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A Structural VAR Investigation

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Leaning Against House Prices: A Structural VAR Investigation

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Abstract

Evidence from monetary VARs suggests that in the U.S., Canada, and the U.K. the impact of monetary shocks on real house prices is about three to five times as large as that on real GDP. Although these trade-offs are not manifestly unfavorable, in the light of the large differences in the magnitudes of house prices and GDP fluctuations, a monetary policy of leaning against the former would inevitably entail significant losses in the latter. I use the identified VARs in order to explore the corresponding trade-offs associated with a monetary policy of weakly, but systematically leaning against house prices. Results from 'modest' (in the sense of Leeper and Zha, 2003) policy

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counterfactuals suggest that, in population, the impact on real house prices is about three times as large as that on real GDP for all of the three countries. Within the specific context of the upsurge in U.S. house prices which pre-dated the financial crisis, a shortfall of one per cent of GDP would have been associated with a decline in real house prices by about four per cent.

Keywords: Structural VARs; house prices; sign restrictions; zero restrictions.

E30; E32

1 Introduction

Both academics and policymakers are largely skeptical about the effectiveness of monetary policies aimed at leaning against credit growth and asset prices' fluctuations. The dominant consensus, expressed e.g. by Svensson (2017), is that whereas the costs of such policies, in terms of output losses and increases in unemployment, are certain and sizeable, the potential benefits in terms of decreases in the probabilities of future crises are much more uncertain, and likely small.

An alternative, minority position associated mainly with the Bank for International Settlements (see e.g. BIS, 2014, 2016), advocates instead a policy of systematically reacting to measures of 'excessive' credit growth or 'disequilibria' in asset prices, by marginally increasing monetary policy rates over and above the values dictated by an exclusive focus on inflation and real activity. As discussed, e.g., by Filardo and Rungcharoenkitkul (2016, p. 4), a key rationale behind this position—stressed by authors such as, e.g., Charles Kindleberger[1]—is that the policy rate, beyond being a powerful macroeconomic tool, plays a crucial role in the determination of the prices of leverage and risk. According to this view, macroprudential instruments and a monetary policy of leaning against credit and asset prices fluctuations should be regarded as complements, rather than substitutes. In particular, because of the

[1] See e.g. Kindleberger and Haliber (2005).

ubiquitous role played by interest rates in the economy, such a policy would allow the central bank to reach into all the nooks and crannies, thus also affecting credit and asset markets' segments not explicitly targeted by macroprudential policies.[2]

In this paper I use monetary VARs in order to explore the trade-off faced by central banks between stabilizing house prices and stabilizing economic activity. I start by proposing a simple methodology for exploring the impact of monetary policy on house prices. As in Arias *et al.* (2019), my strategy for identifying monetary shocks is based on the notion of imposing either zero or sign restrictions on the contemporaneous response of the monetary policy rate to the other series in the VAR. To this set of constraints I then add additional restrictions, as in e.g. Uhlig (2005), on the signs of the responses of the monetary policy rate, commodity prices and monetary aggregates to a monetary shock. Results for the U.S. and Canada suggest that the impact of monetary shocks on real house prices is about three times as large as that on real GDP. The situation appears more favorable for the U.K.—where a larger fraction of mortgages is variable-rate, and average borrowing costs are therefore more sensitive to fluctuations in the monetary policy rate—with the trade-off being equal to nearly five-to-one. Although these trade-offs are not manifestly unfavorable, in the light of the large differences in the magnitudes of house prices and GDP fluctuations, a monetary policy of leaning against the former would inevitably entail significant losses in the latter.

Based on the identified VARs I then propose a simple methodology for exploring the corresponding trade-off associated with a policy of weakly, but systematically leaning against house prices. My approach combines elements of Sims and Zha (2006) and Leeper and Zha (2003). Specifically, from the former paper I borrow the notion of performing a counterfactual exercise in which the monetary policy rule is made marginally more aggressive towards

[2] In the words of Stein (2013), 'while monetary policy may not be quite the right tool for the job, it has one important advantage relative to supervision and regulation, namely that it gets in all of the cracks. The one thing that a commercial bank, a broker-dealer, an offshore hedge fund, and a special purpose ABCP vehicle have in common is that they all face the same set of market interest rates. To the extent that market rates exert an influence on risk appetite, or on the incentives to engage in maturity transformation, changes in rates may reach into corners of the market that supervision and regulation cannot.'

fluctuations in a specific variable—in their case inflation,[3] within the present context the ratio between house prices and rents—by appropriately perturbing the coefficients on that variable within the VAR's structural monetary policy rule. From the latter paper I borrow the methodology for assessing the 'modesty' of a specific policy intervention—in the sense of not being detectable by economic agents, thus not triggering changes in their expectation-formation mechanism—based on the notion that the resulting paths for the variables of interest do not deviate 'too much' from those expected based on their previous historical developments. Working within the set of modest policy interventions, I obtain the following main results:

(i) evidence for any of the three countries suggests that, *in population*, the impact of such a policy on real house prices is about three times as large as that on real GDP.

(ii) Within the specific context of the upsurge in U.S. house prices which pre-dated the financial crisis—i.e., with reference to that *specific historical episode*—a shortfall of one per cent of GDP would have been associated with a decline in real house prices by about four per cent.

In order to better understand the implications of these results, let us focus upon the U.S. A plausible measure of over-valuation of real house prices (the 12-month moving average of the deviation of the log house price/rent ratio from its pre-1995 average) had reached a peak of 0.258 in July 2006. Assuming that 25.8 per cent was the true extent of over-valuation of U.S. real house prices, my results suggest that eliminating *just one-tenth* of it would have required a cumulative output loss of about 2 per cent of GDP. Many (and possibly most) observers would regard this as an excessive price to pay, especially considering that this would only eliminate a small fraction of the overvaluation.

A limitation of these results is that, because of the Lucas critique, my estimates only provide a *lower bound* on the effectiveness of a policy of leaning against house prices. As ar-

[3] See Sims and Zha's (2006, Section VI, Figure 8) 'Greenspan Hawk' counterfactual, in which the coefficient on inflation in the VAR's monetary policy rule had been set equal to twice its estimated value.

gued, e.g., by BIS (2016, pp. 74-75), if such a policy were explicitly announced, and regarded as credible by economic agents, they would necessarily factor it into their decision-making process. As a result, every increase in house prices compared to rents would generate a corresponding expectation of an increase in the monetary policy rate, and therefore in mortgage rates, which would automatically counteract the original house price increase. Because of this, the trade-off between stabilizing house prices and stabilizing economic activity would ultimately turn out to be more favorable compared to that identified herein. The only way to assess the strength of such 'expectational channel', however, would be to use a DSGE model of the housing market. Under this respect, Rudebusch's (2005) evidence pertaining to perturbations of the coefficients of 'standard' monetary policy rules[4] suggests that their impact on the reduced-form structure of the model is, in fact, quite modest: in particular, it is so modest that it cannot be detected based on standard statistical tests. This suggests that the modest policy counterfactuals analized herein might well capture the *first-order impact* of a policy of systematically leaning against house prices.

The two approaches I consider for the purpose of exploring the trade-off faced by central banks between stabilizing house prices and stabilizing economic activity are completely different: whereas the former focuses on the random component of monetary policy, the latter pertains to the systematic reaction of the monetary policy rate to housing market developements. In general, we should have no presumption that the trade-offs they identify should be the same, and the two approaches should therefore be regarded as complementary.

The paper is organized as follows. The next section provides details about the monetary VARs I will use throughout the paper, in terms of both estimation, and the identification of monetary policy shocks. Section 3 discusses the response of the economy to a monetary shock, and analyzes the trade-off between stabilizing real house prices and stabilizing economic activity induced by such shocks. Section 4 discusses the policy counterfactuals I will

[4] I.e., rules in which the monetary policy rate reacts to inflation and the output gap.

study in the rest of the paper, and how I will assess whether they are, or are not, 'modest' in the sense of Leeper and Zha (2003). Section 5 characterizes in population the trade-off between stabilizing house prices and stabilizing economic activity associated with such counterfactuals, whereas Section 6 will study their impact with reference to a specific historical episode: the upsurge in U.S. house prices that pre-dated the Great Recession. Section 7 concludes and outlines possible directions for future research. As I discuss, the methodology explored in this paper naturally lends itself to additional applications, such as, first and foremost, studying the impact of a policy of leaning against the credit cycle.

2 The Monetary VAR

In what follows I will work with the VAR(p) model

$$Y_t = B_0 + B_1 Y_{t-1} + ... + B_p Y_{t-p} + u_t, \qquad E[u_t u_t'] = \Omega \tag{1}$$

where Y_t features the central bank's monetary policy rate; a mortgage rate; and the logarithms of real GDP, a core price index (the core PCE deflator for the U.S., and the core CPI for Canada and the U.K.),[5] an index of commodity prices, a house prices index, the ratio between the house prices index and a rent index, the monetary aggregate M2, and the reserves held by commercial banks at the central bank (for the U.S., non-borrowed reserves).[6] The data are described in detail in Online Appendix A, and they are shown in Figure 1. The sample periods are January 1983-December 2007 for the United States, January 1983-November 2018 for Canada, and January 1983-April 2006 for the United Kingdom.

