Essays on Round-Number Effects and Moral Balancing

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Introduction

> *"There's no better way to build confidence in a theory than to believe it is not testable."*
>
> – Richard H. Thaler, *Misbehaving: The Making of Behavioral Economics*

Many years ago, it was the big societal problems being discussed on television and in magazines that sparked my interest in economics. Issues like economic growth, unemployment, financial crises, and climate change were ubiquitous and continue to shape the public discourse to this day. Initially drawn to these topics, I decided to pursue a degree in economics, where these grand-scale phenomena are typically, at least at the undergraduate level, explored in the field of macroeconomics.

However, throughout the course of my studies, I came to the conclusion that these macroeconomic issues cannot be fully understood without first understanding how and why individuals act. These grand-scale phenomena ultimately are made up by the many decisions made and actions taken by individuals within society. This realization made me shift my academic focus, and I became convinced that, in order to have a solid foundation for economics, one must have a thorough understanding of the behaviors and motivations of individuals. This insight led me to change my focus towards behavioral economics, which is the field within which the projects presented within this dissertation can mainly be allocated. While two of the projects analyze aspects of negotiations and bargaining behavior, a classic topic of economics, in a variety of settings, a third project deals with the consistency of individual behavior in repeated decision-making.

Recent years have seen a plethora of studies that aim at getting a better grasp of human behavior. The rise of behavioral economics allows for the analysis of how real human beings behave in their true, and sometimes non-intuitive nature. Behavioral economics enriches our understanding of economics by incorporating psychological insights into economic models, revealing how emotions, cognitive biases, and social factors influence decision-making. By examining the discrepancies between idealized behaviors predicted by traditional economics and the, at times, imperfect actions observed in real life, this field provides a more realistic foundation for economics as a science. This development not only can improve our comprehension of the economic dynamics behind the grand-scale phenomena mentioned above, but can also enhance the relevance and accuracy of economic predictions for real-world scenarios. This allows economists to design more effective policies and interventions tailored to actual human behavior.

The aim of this doctoral thesis is to add my small and modest contribution to this endeavour. It investigates behavior in both individual and potentially cooperative settings. The three projects presented in this dissertation deal with the analysis of both observational data and the examination of behavior in controlled experiments. While it is certainly desirable to analyze human behavior as closely as possible to natural situations, and ideally directly in the natural environment itself, as the empirical data on real estate transactions and eBay transactions in this dissertation allows for, this often has the disadvantages that the process generating the data might not be fully understood or controllable in all relevant aspects, and that external factors could influence the analysis. Even if the researcher is uncertain about any specific external factors and how they might interact with the results, complete certainty can not be achieved.

The emergence of experimental economics provides a means to circumvent, or at least mitigate, such concerns. By utilizing controlled experiments, researchers can make causal statements with greater confidence. By maintaining consistency in external conditions, any deviations observed beyond random statistical noise can be confidently attributed to interventions made during the experiment. This controlled environment thus enables researchers to make causal interpretations of their findings, offering clearer insights into how specific variables influence behavior. Therefore, in two of the projects of this dissertation, we devise economic experiments to test our hypotheses.

The first project presented in Chapter 2 explores the role of round numbers in real-world real estate negotiations. Round numbers affect behavior in various domains. Marathon runners aim to stay below round-number finishing times, for example, aiming to run a marathon in under four hours (Allen, Dechow, Pope, and Wu, 2017). Also in the cases of professional baseball players' batting averages and high school students' SAT scores, individuals strive to surpass round-number thresholds (Pope and Simonsohn, 2011). Even where the stakes are high, round numbers affect behavior. This is illustrated by Pope, Pope, and Sydnor (2015) in their study of the US residential real estate market, where they find round-number clusters in the price distribution. In line with their findings, we find that residential real estate transactions in Germany also cluster at round-number prices. There are, however, interesting discrepancies as to what seems to be perceived as a round number compared to the data for the United States presented by Pope, Pope, and Sydnor (2015), which are presumably caused by cultural differences. Finally, we extend our analysis to the commercial real estate market, where stakes are even higher and market participants arguably more experienced. For the same type of object, professionals cluster significantly less on round-number prices compared to non-professionals. We employ hedonic regressions and machine learning techniques to show that transactions of family homes and condominiums at round number prices are 2–7% above their hedonic values.

The second project presented in Chapter 3 is directly motivated by the findings in Chapter 2. The goal was to further explore possible reasons for why round-number clusters appear in observational data. One possible explanation is that individuals have an intrinsic preference for round numbers, and are thus more likely to propose and accept round numbers (*bias*). Another explanation is that round numbers are being used as a means to facilitate coordination in cooperative decision making like negotiations (*focal point*). Recent years have seen a growing body of literature on the role of round numbers in decision-making, including round-number bias and focal points. We analyze how these two channels relate to round-number clusters in observational and experimental data on price negotiations, in order to determine if round-numbers clusters are caused by one channel or another, or possibly a combination of both. In a first step, using observational data on eBay negotiations from Backus, Blake, Larsen, and Tadelis (2020), we find a large fraction of successful negotiations ending with round prices. Also, round prices correlate with faster agreements. In a second step, we devise an online experiment to disentangle the two channels. The experiment was conducted on Amazon MTurk and confirms that round numbers are associated with quicker decisions. Moreover, we find evidence for the relevance of both channels - bias and focal points.

The third project presented in Chapter 4 lies at the intersection of behavioral and environmental economics. Many interventions for bolstering environmentally sustainable behavior are aimed at changing individual behavior. However, when designing behavior change interventions, dynamic effects are often overlooked. If individuals were to use pro-environmental but negligible acts to justify acts with a substantial detrimental impact on the environment, the overall welfare effects of behavior change interventions could be overestimated. This project investigates moral balancing, i.e. justifying an immoral act with a previous moral act, in pro-environmental behavior and whether it occurs only for acts with a substantial impact on the environment, or even if the impact of the positive action is negligible. In a two-stage economic experiment, we find that participants who successfully acquired a moral license offset less carbon than those who failed to acquire a license. We exogenously vary the magnitude of the initial pro-environmental act and discover that the magnitude does not systematically affect moral balancing. Since participants with the greatest environmental concerns do not engage in moral balancing, we conclude that environmental concerns moderate moral balancing.

The dissertation is structured as follows. Chapter 2 presents the project on the role of round numbers in real estate transactions. Chapter 3 presents the project in which we disentangle whether round-number effects can be explained by round-number bias, focal points, or both. Chapter 4 presents the project on moral balancing and the dynamic effects in pro-environmental decision-making. Chapter 5 concludes.

2

Round Number Effects in Real Estate Prices: Evidence from Germany[1]

2.1 Introduction

Motivation Decision-makers seem to have a bias in favor of "round numbers", and such behavior has been documented in a wide variety of contexts. For example, Allen, Dechow, Pope, and Wu (2017) show that runners aim to stay below round-number finishing times, for example, aiming to run a marathon in under four hours. In a similar vein, it appears that decision-makers strive to surpass round-number thresholds in performance scales (see e.g., Pope and Simonsohn, 2011, for the cases of professional baseball players' batting averages, high school students' SAT scores, and related lab settings). The influence of round numbers extends to settings in which there are substantial financial stakes. For example, in used-car transactions, there are discrete drops in sales prices at 10,000-mile odometer thresholds (see e.g., Busse, Lacetera, Pope, Silva-Risso, and Sydnor, 2013; Lacetera, Pope, and Sydnor, 2012) and, for vintage cars, at thresholds relating to the car's year of first registration (see e.g., Englmaier, Schmöller, and Stowasser, 2017, for the case of Germany). Converse and Dennis (2018) also find that, in US stock markets, round-number trade volumes occur more frequently than one would expect.[2]

[1] This chapter is based on Englmaier, Roider, Schlereth, and Sebastian (2023).

[2] More broadly, it has been shown that the salience of certain features of an economic problem affects decision-making, see e.g., Englmaier, Roider, and Sunde (2017) for the reaction of workers to incentive

Even where the stakes are high, round numbers affect behavior. This is illustrated by Pope, Pope, and Sydnor (2015) in their study of the US residential real estate market. The authors find that a large share of transactions takes place at round-number prices, such as prices evenly divisible by 5,000, 25,000, and 50,000. Meng (2023) and Best and Kleven (2018) provide similar evidence for real estate transactions in the UK. While some round-number effects might be driven by behavioral biases (for example, consider the apparent desire to finish a marathon in under four hours), Pope, Pope, and Sydnor (2015) convincingly argue that, in the context of real estate transactions, round-number prices serve as focal points (in the sense of Schelling, 1960) in the negotiations between buyers and sellers. In particular, for part of their sample, Pope, Pope, and Sydnor (2015) have access not only to transaction prices, but also to listing prices. They find that objects that sell at round-number prices rarely have round number listing prices,[3] implying that the special attraction of round-number prices particularly emerges in the negotiations, where they arguably serve as particularly prominent focal points.[4]

For most individuals, buying a home will be among the most high stakes decisions of their lifetime. At the same time, transactions like these are generally rare, making it hard to acquire relevant experience. This is particularly true for the German residential real estate market. For example, Kaas, Kocharkov, Preugschat, and Siassi (2021) show that Germany has both the lowest homeownership rate in the developed world and a very low turnover rate for houses and condominiums (averaging at about half that of other Western European countries).

A lack of transactional experience in such markets might affect the appeal of round numbers as focal points in negotiations. For that reason, the following questions emerge. First, it would be interesting to consider commercial real estate transactions (where market participants interact frequently) to explore the importance of round-number prices when market participants are experienced. Second, it might be instructive to directly compare the relevance of round-number prices in settings where some transactions are

provision in firms, Karlan, McConnell, Sendhil, and Zinman (2016) for savings decisions, Stango and Zinman (2014) for the likelihood of incurring checking overdraft fees, Brown, Hossain, and Morgan (2010) and Hossain and Morgan (2006) for bidding in online auctions, and Chetty, Looney, and Kroft (2009) for retail sales.

[3]Listing prices are often “charm prices” (i.e., prices that are just below some prominent threshold, such as $499,000). For family homes in the fourteen largest metropolitan statistical areas in the U.S., Chava and Yao (2017) find that 40% of all listing prices end with 900, while 45% are evenly divisible by 1,000. For final sales prices, these fractions are 9% and 70%, respectively. See Repetto and Solís (2020) for evidence on the Swedish housing market. Hofmann and Stowasser (2023) document the presence of charm pricing in rental markets. For a survey of behavioral phenomena in real estate markets, see e.g., Salzmann and Zwinkels (2017).

[4]For experimental evidence on the relevance of focal points in bargaining, see e.g., Isoni, Poulsen, Sugden, and Tsutsui (2013, 2014).

conducted by "professionals" and others by "non-professionals". Finally, given the important role of round-number prices in real estate markets, it seems worthwhile to explore the degree to which the appeal of round numbers affects price formation. Our project makes a contribution to answering these questions.

Contribution to the Literature We have access to a data set containing 5.38 million real estate transactions in Germany in the period 2003 to 2022, covering various types of residential and commercial properties. This constitutes approximately 30% of all real estate transactions in Germany in this time interval. The data provide information on each object's final sales price and a number of additional characteristics.

We obtain the following findings. First, we show that Pope, Pope, and Sydnor's (2015) main results on residential real estate can be extended to the case of Germany, with an interesting difference. In the German data, there is no pronounced clustering of transactions at prices that are evenly divisible by 25,000, which might be driven by the fact that (culturally) "quarters" play a less pronounced role in Germany than in the US.

Second, we find that also in commercial real estate markets (where market participants are arguably more experienced and interact more frequently, and stakes are relatively high) there is strong evidence of clustering at round-number prices.

Third, in order to assess the relative importance of round-number effects in residential and commercial real estate transactions, we hold the type of object fixed. We look at the sales of condominiums, where some are acquired to be occupied by the buyer (which we interpret as a residential motive) and others that are acquired to be let to third parties (which we interpret as a commercial motive). We document that, in commercial transactions, a significantly smaller fraction of sales takes place at round-number prices. In particular, relative to residential transactions, the fraction of commercial transactions that happen at round-number prices is about a third lower. Nevertheless, a substantial fraction of commercial transactions still involves round-number prices. Thereby, we contribute to the literature on differences in decision-making by what we term "non-professionals" (who do a task only infrequently) and more experienced "professionals".[5] In this respect, Converse and Dennis (2018) and Busse, Lacetera, Pope, Silva-Risso, and Sydnor (2013), discussed above, are the studies most closely related to our own. In particular, Converse and Dennis (2018) find that, in stock trades, round-number trade volumes occur less frequently "with higher investor sophistication" (proxied by the share of institutional ownership in the company under consideration). For used car sales,

[5]When looking at behavioral biases, this literature finds that experience that is closely related to the task at hand reduces or even eliminates such effects. For a survey, see e.g., Fréchette (2016).

Busse, Lacetera, Pope, Silva-Risso, and Sydnor (2013) show that price discontinuities at round-number odometer thresholds are smaller for wholesale than for retail transactions. We extend these findings to the case of high-stakes real estate transactions.

Finally, we explore the degree to which the appeal of round numbers affects price formation. We do so by fitting hedonic models for objects that sell at round-number prices and using the parameter estimates obtained to predict the prices of objects that sell at round-number prices. We use the difference between predicted and actual round number sales prices as a measure of the price distortion from the appeal of round numbers. We conduct this exercise for sales of family homes and condominiums, for which we have the most comprehensive set of covariates. If, instead of OLS, we use regression trees to estimate the hedonic models (which improves the fit considerably), we find that for transactions that take place at round-number prices, on average, the actual price is between two and seven percent larger than its predicted value, i.e., these objects are sold at a premium. Notably, as argued by Meng (2023), such an appeal of round numbers might also affect *future* prices. In particular, Meng (2023) considers repeated real estate transactions in the Greater London area. She documents that if the previous sales price is a round number, the subsequent sales price at a *later* sale is on average 4% higher compared to objects where the previous sales price was a charm price, which seems to be driven by reference-dependent preferences.

Outline The remainder of the chapter is structured as follows. In Section 2.2, we introduce the institutional background and the data. In Section 2.3, we present our evidence on the special appeal of round-number prices in residential and commercial real estate, and Section 2.4 turns to the relative importance of the effect in these real estate categories. In Section 2.5, we turn to the question of how the special appeal of round numbers affects sales prices relative to predicted values based on object characteristics. Section 2.6 concludes. Additional figures and tables can be found in the Appendix A.

2.2 Institutional Background and Data

Our analysis is based on a proprietary dataset of more than 5.38 million residential and commercial real estate transactions in Germany between January 1, 2003 and December 31, 2022. As such, our data cover approximately 30% of all real estate transactions in Germany in the period. We were granted access to this data by *vdp research GmbH*, which is the real estate research institute of the *Association of German Pfandbrief Banks (“Verband deutscher Pfandbriefbanken”).* Nearly 600 regional and national member banks report on the real estate transactions that they finance to *vdp research GmbH*.[6]

Table 2.1 provides descriptive statistics for our data. This shows that transactions related to residential real estate (i.e., family homes and owner-occupied condominiums) comprise 60% of all transactions. The average family home in our sample sold for slightly more than 320,000 Euro, while the average owner-occupied condominium sold for just above 223,000 Euro. Apartment buildings, the largest category in commercial real estate, occupy a share of about 26 percent, although commercial real estate includes a wide range of other categories. The data include the transaction price and date of sale of each object. For residential transactions in particular, we have access to additional characteristics of the respective property, discussed in greater detail in Section 2.5.

2.3 Evidence on Round-Number Effects in Sales Prices

In this section, we replicate the analysis in 2015 for residential real estate in Germany and extend it to the case of commercial real estate. We also investigate whether round-number prices play a bigger role in residential or commercial transactions.

2.3.1 Residential Real Estate Transactions

As discussed above, Pope, Pope, and Sydnor (2015) find that round-number prices serve as focal points in residential real estate transactions in the US. As it turns out, there are also strong round-number effects in the case of Germany. However, there are also interesting differences with respect to the specific prices at which clustering occurs, which might be culturally driven.

In a first step, we illustrate our findings graphically. Panels (a) and (b) of Figure 2.1 display histograms of residential real estate transactions across sales prices for two exemplary price ranges.[7] In both panels, there is a clear clustering of transactions at prices that are evenly divisible by 5,000 (as indicated by the dotted vertical lines). These spikes tend to be more pronounced if the respective price is a multiple of 10,000, 50,000, or even 100,000. Interestingly, and in stark contrast to Pope, Pope, and Sydnor's (2015) findings for the US, in Germany, there do not seem to be strongly pronounced clusters at

[6] In Germany, approximately 90% of all real estate transactions are credit-financed, and according to confidential analysis by *vdp research GmbH* their sample is representative of real estate transactions in Germany. *vdp research GmbH* mainly uses this data for providing various price indices and for consulting purposes.

[7] For reasons of comparability, we consider the same price ranges as depicted in Figures 5 and 6 of Pope, Pope, and Sydnor (2015), where in the present case, prices are in Euro.

Table 2.1. Descriptive Statistics of Sales Prices by Type of Real Estate

No.	Type of Real Estate	Observations	Mean	10th Percentile	Median	90th Percentile
Residential Real Estate		3,240,578	276,801	86,000	222,000	490,000
(1)	Family Homes (Detached)	1,274,934	322,083	125,000	269,000	546,000
(2)	Family Homes (Other)	513,890	315,627	130,000	261,000	537,000
(3)	Condominiums (Owner-Occupied)	1,451,754	223,290	66,000	165,103	400,000
Commercial Real Estate		2,149,232	6,236,457	139,000	500,000	6,500,000
(4)	Condominiums (Let)	132,914	258,329	53,648	130,445	410,000
(5)	Apartment Buildings	1,420,926	1,596,409	165,000	450,000	2,300,000
(6)	Commercial Building Land	2,181	12,200,000	230,000	1,200,000	11,300,000
(7)	Office Buildings	153,377	44,000,000	745,000	8,500,000	102,000,000
(8)	Retail Properties (Small)	62,507	9,059,730	176,000	1,309,115	8,895,000
(9)	Retail Properties (Large)	49,255	29,800,000	1,200,000	6,820,000	66,500,000
(10)	Shopping Malls	15,011	45,500,000	1,560,000	12,100,000	152,000,000
(11)	Warehouses	177,643	4,053,747	74,000	385,000	7,510,000
(12)	Factory Buildings	62,268	6,290,115	255,000	1,200,000	10,900,000
(13)	Hotels (Business)	35,400	5,902,074	332,000	1,394,000	12,300,000
(14)	Hotels (Leisure)	31,358	837,451	125,100	395,000	1,378,000
(15)	Hotels (Other)	6,392	41,000,000	380,000	10,600,000	113,000,000

Note: Sales prices are nominal and denoted in Euro. The category "Retail Properties (Small)" ("Retail Properties (Large)") is comprised of outlets with a sales floor of up to (more than) 800 square meters. "Residential Real Estate" consists of categories (1)-(3), while "Commercial Real Estate" consists of categories (4)-(15).

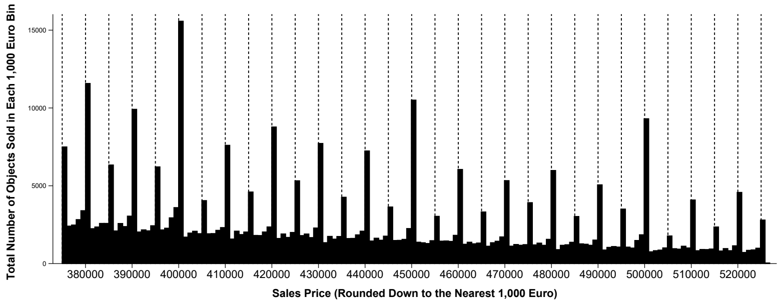
prices that are evenly divisible by 25,000. Intuitively, this might be because, for historical reasons, the "quarter" unit is much more commonly used as a measure in the US than in Germany. For example, there is a quarter dollar coin in the US, while no such partition exists in the Euro. Hence, multiples of 25,000 might be more prominent numbers in the US than in Germany and are thus more likely to serve as salient focal points. This is consistent with findings from social psychology documenting that the salience of certain numbers is influenced by cultural factors (see e.g., Converse and Dennis, 2018).

In our second step, we investigate the statistical significance of these findings through a regression analysis that is based on the specification by Pope, Pope, and Sydnor (2015).[8] In particular, we regress the number of sales at a given price on a set of dummy

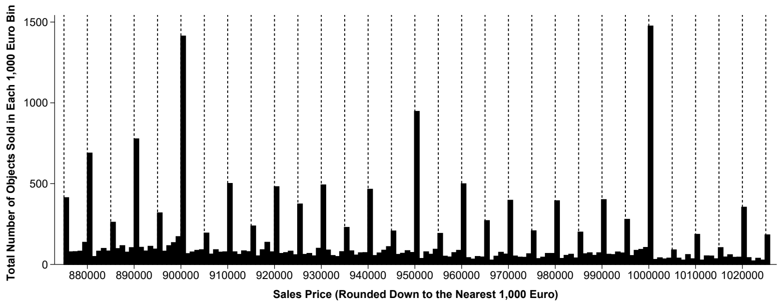
[8] As Pope, Pope, and Sydnor (2015), we restrict attention to observations where sales prices are evenly

Figure 2.1. Number of Transactions Across Sales Prices

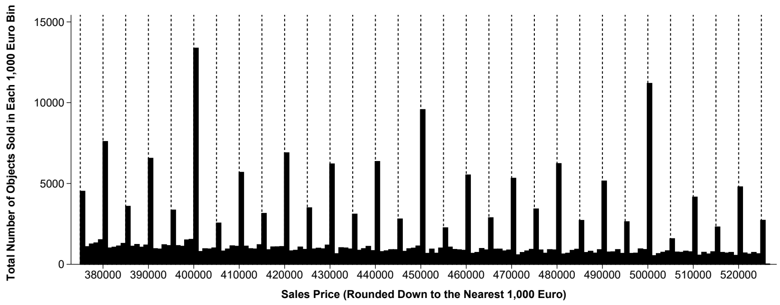
(a) Residential Real Estate (Price Range: 375,000 - 525,000 Euro)



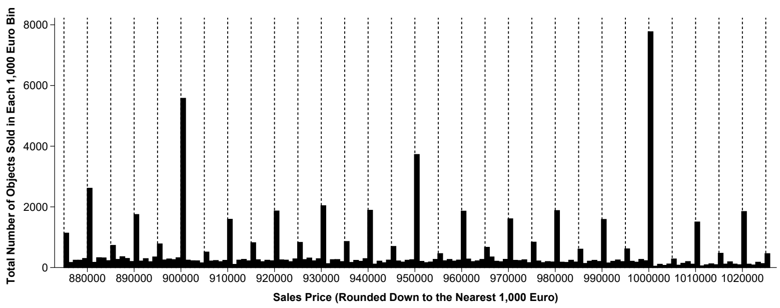
(b) Residential Real Estate (Price Range: 875,000 - 1,025,000 Euro)



(c) Commercial Real Estate (Price Range: 375,000 - 525,000 Euro)



(d) Commercial Real Estate (Price Range: 875,000 - 1,025,000 Euro)



Note: The figure displays the number of transactions across sales prices. Panels (a) and (b) focus on residential real estate transactions, while Panels (c) and (d) focus on commercial real estate transactions. Panels (a) and (c) ((b) and (d)) consider objects whose sales prices were between 375,000 and 525,000 Euro (875,000 and 1,025,000 Euro). In the figure, sales prices are rounded down to the next 1,000 Euro. For example, a value of "400,000" includes all transactions within the price range of 400,000-400,999 Euro. For our definitions of residential real estate and commercial real estate, see Table 2.1.

variables that indicate if the respective price is a "round" number. To this end, we define dummy variables *D5000*, *D10000*, *D25000*, and *D50000* that are equal to 1 if the given sales price is evenly divisible by 5,000, 10,000, 25,000, and 50,000, respectively, and 0 otherwise. Note that these indicators are not defined exclusively. For example, for a sales price of 400,000 Euro all the dummy variables would take the value of 1. Hence, adding up the coefficients on all dummy variables gives the overall effect of being a round number on the number of sales at this price. We control for the overall distributional shape of sales prices by following Pope, Pope, and Sydnor (2015) and by including the seventh-order polynomial of the respective sales price in the regressions. Finally, there may be a concern that, for very high sales prices, almost all transactions might take place at round prices. For example, this could be driven by the fact that in "the relatively high" price ranges, 50,000 Euro might seem the natural increment for price variations. For this reason, in the main analysis, we restrict attention to transactions with sales prices weakly below 1,025,000 Euro, which also serves as the upper bound in Figure 2.1.[9] The results discussed in the following are robust and do not depend on this restriction (see Section 2.3.3 below).

The results of these regressions are reported in Table 2.2. In particular, Column (1) corresponds to the main specification in Pope, Pope, and Sydnor (2015), and it confirms the findings depicted in Figure 2.1, Panels (a) and (b). There are significant increases in the number of transactions at sales prices that are multiples of 5,000 and 50,000. However, there is no additional effect for sales prices that are multiples of 25,000. Hence, market participants do not seem to behave differently at these prices compared to other 5,000-multiples. Rather, Column (2) supports the visual impression from Figure 2.1, and there are significant spikes at prices evenly divisible by 10,000. For this reason, in all subsequent regressions, we additionally include *D10000* as an explanatory variable.

In the next step, as set out in Column (3) of Table 2.2, we investigate whether round-number prices "pull mass", i.e., whether they attract transactions that would otherwise occur at prices slightly lower or slightly higher than the round-number price under consideration. As such, we focus on sales prices that are multiples of 50,000, and we define two additional dummy variables. In line with Pope, Pope, and Sydnor (2015), *Dbelow50000* (*Dabove50000*) is equal to 1 if the respective sales price is at least 2,000 Euro but at most 7,000 Euro below (above) a price divisible by 50,000, and 0 otherwise. Analogous to Pope, Pope, and Sydnor (2015), Column (3) reveals that the findings on

divisible by 1,000.

[9]Recall from Table 2.1 that the 90th percentile of sales prices in residential real estate transactions is 490,000.

Table 2.2. Regression Analysis of the Number of Transactions Across Sales Prices

	Residential Real Estate			Commercial Real Estate	
	(1)	(2)	(3)	(4)	(5)
D5000	4414.52***	2992.13***	3009.45***	1430.67***	1438.67***
	(195.60)	(256.55)	(260.16)	(87.46)	(88.66)
D10000		2844.78***	2810.15***	2382.67***	2366.67***
		(345.94)	(356.24)	(117.93)	(121.40)
D25000	-637.34	785.65	751.15	578.21***	562.28***
	(530.26)	(542.02)	(548.87)	(184.78)	(187.04)
D50000	5732.07***	2886.88***	2921.76***	3212.02***	3228.21***
	(714.73)	(773.98)	(779.11)	(263.86)	(265.50)
Dabove50000			-132.18		-76.53
			(217.74)		(74.20)
Dbelow50000			-3.40		14.63
			(221.96)		(75.64)
7th Order Price Polynomial	yes	yes	yes	yes	yes
Adjusted R-Squared	0.69	0.71	0.71	0.83	0.82
Observations	1025	1025	1025	1025	1025

Note: This table reports OLS regressions. The dependent variable is the number of transactions at a given sales price, where the analysis is restricted to sales prices that are evenly divisible by 1,000 and that are weakly below 1,025,000 Euro. Standard errors are given in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

the special appeal of round-number prices are robust. At the same time, the coefficients on *Dabove50000* and *Dbelow50000* are negative, but not statistically significant. They are also not jointly significant according to an F-test.[10]

We gauge the economic significance of the round-number effects documented in Table 2.2 by considering the example of a sales price of 400,000 Euro. When neglecting the round-number dummy variables, Column (2) predicts 1,617 sales at this price, where this number is calculated by using the (unreported) coefficients on all the other right-hand side variables. However, as 400,000 is evenly divisible by 50,000, 25,000, 10,000, and

[10] The insignificance of the *Dbelow50000* dummy might be driven by "charm prices", i.e., prices that are just below some prominent threshold (such as a round-number price) pulling mass in their own right. This is a pattern that seems to be visible in Figure 2.1. A left-digit bias on the part of buyers might foster sales at such prices (see e.g., List, Muir, Pope, and Sun, 2023; Meng, 2023, for recent evidence on the power of charm pricing). The insignificance of the *Dabove50000* dummy might be driven by the conditions prevailing in the market in Germany in the period under consideration, which might be characterized as a seller's market. As illustrated by Figure A1 in the Appendix, prices for residential real estate steadily increased in Germany from 2003 to 2022. Moreover, our data contain annual, county-level vacancy rates for housing units for the period 2010-2020. This reveals a rather low average vacancy rate of 3.81% but also very little variation; the standard deviation is 1.72, and the 90th percentile is 5.53. Given these observations, the pull factor of round (but lower) prices (which are less favorable for sellers) might have been reduced.

5,000, Column (2) predicts 9,510 additional sales (i.e., $2,887 + 786 + 2,845 + 2,992$) at this price, constituting a 588% increase.

To summarize, our analysis in Section 2.3.1 extends Pope, Pope, and Syndor's (2015) findings on residential real estate transactions to the case of Germany. One notable exception is the lack of clustering on prices that are multiples of 25,000, consistent with quarters being less of a relevant metric in Germany.

2.3.2 Commercial Real Estate Transactions

In this section, we investigate whether round-number effects also play a role in commercial real estate transactions, where market participants are arguably different and where transaction values can be substantially higher than in residential real estate transactions (see Table 2.1).

As discussed in the Introduction, task frequency and stake size might influence behavior. Hence, it could be that professional market participants who frequently engage in commercial real estate transactions rely less heavily on round-number prices as focal points in negotiations (because, for example, they have a clearer, more precise grasp of the value of an object).[11] Hence, whether round-number effects also play a role in commercial real estate transactions is an empirical question and does not immediately follow from the findings of Section 2.3.1.

However, Panels (c) and (d) of Figure 2.1 clearly illustrate that, also for the case of commercial real estate, transactions are clustered at sales prices that are multiples of 5,000, 10,000, and 50,000. Multiples of 25,000 do not create particularly pronounced spikes. These observations are confirmed in the regression analysis reported in Table 2.2. There, Columns (4) and (5) qualitatively yield the same results as Columns (2) and (3). The only difference is that the *D25000* dummy is now significant, but relative to the other dummies its effect is small. Similar to the discussion of the case of residential real estate, consider a sales price of 400,000 Euro. When neglecting the dummy variables, Table 2.2, Column (4), would predict 1,294 sales at this price. However, because 400,000 is evenly divisible by 50,000, 25,000, 10,000, and 5,000, Column (4) suggests 7,604 additional sales (i.e., $3,212 + 578 + 2,383 + 1,431$), which would constitute an increase by 588% (which is the same percentage value that was obtained in the case of residential real estate).

[11] Also, given their greater experience and the higher stakes, it could well be that professional market participants are less prone to a behavioral round-number bias. High-stakes commercial real estate transactions might also involve negotiating teams, and there is evidence that, compared to individuals, teams are less prone to some behavioral biases. For a survey of the literature on team decision-making, see e.g., Kugler, Kausel, and Kocher (2012).

2.3.3 Robustness

Tables A1 and A2 in the Appendix replicate the analysis reported in (main) Table 2.2 for all prices weakly below 2,300,000 Euro and for all prices, respectively. Both tables qualitatively confirm the main findings. As an illustration, see Figure A2, which replicates Figure 2.1 for higher price ranges. One might in principle worry that new objects are constructed to have a round-number value. We have thus also examined the subset of transactions of objects that, at the time of sale, were more than ten years old. Qualitatively, this analysis yields the same results as those reported in Table 2.2.

2.4 Residential and Commercial Real Estate Transactions: Relative Importance of Round-Number Effects

Given that, in Section 2.3, we have established that, both in residential and commercial real estate transactions, substantial round-number effects are present, the question arises in which of these market segments this phenomenon is more prevalent. The back-of-the-envelope calculations in Section 2.3.1 and 2.3.2 suggest that, at a sales price of 400,000, the round number effects in residential and commercial real estate transactions are equally strong at 588%. An important caveat is that the results of this exercise only hold for this specific sales price. Moreover, this analysis does not take into account that the categories of residential and commercial real estate are comprised of quite different objects, in relation to which behavior might differ.

However, our data allow us to overcome this potential problem and investigate more systematically whether commercial real estate transactions are as prone to round-number effects as residential ones. For two reasons, the approach taken in the present section necessarily differs from that taken in Section 2.3. First, recall that in Table 2.2, the dependent variable is the absolute number of transactions at a given sales price. Hence, the finding from Table 2.1 that there are overall more residential than commercial real estate transactions implies that the coefficients of Columns (2) and (4) of Table 2.2 are not directly comparable. Second, when comparing the effects of round-number prices in residential and commercial real estate, the shapes of the distributions of transactions across sales prices need to be taken into account. For example, in commercial real estate, transactions concluded at high prices constitute a greater share than in the residential real estate case (see e.g., the median sales prices as reported in Table 2.1). Arguably, the increments in which transactions at high prices are negotiated are more likely to fall into our round-number definition, and, as a consequence, it would be more likely to

Table 2.3. Characteristics of Objects

	Family Homes		Condominiums (Owner-Occupied)		Condominiums (Let)	
	Mean	SD	Mean	SD	Mean	SD
Location	2.87	1.07	2.24	0.62	2.39	0.68
Features	2.82	1.09	2.33	0.55	2.41	0.58
Condition	2.69	1.07	2.16	0.52	2.23	0.59
Marketability	2.71	0.59	2.60	0.58	2.68	0.65
	Mean	90^{th} Percentile	Mean	90^{th} Percentile	Mean	90^{th} Percentile
Living Area (in sqm)	340	226	103	128	109	130
Year of Construction	1982	2016	1982	2018	1966	2009
Lot Size (in sqm)	804	1017				

Note: This table reports on additional characteristics of the objects, which are only available for family homes and condominiums. Prior to a transaction, these objects are assessed by a specifically trained appraiser as mandated in the contract between *vdp research GmbH* and participating banks. On a six-point Likert scale (where 1 denotes "very good" and 6 denotes "disastrous"), these appraisers evaluated the quality of each object's location, features, condition, and marketability. For family homes, information on marketability is only available for a subset of 184,544 out of a total of 1,788,824 transactions. "Living Area (in sqm)" and "Lot Size (in sqm)" are measured in square meters. "Lot Size (in sqm)" denotes the size of the lot on which the respective object is located; this information is available for family homes only. In addition to means, for "Location", "Features", "Condition", and "Marketability" the table reports standard deviations (SD), while for the remaining three variables, it reports 90th percentiles (as in these cases standard deviations are not informative due to outliers).

observe round-number prices in commercial real estate transactions.

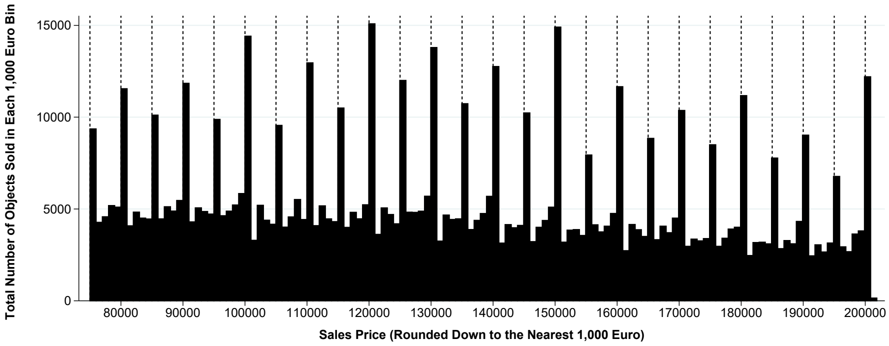
We can get around these issues because our data allow us to compare the role of round-number prices for commercial and non-commercial transactions for the same type of real estate object. Specifically, for the sale of condominiums, we know whether an object is bought to be occupied by the owner (which we label "owner-occupied") or bought to be let (which we label "let"). As a result, our assumption is that buyers who acquire a condominium as an investment (which we interpret as a commercial motive) are presumably more professional and more experienced market participants than buyers who are looking for a home (which we interpret as a residential motive).[12]

The sets of condominiums that are owner-occupied and that are let do not differ substantially with respect to the distributions of their sales prices, where the 10%, 50%, and 90% percentiles are given by 66,000 (53,648), 165,103 (130,445), and 400,000 (410,000) for owner-occupied (let) properties; see Table 2.1. For family homes and condominiums,

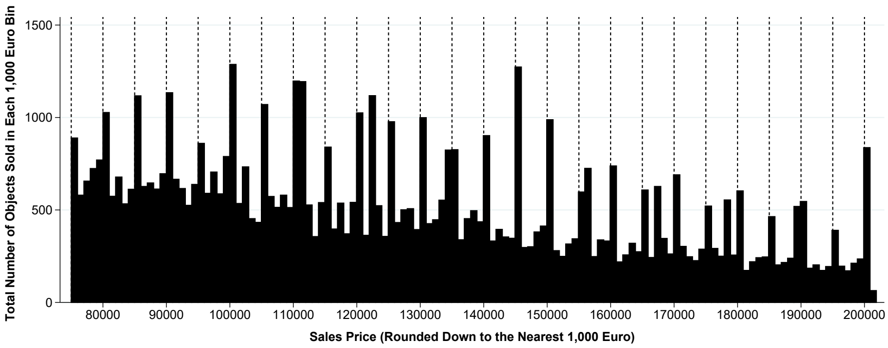
[12]Recall the discussion from the Introduction that, among developed countries, Germany has the lowest homeownership rate and very low turnover rates for houses and condominiums.

Figure 2.2. Number of Transactions of Condominiums Across Sales Prices

(a) Condominiums: Owner-Occupied



(b) Condominiums: Let



Note: The figure displays the number of transactions of condominiums across sales prices. Panels (a) and (b) focus on objects that are acquired to be occupied by the buyer and objects that are bought to be let, respectively. For illustrative purposes, the price range is restricted to 75,000 - 200,000 Euro. Sales prices are grouped into 1,000 Euro bins (rounded down).

we also have data on additional object characteristics; see Table 2.3.

In addition to information on the living area and year of construction, information on quality is included: before its sale, each of these objects is assessed by specifically trained appraisers, who (on six-point Likert scales) evaluate the quality of the property's intra-regional location, features, condition, and marketability.[13] As Table 2.3 suggests, on average, condominiums that are let are somewhat older, in better condition, and slightly larger (in square meters) than condominiums that are owner-occupied.

Turning to the relative prominence of round-number prices, Figure 2.2 displays the number of transactions across a range of sales prices of condominiums that are acquired

[13]This is mandated by the contractual agreement between *vdp research GmbH* and participating banks. For family homes, we also have information on the size of the lot on which the building is located.

