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Assessing the effect of debit cards on households' spending under the uncounfoundedness assumption

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Abstract

The paper proposes an application of some causal inference methods for the purpose of evaluating the effect of the use of debit cards on households' consumptions. Motivated by the evidence that debit card users overspend in comparison to non-users, the analysis wants to investigate the existence of a causal relationship rather than a mere association. The available dataset allows us to introduce a set of pre-treatment variables so that the uncounfoundedness assumption can be adopted. This gives the advantage of avoiding the introduction of assumptions on the link between the observable and unobservable quantities, and it also improves the precision in comparison to other main methodological options. The analysis results in positive effects on a household's monthly spending; it also provides a comparative application of various causal methods to a real dataset.

Keywords: Average Treatment Effects for the Treated; Uncounfoundedness; Payment Instruments.

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1 Introduction

In this paper, some causal inference methods suitable to correct for the self-selection due only to the observable variables will be applied to assess whether and how the use of debit cards[1] affects Italian households' spending. This application allows us to verify if this non-cash payment instrument can drive the consumer's behavior so as to act as an encouragement to spending.

The past few years have seen an increasing trend in the use of non-cash payment instruments like credit, debit, prepaid cards, and electronic money all over the world. However, the proliferation of non-cash payment instruments has been accompanied by surprisingly little research on the psychological effects they have on consumer spending. Moreover, the relevant contributions are mostly in the fields of consumer and marketing sciences, while there is essentially no evidence in the economic theoretical literature. Even if from a theoretical point of view, the payment mechanism has no role in a rational economic evaluation of a purchase, there is substantial evidence that consumers who predominantly use cards overspend compared to those who do not (Burman, 1974; Hirschman, 1979; Tokunaga, 1993; Cole, 1998). However, the resulting significant dependencies between the level of spending and the possession and use of cards is not sufficient to state the existence of causal links. The evidence could be due to differences between the characteristics of card owners and non-owners characteristics, or to differences in the occasions on which cash and cards are the preferred methods of payment. In order to quantify the effects of the most diffused non-cash payment instrument in Italy (debit card) on consumption, we apply statistical methods that allow us to identify causal effects instead of mere associations.

For this purpose, the causal point of view we want to adopt is one based on the concept of potential quantities. Following this approach and adopting the usual experimental terminology, the individual causal effect for the generic statistical unit i is defined by a comparison, typically made by differences, between the potential outcomes that would be observed under different treatments for the same unit. In our study, we assume the household is the statistical unit i, and we define the treatment as a binary variable that will be equal to one if the possession of at least one debit card in the generic household is observed and equal to zero otherwise. Consequently, the in-

[1] Debit cards are defined as cards enabling the holder to have purchases directly charged to funds on his account at a deposit-banking institution (C.P.S.S., 2001).

dividual causal effect of holding at least one debit card will be defined as the difference between the household spending under two hypothetical situations: $y_{i1} - y_{i0}$, where y_{id} is the potential outcome for i under the binary treatment $d = 0, 1$. Obviously the two situations are not both observable; the definition of individual causal effect is indeed only an hypothetical one. We then move from the concept of individual to that of average causal effect, where the latter is defined as the expected value of the difference in the two potential outcomes[2]: $E[Y_1 - Y_0]$.

The great advantage of thinking in terms of average causal effects is the ability to make the inference feasible; the estimation process becomes particularly simple when the assumption of independence between assignments to treatment and potential outcomes is satisfied (Holland, 1986). This is the case in a randomized experiment where a consistent estimate of the average causal effect is obtainable simply by the difference between the average outcome of the units for which $D = 1$ (treated or cases) and the average outcome of the units for which $D = 0$ (untreated or controls).

In the literature concerning the effects of cards on spending, there are three empirical contributions that seem particularly interesting. They are dedicated to credit cards and based on small sample size experiments that guarantee the random assignments of the units to the treatment (Feinberg, 1986; Prelec and Simester, 2001; Soman, 2001). However, these studies suffer the relevant technical inconvenience that the random assignment to treatment is done on a sample of volunteers. The treated and control groups should instead be randomly selected from a target population so that the experimental design to guarantee that the difference between the average outcome of treated and controls is an unbiased estimate of the average causal effect for that population. The extent of the resulting bias will depend on the unobserved difference in the potential outcomes between the sample of volunteers and the target population. Moreover, all but one of the experiments illustrated in Feinberg (1986) and Soman (2001) do not involve real money transactions but only simulated series of payments. The experiment in Feinberg, that is based on real money transactions, also only manipulates exposure to credit card stimuli, not the payment method itself.

Broader randomized experiments based on non-volunteers are not feasible in economics and social sciences. In these fields, for ethical reasons, no coercive methods of experimental design are usually used to randomly

[2] We refer to the distributions of Y_d induced by the sampling from the superpopulation.

assign individuals to a treatment and to maintain treated and untreated groups. In economics, the individuals themselves decide to take or not to take a treatment, so we say they are "self-selected" into the treatment. The main consequence of self-selection is that the estimate of the average causal effect obtained by comparing a non-experimental cases group with a non-experimental controls group could be biased. To resolve this problem, several methods have been proposed in the scientific literature. In particular, great interest has been dedicated to research in methods[3] that allow correction for the self-selection bias due to observable differences between the treated and untreated groups. In this field of research, the key assumption is what is usually called *unconfoundedness*, which states the independence between potential outcomes and the receipt of treatment conditionally on a set of observable pre-treatment variables $\mathbf{x}$.

To evaluate the impact of the use of debit cards on spending, we apply some causal inference methods, under unconfoundedness, to a dataset from a large Italian survey. According to the author's knowledge, there are no other studies based on data from broad surveys for the same purpose. The relevant causal effect will be that due to the treated (Population Average causal effect of Treatment on the Treated: PATT): the subset of the population using the considered payment instrument and to which the causal effect on spending has to be ascribed. Other causal effects could be of interest, but for other purposes. For example, if the aim was to plan a marketing policy aimed to increase spending by stimulating the use of non-cash payment instruments, then the relevant causal effect should be that on the untreated units.

The paper is organized as follows: section 2 illustrates the dataset and provides some descriptive tables that motivate the subsequent transition analysis in Section 3. The main result will be that, in Italy, the possession of debit cards precedes that of credit cards, and it will be useful in defining the target population. Section 4 provides a critical description of the assumed conditions about the casual relationship and of the adopted statistical methods. The last section illustrates the results.

[3]Theoretical research has been particularly productive in the non-parametric field in the last few years. However, there is again a lack of fully implemented versions for these non-parametric estimators.

2 The data

The analysis is based on data from the survey on "Italian Household Budgets" run by the Bank of Italy (Banca d'Italia) from 1965 and aimed to collect information on several aspects of Italian households' economic and financial behavior. The survey is run every two years except for a delay in one year causing the survey on 1997 household budgets to be shifted to 1998. In this paper, the aim is to evaluate the effects of the use of debit cards on the monthly household spending observed in three of the last few years: 1998, 2000 and 2002. For this purpose, the information provided by the surveys concerning these three years have to be integrated with that from the survey on 1995. Quantification of causal effects achieved both by relying on the concept of potential outcomes and by assuming uncounfoundedness has to be based on a population of units to assign to the treatment and for whom a set of pre-treatment variables has to be observed. Therefore, we cannot evaluate any kind of causal effects on an outcome observed in the first of the three aforementioned years, 1998, without introducing information on a set of previously observed variables. Then, the dataset has to be enlarged to the left so that we also introduce the information from the previous survey, that on 1995 household budgets. Moreover, for use in the transition analysis illustrated in the next section, the dataset obtained by merging the information from the surveys on 1995, 1998, 2000 and 2002 will be enlarged by using data from the surveys on the 1993 and 2004 household budgets.

In our casual analysis, the target population will be defined as the set of households having at least a bank current account. This restriction on the whole population of Italian households is necessary in order to satisfy a base condition of the potential outcomes approach in which every unit has to have a chance of receiving each treatment, and it is not possible to have a debit card without having a bank current account.

