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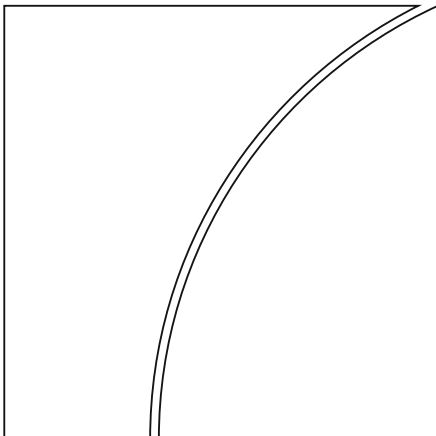
## No 878

### Which credit gap is better at predicting financial crises? A comparison of univariate filters

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Monetary and Economic Department

August 2020



JEL classification: E44, G01.

Keywords: early warning indicators; credit gaps; HP filter; linear projection.

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# Which credit gap is better at predicting financial crises?

## A comparison of univariate filters

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### Abstract

The credit gap, defined as the deviation of the credit-to-GDP ratio from a one-sided HP-filtered trend, is a useful indicator for predicting financial crises. Basel III therefore suggests that policymakers use it as part of their countercyclical capital buffer frameworks. Hamilton (2018), however, argues that you should never use an HP filter as it results in spurious dynamics, has end-point problems and its typical implementation is at odds with its statistical foundations. Instead he proposes the use of linear projections. Some have also criticised the normalisation by GDP, since gaps will be negatively correlated with output. We agree with these criticisms. Yet, in the absence of clear theoretical foundations, *all* proposed gaps are but indicators. It is therefore an empirical question which measure performs best as an early warning indicator for crises. We run a horse race using expanding samples on quarterly data from 1970 to 2017 for 41 economies. We find that credit gaps based on linear projections in real time perform poorly when based on country-by-country estimation, and are subject to their own end-point problem. But when we estimate as a panel, and impose the same coefficients on all economies, linear projections perform marginally better than the baseline credit-to-GDP gap, with somewhat larger improvements concentrated in the post-2000 period and for emerging market economies. The practical relevance of the improvement is limited, though. Over a ten year horizon policy makers could expect one less wrong call on average.

Keywords: early warning indicators; credit gaps; HP filter; linear projection

JEL classifications: E44, G01

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## 1. Introduction

Excessive credit growth has long been recognised as integral to financial booms and busts (Minsky, 1982; Kindleberger, 2000). However, what constitutes growth being “excessive” remains undefined. Borio and Lowe (2002) propose a credit-to-GDP gap measured by the deviations of the credit-to-GDP ratio from a one-sided Hodrick-Prescott (HP) filter with a large smoothing parameter (400 000 for quarterly data). Borio and Drehmann (2009), Drehmann et al (2010) and Drehmann et al (2012) revisit the gap in light of the crisis and do extensive comparisons of its early warning indicator (EWI) properties for systemic banking crises with other variables. They identify the credit-to-GDP gap as the best single EWI across those that they examine. Their work underpins the choice of the Basel Committee for Banking Supervision to single out the credit-to-GDP gap as a useful guide for setting countercyclical capital buffers (BCBS, 2010a).

But the credit-to-GDP gap is only one possible indicator of excessive credit growth.<sup>2</sup> Following the work of Jordà et al (2011), for example, the academic literature has mainly relied on medium-term growth rates in credit-to-GDP. In addition, the HP-based gap has been challenged on conceptual grounds. We address two such challenges here.

Most importantly, many have criticised the use of the HP filter to derive the gap. It has long been known that the HP filter has serious problems. These are succinctly summarised by Hamilton (2018). In particular, the HP filter results in spurious dynamics that are not found in the underlying data, results in filtered data with properties that differ between the middle and ends of the sample, and its typical implementation is at odds with its statistical foundations.<sup>3</sup> Hamilton therefore concludes that you

<sup>2</sup> Since Borio and Lowe (2002), BIS authors have always been careful in emphasising this point (for an overview see Drehmann and Tsatsaronis, 2014). This is also one reason why there is no mechanical link between the credit gap and the countercyclical capital buffer under the Basel III rules (BCBS, 2010a).

<sup>3</sup> Relatedly, Edge and Meisenzahl (2011) document a large difference between real-time and full-sample estimates of credit-to-GDP gaps due to the end-point problem of the HP filter.

should never use the HP filter for any purpose, including for deriving credit-to-GDP gaps. He proposes the use of linear projections as an alternative to derive deviations from trends.

In addition, some authors have criticised the use of GDP to normalise the level of credit in the economy. For instance, Repullo and Saurina (2011) point out that the credit-to-GDP gap will tend to be negatively correlated with GDP, and its use could exacerbate the procyclicality of macroprudential policy. Similar problems were highlighted by the Basel Committee (BCBS, 2010a). Real-credit-per-capita has been proposed as an alternative measure to overcome this potential drawback.

From a conceptual perspective we agree with these criticisms. But, in the absence of clear theoretical foundations, *any* proposed gap measure is nothing more than an indicator, including when derived with more sophisticated empirical methods.<sup>4</sup> What should matter to policymakers is the relative performance of different possible measures, which can be assessed empirically.

In this paper, we therefore run a horse race between different proxies for excessive credit. Given that excessive credit is unobservable, we assess performance based on how well different credit gaps predict systemic banking crises. Performance is judged by the “area under the curve” (AUC), a summary measure of its predictive power. And we focus on (quasi) real time information, which is the relevant case for policymakers who can only use the information they have available at each point in time to predict a crisis.<sup>5</sup>

To keep the analysis concise, we split it into two parts. First, we compare different possible formulations of the linear projection, given the lack of exploratory work elsewhere. We examine a wide range of different combinations of lags in the underlying regression, and also consider projections based on equations estimated both economy-by-economy and in a panel, where the coefficients on the lags are constrained to be the same for all economies.

<sup>4</sup> For instance Buncic and Melecky (2014) and Juselius and Drehmann (2020) derive credit gaps based on a multivariate VARs.

<sup>5</sup> We shorten “quasi-real time” to “real time” for the remainder of the paper. Our real time estimates use only data up to time  $t$  to estimate gaps at time  $t$ , with the sample expanding with each observation. But the data we use are those available at the time of estimation, rather than those available at time  $t$ , and hence are not truly “real time”.

In a second step, we select the best performing of these linear projections and compare it against two alternative means of deriving “gaps”: the HP trend, and 20-quarter changes in credit. For each of these measures, we consider two means of normalising credit, either by using nominal GDP or by transforming it into real-credit-per-capita terms.

The key finding for the different ways to derive projection gaps is that it is crucial to estimate the underlying linear equation as a panel instead of running economy-by-economy regressions. The panel approach results in a material improvement in performance across many forecast horizons and sub-sample specifications. The analysis also points to a potential “end-point” problem of the linear projection gap, especially for small samples. If we compare the forecast performance of the full sample versus the real time gaps, the performance of the real time gaps in the economy-by-economy specification is much weaker. The reason is that during a credit boom, for example in the early 2000s in the US, the estimated coefficients increase in real time so that the residuals that the projection gap is based on don’t increase sharply, and are hence less likely to signal the impending crisis.

The panel helps to alleviate this end-point problem. More generally, it points to the benefits of including international data in the assessment of credit gaps for individual economies: perhaps because of the relative rarity of financial crises, there are material gains in using the experiences of other economies to calibrate and assess early warning indicators.

For practical purposes, it is also interesting to note that different lags structures in the linear projections have a limited effect on crisis prediction performance, provided the included lags are sufficiently long (generally 20 or more quarters).

When we compare the alternative approaches to generate gaps, two findings stand out. First, normalising by GDP results in superior forecast performance over normalising by the population. Second, while the estimated projection gap generally has the highest AUC, differences in performance relative to a gap derived by an HP filter or 20 quarter changes tend to be quantitatively small, albeit in

many cases statistically significant. Larger differences are only found for the post-2000 period and for emerging market economies.