Table 2.4. Condominiums: Regression Analysis of Transactions at Round-Number Prices

	Price Evenly Divisible by 5,000			Price Evenly Divisible by 50,000		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.355*** (0.000)	0.297*** (0.001)	0.304*** (0.001)	0.051*** (0.000)	0.028*** (0.000)	0.029*** (0.000)
Commercial	-0.103*** (0.001)	-0.094*** (0.001)	-0.177*** (0.003)	-0.014*** (0.001)	-0.010*** (0.001)	-0.026*** (0.001)
Price in 100,000 Euro		0.034*** (0.000)	0.030*** (0.001)		0.014*** (0.000)	0.013*** (0.000)
Commercial × Price in 100,000 Euro			0.058*** (0.002)			0.011*** (0.001)
Adjusted R-Squared	0.004	0.008	0.009	0.000	0.004	0.004
Observations	1,429,097	1,429,097	1,429,097	1,429,097	1,429,097	1,429,097

Note: This table reports OLS regressions. The analysis is restricted to observations where the sales price is weakly below 400,000 Euro, which corresponds to the 90th percentile of all condominiums. The dependent variable is an indicator that is equal to 1 if the respective transaction (which is the unit of observation) takes place at a price that is evenly divisible by 5,000 (see Columns (1)-(3)) or evenly divisible by 50,000 (see Columns (4)-(6)), and 0 otherwise. Standard errors are given in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

to be occupied by the buyer (Panel (a)) or bought to be let (Panel (b)).[14] Two preliminary observations can be made. First, the number of transactions is substantially higher in Panel (a) than in Panel (b); that is, more condominiums are bought for occupation than to be rented out. Second, comparing the panels suggests that while round-number effects play a role in both categories, they seem to be more pronounced for condominiums where the transaction has a residential motive. That is, in Panel (a) prices that are evenly divisible by 5,000 clearly stick out, while in Panel (b) the picture is less clear-cut.

This is confirmed by a regression analysis; see Table 2.4. Thereby, the dependent variable is a dummy variable that indicates whether the respective transaction takes place at a round-number price. In particular, this dummy variable is equal to 1 if the respective sales price is evenly divisible by 5,000, and 0 otherwise. That is, in these regressions, the unit of observation is an individual transaction in order to be able to control for the transaction having a commercial or a residential motive.[15] To this end, we define a dummy variable *commercial* that is equal to 1 if the condominium is acquired with the purpose of being let to somebody else, and 0 otherwise. In Table 2.4, we restrict

[14]For the purpose of illustration, Figure 2.2 restricts attention to an intermediate range of sales prices between 75,000 and 200,000 Euro (see Table 2.1).

[15]Recall that in the regressions of Table 2.2 the unit of observation were *all* transactions taking place at a given price.

our attention to transactions that take place at prices that are weakly smaller than the 90th percentile (400,000 Euro) of all transaction prices of condominiums.[16] We estimate linear probability models where the dependent variable is regressed on a constant and on the variables *commercial* (see Column (1)), *commercial* and the sales price measured in units of 100,000 Euro (see Column (2)), and *commercial*, the sales price measured in units of 100,000 Euro, and an interaction term (see Column (3)).

Column (1) of Table 2.4 reveals that 35.5% of the residential transactions take place at a price that is evenly divisible by 5,000. For commercial transactions, this fraction drops significantly to 25.5%; that is round-number prices play a lesser role in these transactions. This finding is confirmed in various robustness checks. Specifically, in Column (2), we additionally control for the sales price. We do this because one could hypothesize that for higher price ranges, it might be more likely that objects are traded at round-number prices simply because the increments by which prices are adjusted in the process of bargaining become larger. The respective coefficient is indeed positive and significant, but the coefficient on *commercial* is basically unaffected.[17] In Column (3), we also include an interaction term of *commercial* and the sales price to investigate whether the differential inclination to trade at round-number prices is more pronounced at higher or lower prices. The positive coefficient on the interaction term implies that it is only at relatively high prices that there are similar shares of residential and commercial transactions at round-number prices. Columns (4)-(6) replicate the analysis of Columns (1)-(3), where the dependent variable is now a dummy that is equal to 1 if the respective sales price is evenly divisible by 50,000, and 0 otherwise. The earlier results are confirmed. For example, Column (4) indicates that 5.1% of the residential transactions are concluded at a price that is evenly divisible by 50,000. For commercial transactions, this drops by about a quarter to 3.8%. To summarize, our analysis of the sales of condominiums suggests that round-number effects play a substantially smaller, but still sizable, role in commercial compared to residential transactions.

[16] This is meant to eliminate the influence of outliers.
[17] Qualitatively the same result obtains when including the seventh polynomial of the price.

2.5 By How Much Does the Appeal of Round Numbers Affect Prices?

In Section 2.3, we have documented that many residential and commercial real estate sales are concluded at round-number prices. In the present section, we aim to assess the strength of the “pull” of round numbers in relation to price formation (i.e., we are interested in the difference between transaction prices and the hedonic values of objects that trade at round-number prices). We conduct this exercise separately for three categories of objects for which our data contain information on objects’ characteristics: family homes (i.e., Categories (1) and (2) of Table 2.1) and the two types of condominiums (owner-occupied and let); see Table 2.3.[18] We find that, on average, objects that trade at a round-number price are sold at a premium.

We proceed in three steps, each explained in greater detail below. In a first step, we use the information on the characteristics to estimate hedonic values of objects that trade at prices that are not round numbers. Thereby, we rely on OLS regressions (but our results are robust when employing regression trees instead). In the second step, we use the regression coefficients obtained in the first step to predict the hedonic values of objects that trade at round-number prices. In the final step, the residuals from the latter exercise (i.e., the difference between predicted and actual values) give an indication of the “pull” of round-number prices.

For the hedonic regression of the first step, we restrict attention to the “non-round subsample” of the object category under consideration. That is, for the estimation of the regression coefficients, we exclude transactions with sales prices that lie within a neighborhood of 7,000 Euro of prices evenly divisible by 50,000 (where this threshold value is in the spirit of the definition of the neighborhood dummies in Section 2.3.1). For example, for the round-number price of 350,000 Euro, we exclude all transactions with sales prices in the interval between 343,000 and 357,000 Euro. For this subsample, we regress the log of the sales price on (i) living area (in square meters), (ii) age (i.e., the difference between year of transaction and year of construction), (iii) age squared (to control for well-known non-linear price effects of age), (iv) transaction-year dummies (to control for changes in the price level), (v) county dummies (to control for the object’s interregional location), (vi) (intra-regional) location, features, condition, and marketability dummies (see Table 2.3), (vii) a constant, and (viii) in the case of family homes, we also control

[18] Again, to reduce the influence of outliers, we only consider transactions taking place at prices weakly below the respective 90th percentile (see Table 2.1).

for lot size (in square meters).[19] Table 2.5 reports the number of observations in these regressions; note, in the non-round subsample the average prediction error (i.e., residual) is zero by construction.

Table 2.5. Average Prediction Errors in the Round-Number Subsamples

	Family Homes	Condominiums (Owner-Occupied)	Condominiums (Let)
Non-Round Subsample			
Number of Observations	99,181	550,124	76,512
R-Squared	0.55 (0.68)	0.38 (0.81)	0.38 (0.79)
Round-Number Subsample			
Number of Observations	7,506	37,585	4,229
Average Prediction Error	0.0806 (0.0698)	0.0910 (0.0205)	0.2228 (0.0670)

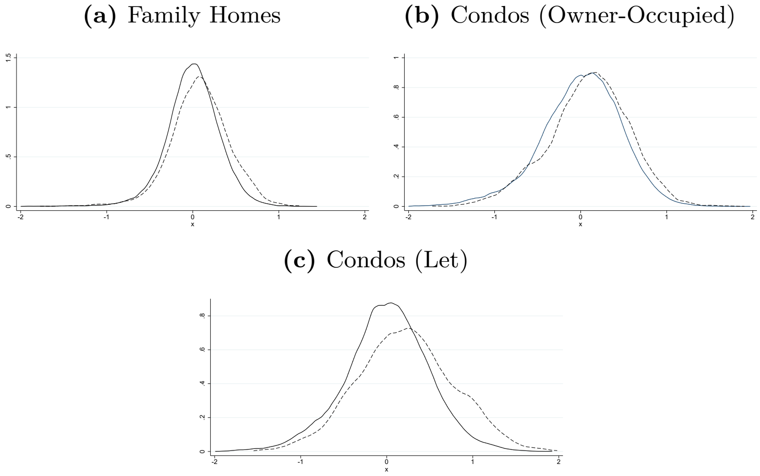
Note: In the non-round subsamples, average prediction errors are zero by construction. The entries pertain to the results of OLS regressions. The entries in brackets obtain, when instead of OLS, we employ regression trees (assuming a complexity parameter $cp = 0.00001$). The dependent variable in the regressions is the log of the sales price. When using OLS, we report the adjusted R-squared.

We then use the coefficients obtained from the above regressions in the non-round subsamples to predict prices for the observations in the "round-number subsamples" (i.e., all transactions at prices that are evenly divisible by 50,000); we thus assume that the hedonic contributions of the right-hand side variables are the same in both subsamples.

Figure 2.3 displays the distributions of the prediction errors (i.e., the differences between actual and predicted values) in the non-round subsample (solid line) and round-number subsample (dotted line). In each of the cases, the average prediction errors in the non-round subsamples are virtually zero, whereas in the round-number subsamples, these are tilted rightwards. In fact, in all three cases the average prediction errors in the round-number subsamples are strictly positive and significantly different from zero (with $p < 0.001$ according to Welch's t-tests), i.e., on average these objects are sold at a premium. For example, family homes in the round-number subsample are, on average, sold at a price that is roughly 8% above the predicted value; see Table 2.5. For a family

[19] Hence, in the analysis, we restrict attention to observations for which the variable "marketability" is available as this improves the fit of the model substantially. Results are qualitatively unchanged when dropping the variable marketability and using all observations instead. Similarly, using the price level instead of the log of the price yields qualitatively similar results.

Figure 2.3. Distributions of the Prediction Errors in the Non-Round Subsample (Solid Line) and the Round-Number Subsample (Dotted Line)



(a) Family Homes **(b)** Condos (Owner-Occupied) **(c)** Condos (Let)

home sold at 300,000 Euro, this would imply a premium of about 24,000 Euro.[20] The observation that round-number prices are, on average, higher than their predicted values is consistent with the fact that in the sample period sellers were arguably in a better bargaining position; see the discussion in Footnote 10.

2.6 Conclusion

The study in this chapter considers a large sample of the German market to study the role of round-number prices in (high-stakes) real estate transactions. With respect to residential real estate, we show that the findings in 2015 for the US can be extended to the German market; there is substantial clustering of transactions at salient round-number prices. We also find pronounced round-number effects in commercial real estate markets, where stakes are even higher and market participants more experienced. When directly comparing the behavior of professionals and non-professionals for the same type of object (i.e., condominiums), it turns out that, compared to non-professionals, the fraction of transactions that professionals settle at round-number prices is significantly lower but still substantial.

In the case of family homes and condominiums (for which additional information on

[20] If we use the level of the sales price (instead of its log) as dependent variable, the average prediction error in the round-number subsample is 20,056 Euro ($p < 0.001$, Welch's t-test). The result is also qualitatively robust if the variable "marketability" is dropped from the regression, which substantially increases the number of observations. In this case, the corresponding percentage value is 6.26% ($p < 0.001$, Welch's t-test).

objects' characteristics is available), we obtain evidence that suggests that objects sold at round-number prices trade at a premium of 2-7% relative to their predicted value. That is, the appeal of round numbers seems to have a substantial positive price effect. In principle, this could be driven by the upward trend in Germany's real estate prices in our sample period, which might allow sellers to suggest focal points as basis for a more favorable agreement.

Finally, our analysis documents that what is salient in bargaining (and hence, influences the bargaining outcome) seems to depend on culture. In particular, in Germany, "quarters" (for example in coinage or as a unit of measurement) do not play a particularly prominent role. This seems to be reflected in the finding of a lack of pronounced clustering at prices that are evenly divisible by 25,000 in the German real estate market.

3

Round-Number Effects in Bargaining: Bias vs. Focal Point[1]

3.1 Introduction

It is a well-established fact that there is a variety of biases when it comes to numbers. There is ample evidence of left-digit bias (Busse, Lacetera, Pope, Silva-Risso, and Sydnor, 2013; Englmaier, Schmöller, and Stowasser, 2017; Lacetera, Pope, and Sydnor, 2012), the role of prominent numbers in decision processes (Converse and Dennis, 2018), and clustering of prices at round numbers in the real-estate market (Pope, Pope, and Sydnor, 2015; Repetto and Solís, 2020) and the energy market (Shah and Lisi, 2020; Ziel and Steinert, 2016). There is a growing body of literature that recognizes the importance of precise ($1.67) and round numbers ($2.0) in decision-making. In particular, initial offers play an important role as anchors in the bargaining literature (Janiszewski and Uy, 2008; Loschelder, Stuppi, and Trötschel, 2014). Mason, Lee, Wiley, and Ames (2013) report making precise offers is a signal of being more informed compared to someone making a round offer. Hukkanen and Keloharju (2019) even advise not to initiate a bargaining process with a round number. Yan and Pena-Marin (2017) argue that round number offers signal "completion" and "goal achievement" and are consequently linked to a higher acceptance propensity.

We study the role of round numbers in bargaining situations and whether their effect can be explained by preferences for round numbers (*round-number bias*) or by their role as a solution for a coordination problem (*focal point*). We hypothesize that faster decisions and higher acceptance frequencies result from a round-number bias, focal

[1] This chapter is based on Lauf and Schlereth (2022).

points, or both. To investigate this, we provide first empirical evidence of a negative correlation between the duration of a bargaining process and the usage of round price offers. For this finding, we exploit the vast data set of Backus, Blake, Larsen, and Tadelis (2020) which covers over 11 million observations of eBay bargaining protocols with the so-called *Best Offer* option enabled. Our finding is robust and highly significant but does not allow for a causal interpretation of the role of round numbers.

For this purpose, we design an online experiment that incorporates a dynamic bargaining game. We record the decisions of participants to accept or reject a random offer that might be round or non-round. Based on our first treatment, we obtain a measure for the preference for round numbers as we eliminate any dependency on the decisions of others for the participant. In the second treatment, we keep the incentive structure, but additionally introduce a coordination problem where the effect of the participant's decisions now depends on the decisions of a second participant. Comparing the first and second treatment sheds further light on the effect of round-number bias or the role of round numbers as focal points in the spirit of Schelling (1960). In our framework, this means that despite potentially having no round-number bias, participants might accept round offers because they believe their partners are more likely to accept round offers. We made sure that there is no reason for the round-number bias to be different across treatments, other than their possible role as tools for coordination.

Our experiment confirms the finding from the eBay data set that round offers facilitate faster acceptance. In addition, we find clear evidence of a round-number bias. Participants are more likely to accept round offers. Finally, we do find evidence for round numbers serving as focal points, but only under certain conditions. We find significant increases in acceptance frequencies of round numbers for female participants under coordination for less advantageous offers. No such effect can be seen for more advantageous offers. The offer's advantageousness is determined by its potential payoff for the participant. For male participants, the patterns look similar, but are less pronounced and non-significant.

This project contributes to various strands of the literature related to negotiations, focal points, the salience of roundness, and round-number bias. In general, negotiations are instruments to find solutions for disagreements in various fields, such as trade, politics, and social life. We are interested in the potential role of round numbers in price negotiations. Here, round numbers might serve as focal points to reach agreements as introduced by Thomas Schelling. He argues that focal points constitute a solution not necessarily depending on logic but frequently on prominence or conspicuousness. When numbers are involved, outcomes show a strong tendency towards simplicity in the form of roundness (Schelling, 1960).

The first strand of literature investigates negotiations in an experimental setting. Recent evidence supports Schelling's argument that payoff-irrelevant but conspicuous labels for players' strategies, e.g., "Option A" or "Option B", facilitate coordination in tacit bargaining, that is, situations in which communication is not possible. Coordination games can model such situations. For example, they may involve two players choosing a strategy from a common set and receiving a payoff only when they chose the same strategy (Mehta, Starmer, and Sugden, 1994b). The literature found saliently labeled strategies can serve as focal points in one-shot coordination games (Bardsley, Mehta, Starmer, and Sugden, 2010; Crawford, Gneezy, and Rottenstreich, 2008; Mehta, Starmer, and Sugden, 1994a; Parravano and Poulsen, 2015). We embedded a form of a coordination game in our second treatment as participants must accept the same offer. Our design differs through its dynamic character and the fact that we place label and payoff in the same domain. The number 10 might not only be the number that lies equidistant between 9 and 11, but could also serve as a salient label.

Besides the salience of labels, other features and payoff asymmetry may influence coordination. For example, Isoni, Poulsen, Sugden, and Tsutsui (2013) introduce the *bargaining table* game and study spatial salience. They find that focal points increase efficiency in a tacit bargaining game, even when these cues induce unequal payoff divisions. Isoni, Poulsen, Sugden, and Tsutsui (2019) find that salient labeling increases coordination success but document minor and major disruptive effects of payoff inequality and conflict of interest. In our experimental design payoffs are symmetrical for now, but our design allows to easily incorporate asymmetric payoffs to further study its impact on coordination.

Empirical work has shown that the visual representation of numbers influences the outcome of economic decision-making. A prominent example is the literature on the left-digit bias. Lacatera, Pope, and Sydnor (2012) provide evidence for a left-digit bias of consumers in the wholesale used-car market. They report threshold effects at 10,000-mile odometer marks resulting in discrete price drops. Moreover, Busse, Lacetera, Pope, Silva-Risso, and Sydnor (2013) analyze retail data on used cars and arrive at the same conclusion. Finally, Englmaier, Schmöller, and Stowasser (2017) also report price discontinuities at salient mileage thresholds for the European market and extend the analysis to the age of the car. One possible explanation of these findings is an overestimation of the distance to the next round mileage when cognitive constraints result in only the leftmost digits being processed. For example, a car with 20,000km is perceived as far less valuable than one with 19,999km. We add to this strand of the literature by studying visually salient round numbers and their effect on decision-making in negotiations.

For this purpose, we conducted an empirical analysis of a recently published data set from a well-known platform - eBay. Besides auctions eBay offers a platform for sequential bargaining which is named “Best offer”. Backus, Blake, Larsen, and Tadelis (2020) collected a data set of eBay transactions and listings. Their study focuses on comparing their results from the data with theoretical predictions from the bargaining literature. In Backus, Blake, and Tadelis (2019), they utilize the same data, and find evidence for cheap-talk signaling in the use of round number listing prices and offers. They document a trade-off. Round-number offers are on average lower, but are more likely to sell. In our analysis, we focus on how round numbers affect the duration of negotiations.

There does, however, also seem to be a perceptional difference between round and non-round numbers, even in the absence of stressful situations. The psychological literature has long recognized the relevance of how numbers are presented, i.e., whether they are *round* or *prominent* (Converse and Dennis, 2018). Rosch (1975) finds that such round numbers serve as reference points in lab settings. Empirical findings for marathon runners (Allen, Dechow, Pope, and Wu, 2017), baseball, SATs, lab experiments (Pope and Simonsohn, 2011), and preventive health behavior (Wadhwa and Zhang, 2019) support this. Converse and Dennis (2018) provide evidence for round number effects in financial market data and experiments. In five studies simulating real-world scenarios, such as buying coffee or selling a textbook, Yan and Pena-Marin (2017) discover that offers’ roundness increases the willingness to accept in experimental bargaining in line with their hypothesis that round numbers symbolize completion. Lin et al. (2020) analyze 2,000 classroom experiments where the simplest form of bargaining, an ultimatum game, was conducted and find clear spikes at offers that are multiples of 10. Our approach provides another perspective, covering cooperation and focal points. We contribute a novel design focusing on the decision to accept an offer without the influence of scenario-based stimuli or communication between subjects.

A synthesis of the strands of literature on focal points and round numbers is the study of Pope, Pope, and Sydnor (2015). They find evidence in support of round numbers serving as focal points in high-stake real estate negotiations. Still, they raise the question, to which extent a round-number bias or the role as focal point is responsible for the relevance of round numbers in negotiations. Our project addresses these questions by providing a novel experimental framework to make a clearer distinction.

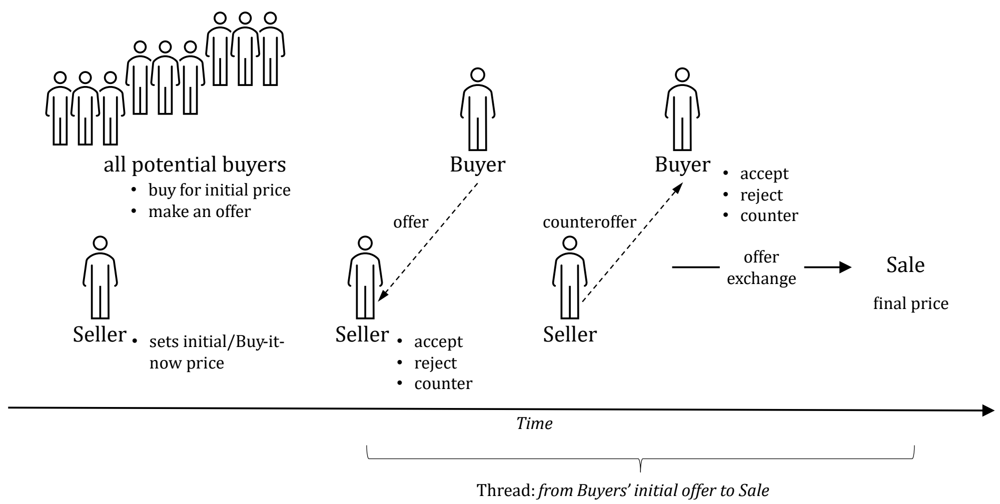
The remainder is structured as follows. Section 3.2 discusses our empirical analysis of the eBay data set and the motivation for our experimental design, which we present in

Section 3.3. In Section 3.4, we present our experimental results. Section 3.5 concludes.

3.2 Empirical Evidence from eBay

It is a common phenomenon that in real-world negotiations such as buying a used car or when participating in auctions and garage sales, the final prices surprisingly often tend to be round. We were interested if this tendency is also associated with other outcomes of the bargaining process, such as the acceptance frequencies and the swiftness of negotiations. We found such a relationship, for which we provide the empirical evidence in the following. For this purpose, we use the data from Backus, Blake, Larsen, and Tadelis (2020) who made available millions of records of single-unit fixed-price listings from May 2012 to June 2013 on the US eBay site, where the "Best Offer" option was enabled.

Figure 3.1. Illustration of the "Best Offer" Option on eBay.



The procedure on eBay illustrated in Figure 3.1 is as follows. First, a seller sets an initial price for the item to be sold, also called the "Buy-It-Now" price. Then, all potential buyers can instantly buy the item for this price or, if the "Best Offer" option is enabled, send the seller an alternative price offer. Next, the seller can accept or reject this offer or make a counteroffer giving the buyer the same possibilities. Both can make at most three offers, and each is valid for 48 hours. Finally, the item is sold for the final price if both parties agree. Otherwise, the negotiations fail.

Backus, Blake, and Tadelis (2019) document that using round numbers is associated with lower prices for the seller as argued in the literature (Hukkanen and Keloharju, 2019; Janiszewski and Uy, 2008; Loschelder, Stuppi, and Trötschel, 2014; Mason, Lee, Wiley, and Ames, 2013). However, Backus, Blake, and Tadelis (2019) argue that past research has ignored an important trade-off: a round price may come at the benefit of a higher likelihood of a sale. They use their own extensive data set and show for 10.5 million listings, that a round initial price, in the form of multiples of \$100, increases the likelihood of a sale by 3%-points to 6%-points while a share of 20% of all listings is sold on average.[2]

We focus on a different aspect by analyzing the role of round numbers within successful negotiations instead of the signaling effect of initial prices. We find that the share of round numbers increases from 15.4% of the initial prices to 41.2% of the final prices. This might be driven by a round-number bias, but could also be due to round numbers being used as focal points to accelerate a settlement within the bargaining process. If round final prices are indeed associated with faster settlements, this will provide evidence for either or a combination of both.

To this end, we use the two data sets provided by Backus, Blake, Larsen, and Tadelis. The first data set consists of *thread*s, which are sequences of offers for one buyer-seller pair bargaining over one item as well as their responses, as shown in Figure 3.1. Hence, one thread consists of multiple observations, but the last (most recent) observation covers the bargaining outcome and the price for which the bargaining parties settled. The second data set holds information on the items within the threads of the first data set, such as its condition and the category it falls within. We developed an algorithm that processed and merged both data sets. In particular, we collected the duration of each thread, its final price, and additional information on the sold item in a new data set. The duration is the time passed between the first and last observation within one thread, and we matched it with details on the corresponding item. Appendix B.1 provides more information about our algorithm. In total, we collected 11.1 million threads and Table 3.1 summarizes the data.

For our empirical analysis of bargaining times, we introduce the following notation. Let i be the identifier of a successful thread, where the seller sold the item for the final price p_i. The thread's duration is captured by Δt_i and represents the time between the buyer's initial offer and the last observation in the thread, which either is the automatic or the manual acceptance of the seller or the manual acceptance of the buyer. The set of round prices is denoted by Υ, and $I_\Upsilon(p_i)$ is an indicator function that is equal to 1 if the

[2]The data set is restricted to *Collectibles* with an initial listing price between \$50 and \$550, where the round numbers are $z \in \{100, 200, 300, 400, 500\}$; more details in section IV.B.3 of Backus, Blake, and Tadelis (2019).

Table 3.1. Descriptive Statistics of the eBay Data

	Mean	Median	SD	Min	Max
Duration (min)	1,049.07	136.60	7,948.14	0.00	802,791.77
Periods	1.51	1.00	0.92	1.00	9.00
Final price ($)	81.11	29.00	142.98	0.99	1,100.00
Round numbers (final price)	0.41	0.00	0.49	0.00	1.00
Initial price ($)	118.84	39.99	3,307.96	0.99	6,000,000.00
Round numbers (initial price)	0.15	0.00	0.36	0.00	1.00
Number of photos	3.26	2.00	2.99	0.00	12.00
Seller's feedback score (%)	99.67	99.86	2.10	0.00	100.00
Observations	11,090,279				

Note: The table summarizes the eBay data set of Backus, Blake, Larsen, and Tadelis (2020) after applying our algorithm. The distribution of the items' conditions and categories can be found in Table B1 and Table B2.

final price is round, and 0 otherwise. Each thread relates to one item, and X_i collects any additional information on this item. We define *round numbers* as the 5-step intervals up to 50, followed by 10-step intervals up to 100 and extended by 50-step intervals up to 1000. In particular, let the set of round prices be given by

$$\Upsilon = \{5, 10, 15, ..., 45, 50, 60, 70, ..., 90, 100, 150, 200, ..., 950, 1000\}.$$

We estimate the model

$$\Delta t_i = \beta I_\Upsilon(p_i) + c + \gamma X_i + u_i, \tag{3.1}$$

where i denotes an observed successful thread, Δt_i is the duration measured in minutes, p_i represents the observed final price of the item, c is a constant, and X_i collects the condition of the item (11 categories, baseline is "New", see Table B1) and the meta category of the item (38 categories, baseline is "Collectible", see Table B2). We only use threads with final prices of up to $1100, covering 98.2% of the successful threads.

The resulting data set consists of 11.09 million threads with a successful sale, where the average initial listing price is $118.84, and only a share of 15.4% of the initial prices is round. So, sellers started roughly every seventh thread with a round price. The parties settled on average at a final price of $81.11, and intriguingly the share of round numbers increased to 41.2% of all final prices. The average duration of a thread is 1,049 min 4.5 s (SD: 7,948 min 8.34 s), but the median is 136 min and 36 s.

Table 3.2 shows the results of OLS regressions both without controls and with controlling for the item's condition and category. The effect of round numbers, captured in Equation (3.1) by β, is reported in the row *Round Numbers.* In all specifications, we find a significant negative relationship between the duration of a thread and the dummy indicating that the item was sold for a round price. In particular, on average threads ending with a round final price were 53 min shorter than threads without a round final price (see our preferred specification in Column (2)). In the Appendix, Table B3 lists the results broken down for all conditions and categories.

Table 3.2. Regression Results. Round Final Prices and the Duration of Successful eBay Sales

	(1) Duration	(2) Duration	(3) Periods	(4) Periods
Round numbers	-24.82***	-53.02***	-0.17***	-0.19***
	(4.91)	(5.21)	(0.00054)	(0.00064)
Constant	1059.31***	1165.92***	1.58***	1.59***
	(3.02)	(9.80)	(0.00038)	(0.0011)
Condition dummies	-	Yes	-	Yes
Category dummies	-	Yes	-	Yes
N	11,090,279	8,144,375	11,090,279	8,144,375

Note: The table reports OLS results for the two dependent variables, Duration and Periods. *Duration* denotes the time between the first observation and the last observation of a thread in minutes. *Periods* denotes the number of offers made between seller and buyer. The table reports the coefficient of the round number dummy as *Round numbers.* There are 11 condition dummies for the item, where the baseline is "New". The meta category of the item has 38 categories and is considered with a corresponding number of dummies, where the baseline is "Collectible". Missing observations are due to incomplete recordings of condition or category. Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

We assess the robustness of our findings by different checks. First, we use another measure for duration and replace Δt_i by the number of periods of the bargaining process, i.e., how often each party made an offer. We observe an average number of periods of 1.48 in the data. We confirm our previous result because when the final price is round, the number of periods is reduced by 0.19 on average, controlling for category and condition of the item (Table 3.2, Column (4)). Moreover, we applied a placebo test by shifting each element of Υ by 1 ahead, i.e., $\Upsilon + 1$. We find that the effect is insignificant in the duration case ($\hat{\beta} = -7.84(12.7)$, standard error in parenthesis) and the effect on the number of periods becomes very small ($\hat{\beta} = -0.005(0.0016)$), yet remains significant. We additionally checked the influence of available covariates in the data set such as initial listing price, number of item's photos, and the seller's feedback score in Equation (3.1), which did not change the results.

3.3 Experimental Design and Implementation

We illustrated the relevance of round numbers in a real-world bargaining setting, with the intriguing finding that the share of round numbers increases for successful negotiations, and that negotiations that end with a round number price are shorter. With our experiment, we want to answer the question if, and to what extent, the acceptance of round numbers is driven by individual behavioral biases or by round numbers serving as focal points in negotiations. An experiment trying to answer this question must possess some essential characteristics. We summarize the desirable properties in the following:

Properties of the Experimental Design:

1. **One-Player and Two-Player**: The design needs to be suitable for a one-player setup (where individual behavioral biases might kick in) and a two-player setup (where, additionally, focal points may play a role).

2. **One Change at a Time**: The extension to the two-player case must be possible with only one change at a time.

3. **No Communication**: The channel of making offers that could serve as signals needs to be closed. Bargaining typically involves making offers to others, receiving offers, and evaluating counter offers by another (human) party. Strategic considerations might induce round number effects already when making offers. Allowing participants to freely exchange offers would make an analysis of acceptance decisions rather difficult.

4. **Abstract Environment**: Context-specific restrictions on the offers need to be eliminated since the bargain's item or the environment presented in the study might determine a particular set of reasonable offers.

5. **Offer Size**: Round offers should not be more financially attractive than non-round offers.

6. **Upside of Rejection**: To avoid that subjects simply accept every offer, there must be some value in rejecting a given offer (i.e., try to get a better offer than the current one).

7. **Downside of Rejection**: At the same time, rejecting a given offer must be costly, so subjects cannot wait infinitely long (i.e., waiting incurs the risk of ending up worse than the current offer).

8. **Same Number of Decisions**: The design must allow eliciting the same number of decisions from each participant for the homogenization of the lengths. The reason is that negotiations generally end when both parties agree or break down when the continuation seems unattractive, leading to an inherent variation in the negotiations' lengths.

Hence, our experiment is necessarily somewhat abstract, but we argue that we have found a way to incorporate the most relevant characteristics of bargaining. Therefore, and in favor of clarity, we limit the following section, 3.3.1, to an outline of the procedure and postpone the detailed explanation of our design choices to the subsequent section, 3.3.2.

3.3.1 Experimental Design

The basic idea of our design is as follows: We randomly present each participant an offer from a pre-defined offer set. Then, participants can either accept or reject that offer. Accepting an offer might result in a payoff corresponding to the offer's size.

We have two treatments, **Single** and **Partner**. In Single, each participant completes 10 periods. Each period consists of two steps, *Preview* and *Decision*. For Period 1, Figure 3.2 shows the two screens that visualize the two steps. In the first step, Preview, participants see the offer set of the current period from which they will get a randomly drawn offer; see panel (a) of Figure 3.2. Each offer is equally likely to be drawn within one period. In the second step, Decision, the participants receive an offer from the set displayed in the previous step. Then, they decide whether to accept or reject it. Additionally, they can see the offer set of the next period; see panel (b) of Figure 3.2. After each period, the three largest offers are removed from the offer set. The next figure, Figure 3.3, illustrates how in Period 2, the first line, Line 1, is removed from the offer set. In Period 3, the next line, Line 2, is removed (*shrinking cake* design). This design was explained to participants in detail prior to the start of the experiment. A participant's payoff is the sum of the participation fee of \$1.50 and a bonus. For the bonus calculation, one period is randomly selected after the participants have completed the study. We call the selected period and all subsequent periods *payoff-relevant*. With this, the bonus equals the first accepted offer in a payoff-relevant period multiplied by the conversion rate, \$0.001. We added a hint about the dependency on the selected period on the Decision page; see panel (b) of Figure 3.2 just above the buttons. At the end of the study, participants can view their decisions and the selected period.

In Partner, everything remains the same except for the bonus calculation. We match

Figure 3.2. The Two Steps of Period 1.

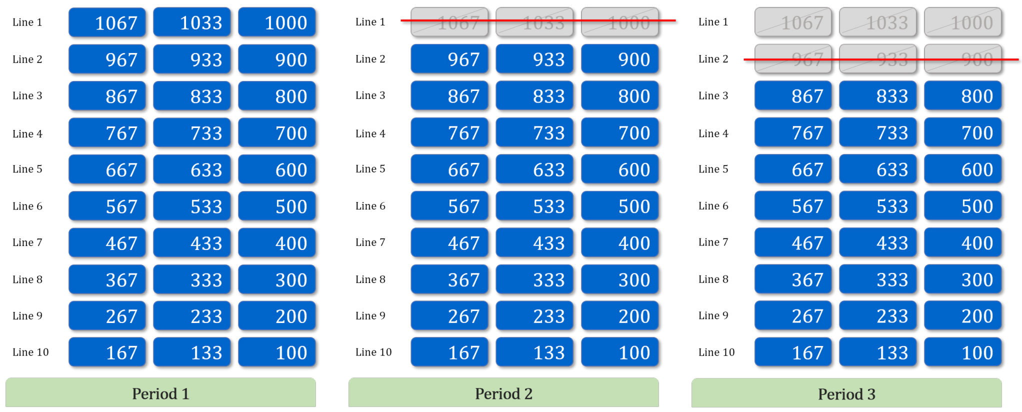
(a) Preview

(b) Decision

Note: The two steps of Period 1. The top panel (a) is the step Preview which shows participants the offer set of the current period. The bottom panel (b) displays the step Decision where participants can accept or reject the randomly drawn offer in the black rectangle. The right side displays the preview of the next period. A gray line marks the removed numbers, and the gray background highlights the removal. The subsequent periods have the same layout, except that additional lines are removed; see Appendices B.3.2 and B.3.3. Only three buttons ("Get your offer","Yes", "No") are clickable.

Figure 3.3. Offer Set for Period 1, Period 2 and Period 3.

(a) Period 1 **(b)** Period 2 **(c)** Period 3



Note: Offer set for Period 1, Period 2 and Period 3. All offers in blue boxes could be drawn with equal probability by the experimental software in the respective period. The green box at the bottom displays the current period. The removal of the numbered lines is illustrated by the gray line crossing out 3 numbers and is highlighted by the gray boxes.

each participant with another participant whom we call the participant's partner. Both see the same sequence of offers across the periods. As before, a randomly selected period specifies the participant's and partner's payoff-relevant periods. However, in Partner, the bonus is the first offer that both players individually accept multiplied by the conversion rate. In other words, we introduce a coordination game in each period. If no offer in a payoff-relevant period is accepted by both players, they each receive a zero payoff. The hint on the Decision page is adjusted so that it additionally includes the dependency on the partner's decision.[3]

In both treatments, participants made their decisions individually and privately. We did not allow any communication. In Partner, participants have no information about their partner's identity or decisions. We made sure that participants understood the shrinking cake design and the role of payoff-relevant periods by a treatment-specific check-up questionnaire before the actual study. In order to control for an equal distribution of the random offers across treatments and participants, we formed groups of 4 participants who saw the same sequence of offers and assigned 2 participants to each treatment. We achieved randomization by sorting people into groups by their time of arrival.

[3]The hint can be found in Figure 3.2 and was: Single: *Remember: Whether you get the offer also depends on the period the computer selects.*; Partner: *Remember: Whether you get the offer also depends on the period the computer selects* ***and the decision of your partner***. (Italic and bold font is added here for readability.)

3.3.2 Discussion of the Required Properties

The first property is easily fulfilled by our design as the participants in Single might exhibit a round-number bias, and Partner introduces coordination with a partner, where round numbers might serve as focal points. The extension to the two-player case is achieved by a single change of the bonus calculation, whereby everything else remains the same. This satisfies property 2. Our design focuses on the decision to accept or reject an offer. Therefore, it intentionally does not allow communication between participants, such as freely making offers or exchanging messages. This way, we obtain property 3. On the one hand, the design prevents participants from sending their partner a signal by using numbers to transmit their intentions or hints about their future behavior. On the other hand, it enables a cleaner comparison between Single and Partner, because, in the former treatment, there is only a single participant to elicit the individual round-number bias. Although we acknowledge that it might be interesting and possibly relevant in observational data, we need to abstract from an open bargaining approach, including communication, to distinguish between round-number bias and round numbers as focal points while keeping the differences between both treatments minimal. Thus, we decided to make offers exogenous. This might, of course, result in us underestimating the role of round numbers as focal points as we omit their usage in the between-participants communication and focus on the decision-making. Nevertheless, making the offers exogenous comes with the additional benefit of having the identical offer sequences in Single and Partner.

All offers in our experiment will be drawn from the set of numbers in Figure 3.3. We chose this set of numbers and its visual representation to have round and non-round numbers blend in naturally with each other. Hence, we obtain property 4. In addition, the intervals between the numbers are evenly spaced, and no number has decimal places. Moreover, contrary to other studies, we avoid any association of the offer set with specific situations, such as buying used cars, investing in stocks, or selling a house, since it might affect the perception of round numbers. For each round offer, there are at least two higher non-round offers. In particular, the average round offer is always smaller than the average non-round offer, so, e.g., in Period 1, the average round offer is 550, and the average non-round offer is 600. We will show in Section 3.4.1 that a large set of (standard) utility functions predicts lower acceptance frequencies for round offers (property 5).

