How to Solve the Price Puzzle? A Meta-Analysis*

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Preliminary, comments welcome

Abstract

The short-run increase in prices following an unexpected tightening of monetary policy constitutes a frequently reported puzzle. Yet the puzzle is surprisingly easy to explain away when all published models are quantitatively reviewed. We collect about 1000 point estimates of impulse responses from 65 articles using vector autoregressions (VARs) and present a simple method of research synthesis for graphical results. Our findings indicate publication selection in favor of the intuitive response of prices to a tightening. Less theory competition for the long-run response is associated with more publication selection. The estimates depend systematically on study design: when misspecifications are filtered out, the price puzzle disappears. The long-run response is driven by the structural characteristics of the economy.

Keywords: Monetary policy transmission; Price puzzle; Meta-analysis; Publication selection bias

JEL Codes: C83; E52

1 Introduction

How does monetary policy affect the price level? The fundamental question of monetary economics still belongs among the most controversial when it comes to empirical evidence. Although both intuition and stylized macro models suggest that prices should decrease following a surprise increase in interest rates, empirical findings challenge the theory. Already Hume (1752) noticed that changes in money supply are followed by an immediate reaction of output

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while it takes longer for prices to adjust. But not only is the reaction of prices slower: almost half of modern studies using VARs to investigate the effects of monetary policy have found that after a tightening prices do actually increase, at least in the short run. The price puzzle was first encountered by Sims (1992). Since then, many different solutions have been proposed, ranging from the alleged misspecifications of VARs to the theoretical models that try to justify the observed rise in prices.

Depending on the point of view, the price puzzle casts serious doubts on the ability of VAR models to correctly identify monetary policy shocks or on the ability of central banks to control inflation in the short run. Since economists have produced a plethora of empirical research on the topic, it seems natural to ask what general effect the literature implies. The method designed to answer such questions is meta-analysis, the quantitative method of research synthesis commonly used in economics (Smith & Huang, 1995; Stanley, 2001; Disdier & Head, 2008). In contrast with narrative literature surveys, meta-analysis takes into account publication selection, the preference of authors, editors, or referees for results statistically significant or consistent with the theory, the bias that has become a great concern in empirical economics research (Card & Krueger, 1995; Ashenfelter & Greenstone, 2004; Stanley, 2008). The method enables researchers to examine the systematic dependencies of reported results on study design and separate wheat from chaff by filtering out the effects of misspecifications. Meta-analysts can create a synthetic study with ideal parameters, such as the maximum number of observations or consensus best-practice methodology, and estimate the underlying effect of monetary policy net of potential misspecification and publication biases. Furthermore, meta-analysis makes it possible to investigate how the strength of monetary transmission depends on the structural characteristics of examined countries, such as openness, average inflation, or central bank independence.

To our knowledge, there has been one meta-analysis on the impact of monetary policy on prices (de Grauwe & Storti, 2004) and only focused on heterogeneity in the reported estimates. Our paper differs from de Grauwe & Storti (2004) in three main aspects. First, additionally to the point estimates of impulse responses we extract their precision, which allows us to test for publication bias and estimate the underlying effect beyond. Second, we restrict the sample to VAR studies, gather more of them (65 compared with 43), examine more time horizons after a monetary policy shock (5 compared with 2), use four times more point estimates, and codify three times more variables that explain heterogeneity. Third, employing multilevel meta-analysis methods we account for within-study dependence and construct a synthetic ideal study to filter out misspecification bias.

The remainder of the paper is structured as follows. Section 2 describes how VAR models are estimated and how we collected the estimates. Section 3 reviews the suggested solutions to the price puzzle. Section 4 tests for publication selection bias and the underlying effect of monetary tightening on prices. Section 5 models method and structural heterogeneity among estimates. Section 6 concludes. Appendix A provides sensitivity checks, and Appendix B lists all studies used in the meta-analysis.

2 The Impulse Responses Data Set

Researchers using VARs to examine the impact of monetary policy usually assume that the economy can be described by the following dynamic model:

$$AY_t = B(L)Y_{t-1} + \varepsilon_t, \tag{1}$$

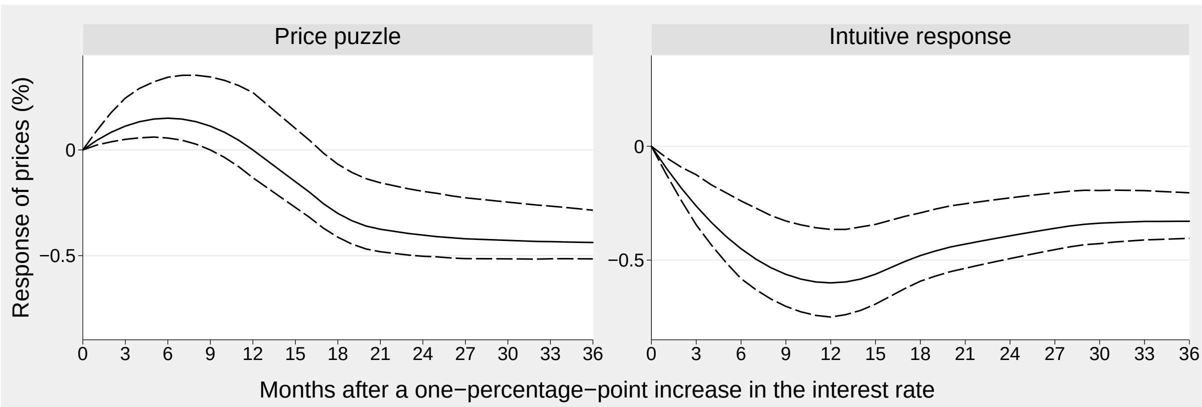
where Y_t is a vector of endogenous variables containing typically a measure of output, prices, interest rates, and in the case of an open economy also the exchange rate. Matrix A describes contemporaneous relationships between endogenous variables, $B(L)$ is a matrix lag polynomial, and ε_t is a vector of structural shocks with the variance-covariance matrix $E(\varepsilon_t \varepsilon_t') = I$. The system is called the structural-form VAR. In order to estimate it, researchers rewrite the system to its reduced form:

$$Y_t = C(L)Y_{t-1} + u_t, \tag{2}$$

where the elements of matrix $C(L)$ are the convolutions of the elements of matrices A and B, and u_t is a vector of reduced-form shocks with the variance-covariance matrix $E(u_t u_t') = \Sigma$; the relationship between structural shocks and reduced-form residuals is $\varepsilon_t = Au_t$. The dynamic responses of endogenous variables to structural shocks can be studied by impulse-response functions.

Figure 1 presents two stylized impulse responses of the price level to monetary tightening that can be found in the literature. The left panel shows the price puzzle: prices increase significantly in the short run and decline in the medium and long run. In contrast, the right panel shows a response which corresponds with theoretical predictions: soon after a tightening the price level declines.

Figure 1: Stylized impulse responses



The first step of meta-analysis represents the selection of studies. While some meta-analysts use both published and unpublished studies, others confine their sample to journal articles (for instance, Abreu *et al.*, 2005). Including working papers and mimeographs does not help alleviate publication bias: if journals systematically prefer some results, rational authors will likely adopt the same preference already in the earlier stages of research as they prepare for journal submission. Indeed, empirical evidence suggests no difference in the magnitude of publication bias between published and unpublished studies (see the meta-analysis of 65 meta-analyses by Doucouliagos & Stanley, 2008). And even if there was a difference, modern meta-regression methods can not only identify but also filter out the bias. Therefore, as a preliminary and simple criterion of quality, we consider only articles published in peer-reviewed journals or in handbooks (such as the Handbook of Macroeconomics).

The following strategy of literature search was employed. First, we examined two literature surveys (Stock & Watson, 2001; Egert & MacDonald, 2009) and set up a search query able to capture most of the relevant studies; we searched both the EconLit and RePEc databases. Next, we checked the references of studies published in 2010 and the citations of the most widely cited study in the VAR literature, Christiano *et al.* (1999). After going through the abstracts of all identified studies, we selected 208 that showed any promise of containing empirical estimates of the response, and examined them in detail. The last study was added on 30 April 2010.

To be able to use meta-analysis methods fully, we exclude studies omitting to report confidence intervals around impulse responses. Unfortunately we thus have to exclude some seminal contributions such as Sims (1992) or a few recent studies that estimate time-varying parameter VARs. To obtain a more homogeneous sample we focus only on studies that define the monetary policy shock as a change in the interest rate. A number of studies investigate a change in the monetary base; since Sims (1992), however, the majority of literature investigate interest rate shocks because most central banks now use the interest rate as their main policy instrument. We include only studies examining the response of the price level; a minority of studies examine the responses of inflation. These homogeneity criteria leave 65 studies in our database. The full list of studies used in the meta-analysis can be found in Appendix B, and the list of excluded studies is available in the online appendix at `meta-analysis.cz/price_puzzle`.

