Unexpectedly Broke:
Expectation Errors and Credit Cycles

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Abstract. Financial crises are usually preceded by debt booms, but the origins of the latter are unknown. We show that inaccurate income expectations are a plausible candidate for excessive debt accumulation across the world. For this, we collect an unbalanced panel dataset of quarterly macroeconomic forecasts by financial institutions for a diverse set of 32 countries since 1989. Predictions about GDP growth serve as a proxy for income expectations in the economy. We find that positive (i.e. too optimistic) mean forecast errors are accompanied by higher credit growth. While household credit is related to expectation errors, firm debt is not. We interpret this finding in light of recent theories which attribute a causal role in the build-up of financial imbalances to biases in agents' consumption-savings decision. This result holds within developing and industrialized countries and is robust to controlling for economic conditions and other expectations. We also shed light on how expectation errors arise in the first place: Forecasters are overprecise in their estimates and they update their expectations too much when they receive new information.

Keywords: credit, financial cycle, survey expectations, forecast errors

JEL Codes: E44, E51, C83, D84, G01

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1 Introduction

Private debt booms hold risks for the economy. Excessive credit growth has been shown to precede recessions, financial crises and to lower returns to bank capital and bonds.[1] However, the question remains why people accumulate so much debt in the first place. If individuals rationally decide how much to consume and save, why do we observe these ups and downs in credit and economic activity?

Understanding the causes of debt booms is essential for providing policy makers with good advice and the right tools to mitigate their effects. Households might rationally take on debt due to higher anticipated earnings or lower interest rates and an increase in debt might therefore not call for a market intervention. But if savings decisions are based on unreasonable income expectations and this could somehow be found out in advance, then there might be room for policy action.

We ask whether errors in income expectations are a plausible explanation for credit accumulation. For this, we propose a new measure of errors in aggregate income expectations. Some studies — e.g. De Stefani (2017) and Rozsypal and Schlafmann (2017) — use consumer surveys for measuring income expectations, but such data is only available for few countries. However, credit cycles are slow-moving and financial crises are rare events, so data with sufficient international coverage is necessary to allow making statements with enough statistical power. Good international data coverage also helps eliminate country idiosyncrasies and to find macroeconomic regularities that hold more generally. This paper uses a collection of professional forecasts by the private data provider *Consensus Economic Forecasts* (CEF). The CEF sends a survey every month to financial firms, banks and economic research institutes and asks them to predict macroeconomic variables.

We obtain quarterly GDP forecasts and calculate forecast errors as the difference between mean predictions and realizations. Taking forecasts as proxies for expectations in the economy, we examine what happens with household debt accumulation in periods of overly optimistic expectations. The resulting quarterly dataset covers 32 countries accounting for 79 percent of global output over almost three decades. Our analysis shows that positive forecast errors in 12 month ahead real GDP growth are contemporaneously correlated with booms in household debt growth. This association holds across time periods, within industrialized and developing countries, controlling for time and country fixed effects, excluding banking crises and when controlling for the state of countries' business cycles and ex ante real interest rates. Household debt reacts strongly, but there is no relevant comovement of expectation errors with firm debt growth. This evidence is in line with Mian, Sufi, and Verner (2017a) who show that higher household debt predicts negative GDP forecast errors. We turn their analysis around and ask what drives debt accumulation. Also, we use a different data source which allows us to use quarterly, not annual, observations.

After establishing our baseline result, we provide additional evidence from another dataset. We

[1] See Kaminsky and Reinhart (1999), Schularick and Taylor (2012), Greenwood and Hanson (2013), Alessi and Detken (2017), Chen and Rancière (2016), Baron and Xiong (2017), Lopez-Salido et al. (2017) and Mian et al. (2017a).

use the ECB's *Survey of Professional Forecasters* in which forecasters provide subjective confidence bands. On the downside, this dataset only covers the euro area. We show unambiguously that panelists suffer from overprecision in their forecasts. The 95 percent confidence intervals that panelists provide are so narrow that they cover only a third of subsequent realizations. We take this as a further support for our approach of using forecast errors as proxies for expectation errors.

We then dive into how panelists update their forecasts when they receive new information. We use a method from Coibion and Gorodnichenko (2012) and Bordalo, Gennaioli, Ma, and Shleifer (2018) to show strong evidence for overreaction by forecasters, a finding that is robust across countries and two levels of observation. In particular in the run-up to the financial crisis up to 2006 expectation formation showed strong signs of overreaction.

This paper is, to our knowledge, the first to provide comprehensive empirical evidence that misaligned income expectations are a plausible explanation for credit growth across the world. This is relevant for economic policy as it provides further support to the recent efforts to monitor — and maybe regulate — lending to households.

2 Theories of credit booms

Since the financial markets turmoil of 2007 onwards, economists' interest in understanding the causes and consequences of financial crises has been rekindled. A robust empirical finding is that private credit tends to rise before trouble hits financial markets and the economy. The literature offers several explanations for debt booms and these theories make predictions on how forecast errors and credit cycles should be related in the data.

Cochrane (2017) surveys the field of macrofinance and lays out the unifying framework of a cyclical bias in the representative agent's consumption-savings decision. This wedge leads to a comovement of economic and financial activity in the economy. These biases take different forms, such as neglecting small probability events, extrapolative expectations or habits. But the effects from these distortions tend to be alike: Lenders overestimate the present value of current and future incomes tempting them to hold more debt than they can stomach, while savers overestimate the capacity of households or firms to repay debts.

Whether it is the providers or the receivers of credit who change their behavior is of relevance to how we expect prices to adapt. If banks become more willing to lend, then we would expect interest rates to drop. Mian et al. (2017a) provide evidence that credit booms are driven by fluctuations in credit supply. This idea is in line with Kindleberger (1978), who argues that "in moments of euphoria" (p.57) banks will come up with new ways to lend and create liquidity, thus increasing the supply of credit. Bordalo, Gennaioli, and Shleifer (2017) provide a rationalization for how this might come about. In their model, the agent's savings decision is distorted by the representative heuristic which leads to extrapolative expectations. This means that in good times, agents underestimate the probability for lending firms to default.

Conversely, if households or firms demand more credit, we expect interest rates to rise. In Mian et al. (2017b), this takes the form of a temporarily lower effective interest rate that households face. This, they argue, might be due to financial deregulation or overoptimistic income expectations. The lower interest rate induces households to take on debt to finance higher consumption. Gennaioli, Ma, and Shleifer (2016) find evidence that firm managers have extrapolative expectations. This could similarly lead them to be too optimistic when times are good. Blanchard, L'Huillier, and Lorenzoni (2013) provide a model in which noisy information about future productivity induce agents to consume more than optimal. This, too, might be an explanation for periodical expansions and contractions in household's saving behavior.

The equilibrium outcome in each case with respect to credit is the same: It rises due to mistakes in the expectation formation. In our empirical analysis, we therefore test whether the economy's indebtedness rises when agents are too optimistic.

3 Data

3.1 Forecasts

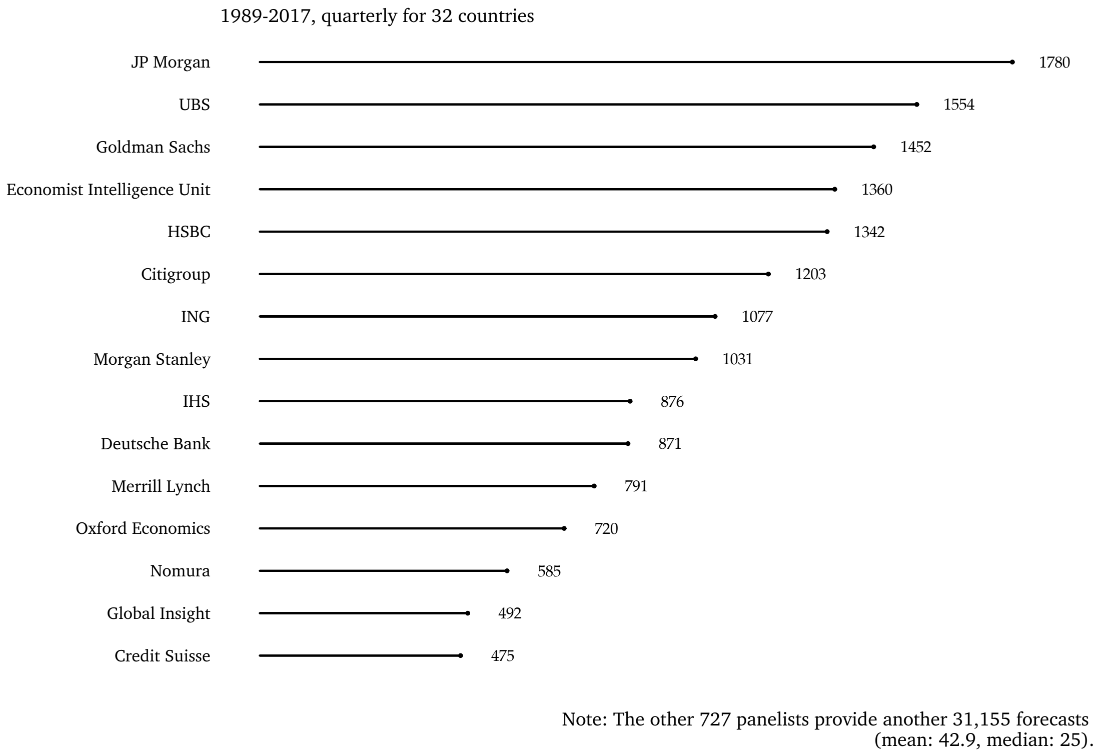
Near the middle of every month, forecasters of 32 countries fill out a survey by the private data provider *Consensus Economics* and predict real GDP growth for the current and next year. The CEF dataset is highly fragmented and manually processed as the data providers sends updates as PDF's and Excel sheets to institutions and Central Banks. We go to great lengths to collect and aggregate all available data to create one unified database of macroeconomic expectations.

Participating firms (or "panelists") are a mix of banks, private and public research institutes, market intelligence firms, industrial unions and business organizations. Figure 1 shows the panelists with the most forecasts (aggregated to quarterly) which are JP Morgan (5340), UBS (4662) and Goldman Sachs (4356). Forecasters or their subsidiaries reside in the country whose economy they predict. Berger, Ehrmann, and Fratzscher (2011) show that geography proximity increases forecaster accuracy. As participating firms might differ in their access to local information, in how they form forecasts and in the effort they exert, the quality of forecasts might also not be the same across firms. However, with about 17 (st.d.: ±5) forecasters per month and country, the weight of any individual forecasts is low.

Figure 2 shows how the number of panelists evolved. The number of panelists was approximately constant for most countries. Over the years, more panelists were surveyed in France and Germany and fewer for Great Britain, for which this number fell from the highest ever in 1994 with 39 forecasts. The Netherlands and Norway had the fewest forecasts with an average of 10 forecasts per month. The approximately constant number of forecasts is a further sign of the quality of the *Consensus Economics* data. This is in contrast to the US *Survey of Professional Forecasters*, where this number fell between 1970 and 1990 (Capistrán and Timmermann, 2009).

Panelists in the dataset are not anonymous, so career and reputational concerns might incentivize participating institutions to exert high effort to provide good forecasts. This is in contrast to other surveys such as the Philadelphia Fed's *Survey of Professional Forecasters* or the European Central Bank's *Survey of Professional Forecasters*, where the names of participating firms are not

Figure 1: Forecasts by panelist



public. However, not being anonymous might also keep panelists from making forecasts that are more unusual for fear of being singled out for large forecast errors.