Our bonus calculation allows us to obtain the remaining properties. Briefly summarizing our calculation, a randomly selected period determines the payoff-relevant periods, but the participants only learn which periods are payoff-relevant after having made all ten decisions. So, if a participant simply accepts every offer, an undesirable offer might be the first offer within the payoff-relevant periods and become the bonus. Hence, rejecting an offer might be advantageous, and accepting all offers is in general not the optimal strategy (property 6). However, rejecting an offer might be costly because the shrinking cake design causes the three highest offers to be removed after each period, reducing the chance of getting a better offer than the current one. Thus, there is also a potential downside to waiting for a better offer (property 7).

Since the period in which the decisions become payoff-relevant will be disclosed at the end of the study only, the participants should not consider previous periods at any decision. Should they have accepted an offer in a previous period that turns out to have been payoff-relevant, further acceptances have no impact on the bonus payment. So there is no downside in accepting further offers in future periods. However, as they can never be certain if a previous acceptance will indeed have been payoff-relevant, the decision in the current period could determine the bonus. The same is true, should they have rejected all previous payoff-relevant offers. Again, the decision in the current period could determine the bonus. Therefore, each decision is incentivized, regardless of previous decisions. Hence, participants should always behave as if the current period will be selected as the beginning of payoff-relevancy and only compare the current offer to possible future ones. The hint towards the payoff-relevancy on the Decision page reminded the participants of the bonus calculation. Additionally, due to the post-study disclosure of the selected period, accepting an offer does not lead to the end of the study. Still, all participants must complete all ten periods, which yields the same number of observations from each participant for each treatment and offer sequence (property 8).

3.3.3 Implementation

The experiment was programmed in oTree (Chen, Schonger, and Wickens, 2016). The study was conducted on Amazon Mechanical Turk (MTurk henceforth) in December 2020 using a sample of MTurk experienced US residents. In total, 924 participants (382 women) took part in the experiment, earning \$1.90 on average with an average completion time of approximately 7 minutes. After the experiment, participants had to fill out a short post-experiment questionnaire. Table 3.3 summarizes descriptive statistics of the sample.[4] The study was preregistered at the AEA RCT Registry under the ID AEARCTR-0006823.

[4]The announcement via which the participants were invited to the study can be found in Appendix B.3.1 and the instructions are summarized in Appendices B.3.2 and B.3.3.

Table 3.3. Descriptive Statistics of the Sample

	Total	Treatments: Single	Treatments: Partner	p-value
	(N=924)	(N=462)	(N=462)	
Gender				
Female	382 (41.3%)	198 (42.9%)	184 (39.8%)	0.350
Male	542 (58.7%)	264 (57.1%)	278 (60.2%)	
Age (years)				
Mean (SD)	37.9 (10.9)	38.0 (10.6)	37.7 (11.1)	0.540
Education				
Less than a high school degree	4 (0.4%)	3 (0.6%)	1 (0.2%)	0.692
High School Diploma	76 (8.2%)	37 (8.0%)	39 (8.4%)	
Vocational Training	7 (0.8%)	2 (0.4%)	5 (1.1%)	
Some College	82 (8.9%)	36 (7.8%)	46 (10.0%)	
Associate's degree	59 (6.4%)	29 (6.3%)	30 (6.5%)	
Bachelor's degree	501 (54.2%)	262 (56.7%)	239 (51.7%)	
Master's degree	177 (19.2%)	83 (18.0%)	94 (20.3%)	
Professional degree	14 (1.5%)	8 (1.7%)	6 (1.3%)	
Doctoral degree	4 (0.4%)	2 (0.4%)	2 (0.4%)	

Note: The table summarizes the characteristics of the subject pool. The column *Total* shows the number of observations for each category of gender, age and education. The columns *Single* and *Partner* report the distribution across treatments. The column *P-value* reports the p-value of tests between the two treatment groups. In particular, for Gender and Education, the χ^2 test was applied, and for Age, the Wilcoxon-Mann Whitney test.

3.4 Experimental Results

In our analysis of the eBay data set (see **??**), we showed that round final prices are associated with faster decisions when the negotiations ended with the sale of an item. Considering this finding, we start the discussion of our experimental results with a look at the time participants needed to make their decision within our experiment. To this end, we define the *decision time* as the time needed to complete a period in seconds.

In a first step, we consider all decisions, acceptances and rejections alike, and Table 3.4 shows the average decision times for each treatment and the two offer types. Comparing the top and bottom row of the table shows that decisions were made quicker for round offers. When the observations are pooled across treatments, we find significantly quicker decisions when a round offer was made (t-Test: 9.07s vs. 10.18s; p=0.0004). The difference in decision times between the two offer types is 1.11s. When we control for the treatments, we find that participants in Partner, who received a round offer, decide significantly quicker (t-Test: 9.07s vs. 10.62s; p=0.0013) while the difference in Single yields a p-value of 0.1006 (t-Test: 9.08s vs. 9.73s).

Table 3.4. Decision Times

		Treatment	
Offer type	Total	Single	Partner
Round	9.07	9.08	9.07
NonRound	10.18	9.73	10.62

Note: Average decision times are reported in seconds.

In a next step, we consider acceptances and rejections separately. When the observations of acceptances are pooled across treatments, we find significantly quicker acceptances when a round offer was made (t-Test: 9.81s vs. 11.05s; p=0.0054). In addition, the difference in decision times between offer types for acceptances is slightly larger (1.24s) than in the previous case (1.11s). When we control for the treatments, we find that participants in Partner accept round offers significantly quicker (t-Test: 9.75s vs. 11.52s; p=0.0112) while the difference in Single (t-Test: 9.88s vs. 10.55s; p=0.2123) is not significant. For the rejections, we observe a similar picture with a difference in decision times of only 0.73s but with shorter decision times in general. The corresponding tables and details can be found in Tables B4 and B5 in Appendix B.2.1.

Generally, there appears to be a difference in decision-making when round numbers are involved. Round numbers seem to trigger faster decisions. Since our offer set for each period is designed so that there are two larger non-round offers for each round offer, and the size of the offer is directly related to the potential payoff, round offers are on average less favorable than non-round ones. Hence, the decision times of rejections being smaller for round offers comes with no surprise, albeit the difference is very small and just barely significant. Intriguingly, round offers were also accepted quicker, leading to a more sizeable decrease in decision times between round and non-round offers than the rejections. We now turn to the likelihood that an offer was accepted and the role of round offers. For this purpose, we calculate the *acceptance frequency* as the fraction of participants that accepted a given offer; alternatively, in more practical terms, the number of participants who clicked on “Yes” in the Decision step in panel (b) of Figure 3.2 relative to all participants.

3.4.1 Acceptance Frequencies

In a preliminary step, we show that within our experimental framework a large set of (standard) utility functions (such as risk aversion, risk neutrality, or quantal response) predict lower acceptance probabilities for round offers and hence would not be consistent with an experimental finding of higher acceptance frequencies for round offers. We will show that this holds before the start of the experiment by showing that it holds before any period.

To this end, let the ten periods be denoted by $t = 1, 2, \ldots, 10$. As outlined in Section 3.3.1, the top line of the offer set is removed after each period. Thus, the offer set in period t is a matrix $X_t = (x_{i,j})$, where $i \in \{1, 2, ..., m_t\}$, $m_t = 11 - t$ and $j \in \{1, 2, 3\}$. By construction, for all i, we have (a) $x_{i,1} > x_{i,2} > x_{i,3}$, (b) $x_{i,1}$ and $x_{i,2}$ are non-round numbers, and (c) $x_{i,3}$ is a round number.

For some offer $x_{i,j}$ from X_t, let $p_t(x_{i,j})$ denote the probability that $x_{i,j}$ is accepted in period t, given some utility function $u(x_{i,j})$, where the outside option, i.e., the expected utility of rejecting and waiting for some future period, is denoted by μ_t. From the perspective of the beginning of period t (i.e., before an offer has been randomly drawn), the probability that given a round offer is made, it will be accepted is given by

$$P_t^R = \frac{\sum_{i=1}^{m_t} p_t(x_{i,3})}{m_t}. \tag{3.2}$$

Analogously, the probability that, given a non-round offer is made, it will be accepted is given by

$$P_t^{NR} = \frac{\sum_{i=1}^{m_t} (p_t(x_{i,1}) + p_t(x_{i,2}))}{2m_t}. \tag{3.3}$$

Proposition. *The probability that non-round offers are accepted is higher than the probability that round offers are accepted (i.e., $P_t^{NR} \geq P_t^R$) if $p_t(x_{i,j})$ is weakly increasing in $x_{i,j}$. In particular, this is the case if the agent has standard risk-neutral preferences, standard risk-averse preferences, or follows quantal-response behavior. In the case of quantal-response behavior, the above inequality holds strictly.*

Proof. Substituting Eq. (3.2) and Eq. (3.3) in $P_t^R \leq P_t^{NR}$ yields

$$\sum_{i=1}^{m_t} p_t(x_{i,3}) \leq \underbrace{\frac{m_t}{2m_t}}_{=\frac{1}{2}} \sum_{i=1}^{m_t} (p_t(x_{i,1}) + p_t(x_{i,2})). \tag{3.4}$$

Inequality (3.4) is satisfied if

$$p_t(x_{i,3}) \leq \frac{1}{2}(p_t(x_{i,1}) + p_t(x_{i,2})) \tag{3.5}$$

is satisfied for all i. The monotonicity of $p_t(\cdot)$ and $x_{i,1} > x_{i,2} > x_{i,3}$ imply that this is the case. □

$p_t(x_{i,j})$ is weakly increasing in $x_{i,j}$ if the decision-maker has a utility function $u(x_{i,j})$ that is weakly increasing in $x_{i,j}$ (which is, for example, the case under risk neutrality or risk aversion): a rational decision-maker would accept any offer $x_{i,j}$ where $u(x_{i,j})$ is weakly greater than the outside option μ_t. Thus, $p_t(x_{i,j}) = 1$ whenever $u(x_{i,j}) \geq \mu_t$, and $p_t(x_{i,j}) = 0$ otherwise.

For a decision-maker following quantal-response behavior, the probability to accept an offer is given by

$$p_t(x_{i,j}) = \frac{1}{1 + e^{\lambda(\mu_t - u(x_{i,j}))}}, \tag{3.6}$$

again resulting in monotonicity of $p_t(x_{i,j})$ in $x_{i,j}$. In this case, $p_t(x_{i,j})$ is strictly increasing in $x_{i,j}$ whenever $u(x_{i,j})$ is strictly increasing in $x_{i,j}$.

Having thus shown that $P_t^{NR} \geq P_t^R$ holds for every period t, it also holds in general.

3.4.2 Round-Number Effects

We now turn our attention to the experimental results as they relate to our research question. We are interested in the presence of round-number effects and possible differences between individual and cooperative decision-making.

Our analysis starts by estimating a simple linear probability model with the offer acceptance as the dependent variable. Offer acceptance is a binary variable equal to 1 if an offer was accepted or 0 otherwise. In a first specification, we regress acceptance on a dummy variable for the treatment and an indicator for round numbers. We also add the interaction of these two variables to allow for differences of round-number effects between treatments. The results of this regression are presented in Column (1) of Table 3.5.

In light of our theoretical predictions, we would expect round numbers to have lower acceptance frequencies, should there be no round-number bias. In Column (1), we do indeed find a negative sign for the round-number dummy, but the effect is not significant. This insignificance provides a first hint at the presence of round-number effects. We find a slight but significant increase in acceptance frequencies in Partner. The interaction term represents the additional effect of round offers in Partner. The negative sign implies that participants are less likely to accept round offers, but it is not significant.

Table 3.5. OLS Regression. Dependent Variable: Offer Acceptance.

	(1)	(2)	(3)	(4)
Sample:	Total	Total	Female	Male
(Intercept)	0.620 ***	0.230 ***	0.225 ***	0.235 ***
	(0.012)	(0.023)	(0.035)	(0.029)
Offer Share		0.596 ***	0.580 ***	0.608 ***
		(0.025)	(0.038)	(0.032)
Treatment: Partner	0.030 *	0.030 *	0.065 **	0.006
	(0.018)	(0.018)	(0.027)	(0.024)
Round Offer	-0.021	0.045 ***	0.056 ***	0.036 **
	(0.014)	(0.014)	(0.020)	(0.018)
Treatment: Partner x Round Offer	-0.010	-0.010	-0.016	-0.005
	(0.021)	(0.020)	(0.030)	(0.026)
N	9240	9240	3820	5420
R2	0.001	0.099	0.098	0.100

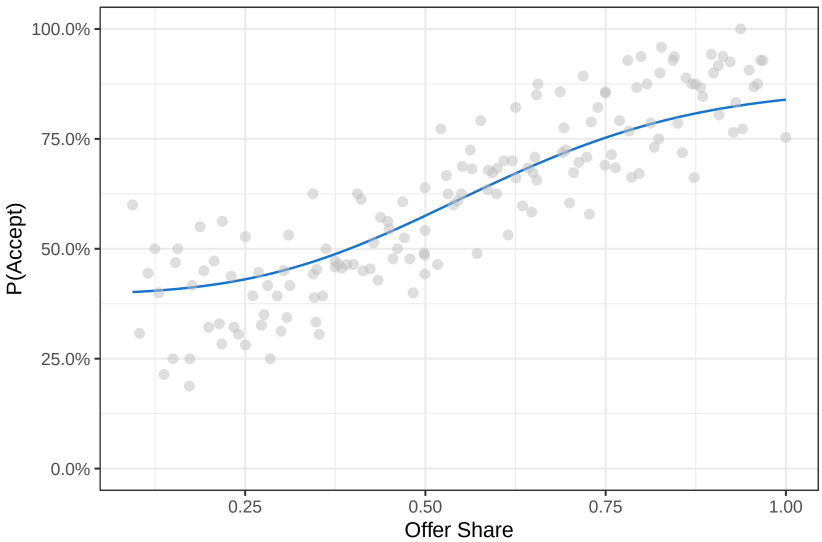
Note: The offer acceptance is a binary variable equal to 1 if the participant accepted an offer and 0 otherwise. Standard errors in parentheses are heteroskedasticity robust and clustered on the individual level. The results in columns (1) and (2) are based on the total sample. The results in columns (3) and (4) are based on the female and male sub-sample, respectively. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

For a more in-depth analysis we control for the size of an offer. It seems quite reasonable to assume that, ceteris paribus, higher offers are more likely to be accepted. In our experimental set-up, however, the size of an offer has to be evaluated in relation to the current period. An offer of 233 might not be attractive in period $t = 1$, but much more attractive in period $t = 8$. We, therefore, need to control for the relative size of the offers in our analysis. Hence, we introduce the *offer share*.

The *offer share* is defined as $s_t = \frac{x_t}{x_{\max,t}}$, where x_t is the offer in period t and $x_{\max,t}$ is the largest possible offer in period t. Thus, for a given period, the offer share measures the relative size of a given offer compared to the largest possible offer. Given our parameters, it follows that $s_1 \in \left[\frac{100}{1067}, 1\right]$ in the first period.

Figure 3.4 shows the acceptance frequencies for each observed offer share (gray dots) and the evaluated fit (curve) of a Nadaraya–Watson kernel estimation where we pool the observations from both treatments. Not surprisingly, the curve illustrates that higher offer shares are more likely to be accepted. This allows us to investigate how round offers affect acceptance frequencies in more detail.

Figure 3.4. Acceptance Frequencies for the Pooled Sample.



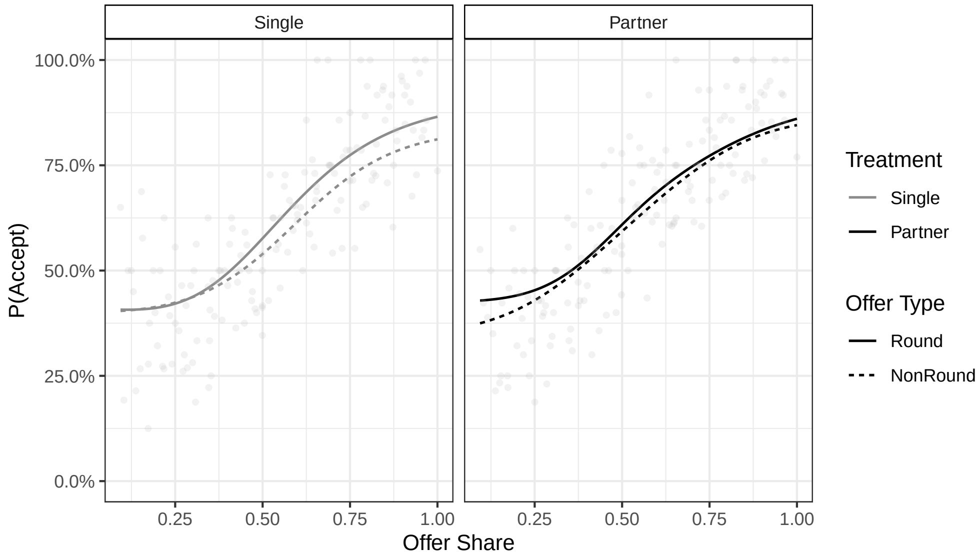
Note: Acceptance frequencies for the pooled sample. The curve is based on a Nadaraya–Watson kernel estimator using a normal kernel with a bandwidth of 0.4. The gray dots represent the frequency of acceptance for a given offer share.

Consider Column (2) of Table 3.5. We add the *offer share* to our linear probability model. Unsurprisingly it is significant and drastically increases R^2. The coefficient on the round-number dummy now turns positive and is significant at the 1%-level. The other coefficients remain qualitatively and quantitatively unchanged. Thus, on average, ceteris paribus, round offers were about 4.5%p more likely to be accepted.

By separating the data by our two treatments, we can observe differences in how these round-number effects influence acceptance decisions. We split the data into four distinct categories: by treatment (Single, Partner) and by offer type (Round, NonRound). For each category, we estimate a Nadaraya–Watson kernel regression. Figure 3.5 visualizes these estimates for each treatment (Single: gray, Partner: black) for round offers (solid line) and non-round offers (dashed line).

We observe an intriguing pattern. To illustrate the pattern more clearly, we present separate figures for each treatment. The left frame of Figure 3.5 shows the Single treatment. For smaller offer shares, we observe no differences between round and non-round numbers. For higher offer shares, the solid line representing round numbers lies above the dashed line representing non-round numbers. This implies that for higher offers, round numbers are more likely to be accepted by our subjects. Conversely, this does not seem to be the case for relatively low offers. In the right frame of Figure 3.5, in Partner,

Figure 3.5. Acceptance Frequencies for the Treatment and Offer Type Sub-Samples.



Note: Acceptance frequencies for the treatment and offer type sub-samples. The curves are based on a Nadaraya–Watson kernel estimator using a normal kernel with a bandwidth of 0.4. The gray dots represent the frequencies to accept for a given offer share. The gray lines represent the Single treatment in the left frame, and the black lines correspond to the Partner treatment in the right frame. The offer types are illustrated with solid lines for round offers and dashed ones for non-round offers.

the pattern is reversed. The solid line lies above the dashed line only for smaller offer shares, while no major differences can be seen for larger offer shares. Thus, if anything, there is a higher frequency of accepting lower round numbers. Nevertheless, in both treatments, the solid line (which represents round offers) is above the dashed line, which indicates that round offers are more likely to be accepted in general. We will refer to the differences between the acceptance frequencies for round and non-round numbers as *round-number effect* for now, as we will later disentangle whether this effect is driven by bias or coordination.

Over the last decades, studies in behavioral economics have shown that women and men exhibit different behavior in several economic domains. For example, it is often found that men are less risk averse than women, less charitable, and more competitive (see e.g. Croson and Gneezy, 2009; Niederle, 2016). It might be possible that there are also differences between men and women when it comes to bargaining and round-number effects.

There are more men than women in our sample (58.7% vs. 41.3%), but we do not find that one sub-sample received higher offers, more round offers, or unequal distributions of men and women into treatments.[5]

Consider now Columns (3) and (4) in Table 3.5. For both sub-samples, there is a positive round-number effect. The effect is stronger for the female sub-sample at 5.6%p with significance at the 1%-level. For the male sub-sample, the coefficient is 3.6%p, and is significant at the 5%-level. Women also react differently to the treatment than men, showing an increase in acceptance frequencies in Partner of 6.5%p at the 5%-level. The interaction term is insignificant in both sub-samples.

In a next step, we repeat the exercise from before to analyze the effect of non-round and round offers in both treatments by splitting each gender sub-sample into four distinct categories and estimating a curve for each of these. A visual analysis confirms that there are indeed behavioral differences between men and women.

The curves in Figure 3.6 visualize for each category the acceptance frequencies, where the left panel presents the female sub-sample and the right one the male sub-sample. Differences with respect to gender are clearly visible here. For the female sub-sample the pattern we observed in Figure 3.5 for the pooled sample is even more pronounced. Again, the solid line in Single lies above the dashed line for higher offer shares, while in Partner it lies above the dashed line only for lower offer shares. In general, for the female sub-sample, round offers are more likely to be accepted than non-round offers as indicated by the fact that the solid line is almost always above the dashed line in each treatment. In the male sub-sample, the curves are very close to each other and almost overlapping. If anything, there is a tendency for the solid line to be above the dashed line implying that round offers are more likely to be accepted. Analogous to the analysis without controlling for gender, men and women are more likely to accept round offers than non-round offers in both treatments. However, the intriguing patterns of the round-number effects in Figure 3.5 appear to be mainly driven by the female sub-sample. Therefore, we focus on the female sub-sample for further analysis.

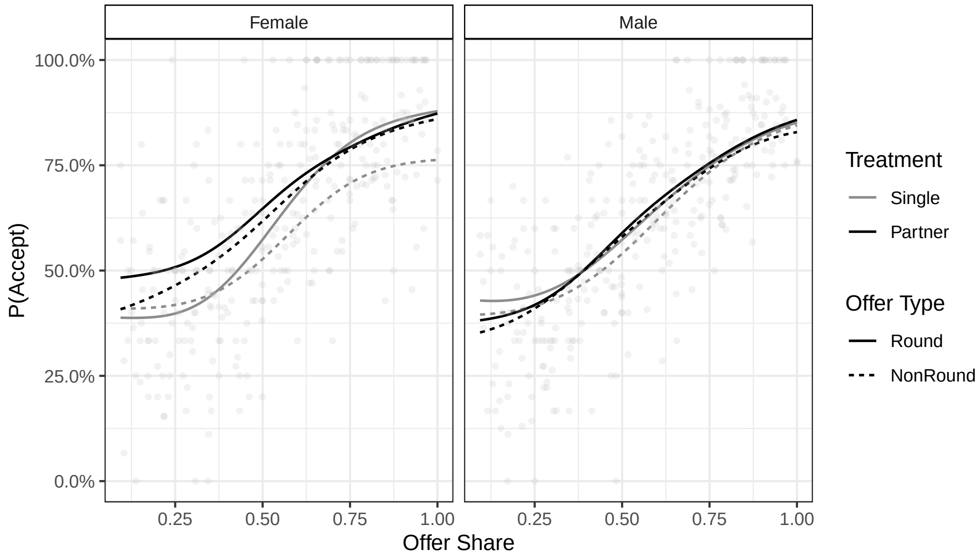
3.4.3 Round-Number Effects in the Female Sample

We summarize our findings in Result 1 and Result 2.

Result 1 In Single, we find evidence of *round-number bias* for sufficiently high offers. For lower offers, this bias vanishes.

[5]The Mann-Whitney test on the offer size between the two sub-samples yields a p-value of 0.1832 and a p-value of 0.2728 for the same test on offer share. The treatments are independently distributed across gender sub-samples (χ^2, $p = 0.3497$). The number of round offers is independent of the gender (χ^2, $p = 0.7796$).

Figure 3.6. Acceptance Frequencies for the Treatments and Offer Types by Gender.



Note: Acceptance frequencies for the treatments and offer types by gender. The curves are based on a Nadaraya–Watson kernel estimator using a normal kernel with a bandwidth of 0.4. The gray dots represent the frequencies to accept. The gray lines represent the Single treatment, and the black lines correspond to the Partner treatment. The offer types are illustrated with solid lines for round offers and dashed ones for non-round offers.

Result 2 Comparing Single and Partner, we find evidence of round offers serving as *focal points* for sufficiently low offers. For higher offers, the usage as focal point vanishes.

We provide evidence for these results by referring to the four panels in Figure 3.7. As shown before, the effects differ for high or low offer shares. Hence, we define four equally spaced segments of offer shares (S.1, S.2, S.3, S.4). In Figure 3.7 these segments are marked by vertical dotted lines. All curves are obtained by a Nadaraya–Watson estimator with a bandwidth of 0.4. Note that offer share does not start at zero but 100/1067, as 100 is the smallest possible offer in every period. We perform χ^2-tests to detect significant differences.

We start by analyzing acceptance frequencies in Single. In this treatment, there is no strategic interaction, and hence decisions reveal pure preferences. The estimated curves can be seen in panel (a) of Figure 3.7. In S.1, the curve for non-round offers is slightly above the curve for round offers, but the difference is rather small. A χ^2-test shows no significant differences between acceptances at round and non-round numbers in S.1

($p = 0.285$).[6] In S.2, there seems to be a switching point. Round-number acceptance increases stronger than non-round number acceptance. However, the difference in this segment is also not significant (χ^2, $p = 0.620$). The difference becomes larger in S.3 and S.4. Round-number acceptance is now clearly above non-round number acceptance and significant for these segments (χ^2, S.3: $p = 0.059$, S.4: $p = 0.017$). This is evidence for the presence of round-number effects in Single. As decisions in Single reveal pure preferences, only a *round-number bias* can explain these differences. Thus, we conclude that round-number bias emerges for sufficiently high offer sizes and grows stronger when the offer size increases (**Result 1**). The regression analysis in Appendix B.2.3 confirms this result.

As for the interpretation of these results, we think this hints towards a heuristic decision process where low offers will be declined right away, notwithstanding whether they are round or not. When offer share increases to a certain level, that makes acceptance at least a possibility, this is where the round-number bias comes into play.

In panel (b) of Figure 3.7 we add the acceptance frequency for round numbers in Partner represented by the gray solid line. The acceptance of round numbers in Partner is clearly higher in Single in S.1 and the difference is significant (χ^2, $p = 0.006$). With increasing offer share in the other segments the difference in the acceptance of round numbers between the treatments vanishes (χ^2, S.2: $p = 0.29$), S.3: $p = 0.732$, S.4: $p = 0.870$). So, when participants have to consider a partner in the Partner treatment, it results in higher acceptance of round numbers for low offer shares.

We now turn our attention to panel (c) of Figure 3.7. Here, we are able to analyze the treatment effect for non-round numbers. We see that the black dashed line is above the gray dashed line over the entire range of offer shares. This implies that the acceptance frequency for non-round numbers increases when the subjects' payoff also depends on the other player's decision. In S.1, this difference is not significant, with a p-value of $p = 0.995$ as illustrated in the figure. With increasing offer share the differences become significant (χ^2, S.2: $p = 0.134$, S.3: $p = 0.008$, S.4: $p = 0.009$). Hence, in Partner, acceptance is more likely.

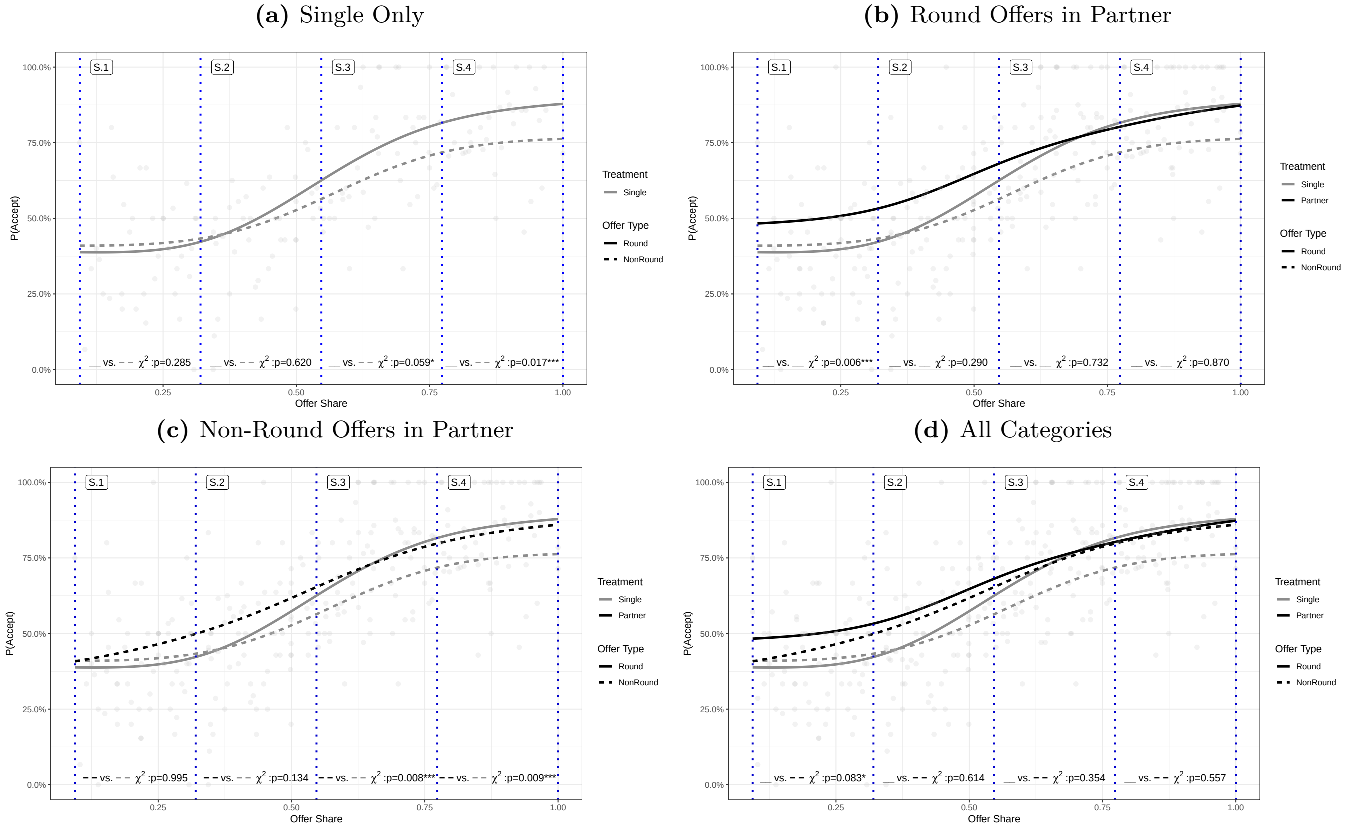
In panel (d) of Figure 3.7, we add the solid black line indicating the acceptance frequencies in Partner at round numbers. There are two interesting observations concerning this line. First, for high offer shares as in S.3 and S.4, there is no visible difference between round and non-round numbers within Partner. Considering the round-number

[6] The χ^2-tests are conducted for each offer share segment. The test hypothesis is at the bottom of each segment. Bar graphs can also represent the frequencies, see Figure B1. Since these tests are conducted segment-wise, the test only controls mildly for offer share. The curves serve as a graphical representation of considering the whole range of offer share to compensate for this.

effects in Single, we would have expected the solid line to be above the dashed line, i.e., again a form of round-number bias as in Result 1. No such bias is present for the Partner treatment in S.3 ($\chi^2, p = 0.354$) and S.4 ($\chi^2, p = 0.557$). Second, for low offer shares, as in S.1, we now find that the black solid line lies above the black dashed line. This difference is significant (χ^2, $p = 0.083$). Thus, there seem to be round-number effects for low offer shares in Partner in segments where we would not expect round number effects, as none were present in the respective segments in Single. This can evidently not be explained by preferences for round numbers, because otherwise, we should have seen round number effects in Single in S.1. A possible explanation is that round numbers serve a coordinative role that only becomes relevant in Partner (**Result 2**). It might be the case that, once offer share is sufficiently large, this need for coordination becomes less important, as acceptance becomes more likely in general. For increasing offers, we see the pattern previously discussed. In S.3 and especially S.4, there are hardly any differences between the solid black line and black dashed line (χ^2, S.3: $p = 0.354$, S.4: $p = 0.557$). This, again, is a striking observation, as it indicates no round-number bias in Partner on segments where we observe a round-number bias in Single. A possible interpretation is that subjects, when confronted with the need for coordination in Partner, shift from heuristic decision-making towards a more thorough computation of expected gains, thereby eliminating the round-number bias. Assuming that subjects operate in such a mode of thorough computation when making decisions in real life, would thus imply that round-number clusters in observational bargaining data are, to a considerable extent, driven by coordination.

To evaluate the robustness of our results, we estimate the linear probability models from Columns (2)-(4) of Table 3.5 separately for each segment. For details, see Appendix B.2.3. The results are qualitatively, quantitatively, and with respect to their statistical significance, in line with our graphical analysis and the non-parametric tests from the previous section.

Figure 3.7. Acceptance Frequencies for the Female Sub-Sample.



Note: Acceptance frequencies for the female sub-sample. The curves are based on a Nadaraya–Watson kernel estimator using a normal kernel with a bandwidth of 0.4. The gray dots represent the frequencies to accept for a given offer share. The gray lines represent the Single treatment, and the black lines correspond to the Partner treatment. The offer types are illustrated with solid lines for round offers and dashed ones for non-round offers. The four segments (S.1, S.2, S.3, S.4) are equally wide and the vertical dotted line marks the segments (at 0.094, 0.320, 0.547, 0.773, 1). At the bottom, the results of a χ^2-tests are reported with *** p<0.01, ** p<0.05, * p<0.10. The test is based on the counts of accepted offers in the respective segments for the indicated category.

3.5 Conclusion

We studied the role of round numbers in bargaining settings. Analyzing observational data, we found that throughout the bargaining process, the share of round-number offers and counteroffers increases. Also, negotiations with a final price that was round were on average shorter, as measured by both, total duration and the length of the offer-counteroffer sequence.

By developing a novel experimental framework, we are able to analyze the differences between round-number effects in individual and cooperative settings. We find robust evidence for the presence of round-number effects in the form of higher acceptance frequencies in our experiment. The channels resulting in these increased acceptance frequencies for round numbers differ between the individual and the cooperative setting, especially for the female sub-sample: Here, in the individual setting we observe round-number effects only for higher offer shares, while in the coordinative setting, we find round-number effects only for lower offer shares. While the first observation can easily be explained by individual behavioral biases, the latter is apparently the result from coordinative considerations.

Thus, we confirm two possible channels that could induce round-number effects in observational data: (a) individual behavioral biases and (b) round numbers as focal points for coordination in the spirit of Schelling. The observation of round-number effects changing with the offer share, point towards a context dependency of round number effects. In our experiment, the bias is the main driver for round-number effects when offers are large. For smaller offers, round-number effects are mainly driven by the role of round numbers as focal points.

Our findings conform to a growing body of literature on round-number effects in observational and experimental data. In particular, with a view to the trade-off between saving time and making a better deal, bargaining parties should carefully evaluate the potential impact of the number format and its signaling effect. Hence, using round numbers might be more beneficial in some situations, and in some other scenarios, precise numbers are more useful. Nevertheless, for future experimental research, we advocate taking the number format into consideration when designing studies, as it influences subjects' decision-making.

4

Be Green or Feel Green? An Experiment on Moral Balancing in Pro-Environmental Decision Making[1].

4.1 Introduction

Many environmental problems such as global warming, air pollution, water shortage, or the loss of biodiversity have in common that they are primarily caused by human behavior (e.g., Steg and Vlek, 2009; Vlek and Steg, 2007). For example, around 70% of global greenhouse gas emissions are linked to household consumption (Hertwich and Peters, 2009). Innovations contribute most effectively to sustainability if the positive impact is not overtaken by an increase in consumption (Gillingham, Kotchen, Rapson, and Wagner, 2013). Thus, despite technical innovations that boost sustainability, such as electric cars or energy-efficient buildings, behavioral change remains crucial (Steg and Vlek, 2009).

With the increasing severity of environmental problems, people frequently face encouragement from governments, organizations, or peers to act more environmentally friendly. Policymakers introduce behavioral change initiatives to induce climate-friendly behavior using nudges, economic incentives, or information campaigns (Clot, Della Giusta, and Jewell, 2022). For example, the UK government's 25-Year Environment Plan

[1] This chapter is based on Schöller and Schlereth (2023)

proposes "scoping out an evidence-based behaviour change strategy to enable further actions by individuals, communities, businesses and government" (HM Government, 2018). From a government perspective, encouraging behavior change is attractive as, unlike regulations, it mostly does not lead to public backlash or diminishing votes (Whitmarsh and O'Neill, 2010).

Many environmental campaigns emphasize that "every change counts" and aim to motivate individuals to make small and painless behavioral changes (such as avoiding plastic straws, double-sided printing, or switching off the lights when leaving a room). The designers of these campaigns often hope small changes will result in higher-impact behavioral changes later on (Thøgersen and Crompton, 2009) in line with the foot-in-the-door effect (Freedman and Fraser, 1966). For example, the UK's Sustainable Consumption Round Table recommends "to drop new tangible solutions into people's daily lives, catalysts that will send ripples, get them talking, sweep them up into a new set of social norms, and open up the possibility of wider changes in outlook and behavior." (Sustainable Consumption Roundtable, 2006).

However, to evaluate the effectiveness and judge the overall welfare effect of an environmental campaign, one has to go beyond the immediate effect on the targeted behavior and also consider the campaign's indirect impact on future environmental behavior (Gilg, Barr, and Ford, 2005; Grieder, Schmitz, and Schubert, 2021). But what are the dynamic effects of pro-environmental behavior? Does promoting (small) behavioral changes affect future decision-making, and if yes, does it induce more or less pro-environmental behavior? In this chapter, we analyze the dynamic effects of pro-environmental behavior and focus on whether the magnitude of the initial actions has a systematic effect on pro-environmental behavior later on. We provide insights on whether balancing only occurs for environmental actions with relevant consequences or already occurs from an individual's impression of having done anything at all for the environment, even if it is not important on a grander scale.