Considering the richness and heterogeneity of the empirical evidence on the effects of monetary policy, it is surprising there has been no quantitative synthesis using modern meta-regression methods. One reason is that the results are typically presented in the form of graphs instead of numerical values. In addition the graphs contain estimates for many time horizons following the monetary policy shock. Researchers usually investigate up to 36- or 48-month horizons when using monthly data and up to 20 quarters when using quarterly data; it is unclear which horizon should be chosen to summarize the effect. All these features of the VAR literature have discouraged researchers from conducting meta-analyses.

Our meta-analysis is designed in the following way. We extract responses at 3- and 6-month horizons to capture the short-run effect, 12- and 18-month horizons to capture the medium-run effect, and the 36-month horizon to capture the long-run effect. We enlarge the graphs of impulse responses, import them into graphical software, and using pixel coordinates we measure the value of the response and the corresponding confidence bounds. The graphs of all impulse responses as well as the extracted values are available in the online appendix. The resulting measurement error is random, similar to the error arising from rounding decimals in reporting numerical results, and thus inevitable in meta-analysis.

The extracted values must be transformed into a common metric to ensure that estimates be comparable. To standardize the estimates to represent the effect of a one-percentage-point increase in the interest rate, we divide the responses by the magnitude of the monetary policy shock used in the study. In the case of factor-augmented VAR (FAVAR) studies, where the responses are usually given in standard deviation units, we normalize the responses by the standard deviation of the particular time series.

Since the confidence intervals around the estimates of impulse response are usually asymmetrical (the estimates are not assumed to be drawn from a normal distribution), the standard errors of the estimates cannot be directly obtained. We approximate the standard error by the distance from the point estimate to the confidence bound closer to zero; that is, we take the lower confidence bound for positive responses and the upper bound for negative responses. This bound determines the significance and would be associated with potential publication selection. Should we use the average of the distance to both confidence bounds, the inference would remain similar; these results are available on request. When the reported confidence interval is presented in standard deviation units (for example, $+/-$ two standard deviations), we can immediately approximate the standard error. Otherwise, we proceed as if the estimates were normally distributed and assume that, for example, the 68% confidence interval represents an interval of one standard error.

Following the recent trend in meta-analysis (Disdier & Head, 2008; Doucouliagos & Stanley, 2009), we use all reported estimates from the studies. Arbitrarily selecting the "best" estimate or using the average reported estimate would discard plenty of useful information about the differences in methods within one study. The number of impulse responses collected for each of the horizons is approximately 200, which in total amounts to more than 1000 point estimates. To be specific, we collect 202 estimates for the 3-month horizon, 206 for 6- and 12-month horizons, 205 for the 18-month horizon, and 194 for the 36-month horizon. For comparison, consider Nelson & Kennedy (2009), who review 140 economic meta-analyses and report that the median analysis uses 92 point estimates from 33 primary studies. The median study in our sample was published in 2005, the data set covers evidence from 36 countries, and we build upon the work of 94 researchers.

3 Collecting the Pieces of the Puzzle

To motivate the selection of explanatory variables in the multivariate meta-regression analysis (Section 5), now we briefly review the solutions to the price puzzle that have been proposed in the literature. Most of these remedies have proven to alleviate the puzzle in some cases; none of them, though, has been fully successful in solving it. Table 1 demonstrates that from the 202 estimates collected for the 3-month horizon almost a half exhibits the price puzzle, and in 21% of them the puzzle is even significant at the 5% level. The table summarizes the effectiveness of the differrent solutions: even in the case of the most effective one, 24% of specificiations still exhibit the puzzle (except for sign restrictions, which is, however, a partly tautological solution).

Table 1: Effectiveness of the suggested solutions to the price puzzle

	Methodology used in the estimation						
	All	Commodity	Trend/Gap	Single	FAVAR	SVAR	Sign
Responses estimated	202	127	33	62	11	57	31
Price puzzle present	94	55	8	22	8	15	3
Price puzzle significant	43	22	3	12	5	4	0

Note: Commodity = Commodity prices are included in the VAR, Trend/Gap = time trend or output gap is included, Single = the VAR is estimated on the sample containing a single monetary policy regime, FAVAR = a factor-augmented VAR is estimated, SVAR = non-recursive identification is used, Sign = shocks are identified by imposing sign restrictions, not necessarily on prices.

3.1 Omitted Variables

Commodity Prices According to Sims (1992) the price puzzle occurs because central banks are forward-looking and react to the anticipated future movements of inflation by rising the interest rate. When a VAR system omits information about future inflation the examined shocks become the combinations of true monetary policy shocks and endogenous reactions to expected inflation. If the central bank does not fully accommodate expected inflation the data might show that an increase in the interest rate, mistakenly recognized as a monetary policy shock, is followed by an increase in the price level. Sims (1992) finds that including commodity prices into the VAR mitigates the price puzzle. Nevertheless, as follows from Table 1, the inclusion of commodity prices does not solve the puzzle automatically.

Output gap Giordani (2004) argues that the use of GDP in the VAR system without controlling for the potential of the economy can bias the estimates and cause the price puzzle. He claims that commodity prices alleviate the puzzle mostly because they contain useful information about output gap, not just because they are a good predictor of future inflation. In a similar spirit Hanson (2004) examines a battery of other indicators and finds little correlation between the ability to solve the price puzzle and the ability to forecast inflation. In our sample approximately 16% of studies use output gap, but some of them still encounter the puzzle.

Factor-augmented VAR To address the major shortcomings of standard small-scale VARs, Bernanke *et al.* (2005) introduce the factor-augmented approach. They argue that, in practice, policy makers take into account hundreds of variables when deciding about monetary policy. Standard VAR models with only three to six variables may therefore suffer from omitted-variable bias; the FAVAR approach on the other hand exploits additional information by extracting principal components from many time series. Nonetheless the results of Bernanke *et al.* (2005) indicate that this approach only mitigates the puzzle and cannot explain it fully.

3.2 Identification

While some researchers stress the role of omitted variables, others argue that the puzzle arises from implausible identification. The usual recursive identification (Cholesky decomposition), which assumes that monetary policy affects output and prices only with a lag, is, for example, not consistent with the New-Keynesian class of theoretical models.

Non-recursive identification Kim (1999) and Kim & Roubini (2000) introduced and applied a non-recursive method of the identification of shocks. The main idea, going back to Bernanke (1986) and Blanchard & Watson (1986), says that the matrix contemporaneously linking structural shocks and reduced-form residuals is no more lower triangular, but that it assumes a general form indicated by theory: the rows of the matrix have a structural interpretation. The restrictions presented by Kim & Roubini (2000) are elicited from the structural stochastic equilibrium model developed by Sims & Zha (1998). Alternatively, researchers may impose a long-run restriction to identify the shocks; this approach is pursued by Blanchard & Quah (1989) and Clarida & Gali (1994), who only allow technological shocks to have a permanent effect on economic activity. Recently Bjornland & Leitemo (2009) combine short-run and long-run restrictions. Although non-recursive identification is appealing, in almost 25% of the responses computed using this strategy the price puzzle still occurs.

Sign Restrictions Canova & Nicolo (2002) present a novel identification approach that assigns structural interpretation to orthogonal innovations by using the signs of the cross-correlation function of responses to shocks; Uhlig (2005) applies a similar method. The method is attractive since sign restrictions can be derived from the canonical dynamic stochastic general equilibrium (DSGE) model. The use of sign restrictions in VARs, however, has been criticized by Fry & Pagan (2007): because impulse responses do not come from one model, the shocks are not orthogonal. Fry & Pagan (2007) argue there is no reason to suppose that sign restrictions generate better quantitative estimates compared with recursive methods.

3.3 Monetary Policy Regime

Another stream of literature suggests that the puzzle is historically limited to the periods of passive monetary policy[1] (Benati, 2008) or that it emerges when the data mix different monetary regimes (Elbourne & de Haan, 2006). For example, if a researcher assumes that the central bank uses the interest rate as its main instrument, although for some part of the sample the monetary or exchange rate targeting was in place, monetary policy shocks in the VAR system become incorrectly identified.

For instance, Hanson (2004) shows that neither commodity prices nor other indicators are able to solve the price puzzle in the 1959-1979 period, suggesting that the puzzle is associated with the period. Similar results are reported by Castelnuovo & Surico (2010), who find the price puzzle in the pre-1979 sample, and the result is independent of controlling for output gap. This finding is reported mainly for the United States, but Benati (2008) presents similar evidence for the United Kingdom.

[1]The monetary policy is considered passive when it violates the so-called Taylor principle. The Taylor principle requires the central bank to increase the interest rate sufficiently after a positive shock to inflation expectations, so that the real interest rate also increases (Clarida *et al.*, 2000).

3.4 Cost Channel

A decrease in the price level following a tightening of monetary policy is predicted by stylized theoretical models stressing the importance of the demand channel of transmission. On the other hand, Barth & Ramey (2002) present evidence for the so-called cost channel. Since firms depend on credits to finance production, their costs rise when the central bank increases the interest rate, and they may increase prices. In this view the price puzzle does not stem from methodological problems, but represents a genuine response to monetary tightening when the cost channel dominates the demand channel.

