Do people respond to tax incentives? An analysis of the Italian reform of the deductibility of home mortgage interests

Tullio Jappelli[a,b,c,*], Luigi Pistaferri[d,c,e]

[a]*University of Salerno, Italy*
[b]*CSEF, Italy*
[c]*CEPR, UK*
[d]*Stanford University, US*
[e]*SIEPR, US*

Abstract

Before 1992 mortgage interests in Italy were fully tax deductible up to 3500 Euro (7000 for two cosigners). In 1992–1994 the government implemented a series of tax reforms whose ultimate effect was to eliminate the relation between the after-tax mortgage rate and the marginal tax rate. Using data from the 1989–2002 Survey of Household Income and Wealth we test if the elimination of incentives has affected the sensitivity of the decision to borrow and the amount borrowed with respect to the marginal tax rate. Regression analysis and difference-in-differences estimates indicate that tax considerations have not affected the demand for mortgage debt, neither at the extensive nor intensive margin. These results are consistent with lack of financial information and credit rationing during the sample period.

*Corresponding author. Tel.: +39 089 963165; fax: +39 089 963169.
   E-mail address: tullioj@tin.it (T. Jappelli).

1. Introduction

The theory of portfolio taxation suggests that investors' portfolio choices are affected by the after-tax returns on each asset, and that the differing fiscal treatment of the various assets creates wedges in the structure of the returns. The empirical literature for the US, as summarized by Poterba (2002), appears to support the view that taxes affect asset selection and allocation. Studies on households' response to changes in the tax treatments of debt are far more limited (Maki, 2001).

The main limitation of empirical studies has been that of identifying the tax effects. Theory predicts that the decisions to borrow (or to invest in a given asset) and how much to borrow are affected, among other variables, by the after-tax interest rate on borrowing. However, since the after-tax borrowing rate depends on the taxpayer's marginal income tax rate, which is inherently correlated with the level of income, in a cross-sectional framework it is difficult to disentangle genuine variation in after-tax interest rates, for given income, from genuine variations in income, for given after-tax interest rates. In the absence of tax incentives this is actually impossible if, at any point in time, all borrowers face the same interest rate. When the interest rate varies across borrowers, it is generally not observed. And even when it is observed, cross-sectional variability in the price of borrowing tends to be correlated with other household characteristics.

In this paper we address the issue of identification of tax effects by exploiting cross-sectional and time-series evidence in the after-tax rate induced by exogenous policy changes, rather than (possibly endogenous) shifts in the income distribution. The change that we consider is the elimination of tax incentives in the Italian mortgage market for borrowers with high marginal tax rates.

Most OECD countries allow mortgage interests deductions for first-time homeowners (Poterba, 2002). We bring new evidence to the portfolio effect of the tax treatment of household liabilities by studying the effect of changes in the tax treatment of Italian mortgages on the propensity to borrow and on the amount borrowed. The data we use, a series of repeated cross-sections drawn from the 1989–2002 Bank of Italy Survey of Household Income and Wealth (SHIW), are representative of the Italian population, include information on household mortgage debt, after-tax income and demographic variables, and cover a period of rapid credit market expansion.

The richness of our data and the features of the tax reform provide a truly unique setting for spotlighting the effect of tax incentives on borrowing in particular, and on household portfolio selection and allocation more generally. From 1982 to 1992 mortgage interests on the principal residence were fully tax-deductible up to approximately 3500 Euro (7 million lire). To get a sense of the magnitudes involved, an individual borrowing 50,000 Euro at the 10 percent interest rate for 20 years was able to deduct *all* interests on his loan. In this regime, the tax incentive was proportional to the borrower's marginal tax rate. During our sample period, the marginal tax rate was between 10 and 50 percent (see Section 3 for more details). For someone with a marginal tax rate of 10 percent, the tax incentive implied a reduction in the after-tax mortgage rate of only 1 percentage point. But for someone with a marginal tax rate of 33 percent, the reduction was 3.3 points (from 10 to 6.7 percent), while if the marginal tax rate was 50 percent, the after tax mortgage rate was reduced by 5 points (from 10 to 5 percent).

Following a sharp overhaul of the tax incentive system, in 1992 the link with the marginal tax rate was broken, and the incentive made proportional—up to the unchanged

3500 Euro limit—to the interests paid (a flat rate of 27 percent, lowered to 22 percent in 1994, and 19 percent in 1998). The reform meant that the reduction in the after-tax rate was the same for all taxpayers: in the example above, the after tax-interest rate was reduced from 10 to 7.3 percent in 1992, to 7.8 in 1994, and to 8.1 percent in 1998, regardless of the marginal tax rate.

As a consequence, after 1992 the incentive to borrow was substantially reduced for the rich, slightly increased for the poor, and unchanged for borrowers in the intermediate tax brackets. Thus, if the tax reform had any impact on the decision to borrow and on the amount borrowed, it should emerge among households in the highest and lowest tax brackets after 1992. This is a simple testable implication of the theory of portfolio taxation. Our repeated cross-sectional data span both pre-and post-reform years (1989–2002), and are therefore ideal to study the impact of taxes on household borrowing decisions.

The topic has important ramifications that go far beyond the specificities of mortgage markets or household liabilities. Studying if and how households react to tax incentives is of paramount importance for understanding the effectiveness of other policy reforms. For instance, under the pressure of unsustainable financial prospects, several European countries have implemented pension reforms and introduced new tax incentive for retirement saving. Another interesting angle of our research is that existing tests of the effect of taxes on household liabilities have been performed in countries with well developed mortgage and consumer credit markets, such as the US and the UK At the same time, one could expect that in these countries the level financial awareness and sophistication is comparatively high. Where rationing is widespread due to regulation and judicial inefficiency, borrowers are not able to realize their unconstrained demand, and shifts in the household tax regime might have limited impact on observed behavior. And where financial sophistication is relatively low, they may not respond to tax changes because they are not sufficiently informed. The tax reform could therefore prove useful also as an indirect test of the prevalence of credit rationing in Italian mortgage markets and of the degree of financial awareness.

The paper has six more sections. Section 2 reviews previous studies on tax incentives to borrow, with particular reference to the US 1986 tax reform and the phasing out of the MIRAS program in the UK. Section 3 provides institutional background on the Italian mortgage market and explains how the 1992–1994 reforms affected the tax treatment of mortgage interests. Section 4 presents the data used in the empirical analysis, and Section 5 the econometric results. There turns out to be no detectable effect of the tax reforms on the demand for mortgage debt or the amount borrowed. In Section 6 we check whether these results can be explained by borrowing constraints or lack of information. We find some evidence that both elements are potentially important. Section 7 concludes.

2. International evidence

In many countries the tax code gives preferential treatment to mortgages, as part of broader government intervention to encourage homeownership. One of the most compelling reasons that justify these incentives is to shift the allocation of wealth towards goods to which society assigns an important weight in creating positive externalities and raising living conditions, much like targeting retirement saving is a remedy to household myopia and potential free-riding problems. On the other hand, critics of the mortgage interests deduction stress that it generates loss of tax revenues, low geographical mobility

(hence less efficient reallocation of labor in response to shocks), and a number of other spatial and economic inefficiencies, see Bourassa and Grigsby (2000) for an assessment of pros and cons.

In this paper we take the system of incentives as given and do not address important questions such as whether the tax code should favor housing in the first place, whether incentives for homeownership should be higher than those for renting, and how homeownership could be promoted. We also ignore general equilibrium issues that have to do with tax incidence.

Poterba (2002) provides international evidence on the type of borrowing incentives for house purchase that exist in nine OECD countries; see also Green and Wachter (2005) for a recent evaluation of international differences in mortgage markets. Three countries, the US, the Netherlands, and France, allow relatively unrestricted deductions for mortgage interests, and a fourth, Italy, allows mortgage interests deductions for first-time homeowners only.[1] In Japan taxpayers are not allowed to deduct mortgage interest payments, but enjoy a special tax credit for first-time home purchase, subject to a time limit.

Historically, the UK featured one of the most generous mortgage incentives programs (MIRAS, or *Mortgage Interest Relief at Source*). Over time, the treatment of mortgage interest was subject to considerable changes. Before 1983, the interest on the first £30,000 of a mortgage was deductible from taxable income. In April 1983, the MIRAS scheme was introduced and initially provided two sources of variability in the after-tax mortgage interest rate. Under MIRAS, a borrower paid the lender the interest less the tax relief, initially equal to the marginal tax rate. Moreover, until 1988 the £30,000 limit applied on single mortgagers rather than the property, so married people could each receive relief on loans up to £30,000, including more than one on the same property.

The MIRAS scheme was criticized as a strongly distortionary measure introducing a bias in favor of owner-occupation and in favor of higher income households who are more likely to finance large houses. Moreover, the relief was believed to result in higher house prices, which prevented new homebuyers from gaining fully from it. This led to several reductions in the relief rate that culminated with the phasing out of MIRAS in April 2000.[2] As we shall see, some of the developments in MIRAS parallel the series of reforms in the tax treatment of mortgage interests in Italy.