For each of the considered surveys, the samples were drawn in two stages, municipalities and households, with a stratification of municipalities by region and size. To take into account the sampling design, all the analyses illustrated hereafter have been conducted by using sampling weights. Datasets from the Bank of Italy's surveys include a sample weight for each household that is inversely proportional to the household probability of inclusion in the sample. The few units with missing data will be deleted from the analyses.

The information on the use of non-cash payment instruments, households Spending, and consumption has been obtained from responses to Section

C and E of the survey questionnaires, respectively. Here, we define the treatment D to be equal to one if the householder or another member of the household possesses at least one debit card and zero otherwise[4]. The two outcomes, Y, on which to evaluate the treatment effect are the monthly average spending of the household on all consumer goods[5] and the monthly average figure for just food consumption[6].

Some relevant demographic and social variables have been chosen to compose the vector of the pre-treatment variables, $\mathbf{X}$. They include the number of earners in the household, the overall household income and wealth, the Italian geographical macroarea where the household lives (classified in three categories), the number of inhabitants of the town where the household lives (four categories), the householder age (five categories), the householder education (six categories[7]), the householder working status (seven categories), and the householder sector (five categories).

The motivation for focusing on debit cards is the large diffusion of this non-cash payment instrument in Italy. Table 2.1 shows, for the surveys of interest, the frequency counts and the relative weighted frequencies of sampled households possessing at least one debit or one credit card (we are considering only the households having at least a bank current account). It is clear that there is a greater diffusion of debit cards, while the difference between the two relative weighted frequencies has maintained almost constant over the years.

[4] Section C contains also a question about the possession of at least one credit card in the household. Credit cards are defined as cards indicating the holder has been granted a line of credit (C.P.S.S., 2001).

[5] The relevant question asks to consider all spending, on both food and non-food consumption, and it excludes only purchases of precious objects, purchases of cars, purchases of household appliances and furniture, maintenance payments, other contributions received from relatives or friends, extraordinary maintenance of your dwelling, rent for the dwelling, mortgage payments, life insurance premiums and contributions to private pension funds.

[6] The relevant question asks to consider the spending on food products and the spending on meals eaten regularly outside the home.

[7] Householder education has been grouped into five categories for data from the survey on 2002.

Table 2.1 Sample frequency counts and weighted relative frequencies of households possessing at least one payment card, per year and type of card.

		1995	1998	2000	2002
Sample size*		5630	5324	5975	5941
Debit card	Frequency count	3289	3643	4185	4223
	Weighted relative frequency	0.579	0.666	0.708	0.730
Credit card	Frequency count	1168	1539	1886	1903
	Weighted relative frequency	0.207	0.278	0.317	0.345
Difference in the weighted relative freq.		0.372	0.387	0.391	0.385

*: samples sizes resulted after dropping households with missing responses:
: 32 households dropped for the 1998 sample, 26 for 2000, and 7 for 2002.

Table 2.2 confirms, for Italy, that consumers who possess cards overspend compared to those who do not. Each cell reports the weighted difference in monthly average spending between households possessing at least a debit (credit) card and households without payment cards. However, this clear evidence is not sufficient to state the existence of causal effects of cards on spending.

Table 2.2 Sample weighted differences in average consumptions (in Euro) between households possessing at least one card and households without payment cards, per year and type of card.

	1995		1998		2000		2002	
	M.A.C.	M.A.F.C.	M.A.C.	M.A.F.C.	M.A.C.	M.A.F.C.	M.A.C.	M.A.F.C.
Debit card	248.53	81.06	290.03	108.78	335.23	97.71	203.78	72.86
Credit card	336.62	96.91	352.42	101.32	392.88	116.90	214.71	69.55

M.A.C.: Monthly Average Consumption; M.A.F.C.: Monthly Average spending on Food Consumption

Table 2.3a and 2.3b show the results of an initial explorative cross-sectional analysis aimed to explain the probability that a household has at least one debit card by means of a set of explanatory variables observed at the same time. We adopt a logit model, where the log of the odds ratio of having at least a debit card at a certain year, $\log(\frac{P(D=1)}{P(D=0)})$, is linearly regressed on the vector of the proposed variables, $\mathbf{x}$, observed at the same year. One category has been included in the intercept for each categorical variable as is usually done in linear models. The contribution of each statistical unit to the likelihood has been weighted using the sampling weights. We observe significant contributions of almost all the explanatory variables for each year. In particular, the probability of observing a household that has at least one debit card increases with the income, the numbers of earners, from the South to North of Italy, the town size, and the householder education, while it decreases with the householder age.

Table 2.3a Weighted MLE for logit models per year.
Dependent variable: logit of the probability an household has at least one debit card.

	1995	1998	2000	2002
Intercept	-1.277 (0.000)	-0.428 (0.177)	-1.153 (0.000)	-1.223 (0.000)
Income	$1.1 \cdot 10^{-5}$ (0.000)	$9.9 \cdot 10^{-9}$ (0.000)	$1.4 \cdot 10^{-5}$ (0.000)	$1.5 \cdot 10^{-5}$ (0.000)
Wealth	.	$-4.9 \cdot 10^{-9}$ (0.944)	$-2.1 \cdot 10^{-7}$ (0.004)	$1.6 \cdot 10^{-7}$ (0.277)
Number of earners:				
1 (intercept)				
2	0.102 (0.164)	0.209 (0.007)	0.251 (0.001)	0.383 (0.000)
3	0.702 (0.000)	0.668 (0.000)	0.470 (0.000)	0.653 (0.000)
4	0.756 (0.000)	0.573 (0.016)	0.000 (0.997)	0.604 (0.018)
more than 4	0.733 (0.084)	0.273 (0.522)	1.172 (0.020)	1.063 (0.177)
Geograph. Area:				
North of italy (interc.)				
Center Italy	-0.604 (0.000)	-0.096 (0.281)	-0.344 (0.000)	-0.350 (0.000)
South of Italy and Islands	-1.006 (0.000)	-0.492 (0.000)	-0.761 (0.000)	-0.793 (0.000)
Town size:				
up to 20,000 (interc.)				
20,000-40,000	0.226 (0.020)	0.095 (0.366)	-0.156 (0.125)	0.089 (0.393)
40,000-500,000	0.473 (0.000)	0.381 (0.000)	0.083 (0.319)	0.227 (0.008)
more than 500,000	0.357 (0.000)	0.556 (0.000)	-0.096 (0.368)	0.254 (0.020)
Householder educat.:				
none (intercept)				
elementary school	0.228 (0.230)	0.271 (0.197)	0.824 (0.000)	0.671 (0.002)
middle school	0.893 (0.000)	0.636 (0.003)	1.155 (0.000)	1.225 (0.000)
profess. secondary sc.	0.758 (0.001)	1.079 (0.000)	1.404 (0.000)	1.333 (0.000)
high school	1.283 (0.000)	1.090 (0.000)	1.806 (0.000)	1.836 (0.000)
university	1.210 (0.000)	1.121 (0.000)	1.812 (0.000)	2.027 (0.000)

Table 2.3b Weighted MLE for logit models per year.
Dependent variable: logit of the probability an household has at least one debit card.

