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The housing price boom of the late '90s: did inflation targeting matter?*

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Abstract

The recent boom in housing markets of most developed economies has spurred criticism that inflation targeting central banks may have neglected the build-up of financial imbalances. This paper provides a formal empirical test of such claims, using a standard program evaluation methodology to correct for a possible bias due to self-selection into inflation targeting. We consider 17 industrial economies over 1980-2006, among which nine countries have targeted inflation at some dates. We find robust evidence of a significant positive effect of inflation targeting on real housing price growth and on the housing price to rent ratio.

JEL classification: E4; E52; E58.

Keywords: Inflation targeting; Housing prices; Treatment effect; OECD countries.

Résumé

L'épisode récent de boom des prix des logements observé dans la plupart des économies développées a nourri une vague de critiques à l'encontre des banques centrales poursuivant une stratégie de ciblage d'inflation, suspectées d'avoir négligé l'apparition et l'aggravation de déséquilibres macrofinanciers. Nous proposons dans cet article un test empirique de la validité de ces critiques, sur la base d'une méthodologie empruntée à la littérature microéconométrique d'évaluation qui permet de corriger le biais de sélection inhérent au choix du ciblage d'inflation par les pays concernés. Nous considérons un échantillon de 17 économies industrialisées de l'OCDE, dont neuf cibleurs d'inflation, sur la période de 1980 à 2006. Nous mettons en évidence un effet positif et significatif du ciblage d'inflation sur le taux de croissance réel des prix des logements ainsi que sur le ratio des prix des logements rapportés aux loyers.

Classification JEL: E4; E52; E58.

Mots-clés : ciblage d'inflation; prix des logements; effet du traitement; pays de l'OCDE.

1 Introduction

The credible anti-inflationist monetary policies conducted in major developed economies since the mid 1980s have been identified as one plausible factor behind the Great moderation episode over the last two decades. However, as the dotcom boom and bust of the early 2000s and the recent subprime mortgage crisis prove it, financial crises associated with assets prices boom and bust episodes are not merely a relic of the twentieth century. This unpleasant fact has recently prompted a debate about the role of inflation targeting (IT) policies in the build-up of imbalances that eventually led to such episodes of financial turmoil. Indeed, some central bank watchers have regularly contended over the recent years that such policies, which aim primarily at stabilizing inflation over a 2-3 years horizon, would actively contribute to damaging financial stability at longer horizons, as they would tend to neglect monetary and financial developments which are deemed irrelevant for future inflation in the short to medium term.[1]

While the consensus is broad in the economic profession that a policy focused at maintaining price stability is a necessary condition for maintaining also financial stability, the experience of the recent years has comforted the view that it is not a sufficient one.[2]

First, there is a view that an inflation targeting central bank may neglect important information about the build-up of financial imbalances which do not materialize rapidly into consumer price pressure. Many reasons may account for such a disconnection between financial and price developments. Among the usual suspects are the impact of globalization in terms of lower import prices and induced dampened domestic inflation pressures, as well as the consequences of structural changes that have affected the functioning of labour and

[1] See notably a series of contributions by Claudio Borio, William White and their coauthors at the BIS (Borio et al., 2003, Borio and White, 2003, White, 2006). Bean (2003) claims on the contrary that inflation targets may be enough provided the central bank is sufficiently forward-looking. Recently, Leijonhufvud (2007) and De Grauwe (2007) have also expressed their concern that central banking could not be reduced to strict inflation targeting without damaging consequences for both financial and macroeconomic stability.

[2] Interestingly, the point has been repeatedly voiced in these terms by Mervyn King, Governor of the Bank of England, in recent speeches. See King (2009). Bordo and Wheelock (1998) and Bordo et al. (2003) have provided historical evidence of the detrimental effect of episodes of monetary instability on financial stability.

financial markets over the last two decades. Some have also argued that the mere success of inflation targeting strategies could have contributed to hampering a proper risk assessment by inflation fighting central banks, what Borio et al. (2003) labelled the "paradox of credibility". Since the anti-inflationary commitment of the central bank becomes more credible, and long-run inflation expectations get more firmly anchored around the central bank's objective, the macroeconomic consequences of "cheap money" -including credit booms that sustain a rise in some asset prices- may take more time to show up into higher inflation. As a conclusion, policy rates may fail to rise promptly enough to stem the build-up of financial imbalances (Borio and White, 2003).[3]

A recent paper by Piazzesi and Schneider (2007) provides another theoretical motivation for our study. They construct a general equilibrium model of housing prices where some agents suffer from inflation illusion, thus disagree about the level of ex ante real interest rates. While smart investors understand the Fisher equation, thus linking the level of nominal bond rates to their inflation expectation, illusionary investors do not, therefore they systematically associate shifts in nominal rates with shifts in the real return of bonds. In this setup, the authors show that, when households are allowed to borrow against housing collateral, then a non-monotonic relationship occurs between house prices and inflation, depending on the level of nominal rates. For instance, in the context of low nominal interest rates where smart investors have inflation expectations below the long-run inflation average and see investment in bonds as more rewarding, illusionary investors see investment opportunities in a leveraged housing portfolio, thus driving up house prices above the equilibrium value. While the authors' focus is more on the consequences of structural change in mortgage markets, their model also suggests a role for inflation targeting in house price mispricing episodes, at least in a transitional phase. Consider for instance that the central bank adopts inflation targeting and that only a portion of households adjust their inflation expectations down to the central bank target. Disagreement about ex ante real rates may then be conducive of a housing price boom in a context of low

[3] A more formal presentation of a similar argument has been put forward by Amato and Shin (2005). In their model, where private agents have diverse private information about the true state of the economy, the public signal provided by the central bank has a disproportionate effect on agents' decisions, is likely to crowd out their private information and then tends to lower the information value of prices.

nominal rates. This is what we observed in the late '90s-early 2000s in many developed economies, a period when (less informed) households may have viewed borrowing as exceptionally cheap in real terms. We do not deny that structural factors, like a widespread shift towards more deregulated mortgage markets, are likely to have played an important role in fueling the recent rise in housing prices and explaining the correlation in housing price booms across most industrial economies. That said, inflation targeting strategies may have been an additional destabilizing ingredient, at least until all agents converged to inflation expectations in line with the central bank target.

We aim in this paper to bring such hypotheses to the data and evaluate whether inflation targeting actually mattered as regards housing price inflation in developed OECD economies. Over the last decade, an abundant empirical literature has tried to quantify the macroeconomic performance of countries that adopted inflation targeting.[4] Most studies focus on inflation performance, in absolute or in relative terms, while some also examine whether adopting an inflation targeting strategy could be made responsible for a more volatile output. However, to our knowledge, there is no comparative empirical work about the consequences of inflation targeting policies for financial stability. We aim at filling this gap, using a program evaluation methodology that has been recently transposed to macroeconomic issues (see notably Persson, 2001 and Lin and Ye, 2007). We thus circumvent some self-selection bias that is likely to plague previous studies on the consequences of adopting inflation targeting.

Our study encompasses 17 industrial economies over the period 1980-2006, among which nine countries have targeted inflation at some dates. We find that the average effect of inflation targeting on house price inflation is positive and statistically significant. These results are robust to various specifications and options of the evaluation procedure. On average, the adoption of inflation targeting is associated with an increase in the level of annual house price inflation by some 2.1 percentage points in targeting countries. Note that the estimated effect is even larger when the control sample is restricted to the most recent sub-period (from 1990 to 2006).