Another important tax change that affected household liabilities was the US Tax Reform Act of 1986. That reform phased out deductions for interest on consumer credit— on the ground that they provided an incentive to invest in consumer durables rather than assets, which produce taxable income—while maintaining the residential mortgage interest deduction—on the ground that homeownership is an important policy goal. In short, the tax reform increased the price of borrowing through mortgages relative to consumer debt. Since there was no restriction on the use of home equity debt, homeowners were given an incentive to shift from consumer debt into mortgage debt. The incentive was higher for

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[1]In the US households can deduct interest up to a limit of $1,000,000 of mortgage debt, but in practice this constraint rarely binds.

[2]Hendershott et al. (2003) use a sample of loans originated in the UK to predict the probability that a loan exceeds the £30,000 limit. These probabilities are used to construct debt tax penalty variables that are then related to the loan-to-value ratio to finance home purchase. The authors find that the removal of deductibility has reduced the initial loan-to-value ratios of unconstrained borrowers by 30 percent.

high-income households, who are subject to high marginal tax rates and more likely to itemize deductions.

The 1986 reform stimulated some empirical studies on the effect of changing the after-tax borrowing rate on consumer credit (Engen and Gale, 1996; Skinner and Feenberg, 1990; Scholz, 1994). The common finding of these studies is that household debt composition is sensitive to the tax treatment of different types of debt. Maki (2001) provides the most recent and careful analysis of the effect of the change in tax treatment of consumer credit on the demand for mortgages. Using data from the Consumer Expenditure Survey he concludes that after the reform high-income homeowners reduced consumer interest paid and increased mortgage interests paid relative to other households. On the other hand, high-income earners who were not homeowners and had therefore no access to home equity borrowing, did not reduce their consumer interest paid relative to other renters.

3. The reform of the tax treatment of mortgage interests

Historically, the Italian mortgage market has been limited by regulation, judicial inefficiency and high enforcement costs. Chiuri and Jappelli (2003) document that the cost of mortgage foreclosure, the length of trials, and judicial inefficiency in Italy are higher than in countries at a similar level of financial development. A further reason for the relatively thin mortgage market is the presence of informal arrangements and various forms of intergenerational transfers (bequests, *inter vivos* transfers, help for down payment or outright purchase, free housing or co-residency), partly overcoming borrowing constraints and reducing the need for mortgage credit. Casolaro et al. (2005) also stress that, compared to other countries, Italy features a lower level of social capital and trust, effecting real and financial transactions.

Despite the fact that the Italian mortgage market is still small by international standards, the process of European financial liberalization has spurred increasing competitive pressure, reducing the cost of debt and increasing the supply of loans. In fact, the household debt-GDP ratio increased from 9 percent in 1985 to almost 20 percent in 2002. National regulatory changes also played an important role, with the removal in 1990 of regulations on entry, limitations of geographical span of lending, and separation of long and short-term lending, see Casolaro et al. (2005). Specific mortgage regulation has eased considerably, and loan maturities and loan-to-value ratios have gradually increased, from 50 percent before 1986 to 80% in 1995.[3] The development of credit reporting system and of credit scoring techniques in the early 1990s has improved the quality of information on prospective borrowers, benefiting mortgage market performance (Jappelli and Pagano, 2002).

After 1992, rent controls, in place since 1978, were gradually removed. Removal of such controls is usually thought to encourage new rental housing construction and to create incentives for the conversion of housing from owner to rental occupancy status, to reduce rental housing searching costs, and to discourage the demand for owning a primary

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[3]Before 1986 the maximum statutory Loan-To-Value (LTV) ratio was 50 percent. In 1986 the LTV ratio for first-time-buyers was raised to 75 percent; the limit for repeat buyers was still 50 percent. In April 1995 the maximum LTV was further raised to 80 percent. However, average LTV ratios remain still low by international standards.

residence relative to renting (Arnott, 1998). However, the removal was gradual, as it affected only new contracts, and owner occupancy rates continued to increase during the nineties, from 60 to 67 percent.[4]

In this paper we do not attempt at explaining the process of regulatory and institutional reforms and the broad trends of the Italian mortgage market. Rather, given the institutional setting and existing constraints, we study how borrowers have reacted to the important tax reforms in the treatment of mortgage debt. In the empirical estimates, however, we will control explicitly for institutional and regulatory changes affecting mortgage markets during the sample period. Nevertheless, it should be kept in mind that, contrary to previous studies investigating tax reforms in the US and the UK, the test is performed in a sample in which borrowers are likely to be prevented from borrowing the desired amount, given regulatory constraints and the cost of recovering collateral in case of default.

Table 1 summarizes the series of tax reforms that are the object of our analysis. Until 1992 the tax code featured substantial incentives for mortgage loans. Mortgage interests up to 3500 Euro/year were made deductible from the borrower's general income tax base. Eligibility applied to mortgages on the principal residence only, and to mortgages incurred not only for home purchase, but also repairs, additions and new constructions (in these cases interests up to about 2000 Euro were deductible).

In the pre-1992 tax regime the price of mortgage debt (i.e., the average cost of debt) was $r(1 - \tau_i \phi_i)$, where r denotes the mortgage interest rate, τ_i taxpayer's i marginal tax rate, and ϕ_i household's i fraction of deductible interests: 100 percent if mortgage interest payments did not exceed $(1 + M_i)L$ Euro (where M_i is a dummy for multiple taxpayers co-signing the contract and $L = 3500$ Euro), and the limit-interest payments ratio otherwise. Therefore,

$$\phi_i = \min\left\{1, (1 + M_i)\frac{L}{P_i}\right\},$$

where P_i denotes mortgage interest payments. The incentive to borrow was therefore broadly comparable to the UK MIRAS scheme in the 1980s, where interests on the first £30,000 of the mortgage were fully tax deductible.[5] In practice, almost 90 percent of borrowers could deduct all interest payments.[6]

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[4]The possible confounding effect of the lifting of rent controls, however, is more important for poor than for rich taxpayers. For the latter, in fact, the disincentive effect of the tax reforms should be exacerbated by the effect of the lifting of rent-controls. As we shall see, both groups appear insensitive to the tax reforms, implying this is unlikely to be a serious bias.

[5]In the 1980s MIRAS allowed tax deductibility of the mortgage interests paid on the first £30,000 of a loan. With an interest rate of 8.75 percent, people could deduct mortgage interests of £2625, which roughly corresponds to the Italian limit of 3500 euro.

[6]The *marginal* cost of debt is a function of the limit. If outstanding mortgage debt is at or below the limit, the marginal cost coincides with the average cost; otherwise, it equals the mortgage interest rate. In principle, the distinction between average and marginal cost induced by the presence of the limit may be important. For instance, one could argue that the decision to borrow depends on the average cost, while that of how much to borrow depends on the marginal cost of debt. In practice, 89 percent of borrowers in the sample can deduct all mortgage interest. Indeed, 84 percent of borrowers fall below the 3500 Euro limit; and 5 percent are multiple earners who qualify with interest payments below 7000 Euro. Furthermore, our difference-in-differences strategy would still allocate households to treatment or control groups based on marginal tax rates, which is the correct procedure if the proportion of borrowers above the limit within each tax bracket is not too different before and after the reform.

Table 1
The reform of mortgage interests deduction

	Price of mortgage debt	Limit	Joint contracts
1982–1992	$r(1-\tau\phi)$	$L \leqslant 3500$ Euro	The 3500 limit on mortgage interests applies to each tax-payer co-signing the contract
1993–1994	$r(1-0.27\phi)$	$L \leqslant 3500$ Euro	For contracts signed before 1993, the 3500-euro limit applies to each taxpayer co-signing the contract. For contracts signed after 1993, the 3500 limit applies to the interests paid on the mortgage
1995–1997	$r(1-0.22\phi)$	$L \leqslant 3500$	
1998–2002	$r(1-0.19\phi)$	$L \leqslant 3500$	

Note: r denotes the mortgage interest rate, τ the marginal tax rate, and ϕ the fraction of deductible interests (see the text for more details on how this is calculated).

