On the Economics of Incentives and Cooperation

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Zusammenfassung

Die vorliegende Dissertation beschäftigt sich mit der ökonomischen Analyse von Anreizen zur Kooperation und verwendet dazu ein theoretisches Modell, ein Verhaltensexperiment sowie eine empirische Untersuchung. Die Arbeit betritt inhaltlich auf verschiedenen Gebieten Neuland und verwendet zudem moderne ökonometrische Methoden, womit sie auch aus methodischer Hinsicht die Literatur bereichert. Der Inhalt der einzelnen Kapitel wird im Folgenden kurz vorgestellt.

Das erste Kapitel stellt die Einleitung zur Arbeit dar. Im zweiten Kapitel wird ein duopolistischer Markt betrachtet, auf dem Verbraucher zusätzlich zum Preis Entfernungskosten auf sich nehmen müssen, um das Gut zu konsumieren. Hierbei wird untersucht, wie die Unternehmen ihren Standort wählen und wie sich diese Entscheidung auf die gesamtwirtschafliche Wohlfahrt auswirkt. Es kann gezeigt werden, dass sich die Unternehmen unter bestimmten Bedingungen so positionieren, dass alle Konsumenten zu den insgesamt geringsten Transportkosten bedient werden können und somit das effiziente Ergebnis erreicht wird, obwohl der Markt auf Anbieterseite konzentriert ist. Diese Standortwahl kann als kooperatives Verhalten gesehen werden, da es beide Unternehmen vermeiden, stärker in Konkurrenz zu treten. Zum einen schließt diese Analyse eine Lücke in der Forschung, zum anderen ist auch das Ergebnis interessant und eröffnet neue Sichtweisen für Theorie und Praxis der Marktregulierung.

Das dritte Kapitel betrachtet ein Experiment, in dem untersucht wird, ob die Erwartungen von anderen Personen Einfluss auf die eigene Entscheidung nehmen können. Hierzu wird ein erweitertes Diktatorspiel gespielt, bei dem der Diktator wie üblich über die Aufteilung eines bestimmten Geldbetrages entscheiden kann, ihm jedoch zusätzlich vor seiner Entscheidung die erwartete Auszahlung des Empfängers vorgelegt wird. Die Ergebnisse des Experiments zeigen, dass bei einer unvorbereiteten ersten Konfrontation mit der Erwartung des Empfängers kein Einfluss auf die Entscheidung nachweisbar ist. Beim Wiederholen der Spielsituation oder nach einer vorherigen gedanklichen Auseinandersetzung mit der Perspektive des Empfängers zeigt sich allerdings sehr wohl ein Einfluss der Erwartung. Somit können diese beiden Praktiken als Erhöhung der Kooperationsbereitschaft gesehen werden. Diese Erkenntnis könnte z. B. bei der Ausgestaltung von anonymen Arbeitsplätzen von Interesse sein und generell bei Kooperationsdilemmata zu besseren Ergebnissen führen.

Das vierte Kapitel nähert sich der Problematik mittels einer empirischen Untersuchung. Erneut wird eine Situation betrachtet, in der Individuen entscheiden können, ob sie sich kooperativ verhalten. In diesem Fall drückt sich die Kooperation als Einhalten der gesetzlichen Regeln aus. Grundlage der Untersuchung sind Daten aus einer Befragung jugendlicher Straftäter während ihrer Inhaftierung. Mit Hilfe dieser Daten werden, unter Verwendung von verschiedenen ökonometrischen Analyseinstrumenten, Einflussfaktoren auf die selbstberichtete Rückfallwahrscheinlichkeit identifiziert. Hierbei wird insbesondere die Hypothese getestet, ob sich eine Verurteilung nach Erwachsenenstrafrecht vorteilhaft auf die Perspektive heranwachsender Delinquenten auswirkt. Die Daten bestätigen diese Hypothese und stehen damit in einem starken Kontrast zu den Ergebnissen US-amerikanischer Studien, die in der Regel zu dem Ergebnis kommen, dass eine Verurteilung nach Erwachsenenrecht die Rückfallwahrscheinlichkeit erhöht.

Insgesamt kann die Arbeit somit interessante und schlüssige Ergebnisse nachweisen, die in ihrem fünften und letzen Kapitel kurz zusammengefasst werden.

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1

Introduction

For me, cooperation is one of the most fascinating outcomes of human behavior. There are two sources of this fascination. First, as can be shown in a simple Prisoner's Dilemma, cooperation can outperform individually rational (utility maximizing) behavior in terms of joint welfare. Second, on top of being more efficient, cooperative behavior also is observed more often than predicted by classic economic models. The fundamental question behind cooperation are the incentives that lead to welfare enhancing behavior.

One important aspect in the attempt to understand these incentives is the individual context. Even though the scientific goal must be the development of widely applicable theories that have general validity, I find it important to allow for the inclusion of situational parameters. Put differently, even though it might be a good idea to stick to the classic utility maximization paradigm which is at the heart of the bulk of the economic literature, it might be fruitful to extend the self-centered view of economics as done by several economists in the last decades (see Camerer (2003) for a general survey and e.g. Bénabou and Tirole (2006), Ellingsen and Johannesson (2008) for social preferences in the context of incentives). Since some economists are still skeptic about the validity of the new approaches I find it important to remind them of the roots of such a view. Already Adam Smith (1759) in his Theory of Moral Sentiments outlined the social dimension of human preferences claiming that "all the social and benevolent affections, when expressed in the countenance or behavior, even towards those who are not peculiarly connected with ourselves, please the indifferent spectator upon almost every occasion" Smith (1759 [1976], p.38-39). As pointed out by Fontaine (1997), he continued on this thought in Wealth of Nations, claiming that mutual understanding of the motives of economic agents is necessary to successfully implement trade. Today, this idea is reflected in the so-called "common knowledge" assumption (Aumann 1976) in game theory, meaning that individuals reflect on the actions of the other players knowing that these do the same. Hence, strategic interaction by definition has an other-regarding component which needs to be properly taken into account into economic models.

The following sections contain essays which look at different economic situations and analyze the incentives of the agents involved and their effects on the willingness to cooperate.

In my first essay, I model a market with two firms and show under which circumstances even here - in a concentrated market and in the absence of other-regarding preferences - the efficient outcome can be established. Based on the seminal paper of Hotelling (1929) and its extension to quadratic demand by d'Aspremont et al. (1979), I introduce elastic total demand by allowing for a reservation value of the consumers. When transportation costs are increasing and/or the valuation of the product is decreasing, firms have an incentive to move towards the quartiles of the location space. This result shows that a duopoly can yield the efficient outcome under certain conditions. The analysis fills a gap in the literature on spatial demand models. In addition, the finding of a potentially efficient market in a duopoly offers new insights for regulation theory.

In the second essay we use an economic experiment to test the guilt aversion hypothesis stating that individuals increase (decrease) their willingness to cooperate if they see that their co-player is (not) expecting cooperation. Recent papers often could not confirm this hypothesis in the lab. We suggest a more refined framework using a two round dictator game and find that dictators do react to other's expectations. While reproducing the result of existing studies and rejecting the guilt aversion hypothesis in the first round, we show that first order beliefs and transfers are positively correlated if dictators experience the situation for the second time. Hence, our results suggest that the concept of guilt aversion should not be rejected but needs to be refined. In addition, the two-round setup allows us to control for individual effects representing a new general approach going well beyond this particular study.

The third essay looks at cooperation from an empirical angle analyzing German prison data. In this context, cooperation can be seen as compliance with the law. Hence, it is interesting to identify conditions that reduce the likelihood of (re-)offending. In particular we test the hypothesis whether sentencing adolescents as adults influences the self-reported probability of recidivism. We first apply standard econometric models to the data and identify several social and socioeconomic factors of expected recidivism, like job expectation, social networks and age. Then, we perform a bivariate probit estimation and apply a regression discontinuity design in order to identify the effect of criminal law on juvenile offenders. Opposed to the bulk of the literature, which mainly relies on US data, we do not find that the application of criminal law increases juvenile recidivism. Rather, our results suggest that sentencing adolescents as adults reduces recidivism in Germany.

In conclusion, the essays work out different aspects that can foster welfare enhancing cooperation as briefly summarized in the last chapter.

CHAPTER

2

On the limits of the Principle of Maximum differentiation

2.1 Introduction

In his seminal paper, Hotelling (1929) presents a one-dimensional model of unit demand showing that competition between two firms would lead them to minimize spatial differentiation. This result has been challenged by d'Aspremont et al. (1979) who show that a minimum differentiation equilibrium in pure strategies fails to exist for sufficiently closely (but not identically) located firms. While Osborne and Pitchik (1987) derive equilibria close to the quartiles by allowing for mixed strategies, d'Aspremont et al. (1979) suggest a modification of the consumer's transportation cost function. When replacing the linear with a quadratic function they find the opposite result: a principle of maximum differentiation.

Besides the advantage of equilibrium existence, quadratic transportation costs can also be motivated intuitively. In the geographic sense, linear transportation costs might still be a good assumption, e.g. think of gasoline expenses.[1] However, even in this context quadratic costs can be appealing when including a convex disutility from traveling. The nonlinear property of distance becomes even more prominent when moving from a geographic location towards a product differentiation context.[2] In this sense, the consumer's location pins down her preferred product type, and transportation costs describe the loss in utility when consuming a product not being identical with his preferred product type. Here, quadratic distance costs seem to be more appropriate as illustrated by the following example. Imagine buying a car with size being the

[1] The linearity assumption might be threatened if the consumer can choose between different modes of transportation allowing for decreasing marginal costs. However, in this context, this issue is seen as less important, assuming the transportation mode as given for a specific application context

[2] Peitz (1999) finds one-dimensional models of unit demand to dominate the literature on product differentiation and summarizes the limits of their transferability.

differentiation dimension: You might still enjoy a car that is slightly smaller than your preferred type while a very tiny car can be expected to induce a disproportionately smaller level of utility.

A drawback of d'Aspremont et al. (1979) is its inelasticity of total demand. As already pointed out by Lerner and Singer (1937) it might be problematic to assume that consumers will always buy a unit of the good, independent of the prices charged by the firms. As a remedy they suggested to include a (limited) valuation of the good which results in a reservation price of the consumers. Economides (1984) builds on this idea and introduces a reservation price to Hotelling's model. He finds different equilibria, ranging from minimum to intermediate differentiation, depending on the reservation price. Hinloopen and van Marrewijk (1999) add to this result by analyzing location equilibria in more detail. However, the literature is still missing the identification of equilibria in the case of quadratic transportation costs.

This paper fills this gap finding equilibrium locations that depend on the joint market parameter $\theta \equiv \frac{v-c}{t}$ which is defined as the ratio of market attractiveness (expressed by the spread between reservation price v and marginal cost c) and market fragmentation (expressed by the transportation cost parameter t).[3] Both high attractiveness and low fragmentation increase the competitiveness in the market. Intuitively speaking, a market with a high θ is a competitive market that is not large enough for firms to avoid competition. With the market parameter being high enough, both firms focus on differentiation. They find it optimal to reduce competition by moving away from each other allowing them to set higher equilibrium prices. This results in maximum differentiation confirming d'Aspremont et al. (1979) which is thus a nested solution of my model. As θ decreases, the competitiveness of the market is reduced and the need to differentiate is reduced. This allows firms to locate more centrally and they move towards the quartiles. When further decreasing the market parameter, the degree of competition also further declines and firms can act as local monopolists. As a consequence, there will not be a single equilibrium location but rather a range of optimal locations around the quartiles.

In conclusion, the model balances two forces: a differentiation tendency that dominates given a sufficiently attractive market (with a relatively high reservation price), and a centralization tendency that emerges given a sufficiently fragmented market (with relatively high transportation costs) allowing firms to act as local monopolists. The resulting equilibrium locations at the quartiles are of particular interest since they represent the efficient outcome. Hence, the regulator should should take this into account when deciding upon market regulations. Even though the optimal regulation would require knowledge about the market parameter, which might be hard to identify, the model shows that it is generally possible to steer firms towards the quartiles, e.g. introducing a suitable taxation policy.

In a related paper, Rath and Zhao (2001) also analyze a model of location choice with quadratic transportation costs and a reservation price. However, they use a different demand structure following Smithies (1941) who suggested individual demand functions that are inversely related to the retail price. They confirm the principle of maximum differentiation for a sufficiently large reservation price and find a tendency of the firms to move towards minimum differentiation as the reservation price decreases. Their demand function allows for the purchase of more than one good from the same firm. While this might be a good model for consumption goods, it does not seem appropriate for durable goods like cars, since consumers will most likely not buy the same car twice.

[3] Even though the results of my model would also hold in the context of product differentiation, I will stick to the terminology of the original spatial location model.

In chapter 2, I describe the setup of the model, chapter 3 analyzes all possible price equilibria while chapter 4 describes equilibria in location choice. The last chapter summarizes the results.

2.2 The Model

Consider a market with consumers uniformly distributed along the unit interval $[0, 1]$. There are two firms a and b that choose own locations x_a and x_b, with $0 \leq x_a \leq x_b \leq 1$ and sell a product of reservation value v to the consumers. Both firms face equal constant marginal cost $c < v$. Each firm i sets a price p_i, inducing a demand q_i and profits $\pi_i = (p_i - c)q_i$. Every consumer can choose between buying one unit of the good or not buying at all. When consuming the product, consumers face quadratic transportation costs between their own location x_c and the location of the firm they buy the good from. Transportation costs are scaled by the transportation cost rate $\frac{t}{2}$, with $t \geq 0$. Net utility of consumers amounts to $U_c(p_i, x_i) = v - p_i - \frac{t}{2}(x_i - x_i)^2$ when buying from firm i.

The setup represents a two stage game: (1) firms choose location, (2) firms compete in prices. The subgame perfect equilibrium of this game can be identified by backward induction. When choosing the optimal strategy, firms consider their individual demand function. I assume that consumers can observe both prices and locations of the firms. This implies that consumers choose to buy the good from firm i if $U_c(p_i, x_i) \geq 0$ and $U_c(p_i, x_i) \geq U_c(p_j, x_j)$, with $i, j\epsilon(a, b)$ and $i \neq j$. Following from this condition we can define each firm's potential demand, i.e. the mass of consumers whose transportation costs $\frac{t}{2}(x_i - x_c)^2$ do not exceed $v - p_i$ yielding nonnegative net utility. The corresponding set of consumers is bounded by firm specific marginal consumers x_{mi}, whose location can be identified by solving $U_{mi}(p_i, x_i) = 0$ for x_{mi}, with $i \,\epsilon\, \{a, b\}$:

$$x_{mi}(p_i, x_i) = x_i \pm \sqrt{2\frac{v - p_i}{t}}. \tag{2.1}$$

Hence, there are two marginal consumers for each firm: one in the firm's hinterland (outer marginal consumer) and one in direction of the other firm (inner marginal consumer). By definition of these marginal consumers, firm i's demand is restricted to the area between them. Hence, (2.1) already reveals the basic structure of demand in this model: it consists of an outer and an inner part and cannot exceed the following sum: $\sqrt{2\frac{v-p_i}{t}} + \sqrt{2\frac{v-p_i}{t}}$. Both summands, however, are subject to further limitations.

First, we look at the outer part (demand in the hinterlands). If a firm specific marginal consumer lies outside the unit interval ($x_{ma} \leq 0$ or $x_{mb} \geq 1$), demand can be realized only up to the boundaries. Second, there is also a limitation to the inner part of demand. If firms are sufficiently close to each other and their potential demand areas overlap, consumers will only buy at the firm where the sum of price and transportation costs is smaller. This results in an additional marginal consumer limiting both firms' demand towards the middle. I call this the within marginal consumer and identify his location by solving $U_m(p_a, x_a) = U_m(p_b, x_b)$ for x_m:

$$x_m = \frac{(x_b + x_a)}{2} + \frac{p_b - p_a}{t(x_b - x_a)}. \tag{2.2}$$

Since transportation costs are convex, consumers' net utility is monotone in distance, and there is only one within marginal consumer x_m.[4] As a consequence, firm a (b) can realize

[4]See Economides (1984) for a general proof for a unique intersection of the value functions.

demand only to the left (right) of x_m, generating a potential demand of $\frac{x_b-x_a}{2}+\frac{p_b-p_a}{t(x_b-x_a)}$ $(\frac{x_b-x_a}{2}-\frac{p_b-p_a}{t(x_b-x_a)})$.

Figure 1 illustrates the locations of the marginal consumers: for exemplary location pairs, reservation price and price plus transportation costs are plotted. In addition, resulting demands are indicated.

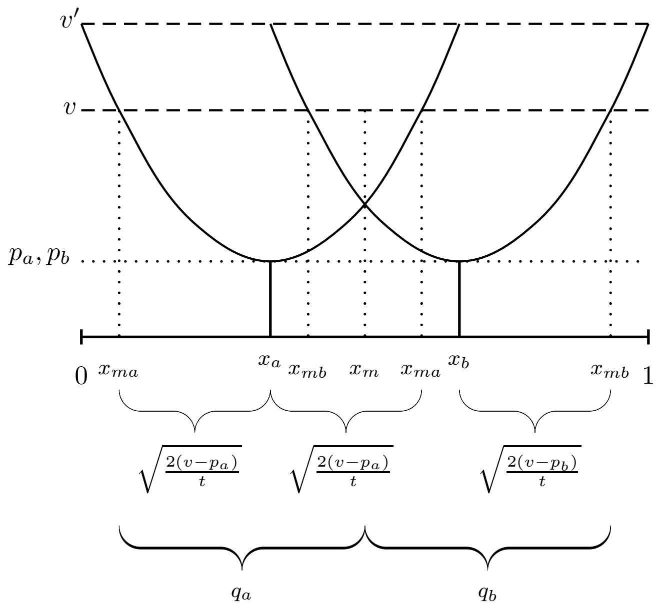

Figure 2.1: Competitive Price Equilibria

Summing up formally, the two parts and its limitations constitute the following demand functions:

$$\begin{aligned} q_a = &\quad \min\left\{x_a, \sqrt{2\tfrac{v-p_a}{t}}\right\} + \min\left\{\tfrac{x_b-x_a}{2}+\tfrac{p_b-p_a}{t(x_b-x_a)}, \sqrt{2\tfrac{v-p_a}{t}}\right\} \\ q_b = &\quad \min\left\{\tfrac{x_b-x_a}{2}-\tfrac{p_b-p_a}{t(x_b-x_a)}, \sqrt{2\tfrac{v-p_b}{t}}\right\} + \min\left\{1-x_b, \sqrt{2\tfrac{v-p_b}{t}}\right\} \end{aligned} \tag{2.3}$$

Due to the minimum operator there are 4 different cases for each firm. Table 2.1 describes these cases for firm a:

Table 2.1: Possible Cases of Demand

Case	Hinterland	Middle	Demand
1	Covered	Covered	$q_a^1 = x_a + \left(\frac{x_b-x_a}{2}+\frac{p_b-p_a}{t(x_b-x_a)}\right)$
2	Not Covered	Covered	$q_a^2 = \sqrt{2\frac{v-p_a}{t}} + \left(\frac{x_b-x_a}{2}+\frac{p_b-p_a}{t(x_b-x_a)}\right)$
3	Covered	Not Covered	$q_a^3 = x_a + \sqrt{2\frac{v-p_a}{t}}$
4	Not Covered	Not Covered	$q_a^4 = \sqrt{2\frac{v-p_a}{t}} + \sqrt{2\frac{v-p_a}{t}}$

Cases 1 and 2 represent equilibria with direct competition (when every consumer between

firms is being served) and cases 3 and 4 represent local monopolies (when the within marginal consumer is not binding). In both groups, hinterlands can either be covered (outer firm specific marginal consumer is binding) or not. The following chapter provides an analysis of different possible combinations.
It will turn out that $\theta \equiv \frac{v-c}{t}$ is crucial for the case distinctions. This ratio combines the market parameters measuring the intensity of competition. It increases in the profitability of the market, being represented by the difference of the reservation value of the consumers (v) and marginal cost (c), and decreases in the transportation cost rate (t) which represents market segregation that prevents competition. Put differently, a market with a low θ is very 'big' since consumers are very sensitive to distance and/or possible profits are low. A market with a high θ will be characterized by attractive profits and low distance costs facilitating competition.

2.3 Price Equilibria

In this section I derive price equilibria for every case in table 2.1. The equilibrium description always consists of two types of information. First, it provides the optimal price given a certain location. Second, it defines the subset of symmetric locations for which the equilibrium is feasible.[5] Price equilibria will be classified as competitive, local monopolistic and touching, as proposed by Economides (1984). The different cases will be numbered from 1 to 4, choice variables will be denoted by $"^{nb}"$ with nb representing the case number.

2.3.1 Fully Competitive Equilibria

First, let's look at equilibria where the within marginal consumer is binding. Depending on location and parameters, hinterlands are either fully covered (case 1) or not (case 2).

Full demand (case 1)

Full demand requires that no firm specific consumer is binding. Formally, in equilibrium the following feasibility condition (FC1) must hold.

$$\begin{aligned} \sqrt{2\frac{v-p_a}{t}} &\geq \max\left\{x_a, \frac{(x_b-x_a)}{2} - \frac{p_a-p_b}{t(x_b-x_a)}\right\} \\ \sqrt{2\frac{v-p_b}{t}} &\geq \max\left\{1-x_b, \frac{(x_b-x_a)}{2} + \frac{p_a-p_b}{t(x_b-x_a)}\right\} \end{aligned} \tag{FC1}$$

In figure 1, this case would be represented for $v > v'$. This scenario is equal to the absence of a reservation price v and has been studied by d'Aspremont et al. (1979). Given these assumptions, demand (2.3) simplifies to:

$$\begin{aligned} q_a^1(x_a, x_b, p_a, p_b) &= \frac{(x_b+x_a)}{2} + \frac{p_b-p_a}{t(x_b-x_a)} \\ q_b^1(x_a, x_b, p_a, p_b) &= 1 - \frac{(x_b+x_a)}{2} - \frac{p_b-p_a}{t(x_b-x_a)} \end{aligned} \tag{2.4}$$

and profits become

$$\begin{aligned} \pi_a^1(x_a, x_b, p_a, p_b) &= (p_a - c)\left(\frac{(x_b+x_a)}{2} + \frac{p_b-p_a}{t(x_b-x_a)}\right) \\ \pi_b^1(x_a, x_b, p_a, p_b) &= (p_b - c)\left(1 - \frac{(x_b+x_a)}{2} - \frac{p_b-p_a}{t(x_b-x_a)}\right) \end{aligned} \quad . \tag{2.5}$$

[5] Here, the symmetry assumption is needed to get a unique subset. It can be motivated since equilibrium profits are monotone in location.

Combining first order conditions gives a unique Nash Equilibrium:

$$\begin{array}{c} p_a^{*1}(x_a, x_b) = c + \frac{t}{3}(x_b - x_a)\left(1 + \frac{x_b + x_a}{2}\right) \\ p_b^{*1}(x_a, x_b) = c + \frac{t}{3}(x_b - x_a)\left(2 - \frac{x_b + x_a}{2}\right) \end{array}. \tag{2.6}$$

Lemma 1 *For*

$$x_a \geq \underline{x_a} = \begin{cases} \frac{3}{2} - \sqrt{2\theta + 1} & if \quad \theta \geq \frac{9}{32} \\ 1 - \sqrt{2\theta} & if \quad \theta < \frac{9}{32} \end{cases}. \tag{2.7}$$

and

$$x_b \leq \overline{x_b} = \begin{cases} \frac{1}{2} + \sqrt{2\theta + 1} & if \quad \theta \geq \frac{9}{32} \\ \sqrt{2\theta} & if \quad \theta < \frac{9}{32} \end{cases}. \tag{2.8}$$

there is a Nash Equilibrium with firms choosing $p_a = p_a^{*1}$ and $p_b = p_b^{*1}$. Profits increase in spatial differentiation.

Proof. Plugging (2.6) into (FC1) and adding the symmetry assumption for locations yields the marginal locations that still guarantee full market coverage when setting p^{*1}. Since $\frac{\partial \pi_a}{\partial x_a} < 0$ and $\frac{\partial \pi_b}{\partial x_b} > 0$ it is optimal to choose the most extreme location possible. For a formal derivation of the marginal locations see in the appendix. ■

Uncovered fringes (case 2)

Next, I characterize possible price equilibria where markets are so "big" (θ is so low) that the optimal price is too high to cover their own hinterland. Intuitively, this requires firms to locate sufficiently central. Figure 1 depicts this case (for $v < v'$).

To meet these assumptions, the inner parts of demand must overlap while the outer parts must not reach the end of consumer space. This requirement can be expressed as the following feasibility condition (FC2):

$$\begin{array}{r} x_a > \sqrt{2\frac{v - p_a}{t}} > \frac{d}{2} - \frac{p_a - p_b}{td} \\ 1 - x_b > \sqrt{2\frac{v - p_b}{t}} > \frac{d}{2} + \frac{p_a - p_b}{td} \end{array}. \tag{FC2}$$

The demand functions then look as follows:

$$\begin{array}{c} q_a^2 = \frac{(x_b - x_a)}{2} + \frac{p_b - p_a}{t(x_b - x_a)} + \sqrt{2\frac{v - p_a}{t}} \\ q_b^2 = \frac{(x_b - x_a)}{2} - \frac{p_b - p_a}{t(x_b - x_a)} + \sqrt{2\frac{v - p_b}{t}} \end{array}. \tag{2.9}$$

Profits will be:

$$\begin{array}{c} \pi_a^2 = (p_a - c)\left(\frac{(x_b - x_a)}{2} + \frac{p_b - p_a}{t(x_b - x_a)} + \sqrt{2\frac{v - p_a}{t}}\right) \\ \pi_b^2 = (p_b - c)\left(\frac{(x_b - x_a)}{2} - \frac{p_b - p_a}{t(x_b - x_a)} + \sqrt{2\frac{v - p_b}{t}}\right) \end{array}. \tag{2.10}$$

One can see that both profit functions only depend on the difference between locations, not on the absolute locations. Hence, individual locations can be reduced to distance $d \equiv x_b - x_a$. Taking first order conditions of (2.10) with respect to own price yields the following two equilibrium conditions:

$$\begin{array}{c} p_a = c + 2(p_b - c) - d^2\frac{t}{2} + \frac{(2v + c - 3p_b)}{\sqrt{\frac{2}{t}(v - p_b)}}d \\ p_b = c + 2(p_a - c) - d^2\frac{t}{2} + \frac{(2v + c - 3p_a)}{\sqrt{\frac{2}{t}(v - p_a)}}d \end{array} \tag{2.11}$$

These conditions yield

$$p^{*2} = \begin{cases} c + \frac{1}{3}t\left(\theta - 14d^2\right) - \frac{2}{3}t\sqrt{s}\cos\left(\frac{1}{3}\arccos\left(A\right)\right) & if \quad \sqrt{\frac{4}{3}\theta} \geq d \\ c + \frac{1}{3}t\left(\theta - 14d^2\right) - \frac{2}{3}t\sqrt{s}\cos\left(\frac{1}{3}\arccos\left(A\right) + \frac{1}{3}\pi\right) & if \quad \sqrt{\frac{4}{3}\theta} < d \end{cases} . \tag{2.12}$$

as symmetric mutual best response, where A and s are functions of d,t and θ (for details see Appendix).

Lemma 2 *If*

$$\min\left\{\frac{x_a - 2x_a^2}{1 - x_a} + x_a^2, -\frac{1}{x_b}\left(-x_b^3 + 4x_b^2 - 4x_b + 1\right)\right\} > 2\theta \tag{2.13}$$

and

$$(x_b - x_a) < (x_b - x_a)_{\max} = \sqrt{\frac{24}{11}\theta} \tag{2.14}$$

there is a Nash Equilibrium with $p_a^* = p_b^* = p^{*2}$. Profits increase in spatial differentiation.

Proof. Combining (2.11) and (FC2) implies the symmetric marginal locations (for details see section 2.6.2 in the appendix). Taking first order condition of (2.12) yields $p_i^* = -q^2\left(p^*\right)\left(\frac{\partial q^2(p_i^*)}{\partial p}\right)^{-1}$. Further, the optimal price will decrease in centralization, which can be shown using the envelope theorem: $\frac{\partial p_i^*}{\partial x_a} = -\frac{\partial q^2(p^*)}{\partial x_a}\left(\frac{\partial q^2(p_i^*)}{\partial p}\right)^{-1} + q^2\left(p^*\right)\left(\frac{\partial q^2(p_i^*)}{\partial p}\right)^{-2}\left(\frac{\partial^2 q^2(p_i^*)}{\partial p \partial x_a}\right) < 0$. Using this result, also $\frac{\partial \pi_i^{*2}}{\partial x_a} = \frac{\partial p_i^*}{\partial x_a} q^2(p_i^*) + p_i^* \frac{\partial q_i^{*2}}{\partial x_a} < 0$. Hence, profits increase in spatial differentiation. ∎

Corner solutions

Figure 2.2 shows the feasible ranges of the derived equilibria in this subsection (cases 1 and 2) for firm a. Shaded in grey are all combinations of location (x_a, on the abscissa) and market paramter ($\theta = \frac{v-c}{t}$, on the ordinate) for which optimal prices p^{*1} and p^{*2} are feasible, i.e. for which these prices generate demand in accordance with the respective case assumptions. As can be seen in figure 2.2, the feasible ranges do not overlap. Rather, there are locations in the inner quartiles, for which both otimal prices are infeasible, meaning that setting p^{*1} would violate (FC1) and setting p^{*2} would violate (FC2).

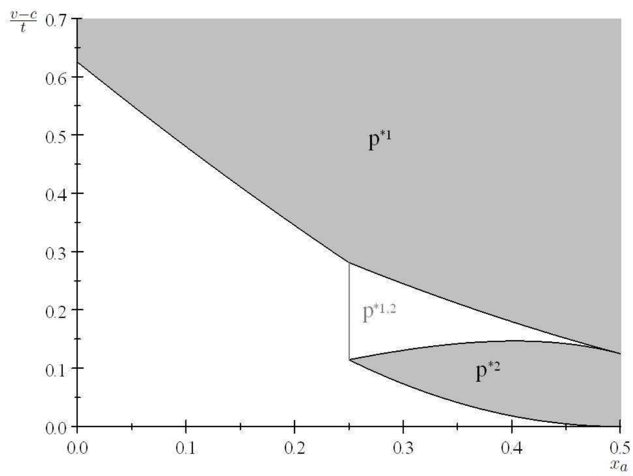

Figure 2.2: Feasible ranges for fully competitive price equilibria

More intuitively, all those combinations of location and the market parameter are excluded, when p^{*1} is too high, inducing demand that does not cover the fringes, while p^{*2} is so low that everyone demands the good even though this was ruled out by the subcase. This result pushes the analysis towards corner solutions. When adjusting both optimal prices to the best feasible price, they coincide and will just guarantee full demand.[6] Corner solutions are defined by (2.17) and depicted in figure 1 for $v = v'$.

