

# The impact of interest-rate subsidies on household long-term debt: Evidence from a large program.\*

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## Abstract

The response of household borrowing to changes in the interest rate is a crucial parameter to understand whether or not monetary policy can promote household investment in housing, and what are the determinants of the consumption durables. We provide an estimate of this response by exploiting a reform in a large program in Portugal: “Credito Bonificado” (CB). The CB program was launched in 1986 and subsidized the interest rate payments associated to mortgage loans to eligible households in the lower part of the income distribution. A reform in the program in 1998 introduced a cap in the value of the house that could be bought using the CB program. Households not eligible for the program were not affected by the reform, and then provide a control group to evaluate the effects of the reform. We use this reform as a quasi-natural experiment to evaluate the effects of mortgage interest rate subsidies on household long-term debt. Our central estimates suggest that an increase in the interest rate of 2 points reduced the probability of signing a new loan by 4 percent.

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# 1 Introduction

The impact of mortgage interest rate subsidies on household long-term debt is an important parameter to assess the effectiveness of many public policies. The interest rate affects the decision of consumers to invest in housing wealth, shaping the effectiveness of monetary policy. Mortgage interest rate subsidies are also a common tool used by the Government to subsidize home ownership (see Poterba, 2001). While there is a substantial literature on the effects of taxation and interest rate subsidies on household saving, much less is known about the impact of interest rate subsidies on household debt.<sup>1</sup> This paper estimates the impact of mortgage interest rate subsidies on household long-term debt using a reform in a large program in Portugal as a quasi-natural experiment.

It is well-known that estimating the response of household debt to interest rates is not a trivial exercise. Identifying the effect of the aggregate-level variation in interest rate on total debt can be misleading, because the temporal variation in interest rates can be confounded with the effects of any other variables trending over time. Also variation across households in interest rates is potentially correlated with other variables that also affect the propensity to hold debt, like income. Previous researchers have identified the effect of tax subsidies on total debt by examining the variation induced by tax reforms. Maki (2000) shows that a reform in the US tax code that eliminated the tax deduction on interest payments associated to consumer debt increased the demand for mortgage debt. In Europe, Jappelli and Pistaferri (2002) provide evidence about the responsiveness of household mortgage debt to tax incentives using a reform in the Italian tax system in 1993. They find that the removal of tax incentives for taxpayers in the upper part of the income distribution and the increase of tax incentives to lower income household did not affect the relative probability of acquiring mortgage loans. Jappelli and Pistaferri (2002) argue that there are informational problems in the Italian credit

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<sup>1</sup>Engelhardt (1999) provides evidence that tax-sheltered accounts toward the purchase of a house in Canada increased household saving. There is also an extensive literature on the effects of tax incentives on some forms of savings. For example, products like 401(k) or IRA Poterba, Venti and Wise (1996) or Engen and Gale (2000). Bernheim (2002) provides a survey of the literature of the impact of taxation on savings.

market, and that households were not aware of the tax advantages associated to acquiring mortgage debt.

This paper identifies the effect of mortgage interest rate subsidies on total household debt exploiting a reform of a large program in Portugal called “Credito Bonificado” (CB). The CB program was launched in 1986 as a large subsidy to low income households who wanted to purchase a house financed by a mortgage loan. In the last quarter of 1998 a national-level cap was established on the value of the house that could be bought using the program, so that eligible households willing to buy a house whose price exceeded the cap did not qualify for any subsidy at all. We provide simple calculations showing that the present value of the stream of payments due by an eligible household could increase by 26% with respect to the pre-reform period. Households in the upper quartile of the income distribution were not eligible for the program and, consequently, were not affected by that change in the regulation. We further exploit the fact that the nature of the reform affected specially individuals living in areas in which the price of real state was expensive prior to the reform. We exploit this asymmetry in the effects of the legal change as a quasi-natural experiment to analyze the impact of interest-rate subsidies on the probability of acquiring long-term debt and on the amount of long-term debt borrowed.

In our opinion, there are four main reasons that make the analysis in the present work an interesting setup to analyze the impact of the interest rate on household long-term debt. The first reason is that interest rate subsidies that operate through the income tax code are likely to be ineffective because they benefit high-income households, who are likely to purchase a house even in the absence of the subsidy.<sup>2</sup> Also, the implicit subsidy in the tax code operates once the payment has been made. The CB program provided two unique features: is was targeted to households in the lower part of the income distribution, and it provided direct subsidies, as opposed to reimbursements after the payment . Second, tax incentives may not be effective because households lack the relevant information about financial markets, or the

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<sup>2</sup>See Glaeser and Shapiro, 2003.

tax code (see Jappelli and Pistaferri (2002a, 2002b)). The CB program was well-known in Portugal. For example, in 1998, two out of three new mortgages signed were subsidized by the CB program.<sup>3</sup> Third, most of the work on the impact of tax incentives on household debt is based on household surveys, which are subject to reporting error. In this work, we have access to administrative records of household debt, that we consider more reliable than survey data. We use a unique sample obtained by merging two surveys: the “Inquerito ao Emprego”, a rotating panel of households with information on the employment status and income of all members of the household, and the “Central de Risco,” the Portuguese Census of all the debt that individuals have with financial institutions. We match the information from both surveys using common variables across the two samples: date of birth of the individual, the NUTS-III region of residence and gender. Finally, having been able to match the administrative records to a large survey on employment, we have an unusually large sample to test the effects of the reform.

Our central estimates suggest that an increase in the interest rate of 2 points reduced the probability of signing a new loan by 4 percent. The estimate is robust to functional form specifications, and we find no evidence that it reflects pre-reform trends in borrowing behavior. We also find that the access to borrowing markets among young adults co-residing with their parents dropped more after the 1998 reform than the access among adults who had already established their own household. These findings contrast to recent findings in the literature on the effect of tax incentives on access to the debt market.

This draft is organized as follows. In Section 1, we give an overview of the industry in Portugal. In Section 2, we describe the Credito Bonificado program and provide a simple analytical framework to understand the effects of the reform. In Section ??, we describe the data. In Section 4, we spell the empirical methodology. Section 5 provides the preliminary empirical estimates. Section 6 concludes.

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<sup>3</sup>Using data from the Inquerito ao Patrimonio, a survey on wealth holdings of Portuguese households, we estimate that 50% of households eligible for the program acquired mortgage debt using the CB program.

## 2 The *Credito Bonificado* Program

In 1986, the Portuguese Government enacted the *Credito Bonificado*, a program intended to facilitate the access to home ownership among young and low-income individuals. The *Credito Bonificado* program (CB, from now on) provided various types of interest rate reductions at source to eligible individuals who financed with a mortgage loan either (a) the purchase of the house of residence or (b) the construction or reparations of the house of residence or (c) the purchase of land toward the construction of the house of residence. Only individuals with taxable income below a given threshold, and who were not currently holding any type of mortgage debt were eligible for the program. The amount of the loan could not exceed the total value of the house, and a person purchasing a house financed with a subsidized loan was not allowed to sell it within a period of five years.<sup>4</sup> Finally, the program set a restriction on the ratio of due installment to income of the household, that could not exceed a limit fixed by law.

The CB program subsidized a proportion of the monthly interest rate payments corresponding to a mortgage loan. That amount was directly given by the Portuguese Ministry of Finance to the lending institution, so that any individual who got the subsidy had his or her monthly payment effectively reduced from the first installment on. The proportion subsidized was constant for the first two years of the loan, fell at a 1% rate during the subsequent three years of the life of the mortgage loan, and at a 2% rate afterwards, until exhaustion.<sup>5</sup> The proportion subsidized depended negatively on the taxable income of the family, after an adjustment for family size.<sup>6</sup>

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<sup>4</sup>If the house was sold before the time limit, the individual ought to return the amount subsidized plus a premium. Under some circumstances, like moving to another region for work-related circumstances, the Government could waive the obligation to return the subsidy.

<sup>5</sup>In addition to the standard CB program, the government provided an extended subsidy to young individuals, called *Credito Bonificado Jovem* (CB-young, from now on.) This extended subsidy was targeted toward singles less than thirty years old or for couples with the sum of ages less than fifty-five. For the young-household program, the subsidy was constant for the first four years, decreased at a 1% rate during the following two years and after that at a 2% rate until exhaustion.

<sup>6</sup>In Portugal, the tax-paying unit is the family. Couples are required to fill taxes jointly, and individuals

The program offered a 44% subsidy on interest rate payments during the first two years of the loan to households whose taxable income was 3.25 times below the minimum yearly wage (class I.) The corresponding starting subsidy was 32.5% for households with taxable income between 3.25 and 3.50 times the minimum yearly wage (class II.) Households with taxable income between 3.5 and 4.25 times the minimum yearly wage (class III) had a subsidy of 21% of interest rate payments, and those with taxable income between 4.25 and 4.75 the minimum wage (class IV), the initial subsidy was 10% of the interest rate payment.<sup>7</sup> Our computations using a Survey of Portuguese individuals between 18 and 55 years of age suggest that 67% of Portuguese taxable units qualified for the maximum subsidy, 7% for the class II subsidy, 5% for the class III subsidy and 3% for the class IV subsidy.

Table 2 illustrates the savings associated with the CB program, assuming a mortgage loan with twenty-five-year maturity, a constant interest rate of 8%, and a mortgage loan of 48,000 EUR. The first column shows the evolution of the amount of outstanding debt. The second column shows the (constant) stream of payments. The fourth column shows the stream of payment in period-zero euros which, by construction, must add up to the initial loan amount 48,000 euro. Finally, the sixth column shows actual payments due by an individual qualifying for the maximum subsidy (in current euro). After appropriate discounting, an eligible person would pay 76% of the amount paid by another individual with the same mortgage, but not qualifying for the subsidy. The ratios of the present value of the stream of payments paid under the program to the present value of the payments without the program would be 84%, 92% and 97%, for classes II, III and IV, respectively.<sup>8</sup>

The subsidy provided by the CB program could potentially have a significant impact on

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above eighteen years of age living with his or her parents, have the option of either filing taxes with his or her parents, at the head's marginal tax rate or filing individually, if personal income exceeds a threshold. Throughout the paper, we assume that individuals who are above eighteen years of age file their taxes separately, regardless of whether or not they live with their parents. From now on, we will consider three types of "households," defined as tax-paying units: (a) the couple and their children under 18 years of age, (b) dependent individuals above age 18 earning some income (c) independent singles.

<sup>7</sup>Decree-law n. 328-B/86, September 30th, 1986.

<sup>8</sup>In Section 2.1 we provide evidence from aggregate records that the loans financed through the CB program did have similar average interest rates than those of mortgage loans not financed through the program.

household net income. Using the 2000 Wave of the *Inquerito ao Património e Endividamento das Famílias* (IPEF), we have estimated that mortgage monthly payments represented, on average, 21% of total net household income.<sup>9</sup> That means that the subsidy on mortgage loans could represent up to 9.3% of monthly net income at the period of maximum subsidy for class 1 households.

