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Can default options lead to credit card default?

An empirical analysis of the effect of altered default options on credit card repayment behavior in Norway

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Abstract

The aim of this thesis is to study the effect on repayment behavior of a regulation imposed on all Norwegian credit card issuing institutions in 2017. The intervention involved changing the invoice default option from the minimum to the full outstanding credit card balance. Using a sample of 25% of Eika Kredittbank's credit card customers, covering all transactions and repayments from May 2015 through December 2019, we document that the Norwegian government's attempt to increase repayment of credit card debt was successful.

We apply a fixed effects estimator at the individual level and find a positive but small average effect of the default option change on repayment ratio. As we believe that the observed small effect could be related to the fact that average repayment in Norway is high, we aimed to examine whether the impact would be larger for customers that initially paid a smaller proportion of their credit card balance. For this purpose, we divided customers into groups based on pre-regulation repayment patterns. The division and subsequent analysis suggest that as a response to the change, near-minimum and medium paying customers show an increase of 19.2 and 7.5 percentage points, respectively. The findings demonstrate the power of the default effect, and suggest that nudging through default options have important implications.

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1 Introduction

The rapid growth of the consumer credit market has become a prominent feature of modern economies across the world. Credit cards allow customers to borrow funds from the issuing bank or financial institution to be used among merchants who accept it as a form of payment (Bloomenthal, 2020). According to Gjeldsregisteret (2020), there are more than 3.2 million Norwegian customers with unsecured debt (credit cards and consumer loans), with a total credit limit of 250.9 billion NOK of which 54 billion is interest bearing. The option of credit spending provides the customer with increased financial flexibility. However, negligent spending can incur substantial costs, as customers who do not pay outstanding debt in time will be subject to high interest rates, often between 15% and 25% annually (Finansportalen, 2020). This particularly impacts customers choosing to only pay the *minimum amount* needed to stay afloat. As credit card default and high interest rates have become an increasing problem in Norway, a key question is how policy makers can prevent this development.

Minimum payments constitute a salient feature of credit cards, which stipulate the lowest payment customers must pay in order to keep their account in good standing in a given month (Keys and Wang, 2019). The minimum amount is calculated as a small percentage of the outstanding debt, often a number between 3% and 5% (Finansportalen, 2020). Hence, for the vast majority of customers, the minimum payment is a lower bound on the optimal repayment due to the arising costs associated with paying less than the minimum. Until recently, credit card invoices in Norway stated the minimum payment as the default option. Consequently, to pay the total outstanding amount, customers actively had to change the monthly payment amount.

In order to limit the growth in credit card debt, the Norwegian government introduced a policy change regarding credit card invoicing. From mid-2017, all credit card issuers were required to issue invoices with the full outstanding amount as the default option, replacing the minimum amount. The regulation aimed to simplify full credit repayment, encouraging customers to repay their total outstanding amount at initial invoicing (Regjeringen, 2016).

The intervention can be viewed as a *nudge* from the Norwegian government, aiming to improve repayment behavior through the change of default option. Previous literature has proven that changes to default options can be effective in altering individuals' decision making (Johnson and Goldstein, 2003). The changed behavior humans exhibit as a cause of choosing the default option, is termed *the default effect*. The default effect is one of the most well-known behavioral biases, which arises because individuals tend to stick with the current or default choices, even when other options are available (van Kleef et al., 2018).

The default effect in the context of credit debt repayment is a field of study lacking research. Our study seeks to examine whether the psychological effects default options entail can lead customers to alter credit card debt repayments. We aim to investigate whether the regulation has had a positive effect on credit card debt repayment, and whether this can be attributed to the default effect. Furthermore, we seek to answer whether the potential impact differs between various customer types. To our knowledge, this empirical study is the first to estimate the causal impact of the default effect on repayment behavior. We believe that our findings will be valuable insight for institutions and governments internationally.

Our thesis is structured as follows: In chapter 2 we provide relevant literature on the topics of interest. In chapter 3 we present background and information about the governmental intervention, as well as our hypotheses. In chapter 4 we detail the data and present descriptive statistics for variables used in our study, before we present the methodology our analysis builds upon in chapter 5. In chapter 6, we report all analysis results. In chapter 7 we interpret and discuss the findings, and comment on limitations to our data and analysis. We finally summarize the results in chapter 8.

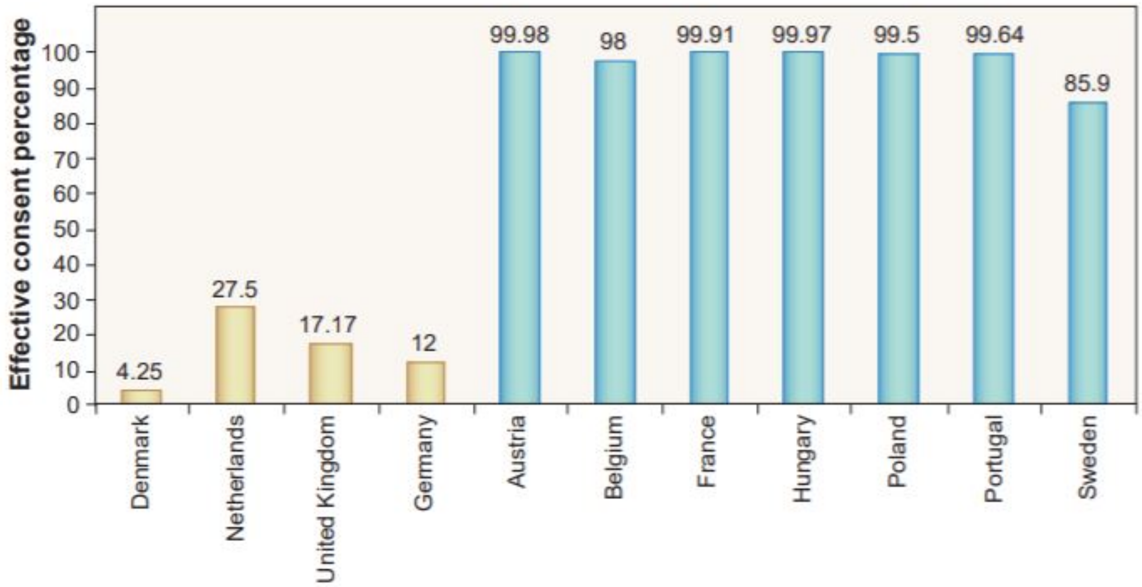
2 Literature Review

In the following chapter, we give an overview of relevant literature to provide a better understanding of the relationship between this study and previous research. As previously pointed out, our study is the first to examine how changing the invoice default option from the minimum to the full credit card balance impacts repayment. Accordingly, there is a lack of relevant literature on the relationship of interest. However, there exists an extensive amount of research on the behavioral implications of default options as well as the general features of credit cards. Thus, the chapter is divided into two main sections: in the first, we present relevant literature on the default effect, the anchoring effect and nudge theory. Second, we consider the literature on credit card debt and minimum payments.

2.1 The default effect

During the last decades, research in economics and psychology has shown that human decision making often is affected by subtle supposedly irrelevant factors (Thaler and Ganser, 2015). Within the field of behavioral economics, *the default effect* is the concept that individuals experiencing choice avoidance, tend to select the default option, thus keeping the status quo (van Kleef et al., 2018). Moreover, individuals might believe that the default is chosen for a reason and accordingly think it is the optimal choice. Whether altered default options can produce significant behavioral change among humans, is consequently an important consideration.

The literature shows several examples of default effects. Johnson and Goldstein (2003) examine the differences in organ donation participation, comparing countries using an opt-in system to those using an opt-out system. In the opt-in system, individuals have to opt-in in order to become an organ donor, and are not enrolled in the program if no action is taken. In contrast, all participants in the opt-out system are organ donors, unless they actively opt out. Figure 2.1 shows the consent rate in European countries with different systems. There is a strikingly large difference in consent between countries which presumably are fairly similar, such as Germany (12% consent rate) and Austria (99.98% consent rate) or Denmark (4.25% consent rate) and Sweden (85.9% consent rate).



Effective consent rates, by country. Explicit consent (opt-in, gold) and presumed consent (opt-out, blue).

Figure 2.1: Figure adopted from Johnson and Goldstein (2003)

The study illustrates the power of the default effect. The authors argue that keeping the default requires less effort than making a switch. Hence, countries facing an opt-out system show a much higher participation rate. Studies on the effect of opt-in versus opt-out systems in organ donation have led more countries to adopt the opt-out system, with the United Kingdom being one of the most recent adopters (Department of Health and Social Care, 2020).

In a field study, Choi et al. (2001) investigated whether automatic enrollment in 401(k) pension plans affects saving decisions. Studying three U.S companies, the authors examine the effect of switching from a volunteer enrollment (opt-in) to an automatic enrollment (opt-out). They found that adopting an opt-out system significantly increases 401(k) participation in all companies. Figure 2.2. illustrates the effect for one of the companies.

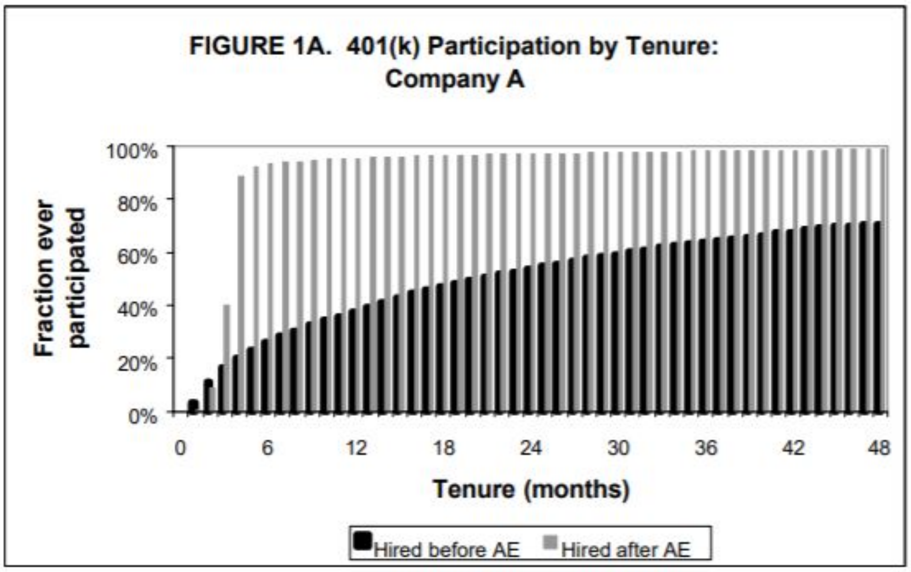


Figure 2.2: Figure adopted from Choi et al. (2001)

When the companies changed to an opt-out system, they also chose a default *contribution rate and fund allocation*. The default contribution rate was set at 2% in company A and 3% in company B and C. On the other hand, the employees enrolled in the opt-in system had to actively choose their contribution rate, as they were not provided with a default option. Interestingly, the authors found that automatic enrollment in 401(k) saving plans led most employees to keep the default contribution rate and fund allocation. For the employees who actively chose to participate in 401(k) saving plans during the opt-in system, the modal contribution rate was 6% in all three companies. The employees automatically enrolled during the opt-out system, however, exhibited a contribution rate of 2% in company A and 3% in company B and C. The findings indicate that automatic enrollment increases the likelihood of passively accepting the default option.

The findings from the organ donation and pension saving studies provide examples of how automatic enrollment can lead to increased participation, attributed to the default effect. One important distinction is that becoming an organ donor does not incur any financial obligations, while enrollment into pension savings do. Hence, the 401(k) savings plan study suggests that even when a presented option has financial consequences, people tend to choose the default. Moreover, economist Stefano Dellavigna (2009) points out that "(...)the finding of large default effects is one of the most robust results in the applied economics literature of the last ten years."