But despite the statistical results, differences between different methods to derive gaps (at least when normalised by GDP) are not meaningful from a practical perspective. Dealing with the inherent uncertainty in identifying credit booms is more important by an order of magnitude. Across the different specification, and independent of the gap method, around 30% of signals are incorrect. And the higher AUC of the projection gap relative to an HP filtered gap results in issuing 2-3 percentage points less incorrect signals in normal times. If policy makers would mechanically follow these gaps this would imply that, over a 10 year period, they could expect that the indicators would give wrong signals for around three years, independent of the gap they chose. Over the same period, the 2-3 percentage points difference of fewer wrong calls in normal times for the projection GDP gap relative to the HP GDP gap amounts to making the right call in just one additional quarter.

Addressing the underlying uncertainty about predicting crises, rather than the choice between these indicators, is therefore the key challenge. One possible source of improvement is to take a broader range of indicators into account. We do not do so in this paper, since we wish to focus on the debate about different methods to derive credit gaps as one fundamental component in early warning indicator models, in light of its importance in the Basel III framework.<sup>6</sup> Therefore, alternative methods, including those focused on multi-variate measures, are beyond the scope of this paper.<sup>7</sup> But even in

<sup>6</sup> The Basel III framework recognises that the so credit-to-GDP gap can only a starting point of discussions about countercyclical capital buffers as authorities should consider all available information (BCBS, 2010a).

<sup>7</sup> Multivariate measures have been shown to have the potential to improve forecast performance, starting with Borio and Lowe (2002). Band-pass filters have been used in both univariate (Aikman et al, 2015) and multi-variate (Drehmann et al, 2011) contexts. Galati et al (2016) extract a financial cycle using a multivariate unobserved components model on the credit-to-GDP ratio, total credit and house prices for six economies, and find that the resulting medium-term cycles vary in terms of length and amplitude across countries and over time. Rünstler and Vlekke (2018) conduct a similar exercise and identify medium-term cycles in credit volumes that are linked to GDP performance at longer frequencies than business cycles. Other recent examples include Schüler et al (2015), who find that a financial cycle based on the common frequencies of credit and asset prices outperforms the credit-to-GDP gap in predicting systemic banking crises at horizons of one-to-three years, Aldasoro et al (2018), who show that combining various indicators of excessive debt with property prices can help to improve financial crisis prediction, Alessi and Detken (2018), who use “random forest” machine learning methods based on a number of economic and financial indicators and find that this out-performs a logit model based on the same explanatory variables in terms of out-of-sample performance, and Lang et al. (2019), who develop a combined indicator that captures risks stemming from domestic credit, real estate markets, asset prices, and external imbalances and that outperforms univariate early warning indicators.

these cases, indicators provide incorrect signals, requiring judgement in practice and the recognition that policy making based on these indicators is fraught with uncertainty.

In the next section, we outline the two challenges to the HP credit gap measure that we examine. Section 3 contains our methodology for comparing the different measures in light of the objective, and Section 4 introduces the data. Section 5 provides a detailed analysis of the performance of different linear-projection based gaps, and Section 6 compares the best of these against alternative measures. Robustness exercises are discussed in Section 7. In Section 8, we consider the practical implications of the differences before we conclude.

## 2. Critiques of the baseline credit gap

Our baseline credit gap was proposed by Borio and Lowe (2002). They suggested measuring the credit gap as deviations of the credit-to-GDP ratio from a one-sided Hodrick-Prescott (HP) filter with a large smoothing parameter (400 000 for quarterly data). This measure has been subject to a number of criticisms. Here we outline two prominent ones: namely that the normalisation is problematic, and the HP filter has undesirable properties.

In order to turn the nominal level of credit into a magnitude that is comparable both across time and across countries, it must be normalised in some manner. In our baseline measure, the normalisation is to divide nominal credit by nominal GDP. Repullo and Saurina (2011) suggest that this could be problematic, since it would suggest reducing capital requirements when GDP growth is high and increasing them when GDP growth is low, hence exacerbating the pro-cyclicality of regulations related to bank capital.<sup>8</sup> As discussed, this was already identified as a potential problem by the Basel Committee (2010a), which identified it as one of the reasons why policymakers' judgement is necessary

<sup>8</sup> Also see the discussion in Jordà (2011).



when setting the countercyclical capital buffer. Jordà et al (2016) and Richter et al (2017) use real-credit-per-capita as their measure of normalised credit instead.

The other key component to measuring a credit gap is the definition of the gap – or, equivalently, defining the trend against which credit will be compared.

Following the original work by Borio and Lowe (2002), the long-term trend of the credit-to-GDP ratio is often calculated by means of a one-sided (ie real time) HP filter. The filter is run in quasi real time, ie recursively, with an expanding sample each period. Thus, a trend calculated for, say, end-1998 only takes account of information up to 1998 even if this calculation is done in 2018. The HP filter also uses a much larger smoothing parameter – 400 000 for quarterly data – than the one employed in the business cycle literature. This choice can be rationalised by the observation that credit cycles are on average about four times longer than standard business cycles and crises tend to occur once every 20-25 years (Drehmann et al, 2010).<sup>9</sup>

Hamilton (2018) points out some serious potential shortcomings with the HP filter in general, in particular that:

1. it produces spurious dynamics that are not based on the underlying data-generating process;
2. the dynamics at the ends of the sample differ from those in the middle;<sup>10</sup> and
3. the standard implementation of the HP filter stands at stark odds from its statistical foundations.

<sup>9</sup> Hodrick and Prescott (1997) set  $\lambda$  equal to 1600 for extracting business cycles in quarterly data. Ravn and Uhlig (2002) show that, for series of other frequencies (daily, annual etc), it is consistent to set  $\lambda$  equal to 1600 multiplied by the fourth power of the observation frequency ratio, implying  $\lambda$  equal to 400 000 if credit cycles are four times longer than business cycles. Empirically,  $\lambda$  equal to 400 000 also delivers the credit-to-GDP gap with the best forecasting performance (Drehmann et al, 2011).

<sup>10</sup> The baseline credit gaps measure uses a one-sided filter, with observations added recursively. On the one hand, this means that we are never comparing an observation from the middle of the sample with one from the end, mitigating the second critique. On the other hand, given that the gaps are taken from samples of different sizes, their properties could still vary.

To avoid these drawbacks, Hamilton suggests an alternative using a “linear projection” based on estimating the equation:

$$y_{t+h} = \beta_0 + \sum_{j=1}^J \beta_j y_{t+j-1} + v_{t+h}. \quad (1)$$

The estimated residual from this equation,  $v_{t+h}$ , is the projection gap that will be assessed as a predictor of financial crises. Richter et al (2017) implement this method, but with one alteration: they normalise the residuals by their standard deviation,  $(\sigma_v)$ , to produce the projection gap.

Hamilton suggests that including four lags ( $J = 4$ ) and a value of  $h$  corresponding to five years (ie  $h=20$  with quarterly data) for applications to debt (or credit) cycles may be appropriate. But given that the baseline HP-filter based credit-to-GDP gap has already been carefully tested with different assumptions about the smoothing parameter (see discussion above), we first examine a range of possible formulations of the linear projection model, with varying numbers and lengths of lags, to see how sensitive the results are. We also compare the results when we estimate the underlying equation economy-by-economy vs in a panel with the  $\beta_j$ 's constrained to be the same for all economies, while allowing separate fixed effects ( $\beta_0$ 's) for each economy.

An alternative approach that we also examine is to detrend by computing growth rates. Taking the 20-quarter change in credit/GDP or real-credit-per-capita provides a filter-free way of extracting a credit gap measure. This approach has been used, for example, in Jordà et al (2011, 2017).

In the following section, we outline the methodology to assess predictive performance that we use for the horse race between different measures of the credit gap to see how they compare.

### 3. Assessing predictive performance

As discussed in the introduction, all proposed gaps are intended to be indicators of excessive credit growth. In line with a long research tradition, we judge performance by how well the different measures predict systemic banking crises.