In the last quarter of 1998, the Portuguese Government introduced a change in the program. To be eligible for the subsidy, households who satisfied the income requirements, could not purchase a house above a cap. The particular limit depended on the taxable income and on the family size of eligible individuals, but not on the place of residence.<sup>10</sup> For example, a two-person household with income below 3.25 times the minimum wage could only be subsidized for the purchase of a house whose price was below 63,500 euro (in euros of 1998.) If the value of the house purchased exceeded the cap price by an euro, the individual was no longer eligible for any type of subsidy (aside from the standard income tax deduction, which is very small in Portugal.) The reform was effective in the second quarter of 1999, and from now on, we will refer to this reform as the 1999 reform.

We have used the 2000 IPEF to compute the average values of the houses for the various eligible classes and compare them to the 1999 caps. According to own computations from the IPEF 2000, the average (median) value of a house bought before 1999 by households eligible for the maximum subsidy was 71,028 EUR (62,350 EUR). We have estimated that the limits introduced by the reform were in the 60th percentile of the distribution of the value of houses bought by eligibles before the 1999 reform.

Figure 1 shows the evolution of the number and the value of mortgages created during the

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<sup>9</sup>The IPEF is a survey on wealth holdings of Portuguese households, conducted for the first time in the year 2000.

<sup>10</sup>The limit was established according to the decree-law n. 349/98, November 11th, 1998. For eligibles of class I, the limit is EUR 62,350, EUR 68,585, EUR 81,055 or EUR 87,290 if the family size was 1,2,3 or 4, and greater than 5, respectively. Conditional on family size, households eligible for the class II subsidy had higher limits: EUR 69,832 (1 individual), EUR 76,815 (2 individuals), EUR 90,781 (three or four individuals) and EUR 97,764 (more than five.) The corresponding limits for class III, were: EUR 77,314, EUR 85,045, EUR 100,508 and EUR 108,239 and, finally, for class IV the limits were: EUR 84,796, EUR 93,275, EUR 110,234 and EUR 118,714.

period spanning between 1990 and 2001.<sup>11</sup> Probably due to the steady decrease in interest rates over the decade and to the increase in competition in the commercial banking sector, mortgage loans significantly increased after 1994.<sup>12</sup> Between 1994 and 1998, the aggregate value of mortgage loans increased from 3,000 million euro until 12,000 million euro. In 1994, one out of two new euros of mortgage debt were borrowed under CB loans. In 1998, the proportion had risen to two out of three euros borrowed through CB loans. After 1999 the total amount of mortgage debt and the relative proportion of euros created through the CB loans decreased dramatically. In 2000 only 43 cents out of each euro of new mortgages were borrowed through the program.<sup>13</sup>

The evolution of the number and composition of mortgage loans followed a similar pattern. In 1994 there were granted 84,445 mortgages with an average value of 35,702 EUR. Among these, CB loans accounted to 43,875 contracts with average value of 34,141 EUR per loan. By 1998, the total number of new mortgages was 216,631 (average value of 48,351 EUR per mortgage). In this year the CB program contributed with 130,335 loans, with average value of 51,239 EUR per loan. By the end of 2001, the number of total new loans decreased to 153,134 (average loan of 64,425 EUR) where the CB loans were 67,351 with average value of 63,115 EUR per loan. The pattern described above suggests that the 1999 reform had a substantial effect on the borrowing behavior of households.

## 2.1 Identification from the 1999 reform

This section discusses our use of the 1999 reform in the CB program to identify the effect of an exogenous change in the interest rate on consumer's borrowing behavior. We lay out an

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<sup>11</sup>Source: Portuguese Ministry of Finance (<http://www.dgt.pt>). EXACT PAGE?

<sup>12</sup>Between the end of 1994 to 2001 the 6-month interest rate benchmark (Lisbor) decreased from 11.3% to 2.64% by May 1999. It increased to 3.57% by the end of 2001. NUNO: BEST TO REPORT 5-YEAR INTEREST RATE

<sup>13</sup>According to owen computations from IPEF 2000, the take-up rate of the CB program among eligible households was 51%. However, this number could be biased, as eligibility class is measured in year 2000, when the household is interviewed, but not at the moment of signing the loan. This take-up rate would be comparable to the take-up rate of 401(k) in the US during the mid-eighties (see Engen and Gale, 2000.)

stylized model of consumption of housing services.<sup>14</sup> A consumer who lives for two periods and cares about non-durable consumption today ( $c_1$ ), tomorrow ( $c_2$ ) and about the amount of housing services ( $h$ ), with utility function  $U(c_1, c_2, h) = u_1(c_1) + \beta u_2(c_2) + v(h)$ . We ignore uncertainty about future housing prices, interest rates and income. In the first period, the consumer chooses whether or not to acquire housing services  $h$  for the total cost of  $p$ . The consumer has an outside option yielding an utility level  $\bar{U}$  that involves consuming a given amount of services  $\bar{h}$ . Any investment in housing at a price  $p$  is valued by the market in period 2 as  $p(1+\theta)$ . We further assume that  $\theta < r$ . The consumer can access the credit market, at an interest rate  $r$ , but only to acquire housing services, and with the constraint that the amount borrowed per unit of housing services  $m$  cannot exceed the total price of housing services,  $p$ . The first-period budget constraint is the following

$$c_1 + s + (p - m)h = y_1 \tag{1}$$

That is, first-period consumption  $c_1$  plus first-period savings  $s$  and own expenditure in housing services must equal first-period income  $y_1$ . The second period budget constraint is

$$c_2 + (1 + r)hm = y_2 + p(1 + \theta)h + s(1 + r) \tag{2}$$

We concentrate in the case in which income in the second period  $y_2$  is sufficiently high relative to the first period  $y_1$ , so that the consumer must finance at least a part of the purchase of services through a mortgage. In that setting, acquiring new housing services requires going to the debt market<sup>15</sup> For a consumer who finances the purchase of housing services with a mortgage loan, we can combine both budget constraints to get the following intertemporal budget constraint

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<sup>14</sup>See Attanasio, Goldberg and Kyriazidou (2000) or Henderson and Ioannides (1983).

<sup>15</sup>Also, we only examine the net borrowing of the consumer, and hence ignore  $s$ , the decision to save. An alternative justification to ignore the saving decisions is if consumers faced a higher interest rate for borrowing  $r_b$  than for lending  $r_l$ . It is easy to prove that, in this situation, the consumer either borrows or saves, but not both -see Attanasio et al. (2000).

$$c_1 + \frac{c_2}{1+r} + ph\left(\frac{r-\theta}{1+r}\right) = y_1 + \frac{y_2}{1+r} \quad (3)$$

(4) represents the budget constraint pre- 1999 reform for an eligible individual. The effective interest rate  $r$  is the after-subsidy interest rate. The budget constraint of an individual eligible for class I subsidy is represented in Figure 2A. The full (dotted) line represents the pre- (post-) 1999 reform budget constraint (ignoring wealth effects caused by the change in the interest rate.) Before the 1999 reform, the consumer could trade-off any amount of housing services  $ph$  and life-cycle non-durable consumption at a relative price of  $\frac{r-\theta}{1+r}$ . The 1999 reform introduced a severe discontinuity, as eligible individuals did not qualify for any subsidy at all if the purchase of housing services exceeded the cap by an euro. At the value of the cap, the relative price of housing services became  $\frac{\frac{r}{1-.24}-\theta}{1+\frac{r}{1-.24}}$ , or  $\frac{1.32r-\theta}{1+1.32r}$ .

The parameter of interest is the response of the borrowing behavior to a change in the interest rate from the first euro borrowed. Hence, we discuss three possible responses to the 1999 reform, according to the preferences of individuals, and how these responses relate to the parameter of interest.

**Case 1** *Individuals with low preference for housing services are not affected by the reform.*

This is the case of individuals who at the pre-reform cost of housing services demanded an amount of  $h$  below the cap. If these individuals had experienced an increase in the housing cost from the first unit of housing services, they would have diminished their propensity to borrow. Hence, the observed change in borrowing for these individuals underestimates the response of the probability of borrowing to a change in housing costs.

**Case 2** *For individuals with intermediate preferences for housing services, the change in the probability to borrow after the introduction of a cap underestimates the uncompensated derivative of the probability to borrow with respect to a change in interest rate.*

The preferences of these individuals are such that their demand of housing services at the pre-reform cost of housing exceeds the cap, and their outside option yields lower utility than the utility of consuming at the cap. Hence, these consumers respond to the reform by purchasing housing services at the value of the cap, while if the cost of housing had increased from the first unit of housing services, they would have chosen the outside option. The situation is illustrated in figure 2A. The indifference curve representing the utility of not participating in the housing market ( $\bar{U}$ ) cuts the pre-reform budget constraint of the consumer to the right of the kink. At the pre-reform cost, the consumer is better off consuming housing services than with the outside option. Nevertheless, if the 1999 reform had not introduced a cap, the consumer would have left the housing market, as her  $\bar{U}$  lies to the northeast of an alternative budget constraint with slope  $\frac{1.32r-\theta}{1+1.32r}$  from the first unit of housing services. Also, among consumers who purchase housing services both at the pre- and post- 1999 interest rates, the introduction of a cap may make it optimal for a consumer to reduce the *amount* of housing services to the kink, such that the new demand of housing services is lower than the optimal choice at the post- 1999 reform interest rate in the absence of a cap. Then, for a given price of housing services, the introduction of a cap can cause the observed change in the *amount* borrowed before and after the 1999 reform to be either an underestimate or an overestimate of the uncompensated derivative of the *amount* borrowed to a change in the interest rate.

**Case 3** *For individuals with high levels of preference for housing services, the change in the probability to borrow after the introduction of a cap provides an estimate of the uncompensated derivative of the probability to borrow to a change in interest rate.*

The desired amount of housing services of this set of consumers is above the cap both at the pre- and post- reform cost of housing. Also, they derive higher utility at the post- reform interest rate than at the cap. These consumers will choose to get additional housing services or not depending on whether or not the utility of the outside options is higher or lower than

the utility acquiring services at the post-reform cost of housing. The change in the propensity to borrow of this set of consumers after the reform is the same as if the post-reform cost of housing was  $\frac{1.32r-\theta}{1+1.32r}$  from the first euro.

In summary, holding  $p$  constant, the introduction of a cap is likely to make the comparison of pre- and post- reform participation in the mortgage market an underestimate of the response of the propensity to borrow to the change in the cost of a mortgage from the first unit of housing services, which is the parameter of interest.