Lemma 3 *If*

$$\frac{1}{4} < x_a < \max\left\{1 - \sqrt{2\theta}, x_b - \sqrt{\frac{24}{11}\theta}\right\} \tag{2.15}$$

and

$$\frac{3}{4} > x_b > \max\left\{\sqrt{2\theta}, x_a + \sqrt{\frac{24}{11}\theta}\right\} \tag{2.16}$$

there is a Nash Equilibrium with

$$\begin{array}{c} p_a^* = p_a^{*1,2} = v - \frac{t}{2}x_a^2 \\ p_b^* = p_b^{*1,2} = v - \frac{t}{2}\left(1 - x_b\right)^2 \end{array} . \tag{2.17}$$

Profits increase in spatial differentiation.

Proof. If $p^{*1,2}$ induces a Nash Equilibrium, both firms must have no incentive to increase or decrease the price. When decreasing price at $p^{*1,2}$, firms face q^1. Since $\frac{d\pi_a^1}{dp_a} \geq 0$ for all $p \leq p^{*1}$ and $p^{*1,2} \leq p^{*1}$ there is no incentive to decrease. When increasing price at $p^{*1,2}$, firms face q^2. Since $\frac{d\pi_a^2}{dpa} \leq 0$ for all $p \geq p^{*2}$ and $p^{*1,2} \geq p^{*2}$ there is also no incentive to decrease. Feasibility conditions are implied by Lemma 2 and 3. ■

2.3.2 Local monopolies

Next, we analyze the cases where the within marginal consumer is not binding and hence firms act as local monopolists. This requires the market parameter θ to be sufficiently low, represented by $v < v''$ in figure 3. In addition, demand in the fringes can either be limited by the end of consumer space, resulting in asymmetric demand (case 3, represented by $v > v'$) or by the firm specific marginal consumer, resulting in symmetric demand (case 4, represented by $v < v'$).

Local monopolies with asymmetric demand (case 3)

Assume a scenario where firms act as local monopolies and face asymmetric demand, i.e. assume $v'' > v > v'$ in figure 3. More explicitly, assume the following feasibility conditions to hold:

$$\begin{array}{c} x_a < \sqrt{2\frac{v-p_a}{t}} < \frac{(x_b-x_a)}{2} - \frac{\Delta p}{t(x_b-x_a)} \\ 1 - x_b < \sqrt{2\frac{v-p_b}{t}} < \frac{(x_b-x_a)}{2} + \frac{\Delta p}{t(x_b-x_a)} \end{array} . \tag{FC3}$$

[6]Technically, a corner solution can only be included in case 1, since case 2 is defined as an open set. Nevertheless, the best feasible response of case 2 converges to the best feasible response of case 1.

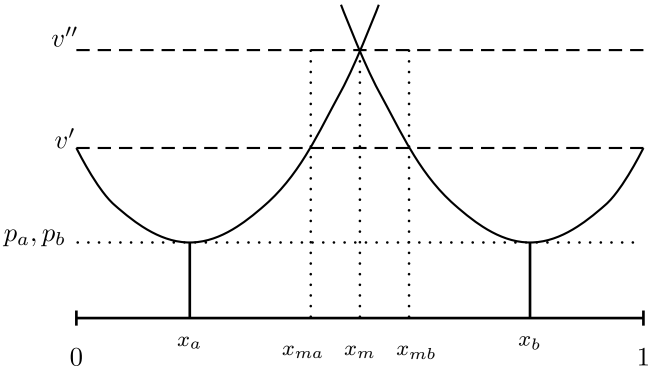

Figure 2.3: Local Monopolistic Equilibria

Hence, firms will face demand according to:

$$\begin{aligned} q_a^3 &= x_a + \sqrt{\tfrac{2}{t}\left(v - p_a\right)} \\ q_b^3 &= 1 - x_b + \sqrt{\tfrac{2}{t}\left(v - p_b\right)} \end{aligned} \quad . \tag{2.18}$$

Emerging profits are

$$\begin{aligned} \pi_a^3 &= \left(p_a - c\right)\left(x_a + \sqrt{\tfrac{2}{t}\left(v - p_a\right)}\right) \\ \pi_b^3 &= \left(p_b - c\right)\left(1 - x_b + \sqrt{\tfrac{2}{t}\left(v - p_b\right)}\right) \end{aligned} \quad . \tag{2.19}$$

Optimal prices can be derived from the first order conditions immediately and are by definition independent of the other firm's location.

$$\begin{aligned} p_a^{*3} &= \tfrac{1}{3}c + \tfrac{2}{3}v - \tfrac{1}{9}tl^2 + \tfrac{1}{9}x_a\sqrt{t^2x_a^2 + 6t(v-c)} \\ p_b^{*3} &= \tfrac{1}{3}c + \tfrac{2}{3}v - \tfrac{1}{9}t\left(1 - x_b\right)^2 + \tfrac{1}{9}x_a\sqrt{t^2\left(1 - x_b\right)^2 + 6t(v-c)} \end{aligned} \quad . \tag{2.20}$$

Lemma 4 *If*

$$x_a < x_{a\max 3} = \min\left\{\sqrt{\frac{2}{5}\theta}, 1 - \frac{1}{2}\sqrt{8\theta + 1}\right\} \tag{2.21}$$

and

$$x_b > x_{b\min 3} = \max\left\{1 - \sqrt{\frac{2}{5}\theta}, \frac{1}{2}\sqrt{8\theta + 1}\right\} \tag{2.22}$$

there is a Nash-Equilibrium with both firms setting p^{*3}. Profits are decreasing in spatial differentiation.

Proof. Plugging (2.20) into (FC3) adding symmetry yields marginal locations (for details see appendix). By definition of this case, demands of the two firms are independent of each other. Hence, a Nash Equilibrium is also a pair of individually optimal prices. It can be shown that for feasible locations $\frac{\partial \pi(p^{*3})}{\partial x_a} > 0$ and $\frac{\partial \pi(p^{*3})}{\partial x_b} < 0$. ■

The derived location tendencies are quite intuitive, since moving away from the demand cutting boundary without sacrificing demand in the interior increases demand and ceteris paribus also profit.

Local monopolies with symmetric demand (case 4)

Now let's look at the case where firms can realize the optimal monopolistic outcome. Case conditions require that the optimal price induces demands that neither overlap nor reach the ends of consumer space. For such a constellation, the following feasibility condition has to hold:

$$\begin{array}{r} \sqrt{2\frac{v-p_a}{t}} \leq \min\left\{x_a, \frac{(x_b-x_a)}{2} - \frac{\Delta p}{t(x_b-x_a)}\right\} \\ \sqrt{2\frac{v-p_b}{t}} \leq \min\left\{1-x_b, \frac{(x_b-x_a)}{2} + \frac{\Delta p}{t(x_b-x_a)}\right\} \end{array} . \tag{FC4}$$

In this case demand is symmetric and amounts to:

$$q^4(p_i) = 2\sqrt{\frac{2}{t}(v-p_i)}. \tag{2.23}$$

The resulting profit function is

$$\pi^4(p_i) = (p_i - c)2\sqrt{\frac{2}{t}(v-p_i)}. \tag{2.24}$$

Optimal prices are

$$p_a^* = p_b^* = p^{*4} = \quad \tfrac{1}{3}c + \tfrac{2}{3}v \tag{2.25}$$

By construction, optimal price and profit are independent of location. Less intuitive, p^{*4} is even independent of t. as shown in Lemma 5.

Lemma 5 *For*

$$x_a \geq \sqrt{\frac{2}{3}\theta}. \tag{2.26}$$

and

$$x_a + \sqrt{\frac{8}{3}\theta} \leq x_b \leq 1 - \sqrt{\frac{2}{3}\theta}. \tag{2.27}$$

there is a Nash Equilibrium with both firms choosing p^{*4}. Profits are independent of spatial differentiation.

Proof. Marginal locations can be derived by plugging (2.25) into (FC4). By case assumptions both firms do not interfere in demand. Thus, a Nash Equilibrium again is a also a pair of individually optimal prices. $\frac{\partial \pi_a^4(p^{*4})}{\partial x_a} = \frac{\partial \pi_b^4(p^{*4})}{\partial x_b} = 0$. ■

Corner Solutions

Again, the ranges where either p^{*3} or p^{*4} is feasible (shaded in grey in figure 2.4, illustrated for for firm a) produce a gap between themselves. Intuitively, for θ-x_a-combinations in between the two feasible regions, p^{*3} is too high to induce demand covering the fringes (FC3), while p^{*4} is too low to constitute uncovered fringes, thereby violating (FC4).

When adjusting both optimal prices to the best feasible price, they coincide and will just cover the fringes.

Lemma 6 *If*

$$\sqrt{\frac{2}{3}\theta} > x_a \geq \sqrt{\frac{2}{5}\theta} \tag{2.28}$$

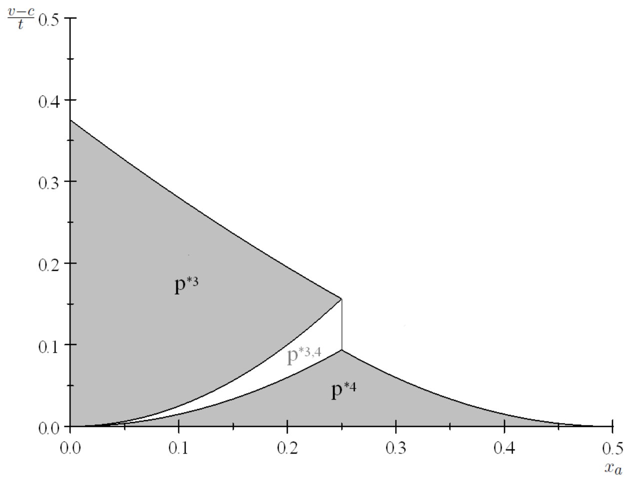

Figure 2.4: Feasible ranges for local monopolistic price equilibria

and

$$\sqrt{\frac{2}{3}\theta} > 1 - x_b \geq \sqrt{\frac{2}{5}\theta} \tag{2.29}$$

there is a Nash Equilibrium with

$$\begin{array}{c} p_b^* = p_b^{*3,4} = v - \frac{t}{2}x_a^2 \\ p_b^* = p_b^{*3,4} = v - \frac{t}{2}\left(1 - x_b\right)^2 \end{array} . \tag{2.30}$$

Profits are increasing in spatial differentiation.

Proof. If $p^{*3,4}$ induces a Nash Equilibrium, both firms must have no incentive to increase or decrease the price. When decreasing price at $p^{*3,4}$, firms face q^3. Since $p^{*1,2} \leq p^{*1}$ and $\frac{d\pi_a^3}{dp_a} \geq 0$ for all $p \leq p^{*3}$, price is already suboptimally low and hence there is no incentive to decrease. This suggests an incentive to increase the price. However, when increasing price at $p^{*3,4}$, firms face q^4. Since $\frac{d\pi_a^4}{dp_a} \leq 0$ for all $p \geq p^{*4}$ and $p^{*3,4} \geq p^{*4}$ there is also no incentive to decrease (see Appendix for details). ■

2.3.3 Touching Equilibria

It remains to analyze the regions, where neither a fully competitive nor a local monopolistic equilibrium exists. In a sense, those equilibria are also corner solutions. However, while so far corner solutions represented scenarios where demand just reached the ends of consumer space, the class of touching equilibria are defined when demands of the firms just cover the center, or just 'touch'. As in the first two classes of equilibria, the fringes again can either be fully covered or not.

Touching equilibrium with fringes covered

For locations in question, p^{*3} will be so low, that it contradicts case conditions of local monopolies. On the other hand, p^{*1} is too high to cover the whole market. Technically, corner solutions are only possible in the full demand world (case 1), since (FC3) defines an open set. However, the best feasible price satisfying (FC3) also converges to the same value. Intuitively, optimal prices will be set such that demands of the two firms just touch.

Lemma 7 *For $x_{a\min 1} > x_a > x_{a\max 3}$ and $x_{b\max 1} < x_b < x_{b\min 3}$ there is a Nash equilibrium with both firms setting p^{*touch}. Profits decrease in spatial differentiation.*

$$\begin{array}{l} p_a^{*touch} = v - \frac{1}{2}t\left(\sqrt{\frac{2}{t}\left(v - p_b\right)} - \left(x_b - x_a\right)\right)^2 \\ p_b^{*touch} = v - \frac{1}{2}t\left(\sqrt{\frac{2}{t}\left(v - p_a\right)} - \left(x_b - x_a\right)\right)^2 \end{array}. \tag{2.31}$$

Proof. If p^{*touch} induces a Nash Equilibrium, both firms must have no incentive to increase or decrease the price. When decreasing the price, resulting demand will be q^1 and thus optimal price would be p^{*1}. Since profits are increasing in price for all $p < p^{*1}$ and infeasibility of p^{*1} implies $p^{*touch} < p^{*1}$, firms have no incentive to decrease the price. For p^{*touch} is already suboptimally low, there would rather be an incentive to increase the price. However, when increasing the price, marginal consumers "lose touch" and their demand function becomes q^3. In this case, optimal price would be p^{*3} and profits are decreasing in price for $p > p^{*3}$. Since infeasibility of p^{*3} implies $p^{*touch} > p^{*3}$, firms have no incentive to increase the price.
It can be shown that $\frac{\partial \pi_a^1\left(p^{*touch}\right)}{\partial x_a} > 0 \frac{\partial \pi_b^1\left(p^{*touch}\right)}{\partial x_b} < 0$ for the feasible range of locations. ■

Touching equilibrium with fringes not fully covered

For locations in question, p^{*2} will be too high to generate demands covering the center, thereby violation (FC2). Also p^{*4} will violate its feasibility constraint, since it is too low to bring on local monopolies. Technically, a corner solution can only be contained in case 2. Hence, p^{*2} will have to be lowered until inner global marginal consumers just touch. The same value emerges when taking p^{*4} to the limits of its feasibility constraint, generating just separated demands.

Lemma 8 *If*

$$\frac{24}{9}\sqrt{\theta} > x_b - x_a > \frac{24}{11}\sqrt{\theta} \tag{2.32}$$

and

$$x_a > \sqrt{2\frac{v - p_a}{t}} \text{ and } 1 - x_b < \sqrt{2\frac{v - p_b}{t}} \tag{2.33}$$

there is a (symmetric) Nash Equilibrium with both firms choosing p^{*touch} as defined in (2.31). Profits increase in spatial differentiation.

Proof. In order to get a Nash Equilibrium there must be no incentive to deviate from p^{*touch}. When increasing p^{*touch}, the firm faces symmetric monopolistic demand q^4. Ideal price thus would be p^{*4}. Since $p^{*touch} > p^{*4}$ by definition of touching equilibria and $\frac{\partial \pi^{*4}}{\partial p} < 0$ for all $p > p^{*4}$, there is no incentive to increase. When decreasing the price, firms face q^2 where p^{*2} would be the optimal price. Since $\frac{\partial \pi^{*4}}{\partial p} > 0$ for all $p < p^{*2}$ and infeasibility of p^{*2} implies $p^{*touch} < p^{*2}$, there is also no incentive to decrease.
It can be shown that $\frac{\partial \pi_a^4\left(p^{*touch}\right)}{\partial x_a} < 0 \frac{\partial \pi_b^4\left(p^{*touch}\right)}{\partial x_b} > 0$ for the feasible range of locations. ■

2.4 Location Equilibria

Now, let's move on to the first stage of the game where firms choose locations. As shown in the previous sections, there are different price equilibria depending on the level of θ and depending on the location. Within the range of a given price equilibrium, profits change monotonically in location. More explicitly, firms will prefer to move to one end of the feasible range of a

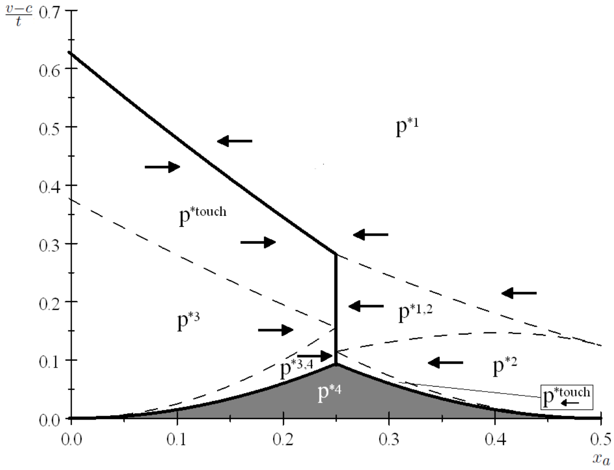

Figure 2.5: Symmetric Equilibrium Locations

given price equilibrium. These location tendencies have already been identified and described in Lemmas 2-9 when deriving the price equilibria. Figure 5 gives a graphical summary of the different equilibrium ranges and its location tendencies, the latter being expressed by arrows. Since these tendencies point to unique locations, optimal (symmetric) locations for a given θ can directly be derived.

For sufficiently high θ, there is only one price equilibrium possible: the equilibrium with full demand (case 1). This yields an unambiguous equilibrium with maximum differentiation.

Proposition 9 *For $\theta \geq \frac{5}{8}$, there is a unique Subgame Perfect Equilibrium with firms choosing to locate at $x_a^* = 0$ and $x_b^* = 1$, setting $p_a^* = p_a^{*1}(0,1) = p_b^* = p_b^{*1}(0,1) = c + \frac{t}{2}$.*

Proof. Using Lemma 1 it is optimal to choose the most extreme location possible, hence $x_a = x_{a\min 1}$ and $x_b = x_{b\max 1}$. Solving jointly for $x_{a\min 1} = 0$ and $x_{b\max 1} = 1$ implies $\theta \geq \frac{5}{8}$. ■

Once $\theta < \frac{5}{8}$, potentially multiple equilibria could occur. However, since in all equilibrium ranges profits are monotone in location and symmetric equilibria exist for all locations, we can focus on symmetric equilibria.

All symmetric equilibrium locations are plotted as thick lines in figure 5. Due to symmetry locations are only plotted for firm L. The formal description of all equilibria can be found in the following final proposition.

Proposition 10 *For $\frac{20}{32} \geq \theta > \frac{9}{32}$ there are symmetric Subgame Perfect Equilibria with firms choosing $x_a = \frac{1}{2} - \sqrt{2\theta+1}$ and $x_b = \frac{1}{2} + \sqrt{2\theta+1}$ and $p = p_l^{*1} = p_l^{*touch}$. For $\frac{9}{32} \geq \theta > \frac{3}{32}$ there are symmetric Subgame Perfect Equilibria with firms choosing $x_a = 0.25$ and $x_b = 0.75$ and $p = p_l^{*touch}$. For $\theta \leq \frac{3}{32}$, firms will choose $x_a \in \left[\sqrt{\frac{2}{3}\theta}, \frac{1}{2} - \sqrt{\frac{2}{3}\theta}\right]$ and $x_b \in \left[\frac{1}{2} + \sqrt{\frac{2}{3}\theta}, 1 - \sqrt{\frac{2}{3}\theta}\right]$ and $p = p^{*4}$.*

Summarizing, there is a unique equilibrium for $\theta > \frac{5}{8}$ leading to maximum differentiation and confirming d'Aspremont et al. (1979). However, whenever θ is sufficiently low, firms will no longer be able to both move towards the end of the location space and serve all customers. As a result, it is no longer optimal to go for maximal differentiation. Rather, firms will move towards the most distant location where they can still realize full demand. This continues until both firms arrive at the quartiles (0.25, 0.75). Firms will prefer to stay at the quartiles even if θ further decreases. Only if $\theta \leq \frac{3}{32}$, there is no longer a clear prediction for symmetric location equilibria. Firms can choose from different locations that will yield identical profits.

The most important result is the existence of constellations where the duopoly will yield the efficient outcome minimizing transportation costs. Given the large range of possible values of θ this result is robust to small changes in the market parameter. Policy makers could take advantage of this possibility and adjust the price level such that it falls into the efficient range. Put differently, a market with $\frac{9}{32} \geq \theta > \frac{3}{32}$ can be efficiently served by two firms.

2.5 Conclusion

With high enough valuation of the good (or small enough transportations costs), results are identical to the findings of d'Aspremont et al. (1979). Hence, the model confirms the general principle that firms relax price competition through product differentiation as pointed out by Shaked and Sutton (1982). With decreasing valuation (or increasing transportation costs), this full demand solution starts to become infeasible, the competitiveness of the market decreases and firms will find it optimal to move towards the quartiles.

To some extent, this outcome is in line with the results from Rath and Zhao (2001) who also model elastic demand in a Hotelling set-up and found that optimal locations move towards the center when transportation costs increase. However, as opposed to their findings, in this paper there is a range of the market parameter where firms stay at the middle of "their half", which induces minimal transportation costs and thus represents an efficient choice of locations. This could be the ground for political interventions that minimize market distortions, e.g. changing the profitability of the market by the introduction of a sales tax. Since the efficient outcome is generated by a range of values of the market parameter ($\frac{9}{32} \geq \theta > \frac{3}{32}$), interventions might still be successful in the presence of noisy market information.

However, even the identification of a noisy market paramter seems to be a hard task and symmetric firms with an identical cost structure might be hard to find. Despite this clear restriction to the applicability of the results, the present model might be the basis for additional future research. There are at least two interesting extensions. One is the introduction of information asymmetries, the second one is the introduction of heterogeneous firms and thus asymmetric equilibrium locations.

2.6 Appendix

2.6.1 Derivation of Lemma 1

Feasibility

Feasibility constraint (FC1) can be split up into four equations which have to hold:

$$\begin{aligned}
x_a &\leq \sqrt{\tfrac{2}{t}\left(v-p_a\right)} \\
\tfrac{(x_b-x_a)}{2}-\tfrac{p_a-p_b}{t(x_b-x_a)} &\leq \sqrt{\tfrac{2}{t}\left(v-p_a\right)} \\
1-x_b &\leq \sqrt{\tfrac{2}{t}\left(v-p_b\right)} \\
\tfrac{(x_b-x_a)}{2}+\tfrac{p_a-p_b}{t(x_b-x_a)} &\leq \sqrt{\tfrac{2}{t}\left(v-p_b\right)}
\end{aligned}$$

After plugging in the optimal prices from (2.6) these conditions can be rewritten as:

$$\begin{aligned}
x_a &\leq \sqrt{\tfrac{2(v-c)}{t}-\tfrac{2}{3}\left(x_b-x_a\right)\left(1+\tfrac{x_b+x_a}{2}\right)} \\
\tfrac{1}{3}-\tfrac{5}{6}x_a+\tfrac{1}{6}x_b &\leq \sqrt{\tfrac{2(v-c)}{t}-\tfrac{2}{3}\left(x_b-x_a\right)\left(1+\tfrac{x_b+x_a}{2}\right)} \\
1-x_b &\leq \sqrt{\tfrac{2(v-c)}{t}-\tfrac{2}{3}\left(x_b-x_a\right)\left(2-\tfrac{x_b+x_a}{2}\right)} \\
-\tfrac{1}{3}-\tfrac{1}{6}x_a+\tfrac{5}{6}x_b &\leq \sqrt{\tfrac{2(v-c)}{t}-\tfrac{2}{3}\left(x_b-x_a\right)\left(2-\tfrac{x_b+x_a}{2}\right)}
\end{aligned}$$

Next, I substitute in the market parameter $\theta=\frac{v-c}{t}$ and impose symmetry on the locations ($x_a=1-x_b$):

$$\begin{aligned}
x_a &\leq \sqrt{2\theta-\tfrac{2}{3}\left(1-2x_a\right)\left(1+\tfrac{1}{2}\right)} \\
\tfrac{1}{2}-x_a &\leq \sqrt{2\theta-\tfrac{2}{3}\left(1-2x_a\right)\left(1+\tfrac{1}{2}\right)} \\
1-x_b &\leq \sqrt{2\theta-\tfrac{2}{3}\left(2x_b-1\right)\left(2-\tfrac{1}{2}\right)} \\
x_b-\tfrac{1}{2} &\leq \sqrt{2\theta-\tfrac{2}{3}\left(2x_b-1\right)\left(2-\tfrac{1}{2}\right)}
\end{aligned}$$

Since both sides are nonnegative I can square the expressions without changing the inequality:

$$\begin{aligned}
\left(x_a-1\right)^2 &\leq 2\theta \\
\left(x_a-\tfrac{3}{2}\right)^2 &\leq 2\theta+1 \\
\left(x_b\right)^2 &\leq 2\theta \\
\left(x_b+\tfrac{1}{2}\right)^2 &\leq 2\theta+1
\end{aligned}$$

Now, we can solve for x_a, x_b:

$$\begin{aligned}
x_a &\geq 1-\sqrt{2\theta} \\
x_a &\geq \tfrac{3}{2}-\sqrt{2\theta+1} \\
x_b &\leq \sqrt{2\theta} \\
x_b &\leq \tfrac{1}{2}+\sqrt{2\theta+1}
\end{aligned}$$

Depending on θ, one of the first (last) two implies the other condition.

2.6.2 Derivation of Lemma 2

Optimality

First order conditions of (2.10), using $x_b - x_a = d$, imply:

$$\frac{\partial \pi_a^2}{\partial p_a} = \frac{1}{2}d - \left(\frac{1}{dt} + \frac{1}{t\sqrt{\frac{2}{t}(v-p_a)}}\right)(p_a - c) + \sqrt{\frac{2}{t}(v-p_a)} - \frac{1}{dt}(p_a - p_b) = 0$$
$$\Rightarrow p_b = p_a - \frac{1}{2}d^2t + (p_a - c) + \frac{d(p_a-c)}{\sqrt{\frac{2}{t}(v-p_a)}} - dt\sqrt{\frac{2}{t}(v-p_a)}$$
$$\Rightarrow p_b = c + 2(p_a - c) - \frac{1}{2}d^2t - \frac{c+2v-3p_a}{\sqrt{\frac{2}{t}(v-p_a)}}d$$

$$\frac{\partial \pi_b^2}{\partial p_b} = \frac{1}{2}d - \left(\frac{1}{dt} + \frac{1}{t\sqrt{\frac{2}{t}(v-p_b)}}\right)(p_b - c) + \sqrt{\frac{2}{t}(v-p_b)} + \frac{1}{dt}(p_a - p_b) = 0$$
$$\Rightarrow p_a = p_b - \frac{1}{2}d^2t + (p_b - c) + \frac{d(p_b-c)}{\sqrt{\frac{2}{t}(v-p_b)}} - dt\sqrt{\frac{2}{t}(v-p_b)}$$
$$\Rightarrow p_a = c + 2(p_b - c) - \frac{1}{2}d^2t - \frac{c+2v-3p_b}{\sqrt{\frac{2}{t}(v-p_b)}}d$$

Inverse best response functions are symmetric. They are also monotonously increasing:
$\frac{\partial p_b}{\partial p_a} = 2 + \frac{3}{\sqrt{\frac{2}{t}(v-p_a)}}d - \frac{1}{2}\frac{(c+2v-3p_a)}{\left(\frac{2}{t}(v-p_a)\right)^{\frac{3}{2}}}\frac{2}{t}d = 2 + \frac{4\frac{v-c}{t} - 3\frac{p_a-c}{t}}{\left(2\frac{v-p_a}{t}\right)^{\frac{3}{2}}}d > 0$ since $v \geq p_a \geq c$
and
$\frac{\partial p_a}{\partial p_b} = 2 + \frac{3}{\sqrt{\frac{2}{t}(v-p_b)}}d - \frac{1}{2}\frac{(c+2v-3p_a)}{\left(\frac{2}{t}(v-p_b)\right)^{\frac{3}{2}}}\frac{2}{t}d = 2 + \frac{4\frac{v-c}{t} - 3\frac{p_b-c}{t}}{\left(2\frac{v-p_b}{t}\right)^{\frac{3}{2}}}d > 0$ since $v \geq p_b \geq c$

Hence, also in equilibrium best responses are symmetric and the following equilibrium condition must hold:
$p = c + 2(p-c) - \frac{1}{2}d^2t - \frac{c+2v-3p}{\sqrt{\frac{2}{t}(v-p)}}d$
$p - c - \frac{1}{2}d^2t = \frac{c+2v-3p_a}{\sqrt{\frac{2}{t}(v-p)}}d$
Rewriting yields:
$\frac{(p_a-c)}{t} = \frac{1}{2}d^2 + \frac{2\frac{v-c}{t} - 3\frac{p_a-c}{t}}{\sqrt{2\frac{v-c}{t} - 2\frac{p_a-c}{t}}}d$
Substituting $\theta = \frac{v-c}{t}$ and additionally $\rho \equiv \frac{(p_a-c)}{t}$ yields

$\rho = \frac{1}{2}d^2 + \frac{2\theta-3\rho}{\sqrt{2\theta-2\rho}}d$
$\left(\rho - \frac{1}{2}d^2\right)^2 = \frac{(2\theta-3\rho)^2}{2\theta-2\rho}d^2$
$\left(\rho - \frac{1}{2}d^2\right)^2(2\theta - 2\rho) - (2\theta-3\rho)^2 d^2 = 0$
$\frac{1}{2}d^4\theta - \frac{1}{2}d^4\rho - 4d^2\theta^2 + 10d^2\theta\rho - 7d^2\rho^2 + 2\theta\rho^2 - 2\rho^3 = 0$
$-2\rho^3 + \left(-7d^2 + 2\theta\right)\rho^2 + \left(10d^2\theta - \frac{1}{2}d^4\right)\rho + \left(\frac{1}{2}d^4\theta - 4d^2\theta^2\right) = 0$
or
$\rho^3 + a\rho^2 + b\rho + c = 0$

with
$a = \left(\frac{7}{2}d^2 - \theta\right)$
$b = \left(\frac{1}{4}d^4 - 5d^2\theta\right)$
$c = \left(2d^2\theta^2 - \frac{1}{4}d^4\theta\right)$

Cubic equations can be solved using Cardano's formula (for a detailed description see e.g. de la Fuente 2000, p. 153). The first step of Cardano's approach requires the discriminant $D = \left(\frac{q}{2}\right)^2 + \left(\frac{r}{3}\right)^3$ with:
$q = \frac{2}{27}a^3 - \frac{1}{3}ab + c = \frac{2}{27}\left(\frac{7}{2}d^2 - \theta\right)^3 - \frac{1}{3}\left(\frac{7}{2}d^2 - \theta\right)\left(\frac{1}{4}d^4 - 5d^2\theta\right) + 2d^2\theta^2 - \frac{1}{4}d^4\theta$
$\quad = \frac{623}{216}d^6 + \frac{53}{18}d^4\theta + \frac{10}{9}d^2\theta^2 - \frac{2}{27}\theta^3$

$r = b - \frac{a^2}{3} = \left(\frac{1}{4}d^4 - 5d^2\theta\right) - \frac{\left(\frac{7}{2}d^2-\theta\right)^2}{3} = -\frac{23}{6}d^4 - \frac{8}{3}d^2\theta - \frac{1}{3}\theta^2$

Plugging in r and q gives:

$$D = \left(\frac{\frac{623}{216}d^6+\frac{53}{18}d^4\theta+\frac{10}{9}d^2\theta^2-\frac{2}{27}\theta^3}{2}\right)^2 + \left(\frac{-\frac{23}{6}d^4-\frac{8}{3}d^2\theta-\frac{1}{3}\theta^2}{3}\right)^3$$

$$= -\frac{5}{768}d^{12} - \frac{31}{288}d^{10}\theta + \frac{85}{432}d^8\theta^2 + \frac{5}{72}d^6\theta^3 - \frac{1}{9}d^4\theta^4 - \frac{2}{27}d^2\theta^5$$

Next, it is necessary to identify the roots of the discriminant. Here, we have two complex and the following two real solutions:

$\theta_1 = \frac{3}{4}d^2$

$\theta_2 = \sqrt[3]{\frac{27}{32}d^6} - d^2 = \left(1 - \sqrt[3]{\frac{27}{32}}\right) d^2 < 0$

The second solution is infeasible since theta must be bigger than zero. Hence we are left with $\theta = \frac{3}{4}d^2$. Checking derivative at this point reveals that this is a local maximum. As can be seen easily, for $\theta = 0$, the Discriminant is still negative. Hence, it is equal to zero for $\theta = \frac{3}{4}d^2$ and negative for all $\theta > 0$. This means that we have the Casus Irreducibilis which is not possible to solve with the standard Cardano approach, since it would not deliver real solutions. Instead, we need to use Moivre's theorem, which allows to solve the equation by an additional trigonometric transformation.