2.1.1 Anchoring

Another central concept to human decision making, is *anchoring*. In their 1974 paper "Judgment under Uncertainty: Heuristics and Biases", Amos Tversky and Daniel Kahneman (1974) explained anchoring as a phenomenon which arises in situations where "(...) people make estimates by starting from an initial value that is adjusted to yield the final answer". Accordingly, the final estimate is biased toward the initial value.

The literature has examined the relationship between default options and anchoring. Park et al. (2000) illustrate this through three studies which aim to examine the difference between *additive- and subtractive* option framing on customers' decision making. The participants were presented with situations where they were assigned a product, and subsequently were able to add or subtract certain options. The customers assigned to the subtractive option framing were presented with a "fully loaded" product and asked to remove the product options they did not want. The customers assigned to the additive option framing were presented with a base product and asked to add the product options they wanted. Moreover, the groups were confronted with the corresponding price of the base- or fully loaded product. The price would increase (decrease) for each option that was added (removed).

The studies showed that customers presented with a subtractive option framing, included significantly more options than customers in the additive group, leading to a higher total price. Accordingly, the authors argue that both the price and the options the customer initially were presented with, served as anchors. The studies suggest that the participants were biased towards their initial assigned status, in other words, towards their default options, which they based subsequent judgement upon. This illustrates how individuals' decision making can be influenced by default options, even when they do not accept the default option itself.

The previously discussed findings imply that individuals presumed rational decision making can be altered simply by the way certain options are presented. Business owners, governments or other stakeholders may use this notion in order to achieve a desired result.

Influencing individuals through such behavioral tweaks, is called *nudging*.

2.1.2 Nudging

Thaler and Sunstein (2009) give the following definition of a nudge:

"A nudge, as we will use the term, is any aspect of the choice architecture that alters people's behavior in a predictable way without forbidding any options or significantly changing their economic incentives. To count as a mere nudge, the intervention must be easy and cheap to avoid. Nudges are not mandates. Putting the fruit at eye level counts as a nudge. Banning junk food does not."

Central to the theory of nudging is choice architecture, meaning how a choice is designed. The theory states that even small tweaks to the choice architecture, implemented correctly, can lead to substantial changes in behavior. Nudges have proven to be effective in certain instances, particularly when individuals are facing complex options or when the benefits from a specific choice are not instantly rewarded (Thaler and Sunstein, 2009). Examples of such situations include incentivizing people to pay their taxes, recycle or eating healthy (Rohaidi, 2020).

Similar examples of successful nudge interventions have led the topic to gain popularity. The field of work is steadily increasing and nudge units have been established in several countries, including the UK's Behavioral Insights Team (The Behavioral Insights Team, 2020) and the US' Social and Behavioral Sciences Team (Shankar, 2015).

2.2 Credit card debt and minimum payments

It has been the aim of governments and other regulatory bodies to slow down the credit markets and limit the interest driven debt (Regjeringen, 2016). Previous research has focused on the design of credit card invoices and the nature of the minimum payment option.

Keys and Wang (2019) conducted a study on a sample of one quarter of the US general-purpose credit card market, where they documented that 29% of customers regularly make payments at or close to the minimum payment. The authors investigated the effect on debt repayment when implementing changes to the minimum payment formula, by increasing or decreasing the minimum amount. Additionally, Keys and Wang (2019) investigated whether various customer groups responded differently to the changes. For that purpose, customers were divided into the groups *exact minimum payer*, *near-minimum payer*, *mixed payer* and *full payer*.

The authors found that 9% of all customers and at least 22% of near-minimum payers responded to the changes in a manner consistent with the anchoring effect. They found that the anchoring effect was immediate, regardless of whether the amount was adjusted upwards or downwards. On this ground, they argue that the minimum payment can serve as an anchor, and that the effect was most prevalent among the low paying customers. The findings imply that adjusting the size of the minimum payment is a useful measure in affecting credit card debt repayment.

In their 2015 paper, Jones et al. (2015) investigated what effect informational nudges implemented in the US credit card market through the The Credit Card Accountability Responsibility and Disclosure (CARD) Act, had on consumers' debt repayment. The act imposed a range of regulations on credit card issuers, where the most significant changes regarded the monthly billing statements. The act required information on payment due date and late fees, payoff times and penalty interest rates to be stated on the front page of the invoice.

The authors found that the monthly repayments (relative to debt) after the CARD act, increased by 5.9 percentage points compared to before the change. Moreover, the study suggests that the likelihood of paying the full outstanding amount for the average customer increased by 10 percentage points after the CARD act was implemented. Lastly, the authors found that customers who experienced recurring month-to-month debt (customers who carry credit card balances month to month), did not demonstrate changed behavior following the CARD act. They argue that the customers with recurring debt are more likely to be liquidity constrained, which prevents them from repaying more than the minimum. The findings imply that informational nudges are integral for credit card repayment. Such nudges make consumers aware of the actual cost of credit card use, which could lead to an increase in debt repayment. The latter finding implies that various customer types tend to respond differently to informational nudges and that nudges might be overshadowed by other, more powerful effects.

3 Background

In the following chapter, we present the motivation behind the government regulation and details surrounding the implementation. Subsequently, we present the motivation behind our study, before we conclude the chapter with a presentation of our hypotheses.

3.1 Motivation behind the regulation

Throughout the last decade, the Norwegian credit market has grown substantially (Wig, 2020). Because credit card debt normally involves high interest rates, deferral of payments can become expensive. The Norwegian Consumer Authority[1] argue that credit card customers value financial institutions providing them with an invoicing solution that encourages them to make the most financially sensible choice (Haugseth, 2016). On that basis, Norwegian authorities imposed the previous guidelines on all credit card issuers in Norway to align with customers' preferences (Regjeringen, 2016).

In 2013, the Financial Supervisory Authority of Norway (FSA) proposed new guidelines for invoicing of credit cards, implying that issuers should display the default option as either unfilled or the total amount due (Regjeringen, 2016). The guidelines were motivated by surveys revealing that credit card issuers did not inform customers adequately about the costs late credit card repayment incurs. Their research revealed that credit card issuing institutions exhibited varying degrees of guideline implementation. As a result, the guidelines were further updated in 2016, encouraging issuing institutions to state the full outstanding credit card debt as the default option.

The issuing institutions and the FSA received many customer inquiries regarding the possibility to be invoiced the total outstanding amount (Regjeringen, 2016). Furthermore, a study showed that approximately half of the customers paid the total outstanding amount or more, a quarter paid more than the minimum amount but less than the total amount, whilst the remaining customers paid the minimum amount. The

[1]The Consumer Authority is an independent administrative body of the responsibility of supervising measures in the market and seek to exert influence on traders to observe the regulatory framework (Forbrukertilsynet, 2020).

survey results along with with the customer inquiries, caused the FSA to enforce the previous guidelines on all relevant institutions. The regulation required all financial institutions to issue invoices displaying the total outstanding amount as the default (Regjeringen, 2017). The main motivation behind the regulation was to make customers more aware of the cost of consumer credit, while simultaneously making it easier to repay outstanding credit card balance (Regjeringen, 2016).

The FSA further argues that *"the regulations can contribute to more customers paying the total outstanding credit when invoiced for the first time. This means less interest costs for customers and, on the other hand, less interest income for credit card issuers. The regulations will help reduce the risk of customers incurring debt which can later impose significant financial burdens on them"* (Regjeringen, 2016). The provision stipulates *how* the invoicing should be carried out, and does not determine the amount the customers are obligated to pay. Hence, the regulation encourages repayment while it continues to preserve the flexibility credit cards provide.

3.1.1 Implementation of the change

The regulation was imposed on all Norwegian credit card suppliers, with the notion that the change of default had to be implemented by June 15th 2017. The bank considered in this study, implemented the default option change on all invoices in February 2017 (appendix figure A.1). In addition to the default option change, the new guidelines mandated the suppliers to provide a sufficient description of the consequences of paying less than the total credit card balance. This included a table showing the costs of different payments, which intended to provide the customers with an accurate depiction of the cost of deferred repayments[2].

3.2 Motivation for our study

Benartzi et al. (2017) argue that interventions aiming to alter decision making through nudges are by nature easy and cost-efficient to implement. However, the implications

[2] In the thesis, we consider customers from a large Norwegian bank which had implemented the mandated cost table on all invoices, prior to the regulation. Hence, changing the invoice default option was the considered bank's only implication of the regulation in 2017.

of such interventions are debated. In a quantitative study, Hummel et al. (2019) find that "(...)only 62% of nudging treatments are statistically significant", that "Nudges have a median effect size of 21% which depends on the category and context" and finally that "Defaults are most effective while precommitment strategies are least effective".

With this in mind, we find it interesting to research whether the Norwegian government's attempt to nudge credit card customers into paying a larger proportion of their debt, has had a significant effect on repayment. Affirmative findings would strengthen the argument that nudge interventions can be efficient, and encourage larger scale research in the future. Moreover, previous research has found that adjustments to information given on credit card invoices (Jones et al., 2015) and modifications to minimum payment formulas (Keys and Wang, 2019), can lead to increased credit card debt repayment. With the aim of expanding on this research, an interesting aspect to our study is whether stating the full credit card balance as the default amount, provides an approach yielding more desired results than changing the required minimum.

As previously pointed out, a survey conducted by the FSA revealed that approximately 50% of Norwegian customers paid off their total credit card balance, 25% paid an amount between the minimum and the total amount whilst the rest paid the minimum amount. Inspired by the survey, we find it particularly interesting to research whether repayment ratio increases as a cause of the regulation implementation, despite the fact that most customers seem to pay the full amount to start with. Secondly, if the customers who paid a low proportion initially did so as a cause of paying the default option, we believe that these customers are likely to increase their repayment relatively more than the other customers. Consequently, we present our two hypotheses:

Hypothesis 1: Changing the default option from the minimum to the full amount led to an average increase in monthly credit card debt repayment in Norway

Hypothesis 2: Changing the default option led to a larger than average increase in monthly credit card debt repayment within the segment of customers who initially paid a low proportion of their monthly debt in Norway

4 Data

In this chapter we present the data used to investigate the effect of the default option change on repayment ratio in Norway. Lastly, we describe the variables included in the main specified regression model and report descriptive statistics.

4.1 Data collection

We collected account-level data from *Eika Kredittbank*, which is a credit card distributor and a part of *Eika Alliansen*. The latter is a financial service company consisting of approximately 60 banks located across Norway (Eika Alliansen, 2020). The credit bank provide approximately 330,000 customers with credit cards, and the collected data contained all customer transactions ranging from May 2015 through August 2020. Additionally, information about transaction date, encrypted account ID, age group, county, credit card limit, negative- and positive transactions and transaction-specific information (amount, currency and in some cases, the type of transaction) were included.

4.2 Data preparation

For analysis purposes, we needed to distinguish consumption from repayments. However, because we exclusively received category information from 2017 onwards, repayments are separated from consumption by categorizing negative transactions as consumption and positive transactions as repayments. Otherwise, data from 2017 and forward would not be consistent with 2015 and 2016 data. Negative transactions are solely consumption, while positive transactions can be repayments, refunds or internal payments. Hence, refunds and internal payments might be mistakenly categorized as repayments, yielding a repayment ratio slightly higher than what is realistic. However, such transactions will presumably occur both before and after the policy change, balancing out the impact from the misplacement. Because the main objective of the research is to examine the difference in repayment before and after the regulation, a slightly higher repayment ratio in both periods should not invalidate our findings.

To increase estimation precision, it is desirable to include as many months as possible. However, the COVID-19 pandemic had a substantial impact on credit card usage and repayment ratio (Eika, 2020). Hence, we find it more appropriate to leave data from 2020 out of our analysis to avoid the noise it entails. It is however worth noting that it remains a critical topic for the credit card market in the months and years to come.

Our prepared dataset contains individual data at the transaction level and ranges from May 2015 to August 2019, which constitutes an *unbalanced panel*. An unbalanced panel implies that the dataset does not contain repayment observations on all customers in all months, as not all customers use their credit card every month. Additionally, we removed customers without categorical information (age, county and credit card limit), as well as customers that do not appear both before and after the change. Hence, we compare the same customers and control for relevant time varying background characteristics. The customers missing categorical information exhibit the same pattern as the other customers (evidence can be provided upon request). Consequently, removing them is not of any concern. The prepared data include 182,471 customers yielding a total of approximately 7.9 million paired observations of consumption and repayments, aggregated per customer per month.

