.50)		(7.27)		(6.70)
Econstud		-10.55		-26.78***		-17.43**
(0=NoEcon, 1=Econstud)		(10.01)		(8.80)		(7.95)
Age		0.33		0.41		-0.99
		(1.20)		(0.95)		(0.92)
Semester		0.17		-0.48		-1.03
		(1.05)		(0.90)		(0.87)
fairnotimp		-11.49		-16.17		-23.81
(0=fairimp, 1=fairnotimp)		(19.54)		(22.93)		(15.98)
fairvimp		8.52		18.76**		11.14
(0=fairimp, 1=fairvimp)		(8.59)		(7.63)		(6.96)
Expfair		3.18		4.12		-5.23
(0=Expunfair, 1=Expfair)		(7.89)		(6.68)		(5.99)

Continuation of Table C.17

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
EarnSoSec		-5.16		-26.98		-5.85
(0=NoSoSec, 1=EarnSoSec)		(28.51)		(23.14)		(21.50)
ConSoSec		-6.47		-6.36		7.61
(0=NoSoSec, 1=ConSoSec)		(23.46)		(19.86)		(18.42)
UncSoSec		-18.47		-25.27		-4.16
(0=NoSoSec, 1=UncSoSec)		(23.91)		(19.90)		(18.76)
Iresp		26.65		42.79**		23.48
(0=rIresp, 1=Iresp)		(20.10)		(18.75)		(15.49)
Resp		36.48		-18.94		-7.90
(0=rIresp, 1=Resp)		(26.37)		(16.71)		(15.94)
uncResp		15.92*		13.81*		5.05
(0=rIresp, 1=uncResp)		(9.23)		(7.87)		(7.74)
$risk^A$		5.66		12.23		1.98
(0=$risk^N$, 1=$vrisk^A$)		(12.69)		(10.83)		(9.99)
$risk^L$		6.05		-1.84		8.83
(0=$risk^N$, 1=$risk^L$)		(10.00)		(8.73)		(8.88)
$vrisk^L$		33.03*		43.20**		40.20**
(0=$risk^N$, 1=$vrisk^L$)		(19.89)		(17.55)		(16.44)
hypownpause		-1.67		-1.26		-2.67
		(2.37)		(2.07)		(1.94)

Continuation of Table C.17

	Lottery		Contest		Strategy	
	I	II	I	II	I	II
StratEasy		16.27*		34.03***		19.38**
(0=LotEasy, 1=StratEasy)		(8.73)		(8.39)		(7.56)
ContEasy		3.04		11.12		7.64
(0=LotEasy, 1=ContEasy)		(10.54)		(9.16)		(8.61)
constant	49.53***	-601.73	44.00***	-760.69	41.16***	2044.00
	(5.77)	(2406.74)	(5.68)	(1891.33)	(5.01)	(1830.73)
σ_u	22.54***	17.20***	22.07***	13.54***	18.85***	12.68***
	(2.70)	(2.27)	(2.84)	(2.15)	(2.41)	(1.88)
σ_e	16.68***	16.68***	17.32***	17.30***	15.96***	15.92***
	(1.00)	(1.01)	(1.02)	(1.02)	(0.96)	(0.96)
Wald-chi2	34.03***	59.41***	19.35***	64.27***	27.05***	63.20***
N	202	202	198	198	193	193

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Dependent variable is *Donation*≥ 1, standard errors in parentheses. Assuming independence of observations.

C.3 Complete Tables from Section 5.2.4

Table C.18: OLS-Regression on Total Donations by Safe Amount

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
Exclusion	7.69*	6.64	9.53**	9.14*	7.44**	10.04**	5.04*	5.44	2.74	3.79	2.05	2.52
(0=NoEx, 1=Ex)	(4.47)	(5.67)	(3.68)	(4.64)	(3.15)	(3.87)	(2.94)	(3.61)	(2.69)	(3.29)	(3.10)	(3.82)
Contest	-0.25	-0.25	-0.19	-0.19	3.01	3.01	0.78	0.78	2.21	2.21	0.61	0.61
(0=Lottery, 1=Contest)	(5.48)	(5.08)	(4.50)	(4.16)	(3.86)	(3.47)	(3.60)	(3.24)	(3.30)	(2.95)	(3.80)	(3.42)
Strategy	-1.51	-1.51	0.14	0.14	0.64	0.64	-1.88	-1.87	0.82	0.82	-1.89	-1.89
(0=Lottery, 1=Strategy)	(5.48)	(5.08)	(4.50)	(4.16)	(3.86)	(3.47)	(3.60)	(3.24)	(3.30)	(2.95)	(3.80)	(3.42)
WTP^a		-3.71**		-2.42**		-1.72*		-0.65		0.29		-0.17
		(1.46)		(1.19)		(1.00)		(0.93)		(0.85)		(0.98)
WTP^d		-9.50		-11.83**		-9.33**		-12.31***		-8.98**		-9.42**
		(6.31)		(5.17)		(4.31)		(4.02)		(3.66)		(4.25)

Continuation of Table C.18

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
Female		1.41		-2.47		-1.61		-4.45		-5.43*		-2.58
(0=Male, 1=Female)		(5.52)		(4.52)		(3.77)		(3.52)		(3.20)		(3.72)
Econstud		-1.74		-7.10		-8.63**		-7.48*		-2.19		3.67
(0=NoEcon, 1=Econstud)		(6.07)		(4.97)		(4.14)		(3.87)		(3.52)		(4.09)
Age		0.29		-0.28		-0.59		-0.42		-0.42		-0.31
		(0.71)		(0.58)		(0.48)		(0.45)		(0.41)		(0.48)
Semester		-0.08		-0.14		-0.41		-0.69		-0.62		-0.87*
		(0.70)		(0.57)		(0.48)		(0.45)		(0.41)		(0.47)
fairnotimp		5.29		-3.88		-7.16		-8.75		-9.02		-7.22
(0=fairimp, 1=fairnotimp)		(10.76)		(8.80)		(7.34)		(6.86)		(6.24)		(7.24)
fairvimp		10.96*		5.65		5.80		5.29		5.64		5.31