There is vast literature finding that pro-social decisions are not made in a vacuum but are affected by previous decisions (for an overview, see, e.g., Blanken, Van De Ven, and Zeelenberg, 2015; Kuper and Bott, 2019; Maki et al., 2019; Simbrunner and Schlegelmilch, 2017). In this project, we focus on moral balancing (Nisan and Horenczyk, 1990).[2] Moral balancing is a cognitive bias that can go in two directions; it is defined as moral licensing (cleansing) if someone is acting immorally (morally) after a moral (an

[2]The literature uses several terms rather interchangeably such as negative spillover effects (Engel and Szech, 2020), compensatory beliefs (West and Zhong, 2015), moral credentials (Monin and Miller, 2001), moral self-regulation (Sachdeva, Iliev, and Medin, 2009), or conscience accounting (Gneezy, Imas, and Madarász, 2014).

immoral) action (for a theoretical consideration, see, e.g., Merritt, Effron, and Monin, 2010; Mullen and Monin, 2016). Bénabou and Tirole (2011) introduce a theoretical model that explains the cognitive bias as balancing one's moral self-image, which positively impacts an individual's utility (Akerlof and Kranton, 2000, 2005; West and Zhong, 2015), and the cost of acting pro-social. Morally or immorally perceived actions are performed to close the gap between desired and perceived moral self-image (Mazar and Zhong, 2010). According to the concept of moral balancing, individuals are more likely to engage in ethical conduct when they feel a previous unethical decision threatens their moral self-image. In contrast, they are less likely to engage in moral behavior when they previously secured their moral self-image by a moral action (Ploner and Regner, 2013). A related theory explains intertemporally dependent altruistic behavior with a fixed "altruistic budget" that determines individuals' altruistic acts over time (Gee and Meer, 2019). However, the literature is inconclusive whether the altruistic budget is fixed or flexible (Gee and Meer, 2019).

As individuals consider various domains to define their self-image, one not only observes moral balancing in the same behavioral domain (same-domain moral balancing), but also between seemingly unrelated behaviors (cross-domain moral balancing) (Mullen and Monin, 2016). Moral balancing has been identified in various domains, suggesting that it can occur in any domain with a positive normative connotation, e.g., pro-environmental behavior (Effron, 2016).

Besides moral balancing that predicts negative spillover effects on later environmental behavior, there is also evidence for positive spillover effects (e.g., Baca-Motes, Brown, Gneezy, Keenan, and Nelson, 2013). Positive spillovers can be explained by the desire to act and be perceived as consistent, in line with cognitive dissonance theory (Festinger, 1957). The mixed evidence on whether positive or negative spillovers prevail and the limited number of studies in the field of pro-environmental behavior stress the importance of further analyses of the dynamic effects of moral behavior.

In an online experiment, we explore the following research questions: do we find moral balancing in pro-environmental behavior, in particular carbon offsetting? Does moral balancing occur even if the initial moral or immoral act has close to no effect or only if the moral or immoral action has a substantial impact? Moreover, is the moral balancing effect moderated by an individual's moral values?

To test how pro-environmental decisions depend on past environmental behavior, we conduct a controlled online experiment that consists of two parts. In Part 1, participants perform a real-effort task. The treatments vary if and how much carbon offset participants receive for succeeding in the task. Thereby, we exogenously vary if a moral license is acquired and of which magnitude. In Part 2, we measure moral balancing by letting participants decide how much to donate for carbon offsetting. The treatments allow us to test whether pro-environmental behavior with only a negligible positive effect on the environment leads to a similar licensing effect as behavior that substantially affects the environment.

We find that participants who successfully solve the real effort task subsequently donate, on average, less to carbon offset than those who failed. This difference is significant at the 10%-level and indicates that moral balancing influences pro-environmental decision-making, in particular in the domain of carbon offsetting. Regarding the magnitude of the initial moral act, we do not see systematic differences in moral balancing. Furthermore, our results indicate that moral balancing depends on individuals' moral values. We do not find moral balancing for participants with the greatest environmental concerns. Participants with somewhat lower concerns regarding global warming engage in moral balancing, but only if the initial act is substantial.

Most studies on environmental behavior only study single actions, neglecting temporal context. We add to the literature by analyzing dynamic aspects of environmental decision-making. We consider that individuals regulate their moral choices, resulting in prior actions affecting subsequent decisions. Given the mixed evidence on the occurrence and determinants of moral balancing, the current experimental study aims to extend the literature on moral balancing and pro-environmental behavior in several ways. First, we analyze moral balancing in the domain of voluntary carbon offsetting, a growing market due to the increasing attention on carbon dioxide as the primary driver of climate change. In 2019, around $320 million worth of carbon offsets were purchased globally, resulting in a reduction of approximately 104 million metric tons of CO2 emissions (Forest Trends' Ecosystem Marketplace, 2020). Second, we exogenously vary the magnitude of the initiating pro-environmental action to investigate whether moral balancing occurs only for substantial or also for negligible moral actions. Moreover, we measure the initial action as well as the moral balancing effect in carbon offsetting. Thereby, we can compare both decisions on a common and quantifiable scale. Third, we incentivize individuals' actions so their decisions have real-world consequences. Exciting work often relies on self-reported behavior (e.g., Chatelain et al., 2018; Lanzini and Thøgersen, 2014) or elicits intention to act (e.g., Jordan, Mullen, and Murnighan, 2011; Margetts and Kashima, 2017). However, self-reported behavior lacks reliability and might be biased in approximating actual decision-making, especially concerning moral behavior. Eliciting intention to act instead of actual behavior can lead to biased results as shown in a meta-analysis by Maki et al. (2019), which reports that pro-environmental behavior results in positive spillovers for intentions, whereas it induces negative spillovers for actual behavior. Fourth, we add to the research on moderators of moral balancing and study in particular the impact of environmental values.

The remainder of the chapter is structured as follows: related literature is discussed in Section 4.2. Section 4.3 presents the experimental design and discusses the data collection. Hypotheses and results are presented in Sections 4.4 and 4.5, respectively. We conclude and discuss in Section 4.6.

4.2 Related Literature

From charitable giving (e.g., Brañas-Garza, Bucheli, Espinosa, and García-Muñoz, 2013; Grieder, Schmitz, and Schubert, 2021) to food consumption habits (e.g., Wilcox, Vallen, Block, and Fitzsimons, 2009), racial prejudice (e.g., Effron, Cameron, and Monin, 2009), or sexism (e.g., Monin and Miller, 2001) moral balancing is found in many different domains (for meta-analyses see Blanken, Van De Ven, and Zeelenberg (2015), Kuper and Bott (2019), Maki et al. (2019), and Simbrunner and Schlegelmilch (2017)) as well as across domains (Hofmann, Wisneski, Brandt, and Skitka, 2014).

Meta-analyses estimate moral licensing to be of small to medium magnitude (Blanken, Van De Ven, and Zeelenberg, 2015; Kuper and Bott, 2019; Simbrunner and Schlegelmilch, 2017). Regarding the welfare effects of moral licensing, Grieder, Schmitz, and Schubert (2021) find that moral licensing decreases future charitable giving, but also that multiple opportunities to behave pro-socially positively impact aggregate donations.

Regarding our research question, we focus in the following brief literature overview on (1) moral balancing in the environmental domain and (2) moderators of moral balancing.

4.2.1 Moral Balancing in the Environmental Domain

In the following section, we focus on studies in which the initial action and the moral balancing effect both lie in the environmental domain. Since the literature predominantly studies cross-domain moral balancing, our study adds evidence on less studied same-domain moral balancing. Compared to cross-domain moral balancing, same-domain moral balancing has been shown to be more likely and of greater magnitude (Blanken, Van De Ven, and Zeelenberg, 2015; Dolan and Galizzi, 2015). Besides same-domain moral balancing, pro-environmental behavior has also been shown to affect behavior in domains such as pro-social decision-making (e.g. Engel and Szech, 2020; Hahnel et al., 2015; Mazar and Zhong, 2010). Similarly, also good deeds in other domains can affect sustainable behavior (Sachdeva, Iliev, and Medin, 2009).

Moral balancing in the environmental domain has been reported regarding pro-environmental actions, intentions to act environmentally friendly (e.g., Burger, Schuler, and Eberling, 2022; Geng, Cheng, Tang, Zhou, and Ye, 2016), and supporting climate-friendly policies (e.g., Noblet and McCoy, 2018). Maki et al. (2019) combine multiple studies in their meta-analysis and find a slightly negative moral balancing effect of pro-environmental behavior on future environmental actions and policy support. In contrast, they find positive spillover effects on intentions. In the following overview, we will focus on moral balancing effects on actual behavior, as this is the main focus of our study.

Results from correlational analyses are mixed. A three-wave panel study with Danish consumers finds that some environmentally friendly behavior is related to more, others to less pro-environmental actions in subsequent years (Thøgersen and Ölander, 2003). While correlational findings do not allow clean identification of moral balancing effects, our experimental design exogenously implements a pro-environmental action.

Several lab experiments confirm moral balancing regarding various forms of environmentally friendly behavior. For example, participants who performed a climate-friendly act by filling out a "green" instead of a conventional shopping list, later conserved less water (Geng, Cheng, Tang, Zhou, and Ye, 2016). Also, exposure to a green advertisement increases water consumption and lowers the intention to choose transportation with a low carbon footprint (Zhang, Jiang, Sun, Gu, and Jiang, 2021). Randomly giving green-committed individuals positive feedback on the environmental friendliness of their shopping decisions reduced their recycling engagement as compared to giving negative feedback. Receiving no feedback leads to a mid-range recycling rate (Longoni, Gollwitzer, and Oettingen, 2014). A drawback of many lab experiments (Clot, Grolleau, and Ibanez, 2013, 2016) is that, in particular, the initial action designed to induce moral balancing is often only imaginary and not performed, thereby limiting the informative value for real-world behavior.

Besides lab experiments, Tiefenbeck, Staake, Roth, and Sachs (2013) report in a field setting that individuals participating in an environmental campaign to save water reduce their water consumption but consume more electricity. In contrast, Carlsson, Jaime, and Villegas (2021) find a reduction in electricity consumption after an information campaign that targets water consumption for individuals that had an efficient use of water prior to the intervention.

All mentioned studies test moral balancing from one pro-environmental decision context to an unrelated one, whereas we study moral balancing of carbon offsetting decisions on subsequent carbon offsetting.

Besides studies documenting moral balancing, there is also empirical evidence finding no interdependencies in pro-environmental decision-making (e.g., Liebe, Gewinner, and Diekmann, 2021). Other studies report that one pro-environmental action increases the probability of further environmentally friendly behavior (e.g., Clot, Grolleau, and Ibanez, 2016; Lanzini and Thøgersen, 2014; Margetts and Kashima, 2017; Panzone, Ulph, Zizzo, Hilton, and Clear, 2021; Sintov, Geislar, and White, 2019). Out of the studies finding positive spillovers, Panzone, Ulph, Zizzo, Hilton, and Clear (2021) is closest to our research design. They ask participants to recall past eco-friendly behavior and, similarly to our research design, inform and congratulate them on the resulting amount of carbon savings. They find that in a subsequent experimental online supermarket, the participants who had been informed of their carbon savings purchased a food basket with a lower carbon footprint. Several replications of well-cited publications find null effects (e.g., Blanken, Ven, Zeelenberg, and Meijers, 2014; Urban, Bahník, and Kohlová, 2019), questioning the role moral balancing plays in decision-making.

Interestingly, not only one's own behavior but also an employer's good deeds can influence participants' behavior (e.g., Grieder, Kistler, and Schmitz, 2020; List and Momeni, 2021). Grieder, Kistler, and Schmitz (2020) find that informing subjects on their employer's donation to an environmental charity increases subjects' donations for the preservation of the environment.[3]

We conclude that the literature on moral balancing in environmental behavior is inconclusive and does not precisely predict whether moral balancing occurs in repeated carbon-offsetting decision-making.

4.2.2 Moderators of Moral Balancing

The inconclusive results regarding moral balancing (see Section 4.2.1) suggest that the occurrence might be sensitive to experimental conditions as well as individual attitudes (e.g., Alt and Gallier, 2022; Blanken, Van De Ven, and Zeelenberg, 2015; Mullen and Monin, 2016). There is a growing literature on factors moderating moral balancing, e.g., the cost of the initial action (Gneezy, Imas, Brown, Nelson, and Norton, 2012), the time that passes between both decisions (Schmitz, 2019), feeling responsible for one's behavior (Engel and Szech, 2020), or the similarity of both tasks (Chatelain et al., 2018; Maki et al., 2019; Truelove, Carrico, Weber, Raimi, and Vandenbergh, 2014). For a literature overview, see Blanken, Van De Ven, and Zeelenberg (2015) and Mullen and

[3]In contrast, List and Momeni (2021) find companies Corporate Social Responsibility activities to increase employees' misbehavior.

Monin (2016). In the following, we will focus on the role of the magnitude of the initial action and environmental values on moral balancing.

To the best of our knowledge, only two studies vary the magnitude of the initial actions. In Gholamzadehmir, Sparks, and Farsides (2019), participants were reminded of past frequent or infrequent pro-environmental actions. Recalling past (in)frequent actions leads to moral licensing (cleansing), and participants were less (more) likely to seek information about calculating their carbon footprint. Grieder, Kistler, and Schmitz (2020) find that an employer's donation to an environmental charity affects participants' donations, but the magnitude of the donation rate (10% vs. 40%) does not matter. These divergent findings regarding the magnitude emphasize the importance of additional research.

The current literature makes it difficult to draw a clear conclusion regarding the moderating effect of environmental values on moral balancing, since various terms are used rather interchangeably (such as environmental consciousness (Garvey and Bolton, 2017), environmental self-identity (van der Werff, Steg, and Keizer, 2014), environmental attitudes (Lacasse, 2016), or pro-environmental values (Thøgersen and Crompton, 2009)). Moreover, there is no standardized way to measure environmental values, but each paper uses different definitions and questionnaires. Therefore, the following overview covers environmental values more broadly and, in particular, includes findings regarding environmental self-identity as it gets the most attention in the current literature.

In general, the literature suggests that building an identity increases charitable giving (e.g., Charness and Holder, 2019; Kessler and Milkman, 2018) and Bénabou and Tirole (2011) predict that a threat to identity induces moral behavior.

Regarding moral balancing in the environmental domain, most papers argue that when climate-friendly actions are performed out of extrinsic (e.g., financial incentives, regulation) instead of intrinsic motivation (e.g., self-identity, concerns, values), they are more likely to generate moral balancing (e.g. Clot, Della Giusta, and Jewell, 2022; Clot, Grolleau, and Ibanez, 2016; Lacasse, 2016; Miller and Effron, 2010; Nilsson, Bergquist, and Schultz, 2017; Noblet and McCoy, 2018; Thøgersen and Crompton, 2009; Thøgersen and Ölander, 2003; Truelove, Carrico, Weber, Raimi, and Vandenbergh, 2014; van der Werff, Steg, and Keizer, 2014). Individuals who self-identify as environmentally friendly will engage less in moral balancing and are more likely to perform a subsequent environmentally friendly action than individuals with lower environmental self-identity (Garvey and Bolton, 2017; Geng, Cheng, Tang, Zhou, and Ye, 2016; Truelove, Carrico, Weber, Raimi, and Vandenbergh, 2014; van der Werff, Steg, and Keizer, 2014). These experimental findings align with the self-perception theory by Bem (1967) that predicts consistent behavior in domains where an individual has integrated past actions in their self-image (Lalot, Falomir-Pichastor, and Quiamzade, 2022).

In contrast, few papers find environmental identity or attitudes not to mediate dynamics in environmental behavior (e.g. Gholamzadehmir, Sparks, and Farsides, 2019; Gleue, Harrs, Feldhaus, and Löschel, 2022). Moreover, Hahnel et al. (2015) find moral cleansing to be stronger for individuals with high environmental motivation.

Little research exists regarding environmental concern as a moderator of moral balancing (Truelove, Carrico, Weber, Raimi, and Vandenbergh, 2014). Truelove, Yeung, Carrico, Gillis, and Raimi (2016) find no general moderating effect of environmental concern on moral balancing. Other papers find that individuals with high concerns regarding climate protection engage in moral balancing to a larger degree (Burger, Schuler, and Eberling, 2022; Hartmann, Marcos, and Barrutia, 2023).

We conclude that most papers find environmental values to decrease moral balancing. However, there is little research regarding environmental concerns.

4.3 Experimental Design

We set up an economic online experiment with oTree (Chen, Schonger, and Wickens, 2016) to test our research question. The experiment was conducted on Prolific (`www.prolific.co`) between the 14th and 21st of June 2021. We obtained informed consent from all participants, and the study follows the relevant guidelines and regulations. Before data collection, the experiment was preregistered on asPredicted.org (Nr. 70521, see `https://aspredicted.org/JTY_GDD`). Participation is restricted to British individuals (country of birth) that live in the United Kingdom (country of residence) to ensure that subjects are equally familiar with the measurement units and understand the English instructions well. Participants are randomly assigned to one of three treatment conditions, each having 300 observations. The median completion time for the study was 10 minutes, with average earnings of £1.70 ($2.40).[4]

The experiment design adheres to the standard two-part structure commonly seen in moral balancing studies. The first part is designed to impact an individual's self-image and initiate a moral balancing effect, which is then measured in the second task.[5]. The two parts are followed by a questionnaire. See Appendix C.2 for the complete instructions.

[4]Exchange rate £ to €: 1.4109 on the 16th of June 2021 (XE.com Inc., 2021).

[5]Some studies deviate from the two parts design, e.g. Brañas-Garza, Bucheli, Espinosa, and García-Muñoz (2013) use multiple periods to identify a dynamic pattern of moral balancing.

In Part 1, subjects can work for two minutes on a real effort slider task (Gill and Prowse, 2012). A slider is solved when the participant sets it to a given value using the computer mouse. Participants had to solve at least 26 sliders to succeed in the task. This threshold was determined in a pilot study, as it resulted in a roughly equal number of successful and unsuccessful participants. Depending on the treatment condition, succeeding in the task leads either to a carbon offset of 10 kg (*LOW*), 100 kg (*HIGH*), or a payment of £0.20 to the participant (*SELF*). We chose the payment in *SELF* to be similar to the donation required to offset 10 kg of CO_2 to make it comparable to *LOW*. The slider task has several advantages compared to other real-effort tasks. It is easy to understand, can be conducted online, does not require prior knowledge from participants, and performance is not improved through guessing (Gill and Prowse, 2012). Additionally, the performance in the slider task is relatively insensitive to the size of incentives, as demonstrated in a between-subject design study by Araujo et al. (2016). As a result, the number of sliders that must be solved to complete the task successfully can be kept constant across all treatment conditions, making it an ideal choice for our study.

In Part 2, subjects play a dictator game (Kahneman, Knetsch, and Thaler, 1986). They receive £2 and split it between themselves and carbon offset, with the current market price of a selected carbon-offsetting charity as the exchange rate. Next, we elicit incentivized beliefs by asking participants to make two guesses: regarding the success rate of other participants in the real-effort task in Part 1, and the average offset of other participants in Part 2. The participants who make guesses within 5% of the actual values receive a bonus payment of £0.20.

Participants are informed that the study consists of two parts, but they only receive details about each part as they proceed. Previous research has shown that when individuals are made aware of the possibility of donating to a charity in the future, they behave less ethically in the present (Cojoc and Stoian, 2014). Moreover, the instructions state that decisions made in one part of the study will not impact the other part. At the end of the study, one part is randomly selected for payment. Restricting payment to one part is essential to rule out wealth effects that would otherwise bias our measurement of moral balancing (Azrieli, Chambers, and Healy, 2018; Charness, Gneezy, and Halladay, 2016).

To ensure that subjects understand the instructions, we include multiple control questions in both parts and an attention check in the questionnaire. Carbon emissions are paired with the corresponding distance in miles that could be traveled by a typical new car that would result in an equivalent amount of carbon emissions. By providing the reference value, we aim to assist participants in evaluating the extent of the carbon offsets. Thereby, we align with the existing research on behavior and emissions (e.g., Falk, Andre, Boneva, and Chopra, 2021; Imai, Pace, Schwardmann, and Weele, 2022; Pace and Weele, 2020).

It is essential that participants believe the instructions, e.g., that the carbon offset is implemented and that they are not subjected to any form of misguidance. We use two approaches to ensure that the carbon offset implementation is credible to the participants. Firstly, we emphasize that the study does not involve deception, specifically that their decisions have real-world consequences, and that the carbon offsets are implemented as stated. Additionally, we include a control question to verify participants' understanding of this aspect. Secondly, the participants are informed that they will receive a private message through Prolific that comprises the computation of the total carbon offset amount and an official donation receipt.

The post-experimental questionnaire elicits (1) demographic characteristics; (2) altruism and patience (Falk et al., 2018); (3) climate change awareness based on the Six Americas Super Short Survey (SASSY) (Chryst et al., 2018); (4) opinion on whether climate change is human-caused (Howe, Mildenberger, Marlon, and Leiserowitz, 2015); (5) beliefs regarding the importance of one's own actions to mitigate climate change; (6) environmental behavior; (7) previous offsetting and (8) opinion on the effectiveness of offsetting. To determine if participants accurately keep track of the offset they accumulate throughout the experiment, we ask them to report the offset they received in both stages combined. Additionally, to control for the perceived difficulty of the task, we ask participants about the effort they exerted in Part 1.

4.4 Hypotheses

First, we hypothesize that moral balancing is prevalent in the decision to offset carbon emissions. As stated above, succeeding in the slider task of our experiment results in a carbon offset for the participants in the *LOW* and *HIGH* treatments. In line with the theory of moral behavior (Bénabou and Tirole, 2011), we assume that acquiring this carbon offset boosts moral self-image, while missing the chance to offset leads to a decline in moral self-image. If moral licensing (cleansing) is prevalent, the higher (lower) moral self-image will lead to less (more) moral behavior in the subsequent offsetting decision. More precisely, we expect a lower donation rate in the *LOW* and *HIGH* treatment for individuals who succeeded in the slider task than for those who failed. In the *SELF* treatment, no offset is acquired. Hence, we assume the moral self-image to stay constant, and we expect similar offsetting rates in Part 2, independent of the outcome of the slider task.

Our first set of hypotheses therefore states:

$$Donation^{Success}_{LOW} < Donation^{Failure}_{LOW},$$

$$Donation^{Success}_{HIGH} < Donation^{Failure}_{HIGH},$$

and accordingly

$$Donation^{Success}_{SELF} = Donation^{Failure}_{SELF}.$$

Second, we test whether moral balancing depends on the initial actions' magnitude. The study exogenously varies the magnitude of carbon offset that can be acquired by solving the slider task in Part 1. In *LOW*, participants can offset 10 kg of CO_2, whereas in *HIGH*, participants can work towards offsetting 100 kg. Our primary focus is the gap in offsetting rates in Part 2, between participants who successfully completed the slider task in Part 1, and those who did not. We compare this gap between *LOW* and *HIGH*. If the gap is similar in *LOW* and *HIGH*, we conclude moral balancing to be independent of the magnitude of the initial action. If we observe a significantly larger gap in *HIGH* than in *LOW*, we infer that the magnitude of the initial action matters for the size of the moral balancing effect. Under the assumption that the initial action does not significantly impact the size of the subsequent donation, we would observe similar rates of moral balancing for the *LOW* and *HIGH* treatments:

$$Donation^{Failure}_{LOW} - Donation^{Success}_{LOW} = Donation^{Failure}_{HIGH} - Donation^{Success}_{HIGH}.$$

In addition, we exploratively investigate potential moderators of moral balancing. In particular, we investigate the moderating effect of environmental values (for an overview of empirical and theoretical evidence regarding environmental values as a moderator for moral balancing, see Section 4.2.2).

4.5 Experimental Results

We present our findings using the following abbreviations: Chi-squared test (χ^2), Kruskal-Wallis test (KW), two-tailed Mann-Whitney U test (MWU), and two-sample Kolmogorov-Smirnov (KS).

4.5.1 Summary Statistics

Through Prolific, we recruited 900 participants, each sorted into one of the three treatments by arrival time. Table 4.1 confirms that the randomization into treatments was successful. The participants in all three treatments are generally comparable concerning demographics[6], altruism, patience, and environmental awareness.

The only major difference between the three treatments is found for the variable *Actions matter to fight climate change* (χ^2, p-value = 0.007), which is the participants' answers regarding the question, as to whether they agree that their personal actions matter to fight climate change, in the post-experimental questionnaire. When comparing effects between treatments, we account for this difference by adding a specification that controls for the variable *Actions matter to fight climate change* in our regression analyses (hereafter denoted by Control A). Since participants filled out the questionnaire after the experiment, the previous tasks may have influenced responses, particularly regarding environmental awareness.

Table 4.1. Summary Statistics by Treatment

	Mean/ Median	*SELF*	*LOW*	*HIGH*	Test Statistic (p-value)
Number of Observations		300	300	300	
Demographics					
Female	Mean	0.623 (0.485)	0.640 (0.481)	0.613 (0.488)	0.465 (0.793)
Male	Mean	0.353 (0.479)	0.357 (0.480)	0.377 (0.485)	0.414 (0.813)
Other	Mean	0.023 (0.151)	0.003 (0.058)	0.010 (0.100)	5.154 (0.076)
Age	Mean	32.197 (12.376)	30.343 (11.115)	32.350 (12.849)	4.073 (0.130)
Has children	Mean	0.337 (0.473)	0.287 (0.453)	0.287 (0.453)	2.366 (0.306)
Education	Median	undergraduate degree (ba/bsc/other)			0.798 (0.671)
Political Orientation: Left	Mean	0.580 (0.494)	0.630 (0.484)	0.547 (0.499)	4.350 (0.114)
Political Orientation: Right	Mean	0.213 (0.410)	0.177 (0.382)	0.243 (0.430)	4.016 (0.134)
Income	Median	10,000 - 29,999	10,000 - 29,999	10,000 - 29,999	1.450 (0.484)
Behavioral Preferences					
Altruism	Mean	-0.042 (0.834)	0.031 (0.841)	0.011 (0.799)	0.279 (0.870)
Patience	Mean	0.062 (1.032)	-0.026 (0.962)	-0.036 (1.005)	3.340 (0.188)
Environmental Awareness					
SASSY segment	Median	Concerned	Concerned	Concerned	0.298 (0.862)
Global warming caused by humans	Mean	0.633 (0.483)	0.663 (0.473)	0.680 (0.467)	1.491 (0.474)
Actions matter to fight climate change	Mean	0.773 (0.419)	0.803 (0.398)	0.697 (0.460)	9.891 (0.007)
Pro-environmental behavior	Mean	7.350 (2.114)	7.523 (1.875)	7.370 (2.202)	0.480 (0.787)
Has offset in past	Mean	0.200 (0.401)	0.200 (0.401)	0.217 (0.413)	0.340 (0.844)
Carbon offset effective	Mean	0.677 (0.469)	0.683 (0.466)	0.653 (0.477)	0.674 (0.714)

Note: Standard deviations in parentheses for variables with means. For categorical variables, we use χ^2-tests; for numerical variables, we use Kruskal-Wallis tests. *SELF*, *LOW*, and *HIGH* denote the treatments. Pro-environmental behavior is measured with respect to its frequency on a scale from 0 (Never) to 10 (Very often).

[6] The only significant difference concerning demographics (χ^2, p-value = 0.076) is observed for *Other*, i.e. participants that indicated "non-binary" or "rather not say" when asked for their gender (7 in *SELF*, 1 in *LOW*, and 3 in *HIGH*).

The following analyses test for moral balancing by comparing participants that succeeded in the slider task with those who failed. Therefore, we divide the subject pool by success in the slider task in Part 1. Participants that succeeded in the slider task by solving at least 26 sliders are subsumed under *Success*, and those who failed under *Failure*. The success rates (*SELF*: 0.500, *LOW*: 0.510, and *HIGH*: 0.487; χ^2, p-value = 0.848) and the number of solved sliders (*SELF*: 26.230, *LOW*: 26.413, and *HIGH*: 25.253; KW, p-value = 0.148) are similar across treatments, implying comparable behavior in Part 1, despite differing incentives to succeed in the real effort task.

Table 4.2 presents summary statistics pooled across treatments, but separated by *Success* and *Failure*. Significant differences exist concerning gender, age, parenthood, education, political orientation, altruism, beliefs regarding the primary causes of global warming, and beliefs regarding the efficacy of personal actions in combating climate change. In order to account for these differences, we will include these variables (hereafter denoted by Controls B) in the relevant regression specifications.

Table 4.2. Summary Statistics by Success in Part 1

	Mean/ Median	*Success*	*Failure*	Test Statistic (p-value)
Number of Observations		449	451	
Demographics				
Female	Mean	0.512 (0.500)	0.738 (0.440)	48.149 (0.000)
Male	Mean	0.477 (0.500)	0.248 (0.433)	49.77 (0.000)
Other	Mean	0.011 (0.105)	0.013 (0.115)	0.000 (1.000)
Age	Mean	28.192 (8.858)	35.053 (13.912)	48.792 (0.000)
Has children	Mean	0.183 (0.387)	0.424 (0.495)	61.707 (0.000)
Education	Median	undergraduate degree (ba/bsc/other)	technical/community college	8.390 (0.004)
Political Orientation: Left	Mean	0.630 (0.483)	0.541 (0.499)	7.380 (0.007)
Political Orientation: Right	Mean	0.196 (0.397)	0.226 (0.419)	1.229 (0.268)
Income	Median	10,000 - 29,999	10,000 - 29,999	1.640 (0.896)
Behavioral Preferences				
Altruism	Mean	-0.067 (0.859)	0.067 (0.784)	4.092 (0.043)
Patience	Mean	-0.032 (1.015)	0.032 (0.985)	0.560 (0.454)
Environmental Awareness				
SASSY segment	Median	Concerned	Concerned	1.804 (0.179)
Global warming caused by humans	Mean	0.697 (0.460)	0.621 (0.486)	5.816 (0.016)
Actions matter to fight climate change	Mean	0.710 (0.454)	0.805 (0.397)	10.914 (0.001)
Pro-environmental behavior	Mean	7.372 (2.079)	7.457 (2.057)	0.610 (0.435)
Has offset in past	Mean	0.207 (0.406)	0.204 (0.403)	0.014 (0.907)
Carbon offset effective	Mean	0.695 (0.461)	0.647 (0.478)	2.290 (0.130)

Note: Standard deviations in parentheses for variables with means. For categorical variables, we use χ^2-tests; for numerical variables, we use Kruskal-Wallis tests. *SELF*, *LOW*, and *HIGH* denote the treatments. Pro-environmental behavior is measured with respect to its frequency on a scale from 0 (Never) to 10 (Very often).

Tables C1, C2, and C3 in the Appendix show summary statistics by *Success* in the slider task in Part 1 separately for each treatment. Within treatments, significant differences exist in particular with respect to gender, age, and having children.

4.5.2 Prevalence of Moral Balancing

To test our first set of hypotheses on whether moral balancing is prevalent in carbon offsetting, we separate the data into *Success* (carbon offset is acquired) and *Failure* (no carbon offset is acquired) in the slider task. According to the theory of moral balancing, participants in *LOW* and *HIGH* who acquire a positive moral self-image by succeeding in the slider task will donate less to carbon offsetting in Part 2 than participants that acquire a negative moral self-image by failing in the slider task.

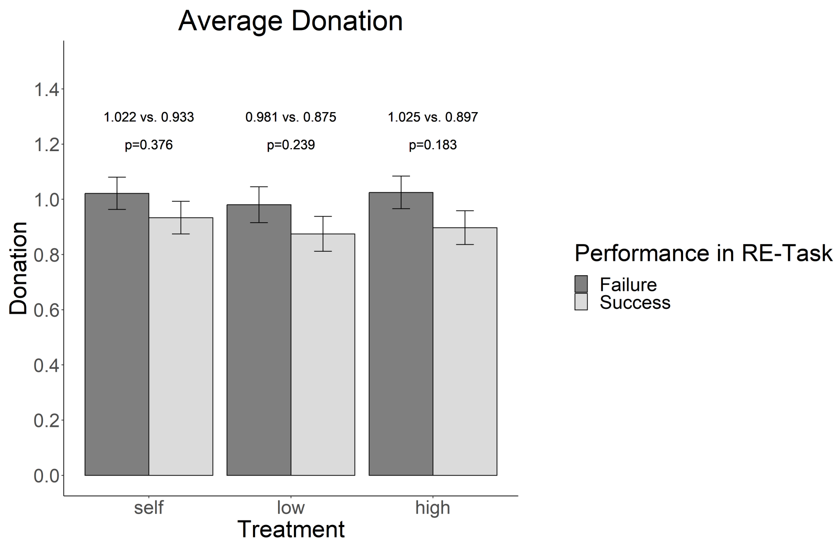
Figure 4.1 displays the average donations for carbon offset by treatment and whether the slider task was completed successfully. In general, successful participants donated, on average, about 12% less than unsuccessful ones. In line with our first set of hypotheses on moral balancing, we find in *LOW* and *HIGH* that participants who succeeded in the slider task donate, on average, less than those who failed. While these differences are non-significant (MWU, *LOW*: p-value $=$ 0.239, *HIGH*: p-value $=$ 0.183), pooling the *LOW* and *HIGH* groups reveals a significant difference at the 10% level between those who acquired a carbon offset through the slider task and those who did not (MWU, p-value $=$ 0.072). In *SELF*, a participant's self-image is not targeted, and the differences between those who succeeded and those who failed are non-significant, as predicted in our hypotheses (MWU, *SELF*: p-value $= 0.376$). The difference in subsequent offsetting between successful and unsuccessful participants indicates that moral balancing plays a role in carbon offsetting decisions.

In addition to the non-parametric tests presented above, we conduct a variety of regression analyses to test for the prevalence of moral balancing. The results are comparable to those of the non-parametric tests and are presented in Table 4.3. In Columns (1) - (3) we split the sample by treatment and regress the amount donated in Part 2 on a dummy capturing success in the slider task. We observe negative but insignificant coefficients for all three treatments (p-values ≥ 0.134). In line with moral balancing, the coefficients are greater in magnitude in *HIGH* and *LOW* as compared to *SELF*.

In Columns (4) - (6), we add additional controls. These include all the variables for which we found significant differences between those who succeeded in the real effort task, and those who failed (Controls B; see the discussion of Table 4.2). The coefficient for *Success* remains insignificant for all three treatments (p-values ≥ 0.288).

In Column (7), we pool the *LOW* and *HIGH* treatments in which participants could obtain a moral license. The dummy capturing success in the slider task displays the expected negative sign and is significant at the 10%-level (coef $= -0.118$; 95% CI $= [-0.239, 0.004]$; p-value $=$ 0.058).

Figure 4.1. Average Donations by Treatment and Success in the Slider Task.



Note: This figure shows average donations for carbon offset in Part 2 by treatment for *Success* and *Failure* in the slider task in Part 1 of the experiment ($n = 900$). The error bars represent the standard errors of the means. The figure also shows the respective means and p-values of MWU tests for differences within treatment between *Success* and *Failure*.

As a robustness check defined in the pre-registration, in Column (8) we exlude all participants that did not pass the attention check in the post-experimental questionnaire (0 observations in *SELF*, 4 in *LOW*, and 2 in *HIGH*). Similarly to the regression specification in Column (7), the coefficient takes the expected negative sign, even increases in magnitude, and becomes significant at the 5%-level (coef = -0.124; 95% CI = $[-0.246, -0.001]$; p-value = 0.048). The results in Columns (7) and (8) suggest that moral balancing influences carbon-offsetting decisions.

In Column (9), we again pool the *LOW* and *HIGH* treatments, as we did in Columns (7) and (8), but add the controls for variables that are significantly different between *Success* and *Failure*, as we already did in Columns (4) - (6). In addition to these, we introduce as further controls the participants' beliefs regarding how much others donate in Part 2 (*Belief Donation Others*) and their beliefs regarding the proportion of participants that succeeded in the slider task of Part 1 (*Belief Success Others*). The coefficient for *Success* is negative but insignificant (coef = -0.013; 95% CI = $[-0.148, 0.122]$; p-value = 0.847). We conclude that the differences in donation rates can not only be explained by moral balancing, but are also driven by individuals' characteristics and beliefs regarding other's pro-environmental behavior.

In Figure 4.2, we take a closer look at donation decisions in Part 2, by plotting the

Table 4.3. Regression Results for Moral Balancing.

	Dependent Variable: Donation								
	SELF	*LOW*	*HIGH*	*SELF*	*LOW*	*HIGH*	*LOW + HIGH* Pooled	*LOW + HIGH* Pooled Attentive	*LOW + HIGH* Pooled
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Success	-0.088	-0.106	-0.128	0.079	0.053	-0.096	-0.118*	-0.124**	-0.013
	(0.083)	(0.091)	(0.085)	(0.082)	(0.091)	(0.091)	(0.062)	(0.062)	(0.069)
Belief Donation Others									0.690***
									(0.047)
Belief Success Others									-0.0001
									(0.001)
Intercept	1.022***	0.981***	1.025***	-0.018	0.531***	0.658***	1.003***	1.009***	0.040
	(0.058)	(0.065)	(0.059)	(0.188)	(0.190)	(0.238)	(0.044)	(0.044)	(0.144)
Controls B	No	No	No	Yes	Yes	Yes	No	No	Yes
Observations	300	300	300	300	300	300	600	594	600
R^2	0.004	0.005	0.008	0.302	0.279	0.174	0.006	0.007	0.401

Note: This table presents the results of OLS regressions for Donation in Part 2. *Success* takes the value 1 if a participant succeeded in the slider task in Part 1, 0 otherwise. "Belief Donation Others" is a participant's estimation of the average donations of other participants, "Belief Success Others" refers to a participant's estimation of the percentage of successful participants in the slider task in Part 1. In Columns (4) - (6) and (9) we also include controls for gender, age, having children, altruism, education, political orientation, opinion on whether climate change is predominantly caused by human activity, and whether participants are of the opinion that their actions matter to fight climate change. For Columns (7) - (9), we pool the *LOW* and *HIGH* treatments. In Column (8) we exclude participants that did not pass the attention check in our questionnaire. Robust standard errors in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

distributions for carbon offsetting donations separately for *Success* and *Failure*. As can easily be seen, a great part of the participants either donate £0.00, £1.00, or £2.00, probably driven by the salient nature of these round numbers.

The distribution of donations for *Success* is tilted to the left as compared to the donations for *Failure*, however mainly so due to those who either gave £0.00 or £2.00. This tilt towards the left implies that participants that acquired a moral license (*Success*) donate less than participants who failed to do so (*Failure*).