4.2.1 Dependent variable of interest

A caveat on this data is that it lacks information on repayments as a share of outstanding debt. In order to measure the effect of altering the default option on debt repayment, we created a proxy for repayment ratio. Because customers are invoiced the month after credit card use, the analysis hinges upon the assumption that the customers pay their credit card balance the month after consumption. Consequently, we calculate the repayment ratio according to the following equation:

$$Repayment\,ratio = \frac{Repayment\,in\,month\,n+1}{Consumption\,in\,month\,n}$$

If a customer repays the entire debt in a given month, repayment should correspond exactly to consumption in the prior month.

Additionally, we have limited the ratio to be a number between 0 and 1, stipulating a repayment between 0 and 100 percent of the outstanding debt. In some cases, the actual ratio is higher than 1 (negative balance), indicating that customers pay more than the outstanding amount. In such instances, our proxy variable would be unrealistically low. The following are examples of scenarios where this could occur:

1: A customer has accumulated debt over several months, and eventually repays a large fraction in a month with relatively low consumption

2: A customer is repaying more than 100% of the debt prior to consumption in order to increase the credit limit for a period (e.g if the customer is planning to make a large purchase, and wants to pay via credit card)

We imposed the limitation in order to get a realistic mean of repayment over time. Without an upper limit on the ratio, a high one-time repayment could lead to an artificially high average for certain customers. For interpretation purposes, we also find it reasonable to report the repayment ratio as a decimal between 0 and 1.

4.2.2 Independent variables

The independent variable we are interested in estimating the effect of, is defined as the dummy *Post intervention* which is 1 after the intervention in February 2017, and 0 before (detailed in chapter 5). For descriptive purposes, we focus on the following control variables[3]:

Monthly transactions: The number of transactions per customer for a given month

Consumption: The total credit card consumption per individual per month in NOK

[3]Month-fixed effects are also included as control variables in the main regression model, which will be further detailed in chapter 5

4.3 Descriptive statistics

For analysis purposes, we utilize a random sample of 25% of our prepared dataset from May 2015 to December 2019, which comprises 45,618 unique customers. The sample includes a total of approximately 2 million aggregated monthly consumption observations including associated repayments. The tables below show summary statistics for the full sample and the 25% sample, respectively.

Table 4.1: Summary statistics for the full sample

Statistic	Median	Mean	Min	1st Qu.	3rd Qu.	Max
Consumption	1,470	3,886	0	450	4,580	1,238,760
Repayment	1,480	3,850	0	460	4,180	1,270,260
Ratio before	1	0.80	0	0.60	1	1
Monthly transactions	4	6.8	1	2	8	538
Average purchase	-	605.8	-	-	-	-

Table 4.2: Summary statistics for the 25% random sample

Statistic	Median	Mean	Min	1st Qu.	3rd Qu.	Max
Consumption	1,470	3,906	0	450	4,600	859,240
Repayment	1,490	3,905	0	490	4,280	859,250
Ratio before	1	0.80	0	0.60	1	1
Monthly transactions	4	6.87	1	2	8	293
Average purchase	-	606	-	-	-	-

Examining table 4.1 and 4.2, we observe that both average monthly consumption and repayment per individual is close to 4000. Another interesting finding looking at *Ratio before* is that customers on average repay 80%, before the intervention. The reported median is 1, pointing out that at least 50% of the observations are full repayments. Moreover, customers carry out approximately 7 transactions on average each month, while the average transaction size is approximately 606 NOK. It is worth noting that the tables depict large sample differences in maximum consumption and repayment. The maximum values are substantially lower for the 25% sample distribution than for the full sample. However, the reported samples' mean and median are approximately similar, which is a consequence of the large sample sizes. Henceforth, we utilize the 25% random sample.

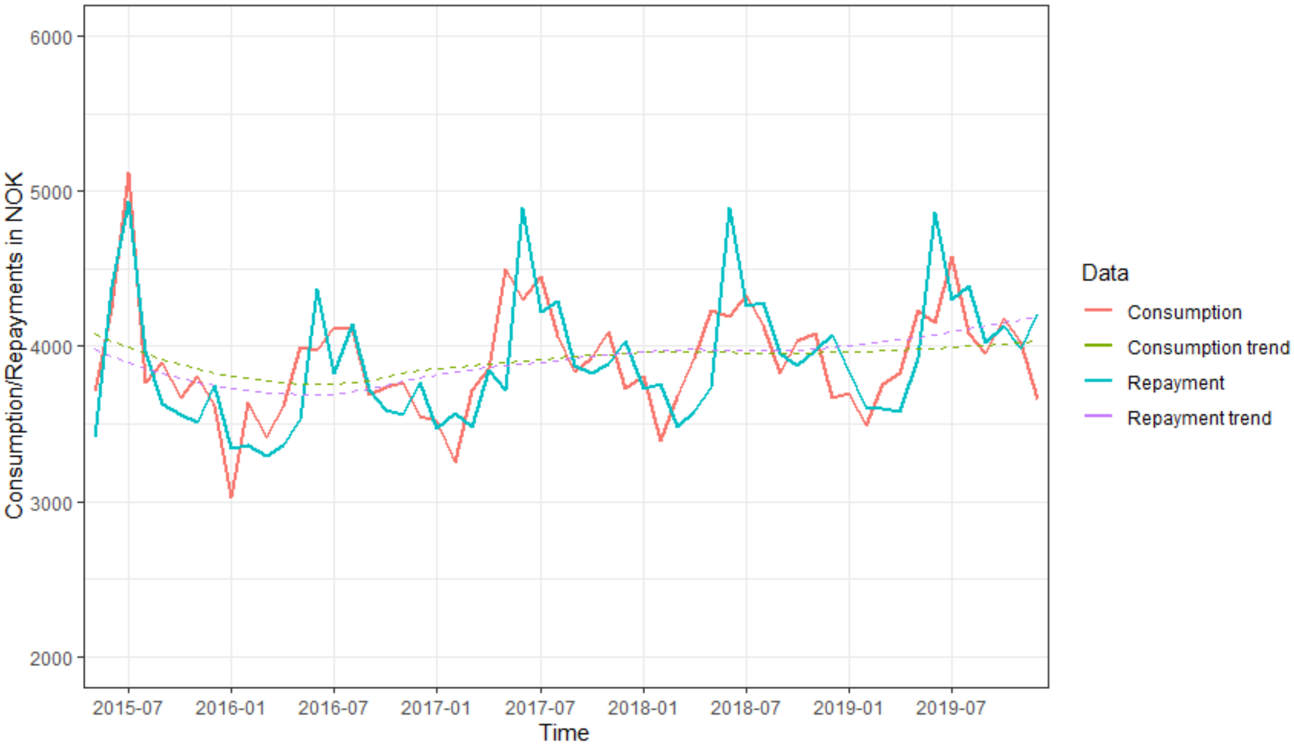


Figure 4.1: Development of average consumption and repayments

Figure 4.1 provides a visual representation of the development of consumption and repayment throughout the time period. It is evident that repayments and consumption strongly covariate. We observe clear fluctuation with peaks in the summer months and troughs in the winter months. Furthermore, there seems to be a minor change in consumption and repayment trends, with relatively higher repayments from mid-2017. However, the figure should be carefully interpreted as macroeconomic factors are likely to exert influence.

4.3.1 Customer characteristics

Figures 4.2, 4.3 and 4.4 show the sample's credit card limit-, age-, and county distribution.