Continuation of Table C.18

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
(0=fairimp, 1=fairvimp)		(6.01)		(4.92)		(4.10)		(3.83)		(3.49)		(4.05)
Expfair		4.65		4.40		4.28		4.86		4.19		3.80
(0=Expunfair, 1=Expfair)		(5.22)		(4.27)		(3.56)		(3.33)		(3.03)		(3.51)
EarnSoSec		-23.24		8.94		-2.17		-4.22		-7.07		-23.52*
(0=NoSoSec, 1=EarnSoSec)		(18.05)		(14.77)		(12.32)		(11.51)		(10.47)		(12.16)
ConSoSec		-2.99		26.99**		12.96		3.70		-8.40		-26.92**
(0=NoSoSec, 1=ConSoSec)		(16.03)		(13.12)		(10.94)		(10.22)		(9.30)		(10.80)
UncSoSec		-15.77		18.69		5.22		-0.41		-8.47		-24.22**
(0=NoSoSec, 1=UncSoSec)		(16.42)		(13.44)		(11.21)		(10.47)		(9.53)		(11.06)
Iresp		6.47		15.99*		21.08***		17.21**		10.13		7.18

Continuation of Table C.18

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
(0=rIresp, 1=Iresp)		(11.19)		(9.15)		(7.63)		(7.13)		(6.49)		(7.53)
rResp		-19.98		-11.62		1.84		4.76		2.27		2.41
(0=rIresp, 1=rResp)		(16.87)		(13.81)		(11.51)		(10.75)		(9.79)		(11.36)
Resp		1.75		7.70		16.02*		12.72		13.25*		9.63
(0=rIresp, 1=Resp)		(12.37)		(10.13)		(8.44)		(7.89)		(7.18)		(8.33)
uncResp		0.88		4.55		6.01		7.90**		4.07		2.88
(0=rIresp, 1=uncResp)		(5.92)		(4.84)		(4.04)		(3.77)		(3.43)		(3.99)
$vrisk^A$		-4.71		5.85		6.51		-1.37		-8.58		-18.29
(0=$risk^N$, 1=$vrisk^A$)		(22.73)		(18.60)		(15.51)		(14.49)		(13.19)		(15.31)
$risk^A$		-6.80		-2.36		-1.40		-5.35		-4.96		-9.83**

Continuation of Table C.18

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
(0=$risk^N$, 1=$risk^A$)		(7.31)		(5.98)		(4.99)		(4.66)		(4.24)		(4.92)
$risk^L$		-2.23		-3.37		-6.20		-5.55		-7.06*		-10.40**
(0=$risk^N$, 1=$risk^L$)		(6.96)		(5.69)		(4.75)		(4.43)		(4.03)		(4.68)
$vrisk^L$		-10.37		3.08		1.95		0.14		-3.75		-6.33
(0=$risk^N$, 1=$vrisk^L$)		(10.94)		(8.95)		(7.46)		(6.97)		(6.34)		(7.36)
hypownpause		-2.69*		-2.06*		-0.43		0.32		0.51		-0.53
		(1.52)		(1.24)		(1.04)		(0.97)		(0.88)		(1.02)
StratEasy		23.32***		22.81***		22.01***		17.18***		16.47***		15.91***
(0=LotEasy, 1=StratEasy)		(5.71)		(4.67)		(3.89)		(3.64)		(3.31)		(3.84)
ContEasy		8.73		11.56**		8.91**		6.69*		6.77**		7.20*

Continuation of Table C.18

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
(0=LotEasy, 1=ContEasy)		(5.89)		(4.82)		(4.02)		(3.75)		(3.42)		(3.97)
constant	22.63***	-534.57	15.25***	554.56	11.28***	1183.67	12.50***	850.79	10.24***	858.34	10.94***	660.25
	(4.47)	(1414.79)	(3.68)	(1157.74)	(3.15)	(965.45)	(2.94)	(901.66)	(2.69)	(820.69)	(3.10)	(952.69)
R2	0.014	0.245***	0.031*	0.265***	0.029*	0.302***	0.016	0.293***	0.007	0.295***	0.004	0.280***

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 216$. Dependent variable is *Donation*, standard errors in parentheses. Assuming independence of observations.

Table C.19: RE Tobit-Regression on Total Donations by Safe Amount

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
Exclusion	22.74	25.59	21.75*	25.76*	22.60**	29.11**	15.62	18.12	10.46	13.19	26.37	23.18
(0=NoEx, 1=Ex)	(16.35)	(18.72)	(11.96)	(13.41)	(10.78)	(11.55)	(11.04)	(11.86)	(11.32)	(12.15)	(19.60)	(21.30)
Contest	-1.30	-1.25	-1.23	-1.27	5.54*	5.55*	0.68	0.74	3.95	3.97	2.56	2.27
(0=Lottery, 1=Contest)	(4.77)	(4.77)	(3.41)	(3.42)	(2.91)	(2.91)	(3.49)	(3.49)	(3.65)	(3.64)	(7.87)	(7.93)
Strategy	-3.44	-3.45	-0.66	-0.68	0.27	0.26	-4.27	-4.25	0.20	0.20	-5.58	-5.66
(0=Lottery, 1=Strategy)	(4.80)	(4.80)	(3.40)	(3.40)	(2.95)	(2.96)	(3.52)	(3.52)	(3.69)	(3.68)	(7.95)	(8.00)
WTP^a		-5.06		-3.04		-2.99		-1.36		0.67		2.33
		(4.53)		(3.23)		(2.75)		(2.81)		(2.88)		(4.43)
WTP^d		-14.74		-23.54		-23.96*		-38.00***		-28.21**		-55.86**
		(20.27)		(14.63)		(12.62)		(13.19)		(13.51)		(23.64)
Female		5.84		-1.06		3.81		-1.29		-5.99		-21.80