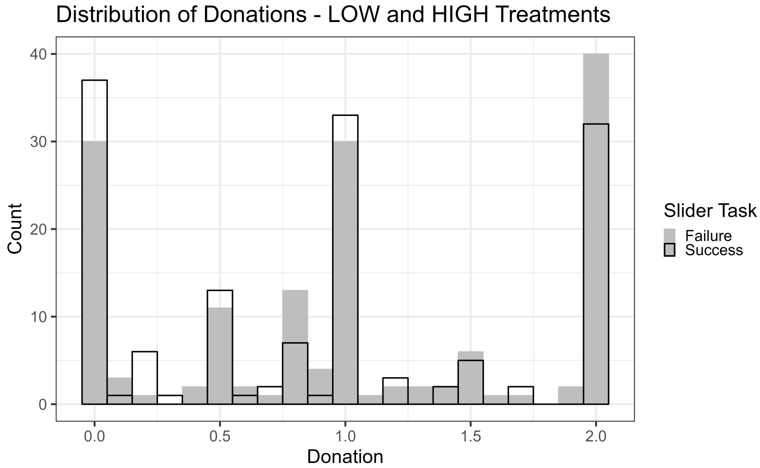
We do, however, not detect significant differences between *Success* and *Failure* (KS, p-value = 0.184). The findings are similar when analyzing *LOW* and *HIGH* separately (KS, p-values $\geq$ 0.447). The corresponding distributions are displayed in Figure C1 of the Appendix.

4.5.3 Disentangling Moral Licensing and Cleansing

In the following, we aim to disentangle the moral balancing effect into moral licensing and cleansing.

To test for moral licensing, we restrict ourselves to successful participants. Figure 4.3 depicts that successful participants in *SELF* donate on average more than those in

Figure 4.2. Distributions of Donation in Part 2 by Success in Part 1.



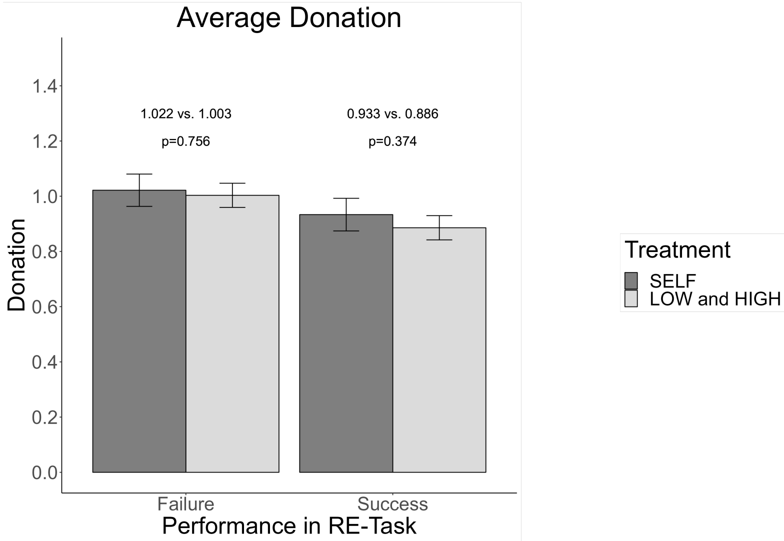
Note: This figure displays the distribution of the donations in Part 2. The sample contains the *LOW* and *HIGH* treatments, and is split by success in Part 1.

the other two treatments. The lower average donation rate in treatments where a license was acquired (*LOW* and *HIGH*) aligns with moral licensing. However, the difference lacks statistical significance (MWU, p-value = 0.374).

To test for moral cleansing, we focus on unsuccessful participants and compare the average donation rate in *SELF*, where no carbon could be offset, with the average in *LOW* and *HIGH*, where participants failed to offset carbon (see Figure 4.3). Moral cleansing would predict a higher donation rate in *LOW* and *HIGH* since participants will try to restore their moral self-image by donating more in Part 2. However, in our setup, the average donation rates in *SELF* are similar in magnitude to the average donations in *LOW* and *HIGH* (MWU, p-value = 0.756).

Regression analyses in Table C4 of the Appendix confirm the findings from the non-parametric tests. The lack of significant results when comparing *SELF* with *LOW* and *HIGH* is probably driven by the fact that we also observe a gap in donations between successful and unsuccessful participants in the *SELF* treatment (compare Figure 4.1), which was designed to serve as a baseline treatment not affecting participants moral self-image. Despite being insignificant, this gap makes it harder to specifically identify moral licensing or moral cleansing. We can only speculate about possible reasons. Maybe participants see their success in the slider task as a good deed towards the experimenter (de Quidt, Haushofer, and Roth, 2018). As a consequence, succeeding in the task increases a participant's moral self-image, whereas failing decreases it, resulting in moral

Figure 4.3. Average Donations by Success in Part 1 and Treatment.



Note: This figure shows average donations in Part 2 for *Success* and *Failure* in the slider task of Part 1. The error bars represent the standard errors of the means.

balancing. In addition, succeeding (failing) in the task might lead to positive (negative) emotions (e.g., happiness, sadness, pride or guilt) that have been shown to affect donation decisions (e.g. Ibanez and Roussel, 2021; Tan and Forgas, 2010). E.g., Ibanez and Roussel (2021) find that after inducing negative emotions, participants donate less to an environmental charity. To subsume, we find weak but insignificant evidence for moral licensing, and no evidence for moral cleansing in our setup.

4.5.4 Magnitude of the Initial Pro-Environmental Action

Regarding our second set of hypotheses, we explore if the magnitude of the initial moral action affects subsequent pro-environmental behavior. Figure 4.1 shows that the difference between the donation of successful and unsuccessful participants is £0.11 in *LOW* and £0.13 in *HIGH*. This difference in average donations between the two treatments of £0.02 is low, especially considering that the monetary equivalent of the difference between *HIGH* and *LOW* is £1.8. Moreover, we compare the difference in donation rates between *LOW* and *HIGH* separately for successful and unsuccessful participants. We do not find significant differences (*Success*: MWU, p -value = 0.767, *Failure*: MWU, p -value = 0.712) which is in support of our second set of hypotheses and indicates that the magnitude of the initial action does not significantly impact the donation decision.

Regression analyses in Table 4.4 confirm the findings from the non-parametric tests. We find that neither for the successful nor for the unsuccessful participants, the donation rates differ between having acquired a substantial (*HIGH*) or a negligible (*LOW*) offset (see Columns (1) and (3)) (p-values $\geq$ 0.613). These results are qualitatively invariant when controlling for *Actions Matter to fight climate change* (see Columns (2) and (4) (p-values $\geq$ 0.321). We thus conclude that the magnitude of the initial action does not significantly impact the size of moral balancing effects.

Table 4.4. Regression Results for the Effect of Magnitude of the Initial Action on Donation.

	Dependent Variable: Donation			
	Failure		*Success*	
	(1)	(2)	(3)	(4)
HIGH	0.044	0.086	0.022	0.061
	(0.088)	(0.087)	(0.088)	(0.085)
Intercept	0.981***	0.704***	0.875***	0.540***
	(0.065)	(0.102)	(0.063)	(0.087)
Control A	No	Yes	No	Yes
Observations	301	301	299	299
R^2	0.001	0.030	0.0002	0.071

Note: This table presents the results of OLS regressions for Donation in Part 2. The sample is restricted to the *LOW* and *HIGH* treatments. The sample is further split by *Failure* (Columns (1) and (2)) and *Success* (Columns (3) and (4)) in the slider task. *HIGH* is a dummy, taking the value 1 if a participant is in the *HIGH* treatment, and 0 if the participant is in the *LOW* treatment. In Columns (2) and (4), we control for whether participants agree that their actions matter to fight climate change, as for this variable, we find significant differences between treatments (see Table 4.1). Robust standard errors in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

4.5.5 Heterogeneous Effects

We preregistered to explore heterogeneous effects in moral balancing. In Table 4.5 we test for heterogeneous effects with respect to gender (Column (1)), age (Column (2)), altruism (Column (3)), education (Column (4)), political orientation (Column (5)), previous offsetting behavior (Column (6)), participants' pro-environmental behavior (Column (7)), and concerns regarding global warming elicited via the SASSY[7] (Column (8)). For each

[7]The SASSY clusters individuals into six distinct segments with respect to their awareness of, and willingness to take action against climate change. The six segments, ordered from highest to lowest environmental concerns, are: *Alarmed*, *Concerned*, *Cautious*, *Disengaged*, *Doubtful* and *Dismissive*. Our

of these variables we define a dummy that splits our sample into two distinct categories. In general, we do not find that individual characteristics moderate moral balancing (see Columns (1) - (7)) (p-values $>$ 0.113). However, we find a significant heterogeneous effect regarding climate change concerns measured by the SASSY (p-value $=$ 0.042) (see Column (8)). We conclude that environmental concerns moderate moral balancing, which is in line with the literature (e.g. Effron, Cameron, and Monin, 2009; Meijers, Noordewier, Verlegh, Willems, and Smit, 2019) (see also Section 4.2.2). In the subsequent section, we provide a more detailed analysis of the impact of environmental concerns.

Table 4.5. Regressions for Heterogeneous Moral Balancing Effects.

	Dependent Variable: Donation							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Interacted Variable	*Female*	*Older*	*More Altruistic*	*Higher Education*	*Right Party*	*Offset Past*	*Pro-env.*	SASSY Alarmed
Success	0.023	-0.017	-0.083	-0.050	0.064	-0.041	0.074	-0.135
	(0.109)	(0.086)	(0.089)	(0.091)	(0.077)	(0.071)	(0.089)	(0.085)
Interacted Variable	0.148	0.240**	0.365***	0.015	0.226**	-0.068	0.158*	0.081
	(0.099)	(0.097)	(0.086)	(0.083)	(0.107)	(0.110)	(0.087)	(0.086)
Success × Interacted Variable	-0.063	-0.063	0.083	0.028	-0.210	0.079	-0.183	0.241**
	(0.128)	(0.121)	(0.117)	(0.117)	(0.144)	(0.143)	(0.115)	(0.118)
Intercept	0.166	0.462***	0.028	0.276*	0.329	0.211	0.091	0.136
	(0.177)	(0.118)	(0.167)	(0.145)	(0.233)	(0.164)	(0.168)	(0.163)
Controls B	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	596	600	600	599	479	600	600	582
R^2	0.195	0.194	0.185	0.193	0.226	0.200	0.204	0.214

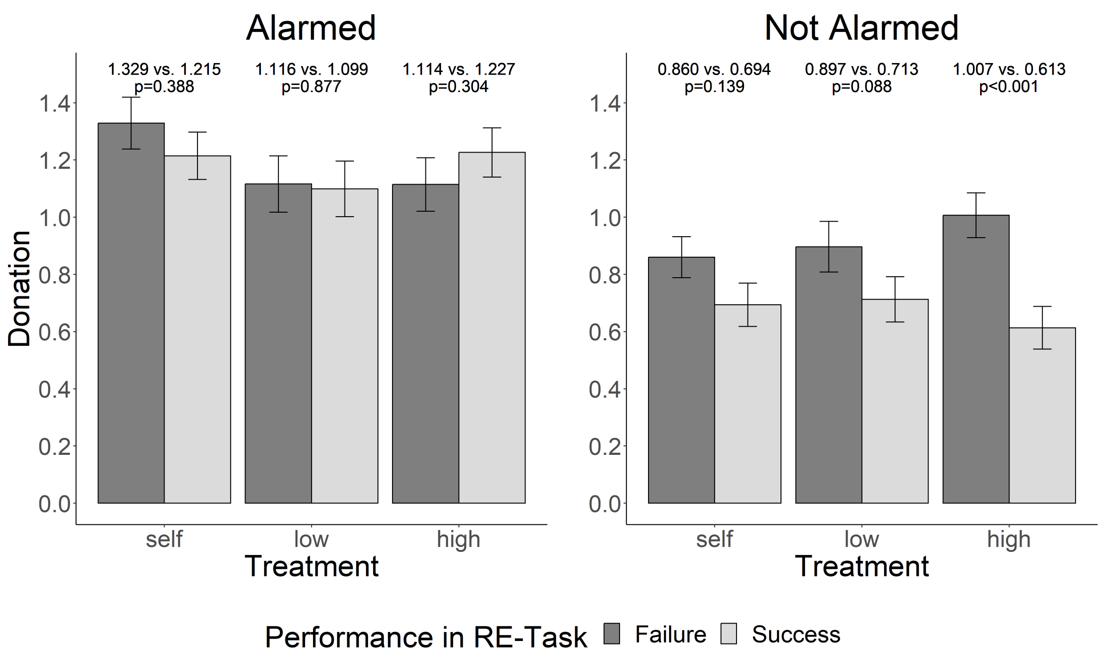
Note: This table presents the results of OLS regressions for Donation in Part 2. The sample is restricted to the *LOW* and *HIGH* treatments. *Female* takes the value 1 if the participant is female, and 0 if male. Participants answering "Non-binary" or "Rather not say" are excluded. *Older* takes the value 1 if age $>$ 28 (median), and 0 otherwise. *More Altruistic* takes the value 1 if Altruism ≥ 0, and 0 otherwise. *Higher Education* takes the value 1 if Education $\in$ {undergraduate degree (Ba/Bsc/other), graduate degree (MA/MSc/MPhil/other), doctoral degree (PhD/other)}, and 0 if $\in$ {no formal qualifications, secondary education, high school diploma/A-levels, technical/community college}. We excluded those who answered {don't know/not applicable}. *Right Party* takes the value 1 if a participant identifies with $\in$ {Conservative, Liberal Democrats}, and 0 if $\in$ {Labour, SNP, Green Party}. We excluded those who answered {other, rather not say}. *Offset Past* takes the value 1 if a participant had ever offset CO_2 before the experiment, and 0 otherwise. *Pro-env. behavior* takes the value 1 if a participant answers an 8 (median) or higher on a scale from 0 (Never) to 10 on the frequency s/he is taking environmentally friendly actions. *SASSY Alarmed* takes the value 1 if a participant is in the sassy segment $\in$ {Alarmed}, and 0 if $\in$ {Cautious, Concerned}. The following control variables are included: gender, age, having children, education, political orientation, altruism, opinion on whether climate change is predominantly human-caused, and whether participants agree that their actions matter in fighting climate change. The respective variable is not included in the set of controls if it is considered in the specification. Robust standard errors in parentheses; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

subjects are generally rather concerned with respect to climate change. About 44% are *Alarmed*, another 41% *Concerned*, and 12% *Cautious*. Only about 3% end up in one of the three lower groups. Since the number of observations in the three latter categories is low, we drop observations classified as either *Disengaged*, *Doubtful*, or *Dismissive* from our analyses relating to the SASSY. We pool *Concerned* and *Cautious* and refer to it as *Not Alarmed*.

4.5.6 Environmental Concern

Figure 4.4 shows the average donation rates by treatment and success in the slider task, separately for *Alarmed* and *Not Alarmed* participants. Participants in *Alarmed*, do not show differing donation rates depending on whether they acquired a moral license by offsetting carbon in Part 1 (*LOW*: MWU, p-value = 0.876; *HIGH*: MWU, p-value = 0.303). For participants in *Not Alarmed*, we find a significant difference in donations between those who succeeded versus those who failed in *HIGH* (MWU; p-value < 0.001) as well as *LOW* (MWU; p-value = 0.088). Hence, participants with the greatest environmental concerns do not engage in moral balancing, whereas participants who show less concern base their donation decisions on previous offsets.

Figure 4.4. Average Donations by Treatment, *Success* and *Failure*, and SASSY Segment.



Note: This figure shows average donations for carbon offset by SASSY segment, treatment, and *Success* or *Failure* in the slider task of Part 1 ($n = 874$). *Not Alarmed* denotes that SASSY segment $\in$ {*Cautious*, *Concerned*}. The SASSY segments *Doubtful*, *Disengaged*, and *Dismissive* are excluded, due to the small number of observations. The bars show the average donation and the error bars represent the standard errors of the means. The figure also shows the respective means, and p-values of MWU tests for differences within treatment between *Success* and *Failure*.

In Table 4.6 we run regression specifications that support our non-parametric findings. In Column (1), we do not observe moral balancing effects for the *Alarmed* sub-sample (F-test, p-values $\geq$ 0.367). For participants in the *Not Alarmed* sub-sample

Table 4.6. Regressions for Heterogeneous Balancing Effects with Respect to SASSY.

	Dependent Variable: Donation	
	(1)	(2)
β_1: *LOW*	-0.213	-0.116
	(0.134)	(0.122)
β_2: *HIGH*	-0.214 *	-0.171
	(0.130)	(0.120)
β_3: *Not Alarmed*	-0.469 ***	-0.284 **
	(0.115)	(0.111)
β_4: *Success*	-0.114	0.030
	(0.122)	(0.115)
β_5: *LOW* × *Not Alarmed*	0.249	0.130
	(0.176)	(0.162)
β_6: *HIGH* × *Not Alarmed*	0.361 **	0.280 *
	(0.168)	(0.160)
β_7: *LOW* × *Success*	0.097	0.008
	(0.185)	(0.169)
β_8: *HIGH* × *Success*	0.226	0.189
	(0.176)	(0.165)
β_9: *Not Alarmed* × *Success*	-0.052	-0.018
	(0.161)	(0.148)
β_{10}: *LOW* × *Not Alarmed* × *Success*	-0.115	-0.029
	(0.243)	(0.223)
β_{11}: *HIGH* × *Not Alarmed* × *Success*	-0.454 *	-0.440 **
	(0.232)	(0.220)
β_0: Intercept	1.329 ***	0.253
	(0.090)	(0.167)
Controls B	No	Yes
N	874	874
R^2	0.086	0.236
H_0 : *Alarmed-SELF*: $\beta_4 = 0$	0.373	0.809
H_0 : *Alarmed-LOW*: $\beta_4 + \beta_7 = 0$	0.891	0.751
H_0 : *Alarmed-HIGH*: $\beta_4 + \beta_8 = 0$	0.367	0.067 *
H_0 : *Not Alarmed-SELF*: $\beta_4 + \beta_9 = 0$	0.141	0.915
H_0 : *Not Alarmed-LOW*: $\beta_4 + \beta_7 + \beta_9 + \beta_{10} = 0$	0.109	0.927
H_0 : *Not Alarmed-HIGH*: $\beta_4 + \beta_8 + \beta_9 + \beta_{11} = 0$	0.001 ***	0.031 **

Note: This table presents the results of OLS regressions for Donation in Part 2. *Alarmed* takes the value 1 if a participant is in the SASSY segment *Alarmed*, and 0 if in *Concerned* or *Cautious*. We exclude participants in SASSY segments *Doubtful*, *Disengaged*, and *Dismissive* due to the low number of observations ($n = 36$). The additional control variables are gender, age, having children, altruism, education, political orientation, opinion on whether climate change is predominantly caused by humans, and whether participants agree that their actions matter in fighting climate change. The rows starting with H_0 report p-values for F-tests regarding the restrictions indicated thereafter. Robust standard errors in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

and in the *HIGH* treatment, we do observe significant balancing effects (F-test, p-value $=$ 0.001). For successfull participants, we see a reduction in the average donation of about 39.126% (£0.39) as compared to those who failed to acquire a moral license.

In Column (2), we include the additional controls for which we observed differences with respect to *Success* and *Failure*. We find a *higher* donation rate for successfull participants for the *Alarmed* and *HIGH* group, which is in contradiction to moral balancing. The difference, however, is only significant at the 10%-level (F-test, p-value $= 0.067$). Participants in *HIGH* and *Not Alarmed* donate on average less after successfully offsetting carbon in Part 1 (F-test, p-value $= 0.031$), which is again in line with moral balancing. We conclude that participants with somewhat lower environmental concerns, i.e. the *Not Alarmed*, engage in moral balancing after (not) acquiring the high carbon offset. Highly concerned participants, i.e. the *Alarmed*, do not base their offset decisions on past acquired offsets. One possible explanation could be that less concerned participants use their previous behavior to justify less environmental behavior later, whereas individuals with a high environmental identity want to act environmentally friendly consistently.

4.5.7 Beliefs Regarding Others' Behavior

To analyze the impact participants' beliefs about others' behavior have on moral balancing, we elicit incentivized guesses on the percentage of participants that succeeded in Part 1, and the average offset in Part 2.[8] Participants guessed a higher success rate in Part 1 (belief: 58.61%, actual success rate: 49.89%) and that others donate less to carbon offset (belief: £0.84, actual donation rate: £0.96).

In Column (1) of Table 4.7, we regress donation in Part 2 on *Success*, a participant's belief on how much other participants donated, and an interaction term for both variables. We restrict the sample to the treatments in which a license could be acquired (*LOW* and *HIGH*).

We find a significant positive correlation between an individual's own offset and his or her beliefs about others' offset (coeff $= 0.719$, p-value < 0.001). The interaction term is positive and significant on the 10%-level (coeff $= 0.158$, p-value $= 0.073$), which suggests that moral balancing correlates with a belief that other participants donated little. However, when controlling for variables for which we observe significant differences between *Success* and *Failure* in Part 1, the interaction becomes insignificant (coef $= 0.100$, p-value $= 0.235$).

We see in Columns (3) and (4) that a participant's belief regarding the proportion of successful participants does not predict donation in Part 2 (p-values ≥ 0.475). Also, the interaction between *Success* and belief is not significant (p-values ≥ 0.110), indicating that the belief regarding others participants' success rate in Part 1 does not moderate moral balancing.

[8]For every guess that was less than 5% away from the actual value, participants received an extra bonus payment of £0.20.

Table 4.7. Regressions for Belief's Influence on Moral Balancing.

	Dependent Variable: Donation			
	(1)	(2)	(3)	(4)
Success	-0.193**	-0.101	0.149	0.294
	(0.091)	(0.086)	(0.207)	(0.201)
Belief Donation Others	0.719***	0.644***		
	(0.068)	(0.066)		
Success × Belief Donation Others	0.158*	0.100		
	(0.088)	(0.084)		
Belief Success Others			0.0004	0.001
			(0.002)	(0.002)
Success × Belief Success Others			-0.004	-0.005
			(0.003)	(0.003)
Intercept	0.366***	-0.087	0.985***	0.118
	(0.073)	(0.146)	(0.093)	(0.172)
Controls B	No	Yes	No	Yes
Observations	600	600	600	600
R^2	0.306	0.402	0.009	0.203

Note: This table presents the results of OLS regressions for Donation in Part 2. The sample only contains the *LOW* and *HIGH* treatments. *Success* takes the value 1 if the participant succeeded in the slider task of Part 1, 0 otherwise. "Belief Donation Others" is a participant's estimation of the average donations of other participants, "Belief Success Others" refers to a participant's estimation of the percentage of successful participants in slider task in Part 1. In Columns (2) and (4) we add additional controls for gender, age, having children, altruism, education, political orientation, opinion on whether climate change is predominantly caused by humans, and whether participants agree that their actions matter in fighting climate change. Robust standard errors in parentheses. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

4.6 Discussion and Conclusion

Motivated by the increasing awareness for environmentally-friendly behavior, we explore in an incentivized online experiment how decision-making in pro-environmental behavior is affected by moral balancing. We add to the literature by testing whether the magnitude of the initial action influences the size of the moral balancing effect. Participants first work on a real-effort task. Depending on the treatment, they either acquire a 10 kg CO_2 offset, a 100 kg CO_2 offset, or a payoff of £0.2 for themselves. Next, participants receive a windfall endowment and decide how much to donate to carbon offset, and how much of it they keep for themselves. We investigate if participants engage in moral balancing, meaning whether they base their offsetting decisions in the second part on previous success or failure in the real-effort task. Since the treatments vary in how much CO_2

can be offset by succeeding in the real-effort task, we can investigate if moral balancing depends on the magnitude of the initial action.

We find evidence for moral balancing in offsetting decisions. Pooling the treatments in which participants can acquire carbon offset, participants who succeeded in the slider task donate on average less than those who failed. The difference is significant at the 10%-level. Regarding the magnitude of the initial pro-environmental action that varies exogenously across treatments, we do not find consistent evidence that it affects the size of the moral balancing effect. In addition, we find that environmental concerns moderate moral balancing. Individuals with the highest level of environmental concerns do not base their offset decision on success in the previous task, whereas somewhat less concerned individuals engage in moral balancing in the treatment where they could offset 100 kg of carbon emissions.

Our findings have implications for designing environmental and green marketing campaigns and the welfare evaluation of the voluntary carbon offsetting market. For example, campaigns targeting somewhat less environmentally-conscious individuals are sensitive to moral balancing and might consequently lead to less environmentally friendly behavior in the aftermath of the campaign. Moreover, environmental campaigns that stress that every act counts could backfire, as moral balancing, in general, seems to not systematically depend on the size of the initial action. Therefore, it could be beneficial to limit environmental messaging to promoting actions with greater positive environmental impact, in order to limit negative adverse spillover effects. Evaluating the benefits of the voluntary carbon offset market solely based on the total amount of carbon offset might lead to an overestimation of its positive impact. If individuals engage in moral balancing and behave less environmentally friendly after acquiring a license, it decreases the positive impact of the initial offset. Our findings stress the importance of considering moral balancing effects when targeting pro-environmental behavior.

5

Conclusion

In concluding this doctoral thesis, we have explored the intricate role of round numbers in decision-making across various contexts in Chapters 2 and 3, while Chapter 4 shifted the focus to the intersection of behavioral and environmental economics, examining the concept of moral balancing in environmental actions.

Our investigation began with the analysis of real estate transactions in Germany, as detailed in Chapter 2. Consistent with prior research, we find significant clustering of transactions at round-number prices for residential real estate, when considering a large sample of real estate transactions in the German market. We also find pronounced round-number effects in commercial real estate markets, where stakes are even higher and market participants arguably more experienced. When controlling for stake size and the type of object of real estate, we find that professionals settle negotiations significantly less often at round-number prices than do non-professionals. However, the fraction of round-number prices still remains substantial, even for professionals. For a subset of objects in our sample, for which we have additional information on the object's characteristics, we are able to model sales prices, thereby obtaining evidence suggesting that objects sold at round-number prices trade at a premium relative to their predicted value. Finally, our analysis documents that what is salient in bargaining (and hence, influences the bargaining outcome) seems to depend on culture. In particular, in Germany, "quarters" (for example in coinage or as a unit of measurement) do not play a particularly prominent role. This seems to be reflected in the finding of a lack of pronounced clustering at prices that are evenly divisible by 25,000 in the German real estate market.

Adding to these findings, Chapter 3 dug deeper into the reasons behind round-number clustering. The study tries to disentangle whether round-number clusters in observational data can be attributed to preferences for round numbers (*round-number bias*) or round numbers being used as a means to facilitate finding a solution to a coordination problem (*focal point*). Analyzing observational data from eBay negotiations, we find that negotiations with a final price that was round are on average shorter, as measured by both, total duration and the length of the offer-counteroffer sequence. We develop a novel experimental framework to further analyze the differences between round-number effects in individual and cooperative settings. The experiment was conducted online via Amazon MTurk. Not only do we find that also in our experiment round numbers are associated with faster agreements, but also find evidence for both of the aforementioned channels, bias and focal point, in shaping round-number clusters.

Chapter 4 shifted our attention to the intersection between behavioral economics and environmental economics. It examines the concept of moral balancing in pro-environmental decision-making. Through an online experiment where participants could earn a moral license by successfully completing a task that offset carbon emissions, we observe moral balancing. Participants who earned this license by completing the task donate less to carbon offset initiatives in a subsequent dictator game when compared to those who failed the task and did not earn the license. We exogenously vary the carbon offset amount in the task, but the findings are inconclusive, indicating that the effect of moral licensing might only be weakly connected to the actual environmental impact of the initial action, if at all. When analyzing the heterogeneity of licensing effects across different groups, we find that participants with a high concern for global warming do not show moral balancing but remain consistent in their actions, while those less concerned engage in moral licensing behavior. These results have implications on how we assess the welfare effects of environmentally driven policies and marketing campaigns.

The goal of this dissertation was to gain a better understanding of economic behavior. I hope I succeeded in this task. For each of the projects discussed in this dissertation, decisions had to be made on how to model certain issues or how to design economic experiments. I believe it is not only possible but likely that other researchers would have made different decisions. Thus, the various projects in this dissertation can always represent only one perspective on the respective issues. If other researchers review these projects, improve aspects of the models or experiments, or view them from an entirely different angle, we are able to gravitate closer to the truth. Hopefully, my work can spark ideas on how the issues addressed here can be further and possibly better investigated. It is in this way, that I hope to have made my modest contribution to this process called science.

Appendices

Appendix A

Appendix: Chapter 1

A.1 Additional Results

Figure A1. Average Sales Prices of Residential Real Estate Transactions over Time: Family Homes (Dotted Line) and Condominiums (Solid Line)

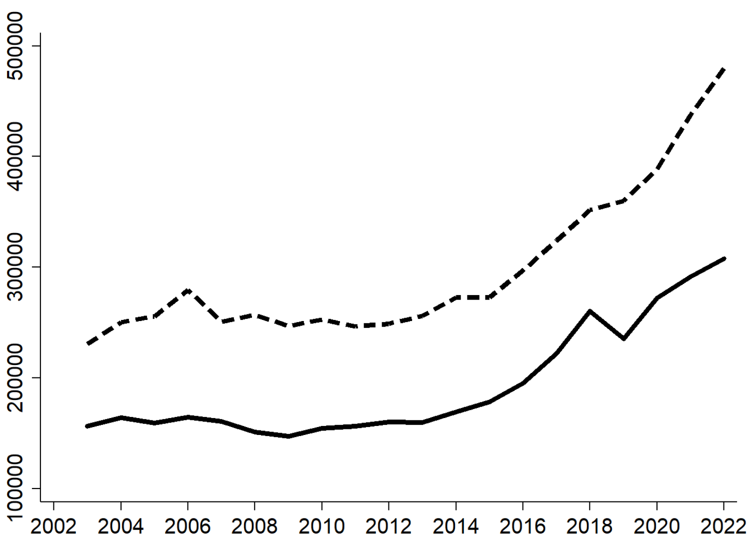


Table A1. Regression Analysis of the Number of Transactions Across Sales Prices (for Sales Prices Weakly Below 2,300,000 Euro)

	Residential Real Estate			Commercial Real Estate	
	(1)	(2)	(3)	(4)	(5)
D5000	2085.25***	1435.82***	1452.60***	704.98***	708.52***
	(113.13)	(150.48)	(152.95)	(56.43)	(57.20)
D10000		1298.86***	1268.00***	1534.32***	1527.24***
		(200.98)	(207.72)	(76.08)	(78.31)
D25000	-261.86	387.57	356.72	321.19***	314.11***
	(304.45)	(317.77)	(322.13)	(120.29)	(121.73)
D50000	2578.03***	1279.16***	1310.17***	2655.80***	2662.96***
	(405.98)	(449.44)	(452.60)	(170.13)	(171.17)
Dabove50000			-127.31		-38.93
			(138.13)		(48.37)
Dbelow50000			3.88		10.60
			(135.26)		(48.37)
7th Order Price Polynomial	Yes	Yes	Yes	Yes	Yes
Adjusted R-Squared	0.62	0.63	0.63	0.72	0.72
Observations	2061	2061	2061	2290	2290

Note: The note below Table 2.2 applies, except that in the current table all sales prices weakly below 2,300,000 are considered.

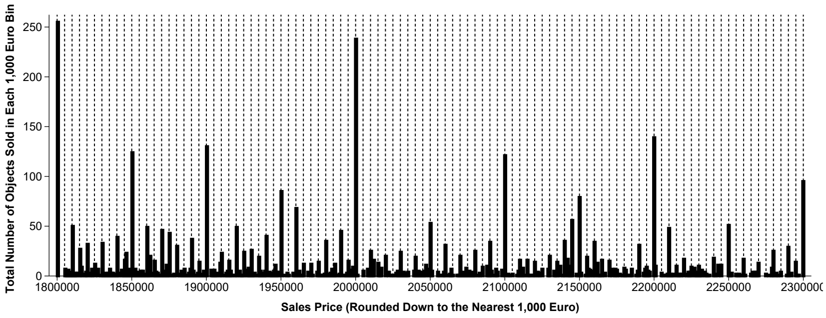
Table A2. Regression Analysis of the Number of Transactions Across Sales Prices (for All Sales Prices)

	Residential Real Estate			Commercial Real Estate	
	(1)	(2)	(3)	(4)	(5)
D5000	1603.66*** (111.67)	1139.26*** (155.90)	1151.22*** (158.31)	218.29*** (27.75)	218.65*** (28.21)
D10000		794.61*** (186.71)	772.25*** (194.76)	317.10*** (31.00)	316.02*** (32.70)
D25000	-198.27 (301.00)	279.33 (320.42)	256.12 (325.30)	53.27 (52.28)	52.19 (53.32)
D50000	1350.21*** (343.15)	609.21 (383.90)	633.79 (388.18)	337.10*** (58.65)	338.19*** (59.58)
Dabove50000			-132.843 (158.51)		-11.32 (28.42)
Dbelow50000			25.81 (151.06)		5.49 (28.15)
7th Order Price Polynomial	yes	yes	yes	yes	yes
Adjusted R-Squared	0.20	0.20	0.20	0.16	0.15
Observations	2924	2924	2924	9095	9095

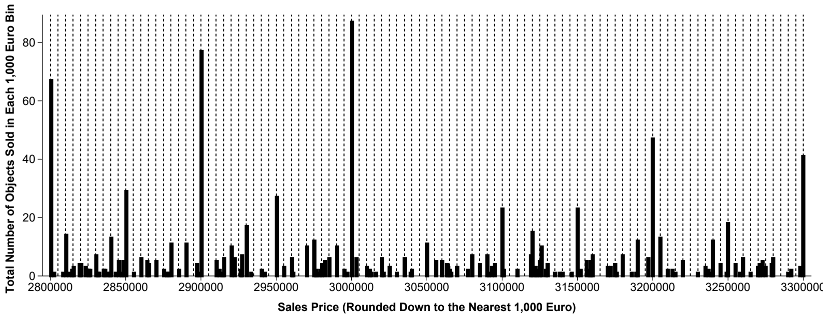
Note: The note below Table 2.2 applies, except that in the current table all sales prices are considered.

Figure A2. Number of Transactions Across Sales Prices (High Price Ranges)

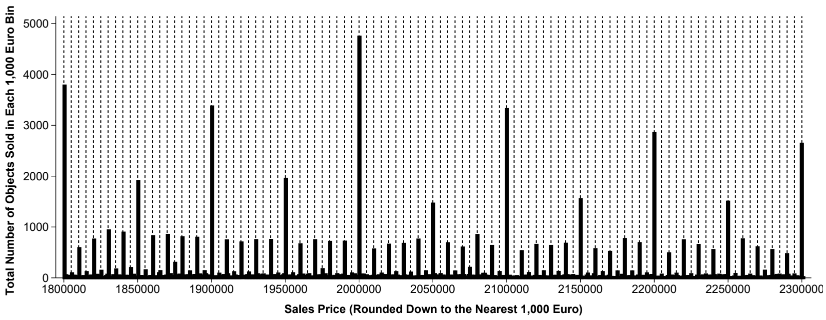
(a) Residential Real Estate (Price Range: 1,800,000 - 2,300,000 Euro)



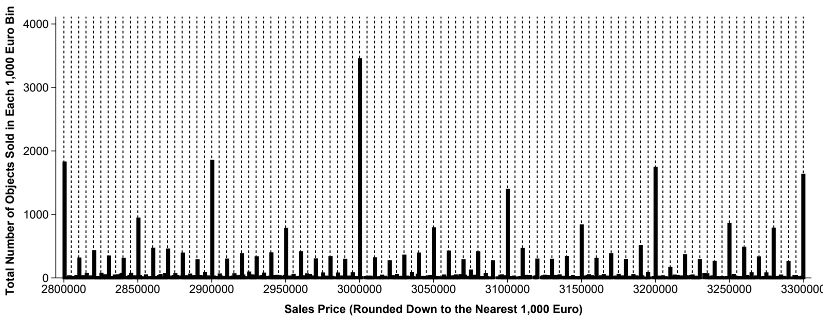
(b) Residential Real Estate (Price Range: 2,800,000 - 3,300,000 Euro)



(c) Commercial Real Estate (Price Range: 1,800,000 - 2,300,000 Euro)



(d) Commercial Real Estate (Price Range: 2,800,000 - 3,300,000 Euro)



Note: The note below Figure 2.1 applies, except that Panels (a) and (c) ((b) and (d)) consider objects with sales prices between 1,800,000 and 2,300,000 Euro (2,800,000 and 3,300,000 Euro).

Appendix B

Appendix: Chapter 2

B.1 eBay Data Processing

Backus, Blake, Larsen, and Tadelis (2020) provide two data sets **threads** and **lists**.[1] The term *thread* identifies a sequence of offers for a given buyer and seller pair bargaining over a given item. Each *observation* in the first data set consists of an offer from the buyer or the seller in a given thread with a time-stamp of the creation time and the response in plain text. Additionally, the current status of the offer (accepted, declined, countered) can be found. Hence, one thread can consist of multiple observations, but the last (most recent) observation covers the final decision and the final price, for which the bargaining parties settled. The second data set includes additional information about the item listing on the eBay platform. The unique item ID connects both data sets. The author's codebook provides more details on the covered variables.

We develop two filtering procedures that collect the data for the empirical analysis. The first one is written for the package data.table for R 3.6 and uses the data set threads. It creates a unique identifier for successful threads and calculates the duration of the negotiation, i.e., the time between the buyer's first offer and the final acceptance marking a successful thread. Additionally, it indicates whether the final price is round or not. The pseudo-code is shown in Algorithm 1. In particular, it first selects only the successful negotiations and then computes the duration for the different cases. Lastly, it creates the indicator for round numbers. The second algorithm is written for STATA MP 16.0, and the pseudo-code can be seen in Algorithm 2. It links the information of the item in the initial listing from the data set **lists** by using the unique item ID to the

[1] The data set is publicly available at `https://www.nber.org/research/data/best-offer-sequential-bargaining`.

data set that the previous algorithm created.

In a last step, the complete data set is saved in the distribution-friendly csv-format and for the descriptive and regression analysis in the dta-format.

Table B1 and Table B2 summarize the distribution of the observations.

```
Data: eBay data threads
Result: data set (i, p_i, Δt_i, I(·)_i))
——————————————————;
for all observations (n = 47,377,200) do
    if offer was accepted (status_id=1) or auto-accepted (status_id=9) then
        keep ID of item, seller, buyer, thread;
        create new unique ID (i's) for each kept quadruple (n = 12,018,417)
    end
end
keep observations in threads by above new ID (n = 17,892,293);
transform plain text dates to interpretable dates;
for all i with only one observation do
    calculate Δt_i between creation date and response date;
    save by i: item id, price, Δt_i;
end
collect (n = 8,534,338);
for all i with more than one observation do
    order by creation date in ascending order;
    calculate Δt_i between creation date of first observation and response date of
      last observation;
    save by i: item id, price p_i, Δt_i;
end
collect (n = 3,317,934);
merge cases and reduce to one observation per ID (n = 11,301,474);
create I(p_i ∈ Υ)_i;
remove duplicates(n = 4,159);
save data set to external file;
get item's IDs (n = 11,297,315)and sort;
save item ID to external file;
```

Algorithm 1: Procedure in R 3.6.1

Data: eBay data **lists**
Result: data set (i, X_i)
————————————;
for *all observations* $(n = 98, 307, 281)$ **do**
  order by item ID;
  if *item ID is in item ID file* **then**
    keep
  end
end
collect $(n = 11, 297, 315)$;
merge with duration file (1:1);

Algorithm 2: Procedure in STATA 16

Table B1. Conditions of the Items in the eBay Data

	N	%	$\sum$%
New	2,300,284	28.24	28.24
New other	695,968	8.55	36.79
New with defects	35,918	0.44	37.23
Manufacturer refurbished	12,032	0.15	37.38
Seller refurbished	36,985	0.45	37.83
Like New	315,149	3.87	41.70
Used	4,286,288	52.63	94.33
Very Good	219,391	2.69	97.02
Good	114,677	1.41	98.43
Acceptable	32,636	0.40	98.83
For parts / not working	95,047	1.17	100.00
Total	8,144,375	100.00	

Note: The table shows the distribution of the item's condition of the eBay data set of Backus, Blake, Larsen, and Tadelis (2020) after applying our algorithm. The conditions are ordered by their numeric ID in the data set.