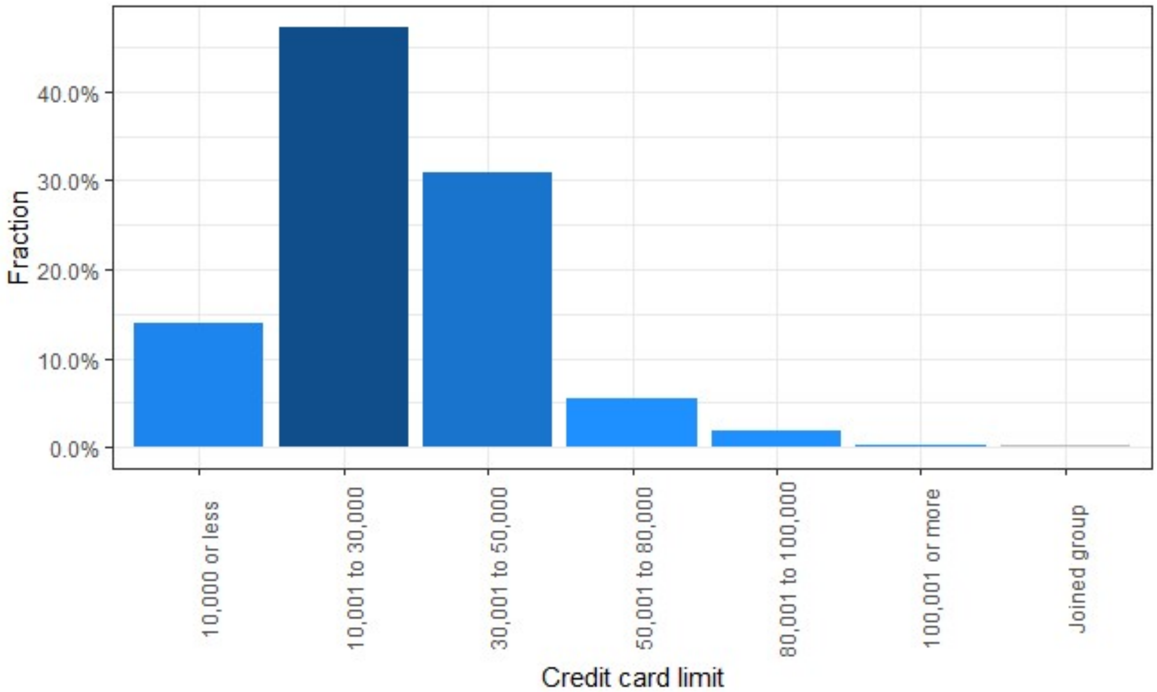


Figure 4.2: Fraction of customers within credit card limit segments

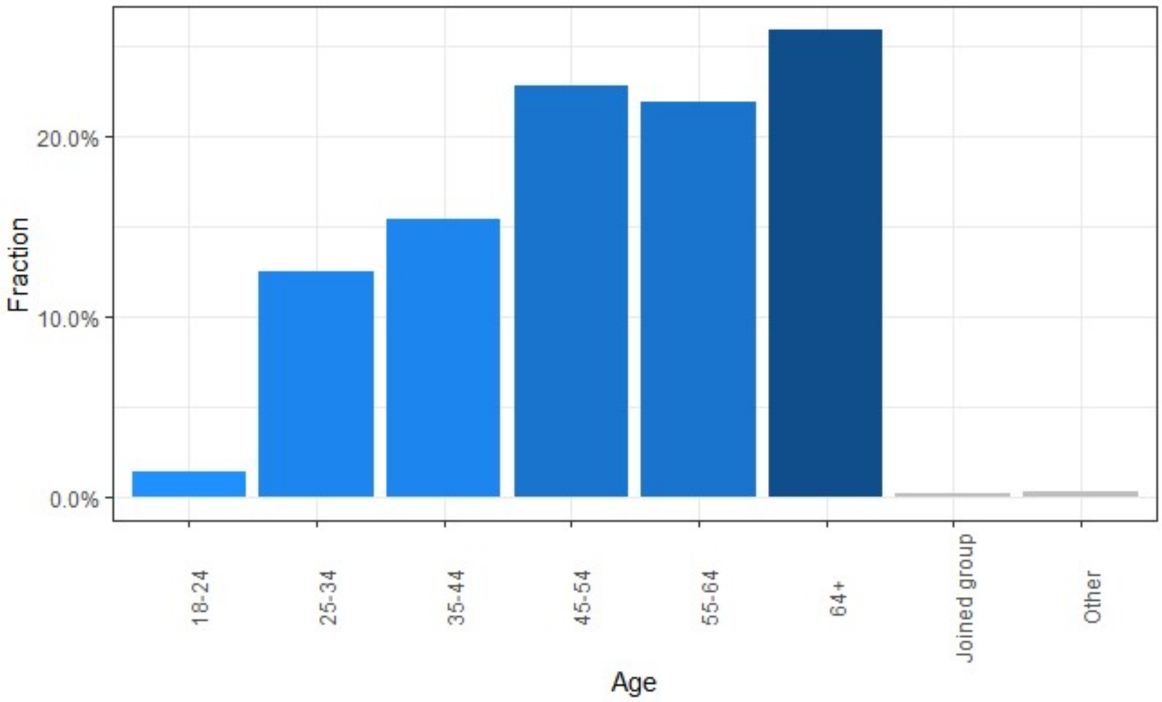


Figure 4.3: Fraction of customers in age segments

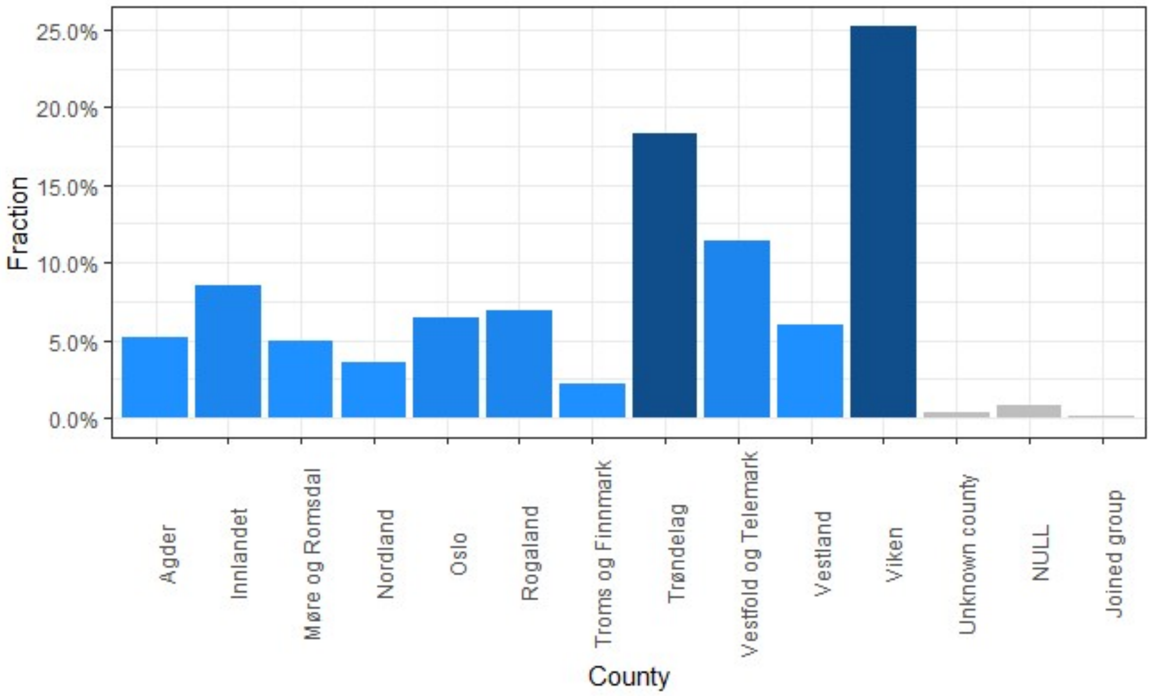


Figure 4.4: Fraction of customers within counties

Examining figure 4.2 and 4.3, we observe that around half of the customer base have a credit card limit between 10,001 and 30,000 and approximately 26% of the customers are 64 years or older. It is worth noting that the age group 64+ is a broader segment than the other groups. Looking at the customer distribution between different counties shown in figure 4.4, we observe that Viken and Trøndelag have the largest proportion of customers. This coincides with the distribution of banks in Eika Alliansen, displayed in appendix table A.1.

4.3.2 Descriptive statistics for near-minimum payers

With the intention of testing our second hypothesis, we separate customers into groups based on their repayment behavior prior to the regulation[4]. We define *near-minimum payers* as individuals with an average initial repayment ratio below 50%. This group constitutes 3,202 customers, which is approximately 7% of our sample. Table 4.3 reports sample statistics for exclusively the near-minimum payers.

[4]The customer divison will be further discussed in chapter 6.

Table 4.3: Summary statistics for near-minimum payers

Statistic	Median	Mean	Min	Pctl(25)	Pctl(75)	Max
Consumption	950	3,255	0	340	3,510	384,400
Repayment	750	3,074	0	0	2,200	500,000
Ratio before	0.2	0.41	0	0	1	1
Monthly transactions	3	6.042	1	2	7	293
Average purchase	-	558	-	-	-	-

The near-minimum payers' average repayment ratio before the intervention is 41%, while the median is 20%, indicating that most repayments are lower than the average. Hence, the repayment ratio distribution is skewed to the left. Furthermore, we observe that average consumption and repayment is lower for near-minimum payers than for the other customers.

5 Methodology

In this chapter, we present the estimation methods conducted to quantify the causal impact of changing the default option on repayment. We present pooled OLS, fixed effects estimation and assumptions underlying the preferred model.

5.1 Pooled OLS

Pooled Ordinary Least Squares (pooled OLS) estimation is the starting point in providing the coefficient estimate of interest. The estimator is applied to panel data structures, and ignores the panel structure of the data by treating all observations equally (Wooldridge, 2016). We illustrate a basic panel data regression model without control variables in the following equation:

$$Y_{it} = \alpha + \beta_1 X_{it} + u_{it} \tag{5.1}$$

In equation 5.1, Y_{it} denotes the dependent variable for a given individual i at time t. X_{it} is the independent variable of interest. The regression line intercept is denoted by α, while u_{it} is the composite error term, defined as $u_{it} = \alpha_i + \epsilon_{it}$. The composite error term consists of both time-invariant unobservables denoted by α_i, and idiosyncratic errors ϵ_{it}, which varies across individuals and time.

The pooled OLS estimator assumes exogeneity, meaning that the composite error term must be uncorrelated with the independent variable of interest. A violation to this assumption would yield endogeneity bias, meaning that the value of the estimated coefficients are not reflective of their true population value (Wooldridge, 2016). A common source to endogeneity is *omitted variables*, which will end up being absorbed by the error term when not controlled for. This would in turn provide our estimate with bias. A way of solving this problem is to include any factors that we suspect to provide our estimate with omitted variable bias. However, including such variables might be difficult in a panel data set, due to variation in two dimensions. In our case, an example could be unobserved individual fixed effects that correlate with the implementation of the invoice regulation. As we wish to reduce the possibility for omitted variable bias, we emphasize the *fixed- and random effects estimators*.

5.2 Choosing a fixed effects or random effects estimator

A central advantage with the fixed- and random effects estimators, is that they eliminate time-constant unobservables that might be correlated with the independent and dependent variable of interest (Wooldridge, 2016). While the fixed effects (FE) estimator allows for correlation between α_i and X_{it}, the random effects (RE) estimator requires α_i to be both random and uncorrelated with X_{it} in order to yield consistent and unbiased estimates. As we wish to allow arbitrary correlation between fixed individual-specific effects and the invoice regulation, the fixed effects estimator seems to be a more convincing tool.

The standard test to determine which of the aforementioned models to use, is the specification test developed by Hausman (1978). The test checks whether estimators yield significantly different coefficient estimates (Wooldridge, 2016). If β estimates are close, the RE method is preferred. Applying the Hausman test to our data[5], the reported results show that there is correlation between α_i and X_{it}, implying that the random effect assumption is violated. Hence, we choose the fixed effects estimator.

5.3 Fixed effects estimator

The fixed effects estimator which is also known as the within estimator, uses transformation to remove the time-invariant effect α_i prior to estimation. Specifically, the estimator demeans away any observation in the regression that is constant across time (Wooldridge, 2016). Equations 5.2, 5.3 and 5.4 demonstrate the demeaning process:

$$Y_{it} = \alpha_i + \beta_1 X_{it} + \epsilon_{it} \tag{5.2}$$

In equation 5.2, Y_{it} denotes the dependent variable, α_i denote the unobserved time-invariant effect while X_{it} is the dependent variable of interest. The time-variant errors are captured by ϵ_{it}. For each individual i, we average equation 5.2 over time t, and get:

[5]The Hausman test results are provided in appendix table B.1.

$$\overline{Y_i} = \alpha_i + \beta_1 \overline{X_i} + \overline{\epsilon_i} \tag{5.3}$$

Subtracting the average from each individual i over time t ($5.3 - 5.2$), we obtain:

$$Y_{it} - \overline{Y_i} = \beta_1 (X_{it} - \overline{X_i}) + (\epsilon_{it} - \overline{\epsilon_i}) \tag{5.4}$$

The crucial feature about the demeaned data shown in 5.4 is that the unobserved fixed effect α_i is eliminated from the equation, which makes pooled OLS estimation consistent, conditional on the idiosyncratic error term ϵ_{it} being uncorrelated with the independent variable. Because the within estimator itself controls for individual heterogeneity, it does not allow inclusion of any time-invariant explanatory variables, as they will disappear in the transformation. Despite the allowance of correlation between α_i and X_{it}, the FE estimator can still be subject to omitted variable bias if the explanatory variable of interest is correlated with the idiosyncratic errors (Wooldridge, 2016).

5.3.1 Assumptions behind the fixed effects estimator

The assumptions underlying the pooled OLS estimator apply to the fixed effects estimator too (Wooldridge, 2016). Given that the assumptions hold, the fixed effects estimator will be the best linear unbiased estimator. We present the assumptions, and elaborate on whether they are fulfilled in our particular case.

Random sample

The random sample assumption relies on inference being drawn from the sample point estimates to the population estimates (Wooldridge, 2016). Our 25% sample is considered a random sample of the entire Eika credit card customer population, hence, the assumption is satisfied.

No perfect collinearity

Perfect collinearity implies that two or more regressors are perfectly correlated, that is, one regressor can be written as a linear combination of the other(s) (Wooldridge, 2016). In such a case, the model will not manage to distinguish the actual effect on each of the variables, introducing a *collinearity* problem. Perfect collinearity affects the precision of the estimates and standard errors of the concerned variables. However, it does not bias the estimate of the independent variable we aim to estimate the causal effect of - $\hat{\beta}$. Hence, we do not address the issue any further.

Zero conditional mean

The assumption of zero conditional mean implies that the expected value of the idiosyncratic errors given the explanatory variables in all time periods and the unobserved effect, is zero (Wooldridge, 2016). Any violation of this assumption, leads to endogeneity problems. According to Woolridge (2010), there are three causes of endogeneity:

1) Omitted variables
2) Simultaneity
3) Measurement errors

Omitted variable bias arises if there exist omitted variables that are correlated with X_{it} and a determinant of Y_{it} (Wooldridge, 2016). In our study, it seems unlikely that any event affecting repayment ratio occurred simultaneously with the change of default option. Thus, omitted variables seem to be a negligible concern.[6]

Simultaneity implies that the dependent variable is also a determinant of the independent variable. Applied to our case, this is not an relevant issue to consider.

Measurement error of the dependent variable is clearly a discussion point in our study. The problem arises because repayment ratio is based upon the assumption that debt is repaid the month after credit card usage. We cannot prove that this is the case for all customers, as some possibly postpone the repayment. Therefore, we are faced with an approximate rather than a perfect measure of repayment. Because there is no reason to suspect that the measurement error is systematic or correlated with any of the independent variables, the concerned issue does not incur any violation of the zero conditional mean assumption (Wooldridge, 2016).

[6]We include control variables to address potential problems with omitted variables, which will be discussed at a later point.