	1995	1998	2000	2002
Householder work status:				
blue-collar or similar (intercept)				
office worker, teacher	0.345 (0.005)	0.016 (0.897)	0.357 (0.023)	0.646 (0.000)
manager, official	0.418 (0.008)	0.556 (0.020)	0.591 (0.012)	0.692 (0.014)
member of the professions, entrepeneur	-0.654 (0.000)	-0.352 (0.035)	-0.113 (0.535)	-0.416 (0.021)
other self-employed	-0.661 (0.000)	-0.806 (0.000)	-0.464 (0.001)	-0.703 (0.000)
pensioner	-1.141 (0.250)	-0.131 (0.331)	.	.
others not employed	-0.198 (0.236)	-0.284 (0.112)	-0.211 (0.123)	0.190 (0.179)
Householder age:				
up to 30 (intercept)				
31-40	0.155 (0.291)	0.003 (0.985)	0.176 (0.337)	0.203 (0.331)
41-50	-0.170 (0.246)	-0.481 (0.006)	-0.110 (0.546)	-0.014 (0.942)
51-65	-0.292 (0.063)	-0.577 (0.002)	-0.725 (0.000)	-0.439 (0.037)
over 65	-1.263 (0.000)	-1.784 (0.000)	-1.817 (0.000)	-1.331 (0.000)
Householder branch of activity:				
agriculture (intercept)				
industry	0.872 (0.000)	0.584 (0.001)	0.814 (0.000)	1.119 (0.000)
public administration	0.576 (0.001)	0.669 (0.000)	0.669 (0.005)	0.567 (0.023)
other sectors	0.714 (0.000)	0.446 (0.015)	0.816 (0.000)	1.009 (0.000)
not employed	0.612 (0.002)	0.364 (0.079)	1.013 (0.000)	0.808 (0.001)

3 A transition analysis

The aim of this section is to ascertain whether, in Italy, households who acquire a credit card already possess at least one debit card. Motivated by descriptive evidence provided by Tables 3.1 to 3.4, the analysis is necessary to correctly define the population for which average causal effects of debit cards on spending will be evaluated. A definition not taking into account a possible significant transit from debit cards holdings to credit cards holdings, would indeed lead to biased estimates of causal effects of interest. In that case, there would indeed be a bad balance of credit card holdings between treated and untreated households (we have defined the treatment as a binary variable indicating if the householder or another member of the household possess at least one debit card). Because of the different probabilities of having credit cards, there would be a prevalence of credit cards holders in the treated group compared to the untreated. Suppose for example, that both types of cards had positive effects on spending. Then, the causal effects of debit cards would be overestimated. Moreover, a transition analysis is supported by the recent findings in Borzekowski and Kiser (2006) and Borzekowski et al. (2006). The authors worked on data from a special survey on payment instruments issued as a part of the Michigan Surveys of Consumers 2004. The results from a set of open-ended questions on the reasons for choosing between debit card and other payment options was used to shed light on substitution patterns. They suggest that a debit card is a substitute for cash, while a credit card is a substitute for a debit card[8]. It is then reasonable that consumers starting from cash, the natural payment instrument, first replace a part of transactions by acquiring a debit card, then they replace a part of debit card transactions by acquiring a credit card.

Table 3.1 contains four two-way cross-sectional tables, one for each of the considered years: 1995, 1998, 2000 and 2002. Each two-way table classifies the households by their possession of at least one debit card and their possession of at least one credit card. We are considering the samples previously used for Table 2.1, and the weighted relative frequencies are reported. We can note the small figures for each cell that considers the households having at least one credit card but no debit cards, 0.022 for 1995, 0.021 for 1998, 0.017 for 2000 and 0.014 for 2002, compared to the cells of households having at least one debit card but no credit cards, 0.395 for 1995, 0.409 for 1998, 0.408 for 2000 and 0.399 for 2002.

Table 3.1 Households' possession of payment cards:
: two-way tables of weighted relative frequencies per year and type of card.

Year		1995		1998		2000		2002	
Credit card		Yes	No	Yes	No	Yes	No	Yes	No
Debit card	Yes	0.185	0.395	0.258	0.409	0.300	0.408	0.331	0.399
	No	0.022	0.397	0.021	0.312	0.017	0.275	0.014	0.255

Tables 3.2, 3.3 and 3.4 provide a simple descriptive extension to longitudinality; each table indeed considers the dataset obtained by merging

[8]Most of the debit card users (48.5 %) claimed to use a debit card instead of cash; most of the debit card non-users (55.1 %) claimed to use a credit card instead of a debit card.

the data on households interviewed at two consecutive surveys (the Bank of Italy survey on household budgets contains indeed a panel component). The three tables present some common evidences. First, we can note how households having at least one credit card at the current observation time, $t = 1998, 2000, 2002$, and at least one debit card at both the current, t, and previous, $t' = t - 2I(t = 2000, 2002) - 3I(t = 1998)$, times[9] show greater relative frequencies compared to the corresponding cells of households not having credit cards at the current time: 0.784 to 0.434 for t=1998, 0.862 to 0.506 for t=2000, 0.884 to 0.566 for t=2002. Secondly, households having at least one credit card at the current time and no debit cards at both the current and previous times show smaller relative frequencies compared to the corresponding cells of households not having credit cards at the current time: 0.058 to 0.351 for t=1998, 0.032 to 0.304 for t=2000, 0.008 to 0.249 for t=2002.

Table 3.2 Possession of debit card at 1995 and 1998: two-way tables of weighted relative frequencies per possession of credit card at 1998.

Credit card at 1998		Yes (564 units)		No (1253 units)	
Debit card at 1998		Yes	No	Yes	No
Debit card at 1995	Yes	0.784	0.135	0.434	0.138
	No	0.023	0.058	0.076	0.351

9 units dropped due to missing responses.

Table 3.3 Possession of debit card at 1998 and 2000: two-way tables of weighted relative frequencies per possession of credit card at 2000.

Credit card at 2000		Yes (934 units)		No (1739 units)	
Debit card at 2000		Yes	No	Yes	No
Debit card at 1998	Yes	0.862	0.089	0.506	0.109
	No	0.017	0.032	0.082	0.304

17 units dropped due to missing responses.

[9] $I(.)$ is an indicator function.

Table 3.4 Possession of debit card at 2000 and 2002: two-way tables of weighted relative frequencies per possession of credit card at 2002.

Credit card at 2002		Yes (903 units)		No (1657 units)	
Debit card 2002		Yes	No	Yes	No
Debit card at 2000	Yes	0.884	0.084	0.566	0.095
	No	0.023	0.008	0.088	0.249

7 units dropped due to missing responses.

These cross-sectional and longitudinal descriptive results provide evidence that households who obtain a credit card already possess at least one debit card and justify further investigation based on an inferential model. For this investigation, we also extend the time-span by considering the 616 households continuously observed from 1993 to 2004 and having at least a bank current account in 1993 and 2004. We adopt a panel version of the first order Markov chain transition model for binary data (Diggle et al., 1994), with a random individual component. The number of households, 616, is indeed too large for supporting the choice of a fixed effects model. The resulting model is

$$log\left(\frac{P(C_{it}=1)}{P(C_{it}=0)}\right) = (1, d_{i,t}, d_{i,t'}, d_{i,t}\cdot d_{i,t'}, \mathbf{x}_{i,t'})\,(\beta, \beta_{b,t}, \beta_{b,t'}, \beta_{b,t\cdot t'}, \boldsymbol{\beta}_{\mathbf{x}})+$$

$$+\, c_{i,t'}\,(1, d_{i,t}, d_{i,t'}, d_{i,t}\cdot d_{i,t'}, \mathbf{x}_{i,t'})\,(\alpha, \alpha_{b,t}, \alpha_{b,t'}, \alpha_{b,t\cdot t'}, \boldsymbol{\alpha}_{\mathbf{x}}) + u_i \qquad (1)$$

where: $t = 1995, 1998, 2000, 2002, 2004$ is the current observation time; $t' = t - 2I(t = 1995, 2000, 2002, 2004) - 3I(t = 1998)$ is the previous observation time; d_{it} ($d_{it'}$) is the observed value of the binary variable, which is equal to one if household i has at least one debit card at t (t') and equal to zero otherwise; C_{it} ($C_{it'}$) is the binary variable that is equal to one if household i has at least one credit card at t (t') and equal to zero otherwise; $\mathbf{x}_{it'}$ is the observed row vector of other explanatory variables observed at the previous time t'; U_i is an individual and time-independent normal component $U_i \sim N(0, \sigma^2)$. With this model, the logit of the probability of having at least one credit card at current time t linearly depends on possession of at least one debit card observed at the current and previous time, the possession of at least one credit card at the previous time, and a set of explanatory variables observed at the previous time.

The parameters in (1) do not have a direct meaning in terms of probabilities; they quantify the contributions to the logit of $P(C_{it} = 1)$. However, this probability can be easily estimated for any specifications of the right hand side variables by simple inverse transformations of (1) and corresponding standard errors by the usual delta method for non-linear models.