For the United States, Christiano *et al.* (2005) build a DSGE model incorporating the cost channel, but only find a minor role for it. In a similar spirit the results of Rabanal (2007) suggest that the demand-side effects of monetary policy dominate the supply-side effects, thus leaving the credit channel relatively unimportant. For the Eurozone, Henzel *et al.* (2009) come to similar conclusions in their baseline model, but show that small deviations from the estimated parameters (for instance, a higher degree of nominal wage rigidity or a lower degree of price stickiness) can create the price puzzle. Since the financial system in Europe is more bank-based relative to the United States, the credit channel might be more powerful in Europe.

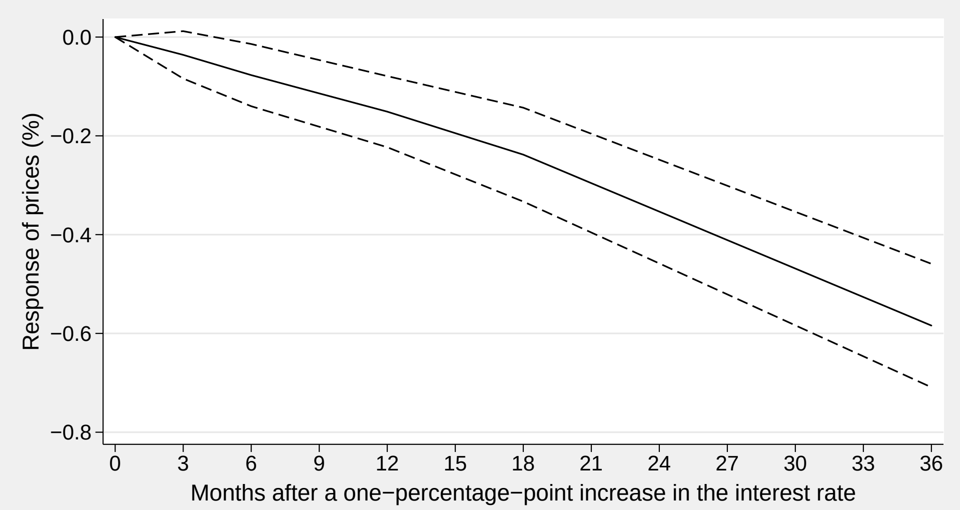
4 Consequences of Publication Bias

After collecting about 1000 estimates from the literature, a natural question arises: what general impulse response does the literature suggest? Meta-analysis was originally developed in medicine to combine many small studies into a large one, and therefore to boost the degrees of freedom. Clinical trials are expensive, and meta-analysis thus became the dominant method of taking stock in medical research. Estimating a VAR model is cheap, but the degrees of freedom are limited in macroeconomics. Hence meta-analysis is useful even here since it combines information from many countries and time periods. Taking a simple mean of all point estimates for each of the five horizons implies the impulse-response function depicted in (Figure 2). This average impulse response shows a relatively intuitive reaction of prices to a one-percentage-point increase in the interest rate: prices decline already in the short run, the decrease becomes significant in the medium run and reaches 0.6% after 36 months.

This approach, however, has two major shortcomings. First, it ignores possible publication selection bias. If some results are more likely to get published than others, the simple average becomes a biased estimator of the true underlying effect. Second, it ignores heterogeneity in the results. Since different researchers use different data and methods, it is unrealistic to assume that all estimates come from the same population. In addition, the discussion in Section 3 indicates that some VAR models are misspecified, and if misspecifications have a systematic influence on results, it may be possible to improve upon the average response by filtering out the bias. We address the first issue in this section and the second one in Section 5.

Stanley (2008), among others, points out that publication selection is of major concern for empirical research in economics. When there is little theory competition for what sign the

Figure 2: Average impulse response implied by the literature



Note: Confidence bands are constructed as +/− one standard error.

underlying effect should have, the estimates inconsistent with the predominant theory will be treated with suspicion or even be discarded. The effect of common currency on trade provides an illustrative example (Rose & Stanley, 2005): it is hard to defend negative estimates of the trade effect of currency unions. The negative estimates likely result from misspecification, and researchers are probably right not to interpret them. On the other hand, it is far more difficult to identify the excessively large estimates of the same effect that also arise from misspecifications. No specific threshold exists above which the estimate would become suspicious. If researchers include the large positive estimates but omit the negative ones, their inference may be biased toward a stronger effect.

A similar selection, perhaps of lower intensity, may take place in the VAR literature as well. Some researchers treat the price puzzle as a clear indication of misspecification error and try to find a "sensible" (that is, intuitive) impulse response for interpretation. Statistical significance is important, too. Significant impulse responses are more convenient for interpretation, and especially in central banks, researchers may be interested in reporting well-functioning monetary transmission with a significant reaction of prices to a change in monetary policy. Selection for significance does not distort the average estimate from the literature if the true underlying effect equals zero, but otherwise it creates a bias; again in favor of the stronger effect since the estimates with the wrong sign are less likely to be significant.

A common way of detecting publication selection bias is an informal examination of a so-called funnel plot. The funnel plot depicts the estimates on the horizontal axis against their precision (the inverse of standard error) on the vertical axis. If there is no heterogeneity and misspecification, the estimates with the highest precision should be close to the true underlying effect. In the absence of publication selection the funnel is symmetric: the reported estimates are dispersed randomly around the true effect. Asymmetry of the funnel plot suggests the presence of publication bias in the literature. For example, if estimates with the positive sign are less likely to be selected for publication, there will be more negative than positive estimates at a similar level of precision. The funnel plots for all five horizons are depicted in Figure 3. The plots resemble funnels commonly reported in economic meta-analyses, which indicates that the employed approximation of standard errors is plausible.[2] As expected, the left part of all funnels is clearly heavier suggesting publication selection against the price puzzle and in favor of the more negative (that is, stronger) effects of monetary tightening on prices. The interpretation of funnel plots, however, is inevitably subjective, and a more formal test of publication bias is required.

Given small samples authors wishing to obtain significant results may be tempted to try different specifications until they find estimates large enough to offset standard errors. In contrast, with large samples even tiny estimates might be significant because of small standard errors, and authors have therefore less incentives to conduct a specification search. If publication selection is present, we should observe a relationship between an estimate and the estimate's standard error (or the square root of the number of observations). The following regression formalizes the idea (Card & Krueger, 1995):

$$\hat{\beta}_j = \beta + \beta_0 SE_j + e_j, \tag{3}$$

where β denotes true underlying effect, $\hat{\beta}_j$ denotes its j-th estimate, β_0 denotes the magnitude of publication bias, SE_j denotes the standard error of $\hat{\beta}_j$, and e_j denotes a disturbance term.

In practice, meta-analysts do not estimate specification (3) since it suffers from heteroscedasticity by definition. Instead, the weighted least squares are used to ensure efficiency, and they require that specification (3) be divided by SE_j, the measure of heteroscedasticity (Stanley, 2008):

$$\frac{\hat{\beta}_j}{SE_j} \equiv t_j = \beta_0 + \beta \left(\frac{1}{SE_j}\right) + \xi_j, \quad \xi_j | SE_j \sim N(0, \sigma^2), \tag{4}$$