Continuation of Table C.19

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
(0=Male, 1=Female)		(17.65)		(12.85)		(11.02)		(11.50)		(11.76)		(21.35)
Econstud		-3.76		-16.71		-16.92		-21.26		-7.34		15.05
(0=NoEcon, 1=Econstud)		(20.41)		(14.86)		(12.70)		(13.37)		(13.64)		(23.34)
Age		-0.13		-1.17		-2.18		-1.84		-1.96		-3.04
		(2.31)		(1.65)		(1.44)		(1.51)		(1.55)		(2.78)
Semester		-0.40		-1.21		-1.99		-2.26		-2.16		-5.31*
		(2.31)		(1.66)		(1.47)		(1.55)		(1.61)		(3.07)
fairnotimp		29.32		2.22		-3.35		-1.90		0.94		-6.92
(0=fairimp, 1=fairnotimp)		(36.84)		(26.70)		(22.63)		(23.40)		(23.76)		(39.19)
fairvimp		21.27		10.56		11.51		16.07		11.74		26.44
(0=fairimp, 1=fairvimp)		(19.40)		(13.74)		(11.71)		(12.11)		(12.49)		(21.79)

Continuation of Table C.19

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
Expfair		17.28		12.50		7.91		6.20		8.08		20.85
(0=Expunfair, 1=Expfair)		(16.57)		(12.21)		(10.64)		(11.35)		(11.70)		(20.12)
EarnSoSec		-49.77		-22.47		-32.41		-21.75		-17.53		-54.41
(0=NoSoSec, 1=Earn-SoSec)		(59.52)		(43.06)		(36.64)		(36.61)		(37.34)		(59.94)
ConSoSec		-5.23		16.25		0.13		-1.65		-17.15		-69.39
(0=NoSoSec, 1=Con-SoSec)		(52.09)		(36.88)		(31.38)		(31.57)		(32.49)		(51.83)
UncSoSec		-27.89		7.73		-6.86		-5.58		-11.36		-68.12
(0=NoSoSec, 1=Unc-SoSec)		(53.70)		(38.33)		(32.54)		(32.69)		(33.44)		(52.37)
Iresp		18.86		30.18		43.72*		44.34*		30.69		53.57

Continuation of Table C.19

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
(0=rIresp, 1=Iresp)		(37.07)		(26.94)		(23.04)		(24.31)		(24.55)		(40.45)
rResp		-282.83		-192.52		-127.87		-124.40		-158.76		-157.18
(0=rIresp, 1=rResp)		(6258.70)		(6504.37)		(3862.07)		(7299.54)		(42192.53)		(8713.63)
Resp		8.33		19.12		36.39		28.26		25.40		49.94
(0=rIresp, 1=Resp)		(40.05)		(28.83)		(25.08)		(29.15)		(29.03)		(50.72)
uncResp		3.68		6.34		14.10		25.04*		17.20		38.81
(0=rIresp, 1=uncResp)		(19.21)		(13.84)		(12.09)		(13.41)		(13.61)		(25.23)
$vrisk^A$		-222.33		-144.06		-117.36		-131.41		-176.77		-232.48
(0=$risk^N$, 1=$vrisk^A$)		(6929.07)		(8858.16)		(5322.09)		(10145.24)		(62010.77)		(12913.39)
$risk^A$		-1.38		3.41		4.56		-3.68		-8.93		-10.31

Continuation of Table C.19

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
(0=$risk^N$, 1=$risk^A$)		(24.30)		(17.64)		(15.40)		(16.08)		(16.51)		(29.66)
$risk^L$		-7.28		-9.76		-10.21		-5.02		-5.51		-29.36
(0=$risk^N$, 1=$risk^L$)		(22.42)		(16.00)		(13.92)		(14.49)		(14.81)		(26.34)
$vrisk^L$		-36.48		-9.06		-2.25		2.60		-3.87		11.81
(0=$risk^N$, 1=$vrisk^L$)		(36.31)		(25.63)		(21.96)		(22.29)		(22.89)		(37.83)
hypownpause		-1.99		-1.34		-0.09		3.04		2.98		-2.93
		(5.00)		(3.57)		(3.09)		(3.25)		(3.35)		(6.49)
StratEasy		42.94**		38.63***		38.49***		32.11***		35.00***		72.00***
(0=LotEasy, 1=StratEasy)		(18.64)		(13.44)		(11.49)		(12.15)		(12.61)		(24.67)
ContEasy		23.51		24.41*		19.55		18.48		20.59		53.19*

Continuation of Table C.19

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
(0=LotEasy, 1=ConEasy)		(20.02)		(14.49)		(12.57)		(13.12)		(13.59)		(27.28)
constant	-5.25	273.69	-9.96	2327.46	-17.31**	4329.57	-14.60*	3645.66	-20.98**	3888.54	-63.76***	6082.26
	(12.61)	(4617.99)	(9.28)	(3304.73)	(8.35)	(2880.37)	(8.72)	(3020.00)	(9.01)	(3099.59)	(17.38)	(5546.64)
σ_u	61.60***	51.02***	44.92***	36.35***	40.48***	30.77***	41.32***	30.84***	42.52***	31.37***	69.70***	43.62***
	(8.07)	(6.77)	(5.64)	(4.59)	(5.15)	(3.91)	(5.65)	(4.26)	(6.16)	(4.65)	(11.86)	(8.39)
σ_e	22.40***	22.42***	16.13***	16.12***	13.33***	13.33***	15.75***	15.71***	15.65***	15.61***	29.35***	29.51***
	(1.99)	(2.00)	(1.29)	(1.30)	(1.13)	(1.14)	(1.36)	(1.36)	(1.39)	(1.39)	(3.32)	(3.37)
Wald-chi2	2.459	18.590	3.438	24.693	8.945**	36.520*	4.284	31.255	2.316	26.751	2.865	24.324

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 216$. Dependent variable is *Donation*, standard errors in parentheses. Assuming independence of observations. Lower limit of the RE Tobit-regression 0, upper limit 100.