We make an initial transition analysis by adopting model (1) and dropping the explanatory variables $\mathbf{x}_{it'}$. Thus, we will have only eight different specifications of the probability $P(C_{it} = 1)$ given the units will be classified by only three binary variables: the holding of debit cards either at t and t', and the holding of credit cards at t'. Tables 3.5 and 3.6 show the results (the analysis has been carried out using the households weights for 2004 in the calculation of MLE). We can note positive estimated values for $\beta_{b,t'}$ and $\beta_{b,t}$, while a negative value is observed for the interaction $\beta_{b,t\cdot t'}$. These results produce the lowest estimated probability of acquiring credit cards (0.020) when $D_{i,t'} = 0$ and $D_{i,t} = 0$, that is, for households having no debit cards either at the previous and current time. To note the negative value of the interaction, $\beta_{b,t\cdot t'}$, leads to a slightly higher probability (0.216) of acquiring at least one credit card $P(C_{it} = 1)$ when $D_{i,t'} = 0$ and $D_{i,t} = 1$, that is, for households acquiring debit and credit cards in the same spell between two surveys, compared with the case $D_{i,t'} = 1$ and $D_{i,t} = 1$ (0.186) of households having debit cards from at least two years. The other estimated parameter values quantifying the interaction with credit card holding at the previous time are coherent in that they resulted in opposite signs. Accordingly, we observe the lowest estimated probability of returning credit cards $(1-0.825)$, for households having debit cards either at the previous and current time, $D_{i,t'} = 1$ and $D_{i,t} = 1$.

The results from the model with explanatory variables[10], given in Table 3.7, present the same signs and order in absolute value of the estimated values for the intercepts, the parameters concerning debit card holdings and interactions. Moreover, while all these parameters are significantly different from zero, the parameters quantifying the contribution of the other explanatory variables are almost all not significantly different from zero. This result tell us that the important variables in explaining the transition to credit cards are those providing information on the holdings of at least one debit card.

Briefly, the analysis shows that there is a significant transition from the acquiring of debit cards to the acquiring of credit cards. Both the acquiring

[10] Again, we calculate weighted MLE using sample weights for 2004.

and the restitution of credit cards are more likely to occur if debit cards have been acquired or returned in the same time-span between two surveys.

Table 3.5 Weighted MLE for model (1) without pre-treatment variables.

Variable	Parameter	Estimated value
Intercept	β	-3.890 (0.000)
Debit card holding at previous time: $d_{i,t'}$	$\beta_{b,t'}$	1.465 (0.154)
Debit card holding at current time: $d_{i,t}$	$\beta_{b,t}$	2.604 (0.004)
Interaction between debit card holdings: $d_{i,t} \cdot d_{i,t'}$	$\beta_{b,t \cdot t'}$	-1.652 (0.152)
Interaction with credit card holding at previous time:		
Intercept	α	4.155 (0.003)
Debit card holding at previous time: $d_{i,t'}$	$\alpha_{b,t'}$	-2.513 (0.119)
Debit card holding at current time: $d_{i,t}$	$\alpha_{b,t}$	-1.618 (0.365)
Interaction between debit card holdings: $d_{i,t} \cdot d_{i,t'}$	$\alpha_{b,t \cdot t'}$	3.006 (0.133)

p-values in parenthesis.

Table 3.6 Estimated $P(C_{it} = 1)$ from the results of Table 3.5.

Debit card holding at previous time, $D_{i,t'}$	Debit card holding at current time, $D_{i,t}$	Credit card holding at previous time, $C_{i,t'}$	$P(C_{it} = 1)$
0	0	0	0.020 (0.183)
1	0	0	0.081 (0.131)
1	1	0	0.186 (0.000)
0	1	0	0.216 (0.011)
1	0	1	0.313 (0.002)
0	0	1	0.565 (0.044)
0	1	1	0.777 (0.000)
1	1	1	0.825 (0.000)

p-values in parenthesis calculated by delta method.

Table 3.7 Weighted MLE for model (1).

Variable		Estimated value		Estimated value
Intercept	β	-7.615 (0.000)	α	7.167 (0.001)
Debit card holding at previous time	$\beta_{b,t'}$	2.367 (0.041)	$\alpha_{b,t'}$	-4.258 (0.021)
Debit card holding at current time	$\beta_{b,t}$	3.787 (0.000)	$\alpha_{b,t}$	-2.785 (0.210)
Interac. between debit card holdings	$\beta_{b,t\cdot t'}$	-3.338 (0.013)	$\alpha_{b,t\cdot t'}$	4.626 (0.057)
$\mathbf{x}_{i,t'}$:	$\boldsymbol{\beta}_{\mathbf{x}}$		$\boldsymbol{\alpha}_{\mathbf{x}}$	
Income		2.8×10^{-5} (0.053)		-2.2×10^{-5} (0.226)
Number of earners:				
1 (intercept)				
2		0.203 (0.658)		0.128 (0.850)
3		1.645 (0.004)		-2.661 (0.002)
4		-0.802 (0.626)		0.312 (0.871)
more than 4		32.60 (1.000)		.
Geographical Area:				
North of Italy (intercept)				
Center Italy		0.937 (0.041)		-1.936 (0.006)
South of Italy and Islands		-0.123 (0.773)		0.003 (0.996)
Town size:				
up to 20,000 inhabitants (intercept):				
20,000-40,000		0.453 (0.440)		0.030 (0.973)
40,000-500,000		0.599 (0.152)		-0.943 (0.151)
more than 500,000		-0.097 (0.894)		0.377 (0.735)
Householder education:				
none		-27.51 (1.000)		.
elementary school (intercept)				
middle school		0.736 (0.272)		0.808 (0.450)
high school		0.923 (0.173)		0.787 (0.443)
university		0.916 (0.288)		0.469 (0.696)
Householder age:				
up to 30		-22.77 (0.000)		24.96 (1.000)
31-40 (intercept)				
41-50		1.074 (0.154)		-1.016 (0.313)
51-65		0.846 (0.261)		-0.591 (0.553)
over 65		0.289 (0.735)		0.737 (0.543)

4 The causal analysis

4.1 The assumptions

The causal analysis will be based on the potential outcomes (or counterfactual) approach, also called the Rubin Causal Model (Holland, 1986). The approach relies on the concept of individual causal effect, that is, on the difference between the observed outcome for the generic unit, i, and the outcome potentially observed if the same unit was not assigned to its actual treatment; $y_{i1} - y_{i0}$, where y_{id} is the potential outcome for i under the binary treatment $d = 0, 1$. This is only a hypothetical comparison that, from a statistical point of view, can be resolved by shifting to the estimation of the average causal effect over a well defined population of units. The goal is estimating the effect of debit cards on the household's monthly spending[11] observed in 1998, 2000, and 2002, and for this purpose, the focus will be on the population of treated units (the households having at least a debit card). This is indeed the subset of the whole population actually using the considered payment instrument and to which the causal effect on spending has to be ascribed; in other words we want to estimate the additional spending due only to the use of debit cards. Adopting the common notation, we will then concentrate on the PATT, the Population Average causal effect of Treatment on the Treated:

$$\text{PATT} \equiv E(Y_1 - Y_0 \mid D = 1).$$

The definition of the target population deserves attention particularly in order to take into account the suggestions from the previous section. The main result in that section was that possession of debit cards significantly precede the acquiring of credit cards. Consequently, the proposed definition of the treatment D, as the holdings of debit cards, implies an unfair distribution of credit cards between treated and untreated when applied to the set of Italian households having at least one bank current account. Households with at least one debit card should be more likely to also have credit cards compared to households without debit cards. Therefore, there is a risk of overestimating the effect of debit cards on spending under the hypothesis that both the payment instruments convey positive effects. To overcome this

[11] As stated in Section 2, the effect of debit cards is evaluated on two different outcomes: the monthly average spending of household on all consumer good and the monthly average figure for just food consumption

hindrance, we restrict the analysis to the subset of households without credit cards at time $t = 1998, 2000, 2002$, and having neither debit nor credit cards at the previous survey's time $t' = t - 2I(t = 2000, 2002) - 3I(t = 1998)$. Therefore, a household for which $D = 1$ is characterized by having acquired their first debit card during the span $t' \to t$ and by never having possessed credit cards during the same span. Table 4.1 reports the sample sizes for each of the three considered spans. There is no information about the moment a treated household has acquired its debit card; we know only it has happened during the two, or three, years of the considered span. In this sense, we are evaluating only the short-time effect of the considered payment instrument.