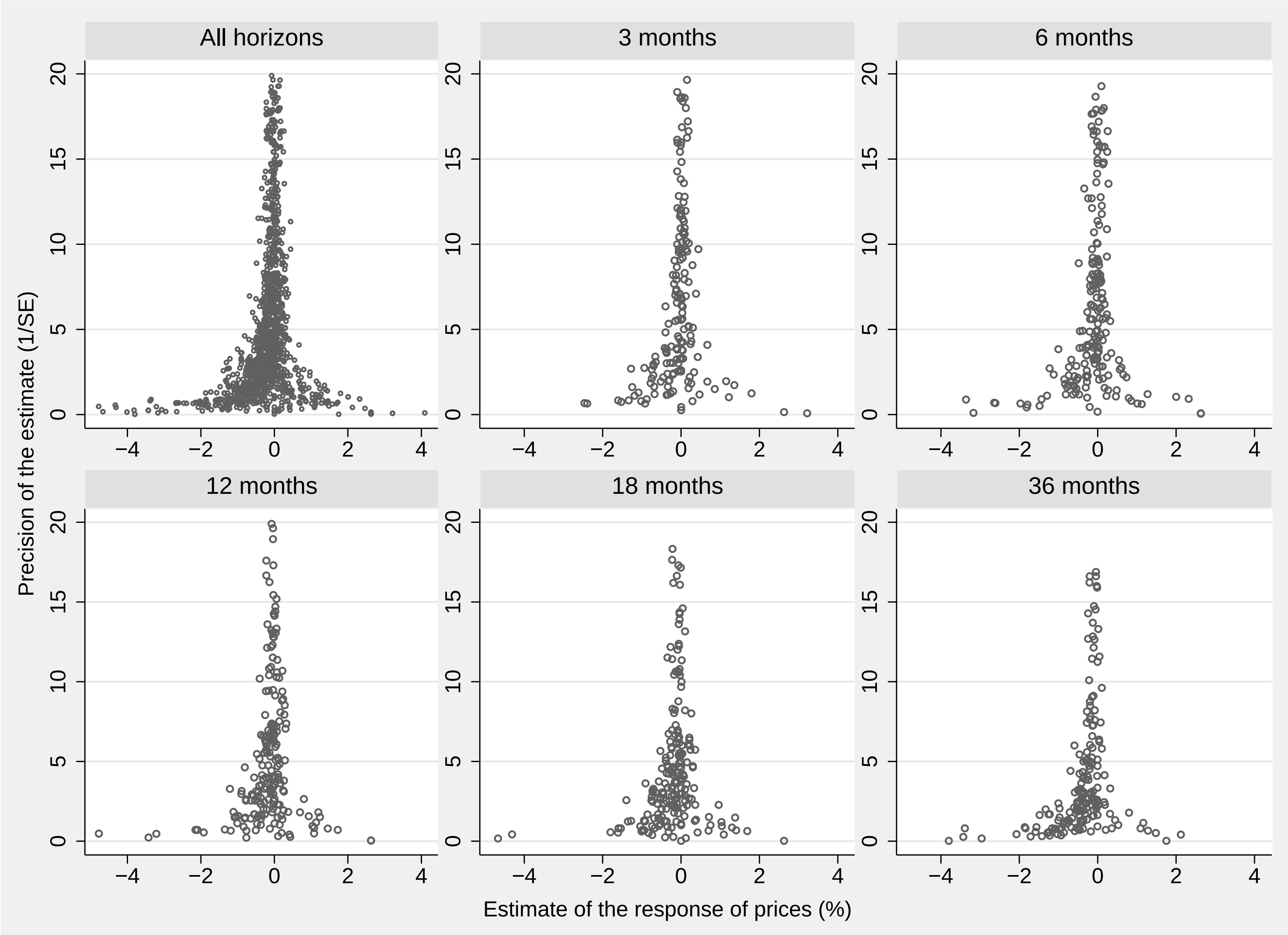
where t_j denotes the approximated t-statistic of the estimate, and the new disturbance term ξ_j has constant variance. Note that the intercept and the slope are now reversed: the slope measures true effect, and the intercept measures publication bias. Testing the significance of β_0 is analogical to testing the asymmetry of the funnel plot; it follows from rotating the funnel plot and dividing the values on the new vertical axis by SE_j. Testing the significance of β constitutes the test for true underlying effect of monetary tightening on prices.

Since we use all reported impulse responses we need to take into account the potential dependence of estimates within one study (Disdier & Head, 2008). In such a case, (4) would be misspecified. As a remedy, the mixed-effects multilevel model is usually employed (Doucouliagos & Stanley, 2009):

$$t_{ij} = \beta_0 + \beta \left(\frac{1}{SE_{ij}}\right) + \alpha_j + \epsilon_{ij}, \quad \alpha_j | SE_{ij} \sim N(0, \psi), \quad \epsilon_{ij} | SE_{ij}, \alpha_j \sim N(0, \theta), \tag{5}$$

[2] A few outlying estimates are trimmed from the funnels to ensure that the main pattern is clearly observable. Nevertheless, all estimates are included in the meta-regressions analysis.

Figure 3: Funnel plots show publication bias



where i and j denote estimate and study subscripts. The overall error term now consists of study-level random effects and estimate-level disturbances ($\xi_{ij} = \alpha_j + \epsilon_{ij}$), and its variance is additive since both components are assumed to be independent: $\text{Var}(\xi_{ij}) = \psi + \theta$, where ψ denotes within-study variance and θ between-study variance. If ψ approaches zero the benefit of using the mixed-effect estimator instead of ordinary least squares (OLS) dwindles. To put the magnitude of these variance terms into perspective the within-study correlation is useful: $\rho \equiv \text{Cor}(\xi_{ij}, \xi_{i'j}) = \psi/(\psi + \theta)$, which expresses the degree of the dependence of estimates reported in the same study, or equivalently, the degree of between-study heterogeneity.

The mixed-effects multilevel model is analogical to the random-effects model commonly used in panel-data econometrics. We follow the terminology from multilevel data modeling, which calls the model "mixed effects" since it contains a fixed (β) as well as a random part (α_j). For the purposes of meta-analysis the multilevel framework is more suitable because it takes into account the unbalancedness of the data (the restricted maximum likelihood estimator is used instead of generalized least squares) and allows for nesting multiple random effects (study-, author-, or country-level), and is thus more flexible (Nelson & Kennedy, 2009).

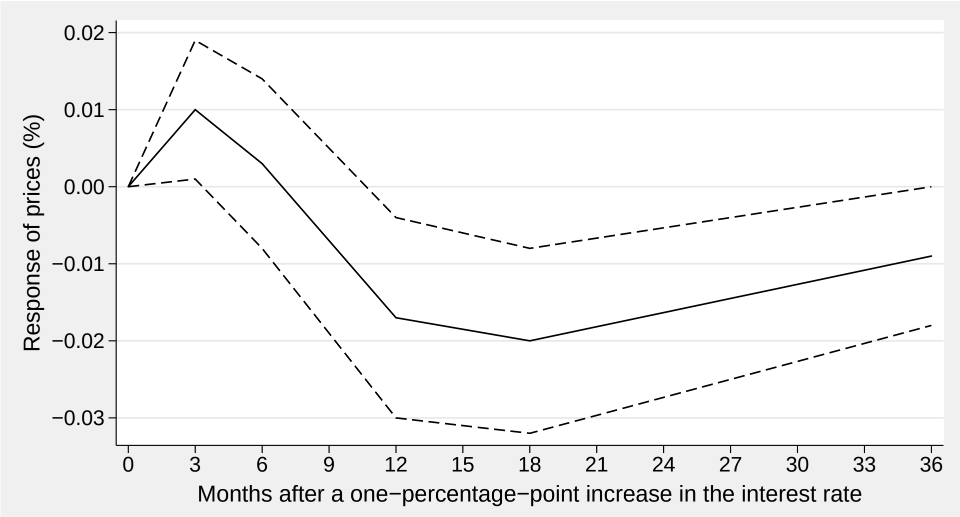
The outcomes of the mixed-effects estimator are presented in Table 2. They are akin to those of OLS with standard errors clustered at the study level (Table A1). The within-study correlation is large, indicating that the mixed-effects estimator is more appropriate, which is moreover confirmed by likelihood-ratio tests. We also experimented with several nested mixed-effects models, but they yield qualitatively similar outcomes. For all horizons the response of prices corrected for publication bias is more positive (that is, weaker) compared with the simple average, which corroborates evidence for publication selection in favor of the more negative (stronger) responses of prices to monetary policy contraction. What is more, the magnitude of publication bias increases with the time horizon after the shock. This result is in line with Doucouliagos & Stanley (2008), who find stronger publication selection for research questions with weaker theory competition. Because of the cost channel, some disagreement occurs about the effects of monetary policy on prices in the short run. On the other hand, a consensus emerges about the long-run effect: most economists accept that prices eventually decrease after monetary policy tightening; estimates showing the opposite would be relatively difficult to publish.

Table 2: Test of true effect and publication bias

	Mixed-effects multilevel				
Horizon	3 months	6 months	12 months	18 months	36 months
Intercept (bias)	0.060	-0.108	-0.211	-0.360***	-0.825***
	(0.173)	(0.168)	(0.144)	(0.127)	(0.122)
$1/SE$ (effect)	0.010	0.003	-0.017	-0.020*	-0.009
	(0.009)	(0.011)	(0.013)	(0.012)	(0.009)
Within-study correlation	0.43	0.58	0.47	0.42	0.12
Observations	202	206	206	205	194
Studies	65	65	65	65	58

Note: Standard errors in parentheses. The effect is in %. Response variable: the approximated t-statistic of the estimate. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Figure 4: Unconditional impulse response corrected for publication bias exhibits the price puzzle



