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* Views expressed are those of the authors and do not necessarily reflect official positions of De Nederlandsche Bank.

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Abstract

We study the effect on savings of the Deposit Guarantee Scheme (DGS) reform in the Netherlands. We study savings allocation in a DGS environment and we empirically investigate how bank accounts allocations of the Dutch households changed as a response to the reform. Moreover, we highlight the indirect effect on consumption and stock market investments and the role of peers in influencing people's financial decisions. We find evidence of bunching behavior at the insurance limit. Results indicate a general positive impact on saving amounts, with heterogenous effects depending on relative peers' financial sophistication: people with unsophisticated peers tend to save more as a response to the reform, while people with sophisticated peers tend to save more cautiously.

JEL codes: G18, D14

Keywords: Savings, Deposit Insurance, Peer Effects

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I - Introduction

The ultimate goal of any Deposit Guarantee Scheme (DGS hereafter) is to safeguard the confidence of small savers. Thanks to a deposit insurance that reimburses a limited amount of depositors' savings in case of default, bank runs are prevented, making bank defaults less likely. Bank runs, i.e. mass withdrawals caused by solvency concerns, played a prominent role in monetary history, with recurrent episodes since the early 1900 (Diamond, Dybvig (1983)). In fact, the prevention of bank runs is at the root of deposit insurance.
For this reason, there is a long-lasting debate on policy interventions aimed at preventing runs: Friedman and Schwartz (1963) suggest that payment restrictions such as convertibility suspension ensure reasonably small effects of bank panics on banks balance sheet. Chari and Jagannathan (1988) confirm the result by directly modeling bank runs as situations where agents, observing large withdrawals, correctly infer that the bank is likely to fail and they precipitate to make withdrawals. Diamond and Dybvig (1983) show that a tax-financed deposit insurance dominates convertibility suspension and lending-of-last-resort as alternative solutions.
Despite the long-lasting debate, we do not have actual evidence of its effectiveness in preventing bank runs (Iyer, Puri (2012)) and little is known on the effects of deposit insurance from an empirical point of view. Moreover, the academic and institutional debate concerning deposit insurance has not developed substantially after the key contributions of the eighties, due to a substantial fall in the occurrence of bank runs. However, bank-panics suddenly re-emerged as a source of public concern, after that a few episodes occurred in the U.S. at the onset of the recent financial crisis (Gertler, Kiyotaki (2015)). As a result, policy makers intervened by promoting a set of macro-prudential policies to limit financial risk in-taking, and the DGS precisely falls within this new stream of rules.
In this paper we empirically investigate the effect of the Deposit Guarantee Scheme introduction. A unique attempt has been proposed by Iyer et al. (2017) that use bank level data to study its effect on banks' funding in Denmark, where the reform resulted in an insurance limit decrease. They find evidence of increasing withdrawals, especially in non-systemic banks. Here, we focus on the opposite case of the Netherlands, where the reform substantially increased the former deposit insurance, as it was aimed at. According to Claessens (2017) macro-prudential policies, by definition, distort individual behaviors. However, the design of these policies usually starts from generic concerns, rather than from first principles, and the existing literature[1] only focuses on the aggregate effects of these policies, such as on financial vulnerability (credit growth, house prices, bank leverage) and the real economy (output). In this paper, we contribute to the literature by taking a micro perspective, and we investigate the effects of the DGS on household savings. Using household level data, we investigate the micro effects of a macro-prudential policy by looking at how the reform shapes individual incentives. We start from two considerations. First, a Deposit Guarantee Scheme (DGS) is designed to safeguard the confidence of small savers. Our research question is whether enlarging public guarantees during crises sustain depositors' con-

[1] See Claessens (2017) for an extensive review.

fidence and, thus, households savings. In other words, we ask whether insurance limit increases can be used as trust-enforcing devices.

The DGS introduction in the Netherlands, which resulted in an insurance limit increase, provides us a natural experiment to answer this question. However, evaluating the effects of the DGS reform is particularly challenging because (*i*) it does not provide direct monetary transfers and (*ii*) households' response can be driven both by the increasing insurance provided by the DGS and/or by the increasing uncertainty over the banking sector. This reform was in fact introduced during a period of unusual uncertainty for the Dutch banking sector, characterized by banks failures and rescues. We overcome this issue by exploiting a particular feature of the DGS: the partial insurance. The intuition is the following: after the reform all deposits below the DGS 100.000 euro threshold are fully insured, so no reallocation is needed. On the contrary, deposits above the insurance limit are still partially at risk and can be reallocated. If perceived riskiness is high, depositors can reallocate their savings and reduce their uninsured balance. If, instead, the insurance limit increase is an effective policy device, it would directly affect perceived riskiness, and people may be more prone to hold uninsured balance accounts, as they know that credible institutions are providing public guarantees. Our Difference-in-Differences approach enables us to exclude such competing mechanisms and to conclude that the effect is entirely driven by the signaling effect of the DGS reform. We show that an increase in the regulatory limit, by providing additional guarantees, can be an effective trust-enforcing device.

Second, the idea is that when the general public observes large withdrawals, fears of solvency grow resulting in even larger withdrawals (Chari, Jagannathan (1988)), and theoretical models describe withdrawals as strategic decisions, that agents take after observing what the others do. The reason is that during bank runs the expected payoff of a claiming agent depends on its place in line, given the sequential nature of bank refunds. This suggests that (despite a DGS designed to tackle this) peers can affect the decision to save, especially during troubled times. Our second research question is whether peers affect deposit decisions.

To answer this question, a major challenge is the unavailability of information on network links, such as friendships. This information is available in very few surveys since it is prohibitively expensive to collect. Therefore, peer effects need to be proxied and researchers usually face two possibilities. One possibility is to proxy peer effects with neighborhood effects using location information, under the assumption that social interactions are local. However, also location information is rarely available because of privacy restrictions. Another possibility is to rely on the results of network studies and to define a social circle on the basis of a common set of characteristics (such as age and education). In both cases, the econometrician never observes the true links, and the resulting proxy can be very poor. To overcome these issues, we take advantage of the DNB Household Survey (DHS) that contains a set of Aggregated Relational Data (ARD hereafter), i.e. questions of the form: "How many of your acquaintances have *trait k*?" or "Which level of *trait k* do most of your acquaintances have?".