In the rest of the paper, section 2 provide a summary view of the recent housing price

[4] See for instance Ball and Sheridan (2004), Lin and Ye (2007), Vega and Winkelried (2005) and the studies collected in Bernanke and Woodford (2004).

boom in developed OECD economies. Section 3 presents our econometric methodology. Section 4 presents the dataset and discusses several empirical issues. Section 5 comments on the results and section 6 concludes.

2 The housing price boom of the last decade

Since 1970, nominal housing price growth has fluctuated widely in developed economies, with four expansionary phases -in the early and late 1970s, in the mid to late 1980s and from the late 1990s to the mid-2000s- and four slowing phases -in the mid 1970s, the early 1980s, the early 1990s and from 2007 to present.[5]. Note that, while housing price busts are normally characterized by a significant drop in real house prices, nominal house price deflation is rare and was associated in the past with episodes of severe economic downturns, such as the recessions in the early 1990s in Finland, Norway and Sweden[6].

Most developed economies have experienced rapidly rising house prices since the mid-1990s.[7] Taken by its magnitude, length and geographical coverage, the latest boom has been quite exceptional. In the 17 OECD countries of our sample[8], the annual rate of growth of nominal housing prices has reached an average of almost 7.5% between 1996 and 2006 (5.5% in real terms), to be compared with only 5.4% over the 1980-1995 period (0.5% in real terms). In addition, the recent boom lasted for almost ten years in most countries, which is roughly twice as long as the duration of past episodes.

An abundant literature has investigated the reasons why this housing boom was so pronounced and in particular decoupled that much from normal business cycle fluctuations.

[5] See for instance Lecat and Mésonnier (2005).

[6] For a description of past housing booms and busts and the size of associated recessions in developped OECD economies, see for instance Claessens, Kose and Terrones (2008).

[7] An exception is Germany whose nominal house prices have been gradually declining since they reached a modest peak in 1994. Japan is another obvious exception, the country being stuck over the whole 1990s in the slump consecutive to the housing price and stock market bust of the beginning of that decade. The Japanese case being quite special, we excluded Japan from our database. Note that since Japan did not target inflation over the period under review, excluding Japan tends to minimize the probability of rejecting the null of no-impact of IT on house price growth.

[8] Countries are listed in section 4 below

Demographic trends such as changes in the composition of households, the impact of financial deregulation affecting mortgage markets via a credit boom (Cardarelli et al., 2008), the loosening of credit standards (Dell'Ariccia et al., 2008) and the declining trend in real interest rates (Girouard et al., 2006) have been proposed as possible factors explaining this phenomenon. To our knowledge however, no empirical study so far tests the impact of the monetary policy regime and in particular of inflation targeting strategies.

We assess the buoyancy of the housing market according to both the annual rate of growth of housing prices in real terms (denoted RHOPG) and a price to rent ratio, which is akin to a price-earning ratio in stock markets (denoted HOPCPIH) and generally stands for an appropriate measure of housing valuation when housing is primarily viewed as an asset in the households' portfolio (instead of a source of housing services).[9] However, reliable and long enough time series on rents are not available for a number of countries and we had to proxy rents with the housing component of consumer price indices. Due to substantial differences in the definition of this component across countries, regarding in particular the valuation of owner-occupied housing services, results related to our measure of the price to rent ratio deserve some additional caution. For this reason, we prefer to focus on real house price growth as our baseline. Although real housing price growth is arguably a very rough measure of possible imbalances in housing markets, we see no reason to suspect that inflation targeting as such could induce any substantive shift in the equilibrium or long run real housing price growth. Therefore, detecting any extra-growth in housing price inflation should be enough to signal a contribution of inflation targeting per se on the build-up of a positive housing price gap.

Figure 1 to 3 show real and nominal house price growth developments as well as the price to rent ratio for each country of the panel. The shaded area indicates whenever the central bank follows an explicit inflation targeting strategy (see section 4 for details). Most economies experienced a sharp rise in residential property prices in the second half of the 1980s, that often followed on a deregulation of the housing finance sector. In the early

[9] Note that we also considered nominal housing price growth as a dependent variable, adding lagged inflation to the conditioning variables listed below in section 4. Results are qualitatively unchanged. We nevertheless preferred to focus on real growth (1) for comparability with other studies and (2) to limit the risk of having conditioning variables that are endogenous to the adoption of formal inflation targeting.

1990s, house prices slowed down or fell, following the US recession in 1990-1991 and the episode of high interest rates in Europe after the ERM crisis in 1992-93. Finally, housing price inflation accelerated in the second half of the 90s for most countries, apparently irrespective of their monetary policy strategy. However, what this graphical evidence cannot tell is whether this surge in housing price inflation was stronger in targeting countries, other things else being equal. This is what our empirical exercise aims to clarify.

3 Methodology

A well-identified issue in empirical studies of the effects of inflation targeting strategies is that countries do not choose their monetary policy regime randomly, i.e. irrespective of their economic environment. When considering IT adoption, monetary authorities are instead likely to wait for some prerequisites to be met in order to make the switch mostly credible and then possibly effective. For instance, countries with more liberalized and more developed financial markets, and notably more deregulated mortgage markets are more likely to opt for an inflation targeting strategy. Indeed, a high degree of financial development, which tends to facilitate the transmission of monetary policy decisions to the economy, is often seen as one prerequisite for successful inflation targeting. From the point of view of the econometrician, there is then an issue of self-selection which ought to be corrected for while estimating the consequences of IT on any variable of interest. To deal with this self-selection bias, we borrow an estimation technique from the program evaluation literature which has been applied to macroeconomic issues in a few recent papers (Persson, 2001, Vega and Winkelried, 2005, Lin and Ye, 2007).

Suppose we want to assess the effect of a treatment $D \in \{0, 1\}$ (which may be a new medicine, a job training program.. or IT) on an outcome y (which may be some measure of health, wages.. or the degree of buoyancy of the housing market). We observe the value of the outcome for a population of individuals denoted by an index i, some of which have received the treatment (i.e. $D_i = 1$). The modern literature on treatment effects begins with a counterfactual setting where each individual has both an outcome with treatment and an outcome without treatment.[10] Let us denote $\left(y_i^1, y_i^0\right)_i$ this twin set of variables.

[10] For a detailed presentation, see e.g. Wooldridge (2001, chap. 18).

The main quantity of interest for policy evaluation is generally what is called the average treatment effect on treated (hence ATT), which is defined as:

$$ATT = E\left[y_i^1 - y_i^0 | D = 1\right]$$

Unfortunately, we face a problem of missing data. Since, in reality, each individual is either treated or untreated, we cannot observe both $\left(y_i^1, y_i^0\right)$ for all i, but only $y_i = D.y_i^1 + (1-D).y_i^0$. Besides, as argued above, it is reasonable in most economic applications to suspect that the treatment is not randomly affected to observed units. Thus, measuring the ATT on the basis of the observed y variable is not straightforward. Indeed, in general, the difference in sample means of the observed outcome for treated and untreated units is not equal to the ATT:

$$ATT = E\left[y_i^1 - y_i^0 | D = 1\right] \neq E\left[y | D = 1\right] - E\left[y | D = 0\right]$$

The usual solution from the program evaluation literature consists in using all the relevant extra information on the sampled individuals that we can gather, so as to randomize the treatment conditionally on this information. More precisely, suppose that we have measures of a list of determinants X_i of y_i which may also matter a priori for IT adoption (IT prerequisites). Then, if conditionally on the X_i, we can reasonably assume that $\left(y_i^1, y_i^0\right)$ is independent of the strategy variable (or at least that they are mean independent)[11], the *conditional* ATT reads:

$$\begin{aligned} ATT(X) &= E\left[y_i^1 - y_i^0 | D_i = 1, X_i\right] = E\left[y_i^1 | D_i = 1, X_i\right] - E\left[y_i^0 | D_i = 0, X_i\right] \quad (1) \\ &= E\left[y_i | D_i = 1, X_i\right] - E\left[y_i | D_i = 0, X_i\right] \quad (2) \end{aligned}$$

which can be calculated using observed outcomes for the sample. It is then a priori easy to get an estimate of the ATT from the estimated conditional $ATT(X)$ by simply averaging out the Xs.[12] However, if X_i is a vector of macro (i.e. generally continuous)

[11] This hypothesis is generally labelled the Conditional Independence Assumption in the evaluation literature.