We consider a random sample of Eika's customers, observing no perfect collinearity nor endogeneity issues. Consequently, the fixed effects estimator should yield unbiased results (Wooldridge, 2016).

No heteroskedasticity or serial correlation

The assumption of *no heteroskedasticity* implies that the variance of the error term for each individual in each time period must be constant. Non-constant variance do not cause estimator bias or inconsistency, but affects the size of the standard errors. Hence, heteroskedasticity implies that t-statistics and confidence intervals are no longer valid (Wooldridge, 2016).

No serial correlation implies that idiosyncratic errors must be uncorrelated with each other, in all time periods. Because our panel data consists of repeated observations for individuals over time, our data might raise the issue of serial correlation. Specifically, a customer's repayment ratio in one period will be an indicator of the repayment ratio for the same customer in the next period. If this is the case, statistical inference becomes unreliable if not corrected for (Wooldridge, 2016).

We test for heteroskedasticity and serial correlation through a Breusch-Pagan and Breusch-Godfrey test[7] which reveal that standard errors are both serial correlated and non-constant. A common approach to solve the inference issue that arises with heteroskedasticity and serial correlation, is to apply clustered standard errors (Wooldridge, 2016). Hence, we apply clustered standard errors on both the individual- and time dimension of our panel data. Given that the presented assumptions hold, the fixed effects estimator is the best linear unbiased estimator. .

[7]Results of the Breusch-Pagan and Breusch-Godfrey tests are reported in appendix table B.2 and B.3

5.4 Main model specification

We employ a within estimator on our panel data. Our main model includes dummies to control for month-fixed effects as well as the control variables *Monthly transactions* and *Log(Consumption)*. The rationale behind the choice of control variables is the expectation that both consumption and the number of transactions are explaining some of the variation in credit card repayment ratio. Another incentive is to reduce potential sources of omitted variable bias. Such a bias could occur if the default option change and repayment behavior is correlated with spending patterns. The motivation behind using the logarithm of consumption is that it leads to coefficients with more appealing interpretations while it also narrows the range, which is particularly true for large monetary values (Wooldridge, 2016). Equation 5.5 presents the regression model we estimate to capture the effect of the the default option change on repayment ratio:

$$Y_{it} = \beta_1 X_t + \beta_2 C_{1it} + \beta_3 C_{2it} + \alpha_i + \gamma_m + \epsilon_{it} \tag{5.5}$$

where the dependent variable Y_{it} is the repayment ratio for a given individual i at time t. X_t is the independent variable we are interested in estimating the effect of, which is a dummy that equals 1 after the regulation and 0 before, denoted as *Post regulation* in the regressions in chapter 6. C_{1it} and C_{2it} represent the time-varying control variables *Log(Consumption)* and *Monthly transactions.* α_i captures unobserved time-invariant effects at the individual level. γ_m represents dummies for month fixed effects, capturing the effect of a specific month m affecting all individuals. Lastly, ϵ_{it} is the time-varying error. Our objective is to estimate β_1, which represents the change in average repayment ratio following the invoice regulation.

6 Analysis

In this chapter, we aim to estimate the effect of changing the default option on repayment behavior. We divide the chapter into two main sections with the purpose of testing the presented hypotheses. Firstly, we estimate the average effect for all credit card customers in the sample. Secondly, we divide the sample into groups with the purpose of testing whether the impact differs among various customers. We utilize our specified main regression model in both instances.

We visualize the evolution in repayment means over the total period in figure 6.1. We observe no clear change in the average size of repayment ratio. However, the plot shows a tendency of slightly higher values after the regulation[8]. Month-specific patterns seem to be evident, which we will look further into at a later point.

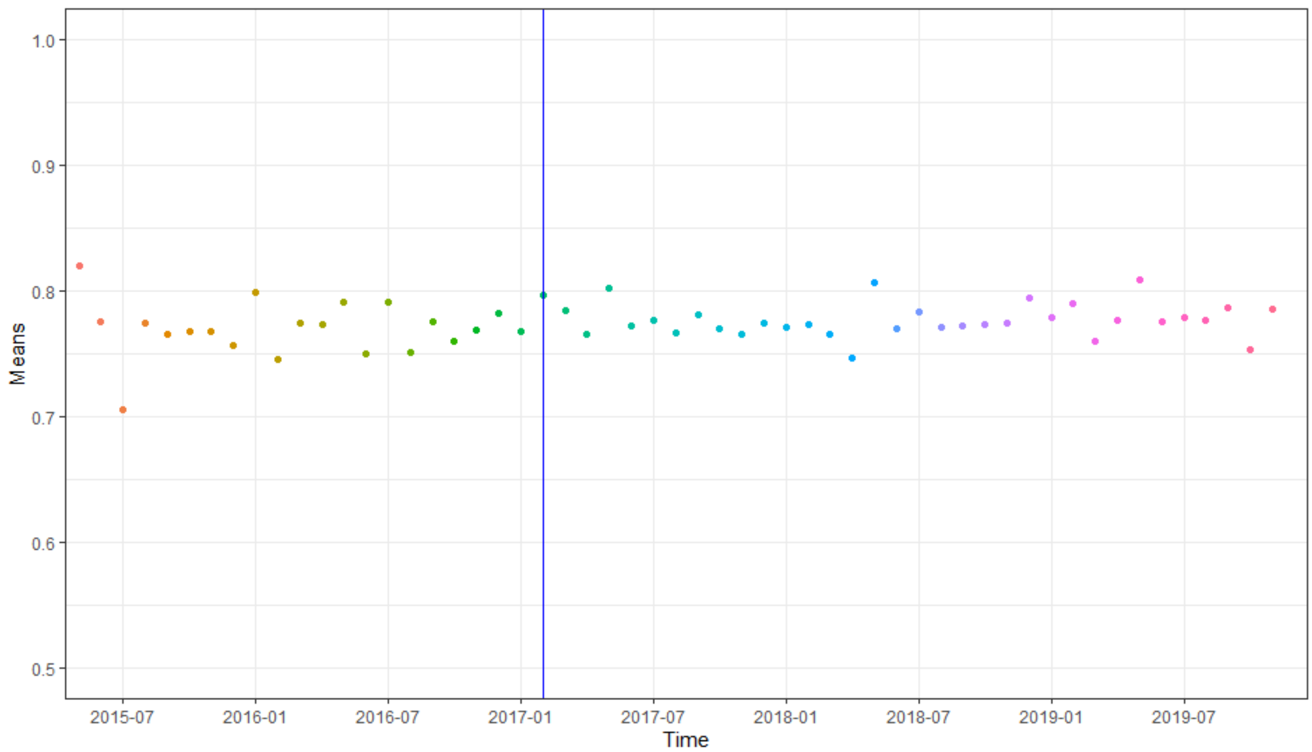


Figure 6.1: Development in average repayment ratio

[8]The vertical blue line marks the time of regulation.

6.1 Testing H1: Average impact

To test our first hypothesis, we start by using a pooled OLS estimator, followed by various fixed effects approaches. Subsequently, we include control variables in the model and report the baseline estimate of interest in column (6).

Regression (1) shows the pooled OLS estimation without control variables, regression (2) is a fixed effects estimation accounting for exclusively individual-specific effects, regression (3) is a pooled OLS estimation including exclusively month fixed effects while regression (4) shows a fixed effects model including both individual- and month fixed effects. Regression (5) and (6) subsequently add the control variables *Log(Consumption)* and *Monthly transactions* to regression (4).

Table 6.1: Pooled OLS and fixed effect models with control variables

	Dependent variable:					
	Repayment ratio					
	(1)	(2)	(3)	(4)	(5)	(6)
	Pooled OLS	Ind. fixed effects	Time fixed effects	Both FE	Both FE with one control	Both FE with two controls
Post regulation	0.008	0.014^{***}	0.009^{**}	0.015^{***}	0.014^{***}	0.015^{***}
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
January			0.001	0.002	−0.001	−0.002
			(0.009)	(0.009)	(0.008)	(0.008)
February			−0.003	−0.004	−0.007	−0.008
			(0.010)	(0.011)	(0.010)	(0.001)
March			−0.009	−0.010	−0.010	−0.011
			(0.008)	(0.009)	(0.009)	(0.009)
April			−0.014	−0.016	−0.014	−0.014
			(0.009)	(0.010)	(0.009)	(0.009)
May			0.027^{***}	0.024^{***}	0.031^{***}	0.031^{***}
			(0.007)	(0.008)	(0.007)	(0.007)
June			−0.010	-0.012^{*}	−0.006	−0.006
			(0.006)	(0.007)	(0.007)	(0.007)
July			−0.010	−0.013	−0.005	−0.004
			(0.014)	(0.016)	(0.014)	(0.014)
August			−0.011	-0.013^{*}	−0.008	−0.007
			(0.007)	(0.007)	(0.007)	(0.007)
September			−0.002	−0.004	−0.001	−0.001
			(0.006)	(0.008)	(0.007)	(0.007)
October			-0.014^{*}	-0.015^{*}	−0.011	−0.011
			(0.007)	(0.008)	(0.007)	(0.007)
November			−0.006	−0.007	−0.003	−0.003
			(0.007)	(0.008)	(0.007)	(0.007)
Log(Consumption)					-0.043^{***}	-0.039^{***}
					(0.0009)	(0.001)
Monthly transactions						-0.002^{***}
						(0.0001)
Constant	0.770^{***}		0.773^{***}			
	(0.006)		(0.005)			
Observations	1,832,529	1,832,529	1,832,529	1,832,529	1,832,529	1,832,529
R^2	0.0001	0.0004	0.001	0.001	0.028	0.029
Adjusted R^2	0.0001	−0.025	0.001	−0.024	0.003	0.004
Residual Std. Error			0.360 (df = 1832516)			
F Statistic	195.836^{***} (df = 1; 1832527)	665.455^{***} (df = 1; 1786910)	154.898^{***} (df = 12; 1832516)	210.431^{***} (df = 12; 1786899)	$3{,}990.602^{***}$ (df = 13; 1786898)	$3{,}845.697^{***}$ (df = 14; 1786897)

Note: Clustered standard errors are reported in the parantheses

r*p<0.1; **p<0.05; ***p<0.01

Interpreting the coefficient estimate of *Post regulation* in regression (1), repayment ratio is approximately 0.8 percentage points higher after the regulation than before, ceteris paribus. The result is not significant. By accounting for individual- and month- fixed effects separately in column (2) and (3) respectively, we obtain positive and significant coefficient estimates while the predictive power of the model also slightly increases. This indicates that both month- and individual fixed effects should be included in the model. By including both individual- and month fixed effects in model (4), the estimate shows a slightly larger and statistically significant effect of a 1.5 percentage points average increase in repayment ratio after the regulation, ceteris paribus.

Basing further analysis on model (4), we add time-variant controls. In column (5) we include the control *Log(Consumption)*, which obtains a positive coefficient estimate with high statistical significance at the 1% level. Hence, it appears to be a relevant control in terms of predicting variation in repayment ratio. Specifically, increasing consumption with 1% is associated with a 0.00043 average decrease in repayment ratio, everything else equal. The coefficient estimate of *Post regulation* barely changes, implying that *Log(Consumption)* is not strongly correlated with *Post regulation*. Furthermore, when we introduce the second control *Monthly transactions* in column (6), the interpretation is that one additional transaction is associated with an average decline of 0.002 in repayment ratio, ceteris paribus. It is evident that both *Log(Consumption)* and *Monthly transactions* are statistically significant and negatively correlated with repayment ratio. The controls do not appear to be highly correlated, as the coefficient estimate of *log(Consumption)* barely changes when including *Monthly transactions*. The *Post regulation* estimate also remains stable. These results suggest that monthly consumption and monthly number of transactions are both predictive and relevant control variables. The result in column (6) implies that the default option replacement led to an average increase in repayment ratio of 1.5 percentage points, everything else equal. In summary, all results except the pooled OLS estimate in column (1) imply that the regulation had a small positive impact on average repayment ratio. Further analysis is based upon model (6), as the results indicate that it is preferred model.[9]

[9] The low R^2 implies that credit card repayment is poorly predicted by the variables included in the model.