The discussion above assumed that  $p$ , the unit price of housing services is constant. Yet, there is substantial variation in the price per unit of housing over Portuguese regions. We will assume that case 1 is specially common in areas with low average prices, as the cap is less likely to affect the behavior of the consumer. If we assume that the reform has no effect on borrowing behavior in low price regions, and that in areas with “high prices,” the three cases above hold, pooling high and low- price regions together makes the observed change in the proportion of individuals with a mortgage a further underestimate of the effect of a change in the interest rate on the probability of borrowing. Hence, we will group consumers who live in areas that were “expensive” or “unexpensive” prior to the 1998 reform. We treat individuals living in low-price areas as less affected by the reform than individuals living in “high-price” areas, and identify the effect of the reform from the differential responses to the 1999 reform between high- and low- price areas. Also given that the evolution of the amount of debt after the reform can be a biased estimate of the derivative of the debt amount to the change in the interest rate, we concentrate on the analysis of the extensive margin: i.e., on the probability of signing a new loan.

### **3 The Data**

Our sample combines survey data including employment status, demographics, income and administrative records of outstanding debt levels for a sample of households that is representative of Portugal. The main source of data is drawn from a quarterly rotating panel called

Inquérito ao Emprego (IE), from 1998 to 2001. The IE collects information on the employment status and occupation of respondents for at most six consecutive quarters. The IE includes information on the educational level of the individual, labor force status, occupation, industry and demographics. This survey would be the Portuguese version of the CPS in the US.

The debt data is taken from the Central de Risco de Crédito database (CRC) collected by the *Banco de Portugal*. This unique dataset is a panel of all Portuguese individuals who hold any debt from financial institutions between 1995 and 2001. As regulated by the central bank, each financial institution allowed to lend credit to consumers, is required to report to the central authority the amount of individual debt and, in the case of short-term debt, credit limits.<sup>16</sup> The information on the CRC sample includes the stock of debt held by a given individual with maturity smaller than a year and the stock of debt with maturity exceeding one year. Hence, for each individual, we do not know whether or not a specific loan is a mortgage or a consumer loan.

The IE and the CRC records were matched by the staff at the *Banco de Portugal* using variables common across the two databases, namely the exact date of birth, the gender of the individual, and the place of residence (county) the individual lived in one year before the interview.

The unit of observation in our analysis is the tax-paying unit, as claimants for the CB program had to present their last tax return to be eligible for the program. The tax-paying unit does not necessarily coincide with the household. In Portugal, married couples must file jointly. Hence, each married couple or each single individual living in their own household and earning income above the minimum yearly wage contribute one observation. The case of dependents above age 18 earning income is slightly more complicated. According to the Portuguese law, these individuals may opt for filing jointly with their parents or for filing by

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<sup>16</sup>The Bank of Portugal centralizes all outstanding individual debt. The CRC database is confidential, but can be used by financial institutions upon certain circumstances. If an individual asks a bank for a loan, the commercial bank is allowed to obtain a report from the Central Bank with the current total outstanding debt of this individual.

themselves, so it is not clear to which tax-paying unit they belong to. Excluding the group of dependents from the analysis is problematic, as that group is likely to benefit the most from the CB program: the transition to home ownership in Portugal does not occur through the rental market, but young adults co-reside with their parents well into their twenties and then purchase the home of residence. Based on these considerations, we consider each dependent individual above 18 co-residing with his or her parents and earning income as a separate tax-paying unit.

We restrict the initial sample to tax-paying units that are between 18 and 55 years of age, are not self-employed and whose reported income exceeds the minimum yearly wage. To determine eligibility, we add the earnings of both spouses in the case of married individuals.<sup>17</sup> Consistent with the discussion above, when computing family size, we exclude individuals in the household who are 18 years or more and report positive income. Each individual contributes at most one observation per year, and we use information from three years: 1998, 2000 and 2001.

The dependent variable takes a value of zero if the individual held zero debt during the four quarters of the survey year, and the change in the amount of outstanding debt changed from zero to more than 5,000 euro during the four quarters of the survey year<sup>18</sup>. We drop individuals who already had debt at the beginning of the survey year, as CB claimants could not have any mortgage.

As discussed in Section 2.1, an important variable in our analysis is whether an individual lives in a “high-price” county prior to the passage of the 1998 reform. The Portuguese National Statistics Agency only started collecting average prices of houses sold in 2001. These prices

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<sup>17</sup>Eligibility for the CB subsidy depends on family income and family size. The Inquerito ao Emprego contains information on monthly net labor income for salaried workers, business income for self-employed, unemployment benefits for unemployed, and pension income for retirees. We only use salaried workers, unemployed or retired individuals, and impute pre-tax labor income from the standard withholding rules in the Portuguese law. We do not have direct information on transfer income nor asset income. Hence, the eligibility measure could be subject to measurement error.

<sup>18</sup>Casual discussions with various mortgage branche managers in Lisbon, from different financial groups, suggest that EUR 5,000 is a lower bound for a mortgage.

may be the consequence of the passing of the reform. To get the distribution of prices by concelho, we deflated prices of houses using an index for prices collected by the *Banco de Portugal* during the period 1998-2001.<sup>19</sup> We restrict the sample to counties in which real estate prices in 1998 are available.

Overall, we have information on 16,792 observations. Table 3 presents the summary statistics of our sample. 83% of the individuals in our sample are eligible for the program and 68.1% can qualify for the maximum subsidy. The table reveals that the probability of having a new mortgage increases with the income classes, from 6% among the households that are eligible to class I to 12% for the households that are not eligible. The amount of new loans is on average 27,743 EUR.

## 4 Methodology

This section lays out the empirical strategy used to identify the effect of mortgage interest rate subsidies on the demand for long-term debt for the extensive margin (probability of having new debt) and intensive margin (amount of new debt).

### 4.1 The proportion of borrowers

We begin by estimating variants of the following model.

$$1(D_{it} > 0) = \alpha_0 + \alpha_1 EL\_1_i * AFTER_t + \alpha_2 EL\_2_i * AFTER_t + \alpha_3 EL\_3_i * AFTER_t + \alpha_4 AFTER_t + \sum_{j=1}^{j=3} \alpha_{4+j} EL\_J_i + \alpha' X_{it} + \theta_i + \eta_{it} \quad (4)$$

First, we discuss the case in which the dependent variable takes value one if the household acquired debt during the year of the interview, and zero otherwise.<sup>20</sup>  $EL\_1_i$ ,  $EL\_2_i$ , and

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<sup>19</sup>The real estate indices are based on monthly data from real estate agencies, i.e., prices of the supply of housing services. This data is gathered by the company *Confidencial Imobiliário*. This firm collects data since 1987.

<sup>20</sup>In previous drafts we estimated the probability of acquiring debt in a given quarter. Nevertheless, the

$EL\_3_i$  are binary variables indicating whether or not the individual is eligible for the program, and what eligibility class does the individual belong to. The omitted group are individuals that are non-eligible for the program.  $AFTER_t$  is a binary variable that takes value 1 if the new loan is signed in 2000 or 2001 and 0 otherwise.  $X_{it}$  contains demographics affecting the propensity to borrow of the individual. Finally, we allow for an individual-specific error term,  $\theta_i$ , that we assume is uncorrelated with the explanatory variables. We have decided to pool the third and fourth eligible groups together. The reason is that the estimates were imprecise, given the relatively small number of individuals who signed loans in these classes.

The parameters of interest are  $\alpha_s$ ,  $s = 1, \dots, 4$ , the coefficients of the interaction between  $AFTER$  and  $EL_{it}$ . The interpretation of  $\alpha_1$ , say, is the difference between the change in the propensity to sign a new loan during the 1998 and 2001 period for eligibles and the corresponding change for non-eligibles. Given that we expect the removal of the subsidy to negatively the demand for long-term debt among eligible individuals we expect the sign of  $\alpha_s$ ,  $s=1, \dots, 4$  to be negative. Note that, given that the take-up rate of CB loans is not one hundred percent,  $\alpha_s$   $s=1, \dots, 4$  cannot be readily interpreted as the uncompensated derivative of the probability of signing a new loan with respect to the interest rate.<sup>21</sup>

Model (1) uses the time variation between 1998 and 2001 for the four eligible groups to identify the effects of changes in the interest rate on the probability of signing a new loan over time. That specification constraints the effects of the reform to be the same within eligible groups. We also estimate the following model

$$1(D_{it} > 0) = \beta_0 + \delta_1 EL\_1_i * H\_P_i * AFTER_t + \delta_2 EL\_2_i * H\_P_i * AFTER_t + \delta_3 EL\_3_i * H\_P_i * AFTER_t +$$

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estimates were rather imprecise. A possibility for the difference in the results is that acquiring long-term debt in a given quarter is a low probability event. Hence, we decided to estimate the probability to acquire long-term debt in a given year.

<sup>21</sup>Even with a less-than-total take-up rate for CB loans among eligibles, we could obtain the structural parameter if we had information on the type of loan of the consumer and the interest rate charged on the loan. 2SLS methods would allow us to recover that parameter.

$$\begin{aligned}
& \sum_{j=1}^{j=3} \beta_j EL\_J_i + \beta_4 AFTER_t + \beta_5 H\_P_i + \sum_{j=1}^{j=3} \beta_{5+j} EL\_J_i * AFTER_t \\
& + \sum_{j=1}^{j=3} \beta_{8+j} EL\_J_i * AFTER_t + \sum_{j=1}^{j=3} \beta_{11+j} EL\_J_i * H\_P_i + \beta' X_{it} + \theta_i + \eta_{it} \quad (5)
\end{aligned}$$

As described in Section 2, the design of the reform makes it likely that there is heterogeneity among the responses of the probability of signing new loans to the change in the interest rate. In particular, given that the 1998 reform set an uniform cap for all Portuguese regions, one could expect that the reform would affect mostly individuals who were living in areas in which the prices of houses were relatively high compared to the cap. We define counties with high prices as those in which the average price of traded houses in 1998 exceeded the value of the cap for a two-member family eligible for the maximum subsidy. Model (2) allows for different responses to the 1998 reform within eligible groups. In particular, the specification in model (2) uses three sources of variation to identify the effects of changes in interest rate incentives on the probability of signing a new loan. First, it exploits the time variation from the reform of the program. Second, model (2) uses the variation in interest rate subsidies across eligible groups and, finally, it permits differential effects by county of residence. The coefficient of interest are  $\delta_1 \dots \delta_4$ . For example,  $\delta_1$  measures the difference in the change in the propensity to borrow during 1998 and 2001 between eligibles for the maximum subsidy living in high-price counties and ineligibles in the same counties, net of the corresponding trend among individuals living in low price counties.