Table C.20: OLS-Regression on Donation Probability by Safe Amount

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
Exclusion	0.22***	0.34***	0.22***	0.31***	0.30***	0.41***	0.20***	0.25***	0.12*	0.12	0.15**	0.19***
(0=NoEx, 1=Ex)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.06)	(0.07)
Contest	-0.01	-0.01	-0.04	-0.04	0.03	0.03	-0.01	-0.01	-0.01	-0.01	-0.00	0.00
(0=Lottery, 1=Contest)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.06)
Strategy	-0.03	-0.03	-0.01	-0.01	-0.01	-0.01	-0.01	-0.01	-0.04	-0.04	-0.01	-0.01
(0=Lottery, 1=Strategy)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.06)
WTP^a		-0.04*		-0.03*		-0.05**		-0.01		0.00		0.02
		(0.02)		(0.02)		(0.02)		(0.02)		(0.02)		(0.02)
WTP^d		-0.13		-0.24**		-0.30***		-0.43***		-0.31***		-0.37***
		(0.09)		(0.09)		(0.09)		(0.09)		(0.09)		(0.08)
Female		0.14*		0.13*		0.18**		0.11		0.01		-0.10

Continuation of Table C.20

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
(0=Male, 1=Female)		(0.08)		(0.08)		(0.08)		(0.08)		(0.08)		(0.07)
Econstud		0.05		-0.07		-0.06		-0.17**		-0.03		0.00
(0=NoEcon, 1=Econstud)		(0.09)		(0.09)		(0.08)		(0.09)		(0.09)		(0.07)
Age		-0.01		-0.01		-0.02**		-0.02*		-0.02		-0.02***
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
Semester		0.01		-0.00		-0.02*		-0.02		-0.01		-0.02***
		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)		(0.01)
fairnotimp		0.38**		0.10		0.05		0.06		0.07		-0.12
(0=fairimp, 1=fairnotimp)		(0.15)		(0.15)		(0.15)		(0.15)		(0.16)		(0.13)
fairvimp		0.08		0.06		0.13		0.19**		0.16*		0.10
(0=fairimp, 1=fairvimp)		(0.09)		(0.09)		(0.08)		(0.09)		(0.09)		(0.07)

Continuation of Table C.20

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
Expfair		0.27***		0.17**		0.07		0.01		0.06		0.10
(0=Expunfair, 1=Expfair)		(0.07)		(0.08)		(0.07)		(0.07)		(0.08)		(0.06)
EarnSoSec		-0.39		-0.40		-0.45*		-0.18		-0.01		-0.28
(0=NoSoSec, 1=EarnSoSec)		(0.26)		(0.26)		(0.25)		(0.26)		(0.26)		(0.22)
ConSoSec		-0.07		-0.13		-0.20		-0.07		-0.05		-0.42**
(0=NoSoSec, 1=ConSoSec)		(0.23)		(0.23)		(0.22)		(0.23)		(0.23)		(0.20)
UncSoSec		-0.12		-0.08		-0.16		-0.02		0.03		-0.34*
(0=NoSoSec, 1=UncSoSec)		(0.24)		(0.24)		(0.22)		(0.23)		(0.24)		(0.20)
Iresp		-0.01		0.09		0.23		0.27*		0.21		0.17
(0=rIresp, 1=Iresp)		(0.16)		(0.16)		(0.15)		(0.16)		(0.16)		(0.14)

Continuation of Table C.20

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
rResp		-0.87***		-0.56**		-0.24		-0.10		-0.21		-0.10
(0=rIresp, 1=rResp)		(0.24)		(0.24)		(0.23)		(0.24)		(0.25)		(0.21)
Resp		0.08		0.22		0.27		0.28		0.17		0.28*
(0=rIresp, 1=Resp)		(0.18)		(0.18)		(0.17)		(0.18)		(0.18)		(0.15)
uncResp		-0.04		-0.00		0.07		0.18**		0.12		0.05
(0=rIresp, 1=uncResp)		(0.08)		(0.09)		(0.08)		(0.08)		(0.09)		(0.07)
$vrisk^A$		-0.31		-0.27		-0.20		-0.12		-0.36		-0.34
(0=$risk^N$, 1=$vrisk^A$)		(0.33)		(0.33)		(0.31)		(0.32)		(0.33)		(0.28)
$risk^A$		-0.05		-0.08		-0.04		-0.08		-0.17		-0.09
(0=$risk^N$, 1=$risk^A$)		(0.10)		(0.11)		(0.10)		(0.10)		(0.11)		(0.09)

Continuation of Table C.20

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
$risk^L$		-0.23**		-0.27***		-0.24**		-0.09		-0.08		-0.15*
(0=$risk^N$, 1=$risk^L$)		(0.10)		(0.10)		(0.09)		(0.10)		(0.10)		(0.08)
$vrisk^L$		-0.62***		-0.37**		-0.27*		-0.12		-0.12		-0.18
(0=$risk^N$, 1=$vrisk^L$)		(0.16)		(0.16)		(0.15)		(0.16)		(0.16)		(0.13)
hypownpause		0.00		-0.00		0.01		0.04*		0.02		-0.01
		(0.02)		(0.02)		(0.02)		(0.02)		(0.02)		(0.02)
StratEasy		0.21**		0.26***		0.35***		0.23***		0.29***		0.29***
(0=LotEasy, 1=StratEasy)		(0.08)		(0.08)		(0.08)		(0.08)		(0.08)		(0.07)
ContEasy		0.16*		0.16*		0.17**		0.18**		0.19**		0.29***
(0=LotEasy, 1=ContEasy)		(0.08)		(0.08)		(0.08)		(0.08)		(0.09)		(0.07)
constant	0.46***	15.46	0.44***	24.76	0.34***	46.05**	0.37***	35.26*	0.37***	33.53	0.22***	45.74***

Continuation of Table C.20

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
	(0.07)	(20.29)	(0.07)	(20.37)	(0.07)	(19.24)	(0.07)	(20.17)	(0.07)	(20.70)	(0.06)	(17.27)
R2	0.051***	0.324***	0.051***	0.323***	0.089***	0.399***	0.042**	0.336***	0.016	0.283***	0.027	0.408***

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 216$. Dependent variable is *Donation-binary*, standard errors in parentheses. Assuming independence of observations.