Table 2
Tax brackets and marginal tax rates, 1989–2002

1989		1991		1993–1995	
Tax bracket	Marginal tax rate	Tax bracket	Marginal tax rate	Tax bracket	Marginal tax rate
$\leqslant 3.30$	0.10	$\leqslant 3.51$	0.10	$\leqslant 3.72$	0.10
3.30–6.56	0.22	3.51–6.97	0.22	3.72–7.44	0.22
6.56–16.43	0.26	6.97–17.41	0.26	7.44–15.50	0.27
16.43–32.90	0.33	17.41–34.92	0.33	15.50–30.99	0.34
32.90–82.18	0.40	34.92–87.19	0.40	30.99–77.48	0.41
82.18–164.41	0.45	87.19–174.4	0.45	77.48–154.96	0.46
>164.41	0.50	>174.4	0.50	>154.96	0.51

1998		2000		2002	
$\leqslant 7.75$	0.19	$\leqslant 10.33$	0.185	$\leqslant 10.33$	0.18
7.75–15.49	0.27	7.75–15.49	0.255	7.75–15.49	0.24
15.49–30.98	0.34	15.49–30.98	0.335	15.49–30.98	0.32
30.98–69.72	0.40	30.98–69.72	0.335	30.98–69.72	0.39
>69.72	0.46	>69.72	0.455	>69.72	0.45

Note: The table reports the tax brackets and the marginal tax rates from 1989 to 2002. Tax bracket figures are in thousand Euro.

In 1992 the tax incentive became a flat 27 percent of the interest paid, and the price of mortgage debt changed to $r(1 - 0.27\phi_i')$. With respect to the pre-1992 regime, the incentive to borrow was thus raised for investors with the lowest marginal tax rate, lowered for those with the highest marginal tax rate, and unaffected for those in the intermediate tax brackets ($\tau = 0.27$). The tax reform also envisioned different tax provisions for contracts signed before or after 1993. Before 1993 the 3500-Euro limit applied to each taxpayer co-signing the contract; after 1993 to the interest paid for the mortgage, $\phi_i' = \min\{1, L/P_i\}$.
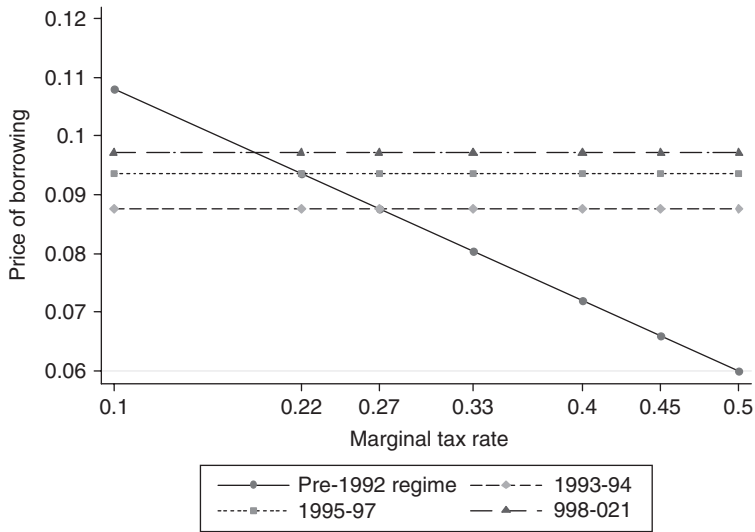
Fig. 1. The price of mortgages, 1989–2002. *Note.* We plot the after-tax interest rate on a mortgage for a borrower paying interests of 3500 Euro for 10 years. We assume that the mortgage is a pre-tax fixed-rate mortgage at the 12 percent interest rate.

The tax incentive was then further reduced to 22 percent in 1994 and 19 percent in 1998, with the limit still fixed, at least in nominal terms.[7]

The change in incentives to borrow induced by the 1992–1994 tax reforms was substantial, especially for borrowers in the top tax brackets. For reference, Table 2 reports the tax brackets and marginal tax rates in place for each of the years for which we have survey data. Several changes are worth noticing. In 1991 the tax brackets were indexed to inflation. In 1992 marginal tax rates were raised by one percentage point for all but the bottom two brackets. In 1998 the seven tax brackets were reduced to five with the elimination of the bottom and top marginal tax rates. Marginal tax rates were further reduced in 2000 and 2002.[8]

To illustrate these effects, in Fig. 1 we plot the yearly difference between the interest paid on a mortgage with no tax incentive and a tax-favored mortgage instrument for a borrower paying interests of 3500 Euro for 10 years (the typical duration of mortgage contracts in Italy over the sample period). We assume that the mortgage is a fixed-rate mortgage at the 12 percent interest rate, approximately the rate prevailing in 1992–1994. This corresponds to a mortgage loan of about 30,000 Euro.

In the pre-1992 regime the price of borrowing declines linearly with the marginal tax rate. A borrower with a marginal tax rate of 33 percent faces an after-tax borrowing rate of 8 percent; in the top bracket, the after-tax rate is 6 percent, and 11 percent in the bottom one. In 1992–1993 the tax deduction is a flat 945 Euro for all tax brackets (0.27×3500), so the after-tax rate is 8.8 percent, *regardless* of the tax rate. The 1994 reform reduced the

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[7]Due to high transaction costs, in Italy the market for mortgage refinancing is very small.

[8]The tax code also features minimum exempt income from labor. The minimum has gradually increased over time, but in practice it affects a negligible portion of our sample. For this reason, our imputation of marginal tax rates does not reflect this feature of the tax system.
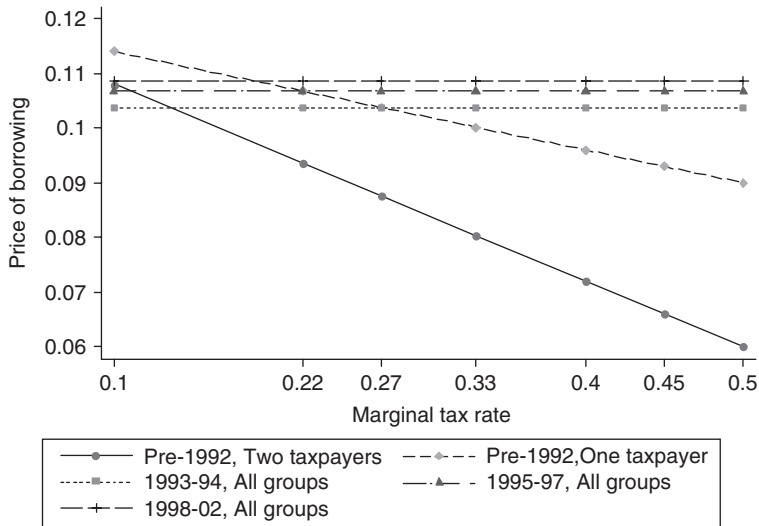
Fig. 2. The price of mortgages for one and two-income taxpayers, 1989–2002. *Note.* We plot the after-tax interest rate on a mortgage for a mortgage paying interests of 7000 Euro for 10 years, distinguishing between one- and two-taxpayers households. We assume that the mortgage is a pre-tax fixed-rate mortgage at the 12 percent interest rate.

deduction to 770 Euro (0.22 × 3500), further lowered to 665 Euro after 1998 (0.19 × 3500). In the example, the after-tax rate increases to 9.4 and 9.7, respectively.

In Fig. 2 we compare the effect of the reform on the after-tax interest rate for a single and two-income household paying interests of 7000 Euro. The graph shows that the change in the average price of borrowing induced by the reform depends also on the number of taxpayers in each household. The two lines for pre-1992 differ because two co-signers could deduct 7000 Euro; one co-signer 3500 Euro only. In 1993 in the top tax bracket the average price of borrowing increases by 4.38 percentage points for households with two taxpayers, and by 1.38 points for single taxpayers. In the lowest tax bracket, the price of borrowing declines by about half a percentage point for multiple taxpayers and by slightly more than one point for single taxpayers.

The elimination of the tax incentive for the rich and the greater incentive given to low-income households should have reduced the formers' propensity to borrow relatively to the latters' and relatively to the group of households in the intermediate tax brackets. Furthermore, among high-income taxpayers, the increase in the price of borrowing is higher for households with two or more income recipients—who lost the possibility of double tax deductions—than for single income households.

The reforms have therefore clear testable implications. We can exploit the fact that they affected different taxpayers in different ways. In particular, we can compare the behavior of groups facing changes in after-tax interest rates before and after the reform (treatment groups) with that of groups that face no changes (control groups), as in a standard difference-in-difference approach. In our case, and in the absence of credit constraints, both poor and rich taxpayers are subject to the treatment (albeit in opposite directions), while single households with intermediate marginal tax rates (for example, 0.27 in 1993 or 0.22 in 1995) face the same cost of borrowing before and after the reform, and so can act as

a control group. Section 5 provides details on the definition of treatment and control groups.

4. The data

The SHIW provides a unique opportunity to test the effect of the tax reform on the demand for mortgage loans. Conducted by the Bank of Italy in 1989, 1991, 1993, 1995, 1998, 2000 and 2002, it spans pre- and post-reform years. In each year it contains information on outstanding mortgage debt, income, and demographic variables. Each survey collects data on a representative sample of about 8000 households. Ideally, we would like to observe mortgage loans and pre-tax interest paid for the purchase of the principal residence. In practice we observe outstanding debt for all home purchases or repairs and do not observe interest paid. Since the tax incentive can only be claimed on mortgages on the principal residence, we drop households with second homes and other real estate (about 15 percent of the sample).[9] Our final sample includes over 47,000 observations.