There is an issue regarding the exogeneity of some of the covariates: the same set of unobserved characteristics may lead individuals to get a mortgage and establish their own household, for example. To assess the robustness of our results to possible endogeneity of the covariates, for each model we estimate three alternative specifications, one without covariates, another with a small set of what we consider “safe” covariates (education dummies, variables related to eligibility, like family size, and a third order polynomial in age) and a final one with a full set of covariates, including county fixed-effects.<sup>22</sup>

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<sup>22</sup>The full set of covariates contains the logarithm of pre-tax income, a dummy indicating whether the

## 4.2 The debt amount.

We also present evidence of the evolution of the debt amount. The model estimated is the following:

$$1(D_{it} > 0) = \gamma_0 + \gamma_1 EL\_1_i * AFTER_t + \gamma_2 EL\_2_i * AFTER_t + \gamma_3 EL\_3_i * AFTER_t + \gamma_5 AFTER_t + \sum_{j=1}^{j=3} \gamma_{5+j} EL\_j_i + \alpha' X_{it} + \varepsilon_{1it} \quad (6)$$

$$\log d_{it} = \beta_0 + \beta_1 EL\_1_i * AFTER_t + \beta_2 EL\_2_i * AFTER_t + \beta_3 EL\_3_i * AFTER_t + \delta_5 EL\_1_i + \delta_6 EL\_2_i + \delta_7 EL\_3_i + \delta' X_{it} + \varepsilon_{2it} \quad \text{if } I_{it}^* > 0 \quad (7)$$

where  $I_{it}^*$  is a latent variable indicating individual's  $i$  desire to borrow at moment  $t$ .  $\log d_{it}$  is the logarithm of the amount of new long-term debt signed by the individual, which is observed only if  $I_{it}^*$  is positive.  $X_{it}$  denotes the same set of demographic variables as in the previous specification.  $\varepsilon_{1it}$  and  $\varepsilon_{2it}$  are distributed normally with correlation  $\rho$ . Separate identification of the equations is achieved through functional form, although we also experimented with exclusion restrictions based on competition measures.<sup>23</sup> As in the previous specifications, the coefficients of interest are  $\beta_1$ ,  $\beta_2$ , and  $\beta_3$ . The interpretation of  $\beta_1$  is the percent change in the average amount of new long-term debt between an individual eligible for the maximum subsidy who chose to acquire after the 1998 reform and another individual with the same

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individual lives with his or her parents, a third-order polynomial in age. It also contains separate intercepts for single, widow and divorced individuals, the inverse of the family size, three intercepts with the educational attainment of the head of the household (no education, first 3 degrees of basic school and any type of secondary school). Covariates include a separate intercept for nonworking individuals, for individuals working in a public firm and for individuals working part-time. For married individuals, we include dummies for self-employed wives, and not employed wives. Finally, in some specifications, we also include separate intercepts for county of residence.

<sup>23</sup>We experimented including in the selection equation: number of bank agencies and number of banks in the NUTS-III level. The identifying assumption in that specification is that banks compete for customers at the NUTS-III level, and increased competition rises the availability of debt, but that, conditional on prices of houses, increased competition does not affect the amount of mortgage desired.

characteristics who chose to acquire prior to the reform.<sup>24</sup> If the removal of the subsidy for pricy houses had any effect on the desired amount of debt, the sign of the coefficients  $\beta_1$ ,  $\beta_2$ , and  $\beta_3$  would be negative.

## 5 Empirical evidence

### 5.1 Experiments using variation across eligible groups and time

Table 3 presents a simple differences-in-differences analysis with the proportion of borrowers. Individuals in the 1998 wave of the IE sample provide the before-reform group analysis. Individuals in the 2000 and 2001 waves provide the post-reform group, and we do not use information in the 1999 wave. The estimates show similar trends in the probability of signing a new loan across all eligible classes with respect to the non-eligible group. For all groups, the probability increases by between 1.8 and 2.3 percent. The relative fall among eligibles for the smallest subsidy is .002. The relative increase among noneligibles is 1.6 per cent. Yet, as discussed in Section 3, the diffs-in-diffs evidence is not the most appropriate specification to test for the impact of the 1999 reform on the probability of holding debt, as it is constraining the effects of the reform to be constant among groups that are affected and others that are not (individuals living in “high-” and “low-” price regions, respectively.)

Table 4 presents a simple difference-in-difference analysis of the average amount of initial debt, exploiting two sources of variation to identify the effect of the CB program. Panel A shows the difference between in average individual debt for eligible group 1 and non-eligibles before and after the reform. Each cell contains the average debt within the eligibility group, including individuals who do not have debt. Prior to the reform, individuals eligible for the maximum subsidy signed initial long-term debt for 1,324 euros, and for 1,561 euros after the reform. The difference between the post and pre-reform initial debt is 237 euros. The

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<sup>24</sup>This interpretation can be viewed as supported by a structural model in which  $I_{it}$  denotes the marginal utility of borrowing net of costs, and  $\log d_{it}$  is the desired amount of debt. See Angrist (2002), for a critical review of the assumptions in this model.

corresponding difference for the non-eligible group is 843 euros. Hence, even though eligibles as a group signed higher initial loans, the increase was smaller than the increase for non-eligibles, who were not affected by the reform<sup>25</sup> Panel B presents the comparison between average initial loans for eligible group 2 and non-eligibles. The initial average amount of initial long-term debt within this group was similar to the initial average amount among non-eligibles: 3,345 euro for non-eligibles, and 2,590 for non-eligibles. While for this eligible class 2, the average initial loan fell to 2,325 euros after the passing of the reform, for non-eligibles the average increased to 4,188 euro. These results are consistent with negative effects of the reform on the amount of initial long-term debt. Nevertheless these results mix both the impact on the probability of signing new debt and the amount of initial debt signed.

## 5.2 Experiments using variation across eligible groups, time and county

This subsection presents the results of estimating model (2). That specification allows for different responses to the change in interest rates for individuals living in high and low price counties. We define a county as high-price if the average price of houses sold in the pre-reform period of 1998 exceeded the national median price: 81,055 euro.<sup>26</sup> To avoid biases associated to individuals responding to the reform by moving to cheaper counties, we do not use the county of residence in the period of getting the loan, but the county of residence one year before.<sup>27</sup>

The first column of Table 5 shows OLS estimates of model (2) without adding additional co-

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<sup>25</sup>The average interest rate of new loans with maturity longer than 5 years fell in Portugal from 8.9% in 1998 to 4.9 in 2000. During 2001, it increased again to 6.4 percent.

<sup>26</sup>81,055 euro is also the cap established by the 1998 reform for a person eligible for the maximum subsidy with a family size of 3 members.

<sup>27</sup>There could still be a problem with individuals responding in the 2001 wave, if they moved in 1999 or 2000, for example. For those individuals, the county of residence one year before getting the loan in 2001 may itself be a response to the reform. Nevertheless, we think that the restrictions in our sample mitigate that potential problem. Individuals who have moved recently are likely to have a mortgage to purchase a new house. Among other sample restrictions described in our data section, we restrict the individuals in our sample to have zero debt holdings prior to getting a loan. Hence, we think that individuals without previous mortgages are not likely to have moved to years before the interview.

variables  $X$ . The coefficient of the interaction between  $ELIG\_1$ ,  $AFTER$  and  $HIGHPRICE$  in the first column is  $-.052$ . The standard error, corrected for heteroscedasticity and arbitrary correlation within observations from the same individuals is  $.032$ .<sup>28</sup> The magnitude of the coefficient implies a 5.2 percent fall of the probability of signing a new loan among individuals eligible for the maximum subsidy who were living in high price regions one year before signing the loan. In column [2] we also report OLS coefficients, but controlling for the same set of covariates in Table 6. The results for the first eligible group do not change substantially when we include additional covariates.

The corresponding estimate of the coefficient of the interaction between  $ELIG\_2$ ,  $AFTER$  and  $HIGHPRICE$  is  $-.10$  (standard error:  $.042$ ). The magnitude of the OLS coefficient for the second group suggests a relative fall of 10% in the probability of signing a new loan among eligibles for the 16% subsidy. The magnitude of the coefficient is larger than in the previous case, although the difference between both coefficients is not significantly different from zero. The result is somewhat surprising, given that, according to the computations in Table 1, the eligible group II was exposed to an increase in the interest rate of 16 percent of interest rate payments, while the first group was exposed to a 24 percent increase. A possible explanation is that eligible individuals for class I subsidy have lower income than individuals eligible for the class II subsidy and, on average, demand cheaper houses than eligibles for class II subsidy. Hence, eligibles for class I subsidy are more likely to adjust in high price counties by going to the kink. An alternative explanation is that individuals eligible for the highest subsidy are younger and have lower income and are more likely to be liquidity constrained. Hence, their borrowing behavior is less responsive to changes in the interest rate than that of higher income groups.<sup>29</sup> Finally, the estimate in column [1] for  $ELIG\_3$ ,  $AFTER$  and  $HIGHPRICE$  is

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<sup>28</sup>We include a correction for autocorrelation within observations from the same county to correct for unobserved county characteristics that we do not account for, and to mitigate problems of autocorrelation in both the dependent variable and the treatment, which may depend on the county (see Bertrand, Duflo and Mullainathan, 2003.) Correcting for arbitrary correlation within individuals, instead of individuals reduces the estimated standard errors of the variables of interest by 10 percent.

<sup>29</sup>Attanasio et al. (2000) illustrate in a durable good context that the elasticity of the amount borrowed by liquidity constrained households to changes in the interest rate is smaller in absolute value than that of

negative, imprecise and not significantly different from zero. The point estimate suggests a 4 percent fall in the probability of signing a new loan, a similar magnitude to the point estimate for group 1.

To overcome the limitations of the linear probability model we have also experimented with a Probit specification. The coefficients of a model without covariates are reported column [3] of Table 5. The coefficient of  $ELIG\_1 * AFTER * HIGHPRICE$  denotes the differential change in the probability of signing a new loan between an eligible individual in a high-price county and another individual a low-price county, holding the rest of the covariates at their average. The magnitude of the coefficient suggests a 2.5 percent fall in the probability of signing a new loan, and the standard error is .017. The effect is significant at the 12% confidence level. The pattern for the second eligible group is also smaller than in the OLS specification -.051 (standard error: .013,) significantly different from zero at the 1 per cent confidence level. These magnitudes almost halve those implied by the OLS results. To further assess to what extent non-linearities in the Probit specification and the point chosen to evaluate the probability are driving the discrepancy in the estimates, we have constructed the parameter reported in the OLS specification

$$\begin{aligned} \gamma = & [\Delta_{elig\_high}P(D = 1|ELIG, PRICE, AFTER) - \\ & \Delta_{non\_el\_high}P(D = 1|ELIG, PRICE, AFTER)] - \\ & [\Delta_{elig\_lowprice}P(D = 1|ELIG, PRICE, AFTER) - \\ & \Delta_{non\_el\_lowprice}P(D = 1|ELIG, PRICE, AFTER)] \end{aligned}$$

In that expression, for example,  $\Delta_{elig\_high}P(D = 1|ELIG, PRICE, AFTER)$  denotes the difference between the pre- and post-reform probabilities of signing a new loan for eligible individuals in high price counties. The probabilities  $P(D = 1|ELIG, PRICE, AFTER)$  are constructed using a Probit specification, and the standard errors are calculated using the

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households with free access to the debt market. Jappelli (1990) provides evidence from the US that households headed by younger individuals with low income are more likely to be denied credit.

delta method. For class 1, we have a value of the parameter of  $-.055$  in a specification without covariates (standard error:  $.03$ ). The corresponding estimate for group 2 is  $-.11$  (standard error:  $.04$ ). Those magnitudes are very similar to the OLS result presented in the first column of Table 7. Once we compute comparable estimates, the Probit and OLS methods in estimating model (2) yield similar estimates of the effect of the reform on the probability of holding debt.