Table C.21: RE Probit-Regression on Donation Probability by Safe Amount

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
Exclusion	1.93*	2.23**	3.70***	4.50***	6.03***	4.77***	3.40***	2.99**	0.67	2.10	1.50*	2.17
(0=NoEx, 1=Ex)	(0.99)	(0.96)	(0.85)	(1.32)	(0.88)	(1.63)	(0.91)	(1.52)	(1.14)	(1.53)	(0.82)	(1.51)
Contest	-0.11	-0.12	-0.57	-0.57	0.38	0.36	-0.17	-0.16	-0.20	-0.18	-0.00	-0.01
(0=Lottery, 1=Contest)	(0.36)	(0.36)	(0.48)	(0.48)	(0.47)	(0.45)	(0.44)	(0.44)	(0.47)	(0.46)	(0.47)	(0.47)
Strategy	-0.23	-0.23	-0.19	-0.18	-0.21	-0.18	-0.17	-0.17	-0.61	-0.57	-0.18	-0.19
(0=Lottery, 1=Strategy)	(0.36)	(0.36)	(0.47)	(0.47)	(0.48)	(0.46)	(0.44)	(0.44)	(0.49)	(0.48)	(0.47)	(0.47)
WTP^a		-0.24		-0.47		-0.64*		-0.33		-0.10		0.23
		(0.23)		(0.38)		(0.38)		(0.33)		(0.32)		(0.32)
WTP^d		-0.91		-3.13*		-4.16**		-5.09***		-3.49**		-4.58**
		(1.03)		(1.62)		(1.92)		(1.76)		(1.39)		(2.06)
Female		1.33		2.23*		2.86*		1.66		0.56		-1.85

Continuation of Table C.21

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
(0=Male, 1=Female)		(0.90)		(1.25)		(1.52)		(1.49)		(1.23)		(1.81)
Econstud		0.38		-1.49		-0.81		-2.12		-1.02		0.55
(0=NoEcon, 1=Econstud)		(0.97)		(1.32)		(1.40)		(1.72)		(1.49)		(1.40)
Age		-0.02		-0.14		-0.31		-0.22		-0.21		-0.24
		(0.11)		(0.17)		(0.19)		(0.21)		(0.16)		(0.18)
Semester		0.08		-0.02		-0.19		-0.18		-0.20		-0.31
		(0.12)		(0.18)		(0.19)		(0.19)		(0.21)		(0.21)
fairnotimp		1.91		0.20		0.27		0.27		1.19		-0.75
(0=fairimp, 1=fairnotimp)		(1.70)		(2.20)		(2.49)		(2.60)		(2.23)		(2.54)
fairvimp		0.63		0.58		0.71		2.05		2.03		1.65
(0=fairimp, 1=fairvimp)		(0.97)		(1.45)		(1.33)		(1.53)		(1.25)		(1.71)

Continuation of Table C.21

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
Expfair		1.79**		2.39*		0.74		0.32		1.40		1.84
(0=Expunfair, 1=Expfair)		(0.87)		(1.32)		(1.19)		(1.38)		(1.24)		(1.61)
EarnSoSec		-2.34		-5.20		-4.33		-1.53		-0.97		-2.49
(0=NoSoSec, 1=EarnSoSec)		(2.96)		(3.47)		(3.81)		(6.48)		(4.32)		(4.07)
ConSoSec		-0.33		-1.52		-1.72		-0.36		-1.40		-3.79
(0=NoSoSec, 1=ConSoSec)		(2.66)		(3.01)		(3.40)		(6.29)		(3.67)		(3.52)
UncSoSec		-0.31		-0.08		-0.27		0.44		-0.49		-3.89
(0=NoSoSec, 1=UncSoSec)		(2.75)		(3.16)		(3.47)		(6.41)		(4.00)		(3.76)
Iresp		-0.49		0.77		1.91		2.67		1.91		3.78
(0=rIresp, 1=Iresp)		(1.64)		(2.27)		(2.40)		(2.72)		(2.25)		(3.23)

Continuation of Table C.21

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
Resp		0.49		3.25		4.53		3.70		2.50		3.68
(0=rIresp, 1=Resp)		(1.87)		(3.12)		(2.88)		(2.95)		(2.47)		(3.14)
uncResp		-0.43		-0.38		1.03		2.19		1.37		2.13
(0=rIresp, 1=uncResp)		(0.94)		(1.29)		(1.40)		(1.46)		(1.26)		(1.77)
$risk^A$		-0.53		-1.29		-0.53		-0.91		-1.35		-0.30
(0=$risk^N$, 1=$risk^A$)		(1.17)		(1.72)		(1.88)		(2.20)		(1.76)		(1.80)
$risk^L$		-1.59		-3.93**		-2.51		-0.82		-0.33		-1.94
(0=$risk^N$, 1=$risk^L$)		(1.15)		(1.83)		(1.74)		(1.82)		(1.58)		(1.92)
$vrisk^L$		-4.32**		-5.03**		-2.32		-0.77		-0.53		-0.34
(0=$risk^N$, 1=$vrisk^L$)		(1.83)		(2.35)		(2.64)		(3.03)		(2.27)		(2.21)

Continuation of Table C.21

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
hypownpause		-0.04		-0.13		-0.12		0.41		0.32		-0.11
		(0.23)		(0.34)		(0.35)		(0.40)		(0.39)		(0.41)
StratEasy		1.47		4.45***		4.06***		2.79**		3.45***		4.06**
(0=LotEasy, 1=StratEasy)		(0.94)		(1.53)		(1.48)		(1.39)		(1.27)		(1.83)
ContEasy		1.04		2.19*		1.58		1.88		2.40*		4.12**
(0=LotEasy, 1=ContEasy)		(0.92)		(1.32)		(1.40)		(1.77)		(1.33)		(2.07)
constant	-0.46	44.77	-1.11*	287.08	-2.89***	612.92	-1.88***	435.48	-1.81***	414.01	-3.77***	478.08
	(0.71)	(217.60)	(0.66)	(334.57)	(0.66)	(373.40)	(0.64)	(419.54)	(0.63)	(328.41)	(0.61)	(353.64)
lnsig2u	2.22***	1.47***	3.33***	2.79***	3.53***	2.30***	3.17***	2.50***	3.43***	2.78***	3.12***	1.91***
	(0.50)	(0.51)	(0.48)	(0.47)	(0.42)	(0.51)	(0.49)	(0.45)	(0.50)	(0.46)	(0.41)	(0.73)
Wald-chi2	4.131	14.896	19.638***	31.048	47.340***	21.434	14.202***	27.597	1.983	34.657*	3.564	9.125
N	216	207	216	207	216	207	216	207	216	207	216	207

Continuation of Table C.21

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Dependent variable is *Donation*, standard errors in parentheses