Table 4.1 Sample sizes for each span.

	Treated	Untreated	Total
1995→98	170	390	560
1998→00	144	531	675
2000→02	151	440	591

A preliminary step in the analysis is to consider the plausibility of the Stable Unit Treatment Value Assumption (SUTVA), Rubin (1980, 1990). The assumption is necessary to the potential outcomes approach, and it states that potential quantities for each unit has to be unrelated to the treatment status of other units. In other words, the received treatment of one individual has to not affect the outcomes of others. SUTVA is not testable, however, it is typically implausible when individuals are permitted to interact with each other like in trading. Here, the individual treatment is the holdings of at least one debit card and it does not seem to affect the potential spending of other individuals. The effects of this treatment can be reasonably thought as acting only within individuals that passively respond to it, consequently SUTVA can be adopted.

From the estimation point of view, we base the analysis on the assumption of uncounfoundedness; a term coined by Rubin (1990) to define a situation where potential outcomes are independent of the treatment conditionally on a set of pre-treatment variables. The current dataset is rich enough to allow a set of pre-treatment variables to be used, and the uncounfoundedness assumption is consequently adopted. Thus, there are some advantages compared to other popular options. We avoid imposing strong conditions on the link between unobservables and osbervables (like in the instrumental variables approach, Angrist et al., 1996), at the same time improving the precision compared to the bounds approach of Manski (1990).

The assumption of uncounfoundedness was introduced first by Rosenbaum and Rubin (1983) to condition the use of propensity scores; it states that treatment has to be independent on the couple of potential outcomes given the vector of pre-treatment variables[12] $\mathbf{X}$:

$$D \perp (Y_1, Y_0) | \, \mathbf{X} = \mathbf{x}.$$

The authors joined the uncounfoundedness with the assumption of overlap in which every unit in the population has to have a chance of receiving each treatment:

$$0 < \Pr(D = 1 | \mathbf{X} = \mathbf{x}) < 1;$$

Altogether, the two conditions form the so-called strong ignorability condition. The two assumptions can be weakened here given we are interested only in the PATT (Heckman et al., 1997) and the outcome distribution for the treated is directly estimable. Then, we can substitute the two assumptions with the uncounfoundedness only for the untreated:

$$D \perp Y_0 | \, \mathbf{X} = \mathbf{x}$$

and with the weak overlap:

$$\Pr(D = 1 | \mathbf{X} = \mathbf{x}) < 1.$$

The uncounfoundedness assumption, or the only for the treated version, is not testable, however, it can be plausible if the objective of the decision maker is distinct from the outcome of interest for the evaluator (Imbens, 2004). In this application it is reasonable to think that spending is not the objective of a debit card non-holder's decision to acquire that payment instrument, even if up to the author knowledge there are no contributions in

[12]Later, Imbens (2000) weakened the uncounfoundedness assumption, by stating the indicator of receiving the treatment is independent only on the pertinent potential outcome given pre-treatment variables $\mathbf{X}$: $I_d \perp Y_d \,|\, \mathbf{X} = \mathbf{x}$, where I_d is the indicator of $D = d$. By imposing only the independence on potential outcome at the treatment of interest, weak unconfoundedness considers the indicator of treatment rather than the treatment level. Imbens (2000) shows the two assumptions are equivalent for the purpose of estimating expected value of the outcome by adjusting for the pre-treatment variables: $E(Y_d|\mathbf{X} = \mathbf{x}) = E(Y_d|I_d = 1, \mathbf{X} = \mathbf{x}) = E(Y_d|D = d, \mathbf{X} = \mathbf{x})$.

the literature that support it in the same decisional context. However, recent closely related works have been done by Jonker (2005), Borzekowski e Kiser (2006) and Zinman (2007)[13]. The former is based on results from a survey[14] aimed to identify price and non-price features of payment instruments that can be used to stimulate the use of electronic payment cards. in this survey, the respondents were also asked about their satisfaction with four payment instruments, including debit cards, with regards to safety, speed, cost and ease of use. These four aspects were selected because supposedly, they largely determine the decision to use a particular payment instrument. More articulated from a decision theory point of view is the work of Borzekowski and Kiser (2006) where a utility function is introduced and estimated. The authors studied the rankings of preferences among a set of payment options that includes debit cards, by adopting a rank-order logit model. The consumers' rankings of the payment instruments' utilities were supposed to depend (conditionally on the consumers' individual characteristics) on three product attributes: transaction time, liquidity, and whether the payment type is electronic. Zinman (2007) proposes a consumer's payment choice model where the relevant variables are acceptance of payment instruments, security, time costs, portability, and transaction costs. These recent papers are dedicated to explaining the choice of using debit cards at the point of sale, and this is different from the situation we are examining here, which is the decision of a non-debit card holder to acquire that payment instrument. However, their proposals lie in closely related situations and, together with considerations of reasonableness, can support the assumption that spending is not an objective of the decision maker.

4.2 The methods

This section illustrates the proposed estimators of the population average treatment effects for the treated, PATT, under the assumption of uncounfoundedness only for the untreated, and weak overlap. The first one is the linear regression estimator, an early estimator whose adoption for causal inference purposes dates back to Rubin (1977). It relies on least squares estimation of the regression function for the untreated:

[13]Other recent works dedicated to investigate the social and demographic characterictics of debit card users are Carow and Staten (1999), Klee (2005), Borzekowski et al. (2006).

[14]Data from the Public Perception survey on POS Payment instruments 2004; this is part of the De Nederlandsche Bank Household Survey.

$$\mu_{D=0}(\mathbf{x}) = \beta' \mathbf{x},$$

from which the counterfactual outcomes for the generic treated unit i can be simply estimated as $\hat{\mu}_{D=0}(\mathbf{x}_i) = \hat{\beta}' \mathbf{x}_i$. The estimated PATT is calculated as the average difference between the observed outcome y_i and the estimated counterfactual outcome:

$$\hat{\tau}_{reg,PATT} = \frac{1}{\sum_i I(D_i = 1)} \sum_i d_i \left[y_i - \hat{\mu}_{D=0}(\mathbf{x}_i) \right].$$

The simple regression estimator, however, may be very sensitive to differences in the distributions of the pre-treatment variables between the treated and the untreated. Large differences indeed make causal effects estimation rely heavily on extrapolation and consequently on the functional specification. Table 4.2 reports for each span the differences in weighted average pre-treatment variables between treated and untreated, normalized by their standard deviations. An asterisk is marks each figure greater than 0.25 in its absolute value, which is a benchmark just advocated by Imbens and Wooldridge (2007). The authors indeed pointed that a t-statistic is not a reliable indicator to highlight problems about the differences in the distributions of the pre-treatment variables between treated and untreated; large t-values could indeed reflect mainly the sample size. We observe a quite large amount of high normalized differences in Table 4.2; 34 of the 111 total entries are indeed over this benchmark even if none of them is particularly exaggerated, with the largest absolute value being 0.672. The resulting estimates $\hat{\tau}_{reg,PATT}$, which suffer from this unsatisfactory balancing in the pre-treatment variables distributions, are reported anyway in Table 4.3. They have been calculated under the simplest linear specification without interaction and power terms. To note a linear specification without interactions between pre-treatment variables in general could be unreliable even in the case of a sufficient level of overlap in the pre-treatment variables distributions; in this case any pre-treatment variable conveys the same contribution to the PATT irrespective of the level of the other pre-treatment variables.

Table 4.2a Normalized differences in weighted average pre-treatment variables between treated and untreated.