6.2 Testing H2: Varying impact between groups

This section extends the analysis by examining customer heterogeneity, with initial repayment ratio as the selection criteria for customer division. The customers are divided into groups based on the following:

Near-minimum payer: Customers who repay less than 50% of their outstanding debt on average before the intervention
Medium payer: Customers who repay between 50% and 70% of their debt on average before the change
High payer: Customers who repay between 70% and 90% of their debt on average before the intervention
Very high payer: Customers who repay more than 90% of their debt on average before the intervention

Table 6.2: Customers divided into groups based on initial repayment

Customer type	Amount of customers	Fraction
Near-minimum payer	3,202	7.0 %
Medium payer	11,757	25.8 %
High payer	18,640	40.9 %
Very high payer	12,019	26.3 %

Table 6.2 displays that the distribution of customer types is skewed, and that the vast majority of customers on average are repaying more than 50% of outstanding debt intially. Following our second hypothesis, we expect that customers who had a low average repayment ratio before the intervention, would increase their repayment ratio relatively more than the other customers as a response to the regulation. The rationale is that customers who initially followed the default, paid the minimum amount stated on the invoice. Given that these customers continued to pay the default following the change, we would expect to see the largest increase in average repayment ratio among the near-minimum payers. The higher paying customers on the other hand, were already paying full or close to the full credit card balance and were thus not able to increase

their repayment in a similar manner. In order to further investigate this hypothesis, we examine the average monthly repayment patterns for the various customer groups over the time period[10].

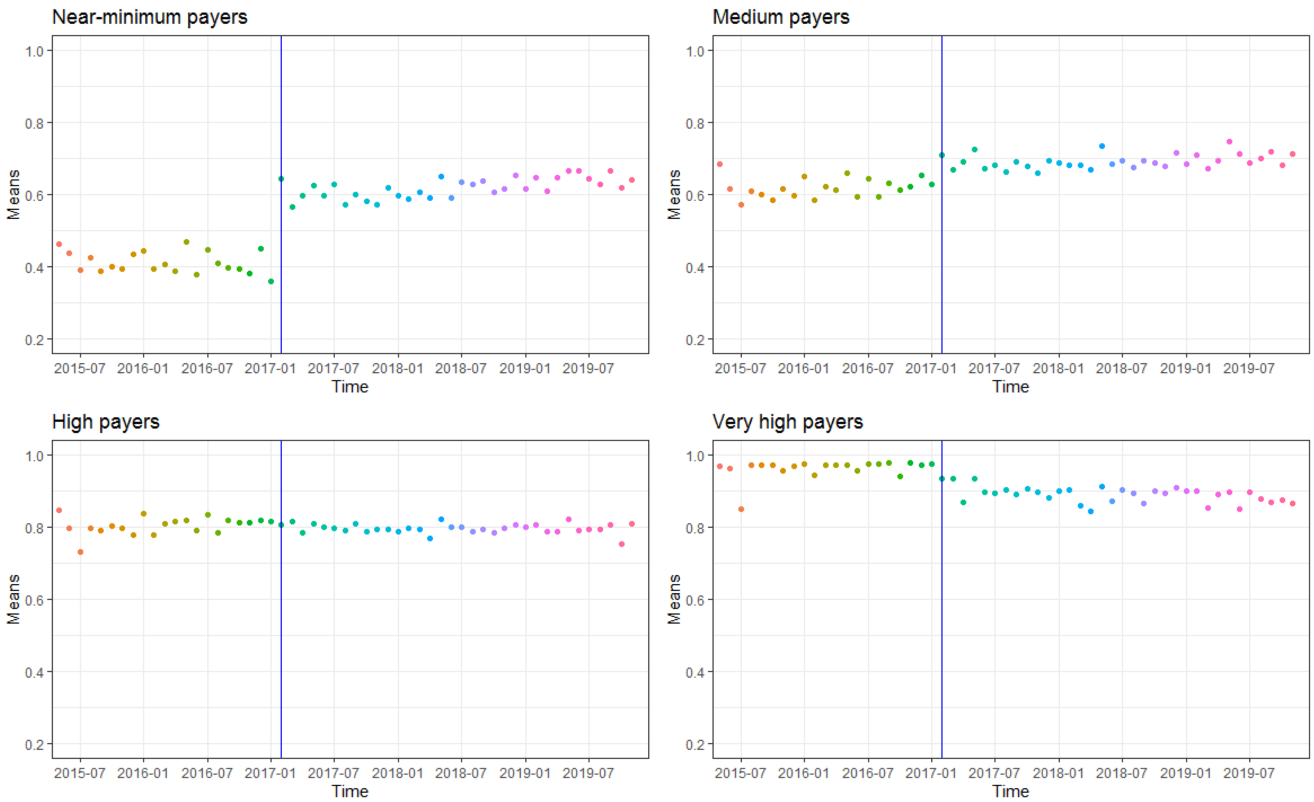


Figure 6.2: Development in average repayment ratio for various customer groups

Figure 6.2 reveals an immediate distinct increase in average repayment ratio for the near-minimum payers, coinciding with the time of the default option change. The increase is smaller, but also evident for the medium payers. The figure shows no visible increase for the high payers, whilst the very high paying group exhibit a slight decline in repayment ratio. The latter could intuitively appear strange, and will be addressed in the discussion chapter. The illustrated development suggests that the altered default option exhibited varying impact among the payer types. In figure 6.3 we illustrate the magnitude of the overall change in average repayment ratio before and after the regulation, for all payer types.

[10]The vertical blue line marks the time of the regulation

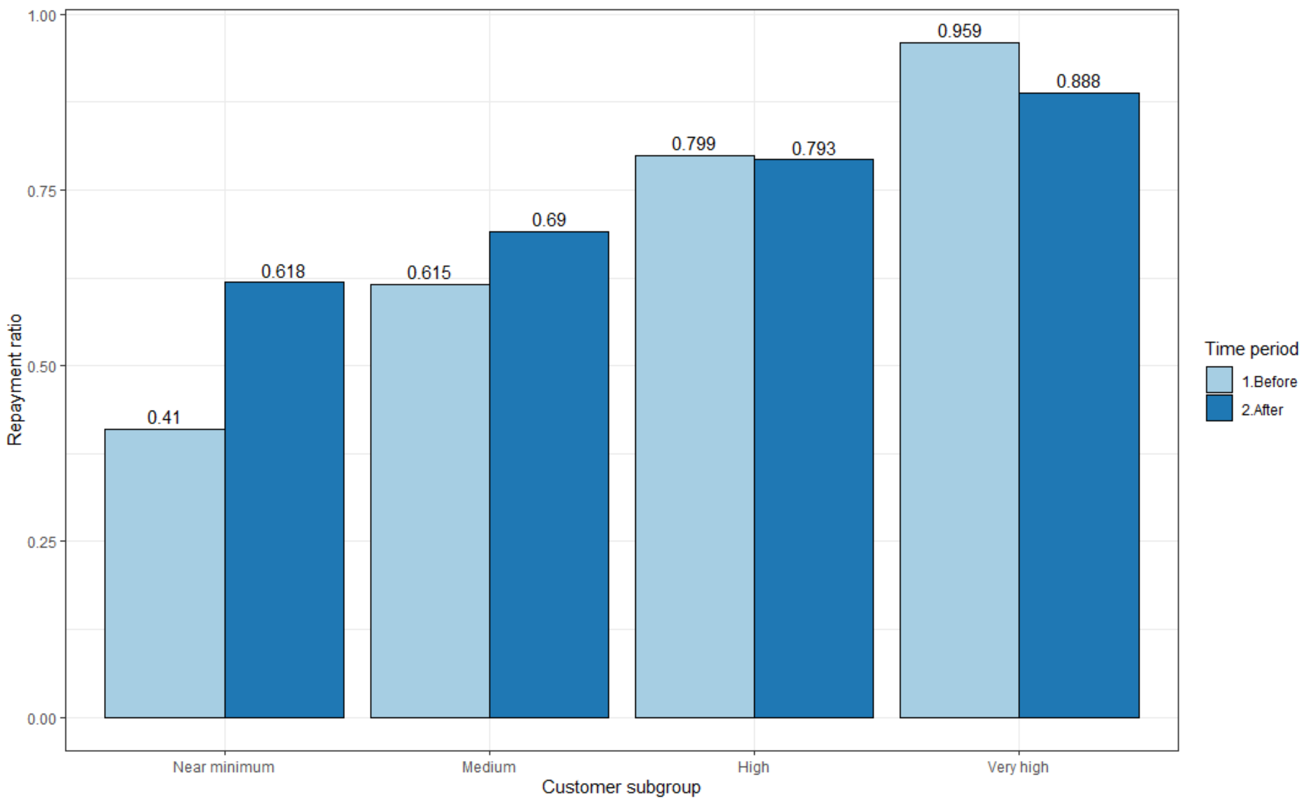


Figure 6.3: Change in average repayment ratio for various customer groups

Figure 6.2 and 6.3 display a development consistent with what we would expect to observe if our second hypothesis is correct. Specifically, the near-minimum payers demonstrate a considerably larger increase in repayment ratio than the other groups. The near-minimum and medium payers show a 50.7% and 12.2% average increase in repayment ratio, respectively. The high payers exhibit a quite stable repayment trend, while the very high payers show a decrease of 7.4%. The fact that the change in average repayment coincided with the time of the altered default option, strongly indicates that the altered behavior is caused by the invoice regulation.

In order to further identify whether the observed increased average repayment is a cause of customers responding to the new default option[11], we investigate the composition of repayments within the various customer groups.

[11]Responding to the new default option implies repaying the full outstanding amount stated on the invoice

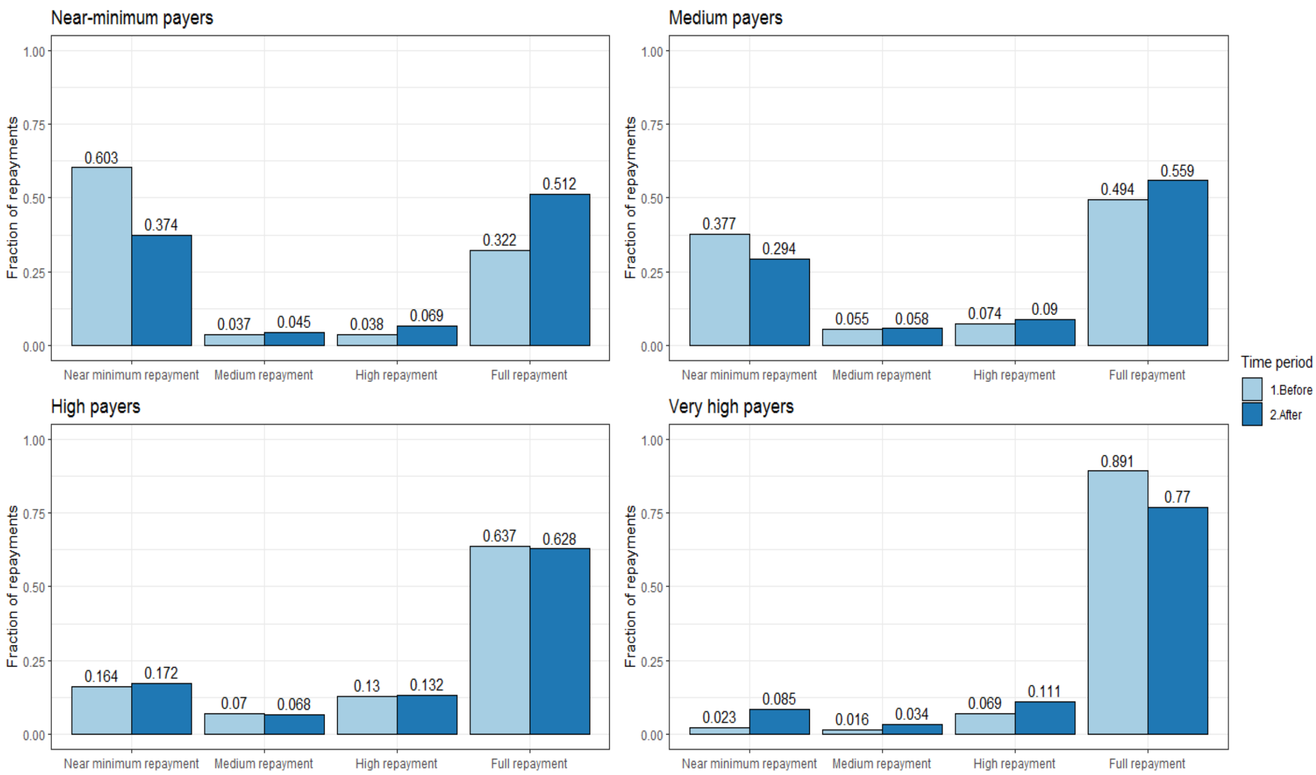


Figure 6.4: Composition of repayments for various customer groups

Figure 6.4 displays the distribution of the various groups' repayments, both before and after the intervention[12]. Figure 6.4 reveals that the average increase in repayment among near-minimum and medium payers (shown in figure 6.2 and 6.3) is a result of a rise in the number of full repayments combined with a fall in the number of near minimum repayments. The high payers do not appear to change repayment behavior, while there is a drop in the number of full repayments among the very high payers. The change in the groups' composition of repayments is consistent with the development in repayment patterns observed in figure 6.2 and 6.3.