Table C.22: OLS-Regression on Donation Level by Safe Amount

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
Exclusion	-4.99	-9.80	2.49	6.87	-4.96	5.68	-3.10	-4.38	-1.96	-5.04	-14.25**	-46.26*
(0=NoEx, 1=Ex)	(5.89)	(7.36)	(5.23)	(7.36)	(4.85)	(7.42)	(4.51)	(6.40)	(4.30)	(6.97)	(6.57)	(23.50)
Contest	0.66	1.37	2.62	1.54	3.60	4.03	2.44	0.89	5.98	4.00	2.77	4.01
(0= Lottery, 1= Contest)	(7.02)	(5.97)	(6.29)	(5.51)	(5.61)	(4.86)	(5.37)	(4.72)	(5.14)	(4.62)	(7.67)	(6.97)
Strategy	-0.51	0.05	1.27	1.40	1.93	1.06	-3.25	-5.26	4.99	1.81	-2.92	0.74
(0= Lottery, 1= Strategy)	(7.07)	(5.94)	(6.20)	(5.38)	(5.71)	(4.91)	(5.37)	(4.74)	(5.21)	(4.66)	(7.80)	(7.25)
WTP^a		-2.65		-2.43		-2.24		-1.65		1.01		-3.13
		(1.83)		(1.70)		(1.50)		(1.28)		(1.26)		(2.64)
WTP^d		-12.36		-4.55		-1.80		-4.31		-1.57		24.11
		(8.43)		(8.24)		(7.31)		(6.97)		(8.41)		(22.55)
Female		-22.18***		-27.02***		-30.29***		-26.92***		-26.07***		-38.07*

Continuation of Table C.22

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
(0=Male, 1=Female)		(7.05)		(6.88)		(6.34)		(6.22)		(6.49)		(20.24)
Econstud		-22.65**		-23.06**		-31.17***		-12.59		-0.44		54.79
(0=NoEcon, 1=Econstud)		(8.81)		(8.89)		(7.77)		(9.21)		(10.72)		(42.62)
Age		1.25		0.56		0.34		-0.20		-0.09		3.87**
		(0.94)		(0.87)		(0.88)		(0.87)		(0.95)		(1.69)
Semester		0.17		0.04		-0.09		-0.34		0.43		0.94
		(0.89)		(0.85)		(0.82)		(0.77)		(0.99)		(1.97)
fairnotimp		-9.66		5.14		3.90		2.45		-2.22		21.50
(0=fairimp, 1=fairnotimp)		(17.38)		(17.66)		(16.25)		(14.09)		(13.98)		(30.40)
fairvimp		21.60***		22.88***		20.33***		7.44		-1.68		15.28
(0=fairimp, 1=fairvimp)		(7.38)		(6.90)		(6.39)		(6.20)		(6.95)		(18.12)

Continuation of Table C.22

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
Expfair		-8.29		4.38		14.50**		16.20***		13.94*		29.84*
(0=Expunfair, 1=Expfair)		(6.35)		(6.69)		(6.23)		(5.84)		(7.11)		(14.85)
EarnSoSec		-34.15		12.22		-15.17		-4.47		-9.73		109.11**
(0=NoSoSec, 1=EarnSoSec)		(23.61)		(22.32)		(19.41)		(19.44)		(21.04)		(48.09)
ConSoSec		-15.51		35.39*		8.98		9.13		-9.39		86.44**
(0=NoSoSec, 1=ConSoSec)		(19.71)		(18.19)		(15.80)		(16.38)		(16.70)		(37.56)
UncSoSec		-36.18*		12.25		-16.62		-7.73		-16.05		54.60
(0=NoSoSec, 1=UncSoSec)		(20.40)		(19.05)		(16.51)		(16.43)		(16.72)		(33.39)
Iresp		12.54		38.91**		40.49**		19.27		8.51		-14.37
(0=rIresp, 1=Iresp)		(17.24)		(16.16)		(16.12)		(15.32)		(16.13)		(29.39)

Continuation of Table C.22

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
Resp		-5.70		-8.50		-9.17		-8.91		12.32		-61.56*
(0=rIresp, 1=Resp)		(16.95)		(15.47)		(16.27)		(18.37)		(18.35)		(35.80)
uncResp		10.88		7.64		2.60		3.81		1.92		20.38
(0=rIresp, 1=uncResp)		(7.77)		(7.38)		(7.25)		(8.05)		(8.16)		(18.21)
$risk^A$		5.40		14.11		10.62		0.47		6.71		-44.50**
(0=$risk^N$, 1=$risk^A$)		(10.02)		(9.86)		(9.29)		(9.27)		(10.05)		(21.28)
$risk^L$		15.53*		14.45*		2.06		-7.97		-9.39		-34.22**
(0=$risk^N$, 1=$risk^L$)		(8.94)		(7.98)		(7.34)		(7.11)		(7.07)		(16.34)
$vrisk^L$		63.85***		34.97**		31.31**		9.77		5.48		-12.02
(0=$risk^N$, 1=$vrisk^L$)		(18.05)		(16.23)		(14.36)		(14.86)		(15.45)		(45.88)

Continuation of Table C.22

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
hypownpause		-1.88		-2.31		-0.06		-2.15		-1.31		-13.67**
		(2.09)		(2.01)		(1.82)		(1.81)		(1.93)		(5.51)
StratEasy		17.23**		27.15***		19.03**		18.97**		21.61***		43.42
(0=LotEasy, 1=StratEasy)		(7.74)		(7.43)		(7.61)		(7.33)		(7.68)		(36.46)
ContEasy		3.68		12.32		1.50		0.97		7.43		-16.86
(0=LotEasy, 1=ContEasy)		(9.55)		(8.42)		(8.09)		(7.47)		(7.17)		(25.54)
constant	49.53***	-2398.33	34.47***	-1110.33	34.40***	-640.09	33.80***	454.83	28.17***	219.29	49.14***	-7688.81**
	(6.10)	(1884.12)	(5.45)	(1748.63)	(5.29)	(1764.63)	(4.76)	(1734.60)	(4.45)	(1906.83)	(6.59)	(3403.83)
R2	0.006	0.440***	0.003	0.400***	0.016	0.434***	0.017	0.418***	0.021	0.429**	0.087	0.551**
N	120	120	116	116	106	106	100	100	89	89	62	62

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Dependent variable is $Donation \geq 1$, standard errors in parentheses. Assuming independence of observations.