There are three possible solutions:

$z_1 = \sqrt{-\frac{4}{3}r}\cos\left(\frac{1}{3}\arccos\left(-\frac{q}{2}\sqrt{-\frac{27}{r^3}}\right)\right)$

$z_2 = -\sqrt{-\frac{4}{3}r}\cos\left(\frac{1}{3}\arccos\left(-\frac{q}{2}\sqrt{-\frac{27}{r^3}}\right) + \frac{1}{3}\pi\right)$

$z_3 = -\sqrt{-\frac{4}{3}r}\cos\left(\frac{1}{3}\arccos\left(-\frac{q}{2}\sqrt{-\frac{27}{r^3}}\right) - \frac{1}{3}\pi\right)$

with $z = \rho + \frac{1}{3}a$.

Plugging in and rewriting in terms of prices delivers

$p_1 = c + \frac{1}{3}t\left(\sqrt{s}\cos\left(\frac{1}{3}\arccos(A)\right) - \left(\frac{7}{2}d^2 - \theta\right)\right)$

$p_2 = c + \frac{1}{3}t\left(\left(\theta - \frac{7}{2}d^2\right) - \sqrt{s}\cos\left(\frac{1}{3}\arccos(A) + \frac{1}{3}\pi\right)\right)$

$p_3 = c + \frac{1}{3}t\left(\left(\theta - \frac{7}{2}d^2\right) - \sqrt{s}\cos\left(\frac{1}{3}\arccos(A) - \frac{1}{3}\pi\right)\right)$

with

$s = 46d^4 + 32d^2\theta + 4\theta^2$

and

$A = \sqrt{\frac{2}{(23d^4+16d^2\theta+2\theta^2)^3}}\left(2\theta^3 - 30d^2\theta^2 - \frac{159}{2}d^4\theta - \frac{623}{8}d^6\right)$

The last solution (p_3) is not feasible, since it is negative for all applicable parameter values. The first solutions (p_1) and (p_2) are feasible for some $\theta - d-$combinations:

$\Rightarrow p_1$ holds for $d^2 \geq \frac{4}{3}\theta$ and is increasing in its range.

$\Rightarrow p_2$ holds for $d^2 < \frac{4}{3}\theta$ and is increasing within its range.

Feasibility

Next, we need to find the set of locations for which the derived solution is in lines with the assumptions of case 2. One approach would be plugging p^{*2} into feasibility constraint (FC2). Due to trigonometric expressions in p^{*2}, the resulting inequalities are hard to simplify.

However, we can use a little trick here. Since we derive feasibility for symmetric locations, we plug in symmetry first. Hence, (FC2) boils down to

$x_a > \sqrt{2\frac{(v-p_a)}{t}} > \frac{1}{2} - x_a$.

Rewriting yields a lower bound for the price: $v - \frac{t}{2}x_a^2 \leq p^{*2}$ and an upper bound: $p^{*2} \leq v - \frac{t}{2}\left(\frac{1}{2} - x_a\right)^2 = v - \frac{t}{2}(d)^2$

Since prices are increasing in distance, we know that at the price limits will also yield the boundaries of the feasible locations. We start with the lower bound and plug $v - \frac{t}{2}x_a^2 = p^{*2}$ into the optimality condition:

$(p_a - c) = \left(\sqrt{\frac{2}{t}(v - p_a)} + x_a - \frac{1}{2}\right) \frac{2t\left(\frac{1}{2} - x_a\right)\sqrt{\frac{2}{t}(v-p_a)}}{2\left(\frac{1}{2} - x_a\right) + \sqrt{\frac{2}{t}(v-p_a)}}.$

Solving for θ gives the marginal market parameter.

$\theta = \frac{1}{2}\left(\frac{x_a - 2x_a^2}{1 - x_a} + x_a^2\right)$

Put differently, $\theta \leq \frac{1}{2}\left(\frac{x_a - 2x_a^2}{1 - x_a} + x_a^2\right)$ and $\theta \leq \frac{1}{2}\left(\frac{x_a - 2(1-x_b)^2}{1-(1-x_b)} + (1 - x_b)^2\right) = \frac{1}{2x_b}\left(x_b^3 - 4x_b^2 + 5x_b + x_a - 2\right)$ implies feasibility constraint $v - \frac{t}{2}x_a^2 \leq p^{*2}$.

Next, we determine the upper bound. Maximum feasible price is $v - \frac{t}{8}(d))^2$.

Hence, we can find marginal θ by plugging $p_a = v - \frac{d^2}{8}$ into the optimality condition:

$\theta = \frac{11}{24}d^2$

rewriting in terms of individual locations yields:

$\theta \geq \frac{11}{24}(x_b - x_a)^2$ implies $v - \frac{t}{8}(x_b - x_a)^2 \geq p^{*2}$.

2.6.3 Derivation of Lemma 5

Optimality

First order conditions of (2.19) imply:

$\frac{\partial \pi_a^3}{\partial p_a} = 9p_a^2 + p_a\left(2tp_a^2 - 6(c + 2v)\right) + (c + 2v)^2 - 2tp_a^2 v = 0$

under the condition that $p_a > \frac{1}{3}c + \frac{2}{3}v$. Due to quadratic equation, two solutions possible.

$p^{*3}(x_a) = \frac{1}{3}c + \frac{2}{3}v - \frac{1}{9}tp_a^2 \pm \frac{1}{9}x_a\sqrt{t^2x_a^2 + 6t(v - c)}$

Since $\frac{d^2\pi_a^3}{dp_a^2} < 0$ for all $c < p < v$ both solutions are local maxima.

However, $p > \frac{1}{3}c + \frac{2}{3}v$ only holds when using positive square root.

Hence, the only feasible solution is

$p^{*3}(x_a) = \frac{1}{3}c + \frac{2}{3}v - \frac{1}{9}tl^2 + \frac{1}{9}x_a\sqrt{t^2x_a^2 + 6t(v - c)}.$

Feasibility

According to (FC3) there are two conditions to be met.

Plugging $p = p^{*3}$ into the first constraint $x_a \leq \sqrt{\frac{2}{t}(v - p_a)}$ and substituting $\theta = \frac{(v-c)}{t}$ gives:

$x_a \leq \sqrt{\frac{2}{3}\theta + \frac{2}{9}x_a^2 - \frac{2}{9}x_a\sqrt{x_a^2 + 6\theta}}$

$\Rightarrow 6\theta - 7x_a^2 \geq 2x_a\sqrt{x_a^2 + 6\theta}$

Since the right hand side is positive, there is no solution if $\theta < \frac{7}{6}x_a^2$.

Using this condition and then squaring both sides yields

$\theta \geq \frac{5}{2}x_a^2$

Plugging $p = p^{*3}$ into the second constraint $\frac{1}{2} + \frac{p_a - p_b}{t(1 - x_b - x_a)} - x_a > \sqrt{2\frac{v - p_a}{t}}$ and substituting $\theta = \frac{(v-c)}{t}$ gives

$\frac{2}{9}x_a\sqrt{x_a^2 + 6\theta} > -\left(\frac{1}{4} - x_a + \frac{7}{9}x_a^2 - \frac{2}{3}\theta\right)$

This holds for all p and x_a when $\theta \leq \frac{3}{8} - \frac{3}{2}x_a + \frac{7}{6}x_a^2$

If $\theta > \frac{3}{8} - \frac{3}{2}x_a + \frac{7}{6}x_a^2$ the condition becomes:
$36x_a^2 \left(x_a - \frac{1}{2}\right)^2 > \left(6\theta - \left(\frac{9}{4} - 9x_a + 9x_a^2\right)\right)^2$
Solving quadratic equation taking into account possible negativities implies
$\frac{3}{8} - x_a + \frac{1}{2}x_a^2 > \theta$

In summary, the feasibility constraints (FC3) require
$\frac{3}{8} - x_a + \frac{1}{2}x_a^2 > \theta \geq \frac{5}{2}x_a^2 \Leftrightarrow x_a \leq \min\left\{\sqrt{\frac{2}{5}\theta}x_a, 1 - \frac{1}{2}\sqrt{8\theta + 1}\right\}.$

CHAPTER

3

On The Cognitive Side of Our Feelings - Guilty or Perplexed? (co-authored with Julija Kulisa)

3.1 Introduction

In the last two decades, many theories in economics emerged trying to explain prosocial behavior. A recent concept originating in social psychology (see, e.g., Baumeister et al. 1994) is guilt aversion stating that people experience disutility when letting others down (e.g. Battigalli and Dufwenberg 2007).

This concept bears fundamental implications for the classical contract theory which assumes that people do something contrary to their self-interest only if it is enforced by an explicit contract. Guilt aversion, or reluctance to disappoint others, could help our understanding of why people provide unobservable effort at work, pay taxes or hold informal promises in cases where they don't risk anything when doing the opposite.

Experimental studies testing guilt aversion theory came to different conclusions. We explain this ambiguity with the fact that subtle differences in the experimental design can lead to competing results because of effects they produce on human emotions and cognition. We build an experiment allowing us to test the guilt aversion hypothesis while taking into consideration findings in social psychology and cognition research. The results of our study provide new evidence of guilt aversion and have critical implications for experimental research in economics in general. We suggest furthermore that the applicability of our experimental design goes well beyond studying guilt aversion.

We review the related economic literature in the next section. Section 3 presents and motivates our experimental design based on psychological literature, highlights our hypotheses and justifies our methods. Finally, we present our results in Section 4 and conclude.

3.2 Related literature

The concept of guilt aversion stems from a long tradition of the analysis of higher order beliefs in social psychology. The developmental psychologists Perner and Wimmer (1985) point out that it is not sufficient to describe what people think about real events (first-order beliefs, FOB) when trying to explain social interaction. They claim that "*interaction between people is to a large degree based on an interaction of minds which can be properly understood only when one takes into account what people think about other people's thoughts (second order beliefs, SOB) and even what people think that others think about their thoughts, etc. (higher order beliefs)*" (Perner and Wimmer 1985, p. 438). Perner and Wimmer (1983) are one of the first to experimentally test for the influence of second order beliefs on actions. In their seminal paper they show that humans starts to consider second order beliefs at the age of 4-6.

The roots of this line of thought can even be found in the economic literature, since Schelling (1960) already points out that it is at the heart of game theory that the optimal choice depends on what the agent "*expects the other to do, knowing that the other is similarly guided, so that each is aware that each must try to guess what the second guesses the first will guess the second to guess and so on*" (Schelling 1960, p. 87). However, it was not until recently that the economic literature analyzed the possibility that there could be an intrinsic disutility when not coming up to somebody else's expectations.

One of the first experimental studies on guilt aversion is due to Dufwenberg and Gneezy (2000) who show within the dictator game framework that dictator's beliefs about recipient's expectations (second order beliefs) significantly affect his donation.

Building on this result, Charness and Dufwenberg (2006) use guilt aversion as an explanation for why communication, and in particular informal promises, foster cooperation in their trust game experiment. In their game, the second mover ("trustee") gets the chance to send a promise to the first mover ("trustor"). After sending this message trustees decided whether to cooperate without knowing the trustors' behavior. In addition, second order beliefs were elicited by asking trustees to guess the average cooperation rate that their trustor expected. Building on the observation that these second order beliefs are correlated with the decision to transfer back, Charness and Dufwenberg explain higher cooperation rate in the communication group with the idea that people experience guilt when disappointing someone's expectations raised by promises they had given. However, this explanation is challenged by the authors themselves who admit that the trustees' second order belief could be formed based on their decision to cooperate instead of causing it. Trustees might see their individual decision as common behavior (the false consensus effect, see Ross et al. 1977) leading to the belief that trustors will anticipate their choice.

Vanberg (2008) slightly modifies the trust game experiment used by Charness and Dufwenberg and rematches some pairs after trustees had sent their promises. In the rematched pairs, trustees were made aware of the promises their new co-player had received in the communication phase. Since trustors were not told whether they had been rematched, their expected rate of cooperation should still be based on their received promise. This should induce the same trustee behavior independent of the rematching process. However, the results show that trustees tend to keep only their own but not a third party's promise, contradicting the (unconditional) guilt aversion theory.

In a more recent paper, Charness and Dufwenberg (2010) pursue their idea in a very similar experiment. In their new study, instead of offering trustees to send a free-form message to their trustors (Charness and Dufwenberg 2006), the authors allow trustees to send only a predefined promise-containing message. No significant relationship between second order belief and trustees' transfers could be found in this setup suggesting a high sensitivity of guilt activation to the situational context.

Ellingsen et al. (2010) tested for the existence of guilt aversion in a dictator game (and a trust game) experiment without communication. To avoid results prone to the false consensus effect, Ellingsen et al. (2010) elicited recipients' (trustors') expectations of their share (first order beliefs) by asking them for the average transfer (cooperation rate) they expected and then passed this expectations to the matched players. From the authors' point of view, positive correlation between the recipients' (trustors') beliefs and dictators' (trustees') decision to cooperate would prove the existence of guilt aversion. Results of the experiments do not provide any evidence of such a correlation and hence reject the guilt aversion hypothesis.

Reuben et al. (2009) use a variation of the trust game. All subjects take four decisions. First, they take on the trustor's role and decide if they want to transfer \$50. Next, subjects have to report their belief regarding the amounts their trustee will return and then take a trustee's decision themselves. Finally, the entire trust game is played again, this time with one half of the subjects playing as trustors and the other half as trustees. Subjects whose first order belief (elicited in the 2nd stage) were either very low (less than 7% of the possible amount) or very high (more than half) are assigned the trustor's role and are informed that their first order belief will be sent to their trustee. Trustees receive the first order beliefs and make their decision. The authors' main reason for using this design was to make beliefs more reliable for the trustees, as opposed to the elicitation strategy in Ellingsen et al. (2010). They let trustees go through the belief elicitation stage themselves which in the opinion of the authors should increase the credibility of the first order belief when being confronted with it as a trustee. Reuben et al. (2009) find that trustees who face a low first order belief transfer back significantly less in the last stage providing support for guilt aversion. Unfortunately, the complexity of their experimental design does not allow one to understand the true reasons for this result. While the authors suggest that their findings are due to their more robust elicitation procedure of first order beliefs, trustees might have acted as guilt averse because of taking their decision repeatedly or due to putting themselves into the shoes of a trustor at the first stages of the experiment.

Based on the vast literature in cognition and social psychology, as well as studies analyzing framing effects, we argue that contrary conclusions regarding the existence of guilt aversion may be due to the fact that subjects' ability to foresee their emotions depends crucially on the situational context. From the cognitive point of view, people are not necessarily aware of their goals (e.g., guilt avoidance) at the moment of making a decision in a new environment (Bargh et al. 2001). However, goals can develop the feature of being automatically activated in situations where they have been repeatedly chosen and pursued in the past (Bargh et al. 2001). Thus, the goal of meeting others' expectation, or avoiding guilt, may be well pursued in the daily life, in usual situations, while staying inactivated in the laboratory environment because of neutral framing, artificiality of the experimental design or novelty of the environment for the subjects. Moreover, in such cases people may simply fail to anticipate their own feelings which would result after committing some act, and this would influence their decision making. Anticipated guilt has been shown to make people involve in a behavior that helps to avoid the guilt feeling (Lindsey 2005). Hence, whether people anticipate guilt in a given experiment can change its outcome drastically.

We believe that the situation in which Charness and Dufwenberg (2006) put their subjects is more natural than those imposed by Ellingsen et al. (2010) and Vanberg (2008). It seems reasonable to believe that in the laboratory environment people are more likely to anticipate guilt feeling which would result after breaking the promise they give - a situation often encountered in daily life - than guilt resulting from breaking someone else's promise Vanberg (2008) or from not cooperating with some anonymous person one never communicated to (Ellingsen et al. 2010). The above mentioned theory of Bargh et al. (2001) suggests however that guilt averse subjects who take a decision repeatedly will learn to take into consideration feelings of others no matter how exotic the environment is initially. The results of Reuben et al. (2009) can be regarded as an illustration of this idea since they observe trustees to become affected by first order beliefs of the trustors after viewing the situation from different angles and making several related decisions.

In this paper we design a neutrally-framed experiment aimed at making the cognitive state in which subjects make their decisions more similar to what they experience routinely in the real life.[1] As compared to Reuben et al. (2009) our setup allows us to disentangle the effects produced by repeating the game and running a thought experiment on subjects' feeling of guilt. Our treatments are described and motivated in the next section.

3.3 Experimental Design and Motivation

308 students of Goethe University Frankfurt took part in our experiment. The average time spent by subjects was around 40 minutes and the average earnings were 8,44 Euro.[2]

Our experiment consists of 3 treatments relying on a common framework. In particular, all of them are the two round dictator games where a dictator (called person B) has to decide how much of 5 Euro to transfer to a recipient (person A) in each round. The players are rematched between the two rounds so that each dictator takes the transfer decision twice and is most probably matched with two different recipients. Players don't know that the game will be repeated when taking their first round decisions.

The experiment was paper based; so in order to define subjects' roles we were first randomly assigning each student some number; then, based on these numbers we were forming two groups: dictators and recipients. Dictators were invited to take a seat in the Laboratory for Experimental Economics equipped with cubicles and thus ensuring maximum anonymity. Recipients were seated at sufficient distance in a separate classroom.

As discussed in the previous section, we are concerned with the question whether, in the very beginning of the experiment, dictators are aware that they might experience discomfort after transferring too little or too much to their anonymous co-players. Our two main treatments are designed so as to improve guilt anticipation by dictators in a natural way. The next subsection introduces the treatment aimed at achieving this after dictators take their decision once and

[1] Neutral framing ensures that the subject is not influenced by the words chosen for description of the game and other players per se. Such framing is interesting for the experimenters since it helps to preclude experimenter effects (Rosenthal 1976) and other noise in the data and allows to generalize the results.

[2] The earnings consist of a show up fee of 3 Euro, the outcome of the game and the guessing game from the belief elicitation stage.

hence receive an emotional feedback before deciding again. Then we describe the treatment where we make dictators more aware of the psychological consequences of their decisions in the first round already; to do so we make them think of their co-player's expectations. Finally, we present our control treatment and our hypotheses.

3.3.1 LEF Treatment (Learning from Emotional Feedback)

The idea of this treatment stems from the modern theories in cognition which hold view that emotions help to coordinate perceptual and cognitive processes and social interactions (Keltner and Gross 1999, Baumeister et al. 2007). Such functional view of emotions states that people use them as a feedback for their past behavior learning hence how to behave in the future. The theory was proved by numerous experimental studies which showed that emotions can have impact on subsequent unrelated (e.g. Lerner et al. 2004) and related Ketelaar and Au (2003) decisions with financial consequences.

Building on Ellingsen et al. (2010), we use a dictator game where dictators are informed about the first order belief of the recipients. In this treatment, we elicit recipients' beliefs and pass them to their matched dictators twice. In fact, each round here is identical to the experiment of Ellingsen et al. (2010) except for the effect of repetition. Hence, we can test the hypothesis of Ellingsen et al. (2010) for the data of two rounds separately.

Technically, we first ask recipients how much they expect to receive from their co-players and announce that the one of them who guesses correctly will receive a prize of 5 Euro or will share this prize with other winners.

Each dictator is assigned 5 Euro and is asked to decide how much of it he would like to transfer to recipient. Together with the decision form, dictators receive the first order belief of their assigned recipient and the information that the latter did not know that their beliefs would be passed to dictators. When all decisions are collected, we announce the second round and repeat the game.[3]

As discussed above, we expect that subjects are not able to anticipate guilt feeling in the beginning of the experiment. Dictators who disappoint their recipients in the first round and experience guilt in the given situation once will anticipate and try to avoid this feeling when taking donation decision for the second time. If guilt aversion theory is true, first order beliefs are positively correlated with the donations in the second but not necessarily the first round. In fact, the first round of this treatment serves us as a robustness check since we expect the results to be identical to those obtained by Ellingsen et al. (2010).

3.3.2 PT Treatment (Perspective Taking)

This treatment differs from the previous in one respect: here, before confronting dictators with the first order beliefs of their co-players in the first round, we elicit their second order beliefs. In particular, after telling the dictator that we have asked his matched recipient how much they expect to receive we ask them to guess this expectation. As before, correct guesses equally share a winner's prize of 5 Euro. After this elicitation stage of second order beliefs the experiment proceeds in exactly the same way as in the previous treatment: there are of the dictator game with dictators confronted with the recipients' first order beliefs, where pairs are randomly matched round in each round.

[3]Technically, we first elicit FOB for both rounds and then continue the experiment with dictators, so that there is very short time interval between the two rounds for both groups of players.

We believe that asking for the second order belief works as a prime[4]: thinking about the expectations of others helps dictator's conscience to activate the guilt avoidance goal ensuring the ability of guilt anticipation.[5] Following this idea we expect guilt averse dictators' transfers to be positively correlated with recipients' first order beliefs in both rounds. As opposed to the previous treatment, in this case there is no need for dictators to experience guilt feeling once in order to anticipate it when taking decisions in the laboratory environment; the guilt notion becomes active once dictators try to put themselves in the other party's shoes. We expect the correlation between transfers and first order belief in the second round to be of a similar magnitude to the one in the previous treatment.

We expect dictators' first round transfers to be also positively correlated with their second order belief. This should come as no surprise due to the false consensus effect mentioned in the introduction: dictators might take their transfer decisions first and believe that everyone acts and thinks as they do when stating their second order belief.[6]

3.3.3 Control Treatment

In order to rule out alternative explanations of the second rounds' results based on the two-round effect and to check how important it is for dictators to be confronted with the expectations of their co-players in order to experience guilt, we run a control treatment with a basic dictator game without belief communication in the first round and confronting dictators with their recipient's first order beliefs in the second round. Contrasting the second round results from this treatment with the ones we obtain in the other treatments allows us to identify the influence of LEF and PT respectively.

3.3.4 Hypotheses

We assume the existence of guilt aversion as proposed by Battigalli and Dufwenberg (2007) which adds a punishment term for negative deviations from expectations in the utility function which are weighted by an individual guilt aversion parameter θ. Following Bargh et al. (2001) who state that goals can stay inactivated in a new environment, we slightly extend the guilt aversion concept of Battigalli and Dufwenberg taking into account that guilt aversion only kicks in when the decision in question represents a familiar situation. Hence, we do not assume a fix guilt aversion parameter but rather an effective parameter θ^E that will depend on the situational context. In a simple model we allow θ^E to equal either zero or the true parameter θ, depending

[4]Priming can be described as procedures activating stored knowledge Higgins (1996).

[5]We suggest two reasons why FOB can be less effective in inducing anticipated guilt than SOB. First, this can be due to subjects' reluctance to do anything that they feel was imposed on them (Falk and Kosfeld 2006). Secondly, if recipients' FOB exceed the amount dictators consider to be fair, instead of trying to meet these expectations, they may prefer to punish them which does not however preclude regretting it later (Yamamori et al. 2008).

[6]An alternative explanation could be that dictators might like to be consistent with beliefs they state, or to appear "good subjects" in case they decide that the experimenter expects them to donate as much as they think the other party is expecting (see (Zizzo 2010)). However, the fact that first order beliefs provided to dictators in the first round of the previous treatment are not producing any experimenter demand effects, leaves unclear why such an effect should exist in this treatment.

on the situation the person is put into. The payoff for dictators can thus be represented as

$$U(d,\tau) = \underbrace{m(5-d)}_{\text{material payoff}} - \underbrace{\theta^{E}\max\{0, G(\tau-d)\}}_{\text{disutility from guilt aversion}} \tag{3.1}$$

with

$$\theta^{E} = \begin{cases} 0 \quad if & \text{situation is new} \\ \theta & \text{otherwise} \end{cases} \tag{3.2}$$

where m is a function of monetary payoff, G is a function describing the disutility from guilt aversion, $d \in [0,5]$ is the dictator's donation to the receiver, $\theta \geq 0$ is the guilt aversion parameter and $\tau \in [0,5]$ is the receiver's expectation of the amount of money transferred to him. Further, we assume that $\frac{\partial m}{\partial d} < 0$, $\frac{\partial G}{\partial d} < 0$, $\frac{\partial G}{\partial \tau} > 0$.

When choosing the optimal d, the dictator faces a trade-off between maximizing his material payoff and reducing the disutility from guilt aversion. Rational dictators make their choice maximizing expected utility. For simplicity, we assume risk neutral dictators implying that we can restrict the analysis to expected values. Hence, the optimality condition is $m'(5-d) = \theta G'(\tau - d)$ and a rational dictator's donation depends positively on θ and τ or $d^{*} = d^{*}(\theta, \tau)$.

While we cannot ask for the guilt aversion parameter directly, we can exogenously change the receiver's expectation and the situation the dictator has to decide in and thereby infer the value θ. As a consequence we can derive the following hypotheses.

Hypothesis 1 Dictators don't anticipate guilt feeling when they are confronted with the expectations of the matched recipients and make their donation decision for the first time: first order beliefs are uncorrelated with donations in the first round of LEF treatment and the second round of the control treatment.

Hypothesis 2 Dictators who decide in the presence of the expectation of the receiver once receive an emotional feedback and activate guilt aversion for similar decisions in the future: first order beliefs are positively correlated with donations in the second round of LEF and PT treatment.

Hypothesis 3 Dictators who are asked to think of expectations of their co-players before making their decision, anticipate guilt they would feel if they consciously disappointed their matched recipients; first order beliefs are positively correlated with donations already in the first round of PT treatment.

Accepting hypothesis 1 would confirm the consistence with findings of Ellingsen et al. (2010) and would also rule out a second round effect different from learning from emotions. Accepting hypotheses 2 or 3 would provide evidence in favor of the influence of others' expectations on decisions and thus also in favor of guilt aversion theory when allowing for deactivation as sketched in this chapter.

3.4 Methodology

We test the above hypotheses in two ways. First, we estimate correlation between first order beliefs and transfers using both a nonparametric rank coefficient (which goes back to Spearman (1904)) and a parametric coefficient based on a linear regression model (which goes back to

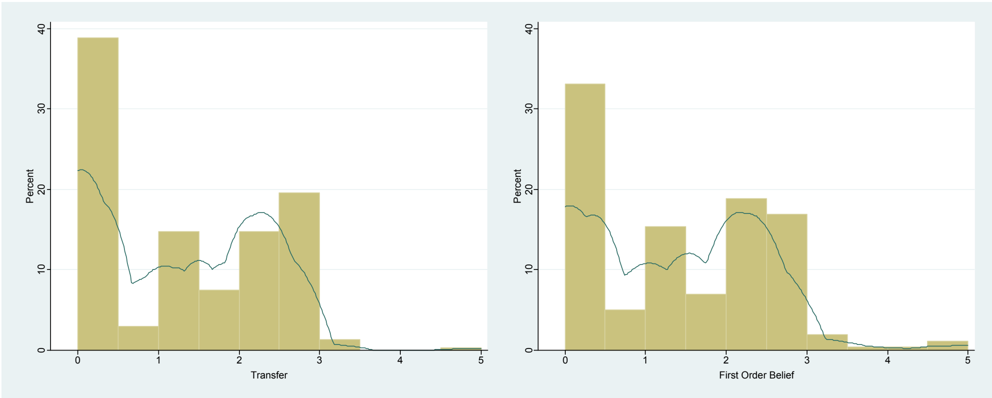

Figure 3.1: Histogram across all tratments

Pearson (1900)). Second, we estimate a more specific model controlling for several covariates. Here, the use of a standard OLS regression approach is threatened by the limited choice set of the game. The histogram of the individual decisions shows a typical picture for a dictator game with peaks at zero, at the equal split and at flat numbers in between (see graph 3.1 as well as graphs 3.5 and 3.6 in the appendix).

The estimated density suggests a truncation of both target and main explanatory variable at zero, hinting towards a violation of the normality assumption. Even though our setting does not represent a censoring model in the narrow sense - since there is no problem of data observability - the peak at zero suggests that subjects might prefer to even give a negative amount if possible. Such a scenario is known as a corner solution model and also qualifies as a category of censoring models in the broader sense as pointed out by Wooldridge (2002a, pp. 517-520). Hence, OLS estimates will be biased, while the Tobit model - which has been proposed by Tobin (1958) for limited dependent variables - represents an appropriate alternative.[7] More specifically, we assume that whenever the actually desired transfer (tr_i^*) is negative we observe an effective transfer (tr_i) of zero. Allowing the latent variable to be influenced by several covariates (X_i'), the formal model becomes

$$tr_i = \begin{cases} tr_i^* & if \quad tr_i^* > 0 \\ 0 & \quad\quad tr_i^* \leq 0 \end{cases} \qquad \text{where} \qquad tr_i^* = X_i'\beta + \varepsilon_i. \tag{3.3}$$

Further, we checked the data for influential observations that might bias the results as e.g. suggested by Davidson and MacKinnon (2003). Cook (1977) developed a metric ("Cook Distance") that serves as a diagnostic technique to identify influential data points for a standard regression model. Barros et al. (2010) extended this metric for tobit models and derived the Generalized Cook Distance (GCD). We implemented this measure in stata (see appendix for the code) and found four observations that are categorized as to have a strong influence on the estimated model coefficients (see table 3.1).[8]

In addition to the Cook distance measure we also looked at a nonparametric estimation of the relationship between first order beliefs and transfers (see table 3.7 in the appendix) which suggested out-of-model behavior for the two observations where receivers expected to get the

[7]Even though the majority of the literature supports this view, there are few voices questioning the use of tobit for corner solution models, see e.g. Sigelman and Zeng (1999). Hence we also include OLS as a robustness check. Results do not differ substantially and can be found in tables 3.7 and 3.8 in the appendix.