Some researchers, such as Dovern, Fritsche, and Slacalek (2012) and Rülke, Silgoner, and Wörz (2016), have also worked with the CEF data to tackle different questions, but we are the first to measure cycles of expectation errors and their financial stability implications. We take the errors that participants make in their forecasts as a sign for optimism and pessimism. Ideally we would like to also know participants' individual uncertainty surrounding their point forecasts to assess whether they were really overconfident in their predictions. An advantage of obtaining the microdata of forecasts is our ability to track firms over time and to provide a measure of forecaster dispersion for which we use the standard deviation of point forecasts across panelists at any point in time. We take this as a proxy for forecaster uncertainty, an approach that Bachmann et al. (2013) find support for. The broad coverage of macroeconomic variables also means that we can control for other relevant expectations, such as the expected inflation rate.

Batchelor (2001) and Loungani (2001) analyze the performance of the CEF forecasts and show while they are better than OECD and the IMF forecasts, they are not very good in absolute terms. Breitung and Knüppel (2017) recently provide evidence that the CEF forecasts might not be informative beyond two to four quarters. For the argument in this paper we require that the predictions voiced by professional forecasters are indicative of the opinions held by agents in the economy. People cannot perfectly predict the course of economy and neither can professional forecasters.

Several assumptions are needed for interpreting forecaster errors as expectations of agents

Figure 2: Number of panelists

in the economy. First, we assume that households hold similar beliefs about the future as do professional forecasters. This might be the case if households and professional forecasters have the same information to construct forecasts. Or it could hold if professional forecasts are published in newspapers and people align their expectations with what they read. A last reason for such a connection between what households expect and what financial firms predict is that both might be driven some third factor such as “optimism", sometimes also called “sentiment" or “exuberance" (Shiller, 2000).

A second assumption becomes necessary when we think about a representative household's savings decision. Typically, households smooth consumption and thus when deciding on how to divide their income into consumption and saving, they not only take next year's income into account, but the discounted sum of all future incomes. So ideally, we would like to measure peoples' lifetime income expectations. However, expectations about GDP growth over the next 12 months are all that we can construct from the CEF data. Several facts mitigate this concern: Households discount incomes more, the further into the future they accrue. Next, if GDP is a random walk, then the one-period (12 month) ahead forecast is the same as the long-run forecast. Neither of these explanations is likely to be exactly true, but both make using the 12 month expected real GDP growth rate a plausible proxy for household's lifetime income expectations.

3.2 Macroeconomic data

For quarterly macroeconomic data we rely on a number of standard sources, such as the OECD, the IMF's International Financial Statistics and Balance, the IMF's Balance of Payments Statistics, national statistical agencies and national central banks.

Data on credit are provided by the BIS. The BIS defines "credit" as loans and debt securities provided to the private non-financial sector which includes non-financial firms, households and non-profit organizations serving households. The lenders can be domestic banks, the rest of the economy and foreigners. We separately use household debt including non-profit organizations serving households (hhd) and non-financial firm debt (fd).

Table 1: Data summary

Number countries	32
(of which developing countries)[1]	(47%)
Coverage starting in 1980s	6
Coverage starting in 1990s	18
Coverage starting in 2000s	8
Share world GDP, PPP (2015)	79%
Forecasts by country-month	17 (±5)

[1]: according to IMF

Table 1 summarizes the resulting dataset. It provides a good coverage of global economies, as countries in the sample accounted for 79 percent of global purchasing power adjusted real GDP in 2015. With 18 out of the largest 20 economies, we cover most major economies. This global coverage also allows us to trace the association between expectation formation and credit accumulation in developing countries which make up about half the countries in the sample. The data for most countries in the sample starts in the 1990s and runs until 2016. For the exact starting dates when countries join our dataset, see Tables A1 and A2.

3.3 Forecasts and realizations

Participating firms provide their forecasts for the annual values of the current and following year. For example, forecasters in June 2016 would provide their guesses for real GDP growth for the year 2016 and 2017. This poses a challenge for the interpretation of this data, as more information becomes available throughout the year. As an extreme case, a forecaster interviewed in mid-December will know with considerable accuracy what happened in the current year. This introduces a seasonality in the forecasts that hinders proper interpretation of this data.

Instead, we would like to use forecasts for the growth rate of real GDP twelve months from survey date. So the challenge is to convert *fixed event* to *fixed horizon* forecasts. Two papers provide methods to overcome this problem, tailored to the data structure of the CEF forecasts. Both use linear weightings of the forecasts for the current and the following year to construct the fixed horizon forecast. The first — Dovern, Fritsche, and Slacalek (2012) — suggests putting progressively less weight on the current year forecast as the year advances. While this holds intuitive appeal, there is no theoretical basis for using this method.

Knüppel and Vladu (2016) instead propose a different weighting which minimizes the expected squared error loss and this method performs better at approximating the fixed horizon forecasts. A key insight from the Knüppel and Vladu (2016) method is somewhat puzzling: For fixed horizon forecasts constructed for the first months in a year, they prescribe to put no weight on the forecasts for the current year. The optimal weights for the current year forecasts even become negative midyear and then positive at the end of the year. Overall, the absolute weight for current year forecasts is very low.[2]

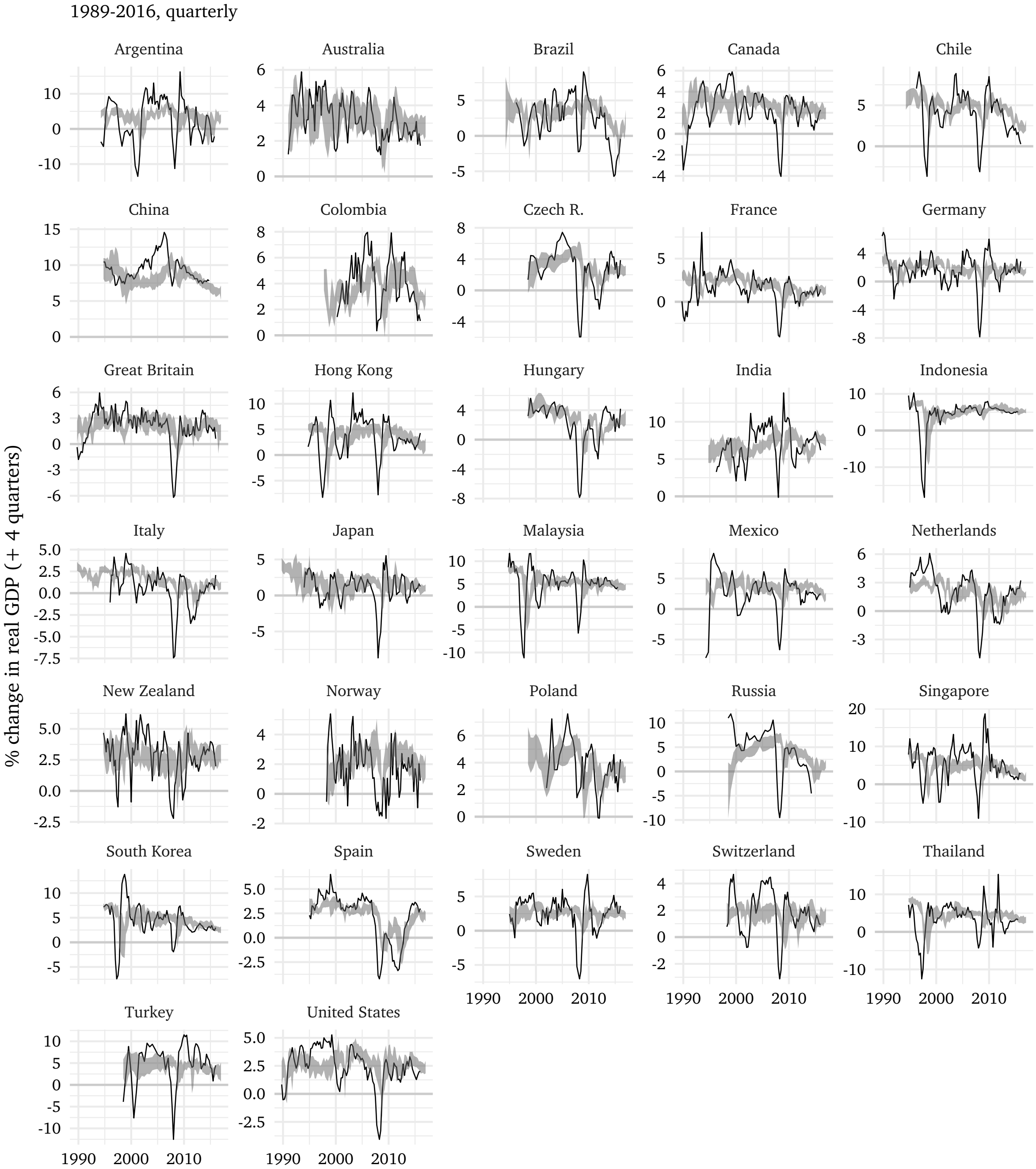
The reason for this difference between the two approaches is that the hypothetical synthetic forecaster in the ad hoc method by Dovern et al. (2012) puts significant weight on the latest information they received. Acknowledging recent changes is different from making a 12 month forecast, a time span in which recent shocks might have subsided. Due to the better theoretical foundation and the provided empirical evidence we choose the procedure by Knüppel and Vladu (2016), but results remain unchanged when applying the method by Dovern et al. (2012).

We only keep forecasts made in the last month in every quarter to be able to compare them against subsequent real GDP growth realizations, which are only available at quarterly frequency. Figure 3 shows forecasts and the subsequent realizations of real GDP growth. The realizations are forward-looking, so the value in the first quarter of 2000 indicates the growth rate of real GDP until the end of the first quarter of 2001. So in the figure, at any point in time we see the four standard error bands (two above and two below) around the consensus forecasts and the ex post true realization. The vertical distance between the two are the forecast errors.

Some of the series (e.g. Argentina) are much more volatile than others (e.g. USA). It is striking how rarely the black line lies within the gray bands, so the GDP growth realizations are more volatile than the forecasts. In particular, forecasts often lag behind the realizations, as if forecasters extrapolated recent realizations. The large recession between 2007 and 2009 surprised forecasters in most countries which led to large positive forecast errors.

[2]The maximum absolute prescribed weight for the current year under our parameterization ($\rho = 0$) is 8 percent, so at a minimum 92 percent of the constructed forecast comes from next year's forecast. Figure A1 plots the weights for the two methods.