As for the United States, following (e.g.) Arias *et al.* (2019) I start the sample in Jan-

[5] The Federal Reserve (which does not have an official inflation target defined by the U.S. Congress) has repeatedly stated that it regards the core PCE deflator as its preferred measure of price developements. On the other hand, both the Bank of Canada and the Bank of England have inflation targets defined by the respective parliaments in terms of the CPI, which motivates my choice of using the core CPI.

[6] Non-borrowed reserves are the ideal aggregate, but they are only available for the U.S.. For Canada and the U.K. I have therefore been compelled to work with the total reserves aggregate.

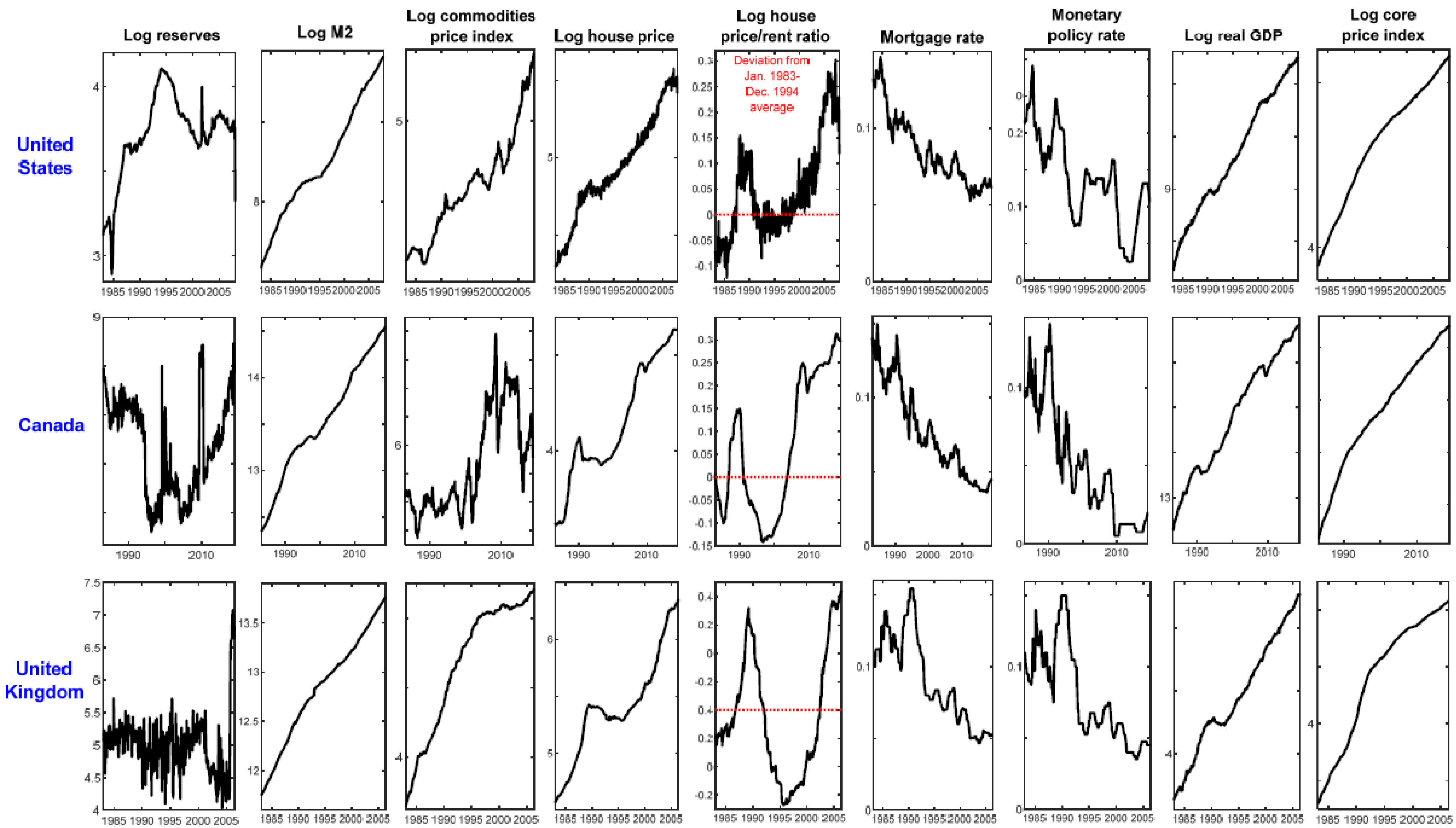

Figure 1 The raw data

uary 1983 for two reasons. First, in order to exclude the Great Inflation episode. Second, because a substantial body of work—see in particular Boivin and Giannoni (2006)—has suggested that the transmission mechanism of U.S. monetary policy may have materially changed following the end of the Volcker disinflation. As for the end of the sample, due to the extraordinary economic turbulence associated with the financial crisis, non-borrowed reserves turned negative in January to November 2008, so that they cannot be entered into the VAR in logarithms (further, in December 2008 the federal funds rate reached, *de facto*, the zero lower bound).

As for Canada, the beginning of the sample is dictated, once again, by the need to exclude the Great Inflation episode. As for the end of the sample I have chosen to include the entire period following the outbreak of the financial crisis because the zero lower bound has never been a constraint on Canadian monetary policy.[7] Further, since the Bank of Canada has never engaged in quantitative easing policies, the reserves held by commercial banks at the central bank have not exhibited, following the outbreak of the financial crisis, the explosion which has instead characterized reserves in the U.S. and the U.K.: their only anomalous feature during this period is a temporary and sizeable increase between April 2009 and June 2010, which in what follows I control for using a dummy variable.

As for the United Kingdom, the beginning of the sample is dictated by data availability, and by the need to exclude the Great Inflation episode. As for the end of the sample, the reform of money market operating procedures introduced by the *Bank of England* in May 2006, by allowing commercial banks to earn interest on the reserves they hold at the central bank, introduced a discontinuity in the series for reserves, which starting in May 2006 has literally skyrocketed. Because of this, I have chosen to end the sample in April 2006.

[7] Since January 1983, the minimum value taken by the Bank of Canada's discount rate has been 0.5 per cent (between April 2009 and May 2010).

2.1 Estimation of the reduced-form VAR

The VAR is estimated *via* Bayesian methods as in Uhlig (1998, 2005). Specifically, Uhlig's approach is followed exactly in terms of both distributional assumptions—the distributions for the VAR's coefficients and its covariance matrix are postulated to belong to the Normal-Wishart family—and of priors. For estimation details the reader is therefore referred either to the Appendix of Uhlig (1998), or to Appendix B of Uhlig (2005). Results are based on 100,000 draws from the posterior distribution of the VAR's reduced-form coefficients and the covariance matrix of its reduced-form innovations (the draws are computed exactly as in Uhlig (1998, 2005)). I set the lag order to p=6.[8]

2.2 Identification of the monetary policy shocks

Conceptually in line with Arias *et al.* (2019), I identify monetary policy shocks by combining zero and sign restrictions. Specifically, monetary shocks are identified based on the restrictions that

(I) the central bank's monetary policy rate reacts contemporaneously only to real GDP, the core price index, and commodity prices. Its reaction to any of the three variables is positive.[9]

(II) Both on impact, and over the subsequent two years, a monetary shock induces a non-negative response in the monetary policy rate, and non-positive responses in commodity prices, reserves, and M2. For all other variables, the responses are left unrestricted both on impact, and at all subsequent horizons.[10]

[8] I also experimented with 12 lags (these results are available upon request). In general, evidence was qualitatively the same as with 6 lags, but some of the impulse-response functions turned out to be somewhat 'jagged', and characterized by much greater uncertainty. Because of this, I tend to prefer the set of results obtained by setting p=6.

[9] This is in line with Arias *et al.*'s (2019) Restrictions 1 and 2.

[10] Restricting the response of the monetary policy rate and a monetary aggregate up to K periods after impact is standard in the literature. E.g., Uhlig (2005) restricted the federal funds rate, non-borrowed reserves, and the price level. Here I restrict the additional monetary aggregate I have beyond reserves (i.e., M2), and the commodity price index—rather than the core price index—because a negative impact of a

Commodity prices and the two monetary aggregates play an important role in disciplining my sign-based identification of monetary shocks. E.g., for the U.S. I explored the robustness of my IRFs to identifying monetary shocks as in Gertler and Karadi (2015, henceforth GK). Specifically, in my set of nine variables I replaced the federal funds rate with GK's preferred monetary policy indicator, the 1-year government bond yield, and I identified the monetary shocks exactly as in GK, based on the full set of their five instruments. The results from this exercise (shown in Figure A.1 in the Online Appendix) can be summarized as follows: (*i*) the responses of GDP, the PCE deflator, real rents, and real house prices are qualitatively the same, and quantitatively close to those based on my approach (shown in Figure 2 below), whereas the response of the mortgage rate is qualitatively the same, but stronger; however (*ii*) commodity prices exhibit a strongly statistically significant temporary increase, and (*iii*) non-borrowed reserves exhibit a strongly statistically significant increase on impact. Since a shock causing increases in both non-borrowed reserves and commodity prices can hardly be thought of as a contractionary monetary shock, the implication is that some of the 'monetary shocks' belonging to the set identified *via* GK's approach should likely not be regarded as such.[11] In order to rule out such implausible monetary shocks, my identification strategy therefore relies on disciplining the responses of commodity prices and monetary aggregates.