To classify households according to mortgage incentives, we need to impute the marginal income tax rate. In estimating this tax rate one should consider that it might be affected by the structure of household portfolios, a problem pointed out by Poterba and Samwick (2003). Although in Italy income from most financial assets (such as bank deposits, mutual funds and government bonds) is subject to a flat rate with-holding tax, dividends and income from capital (e.g., rents) enter the general income tax base and therefore affect marginal tax rates. Most importantly, as we have pointed out, part or all of the mortgage interests can be deducted. Since interest payments appear with a negative sign in the definition of disposable income, the overall marginal tax rate itself might depend on the amount borrowed, generating spurious correlation between amount borrowed and the tax rate. We therefore estimate marginal tax rate only on the basis of labor income, excluding income from capital and interest income paid and received. We proxy the investor's marginal tax rate with that of the household head's labor income, using the tax brackets and marginal rates reported in Table 2. The SHIW collects data on after-tax wages, salaries, self-employment income, income from capital and income from financial assets so imputation of tax brackets is straightforward.

In Fig. 3 we plot the aggregate ratio of outstanding mortgages and household disposable income, drawn from the SHIW and aggregate financial accounts, respectively. Both series show an increasing trend in the debt-income ratio (mortgage debt doubles between 1989 and 2002). However, there are some discrepancies: the level of the two series differs, and the microeconomic data show a decline in borrowing in 1995–1998, whereas the aggregate statistics signal only less vigorous growth with respect to the previous period. This discrepancy is likely due to comparability problems. The financial accounts of the household sector do not provide a breakdown of total debt into mortgage debt and consumer credit on a consistent basis over time. The series plotted in Fig. 3 refers to total financial liabilities of households, unincorporated business with less than 5 employees and no-profit organizations over 12 months of maturity. In contrast, the SHIW data include

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[9]The econometric results are similar if we exclude the self-employed, whose debt is more likely to be business-related.
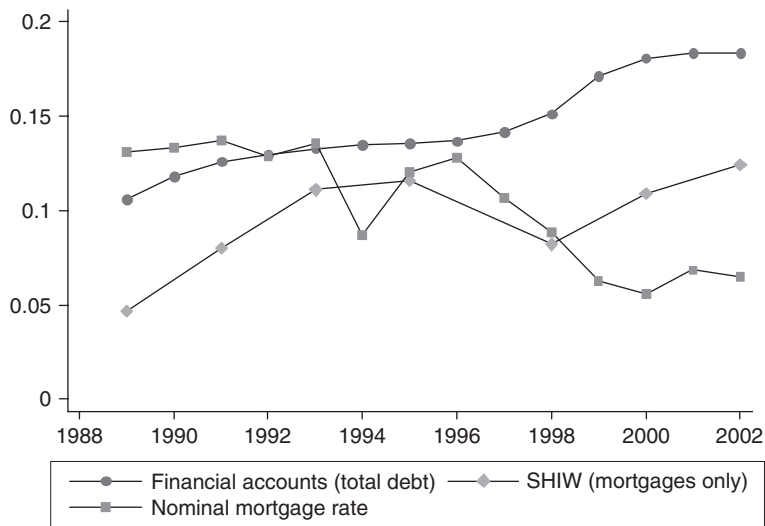
Fig. 3. The debt-income ratio and the mortgage rate, 1989–2002. *Note.* The figure plots the total debt–income ratio estimated from financial accounts data and the mortgage–income ratio estimated from the 1989–2002 SHIW. The nominal mortgage interest rate is drawn from Eurostat data (annual average).
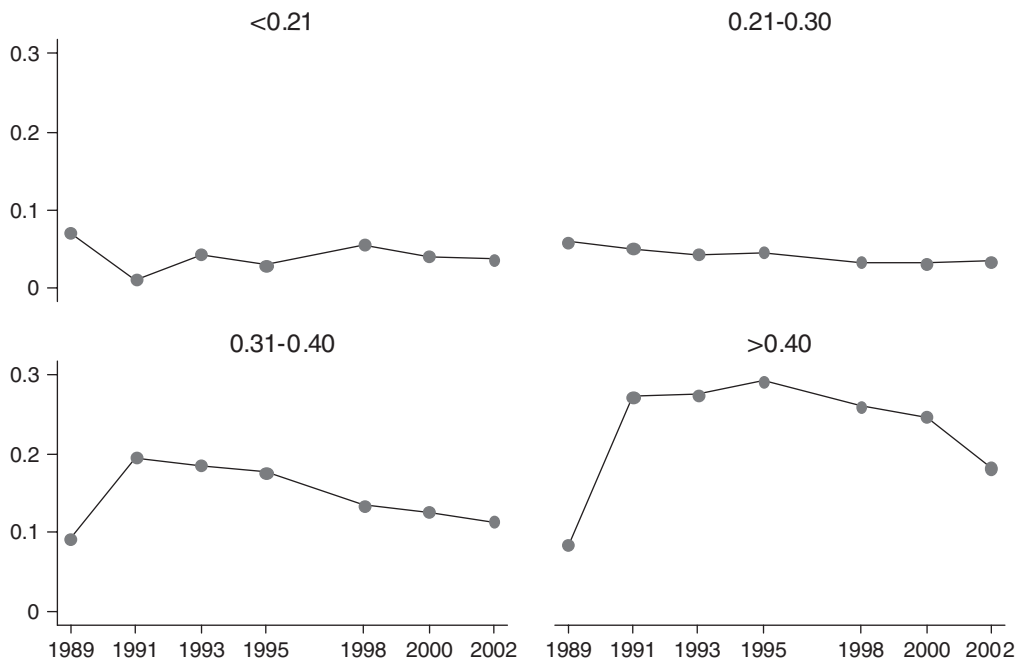

Fig. 4. The propensity to borrow, by marginal tax rates.

only household mortgage debt (rather than all liabilities), regardless of maturity. The positive trend in the debt-income ratio is consistent with the decline in mortgage interest rates, also plotted in Fig. 3. The interest rate is flat between 1989 and 1993
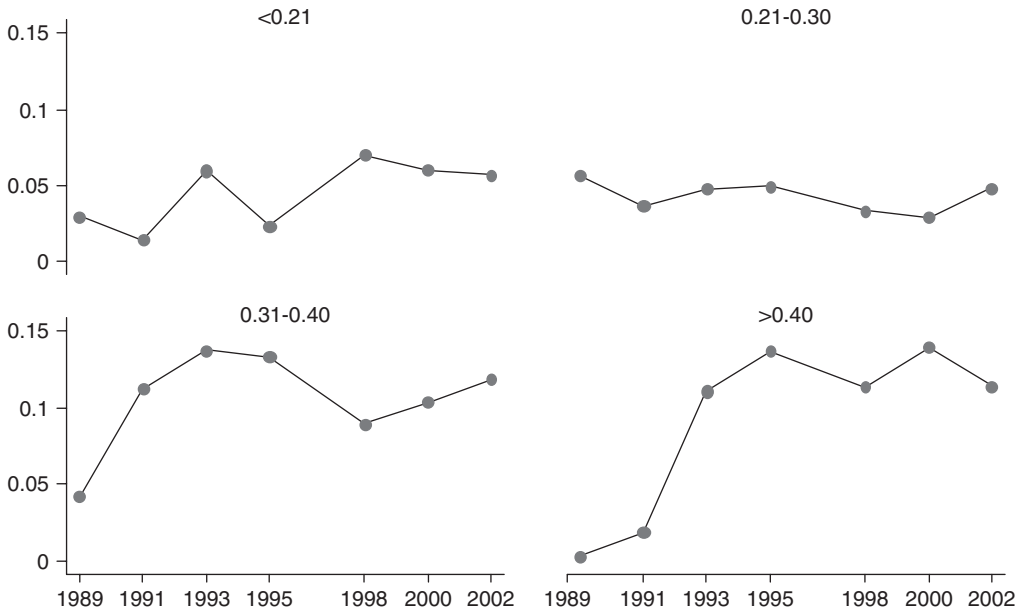
Fig. 5. The mortgage–income ratio, by marginal tax rates.

at around 12 percent and then halves over the following decade (6.3 percent in 2000–2002).[10]

Fig. 4 plots the proportion of borrowers in 1989–2002 by marginal income tax rates. The proportion shifts with the tax rate, indicating that the decision to borrow is correlated with income. For instance, in 2002 only 5 percent of those in the lowest tax brackets had a mortgage, compared with 19 percent in the highest brackets. There is no clear trend in the propensity to borrow in the lowest tax brackets, while for the highest brackets the fraction of households with a mortgage increases between 1989 and 1991, flattens out until 1995, and slightly declines afterwards. The debt–income ratios plotted in Fig. 5 displays similar trends for the lowest tax brackets, and an increase in borrowing for households with income tax rates higher than 0.3.[11]

As is explained in Section 3, the 1992–1994 reforms should have eliminated the sensitivity of the demand for mortgages with respect to the marginal tax rate. Figs. 4 and 5 indicate that mortgage debt is lower for the poor and higher for the rich, and that the rich experienced a strong increase in the propensity to borrow and in the amount borrowed before 1995, and a much slower increase—or even a decline—afterwards. Even though this seems to be in line with some of the features of the reform, it cannot be taken as evidence either for or against the hypothesis that taxation affects borrowing, because different

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[10]The real interest rate on mortgages declined from 6.9 percent in 1989–1993 to 3.4 percent in 2000–2002. Refinancing is not allowed, and thus only new mortgages are affected by the decline in the interest rates.