[12] If for instance X is a scalar taking m discontinuous values c_m, then for each m, we just need to estimate

variables, getting an estimate of the conditional ATT for each possible value of X is obviously a complex matter. Fortunately, Rosenbaum and Rubin (1983) proposed in a seminal contribution a very convenient solution to this dimensionality problem, which relies on the preliminary estimation of a propensity score $p(X)$ of being treated conditionally on the observables: $p(X) = \Pr(D = 1|X)$. They showed indeed that, under the same conditional independence assumption as above, measuring the conditional ATT is equivalent to measuring :

$$\begin{aligned} ATT(p(X)) &= E\left[y_i^1|D_i = 1, p(X_i)\right] - E\left[y_i^0|D = 0, p(X_i)\right] \quad (3) \\ &= E\left[y_i|D_i = 1, p(X)\right] - E\left[y_i|D = 0, p(X)\right] \quad (4) \end{aligned}$$

The standard procedure runs then in two steps: one has first to estimate a propensity score using a probit regression of D on the covariates X and second to match the treated and untreated units according to their propensity scores in order to get estimates of the conditional treatment effect. Averaging over all treated units yields an estimate of the unconditional ATT.

A variety of propensity score matching methodologies can be used. Basically, all matching estimators associate each treated unit to one or more control units with "close" propensity scores. They differ however both in the way the neighborhood for each treated observation is defined and with respect to the weights assigned to the non-treated "neighbors". Although all estimators should converge asymptotically, the choice of a given method may matter in finite samples.[13] Besides, handling with small samples, as is typically the case when microeconometric methods are applied to a macroeconomic context, raises specific concerns.[14] First, gaps are more likely to appear in the common support of controls and

the simple means $E\left[y_i|D_i = 1, X_i = c_m\right]$ and $E\left[y_i|D_i = 1, X_i = c_m\right]$, and then to take the average of all the differences over the m different values of X to get the unconditional ATT.

[13] Note that the choice of a specific method involves a trade-off between bias and variance of the estimator: intuitively, extending the number of control units considered as relevant neighbours of a given treated unit increases the risk of bad matches, hence the bias, but contributes to reducing the variance, since more information is included in the counterfactual observation.

[14] On issues associated with small samples, see Heckman, Ichimura, and Todd (1997).

treated, so that treatment effects may be retrieved for a limited number of treated units only, resulting in a bias. Second, small samples increase the variance of estimated effects, making identification of significant effects more difficult. According to a recent study by Frölich (2004), kernel matching estimators, which compare treated units to a weighted mean of all control units on the common support, prove to be quite robust to small sample problems. We implemented a standard kernel matching method with a caliper of 6% in our baseline estimation.[15] Each treated unit is then matched to all control units weighted in proportion to the distance between the treated unit and the control unit. Formally, the kernel matching estimator of the ATT reads:

$$\widehat{ATT} = \frac{1}{n_1} \sum_{i \in I_1} \left(Y_{1i} - \frac{\sum_{j \in I_0} Y_{0i} G\left(\frac{P_j - P_i}{\alpha_n}\right)}{\sum_{j \in I_0} G\left(\frac{P_j - P_i}{\alpha_n}\right)} \right) \tag{5}$$

where I_1 denotes the set of treated, I_0 the set of controls, n_1 the number of treated units on the common support and p_j is the estimated propensity scores of unit i. G(.) is the kernel function and α_n the bandwidth parameter. In a standard way, we chose an Epanechnikov kernel defined as $G(u) = \left(1 - \mid u \mid^2\right)$ and a bandwidth parameter of 0.06. The variance of this estimator was obtained by a bootstrap with 1000 replications.

4 Empirical issues

4.1 Data and definition of variables

Our data set includes 17 industrial countries, namely Australia, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, the Netherlands, New Zealand, Norway, Spain, Sweden, Switzerland, the United Kingdom and the United States. The database covers the period from 1980 to 2006 with annual frequency. Nine countries—Australia, Canada, Finland, New Zealand, Norway, Spain, Sweden, Switzerland and the United Kingdom—adopted inflation targeting at some point during our sample period. Note that two targeting countries, Finland and Spain, joined the Euro in 1999, thus switching from IT to non-IT in our sample.

[15] All matching procedures have been implemented using the Stata routine PSMATCH2 developped by Leuven and Sianesi (2003).

Whatever the targeting country, the date of IT adoption is not always clear-cut and depends on how inflation targeting is defined. Several choices occur in the empirical literature. While some authors consider the first year when the turn to an IT-like strategy was mentioned or announced by monetary authorities, others adopt a stricter view and date IT adoption to the year when an explicit or fully fledged IT scheme was implemented, including the publication of a quantified inflation objective by the central bank.[16] For robustness, we considered both definitions in the following, that we labelled IT1 for "soft" or merely announced inflation targeting and IT2 for the adoption of a fully fledged targeting scheme. Table 1 shows the adoption dates according to both definitions. Depending on the definition, adoption dates differ for four countries in our sample: Canada (by three years), New Zealand (one year), Spain (one year) and Sweden (two years). In others cases both definitions converge.

Commenting on Ball and Sheridan (2004), Gertler (2004) argues that a host of countries that these authors classified as non-targeters did actually run monetary policies that proved to be close in practice to formal inflation targeting. He concludes that classifying countries according to what they say (their official strategy), not what they do, is probably misleading when assessing the relative performance of countries in terms of inflation stabilization. This issue may be raised regarding the classification of two major non-targeting central banks, the US Federal Reserve of the Greenspan-Bernanke era as well as the ECB, which have been both frequently described as implicit targeters by commentators.[17] In particular, the ECB, which commits itself explicitly to pursuing a quantified inflation objective, is arguably close to the inflation targeting paradigm, although the ECB has steadily denied that its strategy is akin to standard IT. In our baseline experiment, we preferred not to introduce any arbitrariness in our classification scheme and stuck to official statements about the prevailing monetary policy strategies in both the US and the euro area. However, as a variant, we also considered the case of an alternative unbalanced country database where the euro area appears as such and is classified as an inflation

[16] For details about dates of IT adoption, see Vega and Winkelried (2005) and references therein.

[17] Goodfriend (2005) argues e.g. that the recent successes of US monetary policy "...can be attributed in large part to inflation-targeting policy procedures that the Fed has adopted gradually and implicitly over the last two decades".

targeter since its inception in 1999, while EMU member economies are simultaneously dropped from the database from 1999 on.