[8]Here we follow Bollen and Jackman (1990) who recommend to use 4/n as the upper bound for an acceptable value of Cook's distance ("Crit. Value"), where n is the number of observations.

Table 3.1: Influential Observations according to GCD

Nb	GCD Round1	GCD Round2	Crit. Value	Treatment	FOB1	Transfer1	FOB2	Transfer2
2	.00453919	**0.1708124**	0.0727	1 (LEF)	2.01	0.5	**5**	**0.2**
83	.0142511	**0.0798717**	0.0769	2 (PT)	0.99	0	**3**	**0**
101	**0.2784815**	0.0127003	0.0769	2 (PT)	**5**	**0**	2.5	2.5
103	.0142589	**0.0798717**	0.0769	2 (PT)	1	0	**3**	**0**

full amount. Hence, we exclude these two observations (Nb. 2 and Nb. 101, which also have the highest GCD) from the rest of the analysis and conclude that the guilt aversion model might not be appropriate when receiver's expectations are too high.

3.5 Results

In all treatments, the aggregate results are in line with the literature on dictator games. Across treatments, dictators transfer 1.12 Euro representing 22.4% of the potential amount which is slightly below average when compared to other experiments. In the first round, dictators on average donate 1.22 Euro while in the second round only 1.04 Euro are passed on. This difference is significant (p-value=0.02) for the aggregate data. On the treatment level, results are similar with the exception of the PT treatment where the difference between first and second round behavior is insignificant (see figure 3.2).

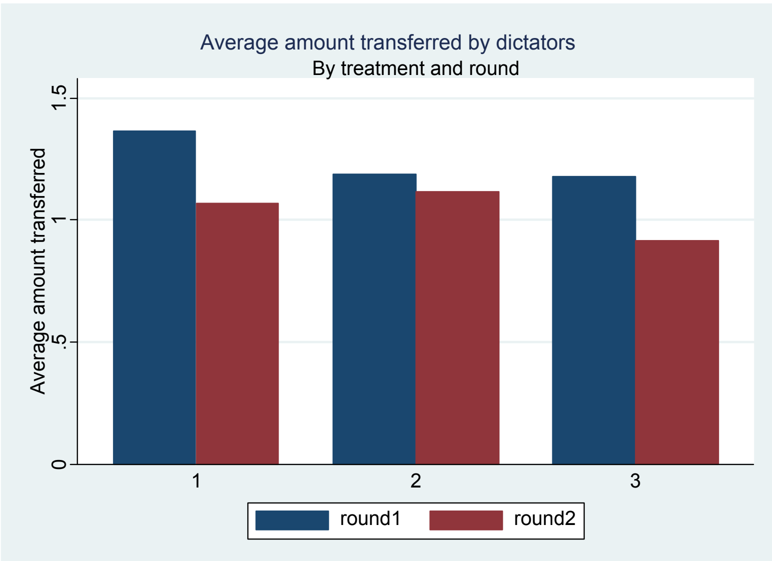

Figure 3.2: Summary of transfers

As a first step, we estimate pairwise correlations between first order beliefs and transfers, both for the unrestricted and for the trimmed sample. We start out looking at the Pearson correlation coefficients assuming a parametric relationship between the two variables. We get a positive and significant correlation in both rounds of the PT treatment independent of sample restriction. In the LEF treatment, we find a positive and significant relationship only in the second round using the trimmed sample (see table 3.2).

Table 3.2: Pearson correlation coefficients

Sample	unrestricted			trimmed		
Treatment	1 (LEF)	2 (PT)	3 (Control)	1 (LEF)	2 (PT)	3 (Control)
Transfer round 1	0.0944	0.2555*		0.1042	0.3501**	
	(0.49)	(0.07)		(0.45)	(0.01)	
Transfer round 2	0.2296	0.3110**	0.1312	0.2518*	0.2916**	0.1312
	(0.23)	(0.02)	(0.38)	(0.07)	(0.04)	(0.38)
N	55	52	46	54	51	46

Pearson correlations; p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The Spearman coefficient abstracts from a parametric relationship and estimates correlation based on ranks. The results using this nonparametric measure confirm the previous findings and additionally identify a significant correlation in the second round of the LEF treatment when the sample remains unrestricted (see table 3.3). This result seems to be reasonable since the sample restrictions have been elaborated based on a parametric estimations approach.

Table 3.3: Spearman correlation coefficients

Sample	unrestricted			trimmed		
Treatment	1 (LEF)	2 (PT)	3 (Control)	1 (LEF)	2 (PT)	3 (Control)
Transfer round 1	0.0896	0.2386*		0.0975	0.2881**	
	(0.52)	(0.09)		(0.48)	(0.04)	
Transfer round 2	0.2226*	0.3276**	0.1486	0.2432*	0.3088**	0.1486
	(0.10)	(0.02)	(0.32)	(0.08)	(0.03)	(0.32)
N	55	52	46	54	51	46

Spearman correlations; p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Next, we present the results from the Tobit regressions. First, we estimate the most simple model including only first order beliefs as regressors. As before, we both estimated the model for the trimmed sample and for the full sample. The results confirm the findings from the correlation tests with similar levels of significance and slightly lower coefficients (see table 3.4).

Table 3.4: Tobit estimations

Sample	unrestricted			trimmed		
Treatment	1 (LEF)	2 (PT)	3 (Control)	1 (LEF)	2 (PT)	3 (Control)
Transfer round 1	0.0772	0.137		0.0815	0.213**	
	(0.473)	(0.132)		(0.455)	(0.030)	
Transfer round 2	0.123	0.207**	0.109	0.183*	0.192*	0.109
	(0.167)	(0.037)	(0.285)	(0.072)	(0.054)	(0.285)
N	55	52	46	54	51	46

Tobit estimates with heteroscedasticity robust std errors; p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

So far, we did not control for any individual effects. However, dictators usually differ in their unconditional propensity to give with a large fraction of subjects transferring positive amounts. In our experiment this unconditional heterogeneity is confirmed in the histograms of transfers (see figure 3.5 in the appendix). To control for individual fixed effects, we use first round behavior as a proxy for the general propensity to give. This results in a richer model with a much bigger more explanatory power (pseudo R^2 is on average six times higher). Estimates can be found in table 3.5 confirming the previous findings at an even higher degree of significance.

Table 3.5: Tobit estimations with controls for first round behavior

Sample	unrestricted			trimmed		
Treatment	1 (LEF)	2 (PT)	3 (Control)	1 (LEF)	2 (PT)	3 (Control)
fob2	0.223***	0.206***	0.0724	0.295***	0.175**	0.0724
	(0.008)	(0.009)	(0.420)	(0.002)	(0.021)	(0.420)
tr1	0.430***	0.573***	0.319***	0.437***	0.619***	0.319***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
N	55	52	46	54	51	46

Marginal effects with heteroscedasticity robust std errors; p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Summarizing, we get consistent and robust estimates for the influence of expectations even when applying different estimation techniques. This allowing us to draw three conclusions.

Result 1 Dictators do not exhibit guilt aversion when confronted with first order beliefs for the first time.

There are two situations which represent a new setting for the dictators: in round 1 of the LEF treatment and in round 2 of the control treatment they hear about first order beliefs for the first time. In both rounds, even in the trimmed sample, p-values are far from any conventional level of significance, both in a simple correlation analysis and in the tobit estimations. Hence, we confirm Ellingsen et al. (2010) who do not find evidence for guilt aversion in the moment when being confronted with first order beliefs for the first time.

Result 2 First order beliefs are positively correlated with donations in the second round of both the LEF and the PT treatment.

In both treatments, we can observe positive correlation in the second round, both when looking at the Spearman correlations and when inspecting the results from the tobit regressions.

Result 3 A simple thought experiment can also activate guilt aversion. The influence of perspective taking is stronger than the real experience from repetition.

In the second treatment (PT) we see correlation of first order beliefs and transfers already in round 1. Since this treatment only differs by the thought experiment as an initial task, the change in behavior can be attributed to this procedure. Both Spearman correlation and estimated coefficients are higher in the first round of the PT treatment then in the second round of the LEF treatment.

3.6 Discussion and robustness checks

One might raise the question whether past transfers are truly exogenous in the tobit equations. This property, however, can only be tested in the presence of a valid instrument, which requires a variable that it is both correlated with transfer in round one and exogenous to second round behavior. In the PT treatment, first order beliefs fulfill both conditions, and the respective test rejects endogeneity (with p=0.910).[9] For the LEF treatment, we cannot test explicitly, but the result is most likely to carry over. However, even if there was correlation with the error term, this would only pose a threat to the consistency of the estimated influence of first round behavior while exogenous regressors which are uncorrelated with the endogenous regressor (like receiver's first order belief) will be estimated consistently (Cameron and Trivedi 2005, p. 92).

A further robustness check would be the inclusion of additional controls. There are several characteristics that have been found to explain behavior in a dictator game, as age, field of study, gender (see e.g. Croson and Gneezy 2009), nationality (see e.g. Oosterbeek et al. 2004). We collected this information in a questionnaire after the experiment, constructed several dummy variables and included them as explanatory variables (see table 3.6).

Table 3.6: Tobit estimations with controls for socio-cultural covariates

	Treatment 1 (LEF)	Treatment 2 (PT)	Treatment 3 (Control)
	trimmed	trimmed	trimmed
	(1)	(3)	(4)
	tr2	tr2	tr2
fob2	0.327***	0.198**	0.0617
	(3.60)	(2.60)	(0.68)
tr1	0.486***	0.520***	0.305***
	(5.21)	(5.72)	(3.94)
ger (d)	0.369**	-0.306	-0.0686
	(2.28)	(-1.57)	(-0.38)
female (d)	-0.266	0.0248	0.0137
	(-1.49)	(0.14)	(0.07)
age	0.0321	-0.0288	0.0147
	(1.08)	(-0.98)	(0.49)
econ (d)	0.00616	-0.132	-0.263
	(0.03)	(-0.68)	(-1.40)
N	53	52	46

Marginal effects; t statistics in parentheses
(d) for discrete change of dummy variable from 0 to 1
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The dummy variable *ger* is set to 1 if the subject has German nationality and does not speak a foreign language fluently (except for English). We created this dummy to control for the ethnic background of the person. It turns out that Germans behave substantially different in the two treatments. While giving on average more in the second round of the LEF treatment, there is a tendency to give less in the PT treatment, even though not being significant. Neither the gender dummy (*female*) nor a dummy for students taking economics as a subject (*econ*) turn out to be

[9] This test uses the residual from the reduced form equation and tests whether it is uncorrelated with the orginial equation of interest (see e.g. Wooldridge 2002b, p. 483).

significant. Most importantly, however, the influence of the recipient's first order belief remains of similar size and becomes even more significant when controlling for the different covariates which strengthens the robustness of our finding.

One might go one step further and allow covariates not only to influence the level of transfer, but to also determine the guilt aversion coefficient. Hence, we also estimated models including interaction terms of first order beliefs and the dummy variables. The estimates (see section 3.8.3 in the appendix) did not bring forth significant interaction effects, but confirmed the general influence of first order beliefs adding further robustness to the results.

Another doubt that might arise, is whether it is really guilt activation that drives the change in behavior. First of all, we can rule out a simple second round effect, since in the control treatment we do not find first order beliefs to influence behavior. Even though we cannot prove the mechanics of guilt aversion, we can analyze the mean reactions of those who potentially could have felt guilt in the first round (when transfer was smaller than first order belief, group "guilt") and those who could not have felt guilt in the first round (those who gave at least as much as the receiver expected to get, group "no guilt"). When contrasting these groups - excluding those who had a zero transfer in both rounds[10] - one can see that there is indeed a reaction, meaning that those who potentially experienced guilt approached the first order belief.

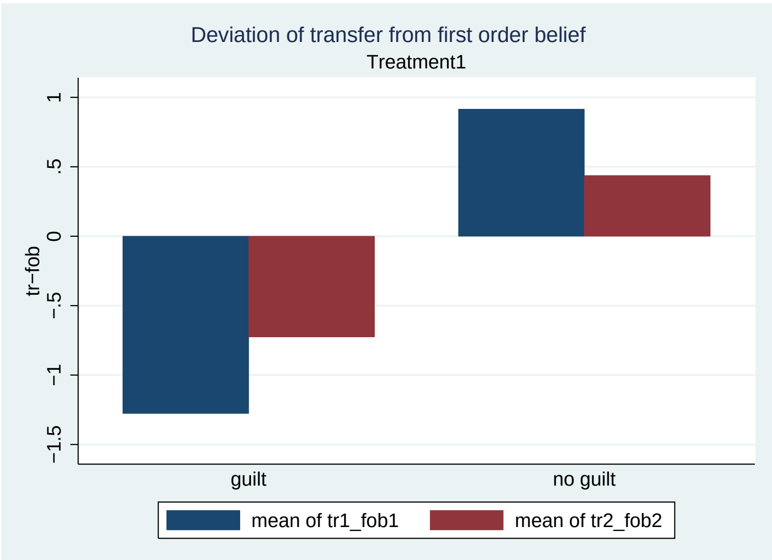

Figure 3.3: Change in behavior (LEF)

The effect is much more prominent in the PT treatment (figure 3.4) but also significant in the LEF treatment (figure 3.3). Note that in both figures the plotted bar is the mean of the difference between transfers and first order beliefs.

[10]In line with the classification of Fischbacher et al. (2001), these types are unconditional defectors (in LEF: 11, in PT: 16 dictators), which remain unimpressed - also by the first order belief.

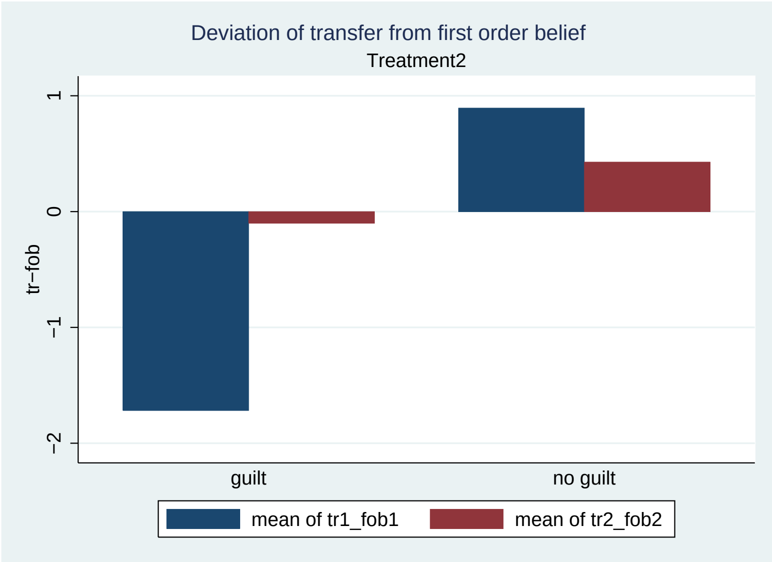

Figure 3.4: Change in behavior (PT)

3.7 Conclusion

We make three main contributions to the literature on guilt aversion.

First, we find significant positive correlation between the recipient's expectations and the dictator's donation in the second round of a two round modified dictator game, while reproducing the results of Ellingsen et al. (2010) of no correlation in the first round.

Second, we show that guilt aversion can be activated through two different channels. Both a thought experiment and the repetition of the situation triggered guilt aversion.

Third, our paper also suggests a new way to deal with heterogeneity in economic experiments with small sample size. We use past behavior as a proxy for individual characteristics which enables us to get strong results.

Our results also have implications for applications. The act of perspective taking is an instrument also used in real life for pedagogic reasons. Based on the presented experiment, we can show how effective such practice is for promoting prosocial behavior. In the design of long distance workplaces - for which the anonymous experimental setup might be a good proxy - putting oneself into the shoes of the other might effectively enhance cooperation.

When thinking about the implications and classification of these results, it might be fruitful to enlarge the scope of the paper and look at other concepts of other-regarding preferences. One interesting approach has been suggested by (Bénabou and Tirole 2006) which contains a general component "self image". Guilt aversion can be seen as part of this building block which would allow reconciliation of the two different approaches. At the same time the comparison reveals the strength of a more specific theory like guilt aversion since it allows for testable implications as opposed to the vague concept of self image.

Further, guilt aversion is most probably an important element of a more general theory of empathy. Whether the ability to feel empathy is a necessary or even a sufficient condition for guilt aversion is yet unclear. A next step could be an attempt to control for the underlying abilities, e.g. by neuoeconomic means that allow for the observation of neurological correlates of decisions. Hence, an extension of this research question to the rapidly growing field of neuroeconomics seems to be promising (see e.g. Singer and Fehr 2005).

3.8 Appendix

3.8.1 Histograms and Densities

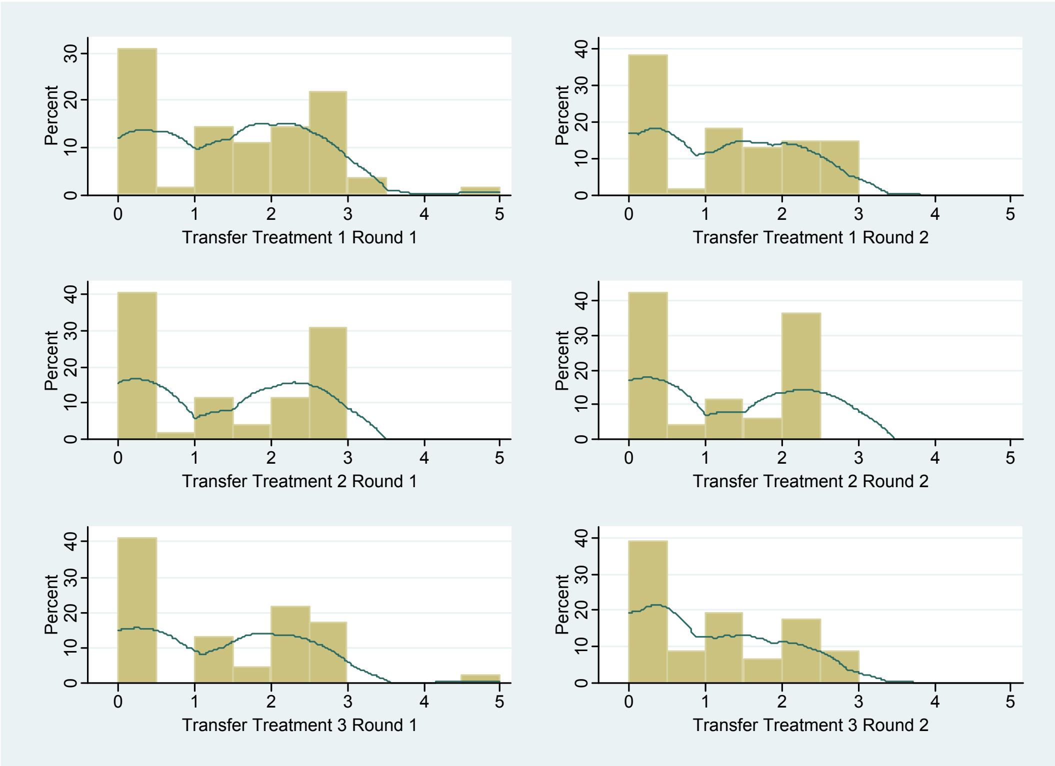

Figure 3.5: Detailed Histograms Transfers

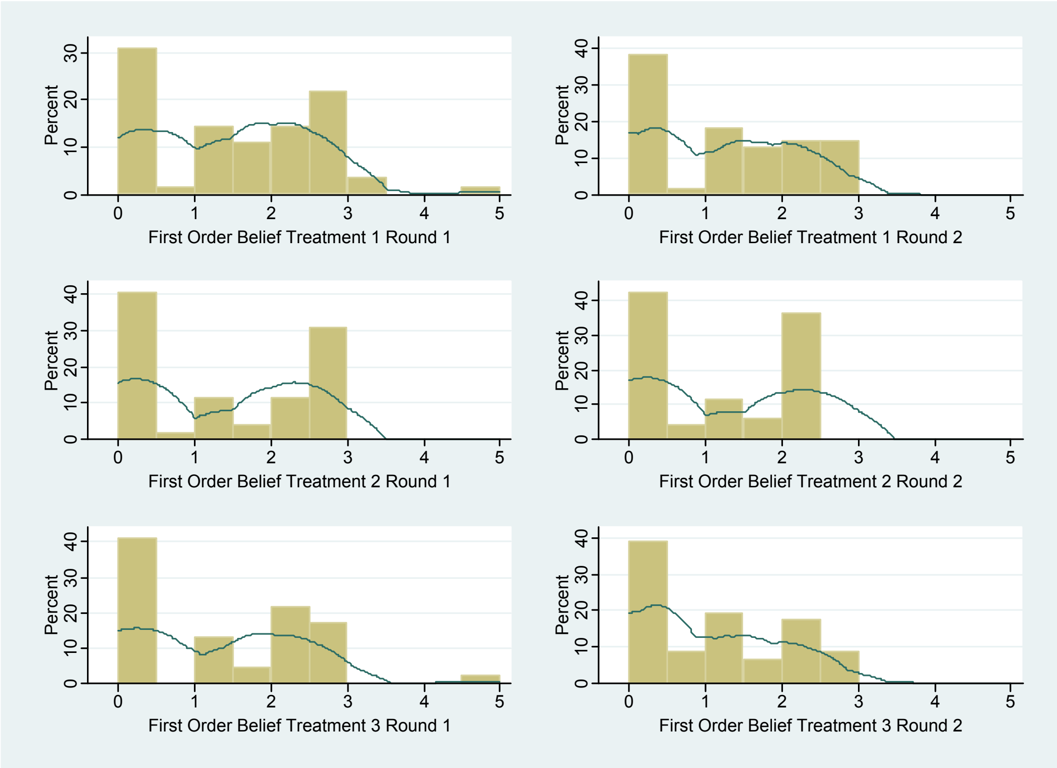

Figure 3.6: Detailed Histograms First Order Beliefs

3.8.2 OLS estimates of main results

Table 3.7: OLS estimations

	(1)	(2)	(3)	(4)	(5)	(6)
	tr1	tr2	tr1	tr2	tr1	tr2
fob1	-0.0256		0.353**			
	(-0.17)		(2.69)			
ger	-0.216	0.498*	-0.611*	-0.746**	0.0143	-0.0633
	(-0.70)	(1.82)	(-2.01)	(-2.64)	(0.04)	(-0.21)
female	0.0902	-0.0559	0.0367	0.0311	0.499	0.163
	(0.30)	(-0.21)	(0.12)	(0.11)	(1.30)	(0.53)
age	0.117**	0.0530	-0.0553	-0.0444	0.0307	0.0383
	(2.26)	(1.16)	(-1.32)	(-1.14)	(0.48)	(0.74)
econ	-0.882***	-0.257	-0.761**	-0.713**	-0.185	-0.324
	(-2.87)	(-0.96)	(-2.47)	(-2.48)	(-0.49)	(-1.07)
fob2		0.260*		0.259**		0.116
		(1.96)		(2.21)		(0.76)
_cons	-0.480	-0.496	2.838***	2.673***	0.319	0.0642
	(-0.38)	(-0.45)	(2.70)	(2.74)	(0.21)	(0.05)
N	54	53	50	52	46	46

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3.8: OLS estimations controlling for past behavior

	(1)	(2)	(3)	(4)
	tr2	tr1	tr2	tr2
fob2	0.375***		0.256***	0.0469
	(3.15)		(2.97)	(0.36)
tr1	0.458***		0.620***	0.411***
	(4.00)		(6.32)	(3.81)
ger	0.572**	-0.536*	-0.383*	-0.0913
	(2.40)	(-1.98)	(-1.78)	(-0.35)
female	-0.104	-0.0235	0.0114	-0.0590
	(-0.45)	(-0.09)	(0.06)	(-0.22)
age	0.00721	-0.103**	-0.0190	0.0210
	(0.18)	(-2.62)	(-0.66)	(0.47)
econ	0.125	-0.636**	-0.214	-0.273
	(0.50)	(-2.31)	(-0.95)	(-1.04)
fob1		0.238*		
		(1.97)		
sob		0.422***		
		(3.58)		
_cons	-0.535	3.210***	0.851	0.144
	(-0.56)	(3.42)	(1.10)	(0.13)
N	53	50	52	46

t statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

3.8.3 Influence of personal characteristics

Influence of ethnicity (Tobit)

	(1)	(2)	(3)	(4)	(5)	(6)
	tr2	tr2	tr1	tr1	tr2	tr2
fob2	0.0980	0.188			0.352***	0.297***
	(0.65)	(1.33)			(2.93)	(2.97)
fob2_ger	0.149	0.146			-0.265**	-0.152
	(1.06)	(1.12)			(-2.20)	(-1.51)
female (d)	-0.144	-0.187	0.00388	-0.0884	0.0494	0.0369
	(-0.71)	(-0.98)	(0.02)	(-0.42)	(0.24)	(0.22)
age	0.0277	-0.00126	-0.0458	-0.0896***	-0.0329	-0.0217
	(0.80)	(-0.04)	(-1.30)	(-2.73)	(-1.03)	(-0.76)
econ (d)	-0.264	-0.00462	-0.601**	-0.507**	-0.528**	-0.131
	(-1.24)	(-0.02)	(-2.31)	(-2.16)	(-2.24)	(-0.67)
tr1		0.330***				0.527***
		(3.46)				(5.82)
fob1			0.391***	0.317**		
			(2.75)	(2.46)		
fob1_ger			-0.245*	-0.270**		
			(-1.68)	(-2.06)		
sob				0.391***		
				(3.96)		
N	53	53	50	50	52	52

Marginal effects for tobit regressions of transfers in round 1 (tr1) and 2 (tr2); t statistics in parentheses

(d) for discrete change of dummy variable from 0 to 1

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

Influence of ethnicity (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)
	tr2	tr2	tr1	tr1	tr2	tr2
fob2	0.103	0.223			0.517***	0.391***
	(0.52)	(1.23)			(3.15)	(3.23)
fob2_ger	0.232	0.222			-0.385**	-0.202
	(1.22)	(1.31)			(-2.30)	(-1.61)
female	-0.0985	-0.154	0.0377	-0.0418	0.0819	0.0358
	(-0.37)	(-0.64)	(0.13)	(-0.16)	(0.30)	(0.18)
age	0.0481	0.00427	-0.0505	-0.101**	-0.0311	-0.0117
	(1.04)	(0.10)	(-1.20)	(-2.57)	(-0.78)	(-0.40)
econ	-0.297	0.0627	-0.729**	-0.594**	-0.721**	-0.207
	(-1.09)	(0.24)	(-2.33)	(-2.14)	(-2.46)	(-0.91)
tr1		0.434***				0.631***
		(3.65)				(6.46)
fob1			0.573***	0.458**		
			(2.99)	(2.68)		
fob1_ger			-0.350*	-0.352**		
			(-1.77)	(-2.03)		
sob				0.441***		
				(3.76)		
N	53	53	50	50	52	52

Marginal effects for OLS regressions of transfers in round 1 (tr1) and 2 (tr2); *t* statistics in parentheses

(d) for discrete change of dummy variable from 0 to 1

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

Influence of economics as field of study (Tobit)

	(1)	(2)	(3)	(4)	(5)	(6)
	tr2	tr2	tr1	tr1	tr2	tr2
fob2	0.196	0.216*			0.252**	0.175*
	(1.45)	(1.76)			(2.29)	(1.91)
fob2_econ	-0.0103	0.128			-0.0822	0.0581
	(-0.08)	(1.04)			(-0.70)	(0.59)
female (d)	-0.0406	-0.128	0.143	0.0341	0.124	0.0648
	(-0.21)	(-0.71)	(0.63)	(0.16)	(0.60)	(0.39)
age	0.0311	-0.00729	-0.0337	-0.0788**	-0.0285	-0.0242
	(0.89)	(-0.23)	(-0.93)	(-2.33)	(-0.88)	(-0.81)
ger (d)	0.388**	0.439**	-0.559**	-0.538**	-0.615**	-0.316
	(2.04)	(2.59)	(-2.03)	(-2.16)	(-2.44)	(-1.61)
tr1		0.379***				0.550***
		(4.12)				(6.19)
fob1			0.293**	0.184		
			(2.15)	(1.47)		
fob1_econ			-0.102	-0.0739		
			(-0.71)	(-0.57)		
sob				0.396***		
				(3.91)		
N	53	53	50	50	52	52

Marginal effects for tobit regressions of transfers in round 1 (tr1) and 2 (tr2); *t* statistics in parentheses

(d) for discrete change of dummy variable from 0 to 1

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

Influence of economics as field of study (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)
	tr2	tr2	tr1	tr1	tr2	tr2
fob2	0.271	0.279*			0.368**	0.240**
	(1.51)	(1.81)			(2.45)	(2.24)
fob2_econ	-0.0270	0.149			-0.117	0.0528
	(-0.15)	(0.95)			(-0.71)	(0.45)
female	-0.00105	-0.110	0.223	0.129	0.179	0.0636
	(-0.00)	(-0.49)	(0.74)	(0.48)	(0.64)	(0.32)
age	0.0551	-0.000597	-0.0378	-0.0909**	-0.0240	-0.00959
	(1.18)	(-0.01)	(-0.84)	(-2.16)	(-0.60)	(-0.34)
ger	0.512*	0.553**	-0.610*	-0.534*	-0.816***	-0.392*
	(1.85)	(2.34)	(-1.88)	(-1.86)	(-2.74)	(-1.81)
tr1		0.464***				0.661***
		(4.25)				(6.99)
fob1			0.453**	0.305*		
			(2.43)	(1.79)		
fob1_econ			-0.173	-0.129		
			(-0.86)	(-0.73)		
sob				0.450***		
				(3.65)		
N	53	53	50	50	52	52

Marginal effects for OLS regressions of transfers in round 1 (tr1) and 2 (tr2); *t* statistics in parentheses