Restrictions (I) and (II) can be efficiently implemented *via* the algorithm for combining zero and sign restrictions introduced by Arias *et al.* (2018). For any of the draws from the posterior distribution of the VAR's reduced-form coefficients, I consider 1,000 random rotation matrices implementing the zero exclusion restrictions on the contemporaneous coefficients of the VAR's structural monetary policy rule,[12] which I generate *via* Arias *et al.*'s

contractionary monetary shock is more plausible for commodity prices. Sims and Zha (2006) also 'introduce stochastic prior information favoring a negative contemporaneous response of [Divisia M2] to the interest rate'.

[11] It is to be noticed that GK's set of variables did not include either commodity prices or monetary aggregates, so that this issue did not arise in their analysis.

[12] So, to be clear, the restriction that the contemporaneous coefficients in the structural monetary policy rule are equal to zero for all variables except real GDP, the core price index, and commodity prices.

(2018) Algorithm 5. Then, if the sign restrictions on both the contemporaneous coefficients of the VAR's structural monetary rule, and the relevant IRFs, are all satisfied, I keep the draw for the resulting structural VAR. Otherwise, I discard it. The number of successful draws is equal to 5,522 for the U.S., 4,463 for Canada, and 2,493 for the U.K. From now on, I will use the word 'draw' as a shorthand for 'successful draw', that is: a draw for which all of the zero and sign restrictions have been satisfied, and which I have therefore kept.

3 The Response of the Economy to a Monetary Shock

Figure 2 shows, for any of the three countries, the response of the economy to a contractionary monetary policy shock.[13] The impulse-response functions (IRFs) have been normalized in such a way that, on impact, the median response of the monetary policy rate is equal to 25 basis points. The IRFs accord well with both conventional wisdom about the impact of a monetary shock, and previous evidence. Specifically,

(1) in all countries the monetary contraction causes a decline in GDP. Statistical significance is very strong for both Canada and the U.K., whereas it is weaker for the U.S., for which the decline is only barely significant at the one standard deviation level. It is to be noticed, however, that at horizons beyond 2 years after impact the bulk of the mass of the distribution of the IRF of U.S. GDP is below zero, thus suggesting that the likelihood of a negative impact is significantly greater than that of a positive one. Based on median estimates, the peak impact is significantly greater, in absolute value, for the U.K. (-0.409 per cent) than for either the U.S. or Canada (-0.175 and -0.180 per cent, respectively).

(2) One possible explanation for such differential impact of a monetary contraction on GDP in the U.K, compared to Canada and the U.S., has to do with the response of borrowing costs for housing (i.e., the mortgage rate), and especially of real house prices. Whereas in

[13] Figure A.2 in the Online Appendix shows the fractions of forecast error variance explained by monetary shocks. In line with previous evidence, they are near-uniformly small, or very small.

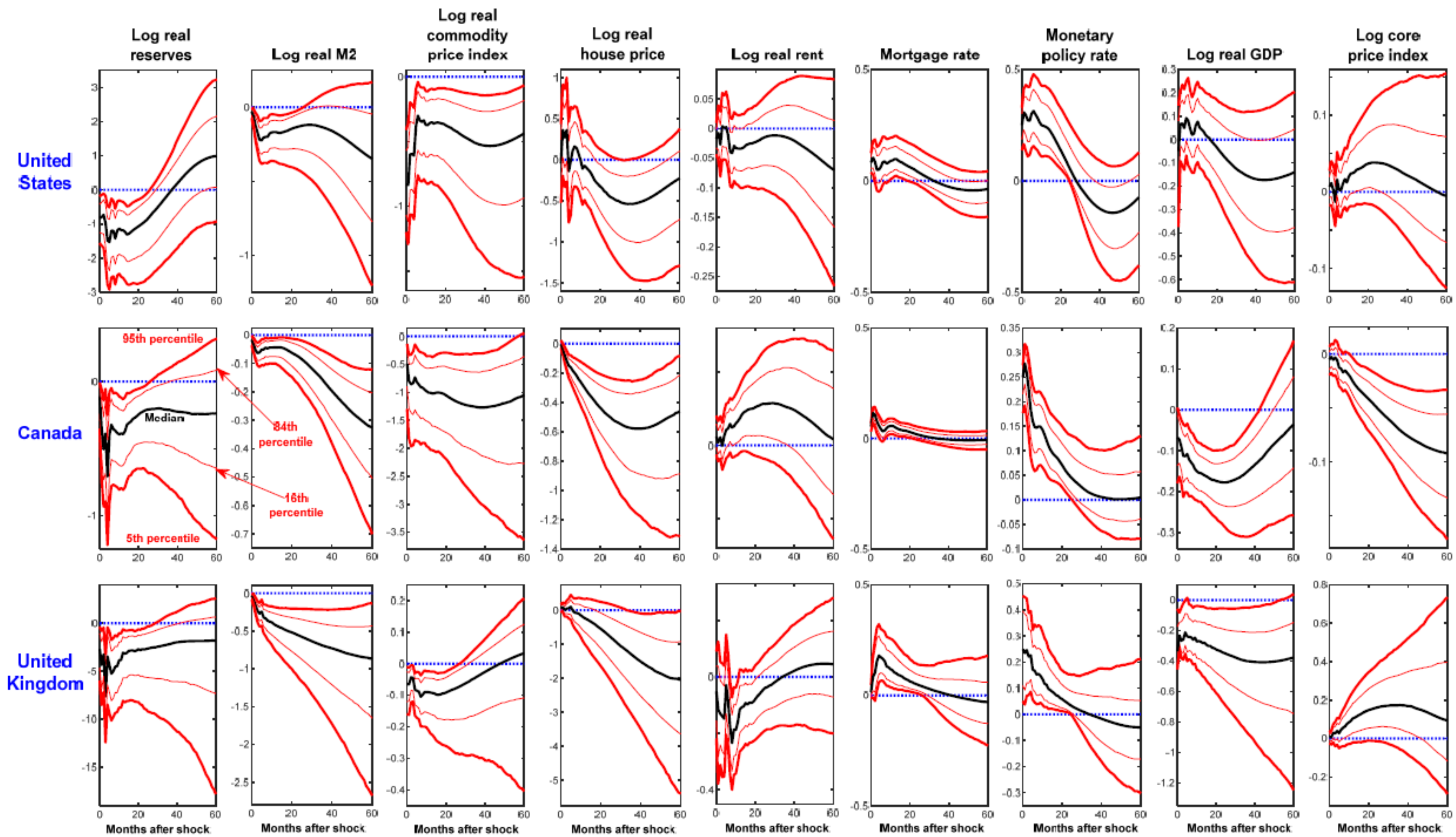

Figure 2 Impulse-response functions to a monetary policy shock (in percentage points)

both the U.S. and Canada mortgages are predominantly *fixed-rate*, in the U.K. a large fraction is either *variable-rate*—with the mortgage rate being typically linked to the Bank of England's monetary policy rate—or fixed-rate for a short period of time (e.g., one or two years), and then variable-rate after that.[14] As a consequence, average borrowing costs for housing in the U.K. are more sensitive to fluctuations in the monetary policy rate than they are in either the U.S. or Canada. And in fact, as the sixth column of Figure 1 shows, a 25 basis points increase in the monetary policy rate has a materially different impact on the three countries' benchmark mortgages rates. Whereas in the U.S. and Canada the impact is quite muted and comparatively small, with peak increases (based on median estimates) equal to 10.5 and 12 basis points respectively, in the U.K. the reaction of the mortgage rate is quantitatively much closer to that of the monetary policy rate, with a median peak increase equal to 18.2 basis points, and a markedly more persistent and drawn-out response. In particular, at all horizons greater than 4 months after impact, the median response of the mortgage rate is virtually indistinguishable from that of the monetary policy rate. Since, for all countries, the mortgage rate entering the VAR is the 'benchmark rate'—i.e. the one that is most relevant for house purchases—this suggests that the ability of a monetary contraction to 'cool off' the housing market is greater in the U.K. than in either the U.S. or Canada. The response of real house prices provides support to this conjecture. Whereas in the U.S. and Canada the median peak response is equal to -0.545 and -0.585 per cent, respectively, in the U.K. it is equal to -2.050 per cent. One possible explanation for the significantly stronger impact of a monetary contraction on GDP in the U.K, compared to Canada and the U.S., is therefore that, due to the much larger impact on real house prices, the transmission mechanism of monetary policy is significantly stronger in the U.K.

(3) In both Canada and the U.S. the response of the real rent is statistically insignificant

[14] In fact (see the data Appendix A), the very way in which the mortgage rates—which in any of the three countries are the 'benchmark' rates—are labelled shows this. In the U.S. the mortgage rate is labelled as '30-year fixed rate mortgage average', whereas in the U.K. it is labelled as 'Household variable mortgage rate'. For Canada, the '5-year conventional mortgage lending rate' is not explicitly labelled, but it is fixed-rate.

at all horizons, whereas in the U.K. it exhibits a statistically significant but short-lived decrease.

(4) In Canada the price level exhibits a statistically significant decline, whereas in the U.S. its response is insignificant at all horizons, and in the U.K. it exhibits a temporary increase which is significant at the 1-, but not at the 2-standard deviations level.