4.2 Propensity scores estimation

We estimate the propensity scores using a pooled probit where the dependent variable is the targeting dummy (i.e. either IT1 for soft targeting or IT2 for fully fledged targeting) and the RHS variables are the factors deemed to influence both the choice of an inflation targeting strategy and the dynamics of house prices. Remember however that the purpose of the probit regression is to reduce the dimensionality of the matching problem, not to provide any plausible model of IT adoption. We must select all regressors that we would expect to have an impact on the ultimate variable of interest, here RHOPG, and could impinge on the IT status, thus implying a bias if we had computed the ATT without correction. Meanwhile, for the CIA to be valid, all conditioning variables should be chosen so that they are not influenced by the adoption of the IT regime. We thus take lags of most regressors so as to limit endogeneity problems.

Having this in mind, we finally selected seven conditioning variables for our baseline specification with reference to standard empirical models of housing price dynamics. These conditioning variables are : the lagged short and long interest rates in real terms (RIRS_1 and RIRL_1), the lagged net household disposable income growth in real terms (NDIG_1), a fixed exchange rate regime dummy (FER), a dummy variable indicating the degree of sophistication of the national mortgage market (MS) and the lagged ratio of the private credit to GDP (CREGDP_1) as a proxy for national financial development[18].

We took special care in correcting for cross-country heterogeneity in mortgage structures. Indeed, a few recent studies suggest that those structures matter for housing price

[18] Data sources and construction are detailed in Appendix A. In some variants of the baseline specification, we replaced the ratio of credit to GDP with the rate of net household savings to their disposable income (SAR), as a proxy of the capacity of housholds to borrow. We also choose a broader indicator of financial development such as liquid liabilities to GDP ratio as in Beck, Demirguc-Kunt, Levine (1999). Finally, we tested the inclusion of a banking crisis dummy (BKCR). None of these changes did affect qualitatively our results. The results are available upon request to the authors.

dynamics.[19] They can also have a bearing on the probability to adopt or not an inflation targeting scheme. Indeed, one may argue that monetary authorities are more likely to implement IT when they gauge that the domestic banking and financial systems are developed enough for monetary transmission to work through quickly and efficiently.[20] In practice, controlling for differences in mortgage structures between countries is not an easy task because most available data on the mortgage market characteristics of OECD countries are qualitative, or given as constant for the last two decades (which means that they may actually refer to different periods of time), thus ignoring possible trend changes in market regulations or practices (as the extension of securitization or the decrease in credit standards over the last decade).

To bypass these data limitations, we can use some proxies for financial development, such as the private credit-to-GDP ratio[21]. Another possibility is to construct a composite index summarizing institutional aspects of the mortgage markets such as the IMF (2008) recently did it. A quick look at mortgage market characteristics as shown in table 2 suggests that IT countries are predominantly countries where for instance variable rate mortgages prevail, mortgage equity withdrawal is at least legally possible and often used and loan-to-value ratios of mortgages are relatively high. However, it is fair to note that some non-targeters do also share the same structural characteristics.

That said, we constructed a dummy variable called MMSI summarizing what we thought to be relevant features of the domestic mortgage markets.[22] More specifically, we focused on those institutional features which appear to matter for monetary policy transmission according to the results in Calza et al. (2009) and Gerlach and Assenmacher-Wesche (2008): the presence or absence of mortgage equity withdrawal, the typical loan-to-value ratio, the extent of securitization, the share of owner-occupied homes, and, last but not least, the dominant type of interest rate adjustment (fixed rate vs adjustable

[19] See Tstatsaronis and Zhu (2004), Gerlach and Assenmacher-Wesche (2008) and Calza et al. (2008).

[20] Mishkin (2004) argues that a sound and well-developed financial system is a necessary condition for the success of an inflation targeting regime.

[21] This measure is widely used in the empirical literature. See Beck, Demirguc-Kunt and Levine (1999).

[22] See the data appendix for more details.

rate mortgages).[23] On this basis, we classified each country as having either a "highly sophisticated" ($MMSI = 1$) or a "less sophisticated" or flexible ($MMSI = 0$) mortgage market. Table 2 gives a view of institutional features of mortgage markets in our sample of countries, as well as the value of our dummy variable. The institutional features of national mortgage markets among OECD countries remain quite heterogenous. Broadly speaking, IT countries (Australia, UK, Sweden and Norway) provide the easiest access to home ownership. In contrast, in non IT countries (France, Italy, Germany, Belgium) the access to housing finance is somewhat constrained. Nevertheless, some exceptions remain such as the US and the Netherlands (both "highly sophisticated" mortgage markets but non IT countries).

Let us turn now to the expected signs of the estimated coefficients in the probit regressions. On the basis of previous studies, we would expect real interest rates and the fixed exchange rate regime to be negatively correlated with the probability of running an inflation targeting strategy. On the contrary, we would expect a positive coefficient for the CREGDP variable and the mortgage structure dummy, since a developed financial system warranting an efficient transmission of monetary policy is often seen as one of the prerequisites for IT adoption.[24]. We would also expect a positive sign for the net disposable income growth.

Table 3 provides summary statistics for housing price growth in real terms and the set of conditioning variables chosen. The comparison of the means of the relevant variables across non-inflation targeting and inflation targeting countries reveals that inflation targeters exhibit on average higher real house price inflation (at least for the years when IT is effectively implemented), as well as a larger banking credit to GDP ratio. However, they display lower inflation, lower short and long term real interest rates as well as lower savings to income ratios. These preliminary statistics hint that a simple comparison of housing price inflation in IT vs non-IT countries is potentially affected by non-random selection of the "treated", due notably to differences in financial systems and the degree of financial

[23] Note that the IMF (2008) index does not cover this latter feature, but focuses instead on the possibility to reimburse pre-emptively without penality.

[24] See for instance Mishkin (2007, p. 411) for a list of prerequisites.

development or sophistication, which should bias the result if they are not controlled for. This again provides support to the program evaluation methodology we adopted here.

Finally, table 4 shows the results of the pooled probit estimations[25] in four cases corresponding to the two different timings of IT adoption and two different time periods for the control group of observations (i.e. 1980-2006 vs 1990-2006). For robustness, we also present in table 4 a model specification based on an alternative measure of financial development (the net households' savings to income ratio, SAR). Constant terms were included in the regressions but are not reported for clarity. The real short term interest rate (RIRS_1), the ratio of private credit to GDP (CREGDP_1), the fixed exchange rate dummy (FER) and mortgage structure dummy (MS) all show up to be significant and with the expected sign. The quality of the fit is reasonably good with a pseudo-R^2 between 0.31 and 0.44 depending on the model. Figure 5 displays the densities of the propensity score for IT and non-IT countries as derived for each of the model specifications in the first four columns of table 4. Although the model has not been designed as a proper model of IT adoption, it is noteworthy that it does a relatively good job in discriminating the two types of countries. Indeed, we can see a marked difference in the densities of propensity score between targeters and non targeters. It can also be seen that changes in the definition of IT adoption dates affect the densities to a lesser extent than changes in the control group. However, whatever the size of the control group or the timing of IT adoption, the densities relative to targeters and non-targeters still have a large common support[26], which warrants that we can implement a matching strategy based on a comparison of the propensity scores.

5 Results of matching

Table 5 reports the main results of the matching procedure. The upper panel shows the results when the control group covers the entire 1980-2006 period, contrasting two timings of IT adoption, while the lower panel shows the results when the control group is restricted

[25] As a robustness test, we estimated a panel probit with random effects to control for unobservable heterogeneity across countries. The magnitude and sign of all the coefficients did not change.

[26] Defined as the intersection of the densities.

to the 1990s and 2000s.

The first column of table 5 refers to our baseline specification. Targeting inflation appears to be associated with a significant average extra-growth of real housing prices, whatever the dates of IT adoption and the size of the control group. The size of the effect is larger and indeed quite large when the control group is restricted to data posterior to 1990, but the point estimator of the ATT is then more likely to be affected by small sample bias and a lack of common support.