We follow the literature and use the area under the ROC curve (AUC) as a statistical measure to judge forecast performance.<sup>11</sup> It is a very intuitive measure. To fix ideas, assume a very simple economy that is in one of two states:  $S = 0$ , or  $S = 1$ . States are not directly observable but a gap measure,  $G$ , carries imperfect information about the current state. In particular, the higher the value of  $G$ , the more likely it is that  $S = 1$ . In an ideal situation, there would be a threshold  $\theta^i$  such that, if  $G > \theta^i$ , we would know that  $S = 1$  (and  $S = 0$  for  $G \leq \theta^i$ ). But, if the signal is noisy, there is a trade-off between the rate of true-positives,  $TPR[S(\theta^i)] = P(G > \theta^i | S = 1)$ , and the rate of false positives,  $FPR[S(\theta^i)] = P(G > \theta^i | S = 0)$ .<sup>12</sup> For very low values of the threshold, for instance, the TPR will be close to one, but the same will also hold for FPR. We therefore look over all thresholds  $\theta^i$ . And the mapping from FPR to TPR for all  $\theta^i$  gives the ROC curve.

The area under this curve, the AUC, can be interpreted as the likelihood that the distribution of  $G$  when  $S = 1$  is stochastically larger than when  $S = 0$ . It is a convenient and interpretable summary measure of the signalling quality. A completely uninformative indicator has an AUC of 0.5. Correspondingly, the AUC for the perfect indicator equals 1. The AUC of an informative indicator falls in between and is statistically different from 0.5. For two competing indicators,  $G_1$  and  $G_2$ , it is also easy to test whether  $AUC(G_1)$  is equal to  $AUC(G_2)$  by using a Wald test.

<sup>11</sup> ROC stands for receiver operating characteristic. The somewhat awkward name goes back to its original use of trying to differentiate noise from signals of radars during World War II. Since then it has been used in many other sciences (eg Swets and Pickett, 1982). Over the last 10 years it has become increasingly popular in the context of crises or recession predictions, following in particular the work of Oscar Jorda (eg Berge and Jorda, 2011, or Jorda et al, 2011).

<sup>12</sup> The FPR and the complement of the TPR correspond to the familiar Type II and Type I errors.

We estimate the AUC non-parametrically with Stata. Standard errors are bootstrapped using 1000 replications. We cluster at the country level. The Wald test for equality of AUCs also uses the joint bootstrap estimated variance-covariance matrix. As such we account for the very high correlation between different gap measures, often in the range of 0.9.<sup>13</sup>

For practical policy purposes, in addition to statistical power to predict crises, the right timing and stability of signals are important (Drehmann and Juselius, 2014). EWIs need to signal a crisis early enough so that policy actions can be implemented in time to be effective. Yet, EWIs should not signal crises too early as there are costs to macroprudential policies, and early adoption could undermine the support for necessary policy measures (eg Caruana, 2010). EWIs should also be stable, as policy makers tend to base their decisions on trends rather than reacting to changes immediately (eg Bernanke, 2004). A gradual implementation of policy measures may also allow policy makers to influence market expectations more efficiently, and to deal with uncertainties in the transmission mechanism (CGFS, 2012).

To assess the appropriate timing of a gap measure  $G_i$ , we follow Drehmann and Juselius (2014) and compute  $AUC(G_{ij})$  for all horizons  $j$  within a three year window before a crisis, ie  $j$  runs from -12 to -1 quarters.<sup>14</sup> When we compute  $AUC(G_{ij})$ , we ignore signals in all other quarters than  $j$  in the window. For example, at horizon -6, the rate of correctly predicted crises is solely determined by signals issued 6 quarters before crises. False alarms, on the other hand, are based on all signals issued outside the three year window before crises occur. We also do not consider signals issued during a crisis, as binary EWIs become biased if the crises periods are included in the analysis (Bussiere and Fratzscher, 2006).

<sup>13</sup> The high correlations are unsurprising given that all gaps are based on credit either normalised by GDP or population.

<sup>14</sup> By looking at each horizon separately, we wish to draw attention to the temporal stability of the EWIs, which is important for policymaking, rather than to the average time pattern.

## 4. Data

Our data covers 41 economies.<sup>15</sup> We use quarterly data with samples from as early as 1970 (depending on data availability) to derive the trend. The sample ends in the third quarter of 2017.

In our baseline specification to test forecast performance we only include gaps for an economy once we have 15 years of quarterly data, leading to an earliest date of 1985q1 in the horse race.<sup>16</sup> This is necessary to ensure adequate data for the calculation of trends with the HP filter or regression coefficients with the linear projections.<sup>17</sup> This starting point also approximately coincides with when many countries liberalised their financial systems, which in turn affected the dynamics of financial cycles and their relation with financial crises (Borio, 2014). For a small number of economies we further delay their inclusion in the panel until an end of a crisis: there is little practical point in beginning to test for crises when an economy is already in one.

Our measure of credit is as published in the BIS database of total credit to the private non-financial sector (see Dembiermont et al, 2013), capturing total borrowing from all domestic and foreign sources. Our nominal GDP series used to generate credit-to-GDP are drawn from national sources. To generate the capita gaps, we use CPI from national sources and population numbers from the IMF and the World Bank.

In total we have 27 crises in our sample. For crisis dating, we rely on the new European Systemic Risk Board crisis data set (Lo Duca et al, 2017) for European countries and on Drehmann et al (2010)

<sup>15</sup> The sample includes Argentina, Austria, Australia, Belgium, Brazil, Canada, Chile, China, Colombia, the Czech Republic, Denmark, Finland, France, Germany, Greece, Hong Kong SAR, Hungary, India, Indonesia, Ireland, Italy, Japan, Korea, Malaysia, Mexico, the Netherlands, Norway, the Philippines, Poland, Portugal, Russia, Saudi Arabia, Singapore, Spain, South Africa, Sweden, Switzerland, Thailand, Turkey, the United Kingdom and the United States.

<sup>16</sup> For 20 economies in our sample, data are available from 1970q1 so they are included from the start in 1985. By 2000q1, 28 economies are included in the sample.

<sup>17</sup> As a robustness check we also used a run-in period of only 10 years with qualitatively similar results. However, we prefer the 15 year specification as 10 years of data for country-specific projections are rather limited. On this basis, one could even argue for a longer sample such as 20 years to estimate stable trends. By doing so, the crises of the late 80s would, however, drop out of the horse race severely limiting the number of observed crises episodes.

for the rest.<sup>18</sup> As discussed, we drop post-crisis periods as identified in Lo Duca et al (2017) and Laeven and Valencia (2012) for European and non-European economies, respectively.

## 5. A horse race between linear projection gaps

Our first exercise is to compare different linear projection gaps in order to get a sense of which performs best. We started with a broad set of options, with  $h$  ranging from 4 to 36 quarters, and 1-8 lags included in the equation. In all cases, we considered two normalisations of credit, namely by GDP (ie the credit-to-GDP ratio, with both credit and GDP measured in nominal terms) and per capita (that is, nominal credit divided by the product of the level of the CPI and the population). These different normalisations are indicated by "GDP" and "capita" respectively.

When using real credit per capita, we face a scaling issue. The reason is that real credit per capita is measured in units of local currency, normalised by the CPI and population. National currencies have, however, very different units, as indicated by simple dollar exchange rates ranging from below one to multiples of thousands. While the growth gap method is invariant to scaling, this is not the case for the HP gap or the projection gap. To overcome the scaling problem for the per capita normalisations, we take natural logs of normalised credit. The gap measure may then be interpreted as the percentage difference between the level and the underlying trend.

We also perform the estimation both economy-by-economy and as a panel with economy fixed effects but with other coefficients constrained to be identical across all economies. We do this recursively, adding one quarterly observation at a time to an expanding sample. With each recursion we take the final residual as a measure of the credit gap in that period. This approach is consistent with

<sup>18</sup> We exclude crises related to transitioning economies or that were imported from abroad based on Lo Duca et al (2017). In addition, we classify the crisis in 2008 in Switzerland as imported.

the idea that we require a measure that is useful in real time; in the same way, our HP-filter results will be based on a one-sided filter.