(5) The responses of monetary aggregates and commodity prices, which are restricted to be negative for two years after the shock, often become insignificant at longer horizons.

The evidence for the U.S. is broadly in line with, e.g., Sims and Zha's (2006, see their Figure 2). In particular, (*i*) their estimated response for the very same price index (the core PCE deflator) is also statistically insignificant at all horizons, and its profile is very similar to that reported in my Figure 2; and (*ii*) their response for real GDP is also negative, statistically significant, and quantitatively close to mine.[15]

On the other hand, the response of U.S. real house prices is weaker than that found in previous studies, and exhibits a different time profile, being delayed and U-shaped, instead of 'front-loaded' and monotonically decreasing in absolute value. Based on a dynamic factor model identified *via* sign restrictions, for example, Del Negro and Otrok (2007) estimated the impact of a one-standard deviation (about 0.16 per cent) monetary shock on U.S. real house prices as equal to about -0.6 per cent on impact,[16] and monotonically converging to zero after that (see their Figure 5). Qualitatively and quantitatively very similar results are obtained by Iacoviello and Neri (2010) based on an estimated DSGE model of the U.S. housing market (see their Figure 3).

[15] A precise quantitative comparison with Sims and Zha's (2006) results is not entirely straightforward, since they did not normalize monetary shocks. Their estimated impact of monetary shocks on the federal funds rate at t=0, however, was equal to about 0.0015, whereas the maximum negative impacts on GDP was equal to about -0.0011. This implies that a 25 basis points shock to the federal funds rate should lead to a peak response in GDP equal to -0.1833 percentage points, which is quantitatively close to my estimate.

[16] For a 25 basis points monetary shock this corresponds to an impact of -0.94 percentage point on real house prices.

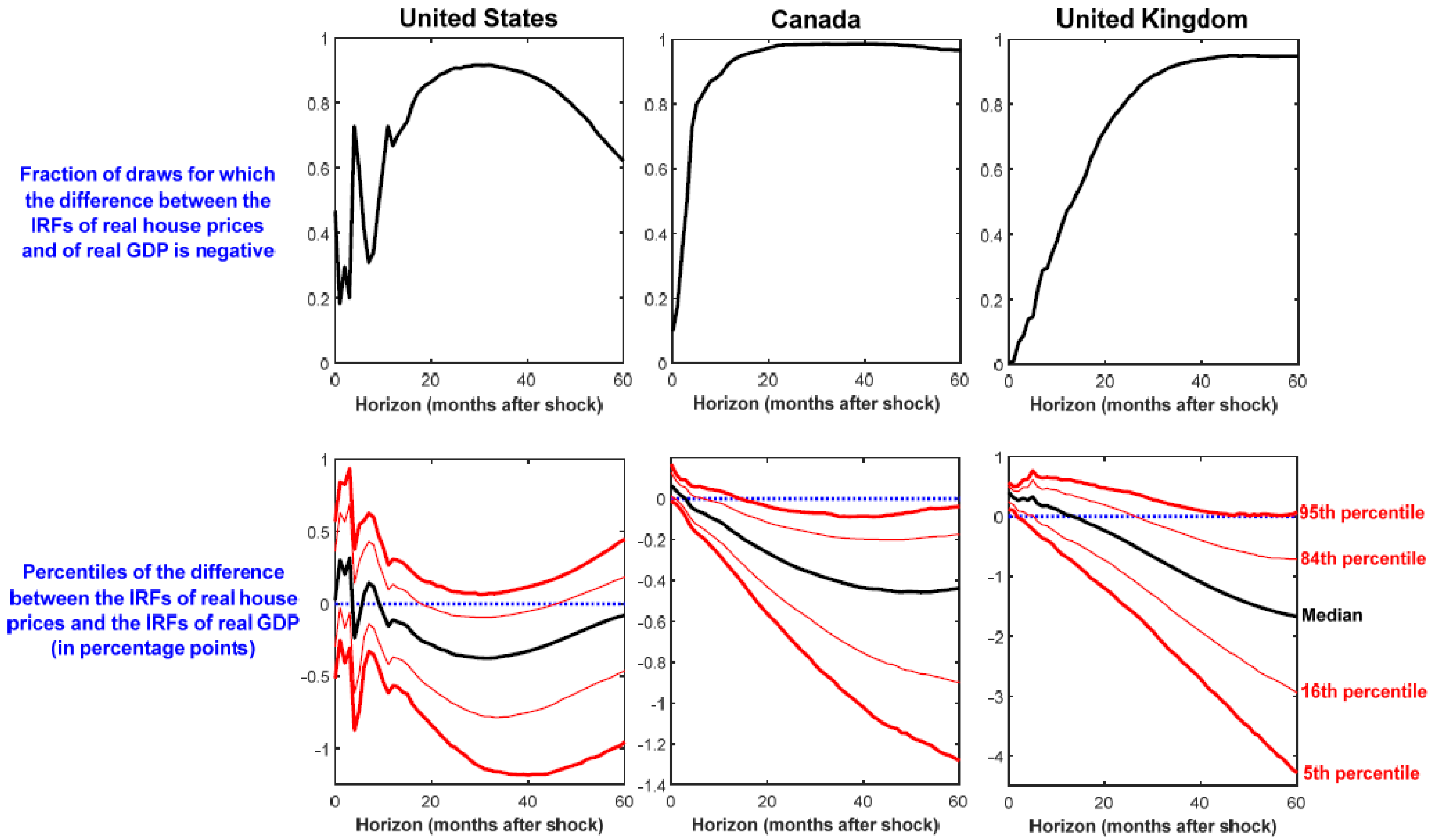

Figure 3 The trade-off between stabilizing real house prices and stabilizing real GDP associated with monetary policy shocks

3.1 The trade-off between stabilizing house prices and stabilizing economic activity

Figure 3 reports evidence on the trade-off between stabilizing real house prices and stabilizing real GDP associated with monetary policy shocks. The figure shows (in the first row) the fractions of draws from the posterior distribution for which the difference between the IRFs of real house prices and of real GDP is negative; and (in the second row) the median and the one- and two-standard deviations percentiles of the posterior distribution of the difference between the two series' IRFs. Evidence that the response of real house prices is stronger than that of real GDP is very strong for Canada, and for the U.K. at all horizons beyond 2 years after impact, whereas it is weaker for the U.S., for which the difference between the two series' IRFs is significantly different from zero only at the one standard deviation level. This evidence is in line with the previously discussed differences between the two series' peak responses: whereas for the U.K. the ratio between the median peak responses of real house prices and real GDP is equal to 5.42, for Canada and the U.S. it is equal to 3.25 and 3.11, respectively. This suggests that whereas the Bank of England may face a comparatively more favorable trade-off between stabilizing house prices and stabilizing real GDP, for both the Bank of Canada and the Federal Reserve the trade-off is not an especially favorable one.

In the light of the fact that fluctuations in real house prices are significantly more volatile than those in real GDP, this suggests that, in order to be able to have a material impact on the former, especially the Bank of Canada and the Federal Reserve ought to be willing to tolerate sizeable, and possibly even large fluctuations in the latter. Some simple 'back of the envelope' calculations provide a rough indication of the magnitudes involved. For the sake of the argument, let us suppose that, based on the previously discussed evidence, the trade-off between stabilizing real house prices and stabilizing real GDP is three-for-one for the U.S. and Canada, and five-for-one for the U.K. Further, let us suppose that real house prices are currently overvalued by 10 per cent,[17] and that the central bank knows this with absolute certainty. The implications of these numbers are sobering. In order to completely eliminate the overvaluation in house prices, both the Federal Reserve and the Bank of Canada should be willing to accept a shortfall of GDP equal to 3.33 per cent, whereas the trade-off for the Bank of England, although more favorable, would still require real GDP to be two percentage points below what it would otherwise have been. Even eliminating only half of the overvaluation, which might be regarded as a meaningful 'insurance policy' against the consequences of a future crash in house prices, would still entail non-negligible losses in real GDP.

3.2 Summing up

The evidence produced by random variation in the monetary policy rate around the path induced by the systematic component of monetary policy suggests that, among the three central banks, only the Bank of England may face a not entirely unfavorable trade-off between stabilizing house prices and stabilizing economic activity. I now turn to exploring the trade-offs associated with 'modest' (in the sense of Leeper and Zha, 2003) policy counterfactuals, in which the monetary policy rate reacts, weakly but systematically, to deviations of the log ratio between house prices and rents from the average value it had taken over the period January 1983-December 1994.

4 Policy Counterfactuals

As first pointed out by Sargent (1979), the crucial limitation of VAR-based policy counterfactuals is that a change in a policy parameter within the underlying general equilibrium model automatically causes a change in the VAR reduced-form representation of the model,

[17] An overvaluation by 10 per cent is hardly implausible: e.g., linearly detrended U.S. real house prices peaked at about 15 per cent in early 2007.

thus invalidating the use of the originally estimated structure in order to perform the counterfactual. A first reaction to this problem has been to simply eschew SVAR-based policy counterfactuals, and to only perform such exercises based on general equilibrium models clearly specifying the primitive features of the economy (technology and preferences), the policy rules, and the characteristics of the driving stochastic processes. A second reaction, associated with Leeper and Zha (2003), has been to argue that, although such models are obviously needed in order to analyze the impact of comparatively large changes in the policy coefficients, for 'modest' changes (i.e. changes so small that agents are not able to detect them, thus severing the expectational channel underlying Sargent's argument) VARs are perfectly appropriate. Conceptually related to this is the point, made by Rudebusch (2005) with reference to changes in the parameters of standard monetary policy rules, that the impact of changes in the policy parameters on the reduced-form structure of the economy is, in fact, quite modest, in particular so modest as to not be detectable based on standard statistical tests.