The clear advantage of using ARD is that it provides a correct summary statistic of a given social circle's characteristic. In fact, by letting the respondent self-define his own social circle, we don't need to proxy his family and friendship ties and, thus, their characteristics. In such a way, we solve the issue of measurement error due to

the misspecification of the proxy variable. A unique attempt in studying peer effects in deposit decisions is due to Iyer et al (2012). Their study case is a run faced by a Indian bank in which, to open a deposit account, one needs an introduction from someone who has already an account opened in the same bank. They show that the probability of running is increasing in the fraction of people running in the introducer network, and they show that a deposit insurance partially helps in mitigating runs. More generally, there is a huge empirical evidence showing that peers can influence people's financial decisions such as saving decisions (Duflo, Saez (2012)), stock market participation decisions (Hong et al. (2004), Brown et al. (2005)), borrowing decisions (Haliassos et al. (2014)) and the decision to insure (Cai et al. (2015)). A common explanation is that financial decisions involve complexities that individuals have difficulties in understanding, based on their own education, information and experience (Cai et al. (2015)). Therefore, gathering information is an expensive activity, and peers carry the most informational content. We contribute to the literature aimed at capturing the effect of peer characteristics (exogenous peer effects) in financial decision making. We employ a Triple Difference (TD) estimator that preserves our identification strategy and allows us to compare the post-reform saving decisions of people sharing sophisticated and unsophisticated social circles. We proxy peers' financial sophistication with ARD on peers education and income. It has already been shown that non financial sophisticated households make severe investment mistakes in the stock markets (Calvet et al (2009)). But differently from stocks, that are held by richer and highly educated people (Haliassos, Bertaut (1995), Guiso et al. (2003)), almost everybody hold deposits. We show that financial sophistication makes a difference even for such basic financial instruments and we employ a battery of robustness checks to exclude competing explanations. Results show that while people with unsophisticated acquaintances save more as a response to the reform, people with financially sophisticated peers tend to be more cautious and keep their savings closer to the insurance limit.
The remainder of the paper is organized as follows: in Section II we provide institutional details about the DGS reform and we discuss the framework that motivates our empirical study. Section IV discusses data and descriptive evidence. In Section V we present the empirical analysis on the effect of the reform and on the role of peers. Section VI concludes.

II - Institutional framework

The Deposit Guarantee Scheme

According to the Financial Stability Forum (2001) the deposit insurance is one of the key elements of the financial safety-net, along with prudential regulation and supervision, and a lender of last resort. By the end of 2008 the EU established an harmonized deposit guarantee scheme able to unify and extend the existing national guarantee schemes. Under the new EU legislation the DGS is entirely funded by financial institutions, in order not to weigh on taxpayers, and it reimburses depositors' savings up to 100.000 euro, across all EU countries.

Before this reform, deposits in the Netherlands were covered by a national deposit insurance (*Depositogarantiestelsel*) that reimbursed deposits up to 38.000 euro. Therefore, depositors gained additional protection on their savings. More precisely, up to October 2008, deposit insurance in the Netherlands guaranteed a 100% coverage of the first 20.000 euro and an additional 90% coverage of the next 20.000 euro, with a total maximum insurance coverage up to 38.000 euro. The DGS covers private individuals and small businesses and it is limited to bank saving products only. All insurance products such as life insurances are excluded.
The DGS is a *per person*, *per bank* insurance scheme, meaning that in case of bank failure the 100.000 euro insurance limit has to be applied to the amounts sum of all the relevant assets within the same bank held by the same account holder. The relevant assets for the DGS are all payment and saving bank products like payment accounts, demand and fixed-term deposits as well as all credit balances of credit cards. On the contrary, life-insurance policies have no insurance protection under the DGS rules.

Wealth allocation under the DGS rules

Suppose an agent is endowed with a liquid amount of wealth a_t using bank deposits as saving device. Banks offer homogeneous saving products, that is they all offer a deposit contract that gives at time $t+1$ a fixed rate of return $R = 1 + r$ for each unit of saving deposited at time t. Next, suppose that a DGS is in place, so that deposits are covered by a deposit insurance up to a threshold τ. As a consequence, for a depositors endowed with $a_t \leq \tau$, the evolution of wealth is deterministic and equal to $a_{t+1} = Ra_t$. On the contrary, depositors endowed with a wealth amount higher than the insurance threshold ($a_t > \tau$) are subject to bank failure risk and their (expected) wealth dynamics is as follows:

$$E(a_{t+1}) = R\tau + [\pi\lambda + (1-\pi)R(a_t - \tau)] \tag{1}$$

Where π is the (subjective) bank's default probability and λ is a recovery rate. With probability π the bank fails and the rate of return on the uninsured balance is the recovery rate $\lambda \in (0, 1)$. With probability $(1-\pi)$ the bank doesn't fail and the agent receives the agreed rate of return $R = 1 + r$. Therefore, the expected future level of wealth is made up of a $safe$ part equal to $R\tau$ and a *risky* part that is equal to $[\pi\lambda + (1-\pi)R](a_t - \tau)$.
Next, a particular feature of the DGS is that it is a *per person*, *per bank* deposit insurance, meaning that depositors are insured up to τ in each different bank they are saving in. Therefore, for an agent endowed with $a_t > \tau$ nothing is lost: he can open deposit accounts in different banks and keep his wealth fully insured by saving an amount at most equal to τ in each account opened. However, opening several deposit accounts is not for free: it typically requires a fixed transaction cost (service fees, taxes) to be paid, other than a general implicit cost (inconveniences, such as many passwords, notifications, bank cards renewals etc.) involved by holding multiple deposits. If the former cost increases linearly with the number of accounts being held, we think that the latter increases more than proportionally relative to

the number of accounts. As a result, a depositor endowed with $a_t > \tau$ and n deposit accounts, leaving part of his wealth unsecured in order to pay less transaction costs, will have an expected wealth dynamics equal to:

$$E(a_{t+1}) = Rn\tau + [\pi\lambda + (1-\pi)R(a_t - n\tau)] - c(n) \tag{2}$$

Where $c(n)$ is a cost function, with $c'(n) > 0$, and π is now the subjective probability of default associated to the bank where the depositor has an uninsured balance, i.e. an amount greater than τ[2]. As it can be easily seen, the wealth dynamic has a cost-benefit structure in which the benefit is represented by the insurance coverage that is increasing and linear in n. The cost of holding is also increasing in n, possibly more than proportionally.

As a result we can write the expected wealth dynamics for an agent having n deposit accounts as follows:

$$E(a_{t+1}) = \begin{cases} Ra_t - c(n) & if \;\; a_t \leq n\tau \\ Rn\tau + [\pi\lambda + (1-\pi)R(a_t - n\tau)] - c(n) & if \;\; a_t > n\tau \end{cases} \tag{3}$$

How does (expected) wealth respond to a change in the parameters? Specifically, what is the response to changes in the insurance limit or in perceived riskiness?
It should be easy to distinguish the two effects, as they have opposite sign, but from a pure empirical perspective it is difficult to identify them for many reasons. First of all many parameters, such as the recovery rate or the perceived bank riskiness, are unobservables. Second, these parameters often change simultaneously: during a crisis there may be increasing banks' fragility, but also increasing support from policy makers who extend public guarantees as a response to the crisis itself. Third, there may be a direct relation between the two, in fact an increase in the insurance limit by extending public guarantees sustains depositors confidence, thus directly affecting perceived riskiness[3], and suggesting a relation of the kind $\pi = f(\tau)$.
For these reasons, to isolate the causal effect of a single determinant out of the other is a very difficult task. However, from eq. (2) and (3) we see that an increase in τ or π only affects a specific subset of depositors. In particular, increases in τ affect everybody, as one either gains full insurance coverage or a reduction of the uninsured balance. On the contrary, increases in π only affect those having uninsured balances, as people having full insurance coverage do not need to reallocate deposits, since they are not subject to bank default risk. This last intuition can help us in dealing with this identification issue, that is addressed in the next section.
Eventually, it is important to stress that changes in the insurance limit or in changes in households' perceived riskiness can affect the relative incentive to consume or invest, other than the direct decision to save. For example, a stochastic consumption-saving

[2]In fact, in case of multiple accounts holding, each account is fully insured if and only if $\omega_j a_t \leq \tau$ $\forall j$, where ω_j is the fraction of wealth deposited in the $j-$th account. If all deposit accounts are insured, the overall wealth is fully insured too.