(d) for discrete change of dummy variable from 0 to 1

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

Influence of gender (tobit)

	(1)	(2)	(3)	(4)	(5)	(6)
	tr2	tr2	tr1	tr1	tr2	tr2
model						
fob2	0.228*	0.380***			0.223**	0.198**
	(1.80)	(3.09)			(2.25)	(2.32)
fob2_female	-0.0502	-0.134			-0.116	-0.00252
	(-0.40)	(-1.16)			(-1.01)	(-0.03)
econ (d)	-0.221	0.0553	-0.619**	-0.521**	-0.567**	-0.140
	(-1.08)	(0.29)	(-2.50)	(-2.32)	(-2.49)	(-0.72)
age	0.0306	-0.00332	-0.0496	-0.0912***	-0.0454	-0.0289
	(0.89)	(-0.10)	(-1.42)	(-2.78)	(-1.44)	(-0.99)
ger (d)	0.385**	0.454***	-0.550**	-0.527**	-0.600**	-0.310
	(2.05)	(2.70)	(-2.08)	(-2.19)	(-2.49)	(-1.58)
tr1		0.376***				0.520***
		(3.93)				(5.61)
fob1			0.239*	0.166		
			(1.97)	(1.49)		
fob1_female			0.0198	-0.0146		
			(0.15)	(-0.12)		
sob				0.375***		
				(3.84)		
N	53	53	50	50	52	52

Marginal effects for tobit regressions of transfers in round 1 (tr1) and 2 (tr2); t statistics in parentheses

(d) for discrete change of dummy variable from 0 to 1

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

Influence of gender (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)
	tr2	tr2	tr1	tr1	tr2	tr2
fob2	0.279	0.454***			0.329**	0.271***
	(1.65)	(2.97)			(2.50)	(2.74)
fob2_female	-0.0337	-0.129			-0.171	-0.0361
	(-0.20)	(-0.86)			(-1.11)	(-0.31)
econ	-0.251	0.134	-0.757**	-0.621**	-0.765***	-0.231
	(-0.95)	(0.54)	(-2.54)	(-2.32)	(-2.76)	(-1.03)
age	0.0527	0.00350	-0.0540	-0.102**	-0.0487	-0.0202
	(1.16)	(0.08)	(-1.28)	(-2.56)	(-1.27)	(-0.70)
ger	0.500*	0.570**	-0.595*	-0.519*	-0.778***	-0.394*
	(1.84)	(2.41)	(-1.93)	(-1.89)	(-2.81)	(-1.83)
tr1		0.471***				0.615***
		(4.09)				(6.16)
fob1			0.328**	0.224		
			(2.08)	(1.56)		
fob1_female			0.0535	0.0289		
			(0.29)	(0.18)		
sob				0.420***		
				(3.58)		
N	53	53	50	50	52	52

Marginal effects for OLS regressions of transfers in round 1 (tr1) and 2 (tr2); *t* statistics in parentheses

(d) for discrete change of dummy variable from 0 to 1

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

3.8.4 Stata code for the Generalized Cook's Distance

Please find below the do.file for stata. Comments are added as follows: /*comment*/.

```
/*Initializing variables for Generalized Cook's Distance (round 1: GCD1; round 2: GCD2)*/
/*as well as for a dummy variable indicating values exceeding the critical value 4/n*/
gen GCD1=0
gen GCD2=0
gen GCDlarge=0

/*Calculating distance measure for Treatment 1 Round 1*/
tobit tr1 fob1 if treatment==1,ll
estimates store model11
matrix b=e(b)
matrix V=e(V)
matrix vinv=inv(V)
forvalues i=1/55 {
ereturn clear
tobit tr1 fob1 if treatment==1 & nb!=`i',ll
estimates store model_temp1
matrix b_temp=e(b)
matrix GCD_temp=(b-b_temp)*vinv*(b-b_temp)'
svmat GCD_temp, names(GCD_temp)
qui sum GCD_temp
gen temp=r(max)
replace GCD1=temp/3 if nb==`i'
drop GCD_temp temp _est_model_temp1
}
replace GCDlarge=1 if GCD1>4/55

/*Calculating distance measure for Treatment 1 Round 2*/
ereturn clear
rreturn clear
tobit tr2 fob2 if treatment==1,ll
estimates store model12
matrix b=e(b)
matrix V=e(V)
matrix vinv=inv(V)
forvalues i=1/55 {
ereturn clear
tobit tr2 fob2 if treatment==1 & nb!=`i',ll
estimates store model_temp2
matrix b_temp=e(b)
matrix GCD_temp=(b-b_temp)*vinv*(b-b_temp)'
svmat GCD_temp, names(GCD_temp)
qui sum GCD_temp
gen temp=r(max)
```

```
replace GCD2=temp/3 if nb==`i'
drop GCD_temp temp _est_model_temp2
}
replace GCDlarge=1 if GCD2>4/52

/*Calculating distance measure for Treatment 2 Round 1*/
ereturn clear
rreturn clear
tobit tr1 fob1 if treatment==2,ll
estimates store model21
matrix b=e(b)
matrix V=e(V)
matrix vinv=inv(V)
forvalues i=56/107 {
ereturn clear
tobit tr1 fob1 if treatment==2 & nb!=`i',ll
estimates store model_temp1
matrix b_temp=e(b)
matrix GCD_temp=(b-b_temp)*vinv*(b-b_temp)'
svmat GCD_temp, names(GCD_temp)
qui sum GCD_temp
gen temp=r(max)
replace GCD1=temp/3 if nb==`i'
drop GCD_temp temp _est_model_temp1
}
replace GCDlarge=1 if GCD1>4/52

/*Calculating distance measure for Treatment 1 Round 2*/
ereturn clear
rreturn clear
tobit tr2 fob2 if treatment==2,ll
estimates store model22
matrix b=e(b)
matrix V=e(V)
matrix vinv=inv(V)
forvalues i=56/107 {
ereturn clear
tobit tr2 fob2 if treatment==2 & nb!=`i',ll
estimates store model_temp2
matrix b_temp=e(b)
matrix GCD_temp=(b-b_temp)*vinv*(b-b_temp)'
svmat GCD_temp, names(GCD_temp)
qui sum GCD_temp
gen temp=r(max)
replace GCD2=temp/3 if nb==`i'
drop GCD_temp temp _est_model_temp2
```

```
}
replace GCDlarge=1 if GCD2>4/52

/*Calculating distance measure for Treatment 3 Round 1*/
ereturn clear
rreturn clear
tobit tr2 fob2 if treatment==3,ll
estimates store model31
matrix b=e(b)
matrix V=e(V)
matrix vinv=inv(V)
forvalues i=108/153 {
ereturn clear
tobit tr2 fob2 if treatment==3 & nb!=`i',ll
estimates store model_temp2
matrix b_temp=e(b)
matrix GCD_temp=(b-b_temp)*vinv*(b-b_temp)'
svmat GCD_temp, names(GCD_temp)
qui sum GCD_temp
gen temp=r(max)
replace GCD2=temp/3 if nb==`i'
drop GCD_temp temp _est_model_temp2
}
replace GCDlarge=1 if GCD2>4/46
```

3.8.5 Instructions

Treatment 1 (PT), Receivers

INSTRUKTIONEN

Vielen Dank für die Teilnahme an diesem Experiment. Sie erhalten 3 Euro für Ihre Teilnahme, alle weiteren Auszahlungen bestimmen sich im Laufe des Experiments. Es ist nicht erlaubt, während des Experimentes mit den anderen Teilnehmern zu kommunizieren.

Für das Experiment werden Sie per Zufall einer Person in einem anderen Raum zugeteilt. Sie werden nicht erfahren, wer die andere Person ist, weder während noch nach dem Experiment. Sie sind in Raum B. Die Ihnen im anderen Raum (Raum A) zugeteilte Person wird darüber entscheiden, wie sie 5 Euro zwischen sich und Ihnen aufteilt. Die individuellen Entscheidungen bleiben anonym, sowohl den anderen Teilnehmern als auch den Experimentleitern gegenüber.

Bevor wir das eigentliche Experiment starten, bitten wir um Ihre Einschätzung, welchen Betrag Ihnen die Person aus Raum A von den 5 Euro überlassen wird. Bitte öffnen Sie dazu den Umschlag. Sie werden darin zwei Blätter finden, ein Entscheidungsblatt und ein Blatt mit Ihrem persönlichen Code.

Welchen Betrag werden Sie Ihrer Meinung nach von der Ihnen zugeteilten Person aus Raum A erhalten? **Die Person, deren Schätzung am nächsten an dem tatsächlich weitergegebenen Betrag liegt, bekommt eine zusätzliche Auszahlung von 5 Euro.** Haben mehrere Personen den erwarteten Betrag gleich gut geschätzt, werden die 5 Euro unter diesen Personen gleichmäßig aufgeteilt.

Schreiben Sie den von Ihnen geschätzten Betrag sowie die drei **Buchstaben** Ihres persönlichen Codes in die dafür vorgesehenen Felder des Schätzblattes und stecken Sie es zurück in den Umschlag. Behalten Sie Ihren persönlichen Code bis zur Auszahlung. Kleben Sie den Umschlag nicht zu. Der Umschlag wird anschließend von den Experimentleitern eingesammelt.

Bitte bleiben Sie nach dem Einsammeln der Umschläge weiterhin stillschweigend an Ihrem Platz sitzen und warten Sie auf weitere Instruktionen.

INSTRUKTIONEN (2)

Sie werden erneut per Zufall einer Person aus Raum A zugeteilt. Diese Person muss daher nicht identisch sein mit der Person aus der ersten Runde. Sie werden nicht erfahren, wer die andere Person ist, weder während noch nach dem Experiment. Auch die Ihnen diesmal zugeteilte Person wird darüber entscheiden, wie sie 5 Euro zwischen sich und Ihnen aufteilt. Die individuellen Entscheidungen bleiben anonym, sowohl den anderen Teilnehmern als auch den Experimentleitern gegenüber.

Ihnen wurde ein Umschlag ausgeteilt. Sie werden darin ein Entscheidungsblatt finden. Bitte schreiben Sie hierauf den Betrag, den Sie Ihrer Meinung nach von der Ihnen diesmal zugeteilten Person erhalten werden. Bitte tragen Sie in das entsprechende Feld weiterhin sowie die drei **Ziffern** Ihres persönlichen Codes ein. **Die Person, deren Schätzung am nächsten an dem tatsächlich weitergegebenen Betrag liegt, bekommt eine zusätzliche Auszahlung von 5 Euro.** Haben mehrere Personen den tatsächlichen Betrag gleich gut geschätzt, werden die 5 Euro unter diesen Personen gleichmäßig aufgeteilt.

Behalten Sie weiterhin das Papier mit Ihrem Code und stecken Sie das Entscheidungsblatt mit dem von Ihnen geschätzten Betrag zurück in den Umschlag. Kleben Sie den Umschlag nicht zu. Der Umschlag wird anschließend von den Experimentleitern wieder eingesammelt.

Bitte bleiben Sie nach dem Einsammeln der Umschläge weiterhin stillschweigend an Ihrem Platz sitzen und warten Sie auf weitere Instruktionen.

Treatment1 (PT), Dictators

INSTRUKTIONEN

Vielen Dank für die Teilnahme an diesem Experiment. Sie erhalten 3 Euro für Ihre Teilnahme, alle weiteren Auszahlungen bestimmen sich im Laufe des Experiments. Es ist nicht erlaubt, während des Experimentes mit den anderen Teilnehmern zu kommunizieren.

Für das Experiment werden Sie per Zufall einer Person in einem anderen Raum zugeteilt. Sie werden nicht erfahren, wer die andere Person ist, weder während noch nach dem Experiment. Dies ist Raum A. Alle Personen in Raum A (Sie) werden darüber entscheiden, wie Sie 5 Euro zwischen sich und der Ihnen zugeteilten Person in Raum B aufteilen. Wir haben die Personen in Raum B vorab gefragt, welchen Betrag er/sie glaubt, von Ihnen zu bekommen.

Ihnen wurde ein Umschlag ausgeteilt. Sie finden darin drei Blätter: Ihren persönlichen Code, Ihr Entscheidungsblatt und ein Blatt, auf dem notiert ist, welchen Betrag die Ihnen zugeteilte Person aus Raum B als Auszahlung erwartet. Die Person in Raum B wusste nicht, dass Ihnen der erwartete Wert vorgelegt wird.

Schreiben Sie auf das Entscheidungsblatt, wieviel Sie von den 5 Euro an die Ihnen zugeteilte Person in Raum B weitergeben. Ihre Entscheidungen treffen Sie ganz alleine. Niemand, auch nicht der Experimentleiter, wird erfahren, welche Entscheidung eine bestimmte Person getroffen hat.

Wenn Sie Ihre Entscheidungen notiert haben, behalten Sie Ihren persönlichen Code und stecken Sie die beiden anderen Blätter wieder zurück in den Umschlag. Kleben Sie den Umschlag nicht zu. Anschließend werden die Umschläge von den Experimentleitern wieder eingesammelt.

Bitte bleiben Sie nach dem Einsammeln der Umschläge weiterhin schweigsam an Ihrem Platz sitzen und warten Sie auf weitere Instruktionen.

Beispiele:
"Ich werde der Person in Raum B folgenden Betrag geben: 0 Euro"
-> Sie erhalten 5 Euro, die Ihnen zugewiesene Person in Raum B 0 Euro.

"Ich werde der Person in Raum B folgenden Betrag geben: 5 Euro"
-> Sie erhalten 0 Euro, die Ihnen zugewiesene Person in Raum B 5 Euro.

INSTRUKTIONEN (2)

Sie werden erneut per Zufall einer Person aus Raum B zugeteilt. Diese Person muss daher nicht identisch sein mit der Person aus der ersten Runde. Sie werden nicht erfahren, wer die andere Person ist, weder während noch nach dem Experiment. Sie werden wieder darüber entscheiden, wie sie 5 Euro zwischen sich und der Ihnen in dieser Runde zugeteilten Person in Raum B aufteilen.

Ihnen wurde ein neuer Umschlag ausgeteilt. Hierin finden Sie zwei Blätter: Ihr Entscheidungsblatt und ein Blatt, auf dem notiert ist, welchen Betrag, die Ihnen diesmal zugeteilte Person aus Raum B als Auszahlung erwartet. Die Person in Raum B wusste nicht, dass Ihnen der erwartete Wert vorgelegt wird. Bitte tragen Sie Ihren persönlichen Code in das dafür vorgesehene Feld ein. Schreiben Sie zudem auf das Entscheidungsblatt, wieviel Sie von den 5 Euro an die Ihnen zugeteilte Person in Raum B weitergeben. Ihre Entscheidungen treffen Sie ganz alleine. Niemand, auch nicht der Experimentleiter, wird erfahren, welche Entscheidung eine bestimmte Person getroffen hat.

Wenn Sie Ihre Entscheidung notiert haben, stecken Sie beide Blätter wieder zurück in den Umschlag und behalten Sie weiterhin Ihren persönlichen Code. Kleben Sie den Umschlag nicht zu. Anschließend werden die Umschläge von den Experimentleitern wieder eingesammelt.

Bitte bleiben Sie danach weiterhin stillschweigend sitzen und warten Sie auf weitere Instruktionen.

Treatment 2 (LEF), receivers

INSTRUKTIONEN

Vielen Dank für die Teilnahme an diesem Experiment. Sie erhalten 3 Euro für Ihre Teilnahme, alle weiteren Auszahlungen bestimmen sich im Laufe des Experiments. Es ist nicht erlaubt, während des Experimentes mit den anderen Teilnehmern zu kommunizieren.

Für das Experiment werden Sie per Zufall einer Person in einem anderen Raum zugeteilt. Sie werden nicht erfahren, wer die andere Person ist, weder während noch nach dem Experiment. Sie sind in Raum B. Die Ihnen im anderen Raum (Raum A) zugeteilte Person wird darüber entscheiden, wie sie 5 Euro zwischen sich und Ihnen aufteilt. Die individuellen Entscheidungen bleiben anonym, sowohl den anderen Teilnehmern als auch den Experimentleitern gegenüber.

Bevor wir das eigentliche Experiment starten, bitten wir um Ihre Einschätzung, welchen Betrag Ihnen die Person aus Raum A von den 5 Euro überlassen wird. Bitte öffnen Sie dazu den Umschlag. Sie werden darin zwei Blätter finden, ein Entscheidungsblatt und ein Blatt mit Ihrem persönlichen Code.

Welchen Betrag werden Sie Ihrer Meinung nach von der Ihnen zugeteilten Person aus Raum A erhalten? **Die Person, deren Schätzung am nächsten an dem tatsächlich weitergegebenen Betrag liegt, bekommt eine zusätzliche Auszahlung von 5 Euro.** Haben mehrere Personen den erwarteten Betrag gleich gut geschätzt, werden die 5 Euro unter diesen Personen gleichmäßig aufgeteilt.

Schreiben Sie den von Ihnen geschätzten Betrag sowie die drei **Buchstaben** Ihres persönlichen Codes in die dafür vorgesehenen Felder des Schätzblattes und stecken Sie es zurück in den Umschlag. Behalten Sie Ihren persönlichen Code bis zur Auszahlung. Kleben Sie den Umschlag nicht zu. Der Umschlag wird anschließend von den Experimentleitern eingesammelt.

Bitte bleiben Sie nach dem Einsammeln der Umschläge weiterhin stillschweigend an Ihrem Platz sitzen und warten Sie auf weitere Instruktionen.

INSTRUKTIONEN (2)

Sie werden erneut per Zufall einer Person aus Raum A zugeteilt. Diese Person muss daher nicht identisch sein mit der Person aus der ersten Runde. Sie werden nicht erfahren, wer die andere Person ist, weder während noch nach dem Experiment. Auch die Ihnen diesmal zugeteilte Person wird darüber entscheiden, wie sie 5 Euro zwischen sich und Ihnen aufteilt. Die individuellen Entscheidungen bleiben anonym, sowohl den anderen Teilnehmern als auch den Experimentleitern gegenüber.

Ihnen wurde ein Umschlag ausgeteilt. Sie werden darin ein Entscheidungsblatt finden. Bitte schreiben Sie hierauf den Betrag, den Sie Ihrer Meinung nach von der Ihnen diesmal zugeteilten Person erhalten werden. Bitte tragen Sie in das entsprechende Feld weiterhin sowie die drei **Ziffern** Ihres persönlichen Codes ein. **Die Person, deren Schätzung am nächsten an dem tatsächlich weitergegebenen Betrag liegt, bekommt eine zusätzliche Auszahlung von 5 Euro.** Haben mehrere Personen den tatsächlichen Betrag gleich gut geschätzt, werden die 5 Euro unter diesen Personen gleichmäßig aufgeteilt.

Behalten Sie weiterhin das Papier mit Ihrem Code und stecken Sie das Entscheidungsblatt mit dem von Ihnen geschätzten Betrag zurück in den Umschlag. Kleben Sie den Umschlag nicht zu. Der Umschlag wird anschließend von den Experimentleitern wieder eingesammelt.

Bitte bleiben Sie nach dem Einsammeln der Umschläge weiterhin stillschweigend an Ihrem Platz sitzen und warten Sie auf weitere Instruktionen.

Treatment 2 (LEF), dictators

INSTRUKTIONEN

Vielen Dank für die Teilnahme an diesem Experiment. Sie erhalten 3 Euro für Ihre Teilnahme, alle weiteren Auszahlungen bestimmen sich im Laufe des Experiments. Es ist nicht erlaubt, während des Experimentes mit den anderen Teilnehmern zu kommunizieren.

Für das Experiment werden Sie per Zufall einer Person in einem anderen Raum zugeteilt. Sie werden nicht erfahren, wer die andere Person ist, weder während noch nach dem Experiment. Dies ist Raum A. Alle Personen in Raum A (Sie) werden darüber entscheiden, wie Sie 5 Euro zwischen sich und der Ihnen zugeteilten Person in Raum B aufteilen.

Ihnen wurde ein Umschlag ausgeteilt. Sie finden darin zwei Blätter: ein Schätzblatt und einen Zettel mit Ihrem persönlichen Code, den Sie später zur Abholung Ihrer möglichen Auszahlung vorlegen müssen. Wir haben die Personen in Raum B gefragt, welchen Betrag er/sie glaubt, von Ihnen zu bekommen. Welchen Betrag hat die Ihnen zugewiesene Person Ihrer Meinung nach genannt? **Die Person, deren Schätzung am nächsten an dem tatsächlich erwarteten Betrag liegt, bekommt eine zusätzliche Auszahlung von 5 Euro.** Haben mehrere Personen den erwarteten Betrag gleich gut geschätzt, werden die 5 Euro unter diesen Personen gleichmäßig aufgeteilt.

Schreiben Sie den von Ihnen geschätzten Betrag sowie Ihren Code in die dafür vorgesehenen Felder des Schätzblattes und stecken Sie es zurück in den Umschlag. Behalten Sie Ihren persönlichen Code bis zur Auszahlung. Kleben Sie den Umschlag nicht zu. Der Umschlag wird anschließend von den Experimentleitern eingesammelt.

INSTRUKTIONEN (1)

Sie wurden nun erneut per Zufall einer Person in dem anderen Raum zugeteilt. Diese Person muss daher nicht identisch sein mit der Person aus der Schätzrunde.

Ihnen wurde ein neuer Umschlag ausgeteilt. Hierin finden Sie zwei Blätter: Ihr Entscheidungsblatt und ein Blatt, auf dem notiert ist, welchen Betrag, die Ihnen zugeteilte Person aus Raum B als Auszahlung erwartet. Die Person in Raum B wusste nicht, dass Ihnen der erwartete Wert vorgelegt wird. Bitte tragen Sie Ihren persönlichen Code in das dafür vorgesehene Feld ein. Schreiben Sie zudem auf das Entscheidungsblatt, wieviel Sie von den 5 Euro an die Ihnen zugeteilte Person in Raum B weitergeben. Ihre Entscheidungen treffen Sie ganz alleine. Niemand, auch nicht der Experimentleiter, wird erfahren, welche Entscheidung eine bestimmte Person getroffen hat.

Wenn Sie Ihre Entscheidung notiert haben, stecken Sie beide Blätter wieder zurück in den Umschlag und behalten Sie weiterhin Ihren persönlichen Code. Kleben Sie den Umschlag nicht zu. Anschließend werden die Umschläge von den Experimentleitern wieder eingesammelt.

Bitte bleiben Sie danach weiterhin stillschweigend sitzen und warten Sie auf weitere Instruktionen.

Beispiele:
"Ich werde der Person in Raum B folgenden Betrag geben: 0 Euro"
-> Sie erhalten 5 Euro, die Ihnen zugewiesene Person in Raum B 0 Euro.

"Ich werde der Person in Raum B folgenden Betrag geben: 5 Euro"
-> Sie erhalten 0 Euro, die Ihnen zugewiesene Person in Raum B 5 Euro.

INSTRUKTIONEN (2)

Sie werden erneut per Zufall einer Person aus Raum B zugeteilt. Diese Person muss daher nicht identisch sein mit der Person aus der ersten Spielrunde. Sie werden nicht erfahren, wer die andere Person ist, weder während noch nach dem Experiment. Sie werden wieder darüber entscheiden, wie Sie 5 Euro zwischen sich und der Ihnen in dieser Runde zugeteilten Person in Raum B aufteilen.

Sie haben einen weiteren Umschlag erhalten. Hierin finden Sie zwei Blätter, ein Entscheidungsblatt und ein Blatt, auf dem notiert ist, welchen Betrag, die Ihnen in dieser Runde zugeteilte Person aus Raum B als Auszahlung erwartet. Die Person in Raum B wusste nicht, dass Ihnen der erwartete Wert vorgelegt wird. Bitte tragen Sie Ihren persönlichen Code in das dafür vorgesehene Feld ein. Schreiben Sie zudem auf das Entscheidungsblatt, wieviel Sie von den 5 Euro an die Ihnen zugeteilte Person in Raum B weitergeben. Ihre Entscheidungen treffen Sie ganz alleine. Niemand, auch nicht der Experimentleiter, wird erfahren, welche Entscheidung eine bestimmte Person getroffen hat.

Wenn Sie Ihre Entscheidung notiert haben, stecken Sie beide Blätter wieder zurück in den Umschlag und behalten Sie weiterhin Ihren persönlichen Code. Kleben Sie den Umschlag nicht zu.

Anschließend werden die Umschläge von den Experimentleitern wieder eingesammelt.

Bitte bleiben Sie weiterhin stillschweigend sitzen und warten Sie auf weitere Instruktionen.

Treatment 2 (LEF), receivers

INSTRUKTIONEN

Vielen Dank für die Teilnahme an diesem Experiment. Sie erhalten 3 Euro für Ihre Teilnahme, alle weiteren Auszahlungen bestimmen sich im Laufe des Experiments. Es ist nicht erlaubt, während des Experimentes mit den anderen Teilnehmern zu kommunizieren.

Für das Experiment werden Sie per Zufall einer Person in einem anderen Raum zugeteilt. Sie werden nicht erfahren, wer die andere Person ist, weder während noch nach dem Experiment. Sie sind in Raum B. Die Ihnen im anderen Raum (Raum A) zugeteilte Person wird darüber entscheiden, wie sie 5 Euro zwischen sich und Ihnen aufteilt. Die individuellen Entscheidungen bleiben anonym, sowohl den anderen Teilnehmern als auch den Experimentleitern gegenüber.

Bevor wir das eigentliche Experiment starten, bitten wir um Ihre Einschätzung, welchen Betrag Ihnen die Person aus Raum A von den 5 Euro überlassen wird. Bitte öffnen Sie dazu den Umschlag. Sie werden darin zwei Blätter finden, ein Entscheidungsblatt und ein Blatt mit Ihrem persönlichen Code.

Welchen Betrag werden Sie Ihrer Meinung nach von der Ihnen zugeteilten Person aus Raum A erhalten? **Die Person, deren Schätzung am nächsten an dem tatsächlich weitergegebenen Betrag liegt, bekommt eine zusätzliche Auszahlung von 5 Euro.** Haben mehrere Personen den erwarteten Betrag gleich gut geschätzt, werden die 5 Euro unter diesen Personen gleichmäßig aufgeteilt.

Schreiben Sie den von Ihnen geschätzten Betrag sowie Ihren persönlichen Code in das dafür vorgesehene Feld des Schätzblattes und stecken Sie es zurück in den Umschlag. Behalten Sie

Ihren persönlichen Code bis zur Auszahlung. Kleben Sie den Umschlag nicht zu. Der Umschlag wird anschließend von den Experimentleitern eingesammelt.

Bitte bleiben Sie nach dem Einsammeln der Umschläge weiterhin stillschweigend an Ihrem Platz sitzen und warten Sie auf weitere Instruktionen.

INSTRUKTIONEN (2)

Sie werden per Zufall nacheinander zwei Personen aus Raum A zugeteilt. Beide Personen entscheiden darüber, wie sie 5 Euro zwischen sich und Ihnen aufteilt. Die Entscheidung der zweiten Person ist maßgeblich für die zusätzliche Auszahlung des besten Schätzwertes. Die Gewinner dieses Schätzwettbewerbes werden innerhalb einer Woche per Email benachrichtigt. Alle anderen Auszahlungen werden direkt im Anschluss an das Experiment ausgezahlt.

Ihnen werden Fragebögen ausgeteilt. Bitte füllen Sie diese aus und warten Sie auf weitere Instruktionen.

Treatment 2 (LEF), dictators

INSTRUKTIONEN

Vielen Dank für die Teilnahme an diesem Experiment. Sie erhalten 3 Euro für Ihre Teilnahme, alle weiteren Auszahlungen bestimmen sich im Laufe des Experiments. Es ist nicht erlaubt, während des Experimentes mit den anderen Teilnehmern zu kommunizieren.

Für das Experiment werden Sie per Zufall einer Person in einem anderen Raum zugeteilt. Sie werden nicht erfahren, wer die andere Person ist, weder während noch nach dem Experiment. Dies ist Raum A. Alle Personen in Raum A (Sie) werden darüber entscheiden, wie Sie 5 Euro zwischen sich und der Ihnen zugeteilten Person in Raum B aufteilen.

Ihnen wurde ein Umschlag ausgeteilt. Sie finden darin drei Blätter: Ihren persönlichen Code und Ihr Entscheidungsblatt. Schreiben Sie auf das Entscheidungsblatt, wieviel Sie von den 5 Euro an die Ihnen zugeteilte Person in Raum B weitergeben. Ihre Entscheidungen treffen Sie ganz alleine. Niemand, auch nicht der Experimentleiter, wird erfahren, welche Entscheidung eine bestimmte Person getroffen hat.

Wenn Sie Ihre Entscheidungen notiert haben, behalten Sie Ihren persönlichen Code und stecken Sie die das andere Blatt wieder zurück in den Umschlag. Kleben Sie den Umschlag nicht zu. Anschließend werden die Umschläge von den Experimentleitern wieder eingesammelt.

Bitte bleiben Sie nach dem Einsammeln der Umschläge weiterhin schweigsam an Ihrem Platz sitzen und warten Sie auf weitere Instruktionen.

Beispiele:
"Ich werde der Person in Raum B folgenden Betrag geben: 0 Euro"
-> Sie erhalten 5 Euro, die Ihnen zugewiesene Person in Raum B 0 Euro.

"Ich werde der Person in Raum B folgenden Betrag geben: 5 Euro"
-> Sie erhalten 0 Euro, die Ihnen zugewiesene Person in Raum B 5 Euro.

INSTRUKTIONEN (2)

Sie werden erneut per Zufall einer Person aus Raum B zugeteilt. Diese Person muss daher nicht identisch sein mit der Person aus der ersten Runde. Sie werden nicht erfahren, wer die andere Person ist, weder während noch nach dem Experiment. Sie werden wieder darüber entscheiden, wie sie 5 Euro zwischen sich und der Ihnen in dieser Runde zugeteilten Person in Raum B aufteilen. Wir haben die Personen in Raum B diesmal vorab gefragt, welchen Betrag er/sie glaubt, von Ihnen zu bekommen.

Ihnen wurde ein neuer Umschlag ausgeteilt. Hierin finden Sie zwei Blätter: Ihr Entscheidungsblatt und ein Blatt, auf dem notiert ist, welchen Betrag, die Ihnen diesmal zugeteilte Person aus Raum B als Auszahlung erwartet. Die Person in Raum B wusste nicht, dass Ihnen der erwartete Wert vorgelegt wird. Bitte tragen Sie Ihren persönlichen Code in das dafür vorgesehene Feld ein. Schreiben Sie zudem auf das Entscheidungsblatt, wieviel Sie von den 5 Euro an die Ihnen zugeteilte Person in Raum B weitergeben. Ihre Entscheidungen treffen Sie ganz alleine. Niemand, auch nicht der Experimentleiter, wird erfahren, welche Entscheidung eine bestimmte Person getroffen hat.