Figure 3: GDP forecasts and realizations



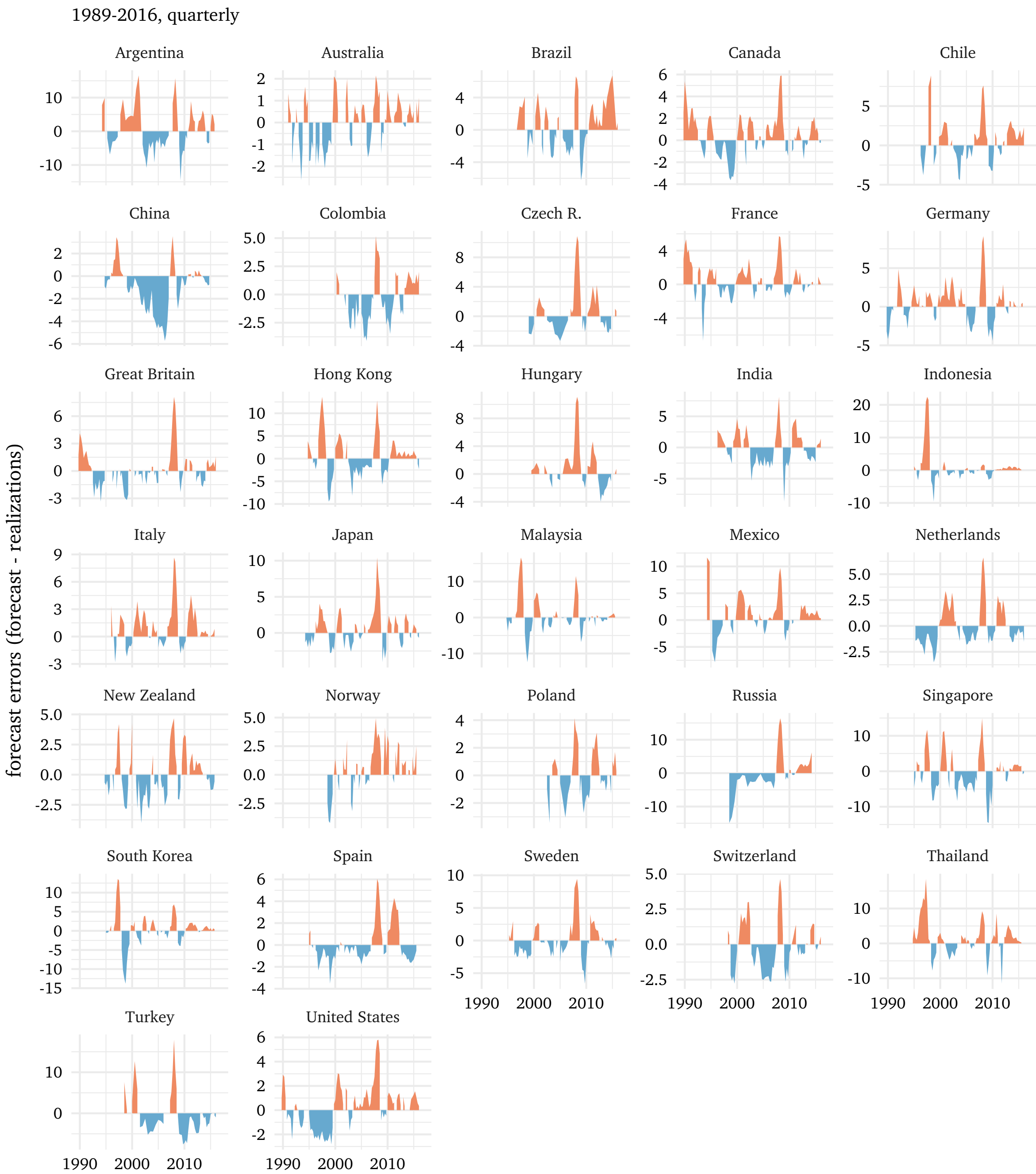
Note: Shaded areas show 2 standard deviation bands around mean forecasts. Black lines are realizations. Both forecasts and growth rates are forward-looking for 12 months ahead.

4 Results

4.1 Forecast errors

Figure 4 plots the the forecast errors, calculated as the consensus (mean) forecasts minus realizations. There are periods of positive (red) and negative (blue) forecast errors among all countries. Forecast errors are particularly high when large recessions strike, a result in line with McNees (1992). Magnitudes are much larger in some countries (Argentina, Hong Kong, Malaysia, Russia, Singapore, South Korea, Thailand and Turkey) and this is mostly driven by

Figure 4: Forecast errors



Note: Positive values (red) show real GDP growth (4 quarters ahead) mean (consensus) forecasts larger than realizations. Vice versa for negative values (blue).

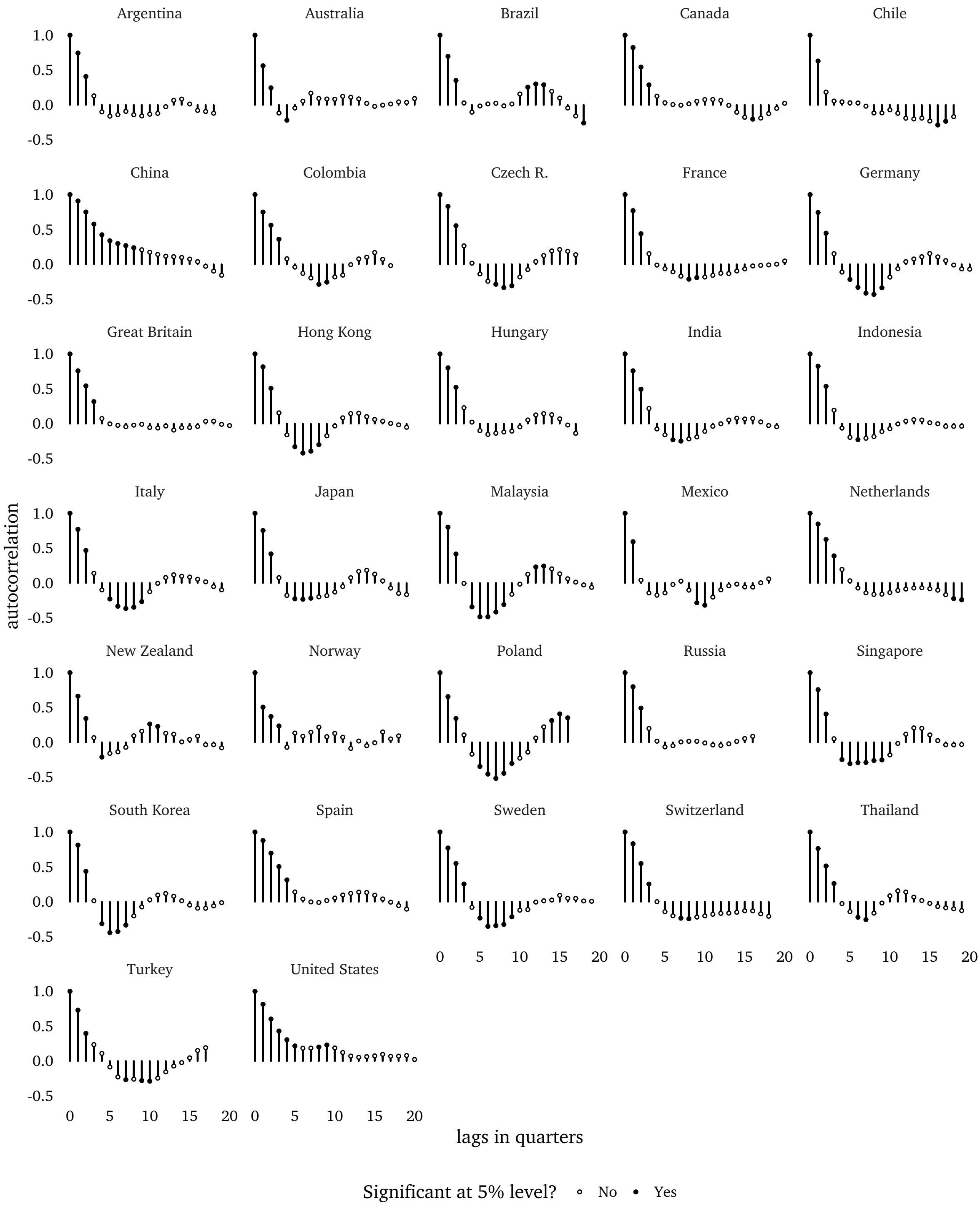
much higher macroeconomic volatility in developing countries.

To analyze the persistence in the forecast errors, Figure 5 shows the autocorrelations of forecast errors. The errors are significantly positively autocorrelated within one year (lag 1 to 4) for most countries. For some countries there is a significant reversal towards a negative autocorrelation after about two years (lag 8).[3]

These persistent errors raise the question of how well the hypothetical consensus (mean)

[3]As Kučinskas and Peters (2018) explain, the existence of autocorrelation in forecast errors alone is a strong sign of a bias in expectation formation. We will explore this further in Section 5.2.

Figure 5: Autocorrelations of forecast errors



forecaster for each country is calibrated.[4] As reported in Table A1 and A2, errors are significantly positive at the 95% level for Canada, France, Italy, Japan, Mexico and Thailand, so in these countries forecasters overestimated output growth on average. China is a special case as well: Forecasters underestimated China's real GDP growth in every quarter between the first quarter of 1999 and the first quarter of 2007.

[4]Figure A2 in the Appendix shows kernel density plots of forecast errors for each of the country in the sample.

Table 2: Baseline regressions

	Dependent variable: $\text{hhd}_{i,t}$ (household debt growth)					
	(1)	(2)	(3)	(4)	(5)	(6)
Forecast errors	0.91**	0.70**	0.71**	0.72**	0.73**	0.70**
	(0.36)	(0.32)	(0.33)	(0.34)	(0.35)	(0.33)
Real GDP gr.		1.22***	1.35***	1.29***	1.37***	1.38***
		(0.31)	(0.31)	(0.30)	(0.34)	(0.35)
Exp. inflation			0.61***	0.89***	0.89***	0.91***
			(0.20)	(0.27)	(0.26)	(0.25)
Interest rate				-0.24	-0.26	-0.27
				(0.22)	(0.21)	(0.21)
Uncertainty					2.42	2.28
					(2.49)	(2.38)
Banking crises						2.28
						(4.41)
Observations	2348	2301	2301	2295	2295	2295
R^2	0.18	0.24	0.26	0.26	0.26	0.26

Note: All models include country and time fixed effects. Robust standard errors in parentheses. GDP: real GDP y-o-y growth. Exp. inflation: 12-month ahead expected CPI growth. Exp. inflation: 12-month ahead expected CPI growth. Uncertainty: Standard deviation of forecasts. Banking crises are defined by Laeven and Valencia (2012).*** p<0.01, ** p<0.05, * p<0.1.

Overall, these forecast errors point to extended periods when even professional forecasters were strongly mistaken about aggregate income growth over the coming year. In the following empirical analysis, we examine what else characterized these periods of booms and busts in expectations.

4.2 Comovement with credit cycles

We compare periods of positive or negative expectation errors with cyclical expansions and contractions in lending in the economy. Our proxy for the financial cycle are household and firm debt growth. We graph these variables in Figures A3 and A4 and we can see distinct financial cycles across countries. Some countries experience only one cyclical swing (in household debt growth) in the sample period (Brazil, Czech Republic, Japan, Spain and Sweden), while for others the measure is trending in this period (Indonesia and the Netherlands). When banking crises hit, GDP in many countries contracts strongly, so we observe sharp drops in their credit growth rates. In the empirical analysis, we carefully exclude the possibility that the association we find is driven by these periods

We investigate the contemporaneous comovement between forecast errors and credit growth using the following panel regression,

$$c_{i,t} = \gamma_i + \delta_t + \beta_1 f_{i,t} + X_{i,t}\beta_2 + \varepsilon_{i,t}\,, \tag{1}$$

where $c_{i,t}$ are the credit variables, γ_i is a country fixed effect, δ_t is a time fixed effect, β_1 is the regression coefficient of interest, $f_{i,t}$ are forecast errors, $X_{i,t}$ are controls and $\varepsilon_{i,t}$ is the error term. Credit variables, $c_{i,t}$, are known to be autocorrelated (Drehmann et al., 2018) and we have shown the same for the forecast errors $f_{i,t}$ (Figure 5). We therefore use robust standard errors. For the covariates $X_{i,t}$, we use real GDP growth (backward-looking, over last 12 months), expected inflation (also from the CEF), interest rates and forecast dispersion (standard deviation across panelists) and banking crises dummies by Laeven and Valencia (2012).