[11]Analyzing the amount borrowed as well as the decision to borrow is important if households respond to the increase in the price of borrowing induced by the reform buying smaller housing units, rather than reducing the frequency of mortgage loans.

socioeconomic characteristics across groups and group-specific trends could mask the effect of the reform and/or induce spurious correlation in the data.

In the remaining of the paper we use a difference-in-difference estimator and regression analysis to test the hypothesis that the propensity to borrow of the poor has increased faster than that of the rich after the reform and that, for any given marginal tax rate, multiple income households have reduced the propensity to borrow more than single income households.

5. Empirical results

5.1. Difference-in-difference estimates

Since marginal tax rates are a function of a household's taxable income, which in turn is a function of income from assets, one cannot disentangle income effects from tax rate effects in the cross-section (see Poterba and Samwick, 2003, for an extensive discussion). The 1992–1994 Italian tax reforms allow us to tackle this identification problem directly by providing exogenous time-series variation in the after-tax rate. This suggests a simple difference-in-difference strategy.

The standard difference-in-difference estimator requires distinguishing between individuals affected by the reform (the treatment group) and those unaffected (the control group). Consistently with the features of the reform, we assume that intermediate taxpayers are not affected by it (and thus constitute our control group), while people in the high (low) marginal tax rate group face an exogenous increase (decrease) in the after-tax borrowing rate, and so they should reduce (increase) their demand for mortgage debt relatively to people in the control group. This makes people with high and low marginal tax rates qualify as treatment groups; the availability of multiple treatment groups facing price changes of different sign adds further interest to our empirical exercise. Below, we illustrate the difference-in-difference estimator in relation to the effect of the tax reform on the amount borrowed by the group of rich taxpayers.

Denote by $b_{i,j,t}$ mortgage debt of borrower i in group j ($j = 0$ denote the control group, $j = 1$ the treatment group) in period t ($t = 0$ denote the period before the reform, $t = 1$ after the reform). Between period 0 and 1 a tax reform takes place that changes borrowers' tax incentive in the treatment group. For instance, the 1992–1994 reforms affect the treatment group of rich tax-payers (with a marginal tax rate greater than 0.22) by eliminating the link between after-tax returns and marginal tax rates, but not the control group (those with a marginal tax rate equal to 0.22).

We assume that the demand for mortgage debt is

$$b_{i,j,t} = \beta_t + f_j + \delta \times \mathbf{1}\{j = 1, \ t = 1\} + v_{i,j,t}$$

for $j = \{0,1\}$, $t = \{0,1\}$, and where $\mathbf{1}\{.\}$ is an indicator function that equals 1 if the statement in bracket is true and zero otherwise. Both groups are subject to an aggregate shock β_t. Long-term differences between groups are captured by the fixed effect f_j. We assume that $v_{i,j,t}$ is an i.i.d. error term, so in the absence of such differences across groups, the average debt is equal for all j. According to this specification, the reform affects mortgage debt by an amount δ in the treatment group. Given this structure, one can

identify the effect of the reform using the difference-in-difference estimator:

$$E(b_{i,1,1} - b_{i,1,0}) - E(b_{i,0,1} - b_{i,0,0}) = \delta.$$

The identifying assumption, then, is that controlling for group and time effects, the error term v has mean zero. Note that panel data are not required to compute the conditional means that form the basis of the difference-in-difference estimator. What we need to observe is a representative sample of the two groups $j = 0$ and 1 in each of the two periods $t = 0$ and 1. For our purposes therefore one can rely on repeated cross-sectional data.

Since time effects are common to both groups, in the pre-reform period the control and treatment groups differ only in long run fixed effects, $(f_1 - f_0)$. Thus, the model is consistent with the fact that high-income borrowers behave differently than those in low tax brackets, regardless of policy interventions. In the post-reform period the treatment group now differs not only because of fixed effects, but also because of the tax reform $(f_1 - f_0 + \delta)$.

A finding that $\delta < 0$ signals that the reform has reduced the propensity of rich tax-payers to borrow relatively to the control group. By appropriately redefining the variable b or the treatment group, one can readily extend this framework to examine the propensity to borrow rather than the amount borrowed and the separate behavior of the low-income group after the reform (where theory suggests $\delta > 0$).

The validity of the difference-in-difference estimator rests on two assumptions: (1) the tax reform is exogenous with respect to the demand for debt; (2) it is exogenous with respect to changes in sample composition.

As far as assumption (1) is concerned, we believe that the possible endogeneity of the reform can be safely ruled out. The 1992 reform was not implemented in order to offset the different paths of borrowing by taxpayer groups (if this had been the case, there would be a problem of policy endogeneity). Rather, the reform was part of a major deficit-reduction package, prompted by a severe political crisis coupled with the dramatic devaluation of the lira; and it was followed shortly by the deepest recession of the post-war era.[12]

Assumption (2) posits that shifts in sample composition are exogenous with respect to the decision to borrow and to the amount borrowed. In essence, we require that movements across the tax distribution (into higher or lower brackets) are independent of borrowing decisions, i.e. that borrowers did not move within the income distribution as a result of the tax reform itself. Before 1992 for people at the margin a tax deduction of 3500 Euro may actually change the relevant tax bracket. On this front, we present evidence that our results are robust to possible tax bracket shifts induced by the reform.

To introduce the difference-in-difference results, Table 3 reports sample statistics for households with and without a home mortgage in the pooled 1989–2002 sample (excluding the intermediate 1993 year), a total of 40,806 observations. On average, people with a mortgage are younger, more educated, more likely to have children and being married, and their family income is higher. Average outstanding mortgage debt, conditional on having a mortgage, is 18,720 Euro. Within tax brackets the sample composition (by age, education,

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[12]The 1992 reform of the tax treatment of mortgage interest payments mirrors, in structure and timing, the reform of the tax incentives to life insurance. In previous work we analyzed the effect of this reform and found that it had no impact on the decision to purchase life insurance or on the contribution rate (Jappelli and Pistaferri, 2003). We considered as likely explanations minimum investment requirements, borrowing constraints, lack of commitment to long-term saving and insufficient financial information on the changes in tax incentives.

Table 3
Sample statistics

Variable	With mortgage	No mortgage	Total sample
Age	45.01	54.70	53.85
Years of schooling	10.14	7.76	7.97
Family size	3.30	2.75	2.80
Self-employed	0.18	0.13	0.13
More than one taxpayer	0.66	0.51	0.53
Resident in the South	0.32	0.44	0.43
Disposable income	23.26	16.93	17.49
Mortgage debt	18.72	0	1.65
Observations	3,741	37,065	40,806

Note: The table reports sample statistics from the 1989–2002 SHIW (excluding 1993). Statistics are computed using sample weights. Income and mortgage debt are expressed in thousand Euro (2000 values).

and family size) is stable over time. We also check the sensitivity of the results using group specific trends in the regression analysis of Section 5.2.

In Table 4 we report separate estimates for the effect of the 1992–1994 reforms on the decision to borrow (Panel A) and the amount borrowed (Panel B). The pre-reform period is 1989–1991, the post-reform period is 1995–2002. The transitional year 1993 is omitted. Ideally one would like to single out a control group that is clearly and cleanly unaffected by the reform. In practice the distribution of marginal tax rates in Table 2 shifts over time, and one can define a control group only with some degree of approximation. We choose to define four tax-payers groups: marginal tax rates less than 22 percent (low-income taxpayers), 22–27 percent (the control group), 28–39 percent (medium-income taxpayers), and 40 percent or more (high-income taxpayers). Similar results are obtained if we collapse medium- and high-income taxpayers into a single treatment group.

Panel A of Table 4 shows that the difference-in-difference estimates for the propensity to borrow are positive in all groups, regardless of marginal tax rate. In the low-income group, for instance, the difference-in-difference estimate is 0.4 percentage points (although statistically insignificant), indicating that this group increased participation slightly more than those who were not affected by the reform. However, the results for the medium- and high-income group (1.5 and 8.6 percentage points) are not consistent with the hypothesis that the group of medium- and high-income taxpayers should have reduced participation relative to the control group. Panel B documents that the amount borrowed declines for low-income taxpayers and increases among high-income taxpayers.