Hence, according to the estimates in Table 5, we find that the probability of signing a loan responds to changes in the interest rate. The response varies over the income distribution. In particular, for the lower two thirds of the income distribution, the removal of a subsidy of 26% reduced the probability of holding debt by between 5 and 6 per cent. These estimates are significantly different from zero at the 10% significance level. For the following decile of the income distribution that, as a group experienced an increase in the interest rate of 16% subsidy amounted for a reduction in the probability of holding debt by 10%, and the estimate is significantly different from zero at the 2 percent confidence level.

Next, we convert the estimates above into elasticities by running the following regression:

$$\begin{aligned}
1(D_{it} > 0) = & \delta_0 + \delta_1 INT\_RATE_{it} + \\
& \sum_{j=1}^{j=3} \beta_j EL\_J_i + \beta_4 AFTER_t + \beta_5 H\_P_i + \sum_{j=1}^{j=3} \beta_{5+i} EL\_J_i * AFTER_t \\
& + \sum_{j=1}^{j=3} \beta_{8+i} EL\_J_i * AFTER_t + \sum_{j=1}^{j=3} \beta_{11+i} EL\_J_i * H\_P_i + \beta' X_{it} + \theta_i + \eta_{it}
\end{aligned}$$

In the expression above,  $INT\_RATE_i$  denotes the interest rate. We assume that all individuals face the same pre-subsidy interest rate, and that the subsidy applied to all eligible individuals. Finally, we assume that individuals living in high price regions lost access to the subsidy. The expression above implicitly constraints the response of borrowing behavior to the change in the interest rate to be constant across eligible groups. We instrument  $INT\_RATE_i$  using  $EL\_1_i * H\_P_i * AFTER_t$ ,  $EL\_2_i * H\_P_i * AFTER_t$  and  $EL\_1_i * H\_P_i * AFTER_t$ . The parameter of interest is  $\delta_1$ , that measures the response of the probability to borrow to

a unit increase in the interest rate. The estimate from that specification is  $-.022$  (standard error:  $.013$ ). That is, according to our estimates, an exogenous increase in the interest rate of 1 basis point reduces the probability of getting a new loan by 2.2 percent. Given that the probability of signing a loan prior to the reform was 5 per cent, our estimate looks rather large.

These results differ from recent experiments in Europe. For example, Jappelli and Pistaferri (2002), exploit a tax reform in Italy between 1992 and 1994 that increased the after-tax interest rate for high-income households and reduced it for low income-households and do not find evidence that changes in the incentive to borrow affect the probability of getting a mortgage. In the case under study, we find a significantly negative decrease of the probability of signing a new loan among groups who were affected by a change in eligibility. We can find two explanations of this divergence of results. The first is the public availability of information about the program. JP stress that Italian households lack financial sophistication. Conversely, the CB program involved the participation of banks, which are informed agents, and could advise their customers about the availability of the program. The second explanation is the difference in the datasets, as JP restrict themselves to head of households, and use indicators of whether the individual holds any debt, rather than indicators of whether the individual signed a new loan in the period. To assess the difference in the data, we ran model (2) using a dependent variable that takes value 1 if the individual holds debt and zero otherwise. The coefficients were imprecise, and not significantly different from zero. Still, we obtained negative signs, consistent with the theory, while JP found in some cases signs that contradict the theory. That suggests that information may still play a role.

### **5.2.1 Effect on co-residing adults and falsification exercises.**

The Portuguese housing market shares common features with other Southern European markets. Among other characteristics, a relatively low percentage of young individuals live on rented houses, and a substantial fraction of young households co-reside with their parents

well into their twenties.<sup>30</sup> The availability of parental help through housing services can make young adults able to delay “nest-leaving” in response to changes in the interest rate, so that the borrowing decisions of this group are specially responsive to interest rate changes. Compared to this group, older adults may have a demand of housing services and borrowing decisions more inelastic to the interest rate, as their demand is shaped by other demographic considerations, like family size.<sup>31</sup> In Table 6, we run separate OLS regressions for individuals who coreside with their parents. The results suggest that the 1999 reform affected more the borrowing behavior of individuals coresiding with their parents. For the group eligible for the maximum subsidy, the relative proportion of borrowers in high-price counties fell by almost 11 per cent (significantly different from zero at the 5 per cent significance level.) The results for the other groups are in that order of magnitude, but more imprecise, as the number of loans for those groups is relatively small. Hence, within individuals eligible for the maximum subsidy, the effects of the 1999 reform seem to be concentrated among co-residing adults.<sup>32</sup>

Even if the 1998 reform had no effects on the propensity to borrow, the coefficients in Model (2) could pick up differential trends in borrowing behavior between eligibles and non-eligibles in high price areas. Over the late nineties, the banking industry in Portugal became more competitive, specially in high-price areas.<sup>33</sup> The increase in competition may have created a different trend in high-price areas if banks competed to offer debt to high-income individuals, most likely to be ineligible for the CB program. To examine whether or not any pre-existing differential trend underlies our results, we created a “fake” reform in 1998. For such experiment, we use the 1998 and 1999 waves of the IE, plus additional information of the CRC debt. In principle, we only have four quarterly surveys of pre-reform data. (the four

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<sup>30</sup>See Manacorda and Moretti, 2002, or Martínez-Granado and Ruiz-Castillo 2002 for evidence about co-residence in Italy and Spain, respectively.

<sup>31</sup>Chiuri and Jappelli (2002) provide some evidence showing that age of home-ownership is higher in Southern European countries like Italy and Spain than in Northern European countries or in the US.

<sup>32</sup>We have also split the sample by age, namely by individuals younger than 35 and individuals older than 35. The results (not shown) suggest that the change in the interest rates had larger effects for individuals below age 35.

<sup>33</sup>For example, Standard and Poor’s ratingsdirect report (2002) stress the increase during the 90s in the number of establishments in the high-price areas of Lisbon and Portugal.

surveys in 1998). Yet, we know the history of debt of individuals from 1995. Hence, we can establish whether or not an individual interviewed in the 1998 wave signed a loan during the 1997 wave. We can also establish whether or not an individual interviewed in the 1999 wave signed a loan during the 1998 wave. In order to establish if these individuals qualified for the CB loan at the time of signing the loan, we recomputed eligibility at the time of signing the loan. We can establish family size at the moment of the interview using information on the dates of birth of the members of the family and not counting members born by the time of the period in which the loan was subscribed. As for income, we do not know the value of income at the time of the interview. Hence, we deflated current income using the growth rate in the minimum wage in Portugal.

Next, we ran another version of model (2), but now assigning a value of 1 to the variable  $AFTER_t$  if the loan was signed in 1998, when the CB program did not impose any restriction on the value of the house purchased. The interaction between  $ELIG\_1$ ,  $AFTER$  and  $HIGHPRICE$  should pick up any differential pre-reform trend in the borrowing behavior among eligibles. The first column in Table 8 shows the OLS estimates of that model. The coefficient of the interaction between  $ELIG\_1$ ,  $AFTER$  and  $HIGHPRICE$  is -.012 (.018). It is not significantly different from zero at any popular confidence level. The corresponding coefficient in Table 7 was -.055, with a standard error of .029. The coefficient of the interaction between  $ELIG\_2$ ,  $AFTER$  and  $HIGHPRICE$  is 0 (the standard error is .03.) A non-linear Probit specification shows estimates for the first group that range between .009 (standard error: .009) and .013 (standard error: .01). From this evidence, we interpret that pre-existing trends are not likely to be driving the results in Table 7.

### 5.3 Other results: The amount of debt

Next, we analyze the effect of the interest rate reduction on the amount of debt signed. We expect the 1999 reform to reduce the amount of debt among eligibles for two reasons. Eligible individuals faced the choice between financing the purchase of a house with market value

above the cap and buying a cheaper house financed by a CB loan. In other words, the reform created a kink in the budget constraint that induced individuals willing to borrow to reduce their optimal debt amount. The evolution of debt holding is likely to be an underestimate of the effect of a reduction in the interest rate on the amount of debt.

Table 8 presents estimates of model (4) using both ordinary least squares on the sample of individuals who have signed a loan during the year of the interview. Given that an OLS specification does not take into account the truncation in the dependent variable, we also report coefficients from a Maximum Likelihood specification.<sup>34</sup>

The interpretation of the coefficient of  $ELIG\_1 * AFTER_t$  is the percentage difference in the change in the amount of initial long-term debt of eligible individuals who choose to acquire new debt, relative to the change in the same amount held by non-eligibles, holding constant the set of covariates in footnote X . Our preferred specification in Table 2 is the estimation of model (3) by Maximum Likelihood, which is reported in the second column. The point estimate of the coefficient of  $ELIG\_1 * AFTER_t$  is  $-.31$  (standard error:  $.13$ ), suggesting that, among eligibles for the maximum subsidy, the initial loan amount diminished by a 31% with respect to non eligibles. The estimate of  $ELIG\_1 * AFTER_t$  in column (1) of Table 9 shows virtually identical results in an OLS specification. The coefficient of  $ELIG\_2 * AFTER_t$  in the first column of Table 8 implies a percentage reduction in the average amount of debt of 47 percent. The point estimate is  $-.496$  (standard error:  $.20$ ) is larger than for the first group.<sup>35</sup> The result is in line with the result in the previous section, when we find that the response to the 1999 reform was larger among consumers eligible for the second largest subsidy. The combined point estimate for the third and fourth eligible groups is positive and

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<sup>34</sup>Following most of the literature on wealth and savings, we choose a specification in logarithms. The reason is that interest rates were going down for most of our sample period. Given that the average debt varied across groups (see Table III), a specification in levels can lead to spuriously negative effect of the reform. Assume that the program has no effect and that, in response to a fall in the interest rate, all households increase their debt by a fixed percentage. A specification in levels would find lower growth in the amount of debt among eligibles, suggesting a negative effect of the reform. See Engen and Gale (2000), for a similar reasoning when evaluating whether 401(k) programs have increased savings in the US.

<sup>35</sup>The standard errors in the Maximum Likelihood specification are not corrected by heteroscedasticity nor by autocorrelation.

nonsignificantly different from zero

We have also run regressions in which we allow the effect of the change in the interest rate on the amount of debt to change with the location of the household. The estimates are shown in columns 3 and 4 of Table 8. We do not find evidence of differential responses in this case. We do not have a good explanation for this result, which looks at odds with the evidence on the effect of the reform on high price counties. A possible explanation is that most individuals in low price counties responded to the reform by purchasing houses at the cap set by the 1999 reform, while in high price counties, a share of individuals responded by purchasing houses at the cap (and hence reducing their initial debt) and another share of individuals chose not to purchase.