Table 2 displays the baseline results. The first column shows the results for the bivariate regression for which the estimated coefficient, $\hat{\beta}_1$, is positive and significant. This means that when professional forecasters were 1 percentage point too optimistic ($f_{it} = 1$), household debt growth was on average 0.91 percentage points higher. This association stays significant when we control for the states of the business cycle, in column (2), the expected ex ante real interest rate in column (3) and (4) and proxies for uncertainty in (5) and dummy variables for banking crises in model version (6).[5]

This establishes the main result: Periods of ex-post excessively optimistic GDP growth expectations also saw expansions in cyclical lending in the economy. In the rest of this section, we explore the heterogeneity in our results by providing separate estimates for different time periods and country subgroups.

We first check whether the observed pattern might be driven by the global financial crisis of 2007-2009. This episode is not classified as a banking crises for all countries, but many countries experienced a strong recession nonetheless. During the crisis, real GDP dropped precipitously for most countries, but forecasters were slow to adapt their expectations (see Figure 3). The resulting forecast error is therefore strongly positive, suggesting along our line of argument that during the financial crisis people were far too optimistic about the path of their future incomes. In fact, we find the opposite (see Table 3); results hold even when we exclude the financial crisis (column (1)-(2)). They are also robust to estimating the model on the data only before the crisis (columns (5)-(6)), but not on data after the crisis (columns (7)-(8)). During the financial crisis that took place between 2007 to 2009 (columns (3)-(4)) the coefficient becomes insignificant which might be due to the lower number of observations.

In Table 4, we report results for country groups. The results for industrialized and developing countries point in the same directions. The greater macroeconomic volatility of developing countries might explain the larger estimates for these countries. When adding additional macroeconomic controls, the results are more pronounced for developing countries. The full model with all covariates is insignificant for the whole sample for industrialized countries, but the association holds before 2007. This is the most relevant period, as it coincides with the build-up of financial imbalances before the financial crisis of 2008. China is a special case as forecasters strongly underestimated its growth performance over several consecutive years. Our results become even stronger when we exclude it in columns (7)-(9).

Strikingly, the relationship between firm debt and forecast errors is mostly insignficant (Tables A3, A4 and A5). We therefore find that expectation errors about aggregate income are contempor-

[5] Adding quarterly dummies does not affect results.

Table 3: Regressions by subperiods

	Dependent variable: hhd$_{i,t}$ (household debt growth)							
	no crisis		2007-2009		-2006		2010-	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Forecast errors	1.12**	0.94**	0.64*	0.13	1.21***	0.93**	0.16	0.077
	(0.45)	(0.41)	(0.35)	(0.55)	(0.41)	(0.37)	(0.26)	(0.26)
Observations	1969	1916	379	379	1188	1144	781	772
R^2	0.18	0.25	0.25	0.32	0.15	0.23	0.12	0.21
Add. controls		✓		✓		✓		✓

Note: All models include country and time fixed effects. Robust standard errors in parentheses. Additional controls are real GDP y-o-y growth, expected inflation, interest rates and forecaster dispersion (uncertainty). *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Regression by country groups

	Dependent variable: hhd$_{i,t}$ (household debt growth)								
	Industrialized			Developing			-China		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Forecast errors	0.49**	0.24	0.58***	1.47**	1.26**	1.54**	0.89**	0.71*	0.93**
	(0.20)	(0.18)	(0.19)	(0.49)	(0.47)	(0.64)	(0.36)	(0.35)	(0.37)
Observations	1588	1567	901	760	728	243	2316	2263	1144
R^2	0.28	0.37	0.21	0.31	0.42	0.45	0.17	0.26	0.23
Countries	18	18	18	14	14	14	31	31	31
Add. controls		✓	✓		✓	✓		✓	✓
< 2007			✓			✓			✓

Note: All models include country and time fixed effects. Robust standard errors in parentheses. A minus indicates excluding countries. Additional controls are real GDP y-o-y growth, expected inflation, interest rates and forecaster dispersion (uncertainty). *** p<0.01, ** p<0.05, * p<0.1.

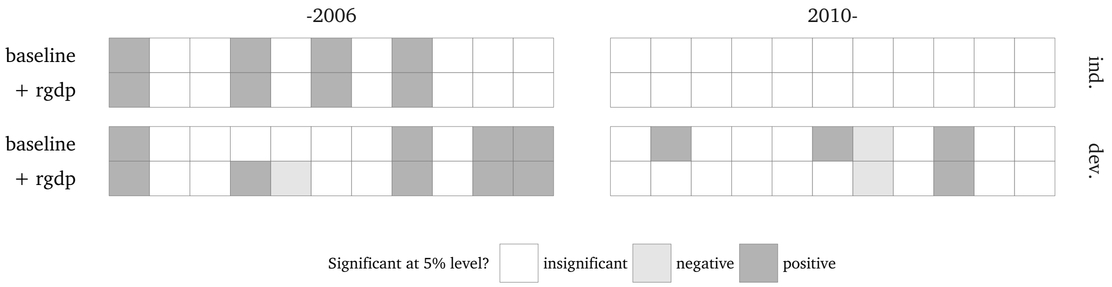
aneously related to the growth rates of household debt, but not to non-financial firm debt. This might be a sign that is the expectations by households (or by banks about households), not expectations by firms (or by banks about firms) that are occasionally misaligned.

4.3 Robustness checks

As explained before, there are different ways of converting *fixed event* to *fixed horizon* forecasts. Results are unchanged when we use the alternative Dovern et al. (2012) weights, as we show in Tables A6 to A11.

Our prefered way of measuring overoptimism is to use forecast errors. One might also define these proxies differently, by taking into account the sign and persistence of errors and comparing them to trend output growth rates. We define eleven alternative ways (see Appendix Section C) to define overoptimism and provide a concise summary of how results change in Figure 6. This plot shows the signs and significance of the $\hat{\beta}$ coefficient in Equation 1. We show results for the basline bivariate regression without covariates, but including time and country fixed effects and using robust standard errors. Secondly, we also control for the state of the business cycle by including current real GDP growth. We split samples into before 2006 and 2010 and into industrialized and developing country groups.

Figure 6: Robustness to alternative overoptimism definitions



Note: Squares indicate the sign and significance of $\hat{\beta}$. The baseline is a bivariate of household debt growth on the variable of interest, controlling for time and country fixed effects and using robust standard errors. Columns are estimates using the alternative overoptimism definitions 1, 2, ..., 11. Left panels show results for the sample up to 2006 and right panes for after 2010. Top panels show results for industrialized countries and bottom panes those for developing countries.

What becomes apparent is that the estimates are either positively significant or insignificant across most specifications. Especially before 2006, many of the alternative overoptimism definitions also return positive estimates for the comovement with household debt growth. This mirrors our baseline findings which were also only significant for the period before 2006. For developing countries, several of the alternative indicators still return positive estimates after 2010. This is also similar to the findings from Table 4.

5 Explaining forecast errors

5.1 Uncertainty of individual forecasters

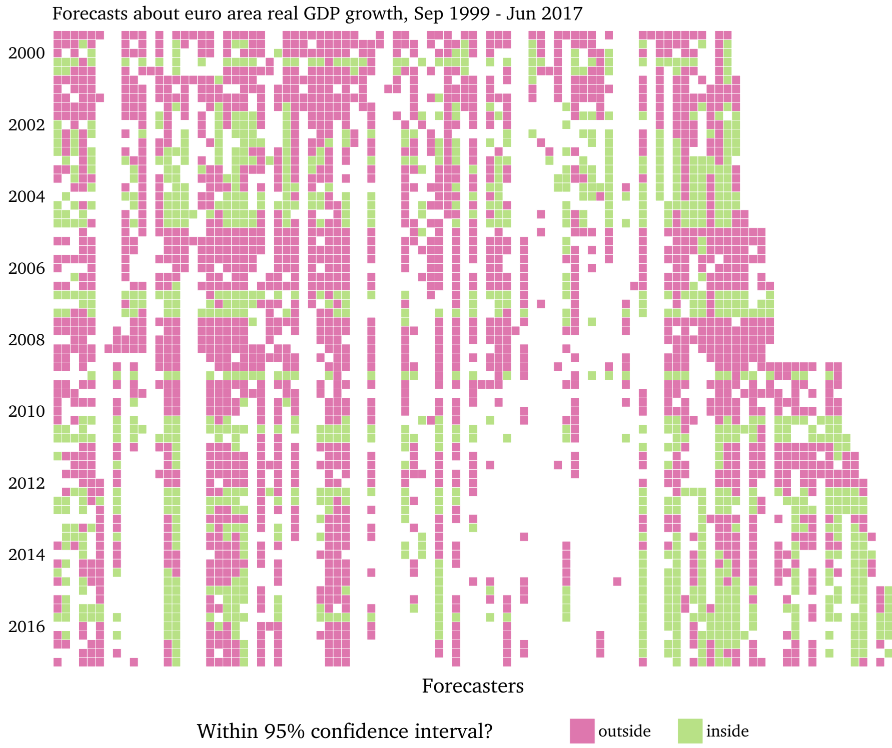
In this paper, we analyze the predictions by professional forecasters. However, every firm only submits point forecasts and they do not report how certain they are about their predictions. We interpret differences between forecasts and subsequent realizations as forecast errors. But how can we do this if we do not know how uncertain forecasters were individually? They might have had very wide distributions of outcomes in mind, but were forced to submit point forecasts.

Our data does not permit us to analyze this further. So we turn to a dataset that does allow us to do so, the *Survey of Professional Forecasters* (SPF) by the European Central Bank.[6] In this survey, participants report a point forecast and subjective probabilities they assign to different bins. This allows us to calculate the width of individual forecasters' confidence bands. Figure A5 shows the mean of this value across forecasters.

On average, forecasters stated that they were 95 percent certain that one-year-ahead real GDP growth would lie in an interval with a width between 1.1 and 2.6 percentage points. However, Figure 7 shows that forecasters were far off with their forecasts. Only 34 percent of

[6]A limitation is that this survey only makes forecasts for the euro area. So what we gain in detail, we loose in generalizability.

Figure 7: Overconfidence in the ECB's *Survey of Professional Forecaster*



Note: Shows whether subsequent realizations of 12 month ahead real euro area GDP growth was within the 95 percent prediction interval of individual forecasters. Columns show the predictions by individual forecasting firms with numbers from 001 to 115. Empty squares show missing data. *Source:* ECB SPF

realizations lie within the 95 percent confidence bands. This immediately tells us that panelists were overconfident in their forecasts. This is puzzling as firms have no incentive to make such narrow predictions. Forecasters are not identified by name in the SPF and there is no scoring of predictive accuracy that might reward more aggressive predictions. Figure A5 displays an upward trend in the width of confidence bands, so participants have become more cautious with their predictions.