[3]see Diamond Dybvig (1983) on the discussion of self-fulfilling expectations leading to bank runs and the role of policy interventions in preventing runs, or Iyer and Puri (2012) on the discussion of how limit extensions or beliefs on deposit insurance prevent bank runs.

model would predict that, as returns riskiness increases, people cut current consumption (Hansen, Singleton (1983)). In our empirical investigation, we also take into account the indirect effect on investment and consumption of a change in the determinants of saving.

III - Data and Descriptive Analysis

Data

For the empirical investigation that follows, we use data from the DNB Household Survey (DHS). The DHS is a panel data survey representative of the Dutch speaking population and consists of about 2000 households interviews in every year since 1993. The DHS contains very detailed information about Dutch households' assets and liabilities, as well as psychological and economic aspects of financial behavior and information about personal characteristics and living conditions. Eventually, DHS contains information both at the household and the household member level.
A nice feature of the DHS panel survey is that respondents are asked to list every single deposit account, as well as any other saving and investment product, and to indicate the corresponding account balance and the financial institution in which the account is registered. For our empirical application we first obtain information on household financial assets by aggregating all assets held by all household members. Then, since the DGS is a per person - per bank deposit insurance, we focus on saving deposits and we sum up all deposit amounts held by the same account holder in the same bank, and we reshape the data at the deposit level. After this procedure, we obtain an unbalanced panel where our statistical unit consists of the deposit amount held by each household member in each bank, with one observation for every year the household participates to the survey.
Table 1 reports descriptive statistics on saving accounts held by Dutch households. Saving accounts are bank accounts used mainly for saving purposes, in which people usually deposit considerable amounts. These accounts are distinct from checking accounts that are used mainly for paying or receiving the salary, and that usually have very low deposited amounts. For this reason we exclude them from our analysis. The number of saving deposits that Dutch households hold ranges from one to twenty, even though the majority of them hold one or two saving accounts, which correspond to the median values. The table also shows that most deposits are concentred in the same banks, as the average number of banks is lower than the average number of accounts. The average deposit amount ranges from about EUR 16.000 in 2007 to about EUR 24.000 in 2010. The average deposit amount, as well as the maximum, is quite high and is far above the median value. This is not surprising since Dutch people often take out saving mortgages, i.e. mortgages in which instead of paying back the principal, the borrower contribute to a saving deposits.
Figure 1 and 2 show the distribution of deposit amounts before and after the policy change, in a narrow window around the old and the new insurance limit, respectively. The vertical line denotes insurance limit. In Figure 1, the considerable deposit mass

just below EUR 38.000 in panel (a) disappears after the policy change in panel (b): the corresponding distribution displays more mass for values both lower or greater than the limit insurance. Possibly, part of the deposits mass below 38.000 euro reflects strategic allocations (bunching at the insurance limit). This is confirmed in Figure 2, showing the same comparison (the amount distribution before and after the policy change) around the new insurance limit. A huge mass of deposits around EUR 100.000 emerges, with a density that is two or three times higher than the density in nearby bins. Moreover, there is no mass in correspondence of deposit amounts just greater than 100.000 euro. If the evidence was not the clearest in Figure 1, Figure 2 gives strong descriptive evidence of depositors strategically bunching at the threshold, i.e. adjusting their allocations in such a way to have full insurance coverage and to hedge or at least minimize banks' default risk. This descriptive evidence motivates the empirical analysis in the proceeding sections.

IV - Empirical Analysis

Identification strategy

In this section we analyze the households' response to the 2008 DGS reform to see whether insurance limit increases sustain depositors' confidence. As anticipated, the challenge is that their response can be driven both by the increasing insurance provided by the regulation and by the increasing riskiness that characterizes this crisis period. We overcome this issue using a difference in differences (DiD) approach in a deposit level analysis. We base our identification strategy on the framework discussed in Section II and on the descriptive evidence of Section III: while the DGS reform provides more insurance protection to everybody, from eq. (3) a change in perceived riskiness only affects those having uninsured balance accounts. Moreover, from Figure (2) we see that the insurance limit represents a reference point, as some people do bunch at the threshold. Therefore we take the insurance limit as a sharp rule to define the treatment (T) and the control (C) groups of our DiD design. We set as T units all deposits with an amount greater than the insurance limit threshold: $T = 1[a_t > \tau]$. Conversely, the C group consists of deposits with amount equal or below than the same threshold. Again, after the reform all deposits below the 100.000 euro threshold are fully insured, so no reallocation is needed. On the contrary, deposits above the insurance limit are still partially at risk and can be reallocated. Therefore, risk exposure represents the treatment of our quasi-natural experiment: if riskiness increases depositors should withdraw their uninsured balance; if instead riskiness decreases due to the larger guarantees, depositors will be more willing to let uninsured amounts on their saving accounts.

Eventually, we compare T and C outcomes in a sufficiently narrow window of observations around the insurance limit, in the years before and after reform. As a result, the resulting estimate will reflect a local effect. The choice of the observation interval poses a tradeoff: on one hand, by excluding T and C units too far away from the threshold we would identify the local effect properly, but we would loose estimation precision as the number of observations falls. On the other hand, by taking a wide interval we would increase sample size and thus estimation accuracy, but we would include observations that, being too far away from the insurance limit, may not respond to its change. According to this tradeoff we select the $(20.000 - 300.000)$ interval. Also, we select a four year window around the reform, from 2007 to 2010. We consider the following linear model for DiD:

$$y_{i,t} = \alpha + \beta_0\, T_{i,t} + \sum_{t=1}^{T} \beta_t\, 1[t \geq \tau] + \sum_{t=1}^{T} \beta_t^{ATT} \big(T_i \times 1[t \geq \tau]\big) + \epsilon_{i,t} \tag{4}$$

Where $1[A]$ is an indicator function taking value one when event A occurs, τ is the reform year and T is the treatment group identifier. In this linear model for DiD we allow for multiple time periods so that the average treatment effect on the treated (ATT) is captured by the coefficients $\beta_t^{ATT} = E(y_{\tau+t}(1) - y_{\tau+t}(0)|T = 1)\ \forall t$. Given our empirical application, we can conveniently re-write the model as follows:

$$ihs(dep)_{i,j,t} = \alpha + \beta_0\, Above_{i,j,t} + \beta_t' After_t + \beta_t^{ATT\,\prime}\, d_{i,j,t} + \gamma' X_{j,t} + c_i + \epsilon_{i,j,t} \tag{5}$$