Overall, the results in Table 8 are consistent with negative effects of the removal of the subsidy for high price houses on the amount of initial debt. For the two groups that enjoyed larger subsidies prior to the reform, the removal of the subsidy resulted in average falls in the amount of new debt. We interpret that these results rule out theories of individuals not responding to changes in the interest rate.

## 6 Conclusions

Mortgage interest rate subsidies are prevalent among OECD countries, mainly through tax deductions. In this draft, we have used a reform in a Portuguese program subsidizing interest rate payments on mortgage loans held by low-income households. We consider that our analysis give special insights to assess the responsiveness of long-term debt to changes in the interest rate. First, we are analyzing a program that included substantial subsidies to a large fraction of households. Second, as opposed to other deductions, the *Credito Bonificado* program was well-publicized. Third, we use a unique dataset containing accurate information on household debt, income and demographic variables. Our results suggest that the cancellation of incentives had negative effects on the access to borrowing.

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## **7 Data appendix**

(to be completed)

**Table 1: Savings associated to the CB program, an example**

Computations assume an interest rate of 8%

Maturity of the loan: 25 years.

No inflation, constant interest rate.

Age loan	Debt	Paymt	1/R	=Paymt*(1/R)	% Subsidy Class I	Paymt. CB
0	48000					
1	47400	4440	0.926	4111	0.44	2751
2	46751	4440	0.857	3807	0.44	2772
3	46051	4440	0.794	3525	0.43	2832
4	45295	4440	0.735	3264	0.42	2893
5	44478	4440	0.681	3022	0.41	2955
6	43596	4440	0.630	2798	0.39	3053
7	42643	4440	0.583	2591	0.37	3150
8	41614	4440	0.540	2399	0.35	3246
9	40503	4440	0.500	2221	0.33	3342
10	39303	4440	0.463	2057	0.31	3436
11	38007	4440	0.429	1904	0.29	3529
12	36607	4440	0.397	1763	0.27	3619
13	35095	4440	0.368	1633	0.25	3708
14	33463	4440	0.340	1512	0.23	3795
15	31699	4440	0.315	1400	0.21	3878
16	29795	4440	0.292	1296	0.19	3959
17	27738	4440	0.270	1200	0.17	4035
18	25517	4440	0.250	1111	0.15	4107
19	23118	4440	0.232	1029	0.13	4175
20	20527	4440	0.215	953	0.11	4237
21	17729	4440	0.199	882	0.09	4293
22	14707	4440	0.184	817	0.07	4341
23	11443	4440	0.170	756	0.05	4382
24	7918	4440	0.158	700	0.03	4413
25	4111	4440	0.146	648	0.01	4434
26	0	4440	0.135	600	0	4440

Discounted Payment = Sum (Payment\*(1/R))

48,000

36,408

Ratio subsidy/without I **0.76**

Ratio subsidy/without II **0.84**

Ratio subsidy/without III **0.92**

Ratio subsidy/without IV **0.97**

**Table 2: Descriptive Statistics of the main sample**

	Class 1		Class 2		Classes 3 and 4		Non Eligible		Whole Sample	
Number of observations:	11411		1308		1379		2694		16792	
	68.0%		7.8%		8.2%		16.0%		100.0%	
	Mean	St.Dev.	Mean	St.Dev.	Mean	St.Dev.	Mean	St.Dev.	Mean	St.Dev.
Signed new loan	0.05	0.23	0.09	0.28	0.11	0.31	0.13	0.34	0.07	0.26
Amount, if positive	29210.00	28380.00	27158.28	27482.00	28402.00	27656.00	33072.00	32043.00	30015.00	29334.00
Gross Income	9193.97	2828.52	15162.54	956.55	18265.00	1503.73	35433.00	19938.00	14613.00	12652.00
Family size	2.20	1.37	2.18	1.23	2.22	1.23	2.48	1.31	2.24	1.34
Age	33.86	11.17	35.07	10.31	35.42	9.91	38.81	9.48	34.87	10.90
Married	0.47	0.50	0.53	0.50	0.54	0.50	0.61	0.49	0.50	0.50
Single	0.45	0.50	0.41	0.49	0.39	0.49	0.31	0.46	0.42	0.49
Divorced	0.05	0.23	0.05	0.21	0.05	0.22	0.07	0.25	0.06	0.23
Widow	0.02	0.23								
Single female	0.22	0.42	0.19	0.39	0.18	0.39	0.20	0.40	0.22	0.49
Co-resides with parents	0.47	0.50	0.42	0.49	0.39	0.49	0.27	0.44	0.42	0.17
Does not read	0.04	0.20	0.02	0.14	0.01	0.09	0.00	0.04	0.03	0.17
Completed 6th grade in basic school	0.63	0.48	0.48	0.50	0.41	0.49	0.16	0.36	0.52	0.50
Basic school or occupational degree	0.31	0.46	0.39	0.49	0.41	0.49	0.40	0.49	0.34	0.47
College degree	0.02	0.04	0.10	0.30	0.17	0.38	0.44	0.50	0.25	0.44
Works for public Sector	0.06	0.23	0.12	0.32	0.13	0.34	0.22	0.41	0.09	0.29
Part time	0.02	0.15	0.02	0.13	0.01	0.10	0.02	0.14	0.02	0.14
Secondary earner self-employed	0.04	0.20	0.03	0.17	0.03	0.18	0.03	0.18	0.04	0.20
Secondary earner does not work	0.15	0.35	0.02	0.14	0.56	0.50	0.04	0.20	0.11	0.32
Lived in High price county	0.49	0.49	0.52	0.50	0.46	0.50	0.62	0.49	0.52	0.50
Average Price in the county in 1998	82078.00	11163.00	83564.00	12838.00	8578.00	13535.00	123703.00	14328.00	83282.00	12221.00

All monetary magnitudes in constant euro of 1999

Family size corresponds to "tax-paying units." In the case of adults living in their own household, exclude co-residing adults above 18

Married individuals contribute only one observation, and the report of age, education or labor market situation corresponds to the main earner, unless otherwise stated.

Lived in a High Price county refers to the year prior to the interview.

New loan refers to loans maturity longer than one year.

**Table 3: Differences-in-differences results: proportion of borrowers**

<u>Panel A: Eligible group 1: Subsidy reduction of 25%</u>			
	<u>Eligibles</u>	<u>Non-eligibles</u>	<u>Difference between groups</u>
After the reform	0.063	0.132	0.068
Before the reform	0.045	0.116	0.071
Difference within groups	0.018	0.016	0.002

<u>Panel B: Eligible group 2: Subsidy reduction of 16%</u>			
	<u>Eligibles</u>	<u>Non-eligibles</u>	<u>Difference between groups</u>
After the reform	0.097	0.132	0.035
Before the reform	0.079	0.116	0.036
Difference within groups	0.018	0.016	0.002

<u>Panel C: Eligible group 3 and 4: Subsidy reduction of 8% and 4%</u>			
	<u>Eligibles</u>	<u>Non-eligibles</u>	<u>Difference between groups</u>
After the reform	0.110	0.132	0.022
Before the reform	0.088	0.116	0.028
Difference within groups	0.022	0.016	0.006

**Table 4: Differences-in-differences results: average debt amount**

Panel A: Eligible group 1, Subsidy reduction of 25%			
	Eligibles	Non-eligibles	Difference between groups
After the reform	1561.874	4188.89	2627.016
Before the reform	1324.343	3345.189	2020.846
Difference within groups	237.531	843.701	<b>-606.17</b>

Panel B, Eligible group 2: Subsidy reduction of 16%			
	Eligibles	Non-eligibles	Differences between groups
After the reform	2352.72	4188.89	1836.17
Before the reform	2590.686	3345.189	754.503
Difference within groups	-237.966	843.701	<b>-1081.667</b>

Panel C: Eligible group 3 and 4: Subsidy reduction of 8% and 4%			
	Eligibles	Non-eligibles	Differences between groups
After the reform	3261.118	4188.89	927.772
Before the reform	2038.177	3345.189	1307.012
Difference within groups	1222.941	843.701	<b>379.24</b>

**Table 5: The impact of the 1999 reform on the probability of holding debt**

Dependent variable takes value 1 if individual acquired long-term debt during the year

Estimation method: Ordinary Least Squares

	(1) [No covariates]	(2) [Limited Covariates]	(3) [County Fixed-effects]
ELIG1*AFTER*HIGHPRICE	-0.052 (0.029)*	-0.054 (0.029)*	-0.055 (0.032)*
ELIG2*AFTER*HIGHPRICE	-0.103 (0.042)**	-0.102 (0.042)**	-0.094 (0.041)**
ELIG3*AFTER*HIGHPRICE	-0.043 (0.044)	-0.040 (0.044)	-0.039 (0.053)
ELIG1*AFTER	0.032 (0.023)	0.033 (0.023)	0.030 (0.021)
ELIG2*AFTER	0.054 (0.031)*	0.054 (0.031)*	0.045 (0.031)
ELIG3*AFTER	0.014 (0.035)	0.013 (0.034)	0.013 (0.042)
ELIG1*HIGHPRICE	0.031 (0.021)	0.032 (0.021)	0.032 (0.021)
ELIG2*HIGHPRICE	0.071 (0.031)**	0.070 (0.031)**	0.068 (0.029)**
ELIG3*HIGHPRICE	0.019 (0.033)	0.018 (0.033)	0.020 (0.035)
ELIG1	-0.095 (0.017)***	-0.078 (0.017)***	-0.054 (0.016)***
ELIG2	-0.081 (0.023)***	-0.072 (0.023)***	-0.058 (0.024)**
ELIG3	-0.030 (0.026)	-0.025 (0.026)	-0.018 (0.027)
AFTER	-0.021 (0.022)	-0.019 (0.022)	-0.014 (0.019)
AFTER*HIGHPRICE	0.060 (0.028)**	0.059 (0.028)**	0.056 (0.029)**
HIGHPRICE	-0.040 (0.020)**	-0.040 (0.020)**	0.000 (0.000)
(Age - 30)/10		0.008 (0.005)*	0.005 (0.005)
(Age - 30) * (Age - 30)/100		-0.022 (0.004)***	-0.020 (0.004)***
(Age - 30)*(Age - 30)(Age - 30)/1000		0.004 (0.002)**	0.003 (0.002)*
Single female		-0.025 (0.005)***	-0.018 (0.005)***
Number of adults - 2		-0.007 (0.005)	-0.010 (0.006)*
Inverse of family size -1/2		-0.072 (0.011)***	-0.284 (0.080)***
Main earner does not read		-0.043 (0.009)***	-0.036 (0.010)***
Main earner completed 6th grade		-0.019 (0.005)***	-0.017 (0.007)**
Main earner completed college degree		-0.001 (0.010)	-0.007 (0.013)
Logarithm of per capita family income			0.030 (0.009)***
Dependent			-0.023 (0.008)***
Single			-0.031 (0.010)***
Divorced			-0.037 (0.013)***
Widow-er			-0.050 (0.011)***
Main earner works for public sector			0.013 (0.009)
Main earner works part time			0.002 (0.011)
Secondary earner self-employed			0.037 (0.013)***
Main earner not employed			-0.011 (0.012)
Secondary earner not employed			0.004 (0.007)
Constant	0.147 (0.016)***	0.172 (0.018)***	0.180 (0.012)***
Observations			
R-squared	0.01	0.03	0.04

Notes: Standard errors, in parentheses, are corrected for heteroscedasticity and arbitrary correlation between observations from the same individual in specifications (1) and (2). In (3), they allow for autocorrelation within the county

\*, \*\*, \*\*\* denote that the hypothesis of the coefficient being different from zero is rejected at the 10%, 5% and 1% significance level

The logarithm of per capita family income is the deviation from sample means. The omitted education group is completed primary schooling. The omitted marital status group is married

The omitted employment status group are employed individuals working in the private sector.