Wenn Sie Ihre Entscheidung notiert haben, stecken Sie beide Blätter wieder zurück in den Umschlag und behalten Sie weiterhin Ihren persönlichen Code. Kleben Sie den Umschlag nicht zu. Anschließend werden die Umschläge von den Experimentleitern wieder eingesammelt.

Bitte bleiben Sie danach weiterhin stillschweigend sitzen und warten Sie auf weitere Instruktionen.

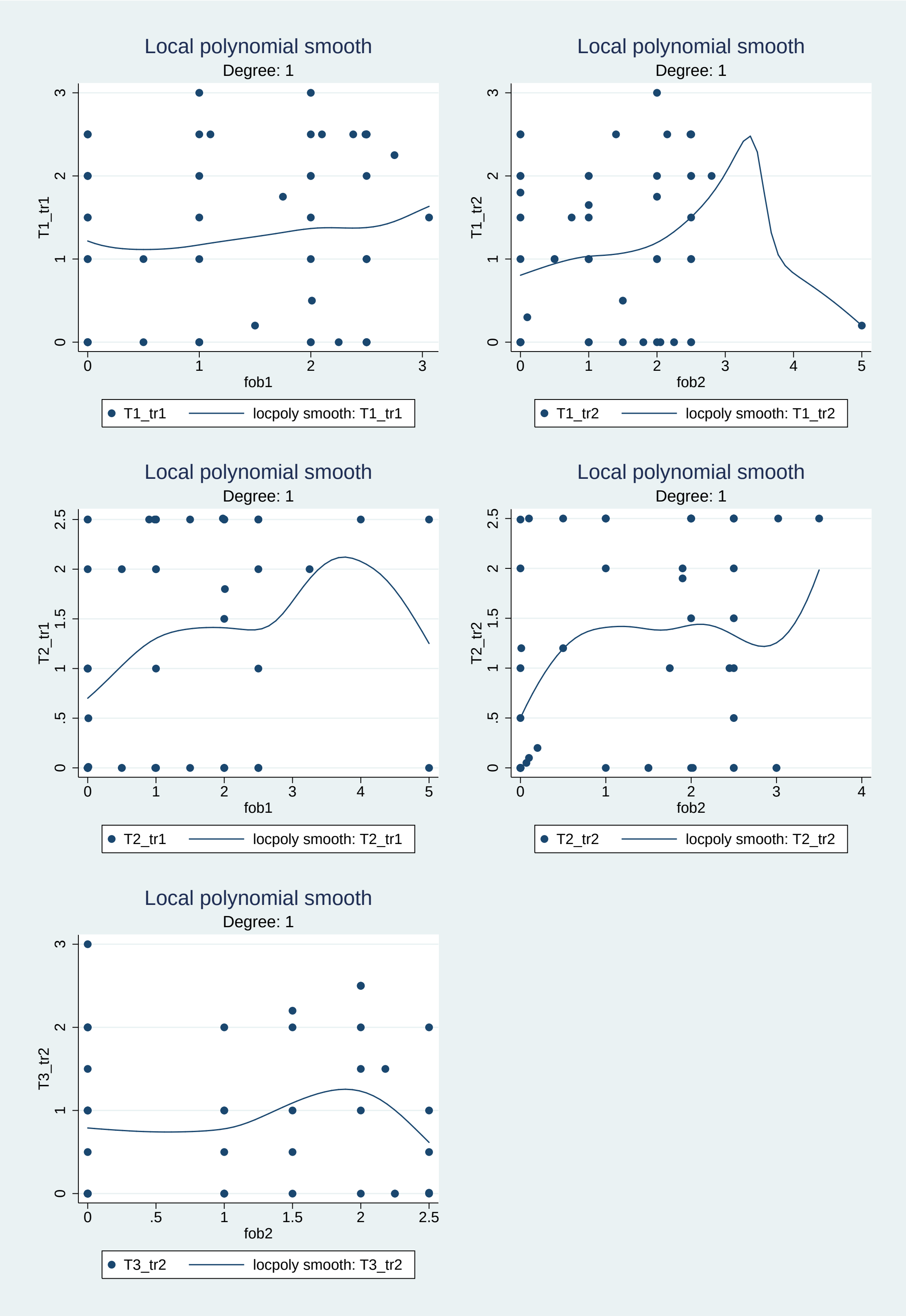

Figure 3.7: Local Polynomial Estimation

CHAPTER

4

Juvenile Law and Recidivism in Germany - Lessons for Europe? (co-authored with Stefan Pichler)

4.1 Introduction

[1]

"The history of criminal justice clearly shows that cruel punishments have been superseded by milder ones. The advance from brute to more human, from simple to more diversified forms of punishments has proceeded while now future paths emerge that need yet to be followed." BVerfGE 45, S. 187, 229.

Crime has been a major problem in all societies throughout time. Over the centuries different methods for punishment have been applied. However, there is still no clear answer to the debate on optimal criminal legislation. From an economist's perspective crime can be seen as the result of rational choice behavior. According to this approach, which goes back to Becker (1968), it is individually rational to commit a crime if illegal income opportunities outreach the legal ones. Hence, legislation should result in severe punishments increasing the expected costs of crime and thus general deterrence. However, once an individual has been caught offending, the goal shifts to specific deterrence, meaning that the then optimal sentence minimizes the probability of the individual to reoffend. This reveals a potential dilemma. On the one hand, the optimal punishment should result in costs high enough to deter potential offenders ex ante, while on the

[1] Note: This study uses Data collected in an inmate survey in 2003/2004 as part of the research project "Cost and Benefit of incarceration and deterrence" („Kosten und Nutzen von Haft und Haftvermeidung") financially supported by VolkswagenStiftung. Originator of the project was Prof. Dr. Horst Entorf, TU Darmstadt, Chair for Applied Econometrics. Both the originator and his affiliation do not bear any responsibility for the use of the data in this study.

other hand it should not deteriorate the offender's chances to reenter the legal labor market ex post.

This ambivalence is of particular importance, if delinquents suffer from some kind of myopia - or simply do not correctly anticipate their future income opportunities - and commit crimes even though a fully rational actor would not have taken this decision. Youths seem to be especially prone to this kind of behavior. The literature on personal development found that they suffer from a maturity gap (Moffitt 1993) which contemporarily increases their inclination towards criminal activity (e.g. Heinz 2004, Thornberry et al. 2004). This leads to the belief that juveniles are more rehabilitatable and less culpable then adults (Mears et al. 2007). As a consequence, in the case of young offenders the general deterrence effect of harsh sentences is limited while the effect on reintegration into the legal job market gains relative importance.

In many countries, this line of thought induced a special treatment of juvenile offenders.[2] However, in the last decades, an increasing number of serious and highly aggressive acts of violence committed by minors questioned this policy (see Aebi 2004, Oberwittler and Höfer 2005). In the 1980s, there has been decreasing public support for a preferential treatment of juvenile offenders in the US which resulted in tougher laws transferring more juvenile offenders to a criminal court (Moon et al. 2000). In Germany, the recent and ongoing coverage of violent crimes in the media resulted in a strong pressure on politics (Bundestag 2009) and leading criminologists (Heinz 2008) to touch upon the question of how to deal with juvenile and adolescent offenders.

At first glance, German survey data seems to suggest an increased rate of recidivism of those sentenced under juvenile law. Jehle et al. (2003) analyzed the official register survey data on recidivism for the years 1994 to 1998. The recidivism rate within four years after unconditional prison sentence under juvenile law was 79.0%, whereas it was 43.6% for those under criminal law. Does that mean that juvenile law has failed in Germany? Of course, descriptive statistics do not allow for causal interpretation and inference. There is no information on the individual propensity to recidivate before treatment assignment takes place. Hence, it might not be the legal sanction that makes them recidivate, but personal characteristics. In addition, it is possible that less severe but more frequent crimes might be penalized under juvenile law, while more severe but less frequent crimes are sentenced according to criminal law. Based on this possibility, (Heinz 2004) raises doubts whether the observed higher recidivism rate following juvenile law sentences is due to sanction. However, there is little empirical evidence in favor or against these doubts.

Our paper sheds new light on the impact of juvenile legislation on recidivism making several contributions to the existing literature. In contrast to large parts of the empirical literature, we base our research on German data providing one of the first micro-level studies on the drivers of juvenile recidivism in Germany. Further, we apply modern econometric techniques to identify the effect of being sentenced as an adult on juvenile recidivism. These methods are necessary, since treatment assignment is likely to lead to a selection bias. We hypothesize that there are unobservable factors influencing both treatment assignment and outcome variable and confirm this hypothesis in a bivariate probit approach which models the selection process. Our identification strategy is twofold. On the one hand, we perform a simultaneous maximum likelihood estimation of selection and treatment equations. Next, we assume randomly distibuted

[2]The Illinois Juvenile Court Act of 1899 marks the beginning of an organized juvenile court system in the USA (Bishop and Decker 2006, p. 17). In Germany, courts started developing special court chambers dealing with young delinquents in 1908 while the Juvenile Justice Act (JJA – Jugendgerichtsgesetz) was passed in 1923 (Dünkel 2006, p. 226).

individuals around the points when law assignment has a discontinuity and apply a regression discontinuity design. Our analysis shows that adolescents sentenced as adults have a lower self-reported probability of recidivism. This result is obtained in both identification strategies and persists in several robustness checks.

These findings offer new insights, especially since prison conditions and legislation in Germany are substantially different as compared to the Anglo-Saxon world - questioning the external validity of US findings. Exceeding a national interest, our results also have implications for juvenile legislation across Europe, since the Committee of Ministers of the Council of Europe is trying to establish European standards of juvenile law explicitly mentioning the German rules as a good example (see memorandum to recommendation Rec(2003)20). The exemplary character of German juvenile legislation is based on both its flexible mechanism and the general state based legal framework which resembles the legal structure of the European Union (Bochmann 2009, p. 122).

The remainder of the paper is organized as follows. Section 2 summarizes the related literature. Section 3 describes the database and provides summary statistics of the sample. Section 4 provides the empirical specification. Sections 5 and 6 describe the identification strategies and report the results of our two alternative approaches, namely bivariate probit and regression discontinuity. In section 7 we discuss the results and section 8 concludes.

4.2 Related Literature

4.2.1 Empirical Evidence

The empirical literature has studied the influence of juvenile law on both general and specific deterrence. We start out looking at the empirical evidence on general deterrence. The literature provides an ambiguous answer to the question whether transferring juveniles to criminal courts deter any would-be offender (see Redding (2006) for a good survey on this field). Levitt (1998) found increased general deterrence when transferring adolescents to adult courts. This would suggest rational behavior of the youths confirming the Becker hypothesis. However, other studies have found no general deterrence effect (Singer and McDowall 1988, Steiner et al. 2006) or even increased arrest rates (Jensen and Metsger 1994). In a more recent paper, Lee and McCrary (2009) found evidence that young adults did hardly respond to the harsher punishments they are faced with upon turning age 18. They argue that young offenders misjudge likelihood and severity of the imminent punishments and can thus be characterized as myopic. Summarizing, even though there is no clear answer, the more recent - and maybe more sophisticated - studies confirm the behavioral findings mentioned above questioning the rational offender hypothesis for the case of juvenile delinquents.

With respect to specific deterrence there is much clearer evidence. The majority of the studies using US data find that trying and sentencing juvenile offenders as adults increases the likelihood that they will reoffend. Fagan (1996) studied differences in recidivism rates of 15- and 16-year-old juveniles, taking advantage of the fact that in New Jersey young delinquents were sentenced by a juvenile court while in New York they were charged by a criminal court. He found significantly lower recidivism rates for those sentenced by juvenile courts suggesting that the special jurisprudence for juvenile crimes is an effective measure. Being confronted with the critique that the results might be driven by a selection bias, Kupchik et al. (2003) replicated the study including several control variables confirming the original results. In a related study, Bishop et al. (1996) analyzed recidivism in Florida where the transfer of delinquents depends on the decision of the prosecutor. They found higher recidivism rates for those delinquents transferred to criminal courts. Again, they could not rule out the existence of a selection bias distorting the results. However, in a follow-up study Lanza-Kaduce et al. (2005) still found a positive effect of transfers when using both a richer dataset and matching techniques. Further studies by Myers (2003), Podkopacz and Feld (1995) and Thornberry et al. (2004) point into the same direction.

Summarizing, the empirical evidence is mainly US based and generally supports the claim that the application of criminal law increases juvenile recidivism. However, it is questionable whether these findings are also valid for Germany due to substantial differences in the legal systems. Most of the US evidence is based on the comparison of minors being either sent to a criminal or a juvenile court. The German legal system does not allow for such a situation as summarized in the next subsection.

4.2.2 Juvenile Law in Germany

In Germany, juvenile law is mandatory to all minors, i.e. to all persons who have not turned 18 yet at the time the criminal act was committed. For adolescent delinquents, i.e. those aged between 18 and 21 years when offending, the legislator left the decision to the courts whether to apply juvenile or criminal law. In more detail, courts are asked to apply juvenile law whenever the offender acts "equal to a juvenile regarding moral and mental development at the time of the act" (§ 105 (1) Juvenile Justice Act – Jugendgerichtsgesetz). Finally, delinquents of at least 21 years have to be sentenced under criminal law. Even though US laws are very heterogeneous, there is no state that extended the maximum age of the application of juvenile law as far as in Germany. In 2006, the automatic treatment as adult started either with age 18 (37 states), age 17 (10 states) or age 16 (3 states) (see Bishop and Decker 2006, p. 13). Summarizing, German legislation allows for a much wider application of juvenile law then its US counterpart.

In order to model the process of law assignment correctly, one has to know the criteria on which the decision is based. According to (Dünkel 2002) judges think strategically when choosing whether to apply criminal or juvenile law.[3] Juvenile law allows for milder sanctions, since certain minimum sanctions that exist under criminal law (e.g. 3 years in the case of robbery) do not have to be considered. This suggests that juvenile law is applied when judges find shorter punishment to be advantageous. Given this selection process, it seems to be very likely that offenders selected for juvenile law differ systematically from those who are not, also in the expected likelihood that they recidivate.

Besides the length of the punishment, also the type of custody can potentially influence recidivism. § 92 of the German Juvenile Justice Act (Jugendgerichtsgesetz) states that juveniles and adults have to be kept in separate prisons or at least in separate departments of the same prison in order to avoid contact between adult and juvenile offenders. Following Lange (2007) the most notable difference between juvenile and criminal prisons is that criminal prisons have the primary goal of punishment, while juvenile prisons are focused on social education e.g. by

[3] The transferability of Dünkel's result might be limited since he is looking at the whole range of sentences, while we only consider incarceration.

the provision of personal custodians for the delinquents. Furthermore, according to Dölling et al. (2007), juvenile law is generally less stigmatizing as opposed to criminal law.

Entorf et al. (2008, p. 139-152) summarize differences of juvenile and criminal prisons. The authors find that juvenile prisons on average have more money at their disposal and thus can offer a more convenient and stimulating environment. E. g. juvenile prisons offer more public rooms for eating, sports and other activities. Also, a higher fraction of juvenile delinquents is placed in a single room (83%) as compared to adult delinquents (55%). While in a criminal prison there are less than 50 employees for 100 inmates, there are almost 70 employees in juvenile prisons. This allows juvenile prisons to provide schooling opportunities and to offer more seminars, e.g. on how to deal with drug and alcohol problems.

The different facilities can affect recidivism in two ways. On the one hand, being inmate in a more convenient prison can dampen the deterrence effect and lead to higher recidivism rates. On the other hand, juvenile prisons might decrease the likelihood of recidivism due to their educational concerns and their less stigmatizing effect on future job chances. Our analysis will provide an answer to the question which of the two effects dominates.

4.3 Data

Our analysis is based on a prison survey that was collected in 31 German prisons in 2003 and 2004 using a questionnaire with 123 questions. The survey has been initiated and carried out by Horst Entorf and a team of researchers from Darmstadt University of Technology. It uses a two-stage approach combining stratified and random sampling. First, a representative sample of the population of prisons in Germany was created. Second, a random draw out of this population completed the sampling.

The questionnaire was given to 13,340 selected inmates in either German, Turkish, Serbo-Croatian, Russian, Polish or English language to account for the different nationalities of the inmates (see Meyer (2007) for more details on the survey). It was completed by 1,771 respondents resulting in a response rate of 13.3%. The low response rate might raise doubts on a potential selection bias. Interviews with judicial employees suggest an overrepresentation of the more active group of inmates. However, when comparing sample characteristics to those of the average prison population in Germany, there is no evidence of a selection bias.[4]

The original dataset can be grouped into three subsamples: inmates in pretrial custody, inmates sentenced under juvenile law and inmates sentenced under criminal law. Since we are interested in the effect of the applied law type, we only use the last two subgroups and drop the inmates in pretrial custody. Further, our analysis focuses on adolescent delinquents. Hence, we also drop all individuals younger than 14 and older then 25 when committing a crime. This leaves us with a sample of 245 inmates. When estimating the treatment assignment function we further restrict the sample to adolescents, yielding a subsample of 90 observations. The descriptive statistics for both samples can be found in table 4.1.

[4]For a more detailed analysis of this issue see Entorf (2009).

Table 4.1: Summary statistics

Sample	$14 \leq$ **ageoffense** ≤ 25				$18 \leq$ **ageoffense** ≤ 21	
Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**Mean**	**Std. Dev.**
expected recidivism	0.2531	0.4357	0	1	0.3	0.4608
age	22.8796	3.2604	16.5	35.5	21.4222	1.63
ageoffense	20.5276	2.666	14.5833	25	19.4546	1.0189
female	0.102	0.3033	0	1	0.0333	0.1805
married	0.0943	0.2928	0	1	0.1	0.3017
city	0.3602	0.4811	0	1	0.4886	0.5027
poor social capital	0.4898	0.5009	0	1	0.4556	0.5008
no church	0.387	0.4881	0	1	0.4458	0.5001
addiction	0.3029	0.4605	0	1	0.2778	0.4504
social contact	0.5432	0.4992	0	1	0.5444	0.5008
crim parents	0.1345	0.3419	0	1	0.1685	0.3765
drugs	0.1959	0.3977	0	1	0.2	0.4022
drugs (deal)	0.1633	0.3704	0	1	0.1556	0.3645
drugs (consume)	0.0857	0.2805	0	1	0.0889	0.2862
abi	0.0372	0.1896	0	1	0.0333	0.1805
job	0.1966	0.3983	0	1	0.186	0.3914
job contact	0.3077	0.4625	0	1	0.3256	0.4713
theft	0.3918	0.4892	0	1	0.3778	0.4875
robbery	0.2776	0.4487	0	1	0.3333	0.474
fraud	0.1837	0.388	0	1	0.2	0.4022
bodily injury	0.3265	0.4699	0	1	0.4222	0.4967
vandal	0.0939	0.2923	0	1	0.1444	0.3535
sexual	0.049	0.2163	0	1	0.0333	0.1805
murder	0.1184	0.3237	0	1	0.1111	0.316
sentence length	3.5192	3.1234	0.0833	15	2.9963	2.0739
percseated	0.3731	0.2399	0	1	0.3377	0.2232
open	0.1639	0.371	0	1	0.1111	0.316
criminal law	0.4939	0.501	0	1	0.1333	0.3418
Nobs	245	245	245	245	90	90

4.3.1 Expected Recidivism

Our target variable is a self-reported measure for expected recidivism. It is constructed from the response to the following survey question:

"Could it occur that after your release from custody you come into conflict with the law and end up in prison?"

Inmates were asked to answer this question on a 5 point scale, whereat a 1 stands for "no, never" and 5 corresponds to "absolutely certain". For reasons of small sample size, we translate the answers to this question into a binary variable *recidivism*. In the data, the answers are positively skewed: 43.5% of the respondents answered with the lower extreme “no, never” while only 4% said they were absolutely certain to reoffend. Therefore, we set recidivism to zero if the respondent chose either answer 1 or 2, and set the binary variable to one for those with a higher self-reported probability of ending up in prison again (answers 3-5).[5]

One might raise objections against using self-reported recidivism as a proxy for real recidivism. There are at least three arguments in favor of our approach. First, there is evidence that self-reported and real recidivism are correlated (Corrado et al. 2003). Second, using expected recidivism, as compared to actual recidivism, avoids the problem of a selection bias when conducting a follow-up survey to collect actual recidivism. Third, we do not face the problem of a potential omitted variable bias due to additional factors that influence actual recidivism after the release from prison and that can hardly be controlled for.

Nevertheless, it might be enlightening to discuss possible biases. A general bias, affecting all indivuals in the same way and resulting in a generally too high (or too low) rate of recidivism would not pose a threat to the validity of the use of this variable. We are not interested in getting an unbiased measure of recidivism, but rather want to find out factors that change its level. Hence, our results only loose validity, if individuals in the treatment group have a different bias than the control group. To generate such an effect, the applied law type must change the precision of the self-reported measures. E.g. those in adult prisons might have a more precise estimate of their future while those in juvenile prisons systematically over- or underestimate their propensity to recidivate. Even though such effects are not likely to drive the results, we take this possibility into account when discussing our findings.

4.3.2 Age at offense

As shown in section 4.2.2, the age when the crime was committed (*ageoffense*) is the crucial variable for the assigned type of law. Since this information did not appear in the survey directly, we constructed it using time and age when surveyed and the time when the crime was committed (both given at a monthly precision level). With regard to the latter, inmates could choose to indicate either a point in time or an interval. For a given point in time the calculation is straightforward. When dealing with an interval, however, it is not clear which date determines the applied type of law. In the case of joint trials - in other words, when dealing with several offenses at a time - German juvenile law asks judges to apply either juvenile or criminal law only, depending on the age at the “main offenses” (§ 32 Juvenile Justice Act – Jugendgerichtsgesetz).

[5] This strategy has been suggested and used by Entorf (2009). We also tried different ways of bundling the original multinomial variable which did not change the results.

Since the data contains only the dates of the offenses and not their severity, a natural proxy would be the mean of the interval. However, the judge might lack information on the start of the criminal activity and thus base his decision on the last known crime(s). In addition, it can be assumed that more recent offenses are perceived to be more important which also would result in a bias towards the end of the interval. Based on these assumptions, we construct our main variable *ageoffense* using the end of the interval representing the most accurate proxy for the real age at offense we can get.[6] We should keep in mind, however, that our calculation method might involve an upward bias, letting some individuals appear older than they are.

In addition, we have to deal with different precision levels of the relevant points in time. Age when surveyed is reported in years, which gives rise to a possible error of nearly 12 months. In order to minimize this mistake we added 6 months to the calculated age at offense.[7] The missing precision of this variable might threaten the regression discontinuity analysis, since *ageoffense* is the variable that is crucial for the applied type of law. In order to counteract this potential imprecision, we searched for contradicting observations, i.e. inmates where *ageoffense* and the applied law type did not match the treatment assignment mechanism. Since we only found (and dropped) 2 contradicting observations, we have reasons to believe that the constructed variable *ageoffense* is quite accurate.

4.3.3 Additional Regressors

Throughout the study we use several control variables. First, we include personal characteristics of the inmate, such as *age* (at the time of the interview) and gender (*female*). Consistent with national criminal survey statistics, there is a strong majority (89.8%) of male inmates in our sample. We also control for the size of the inmate's home town (*city*) and marital status (*married*). In our sample, only 9.4% are married, which can be explained by the fact that we are only looking at inmates aged 14 to 25 when committing the crime. A variable that might replace the marriage property for young individuals is frequent contact to a partner in the month before incarceration (*social contact*), which has been affirmed by approximately every second inmate in the sample (54.3%). Further, we measure participation in social clubs, e. g. sports clubs or the voluntary fire brigade, mapping the lack of active participation into the dummy variable *poor social capital*. Almost half of the inmates in the sample (49.0%) reported no active participation in social clubs. Religion might also affect expected recidivism by its influence on future beliefs and intrinsic motivation. The corresponding dummy variable *no church* is set to one for all inmates that do not belong to any religion which holds true for 39.7% in our sample. Further, we control for alcohol or drug addiction using the binary variable *addiction*. In the sample, 30.3% of the inmates suffer from one of these addictions. This might be linked to recidivism both in a direct way – in the sense that addicted people might more easily commit crimes under the influence of drugs – and in an indirect way in the sense that these people might see criminal behavior as a way to finance their addiction (see Entorf and Winker 2008, Goldstein 1985, Harrison 1992). Criminal family background is another ingredient that could matter for expected recidivism: the dummy variable *crim parents* captures past convictions of parents or siblings and applies for 13.5% of the inmates in our

[6] We also performed the same analysis with different specifications of ageoffense and the results did not change.

[7] Assuming a uniform distribution of the variable, the transformation allows for a reduction of the average mistake from 0.5 to 0.25.

sample.

Another interesting aspect are controls that might affect the opportunities in the legal job market. We include the binary variable *abi* set to one for all delinquents with a German high school diploma equivalent which is the case for 3.7% in the sample. It is included based on the expectation that education is a positive asset in the legal labor market but of limited value for criminal activities, thereby reducing the probability to reoffend. Further, we include self-reported job market opportunities.[8] *Job* is a binary variable containing the information whether inmates reported to have a job opportunity when leaving jail. This the case for 19.5% of the relevant inmates. If the inmates reported to have at least contacted a future employer the variable *job contact* is set to one which holds true for 30.8% of the subjects in the sample.

We also have information on the type of offense that led to the present incarceration. It is likely, that different types of crime are connected with different probabilities of recidivism. E. g. for organized and drug related crimes there might be higher chances of relapse due to physical addiction and the influence of the social network. Observe that inmates were allowed to report more than one type of crime which means that the crime frequencies will not add up to one. In our sample, the most frequently reported crime is *theft* (39.2%), followed by *bodily injury* (32.7%) and *robbery* (27.8%). With regard to drug related crimes, we distinguish between drug dealing (*drugs (deal)*) and consumption (*drugs (consume)*). If at least one of the two applies, the binary variable *drugs* is set to one.

In addition, we include length and type of the sentence the inmate is currently serving. In terms of applied legislation, 49.4% of the delinquents were sanctioned under criminal law captured in the binary variable *criminal law*. Further, we know which prison the delinquent has been assigned to (see table 4.2). We also observe the individual *sentence length* measured in years. In line with German legislation, we put lifelong punishments as a 15 year sentence, which is also the maximum length for our sample. We can also infer the period of time the inmate already spent in prison, yielding the variable *percserved* which measures the share of the sentence length that has already been served. On average, this share is 37.3% which might hint to the participation of more recently incarcerated individuals. However, it is even more likely that it just reflects the fact that the German Penal Code (§§ 57, 57a, 57b Strafgesetzbuch) allows for early release when the inmate has served two thirds of his sentence length.

Table 4.2: Prisons of inmates in sample

JVA #	Location	Tried as Adults (Adolescents)	Tried as Adults (Total Sample)
1	Adelsheim	0.0%	0.0%
4	Bayreuth	0.0%	60.0%
8	Bützow	57.1%	80.0%
15	Flensburg	100.0%	100.0%
19	Heilbronn	0.0%	66.7%
20	JSA Berlin	0.0%	0.0%
23	JSA Rockenberg	0.0%	0.0%
27	Lübeck	100.0%	100.0%
38	Schwäbisch Gmünd	60.0%	87.0%
46	Würzburg	100.0%	100.0%
	Total	13.3%	49.4%

[8]See Entorf (2009) for a discussion of the influences of school and job market on criminal behavior and recidivism.

Based on the assignment probability of criminal law we split the sample into three groups. The first group consists of offenders who can only be sentenced as juveniles (table 4.12 in the appendix, 46 observations). The second group contains all those offenders falling into the discretionary range where the type of law is determined by the courts (table 4.13 in the appendix, 90 observations). Finally, there is a third group in our sample that will always be sentenced under criminal law (table 4.14 in the appendix, 109 observations).

4.4 Empirical Specification

The goal of this study is to analyze the effect of being sentenced under criminal law (as opposed to juvenile law) on adolescent offenders' recidivism. Considering criminal law to be a treatment that influences recidivism, this translates into the identification of the corresponding treatment effect. Defining ER_i as a measure of expected recidivism and $T_i \in \{0,1\}$ as the treatment indicator of individual i, we can write

$$ER_i = (1-T_i)ER_i^0(X_i) + T_i ER_i^1(X_i) = \begin{cases} ER_i^0(X_i) & if \quad T_i = 0 \\ ER_i^1(X_i) & if \quad T_i = 1 \end{cases} \tag{4.1}$$

where $ER_i^0(X_i)$ is expected recidivism when juvenile law has been applied, while $ER_i^1(X_i)$ is expected recidivism when criminal law has been applied. Both expressions are a function of a list of variables X_i. While the influence of a continuous variable is usually measured in its marginal effect, the corresponding expressions for a binary variable like the treatment indicator are different conditional means (see e.g. Heckman and Navarro-Lozano 2004). The most intuitive measure is the average treatment effect (ATE) which simply is the expected difference in the outcome variable conditional on the covariates. Based on the setup in (4.1) and dropping the observation indices (i), this effect is defined by

$$\text{ATE} = E[ER^1 - ER^0|X]. \tag{4.2}$$

A related concept is the average treatment effect on the treated (ATET) which in our setup is defined by

$$\text{ATET} = E[ER^1 - ER^0|X, T = 1]. \tag{4.3}$$

Note that both effects describe a counter-factual outcome and would require the observation of the same individual in both situations, once getting the treatment and once not getting it. Since the two situations are mutually exclusive, every individual is observed only once. Hence, observational data only allow to contrast the mean group outcomes conditional on covariates and treatment status.

$$\Delta_T = E[ER^1|X, T = 1] - E[ER^0|X, T = 0] \tag{4.4}$$

If treatment assignment is random and the sample is large enough, individuals in both groups have identical characteristics and $E[ER^j|T = 1] = E[ER^j|T = 0] = ER^j$ for $j \in (0,1)$. In this case, the three measures (4.2)-(4.4), coincide and can be identified by a simple treatment dummy whose estimate is the sample equivalent of Δ_T. However, if treatment assignment is not perfectly random the three measures can have different values. It is possible to rewrite (4.4) and decompose Δ_T into a sum of the ATET and a selection bias.

$$\Delta_T = \underbrace{E[ER^1 - ER^0|X, T=1]}_{\text{ATET}} + \underbrace{E[ER^0|X, T=1] - E[ER^0|X, T=0]}_{\text{Selection bias}} \tag{4.5}$$

In our context, the selection bias in (4.5) is different from zero if the individuals sentenced as adults have a different general propensity to recidivate then those sentenced as juveniles. If in addition the untreated offenders would respond differently to the treatment also ATET and ATE diverge which we call a reaction bias.

$$\text{ATET} = \text{ATE} + \underbrace{E[ER^1 - ER^0|X, T=1] - E[ER^1 - ER^0|X]}_{\text{Reaction bias}} \tag{4.6}$$

Summarizing, whenever law assignment is determined at least in parts by the value of a (potentially unobserved) variable which is correlated with expected recidivism, the sample analogue of Δ_T cannot identify a treatment effect. Hence, we have to model the selection process. First, law assignment obviously also depends on the age at offense which becomes clear when explicitly modeling the global treatment assignment function (GT_i) based on the German legal framework:

$$GT_i(ageoffense, W_i) = \begin{cases} 0 & if \quad ageoffense < 18 \\ T_i(W_i) & if \quad 18 \leq ageoffense < 21 \\ 1 & if \quad ageoffense > 21 \end{cases} \tag{4.7}$$

When restricting the sample to adolescents, cases with predetermined treatment assignment based on age at offense disappear. In this case, treatment assignment depends on a further set of variables (W). As described in section 4.2.2, German juvenile law ask judges to apply a maturity criterion in the selection process. Since maturity of the offender might also affect the likelihood of recidivism we have to assume a selection bias based on unobservable characteristics driving both the court's treatment selection and the outcome variable.