Table C.23: RE Tobit-Regression on Donation Level by Safe Amount

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
Exclusion	-7.29	-15.89	2.37	6.08	-2.70	6.15	-2.93	-3.42	-2.00	-5.11	-18.27**	-53.29***
(0=NoEx, 1=Ex)	(10.25)	(11.23)	(7.72)	(9.26)	(6.96)	(8.34)	(6.21)	(6.77)	(5.54)	(6.17)	(8.63)	(20.57)
Contest	1.46	1.56	2.52	2.27	4.52	4.53	1.45	0.87	5.49	4.17	3.75	4.98
(0=Lottery, 1=Contest)	(3.97)	(3.99)	(3.39)	(3.37)	(2.97)	(2.94)	(3.66)	(3.65)	(3.95)	(3.87)	(6.99)	(5.82)
Strategy	-0.45	-0.32	1.07	1.11	1.05	0.85	-4.54	-5.41	3.81	1.89	-2.46	0.55
(0=Lottery, 1= Strategy)	(3.99)	(4.00)	(3.30)	(3.27)	(2.95)	(2.92)	(3.66)	(3.66)	(3.99)	(3.91)	(7.16)	(6.02)
WTP^a		-3.09		-2.58		-1.89		-1.51		1.01		-4.16*
		(2.52)		(1.97)		(1.67)		(1.36)		(1.12)		(2.42)
WTP^d		-7.44		-3.78		-0.72		-3.45		-1.41		28.18
		(12.55)		(10.40)		(8.90)		(7.57)		(7.46)		(19.12)
Female		-23.32**		-26.27***		-28.72***		-25.98***		-25.99***		-47.15**

Continuation of Table C.23

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
(0=Male, 1=Female)		(10.46)		(8.68)		(7.70)		(6.68)		(5.76)		(19.17)
Econstud		-21.97*		-22.52**		-31.42***		-12.08		-0.53		56.72
(0=NoEcon, 1=Econstud)		(13.23)		(11.29)		(9.63)		(9.85)		(9.51)		(35.40)
Age		1.84		0.48		0.33		-0.19		-0.07		4.50***
		(1.41)		(1.11)		(1.05)		(0.94)		(0.84)		(1.48)
Semester		0.69		0.01		-0.15		-0.38		0.43		1.13
		(1.36)		(1.08)		(0.99)		(0.82)		(0.88)		(1.66)
fairnotimp		-11.85		9.96		7.13		3.74		-1.89		35.15
(0=fairimp, 1=fairnotimp)		(24.31)		(21.72)		(18.58)		(14.84)		(12.42)		(27.96)
fairvimp		27.40**		22.56***		20.26***		7.55		-1.57		23.37
(0=fairimp, 1=fairvimp)		(11.06)		(8.47)		(7.24)		(6.47)		(6.16)		(17.28)

Continuation of Table C.23

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
Expfair		-10.53		5.08		13.95*		16.09**		14.05**		35.38***
(0=Expunfair, 1=Expfair)		(9.48)		(8.56)		(7.63)		(6.30)		(6.32)		(13.35)
EarnSoSec		-28.21		14.64		-14.48		-4.17		-9.68		125.93***
(0=NoSoSec, 1=Earn-SoSec)		(36.30)		(28.54)		(24.15)		(21.04)		(18.73)		(43.34)
ConSoSec		-3.83		37.06		8.78		9.10		-9.21		100.57***
(0=NoSoSec, 1=Con-SoSec)		(31.18)		(23.45)		(19.96)		(17.87)		(14.89)		(33.80)
UncSoSec		-32.19		13.18		-16.51		-8.19		-16.01		62.77**
(0=NoSoSec, 1=Unc-SoSec)		(31.55)		(24.36)		(20.43)		(17.80)		(14.89)		(29.03)
Iresp		20.59		33.93*		38.12**		17.88		8.31		-5.84

Continuation of Table C.23

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
(0=rIresp, 1=Iresp)		(24.65)		(19.75)		(17.89)		(15.90)		(14.29)		(26.98)
Resp		-8.28		-10.76		-11.47		-7.51		12.17		-76.83**
(0=rIresp, 1=Resp)		(24.91)		(19.29)		(18.20)		(19.89)		(16.39)		(32.43)
uncResp		14.81		6.77		2.71		3.79		2.03		23.25
(0=rIresp, 1=uncResp)		(11.76)		(9.28)		(8.42)		(8.51)		(7.26)		(15.19)
$risk^A$		4.68		15.64		13.34		1.74		6.94		-49.12***
(0=$risk^N$, 1=$risk^A$)		(14.69)		(12.46)		(11.23)		(9.88)		(8.90)		(18.34)
$risk^L$		18.49		14.58		3.24		-7.12		-9.42		-38.03***
(0=$risk^N$, 1=$risk^L$)		(13.26)		(9.94)		(8.79)		(7.67)		(6.27)		(13.96)
$vrisk^L$		72.10***		33.36		32.04*		9.91		5.55		-4.89

Continuation of Table C.23

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
(0=$risk^N$, 1=$vrisk^L$)		(26.99)		(20.53)		(17.68)		(16.00)		(13.76)		(39.65)
hypownpause		-3.02		-2.24		0.36		-1.77		-1.29		-16.90***
		(3.13)		(2.50)		(2.22)		(1.91)		(1.70)		(6.05)
StratEasy		17.28		27.79***		21.25**		19.46**		21.64***		49.65
(0=LotEasy, 1=StratEasy)		(11.51)		(9.27)		(8.55)		(7.65)		(6.77)		(30.47)
ContEasy		1.39		12.35		2.73		1.82		7.39		-20.20
(0=LotEasy, 1=ContEasy)		(13.88)		(10.49)		(9.24)		(7.81)		(6.34)		(21.52)
constant	51.50***	-3582.41	33.68***	-947.89	30.96***	-625.62	33.35***	426.08	27.70***	183.39	51.82***	-8946.27***
	(8.24)	(2827.00)	(6.22)	(2213.47)	(5.82)	(2093.76)	(5.26)	(1881.49)	(4.79)	(1689.40)	(7.64)	(2970.37)
σ_u	32.18***	21.57***	23.21***	15.87***	20.07***	13.18***	16.35***	8.79***	12.72***	3.26	13.99***	0.00
	(4.03)	(3.01)	(2.88)	(2.24)	(2.56)	(1.93)	(2.49)	(2.25)	(2.49)	(3.90)	(5.02)	(2.91)