	1995→98	1998→00	2000→02
Income	0.279*	0.025	0.000
Wealth	no obs	-0.096	-0.048
Number of earners:			
1	-0.259*	0.173	-0.060
2	0.154	-0.252*	0.026
3	0.142	0.091	0.125
4	-0.144	0.047	0.098
more than 4	-0.068	0.075	.
Geograph. Area:			
North of Italy	-0.499*	0.179	-0.014
Center Italy	0.090	-0.211	-0.054
South of Italy and Islands	0.471*	-0.010	0.060
Town size:			
up to 20,000	-0.104	0.267*	0.006
20,000-40,000	0.049	-0.152	0.026
40,000-500,000	0.011	-0.011	-0.028
more than 500,000 inhabitants	0.130	-0.266*	-0.022
Householder education:			
none	-0.247	.	-0.257*
elementary school	-0.384*	0.295*	-0.340*
middle school	0.209	0.043	0.176
professional secondary school	0.104	0.295*	.
high school	0.322*	0.221	0.350*
university	0.066	0.001	0.035

Table 4.2b Normalized differences in weighted average pre-treatment variables between treated and untreated.

	1995→98	1998→00	2000→02
Householder work status:			
blue-collar or similar	0.430*	0.526*	0.125
office worker, teacher	0.413*	0.252*	0.201
manager, official	0.228	-0.026	0.275*
member of the professions, entrepeneur	-0.274*	0.079	- 0.080
other self-employed	0.107	no obs	0.013
pensioner	-0.634*	-0.512*	-0.309*
others not employed	0.055	0.171	0.117
Householder age:			
up to 30	0.242	0.452*	0.190
31-40	0.425*	0.217	0.319*
41-50	0.467*	0.265*	0.123
51-65	-0.093	-0.142	0.143
over 65	-0.672*	-0.373*	-0.497*
Householder branch of activity:			
agriculture	-0.274*	-0.135	-0.054
industry	0.049	0.102	0.224
public administration	0.336*	-0.085	0.046
other sectors	0.059	-0.078	0.174
not employed	-0.107	0.148	-0.280*

A second class of estimators are those based on the propensity score, a popular causal inference tool proposed for the binary treatment case by Rosenbaum and Rubin (1983) in order to eliminate the bias due to self-selection under strong ignorability. The propensity score is defined as the probability of assignment to a binary treatment, $D = \{0, 1\}$, given a vector of observable pre-treatment variables $\mathbf{x}$:

$$p(\mathbf{x}) \equiv P(D_i = 1 | \mathbf{X} = \mathbf{x}) = E(D_i | \mathbf{X} = \mathbf{x}).$$

A useful property of the propensity score is the fact that it is a balancing score, that is, a function of the pre-treatment variables such that the treatment D is conditionally independent of $\mathbf{X}$:

$$D \perp \mathbf{X} | \, p(\mathbf{x}).$$

As a balancing score, $p(\mathbf{x})$ can be used to group treated and untreated units. Moreover, Rosenbaum and Rubin (1983) show that the propensity score is the coarsest balancing score: the functional providing the fewer groups of comparable units. Another point is that if assignment to treatment is unconfounded given the pretreatment variables, then assignment to treatment is unconfounded given the propensity score, $p(\mathbf{x})$:

$$Y_d \perp D \,|\, p(\mathbf{x});$$

then, conditionally on the observable pre-treatment variables, $\mathbf{X} = \mathbf{x}$, or on the propensity score, $p(\mathbf{x})$, there is no systematic pre-treatment difference between the treated and untreated units. The result allows us to identify the PATT, as:

$$\text{PATT} \equiv E(Y_1 - Y_0 \,|\, D = 1) = E_{p(\mathbf{X})}[E(Y_1 - Y_0 \,|\, D = 1,\, p(\mathbf{x}))] =$$

$$E_{p(\mathbf{X})}[E(Y_1 | \, D = 1,\, p(\mathbf{x})) - E(Y_0 \,|\, D = 0,\, p(\mathbf{x})) \,|\, D = 1].$$

The intuition for the propensity score is that instead of conditioning on the vector $\mathbf{X} = \mathbf{x}$ (that is to find observations with the same values of the selected pre-treatment variables), we can condition on the one-dimensional propensity score $p(\mathbf{x})$, because the observations with the same value of $p(\mathbf{x})$ have the same distribution of $\mathbf{X}$ (Dehejia and Wahba, 1999).

There are a variety of estimation methods based on the propensity score. They allow us to estimate average treatment effects by comparing the outcomes of units that are in different treatment groups but with similar values of the propensity score. A basic distinction has to be made depending on whether the propensity score is known or unknown. When the propensity score is known, the advantages are a reduction in the pre-treatment variables adjustment to a one-dimensional analysis and provision of natural weighting schemes that yield unbiased estimates of treatment effects. The estimators based on adjusting on the known propensity score are not as efficient as those based on adjusting on the entire vector of pre-treatment variables (Hahn, 1998). However, given they do not rely on high-dimensional regressions, their proprieties for finite samples would be attractive (Imbens, 2004). Predominant in the economic and social sciences is the case where the propensity score is unknown; here the advantages of average causal effects estimators are less evident. The efficiency bound for PATT estimators illustrated in Hahn (1998) can be achieved by conditioning on the non-parametric Series Logit Estimates of the propensity scores (Hirano et al., 2003). However, according to the author's knowledge, computational proprieties of these efficient estimators are an open question and software to implement them are not available at the moment. More common methods such as those based on regression, blocking, or matching on the estimated propensity score have not yet established their asymptotic proprieties when the propensity score in not known. Moreover, there is not a clear reduction in the dimensionality compared to the regression estimators; the high-dimensional regression of the outcome on the pre-treatment variables is replaced by the equally high-dimensional procedure to estimate the propensity score. Some evidence of the good performance of methods based on parametric estimation of the propensity score are provided by Dehejia and Wahba (1999, 2002). However, the authors base their works on empirical comparisons between the estimated effects from experimental treatment groups and those obtained when replacing the untreated units with other groups from specific datasets from non-experimental sources, without any formal results about the proprieties of the estimators.

The propensity score remains however the basic tool in assessing the overlap condition. Thus, Figures 1 to 6 illustrate the histograms of estimated propensity scores for treated and untreated for each span, where estimates have been calculated adopting a logistic model with the simplest linear specification for the pre-treatment variables without interaction and power terms. We observe a satisfactory overlap for the span 1995–>1998, while there is an absence of untreated units at high value of the propensity score for the other two spans. Particular attention has to be paid to outlying treated units given they will not have comparable untreated units and consequently, an increase in the variance of the PATT estimates has to be expected. For the purpose of improving the overlap, treated units with an estimated propensity score greater than 0.77 for the span 1998–>2000 (6 units) and greater than 0.66 for the span 2000–>2002 (7 units) have been discarded.

Estimated PATT from three different methods based on the propensity score are illustrated in Tables 4.3a and 4.3b. We are adopting the regression, blocking, and matching on the estimated propensity scores. Each method relies on predicting the counterfactual outcome for each treated unit then estimating the PATT by averaging over the resulted estimated individual causal effects. In the first method, counterfactual outcome for the treated are obtained by predictions on the estimated regression function (OLS here) of the outcome on the previously estimated propensity score for the untreated, $\hat{\mu}_{D=0}(\mathbf{x}) = \hat{\alpha}_{OLS} + \hat{\beta}_{OLS}\hat{p}(\mathbf{x})$. The blocking method consists of dividing the range of variation of the propensity score in intervals such that within each interval, treated and untreated units have the same average propensity score. Then, within each interval the difference between the average outcomes of the treated and of the controls is computed, and finally the PATT is obtained by a weighted average of the PATT of each interval. The matching method consists of taking each treated unit and then searching for the untreated unit with the closest propensity score. The PATT is obtained by averaging the differences between the outcome of each treated unit and the outcome of the closest untreated. Results from the three methods have been obtained by the Becker and Ichino (2002) programs for STATA, where standard errors are computed using bootstrapping with 500 replications.