Overall, we take thees findings as a sign that panelists in the survey are indeed too confident about their forecasts. While we cannot with certainty extrapolate the findings from the ECB SPF to the CEF, the vast amount of overconfidence in the former strongly suggests that a related mechanism might explain the pronounced and persistent forecast errors that we find for many more countries in the CEF.

5.2 Forecast revisions and information processing

We have documented that positive forecast errors are associated with debt growth in the economy and that positive forecast errors are likely to be a proxy for overoptimism of forecasters. But this begs the question why forecasters become overoptimistic in the first place. An active literature uses forecast *revisions* to analyze how panelists change their forecast when they receive new information. We use the methodology developed by Coibion and Gorodnichenko (2012) and Bordalo, Gennaioli, Ma, and Shleifer (2018) to test how forecasters in our sample react to new information and discuss how this effect varies over the credit cycle.

On the level of individual forecasters, we regress forecast errors on the change in the forecast over the last month:

$$f_{i,k,t} = \beta \, \mathrm{rev}_{i,k,t} + u_{i,k,t} \, . \tag{2}$$

Here, $f_{i,k,t}$ is the forecast error for country i, forecaster k in year t, $\mathrm{rev}_{i,k,t}$ are forecast revisions and $u_{i,k,t}$ is the residual.

Every panelist in the CEF makes two forecasts at the same time, one for the current year and one for the next. We previously aggregated those two fixed horizon forecasts to one fixed event forecasts, but that is not necessary now. Instead, we report separate estimates for the coefficient $\hat{\beta}$ for both forecasts the panelists make.

We also add some indicator variables for the month the forecasts were made in, to control for seasonal trends in information updating. Such patterns are likely, as new information is revealed at fixed points during the year, for example when new GDP forecasts are made public. Also, forecasts mechanically get better as the year progresses. Using microdata on the level of individual panelists shows the strength of the highly detailed dataset we use in this paper. It enables us to also control for the panelist which eliminates panelist idiosyncracies.

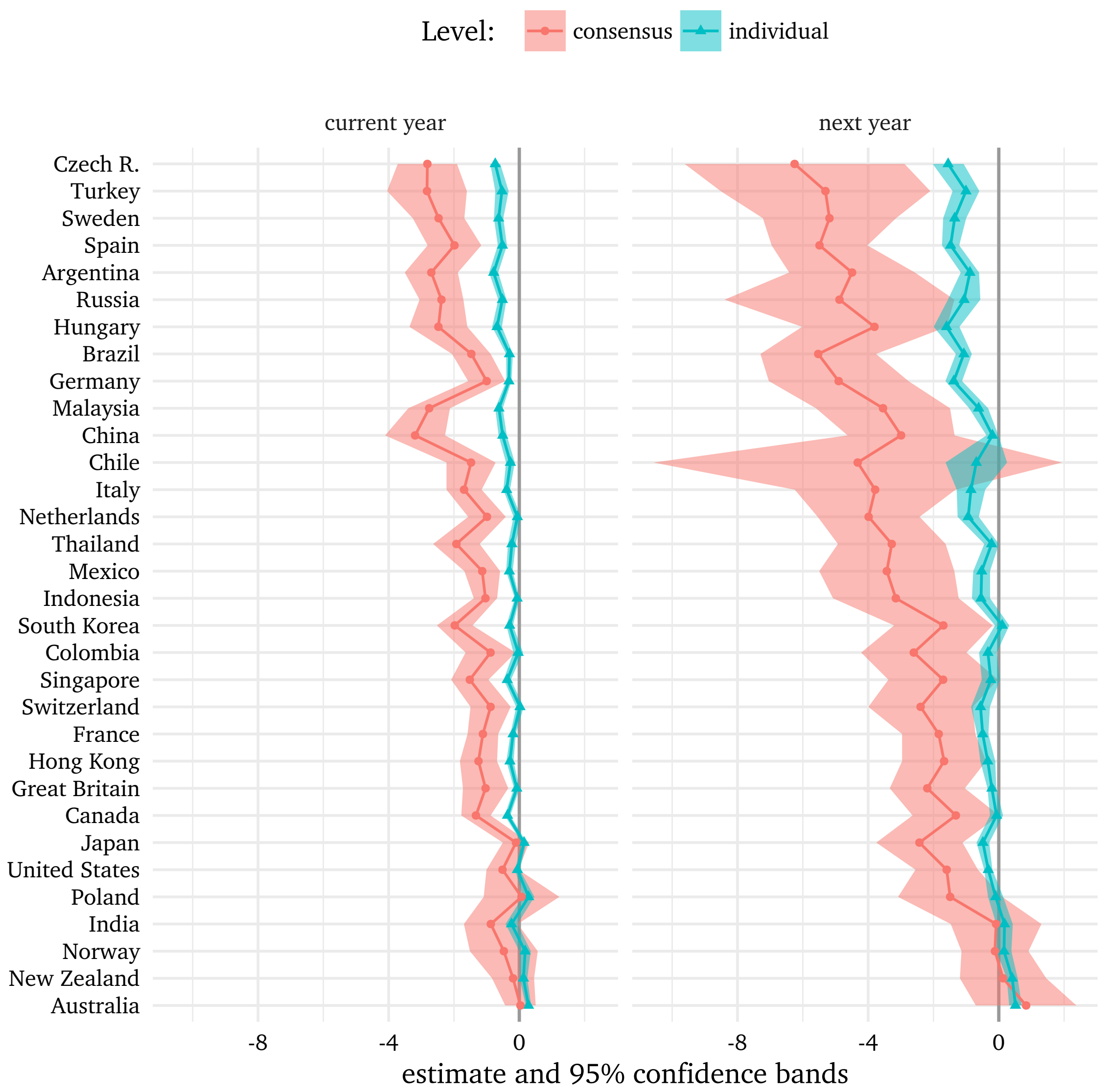
As explained by Coibion and Gorodnichenko (2012) and Bordalo et al. (2018), the coefficient $\hat{\beta}$ shows how forecasters react to new infomation. If panelists updated their forecasts rationally, we would not expect a significant relationship with the forecast error $f_{i,k,t}$. But say they received positive news and updated their forecasts upwards. If they overshot and reacted too strongly to new information, the resulting forecast error would be negative. This means that a negative $\hat{\beta}$ coefficient is symptomatic of *overreaction*. If they instead did not adjust their forecast upwards enough, forecast errors would be positive – a sign of *underreaction*.

This method has previously been used to differentiate between classes of macroeconomic models. Coibion and Gorodnichenko (2012) found evidence for underreaction, but Bordalo et al. (2018) argue that this is due to their use of consensus (mean) forecasts. They show that – at least for the United States – overreaction dominates.

Our results are shown in Figure 8 for each country, split for forecasts for the current and next year and using consensus and individual data. The figure shows the OLS coefficient and 95 percent confidence bands. There is strong evidence for overreaction. This holds for most countries in the sample using consensus or individual data. The results are more pronounced for the forecasts for the next year than for the current year. This is to be expected, as panelists have much more information on the current year and forecasts errors are much smaller (and plausibly also better calibrated) as a result. In contrast to Bordalo et al. (2018) we also find stronger evidence for overreaction when using aggregate data as opposed to using microdata.

However, this association is strongly driven by the inclusion of the financial crisis period 2007 to 2009 during which period there was pronounced overreaction across countries. We also investigate how information processing changes through the credit cycle. For this, we run the same analysis as before, but pool the data from several countries to get a dense dataset that

Figure 8: Information processing

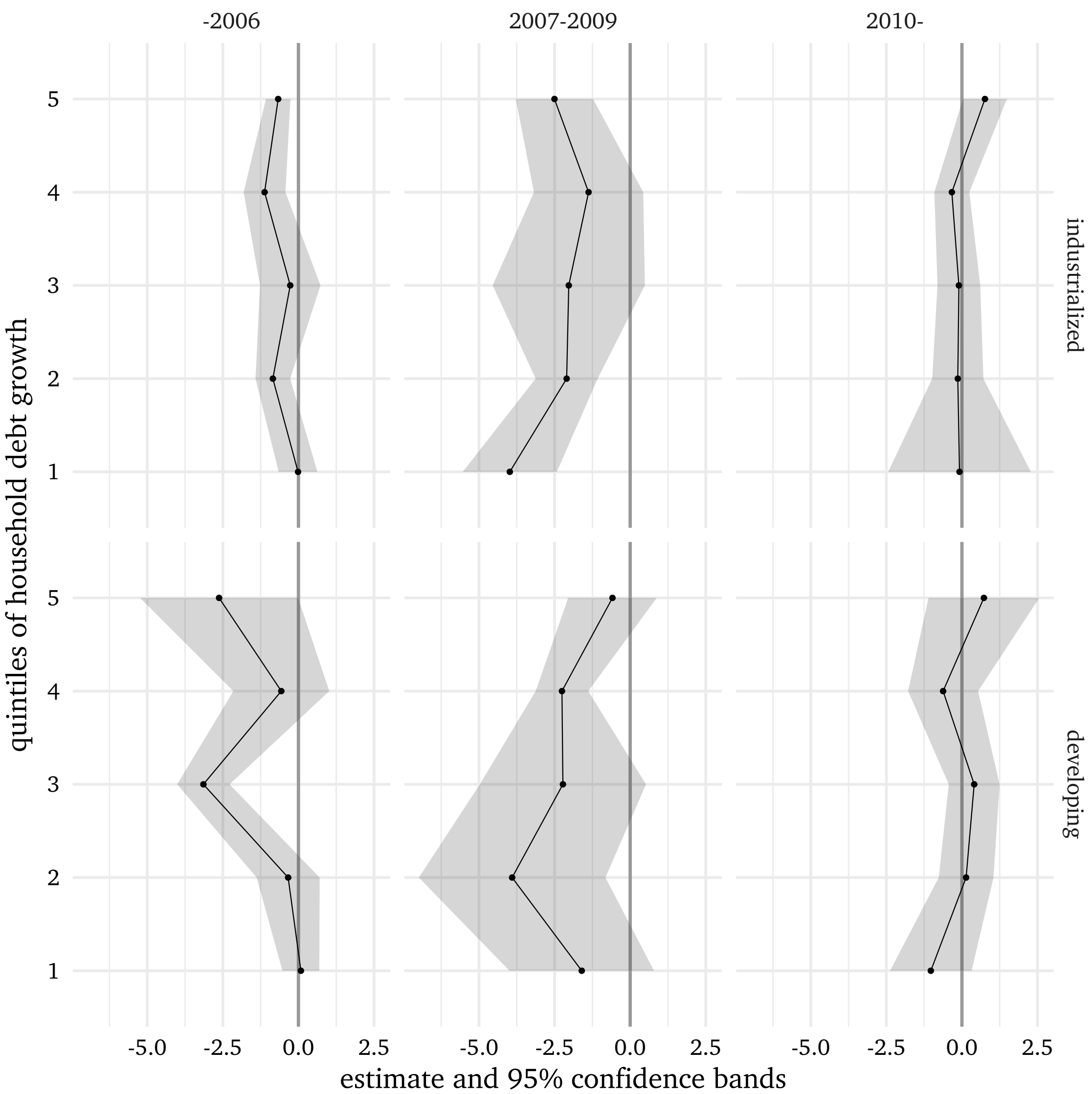


Note: Uses one-month forecast revisions for consensus (mean) and for individual forecasts on 1991-2016 monthly forecasts about annual real GDP growth. Shows separate estimates by country of regressing forecast errors on forecast revisions controlling for forecast month. Also controls for the panelists in the case of panelist-level data.

allows us to display highly detailed results. For this, we run a panel data analysis including country fixed effects. In addition, we partition the dataset depending on a country's position in its quintile of household debt growth. Figure 9 shows these results, separated by the periods before, during and after the financial crisis and split into industrialized and developing country groups.