Hence, we reject any model that simply includes a treatment dummy as a regressor. Rather, we suggest two appropriate alternative approaches that allow to identify the causal effect of treatment. First, we define a bivariate probit model which allows us to explicitly control for treatment assignment and the emerging biases. Second, we apply a regression discontinuity framework which relies on jumps in the treatment assignment function to locally reestablish the random assignment property.

4.5 Bivariate Probit Approach

Heckman (1978) proposed a general class of simultaneous equation models with endogenous variables to control for a selection bias. However, since our target variable (*recidivism*) is binary,[9] and thus cannot lie outside the unit interval, the OLS based estimator on the second stage will suffer from truncation bias (see e.g. Greene and Hensher 2010, p. 106). This calls

[9]To use the original multinomial target variable for recidivism we would have to either assume identical differences between the categories and use OLS or use a multinomial ordered choice model. While the first assumption seems too strong, the weakness of a multinomial model is that it requires the estimation of four cut-points in addition to the target variable. This will hamper the interpretation of the model coefficients and reduce efficiency in a small sample which made us stick to the probit model. As a robustness check we nevertheless estimated the equation using an Ordered Probit model which did not yield any substantially different results.

for the use of a binary choice model also on the second stage. We use a probit model which goes back to Bliss (1934) who suggested to base the binary outcome on a latent function with a normally distributed error term.[10] Maddala (1983) was one of the first to extend Heckman's idea to a bivariate probit setting. The canonical bivariate probit model consists of two probit equations with jointly normally distributed error terms (see e.g. Greene and Hensher 2010, p. 83). In our case, the structural probit equation contains expected recidivism as a function of regressors X_i and the potentially endogenous dummy for treatment assignment

$$ER_i^{j*} = X_i'\beta + T_i\delta + \varepsilon_i \qquad \text{and} \qquad ER_i^j = \begin{cases} 1 & if \quad ER_i^{j*} > 0 \\ 0 & \qquad otherwise \end{cases}, \tag{4.8}$$

where $j \in (0,1)$ and the latent variable is denoted with a star ("*"). The second probit (reduced form) equation models (local) treatment assignment as a function of another set of covariates (W_i').

$$T_i^* = W_i'\gamma + \eta_i \qquad \text{and} \qquad T_i = \begin{cases} 1 & if \quad T_i^* > 0 \\ 0 & \qquad otherwise \end{cases} \tag{4.9}$$

However, it is necessary to impose an identifying restriction. In our context, this can be the assumption of an exclusion restriction, meaning that there must be at least one variable in W that is not included in X. We use *ageoffense* for our exclusion restriction, since it should have no direct effect on recidivism. This age measure is only relevant for treatment assignment, while what matters for recidivism is the actual age only.

In line with the standard bivariate model, we assume that the error terms of both processes, (4.8) and (4.9), share the following joint normal distribution

$$\begin{bmatrix} \varepsilon_i \\ \eta_i \end{bmatrix} \sim N\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix}\right) \tag{4.10}$$

where ρ captures their correlation. The joint density of the two error terms then equals

$$\phi(\varepsilon_i, \eta_i) = \frac{1}{2\pi\sqrt{1-\rho^2}} \exp\left[-\frac{1}{2}\left(\frac{\varepsilon_i^2 + \eta_i^2 - 2\rho\varepsilon_i\eta_i}{1-\rho^2}\right)\right]. \tag{4.11}$$

Now, the conditional expectations in the expression for the selection bias in (4.5) can be rewritten as

$$\begin{aligned} E[ER_i^0|X_i, T_i = 1] &= \Pr(ER_i^0 = 1|X_i, T_i = 1) \\ &= \Pr\left(\varepsilon_i > -X_i'\beta|X_i, \eta_i > -W_i'\gamma\right) \end{aligned} \tag{4.12}$$

and

$$\begin{aligned} E[ER_i^0|X_i, T_i = 0] &= \Pr(ER_i^0 = 1|X_i, T_i = 0) \\ &= \Pr\left(\varepsilon_i > -X_i'\beta|X_i, \eta_i \leq -W_i'\gamma\right) \end{aligned} \tag{4.13}$$

[10] The latent function is mapped into the binary choice using a normal distribution. A second popular approach is the assumption of a logistic distribution function. However, the analysis of a bivariate logit model is fairly inconvenient (see e.g. Imai et al. 2007).

where the difference of the two defines the selection bias. These equations reveal that correlation in the error terms, i.e. when ρ is not zero, posing a threat to the validity to a single equation model. In the literature, there are many examples where simple comparisons or even regression-adjusted comparisons provided misleading estimates of causal effects (see e.g. Ashenfelter and Card 1985). As Angrist and Pischke (2009, p. 243) point out, this may reflect some sort of omitted variables bias, that is, a bias arising from unobserved and uncontrolled differences between the two groups. In the present identification problem it is quite likely that the judge's decision hinges on information not contained in the data that will also determine recidivism. Hence, we should also expect a selection bias due to unobservables.

A solution to this problem is a simultaneous Maximum Likelihood estimator for both equations. An expression for the Log-Likelihood function can be found e.g. in Maddala (1983, p. 123). Observe that the Maximum Likelihood estimation will not be biased due to the presence of the endogenous parameter in the first equation. As Greene and Hensher (2010, p. 75) point out: "We can ignore the simultaneity in this model and we cannot in the linear regression model because, in this instance, we are maximizing the log-likelihood, whereas in the linear regression case, we are manipulating certain sample moments that do not converge to the necessary population parameters in the presence of simultaneity." Hence, it is possible to estimate ATET and ATE directly.

In the following subsections, we present the results from both single equation and simultaneous estimations of the two binary choice structures. This allows us to analyze the model in more depth since we can compare the estimates between specifications and can identify the size of the selection bias.

4.5.1 Drivers of expected recidivism

First, we perform a single equation estimation of equation (4.8), which is a probit regression of the target variable on a set of covariates. We used different specifications of the probit model. The estimates can be found in table 4.3, where column 1 contains a basic model with few regressors while columns 2 and 4 represent extended models. Marginal effects at the means are reported in columns 3 and 5. We will base our interpretations on the model with the highest explanatory power, which is the one in columns 4 and 5.

Most importantly, we find that *age* has a significant (negative) influence on expected recidivism confirming our initial assumption. The best model for *age* is a quadratic expression, resulting in a monotonously decreasing and convex function. The nonlinear curve thus captures a general negative trend and a decreasing marginal change which is both in line with the literature. Starting from the average age of 22.42 one additional year decreases the probability to recidivate by 2.31%.[11]

In addition, we find that the propensity to recidivate decreases by 15.1% when the inmate has a job offer or at least job contacts (*job contact*). The negative influence of job opportunity on recidivism is in line with the literature which finds broad evidence that worse general job market conditions increase crime rates (Fougère et al. 2009, Lin 2008, Machin and Meghir 2004, Raphael and Winter-Ebmer 2001). Including variables for both dealing and consuming drugs we find

[11] The marginal effect in this case needs additional computation and corresponds to $\frac{\partial \Phi(\overline{X}\widehat{\beta}+\overline{T}\widehat{\delta})}{\partial x_{age}} = \phi(\overline{X}\widehat{\beta} + \overline{T}\widehat{\delta}) * \left(\widehat{\beta_{age}} + \overline{x_{age}}\widehat{\beta_{age2}}\right)$ where overlines represent sample means and hats denote estimated values (see e.g. Greene and Hensher 2010).

Table 4.3: Drivers of expected recidivism

	(1) recidivism	(2) recidivism	(3) marginal	(4) recidivism	(5) marginal
age	-4.541***	-4.885***	-1.696***	-4.902***	-1.582**
	(0.000)	(0.000)	(0.000)	(0.001)	(0.011)
age2	0.103***	0.113***	0.0391***	0.113***	0.0363**
	(0.000)	(0.000)	(0.000)	(0.001)	(0.016)
job contact	-0.539***	-0.611***	-0.211***	-0.468***	-0.151***
	(0.001)	(0.000)	(0.000)	(0.003)	(0.009)
social contact	0.0398	0.127	0.0437		
	(0.901)	(0.673)	(0.661)		
crim_parents	0.502***	0.526***	0.195***	0.320***	0.110***
	(0.000)	(0.000)	(0.000)	(0.003)	(0.001)
open	0.612**	0.691**	0.262**	0.613**	0.221**
	(0.022)	(0.021)	(0.026)	(0.028)	(0.040)
theft	-0.228	-0.230	-0.0785	-0.385**	-0.119**
	(0.246)	(0.218)	(0.196)	(0.040)	(0.033)
addiction	0.227*	0.186	0.0658		
	(0.076)	(0.327)	(0.353)		
drugs (deal)		0.00917	0.00319	0.367***	0.126**
		(0.922)	(0.922)	(0.004)	(0.010)
drugs (consume)		0.365**	0.135**	0.304	0.105
		(0.015)	(0.012)	(0.196)	(0.202)
robbery				0.204	0.0672
				(0.176)	(0.132)
city				-0.386	-0.123
				(0.208)	(0.137)
poor social capital				0.503	0.164
				(0.115)	(0.167)
prison experience				0.333*	0.106*
				(0.082)	(0.098)
criminal law		-0.608	-0.180	-0.428	-0.122
		(0.400)	(0.309)	(0.613)	(0.572)
Constant	49.14***	52.33***		52.37***	
	(0.000)	(0.000)		(0.000)	
Observations	82	82	82	76	76
Pseudo R^2	0.111	0.127	0.127	0.147	0.147

p-values in parentheses, marginal effects calculated at means
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

that it is drug dealing (*drugs (deal)*) that increases the probability of the inmate to reoffend by 12.6%.

As a robustness check, we also estimated the full young offender sample (see table 4.15 in the appendix). The sample restriction seems to affect some coefficients while others do hardly change. The influence of *age* for example is still significant and negative. Also the marginal effect of on average -1.8% is similar and reflects the inclusion of the less sensitive older group of offenders. Also, job contact seems to have a similar effect reducing the propensity to recidivate by 9.6%. The influence of a criminal background of the parents is similar (+8%) but not significant. We find contradicting results for *thefts* and inmates in *open* prisons. While we find adolescent thieves to be less likely to recidivate (-11.9%) and open prison to simulate recidivism for this age category (+22.1%), we get the opposite results in the extended sample where individuals are on average older (see table 4.1). Given the small sample size these results might be artifacts. However, it could also mean that open prisons are less successful for the group of adolescents.

4.5.2 Drivers of treatment assignment

Next, we estimate equation (4.9) as a single equation probit regression of the (local) treatment assignment function on a set of covariates. The results can be found in table 4.4, where column 1 contains the coefficient estimates for a basic model and columns 2 and 4 contain the estimates for extended models. In columns 3 and 5 the average marginal effects are reported. Again, we will base our interpretations on the last two columns which represent the model with the best fit.

We include two groups of covariates that might explain treatment assignment. On the one hand we control for types of crime assuming that the treatment decision could be influenced by the crime specific ranges of punishments under the two different types of law. On the other hand, we also include offender characteristics which might proxy maturity. An obvious proxy for maturity is *ageoffense*, since it separates the two treatments and might be used as a moderator between the two regimes. In addition, we found criminal background of the parents (*crim parents*), *poor social capital*, high school graduation (*abi*) and being *female* to be positively correlated and good job market perspectives (*job*) and living in a *city* to be negatively correlated with treatment assignment.[12] Other factors that are known to influence recidivism, like drug consumption, have no influence on the applied law type. The assumed influence of crime types could not be confirmed - most crime dummies were dropped due to insignificance.

Our findings shed some interesting light on the decision schemes of judges in Germany and at the same time raise the question whether this assignment rule is optimal. The observed higher probability of criminal law for high school graduates could deserve further investigation. It suggests that individuals with a higher education are perceived to be more mature and thus more culpable when compared to other offenders of the same age. However, Heinke (2009) finds that the level of understanding the consequences of crime is not influenced by school type. Hence, a preferred treatment of delinquents without high school graduation might not be justified.

[12] Note that the inclusion of *job* as a regressor might be problematic, if its value in fact was caused by treatment assignment instead of causing it. We nevertheless include it based on the assumption that it serves as a proxy for general labor market connectiveness that were already present when deciding upon treatment.

Table 4.4: Treatment assignment

	(1) crim_law	(2) crim_law	(3) marginal	(4) crim_law	(5) marginal
ageoffense	1.078***	1.308***	0.029***	1.412***	0.030***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
female	1.807*	2.525**	0.059**	2.227	0.049**
	(0.068)	(0.048)	(0.031)	(0.070)	(0.035)
job	-1.739***	-3.066***	-0.074***	-3.175***	-0.076***
	(0.000)	(0.000)	(0.000)	(0.020)	(0.000)
social contact	0.272	0.825	0.018		
	(0.635)	(0.932)	(0.281)		
abi	1.880***	3.446***	0.079***	3.131***	0.068***
	(0.007)	(0.001)	(0.000)	(0.000)	(0.000)
city		-1.942***	-0,045	-1.302***	-0.028***
		(0.000)	(0.000)	(0.003)	(0.009)
drugs (consume)		-0.307	-0.006		
		(0.755)	(0.709)		
poor social capital		1.317	0.030**	1.106**	0.025***
		(0.122)	(0.023)	(0.016)	(0.000)
addiction				-0.941	-0.020
				(0.347)	(0.122)
crim_parents				0.4739	0.010*
				(0.3470)	(0.090)
Constant	-22.84***	-27.96***		-27.61***	
	(0.000)	(0.000)		(0.000)	
Observations	86	85	85	79	79
Pseudo R^2	0.384	0.5596	0.5596	0.5690	0.5690

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.5.3 Simultaneous Estimation

Summarizing the results from the single equations we see that the existence of a selection bias is very likely. We found *age* to be a driver of recidivism and *ageoffense* to determine treatment selection. Even though these variable are not identical, they are highly correlated (Spearman rank correlation test: $r_{spear} = 0.92$, $p < 0.001$). Hence, we have to reject the single equation models and estimate both equations simultaneously, allowing for correlation in the error terms.

The results from the joint estimation can be found in tables 4.5 and 4.6. In column 1, we test a very simple model and find a negative impact of criminal law on recidivism. However, the result is not significant (p-value around 13%). Then we include the most significant covariates from the single equation estimations in column 2 and confirm the influence of criminal law. As a robustness check, we try further model specifications in columns 3 to 5.

The influence of *criminal law* does not change a lot across the different model specifications. The estimated coefficients lie in each other's confidence interval yielding a very robust finding. The coefficients of the remaining covariates are mainly in line with literature, intuition and the results from the single equation models which gives further support for the estimated models (see previous sections). The estimate for the correlation between the two equations (*athrho*) is significant in columns 3 and 4 and has a p-value smaller 20% in the other two specifications. Given that the estimate of the correlatoin between the error terms ($\widehat{\rho}$) is always positive and roughly of similar size, also this parameter is quite robust.

Table 4.5: Biprobit Equation 1: Drivers of expected recidivism

	(1)	(2)	(3)	(4)
	recidivism	recidivism	recidivism	recidivism
age	-2.757**	-3.820***	-4.304***	-4.030***
	(0.017)	(0.001)	(0.000)	(0.000)
age2	0.0643**	0.0882***	0.0991***	0.0933***
	(0.022)	(0.002)	(0.000)	(0.000)
criminal law	**-1.183**	**-1.247****	**-1.157***	**-1.370*****
	(0.131)	**(0.025)**	**(0.083)**	**(0.008)**
drugs (deal)		0.397***	0.444***	0.315**
		(0.002)	(0.000)	(0.036)
job contact		-0.433***	-0.279***	-0.511***
		(0.000)	(0.007)	(0.001)
poor social capital		0.473*	0.508	0.447
		(0.067)	(0.114)	(0.125)
robbery			0.0838	
			(0.605)	
fraud			0.156	
			(0.683)	
open			0.578**	0.625**
			(0.033)	(0.049)
theft			-0.472**	-0.279
			(0.039)	(0.125)
prison experience			0.268	
			(0.185)	
city				-0.369
				(0.240)
crim_parents				0.472***
				(0.002)
Constant	29.02**	40.70***	45.71***	42.87***
	(0.016)	(0.001)	(0.000)	(0.000)
Observations	90	86	80	85

p-values in parentheses

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

Table 4.6: Biprobit Equation 2: Treatment Assignment

	(1) recidivism	(2) recidivism	(3) recidivism	(4) recidivism
ageoffense	0.921***	1.238***	1.025***	1.088***
	(0.000)	(0.000)	(0.000)	(0.000)
poor social capital		1.075***		0.899**
		(0.007)		(0.013)
robbery		-7.574***	-6.789***	-7.043***
		(0.000)	(0.000)	(0.000)
fraud		-0.560	-0.861	
		(0.299)	(0.203)	
female			2.337***	
			(0.000)	
abi			0.508	
			(0.141)	
vandal				-6.176***
				(0.000)
city				-0.848
				(0.203)
Constant	-19.48***	-26.04***	-21.29***	-22.95***
	(0.000)	(0.000)	(0.000)	(0.000)
athrho	0.418	0.609	0.936**	0.727*
	(0.154)	(0.160)	(0.015)	(0.078)
Observations	90	86	80	85

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

To facilitate interpretation and comparison between the subsequent regression discontinuity design, we also report the average treatment effects. Following Christofides et al. (1997) and Greene (1998), the conditional means of a dummy variable are identical to the univariate probit case and can be computed as defined in (4.14) and (4.15). Hence, the average treatment effect can be computed as the average value of the individual changes in the likelihood to recidivate, induced by the treatment:

$$\begin{aligned} \text{ATE} &= \Pr(ER^1 = 1|X) - \Pr(ER^0 = 1|X). \\ \widehat{\text{ATE}} &= \tfrac{1}{N}\textstyle\sum_{i=1}^{N} \Phi(X_i\widehat{\beta} + (1-T_i)\widehat{\delta}) - \Phi(X_i\widehat{\beta} - T_i\widehat{\delta}) \end{aligned} \tag{4.14}$$

where ϕ is the standard normal density and $\widehat{\delta}$ is the estimated coefficient of *criminal law* treatment and T_i is a dummy for treatment assignment. Further, the average treatment affect on the treated is

$$\begin{aligned} \text{ATET} &= \Pr(ER^1 = 1|X, T=1) - \Pr(ER^0 = 1|X, T=1). \\ \widehat{\text{ATET}} &= \tfrac{1}{N_T}\textstyle\sum_{i=1}^{N_T} \Phi\left(\frac{X_i\widehat{\beta}+(1-T_i)\widehat{\delta}-\widehat{\rho}W_i\widehat{\gamma}}{\sqrt{1-\widehat{\rho}^2}}\right) - \Phi\left(\frac{X_i\widehat{\beta}+T_i\widehat{\delta}-\widehat{\rho}W_i\widehat{\gamma}}{\sqrt{1-\widehat{\rho}^2}}\right) \end{aligned} \tag{4.15}$$

where the ATET includes all observations that received the treatment (N_T). The estimated values give a clear result. The estimated average treatment effect is highly significant and robust across model specifications while the estimated average effect on the treatment has more variation but is still negative and significant (see table 4.7).

Table 4.7: Average Treatment Effects Bivariate Probit

	(1)	(2)	(3)	(4)
ATE	-0.290***	-0.299***	-0.286***	-0.316***
	(0.048)	(0.052)	(0.046)	(0.060)
ATET	-0.394***	-0.482***	-0.674**	-0.597***
	(0.062)	(0.076)	(0.235)	(0.087)

standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.6 Regression Discontinuity Design

In a second step, we check whether the result from the bivariate probit estimations can be confirmed in a Regression Discontinuity (RD) approach. RD is appropriate whenever treatment assignment is determined at least in parts by the value of an observed variable which also influences the outcome variable and thus a simple differences approach cannot identify the treatment effect (see Van der Klaauw (2008) for a formal derivation). Introduced by Thistlethwaite and Campbell (1960), two psychologists, this approach did not draw too much of the attention in the economic literature until the late 1990s. However, today, there is a growing body of literature on RD applications which was initiated by Angrist and Lavy (1999) and Black (1999) amongst others. In a recent survey article, Lee and Lemieux (2010) summarize this emerging strand of the empirical literature.

Whereas in experiments with randomized assignment, experimenters can take care of comparability between treatment group and control group, assignment in our context has been shown to be nonrandom and persons receiving treatment differ systematically from those who do not. The use of RD, however, avoids the problem of a selection bias, since it takes advantage of a discontinuity in treatment assignment, at a so-called cut-off point (c). Therefore, an RD analysis requires some knowledge on the treatment assignment rule including a hint to the existence of a cut-off point. The global treatment assignment function (4.7) suggests two potential discontinuities: at 18 and 21 years of age at offense.

In an RD approach, only individuals just below and just above the cut-off point are used - e.g. we compare individuals who are 21 or a little older to their peers a little younger than 21. Instead of contrasting conditional means based on treatment status, here we will contrast means based on a dummy variable that captures whether the individual has passed the cut-off point or not. This allows an identification of the treatment effect because in the limit we are comparing the same individuals (since there is only small variation in age at offense) while the treatment assignment probability will be different on both sides of the cut-off. An offender with less then 21 years of age when offending will be sentenced as adult only if the judge chooses to do so, while upon turning 21 the judge has no choice and will apply criminal law for sure. A similar logic applies at 18.

Following Imbens and Lemieux (2008) we estimate the average treatment effect τ by

$$\begin{aligned} \widehat{\tau} \quad = E\left[\beta|(X_i = c)\right] \quad &= \frac{\lim_{x\downarrow c} E[ER_i|X_i=x] - \lim_{x\uparrow c} E[ER_i|X_i=x]}{\lim_{x\downarrow c} E[T_i|X_i=x] - \lim_{x\uparrow c} E[T_i|X_i=x]} \\ &= \frac{\widehat{\alpha}_{ERr} - \widehat{\alpha}_{ERl}}{\widehat{\alpha}_{Tr} - \widehat{\alpha}_{Tl}} \end{aligned} \tag{4.16}$$

where X_i is the variable *ageoffense*. The numerator of this ratio is the difference in limits of the value of the dependent variable at the cut-offs (18 and 21) approximated both from the left and the right. More intuitively, $\widehat{\alpha}_{ERr} - \widehat{\alpha}_{ERl}$ is the difference in the estimated intercepts, when regressing estimated recidivism on age: $\widehat{\alpha}_{ERr}$ is the intercept when taking into account only observations with an age above the cut-off and $\widehat{\alpha}_{ERl}$ is the intercept when using only those below the cut-off age. The same intuition holds for the denominator which represents the differences in the limit of treatment probability from both sides of the cut-offs. These limits can be represented as the estimated intercepts $\widehat{\alpha}_{Tr}$ and $\widehat{\alpha}_{Tl}$, stemming from regressions of the treatment indicator T on age. Dividing by the difference in treatment probability can be seen as a normalization which yields the treatment effect if all subjects got the treatment. A special case included in this setup is a so called "sharp" discontinuity where the treatment assignment changes from 0 to 1 upon passing the cut-off. In this case, (4.16) reduces to the numerator. In our setting, however, the jump in treatment assignment is smaller than one since some fraction of adolescent offenders where sentenced as adults. Hence, we have a so called "fuzzy" discontinuity which requires to weight the numerator by the jump in treatment probability.

Note the similarity of their concept to a well-known "Wald" estimator. The analogy to an instrumental variable approach has first been pointed out by Hahn et al. (2001). We are thus using the property "having passed the cut-off point" as an instrument for treatment assignment. In this sense the numerator of (4.16) is the result of the first stage regression of *criminal law* on age at offense while in the denominator we have the second stage regression of expected recidivism on the list of age at offense.

The elements of (4.16) can be estimated either nonparametrically or local-linearly. In addition, further covariates can be included in the regressions or not. The regression design approach requires the assumption that, within a small bandwidth to the left and to the right of the cut-off points, individuals are identical with respect to all regressors influencing recidivism (except for treatment assignment). The trade-off we face when choosing the bandwidth is comparability of the sample versus sample size; for smaller bandwidths we can use less observations, however we do not move too far away from the cut-offs and thus the claim that our individuals on both sides of the cut-off are comparable is easier to accept. This trade-off will also be described in more detail below, when we compare the two samples to the left and right of the cut-off. We calculate the optimal bandwidth according to Imbens and Kalyanaraman (2009) yielding a size of 2 years. In addition, we will also apply different bandwidths to increase the robustness of the estimates.

4.6.1 Test for sample characteristics

First, we have to check for self selection. Even though RD avoids a bias based on the treatment assignment function, there is a chance of self selection by the offenders, in the sense that they try to bring forward the offense to a point in time when milder punishments will still be applied. However, the following table shows that this is very unlikely to be the case:

Table 4.8: Observations RD bins

range ageoffense	17-18	18-19	19-20	20-21	21-22
# of observations	25	30	22	29	27

If self selection really was an issue we should have a peak in density shortly before 18 and shortly before 21. However, this is not the case. We rather seem to have a uniform distribution of inmates based on their age at offense.

With regard to covariates, we might not be able to test the assumption of comparability for all relevant variables, because there might be some unobservables which we cannot analyze. However, we can compare the observable characteristics of the two groups for the two given cut-off points.

Table 4.9: Covariates with significantly different means at 21 and 18

	21+	21-	bdw 2	18+	18-	bdw 2
N	54	51	105	52	45	97
age	25.07	22.25	2.83***	21.08	19.26	1.82***
	(0.31)	(0.2)	(0.37)	(0.2)	(0.18)	(0.28)
ageoffense	22.06	20.23	1.83***	19.03	17.19	1.85***
	(0.07)	(0.08)	(0.11)	(0.09)	(0.08)	(0.12)
female	0.24	0.04	0.2***	0.02	0.04	-0.03
	(0.06)	(0.03)	(0.07)	(0.02)	(0.03)	(0.04)
crim parents	0.06	0.16	-0.1*	0.18	0.19	-0.01
	(0.03)	(0.05)	(0.06)	(0.05)	(0.06)	(0.08)
drugs (deal)	0.15	0.16	-0.01	0.21	0.09	0.12*
	(0.05)	(0.05)	(0.07)	(0.06)	(0.04)	(0.07)
theft	0.37	0.39	-0.02	0.27	0.6	-0.33***
	(0.07)	(0.07)	(0.1)	(0.06)	(0.07)	(0.1)
viol	0.17	0.39	-0.23***	0.46	0.49	-0.03
	(0.05)	(0.07)	(0.09)	(0.07)	(0.08)	(0.1)
vandal	0	0.16	-0.16***	0.1	0.2	-0.1
	(00)	(0.05)	(0.05)	(0.04)	(0.06)	(0.07)
criminal law	1	0.24	0.76***	0.04	0	0.04
	(00)	(0.06)	(0.06)	(0.03)	(00)	(0.03)

standard errors in parentheses, one sided test of mean equality
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

On table 4.9 we summarize the variables with significantly different means (significance level below 10%) to the left and right of at least one cut-off point. Please refer to table 4.16 in the appendix for a list with all variables. Looking at the treatment (*criminal law*) we see that there is no significant difference at the cut-off of 18. Once the offender has turned 18 when committing the crime, judges can apply criminal law. However, our data show that they rarely do so. Therefore, a discontinuity at 18 would not be due to the effects of the applied criminal law, but could only capture the regime change, since criminal law is now applicable. Looking at 21, we can reject the hypothesis of mean equality and do find a discontinuity in treatment assignment. we find a jump from around 25% just before 21, to 100% after 21.

Comparing the means of the other variables, our observations certainly differ in terms of age, which by itself is not a problem since the differences are rather small. Also there seem to be more female inmates above 21 then below. A notable difference is in the type of crime committed. Here we find that younger individuals commit more "juvenile" crimes such as vandalism, theft and violent crimes. Drug dealing is committed more by people above 18. For other crimes we do not find significant differences.

This differences in covariates might have some effect on recidivism, therefore later on we will control for these and other variables in order to avoid that the estimated effect on recidivism is really a causal effect due to appliance of criminal law and not due some other covariates. Another threat is that individuals in Germany become of age once they are 18. This might change their way of thinking which influences recidivism and will not necessarily be reflected in the observable variables. Therefore we can not be sure whether we really can identify a causal effect at 18. At 21 there seems to be no obvious problem. Although it might be true that most individuals are mature once they reached this age, maturity changes gradually and differs among individuals. We look at the cut-off for 21 for all individuals so maturity will not be a problem for our identification strategy.

4.6.2 Estimated jumps in expected recidivism

We apply the RD design using a nonparametric regression. The first thing to note is that the smaller the bandwidth the smaller the sample size and the bigger the effect, but also the bigger the noise.