The dependent variable is the deposit amount held in bank i by household j at time t and $ihs(-)$ denotes its inverse-hyperbolic sine transformation (Burbidge et al. (1988)). $After_t = [2009, 2010]$ is a vector of post-reform years dummies, $Above_{i,j,t}$ identifies all deposits above the insurance threshold and $d_{i,j,t}$ are the interaction terms between the treatment indicator and each of the post-reform years.
The coefficients β_t' represent the pure before-after effect, as they capture the effect of the additional guarantees given to everybody after the DGS reform, while the ATT is identified by the coefficients $\beta_t^{ATT\,\prime}$ that capture the effect of the reform on perceived riskiness (what we call the signaling effect), which is limited to those still having uninsured balance accounts after the DGS reform. Eventually, $X_{j,t}$ are household level covariates and c_i are unobserved common factors at the deposit level. Note that because of our observation window selection, it is unlikely that a household has more than one eligible account. Therefore, c_i is a common factor at household level. For a few cases, when there is more than one eligible accounts, c_i is a common factor at the deposit level and we control for the correlation among accounts held by the same holder by clustering the residuals at the household level.
The selected covariates are: household income, the outstanding total debt towards the bank where the deposit is registered, a dummy variable indicating whether this year expenses are going to be unusually high and a set of demographics[4]. Moreover, following Iyer and Puri (2012) we account for bank's cross selling with a variable ranging from 1 to 6 indicating the number of different contract types held with the same bank, including deposit and checking accounts, mortgages and personal loans, deposit books and saving certificates[5]. In fact, the higher is the number of contracts with a customer, the higher is the amount of soft information that the bank obtains, and the higher will be the opportunity cost of changing the bank, since it is unlikely

[4]We control for a quadratic in age and for the employment status dummies. All other time invariant controls (bank FE, education, household composition etc.) are captured by the unit fixed effect.

[5]Iyer and Puri (2012) can only use the loan link indicator as a proxy for the depth of bank-client relationship (cross selling), i.e. whether the depositor has a loan with the same bank.

for the customer to find the same economic conditions in other banks. For this reason, they claim that cross selling represents a complementary insurance against the risk of having a run. The next section discusses the estimation results.

The general effect of the reform

We estimate the effect of the DGS on savings amounts using the linear model for DiD in eq. (5). We estimate it using FE to account for all unobservable factors such as financial literacy and risk aversion, that may bias the estimate through their correlation with one or more covariates (for example *Above*) and hence cause inconsistency of the estimates. Results are reported in Table 2. Standard errors are clustered at household level. Specification (a) is without controls, in (b) we control for demographics and in (c) we also add our set of covariates. Table 2 shows a positive effect on saving amounts of the DGS reform: deposits above the threshold significantly increase in 2009, just few months after the reform took place.
The average treatment effect (ATT), as captured by the coefficient of $above \times 2009$, is positive and statistically significant and its magnitude indicates that in 2009 the average deposit amount above the insurance limit increased on average by EUR 11.720,00 relative to the pre-reform trend[6]. Moreover, the magnitude and the significance of the ATT is robust across all three specifications.
Results from Table 2 indicate that insurance limit changes can be effective policy tools to boost depositors' confidence: after the reform, people with deposits above the insurance limit increased their saving amounts (and hence their uninsured balances) despite the high uncertainty characterizing the banking sector in this period. This is because the DGS reform provides explicit as well as implicit guarantees on the willingness of policy makers to avoid bank crises, bank runs and depositors losses. Based on this evidence we conclude that the DGS reform triggered a confidence boost. Figure 3 (a) provides a graphical representation of the result in Table 2, showing the average saving amounts above and below the threshold, in each of the sample years. It shows that the trends in the two groups are parallel in the pre-reform period, while between 2008 and 2009 (as the reform is introduced) the average amount in treated deposits increases sharply relative to the average amount in the control group, which remains stable. Then, the two trends return parallel.

Peer effects in saving decisions

In this section we investigate whether peers affect the decision to save in periods of high uncertainty. From the previous section we know that after the reform those who still had uninsured balance accounts increased, on average, their savings as a result of additional public guarantees. However the descriptive evidence of Figures 1 and 2 suggests something more going on: after the policy change there have been episodes of strategic reallocation, with some depositors bunching at the threshold to maximize

[6]See the Appendix for the estimation of the ATT magnitude.

insurance coverage. This strategic behavior also seems to "come from above", i.e. seems to be due to reallocations and withdrawals on accounts above EUR 100.000.
To further investigate on this, the more straightforward way to proceed is to look more in detail at the T group. Here, we extend our previous analysis and we employ a Triple Differences (TD) estimator that enables us to investigate heterogeneous effects, i.e. within group differences in the response to the DGS reform, while preserving our identification strategy. In fact according to Lee (2005), once the parallel trend assumption holds, the corresponding assumption for TD[7] is likely to hold too. More precisely, within the group of depositors with uninsured balance accounts after the reform, the TD compares the responses to the reform of those sharing financially sophisticated and unsophisticated social circles to investigate peer effects in saving decisions.
We proxy peers' financial sophistication with peers' income and education. Precisely, we define the degree of peers' financial sophistication as the level of income and education that most people in each respondent's social circle have, by using the corresponding set of Aggregated Relational Data (ARD). Then, we build a binary indicator that measures peers' financial sophistication on a relative basis, and we define respondents having sophisticated friends as those having friends that are more educated and richer than they are[8]. The possible channel from peers financial sophistication to individual saving decisions is the following: gathering information on bank fundamentals is a difficult and expensive task, therefore people can take advice from sophisticated peers, who carry the most informational content[9].
Let $G_j = \{0, 1\}$ be the qualified group indicator taking value one for households having sophisticated acquaintances inside their social circle. The TD estimator identifies the following statistic:

$$TD = E\big(y_{\tau+t}(1) - y_{\tau+t}(0)|X, G = 1, T = 1\big) \qquad \forall t$$

The TD estimator identifies the average treatment effect on the subgroup G of the treatment group T. In our specific case, it identifies the impact of the DGS reform on the subgroup of respondents having uninsured balance accounts ($T = 1$) and financially sophisticated friends ($G = 1$).
Results of a linear model for TD are reported in Table 3. A graphical representation of the result is given in Figure 3 (b). Again, all estimates are FE. The coefficient of *Above×2009* is statistically significant, while the coefficient of *soph.peers×Above×2009* is not. Also, the coefficient of *soph.peers×Above×2010* is negative and statistically significant, and compensates the coefficient of *Above×2010*. This means that, relatively to treated individuals without sophisticated peers, the change in deposit amounts after the reform for treated individuals with sophisticated friends is equal to -13.000

[7] It requires the difference between the T and C time effects to be the same for both G subgroups. In formulas, the difference $[E(y_{t+1}(0) - y_t(0)|G = 1, T = 1) - E(y_{t+1}(0) - y_t(0)|G = 1, T = 0)]$ should be equal to $\{E(y_{t+1}(0) - y_t(0)|G = 0, T = 1) - E(y_{t+1}(0) - y_t(0)|G = 0, T = 0)\}$. The identifying assumption of TD is thus weaker than the corresponding assumption for DiD, as it does not require the two terms in $[-]$ and $\{-\}$ to be jointly zero, but it only requires them to be equal.

[8] We also control for own financial sophistication, namely own income and education.

[9] This channel of influence is the most relevant in the literature on peer effects in financial decisions, and it is similar to the one investigated by Hong, Kubik and Stein (2004).

euro in 2009 and -36.000,00 euro in 2010[10]. In words, this result shows that among those having uninsured balance accounts (treated depositors), respondents without sophisticated peers save more after the reform, while respondents with sophisticated peers tend to stay more cautious: they don't increase their uninsured balances in a period of unusual uncertainty and they keep their deposit amounts closer to the insurance limit. This result points toward the evidence of bunching found in the descriptive analysis.