In this section I start by detailing how the posterior distribution of the parameters of the VAR's structural monetary policy rule is perturbed, and I then turn to discussing how the issue of whether the resulting policy counterfactuals are, or are not modest in the sense of Leeper and Zha (2003) is assessed.

4.1 Perturbing the VAR's structural monetary policy rule

Let $Y_t = B_{0,j} + B_{1,j}Y_{t-1} + ... + B_{p,j}Y_{t-p} + A_{0,j}\epsilon_t$ be the structural VAR associated with draw j from the posterior distribution, and let $Y_t'[A_{0,j}^{-1}]' = [A_{0,j}^{-1}(B_{0,j} + B_{1,j}Y_{t-1} + ... + B_{p,j}Y_{t-p})]' + \epsilon_t'$, that is,

$$Y_t'\tilde{A}_{0,j} = \tilde{B}_{0,j} + Y_{t-1}'\tilde{B}_{1,j} + ... + Y_{t-p}'\tilde{B}_{p,j} + \epsilon_t' \tag{2}$$

be its associated structural form, with $\tilde{A}_{0,j} \equiv [A_{0,j}^{-1}]'$ and $\tilde{B}_{k,j} \equiv [A_{0,j}^{-1}B_{k,j}]'$, k = 0, 1, 2, ..., p. Within the present context the first shock in ϵ_t is the monetary policy shock, thus automatically implying that the first equation in (2) is the VAR's structural monetary policy rule (see Arias *et al.*, 2019). Since, according to identifying restriction I (see sub-section 2.2) the monetary policy rate is postulated to react contemporaneously only to real GDP, the core price index, and commodity prices, for the purpose of the perturbation only the coefficients on the lagged log house price/rent ratio in the structural monetary policy rule are relevant. For draw j from the posterior distribution, let these coefficients be labelled as $\gamma_{l,j}$, for $l = 1$, 2, ..., p. For each draw j, and each $l = 1, 2, ..., p$, I perturb the $\gamma_{l,j}$'s by rescaling them as

$$\gamma_{l,j}^{*} = \gamma_{l,j} + K \cdot |\gamma_{l,j}| \tag{3}$$

where $|\cdot|$ means 'absolute value of', and K is a 'small number', which in what follows I will choose so that median counterfactual real house prices at the 5-year horizon—i.e., 5 years after the beginning of the policy intervention—are 1 per cent below what they would have been without the intervention. In plain English this means that, for each draw from the posterior, and for each lag, I increase the relevant coefficient by a small percentage amount, thus slightly 'shifting upwards' the entire posterior distributions of the $\gamma_{l,j}$'s. In practice, this causes the monetary policy rate to become marginally more aggressive towards deviations of the house price/rent ratio from the benchmark value (to be discussed in the next sub-section). On the other hand, I leave all of the other coefficients in the structural monetary policy rule unchanged. Based on the resulting counterfactual (or 'perturbed') structural form, I then recover the corresponding counterfactual structural VAR for draw j, $Y_t = B_{0,j}^c + B_{1,j}^c Y_{t-1} + ... + B_{p,j}^c Y_{t-p} + A_{0,j}^c \epsilon_t$,[18] which I then use to re-run the history of the economy conditional on the previously identified structural shocks,[19] thus obtaining

[18] To be precise, since the monetary policy rate is postulated not to react contemporaneously to the house prices/rent ratio, $A_{0,j}^c = A_{0,j}$, so that $B_{0,j}^c = B_{0,j}$.

[19] So, to be absolutely clear, in *all* of these counterfactuals I *only* perturb the parameters on the (lagged) house price-rent ratio in the structural monetary policy rule, whereas the structural shocks—which, for each draw j, I had previously computed based on the original (i.e., non-perturbed) structural VAR as $\epsilon_t = A_{0,j}^{-1}[Y_t - B_{0,j} - B_{1,j}Y_{t-1} - ... - B_{p,j}Y_{t-p}]$—are left unchanged.

counterfactual paths for any of the variables.

It is worth stressing that, although in what follows I will assess whether the resulting policy counterfactuals are, or are not modest in the sense of Leeper and Zha (2003), the type of interventions I am considering here are different from theirs: whereas they worked by manipulating *monetary policy shocks*, here I am instead perturbing the parameters of the *structural monetary policy rule*. My experiments are rather conceptually the same as the 'Greenspan Hawk' policy counterfactual performed by Sims and Zha (2006, Section VI, Figure 8), in which the coefficient on inflation in the VAR's monetary policy rule had been set equal to twice its estimated value. As discussed by Sims and Zha (2006), for sufficiently large perturbations of the parameters of the policy rule it is an open question whether the Lucas critique might become empirically relevant.

4.2 The benchmark value of the log house price/rent ratio for the policy counterfactual

For all countries I set the benchmark value of the log house price/rent ratio equal to its average value over the period January 1983-December 1994. This value is therefore implicitly regarded as a 'normal', equilibrium value, so that deviations of the log ratio between house prices and rents from such benchmark are taken as an indication that real house prices are out of equilibrium. The rationale for such interpretation is that, as documented by Gallin (2008), the relationship between house prices and rents is analogous to those between GNP and consumption, and between stock prices and dividends, explored by Cochrane (1994). In particular, in the same way that a positive (negative) deviation of the consumption/GNP ratio from its unconditional mean signals GNP being below (above) potential, a deviation of the house price-rent ratio from its historical average suggests that house prices have departed from their long-run, equilibrium value.

In particular, a standard DSGE model with housing implies that, in equilibrium, the rent-price ratio ought to be equal to the real interest rate.[20] Over the post-WWII period, the *ex post* real monetary policy rate has been strongly stationary in all of the three countries I analyze.[21] This implies that the rent-price ratio ought to be mean-reverting, so that deviations from its unconditional mean point towards the relationship between house prices and rents as being out of equilibrium. In practice, standard analyses of the housing market[22] posit that, at each point in time, the rent/price ratio ought to be equal to the user cost of housing, which, beyond the the real interest rate, includes depreciation; the running costs of owning a house (including costs such as repairs and insurance); transaction costs (such as the real estate agent's commission, and stamp duty costs); and the expected change in the real price of the property. To the extent that such additional determinants of the user cost of housing are either fixed, or stationary—as it appears reasonable to assume—this still implies stationarity of the log house price/rent ratio, so that deviations from its sample average should still be taken as a broad indication that house prices have deviated from equilibrium.

The fifth column of Figure 1 shows the deviation of the log ratio between house prices and rents from its average value over the period 1983-1994. For the U.S., the years leading up to the Great Recession had been characterized by a protracted and large positive deviation of up to about 0.3 on a logarithmic scale, which naturally suggests that house prices had been quite significantly over-valued. Evidence for Canada over the last decade is qualitatively the same, with the deviation from the pre-1995 average reaching, again, about 0.3 on a log scale towards the end of the sample.[23] Finally, evidence for the U.K. is even more dramatic, with the deviation reaching, in 2006, values in excess of 0.4.

For the purposes of the counterfactual exercises in which the monetary policy rate reacts to deviations of the log house price/rent ratio from its 1983-1994 average value, the key issue

[20] I wish to thank Matteo Iacoviello for an helpful email exchange on this.

[21] E.g., bootstrapped p-values for Elliott, Rothenberg, and Stock (1996) unit root tests are uniformly below 0.1; this evidence is available upon request.

[22] See e.g., McCarthy and Peach (2004), Himmelberg, Mayer and Sinai (2005), and Fox and Tulip (2014),

[23] This provides further validation to the point I made in footnote 17 that an overvaluation by 10 per cent or more is hardly implausible.

is not whether such value is, or is not equal to the log ratio's true equilibrium value, but rather whether it represents a *reasonable benchmark* which the central bank could have used in order to implement such a policy. Another way of putting this is that the modest policy interventions I will explore boil down to addressing the following question: 'Suppose that the central bank had regarded the average value taken by the log ratio over the period 1983-1994 as the true equilibrium value, and had acted upon it: what would have happened?' Under this respect, the only relevant issue is whether this value represents a reasonable benchmark. The evidence in the fifth column of Figure 1 suggests that this is indeed the case: in line with conventional wisdom, the evolution of the deviation of the log house price/rent ratio from the benchmark value suggests that house prices in the U.S. and Canada have been significantly overvalued during the years leading up to the financial crisis and, respectively, over the most recent decade.

I now turn to the issue of how to assess whether the policy counterfactuals are, or are not modest in the sense of Leeper and Zha (2003).