As well as adding observations with each recursion, we also add economies as data becomes available. For comparability between panel and economy-specific estimation, we only include an economy in the panel once we have 15 years of data for reasons discussed above.

Consistent with the intuition of Hamilton (2018), linear projections based on low values of  $h$  do not perform well.<sup>19</sup> In addition, performance generally drops off with additional lags. We hence report a range of results for  $h \in \{20, 24, 28, 32, 36\}$ , each with one, two and four lags. Combined with two different normalisations and both economy-by-economy and panel estimation, we are comparing the AUCs of 60 different formulations of linear projections for each of 12 different horizons.

The key takeaways are summarised in Graph 1.<sup>20</sup> For each panel, the solid line in Graph 1 represents the AUC at different horizons, up to 12 quarters. Symmetric dotted lines indicate 95% confidence bands around the point estimates. In addition, the dot-dash black lines indicate the results for our, ultimately, preferred specification for the projections gaps, based on the panel estimates, normalised by GDP and with lags 28 and 29.

To highlight difference across the specifications, we add yellow diamonds and green dots. They are defined as:

- Yellow diamonds: Highest AUC across all of the 60 specifications at that given forecast horizon;
- Green dots: AUC is not statistically different from the highest AUC at a 95% confidence level, based on bootstrapped critical values using 1000 replications.

Comparing the results, two results are evident from Graph 1:

<sup>19</sup> Full results are available on request.

<sup>20</sup> The graphs and tables of underlying data for all 60 different formulations are shown in the online annex (Graph OA1 and Table OA2).

1. Normalising by GDP statistically dominates normalising by population.
2. The panel estimation dominates estimation by each economy separately.

We therefore focus on cases where credit is normalised by GDP, and the linear model is estimated as a panel.

Reading across all different specifications in the online appendix (Graph OA1), it is also clear that:

3. The lag length and choice of  $h$  make little difference for the predictive performance of the different projection gaps, at least when  $h$  is between five and nine years.

This is not surprising as the different gaps share very similar cyclical properties, with an average cycle length of 16 years.<sup>21</sup> Given that we judge performance by the AUC, we ultimately chose the specification with the highest AUC on average. As such we focus on the model with  $h = 28$  and two lags as our preferred linear projection model for the remainder of the paper.<sup>22</sup> However, as a robustness check, we will also assess the original specification for the projection gap suggested by Hamilton (2018) later.

To further uncover the sensitivity of the linear projection gap's performance to modelling assumptions, Graph 2 reports the AUCs for the projection gaps based on real time information vs over the full sample; estimated economy-by-economy (labelled "separate") vs as a panel; and normalised by GDP vs population. All panels in Graph 2 are based on our preferred projection specification.

In line with the results above, it is clear that normalising by GDP generally generates higher AUCs than normalising by the population, especially when estimating in real time. The improvement based on full sample averages 0.06, whereas for real time it is a larger 0.11.

<sup>21</sup> Cyclical properties, based on a turning point analysis, are presented in Table OA1 in the online annex for all different gaps discussed in the paper. The average cycle length of 16 years is similar to that of the baseline HP filtered credit-to-GDP gap. It also in line with the financial cycle literature (eg Drehmann et al, 2012; Claessens et al, 2012; and Aikman et al, 2015).

<sup>22</sup> The average AUC for including only one lag of 28 quarters is marginally higher at the fourth decimal (the average AUC for using lags 28 and 29 or only lag 28 are 0.803 to three decimal places). We prefer two lags, mindful of the original justification of Hamilton (2018) for proposing four lags for the linear projection:  $d$  lags should in principle work with any process up to order  $I(d)$ .



The graph also highlights that estimating using a panel instead of on each country individually makes little difference when applied to the full sample (fourth row of graph versus second; the average AUC declines by 0.02) but dramatically improves performance when applied to expanding samples (first versus third rows; the average improvement in AUC is 0.16).

This points to a potential “end-point” problem of the linear projection gap, especially for small samples. The reason is that during a credit boom, for example in the early 2000s in the US, the estimated coefficients increase in real time so that the residuals that the projection gap is based on don’t increase sharply, and are hence less likely to signal the impending crisis (see graph OA2 in the online annex).

Finally, using the full sample rather than expanding sample regressions generally improves AUCs, although this is not a practical option for policymakers seeking to construct an EWI. Comparing analogous panels between rows one and two, and also rows three and four, full-sample estimation dramatically improves the AUC when the normalisation is by population (by an average of 0.15) or the estimation is economy-by-economy (by 0.19) or both (by 0.26). By contrast, the difference is trivially negative when panel estimation is applied to credit normalised by GDP (-0.01).

These results suggest caution in interpreting some implementations of the linear projection. For example, Richter et al (2017) use the linear projection-based gap in country-by-country estimation on the full sample based on credit normalised by population with  $h = 20$  and four lags. In the context of our Graph 2, their results are closest to the second panel on the right column. However, if the objective is to assess the usefulness of measures of the credit gap to policymakers, the real time results are the relevant ones to focus on. These are given in the top-right panel. The point AUCs here are less than 0.5 at some horizons and never statistically significantly different from an uninformative indicator, indicating that this implementation of the linear projection has no statistical power for predicting crises in real time in our panel.

## 6. Widening the field

Given our preferred linear projection model, we now compare it against alternatives. We consider six gaps, as summarised in Table 1. As in the previous section, we focus on two different normalisations of credit, namely by GDP and per capita. For each ratio, we apply three possible gap measures:

- 1) the difference from a one-sided HP filtered credit with a smoothing parameter of 400 000 (the HP gap);
- 2) 20 quarter (five year) growth rates (the growth gap); and
- 3) the residual from real time linear projections with  $h = 28$  and 2 lags (the projection gap).

As before, we include the gaps for a country for each of the measures once we have 15 years of underlying credit data for the country.<sup>23</sup>

Graph 3 presents the main results (Table OA3 in the online annex shows the underlying statistics). Panels in the left-hand column are based on credit-to-GDP ratios, and the right-hand column on real-credit-per-capita. The top row shows the HP gaps, the middle row the growth gaps and the bottom row the projection gaps. For each panel, the solid line represents the AUC at different horizons, up to 12 quarters. Symmetric dotted lines indicate 95% confidence bands around the point estimates.

The graph summarises the key take-always from the horse race:

First, normalising by GDP results in superior forecast performance over normalising by the population. This holds across all methods to derive the gaps and for most forecast horizons.

Second, the panel projection GDP gap has the highest AUCs of all the different gap measures for all horizons, although the differences vis-à-vis the AUCs of the HP GDP gap are very small and never statistically significant. This is in stark contrast to the comparative performance of the same model when applied economy-by-economy as we have shown in an earlier version of this paper (Drehmann

<sup>23</sup> Graphs of the underlying credit gap data, by economy, are available in the online annex (Graph OA3).

and Yetman, 2018). Then, the HP gap consistently outperforms the projection gap. The forecast performance of the growth GDP gap is also not much worse, albeit with some significant differences to the projection gap.

Third, while the differences are sometimes statistically significant, they are generally not large from a policy perspective. The average AUC differences across horizons between the best performer and the other gaps are less than 0.04 for both the HP GDP gap and the growth GDP gap and 0.06 for the projection capita gap.

## 7. Robustness checks

As robustness checks, we consider splitting the sample in three different ways: by time, between advanced and EM economies and between countries that experienced a (domestically driven) crisis during the GFC and those that did not. We also compare the result with those obtained using the original specification for the projection gap suggested by Hamilton (2018) for credit gap calculations, based on lags 20-23 (instead of 28-29). To preserve space, we only show the gaps normalised by GDP; full versions of the graphs and the underlying data are reported in the online appendix.