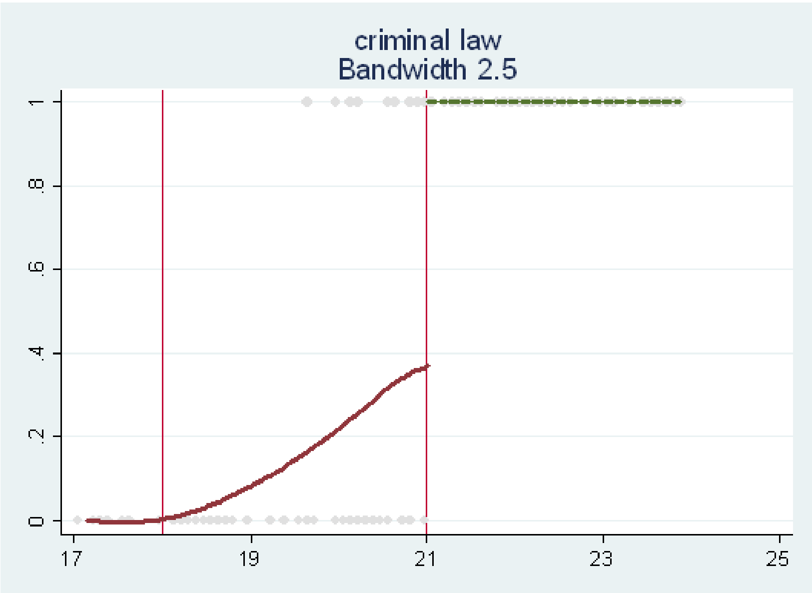

Figure 4.1: Treatment assignment over age at offense

Looking at the data, the cut-off of 21 seems to have a much stronger appeal than 18. The graphical analysis in figure 4.1 shows a jump in treatment assignment at 21 of 0.6-0.7 but no change at 18 (see figure 4.2). Judges do not seem too eager to apply criminal law when offenders were still close to the cut-off point. Also with regard to recidivism, we see no change at 18, but we find a drop at 21 with a magnitude around 0.2. Therefore, at 18 we only have a theoretical change in treatment assignment, but not an effective one, because we have too few observations where criminal law is applied. In addition, this cut-off point includes further disadvantages in terms of new potential biases. First of all in Germany as already mentioned people become of age at 18 and thus are given a set of new rights, e.g. the right to drive a car and to marry. This might lead to biases, if e.g. the possibility to drive a car changes the likelihood to commit certain crimes leading to a different offender sample independent of sanctions.

In table 4.10, we provide estimates for the average treatment effect as defined in (4.16) using different specifications and bandwidths. We see a drop in expected recidivism with a magnitude between 0.2 and 0.3 depending on the bandwidth. The more interesting thing however are the covariates. For the smallest bandwidth the jump in recidivism is significant. For larger

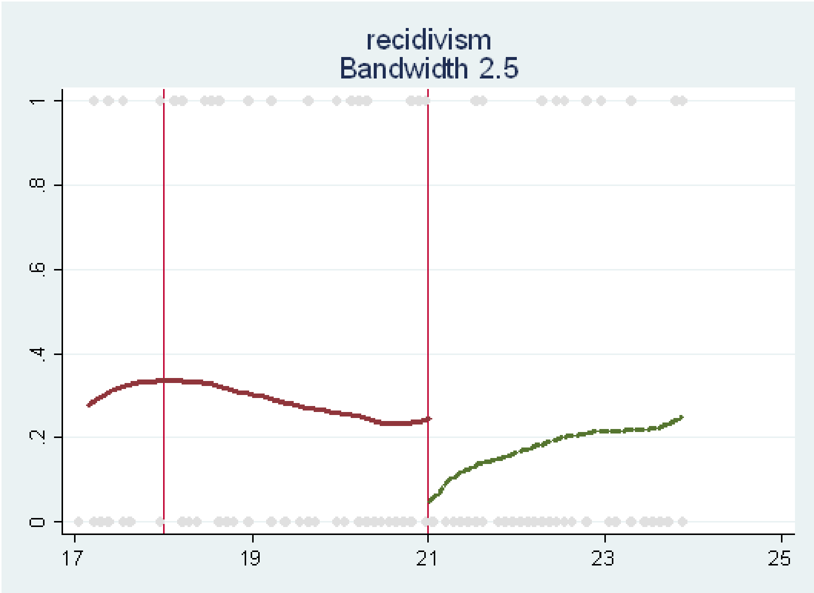

Figure 4.2: Expected recidivism over age at offense

bandwidths significance goes down to 12-13%. However controlling for different covariates we manage to have a much better fit and thus decrease the standard error substantially and we find a significant jump. The additional covariates interestingly have a high impact on the standard error, which is reduced a lot. However our estimations are only slightly changed. This gives an additional indication that our finding is due to the treatment change and not due to some bias. Also, including the covariates where we found significant differences (in table 4.9) in the last specification does not change our results, which seems very robust. Dividing the jump in recidivism by the jump in treatment assignment can be thought of as a normalization giving the result as if the jump in probability would have been from 0 to 1. As a consequence, the size of the effect increases, estimating a drop in recidivism of 0.36 to 0.56 if all delinquents got criminal treatment.

For the second cut-off (c=18, table 4.11) we did not find any significant jump in the treatment assignment function. We rather checked this cut-off suspecting a general regime change. The probability of being treated with the new legal regime jumps from zero to one when turning 18. Hence, the denominator becomes 1 and the treatment effect boils down to the Jump of *recid* ($\alpha_{ERr} - \alpha_{ERl}$). For this cut-off we do not find any significant results. Without covariates, the jump is positive but not significant. When we include further covariates the jump does not change too much, but also significance stays at the same level. However as noted above even if there would be a jump it is much harder to attribute it to the different treatment because at 18 many things change that could harm our identification. Therefore we can not really draw any conclusions from this finding.

Summarizing, we can not find any conclusive evidence at 18, but we find a significant drop at 21.

4.6.3 Robustness Check: Placebo estimates

Having found the drop at 21 we want to be sure that it was actually due to a causal effect of criminal law on recidivism and not due to other factors. We partly checked this already by

Table 4.10: RD estimates Part A Cutoff 21

21	(1) bdw=1	(2) bdw=2	(3) bdw=2.5	(4) bdw=2	(5) bdw=2	(6) bdw=2
N	50	102	131	102	100	100
bad_pr 21-(α_{ERr})	0.264	0.252	0.245	0.252	0.308	0.203
bad_pr 21+ (α_{ERl})	-0.038	0.034	0.048	0.018	-0.011	-0.031
diff	-0.301*	-0.218	-0.197	-0.234*	-0.32*	-0.234**
	(0.051)	(0.126)	(0.135)	(0.063)	(0.052)	(0.042)
adult 21-(α_{Tr})	0.229	0.35	0.37	0.347	0.429	0.461
adult 21+(α_{Tl})	1	1	1	1	1	1
diff	0.771	0.65	0.63	0.653	0.571	0.539
ATE (τ)	-0.391*	-0.335	-0.313	-0.359*	-0.56*	-0.435*
	(0.061)	(0.134)	(0.141)	(0.057)	(0.055)	(0.059)
age	no	no	no	no	yes	no
female	no	no	no	no	no	yes
city	no	no	no	yes	no	yes
viol	no	no	no	no	no	yes
crim parents	no	no	no	no	yes	yes
drugs (deal)	no	no	no	yes	no	no
vandal	no	no	no	no	no	yes
poor social capital	no	no	no	yes	no	no
social contact	no	no	no	no	no	yes

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

using different bandwidths and covariates, however below we will try to increase robustness of the estimation by performing placebo estimates.

Using the same specifications as above we will try to estimate discontinuities in expected recidivism for cut-offs where no actual law change in terms of punishment arises. We will perform this placebo estimates every 6 months starting from 17 up to 22 and will thus run the 6 RD specifications of above, using the different bandwidths and covariates. If we find significant effects for some cut-offs except 18 and 21 this means that our RD results could be caused by the change in the regulation, but they could also be caused by spurious results due to some unobserved factors or biases. Since there is no law change at the placebo cut-offs we will not divide by the change in treatment (the denominator of 4.16). We only look at the change in recidivism. The full estimates can be found in the appendix (Table 16).

Looking at the results of our placebo estimates we find that the cut-off at 21 has the highest level of significance in most specifications. The second highest significance can be found at the cut-off of 20, where we seem to have a positive jump. However the estimates are only significant in 2 out of 6 specifications, compared to 21 which is significant in 4 out of 6. For all other placebos we do not find any significant jumps. Therefore we can be confident about having found a causal effect of criminal treatment on recidivism.

Summing up, at 18 we are not able to isolate a causal effect of criminal law on recidivism. At 21 however we find a drop in recidivism when criminal law is applied more frequently indicating a negative causal relationship between the application of criminal law and criminal prisons on recidivism.

Table 4.11: RD estimates Part B Cutoff 18

18	(1) bdw=1	(2) bdw=2	(3) bdw=2.5	(4) bdw=2	(5) bdw=2	(6) bdw=2
N	53	93	107	93	89	87
bad_pr 18-(α_{ERr})	0.337	0.319	0.318	0.299	0.34	0.398
bad_pr 18+ (α_{ERl})	0.473	0.495	0.459	0.421	0.495	0.515
ATE (τ)	0.136	0.175	0.141	0.122	0.156	0.117
	(0.31)	(0.35)	(0.40)	(0.58)	(0.444)	(0.579)
age	no	no	no	no	yes	no
female	no	no	no	no	no	yes
city	no	no	no	yes	no	yes
viol	no	no	no	no	no	yes
crim parents	no	no	no	no	yes	yes
drugs (deal)	no	no	no	yes	no	no
vandal	no	no	no	no	no	yes
poor social capital	no	no	no	yes	no	no
social contact	no	no	no	no	no	yes

p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.7 Discussion

The main result of our analyses is that the treatment *criminal law* does not stimulate recidivism, as suggested by many US studies, but rather decreases the likelihood to recidivate. The results of our two approaches are similar in sign and significance. Even though the size of the effect differs in our two estimations, a t-test for equality of the two estimated treatment effects cannot be rejected[13]. It is possible that the small but insignificant differences are due to different samples underlying the estimations: while in the bivariate probit model we look at adolescents only, the regression discontinuity design requires observations beyond the cut-off point (age 21). Hence, on average individuals in the latter analysis are much older. In addition, a regression discontinuity design gives most weights to the observations close to the cut-off point and thus only provides a weighted average treatment effect (Lee and Lemieux 2010). Even though the deviation of the estimated effect from the average treatment effect cannot be identified, given the results we have, it is possible, that the effect is bigger for those close to the cut-off then for the rest of the population.

To what extent could the results be driven by a bias in the outcome variable? Continuing on the discussion in section 3.1, our proxy for recidivism might be subject to a bias. What could be the direction of such an effect? In juvenile prisons, there are more schooling possibilities leading to a temporary underestimation of the real rate of recidivism. In contrast, one might also think of a stronger peer pressure in juvenile prisons which might lead to competition in toughness and an exaggerated report of recidivism. While the first case should lead to on an underestimation of the treatment effect, the second case might reult in an issue. However, if the described peer effects exists, it is likely to not only affect self-reported measures of recidivism but might also drive the real behavior after the release. Hence, we cannot find a convincing story that would damage our results.

[13]In fact the two estimates lie within one standard deviation from each other in most specifications.

4.7.1 Further robustness checks

In addition to the presented results we performed several robustness checks which are briefly summarized in this subsection. First, we also estimated a bivariate ordered probit version of the model. The extension of the described specification is straightforward. The results confirmed the estimates increasing the robustness of our findings.

Second, we found it interesting to analyze whether the observed effect of sanction type is contingent on the possible sanction type the inmate would have to expect upon reoffending. One way to test this hypothesis is to check whether there is an additional effect when the "age when leaving" supersedes 21. If the inmate can expect to leave prison after turning 21 he can be sure that criminal law will be applied for in case of reoffending. This could result in a different probability of recidivism when compared to a subject that leaves prison before turning 21 (the same logic applies at 18). We tested for this possibility by including both "age when leaving" and a dummy if this age was smaller than 21. However, the regressors were almost never significant and did not change our finding on the causal effect of criminal law on recidivism. This might be due to the fact that we are mainly analyzing adolescents and thus most are already older than 21 when leaving prison (average leaving age is 23.5 years). In addition, there is a lot of uncertainty with regard to the actual point in time when the inmate leaves the prison since German penal code includes the possibility of early release (see §§ 57, 57a, 57b Strafgesetzbuch).

4.7.2 Reconciliation with US findings

Furthermore, the question arises why our results are so different from the US evidence? One explanation could be the different attitudes toward crime in the two countries. As Whitman (2003) writes in the introduction to his book on the difference between the legal systems in the two continents, "criminal punishment in America is harsh and degrading–more so than anywhere else in the liberal west." Thus, in the US system adolescents are punished more severely in general - especially after ending up in criminal prison - and therefore they might not be able to reintegrate into society after such an experience. In contrast, the German system is rather mild and sees incarceration as "ultima ratio", especially for juveniles. Therefore, criminal prison seems more adapt to make criminals aware of their wrong doing and showing them the severeness and harm of their crime without destroying the perspectives of young people after incarceration. Thus, the relationship between harshness and recidivism seems to have a U-shaped pattern on adolescents, with too low and too high levels of harshness resulting in high recidivism. Keeping this picture, German juvenile prisons seem to be to the left of the minimum point - and thus more harshness in criminal prisons reduces recidivism. US criminal prisons on the other hand seem to be already to the right of the optimum - and thus more harshness increases recidivism. The results of Hjalmarsson (2009) also points in this direction. Her findings show that incarceration in juvenile facilities can be an effective measure of combating juvenile crime as opposed to even milder punishments such as a probation or a fine. Therefore, probations and fines in the U.S. might be too mild (and thus also to the left of the optimum) when compared to incarceration.

Moreover combining our findings with the recent literature on general deterrence it seems that juveniles ex ante do not have too much information about the consequences of their behavior. They might be myopic, as e.g. found by Lee and McCrary (2009), or simply be misinformed about the outcome. However, after they undergo their punishment with the right degree of harshness delinquent juveniles become aware of their wrongdoing, update their believes about the consequences and offend less. This is also in line with the literature on personal development we already analyzed in the introduction.

4.8 Conclusion

With this paper, we add empirical evidence to the literature on the influence of social and socioeconomic factors on expected recidivism. Furthermore, we have analyzed the impact of sanction type on inmates' expectations on their subsequent criminal behavior. Given the identified bias due to the selection process into criminal law, a simple difference could not be applied. To overcome this identification problem, we first used a bivariate probit model that provides an unbiased estimate of the treatment coefficient given the model is correctly specified. In a second step we exploited the fact that in Germany there are two potential jumps in the probability of being sentenced under criminal law. By taking advantage of the discontinuity at the age of 21, we isolated the causal impact of criminal law on expected recidivism in a regression discontinuity design.

The results from both approaches suggest that being sentenced under criminal law discourages young people from recidivism. This finding is in stark contrast to the literature on US transfer laws and shows that the legal framework in Germany seems to be substantially different from its North American counterpart.

In terms of policy recommendations, our results tend to suggest that it might be fruitful to enlarge the application of criminal law. However, it is important to remember that both the selection model and the regression discontinuity approach are only valid at the given age ranges. Thus, criminal law might have a different effect when applied to other parts of the population. However, the group of adolescents is exactly the group for which recommendation Rec(2008)11 "European Rules for Juvenile Offenders Subject to Sanctions and Measures" suggests an extended application of juvenile law. Our results question the optimality of an excessive use of juvenile law.

One last remark is that Germany should catch up with the English speaking countries in terms of data gathering and should collect data from inmates on a regular basis. In this way researchers could get even more conclusive results, enabling them to provide more robust policy advise.

4.9 Appendix

Table 4.12: Summary statistics (ageoffense<18)

Variable	Mean	Std. Dev.	Min.	Max.	N
expected recidivism	0.3043	0.4652	0	1	46
age	19.0217	1.3943	16.5	22.5	46
ageoffense	16.663	0.7497	14.5833	17.75	46
female	0.0435	0.2062	0	1	46
married	0	0	0	0	45
village	0.186	0.3937	0	1	43
poor social capital	0.4348	0.5012	0	1	46
no_church	0.3409	0.4795	0	1	44
addiction	0.2609	0.444	0	1	46
social contact	0.4318	0.5011	0	1	44
crim_parents	0.1429	0.3542	0	1	42
drugs	0.1522	0.3632	0	1	46
drugs (deal)	0.1304	0.3405	0	1	46
drugs (consume)	0.0652	0.2496	0	1	46
child	0.0698	0.2578	0	1	43
schooling	9.1522	1.4371	5.5	12	46
abi	0	0	0	0	46
job	0.2558	0.4415	0	1	43
job_contact	0.3256	0.4741	0	1	43
theft	0.6087	0.4934	0	1	46
robbery	0.413	0.4978	0	1	46
fraud	0.087	0.2849	0	1	46
bodily injury	0.5217	0.505	0	1	46
vandal	0.2174	0.417	0	1	46
sexual	0.0217	0.1474	0	1	46
murder	0.1304	0.3405	0	1	46
sentence length	2.8696	2.2397	0.5	9.5	46
percseated	0.4132	0.2376	0	0.875	46
open	0.2609	0.444	0	1	46
criminal law	0	0	0	0	46

Table 4.13: Summary statistics ($18 \leq$ ageoffense ≤ 21)

Variable	Mean	Std. Dev.	Min.	Max.	N
expected recidivism	0.3	0.4608	0	1	90
age	21.4222	1.63	18.5	26.5	90
ageoffense	19.4546	1.0189	18	21	90
female	0.0333	0.1805	0	1	90
married	0.1	0.3017	0	1	90
village	0.1136	0.3192	0	1	88
poor social capital	0.4556	0.5008	0	1	90
no_church	0.4458	0.5001	0	1	83
addiction	0.2778	0.4504	0	1	90
social contact	0.5444	0.5008	0	1	90
crim_parents	0.1685	0.3765	0	1	89
drugs	0.2	0.4022	0	1	90
drugs (deal)	0.1556	0.3645	0	1	90
drugs (consume)	0.0889	0.2862	0	1	90
child	0.1954	0.3988	0	1	87
schooling	9.8111	2.1705	3	17	90
abi	0.0333	0.1805	0	1	90
job	0.186	0.3914	0	1	86
job_contact	0.3256	0.4713	0	1	86
theft	0.3778	0.4875	0	1	90
robbery	0.3333	0.474	0	1	90
fraud	0.2	0.4022	0	1	90
bodily injury	0.4222	0.4967	0	1	90
vandal	0.1444	0.3535	0	1	90
sexual	0.0333	0.1805	0	1	90
murder	0.1111	0.316	0	1	90
sentence length	2.9963	2.0739	0.3333	10	90
percseated	0.3377	0.2232	0	1	88
open	0.1111	0.316	0	1	90
criminal law	0.1333	0.3418	0	1	90

Table 4.14: Summary statistics (ageoffense>21)

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
expected recidivism	0.1927	0.3962	0	1	109
age	25.711	2.1946	22.5	35.5	109
ageoffense	23.0443	1.12	21.0833	25	109
female	0.1835	0.3889	0	1	109
married	0.1284	0.3361	0	1	109
village	0.2952	0.4583	0	1	105
poor social capital	0.5413	0.5006	0	1	109
no_church	0.3592	0.4821	0	1	103
addiction	0.3429	0.4769	0	1	105
social contact	0.5872	0.4946	0	1	109
crim_parents	0.1028	0.3051	0	1	107
drugs	0.211	0.4099	0	1	109
drugs (deal)	0.1835	0.3889	0	1	109
drugs (consume)	0.0917	0.29	0	1	109
child	0.3241	0.4702	0	1	108
schooling	10.4352	1.9636	2	16	108
abi	0.0566	0.2322	0	1	106
job	0.181	0.3868	0	1	105
job_contact	0.2857	0.4539	0	1	105
theft	0.3119	0.4654	0	1	109
robbery	0.1743	0.3811	0	1	109
fraud	0.211	0.4099	0	1	109
bodily injury	0.1651	0.373	0	1	109
vandal	0	0	0	0	109
sexual	0.0734	0.262	0	1	109
murder	0.1193	0.3256	0	1	109
sentence length	4.2252	3.9365	0.0833	15	109
percseated	0.3852	0.2525	0	0.9545	104
open	0.1667	0.3744	0	1	108
criminal law	1	0	1	1	109

Table 4.15: Drivers of expected recidivism (extended sample)

	(1) recidivism	(2) recidivism	(3) marginal	(4) recidivism	(5) marginal
age	-0.0479*	-0.0472*	-0.0145**	-0.0593**	-0.0179**
	(0.062)	(0.058)	(0.042)	(0.042)	(0.022)
female	-0.447***	-0.575***	-0.145***	-0.737***	-0.171***
	(0.003)	(0.000)	(0.000)	(0.002)	(0.000)
job contact	-0.272*	-0.294**	-0.0901**	-0.318**	-0.0959**
	(0.062)	(0.041)	(0.029)	(0.050)	(0.045)
social contact	-0.282*	-0.279	-0.0865		
	(0.088)	(0.106)	(0.127)		
crim_parents	0.271	0.314	0.104	0.249	0.0801
	(0.181)	(0.108)	(0.145)	(0.147)	(0.184)
open	-0.388**	-0.341*	-0.0958*	-0.344	-0.0949*
	(0.026)	(0.090)	(0.059)	(0.129)	(0.090)
theft	0.280**	0.262**	0.0817**	0.344*	0.107*
	(0.014)	(0.020)	(0.017)	(0.058)	(0.067)
addiction	0.297*	0.251	0.0797		
	(0.072)	(0.136)	(0.148)		
drugs (deal)		0.0223	0.00688	0.162	0.0511
		(0.898)	(0.898)	(0.508)	(0.519)
drugs (consume)		0.577**	0.202**	0.543*	0.188*
		(0.028)	(0.046)	(0.069)	(0.098)
robbery				0.106	0.0326
				(0.524)	(0.530)
city				-0.180	-0.0532
				(0.389)	(0.376)
poor social capital				0.0688	0.0208
				(0.809)	(0.808)
prison experience				0.0401	0.0121
				(0.826)	(0.825)
criminal law				0.0401	0.0121
				(0.826)	(0.825)
Constant	0.553	0.499		0.634	
	(0.286)	(0.303)		(0.351)	
Observations	223	223	223	208	208
Pseudo R^2	0.084	0.097	0.097	0.093	0.093

p-values in parentheses, marginal effects calculated at means

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

Table 4.16: Covariates mean comparison at 21 and 18

	21+	21-	bdw 2	18+	18-	bdw 2
age	25.07	22.25	2.83***	21.08	19.26	1.82***
	(0.31)	(0.2)	(0.37)	(0.2)	(0.18)	(0.28)
ageoffense	22.06	20.23	1.83***	19.03	17.19	1.85***
	(0.07)	(0.08)	(0.11)	(0.09)	(0.08)	(0.12)
female	0.24	0.04	0.2***	0.02	0.04	-0.03
	(0.06)	(0.03)	(0.07)	(0.02)	(0.03)	(0.04)
married	0.17	0.12	0.05	0.08	0	0.08
	(0.05)	(0.05)	(0.07)	(0.04)	(00)	(0.04)
city	0.27	0.16	0.11	0.06	0.16	-0.1
	(0.06)	(0.05)	(0.08)	(0.03)	(0.06)	(0.06)
poor social capital	0.57	0.49	0.08	0.37	0.49	-0.12
	(0.07)	(0.07)	(0.1)	(0.07)	(0.08)	(0.1)
no_church	0.35	0.42	-0.08	0.42	0.42	0
	(0.07)	(0.07)	(0.1)	(0.07)	(0.08)	(0.1)
addiction	0.38	0.27	0.11	0.31	0.27	0.04
	(0.07)	(0.06)	(0.09)	(0.06)	(0.07)	(0.09)
social contact	0.48	0.59	-0.11	0.58	0.37	0.2
	(0.07)	(0.07)	(0.1)	(0.07)	(0.07)	(0.1)
crim parents	0.06	0.16	-0.1*	0.18	0.19	-0.01
	(0.03)	(0.05)	(0.06)	(0.05)	(0.06)	(0.08)
drugs (deal)	0.15	0.16	-0.01	0.21	0.09	0.12*
	(0.05)	(0.05)	(0.07)	(0.06)	(0.04)	(0.07)
drugs(consume)	0.15	0.1	0.05	0.08	0.04	0.03
	(0.05)	(0.04)	(0.06)	(0.04)	(0.03)	(0.05)
child	0.21	0.17	0.04	0.21	0.1	0.12
	(0.06)	(0.05)	(0.08)	(0.06)	(0.05)	(0.08)
schooling	10.19	10.23	-0.04	9.56	9.43	0.12
	(0.28)	(0.29)	(0.4)	(0.31)	(0.21)	(0.38)
abi	0.04	0.04	0	0.04	0	0.04
	(0.03)	(0.03)	(0.04)	(0.03)	(00)	(0.03)
job	0.15	0.21	-0.06	0.14	0.24	-0.1
	(0.05)	(0.06)	(0.08)	(0.05)	(0.07)	(0.08)
job contact	0.43	0.5	-0.07	0.48	0.57	-0.09
	(0.07)	(0.07)	(0.1)	(0.07)	(0.08)	(0.11)
theft	0.37	0.39	-0.02	0.27	0.6	-0.33***
	(0.07)	(0.07)	(0.1)	(0.06)	(0.07)	(0.1)
robbery	0.19	0.31	-0.13	0.33	0.47	-0.14
	(0.05)	(0.07)	(0.08)	(0.07)	(0.08)	(0.1)
fraud	0.19	0.2	-0.01	0.21	0.11	0.1
	(0.05)	(0.06)	(0.08)	(0.06)	(0.05)	(0.08)
viol	0.17	0.39	-0.23***	0.46	0.49	-0.03
	(0.05)	(0.07)	(0.09)	(0.07)	(0.08)	(0.1)

Table 4.16: (continued)

	21+	21-	bdw 2	18+	18-	bdw 2
vandal	0	0.16	-0.16***	0.1	0.2	-0.1
	(00)	(0.05)	(0.05)	(0.04)	(0.06)	(0.07)
sexual	0.11	0.04	0.07	0	0.04	-0.04
	(0.04)	(0.03)	(0.05)	(00)	(0.03)	(0.03)
murder	0.13	0.1	0.03	0.15	0.13	0.02
	(0.05)	(0.04)	(0.06)	(0.05)	(0.05)	(0.07)
open	0.17	0.1	0.07	0.13	0.16	-0.02
	(0.05)	(0.04)	(0.07)	(0.05)	(0.05)	(0.07)
criminal law	1	0.24	0.76***	0.04	0	0.04
	(00)	(0.06)	(0.06)	(0.03)	(00)	(0.03)

standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ (one sided test of mean equality)

Table 4.17: Placebo estimates

	(1)	(2)	(3)	(4)	(5)	(6)
17	43	80	89	80	77	75
	-0.109	-0.091	-0.045	-0.118	-0.071	0.006
	(0.636)	(0.647)	(0.814)	(0.454)	(0.732)	(0.968)
17.5	50	85	101	85	82	80
	0.119	0.125	0.162	0.098	0.169	0.155
	(0.746)	(0.577)	(0.413)	(0.64)	(0.491)	(0.304)
18	53	93	107	93	89	87
	0.136	0.175	0.141	0.122	0.156	0.117
	(0.613)	(0.396)	(0.449)	(0.525)	(0.444)	(0.579)
18.5	49	96	122	96	92	90
	0.241	0.073	0.053	0.108	0.064	0.15
	(0.449)	(0.719)	(0.777)	(0.578)	(0.749)	(0.409)
19	50	103	126	103	100	99
	0.157	-0.071	-0.092	-0.154	-0.058	-0.048
	(0.626)	(0.73)	(0.614)	(0.436)	(0.772)	(0.802)
19.5	46	101	129	101	99	99
	-0.056	0.026	-0.002	0.059	0.041	0.072
	(0.81)	(0.89)	(0.991)	(0.732)	(0.82)	(0.673)
20	50	105	130	105	104	104
	0.463	0.305	0.265	0.32	0.343	0.278
	(0.102)	(0.11)	(0.122)	(0.076)	(0.05)	(0.134)
20.5	52	105	131	105	103	103
	-0.288	-0.179	-0.144	-0.265	-0.298	-0.158
	(0.224)	(0.32)	(0.374)	(0.118)	(0.076)	(0.324)
21	55	102	131	102	100	100
	-0.301	-0.218	-0.197	-0.234	-0.32	-0.234
	(0.051)	(0.126)	(0.135)	(0.063)	(0.052)	(0.042)
21.5	59	107	130	107	105	105
	0.322	0.163	0.144	0.132	0.145	0.098
	(0.041)	(0.22)	(0.239)	(0.337)	(0.318)	(0.48)
22	52	109	138	109	108	108
	-0.06	0.154	0.14	0.224	0.143	0.242
	(0.64)	(0.274)	(0.294)	(0.123)	(0.35)	(0.104)
22.5	55	116	135	116	115	115
	0.234	0.213	0.202	0.255	0.207	0.231
	(0.291)	(0.216)	(0.195)	(0.119)	(0.239)	(0.16)

Specifications of RD
p-values in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

CHAPTER

5 Conclusion

In the previous chapters I was looking at different situations in which the degree of cooperation is one of the key output variables. In this last section, I briefly summarize the results and point to possible impacts of the presented results on practice and research.

The first essay shows how different market settings provide different outcomes in a duopoly where consumers have a reservation price and incur quadratic transportation costs when consuming the good. This paper reveals that firms can efficiently cooperate and serve the whole market. Knowledge about the market parameters helps to understand the observed locations and thus can yield adequate market interventions.

In the second essay we design an experiment to test for guilt aversion. While many recent studies have failed to find traces of guilt aversion in the lab, our analysis can show its existence. We find different results depending on the situational context. Both a repetition of the situation and the act of putting oneself into the shoes of the other increase the influence of the other person's expectation on decision making. In terms of policy advise, this means that the old pedagogical trick of perspective taking can be an effective measure to foster cooperation. This result can be useful in any cooperative relationship, especially in the case of long distance work places where social distance is very large - even though perspective taking can be a more difficult task in this case. In terms of methodology, a key feature of our econometric model is the control for individual fixed effects. This tool so far is, to our knowledge, rarely used in experimental economics despite its positive effect on explanatory power and fit of the estimated model. Hence, controlling for past behavior is a methodological tool which might promise interesting results also in different experimental settings. In terms of extension to the paper, we started to collect data from psychological questionnaires which capture additional personality features that might explain differences in behavior. Further, one could use neurological correlates as further controls. This path surely represents a very promising and interesting future path in modeling human decision making.

In the third essay, we found that sentencing adolescents as adults decreases their self-reported probability of recidivism. This result again shows, how the same treatment, criminal law, can have different impacts when applied in a different context. Our results stand in stark contrast to the previous US findings, which find criminal law to increase recidivism when applied to minors. The different findings can be reconciled when allowing for different reactions depending on age and considering different levels of harshness in the two countries. In terms of policy advise, this suggests that Germany might apply a too soft legal system for adolescents while the US framework is too harsh.

In summary, the three essays identified different incentives for cooperation showing how difficult it is to implement a framework that effectively stimulates cooperation.

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Hiermit erkläre ich, dass ich die Arbeit - abgesehen von den in ihr ausdrücklich genannten Hilfen - selbstständig verfasst habe.

Daniel Römer

Kurzbiographie

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