The last class of estimators are those where, in order to estimate the counterfactual outcomes for the treated, matching between treated and untreated units depends on distances in the space of the pre-treatment variables according to a specific metric. In other words, the generic treated unit, i, is matched to the closest m untreated, whose average outcome will be taken as the estimated counterfactual outcome for i. As before, the estimated PATT will result from a weighted average of estimated individual effects. If the number of matches is fixed and matching is done with replacement, Abadie and Imbens (2006) show the bias is $O(N^{-\frac{1}{k}})$, where k is the number of continuous pre-treatment variables, while the variance of the estimator is $O(N^{-1})$. In our study, $k = 2$ so that asymptotically, the bias will not disappear and the standard confidence interval will not be necessarily valid. Moreover, the matching estimator is not efficient even if $k < 2$, due to the fact that the number of matches, m, do not increase with the sample size. To improve the asymptotic proprieties of matching estimators, Abadie and Imbens (2002) propose a mixed method where for each treated unit, matching is followed by local regression adjustments. The idea is to adjust for the differences in the pre-treatment variables between the treated unit, i, and the set of m closest matched units. If this adjustment is made by non-parametric estimation of the regression function, $\hat{\mu}_{D=0}(\mathbf{x})$, using only the m matched untreated units, the estimated counterfactual outcome for i will then be $\hat{\mu}_{D=0}(\mathbf{x}_i)$; Abadie and Imbens (2002) show the resulting PATT estimator is consistent and asymptotically normal, with its bias dominated by the variance. The authors also note that bootstrapping is not appropriate for variance estimation for matching estimators due to discreteness in the distribution introduced by the computational procedure, and they make their proposal for a variance estimator. Results from the mixed method, matching and regression, are reported in Tables 4.3a and 4.3b. They have been obtained by Abadie et al. (2001) programs for STATA, where bias adjustment is made by local least squares regressions and standard errors are computed using the proposed estimator. The number of matched units has been fixed at 6, and the default metric is adopted[15]; for comparison, we also estimate simple matching PATT estimates with only one matched unit.

5 Results and conclusions

Tables 4.3 a and 4.3b report the calculated effects of debit cards on the two categories of a household's monthly consumption as determined by the proposed estimators. The ratios between each estimated PATT and relevant Weighted Average Outcome for the Treated (WAOT) are also reported. The results have to be considered taking into account advantages and limitations for each method. In this respect, I think the more reliable results are those from the method where matching is mixed with regression (last column of Table 4.3b), since it is the one with the more convincing asymptotic proprieties. The simple linear regression estimator relies heavily on extrapolation if normalized differences in average pre-treatment variables between treated and untreated are not satisfactory such those obtained here (Tables 4.2a and 4.2b). Methods based on the unknown propensity score do not have established asymptotic proprieties despite their large diffusion, while simple matching estimators suffer from bias for finite samples. The mixed method instead combines the consistency of matching estimators with the information about the correlation between the outcome and the pre-treatment variables provided by the regression in order to eliminate the remaining bias.

We have obtained positive values for any estimated PATT, even with different levels of precision. Note that the span 1995–>98 emerges for presenting the smaller differences in estimated PATTs between methods for both of the

[15] Default metric in the Abadie et al. (2001) software is $\|\mathbf{x}\| = (\mathbf{x}'\mathbf{V}\mathbf{x})^{1/2}$, where $\mathbf{V}$ is the diagonal matrix of the inverses of the pre-treatment variables' variances.

categories of consumption; the effect on monthly household's spending on all consumer good ranges from 130.34 to 136.28 Euro (from 12.5% to 13.1% of the monthly spending)[16] and the effect on monthly household's spending on just food consumption ranges from 60.02 to 66.64 Euro (12.9% to 14.4%). Moreover, the estimated PATTs for this span are significant at the level of 95% adopting the usual t-statistic[17]. Differences appear in the cases of the other two more recent spans. For the span 1998–>2000, on one side estimated PATTs on the spending on all consumer goods are significant and quite stable ranging from 122.87 Euro to 155.30 Euro (11.8% to 13.9%)[18]. On the other side, the resulting effects on food consumption are not significant. The span 2000–>02 presents stable and significant results but only for the estimated PATTs on food consumption, from 49.99 Euro to 72.99 Euro (10.2% to 14.9%).

The stable results for the span 1995–>98 could be justified by noting that it is the only span three years long. We pointed out that the exact moment when the debit card is acquired is not known, we know only it has happened during the two, or three, years of the considered span. In this case, one more year can be useful in order for the possession of debit cards to influence the consumers' behavior, thus producing more stable and precise results. The stability in the results between methods for the span 1995–>98 can also support the reliability of the simple linear specification, without interactions and power terms, adopted both in the linear regression and for estimating the unknown propensity scores.

Desirable further directions for research in this field are twofold. From the economic point of view, the interest could be both in applying the same concepts to similar datasets for other countries, and in applying causal methods to suitable datasets that allow for enlarging the spans for the purpose of investigating the time extent of cards' effects. From the methodological point of view, the computational implementation of non-parametric methods could allow for relaxing the distributional assumptions so that the analysis could rely on weaker conditions.

[16] Apart from the result from matching on the propensity score.

[17] Apart from the effect on spending on all consumer goods as resulted from the matching on propensity score method.

[18] Apart that from the linear regression method

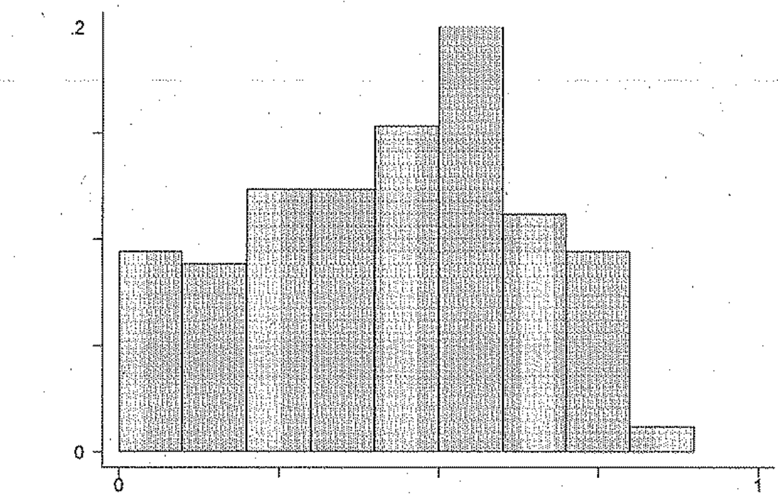

Figure 1: estimated propensity scores for the treated; span: 1995–>98.

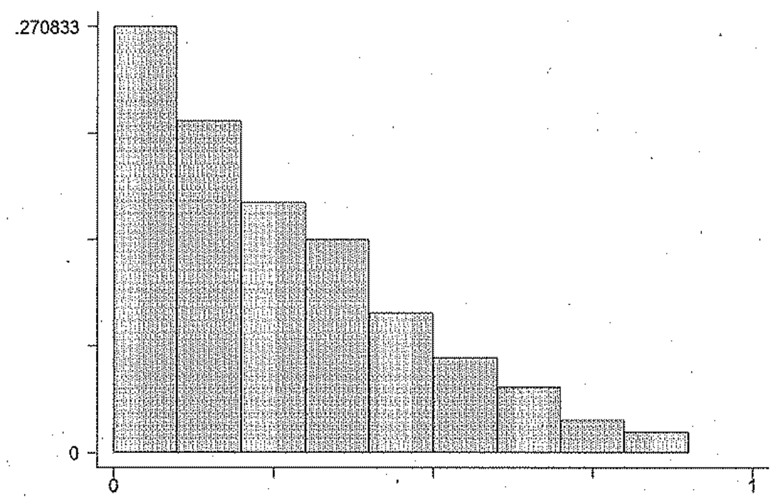

Figure 2: estimated propensity scores for the untreated; span: 1995–>98.