For the first exercise, we split the sample at the end of 2000. The results are reported in Graph 4. They illustrate the key role that later periods play in the strong performance of the projection GDP gap. This measure no longer has the highest AUC at the longest horizons for the early sample split, although it is never statistically significantly different from the best performer. However, in the later subsample the projection GDP gap is the best performing EWI at all horizons, and the difference is always statistically significant.

We next compare advanced and emerging market economies in Graph 5. For the advanced economies there is little to choose between any of the measures statistically at most horizons. By contrast, for EMEs AUC performance is lower and more dispersed and confidence bands are much wider, suggesting that crisis prediction is inherently harder in EMEs.

Results also seem not to be driven by the GFC: they are very similar for economies that had a domestically driven crisis during the GFC and those that did not (Graph 6).

Finally, we compare the results with those based on projection gap parameters originally suggested by Hamilton (2018), using lags 20-23, to see how sensitive our results could be to the risk of overfitting of the projection equation. The results are displayed in Graph 7. While there are differences between this specification and the one using lags 28-29, these are quantitatively small and the projection GDP gaps continues to perform best, by a small margin, at all horizons.

These results support our main takeaways above, that the projection GDP gap is the best performing EWI overall in our sample, but differences are sometimes small, and sample dependent.

## 8. Practical implications

The analysis so far has several important practical implications for deriving indicators that signal “excessive” credit growth.

The core takeaway from our first set of results is that, when deriving projection gaps, it is crucial to use a panel approach rather than running country-by-country regressions.<sup>24</sup> It is also important to assess predictive performance by using real-time estimates, as this is what policy makers can do in practice and results can differ significantly from a full sample analysis. The question of lag length, on the other hand, is second order as long as  $h$  is between five and nine years.

Across all the results it also stands out that normalising credit by GDP results in superior forecast performance than normalising by the population.

Our analysis is, however, less clear cut on the best approach to derive gaps. The statistical results (Graphs 3-7) suggest that the projection GDP gap is marginally better than the HP GDP gap, which in

<sup>24</sup> While our analysis is clear that a panel approach is important, the optimal panel of countries may differ for specific practical purposes.

turn somewhat outperforms the growth GDP gap. But despite the statistically significant differences in forecast performance between the different gaps, they are not meaningful from a practical perspective. The main uncertainty policy makers face is that indicators give wrong signals: they may miss crises or may issue wrong crises calls in calm times.

To illustrate this, we take the HP and the projection gaps and undertake a simplified analysis where we do not look at 12 different forecast horizons but instead differentiate between no forthcoming crisis (labelled "normal") and pre-crisis periods. The pre-crisis periods are the 12 quarters in the run-up to crises. As before, we drop the observations during actual crises. In this analysis, the AUC of the projection GDP gap (0.80) is higher than the AUC of the HP GDP gap (0.77) but the difference is not statistically significant at the 5% level. We then pick, for each of the GDP gaps, one particular threshold which, if breached, is seen as a crisis signal. This threshold is the one with the lowest noise-to-signal ratio that signals at least a 66% probability of a crisis in the pre-crisis periods.<sup>25</sup> The identified thresholds are 6.0 for the HP GDP gap and 14.9 for the projection GDP gap.

To highlight the real time uncertainty, Table 2 shows the fraction of correct and incorrect signals in the normal and pre-crisis (ie in the 12 quarters before crisis) periods for the full sample and also the average across the robustness checks run in the previous section. Numbers in italics show the fraction of correct/incorrect signals for the individual indicators, while the other numbers provide the percentage of observations where both signals are giving the same or different messages.

The results have important implications from a policy perspective. First, independent of the indicator, around 30% of signals are wrong. Second, there is disagreement between the indicators in around 10% of the cases. Third, both indicators perform exactly equal in pre-crisis periods. Fourth, the

<sup>25</sup> This assumes that policy makers are more worried about missing crises than false alarms, and follows some of our earlier work (eg Borio and Drehmann, 2009; and Drehmann et al, 2011). However, the exact specification is arbitrary and many different approaches are possible and sophisticated policy analysis often uses a range of different rule for robustness (see eg Alessi et al, 2014).

projection gap performs marginally better in normal times by issuing 2-3 percentage points less wrong calls.

If policy makers would mechanically follow this rule this would imply that, over a 10 year period, they could expect that the indicators would give wrong signals for around three years with either of the two gaps. Over the same period, the 2-3 percentage points difference of fewer wrong calls in normal times for the projection GDP gap relative to the HP GDP gap amounts to a single quarter. As such, dealing with the inherent uncertainty in identifying credit booms is an order of magnitude more important in practice than the choice between the different credit-to-GDP gaps.<sup>26</sup> Note, however, that despite this inherent uncertainty, all these gaps perform better than simple coin tosses. Thus, using them to calibrate prudential policies improves welfare, the more so if we consider the high typical costs of systemic crisis –100% of GDP or more (eg Fender and Lewrick, 2016; BCBS, 2010b).

## 9. Conclusions

The credit gap, defined as the deviation of the credit-to-GDP ratio from a one-sided HP-filtered trend with a smoothing parameter of 400 000 (for quarterly data), has been suggested as a useful measure for predicting crises. Two criticisms levelled at this measure are that i) the normalisation may be problematic because of the positive correlation between credit and GDP, and ii) the HP filter has undesirable properties.

In this paper, we examine alternative measures of the credit gap that have been advocated by others to address these concerns.

We find that credit gaps based on linear projections in real time perform poorly in real time when based on country-by-country estimation. But when we estimate as a panel, and impose the same coefficients on all economies, linear projections perform marginally better than the baseline credit-to-

<sup>26</sup> This also holds true if we add the growth GDP gap into the comparison.

GDP gap, with larger improvements concentrated in the post-2000 period and for emerging market economies, although the differences across the measures are often statistically small. The improvement in performance between the linear projection using a panel instead of applied to individual economies points to the importance of considering international evidence when calculating credit gaps for individual economies. That said, the practical relevance of the improvement is limited. Over a ten year horizon policy makers could expect one less wrong call on average.