We know from Section 3 that before 1993 the 3500 Euro limit applied to each taxpayer co-signing the contract, and that the reform eliminated the possibility of interest deductions for mortgage co-signers. This means that the effect of the reform should be different for single-vs. multiple-income households. In particular, among low-income households, the effect of the reform should be positive for both single and multiple income households, but the former should respond more. Vice versa, among high-income taxpayers, the effect of the reforms should be negative for both single and multiple income households, but the former should respond less. Using the notation above, this further feature of the reform implies $\delta_{single} - \delta_{multiple} > 0$ regardless of tax group.

Table 4
Difference-in-difference results

	Low-income taxpayers	Control group	Between-group difference	Medium-income taxpayers	Control group	Between-group difference	High-income taxpayers	Control group	*Between-group difference*
Panel A: Proportion of borrowers									
After the reform	0.043 (0.003)	0.039 (0.003)	0.004 (0.004)	0.129 (0.003)	0.039 (0.003)	0.089 (0.004)	0.234 (0.010)	0.039 (0.003)	0.195 (0.010)
Before the reform	0.052 (0.014)	0.053 (0.002)	0.000 (0.014)	0.127 (0.005)	0.053 (0.002)	0.074 (0.006)	0.162 (0.017)	0.053 (0.002)	0.109 (0.017)
Within-group difference	−0.010 (0.014)	−0.014 (0.003)		0.002 (0.006)	−0.014 (0.003)		0.073 (0.019)	−0.014 (0.003)	
	Difference-in-difference: 0.004 (0.015)			Difference-in-difference: 0.015 (0.007)			Difference-in-difference: 0.086 (0.020)		
Panel B: Debt amount									
After the reform	18.25 (1.88)	16.29 (0.76)	1.97 (2.03)	23.85 (0.60)	16.29 (0.76)	7.56 (0.97)	24.76 (1.25)	16.29 (0.76)	8.48 (1.46)
Before the reform	19.43 (7.47)	7.63 (0.55)	11.80 (7.49)	9.64 (1.11)	7.63 (0.55)	2.01 (1.24)	13.93 (2.63)	7.63 (0.55)	6.30 (2.68)
Within-group difference	−1.18 (7.70)	8.66 (0.94)		14.21 (1.26)	8.66 (0.94)		10.84 (2.91)	8.66 (0.94)	
	Difference-in-difference: −9.83 (7.76)			Difference-in-difference: 5.56 (1.57)			Difference-in-difference: 2.18 (3.06)		

Note: Panel A reports difference-in-difference results for the proportion of borrowers. Panel B reports difference-in-difference results for the amount of debt (in thousand Euro), conditioning on having a mortgage. In each panel, results are separately reported for three treatment groups: low-income taxpayers ($\tau < 0.22$), Medium-income taxpayers ($0.27 < \tau < 0.4$), and High-income taxpayers ($\tau \geqslant 0.40$). In all cases the control group includes households with $0.22 \leqslant \tau \leqslant 0.27$. The pre-reform period is 1989–1991, and the post-reform period is 1995–2002. Standard errors are reported in parenthesis.

Table 5
Difference-in-difference results: Single and multiple taxpayers

	Treatment group		Difference-in-difference estimate	Standard error	No difference within tax group (test p-value)
	Tax group	Number of taxpayers			
Proportion of borrowers	$\tau < 0.22$	Single	0.031	0.017	
		Multiple	−0.010	0.024	0.08
	$0.22 \leqslant \tau \leqslant 0.27$	Single	Control group		
		Multiple	0.011	0.007	—
	$0.27 < \tau < 0.4$	Single	0.005	0.009	
		Multiple	0.028	0.010	0.96
	$\tau \geqslant 0.40$	Single	0.101	0.026	
		Multiple	0.070	0.028	0.21
Debt amount	$\tau < 0.22$	Single	−10.80	11.96	
		Multiple	−8.63	10.12	0.56
	$0.22 \leqslant \tau \leqslant 0.27$	Single	Control group		
		Multiple	0.77	1.95	—
	$0.27 < \tau < 0.4$	Single	2.71	2.22	
		Multiple	7.78	2.36	0.94
	$\tau \geqslant 0.40$	Single	2.49	4.84	
		Multiple	3.12	3.99	0.54

Note: The table reports difference-in-difference estimates for the propensity to borrow and the debt amount (in thousand Euro), conditioning on having a mortgage. The pre-reform period is 1989–1991 and the post-reform period is 1995–2002. The control group consists of single taxpayer households with $0.22 \leqslant \tau \leqslant 0.27$. The last column reports a test of the null hypothesis that there is no difference between single and multiple taxpayer households within a given tax group, against the alternative that the effect is lower among multiple taxpayer households.

To address these issues we redefine our control group to include only single taxpayer households with $0.22 \leqslant \tau \leqslant 0.27$, create a new treatment group (multiple taxpayers with $0.22 \leqslant \tau \leqslant 0.27$, where we would expect the effect of the tax reform to be negative), and separate the three treatment groups of Table 3 (with $\tau < 0.22$, $0.27 < \tau < 0.40$, and $\tau \geqslant 0.40$) into single- and multiple-taxpayers households. To check whether the effect of the reform is different for single and multiple income households, we assume sampling independence and test the null hypothesis of no difference between groups against the alternative that the difference is consistent with the theory of portfolio taxation.

The results, reported in Table 5, are qualitatively similar to those obtained ignoring the change in the deductibility limit. There is an across-the-board increase in the proportion of mortgage borrowers that is at variance with the theory's predictions. Moreover, the amount borrowed declines among the poor and increases among the rich, again in contrast with the theory. We test the hypothesis that, for any given tax group, the effect of the reform is the same for households with single and multiple earners, against the alternative that the difference is consistent with the theory of portfolio taxation. Results for the propensity to borrow and for the amount borrowed, reported in the last column, do not

signal group-specific different reactions to the reform in the direction expected by the theory.

There are several reasons why the difference-in-difference estimates may not pin down the effect of the tax reform. First, the effect could be diluted because other determinants of borrowing induce different behavior across groups: one possibility is that there are events, other than tax reforms, that provide alternative explanations for the results. Second, trends in outcomes specific to groups may produce changes as a function of time per se, not of the tax reform. By the same token, differential trends in treatment and control groups that change in different ways for treatment and control group (for instance, a time trend in the treatment group that is not present in the control group) may be responsible for the results. Third, we have assumed that there are no changes of the supply side of the mortgage market. Finally, we have not contemplated the potential impact of borrowing constraints. Some households (especially those in the lowest tax brackets) may be denied loans and have no access to credit. Moreover, taxes affect borrowing only if potential borrowers are informed about the tax reform.

In the next section we turn to Probit and Tobit analysis for the decision to borrow and the amount borrowed, controlling for household and housing characteristics, group-specific trends in outcomes. To shed further light on borrowers' behavior, in Section 6 we estimate the potential impact of the reform splitting the sample with indicators of borrowing constraints and financial sophistication.

5.2. Regression results

To translate the difference-in-difference approach into a regression equation, we consider two time periods, $t = 0$ and 1, and k groups of borrowers affected or unaffected by the reforms. The demand for mortgage debt is

$$b_{i,j,t} = X'_{i,j,t}\theta + u_{i,j,t}$$

for $j = 0$ (the control group), $1, \ldots, k$ (the treatment groups). The term $u_{i,j,t}$ captures variability in the demand for mortgage debt not explained by observable demographic and socioeconomic characteristics $X_{i,j,t}$. We assume that

$$u_{i,j,t} = \beta_t + f_j + \delta_j \times \mathbf{1}\{j > 0, \ t = 1\} + v_{i,j,t}.$$

The reduced form demand for mortgage debt can be written as

$$b_{i,j,t} = (\beta_0 + f_0) + X_{i,j,t}\theta + (\beta_1 - \beta_0)T + \sum_{j=1}^{k}(f_j - f_0)G_j + \sum_{j=1}^{k}\delta_j G_j T + v_{i,j,t}. \quad (1)$$

The group dummies G_j and the time dummy T measure, respectively, permanent differences between groups and shifts due to common time effects. The interaction terms $G_j T$ identify the impact of the reform ($\delta_j < 0$ for rich taxpayers and $\delta_j > 0$ for poor taxpayers). In contrast to the difference-in-difference approach, this framework allows us to consider additional explanatory variables that affect mortgage debt and to control for group-specific time trends.