4.3 Assessing the modesty of the policy counterfactuals

Leeper and Zha (2003) proposed to assess the modesty of a specific policy intervention over the horizon $\tau+h$, $h = 1, 2, 3, ..., H$, based on the (im)plausibility of the resulting counterfactual path(s) for the monetary policy rate (and possibly other series) from the perspective of a forecast based on information at time τ.[24] As extensively discussed by Adolfson *et al.* (2005), there are two issues involved in performing such an assessment:

(i) whether the forecast based on information at time τ is computed conditional on *all shocks*, or *only monetary shocks*, and

(ii) whether the assessment is performed (i.e., the modesty statistic is computed) with

[24] Focusing, for the sake of the argument, on the monetary policy rate, the intuition is that if its counterfactual path deviates to a non-negligible extent from its median forecast conditional on information at τ, this can be regarded as evidence that something in the monetary policy rule has changed.

reference to *only the monetary policy rate*, or to *all series*.

With reference to (i), Leeper and Zha (2003) originally conditioned the forecasts uniquely on monetary policy shocks. As argued by Adolfson *et al.* (2005), however, since the economy is routinely hit by a multitude of structural disturbances, and—crucially—most VAR evidence suggests that the importance of monetary shocks is comparatively modest, a more sensible choice might be to condition the forecast on all of the structural disturbances. As for (ii), on the other hand, there is no clear-cut argument in favor of assessing the modesty of the policy intervention with reference to only the monetary policy rate, or to all series. In fact, my results are qualitatively the same, and (based on median estimates) quantitatively quite close for any of the four possible ways of assessing the modesty of the policy intervention.[25] In the main body of the paper I will therefore only report and discuss a handful of such results, whereas the remaining results are reported in the Online Appendix.

Following Adolfson *et al.* (2005), in the most general case in which the modesty statistic is computed with reference to all series, the statistic for period τ and horizon h is given by

$$M_{\tau,h}(\epsilon_t^*) = [Y_{\tau+h}(\epsilon_{\tau+h}^*) - Y_{\tau+h|\tau}]'\Sigma_{\tau+h}^{-1}[Y_{\tau+h}(\epsilon_{\tau+h}^*) - Y_{\tau+h|\tau}] \tag{4}$$

for $h = 1, 2, 3, ..., H$, where ϵ_t^* is the vector of shocks conditional upon which forecasts at time τ are computed (so that it features either all shocks, or only monetary policy shocks); $Y_{\tau+h}(\epsilon_{t+h}^*)$ is a specific path for the vector of variables Y_t in (1), which has been generated by a sequence of structural shocks $\epsilon_{\tau+h}^*$ starting from initial conditions Y_τ, $Y_{\tau-1}$, ..., $Y_{\tau-p+1}$; $Y_{\tau+h|\tau}$ is the forecast of Y_{t+h} conditional on information at τ, which following Adolfson *et al.* (2005), I set equal to the median of the distribution of $Y_{\tau+h}(\epsilon_{\tau+h}^*)$ generated by simulating the VAR into the future starting from initial conditions Y_τ, ..., $Y_{\tau-p+1}$, and randomly drawing the shocks $\epsilon_{\tau+h}^*$ from a multivariate N(0, 1) distribution; and $\Sigma_{\tau+h} = \text{Cov}[Y_{\tau+h}(\epsilon_{\tau+h}^*) - Y_{\tau+h|\tau}]$.

[25] That is, any of the four possible combinations resulting from (i) computing the time-τ forecast conditional on either all shocks, or only monetary shocks, and (ii) computing the modesty statistic with reference to only the monetary policy rate, or to all series.

As pointed out by Adolfson *et al.* (2005), since $Y_{\tau+h}(\epsilon^*_{\tau+h})$ follows a multivariate normal distribution, the distribution of the modesty statistic (4) under the no policy intervention scenario is chi-squared with N degrees of freedom, where N is the number of variables used to compute the statistic. Therefore, if a specific policy intervention produces a path for $Y_{\tau+h}$, $h = 1, 2, 3, ..., H$, such that the corresponding modesty statistic $M_{\tau,h}$ lies 'too far out' in the upper tail of the chi-squared distribution, this suggests that the intervention is not modest. In what follows I will define a modest policy intervention as a perturbation of the monetary policy rule such that, for *all* $h = 1, 2, 3, ..., H$, the modesty statistic associated with the resulting counterfactual path for $Y_{\tau+h}$ lies uniformly below the 90th percentile of the distribution generated under the no policy intervention scenario. Within the present context such a definition of modesty is a pretty conservative one, since it rules out the possibility that a counterfactual path be perceived as immodest even for just a single month. A less stringent definition would label a specific policy intervention as immodest if, e.g., the resulting modesty statistic exceeded the 90th percentile of the chi-squared distribution for Z months in a row. In fact, the alternative sets of results obtained by setting Z to either 3 or 6 months are qualitatively the same, and quantitatively close to those I discuss in what follows (these results are available upon request).

I now turn to exploring the trade-off between stabilizing real house prices and stabilizing real GDP induced by policy counterfactuals (as defined in sub-section 4.1). In the next section I analyze the trade-off *in population*, whereas in Section 6 I explore it with reference to a *specific historical path* for the variables of interest, i.e. the path travelled by the U.S. economy during the years leading up to the financial crisis.

5 The Trade-Off Induced by Policy Counterfactuals In Population

For each of the three countries I perform the following experiment. I start by generating the distribution of the modesty statistic $M_{\tau,h}(\epsilon_t^*)$ under the no policy intervention scenario exactly as in the previous sub-section, based on either all series or only the monetary policy rate, and with ϵ_t^* featuring either all shocks, or only monetary policy shocks. As for τ I consider three possibilities, i.e. either the beginning (τ=p+1), middle, or end of the sample. Since the results produced by the three alternative choices for τ are near-uniformly quantitatively close, for reasons of space in what follows I will only discuss those obtained by setting τ equal to the middle of the sample. Conditional on Y_τ, $Y_{\tau\text{-}1}$, ..., $Y_{\tau\text{-}p+1}$, and for each draw j from the posterior distribution, I stochastically simulate the structural VAR into the future, drawing the structural shocks from a multivariate N(0, 1) distribution, thus generating artificial paths $Y_{j,\tau+h}^*$ for the series in the VAR *under no policy intervention*. The type of shocks I use for simulating the VAR are the same featured in ϵ_t^* for the computation of the modesty statistic, that is, if ϵ_t^* features all shocks I also simulate the VAR based on all structural disturbances, otherwise I only simulate it based on monetary policy shocks. In fact, for the reasons given by Adolfson *et al.* (2005)—see the discussion in the previous sub-section—in what follows I will mostly focus on the case in which ϵ_t^* features all structural shocks. For each draw j, and each artificial path $Y_{j,\tau+h}^*$ I then perform the policy intervention as described in sub-section 4.1, thus obtaining an artificial *counterfactual* path $Y_{j,\tau+h}^{*,C}$. Finally, for each $Y_{j,\tau+h}^{*,C}$ I compute the associated modesty statistic, $M_{\tau,h}^{*,C}$, as described in the previous sub-section. If $M_{\tau,h}^{*,C}$ is such that the policy intervention can be labelled as modest—where modesty is defined as detailed in the previous sub-section, i.e. based on the 90th percentile of the distribution of $M_{\tau,h}(\epsilon_t^*)$ generated under the no policy intervention scenario—I keep both the original artificial path, $Y_{j,\tau+h}^*$, and its counterfactual counterpart, $Y_{j,\tau+h}^{*,C}$; otherwise I