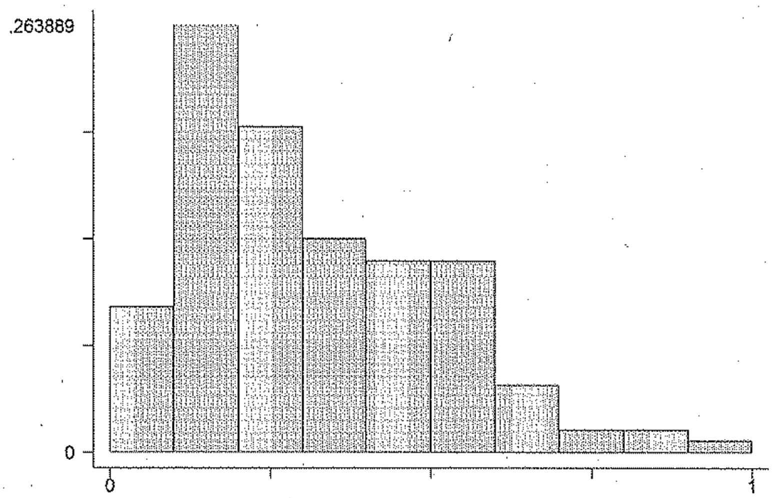

Figure 3: estimated propensity scores for the treated; span: 1998–>00.

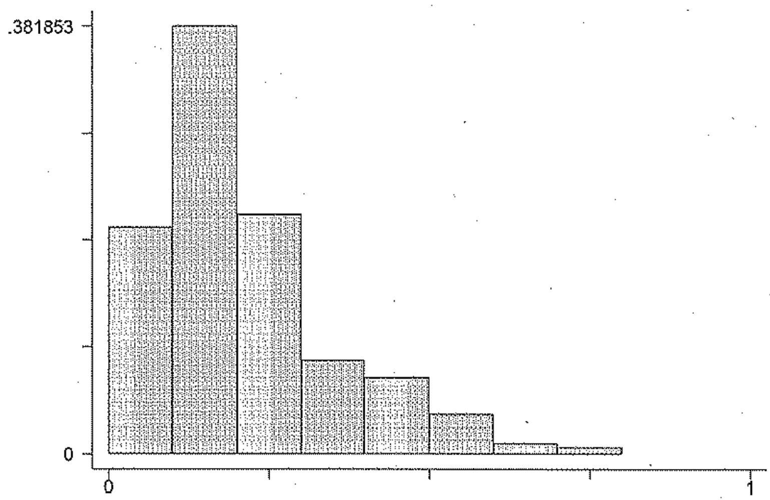

Figure 4: estimated propensity scores for the untreated; span: 1998–>00.

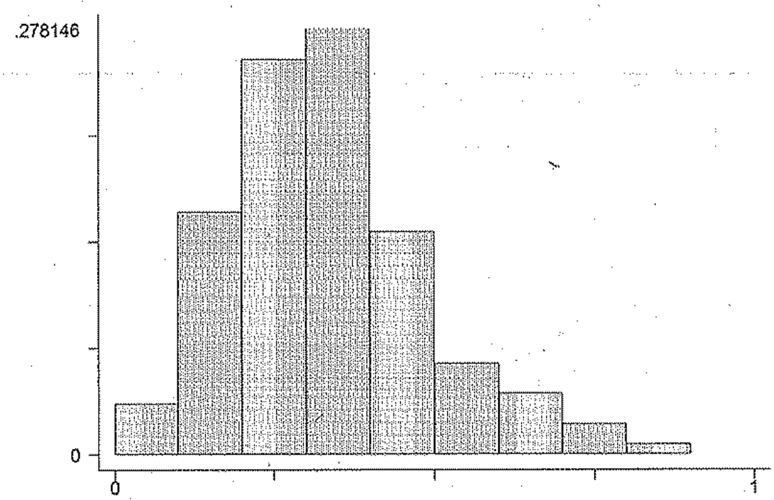

Figure 5: estimated propensity scores for the treated; span: 2000->02.

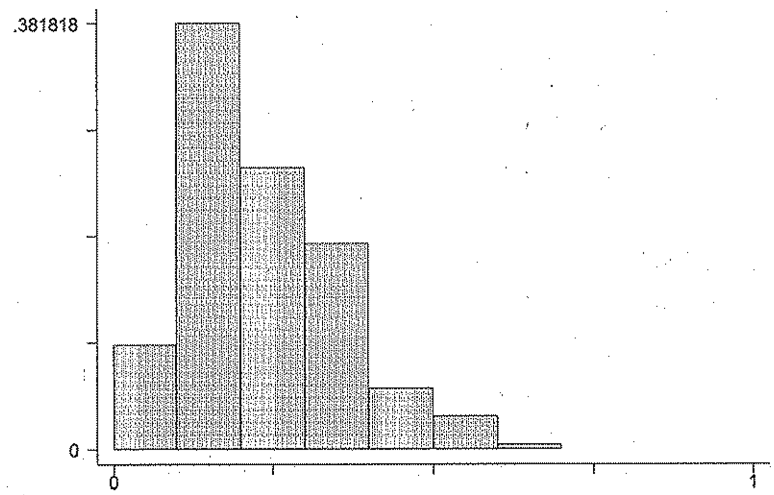

Figure 6: estimated propensity scores for the untreated; span: 2000->02.

Tab. 4.3a Estimated PATT (in Euro), from different methods, on spending on all consumer goods and on just food consumption.

Method		Linear regression		Blocking on the prop. score		Mathcing on the prop. score	
	WAOT	PATT	$\frac{PATT}{WAOT}$	PATT	$\frac{PATT}{WAOT}$	PATT	$\frac{PATT}{WAOT}$
Spending on all consumer goods							
1995→98	1041.7	135.04*	0.129	132.7*	0.127	103.24	0.099
		(44.14)		(45.82)		(60.99)	
1998→00	1042.0	82.43	0.079	131.88*	0.126	155.30*	0.139
		(44.08)		(45.31)		(64.04)	
2000→02	1079.9	40.74	0.004	103.40*	0.096	81.80	0.075
		(47.49)		(48.72)		(61.10)	
Spending on just food consumption							
1995→98	462.3	64.53*	0.139	63.7*	0.137	60.02*	0.129
		(21.97)		(21.56)		(28.64)	
1998→00	476.7	1.86	0.000	40.69	0.085	62.82*	0.131
		(23.28)		(23.50)		(31.96)	
2000→02	488.0	49.99*	0.102	57.81*	0.118	59.14*	0.121
		(22.25)		(23.07)		(29.64)	

WAOT: Weighted Average Outcome for the Treated.

Standard errors in parenthesis.

*: significant effect at the 95% level by the usual t-statistic.

Tab. 4.3b Estimated PATT (in Euro), from different methods, on spending on all consumer goods and on just food consumption.

Method		Regression on the prop. score		Matching (1 match)		Mixed method (6 matches)	
	WAOT	PATT	$\frac{PATT}{WAOT}$	PATT	$\frac{PATT}{WAOT}$	PATT	$\frac{PATT}{WAOT}$
Spending on all consumer goods							
1995→98	1041.7	130.79*	0.125	136.28*	0.131	130.34*	0.125
		(45.36)		(62.41)		(47.32)	
1998→00	1042.0	144.69*	0.138	122.87*	0.118	133.56*	0.128
		(44.24)		(53.56)		(46.19)	
2000→02	1079.9	50.55	0.046	71.07	0.066	96.40	0.089
		(48.59)		(62.49)		(59.82)	
Spending on just food consumption							
1995→98	462.3	66.64*	0.144	63.01*	0.136	62.26*	0.134
		(22.12)		(26.26)		(22.97)	
1998→00	476.7	29.33	0.061	32.50	0.068	26.67	0.055
		(23.33)		(30.11)		(26.08)	
2000→02	488.0	55.13*	0.112	72.99*	0.149	56.55*	0.115
		(22.01)		(25.32)		(24.73)	

WAOT: Weighted Average Outcome for the Treated.
Standard errors in parenthesis.
*: significant effect at the 95% level by the usual t-statistic.

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