The specification (1) neglects the effect of the change in the deductibility limit that affects differently single- and multiple-taxpayer households. Denote with $n = \{s, m\}$ an indicator for single- and multiple-taxpayer households. Re-define the control group to include only

Table 6
Regression results

	Probit regressions		Tobit regressions	
$\tau < 0.22$	0.017 (0.023)	0.009 (0.021)	2.143 (2.213)	1.256 (2.211)
$0.27 < \tau < 0.40$	0.031 (0.005)**	0.043 (0.005)**	3.416 (0.546)**	4.733 (0.567)**
$\tau \geqslant 0.40$	0.047 (0.013)**	0.084 (0.017)**	4.349 (1.061)**	7.096 (1.097)**
$\tau < 0.22$, after the reform	−0.009 (0.019)		−1.485 (2.322)	
$0.27 < \tau < 0.40$, after the reform	0.029 (0.007)**		3.170 (0.722)**	
$\tau \geqslant 0.40$, after the reform	0.066 (0.017)**		5.766 (1.198)**	
Multiple income household		0.032 (0.005)**		3.730 (0.565)**
$\tau < 0.22$, after the reform, single		0.018 (0.026)		1.643 (2.496)
$\tau < 0.22$, after the reform, multiple		0.013 (0.025)		1.122 (2.504)
$\tau = 0.22$, after the reform, multiple		0.028 (0.012)*		3.071 (1.071)**
$0.27 < \tau < 0.40$, after the reform, single		0.057 (0.013)**		5.608 (1.033)**
$0.27 < \tau < 0.40$, after the reform, multiple		0.049 (0.013)**		5.055 (1.138)**
$\tau \geqslant 0.40$, after the reform, single		0.096 (0.025)**		7.954 (1.523)**
$\tau \geqslant 0.40$, after the reform, multiple		0.083 (0.023)**		6.933 (1.507)**
Age	−0.002 (0.000)**	−0.002 (0.000)**	−0.245 (0.012)**	−0.251 (0.012)**
Years of education	0.002 (0.000)**	0.002 (0.000)**	0.222 (0.037)**	0.237 (0.037)**
Family size	0.013 (0.001)**	0.011 (0.001)**	1.445 (0.119)**	1.277 (0.120)**
Self-employed	−0.007 (0.003)*	−0.007 (0.003)*	−0.702 (0.375)	−0.691 (0.374)
Second income quintile	0.022 (0.006)**	0.011 (0.006)	2.300 (0.613)**	1.185 (0.632)
Third income quintile	0.030 (0.007)**	0.003 (0.006)	3.138 (0.631)**	0.356 (0.727)
Fourth income quintile	0.039 (0.007)**	0.001 (0.007)	3.990 (0.676)**	0.094 (0.793)
Fifth income quintile	0.044 (0.009)**	−0.004 (0.008)	4.425 (0.755)**	−0.475 (0.892)
Resident in the South	−0.002 (0.004)	0.001 (0.004)	−0.182 (0.482)	0.149 (0.481)
Log of the stock of pending trials	−0.023 (0.004)**	−0.023 (0.004)**	−2.693 (0.447)**	−2.741 (0.447)**
Observations	40806	40806	40806	40806

Note: Each regression also includes a full set of year dummies. The sample period is 1989–2002 (excluding 2003). The Probit coefficients indicate marginal effects; standard errors of the coefficients are reported in parenthesis. One and two stars indicate that the coefficient is statistically different from zero at the 5 and 1 percent levels, respectively.

single-taxpayer households, so that the demand shift for this group after the reform is $\delta_{0,s} \equiv 0$. The reduced form demand for mortgage debt can now be written as

$$b_{i,j,n,s} = (\beta_0 + f_0 + \gamma_s) + X'_{i,j,n,t}\theta + (\beta_1 - \beta_0)T + \sum_{j=1}^{k}(f_j - f_0)G_j + (\gamma_m - \gamma_s)D_m$$

$$+ \sum_{j=0}^{k}\sum_{n=s,m}\delta_{j,n}G_jD_nT + v_{i,j,n,t}, \tag{2}$$

where the dummies D_m and $D_s = 1 - D_m$ are for single- and multiple-taxpayer households, respectively. The parameters γ_s and γ_m measure long-run differences in the demand for mortgage debt of single- vs. multiple-taxpayers. The interaction terms G_jD_nT identify the effect of the reform ($\delta_{j,n} > 0$ for poor taxpayers, $\delta_{j,n} < 0$ for rich taxpayers, and $\delta_{j,m} < \delta_{j,s}$ for all tax groups).

Table 6 reports Probit results for the probability of having debt and Tobit results for the (log of the) amount borrowed. We present the results of the two specifications (1) and (2). In both cases, we control for a basic set of socio-economic characteristics (age, region of

Table 7
Probit estimates for recent homebuyers, young households, and omitting 1993–1995

	Recent homebuyers	Young households	Omitting 1993-1995
$\tau < 0.22$, after the reform	−0.149 (0.097)	0.019 (0.064)	−0.007 (0.018)
$0.27 < \tau < 0.40$, after the reform	0.097 (0.043)*	0.044 (0.019)*	0.022 (0.008)**
$\tau \geqslant 0.40$, after the reform	0.143 (0.071)*	0.137 (0.050)**	0.049 (0.017)**
Observations	3840	9522	34299

Note: "Recent homebuyers" are defined as households who have purchased their primary residence in the four years prior to each survey. "Young households" are defined as households headed by individuals 40 years old or less. Each regression also includes the same set of variables of the specification of Table 6, column (1) and a full set of year dummies. The sample period is 1989–2002. The Probit coefficients indicates marginal effects; standard errors of the coefficients are reported in parenthesis. One and two stars indicates that the coefficient is statistically different from zero at the 5 and 1 percent levels, respectively.

residence, years of education, family size, a dummy for self-employment, a geographical indicator of judicial efficiency, and income quintile dummies).[13]

The Probit coefficients indicate that the propensity to borrow decline with age and in the South, increase with income, education and family size. We also find that the self-employed are less likely to have mortgages. To capture the impact of enforcement costs on the operation of mortgage markets, we use the "Log of pending civil trials". In case of disputed loans, this variable is directly related to the duration of trials, and therefore to legal expenses and interest income forgone when collateral does not cover judicial costs (Jappelli et al., 2005).[14] The coefficient is negative and statistically different from zero, showing that the volume of mortgage lending is lower in judicial districts with the largest backlogs of trials. This result is therefore in agreement with the view that the cost of enforcing contracts is a key determinant of credit markets activity (La Porta et al., 1997).

Turning to the tax-related variables, we find that the propensity to borrow is higher for people in the top tax brackets. However, looking at the interaction terms, we find that there is either no detectable effect of the tax reform on the propensity to borrow, or a pattern that is at variance with what suggested by the theory of portfolio taxation. The findings are similar when we look at the intensive margin and basically agree with the simple difference-in-difference estimates of Tables 4 and 5.

In Table 7 we perform sensitivity analyses on the robustness of the results. A first issue is that the dependent variable might be inappropriate for analyzing if the demand for debt becomes insensitive to the marginal tax rate after the reform. Indeed, recall that we observe the amount of outstanding debt rather than interest payments (i.e., we are using the stock

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[13]The results are unchanged if we include house characteristics, such as number of years since purchasing the house, house size (in square meters), and dummies for the quality of housing (results not reported). The inclusion of these variables is justified as a way to control for the effect of hedonics and different quality on the size of the outstanding mortgage. Since housing characteristics are missing for many households, we report only the results for the whole sample with these characteristics omitted.

[14]The indicator of judicial inefficiency is the number of pending civil trials per thousand inhabitants of the corresponding judicial district. Source: *Annuario Statistico dei Procedimenti Giudiziari Civili*, various years, Italian National Institute of Statistics (ISTAT). The indicator exhibits substantial variability over time as well as across judicial districts. In our repeated cross-sectional framework, variability of the backlog between different years and different districts is crucial to identify the effect of judicial inefficiency on mortgage market performance.

of debt rather than the flow of mortgage interests). If there is persistence in the marginal tax rate—a very likely event—a large stock of debt after the reform just reflects the fact that those with persistently high marginal tax rates took more debt when it was convenient to do so, i.e., before the reform. Thus, we will observe sensitivity to the tax rate even after the reform. The problem is similar for older individuals, who are likely to have taken their mortgage in earlier years.

In Table 7 we restrict the sample to "recent homebuyers" or to "young households". For brevity, we report only the coefficients of interest for the extensive margin decision and neglect the multiple-income issue (the results are similar for the amount borrowed and if we use the extended specification (2)). The results confirm the basic pattern of results of Table 6.

A second issue is that information on the tax reform might not be readily available to individuals contemplating to purchase a house. Therefore, the impact of the reform might be seen only a few years after it is implemented. To check for this possibility, we exclude the intermediate years of our sample (1993–1995) in the third regression of Table 7, which now include the 1989–1991 surveys as "pre-reform period" and 1998–2002 as "post-reform". The estimates again signal that the effects of the reform do not agree with expectations.