We checked the robustness of this positive effect of IT strategies on housing prices along several dimensions. As a first check, we implemented various alternative matching procedures to estimate the ATT.[27] As is commonly the case, we find that our results are quite robust to the choice of the matching procedure, although the effect tends to be less significant when matched controls are taken from a narrowing neihgborhood around a given treated unit (e.g. within a caliper of 1%). Indeed, whenever a caliper is set with a value below 2%, up to a third of treated units can be automatically dropped because of the vanishing common support. Nevertheless, it is questionable whether applying such a strict caliper limit in the case of a relatively small database of macroeconomic outcomes is warranted, although it is common in the microeconometric literature.

Table 5 also displays the estimated ATT when some outlier observations are first discarded from the sample, when alternative conditioning variables are considered in the propensity score estimation or when the ATT is computed for alternative dependent variables. In column 2, we discarded observations within the first percentile of the real housing price growth variable, so as to temper the possible favorable impact of severe housing price busts which occurred in non targeting countries in the 1980s and early 1990s.[28] Column 3 shows the estimated ATT when the level of financial development is measured by the ratio

[27] We tested one and three nearest-neighbors matching with replacement, with and without a caliper, radius (or caliper) matching, using caliper from 1% to 5%, as well as local linear matching. The results remained qualitatively unchanged. Detailed results are not reproduced here for brevity but are available upon request to the authors.

[28] The deleted observations, with negative annual growth rates of house prices between 18.6% and 24.4% in real terms, refer to Finland in 1991 and 1992, Sweden in 1992, and Denmark and the Netherlands in 1981.

of net savings of households to their net disposable income instead of the ratio of private credit to GDP. The estimated effect is somehow dampened when using control units from the whole sample period, but remains elevated and significant whenever control units are taken from the most recent subperiod.[29] Column 4 tests the impact of targeting inflation on nominal housing price growth (HOPG) instead of real growth.[30] Finally, the last column shows estimates of the ATT for the housing price to rent ratio (HOPCPIH). Targeting inflation also appears to be systematically associated with a higher price to rent ratio. The effect is positive and significant when strict targeting is considered and controls are taken in the whole period of observation, but is larger and very significant if the control sample is restricted to the post-1990 period.

Note that, considering the small size of our sample compared with typical datasets used in the program evaluation literature, an important issue is whether the results are driven by housing market developments in one particular country. To check this, we exclude sequentially each country and estimate the ATT on the basis of the remaining 16 countries only. Table 7 shows the estimated ATT on RHOPG (in the baseline specification). Globally, the results are unaffected by the exclusion of any individual country.

As a variant, table 6 presents the results of the same ATT estimations when the euro area is added to the database as a genuine inflation targeter from 1999 to 2006, instead of individual EMU member countries. Again, the conclusions reached from the baseline experiment are qualitatively unchanged. Overall, we thus find a robust evidence of a positive and significant effect of running an IT strategy on housing price inflation and the house price to rent ratio. Ceteris paribus, inflation targeting is associated with an increase in real housing price growth by some 2.2 percentage points, averaging our findings across all model variants, while the price to rent ratio is increased by some 10 percentage points.

To end with, table 8 provides standard quality-matching indicators. Technically, the self-selection bias which motivates the matching procedure is controlled for if the covariates

[29] Note also that the savings to income ratio is not available for all countries over the whole 1980-2006 period.

[30] The set of conditioning variables for the computation of the propensity score is then augmented with CPI inflation (CPIG), while real interest rates are replaced with the corresponding nominal interest rates.

X's are sufficiently balanced by the matching process, i.e. they appear to have the same distribution for matched treated and non-treated. Overall, we find that matching on the estimated propensity score balances the X's in the matched samples quite well. The standard balancing test proposed by Rosenbaum and Rubin (1983) is a two-sample t-test of the null that there are no significant differences left in the means of conditioning variables across both groups. The null was never rejected at 10% for any covariate, whatever the sample and model variant. Consistently with the results of the t-tests for individual covariates, the median covariate bias between the treated and control groups, which ranged between 4.2% and 13.25% before matching, is reduced to below 1% by the matching procedure.[31] As expected, the pseudo-R^2 of the probit decreases sharply after matching, which suggests that there is no systematic difference left in the distribution of covariates between both groups. In addition, the joint significance of the regressors is always rejected after matching, while it is always accepted before matching. Hence, we can conclude that our kernel matching procedure proved able to wipe out most of the initial selection bias.

6 Conclusion

In this study we implemented standard program evaluation techniques to assess whether the choice of an inflation targeting (IT) strategy by the central bank had any significant impact on housing price dynamics in 17 developed OECD economies. This exercise was thought as an empirical test of recurrent but generally informal critics that are addressed to the inflation targeting paradigm from the perspective of its possible detrimental impact on financial stability.

Our central findings support the idea that the adoption of IT had a significant impact on the growth rate of house prices as well as on the house price to rent ratio. These results appear to be quite robust, but it is fair to say that they may suffer from several data limitations, in particular regarding the quality and comparability of house price

[31] Following Rosenbaum and Rubin (1985), for a given covariate X, the standardized difference before matching is the difference in the sample means in the full treateed and non-treated groups as a percentage of the square root mean of the sample variances in both groups. After matching, the respective means are computed over the treated and the non-treated units that fall on the common support.

data across countries. In addition, given that, for most countries, data on credit for house purchase is not available on a sufficiently long period, testing simultaneously for an impact of IT on mortgage credit growth remained out of reach. This would have usefully completed the picture, since the latest housing price boom was clearly sustained by a concomitant credit boom in most countries.

Overall, the evidence presented provides an impetus for further research, both theoretical and empirical, on the relatively neglected issue of the consequences of inflation targeting strategies for asset price fluctuations.

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Appendix A: data

We use yearly data for 17 OECD countries covering a period that ranges from 1980 until 2006. Data are seasonally adjusted excepting interest rates. We detail in this appendix the data sources and the construction of specific variables:

- Residential property prices (nominal and real, deflated by the CPI) were provided by the BIS. We used yearly house price series. Series are indices at the end of the year[32].

- Real net household disposable income is from the OECD Economic Outlook database. Data were expressed in billions of national currency units. Growth is defined as year-on-year changes (NDIG).

- The short-term interest rate (IRS) is a 3-month money market rate taken from the OECD Economic Outlook database. The long-term interest rate (IRL) is the yield on long-term government bond on the secondary market with residual maturity of about 10-years. The interest rates used are yearly averages of daily figures taken from the OECD Economic Outlook database. Real rates are computed as ex-post real interest rates using annualized CPI inflation rates (RIRS and RIRL).

- Credit to the private sector (CRE) is taken from the IMF IFS database (code 32d which includes gross credit from the financial system to individuals, enterprises and non financial public entities). Series are outstanding amounts at the end of the year. Many of the IMF credit series displayed large level shifts owing to changes in definitions or re-classifications. So, when series showed significant structural breaks as indicated by a TRAMO application, breaks have been corrected one by one. The level of the series was then adjusted by backdating the series starting from the sample end and based on the adjusted series. We detected level shifts and therefore we adjusted series for Belgium, Canada, Denmark, Ireland, New Zealand and Sweden. Then, we calculated, as in Borio and Lowe (2002), the ratio CREGDP of nominal credit to nominal GDP (as taken from the OECD EO database).

[32] More details on the house-price series are available in Arthur (2005).