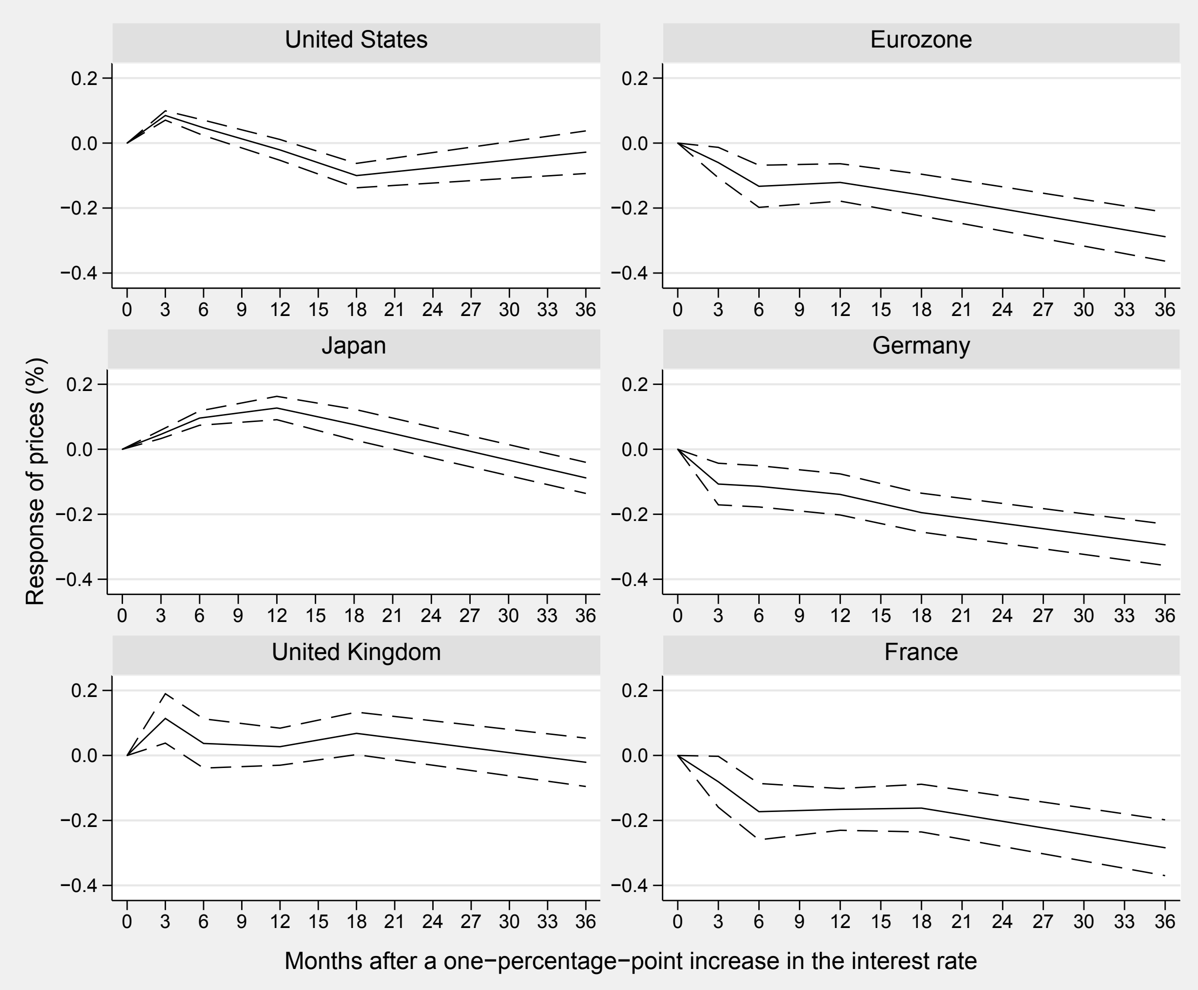
The impulse-response function corrected for publication bias is depicted in Figure 4: it exhibits the price puzzle. In the medium run, though, prices decrease and bottom out 18 months after the tightening. Because publication bias is filtered out, the function shifts upwards from the average response reported in Figure 2, especially in the long run. Figure 4 would be our best estimate of the underlying impulse response if all heterogeneity between studies was random; the estimate is unconditional on the characteristics of the examined countries and on the used methodology. In the next section we relax the assumption of random heterogeneity and explain the differences in the reported estimates.

5 What Explains Heterogeneity

Two sources of heterogeneity can drive the differences in results. On the one hand, the strength and speed of monetary transmission may vary from country to country. We call it "structural" heterogeneity and examine a battery of time- and country-specific explanatory variables. On the other hand, as discussed in Section 3, the different specifications of VAR models are likely to produce different results. We call it "method" heterogeneity and add variables reflecting study design, study quality, and author characteristics.

As a motivation for the investigation of structural heterogeneity consider Figure 5, which depicts the differences in monetary transmission among selected countries. We use a simple random-effects meta-analysis to compute impulse-response functions. The simple meta-analysis weighs each estimate by its precision and adds an estimate-specific random effect; it does not correct for publication bias. We use the simple meta-analysis for estimation by countries since it requires less degrees of freedom than the meta-regression. Figure 5 shows that the impulse responses for the United States, United Kingdom, and Japan exhibit the price puzzle, but that in the countries using the euro monetary transmission seems to work intuitively and prices decline soon after a tightening. Of course, a part of these differences may arise from the use of diverse methods since some countries are only examined in a few studies.

Figure 5: Aggregate impulse responses for individual countries suggest structural heterogeneity



Note: Confidence bands are constructed as +/− one standard error.

To account for heterogeneity we extend meta-regression (5) to the following multivariate version:

$$t_{ij} = \beta_0 + \frac{\beta}{SE_{ij}} + \sum_{k=1}^{K} \frac{\gamma_k Z_{ijk}}{SE_{ij}} + \alpha_j + \epsilon_{ij}, \tag{6}$$

where Z denotes explanatory variables assumed to affect the reported estimates. The exogeneity assumptions become $\alpha_j|SE_{ij}, Z_{ijk} \sim N(0, \psi)$ and $\epsilon_{ij}|SE_{ij}, \alpha_j, Z_{ijk} \sim N(0, \theta)$.

Table 3 presents descriptions and summary statistics of all explanatory variables we consider. They can be divided into five groups: variables capturing structural heterogeneity explain the real determinants of the strength of monetary policy, data characteristics explain the differences in the data used, specification characteristics explain the differences in the basic design of the estimated models, estimation characteristics explain the differences in the econometric techniques, and publication characteristics explain mainly the differences in quality not captured by other variables.

Table 3: Description and summary statistics of regression variables

Variable	Description	Mean	St. dev.
Response (3M)	The percentage response of prices 3 months after a tightening.	-0.036	0.695
Response (6M)	The percentage response of prices 6 months after a tightening.	-0.077	0.899
Response (12M)	The percentage response of prices 12 months after a tightening.	-0.151	1.028
Response (18M)	The percentage response of prices 18 months after a tightening.	-0.238	1.368
Response (36M)	The percentage response of prices 36 months after a tightening.	-0.584	1.736
$1/SE$	The precision of the estimate of the response (all horizons).	6.834	7.868
Structural heterogeneity			
GDP per capita	The logarithm of the country's real GDP per capita.	2.288	0.044
GDP growth	The average growth rate of the country's real GDP.	2.691	1.063
Inflation	The average inflation of the country.	8.053	14.82
Inflation volatility	The standard deviation of the country's Hodrick-Prescott-filtered inflation.	6.753	35.28
Financial development	The financial development of the country measured by (domestic credits to private sector)/GDP.	0.821	0.419
Openness	The trade openness of the country measured by (exports + imports)/GDP.	0.470	0.405
CB independence	A measure of central bank independence (Arnone *et al.*, 2009).	0.776	0.145
Data characteristics			
Monthly	=1 if monthly data are used.	0.612	0.488
Time span	The number of years of the data used in the estimation.	18.13	10.05
Observations	The logarithm of the number of observations used.	4.835	0.644
Average year	The average year of the data used (2000 as a base).	-8.512	7.716
Specification characteristics			
GDP deflator	=1 if the GDP deflator is used instead of the consumer price index as a measure of prices.	0.184	0.388
Single	=1 if the VAR is estimated over a period of a single monetary policy regime.	0.301	0.459
Lags	The number of lags in the model, normalized by frequency: lags/frequency	0.592	0.338
Commodity	=1 if a commodity price index is included.	0.616	0.492
Money	=1 if a monetary aggregate is included.	0.524	0.499
Foreign	=1 if at least one foreign variable is included.	0.131	0.338
Time trend	=1 if a time trend is included.	0.131	0.338
Seasonal	=1 if seasonal dummies are included.	0.155	0.362
Variables	The logarithm of the number of endogenous variables included in the VAR.	1.737	0.374
Industrial production	=1 if industrial production is used as a measure of economic activity.	0.437	0.496
Output gap	=1 if output gap is used as a measure of economic activity.	0.029	0.168
Other	=1 if another measure of economic activity is used (employment, expenditures).	0.126	0.332
Estimation characteristics			
BVAR	=1 if a Bayesian VAR is estimated.	0.116	0.321
FAVAR	=1 if a factor-augmented VAR is estimated.	0.053	0.225
SVAR	=1 if non-recursive identification is employed.	0.277	0.448
Sign restrictions	=1 if sign restrictions are employed.	0.150	0.357
Publication characteristics			
Study citations	The logarithm of [(Google Scholar citations of the study)/(age of the study) + 1]. Collected in May 2010.	1.829	1.252

Continued on next page

Table 3: Description and summary statistics of regression variables (continued)

Variable	Description	Mean	St. dev.
Impact	The recursive RePEc impact factor of the outlet. Collected in May 2010.	0.869	2.369
Central banker	=1 if at least one co-author is affiliated with a central bank.	0.442	0.497
Policymaker	=1 if at least one co-author is affiliated with a Ministry of Finance, IMF, OECD, or BIS.	0.058	0.234
Native	=1 if at least one co-author is native to the investigated country.	0.427	0.495
Publication year	The year of publication (2000 as a base).	5.136	3.768

Structural heterogeneity When constructing variables that capture structural heterogeneity we use average values that correspond with the sample employed in the estimation of the impulse response. For instance in the case of inflation, when the impulse response comes from a VAR model estimated on the 1990:1–1999:12 Italian data, we use the average inflation in Italy for the period 1990–1999. This approach increases the variability in regressors and describes the estimates more precisely than using the same year of structural variables for all extracted impulse responses.