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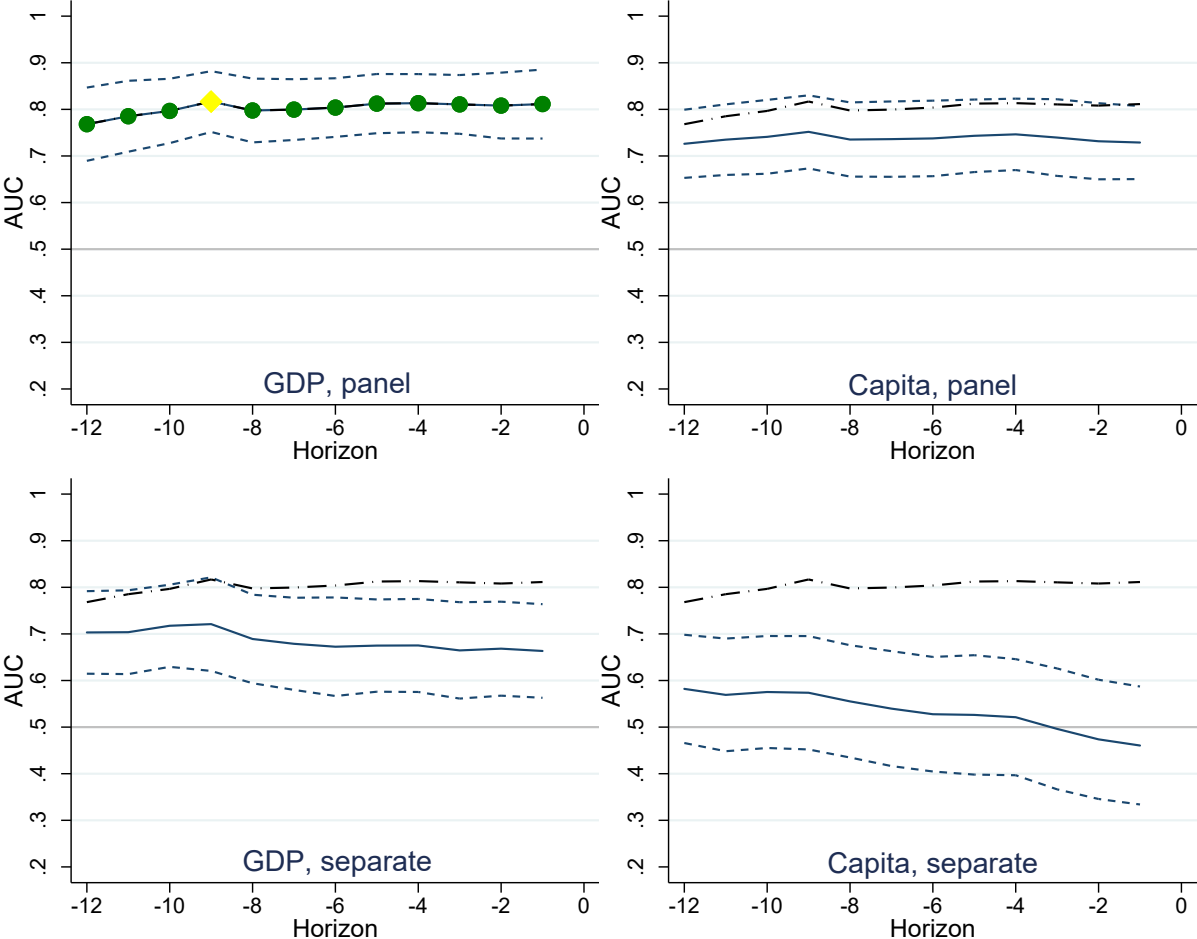
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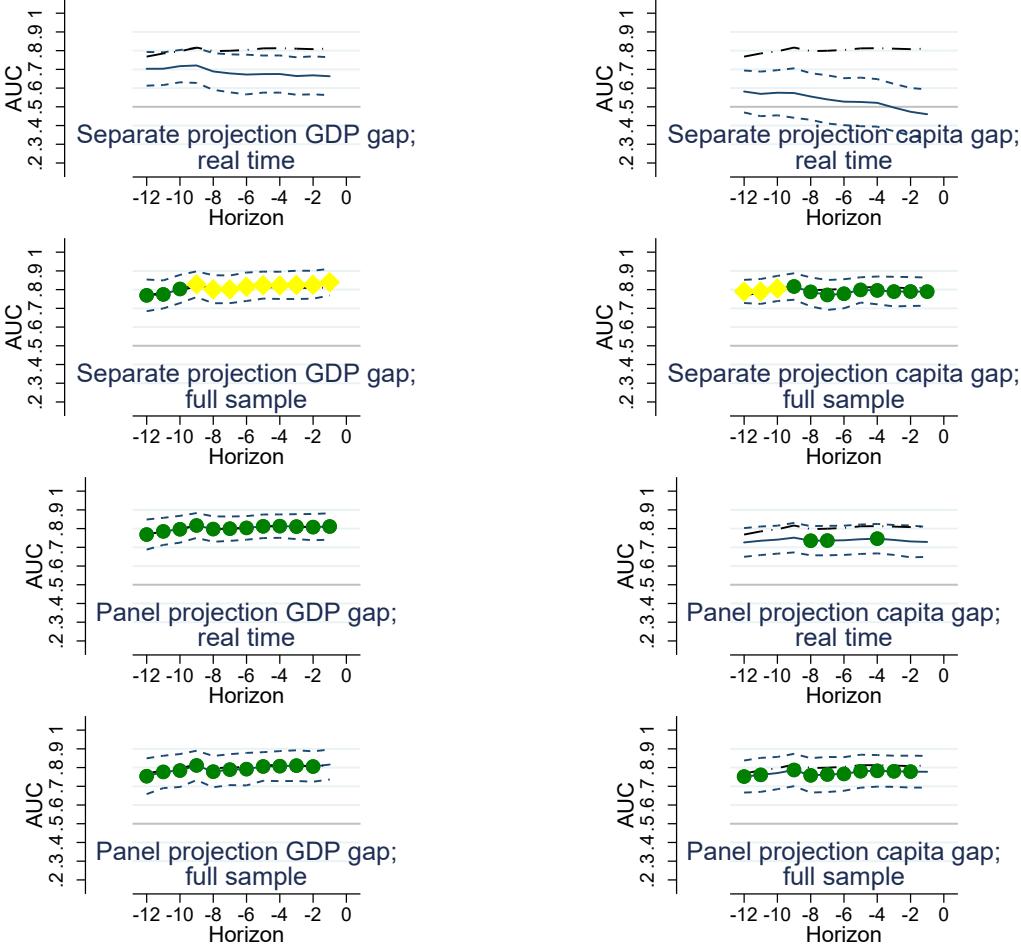
# Manuscript graphs and tables

Graph 1: AUCs for different measures of the linear projection gap based on lags 28-29



Note: AUCs for different forecast horizons based on lags 28-29. A dot-dash line indicates the results for panel estimation with GDP normalisation on lags 28-29, for ease of comparison. For the full set of graphs for  $h \in \{20, 24, 28, 32, 36\}$ , each with one, two and four lags, please see the online annex Graph OA1. Horizon: quarters before crises. Solid line: point estimates; dashed lines: 95% confidence intervals. Yellow diamond: highest AUC across the 60 specifications at that given forecast horizon. Green dot: AUC is not statistically different from the highest AUC at this horizon at 95% confidence level, based on bootstrapped critical values using 1000 replications.

Graph 2: AUCs for different measures of the linear projection gap for  $h = 28$  with two lags



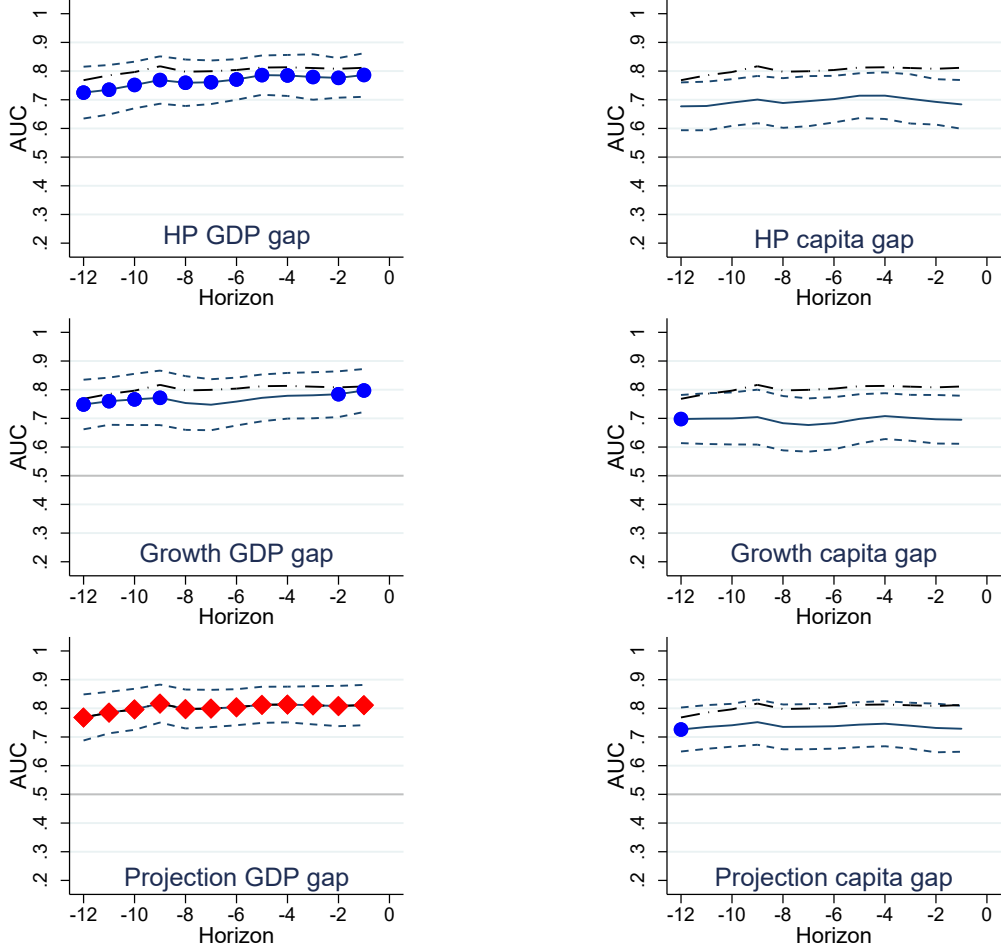
Note: AUCs for different forecast horizons. A dot-dash line indicates the results for real-time panel estimation with GDP normalisation, for ease of comparison. Horizon: quarters before crises. Solid line: point estimates; dashed lines: 95% confidence intervals. Linear projections based on economy-by-economy real time estimates (top row), economy-by-economy full sample estimates (second row), panel real time estimates (third row) and panel full sample estimates (bottom row). Left column is credit normalised by GDP, and right column is based on real credit per capita. Yellow diamond: highest AUC across the eight specifications at that given forecast horizon. Green dot: AUC is not statistically different from the highest AUC at this horizon at 95% confidence level, based on bootstrapped critical values using 1000 replications.