Table B2. Categories of the Items in the eBay Data

	N	%	$\sum$%
Collectibles	1,414,232	12.75	12.75
Everything else	42,679	0.38	13.14
Toys and Hobbies	658,335	5.94	19.07
Dolls and Bears	125,574	1.13	20.21
Stamps	106,335	0.96	21.16
Books	302,895	2.73	23.90
Jewelry and Watches	711,072	6.41	30.31
Consumer Electronics	263,319	2.37	32.68
Specialty Services	930	0.01	32.69
Art	108,317	0.98	33.67
Musical Instruments and Gear	178,088	1.61	35.27
Cameras and Photo	144,795	1.31	36.58
Pottery and Glass	209,958	1.89	38.47
Sporting Goods	421,476	3.80	42.27
Video Games and Consoles	184,018	1.66	43.93
Pet Supplies	14,453	0.13	44.06
Tickets and Experiences	28,727	0.26	44.32
Baby	28,417	0.26	44.58
Travel	10,717	0.10	44.67
Real Estate	81	0.00	44.67
Coins and Paper Money	283,656	2.56	47.23
DVDs and Movies	108,607	0.98	48.21
Music	212,624	1.92	50.13
Clothing Shoes and Accessories	2,487,553	22.43	72.56
Home and Garden	330,926	2.98	75.54
Business and Industrial	393,465	3.55	79.09
Crafts	93,174	0.84	79.93
Cell Phones and Accessories	135,474	1.22	81.15
Antiques	202,743	1.83	82.98
Health and Beauty	136,147	1.23	84.21
Entertainment Memorabilia	135,324	1.22	85.43
Computers or Tablets and Networking	356,458	3.21	88.64
Sports Mem Cards and Fan Shop	1,258,065	11.34	99.99
Gift Cards and Coupons	1,645	0.01	100.00
Total	11,090,279	100.00	

Note: The table shows the distribution of the item's category of the eBay data set of Backus, Blake, Larsen, and Tadelis (2020) after applying our algorithm. The categories are ordered by their numeric ID in the data set.

B.1.1 Detailed eBay Regression Results

Table B3. Detailed Regression Results of Duration or Number of Periods on Round Prices

	(1) Duration		(2) Duration		(3) Periods		(4) Periods	
Round numbers	-24.82***	(4.91)	-53.02***	(5.21)	-0.17***	(0.00054)	-0.19***	(0.00064)
Condition								
New			0.00	(.)			0.00	(.)
New other			11.65	(10.4)			-0.05***	(0.0014)
New with defects			199.56***	(45.3)			-0.01**	(0.0051)
Manufacturer refurbished			-34.89	(57.2)			0.09***	(0.0098)
Seller refurbished			-11.81	(33.6)			0.04***	(0.0054)
Like New			32.84	(21.0)			-0.05***	(0.0022)
Used			-79.84***	(5.98)			-0.10***	(0.00079)
Very Good			-40.32**	(20.5)			-0.10***	(0.0025)
Good			-51.99**	(26.2)			-0.11***	(0.0031)
Acceptable			-36.38	(43.4)			-0.11***	(0.0051)
For parts / not working			332.98***	(30.2)			-0.08***	(0.0033)
Category								
Collectibles			0.00	(.)			0.00	(.)
Everything else			-1159.55***	(9.80)			-0.59***	(0.0011)
Toys and Hobbies			-138.58***	(12.3)			0.10***	(0.0015)
Dolls and Bears			-310.26***	(20.5)			-0.04***	(0.0026)
Stamps			167.65	(394.9)			-0.06*	(0.037)
Books			-165.24***	(23.4)			-0.07***	(0.0025)
Jewelry and Watches			-18.92	(13.3)			0.13***	(0.0016)
Consumer Electronics			-191.30***	(14.6)			0.19***	(0.0022)
Art			81.57	(115.6)			0.02*	(0.0086)
Musical Instruments and Gear			-113.83***	(16.0)			0.22***	(0.0026)
Cameras and Photo			-162.89***	(18.5)			0.21***	(0.0029)
Sporting Goods			-194.64***	(12.6)			0.19***	(0.0018)
Video Games and Consoles			-374.33***	(17.3)			0.25***	(0.0028)
Pet Supplies			-246.58***	(54.7)			0.04***	(0.0078)
Baby			-363.80***	(20.7)			0.14***	(0.0058)
Travel			-185.42***	(42.2)			0.18***	(0.010)
Coins and Paper Money			30.11	(239.2)			0.01	(0.025)
DVDs and Movies			-324.70***	(23.1)			0.03***	(0.0031)
Music			8.51	(23.6)			-0.06***	(0.0020)
Clothing Shoes and Accessories			-206.50***	(9.79)			0.05***	(0.0010)
Home and Garden			-211.41***	(12.9)			0.06***	(0.0018)
Business and Industrial			530.35***	(21.2)			0.06***	(0.0017)
Crafts			-412.20***	(19.1)			-0.13***	(0.0027)
Cell Phones and Accessories			-645.60***	(11.2)			0.26***	(0.0030)
Antiques			120.52	(242.8)			-0.10***	(0.037)
Health and Beauty			-251.97***	(19.4)			0.01***	(0.0026)
Entertainment Memorabilia			255.81*	(137.1)			0.02**	(0.012)
Computers or Tablets and Networking			-256.83***	(12.6)			0.13***	(0.0019)
Sports Mem Cards and Fan Shop			149.11***	(18.7)			0.11***	(0.0020)
Constant	1059.31***	(3.02)	1165.92***	(9.80)	1.58***	(0.00038)	1.59***	(0.0011)
N	11,090,279		8,144,375		11,090,279		8,144,375	

Note: The table reports OLS results for the two dependent variables, Duration and Periods. *Duration* denotes the time between the first observation and the last observation of a thread in minutes. *Periods* denotes the number of offers made between seller and buyer. The table reports the coefficient of the round number dummy as *Round numbers*. There are 11 condition dummies for the item, where the baseline is "New". The meta category of the item has 38 categories and is considered with a corresponding number of dummies, where the baseline is "Collectible". Missing observations are due to incomplete recordings of condition or category. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

B.2 Additional Experimental Results

B.2.1 Acceptance and Rejection Times

In this section, we provide details on the decisions times for acceptances and rejections separately in addition to Section 3.4. Table B4 summarizes the decision times when an offer was accepted. The discussion can be found in Section 3.4.

Table B4. Decision Times Conditional on Acceptance

		Treatment	
Offer type	Total	Single	Partner
Round	9.81	9.88	9.75
NonRound	11.05	10.55	11.52

Note: Average decision times are reported in seconds.

Table B5 summarizes the decision times when an offer was rejected. When the observations of rejections are pooled across treatments, we find significantly quicker rejections when a round offer was made (t-Test: 7.92s vs. 8.65s; p=0.0527). Furthermore, the difference in decision times between offer types for rejections is the smallest (0.73s) compared to the previous cases when decisions were pooled (1.11s) or when only acceptances were considered (1.24s). When we control for the treatments, we find that participants in Partner reject round offers significantly quicker (t-Test: 7.96s vs. 8.95s; p=0.0479) while the difference in Single (t-Test: 7.87s vs. 8.37s; p=0.3730) is not significant.

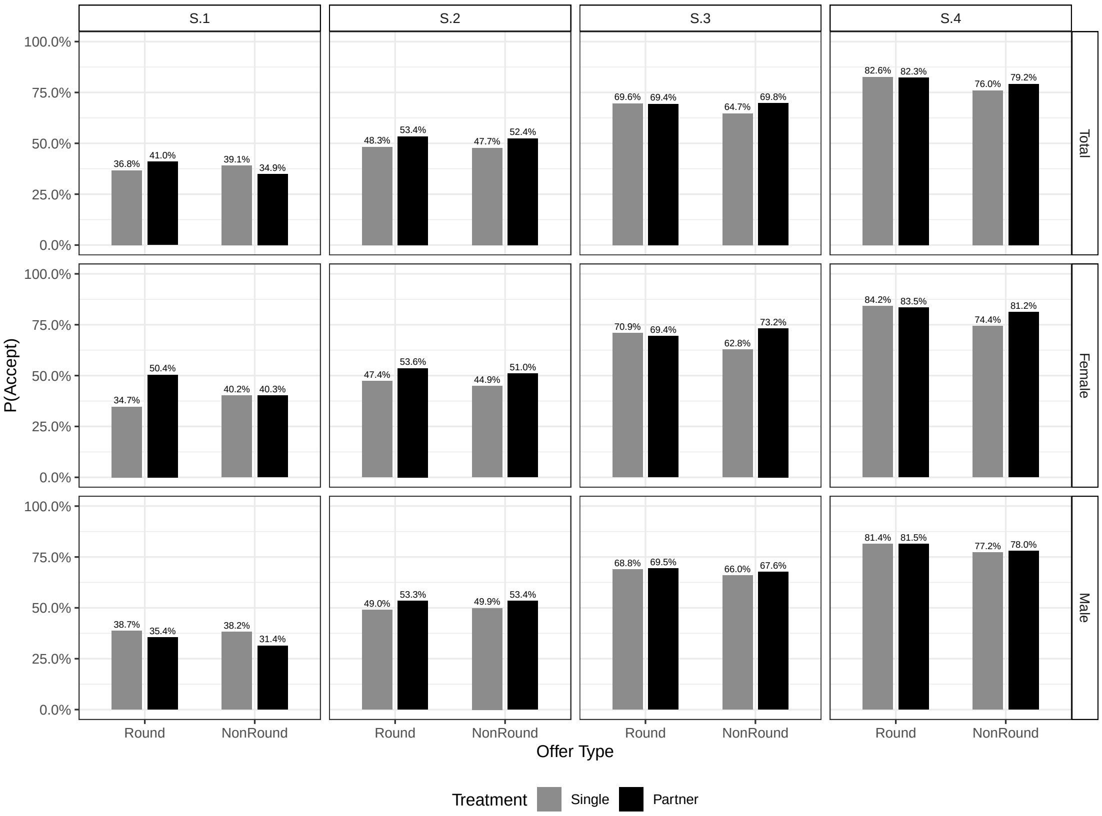
Table B5. Decision Times Conditional on Rejection

		Treatment	
Offer type	Total	Single	Partner
Round	7.92	7.87	7.96
NonRound	8.65	8.38	8.95

Note: Average decision times are reported in seconds.

B.2.2 Acceptance Frequency Bar Plot

Figure B1. Acceptance Frequencies as Bar Plot for Each Segment.



Note: Acceptance frequencies as bar plot for each segment. The rows of the figure correspond to the total, female and male sample as indicated by the right legend. Each column corresponds to a segment of offer shares (S.1, S.2, S.3, S.4) which are equally wide. For each cell of the figure, the share of accepted round and non-round offers in the two treatments is illustrated. The gray bars represent the Single treatment, and the black bars correspond to the Partner treatment.

B.2.3 Regression Analysis

To evaluate the robustness of our results in Section 3.4, we estimate a linear probability model by OLS, where standard errors are clustered on the individual-level. The dependent variable is the binary variable offer acceptance. We control for offer share, treatment, offer type, and the interaction of the latter two.

As we have seen in our analysis from Figure 3.7, a higher propensity to accept is associated with round numbers in all treatments, but they are likely caused by different channels for high and low offer shares. Thus, it is not surprising to see significant round-number effects and no significant interaction term without restricting the offer share, as the round-number dummy simply captures the whole round-number effect. To control for this and to keep the estimated models as parsimonious as possible, we divide our sample into the four offer share segments as before and estimate the same model separately for each segment. Table B6 shows the results of the estimations. The first three columns summarize the estimates for S.1. We find significant positive interaction terms. In the total sample, being in Partner and receiving a round offer increases the acceptance frequency by 8.4%p ($p = 0.080$) on average. The effect is especially pronounced in the female sample, where, ceteris paribus, a round offer has a 15.7%p higher chance of being accepted ($p = 0.035$). Thus, round numbers have a higher acceptance frequency in Partner for lower offer shares (**Result 2**). For S.2, we do not find any significant treatment or round-number effects. Again, this is in line with the graphical analysis (Column (4) to (6)). For S.3 (Column (7) to (9)), in the total sample, there are significant treatment and round number effects. The interaction is insignificant. This confirms our argument for round-number bias (**Result 1**). Only looking at the female sample, the results are qualitatively similar, but now the interaction is significantly negative. This could already be seen in the figures above and further confirms our conjecture that in Partner, subjects were more careful in their decision-making, thereby reducing potentially unconscious biases for round numbers. For S.4, we get qualitatively similar results. Only now the interaction term for the female sample becomes insignificant, yet still has the negative sign as in S.3 (Column (10) to (12)).

Table B6. OLS Regression for Segements. Dependent Variable: Offer Acceptance.

	Segment 1			Segment 2			Segment 3			Segment 4		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Sample:	Total	Female	Male	Total	Female	Male	Total	Female	Male	Total	Female	Male
(Intercept)	0.420 ***	0.354 ***	0.461 ***	0.129	0.069	0.172	0.311 ***	0.195	0.386 ***	0.606 ***	0.809 ***	0.469 ***
	(0.056)	(0.085)	(0.076)	(0.081)	(0.126)	(0.106)	(0.096)	(0.152)	(0.124)	(0.087)	(0.129)	(0.117)
Offer Share	-0.125	0.208	-0.337	0.804 ***	0.881 ***	0.754 ***	0.513 ***	0.661 ***	0.420 **	0.168 *	-0.071	0.332 ***
	(0.203)	(0.313)	(0.265)	(0.174)	(0.273)	(0.228)	(0.142)	(0.226)	(0.182)	(0.094)	(0.141)	(0.125)
Treatment: Partner	-0.042	-0.000	-0.068	0.046	0.061	0.035	0.051 *	0.105 **	0.016	0.033	0.068 **	0.008
	(0.041)	(0.064)	(0.052)	(0.033)	(0.050)	(0.043)	(0.029)	(0.044)	(0.039)	(0.020)	(0.031)	(0.026)
Round Offer	-0.027	-0.049	-0.005	0.007	0.019	-0.003	0.048 *	0.080 **	0.026	0.076 ***	0.094 **	0.061 *
	(0.033)	(0.049)	(0.046)	(0.033)	(0.049)	(0.046)	(0.025)	(0.039)	(0.032)	(0.025)	(0.037)	(0.034)
Treatment: Partner x Round Offer	0.084 *	0.157 **	0.035	0.005	0.014	0.000	-0.053	-0.118 **	-0.010	-0.036	-0.076	-0.009
	(0.048)	(0.074)	(0.063)	(0.046)	(0.071)	(0.062)	(0.036)	(0.058)	(0.047)	(0.033)	(0.050)	(0.044)
N	1520	646	874	2120	899	1221	2464	988	1476	3136	1287	1849
R2	0.003	0.013	0.006	0.013	0.016	0.011	0.008	0.017	0.004	0.005	0.009	0.005

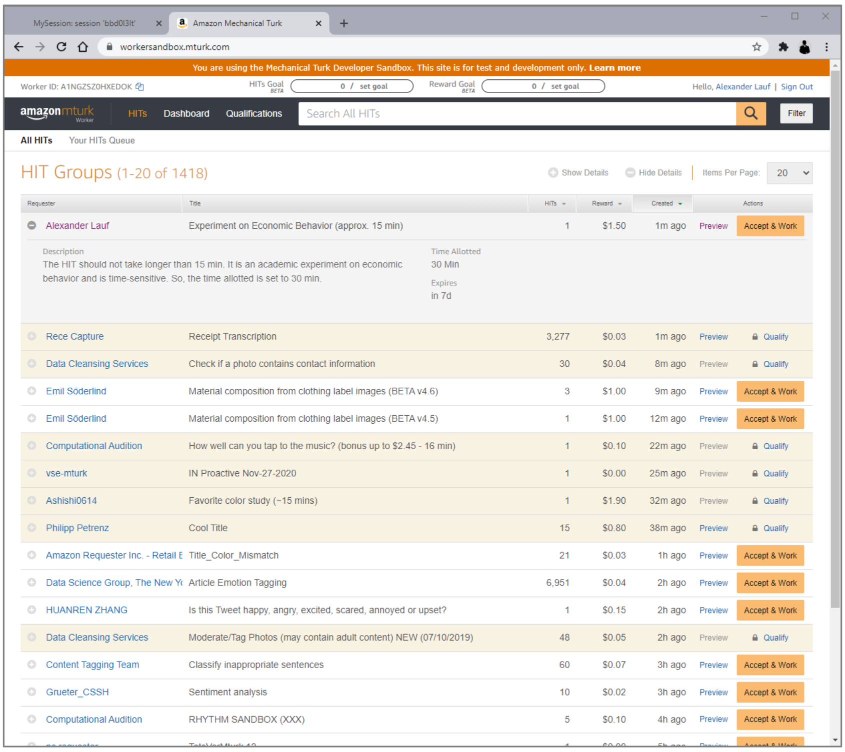
Note: The offer acceptance is a binary variable equal to 1 if the participant accepted an offer and 0 otherwise. Standard errors in parentheses are heteroskedasticity robust and clustered on the individual level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

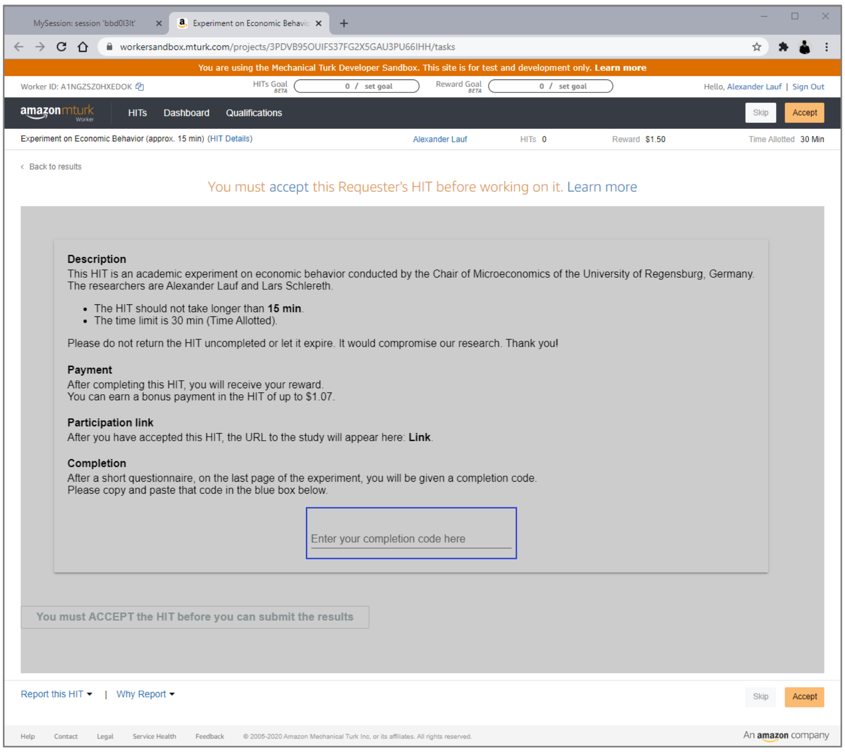
B.3 MTurk and oTree Instructions

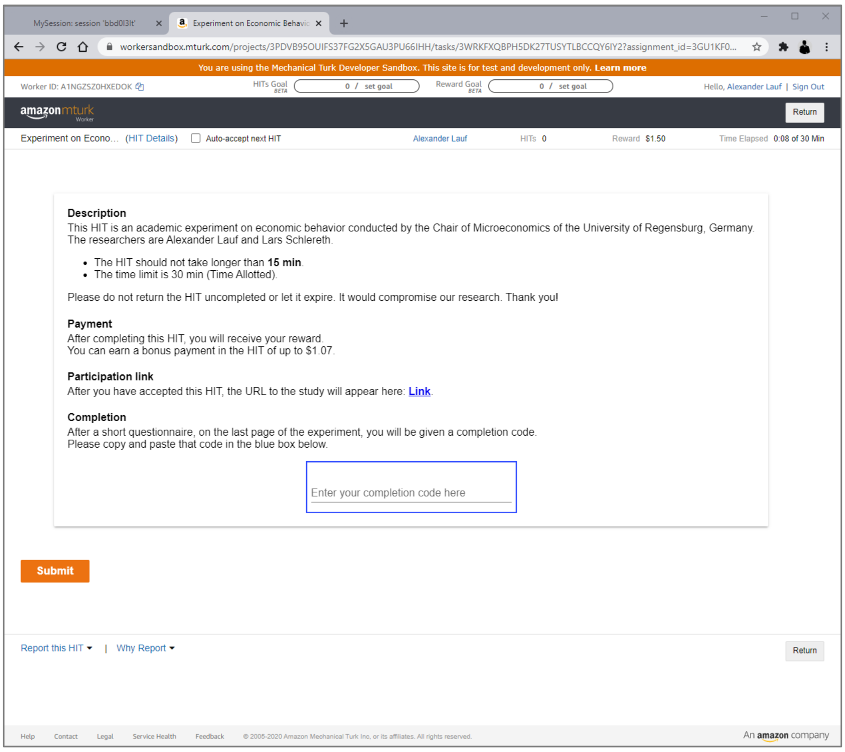
This section provides screenshots of the human intelligence task (HIT) published on Amazon Mechanical Turk under the name of Alexander Lauf as the requester and the instructions of the experiment in oTree for both treatments. Please note that these pictures represent websites. The oTree code is available on request. In particular, the following is covered:

1. HIT - Design and Description,
2. Experimental Design: Single,
3. Experimental Design: Partner.

B.3.1 HIT - Design and Description






You are using the Mechanical Turk Developer Sandbox. This site is for test and development only. Learn more
Worker ID: A1NGZSZ0HXEDOK
HITs Goal
Reward Goal
Hello, Alexander Lauf | Sign Out
amazon mturk
Return
Experiment on Econo... (HIT Details)
Auto-accept next HIT
Alexander Lauf
HITs 0
Reward $1.50
Time Elapsed 0:08 of 30 Min
Description
This HIT is an academic experiment on economic behavior conducted by the Chair of Microeconomics of the University of Regensburg, Germany.
The researchers are Alexander Lauf and Lars Schlereth.
The HIT should not take longer than 15 min.
The time limit is 30 min (Time Allotted).
Please do not return the HIT uncompleted or let it expire. It would compromise our research. Thank you!
Payment
After completing this HIT, you will receive your reward.
You can earn a bonus payment in the HIT of up to $1.07.
Participation link
After you have accepted this HIT, the URL to the study will appear here: Link
Completion
After a short questionnaire, on the last page of the experiment, you will be given a completion code.
Please copy and paste that code in the blue box below.
Enter your completion code here
Submit
Report this HIT
Why Report
Return
An amazon company

B.3.2 Experimental Design: Single

Welcome!

Universität Regensburg

Experiment on Economic Behavior

Thank you very much for participating!

This is an experiment on economic behavior conducted by the Chair of Microeconomics of the University of Regensburg (Alexander Lauf and Lars Schlereth).

In the experiment, you can earn points. These points will be converted to money.

The more points you earn, the more money you get.

Regardless of the points you earn, you get a participation fee of **$1.50** for completing the experiment.
This is the reward of the MTurk assignment.

On the next page, you will receive detailed instructions on what you have to do.

Thank you for your time!
Alex and Lars

Let's start

Instructions

What do you have to do?

In this experiment, you will make one decision in each of **10 periods**, i.e., a total of 10 decisions. In this experiment, you can earn points. After this experiment, the points will be converted into real money **(1000 Points = 1 Dollar)** and paid to you as a bonus later.

The points you earn in the experiment depend on your own decisions and the decisions of the computer in a way explained below.

In each period, you will receive an **offer**. The computer **randomly selects** one of the numbers in a blue box below. Each number is **equally likely** to be drawn. You can see the box for Period 1 below.

You can **accept** or **reject** the offer.

Line 1	1067	1033	1000
Line 2	967	933	900
Line 3	867	833	800
Line 4	767	733	700
Line 5	667	633	600
Line 6	567	533	500
Line 7	467	433	400
Line 8	367	333	300
Line 9	267	233	200
Line 10	167	133	100

Period 1

In each next period, **numbers are removed** from period to period. In Period 2, 'Line 1' is removed. In Period 3, additionally, 'Line 2' is removed. This is repeated until period 10, in which only the last line remains.

In each period, you decide to either **accept** or **reject** the offer. In subsequent periods, you can get a higher or lower offer, but keep in mind that the **three highest numbers** are removed in each period.

You can click on the right-arrow in the slider gallery below to see how these lines are removed.

Line 1	~~1067~~	~~1033~~	~~1000~~
Line 2	967	933	900
Line 3	867	833	800
Line 4	767	733	700
Line 5	667	633	600
Line 6	567	533	500
Line 7	467	433	400
Line 8	367	333	300
Line 9	267	233	200
Line 10	167	133	100

Period 2

After you have made your decisions for all 10 periods, your bonus in points is determined as follows:

The computer **randomly selects** a period out of Period 1 to 10. Then, the computer moves step-by-step from **this randomly selected period** to Period 10. The computer stops in the period, where for the first time you have accepted the respective period's offer. This offer is your bonus in points.

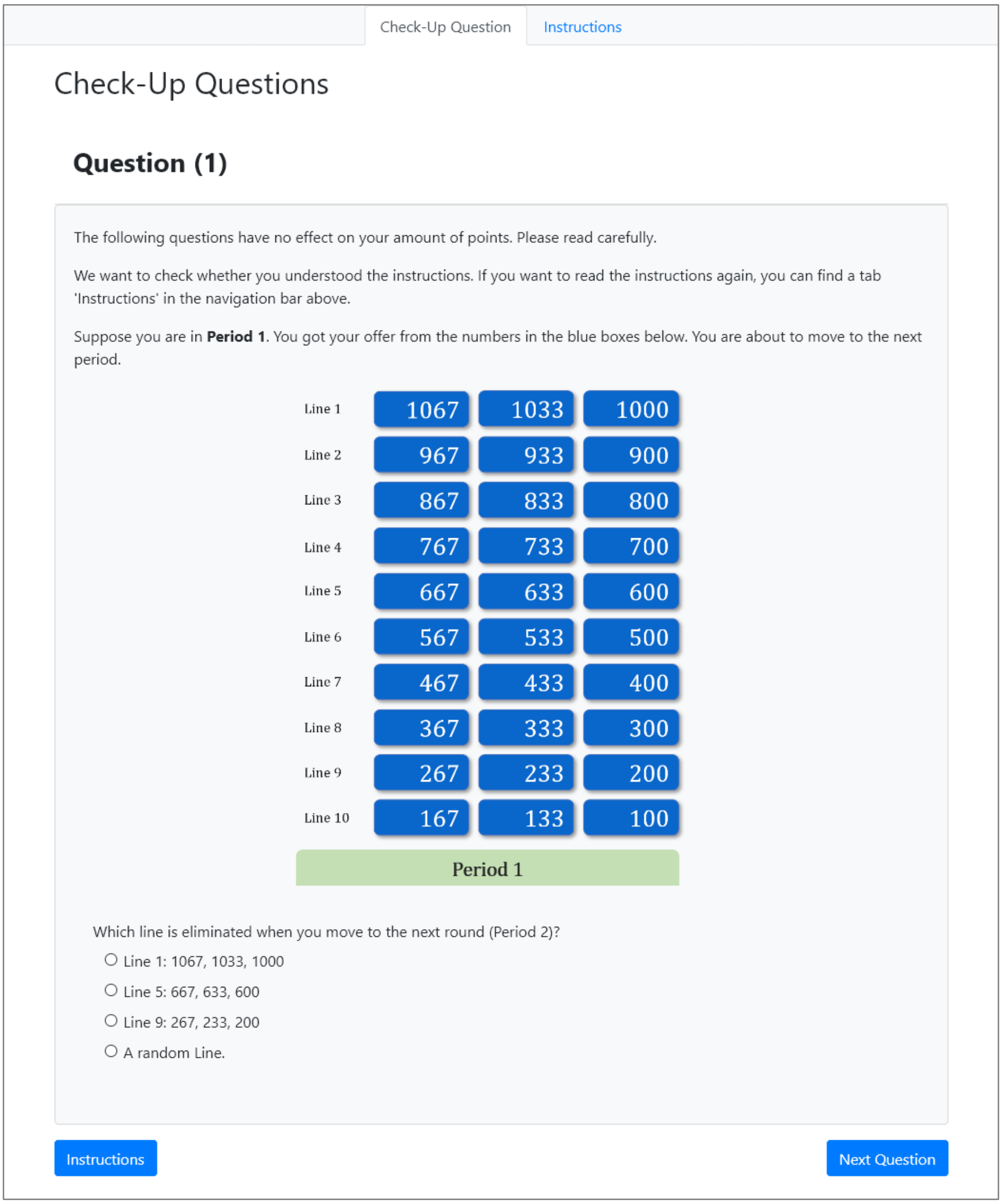
Therefore, you should play every period as if the computer selected the current period.

Your total payment in dollars is the sum of participation fee and your bonus in points converted to dollars (1000 Points = 1 Dollar):

$$\text{Payment} = \text{Participation Fee} + \frac{\text{Your Points}}{1000}$$

Now we ask you a few check-up questions. They do not have any impact on your payment. After you have finished the experiment, we will conduct a short survey.

Go to Check-Up Questions



Check-Up Question | Instructions

Check-Up Questions

Question (1)

The following questions have no effect on your amount of points. Please read carefully.

We want to check whether you understood the instructions. If you want to read the instructions again, you can find a tab 'Instructions' in the navigation bar above.

Suppose you are in **Period 1**. You got your offer from the numbers in the blue boxes below. You are about to move to the next period.

Line 1	1067	1033	1000
Line 2	967	933	900
Line 3	867	833	800
Line 4	767	733	700
Line 5	667	633	600
Line 6	567	533	500
Line 7	467	433	400
Line 8	367	333	300
Line 9	267	233	200
Line 10	167	133	100
Period 1			

Which line is eliminated when you move to the next round (Period 2)?

- ○ Line 1: 1067, 1033, 1000
- ○ Line 5: 667, 633, 600
- ○ Line 9: 267, 233, 200
- ○ A random Line.

Instructions | Next Question

Check-Up Question | Instructions

Check-Up Questions

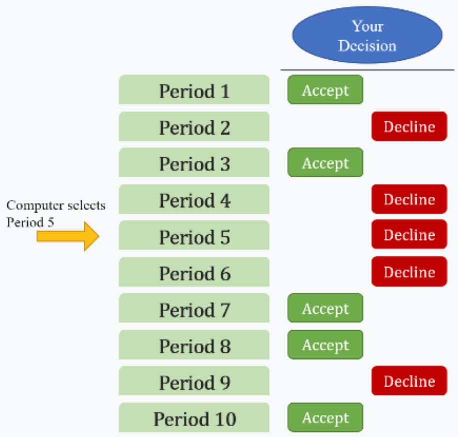
Question (2)

The following questions have no effect on your amount of points. Please read carefully.

We want to check whether you understood the instructions. If you want to read the instructions again, you can find a tab 'Instructions' in the navigation bar above.

Suppose you have already made **all decisions**. In the picture below, you can see all of your decisions.

The computer has selected **Period 5**. As mentioned in the instructions, the computer starts in Period 5 and goes step-by-step to Period 10.



From which period do you get the offer as final payment?

- ○ Period 5
- ○ Period 10
- ○ Period 7
- ○ A random Period.

Instructions | Next Question

Check-Up Question | Instructions

Check-Up Questions

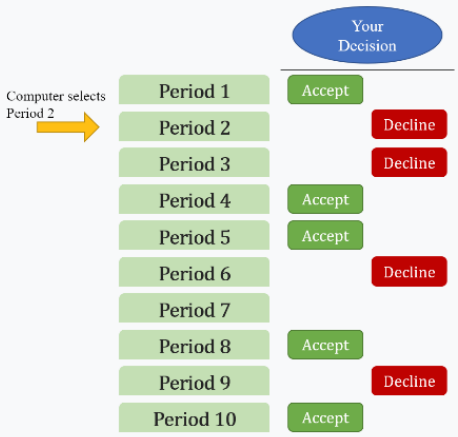
Question (3)

The following questions have no effect on your amount of points. Please read carefully.

We want to check whether you understood the instructions. If you want to read the instructions again, you can find a tab 'Instructions' in the navigation bar above.

Suppose you have already made **all decisions**. In the picture below, you can see all of your decisions.

The computer has selected **Period 2**. As mentioned in the instructions, the computer starts in Period 2 and goes step-by-step to Period 10.



From which period do you get the offer as final payment?

- ○ Period 4
- ○ Period 8
- ○ Period 9
- ○ A random Period.

Instructions | Next Question

Check-Up Question | Instructions

Check-Up Questions

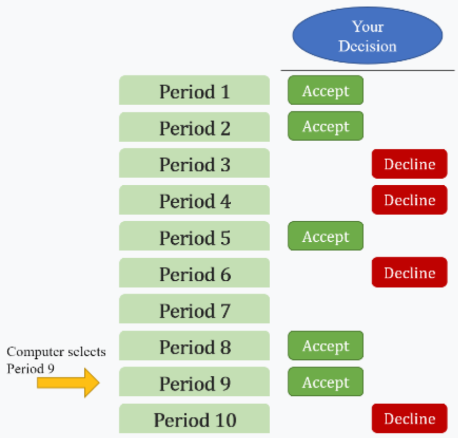
Question (4)

The following questions have no effect on your amount of points. Please read carefully.

We want to check whether you understood the instructions. If you want to read the instructions again, you can find a tab 'Instructions' in the navigation bar above.

Suppose you have already made **all decisions**. In the picture below, you can see all of your decisions.

The computer has selected **Period 9**. As mentioned in the instructions, the computer starts in Period 9 and goes step-by-step to Period 10.



From which period do you get the offer as final payment?

- ○ Period 2
- ○ Period 9
- ○ Period 5
- ○ A random Period.

Instructions | Continue

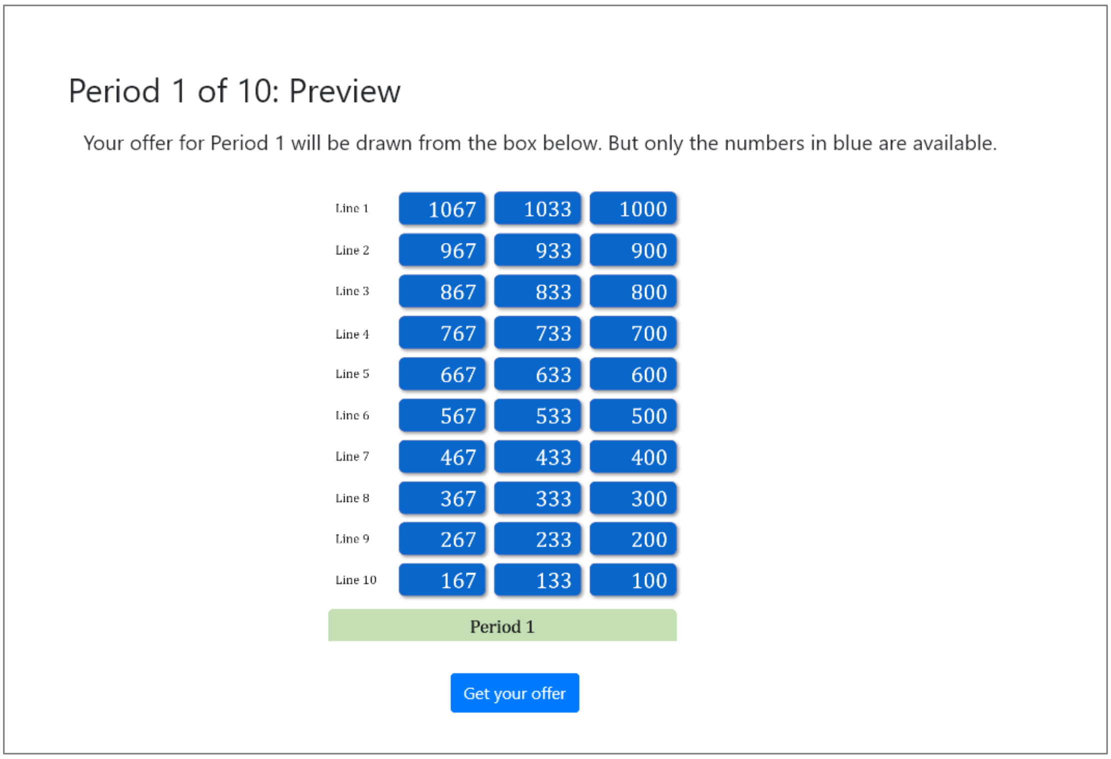
Start of the Experiment

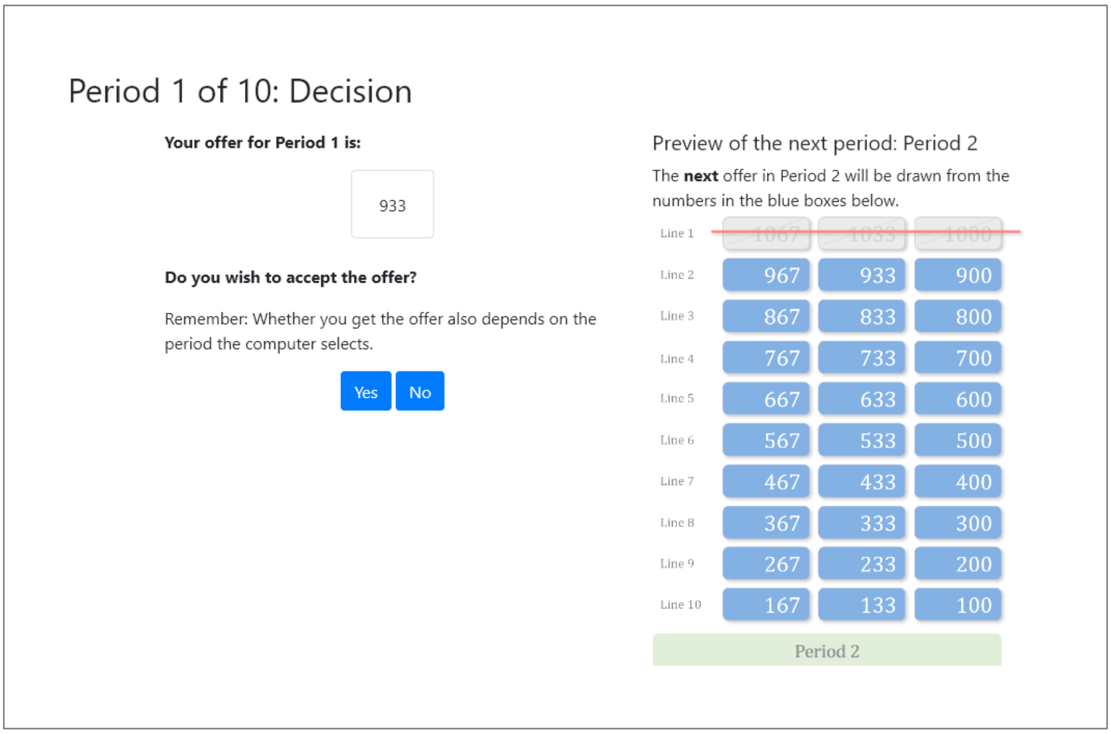
You have finished the check-up questions.