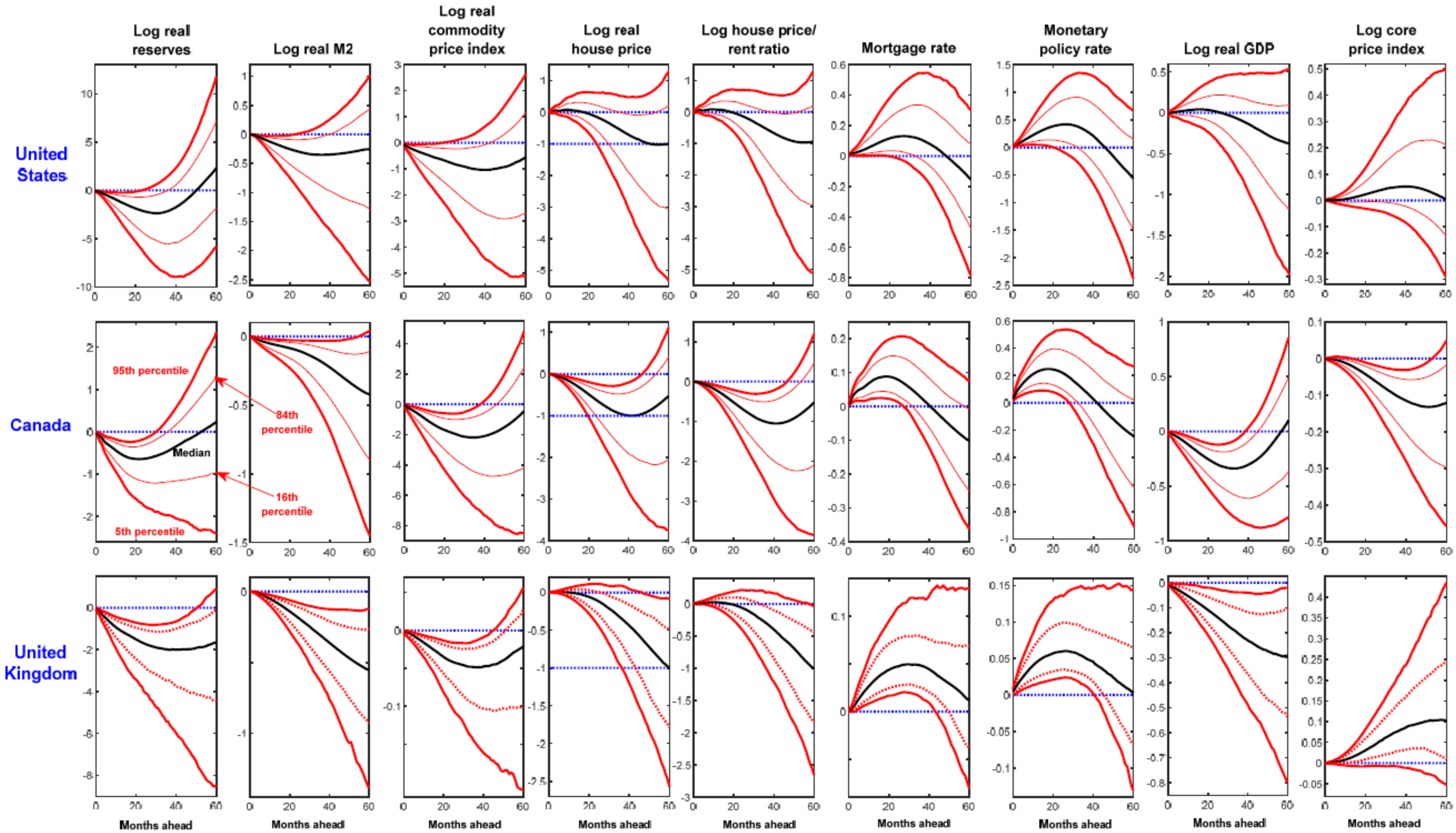

Figure 4 Evidence from modest policy interventions in population: counterfactual minus actual series in percentage points (forecasts conditional on all shocks; modesty statistic computed based on all series)

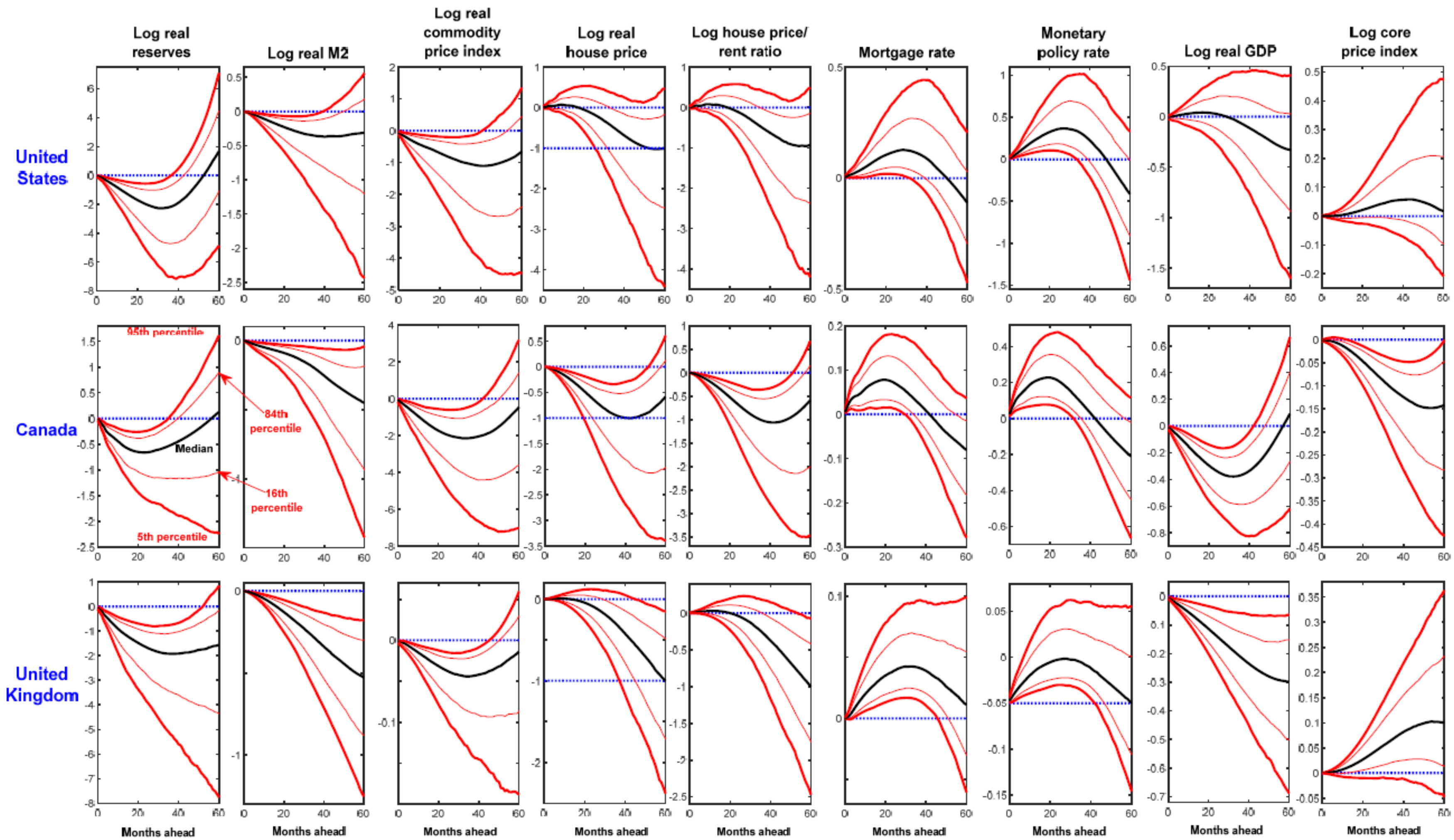

Figure 5 Evidence from modest policy interventions in population: counterfactual minus actual series in percentage points (forecasts conditional only on monetary shocks; modesty statistic computed only based on the monetary policy rate)

discard them. In this way I generate distributions of counterfactual paths produced by modest policy interventions, and of the associated original non-perturbed paths, thus allowing to fully characterize, in population, the impact of modest policy counterfactuals.

Figures 4 and 5 show some of the results. Either figure shows, for any of the three countries, and for each individual series, the median and one- and two-standard deviation percentiles of the distribution of $Y^{*,C}_{j,\tau+h}$-$Y^{*}_{j,\tau+h}$, i.e. the deviation of the series from the original, non-perturbed path caused by the policy intervention. I calibrate the magnitude of the perturbation—i.e., K in (3)—so that the minimum of the median of the distribution of the difference between counterfactual and actual real house prices over the horizon up to 5-years is equal to minus one per cent. The results in Figure 4 have been produced with ϵ^*_t featuring all shocks and the modesty statistic being computed based on all variables, whereas those in Figure 5 are based on ϵ^*_t featuring only monetary shocks and $M^{*,C}_{\tau,h}$ computed only based on the monetary policy rate.

The evidence in the two figures is qualitatively the same and quantitatively very close. A policy intervention inducing a maximum (in absolute value) median deviation of real house prices from their original path equal to one per cent over the horizon up to five years is associated with a corresponding maximum median shortfall of GDP equal to about 0.3/0.4, corresponding to a trade-off between stabilizing real house prices and stabilizing real GDP of about 2.5/3.3-to-one. Figures A.3 and A.4 in the Online Appendix report the alternative sets of results obtained either (i) based on ϵ^*_t featuring all shocks, and the modesty statistic computed only based on the monetary policy rate, or (ii) based on ϵ^*_t featuring only monetary shocks and $M^{*,C}_{\tau,h}$ computed based on all series. A comparison between the alternative sets of results shows that the trade-off between stabilizing real house prices and stabilizing economic activity induced by policy counterfactuals is robust to alternative ways of assessing the modesty of the intervention.

For the U.S. and Canada, these trade-offs are quite close to those induced by monetary policy shocks which we analyzed in sub-section 3.1: as we discussed there, the ratio between the median peak responses of real house prices and real GDP to a monetary shock is equal to 3.11 for the U.S. and to 3.25 for Canada. For the U.K., on the other hand, the ratio between the median peak responses of real house prices and real GDP to a monetary shock, equal to 5.42, pointed towards a more favorable trade-off. The implication of the set of results produced by policy counterfactuals is therefore that, in general, central banks do not face a favorable trade-off between stabilizing real house prices and stabilizing economic activity, and to the extent that they want to pursue a policy of leaning against house prices, they ought to be willing to tolerate non-negligible shortfalls in GDP.

An important point to stress is that, by the very nature of the exercise performed herein, the trade-off has been computed by disregarding the impact of the expectational channel (i.e., by assuming that economic agents do not perceive any change in the monetary policy rule). Because of this, the trade-off I have characterized should be regarded as a *lower bound* for the true trade-off central banks face. The reason is straightforward: if central banks explicitly announced a policy of gently leaning against house prices, rational economic agents would factor such a policy into their decision-making process. As a result, every increase in house prices compared to rents would generate a corresponding expectation of a marginal increase in the monetary policy rate, and therefore in mortgage rates, which would act to automatically, marginally 'cool off' house prices. The overall result would be to make the trade-off between stabilizing real house prices and stabilizing economic activity more favorable compared to the one identified herein.