- We constructed a banking crisis dummy variable (BKCR) that takes the value 1 during the crisis. To identify banking crisis episodes, we rely on the updated database of Caprio and Klingebiel (2003) maintained by the World Bank .

- The FER variable is a dummy variable. We use the exchange rate classification proposed by Reinhart and Rogoff (2004). We consider the first two categories of the Reinhart and Rogoff's classification as fixed regimes (the dummy variable equals 1) and for the other categories, the dummy variable equals 0.

- The IMF mortgage market index summarizes 5 institutional characteritics of the mortgage market (see Table 1 in IMF, 2008): mortgage equity withdrawal, the existence of early repayment fees, the loan-to-value ratio, the development of the covered bonds market and the mortgage-backed securities market. Regarding our own Mortgage market sophistication index (MMSI), we considered the following variables: the presence of mortgage equity withdrawal, the loan-to-value ratio, securitization, the share of owner-occupied homes, the type of interest rate adjustment (fixed or variable). Sources are available in IMF (2008), in Calza et al. (2009) and in Assenmacher-Wesche and Gerlach (2008). For securitization, values of 0, 0.5, and 1 are assigned to each country depending on whether this feature is nonexistent, limited, or widespread, respectively. For loan-to-value ratio and share of owner-occupied homes, each country is assigned a value between 0 and 1, equal to the ratio to the maximum value across all countries. Then, our index is computed as a simple average of these four features. It ranges from 0 to 1 with higher values indicating easier access to household mortgage finance. The group of 17 countries is then split into two groups where each country is classified as having either a "highly sophisticated" (MMSI equals 1) or a "less sophisticated" (MMSI equals 0) mortgage market (see table 2)

Table 1: Inflation targeters and dates of IT adoption

Countries	Starting year of IT strategy	
	soft (IT 1)	explicit (IT 2)
Australia	1994	1994
Canada	1991	1994
Finland	1993	1993
New Zealand	1990	1991
Norway	2001	2001
Spain	1994	1995
Sweden	1993	1995
Switzerland	2000	2000
United Kingdom	1992	1992

Source: Vega and Winkelried (2005).

Table 2: Structural features of national mortgage markets

	Mortgage equity withdrawal	Loan-to-value ratio (in %)	Interest rate adjustment	Securitisation	Share of owner occupied homes (%)	IMF MM index	MMSI
Australia	yes	80	V	yes	70	0.69	1
Belgium	no	83	F	no	72	0.34	0
Canada	yes	75	F	yes	66	0.57	1
Denmark	yes	80	F	no	59	0.82	0
Finland	yes	75	V	no	64	0.49	1
France	no	75	F	no	56	0.23	0
Germany	no	70	F	no	42	0.28	0
Ireland	limited	70	V	yes	78	0.39	1
Italy	no	50	F	no	80	0.26	0
Netherlands	yes	90	V	yes	53	0.71	1
New Zealand	yes	-	F	-	-	-	0
Norway	yes	70	V	no	77	0.59	1
Spain	limited	70	V	yes	85	0.4	1
Sweden	yes	80	F	no	61	0.66	0
Switzerland	no	-	V	no	36	-	0
UK	yes	75	V	yes	70	0.58	1
US	yes	80	F	yes	69	0.98	1

F = fixed and V = variable. MMSI stands for Mortgage market sophistication index.

Sources: IMF (2008); Calza, Monacelli and Stracca (2008); Gerlach and Assenmacher-Wesche (2008).

Table 3: Descriptive statistics (1980-2006). Targeters refer to the nine countries that eventually adopted IT (including observations before adoption). Non-targeters refer to other countries. IT2=1 refers to country-years observations whenever strict IT is followed.

	Non-targeters		Targeters		IT2=1	
Variables	Mean	Std.Dev.	Mean	Std.Dev.	Mean	Std.Dev.
RHOPG	2.18	6.54	2.87	8.05	4.46	5.74
HOPG	5.45	5.99	6.81	7.96	6.25	5.60
HOPCPIH	1.03	0.28	0.97	0.24	1.05	0.26
CPIG	3.45	3.22	4.06	3.25	1.91	0.99
RIRS	3.75	3.27	4.12	2.63	3.15	1.81
RIRL	4.71	2.82	4.29	2.12	4.13	1.74
NDIG	2.20	2.43	2.33	2.35	2.54	2.53
CREGDP	0.76	0.40	0.78	0.46	1.03	0.36
MS1	0.38	0.49	0.67	0.47	0.62	0.49
SAR	14.26	10.73	6.64	5.59	5.47	5.11
Nb. obs.	208		234		91	

Table 4: Probit regressions

Variables/regression	IT1	IT2	IT1 (1990-2006)	IT2 (1990-2006)	IT2 with SAR	IT2 EMU
RIRS(-1)	-0.14	-0.24	-0.13	-0.24	-0.26	-0.25
	(0.06)**	(0.06)***	(0.07)*	(0.07)***	(0.06)***	(0.06)***
RIRL(-1)	0.30	0.39	0.51	0.58	0.33	0.35
	(0.07)**	(0.07)***	(0.11)***	(0.12)***	(0.07)***	(0.07)***
NDIG(-1)	-0.01	0.03	-0.02	0.04	-0.01	0.02
	(0.04)	(0.04)	(0.05)	(0.05)**	(0.04)	(0.04)
FER	-1.98	-2.18	-2.34	-2.47	-1.91	-1.89
	(0.25)***	(0.25)***	(0.24)***	(0.25)***	(0.22)***	(0.28)***
MMSI	0.38	0.35	0.43	0.35	0.01	0.43
	(0.18)**	(0.20)*	(0.19)**	(0.21)	(0.18)	(0.23)*
CREGDP(-1)	1.71	1.65	1.34	1.22	-	1.83
	(0.23)***	(0.25)***	(0.32)***	(0.35)***	-	(0.29)***
SAR(-1)	-	-	-	-	-0.05	-
	-	-	-	-	(0.01)***	-
BKCR(-1)	-0.40	-0.75	-1.28	-1.56	-0.94	-0.66
	(0.33)**	(0.33)**	(0.45)***	(0.43)***	(0.29)***	(0.33)**
Obs.	425	425	289	289	425	367
Countries	17	17	17	17	17	18
Pseudo-R^2	0.39	0.42	0.46	0.48	0.37	0.40

Robust standard errors are shown in parentheses.

Table 5: Estimates of the ATT of inflation targeting: alternative specifications and dependent variables (Kernel matching method).

		Dependent variable / Model specification				
		RHOPG baseline	RHOPG no out.	RHOPG SAR	HOPG	HOPCPIH
		(1)	(2)	(3)	(4)	(5)
Controls over whole sample period (1980-2006)						
IT1	ATT	2.78	2.73	0.63	3.28	0.09
	SD	(1.20)	(1.18)	(0.92)	(1.51)	(0.04)
	Treated /Control units	99/326	99/323	98/322	99/326	99/315
IT2	ATT	2.64	2.58	1.28	2.32	0.08
	SD	(0.99)	(0.96)	(0.95)	(1.07)	(0.04)
	Treated /Control units	92/333	92/330	91/329	92/333	92/322
Controls over 1990-2006						
IT1	ATT	5.97	5.64	2.91	5.65	0.12
	SD	(1.62)	(1.34)	(1.18)	(1.62)	(0.04)
	Treated /Control units	99/190	99/187	98/190	99/190	99/190
IT2	ATT	4.35	4.12	2.86	4.18	0.12
	SD	(1.21)	(1.16)	(1.15)	(1.29)	(0.04)
	Treated /Control units	92/197	92/194	91/197	92/197	92/197
Bootstrapped standard errors for ATT are reported in parenthesis (1000 reps)						

Table 6: Estimates of the ATT of inflation targeting (variant with EMU as an inflation targeter): alternative specifications and dependent variables (Kernel matching method).