Table 1: Different credit gap measure labels

		<b>Gap measure</b>		
		<b>Difference from one-sided HP trend</b>	<b>5 year growth</b>	<b>Residual from linear projection</b>
<b>Normalisation</b>	<b>GDP</b>	HP GDP gap (Baseline)	Growth GDP gap	Projection GDP gap
	<b>Real credit per capita<sup>1</sup></b>	HP capita gap	Growth capita gap	Projection capita gap

Note: <sup>(1)</sup> To overcome the scaling problem of real credit per capita, ln(real GDP per capita) is used.

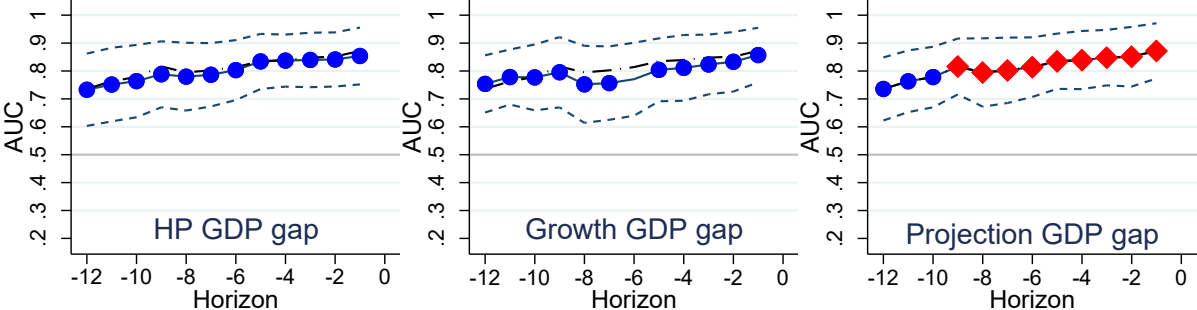
Graph 3: AUCs for different measures of the credit gap



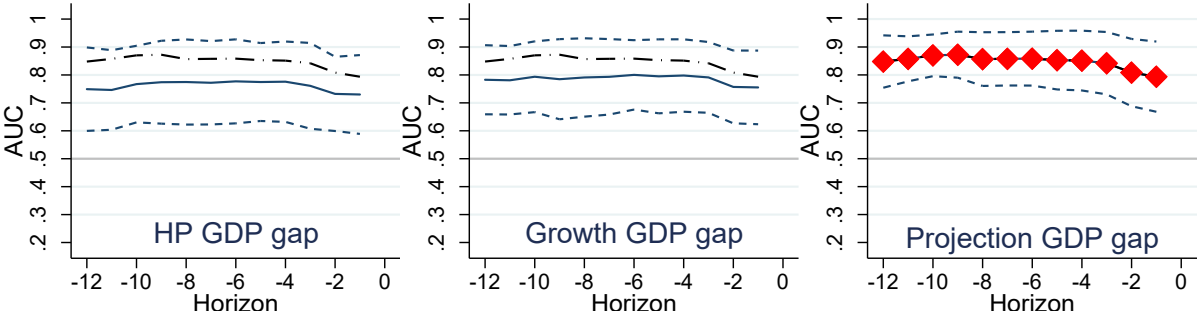
Note: AUCs for different forecast horizons. A dot-dash line indicates the results for the projection GDP graph, for ease of comparison. Horizon: quarters before crises. Solid line: point estimates; dashed lines: 95% confidence intervals. Red diamond: highest AUC across the six specifications at that given forecast horizon. Blue dot: AUC is not statistically different from the highest AUC at this horizon at 95% confidence level, based on bootstrapped critical values using 1000 replications. See the online annex for the underlying statistics (Table OA3).

Graph 4: AUCs for different measures of the credit gap – different time periods

1985q1-2000q4



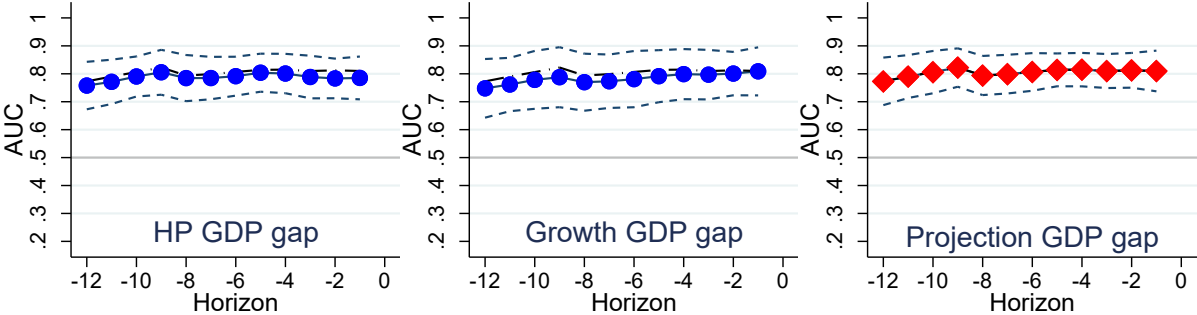
2001q1-2017q3



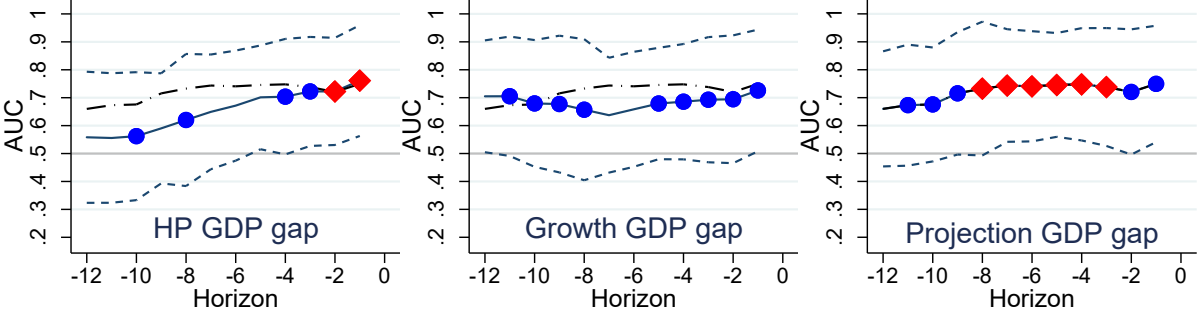
Note: AUCs for different forecast horizons. A dot-dash line indicates the results for the projection GDP gap for the respective time periods. Panels based on normalising by population are excluded to save space, but are available in the online annex (Graphs OA4.1 and OA4.2). Horizon: quarters before crises. Solid line: point estimates; dashed lines: 95% confidence intervals. Red diamond: highest AUC across the six specifications at that given forecast horizon for the respective time period. Blue dot: AUC is not statistically different from the highest AUC at this horizon at 95% confidence level, based on bootstrapped critical values using 1000 replications (for the respective time period). See the online annex for the underlying statistics (Tables OA4.1 and OA4.2).