Variable GDP per capita reflects the importance of the development of the economy for monetary transmission. To investigate whether the strength of transmission depends on the phase of the economic cycle, we include variable GDP growth into the meta-regression. A reason why the effects of monetary policy may vary between boom and bust periods is the existence of the credit market imperfections that could amplify the effects of shocks in the stages of lower growth (Bernanke & Gertler, 1989). Next, we examine variables implied by various channels of the transmission mechanism. We include the trade openness of the economy to capture not only the importance of foreign developments for domestic monetary policy, but also the exchange rate channel of monetary transmission. A measure of financial development, approximated by the ratio of private credits to GDP, is included to account for the credit channel.

We add the average level and volatility of inflation to examine their influence on the formation of inflation expectations, and therefore on the strength of transmission as well. The inclusion of an index of central bank independence is motivated by Alesina & Summers (1993), who find a negative relation between central bank independence and inflation; a similar result is reported in a recent meta-analysis by Klomp & de Haan (2010). We test whether the degree of central bank independence affects the strength of monetary transmission.

Concerning the sources of the data, openness, GDP growth, and GDP per capita are obtained from Penn World Tables. Consumer price index, used to compute the average inflation and inflation volatility, is obtained from the IMF's International Financial Statistics. The ratio of domestic credits to GDP is obtained from the World Bank's World Development Indicators, and the index of central bank independence is extracted from Arnone *et al.* (2009).

Data characteristics We control for the frequency of data used in the VAR: 61% of specifications use monthly, the rest quarterly data. To account for possible changes in transmission not explained by the structural variables (for example, changes caused by globalization or financial innovations, Boivin & Giannoni, 2006), we include the average year of the sample period used in the estimation. Finally, we add the total number of observations to assess whether smaller models yield systematically different outcomes.

Specification characteristics To account for the different measures of the price level we include a dummy which equals one when the GDP deflator is used instead of the usual consumer price index (18% of specifications). We add a dummy for the case when the data cover a period of a single monetary policy regime (30%). Next, we include the VAR's lag order normalized by frequency. We account for the cases when commodity prices, a money aggregate, foreign variables, a time trend, and seasonal dummies are included in the VAR. We also control for the number of endogenous variables in the model. Since the results might vary substantially depending on the measure of economic activity, we introduce dummies for the cases when industrial production, output gap, or another measure is used instead of GDP.

Estimation characteristics Most of the studies in our sample estimate VAR models using the standard methods (OLS or Maximum Likelihood); we control for studies using Bayesian methods to address the problem of overparametrization (12% of specifications), and for studies using the FAVAR approach to address the problem of omitted variables (5%). As for identification strategies, most of the studies employ recursive identification; we include a dummy for non-recursive identification (28%) and a dummy for identification using sign restrictions (15%).

Publication characteristics To proxy study quality we use the recursive RePEc impact factor of the outlet and the number of Google Scholar citations of the study normalized by the study's age. We add a dummy which equals one when the author is affiliated with a central bank, and a dummy for authors working at institutions such as a Ministry of Finance, the International Monetary Fund, or the Bank for International Settlements. We include a dummy for the case when at least one co-author is native to the examined country: such authors may be more familiar with the data at hand, which can contribute to the quality of the analysis. We consider authors native if they either were born in the country or obtained an academic degree there. Finally, we use the year of publication to account for possible improvements in methodology that are otherwise difficult to codify.

In the first step we estimate a general model containing all explanatory variables; the general model is not reported but available on request. All variance inflation factors are lower than 10 indicating that multicolinearity is not dramatic. In the second step, we drop the variables which are, for each horizon, jointly insignificant at the 10% level. For example, GDP per capita, the number of lags used, and most publication characteristics belong to the dropped variables. The resulting, more parsimonious model is presented in Table 4 (structural heterogeneity) and Table 5 (method heterogeneity). The specifications reported in this section are based on the mixed-effects multilevel estimator, but the inference would be similar from OLS with standard errors clustered at the study level; these sensitivity checks are available

Table 4: Structural heterogeneity

	Mixed-effects multilevel				
Horizon	3 months	6 months	12 months	18 months	36 months
GDP growth	-0.0066	0.0074	0.0231**	0.0233**	0.0388***
	(0.0078)	(0.0103)	(0.0112)	(0.0113)	(0.0113)
Inflation	0.0004	-0.0007	0.0029	0.0049*	0.0091***
	(0.0028)	(0.0033)	(0.0034)	(0.0028)	(0.0028)
Inflation volatility	-0.0004	0.0003	-0.0011	-0.0021*	-0.0043***
	(0.0011)	(0.0013)	(0.0014)	(0.0012)	(0.0012)
Financial development	0.101***	0.084*	0.144**	0.089	-0.022
	(0.036)	(0.047)	(0.062)	(0.061)	(0.066)
Openness	-0.027	-0.046	-0.064	-0.082*	-0.281***
	(0.039)	(0.048)	(0.055)	(0.048)	(0.040)
CB independence	0.077	-0.020	-0.019	-0.145*	-0.293***
	(0.072)	(0.091)	(0.095)	(0.083)	(0.074)
Within-study correlation	0.33	0.41	0.33	0.36	0.36
Observations	202	206	206	205	194
Studies	65	65	65	65	58

Note: Standard errors in parentheses. The effect is in %. Response variable: the approximated t-statistic of the estimate. All explanatory variables are divided by the approximated standard error of the estimate at the corresponding horizon. The intercept, precision, and variables capturing method heterogeneity are included in all specifications (these results are reported in Table 5).
***, **, and * denote significance at the 1%, 5%, and 10% levels.

in Appendix A. The similarity between the outcomes of these two estimators indicates that the exogeneity assumptions made in the mixed-effects estimation hold; in meta-analysis it is difficult to test exogeneity formally because the extreme unbalancedness of the data does not permit construction of a reasonable fixed-effects model (some studies report only one impulse response). We prefer mixed effects over OLS because likelihood-ratio tests reject the hypothesis of zero within-study variance, and OLS is thus misspecified.

Concerning structural heterogeneity, a result worth noting is that the GDP growth, the openness of the economy, the level and volatility of inflation, and the degree of central bank independence systematically affect the estimated impulse response of prices to monetary tightening in the medium to long run. The response weakens in the periods of higher GDP growth. This result is consistent with Bernanke & Gertler (1995), who argue that asymmetric information and other credit market frictions could amplify the effects of monetary policy through the so-called financial accelerator. In the periods of lower growth the firms' dependence on external financing increases, and changes in the interest rate hence become more important.

The expectation channel of monetary transmission can explain why the impact of monetary policy diminishes in the periods of higher inflation: high inflation impedes the credibility of the central bank and restricts its ability to control the price level. Furthermore, if the volatility of inflation increases, the effect on prices strengthens, possibly because higher volatility makes the central bank more aggressive. Monetary policy is more effective in open economies, where its impact can be amplified through the exchange rate channel. As expected, monetary policy is more powerful if the central bank enjoys more independence, which corresponds with the

Table 5: Method heterogeneity

	Mixed-effects multilevel				
Horizon	3 months	6 months	12 months	18 months	36 months
Intercept (bias)	-0.099	-0.168	-0.224*	-0.212*	-0.569***
	(0.135)	(0.139)	(0.131)	(0.122)	(0.135)
$1/SE$	-0.087	-0.112	-0.282	-0.295*	-0.169
	(0.119)	(0.149)	(0.178)	(0.167)	(0.190)
Data characteristics					
Observations	0.018	0.026	0.039	0.075***	0.152***
	(0.019)	(0.025)	(0.029)	(0.029)	(0.031)
Average year	0.0022	-0.0001	0.0025	0.0055**	0.0139***
	(0.0021)	(0.0026)	(0.0029)	(0.0028)	(0.0033)
Specification characteristics					
GDP deflator	0.019	0.041	0.134***	0.178***	0.218**
	(0.025)	(0.034)	(0.047)	(0.060)	(0.109)
Single	0.029	0.030	0.021	0.011	0.082**
	(0.020)	(0.026)	(0.033)	(0.034)	(0.035)
Commodity	-0.045***	-0.058***	-0.122***	-0.150***	-0.216***
	(0.016)	(0.021)	(0.029)	(0.031)	(0.031)
Foreign	0.014	0.031	0.063**	0.078**	0.136***
	(0.017)	(0.023)	(0.031)	(0.034)	(0.043)
Variables	-0.021	-0.027*	-0.025	-0.044*	-0.183***
	(0.014)	(0.015)	(0.022)	(0.026)	(0.046)
Industrial production	0.024	0.058**	0.075**	0.083**	-0.008
	(0.024)	(0.029)	(0.035)	(0.038)	(0.036)
Output gap	-0.256	-0.304**	-0.210**	-0.125*	0.0121
	(0.164)	(0.136)	(0.082)	(0.067)	(0.004)
Other	-0.074**	-0.047	-0.015	0.002	-0.010
	(0.030)	(0.043)	(0.056)	(0.063)	(0.085)
Estimation characteristics					
BVAR	0.112***	0.100**	0.044	0.045	0.054
	(0.034)	(0.046)	(0.063)	(0.080)	(0.133)
FAVAR	-0.134***	-0.185***	-0.116	0.016	0.295***
	(0.037)	(0.059)	(0.080)	(0.083)	(0.114)
SVAR	-0.064***	-0.094***	-0.121***	-0.141***	-0.064***
	(0.017)	(0.019)	(0.023)	(0.022)	(0.024)
Sign restrictions	-0.302***	-0.294***	-0.284***	-0.293***	-0.236*
	(0.037)	(0.055)	(0.072)	(0.088)	(0.143)
Publication characteristics					
Central banker	0.037*	0.060**	0.067**	0.073**	0.129***
	(0.022)	(0.028)	(0.032)	(0.033)	(0.035)
Policymaker	-0.058*	-0.041	0.056	0.096***	0.166***
	(0.035)	(0.044)	(0.040)	(0.037)	(0.042)
Within-study correlation	0.33	0.41	0.33	0.36	0.36
Observations	202	206	206	205	194
Studies	65	65	65	65	58