Overall, the figures provide valuable insight. Firstly, we observe an immediate and sharp increase in repayment among the groups that initially paid the least, which is also a persistent pattern. Secondly, we see that repayments in both periods have a bimodal distribution with repayments located close to the minimum and full amount. What is interesting is that at the time of the default option change, we observe a rise in the number of full repayments concurrently with a fall in the number of near-minimum

[12]In the figure, *Near minimum repayments* are repayments of less that 50% of total debt, *Medium repayments* are repayments between 50 and 70% of total debt, *High repayments* are between 70 and 90% of debt, while *Full repayments* are repayments of the entire debt

repayments. This strongly suggests that several customers paid the default option amount, and consequently increased repayments, as a result of the invoice adjustment. Lastly, we see that the impact of the regulation differs for various customer groups, with the low payers being more affected than other customers. In summary, the findings substantiate our second hypothesis and suggest that the observed improved repayment behavior is attributed to the default effect. In order to analyze the causal impact of the intervention on the customer groups, we employ our main specified regression model on each group. Results are reported in table 6.3.

Table 6.3: Regression table for customer groups

	Dependent variable:			
	Repayment ratio			
	(1)	(2)	(3)	(4)
	Near-minimum payers	Medium payers	High payers	Very high payers
Post regulation	0.192***	0.075***	−0.004	−0.057***
	(0.007)	(0.004)	(0.004)	(0.007)
January	−0.043***	−0.008	0.006	0.004
	(0.017)	(0.012)	(0.009)	(0.007)
February	−0.023	−0.017*	−0.007	0.002
	(0.016)	(0.013)	(0.009)	(0.012)
March	−0.043***	−0.023**	0.002	−0.013
	(0.013)	(0.011)	(0.009)	(0.017)
April	−0.030**	−0.015***	−0.007**	−0.024*
	(0.014)	(0.011)	(0.009)	(0.013)
May	0.023*	0.044**	0.032***	0.011
	(0.012)	(0.011)	(0.008)	(0.009)
June	−0.021	−0.012	0.003	−0.019**
	(0.016)	(0.012)	(0.008)	(0.009)
July	−0.001	−0.008	0.001	−0.020
	(0.010)	(0.013)	(0.016)	(0.022)
August	−0.021	−0.019	−0.002	−0.002
	(0.014)	(0.012)	(0.008)	(0.007)
September	−0.013	−0.003	0.008	−0.013
	(0.015)	(0.012)	(0.009)	(0.009)
October	−0.035***	−0.018*	−0.006	−0.008
	(0.010)	(0.011)	(0.010)	(0.009)
November	−0.033**	−0.010	0.008*	−0.008
	(0.014)	(0.013)	(0.008)	(0.009)
Log(Consumption)	−0.043***	−0.048***	−0.048***	−0.009***
	(0.003)	(0.001)	(0.001)	(0.002)
Monthly transactions	−0.004***	−0.003***	−0.001***	−0.0004*
	(0.0004)	(0.0002)	(0.0001)	(0.0002)
Observations	112,214	492,064	818,954	409,306
R^2	0.068	0.041	0.039	0.021
Adjusted R^2	0.040	0.016	0.017	−0.007
F Statistic	566.515***	1,473.434***	2,311.404***	620.105***
	(df = 14; 108998)	(df = 14; 479688)	(df = 14; 800377)	(df = 14; 397832)

Note: Clustered standard errors are reported in the parantheses r*p<0.1; **p<0.05; ***p<0.01

Examining the coefficient estimate for near-minimum payers in regression (1), the regulation implied an average increase in repayment ratio of 19.2 percentage points, ceteris paribus. Further, column (2) shows that the medium payers increase their repayment ratio by 7.5 percentage points following the regulation. Both estimates are statistically significant at the 1% level. Column (3) yields a slightly negative coefficient estimate, however not significant, implying no changed repayment behavior among high payers. Lastly, the result from column (4) implies that the intervention had a negative impact on the very high payers. The findings comply with the aforementioned figures. Generally, the results show that the impact of the intervention is largest for customers that are most likely to make low repayments.

6.3 Robustness tests

In order to test the robustness of our results, we conducted a *sensitivity test* to investigate whether altering the time frame impacts our results. Subsequently, we conducted a *placebo test* on a part of the dataset where no real intervention is evident (before the intervention), to verify that the observed estimated effects are not attributed to other effects than the invoice regulation. We employ our main regression model in both tests, and compare the obtained estimates with the findings from table 6.1 and 6.3.

Sensitivity test

The sensitivity test is conducted both for all customers in the sample and the near-minimum payers exclusively. Tables C.1 and C.2 in the appendix show the effects of the regulation when we limit the post-change period to be 3, 6, 9 and 12 months, respectively. The yielded coefficient estimates are fairly similar to our baseline estimates (provided in column (6) in table 6.1 and column (1) in table 6.3). The results show that the effect is prevalent regardless of the time range, with a slightly larger magnitude in the short term.

Placebo test

We conducted a placebo test by regressing our main model on the full period before the real intervention. We created an artificial independent variable *Post regulation*, which is a dummy that is 1 after February 2016 and 0 before.[13]. The placebo test discloses a

[13]The period before the artificial invoice regulation ranges from May 2015 to February 2016, while the after period is from February 2016 to February 2017

slight decrease in repayment ratio of 1.4 percentage points for both the sample and the near-minimum payers, following the artificial change. The results (appendix table C.3) are however not statistically significant. Overall, the placebo test provides evidence that no similar effect was apparent before the invoice regulation, which further supports our hypotheses.

7 Discussion

The previous chapter provided our findings on the effect of the default option change on repayment ratio. In the following chapter, we discuss the interpretation of the findings, which implications they have and to what extent they can be trusted.

The results provide unequivocal and strong support in favor of our hypotheses, suggesting that the default option replacement had a positive impact on Norwegian credit card customers' repayment behavior. However, the average effect is small, which is a consequence of the majority of customers already repaying their entire credit card balance. What is interesting, is that the overall increase appears to be driven by customers who initially paid the least, as they tend to be most responsive to the default option change, as shown in figure 6.2. The estimated effects passes all robustness checks, which provides further evidence in favor of our hypotheses.

Contrary to the findings of Jones et al. (2015), we find that the effect is largest for customers who initially repaid a low proportion of their credit card debt. The results provide evidence that the intervention led to a distinct and immediate improvement in repayment behavior for the near-minimum and medium payers. Most striking is the finding that the near-minimum payers increase their repayment ratio by 19.2 percentage points, following the regulation. By investigating the composition of repayments for the near-minimum and medium payers, it is evident that the majority of repayments are located around the minimum and the full repayment in both periods (shown in figure 6.4). What is interesting, is that we observe a change in the composition of repayments in association with the intervention. Specifically, there is a considerable rise in the number of full repayments simultaneously with a decline in near-minimum repayments. The relative change is largest for the near-minimum payers, but also prevalent for medium payers. Taken together, the findings suggest that a substantial proportion of customers who tended to pay an amount close to the minimum before the intervention, more often pay the full amount as a result of the default option change.

As shown in figure 6.3 and 6.4, the high payers do not change their average repayment behavior nor the composition of their repayments. The very high payers on the other hand, appear to decrease their repayment post-intervention, which could seem counter intuitive. Such a finding could imply that what we observe is in reality a *regression towards the mean*, meaning that extreme outcomes tend to be followed by moderate ones. This would imply that there is no actual increase or decrease and that the observed differences are rather due to pure chance (Bland and Altman, 1994). Another plausible explanation is that customers initially repaying in full, start paying the default amount following the regulation. Prior to the regulation, these customers had to actively alter the amount in order to pay the full credit card balance. As the regulation implied the invoice displaying the full credit card balance as the balance, they no longer had to adjust the repayment. However, the default amount does not include debt incurred from the end of the previous month until the due date of the invoice. In that respect, it is reasonable to assume that some customers who would previously include recent consumption in their repayments, merely accept the default option post-intervention. Accordingly, we would expect the intervention to imply a slightly lower average repayment ratio for this group. Following this argument, we would expect the very high payers to respond in a manner similar to what we observe in figures 6.2, 6.3 and 6.4, as a result of paying the novel default option stated on the invoice. Thus, a default effect would be attributed to this group too.

It is worth noting that we can not be certain whether the changed repayment behavior is caused by a default effect, an anchoring effect or other potential factors[14]. However, the varying impact among the groups suggests that at least parts of the observed effect is attributed to the default effect. The rationale is that, in the event of an anchoring effect we would expect the customer groups to exhibit a relatively similar magnitude of average adjustment. In contrast, our results imply that the regulation affected the groups with varying magnitude, which advocates a default effect. This argument is further supported by the bimodal distribution of repayments for near-minimum and medium payers, displaying that most repayments are located around the near-minimum and full amount. However, we cannot fully disregard the possibility of one or the other effect, or

[14]Note: The distinction between the default effect and anchoring effect is provided within the literature review in chapter 2.

that it might be a composited effect.

Our findings suggest that the intervention produced overall positive effects in its mission to improve repayment behavior, consistent with previous research on the default effect (Choi et al., 2001; Johnson and Goldstein, 2003). Equivalent to the finding that automatic enrollment in organ donation and pension saving programs led to increased participation, the changed invoice default option seems to automatically "enroll" choice averse customers into paying the novel default option. Moreover, because consistently repaying the minimum amount would entail the largest costs in terms of interest and fees, the finding that the near minimum payers are increasing their repayment the most, implies that the intervention also succeeded in targeting the customers most crucial to affect.

7.1 Limitations

Throughout this section, we will shed light on what limitations we believe are the most prominent in our research.

7.1.1 Data limitations

The data preparation introduces potential caveats. Firstly, we have characterized all negative transactions as consumption and all positive transactions as repayments. Such a division will be appropriate in most cases, although there might be some transaction misplacements as discussed in the subsection on data preparation. Secondly, the repayment ratio is limited to be a number between 0 and 1, in order to prevent particularly high repayments to push the distribution in any direction. Nonetheless, it is worth noting that this limitation might yield an artificially low ratio for some customers in certain periods.

7.1.2 Internal validity

The *internal validity* of the study regards whether the established causal relationship is net of all other confounding factors (Gertler et al., 2016), and thus, can be trusted. A high internal validity implies that we are estimating the true impact of an intervention on an outcome.