We perform a number of additional sensitivity checks, whose results are omitted for brevity. First, we eliminate the level effect by defining the dependent variables in the Tobit regressions as the loan–income ratio (the first two columns) or the loan–home value ratio (the last two). Once more, the results remain firmly against the simple implications of the theory. Second, we estimate Heckman selectivity regressions for the amount borrowed, confirming the patterns in Table 6. Since it is difficult to single out variables that affect the decision to borrow but not the amount borrowed, identification is achieved introducing a full set of regional dummies in the selectivity equation. Alternative experiments do not affect the pattern of results. Finally, since we cannot rule out that the tax reform has the predicted effect on borrowing, but that the effect is masked by the presence of different underlying trends in the decision to borrow that differ across tax-payer groups, we include group-specific time trends in estimation. The results are again unchanged.

In principle, our results could be attributed to sample misallocation, i.e., allocating households to the wrong income group because of miscalculation of the "true" marginal tax rate. Due to the complexities of the tax system, computing the marginal tax rate is bound to produce errors. For example, our procedure neglects income from dividends and rents which raise taxable income, and thus our imputed rate may be underestimated. On the other hand, we do not take into account tax deductions that depend on household size and composition, occupation and selected expenditures (such as out-of-pocket medical expenses, charitable donations and home mortgage interest). These deductions reduce borrower's actual marginal tax rate, so that our imputed rate may be over-estimated. It is unlikely that such misclassification could account for the results. First of all, we only consider few broad tax brackets. For instance, we have only four groups in Table 4 ($\tau < 0.22$, $0.22 \leqslant \tau \leqslant 0.27$, $0.27 < \tau < 0.4$, and $\tau \geqslant 0.40$), and as we mention we obtain similar results if we merge the top two groups into one. Second, even if we restrict the group of rich taxpayers to people with $\tau \geqslant 0.40$, where either underestimation or overestimation is unlikely to create misallocation bias, we still find no effects from the tax reforms.

6. Borrowing constraints and information

There are two reasons why borrowing might not be sensitive to changes in incentives: lack of financial information about the tax reform and borrowing constraints. The interpretation in terms of financial information would be in line with the recent findings of Gemmell et al. (2004), who analyze microeconomic data on tax perceptions from the British Social Attitudes Survey and conclude that "many voters are quite inaccurate in their estimates of their tax liabilities, in their awareness of the relative merits of income tax and VAT, and in their assessment of the redistributive properties of these taxes." (p. F135). It should also be stressed that in comparison with other industrialized countries, Italy lags behind in terms of financial information, economic literacy and education in finance.[15]

To check if lack of financial information explains the pattern of results we estimate the regressions distinguishing between people with different degree of a constructed index of financial information. Each household head reports in 1995–1998 (but not in other survey years) whether he or she is aware of the existence of specific types financial assets (stocks, bonds, mutual funds, etc.).[16] Some of the categories correspond to a single asset (for instance, checking accounts and specific types of government bonds), but others refer to broad groups of assets (for instance, stocks, mutual funds, and corporate bonds). We use the data to construct a summary indicator of financial information, as the number of assets that each individual knows divided by the number of potential assets (14 in all). On average, respondents are aware of 63.9 percent of the assets. We then relate the index to a wide set of socioeconomic variables—including occupation, sector, region, household resources—and predict the index in years other than 1995–1998. Finally, we use the 63.9 percent cutoff to split the sample between individuals that are likely to be more or less financially informed.

When we replicate our regressions for borrowing splitting the sample by the degree of information, one fourth of the sample is allocated to the "High Information" group, while the remaining observations are "Low Information" households. The results are reported in the first two columns of Table 8. We find that in the group with respondents that are less financially informed, there are statistically significant effects of the reform contrary to the hypothesis that taxes affect portfolio decisions. For those with low information, the coefficients are not statistically different from zero, providing some support for the information-based explanation.

As mentioned in Section 2, the Italian mortgage market features high enforcement costs and judicial inefficiency, leading to prevalence of credit constraints during the sample period. The fact that the probability of having a mortgage increases with the marginal tax rate is indeed consistent with the idea that the marginal tax rate conveys information about household resources in addition to the direct income effect. Thus one interpretation of our findings is that the tax variables are correlated with access to other assets and credit,

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[15]The *Annual Survey of the World Competitiveness Indicator* indicates that Italy ranks 38th and 43th out of the 49 countries examined in terms of economic literacy and education in finance among the population. On a 1 to 10 scale, Italy receives a score of 3.98 in economic literacy and 3.73 in education, much lower values than, to name a few, the US, Sweden, Netherlands, Australia and Japan (all above 6).

[16]For each of 14 assets, the question is: "I will show you a list of possible forms of saving. I would like you to tell me which forms of saving you know, even if only by hearsay." Guiso and Jappelli (2005) document that many Italian households lack basic financial information and to study the effect of lack of information on portfolio choice.

Table 8
Probit estimates for sample splits by asset knowledge and credit card holding

	Low knowledge	High knowledge	No credit card	Credit card
$\tau < 0.22$, after the reform	−0.005 (0.016)	—	−0.003 (0.018)	−0.144 (0.050)**
$0.27 < \tau < 0.40$, after the reform	0.024 (0.007)**	0.011 (0.034)	0.029 (0.007)**	−0.001 (0.034)
$\tau \geqslant 0.40$, after the reform	0.085 (0.027)**	0.040 (0.043)	0.094 (0.026)**	0.016 (0.042)
Observations	32728	8078	34640	6166

Note: Asset knowledge is the sum of financial assets knows (stocks, mutual funds, investment accounts, CDs, various types of T-bills, corporate bonds), as reported by households interviewed in 1995 and 1998, divided by the total number of assets potentially known (17 in total). The variable ranges from 0 (no asset is known) to 1 (all 17 assets are known). The index is imputed to years other then 1995 and 1998 using a regression which relates the index to sector, occupation, income, and the variables included in the Probit for the propensity to borrow. High knowledge is a dummy equal to one for households in the top quartile of the index distribution; low knowledge is the residual bottom three quartiles of the sample. The coefficient of the poor taxpayer variable is omitted due to lack of observations in this category. The sample split for credit card is defined on the basis of a dummy variable available in all sample years equal to one for households with credit card, and zero otherwise. Each regression also includes the same set of variables of the specification of Table 6, column (1) and a full set of year dummies. The sample period is 1989–2002 (excluding 1993). The Probit coefficients indicate marginal effects; standard errors of the coefficients are reported in parenthesis. One and two stars indicate that the coefficient is statistically different from zero at the 5 and 1 percent levels, respectively.

improving the chance to enter owner occupation. Since the tax reform did not remove borrowing constraints from the mortgage market, people might not have changed their borrowing decisions even in the face of the more generous tax incentives available to them after 1992.

Previous tests for liquidity constraints using consumption data have frequently split the sample on the basis of wealth or income, arguing that poor consumers are more likely to be constrained, or on the basis of more direct indicators for liquidity constraints, such as the availability of a credit line or credit card. In the last two columns of Table 8 we follow the latter strategy and allocate the households in our sample to two groups based on whether the household has a credit card. Consistently with the process of credit market development going on over the sample period, this indicator more than doubles in our sample, from 11 percent in 1989 to 24 percent in 2002. We run regressions for the probability to borrow in the two groups and find that in the group with no credit cards the coefficients are statistically different from zero, and contrary to the theory. For those who have credit cards, the results are insignificant (at least for rich taxpayers). Tobit regressions and other refinements (such as different sample splitting criteria) give similar results.

7. Conclusions

Before 1992 mortgage interests were fully tax deductible up to 3500 Euro (7000 for two co-signers). In 1992 the Italian government implemented a tax reform whose ultimate effect was to cancel the relation between the after-tax mortgage rate and the marginal tax rate. In the new regime the tax incentive applies to only one taxpayer and is proportional to the interests paid regardless of the marginal tax rate. This reform of incentives should have eliminated the relation between the marginal tax rate and the propensity to borrow

after the reform, and reduced the propensity to borrow of high-income taxpayers relative to other population groups and of multiple income households relative to single income.

In the empirical analysis we single out a wide set of variables affecting the mortgage market during a period of rapid growth: demographic characteristics, household resources, quality of housing, and judicial costs. However, we find no evidence that tax considerations shape the demand for mortgage debt after the tax reform, either at the extensive or the intensive margin. The most plausible interpretation of the results is that in the presence of borrowing constraints or lack of financial information about changes in the after-tax interest rate do not affect the demand for borrowing. Sample splits based on financial information and proxies for credit constraints provide some support for these hypotheses. This might explain also why our findings contrast with the US literature, where credit markets are much more developed and there is broad consensus that elimination of tax incentives for consumer credit have induced portfolio shifts towards mortgages. Our results may also reflect the possibility that the tax reforms induced a decline in "hidden costs" of borrowing (such as those related to gathering information, or red-tape related costs), which may have increased the demand for loans by rich taxpayers despite the concurrent increase in after-tax interest rates.

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