Graph 5: AUCs for different measures of the credit gap – different country groups

Advanced economies



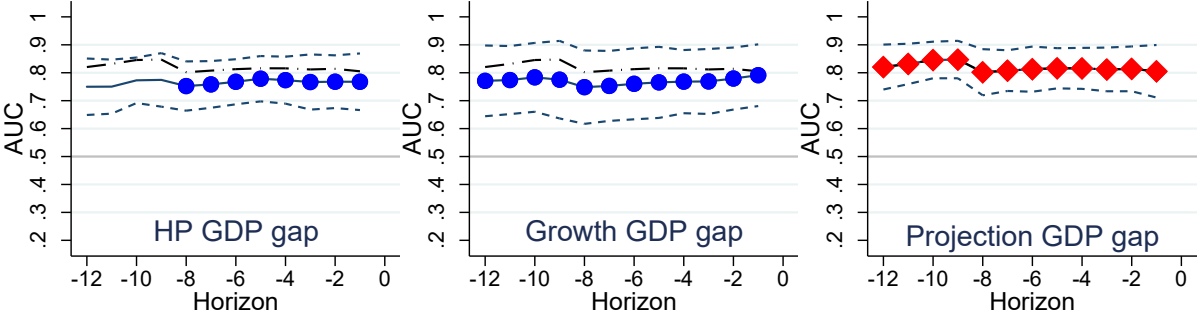
Emerging market economies



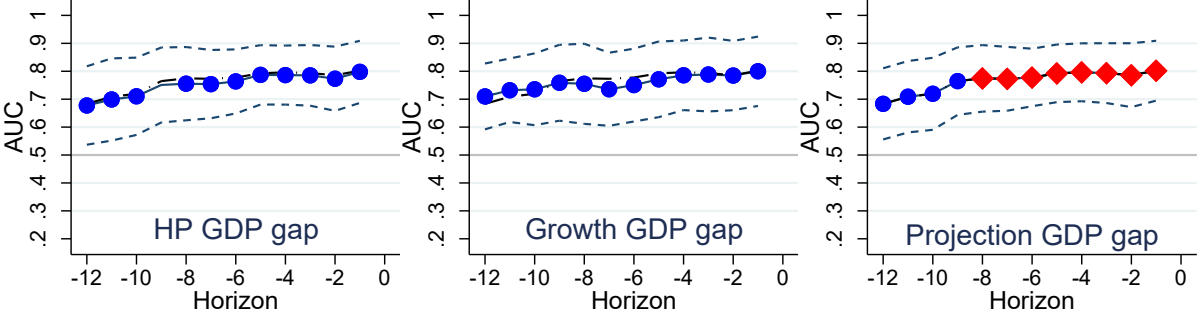
Note: AUCs for different forecast horizons. A dot-dash line indicates the results for the projection GDP gap for the respective country group. Panels based on normalising by population are excluded to save space, but are available in the online annex (Graphs OA5.1 and OA5.2). Horizon: quarters before crises. Solid line: point estimates; dashed lines: 95% confidence intervals. Red diamond: highest AUC across the six specifications at that given forecast horizon for the respective country group. For horizons 9 to 12, the growth capita gap has the highest AUCs. Blue dot: AUC is not statistically different from the highest AUC at this horizon at 95% confidence level, based on bootstrapped critical values using 1000 replications (for the respective country group). See the online annex for the underlying statistics (Tables OA5.1 and OA5.2).

Graph 6: AUCs for different measures of the credit gap – countries with or without crises in GFC

With crisis in the GFC



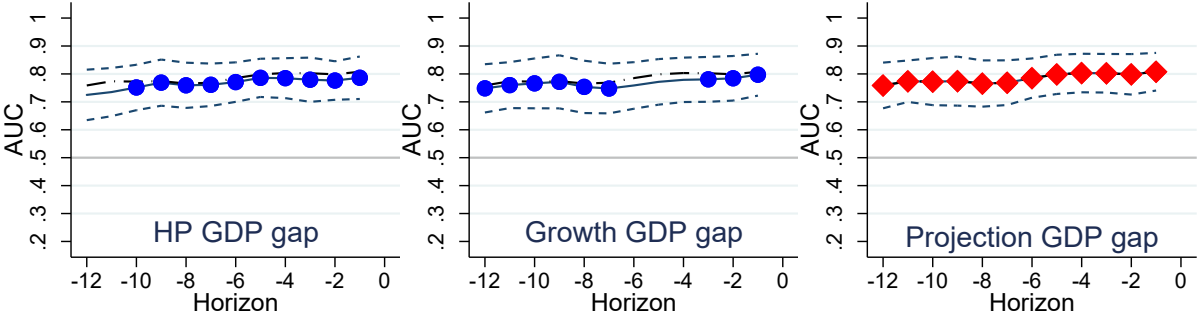
Without crises in GFC



Note: AUCs for different forecast horizons. A dot-dash line indicates the results for the projection GDP gap for the respective country group. Panels based on normalising by population are excluded to save space, but are available in the online annex (Graphs OA6.1 and OA6.2). Horizon: quarters before crises. Solid line: point estimates; dashed lines: 95% confidence intervals. Red diamond: highest AUC across the six specifications at that given forecast horizon for the respective country group. For horizons 9 to 12, the growth capita gap has the highest AUCs for countries without crises in the GFC. Blue dot: AUC is not statistically different from the highest AUC at this horizon at 95% confidence level, based on bootstrapped critical values using 1000 replications (for the respective country group). See the online annex for the underlying statistics (Tables OA6.1 and OA6.2).



Graph 7: AUCs for different measures of the credit gap, Projection GDP gap based on lags 20-23



Note: AUCs for different forecast horizons. A dot-dash line indicates the results for the projection GDP gap. Panels based on normalising by population are excluded to save space, but are available in the online annex (Graph OA7). Horizon: quarters before crises. Solid line: point estimates; dashed lines: 95% confidence intervals. Red diamond: highest AUC across the six specifications at that given forecast horizon. Blue dot: AUC is not statistically different from the highest AUC at this horizon at 95% confidence level, based on bootstrapped critical values using 1000 replications. See the online annex for the underlying statistics (Table OA7).

Table 2: The fraction of correct and wrong signals of the HP and projection gap  
In percent

			Projection GDP gap				<i>Total</i>
			Normal		Pre-crisis		
			Correct	Incorrect	Incorrect	Correct	
<b><i>Full Sample</i></b>							
<b>HP GDP gap</b>	<b>Normal</b>	Correct	66	5			71
		Incorrect	7	22			29
	<b>Pre-crisis</b>	Wrong			25	9	34
		Incorrect			9	57	66
<b>Total</b>			73	27	34	66	
<b><i>Robustness checks (average)</i></b>							
<b>HP GDP gap</b>	<b>Normal</b>	Correct	64	5			69
		Incorrect	8	23			31
	<b>Pre-crisis</b>	Wrong			27	7	33
		Incorrect			7	60	67
<b>Total</b>			71	29	33	67	

Note: Fraction of correct and incorrect signals in normal and pre-crisis periods. A crisis signal is issued if the gap breaches the critical threshold. For the projection gap the threshold is 14.9. For the HP GDP gap the threshold is 6.0. If a crisis signal is issued in a pre-crisis period, it is counted as correct. If it is issued in normal times, it is counted as incorrect. Pre-crisis periods are the 12 quarters in the run-up to a crisis. For robustness, we also show the average fraction in each cell for the sample splits shown in Graphs 4-6 (pre 2001, post 2001, advanced economies, emerging markets, countries that had a crisis during the GFC, countries that did not have a crisis during the GFC).

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