Across specifications, we find that overreaction and insignificant results outnumber underreaction. Especially up to 2006 there is very robust evidence of overreaction throughout the household debt growth distribution. As mentioned before, the period from 2007 to 2009 was a period of strong overreaction. Most estimates are insignificant from 2010 onwards. This finding helps explain why forecasters - and other agents in the economy - might have become overoptimistic: During boom times of the Great Moderation before the financial crisis people

Figure 9: Information processing over the credit cycle



Note: Uses one-month forecast revisions for consensus (mean) forecasts on 1991-2016 monthly forecasts about annual real GDP growth. Uses next year forecasts. Shows panel estimates including country fixed effects (but not time fixed effects), controlling for forecast month. Sample is split into five bins depending on country-specific household debt growth quintiles.

received positive news and their expectations adjust upwards and overshot. If households form their expectations similarly to professional forecasters, they may have underestimated future risks and therefore took on too much debt in the run-up to the financial crisis.

6 Conclusion

This paper seeks to inform the discussion on the buildup of imbalances in the international financial system. We identify periods of positive and negative mistakes in output growth expectations of professional forecasters and show that these periods are characterized by strong credit growth. While household debt rises in such periods of excessive income expectations, firm debt does not respond. These findings are in line with theories in which biased income-savings decisions drive unsustainable debt booms, with harmful consequences for the economy.

We provide more detailed findings that reveal the psychological mechanisms for the formation of expectation errors. First, panelists (for at least the case of the euro area, where we can be sure) display strong signs of overprecision, so they are too confident about their predictions. Second, panelists overshoot when they receive new information. This second insight emerges from an analysis of forecast revisions using established methods from the literature. This overreaction of forecasts was particularly strong before 2006, when leverage in the financial system was growing.

A limitation of this study is the late start of the time series dimension with our first observations starting in 1989. This means that we miss important swings of the national and global financial cycle and are limited to the end of the "financial hockey stick" (Jordà et al., 2016). On the upside, the dataset used in this study has a broad international coverage of 32 countries, allowing us to control for circumstances that might be specific to individual countries and making it possible to report results separately for industrialized and developing countries. The results reported here hold for both subgroups, which is particularly striking considering the differences in macroeconomic volatilities and financial systems of these groups of countries. Access to the microdata of forecasts also allows us to control for the dispersion across predictions which serves as a proxy for uncertainty and to use forecaster fixed effects for some of our analyses.

A downside of our method is that surveys of professional forecasts might only be correlated weakly with expectations of households or firms. A better way forward would be to collect such expectations across groups of agents and for many countries. Rozsypal and Schlafmann (2017), using consumer surveys, are able to measure expectations across household income distribution. Expanding such an approach across countries is a promising direction for future research.

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A Additional tables

Table A1: Country data sources and summary statistics

	Country	Dev.	Source	First	Last	Obs.	Nr. forec.	CI forec.
1	Argentina	✓	indec	1995 Q2	2015 Q4	71	20 (±5)	[-1.3, 1.9]
2	Australia		oecd	1991 Q1	2016 Q4	104	17 (±2)	[-0.2, 0.2]
3	Brazil	✓	oecd	1996 Q2	2016 Q1	73	18 (±3)	[0.0, 1.3]
4	Canada		oecd	1989 Q4	2016 Q4	109	16 (±2)	**[0.1, 0.7]**
5	Chile	✓	oecd	2003 Q4	2016 Q4	53	18 (±2)	[0.0, 1.3]
6	China	✓	Atlanta Fed[1]	2007 Q1	2014 Q4	32	18 (±3)	[-0.6, 0.3]
7	Colombia	✓	oecd	2000 Q2	2016 Q4	65	13 (±2)	[-0.8, 0.2]
8	Czech R.		oecd	1998 Q3	2016 Q4	57	18 (±2)	[-0.4, 1.2]
9	France		oecd	1989 Q4	2016 Q4	109	19 (±3)	**[0.1, 0.8]**
10	Germany		oecd, destatis	1989 Q4	2016 Q4	109	28 (±2)	[-0.2, 0.7]
11	Great Britain		oecd	1989 Q4	2016 Q4	109	28 (±5)	[-0.3, 0.4]
12	Hong Kong		C&SD[2]	1994 Q4	2016 Q1	86	16 (±2)	[-0.5, 1.4]
13	Hungary	✓	oecd	1998 Q3	2016 Q4	57	17 (±3)	[-0.2, 1.4]
14	India	✓	oecd	2008 Q2	2016 Q4	35	16 (±2)	[-1.0, 0.8]
15	Indonesia	✓	fred, aric[3]	2002 Q4	2015 Q3	52	14 (±2)	[-0.4, 0.2]
16	Italy		oecd	1996 Q1	2016 Q4	84	15 (±2)	**[0.4, 1.3]**
17	Japan		oecd	1989 Q1	2016 Q4	109	20 (±2)	**[0.2, 1.0]**
18	Malaysia	✓	aric[3]	2007 Q1	2015 Q3	35	15 (±2)	[-0.8, 1.5]
19	Mexico	✓	fred	1995 Q4	2016 Q1	71	19 (±3)	**[0.2, 1.4]**
20	Netherlands		oecd	1995 Q1	2016 Q4	88	10 (±2)	[-0.5, 0.3]
21	New Zealand		oecd	1994 Q4	2016 Q4	89	14 (±1)	[-0.6, 0.2]
22	Norway		oecd	1998 Q2	2016 Q4	75	10 (±2)	[0.0, 0.9]
23	Poland	✓	oecd	2002 Q1	2016 Q4	50	19 (±3)	[-0.6, 0.4]
24	Russia	✓	oecd, fred	1999 Q1	2016 Q4	56	17 (±3)	[-0.7, 1.8]
25	Singapore		singstat[4]	1994 Q4	2016 Q1	86	15 (±2)	[-1.6, 0.7]
26	South Korea		oecd	1994 Q4	2016 Q4	89	16 (±2)	[-0.6, 1.1]
27	Spain		oecd	1995 Q1	2016 Q4	88	15 (±2)	[-0.4, 0.3]
28	Sweden		oecd	2000 Q4	2016 Q4	88	14 (±2)	[-0.5, 0.3]
29	Switzerland		oecd	2000 Q4	2016 Q4	65	14 (±2)	[-0.5, 0.3]

Continued.

Table A2: Country data sources and summary statistics

	Country	Dev.	Source	First	Last	Obs.	Nr. forec.	CI forec.
30	Thailand	✓	BoT[5]	1994 Q4	2015 Q3	84	14 (±3)	**[0.3, 2.3]**
31	Turkey	✓	oecd	1998 Q3	2016 Q4	57	15 (±3)	[-2.3, 0.5]
32	USA		oecd	1989 Q4	2016 Q4	109	27 (±3)	[-0.1, 0.5]

Note: Developing country classifications ("Dev.") are according to IMF "World Economic Outlook Report" (2017). "Obs." are the number of quarterly observations with complete data for a respective country. "Nr. forec" are the means of the number of forecasts used to calculate an aggregated mean quarterly forecast with standard errors in parentheses. "CI forec." are the 95% confidence intervals of the forecast errors of 4-quarters ahead real GDP growth (boldface shows significance).

[1]: Higgins and Zha (2015), Atlanta Fed; [2]: Census and Statistics Department

[3]: Asia Regional Integration Center; [4]: Statistics Singapore; [5]: Bank of Thailand

Table A3: Baseline regressions (firm debt)

	Dependent variable: $\text{fd}_{i,t}$ (firm debt growth)					
	(1)	(2)	(3)	(4)	(5)	(6)
Forecast errors	0.12	0.20	0.22	0.22	0.21	0.23
	(0.33)	(0.29)	(0.14)	(0.15)	(0.14)	(0.14)
Real GDP gr.		-0.13	0.17	0.18	0.13	0.13
		(0.43)	(0.36)	(0.34)	(0.33)	(0.34)
Exp. inflation			1.48**	1.42***	1.42***	1.41***
			(0.56)	(0.50)	(0.50)	(0.49)
Interest rate				0.057	0.068	0.072
				(0.13)	(0.13)	(0.13)
Uncertainty					-1.46	-1.38
					(1.66)	(1.67)
Banking crises						-1.24
						(1.52)
Observations	2330	2283	2283	2277	2277	2277
R^2	0.13	0.14	0.30	0.30	0.30	0.30

Note: All models include country and time fixed effects. Robust standard errors in parentheses. GDP: real GDP y-o-y growth. Exp. inflation: 12-month ahead expected CPI growth. Exp. inflation: 12-month ahead expected CPI growth. Uncertainty: Standard deviation of forecasts. Banking crises are defined by Laeven and Valencia (2012).*** p<0.01, ** p<0.05, * p<0.1.

Table A4: Regressions by subperiods (firm debt)

	Dependent variable: $\text{fd}_{i,t}$ (firm debt growth)							
	no crisis		2007-2009		-2006		2010-	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Forecast errors	0.085	0.19	0.32	0.090	-0.016	0.23	0.46**	0.46*
	(0.42)	(0.15)	(0.29)	(0.43)	(0.55)	(0.20)	(0.22)	(0.24)
Observations	1951	1898	379	379	1170	1126	781	772
R^2	0.095	0.28	0.34	0.38	0.090	0.34	0.19	0.21
Add. controls		✓		✓		✓		✓

Note: All models include country and time fixed effects. Robust standard errors in parentheses. Additional controls are real GDP y-o-y growth, expected inflation, interest rates and forecaster dispersion (uncertainty). *** p<0.01, ** p<0.05, * p<0.1.

Table A5: Regression by country groups (firm debt)

	Dependent variable: $\text{fd}_{i,t}$ (firm debt growth)								
	Industrialized			Developing			-China		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Forecast errors	0.41**	0.11	0.17	-0.096	0.25	0.099	0.094	0.18	0.23
	(0.15)	(0.17)	(0.15)	(0.51)	(0.30)	(0.64)	(0.34)	(0.14)	(0.20)
Observations	1570	1549	883	760	728	243	2298	2245	1126
R^2	0.26	0.38	0.35	0.26	0.42	0.53	0.14	0.31	0.34
Countries	18	18	18	14	14	14	31	31	31
Add. controls		✓	✓		✓	✓		✓	✓
< 2007			✓			✓			✓

Note: All models include country and time fixed effects. Robust standard errors in parentheses. A minus indicates excluding countries. Additional controls are real GDP y-o-y growth, expected inflation, interest rates and forecaster dispersion (uncertainty). *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Baseline regressions (alternative weighting)

	Dependent variable: $hhd_{i,t}$ (household debt growth)					
	(1)	(2)	(3)	(4)	(5)	(6)
Forecast errors	1.05***	0.69**	0.72**	0.73**	0.73**	0.70**
	(0.34)	(0.31)	(0.32)	(0.33)	(0.35)	(0.34)
Real GDP gr.		1.11***	1.23***	1.17***	1.19***	1.21***
		(0.30)	(0.29)	(0.27)	(0.30)	(0.31)
Exp. inflation			0.63***	0.90***	0.91***	0.92***
			(0.19)	(0.27)	(0.26)	(0.25)
Interest rate				-0.23	-0.24	-0.25
				(0.23)	(0.21)	(0.21)
Uncertainty					0.76	0.59
					(3.34)	(3.17)
Banking crises						2.28
						(4.43)
Observations	2348	2301	2301	2295	2295	2295
R^2	0.19	0.24	0.26	0.26	0.26	0.26

Note: Uses alternative Dovern et al. (2012) weighting. All models include country and time fixed effects. Robust standard errors in parentheses. GDP: real GDP y-o-y growth. Exp. inflation: 12-month ahead expected CPI growth. Exp. inflation: 12-month ahead expected CPI growth. Uncertainty: Standard deviation of forecasts. Banking crises are defined by Laeven and Valencia (2012).*** p<0.01, ** p<0.05, * p<0.1.