Non-experimental design

The policy change of interest introduces a potential challenge regarding empirical estimation. Ideally, we would want to divide credit card customers into treatment- and control groups to observe whether the control group demonstrated a counterfactual outcome. However, as the intervention affected all credit card holders in Norway, we cannot divide the sample into control- and treatment groups. Hence, the non-experimental design makes it impossible to identify how repayment patterns would have evolved in absence of the policy change. Accordingly, the internal validity of our estimated effects is weaker than it would have been through an experimental design.

Potential confounding factors

One could argue that the rise in repayment ratio observed after the intervention, rather is a result of general economic conditions than the actual default option change. For illustration, if taxes are reduced simultaneously with the regulation and lead people to improve repayment behavior, not controlling for this tax relief will provide our estimate with omitted variable bias. However, if the significant increase in repayment ratio was in fact a cause of other factors, we would expect the various payer types to exhibit a more similar development in repayment patterns. Although confounding factors seems to be an unlikely issue, it still poses a minor risk.

7.1.3 External validity

Important to any study is the *external validity*, the extent to which the evaluation sample is representative of the population of interest (Gertler et al., 2016). Because we utilize a random sample of Eika's credit card customers, it is considered representative for the credit bank's entire population. Accordingly, the obtained regression results can be generalized to the bank's entire customer base. Eika's credit card customers could however differ from other credit card customers, limiting the external validity of the study. We make the argument that because Eika's credit card population constitutes a substantial proportion of all Norwegian credit card customers[15], the results are reasonably valid for the entire Norwegian population.

[15] According to Norsk Gjeldsinformasjon (2020), there is approximately 3 million customers with credit cards in Norway. As Eika has approximately 330,000 credit card customers, their proportion of the market is approximately 11%

7.2 Further research

Another interesting consideration is the economic impact of customers repaying a larger proportion of their credit card debt. A conceivable possibility is that repaying a larger part of the credit card balance have an adverse effect on other financial obligations. Hence, collecting comprehensive data on the customers' financial obligations, would make it possible to identify the total financial implications of the regulation.

Additionally, equivalent research could be conducted across different countries to identify potential contrasting responses to such an intervention. As shown in table 4.2 and in surveys conducted by the FSA (Regjeringen, 2016), Norwegian credit card customers exhibit a high degree of debt repayment in general. Consequently, a potential topic for further studies is to conduct similar research in a country where average repayment ratio is lower.

8 Conclusion

Within the field of behavioral economics, the default effect is a prominent concept describing how individuals exhibit behavior that differs from standard economic theory (DellaVigna, 2009). It has been the basis for several behavioral nudges aiming to affect individuals' behavior, often yielding promising results.

The purpose of our study was to quantify the causal impact of a regulation imposed on all Norwegian credit card issuing institutions in 2017. The regulation involved changing the default repayment option from the minimum amount to the full outstanding credit card balance. In that respect, we wished to find out whether the intervention had any impact on repayment behavior, for whom and to what extent.

In order to test the hypotheses, we utilized all available account-level data gathered from Eika Kredittbank, and extracted a random 25% sample ranging from May 2015 to December 2019. For that purpose, we calculated a proxy for repayment ratio defined as lagged repayment divided by consumption for each customer in each month. We employed a fixed effects estimator controlling for individual heterogeneity, month-fixed effects, monthly consumption and the number of transactions.

We find evidence that replacing the minimum amount with the entire credit card balance as the default option, led to improved repayment behavior among Norwegian credit card customers. Specifically, we observe a small increase in average repayment ratio of 1.5 percentage points. By dividing the customers into various groups with pre-intervention repayment ratio as the selection criteria, we observed substantial effects. Near-minimum and medium paying customers show an increase in average repayment ratio of 19.2 and 7.5 percentage points, respectively. There is no evident change in repayment pattern for the high paying group, while the very-high payers exhibit a decrease in average repayment ratio. We argue that the latter could be a result of very-high paying customers passively accepting the new default amount, and accordingly no longer include newly incurred debt in their repayments. Furthermore, we witness that the sharp and distinct change in repayment ratio occurs immediately after the imposed regulation for all affected customer groups. Interestingly, the composition of repayments strongly indicates that customers who initially repaid the least tend to repay the full amount more often after the change. In summary, the results strengthen the argument that the changed repayment behavior is in fact a causal implication of the regulation, attributed to the default effect.

Our study joins the ranks of several studies aiming to improve repayment behavior through implementation of regulations. To our knowledge, we are the first to find such a strong link between an altered default option and improved repayment behavior. In contrast to the findings of Jones et al. (2015), we find that the governmental intervention succeeded in targeting the customers most crucial to affect, which suggests that a nudge through the default option can be more effective than other complex regulations.

In summary, our overall findings show that default options matter in human decision making. Most importantly, the results show that the simple act of changing a default option can be an effective measure in combating increasing interest costs that follows incautious credit card use. The findings demonstrate that current and future policy makers should recognize that default options can serve as powerful and cost-efficient tools in altering human decision making.

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Appendix

A Eika Alliansen

eika.

NB! Merk deg
1. februar

Viktig informasjon vedrørende
endring av våre faktureringsrutiner!

For å ivareta de nye retningslinjene fra Finanstilsynet har Eika Kredittbank besluttet å endre sine faktureringsrutiner.

Fra 1. februar 2017 vil faktura bli sendt ut med fullt utestående beløp, selv om det fortsatt selvfølgelig er mulig å fordele betalingen over tid. Du vil alltid finne «Minimum å betale ved forfall» på din faktura. Uavhengig om du har papirfaktura eller eFaktura, kan du enkelt endre til ønsket beløp å betale.

Figure A.1: Informational letter issued to Eika credit card customers regarding the implementation of the invoice regulation

Table A.1: Bank distribution by county (Eika Alliansen, 2020)

Agder	Innlandet	Møre og Romsdal	Nordland
Arendal og Omegns Sparekasse	Etnedal Sparebank	Romsdalsbanken	Gildeskål Sparebank
Birkenes Sparebank	Grue Sparebank	Sunndal Sparebank	Grong Sparebank
Evje og Hornnes Sparebank	Jbf bank og forsikring	Surnadal Sparebank	Jbf bank og forsikring
Jbf bank og forsikring	Odal Sparebank	Ørskog Sparebank	Sparebank 68 Grader Nord
Kvinesdal Sparebank	Tolga-Os Sparebank		Sparebanken Narvik
Valle Sparebank	Totens Sparebank		Ofoten Sparebank
Østre Agder Sparebank	Valdres Sparebank		

Oslo	Rogaland	Vestfold og Telemark	Vestland
Bank2	Hjelmeland Sparebank	Andebu Sparebank	Jbf bank og forsikring
Bien Sparebank	Jbf bank og forsikring	Drangedal Sparebank	Sogn Sparebank
Jbf bank og forsikring	Jæren Sparebank	Hjartdal og Gransherad Sparebank	Tysnes Sparebank
RørosBanken	Sandnes Sparebank	Larvikbanken	Vekselbanken
	Troms og Finnmark	NORDirektebank-del av Skagerrak Spb.	
	Sparebank 68 Grader Nord	Skagerrak Sparebank	
		Sparebanken DIN	
		Tinn Sparebank	

Trøndelag	Viken	Others
Aasen Sparebank	Askim og Spydeberg Sparebank	Forsikring Helgeland
Bjugn Sparebank	Aurskog Sparebank	Jbf bank og forsikring
Grong Sparebank	Berg Sparebank	Penger.no
Haltdalen Sparebank	Blaker Sparebank	Sparebanken Møre
Hegra Sparebank	Eidsberg Sparebank	
Hemne Sparebank	Fornebu Sparebank	
MelhusBanken	Høland og Setskog	
Nidaros Sparebank	Hønefoss Sparebank	
Oppdalsbanken	Jbf bank og forsikring	
Orkla Sparebank	LillestrømBanken	
RørosBanken	Marker Sparebank	
Rindal Sparebank	Skue Sparebank	
Selbu Sparebank	Strømmen Sparebank	
Soknedal Sparebank	Trøgstad Sparebank	
Stadsbygd Sparebank		
Ørland Sparebank		
Åfjord Sparebank		

B Statistical tests

B.1 The Hausman test

Table B.1 presents the results from the Hausman test, which is used to test whether to use a fixed- or random effects estimator. The results show that the random effects estimator is inconsistent and biased, hence we employ the fixed effects estimator in further analysis.

Table B.1: Hausman test

Chisquared	DF	P-value	Conclusion
581	12	2.2e-13	Inconsistent

B.2 Breusch-Pagan test

Table B.2 exhibits the results from a studentized Breusch-Pagan test for the prevalence of heteroskedasticity. The results provide evidence of heteroskedasticity. We make the results robust for heteroskedasticity by applying clustered standard errors in all regressions.

Table B.2: Breusch-Pagan test

BP	**DF**	**P-value**	**Conclusion**
1677	12	2.2e-13	Heteroskedasticity

B.3 Breusch-Godfrey test

Table B.3 displays the results from running the Breusch-Godfrey/Woolridge test for serial correlation in panel models. Based on this test, serially correlated standard errors are present. Hence, we apply clustering to the standard errors to control for serial correlation.

Table B.3: Breusch-Godfrey test

Chisquared	**DF**	**P-value**	**Conclusion**
26425	2	2.2e-13	Serial correlated errors

C Robustness tests

C.1 Sensitivity tests

Table C.1: Sensitivity check: main model regressions for all customers for shorter post-change intervals

	Dependent variable:				
	Ratio				
	(3 months)	(6 months)	(9 months)	(12 months)	(Full period)
Post intervention	0.023***	0.024***	0.020***	0.017***	0.015***
	(0.006)	(0.006)	(0.005)	(0.004)	(0.004)
Observations	815,878	922,083	1,026,335	1,128,125	1,832,529
R^2	0.034	0.033	0.032	0.031	0.029
Adjusted R^2	−0.023	−0.018	−0.013	−0.010	0.004
F Statistic	1,950.552***	2,111.132***	2,284.569***	2,453.952***	3,845.697***
	(df = 14; 770246)	(df = 14; 876451)	(df = 14; 980703)	(df = 14; 1082493)	(df = 14; 1786897)

*p<0.1; **p<0.05; ***p<0.01
Note: Month-fixed effects, the log of monthly consumption and monthly transactions are included as control variables. Clustered standard errors are reported in the parantheses

Table C.2: Sensitivity check: main model regressions for near-minimum payers for shorter post-change intervals

	Dependent variable:				
	Ratio				
	(3month)	(6month)	(9month)	(12month)	(Full period)
Post intervention	0.194***	0.185***	0.181***	0.178***	0.192***
	(0.016)	(0.011)	(0.008)	(0.008)	(0.007)
Observations	44,047	51,310	58,377	65,332	112,214
R^2	0.057	0.062	0.062	0.064	0.068
Adjusted R^2	−0.017	−0.001	0.008	0.015	0.040
F Statistic	177.052***	226.818***	262.066***	301.287***	566.515***
	(df = 14; 40831)	(df = 14; 48094)	(df = 14; 55161)	(df = 14; 62116)	(df = 14; 108998)

*p<0.1; **p<0.05; ***p<0.01
Note: Month-fixed effects, the log of monthly consumption and monthly transactions are included as control variables.
Clustered standard errors are reported in the parantheses

C.2 Placebo test

Table C.3: Placebo test: main model regressions before the intervention

	Dependent variable:	
	Ratio	
	(All customers)	(Near-minimum payers)
Post intervention	−0.014*	−0.014
	(0.007)	(0.009)
Observations	676,417	34,422
R^2	0.033	0.019
Adjusted R^2	−0.037	−0.082
F Statistic	1,545.453*** (df = 14; 630785)	43.589*** (df = 14; 31206)

*p<0.1; **p<0.05; ***p<0.01
Note: Month-fixed effects, the log of monthly consumption and monthly transactions are included as control variables.
Post intervention is an artificial dummy that is 0 from May 2015 until February 2016 an 1 between February 2016 and February 2017.
Clustered standard errors are reported in the parantheses