Specifications [1] and [2] also include district dummies and controls for the number of bank agencies per bank in the county

Log File: C:/projects/Credit/0803.dir/logfiles/elig34/tab3deols.log

**Table 5 (cont.): The impact of the 1999 reform on the probability of holding debt**

Dependent variable takes value 1 if individual acquired long-term debt during the year

	Estimation method: Probit		
	(4) [No Covariates]	(5) [Limited Set of Covariates]	(6) [Concelho Fixed-Effects]
ELIG1*AFTER*HIGHPRICE	-0.025 (0.017)	-0.025 (0.016)	-0.028 (0.016)*
ELIG2*AFTER*HIGHPRICE	-0.051 (0.013)***	-0.048 (0.012)***	-0.044 (0.012)***
ELIG3*AFTER*HIGHPRICE	-0.023 (0.023)	-0.021 (0.022)	-0.025 (0.023)
ELIG1*AFTER	0.027 (0.017)	0.026 (0.016)*	0.026 (0.015)*
ELIG2*AFTER	0.052 (0.036)	0.051 (0.035)	0.042 (0.034)
ELIG3*AFTER	0.009 (0.025)	0.009 (0.024)	0.012 (0.031)
ELIG1*HIGHPRICE	0.014 (0.016)	0.015 (0.016)	0.019 (0.016)
ELIG2*HIGHPRICE	0.070 (0.040)*	0.066 (0.039)*	0.066 (0.037)*
ELIG3*HIGHPRICE	0.011 (0.026)	0.012 (0.025)	0.019 (0.028)
ELIG1	-0.091 (0.015)***	-0.065 (0.014)***	-0.030 (0.012)**
ELIG2	-0.046 (0.010)***	-0.038 (0.011)***	-0.026 (0.014)*
ELIG3	-0.017 (0.014)	-0.012 (0.014)	-0.006 (0.016)
AFTER	-0.013 (0.014)	-0.010 (0.013)	-0.009 (0.011)
AFTER*HIGHPRICE	0.042 (0.021)**	0.040 (0.020)**	0.040 (0.021)*
HIGHPRICE	-0.027 (0.013)**	-0.025 (0.012)**	-0.128 (0.017)***
(Age - 30)/10		0.014 (0.005)***	0.010 (0.005)*
(Age - 30) * (Age - 30)/100		-0.029 (0.005)***	-0.024 (0.005)***
(Age - 30)*(Age - 30)(Age - 30)/1000		0.007 (0.002)***	0.005 (0.002)***
Single female		-0.027 (0.005)***	-0.020 (0.006)***
Number of adults - 2		-0.009 (0.005)*	-0.011 (0.005)**
Inverse of family size -1/2		-0.065 (0.010)***	-0.435 (0.069)***
Main earner does not read		-0.038 (0.007)***	-0.034 (0.008)***
Main earner completed 6th grade		-0.018 (0.005)***	-0.017 (0.006)***
Main earner completed College degree		0.001 (0.007)	-0.006 (0.008)
Logarithm of per capita family income			0.045 (0.007)***
Dependent			-0.017 (0.007)**
Single			-0.019 (0.009)**
Divorced			-0.026 (0.009)***
Widow-er			-0.040 (0.007)***
Main earner works for public sector			0.012 (0.007)
Main earner works part time			0.008 (0.012)
Secondary earner self-employed			0.034 (0.012)***
Main earner not employed			-0.010 (0.012)
Secondary earner not employed			0.007 (0.006)
Constant	16792	16792	16370
R_squared			

Notes: Standard errors, in parentheses, are corrected for heteroscedasticity and arbitrary correlation between observations from the same individual in specifications (1) and (2). In (3), they allow for autocorrelation within the county

\*, \*\*, \*\*\* denote that the hypothesis of the coefficient being different from zero is rejected at the 10%, 5% and 1% significance level

The logarithm of per capita family income is the deviation from sample means. The omitted education group is completed primary schooling. The omitted marital status group is married

The omitted employment status group are employed individuals working in the private sector.

Specifications [1] and [2] also include district dummies and controls for the number of bank agencies per bank in the county

**Table 6: The impact of the 1999 reform on the probability of holding debt, individuals living with parents**

Dependent variable takes value 1 if individual acquired long-term debt during the year

Estimation Method : OLS

	(1) [No Covariates]	(2) [Full Set of Covariates]	(3) [Concelho Fixed-Effects]
ELIG1*AFTER*HIGHPRICE	-0.106 (0.048)**	-0.109 (0.048)**	-0.110 (0.047)**
ELIG2*AFTER*HIGHPRICE	-0.109 (0.062)*	-0.109 (0.062)*	-0.092 (0.059)
ELIG3*AFTER*HIGHPRICE	-0.137 (0.066)**	-0.133 (0.066)**	-0.133 (0.061)**
ELIG1*AFTER	0.071 (0.037)*	0.073 (0.037)*	0.068 (0.029)**
ELIG2*AFTER	0.072 (0.048)	0.071 (0.048)	0.054 (0.036)
ELIG3*AFTER	0.121 (0.051)**	0.113 (0.051)**	0.113 (0.044)**
ELIG1*HIGHPRICE	0.069 (0.038)*	0.075 (0.038)**	0.071 (0.033)**
ELIG2*HIGHPRICE	0.066 (0.048)	0.070 (0.048)	0.054 (0.046)
ELIG3*HIGHPRICE	0.070 (0.050)	0.070 (0.050)	0.066 (0.040)
ELIG1	-0.120 (0.032)***	-0.090 (0.035)**	-0.080 (0.032)**
ELIG2	-0.095 (0.039)**	-0.084 (0.040)**	-0.064 (0.036)*
ELIG3	-0.095 (0.040)**	-0.085 (0.040)**	-0.081 (0.032)**
AFTER	-0.060 (0.037)	-0.062 (0.036)*	-0.055 (0.027)**
AFTER*HIGHPRICE	0.100 (0.047)**	0.099 (0.047)**	0.098 (0.044)**
HIGHPRICE	-0.071 (0.037)*	-0.072 (0.038)*	-0.065 (0.037)*
Observations	7136	7137	7138

## Notes

Standard errors, in parentheses, are corrected for heteroscedasticity and arbitrary correlation between observations from the same individual.

one, two and three asterisks denotes that the hypothesis of the coefficient being different from zero is rejected at the 10%, 5% and 1% significance level

The logarithm of per capita family income is the deviation from sample means.

Omitted education group is completed primary schooling, or professional training.

The omitted marital status group is single head. The omitted marital status group are households in which the head is single

The omitted employment status group are employed individuals working in the private sector.

Log File: C:/projects/Credit/0803.dir/logfiles/elig34/tab3dedep1.log

**Table 7: The impact of a fake 1998 reform on the probability of holding debt**

Dependent variable takes value 1 if individual acquired long-term debt during the year					
Estimation method:	OLS	OLS	OLS	Probit	Probit
	(1)	(2)	(3)	(4)	(5)
	[No covariates]	[Limited Covariates]	[County fixed-effects]	[No covariates]	[Limited Covariates]
ELIG1*AFTER*HIGHPRICE	-0.012 (0.019)	-0.012 (0.019)	-0.011 (0.019)	-0.011 (0.010)	-0.009 (0.009)
ELIG2*AFTER*HIGHPRICE	0.008 (0.034)	0.008 (0.034)	0.006 (0.034)	0.004 (0.026)	0.007 (0.026)
ELIG3*AFTER*HIGHPRICE	0.034 (0.035)	0.034 (0.035)	0.033 (0.036)	0.047 (0.047)	0.045 (0.044)
ELIG1*AFTER	0.015 (0.016)	0.014 (0.016)	0.013 (0.016)	0.020 (0.012)	0.017 (0.011)
ELIG2*AFTER	0.010 (0.022)	0.010 (0.022)	0.011 (0.023)	0.010 (0.020)	0.006 (0.018)
ELIG3*AFTER	0.002 (0.030)	0.002 (0.030)	0.003 (0.031)	-0.005 (0.018)	-0.005 (0.016)
ELIG1*HIGHPRICE	-0.000 (0.015)	-0.001 (0.015)	-0.004 (0.015)	0.003 (0.011)	0.001 (0.010)
ELIG2*HIGHPRICE	0.008 (0.020)	0.006 (0.020)	0.009 (0.021)	0.009 (0.018)	0.007 (0.016)
ELIG3*HIGHPRICE	-0.032 (0.022)	-0.032 (0.022)	-0.029 (0.023)	-0.025 (0.008)***	-0.023 (0.007)***
ELIG1	-0.051 (0.011)***	-0.044 (0.011)***	-0.024 (0.013)*	-0.055 (0.011)***	-0.040 (0.010)***
ELIG2	-0.036 (0.015)**	-0.033 (0.016)**	-0.023 (0.017)	-0.021 (0.007)***	-0.016 (0.008)**
ELIG3	-0.003 (0.018)	-0.003 (0.018)	0.003 (0.019)	0.009 (0.015)	0.010 (0.014)
AFTER	-0.001 (0.016)	-0.001 (0.015)	0.000 (0.016)	-0.001 (0.009)	0.000 (0.008)
AFTER*HIGHPRICE	0.005 (0.018)	0.005 (0.018)	0.004 (0.019)	0.003 (0.011)	0.001 (0.010)
HIGHPRICE	0.003 (0.014)	0.005 (0.014)		0.002 (0.008)	0.003 (0.007)
Constant	0.071 (0.010)***	0.090 (0.011)***	0.103 (0.017)***		
Observations	15503	15503	15503	15503	15503
R-squared	0.01	0.02	0.03		