		Dependent variable / Model specification				
		RHOPG baseline	RHOPG no out.	RHOPG SAR	HOPG	HOPCPIH
		(1)	(2)	(3)	(4)	(5)
Controls over whole sample period (1980-2006)						
IT1	ATT	3.07	2.81	0.86	2.84	0.08
	SD	(1.32)	(1.26)	(0.93)	(1.41)	(0.04)
	Treated /Control units	105/262	105/260	104/258	105/262	105/251
IT2	ATT	2.72	2.60	1.32	2.18	0.10
	SD	(1.16)	(1.10)	(0.92)	(1.09)	(0.04)
	Treated /Control units	98/269	98/267	97/265	98/269	98/258
Controls over 1990-2006						
IT1	ATT	4.40	4.42	2.72	5.06	0.14
	SD	(1.52)	(1.24)	(1.10)	(1.47)	(0.04)
	Treated /Control units	105/126	105/124	104/126	105/126	105/126
IT2	ATT	3.79	3.94	2.62	3.93	0.12
	SD	(1.10)	(1.10)	(1.08)	(1.16)	(0.03)
	Treated /Control units	98/133	98/131	97/133	98/133	98/133
Bootstrapped standard errors for ATT are reported in parenthesis (1000 reps)						

Table 7: Estimates of the ATT of inflation targeting on real housing price growth (baseline): sequential exclusion of individual countries from the sample (Kernel matching method).

Country excluded	IT1	IT2	IT1 (post '90)	IT2 (post '90)
AU	3.69*	2.37	6.66**	4.04**
BE	2.85*	2.62*	5.81**	4.25**
CA	3.88**	3.11**	5.07**	4.80**
CH	-0.52	2.66	5.99	3.52*
DE	2.60*	2.55*	5.55**	4.16**
DK	3.08*	2.55*	5.98**	4.17**
ES	2.81	3.04**	5.58**	4.09**
FI	3.32**	3.34**	6.03**	4.15**
FR	2.71	2.64*	5.86**	4.28**
GB	2.36	2.32*	5.11**	3.58**
IE	2.96*	3.08**	6.04**	4.31**
IT	2.57	2.48*	6.01**	4.10**
NL	2.84*	3.37**	6.21**	4.33**
NO	2.89*	2.63**	7.44**	4.82**
NZ	2.71*	3.03**	5.56**	3.62**
SE	2.25	3.52**	5.72**	4.77**
US	2.29	2.77*	4.42*	3.81**

Note: * and ** denote here significativity at the 5%, resp. 1% level

Table 8: Indicators of covariate balancing, before and after matching (strict targeting IT2, controls over 1980-2006, kernel matching method)

		ATT	Probit ps.-R^2 before	Probit ps.-R^2 after	Pr $>\chi^2$ after	Median bias before	Median bias after
				Whole sample			
RHOPG	IT1	2.78	0.39	0.03	19.64	13.25	0.32
	IT2	2.64	0.42	0.01	23.27	8.79	0.93
HOPCPIH	IT1	0.09	0.40	0.02	17.76	10.21	0.59
	IT2	0.09	0.43	0.02	25.44	6.37	0.78
				Post-1990 sample			
RHOPG	IT1	5.97	0.46	0.01	26.66	6.91	0.98
	IT2	4.35	0.48	0.03	23.90	8.35	0.44
HOPCPIH	IT1	0.13	0.46	0.01	26.66	6.91	0.98
	IT2	0.12	0.48	0.03	23.90	8.35	0.44
Variant: EMU as IT country							
				Whole sample			
RHOPG	IT1	3.07	0.37	0.04	33.14	9.94	0.11
	IT2	2.72	0.40	0.01	39.03	4.20	0.98
HOPCPIH	IT1	0.08	0.38	0.03	32.55	5.85	0.42
	IT2	0.10	0.41	0.00	39.07	4.03	0.99
				Post-1990 sample			
RHOPG	IT1	4.40	0.37	0.01	27.85	5.94	0.94
	IT2	3.78	0.40	0.02	41.14	5.33	0.63
HOPCPIH	IT1	0.14	0.37	0.01	27.85	5.94	0.94
	IT2	0.12	0.40	0.02	41.14	5.33	0.63

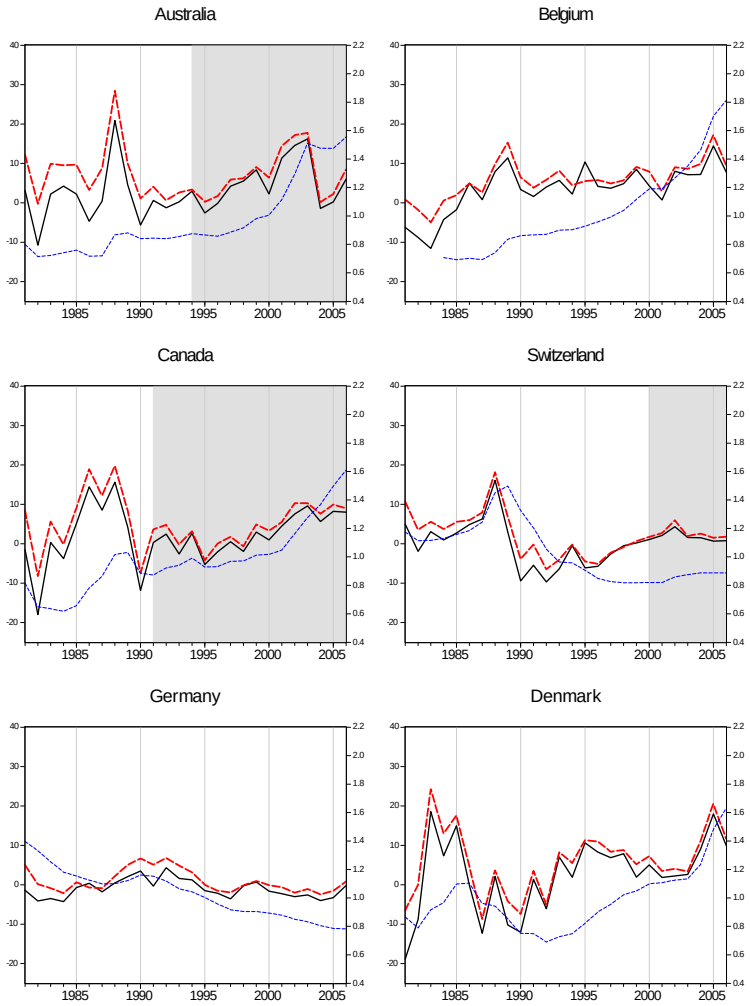

Figure 1: House prices and house price to rent ratio. Real house price growth (yoy in %, left axis, solid line), nominal house price growth growth (yoy in %, left axis, dashes), house price to rent ratio (right axis, dotted line). Shaded area indicates inflation targeting regime.