Note: Standard errors in parentheses. The effect is in %. Response variable: the approximated t-statistic of the estimate. All explanatory variables are divided by the approximated standard error of the estimate at the corresponding horizon. Variables capturing structural heterogeneity are included in all specifications (these results are reported in Table 4). ***, **, and * denote significance at the 1%, 5%, and 10% levels.

findings of Alesina & Summers (1993).

In contrast, the structural variables are not so significant for the short-run response, with the exception of the financial development indicator. Our results suggest that a higher development of the financial system makes the short-run impact of monetary policy weaker. The finding is consistent with Cecchetti (1999), who reports that the effects of monetary policy are more important in countries with many small banks, less healthy banking systems, and underdeveloped capital markets.

Concerning data characteristics, the results presented in Table 5 indicate that the number of observations systematically influences the estimated long-run effect: more data make the reported response of prices weaker. In line with Boivin & Giannoni (2006) the reported long-run response weakens when newer data are used. Specification characteristics are important as well. The GDP deflator reacts less to monetary tightening than the consumer price index does. The inclusion of commodity prices is important for all horizons and amplifies the estimated decrease in prices. When industrial production is used instead of GDP as a measure of economic activity, the reported response becomes weaker; the response becomes stronger when output gap is used.

Estimation methods are important especially for the short-run response. In the 3- and 6-month horizon, Bayesian estimation produces a smaller decrease in prices compared with the baseline VAR. The use of FAVAR, non-recursive identification, and sign restrictions contributes to reporting more effective monetary policy. It is worth noting that all methodological explanations of the price puzzle that were discussed in Section 3 indeed contribute to alleviating the puzzle and therefore to estimating intuitive impulse responses (with the exception of the effect of a single regime of monetary policy, which has the opposite sign, but is statistically insignificant).

Our results suggest that authors affiliated with central banks report less effective monetary policy. This is surprising since central bankers are naturally interested in presenting well-functioning transmission of monetary shocks. As noted by Stanley *et al.* (2008), though, when submitting their work to journals, researchers may try to compensate possible prejudices to persuade the editor that their research is unbiased. For instance, female researchers were found to report systematically weaker evidence for the wage discrimination of women (Jarrell & Stanley, 2004).

The multivariate meta-regression corroborates evidence for publication selection reported in Section 4. The intercept, a measure of publication bias, is significant for the 12-, 18-, and 36-month horizons. The estimate of true effect in the multivariate model, however, is not simply represented by the regression coefficient for $1/SE$, but is conditional on variables capturing heterogeneity. In order to estimate true effect we need to choose the preferred values of explanatory variables, thus defining the so-called "best practice." In this way we create a synthetic study with ideal parameters. Suitably defined best-practice estimation can filter out misspecification bias from the literature, although the approach is subjective as different researchers may have different opinions on what constitutes best practice.

We define best practice by selecting methodology characteristics based on the discussion in Section 3: we prefer output gap over GDP as a measure of economic activity, non-recursive identification over the vanilla VAR, data covering a single monetary policy regime over mixing more regimes, and the inclusion of commodity prices and foreign variables over omitting them. In addition, we prefer Bayesian estimation since over-parametrization can be a problem even for systems of modest size (Banbura *et al.*, 2010). We plug in sample maximums for the number of observations, the year of the data, and the number of endogenous variables. The other variables, including all that explain structural heterogeneity, are set to their sample means. The estimated impulse response implied by best practice is depicted in Figure 6: after controlling for both publication and misspecification bias, the puzzle disappears and prices bottom out six months after a one-percentage-point hike in the interest rate; the maximum decrease in the price level reaches 0.3%.

Figure 6: Impulse response implied by best practice shows no price puzzle

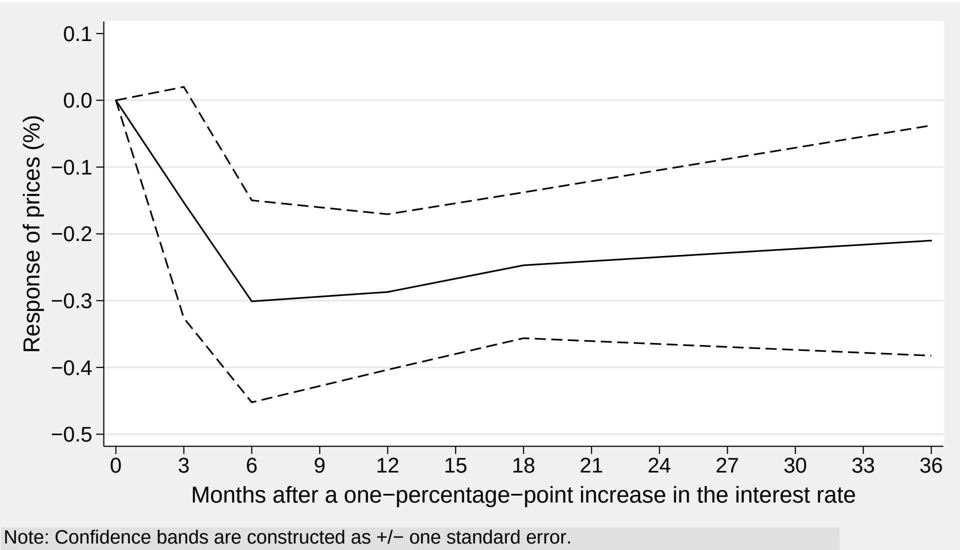


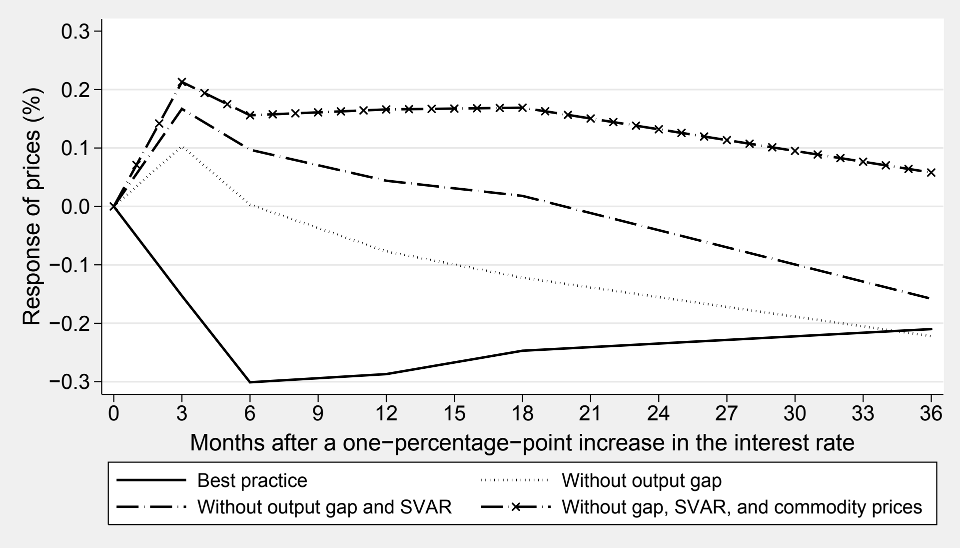
Table 6: Consequences of misspecifications

	Linear combination of regressors' values				
Horizon	3 months	6 months	12 months	18 months	36 months
Best practice	-0.153	-0.301**	-0.287**	-0.247**	-0.210
Without output gap	0.103	0.003	-0.077	-0.122	-0.222
Without gap and SVAR	0.167**	0.097	0.044	0.018	-0.158
Without gap, SVAR, and com. prices	0.213**	0.156	0.166	0.169	0.058

Note: The effect is in %.
Without output gap = Best practice omitting output gap. Without gap and SVAR = Best practice omitting output gap and using recursive identification. Without gap, SVAR, and com. prices = Best practice omitting output gap, using recursive identification, and omitting commodity prices.
***, **, and * denote significance at the 1%, 5%, and 10% levels.