Figure 3 (b) reports a graphical representation of the result. Before the reform respondents with (dashed lines) and without (solid line) sophisticated peers share the same trends, while after the reform the increase in savings is entirely driven by the latter, as treated depositors with sophisticated peers keep following their pre-reform trend. In this period the Dutch banking sector was characterized by unusual uncertainty, with banks rescues (Fortis) and failures (DSB bank, Icesave). In particular, the DSB default[11] caused a severe confidence drop, despite the recent upward trend (DNB Annual report, 2009). Our interpretation is that financially sophisticated individuals may have realized that bank bailouts cannot be given for granted. In fact, being highly educated and with high income, they are likely to be better informed and also to have a higher stake at risk, as they may hold uninsured balance accounts too. As a result, they keep saving more cautiously and don't increase their deposits during risky periods. Also, the result possibly reflects information sharing inside the social circle, as we have regressed an individual outcome (deposits) on a group-level characteristic (income and education), where the latter represents a widely used proxy of financial sophistication and awareness used in the Household Finance literature.

The indirect effect of the DGS reform

Generally speaking, results from previous sections show that the DGS reform, which resulted in a insurance limit increase, had on average a positive (but heterogenous) effect on household savings. In fact, by providing additional guarantees, the DGS reform affects the decision the save. However, the decision to save and the decision to consume or invest are considered as simultaneous. In this section we investigate the indirect effects on consumption and stock market investments of the DGS reform. In other words, we ask whether the increase in saving came at the cost of investment or consumption. To answer the questions, we move from a deposit level analysis to a household level analysis and we set as treated units the households having at least one uninsured deposit account, in order to have a one-to-one correspondence with the deposit level analysis. Conversely, we set as C units all households with no uninsured balances. Again, to be consistent with the previous analysis, we exclude families having all that deposits outside the EUR 20.000-300.000 interval. We repeat

[10] See the Appendix for the estimation of the ATT magnitude.

[11] DSB bank (Dirk Scheringa Beheer) was a relatively small Dutch bank whose main activity was the provision of mortgages. It went bankrupt on October 1, 2009 after that the representative of unsatisfied DSB-customers, Mr. Pieter Lakeman, motivated depositors at the DSB bank to withdraw their money. The Dutch National Bank let DSB fail and activated the new Deposit Guarantee Scheme. This conduct was in sharp contrast with the recent past, as written in the report, "*to discourage unduly risk conduct, the collapse of a bank can and must never be ruled out*".

our TD estimation using consumption and investment as dependent variable. The latter is a continuous variable of household total financial wealth, that excludes all DGS relevant assets as well as business equity. The former is a binary indicator taking value one when the household is planning to do big expenses in durable consumption during the year. Note that since the durable consumption variable is binary, we use a linear probability model in the consumption specification, as nonlinear models such as Logit and Probit give inconsistent estimates of the marginal effect of the interaction term (Ai, Norton (2003)), that in our specification captures the Average Treatment Effect on the Treated (ATT).

Results are reported in Table 4. Columns (a) and (b) refer to financial wealth, while (c) and (d) refer to durable consumption. Table 4 shows a nonsignificant indirect effect on investments in financial wealth (FW) in both groups of treated: a small level of significance of *soph.peers*$\times$*Above*$\times$*2009* emerges, but suddenly vanishes as we control for our set of covariates in specification (c). On the other hand, we do find a significant indirect effect on expenses in durable consumption (DC): the coefficient of *above*$\times$*2009* is negative and significant, and compensates the coefficient of *soph.peers*$\times$ *above*$\times$*2009*. Note that since we use a linear probability model for this specification, the coefficients represent conditional probabilities of making big expenses in durable consumption. In summary, we find that people sharing a mostly unsophisticated social circle save more after the DGS reform, and that the increase in saving amounts above the insurance limit came at the cost of consumption. Durable consumption is in fact easier to postpone than consumption for primary goods. This result possibly reflect precautionary motives: in times of uncertainty, they cut consumption and increase savings. Conversely, the increase in savings cannot be explained with portfolio reallocations from risky to (relatively) safe assets. People with sophisticated peers, instead, keep saving more cautiously after the DGS reform and, correspondingly, their consumption and stock investment paths keep following the pre-reform trends.

Internal validity and robustness

Our identification strategy relies on three main assumptions. First, we employ Difference-in-differences and Triple-differences to identify the treatment effect on the treated under the assumption that, in absence of the treatment, the T and C groups would follow parallel trends. Second, in our design we assign deposits to treatment and control group on the basis of risk exposure, i.e. whether depositors have uninsured balances (deposits above EUR 100.000) after the reform. Third, we claim that the effect is local and the results in Table 2 are estimated using a specific window of observations around the new insurance limit of EUR 100.000.

To test the validity of the common trend assumption, we exploit the panel feature of our data and we use the four years period prior to the reform to repeat specification (c) of Table 2. Correspondingly, we pretend the reform year to be the second, 2006. Results are reported in Figure 4: over the period 2005-2008 the T group (red solid line) and the C group (blue dashed line) shared the same untreated response, since the two lines are perfectly parallel. This provides further evidence of the plausibility of the common trend assumption and of the validity of our identification strategy.

Second, in our design we assign deposits below EUR 100.000 to the control group, since they are subject to full insurance coverage. Conversely, deposits above EUR 100.000 are assigned to the treatment group, as they are still at risk and can possibly be reallocated. As a consequence, after the reform the dynamics of the latter should reflect perceived risk, other than household specific needs of consumption and saving.
To validate our design, we follow our line of reasoning and we perform the following Placebo test: we set as (placebo) treatment group all deposits *below* 38.000 euro. These deposits are subject to full insurance coverage before and after the DGS reform, so the reform didn't affect them as they have been never exposed to default risk. Then, we repeat the same DiD specification of Table 2, using now as treatment the group that is known not to be affected by the policy (Placebo treatment). Results are reported in Table 5[12]: across all specifications neither the coefficient of *below* $\times$ 2009 nor the coefficient of *below* $\times$ 2010 are statistically significant. A small level of significance emerges in 2010 only, but the effect suddenly vanishes as we control for our set of covariates. Again, this result seems to validate our empirical design.
Eventually, we investigate whether results are driven by a specific selection of the observations window. On the one hand by taking tighter intervals we carefully target the local effect, but on the other hand estimation precision falls because of the loss in the number of observations, especially in the left tail of the deposit distribution. The interval chosen for the baseline regressions was the (20.000-300.000) window. Here we check whether our baseline results of Table 2 change as the observations window changes. The results obtained using specification (c) of Table 2 are reported in Table 6 and show that as we further restrict the observation interval, the ATT remain robust and stable across all different selections.
Taken as a whole, these robustness checks allow us to conclude that the identification strategy seems to be valid since (*i*) the estimates are able to detect the true null hypothesis of no treatment effect when the given treatment is placebo and (*ii*) the estimates are not driven by specific window selections.