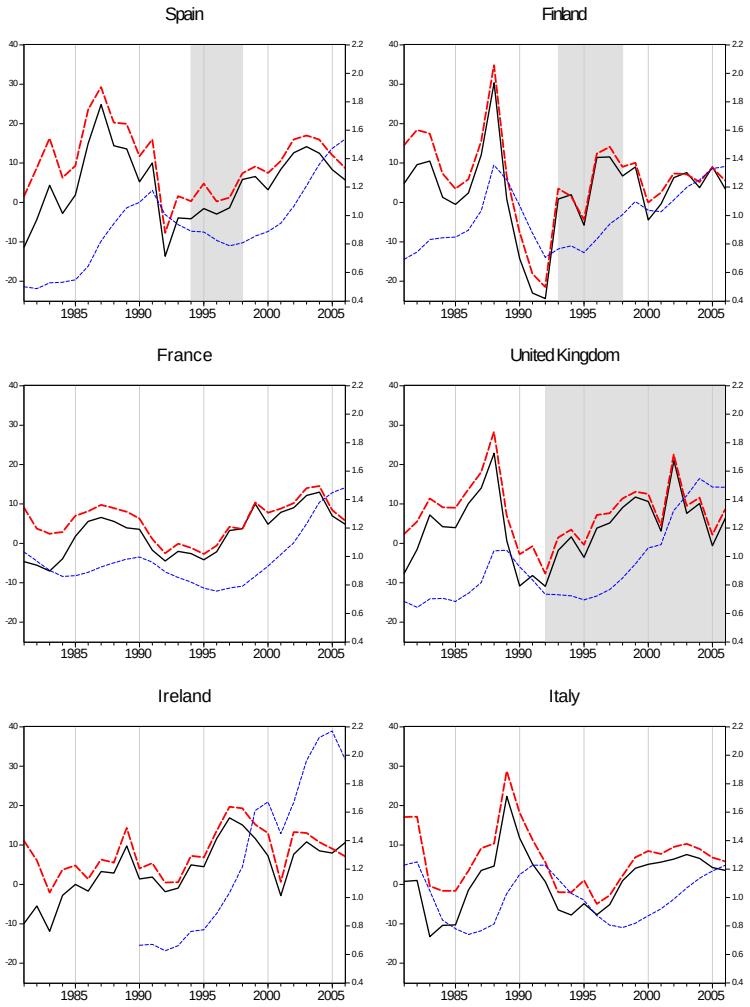

Figure 2: House prices and house price to rent ratio (cont'd).

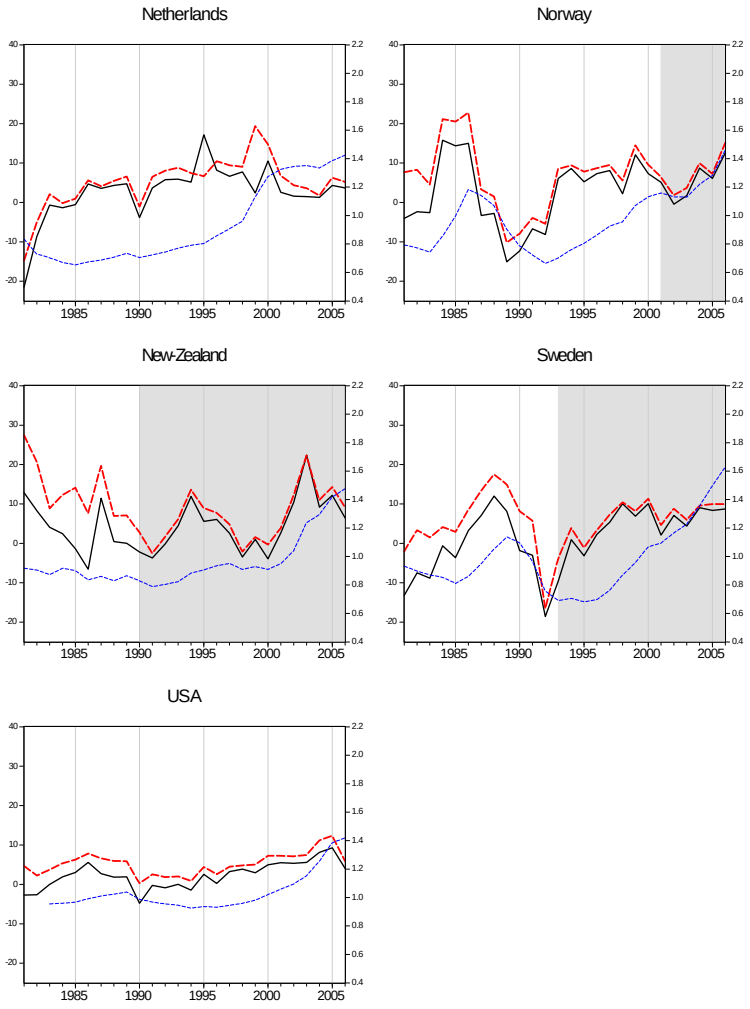

Figure 3: House prices and house price to rent ratio (cont'd).

Figure 4:

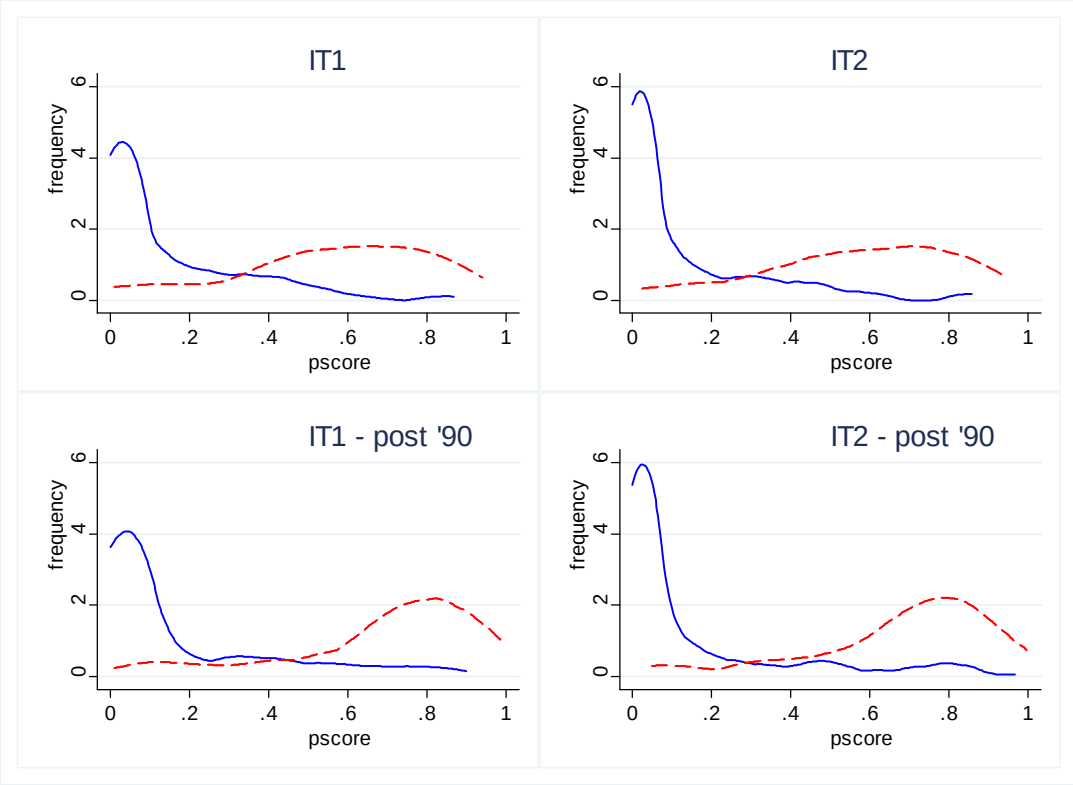

Figure 5: Densities of estimated propensity scores. First row: controls over 1980-2006, second row: controls over 1990-2006. First column: IT1, second column: IT2.

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