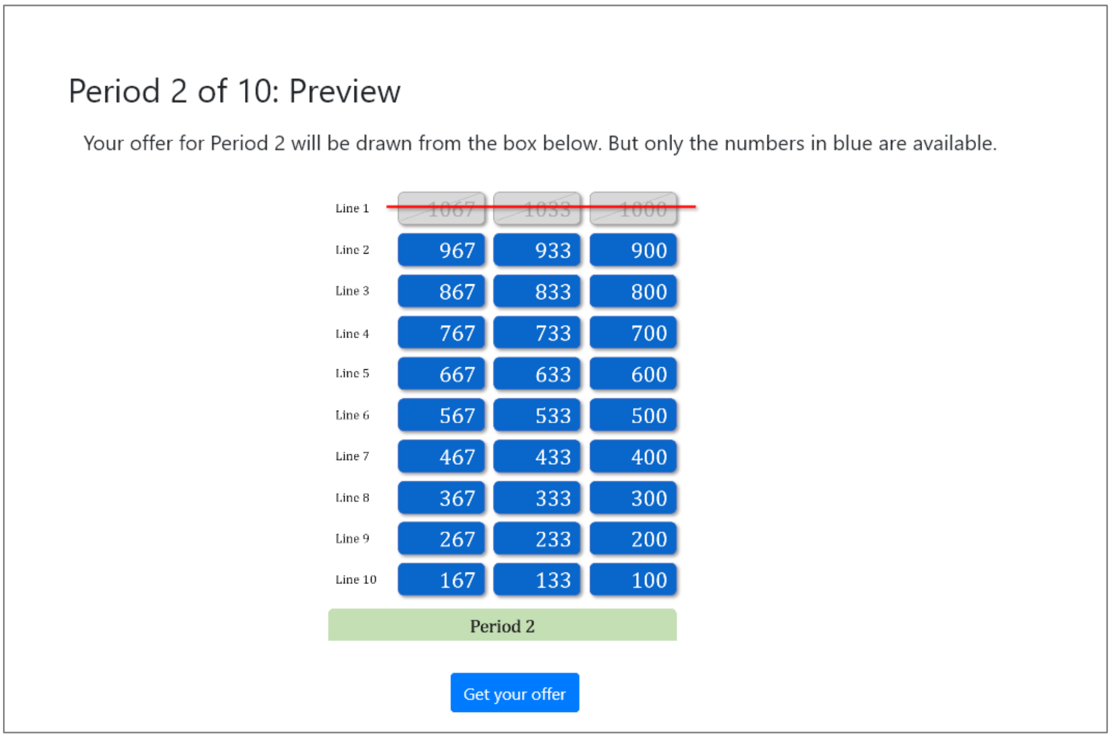
You can now start the experiment by clicking on the button below.

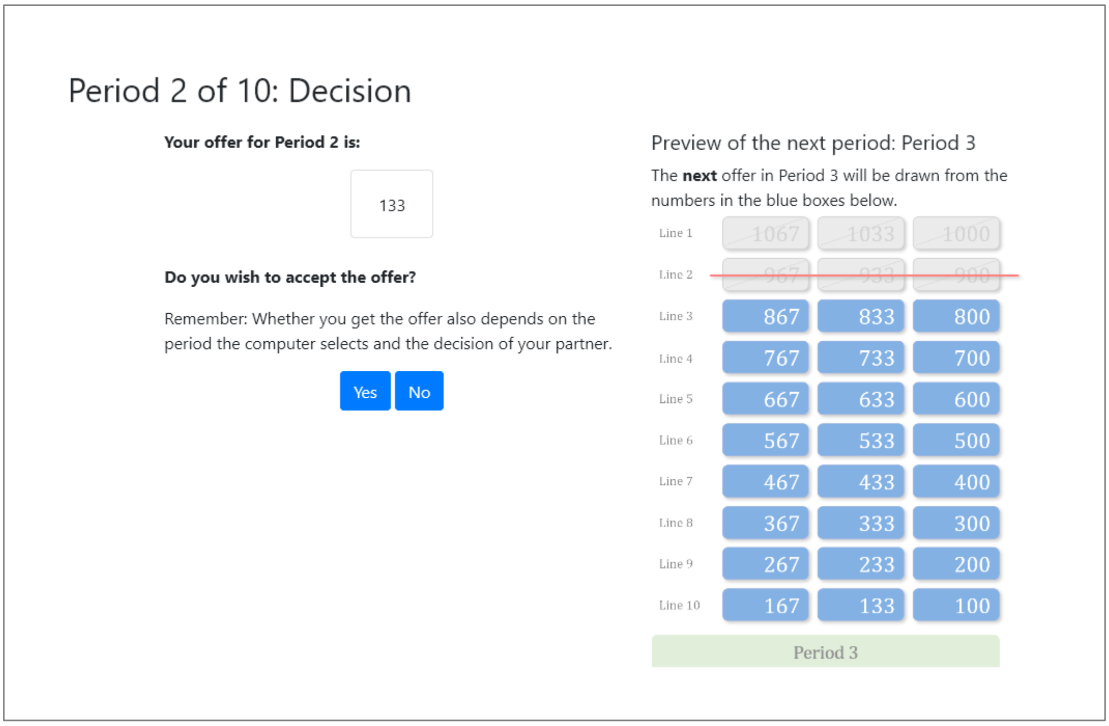
All your answers to the following questions will be relevant for your final payoff.

Start the Experiment!


Period 1 of 10: Preview
Your offer for Period 1 will be drawn from the box below. But only the numbers in blue are available.
Line 1 1067 1033 1000
Line 2 967 933 900
Line 3 867 833 800
Line 4 767 733 700
Line 5 667 633 600
Line 6 567 533 500
Line 7 467 433 400
Line 8 367 333 300
Line 9 267 233 200
Line 10 167 133 100
Period 1
Get your offer


Period 1 of 10: Decision
Your offer for Period 1 is:
933
Do you wish to accept the offer?
Remember: Whether you get the offer also depends on the period the computer selects.
Yes
No
Preview of the next period: Period 2
The next offer in Period 2 will be drawn from the numbers in the blue boxes below.
Line 1 1067 1033 1000
Line 2 967 933 900
Line 3 867 833 800
Line 4 767 733 700
Line 5 667 633 600
Line 6 567 533 500
Line 7 467 433 400
Line 8 367 333 300
Line 9 267 233 200
Line 10 167 133 100
Period 2


Period 2 of 10: Preview
Your offer for Period 2 will be drawn from the box below. But only the numbers in blue are available.
Line 1 1067 1033 1000
Line 2 967 933 900
Line 3 867 833 800
Line 4 767 733 700
Line 5 667 633 600
Line 6 567 533 500
Line 7 467 433 400
Line 8 367 333 300
Line 9 267 233 200
Line 10 167 133 100
Period 2
Get your offer


Period 2 of 10: Decision
Your offer for Period 2 is:
133
Do you wish to accept the offer?
Remember: Whether you get the offer also depends on the period the computer selects and the decision of your partner.
Yes
No
Preview of the next period: Period 3
The next offer in Period 3 will be drawn from the numbers in the blue boxes below.
Line 1 1067 1033 1000
Line 2 967 933 900
Line 3 867 833 800
Line 4 767 733 700
Line 5 667 633 600
Line 6 567 533 500
Line 7 467 433 400
Line 8 367 333 300
Line 9 267 233 200
Line 10 167 133 100
Period 3

Placeholder

Round 3 to Round 10

The above scheme is repeated.

Questionnaire

Please answer the following questions.

Please indicate your gender.

What is your age (in years)?

What is your highest level of education completed?

In your daily life, how often do you find yourself in bargaining situations?

How do you see yourself: Are you generally a person who is fully prepared to take risks or do you try to avoid taking risks?
Please indicate on the scale from 0 ("not at all willing to take risks") to 10 ("very willing to take risks").

Not at all willing ○ 0 ○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7 ○ 8 ○ 9 ○ 10 *Very willing*

How much do you agree with the following statements?
Please indicate your answer on a scale from 0 ("Disagree") to 10 ("Agree").

When someone does me a favor I am willing to return it.

Disagree ○ 0 ○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7 ○ 8 ○ 9 ○ 10 *Agree*

The number 400 is in some way more prominent compared to 433 or 467.

Disagree ○ 0 ○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7 ○ 8 ○ 9 ○ 10 *Agree*

Here are a number of personality traits that may or may not apply to you.
Please indicate for each statement the extent to which you agree or disagree with that statement.
You should rate the extent to which the pair of traits applies to you, even if one characteristic applies more strongly than the other.

Extraverted, enthusiastic.	---------
Critical, quarrelsome.	---------
Dependable, self-disciplined.	---------
Anxious, easily upset.	---------
Open to new experiences, complex.	---------
Reserved, quiet.	---------
Sympathetic, warm.	---------
Disorganized, careless.	---------
Calm, emotionally stable.	---------
Conventional, uncreative.	---------

What do you think was the purpose of this experiment? (Optional) :

Submit

Thank you for participating!

This concludes the experiment. Please scroll down for the completion code.

Period	Offer	Your decision: Accept the offer?
1	933	Yes
2	133	No
3	300	No
4	167	Yes
5	667	Yes
6	567	Yes
7	300	Yes
8	167	Yes
9	200	Yes
10	167	Yes

The computer selected:

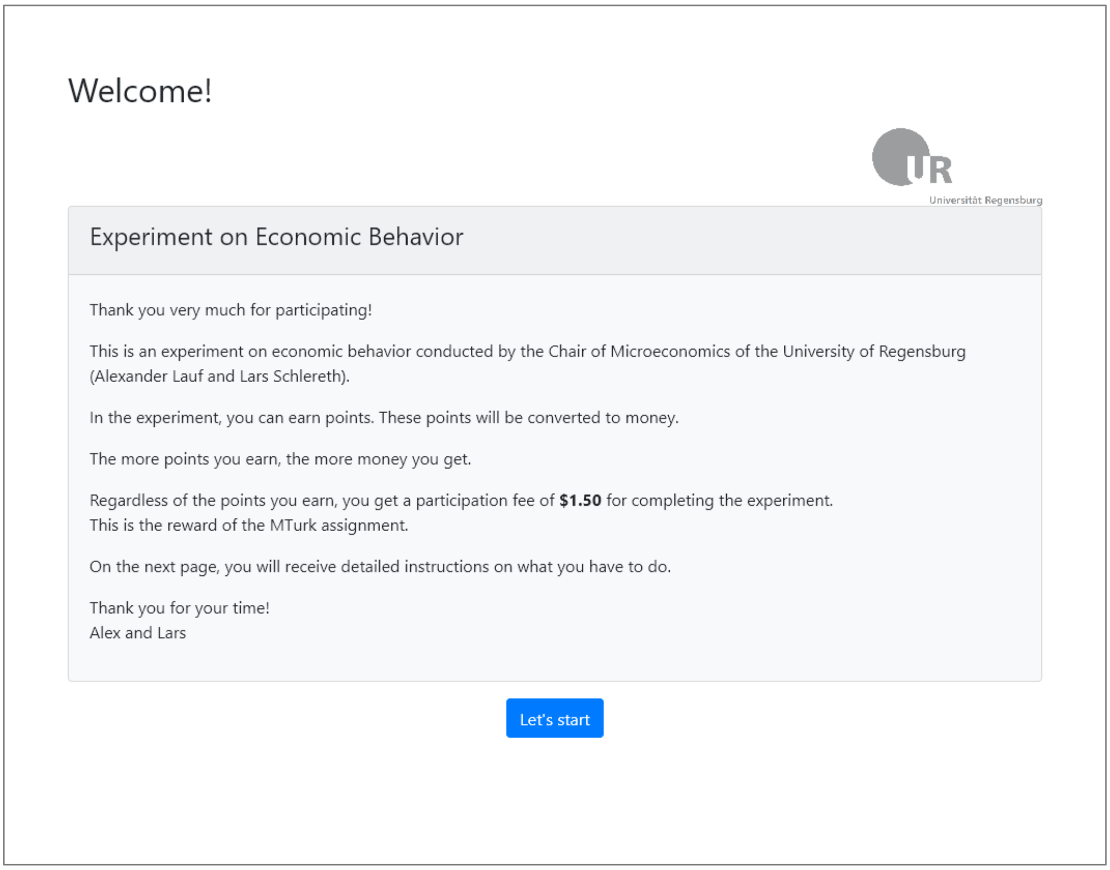
Period 8

The computer will determine your points by going from this period to Period 10 and check, which offer you accepted. Then the points will be converted to dollars and sent to you as bonus within the next days.

Your completion code:

03BF65

B.3.3 Experimental Design: Partner



Welcome!

Universität Regensburg

Experiment on Economic Behavior

Thank you very much for participating!

This is an experiment on economic behavior conducted by the Chair of Microeconomics of the University of Regensburg (Alexander Lauf and Lars Schlereth).

In the experiment, you can earn points. These points will be converted to money.

The more points you earn, the more money you get.

Regardless of the points you earn, you get a participation fee of **$1.50** for completing the experiment.
This is the reward of the MTurk assignment.

On the next page, you will receive detailed instructions on what you have to do.

Thank you for your time!
Alex and Lars

Let's start

Instructions

What do you have to do?

In this experiment, you will make one decision in each of **10 periods**, i.e., a total of 10 decisions. In this experiment, you can earn points. After this experiment, the points will be converted into real money **(1000 Points = 1 Dollar)** and paid to you as a bonus later.

In the experiment, you will be matched with one **randomly selected player**, i.e., your 'partner'. The points you earn in the experiment depend on your own decisions and the decisions of your partner in a way explained below. You and your partner receive the **same instructions**.

In each period, you will receive an **offer**. Your partner will receive the same offer. The computer **randomly selects** one of the numbers in a blue box below. Each number is **equally likely** to be drawn. You can see the box for Period 1 below.

You can **accept** or **reject** the offer.

Line 1	1067	1033	1000
Line 2	967	933	900
Line 3	867	833	800
Line 4	767	733	700
Line 5	667	633	600
Line 6	567	533	500
Line 7	467	433	400
Line 8	367	333	300
Line 9	267	233	200
Line 10	167	133	100

Period 1

In each next period, **numbers are removed** from period to period. In Period 2, 'Line 1' is removed. In Period 3, additionally, 'Line 2' is removed. This is repeated until period 10, in which only the last line remains.

In each period, you decide to either **accept** or **reject** the offer. In subsequent periods, you can get a higher or lower offer, but keep in mind that the **three highest numbers** are removed in each period.

You can click on the right-arrow in the slider gallery below to see how these lines are removed.

Line 1	~~1067~~	~~1033~~	~~1000~~
Line 2	967	933	900
Line 3	867	833	800
Line 4	767	733	700
Line 5	667	633	600
Line 6	567	533	500
Line 7	467	433	400
Line 8	367	333	300
Line 9	267	233	200
Line 10	167	133	100

Period 2

In each period, you **privately** decide to either **accept or reject** the offer. Your partner also privately makes such decisions in each period. When you make your decisions, you do not know your partner's decisions and your partner does not know your decisions.

<u>After</u> you and your partner have made decisions for all 10 periods, your bonus in points is determined as follows:

The computer **randomly selects** a period out of Period 1 to 10. Then, the computer moves step-by-step from **this randomly selected period** to Period 10. The computer stops in the period, where for the first time **both of you have accepted** the respective period's offer. This offer is your bonus in points. Your partner gets the same bonus.

Therefore, you should play every period as if the computer selected the current period.

Your total payment in dollars is the sum of participation fee and your bonus in points converted to dollars (1000 Points = 1 Dollar):

$$\text{Payment} = \text{Participation Fee} + \frac{\text{Your Points}}{1000}$$

Now we ask you a few check-up questions. They do not have any impact on your payment. After you have finished the experiment, we will conduct a short survey.

Go to Check-Up Questions

Check-Up Question | Instructions

Check-Up Questions

Question (1)

The following questions have no effect on your amount of points. Please read carefully.

We want to check whether you understood the instructions. If you want to read the instructions again, you can find a tab 'Instructions' in the navigation bar above.

Suppose you are in **Period 1**. You got your offer from the numbers in the blue boxes below. You are about to move to the next period.

Line 1	1067	1033	1000
Line 2	967	933	900
Line 3	867	833	800
Line 4	767	733	700
Line 5	667	633	600
Line 6	567	533	500
Line 7	467	433	400
Line 8	367	333	300
Line 9	267	233	200
Line 10	167	133	100

Period 1

Which line is eliminated when you move to the next round (Period 2)?

- Line 1: 1067, 1033, 1000
- Line 5: 667, 633, 600
- Line 9: 267, 233, 200
- A random Line.

Instructions | Next Question

Check-Up Question | Instructions

Check-Up Question

Question (2)

The following questions have no effect on your amount of points. Please read carefully.

We want to check whether you understood the instructions. If you want to read the instructions again, you can find a tab 'Instructions' in the navigation bar above.

Suppose you have already made **all decisions**. Your partner has made all his or her decisions as well. In the picture below, you can see all of your decisions.

The computer has selected **Period 5**. As mentioned in the instructions, the computer starts in Period 5 and goes step-by-step to Period 10.

From which period do you get the offer as final payment?

- ○ Period 5
- ○ Period 10
- ○ Period 8
- ○ A random Period.

Instructions | Next Question

Check-Up Question | Instructions

Check-Up Question

Question (3)

The following questions have no effect on your amount of points. Please read carefully.

We want to check whether you understood the instructions. If you want to read the instructions again, you can find a tab 'Instructions' in the navigation bar above.

Suppose you have already made **all decisions**. Your partner has made all his or her decisions as well. In the picture below, you can see all of your decisions.

The computer has selected **Period 2**. As mentioned in the instructions, the computer starts in Period 2 and goes step-by-step to Period 10.

		You Decisions	Your Partner Decisions
	Period 1	Accept	Decline
Computer selects Period 2	Period 2	Decline	Accept
	Period 3	Accept	Decline
	Period 4	Decline	Decline
	Period 5	Accept	Accept
	Period 6	Decline	Accept
	Period 7	Accept	Decline
	Period 8	Accept	Accept
	Period 9	Decline	Decline
	Period 10	Accept	Accept

From which period do you get the offer as final payment?

- ○ Period 5
- ○ Period 8
- ○ Period 10
- ○ A random Period.

Instructions | Next Question

Check-Up Question | Instructions

Check-Up Question

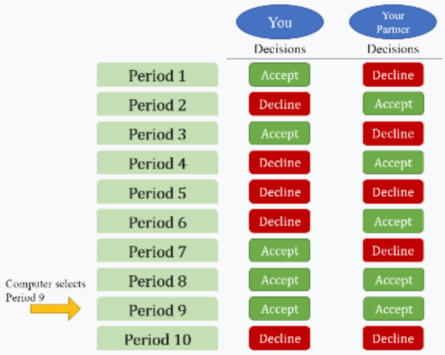
Question (4)

The following questions have no effect on your amount of points. Please read carefully.

We want to check whether you understood the instructions. If you want to read the instructions again, you can find a tab 'Instructions' in the navigation bar above.

Suppose you have already made **all decisions**. Your partner has made all his or her decisions as well. In the picture, below you can see all of your decisions.

The computer has selected **Period 9**. As mentioned in the instructions, the computer starts in Period 9 and goes step-by-step to Period 10.



From which period do you get the offer as final payment?

- ○ Period 2
- ○ Period 9
- ○ Period 6
- ○ A random Period.

Instructions | Continue

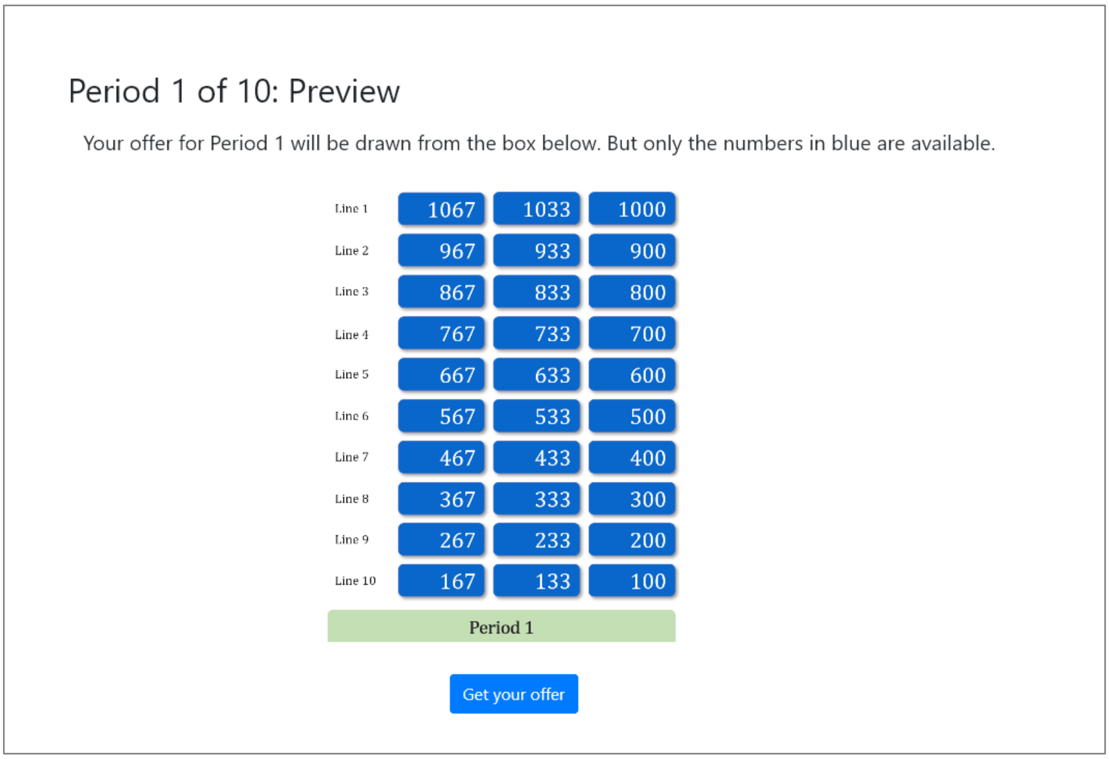
Start of the Experiment

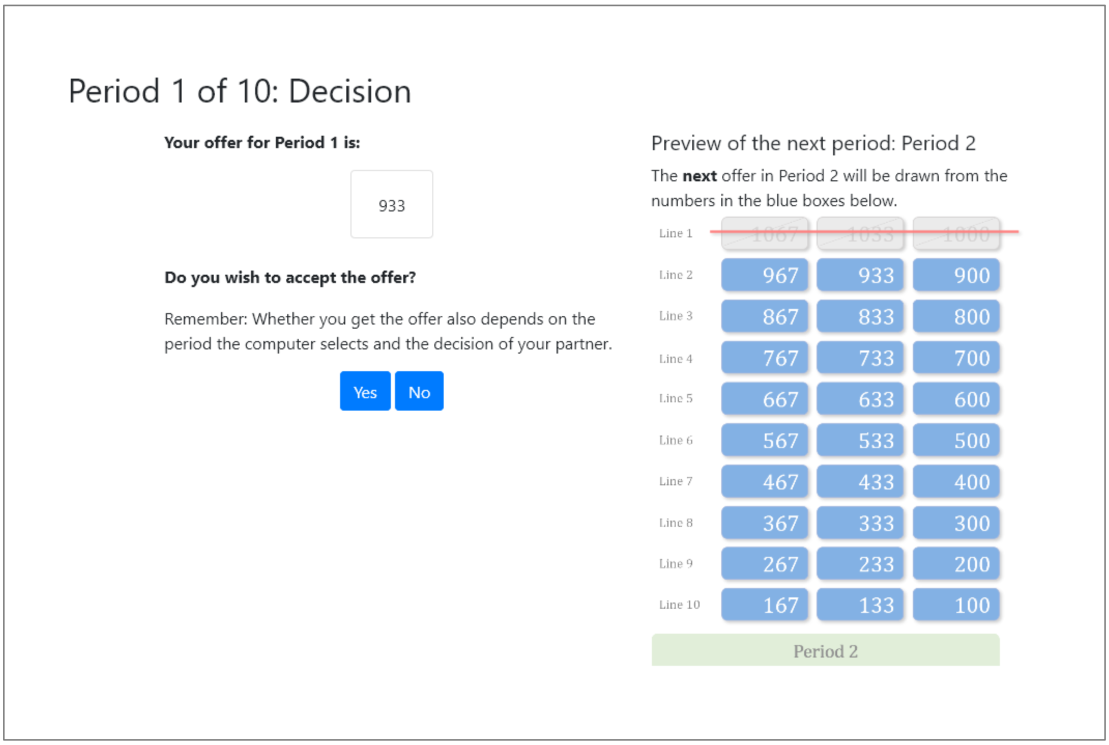
You have finished the check-up questions.

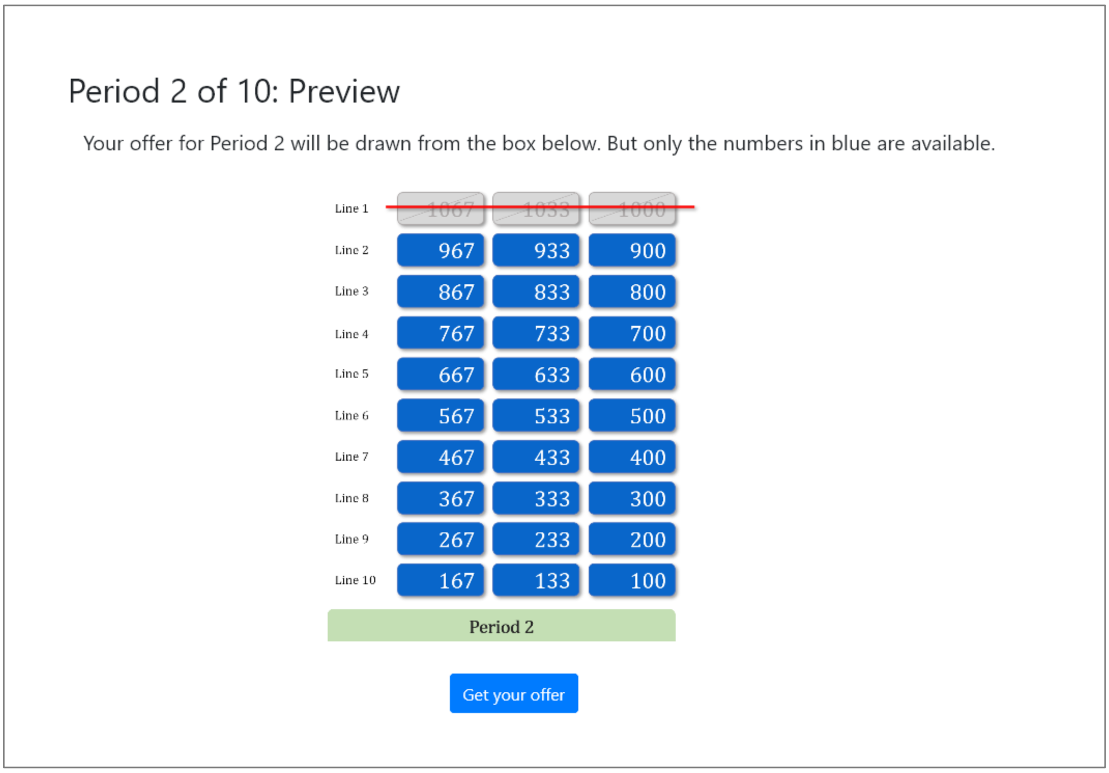
You can now start the experiment by clicking on the button below.

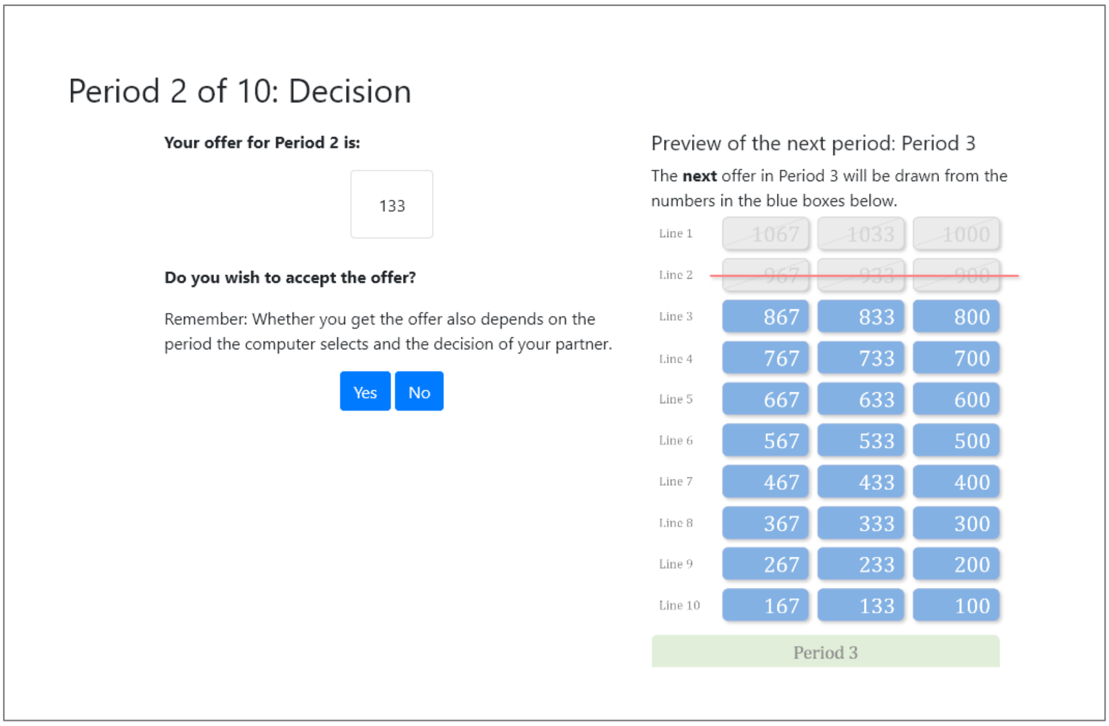
All your answers to the following questions will be relevant for your final payoff.

Start the Experiment!


Period 1 of 10: Preview
Your offer for Period 1 will be drawn from the box below. But only the numbers in blue are available.
Line 1 1067 1033 1000
Line 2 967 933 900
Line 3 867 833 800
Line 4 767 733 700
Line 5 667 633 600
Line 6 567 533 500
Line 7 467 433 400
Line 8 367 333 300
Line 9 267 233 200
Line 10 167 133 100
Period 1
Get your offer


Period 1 of 10: Decision
Your offer for Period 1 is:
933
Do you wish to accept the offer?
Remember: Whether you get the offer also depends on the period the computer selects and the decision of your partner.
Yes
No
Preview of the next period: Period 2
The next offer in Period 2 will be drawn from the numbers in the blue boxes below.
Line 1 1067 1033 1000
Line 2 967 933 900
Line 3 867 833 800
Line 4 767 733 700
Line 5 667 633 600
Line 6 567 533 500
Line 7 467 433 400
Line 8 367 333 300
Line 9 267 233 200
Line 10 167 133 100
Period 2


Period 2 of 10: Preview
Your offer for Period 2 will be drawn from the box below. But only the numbers in blue are available.
Line 1 1067 1033 1000
Line 2 967 933 900
Line 3 867 833 800
Line 4 767 733 700
Line 5 667 633 600
Line 6 567 533 500
Line 7 467 433 400
Line 8 367 333 300
Line 9 267 233 200
Line 10 167 133 100
Period 2
Get your offer


Period 2 of 10: Decision
Your offer for Period 2 is:
133
Do you wish to accept the offer?
Remember: Whether you get the offer also depends on the period the computer selects and the decision of your partner.
Yes
No
Preview of the next period: Period 3
The next offer in Period 3 will be drawn from the numbers in the blue boxes below.
Line 1 1067 1033 1000
Line 2 967 933 900
Line 3 867 833 800
Line 4 767 733 700
Line 5 667 633 600
Line 6 567 533 500
Line 7 467 433 400
Line 8 367 333 300
Line 9 267 233 200
Line 10 167 133 100
Period 3

Placeholder

Round 3 to Round 10

The above scheme is repeated.

Questionnaire

Please answer the following questions.

Please indicate your gender.

What is your age (in years)?

What is your highest level of education completed?

In your daily life, how often do you find yourself in bargaining situations?

How do you see yourself: Are you generally a person who is fully prepared to take risks or do you try to avoid taking risks?
Please indicate on the scale from 0 ("not at all willing to take risks") to 10 ("very willing to take risks").

Not at all willing ○ 0 ○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7 ○ 8 ○ 9 ○ 10 *Very willing*

How much do you agree with the following statements?
Please indicate your answer on a scale from 0 ("Disagree") to 10 ("Agree").

When someone does me a favor I am willing to return it.

Disagree ○ 0 ○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7 ○ 8 ○ 9 ○ 10 *Agree*

The number 400 is in some way more prominent compared to 433 or 467.

Disagree ○ 0 ○ 1 ○ 2 ○ 3 ○ 4 ○ 5 ○ 6 ○ 7 ○ 8 ○ 9 ○ 10 *Agree*

Here are a number of personality traits that may or may not apply to you.
Please indicate for each statement the extent to which you agree or disagree with that statement.
You should rate the extent to which the pair of traits applies to you, even if one characteristic applies more strongly than the other.

Extraverted, enthusiastic.	---------
Critical, quarrelsome.	---------
Dependable, self-disciplined.	---------
Anxious, easily upset.	---------
Open to new experiences, complex.	---------
Reserved, quiet.	---------
Sympathetic, warm.	---------
Disorganized, careless.	---------
Calm, emotionally stable.	---------
Conventional, uncreative.	---------

What do you think was the purpose of this experiment? (Optional) :

Submit

Thank you for participating!

This concludes the experiment. Please scroll down for the completion code.

Period	Offer	Your decision: Accept the offer?
1	933	Yes
2	133	Yes
3	300	Yes
4	167	Yes
5	667	Yes
6	567	Yes
7	300	Yes
8	167	Yes
9	200	Yes
10	167	Yes

The computer selected:

Period 8

The computer will determine your points by going from this period to Period 10 and check, which offer you and your partner jointly accepted. Then the points will be converted to dollars and sent to you as bonus within the next days.

Your completion code:

02FBC3

Appendix C

Appendix: Chapter 3

C.1 Additional Results

Table C1. Summary Statistics by Success in Part 1 for *SELF*.

Treatment: *SELF*	Mean/ Median	*Success*	*Failure*	Test Statistic (p-value)
Number of Observations		150	150	
Demographics				
Female	Mean	0.493 (0.502)	0.753 (0.433)	20.501 (0.000)
Male	Mean	0.480 (0.501)	0.227 (0.420)	19.972 (0.000)
Other	Mean	0.027 (0.162)	0.020 (0.140)	0.000 (1.000)
Age	Mean	28.12 (9.043)	36.273 (13.860)	27.510 (0.000)
Has children	Mean	0.213 (0.411)	0.460 (0.500)	20.366 (0.000)
Education	Median	undergraduate degree (ba/bsc/other)	undergraduate degree (ba/bsc/other)	0.310 (0.577)
Political Orientation: Left	Mean	0.653 (0.478)	0.507 (0.502)	6.601 (0.010)
Political Orientation: Right	Mean	0.153 (0.362)	0.273 (0.447)	6.414 (0.011)
Income	Median	10,000 - 29,999	10,000 - 29,999	5.969 (0.309)
Behavioral Preferences				
Altruism	Mean	-0.128 (0.917)	0.044 (0.735)	1.420 (0.233)
Patience	Mean	0.117 (0.994)	0.007 (1.069)	0.663 (0.416)
Environmental Awareness				
SASSY segment	Median	Concerned	Concerned	2.545 (0.111)
Global warming caused by humans	Mean	0.707 (0.457)	0.560 (0.498)	6.924 (0.009)
Actions matter to fight climate change	Mean	0.713 (0.454)	0.833 (0.374)	6.141 (0.013)
Pro-environmental behavior	Mean	7.373 (2.138)	7.327 (2.097)	0.049 (0.825)
Has offset in past	Mean	0.167 (0.374)	0.233 (0.424)	2.076 (0.150)
Carbon offset effective	Mean	0.667 (0.473)	0.687 (0.465)	0.137 (0.712)

Note: The sample is restricted to the *SELF* treatment. Standard deviations in parentheses for variables with means. For categorical variables, we use χ^2-tests; for numerical variables, we use Kruskal-Wallis tests. *SELF*, *LOW*, and *HIGH* denote the treatments. Pro-environmental behavior is measured with respect to its frequency on a scale from 0 (Never) to 10 (Very often).

Table C2. Summary Statistics by Success in Part 1 for *LOW*.