V - Concluding Remarks

This paper provides an evaluation of the effects of the DGS introduction in the Netherlands. We focused on both the direct effect on households' savings and on the indirect effects on consumption and stock market investment. The DGS was introduced during a period of unusual uncertainty to sustain depositors' confidence, and resulted in a insurance limit increase that provided additional guarantees to depositors' savings. We observe bunching behavior at the (new) insurance limit, that distinguishes insured deposits from (partially) uninsured ones in the distribution of savings. Using Difference-in-Differences and Triple-Differences we compare insured and unbalance deposit accounts to disentangle the effect of larger public guarantees on perceived riskiness, and we find significant effects of the DGS reform on saving

[12]Since we set all deposits below 38.000 as (placebo) T group, differently from our baseline specifications, we also consider all deposits below 20.000 euro in the estimate, in order to preserve balance across the T and C group. For such a reason, the number of observations is now higher than before.

amounts: results show an average increase of (uninsured) saving amounts by EUR 11.700 relative to the pre-reform trend, suggesting that insurance limit increases are effective policy tools in boosting depositors' confidence. Importantly, results show households' response heterogeneity to the DGS reform: treated depositors in unsophisticated social circles start saving more and that the increase in savings come at the cost of consumption, i.e. they postpone the purchase of durable goods. On the contrary, respondents with sophisticated peers save more cautiously: they don't increase their uninsured balances in risky times for the banking sector (characterized by failures and rescues). The Triple Difference estimate indicates a difference up to EUR -36.500 among the two groups of treated households. This result highlights the role of peers in influencing people's financial decisions even for the simplest financial instruments such as saving deposits. Also, the result contributes to the empirical literature on peer effects in financial decisions, and partially supports theoretical models in which the decision to deposit and withdraw is strategic, and explicitly depends on the decision of others. Eventually, the possible channel from peers' sophistication to individual savings decisions is information sharing within the social circle.

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Tables and Figures

Table 1: Descriptive statistics

	2007	2008	2009	2010
Deposit amount				
mean	16.717,88	16.946,81	18.865,74	24.338,34
median	5.597,00	6.674,00	7.966,50	9.000,00
max	556.586,00	650.759,00	850.000,00	500.000,00
N. of accounts				
mean	1.16	1.24	1.32	1.34
median	1	1	2	2
max	15	20	20	17
N. of banks				
mean	1.15	1.17	1.23	1.25
median	1	1	1	1
max	4	4	4	5

Note: Descriptive statistics at household level. The top Panel the mean, the median and the maximum amount deposited in a saving account by the Dutch households in each year. The middle panel reports the mean, the median and the maximum number of saving accounts that Dutch households hold in each year. The bottom panel reports the mean, the median and the maximum number of different banks in which Dutch households hold saving accounts, for each year.

Table 2: The general effect of the DGS reform

	(1) FE	(2) FE	(3) FE
above	0.489***	0.486***	0.444***
	(0.0936)	(0.0950)	(0.1201)
2009	0.031	-0.032	-0.023
	(0.0354)	(0.0461)	(0.0483)
2010	0.167**	0.085	0.128
	(0.0844)	(0.0979)	(0.0984)
above×2009	0.237*	0.248*	0.311**
	(0.1233)	(0.1264)	(0.1420)
above×2010	0.451	0.455*	0.454
	(0.2991)	(0.2739)	(0.2888)
household income			-0.002**
			(0.0012)
outstanding bank debt			0.001
			(0.0008)
high future expenses			0.041
			(0.0569)
cross selling			0.152
			(0.0992)
controls	NO	YES	YES
R-squared	0.221	0.235	0.261
N obs.	1134	1122	1023
N households	517	509	452
N deposits	836	825	744

Note: The dependent variable is the amount deposited in the saving account i, by household j at time t, in its inverse hyperbolic sine (ihs) transformation. All estimates are Fixed-Effects Difference-in-differences. Standard errors are clustered at household level. The set of covariates include the net annual household income, the outstanding debt towards the same bank where the account is registered, a dummy equal to one when the household head expects high household expenses over the year, and a variable equal to the number of different contract types the household has in the bank. The set of controls include age, its square, and a set of employment status dummies. The symbols *, **, and *** denote ten, five and one percent statistical significance levels, respectively.

Table 3: Peer effects in saving decisions

	(a)	(b)	(c)
above	0.485***	0.485***	0.410***
	(0.1145)	(0.1024)	(0.1371)
2009	0.102*	0.032	0.040
	(0.0612)	(0.0723)	(0.0715)
2010	0.306**	0.228	0.275*
	(0.1279)	(0.1494)	(0.1511)
soph.peers	0.082*	0.061	0.036
	(0.0442)	(0.0465)	(0.0495)
above×2009	0.442**	0.461**	0.550**
	(0.1973)	(0.1890)	(0.2196)
above×2010	1.080***	1.019***	1.070***
	(0.1846)	(0.1932)	(0.2028)
soph.peers×above	-0.074	-0.061	-0.014
	(0.1256)	(0.1198)	(0.1349)
soph.peers×2009	-0.104	-0.094	-0.098
	(0.0758)	(0.0766)	(0.0736)
soph.peers×2010	-0.217	-0.232	-0.246
	(0.1644)	(0.1701)	(0.1674)
soph.peers×above×2009	-0.263	-0.285	-0.327
	(0.2315)	(0.2235)	(0.2348)
soph.peers×above×2010	-0.856***	-0.778***	-0.854***
	(0.2339)	(0.2559)	(0.2551)
controls	NO	YES	YES
R-squared	0.255	0.265	0.292
N obs.	1134	1122	1023
N households	517	509	452
N deposits	836	825	744

Note: The dependent variable is the amount deposited in the saving account i, by household j at time t, in its inverse hyperbolic sine (ihs) transformation. All estimates are Fixed-Effects Triple-differences. Standard errors are clustered at household level. The set of covariates include the net annual household income, the outstanding debt towards the same bank where the account is registered, a dummy equal to one when the household head expects high household expenses over the year, and a variable equal to the number of different contract types the household has in the bank. The set of controls include age, its square, and a set of employment status dummies. The symbols *, **, and *** denote ten, five and one percent statistical significance levels, respectively.

Table 4: The indirect effect on Financial Wealth and Durable Consumption

	(b) FW	(c) FW	(b) DC	(c) DC
Above	-23.977	-24.999	0.261	0.299*
	(39.1677)	(38.7063)	(0.1599)	(0.1588)
2009	-10.228	-8.301	0.100	0.132
	(15.2737)	(15.9815)	(0.1068)	(0.1149)
2010	-17.602	-18.436	0.069	0.076
	(25.7827)	(27.3809)	(0.1975)	(0.2161)
soph.peers	-3.332	-3.061	0.010	0.018
	(5.2716)	(5.9032)	(0.0511)	(0.0522)
Above×2009	-5.333	-6.781	-0.568***	-0.611***
	(28.2076)	(27.5916)	(0.1774)	(0.1833)
Above×2010	8.044	7.595	-0.257	-0.185
	(46.6688)	(47.7471)	(0.2469)	(0.2996)
soph.peers×Above	157.889	156.156	-0.157	-0.199
	(110.0524)	(109.5150)	(0.1989)	(0.1934)
soph.peers×2009	-5.403	-7.645	-0.126	-0.155
	(11.0745)	(11.9307)	(0.1097)	(0.1181)
soph.peers×2010	-8.409	-9.620	0.118	0.133
	(21.7221)	(24.0060)	(0.1928)	(0.2120)
soph.peers×Above×2009	-253.731*	-254.390	0.464**	0.531**
	(153.7923)	(154.5367)	(0.2166)	(0.2221)
soph.peers×Above×2010	-291.327	-287.191	0.127	0.051
	(187.2081)	(193.2369)	(0.2751)	(0.3163)
controls	YES	YES	YES	YES
covariates	NO	YES	NO	YES
R-squared	0.203	0.204	0.050	0.067
N obs.	965	917	911	874
N households	509	484	470	452