Table A7: Regressions by subperiods (alternative weighting)

	Dependent variable: $hhd_{i,t}$ (household debt growth)							
	no crisis		2007-2009		-2006		2010-	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Forecast errors	1.27***	0.96**	0.56*	-0.022	1.34***	0.90***	0.29	0.098
	(0.42)	(0.40)	(0.30)	(0.46)	(0.40)	(0.32)	(0.20)	(0.25)
Observations	1969	1916	379	379	1188	1144	781	772
R^2	0.19	0.25	0.25	0.31	0.17	0.23	0.13	0.21
Add. controls		✓		✓		✓		✓

Note: Uses alternative Dovern et al. (2012) weighting. All models include country and time fixed effects. Robust standard errors in parentheses. Additional controls are real GDP y-o-y growth, expected inflation, interest rates and forecaster dispersion (uncertainty). *** p<0.01, ** p<0.05, * p<0.1.

Table A8: Regression by country groups (alternative weighting)

	Dependent variable: hhd$_{i,t}$ (household debt growth)								
	Industrialized			Developing			-China		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Forecast errors	0.52***	0.24	0.57***	1.69***	1.28**	1.45**	1.04***	0.72*	0.90***
	(0.18)	(0.18)	(0.18)	(0.45)	(0.49)	(0.53)	(0.35)	(0.36)	(0.32)
Observations	1588	1567	901	760	728	243	2316	2263	1144
R^2	0.28	0.37	0.22	0.33	0.42	0.44	0.19	0.27	0.23
Countries	18	18	18	14	14	14	31	31	31
Add. controls		✓	✓		✓	✓		✓	✓
< 2007			✓			✓			✓

Note: Uses alternative Dovern et al. (2012) weighting. All models include country and time fixed effects. Robust standard errors in parentheses. A minus indicates excluding countries. Additional controls are real GDP y-o-y growth, expected inflation, interest rates and forecaster dispersion (uncertainty). *** p<0.01, ** p<0.05, * p<0.1.

Table A9: Baseline regressions (firm debt, alternative weighting)

	Dependent variable: fd$_{i,t}$ (firm debt growth)					
	(1)	(2)	(3)	(4)	(5)	(6)
Forecast errors	-0.029	0.051	0.12	0.12	0.11	0.12
	(0.44)	(0.37)	(0.20)	(0.20)	(0.19)	(0.19)
Real GDP gr.		-0.12	0.16	0.18	0.14	0.14
		(0.37)	(0.33)	(0.32)	(0.30)	(0.31)
Exp. inflation			1.49**	1.42***	1.41***	1.41***
			(0.55)	(0.50)	(0.49)	(0.49)
Interest rate				0.062	0.072	0.075
				(0.13)	(0.13)	(0.13)
Uncertainty					-1.28	-1.21
					(1.89)	(1.91)
Banking crises						-0.93
						(1.54)
Observations	2330	2283	2283	2277	2277	2277
R^2	0.13	0.14	0.30	0.30	0.30	0.30

Note: Uses alternative Dovern et al. (2012) weighting. All models include country and time fixed effects. Robust standard errors in parentheses. GDP: real GDP y-o-y growth. Exp. inflation: 12-month ahead expected CPI growth. Exp. inflation: 12-month ahead expected CPI growth. Uncertainty: Standard deviation of forecasts. Banking crises are defined by Laeven and Valencia (2012).*** p<0.01, ** p<0.05, * p<0.1.

Table A10: Regressions by subperiods (firm debt, alternative weighting)

	Dependent variable: $\text{fd}_{i,t}$ (firm debt growth)							
	no crisis		2007-2009		-2006		2010-	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Forecast errors	-0.14	0.036	0.34	0.15	-0.35	0.030	0.48**	0.47*
	(0.54)	(0.21)	(0.27)	(0.42)	(0.74)	(0.28)	(0.22)	(0.23)
Observations	1951	1898	379	379	1170	1126	781	772
R^2	0.096	0.28	0.34	0.38	0.098	0.34	0.19	0.21
Add. controls		✓		✓		✓		✓

Note: Uses alternative Dovern et al. (2012) weighting. All models include country and time fixed effects. Robust standard errors in parentheses. Additional controls are real GDP y-o-y growth, expected inflation, interest rates and forecaster dispersion (uncertainty). *** p<0.01, ** p<0.05, * p<0.1.

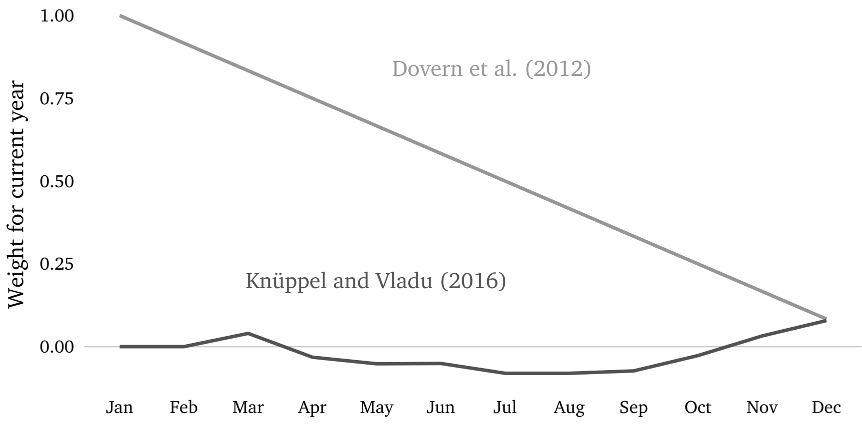
Table A11: Regression by country groups (firm debt, alternative weighting)

	Dependent variable: $\text{fd}_{i,t}$ (firm debt growth)								
	Industrialized			Developing			-China		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Forecast errors	0.43**	0.094	0.12	-0.40	0.062	-0.31	-0.062	0.071	0.030
	(0.16)	(0.19)	(0.16)	(0.66)	(0.36)	(0.75)	(0.44)	(0.19)	(0.28)
Observations	1570	1549	883	760	728	243	2298	2245	1126
R^2	0.26	0.38	0.36	0.26	0.42	0.53	0.14	0.31	0.34
Countries	18	18	18	14	14	14	31	31	31
Add. controls		✓	✓		✓	✓		✓	✓
< 2007			✓			✓			✓

Note: Uses alternative Dovern et al. (2012) weighting. All models include country and time fixed effects. Robust standard errors in parentheses. A minus indicates excluding countries. Additional controls are real GDP y-o-y growth, expected inflation, interest rates and forecaster dispersion (uncertainty). *** p<0.01, ** p<0.05, * p<0.1.