Treatment: *LOW*	Mean/ Median	*Success*	*Failure*	Test Statistic (p-value)
Number of Observations		153	147	
Demographics				
Female	Mean	0.523 (0.501)	0.762 (0.427)	17.568 (0.000)
Male	Mean	0.477 (0.501)	0.231 (0.423)	18.689 (0.000)
Other	Mean	0.000 (0.000)	0.007 (0.082)	0.000 (0.984)
Age	Mean	28.065 (9.052)	32.714 (12.515)	8.518 (0.004)
Has children	Mean	0.203 (0.403)	0.374 (0.486)	10.752 (0.001)
Education	Median	undergraduate degree (ba/bsc/other)	undergraduate degree (ba/bsc/other)	1.921 (0.166)
Political Orientation: Left	Mean	0.654 (0.477)	0.605 (0.490)	0.743 (0.389)
Political Orientation: Right	Mean	0.190 (0.393)	0.163 (0.371)	0.355 (0.551)
Income	Median	10,000 - 29,999	10,000 - 29,999	10.933 (0.053)
Behavioral Preferences				
Altruism	Mean	-0.036 (0.869)	0.100 (0.809)	1.236 (0.266)
Patience	Mean	-0.128 (1.009)	0.080 (0.901)	2.736 (0.098)
Environmental Awareness				
SASSY segment	Median	Concerned	Concerned	0.001 (0.982)
Global warming caused by humans	Mean	0.699 (0.460)	0.626 (0.486)	1.807 (0.179)
Actions matter to fight climate change	Mean	0.752 (0.433)	0.857 (0.351)	5.265 (0.022)
Pro-environmental behavior	Mean	7.562 (1.747)	7.483 (2.005)	0.065 (0.798)
Has offset in past	Mean	0.203 (0.403)	0.197 (0.399)	0.013 (0.908)
Carbon offset effective	Mean	0.706 (0.457)	0.660 (0.475)	0.731 (0.392)

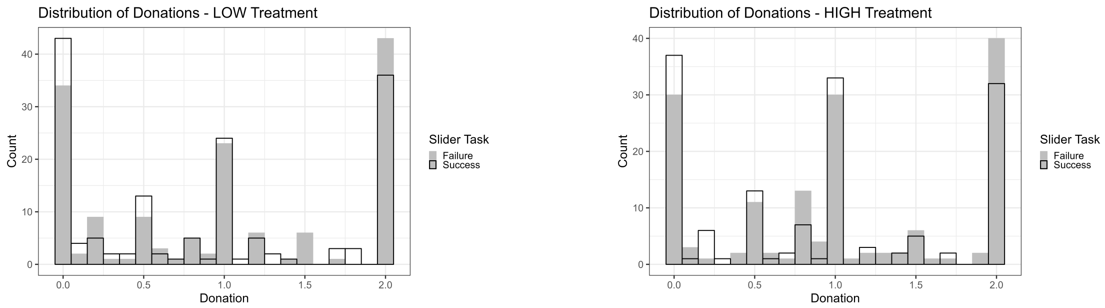
Note: The sample is restricted to the *LOW* treatment. Standard deviations in parentheses for variables with means. For categorical variables, we use χ^2-tests; for numerical variables, we use Kruskal-Wallis tests. *SELF*, *LOW*, and *HIGH* denote the treatments. Pro-environmental behavior is measured with respect to its frequency on a scale from 0 (Never) to 10 (Very often).

Table C3. Summary Statistics by Success in Part 1 for *HIGH*.

Treatment: *HIGH*	Mean/ Median	*Success*	*Failure*	Test Statistic (p-value)
Number of Observations		146	154	
Demographics				
Female	Mean	0.521 (0.501)	0.701 (0.459)	9.577 (0.002)
Male	Mean	0.473 (0.501)	0.286 (0.453)	10.367 (0.001)
Other	Mean	0.007 (0.083)	0.013 (0.114)	0.000 (1.000)
Age	Mean	28.397 (8.512)	36.097 (14.998)	16.488 (0.000)
Has children	Mean	0.130 (0.338)	0.435 (0.497)	33.965 (0.000)
Education	Median	undergraduate degree (ba/bsc/other)	technical/community college	9.028 (0.003)
Political Orientation: Left	Mean	0.582 (0.495)	0.513 (0.501)	1.444 (0.230)
Political Orientation: Right	Mean	0.247 (0.433)	0.240 (0.429)	0.016 (0.899)
Income	Median	10,000 - 29,999	10,000 - 29,999	4.242 (0.515)
Behavioral Preferences				
Altruism	Mean	-0.039 (0.787)	0.059 (0.810)	1.754 (0.185)
Patience	Mean	-0.085 (1.032)	0.011 (0.980)	0.313 (0.576)
Environmental Awareness				
SASSY segment	Median	Concerned	Concerned	0.523 (0.470)
Global warming caused by humans	Mean	0.685 (0.466)	0.675 (0.470)	0.032 (0.859)
Actions matter to fight climate change	Mean	0.664 (0.474)	0.727 (0.447)	1.398 (0.237)
Pro-environmental behavior	Mean	7.171 (2.320)	7.558 (2.074)	1.832 (0.176)
Has offset in past	Mean	0.253 (0.436)	0.182 (0.387)	2.257 (0.133)
Carbon offset effective	Mean	0.712 (0.454)	0.597 (0.492)	4.356 (0.037)

Note: The sample is restricted to the *HIGH* treatment. Standard deviations in parentheses for variables with means. For categorical variables, we use χ^2-tests; for numerical variables, we use Kruskal-Wallis tests. *SELF*, *LOW*, and *HIGH* denote the treatments. Pro-environmental behavior is measured with respect to its frequency on a scale from 0 (Never) to 10 (Very often).

Figure C1. Distribution of Donations separately for *LOW* and *HIGH*.



Note: This figure displays the distribution of the donations in Part 2, split by success in Part 1, and separately for the *LOW* and *HIGH* treatments.

Table C4. OLS Regressions to Test for Moral Balancing Between Treatments.

	Dependent Variable: Donation			
	Failure		*Success*	
	(1)	(2)	(3)	(4)
LOW/HIGH	-0.018	-0.004	-0.048	-0.046
	(0.073)	(0.072)	(0.074)	(0.073)
Intercept	1.022***	0.741***	0.933***	0.682***
	(0.058)	(0.088)	(0.059)	(0.082)
Additional Control A	No	Yes	No	Yes
Observations	451	451	449	449
R^2	0.0001	0.032	0.001	0.047

Note: This table presents the results of OLS regressions for donation in Part 2. The sample is split by *Failure* (Columns (1) and (2)) and *Success* (Columns (3) and (4)) in the slider task of Part 1. *LOW/HIGH* is a dummy, taking the value 1 if a participant is in the *LOW* or *HIGH* treatment, and 0 a participant is in the *SELF* treatment. In Columns (2) and (4), we control for whether participants agree that their actions matter to fight climate change, as for this variable, we find significant differences between treatments (see Table 4.1). Robust standard errors in parentheses. *p<0.1; **p<0.05; ***p<0.01.

C.2 Instructions

The following pages contain screenshots of the online experiment conducted via Prolific. The study showed each participant partly different slides, depending on the assigned treatment condition and the participant's performance in the real-effort task. Participants were randomly allocated to one of three treatment conditions. Headlines stating the specific treatment as well as failure or success in the real-effort task mark the varying screens. All other pages were identical.

Welcome!

Thank you for participating in this study!

Please, read the instructions carefully. The estimated time to complete the study is **10 minutes**.

For completing the study, you will receive a fixed amount of **£1.20**. You can **earn additional money** depending on your decisions in this study. All payments will be conducted via Prolific.

Please note that all decisions you make and all data collected with the questionnaire are **anonymous** and used only for scientific purposes. We do not get any personally identifiable information from Prolific.

This study does not use deception, meaning we will not lie to you or mislead you.

Your decisions have **real financial consequences** that will be implemented as described in the instructions.

If you have any questions during or after taking part in this study or want to delete your information, you can contact us via e-mail: vanessa.schoeller@ur.de or write a message on Prolific.

Thank you again for your time and participation,

Lars Schlereth, MSc and Vanessa Schoeller, MSc
Chair of Microeconomics, University of Regensburg

Start the study

Data protection and consent

Information on participation:

Participation in the study is voluntary. You are free to terminate your participation in the study at any time and without giving reasons. Please understand that we cannot pay compensation in this case.

Information on data protection:

Your data will be treated confidentially. Demographic data and personal opinions that you provide in the questionnaire do not allow to identify you. At no point throughout this study will we ask you to provide your name or other identifying information.

All data collected in this study will be processed in accordance with article 13 EU GDPR (General Data Protection Regulation of the European Union), collected exclusively for scientific research purposes. Your data is collected anonymously and only associated with your anonymous Prolific ID. It is therefore not possible to associate the data with personal information. Access to your data and, if required, deletion of your data is therefore only possible during or directly after your participation. If you wish to delete your data, please contact us through the messenger of Prolific before we can verify and pay your participation compensation (within a max. 24 hours).

The fully anonymised data of this study will be stored for at least ten years. Furthermore, we may make the anonymous data available for further analysis and replication in publicly accessible data archives.

☐ I hereby confirm that I am over 18 years old, have read and understood the given information on participation, and voluntarily participate in the following study. I agree that my data will be stored in anonymised form and published for scientific purposes. I am aware that I can only request the deletion of my data during or within the following 24 hours.

Next

General Instructions 1/2

Your progress in the study: 5%

In this study, you can engage in helping to mitigate climate change and earn money.

Climate change is seen by the vast majority of experts as one of the greatest challenges of our time.

Through your actions in this study, you can support the fight against climate change. Depending on your actions throughout the study, we offset CO_2 by donating to the non-profit organisation *Atmosfair*.

Atmosfair actively contributes to CO_2 mitigation by promoting, developing, and financing renewable energies in over 15 countries worldwide. They build and maintain, for example, solar energy, hydropower, biogas, and wind energy. Currently, 90% of *Atmosfair's* carbon offset projects adhere to the Clean Development Mechanism Gold Standard, the strictest standard available for climate protection projects.

If you want to learn more about *Atmosfair*, you can access their website https://www.atmosfair.de/en/.

All the CO_2 offsets you purchase are real.

You can verify the donation:

A couple of days after your participation, we will send you a link via Prolific private messages. The link will include a donation certificate stating how many CO_2 certificates were purchased in total due to this study.

Next

General Instructions 2/2

Your progress in the study: 10%

This study consists of 3 parts.

First, you will go through Part 1 of the study; after completion, you will continue with Part 2.

Your decisions in Part 1 will not affect Part 2, nor vice versa.

At the end of the study, the computer will **randomly** select either Part 1 or Part 2 for payment, with equal probability. All payments and CO_2 offsets of the selected part will be conducted.

Part 3 consists of a short questionnaire.

Once you have pressed the 'Next' button at the end of each page, you cannot change the selection you have made. You cannot go back one page and should not try to reload the page.

Next

Treatment: 0.20 pound

Instructions on Part 1

Your progress in the study: 15%

Please read the instructions carefully, as you will have to answer questions that check your understanding to proceed.

In Part 1 of the study, you can solve sliders.

You will be given a total of 50 sliders. If you **solve at least 26 sliders within 2 minutes, you receive an additional payment of £0.20**.

After the task, you will be informed on whether you succeeded in the task or failed.

Solving sliders is voluntary. It does not affect what will happen in Part 2 of the study.

Each slider has a number above, indicating its current position.

A slider counts as solved if you have set it precisely to the value indicated above the slider (in the practice slider below: 50). You can change the position of the slider with your computer mouse as many times as you like.

You will be presented with five sliders simultaneously. After finishing all sliders on a page, click on the 'Next'-button to continue with the following five sliders. There will be 50 sliders presented on ten pages.

Please set the slider to 50 to continue.

On the next page, you can familiarize yourself with the task by performing a **30-second practice round**.

Your performance in this practice round will not affect your payoff.

When you are ready, click the 'Start the practice round' button below.

Start the practice round

Treatment: 10 kg CO2 offset

Instructions on Part 1

Your progress in the study: 15%

Please read the instructions carefully, as you will have to answer questions that check your understanding to proceed.

In Part 1 of the study, you can engage in helping to mitigate climate change.

You will be given a total of 50 sliders. If you **solve at least 26 sliders within 2 minutes, you offset 10 kg of CO_2** by buying CO_2-remission certificates from *Atmosfair*.

After the task, you will be informed on whether you succeeded or failed in the task. If you succeed, 10 kg of CO_2 will be mitigated on your behalf.

10 kg of CO_2 is equivalent to driving about 47.7 miles with an average new car.

Solving sliders is voluntary. It does not affect what will happen in Part 2 of the study.

Each slider has a number above, indicating its current position.

A slider counts as solved if you have set it precisely to the value indicated above the slider (in the practice slider below: 50). You can change the position of the slider with your computer mouse as many times as you like.

You will be presented with five sliders simultaneously. After finishing all sliders on a page, click on the 'Next'-button to continue with the following five sliders. There will be 50 sliders presented on ten pages.

Please set the slider to 50 to continue.

You have selected 50. This value is correct!

On the next page, you can familiarize yourself with the task by performing a **30-second practice round**.

Your performance in this practice round will not affect your payoff or the offset.

When you are ready, click the 'Start the practice round' button below.

Start the practice round

Treatment: 100 kg CO2 offset

Instructions on Part 1

Your progress in the study: 15%

Please read the instructions carefully, as you will have to answer questions that check your understanding to proceed.

In Part 1 of the study, you can engage in helping to mitigate climate change.

You will be given a total of 50 sliders. If you **solve at least 26 sliders within 2 minutes, you offset 100 kg of CO_2** by buying CO_2-remission certificates from *Atmosfair*.

After the task, you will be informed on whether you succeeded or failed in the task. If you succeed, 100 kg of CO_2 will be mitigated on your behalf.

100 kg of CO_2 is equivalent to driving about 476.6 miles with an average new car.

Solving sliders is voluntary. It does not affect what will happen in Part 2 of the study.

Each slider has a number above, indicating its current position.

A slider counts as solved if you have set it precisely to the value indicated above the slider (in the practice slider below: 50). You can change the position of the slider with your computer mouse as many times as you like.

You will be presented with five sliders simultaneously. After finishing all sliders on a page, click on the 'Next'-button to continue with the following five sliders. There will be 50 sliders presented on ten pages.

Please set the slider to 50 to continue.

You have selected 25. This value is too low!

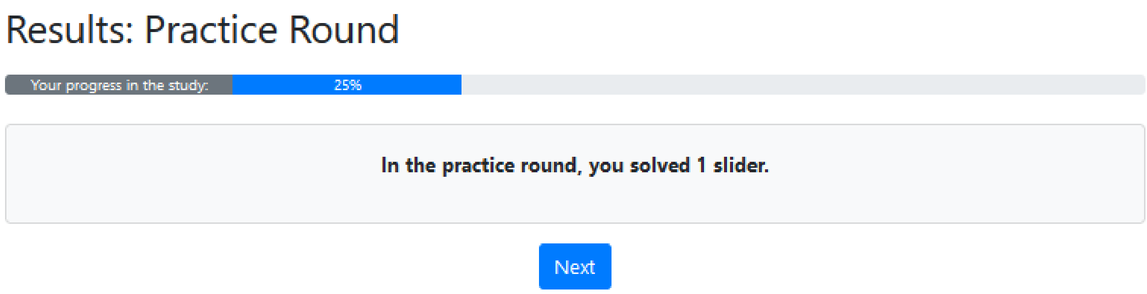
On the next page, you can familiarize yourself with the task by performing a **30-second practice round**.

Your performance in this practice round will not affect your payoff or the offset.

When you are ready, click the 'Start the practice round' button below.

Start the practice round


Practice round - 30 seconds
Time left to practice: 0:07
Your progress in the study:
20%
On this page, you have 30 seconds to get to know the task and practice.
Set the slider to 22
You have selected 22!
Set the slider to 19
You have selected 56!
Set the slider to 6
Set the slider to 4
Set the slider to 37
You can continue after 30 seconds.
Continue


Results: Practice Round
Your progress in the study:
25%
In the practice round, you solved 1 slider.
Next

<u>Treatment: 0.20 pound</u>

Please answer the following questions:

Your progress in the study: 30%

Before proceeding to the next page, we need you to answer a few questions. **You will only be able to progress if you answer all questions correctly.**

1. Does your own payoff depend on how many sliders you solve?

◉ Yes

○ No

2. If you solve at least 26 sliders and Part 1 is randomly selected, you will receive an additional payment of £0.2.

◉ True

○ False

3. All carbon offsets in this study are real because this study does not use deception. Type **yes** if this statement is true, **no** if it is false.

no

You answered the question above incorrectly. Please try again.

You did not answer all questions correctly.

You need to answer all questions correctly before you can proceed.

Next

Treatment: 10 kg offset

Please answer the following questions:

Your progress in the study: 30%

Before proceeding to the next page, we need you to answer a few questions. **You will only be able to progress if you answer all questions correctly.**

1. Does your own payoff depend on how many sliders you solve?

○ Yes

○ No

2. If you solve at least 26 sliders and Part 1 is randomly selected, you will offset 10 kg of CO_2.

○ True

○ False

3. All carbon offsets in this study are real because this study does not use deception. Type **yes** if this statement is true, **no** if it is false.

Next

Treatment: 100kg CO2 offset:

Please answer the following questions:

Your progress in the study: 30%

Before proceeding to the next page, we need you to answer a few questions. **You will only be able to progress if you answer all questions correctly.**

1. Does your own payoff depend on how many sliders you solve?

- ○ Yes
- ○ No

---

2. If you solve at least 26 sliders and Part 1 is randomly selected, you will offset 100 kg of CO_2.

- ○ True
- ○ False

---

3. All carbon offsets in this study are real because this study does not use deception. Type **yes** if this statement is true, **no** if it is false.

Next

Treatment: 0.20 pound

Part 1: slider task

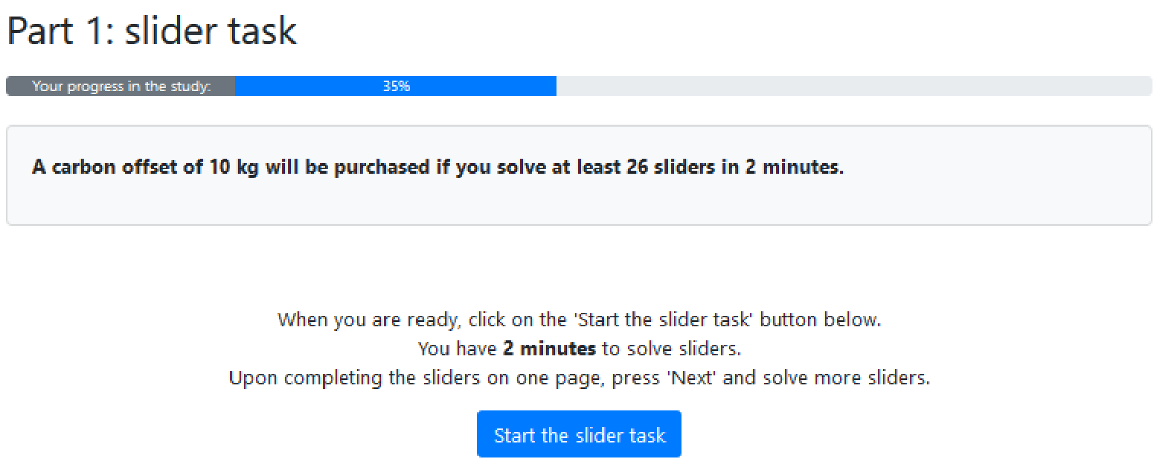
Your progress in the study: 35%

You will earn £0.20, if you solve at least 26 sliders in 2 minutes.

When you are ready, click on the 'Start the slider task' button below.
You have **2 minutes** to solve sliders.
Upon completing the sliders on one page, press 'Next' and solve more sliders.

Start the slider task

<u>Treatment: 10 kg CO2 offset</u>



Treatment: 100 kg CO2 offset

Part 1: slider task

Your progress in the study: 35%

A carbon offset of 100 kg will be purchased if you solve at least 26 sliders in 2 minutes.

When you are ready, click on the 'Start the slider task' button below.
You have **2 minutes** to solve sliders.
Upon completing the sliders on one page, press 'Next' and solve more sliders.

Start the slider task

Treatment: 0.2 pound

Slider task

Time left to complete the task: **1:55**

Solve at least 26 sliders to earn £0.20.

Set the slider to 74

Set the slider to 87

Set the slider to 83

Set the slider to 24

Set the slider to 2

Next

Treatment: 10 kg CO2 offset

Slider task

Time left to complete the task: **1:56**

Solve at least 26 sliders to offset 10 kg of CO_2, which is equivalent to driving about 47.7 miles with an average new car.

Set the slider to 74

Set the slider to 87

Set the slider to 83

Set the slider to 24

Set the slider to 2

Next

Treatment: 100 kg CO2 offset


Slider task

Time left to complete the task: **1:56**

Solve at least 26 sliders to offset 100 kg of CO_2, which is equivalent to driving about 476.6 miles with an average new car.

Set the slider to 74

Set the slider to 87

Set the slider to 83

Set the slider to 24

Set the slider to 2

Next

Treatment: 0.2 pounds, Sucess

Your performance in the slider task

Your progress in the study: 40%

Congratulations! You solved the required number of sliders in time!

You solved 38 sliders in total. At least 26 sliders were needed to complete the task successfully.

Thereby, you earn £0.20 in Part 1.

Continue with Part 2

Treatment: 0.2 pounds, Failure

Your performance in the slider task

Your progress in the study: 40%

Unfortunately, you did not solve the required number of sliders in time!

You solved 0 sliders in total. At least 26 sliders would have been needed to complete the task successfully.

Thereby, you do not earn £0.20 in Part 1.

Continue with Part 2

Treatment: 10kg CO2 offset, Success

Your performance in the slider task

Your progress in the study: 40%

Congratulations! You solved the required number of sliders in time!

You solved 26 sliders in total. At least 26 sliders were needed to complete the task successfully.

Thereby, you offset 10 kg of CO_2, which is equivalent to driving 47.7 miles with an average new car.

This means you actively contributed to reducing CO_2 emissions, which are the main driver of climate change.

Continue with Part 2

Treatment: 10kg CO2 offset, Failure

Your performance in the slider task

Your progress in the study: 40%

Unfortunately, you did not solve the required number of sliders in time!

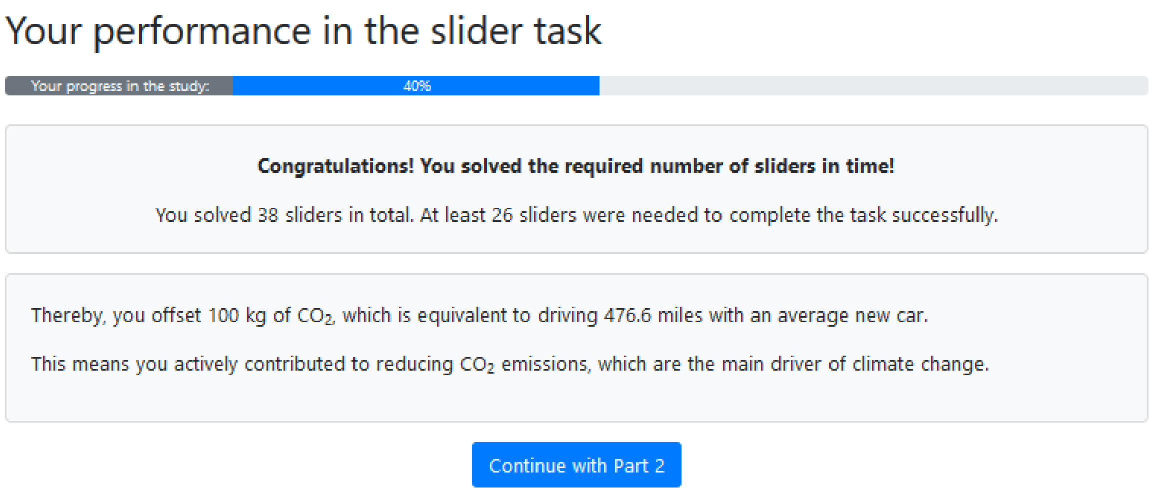
You solved 0 sliders in total. At least 26 sliders would have been needed to complete the task successfully.

Thereby, you do not offset 10 kg of CO_2, which is equivalent to driving 47.7 miles with an average new car.

This means that you did not actively contribute to reducing CO_2 emissions, which are the main driver of climate change.

Continue with Part 2

<u>Treatment: 100kg CO2 offset, Success</u>



Treatment: 100kg CO2 offset, Failure

Your performance in the slider task

Your progress in the study: 40%

Unfortunately, you did not solve the required number of sliders in time!

You solved 0 sliders in total. At least 26 sliders would have been needed to complete the task successfully.

Thereby, you do not offset 100 kg of CO_2, which is equivalent to driving 476.6 miles with an average new car.

This means that you did not actively contribute to reducing CO_2 emissions, which are the main driver of climate change.

Continue with Part 2

Instructions on Part 2

Your progress in the study: 45%

Please read the instructions carefully, as you will have to answer questions to check your understanding so you can proceed.

You completed Part 1 of the study.

In Part 2 of the study, you will be given a budget of £2.00. This budget is independent of the £1.20 you will receive for sure if you complete the study.

You can now decide how to divide the £2.00 between yourself and carbon offset. The money you keep for yourself will be transferred to you after the study via Prolific. To compensate the amount of CO_2 you chose to offset, we will donate the equivalent amount to the non-profit organisation *Atmosfair*.

All CO_2 offsets you purchase are real.

Also, you can verify the donation. A few days after your participation, we will send you a link via Prolific private messages. The link will include a donation certificate stating how many CO_2 certificates were purchased in total due to this study.

Proceed to questions that check your understanding

Please answer the following questions:

Your progress in the study: 50%

Before proceeding to the next page, we need you to answer a few questions. **You can only progress if you answer all questions correctly.**

1. In the following part, you can offset carbon emissions.

○ True

○ False

2. Your decision on how much carbon emissions to offset does not impact your participation fee of £1.2.

○ True

○ False

3. You decide on how much to offset and which amount to keep for yourself. Offsetting is done by the non-profit organization Atmosfair. The amount you keep for yourself will be transferred to you via Prolific.
Type **yes** if this statement is true, **no** if it is false.

Next

Part 2: please make a decision

Your progress in the study: 55%

You have a budget of £2.00.

You can now decide how much CO_2 to offset. The money you keep for yourself will be transferred to you after the study via Prolific.

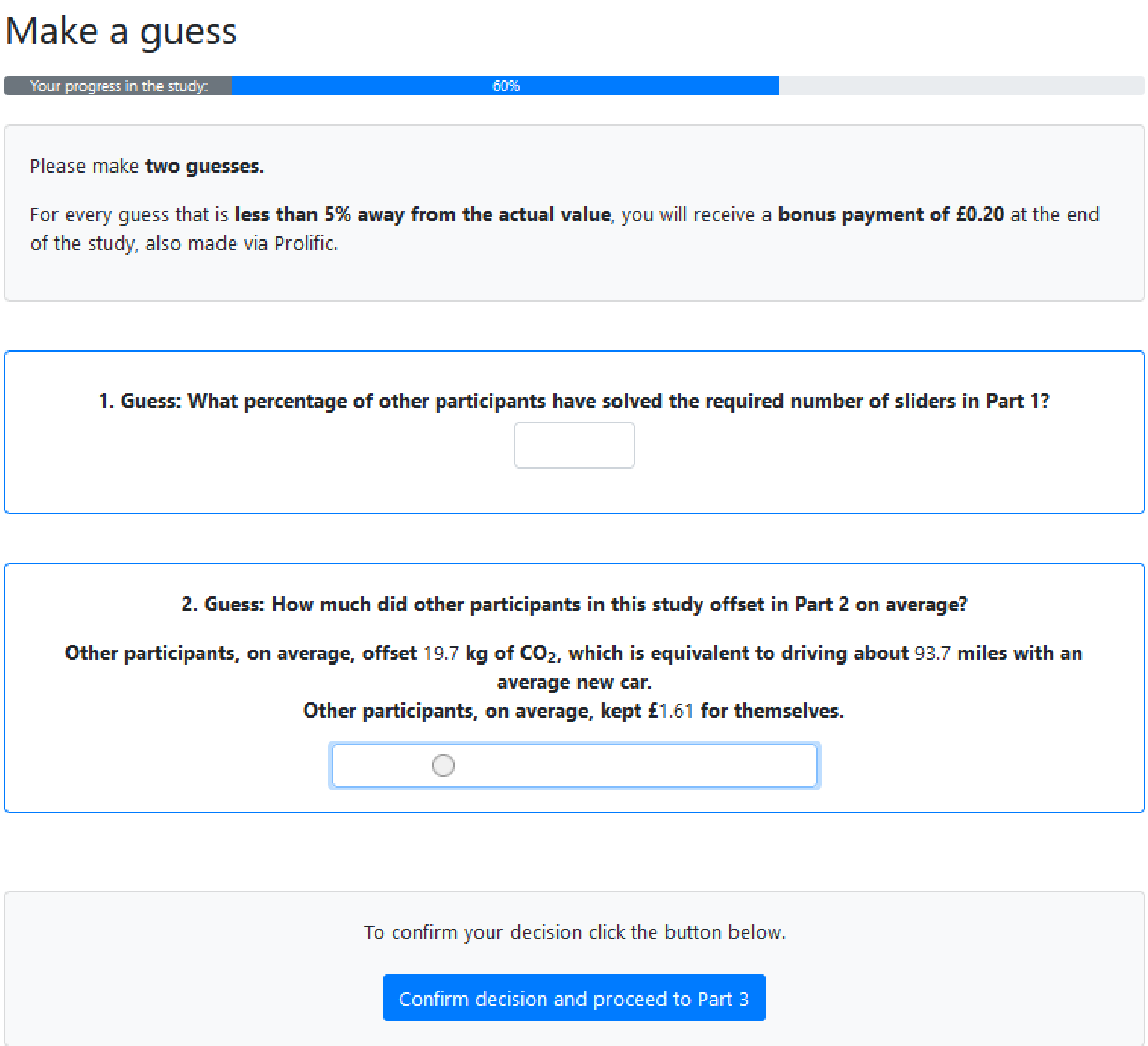
Please make your decision by changing the position of the slider below.

Click on the bar below to reveal the slider.

You have selected to offset 13.1 **kg of CO_2, which is equivalent to driving about** 62.5 **miles with an average new car. You keep £**1.74 **for yourself.**

To confirm your decision click the button below.

Confirm decision and proceed to Part 3



Make a guess

Your progress in the study: 60%

Please make **two guesses.**

For every guess that is **less than 5% away from the actual value**, you will receive a **bonus payment of £0.20** at the end of the study, also made via Prolific.

1. Guess: What percentage of other participants have solved the required number of sliders in Part 1?

2. Guess: How much did other participants in this study offset in Part 2 on average?

Other participants, on average, offset 19.7 **kg of CO_2, which is equivalent to driving about** 93.7 **miles with an average new car.**
Other participants, on average, kept £1.61 **for themselves.**

To confirm your decision click the button below.

Confirm decision and proceed to Part 3

Instructions on Part 3

Your progress in the study: 65%

You completed Part 2 of the study.

You are now in the third and last part of this study. Here, we kindly ask you to answer a short questionnaire.

We can only pay you if you complete the questionnaire.

Proceed with questionnaire

Please answer the following questions

Your progress in the study: 70%

How many kg of CO_2 do you think you offset in both parts of the study combined?

How much effort did you put into solving sliders in Part 1?

- Very high level
- High level
- Medium level
- Low level
- Very low level

What is your gender?

- Male
- Female
- Non-binary
- Rather not say

What is your age?

Do you have any children?

- Yes
- No

Next

Please answer the following questions

Your progress in the study: 75%

Which of the following political parties do you most identify with?

Which of these is the highest level of education you have completed?

What is your personal income per year (after tax) in GBP?

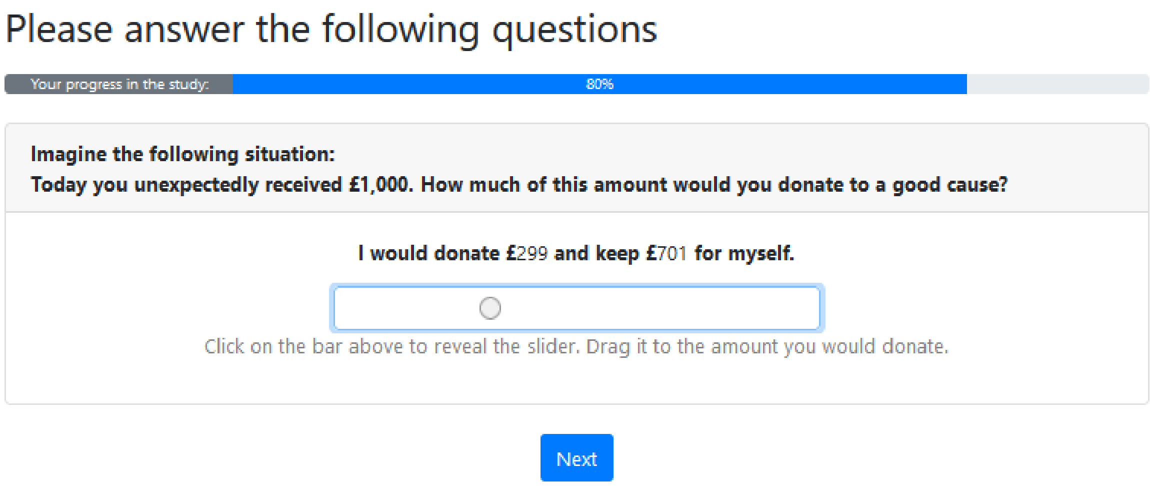
Do you consider yourself religious or spiritual?

- ○ Yes
- ○ No
- ○ Rather not say

How often do you drive a car?

- ○ Very often
- ○ Quite often
- ○ Sometimes
- ○ Never

Next



Please answer the following questions

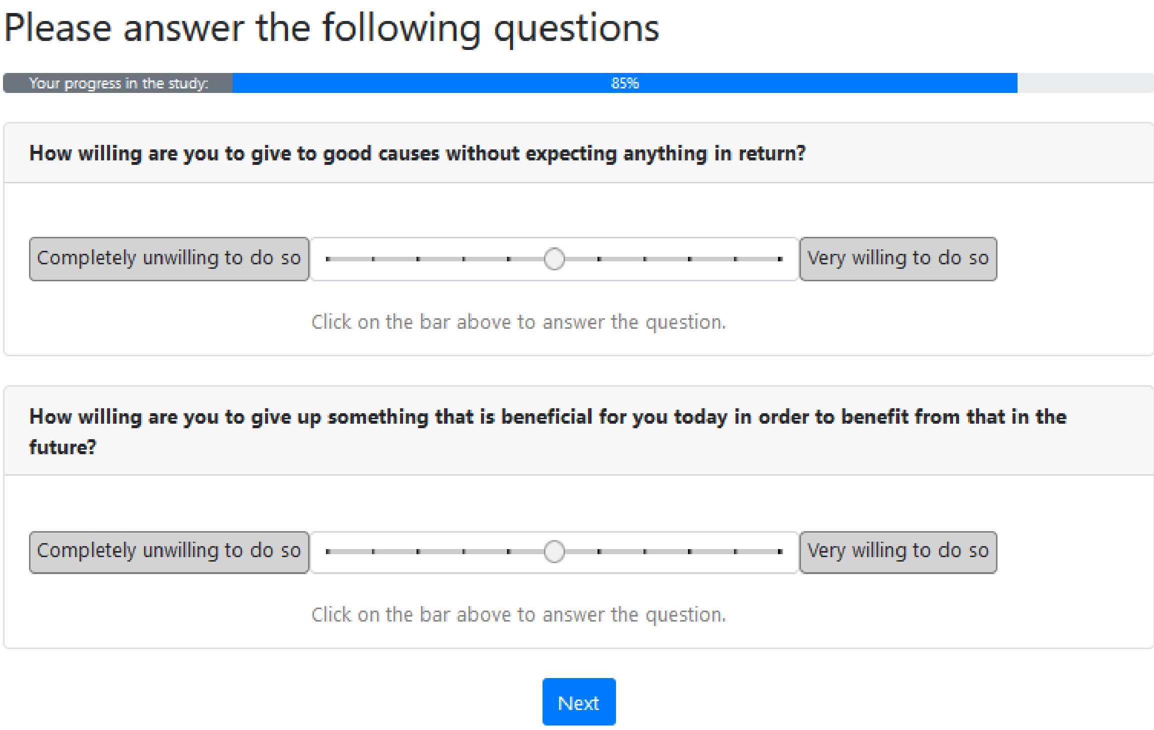
Your progress in the study: 80%

Imagine the following situation:
Today you unexpectedly received £1,000. How much of this amount would you donate to a good cause?

I would donate £299 **and keep £**701 **for myself.**

Click on the bar above to reveal the slider. Drag it to the amount you would donate.

Next



Please answer the following questions

Your progress in the study: 85%

How willing are you to give to good causes without expecting anything in return?

Completely unwilling to do so | Very willing to do so

Click on the bar above to answer the question.

How willing are you to give up something that is beneficial for you today in order to benefit from that in the future?

Completely unwilling to do so | Very willing to do so

Click on the bar above to answer the question.

Next

Please answer the following questions

Your progress in the study: 90%

Assuming global warming is happening, do you think it is...:

- caused mostly by human activities.
- caused by human activities and natural changes.
- caused mostly by natural changes in the environment.
- neither because global warming isn't happening.

How important is the issue of global warming to you personally?

- Extremely important
- Very important
- Somewhat important
- Not too important
- Not at all important

It is important that you pay attention during this study. To prove that you are still attentive, please choose 'Agree' in this question.

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree
- Don't know

How worried are you about global warming?

- Very worried
- Somewhat worried
- Not very worried
- Not at all worried

How much do you think global warming will harm you personally?

- A great deal
- A moderate amount
- Only a little
- Not at all
- Don't know

Next

Please answer the following questions

Your progress in the study: 95%

How much do you think global warming will harm future generations of people?

- ○ A great deal
- ○ A moderate amount
- ○ Only a little
- ○ Not at all
- ○ Don't know

I believe that carbon offset is an effective instrument to fight climate change.

- ○ Strongly agree
- ○ Agree
- ○ Neither agree nor disagree
- ○ Disagree
- ○ Strongly disagree
- ○ Don't know

I think my personal actions matter to fight climate change.

- ○ Strongly agree
- ○ Agree
- ○ Neither agree nor disagree
- ○ Disagree
- ○ Strongly disagree
- ○ Don't know

I take actions that are considered environmentally friendly (e.g., take fewer flights, use public transport, switch off lights in rooms that aren't used, turn down heating at night).

Never — Very often

Click on the bar above to answer the question.

Apart from this study, have you ever donated money to a charity that offsets carbon emissions?

- ○ Yes
- ○ No

Next

Thank you very much for participating!

Your progress in the study: 100%

The computer randomly selected: **Part 1**

In this study, you earned:

- £1.20 for participating
- £0.20 for each guess that was close enough. After all participants completed the study, we will calculate the actual values and pay the bonuses through Prolific.

Therefore, your preliminary **total payment is £1.20.**

Your completion code is:

2920ADE3

Click on the link below to be redirected to Prolific. Clicking is necessary to get rewarded for participating in this study.

https://app.prolific.co/submissions/complete?cc=2920ADE3

For further questions, please write us a message in Prolific or contact us via e-mail: vanessa.schoeller@ur.de.

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