Note: In the first two columns, the dependent variable is the household total financial wealth, excluding DGS-relevant items (saving and checking accounts, deposit books, saving certificates) and business equity. In the last two columns, the dependent variable is a binary indicator taking value one if the household plans to do expenses in durable consumption during the year. All estimates are Fixed-Effects Triple-differences at household level. The Table reports the coefficients associated with the ATT of interest. The set of covariates include the net annual household income, the outstanding debt towards the same bank where the account is registered, a dummy equal to one when the household head expects high household expenses over the year, and a variable equal to the number of different contract types the household has in the bank. The set of controls include age, its square, and a set of employment status dummies. The symbols *, **, and *** denote ten, five and one percent statistical significance levels, respectively.

Table 5: Placebo Test

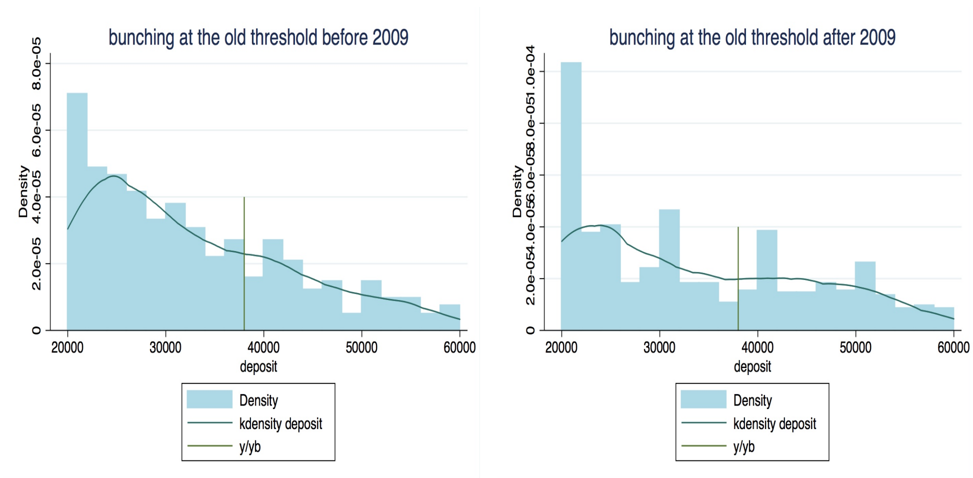
	(1)	(2)	(3)
below	-1.535***	-1.513***	-1.551***
	(0.1532)	(0.1484)	(0.1640)
2009	0.067	0.170	0.262**
	(0.0969)	(0.1081)	(0.1172)
2010	0.194	0.381**	0.495**
	(0.1385)	(0.1891)	(0.1947)
below×2009	0.012	0.007	-0.044
	(0.1166)	(0.1202)	(0.1246)
below×2010	-0.407	-0.459*	-0.241
	(0.2581)	(0.2357)	(0.1999)
controls	NO	YES	YES
covariates	NO	NO	YES
N obs.	4820	4719	4094

Note: The dependent variable is the amount deposited in the saving account i, by household j at time t, in its inverse hyperbolic sine (*ihs*) transformation. All estimates are Fixed-Effects Difference-in-differences. Standard errors are clustered at household level. The Table reports the coefficients associated with the ATT of interest. *below* denotes the Placebo Treatment. The symbols *, **, and *** denote ten, five and one percent statistical significance levels, respectively.

Table 6: Observations window robustness check

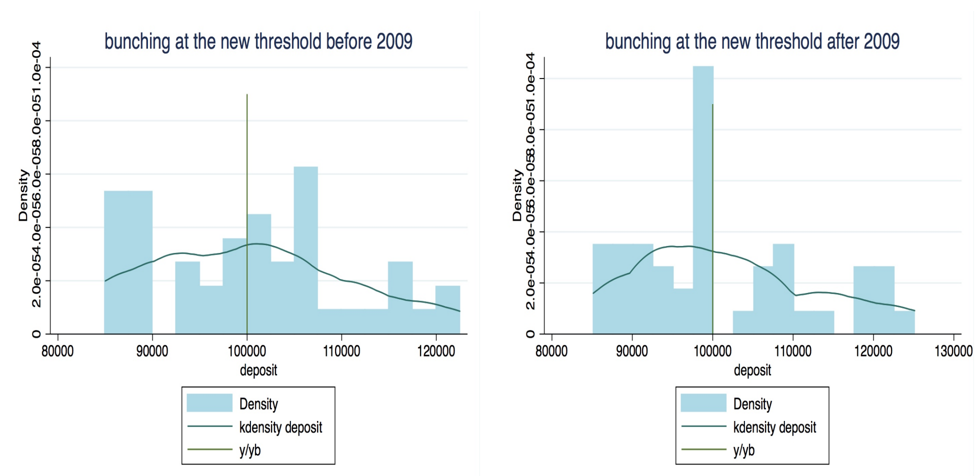
	(1)	(2)	(3)	(4)	(5)
amounts window ('000)	18-350	20-280	20-320	22-280	25-250
above	0.502***	0.468***	0.444***	0.458***	0.453***
	(0.1311)	(0.1255)	(0.1201)	(0.1155)	(0.1169)
2009	-0.088*	-0.032	-0.023	0.046	0.064
	(0.0503)	(0.0481)	(0.0483)	(0.0512)	(0.0476)
2010	0.086	0.110	0.128	0.141	0.147
	(0.0923)	(0.0988)	(0.0984)	(0.1114)	(0.1032)
above×2009	0.467**	0.303**	0.311**	0.268*	0.266*
	(0.1830)	(0.1439)	(0.1420)	(0.1362)	(0.1373)
above×2010	0.592*	0.623*	0.454	0.481*	0.492*
	(0.3252)	(0.3185)	(0.2888)	(0.2891)	(0.2777)
controls	YES	YES	YES	YES	YES
covariates	YES	YES	YES	YES	YES
N obs.	1022	1028	931	817	1141

Note: The dependent variable is the amount deposited in the saving account i, by household j at time t, in its inverse hyperbolic sine (*ihs*) transformation. All estimates are Fixed-Effects Difference-in-differences. Standard errors are clustered at household level. The Table reports the coefficients associated with the ATT of interest. Each column reports a different observation window selection, from the most wide to the most narrow. The symbols *, **, and *** denote ten, five and one percent statistical significance levels, respectively.