The only way to gauge an idea about the magnitude of such expectational channel, however, would be to perform the exercise based on a DSGE model, with all the associated issues and uncertainties about modelling choices. Although, to the best of my knowledge, no rigorous such assessment along the lines of (e.g.) Rudebusch (2005) has been performed, Rudebusch's evidence within a different context (pertaining to changes in the coefficients of 'standard' monetary policy rules) suggests that the impact of the Lucas critique on the reduced-form structure of the economy is, in fact, quite modest. In particular, it is so modest as to not be detectable based on standard statistical tests. This suggests that the evidence in the present work might reasonably be thought to capture the *first-order impact* of a monetary policy of systematically leaning against house prices.

I now turn to a specific application of this methodology, addressing the issue of whether the large upsurge in U.S. house prices which pre-dated the Great Recession might have been at least partly prevented.

6 Could the Upsurge in U.S. House Prices That Pre-Dated the Financial Crisis Have Been Prevented?

Figure 6 reports evidence from a modest (in the sense discussed previously) policy counterfactual in which, starting from January 1995, the Federal Reserve's monetary policy rule is perturbed in such a way that the federal funds rate reacts in a marginally more aggressive way to deviations of the house price/rent ratio from its 1983-1994 average. There are two main reasons why I start the counterfactual in the mid-1990s.

First, as stressed by several commentators, the upsurge in house prices started in the second half of the 1990s. Bernanke (2010), for example, pointed out that

> 'house prices began to rise more rapidly in the late 1990s. Prices grew at a 7 to 8 percent annual rate in 1998 and 1999, and in the 9 to 11 percent range from 2000 to 2003. [...] Shiller (2007) dates the beginning of the boom in 1998.'

Second, since I am here considering very small interventions, in order to allow them to have a non-negligible cumulative impact on real house prices they must be allowed to operate for a sufficiently long period of time. This means that, for example, starting the

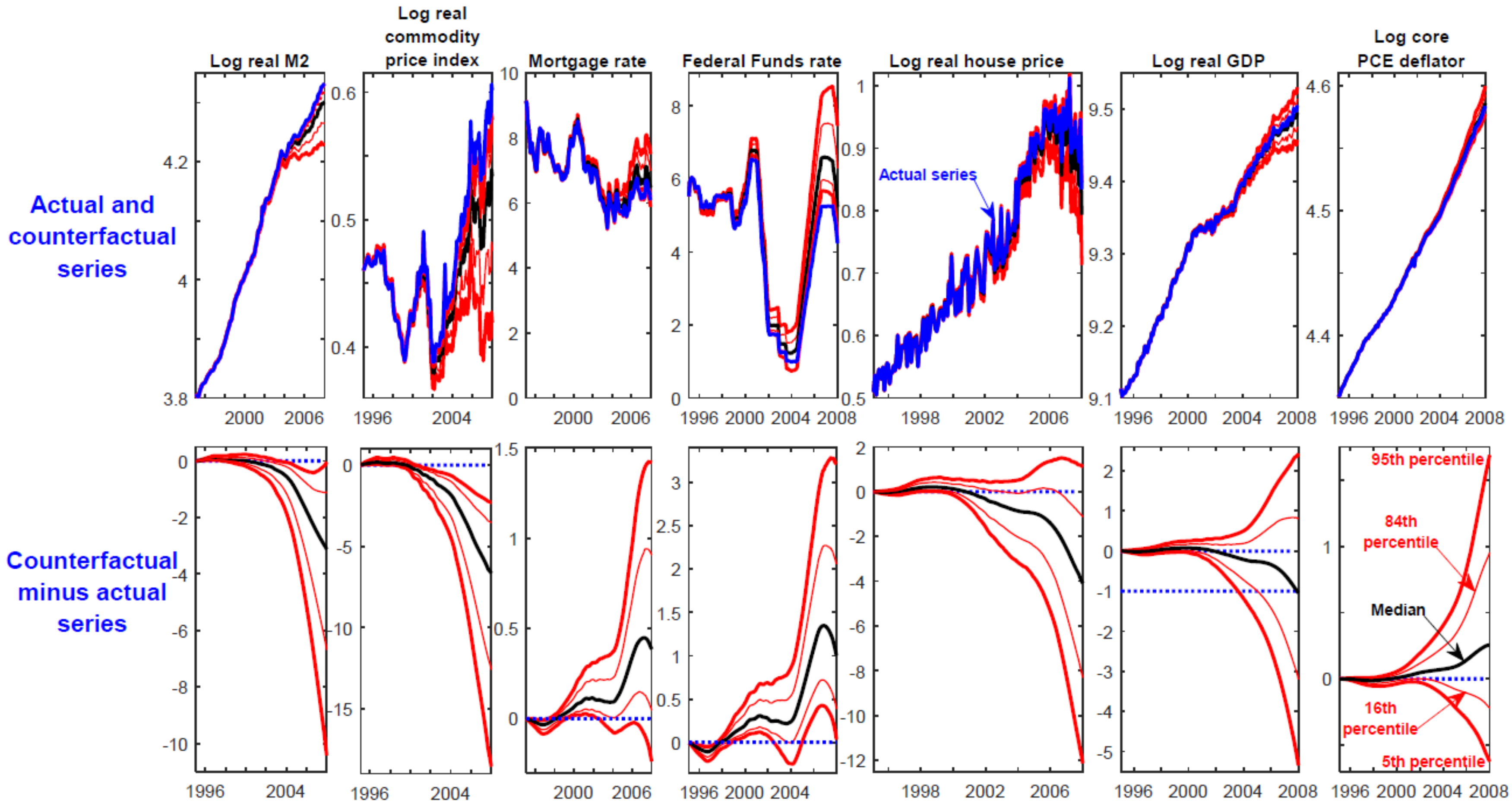

Figure 6 United States: evidence from a modest policy intervention based on actual data (forecasts conditional on all shocks; modesty statistic computed based on all series)

counterfactuals in (say) 2002 or 2003—when the upsurge was already in full swing—simply does not work,[26] because by then the winds blowing house prices up were already so strong that only a comparatively large, and therefore *immodest* policy intervention might have reigned the upsurge in (and so, since it is immodest, it is not clear what to make of the entire experiment).

Within the class of modest policy interventions—the modesty statistic is here computed based on all series, and the forecasts are computed conditional on all shocks[27]—I calibrate the size of the perturbation in such a way that the median cumulative impact on real GDP in the last month of the sample (December 2007) is equal to minus one per cent. Figure 6 shows the results for selected series. A one per cent median shortfall of GDP in December 2007 would have been associated with a corresponding median decrease of real house prices, compared to their actual historical path, of minus four per cent. This points towards a four-to-one trade-off between stabilizing real house prices and stabilizing real GDP within the set of modest policy interventions, and—crucially—conditional on the *actual historical path* travelled by the U.S. economy during the period 1995-2007. To put it differently, the fact that this trade-off is more favorable than the roughly 2.5/3.3-to-one identified in Section 5 originates from the fact that whereas there I had characterized the trade-off in population, here I am exploring it conditional on the specific historical path travelled by the U.S. economy.

Assuming that these results correctly characterize the trade-off faced by the Federal Reserve between stabilizing house prices and stabilizing economic activity during the years leading up to the financial crisis, the obvious question is what the costs of a plausible policy of leaning against house prices would have been. Simple back-of-the-envelope calculations

[26] This evidence is available upon request.

[27] As a check on the robustness of the results shown in Figure 6, Figure A.4 in the Online Appendix reports the corresponding set of results obtained by computing the modesty statistic only based on the federal funds rate, and with the forecasts conditional on all shocks. The evidence there is quantitatively close to that in Figure 6.

suggest that the costs in terms of lost output would have been quite substantial. The 12-month moving average of the deviation of the log house price/rent ratio from its pre-1995 average had reached a peak of 0.258 in July 2006. Assuming, just for the sake of the argument, that 25.8 per cent had been the true extent of over-valuation of real house prices, if the Fed had wanted to eliminate 10 per cent of it, it should have engineered a monetary policy leading to a cumulative output loss of about 2 per cent of GDP.

7 Conclusions, and Directions for Future Research

For several years, an intense debate has centered on the meaningfulness of pursuing monetary policies aimed at leaning against credit growth and asset prices' fluctuations. The debate has gained new urgency following the outbreak of the Great Recession, which was triggered by the collapse of what many perceived had been a large overvaluation of U.S. house prices. Whereas the vast majority of academics and policymakers subscribe to the skepticism expressed e.g. by Svensson (2017), a minority position associated mainly with the Bank for International Settlements advocates instead a policy of systematically reacting to measures of 'excessive' credit growth or 'disequilibria' in asset prices, by marginally increasing monetary policy rates over and above the values dictated by an exclusive focus on inflation and real activity. In this paper I have proposed a simple methodology for exploring the trade-off between stabilizing real house prices and stabilizing economic activity associated with a policy of systematically leaning against house prices, combining elements from Sims and Zha (2006) and Leeper and Zha (2003). Working within the class of 'modest' policy interventions, I have shown that such trade-off is likely not a favorable one, as one percentage point decline in GDP is associated with a decrease in real house prices equal to about three per cent.

The methodology proposed herein is of more general applicability, and lends itself to the analysis of conceptually related policies. In the light of the long-standing debate about the meaningfulness of pursuing a policy of leaning against credit fluctuations, one natural application is the study of the impact of a policy of leaning against the credit cycle.

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