**Table 8: The impact of the 1999 reform on the logarithm of new debt amount**

	(1) [OLS]	(2) [Max. Likel.]	(4) [OLS]	(5) [Max. Lik.]
ELIG1*AFTER	-0.292 (0.130)**	-0.322 (0.127)**	-0.353 (0.192)*	-0.387 (0.194)**
ELIG2*AFTER	-0.467 (0.197)**	-0.495 (0.205)**	-0.761 (0.309)**	-0.850 (0.321)**
ELIG3*AFTER	0.160 (0.182)	0.158 (0.187)	-0.316 (0.276)	-0.304 (0.277)
ELIG1*AFTER*HIGHPRICE			0.073 (0.266)	0.068 (0.260)
ELIG2*AFTER*HIGHPRICE			0.478 (0.400)	0.565 (0.436)
ELIG3*AFTER*HIGHPRICE			0.792 (0.366)**	0.812 (0.375)**
ELIG1	0.218 (0.138)	0.270 (0.141)*	0.214 (0.164)	0.231 (0.176)
ELIG2	0.195 (0.168)	0.221 (0.172)	0.470 (0.248)*	0.524 (0.265)**
ELIG3	-0.182 (0.144)	-0.190 (0.152)	-0.127 (0.232)	-0.002 (0.210)
AFTER	0.090 (0.107)	0.094 (0.102)	0.220 (0.163)	0.259 (0.158)
Logarithm of per capita family income	0.139 (0.134)	0.079 (0.152)	0.134 (0.135)	0.112 (0.163)
Main earner is head of household	-0.143 (0.117)	-0.169 (0.115)	-0.179 (0.122)	-0.148 (0.116)
(Age - 30)/10	0.011 (0.008)	0.007 (0.009)	0.012 (0.008)	0.008 (0.009)
(Age - 30)* (Age - 30)/100	-0.003 (0.001)**	-0.002 (0.001)**	-0.003 (0.001)**	-0.002 (0.001)**
(Age - 30)*(Age - 30)(Age - 30)/1000	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Single	0.061 (0.171)	0.109 (0.171)	0.157 (0.190)	0.105 (0.172)
Divorced	0.195 (0.167)	0.250 (0.190)	0.276 (0.176)	0.243 (0.192)
Widow-er	-0.366 (0.295)	-0.223 (0.352)	-0.370 (0.295)	-0.303 (0.359)
Single female	0.153 (0.104)	0.196 (0.109)*	0.131 (0.106)	0.165 (0.113)
Number of adults - 2	-0.032 (0.072)	-0.003 (0.075)	-0.071 (0.076)	-0.020 (0.076)
Inverse of family size	-1.498 (1.285)	-0.918 (1.479)	-1.789 (1.299)	-1.235 (1.582)
Does not read	0.152 (0.246)	0.263 (0.239)	0.160 (0.252)	0.223 (0.244)
Completed 6th grade	-0.141 (0.065)**	-0.112 (0.070)	-0.143 (0.066)**	-0.126 (0.073)*
College degree	-0.025 (0.093)	-0.031 (0.093)	-0.040 (0.095)	-0.050 (0.093)
Main earner works for public sector	-0.171 (0.077)**	-0.189 (0.085)**	-0.174 (0.078)**	-0.181 (0.085)**
Main earner works part time	0.185 (0.191)	0.158 (0.202)	0.178 (0.190)	0.166 (0.200)
Secondary earner self-employed	0.049 (0.111)	0.004 (0.121)	0.085 (0.112)	0.048 (0.125)
Main earner not employed	0.006 (0.156)	0.022 (0.167)	0.010 (0.161)	0.016 (0.166)
Secondary earner not employed	0.041 (0.088)	0.021 (0.090)	0.058 (0.088)	0.040 (0.089)

Notes: Standard errors, in parentheses, are corrected for heteroscedasticity and arbitrary correlation between observations from the same individual in specifications (1) and (2). In (3), they allow for autocorrelation within the county

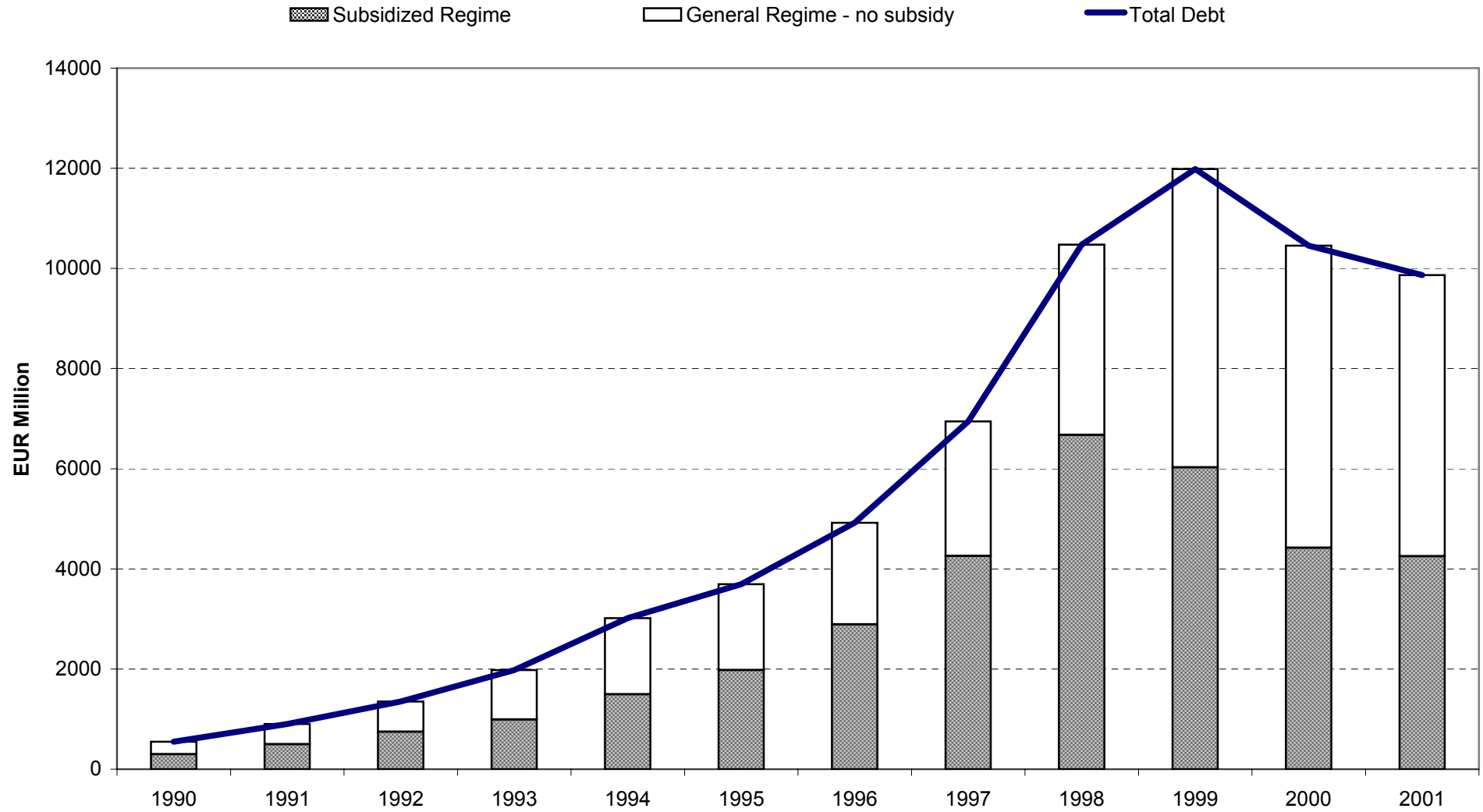
\*, \*\*, \*\*\* denote that the hypothesis of the coefficient being different from zero is rejected at the 10%, 5% and 1% significance level

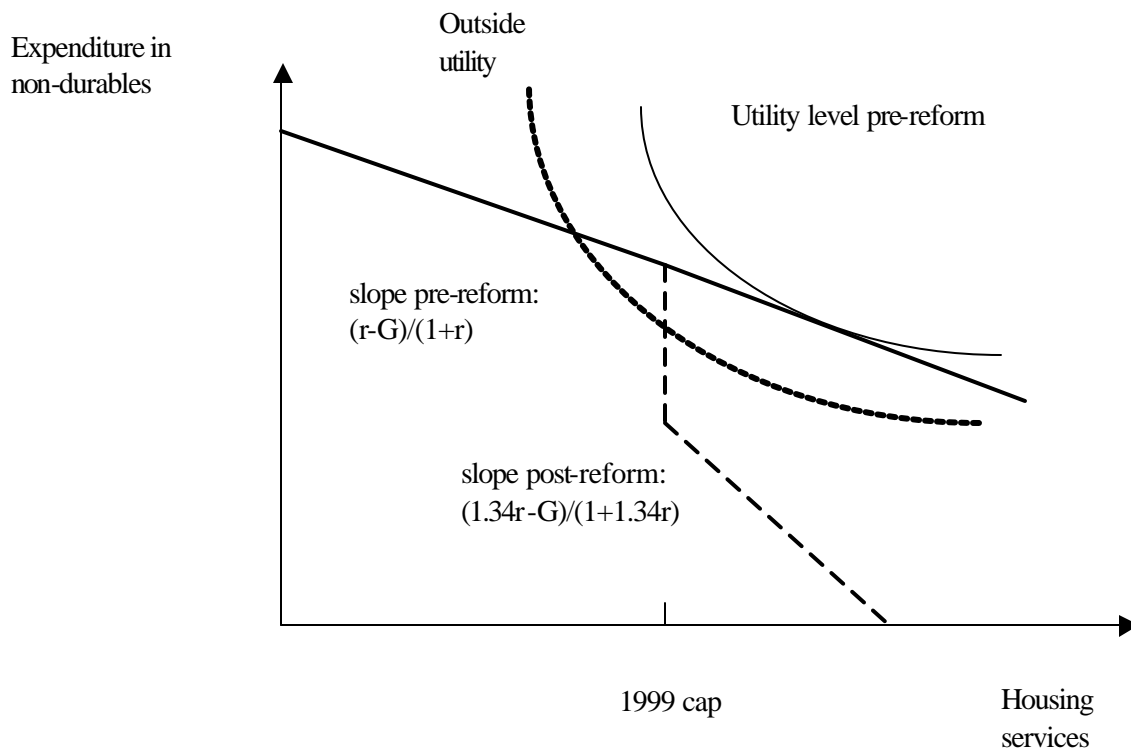
The logarithm of per capita family income is the deviation from sample means. The omitted education group is completed primary schooling. The omitted marital status group is married

The omitted employment status group are employed individuals working in the private sector.

Specifications [1] and [2] also include district dummies and controls for the number of bank agencies per bank in the county

### Total Value of New Long-term Debt Contracts for the Three Types of Debt Commitments





**Figure 3: The effect of the 1999 reform of the “CB” program on the budget constraint of an individual eligible for the maximum subsidy.**

The full line represents the budget constraint of an eligible household before the 1998 reform.

The dotted line represents the budget constraint of an eligible household after the 1998 reform.