Continuation of Table C.23

	S.A.=0		S.A.=20		S.A.=40		S.A.=60		S.A.=80		S.A.=100	
	I	II	I	II	I	II	I	II	I	II	I	II
σ_e	16.45***	16.47***	14.32***	14.23***	11.95***	11.83***	14.56***	14.60***	15.04***	14.71***	22.12***	18.02***
	(1.50)	(1.51)	(1.20)	(1.18)	(1.06)	(1.03)	(1.32)	(1.33)	(1.44)	(1.37)	(2.83)	(1.72)
Wald-chi2	0.757	41.906***	0.643	38.581**	2.818	44.428***	3.144	46.959***	2.251	61.080***	5.523	63.534***
N	120	120	116	116	106	106	100	100	89	89	62	62

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Dependent variable is *Donation* ≥ 1, standard errors in parentheses. Assuming independence of observations. Lower limit of the RE Tobit-regression 1, upper limit 100.

Table C.24: OLS-regression on Determinants of *Donationfair*

Exclusion	0.65***
(0=NoEx, 1=Ex)	(0.09)
Contest	-0.00
(0=Lottery, 1=Contest)	(0.08)
Strategy	-0.00
(0=Lottery, 1=Strategy)	(0.08)
safeamount	-0.00
	(0.00)
WTP^a	0.02
	(0.02)
WTP^d	-0.68***
	(0.10)
Female	0.59***
(0=Male, 1=Female)	(0.09)
Econstud	0.00
(0=NoEcon, 1=Econstud)	(0.10)
Age	0.01
	(0.01)
Semester	0.03***
	(0.01)
fairnotimp	-0.22
(0=fairimp, 1=fairnotimp)	(0.18)
fairvimp	0.56***
(0=fairimp, 1=fairvimp)	(0.10)
Expfair	0.99***
(0=Expunfair, 1=Expfair)	(0.09)

EarnSoSec	-0.68**
(0=NoSoSec, 1=EarnSoSec)	(0.30)
ConSoSec	-0.57**
(0=NoSoSec, 1=ConSoSec)	(0.26)
UncSoSec	-0.68**
(0=NoSoSec, 1=UncSoSec)	(0.27)
Iresp	0.06
(0=rIresp, 1=Iresp)	(0.18)
rResp	-1.55***
(0=rIresp, 1=rResp)	(0.28)
Resp	0.69***
(0=rIresp, 1=Resp)	(0.20)
uncResp	-0.20**
(0=rIresp, 1=uncResp)	(0.10)
$vrisk^A$	-1.43***
(0=$risk^N$, 1=$vrisk^A$)	(0.37)
$risk^A$	-0.17
(0=$risk^N$, 1=$risk^A$)	(0.12)
$risk^L$	0.13
(0=$risk^N$, 1=$risk^L$)	(0.11)
$vrisk^L$	-1.40***
(0=$risk^N$, 1=$vrisk^L$)	(0.18)
hypownpause	0.08***
	(0.02)
StratEasy	0.67***
(0=LotEasy, 1=StratEasy)	(0.09)
ContEasy	0.39***

(0=LotEasy, 1=ContEasy)	(0.10)
constant	-13.20
	(23.25)
R2	0.398

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. $N = 1296$. Dependent variable is *Donationfair*, standard errors in parentheses. Assuming independence of observations.

C.4 Predictive Margins of Donation Probability

Table C.25: Predictive Margins of Donation Probability by Safe Amount and Treatment (OLS-based)

S.A. x Treatment	Margin	Std. Err.	z	P>|z|	[95% Conf.	Interval]
0 x NoEx	.4094687	.0419838	9.75	0.000	.3271028	.4918347
0 x Ex	.7016424	.0419838	16.71	0.000	.6192764	.7840083
20 x NoEx	.3909502	.0419838	9.31	0.000	.3085842	.4733161
20 x Ex	.6831239	.0419838	16.27	0.000	.6007579	.7654898
40 x NoEx	.3076169	.0419838	7.33	0.000	.2252509	.3899828
40 x Ex	.6738646	.0419838	16.05	0.000	.5914987	.7562306
60 x NoEx	.3261354	.0419838	7.77	0.000	.2437694	.4085013
60 x Ex	.5997905	.0419838	14.29	0.000	.5174246	.6821565
80 x NoEx	.3168761	.0419838	7.55	0.000	.2345102	.3992421
80 x Ex	.507198	.0419838	12.08	0.000	.424832	.5895639
100 x NoEx	.1779872	.0419838	4.24	0.000	.0956213	.2603532
100 x Ex	.3960868	.0419838	9.43	0.000	.3137209	.4784528

Table C.26: Predictive Margins of Donation Probability by Safe Amount and Treatment (RE Probit-based)

S.A. x Treatment	Margin	Std. Err.	z	P>|z|	[95% Conf.	Interval]
0 x NoEx	.3914108	.0610207	6.41	0.000	.2718123	.5110092
0 x Ex	.7017855	.0589637	11.90	0.000	.5862189	.8173522
20 x NoEx	.3786387	.0607279	6.24	0.000	.2596142	.4976631
20 x Ex	.7009821	.0603396	11.62	0.000	.5827186	.8192455
40 x NoEx	.3082483	.057179	5.39	0.000	.1961795	.4203171
40 x Ex	.6898622	.0611224	11.29	0.000	.5700645	.8096599
60 x NoEx	.3226813	.0579542	5.57	0.000	.2090932	.4362694
60 x Ex	.6115427	.065664	9.31	0.000	.4828437	.7402417
80 x NoEx	.3158853	.0576077	5.48	0.000	.2029764	.4287943
80 x Ex	.5166134	.0681929	7.58	0.000	.3829577	.650269
100 x NoEx	.2061573	.0487895	4.23	0.000	.1105316	.3017831
100 x Ex	.4074678	.0675196	6.03	0.000	.2751318	.5398038