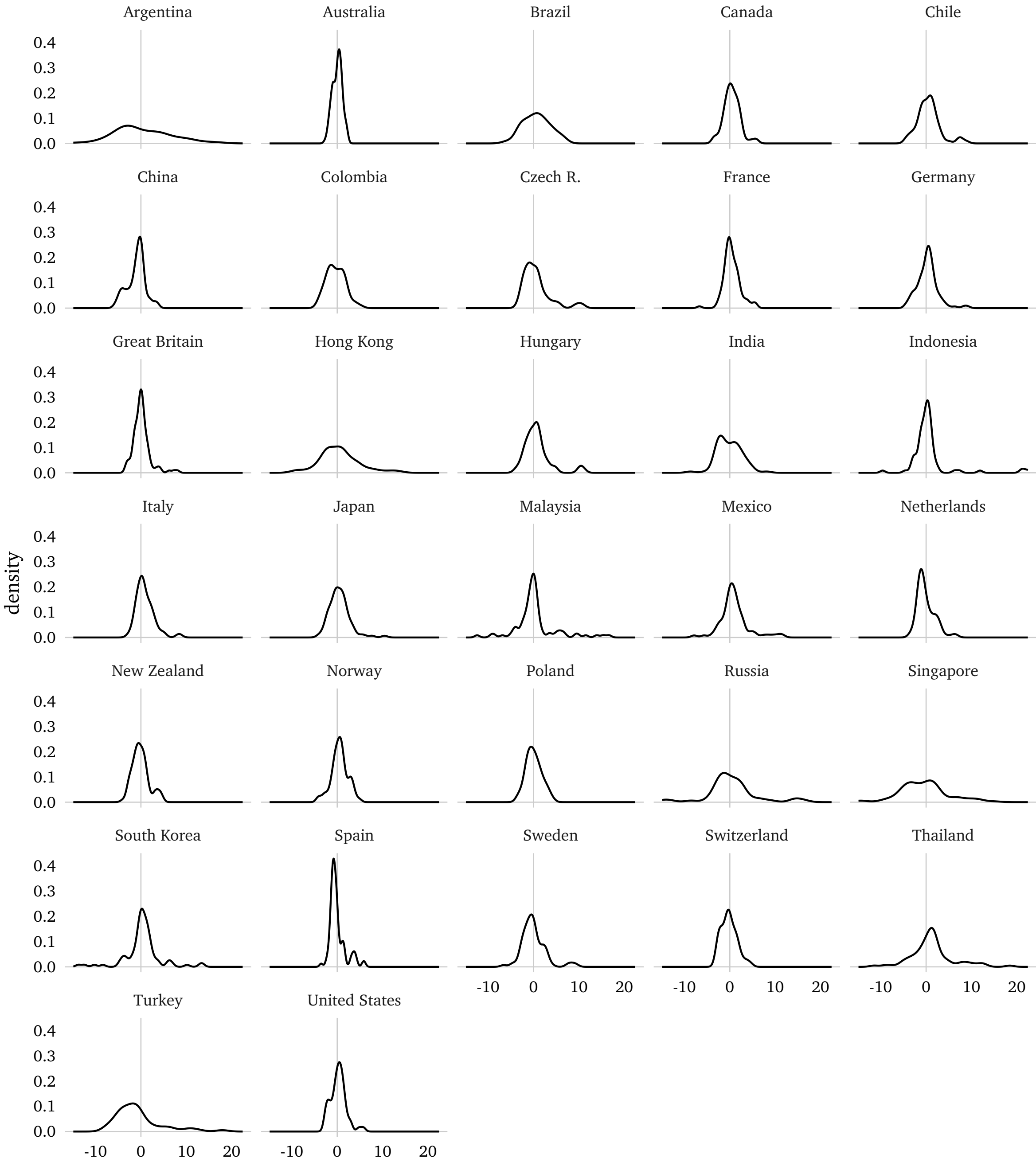
B Additional figures

Figure A1: Weights to convert fixed event to fixed horizon forecasts



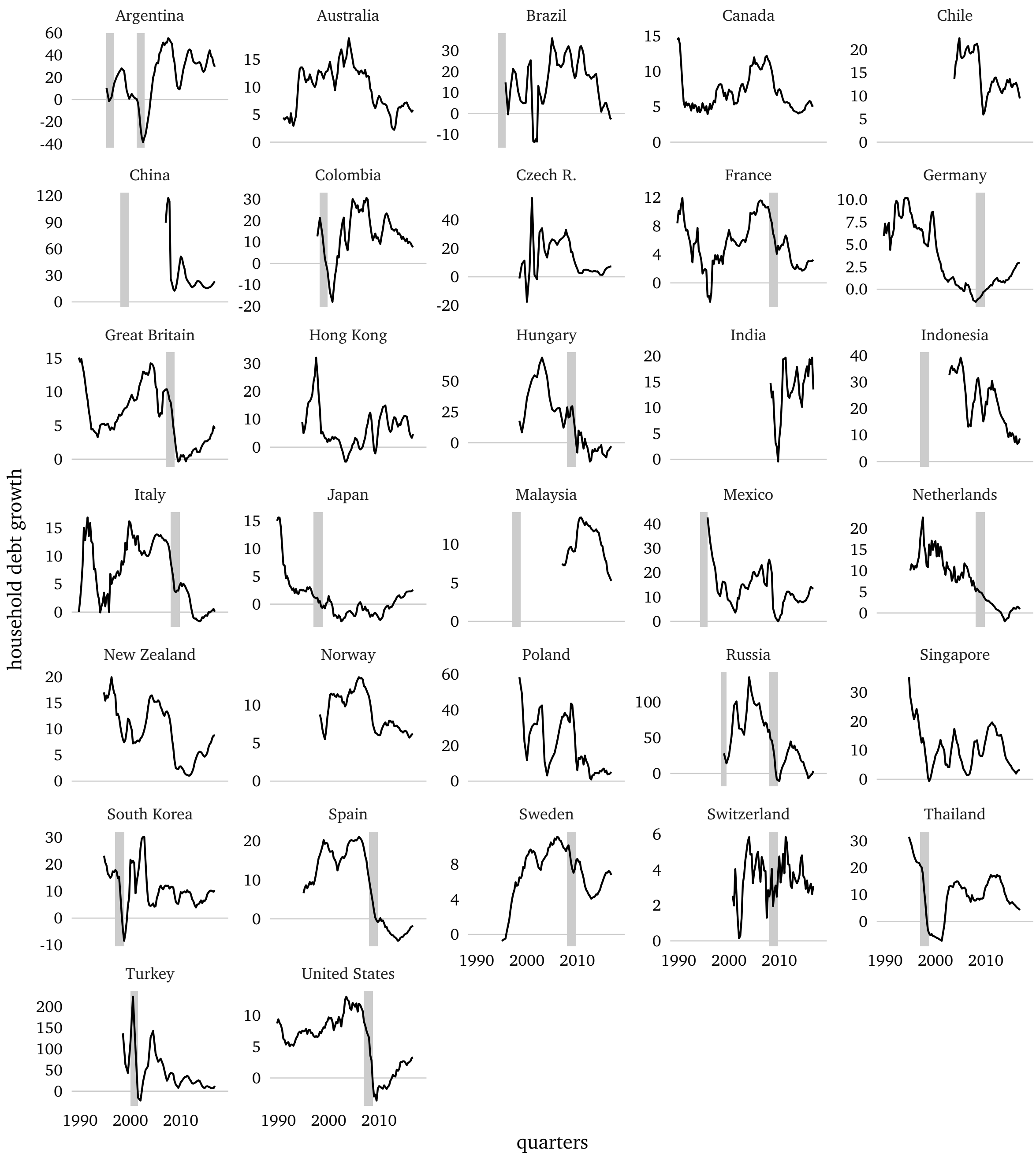
Note: The weight put on the forecast for the subsequent year is one minus the weight for the current year. The Knüppel and Vladu (2016) values are shown for $\rho = 0$.

Figure A2: Distribution of forecast errors



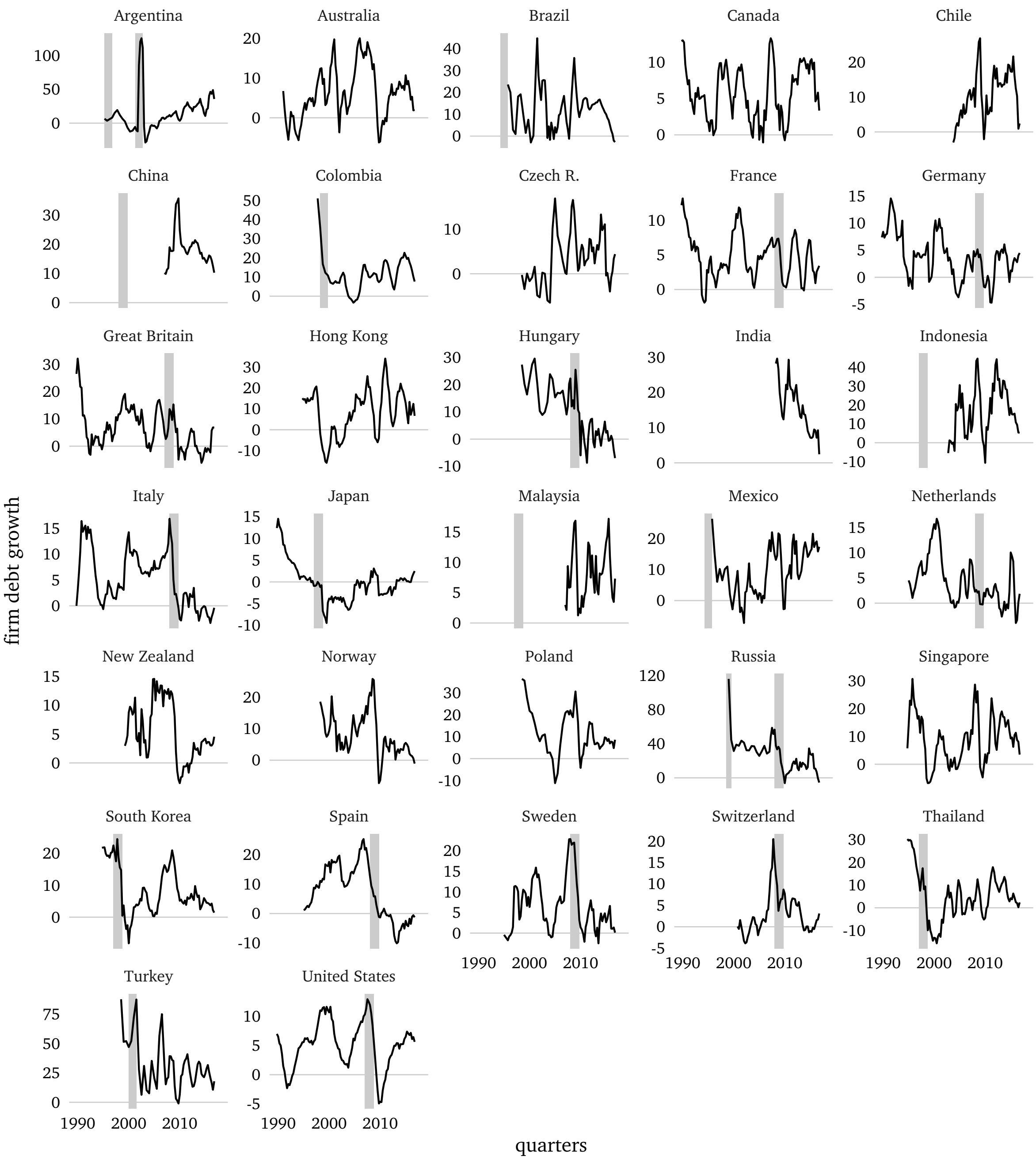
Note: Kernel density plots of the distribution of forecast errors across countries. Positive values show real GDP growth (4 quarters ahead) mean (consensus) forecasts larger than realizations.

Figure A3: Household debt growth



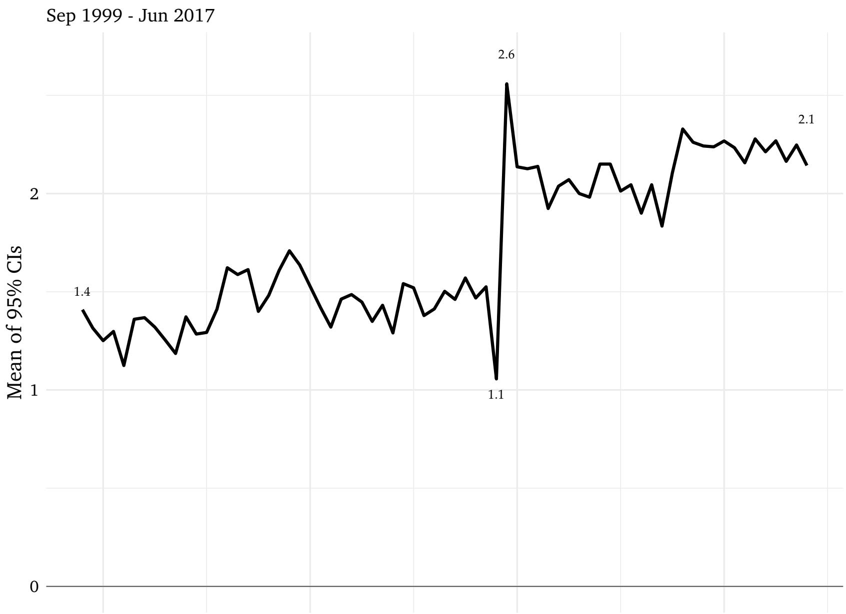
Note: Backward-looking y-o-y growth rates in credit to households and non-profit organizations serving households. Bars indicate banking crises as classified by Laeven and Valencia (2012). *Source:* BIS and own calculations.

Figure A4: Firm debt growth



Note: Backward-looking y-o-y growth rates in credit to non-financial (private and public) firms. Bars indicate banking crises as classified by Laeven and Valencia (2012). *Source:* BIS and own calculations.

Figure A5: Individual uncertainty



Source: ECB SPF

C Alternative overoptimism definitions

In Section 4.3 explains how results change when we use different definitions for overoptimism. These are the following:

1. Forecast errors as raw values. *(Continous variable)*
2. Dummies for periods with positive forecast errors. *(Categorical variable)*
3. Uses only positive forecast errors scaled by magnitude of forecast error. Assigns missing values if forecast error is negative. *(Continous variable)*
4. Same as 3., but assigns zero if forecast error is negative. *(Continous variable)*
5. Dummies if both realizations and forecast errors are positive. *(Categorical variable)*
6. Same as 5., but uses absolute magnitude of forecast error in those periods. Assigns zero instead. *(Continous variable)*
7. Uses forecast errors in periods where forecast errors are positive for at least three continuous periods. Assigns missing values if condition is not met. *(Continous variable)*
8. Same as 7., but assigns zeros if condition is not met. *(Continous variable)*
9. Realizations and forecast errors are positive for at least three continuous periods. *(Categorical variable)*
10. Dummy periods where forecast errors are positive and consensus forecasts are above the long-run real GDP trend. *(Categorical variable)*
11. Scales forecasts by the share of individual forecasts per year that are above the long-run real GDP growth trend. *(Continous variable)*

The long-run growth trends in versions 10. and 11. are taken from the *Penn World Tables 9.0* and are estimated as ten-year moving averages, interpolated to quarterly frequencies.