The dissolution of the price puzzle is robust both individually and cumulatively to other possible definitions of best practice: selecting the FAVAR approach instead of the Bayesian ap-

Figure 7: Deviations from best practice: the price puzzle can be explained by misspecifications



proach, selecting sign restrictions instead of non-recursive identification, or selecting the sample mean of the number of endogenous variables instead of the sample maximum. To illustrate the consequences of misspecifications on the reported impulse responses, Table 6 and Figure 7 investigate the cases when some aspects of methodology deviate from best practice. When the model does not control for the potential of the economy, the price puzzle occurs, but prices at least decline in the medium and long run. When the model combines the omission of output gap with the use of recursive identification, the puzzle gets stronger and prices decline below the initial level only after 18 months. When moreover the model omits a measure of commodity prices, the price level is reported never to decline below the initial level during the 3-year horizon after monetary tightening. Hence the VAR literature combined indicates that the price puzzle arises largely from misspecifications of the estimated model.

6 Conclusion

We examine the impact of monetary policy tightening on the price level by employing quantitative synthesis of impulse-response functions from published VARs. We collect estimates of impulse responses for 36 countries produced by 94 researchers and regress the estimates on variables reflecting study design and author and country characteristics. To account for within-study dependence in the estimates, we employ mixed-effects multilevel meta-regression. Recently developed meta-analysis methods allow us to estimate the underlying effect of monetary policy implied by the entire literature net of the bias caused by publication selection and misspecifications of some VAR models.

Our results indicate that estimates reporting more effective monetary policy (that is, a greater decrease in the price level following monetary tightening) tend to be preferentially selected for publication. The longer the horizon after a tightening, the stronger the selection.

For the short-run response some theory competition exists since the counterintuitive increase in prices can be explained by the cost channel. In contrast, no widely accepted theory can explain why prices should stay above the initial level even in the long run. This relation between publication selection and theory competition corroborates the findings of Doucouliagos & Stanley (2008), who report a similar phenomenon for may areas of empirical research. The VAR literature, on average, seems to substantially exaggerate the long-run response of prices.

The responses are systematically affected by study design and structural country-specific characteristics. Study design is particularly important for the short-run response: the reported short-run increase in prices after a tightening is well explained by the effects of the commonly questioned aspects of methodology, such as the omission of commodity prices, the omission of potential output, or the use of recursive identification. When these are filtered out, the impulse-response function inferred from the entire literature is U-shaped with no evidence of the price puzzle. The maximum decrease in the price level following a one-percentage-point increase in the interest rate reaches 0.3% and occurs already half a year after the tightening.

The long-run response depends on the characteristics of the examined country; on average, the decrease in prices is quite persistent. The effect of monetary policy weakens when inflation rises, possibly because high inflation hampers the credibility of the central bank. Monetary policy strengthens with a greater openness of the economy. A plausible explanation involves the exchange rate channel: the increase in the interest rate appreciates the exchange rate and hinders exports. Monetary policy also strengthens when the central bank has more independence.

The robustness of our results could be further examined in two ways. First, researchers may add all unpublished studies into the sample of literature; this would require collecting information from hundreds of additional manuscripts, but enable the researchers, for instance, to focus exclusively on one selected country. Second, researchers may conduct a meta-analysis of the effect of monetary policy on inflation. For the sake of compatibility, in this paper we include only studies using the price level and leave the investigation of the response of inflation for future research. The method of quantitative synthesis of graphical results can moreover be applied to any other field that uses VARs as a research tool.

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Appendix A: Sensitivity Checks

Table A1: Test of publication bias and true effect, OLS

	OLS with clustered standard errors				
Horizon	3 months	6 months	12 months	18 months	36 months
Intercept (bias)	-0.297	-0.453**	-0.393**	-0.440***	-0.821***
	(0.181)	(0.188)	(0.154)	(0.140)	(0.114)
$1/SE$ (effect)	0.033**	0.033	-0.0071	-0.027**	-0.016**
	(0.014)	(0.022)	(0.016)	(0.013)	(0.007)
Observations	202	206	206	205	194
Studies	65	65	65	65	58

Note: Standard errors, clustered at study level, in parentheses. The effect is in %. Response variable: the approximated t-statistic of the estimate.
***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table A2: Structural heterogeneity, OLS

	OLS with clustered standard errors				
Horizon	3 months	6 months	12 months	18 months	36 months
GDP growth	-0.009	0.008	0.024*	0.027*	0.035
	(0.008)	(0.011)	(0.014)	(0.015)	(0.024)
Inflation	-0.0002	-0.0027	0.0033	0.0054**	0.0079***
	(0.0035)	(0.0043)	(0.0033)	(0.0022)	(0.0016)
Inflation volatility	-0.0001	0.0012	-0.0012	-0.0021**	-0.0036***
	(0.0014)	(0.0017)	(0.0013)	(0.0009)	(0.0009)
Financial dev.	0.092***	0.085	0.167**	0.124*	-0.046
	(0.029)	(0.055)	(0.074)	(0.066)	(0.062)
Openness	-0.026	-0.050	-0.086*	-0.119**	-0.261**
	(0.031)	(0.049)	(0.049)	(0.051)	(0.120)
CB independence	0.025	-0.160	-0.095	-0.200	-0.336***
	(0.069)	(0.112)	(0.129)	(0.122)	(0.058)
Observations	202	206	206	205	194
Studies	65	65	65	65	58

Note: Standard errors, clustered at study level, in parentheses. The effect is in %. Response variable: the approximated t-statistic of the estimate. All explanatory variables are divided by the approximated standard error of the estimate at the corresponding horizon. The intercept, precision, and variables capturing method heterogeneity are included in all specifications (these results are reported in Table A3).
***, **, and * denote significance at the 1%, 5%, and 10% levels.

Table A3: Method heterogeneity, OLS

	OLS with clustered standard errors				
Horizon	3 months	6 months	12 months	18 months	36 months
Intercept	-0.119	-0.167	-0.268**	-0.259**	-0.556***
	(0.155)	(0.139)	(0.131)	(0.121)	(0.127)
$1/SE$	-0.061	-0.086	-0.247	-0.247	-0.057
	(0.068)	(0.115)	(0.171)	(0.167)	(0.218)
Data characteristics					
Observations	0.025*	0.042*	0.043*	0.069**	0.133***
	(0.013)	(0.024)	(0.025)	(0.028)	(0.046)
Average year	0.0011	-0.00033	0.0032	0.0063**	0.0135***
	(0.0013)	(0.0023)	(0.0024)	(0.0026)	(0.0030)
Specification characteristics					
GDP deflator	-0.0005	0.021	0.123***	0.156***	0.165**
	(0.0151)	(0.026)	(0.046)	(0.058)	(0.071)
Single	0.039**	0.030	0.012	0.004	0.093*
	(0.015)	(0.022)	(0.026)	(0.029)	(0.053)
Commodity	-0.047***	-0.062***	-0.135***	-0.164***	-0.208***
	(0.008)	(0.017)	(0.022)	(0.028)	(0.060)
Foreign	0.011	0.039**	0.071**	0.087**	0.099*
	(0.016)	(0.016)	(0.032)	(0.038)	(0.057)
Variables	-0.026*	-0.026**	-0.025	-0.042	-0.159***
	(0.014)	(0.010)	(0.019)	(0.026)	(0.039)
Industrial prod.	0.021	0.061***	0.081**	0.094**	-0.016
	(0.016)	(0.020)	(0.032)	(0.037)	(0.039)
Output gap	-0.267***	-0.327***	-0.216***	-0.123***	0.013
	(0.091)	(0.103)	(0.055)	(0.035)	(0.032)
Other	-0.098***	-0.081**	-0.017	0.014	0.035
	(0.023)	(0.036)	(0.058)	(0.069)	(0.069)
Estimation characteristics					
BVAR	0.138***	0.115***	0.043	0.027	0.015
	(0.027)	(0.033)	(0.050)	(0.072)	(0.131)
FAVAR	-0.082***	-0.116***	-0.081	0.0092	0.271***
	(0.026)	(0.040)	(0.051)	(0.059)	(0.071)
SVAR	-0.086***	-0.128***	-0.144***	-0.156***	-0.048
	(0.019)	(0.028)	(0.030)	(0.028)	(0.033)
Sign restrictions	-0.305***	-0.310***	-0.298***	-0.305***	-0.174
	(0.031)	(0.041)	(0.058)	(0.088)	(0.141)
Publication characteristics					
Central banker	0.026*	0.066**	0.079**	0.095**	0.128***
	(0.014)	(0.026)	(0.033)	(0.038)	(0.035)
Policymaker	-0.052**	-0.014	0.077**	0.099***	0.119***
	(0.024)	(0.023)	(0.033)	(0.033)	(0.033)
Observations	202	206	206	205	194
Studies	65	65	65	65	58

Note: Standard errors, clustered at study level, in parentheses. The effect is in %. Response variable: the approximated t-statistic of the estimate. All explanatory variables are divided by the approximated standard error of the estimate at the corresponding horizon. Variables capturing structural heterogeneity are included in all specifications (these results are reported in Table A2).
***, **, and * denote significance at the 1%, 5%, and 10% levels.

Appendix B: Studies Used in the Meta-Analysis

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