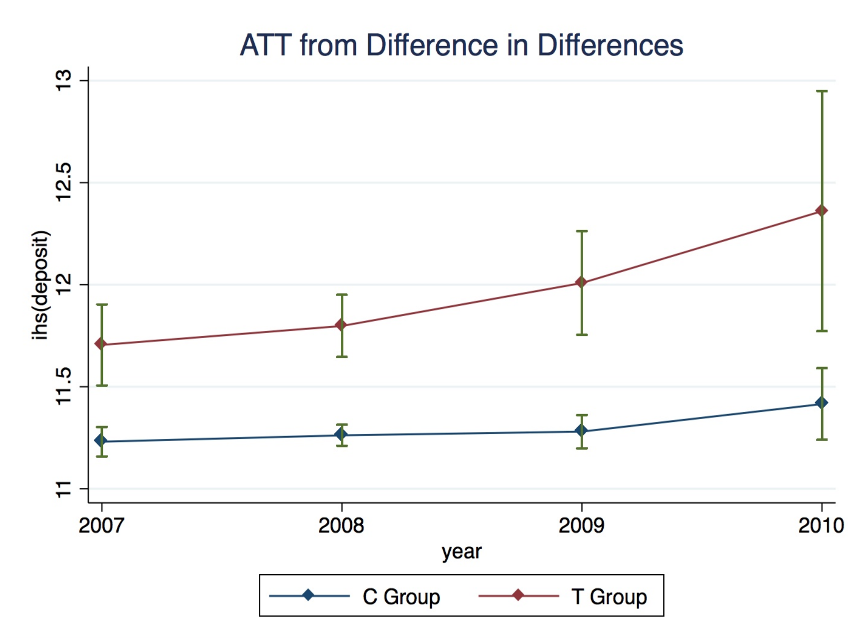
(a) (b)

Figure 1: Empirical distribution of deposits around 38.000 euro

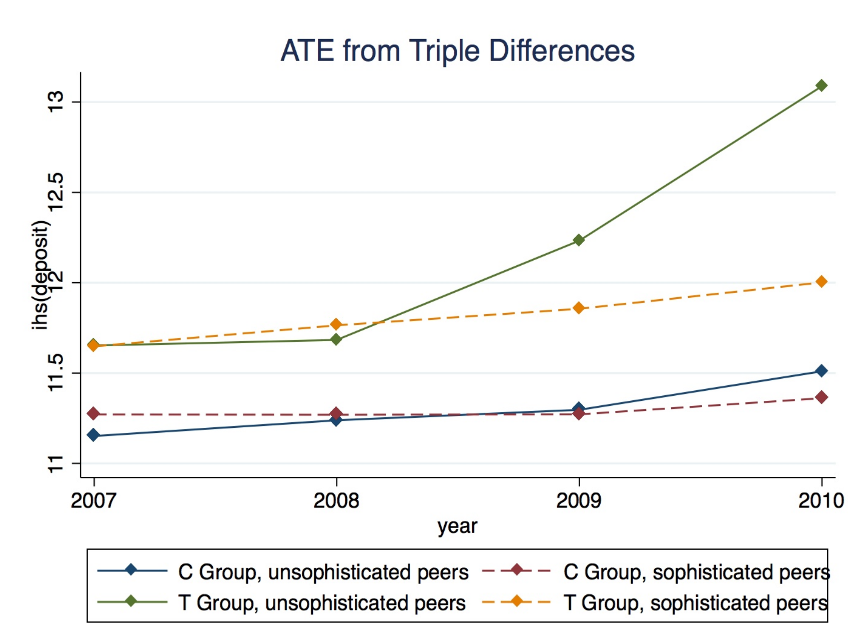


(a) (b)

Figure 2: Empirical distribution of deposits around 100.000 euro



(a)



(b)

Figure 3: Deposit amounts, treatment status and qualified subgroups

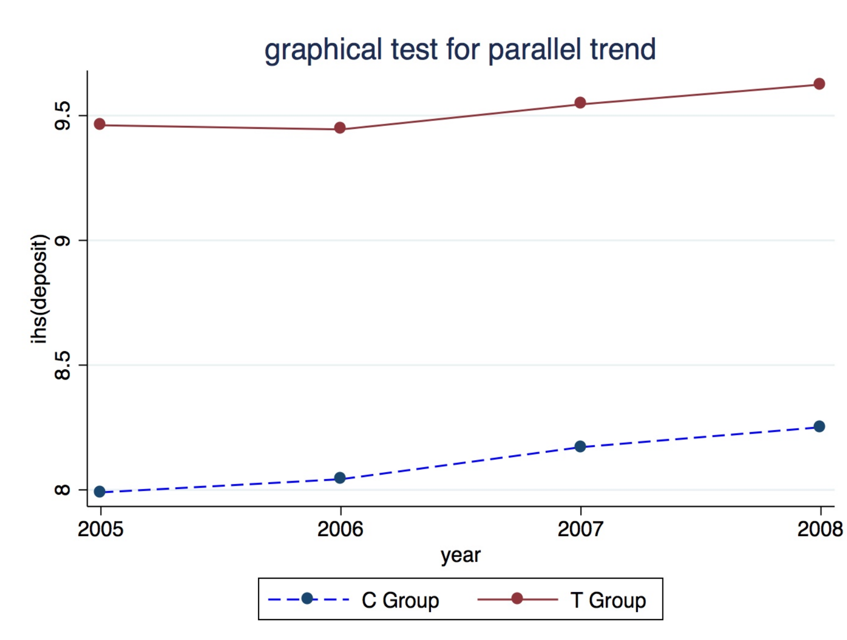


Figure 4: Parallel trend graphical test

Appendix: ATT magnitudes

Table 7: ATT from DiD

	2008	2009	2010	δ_1	δ_2
above	152.490,4	164.420,0	166.679,4	11.929,6	14.189
				(14.792,6)	(14.186,2)
below	39.552,1	39.762,6	42.194,5	210,5	2.642,4
				(1.701,9)	(1.697,1)
			(ATT)	11.720***	11.547***
				(930,1)	(802,5)

Note: The Table reports estimated magnitudes of the Average Treatment Effects on the Treated (ATT). The top panel reports conditional group means, and δ_1, δ_2 represent the 2009 and 2010 within group changes with respect to the pre-reform year. The bottom panel reports the estimated ATT. The symbols *, ** and *** denote conventional significance levels.

Table 8: ATT from TD

$G = 1$	2008	2009	2010	δ_1	δ_2
above	173.353,5	175.752,7	176.428,4	2.390,2	3.074,9
				(19.457,4)	(20.453,6)
below	39.711,0	38.896,8	42.823,0	-814,2	3.112,0
				(2.145,3)	(2.186,0)
			DiD_1	3.429**	-38,9
				(1.523,6)	(1.448,0)
$G = 0$	2008	2009	2010	δ_1	δ_2
above	119.705,7	138.921,3	159.044,6	19.216	39.339
				(12.949,1)	(16.776,5)
below	39.232,6	41.763,0	42.157,0	2.530,4	2.924,4
				(2.805,7)	(2.671,2)
			DiD_2	16.685***	36.414***
				(1.549,3)	(1.568,2)
			(ATT)	-13.256***	-36.452,9***
				(650,3)	(471,0)

Note: The Table reports estimated magnitudes of the Average Treatment Effects on the Treated (ATT). The top panel reports conditional group means, δ_1, δ_2 represent the 2009 and 2010 within group changes with respect to the pre-reform year and DiD_1, DiD_2 represent the 2009 and 2010 within group DiD. The bottom panel reports the estimated ATT. The symbols *, ** and *** denote conventional significance levels.

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